MODELLING SOCIAL INDICATORS: SOME PROBLEMS, PROSPECTS, AND ILLUSTRATIONS

Robert W. HODGE

Ricardo KLORMAN

State University of New York
at Stony Brook

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INTRODUCTION

Although the compilation of systematic socioeconomic indicators can be dated in centuries rather than in years, a veritable explosion of social monitoring activities has occurred the post-World War II era. These data, which pertain to matters as diverse as presidential popularity and chronic physical ailments, provide the basis for developing a dynamic model of society. Although several series, especially economic and demographic ones, can be studied over a substantial period of time, many of the key variables which would obviously enter into a model of the society such as educational attainment, church attendance, the occupational division of labor, and geographic mobility have been recorded on an annual basis in our society only for the past two or three decades.

For the past three years we have been systematically exploring the expanding pool of annual social indicators in an effort to assess the feasibility of constructing a dynamic model of American society which complements existing economic models by virtue of its inclusion of social, demographic, and political indicators. In the course of our efforts, we have encountered numerous technical and methodological obstacles. Our purpose in this essay is to discuss some of these difficulties and to provide substantive illustrations of them. To the best of our knowledge, all of the problems discussed herein are well known to econometricians;

the main purpose of this essay is simply to pose and illustrate these difficulties in the context of sociopolitical, rather than purely economic indicators.

In preparing this survey of some methodological issues encountered in the analysis of time series and the construction of structural equation models based upon them, we have attempted to reduce technical details to a bare minimum in order to make this essay accessible to the widest possible audience. More formal treatments are already available in most standard econometric texts; little would be served by duplicating those formalities herein. Although this essay is more broadly conceived, we should also note that many of the technical problems broached herein have also been raised in the sociopolitical literature by Russett (1971), in an essay which leans solely upon the analysis of defense expenditures for illustrative material. His essay can, in our judgement, be profitably read in conjunction with the present review.

1. THEORIES AND MODELS OF SOCIETY

The analogue in economics to the type of model we are attempting to build for the society are the early, annual time-series models such as those of Tinbergen (1939) and of Klein and Goldberger (1955). In pursuing this type of model building, the economists have an enormous advantage owing to the development of a theoretically grounded system of national economic accounts. The concepts which routinely appear in economic theory -- prices, wages, short and long-term interest rates, household assets, savings, and so forth -- are largely measurable variables with established definitions. The connection between social theory and social statistics is by no means so orderly.

The theory of society and social organization as it now stands is more nearly about <u>concepts</u> than <u>variables</u>. It is informed in a large degree by things which have yet to be measured satisfactorily--notions like anomie and alienation, legitimacy and latent pattern maintenance, co-optation and collective conscience, opportunity structures and opinion leadership, social status and social solidarity, and so on and so forth. The problem is not so much that these concepts are inherently unmeasurable, for we believe they can be measured, but that the theories about them do not provide us with definitive guidelines about how to measure them. Considerable progress has, of course, been made in measuring many of these concepts in cross-sectional studies; however, the connection between the theory of society and the considerable body of social statistics which are available on an annual basis is far from clear. Many social and demographic statistics were never designed to serve the interest of social theory in the first place, but as tools to guide government planning in such areas as health, education, intergroup relations, and welfare.

The upshot of these remarks is that the correspondence between the concepts which appear in the theory of society and the array of currently available, annual social measurements is very weak. Given that one nevertheless desires to model existing indicators, there are several strategies for model-building. The most obvious one, of course, is that pursued by the psychologist, Raymond B. Cattell, and his students in a series of papers which are by now quite dated (Cattell, 1953; Cattell and Adelson, 1951; Gibb, 1956). Their strategy was simply to assemble a number of easily accessible indicators and to submit their matrix of temporal intercorrelations to a factor analysis. A model of this sort lies in a theory of society whose dimensions are specified by letting their measurements emerge from the data. The sense in which one "tests" the theory rests in the correspondence between whatever factors emerge from the analysis and whatever preconceived notions one might have about how the indicators incorporated in the analysis relate to the organization of a society.

This approach to building a macro-sociological model strikes us as defective in two fundamental ways. First, what one finds is very largely a function of which variables are entered into the analysis. Leaving a cluster of variables in or out of the analysis can very easily alter the properties of the factor space. If one has a clear conception of the measured variables which should be included in the analysis, it seems very likely that one's theory can already be stated at least partially in terms of manifest variables rather than solely in terms of latent ones (as is the case with factor analysis). Second, and what strikes us as more important, there are no criteria for specifying the extent of intercorrelation between the underlying latent variables. Consequently, the typical procedure in many factor analyses is to force the extracted factors to be orthogonal to each other. This is an extraordinarily peculiar model of society. In general, we think that important forces in society are causally related to each other, those causal linkages being one of the criteria by which their significance is assessed. Selecting a factor model which forces the underlying latent variables to be orthogonal violates our accumulated knowledge about society which, inadequate though it may be, makes it very difficult to relinquish the idea that significant social forces are, indeed, associated with each other. Factor analyses of social indicators probably accomplish little more than to confuse causes and their effects by lumping them together as though they were indicators of the same thing.

The above remarks should not be interpreted as an indictment of factor analysis as a method of statistical analysis. There are situations in which it is an appropriate tool. Recent developments, particularly Jöreskog's work (1969, 1970) on confirmatory factor analysis, have cemented the usefulness of factor models within the more general processes of theory testing and theory construction. Our remarks should be interpreted only as indicative of our evaluation of the likely strides to be made toward constructing a macrosociological model of society by collecting together a bunch of social time-series and blindly extracting orthogonal factors from them. Such an effort to let the data speak for themselves by forcing a particular structure upon them is just plain misleading in so far as there are ample theoretical grounds for believing the data do not conform to the mathematical model being imposed upon them.

A second strategy for constructing a macrosociological model of society, in view of the weak correspondence between social measurements and the theory of society, is to develop new conceptual schemes which are more appropriate to the available social time-series. This, in part, is one of the implicit goals of the so-called social indicators movement. Numerous proposals and perspectives have been advanced in this direction. They are much too diverse to be adequately reviewed in a brief paper. We may, however, note that two of the key concepts which keep reappearing in the social indicators literature in one or another guise are those of "societal health" (or the related notion of "quality of life") and "social money" (or the related notion of a basic unit for social accounting such as time expenditures). Both concepts have been developed via analogy to parallel concepts in economics.

The notion behind the concept of "societal health" is basically that of developing social indicators which will monitor the society in much the same way that the economy is monitored by economic ones. "Health" comes into the picture because ultimately values, as expressed in stated social and economic goals, are involved. We commonly assess the "health" of the economy by comparing the values of economic indicators with the values we think they should have were our economic goals attained. The economic goals we hear the most about these days are those of full employment and modest inflation. We monitor our economic performance in these regards by recourse to the unemployment rate and the consumer price index. Since frictional unemployment probably runs at about 4 percent, we start worrying when the unemployment rate creeps much above that level, just as we personally start worrying about inflation whenever prices are rising faster than our wages.

Evidently, we could assess the health of the society in much the same way as we routinely assess the health of the economy if we could settle upon some key social goals and devise some indicators whose values are known when the goals have been achieved. The basic problem with the concept of "societal health" is identifying social goals which have not already been achieved and about which there is uniform consensus. Most people think one should be able to get a job if one wants one, so they can support a policy of full employment. Similarly, most people would not want their real wages to decline; consequently, they can support a policy to keep inflation under control because their actual wages are often fixed in the short run. Finding an equivalent level of consensus about most areas of social life and, indeed, about many facets of the economy is easier said than done. As between the several states, there is substantial variation in the existence and enforcement of laws governing such basic social institutions and processes as marriage, family dissolution, incarceration of convicted offenders, prostitution, labor-management relations, intergroup relations, and professional practices, among other things. Such diversity in legal codes and sanctions suggests an equivalent diversity in conceptions of society and the acceptability of alternative social goals.

The analogy between societal and economic health not only may flounder upon the problem of identifying common social goals, but also loses sight of a significant raison d'etre for society. In some measure, all societies are in part sustained as vehicles for the peaceful resolution of interpersonal and intergroup conflict. Individuals and groups left free to pursue their own ends evitably come into contact and conflict; when that happens, society and its social institutions are there to adjudicate the dispute. In this view, the business of society and, in particular, the state, is exhausted by the establishment of an institutional framework which allows individual citizens to pursue their own personal goals with reasonable security from violence and provides for the orderly reconciliation of individual and intergroup conflict. Societies solve the problem of order and they do it, obviously, in different ways. Once that goal is achieved, the price of further collective goal setting is the limitation of individual freedom, for the institutionalization of goals beyond the establishment of order promotes conformity and social homogeneity, rather than fostering individual differentiation and the liberty to pursue personal ends.

In sum, one cannot talk about "societal health" without also specifying a set of social goals against which the performance of the society may be judged. Since one can plausibly maintain that good governance is limited governance, the very process of setting forth an expanded array of social goals can be attacked as counter-productive because it involves further collective interference with individual freedom. This doesn't mean that the concept of "societal health" is not a viable one, but it does mean that there are as many versions of it as there are defensible political philosophies. Refining such a concept does not seem a likely way to make much progress toward a macrosociological model of how society works.

Since the problem of order must be addressed by an political philosophy, there is a temptation to define the health of a society in terms of its remove from anarchy. Statistics concerning violent crimes, attempted assassinations, and other forms of aggression directed against persons, property, and the state would become the pivotal indicators of "societal health." The main flaw in this line of development is that, at least since Durkheim, sociologists have regarded some degree of conflict and norm violation as itself productive of social solidarity. The only way that we can possibly know that social sanctions are effective is to have them periodically tested, and the only way we can know that the process of adjudication peacefully resolves conflicts is to have conflicting parties put that system to work. Consequently, we are at a loss to specify the optimum level of conflict in a social system. We can plausibly argue that it should not be zero, and we surely know there are limits that it cannot exceed. However, the precise values of those bounds are not known. Thus, we could not evaluate "societal health" by recourse to statistics on violence and related matters because we would not know whether an observed level of norm violations was above or below the optimum.

The second concept which emerges in the social indicators literature is that of "social money." This concept, also borrowed by analogy from economics, involves the isolation of a unit of social exchange parallel to the role of money in economic exchange. Presumably, the yardstick of "social money" could be or, for that matter, is already implicitly used to assess the "social value" of the reciprocal actions which occur over the course of a social exchange. Different authors have held this concept in different regards. Indeed, some authors want it both ways. For example, Blau claims (1964, p. 94), "In contrast to economic commodities, the benefits involved in social exchange do not have an exact price in terms of a single quantitative medium of exchange. . ," but subsequently he informs us that (*ibid.*, p. 151), "In the course of social exchange, a going rate of exchange between two social benefits becomes established."

Evidently, social and, indeed, economic exchanges do occur without use of the medium of money; such exchanges are governed by a concept of equivalent value, which at least suggests they could be translated into an arbitrary scale of values. Intuitively, the existence of "social cents" makes social sense, but the scale of values involved should itself prove amenable to expression in terms of ordinary money. In other words, we doubt if "social money" proves to be different from the kind we already have. In any case, we suspect that sociologists will continue to be intrigued by the possibility of defining a unit of value analogous to economic currency and building up a theory of social accounts analogous to economic ones. We are dubious about the veracity of the analogies and, at least as of this writing, there has been no substantial payoff from this line of inquiry.

A third strategy for constructing macrosociological models in view of the weak correspondence between social measurements and the theory of society, is simply to accept available social indicators as significant parameters of society in their own right and proceed to build models of them directly, eschewing the latent construct approach altogether and retaining the notion of latent variables only in the sense of the true values of measured ones. Considerable progress has been made on this front, largely by the English economist Richard Stone (1966, 1971) whose social accounting models, particularly of the educational sector, have greatly expanded the substantive purview of the basic demographic model of birth and death processes. This basic strategy of modelling the available measures, rather than latent constructs of which they are mere indicators, has been followed in our own work. However, our endeavors diverge considerably from those of Stone. The reason for this is simple enough: in our work we are treating as endogenous many of the parameters which are exogenous constants in a demographic accounting model. Consequently we are rarely able to take advantage of the accounting identities upon which stable population and related theories rest, though inherently there is no reason why our efforts could not be wedded to such an accounting model. Before that is done, however, we would need considerably more confidence in our equations for fertility, mortality, marriage, family dissolution, etc., than we now have.

In our own work, we have found little difficulty in developing theoretically grounded equations for most of our endogenous variables. The rich backlog of middle range theory and the accumulated results of empirical research provide an adequate basis for specifying most equations. Of course, things do not always turn out the way we think they should, and that is a source of constant irritation since one is always loathe to throw away established conceptions simply because they are inconsistent with a single data set. The main difficulty with this approach in our view is that the end result is more nearly a <u>collection of equations</u> than a well developed <u>model</u> in which not only the justification for each equation is known, but also the nature of the interconnections between the several equations.

This discussion by no means exhausts the theoretical problems which must be coped with in attempting to develop a macro-sociological model of society. We hope, however, that it does provide a sense of the enormous task of theory construction which must accompany the empirical development of such a model. Not only is there no clear point of departure in the current literature, but the broad substantive areas covered by such a model requires an investigator to have a working knowledge of most fields of sociological inquiry, as well as many areas of demography, political science, and economics. It is probably foolhardy to believe any one or two investigators could develop a wholly satisfactory model of the entire society; the best one can hope for is that whatever preliminary, overall model one develops will stimulate specialists in the several areas it covers to construct more adequate specifications of the sub-sectors of the model which touch upon their areas of expertise. We now turn to successive discussions of some of the common problems encountered in work of this kind.

2. THE BOUNDARY PROBLEM

A perennial problem in empirical research is the limitation of one's domain of inquiry to manageable proportions. There are several facets to this general problem, including the definition of the population to which the investigation pertains and the selection of the appropriate variables for analysis. The latter of these tasks--the allocation of variables and the measurements of them to alternative domains of inquiry--serves to define, if only implicitly, system boundaries.

A considerable amount of prose has and could be written about system boundaries and the boundary maintaining properties of socioeconomic systems. In practice, however, such boundaries are only constructions of reality which serve the purpose of scientific inquiry, rather than mirror images of intersystem divisions observable in the world around us. The boundaries we draw are both arbitrary and artificial; our main guide in constructing them is their potential usefulness in reducing a problem or a subject matter to tractable proportions without distorting systems of actual behavior beyond recognition.

Writing in Social Change in the Industrial Revolution, Neil Smelser observes,

> To characterize an industrial goal raises several general problems. Empirically the units of an industry do not coincide with the analytical categories exactly. To illustrate this point, let us consider the educational system for a moment. Empirically this concept refers to an aggregate of schools, academies, and institutes.

> These units form a social system whose primary goal is to transmit the culture's cognitive elements. Glaring cut-off problems appear at once, however. The school system does not exhaust the society's educational system; the family, churches, industries, government, peer groups, etc., all educate. In analyzing concrete problems, however, we generally select an aggregate of roles and organizations with educational primacy and ignore the rest. (1959, pp. 22-23)

It is evident from Smelser's observations that the empirical classification of institutions and organizations which can be made by invoking analytical distinctions between their goal primacies (or principal functions) is seldom isomorphic to the total array of institutions and organizations engaged in the analytical function which serves as the basis for our classificatory scheme. The reason for this is simple enough: there are few functional monopolies. Furthermore, most extant institutions and organizations are functionally diversified, if only in a modest way. Thus, if we want to develop an empirical classification of institutions which parallels an analytical typology of social functions, we must necessarily draw arbitrary boundaries about empirical systems, using, following Smelser's suggestion, the functional primacy of institutions as a major classificatory principle.

The English economist, Richard Stone, arrives at much the same conclusion, though with only modest traces of functionalism. As he elegantly puts the matter,

> Just as in social accounting we have to decide what activities to include in the concept of production, so in the present case we must decide what activities

> to include in the concept of education. In the first case we use the idea of a production boundary which is notionally drawn around the activities to be included. Not all countries draw this boundary in the same place. The most restrictive boundary is used by the socialist countries which employ the material products concept. This boundary is drawn around activities concerned with the production and handling of goods and excludes a number of service activities such as government services and passenger transport. The boundary used in the system of national accounts drawn up by the United Nations includes these other service activities but excludes the unpaid services of household members and amateurs. The reasons for drawing the boundary in any particular place, and the conventions and imputations needed to obtain a manageable system, need not concern us here. The point is that a precise boundary must be drawn; it does not exist in nature. (1966, p. 106)

What guidelines are to be followed in drawing boundaries? Stone's discussion of the educational system reveals some of the considerations which must occupy an investigator's attention. He notes,

> A similar problem arises in defining the educational system. As with the economy at large, it is convenient to draw the educational boundary so as to exclude the educative effects of family members on one another; these may be regarded as a part of life in general for which we do not try to account. We are left, then, with various institutionalized forms of education, mainly taking place in schools, colleges and universities. As in the case of social accounting, family activities are excluded not because they are unimportant but because they are not the subject of general policy decisions and because they are virtually unrecorded.
>
> The identification of the educational system with the activities of schools, colleges and universities is not, however, satisfactory: in one way it is, perhaps, too broad; in another way it is too narrow. If we begin at the younger end of the age distribution we find nursery schools and kindergarten whose function is in large part to release the mother from the constant minding of small children. These activities, like

> family activities, are important, but for similar reasons it does not seem necessary in the first instance to introduce them into an educational model, though eventually they will be needed in connection with the demand for teachers.
>
> At the older end of the age distribution students may become members of industrial firms or professional bodies which provide training for the qualifications needed to practice specific skills. After apprenticeship, an individual may be recognized as a qualified carpenter or fitter; after passing a professional examination he may be recognized as a qualified lawyer or accountant. Since one of the purposes of the model is to link educational qualifications with the skills required in the economy, it is desirable that all these forms of further training and retraining should be included among the educational processes. (1966, pp. 106-107)

Buried in this passage are a number of significant and general considerations which one must consider when drawing boundaries about social processes.

