Social Science Frontiers

Occasional Publications Reviewing New Fields
for Social Science Development

**Social Forecasting Methodology:
Suggestions for Research**

by Daniel P. Harrison

Russell Sage Foundation
1976

Single copies of this paper are available free of charge.
The cost of additional copies is $1.00 per copy.
Orders for multiple copies should be accompanied by a check
or purchase order and sent to Russell Sage Foundation,
230 Park Avenue, New York, New York 10017.

Russell Sage Foundation
230 Park Avenue
New York, N.Y. 10017

Printed in the United States of America.
Library of Congress Catalog Card Number: 75-41511
Standard Book Number: 87154-376-1

Contents

Introduction

There is no need to justify research on social forecasting methods; Duncan, de Jouvenel, Helmer, Zarnowitz, Hoos, and others have already done this in some detail. Nor is there a need to argue about the feasibility of doing social forecasting; the empirical record clearly indicates that it is feasible. Nevertheless, the consensus of critics is that social forecasting methodology is in an underdeveloped state. This paper suggests a number of areas in which methodological research could be usefully done. Readers more expert than I in any or all of the forecasting areas mentioned will undoubtedly be able to revise or add to the list; it is hoped that they will do so.

The usual starting point in discussing forecasting methods is to present an inventory of methods currently in use. Since there are frequently new developments in methodology, there is always a need to update the inventory from time to time. While there are several categorized inventories to choose from, I have found the following designations to be reasonably comprehensive. Further details on these methods can be found in the section entitled "Critical Assessments of the Methods."

Consider first the most frequently used forecasting methods.

Extrapolative Forecasting

The procedure consists of identifying an underlying historical trend or cycle in social processes that can be extrapolated by means as varied as multiple regression analysis, time series analysis, envelope curve fitting, three-mode factor analysis, correlational analysis, averages, or any other method that takes current and historical data as the principal basis for estimating future states in a given variable.

Intuitive Forecasting

Expert judgment is used to estimate future states in given variables. Reliance is placed entirely on the expert's knowledge; formal analyses may or may not underlie such judgment, and forecasting rationales may or may not be explicitly stated. Sometimes the expert's judgment is exposed to competitive estimates by the expert's peers and then reassessed by the expert in an iterative fashion until a consensus emerges within the group. This conferencing technique is often referred to as the "Delphi method." Writers of plausible scenarios are also engaging in intuitive forecasting.

Analogy Forecasting

Mathematical, physical, biological, animal, chemical, or other processes in the nonhuman domain or individual, small group, or societal processes in the human domain are used to estimate probable futures in yet another social domain. Biological growth curves, gravity models, much of the General Systems literature, personifications of nation-state behavior, and logistic curves exemplify the kinds of techniques commonly employed in this mode of forecasting. Historical analogy, employing comparisons of events that occurred in different places or times and in the context of different underlying backgrounds and motivating conditions, also falls in this category.

Modelling Forecasting

Models or abstract representations of a social process are constructed either deductively (a less frequently used approach) or inductively. Econometric modelling, the system dynamics procedure employed by Forrester and Meadows, the hierarchical system dynamics procedure employed by Mesarovic and Pestel, and the axiomatic models of Brams and O'Leary serve as examples of this kind of methodology. Relationships among variables are typically expressed in quantitative terms (although this is not a prerequisite) using either empirical data (the inductive mode) or a priori estimates of relationships (the deductive mode). These representations are designed to re-create at least the key dynamic

properties of social processes via mathematical functions, measures of association, probabilities, or — usually — least square fits among data points.

Survey Forecasting

The procedure is to administer survey instruments to systematically selected samples of respondents either in a panel design (using the same respondents in successive surveys) or in a cross-sectional design (using samples of individuals drawn at successive points in time), both with a view to identifying emergent trends in or point estimates of attitudes. If trends can be identified, they are seen by some as good predictors of behaviors or other attitudes, albeit typically for a relatively short time horizon. Point estimates are also considered useful for forecasting purposes, as, say, in consumer confidence surveys.

Criterion Analysis

This awkward term, which I have coined for lack of an agreed-upon name, applies to analyses that attempt to optimize resource allocations given normative (i.e., value judgmental) constraints — the criteria. Strictly speaking, criterion analysis is a form of model building, but because of the special preoccupation with optimization and because the models developed are often highly abstract mathematical representations lacking theoretical or empirical justification, this mode of forecasting is treated in a separate category. Operations researchers, who typically use this form of analysis, are very inclined to speak of "modelling" a process, but such modelling, as noted, is done in a very abstract way; this does not make it any less a model, however. Analysis utilizing such techniques as integer, linear, quadratic, or dynamic programming, queueing theory, or other techniques of operations research is usually used to estimate such items as optimal allocations of time, dollars, or manpower to achieve given objectives, such as the optimum number of nurses to service patient needs over the course of a hospital day, the most efficient specification of teaching schedules in a school, optimal research and development allocations, etc. The forecasting character of

these procedures derives from their influence on decisionmaking; to the extent that the analysis is incorporated into a plan and the plan is executed, the analysis serves as an indirect forecasting procedure. Application to date has been largely in engineering and economic areas where quantification and mathematization are well entrenched.

We now consider forecasting methods that are used less often.

Environmental Prediction

This mode of prediction focuses selectively on the environmental constraints or structural givens that limit the freedom to act. Disregarding the causal complexity and creative richness of a behavioral arena, this procedure utilizes the identification of boundaries to action as a significant, nontrivial predictor of behavior. Where methods and knowledge of societal processes are limited or variables difficult to quantify, as in sociology, political science, military science, or anthropology, such an approach is quite popular.

Network Analysis

In this method, deterministically or probabilistically computed costs of alternative, hierarchically arrayed patterns of action (networks) are estimated and the most efficient, most probable, or least costly action is chosen as the operational plan, subject to normative requirements. As in criterion analysis, the commitment to execute a plan is tantamount to a forecast. These analyses are carried forth with a variety of techniques, such as Critical Path Method (CPM), Program Evaluation and Review Technique (PERT), or relevance analysis, and are typically diagrammatically represented in the form of a logical process tree or network — thus the reference to network analysis. There are other techniques, such as morphological analysis, perspective trees, Planning Assistance Through Technical Evaluation of Relevance Numbers (PATTERN), and Bayesian decision analysis, that add to the summative computation of various combinations of costs (dollars, time, manpower, etc.) or process options, a judgmental estimation of weights or probabilities and thus fall in a gray area between network analysis and multimethod forecasting (see page 9).

Predictive Social Laws

The approach here is to identify regular patterns of action in social processes, conditional and unconditional, that can be used for predictive purposes. Some historical and political science research, for example, attempts to identify such general laws of social behavior across time and in various behavioral contexts. If antecedent conditions are empirically identified and consequents systematically deduced, such analysis falls in this category. A full variety of conventional statistical procedures and mathematical curve fitting is used in an empirical format in studies of this kind.

Clinical Prediction

This method involves the forecasting of what individuals or societies will do, given information about attitudes, beliefs, past experiences, etc. and observations on their or its behavior. The transfer of this procedure from the clinic to the society at large is referred to as "social psychiatry"— an enterprise that is generally subject to severe criticism as being too superficial and anthropomorphic. At the individual level, however, the insights of clinical psychologists and psychiatrists are brought to bear on data on the behavioral history of and personal interview information from an individual to estimate likely future response patterns and self-initiated behaviors. Clinical psychologists and psychiatrists are quick to point out that they are merely focusing on treatment prognoses, but there are instances, as in court testimony, where behavioral forecasts are offered. This approach is particularly congenial to the study of societal elites or influentials where the claim is that the prediction of elite behavior will do much to predict the direction of the society as a whole.

The methodological basis for clinical prediction is divided between actuarial prediction (prediction based on the performance of individuals on intelligence tests, standardized personality tests, or other relatively precise measurement instruments) and analysis (prediction based on the observation and interviewing of individuals, theory, biographical information, and, of course, professional judgment), although some have attempted to combine the two approaches. Assuming that clinical prediction can work, at least for individuals, there is still an unresolved

debate as to the role of individuals in societal history. Students of elite studies feel that they have an explanatory and predictive handle on the general direction of societal events. Critics argue that everyday workings of social processes remain unexplained in elites research and suggest that overall environmental conditions within and about a society are a more important determinant of events than a handful of elites.

Finally, we consider some forecasting methods that are used infrequently.

Theoretical Forecasting

The potential overlap between this method of forecasting and modelling is very strong. At present, the two are differentiated in practice by the *reasoned* explicitness with which theoretical forecasting links the causal relationships among variables, which modelling forecasters generally do not do. The theory may or may not be empirically grounded. Its special advantage is that it offers specific, testable causal predictions of phenomena, the accuracy of which can be tested as events unfold. Here the reasons for a failure to predict, as well as for successes in prediction, can be understood, and, thus, unlike many other modes of prediction, one learns from failures as well as successes in prediction. Theoretical forecasting typically suffers from ambiguity in expression, however; it seems that the favorite pastime of many scholars is to try to determine what the theory "really" means or to deduce unstated corollaries to theorems.

Intuitive Planning

Again, a plan is considered to be a forecast when given the sanction of a commitment to action. Strictly speaking, intuitive planning is not a method — rather, it is a subjective, nonexplicit, nonreproducible estimation of a likely course of events in a plan. Perhaps if one has an authentic clairvoyant or genius, it might be successful beyond the level of chance. However, an evident paucity of proven experts on social processes has not prevented some policymakers from adopting this fast, inexpensive, and methodologically sloppy approach in their work.

Theoretical Planning

This procedure is identical to theoretical forecasting except that the result is a plan of action, based on explicit theoretical causal considerations that, given a commitment to execution, serves as a forecast. The properties of this procedure are the same as for theoretical forecasting. Since most areas in societal theory, outside of economics, are underdeveloped, the procedure has not seen a great deal of use. Where data are not obtainable or are suspect, it can be of considerable use.

Multimethod Forecasting

It is quite common to find a number of the preceding types of forecasting methods combined in various ways to produce a forecast. Because this mode of forecasting has unique methodological problems deriving from the fact of aggregation of method, it is regarded as yet another type of forecasting termed "Multimethod Forecasting." In terms of frequency of use this mode of forecasting belongs in the first "frequently used" grouping. No guidelines have been formulated for the best mix of methods to use for a given social forecasting problem.

Two basic approaches are used to combine forecasting methods. One is an attempt to arrive at a convergent forecast by applying several different forecasting methods to the same set of data; if a convergent forecast does result, the researcher assumes that he can state his forecast conclusions with a greater degree of confidence (the exact degree is unknown) than if only one method is used. The basis for the assumption of increased accuracy via convergence is not explicitly elaborated. The other employs a strategy for forecasting method selection based on the claim that different aspects of social process are best analyzed with different forecasting methods. The second approach is the most frequently used, but the specific rationale for method selection is generally not articulated. Indeed, at this level of methodological development, method combining seems to be guided by intuition or purely pragmatic considerations. For example, clinical forecasting procedures are used to estimate the probable actions of key decisionmakers in an elites concept of social process; extrapolative and analogy forecasting methods are used to estimate the demographic characteristics of the social

fabric; survey forecasting techniques are used to estimate trends in values in social classes, age groups, or other broad social categories; gravity models are used to forecast transportation patterns or population migration patterns; and so forth. A total social forecast covering a variety of topic areas could therefore employ a variety of methods.

Alternatively, a single forecasting method could employ two or more submethods. Thus, for example, an attempt to develop a forecast employing a model of social process might rely on the estimation of key parameters in the model at current or future levels by expert opinion (intuitive forecasting) or survey forecasting estimates. The PATTERN procedure employs a mix of subjective judgments, extrapolative forecasting modelling, and network analysis to yield a forecast. Survey forecasts, particularly in panel analysis designs, frequently employ a mix of cross-sectional surveys and trend (extrapolative) forecasts. Finally, variations of single type of forecasting method are also employed, such as a mix of time series analysis techniques or a mix of curve fitting and envelope techniques, to yield an extrapolative forecast.

A person interested in finding a sound description of available social forecasting techniques is bound to be disappointed. There is as yet no single source that describes the full range of forecasting techniques in sufficient detail that another researcher could reproduce the procedure in an actual forecasting application although some texts explain how to apply particular forecasting methods.* The broad forecasting methods surveys that are available with the possible exceptions of Ayres; Martino; Butler, Kavesh, and Platt; and Chisholm and Whitaker are too superficial

* For example, on intuitive forecasting, see Helmer and Gordon; on modelling forecasting, see Forrester, Meadows, Mesarovic, Brunner and Brewer, and Johnston; on extrapolative forecasting, see Ayres, Martino, Draper and Smith, and Quenouille; on survey forecasting, see Cohen, Glock, Moser and Kalton; on analogy forecasting, see Ayres, Mazlish, and Martino; on criterion analysis, see Bellman and Dreyfus, Dantzig, and Thierauf and Klekamp; on environmental prediction, see Young, Sprout and Sprout, and Chapman; on network analysis, see Raiffa, Phillips, Swager, Moder and Phillips, Sheppard, and Zwicky; on predictive social laws, see Singer, Richardson, Denton and Phillips, and Sorokin; on clinical prediction, see Rotter, Waldhorn, Wolman, Webber, and Peterson; on theoretical forecasting, see Duncan; on intuitive planning, see Steiner; on theoretical planning, see Stolper; and on combined methods, see Abt, Bates and Granger, and Martino. Butler, Kavesh, and Platt; Jantsch; Bright and Schoeman (1973); and Chisholm and Whitaker provide useful methodological insight on a number of the above forecasting methods.

to substitute for the specialized texts. The specialized texts that are available tend toward technical and economic subjects rather than social forecasting. It is a major research task in itself to scan the range of forecasting literature in a number of disciplines and application areas to come up with a comprehensive listing of available forecasting methods, some of which might be applicable to social forecasting.

When one talks about research on social forecasting methods, perhaps one of the first things that comes to mind is the viability of the procedures and algorithms employed in the methods. It is possible to evaluate the properties and limitations of forecasting methods in the abstract; the analysis is given more concrete relevance by considering each method in terms of some aspect of social process it would be likely to be applied to. We will consider needed research in these areas for each of the thirteen types of forecasting methods previously mentioned plus the combined forecasting methods approach.

Extrapolative Forecasting

There are, as noted, a number of quantitative techniques that may be employed to do extrapolative forecasting. A frequently employed technique is linear and curvilinear multiple regression. Brunner and Brewer have shown that regression is very sensitive to changes in parameters such that projections beyond a short time period (say, four or five time periods) are subject to substantial misestimation. Furthermore, it is common knowledge

that regression in its usual form yields a biased trend estimation due to its minimization of vertical distances among data points, as opposed to orthogonal distances vis-à-vis the trend line; this problem is aggravated by deviant (outlier) values that give disproportionate weight to larger over smaller values. Not surprisingly, regression based on just a relatively few data points is very unstable compared with regression based on a number of data points where deviant values might be averaged out. Regression is not particularly effective with circular or widely scattered data patterns although this can be compensated for, to some extent, by appropriate transformations of data; this is not to say that totally unrelated variables will be reconciled by the use of transformations, however. More difficult to deal with are two-way causation between independent and dependent variables, intercorrelation among independent variables (multicollinearity), correlation between independent variables in successive time periods (autocorrelation), and omission of important variables. Some remedial steps, such as lagging of independent variables in cases of multicollinearity, solve some problems but raise others.

The preceding are problems that are currently the concern of statisticians but remain unresolved; it appears for the present that resolution might best lie in terms of investigation in concrete application cases. Evidently, where short-term forecasts will do, where a good number of observations are available and are of reasonable accuracy, where data values are relatively linear as opposed to circular in pattern, where social processes are relatively stable as opposed to rapidly changing, and where outliers are at a minimum after transformations of data, regression is a viable method to use.

What kinds of social process exhibit "well-behaved" properties of this kind? This is an empirical question to be sure, but some candidates for application immediately come to mind — predicting socioeconomic status (SES) from level of education from parental SES, predicting party identification from parental party ID, predicting SES from race, and so forth. And then there appear to be some very unpromising or highly variable topics for the use of regression forecasting — predicting crime levels from urban versus suburban residential location, predicting female faculty appointments in higher education from male and female graduate school grades, predicting residential location from place of first full-time employment, etc.

Some properties of regression, such as the vertical best-fit bias, can be compensated for by adjusting computational results with the use of orthogonal transformations of distances between data points and the computed regression line. Method specialists can do much to improve the algorithm for regression and the effectiveness with which it is applied to data. At the least, users of this method should give consideration to the limitations of the method and the properties of their data before dashing headlong into an application of the regression technique for short-run forecasting. It might be good practice to notify the reader of defects in a procedure that might bias a forecast.

Another extrapolative forecasting methodology, time series analysis, is an extremely heavy consumer of data, particularly spectral analysis. Unless one has a great number of observations to work with, the latter technique is unlikely to be of use. One common form of time series analysis is based on regression analysis applied iteratively to successive time periods; however, the procedure is highly sensitive to autocorrelation among variables in successive time periods — a phenomenon that can be compensated for by examining residuals in trends but which seems to be an unrealistic requirement to make of social processes since they are often highly linked from time period to time period.

