CHAPTER TWELVE

# APPLICATIONS OF ECONOMETRIC ANALYSIS TO FORECASTING IN INTERNATIONAL RELATIONS

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## I. INTRODUCTION

This chapter examines some key issues and difficulties encountered in the course of applying econometric analysis to forecasting in international relations. We will note the problems involved and the solutions adopted, and indicate the consequences of faulty analysis, analytical bias, or measurement error. In so doing, we shall draw upon our recent investigations into the long-range causes of international conflict. Our objective, during the past several years, has been to develop systematic procedures for isolating the determinants of international violence. The general approach we have employed is one common to any econometrician concerned with the analysis of time series data, or any statistician examining the properties of small samples.[1] But our applications of these methods are not common to political analysis. Economists, for example, appear to know much more about the nature of market systems, business cycles, inflation, and so forth, than political analysts know about conflict and warfare, arms races, lateral pressure, or international alignments.[2]

In the course of our inquiries we have developed a partial theory of the dynamics in question, translated this theory into a model from which structural equations were developed, and then estimated the unknown parameters. The purpose of

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this enterprise was to investigate the implications of alternative parameter estimates upon the behavior of the system as a whole. Experimenting with "high" and "low" coefficients, and comparing these with baseline parameters and system outputs provided us with a reliable means of looking into alternative outcomes and alternative futures.

It is not our objective here to question the nature of causality, or to dispute the assumptions underlying the social and behavioral sciences. Others have done this elsewhere.[3] Nor is it our intent to deliver an introductory lecture on the algorithms upon which elementary statistical methods are based. Rather, our purpose is to make explicit the critical problems inherent in econometric analysis and the ways we have sought to resolve them.[4] Toward this end we discuss: (1) the structure of our model of international conflict as an extension of the general linear model in regression; (2) methodological implications of alternative perspectives upon causality; (3) some key statistics and common problems in causal inference; (4) simultaneous estimation and the problem of identifiability; (5) serial correlation and time dependent corrections; (6) the use of instrumental variables and generalized least squares; (7) system change and breakpoint analysis; and finally (8) procedures employed for simulation, forecasting, and policy analysis and some practical illustrations.

## II. A MODEL OF INTERNATIONAL CONFLICT: EXTENSIONS OF THE GENERAL LINEAR MODEL

In a recent study of international politics, we have argued that the roots of conflict and warfare can be found in the basic attributes and characteristics of nations and that the most critical variables in this regard are population, resources, and technology. We have then attempted to specify the intervening sequences between these three sets of variables on the one hand and conflict and warfare on the other. On the basis of empirical and historical analysis, we suggest that the chain of developments relating population, resources, and technology to violence appears to be the following:

A combination of *population* and developing *technology* places rapidly increasing demands upon *resources*, often resulting in internally generated pressures. The greater this pressure, the higher will be the likelihood of extending national activities outside territorial boundaries. We have termed this tendency to extend behavior outside national boundaries *lateral pressure*. To the extent that two or more countries with high capability and high pressure tendency (and high lateral pressure) extend their interests and psycho-political borders outward, there is a strong probability that eventually the two opposing spheres of interest will intersect. The more intense the intersection, the greater will be the likelihood that *competition* will assume *military* proportions. When this happens, we may expect competition to be transformed into *conflict* and perhaps an *arms race* or cold *war*. At a more general level of abstraction, *provocation* will be the final act that can be viewed as the stimulus for large-scale conflict or violence. But an act will be considered provocation only in a situation that has already been characterized by high lateral pressure, intersections among spheres of influence, armament tensions and competitions, and an increasing level of prevailing conflict.

Major wars, we have argued, often emerge through a two-step process: in terms of internally generated pressure (which can be traced to population dynamics, resource needs and constraints, and technological development) and in terms of reciprocal comparison, rivalry, and conflict, on a number of salient capability and behavior dimensions. Each process tends to be closely related to the other, and each, to a surprising degree, can be accounted for by relatively nonmanipulable variables (or variables that are controllable only at high costs). And it is these variables, we hypothesize, that provide the long-range roots of conflict and warfare.

The first step in the transition from a general theoretical statement to a model capable of sustaining the empirical test is to identify the variables to be explained. These will eventually serve as the outputs of the model. The second is to specify those effects that contribute to outcome variables by developing equations designed to explain the behavior of each of the dependent variables.

Those explanatory variables that are thought to contribute to our understanding of the outcomes in question can be other dependent variables (lagged or unlagged) or they may be exogenous variables and not to be explained by the model. For policy purposes it is important to select at least some explanatory variables that are manipulable by the policy maker. For obvious reasons, it would not be useful to select only variables that are all "givens" or variables that are manipulable at very high costs, unless, of course, one's objectives were to test for the extent to which nonmanipulables dominated system behavior.

Our theoretical statement can thus be transformed into graphic relationships, as noted in Figure 12.1. These relationships can then be translated into structural equations, the parameters of which could then be estimated in the context of the general linear model. This particular model pertains to the pre-World War I period, 1870–1914.

The general linear model in econometrics and causal modeling is a conceptual mechanism to determine the values of variables when quantitative data are supplied.[5] This mechanism includes a set of equations, their functional form, and an accompanying set of specifications and restrictions. We combine observed data, specifications of a model, and the laws of probability to obtain estimates of unknown parameters.[6]

This basic linear model is of the following form:

$$Y = X\beta + U$$

where $Y$ represents a vector of observations of the dependent or endogenous variable over time;

$X$ represents the matrix of independent variables (explanatory, predetermined, and exogenous);

$\beta$ is the vector of coefficients to be estimated from empirical data;

$U$ represents the vector of error or disturbance term, which has three error components: (1) error due to a linear approximation of the "true" functional form, (2) error resulting from erroneously included or left out variables, and (3) random noise.

The general linear form can be extended to the case of $m$ independent variables and $n$ equations, with the assumption that each dependent variable can be expressed as a linear function of the independent or exogenous variables (linear in the parameters only; the variables can be nonlinear

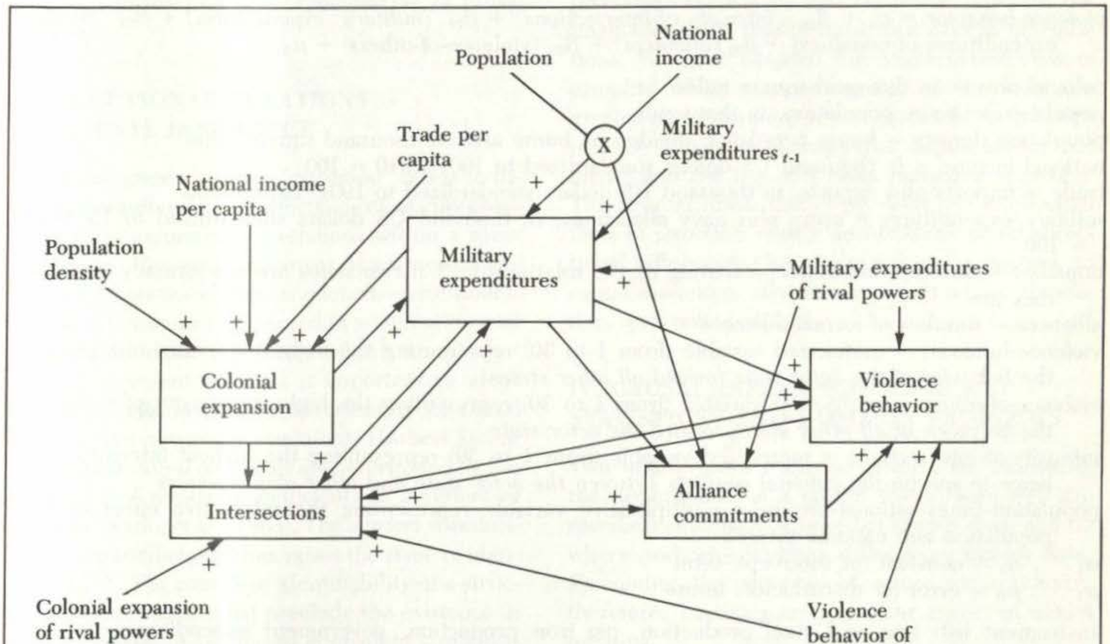


Figure 12.1 Dynamics of international violence: the model.