First, one needs to keep the goals of one's own study in clear view. For example, a model of the welfare system devised for policy evaluation studies would almost surely exclude mutual aid between friends, neighbors, and relatives, since such social exchanges are not subject to policy manipulation, at least in our society. However, a model of the welfare system designed to project its likely case load would almost surely want to include exchanges of aid in extended social networks as part of the welfare system, since such exchanges evidently are one device which enables persons to avoid the stigma of being on the dole. Second, one needs to recognize that in drawing boundaries one is imposing artificial barriers between institutions and organizations which are, in fact, interlocking and interpenetrating in everyday life. One can capture part of these intersystemic linkages by drawing the boundaries broad enough to incorporate all forms of behavior relevant to the particular system of inputs and outputs under consideration. The boundaries of different institutional sectors may well be overlapping, at least until all the discrete models of the economy, the educational enterprise, the welfare system, etc., are drawn together into a single whole. Third, of course, one must always be attentive to the restrictions imposed by the available data. Drawing elegant boundaries across terrain which remains to be mapped will not take us very far in understanding the phenomenon at stake. This, of course, does not mean that an investigator must be saddled with available information and proceed despite its deficiencies. Rather it implies that the investigator must carefully consider whether the available materials are amenable to sensible boundary construction at all; if not, a case can be made for collecting novel indicators. There are, of course, guidelines to drawing boundaries beyond those set forth here, but they need not distract us further.

The upshot of the foregoing remarks is that the boundary problem is entirely an analytical, rather than an empirical problem. System boundaries are not where we find them, but where we make them. This is no less true of longitudinal studies than it is of cross-sectional ones. However, the temporal dimension of time-series analysis imposes a further problem in boundary definition which is not incurred in most cross-sectional investigations. We will discuss this point below, but first we must consider alternative designs of longitudinal studies.

3. SOME ALTERNATIVE PRINCIPLES FOR DESIGNING LONGITUDINAL RESEARCH

There are at least two sorts of longitudinal studies which one can imagine: (1) panel studies in which repeated measurements are made upon the very same units at two or more points in time and (2) replicative studies in which repeated measurements are made at two or more points in time upon different samples selected to be representative of the very same universe. The difference between these two types of inquiry rests upon whether the universe from which the observations are drawn is held constant, or whether the units upon which the observations are made are held constant. In panel studies, the units of observation are held constant; in replicative studies, the universe from which the observations are drawn is held constant. Generally, in panel studies, one buys the advantage of being able to study individual change at the price of a change in the definition of the universe from which the observations are drawn, while in replicative studies one secures the comparability of universes at the cost of being limited to the reporting only of aggregative changes.

The advantages of both panel and replicative designs can be secured and their disadvantages avoided by <u>augmented panel designs</u>. In this type of design, a sample of a particular universe at one point in time is followed as a panel at another point in time, but is augmented at the second point in time by a new sample designed to make it fully representative of the initial universe. For example, the basic universe for a study of electoral behavior is comprised of potentially eligible voters, i.e., persons who could vote if they chose to register and exercise their privilege to vote. A sample representative of the population of eligible voters at one point in time will not, of course, be representative of the population of eligible voters at some future point in time. There are several reasons for this, apart from the obvious ones of mortality and differential panel losses. Some persons eligible to vote at the first point in time may not be eligible to vote in second by virtue of changes in residence, emigration to another country, or failure to meet other legal requirements. More important, however, is the fact that in the second time period a number of persons not eligible to vote in the initial time period will be eligible to vote in the second by virtue of either acquiring citizenship or reaching voting age. Thus, if one follows through a <u>panel</u> of potentially eligible voters from one election to the next, it will not be representative of the universive of eligible voters in the second period. By augmenting the sample contained in the initial panel to cover new eligible voters and panel losses one can retain the advantages of both panel and replicative studies while avoiding the disadvantages of both.

The Current Population Survey of the U.S. Bureau of the Census combines elements of both replicative and panel studies. Basically, the Current Population Survey embodies a replicative design, so that for any cross-section it is representative of the universe of persons or households to which it refers. However, the sampling design for the Current Population Survey involves the utilization of so-called "rotation groups," segments of the total sample which are followed from month to month over an initial period, are subsequently dropped from the sample, and are then reinstated as part of the sample for a final period before leaving the sample entirely. Thus, the Current Population Survey, embodies both the elements of a panel and a replicative design. At this writing, little advantage has been taken of this feature of the design of the Current Population Survey, which continues to be analyzed largely as replicative investigation.

In passing, it may be worth noting that replicative and panel designs can be embedded in the same longitudinal study when different levels of observation are involved. The best illustration of this possibility known to the present writers is contained in the rather straightforward relication of a well known study of occupational prestige (NORC, 1947) by Hodge, Siegel and Rossi (1964). At one level the study by Hodge, Siegel, and Rossi is a purely replicative one: samples of different persons representative of comparable universies were drawn at two points time and asked to assess the general standing of various occupations. At another level, to wit, that of occupations, the study involves a panel design: the occupations evaluated (and the technique by which they were evaluated) are identical between the initial 1947 survey and its replication in 1963. Thus, this particular inquiry provides an example of a study which is replicative at one level, i.e., the universe of respondents from which the prestige ratings are solicited, while being a panel investigation at another level, i.e., the occupations evaluated by the sample of respondents drawn from comparable universes are unchanged. Since the universe of occupational pursuits in 1947 was rather different than in 1963, one may conclude that the panel feature of this study, <u>at the level of occupations</u>, was secured (even though the occupations evaluated were the same) by selecting rather different samples from the univserse of occupations which might conceivably have been evaluated in the two periods.

4. TIME AS A BOUNDARY PROBLEM

The analysis of time-series data requires the investigator to select not only a substantive domain of inquiry for which one can specify what is endogenous and what is exogenous, but also to select a time frame for the investigation. Thus, in time-series analysis the boundary problem is two-dimensional, requiring the imposition of boundaries upon empirical systems and the identification of the time frame within which the empirical system at stake is presumed to operate. The choice of empirical domains and the choice of appropriate time spans for their analysis are necessarily intertwined.

The interaction of one's choice of time periods and one's choice of substantive content is evident in many areas of inquiry. Consider, for example, the analysis of voter turnout and preference in the United States. Owing to the continuity of the American federal government since its founding, one might suppose that a time frame encompassing the history of the Republic since its founding would be appropriate. On closer scruting, however, one can identify a number of significant changes in American society which might well occasion qualitative changes in patterns of voter participation and preference. Among these potential watersheds one would surely count the following: (1) the westward expansion of the Republic and the gradual political development of these new territories into new states, (a) the Civil War and the demise of slavery, (3) the rise of immigration in the late 19th century and its dramatic decline in the 1920's, (4) the increasing role of federal government in most phases of local community life beginning especially with the New Deal and (5) the emergence of the United States as not only a world power, but a world policeman in the aftermath of World War II. In addition, there have been significant changes in the composition of the electorate itself over the history of the Republic, as the voting franchise was extended to non-property owners, to blacks, to women, and to 18 year olds, among others.

What do these changes imply for the analysis of voter participation and preference? Do they, for example, simply imply that one may take the entire history of the Republic as a time frame, but that one must extend the empirical domain of the model to include--at least as exogenous predictors--indicators of territorial expansion, the political development of new states, the extent and perhaps profitability of slavery, the volume of immigration, the size of the federal government, international commitments, and the scope of the franchise? If so, is not what began as a narrowly conceived investigation of voter participation and preference brought closer to a full model of the society as one begins to bring these massive social changes into the picture and seeks to understand why they do or do not affect voting behavior? Or, to the contrary, does not the mere presence of these large scale changes suggest that a time frame which encompasses the history of the Republic is inappropriate and that one's analysis should be restricted to historical periods of limited duration in which these changes are more nearly constant? But doesn't such a strategy invite one to miss the forest for the trees, since it holds forth no promise whatsoever of accounting for the transformations from one period to the next?

The answers to these questions are by no means easy and different investigators would doubtless come to contrary decisions about the choice of research strategies which lies beneath them. Empirically, we now know enough, however, to state unequivocally that what one finds will be conditioned by one's definition of the time frame. Owing to empirical work by Key (1955, 1959), MacRae and Meldrum (1960) and others (Burnham, 1970; Pomper, 1967; Sellers, 1965; Sundquist, 1973), there is ample evidence of major party realignments among significant voting groups during this century, let alone the entire history of the Republic. Such realignments of the voting patterns observed among social and economic groups are called critical elections. The pattern of party support observed among such groups as farmers, the urban proletariat, Catholics, blacks, and so forth shifts between critical elections. The existence of such voting realignments has profound implications for efforts to model voter participation and preference, for a model written over a period which includes a critical election may only wash out the effects of variables which are related to the vote in contrary ways on either side of the critical election. Evidently, a full model of voting behavior would involve an understanding of two things: the patterns of voting observed by social groups during the periods between critical elections and the forces which account for these realignments at a critical election. While it is not inconceivable that such effects could be incorporated into a single model, it is very clear that such a complete model would--at the very least--incorporate interactions (in the statistical sense) between the factors which explain the occurrence of critical elections and those which account for the voting behavior of socioeconomic groups between two critical elections. Without the presence of such interactions, there would be no way to incorporate the observed phenomena of shifting party allegiances among various social groups into a complete model of voting behavior. This circumstance is, of course, exacerbated by changes in the party system itself since the inception of the Republic.

Among sociopolitical phenomena, as opposed to economic ones, the records on aggregate voting preferences are relatively lengthy ones. It is largely for this reason that political scientists have been able to detect the phenomenon of critical elections. Left to work with the shorter period of the post-World War II era, there would be no hope of discovering the phenomenon of a critical election, for the simple reason that as best we can tell there has been no critical election within that time frame.

Unfortunate as it may be, most sociopolitical time series--excluding a few demographic ones which can be reconstructed from census data (e.g., Coale and Zelnick, 1963)--date only from the postwar period. Although periodic readings on such matters as the occupational distribution and church attendance can be found for the distant past, annual observations on these and most other sociopolitical phenomena are a product of the postwar period in which Karl Mannheim's vision (1950) of democratic planning has gradually surfaced. There is little question that one can take rather substantial steps toward building an annual, dynamic model of society on the basis of the sociopolitical and demographic time-series which are available for the United States and other societies in the postwar period. However, the experience of political scientists and sociologists in modelling the much longer run data on voter turnout and preference suggests that such an endeavor must proceed with caution. Critical elections have been identified as significant turning points in the voting behavior of signal socioeconomic groups. The available, annual time series on most sociopolitical phenomena are simply not long enough to identify turning points at which the underlying relationships in a short-run equilibrium are changed. One can, of course, go ahead--as, indeed, we are trying to do--and model the available data. One does so, however, only by benefit of the <u>gratuitous</u> assumption that the <u>same</u> short or long run state characterizes the society through the entire period of one's observations. In sum, modelling most sociopolitical time series engenders the considerable act of faith that one is not saddling the functional equivalent of critical elections in whatever substantive series one happens to be modelling. (The economists' solution to this difficulty is as follows: if the data agree with the theory, accept the latter; if the data do not agree with the theory, reject the former. That one branch of social science is largely pursuing data that agree with theories does not exclude the possibility that other branches of the same science might devote equal time to pursuing theories that agree with the data).

In practice, the time frame imposed upon estimates of dynamic models is that circumscribed by the available data. These limits are often quite narrow, but nevertheless they often admit of appreciable judgement on the part of the investigator. The Klein-Goldberger model of the American economy (Klein and Goldberger, 1955; Goldberger, 1959) is, for example, defined over the period 1929 through 1950 or 1952, deleting the years 1942 through 1945. War is a perturbation to an economy and it is not reasonable to think that a model of a peacetime economy will hold for a wartime economy. Were it not for Keynes' brilliant prognosis of World War II (1920), one would like to assert that there are some exogenous variables which are truly random and, therefore, beyond prognostication.

The basic idea incorporated in the Klein-Goldberger model that war perturbs the economy and that war years should, therefore, be excluded from data entering into estimates of its basic parameters is a congenial one. War is not peace. But what is war? Does the Korean Conflict count? What happens during the postwar adjustment to the economy? The Klein-Goldberger model encompasses the immediate postwar period between 1946 and 1948. Is the movement of the economy in the aftermath of a major war governed by the same parameters as those which governed it before that war?

We have no answers to these questions, which are serious ones. We do, however, have some illustrative findings which suggest that how one chooses to answer them will dramatically affect one's conclusions about the relationships between variables. (Nothing that follows should be interpreted as a criticism of the Klein-Goldberger model; we are certain they had good reasons for estimating their model over the period chosen; our purpose herein is solely that of demonstrating that one's choice of period can influence one's conclusions). Figure 4.1 shows the relationship between the percentage change from year to year between 1946 and 1970 in the number of employed persons in the major occupation group "craftsmen, foremen, and kindred workers" and the corresponding percentage changes in the total numbers of military personnel on active duty. Over the whole period, as one can see from the scatterplot, the relationship between the annual growth rate in military personnel and the annual growth rate in the numbers of craftsmen and related workers is practically zero, the correlation over the full period being .0571. The year 1960 is deleted from this and all further calculations involving these variables because the data base for reporting occupational data changed between 1959 and 1960 by the inclusion of Alaska and Hawaii and by the exclusion of the fourteen and fifteen year olds; the analysis cannot be extended beyond 1970 owing to major changes in the detailed job content of the major occupational groups. (See Bregger, 1971, for a discussion of changes in the definitions of major occupational groups)

Insert Figure 4.1 about here

As one can see by inspection of Figure 4.1, the year 1951, which was marked by the buildup of the armed forces for the Korean conflict, is an extreme outlier. Deleting this and the other Korean War years of 1952 and 1953, we find the association between the percentage change in military personnel and the percentage change in the numbers of craftsmen becomes both negative and substantial, its value resting at -.6030. This is the relationship we expected to find between these variables, on the grounds that expansion of the military disproportionately draws upon skilled and semi-skilled workers and upon young men--

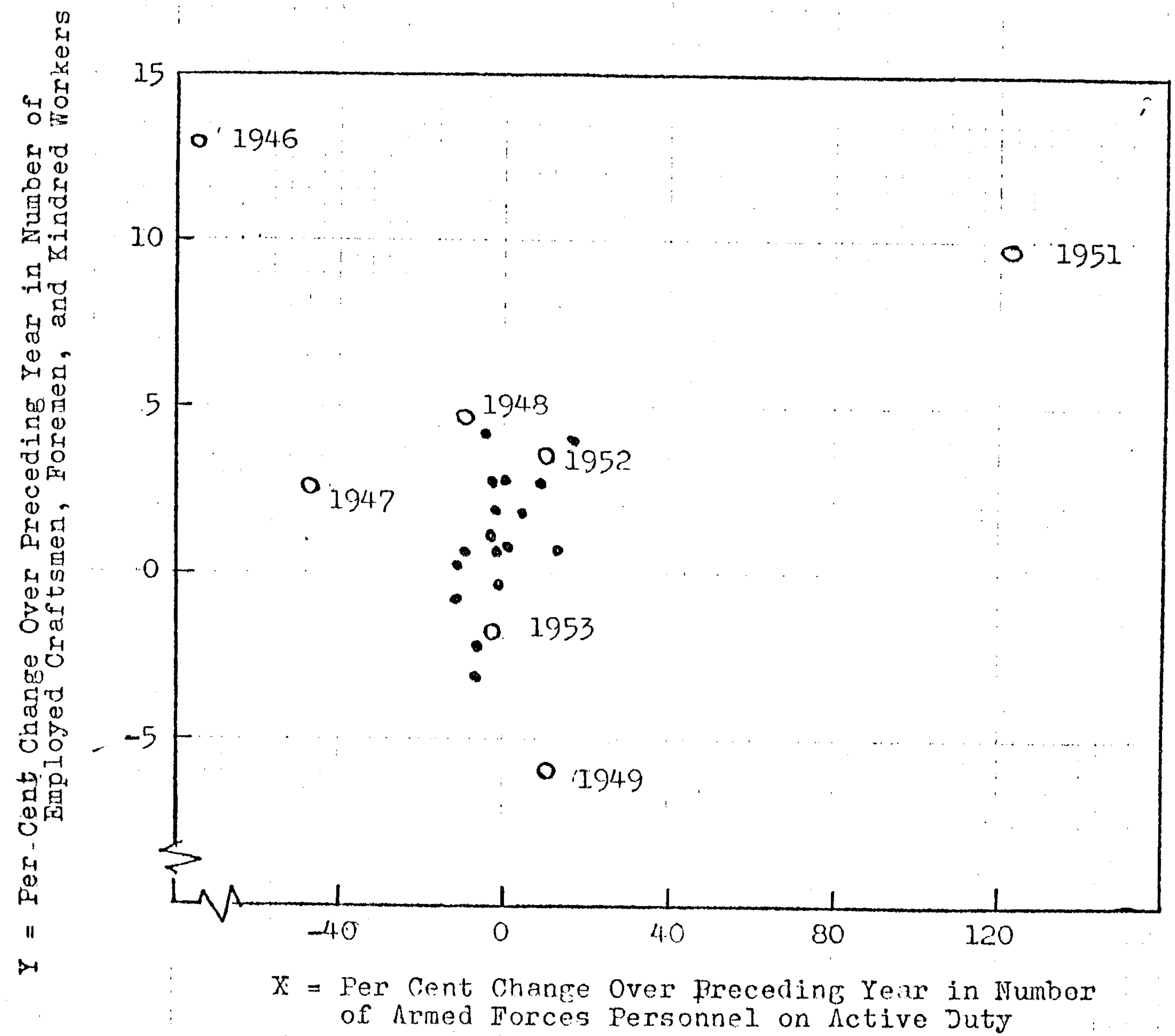

FIG. 4.1.--RELATIONSHIP BETWEEN ANNUAL CHANGES IN THE SIZE OF THE MILITARY AND THE NUMBER OF CRAFTSMEN, FOREMEN, AND KINDRED WORKERS, UNITED STATES, 1946-1959 AND 1961-1970.

unable to receive educational and occupational deferments--who would otherwise have entered these lines of work. The inverse relationship between military expansion and the growth of skilled tradesmen and foremen is, however, rooted in the behavior of the immediate postwar period. If, in addition to the Korean War years, we delete the observations for the postwar period 1946 through 1949, we find the association becomes a modestly positive .4688. Evidently, here is a clear case in which what one finds depends upon one's time frame. The relationship is nil when one studies the whole period for which data are available, negative when one deletes the Korean War period, and positive when one deletes both the Korean War years and the period of postwar adjustment. The deletions are plausible ones, rather than being arbitrary exclusions of miscellaneous years to make the data yield contrary conclusions. Indeed, one might well have specified the pattern of deletions a priori. (The sources of the data for this and all subsequent empirical illustrations in this paper are given in the appendix).