A test statistic for the significance of inter-series association frequently used in conjunction with the regression analysis of time series is the Durbin-Watson statistic; however, apart from the range of data values for which the Durbin-Watson statistic is inconclusive, the choice of significance level can alter greatly the kinds of conclusions one can draw from such an analysis. There needs to be some discussion of the kinds of guidelines that should be used in selecting significance levels in a social forecasting exercise employing the Durbin-Watson statistic, or at least some defense of a particular significance level chosen in the regression analyses of time series. The Durbin-Watson statistic also suggests spuriously significant lead and lag associations when regression weights between series are low. There needs to be some discussion of the minimum acceptable levels for regression weights in such Durbin-Watson computations. Research should be undertaken to find computationally manageable ways to improve upon current procedures for estimating the Durbin-Watson measure for inconclusive data values and alternatives to the Durbin-Watson statistic.

Some thought needs to be given to the kinds of social processes that have sufficient observation points to permit time series studies to be made. Studies have been done, for example, on business cycles, fire company and police responses to alarms, congressional voting patterns, and global internation interaction patterns.

A popular mode of forecasting, particularly in technological forecasting, is envelope curve analysis. This method disaggregates a forecast into subcomponent forecasts across time, utilizing curve fitting techniques to fit the data points of the subcomponents. No attempt is made to find a complex polynomial or exponential function that will fit all data points for all components. The envelope curve is the smoothest, usually hand-fitted, curve that will pass as tangentially as possible to all the subcomponent curves (the envelope property) over time and serves as the forecasting curve for the aggregate.

The procedure works best with subcomponent variables that exhibit change over time, rather than variables that are subject to constraints that are not time dependent. If one has as the dependent variable an SES measure for a population subgroup, then time variant independent variables, which are subcomponents of SES, might be mean levels of education attained for the subgroup, mean earned income levels of the subgroup, percent of individuals in the subgroup owning their own homes, and family size. The expectation would be that whatever the rate of growth of these independent variables over time, they would eventually level off since resources and job opportunities are finite and economic conditions are subject to change; as a result the envelope approach would be of limited use in this example.

Some empirical rules of thumb would be needed to establish the likely time horizon of an envelope forecast, but this is as close to being scientific as the procedure gets. The assumption is that broad trends represented by the envelope curve are not subject to rapid change, an assumption that greatly limits the range of applicability of the procedure to the study of social processes. Unlike the regression procedure there is no single computing algorithm to critique since there are an indefinite number of polynomials or exponential functions that could be fitted to the data points; however, goodness of fit measures, such as Chi-square and the coefficient of determination (R^2), should be employed to estimate a given fit. The envelope curve fitting technique is

difficult to use when data are not well-behaved although data transformation procedures can be used to dampen problems, such as excessive variability. To some extent these are all empirical matters, which, if envelope curves are to be used for social forecasting, need to be resolved by researchers.

In social forecasting there is a great need in almost all the known extrapolative methods for an explicit statement of the algorithmic, theoretical, and empirical weaknesses or sensitivities of such procedures. Such a discussion, as noted, would be more meaningful if carried on in the context of an analysis of some specific aspect or aspects of social process.

Intuitive Forecasting

By definition this procedure for obtaining forecasts is not explicitly method based. While the expert making his forecast may perform some kind of mental calculation, as far as a communicable method is concerned there is none. There is a technique, the Delphi technique that does attempt to superimpose some systematization of the intuitive process by first obtaining an estimate from a group of experts and then using the results of the first forecast estimate as feedback to the same group of experts, who are then invited to revise initial estimates in light of the group norm. The process is repeated until either a convergence in estimates develops or no further change in views can be seen. Convergence usually seems to develop although on some of the tough key issues agreement is difficult to elicit.

Whether it is a desire to conform to group norms or a procedure that enables correct forecasts to be ferreted out via the group estimation process that accounts for the tendency toward convergence in Delphi estimates is not clear. The users of the procedure point out that peer group pressures are minimized by conducting the evaluation in such a way that the sources of individual estimates are not identified while the logic of the estimates of the group as a whole is made explicit for individual expert consideration and possible adjustment. While some studies have been done to assess the conformity hypothesis, more research would be useful particularly for social forecasting; such research could also assess the logic and preparation used by intuitive forecasters since it might be that the experts had in fact done a

systematic investigation mentally or recalled the results of such an investigation without indicating that on paper or in a conferencing interview situation.

Study needs to be made of the manner in which experts are identified and selected to be on the social forecasting Delphi panels in the first place. Does this predetermine results since the views of experts, by definition, are known in advance? If experts are picked as experts in one field or subject area and are asked to forecast in unfamiliar subject areas, would this not weaken the method? As noted, identifying experts on social process is not likely to be an easy, or, possibly, even feasible, task — who, as Dror notes, after all, *are* the experts on social process? The answer at this point would probably have to be that the experts are expert on selected aspects of social process; the selection of a Delphi panel would have to be well-balanced to cover all the areas of relevance to a particular social forecast topic. It is not clear that there could be truly authoritative convergence from divergent areas of expertise; convergence would have to be investigated beyond mere empirical agreement. There are some social scientists who are interested in studying the reasons for the analytical success of geniuses and clairvoyants. All things considered, however, the intuitive forecasting procedure is likely to be a rather barren area for methodological inquiry.

Analogy Forecasting

Analogy forecasting suffers from intellectual difficulties not unlike those of extrapolative forecasting — there is always the problem of deciding just how far the analogy can be pushed. Some, such as the General Systems theorists, feel that a systematic examination of analogous processes at varying levels of complexity in living organisms — from the cellular to human to societal or even intersocietal levels — will reveal laws of process common to them all. But as yet such a claim remains conjectural (see the writings of Von Bertalanffy and Miller on GST) and does not extend from nonliving to living systems. Methodologically at this point analogy remains a heuristic tool. This leaves the task of evaluating the methodological limitations of systematic procedures that are used to develop analogy-based social forecasts.

I pass over the verbal biological metaphors, such as Social

Darwinism, which provide theoretical bases for explaining or predicting social behavior and, instead, consider specific methods offered for forecasting social processes by analogy. Brewer offers a scathing critique of the use of the magnetic hysteresis loop as a computer-compatible means of predicting the effect on rent rates of the demand for rental properties in a large city (San Francisco). The difficulty is that rent rates are subject to the speculative practices of entrepreneurs (including deliberate underutilization of rental space), discriminatory housing practices, limited numbers of available housing of a particular type in a preferred location, work commuting difficulties, and nonlinearities in supply and demand relationships, which all act to invalidate the use of the analogy for the prediction of true residential rent pressures. Brewer cites empirical studies confirming these inadequacies in the analogy — studies that were available at the time the analogy was selected for use in the San Francisco study.

Not all analogies are unsuccessful, however. Pearl produced a forty-year United States population forecast exhibiting 1.5 percent error by using a logistic (symmetric S-shaped) curve, the next ten years produced a 7 percent error, and the final ten years an 18 percent error; this indicates that some basis for selecting a cutoff point for the applicability of the analogy was required. A nonsymmetrical logistic curve devised by Gompertz has been used successfully to estimate mortality rates and income distributions.

A third similar S-shaped growth curve has been developed by Von Bertalanffy. Other curves of varying degrees of complexity are also used for analogy forecasting, such as learning curves (S-shaped) to generalize individual performance/time to large social organization performance/time.

The parameters of such logistic curves can be estimated using data analysis techniques, such as regression analysis (subject to corrections for the effects of divergent data on regression estimates); with relatively few data points — a key advantage of the use of analogy in new situations — logistic curves can be used to yield a forecast of appreciable duration. The goodness of fit of forecast estimates to real data can be determined and the adequacy of the models as forecasting devices can be assessed. Assuming that the fit is satisfactory the validity of the growth curve forecasting procedure depends on the validity of the assumption of the particular limit value selected.

There is a great deal of intellectual resistance to the use of such growth curves for subjects as diverse as forecasts of population or the growth of scientific knowledge because of the lack of explicit rationale underlying such measures. If a pragmatic test is the only one to be used, there is no certainty that a growth pattern in yeast will predict the growth of urban crime unless historical case studies show that, in fact, the two are very similar. Until such studies are made there is no certainty that such curves will be of forecasting use. This would be a useful research task for those interested in this mode of prediction.

Modelling Forecasting

Modelling forecasting is one of the most difficult of the forecasting techniques. Being abstractions from reality model variables are *relatively* few and strategically selected. The principal bases for assessment of model adequacy are model behavior (output and intermediate throughput) which is often quite abstract compared with referent world behavior, choice of variables, operationalization of variables, and the reasonableness of the postulated model structure. Where verbal models are used such estimates, of necessity, are made with less precision.

No fixed rules are available to tell the would-be modeller how complex to make his model in terms of structure or number of variables, except possibly the observation that the number of measures required to validate stated estimates in model variables increases exponentially with the number of variables; model estimates should also be in the proper direction as well as in the expected magnitudes. Nor, in the case of social processes, is there a broad base of theory to use to construct models. Data problems compound the difficulty of the modelling task even more.

Model parameters can to some extent be estimated by data analysis procedures, but there is no assurance that parameter values will remain fixed for the duration of the forecast. Some modelling efforts attempt to anticipate changes in parameters by using intuitive and other kinds of forecasting methods to supply look-up tables for parameter values that would register parameter changes over time. Other modelling efforts attempt to get around the parameter instability problem by using an iterative computational procedure. Nevertheless, with all these problems,

does anyone seriously attempt to model social processes for forecasting purposes? The answer is "yes" but not a great many.

Some models take the form of sets of interdependent regression equations (econometric models), specified in terms of theory or research findings. These can become extremely complex, exceeding 1,200 equations in the case of the Data Resources Inc. model of the United States economy. Other models take the form of interdependent sets of algebraic equations defined in terms of social science theory and related empirical research. These are usually much simpler than econometric models although, as in the case of Orcutt or Mesarovic, this is not always the case.

The econometric approach is plagued by misspecification problems that affect the validity of model estimates; guidance for specification (inclusion or exclusion of variables or incorrect variable form) and measurement of specification adequacy are only starting to get the attention they deserve from econometricians with few guidelines for such measurement being available at this time.* Recent failures in econometric model forecasts and inaccurate analyses of economic dynamics have perhaps caused a closer look at the specification issue — an issue that econometricians once disregarded saying that economic theory could serve as a sufficient guide for specification.

Certain artificialities are introduced into the econometric modelling procedure: for example, for the sake of obtaining a just identified equation system (the proper number of independent [endogenous] variables [i.e., equal to the number of equations in the model less one] are excluded from any given model equation to permit a unique solution to be found to the equation system), variables may be included that have little theoretical significance, or, conversely, important variables may be omitted for the same reason. The outside observer will doubtless be struck by the unidirectional character of the regression model with feedback properties of processes treated as exogenous input variables or lagged input variables. For some social processes characterized

* The currently available procedure for econometric model equation specification is to use theory or empirical research, preferably explicitly, for variable selection, using dummy variables if need be to enable the equation system to become just identified. Where theory is not yet developed, social forecasters will have their hands full, perhaps in some cases too full, doing the requisite correlational or other analyses necessary to determine the variables that are appropriate for inclusion or exclusion in the model. Too many econometric modelling efforts produce unacceptable results because of the ill-founded character of their variable specifications.

fundamentally by flow, rather than feedback properties, this is not a severe constraint, but for highly interactive social processes — probably the bulk of social processes — this is highly constraining. Social researchers will demand to know what evidence or theory can be mustered to support the unidirectional representation of social processes.

An additional problem is one of instability in model parameter estimates. Numerous econometric forecasters have been embarrassed by unforeseen changes in model parameters in economic subject areas; the same, if not more, could be expected of efforts to forecast social processes. A few econometric modelling efforts, as noted, have dealt with the problem of unstable parameter values by using forecasting techniques to estimate parameters and then incorporating such estimates in look-up tables. Other researchers have dealt with the problem by using iterative regression estimates over relatively short time periods, computed with the aid of numerical analysis procedures (see Millstein and Mitchell). However, this remains an area of much needed research in econometric model building.

Problems of measurement loom especially large in econometric modelling, for, somehow, even the most elusive concepts have to be operationalized for numerical computation.

Once the model equations are specified, there are a number of estimating procedures that are typically used — ordinary least squares (OLS), two-stage least squares (2SLS), and three-stage least squares (3SLS). Full information maximum likelihood estimates are also used but are computationally much more cumbersome, and, thus, less popular. When one has dozens of equations and hundreds of variables as is sometimes the case, it can be seen that solution of the equation system is anything but a trivial matter. There is a need to run Monte Carlo studies to assess the relative performance of these alternative estimation procedures to determine sensitivity to specification error, multicollinearity, distortion in parameters, autocorrelation in disturbances, and measurement error. Johnston reports some studies of this kind, but more are needed for social process models.

There is also some dissatisfaction with aggregate measures of model goodness of fit to data via variance accounted for, but not much offered as an alternative measure; there is clearly a need for research to develop new kinds of goodness of fit measures. Other difficulties associated with regression forecasting procedures

mentioned earlier are applicable here as well, at least for OLS, 2SLS, and 3SLS estimating procedures.

More formal deductive models in the axiomatic sense — developing a model from explicit assumptions — are often dismissed as irrelevant to the analysis of societal processes, either because of their extreme simplicity vis-à-vis the processes they are supposed to represent or because of the untenable character of the assumptions of the models. In the case of descriptive mathematical representations of societal processes, as might be expected, goodness of fit results and the rationales for the models are what is usually disputed. The strongest criticism is usually reserved for the manner in which model variables are operationalized or aggregated — a particularly thorny problem when societal phenomena are vaguely conceptualized and needed data are often very difficult to come by, if available at all. In addition some critics argue that not enough is known about the theory of societal processes to permit viable models of such processes to be constructed. Given the preceding points of opposition to formal models in sociological, political, historical, and anthropological study, it is not surprising that formal modelling efforts are offered almost apologetically with highly visible assurances that the proposed models are merely "heuristic."

Perhaps one of the most publicized debates revolving around model construction is the "limits to growth" controversy precipitated by the publication in 1972 of a book of this title by Meadows, Meadows, Randers, and Behrens. Using techniques developed by Jay Forrester in the industrial dynamics laboratory at MIT and a groundbreaking study published by Forrester in 1971 entitled *World Dynamics*, these authors represented the dynamic processes of worldwide population growth, world industrial production, world food production, pollution, nonrenewable natural resource availability, available industrial and agricultural production capital, GNP per capita, and arable land availability in a very complex causal model (with linkages not always empirically or theoretically validated, as critics have been quick to point out) having a complex feedback loop structure and formalized via ordinary differential equations (not specified in the text) and look-up tables of raw time series data (in lieu of extremely complex functional forms difficult to model).

The highly aggregated character of the Meadows, et al. effort has been challenged by a second study by Mesarovic and Pestel

that disaggregates the total global analysis into ten regional analyses, each presumably characterized by different needs, resources, causal linkages, and properties. Each region, in turn, is postulated to be hierarchically subdivided into five types of processes: from lowest to highest in the hierarchy are the individual, the group, demographic-economic processes, technology, and the environment. In the Mesarovic and Pestel model parameters may be altered over time as the analyst desires. These processes are operationalized in different ways, depending on the type of problem being studied — population growth, development aid, resource depletion (in oil), and other energy resources needed to produce a per capita GNP or an agricultural yield. Model structures, except for the Cobb-Douglas production function in the economic model, are not specified in the Mesarovic and Pestel study. Mesarovic and Pestel undertook their hierarchical, multilevel modelling approach in reaction to the Meadows, et al. model, claiming that the aggregative approach does not allow for variations in subsystem performance, the failure of any one of which could have disastrous implications for the world as a whole. Mesarovic and Pestel reject the claim of Forrester that large-scale social systems operate in counterintuitive ways.

The debate that these texts, particularly the former, has engendered has been prodigious, and numerous criticisms in addition to that indicated by Mesarovic and Pestel have been stated. Here is a sampling from the literature concerning the Meadows, et al. study, although some is applicable to the Mesarovic and Pestel study as well. The Meadows, et al. model assumes a closed, totally defined system when living systems, if anything, are open and variable in relationships and performance over time. The causal logic of the model loops is frequently not made explicit. Counterintuitive model dynamics can be traced to computational artifacts in the model algorithms, rather than to supposedly inscrutable counterintuitive social process dynamics. Similarly, model output is influenced by model structure despite the substantive character of the data fed into the model. Postulated functional forms in the model are not supported by theory or research, with some exceptions. The model assumptions are too rigid; they do not allow for public responses to disastrous situations, such as food shortages. The model omits key political and other behavioral variables that would permit such model

policy changes to be introduced. The quality of life variable is too simplistic. Model validation is inadequate, being limited to "Turing tests" of acceptance by actual government policymakers (who conclude that the model output and throughput seem to work the way real societal processes do). The model does not exhibit adaptive behavior consonant with that of the real world. Model sensitivity is not tested against various parameter values, another aspect of model validation research. There is no sense of selection for the key variables on which study should be focused. Parameters are quantified arbitrarily or when they "cannot" be. The model has too many variables to permit any conceivable systematic evaluation of sensitivity to be done. There are not enough data available now to build a satisfactory world model, and some of the data that are available are used in a faulty manner. The model does not allow for chance events in its predictions. Model delay functions in feedback loops are in some cases too extensive, leading to artificial "catastrophes" in key variables, such as pollution. Some of the model assumptions are not historically valid. The overall model is not sensitive to some of the key subsystem models, such as the population submodel. The model does not sufficiently allow for technological progress to deal with various kinds of problems, such as food production. Important feedback processes have been omitted. Some model equations are faulty. Pessimistic biases have been built into the model. Criticisms such as these have not gone undisputed, but they do point out the character of criticisms that have been leveled against these and other models, such as the more complex urban simulation models.