functions of other variables). It is also assumed that empirical observations are generated by a stochastic mechanism. In the case of the general model, ordinary least squares (OLS) provides the best linear unbiased estimates of the parameters only if the following assumptions or *a priori* constraints are not seriously violated: (1) that the disturbance terms ($U$) are random variables, with zero mean and homogenous variance; (2) that the disturbances are uncorrelated over time; and (3) that the exogenous variables are not correlated with the disturbances.

The model we have developed is more complex than the general linear case. Some of the complexity is due to the nature of the processes being modeled, the procedures we have employed to correct for significant departures from the assumptions underlying an ordinary least squares solution of the general linear model, and the use of simultaneous equation estimators to obtain unbiased coefficients of feedback systems. The resultant system of equations is presented in Table 12.1.

The entire analysis was undertaken on TROLL/1, an interactive computer system developed at the

**Table 12.1** Systems of simultaneous equations used to represent the dynamics of international violence (italics indicate variables endogenous to the system).

*colonial area* = $\alpha_1 + \beta_1$ (population density) + $\beta_2$ (national income per capita) + $\beta_3$ (trade per capita) + $\beta_4$ *(military expenditures)* + $\mu_1$.

*intensity-of-intersections* = $\alpha_2 + \beta_5$ *(colonial area)* + $\beta_6$ *(military expenditures)* + $\beta_7$ (colonial-area-of-nonallies) + $\beta_8$ *(violence-behavior)* + $\beta_9$ (violence-of-others) + $\mu_2$.

*military expenditures* = $\alpha_3 + \beta_{10}$ *(military expenditures$_{t-1}$)* + $\beta_{11}$ (military-expenditures-of-nonallies) + $\beta_{12}$ *(intensity-of-intersections)* + $\beta_{13}$ *(colonial area)* + $\beta_{14}$ (population-times-national-income) + $\mu_3$.

*alliances* = $\alpha_4 + \beta_{15}$ *(military expenditures)* + $\beta_{16}$ *(intensity-of-intersections)* + $\beta_{17}$ (military-expenditures-of-nonallies) + $\beta_{18}$ (population-times-national-income) + $\mu_4$.

*violence-behavior* = $\alpha_5 + \beta_{19}$ *(intensity-of-intersections)* + $\beta_{20}$ *(military expenditures)* + $\beta_{21}$ (military-expenditures-of-nonallies) + $\beta_{22}$ *(alliances)* + $\beta_{23}$ (violence-of-others) + $\mu_5$.

colonial area = in thousand square miles
population = home population, in thousands
population density = home population divided by home area (in thousand square miles)
national income = in thousand US dollars standardized to 1901–1910 = 100
trade = imports plus exports, in thousand US dollars standardized to 1901–1910 = 100
military expenditures = army plus navy allocations, in thousand US dollars standardized to 1901–1910 = 100
nonallies = dummy variable representing dyadic relationship: 1 if two states are not formally allied, 0 if they are
alliances = number of formal alliances
violence-behavior = metricized variable (from 1 to 30) representing the highest intensity of violence of the behavior of the *actor state toward all other states*
violence-of-others = metricized variable (from 1 to 30) representing the highest intensity of violence of the behavior of *all other states toward the actor state*
intensity-of-intersections = metricized variable (from 1 to 30) representing the highest intensity of violence in *specifically colonial conflicts between the actor state and other major powers*
population-times-national-income = multiplicative variable representing the interactive effect of home population and national income
$\alpha_1 \ldots \alpha_5$ = constant (or intercept) term
$\mu_1 \ldots \mu_5$ = error (or disturbance) term

Instrument list: iron and steel production, pig iron production, government expenditures, merchant marine tonnage, military expenditures of nonallies, colonial area of nonallies, population density, population times national income, national income per capita, trade per capita, intensity-of-intersections$_{t-1}$, violence behavior$_{t-1}$, violence of others, alliances$_{t-1}$, wheat production, coal production.

Massachusetts Institute of Technology for the analysis of econometric models and complex systems.[7] We have used generalized least squares, transforming the independent variables according to the structure of the serial correlation in the disturbances, in conjunction with two stage least squares (a limited information maximum likelihood estimator), so as to incorporate a time-dependent correction as well as simultaneous effects in the final estimates of the parameters.[8]

It is important to appreciate that the parameters of an equation cannot be estimated purely on the basis of empirical data, no matter how complete, reliable, or extensive these may be.[9] The role of data is as follows: Information is useful for identification purposes only if it can serve to distinguish among structural equations. Observational data alone *cannot* perform this necessary step in model building, although analysis of one set of data can provide clues for specification of the next set. Nonetheless, only in conjunction with *a priori* restrictions and specifications can empirical data be put to good usage.[10] But the most basic issue of all in making the transition from a theoretical statement to a formal model is specification of causal ordering.

## III. DIRECTIONAL RELATIONS AND CAUSAL INFERENCE

In the most general sense, "causation" refers to hierarchies of influences or effects, most readily characterized by asymmetrical relations within a specified system. Causation, however, is not necessarily implied by a particular time sequence—a consideration that is commonly neglected in systematic social and political inquiry. Because of this simple but almost self-evident point, it is important to adopt alternative criteria for the specification of causal relations. In a persuasive argument, Herbert Simon suggests that causal orderings are determined by the appearance of non-zero coefficients in a system of equations (Ando, *et al.*, 1963). The *a priori* specification of zero coefficients thus raises the issue of identifiability.[11] "For complete identifiability of a structure those restraints must preclude the existence in the same model of a different equivalent structure, that is (in linear models), a different set of equations whose members are linear combinations of the original equations" (Ando, *et al.*, 1963). Causation is, therefore, closely related to identifiability, while the requirements of identifiability, by necessity, impose certain constraints on the process of model building.