Although the example at hand was contrived to provide a clear example of how the selection of alternative, but plausible time frames can lead to quite different conclusions, we could produce additional ones by the carload lot. During the course of our own substantive work, data have become available for a few additional years. Our initial data sets ran largely from 1947 through 1970. In routinely updating our series through

1972, we often found that just adding the additional two years to our time period would often appreciably alter the estimated values of coefficients. We have also experimented with modelling a few longer time series both in the postwar period and over the full period for which they are available. Again it is not uncommon for coefficients to change their signs or move from significant to insignificant levels.

There is no general solution to the problem of selecting the temporal boundaries for one's study. Evidently, one is well advised to examine scatterplots and to estimate relationships over slightly different time periods. Obviously, one would have rather more confidence in those relationships which are most nearly identical when estimated from alternative sets of years. Unfortunately, our experience suggests that the equations one can write for many demographic and social phenomena which one would almost surely want to incorporate as endogenous variables in a model of American society can prove wildly different when estimated over time periods of very modest difference in length.

5. SOME RECURRENT PROBLEMS IN ASSEMBLING TIME SERIES ON SOCIAL INDICATORS

Quite apart from statistical complications introduced by the analysis of time series, the sheer activity of assembling the data file may prove to be highly problematic, with serious consequences for the analysis stage. Typically, the data matrix consists of repeated observations on the variables of interest across equally spaced intervals of time. Not infrequently, values for a series will not be available for the full period of interest. In some instances, a series will be reported only after the period under consideration is well advanced. For instance, there are no yearly data on vacancy rates in total rental housing prior to 1956. This state of affairs may prove utterly devastating to someone interested in incorporating such an indicator in a model of marriage markets in the postwar era. Similar late-starting dates characterize other series, such as median income of families headed by black persons, incidence of new narcotic addicts, and extent of daily watching of television. Other types of series are inherently of a cyclical nature, and cannot be obtained for more frequent intervals. By far, the most notorious members of this category are the series relating to presidential elections, such as electoral turnout, campaign expenditures, direction of the vote, campaign interest and participation, etc. Series on non-presidential elections, are available on a cycle of shorter length. To be sure, one is always able to merge into the electoral file other indicators for the corresponding periods of time. However, the problem arises when attempting to merge electoral data into a file based on more frequent time intervals. The analyst must resort to some sort of interpolation (see, e.g. David, 1972), or suffer a considerable reduction in the number of observations. Clearly, the loss of observations due to either of the foregoing types of problems will automatically reduce the maximum number of predictors that may be jointly considered.

Equally vexing is the presence of series for which sporadic readings in non-contiguous periods are available, prior to uninterruped reports for the balance of the period. Two characteristic series in this category are Gallup's query on church attendance during the previous week, and reported median income of families headed by black persons. In these illustrative examples it is clear that the collecting agency decided to routinely report the series after indulging in an intermittent trial period. Sometimes the indicator is discontinued altogether. Such was the fate of the abortive series on the incidence of civilian marriage ceremonies, reported by Vital Statistics. In some instances, the reverse process may be observed. Thus, the U.S. Environmental Protection Agency (1973) has monitored levels of air pollutants since the late fifties. However, EPA also reports selected retrospective estimates for 1940 and 1950. There is little reason to assume that the intervening trends may be easily extrapolated from the observations a decade apart. Much of the same type of problem besets the inventory of surveys codified under Roper's massive volume, Survey Data for Trend Analysis (1975), which contains few yearly series. In all of these instances, gaps in the vector of observations may also truncate the period under analysis, and preclude the utilization of estimating techniques which resort to taking differences between contiguous observations or the consideration of leading or lagged values of the indicators. While these broken series may be readily analyzed by ordinary least squares (with some provision for handling the missing data), they will certainly prove intractable to the full range of econometric techniques. To be sure, several of these late starting or broken series are intrinsically interesting and ought to be exploited. Moreover, their continued reporting will eventually generate an unbroken series of considerable length.

Still another complicating source lies in series which were generated with considerable effort and ingenuity at one point in time, typically by academicians, and have not been updated since. Illustrative examples that readily come to mind include Coale and Zelnick's long-ranging estimates of fertility for whites (1963), Kuznets' extensive series on income shares (1953), the Feirabends' cross national series on domestic conflict indicators

(1965), and Singer and Small's monumental variety of series on international conflict (1972).

Quite often the series of interest will be reported for periods which are not fully comparable. Thus, labor force statistics refer to calendar years, much of governmental finance and employment data tend to be reported for fiscal years, educational data are generally summarized by school years, and congressional statistics are reported by legislative sessions. In some instances, government agencies report series which have been concocted from more than one of the type of series enumerated above. For instance, the Digest of Educational Statistics publishes a series on educational expenditures as a percent of the gross national product, mixing school year data (expenditures) with calendar year data (GNP). Sometimes these mixed types of data may be placed on a common footing, provided that one is able to disaggregate the observations into monthly or quarterly intervals. Whenever dealing with cost-related data, this procedure requires the prior step of deflating all prices into constant dollars. An alternative strategy for rendering most of these types of series more comparable involves the computation of two-year moving averages. For instance, given a series on the number of government employees since fiscal year 1940 on, the average of the observed values for fiscal years 1940 and 1941 furnishes the approximate equivalent for calendar year 1941, the corresponding average of fiscal years 1941 and 1942 generates the value for calendar year 1942, and so on.

A different sort of quandary is presented by the need to include data up to the most current period of time. Some series, such as those reported in the *Vital Statistics* volumes, lag notoriously far behind in their coverage. Although the *Monthly Vital Statistics* issues alleviate some of this gap, the investigator requiring more detailed statistics must normally await the appearance of the yearly report. A somewhat related problem facing the researcher requiring up-to-date series lies in obtaining preliminary estimates, which are subject to revision. A well known instance is the series on gross national product and its components, which is routinely revised for three consecutive years. These revisions often result in widely fluctuating values, which require continuous updating of the data file, usually including as well a whole range of subsidiary series normed by the gross national product. The reader may obtain a notion of the magnitude of these revisions by considering that prior to settling on the final estimate of the GNP for 1971 ($1054.9 billions), its successive previous estimates were $1055.4, $1050.4, and $1046.8. Of course, frequent revisions also tend to accompany non-economic series, such as the age structure of the population, which in turn depend on other revision-prone vital statistics.

Sometimes a series will be revised for a long time span in one stroke, falling short, however, of a total revision of the entire series. One case in point is the ex post revision of the series on employed persons by major occupation, which shifted its coverage in 1967 to persons 16 years and older. BLS published figures backdating the revised series to 1958. For previous years, however, only the original data for individuals 14 years and older are available. In some unfortunate instances, one may obtain different readings for the same period of time without any explanation for the discrepancy. Thus, two successive issues of the *Statistical Abstract of the United States* reveal that the average speed of motor vehicles in the highways during 1964 was 55.6 miles per hour, whereas later volumes of the *Abstract* disclose a corresponding speed of 55.9 miles per hour. Typically, the person collecting the data may not have noticed the discontinuity since, to continue with the 1964 example, one normally would move on to later issues of the *Abstract*, searching for post-1964 values. This type of error may be minimized by giving priority to summary tables for broad periods of time, by gathering the data starting with the most recent period, by carefully watching out for such discrepancies, and by contacting the reporting agency for clarification. The problems discussed in this section also may preclude various types of analyses as data are unavailable, or may contribute to increased unreliability of the estimated parameters due to measurement error. If at all possible, it is best to wait for the dust to settle down in the collecting agencies. The next best strategy consists of utilizing the most recent indicators available, resigning oneself to stoically monitoring the release of updated values to be incorporated in the data file for replication of the original analyses.

Finally, the researcher may be faced by changes in the operational definition of the indicator, which renders the series incomparable in some often unknown degree. At worst, such a redefinition spells the end of the series as a reliable indicator. One such example is the series on new housing units started, which was redefined in 1962 to include farm housing. As already noted, the series on employed persons by major occupation was also redefined by the exclusion of the fourteen and fifteen year olds since 1967. In addition, there have been various reshufflings of the components of the major occupations, altering their strict comparability over time. Examples need not be proliferated to extol the wisdom of assessing the reliability of the series prior to blindly coding the values. In the case of the occupational counts, changes of definition would not markedly alter the percentage distributions of the major occupations over time. However, year to year rates of change in the frequency of employed persons by major occupation would evince large discontinuities after each change of definition, such as the shift in age categories, the reclassification of the detailed occupations, and the inclusion of data for Alaska and Hawaii after their statehood. Although government agencies scrupulously report changes in their operational procedures, researchers do not invariably pay heed to such considerations.

Sometimes a series traditionally reported by one agency is taken over by another one. In some instances, such as the series on hospital beds reported by the American Medical Association (until 1953) and the American Hospitals Association (since 1946), the series are fairly compatible. Overlapping coverage permits an assessment of the reliability of these competing series. In other instances, the reliability of the series is compromised by differences in sampling, measurement techniques, and even reporting units. Typical instances would certainly include the series on domestic production of firearms for private sale (estimated retrospectively by the staff of the National Commission on the Causes and Prevention of Violence, and updated (for fiscal years instead) after a short hiatus by the Bureau of Tobacco, Alcohol and Firearms), the likelihood of expected changes in family income (variously handled by the Survey Research Center at the University of Michigan and the Bureau of the Census), and a variety of marriage and divorce statistics reported by *Vital Statistics*, reliant on a changing number of reporting states, which still excludes Nevada.

The predicaments treated thus far will most certainly plague the investigator attempting to operate under a long time span with a wide-ranging domain of indicators. Within more modest confines, one may escape from these problems altogether. In general, labor force or business statistics are widely available for the post-war era, even on a quarterly or monthly basis. Such cornucopia is hardly manifest by social statistics, which are not as closely monitored. Nonetheless, a breathtaking variety of social series are available for a sufficient period of time, certainly supportive of respectable studies. The problems codified above are unique to time series only in the sense that as the time horizon expands, the reliability and the availability of the indicators may be sharply reduced or negated. At one extreme, the researcher may be unable to implement the research design for lack of a sufficiently long and uninterrupted series of reliable indicators. At the other extreme, the researcher may be blessed by a complete data matrix for the entire period under consideration. In between, the reality principle operates and one must make pragmatic compromises in the selection of variables, time periods, and statistical techniques. It is evident that data-gathering and data-analysis are interdependent. Neither phase of the research cycle ought to be taken for granted or considered in isolation.

6. TRENDS AND CYCLES

A general source of confusion in the analysis of time series data rests on the distinction between trend and cycle. We can regard the observed temporal fluctuations in successive observations on a variable as having two components, one due to the monotonic upward or downward drift in the values of the variable and the other due to systematic and/or random oscillations about this general movement. Just as the variance in a single variable may be allocated (conceptually) between trend and cycle, the covariance between a pair of variables may be divided into components reflecting the covariation between the trends and cycles of the two variables.

Decomposition of the correlation between two time series is facilitated by inspection of Figure 6.1, which is a simple path diagram indicating how the total association between a pair of variables, X_1 and X_2, is built up from the associations between their constituent cycles, C_1 and C_2, and trends, T_1 and T_2. Each variable is, of course, completely determined by its cyclical and trend components, so that the equations

$$X_1 = p_{X_1C_1}C_1 + p_{X_1T_1}T_1 \quad \text{(Eq. 6.1a)}$$

and $$X_2 = p_{X_2C_2}C_2 + p_{X_2T_2}T_2 \quad \text{(Eq. 6.1b)}$$

are exact identities which hold for every observation on each variable. Assuming all the variables are expressed in standard form, we may arrive at the association between the two variables by simply multiplying the corresponding sides of the above equations, summing over the observations and dividing through by their numbers. This gives

Insert Figure 6.1 about here

$$r_{X_1X_2} = p_{X_1C_1} p_{X_2C_2} r_{C_1C_2} + p_{X_1T_1} p_{X_2T_2} r_{T_1T_2} + p_{X_1C_1} p_{X_2T_2} r_{C_1T_2} + p_{X_1T_1} p_{X_2C_2} r_{C_2T_1}, \qquad \text{(Eq.6.2)}$$

or, in words,

Total Association = Part Due to Correlated Cycles +
Part Due to Correlated Trends +
Part Due to Cross-Correlations Between the Cycles of One Variable and the Trends of the Other.

As this decomposition makes clear the causal linkages between two time-series may operate (1) across their cycles, (2) across their trends, and (3) between their cycles and trends. One is not *a priori* at liberty to dismiss any of these sources of covariation. A gross association between two time-series can conceal any combination of these sources of covariation, including cyclical and trend components of opposite sign.

The root problem in the decomposition of time-series correlations into cyclical and trend components derives from the very simple observation that the model displayed in Figure 6.1 is, to use the language of econometrics, underidentified and,

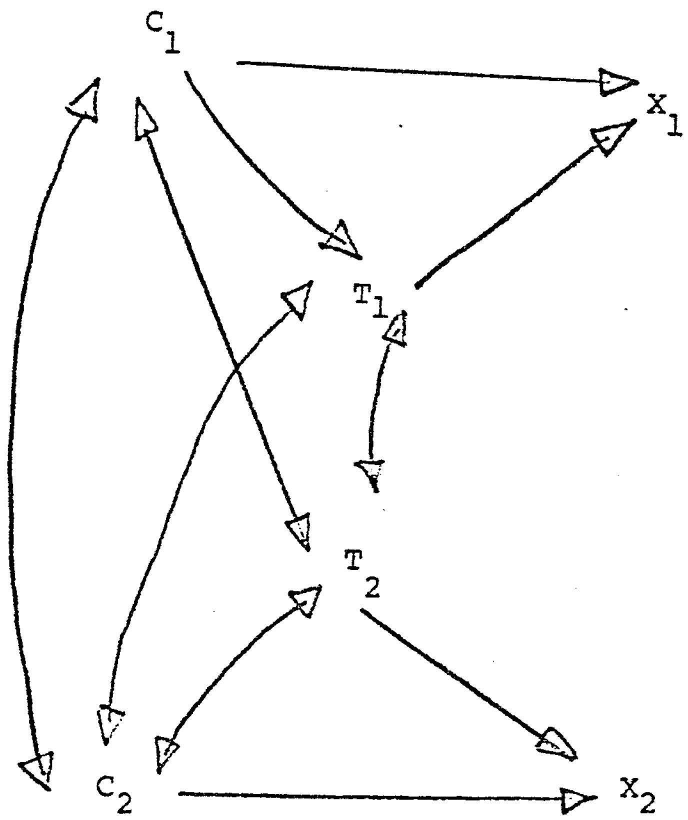

FIG. 6.1.--Path Diagram of the Sources of Association Between Two Time-Series.

one might, add, rather severely so. In all, there are ten parameters to the model given by Figure 6.1--4 paths decomposing each observed variable into its cyclical and trend components, 4 correlations involving a cyclical or trend component of one variable and a cylical or trend component of the other, and 2 associations representing the association between the trend and cyclical components of the two observed variables. To this situation, we bring just three pieces of information--the two identities of complete determination given by Eqs. 6.1a and 6.1b and the observed correlation between the two variables. Evidently, it is considerably easier to talk about the cyclical and trend components of time-series than to estimate their relative contributions to the variances of variables and the covariances between them.

The theoretical significance of the methodological distinction between trend and cycle can be appreciated by recourse to analogy. Suppose we thought of the evolutionary course of a society as being represented by an inclined plane in which there are a number of shallow and/or deep sinks, rather like a road which is going somewhere but upon which one's route is made circuitous by recurring chuckholes (short run cycles) and detours (long run cycles). Pursuing this imagery, one can regard a society as an energized ball which moves across the surface of this plane. Whenever it falls into a sink, short run equilibrium is achieved, but a considerable amount of cyclical fluctuation may be observed as it rolls back and forth within the sink. Forces within the society itself or exogenous shocks may move it from one sink to another and trends will be evident if the movement from one sink to another follows an orderly progression up, down, or across the plane. The factors which govern the behavior of a society within a sink are not necessarily the same as those which govern its movement between them.

The analysis of time-series correlations involves, as we have already seen, simultaneous analysis of both trends and cycles. There is no way the analyst can be certain that models derived from such data rest primarily upon associated trends or associated cycles, whether they are describing the movement of a society within sinks or between sinks on its evolutionary course. Quite apart from the obvious advantage of knowing whether one is modelling trends or cycles, a separation of these two phenomena is of considerable methodological significance. While the trends exhibited by different variables may assume quite different forms, their monotonicity alone assures that the trends in different variables will be very highly correlated. The resulting multicollinearity in the trend components means that it is virtually impossible to effect a clear resolution of the causal forces operating in these trends, for any variable whose variation is largely attributable to its trend may be replaced in a model by any other variable whose variation is also substantially due to its trend without any appreciable loss of information. If the two variables in question are associated with each other and with the dependent variable largely because of their trend components, one has scant hope of securing stable estimates of their coefficients since one is, in effect, attempting to estimate their separate effects from a single piece of information, viz. the effect of their single common factor represented by their joint trend.

The upshot of this situation is that the analyst has a considerable amount of leeway for his theoretical predispositions when working with variables whose primary source of covariation is their trend. Competing models are unlikely to perform very differently and forcing all the potential causal factors into the model often yields implausible parameter estimates. Consequently, what stays in and out of a particular equation in the model is often at the discretion of the investigator, for it is often the case that several competing variables do equally well when left in an equation by themselves. One needs a very strong theoretical rationale to defend a model which captures a trend and only a trend, for it will almost certainly have some plausible competitors. Not the least of these competitors will be rather simple models which ignore the causal linkages between variables by expressing every variable only as function of time and/or its previous values.

