"ECONOMIC FLUCTUATIONS, STRUCTURE, AND MEASUREMENT"

(THREE ESSAYS ON 'SOFT' ECONOMETRICS)

BY

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ABSTRACT

Research practices of econometricians evolve rapidly. They have changed as much in the last two decades as they did in the preceding two decades when the primacy of the simultaneous-equation model was established. Much of what econometricians do now does not fit into the framework indicated by a preoccupation with statistical inference. Given this background, it is convenient to distinguish soft econometrics from its hard counterpart, which stresses mathematical statistics. This distinction is derived from a parallel in Cox's discussion of the behaviour of statisticians and of the nature of data. To clarify the substance of soft econometrics and to reveal its occurrence, three essays are provided. One essay considers the potential use of econometrics in long-wave research. A second essay looks at the impact of changing views on the concept of structure, while the final essay deals with the interaction of soft econometrics and significant issues of measurement. The concluding comments emphasize the complexity and variety of modern econometrics.

RESUME

Les pratiques de recherche des économétriciens évoluent rapidement. Elles se sont modifiées autant au cours des deux dernières décennies que durant les deux décennies précédentes lorsque fut établi la primauté du modèle d'equation simultanée. Une grande part de ce que font aujourd'hui les économétriciens ne s'intèqre pas dans le cadre de travail découlant de l'importance accordée à l'inférence statistique. Compte tenu de ces considérations, il convient de distinguer l'économétrie douce de sa contrepartie plus rigide, laquelle met l'accent sur les statistiques mathématiques. Cette distinction provient d'un parallèle qu'expose Cox entre le comportement des statisticiens et la nature des données. Dans le but de clarifier l'essentiel de l'économétrie douce et d'en démontrer la pertinence, trois essais sont présentés. Un essai porte sur l'usage potentiel de l'économétrie dans le recherche sur les grandes ondes. Le second essai examine l'impact du changement de perspective sur le concept de structure alors que l'essai final traite de l'interaction de l'économétrie douce avec les questions de mesure importantes. Les remarques de la conclusion mettent l'emphase sur la complexité et la diversité de l'économétrie moderne.

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CHAPTER ONE

INTRODUCTION: "SOFT" ECONOMETRICS

The Econometric Society was established about a half century ago. Its founders sought to bring together many of the strands of earlier empirical research by economists and to combine the extension of such research with concomitant developments in economic theory, mathematics, and the new discipline of statistics. Frisch (1933), who is usually accepted as the first user of the term "econometrics", provided a definition in his editorial for the first issue of the journal Econometrica: "Experience has shown that each of these three viewpoints, that of statistics, economic theory, and mathematics, is a necessary, but not by itself a sufficient, condition for a real understanding of the quantitative relations in modern economic life. It is the unification of all three that is powerful. And it is this unification that constitutes econometrics". Subsequent restatements and adjustments to this definition during the early phase of econometrics are summarized in Tintner (1953). Even today it is widely agreed that the subject draws from a diversity of sources. Mizon (1979), for example, expresses the common view: "The econometrician requires knowledge of, and expertise in, economic theory, mathematics, statistical theory, economic statistics, economic history, and computational methods". What seems to have been lost in the last half century is the stress provided by Frisch and the other founding fathers on unification.

Suppose we had collected a sample of recent statements concerning the nature of econometrics from textbooks now used in econometrics courses in British and American universities. Then we would undoubtedly find that, while some note is often given to Frisch's definition, there is clear movement away from his even treatment of the subject's components and their combination and toward an unbalanced stress on statistical estimation and inference at the expense of other components. This shift is even more striking when the contents of the textbooks are appraised. These contents seldom do justice to the definitions of econometrics presented there unless the latter are preoccupied with statistical issues. Many topics of deep concern to econometricians in past years are wholly ignored and there seems to be developing a clear separation between econometric theory (with an emphasis on the theoretical properties of the estimators and the nature of confidence statements) and applied econometrics (which covers everything else). This split is harmful and unnecessary. We feel that we should adopt the distinctions within the subject that agree with common practice in other related subjects. In particular, we might note the interaction of statistical theory and practical considerations and the use of qualifiers "hard" and "soft" that appear in Cox's (1981) presidential address to the Royal Statistical Society and in Moser's response to the address.

There seems to be some value in using the term "hard" to describe theoretical proofs in statistical estimation and inference. Thus "hard econometrics" would refer to firm results that cannot be challenged, such as BLUE properties of the least-squares estimators in the Classical Linear Model or the formula for the asymptotic dispersion matrix of the two-stage least-squares estimator in the Simultaneous-Equation Model (SEM) of the Cowles Foundation. The term "soft" might be used to cover situations in which simple firm statements are difficult to justify. It would cover many areas of imprecision and the practical factors that usually dominate empirical research. Thus it might be associated with choice of measures or economic indicators as affected by economic theory, sampling frames, costs of collection, mathematical consistency, and intended use. It could also be applied to two areas of statistical and computational interest that are currently popular; namely, Monte-Carlo or simulative experiments and Tukey-style data analyses. Both contain subjective elements open to question and leave final decisions with tentative character. Simulative experiments could, for example, be markedly affected by a restricted choice of hypothetical models and by technical questions such as the generation of appropriate perturbations.

This dissertation is concerned with some of the softer areas of econometrics. It takes three topics and considers their present status in relation to research in earlier times or to pressing current problems. They illustrate areas of empirical research that have stimulated considerable discussion and are chosen, in part, for their lasting significance. The first topic stems from recent fears of world-wide economic depression.

The second takes up the dissatisfaction with large-scale economy-wide econometric models and the increasing preference for time-series approaches. The final topic addresses the present crises affecting the acceptability of economic indicators. All involve aspects of soft econometrics. Treatment of the topics is isolated in the sense that the three essays can be read separately and in any order. The underlying connection, essentially the characterization and illustration of soft econometrics, is often muted. Where a topic reveals significant issues primarily of interest to the economists rather than the econometrician (as for example in the consideration of long-wave theories) we did *not* feel restricted to forego discussion. This lack of discipline does not come from acceptance of Mizon's assertion that "economics is but a part of econometrics". Rather it stems from the recognition that the line of demarcation in our discipline is sometimes difficult to fix and is an unnecessary barrier if the issues that arise have multiple dimensions.

Chapter Two contains an account of a revival of the long-wave. Since Kondratiev first brought the notion of cyclical fluctuations with long periodicity to a wider audience in the decade before 1935, the "long wave" or "long cycle" has enjoyed a checkered career. Variants of it have been discussed whenever adverse economic conditions occurred and have been ignored at most other times. Sometimes the proponents of wave theories have been dismissed as cranks while, at other times and by other economists, they appeared perspicacious. Since the onset of the oil crisis, and in conjunction with declines in productivity, investment and growth and increases in inflation, unemployment and radical economic adjustments, there has been a revival of interest in the long wave. We chose this revival as the basis for our first essay. It is as far removed from hard econometrics as one can move without completely leaving data-processing. Our account of recent discussions omits almost all of the debates that preceeded 1970 including the research that first explored the feasibility of spectral methods to determine the existence of waves. We have arranged the chapter in a number of sections, each of which deals with some particular corner of recent literature. Thus, for example, Forrester and Rostow have developed views considered in two of these sections. We feel that Forrester's technique of system-dynamics models can best be treated as part of soft econometrics rather than as a rival to "econometric estimation". The long wave is an ideal area for determining the merits and problems associated with this technique. Most of the sections in this chapter involve econometric issues such as instability, evolutionary modelling, inadequate data, and the hazards of data adjustment. They also have striking significance for economists.

The content of Chapter Three is influenced by a feeling that econometricians frequently lack awareness of their own earlier literature. They often fail to place ongoing research in the context of a long perspective. This is revealed by our exploration of a period of two decades, roughly 1940 to 1960, during which the SEM was introduced and became the primary framework within which aspects of hard econometrics were established. More specifically, we assess the decline and potential demise of structural estimation as represented in the discussions among econometricians during this reference period and, also, more recently as part of the ongoing reappraisal of current econometric methodology. Some attention is given to the concepts of autonomy, identification and exogeneity and their implications. The organization of the chapter again involves distinct sections. We begin with accounts of some present difficulties, periodization, and particular historical issues before giving special attention to seven sub-topics. These are the partition of variables into endogenous and exogenous categories; the primacy of the probability approach; interactions among variables (affecting interpretation of estimated multipliers, for example); false constraints as indicated by Liu; proximity; purpose; and normalization and asymmetry. Within this list, we can locate major sources of softness.

Our next chapter deals with the third major dimension of value in clarifying the incidence and nature of soft econometrics. The fallibility of economic indicators is insufficiently recognized and has many significant implications for specification, estimation and interpretation of fitted regression models. In part, this lack of recognition is due to the existence of a gap between the providers and users of data but there are other factors involved too. After some preliminary comments on the public appetite for key economic indicators, we look at the "small area" problem and rotation or response biases for economic measurements derived from sample surveys before turning to the dramatic structural impacts of environmental transformations. These latter are illustrated by financial innovation and the impact of socio-economic and demographic shifts on the labour market. The search for new monetary aggregates, the concern for apparent instability being experienced with money-demand relationships and the advocacy of re-weighted unemployment rates are the more visible features of these transformations within econometrics.

Many of the problems identified in the early section of the chapter are unavoidable. However, those found when we consider data preparation and prior adjustment are often the consequence of explicit choices. The Yule-Slutsky effect in long-wave research, some aspects of seasonal adjustment, and the view of data as an intermediate good are used as illustrations to explore potential sources of softness. Then we look at revisions to governmental statistics with attention given to the compounding influence of estimation techniques. The Almon lag is an excellent source of model sensitivity to revisions in national income and expenditure data even though it is widely used in calculating the RDX and CANDIDE economy-wide econometric models for Canada. The final sections of Chapter Four deal with temporal intervals and aggregation, the use of quasi-structural models to supplement the incomplete provision of data on states and duration in the dynamic representation of labour markets, and with the soft concepts that arise in economic theory and lack operational firmness. All contribute to the general impression that empirical research involves much more than the formal theories of our textbooks.

The final chapter collects views that are expressed in earlier ones. It summarizes some of our discussions and ends on an optimistic note with the assertion that softness does not mean that econometrics is irrelevant. Rather softness means that the subject-matter of econometrics is wider and more complex but also consistent with maturity after a half century of evolution from the establishment of the Econometric Society.

References

COX, D.R. (1981), "Theory and General Principle in Statistics", Journal of the Royal Statistical Society, Series A, Vol. 144, Part 3, pp. 289-297 (including Discussion).

FRISCH, R. (1933), "Editorial", Econometrica, Vol. 1, No. 1, pp. 1-4.

MIZON, G.E. (1979), Econometrics: A Ray of Hope or Dismal Too?, Discussion Papers in Economics and Econometrics, University of Southampton, No. 8109.

TINTNER, G. (1953), "The Definition of Econometrics", Econometrica, Vol. 21, January, pp. 31-40.

CHAPTER TWO

THE REVIVAL OF THE LONG WAVE

The economic environment in many countries has been extraordinarily disturbed since the onset of the oil crisis over a decade ago. Both professional literature and the popular press have been full of references to crises in productivity, inflation, unemployment, international flows of trade and funds, and other areas of economic and social concern. In recent years, the term "depression" has been taken out of the drawer into which it was placed out of sight after the apparent success of economic stabilization policies in the period following the end of World War II. Use of the term is increasingly evident and it has ceased to be dismissed outright as a sign of unrealistic eccentricity. As might be expected in the light of economic developments, a substantial loss of confidence in particular economic policies and their theoretical bases can be seen. The emergence of the "new" classical macroeconomics, essentially a revival and transformation of earlier business-cycle theory in association with developments in econometric models of multiple time series and in computer software, was made possible by the downturn in economic activity and, especially, by the long delay that occurred prior to substantial recovery.

If we look at economic literature during the 1970s, there is a clear drift to more radical attitudes. The oil crisis and the problems in international adjustments that accompanied it were first seen as transitory phenomena. Such confidence had been built up in the two decades before the onset of the crisis that it was assumed the economy of the United States (and, consequently, the economies of other developed countries) would recover quickly from a single and sudden transfer of real income and upward push to prices. The decline of productivity in the U.S. brought forward debates that are reminiscent of the controversies surrounding the "climacteric" of British productivity at the end of the nineteenth century as recorded in Phelps-Brown and Handfield-Jones (1952), Lewis and O'Leary (1965), and Coppock (1956). The problems of an "aging economy" and its loss of economic and political hegemony were no longer associated solely with the British decline and became linked with current developments in the U.S. This revived the search for historical analogies and the "periodization" of economic experience.

The inevitable consequences of this search for historical models during a period of markedly reduced growth in economic activity in many developed countries were apparent. They involved both more interest in the Great Depression of the 1930s and more critical appraisal of economic indicators. The former is illustrated by Wilson (1980), Capie and Collins (1977), and Phelps-Brown (1972) while the latter is clear in Mayer (1979). More references are provided in an appendix. Kindleberger, Balogh and others raised forgotten issues concerning the failure of leadership for the world economy, hot money flows and the transfer problem using the experience of 1930s and 1940s to consider the changing environment of the 1970s. With respect to economic indicators, the concern was two-fold. It dealt first with the accuracy of measurement fifty years ago and then with "evidence in favor of Keynesian stabilization policies" as the basis for successful performance of the U.S. economy in 1950-1970.

The most surprising event in this unsettled stage was the revival of interest in the long wave. It must be clear from the developing concern with the two depressions we have cited that some contact with long wave literature was inevitable. However, the amount of attention accorded to the theories of long waves far exceeded what might have been expected. The end of decade of the 1960s had seen the acceptability of a homogeneous process generating economic development in the long term at a very low ebb. As we indicate below, the use of spectral analysis to explore this process was drifting without much encouragement toward a negative verdict. The change in position revealed by Abramovitz (1961, 1968) is the most prominent expression of this decline in acceptability although he uses the shorter Kuznets cycle as his frame of reference. The outcome is also plain in Gottlieb (1976), where the "bonds of historical context" are taken to outweigh the similarities that might underlie "some average type or representative long swing". Gottlieb, from the perspective of his interest in the U.S. construction industry, indicates a prevalent view: "On balance our judgement - reached early in the sixties - held that the feedback process between construction and the total economy and between residential building and real estate markets had altered in so many fundamental respects that, given our purpose of isolating and illuminating this feedback process, it seemed worthwhile to drop out of review the thirty years after the Great Depression trough (1933)..." Given the immense current interest in the long wave, as reflected in the many entries to the bibliography that we provided at the end of this chapter, it is clear that there has indeed been a pronounced revival of the long wave.

There are two basic traditions in the long-wave literature. One of these stays within the time frame initially provided by Kondratiev while the other prefers the shorter interval that was first advocated by Kuznets. We have used the Kondratiev basis since many of the recent contributors to discussion in this area have mentioned this basis explicitly. This choice permits easier classification of these contributors according to the issues stressed by them. It also facilitates the use of this literature to clarify the theme of "soft" econometrics that we seek to explore and which is the primary focus of our discussion.

Kondratiev has been described as "a mediocre technician with a lurid imagination" (International Currency Review, 1979, p. 28). Until recently, the primary accounts in English of his research are his two papers (1925, 1935), the survey and commentary of Garvy (1943) and the interesting historical account of Day (1981). Garvy and Day discuss the conflict within the community of Russian economists and the outright rejection of his views prior to his expulsion from Europe. Summaries of the characteristics of particularly relevant recent schools of thought on the existence and nature of long waves are provided by Eklund (1980), Freeman et al. (1982), Kahn (1979), van Duijn (1983) and Wallerstein (1979) but the best way to approach this body of literature remains Garvy's paper. This contains many elements that were picked up by later writers.

The presence of Rostow among these writers that we shall cite is hardly surprising. He is readily associated with the dating and periodization of growth patterns within individual countries, the search for common (if staggered) patterns among groups of countries, and the detailed examination of the British historical experience. He and Lewis took the price side of the original Kondratiev formulation and recast it in terms of relative prices. This was combined with a translation of the earlier discussions of differential developments in agriculture and industry to international trade flows between developed capitalist countries and developing countries. Rostow has also used the long wave as a framework for assessing the difficulties in extending economic theory to an evolving, dynamic environment. He, as Kondratiev (1925) and some of the Russian commentators cited by Garvy, sought to interpret and refine concepts of long-run equilibria as distinct from trend and cyclical movements in economic time series. We have attempted in the next section to describe Rostow's efforts to use the long wave. It is significant that, in his most recent activity, he has left behind the simple tabulations of aggregate indicators that characterized earlier years. Instead he has moved toward computer modelling and simulative experimentation. He has succumbed to the attractions of soft econometric approaches.

Many critics have claimed that Kondratiev did not provide any theoretical basis for long waves. Garvy's survey shows that this is not true. A basis was offered by Kondratiev and quickly rejected by most of readers. This basis has several strands, of which two are given special significance. Kondratiev took from Marx the reinvestment cycle in capital expenditures and made this the essential backdrop of his theory. He took Tugan Baranowsky's theory of free loanable funds as the other major element in his theory. The stress on investment has been given fresh life by the persistent efforts of Forrester and his associates, primarily at the Massachusetts Institute of Technology, to use system-dynamic techniques to develop their "National Model". This body of recent research is considered in our second section. It reflects Kondratiev's views in two other areas, both of which are contentious among econometricians.

The first area is a rejection of the emphatic use of exogenous variables in the Cowles Commission's simultaneous-equations model. Forrester prefers to follow Kondratiev's lead with integration of few exogenous elements. His advocacy of multiple feedback loops is also markedly different from the conventional treatment of lagged endogenous variables in the SEM context . However his approach can be treated as a straightforward extension of econometric developments that sought to explore the non-linear growth models of Goodwin. These developments abandoned the search for analytical solutions that had been associated with linear models whether deterministic or stochastic. They turned instead to the computer and sought analog alternatives. The synthesis of dynamic behaviour for non-linear economic phenomena was their primary concern while the slow development of computational software was their principal constraint. A typical example of this earlier effort is provided by Strotz, McAnulty and Naines (1953).

The second area in which proponents of the system-dynamic approach mirror Kondratiev's views lies in the stress that they attach to qualitative features rather than to determinate quantitative forecasts. In this respect, tuning constants are manipulated to yield cyclical patterns that can be interpreted. This method is thus quite contrary to conventional econometric methods which emphasize parametric estimation and which have substantial data requirements. It should be recognized as one response to the integration of the computer into econometrics with an attendant disturbance to many past patterns of thought and research techniques. We ought to point out that the "new" classical macroeconomists seem to share both some of this qualitative emphasis and the reluctance to accept the possibly excess reliance on exogenous (external) forces.

Our third section takes up the discussion of technology in the determination of long waves. Technological change has long been acknowledged as a major source of structural instability and growth. It would seem especially difficult to identify (in any systematic way) technological change or innovation with regular long cycles of fixed periodicity. Nevertheless, this was one aspect of Kondratiev's discussion that Schumpeter (1939) sought to magnify with the use of his notion of creative destruction implicit in the evolution of capitalist economies. He provided a strong impetus to direct studies of innovative investment, their clustering within particular time periods and industries, and the diffusion of their influence from an historical perspective. Without any doubt, he established the practice of periodization for long-wave chronologies and the familiar linkage of each long wave with particular innovations or rapid transformations in specific "leading" sectors of industrial activity. Recent neo-Schumpeterian approaches to long waves fall into two broad classes, both of which we shall cite in this section.

One class embraces simulative methods rather than simple statistical analyses or data tabulations. Here the approaches owe much to the efforts of Nelson, Winter and their associates to shift static economic theory toward evolutionary growth models for individual industries or firms in the tradition of Schumpeter but with the general use of computer experiments. We shall note the particular attempt of Hartman and Wheeler to extend this potential framework for long waves to include trend phenomena such as the emergence of growth sectors, shifts in relative prices and attendant terms of trade, and the well-known fluctuations in migration and population characteristics. The second class has narrower emphases on innovation, inventions and fundamental S-curves of economic development with researchers using standard regression techniques and clustering algorithms. Major contributors within this class include Freeman, Mensch and Kleinknecht who deal with political implications of this Schumpeterian view of economic change as well as with technical issues. We should also recognize Kindleberger's consideration of "aging economies" and Rostow's treatment of the imminent decline of the U.S. automobile industry and its satellite industries. Both involve Schumpeterian elements.

The fourth section below returns to Kondratiev's concern with loanable funds and, more generally, considers the question of credit crunches, ease of credit availability, and institutional adjustments in the credit industry. As we will observe in a later chapter, the transformation of payments systems and the possible change in systemic uncertainty have dramatic impacts on the acceptability of economic measurements and their involvement in economic models. Minsky has been a prominent voice in the debate on financial fragility with special concern for its potential consequences for the economic well-being of the U.S. economy. Our discussion of Minsky's themes and two recent accounts of Hester and Wojnilower which deal with similar matters is restricted to a long-wave interpretation linking depression with financial fragility. This connection can be traced back to the treatment of debt depressions by Irving Fisher. We could have supplemented this discussion by consideration of Balogh's treatment of the safety net and the transfer problem or Kindleberger's persistent attempt to clarify the role of international lender of last resort but this would take us too far away from both long waves and soft econometrics. Minsky began his fight for recognition of the developing financial fragility in the United States before the onset of the oil crisis. The attention given to his views only grew appreciably after economic performance worsened. Their implications for econometrics have yet to be fully expressed.

Returning to Garvy's survey, it is clear that Kondratiev's views were rejected by an overwhelming majority of Russian economists. It is, thus, worth noting that the long wave has experienced as large a resurgence among researchers on the left of the political spectrum as it has elsewhere. There are at least three distinct "schools" of thought among researchers with a perspective from the left. To isolate the principal features of the three schools, we note separable views of Mandel, Gordon, and Wallerstein in the fifth seotion of this chapter. All of these writers seek to combine the ideas of Kondratiev with those of his critics in the light of developments both after the period considered by Kondratiev and before it. Mandel persists with the emphasis on investment although he modifies it to deal with certain adverse comments by Trotsky. He stresses over-production and a falling rate of profit as mutually reinforcing factors of crisis in capitalist economies. To accommodate obvious features of post-World War II experience, he deals with cyclical reinforcement of the bargaining position of labour. Perhaps the greatest deficiency in his work is his excesses in trying to explain too many of recent developments within his simple framework. He makes no attempt to use econometrics in any of his papers on this topic.

Gordon seems to combine features drawn from a variety of sources. He avoids some of the apparent weakness of the reinvestment theory by distinguishing different classes of fixed capital and by downplaying the prominence of the impact of lumped investment except as might accompany waves of infrastructural innovation. Two important areas of special significance in his formulation of long-wave developments are a superior treatment of the labour market (taking account of a less rigid view of the exploitation of labour) and a clear statement of the irreversible adjustments that follow each major crisis in the trough of the Great Depressions. In this latter area, he mirrors the comments of one of Kondratiev's critics, Studensky (1930, cited in Garvy) who asserted "...each new wave of technical change results in the shifting of the economic system to a new, qualitatively different, stage of organization and technique, with a resulting number of important socio-economic changes."

In the third view from the left, Wallerstein and his associates have launched an imposing programme to describe the evolving form of the "World Economy". Kondratiev had stressed the international character of long waves and his list of their empirical characteristics included both economic features and social upheavals such as wars and revolutions. Wallerstein, taking a much longer perspective that includes historical shifts since 1450, developed a framework giving special attention to the changing interactions between the core of the capitalist economy and the periphery of resource-based regions. The geographical spread and evolving focus of the world economy implies that capitalism should not be identified with individual countries so national data may not be the appropriate level of aggregation for economic information. Further, Wallerstein and his associates identify the roles of joint economic and political power with the concept of "hegemony" which is opposed to an alternative of rivalry. We only touch briefly on the perceived features of the world system. Following in part a hint by Chase-Dunn, we are more interested in the feasibility of developing appropriate econometric procedures for the analysis of this system.

In the final descriptive section, we have looked at econometric elements in the remaining literature on long waves. It should not be surprising if we give attention to persistent attempts by some researchers to apply spectral analysis to long-wave theories. These occur despite the outcome of studies undertaken in the 1960s and the general awareness of the hazards of induced waves that could stem from the use of moving-average transformations to smooth erratic observations in time series.

On this Yule-Slutsky effect, Garvy's survey is again a useful backdrop although the standard treatment in long-wave theory is due to Howrey. After this final section is complete, we offer some modest conclusions and take a brief second look at the "soft" econometrics involved in each of the earlier sections. Details on references are contained in two appendices. One of these lists papers and books published during the 1970s and 1980s as part of the revival of the long wave. The other appendix cites references of historical significance or of relevance to the issues that we raise.

Rostow

Kondratiev believed that the movement of capitalistic economies entailed two cycles. One of these repeated in about ten years while the longer wave had a period of about fifty years. These waves were components in a broad pattern of historical continuity, a systemic moving equilibrium, which was contrary to the notions of systemic disintegration held by some Marxists. Rostow, as a prominent opponent of such notions, might then be favourably inclined to consider the views of Kondratiev. His work contains many elements derived from Schumpeter's treatment of economic change although, recently, he writes of the "Marshallian Long Period" as his theoretical framework with price as its major focus. More specifically, this period allows for "very gradual or secular movements of normal price, caused by the gradual growth of knowledge, of population and of capital, and the changing conditions of demand and supply from one generation to another"

[Marshall, cited by Rostow (1980b), p. xiii]. Rostow suggests that the process of economic growth can be conceived as "a 'moving equilibrium', embracing irreversible changes in technology, the supply of basic commodities, population, tastes, and the quality of entrepreneurship" (Rostow, 1980b, p. xiv). This radical perspective is markedly different from both mainstream Keynesian and neo-classical theory. Indeed he places himself, using the analogy of Young and Clapham, in a separate category. "[The] not quite empty box I occupy should be designated Keynesian-Monetarism-Plus. The plus is no more or less than the linking of macro-analysis to the systematic treatment of the Marshallian long period, increasing returns and all." This is elaborated in Rostow (1982).

In the Marshallian long period, many of the restrictive assumptions that inhibit economic adjustments in shorter intervals are relaxed. This provides a distinctive and novel representation of supply conditions with some interesting features. First, increasing returns can be linked to technological innovation as implemented in particular industrial configurations. Second, the S-curves of economic development may be associated with the incidence of such innovation in leading sectors or countries (early-comers) and with the diffusion of new techniques to other sectors or countries (late-comers) prior to the onset of a slowdown of growth with maturation. This diffusion process is explored by Rostow and Fordyce (1978) using both aggregate indicators, such as growth rates in per capita real income, and the stages-of-growth methodology that was introduced a quarter century ago.

by Rostow. Third, supply in the long term is affected by large discontinuities especially in the flows of agricultural products and raw materials. Finally, the intertemporal transformation of tastes implies that demand and supply are not independent while both are subject to irreversible changes.

These features are in marked contrast to those generally found in conventional econometric models of, say, agricultural supply. In such models, the "long run" emerges from the estimated comparative-static responses that may be derived from distributed-lag formulations. The estimates presume the acceptability of a host of statistical assumptions including stationarity, stability, continuity, homogeneity of population and parametric fixity. None of these fit the Marshallian long period as envisaged by Rostow. It is therefore not surprising to find him unable to use standard regression procedures in his analysis and unwilling to accept the empirical estimates for elasticities and dynamic responses that are common in our professional literature. Too often such estimates are based on short-term economic models (with their implicit restrictive assumptions forgotten) or on ad hoc adjustments to such models (that may introduce fundamental inconsistencies such as static expectations). The contribution of economic theory to the structural specification of stable econometric models is obviously deficient for the complex dynamic world that Rostow seeks to handle. It is possible to conclude from this deficiency that econometric estimation has no role to play in the exploration of long waves. This is, in our view, excessive as we hope to demonstrate. Significantly Rostow rejects only "high powered econometric techniques" in this context. His rejection is quite consistent with a positive contribution from the application of soft econometrics. We shall return to this shortly.

According to Garvy, Kondratiev first arrived at the hypothesis of long waves by studying price movements and later endeavoured to supplement his statistical evidence by value and production series. His basic approach was to inspect patterns in these data after scaling them with measures of population and removal of a secular trend. Deviations from this trend were also smoothed with a moving-average filter to eliminate the shorter cycle that we cited earlier as part of his two-cycle scheme. [The consequences of this preparation of data, which was influenced by Persons (1919-1920) and the quantitative school at Harvard University, are discussed by reference to the Yule-Slutsky effect in our sixth section.] Clearly visual inspection of transformed series permits a considerable degree of subjective judgement especially in the timing of directional changes in economic activity.

Kondratiev used twenty-five time series from a number of countries and found three cycles in his tentative periodization. His conclusions included both prices and real variables. "Regarded as a whole, economic reality represents a non-reversible process, in which progress is accomplished by stages. But the individual economic elements, while they are thus subject, as parts of the whole, to a non-reversible process of variation, in some cases developed, when considered separately, through a reversible process. A considerable group of economic elements, such as, on the one hand, prices, rates of interest, and wages, and, on the other hand, percentages of unemployment and business failures, exhibit processes of reversible, wave-like variations" (Kondratiev, 1925, p. 583). Several of his early Russian critics were prepared to accept long waves as revealed in the movement of prices and interest rates but they rejected their presence in real variables. Rostow believes that Kondratiev cycles are real phenomena but their periodization is to be fixed by shifts in price indicators.

Rostow and Lewis (1978, 1980) point to the changes in relative prices of major commodities that are induced by real changes. In developing this shared approach, they treated money supply as passive with velocity flexible. This provoked a monetarist reaction as illustrated by Bordo and Schwartz (1980), who dispute the appropriateness of the Rostow-Lewis hypotheses for the period of British climacteric at the turn of the century. The criticism by the monetarists involved econometrics in its softest form. Ignoring the need to appraise the adequacy of their simple statistical framework, they fitted the prices of agricultural commodities in the United States as dependent on a constant, the ratio of money stock to real net national product and residual from a supplemental equation for velocity (itself linked to real per capita net national product and the rate on commercial paper). This illustrates, however poorly conducted in this case by Bordo and Schwartz, one feasible use of econometric methods. They can be applied to explore the consistency of particular components of long-wave theories with developments in certain historical periods of special significance. All we have to remember here is the inherent softness that arises from incomplete theory and fallible data and that must be expressed in the questioning of statistical assumptions and in the subsequent implications for statistical inference.

Another interesting aspect of Rostow's work is the stress that he attaches to disaggregation. Although, at various times, he combines data at the national level with data at lower levels of aggregation, it is quite clear that his theoretical framework envisages, in a fundamental way, heterogeneity both intertemporally and spatially for countries, sectors, industries and firms. This implies the presence of a difficulty in the definition of the populations from which data may be treated as samples. When he suggests that his framework "does not lend itself easily to high-powered econometric exercises" (Rostow, 1978a, p. xlii), he quickly attributed part of this problem to the fact that "data are not available in appropriate forms". This problem of linking data to probabilistic populations is implicitly recognized when he cites the componental areas of his theoretical approach where econometric methods might prove fruitful. "I do believe ... that a good many of the issues raised here can be isolated and pursued with more rigorous statistical methods; for example, the relative role in trend-period phenomena of impulses from relative prices, leading sector retardation, and migration; the comparative aggregate and sectoral analysis of pairs of countries at similar stages of economic growth" (Rostow, 1978a, p. xlii). The isolation is to be seen as a search for manageability and homogeneity.

It seems appropriate to delay further consideration of most other aspects of Rostow's treatment of long waves to our later sections. This is particularly so for Schumpeterian elements and periodization. However we should note, in passing, that Rostow's use of price indicators to guide periodization yields a markedly different chronology in the post World War II era as compared to those accepted by other contributors. This is apparent in Table One; which is presented and described in our third section. The Schumpeterian elements fit conveniently in the general discussion of innovation, invention and evolutionary aspects which are also described in that section. Our final present interest in Rostow's work is the shift towards synthesis and computer simulations. This is best represented in Rostow and Kennedy (1979), where a two-sector neo-classical growth model is extended to capture some of the elements of the Marshallian long period. The use of computer modelling is an important change in Rostow's methods, although his record is consistently favourable to data analysis and some statistical manipulations ever since his early work on cycles with Schwartz over thirty years ago. His new tool retains some of the flexibility that might be lost with standard regression analysis.

The use of a neo-classical growth model by Rostow requires further explanation. It appears to be inconsistent with his frequent dismissal of such models as being unsuitable bases for the representation of historical experience. Indeed a concomitant of his advocacy of the Marshallian long period is the provision of a list of perceived deficiencies in growth models due to their excessive abstraction from actual historical complexity. Resolution of the inconsistency is found in the ambiguous notion of trend. Mitchell (1927, pp. 212-213) characterized the unsatisfactory treatment of trend by economists. "Secular trends of time series have been computed mainly by men who were concerned to get rid of of them. Just as economic theorist have paid slight attention to the 'other things' in their problems which they suppose to 'remain the same' so the economic statisticians have paid slight attention to their trends beyond converting them into horizontal lines. Here little is yet known about the trends themselves, their characteristics, similarities, and differences." Little has changed in the last sixty years to cause this view to be amended. Trends are almost never explained while deviations from them generally form the focus of analysis. In some recent long-wave theories, the trend is separated from the Kondratiev wave and treated as a smooth curve to be associated with "equilibrium", "normal capacity" or a similar vague concept.

Rostow and Kennedy use the neo-classical growth model to generate a trend against which the long wave is revealed. Rostow (1978a, p. xl) presents his view of this succinctly.

"What we observe ... are dynamic, interacting national economies, trying rather clumsily to approximate optimal sectoral equilibrium paths, tending successively to undershoot and overshoot those patterns." These deviations by their serial correlation identify the long wave or "trend periods". The basis for reference or the starting point in this treatment is a dynamic equilibrium path in a closed economy with neither sectoral imbalances nor misallocation of investment. It is an "abstract, disaggregated, moving equilibrium". This view finds a parallel expression in the comments of Oparin, an early Russian critic of Kondratiev, which are cited by Garvy (p. 209): "In order to measure swings in economic life, it is necessary to establish a scheme proper to the phenemenon. Swings in economic life can be scientifically analyzed only as departures from the schematic equilibrium. Consequently, swings in economic life must be measured, not in relation to a previous time period, but to an established equilibrium system".

The choice of trend line is not a trivial matter either theoretically or empirically. Different decompositions of economic time series frequently indicate alternative patterns. Thus, if we have an insufficient basis for choosing a particular trend, there occurs considerable softness in numerical estimates and the patterns that they contain.

This softness enhances the acceptability of computer experiments and simulation, relative to conventional regression approaches, since they permit flexibility in specification and facilitate comparisons rather than simple elaboration of a given model. Rostow and Kennedy set the values of the parameters in their neo-classical growth trend without using any statistical procedure. Then they introduce a series of disequilibrating complications that represent individual elements in Rostow's theory such as (i) "lumpy" (clustered) increments in the capacity of one sector with various lags in recognition, exploitation and gestation; (ii) changes in the average rate of technical progress to represent technological innovation; (iii) war effects; (iv) stagflation due to frictions in money supply and wages; and (v) restrictions on the availability of raw materials as found in the debate on limits to growth.

Although restrained in scope, this effort is an excellent illustration of soft econometrics as expressed in experimental or simulative form. Rostow and Kennedy (p. 33) deserve the last word here. "It is quite possible to set up models that capture elements in the trend behavior of the world economy over the past two centuries: these models can illuminate lucidly processes not fully developed in either conventional economic history or contemporary economic theory. ... But they fall considerably short of re-creating economic history. And we take it that it is the duty of model builders to be conscious of what their fabrications fail to embrace as they are of the piece of reality they capture". By trend behaviour, they mean the wave-like patterns around the equilibrium growth trend rather than this trend alone.

Forrester

The shift toward computer experiments and simulative approaches involves a radical challenge to the working practices of many econometricians. We face a disturbed situation in which large-scale economy-wide models are often first estimated by conventional methods and then simulated as part of a process involving validation and verification. Many model builders seem to ignore the apparent deficiencies of individual equations provided their complete systems yield satisfactory dynamic simulations. This attitude is consistent with the widespread occurrence of ad hoc adjustments that override initial estimates for fitted equations. The relative stress on estimation and simulation varies considerably between researchers and creates heated debates amongst them. A typical criticism of "equation neglect" is provided by Ando's response to the model of Evans in Meyer (1981). This response is quite restrained by comparison with the general difficulty experienced in reconciling Forrester's system-dynamics approach, which stresses simulation and dismisses the traditional focus on estimation, with the alternative formulations that are found in most econometric textbooks. This difficulty is a primary focus of our treatment of Forrester's explorations of long waves. We return to it after considering some of the significant elements of his efforts, beginning with the interaction of computing and non-linear systems of equations noted earlier. Recognition of this linkage places Forrester's research in the mainstream of economic theory, the lineal descendent of a substantial body of attempts to explore non-linear growth models, rather than on the eccentric fringes of this theory. His own style of presentation and that of his opponents often obscure this place.

Koopmans (1957, p. 215) reveals the importance of earlier uses of analog and digital computers in this context. "These studies constitute a rehabilitation of the numerical example as a tool of analysis in situations where general mathematical analysis is either too difficult, or shows the outcome to be highly dependent on the numerical values of the model." Their most recent analogies are found in the experiments reported by Day (1982) which involve mathematical theories of chaos and reveal the hazards of making predictions. In one experiment with a neo-classical model of capital accumulation, for example, Day found that "when sufficient nonlinearities and a production lag are present, the interaction alone of the propensity to save and the productivity of capital can lead to growth cycles that exhibit a wandering, sawtooth pattern not unlike those observed in reality". Such fluctuations may be quasi-periodic but are not necessarily so. They are interspersed with erratic or chaotic trajectories which affect prediction and are, themselves, influenced by their sensitivity to small changes in the specification of both initial conditions and model parameters (tuning constants).

With this backdrop, Forrester developed his system-dynamics approach in the 1950s and has continued to extend its adoption in analyses of industries, urban regions, and national economies from the inception to the present. His view, excellently described by Meadow (1980), embraces ideas from control engineering (such as the concepts of feedback and system self-regulation), cybernetics (the nature of information and its role in control systems) and organizational theory within representations of complex, nonlinear and multiloop feedback systems. The spread of this perspective was substantially enhanced by the development of the DYNAMO software package, which permits easy computational manipulation of systems with nonlinearities and time delays despite their mathematical intractability.

The manipulative ease is a consequence of the imposition of a <u>closed boundary</u> for the system, essentially the counterpart of restricting exogeneity. All meaningful interactions are assumed to occur within this boundary. Forrester (1969, p.17) makes this explicit. "The cause and effect relationship between enviroment and system are uni-directional, whereas the internal elements are structured into feedback loops that cause the internal elements to interact. The environment can affect the system, but the system does not significantly affect the environment." Here the internal elements are essentially endogenous in the causal sense (rather than by the statistical definition discussed in Chapter Three and often found in simultaneous-equation models) while the environment is a collective term for exogeneous factors. The later are treated as random perturbations that "do not themselves give the system its intrinsic growth and stability characteristics" and, hence, appear contrary to the Wicksell-Slutsky-Frisch-Kalecki view (Howrey, 1972) where erratic shocks induce persistent cyclical oscillations even in stable models.

Forrester (1980, p. 573) has recently clarified his treatment of exogenous influences and their role in changing cyclical patterns in economic development. The system-dynamics approach generally starts with the identification and examination of the "deterministic central structure" of the system, its endogenous component. This is supposed to improve our understanding of the dynamic behaviour implicit in this component. Introduction of exogenous randomness may indeed activate damped oscillatory models such as business cycles and construction cycles. In Forrester's National Model, this randomness from modest exogenous shocks "will even feed through to change the successive shapes and periodicities of long wave". However system dynamics, as with other methods of dealing with nonlinearities and mixed difference-differential equations, loses much of its appeal when more extensive exogeneity is present. This use of a closed boundary is consistent with Kondratiev's insistence that long waves are endogenous. "In asserting the existence of long waves and in denying that they arise out of random causes, we are also of the opinion that the long waves arise out of causes which are inherent in the essence of the capitalistic economy." (Kondratiev, 1935, p. 42) Casual extra-economic circumstances and events (such as changes in technique, wars and revolutions, assimilation of new countries through the geographic expansion of the world economy, and fluctuations in gold production) which are listed by Kondratiev fit into Forrester's concept of the environment. They are thus outside the closed boundary of the National Model. They are unnecessary for the explanation of the long wave.

Unlike the typical reports on simultaneous-equation models, those of Forrester and his associates seldom list the equations that form their structure. Instead they illustrate linkages between endogenous elements with stylized charts. Diagrams representing circular loops of influence have long been used in economics. Patinkin (1973) presents some early examples while several generations of economists have been introduced to such loops in elementary textbooks such as that of Samuelson (1948). Forrester and his associates are perhaps unique for the central position in which they place circular loops. Their theoretical stance is dominated by them. Within the closed boundary of the system, feedback loops contain alternating levels and rates. The loops may be self-reinforcing and amplify disturbances or they may dampen them. System dynamicists combine such positive and negative feedback to construct their models. Thus, for example, oscillatory behaviour is connected with the presence of a negative feedback loop involving a time delay while S-curves of economic development stem from the nonlinear interaction of both types of feedback without time delays.