The question of causation gives rise to a related set of philosophical and empirical problems (Ando, *et al.*, 1963, p. 23; see also Orcutt, 1952, pp. 305–311). The long-standing debate among social scientists regarding causal perspectives upon the "real world"—whether it be essentially hierarchical or recursive, or whether it be essentially nonrecursive or simultaneous—can be resolved through a combination of these two positions, namely that the overall framework or system of relations (or equations) in the structure under consideration may basically be recursive (thus negating simultaneous relations at a macro level), but that small components (or blocks) thereof may be nonrecursive (thus allowing for feedback relations within a localized context). For applied analysis, the approach one takes has one important effect: How one perceives the phenomena one seeks to model (whether they are considered basically recursive or nonrecursive) will dictate the kind of estimation procedure employed, and the ways in which the phenomena are represented in a system of equations. We have adopted the nonrecursive view of causality while recognizing that in the long run greater understanding of the dynamics in question may be obtained through the expansion of our model and the use of a block-recursive approach. The general linear model provides the intellectual tools to structure reality and to think about directional influences. Our analysis goes far beyond, to causal modeling, simultaneous estimation, simulation, and policy analysis.

## IV. CAUSAL INFERENCE: SOME KEY STATISTICS[12] AND COMMON PROBLEMS

Two of the more common criteria for evaluating the performance of a model are (1) how well the specified equations can predict known data, and (2) where and why findings differ from known data. Examining the patterns of errors (or residuals), therefore, becomes an important aspect of model building.

The variance of the coefficient (or standard error) indicates the precision of the coefficient as derived from empirical data. The statistical significance of a parameter is inferred from the magnitude of the *t* statistic, and the significance of several parameters is inferred from the *F* ratio. In a regression equation, the value of *F* measures the joint significance of the parameter estimates. The summary statistic, $R^2$, refers to the amount of variance in the dependent variable explained by the independent variables (and the associated stochastic mechanism). A very high $R^2$ may imply an identity or a trivial regression equation, while a low $R^2$ does not necessarily indicate an invalid equation.[13] Other summary statistics are needed before an educated judgment is drawn, such as the standard errors around the parameters. In practical applications, however, these statistics are often subject to bias in the parameters.[14] When the disturbances are serially correlated, the variances and standard errors will be deflated, producing inflated $t$, $F$, and $R^2$ statistics, leading to possible erroneous inferences. Correcting for serial correlation is a crucial aspect of causal modeling, highlighting the importance of the Durbin-Watson statistic.

The Durbin-Watson statistic, otherwise known as the *d* statistic, is a test of the significance of serial correlation in the autocorrelation parameter.[15] The statistic is not applicable in cases with lagged endogenous variables—since the test was developed for nonstochastic vectors of explanatory variables. The Durbin-Watson statistic is no longer valid when there is a coincidence of lagged endogenous variables and autocorrelated disturbances. In that case, the statistic is asymptotically biased upward and no longer tests for autocorrelation. Thus, a nonsignificant *d* statistic does not preclude the possibility that OLS estimates are inconsistent when there are lagged endogenous variables in the equation. In the case of simultaneous systems, the same problem exists for the system endogenous variables. The endogenous (including lagged endogenous) variables must be replaced by instrumental variables (see Section VII below).

A common difficulty in statistical analysis is high collinearity among the explanatory variables. But we cannot rule out the use of a particular variable or the estimation of a particular equation simply because of multicollinearity. Other problems might arise (see Rao and Miller, 1971, p. 48). High intercorrelations result in the loss of precision, but the exclusion of a theoretically relevant variable on those grounds might exacerbate serial correlation in the disturbances.[16] Further, multicollinearity affects the precision of coefficient estimates rather than their values.

By far the most serious problem in data analysis and parameter estimation involves measurement error. It is customary to equate measurement error with faulty data or erroneous quantitative measures. While such problems are undoubtedly the source of much distortion in both analysis and results, it is important to broaden the conventional definition in at least two ways. First, specific estimates of the error in quantitative measures may be obtained from the measures themselves and incorporated as confidence intervals around the basic data for purposes of modifying the results according to the degree, magnitude, and direction of cumulated error.[17] The second extension of measurement error thinking lies in the structure of the underlying equation itself. Measurement error may be attributed to cases where the magnitude of the disturbance of the error term raises serious questions concerning the validity of the equation and the viability of the resulting specification. Ideally, the most desirable situation is one in which (1) errors in the quantitative measures are known to be negligible and (2) the disturbance term is small and exhibits no discernible trend of either positive or negative serial correlation. In practice, however, neither of these conditions hold: The extent of fault in the data is often not known, and the disturbance term exhibits significant serial correlation, especially in trend analysis of time series data.[18] The methods employed to minimize the effects of serial correlation will be discussed below.

## V. SIMULTANEOUS INFERENCE AND THE PROBLEM OF IDENTIFIABILITY

When there is mutual dependence among the endogenous variables, simultaneous estimation of the parameters is called for (see Christ, 1960, pp. 838–871). This set of procedures is more complex than standard regression analysis. Estimation in the classical regression mode involves one dependent variable and several independent ones. In the simultaneous case there are several jointly dependent variables. This situation generates an identification problem. This means that even if infinite data were available from which the reduced form of the parameters could be derived exactly, the values of the coefficients cannot be estimated without some *a priori* theoretical restriction upon the number of exogenous and endogenous variables in each equation.[19]

The addition of *a priori* restrictions to identify an equation is useful only if the same restrictions are not employed to identify other equations as well. However, such additional restrictions generally occur in the form of linear inequalities for the coefficients to be estimated. Inequalities of this nature add to the efficiency of the estimates but do not assist in the identification of a particular equation. Furthermore, if a model is not identifiable, manipulating the equations or the order of constituent variables will not assure identification—either a model is identifiable or it is not.

The problem of identifiability is thus closely related to theory and method, and is central to any model building effort. An equation is identifiable when a combination of *a priori* assumptions and empirical observations allows for a distinction between the parameters of the equation and those of other equations. By extension, a model is identifiable if each equation represents a distinct set of relationships. The problem is one of having sufficient *a priori* information to distinguish among equations. A certain minimum is necessary. Beyond that, any added information may be put to use. In *just-identified* equations there is exactly one way to obtain the "true" equation from the reduced form. In over-identified cases there is more than one way. In *under-identified* situations, where *a priori* information is insufficient to provide a discriminating service, there is no way in which the "true" equation may be recovered or distinguished from others in the same functional form. The model we have developed through experimentation and alternative specification is an *over-identified* set of equations: There is more than one way to retrieve the reduced form of each original equation. In practical terms, the problem is generally one of choosing among the various alternatives for an over-identified equation or model.

Standard statistical theorems, developed for the case in which the explanatory variables are treated as if they were fixed in repeated sampling, cannot be used when there are lagged endogenous variables. Furthermore, the coincidence of lagged endogenous variables *and* autocorrelated disturbances inflates the $t$ statistic and may signal erroneous inferences. Marked departures from the assumptions underlying the general linear model produce biased parameter estimates, often necessitating equally marked departures from standard regression procedures. The practical implications of serial correlation in simultaneous systems for parameter estimation are sometimes overwhelming.

## VI. SERIAL CORRELATION AND TIME DEPENDENT CORRECTION[20]

Because the nature of the serial correlation in the disturbances is often unclear—if it were known, the solution to the problem would be simply to adjust the parameter estimates accordingly—we are confronted with the necessity of estimating the nature of the autocorrelation parameter empirically and identifying the underlying stochastic process. This involves (1) isolating the systematic component of the disturbances, and (2) adjusting the independent variables so as to develop consistent estimates of the parameters.