One standard resolution to this situation, where variables are associated primarily because of their trends, is to effect a decomposition of their cyclical and trend components, remove the variation attributable to the trend, and work only with the residual or cyclical variation. While this procedure is attractive on some counts, it also suffers from some serious drawbacks. First, as we can infer from Figure 6.1, it is not at all apparent how to remove the trend from a variable and, indeed, this can be accomplished only by invoking some very restrictive and dubious assumptions. Second, and what seems to us more important, removing the trend from a variable stands a fair chance of throwing away the baby with the bath water. Causal forces are operative in trends, as well as in cycles, and removing the trend may in fact succeed only in ignoring the primary causal forces at work between variables. Picking out the agents of causation from trends is, of course, difficult if not just plain impossible, but at this juncture we doubt if we can afford not trying to do so. Most of our own work with time series stands or falls on the theory, however primitive, which informs it, for our efforts to remove trends have typically been rewarded with nonsense correlations.

7. REMOVING THE LINEAR TREND

The problem of identification encountered in the general model given by Figure 6.1 is dissipated in the special case where we restrict the analysis (and/or definition) of trends to their <u>linear</u> component. Since the linear trends in different variables are perfectly correlated, T_1 and T_2 in Figure 6.1 may be replaced by a single variable T which represents the linear trend and simply takes on values equivalent to any linear transformation of the dates at which the observations on X_1 and X_2 were made. We must also now replace the cyclical components C_1 and C_2 in Figure 6.1 with the new variables C_1' and C_2' which include not only cyclical sources of variation, but any nonlinear components of the trends in X_1 and X_2.

Conceptually, the cyclical and trend components of a variable can themselves be correlated. The most obvious case of this circumstance is the one in which the period spanned by one's observations fails to contain a full cyclical movement, capturing instead only an upswing or downswing which will, of course, prove correlated with whatever secular trend the variable is exhibiting through the period. For this reason one has to allow in the general model given by Figure 6.1 for the possibility that the cycles and trends of X_1 and X_2 are intercorrelated. However, once we restrict ourselves to the examination of only the linear component of the trend in a variable and group any nonlinear trends together with the cyclical fluctuations, we have defined out that portion of the trend which is by definition orthogonal to any residual variation, viz., to the sum of the variation due to cyclical movements and nonlinear trends. Thus, while we must allow for C_1 and T_1 and for C_2 and T_2 to be correlated in Figure 6.1, we know that C_1' and C_2' are constructed so as to be orthogonal to T. The complicated and underidentified model given by Figure 6.1 devolves into a special case of partial correlation once we specify the trends as linear and identify the cyclical movements with the residual variation about those linear trends. This model and its general solution are given in Figure 7.1.

Insert Figure 7.1 about here

Since associations between trends and cycles must arise from associations between cycles and nonlinear components of trends, all association between cycle and trend is buried in Figure 7.1 into the association between C'_1 and C'_2 which is built up from both associated cycles and associated nonlinear trends. We are left, then, with a decomposition of the association between two time-series into only two parts--that due to their linear trends and that due to everything else. Explicitly, we have

$$r_{X_1X_2} = r_{X_1T}r_{X_2T} + (1 - r^2_{X_1T})^{1/2} r_{C'_1C'_2} (1 - r^2_{X_2T})^{1/2}, \quad \text{(Eq.7.1)}$$

where the first term on the right hand side of the equation is that part of the total association attributable to linear trends and the second term is the residue attributed to associated cycles and nonlinear trends. This decomposition of a time-series association can always be effected,since it surmounts the problem of identification in the more complete model of Figure 6.1 by defining the trends as linear.

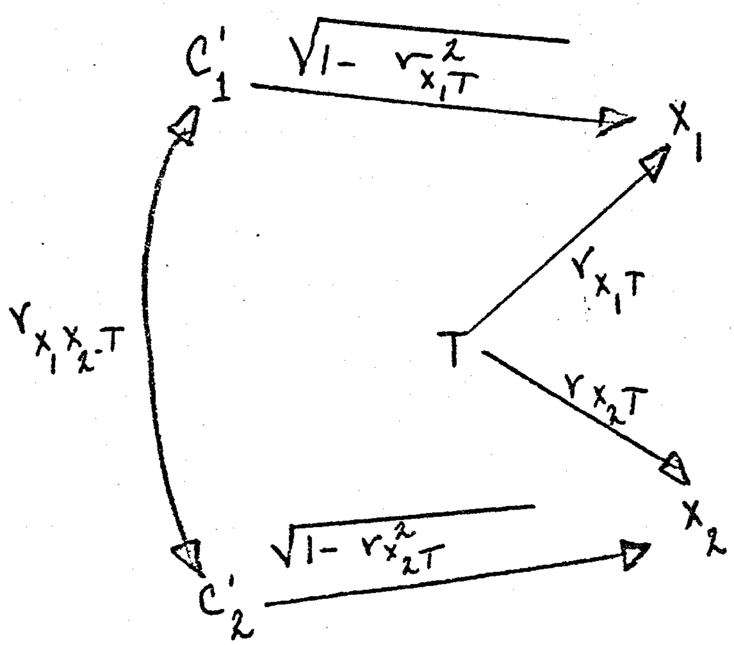

FIG. 7.1.-- Path Diagram Implied by Removal of Linear Trends from Time-Series.

8. AN EMPIRICAL ILLUSTRATION OF TREND REMOVAL

In many areas of substantive inquiry, time-series correlations prove very sensitive to trend removal. Often the variables are composed of trend and cylical components which are correlated in contrary ways, so that insignificant gross associations can prove significant when the trends of both variables are removed. Furthermore, the observed temporal variance of many variables is dominated by their trend component. In such circumstances, it is not unusual for the gross association between two variables to be significant in one direction owing to their linear trends, while their nonlinear components are significantly correlated in the opposite direction.

To provide an empirical illustration of what can happen in routine detrending of time-series, we have removed the linear trend from each of 12 variables included in a model of various facets of health with which we have been working. The variables in question, all measured annually over the period 1935-1972, are as follows: (1) the percentage of persons aged 65 and over, (2) educational expenditures expressed as a percentage of gross national product, (3) per capita gross national product in constant dollars, (4) the farm population expressed as a percentage of the total population, (5) automobiles per capita, (6) food consumption per capita, (7) hospital beds per 1000 persons (8) the percentage of births in hospitals, (9) the maternal mortality rate, (10) the neonatal mortality rate, (11) the postnatal mortality rate, and (12) the ratio of the number of medical school to nursing school graduates in each year.

There are a total of 12(12 - 1)/2 = 66 correlations between these 12 indicators. In Figure 8.1, we have simply plotted the gross correlation between each pair of variables against the association observed between the same pair of variables when the linear trend is removed from both of them. (The detrended correlations are, of course, equivalent to the partial correlation between original variables with time held constant). Points plotted to the left of the leftmost vertical line and to the right of the rightmost vertical line in Figure 8.1 represent detrended correlations which are significantly different from zero at the .01 level. Similarly, points plotted above the top horizontal line and below the bottom horizontal line represent gross associations which are significantly different from zero at the .01 level.

Insert Figure 8.1 about here

Casual inspection of Figure 8.1 reveals that there is very little relation between the gross and the detrended correlations observed between these 12 indicators. A few summary statistics will make the point clear. We observe that 76 percent of the gross associations are significantly different from zero at the

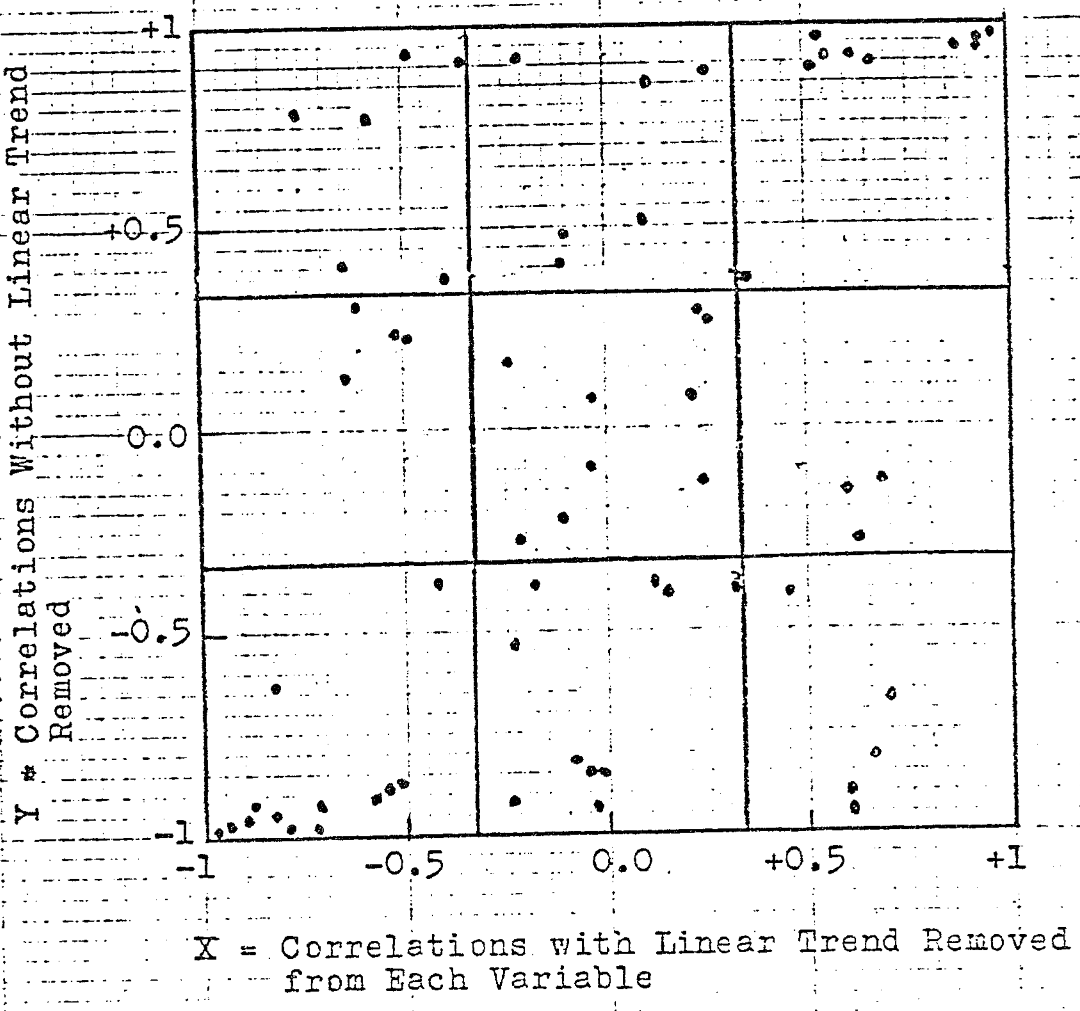

Fig. 8.1--Scatterplot of Correlations Observed Before and After Removal of Linear Trends, for Associations Between 12 Selected Indicators in a Health Model.

.01 level, while 62 percent of the detrended associations are significant at the same level. We also find that in 59 percent of the cases the gross and detrended associations are of the same sign, but in only 39 percent of the cases are the gross and detrended correlations both significantly different from zero and of the same sign. In 35 percent of the cases either the gross or the detrended correlation, but not both, is significant and in nearly one-fifth of the cases--17 percent--both the detrended and the gross associations are significantly different from zero and of opposite sign.

These results cannot, of course, be treated as representative of what happens when gross time-series associations are de-trended. Furthermore, one must keep in mind the fact that these observations are not independent of each other, since it is often the case that one variable is highly enough correlated with two other variables for one to infer the sign of the correlation between the latter two variables. Nevertheless, these results do illustrate just how sensitive time-series correlations can be to trend removal. It is rather apparent that what one would conclude about the relationships between these indicators depends to a very considerable extent upon whether one does or does not remove their linear trends.

We do not believe a general rule about when to remove and when not to remove trends can be given, since we are inclined to believe each situation must be evaluated on substantive particulars. The present illustration should suffice, however, to convince the reader that the failure to remove trends can leave one with associations which are built up largely from trends alone and that in the process of removing trends one can substantially alter the observed temporal covariation between variables. This, of course, is an obvious and well known point, but it is an obvious source of difficulty in constructing a macrosociological model from time-series data. This is particularly so in those situations where what one infers about the relationships between variables depends upon whether one leaves the trend in or out.

9. LAGS, LEADS, AND CAUSAL INFERENCES

One advantage with time-series analysis seemingly rests in the possibility of inferring causal relations via examination of the structure of lags and leads relating variables to each other. As it turns out, this advantage is more apparent than real. The difficulty, as was the case in the trend removal problem, turns on the problem of identification.

The basic situation is displayed in the form of a path diagram in Figure 9.1, which gives all the logically possible relationships between two variables X_t and Z_t and their lagged values X_{t-1} and Z_{t-1}. A priori, one cannot rule out the possibility of persistence, i.e., that the previous values of a variable are causes of its successive ones. This, for example, is evidently the case with many stock variables which are expanding or contracting at a fixed rate. Consequently, one must allow for $p_{x_t x_{t-1}}$ and $p_{z_t z_{t-1}}$. One also cannot rule out the possibility that the lagged values of one variable are potential causes of the contemporary values of the other, so one must also enter $p_{x_t z_{t-1}}$ and $p_{z_t x_{t-1}}$ into the diagram. Similarly, joint determination of the two variables within a time period cannot be ruled out, so one must predicate that both $p_{z_t x_t}$ and $p_{x_t z_t}$ are potentially non-zero. Once simultaneity enters the scenario, the prospect of correlated error enters the picture. Consequently, one must allow for $r_{\alpha\beta}$ to be non-zero.

Insert Figure 9.1. about here

The source of the difficulty is clear upon inspection of Figure 9.1. If we count up the number of paths in the diagram, we find there are exactly 10. To this situation we bring but 8 pieces of information--the (4) (4 - 1)/2 = 6 correlations between the variables and the two assumptions of complete determination in X_t and Z_t. Thus, this model is formally underidentified. A priori, there simply is no way one can manipulate the observed correlations between two time series and their lagged values to uncover the causal relations between them. Although Figure 9.1 has no general solution, in any particular substantive situation one may bring about a resolution of the identification problem which plagues it by (1) judicious equation of some possible effects to zero, (2) judicious assumption of the equivalence of some possible effects (usually $p_{x_t z_t} = p_{z_t x_t}$), or (3) selective introduction of additional variables, some of whose effects can be set to zero, and/or (4) some combination of the foregoing strategies. It is only by the introduction of such causal assumptions that one can proceed to estimation of the remaining effects. Needless to say, no single set of causal assumptions will be plausible in every case and those made in each particular instance would have to be evaluated upon their

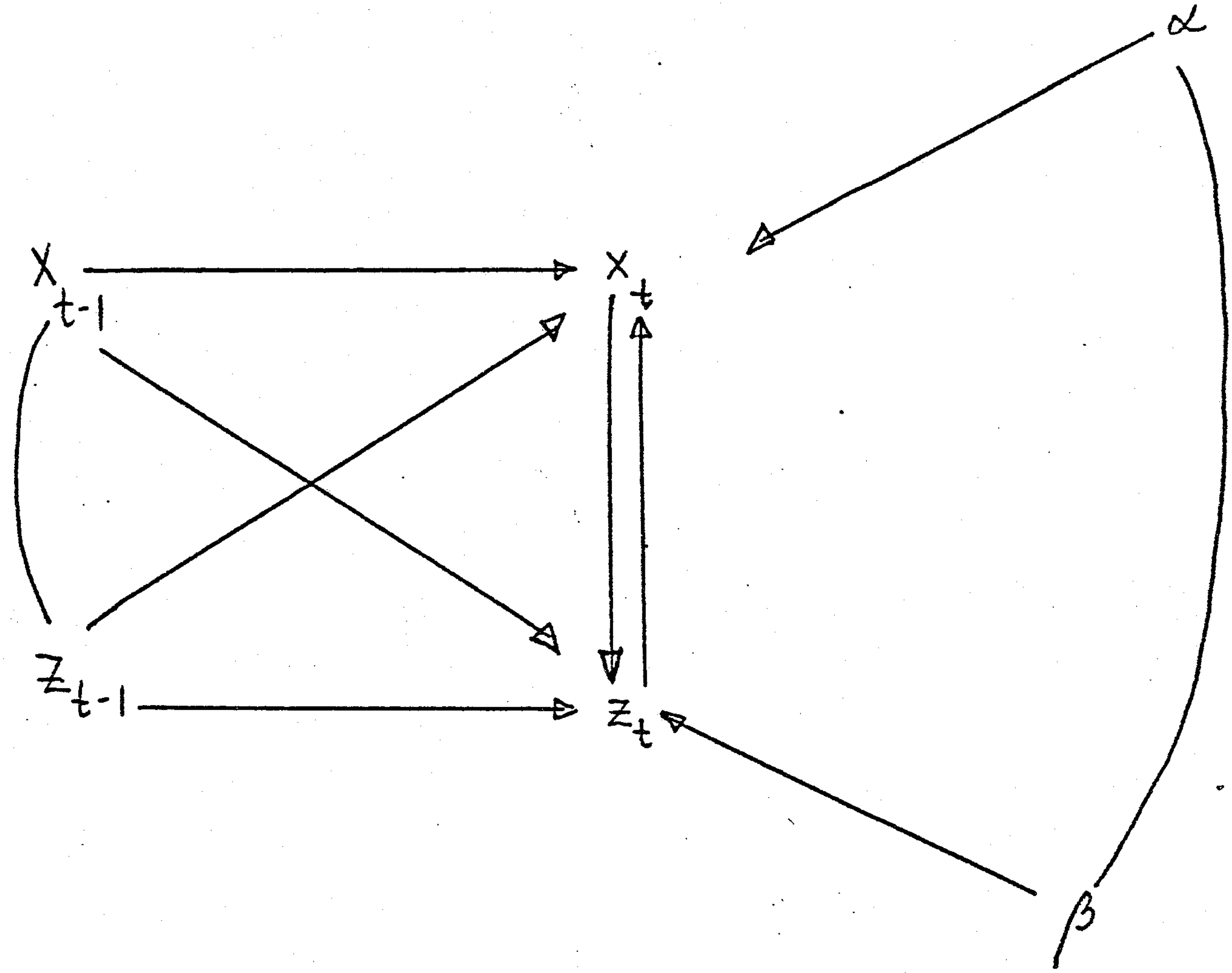

FIG. 9.1 -- LOGICALLY POSSIBLE CAUSAL RELATIONS BETWEEN THE CONTEMPORARY AND LAGGED VALUES OF TWO VARIABLES.

substantive and theoretical plausibility in that situation.