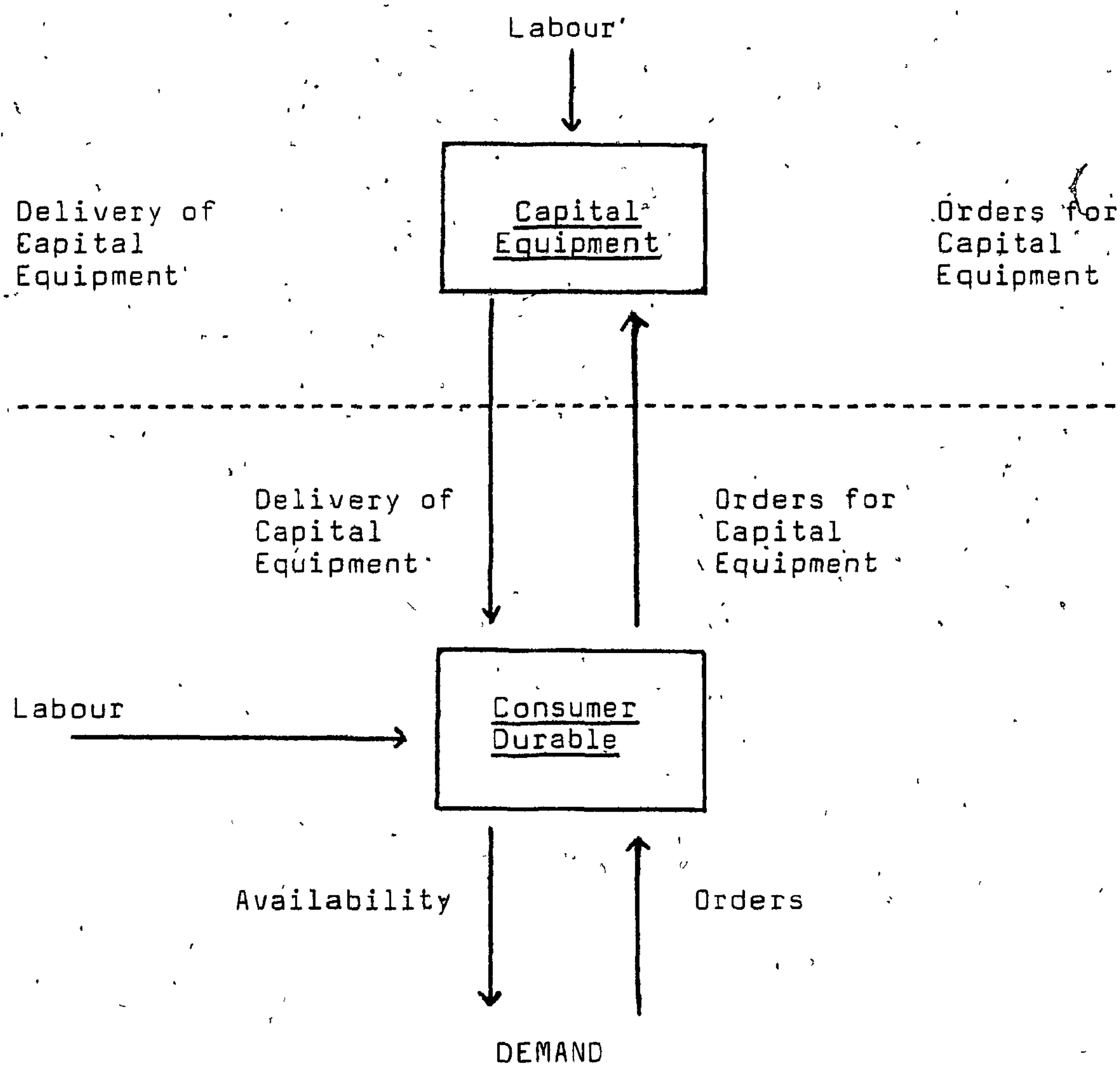
Before considering the particular chart with which Forrester frequently explains the long wave, it is appropriate to consider the implied lack of simultaneity in the system of loops for his approach. This lack of simultaneity is a major factor in the computational ease of DYNAMO software and, thus, is a significant element in the practical use of his approach. In its conceptual form, a system-dynamics model is a set of integral equations in continuous time. This needs to be converted into a discrete approximation for computing. The general habit of econometricians would be to use a quarterly (or monthly) time interval to fit the sampling frame of existing data. This is described by Rowley and Trivedi (1975, Section 3.6). Forrester advocates the use of a much smaller interval. This, when combined with the alternation of levels and rates, yields a framework quite distinct from the conventional one. Rates are only permitted to depend on the integration of rates and not on simple rates alone so that a system-dynamics model is serially recursive rather than simultaneous. Simulation is developed by updating each equation sequentially rather than solving all equations simultaneously, a considerable advantage for exploring nonlinear dynamic systems.

The choice of time interval is a sensitive area of contention. Many economic models use differential calculus and assume both continuity and differentiability at moments in time. The period of decision is seldom specified in the theoretical models so a major source of softness arises when they are to be converted into forms for either estimation or simulation. We have no basis for assuming that data will be collected at intervals which are suitable for matching with theoretical models. Thus we have few grounds for choosing between the interval advocated by Forrester and that fixed by collection agencies. The theoretical consequences of this shortfall in our scientific methods affect the treatment of flows and stocks for economic variables, the reliance on data-based approaches, and the inferences derived from particular formulations. It should not be surprising, therefore, to find that results from system-dynamics modelling provide challenges to many popular views. Contributions to Randers (1980) illustrate these challenges while the Forrester-Zellner exchange, that we shall discuss later, reveals the difficulty of finding a suitable format for cross-fertilization of competing views.

Figure One illustrates the typical use of diagrams containing circular loops for feedback. This particular chart is found in most of Forrester's discussions of long waves and represents a two-sector structure with consumer durables and capital equipment. With the linkages of the chart in mind, Forrester (1979, p. 94) provides the following argument. "Recent computer simulations using partial assemblies of the System Dynamics National Model suggest that a long-period cyclical behaviour can arise from the physical structure connecting consumer goods sectors and the capital sectors. A sufficient cause for a 50-year fluctuation lies in the movement of people between sectors,

FIGURE ONE

Forrester's Two-Sector Model For Consumer Durables and Capital Equipment



Source: Forrester (1979), p. 95.

Note: The Section above the broken line represents a "bootstrap" structure in the capital sector, which acts as a major anti-damping element and lengthens period of fluctuation.

the long time-span to change production capacity of capital sectors, the way capital sectors provide their own input capital as a factor of production, the need to develop excess capacity to catch up with deferred demand, and the psychological and speculative forces of expectations, which can cause over-expansion in capital sectors." This repeats Kondratiev's focus on capital accumulation but is much more complicated than the straightforward reinvestment hypothesis.

Since our concern is primarily with soft econometrics rather than with the fine detail of this argument, it is appropriate to focus attention on the way in which the system-dynamics model is used here. Forrester and his associates did not initially set out to find a Kondratiev-type wave or to establish its determinants. When their effort began, reinvestment was usually linked to stabilization and cycles of greater frequency. They wanted to consider a host of dynamic phenomena extending from the business cycle to the "life cycle of economic development", which might cover over two hundred years. Multiple simulations could be used to explore whether different model components can be linked to particular cyclical developments. Further simulations could reveal the sensitivity of these perceived linkages to model enlargements, parametric changes and other adjustments. The essential requirements for an experimental method are flexibility of simulation and simplicity of attribution from causes to effects. This is quite at odds with the process of taking a given structure and confirming the numerical range of parameters. Both approaches are fallible since we have little depth in our perception of long-term economic developments. Forrester cast out the false impression of firm structures and opted for a method in which softness is blatant. He reversed the normal order of model development which exaggerates both the contribution of economic theory to equation specification and the applicability of statistical procedures. This choice makes him highly susceptible to criticism and, perhaps, liable to excessive claims for his approach. System-dynamics modelling should be viewed as an additional tool that freely permits the imaginative elaboration of theories. Its value is limited to areas of economics that are poorly developed but the long wave is a prime example of such an area.

Forrester's experiments, which are generally well-documented, reveal how simulation strengthens impressions of causal linkages or dampens them. In Forrester (1979, pp. 94-97), for example, he considers the potential consequences for the long wave of amended models which include the household (consumption) sectors, the banking system, and the Federal Reserve System. His experiments therefore may be indicative of hypotheses that should be pursued further as well as confirmatory in a soft sense for some notions with current support. The creative dimension of system dynamics is also evident in Mass (1980), who stresses the need to give adequate attention to stocks as well as flows and, especially, to the interactions of stocks and flows in situations of dynamic disequilibrium.

Our final observations on Forrester's treatment of the long wave arise in the context of his acrimonious debate with more conventional econometricians as found, for example, in Forrester (1980). Before presenting these, it is appropriate to briefly digress and consider the characteristics of the new equilibrium theories of business cycles as propounded by Lucas and others. Although not concerned with long waves, these theories do share some features with system-dynamics modelling of which the most notable are criticisms of the treatment of exogeneity and their emphasis on qualitative aspects of cyclical developments. There are also considerable differences not least in the recourse to notions of equilibrium and in the choice of method. Lucas, Sargent, Sims and others frequently stress time-series models that are far removed from the system-dynamics framework. We consider the problem of exogeneity in the next chapter. With respect to qualitative conditions, Lucas (1977), for example, develops his equilibrium cycles in the framework of co-movements in economic time series and finds both that business cycles are qualitatively alike and that they are not necessarily associated with accurate conditional forecasts. He uses a criterion for model performance that is very like that of Forrester. "One exhibits understanding of business cycles by constructing a model in the most literal sense: a fully artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies." (Lucas, 1977, p. 11) This imitative behaviour could be modelled either by time series models with innovative errors and few exogenous elements or by system-dynamics models. Both are far removed from the familiar "structural" approaches. Lucas and his associates move toward the search for invariance and stability while Forrester emphasizes simulative convenience. Neither is concerned with the problem of finding "good fits" since this criterion may not give adequate attention to qualitative aspects of economic development.

The radical features of system-dynamics models are listed by Forrester (1980). We have already pointed to non-linearities, exogeneity, simulative ease, lack of simultaneity, time intervals, forecasts, qualitative criteria and the place of economic theory as areas in which his approach differs from that of many other econometricians. However, the primary focus of his dispute with conventional econometrics is not found in this list. It is found in his strong rejection of statistical inference for the soft contexts that he seeks to explore. This is made clear in Zellner's comment on this paper. Zellner finds some common practical features, all of which we would argue are sources of soft econometrics. "Many econometric model builders have used institutional, judgemental, and other types of non-numerical data in their model-building efforts. In addition, numerical data have been employed to check models' performance in simulation experiments and tracking performance. Further, in arriving at parameters' values or estimation, many model builders use judgement as well as data, usually informally in arriving at their parameter values. ...One difference between Forrester's approach and those of others, however, is that Forrester apparently does not make explicit use of formal statistical inference techniques, whereas other model builders attempt to do so." [Zellner, after Forrester (1980), p. 567]

Zellner would prefer Bayesian methods of integrating this prior soft information and a forecasting test for validating econometric models. Forrester rejects both as well as frequentist alternatives. The latter can be attacked, as in Senge (1977), for their sensitivity to imperfections in data and model specification but the fundamental difference with respect to statistics between system dynamics and conventional approaches is more profound than this criticism. We can begin with a stochastic formulation in which the model characterizing cyclical developments is dominated by its probabilistic features (as in conventional econometrics) or we can add random perturbations or innovations to a deterministic central structure after its dynamic implications have clearly been appraised (as in system dynamics). Choice between these rival approaches must depend on the softness of the real-world context. Both may be considered part of econometrics so that they appear complementary rather than antagonistic. This view is considered by Meadows (1980).

Returning to the long wave itself, Forrester identifies sequences in its evolution primarily by reference to capital accumulation with a peak occurred about a decade ago. The lowest point of the depression has slow growth in capital sectors and gradual decay in existing capital stock until replacement needs become apparent in these sectors. Recovery begins with the recirculation of the output of the capital sectors to their own inputs, or "bootstrapping" as Forrester terms this process. Then wage increases and labour shortages in consumer sectors provide further impetus to production of capital equipment in excess of replacement. Deferred needs stimulate overexpansion of capital sectors until their output cannot be fully absorbed and unemployment arises in these sectors. Finally deficient demand leads to rapid collapse of the capital sector which affects overall economic buoyancy and completes the cycle. The role of credit and liquidity is imprecise here. Forrester suggests that ignoring constraints on liquidity may inadvertedly accentuate the long wave in the absence of credit creation by central banks (the Federal Reserve). Credit from this source is linked to the overexpansion of capital plant and hence to "the creation of the long-wave mode". Such considerations require further experiments that have yet to be publically released.

The onset of recovery in the capital sector in this picture is obviously too simple. Similarly, then the translation of excess capacity into a cumulative decline needs refinement. These turning points and the dynamic processes linking them were illuminated by Schumpeter in his treatment of long waves. It is to the recent research in the Schumpeterian tradition with clarification and assessment of his concepts that we now turn. Although simulation remains an important research tool, the explanation of turning points and discontinuities leads to other dimensions of soft econometrics.

Innovation and Evolution

Inventions and changes in techniques were discussed by Kondratiev in his account of long waves but his treatment of them was slight. Without much clarification of the sources of his insights, he produced two hypotheses that covered the timing of inventions and their integration in productive processes. First, as one of his empirical characteristics, the clustering of a large number of important discoveries and inventions was placed in the recession phase of the wave. Second, although stimulated by "the necessities of real time and of the preceding development of science and technique", inventions remain ineffective until economic conditions are favourable for their application. Thus they cannot be applied in a large way until the long upswing has commenced. They are not the initiating force for the recovery although thay may intensify its progress once begun. Kondratiev, as Forrester, stressed replacement demands in the capital sector as the initiating force. It seems appropriate to dispose of the reinvestment theory (or "echo principle") before we consider the significant issues that emerge from the two hypotheses on inventions that we have just cited.

The reinvestment theory has a long history that is best described by Einarsen (1938a, b), who points to the efforts of Marx, Robertson, Pigou, Spiethoff, Aftalion and Clark to elaborate it as the generator of either elastic or inelastic cycles. Einarsen (1938a, p. 11) reveals the two critical features. "[The] reinvestment theory will provide a satisfactory and probable explanation of the business cycles, as it is able to explain the turning points both from depression to the period of revival and from prosperity to recession and also to explain the periodicity of the fluctuations." Explanations of turning points and periodicity clearly are major demands on long-wave theorists. The reference to business cycles in this quotation stems from Marx's emphasis on ten-year patterns of replacement that he uses to connect the average duration of capital lives and business-cycle waves. Periodicity is thus linked to the life expectancy of large capital assets while the impetus to recovery stems from the more or less simultaneous wearing out and re-ordering of these assets. The degree of elasticity in period must then depend both on the rigidity, or non-postponability, of replacement and on the historical time path of gross investment.

Tinbergen (1938) provides a simple illustration of an inelastic formulation that represents what he terms "life fluctuations". Suppose we have I, R and N to denote, respectively, gross investment, replacement and net investment. Then the definitional identity

$$(1) \qquad I_t = R_t + N_t$$

can be combined with a simple hypothesis for replacement

$$(2) \qquad R_t = \tfrac{1}{2}(I_{t-6} + I_{t-8}),$$

which assumes one-hoss-shay replacement with two lives (six years and eight years). The model can be completed by assuming net investment is always proportional to gross investment without any delay:

$$(3) \qquad N_t = b\, I_t.$$

The final equation of the model is obviously found by substitution for N and R. It is a simple difference equation for gross investment:

$$(4) \qquad I_t = c(\, I_{t-6} + I_{t-8}\,).$$

Here c is a function of the proportionality factor b. For suitable values of b, the difference equation will generate "echo" effects although not necessarily at intervals equal to the two assumed asset lives. Acceptance of this rigid formulation implies that we need data on the age distribution of physical capital assets and on the incidence of replacement orders as distinct from expansion orders. Such information might then be used to extend the simple model and to determine its dynamic characteristics. There is a surprising absence of this information even in developed economies. However, the components in the model (one-hoss-shay assumptions, passive net investment, limited number of lives, rigid replacement) clearly illustrate the severe restrictions that must be made in order for the re-investment theory to be a valuable explanatory element in long waves. It is difficult to see how we can continue to accept the view expressed by Einarsen. Replacement may have something to do with turning points and duration of long waves but preoccupation with replacement is much too fragile a basis for explaining major economic developments. Other explanations have to be sought. Kondratiev's two hypotheses provide a starting point.

Schumpeter (1935) posed a fundamental question that slightly modifies these hypotheses. "Why should the carrying into effect of innovations (as distinguished from 'invention' or experimentation ...) cluster at certain times, and not be distributed in so continuous a way as to be capable of being continuously absorbed as the current increase in the supply of labor is?" Responses to this question provide many instances of soft econometrics with interesting issues of data, estimation technique and interpretation of empirical evidence. We need, however, a more comprehensive framework than that given by Kondratiev. Indeed most responses are properly described as neo-Schumpeterian although they frequently involve pre-Schumpeter notions such as the S-curve for product or industry life cycle, as found in Kuznets (1929). Before leaving Kondratiev, it is worth recalling that he wrote of "important" discoveries and did not get embroiled in the troublesome distinction between inventions and innovations. Much recent discussion has pointed to "basic" innovations rather than to discoveries. Clearly qualifiers such as "important" or "basic" need to be made operational either by consideration of prior notions or of empirical rules. The latter generally introduce econometric methods.

We should also recall the problems of dating inventions and of demonstrating their clustering, both of which were quickly pointed out by the early critics of Kondratiev as cited by Garvy. Econometric efforts may be far more useful in exploring the occurrence of clusters of inventions (or innovations) than in the dating of individual inventions. However, in both areas, there are immense difficulties in resolving data deficiencies. Our immediate problem is choosing a starting point from which to illustrate the role of soft econometrics in the elucidation of these matters. It would be easy to be swept away by the enthusiasm expressed by proponents of economic theories for innovations and forget that our primary concern is with the narrower topic. Suppose we temporarily set aside questions of data and focus attention on clusters and the various hypotheses that have been attached to them. The incompleteness of our account of important economic theories may be eliminated by contact with the major surveys of van Duijn (1983), Freeman, Clark and Soete (1982), and Mensch (1979).

Some questions immediately spring to mind. Do clusters of inventions or innovations occur? If they exist, are they found in the recession of the long wave, as Kondratiev suggested, or are they part of the reviving influence at the onset of the long recovery? Are clusters to be identified only with particular categories of innovations? Can we look at their role in causality and, hence, determine whether they are exogenous or endogenous? Are there important connections between clusters of innovations and the uneven emergence of excess capacity? Can the concept of innovation be enlarged so as to explain the upper turning point as well as the lower one? Do clusters affect the "shape" of the long wave? May they be used to name different waves so that each can be identified with specific industrial developments? What have clusters to do with the duration of waves and their quasi-periodicity? In the treatment of clusters, should we use periodizations of the long waves based on investment, output, prices or some other variable in determining cyclical phases? Since economic developments and inventions are international in scope, which countries' experiences should be used in order to link clusters and cyclical phases? Clearly there is adequate room for the use of econometric approaches in almost all of the areas that underlie these questions. These generally involve simple regression analyses dealing with tentative structures or, when consistent bodies of data are available, the application of clustering algorithms. They are, however, not restricted to these forms. Sahal (1980, 1981), for example, has used spectral and cross-spectral techniques while Marchetti (1980, 1983) opts for the logistic curve. Both presented conclusions favourable to the occurrence of long waves, in relation to technological activity, and touched on such matters as exogeneity and product versus process innovations.

Although econometric approaches could be used here, they are not common in practice with some important exceptions. The general picture is one of underutilization and of excessively simple formulations. Kuczynski (1978, 1980) used clustering algorithms to delineate cyclical phases while Kleinknecht, Mensch and a few others fitted time trends with linear regression models and analyzed residuals. Frequently, however, researchers seem satisfied with lists of innovations or inventions, prolonged disputes of conceptual issues, simple tabulations of the values of aggregate economic variables, stylized diagrams of theoretical curves with S-shape and some graphs for real data. This situation is inadequate and not fully explicable in terms of the inherent softness of data and concepts or the insufficient elaboration of theoretical structures. Perhaps the answer lies in a mismatching of the willingness to pursue interest in long waves with the mathematical and statistical skills that are needed for use of soft econometric methods.

This criticism is evidently less relevent when we consider the second basic stream of neo-Schumpeterian theories that focus attention on evolutionary theories of growth rather than innovation. We look at the fruitful exercise of simulation for this stream below but, before then, we should look at periodization and duration of long waves as explicated in the literature stressing innovation. Table One contains the periods indicated by a number of contributors. The choices have many common features and a few pronounced disparities such as Rostow's premature recognition of a fifth Kondratiev wave, Dupriez's

TABLE ONE

PERIODIZATION: LONG-WAVE CHRONOLOGIES

	First Wave		Second Wave		Third Wave		Fourth Wave	
	Lower	Upper	Lower	Upper	Lower	Upper	Lower	Upper
Kondratiev (1926)	ca. 1790	1810-17	1844-51	1870-75	1890-96	1914-20	-	-
de Wolff (1929)	-	1825	1849-50	1873-74	1896	1913	-	-
Schumpeter (1939)	1787	1813-14	1842-43	1869-70	1897-98	1924-25	-	-
Clark (1944)	-	-	1850	1875	1900	1929	-	-
Dupriez (1947, 1978)	1789-92	1808-14	1846-51	1872-73	1895-96	1920	1939-46	1974
Rostow (1978)	1790	1815	1848	1873	1896	1920	1935	1951
Mandel (1973)	-	1826	1847	1873	1893	1913	1939-48	1966
van Duijn (1983)	-	-	1845	1872	1892	1929	1948	1973
Amin (1975)	1815	1840	1850	1870	1890	1914	1948	1967
Research Working Group (1979)	1798	1815	1850	1873	1897	1913-20	1945	1967
Kuczynski (1981)	-	-	1850	1866	1896	1913	1951	1969

Source: Hopkins et al. (1979), Bieshaar and Kleinknecht (1983), van Duijn (1983).

delay for the upper turning point of the fourth wave and the surprising dates of Amin for the first wave. Our list does not include attempts to push the notion of long waves to earlier periods that precede Kondratiev's initial specification. These are briefly noted when we consider views from the left in a later section.

The dating of cycles, or periodization, is generally based on informal methods that combine theoretical attitudes with consideration of both aggregate data and the arrangement of inventions or innovations in time. Indeed the researcher who gives most attention to formal econometric criteria, Kuczynski indicates a date for the lower point of the fourth wave that is much too late relative to other choices. The overall picture emerging from the entries in the table confirms the overall duration indicated by Kondratiev even though the rows are often based on different evidence. It seems worthwhile to explain how some of the contributors treat turning points, duration and corresponding issues.

Rostow (1978b, p. 2) suggests, for example, that the duration of long waves "seems to lie in the fact that the opening up of new sources of food and raw materials required substantial periods of time- much more time than it takes to build a new factory or house." This is markedly different from explanations based on capital lives or the mechanical application of the reinvestment theory. Clearly Rostow's view is not easy to translate into numerical criteria for turning points.

He stresses prices and looks for changes in their trends. Thus periods when prices rose relatively for agriculture are used to represent the long upswing from the lower turning point to the upper one while the downswing is identified with relative declines in agricultural prices. In a recent elaboration, Rostow (1978a, pp. 109-110) describes three types of period including the upswing, the downswing and the long-wave peak. These may be illustrated by his list of attributes for the downturn. "Periods when the trend of prices in general, agricultural and raw-material prices in particular, and interest rates are falling or low, relative to previous and subsequent periods; income distribution tends to shift in favour of urban real wages, while profits in industry and agriculture are under pressure." His focus on prices, even with the supplemental attributes identified here, leads to an awkward treatment of the periods from 1951 to 1972 and from 1973 to the present but we should not be overly critical since many economists have been unable to fully account for price developments in recent decades. Rostow (1980a) reveals his treatment of data and his selection of indicators to support his contrary perspective which includes the early specification of the fifth Kondratiev upswing.

In this focus on prices, Rostow is following the lead provided by Kondratiev who employed English, French, German and U.S. price series. Schumpeter maintained the basic choices of Kondratiev without much change. He radically altered the approach to long waves by focusing attention on innovation and life cycles as well as by providing a link with particular complexes of products and industries but he made no immediate impact on periodization. Clark (1944), on the other hand, represents a major shift toward real rather than value indicators. He chose to distinguish "capital-hungry" from "capital-sated" phases according to the pace of investment. Another dramatic shift is suggested by van Duijn (1977, p. 561) who effectively develops some Schumpeterian notions. "The perception of the long wave .... would entail a chronology in which the length of the different phases is co-determined by the length of the different segments of the growth sector life cycles." We have thus moved from capital lives to sectoral lives but the step is not encouraged by the softness with which the latter can be determined. "Our knowledge of the length of these industry life cycles, however, is very limited. In establishing a long wave chronology we will have to employ those scarce sources of information that are available. This way, however, much room is left for subjective interpretation." Duration is, of course, fixed in this perspective by the growth curves for sectoral developments with the upper turning point associated with maturation or the switch from one dominant sector to a newer rival one. Innovations are associated with each industry life cycle but are not used by van Duijn to assist periodization.

A more direct use of innovations to choose chronologies is handicapped by the obvious data deficiencies but some examples may be found. An outstanding illustration is given by Kleinknecht (1981b), who also explores the value of innovation intensity as indicator for appraising band-wagon, continuity and innovation-effect hypotheses which cover the sectoral allocation of innovations, their temporal clustering, correlations between innovative activity and sectoral growth, and other issues. This study shows how soft econometric techniques can enhance our understanding of long waves even if we choose to formulate the dynamic elements of our explanations of these waves in terms of imprecise concepts and fallible data. Kleinknecht also points to the feasibility of exploring causal linkages for new Schumpeterian industries.

This list could be extended but the image of picking critical dates by considering aggregate indicators and selective use of regression analysis, chi-square statistics and other procedures persists. Econometrics has a modest role giving support for specific hypotheses rather than the more ambitious modelling of competing views of the whole long-wave process. Its softness is evident due to the softness of much data, the fundamental complexity of overall and sectoral economic growth, and the general underdevelopment of theory.

The life-cycle view of firms, industries, products and sectors, to which we now turn, is more compatible with simulation than regression analysis. The evolutionary perspective introduced by Schumpeter seems well-matched with the flexibility of simulative experiments. In many respects, evolutionary theories are a direct challenge to neoclassical approaches for the modelling of technical change and economic growth. They emphasize differences between firms and reject the notion of moving equilibrium with obvious implications for the inclusion of aggregate economic data and homogeneity assumptions in regression analysis. Nelson and Winter (1974, p. 903) reveal some of the underpinnings of the evolutionary approach. "The extent of the rewards and penalties, and the rates of introduction and diffusion of new techniques, depends on a complex of environmental and institutional considerations that differs sharply from sector to sector, country to country, and period to period. ... [The] diversity and change that are suppressed by aggregation, maximisation and equilibrium are not the epiphenomena of technical advance. They are the central phenomena." The emphases on diversity, institutional context, individual experiences, non-maximisation and disequilibrium affect the choice of research method as well as our ability to interpret present developments, predict and generalize from past regularities. One problem for statistical methods is the fiction of drawing a sample from a hypothetical population with stable characteristics. This is hard to sustain for the unstable environment that is perceived by the evolutionary school of economists. Clearly the validity of the classical linear model and its more complex extensions is capable of very little support. We are severely constrained in the testability of basic premises for evolutionary theories.

Simulation provides a framework for checking the consistency of assumptions and for elaborating the implications of particular assumptions so evolution is a mixture of experimentation and theoretical reformulations. There are many similarities with system-dynamics methodologies although the "agents" of change are more explicit in evolutionary models and the questions raised are more specific to innovation, particular products and behavioural models. Given these similarities, we do not need to repeat the econometric consequences of the shift in emphasis from estimation to simulation. We can look instead, quite briefly, at the potential roles of evolutionary theories in advancing our awareness of long waves. There are essentially three positive roles. First, the pursuit of evolutionary theories with simulative experiments permits small components of long-wave complexes to be validated. Many of the recent papers of Nelson, Winter, Metcalfe, Schuette and others fulfil this potential and illuminate the pace and structure of change for individual enterprises. Second, building on this micro-level information, we could attempt to explore the integration of the revealed structures at the macro-level of aggregation. Hartman and Wheeler (1979), for example, seek to use such insights in a more aggregative investigation of the long wave. Finally, the evolutionary perspective can provide further theoretical constructs and notions, such as "technological mutation" (Nelson and Winter, 1975), with which we can transform our organization of dynamic elements in long-wave theories.

Financial Fragility

Rostow, Forrester and almost all of the proponents of neo-Schumpeterian approaches stressing innovation and evolution ignore significant financial aspects of economic change. Brief discussions of profit motives and income distribution do not generally develop into appraisals of credit and financial institutions. Marchetti (1980, pp. 280-281) is extreme in his view of econometric models and the attitudes of econometricians to their component variables. His devotion to real magnitudes, as revealed in the following quotation, is common among proponents of long-wave theories. "To a physicist's eye, present-day econometric models still look much like toddling and stuttering. What I think most dangerous and misleading is their blind devotion to monetary concepts. All my analysis of economic systems tends to show that monetary variables are the manifestation of a deeper stratum of phenomena, where the real mechanisms lie." The neglect of credit is surprising since both Kondratiev and Schumpeter sought to stress its importance in relation to real capital accumulation and valuation of capital assets.

Kondratiev, following Tugan Baranowsky, pointed out that the material basis of long waves is the replacement and increase of basic capital goods which requires huge amounts to loanable funds. Major upswings required, in his view, a high propensity to save, a relatively large supply of loan capital that is available at low rates to entrepreneurial and financial groups, and low prices. Loanable funds are thus important for the expansion which is limited in the end by increases in interest rates and a capital shortage. The upper turning point is explained by Kondratiev, therefore, in terms of monetary over-investment. This naive treatment of free loanable funds is clearly unsatisfactory both for the periods described by Kondratiev and for the present. Since it also did not introduce any novel concepts, the neglect of this particular treatment is easy to explain. We have more difficulty in dealing with the neglect of Schumpeter's comments on credit but it seems appropriate to account for this with the brilliance of his treatment of other aspects of economic change and the attractive stimulus of his new concepts there. Certainly Schumpeter felt that the expansion and contraction of credit was a very important part of the long-wave mechanism.

The subsequent neglect of these views was not total. Dupriez (1947, 1978) and Mensch, Coutinho and Kaasch (1981) are, for example, interesting exceptions. Dupriez, who dates the end of ~~the~~ fourth Kondratiev wave in 1974 primarily by reference to credit and price developments, is forceful in his stress on monetary elements. He argues that "swings in the rate and size of credit expansion were a more basic and fundamental element of long movements than variations in goods production." His account of the fourth wave , as given in Dupriez (1978, pp. 202-203), is straightforward. "[The] upswing in the long phase started when recourse to credit was moderate in relation to disposable funds and when interest rates were low

They moved up as recourse to credit grew. Developments were such to 1970 that the system worked smoothly on the surface, notwithstanding growing tensions. But the movement ended up in swift and intense increases in all types of interest rates. This happened precisely at the moment when the monetary system crashed, as a result of the growing tensions of the upswing." Looking at monetary developments, Dupriez stress crises in the monetary system. This is consistent with the treatment of the U.S. economy by Minsky which we shall explore below. It is different from Kondratiev since the latter's use of waves was partially motivated by a rejection of the notion of discontinuous crises transforming capitalist economies. (This is clear in his debates with early critics, as recorded by Garvy.)

Dupriez (1978, p. 206) points to institutional transformations with an international flavour. "Money has always been involved in the downturns of the long waves, very seriously indeed: whereas the monetary system as it stood worked smoothly in the upswings up to a point where tensions developed, the downturn stood in the center of monetary crises and reforms. Indeed, the institutional setup of the monetary system was transformed at those very moments. ... The gold standard system was introduced in England in 1818; the gold-silver bimetallism crashed in 1874; a general upheaval of currency parties was introduced after 1920." This international aspect is developed by Kindleberger (1978) who widens the vision to include financial rather than monetary crises. He also revives issues of the role of lenders of last resort in the restraint of financial panics and the international propogation of financial crises.

With respect to domestic considerations, the lender of last resort for a banking system "stands ready to halt a run out of real and illiquid financial assets into money, by making more money available" without creating a moral-hazard problem of banking irresponsibility. The international counterpart is similar with balances of payment and countries replacing their domestic analogues. Rising interest rates can be traced to international competition for funds in addition to the domestic demands that we have already noted. This may be readily combined with international extensions of the concepts developed in the innovation literature. For example, the S-curve of economic development for individual products or industries can be amended as in the product cycle of Vernon, which deals with the international transmission of technology and the geographic mobility of production. Diffusion of technology from an initial source to late-comers may, as in recent U.S. experience, be eventually associated with the shift from a current-account surplus to a deficit and thus to pressures on interest rates. Kindleberger has introduced the term "aging economy" to cover this life cycle description. (It has some similarities with elements of Wallerstein's theories of hegemony and competition to which we shall turn in the next section.

One possible consequence of the financial crises, as viewed from an international perspective, is the phenomena of "hot money" flows which are large short-term balances that are geographically mobile. Expansion of such balances crowds out the longer -term funds that are necessary for financing real capital enlargement and, thus, adversely affects economic development by raising the cost of longer-term funds and creating capital shortages. The similarities of these difficulties as they occur now and as they occured in the corresponding period in the last Kondratiev wave are found in the analogies noted by Balogh and Graham (1979) and elsewhere by both Balogh and Kindleberger. The present world debt crises can then be compared with that of the last great depression. Its involvement of third-world countries is quite consistent with the geographical expansion of capitalistic production as envisaged by Kondratiev.

The second illustrative exception to the neglect of credit and financial issues by some schools of long-wave theorists is provided by Mensch, Coutinho and Kaasch (1981, p. 283), who also differ from Kondratiev on the continuity to be found at the break of long-wave expansions. Mensch has proposed a "metamorphosis model" which is composed of a temporal sequence of S-curves rather than waves. He stresses the sharp discontinuities that occur between successive S-curves. A major element in this perspective is the contingency theory of changes in capital values, which links these changes with the propensity to innovate. There is an important movement of emphasis from capital shortage to its valuation within this discontinuity approach. "[Under] certain circumstances (which we identify as typical of either expansionist or recessional structural change), entrepreneurs find that their stocks of fixed capital in plant and equipment either enjoy or suffer systematic up- or downgrading in operative value (appreciation or depreciation). These shifts in value of parts of the capital stock induce capital owners to either invest their funds in more of the same type of capital goods, or induce them to invest in alternative types of capital goods." Treating ownership of capital assets as financial investment, we can explain the shift of funds into international capital and money markets as rates of return in production decline. A crisis occurs when this shift is pronounced so as to create a surge in short-term liquidity. When demand satiates at the long-wave peak and reinvestment in traditional lines of production becomes unattractive, the financial managers of firms [argues Mensch (1979, p. 20)] "cannot resist the movement toward lucrative currency speculation and paper investment budgeting even if they would rather not participate in such activities". This is a concomitant of the technological stalemate when the maturation of product life-cycles cannot be stimulated even by pseudo-innovations. The corresponding shifts in capital values lead the drop from the end of one upswing to the depressed state that precedes the next upswing when the reviving demand affects rates of returns on some capital goods. This view is thus markedly more complex than the focus of Kondratiev. However, it shares his failure to identify actual institutional adjustments since discontinuities are not associated with changed institutional arrangements.

The major contribution to our understanding of institutional changes in financial markets following periodic crises is by Minsky. In contrast to the researchers that we have already cited, he gives little attention to technology, S-curves and innovation. His myopic focus is frequently only the U.S. financial system. The connections between his vision of fragility within that system and Kondratiev-type waves is seldom made explicit. However, his research fits neatly in the long-wave framework with its inherent instabilities, disequilibria, long-term horizons, evolving characteristics, institutional adjustments, qualitative changes and fundamental challenges to the neoclassical view of appropriate economic theorizing. It is no accident that the increasing attention given to his efforts is contemporaneous with the revival of the long wave.

The closest link with long waves is evident in Minsky (1964), where he argues that financial panics are endogenous and systematic events rather than exogenous and episodic. Indeed he sees the stable generating mechanism for long waves as centred around the cummulative changes in financial variables within the long-wave expansions and contractions. From the beginning, institutional changes have affected these developments and given them an evolving appearance so that successive waves reveal structural amendments. Minsky argues that the U.S. financial system lacks stability and that standard approaches of economists frequently lose their validity when financial crises occur . "The neoclassical synthesis ...does well enough in explaining the behavior of our economy in an age of financial tranquility.... [It] cannot provide a relevant framework for our type of economy in the past decade." (Minsky, 1977b, p. 844) With respect to Keynes, Minsky (1975c, p. 129) argues that "the missing step in the standard Keynesian theory was the explicit consideration of capital finance within a cyclical and speculative context."

This rejection stems from the view that major financial crises in the U.S. are not temporary aberrations but part of a pattern indicating systematic financial fragility. In particular, he points to the credit crunch and runoff of certificates of deposit in 1966, the commercial paper crisis including the failure of Penn Central Railroad in 1970, the 1974-1975 financial trauma with the failure of Franklin National Bank and the bankruptcy of real estate investment trusts, and the troubles of 1980 which involved the Hunt-Bache silver debacle, Chysler refinancing and difficulties of the First Bank of Pennsylvannia. The nationalization of Continental Illinois National Bank in 1984 can thus to be viewed as simply the most recent of a series of crises that have occurred over a decade and a half of U.S. economic experience.

Fragility is defined as the characteristic whereby normal functioning of a financial system can be disrupted by some not unusual event. Essentially it involves a lack of resilience to minor shocks or 'displacements' as well as to more substantial ones. This fragility is said to be systematic if the susceptibility to disturbance is not due to either accidents or errors of policy. Minsky suggests that situations of financial fragility reveal incoherent behaviour where reactions antidampen economic adjustments. It is important that this behaviour be recognized as an "essential attribute of capitalism" that is centred in the financing of investment activity and ownership of the stock of capital assets. A straightforward account of the anatomy of a typical crisis from the Minsky perspective is provided by Kindleberger (1978b, Ch. 2) with credit and valuation elements made clear.

A displacement brings forth opportunities for profit in some new and existing lines while closing out others. The consequent boom is fed by an expansion of bank credit which enlarges money supply. "For a given banking system at a given time, monetary means of payment may be expanded not only within the existing system of banks, but also by the formation of new banks, the development of new credit instruments, and the expansion of personal credit outside of banks." (Kindleberger, p. 16) The speculative urge may then be transmitted into effective demand for goods or financial assets until it presses against existing capacity. We can see the basis for a cumulative process of positive feedback, which Minsky calls "euphoria". Eventually (but not necessarily) overtrading may occur with pure speculation for price rise, overestimates of prospective returns or excessive gearing through inadequate margins. Kindleberger (p.18) shows how this development can be given an international flavour through the conduits of commodity price increases, internationally-traded securities and speculation in exports, imports and foreign securities. The credit system is stretched tighter as a speculative boom continues with increasing interest rates, velocity of circulation and prices. Then, after some hesitation, prices begin to level off and an uneasy period of "financial distress" may ensue with growing awareness of the possibility of a rush toward liquidity. The projection of this picture into one of critical proportions is thus clear with eventual "revulsion" and "discredit". Throughout "it is finance that acts as the sometimes dampening, sometimes amplifying governor for investment" so that "finance sets the pace for the economy" (Minsky, 1975c, p. 130).

This radical view has elements that can be traced to Fisher and Keynes. Kindleberger links it with early classical ideas of overtrading as expressed by Smith, Mill, Wicksell and Fisher while Minsky (1977a) attributes to Keynes the important result that "a capital-using capitalist economy with sophisticated financial practices (i.e., the type of economy we live in) is inherently unstable." His debt to Keynes is even more explicit in Minsky (1975c). Our account is inevitably brief. Further details may be found in the references cited in our first appendix. For the long wave, Minsky provides a potential explanation of the last part of the long upswing and of the collapse. His stresses of endogeneity and credit match those of Kondratiev but he provides more institutional detail and gives significant attention to changes induced by critical financial developments. The impacts of his ideas and those of Kindleberger in developing notions of financial instability and lenders of last resort are revealed by contributions to Kindleberger and Laffargue (1982). These range much beyond our present concern.