There has, in general, been a reaction against model complexity in the face of insufficient or poor quality data and insufficient theoretical and empirical research back-up for complex societal modelling efforts. In addition there has been a reaction to excessively deterministic model construction in the face of variable parameters, variable human actions, and chance events, and also a reaction to models that are too exclusivistic in treatment (all economic, all technical, all resources, etc.). On the other hand, modellers who have focused on a manageable, limited portion of social process have been accused of grasping so little that they cannot appreciate the effects of larger forces in determining subsystem behavior. This level-of-analysis problem has not yet been resolved.

Recommendations for research on modelling forecasting of the formal kind (axiomatic or descriptive) are highly dependent on the kind of problem being analyzed — family dynamics are, most likely, going to have a different character than aggregate societal dynamics. However, whatever the particular aspect of societal processes one is interested in developing a forecast for, the viability of assumptions, the quality and availability of supporting theory and research, the adequacy of the formalization of the model, the rationale for the simplifications of the model, the availability and quality of data, the adequacy of the operationalization of model variables, and the adequacy with which model parameters and indeterminacies are represented will be central.

A modeller obviously cannot do all the theoretical and empirical research development de novo in order to do a study; however, he can point out the areas in which a minimum amount of research or theory development has to occur in order for a model to be developed adequately, perhaps using some push to get such research going. He can make assumptions and rationales for model formalization explicit. And, apropos this section of the paper, he should present evidence on the dynamic behavior of his models in terms of throughput behavior, as well as output behavior, via sensitivity analyses with specially constructed data sets and parameter settings. These are all things that should be done for social process models; certainly models that are now in vogue for social forecasting purposes should be evaluated in such terms, if they have not been already.

Much might be learned from a study of formal models of social process that have performed well in a particular domain of interest or in behaviorally isomorphic subject matters. Models that would seem particularly promising for the study of social process would be: (a) those containing a mix of probabilistic factors (to deal with parametric and random variability characteristic of social processes), or, as with the Mesarovic and Pestel model, specifying changes in parameters via an interactive mode with a computer; (b) formalization that can use nominal or ordinal level data characteristic of much of the most crucial social science data; (c) sufficiently disaggregated subsystem models that the bulk of societal behavior can be predicted from its key component parts to avoid reliance on broad social laws having few empirical behavioral referents; (d) a model whose formalization algorithm

is capable of handling nonlinearities and discontinuities characteristic of social behavior and other idiosyncratic factors characteristic of the particular social process being studied; (e) a model capable of handling feedforward and feedback processes characteristic of social behavior; and (f) a model robust enough to perform well with a certain amount of faulty or missing data. It should be noted with respect to point (c) that such disaggregation should be sufficiently great to permit a direct causal path to be traced from the smallest *relevant* individual components right up to the largest component deemed important in determining the desired social forecast, but not so detailed as to preclude any conceivable systematic analysis (see Brewer; and Brunner and Brewer on the need for systematic simplification of the modelling process). A general purpose model, therefore, is probably not a wise research objective.

It is worth the effort to do these things, even in part; because of its capacity to take complex societal interdependencies and dynamics into account, its explicitness, the reproducibility of its results, and its capacity for quantitative or symbolic representation, modelling offers one of the most promising forecasting tools in the methodological tool kit of the social forecaster.

Survey Forecasting

Survey research methods are one of the most advanced in the social science repertoire, but there are, nonetheless, a number of areas where research is needed to make it an effective forecasting tool. There is still an unresolved debate as to the exact linkage between an attitude as recorded in a survey questionnaire and the respondent's supposed relevant behavior. Indeed, there is no full agreement on the unidimensional or multidimensional definition of attitudes, or how various psychological components interact with role prescriptions and various social environmental stimuli to produce or modify an attitude. In matters of behavioral forecasting via attitudes this is crucial — which attitudes produce which behaviors?

The volatility of the attitudinal measure is reflected in another difficulty with survey forecasts — they have very short forecasting time horizons. Political pollsters, for example, do not seem to regard the polls to be valid for more than a week or so, or perhaps

even a period of twenty-four hours in some cases, and even this forecasting horizon can be drastically shortened by widely publicized but not necessarily important events. Consumer buying and economic confidence forecasts have longer time horizons (an economic quarter, up to a year) but these, too, are somewhat volatile. Where long term social forecasts are desired, these time horizons are clearly too short. There is as yet no solid empirical guideline for maximum attitudinal forecast time horizon.

However, some, such as Crespi, show that the identification of highly specific, low-order abstraction attitudes of persons having a high likelihood of doing something (buying soap, attending the movies, voting a strong preference) can yield high correlations between attitudes and behavior (identifying a specific brand name of soap, attending a specific movie, going to the polls). People's attitudes are predictive when playing out roles and not predictive when they are in more loosely structured situations. Crespi's research suggests how linkages can be more accurately inferred between attitudes and behavior, a form of analysis that could perhaps be profitably transferred to other attitude-to-behavior contexts. Other survey analyses also indicate that the predictability of behaviors depends on the state of the preparatory action process in which the survey is administered.

Some of the difficulties in using cross-sectional, single-time-frame data for survey forecasting are attributed to the insufficiently fine scaling of respondent responses that would permit interpersonal differences in attitudes and intentions to be more clearly discriminated (see Maynes).

It is not at all clear that surveys are helpful in interpreting the behavioral significance of highly unusual events. Nor is it clear just how sensitive attitudinal surveys are to more persistent or regular social forces in predicting behavior, a matter exacerbated by the finding that a good bit of behavior can be predicted from background variables without any knowledge of the individual's attitudes or behavioral history; change variables predict better than level variables, it turns out. In predicting the direction of social processes surveys have to somehow reflect distinctions between power holders and those not in these lines of control, a linkage requiring a knowledge of social structure and process as a precondition for defining a viable sampling stratification plan; there is a need for this kind of analysis. Some, such as Goecke,

argue that a trend in attitudes over a long period of time is predictive of behavior, but this has yet to be demonstrated publicly and empirically in specific terms, and other studies reject this view. And even if true, how long a trend is required for how long a forecast and for what topics?

Effects to measure the "deeper" motivating values that remain relatively unchanging over time have been only partially successful, and so also, therefore, have been efforts at forecasting values. Nor has the linkage between values and behavior been demonstrated in specific enough terms that behavioral forecasts can be precisely derived from value forecasts.

Survey instruments are highly sensitive to wording and administration. The only criteria for instrument validity used today are face validity — the survey looks like it is measuring what it is supposed to measure — and consistency — an intercorrelational analysis of pretested survey instrument questions appears to yield expected positive and negative correlations in the expected magnitudes. Systematic semantic analyses of the texts of survey instruments have not been done, a research task that is worth experimenting with, given the considerable ambiguity of the language. Nor have efforts been made to design alternative, but semantically equivalent, questionnaires to administer to the various levels of language sophistication and dialect used by the public at large.

Additional questions have been raised about the amount of variability in polling due to the personality and techniques of the field interviewers administering the polls; pollsters have long been aware of such effects but systematic measures of these effects are not regularly indicated. The extent to which systematic procedures are regularly used to select alternative respondents when intended respondents cannot be reached are not typically reported in a poll, although systematic procedures for dealing with this problem are widely known and used. But even if such practice is kept within reasonable bounds it is not clear how the procedure works when large subgroups, as racial minorities, refuse to be interviewed or be candid in the interview process.

Survey techniques have proven to be of limited use in situations where access is difficult, such as government policymaking circles, executive offices of businesses, judges in the courts, or other societal elites. These are not probability samples but they

could benefit from a systematization of the interviewing process. The same could be said of surveys of defined subgroups that are not probability sampled.

There is a need to evaluate the results of past surveys, perhaps with reinterviews of respondents to see what caused deviations in forecasted behaviors or attitudes. There appears to be an uncomfortably close margin between unsystematic polling technique results and carefully sampled polling results. There is, in short, no lack of activity that can and should be undertaken to improve survey methods as a tool for social forecasting. Applications of regression, Markov process, time series, or other forecasting methods to survey research data are subject to the limitations of such methods and are treated in the combined methods and individual forecasting methods sections.

Criterion Analysis

This is a widely used method deriving largely from operations research and is more mathematical than most forecasting/planning methods. The attempt is to optimize the distribution of resources, given certain limiting conditions (constraints). In matters of hardware, dollars, manpower, or other resource allocations optimizing criteria can be systematically defined — find the minimum staff of nurses needed to meet patient needs over the course of a day to reduce hospital costs; devise a school timetable matching teachers with classes, subject to preferred class assignments, preferred hours, preferred teaching sequences, and preferred lunch periods; find the optimum allocation of dollars to various research groups to raise the effectiveness of a corporate or military research and development program.

There are a number of limitations to the approach. Arrow has presented an analysis indicating that it is impossible to devise a utility function that will fully satisfy the preferences of a diversity of individuals, suggesting a limited use for the optimizing method in social processes involving large numbers of individuals; the Arrow analysis has been subject to dispute, however. Quantification of a large number of social phenomena and related concepts, such as power, quality of life, anomie, or social integration, has not yet been satisfactorily accomplished,

suggesting large areas where these strictly mathematical approaches are not ready for application although rank-order measuring techniques developed by Coombs and others suggest how some previously intractable social measuring problems can be solved.

Operations research optimizing methods most frequently employed include linear and nonlinear programming, integer programming, stochastic programming, dynamic programming, and probabilistic queueing models. In linear programming constraints must be positive and nonzero. Various activities are assumed to be measured in common units, to be independent of other activities, and to be additive rather than multiplicative, etc. Furthermore, as noted, the payoffs for each activity are assumed to have an explicit utility associated with them that would permit an optimal selection among alternative allocations of resources. Practical decisionmakers will recognize right away that activities are seldom independent, measures often not commensurate when dealing with societal phenomena, not always additive, not easily measured in terms of explicit utilities, particularly for superordinate choices or vaguely defined concepts — not to mention the Arrow paradox alluded to. Because of the inherent limitations of the linear model, other varieties of programming have been devised that relax some, but not all, of the linearity assumptions, the most popular being varieties of dynamic programming employing iterative, stepwise solutions to optimization problems.

Dynamic programming analysis assumes that once an initial allocation of resources is specified for activity that is optimal, subsequent stages of allocation for other activities can be made, one at a time, that are also optimal — a multistage decision process concept. In practice rarely are decisions so neatly and independently sequenced. Since ordinary calculus optimization techniques are not applicable due to the functional properties of much data, numerical enumeration techniques are employed in a comparative search strategy. However, the number of possible numerical enumerations is so great per resource allocation that the number of variables that can be considered is limited, thus limiting the complexity of the societal process to which it is applicable.

The solution of complex systems of equations involving multiple resources and multiple constraints requires some guesswork to

select optimal allocation values and then run calculations to see if the result is indeed approximately optimal — hardly an elegant methodological procedure although some special search techniques have been developed to reduce guessing to a minimum. It is not always clear using this procedure whether one has achieved a relative or an absolute optimization, however (apart from an assessment of the pattern of optimal estimates about the region of supposed absolute optimization — a method of optimization that does not work where there are wide deviations in functional forms confined over a relatively small interval). When stochastic (random) factors are involved in the optimization problem, random variables based on average performance are usually used to derive expectations; however, in many social processes relationships are often nonlinear, making estimates based on average performance a risky criterion.

The mathematics of approximating solutions in the most efficient manner possible for complex dynamic programming problems, such as problems involving nonlinear adaptive feedback control processes, is varied and complex and in need of critical review by those knowledgeable in both mathematics and social science research, so that the plausibility of assumptions and algorithms used in modelling social processes can be assessed from a social process point of view and the logical validity or efficiency (elegance) of the mathematical treatment can be assessed from a mathematical point of view. I have made some comments on the former but leave comments on the latter to professional applied mathematicians.

There is certainly a need to develop new methods of optimization estimation for the more complex, "badly behaved" social processes where present computational constraints are unrealistic; there is also a need to critique the external validity of programming techniques and other optimizing techniques that have been applied to social processes even though the presently developed mathematical procedures are well established and probably valid from a purely mathematical point of view. Operations researchers are notorious for their interest in model building without a concomitant interest in model validation and assessment (see Mayne).

It should be incumbent on the user of environmental prediction, say in the estimation of international military threats in terms of another nation's military hardware and assumed adversary status, to make explicit the underlying models that suggest how such weapon aggregations can constitute a threat so that the logic can be critiqued. This model could be verbal or quantitative, depending on the available data and adequacy of measures for model concepts. If it is said that the gross national product places an upper limit on the amount of welfare assistance that can be provided to the poor, that the ownership of the means of production determines whether workers will be exploited or not, or that pollution in excess of a certain level will be lethal for a certain percentage of the population, the forecaster should explain why or present evidence in support of this claim.

At one level it makes sense to suppose that the availability of energy, resources, skilled manpower, plant, equipment, communications, and transportation, as well as a large consumer demand for goods will set the outer limits for the productivity of that society as compared with the same parameters of another society or to suppose that a person's SES is largely predictable from the income and education of his parents; public policy can be developed within such a framework. It is quite another thing to prove that these boundaries are indeed the outer limits of performance, and it is this that needs to be researched in this mode of forecasting.

One of the fundamental limitations to environmental prediction is the empirical determination of relevant environmental constraints — a concept much easier to talk about than to operationalize, possibly because of the complex causal mechanism which is subsumed by such a concept. Nor is it conceptually an easy task to specify which of the total range of environmental components can be said to constitute a complete, relevant set of constraints on a particular component of social process sufficient to permit a valid social forecast. The current procedure is to use empirical studies, a formal theory of social process, or a subjective theory of social process to select such key variables; typically an intuitively appealing theory of relationship is the one used, rather than empirical research or formal theory. One can speak of *a* constraint, but as a rule, there is little assurance that this

selected constraint is strong enough to affect decisively the behavior being forecast. The language becomes even more imprecise when the term "boundary" is substituted for constraint, for the term conjures up notions of invariable, fixed limits.

Recommendations for needed research on methods for this mode of prediction focus on conceptual clarification, explicitness in causal mechanisms believed to underlie constraining factors, and supporting empirical studies establishing that constraints are in fact related to forecast variables. Actual methods employed in estimating future environments are not different from those in other modes of forecasting, such as extrapolative or modelling forecasting, and are subject to the same kinds of methodological research needs.

Network Analysis

Network analysis typically proceeds without explicit recognition of certain "decay" factors, such as human exhaustion or boredom although it is possible to compensate for such in a network analysis, such as PERT or CPM. PERT uses probabilistic estimates of time and cost factors at network branch points, such that an optimum path or critical bottlenecks in the network can be identified; however, such probabilities are usually subjectively estimated. The procedure organizes and focuses thought to consider the sequential stages, analytical hierarchy (relevance trees), or functional prerequisites and means of achieving objectives in a process; this is usually done intuitively or in terms of a knowledge of how a process works, rather than as a result of empirical research or theoretical guidance. Typically, the network is treated as a management aid, rather than as a systematic forecasting or planning procedure.

As with criterion analysis and other forecasting and planning procedures, the success of the method requires use of commensurate measures, such as dollars or time, to permit choices among alternative network paths or to identify bottlenecks — unless probability techniques are used. Frequently, incommensurables are treated via the dubious practice of arbitrarily assigning weights. The estimation of utilities in the entity being maximized (objective function), as noted for criterion analysis, is a methodological tripping point in social

forecasting since group utilities are not known. High quality, reliable data are also lacking when it comes to making such estimates for social processes.

Naturally, unanticipated confounding factors, such as strikes, subcontractor production delays, or technological breakthroughs, can affect the validity of network estimates although some judgmental effort is usually made to anticipate such factors. Misestimates are often amplified via cumulative or multiplier effects throughout the network, a difficulty with the method and a problem in decisionmaking processes. The use of a network to identify relevant components for a forecast is usually treated as the appropriate realm for intuitive creativity.

To the extent that network analysis relies on judgmental factors there is relatively little leeway for systematization, save for the use of expert conferencing techniques as used in the Delphi method of intuitive forecasting. The network approach also becomes extremely unwieldy when dealing with complex social processes. There is also opportunity and need to examine the logic of the proposed sequential network.