Perhaps the best known substantive problem which has the structure given by Figure 9.1 concerns the relationship between the unemployment and labor force participation rates. On the one hand, the availability of jobs in a growing economy will stimulate potential workers to enter the labor market and seek work, thereby raising the short run unemployment rate. This phenomenon, for example, appears to be partially responsible for the currently high level of unemployment. On the other hand, according to the discouraged worker hypothesis, a high level of unemployment will promote a reduced level of labor force participation for the simple reason that it increases the time-money costs of job seeking. Thus, the relationship between unemployment levels and labor force participation has a structure like that displayed in Figure 9.1 and one could not, studying these two variables only, plausibly equate any of the effects in the model to zero. In other cases, however, one can make plausible identifying assumptions and proceed to estimation. Consider, for example, the relationship between fertility and the labor force participation rate of women. Last year's labor force participation rate for women may well be implicated in this year's fertility level, just as last year's birth rate may be a factor in this year's level of labor force participation among women. However, there is no plausible way in which this year's labor force participation level among women could plausibly affect this year's fertility level owing to the lag between conception and giving birth. Thus, in this case, identification in the model given by Figure 9.1 is achieved because the simultaneity between X_t and Z_t, which must be *a priori* be postulated, makes little substantive sense. These examples, we hope, should suffice to illustrate why the model given by Figure 9.1 may or may not devolve to an identified system of equations, depending upon the particular substantive variables involved.

Although we have discussed the model given by Figure 9.1 in the context of two lagged and unlagged time series, we should note that its formal structure is exactly the same as that encountered in two wave, two variable panel designs. The coefficients $p_{x_t z_{t-1}}$ and $p_{z_t x_{t-1}}$ are analogous to the cross-lagged partials whose comparison is recommended by some (Pelz and Andrews, 1964) to ascertain causality in panel designs, the major difference being that $p_{x_t z_{t-1}}$ and $p_{z_t x_{t-1}}$ are regression rather than correlation coefficients. Reflection upon the remarks in this section should convince one that computation of cross-lagged partials in panel designs is, in general, a misleading procedure for inferring causality (cf., Duncan, 1969, 1975).

10. ON THE LEAD TIME FOR THE MANIFESTATION OF CAUSAL EFFECTS

It is generally recognized that it may take some time for a causal force to work its way through a system. Once a causal agent has been set in motion, its effects may be manifested only at a substantially later date. Time series offer the opportunity to inspect the lag between the time a movement is observed in one series and the time it is reflected in another series. Often this is done by inspection of the troughs and peaks in associated series and it is not unusual to observe that the lags are themselves highly variable (see, e.g., Friedman, 1961). Since we ordinarily postulate in structural equation models an invariant period between the movement of one variable and its causal effect upon another, our models will err insofar as causal forces take differential lengths of time to work their way through a system under different state conditions.

Even when the assumption of uniformity in the time frame for causality is met, the investigator is faced with selecting the proper length of lag. Sometimes, of course, an appropriate lag can be specified *a priori*. This, for example, is the case with fertility, but such cases, in our experience, are the exception rather than the rule. In other cases, the effects of causal forces may be manifested anywhere along a continuum from instantaneous to delays of many years. On the surface, at least, it would appear that one could empirically resolve the problem of selecting the proper interval of causation by simply trying out alternative lags and choosing that which yields the best fit. Unfortunately, things do not always work out so neatly.

We can illustrate the sort of difficulties one may encounter when resorting to empirical determination of the interval of causation by reference to the relationship between suicide and the business cycle. There is, of course, a long and established tradition of sociological research on the relationship between economic cycles, suicide, and other indicators of anomie (see, e.g., Durkheim, 1951; Halbwachs, 1930; Henry and Short, 1955; Pierce, 1967). With the exception of Henry and Short, few writers have paid much attention to the problem of specifying the proper time lag between economic fluctuations and movements in the suicide rate. There are reasons to believe the effects are far from instantaneous, for, barring a total collapse of the economy, it is reasonable to surmise that it would take a period of months and quite possibly longer for the consequences of an economic boom or bust to be manifested in personal lives and life styles.

We investigated various lags and leads between the age-standardized suicide rate and the NBER composite index of five coincident business cycle indicators over the period 1947-1972. The linear trend--substantial in the case of the business cycle measure and negligible for the suicide rate--was removed from both series. The results are displayed in Table 10.1, where it can be seen that the correlation between suicide and the business cycle monotonically decreases the further the business cycle is lagged behind the suicide rate. The expected negative relationship appears only after a four year lag is allowed. Thus, taking these data at face value, there appears to be little short run response of the suicide rate to economic fluctuations. Only after very substantial delays do booms and busts show up in corresponding, inverse fluctuations in the suicide rate. Needless to say, one should take a very skeptical view of these findings, since it strikes us as very implausible that the interval of causation between economic fluctuations and the suicide rate should be as long as eight or more years.

INSERT TABLE 10.1 ABOUT HERE

Although we would not want to argue that the suicide rate as such influences business conditions, we have nevertheless also lagged the suicide rate behind the business cycle indicator in order to illustrate the problems one would encounter either in inferring causality from the strength of relationships or in attempting to derive the interval of causation solely from empirical observations. Inspection of Table 10.1 reveals that, through lags up to and including six years, the suicide rate is more closely connected with subsequent business fluctuations than vice versa.

Post hoc interpretation of these findings is not difficult, once one interprets the suicide rate as an indicator of anomie. The basic explanation would go roughly as follows. During periods of disorganization characterized by personal anomie, individuals tend to suspend their consumption schedules until their personal lives and their surrounding social environment return to a higher level of stability and equanimity. However, once stabilization is achieved, individuals also tend to make up for "lost" consumption,

TABLE 10.1.--CORRELATIONS BETWEEN DETRENDED AGE-STANDARDIZED SUICIDE RATES AND DETRENDED NBER COMPOSITE BUSINESS CYCLE INDICATOR, FOR ALTERNATIVE LAGS AND LEADS, UNITED STATES, 1947-1972

		Dependent Variable	
Lag Length	Period	Business	Suicide
0	1947-1972	.2825**	.2825**
1	1948-1972	.3896**	.1572
2	1949-1972	.4562**	.1192
3	1950-1972	.5485**	-.0001
4	1951-1972	.5688**	-.3872*
5	1952-1972	.5735**	-.5115**
6	1953-1972	.6032**	-.5268**
7	1954-1972	.4762**	-.5813**
8	1955-1972	.2388	-.6384**

* Correlation coefficient significantly different from zero at the .05 level with a one-tailed test

** Correlation coefficient more than twice its standard error creating a modest run on consumer durables and a small boom in the market. Thus, one finds a positive association between suicides and the business cycle when the latter is taken as the leading indicator.

Needless to say, we would not defend the foregoing post hoc interpretation. The results in this section are simply presented to illustrate the surprises one may find in time series data if one takes a strictly empirical approach to determining causality and causal intervals. In the present case, the data alone would lead one to conclude (1) the suicide rate has more influence over the business cycle than economic fluctuations do over suicides and (2) the appropriate lag between these series is approximately six years. Such conclusions do not strike us as tenable ones: the proposed lag is too long to be plausible and it is very difficult to swallow the idea that the suicide rate operates on economic cycles rather than vice versa. Once again we are led to the underlying theme of this essay, to wit, that letting the data speak for themselves is a poor excuse for a carefully constructed theory of the phenomena one is attempting to study.

The difficulties examined somewhat casually in this section and the foregoing ones on trends, cycles and the linear detrending of social time series can be coped with in the context of spectral analysis. A detailed exposition of this method goes well beyond the scope of the present essay. However, other, albeit formidable exegeses of univariate and multivariate spectral analysis are available for the interested reader. For sociologists, a useful introduction to the subject may be found in an essay by Mayer and Arney (1974), but anyone desiring to attain mastery of the technique will wish to turn to considerably more detailed treatments (e.g., Engle, 1976; Granger and Hatanaka, 1964; Jenkins and Watts, 1968; and Parzen, 1962). Here we can provide only a very rough sketch of the nature of spectral analysis.

Adjacent observations in many time series are themselves associated in the sense that the covariances between observations in the same series, separated by different lag lengths, are non-zero. The autocovariance function of a temporal stochastic process is simply the relationship between the autocovariance between successive observations in a time series and the length of the lag between them. The process (and the series) is stationary when the autocovariance between successive observations depends only upon the length of the lag between them and not upon the particular time period for which the autocovariance is estimated. For example, the monthly unemployment rate is more or less stationary, fluctuating around a value of about 4.6 percent for the period from 1948 through the first third of 1966 (see Nelson, 1973, p. 23). Other series with pronounced trends, such as the infant mortality rate or the percentage of the labor force engaged in professional, technical or kindred occupations, are evidently nonstationary, since the autocovariance observed between successive values of these series will depend not only upon the length of the lag between the observations, but also upon the time period through which the autocovariance is observed.

At the risk of considerable oversimplification, univariate spectral analysis can be described as the treatment of observed time series as representations of stationary stochastic processes. The main vehicle of the method is Fourier analysis of autocovariance functions, which involves representation of the latter as a sum of periodic (or cyclical) processes. Roughly speaking, bivariate spectral analysis involves decomposition of the relationship between two different stationary stochastic processes into the relationships which obtain between the periodic functions of which each is composed--accomplished by analysis of the covariances observed at different lag lengths between the paired series. Multivariate spectral analysis is just a generalization of the bivariate case.

The assumption of stationarity seemingly limits the application of spectral analysis to social time series, many of which exhibit pronounced trends. However, in practice the stationarity assumption presents only modest difficulties. Most time series--even those exhibiting an appreciable trend--can be transformed or "filtered", to use the lingo of spectral analysis, into a series which will approximate stationarity. Common filters involve nothing more than taking first differences of the initial series or taking first differences of the log transform of the initial series (see, for example, Jenkins and Watts, 1968, pp. 6-8, and Nelson, 1973, pp. 177ff).

A rather more severe limitation to the application of spectral analysis to sociopolitical time series are the data requirements. Spectral analysis requires a time series of substantial length, typically in excess of 100 (equally spaced) data points (see, for example, Mayer and Arney, 1974, pp. 345-346). While a number of economic series are of sufficient length to sustain spectral analysis (see, e.g., Granger and Hatanaka, 1964), few demographic and sociopolitical series span a comparable time horizon. This is particularly true of the annual postwar time series which provide the empirical building blocks for a dynamic model of American society. Despite its promise of untangling the periodic components of stationary processes, spectral analysis at the present time is of limited use in analyzing the vast majority of social, demographic and political time series. Nevertheless, spectral analysis has been profitably applied to a few demographic and political series of substantial length. Singer and Small (1972, pp. 208ff), for example, retrieve evidence of 20 to 40 year cycles in amount of war underway at the international level from a spectral analysis of annual time series on conflict over the period 1816 to 1965. The pattern is even more pronounced when only conflicts involving the central international actors are considered over the shorter period 1816-1919. In addition, Lee's inspired analysis of the relationship between births and marriages (1975), using spectral methods, casts a long shadow over the wisdom of conventional specifications in demographic research of the relationship between fertility and economic conditions. While data limitations prohibit extensive use of spectral methods on most socioeconomic and sociopolitical time series at the present time, the applications already in hand suggest that the difficulties surveyed in this and the preceding two sections will be illuminated once the time frame of our observations is sufficient to allow the utilization of spectral analysis. Having studied a few forbidding volumes dealing with spectral techniques, we are happy to report that those developments are unlikely to occur in our lifetimes.

11. AUTOCORRELATION AND ALTERNATIVE ESTIMATING STRATEGIES

One of the best known difficulties encountered in the statistical analysis of time series is the problem of autocorrelation in the error terms associated with stochastic equations. Autocorrelation in the disturbance terms has three main consequences (cf. Johnston, 1972, p. 246). First, although the estimates of the regression coefficients obtained by ordinary least squares are unbiased in the presence of autocorrelation in the disturbance terms, their sampling variance may be unduly large relative to those which can be achieved with alternative estimating techniques. Second, and in our experience of particular importance, one is quite likely to underestimate substantially the sampling variance of the estimated regression coefficients by use of the formulae appropriate when ordinary least squares applies and autocorrelation is not present. Finally, use of equations estimated by ordinary least squares for purposes of prediction, when autocorrelation in the error terms is present, will have unnecessarily large sampling variances.

A full discussion of the problem of autocorrelation in the disturbance terms and alternative ways of dealing with it can be found in any standard econometrics text (e.g., Goldberger, 1964; Johnston, 1972; Kmenta, 1971). An excellent technical survey of this literature, as well as substantive applications drawn from the political science literature may also be found in Hibbs (1974). In the present context, we wish only to review the problem briefly so that the reader unfamiliar with these matters can grasp the basic idea behind one particular strategy used to cope with it that is substantively illustrated below. Suppose one wishes to estimate an equation with the general form

$$Y_t = k + \alpha X_t + \beta W_t + e_t, \qquad \text{(Eq. 11.1)}$$

where Y_t, X_t, and W_t are the observations on three time-series in the _t_th year, _k_ is a constant (intercept), α and β are the coefficients associated with X_t and W_t, respectively, when they are taken as predictors of Y_t, and e_t is a randomly distributed disturbance term with mean zero. As an initial step one can proceed to estimate this equation with ordinary least squares, in which case the Durbin-Watson statistic (Durbin and Watson, 1950, 1951) may be utilized to test for the presence of first order autocorrelation between successive error terms. If first order autocorrelation of the error terms is present, then Eq. 11.1 must be written as

$$Y_t = k + \alpha X_t + \beta W_t + v_t + \rho e_{t-1}, \qquad \text{(Eq. 11.2)}$$

where $e_t = v_t + \rho e_{t-1}$, ρ is the correlation between successive error terms, e_{t-1} is the disturbance for year t-1, v_t is a randomly distributed component of e_t, and the remaining variables and parameters are as defined above. With autocorrelation present, the ordinary least squares estimates of the coefficients may have unduly large sampling variances and the use of ordinary least squares methods to estimate their sampling variances will likely underestimate them. Thus, we are in a situation where the use of ordinary least squares methods may, when we turn to statistical tests of the estimated coefficients, make significant ones look insignificant and insignificant ones appear significant.

A technique for dealing with first order autocorrelation has been proposed by Cochrane and Orcutt (1949). From Eq. 11.1, we observe that the observation on Y_t in year t-1 can be written as

$$Y_{t-1} = k + \alpha X_{t-1} + \beta W_{t-1} + e_{t-1}. \qquad \text{(Eq. 11.3)}$$

Multiplying this equation by ρ and subtracting from Eq. 11.2 leaves

$$Y_t - \rho Y_{t-1} = k(1 - \rho) + (X_t - \rho X_{t-1}) + (W_t - \rho W_{t-1}) + v_t. \qquad \text{(Eq. 11.4)}$$

The disturbance term v_t in Eq. 11.4 is now free from first order autocorrelation. Were an estimate of ρ available, one could construct the generalized first differences in Eq. 11.4 and proceed to estimate it by ordinary least squares. An estimate of ρ can be obtained from an iterative procedure. First, Eq. 11. 1 is estimated by ordinary least squares and estimates of the disturbance terms (the e_t's) are calculated. A preliminary estimate of ρ is then given by

$$\hat{\rho} = (\sum e_t e_{t-1})/(\sum e^2_{t-1}). \qquad \text{(Eq. 11.5)}$$

This provisional estimate of ρ is then used to create the generalized first differences in Eq. 11.4, which are in turn used to construct a second estimate of Eq. 11.1. The residuals are again calculated and a new estimate of ρ is derived from them. The procedure is repeated until the estimated value of ρ converges, the final estimate of ρ being used to estimate Eq. 11.4 via construction of the generalized first differences and use of ordinary least squares procedures to obtain the coefficients relating them.

The procedure sketched above can be generalized to handle higher order autocorrelation schemes. Other techniques for dealing with autocorrelated disturbances are also available, but we have used the one sketched above in our work by virtue of its availability in widely distributed computer programs for the analysis of time series. In the presence of autocorrelated error, parameter estimates and tests of significance obtained by the Cochrane-Orcutt method have statistical properties which make them superior to those given directly by application of ordinary least squares.

Although it is not evident from our discussion of the Cochrane-Orcutt method or, for that matter, from discussions of it and related techniques that appear in many econometrics texts, parameter estimates obtained by the Cochrane-Orcutt procedure can differ wildly from those obtained by ordinary least squares. We have often witnessed coefficients which are significant under ordinary least squares estimation become insignificant when the Cochrane-Orcutt method is used, and vice versa. We have also observed instances in which variables prove statistically significant when either method is used, but are of <u>different</u> sign. Obviously, such discrepant results can only occur because of autocorrelation in the disturbance terms, the methods being equivalent when successive disturbances are uncorrelated.

Differences of the sort noted above can and do occur even when the degree of autocorrelation is modest. The Durbin-Watson statistic often falls in an indecisive range, allowing one neither to reject the hypothesis of zero correlation between successive disturbance terms nor to reject the hypothesis that they are correlated. Turning to the Cochrane-Orcutt procedure in such instances can (as one would expect) produce an estimated value of autocorrelation between successive disturbances which may prove either significant or insignificant. However, even when the estimated autocorrelation does not differ significantly from zero, we have encountered cases in which the significance levels and even the signs of variables have changed. Just what one should do in such cases is not entirely clear. In practice one implicitly acts like a Bayesian by accepting the results which are substantively most sensible even though one has not spelled out the conditions which justify one's decision.