Where does soft econometrics fit in the topic of credit and financial fragility in long waves? Mensch and his associates use regression techniques and other simple methods to explore small components of his theory. Random-walk phenomena can be used to assess the clustering of innovations, discontinuities may be determined with tests of structural change (within linear statistical models, as Chow tests or residual analysis) as well as with the intervention time-series methods of Box and others, and the interactions of real and financial investment are compatible with various econometric procedures. Instabilities and discontinuities do not preclude the use of formal statistical methods. Rather they reveal the need for our shift from hard to soft interpretative skills. Both Kindleberger and Minsky have underutilized these methods but their neglect is easy to justify. They are concerned with "large" issues rather than the componental research where econometrics might be useful. Changing the attitudes of the vast majority of professional economists, amending the operating characteristics of the Federal Reserve System (even of capitalism as whole), transforming prevalent political support for monetarist policies or for international isolationism are quite distinct from the modest claims for soft econometrics. Clearly the tasks of Minsky, Kindleberger and some other contributors to this area are not served by citing values of R^2s, t-statistics and so on.

This does not mean that econometrics may trivialize long-wave theories involving financial crises. There are important themes here that econometric analyses may enhance. The prime example lies in financial innovation and the impact of such innovation on both the interpretation of monetary aggregates and their role as targets in policy rules. A second example is found with the issue of predictability in the face of evolving financial structure and inherent instability. We recall that Minsky's theory envisages a process of irregular financial innovations, affecting credit availability, as the unstable financial structure evolves. Innovations are an endogendus part of this change. They can arise as economic agents seek to maintain levels of credit despite regulatory constraints but they can also arise spontaneously and independent of regulations.

It is commonplace to acknowledge the confusion that affects measurement of monetary aggregates when financial innovations occur. In Chapter Four, we discuss the perceived instability of money-demand relationships as determined by changes in the interpretation of monetary aggregates. This discussion is directed to the period stressed by Minsky. The innovation-regulation-control complex is elaborated by Hester (1981, 1982), Wojnilower (1980) and Mayer (1982). We shall delay our treatment of this area, and specially the implications for soft econometrics, to the later chapter. The second example, dealing with predictability, is considered by Sinai (1977) who is deeply involved with the DRI econometric model. Sinai (p. 189), who suggests that Minsky downplays the role of econometrics in understanding the U.S. economy, insists that "the Minsky conception lacks the predictive content required for empirical testing. The structure of the theory does not lend itself to validation or refutation. Nor can the degree of financial fragility be measured to determine how near a crisis is." Eventually Sinai takes a view that is consistent with our earlier arguments. "Econometric modeling and simulation offer the most promising method for determining the impact of the factors mentioned by Minsky on the evolution of financial crises." We can differ with Sinai on the issue of predictability while finding substantial common ground in the actual use of econometrics.

Views From The Left

Kondratiev's personal difficulties in the 1920s are described by Day. They stemmed from the inability of many of his critics to reconcile long waves, and especially cyclical recoveries, with the heritage of Marx. Yet there are elements in Marx's writings that are compatible with cycles even in a longer-term context. These are explicated in modern language by Bellamy (1976) and Itoh (1980, Ch. 5). They are not markedly at odds with some of the views that we have already cited. Despite the possibility of reconcilation, the adverse reaction to Kondratiev's efforts by his contemporaries was severe. More recent writers who would be placed on the left of the political spectrum have met far less resistence to their long-wave theories. At least three major schools of thought can be detected among these writers. We couple these schools with Mandel, Gordon and Wallerstein as their major representatives. There are profound differences of interest among the groups as we shall reveal below. There are, also, variations in their use of empirical evidence and in potential uses of econometric methods. Further, there are disparities in periodization (especially with respect to pre-Kondratiev chronologies), geographic coverage, the shape of individual waves, and the nature of discontinuities.

Mandel suggests that the waves are caused by surges of new technology and finds two phases in each wave. The first phase has rising profit rates as the new technologies are developed while the second phase contains falling profit rates as the possibilities introduced by the technologies are exhausted. Increased profits are, therefore, associated with the creation of production sites and their decline with rapid diffusion. His basic position is simply stated in Mandel (1980, p. 9 and 11). "[Any] Marxist theory of the long waves of capitalist development can only be an accumulation-of-capital theory or, if one wants to express the same idea in a different form, a rate-of-profit theory... [The] essential movements, those that determine the basic trends of the system, remain the fluctuations in the average rate of productive capital accumulation." The cumulative process of the upswing, once it has been initiated, and the transition from this expansion to the period of stagnation are both treated as endogenous. Each upswing is supposed to encompass a "technological revolution" in the capital goods sector of which the latest is one of "automation" (mechanical handling, continuous production, and electronic and computer control), and, like Rostow, Mandel accepts the notion of industrial leading sectors. Innovations do not, however, trigger the upswings. They encourage expansion once this has started.

The upper turning point is primarily determined by the growth of capital intensity which eventually causes average rates of profits to decline. This view associated with diffusion of technology is very similar to those from a neo-Schumpeterian perspective but with an additional stress on profits. As such, the analysis seems forced so that Itoh (1979) is perhaps correct to criticize Mandel for fundamentalism. The lower turning point is, on the other hand, historically contingent unlike Kondratiev's approach. A host of exogenous influences or system shocks are essential to the initiation of cumulative growth after a reserve fund of capital is established. "These radical changes in the overall social and geographical environment in which the capitalist mode of production operates in turn detonate, so to speak, radical upheavals in the basic variables of capitalist growth (i.e., they can lead to upheavals in the average rate of profit)." (Mandel, 1980, pp. 21-22) Clearly there is something unsatisfactory in treating these extra-economic shocks as the basis for waves with regular periodicity.

The second school, associated with Gordon, Bowles, Weisskopf, Edwards and Reich, makes much less use of fundamentalist positions and is more attentive to the character of labour markets. Like Itoh (1979,1980), they formulate theories of capitalist development in stages along lines that they attribute to the Japanese economist Kōzō Uno. The history of long waves is, therefore, treated as a history of successive stages of capitalist accumulation with pronounced qualitative adjustments occurring between successive stages. Again capital accumulation is assumed to provide the major force for change in capitalist economies but such accumulation is now considered to be highly dependent on the perceived stability of institutions that facilitate the movement of capital. The breakdown of one set of social institutions and its replacement by a successor are thus the primary characteristic of both critical periods and the onset of recovery. Capitalist economies, in this view, have moved through a series of universal crises which are identified with transformations of the social structures of accumulation.

Accumulation is affected by credit availability and solvent or effective demand but it is also influenced by the "reliability" of the labour force, the hospitality and stability of governments in recipient countries as geographical expansion in resource extraction and basic production occurs, and the supportiveness of domestic (home-country) government policies. Five stages may be determined in each long wave. The first stage involves the establishment of an appropriate social structure. This is followed by a period of expanded reproduction and, then, by one of deceleration in the rate of accumulation. Acceleration of institutional instability marks the fourth stage. It is followed by the unfolding of economic crisis. More comprehensive descriptions of these five stages are provided by Gordon (1980 a,b).

A generalization of the reinvestment theory is used by Gordon (1980b, p. 27) to link an endogenous cycle with infrastructural investment. "The economy will receive a strong boost at the beginning of the stage of accumulation and will experience considerable economic drag after the need for infrastructural investment has dried up. The economy would only get another comparable boost if and when new infrastructural investment was needed and it became possible to finance it." He hypothesizes that this cycle lasts roughly a half century "because of the relationship between the scale of investment required at the beginning of that cycle and supply of potentially investible funds available to finance that investment." This hypothesis amends the traditional reinvestment theory by focusing attention on infrastructural investment (a "centre of gravity" dimension) and by stressing clusters of such investment (the scale dimension).

Mandel acknowledges the significance of labour elements when economic momentum is growing. He points to the memory of long-term unemployment from the recession (with attendant reductions in bargaining power and shaken self-confidence), the wage-lag in the initial part of the upswing and relative growth rates of real wages and labour productivity. Overall, as demonstrated by Coombs (1983), his treatment of labour processes is casual. A consequence of this is that his work suffers in comparisons with the treatments of Gordon et al. and of Freeman et al. Gordon and his associates typically discuss social structure in terms that include the moderation of class struggle by integration and partial cooperation of organized workers with their employers and by the segmentation of the labour force, which separates certain groups from these organized workers. They also stress the reproduction of labour force as channelled through education. Labour market stability is, they would argue, maintained through both cooperative collective bargaining and segmentation. The economic system falters as this stability weakens from endogenous strains so we should "appreciate the critical importance of the increasingly structural orientation of economic struggles as economic crisis deepens".

Because of their greater attention to detail, this second school have made much more use of statistical analyses than Mandel. In contrast to his focus of simple aggregate indicators such as the rates of output growth or sales growth, they frequently use relational techniques in addition to tabulations of GDP, industrial production, gross domestic investment in plant and equipment, real unit labour costs and unemployment rates. They are also, as in Weisskopf, Bowles and Gordon (1983), able to advance particular hypotheses with mathematical representations that yield suitable structural equations for regression analysis. It is significant that they try many alternative formulations and use robustness criteria as we might reasonably expect in applications of soft econometrics.

The final school that we shall consider is based at the State University of New York at Binghamton, where Wallerstein and his associates have established annual conferences on the "Political Economy of the World System". Wallerstein has taken up the notion of geographic expansion of the world capitalist economy from Kondratiev and he has stressed the mobility of control, or hegemony, from the Dutch to the English and, then, to the U.S. economy. The distinction between core and periphery in this world system is an essential characteristic of his vision of long waves while the mobility of the core has been used both to create "pairs" of long waves and to extend their identification to earlier historical periods. Just as Forrester integrated a very long cycle in his cyclical classification, Wallerstein draws on the insight of historians to identify "logistics", which have periodicities in the range of 150 to 300 years. Clearly these logistics cannot be established by any statistical procedure.

Gordon (1980b) attempts to find common ground with this approach. He fails because his approach is dominated by U.S. economic experience whereas the World-System perspective demands an international base and challenges the use of national data. Gordon and his associates also have difficulty in accepting both core-periphery frameworks as a source of long-wave phenomena and the possibility of projecting waves backwards when considering it a feature of capitalistic development. The core-periphery distinction can closely parallel high wage-low wage contrasts between groups of countries so the efforts of Lewis and Rostow to base long waves on terms-of-trade arguments could be expected to link these researchers too with the World-System perspective. They do share concern with shifts in income distribution between geographical areas but, otherwise, differences prevail.

Wallerstein's model has two zones, one with high wages (HW) and the other with low wages (LW). These are associated in turn with HW and LW commodities. Hopkins et al. (1979), the Research Working Group on Cyclical Rhythms and Secular Trends, explain the interactions of zonal activities that yield waves in production. Attempts to reduce costs by shifting the locus of production to lower wage zones occur during periods of stagnation. Recovery is then associated with the incorporation of new producers into the world economy from former frontiers, with greater dependence of households on wage labour, and with the utilization of technological advances in 'dynamic' sectors of production. Rivalry between national states of the core is normal in this process. Wallerstein points, within pairs of Kondratiev waves or logistics, to the pre-eminence or decline of influence for particular national stages. The intensity, focus and geographical spread of the capitalist economy is thus linked with economic, political and social phenomena.

Clearly this perspective demands multidisciplinary research and presents severe problems for measurement and operationalization. Chase-Dunn (1978) lists some of these problems which include bounding and mapping the system and its constituent zones; validity and reliability of economic indicators over time; limitations of aggregating data on national states to create contextual world-system variables; sampling intervals; and noncontinuous sources of data. This is a considerable challenge for econometrics. Two positive responses are possible. First, synthetic data can be manipulated in simulative experiments as has proved useful in most areas of long-wave theory. Second, fallible data can be reconciled with formal models that are designed with structural instability and measurement errors in mind. The soft context simply weakens the strength of inference and implies the need to search for robust approaches with conditional or provisional inference replacing hard-econometric alternatives.

A Multiplicity of Waves

This account of long waves is incomplete. The revival of interest has spread rapidly over the last decade of depressed economic conditions. Such diffusion has accompanied a growing malaise in economic theory for shorter business cycles. Haberler (1941, p. 273) suggested, many years ago, that prior analysis of short waves was necessary before we can hope to understand longer ones. "It would seem that... questions about the nature of the long waves can be answered only after a fairly full insight into the mechanism of the short cycles has been attained. For the forces which are said to produce the long waves do not work independently of, and alternatively to, those that produce the short cycle. They work through the latter... Until the working of the mechanism of the short cycle has been explored, the nature of the long waves cannot be understood. We are therefore compelled to attack first of all the problem of the business cycle." This view prevailed when first expressed but, perhaps surprisingly, is not evident now. Instead short cycles are increasingly being seen as understandable only within the context of longer-term economic developments. Many theorists also feel little compulsion to defend their use of a long horizon.

The existence of waves with different periodicities has been accepted since Kondratiev's efforts reinforced the notion of waves as contrasted to crises. These efforts were, of course, preceded by the efforts of other researchers who used Fourier's decomposition of individual functions (time series) into orthogonal component functions (usually either polynomials or sinusoidal functions). The standard tool of analysis of this group was the periodogram, which sought to detect the "hidden periodicities" of sine waves underlying a given time transient. When Kondratiev was active, Khinchin and the Russian school of probabilists were establishing the theoretical nature of stochastic process and beginning the transformation of periodogram analysis into its stochastic counterpart, spectral analysis. The detection of frequencies consistent with long-wave periodicities was attempted a quarter century ago as important computational developments made spectral analysis of economic time series feasible. A typical example is provided by Suzuki (1965) who concluded that, for Japan since 1879, "long swings and business cycles are meaningful phenomena in many of the time series". He also used cross-spectral methods to identify lead-lag relationships. More recent examples falling within the revival are provided by Dowling and Poulson (1971, 1974), Soper (1975, 1978), Klotz (1979), Sahal (1980) and Haustein and Neuwirth (1982).

These researchers generally use similar techniques with spectral estimates obtained after prior adjustment and with cross-spectral checks for potential causal linkages. As might be expected, spectral analysis is severely limited to the dynamic characteristics of single time series or pairs of them (with fixed sampling intervals). It lacks many of the rich details that make most long-wave formulations so appealing. However, with soft interpretation, spectral techniques can be illuminating for exploratory models. They can generate hypotheses as well as provide evidence of consistency with prior notions. Sahal, for example, found support for cycles in technological activity with Kondratiev, Kuznets and Juglar durations. His results indicated that the periods of cycles in product innovations were much longer than those of cycles in process innovations. Haustein and Neuwirth found "nondominant long waves do appear in the interaction between innovations, productions, patents, and energy consumption" and concluded that their investigation shed light on the causal structure of the innovation system. Similar findings have stimulated a large-scale commitment to the use of spectral techniques in exploring long waves at the International Institute for Applied Systems Analysis.

We should close on a qualifying note. Decompositions of economic behaviour into sine-wave components usually require data to be made stationary in the stochastic sense. Interest in particular ranges of frequencies requires the exclusion of disjoint ranges. These considerations imply prior adjustments such as trend removal and smoothing. The term "Yule-Slutsky effect" is attached to problems arising when the prior adjustments are inadequate. We discuss this effect in Chapter Four using the long-wave literature as a convenient backdrop. For the moment, we should note the problem of false inferences as data transformations introduce spurious frequencies. The final word, for now, rests with Bird, Desai, Enzler and Taubman (1965, p. 239). "We have shown that the use of both a fixed averaging period and a variable averaging period may yield a long cycle in the transformed data. Therefore, if long cycles have been found in economic data after using either transformation, it can mean that long cycles actually exist, or that they were created by the transformations." Such doubt is the pulse of soft econometrics.

Some Concluding Comments

Long-wave theories can be fascinating and we feel it appropriate to have outlined some of their attractiveness. The economic element of econometrics, it should be recalled, is an equal partner with statistics, mathematics and computing. Economic developments in the long term are characterized by discontinuities, instabilities, and a host of irregularity problems. Econometricians can never expect to provide a comprehensive model that will give adequate attention to the full dimensions of dynamic adjustments in the long term. They must be content to acknowledge the softness of their context and follow one of two basic strategies. The first of these follows traditional methods and treats components of long-wave theories in relative isolation with an exploratory emphasis replacing the confirmatory one of a controlled environment. Softness also results in the shift towards criteria such as robustness and conditional inference rather than criteria such as unbiasedness, consistency and predictability. The second strategy has become possible with the rapid improvements in computational capabilities. We now have important experimental options involving simulation with actual or synthetic data. Such experiments are also exploratory. Both strategies are dynamic in the sense that they can generate hypotheses and provide a process for exploring their implications and consistency with real developments. This dynamicism is much closer to what econometricians actually do in all areas of their endeavours rather than the static confirmatory image emerging from our textbooks.

APPENDIX ONE

REVIVAL OF THE LONG WAVE - A RECENT BIBLIOGRAPHY

AMIN, S. (1982), "Crisis, Nationalism, and Socialism", in S. AMIN et al., Dynamics of Global Crisis (New York: Monthly Review Press), pp. 167-231.

ARRIGHI, G. (1978), "Towards a Theory of Capitalist Crisis", New Left Review, No. 111, October-November, pp. 3-29.

ARRIGHI, G. (1982), "A Crisis of Hegemony", in S. AMIN et al., Dynamics of Global Crisis (New York: Monthly Review Press), pp. 55-108.

BARR, K. (1979), "Long Waves: A Selective Annotated Bibliography", Review, Vol. 2, No. 4, Spring, pp. 675-718.

BARR, K. (1980), "Long Waves and the Cotton-Spinning Enterprise, 1789-1849", in (eds.) T.K. HOPKINS and I. WALLERSTEIN, Processes of the World System (Beverly Hills: Sage), pp. 84-100.

BEENSTOCK, M. (1983), The World Economy in Transition (London: George Allen and Unwin).

BIERSHAAR, H., and A KLEINKNECHT (1983), Kondratieff Long Waves in Aggregate Output: An Econometric Test, Research Memorandum 1983-12, The Free University, Amsterdam, Faculty of Economics.

BOUSQUET, N. (1980), "From Hegemony to Competition: Cycles of the Core?" in (eds.) T.K. HOPKINS and I. WALLERSTEIN, Processes of the World System (London: Sage), pp. 46-83.

BRUCKMANN, G. (1983), "The Long-Wave Debate", Options, IIASA, No. 2, pp. 6-9.

CANARELLA, G., and D. SNYDER (1977), "The Long Swing: A Spectral and Cross-Spectral Comparison of Nineteenth and Twentieth Century United States Experience", Nebraska Journal of Economics and Business, Vol. 16, No. 2, Spring, pp. 11-24.

CAPIE, F.H., and M. COLLINS (1977), "The Depression in Perspective", The Banker, March, pp. 49-51.

CARGILL, T.F. (1971), "Construction Activity and Secular Change in the United States", Applied Economics, Vol. 3, pp. 85-97.

CHASE-DUNN, C.K. (1979), "Comparative Research in World-System Characteristics", International Studies Quarterly, Vol. 23, No. 4, December, pp. 601-623.

CLARK, J., C. FREEMAN and L. SOETE (1981a), "Long Waves, Inventions, and Innovations", Futures, Vol. 13, No. 4, August, pp. 308-322.

CLARK, J., C. FREEMAN and L. SOETE (1981b), "Long Waves and Technological Developments in the 20th Century", in (eds.) D. PETZINA and G. VAN ROON, Konjuktus, Krise, Gesellschaft (Stuttgart: Klett-Cotta), pp. 132-169.

COOMBS, R.W. (1981), "Innovation, Automation, and the Long-Wave Theory", Futures, Vol. 13, No. 5, October, pp. 360-370.

COOMBS, R.W., and A. KLEINKNECHT (1983), New Evidence on the Shift Toward Process Innovation During the Long Wave Upswing, Research Memorandum 1983-7, The Free University, Amsterdam, Faculty of Economics.

CRONIN, J.E. (1979), Industrial Conflict in Modern Britain (London: Croom Helm).

CRONIN, J.E. (1980), "Stages Cycles, and Insurgencies: The Economics of Unrest", in (eds.) T.K. HOPKINS and I. WALLERSTEIN, Processes of the World-System (London: Sage), pp. 101-118.

DAY, R.B. (1976), "The Theory of the Long Cycle: Kondratieff, Trotsky, Mandel", New Left Review, No. 99, September-October, pp. 67-82.

DAY, R.B. (1981), The 'Crisis' and the 'Crash' (London: NLB).

DELBEKE, J. (1981), "Recent Long-Wave Theories: A Critical Survey", Futures, Vol. 13, No. 4, August, pp. 246-257.

DOWLING, J.M., and B.W. POULSON (1974), "Long Swings in the U.S. Economy: A Spectral Analysis of 19th and 20th Century Data", Southern Economic Journal, Vol. 40, No. 3, January, pp. 473-480.

DUPRIEZ, L.H. (1978), "1974. A Downturn of the Long Wave", Banca Nazionale del Lavoro Quarterly Review, No. 126, September, pp. 199-210.

DUIJN, J.J. VAN (1977), "The Long Wave in Economic Life", De Economist, Vol. 125, No. 4, pp. 544-576.

DUIJN, J.J. VAN (1981), "Fluctuations in Innovations Over Time", Futures, Vol. 13, No. 4, August, pp. 264-276.

DUIJN, J.J. VAN (1983), The Long Wave in Economic Life (London: George Allen and Unwin).

EKLUND, K. (1980), "Long Waves in the Development of Capitalism?", Kyklos, Vol. 33, Fasc. 3, pp. 383-419.

FORRESTER, J.W. (1975a), "A National Model for Understanding Social and Economic Change: Part 1", Simulation, Vol. 24, No. 4, April (Simulation Today, pp. 125-128).

FORRESTER, J.W. (1975b), "A National Model for Understanding Social and Economic Change: Part 2", Simulation, Vol. 24, No. 5, May (Simulation Today, pp. 129-132).

FORRESTER, J.W. (1976a), "Business Structure, Economic Cycles and National Policy", Business Economics, Vol. 11, No. 1, January, pp. 13-24.

FORRESTER, J.W. (1976b), "Business Structure, Economic Cycles and National Policy", Futures, Vol. 8, No. 3, June, pp. 195-214.

FORRESTER, J.W. (1976c), "A New View of Business Cycle Dynamics", The Journal of Portfolio Management, Fall, pp. 22-32.

FORRESTER, J.W. (1977a), "How the Long-Wave Theory May Explain the Sluggishness in Capital Formation", Financier, September, pp. 34-38.

FORRESTER, J.W. (1977b), "Growth Cycles", De Economist, Vol. 125, pp. 525-543.

FORRESTER, J.W. (1978a), "Changing Economic Patterns", Technology Review, Vol. 80, No. 8, August/September, pp. 47-53.

FORRESTER, J.W. (1978b), "We're Headed for Another Depression", Interview in Fortune, January 16, pp. 145-146, 148.

FORRESTER, J.W. (1979), "An Alternative Approach to Economic Policy- Macrobehavior From Microstructure", in (eds.) N. KAMRANY and R. DAY, Economic Issues of the Eighties (Baltimore: The Johns Hopkin Press), pp. 80-108.

FORRESTER, J.W. (1980), "Information Sources for Modeling the National Economy", Journal of the American Statistical Association, Vol. 75, No. 371, September, pp. 555-566. Discussion and Rejoinder, pp. 567-574.

FORRESTER, J.W. (1981), "Innovation and Economic Change", Futures, Vol. 13, No.4, August, pp. 323-331.

FORRESTER, J.W., G.W. LOW and N.J. MASS (1977), Capital Formation and the Long Wave in Activity, System Dynamics Group, Sloan School of Management, MIT, July.

FORRESTER, J.W., N.J. MASS and C.J. RYAN (1976), "The System Dynamics National Model: Understanding Socio-Economic Behavior and Policy Alternatives", Technological Forecasting and Social Change, Vol. 9, Nos. 1/2, pp. 51-68.

FORRESTER, J.W., B.W. RICHMOND and N.J. MASS (1977), Analyzing the Production Sector of the System Dynamics National Model, System Dynamics Group, Sloan School of Management, MIT, July.

FRANK, A.G. (1980), Crisis: In the World Economy (New York: Holmes and Meier).

FRANK, A.G. (1982), "Crisis of Ideology and Ideology of Crisis", in S. AMIN et al., Dynamics of Global Crisis (New York: Monthly Review Press), pp. 109-166.

FRANKS, C.M., and W.W. McCORMICK (1971), "A Self-Generating Model of Long Swings for the American Economy, 1860-1940", The Journal of Economic History, Vol. 32, June, pp. 295-343.

FREEMAN, C. (1978a), Government Policies for Industrial Innovation, The Ninth J.D. Bernel Lecture, Birkbeck College, London, May.

FREEMAN, C. (1978b), Technology and Employment: Long Waves in Technical Change and Economic Development, Holst Memorial Lecture, December.

FREEMAN, C. (1979), "The Kondratiev Long Waves, Technical Change and Unemployment", in O.E.C.D., Structural Determinants of Employment and Unemployment, Vol II (Paris: O.E.C.D), pp. 181-196.

FREEMAN, C., J. CLARK and L. SOETE (1982), Unemployment and Technical Innovation (London: Frances Pinter).

GORDON, D.M. (1980a), "Up and Down the Long Roller Coaster", in Political Economy of the New School, Department of Economics, New School of Social Research, New York, pp. 61-81.

GORDON, D.M. (1980b), "Stages of Accumulation and Economic Cycles", in (eds.) T.K. HOPKINS and I. WALLERSTEIN, Processes of the World System (Beverley Hills: Sage), pp. 9-45.

GORDON, D.M., R. EDWARDS and M. REICH (1982), Segmented Work, Divided Workers (Cambridge: Cambridge University Press), Ch. 2.

GORDON, D.M., T.E. WEISSKOPF and S. BOWLES (1983), "Long Swings and the Nonreproductive Cycle", American Economic Review, Papers and Proceedings, Vol. 73, May, pp. 152-157.

GOTTLIEB, M. (1976), Long Swings in Urban Development (New York: Columbia University Press).

GRAHAM, A.K., and P.M. SENGE (1980), "A Long-Wave Hypothesis of Innovation", Technological Forecasting and Social Change, Vol. 17, No. 4, pp. 283-311.

HALL, P. (1981), "The Geography of the Fifth Kondratieff Cycle", New Society, March 26, pp. 535-537.

HARTMAN, R.S., and D.R. WHEELER (1979), "Schumpeterian Waves of Innovation and Infrastructure Development in Great Britain and the United States: The Kondratieff Cycle Revisited", Research in Economic History, Vol. 4, pp. 37-85.

HARTZ, C.D. (1978), "Kondratieff, Updating the Full Employment Act and U.S. Economic Growth to 1986. How is Your Business These Days? A Thought Provoker...", Business Economics, Vol. 13, No. 2, March, pp. 20-22.

HAUSTEIN, H.-D., and E. NEUWIRTH (1982), "Long Waves in World Industrial Production, Energy Consumption, Innovations, Inventions, and Patents and Their Identification by Spectral Analysis", Technological Forecasting and Social Change, Vol. 22, No. 1, September, pp. 53-89.

HEILBRONER, R.L. (1978a), Beyond Boom and Crash (New York: Norton).

HEILBRONER, R.L. (1978b), "Reflections: Boom and Crash", The New Yorker, Vol. 54, August 28, pp. 52-73.

HOBSBAWM, E. (1975), "The Crisis of Capitalism in Historical Perspective", Marxism Today, Vol. 19, No. 10, October. Reprinted in (eds.) R.C. EDWARDS, M. REICH and T.E. WEISSKOPF (1978), The Capitalist System (Englewood Cliffs, N.J.: Prentice-Hall), Second Edition, pp. 431-441.

HOPKINS, T.K., et al. (Research Working Group on Cyclical Rhythms and Secular Trends), (1979), "Cyclical Rhythms and Secular Trends of the Capitalist World-Economy: Some Premises, Hypotheses, and Questions", Review, Vol. 2, No. 4, Spring, pp. 483-500.

INTERNATIONAL CURRENCY REVIEW (1979), "The Last Great Depression and the Next", Vol. 11, No. 4, pp. 25-31.

KAHN, H. (1979), World Economic Development: 1979 and Beyond (New York: Morrow Quill Paperbacks).

KAHN, H. (1983), The Coming Boom (London: Hutchinson).

KAISER, R.W. (1979), "The Kondratieff: Investment Strategy Tool or Fascinating Coincidence?", Financial Analysts Journal, May-June, pp. 57-66.

KEET, A. (1982), "The Long Wave: Models and Approaches", Cyclical Fluctuations: An Interdisciplinary Approach, Section C36, Eighth International Economic History Congress, Budapest. Paper available from the Free University of Amsterdam, Research Group on Long Term Fluctuations.

KINDLEBERGER, C.P. (1975), "Lessons from 1929 for the Petro-Dollar Problems", Technology Review, Vol. 77, No. 6, May, pp. 46-55.

KINDLEBERGER, C.P. (1978a), "The Ageing Economy", Weltwirtschlaftliches Archiv, Band 115, pp. 407-421.

KINDLEBERGER, C.P. (1978b), Manias, Panics, and Crashes (New York: Basic Books).

KINDLEBERGER, C.P. (1980), "The Rise and Fall of the United States in the World Economy", in Joint Economic Committee, The Business Cycle and Public Policy, 1929-1980, 96th Congress of the U.S., 2nd Session, Nov. 28 (Washington, D.C.: U.S. Government Printing Office), pp. 67-79.

KLEINKNECHT, A. (1981a), "Observations on the Schumpeterian Swarming of Innovations", Futures, Vol. 13, No. 4, August, pp. 293-307.

KLEINKNECHT, A. (1981b), Prosperity, Crisis, and Innovation Patterns: Some More Observations on Neo-Schumpeterian Hypotheses, Research Memorandum 1981-23, November, The Free University, Amsterdam, Faculty of Economics.

KLEINKNECHT, A. (1982a), "Innovation Patterns and Long-Term Growth", Cyclical Fluctuations: An Interdisciplinary Approach, Section C36, Eighth International Economic History Congress, Budapest. Paper available from the Free University of Amsterdam, Research Group on Long Term Fluctuations.

KLEINKNECHT, A. (1982b), Patenting in Dutch Industry: A Cross-Section Test on the Industry Life Cycle, Research Memorandum 1982-30, November, The Free University, Amsterdam, Faculty of Economics.

KLOTZ, B.P. (1979), "Long Cycles: A New Look At The Evidence", Eastern Economic Journal, Vol. 5, No. 3, October, pp. 403-408.

KLOTZ, B., and L. NEAL (1973), "Spectral and Cross Spectral Analysis of the Long Swing Hypothesis", Review of Economics and Statistics, Vol. 60, August, pp. 291-298.

KUCZYNSKI, T. (1978), Spectral Analysis and Cluster Analysis as Mathematical Methods for the Periodization of Historical Processes- A Comparison of Results Based on Data About the Development of Production and Innovation in the History of Capitalism: Kondratieff Cycles- Appearance or Reality? Proceedings of the Seventh International Economic History Congress, Edinburgh.

KUCZYNSKI, T. (1980), "Have There Been Difference Between the Growth Rates in Different Periods of the Development of the Capitalist World Economy Since 1850? An Application of Cluster Analysis in Time Series Analysis", Historisch - Sozialwissenschaftliche Forschungen, Vol. 6 (Stuttgart: Klett-Cotta).

LEWIS, W.A. (1978), Growth and Fluctuations, 1870-1913 (London: George Allen and Unwin).

LEWIS, W.A. (1980), "The Slowing Down of the Engine of Growth", American Economic Review, Vol. 70, No. 4, September, pp. 555-564.

MADDISON, A. (1977), "Phases of Capitalist Development", Banca Nazionale del Lavoro Quarterly Review, Vol. 121, June, pp. 103-137.

MADDISON, A. (1979), "Long Run Dynamics of Productivity Growth", in (ed.) W. BECKERMAN, Slow Growth in Britain (Oxford: Clarendon Press), pp. 194-212.

MADDISON, A. (1982), Phases of Capitalist Development (Oxford: Oxford University Press).

MANDEL, E. (1975), Late Capitalism (London: NLB).

MANDEL, E. (1978), The Second Slump (London: NLB).

MANDEL, E. (1980), Long Waves of Capitalist Development (Cambridge: Cambridge University Press).

MANDEL, E. (1981), "Explaining Long Waves of Capitalist Development", Futures, Vol. 13, No. 4, August, pp. 332-338.

MANSFIELD, E. (1983), "Long Waves and Technological Innovation", American Economic Review, Papers and Proceedings, Vol. 73, No. 2, March, pp. 141-145.

MARCHETTI, C. (1980), "Society as a Learning System, Discovery, Invention, and Innovation Cycles Revisited", Technological Forecasting and Social Change, Vol. 18, No. 4, December, pp. 267-282.

MARCHETTI, C. (1983), "Recession: Ten More Years to Go?", Options (IIASA), No. 1, pp. 10-13.

MASS, N.J. (1975), Economic Cycles: An Analysis of Underlying Causes (Cambridge, Mass.: Wright-Allen Press).

MASS, N.J., and J.W. FORRESTER (1976), "Understanding the Changing Basis for Economic Growth in the United States", in Joint Economic Committee, 94th Congress, 2nd Session, U.S. Economic Growth From 1976 to 1986: Prospects, Problems and Patterns. Vol. 6: Forecasts of Long-Run Economic Growth, December 15 (Washington, D.C.: U.S. Government Printing Office), pp. 38-75.

MAYER, T. (1979), "A Comparison of Unemployment Rates and Income Fluctuations Prior to the Great Depression and in the Postwar Period", Review of Economics and Statistics, Vol. 61, No. 1, February, pp. 142-146.

MENSCH, G. (1978), "1984: A New Push of Basic Innovations?", Research Policy, Vol. 7, No. 2, April, pp. 108-122.

MENSCH, G. (1979), Stalemate in Technology (Cambridge, Mass.: Ballinger).

MENSCH, G., C. COUTINHO and K. KAASCH (1981), "Changing Capital Values and the Propensity to Innovate", Futures, Vol. 13, No. 4, August, pp. 276-292.

MENSCH, G., K. KAASCH, A. KLEINKNECHT and R. SCHNOPP (1980), Innovation Trends, and Switching Between Full- and Under- Employment Equilibria, 1950-1978, Discussion Paper No. 80-5, International Institute of Management, Berlin.

METCALF, J.S. (1981), "Impulse and Diffusion in the Study of Technical Change", Futures, Vol. 13, No. 5, October, pp. 347-359.

MINSKY, H.P. (1972), "Financial Instability Revisited: The Economics of Disaster", in Board of Governors of the Federal Reserve System, Reappraisal of the Federal Reserve Discount Mechanism, Vol. 3, (Washington, D.C.), June, pp. 95-136. Reprinted in MINSKY (1982b).

MINSKY, H.P. (1974), "The Modelling of Financial Instability: An Introduction", Modelling and Simulation, Vol. 5 (Pittsburgh: Instrument Society of America), pp. 267-273.

MINSKY, H.P. (1975a), "Financial Resources in a Fragile Financial Environment", Challenge, Vol. 18, July/August, pp. 6-13.

MINSKY, H.P. (1975b), "Financial Instability, the Current Dilemma, and the Structure of Banking and Finance", in Committee on Banking, Housing, and Urban Affairs, Compendium on Major Issues in Bank Regulation, 94th Congress, U.S. Senate, August, pp. 311-353.

MINSKY, H.P. (1977a), "A Theory of Systematic Fragility", in (eds.) E.I. ALTMAN and A.W. SAMETZ, Financial Crises: Institutions and Markets in a Fragile Environment (New York: Wiley), Ch. 6.

MINSKY, H.P. (1977b), "How 'Standard' is Standard Economics?", Society, March-April, pp. 24-29.

MINSKY, H.P. (1977c), "Banking and a Fragile Financial Environment", Journal of Portfolio Management, Summer. Attached with MINSKY(1977b) to his statement before the Joint Economic Committee, Congress of the United States, Special Study on Economic Change, Hearings, 95th Congress, 2nd Session, Part 3 (June 20, 1978), pp. 841-847, 847-854.

MINSKY, H.P. (1977d), "The Financial Instability Hypothesis: An Interpretation of Keynes and an Alternative to 'Standard' Theory", Nebraska Journal of Economics and Business, Vol. 16, Winter, pp. 5-16.

MINSKY, H.P. (1978), The Financial Instability Hypothesis: A Restatement, Thames Papers in Political Economy (London: Thames Polytechnic). Reprinted in MINSKY(1982b).

MINSKY, H.P. (1980), "Finance and Profits: The Changing Nature of American Business Cycles", in Joint Economic Committee, The Business Cycle and Public Policy, 1929-1980, 96th Congress of the United States, 2nd Session, Nov. 28 (Washington, D.C.: U.S.Government Printing Office), pp. 209-244. Reprinted in MINSKY (1982b).

MINSKY, H.P. (1981), "Financial Markets and Economic Instability, 1965-1980", Nebraska Journal of Economics and Business, Vol. 20, No. 4, Autumn, pp. 5-16.

MINSKY, H.P. (1982a), "The Financial Instability Hypothesis: Capitalist Processes and the Behavior of the Economy", in (eds.) C.P. KINDLEBERGER and J.-P. LAFFARGNE, Financial Crises: Theory, History and Policy (Cambridge: Cambridge University Press), pp. 13-47.

MINSKY, H.P. (1982b), Can 'It' Happen Again? (Armonk, N.Y.: Sharpe).

MINSKY, H.P. (1982c), Financial Innovations and Financial Instability: Observations and Theory, Working Paper No. 42, Department of Economics, Washington University, St. Louis.

MINSKY, H.P. (1982d), The Potential for Financial Crises, Working Paper No. 46, Department of Economics, Washington University, St. Louis.

MINSKY, H.P. (1982e), "Debt Deflation Processes in Today's Institutional Environment", Banca Nazionale del Lavoro Quarterly Review, No. 143, December, pp. 374-393.

NEAL, L. (1974), "Structural Breaks, Shifting Harmonics, or Random Effects?: Spectral Results of the Death of the Long Swing", Proceedings of the American Statistical Association, pp. 34-43.

NEAL, L. (1976), "Cross-Spectral Analysis of Long Swings in Atlantic Migration", Research in Economic History, Vol.1.

NEAL, L. (1978), "Is Secular Stagnation Just Around the Corner? A Survey of the Influences of Slowing Population Growth Upon Investment Demand", in (eds.) T.J. ESPENSHADE and W.J. SEROW, The Economic Consequences of Slowing Population Growth (New York: Academic Press), pp. 101-125.

PHELPS-BROWN, E.H. (1972), "Then and Now: The British Problem of Sustaining Development, 1900s and 1960s", in (eds.) M. PRESTON and B. CORRY, Essays in Honor of Lord Robbins (London: Weidenfeld and Nicolson), pp. 187-210.

PIATIER, A. (1981), "Innovation, Information, and Long-Term Growth", Futures, Vol. 13, No. 5, October, pp. 371-382.

POULSON, P.W., and J.M. DOWLING (1971), "Background Conditions and the Spectral Analytic Test of the Long Swing Hypothesis", Explorations in Economic History, Vol. 8, No. 3, Spring, pp. 343-351.

RAY, G. (1980), "Innovation in the Long Cycle", Lloyds Bank Review, No. 135, January, pp. 14-28.

ROON, G. VAN (1981), "Historians and Long Waves", Futures, Vol. 13, No. 5, October, pp. 383- .

ROSENBERG, N., and C.R. FRISCHTAK (1983), "Long Waves and Economic Growth: A Critical Appraisal", American Economic Review, Papers and Proceedings, Vol. 73, No. 2, May, pp. 146-151.

ROSTOW, W.W. (1975a), "The Developing World in the Fifth Kondratieff Upswing", Annals of the American Academy of Political and Social Science, No. 420, July, pp. 111-124.

ROSTOW, W.W. (1975b), "Kondratieff, Schumpeter, and Kuznets: Trend Periods Revisited", Journal of Economic History, Vol. 35, No. 4, December, pp. 719-753.

ROSTOW, W.W. (1976a), "The Bankruptcy of Neo-Keynesian Economics", Intermountain Economic Review, Vol. 7, No. 1, Spring, pp. 1-12.

ROSTOW, W.W. (1976b), "Technology and Price System", in (eds.) W. BREIT and W.P. CULBERTSON, Jr., Science and Ceremony. The Institutional Economics of C.E. Ayres (Austin, Texas: The University of Texas Press), pp. 75-113.

ROSTOW, W.W. (1977a), "Caught by Kondratieff", The Wall Street Journal, March 8, p.20.

ROSTOW, W.W. (1977b), "Regional Change in the Fifth Kondratieff Upswing", in (eds.) D.C. PERRY and A.J. WATKINS, The Rise of the Sunbelt Cities (Beverly Hill: Sage), pp. 83-103.

ROSTOW, W.W. (1978a), The World Economy: History and Prospect (Austin, Texas: The University of Texas Press).

ROSTOW, W.W. (1978b), "The Fifth Kondratieff Upswing and the Future of Economic Theory and Growth", 1978 Proceedings of the Business and Economic Statistics Section, American Statistical Association (Washington, D.C.: ASA), pp. 61-67.

ROSTOW, W.W. (1979a), Getting From Here to There (London: Macmillan).

ROSTOW, W.W. (1979b), Alternative Theories of the 1970s, Notes for Seminar Address, Economic Council of Canada, May 22.

ROSTOW, W.W. (1980a), "Cycles in the Fifth Kondratieff Upswing", in Joint Economic Committee, The Business Cycle and Public Policy, 1929-1980, 96th Congress of the U.S. 2nd Session, Nov. 28 (Washington, D.C.: U.S. Government Printing Office), pp. 34-66.