Some efforts have been made to systematize the estimation of cumulative branch point probabilities in a network by using Bayesian statistics. Bayesian estimates of probabilities by subjective means are rejected by some researchers in favor of other estimating methods, such as maximum likelihood, on the grounds that there is insufficient basis to make such estimates, particularly for complex societal processes. The selection of an estimation procedure is more than a matter of mere preference since the item being predicted is finite, not based on an infinite sample, and different statistical estimation procedures are based on different assumptions in this regard. Furthermore, it is assumed that events at network branch, or choice, points are independent (mutually exclusive) which in real social processes is very unlikely.

If experimental observations are not possible to assist in picking probabilities, some argue that an arbitrary assignment of probability is permissible and, indeed, is preferred to attempts to slant probabilities to correspond to some preconceived notion of social process; this is a reasonable strategy provided the range of outcomes is known (which, in the case of social processes, is unlikely) and provided choices are not affected by superordinate outcome preferences (also unlikely). It is possible to infer such

probabilities indirectly via variables shown to be related in some statistical sense or via new variables that create conditional independence between given variables.

Experts could attack different parts of a probability estimation problem where their expertise lies. If a panel of experts is used, however, it might be difficult to know how to reconcile diverse estimates of probabilities apart from conventions, such as Pareto optimality. In situations where repeated observations are possible to assess the plausibility of probability estimates (i.e., expected values) this is less of a problem, but in nonroutine situations the method is more problematical.

Where measures are biased or inaccurate, estimation is further eroded. Where processes are complex, it is difficult to know in any objective fashion how many factors to take into account in a probability assessment—what factors to isolate out for consideration—other than to rely on "experience." If errors in estimation of probabilities are made, it is difficult to pinpoint just where they occurred although the ramifications for overall estimates can be ascertained.

Methods for probability estimation in complex situations are underdeveloped at this time. Consider the Bayesian approach again. Objections to Bayesian analyses on grounds of subjectivity can be challenged by demonstrations that regardless of initial priors, proper use of the method will yield an estimate that converges to a common value. Objections on grounds that parameter values that are supposed to be fixed are allowed to have variable values are rejected on grounds that the formal mathematics of Bayesian statistics support such a position. Objections on grounds that data are prematurely assessed in the Bayesian approach are rejected on grounds that *good* researchers *do* have an idea of what the data will suggest. Furthermore, Bayesians object to classical approaches that fail to take advantage of information in the name of objectivity. Bayesians also reject the notion that their procedures cannot be carried out successfully in complex situations involving a great number of variables (see Raiffa on these points). This is a debate that needs to be resolved by mathematical statisticians and one that needs to be brought out explicitly in the social forecasting literature. Also needed are strategies for problem simplification by the elimination of irrelevant choice branches in the network (based

on prior research, theory, etc.); see Platt for a discussion of problem solving and research strategies of a similar kind.

Predictive Social Laws

This mode of social forecasting is frequently faced with problems of insufficient data to support broad generalizations or difficulty in operationalizing vague concepts. Some efforts at identifying predictive laws are based on theoretical notions, but some prediction can proceed empirically without much theoretical or research guidance, searching for general laws or patterns in social processes that can be used for predictive purposes. To some extent the results of such an analysis are accidental — patterns happen to be found in data that can serve as rule-of-thumb forecasting guides.

Various kinds of clustering techniques and other pattern recognition methods can be employed to assist in such analysis. Some, as factor analysis, might arouse some suspicion because of the regularity with which they identify about the same number of limited patterns. It is possible for clustering techniques through some computational artifact to produce nonsense groupings that creative researchers might successfully give plausible meanings to. Some (see Fleiss and Zubin) suggest that a test for clusters in a homogeneous distribution of data could be attained only with the aid of an outside evaluational model. There are plenty of questions that can and should be asked about clustering. Any pattern recognition method should be evaluated to determine if patterns are being identified because of an algorithmic artifact regardless of the substance of the data. Measures of similarity (correlational association, distance measures, deviations from a probabilistic norm, interval or rank orderings of preferences, nominal categories specifying acceptance or rejection of a datum) employed in clustering are highly problematic, particularly with variables that are difficult to quantify. These measures also need to be evaluated for application in specific contexts.

Similarly, a method should be investigated in terms of its robustness in the face of missing or faulty data and for its ability to process data measured in any one of a number of

levels from nominal to interval. Would pattern analysis results be better if some data are left out? Why? Patterns of data matrices are known to affect factor analysis results; we need to know how the clustering method chosen is affected by similar matrix properties. How does the overall configuration of a data set affect the best choice of clustering technique to analyze it? How are clusters to be combined? How does one test for a multimodal distribution? (On many of the preceding questions, see Ball and Hall.)

Some pattern estimating techniques, such as the Fourier analysis of time series, require a considerable amount of mathematical ingenuity to implement, suggesting that a mix of artistic skill as well as formal mathematical procedure are required to make such techniques work. Other techniques, such as multidimensional scaling and canonical correlation, require perceptual ingenuity on the part of the analyst to pick out identifiable patterns in results. Even a simple correlation can pose difficult interpretive questions. Still other techniques attempt to develop analog or digital models to simulate a supposed predictive social law, such as the Richardson mathematical models of arms races where a crossover point of no return is predicted for arms races and eventual international war; here the critique and recommendations for research offered for modelling forecasting are applicable, suggesting a possible convergence between modelling forecasting and some forms of predictive social law methodology.

A variant of comparative studies methodology used to find generalizations across societies and cultures or within societies via multiple case studies is used as an alternative to the hyper-empirical approach, with most debate, as might be expected, revolving around the degree of comparability in the entities being compared. Problems of identifying and developing measures of formally equivalent but apparently different concepts and structures occupy much of the time of researchers of this kind. The comparative method can be executed on a purely empirical basis, but at its best it tries to use both theory and research in analysis.

Once the preliminary conceptual and operationalizing work is done, measures of similarities and differences are required — conventional techniques which are subject to conventional methodological problems, such as determining the applicability of

tests of significance or the meaning of correlational measures. The results of comparative studies can also be expressed in terms of sometimes competing process theories, inviting operationalization, testing, and choice. To date such theories have proven to be very difficult to operationalize and the results of pure empiricism have yielded few generalizations of predictive use; this is not to say that the comparative method has not yielded a good deal of information illuminating how various social processes work, by providing a general framework for such analysis, or could not be made to yield more testable general laws.

In any effort to apply general laws, certain preconditions or assumptions qualifying the laws are specified to enable the laws to hold. With pure empirical studies such a statement is usually not offered, perhaps because the preconditions are not known — suggesting the need to identify the hidden preconditions of the empirical law. Preconditions are, of course, subject to change and, thus, need to be periodically reassessed and the impact of such changes on the validity of the "laws" also reassessed. Methods employed to make such assessments fall in the realm of conventional analysis with the methodological difficulties of such conventional analysis.

Methods to assess future states in preconditions are also a legitimate area of inquiry, assuming that the general law is supposed to have future applicability as well; however, most of the research of this kind has been judgmental, rather than systematic — an additional needed area of research. Analytical methods used, as with environmental forecasting, fall within the conventional forecasting method repertoire with the methodological problems these methods entail.

Clinical Prediction

Clinical prediction is one of those forecasting methods which, strictly speaking, is — as with environmental prediction and predictive social laws — not a unique method at all, but a particular focus of study dealing with components or properties of social process that are useful in developing viable social forecasts; often a mix of methods mentioned in other categories of this paper does the actual work of forecasting under this rubric. But, as with environmental prediction and predictive

social laws, it is the strong focus of investigation in clinical prediction that suggests the need for special attention to this area of inquiry; this, combined with a body of scientific knowledge developed in psychology and psychiatry, makes it fruitful to treat clinical prediction as a special category of social forecasting methodology.

There is a long running debate among practitioners of clinical psychology and psychiatry as to the relative effectiveness of statistical as opposed to clinical means of predicting individual behavior. Both approaches are treated here as clinical prediction. Neither approach, it turns out, is particularly distinguished in terms of performance; perhaps, given the state of the art of clinical prediction it would be best to speak of reducing uncertainty in behavioral prediction, rather than seeking to achieve a particular achievement level in forecast accuracy. Still, it is the unique nature of this mode of prediction — the intelligent and emotional mechanism motivating social process — that warrants an effort to improve the available methodology.

There are many methodological difficulties facing the clinical prediction process; some of these difficulties are as follows. Clinical practice is hindered by the variability in competence of assessors of personality; the standardization of procedure afforded by statistically based personality tests is a distinct advantage in this respect. Still clinical prediction has a number of areas, regardless of whether a statistical or clinical approach is used, where, at present, the assessment procedure is partly a matter of judgment: the definition of the behavior to be predicted, the selection of a measure to undertake such prediction, the identification of selected intervening variables within the psychology of the individual and the environment in which he is embedded, the selection of the best measuring instruments to make the predictive measures, the pretesting of the method on a sample of individuals representative of the particular application being sought, and the checking for errors in objective or standardized data coding (see Holt on these and other comments). The clinician combines whatever systematic statistical measures he may have used with personal judgment in making personality forecasts and some attempts at behavioral forecasts as well; the statistical testing alternative makes the personality or behavioral forecasts directly.

Statistical forecasts have been plagued by a number of failures

in method, including a failure to recognize the role of judgment in problem definition, in measurement definition, and in measurement selection, errors in data collection, failures to test methods on control samples, invalid measures for the behavior to be predicted, excessively vague definitions of behavior that preclude precise measurement of behavioral prediction, attempts to predict behavior that are confounded by unstudied environmental variables (Holt). These same criticisms apply to clinical judgment.

Hard-core clinicians, on the other hand, are convinced of the "specificity" of behavior and prefer qualitative information (such items as the individual's history, personal observation of the individual's behavior, clinical interview information from the individual, graphic data, such as drawings the individual is asked to make and perhaps discuss, dream interpretation, and standard projective tests, such as the TAT or Rorschach where there is wide latitude for interpretation by the clinician, but still some element of standardization — all conducted in an atmosphere of reassuring empathy that a purely statistical analysis cannot provide) informed by psychological theory, as from Freud, Jung, or Horney, to develop forecasts. Such forecasts are not amenable to formal methodological improvement, save in the improvement of their underlying interpretive theories — a matter of conventional theoretical assessment — and explicitness in procedure. This is not to say that clinicians do not attempt to conduct their inquiries systematically, learn from their experiences, and keep track of the accuracy of their assessments and the reasons for failures to predict. These hard-core clinicians also use statistical tests to predict socially relevant behavior (an area where methodology can be improved) and "absorb" these estimates judgmentally in the clinical estimation process.

Clinicians claim that computerized procedures are not sophisticated enough to process the verbal nuance required in high quality clinical analysis, not to mention the nonverbal modes of communication that are important in analysis. Statisticians object to the use of projective tests that do not have high validity and reliability ratings. Clinicians do not take the characteristics of the population the tests are normed against into account when applying them, the statisticians argue. Others find clinicians do not take the individual's environment sufficiently into account when predicting behavior, relying almost entirely on psychological variables; one finds sociologists conducting inquiry with little

concern with the impacts of individuals on social processes and psychologists and psychiatrists doing the opposite, suggesting one of the difficulties in using a purely disciplinary approach to social forecasting.

It can be argued that there are no scientific criteria for determining whether a clinician is sufficiently competent to make valid judgmental analyses. Clinicians, it is claimed by some, attempt to understand, while psychometricians attempt to predict without understanding — a division of views reflecting the state of the art of quantitative personality and behavioral analysis; neither group, as noted at the outset, predicts very well at present. One of the difficulties with clinical analysis is that personality theories are so vaguely stated that it is difficult to apply them with any precision in an individual case. This inhibits the communication of results and the "intercoder reliability" of various clinical assessments.

I am not in a position to tell professional clinical psychologists and psychiatrists how to improve their clinical craft, but for purposes of social forecasting, there is a need to draw out those aspects of the method that can be applied and to devise ways of making at least the pragmatic aspects of that method useful for social forecasting.

One fact that has to be immediately faced is that by-and-large the fund of information on a given individual outside the clinical environment is extremely limited; also, clinicians are not likely to take a professional interest in non-medical forecasting. This means that any attempt at clinical forecasting using the methods of clinical analysis is generally going to be implemented by amateur would-be clinicians (although this need not be the case) and is going to be circumscribed by data limitations and limited access — individuals are *not* likely to permit detailed inquiries into their personalities, private lives, private thoughts, etc., outside a medical context. This means that applications of the clinical method are going to have to rely to a great extent on secondary biographical information, analysis of speeches, available private correspondence, public media news reports, memos, and other limited and selective sources indicating verbalization; interview data (when available); information from acquaintances, relatives, and informants; analysis of the individual's social environment; and, possibly (but not likely) the results of standardized psychological tests that the individual

agrees to take. More detailed clinical analyses, such as dream interpretation or projective tests, are most unlikely to be implementable.

Analysis at a distance, then, is what is to be done — a task professional clinicians would regard with considerable scepticism. Yet we know that there have been notable instances when such analyses have been attempted in the public sector (see Barber, Langer, and George and George) and have been impressive with their plausibility and, in some cases, their success in prediction.

First, an attempt should be made to develop very short and inoffensive tests which can be administered very quickly to a cooperating respondent to predict at least a small but significant aspect of behavior. Clinicians are sceptical about the depth of statistical tests, but if these tests can be normed on the appropriate subgroup to which an individual who is the object of clinical prediction could be said to belong, it is an empirical fact that they can predict at least some significant aspects of individual behavior. Identifying and obtaining the normed results is, of course, problematical.

Second, a procedure should be outlined for correlating the individual's known social background and current social environment with other studies of behavioral or cultural constraints on the individual's behavior.

Third, an attempt should be made to collect, code, and analyze systematically the themes and imagery of the individual's public statements, speeches, autobiography, correspondence, recorded interview comments, etc., and then to correlate these themes with various types of themes and images employed by other individuals whose behavior is known in order to develop a forecasting handle. Here the use of canonical correlation seems particularly appropriate.

Fourth, if an interview can be arranged, questions can be asked indicating the individual's perceptions of his goals (relevant to the particular kinds of behavior of interest), his role (with respect to these behaviors), his instrumentalities (how he expects to achieve his goals with respect to these behaviors), his constraints (apart from role prescriptions, his prior commitments, his legal latitude for action, his available resources, the complexity of the environment he has to deal with, etc.), his estimation of success likelihood (via his instrumentalities and constraints), or other questions that experienced clinicians could suggest that

would reveal a propensity to act with reference to a particular kind of behavior. A Lasswellian analysis of the policy process the individual is embedded in (if public policy is the topic of the study) could provide some information of this kind as well as additional information on the environmental constraints shaping his behavior; an alternative model of social process could be used for a nonpolicy framework. It would be expected that the heavy cost of such an analysis would be restricted to limited key actors in social processes (the elites approach).

Fifth, interview information from persons knowledgeable about or having had or now having some contact with the individual could be perhaps more easily gathered and analyzed to see if there are predictable patterns of behavior or possible restraints on behavior. Perhaps some correlation could be made between the respondent's verbal statements and evidence of his behavior as indicated by historical information or information from his contemporaries.

Finally, secondary source materials, such as biography or news reports, can be analyzed for additional clues as to behavior patterns and constraints.

All this disparate information has to be absorbed in some systematic fashion either in a clinical theoretical framework (that should be made explicit so it can be evaluated by others) or an empirical data analysis process (such as multiple correlation analysis, multiple regression analysis, or modelling). The difficulty with empirical analysis is in accommodating such disparate kinds of information (social process constraints, attitudes, behavioral propensities under various conditions, apparent objectives, available resources, etc.) when trying to predict a course of action. How to weigh these various kinds of factors is a first-class unsolved research headache; a "field of forces" approach has been suggested as a starting point.

Perhaps the key in making this case study analysis manageable is to focus analysis on the prediction of particular categories of behavior, such as power seeking, timing of action, action style, confrontation versus accommodative behavior, or promotion of a particular kind of policy. Obviously, whichever procedure is used (or if both a theoretical and empirical approach are used), data have to be collected in an objective, reliable, and orderly fashion, as might be expected in any purportedly scientific inquiry. No case study is likely to have all the relevant facts, however.

Given the almost impossible task of predicting the behavior of exceptional individuals — the typical key elite — the best that can be hoped for, as noted at the outset, is a finite reduction in uncertainty, rather than a comprehensive prediction. Perhaps such inquiry can be enhanced by a comparative study of elite forecasts to see if there are certain methods or concepts that tend to be better than others in producing such forecasts.

There is another branch of application of clinical forecasting to societal subgroups (see Liebert) and entire societies (see Fromm), but if the forecasting of individual behavior is somewhat questionable because of the access problem, the study of larger aggregates is vastly more risky. National character studies, coming from the cultural anthropology tradition (see Benedict and Mead, for example), rather than a psychological or psychiatric one, are faced with similar problems of diffuseness. Both aggregate analysis traditions are theoretical and subjective, rather than quantitative and statistical, and their plausibility (or the lack of it) derives from the logic of argument rather than explicit data analysis. Empirical data used in this approach are to a considerable extent anecdotal or descriptive. The basic areas for improvement in method here are conceptual and theoretical with assumptions and causal models requiring explicit elaboration for critical evaluations and a watchful eye for the reification of abstractions via the attribution of individual psychological mechanisms and motivations to social aggregates. There are efforts to systematize data collection and presentation but much room for improvement remains in this area.