A substantive illustration of the differences which can occur when one adopts the alternative estimating strategies discussed above is in order. One variable which will obviously enter into even a partially complete model of American society is the marriage rate. Among the factors which are plausible candidates to affect the course of the marriage rate are job opportunities for women and economic conditions. In general, as job opportunities for women expand, more women will choose careers in preference to marriage and a family. In addition, hard times will occasion couples to postpone marriage until their economic horizons have improved. We have two measures of job opportunities for women: (1) a direct measure, the percentage of women in the labor force (F_t), and (2) an indirect measure, the number of armed forces personnel expressed as a percent of the total labor force (A_t). The latter measure is expected to reduce the marriage rate by two independent mechanisms. Since the bulk of the armed forces is comprised by male personnel, expansion of the military will open up jobs in the civilian sector for women, which they would otherwise have to compete for with their male counterparts. However, the expansion of the military will also decrease the opportunities for marriage among both sexes. We have both the percent unemployed (U_t) and the NBER coincident business cycle indicator (B_t) as measures of economic conditions. Taking marriages per thousand men aged 15 and over as the dependent variable and using ordinary least squares, we find through the period 1947-72 that

$$\hat{M}_t = \underset{(38.92)}{293.7} - \underset{(1.497)}{5.875}(F_t) - \underset{(1.138)}{4.483}(A_t) + \underset{(.1745)}{.5203}(B_t) - \underset{(1.152)}{2.197}(U_t). \qquad \text{(Eq. 11.6)}$$

Following the usual convention, the estimated standard errors of the coefficients are reported in parentheses beneath the estimates of their respective values. As the reader can see, the coefficients of each variable save the unemployment rate are several times larger than their standard errors. With a one-tail test, even the unemployment rate is obviously significant, since its coefficient is nearly twice its standard error. The signs of the variables are in the expected direction and the coefficient of determination associated with this equation is .8184, corrected for degrees of freedom. On the surface this looks like a plausible marriage equation: the coefficient of determination is perhaps a bit low, but all the variables have their expected signs and each is significant by conventional standards.

Our trouble begins with the Durbin-Watson statistic associated with the foregoing regression. Its value proves to be an indecisive 1.45, which does not enable us to reject the null hypothesis of no autocorrelation in the error terms. Thus, we turn to the Cochrane-Orcutt procedure, which yields

$$\hat{M}_t = \underset{(36.04)}{81.26} - \underset{(1.140)}{.3156}(F_t) - \underset{(.8094)}{.3371}(A_t) + \underset{(.1245)}{.1191}(B_t) - \underset{(.5417)}{.5530}(U_t), \quad \text{(Eq. 11.7)}$$

where the standard errors are reported beneath the coefficients. As the reader can see, the coefficients continue to retain the appropriate signs, but only one of them, save the constant term, even so much as exceeds the size of its standard error. For this equation, the estimated value of ρ is .8100 and almost seven times larger than its standard error. While this value of ρ may appear large to those who have not worked extensively with time series data, it is not astoundingly so. We frequently observe estimates of ρ in excess of .9 and .95 when working with socio-economic time series. Because the estimate of ρ is as high as it is, the coefficient of determination associated with the foregoing regression is also quite large, despite the fact that none of the exogenous predictors feature a significant coefficient. Its value is .9194, corrected for degrees of freedom.

What does one do in a situation like this, where the results obtained from two estimating strategies are markedly different with the appropriate estimating technique yielding substantive results which do not support one's initial ideas? The most obvious strategy is to retrieve as much of one's original conception as possible by pruning the equation of some of the predictor variables. Alternatively, one can proceed by expanding the predictor variables to cover areas not originally thought to be implicated in the determination of the endogenous variable at hand. Such work is tiresome and frustrating, particularly when one knows that an improperly estimated equation provides results which are consonant with one's theoretical preconceptions about how the society works. Even when such work is rewarded with a measure of success, one remains acutely aware that one is saddled with an equation describing some feature of society which fails to mirror one's vision of how the society works.

One should not conclude from the particular illustration provided herein that proper estimation, in situations where autocorrelated disturbances are present, is necessarily disastrous. Using ordinary least squares, Barth and Bennett (1975) reported evidence showing insignificant seasonal variation in the interest rate. Using the Cochrane-Orcutt method on one of the series studied by Barth and Bennett, Huang (1976) was able to detect significant monthly variations in the 90-day Treasury bill rate.

In this particular case, the estimated value of ρ was an astronomical .977. One should not ignore the problem of auto-correlation; if coping with it is frustrating on some occasions, it can also be revealing in others.

12. SOME NOTES ON SPECIFICATION BIAS

The generic problem common to all nonexperimental work is that of specification bias. One can never be certain that the effects one does observe are operative ones because there is no way of ruling out the prospect that they are washed out by causal forces one did not include in the analysis. This difficulty is eliminated from experimental work by the random assignment of subjects to control and experimental groups. Here we discuss the problem of specification by discussing the well known relationship between unemployment and inflation.

A fair number of people would agree that an economic utopia would be characterized by full employment and only modest, if any, inflation. The former ailment disproportionately afflicts younger and minority group workers, while the latter takes its toll among those living on fixed or nearly fixed incomes--pensioners, widowers, and middle income earners whose salaries are not directly tied to the consumer price index. Unfortunately, the combination of modest inflation and full employment remains an unrealized dream. While it would be presumptuous to claim it is impossible to attain these goals simultaneously, experience suggests it will be very difficult to do so. The reason for this is that full employment policies tend to be inflationary ones, while efforts to keep inflation under control tend to reduce consumer demand and thereby stimulate unemployment.

Everyone knows that prices and unemployment are inversely related; there is even considerable discussion of this relationship in the popular press, stimulated by the tendency of the major political parties to endorse alternative economic policies which place differential importance upon controlling inflation and creating full employment. The Republicans, who traditionally draw support from the aged and salaried income groups that are hard pressed by inflation, are more likely to think that a little unemployment is not too high a price to pay for modest inflation. For big spending northern Democrats, with their massive support from minority and low income groups who bear the burden of unemployment, full employment is the number one economic priority. Political rhetoric about economic goals is, of course, a far cry from an efficacious economic policy. Nevertheless, the performance of the postwar American economy between the years 1946 and 1972 was slightly different when there was a Republican than when there was a Democratic incumbent of the White House. Under Republican presidents, the average annual increase in the consumer price index was 2.5 percent, while under Democratic presidents it was 3.8 percent. The average annual total unemployment rate stood at 4.5 percent under the Democrats and 4.9 percent under the Republicans. Even if these differences, which are in the expected direction, reflect alternative economic policies of the two parties, the underlying data revealed that neither party exercised any appreciable control over the economy because both the rate of inflation and the level of unemployment fluctuated markedly under both Democratic and Republican leadership. The correlation between the unemployment rate and a dummy variable identifying a Democratic controlled White

House is -.1795; the same variable has a correlation of .1939 with year to year percentage changes in the price index. Evidently, there is more temporal variation in unemployment and inflation <u>within</u> the administrations of either party than there is <u>between</u> the administrations of the two parties.

The relationship between the consumer price index and the total unemployment rate through the period 1946 to 1972 is shown in Figure 12.1, where small line segments connect the points for adjacent years. As one can see by inspection of the figure, the price index drifts upward throughout this period, but it rises most rapidly through the end of the period when stagflation--inflation and high unemployment combined--was setting in. No relationship between the movement of the price index and fluctuations in the level of unemployment is readily apparent from the figure. Indeed, through the whole period the relationship between the two indicators is positive rather than negative, the correlation between them being a modest .2003.

Closer scrutiny of the figure reveals some evidence of the inverse relationship between unemployment and prices that one expects to find. During two periods, between 1961 and 1969 and between 1949 and 1953, there is a rather clear inverse, but curvilinear relationship between unemployment and prices.

Insert Figure 12.1 about here

Oddly enough, in both of these periods unemployment drops from a very high level to one well within the bounds of frictional

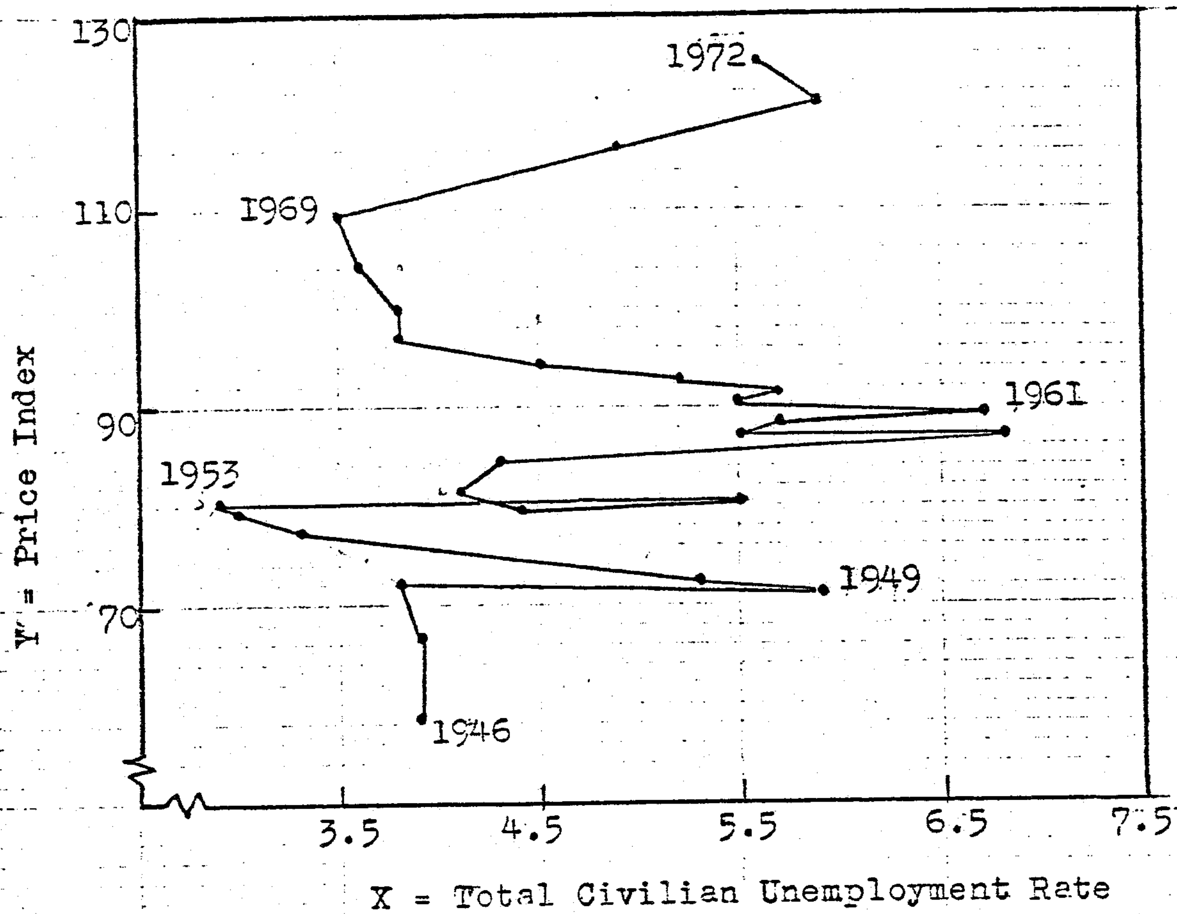

FIG. 12.1 --RELATIONSHIP BETWEEN THE PRICE INDEX AND THE UNEMPLOYMENT RATE, UNITED STATES, 1946-1972.

unemployment, i.e., the amount of unemployment engendered by imperfect knowledge of potential employees about vacant jobs and by imperfect knowledge of employers with openings about available candidates. These time periods also share two additional communalities: (1) they both encompass a foreign military adventure of appreciable scope--the Korean War in the first period and the Viet Nam War in the second--and (2) they are both periods in which the control of the White House was largely in Democratic hands. There is, then, a hint in Figure 1 that unravelling the relationship between unemployment and prices may require controlling for either the incumbency of the White House or for military activity.

Any further progress on the matter at hand requires specification of a model relating prices and unemployment. We begin by observing that prices in year _t_ are related to those in year _t_ - 1 by the following accounting identity:

$$P_t = (1 + r_t)P_{t-1}, \qquad \text{(Eq. 12.1)}$$

where r_t is the rate of inflation between years _t_ and _t_ - 1, P_t is the value of the consumer price index in year _t_, and P_{t-1} is its value in the preceding year. The form of this accounting identity suggests that an appropriate specification of a price equation might well take the multiplicative form given by

$$P_t = k(P_{t-1})^a(U_t)^b(D_t)^c(e_t), \qquad \text{(Eq. 12.2)}$$

where P_t and P_{t-1} are defined as before, U_t is the percent unemployed in the total civilian labor force, D_t is given by defense expenditures expressed as a percent of gross national product, e_t is an error term which is randomly distributed with mean zero in its logarithm, k is a constant of proportionality, and the elasticities of prices with respect to lagged prices and the exogenous variables U_t and D_t are given by a, b, and c respectively.

The interpretation of Eq. 12.2 is relatively straightforward. Recourse to the accounting identity given by Eq. 12.1 suggests that the elasticity of lagged prices with respect to current prices should be unity, i.e., a = 1. The coefficient of proportionality k can be interpreted by setting it equal to q(1 + r), where r is the implicit, equilibrium rate of inflation. Viewed in this way, the value of $q^{-1}(U_t)^b(D_t)^c$ may be interpreted as a factor determining the short run (i.e., annual) rate of inflation owing to the levels of unemployment and defense expenditures, q^{-1} being the appropriate constant of proportionality.

The multiplicative model specified by Eq. 12.2 is additive when expressed in logarithmic form. Taking natural logs on both sides of the equation, we find using ordinary least squares that

$$\widehat{\ln P_t} = \underset{(.1426)}{.2188} + \underset{(.0348)}{.9986}(\ln P_{t-1}) - \underset{(.0235)}{.0554}(\ln U_t) - \underset{(.0206)}{.0463}(\ln D_t), \qquad \text{(Eq. 12.3)}$$

where the standard errors of the coefficients are reported in parentheses beneath them, and the coefficient of determination associated with the regression is .9705, corrected for degrees of freedom. As the reader can see, all of the coefficients are more than twice their standard errors and the elasticity coefficient associated with unemployment now has the appropriate negative sign. The reader can also see that the elasticity coefficient associated with lagged prices is close to unity. In fact, it does not differ significantly from unity, since $1 - .9986 = .0014 < .0348$, the standard error of the estimated coefficient. In view of this observation, one is well advised to estimate Eq. 12.3 again, constraining the elasticity coefficient associated with lagged prices to unity.

A constrained estimate of elasticity coefficient of lagged prices is easily constructed. Setting a = 1 in Eq. 12.2, and dividing by P_{t-1}, and invoking the identity given by Eq. 12.1, we are left with

$$(P_t)/(P_{t-1}) = (1 + r_t) = k(U_t)^b(D_t)^c(e_t). \qquad \text{(Eq. 12.4)}$$

Taking natural logs on both sides and estimating by ordinary least squares, we find

$$\widehat{\ln (1 + r_t)} = \underset{(.0571)}{.2135} - \underset{(.0220)}{.0556}(\ln U_t) - \underset{(.0191)}{.0466}(\ln D_t) \qquad \text{(Eq. 12.5)}$$

which provides estimates of the elasticity coefficients of unemployment and defense expenditures which differ in no substantively important way from those already available when the elasticity coefficient of lagged prices was unconstrained. Since Eq. 12.5 is written in terms of annual differences in the log of prices, the coefficient of determination associated with it is small: .1443, corrected for degrees of freedom. However, the coefficients of $\ln U_t$

and ln D_t are still significant; all the low coefficient of determination means is that (in the context of this model) the main thing affecting this year's prices is last year's prices.

The results above straighten out the slight positive relation between unemployment and prices observed in Fig. 12.1, making the net relationship between them the inverse one expected. The inverse relationship between log prices and log defense expenditures appeared somewhat incongruous to us at first. However, the relationship between prices and defense expenditures must, in the final analysis, be a complex one. If high levels of defense expenditures are accompanied by increased taxes to cover them or even by the public's expectation that a temporarily inflated defense budget will ultimately be reflected in their payrolls, the resulting decrease in consumer demand will hold prices in check. Similarly, an implicit or explicit policy of wage and price controls during periods of increased defense expenditures or extended military engagements may serve to depress the rate of inflation.

Dropping defense expenditures from the foregoing estimates neither appreciably alters the magnitude nor changes the significance of the elasticity coefficient associated with unemployment. If we define a new variable $W_t = e$, if the Democrats control the White House, and $W_t = 1$, if the Republicans control it, then replacing D_t with W_t in the foregoing regressions still leaves the elasticity coefficient of unemployment significant and on the same order of magnitude. The coefficient of the political variable, W_t, which becomes an ordinary dummy variable on taking its natural logarithm, does not achieve significance--a relief to those of us who think that consistent policies and Washington policies, regardless of who is at the helm, are disjoint phenomena.

Although the foregoing results appear substantively plausible to us, we surely would not care to defend our specification of the price equation. In the first place, we are not economists and, in the second place, we are not interested in price economics. Undertaking this excursion into the postwar behavior of consumer prices was designed to illustrate a much more general point about the difficulties which will be incurred in developing a dynamic model of American society.

At this juncture the reader is invited to reconsider the relationship between prices and unemployment exhibited in Fig. 12.1. As best we can tell, there is no obvious one. If there is any relationship at all, it is apparently a positive one, as revealed by the weak zero order association between unemployment and prices reported above. Nonetheless, we were able to specify a model in which the expected inverse relationship between prices and unemployment held. Our ability to do so, despite the fact that neither of us has any pretensions to <u>savoir faire</u> in economics, is clearly rooted in a substantial body of economic theory and research which informed us of what we should be looking for.