ROSTOW, W.W. (1980b), Why the Poor Get Richer and the Rich Slow Down (Austin: University Of Texas Press).

ROSTOW, W.W. (1982), "Comment From a Not Quite Empty Box", The Economic Journal, Vol. 92, No. 365, March, pp. 156-160.

ROSTOW, W.W. (1983), Review of Freeman et al., Journal of Economic Literature, Vol. 21, March, pp. 129-131.

ROSTOW, W.W., and F.E. FORDYCE (1978), "Growth Rates at Different Levels of Income and Stage of Growth: Reflections on Why the Poor Get Richer and the Rich Slow Down", Research in Economic History, Vol. 3, pp. 47-86.

ROSTOW, W.W., and M. KENNEDY (1979), "A Simple Model of the Kondratieff Cycle", Research in Economic History, Vol. 4, pp. 1-36.

ROWTHORN, B. (1976), "Late Capitalism", New Left Review, No. 98, July-August, pp. 59-83.

SAHAL, D. (1980), "The Nature and Significance of Technological Cycles", International Journal of Systems Science, Vol. 11, No. 8., pp. 985-1000.

SAHAL, D. (1981), Patterns of Technological Innovation (Reading, Mass.: Addison-Wesley).

SAMUELSON, R.J. (1979), "Introduction" (to "The Long Waves in Economic Life" by N.D. Kondratiev), Economic Impact, No. 26, pp. 60-61.

SHUMAN, J.B., and D. ROSENAU (1972), The Kondtratieff Wave (New York: Dell).

SOPER, J.C. (1975), "The Long Swing Revisited: Kuznets Cycles in Britain", Southern Economic Journal, Vol. 41, No. 4, April, pp. 570-579.

SOPER, J.C. (1978), The Long Swing in Historical Perspective (New York: Arno Press).

STOKES, C.J. (1980), "A Long Range View Based on the Kondratieff Cycle", Business Economics, January, pp. 20-23.

STOUT, D.K. (1980), "The Impact of Technology on Economic Growth in the 1980s", Daedalus, Winter, pp. 159-167.

TINBERGEN, J. (1981), "Kondratiev Cycles and So-Called Long Waves", Futures, Vol. 13, No. 4, August, pp. 258-263.

VOLCKER, P.A. (1978), The Rediscovery of the Business Cycle (New York: The Free Press).

WALLERSTEIN, I. (1978), "The Crisis of the Seventeenth Century", New Left Review, No. 110, July-August, pp. 65-73.

WALLERSTEIN, I. (1979), "Kondratieff Up or Kondratieff Down?", Review, Vol.2, No. 4, Spring, pp. 663-673.

WALLERSTEIN, I. (1980), "The Future of the World Economy", in (eds.) T.K. HOPKINS and I. WALLERSTEIN, Processes of the World System (London: Sage), pp. 167-180.

WALLERSTEIN, I. (1982), "Crisis as Transition", in S. Amin et al., Dynamics of Global Crisis (New York: Monthly Review Press), pp. 11-54.

WEISSKOPF, T.E., S. BOWLES and D.M. GORDON (1983), "Hearts and Minds: A Social Model of U.S. Productivity Growth", Brookings Papers on Economic Activity, No. 2, pp. 381-441.

WILSON, T. (1980), "1929-33 - Could It Happen Again?", The Three Banks Review, No. 128, December, pp. 3-20.

APPENDIX TWO

ADDITIONAL REFERENCES

(A) Historical Significance

CLARK, C. (1944), The Economics of 1960 (London).

EINARSEN, J. (1938a), "Reinvestment Cycles", Review of Economic Statistics, Vol. 20, No. 1, February.

EINARSEN, J. (1938b), Reinvestment Cycles and Their Manifestation in the Norwegian Shipping Industry (Oslo: The University Institute of Economics), Publication No. 14.

GARVY, G. (1943), "Kondratieff's Theory of Long Cycles", Review of Economic Statistics, Vol. 25, No. 4, November, pp. 203-220.

KONDRATIEFF, N.D. (1925), "The Static and Dynamic View of Economics", Quarterly Journal of Economics, Vol. 39, pp. 575-583.

KONDRATIEFF, N.D. (1935), "The Long Waves in Economic Life", Review of Economic Statistics, Vol. 17, No. 6, November, pp. 105-115. This is a translation of "Die Langen Wallen der Konjunktur", Archiv für Sozialwissenschaft und Sozialpolitik (1926), Vol. 56, No. 3, pp. 573-609, The translated paper has been reprinted in Readings in Business Cycle Theory (1944), Lloyds Bank Review (1978), National Journal (1978), Economic Impact (1979) and Review (1979). References in text are from the 1944 reprint.

KUZNETS, S. (1929), "Retardation of Industrial Growth", Journal of Economic Business History, Vol. 1, pp. 534-560.

SCHUMPETER, J. A. (1935), "The Analysis of Economic Change", Review of Economic Statistics, Vol. 17, No. 4, May, pp. 2-10. Reprinted in (eds.) G. HABERLER et al. (1951), Readings in Business Cycle Theory (Philadelphia: The Blakiston Co. for the American Economic Association), pp. 1-19.

SCHUMPETER, J.A. (1939), Business Cycles, Vol. I (New York: McGraw Hill).

TINBERGEN, J. (1938), "Lag Cycles and Life Cycles", in (eds.) L.H. KLAASEN, L.M. KOYCK and H.J. WITTEVEN (1959), Selected Papers (Amsterdam: North Holland).

(B) Other Cited References

BALOGH, T., and A. GRAHAM (1979), "The Transfer Problem Revisited: Analogies Between the Reparations Payments of the 1920s and the Problems of the OPEC Surpluses", Oxford Bulletin of Economics and Statistics, Vol. 41, No. 3, August, pp. 183-191.

BELLAMY, R. (1976), The Causes of Capitalist Accumulation, Crises as Periodic Interruptions in its Rhythm. The Historical Tendency of Capital Accumulation, Mimeo., University of Leeds.

BORDO, M.D., and A.J. SCHWARTZ (1980), "Money and Prices in the Nineteenth Century: An Old Debate Rejoined", The Journal of Economic History, Vol. 40, No. 1, March, pp. 61-67.

COOMBS, R. (1983), The Diffusion of Mechanization Technology and Long-Wave Theories, Mimeo., University of Menchester Institute of Science and Technology.

DAY, R.H. (1982), "Irregular Growth Cycles", American Economic Review, Vol. 72, No. 3, June, pp. 406-414.

FORRESTER, J.W. (1969), Urban Dynamics (Cambridge, Mass: The MIT Press).

HABERLER, G. (1941), Prosperity and Depression, Third Edition (Geneva: League of Nations).

HESTER, D.D. (1981), "Innovations and Monetary Control", Brookings Papers on Economic Activity, No. 1, pp. 141-199.

HESTER, D.D. (1982), "On the Adequacy of Policy Instruments and Information When the Meaning of Money is Changing", American Economic Review, Papers and Proceedings, Vol. 72, No. 2, May, pp. 40-44.

HOWREY, E.P. (1972), "Dynamic Properties of a Condensed Version of the Wharton Model", in (eds.) B.G. HICKMAN, Econometric Models of Cyclical Behavior (New York: Columbia University Press), Vol. II, pp. 601-663.

ITOH, M. (1979), Review of 'Late Capitalism', Science and Society, Vol. 43, No. 1, Spring, pp. 106-109.

ITOH, M. (1980), Value and Crisis (London: Monthly Review Press).

KINDLEBERGER, C.P., and J.-P. LAFFARGNE (eds.) (1982), Financial Crises: Theory, History and Policy (Cambridge: Cambridge University Press).

KOOPMANS, T.C. (1975), Three Essays on the State of Economic Science (New York: McGraw-Hill).

LUCAS, R.E. (1977), "Understanding Business Cycles", in (eds.) K. BRUNNER and A.H. MELTZER, Stabilization of the Domestic and International Economy (Amsterdam: North-Holland), pp. 7-29.

MASS, N.J. (1980), "Stock and Flow Variables and the Dynamics of Supply and Demand", in RANDERS (1980), Ch. 5.

MAYER, T. (1982), "Financial Innovation- The Conflict Between Micro and Macro Optimality", American Economic Review, Papers and Proceedings, Vol. 72, No. 2, May, pp. 29-34.

MEADOWS, D.H. (1980), "The Unavoidable A Priori", in Randers (1980), pp. 23-57.

MEYER, L.H. (ed.) (1981), The Supply-Side Effects of Economic Policy (St. Louis: The Federal Reserve Bank of St. Louis).

MINSKY, H.P. (1975c), John Maynard Keynes (New York: Columbia University Press).

MITCHELL, W.C. (1927), Business Cycles, The Problem and the Setting (New York: NBER).

NELSON, R.R., and S.G. WINTER (1974), "Neoclassical vs. Evolutionary Theories of Economic Growth: Critique and Prospectus", Economic Journal, Vol. 84, No. 336, December, pp. 886-905.

NELSON, R.R., and S.G. WINTER (1975), "Growth Theory From an Evolutionary Perspective: The Differential Productivity Puzzle", American Economic Review, Papers and Proceedings, Vol. 65, No. 2, May, pp. 338-344.

NELSON, R.R., and S.G. WINTER (1977), "In Search of Useful Theory of Innovation", Research Policy, Vol. 6, No. 1, January, pp. 36-76.

PATINKIN, D. (1973), "In Search of the 'Wheel of Wealth': On the Origins of Frank Knight's Circular-Flow Diagram", American Economic Review, Vol. 63, No. 5, December, pp. 1037-1046.

RANDERS, J. (ed.) (1980), Elements of the System Dynamics Method (Cambridge, Mass: The MIT Press).

ROWLEY, J.C.R., and P.K. TRIVEDI (1975), Econometrics of Investment (London: John wiley).

SAMUELSON, P.A. (1948), Economics: An Introductory Analysis (New York: McGraw-Hill).

SENGE, P.M. (1977), "Statistical Estimation of Feedback Models", Simulation, Vol. 28, No. 6, June, pp. 177-184.

SINAI, A. (1977), Discussion in (eds.) E.I. ALTMAN and A.W. SAMETZ, Financial Crises: Institutions and Markets in a Fragile Environment (London: Wiley), pp. 187-203.

SUZUKI, M. (1965), "A Spectral Analysis of Japanese Economic Time Series Since the 1880's", Kyklos, Vol. 18, Fasc. 2, pp. 227-258.

WOJNILOWER, A.M. (1980), "The Central Role of Credit Crunches in Recent Financial History", Brookings Papers on Economic Activity, No. 2, pp. 277-339.

(C) Further References

ABRAMOVITZ, M. (1961),"The Nature and Significance of Kuznets Cycles", Economic Development and Cultural Change, Vol. 9, No. 3, April, pp. 225-248. Also reprinted in (eds.) R.A. GORDON and L.R. KLEIN (1966), Readings in Business Cycles (London: George Allen and Unwin), ch. 27.

ABRAMOVITZ, M. (1968), "The Passing of the Kuznets Cycle", Economica, Vol. 35, November, pp. 349-367.

CHAPTER THREE

THE DEMISE OF STRUCTURAL ESTIMATION?

The "face" of econometrics may be represented by three principal features. First, economic theory provides a realistic account of some phenomena, often expressed in mathematical form. Realism is an essential requirement here since this theory is to be combined with data from real sources. Second, estimation by statistical procedures is added to this account. This yields estimated values for some parameters in the mathematical expressions that represent either individual or average experience. Finally, inference (as "significance", "confidence" or some other statistical notion) has a quite modest role acknowledging the potential imprecision of these values. These three features, taken together, comprise the predominant form of econometrics for which the structural component is essential. Chow (1978, p. 565), rearranging these features, provides a typical view. "As is well known, the application of econometrics consists of two steps: The formulation of a statistical model or a set of hypotheses concerning the economic phenomena in question, and the application of statistical methods to test selected hypotheses, to estimate the parameters, to make forecasts, to study the dynamic properties of the model, and/or to make decisions".

Such features or stages are consistent with most definitions of econometrics as found in our professional literature. The basic involvement with structural equations is commonplace. Kelejian and Oates (1974, p.237), for example, indicate that such equations are "suggested by economic theory". Unfortunately the structural basis is too readily accepted without adequate examination either of its validity or of the individual features. Some deficiencies are readily apparent. When Dutta (1975, p. 10), in describing econometric research, suggests that "the prime objective is to analyze the behavior pattern of mankind" and that the basis of structure is a belief that "the true or fundamental behavior pattern of mankind is based on postulates of rational and consistent behavior", it is perhaps clear that his comments have little direct bearing in practice. However, the simpler statement by Kelejian and Oates is little more informative. It could be argued that this reflects a glib avoidance of a necessary degree of explanation and that the brevity is consistent with the proposition whereby "econometrics is something that should be done, rather than talked about."

Much of the critical reappraisal of econometrics in recent years should be seen as questioning certain aspects of structure. A secondary theme would be the development of alternative operational procedures to replace or supplement the structure-confirming procedure that is implicit in the three simple features described above. It is significant that Chow felt impelled to add a further stage, involving the reformulation of a model after statistical analysis, to his two-stage framework. Such structure-seeking is an important change in focus since it casts doubt on the sufficiency of economic theory as the source of structural specification. The critical reappraisal, which we shall illustrate below, involves both the dissatisfaction of economists with past efforts in research and the changes stemming from rapid computational advance. These two aspects can be illustrated by reference to Haavelmo and to Box and Jenkins.

Haavelmo (1958, pp. 354-5) provides an excellent example of an economist who was very influential in the development of the structural component in econometrics but became sceptical with the results achieved in practice. "It has become almost too easy to start with hard-boiled and oversimplified 'exact' theories, supply them with a few random elements, and come out with models capable of producing realistic-looking data." His view is that part of the problem here is due to "the somewhat passive attitude of many econometricians when it comes to the choice of axioms and economic content of the models we work on." The obvious implication to be drawn here is the need for greater emphasis on economic theory and careful reconciliation of fitted models with their theoretical underpinnings. Inadequacy of theoretical linkages would then seem a primary source of structural instability. We shall return to this when we consider Frisch's concept of autonomy below. We shall argue that this particular concept is an invaluable means of connecting the "rational-expectations revolution" (and its critical reappraisal of Keynesian macro-econometric models) with the mainstream of econometrics.

With respect to the impact of computational advances, it is important to note significant changes in the organization of research methods as well as the additional complexity of individual techniques. The key fact for structural analysis is that data are now handled many times and in novel ways. Thus the choices of representations for economic theory are often modified within research rather than fixed at the outset. These choices are adjusted at various occasions within an explicit sequence of stages. The sequence begins with the "face" described earlier and repeats it as often as the researcher wishes to continue re-expression of the theory. Clearly the lengthening of this process undermined any notion that economic theory, in isolation, has primacy in determining structural forms. Some years ago, this would be termed "data mining" by economists. Now "data analysis" or "criticism" might be preferred and the acceptability of such behaviour has markedly increased. So much so that statistical textbooks are beginning to contain descriptions of iterative procedures. Box and Jenkins (1970, Ch. 1) provide an influential example with their account of sequential analysis, although they fail to give adequate attention to the statistical consequences of following their scheme. They indicate well-defined stages in model-building that are not necessarily based on the fiction of a given structure. Similar schemes (procedures) may, in fact, be structure-seeking rather than structure-confirming but sometimes no structure (other than one of convenience) is even envisaged. Box (1979) seeks to clarify such matters.

Given this background, it is worth recalling a view expressed by Klein (1947, p. 111) to show how fundamentally the bases of econometrics have changed with these adjustments. "It is desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgement of a particular investigator. Econometric models are put forth in this scientific spirit, because these models, if fully developed and properly used, eventually should lead all investigators to the same conclusions, independent of their personal whims." This ideal of objectivity is quite inconsistent with the softness of present econometric practice. It might have been acceptable in an environment where economic theory is sufficiently well developed to give clear and substantial guidance for the specification of structure and where statistical theory indicates only one optimal method of estimation. Data would also need to be compatible with economic concepts and to be collected at sampling intervals equal to the planning horizons of economic agents. That this environment does not prevail is now obvious. The extraordinary zeal that characterized the early days of the Cowles Commission (and is apparent in Klein's comments) has disappeared to be replaced by pragmatism. The principal victim of this change in ideals is the notion of structure.

We intend to look at a critical period in the history of econometrics with the hope that a re-opening of past controversies will enhance the understanding of the present reappraisals of econometrics. As familiar conventions are being challenged, it seems appropriate to reconsider the period during which they emerged and became predominant. The period extends from about 1940 to 1960. It begins with the initial discussions of Tinbergen's economy-wide econometric models and ends with the sterile development of approximation theorems that foreshadowed the movement of such models toward commercial success. Klein (1971b) describes this as the beginning of the modern era of econometrics and the first part of the subsequent era of consolidation. We shall consider his periodization after a brief look at current controversies. Then we turn to particular issues that dominated econometric discussions during the two decades under review. It is clear that these discussions were preoccupied with structure. We believe the discussions reveal the gradual recognition of softness in econometrics that separates the formal presentation of mathematical theorems and actual practices of researchers. At the end of the period, this separation can be discerned in the polarization of two distinct lines of development. One line pursued the extension of statistical theory without giving adequate attention to the economic context. The other line ignored many of the theoretical developments in statistics and, influenced by the extensions of computational facilities and increased availability of data, became "applied" econometrics.

Progress in structural analysis was inhibited by this polarization and by the features that have come to represent each line of development. The loss of structural content in research that is narrowly focused on statistical theory is obvious. It may be less apparent in applied econometrics. Here again Haavelmo (1958, pp. 356-7) provides a useful characterization of the weak line between economic theory and its expression in so-called structural models. "From this veritable maze of interrelations our customary economic theory extracts some would-be 'net' relations between statistically observable data on prices, quantities, etc., in the economy. The only trace left of the whole 'background structure' will then be the presumably constant parameters of the 'net' relationships derived. At this final stage, the thread between the original, hypothetical invariants of the theory and the derived relationships between market variables has indeed become long and thin." Although a quarter century has passed since these comments were presented in a presidential address to the Econometric Society, they seem just as relevant today as they were then. The softness of this linkage remains a major obstacle in the interpretation of fitted economic models.

Some Present Difficulties

The problems of econometrics can be approached from two perspectives. We could follow the path of Thurow (1983, p. 105) and point to the "failure" of econometrics to live up to expectations generated by the early activities of Tinbergen, the Cowles Commission, and subsequent advocates of its use to guide policy decisions and to clarify significant issues. "Econometrics has not proven capable of providing either accurate forecasts or conclusively settling economic disputes. Key variables ... don't seem susceptible to econometric modeling. In many areas, the stable equations that economic theory depends on don't seem to exist. Economic evidence is often contradictory, and even where it is consistent, the conclusions have been wrong so many times that the credibility of even consistent results is suspect and can be ignored by those who want to." We could point to the inability of economic theory to specify secondary variables, the need to use proxy variables for unmeasured theoretical concepts, the uncertainty of functional forms for structural equations, the widespread use of search strategies in computation, the preoccupation among many economists with familiar static theory rather than dynamic theory, and the observational indistinguishability of rival theories.

The alternative perspective , with which we concur, rejects this stress on failure as excessive. The expectations were always unreasonable since they ignored the inherent softness of econometrics in practice. Thurow uses "Econometrics - An Icebreaker Caught in the Ice" as the title of the chapter where he discusses these matters. We suggest that the structural framework of econometrics was never adequate to serve as an "icebreaker" so the criticism is ill-directed. The current problems of econometrics do not stem from these failures of "applied" econometrics but rather with our difficulties in interpreting the outcome of research activity. Indeed the failures represent the consequences of repeated overstatement concerning the firmness with which empirical evidence could be used. This overstatement finds expression too in the widespread neglect of qualifying statements in our literature. There occurs, as a result, much confusion of soft and hard elements in econometric practice. We illustrate the confusion by considering Sims' test of causality in Rowley and Jain (1984). It is our hope that a review of the disputes that occurred among econometricians during the period 1940-1960 will yield a better understanding of inherent softness. That this was neglected in the last forty years is really not a failure of econometrics. It is a failure of economists to accept the full demands of statistical procedures, which involve qualifications and imprecisions as well as simple technical exercises.

The stress on interpretation of the second perspective directs attention away from the disappointments of economists and toward technical issues that cloud our understanding of empirical evidence. These issues are worth listing and it is convenient to put them into six groups. It should not be surprising if we argue that structural dimensions are fundamental in all of the groups. Nor should it be surprising if we eventually conclude that the technical issues reveal the struggles of econometricians to accept softness in specification, experimentation, and estimation. The key is to accept softness and to interpret evidence accordingly. Econometrics is enhanced by such steps. It will never satisfy Klein's early ideal of objectivity and it will not eliminate all of the practical misunderstandings noted by Thurow. It will, however, have a chance to provide a scientific basis for treating empirical evidence on economic matters involving confirmation, explanation and prediction. The six groups of technical issues can be characterized by the following headings: structural instability, non-structural purposes, statistical constraints, sequential analysis, simulative experiments, and robustness.

Structural instability is so frequently reported by economists that there is no problem in finding examples which have profoundly affected both economic theory and econometric practice. Among these, the Phillips curve and money-demand relationships should be noted. Their apparent instability (apart from its encouragement to the use of recursive residuals and generalization of the Chow test) is associated with radical adjustments to theories of inflation and wage determination, to measures of unemployment, and to the acceptability of particular monetary aggregates especially in conjunction with the advocacy of monetary targets and rules. Instability is also a major problem for estimation criteria such as consistency, asymptotic unbiasedness and asymptotic efficiency, which are frequently cited by econometricians. It has also been a major component in the Lucas critique of Keynesian macroeconometric models, as illustrated in Lucas (1976) and Lucas and Sargent (1979), which we shall consider below.

Non-structural purposes is a heading that can be attached to situations in which time-series models, of the type associated with Box and Jenkins, for example, are considered as alternatives to the structural models of the simultaneous-equation type. There are many instances where the time-series models could provide superior predictions and it is commonplace to find this superiority acknowledged even by proponents of structural models. In fact, there is not much in the statistical framework of structural models that can be used to demonstrate their desirability outside the narrow confines of certain arbitrary criteria. This is especially true for non-interventionary post-sample predictions. For several years, there was competition between the two approaches. This was unfortunate and we want our heading to also include the more sensible situations in which they were combined. With this in mind, we should point to Granger-consistency (where model assumptions are linked to the known range, for example, of variables), to SEMTSA discussions that stem from Tinbergen and Quenouille but which are now principally associated with Zellner and Palm (1974) and to the use of the time-series characteristics of economic variables by proponents of "rational expectations" to determine parameters of distributed lags. In such situations, time-series elements provide valuable assistance in the specification of structural models. Eventually they may result in less instability and simpler interpretation of empirical results.

The third group covered by statistical constraints contains the reformulation of exogeneity by Hendry, Richard, Geweke and others, which is clearly foreshadowed by the discussions of econometricians in 1940-1960. Partitioning of measured variables into types involving exogeneity will form a major topic in our description of these discussions. The choice of partitions in practice is an important source of softness for the elimination of which few effective solutions have been offered. Index models are also included in this group of technical issues. These discard many statistical constraints that have been used since about 1944 and replace them with others. The result is a two-fold characterization of blocks of economic variables in terms of a constrained vector autoregressive process and of a vector moving-average representation. The first form picks up the basic notion of co-movements of economic variables through business cycles while the second one can be linked to a system of "innovation-accounting", whereby the effects of shocks on economic structures can perhaps be traced from their inception. Sims (1982) illustrates the feasibility of this VAR approach and attempts to demonstrate its usefulness. The principal feature of the approach, as revealed in Sargent and Sims (1977), is abandonment of "conventional structural macroeconometric models". Koopmans (1947) had used the slogan "Measurement Without Theory" to advance the acceptance of such models at the expense of certain attitudes associated with the NBER in the interwar period. With a clever inversion of his slogan, Sargent and Sims sought to identify their index model with NBER methodology and to reject the approach of the Cowles Commission as propounded by Koopmans. One structural type was being substituted for another. We consider this below when we discuss the primacy of the probability approach to econometrics as claimed by Haavelmo and Koopmans in the 1940s.

Some aspects of the fourth group of technical issues were discussed by Bancroft before structural analysis gained its prominence in the empirical research of economists. In 1944, he pointed to the potential consequences of sequential analysis due to incomplete specification of structure. The use of significance tests to guide structural choice was identified with "pre-test" bias. Judge and Bock (1978) show how this argument was revived in the 1970s after it had been largely neglected during the period that we shall consider. Bancroft had established the need to adjust probabilistic statements when structure-seeking behaviour is based on insufficient prior knowledge. Many economists were unaware of his contribution. They combined structure-confirming probabilistic statements with computational software that "found" best equations as if the validity of such statements persisted in softer contexts. Clearly this delinquent behaviour could be a major factor in the shortfalls of "applied" econometrics noted by Thurow. Unfortunately, it cannot be adequately treated here since, as we have already suggested, the debates that occurred during 1940-1960 seldom gave any attention to sequential analysis. Instead we shall resort to a tactic of noting corresponding technical issues as they appear relevant to the projection of the contents of earlier debates to other current difficulties.

Turning to the fifth group, we find such a dramatic change in focus that many econometricians fail to identify them as part of their own discipline. We feel that simulative experiments should be considered part of econometrics even when they do not involve either statistical estimation or real data. To some extent, they represent a soft extreme that has yet to find a secure position in the splitting of economics into componental areas. Traditional definitions of econometrics, as modified to recognize the new feasibilities of computers, are not unduly strained by the inclusion of simulative experiments. Since we have already taken time-series models as part of econometrics, the further addition might not be surprising. It is, however, contrary to the treatment by Zellner (1980), for example.

There is little difficulty with the integration of one particular form of simulation. This involves the use of a fitted structural model to predict future paths of variables perhaps as part of a hypothetical experiment involving known changes of policies or exogenous shocks. Two other forms of simulation have been used in economic contexts. One involves the system-dynamics framework of Forrester and his associates while the other might be termed micro-econometric synthetic modelling. Examples are provided by Forrester (1980) and Smith (1982). They reveal novel uses of structural concepts. The system-dynamics approach discards statistical estimation of parameters in favour of the adjustment of "tuning constants" so that the cyclical characteristics of time-series data are replicated. Smith (p. 929) supports his advocacy of the second approach by pointing to a major problem of conventional econometrics. "Rarely are we able to obtain a test of the model specification. Hence, an econometric model provides a mapping from specifications into conclusions about preferences, technology, and institutions. Insofar as the conclusions are sensitive to the specifications, we are left with scientific propositions that are opened with respect to the environment, institutions, and agent behavior." The laboratory-based, or controlled-field, experiments of Smith and his associates take structural sensitivity as their principal focus rather than leaving it to be discussed by appraisal of later post-sample model "failures".

Sensitivity is also a major element in the final group of technical issues. Robustness considerations presume that potential mis-specification of models is to guide the choice of estimation techniques. Instead of the prior choice of a single structural framework to be explored in isolation, the existence of alternative ones (especially as they affect the distributions of errors) consistent with the economic context must be acknowledged. Then a final choice represents a compromise among alternative frameworks. The structural basis is not rejected. It is simply more tentative, or soft as we have preferred to call such situations.

The overall impression to be derived from this brief list of issues is that the use of structure by econometricians is being reviewed from many viewpoints. We see a radical evolution of econometric practice away from the conventions established in the period 1940-1960 and toward approaches with softer bases. The rigid application of the simultaneous-equation model as confirmation of a given structural form is being overturned by innovations that often use structural notions with less precision. The current crisis of econometrics is due to the abundance of methods. It was comfortable both to have a consensus as to how to proceed with research and to be free from the need to justify particular steps. This state disappeared with the failures of "applied" econometrics and the burgeoning of econometric techniques.

Periodization

The origins of econometrics can be traced back over seventy years although its formal organization began with the founding of the Econometric Society a half-century ago. Few have attempted to divide the history of econometrics into reasonably distinct periods. Klein (1971b) is an important exception. It seems appropriate to note his attempt since the interval 1940-1960, on which we want to focus attention, does not quite fit his periodization. We differ with respect to both ends of our interval and we would argue that Klein's particular choices are affected by his close identification with the conventional simultaneous-equations model (SEM) and by his stress on the size dimension of computing. Clearly

Klein was influenced by his personal experiences and preferences. He was earlier part of the group at the Cowles Commission that did so much to establish the SEM. His description fails to give adequate attention to historical developments with respect to time-series analysis and to the activities of the NBER, perhaps because of a narrower vision of the boundaries for econometrics. In discussing the impact of computational improvements, it is significant that Klein looked to the inclusion of input-output matrices and to geographic expansion both within developed countries and from these to developing countries. During the 1970s, he was to provide leadership for completion of these tasks. He ignored the impact of computational improvements on finite-sample analysis and robust statistics. In summary, he ignores deviations from the SEM structural paradigm of the Cowles Commission (many of which we have noted above). The result is an exaggeration of the lasting value of the SEM but a good representation of attitudes that were common a decade ago.

Klein distinguishes a "pre-econometrics" period, when empirical results were concerned with simple formulae such as those associated with Malthus and Pareto, and a founding era under the impetus of the classical statistical theory of Karl Pearson and Yule. In the latter period of "early econometrics", he separates the use of multiple correlation by Moore (1914), Douglas (1928), Schultz (1938) and Tinbergen (1939) from the efforts of Working (1927) and Frisch (1934), which were precursors of later views due to their treatment of such concepts as identification, autonomy and normalization. This partition is partisan, giving insufficient credit to the activities of the first set of authors. Schultz's book, which summarizes earlier efforts and gives special credit to Moore, remains an excellent account of the difficulties faced in the derivation of a stable structural expression of economic theory. When Thurow (1983, p. 115) insists "econometrics functions cannot be regarded as solid and permanent", he is only reporting a position given its best exposition in Schultz. Turning to Tinbergen, we note the first attempts to develop economy-wide econometric models. His analysis of the final form of such models was, as we have already indicated, the first integration of the stochastic difference equations of economic time series with structural models (or SEMTSA in the mnemonic adopted by Zellner). Tinbergen's influence in this respect can be continuously traced from his initial efforts through the work of Orcutt (1948), Quenouille (1957), Goldberger (1959) and others to the present. He cannot be dismissed as "non-modern" simply because he preferred to use least-squares estimates! Klein omits this linking due to his neglect of time-series developments in his periodization.

The onset of the "modern" era is attributed by Klein to the adjustments that followed the innovations of Haavelmo (1943-1944) and Mann and Wald (1943) "who formulated the econometric problem in terms of the theory of statistical inference". This represents a major shift in emphasis. "From their contributions the subject of econometrics became a special branch of mathematical statistics- the making of statistical inferences from non-experimental data". This is a dangerous exaggeration which diminishes econometrics (by disturbing the balance of economics and statistics) while treating it as a portmanteau for explorations of non-experimental data. The economic flavour is supposed to arise "from being tied to the equation systems that are derived from propositions of theoretical (mathematical) economics".

Klein indicates a period of " consolidation" extending from the early 1950s to the mid-1960s. It was followed by the "computer age". We prefer to maintain the integrity of the span from the situation established by Tinbergen, Schultz and others (about 1940) to the loss of structural impetus in about 1960. This period saw the establishment of the SEM as the conventional approach to econometrics. After a short period of intense lobbying, the SEM came to dominate much econometrics for four decades, especially in textbooks and increasingly in professional journals. With some lack of reverence, it might be called the state religion. This acceptability is now under severe strain from the controversies surrounding the technical issues we have listed above. Awareness of challenges to the primacy of the SEM is heightened by the calm of the 1960s when debate was limited. In contrast, inspection of period from 1940 to 1960 reveals substantial questioning of structural analysis as embedded in the SEM before criticisms subsided and interest shifted either to sterile mathematical puzzles, such as approximation theorems, or to the practical demands of large-scale computing.

Focus on the period 1940-1960 yields a number of propositions. First, many of the recent criticisms of econometrics are best understood as flowing from issues raised in the first two decades of the modern era and subsequently neglected. Second, the shift toward mathematical and computational biases, already clear by 1960, resulted in one and a half decades of lost opportunities. Present crises may represent a reversal to greater eclecticism and a revival of progressive elements in our subject-matter. Mathematical and computational strands remain significant but their role is being reinterpreted. In summary, our vitality is returning after years of atrophy. Our final proposition is a useful means of revealing this vitality. Econometrics is not a special branch of mathematical statistics, which view must inhibit the recognition of its inherent softness due to the use of non-experimental data and to a host of other factors. With this backdrop, we turn to some particular issues that arose in the first two decades of the modern era.

Particular Historical Issues

During his exchange with Tinbergen in 1930-1940, Keynes referred to econometrics as "statistical alchemy" and questioned whether it was ripe to become a branch of science. Resilience in the face of his criticism showed that a new confidence was coming to characterize the efforts of econometricians. This is especially apparent in the efforts of Haavelmo (1940, 1943b) to widen the appreciation of the consequences of stochastic errors in economic models. First, he took up the prevailing interest in trade cycles and emphasized that dynamic behaviour of a variable generated by stochastic difference equation will depend on the nature of the errors disturbing this process. Theoretical solutions of deterministic models would therefore not be appropriately compared with actual movements since they only focused on the values of signal parameters. A search of trade-cycle theory readily reveals the preoccupation with such values. Samuelson's classic combination of the multiplier and accelerator is now the best-known illustration. Haavelmo (1940, p.321) provides a strong econometric challenge to this preoccupation. He indicates the hazards of developing economic theory with only a casual inspection of real circumstances and without a sufficient econometric basis. "'Correction' of the form of a priori theory by pure inspection of the apparent shape of time series is a very dangerous proceeding and may lead to spurious 'explanations'." His criticism of routine trend fitting without stochastic modelling is still relevant. Its present counterpart is provided by Nelson and Kang (1981, 1984). However, the principal thrust of his arguments is to derive an important wedge between non-econometric explorations of cyclical behaviour and the "class of admissible hypotheses" for such cycles.

This challenge was enlarged in scope when Haavelmo (1943b) sought to clarify the "testability" of theories against facts as part of a defence to Tinbergen against Keynes' strictures. The pioneering efforts of Tinbergen, Schultz and others were providing a strong base for econometrics to move from relative obscurity into a position of greater prominence. We feel that this move is the turning point that marks the beginning of the modern era in econometrics. The need for a proper framework within which to confront structural theories with facts is the key element here. This is quite independent of a presumption that this framework should take the narrow form that developed from Haavelmo's acceptance of a particular perspective. As we shall indicate below, this form ultimately failed to recognize the inherent softness of the economic context. The need to find means of confronting theories and facts remains whether we use the SEM, which emerged as the Cowles Commission pursued Haavelmo's particular perspective, or alternative approaches that lack its firmness.

Haavelmo (1943b, p.13) makes three simple claims. These are the backdrop for our commentary. He suggests (i) "there is no harm in considering economic variables as stochastical variables having certain distribution properties"; (ii) "only through the introduction of such notions are we able to formulate hypotheses that have a meaning in relation to facts"; and (iii) "these notions are precisely the tools for an objective and intelligent discussion" of the issues raised in the Keynes-

Tinbergen exchange. We shall argue that there is harm in assuming <u>certain</u> distributional properties, that the introduction of stochastic formulations (though desirable) may be insufficient, and that excessive rigidity in the use of statistical tools has indeed harmed the chances of objective and intelligent discussion of economic structures. In making these arguments, we are not rejecting the three simple claims. We are qualifying them in the light of actual developments over the last four decades. These qualifications do not weaken the underlying theme of Haavelmo's work. They just recognize more complexity in applying that theme to economic phenomenon. This is consistent with econometrics as a branch of science.

<u>Organization - A List of Topics</u>

A common feature in modern econometrics is the partition of measured economic variables into two polar types that are labelled "endogenous" and "exogenous". The criteria used for partitions vary but their essential ingredient, when dealing with the statistical properties of estimators, involves either independence or lack of correlation. They are, therefore, associated with Haavelmo's first simple claim for assuming distributional properties of variables. Consideration of exogeneity will be the first topic that we shall address below. Our primary concerns will be with the definition of exogeneity and with the confusion between control and exogeneity. The former is a difficulty for determining how operational are the guidelines on exogeneity given in our professional literature. The latter affects both the acceptability of certain hypothetical experiments and the interpretation of multipliers that are based on fitted structures after a partition of the variables into the polar types.

Our second topic is the primacy of the so-called "probability approach" to econometrics. This too can be linked with Haavelmo (1943b, p. 18), who suggested that our treatment of a system of economic variables should "be such that it can finally be boiled down to a statement about the joint probability law of the observable variables involved". This view yielded the title of his important supplement to Econometrica in 1944 and dominated the subsequent activities of the Cowles Commission to find final expression in the SEM. The topic involves several components, some of which will be stressed. They are very significant for current reappraisals of econometrics and serve to link attitudes concerning autonomy and stability that have been present throughout the history of the subject. To heighten this continuity we shall move outside the period 1940-1960 to note both the significant role of Frisch and the unsettling criticisms of the economists who assume "rational" expectations.

The third topic was initiated by Orcutt (1952), who pointed to the restricted potential for control of exogenous variables. Although many published papers contain graphical displays or tabulations of dynamic multipliers that represent the impact of changes in individual exogenous variables (or policy variables) on endogenous variables, such calculations have little merit unless they acknowledge the interactions among exogenous variables and the feasibilities of control. The emphasis here is the treatment of such interactions and Orcutt's stress on the need for testing exogeneity. We shall also consider the eo ipso predictors of Wold and the problem of ceteris paribus assumptions in a regression model with latent, or "lurking", variables. Looking outside the period 1940-1960, we shall briefly cite Wright's development of path-coefficient models which attempt to deal with interactions among exogenous variables. We also look at the recent revival of interest in exogeneity that stems from attempted classifications of causality by Granger (1969), Sims (1972a,b), and Engle, Hendry and Richard (1983).

It should be clear that these three topics are closely connected to the ideal of the SEM and, as such, have to be discussed with the early contributions of the group of econometricians at Cowles Commission very much in mind. This linkage is somewhat reduced when we turn to the next three topics. In turn, we shall look at false constraints, proximity and purpose. The first of these will serve as a basis for exploring the contribution of economic theory to model specification. Klein (1982, p. 112) has suggested for the 1940s that "the rooting of model specification in received economic doctrine was firmly established" but this is difficult to sustain when we inspect fitted models. In Klein (1971a, p. 136), he has made apparent some arbitrariness. "The 'true' economy is a complicated Walrasian type model... But if we drop the Walrasian objective and content ourselves with an aggregative system, where shall we draw limits on size? There is no unique aggregative representation of an economy. Many alternative versions provide different approximations to the 'true' system." Often the Walrasian objective is reduced to an argument for large structures rather than small ones. Klein is the most distinguished builder of large Keynesian macroeconometric models that reveal few Walrasian features. Indeed critics of such models, including Lucas and Sargent (1979) and Miller (1978), find the constraints of them inconsistent with general equilibrium theories. An early opponent of these constraints was Liu (1955, 1960). We shall use his papers to support our discussion of false constraints and the size of models.

The topic of proximity begins with the efforts of Wold to consider the consequences of applying the least-squares principle outside the classical linear model. His treatment of sensitivity foreshadows the shift toward the derivation of approximation theories and the use of block-recursivity to justify models of reduced size or ARMAX models. Although Wold's efforts were initially seen as a conservative reaction seeking to preserve the acceptability of least-squares estimation and single-equation formulations in the face of an onslaught from proponents of the SEM, this view is unfair as we hope to show. When we turn to purpose of their empirical research with the statistical procedures that they choose. Marschak and other members of the group at the Cowles Commission sought to justify the use of the structural form of the SEM for policy experiments. This justification has been combined with one of relative stability to yield a demand for asymptotic unbiased and consistent estimation. However there are weaknesses in the steps of this argument. There is little in asymptotic properties of estimates to confirm optimality in conditional predictions. In discussing corresponding issues, we have fewer contributions on which to draw and the overall impression that emerges (for now as well as for 1940-1960) is one of dissatisfaction.

The final topic that we consider is better served by econometric literature although its implications are seldom given full attention. Systems of equations often require the imposition of normalization constraints that will permit them to be written in a form acceptable for some methods of estimation. We know, for example, that both least-squares estimation and two-stage least-squares estimates are sensitive to the choice of normalization rule. There are several dimensions of the topic to be explored. First, we have to consider Frisch's rejection of asymmetric models that are contrary to the Walrasian ideal. This contrasts with Tinbergen's recursive models and points to the conflict between equilibrium and disequilibrium bases for structural analysis. Second, there are the problems of interpreting equation errors. Most economic models are deterministic so errors have to be introduced at inappropriate stages and have uncertain circumstances. Haavelmo's advocacy for the probabilistic framework leaves the nature of the equation errors unsettled. This means that the choice of regression line (a problem known since the pioneering days of K. Pearson and Yule) is difficult to resolve. Wold has tried to demonstrate, with cause-effect arguments, the irreversibility of equations which would make normalization a part of the structural specification from economic theory but he failed to be fully convincing.