Aitken has demonstrated that the generalized least squares estimator produces an unbiased estimate of the error variance when disturbances are autocorrelated (Aitken, 1935, pp. 42–48). But the estimate is not the "true" autocorrelation parameter $\rho$. However, it does have a known statistical distribution and in small samples it is consistent.[21] Our objective is to identify the theoretical structure of the time dependent parameter, and to determine its statistical properties.

Four disturbance structures have properties that are tractable and well known: (1) first order autoregressive process, where each error term $(u_t)$ depends only upon its previous value $(u_{t-1})$ plus a random component $(\epsilon_t)$; (2) second order autoregressive structures where $u_t$ depends upon $u_{t-2}$ and $u_{t-1}$, plus a random component $(\epsilon_t)$; (3) first order moving averages, where the disturbances depend only upon a series of temporally adjacent, independently disturbed, random variables, and hence all the disturbances prior to $u_{t-1}$ do not contribute to generating $u_t$; and (4) second order moving averages, where, for the same reason, the autocorrelation of $u_t$ is effectively zero with all terms beyond $u_{t-2}$. In the "real world," higher order structures are probably operative, but their statistical tractability amounts to a major computational problem, and it is not always clear that the benefits accrued by computational complexity are greater than the costs incurred.[22]

We seek to identify the structure of serial correlation parameters so as to obtain unbiased general least squares (GLS) estimates of the parameter values and their statistical variance and other attributes. A critical aspect of GLS involves a careful analysis of the residuals. There are at least two ways in which this can be done. The first, a correlogram analysis, involves retrieving the residuals from regression analysis and then correlating the first $t/5$ terms with the initial value of the residual, generating empirical values. These empirical values are then compared to the "theoretical" values that would be expected from a particular autoregressive structure. The second way, applicable only for autoregressive processes, involves regressing the residuals ($u_t$) upon their previous values ($u_{t-1}$ for AUTO1 and $u_{t-1}$, $u_{t-2}$, for AUTO2,) and observing the statistical significance of the two equations and the value of the Durbin-Watson statistics. In applied analysis, however, it is often difficult to distinguish moving average processes from autoregressive processes that dampen off sharply (see Hibbs, 1972, p. 51 and Hanna, 1960). There are also difficulties in determining whether the discrepancy between the theoretical autocorrelation parameter and its empirical counterpart is significant rather than attributable to noise. Identifying the structure of serial correlation and making appropriate adjustments amount to an important aspect of any such investigations.

## VII. INSTRUMENTAL VARIABLES AND GENERALIZED LEAST SQUARES

As noted earlier, OLS yields inconsistent parameter estimates in dynamic models with lagged endogenous variables and serial correlation in the error term. The OLS residuals are no longer the "true" underlying disturbances, in that $Y_{t-1}$ has a tendency to co-opt the systematic component of the disturbances.[23] This results in an upward bias for the coefficient of the lagged endogenous variable and a downward bias for the other exogenous or explanatory variables, frequently leading to erroneous inferences. This was a particularly serious problem in our investigations since determining the effects of the previous year's military allocations upon next year's budget amounted to an important aspect of our research. For this reason we must find ways of compensating for expected distortions.

One important assumption of least squares is that the errors are uncorrelated with the co-terms and uncorrelated with each other.[24] To meet this assumption, instrumental variables—which are assumed to be uncorrelated with the error but highly correlated with the original co-terms—are created. The constructed variables are linear combinations of the original terms and, therefore, assumed to be uncorrelated with the disturbances. They can thus be used to estimate the coefficient of the original equations. The *original* data, and not the constructed terms, are used to calculate the residuals (Eisner and Pindyck, 1972). Good instruments must have the following properties: (1) they must be truly exogenous and, in theory, uncorrelated with the disturbances, as a lagged endogenous variable usually is not;[25] (2) there must be no simultaneous feedback loops connecting the equations to be estimated with the equations explaining the potential instrument; (3) the disturbances in the equation to be estimated must not be correlated with the explanatory variable.

One question remains: Should the time dependent correction be made before or after the instrumental variable substitution?[26] In the analysis reported below we have followed the algorithms implemented in TROLL by undertaking generalized least squares first, then the instrumental variable substitution. But we have tested empirically for the differences that are yielded when the reverse procedure is employed; that is, first the instrumental variable substitution and then generalized least squares, and have found no significant differences for the model in Table 12.1. Several rounds of generalized least squares rarely produce theoretically meaningful results. For this reason, if an initial use of GLS does not appear to correct for serial correlation adequately, respecification is definitely called for.

In sum, the correction for the coincidence of lagged endogenous variables and serial correlation involves a two-stage instrumental variable substitution and the use of generalized least squares. If we treat lagged endogenous variables as endogenous, then a consistent estimate of the equation can be obtained using an instrumental variable estimator with current and lagged exogenous variables as instruments, provided the system has a sufficient number of exogenous variables. This estimator is robust against all forms of autocorrelation in the disturbances, but not against serial correlation in the explanatory variables. In this case, it becomes necessary to estimate the structure of the disturbance and then confront the problem of sequencing with respect to generalized least squares and two stage least squares, as noted above.

## VIII. SYSTEM CHANGE AND BREAKPOINT ANALYSIS

The occurrence of breakpoints and problems relating to the estimation of system change and prediction beyond the break are central issues in model building and forecasting. Sharp shifts in dynamics may signify discontinuities in some underlying empirical realities (but they may well be quite natural regularities of other empirical realities). Often breakpoints indicate incompleteness of theoretical specification.

We can think of breakpoints either as sharp changes in slope, or as nonlinearities. Some shifts may signify discontinuities which may be directly included in the equation as dummy variables (as we have done when defining changes in rivaling powers).[27] The incorporation of a break directly in the analysis increases the fit between historical and estimated data and between historical and simulated dynamics.

In some instances the break results from quantitative changes. In others it results from qualitative changes. There are as yet no known methods whereby the particular points at which a significant shift has occurred may be identified precisely (other than costly and complicated iterative procedures). For this reason, the best alternative is to plot the data, then to hypothesize the occurrence of a break based on empirical observation and to test for its statistical significance. The Chow test is still the most appropriate significance test for breakpoints. Quasi-experimental techniques for coping with such problems provide additional perspectives upon these issues, but they are cumbersome and complicated.[28]

The Chow test, modified recently by Fisher, involves the comparison of a set of coefficients with those of another array of which it is a subset.[29] We have inquired into the statistical significance of differences among two sets of regressions, one yielding coefficients for the period as a whole, the other for a particular subperiod. Cases where a significant difference emerged provided important clues into system change or transformation. Phase shifts can be identified with systemic breaks. But breaks that are more in the nature of nonlinearities may not always be identified as such. The result is simply a "bad" fit that cannot be attributed to an underlying break, but rather to nonlinearities that are not specified in the functional form of the equation. A search for breakpoints also assists in identifying poor specification or areas of misspecification.