Unfortunately, the variables which enter into a dynamic model of society are not so well researched as those which enter into models of the economy. Our ability to untangle the complex relationship between prices and unemployment exhibited by Figure 12.1 was rooted in a theory about what their relationship should be. But let us consider the variables which would enter into a model of society writ large, rather than the economy as such. These would surely include indicators of matters as diverse as church attendance, school enrollments, divorce, incidences of various criminal activities, suicides, fertility, the party composition of the federal legislature, urbanization, migration, and the ratio of private to public philanthropy, just to mention a few social phenomena for which there are plausible measures. There is no theoretical or research basis for untangling plots of the joint movement of most of these variables through time. Observing an essentially nil relationship, such as that exhibited in Figure 1 between prices and unemployment, between social variables of this sort invites one to dismiss their relationship altogether. And, even when a clear relationship between such variables obtains, one might well be hard put to explain it.

The upshot of the foregoing remarks is twofold. On the one hand, social measurement has proceeded well beyond the confines of social theorizing. Government agencies, themselves constrained by the necessity of justifying their activities, have proceeded to create social indicators of their own performance. On the other hand, social theory has advanced well beyond the confines of social measurement, delving with considerable depth into the logical parameters which might govern social behavior without addressing the question of how those parameters are to be measured. The problem is exacerbated because most sociologists who claim to be social theorists do not know the difference between a variable and a constant and, in addition, because a fair number of research sociologists have no conception whatsoever of how their discrete findings might possibly cumulate into the confirmation or rejection of a body of social theory. There just is no obvious link between social theory and social statistics, when each is taken as a whole. This observation does not, of course, deny the real and close connection which exists between theory and government-sponsored data collection in a subfield of sociology such as demography. But even there, the congruence between the statistical series one would like to have and those available is far from perfect. In other areas, the relationship between the available time series and the theoretical concerns of the discipline is often not apparent at all.

The body of extant social time series is large and covers a wide range of social phenomena; we have little question that they provide a plausible substantive base from which one can begin to construct a dynamic model of American society. The construction of these social time series is not, however, well grounded in social theory or, for that matter, in any other kind of theory. Making a model of society from them is, without question, going to incur a considerable amount of specification bias. Structural relationships obscured by trends will doubtless go undetected and, conversely, spurious relationships built up from common causes and coincidental trends will doubtless pass as structural ones. Considerable wisdom, time, patience, and effort will be required to develop a plausible model of American society from extant time series. En route, a lot of mistakes are surely going to be made. Foundations and other granting agencies choosing to support this line of model development must understand that the payoffs from their investments are far from certain. In these hills, there is a lot of fool's gold; beneath it all there may well be a true vein, but it will be found, save by accident, only by the extension of considerable research funds -- a fair fraction of which will serve only to close off unprofitable lines of prospecting.

13. SPECIAL PROBLEMS OF ESTIMATION

In the development of an annual econometric model of American society, one is likely to encounter occasions which call for the use of special and seldom used techniques of estimation. These circumstances are typically created by virtue of the specification of particular structural equations, which prove to be both nonlinear in the parameters and impossible to express in a form subject to estimation by ordinary least squares or other familiar estimating techniques. In these situations, some inventiveness may be required on the part of the investigator. However, econometricians have developed a variety of techniques for handling such cases; often a modest variant of one or another of these methods, which have not been used extensively (if at all) in the sociological literature, will prove appropriate, albeit quite cumbersome.

Special problems of estimation are frequently encountered in situations where not only the present, but also the previous values of an independent variable enter an equation as predictors, i.e., in circumstances where lagged effects are present. Special strategies of estimation are also encountered in other circumstances as well, but we will illustrate the case of lagged predictors, with reference to the level of female labor force participation.

One of the most noteworthy features of postwar American society has been the rising level of labor force participation among women. Among the factors associated with female labor force participation are divorce, the relative size of the armed forces, and the educational attainment of women. Divorced women, like single ones, often must support themselves and, since they are on the average somewhat older than single women, divorcees are less likely to be able to call upon their families of origin for financial support. For example, in 1970, almost two-thirds of divorced women aged 16 and over residing in New York State were in the labor force, as compared to 55.2 of single women and 37.5 percent married women in the same area and age grade. (U.S. Bureau of the Census, 1972, Table 165, p. 34-848.) The military enters the picture of female labor participation because it drains younger males from the labor market, creating opportunities for which women would otherwise have to compete with men more fiercely. There are a variety of reasons why the educational level of women enhances their labor force participation rate, perhaps the most obvious one being that the income foregone by housekeeping and childbearing rises with years of school completed.

Annual data on female labor force participation, the divorce rate, and the relative numbers of armed forces personnel are available throughout the postwar period. For present purposes, these variables may be defined as the proportion of females aged 16 and over who are in the labor force (F_t), the number of divorces per 1000 married women aged 15 and over (D_t), and the number of armed forces personnel on active duty expressed as a percentage of the total civilian labor force (M_t). Comparable annual data on educational attainment are not, however, available for the entire postwar period; indeed, they do not become available on a continuous annual basis until the 1960s. Consequently, any analysis which spans the postwar period must rely upon a proxy for educational attainment. The best available indicator, in our judgement, is total educational expenditures expressed as a percentage of gross national product (E_t). For 1962 and the period 1964-72, for which both annual data on educational attainment and this indicator are available, we find a correlation of .9668 between E_t and the median educational attainment of women aged 14 and over.

Evidently, the value of educational expenditures relative to gross national product is a plausible proxy for the educational attainment of women. As relative educational expenditures expand, the schooling opportunities for both men and women likewise increase; the 1960s proved to be a period in which the relative numbers of women entering graduate school and completing higher degrees was expanding. One could (and in preliminary work one would) simply treat the female labor force participation rate as a linearly additive function of the divorce indicator (D_t), the military variable (M_t), and the proxy for educational attainment (E_t). A moment's reflection suggests, however, that the relationship between E_t and female labor participation may prove a bit more complicated than this. While it is reasonable to assume that the impact of educational attainment upon female labor participation is both immediate and continuously renewed from year to year, it is not quite so plausible to make a similar assumption about the proxy we have for it--to wit, relative educational expenditures. It is quite conceivable that not only this year's, but last year's educational expenditures, not to mention those for even previous years, have an impact upon current levels of female labor participation. These lagged effects are in part attributable to the time lapse between the completion of formal studies and completion of the relevant degrees.

With the potential lagged effects of relative educational expenditures in mind, we are led to postulate that

$$F_t = \alpha + \sum_{i=0}^{\infty} \beta_i E_{t-i} + \gamma D_t + \chi M_t + e_t, \qquad \text{(Eq. 13.1)}$$

where e_t is randomly distributed error term, the β_i's, α, γ, and χ are the coefficients we wish to estimate, and the remaining variables are as defined in the preceding text. Evidently, Eq. 13.1 cannot be estimated in any straightforward manner as it now stands. We can get closer to an estimable version of Eq. 13.1 by imposing an assumption about the interrelationship between the successive values of the β_i's. In particular, we can treat the relationship between F_t and E_t as ordinary problem in distributed lags by assuming the β_i's are exponentially decreasing, so that

$$\beta_i = \beta\lambda^i, \text{ where } i = 0, 1, 2, \ldots, \text{ and } 0 < \lambda < 1. \qquad \text{(Eq. 13.2)}$$

With this assumption, Eq. 13.1 becomes

$$F_t = \alpha + \sum_{i=0}^{\infty} \beta\lambda^i E_{t-i} + \gamma D_t + \chi M_t + e_t. \qquad \text{(Eq. 13.3)}$$

We may also observe that in this case

$$F_{t-1} = \alpha + \sum_{i=0}^{\infty} \beta\lambda^i E_{t-1-i} + \gamma D_{t-1} + \chi M_{t-1} + e_{t-1}. \qquad \text{(Eq. 13.4)}$$

Multiplying Eq. 13.4 by λ and subtracting it from Eq. 13.3 leaves us with

$$F_t - \lambda F_{t-1} = \alpha^* + \beta E_t + \gamma(D_t - \lambda D_{t-1}) + \chi(M_t - \lambda M_{t-1}) + e_t^*, \qquad \text{(Eq. 13.5)}$$

where $\alpha^* = \alpha(1 - \lambda)$ and $e_t^* = e_t - \lambda e_{t-1}$. This is a rather thinly disguised problem in distributed lags and, were it not for the terms involving the divorce and military variables on the right hand side of Eq. 13.5, we could estimate it directly upon adding λF_{t-1} to both sides of the equation.

Unfortunately, the ordinary strategy for dealing with exponentially declining lags is not open in the present case, for the reason noted, to wit the implication of λ in the terms involving the military and divorce variables. However, if the value of λ were known, one could simply construct the values of $F_t - \lambda F_{t-1}$, $D_t - \lambda D_{t-1}$, and $M_t - \lambda M_{t-1}$ and proceed to estimate Eq. 13.5 with ordinary least squares. While λ is not, in fact, known, we can assume that its value lies between 0 and 1; consequently, we can institute a search procedure by selecting alternative values of λ, estimating Eq. 13.5 for each of these alternative values, and selecting the value of λ which minimizes the variance of e^*_t (within the degree of accuracy superimposed upon the alternative trial values of λ).

Insert Figure 13.1 and Table 13.1 about here

The search procedure outlined above is, needless to say, a tedious one. For the present illustrative purposes, we have simply experimented with values of λ given by $\lambda = (.1)i$, where $i = 1, 2, . ., 9$. The results are given in Table 13.1 and graphically displayed in Figure 13.1. Although we certainly had no grounds for knowing it at the outset, the results indicate that the present

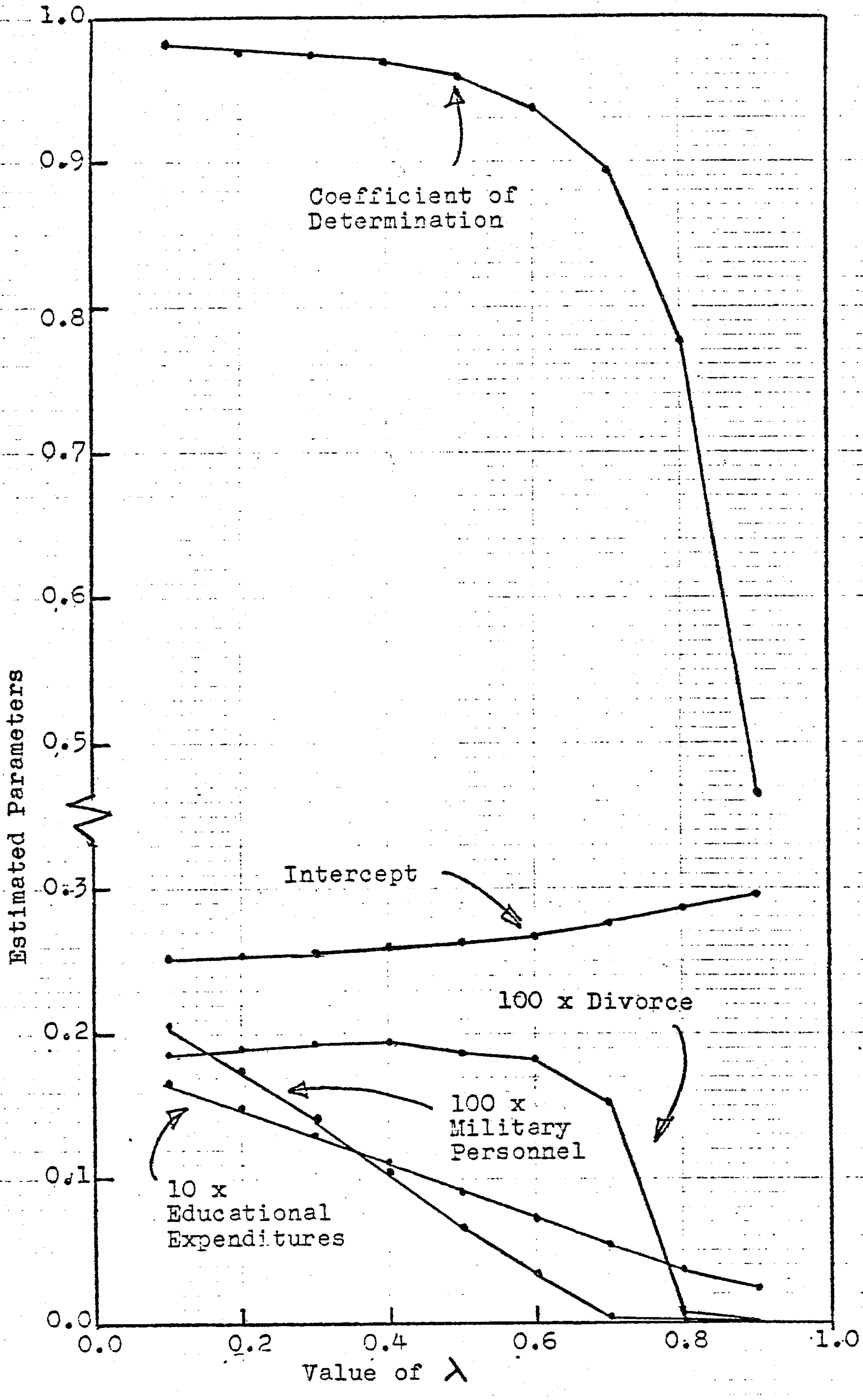

Fig. 13.1 --Estimated Parameters of Female Labor Force Participation Equation, for Alternative Values of λ , United States, 1948-1972.

Table 13.1.--Estimated Parameters of Equation for Female Labor Force Participation Under Alternative Assumptions About λ, United States, 1948-72

Value of λ	Intercept	Coefficients: Educational Expenditures	Coefficients: Military Personnel	Coefficients: Divorce Rate	R^2
	Ordinary Least Squares Estimates				
.1	.2523**	.01696**	.002059+	.001886**	.9807
.2	.2544**	.01500**	.001768+	.001900**	.9787
.3	.2570**	.01304**	.001431	.001911**	.9755
.4	.2603**	.01108**	.001061	.001914**	.9699
.5	.2644**	.00914**	.000688	.001896**	.9596
.6	.2698**	.00724**	.000361	.001808+	.9391
.7	.2773**	.00545**	.00015	.001527	.8935
.8	.2875**	.00386**	.00011	.00080	.7786
.9	.2980**	.00246**	.00028	-.00046	.4678

** Coefficients more than twice their standard errors.

+ Significantly greater than zero at the .10 level with a one-tail test.

illustrative problem has a very simple structure. As one can see in Table 13.1 and Figure 13.1, the coefficient of determination declines monotonically as λ increases from .1 to .9; the estimated value of β (the coefficient associated with E_t) likewise declines monotonically, as does that associated with the military variable. Only the intercept (as expected, given the behavior of the coefficients of the variables) rises as λ increases. The coefficient of the divorce variable rises almost imperceptibly as λ goes from .1 to .4, but then falls off rapidly.

The illustrative calculations presented above suffice to document some of the unusual strategies of estimation which one is likely to encounter in developing a well specified model of American society; we think this is particularly the case if one models the measured variables rather than the theoretical variables for which they are proxies. Evidently, in the present illustration, one would proceed to make more refined estimates of the underlying parameters by using trial values of λ accurate to at least two places through the range between 0 and .2. We have not done so both because we would not want to defend our specification of the female labor force equation very vigorously, and because the results already in hand suffice to illustrate how search procedures can be utilized to estimate certain equations which are otherwise intractable. One needs to be appraised of these and related strategies of estimation, though it seems likely that the intellectual bases for any initial model of American society are apt to be too primitive to justify any but the simplest of additive or multiplicative specifications. We should note, however, that at least one effort to describe the time path of congressional voting patterns has already utilized a strategy of estimation very similar to the one illustrated herein (see Kramer, 1971). Complexities of this kind will not long be kept from dynamic models of American society once an initial version has been subjected to professional scrutiny.

Insert Fig. 13.2 about here

Further study revealed that for most of the equations estimated in Table 13.1 we could not discard the null hypothesis of zero autocorrelation in the error terms. Consequently, we re-estimated all the regressions utilizing the Cochrane-Orcutt technique. As Figure 13.2 reveals, the estimated value of ρ and the coefficient of determination decrease monotonically as λ increases. Although we do not burden the reader with the detailed display of the estimated regression coefficients, with the exception of the military variable, all the estimated coefficients exceed their standard errors by at least a factor of two, and their values are very close to the estimates reported above. As we note elsewhere in this paper, the use of estimating techniques that correct for autoregressive disturbances does not invariably wreak havoc with the findings from the inappropriate application of ordinary least squares.