The general objectives in raising these seven topics and citing past debates are to clarify the use of structural estimation and to emphasize the intrusion of soft elements within econometric practice. When we have finished our account, we can deal with the fundamental question that is implied in our title.

The Partition of Variables

Altough econometricians often comment on the potential statistical properties of particular estimators, we should not exaggerate the extent of general interest in these properties among economists. Any review of our professional literature will reveal that many economists make elementary mistakes when they describe the statistical interpretation of models that have been fitted to data. A common feature of "applied" econometrics involves the use of familiar methods even when they are unsuitable in the context being explored. Similar myopia can be found in appraisals of empirical results that stem from the research activities of other economists. There the focus is usually placed on the interpretation of particular numerical values rather than on the potential statistical characteristics of the procedures that generated these values. At least two explanations of this situation can be put forward. It may be due to incomplete advocacy by econometricians or it could stem from views held among economists that the econometric perspective is itself excessive since this fails to recognize the difficulties of extending probabilistic notions to economic phenomena. Whatever explanantion is accepted, the situation is an uncomfortable one.

Any demonstration of the statistical properties of estimators requires the specification of a probabilistic framework. In econometric textbooks, this requirement is expressed in the accounts of "ideal" models, such as the classical linear model or the SEM. The properties of estimators must depend on which ideal model is closest to the economic context being explored. Softness arises to the extent that the use of an ideal model distorts this context. We shall argue that the only clear definition of an exogenous variable is one stressing its stochastic character (as part of a given probabilistic framework), which involves either lack of correlation or independence with respect to equation errors. We shall also assert that, as our ability to find adequate prior justification for invoking such properties or to test for their occurrence is inevitably limited, there must occur significant room for softness to be present for both estimation and interpretation of their empirical results by economists. To establish the validity of these opinions, we should begin by pointing out the connection between exogeneity and estimation and by remembering the deterministic source of almost all of the equations that are considered by economists. First, the statistical consequences of any method of estimation will always depend on the probabilistic framework. With exogeneity defined in the statistical sense, this can be restated. The statistical consequences of any method of estimation will always depend on the presence or absence of exogeneity of variables involved in applying the method. Second, the primary contribution of economic theory takes the form of equations that are derived from mathematical manipulations of deterministic models. Such manipulations frequently impose hypothetical constraints so that, for the purpose at hand, certain variables are treated as "given" or "fixed". These constraints are quite different from those of exogeneity in the statistical sense. Indeed the deterministic models provide no guidance for partitioning variables in their stochastic counterparts if this partition is intended to motivate the choice of estimation technique and to permit the use of statistical inference.

This confusion of mathematical fixity and stochastic exogeneity is found much too often in economic literature (and even in econometric textbooks as we shall reveal below). To facilitate discussion of this issue, we have assembled some comments on partitions of variables in Table One. The choice of contributors to this list is easy to justify. Most of them have enjoyed distinguished careers in econometrics. Some quotations are extracted from significant papers that have affected the evolution of econometrics, while others are representative of a host of similar views. We have carefully avoided citing persons on the fringe of the subject so that it is fair comment to claim that the apparent heterogeneity of views can be taken as an accurate reflection of the mainstream's confusion. Clark (1947, p. 77) made a plea for improved communicability between mathematical economists and others. "In manipulation, abstract symbols can be made to do things foreign to the nature of the economic realities they represent; hence symbols do not automatically eliminate loose thinking." The relevance of this comment for econometrics will be established as we look at the contents of the table. We like to interpret the nature of economic realities as involving, in part, the probabilistic characteristics of economic variables and their interactions.

Turning to the table, we see it is arranged in six sections that extend from the beginning of the modern era to the present. The first two sections cover the period during which Koopmans challenged non-statistical definitions of exogeneity and, in our view at least, established the primacy of one definition for econometricians. These are followed by a collection of extracts from econometric textbooks that were published over a decade after Koopman's challenge. Next, in Sections D and E, we illustrate reappraisal of research methods due to computational

TABLE ONE

THE PARTITION OF VARIABLES: EXOGENEITY AND ENDOGENEITY

(A) Early Days

Exogenous variables (often identified with non-economic variables, like temperature, rainfall) influence the (economic) endogenous variables, but not vice versa. Accordingly the equation system is constructed to 'explain' only the endogenous variables.

Koopmans (1945, p. 463)

For the economy as a whole, endogenous variables can be roughly identified with what is often called 'economic variables'. These are usually the quantities (stock or flows) and prices of goods and services, or their aggregates and averages, such as national income, total investment, price level, and so on. The exogenous variables and the structural parameters are roughly, 'noneconomic variables' (also called 'data' in the economic literature) and may include weather and technological, psychological, and sociological conditions as well as legal rules and political decisions. But the boundary is movable. Should political science ever succeed in explaining political situations (and hence legislation itself) by economic causes, institutional variables like tax rates would have to be counted as endogenous.

Whenever we use weekly or even quarterly instead of annual time-series, we must be wary of predictions that use lagged endogenous variables as though they were exogenous.

Marschak (1953, pp. 10, 23)

The first task in the development of a system of relationships involving the variables that appear in the social accounting scheme is to decide upon the limits of our theory. We must decide, in a rough sense, upon those variables that are to to be explained by the theory or the model, to use another expression....[The] model tries to explain the endogenous variables in terms of the exogenous variables. In the endogenous category we shall place all those variables that belong wholly to economics and in the exogenous category those variables that are wholly explained by other disciplines. Naturally, we want to go as far as possible in placing variables in the endogenous category.

Klein (1953, p. 70)

Which variables are endogenous and which are exogenous will depend upon the theoretical assumptions underlying the economic model. In a short-run model we will, for instance, treat fixed capital as an exogenous variable. But fixed capital will be an endogenous variable in a long-run economic model.

Tintner (1952, p. 156)

In principle, [the specification of the variables chosen as exogenous] should be based, in my opinion, on a priori rather than on statistical considerations. Generally speaking the exogenous variables are either non-economic or outside the market system studied. It is true that it is only by hypothesis that these variables do influence the endogenous variables without being themselves influenced by them; and it should be admitted that the testing of this hypothesis is useful and necessary. It is also true that in recent econometric and economic analysis certain variables are somewhat too easily assumed to be exogenous.

Tinbergen (1952, p. 205)

The main problem of building models is rather that of making them complete from the point of view of both economic theory and statistics. If this criterion is followed, the group of exogenous variables cannot be arbitrarily set, neither can the subgroup of controllable exogenous variables be so set. In each particular case they are determined by the structure of the problem under study. And this is why the same variable must be endogenous in one case, and exogenous in another.

Georgescu-Roegen (1952, p. 208)

(B) Exogeneity in the Statistical Sense

[The] assertion that certain variables entering into a certain complete (sub-) model are exogenous can be substantiated only by information about the form of equations outside that (sub-) model-'form' meaning here both the set of variables entering in and the policies impinging on these additional equations.

Assurance that a given variable is exogenous can only be obtained by qualitative knowledge of the variables causally involved in its generation. If the model can be extended by additional equations describing the generation of the presumably exogenous variables, the needed information is of the same type as that required for identifiability: lists of variables occurring in the additional equations that make the model self-contained. Where the variables in question are often non-economic in character, the required knowledge may not at present be attainable by explicit extension of the model to cover a wider range of phenomena.... The cost of misjudgement is obvious.

Koopmans (1952, p. 203)

Despite the fact that policy implications of the obtained econometric models depend critically on which variables are considered exogenous and which endogenous, econometricians have not introduced evidence supporting their choices, although it could hardly be maintained that the variables chosen as exogenous are obviously not affected by movements of those variables chosen as endogenous.

Orcutt (1952, p. 198)

In determining which variables are set aside as exogenous, two main principles are implicitly or explicitly applied in economic literature.... The departmental principle treats as exogenous those variables which are wholly or partly outside the scope of economics, like weather and climate, earthquakes, population, technological change, political events. The causal principle... regards as exogenous those variables which influence the remaining (endogenous) variables but are not influenced thereby.

[For] purpose of statistical estimation the concept of exogenous variables must be defined more strictly and narrowly than for some purposes of economic theory.

Both the distinction between exogenous and endogenous variables and that between predetermined and dependent variables are based on a subdivision of the complete set of equations 'explaining' the formation of all variables into subsets of equations. In both cases it is necessary to stipulate... that the disturbances affecting questions thereby placed into different subsets should be independently distributed.

Koopmans (1950; pp. 393-394, 399, 405)

(C) From Consolidation to Present

The model tries to explain how certain of the quantities considered are determined. The quantities, and the variables which represent them, can be arranged in two distinct groups according to whether they are or are not explained by the model. The former called endogenous, are considered to be determined by the phenomena expressed in the model. The latter, called exogenous, occur in the equations that are considered to be determined independently.

In a general way we can state that a model represents the determination of endogenous variables on the basis of exogenous variables...The importance of the assumption that the exogenous variables are determined independently of the phenomenon represented must be emphasized... In practical applications we must always ask ourselves if the assumption is admissible. It will often be allowed only as a first approximation.

Malinvaud (1966, p. 56-57)

Endogenous variables are economic variables whose interaction determines the economic system; for example, quantities sold and bought, interest rates, and so on. The number of endogenous variables must normally be equal to the number of equations in the system in question. In addition, there are the predetermined variables - variables which influence the system but are not influenced by it (exogenous variables like the weather and lagged values of the endogenous variables like past prices).

Tintner (1968, p. 76)

Endogenous variables are those whose values are determined by the structure, given the values of the exogenous variables. The exogenous variables are predetermined, and their values are given for the study.

Exogenous variables are known and affect determination of the endogenous variables, but they themselves are assumed not to be affected by the endogenous variables. That is, there is a flow of influence from the exogenous to the endogenous variables but not vice versa.

A variable is exogenous and predetermined if it is independent of the disturbance term in a stochastic equation. In practice the choice is often arbitrary.

[We] can state that a variable is predetermined with respect to the k-th equation if only if it is uncorrelated in the probability limit with the disturbance of the k-th equation.

Dutta (1975, p. 11, 279)

[Exogenous variables] are the variables whose values are determined by forces external to our model. Somewhat more formally, the values of the exogenous variables are assumed to depend on variables that are not related in any way to the endogenous variables, or to the disturbance terms, of our model. In a sense these variables are 'beyond' the scope of our analysis. We simply take their values as given without attempting to explain them.

Kelejian and Oates (1974, p. 236)

The objective of an equation system is to describe some of its variables, the endogenous variables, in terms of the other variables, the exogenous variables. The latter variables are determined 'from outside', i.e. independently of the process described by the equation system. The former (endogenous) variables are simultaneously determined by the exogenous variables and the disturbances in the way prescribed by the equations of the system.

For estimation purposes it is necessary to specify what exogenous ('determined from the outside') means in statistical terms. This specification amounts to the assumption that the values taken by the exogenous variables are stochastically independent of the disturbances of the system.

Theil (1978, pp. 320-321)

Typically, the exogenous variables are left as just that because they are too hard or impossible to forecast or because they are too much influenced by factors outside the purview of economists; that is, they cannot be predicted by the variables used by economists.

Thurow (1983, p. 111)

(D) Simulative Perspective

An exogenous variable is a datum that is predetermined in the sense that its value must be specified before the model is solved and its value is not altered by the solution process.

An interesting additional subdivision can be made by dividing exogenous variables into policy and nonpolicy variables. Exogenous policy variables generally include the fiscal monetary parameters that are assumed to be under the control of government agencies and authorities. ... Nonpolicy exogenous variables include many items that can be treated mechanically, such as seasonal indicators... and time trends, but they also encompass data which must be treated with the same care and sophistication as policy variables when forecasting.

Klein and Young (1980, p. 15)

[One] is confronted with the problem of choosing a point at which variables which clearly are going to be affected by variables that are endogenous to the system are exogenized because an adequate explanation of their behavior would go beyond the bounds of resource availability. A glaring example present in most U.S. macroeconometric models is the assumption that the world economy is exogenous to behavior of the U.S. economy.

[The] forecaster must himself develop feedback from the forecast to what might be termed semiexogenous variables. If the U.S. economy is expected to exhibit slow growth, for example, it is unlikely that world trade will achieve high levels of activity, and assumptions must reflect this.

Klein and Young (1980, pp. 57-58)

(E) Tests for Exogeneity

The interest of econometricians has been too much preoccupied with estimating interrelations in the economic system to the almost complete neglect of testing hypotheses about which variables are wholly or partially exogenous to the economic system.

Orcutt (1952, p. 197)

[It] would be very important to have a test of exogeneity in the stochastic sense, a test that has some power of discrimination. Unfortunately, if I may venture a conjecture, it does not seem to me that the chances are good for such a test to be really informative, if applied to actual data. The difficulty lies in the necessity, in all statistical testing, to specify a set of maintained (unquestioned) hypotheses.

If no promising tests of exogeneity are found, the task remains of assessing the limits of error inherent in policy conclusions drawn in a state of uncertainty with regard to the exogenous character of certain variables. ...If doubt remains about a basic specification not subject to conclusive test, the only remaining line of defense is a study of the effect on policy conclusions of presumably possible degrees of departure from the specification in question.

Koopmans (1952, pp. 204-205)

(F) Unsettled Times

The classification of variables into exogenous and endogenous was...done on the basis of prior considerations. In general, variables were classed as endogenous which were, as a matter of institutional fact, determined largely by the actions of private agents (like consumption or private investment expenditures). Exogenous variables were those under government control (like tax rates or the supply of money). This division was intended to reflect the ordinary meanings of the words endogenous-'determined by the [economic] system' - and exogenous - 'affecting the [economic] system but not affected by it'.

Lucas and Sargent (1979, p. 300)

The a priori setting of variables into exogenous and endogenous variables is unjustified on statistical grounds. The hypothesis of statistical exogeneity can be tested.

Miller (1978, p. 583)

Rejecting the exogeneity hypothesis of certain variables does not bring any useful information if the result has been obtained inside a specification that omits a large proportion of variables with an economic importance.

Rejecting the exogeneity of a variable shows that the estimation should take feedbacks affecting this variable into account; but this obviously does not mean that this variable is not a good instrument of economic policy.

Malinvaud (1981, p. 1373)

advances and with the perceived feasibility of testing for exogeneity. Finally we illustrate the unsettled times that followed the integration of Sims' test for exogeneity in an attack by proponents of the "rational-expectations" perspective on what they called Keynesian macroeconometric models. This attack is not directed against Koopmans' treatment of exogeneity but rather at the presumption that its application must be based on an a priori partition of variables instead of on a statistical test. Before considering the contributions to each section, it is appropriate to recall the recent technical issues that were identified earlier. These are summarized again in Table Two.

Structural instability can affect both a priori and test approaches to exogeneity in the statistical sense. There is little in either mathematical or statistical models to preclude changes in regimes and corresponding changes in the exogeneity of individual variables. Indeed the introduction of policy rules to govern levels of controlled exogenous variables by reference to the experiences of endogenous variables could change the statistical status of the controlled variables and make them endogenous. Since policy rules may be amended during a period under review, this implies that a variable can be both exogenous and endogenous within a given sample! For the technical issue of non-structural purposes, the relevance of exogeneity is unclear. Exogeneity in the statistical sense is associated with unbiased or asymptotically unbiased and consistent estimates. Such properties may be significant in structural analysis but their importance is not readily extended to other objectives such as prediction. Looking at the quotations in Table One, we shall have to stress the distinctions between control (which may be valuable in prediction) and such notions as correlation, statistical independence, influence, explanation and determination (which may not be valuable unless their meaning is either stretched or clarified).

Sequential analysis is involved in at least two parts of the historical debates on exogeneity that the quotations illustrate. As we have already noted, it arises in the determination of pre-test complications of fitting statistical equations after the use of Sims' test for exogeneity. It is also present when there is uncertainty concerning the size of models (for example, the number of equations involved in them) and several alternatives are explored. We shall also find the impact of simulative experiments and questions of robustness as individual quotations are appraised.

Section A of Table One begins with a comment by Koopmans (1945) that markedly differs from the important position that he was to take five years later. It fits with the observations of Marschak and Klein that exogeneity is to refer either to non-economic variables or to variables that the economic researcher did not wish to explain. Clearly this view is concerned with the scope of models rather than with the statistical characteristics of the estimated parameters of these models. Neither

TABLE TWO

RECONCILIATION OF PAST AND PRESENT

Recent Technical Issues		Historical Topics	
(A1)	Structural Instability	(B1)	Partition of Variables
(A2)	Non-Structural Purposes	(B2)	Primacy of Probability Approach
(A3)	Statistical Constraints	(B3)	Interaction Among Variables
(A4)	Sequential Analysis	(B4)	False Constraints
(A5)	Simulative Experiments	(B5)	Proximity
(A6)	Robustness	(B6)	Purpose
		(B7)	Normalization and Asymmetry

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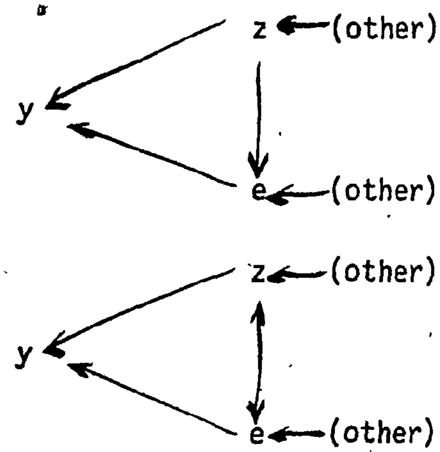
influence nor explanation implies correlation or statistical dependence. In the absence of explicit specifications linking exogenous variables (in this non-statistical sense) and equation errors, it is really not determined how exogenous variables will influence endogenous ones. For example, an individual exogenous variable might affect endogenous variables both directly, and through its interaction with the equation errors, which in turn have a direct impact on endogenous variables. The arrows in Figure One trace such influences. (Such arrow scheme were introduced to economists by Tinbergen and Wright in the 1930s). Let y, z and e denote endogenous variables, exogenous variables and errors. Then the two diagrams in the figure are consistent with the non-statistical approach to exogeneity. If b is an unknown parameter, we can interpret

FIGURE ONE

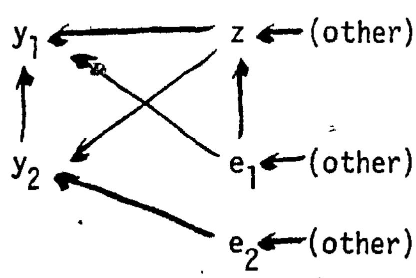
IMPRECISE INFLUENCE



$$y = bz + e$$

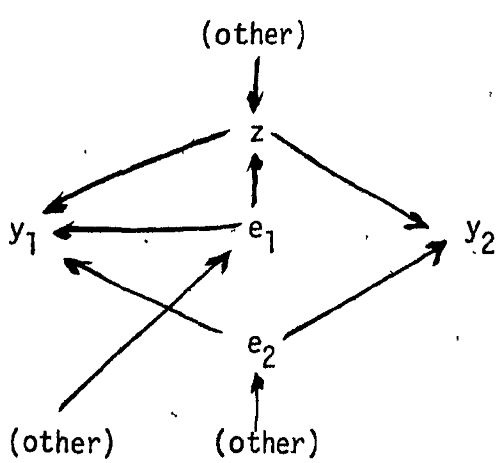
FIGURE TWO

NON-UNIQUENESS OF REPRESENTATIONS



$$y_1 = cy_2 + b_1z + e_1$$

$$y_2 = b_2z + e_2$$



$$y_1 = (cb_2 + b_1)z + e_3$$

$$y_2 = b_2z + e_2$$

$$e_3 = e_1 + ce_2$$

the diagrams as having y linearly influenced by both z and e and as having e affected by z too. Such representations are non-unique so the language of "influence" involves some imprecision. The two diagrams of Figure Two capture the linkages of a slightly more complicated model and reveal the hazards of identifying arrows with directions of influence. Both representations can be used to explain the endogenous variables. If the interaction among the three errors is omitted, we might not recognize the equivalence of the representations.

The mobility of the boundary between exogenous and endogenous variables is found in the elaboration of the arrows of influence affecting z. The only restriction of exogeneity is that there would be no arrow indicating a connection from y to z (either directly or through a loop involving intermediaries). Both Klein and Marschak seem to prefer large models with many endogenous variables but this preference has little to do with the definition of exogeneity, except in the statistical sense. Must all endogenous variables be explicable? How many equations should be used to explain these variables? Such questions of size arise with non-statistical definitions. As with the preferences of Klein and Marschak, the response to these questions is left to individual choice. We shall return to the issue of size when we discuss the primacy of the probability approach to econometrics.

Tintner and Marschak have also provided comments on exogeneity that involve time. It is clear that Tintner has an economic framework in mind and his distinction between long-run and short-run is essentially based on the "fixity" of capital rather than on its statistical characteristics. Indeed his comment might have taken directly out of an elemantary economics textbook with no consideration of estimation. Marschak's worry about predictions based on data from different sampling intervals has a statistical basis. The choice of intervals will affect the specification of econometric models both with respect to explicit stock-flow considerations and to the potential correlation of equation errors. Some econometricians have argued that the smaller the sampling interval, the more likely will be autocorrelated errors. Thus, it is suggested, lagged endogenous variables might not be predetermined in a statistical sense when small time intervals are used. There is a further issue concerning the generation of forecasts when lagged endogenous variables are present. We shall deal with this when we explore the views of Klein and Young as recorded in Section D.

The final contributors to Section A are Tinbergen and Georgescu-Roegen. Although their views were initially printed as responses to a paper by Orcutt which is part of the focus of Section B, they fit with the views that we have already cited. Tinbergen was never able to resolve his position on exogeneity. He seems to embody both non-statistical and statistical elements in his treatment of exogeneity. Georgescu-Roegen is more interested in size. His view reflects the early attitude of the Cowles-Commission researchers that estimation must be based on a complete system. This attitude was already being undermined by the efforts of Wald (1950) and Anderson and Rubin (1949, 1950), which saw the derivation of the limited-information maximum likelihood estimator. The assertion that a variable can be both endogenous and exogenous depending on context is sensible and quite consistent with different definitions of exogeneity.

Klein released his first textbook on econometrics in 1953. Arrow (1954) reviewed this for the Journal of the American Statistical Association. "Perhaps the greatest weakness is the cursory handling of the distinction between exogenous and endogenous variables. The special role of exogenous variables is absolutely essential in simultaneous equations estimation; but Klein gives no explicit definition in terms of the probability distributions and only a vague verbal definition." He suggested that the "only really satisfactory treatment" is provided by Koopmans (1950). This perhaps illustrates the significant impact of Koopmans' clarification of exogeneity. Despite later obfuscation due to the use of non-technical language, Koopmans' definitions have persisted as the fundamental basis for statistical treatments of exogeneity. Arrow's review is itself interesting. We have been unable to find any later reviews of econometric textbooks that stress the central role of exogeneity and point out the deficiencies of particular definitions. Yet, as the entries in Section C reveal, the treatment of exogeneity is often imprecise, incorrect and bewildering.

Koopmans (1950) posed a fundamental question: if we wish to study the statistical implications of the fact that economic data are governed by a system of simultaneous equations, how should we define exogeneity and endogeneity? His response is straightforward. We should adopt definitions that permit us to assess the consistency and asymptotical unbiasedness of the estimated parameters for the economic relations. This principle leads to the definition of exogeneity in statistical sense, which is stricter and narrower than the approaches found in economic theory (and illustrated above). In the SEM, use of this principle is simple. There are a number of equations containing errors and measured variables. The latter are partitioned into endogenous and predetermined variables according to their statistical independence (dependence) of the concomitant errors. Predetermined variables are then further partitioned according to statistical independence of all errors. There is nothing here of explanation, influence or determination. Such notions involve additions to this simple statement.

To elaborate this partition, Koopmans (1950, 1952) and Simon (1953) developed a block-recursive format in which the initial set of economic equations is augmented to form a larger system. Then a priori zero constraints on certain parameters of this large system (for both its signal portion and the contemporaneous dispersion matrix of equation errors) will suffice as indicators of predetermination in the initial system.

Since zero constraints are equivalent to omissions of variables, Koopmans cites the need for "qualitative" information but this is misleading. It neglects the probabilistic constraints on the two blocks of equation errors in the augmented system. Thus exogeneity is not simply a matter of lists of variables as suggested by Koopmans (1952, p. 203) for he ignores his own statement of a necessary condition in Koopmans (1950, p. 405) that involves the statistical independence of the initial equation errors and those of the augmentation.

This structure is technically elegant but it has rarely been used in practical situations outside some tentative attempts by the builders of large economy-wide econometric models to sub-divide their r~~esearc~~h efforts into feasible components. The definition of exogeneity in the statistical sense is quite consistent with individual equations and need not be involved with the full complexity of the SEM. For a given equation, knowledge of the statistical interactions of all its measured variables with the equation error is the essential ingredient in the use of this concept of exogeneity and, hence, for the determination of statistical properties of estimated parameters. To the extent that such knowledge is uncertain, we have inherent softness in this determination of statistical properties. The development of tests of exogeneity should be seen as attempts to eliminate this softness. Advocacy of such tests usually involves criticism of the imposition of prior zero constraints on the augmented system used by Koopmans and Simon.

Orcutt (1952) provides a major contribution to the early debate on exogeneity. His complaint that econometricians have not introduced evidence supporting the placing of particular variables into categories is as important now as it was when he wrote. Even if we accept the statistical approach to exogeneity, we should not simply "assume away" difficulties either explicitly by imposing convenient exogeneity constraints (without attempting a justification for their imposition) or implicitly by ignoring them. It would be interesting to take the content of twenty or so leading journals for 1973 and to explore the incidence of adequate discussions of exogeneity. Of course, we can already predict the outcome of such an exploration. It is likely to be deeply embarrassing for "applied" econometrics. The departmental approach to exogeneity in practice may have been replaced by a myopic alternative based on convenience and leading to misleading statistical inference. Koopmans (1952) suggests that the cost of misjudgement in the partition of variables is "obvious" but it is not clear that this cost is recognized even now. The acceptability of the statistical approach to exogeneity loses much of its practical appeal if we do not use it to appraise applied research and to guide the rejection of particular values that have been produced as estimates without complete specification of economic models.

In Section C of Table One, we have collected comments on the partition of variables from major textbooks that were published after the end of the historical period that is serving as our principal framework for discussion. The contents of this section reveal the assimilation of Koopmans' efforts and the persistence of the departmental approach that he condemned. They also reveal the complacency of this era of consolidation in the history of econometrics. There is distressing absence of adequate discussion of the concept of exogeneity itself and of the demands for further clarification by Orcutt. Malinvaud's textbook is excellent in many respect. We have reproduced the only treatments of exogeneity and endogeneity that are cited in the indexes of its two editions. Although he invariably uses exogeneity in the statistical sense when he discusses estimation, this is not given expression in the two quotations that we have reproduced. There he has resorted to determination and explanation rather than statistical independence and lack of correlation.

Tintner's definition is very similar to those that he provided a decade and a half earlier. Again it is devoid of probabilistic notions. The contributions of Dutta, Kelejian and Oates, and Theil represent the textbooks of a later generation. Dutta manages to combine different approaches to the partition of variables without attempting to reconcile them. Lack of correlation in the probability limit is obviously not the same as statistical independence so, even when he has a single concept of exogeneity in mind, the ambivalence of his account emerges. The notion that values of exogenous variables are fixed is either a reversal to the characteristic of mathematical modelling or an implicit reference to a device used for convenience at the Cowles Commission to derive maximum-likelihood estimates. It is unnecessary in fact and eliminates the ease of movement from a superior treatment of exogeneity to the augmented model of Koopmans and Simon and to the pleas for tests of exogeneity by Orcutt and, later, by Sims (1972a, b). The contribution of Kelejian and Oates is equally defective. Its treatment of some variables as beyond the scope of analysis and unexplained is unacceptable. The role of additional variables that themselves affect exogenous variables is a strange innovation. Finally, among textbooks we see that Theil gives a correct statement of exogeneity in the statistical sense after a preamble that touches on the departmental approach but quickly drops this. We have also included a comment by Thurow even though he is not providing a textbook. His statement shows the bewilderment that stems from the treatments of exogeneity. Taken at face value, his statement provides yet another definition of the concept, one that seems to involve difficulties in predictability and dependence on non-economic factors. We have been unable to link this treatment with definitions in the mainstream of econometric history.

The overall impression to be drawn from Section C is that our textbooks often provide deficient accounts of exogeneity which may be a factor in the disappointing features of applied research. They usually treat econometrics in a static non-experimental framework and fail to deal with the challenges to this framework that come from computational advances. In Section D, we illustrate a new perspective that stems from the evolution of a series of large econometric models and the use of simulative experiments. With computational developments, the feasibility of simulation was enhanced and its use has become a standard tool in modelling. Klein and Young reveal a marked adjustment in their treatment of exogeneity. This represents for Klein the culmination of a steady movement from the position of his youth as associate of the Cowles Commission to the pragmatic position of his recent past with its heavy involvement in the management of large commercial models. This movement is associated with a shift in stance from the theoretical focus of the Cowles Commission on estimation to the practical demands of economic application.

The background of simulative experiments is different from those of structural confirmation and structural search. Each experiment, except in system-dynamic models, begins with an estimated structure. This is usually amended until a "solution" of the approximating structure, with endogenous variables assumed to respond to given time paths for exogenous variables, converges in an acceptable fashion. There is, therefore, a discarding of structural content and the introduction of non-statistical criteria for acceptability. The "tracking" of the paths of endogenous variables by the predictions of the approximating structure is a common indicator of satisfactory convergence of a solution. Clearly this is a shift away from the static properties of estimating procedures and toward dynamic replication. It is not surprising, then, if we might want to use a different concept of exogeneity. Simulation usually involves taking the solution as a starting point and either amending the paths of exogenous variable (away from historical experience) or the structural equations as part of hypothetical experiments. When the historical paths of exogenous variables are replaced, it is assumed that the structural equations would not have been affected by this replacement had it actually occurred. This would seem to constrain the extent of changes of values that might be considered. When individual structural equations are replaced, it is assumed that other equations will remain pertinent. We are faced then by a combination of counterfactual adjustments and assumed stability. This is also a presumption in many experiments that certain variables are controllable so that the experiments can reveal the implications of controlled changes in their values. Klein and Young term such variables "policy variables" and consider them exogenous. This is a difficulty as the values imposed on them might be consistent with changing an exogenous variable to an endogenous one, as for example in feedback policy rules.

Simulations need not be based on the whole of a fitted structure. Whether constrained by the level of research funding or by commercial viability, resource availability is a significant factor in determining the size of approximating structures. Klein and Young give one "glaring example" but there are many other instances of reduced structures. Convergence of the initial solution and of the hypothetical experiments is affected by the size of models as well as their form. Indeed cost may be as significant a factor as structural accuracy. Frequently there are attempts when choosing revised values of exogenous variables and of constant terms to compensate for omitted structural components. We can see the Klein-Young approach as taking endogenous variables and "exogenizing" them but then introducing concomitant adjustments to reduce the impact of this treatment. The overall impression is of a markedly different attitude to structure, partitions of variables, and to the purposes of modelling. There is no standard method for simulation if we mean by that objective rules for the steps that are taken when experiments are undertaken. Contrast this with the objectivity ideal of Klein (1947) that we cited earlier. We are simply in a soft context without hard and fast rules of behaviour, other than the need for some record of the adjustments made and decisions taken.

To leave simulative experiments for tests of exogeneity is to leave an area where subjectivity is open and softness is apparent for a second area where there is perceived objectivity and non-softness. Section E of Table One contains contributions by Orcutt, Koopmans and Sims. As we have noted, Orcutt was very concerned about the treatment of exogeneity by economists. His plea for evidence, perhaps in the form of test results, went unanswered until Sims proclaimed the appropriateness of his particular testing procedure. Unfortunately, the Sims test suffers from a number of deficiencies. For example, it requires all measured variables to be covariance stationary and errors to be normally distributed. Its use beyond simple two-equation systems is not readily discernible. The unrecognized complications of this use is foreshadowed in the comments of Koopmans (1952), who notes the problem of establishing maintained hypotheses. When Sims moves from a mathematical theorem concerning the characterization of a theoretical regression line to a "practical" test, he must introduce a host of supplemental assumptions to serve as maintained hypotheses. His failure to question the acceptability of these assumptions or the sensitivity of his findings to their potential inaccuracy means that he has simply pushed imprecision and uncertainty into a less visible area. They have not been eliminated, merely displaced and forgotten. This is discussed in Rowley and Jain (1984). Koopmans, while doubting the feasibility of finding an informative test of exogeneity, makes a valuable suggestion concerning how we might proceed when no conclusive test is available. He points to what would now be called sensitivity analysis or robustness. We have been unable to locate explorations of the impact on the Sims test when the intertemporal partitioning into exogeneity and endogeneity changes within a given sample. Sims' own accounts seem to take the structural format as free from structural instability of this type or of any other kind. We are thus left with a situation in which the a priori constraints for exogeneity in the Koopmans-Simon augmented system are justifiably criticized while the testing alternative is neither fully developed nor comprehensive.

We are in unsettled times. Lucas and Sargent (1979) have been major advocates for the Sims' test. There are many economists, such as Miller, who assume that the well-established treatment of exogeneity by Koopmans has been removed from its central position in econometrics. The net outcome of current disturbance must await the provision of a full response to advocates of the alternative testing procedure. We suspect that elements of both approaches will persist in soft versions. Tests and prior constraints are both capable of generating statistical inferences conditional on their assumptions. Combine this situation with the ease of conducting robustness or sensitivity analysis and we have a basis for compromise. Whatever happens, we have softness in our methodology due to the need to partition variables for estimation, simulation and other activities. Even if there were not softness in this respect, it would still arise for purposes of guiding economic policy decisions. As Malinvaud (1981) reminds us, the linkage between exogeneity and policy instruments is uncertain.

The Primacy of the Probability Approach

When Klein suggested 1943-1944 as the time of birth of modern econometrics, he cited the efforts of Haavelmo (1943a, b; 1944) and Mann and Wald (1943 - 1944). These established the probability approach to econometrics, the acceptability of asymptotic criteria for determining the optimality of estimators, and the feasibility of an order calculus for statistical variables that was analogous to the familiar limit calculus of mathematical variables. Obviously individual papers did not produce a revolution. They had to be taken up and their significance proclaimed by proponents of a "new" approach. The Cowles Commission served as the focus for these proponents' activities. Indeed the publication of the two conference reports edited by Koopmans (1950) and by Hood and Koopmans (1953) were major events in the history of econometrics.

Klein (1982, pp. 111-112) has described the activities of the Commission in the 1940s from the perspective of someone who was a participant. "This academic research group... concentrated on application of the methods of modern statistical inference to the estimation and testing of models. Considerable emphasis was devoted as well to problems of identification and specification. At this time, the rooting of model specification in received economic doctrine was firmly established." While leaving the final sentence for later comment, we must acknowledge the role of Cowles Commission in determining the primacy of the probability approach during the 1940s and 1950s. This approach, as we indicated earlier, has dominated econometric thought and practice for four decades. However, the nature of this dominance has not been constant. We feel that the probabilistic focus peaked at about the end of the period under review as the practical demands of the management of large economy-wide models begin to exert other pressures. This peak can again be illustrated by quoting Klein (1964, p. 55). "The method of full information-maximum-likelihood (FIML) occupies a role in econometric analysis much like that of competitive equilibrium in economic analysis. It is an idealized goal towards which we strive and reach only approximately or not at all." The FIML method was the ultimate expression of the probability approach to econometrics. This ideal does not provide the same motivation today. Although the SEM framework for the Cowles Commission continues as the primary format for statistical analysis, its probabilistic origins are generally less visible. We would, for example, expect to see systems estimated much more often by the three-stage least squares (3SLS) method than by the FIML method (which involves a more comprehensive specification of the probability density function of the equation errors).

Our purpose in considering the probability approach is served if we can use it to explore three important aspects of the approach. The first of these involves the identification of structural equations, their indistinguishability or the problems of confluence. The second one deals with the concept of completeness and its role in determining the size of fitted models whereas the final aspect is centred on the key concepts of autonomy and relative instability of alternative representations of systems of equations. This consideration is best begun by looking at some parts of Haavelmo's justly famous supplement to Econometrica in 1944. The connections with the current technical issues that we described earlier will emerge as our consideration proceeds.

The basic proposition of the probability approach is that estimation problems are essentially the need to study the joint probability distribution of random observable variables bound together in a stochastic equation system. Haavelmo always starts with the joint probability distribution of equation errors and the transformation of this to the distribution of a group of endogenous measured variables. To the extent that the Jacobian of this transformation is not a fixed constant, maximum likelihood estimates of systems with normal errors will differ from the least-squares estimates that had been used by Tinbergen and other early econometricians. There are also differences in regression lines (conditional expectations) that stem from the joint occurrence of a system of equations as contrasted with a single isolated equation. Discussion of such regression lines can be delayed until we consider Wold's advocacy of recursive systems and eo ipso predictors in connection with a later topic. Wold has also provided a useful qualification to counter the more extreme expressions of the probabilistic approach. He indicated situations in which the jacobian of transformation was a fixed constant so that least squares estimates and maximum likelihood estimates could be equivalent.

Some statements of the basic proposition of the probability approach are reproduced in Table Three. Those attributed to Haavelmo and Koopmans already contain an insistent tone as if only one approach was correct in econometric analyses. The most extreme version of this single-mindedness is provided by Girshick and Haavelmo (1947), who seem to set an impossible task for specification but who in fact failed to meet their own standard by using excessively simple structures for their own illustrative examples. The comment by Klein and Young is added as a counterbalance to show that advocacy of a single method for estimation fails when we move away from the purpose of a descriptive structure to a manipulated one as in forecasting. Haavelmo, Koopmans and Girshick appear to suggest that the choice of estimation method is independent of the purpose of estimation, independent of criteria other than statistical ones (such as might be linked to sampling schemes), and has to be based on awareness of some characteristics of all equations in a system describing the "formation" of economic variables. Klein and Young indicate that such views are unworkable in the forecasting models with which they have been involved so that, in the context of these models, the probabilistic basis is discarded. This conclusion is disturbing since, as we shall show, the whole edifice of the SEM as developed at the Cowles Commission from the probabilistic basis of Haavelmo was justified in terms of a specific type of hypothetical predictions of structured responses to policy changes.

TABLE THREE

THE PROBABILITY APPROACH TO ECONOMETRICS

(A) Basic Proposition

[If] one assumes that the economic variables considered satisfy, simultaneously, several stochastic relations, it is usually not a satisfactory method to try to determine each of the variables separately from the data, without regard to the restrictions which the other equations might impose upon the same variables.

Haavelmo (1943a, p. 2)

It is...clear that the joint probability law of all the observable random variables in an economic system is the only general basis for estimating the unknown parameters of the system.

Haavelmo (1944, p.88)

The meaning of Haavelmo's work is that economic statistics has now caught up with economic theory. It has now become clear that methods of statistical estimation, even of a single equation of economic behavior, must in some way take account of the fact that the variables entering that equation are part of the wider set of relevant economic variables which are determined by a complete system of simultaneous equations.

Koopmans (1945, p. 462)

Any statistical method of estimation derives its meaning and area of applicability from the concept of a well-defined sampling model. ....The conditions under which a sample is obtained have always received close attention from statisticians in interpreting the information gained from the sample. Application of that principle to economic statistics naturally leads to the requirement that statistical methods of fitting take into account the formation of economic variables through a complete system of equations.

Koopmans (1945, p. 448,462)

[It] is impossible to derive statistically the demand functions from market data without specification of the supply functions involved. More generally, if we wish to estimate any particular economic relationship on the basis of market data we are forced to consider, simultaneously, the whole system of economic relations that together represent the mechanism that produces the data we observe in the market.

Girshick and Haavelmo (1947, p.83)

Given the properties of the OLS estimates, that is, that they can be expected to be inconsistent and biased, the typical methods used for validating models are examination of simulation properties, multiplier analysis, and forecasting results.

The major point to be recognized in validating forecasting models is that there are no generally recognized methods of statistical inference available. ...The validation methods are quantitative and descriptive, but they are not generally based on probability calculations from established distributions.

Klein and Young (1980, pp. 60-61)

Theoretical econometricians have interpreted scientific objectivity to mean that an economist must identify exactly the variables in the model, the functional form, and the distribution of the errors. Given these assumptions, and given a data set, the econometric method produces an objective inference from the data set, unencumbered by the subjective opinions of the researcher. This advice could be treated as ludicrous, except that it fills all the econometric textbooks. Fortunately it is ignored by applied econometricians.

Leamer (1983, p. 36)

(B) Indistinguishability and Identification

Clearly no more complete description of the interconnections between a certain number of random variables can be given than that which is contained in their joint probability law. If, therefore, two different formulations of an economic theory lead to identically the same joint probability law of the observable random variables involved, we can not distinguish between them on the basis of observations.

If two stochastical equation systems lead to the same joint probability law of the observable random variables, they are indistinguishable (on the basis of observations).