Whatever the method employed in clinical prediction, there is much that remains to be done toward methodological improvement; to date the use of this method for social forecasting purposes is limited to a relatively few cases, so there is little experience to draw on, outside the medical context. The unique contribution that clinical prediction can make to social forecasting methodology makes such methodological development entirely worthwhile.

Theoretical Forecasting

Here methods are not applicable in the usual sense except as back-up research to provide evidence for or to test the proposed

theoretical forecast. In social thought the tendency has been for theory to run far ahead of evidence, leading to calls by Merton and others for the development of social theories of the middle range — to bridge the gap between evidence and insight. Greater effort needs to be made to state theories in operational rather than vague terms that would permit theories to be tested (some say falsified) against data, to collect needed data, and to undertake actually such theoretical assessment. It may be that the serious forecaster using this mode of analysis will have to identify a set of subproblems that will have to be researched before an adequate test of a theory can be formulated; similarly, theorists might do well to make a greater effort to identify subproblems that will have to be researched before a theory is offered.

Platt's advocacy of strong inference seems to be a good way to deal efficiently with this kind of theory building and testing activity. One does not need a bit of data to assess the logical validity of a theoretical forecast, just as Fink assessed the validity of a particular theory of nuclear deterrence; more work of this kind needs to be done. For a strongly presented rationale for the development of theoretical forecasting methodology on grounds of explanatory richness and falsifiability, see Duncan.

Intuitive Planning

This mode of prediction might at first glance appear to be a variety of intuitive forecasting; it is treated as a separate category here because of the normative component used in planning that is *not* present in conventional intuitive forecasting and because the forecasting aspect derives in a secondary way from a decision process, not a direct effort at forecasting. It is nevertheless the mode in which a good deal of future social behavior is defined. Intuitive planning could benefit procedurally from an orderly, explicit presentation or reasoning, so that others could make a reasoned evaluation of the analysis. As with conferencing techniques discussed under the category of intuitive forecasting, intuitive planning might benefit from a consensus judgment of experts; however, as with intuitive forecasting, the prospects for methodological development of this approach to "forecasting" (plans committed to action being regarded as equivalent to forecasts) are quite barren.

Theoretical Planning

The separate treatment of theoretical forecasting vis-à-vis theoretical planning is undertaken for the same reasons as for the separation between intuitive forecasting and intuitive planning. Theoretical planning has the same limited methodological opportunities as theoretical forecasting — the need for conceptual clarity, the need to use more empirical research for back-up (grounded theory), the need for explicitness in logic, and the need to develop viable operationalizations of key dependent and independent variables that would permit tests of the planning theory. In addition there is a need to investigate in some systematic manner the reasonableness of the objectives of plans; objectives can be set so unrealistically that there is no feasible combination of resources, manpower, or properties of the social system that could permit them to be achieved; the same could be said to apply to intuitive planning, network analysis (used in the planning mode), and criterion analysis (see Crecine).

Multimethod Forecasting

It seems obvious that one should use a mix of forecasting methods either to see if results are corroborated or to take advantage of the particular strengths of a forecasting method to deal with a particular aspect of social process. But there is very little systematic research on either of these points. What exactly does one prove by showing that an analysis of percentages and arithmetic means yields the same results as factor analysis, correlations, t-tests, and Chi-square tests? Is it sufficient to assume without proof that because each method measures the same social process from a slightly different point of view or in a slightly different manner that if the results converge the variability in social data will be accounted for and the forecast will be reliable? Furthermore, how much convergence is enough? Implicit models and assumptions, rather than converging toward a mean, might be quite incompatible, yielding nonsense results. These are research questions and issues that need to be addressed in the literature.

On the other hand, if one supposes that different forecasting methods can be used best to analyze *different aspects* of social process, what systematic guidelines are available for such a selection? None at present. Intuitively, it makes sense to take

advantage of such a division of labor, but there are no guidelines for method selection in terms of the properties of social dynamics (continuous versus discontinuous action, accelerating versus constant rate action, cyclical versus trend action, transformations in action patterns, etc.), in terms of the levels of measurement in available data (nominal, ordinal, interval, ratio), in terms of normative statuses (whether one is taking a deterministic or interventionistic position with respect to social process), in terms of the level of aggregation of the social process being studied (individual, small group, organization, community, entire society, interactions among societies), in terms of structural social attributes (family, judicial system, governmental system, business sector, cultural and belief systems, socioeconomic stratification, etc.), in terms of functional attributes (goal attainment, integration, pattern maintenance, adaptation, etc.), or in terms of forecast time horizon, data needs, or analysis costs.

Nor is there a systematically formulated strategy for combining methods in an optimal fashion vis-à-vis these various considerations. One cannot assume, without further study, that only the advantages and not the disadvantages of a method will be compounded by the combining of forecasting methods. We do know that, on whatever basis, combined methods are now being used in some of the types of forecasting methods noted (see, for example, clinical prediction, environmental prediction, and predictive social laws); what we do not know is whether these mixes are the best that could be found in their respective domains.

The intuitive appeal of combining forecasting methods on a "division of labor" basis is strong — use clinical prediction for studies of key individuals occupying key decisionmaking roles in social process, use modelling forecasting to simulate the effects of alternative policies or conditions on social processes (say, as a planning aid), use environmental prediction to estimate the boundary conditions for social processes and coping actions, use extrapolative and survey forecasting to estimate current and possible future states in model parameters, use theoretical forecasting to assist in the design of the modelling forecasting model and to aid in planning model experiments, etc. — but not sufficient to substitute for rigorous scientific inquiry on method aggregation.

All the preceding discussion of needed research on social

forecasting methods assumes that the objective is as accurate a scientific forecast as possible; however, in practice, it may be that user needs for forecast accuracy or time horizons are less (or more) demanding than current capabilities, suggesting the possibility of additional kinds of tradeoffs in combining methods (or the elimination of apparently plausible but insufficiently accurate or far-reaching methods). Ikle suggests, in addition to accuracy and time horizon, that forecasts be evaluated in terms of user non-scientific needs for forecasts (to entertain, to develop an aura of authoritative competence, to engage in a heuristic exercise in forward thinking in order to sharpen managerial thinking, to give planning departments something to do, etc.), but these are problems in the management of forecasting and in policymaking and will be considered under the category of user needs for forecasts.

I think it would have to be said for all types of forecasting methods discussed here that there is a need to make the logic explicit and the procedures both explicit and communicable to the greatest extent possible if social forecasting methodology is to aspire to scientific status. In some cases, as for intuitive forecasting and intuitive planning, logical explication will be the strongest likely achievement, but for the other methods explicitness and communicability will be but one key area of methodological improvement. In addition to explicitness and communicability in social forecasting methods, there is a need to state as explicitly as possible the limitations of the several forecasting methodologies and the implications of these limitations for the forecast information produced; this is an all-too-seldom executed task but an important one for credible and scientifically sound social forecasting. There is some debate on the extent to which value preferences should be articulated by the social scientist in order to provide insight into possible bias in the analysis; my own view on the matter is that such activity might lead to a politicization of the research process, is an invasion of the privacy of personal thought, and is not necessary in a properly explicit scientific analysis that can be judged on its own merits.

Methodological advance would also be facilitated by the rejection of the scientifically deplorable practice so prevalent in industry and government of conducting social forecasting research on a classified or proprietary basis where it cannot be put up for public scientific scrutiny. Finally, as noted earlier, researchers

might consider the suggestions of Platt that the research design procedures of molecular biology and nuclear physics be adopted through the use of logical inquiry trees (strong inference) to increase the efficiency and speed of the development of research — in this case, methodological development. Those who insist that social forecasting is and always will be an art and not a science (see, for example, Sisk and Nisbet) will probably always be with us;* this should not inhibit serious step-by-step efforts at methodological development, however.

*However, see de Jouvenel for the interesting argument, in part, that forecasting methodology can never aspire to scientific status since it deals with data which cannot be verified as facts (because they lie in the future). Resolution of the argument appears to lie with a closer examination of the concept of scientific prediction: Must verification always occur in the near present to be scientific? De Jouvenel's argument is more subtle than this and deserves to be read in full.

This is an area in which there is a need for a great deal of research for most types of forecasting methods. If we look at the assumptions of the more frequently used forecasting methods, for example, the significance of using untested or unstated assumptions is apparent. In extrapolative forecasting there is little empirical guidance for the underlying assumption of stability or periodicity in social processes. When pushed to the wall the social forecaster will not be able to define the time frame for which his assumption of continuity in process obtains, because he has no precise empirical sense of the rhythm of the social processes he is studying. The intuitive forecaster is not methodologically inclined, but he is bound to state the assumptions underlying his estimates. In practice such an exposition is rarely given, particularly in social forecasting. As noted, there is doubt that enough is known about complex social processes for individuals to claim to be qualified experts for participation in conferencing techniques for the estimation of social futures (see Dror).

Analogy forecasting walks a tightrope between excessive specificity on the one hand and unacceptable vagueness on the other. An analogy, if pushed too far, will be shown in its assumptions or its actual process to be different than that aspect

of societal process it is supposed to represent; on the other hand, if it is too vaguely stated, it will not be even heuristically convincing. It may be in the nature of the method that there are no solid guidelines for specificity in the stating of assumptions used in analogies and, yet, it is precisely at the level of assumptions that the validity of an analogy is likely to be challenged.

Modelling forecasting, particularly the inductive kind, is especially subject to the criticism that it is insufficiently explicit in its statement of underlying assumptions — a matter of particular significance since a model always abstracts and simplifies a more complex reality. Functional forms and postulated linkages among model components require explicit empirical or theoretical bases; so does the decision to include some variables and exclude others. The whole matter of specificity in the stating of assumptions is crucial for the adequate testing of the validity of a model. As with extrapolative forecasters, modellers are faced with a problem of limited knowledge about societal processes to guide model construction; they also are ill-equipped to handle the variability and irregularity of societal processes. Models have been subject to considerable criticism because of the conservative character of their underlying and unstated assumptions, particularly those utilizing closed system and equilibrium concepts. As with analogy, models are often judged inapplicable to public policy because of the untenability of their assumptions — a situation that does little to motivate professional modellers to make their assumptions explicit. In only a few instances are the assumptions explicitly stated. Brewer indicates a number of dimensions (ethical, pragmatic, technical, etc.) on which model assumptions and other model properties can be evaluated.

Survey forecasters face difficult problems in sampling, communication, attitude measurement, data irregularities, and attitude stability over time and in establishing scientifically valid linkages between attitudes and behaviors. As for extrapolative forecasters it is difficult at present for survey researchers to state exactly the time frame for which their forecasts are to be applicable; the claim of Goecke and others that long trends in attitudes will predict what people will do is subject to dispute. Survey research has developed some sophisticated techniques of analysis, but the principal assumptions linking the survey

instrument to respondent attitudes and respondent attitudes to respondent behavior remain a matter of considerable concern. There is also an unresolved problem on how to link the results of a valid survey research investigation to specific target subgroups for public policy purposes, in more general terms known as the "ecological correlation problem."

Difficulties in assumptions underlying criterion analyses are reflected in the development of varied methodologies designed to relax assumptions found to be objectionable for one reason or another. For example, integer programming arose out of a need to deal with independent variables that cannot be maximized (or minimized) in fractional parts, in terms of some constraint—variables, such as medical personnel in a hospital or trucks in a delivery network. Dynamic programming arose out of a need to deal with processes that are discontinuous, processes that have several relative maxima, unstable processes, constraints with nonzero derivatives at interval extremes, and to deal with nondifferentiable functions. On the other hand, the methods might retain assumptions that can prove stumbling blocks to acceptance in applications. For example, dynamic programming assumes a multistage decision process where resources are allocated one at a time; by conducting the analysis over very short time intervals it is possible to compensate for multiple simultaneous allocations for many purposes, but for some analysts this represents a fundamentally incorrect way to deal with social processes characterized by feedforward effects, feedback effects, simultaneous actions, and anything but analytically neat multistage actions. For others, there is considerable scepticism that some of the most important aspects of social process, such as social cohesion, can be measured in any precise way for purposes of quantification, that resources can always be independently aggregated, or that aggregate social utility functions can be defined for purposes of determining an optimal allocation of resources over a range of activities.

The sometimes used and infrequently used forecasting procedures need also to have their assumptions articulated, with the possible exception of theoretical forecasting and theoretical planning, which do make a point of stating assumptions (but suffer from other deficiencies, such as dependence on social theory which, in turn, is in an uneven state of development).

It is not clear at this point whether the gains afforded by the

combining of forecasting methods is sufficient to offset a significant portion of the shortcomings in the assumptions of the individual forecasting methods or whether the problem is merely compounded by interactions in uncertainties.

Another dimension to the assumptions of forecasting methods problem is the evaluation of forecasting methods assumptions in terms of the kinds of problems they would be applied to. It is possible to examine methodological assumptions at a meta-level, much as one might study the mathematical properties of functional forms, but problems raised by actual applications introduce another dimension to evaluation. If, for example, the objective is to forecast socioeconomic mobility for minority youths and young women, then certain factors become salient. Here it is institutional structures that must give way to accommodate additional upwardly mobile persons. Extrapolative methods with their assumption of continuity are not likely to be of great use in identifying a relatively new phenomenon of this kind. Survey methods, on the other hand, will reveal something about the attitudes of policymaker incumbents in these social institutions and, thus, may reveal fairly early potential changes in institutional access policy vis-à-vis minority groups and women — a partial solution. Analogy forecasts might also be suggestive by referring to studies of the mobility patterns of predecessor minorities, providing the assumption of similarity carries over to psychological dimensions, such as racism or role stereotyping. Environmental prediction would be of little use because environmental parameters are in the process of change in this example, violating the assumption of stability in that dimension. Theoretical forecasts would have great appeal for problems of this kind where there is much leeway for assumptions and speculation, albeit constrained by available research findings.

If another problem were to be picked, perhaps a different set of forecasting methods would be appropriate in terms of the viability of assumptions. Research could be done to identify those methods having the most relevance in terms of assumptional viability to a selection of key social forecasting problems — a matter that may be of concern when research resources are limited and potential social forecasting problems are numerous and difficult. Selectivity in terms of assumptions could be extended to other criteria, such as data availability, cost, algorithmic viability, user needs for forecast accuracy, etc. — topics that will be discussed.

The literature on forecasting accuracy is quite scattered and much of it unpublished, but, with enough searching, examples can be found for most of the types of forecasting methods described. It may be indicative of the highly underdeveloped state of the art of social forecasting methods (or other kinds of forecasting methods as well) that there is little empirical concern with forecasting accuracy.

There are few rewards for those who do social forecasting evaluation research and for those who provide the funding for undertaking such evaluative research, and there may, in fact, be penalties for the conscientious execution of such analysis. For those doing most of the social forecasting research—the think tanks, the corporate planning offices, government agencies, and outside consultants—there are strong economic, time, and professional prestige motivations to produce social forecasting studies that look good; forecast evaluations might jeopardize that image and possibly make it necessary to absorb the cost of redoing some forecasting analyses. For those commissioning the social forecasting research, particularly elected public officials and bureaucrats in public and private agencies, there is a curious lack of interest in and financial support for evaluative

studies of forecasts. It could be that overzealous researchers who find forecasts to be inaccurate might find that their think tank is not awarded an expected lucrative research contract or find that the forecasting component of their corporate organization is eliminated while agencies might face embarrassment and possible public displeasure at faulty performance. It would be interesting to do a study to see if this lack of interest in forecasting evaluation research is due to fears on the part of such entities that inaccurate forecasts would reflect badly on their performance or if it reflects a more fundamental rejection of the feasibility of social forecasting efforts.

Even in the principal journals of forecasting methodology and research (*Futures, Technological Forecasting and Social Change, Analyse & Prevision,* and *The Futurist*), there is just a handful of studies — about 5 percent — concerned with evaluating forecasting accuracy. It might be a fascinating area of study in the sociology and history of science to determine why among social scientists there is such an emphasis on forecast development and not on evaluation. Some areas, notably population forecasting, economic forecasting, and land use and transportation forecasting, have developed an extensive literature on forecasting evaluation, but such development is the exception rather than the rule; in the area of social forecasting the systematic evaluation literature is very small.

In another paper (Harrison, 1973) I summarized the results of over fifty studies of forecasting accuracy—if one looks hard enough one can eventually find examples — in eleven of the fourteen types of forecasting (including combined forecasting methods) and found that the amount of error ranged between just a few percentage points (for example, with econometric models, survey forecasting, and biological growth curves) to over 40 percent (clinical prediction), depending on the method and time horizon used. This range of error, other things being equal, could be considered to be due to either methodological underdevelopment in a given type of forecasting method or to the intractability of the particular aspect of social process being studied; probably both are true. "Hardware-oriented" technological forecasts seem to do better than social forecasts. The difficulty there is in forecasting particular aspects of social process suggests, incidentally, an important accuracy limiting factor in the combined methods approach to forecasting.