We would not like to leave the erroneous impression that distributed lags always take the geometric form illustrated in this section. The geometric lag distribution is the appropriate

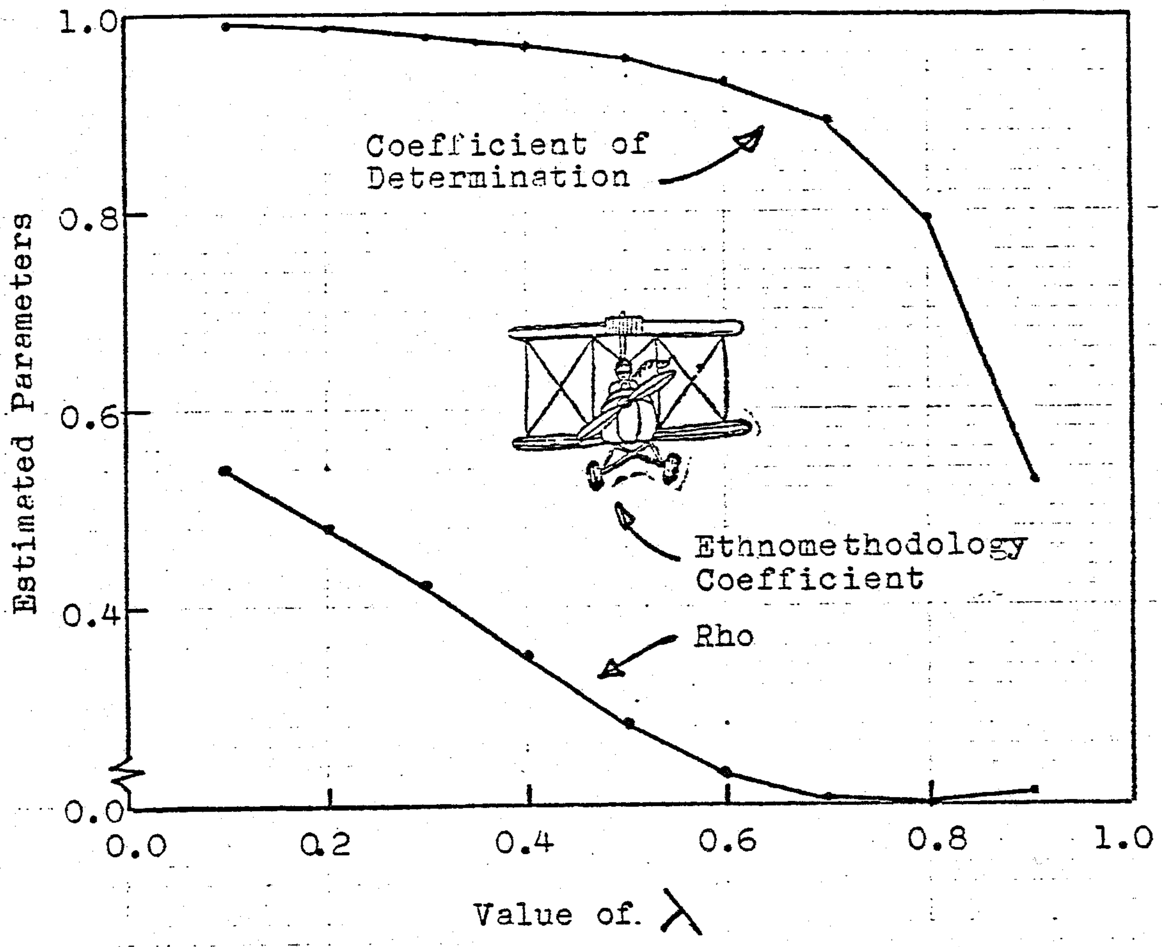

Fig. 13.2 --Estimated Values of Rho and R^2 for Female Labor Force Participation Equation Derived from Cochrane-Orcutt Iterative Procedure, for Alternative Values of λ, United States, 1949-1972.

functional form whenever the effects of the lagged predictors extend into the past, but decline exponentially. Sometimes the investigator may prefer to postulate a causal scheme in which the coefficients of the lagged variable rise in the more recent past, prior to declining monotonically. A special instance of such an inverted V-lag model, which enjoys widespread popularity in econometric circles, assumes that the coefficients of the lagged variable lie within a polynomial of a specified degree. Considerable trial and error are involved in finding the appropriate length of the lag and the optimal degree of the polynomial. To be sure, such polynomial distributed lags necessarily require a large number of observations to allow sufficient degrees of freedom for estimation. Annual postwar time series of social indicators are still too short to take full advantage of this technique. In addition, while further complications may arise whenever auto-correlated residuals are present in any of the schemes of distributed lags, they may be routinely handled via generalized least squares estimation.

14. OVERVIEW AND CONCLUSIONS

Our purpose in this essay has been to review some, but not all, of the major technical and methodological problems likely to be encountered by any sustained effort to analyze time series of social indicators. In particular, we have glossed over the topics of multicollinearity and aggregation. These problems, however, are by no means unique to time series analysis and there are numerous discussions of them in both the sociological and economic literature (see, e.g., on aggregation, Alker, 1969; Hannan, 1971; Rosenthal, 1973; Shively, 1969; and Theil, 1964; and on multicollinearity, Blalock, 1963; Farrar and Glauber, 1967; Johnston, 1972, pp. 159-168). Our reluctance to discuss the aforementioned problems in the present context is not a reflection of their significance, since they pose serious barriers to the analysis and/or interpretation of many time series. In view of the substantial literature already accessible on these subjects, anything we might say about them in a short review essay would be particularly superficial. For that reason, we have neglected them in order to provide empirical illustrations of some problems which are more nearly unique to the analysis of time series data. We should note, however, that many of the difficulties surveyed above are themselves intertwined with one or the other of these problems. For example, the multicollinearity between time series is often responsible for shifts in the signs and significance levels of structural coefficients when additional predictors are included and, perhaps more signally, when observations are deleted from or added to the period under investigation.

The problems surveyed herein are serious barricades which will be encountered en route to the development of a dynamic, macro-sociological model of American society. However, while it would be foolhardy to discount these difficulties, there are two very good reasons to believe they are not insurmountable ones. First, in principle, the problems **alluded to** are identical to those faced by those who have and who continue to develop structural equation models of the economy. At this juncture, there are numerous competing econometric models of our economy (e.g., Duesenberry, 1965; Hickman, 1972). While these models evidently fall short of enabling us to exercise the technical mastery of the economy which the physical sciences have given us of an enlarging fraction of the world, only few would question the assertion that the development of these models has enhanced our understanding of how the economy works. Structural equation models of the economy are without neither their pitfalls (see, e.g., Cooper, 1972) nor their detractors (see, e.g., Fair, 1971). Any model of the society at large which is developed at the present time will surely be subject to criticisms and refinements. Despite their shortcomings, we believe that development of such models will provide new insights into social dynamics, though we would neither expect nor, for that matter, want their construction to provide a tool for social manipulation and control.

The second good reason to believe that the barriers to the development of a dynamic model of society are by no means insurmountable ones is rooted in the record of progress already made on this front. From demography (cf., Keyfitz, 1968) and closely related social accounting models (e.g., Stone, 1966, 1971), a foothold has definitely been made on understanding the dynamics of change in socioeconomic spheres. Furthermore, at least two teams of independent investigators have already proposed somewhat loosely structured models of postwar American society (Land and Felson, 1975; Klorman and Hodge, 1975). The rationalization of these models is still underway, but it is almost surely the case that one of these or yet another preliminary formulation will provide the impetus for future work on this subject. There is really not much question that we are going to have dynamic, structural equation models of our own society and eventually of others. Questions will undoubtedly be raised about these models. Those questions will not, in fact, differ very much from those which already have been and will be put to extant models of the economy.

A review of the problems raised in this essay suggests some of the deficiencies which will characterize initial and necessarily primitive, dynamic models of society. Among those deficiencies are at least these:

1. The model will be grossly underidentified in the sense that there will be more variables in it than there are data points for estimating it. In a venture of this kind there is just simply no way in which one can let the data speak for themselves, because there are not, at the present writing, enough degrees of freedom to let them do so.

2. Given the data constraint noted above, it is more than evident that even the most rudimentary dynamic model of the society is, of necessity, going to be informed by extant social theory. Some potentially non-zero parameters are going to be set to zero; the justification for doing so is itself necessarily rooted in a theory of how society works. Somewhat paradoxically, one is locked in the curious box, given available knowledge and data constraints, of proposing to figure out how a society works via a method which requires a priori knowledge of something about how it in fact works.

3. The upshot of the foregoing remark is clear: persons approaching the construction of a dynamic model of the American or any other society at the present time are very likely to be divided over their theoretical premises. Our experience has been not only that alternative specifications perform equally well, but also that no one of them fits very well with the extant cross-sectional literature. Choosing between such specifications is largely a matter of theory (if one chooses to elevate personal biases to that level). The data now in hand are just not sufficient to make even tentative resolutions between competing theories. That prospect will occur when the number of data points available for analysis begins to exceed the number of variables which might plausibly enter the equations. Unfortunately, that prospect is most likely

well beyond the life horizon of anyone reading this essay. Social time series just do not have the historical depth which characterizes selected demographic and economic series.

4. Any dynamic model of American society developed in the near future, which adheres to any rigid requirements about the statistical significance of the variables which enter into it, will almost certainly entail an appreciable oversimplification of how the society functions. Much of the available data which could be utilized to develop such a model are available only on an annual basis for periods that are often substantially shorter than the entire post-World War II era. Consequently, any initial model constructed at the present time will necessarily rest on a limited number of data points. While the range of the available data and the length of their time spans are, in our judgment, sufficient to support preliminary efforts at modelling the society, they surely do not provide the bases for teasing out subtleties of the ways in which the variables comprising such a model are interrelated. The precision and sophistication which can be injected into such efforts at model building will be enhanced enormously as the available time series are extended. One of the byproducts from the successful construction of a preliminary model of the

society based on extant data is to help convince the private and public agencies responsible for the collection of the underlying data of the long run possibilities for understanding how our society works. Such a demonstration effect may well support the justification and expansion of the agencies' data acquisition activities.

5. Any effort to build a dynamic model of American society at the present time is also likely to be seriously deficient, not only in the sense noted above that it will likely prove considerably simpler than the society which it purports to mirror, but also in the sense that important components of a full model will be missing because they have yet to be adequately measured systematically from year to year over any substantial period of time. For example, extant statistics enable one to trace trends in such matters as public trust in government officials, the prestige or social standing attributed to occupations and ethnic groups, the level of white prejudice toward blacks, and the public's knowledge and/or acceptance of basic civil liberties over substantial fractions of the postwar period. However, since the readings on these phenomena are sporadic, the inclusion of variables such as these in a dynamic model -- which many would regard as first order business -- could be accomplished only by recourse to a mixed strategy of estimation whereby some

coefficients are derived from individual level data from one or two cross-sectional surveys and others are based on estimates derived from annual, albeit highly aggregated data.

6. The estimated parameters of any model derived from currently available data are likely to prove quite unstable. Among the factors contributing to this problem are autocorrelation and pronounced multicollinearity in the available series, not to mention one's inability at the present juncture to do much more than speculate about the likely impact of some crucial missing variables.

The difficulties noted above are not intended to exhaust the problems likely to be encountered by an effort to develop a dynamic model of American society at the present time. They are, however, sufficient to cast considerable doubt about the possibility of currently developing a satisfactory model of this type. Our view, nevertheless, remains an optimistic one. The very effort to develop such a model is likely to prove a very provocative and fruitful venture. Even if one cannot successfully construct a full account of how the society works, one is very likely to learn a great deal about its inner workings by the effort to model extant series. Such an effort is also likely to provide guidelines into which series should be monitored periodically. Needless to say, even the most primitive dynamic model will provide a reasonably succinct summary of what can be said with extant data about short run social change and will provide a dynamic context in which cross-sectional studies can be placed and partially interpreted.

For most of the history of mankind, people were puzzled by the phenomenon of flight. Now, in less than a century, we have learned how to construct flying machines, including ones like the Concorde and the B1 which substantial numbers of persons think we could well do without. People have probably been puzzled for nearly as long about how the societies in which they lived have worked. We have accumulated some substantial insights into social and economic behavior during the period since the effort to build a flying machine was taken as a technologically feasible adventure. We have not yet mastered social and economic affairs to the extent that we have conquered flight, but progress is being made and is likely to continue.

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BIBLIOGRAPHIC APPENDIX

Data sources have been relegated to this section to avoid unnecessary cluttering of the text with footnotes. At the time these data were collected, current data from Vital Statistics were unavailable and the bicentennial edition of Historical Statistics had not been as yet published. Hence, most series had to be extracted from a multiplicity of sources. Some unavailable observations were interpolated. Complete documentation will accompany our substantive analyses of these series.

A somewhat abbreviated notation will be used throughout this section. The first column of Table A.1, entitled reference, points to specific sections of this essay in which data items (listed under the second column of Table A.1) were utilized.

Anyone checking the sources identified below will encounter some overlapping series; throughout we have taken the most recent estimates available.

Table A.1.--DATA SOURCES

REFERENCE	ITEM	SOURCE
Fig. 4.1	employed craftsmen	The Labor Force Bulletin 7:9; Supplement to the Monthly Report on the Labor Force 59-S:5; Current Population Reports (CPR) Series P-57, No. 63, p. 12, Nos. 65, 68, 70, 73 and 76 (p. 11); CPR, Series P-50, No. 19, p. 24; Statistical Abstract of the United States, (ABS) 1972, pp. 67-68; CPR, Series P-50, Nos. 40 (p. 26), 45 (p. 24), 67 (p. 39), 59 (p. 28), 72 (p. 29); Employment and Earnings, January 1960, p. xi; U.S. Department of Labor, Statistics on Manpower, A Reprint from the 1973 Manpower Report of the President, p. 141.
Fig. 4.1	military personnel on active duty	The data on military personnel come from: U.S. Department of Commerce, Bureau of the Census, Historical Statistics of the United States, Colonial Times to 1957 (HSTAT), p. 736; Historical Statistics of the United States, Continuation to 1962 and Revisions (CONT), p. 736; U.S. Department of Defense, Directorate for Information Operations, Selected Manpower Statistics, April 15, 1972, p. 11.

REFERENCE	ITEM	SOURCE
section 5, p.5	estimated GNP for 1971	U.S. Department of Labor, Handbook of Labor Statistics (HDBK), 1975 (p. 445); HDBK, 1974 (p. 420); HDBK, 1973 (p. 404); HDBK, 1972 (p. 389)
section 5, p.6	average speed of motor vehicles in open highways, 1964	ABS, 1965, (p. 572); ABS, 1966 (p. 569), For discrepant estimates see: ABS, 1967 (p. 561); ABS, 1971 (p. 538).
Fig. 8.1	total population, population 65 years of age and over	CPR, Series P-25, Nos. 311, 314, 385, 441, 519
Fig. 8.1	expenditures for education as a percent of the GNP	U.S. Department of Health, Education, and Welfare, National Center for Educational Statistics, Digest of Educational Statistics, 1973, p. 25
Fig. 8.1	GNP in constant dollars	1972 Economic Report of the President, p. 196
Fig. 8.1	farm population	U.S. Department of Agriculture, Rural Development Service, Statistical Bulletin No. 523, Farm Population Estimates, 1910-1970, July, 1973, pp. 14-15; CPR, Series P-27, No. 45, p. 1
Fig. 8.1	total automobile registrations	HSTAT, p. 462; CONT, p. 65; ABS, 1964 (p. 556); ABS, 1965 (p. 571); ABS, 1966 (p. 572); ABS, 1967 (p. 594); ABS, 1968 (p. 553); ABS, 1969 (p. 550); ABS, 1970 (p. 545); ABS, 1971 (p. 535); ABS, 1973 (p. 547)
Fig. 8.1	per capita food consumption index	U.S. Department of Agriculture, Economic Research Service, Food Consumption Prices, Expenditures, Supplement to Agricultural Economic Report, No. 138, p. 9
Fig. 8.1	hospital beds	HSTAT, p. 35; CONT, p. 7; American Hospital Association, Hospitals, vol. 39, pt. 2, p. 448, vol. 45, pt. 2, p. 460, vol. 48, pt. 2, p. 17

REFERENCE	ITEM	SOURCE
Fig. 8.1	percentage of live births attended in hospitals	U.S. Department of Health, Education and Welfare, National Center for Health Statistics, Vital Statistics of the United States (VSTAT), 1968, p. 1-20; correspondence with the National Center for Health Statistics.
Fig. 8.1	maternal mortality rate, neonatal mortality rate, postnatal mortality rate	HSTAT, p. 25; CONT, p. 5; Monthly Vital Statistics Report (MVSTAT) 24:13:8
Fig. 8.1	ratio of medical school graduates to nursing school graduates	The data on medical graduates come from: U.S. Department of Health, Education and Welfare, Public Health Service, Health Manpower Source Book, Section 9, Washington, D.C., 1959, p. 9; Health Resources Statistics (HRSTAT), 1974, p. 184. The corresponding data on nursing graduates come from: HSTAT, p. 34; CONT, p. 7; HRSTAT, 1974, p. 207
Table 10.1	NBER Coincident business cycle indicator	Business Conditions Digest, May 1974, p. 111
Table 10.1	age-adjusted suicide rate	U.S. Department of Health, Education, and Welfare, National Center for Health Statistics, Vital Statistics Rates in the United States, 1940-1960, p. 533; VSTAT (volumes for 1961-1969), vol. 2 pt. A; MVSTAT (supplements) 22:11:4-5; 23:3:4-5; 23:8:4-5. These rates were standardized by the 1960 age distribution
Eqs. 11.6, 11.7	marriage rate per 1000 men 15 years old and over	VSTAT, 1969, vol. 3, p. 1-5; correspondence with the National Center for Health Statistics

REFERENCE	ITEM	SOURCE
Eqs. 11.6, 11.7	total unemployment rate	HDBK, 1973, p. 27
Eqs. 11.6, 11.7	female participation rate in the labor force	HDBK, 1973, pp. 32 and 40
Eqs. 11.6, 11.7	NBER coincident business cycle indicator	see sources to Fig. 4.1.
Eqs. 11.6, 11.7	military personnel as a percent of the labor force	Numerator referenced under sources for Fig. 4.1 The data on the size of the labor force were taken from HSTAT, p. 70 (through 1946); HDBK, 1973, pp. 27-28 (from 1947 on).
Fig. 12.1, Eqs. 12.3, 12.5	consumer price index	HDBK, 1973, p. 287
Fig. 12.1, Eqs. 12.3, 12.5	total unemployment rate	see sources to Eqs. 11.6, 11.7
Fig. 12.1, Eqs. 12.3, 12.5	Defense expenditures as a percent of GNP	ABS, 1966 (p. 252); ABS, 1971 (p. 240); ABS, 1974 (p. 306)
Section 13, p.3	median educational attainment for females 14 years of age and over	CPR, Series P-20, Nos. 121 (p. 7), 138 (p. 9), 158 (pp. 7 and 18), 169 (p. 8), 181 (p. 9), 194 (p. 9), 207 (p. 11), 229 (p. 13), 243 (p. 13)
Table 13.1, Figs. 13.1, 13.2	divorce rate per 1000 married women 15 years of age and older	VSTAT, 1969, vol. 3, p. 2-5; MVSTAT (supplement) 25:1:2
Table 13.1, Figs. 13.1, 13.2	military personnel as a percent of the labor force	see sources to Fig. 4.1 and Eqs. 11.6, 11.7.
Table 13.1, Figs. 13.1, 13.2	educational expenditures as a percent of the GNP	see sources to Fig. 8.1
Table 13.1, Figs. 13.1, 13.2	female participation rate in the labor force	see sources to Eqs. 11.6, 11.7