Haavelmo (1944, p. 88, 91)

The identification problem in general arises from the fact that a given system of equations, in the mathematical sense of a set of restrictions on the movements of a number of variables, can be written in many ways. For instance, if the equations are linear, they can be replaced by any set of independent linear combinations of them. But there is only one (possibly unknown) way of writing the system such that a specified economic meaning attaches to each equation.

Koopmans (1945, pp.450-451)

[The] identification problem is concerned with the unambiguous definition of the parameters that are to be estimated— a logical problem that precedes estimation. It is therefore not a problem in statistical inference, but a prior problem arising in the specification and interpretation of the probability distribution of the variables. As such it deserves separate classification.

Koopmans, Rubin and Leipnik (1950, p.70)

[The] simultaneous equations method can always be applied in such a form that it will preserve, and give effect to, a priori information as to which variables enter into which equations. This is a strong point of the method, since it has often been pointed out that such information is truly indispensable to give economic significance to the statistical measurement of economic relations.

Koopmans (1945, p. 459)

As a substitute for experimental control, the non-experimental researcher is obligated to include in the regression equation all variables that might have an important effect. ...Though the number of observations of any phenomenon is clearly limited, the number of explanatory variables is logically unlimited.

[No] model with a finite number of parameters is actually believed, whether the data are experimental or non-experimental.

Leamer (1983, pp. 34-36)

A priori information is not rich enough to provide us with complete specification. ...The precise nature of the lag structures, nonlinearities, the degree of aggregation, and the selection of exogenous variables are not fixed a priori.

Howrey, Klein and McCarthy (1974, p.367)

Prior identifying information of the Cowles Commission variety, that is, mainly exclusion restrictions, plays a much smaller role in dynamic equilibrium models. Nonlinear cross-equation restrictions implied by dynamic theory are used extensively. This shift involves important modifications of past ways of thinking about identification and estimation.

Sargent (1981, p. 217)

Alternatives to the structural models have been sought because of increasingly compelling suspicions that the a priori restrictions used in existing structural models are not implied by good dynamic economic theory and that the interpretations and policy conclusion based on those faulty a priori restrictions are worth little.

Sims (1979, p. 8)

For practicing econometricians, extracting propositions from existing economic theory that are usable for specifying and identifying estimable equations is an excruciating difficult task. I believe that this is partly because most of economic theory consists of comparative static propositions, while historical data are generated by a dynamic economy and does not directly bear evidence on comparative static propositions of economic theory.

Ando (1981, p. 329)

In practice, ...when a 'moderate' – size econometric model of 400 or so equations is specified, it is beyond the scope of current macroeconomic theory. The theory is hardly capable of specifying all of these equations in any kind of detail; at most it may indicate potentially relevant variables for inclusion in each equation. Frequently the theory is little more than a plausible story, and only rarely does the theory being applied help to specify the lag structure of the variables.

Granger (1981, p. 124)

The true economy is a complicated Walrasian type model.....But if we drop the Walrasian objective and content ourselves with an aggregative system, where shall we draw limits on size? There is no unique aggregative representation of an economy. Many alternative versions provide different approximations to the 'true' system. This is, indeed, one of the sources of error in the stochastic specification of models.

Klein (1971a, p. 136)

(C) Autonomy and Instability

Theory always means reducing things to constancy. ...Complicated theories will establish the constancy in a complicated way, but will, in the end, also look for something constant. Describing phenomena without any sort of regularity or constancy behind them is no longer theory. An author who does not bind himself to some 'laws' is able to 'prove' anything at any moment he likes. But then he is telling stories, not making theory.

Tinbergen (1940, 1951, p. 71)

In trying to establish relations with high degree of autonomy we take into consideration various changes in the economic structure which might upset our relations, we try to dig down to such relationships as actually might be expected to have a great deal of invariance with respect to certain changes in structure that are 'reasonable'.

Haavelmo (1944, p.29)

[It] would be natural to require that the equations of the system should be autonomous; i.e., if the patterns of behavior described by one equation were assumed to change, this change should not affect the other patterns. This requirement is of importance if the system is used for studying the effect of 'structural changes'.

Wold (1949a, p. 15)

The economic units of the model should be autonomous in the sense that any unit may change or modify its behaviour pattern (as described by one or more causal relations), without the changing influencing the behaviour pattern of the other economic units of the model.

Wold (1954, pp. 173-174)

The economist is often required to estimate the effects of a given (intended or expected) change in the 'economic structure', i.e., in the very mechanism that produces his data.....The economist can do this if his past observations suffice to estimate the relevant 'structural parameters' prevailing before the change. Having estimated the past structure, the economist can estimate the effects of varying it. He can thus help to choose those variations of structure that would produce — from a given point of view — the most desirable results. That is, he can advise on policies (of a government or a firm).

Marschak (1948, p. 53)

For purposes of conditional forecasting..., one needs to know the structural parameters (for example, those describing the past behavior of the policy variables themselves) and therefore affects the reduced form parameters in a highly complex way. ...Unless one knows which structural parameters remain invariant as policy changes and which change (and has), an econometric model is of no value in assessing alternative policies.

[We] see no reason to believe that these models have isolated structures which will remain invariant across the class of interventions that figure in contemporary discussions of economic policy.

Lucas and Sargent (1979, p. 298, 302)

[Included] in a prominent way among the 'structural equations' have been equations describing the rules of choice for private agents. Consumption functions, investment schedules, demand functions for assets, and agricultural supply functions are all examples of such rules of choice. In dynamic settings, regarding the parameters of the rules of choice as structural or invariant under interventions violates our simple principle from economic theory.

Sargent (1981, p. 214)

[A] successful theoretical analysis required understanding the way in which optimizing agents make their decision rules depend on the dynamic environment in general and government policy rules in particular. The econometric ideal of discovering objects that are structural, in the sense that they are invariant with respect to the class of policy interventions to be analyzed, imposes that criterion for success...

Sargent (1982, p. 383)

Changes in the environment — including not only government policy but many other conditioning factors, demographic, technological, institutional, international, cultural — are bound to alter structural behavioral equations. Thus models become obsolete and must be revised or replaced. The quest for timeless and permanent regularities in social science is important. But any 'laws' thus discovered, other than identities, are likely to be qualitative and general, of little value in forecasting and policy evaluation.

Tobin (1981, p. 392)

The a priori setting of coefficients to zero in econometric models contradicts some basic tenets of general equilibrium theory. General equilibrium models imply that individuals' excess demand functions should all depend on the same arguments. ...No consistent model would ever imply these variable exclusion restrictions.

Miller (1978, pp. 582-583)

It is common practice in constructing econometric equations and models to try several different theoretically plausible forms of each equation. ...As far as I know everyone who estimates equations does some of this sort of experimenting.

Christ (1956, 1966, p. 325)

The underlying Walrasian model motivating or justifying large scale econometric modelling offers... no resting place or natural fix point for the analysis. It is, if we insist on its existence, an entity drifting over time according to an unknown stochastic process. But this implication destroys any real significance of an encompassing Walrasian model operating as a convergence limit with respect to actual model constructions guided by measures of goodness of fit.

Brunner (1981, p. 132)

Earlier we cited the stress attached to an objectivity ideal for econometrics by Klein in 1947. The pronounced change in stance represented by the contents of the Klein-Young book on forecasting with macroeconometric models is clear. This change fits with our stress on the inherent softness of practical econometric modelling. It is tempting to conclude that the experiences of developing economy-wide models and of demonstrating their potential uses to a steadily widening audience have made Klein aware of the inadequacies of some views that he expressed in the first decade of the modern era. To conclude the group of quotations that we have chosen to illustrate the basic proposition of the probability approach, we have added a severe judgement by an outstanding econometrician of a newer generation. Leamer rejects the probability approach of Haavelmo, Koopmans and the early Klein. The informational requirements of this approach are so excessive as to make the objectivity ideal of this "hard" representation of econometrics unworkable. Leamer has abandoned structural confirmation of the SEM for "specification searches" and a quite different perspective on the role of probabilistic notions in econometrics.

Whether the probability approach to econometrics is accepted or rejected, the importance of the three concepts of identification (or distinguishability), completeness and autonomy cannot be d~~enied in the his~~tory of the subject. The present character of econometric research cannot be understood without them. We intend to look at each in turn. In Section B of Table Three, some views on identification, prior restrictions, and the interaction between the structural form of the SEM and economic theory are reproduced. The clear visions of the Cowles-Commission researchers in 1944-1950 are in marked contrast with the unsettling features of the later opinions that are cited.

There are three major lines of development for the concept of identification if we ignore non-structural (time-series) models and Kalman-type models. One began in demand analysis as data on prices and quantities of some commodities became available. This was initiated by Moore over sixty years ago and is linked to the present through Working (1927), Schultz (1938) and the first textbook by Klein on econometrics. It is still common in recent textbooks. Data provide the initial focus. Identification is treated as the assessment of what they reveal for demand and supply schedules. Each data point is considered the outcome of equilibrium (the intersection of demand and supply equations). There are three major elements in this line of development. These are the assumptions of moving equilibrium which generate different sample observations, the need for a stable environment for economic agents which is required for structural stability of the equations involved, and the stress on single equations (for example, demand for a particular commodity) despite the recognition of their embeddedness in a system. There is little discussion of randomness. Shifts are primarily attributed to changes in known explanatory variables. Softness arises from a number of sources. Disequilibrium implies that not all of the data are informative for each equation. It may be revealed in "switching" of regimes (equations) for the data points no longer represent the intersection of demand and supply schedules. Softness comes also from the instability of these schedules. Economic theory based on individual preferences and constrained situations is markedly deficient as a source of intertemporal stability. Indeed this shortfall is a severe handicap for any structural estimation. The perceived primacy of the probability approach is considerabily weakened if structural instability implies a growing number of structural parameters as sample duration increases. In the extreme, maximum likelihood estimation would be inapplicable. Within the moving-equilibrium format, exclusion of variables (zero restrictions on structural parameters) is commonly justified by introspective reflection on the determinants of supply and demand.

The second line of development makes much more use of dynamic modelling and recursive behaviour. Prime examples are the efforts of Tinbergen and Wold to develop economy-wide econometric models and to clarify the use of causality in interpretation and specification of such models. Here zero restrictions on parameters arise naturally as the representation of delayed response of particular economic variables to changes in other economic variables. This delay could stem from institutional characteristics, such as the inevitable lapse for construction of equipment and structures in investment equations or for the provisions of appropriate levels of funding, and from behavioural responses to "permanent" changes in determining factors rather than transitory ones. The consequence of this delay, especially if it yields recursivity, is to remove problems of indistinguishability and to demonstrate a basic asymmetry in economic equations which we shall later consider. Although this line of development often involves the assumption of zero correlations, it does not require the same degree of prior specification as that exhibited in the probability approach.

The final line of development for the concept of identification (and the one generally associated with the probability approach) stems from an unpublished memorandum that was prepared by Frisch in 1938 for a conference dealing with Tinbergen's models for the League of Nations. Frisch's discussion was taken up by his student Haavelmo and given a particular flavour in the treatment by Koopmans, Rubin and Leipnik (1950), who derive the now familiar rank and order conditions for identifiability with exclusion constraints.

The first four entries in Section B of Table Three illustrate the role of probability notions in this line of development. Haavelmo (1943b, p. 18) indicates a straightforward difficulty. "Theories with different economic meaning might lead to exactly the same probability law of its observable variables, just as different pairs of supply and demand curves might have the same intersection point." Thus theories are indistinguishable as far as their observable effects are concerned. Koopmans et al. argue that this makes identification an aspect of estimation. They prefer to treat it as a problem distinct from estimation and logically prior to it. If a system of equations is not identified, they would argue that structural parameters are not unambiguously defined. Does this matter? There are two rival responses to this question. One accepts identification as a major concern. If the primary purpose of structural estimation is to permit discussion of hypothetical experiments involving known changes in structural parameters (as a contribution to "policy" determination), the identification of all structural parameters is desirable. Clearly structural estimation has then an important role in policy formation. The alternative response is less favourable to the concept. Since the rank condition for identifiability is expressed in terms of unknown reduced form parameters, we can never know that a system of equations is identified. The order condition does indicate situations in which the system is not identified (but not necessarily all of them). However it is sensitive to the choice of prior restrictions and to a host of complications that are seldom raised in the statement of the order theorem. The remaining contributions to Section B illustrate this softness and, consequently, the weakening of support for estimation of the structural form of the SEM.

The sources of these contributions are mixed. They include both prominent Keynesians and proponents of the new classical school. All involve criticism of identification as a guide for econometric research. Some argue that economic theory is insufficient to provide an adequate basis for the firm specification of structural equations, as required for rank and order conditions. Others argue the economic theory yields constraints on parameters that are not of the exclusion variety. There is a fundamental problem here. Structural equations are supposed to represent economic theory even though most theory is qualitative, deterministic and non-dynamic. Exclusions and predetermination of variables are also supposed to be generated from this economic basis. Frisch was clearly affected by Walrasian models and his influence persisted at the Cowles Commission. Yet many of the models developed in the last four decades have a Keynesian flavour. Indeed, since the Lucas critique of these models, they are frequently termed Keynesian macroeconometric models. When Koopmans (1945, p. 459) suggests that it has often been pointed out that exclusion information "is truly indispensable to give economic significance", he does not indicate how this information stems from economic theory. This linkage is not a trivial matter. It is clear from our choice of contributions that there is widespread rejection of the view that exclusions represent economic theory in practical situations. When we add dynamic dimensions (Sargent, Sims, Ando), aggregation (Howrey et al., Klein), non-linearities (Howrey et al., Sargent), and experimentation (Christ), the contribution to structural forms of the SEM from economic theory is obfuscated.

The primacy of the probability approach to econometrics is effectively shattered by this softness in the applicability of its form of the identification concept. The final word might be left to Koopmans. "[The] economic literature does not offer us anything like a systematic dynamic theory to work with, but rather a variety of incidental ideas, as yet full of gaps and ambiguities. This was precisely the difficulty of the econometricians who looked toward theory as a framework of reference from which to interpret systematic observations. They often had to supplement or even produce theories in order to advance in the direction they took."

The probability approach also involves the concept of completeness. For many purposes, a structural approach requires explicit modelling of all equations in a system. With hypothetical experiments involving structural change, the impact of such change is appraised from an amended reduced form of the SEM with a multiplicity of changes affecting the reduced-form parameters. To obtain estimates of these secondary changes, a complete system of structural equations must be examined. In the absence of a decomposable structural system, the estimates of changes in reduced-form parameters cannot be based on an incomplete specification of structural equations. The probability approach has to be derived from the joint distribution of equation errors so it must begin with a specification of the number of such errors. Thus size and completeness are essential ingredients of this approach. We can use "size" to represent the number of equation errors (or the number of endogenous variables if this is different) and "completeness" to indicate the equality of the number of equations and the number of endogenous variables. The choice of size is arbitrary, it seems, from the entries of Table Four. These are derived from a survey by Kelejian and Vavrichek (1981). The range of specification for size is considerable. Were these choices based on completeness? We were unable to locate a single justification of size on this basis. It is highly likely that some variables described as "exogenous" or "predetermined" in these models were potentially correlated with contemporaneous equation errors. Thus the concept of completeness does not motivate model specification in practice.

The final use of completeness in the SEM (in addition to the hypothetical experiments and size already noted) is concerned with estimation. All treatments in our textbooks of estimators, other than least-squares estimators, take the number of predetermined variables in a system as given. This requires the specification of all equations in a complete system to be set. Mathematical expressions of the two-stage least-squares estimator, for example, will include a symbol to represent a matrix of observations on all predetermined variables. These treatments are dated since the development of an instrumental-variable focus has removed the need for completeness in this respect. The derivation of the limited-information maximum-likelihood estimator by Anderson and Rubin (1949, 1950) and the consideration

TABLE FOUR

INDICATORS OF MODEL SIZE

	Number of Equations:		Number of Variables:	
Model	Total	Stochastic	Exogenous	Predetermined
BEA	196	108	150	249
Chase	350	150	150	350
Data Resources	831	350	178	628
Michigan	81	47	76	105
Wharton	695	299	242	677
Fair	97	29	83	139
St. Louis	7	5	5	7

Source: Kelejian and Vávrichek (1981), p. 111.

of incomplete systems by Wald (1950), both at the Cowles Commission, were the first steps to the weakening of the completeness requirement for estimation. It should be clear that the greater the dependence on prior specification, the more soft will be the bases for estimates in practice. Malinvaud (1981) provides a qualification for this statement when false constraints are deliberately imposed to improve efficiency of estimates at the expense of additional bias. Such behaviour is quite at odds with the emphasis by the Cowles-Commission group on asymptotic unbiasedness and consistency. It is thus not surprising to find Malinvaud's comments in a review of Sims' advocacy of non-structural VAR models.

The final concept of the probability approach that we wish to explore is that of autonomy, which is due to Frisch. Tinbergen (1939, p. 14) described this as a requirement that models involve "relationships which are as little as possible affected by structural changes in departments of economic life other the one they belong to" while Arrow (1960), more succinctly, refers to "invariance under conceivable changes in the rest of the system". The concept is thus associated, in older terminology, with the degree of permanence of economic laws (Haavelmo, 1944, Ch. II) and, in more recent language, with instability of structural equations. Section C of Table Three illustrates views on autonomy. Those of Tinbergen, Wold and Haavelmo set the scene for the beginning of the modern era in econometrics. Marschak, although he does not explicitly cite autonomy, describes the hypothetical experiment to which we have already made reference. His description needs to be supplemented. Proponents of structural-form estimation, such as Marschak, feel that the equations of the structural form of the SEM are much more autonomous than those of reduced form. They are, therefore, less prone to instability and provide a suitable basis on which to frame hypothetical experiments. If the structural form did not exhibit a high degree of autonomy, its parameters could not be adequately estimated so as to provide the necessary input to Marschak's policy advisor.

We have to ask whether the assumed autonomy of structural estimates is justified. Sargent (1981, 1982) and Lucas and Sargent (1979) made a strong case for rejecting autonomy. Their "rational expectations" perspective yields cross-equation constraints that were seldom discussed in the early days of the Cowles Commission. Their impact on the autonomy of the SEM can be illustrated with a simple example. Suppose we consider five variables linked by a pair of structural equations

$$(1) \qquad y_{1t} + a_1 y_{2t} + a_2 y^*_{3t} + a_3 z_{1t} = e_{1t}$$

$$(2) \qquad y_{2t} + b_1 y^*_{3t} + b_2 z_{2t} = e_{2t}$$

Here y_{1t} and y_{2t} are measured endogenous variables, z_{1t} and z_{2t} are measured exogenous variables, and e_{1t} and e_{2t} are random errors with zero means, free from autocorrelation and heteroscedasticity. The fifth variable y^*_{3t} is an "anticipated" ("expected") one that needs further specification. The terms involving a and b are fixed parameters of the equations. The variable y^*_{3t} is generated by the third equation

$$(3) \qquad y^*_{3t} = c_1 y_{3t-1} + c_2 y_{3t-2}$$

This equation is exact, having no error, and its fixed parameters c_1 and c_2 are assumed to be determined by the time series characteristics of a measured variable y_{3t} so that

$$(4) \qquad y^*_{3t} = E(y_{3t}/y_{3t-1}, y_{3t-2}, \ldots)$$

The set of equations (1), (2) and (3) illustrate specifications that are common in the history of econometrics. They are conventionally combined to form a ("derived") structural form with two equations.

$$(5) \qquad y_{1t} + a_1 y_{2t} + a_3 z_{1t} + a_4 y_{3t-1} + a_5 y_{3t-2} = e_{1t}$$

and

$$(6) \qquad y_{2t} + b_2 z_{2t} + b_4 y_{3t-1} + b_5 y_{3t-2} = e_{2t}$$

where the "new" parameters (a_4, a_5, b_4, b_5) are connected to the original parameters (a_2, b_1, c_1, c_2) by the reconciling constraints:

$$(7) \qquad \begin{array}{ll} a_4 = a_2 c_1 & a_5 = a_2 c_2 \\ b_4 = b_2 c_1 & b_5 = b_2 c_2 \end{array}$$

These constraints for reconciliation have often been ignored. They can be expressed in an alternative form involving cross-equation restrictions. It is apparent that they imply

$$(8) \qquad a_5/a_4 = b_5/b_4 = \lambda$$

where λ is a constant given by the ratio of c_2 to c_1.

The derived structural form is typical of models involving distributed-lag response. "Rationality" is assumed to imply that (4) is an appropriate specification for both (1) and (2). The constraint (8) follows directly from the assumptions of rationality and the particular time-series model underlying equation (3). This model is a Yule process

$$(9) \qquad y_{3t} = c_1 y_{3t-1} + c_2 y_{3t-2} + e_{3t}$$

where the error e_{3t} has a zero mean and is free from autocorrelation and heteroscedasticity. The anticipation variable y^*_{3t} is *exogenous* when e_{3t} is *independent* of both e_{1t} and e_{2t}. The standard simultaneous-equations model would be formed by (5) and (6) as its "structural form". It would omit the implicit constraint (8). There are two major conclusions to be drawn from this example. First, hypothetical experiments with structural parameters (as may be involved in predictions of policy changes) assume they are "free" in the sense that any can be changed without affecting others. The cross-equation constraint indicates that the parameters a_4, a_5, b_4 and b_5 are not "free" so hypothetical experiments (predictions) must take their connectedness into account. Thus the equations are not autonomous. Second, if the time-series characteristics of y_{3t} change, perhaps due to policy changes or to alterations in exogenous variables, then the requirement (4) of y^*_{3t} as a conditional expectation implies changes in c_1 and c_2. The parameters a_4, a_5, b_4 and b_5 will change. Thus any sequence of intertemporal influences that changes the time series characteristics of y_{3t} will make the SEM structural form unstable.

These are severe criticisms of the SEM. In the extreme, they support Brunner's view of perpetual instability. The view of Tobin is less extreme but it is equally disconcerting for the probability approach. There are also difficulties for methods of analysis that do not stress the structural form or the reduced form. Tinbergen's analysis of his economy-wide models included the derivation of a final equation of these models, essentially a time-series form. Wold's exploration of causality led to his advocacy of a recursive form. Both of these derivations can be extended. In one attempted extension, Sims has stressed the moving-average representation of stationary economic variables. With Sargent, he has also explored the autoregressive characterization of these variables and their embodiment in index models. These MAR and VAR approaches, although responses to the perceived lack of autonomy of the structural form, have uncertain properties. It is difficult to see why they should be less unstable than their more common rival.

Interactions Among Variables

The probability approach to econometrics obviously stresses the interactions among measured variables as well as the probability density function of equation errors. However the treatment of interactions is not comprehensive. This stems from the use of "complete" systems of equations, where the transformation of densities is focused only on endogenous variables, rather than "self-contained" systems. The structural form and the reduced form of the SEM are based on the probability density function of the endogenous variables conditional on values of the exogenous or predetermined variables. This conditionality is important. It may be a major factor in the insufficiency of most treatments of exogenous variables and, especially in, the almost total lack of discussion of interactions among the exogenous variables themselves. The statistical definition of exogeneity can be used without full specification of the interactions. In many situations, therefore, consistent estimation of model parameters is possible without further exploration of exogenous variables. There are a number of hidden complications, however, that should be considered. These affect the derivation of multipliers from structural models, control and policy prescriptions, simulative experiments and certain other aspects of the use and interpretation of fitted models (not least of which involves the connection between the SEM and time-series models). It is appropriate to begin our discussion of these complications by rediscovering the important paper of Orcutt (1952). We shall consider his comments in conjunction with the treatment of causal ordering and identifiability by Simon (1953), which is often cited as the principal basis for the conditionality treatment of exogeneity in the SEM. It will also be interesting to note the adjustments that Tinbergen made to replace exogenous variables and obtain his final form from his fitted equations. We shall link his approach with more recent attempts to find the "typical" spectral shape of economic variables.

Simon's formulation begins with a self-contained system of equations linking measured variables. This can be illustrated with a linear specification such as

$$(10) \qquad Ax_t = u_t$$

Here A is square matrix of fixed parameters, x_t is a vector of observations that are observed in a given time period and u_t is a vector of equation errors. (We can ignore non-exogenous predetermined variables without any loss of generality for our account.) The triple of elements in this system of equation (A, x_t, u_t) is assumed to be affected by zero constraints and re-expressed in the form

$$(11a) \qquad A_{11}y_t + A_{12}z_t = u_{1t}$$

$$(11b) \qquad A_{22}z_t = u_{2t}$$

where y_t and z_t are two parts of x_t. The initial vector of equation errors has been split into u_{1t} and u_{2t}, which are assumed to be uncorrelated. This re-expressed specification has, for A_{11} square, the familiar complete SEM in (11a) and a supplemental collection of interactions among the variables in z_t in (11b). Using the statistical definition of exogeneity, z_t is exogenous and y_t endogenous.

A common practice in economic research is to ignore (11b) and to estimate the parameters in A_{11} and A_{12} after imposing a normalization rule. Multipliers are then based on the estimated $A_{11}^{-1}A_{12}$. Simulated experiments are an additional exercise with this estimated structure augmented by prescribed paths for exogenous variables, possible amendments of estimated structural parameters and, sometimes, random perturbations to represent the errors' impact. This behaviour is little affected if we replace the elements in A by polynomial in the lag operator with fixed coefficients. For the hypothetical policy experiments of early proponents of the probability approach at the Cowles Commission to be meaningful in this context, several conditions must hold. Their fulfilment is not simply a question of mathematical convenience. It is rather a matter of matching assumptions with the real context. We should note four of the conditions.

First, the basis for partitioning measured variables into polar types must be appropriate. If A_{11} is too "small" in size (as when too few endogenous variables are acknowledged), not only are estimated parameters potentially inconsistent and asymptotically biased but also interactions among endogenous variables are mis-stated. The whole procedure of calculating multipliers and simulating responses is compromised. This was stressed by Orcutt who, as we pointed out earlier, criticized the arbitrariness of most partitions of variables and advocated the development of appropriate tests for exogeneity. Two decades passed before Sims (1972a,b), Wu (1973), and Revankar and Hartley (1973) revived interest in such tests.

The second condition requires the prescribed paths for exogenous variables to be consistent with the omitted equations (11b), which constrain the independent movement of exogenous variables, unless these are unstable. Even with instability, these variables are constrained by the successors of (11b). Since little attention is deliberately given to these interactive constraints in practice, it is difficult to believe that this condition will ever be satisfied. Indeed it is highly likely that prescribed values for exogenous variables will be mutually inconsistent. Orcutt (p. 199) puts this problem in simple terms. "The real difficulty is that with existing econometric models no claim is made or evidence presented that the included exogenous variables are uninfluenced by each other, nor is any information provided of the way their movements are related. Clearly, if when one exogenous variable is moved another exogenous variable in any part of the model also systematically moves, then its movements must be taken into account in order to predict from the model the effect of the action." There is another complication of these interactions that affects tests for exogeneity and does not seem to have received much attention. How do we test for exogeneity when there are interactions among potential exogenous variables and when these are not explicitly modelled? The slow progress in extending Sims' test to more than two-variable pairs of equations is hardly an optimistic sign for the feasibility of a constructive answer to this question.

The next condition involves the time-series characteristics of economic variables and can be linked to the criticisms of standard simulative practice from those who accept rational expectations. Mishkin (1979) describes some of these criticisms. The condition requires that changes planned or anticipated for exogenous variables, so as to affect the temporal behaviour of these variables, must not lead to parametric changes in the structural form. Suppose part of the auxiliary model (11b) reflects the presumption that this model is consistent with the time-series behaviour of one or more exogenous variable. Now suppose this behaviour is assumed to change as part of a simulative experiment or of a hypothetical one. Then the parameters in A_{22} will change and, hence from the rational-expectations perspective, so will the parameters of A_{11} and A_{12} be expected to change. For a simulative experiment, this implies that the "control simulation" (which is generated by setting exogenous variables at their historical values) is an inadequate path with which to compare the predicted paths associated with experiments. Differences between paths will not reflect the changed structure and may give a markedly distorted impression of the impact of a policy change or of a truly exogenous event.

The final condition that we shall indicate is again concerned with instability of the auxiliary equations (11b) but now in a passive way rather than through the assumption of rational expectations. Orcutt writes of a condition of continuity of non-controlled variables. If research is focused on the consequences of the manipulation of controlled exogenous variables, it must recognize the inherent delay in responses to such manipulation. However, the presence of delay means that predicted responses presume the continuity of other variables from the initial step of manipulation and throughout the total period covering both delay and the working out of the final cumulative response. Orcutt (p. 196) suggests that more study of the continuity properties of economic time series is "needed as a basis for specifying what kinds of lags can be tolerated in the impact of the control instruments" but there is little evidence that his suggestion has generated sufficient attention. This is surprising since a lack of continuity was detected in the first attempts at simulative experiments with time series by Slutsky almost sixty years ago.

Given the probable non-fulfilment of these and other conditions in practice, it seems appropriate to consider the origins of a particular procedure that is often used to generate values of exogenous variables for post-sample simulations. This stems from Tinbergen's derivation of a final form which indicated a common time-series character for many economic varies that are bound together in a complete SEM. The reduced form of (11a) is obtained by multiplication with the inverse of A_{11}. An intermediate form is obtained by multiplying instead by the adjoint of this matrix. This yields

$$(12) \qquad |A_{11}|y_t = -(\text{adj } A_{11})A_{12}z_t + (\text{adj } A_{11})u_{1t}$$

which is a non-homogeneous difference equation in y_t if the elements of A_{11} are polynomial in the lag operator. The same scalar $|A_{11}|$ is associated with each endogenous variable.

Tinbergen (1939, p. 130; 1940, p. 68) uses estimates of A_{11} to discuss the homogeneous component of this equation, namely

$$(13) \qquad |A_{11}| y_{it} = 0$$

for the i-th endogenous variable. He describes this as the "natural tendency" of the system while its source (12) has given a variety of names including the "separated form" (Marschak, 1950), the "transfer equations" (Zellner and Palm, 1974) as well as Tinbergen's choice of "final form". Dhrymes (1971) suggested "the autoregressive final form" to stress the AR characteristics of $|A_{11}|$.

Our purposes in describing (12, 13) are to reveal again a link between the SEM and time-series models and to find softness in this linkage. The existence of the connection between models is the focus of the SEMTSA framework of Zellner and Palm. Softness arises both from an inadequate treatment of exogenous variables and from a build-up of potential inadequacy in the elimination process (that is, in the computational equivalent of multiplying by the adjoint). We can illustrate the former using Orcutt (1948) while Koopmans (1941, p. 134) points to the elimination hazard. We shall consider these references before turning to the treatment of the exogenous variables in post-sample simulation. Faced with (12), Orcutt (1948, p. 7) argues that "for a short series, it might not be unreasonable to hope that the exogenous variables might be represented as a constant plus a random component, and, therefore, we might entertain the further hope that the economic series themselves might be drawings from a population of linear stochastic series, all having the same underlying autoregressive structure". This is surprising expression of hope in view of the stress on interactions among exogenous variables in Orcutt (1952), which we have already noted. Both Tinbergen and the early Orcutt seem to be prepared to ignore the right hand side of (12), its non-homogeneity, in favour of the myopic autoregression. Their estimated final equations are

$$(14) \qquad (1 - 0.398\theta + 0.220\theta^2 - 0.13\theta^3 - 0.027\theta^4)y_t = v_t \text{ (Tinbergen)}$$

$$(1 - 1.3\theta + 0.3\theta^2)y_t = v_t \quad \text{(Orcutt)}$$

using the same U.S. data. Tinbergen's result was derived by elimination after his structure was estimated by the least-squares method while Orcutt used a time-series approach based on the common presence of $|A_{11}|$ for all endogenous variables. Taken together, their efforts integrate the SEM and time-series approach provided the exogenous variables are passive and provided we do not mis-specify the error in the attendant time-series model. This complication with the error can be illustrated with a simple model of Hurwicz (1944, p. 118).

Suppose x_t and y_t are two individual endogenous variables bound together by the pair of equations

$$(15a) \qquad y_t = ay_{t-1} + b_1x_{t-1} + b_2x_{t-2} + u_{1t}$$

$$(15b) \qquad x_t = cy_{t-1} + u_{2t}$$

where u_{1t} and u_{2t} are assumed to be free from autocorrelation.

Elimination yields the final form

(16a) $$y_t + d_1 y_{t-1} + d_2 y_{t-2} + d_3 y_{t-3} = e_t$$

where (d_1, d_2, d_3) depend on the structural parameters (a, b_1, b_2, c) and e_t is a composite error given by

(16b) $$e_t = u_{1t} + b_1 u_{2t-1} + b_2 u_{2t-2}$$

Clearly this composite error is not a purely random component. It is autocorrelated so (16a) is an ARMA process rather than the simpler AR one selected by Orcutt. "That this phenomenon has not been given attention is probably due to the fact that the original set of difference equations is usually treated as a homogeneous one and the 'disturbance' is introduced as a deus ex machina only after the reduction process has been completed." This is an unfortunate oversight in the history of econometrics that was embedded in research practice for over three decades in the form of Orcutt-Cochran autoregressive "corrections". These presume equation errors are linear combinations of omitted factors, all with the common time-series characteristics given by $|A_{11}|$.

Turning to the passivity of exogenous variables, it is clear that this assumption places strong restrictions on the auxiliary equations (11b). It is difficult to accept that $A_{22}^{-1}u_{2t}$ is simply the sum of a constant and a random component. More generally this should be substituted for z_t in (12) so that the equation is re-expressed as

(17) $$|A_{11}| y_t = -(\text{adj } A_{11}) A_{12} A_{22}^{-1} u_{2t} + (\text{adj } A_{11}) u_{1t}$$

which obviously has a complicated composite error that has seldom been acknowledged in time-series models. This error's generating process will depend on the interactions among exogenous variables through the presence of A_{22}^{-1} . It will also depend on structural parameters and, consequently, is subject to the instabilities that we have already considered. Such complications must also affect our interpretation of the attempts by Granger (1966), Nerlove (1972), Nerlove et al. (1979, ch. 9) and Dubbelman et al. (1978) and others to establish a typical spectral shape for economic time series. These attempts are the straightforward counterpart in the frequency domain of Orcutt's efforts using autocorrelations. We can surely find equivalent sources of softness.

The problem with elimination to find the final form can now be addressed. Koopmans (1944) pointed out that the use of distributed lags in dynamic economics is hampered by the rigidities of sampling intervals as they affect the mathematical representations of models. Lagged responses are usually taken at integral values of such intervals. For example, in a model of investment, this variable will be specified as dependent on successive annual changes of output if the data for estimation are collected for annual sampling intervals. Different specifications would be made for use with quarterly or semi-annual data. In any structural form, we can see that it is likely that some inaccuracies will arise because economic behaviour is based on nonintegral timing while the equations represent integral lags. When the estimated structure is manipulated to obtain reduced or final forms, it is possible that there will occur a "gradual widening of lag distributions in the process of elimination, due to the repeated replacement of terms with nonintegral timing by a linear combination of two terms with the nearest integers as timing indices" so that the implied dynamic pattern in these secondary forms is misleading. The larger the structure and the sampling interval, the more severe may be the problem. If we do not ignore the autocorrelation of the errors in the final form, the extent of the problem as it affects the ARMA process will be affected by interactions among exogenous variables especially if they contain lag structure themselves.

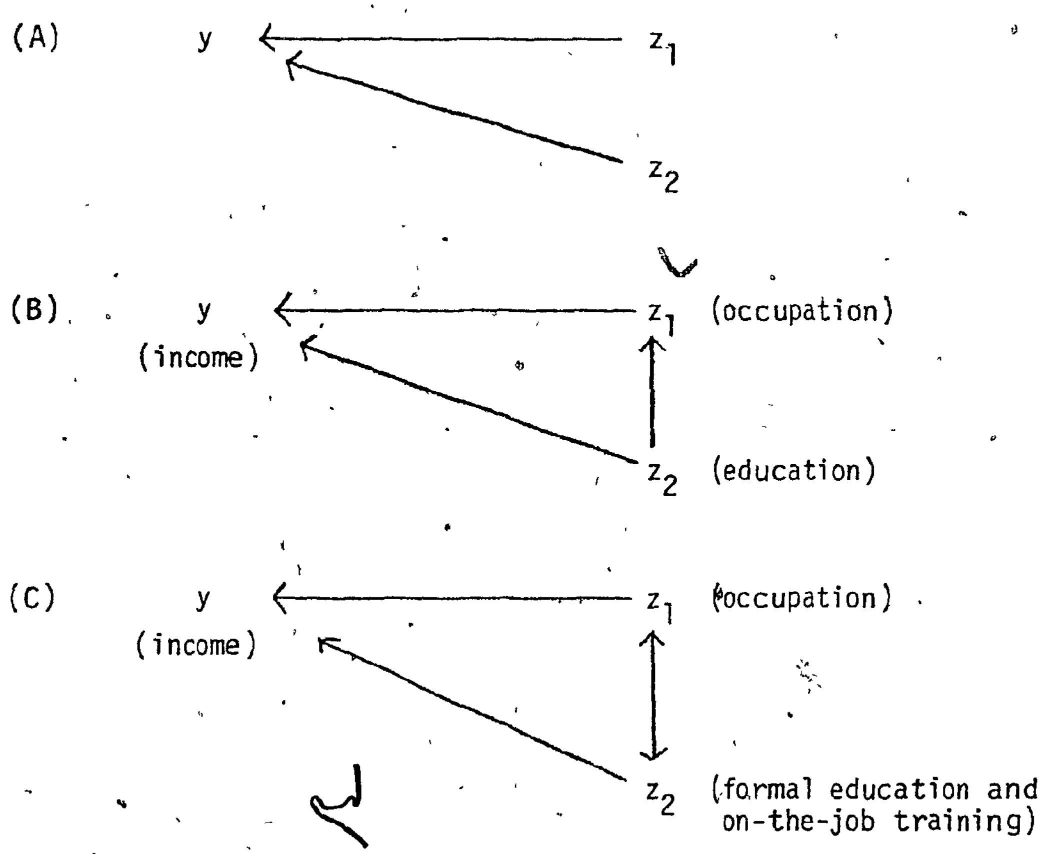
The treatment of exogenous variables in preparations for simulative experiments will depend on whether they are considered to be controlled or uncontrolled. For the latter, the most common procedure is to fit a time-series model, usually an AR version, to the sample observations for a particular variable and then to extrapolate this fitted model throughout the period covered by the experiment. Although the auxiliary equations (11b) could be used to project values of exogenous variables as generated by a vector ARMA process, this is seldom done. Usually separate time-series models are fitted to the variables ignoring their interactions. Changes in controls are taken to leave other exogenous variables unaffected.

Interactions among variables are markedly different in the newer business-cycle models of Lucas and others. Their focus on co-movements is revealed by Lucas (1977, p.9). "Technically, movements about trend in gross national product in any country can be well described by a stochastically disturbed difference equation of very low order. These movements do not exhibit uniformity of either period or amplitude ..... Those regularities which are observed are in the co-movements among different time series." As translated into econometric models, this concern has stimulated much more interest in the co-movements of exogenous variables. In a vital sense, these have become centre of attention either as unrestricted VAR or in the index models of Sargent and Sims (1977), which add arbitrary restrictions on dimensionality. Sims (1981) and Sargent (1979) discuss this focus. Their accounts reveal a radical reappraisal of structural estimations. Sargent (p. 8) illustrates this when he asserts that "the idea is to estimate vector autoregressions with many free parameters and to introduce restrictions not directly motivated by economic theory but rather aimed at forecasting better, that is, delivering estimators with small mean squared errors". The index models may be linked to an intuition of Mitchell (1951) that movements in many macroeconomic variables can be viewed as reflecting one underlying index. This yields the conformity or coherence of their time-series data.

So far we have considered the neglect of interactions among exogenous variables. A complete appraisal requires an account of methods that have been developed to deal with these interactions when the impact of one variable on another is of special interest. The path models of Wright provide a suitable framework for this account. These were used by Wright to explore agricultural demand and supply relations as early as 1925. Present techniques are little different from those that he used. Suppose we consider the three cases indicated in Figure Three. In (A), a variable y is affected by variables z_1 and z_2. A simple estimate of the impact of these influences could be obtained from fitted regression coefficients for a single equation with z_1 and z_2 as carriers. In (B), the variable z_2 has a two-fold impact on y, one direct and one indirect through its influence on z_1. We can, for example, see education affecting income both directly and through its impact on occupational choice. Path models would attach regression coefficients to all sources of influence with the correlation between z_1 and z_2 added as a multiplicative factor for the indirect influence. This addition serves as an alternative to the explicit modelling of the determinants of z_1, including z_2. A complication arises when there is reciprocal influence as in case (C). We can extend the income example to allow education to include both formal schooling and on-the-job training. The latter might be affected by occupational choice. The impact of z_2 (education) on y (income) is then difficult to determine within this path-model framework. It is clear that the arrow scheme of Wright and Tinbergen does aid the qualitative

FIGURE THREE

CAUSAL INFLUENCE: THREE CASES



interpretation of causal relationships. Unfortunately, as must be clear, the use of correlations as weights in measures of indirect effects is difficult to justify except on grounds of simplicity. It might be argued that the use of the correlations may be less harmful than the total neglect of (11b) in the SEM. However, the only satisfactory treatment must surely involve giving more attention to the explicit modelling of linkages among exogenous variables, or determining factors. If we consider more recent expositions of path models, such as Heise (1975) and Kenny (1979), we find little to undermine such demands for explicit treatment of auxiliary equations.