In almost every instance where forecast accuracy was assessed in terms of estimated user needs in particular contexts of use *none* of the methods could be said to perform at an adequate level; this may help to account for the modest impact forecasts have had on decisionmaking. This investigation would have to be repeated in a more systematic fashion, both in terms of having authors state forecast results in more precise terms and in terms of a more extensive and representative sampling of the forecasting literature (including some of the currently classified and proprietary forecasting literature and perhaps a prescreening to assure a minimum level of methodological quality vis-à-vis the type of forecasting method being evaluated), in order to draw firm conclusions on the forecasting performance of the various types of social forecasting methods. Similarly, a closer study would have to be made of user needs for forecast accuracy and of user utilization of forecast information before one could seriously assess the performance of social forecasting methods in terms of user needs for forecast accuracy.

Whether or not users' needs for forecast accuracy are met or whether or not these users actually utilize forecast information in their decisionmaking processes, it is a matter of purely scientific interest to know how well a social forecasting method worked and why. This is particularly true for those instances where forecasts are produced neither to be used in some decision-making process* nor to be attended to because of their normative implications, but simply to understand the developing properties of some aspect of social process.

The assessment of forecasting accuracy is just one aspect of the evaluation process; as Duncan notes, even the failure to

* The reasons for failure to utilize forecast information are varied and include any or all of the following: the results are not believed for various reasons—face validity, presumed forecasting staff incompetence, assumptions, forecasting methods used, measures, data quality, missing data; the user does not want to use a forecast—the forecast is just for show; the user is ideologically opposed to forecasting—a much more widely shared view than one might suppose; the user is constrained by prior commitments, new events, statutes, or bureaucratic politics not to use forecast information; the user is reluctant to plan far enough ahead to utilize forecast information; the forecast has become dated by the time of publication; the forecast is not specific enough; the forecast time horizon is not long enough; the forecast is not specific enough to be used in policymaking; the forecaster was not a strong enough advocate for his findings; the forecast is believed to be unneeded because of in-house research and development capabilities that can meet environmental challenges as they arise (see, for example, Dory and Lord, Rothstein, Vancil, and Ikle for a fuller discussion of the non-use of forecasting information by policymakers).

predict need not necessarily vitiate the usefulness of a forecasting method if the reason for the failure to predict can be ascertained. The assessment of forecast accuracy is perhaps just one of the first steps that comes to mind when assessing forecast performance; other questions, such as the validity of forecasting method assumptions, algorithmic bias, data quality, costs, and, perhaps, user needs for forecast accuracy, will be of interest as well. As indicated, there is a deplorable and perhaps scandalously overdue need to present some empirical assessment of forecast performance.

In policy process areas, however, it might be wise to remember that the work of successful decisionmaking might be to negate an undesirable forecast, such as mass starvation due to overpopulation, and that in these instances a failure to predict would not necessarily reflect badly on a given social forecasting method (although I suppose it could be argued that a good social forecast would also anticipate corrective policymaking).

It may be well also to regard the forecasting effort as an attempt to reduce uncertainty about social processes, particularly with some types of forecasting methods, such as clinical prediction, rather than to try to achieve a perfect social forecast, given the current state of knowledge about social processes and the current level of development of social forecasting methods. This is *not* to say sloppy social forecasts can therefore be tolerated or that scientific standards should not be sought.

The assessment of forecast accuracy leads into the related task of attempting to determine reasons for failures to predict (or, less frequently, reasons for success in forecasting); this could be due to methodological flaws, improper analysis procedures, faulty data, unusual social process developments, insufficient specificity, invalid underlying theory, flawed research, invalid assumptions, or excessive forecasting time horizons. Perhaps other reasons as well.

It might then be useful to compare the performance of various forecasting methods for particular types of social process so that a basis for selection among methods can be formulated — perhaps also a first step in developing a viable strategy for multimethod forecasting. It would be interesting, too, to find out why some methods work better for some aspects of social process than other methods. Thus for slow-moving, stable processes extrapolative or analogy forecasting might perform quite well while fast-moving,

unstable social processes might best be analyzed by survey forecasting or clinical forecasting methods.

Similarly, one might attempt to determine which methods would work best with very limited available data (analogy forecasting might fall in this category) and which would require extensive time series of data (extrapolative time series analysis would fall in this category) to produce acceptable forecasts. Studies of past efforts at forecasting, both for the assessment of individual forecasting methods or comparative assessments of forecast accuracy could be useful grist for this research mill although the results would probably be limited since past efforts at social forecasting have not been particularly sophisticated.

A third aspect of empirical performance assessment might be to assess the time horizon for reliable forecasting for particular methods or combinations of methods for particular aspects of social processes, as noted. This could be done via analysis of past or current forecasts or could be the object of a specific research design to assess experimentally just how far into the future a particular method could be used for a particular aspect of social process, perhaps, again, comparing methods for forecast time horizon capability. This would lead also into a consideration of the reasons for failures to predict (or successes in prediction) as the time horizon is extended. This time horizon information could also be used in the development of a forecasting method selecting and combining strategy.

If social forecasting is going to achieve the status of a science, it will have to develop scientific and not purely pragmatic or utilitarian criteria for performance assessment, notwithstanding the arguments of those who claim that it does not make sense to assess forecast performance in the abstract without taking user needs for forecasting performance into consideration. The two objectives need not be incompatible.

Another empirical social forecasting method assessment area lies in the deliberate manipulation of forecasting procedures with artificially patterned or random data to test the discriminability and robustness of such methods, sometimes called "Monte Carlo studies." This kind of analysis is not reported in the social forecasting literature except for occasional studies of econometric and simulation models. Where poor data plague social forecasting research, robustness is of considerable importance. The matter is of considerable concern, too, in assessing complex algorithmic forecasting machinery that could have inherent biases, say given data with highly disparate magnitudes, as noted for regression forecasts. Where large numbers of variables are involved, such analyses are especially helpful in establishing the validity of the algorithmic computations; there may be too many variables to permit an ordinary examination of the implications of functional forms or the accumulated effects of changes in parameters and variables.

No new social forecasting method should be offered for research application without having first been subjected to tests of robustness, particularly where data are poor or social processes highly volatile. The proposed methods should also be

demonstrated empirically to be sensitive enough to discriminate among different pre-patterned sets of test data. Since social processes can be expected to be very complex and involve a good number of variables, efforts to assess social forecasting methods by logical rather than empirical means are likely to be faulty, as noted. Again, it should be possible to assess method selection utility on the basis of such assessments, individually or on a combined basis, with a view toward the development of a forecasting method selection strategy.

Hoos has argued that a precondition to any successful forecasting effort is the availability of data of relevance and good quality — this, unfortunately, is not the case with government statistics, she claims. Morgenstern, Mincer, Zarnowitz, and others have demonstrated that poor data *alone* can create margins of error in economic forecasts greater than that tolerable to economic policymakers. It appears, too, that the margin of error that is tolerable is highly related to the aspect of social process that is being studied and the kind of expectations that have developed with respect to the forecast accuracy of particular kinds of methods. Thus a survey response rate of 90 percent would generally be considered satisfactory for data to be used in a survey forecast, while a 10 percent missing data rate might well prove intolerable in a national economic study utilizing econometric models; data measurement error has similar implications.

The economists and survey forecasters are apparently the bulk of those who have considered in any depth the forecasting significance of missing and inaccurate data; there is a great need to undertake research on the implications of missing, unavailable, or inaccurate data for all of the types of social forecasting

methods. Such an analysis might eliminate, on practical grounds, the use of a particular forecasting method until better data can be found. Special studies, such as the Monte Carlo studies, can be run to test the sensitivity of methods to various kinds of data error.

It will be expected that different social forecasting methods have different data needs in terms of the pure bulk of data required. Modelling forecasting is a vast consumer of data—data required to determine parameter values, to run sensitivity tests to evaluate model structure and parameter settings, and to accommodate usually large numbers of variables. Analogy forecasting is very helpful when data are limited to just a few observations. Theoretical planning and forecasting is even better to use than analogy forecasting when data are very limited. Extrapolative forecasting is a large data consumer although not so extensive as modelling forecasting. Network analysis, criterion analysis, clinical prediction, and survey forecasting are all heavy data users. Intuitive forecasting and planning requires little data. Environmental prediction and predictive social laws are moderate to heavy users of data. Combined methods can be anything from light to heavy data users, depending, obviously, on the methods being combined.

Data availability and requirements depend on the subject matter being studied, but as a rule it would be useful to know at the outset what one is getting into in using one type of forecasting method as opposed to another, as far as data requirements or availability are concerned. Studies that illuminate this question would be of use to social forecasters and policymakers alike in developing grounds for a strategy for forecast method selection.

Not only the volume of data but also the type of data should be considered in such an evaluation. Clinical forecasting data with its heavy demands for typically private data on individuals are exceedingly difficult to come by, particularly for top decision-makers (and even more so for foreign leaders). Theoretical forecasting and planning, on the other hand, are not nearly so demanding. It should be possible to characterize beforehand the type of data needed for each type of social forecasting method and thereby eliminate or select certain forecasting methods for use in terms of this information, assuming a choice exists.

It will happen that the type of social forecasting problem will also constrain data availability—a matter which would have

little to do with social forecasting methodology per se but will have an effect on the viability of a forecasting effort. Some thinking about the implications for social forecasting method selection of the type of data, volume of data, and the type of data required per type of forecasting problem is in order.

Treating problems, data, and methods in terms of categorical types is a useful way to organize methodological inquiry and to guide in social forecasting method selection as long as one keeps in mind the possibility of exceptions to categorization and does not, therefore, adhere too rigidly to such an analysis. It is quite possible that many social forecasting attempts will have to be stated quite tentatively or not attempted at all because of the quality or availability of social process data.

The identification of areas of non-available data should motivate efforts to devise ways to get the needed information; a cooperative effort at problem selection by social forecasters might do much to lay the outlines for a strategy for data collection to get maximum use of scarce research resources (and, possibly, to eliminate costly duplication in data collection). An explicit theoretical framework could assist in such a problem selection effort. The temptation to avoid important social forecasting problems because of data problems should be resisted until it can be clearly shown that missing, poor quality, or unavailable data are the problem and not merely researcher laziness.

There are two kinds of measurement problems the social forecaster has to contend with. One is the conventional concern with precision, a matter not unrelated to user needs for forecast accuracy, as noted. Data errors can and do derive from missing data or imprecision in measurement (assuming valid measures are used to begin with) and, as noted, can and do have important effects on forecast precision. Unfortunately, it is difficult to get high quality social forecasting data: data on a drug treatment program are defective because the participants secretly discard medications (see Hoos), a poll fails to call a primary vote correctly because of a poorly worded and communicated polling instrument; criminal statistics are reported that undercount the true incidence of burglaries, rapes, and muggings; the Census Bureau is unable to reach a significant proportion of minority blacks and Puerto Ricans in a number of large urban centers, and their numbers are therefore undercounted and federal and other aid underprovided; a commercial research organization provides a national sample of personal income data to a federal agency without pre-screening the data to test for missing, spurious, or improperly sampled or coded data; the Labor Department produces artificially deflated estimates of unemployment that are

hotly disputed by minority groups and big city mayors, and so on.

The point is that not all publicly available statistics are worthless, but that any given set of data should be used with caution until data quality control reports are available; it may be that data error accounts for a substantial proportion of maximum tolerable forecasting error. These problems are discussed in the preceding section on data problems. Again, the temptation to avoid certain kinds of social forecasting analyses because of measurement difficulties should be resisted until it is clearly shown that measurement and not researcher procedural error is the true source of difficulty.

The second problem in measurement is even more fundamental — the entity that is supposed to be measured proves to be quite intractable to precise operationalization. This is not to say that it will be forever impossible to devise a conceptualization and measurement for a particular entity, but that at present levels of knowledge no one knows how to do it. Alternatively, one may have an operationalization that satisfies some but is seen as imprecise or ill-founded by others. For example, the federal definition of poverty or Social Security benefits eligibility is stated in arbitrary dollar terms, rather than in terms of basic functional categories, such as housing, food, clothing, and medical care; it is little wonder that there is a great deal of disagreement on the prevalence of poverty or its measurement given such an arbitrary definition of poverty or eligibility.

There is much talk about the sharing or exercising of power by various key subgroups in society, but a close look at the term reveals a considerable disagreement as to its meaning and composition. For some power is equated with economic wealth; for others power is something a closely knit mix of educated elites from Ivy League colleges, government in-and-outers heading private foundations and public policy institutes, Wall Street lawyers and investment brokers, bankers, and corporate elites have and use collectively to control financial and government policy at the city, state, and federal levels; for still others power is public policy control via incumbency in political and judicial offices, chairmanships of key government committees, and leadership of the major political parties. Nor is there agreement on what power does: for some it is coercion, for others it is control of decisionmaking centers, and for still others it is control of resources. A person assigned to forecast the distribution of power

in society in the year 2000 would have to choose among these competing concepts of power and the choice would affect the character of his results. If the person attempted to devise a broad definition encompassing all the various properties attributed to power, he would stand in danger of having a concept so vague and broad as to defy precise operationalization for research purposes.

There is much interest in assuring that all citizens are provided a good public education, but there is a great deal of disagreement as to the appropriate measure for educational system performance. One frequently used output measure, student achievement on standardized tests, such as the New York State Regents exam, is apparently a subject of much dispute between those favoring learning skills and those favoring substantive knowledge; there are also those who attribute learning only partly to the school system, the students' environments also being considered a key contributor to students' learning. A forecast of unemployment would first have to choose among competing definitions of unemployment, the nature of which would significantly alter the forecast conclusions (see Rosenblum). The same could be said for forecasts of the incidence of various categories of crime, the demand for health care, or any number of measures of social process.

These are all fundamental conceptual problems in measurement, and until they are resolved social forecasting results involving such measures are going to be subject to dispute. Facile or self-serving operationalizations of social process concepts should be guarded against, and the temptation toward premature abandonment of efforts at conceptual clarification for data collection and analysis purposes should be resisted until it can be shown that conceptual intractability and not intellectual laziness is the problem.

It would be good scientific practice at any time for social forecasters to define their terms very carefully in the interest of precise operationalization and the communicability of methods and results, but at the present state of development of social science and social forecasting methods such explicitness should be obligatory. Serious social forecasters might want to contribute what they can to resolve at least some of these problems in measurement definition and precision to enable them to undertake improved social forecasting (and in some cases to be

able to undertake a particular social forecasting project at all), to improve the communicability and testability of their results, and to give greater credibility to their results. Until such issues of conceptual clarification are resolved, definitional explicitness, albeit provisional, is a credible interim solution.

Social Processes and Social Forecasting Method Selection

It should be possible to identify social forecasting methods that are best suited to deal with social forecasting problems exhibiting particular kinds of social dynamics. Here, the first task would be to identify the various principal types of social dynamics one is likely to find in social processes. For example, consider the following:

1. continuous, sustained action
2. action characterized by a very few major discontinuities
3. action characterized by numerous discontinuities
4. accelerating or decelerating action
5. cyclical action
6. action characterized by a fundamental transformation in key parameters
7. action characterized by delayed, feedback, or feedforward effects
8. continuous rate, constant speed action
9. catalytic action
10. parallel but unconnected action
11. exponential growth or decay processes
12. action characterized by a particularly high or low level of intensity
13. off versus in-phase action
14. reinforced or dampened action
15. aggregated or decomposed streams of action

16. idiosyncratic human input
17. stochastic, random input
18. action characterized by a few versus multiple actors
19. structured versus unstructured action

The next task would be to identify the methods that would be best to analyze or model a particular kind or combination of social dynamics. For example, while all forecasting methods could be used to analyze continuous sustained action (1), it is very unlikely that this could be done for processes exhibiting fundamental transformations in key parameters (6). Thus extrapolative forecasting, analogy forecasting, intuitive forecasting, intuitive planning, criterion analysis, and network analysis would not be well suited to such a task while environmental prediction, modelling forecasting, clinical prediction, and multimethod forecasting would be because of the rapid accommodation to transformed parameters such methods afford. Cyclical action (5) could be analyzed well by extrapolative forecasting, analogy modelling, or combined forecasting methods through algorithmic adjustment, which other social forecasting methods could not easily make; the same would apply to phased or out-of-phase action (13). Idiosyncratic input (16) could best be assessed in the case of individuals by clinical prediction or for groups by survey forecasting. Theoretical forecasting could make an estimate of idiosyncratic input for groups but with uncertain results, due to the complex behavior patterns of groups; it would be of little use in individual forecasting, except for the theories underlying clinical practice. Other forecasting methods would not apply in this instance.

The point is that a careful, explicit investigation of forecasting algorithms and related methodological properties on the one hand and a similarly careful, explicit investigation of the dynamic properties of social processes on the other hand should make it possible to identify social forecasting methods that are especially useful for the analysis of particular kinds of social dynamics and to identify social forecasting methods that are not likely to work for the same kinds of social dynamics, and, furthermore, possibly to estimate a rank order of performance for those methods that could be used in each instance. Once one knows the kind of social processes that are to be forecast, it is possible to analyze the dynamic properties of such processes; it would then be possible to make a rapid selection of social forecasting methods that would

be best to use for such dynamic properties, given the preceding preparatory analysis. This, then, would be a useful area for research.