In sum, the analysis of residuals and identification of breakpoints becomes, much like sensitivity analysis, a critical aspect of the research enterprise.[30]

## IX. SIMULATION, FORECASTING, AND POLICY ANALYSIS

The next step in this analysis is to develop variable simulations of the system as a whole and to observe its behavior under various conditions. This is done in two stages: The five equations are simulated equation by equation (by employing historical values at each iteration in place of calculated endogenous variables), and then the entire system is simulated in simultaneous mode (by employing calculated values for all endogenous variables). A successful (single equation) forecast increases the probability of a valid simulation: a successful simulation almost certainly implies a successful forecast.[31] A forecast (of a single equation) is conducted independently of the other equations and its solution depends primarily upon the existence of historical values for the endogenous variable, period by period. A simulation involves the entire system of equations, solving for the jointly dependent variables without recourse to their historical observations. A completely self-contained structure is operative in a simulation, allowing a fairly controlled method of varying parameters and observing the implications for the system as a whole.[32]

The TROLL/1 facilities, upon which our simulation of the system of simultaneous equations was undertaken, calculate values of the jointly endogenous variables in the model over a period of time for which exogenous data are available, or for any subperiod therein. For simulation, four types of information are required: the structure of the model itself, initial historical (or known) values for the endogenous variables, data for the exogenous variables, and constant files (coefficients and parameters that have been estimated earlier).[33]

A dynamic simulation proceeds as follows: For a given model in which $Y$ and $Z$ are endogenous variables, and $A$, $B$, $X$ are exogenous variables:

$$Y_t = a_1 + b_{11}A_t + b_{12}Z_{t-1} + u_1.$$
$$Z_t = a_2 + b_{21}X_t + b_{22}B_t + u_2.$$

In the first period, $Y$ and $Z$ are calculated using exogenous values for $A_t$, $B_t$, and $X_t$, and an exogenous starting value for the endogenous variable $Z_{t-1}$. In the second period, $(t + 1)$, $Y_{t+1}$ and $Z_{t+1}$ are computed using exogenous values for $A_{t+1}$, $B_{t+1}$, and $X_{t+1}$ and the simulated endogenous value for $Z_t$ from the previous period. Historical values for the endogenous variables are no longer employed. This procedure then continues, calculating the endogenous variables from their simulated values during the previous period and the current value of the exogenous variables. It must be noted that at each step subsequent to the initial $t$, historical values for the exogenous variables must be provided.

The solution for a variable at any given period is a function of a series of iterations in which all the equations in the block are solved and iteration values of the endogenous variables produced. Convergence criteria identify the point at which the iteration has reached a solution. Sometimes it is necessary to relax the convergence criteria in order to obtain a solution. A common procedure for checking the performance of the simulation when convergence is attained, is to examine the summary statistics, particularly percent error, and compare the simulated values of the endogenous variables with the actual, or known historical values.[34]

There are several sources of error in a simulation: First, the disturbance in period $t$ may not be accurately forecasted; second, there may be errors when estimating the parameters from observed samples (errors arising during the sampling period or measurement error), and third, there may be errors in forecasting the exogenous and lagged endogenous variables for period $t$.[35]

The basic procedure for undertaking simulation experiments is to resimulate the model with different inputs (or sets of information) from those used in the base simulation. Changes in parameters/values, in estimated coefficients, in endogenous variables, or in exogenous files may be made. To compare the results, we note the discrepancies between historical data output for the initial simulation and that for the modified simulation. For policy purposes it is necessary to modify the *coefficients* of key variables and then observe the effects upon the simulated output. This is done by changing coefficients one by one and obtaining the simulated output after *each* modification. Only in this way is it possible to identify the effects of policy changes upon the entire simulation.[36]

## X. SIMULATION, FORECASTING, AND POLICY ANALYSIS: THE BRITISH CASE

By way of providing some empirical reference to the above discussion, we draw upon recent investigations of the British case, 1871–1914. Table 12.2 presents summary statistics of the mean values for historical data, simulation values, and forecasts for each of the dependent variables in the system of simultaneous equations depicted earlier in Table 12.1 and, in diagram form, in Figure 12.1. These summary statistics (and the plots noted below) provide useful insights into the structure of the dynamic system modeled. Space limitations prevent an extensive commentary upon the political significance of these results. Some brief observations may be in order concerning the quantitative findings and their "real-world" implications.

In terms of colonial expansion, the simulation of British territorial acquisitions began slightly below the real-world level, but the two remained fairly close until 1880, when the simulation (and the single equation bootstrap projection) continued an upward trend and failed to replicate a slight drop in the real-world level. Between 1885 and 1889 the simulation and the real-world data were again close, but in 1890 and 1891 the simulation failed to replicate two sharp increases in the real-world level largely accounted for by British territorial gains in Africa. The two plots (and also the single equation bootstrap projection) were close from 1896 until 1899 and 1900, when the real-world level, reflecting additional British gains in Africa and elsewhere, underwent further sharp increases. In 1909 the simulation moved on above the real-world level.

In general, the simulations of military expenditures in the Great Power systems were quite successful. The British simulation ran slightly lower than the real-world expenditure levels during the 1870's. In the earlier years of this period, Britain fought the Ashanti Wars and was involved in other colonial conflicts, but in many respects the period was characterized by an 1874 declaration from the throne of friendly relations with all powers. Military expenditures remained fairly stable into the early 1880's. At this point, the simulation overtook the real-world levels of expenditure and became consistently a trifle higher. Between 1895 and 1900 the simulation overshot the actual levels consistently. By the outbreak of the Boer War in 1899 the simulation was registering well above real-world expenditures. A year later the two were close. Then the real-world data rose to a sharp peak in 1903, leaving the simulation behind but above its 1900 level. At this peak point, the single equation bootstrap projection was closer to the real-world data than was the simulation. After the 1903 peak, the simulation and the real-world data both dropped back and then rose more slowly to substantially the same 1914 level (see Figures 12.2 and 12.3).

Although the mean values for the simulations and forecasts of intersecting spheres of influence were close to the mean historical values, the percentage errors—calculated over the entire period—were considerable. Percentage errors take into account each deviation from the mean in a calculation of the overall percentage. Since the metrics involved were of small magnitudes—covering the range of the interaction scale from 1 to 30—any increment of deviation makes a greater impact on the percentage error calculations than similar increments in the cases where the metric itself involves large numbers—such as military expenditures in monetary values or colonial area in thousands of square miles.

The actual discrepancy or error between the historical alliance commitments and the simulated or forecasted commitments was small. But, because of the nature of the metric involved—low values and variance in the alliance commitment series—these minor discrepancies in absolute terms become

**Table 12.2** Some comparative statistics: Historical data, simulation, and forecasting. The British case, 1871–1914.

| *Variable* | *Historical Mean* | *Simulated Mean* | *Mean of % Error: Simulation* | *RMS of % Error: Simulation* | *Forecasted Mean* | *Mean of % Error: Forecast* | *RMS of % Error: Forecast* |
|---|---|---|---|---|---|---|---|
| Lateral pressure (Colonial area: sq. mi.) | 10,968,400 | 10,919,900 | −0.206 | 3.354 | 10,920,400 | −0.204 | 3.308 |
| Intersections (level: scale 1–30) | 12.989 | 12.896 | 73.917 | 211.261 | 12.988 | 72.705 | 264.524 |
| Military expenditures (1906 US $) | 212,392,000 | 211,742,000 | 1.563 | 24.396 | 211,856,000 | 0.934 | 27.762 |
| Alliance commitments (number) | 1.568 | 1.578 | −15.627 | 27.270 | 1.581 | −11.645 | 34.829 |
| Violence behavior (level: scale 1–30) | 20.364 | 20.419 | 67.101 | 276.158 | 20.364 | 70.664 | 307.747 |