Wold's advocacy of eo ipso predictors and causal chains provide a final illustration of softness due to the imprecise recognition of interactions and of the attendant difficulties in the specification of structural equations. He argues that instead of the structural form of the SEM, we should use a system in which each equation can be interpreted as based on the conditional expectation of one endogenous variable given values of all other included variables. The resulting system is the recursive form of the SEM and can be seen as a direct extension of the classical statistical models developed by Karl Pearson and Yule. Wold (1964c) gives a simple statement of his position. "In stochastic model building it is an obvious requirement that relations intended for use in forecasting should be designed as conditional expectations subject to residual disturbance, or as we shall say, as eo ipso predictors." Within this framework, Wold finds no need to set the joint distribution of variables so that the recursivity imposes constraints on parameters for endogenous variables while leaving the interactions among exogenous variables unspecified. Strotz and Wold (1960, p. 427) compare this framework with that of the Cowles Commission, which they term "interdependent", and provide an important element for the use of economic theory in specification. "If a causal interpretation of an interdepe-ndent system is possible it is to be provided in terms of a recursive system. The interdependent system is then either an approximation to the recursive system or a description of its equilibrium state." In a disequilibrium model, the inter-actions among exogenous variables will be dynamic and they may be based in part on the actual time-series characteristics of these variables. The situation for traditional equilibrium models is less clear. Comparative-static equilibria involve fixed values of exogenous variables and leave dynamic motions either unrecognized or qualitative. Can equilibrium theory provide a specification for the auxiliary equations (11b) of the SEM if these are part of an equilibrium framework?

The new classical approaches of Lucas and others, who stress "equilibrium business-cycle theory", are radically different since they cast out the traditional use of equilib-rium and provide an alternative that is not only dynamic but, in fact, markedly dependent on the joint intertemporal movements of exogenous variables. Clearly such approaches, in their use of rational expectations, also borrow Wold's focus on conditional expectations. However they differ in significant ways. In the rational-expectations framework, the interactions among exogenous variables are passive and not linked to control. They can be observed in time-series data. Wold would, on the other hand, base them on disequilibrium theory and allow them to be amended by controls. Their specification would then need to be linked to economic theory. In practice, we can see the direct use of time-series criteria for identification (such as those of Akaike and Schwarz) in choosing structural specification for the new equilibrium models while these are secondary to economic theory in Wold's models.

False Constraints

Although econometricians are now familiar with the deliberate use of constraints (possibly false) to provide estimators with lower mean square errors,this was not much in evidence during the period from 1940 to 1960. The stress on asymptotic bias and consistency and the severe attack on least-squares bias took the avoidance of bias as a major objective. Constraints arose in the discussion of identification and exogeneity. These also arose in the choice of estimators and in appraisals of the relative asymptotic efficiency. However their potential falsity was not made explicit until Liu (1955, 1960) raised the issue. We can contrast his comments with the defensive ones of Klein (1960a,b). Despite the contents of most econometric textbooks, it is clear that much of recent research has accepted some of Liu's objections to the excessive use of constraints. Chow (1981, p. 437) provides a typical view. "[An] econometrician might wish to estimate not the true model but an approximate model because the sample is finite. One realizes that the conditions stated by Koopmans for defining the exogenous and/or predetermined variables are never met, that the coefficients of many endogenous and exogenous variables in a structural equation are not zero as Liu has pointed out, and that the true model is not strictly recursive in the sense of Wold.... One is seldom in a position to estimate the parameters of the true model because the number of available observations is often smaller than the number of its parameters."

Liu's criticism of the SEM and the approach to structural estimation stemming from the Cowles Commission has several important ingredients. It builds on two fundamental positions. First, structural equations are underidentified. "The complexity of modern economic society makes it much more likely that the true structural relationships are underidentified rather than overidentified. That the existing empirical structural relationships are overidentified may very well be the result of unconscious but determined efforts to obtain 'significant' structural estimates." (Liu, 1960, p. 856) Second, the presence of multicollinearity in economic data should be acknowledged and then utilized in making forecasts.

Taken together, underidentification and multicollinearity imply that "no reliable estimates can be made of the coefficients in the structural relationships" (Liu, 1955) so we should go beyond the SEM to see if alternative forecasting procedures can be developed. Liu introduced both a pseudo-structural form and a pseudo-reduced form, which are exploratory and not structure-confirming. These are important in the process of choosing variables and in forecasting. The exploratory structure is fitted for alternative lists of included variables by the least-squares method. Its purpose is not to find "structure" but rather to provide a manageable number of predetermined variables for use in the pseudo-reduced form or forecasting equations. The estimated values of structural coefficients are not used in making forecasts. Variables that are retained for forecasting equations are representative of a host of variables due to multicollinearity. This softness in the precision of structural estimates is a source of strength for unconditional forecasting of the type envisioned by Liu. "[We] are dependent upon these collinearity tendencies to 'catch' the important forces acting upon a given endogenous variable so that forecasting may be successful." This is similar to the recent use of principal components in regression models affected by collinearity and, also, to the basic notions of index models.

Klein's defence against these criticisms is not one of strong support for structural estimation. He concedes that it is misleading to look at individual structural parameters (even though these are at the heart of the hypothetical experiment that was used to generate support for the structural form, as for example by Marschak). By 1960, he had already shifted his interest to the solution of the SEM for given conditions. He stresses the needs for an overall summary statistic with which to compare alternative methods of estimation, while maintaining that "I believe that the general rule in realistic econometric models is heavy overidentification", and for the use of the reduced form in forecasting. Klein's response is also interesting for its acknowledgement that small changes in estimated structural equations often lead to pronounced changes in the implied reduced forms that are derived from them. Thus forecasts may be very sensitive to structural estimates, which is particularly unfortunate if combined with the problem of multicollinearity that was noted by Liu. Klein also pointed to the limited potential for improvement to forecasts from changes in statistical methods. This contrasts with the improvements that he expected in 1960 to stem from better data and better knowledge of economic institutions, areas of softness that deserve attention.

Liu (1960, pp. 858-860) looks at the principal characteristics of the actual behaviour of econometricians in the 1950s. They seem remarkably similar to those that generally prevail today. "The habit of fitting even more oversimplified relationships has been so deeply and firmly implanted in us that we may consider even six variables to be an extraordinarily large number." His account of the criterion used by Klein and Goldberger to choose variables is just as relevant for appraising the structural content of later economy-wide econometric models. Such content may indeed be indeterminate because of the environment in which we work and because of the procedures we use. He deserves the last words on the imposition of false constraints. "[No] econometrician can be an anti-structural estimationist. Our ultimate aim is naturally to estimate economic structures, but it is not our job to derive 'structural' relationships by artificially overlapping the structure and thereby getting around data limitations." (Liu, 1960, p. 860)

Proximity

Given that structural equations will be mis-specified, the robustness of estimation procedures become a major consideration. Wold, with his concern for the properties of the least-squares estimators outside the classical linear model, is the first econometrician to make sensitivity and robustness a primary focus in determining the properties of fitted structural equations. Three years after Haavelmo pointed to the "least-squares bias" in interdependent (SEM) systems, Bentzel and Wold (1946) demonstrated the equivalence of maximum-likelihood and least-squares estimators and the absence of this bias when the SEM was recursive. They also began the exploration of temporal aggregation, defining as "the Malmqvist bias" the impact on estimators of an inadequate sampling interval. This latter interest foreshadowed the efforts of Engle and Liu (1972) and others to explore the impact of temporal aggregation on the properties of particular estimators and on appropriate specifications. It also motivated later discussions of distributed-lag models as discrete approximations to continuous differential-equation stochastic models. These were significant in the history of econometrics as temporal aggregation may remove the recursivity of causal chains and may introduce autocorrelated errors, which were given inadequate attention in the structural analyses of 1940-1960.

Wold (1948; 1949a, b) initiated the search for proximity theorems by econometricians. His efforts brought forth practical considerations of robustness and began the approach that led to the sterile interest in such theorems that we have used to characterize the turning point in about 1960. Proximity theorems begin with a presumption of mis-specification are within acceptable bounds. Sterility arises when the conditions cannot be linked to real situations facing econometricians. Wold (1953) and Wold and Faxér (1957) developed his earlier ideas. The latter define specification error as the error that "arises if the population is not correctly described in the assumptions that form the basis of the estimation method" which can be contrasted with sampling error.

The simplest proximity theorem of Wold can be illustrated with a model in which an endogenous variable y is linked to another variable x and a random equation error e. The asymptotic bias of the least-squares estimate for the coefficient of x will depend on the correlation of x and e, on the variance of x and the variance of e. When the last factor is small, the asymptotic bias may be negligible even if x and e are correlated. Wold also explored the impact of autocorrelated errors on least-squares estimates and looked at residual correlations for MA and AR processes. Indeed his procedure (1949b, p. 7) for residual analysis is precisely the one now followed by econometric students in using the ACF of least-squares residuals. This is structure-searching rather than structure-confirming.

Clearly the activities of Wold and Liu, as described in the last section, make visible the softness of structural estimation. Both indicate practical procedures that emphasize this softness. It is therefore reasonable to ask why econometricians continued, for at least two decades, to give insufficient attention to specification error. Perhaps we can find an answer to this question if we consider Fisher (1961) as representative of the mainstream response to the efforts of Wold and Liu at the end of our reference period. He recognizes several elements in Liu's critical comments and invokes Wold's use of proximity theorems and block recursive models as a defensive framework from which to address the critical comments. Somehow the strength of this defence removed the initial cause of concern from active consideration until the flood of criticism revived with the rational-expectations revolution.

Suppose we begin with Fisher's account of Liu's objectives. First, structural equations are underidentified since economic theory does not provide the zero constraints that are conventionally imposed to yield fitted "overidentified" structures. Second, the number of endogenous variables is generally understated so both the structural form and the reduced form are mis-specified. Clearly the identifiability criteria are misleading when variables are misclassified and when incomplete systems are treated as complete. These would seem to be devastating to the probability approach in particular and to structural estimation in general. Fisher (1961, pp. 34-35) turns to approximation as a means of weakening their impact. Thus the proper question is not, in his view, whether parameters are zero in fact but rather whether they are "in some sense sufficiently small" so the zero approximation is acceptable. Further the discontinuous choice between identified and underidentified specifications is misleading. The problem is rather "one of diminishing estimation inconsistency as the restrictions are better and better approximations". The solution in a large system is to use approximately correct restrictions and break this system into a block recursive pattern.

Then Liu's criticism of insufficient size is weakened. "Structural estimation is seen to be entirely possible in general, so that discussion and criticism must be directed toward the goodness or badness of the approximate assumptions in a particular case and not toward the truth or falsity thereof." The rival estimating methods for structural parameters will be inconsistent but, provided approximations are "good enough", such inconsistencies are negligible. Although many approximations may indeed not be good enough, this "must be decided on a case by case basis and no general a priori argument can be made to this effect".

Where does this argument leave structural estimation? There is a tremendous temptation for an individual researcher to concede the general thrust of Liu's comments yet to assume that, for his particular situation, approximations are sufficiently good. Fisher failed to provide guidance for determining the appropriateness of approximations or for measuring degrees of inconsistency. Although the large Brookings econometric model for the U.S. economy made reference to block recursivity, this notion is not a prominent feature of subsequent models. The blocks in these models have been determined by their manageability and by the availability of data rather than by a careful review of the approximation being employed. It is extremely difficult to find any discussion of specification error due to false constraints in the mainstream of applied econometrics within the last two decades. Given computational advances, there is really little excuse for the neglect of sensitivity experiments and other explorations of robustness. In sum, only part of Fisher's position has been assimilated. We have failed to provide the case-by-case assessment that is essential for the acceptability of proximity arguments in practical research. While Wold's investigation of specification error yields the routine inspection of the ACF for residuals, Fisher's arguments fail to stimulate equivalent concern for direct testing procedures. The final shortfall that we should note is present in our textbooks. How many of them described any methods of checking for the appropriateness of exclusion constraints? How many of them even raise the issue except as a curious historical events?

Purpose

We have already touched on the difficulty in interpretation of individual estimated parameters. This may not matter if such estimates are not of direct interest and we should turn to the general issue of how the purpose of a research project interacts with the specification of structural effects and the choice of estimating method. We have already seen how the use of a hypothetical experiment was used to justify the estimation of the SEM's structural form as a background for conditional forecasting. We have also seen Klein's recognition of the sensitivity of reduced form parameters to small changes in estimated structural parameters so that forecasts might better be based on the reduced form itself. Clearly if instability is a common attribute of these forms, there is difficulty in accepting either as an appropriate basis for forecasts, conditional or otherwise. Forty years ago, it was often expressed that the goal of econometrics was to achieve "reliable predictions", generally with a view to guidance of policy decisions. The common failure of this goal provided the fertile ground on which fell the seeds of the Lucas critique with its focus on structural instability. Surprisingly, we can still find evidence of the confusion between estimation and prediction. If the goal is prediction, why should asymptotic properties of estimates such as consistency and asymptotic efficiency have any relevance? There is no obvious link between these criteria and predictive success. Clearly there is a pressing need for econometricians to clarify their objectives, their research methods, and the connections between them.

In the reference period 1940-1960, there was discussion of "purpose" but this was limited and often obscured by other concerns. Waugh (1961) provides a refreshing break in this picture when he raises the question of 'what do we want to estimate?' and suggests that the answer may not be the parameters of theoretical economic structures. "So far, the main interest in the various methods of structural analysis seems to be in estimating the 'true theoretical relations', - for example, the 'true demand curve' for some commodity. This is legitimate, of course .... But econometrics should not limit its interest to pure theory. The econometricians should help the economist and the statistician to find practical answers to real economic problems." The background for Waugh's plea is again the probability approach to econometrics. Suppose we consider the following pair of equations

$$(18)\qquad \begin{aligned} y_{1t} &= a_1 y_{2t} + e_{1t} \\ y_{2t} &= a_2 y_{1t} + e_{2t} \end{aligned}$$

where e_{1t} and e_{2t} are independently-distributed normal variables. Haavelmo and other members of the group at the Cowles Commission put forward several simple structures of this type to show that each equation could not be interpreted as a conditional expectation in the sense that, for example, the expected value of y_{1t} given y_{2t} is not $a_1 y_{2t}$. They concluded from this that least-squares estimates would be biased for a_1 and a_2, the parameters of economic theory. These estimates may, however, be useful in illuminating $E(y_{1t}/y_{2t})$ as Waugh notes. The critical question is, as we already cited, one of the purpose for estimation. Haavelmo (1943a, p. 11) seems to have accepted this statement. "For prediction purposes the original equations of the system have no practical significance, they play only the role of theoretical tools by which to derive the prediction equations." Clearly there are many situations in which the parameters of the original (structural) equations have little value for the purpose at hand.

The original equations, themselves, are also a matter of choice. Strotz and Wold (1960, p. 417), in their comparison of recursive and nonrecursive systems, make this clear. "The first thing to consider when constructing an economic model is its purpose, that is, how it is to be applied in dealing with economic facts." There is no necessity to begin with the structural form of the SEM. That this should still need to be stated is a strong indictment of the evolution of econometrics during the last four decades. Perhaps we can detect a portmanteau attitude to econometric models. Given the availability of appropriate data and software, a model is constructed without any particular purpose in mind but with the awareness that, when fitted to the data, it may be used for a number of different purposes (some of which may not be evident prior to the completion of both specification and estimation stages). The structural form of the SEM is taken as a provisional basis since it is habitual to do so!

This portmanteau approach has been eroded as simulation became standard. The radical treatment of exogeneity by Klein and Young, which we noted earlier, is matched by revisions to criteria of acceptability and purpose in many macroeconometric models. Intially these arose as part of historical verification whereby the validity of a model was taken to depend on its ability to track the behaviour of the actual economic system during the period for which data are available. To enhance tracking qualities, individual estimates are often altered irrespective of their asymptotic properties. Naylor

(1971, p. 223) illustrates the background for these adjustments. "Econometric models which have been estimated properly and are based on sound economic theory may yield simulation results which are nonsensical. That is, the simulations may 'explode', and inherently positive variables may turn negative, leading to results which are in complete conflict with reality." The desire to provide a tracking performance is clearly the purpose of the adjustments.

This shift in emphasis can be taken further. We can introduce a qualitative criterion for the choice of estimation procedure and for specification. Optimality may then be defined as the choice by which the qualitative attributes of variables are replicated or simulative properties enhanced. This seems little different from the motives expressed in systems-dynamic modelling by Forrester and his associates. Their adjustment of tuning constants is the counterpart of con-adjustments and "tender loving care" as found in more conventional economy-wide econometric models. Perhaps SD models are econometric after all.

Normalization and Asymmetry

Much of economic theory is non-stochastic. This deterministic character creates difficulties for the interpretation of errors in econometric models. Since errors are generally not present in economic theory, there is ample room for alternative opinions concerning their nature. This uncertainty as to how stochasticity should be treated was expressed in several ways during the early phase of econometric history. (For example, Frisch advocated "orthogonal mean regression" and Schultz regularly calculated two estimates of demand elasticities by fitting dual regression equations.) The uncertainty persists today in the issue of normalization and in the confusion between regression and structural equations. On a practical level, it arises in economic research at an intermediate stage between mathematical modelling and fitting. At the first stage, the functional relations of mathematical analysis are manipulated to obtain equations that will be taken structural. The parameters of these equations are estimated in a final stage but only after a metamorphosis occurs. Somehow errors appear and their interaction with measured variables (determining exogeneity and endogeneity) is fixed. Some mathematical expressions are seldom unique, there is potential for alternative and mutually inconsistent specifications of statistical properties as errors are introduced. The possibility of alternative choices, not necessarily explicated by economic theory, is a major source of softness in econometrics and it severely impacts on the interpretation of structural estimation. In this final topic, we shall illustrate some problems by looking at the attempts of Wold and Frisch to overcome them. This topic is often described as the choice of regression or choice of "dependent" variable.

Frisch (1929) was clearly influenced by the mathematical symmetry of the Walrasian system of equations. Within this framework, there is no basis for choosing a particular variable in an equation as its dependent one. The least-squares principle for estimation minimizes the sum of squared residuals taken in a particular direction so some variables must be chosen prior to the use of this principle. Thus, within the Walrasian system, there is an inherent contradiction for such estimations. Frisch opted for "orthogonal mean regression" as a compromise choice but this, while not picking a dependent variable, is sensitive to measurement scales. It is just as arbitrary as the choices that it avoids. Ultimately, Frisch's suggestion can be traced through the efforts of his student Haavelmo to the probability approach to econometrics and the advocacy of maximum likelihood estimates that may not be affected by the choice of normalization rule.

Wold took a different line of approach. Instead of beginning with the equilibrium of the Walrasian system, he sought to combine cause-effect notions and conditional expectation as the bases for econometric modellings. These are essentially asymmetric and irreversible in contrast with the symmetry stressed by Frisch. They may also involve disequilibrium rather than equilibrium so that causal influences could be more easily explicated. Finally the treatment of equation errors is different with symmetric and asymmetric perspectives. With the former, errors could represent the collective impact of omitted factors and they are not identified with a dependent variable in a specific fashion. On the other hand, in the asymmetric variant, errors have a specific link with particular endogenous variables in the sense that the dependent variable in any given equation is fixed and thus identified with the error for that equation.

With an equation linking stochastic variables, there are many alternative regression forms that are amended as their chosen dependent variable is changed. Wold (1952, Ch. 2) notes the general awareness of this "duality problem" by statisticians. "From the very beginning, in the pioneering works of Galton, K. Pearson and Yule, this plurality has been stressed as a characteristic feature of regression analysis ... in contradiction to the functional relations of mathematical analysis." Wold argues that the symmetry among alternative regression lines is merely formal since the causal interpretation of regression in both experimental and non-experimental situations singles out the variable that should be taken as dependent. Sometimes this perspective can be extended to models involving instantaneous equilibrium as the following example illustrates. Suppose d, s and p denote demand, supply and price of some agricultural commodity. In equilibrium, d and s will be equal. Let their equilibrium value be denoted by q. These economic variables may be assumed to be connected by the system:

$$d_t = a_0 + a_1 p_t + e_{1t} \qquad \text{(demand)}$$

$$s_t = a_2 + a_3 p_{t-1} + e_{2t} \qquad \text{(supply)}$$

$$d_t = s_t = q_t \qquad \text{(equilibrium)}$$

Since p_t and e_{1t} are generally correlated, we can infer that $E(q_t/p_t)$ is not $(a_0 + a_1 p_t)$ as we discussed earlier. However the system could be expressed with

$$d_t = a_0 + a_1 p_t^* + e_{1t}^*$$

where p_t^* is $E(p_t/p_{t-1})$ and e_{1t}^* is the corresponding error. Then the conditional expectation $E(q_t/p_t^*)$ is $(a_0 + a_1 p_t^*)$. This example, due to Wold (1964a), is a little forced but it shows the feasibility of an asymmetric approach based on conditional expectation.

In this example, the revised error is obviously uncorrelated with p_t^*. This error is defined only by reference to the chosen dependent variable d_t. It clearly differs from the initial error which could be consistent with the symmetric treatment of price and quantity. We may, therefore, conclude that a given structure is compatible with different views of errors and with different views of measured variables. These lead to different optimal estimation procedures. The essential problem is that economic theory needs to be supplemented before we can choose among alternatives.

Some Concluding Comments

We have struggled through this long account of sensitive issues, revealing elements of softness in structural estimation. This preoccupation does not yield an unequivocal question to our implicit question concerning the demise of structural estimation. It is clear that our procedures may indeed preclude structural estimation due to the use of false constraints, the insufficiency of economic theory as a source of structural information, and the general difficulty of developing statistical models from deterministic ones. On the other hand, economists seem to want structural frameworks that can be linked, however tentatively, with the formal expressions of mathematical economics. We can say that the primacy of the probability approach has been weakened both by the routine behaviour and model builders (involving iterative search, simulation, and tender-loving-care adjustments) and by the developments that have occurred in statistical theory (such as the interest in robustness, the recognition of conditional specification, the move away from asymptotic criteria to finite-sample theory and a host of others). We hope that our use of historical references permits a better understanding of the present debates on structural estimation.

References

ANDERSON, T.W., and H. RUBIN (1949), "Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations", Annals of Mathematical Statistics, March, pp. 46-63.

ANDERSON, T.W., and H. RUBIN (1950), "The Asymptotic Properties of Estimates of the Parameters of a Single Equation in a Complete System of Stochastic Equations", Annals of Mathematical Statistics, December, pp. 570-582.

ANDO, A. (1981), "On a Theoretical and Empirical Basis of Macro-econometric Models", in KMENTA and RAMSEY (1981), pp. 329-367.

ARROW, K.J. (1959), Review of Klein's Textbook, Journal of the American Statistical Association, June, pp. 393-395.

ARROW, K.J. (1960), "The Work of Ragnar Frisch, Econometrician", Econometrica, Vol. 28, No. 2, April, pp. 175-192.

BENTZEL, R., and H.O.A. WOLD (1946), "On Statistical Demand Analysis From the Viewpoint of Simultaneous Equations", Skandinarisk Aktuarietidskrift, pp. 95-114.

BOX, G.E.P. (1979), "Strategy of Scientific Model Building", in (eds.) R.L. LAUNER and G.N. WILKINSON, Robustness in Statistics (New York: Academic Press).

BOX, G.E.P., and G.M. JENKINS (1970), Time Series Analysis, Forecasting and Control (San Francisco: Holden Day).

BRUNNER, K. (1981), "The Problem With What They Do and Why They Do It", in KMENTA and RAMSEY (1981), pp. 129-138.

CHOW, G.C. (1978), "Are Econometric Methods Useful for Forecasting?", Journal of Business, Vol. 51, No. 4, pp. 565-568.

CHOW, G.C. (1981), "Evaluation of Econometric Models by Decomposition and Aggregation", in KMENTA and RAMSEY, pp. 423-446.

CHRIST, C.F. (1956), "Aggregate Econometric Models", American Economic Review, Vol. 46, June. Reprinted in (eds.) R.A. GORDON and L.R. KLEIN (1966), Readings in Business Cycles (London: George Allen and Unwin), Ch. 17.

CLARK, J.M. (1947), "Mathematical Economists and Others: A Plea for Communicability", Econometrica, Vol. 15, No. 2, April, pp. 75-78.

DHRYMES, P.J. (1970), Econometrics: Statistical Foundations and Applications (New York: Harper and Row).

DOUGLAS, P.H. (1927), "Elasticity of Supply as a Determinant of Distribution", in (ed.) J. HOLLANDER, Economics Essays in Honor of John Bates Clark (New York: Macmillan).

DUBBELMAN, C., A.S. LOUTER and A.P.J. ABRAHAMSE (1978), "On Typical Characteristics of Economic Time Series and the Qualities of Five Autocorrelation Tests", Journal of Econometrics, Vol. 8, pp. 295-306.

DUTTA, M. (1975), Econometric Methods (Cincinnati: South-Western Publishers).

ENGLE, R.F., D.F. HENDRY and J.-F. RICHARD (1983), "Exogeneity", Econometrica, Vol. 51, No. 2, March, pp. 277-304.

ENGLE, R.F., and T.-C. LIU (1972), "Effects of Aggregation Over Time on Dynamic Characteristics of an Econometric Model", in (ed.) B.G. HICKMAN, Econometric Models of Cyclical Behavior, Vol. 2, Studies in Income and Wealth No. 36 (New York: Columbia University Press for NBER), pp. 673-737.

FISHER, F.M. (1961), "On the Cost of Approximate Specification in Simultaneous Equation Estimation", Econometrica, Vol. 29, No. 2, April. Reprinted in (eds.) A. ANDO, F.M. FISHER and H.A. SIMON (1963). Essays on the Structure of Social Science Models (Cambridge, Mass.: The MIT press), pp. 32-63.

FORRESTER, J.W. (1980), "Information Sources for Modeling the National Economy", Journal of the American Statistical Association, Vol. 75, No. 371, September, pp. 555-566.

FRISCH, R. (1929), "Correlation and Scatter in Statistical Variables", Nordisk Statstisk Tidskrift, Vol. 8, p. 36. Revised as Memorandum fra Sosialøkonomisk Institutt, Universitetet: Oslo, May 1951.

FRISCH, R. (1934), Statistical Confluence Analysis By Means of Complete Regression Systems, University of Economics, Oslo.

GEORGESCU-ROEGEN, N. (1952), Comment following ORCUTT (1952), pp. 206-211.

GIRSHICK, M.A., and T. HAAVELMO (1947), "Statistical Analysis of the Demand for Food: Examples of Simultaneous Estimation of Structural Equations", Econometrica, Vol. 15, No. 2, April, pp. 79-110.

GOLDBERGER, A.S. (1959), Impact Multipliers and Dynamic Properties of the Klein-Goldberger Model (Amsterdam: North-Holland), Ch. 6.

GRANGER, C.W.J. (1966), "The Typical Spectral Shape of an Economic Variable", Econometica, Vol. 34, No. 1, January, pp. 150-161

GRANGER, C.W.J. (1969), "Investigating Causal Relations by Econometric Models and Cross-Spectral Methods", Econometrica, Vol. 37, pp. 424-438.

GRANGER, C.W.J. (1981), "The Comparison of Time Series and Econometric Forecasting Strategies", in KMENTA and RAMSEY (1981), pp. 123-128.

HAAVELMO, T. (1940), "The Inadequacy of Testing Dynamic Theory by Comparing Theoretical Solutions and Observed Cycles", Econometrica, Vol. 8, No. 4, October, pp. 312-321.

HAAVELMO, T. (1943a), "The Statistical Implications of a System of Simultaneous Equations", Econometrica, Vol. 11, No. 1, January, pp. 1-12.

HAAVELMO, T. (1943b), "Statistical Testing of Business-Cycle Theories", The Review of Economic Statistics, Vol. 25, No. 1, February, pp. 13-18.

HAAVELMO, T. (1944), The Probability Approach in Econometrics, Supplement to Econometrica, Vol. 12, July.

HAAVELMO, T. (1958), "The Role of the Econometrician in the Advancement of Economic Theory", Econometrica, Vol. 26, No. 3, July, pp. 351-357.

HEISE, D.R. (1975), Causal Analysis (New York: Wiley).

HOOD, W.C., and T.C. KOOPMANS (eds.) (1953), Studies in Econometric Method (New York: Wiley).

HOWREY, E.P., L.R. KLEIN and M.D. McCARTHY (1974), "Notes on Testing the Predictive Performance of Econometric Models", International Economic Review, Vol. 15, No. 2, June, pp. 366-383.

HURWICZ, L. (1944), "Stochastic Models of Economic Fluctuations", Econometrica, Vol. 12, No. 2, April, pp. 114-124.

JUDGE, G.G., and M.E. BOCK (1978), The Statistical Implications of Pre-Test and Stein-Rule Estimators in Econometrics (Amsterdam: North-Holland).

KENNY, D.A. (1979), Correlation and Causality (New York: Wiley).

KELEJIAN, H.H., and W.E. OATES (1974), Introduction to Econometrics: Principles and Applications (New York: Harper and Row).

KELEJIAN, H.H., and B. VAVRICHEK (1981), "An Evaluation of the Forecasting Performance of Macro Economic Models, With Special Emphasis on Model Size", in KMENTA and RAMSEY (1981), pp. 93-122.

KLEIN, L.R. (1947), "The Use of Econometric Models as a Guide to Economic Policy", Econometrica, Vol. 15, No. 2, April, pp. 111-139.

KLEIN, L.R. (1953), A Textbook of Econometrics (Evanston, Illinois: Row, Peterson).

KLEIN, L.R. (1960a), "The Efficiency of Estimation in Econometric Models", in (ed.) R.W. PFOUTS, Essays in Economics and Econometrics (Chapel Hill: University of North Carolina Press), pp. 216-232.

KLEIN, L.R. (1960b), "Single Equation vs. Equation System Methods of Estimation in Econometrics", Econometrica, Vol. 28, No. 4, October, pp. 866-871.

KLEIN, L.R. (1964), "Problems in the Estimation of Interdependent Systems", Model Building in the Human Sciences, Entretiens de Monaco en Sciences Humaines (Monaco: Editions 'Sciences Humaines'), pp. 51-87.

KLEIN, L.R. (1971a), "Forecasting and Policy Evaluation Using Large Scale Econometric Models: The State of the Art", in (ed.) M.D. INTRILIGATOR, Frontiers of Quantitative Economics (Amsterdam: North-Holland), Ch. 4.

KLEIN, L.R. (1971b), "Whither Econometrics?", Journal of the American Statistical Association, Vol. 66, No. 334, June, pp. 415-421.

KLEIN, L.R. (1982), "The Scholarly Foundations of the Econometrics Industry", in (eds.) S.H. HYMANS, Economics and the World Around It (Ann Arbor: The University of Michigan Press), pp. 111-122.

KLEIN, L.R., and R.M. YOUNG (1980), An Introduction to Economic Forecasting and Forecasting Models (Lexington, Mass.: Lexington Books).

KMENTA, J., and J.B. RAMSEY (1981), Large-Scale Macro-Econometric Models (Amsterdam: North-Holland).

KOOPMANS, T.C. (1945), "Statistical Estimation of Simultaneous Economic Relations", Journal of the American Statistical Association, Vol. 40, December, pp. 448-466.

KOOPMANS, T.C. (1947), "Measurement Without Theory", Review of Economic Statistics, Vol. 29, No. 3, August, pp. 161-172.

KOOPMANS, T.C. (1949), "Identification Problems in Economic Model Construction", Econometrica, Vol. 17, No. 2, April, pp. 125-144.

KOOPMANS, T.C. (ed.) (1950a), Statistical Inference in Dynamic Economic Models (New York: Wiley).

KOOPMANS, T.C. (1950b), "When is an Equation System Complete for Statistical Purposes?", in KOOPMANS (1950a), pp. 393-409.

KOOPMANS, T.C., H. RUBIN and R.B. LEIPNIK (1950), "Measuring the Equation Systems of Dynamic Economics", in KOOPMANS (1950a), pp. 53-237.

LEAMER, E.E. (1983), "Let's Take the Con Out of Econometrics", American Economic Review, Vol. 73, No. 1, March, pp. 31-44.

LIU, T.-C. (1955), "A Simple Forecasting Model for the U.S. Economy", IMF Staff Papers, Vol. 4 (3), pp. 434-466.

LIU, T.-C. (1960), "Underidentification, Structural Estimation, and Forecasting", Econometrica, Vol. 28, No. 4, October, pp. 855-865.

LUCAS, R.E. (1976), "Econometric Policy Evaluation: A Critique", Journal of Monetary Economics, Vol. 2, Supplement, pp. 19-46.

LUCAS, R.E. (1977), "Understanding Business Cycles", Journal of Monetary Economics, Carnegie-Rochester Conference Series, Vol. 5.

LUCAS, R.E., and T.J. SARGENT (1979), "After Keynesian Macro-economics", Federal Reserve Bank of Minneapolis Quarterly Review, Vol. 3, No. 2.

MALINVAUD, E. (1966), Statistical Methods of Econometrics (Amsterdam: North-Holland).

MALINVAUD, E. (1981), "Econometrics Faced With The Needs of Macroeconomic Policy", Econometrica, Vol. 19, No. 6, November, pp. 1363-1375.

MANN, H.B., and A. WALD (1943a), "On the Statistical Treatment of Linear Stochastic Difference Equations", Econometrica, Vol. 11, Nos. 3/4, July-October, pp. 173-220.

MANN, H.B., and A. WALD (1943b), "On Stochastic Limit and Order Relationships", Annals of Mathematical Statistics, Vol. 14, pp. 217-226.

MARSCHAK, J. (1948), "Abstract. Statistical Inference From Non-Experimental Observations" (Washington Meeting, Sept. 1947), Econometrica, Vol. 16, No. 1, January, pp. 53-55.

MARSCHAK, J. (1950), "Statistical Inference in Economics: An Introduction", in KOOPMANS (1950a), pp. 1-50.

MARSCHAK, J. (1953), "Economic Measurements for Policy and Prediction", in HOOD and KOOPMANS (1953), pp. 1-26.

MILLER, P.J. (1978), "Forecasting With Econometric Methods: A Comment", Journal of Business, Vol. 51, No. 4, pp. 579-584.

MISHKIN, F.S. (1979), "Simulation Methodology in Macroeconomics: An Innovation Technique", Journal of Political Economy, Vol. 87, No. 4, pp. 816-836.

MITCHELL, W.C. (1951), What Happens During Business Cycles: A Progress Report, Studies in Business Cycles No. 5 (New York: NBER).

MOORE, H.L. (1914), Economic Cycles: Their Law and Cause (New York).

NAYLOR, T.H. (1971), "Policy Simulation Experiments with Macro-econometric Models: The State of the Art", in (ed.) M.D. INTRILIGATOR, Frontiers of Quantitative Economics (Amsterdam: North-Holland), Ch. 6.

NELSON, C.R., and H. KANG (1981), "Spurious Periodicity in Inappropriately Detrended Time Series", Econometrica, Vol. 49, pp. 741-751.

NELSON, C.R., and H. KANG (1984), "Pitfalls in the Use of Time as an Explanatory Variable in Regression", Journal of Business and Economic Statistics, Vol. 2, No. 1, January, pp73-

NERLOVE, M. (1972), "On the Structure of Serial Dependence in Some U.S. Price Series", in (ed.) O. ECKSTEIN, The Econometrics of Price Determination (Washington, D.C.: The Board of Governors of the Federal Reserve System), pp. 60-112.

NERLOVE, M., D.M. GRETHER and J.L. CARVALHO (1979), Analysis of Economic Time Series: A Synthesis (New York: Academic Press).

ORCUTT, G.H. (1952), "Towards Partial Redirection of Econometrics", Review of Economics and Statistics, Vol. 34, No. 3, August, pp. 195-213.

QUENOUILLE, M.H. (1957), The Analysis of Multiple Time Series (London: Charles Griffin).

REVANKAR, N.S., and M.J. HARTLEY (1973), "An Independence Test and Conditional Unbiased Prediction in the Context of Simultaneous Equations Systems", International Economic Reviw, October, pp. 625-631.

ROWLEY, R., and R. JAIN (1984), "Sims on Causality: An Illustrarion of Soft Econometrics", Working Paper 1/84, Department of Economics, McGill University.

SARGENT, T.J. (1979), "Estimating Vector Autoregressions Using Methods not Based on Explicit Economic Theories", Federal Reserve Bank of Minneapolis Quarterly Review, Vol. 3, No. 3, Summer, pp. 8-14.

SARGENT, T.J. (1981), "Interpreting Economic Time Series", Journal of Political Economy, Vol. 89, No. 2, April, pp. 213-248.

SARGENT, T.J. (1982), "Beyond Demand and Supply Curves in Macroeconomics", American Economic Review, Papers and Proceedings, Vol. 72, No. 2, May, pp. 382-389.

SARGENT, T.J., and C.A. SIMS (1977), "Business Cycle Modeling Without Pretending to Have Too Much A Priori Economic Theory", New Methods in Business Cycle Research: Proceedings From a Conference (Federal Bank of Minneapolis).

SCHULTZ, H. (1938), The Theory and Measurement of Demand (Chicago: The University of Chicago Presss), Ch. II.

SIMON, H.A. (1953), "Causal Ordering and Identifiability", in HOOD and KOOPMANS (1953), Ch. 3.

SIMS, C.A. (1972a), "Money, Income and Causality", American Economic Review, Vol. 62, pp. 540-552.

SIMS, C.A. (1972b), "Are There Exogenous Variables in Short Run Production Relations", Annals of Economic and Social Measurement, Vol. 1, January, pp. 17-36.

SIMS, C.A. (1981), "An Autoregressive Index Model for the U.S., 1948-1975", in KMENTA and RAMSEY (1981), pp. 283-327.

SIMS, C.A. (1982), "Policy Analysis With Econometric Models", Brookings Papers on Economic Activity, No. 1, pp. 107-152.

SMITH, V.L. (1982), "Microeconomics Systems as an Experimental Science", The American Economic Review, Vol. 72, No. 5, December, pp. 923-955.

STROTZ, R.H., and H.O.A. WOLD (1960), "Recursive vs. Nonrecursive Systems: An Attempt at Synthesis", Econometrica, Vol. 28, No. 2, April, pp. 417-427.

THEIL, H. (1978), Introduction to Econometrics (Englewood Cliffs, N.J.: Prentice-Hall).

THUROW, L. (1983), Dangerous Currents: The State of Econometrics (New York: Random House).

TINBERGEN, J. (1939), Statistical Testing of Business-Cycle Theories. Vol. II: Business Cycles in the United States of America, 1919-1932 (Geneva: League of Nations, Intelligence Services).

TINBERGEN, J. (1940), "Econometric Business Cycle Research", Review of Economic Studies, Vol. 7, pp. 73-90. Reprinted in (eds.) G. HABERLER et al. (1951), Readings in Business Cycle Theory (Philadelphia: Blakiston), pp. 61-86.

TINBERGEN, J. (1952), Comment following ORCUTT (1952), pp. 205-206.

TINTNER, G. (1952), Econometrics (New York: Wiley).

TINTNER, G. (1968), Methodology of Mathematical Economics and Econometrics (Chicago: University of Chicago Press).

TOBIN, J. (1981), "Comment on Albert Ando 'On a Theoretical and Empirical Basis of Macroeconometric Models' ", in KMENTA and RAMSEY (1981), pp. 391-392.

WALD, A. (1950), "Remarks on the Estimation of Unknown Parameters in an Incomplete System of Equations", in KOOPMANS (1950a), pp. 305-310.

WAUGH, F.V. (1961), "The Place of Least Squares in Econometrics", Econometrica, Vol. 29, No. 3, July, pp. 386-396.

WOLD, H.O.A. (1948), "Abstract Report of the Washington Meeting, September 6-18, 1947", Econometrica, Vol. 16, pp. 33-36.

WOLD, H.O.A. (1949a), "Statistical Estimation of Economic Relationships", Econometrica, Vol. 17, Supplement, pp. 1-22.

WOLD, H.O.A. (1949b), "On Least Squares Regression With Auto-correlated Variables and Residuals", Transactions of the International Institute of Statistics, Session at Berne, September, pp. 1-13.

WOLD, H.O.A. (1952), Demand Analysis: A Study in Econometrics (New York: Wiley).

WOLD, H.O.A. (1953), Demand Analysis (New York: Wiley), Ch. 2.

WOLD, H.O.A. (1954), "Causality and Econometrics", Econometrica, Vol. 22, No. 2, April, pp. 162-177.

WOLD, H.O.A. (1964a), "The Approach of Model Building: Crossroads of Probability Theory, Statistics, and Theory of Knowledge", Model Building in the Human Sciences, Entretiens de Monaco en Sciences Humaines (Monaco: Editions 'Sciences Humaines'), pp. 1-38.

WOLD, H.O.A. (1964b), "On the Definition and Meaning of Causal Concepts", as WOLD (1964a).