An area of analysis closely related to the categorization of social dynamics tasks is empirical study to identify the natural periodicity or speed of action for particular social processes, so that it would become possible to identify better the time frame of applicability of particular social forecasting methods assuming stabilities or continuities in social processes, such as extrapolative forecasting, predictive social laws, or criterion analysis. This proposal is not new to be sure (for example, see Sorokin or Denton and Phillips), but its implementation would be of assistance in the practical selection of forecasting methods.

It might be noted that the recommendation that social forecasting methods be selected in terms of their capabilities for dealing with various kinds of properties of social dynamics also has some precedent — in operations research. Operations researchers have developed analytical techniques to deal with a variety of types of managerial problems that tend to recur with considerable frequency in analysis, some of which have process dynamic properties and some of which have decisional and allocational properties: inventory, resource allocation, queueing, task sequencing, transportation routing, equipment replacement, decisional competition, and informational search problems (Ackoff).

A reading of the forecasting method literature suggests a need to develop new social forecasting methods; many of the procedures now available seem to be plagued by arbitrary and unrealistic assumptions that are not consonant with the way social processes are known or thought to work, are too simplistic or clumsy to deal with anything but a minimum amount of social complexity, or are not suited to the analysis of particular aspects of social process. The social indicators movement has arisen from a perceived need to develop better measures of key social processes and to collect data on them. It is expected that social forecasting efforts will be facilitated by such work to the extent that social forecasting methods are hampered by missing or unavailable data on and inadequate measures of social processes.

From the other — methodological — end, however, there is a need to develop robust methods that will operate effectively with nominal or ordinal data, missing data, and data contaminated by measurement error. There is a need to develop social forecasting methods that can take advantage of the computational and memory capabilities of the new generation of computers (where analysis takes this form) to analyze some of the more complex and hitherto unanalyzable aspects of social process. There is a

need to develop new methods to fill gaps in capabilities to analyze particular aspects of social process, as noted earlier. There is a need to take a look at the developments of mathematics, statistics, and control theory over the last decade to see if these can be applied to the analysis of social processes, particularly the multivariate and hard-to-quantify aspects of social process. The operations research people appear to be particularly adept at this (except for social process measurement problems). The technology exists to process data from a large number of individuals in the social aggregate; this would permit aggregate social utility functions to be approximated and used in social forecasting. Research and communications technology and social research findings can be melded in new ways to obtain hitherto unobtainable social and individual data.

The other side of the R & D coin is to refine existing social forecasting methods to make them more serviceable for social forecasting purposes; this I discussed at length in the sections entitled "Evaluating Assumptions" and "Critical Assessments of the Methods." Existing forecasting methods currently used for technological or narrowly defined social science purposes can be adapted to broader social process applications. Thus operations research techniques (see, for example, criterion analysis and network analysis) have a heavy hardware orientation at present and would require considerable development in, say, social measurement and lower-order-metric analytical techniques to permit their application to social problem areas. Social forecasters might find it to their advantage to do some anticipatory research in this regard by scanning the probable social problem areas that will be most likely to require social forecasting analyses and then building up a methodological capability in forecasting in these areas. This can be done either by undertaking basic research on social forecasting methods or the development of existing forecasting methods. If it were already being done there would be no need to make these obvious points on social forecasting methods research and development.

Many of the points of this section have already been made. Social forecasting should develop on as sound a scientific footing as possible; however, it should be recognized that in some cases, such as clinical prediction, current scientific best is not good enough. As noted, Ikle suggests that there are many policy uses of forecasting results ranging from public relations gimmickry and bureaucratic politics to a great concern that there be as rigorous and accurate forecasting as possible for serious policy-making analysis.

In this paper it is assumed that the objective is to do as accurate forecasting as possible and that the other non-scientific uses will take care of themselves. The key thing to look out for, of course, is that scientific state-of-the-art social forecasting methods are good enough to meet user standards; if it turns out that user needs can be met with the current level of development of a given social forecasting method, then perhaps one can shift scarce methodological research resources to the upgrading of other needed social forecasting methods, but this is only a temporary compromise and should not preclude the development of excellent social forecasting methods per se.

As noted, there is a need for studies to make a sorting of methods

in terms of accuracy on the one hand and user needs for accuracy on the other — not in the abstract, but in terms of specific kinds of problems a particular user or type of user has to deal with. Where the user objective in forecast accuracy is met, it might be an acceptable compromise, as noted, to focus first on other needed research on social forecasting methods before expending excessive scarce resources for a slight increment in forecasting accuracy. Ignoring the non-scientific uses of social forecasting methods and results by policymakers, it might be a useful early step in social forecasting method development to establish user needs for forecast accuracy — a step that is yet to be taken in any systematic manner for social forecasting purposes: who are the users of social forecasts and what are the forecasting accuracy requirements of these users in terms of the types of social problems they are studying or attempting to solve? It should be possible to do these things without losing track of the need to develop social forecasting methodology per se such that various key aspects of social processes can be successfully analyzed, whether or not policymakers are relating to these processes at the moment.

Continuing the argument of the previous section, social forecasting methods specialists should learn how to link up effectively with social theorists and policymakers who are trying to solve problems in their respective domains. This calls for more than the providing of social forecasts having sufficient accuracy to meet user needs for accuracy although this is by no means a small task. It also calls for an understanding of the problem the policymaker(s) is trying to solve or an understanding of the theoretical issue the social scientist is trying to resolve. In addition, there is a need to understand the time constraints and resources that will be allocated to the social forecaster to undertake the proposed study; the research cannot be done on a dime or in an afternoon.

When social forecasting methodologists are not also wearing the hats of policymakers or social theorists, they are going to be dealing with problems defined by others. An ineffectively communicated or misunderstood problem may result in minimally useful (and minimally used) forecasting information, even though it is competently, scientifically based. In the case of policy-relevant social forecasting, the policy sciences literature (see, for example, Lasswell or the journals *Policy Sciences* and

Policy Analysis) suggests ways to determine the true character of the policymaker's problem and to determine whether he or she can make a useful contribution toward resolving it as a social scientist (in some cases, for example, the social scientist as social forecaster may find a sophisticated formal forecasting procedure is not required — the forecast is for window dressing only).

In the case of purely social scientific forecasting issues, the problem is almost entirely an intellectual one in the tradition of scientific inquiry; there are reams of literature discussing how best to resolve issues of this kind. Perhaps it is not too late in this paper to emphasize that the problems of social forecasting are the problems of all social science inquiry related to social processes, and the modes for resolving these problems are in many ways the same.

Given the fact that many social forecasts are ignored by the supposed users, it might be useful to expand on the work of Vancil, Dory and Lord, and others, which investigates the reasons for the non- or subutilization of forecasting information, perhaps in examples taken to some extent outside the social forecasting domain. Is the social theory operationalized correctly or otherwise misinterpreted such that the social forecast produced is useless? Does the theorist reject the possibility of systematic social forecasting on some basis, such as the "inevitably" confounding effects of idiosyncratic events or quantification problems? Is the social forecast ignored by a policymaking group that finds that the social forecast is not specific enough to permit concrete policy steps to be implemented to deal with a social problem or that is ideologically opposed to the concept of a social forecast? Does the group understand the forecast and how to use it?

Once the sources of difficulty in accepting or using forecasts are analyzed, then researchers can begin to study ways of dealing with the non- or subutilization of forecasting information. For example, part of the legitimizing process—if that is what is needed — might be to be more modest in indicating the kind of results that can be expected in a social forecasting effort. The effort to sell forecasting methods to policymakers has underplayed the gaps in data, knowledge, and technique that need to be filled before truly adequate and broad social forecasts can be produced. As a consequence, this selling process has built up unrealistic expectations, which, not met, have led in some cases to the

rejection of social forecasting on any basis (see Brewer on this point). Now that the initial fad of the futurist movement has mellowed a bit, perhaps it is a good time to proceed with some serious in-depth research on social forecasting methods to counteract some of these unrealistic expectations. A similar legitimizing step might be to recognize that for some kinds of forecasting efforts, such as intuitive, theoretical planning, network analysis, or criterion analysis, it is not the role of the social forecaster to make important normative choices in alternatives or objectives and the social forecaster should forbear from such activity unless specifically authorized to do so.

Problems as to the level of complexity at which a social forecast should be developed might to some extent be resolved by a better awareness of the policymaker's problem; this will not, of course, remove the burden of relying upon scientifically informed or theoretical judgment from the social forecaster in deciding what variables to include and exclude from the forecast to make the problem manageable, yet meaningful.

The forecaster is reliant on having accurate information from the policymaker or theorist as to the character of the problem, including requirements for social forecast subject matter, accuracy, time horizon, and specificity, but the thrust of this discussion of proposed research is to place the burden of investigation of these matters on the *forecaster*, for it is he that will better understand the implications of such information for forecasting methodology and know what kind of questions to ask to get the information needed to do the forecast—even though the policymaker or theorist is the one who is actually defining the problem. It is also incumbent on the social forecaster to communicate the results of his analysis in such a fashion that the user will be able to understand them. Research might have to be done to determine optimal presentation formats as well.

Having said this, I think it should be recognized that social forecasters can and should define social forecasting problems on their own. As a matter of pure social science inquiry it is possible that much useful insight into both social processes and social forecasting methodology could derive from such inquiry that could be used in other areas as well. It is possible, too, that policy-makers and social theorists might have missed or incorrectly formulated key social process problems worthy of study and that social forecasters might discover and compensate for this.

Costs

It may be rather dull to talk about such things as data gathering costs, computer data processing costs, manpower costs, and time requirements, but in an environment of scarce social science research resources there is a need to assess social forecasting methods in terms of such factors. There may have to be a tradeoff between social forecast accuracy or time horizon on the one hand and data collection and analysis costs on the other. There is a need to present some tabulations of costs per the various types of social forecasting methods applied to various aspects of social process or particular types of problems, so that a practical determination can be made as to the best forecasting method to use in a particular social forecasting task characterized by certain time and budgetary constraints—this in addition to the usual methodological constraints.

Social forecasting methods and related data processing equipment will improve and cost tabulations will vary over time; nevertheless it is advisable to take these considerations of cost into account on a systematic basis not only to get maximum mileage out of scarce research resources but also to minimize the possibility of endangering the legitimacy of the social forecasting and methodological research enterprise via purely subjective cost and

time estimates for contract research for clients or grant-funded basic research which prove to be seriously in error. In some cases where the social forecasting technique is unique, as in survey and clinical forecasting, methodological substitutions will not be feasible; in other cases substitutions will be possible and economies achievable. Within a given type of social forecasting method there will perhaps be a number of component methodologies to choose from, again offering opportunities for economies and tradeoffs. It is better to study these matters than to guess at them.

Conclusion

The state of the art of social forecasting clearly provides vast opportunity and need for research on social forecasting methods although fairness suggests that recognition be given to the considerable amount of research that has been done on social forecasting methodology to date. The best stance, I believe, is to treat the matter as one of *primarily* scientific inquiry and subject to scientific standards. I have taken pains to indicate that this stance is not and should not be incompatible with contributions to public policy problem solving. Whether or not the reader agrees with the point of view or research suggestions of this paper, if it serves to provoke thought and research on social forecasting methods it will have served its purpose.

Selected References

Abelson, Robert, "Simulation of Social Behavior," in Gardner Lindzey and Elliott Aronson, eds. *The Handbook of Social Psychology*, vol. II. Reading, Mass.: Addison-Wesley Publishing Co., 1968.

Abt, Clark, "An Approach to Methods of Combined Sociotechnological Forecasting," *Technological Forecasting and Social Change*, 2 (1970).

Ackoff, Russell, "Operations Research," in David Sills, ed. *International Encyclopedia of the Social Sciences*. New York: The Free Press and the Macmillan Co., 1968.

Anderson, James, "Causal Models and Social Indicators: Toward the Development of Social Systems Models," *American Sociological Review*, 38 (June, 1973).

Ansoff, H. and Dennis Slevin, "An Appreciation of Industrial Dynamics," *Management Science*, 14 (March, 1968). Reply by Jay Forrester in 14 (May, 1968).

Ansoff, H., et al., "Does Planning Pay? The Effect of Planning on Success of Acquisition in American Firms," *Long Range Planning*, 3 (December, 1970).

Arrow, Kenneth, *Social Choice and Individual Values*. New York: John Wiley & Sons, 1951.

Ayres, Robert, *Technological Forecasting and Long Range Planning*. New York: McGraw-Hill Book Co., 1969.

Baier, K. and N. Rescher, eds. *Values and the Future*. New York: The Free Press, 1969.

Ball, Geoffrey and David Hall, "Background Information on Clustering Techniques," Stanford Research Institute, Menlo Park, Calif., 22 July 1968, mimeo.

Barber, James, *The Presidential Character: Predicting Performance in the White House*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972.

Barkun, Joel and James Bruno, "Operations Research in Planning Political Campaign Strategies," *Operations Research*, 20 (September–October, 1972).

Bates, J. and C. Granger, "The Combination of Forecasts," *Operational Research Quarterly*, 204 (December, 1969).

Baumol, William, "Activity Analysis in One Lesson," *American Economic Review*, 48 (1958).

Bell, Daniel, *The Coming of Post-Industrial Society: A Venture in Social Forecasting*. New York: Basic Books, Inc., 1973.

Bell, Daniel, "Futurism and Forecasting: An Assessment of the Field." Research proposal submitted to the National Science Foundation, 1 March 1972, mimeo.

Bell, Daniel, ed. *Toward the Year 2000: Work in Progress*. Boston: Beacon Press, 1967.

Bell, Daniel, "Twelve Modes of Prediction: A Preliminary Sorting of Approaches in the Social Sciences," *Daedalus*, 93 (Summer, 1964).

Bellman, Richard and Stuart Dreyfus, *Applied Dynamic Programming*. Princeton, N.J.: Princeton University Press, 1962.

Benedict, Ruth, *The Chrysanthemum and the Sword*. Boston: Houghton Mifflin Co., 1946.

Bergin, Allen and Sol Garfield, eds. *Handbook of Psychotherapy and Behavior Change*. New York: John Wiley & Sons, 1971.

Box, George and Gwilym Jenkins, *Time Series Analysis, Forecasting, and Control*. San Francisco: Holden-Day, Inc., 1970.

Brams, Steven and Michael O'Leary, "An Axiomatic Model of Voting Bodies," *American Political Science Review*, 64 (June, 1970).

Brewer, Garry, *Politicians, Bureaucrats, and the Consultant: A Critique of Urban Problem Solving*. New York: Basic Books, Inc., 1973.

Bright, James, "A Few Kind Words for Trend Extrapolation," *Futures*, 5 (August, 1973).

Bright, James, "Some Insights from the Analysis of Past Forecasts," in James Bright and Milton Shoeman, eds. *Technological Forecasting*. Canoga Park, Calif.: XYZYZ Information Corp., 1970.

Bright, James, ed. *Technological Forecasting in Industry and Government: Methods and Applications*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968.

Bright, James and Milton Schoeman, eds. *A Guide to Practical Technological Forecasting*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1973.

Brown, Robert, *Smoothing, Forecasting, and Prediction of Discrete Time Series*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962-63.

Brunner, Ronald and Garry Brewer, *Organized Complexity: Empirical Theories of Political Development*. New York: The Free Press, 1971.

Burch, S. and H. Stekler, "The Forecasting Accuracy of Consumer Attitude Data," *Journal of the American Statistical Association*, 64 (December, 1969).

Butler, William and Robert Kavesh, eds. *How Business Economists Forecast.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966.
Butler, William, Robert Kavesh, and Robert Platt, *Methods and Techniques of Business Forecasting.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1974.

Cetron, Marvin and Christine Ralph, *Industrial Applications of Technological Forecasting: Its Utilization in R & D Management.* New York: John Wiley & Sons, 1971.
Chapman, John, "Political Forecasting and Strategic Planning," *International Studies Quarterly,* 15 (September, 1971).
Chisholm, Roger and Gilbert Whitaker, Jr., *Forecasting Methods.* Homewood, Ill.: Richard D. Irwin, 1971.
Churchman, C. West, et al., *Introduction to Operations Research.* New York: John Wiley & Sons, 1957.
Cohen, Morris, "Surveys and Forecasting," in William Butler and Robert Kavesh, eds. *How Business Economists Forecast.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1966.
Converse, Jean and Howard Schuman, *Conversations at Random: Survey Research as Interviewers See It.* New York: John Wiley & Sons, 1974.
Coombs, Clyde, *A Theory of Data.* New York: John Wiley & Sons, 1964.
Crecine, John, "Defense Budgeting: Constraints and Organizational Adaptation." Institute of Public Policy Studies Discussion Paper 6, University of Michigan, Ann Arbor, Mich., July, 1969, mimeo.
Crecine, John, *Governmental Problem Solving: A Computer Simulation of Municipal Budgeting.* Chicago: Rand McNally and Co., 1969.
Crespi, Irving, "What Kinds of Attitude Measures Are Predictive of Behavior?," *Public Opinion Quarterly,* 35 (Fall, 1971).