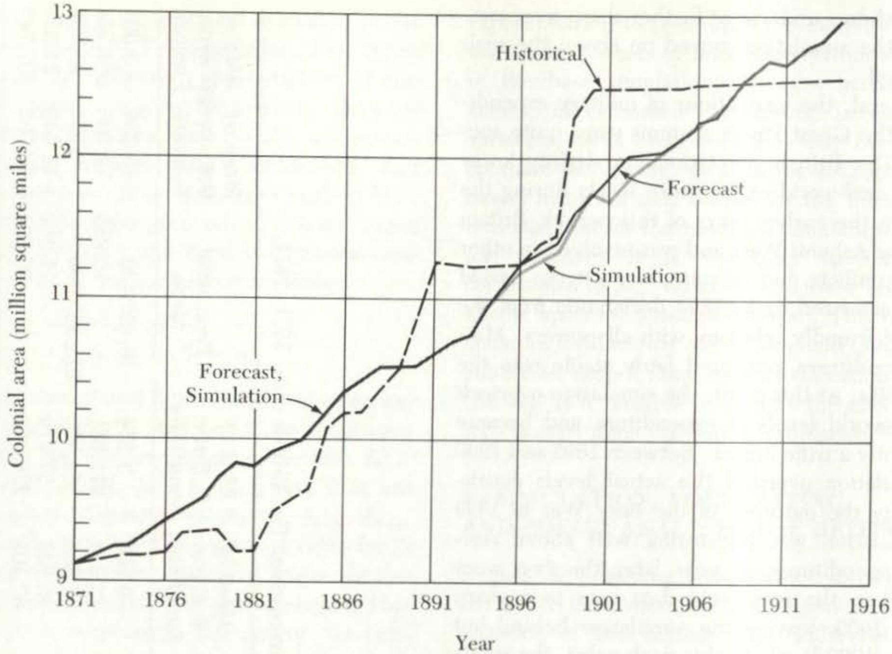


**Figure 12.2** Simulating lateral pressure: British colonial areas.

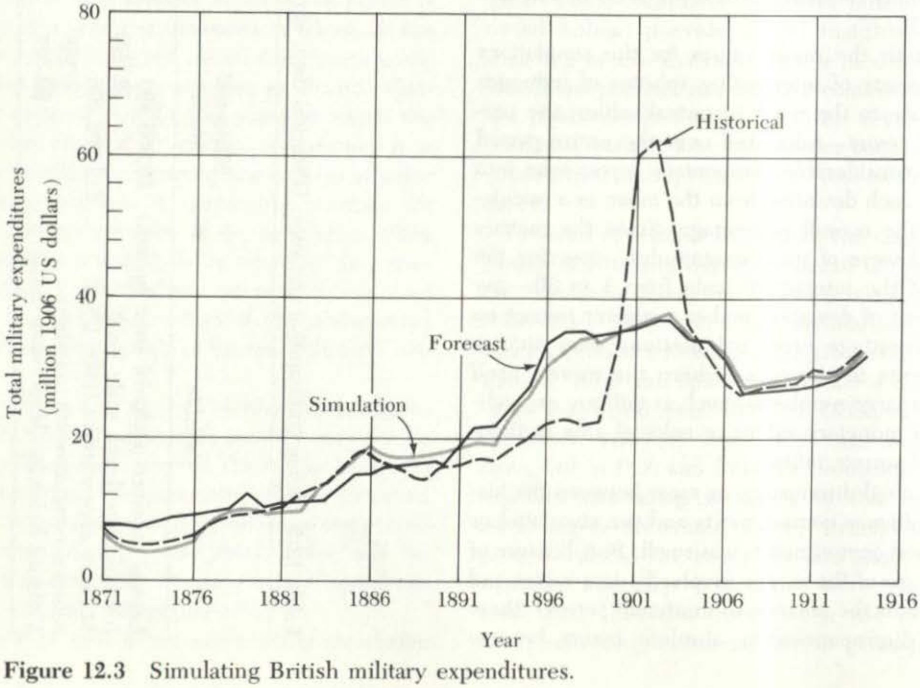


**Figure 12.3** Simulating British military expenditures.

major ones in percentage terms. In such cases, we can only observe these two sets of statistics and draw the appropriate inferences. Since the actual error between historical and simulated alliance commitments was very small, we find it reasonable to conclude that our simulation of these dynamics captured much of the underlying processes. Such an inference is reinforced by the high congruence between the actual or historical changes in alliance commitments and our simulation of these changes. The correspondence between the two is almost perfect. As much cannot be claimed with respect to percentage change over time, however. But although the correspondence between actual and simulated percentage changes in alliance commitments is not as good as in the case of actual changes, the degree of fit is still within bounds that define a fairly successful simulation.

A similar assessment may be made with respect to the results of the simulation of prevailing levels of international violence: There was a high level of congruence between the actual level of violence—as measured by scaled interaction data—and the simulation and forecast of these levels. The actual error between simulation and forecast, on the one hand, and real-world data, on the other, was negligible, but the percentage errors were considerable. Again, much as in the cases of the intersection and alliance variables, this outcome is due to the nature of the metrics involved. Changes in the violence behavior of the powers were also extremely well replicated, both in terms of simulating the violence variables within the five equation systems and in terms of simulating violence as a single equation forecast. In each case the artificial replication coincided closely with the real-world data. But the year-to-year percentage changes were not reproduced as satisfactorily as the actual changes.

A successful simulation model should do more than enhance our understanding of the dynamics of a system and the interdependence among its components. Once such a model is developed and its parameters estimated from empirical data—the values being robust and the coefficients statistically significant—we must still address ourselves to the

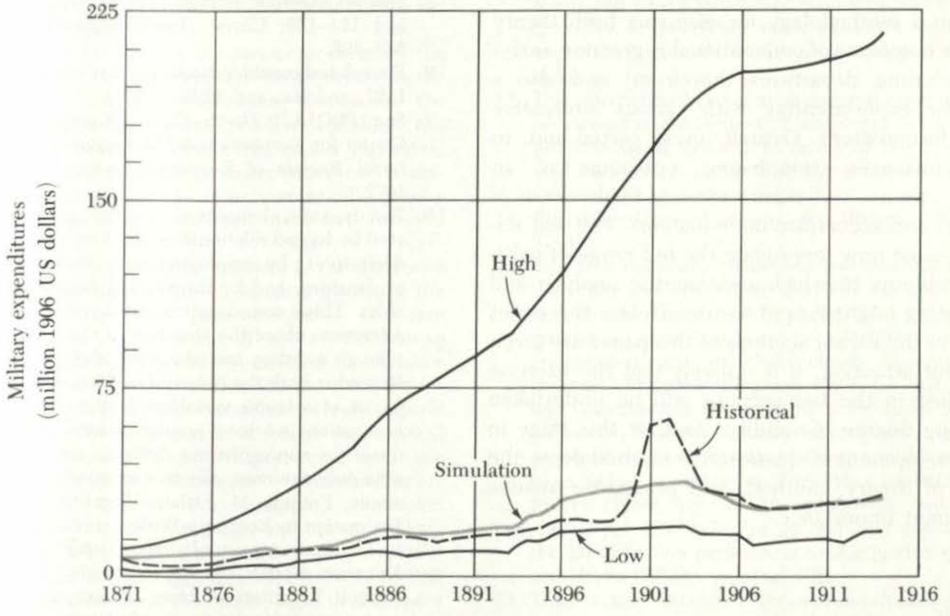


**Figure 12.4** Policy experiment: Explosive change. The impact of changing the coefficient for military expenditures $t$-1 (in the military expenditure equation) upon British military expenditures.