Dalkey, Norman and Olaf Helmer, "An Experimental Application of the Delphi Method to the Use of Experts," *Management Science,* 9 (April, 1963).
Dantzig, G., *Linear Programming and Extensions.* Princeton, N.J.: Princeton University Press, 1963.
de Jouvenel, Bertrand, *The Art of Conjecture.* New York: Basic Books, Inc., 1967.
Denton, Frank and Warren Phillips, "Some Patterns in the History of Violence," *Journal of Conflict Resolution,* 12 (July, 1968).
Dory, John and Robert Lord, "Does TF Really Work?," *Harvard Business Review,* 48 (November–December, 1970).
Draper, N. and H. Smith, *Applied Regression Analysis.* New York: John Wiley & Sons, 1966.
Dror, Yehezekel, "Alternative Domestic Politics Futures (ADPF): Research Needs and Research Design." Santa Monica, Calif.: Rand Corporation, February, 1970, P-4306.
Dror, Yehezekel, *Public Policymaking Re-examined.* San Francisco: Chandler Publishing Co., 1968.
Duncan, Otis Dudley, "Social Forecasting: The State of the Art," *The Public Interest,* 17 (Fall, 1969).

Feller, William, *An Introduction to Probability Theory and Its Applications*, 2nd ed., vol. 1. New York: John Wiley & Sons, 1957.

Fink, Clinton, "More Calculations About Deterrence," *Journal of Conflict Resolution*, 9 (March, 1965).

Fleiss, Joseph and Joseph Zubin, "On the Methods and Theory of Clustering," *Multivariate Behavioral Research*, 4 (April, 1969).

Forrester, Jay, "Counterintuitive Behavior of Social Systems," *Technology Review* (January, 1971).

Forrester, Jay, *World Dynamics*. Cambridge, Mass.: Wright-Allen Press, Inc., 1971.

Fromm, Erich, *The Sane Society*. New York: Holt, Rinehart, and Winston, 1955.

The Futures Group, "Trend Impact Analysis." Mimeo, n.d. (est. 1974–75).

George, Alexander and J. George, *Woodrow Wilson and Colonel House*. New York: John Day Co., 1956.

Geyl, Pieter and Arnold Toynbee, "Can We Know the Pattern of the Past?—A Debate," in Patrick Gardiner, ed. *Theories of History*. Glencoe, Ill.: The Free Press, 1959.

Glock, Charles, ed. *Survey Research in the Social Sciences*. New York: Russell Sage Foundation, 1967.

Goecke, Joseph, "America's Future as Reflected in Public Opinion Trends," *The Futurist*, 5 (April, 1971).

Gordon, Theodore, et al., "The Use of Cross-Impact Matrices for Forecasting and Planning." Institute for the Future, Middletown, Conn., April, 1970, R-1.

Gordon, Theodore and O. Helmer, "Report on a Long-Range Forecasting Study." Santa Monica, Calif.: Rand Corporation, September, 1964, P-2982.

Gough, Harrison, "Clinical Versus Statistical Prediction in Psychology," in Leo Postman, ed. *Psychology in the Making*. New York: Alfred A. Knopf, 1962.

Gueron, Georges, "Reflections on Social Forecasting," *Futures*, 5 (December, 1973).

Hadley, G., *Nonlinear and Dynamic Programming*. Reading, Mass.: Addison-Wesley Publishing Co., 1964.

Hall, Edward T., *The Silent Language*. Garden City, N.Y.: Doubleday & Co., 1959.

Hall, William, "Forecasting Techniques in the Corporate Planning Process," *Managerial Planning*, 21 (November–December, 1972).

Harrison, Daniel, "Social Forecasting Methods." Unpub. manuscript, February, 1973.

Hays, William, *Statistics for the Social Sciences*, 2nd ed. New York: Holt, Rinehart, and Winston, Inc., 1973.

Helmer, Olaf, "An Agenda for Futures Research," *Futures*, 7 (February, 1975).

Hennessy, Bernard and Erna Hennessy, "The Prediction of Close Elections: Comments on Some 1960 Polls," *Public Opinion Quarterly*, 25 (Fall, 1961).

Hilsman, Roger, *The Crouching Future International Politics and U.S. Foreign Policy, A Forecast.* New York: Doubleday and Co., 1975.
Holt, Robert, "Yet Another Look at Clinical and Statistical Prediction; or, Is Clinical Psychology Worthwhile?," *American Psychologist,* 25 (April, 1970).
Hoos, Ida, "Criteria for 'Good' Futures Research," *Technological Forecasting and Social Change,* 6 (1974).

Ikle, Fred, "Can Social Predictions Be Evaluated?," in Daniel Bell, ed. *Toward the Year 2000: Work in Progress.* Boston: Beacon Press, 1967.
Ikle, Fred, "Social Forecasting and the Problem of Changing Futures," *Futures* 3 (June, 1971).

Jantsch, Erich, *Technological Forecasting in Perspective.* Paris: OECD, July, 1967.
Johnston, Denis, "Forecasting Methods in the Social Sciences," *Technological Forecasting and Social Change,* 2 (1970).
Johnston, J., *Econometric Methods,* 2nd ed. New York: McGraw-Hill Book Co., 1972.

Kahn, Herman and Anthony Wiener, *The Year 2000: A Framework for Speculation for the Next Thirty-Three Years.* New York: The Macmillan Co., 1967.
Kernberg, Otto, et al., "Psychotherapy and Psychoanalysis: Final Report of the Menninger Foundation's Psychotherapy Research Project," *Bulletin of the Menninger Clinic,* 36 (January–March, 1972).
Knapp, Peter, "Suitability for Psychoanalysis," *Psychoanalytic Quarterly* (1960).

Langer, Walter, *The Mind of Adolph Hitler.* New York: Basic Books, Inc., 1972.
Lasswell, Harold, "The Garrison State Hypothesis Today," in Samuel Huntington, ed. *Changing Patterns of Military Politics.* New York: The Free Press, 1962.
Lasswell, Harold, *A Pre-View of Policy Sciences.* New York: American-Elsevier Publishing Co., Inc., 1971.
Levin, Richard and C. Kirkpatrick, *Planning and Control With PERT-CPM.* New York: McGraw-Hill Book Co., 1966.
Liebert, Robert, *Radical and Militant Youth: A Psychoanalytic Study.* New York: Praeger Publishing Co., 1971.

Martino, Joseph, "The Consistency of Delphi Forecasts," *Futurist,* 4 (April, 1970).
Martino, Joseph, *Technological Forecasting for Decision Making.* New York: American-Elsevier Publishing Co., Inc., 1972.
Mayne, John, "Accuracy, Precision, and Reliableness in Operational Research," *Operations Research,* 11 (November–December, 1963).

Maynes, E. Scott, "An Appraisal of Consumer Anticipations Approaches to Forecasting," in American Statistical Association, *1967 Proceedings of the Business and Economic Statistics Section.*

Mazlish, Bruce, ed. *The Railroad and the Space Program: An Exploration in Historical Analogy.* Cambridge, Mass.: MIT Press, 1965.

Mead, Margaret, "The Study of National Character," in Daniel Lerner and Harold Lasswell, eds. *The Policy Sciences.* Stanford, Calif.: Stanford University Press, 1951.

Meadows, Dennis, William Behrens, III, Donella Meadows, Roger Naill, Jorgen Randers, and Erich Zahn, *Dynamics of Growth in a Finite World.* Cambridge, Mass.: Wright-Allen Press, Inc., 1974.

Meadows, Donella, Dennis Meadows, Jorgen Randers, and William Behrens, III, *The Limits to Growth.* New York: Universe Books, 1972.

Meehl, Paul, *Clinical Versus Statistical Prediction: A Theoretical Analysis and A Review of the Evidence.* Minneapolis, Minn.: University of Minnesota Press, 1954.

Mendelsohn, Harold and Irving Crespi, *Polls, Television, and the New Politics.* Scranton, Pa.: Chandler Publishing Co., 1970.

Merton, Robert K., *Social Theory and Social Structure,* 2nd ed. New York: The Free Press, 1957.

Mesarovic, Mihajlo and Eduard Pestel, *Mankind at the Turning Point.* New York: E. P. Dutton and Co., Inc., 1974.

Miller, James, "Living Systems: Basic Concepts," *Behavioral Science,* 10 (July, 1965); also second part, "Structure and Process," *Behavioral Science* 10 (October, 1965).

Millstein, Jeffrey and William Mitchell, "Dynamics of the Vietnam Conflict: a Quantitative Analysis and Predictive Computer Simulation," Peace Research Society (International), *Papers,* 10 (1969).

Mincer, Jacob, ed. *Economic Forecasts and Expectations: An Analysis of Forecasting Behavior and Performance,* New York: National Bureau of Economic Research, 1969.

Moder, Joseph and Cecil Phillips, *Project Management With CPM and PERT.* New York: Van Nostrand–Reinhold Co., 1970.

Morgenstern, Oskar, *On the Accuracy of Economic Observations.* Princeton, N.J.: Princeton University Press, 1963.

Moser, C. and G. Kalton, eds. *Survey Methods in Social Investigation.* London: Heinemann Educational Books Ltd., 1971.

Naroll, R., *Data Quality Control.* Glencoe, Ill.: The Free Press, 1962.

Nisbet, Robert, "The Year 2000 and All That," *Commentary,* 45 (June, 1963), 60-66.

Orcutt, Guy, et al., *Micro-Analysis of Socio-Economic Systems: A Simulation Study.* New York: Harper and Row, 1961.

Peterson, Donald, *Clinical Study of Social Behavior.* New York: Appleton-Century-Crofts, 1972.

Phillips, Lawrence, *Bayesian Statistics for Social Scientists.* London: Thomas Nelson and Sons, Ltd., 1973.

Platt, John, *The Step to Man.* New York: John Wiley & Sons, 1966.

Pocock, J., "PERT as an Analytical Aid for Program Planning—Its Payoff and Problems," *Operations Research,* 10 (November–December, 1962).

Quandt, Richard, "Programming," in David Sills, ed. *International Encyclopedia of the Social Sciences.* New York: The Free Press and the Macmillan Co., 1968.

Quenouille, M. H., *Associated Measurements.* New York: Academic Press, 1952.

Raiffa, Howard, *Decision Analysis: Introductory Lectures on Choices Under Uncertainty.* Reading, Mass.: Addison-Wesley Publishing Co., 1968.

Rashevsky, Nicholas, *Looking at History Through Mathematics.* Cambridge, Mass.: MIT Press, 1968.

Rastogi, P. N., "The Problem of Prediction in the Social Systems," *International Review of Sociology* 5 (December, 1969).

Richardson, Lewis, *Arms and Insecurity: A Mathematical Study of the Causes and Origins of War.* Pittsburgh: The Boxwood Press, 1960.

Rivlin, Alice, *Systematic Thinking for Social Action.* Washington, D.C.: The Brookings Institution, 1971.

Roberts, Edward, "Exploratory and Normative Technological Forecasting: A Critical Appraisal," *Technological Forecasting,* 1 (Fall, 1969).

Rosenberg, Morris, *The Logic of Survey Analysis.* New York: Basic Books, 1968.

Rosenblum, Marc, *"Federal Manpower Forecasting: An Analysis and Evaluation,"* Ph.D. diss., University of Minnesota, June, 1972.

Rothstein, Robert, *Planning, Prediction, and Policymaking in Foreign Affairs.* Boston: Little, Brown, and Co., 1972.

Rotter, Julian, *Clinical Psychology.* Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971.

Rummell, Rudolph, "Forecasting International Relations: A Proposed Investigation of Three-Mode Factor Analysis," in Joseph Martino and Thomas Overbeck, eds. *Approaches to Long Range Forecasting: A Symposium.* Almagordo, New Mexico, 29-30 April 1969, Washington, D.C.: USGPO, 1970.

Schlaifer, Robert, *Analysis of Decisions Under Uncertainty.* New York: McGraw-Hill Book Co., 1967.

Schuessler, Karl, "Prediction," in David Sills, ed. *International Encyclopedia of the Social Sciences.* New York: The Free Press and the Macmillan Co., 1968.

Sheppard, William, "Relevance Analysis in Research Planning," *Technological Forecasting,* 1 (1970).

Shubik, Martin, "Modelling on a Grand Scale," *Science,* 174 (3 December 1971).

Singer, J. David, "Correlates of War Project: Interim Report and Rationale," *World Politics,* 24 (January, 1972).

Sisk, John, "The Future of Prediction," *Commentary*, 49 (March, 1970).
Smoker, Paul, "Social Research for Social Anticipation," *American Behavioral Scientist*, 12 (July–August, 1969).
Sorokin, Pitirim, *Social and Cultural Dynamics*. New York: Bedminster Press, 1962.
Sprout, Harold and Margaret Sprout, *The Ecological Perspective on Human Affairs With Special Reference to International Politics*. Princeton, N.J.: Princeton University Press, 1965.
Steiner, George, ed. *Top Management Planning*. New York: The Macmillan Co., 1969.
Stekler, H., "Forecasting With Econometric Models: An Evaluation," *Econometrica*, 36 (July–October, 1968).
Stolper, Wolfgang, *Planning Without Facts: Lessons in Resource Allocation From Nigeria's Development*. Cambridge, Mass.: Harvard University Press, 1966.
Suits, Daniel, "Forecasting and Analysis With an Econometric Model," *American Economic Review*, 52 (March, 1962).
Swager, William, "Perspective Trees: A Method for Creatively Using Forecasts." Columbus, Ohio: Battelle Memorial Institute, 27 May 1971, mimeo.
Swaylian, B. and L. Stanfel, *Optimization Techniques in Operations Research*. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1975.

Tanter, Raymond, *Modeling and Managing International Conflicts: The Berlin Crisis*. Beverly Hills, Calif.: Sage Publications, 1974.
Taviss, Irene, "Futurology and the Problem of Values," *International Social Science Journal*, 21 (1969).
Theil, Henri, *Applied Economic Forecasting*. Amsterdam: North-Holland Pub., 1966.
Thierauf, Robert and Robert Klekamp, *Decision Making Through Operations Research*, New York: John Wiley & Sons, 1975.

University of Sussex Science Policy Research Unit, United Kingdom, "The Limits to Growth Controversy," special issue of *Futures* 5 (February, 1973), with response by Dennis Meadows, et al.

Vancil, Richard, ed. *Formal Planning Systems: 1971*. Cambridge, Mass.: Harvard Business School, 1971 (part of an annual series on the same subject).
von Bertalanffy, Ludwig, *General Systems Theory: Foundations, Development, Applications*. New York: George Braziller, 1968.

Waldhorn, Herbert, *Indications for Psychoanalysis*. New York: International Universities Press, 1967.
Webb, Eugene, et al., *Unobtrusive Measures*. Chicago: Rand McNally & Co., 1966.
Webber, John, "Prediction of Outcomes," *Psychiatric Quarterly*, 48 (1974).
Weil, Herman, "Forecasting Conflict: A Multimodel-Multimethod Approach." Paper presented at the 1975 annual meeting of the International Studies Association, Washington, D.C., February, 1975, mimeo.

Weiss, Paul, *Hierarchically Organized Systems in Theory and Practice*. New York: Hafner Press, 1971.
Werbos, Paul, *"Beyond Regression: New Tools for Prediction and Analysis in the Behavioral Sciences,"* Ph.D. diss., Harvard University, 1974.
Wildavsky, Aaron, "Does Planning Work?," *The Public Interest* (Summer, 1971).
Winthrop, Henry, "The Sociologist and the Study of the Future," *The American Sociologist*, 3 (May, 1968).
Wolman, Benjamin, *Handbook of Clinical Psychology*. New York: McGraw-Hill Book Co., 1965.

Young, Michael, ed. *Forecasting and the Social Sciences*. London: Heinemann, 1968.
Young, Robert, et al., "A General Handbook for Long-Range Environmental Forecasting," vol. I, and "Quantitative Methods for Long-Range Environmental Forecasting," vol. II. Arlington, Virginia: Consolidated Analysis Centers, Inc., 1973, mimeo.

Zarnowitz, Victor, *An Appraisal of Short-Term Economic Forecasts*. New York: National Bureau of Economic Research, 1967.
Zarnowitz, Victor, ed. *The Business Cycle Today*. New York: National Bureau of Economic Research, 1972.
Zipf, George, *Human Behavior and the Law of Least Effort*. Reading, Mass.: Addison-Wesley Publishing Co., 1949.
Zubin, Joseph, "Clinical Versus Actuarial Prediction: A Pseudo-Problem," in Ann Anastasi, ed. *Testing Problems in Perspective*. Washington, D.C.: American Council on Education, 1966.
Zwicky, Fritz, "The Morphological Approach to Discovery, Invention, Research, and Construction," in Fritz Zwicky and A. Wilson, eds. *New Methods of Thought and Procedure*, New York: Springer-Verlag, 1967.