"so what?" query. By allowing us to raise questions of a "what if" or "if . . . then . . . " nature, a viable simulation should identify critical intervention points where policy changes (alterations in coefficients) will yield specific future outcomes.

By modifying the parameters in each equation and observing the changes in the behavior of the dependent variables, it is possible to draw inferences concerning real-world equivalences and expected behaviors. Although even a summary discussion of our policy analysis for the British case cannot be presented here, suffice it to add that the entire system was much more sensitive to *upward* swings in the dynamics under consideration than to downward swings. In other words, the dynamics in question were imbedded, seemingly, in explosive tendencies that surfaced with any slight upward changes in key parameters, whereas the system did not respond as dramatically to counterbalancing downward changes in the same parameters (see Figure 12.4).[37]

Such findings bear witness to the complexities of decision making and indicate the counterintuitive tendencies and behavioral characteristics of many large social systems. This type of experimental application of econometric analysis to political inquiry provides a methodology for assessing both theory and the outcomes of conventional regression analysis (including departures therefrom) and also a basis for experimenting with various alternative policy formulations. Overall, these partial and, in some instances, nonobvious outcomes of an "if . . . then . . ." nature serve as further tests of a model and accompanying equations. Political scientists must now investigate the full range of political problems to which econometric analysis and forecasting might be put to use. Unless the issues raised in the earlier sections of this paper are given sufficient attention, it is unlikely that the exercise described in the last sections will be undertaken with any degree of validity. And, at this stage in the development of quantitative methodology, the issues of theory, method, and procedure assume paramount importance.

## NOTES

1. See, for example, Deusenberry, *et al.* (eds.) (1965 and 1969).
2. Dynamic modeling, which is current in econometric analysis, can be used for political inquiry to provide (1) an aid to understanding political dynamics, (2) a tool for simulation, and forecasting political behavior and outcomes, and (3) a guide to the choice of public policy. The crucial test of a model lies in its internal and statistical validity. Its prime usefulness is to make forecasts and compare the forecasts with actual historical values as a means of understanding how systems behave. For a survey of the development of econometrics as a field of inquiry, see Klein (1971), pp. 415–421. For an instructive application of econometric analysis to political inquiry, see Kramer (1971).
3. See, for example, Blalock and Blalock (1968) and Ando, *et al.* (1963).
4. Although the broad lines of our investigations are common in econometric analysis, we have found that applied econometrics is not always consonant with econometric theory. In many cases we have also found that the problems confronting us—such as the coincidence of lagged endogenous variables and serial correlation in the disturbances—are raised in econometric texts as critical problems, but rarely are sufficient guidelines or practical direction provided to assist in resolving such issues. For this reason, our approach has been highly exploratory, and the solutions we have adopted amounted to practical applications of theoretical arguments. Since there are, as yet, no clear cut solutions to problems such as these, much of what we have done is both controversial and experimental.
5. See, for example, Johnston (1972), especially pp. 1–8 and 121–176; Christ (1966), especially pp. 1–15, 243–298.
6. For related considerations, see Fennessey (1968), pp. 1–27, and Rao and Miller (1971).
7. See TROLL/1 *User's Guide*, Computer Research Center for Economics and Management Science, National Bureau of Economic Research, Inc., June, 1972.
8. The dynamic elements in a model are usually generated by lagged relationships, by first (or higher order) derivatives, by employing endogenous variables as explanatory, and by introducing random shock variables. These considerations are important in drawing inferences about the structure of the system of equations in question and about the ability of the system to predict both the behavior of the model and the behavior of outcome variables. In the course of our investigations we have employed each of these procedures for approximating dynamic systems. Here we note only the most effective approaches. See, for example, Franklin M. Fisher, "Dynamic Structure and Estimation in Economy-Wide Econometric Models," in Deusenberry, *et al.* (eds.) (1965), pp. 590–635. Dynamic models can be constructed by employing explicit functions of time, linear approximations, exponential functions, quadratic trends, first and higher order differences, distributed lags and spectral analysis. The result is a system of equations in the correct form, whose parameters are subject to probability error associated with the inference procedure

used. We solve the estimated equation of the model in order to obtain an estimate of the reduced form. An earlier version of this analysis was undertaken with the use of rates of change variables on both sides of the equations. In that case, we have found that the resulting parameter estimates were surprisingly fragile throughout.

9. The necessity of *a priori* specifications, endemic to the question of causality, is predicted on two considerations. First, these specifications must allow the investigator to develop a particular system of equations, and to identify the dependent and independent variables, and the nature of the inequalities. This initial specification in itself constitutes an operational statement of theory, however vague, inarticulated, or implicit it may be. Second, *a priori* information is necessary for the distinction of one equation from another. Information of this nature generally constitutes restrictions on the coefficients of the variables (where some are set at zero) and on the nature of the random or disturbance term. Without the specification of zero coefficients for *some* variables in *each* equation, there is no way to distinguish one equation from another. See Fisher (1966), Chapters 1 and 2.
10. For a theoretical treatment of data, see Coombs (1964).
11. For conditions of identifiability, see Fisher (1966).
12. The formulae for the statistics discussed below can be found in any standard econometric text. Here we are concerned primarily with the problem of inference. See, for example, Johnston (1972); Christ (1966); and Rao and Miller (1971).
13. The smaller the variance of a parameter estimate, the less sensitive the estimate will be to errors in the dependent variable. Furthermore, the smaller the correlation among the independent variables, the higher the precision of the regression estimates. However, computation precision does not necessarily guarantee that the most theoretically precise estimation procedure has been used. See Rao and Miller (1971), p. 24.
14. The "bias" of a parameter estimate is the difference between the mean value of the distribution of the estimate and its "true" parameter value. Bias may also result from the omission of relevant variables in the equation. But this will not increase the variance of the estimates of the coefficients, nor does the introduction of superfluous variables severely impede the precision of the estimate. Although no statistical tool is a substitute for good theory, some errors are likely to have greater consequences for robust inferences than others. For example, regression coefficients with the wrong sign indicate most likely that some misspecification has taken place, or that the variables are not appropriately defined, or that we are mistaken about the "right" sign, or that there is an interactive effect that has not been taken into account. It is often difficult to identify the "real" reason for a "wrong" sign. See Rao and Miller (1971), pp. 27–35. "Precision" seeks the minimum variance estimate, regardless of bias. As a summary statistic, the mean square error provides importance to bias and to precision:

$$\text{MSE} = \text{Var}(\hat{\beta}) + [\text{Bias}(\hat{\beta})]^2.$$

When the estimated equation is the "true" equation, ordinary least squares provides the minimum variance unbiased estimate. See, also for example, Kendall (1954), pp. 403–404.
15. Durbin and Watson (1950 and 1951); also see Johnston (1972), pp. 250–254. See also Section VI of this chapter.
16. The precision of the parameter estimate depends upon the serial correlation parameter as well as upon the process generating the independent variables. Ordinary least squares is still unbiased in the presence of serial correlation, but it does not have minimum variance. If we can identify the structure and value of the autocorrelation parameter, then by an appropriate transformation of the variables we can use ordinary least squares to provide minimum variance estimates. This is appropriate only in the single equation case where simultaneous effects are not thought to operate. When the dependent variables in the equation are also serially correlated, then the bias depends also on the parameters that generated their serial correlation. And when the variance in the error term is not constant, ordinary least squares does not produce the best linear unbiased estimates. See also Schink and Chiu (1966), pp. 36–67. We have attempted to attain high precision (by seeking sharp and robust parameter estimates) and minimize bias (by respecifying each equation to account explicitly for the effects of separate independent variables.)
17. The conventional use of measurement error may thus be viewed in the context of confidence intervals, the problem being defined in terms of the absence of vital information rather than the presence of known error in the quantitative measures.
18. For related considerations, see Blalock (1965), pp. 37–47.
19. The two necessary conditions for identifiability are the order and rank conditions. For the order condition to hold, there must be at least $M$-1 independent restrictions in an equation where $M$ is the number of endogenous variables. This is clearly an exclusion restriction. The rank condition stipulates that at least one nonvanishing determinant of the order $M$-1 can be formed from the ordinary least square structure of an equation, corresponding to the variables excluded by *a priori* specification from that equation. See Fisher (1966), pp. 39–42 and 60–62; and Fisher (1959), pp. 431–447. For an excellent exposition of the identification problem in multiequation systems, see Hibbs (1973b), Appendix III.
20. This section discusses the nonsimultaneous, nonlagged endogenous case. See below for the simultaneous and/or lagged endogenous case.
21. See Hibbs, Jr. (1972) for a derivation of the residuals in the generalized model, and Goldberger (1965), Chapter 5, for a derivation of the disturbance vari-

ance. See also Fisher (1970a) and Rao and Miller (1971), especially pp. 70–74. For a comprehensive treatment of issues in time series analysis, see Hannan (1960) and Anderson (1942), pp. 1–13.

22. Econometricians have focused primarily upon first order autoregressive structure (due to the case of computation) and, as a result, a general tendency to assume that the world is of a first order autoregression pervades much of the econometric literature. In our investigations, however, we have rarely encountered an AUTO1 structure. An AUTO2 usually appears to be a suitable trade-off between complexity and accuracy. For empirical analyses, see Rao and Griliches (1969), pp. 253–272, and Orcutt and Winokur, Jr. (1969), pp. 1–14.
23. See Rao and Miller (1971), Chapter 7. The true error does not depend on the value of the independent variables, but the residuals do. Residuals, therefore, reflect the properties of the independent variables as well as the errors and the effects of left out variables. If errors are homoscedastic and random, the residual corresponding to a particular value of the independent variables ($X_n$) has a statistical distribution with zero mean and small variance. See Christ (1966), pp. 394–395; Goldberger (1964), pp. 232–235; and Johnston (1972), pp. 208–242.
24. In cases where collinearity among the instrumental variables is high, principal component transformation produces a new set of variables that are orthogonal linear combinations of the original variables. The new variables are so ordered so that each variable explains as much of the remaining variance of the original variables as possible. In such cases, it is possible to use a smaller number of variables while still accounting for the major fraction of the variance explained by the original equation. We employed a principal components solution only when it was not possible to create instruments in any other way due to excessive collinearity among the instruments.
25. The choice of instruments is theoretically intuitive. A predetermined list can be refined in two ways: (1) through the use of principal components. This method reduces multicollinearity since the components are mutually orthogonal, and principal components summarize the information in the list of instruments; and (2) through structurally ordering instrumental variables by first establishing a list of preference ordering of instruments relative to a particular explanatory term; then regressing the endogenous variable on the instruments in differing combinations to determine whether an instrument further down the list has an effect or whether its contribution is simply using up a degree of freedom; the constructed elements of $Y_t$, together with the elements of $T_t$, are then employed as instrumental variables in constructing Y. See Rao and Miller (1971); and Eisner and Pindyck (1972).
26. There are differences of views concerning this ordering, and hence, the residuals to be employed when undertaking an instrumental variable substitution. When combining time dependent corrections, generalized least squares, instrumental variables, and two stage least squares, it is not intuitively obvious which residuals, and at which stage, should be used in calculating the relevant statistics for evaluating the parameters at the final stage. On the one hand, it is argued that when generalized least squares and instrumental variables are combined, the transformed residuals should be calculated without the substitution. On the other, it is maintained that substitution should first take place, and then the time dependent corrections performed. In the latter case, the proper asymptotic variance-covariance matrix must contain the instrumental variable substitution. In the former, it does not. See Hibbs (1972) and Wallis (1967), for the single equation case, and Eisner and Pindyck (1972). For other ways of dealing with this problem, see Fair (1970).
27. For other illustrations, see Theil (1970), pp. 103–154.
28. Chow (1960), pp. 591–605; and Campbell and Stanley (eds.) (1966).
29. In our analysis, we have compared the residuals generated by the regression of the $n$ observations with those of the $m$ observations (given $k$ number of variables) and it becomes clear that in instances where the deviations are great, the $F$ test picks these and registers them as statistically significant, thereby rejecting the null hypothesis. See Fisher (1970a), pp. 361–366; and Johnston (1972), p. 206–207.
30. For purposes of experimentation and increasing our understanding of the model we have developed, we found it desirable to identify and test for breakpoints (using the Chow test) in cases where the coefficients were estimated with and without the uses of instrumental variables. We found, generally, that there were no significant differences in terms of the results obtained with and without the use of instrumental variable substitution.
31. Econometricians generally talk of forecasting when the endogenous variable in each equation is replaced by historical values at each point, and of simulation when the coefficients, the exogenous variables, and the error terms together with the jointly dependent variables are employed to generate an artificial replication of the entire system. This replication is commonly referred to as simulation. In looser parlance, we often talk of forecasting as simulation beyond the existing data that was used to estimate the coefficients initially. Clearly, that is not the usage intended in this paper.
32. See Naylor, *et al.* (1968), pp. 184–200 for an informative study.
33. The following observations are based on Chapter 8 of the *TROLL/1 User's Guide*, June, 1962.
34. If the object is short-term forecasts, multicollinearity need not be a necessary drawback. If some of the explanatory variables are multicollinear, the prediction interval obtained will be large. By eliminating some collinear variables, one can reduce prediction interval for a given value of the included independent variables. But the actual outcome will change

very little. Pragmatic forecasts and simulation would be indifferent to the extent of collinearity while sophisticated ones will not. Both will make similar forecasts and the errors will be very similar. See Kuh and Meyer (1957), pp. 380–393.

35. The root mean square of the error (RMS) is the most important summary statistic in indicating how well the simulated model tracks empirical observations. Other important summary statistics include the mean of the forecast and the mean of the simulation, the percentage error for each, their mean errors, the mean of their first differences, and the mean of their percentage first differences. These statistics, presented further along, are compared with counterpart statistics for the historical data, and the discrepancy indicates the extent of fit between actual observations and simulated values. *TROLL/1 User's Guide*, 1972, pp. 8–28.

36. This procedure assumes that changes in one coefficient will not lead to counterbalancing changes in others.

37. See Chapter 17 of Choucri and North (1975) for a detailed discussion of the experimental analysis.