WOLD, H.O.A. (1964c), "Forecasting with the Chain Principle", in (ed.) H.O.A. WOLD, Econometric Model Building: Essays on the Causal Chain Approach (Amsterdam: North-Holland), pp. 5-36.

WOLD, H.O.A., and P. FAXER (1957), "On the Specification Error in Regression Analysis", Annals of Mathematical Statistics, Vol. 28, No. 1, March. pp. 265-267.

WORKING, E.J. (1927), "What Do Statistical 'Demand Curves' show?", Quarterly Journal of Economics, Vol. 41, pp. 212-235.

WU, D.-M. (1973), "Alternative Tests of Independence Between Stochastic Regressors and Disturbances", Econometrica, pp. 733-750.

ZELLNER, A. (1975), "Time Series Analysis and Econometric Model Construction", in (ed.) R.P. GUPTA, Applied Statistics (Amsterdam: North Holland), pp. 373-398.

ZELLNER, A., and F. PALM (1974), "Time Series Analysis and Simultaneous Equation Econometric Models", Journal of Econometrics, Vol. 2, No. 1, May, pp. 17-54.

CHAPTER FOUR

FALLIBLE INDICATORS

Over a hundred years ago, Francis A. Walker suggested that "[The] country is hungry for information; everything of a statistical character, or even of a statistical appearance is taken up with an eagerness that is almost pathetic; the community have not yet learned to be half skeptical and critical enough in respect to such statements". This comment is clearly as relevant now as it was in 1873. Consider recent developments in the U.S. and Canada. During the last few years, the U.S. census for 1980 has been acknowledged to undercount 1.3 million blacks (actually a substantial improvement over its performance a decade earlier), the C.D. Howe Research Institute has accused Statistics Canada of consistently underestimating the rate of growth and thus caused the Federal Department of Finance to develop inaccurate projections, the Chairman of the Joint Economic Committee of the U.S. Congress has pointed to a "potential calamity for the Federal Statistical System" due to budget cuts that will undermine data quality (with delays in data collection, reductions in sample sizes and elimination of some vital sources), and a study for the Economic Council of Canada has argued that the conventional definition of productivity should be discarded even though the Council has been a leading advocate of this definition for about two decades. In addition, we can identify major changes in the focus and measurement of both monetary aggregates and labour-market variables following influential reports by the Bach Advisory Committee on Monetary Statistics and the National Commission on Employment and Unemployment Statistics in the U.S. With this backdrop, we would expect a marked change from the situation described by Walker. Yet despite the existence of substantial grounds for severe scepticism concerning the accuracy and interpretation of many economic indicators, we see the monthly releases of data treated as major news items without clear statements of appropriate qualifications. Even substantial revisions of published data from governmental agencies have failed to shatter the persistent habit of treating economic indicators as infallible. This acceptance occurs despite the transformation of economic theory to include new concepts associated with qualifying names such as "natural", "underlying" and "permanent". Each new concept involves, in principle, amendments to conventional definitions of economic indicators.

The overall impression is of a curious mixture of complacency and potential change due to collection failures, shifts in theoretical emphases and political pressures. The impact of fallible indicators for econometrics is multi-dimensional affecting specification, biases, generalizability of fitted regression lines, influential statistics, structural instabilities and a host of other complications. These imply a considerable degree of inherent softness in econometrics both because of this awkward environment and because of the failures of econometricians to give adequate attention to issues of measurement.

Clearly the extent of such issues is too comprehensive for us to cover all of them. We settle for some significant ones in the seven sections of this chapter that follow. We begin with the simple requirement that the origins of many economic indicators in sample surveys have to be explicitly recognized so that their stochastic consequences can be dealt with in estimation, prediction and interpretation of empirical fits. The second section focuses attention on environmental transformations due to financial innovation and labour-market developments. We touch briefly on adjustments to monetary aggregates and reweighted unemployment rates as well as on the recent debates concerning the instability of fitted money-demand relationships. The third section looks at data preparation and prior adjustments giving attention to the Yule-Slutsky effect (which is linking to our earlier discussion of spectral approaches to long waves in economic activity), seasonal adjustment, and data as an intermediate good. This is followed by a brief section on the issue of revisions in measurements with monetary targets and seasonal adjustment as particular illustrations.

The remaining three sections consider temporal intervals and aggregation, the search for data on duration in economic states (such as unemployment), and the softness of economic concepts themselves as bases for measurement. The first of these focuses attention on the major difficulty that stems from the failure of economists to establish the fundamental time intervals for discrete models. We can have little confidence in the assumption that sampling intervals for data will be appropriate ones for theoretical economic models. Temporal aggregation is a major form of measurement error and can be linked to biased estimation, autocorrelated errors in regression models, and even sign reversals for parameters.

Our brief treatment of duration data is included both as a reminder that the common preoccupation with flows is unduly restrictive and as a means of indicating how supplemental probabilistic models have been introduced to assist modifications to "dynamic" indicators based on cross-section data (such as are obtained from the Canadian Labour Force Survey). The final section, dealing with soft concepts in economics, picks up some of the issues raised in other sections. It attempts to show how much of the softness in econometrics stems from the insufficient attention given by economists to making their theoretical notions operational. In part, this is due to the vagueness of these notions and to the deterministic and individualistic format of many theoretical models. The chapter ends with a few concluding comments linking measurement and soft econometrics.

Indicators as Statistics

We have often stressed the problem of reconciling the mathematical expressions of economic theory with an appropriate statistical framework. The choice of measurements or indicators to represent theoretical variables is a major problem facing economists in this process of reconciliation. Sociologists, psychologists and some other social scientists have at least addressed the distinction between indicators and theoretical constructs in a systematic way. Economists, however, have until recently been much less attentive to it. They have also neglected to recognize the straightforward origins of measurements for major variables (such as prices, unemployment and incomes, for example) in sample surveys. We intend to begin our treatment of fallible indicators by briefly looking at some aspects of this source for measurements and by considering the consequences for econometricians who use regression.

Most courses in econometrics within our universities involve a series of ideal models. These include the classical linear model, the generalized linear model due to Aitken, the simultaneous-equations model of the Cowles Commission, time-series models, and some variants with distributed lags and unknown autoregressive parameters. This list might be supplemented with stochastic parameters, nested errors and similar modifications. None of these items, however, recognize measurement errors or consider the intrinsic characteristics of indicators.

In fact, many students in econometrics can graduate without ever meeting problems of measurement. Others will only meet measurement error in the context of Friedman's use of permanent income in consumption functions or in the context of principal components. Even then the error will be attributed with white noise properties or with normality because of the tractability of such assumptions. The character of error will not be linked with the origin of measurements. There are two major dimensions of this neglect. First, the convention of treating explanatory variables in regression as fixed or exogenous without significant measurement error is continued even when the quality of data is poor, the theoretical construct is imprecise, and the level of aggregation is unsuitable. Second, economists have become accustomed to making "instrumental assumptions" rather than realistic ones that could stem from an explicit investigation of the potential source, form and correlative properties of measurement errors. These deficiencies affect the softness of applied econometric modelling even if they remain under-recognized. They have visible consequences in the erratic shifts of refitted regression lines, incorrect signs or magnitudes for estimated parameters, and unacceptable features in residuals that appear during the stage of criticism.

To look at aspects of the sample sources for measurement, we can consider the Labour Force Survey (LFS) and some elements in the production of data for the consumer price index and for summaries of the size distribution of income. These illustrations will be focused here on the "small area" problem, the issues of rotation bias and response adjustments, as well as the feasibility of applying "super-population" theory. Such matters are frequently raised by governmental statisticians but their journals, such as Survey Methodology which is produced by Statistics Canada, attract only a narrow audience and are hardly noticed by most econometricians.

The small-area problem arises when estimates (data) are required from sample surveys for domains whose boundaries do not coincide with those of design strata. Several types of domains have been identified. "Planned" domains are those for which separate samples have been planned, designed and selected. "Characteristic" domains cut across sample units as with age-sex, occupation and industry domains. Finally, "unplanned" domains are those that are not distinguished at the time of the sample design. These could include federal electoral districts, census divisions or manpower planning regions rather than Canada as a whole or provinces. When measurements to be used in fitted regression line are derived from a particular survey, these inherent domains affect the "domain of applicability" for the regression line. They must be acknowledged in determining the range of conclusions, predictions and general uses of empirical evidence. Constraints stem from the data extremes for explanatory variables and from the survey domains.

Unemployment rates and measures of labour force participation based on responses to the LFS are valid replicates of population rates and measures provided they correspond to accurately planned domains or to characteristic domains with sufficient coverage in the sample. They are generally invalid for non-planned domains. Thus we ought to be sceptical of disaggregated unemployment rates, for example, for sub-populations that were not explicitly recognized in the design strata. Their sample properties may be inappropriate.

Consideration of the domain of measurements is also relevant in another situation; namely, that sometimes associated with "contextual variables". Suppose the dependent variable in a wage equation is based on wage contracts or on familiar series for earnings. Then, within the conventional choices for the Phillips curve, there is a tendency to include a measure of unemployment as an explanatory variable. This measure will stem from the LFS but its reference population will be different from that of the dependent variable, which is not derived from the LFS. Thus the unemployment rate for an overall group or for prime-age males in the labour force might be associated with wage rates for part of the unionized labour force. When the reference population for the measurements of an explanatory variable is more comprehensive than that for the dependent variable, the former is termed a contextual variable. The hazards of this situation seem clear (affecting measures for prices and productivity as well as for unemployment) when described in this way. Unfortunately such descriptions are notably absent from the wage-equation literature, which suggests that many empirical findings may be affected by some variant of small-area bias.

Turning to the second illustration, we focus on rotation bias and response adjustments. These depend upon a revolving series of samples and upon differential responses between waves in these samples or between sub-populations. Again the LFS provides a suitable example. A second example can be drawn from the Surveys of Consumer Finances (SCF), which are our primary source of information on family incomes outside the censuses. The LFS sample follows a rotation scheme that permits replacement of one-sixth of the households in the sample each month. Any panel or wave remains in the sample for six consecutive months. It is well known among survey statisticians that rotation designs may lead to the estimates (data) from different rotation groups having different expected values. Bailar (1975) and Ghangurde (1982) provide a more comprehensive account of this phenomenon. Several factors can be identified for the attendant "rotation group bias". For the LFS, non-response rates for households vary with their time within the sample. Further, non-respondent households have different average characteristics as compared to respondent households. (Non-respondents have markedly higher levels of employment, for example.) Thus the reconciliation of sample statistics with population characteristics might involve adjustments with variable scaling factors. Deficiencies in adjustments leave errors that are far from white noise and that are linked with the characteristics of households so as to cause potential inconsistencies for parametric estimates in regression models using data from the LFS. There are hidden interactions among variables.

The incidence of scaling adjustments for differential responses and differing group characteristics is often limited. In the SCF, for example, scales permit overall totals for various components of income to be checked with data from National Accounts. The scales are also applied at intermediate levels but the SCF samples remain adversely affected by poorer responses on investment income and transfers than on earnings. Measurement error is therefore dependent on the composition of income. This means that quantile regression models and equations linking Gini-coefficients with qualitative characteristics of families are both susceptible to problems of measurement due to the form of sample responses. It is surprising that this softness has not received the same attention as that accorded to the impact of grouping data with the use of approximating formulae in calculating indices.

The final illustration is concerned with attempts to find better means of describing the statistical properties of aggregate indicators from sample origins. Wilkerson (1967) argued for treating the U.S. price index as a sample statistic affected by the design and response patterns of its originating survey. However, his suggestion received little attention.

Recently, it was revived by Kott (1984) who surveyed similar (theoretical) attempts to determine sampling biases for price indices from the viewpoint of the superpopulation approach due to Godambe (1955). Price trends (and many other economic variables) are not random elements that can be interpreted as stemming from a fixed and stationary population. Kott (p. 89) concedes that "they are a finite group of variables dependent on a variety of economic factors that do not affect all units uniformly" but suggests the feasibility and desirability of developing statistical properties of economic indices from superpopulation theory and collections of reasonable assumptions. It is premature to expect much from this methodology but at least its existence will remind us that economic indicators have statistical properties affecting their use and implying softness in the interpretation of regression lines that have been fitted to data from sample survey sources.

Environmental Transformations

In the discussion of long waves in Chapter Two, we noted the stress attached to financial innovation and crises by Minsky. This is one aspect of environmental transformations that modify the structural frameworks with which economic behaviour can be described. We hope to use financial innovation and unemployment as two illustrative areas to clarify the impact of environmental transformations on measurement. Within financial innovation, we shall touch upon the changing nature of monetary aggregates, the instability of money-demand relationships as as well as the literature associated with Minsky's discussions of financial fragility in the U.S. economy. With respect to unemployment, we look at attempts to maintain intertemporal comparability of unemployment rates when pronounced structural changes have modified the labour market. These changes are associated with increased labour-force participation of married women, baby booms of earlier years, and the rise of part-time employment. They have been linked to advocacy of weighted rates. We could also have focused attention on the structural shifts identified with changes in governmental policies (such as in the unemployment insurance programmes) but eventually we chose to ignore these. They would not have introduced any new problems of softness that are not already present in the socio-economic and demographic changes that we shall actually consider. Our discussion is reasonably brief with some arguments concerning measurement of monetary aggregates and unemployment left to our later section dealing with soft concepts.

Financial innovations create many problems at the macroeconomic level. Mayer (1982), for example, notes two of these. They create confusion about the measurement of money during the periods in which the innovations occur. They also create the possibility that money stock will be destabilizing. Before discussing the question of confusion of measurement, the second problem might be considered since it is closer to Minsky's concerns with crises and fragility. This "loose cargo" argument for destabilization is based on the presumption that financial innovations, often responses to regulatory efforts, increase liquidity by creating financial assets with near-money properties and thus accommodate spending. The availability of credit is transformed, transaction costs of switching among near-monies is reduced, and the overall level of money is more difficult to control as a governmental policy. There occurs a curious mixture of increased efficiency, quicker adjustments and incipient instability. As a whole, as Mayer points out, the loose-cargo argument may be insufficient to justify restricting financial innovation. It needs to be enhanced with credit crunches before financial crises can arise to provide a potential for the pronounced decline of the long-wave collapse from its cyclical (or S-curve) peak. Such crunches in recent U.S. history have been described by Minsky and by Wojnilower (1980). In their view, institutional changes are part of a process that has greatly intensified the propensity of the U.S. economy to excessive credit expansion and that may lead to more serious crises of endogenous bankruptcy. For econometrics, the financial-instability hypotheses of Minsky and Wojnilower imply pronounced structural breaks following significant innovations. The time-series character of some economic indicators might, therefore, be better represented by the interrupted model of intervention analysis or by threshold autoregressions rather than the conventional models which assume stationarity or orderly movement between successive regimes.

Even if crises are an exaggeration, it is clear that financial innovations affect both monetary controls and targetting. Hester (1981, 1982) and Judd and Scadding (1982) clarify developments in this respect while Simpson (1984) anticipates future impacts with recent events treated as part of a period of transition. Many of the issues here go much beyond our immediate concern with softness in econometrics so we shall not pursue them. However, there are some direct connections with econometrics since the operation of controls and the choice of targets are generally based on what are considered to be stable empirical relationships. Thus the recent failure to find money-demand equations with sufficient intertemporal stability is disturbing. The impact of this failure is enhanced by the search for revisions of monetary aggregates that led to the establishment of the Bach Advisory Committee by the Board of Governors of the Federal Reserve System in 1974 and has continued without any sufficient diminuition of effort to the present. Of somewhat less relevance are the attendant modifications to econometric components in models for designing optimal monetary controls. Hester (1981) demonstrates some of these. They include stochastic parameters (perhaps with endogenous drift), vector difference equations and rational expectations.

Returning to measurement itself, financial innovation has made a wider class of indicators available for use. The definitions accepted by the Federal Reserve System have generally stayed within simple sums of conventional aggregates when introducing M-1A and M-1B measures in 1976 and when discarding them for replacements two years later. This has not precluded interest in more complex modifications, most notably as Divisia or Superlative aggregates. Cockerline and Murray (1981a, b) compare these alternatives for Canada while the special studies papers of the Division of Research and Statistics at the Federal Reserve Board are a major source of comparisons and other information for the U.S. (Our list of references cites some of these and gives appropriate access to the efforts of Barnett and Spindt.) The essential difference in calculations of Divisia and conventional simple-sum indicators is the use of weights for sub-aggregates. Divisia components are multiplied by measures of "moneyness", usually based on differences between interest rates. Since we have only vague notions of how "moneyness" should be measured, the resulting aggregate indicators are essentially soft. Econometric models that embody them are, therefore, to be subject to careful interpretation. Errors of measurement are inevitable and, if they stem from inaccurate weights, must depend upon the relative size of components. They cannot be assumed white noise. Excellent accounts of Divisia weights are provided by Barnett (1983a, b; 1984) and in the references that he gives. From these, it is clear that the choice of an "incorrect" monetary indicator in a regression model cannot be adequately offset by adjustments to constants or to estimated slope parameters. This awkward situation prevails whether innovation procedes smoothly or is clustered. Biases are pervasive. These might be revealed by poor tracking properties of fitted regression lines and by the instability of estimated parameters when fitted to different time periods. Thus the problem of measuring aggregate monetary indicators is not distinct from that associated with unstable money-demand equations.

This instability issue is summarized by Judd and Scadding (1982) and by Tinsley, Garrett and Friar (1978). An international perspective is provided by Blundell-Wignall et al. (1984). The fundamental need for stable money-demand relationships in advocacy of monetarist policies is clear. It was, therefore, very unsettling for monetarists when empirical evidence for these relationships revealed dramatic instability just as monetarist perspectives became fashionable. Several responses to this situation were possible. The instability could be attributed to the excessive restrictions of the classical linear model (implying need for stochastic parameters or switches in regression regimes fixed by changes in external influences) or to inadequate measurement of aggregate monetary indicators (and, hence, experimentation with alternative measures until stability is found). The latter response provides a new approach in econometrics and, perhaps, a disturbing one for conventional inferential procedures. Instead of confirming an existing relationship for known variables, we are being asked to believe in its stability and to determine the choice of an indicator by the stability of equations within which it is to be embedded. Softness arises from the choice of criteria to assess a reasonable degree of stability and, also, from doubt concerning the initial belief in stability itself.

Turning to our second area of illustration, we find similar questions arising. Thus we can locate demands for reweighted unemployment rates and insistent claims that the instability of fitted Phillips' curves should be attributed to incorrect choices of measures for unemployment. The former reflect concerns that the transformations of the labour market (due to changes in the relative supplies of different "qualities" of labour) mean that the estimated unemployment rates for different time periods are not comparable. We are, therefore, required to find suitable adjustments by which we can determine current equivalents for earlier values of unemployment rates. The weights for the adjustments might be based on demographic information so that a time-series is linked with variable corrections. Alternatively, we can standardize recent rates to hypothetical equivalents assuming non-changing populations since an initial period. Attempts to deal with such matters are provided by Perry (1970, 1971), Flaim (1979), Cain (1979) and Antos et al. (1979), while a variant stressing the impact governmental policies rather than demographic shifts is revealed in Clarkson and Meiners (1977, 1979). The consequences of these environmental transformations for econometrics are essentially the same as we have noted for financial innovation. Softness is associated with criteria for choice, methodological stances and specification of statistical frameworks.

Data Preparation and Prior Adjustment

So far we have drawn attention to the problems of measurement stemming from the origins of data in sample surveys and from environmental transformations. In most respects, the users of economic data find these problems unavoidable although they may ignore them or misunderstood and mis-represent them. Data preparation and prior adjustment, on the other hand, are frequently the consequences of explicit choices by researchers. They involve conscious decisions to smooth data with graduation formulae or seasonal adjustment software such as X11-ARIMA to remove certain "uninteresting" frequencies in the spectral domain, to obscure individual responses to survey questionnaires and maintain both confidentiality and credibility, and to anticipate the compromise among alternative uses of data. We intend to illustrate some of these considerations by describing the Yule-Slutsky effect as it appears in studies of long waves, by mentioning part of the radical reappraisal of seasonal adjustment that is currently underway, and by looking at the treatment of data as an intermediate good.

Clearly these three topics do not exhaust the area of data preparation. We shall ignore the choice of transformations such as in the Box-Cox approach, differencing, "pre-whitening" and Orcutt-Cochrane adjustments for stationarity and freedom from autocorrelation, and trend removal. The generalized Box-Cox procedure, in which optimal transformations are estimated for both dependent and explanatory variables, is highly non-linear so its use eliminates many of the standard measures of estimated standard error in finite samples. Differencing, with integral powers or the fractional ones considered by Granger, requires criteria for determining which power of difference to choose so an element of subjective judgement arises in its use. Similar intrusions of softness can be identified for Orcutt-Cochrane adjustments, pre-whitening and trend removal. Data preparation should be treated as a specific stage in modelling so research becomes a multi-stage process, which generally means that softness is present . The Frisch-Waugh theorem indicates a few cases where the softness is manageable but these are quite rare. This interaction of softness and multi-stage methods is increasingly evident as the literature on diagnostics and criticism grows.

The Yule-Slutsky effect was mentioned in Chapter Two where spectral approaches to long waves were cited. It is essentially a variant of the fundamental theorem of linear filtering. Suppose $a(B)$ is a polynomial in the lag operator B, then the impact of a linear filter on a given time series x_t can be represented as $a(B)x_t$, or y_t say. Then, if $f_y(\lambda)$ and $f_x(\lambda)$ are the spectral density functions of y and x, they are connected by the relationship

$$(1) \qquad f_y(\lambda) = |a(e^{i\lambda})|^2 \; f_x(\lambda).$$

The spectral density of the adjusted series will depend upon that of the initial series and, also, on another factor determined by the form of adjustment. Here the symbols e and i represent an exponential function with a complex argument. Suppose, in addition, the initial series can be written in terms of white noise as $b(B)\eta_t$, where $b(B)$ is another polynomial in the lag operator. Then there is a potential for forgetting the impact of prior adjustment so as to confuse $|b(e^{i\lambda})|^2$ with $|a(e^{i\lambda})b(e^{i\lambda})|^2$. We obtain "false" signals from the presence of the second filter. The enlargement of the factors in the spectral density function is the Yule-Slutsky effect.

Time domain representations of this effect have been recognized since linear graduation and smoothing of series became common. Moulton (1938), Wald (1939) and Dodd (1939,1941), are typical illustrations of this historical recognition. Fishman (1969, pp. 45-49), Granger (1964, pp. 41-42) and Harvey (1981, pp.81-83) provide more recent illustrations in the spectral domain. The problem for long-wave research has been put succinctly by Bird et al. (1965, p. 239). "We have shown that the use of both a fixed averaging period or a variable averaging period may yield a long cycle in the transformed data, therefore, if long cycles have been found in economic data after using either transformation, it can mean that long cycles actually exist, or that they were created by the transformation." Since linear filters have been used to smooth data in long-wave research for over half century, we face a significant problem for secondary analysis of existing studies in determining whether inferences concerning the existing of long waves and their interactions are robust to data preparation.

A second example of the Yule-Slutsky effect occurs in simulative experiments with large economy-wide econometric models. When a fitted model is linearized to facilitate stochastic simulation, the paths generated by endogenous variables reflect the behaviour assumed for exogenous variables and the implicit filtering (inversion of the fitted linear structure) of stochastic perturbations. The major implication of such simulations is that the filtering of model error can generate cyclical phenomena so that the signal part of the structure need not be forced to include specifications with dynamic characteristics for cycles. The analysis of the Klein-Goldberger model by Adelman and Adelman (1959) is the best-known illustration of this locational analysis for particular cyclical frequencies. Their conclusions reveal the softness of linkages between specification, estimation and simulation. However, they are not associated with the fallibility of indicators and thus lie outside our immediate concern in this chapter.

The fundamental theorem of linear filtering is also relevant in seasonal adjustment of data. Both long-wave and seasonal preparation of economic series seek to enhance the visibility of certain frequencies by "smoothing" out other frequencies. The difficulty represented by Yule-Slutsky effect is that this preparation may induce false signals rather than suppressing uninteresting ones. Often the suitability of particular seasonal adjustments is explored by representing them as exact or approximate linear filters and, then, considering the implications for spectral densities. An excellent example is provided by Dagum (1983), who considers the properties of the X11-ARIMA procedure.

Decomposition of a given economic indicator into four components, one of which is a seasonal, was popularized by Persons sixty five years ago. By the mid-1930s, according to Bell and Hillmer in a recent survey, seasonal adjustment was characterized by four major ideas. These are associated with "changing seasonality", nonstationarity of trends and certain cycles, unsuitability of explicit mathematical functions of time as representations of either seasonals and trends, and the need to deal with outliers. All of these involve softness, which was not dispelled when computational developments transformed seasonal adjustments and the statistical theory of stochastic processes widened the scope of decomposition to spectral analysis of bands of frequencies. Indeed these changes added other difficulties. Although more series could be treated, less time was spent on the appraisal of each series' cyclical characteristics. Also there was a pronounced shift of responsibility for prior adjustment from analyzers of data to their publishers. Many researchers now leave seasonal adjustment to government statisticians or routinely use dummy-variable and X11 procedures.

The present crisis of seasonal adjustment arises from this soft situation. It is far from insignificant since most of the economic indicators that are highly controversial (money supply, unemployment levels) have been adjusted prior to their release. The crisis has several dimensions: the choice of seasonals at annual intervals or concurrently as pioneered at Statistics Canada by Dagum; asymmetric versus symmetric filters; and the feasibility of model-based seasonal adjustment. Eventually our difficulties in determining appropriately adjusted data or schemes for their generation carry over into regression models that use the data. Adjustments change correlative pro- ~~per~~ties both within the serial character of individual variables and between those of several series. Thus checks for exogeneity and causal ordering, for example, are adversely affected.

Our final illustration of fallible indicators stems from the concept of data as an intermediate product. Deville and Malinvaud (1983) raise a number of interesting issues in this context. Their discussion (p. 337) reflects experiences at the Institute National de la Statistique et des Etudes Economiques in Paris. "The same data-set often interests different groups of people, who look at it for answers to different questions, some of which are directly related to various concerns, others raised by specific research projects. Typically data analysis made by official statisticians is not addressed only to one particular type of use but to a more or less wide range of uses. Data analysis may then be viewed as providing an intermediate product, ready for further processing by specialists dealing with particular questions." Since data must satisfy many purposes, its eventual preparation will never be entirely satisfactory. Mis-matches between preparation and diverse uses can be reduced by concerns for efficiency, robustness and standardization, as Deville and Malinvaud point out, but they will persist as contributors to soft econometrics. The concept of intermediate product could, in fact, transform econometric models, especially those that acknowledge measurement error or the latency of underlying economic variables. If taken up by central statistical agencies, it would revive the detailed appraisal of individual time series by non-governmental researchers and improve the interaction of data producers and users. The likelihood of this occuring is, however, small in the present environment of budget constraint for these agencies.

Revisions

In earlier sections, we have discussed some stochastic sources of measurement error, shifting populations due to changes in economic environments, and relatively static issues in data preparation and prior adjustment. We have so far ignored the fact that data are often modified in sequences of releases. These "revisions" of data may be substantial. It is possible to point to several recent instances when revisions were sufficient to change perceptions of the state of economic recovery or growth. They occur usually in data for which preliminary estimates are based on conventional rules before more complete records are available. Surprisingly, preliminary or first stage estimates typically receive much more attention than final estimates. The latter are generally released in consolidated tabulations after considerable delay so they lack immediacy. However, given the magnitude of particular revisions, some weakening of interest in the earlier figures or suitable qualifications of them might have been expected. Their absence reveals how widespread is the unsophisticated knowledge of data sources and of their fallibility. This picture exactly matches the climate revealed in the quotation with which we began this chapter. Economic indicators, however erroneous and subject to revision, are part of the political fabric and thus receive ephemeral but dramatic attention when first their values are released. Loeys (1984, p. 9) indicates a typical attitude. "The highlight of the week for any true 'Fed Watcher' is the Thursday afternoon announcement of the Federal Reserve's most recent estimates of the monetary aggregates. In recent years, financial markets throughout the world have reacted strongly to these announcements." The reactions have been so extreme that arguments have been presented by the Chairman of Federal Reserve for ending the weekly announcements. These arguments gain tentative support whenever dramatic errors in money supply figures occur but such support quickly subsides.

What are consequences for econometrics of revisions? Loeys ignores them and, assuming fundamental irrationality among money-market participants, attempts to model the impact of "announcements". The instability of his empirical results (as found in shifts for October 1979, January 1982 and December 1982) may, however, stem from awareness among participants that data would be revised and their erratic elements smoothed. It is also connected to the reappraisal of the choice of monetary aggregates that we have already noted. Further detail is provided by Loeys and in the papers that he cites. Instability may also be attached to seasonal adjustment techniques as we shall discuss below.

In the wider context of revisions, the traditional response by econometricians was to search for factors in determining the magnitude of revisions. Attempts to link changes from provisional estimates of national income and expenditure with "explanatory" variables using regression techniques are commonplace. There are, however , other aspects of revisions which are less evident but more interesting, especially if connected to the formulation and implementation of economic policies or to the substantial instability of estimated economy-wide econometric models. Here choices of estimating technique may indeed exacerbate the impact of revisions. We shall illustrate this by considering the use of the Almon-lag procedure in construction of successive versions of RDX and CANDIDE econometric models for the Canadian economy.

Suppose we begin our illustrations for revisions by considering revisions of seasonal filters. Dagum (1982, p.173) establishes the connection with economic policy. "The current seasonal adjustment of economic time series is very important for policy making at any level of the economic activity. The seasonally adjusted data are mainly useful to assess the stages of the business cycle at which the economy stands. Because of this, current seasonally adjusted series subject to frequent and high revisions are disliked by policy makers, particularly, if the revisions show a change in the direction of the cyclical movement." Two of her findings reveal the potential feedback from revisions to the choice of seasonal adjustment techniques and, thus, to choice of data. First, for the series that she considers, revisions of concurrent and forecasting filters of X 11-ARIMA are from 30 to 50 percent smaller than those of X 11. Second, revisions for concurrent seasonal filters are nearly a half of those for forecasting filters of both methods. Thus the incidence of significant revisions is highly dependent on choices of prior adjustment. Econometricians should not, therefore, consider the use of seasonally-adjusted economic indicators in fitting structural models without exploring the impact of different choices for the adjusted data.

Estimation is not the only interest here. ~~The~~ Report of the Committee of Experts on Seasonal Adjustment Techniques for the Federal Reserve, which is summarized by Pierce (1983), reveals other concerns which involve revisions. The chief among these link revisions directly with monetary targets and controls. Maravall and Pierce (1983, 1984) and Cook (1984) discuss suggestions that targets be fixed in terms of seasonally adjusted data and explore the presence of noise both in preliminary money supply figures and in subsequent revisions. Clearly noise can be associated with false signals and with difficulties in interpreting breaches of the tolerance range set by the Federal Open Market Committee for growth rates of monetary indicators. Maravall and Pierce (1984) determined a high frequency of misleading preliminary figures (about 40 per cent) in the decade of the 1970s, with seasonal revisions identified as the primary cause of wrong signals.

Turning to revisions and instability of fitted models, we see the Almon lag as a means of apparently reducing dimensionality in the classical linear model. Suppose we have, in familiar notation,

$$(2) \qquad y = Xb + e$$

where y and X record observations for variables, b is an unknown parametric vector and e represents the error. The Almon-lag procedure requires b to be replaced by Ac where A is a known matrix and c is an unknown vector of shorter length than b. Then, if v is the discrepancy between b and Ac,

$$(3) \qquad y = XAc + (e+Xv).$$

Least-squares estimates are obtained for c ignoring the composite character of the error in (3). When multiplied by A, these yield implicit estimates of b. This procedure is used in many economy-wide econometric models, most notably in the RDX and CANDIDE models for Canada and in their U.S. counterparts. For almost two decades, these models have revealed instability of specification when refitted to revised data for National Income and Expenditure after initially being fitted to data including preliminary values. This occurs at about two-year intervals in Canada as revisions are embodied in consolidated tables. We suggest that the combination of revisions and the use of the Almon lag can explain this instability of fitted relationships. If the error e and the specification (2) satisfy the assumptions of the classical linear model, then the Almon estimate for b is biased with the bias being dependent on X, A and b itself. Thus, if revisions change X, the bias will change and estimates will be unstable even if the underlying relationship is not. The methods of estimation (with its false constraints) and data revisions combine to yield an unsatisfactory situation.

It is obvious from these illustrations that revisions of data can be a major component in soft econometrics and in the acceptability, interpretation and tolerance regions of monetary targets as well as many issues affecting seasonal adjustment.

Temporal Intervals and Aggregation

The fifth area of fallibility of measurement that we want to discuss is markedly different from those cited above. It involves, in a fundamental way, the distinction between stocks and flows among economic variables and stems from imprecise treatment of temporal intervals by economists. Although references to "long-run" and "short-run" are common, their definition in terms of time rather than of the flexibility of change is rare. The short-run period is not a fixed number of days or months and is certainly unrelated to the sampling intervals adopted by statisticians in the production of data. As a consequence, the intervals assumed in economic theory (which are relatively imprecise and not necessarily of constant width) are not those underlying data. Thus fitted models must involve some temporal imbalance. Does this basic mis-matching matter? We shall argue that it is a major problem affecting both specification and the determination of statistical properties for estimates of model parameters. To support our arguments, we shall draw on a framework described by Rowley and Trivedi (1975). This simplifies the real situation by assuming sampling intervals are integral sums of theoretical intervals. For example, we can think of annual data when theory is appropriate for quarterly intervals. This simplification is a significant restriction but it yields some results that are, perhaps, surprising for many economists and econometricians. Rowley and Trivedi cite the principal users of this framework. At the end of this section, we shall briefly indicate complications that it obscures. Most notable of these is a problem of irregular spacing which was first discussed by Quenouille.

Before beginning our discussion of temporal aggregation, it seems appropriate to notice a theorem of aggregation for time-series elements. This does not involve temporal aggregation. Rather it reveals a problem for specifying the error in a structural model when this error is assumed to be a linear combination of omitted factors. Following Orcutt, these omissions might be assumed to be generated individually by time-series processes. Suppose z_{1t} and z_{2t} are two elements determined by ARMA (p_1, q_1) and ARMA (p_2, q_2) processes. Then their sum $(z_{1t}+z_{2t})$ is determined by an ARMA (p^*, q^*) process, where p^* is the sum (p_1+p_2) and q^* is the maximum of (p_1+q_2) and (p_2+q_1). For example, if z_{1t} and z_{2t} are determined by two Markov processes AR(1), then their sum (which is a simplification for the error in a structural model) is determined by an ARMA (2,1) process. This theorem suggests that it is very unlikely that errors in structural models have the simple properties that are conventionally associated with them. It is especially clear that, even if error components are Markov, their sum is not. This seems to invalidate routine use of autoregressive transformations. The theorem introduces two elements that we shall also find in temporal aggregation. First, the process generating an aggregate variable can be found from those generating its components but it generally differs from them. Second, aggregation seems to imply the common presence of moving-average features. These MA features are awkward to deal with in estimation and have been widely overlooked both in econometric research and in our textbooks.

We shall explore four illustrative cases for temporal aggregation. The first and second cases deal with the reduction of autocorrelation among errors. These are followed by cases involving lagged dependent variables and exogenous variables. We shall consider three specifications of an inherent model that is based on economic theory:

$$(4) \qquad y_t = a + e_t \qquad \text{(Cases A,B)}$$

$$(5) \qquad y_t = by_{t-1} + a + e_t \qquad \text{(Case C)}$$

$$(6) \qquad y_t = by_{t-1} + dx_t + e_t \qquad \text{(Case D)}.$$

Here y_t is a variable of interest, x_t is an exogenous factor, e_t is an error and the other symbols represent parameters to be estimated.

Similarly we have three specifications for the process generating the error:

$$(7) \qquad e_t = n_t + cn_{t-1} \qquad \text{(Case A)}$$

$$(8) \qquad e_t = ce_{t-1} + n_t \qquad \text{(Case B)}$$

$$(9) \qquad e_t = n_t \qquad \text{(Cases C and D)},$$

where c is a parameter, n_t is white noise and c is assumed less than unity in absolute value.

The fitted model will depend on the choice of sampling interval; that is, on the degree of aggregation. In Case A, the fitted model is

$$(10) \qquad Y_t = ha + E_t \qquad \text{for} \quad t=h,2h,3h,\ldots$$

where h, an integer, is the number of "theoretical time units" in a sampling interval. Y_t is the h-interval aggregate and E_t is the corresponding error. It is reasonably straightforward to establish that E_t is generated by an MA process with lower autocorrelations than those for e_t. For example, with 4-period aggregates, the autocovariances of the two errors are

$$e_t : [1+ (1+c)^2 + c^2], c ,0,...$$
$$E_t : [1+3(1+c)^2 + c^2], c ,0,...$$

if we suppress the scale factor for the variance of the white-noise input.

In Case B, the fitted model is again (10) but the error generating process differs. The aggregate errors are no longer generated by a Markov process. Although autoregressive transformations might be appropriate for the theoretical model, they are unsuitable for the fitted models since aggregation has affected the error-generating process. Taken together, the two cases show that the size of the constant varies with the degree of aggregation, moving-average features persist or are introduced, while autoregressive elements are compounded with MA ones. Finally, even if economic theory or some other sources could identify a time-series structure for the theoretical model, the eventual error will be equally dependent on aggregation. The failure of economic theorists to fix intervals for theoretical models implies that we can never be sure what degree of aggregation has occurred. Consequently we have difficulty in the interpretation of the size of the constant and in choosing an appropriate prior specification of the error. Softness is inevitable.

Turning to Case C, which involves (5) and white noise for the error, additional complications arise. The fitted model for 2-period aggregates is

$$(11) \qquad Y_t = b^2 Y_{t-2} + A + E_t \quad \text{for } t = 2,4,6,\ldots,$$

where A depends on both a and b and E_t also depends on b as given by

$$(12) \qquad \begin{cases} A = 2(a + ab) \\ E_t = n_t + (1+b)n_t + bn_{t-2} \end{cases}$$

The aggregate error has autocovariances that indicate a MA process. Further this error is correlated with Y_{t-2} so least-squares estimates of b^2 will be inconsistent and asymptotically biased. To summarize, the parameters of the fitted model are markedly different from those of the initial theoretical model and the error (solely as a consequence of aggregation) has very unfortunate properties. Notice the sign of b^2 can be different from that of b so rejection of model with the "wrong" sign could be due entirely to aggregation.

When the constant is replaced by the exogenous element dx_t as in the move from Case C to Case D, the "fitted" model is also changed to

$$(13) \qquad Y_t = b^2 Y_{t-2} + dX_t^* + E_t \quad \text{for } t = 2,4,6\ldots$$

where X_t^* is given by

$$(14) \qquad X_t^* = x_t + (1+b)x_{t-1} + bx_{t-2}.$$

Now the appropriate explanatory variable cannot be used unless b is known. Measurement error is inevitable and a significant non-linearity has been introduced.

All of these difficulties stem from the single deficiency of theory to provide an adequate basis for specification by econometricians. Indicators are fallible because we are uncertain considering their temporal span, their correlation with errors, and their accuracy. Aggregation, even of the simple kind discussed here, introduces complexity, biases and nonlinearities. When data are collected at irregular intervals or economic decisions (the theoretical components) occur at irregular intervals, the situation for estimation becomes even more difficult. Dunsmuir and Robinson (1981) discuss time-series estimation when observations are missing. Their approach might indicate one means of dealing with irregularities in data collection or sampling intervals but this has yet to be determined. Rowley and Wilton (1977) explore the combination of irregular decisions and stable sampling intervals with information from wage contracts. Their work is pessimistic since it implies that we could only eliminate the consequences of temporal aggregation by collection of a substantial body of micro-data. The problem of irregular spacing is clearly in need of much more attention. Unfortunately, it is not clear how the simple framework that we have used could be modified for this irregularity.

States and Duration

Much of modern economics is preoccupied with flows. The stock-flow distinction is overlooked or blurred by convenient theoretical devices. Often, when stocks are involved in economic models (such as, for example, equations for the derived demand for productive factors), they are transformed into flows using a notion of implicit "services" associated with the stocks. Thus multiplicative scaling factors, with a time dimension, permit capital stock and labour supply to be treated as if they were capital services and labour services, respectively, especially in neoclassical formulations. In many econometric explorations using data for stocks in linear models, this flow dimension is implicit in their estimated parameters and may affect their intertemporal stability. It is generally hidden from view. Quite apart from the issues raised by the potential instability of estimates and the imprecision of the services concept itself, the use of data for stocks has been linked to some major difficulties in interpretation and specification for econometricians. The difficulties are not simply technical ones of limited interest. They are important ingredients in controversies among economists and policy-makers. We shall use recent discussions of unemployment to illustrate significant issues of stocks as they impact on economic and political controversy and on econometric softness. Our framework is essentially that provided by reactions to the "new view" of unemployment, which is often attributed to Feldstein. For economists, this view stresses "voluntary" unemployment of short duration. It has stimulated a reappraisal of unemployment statistics and of potential methods for augmenting them so as to establish the principal dynamic features of employment, unemployment and labour-force participation in relation to the attributes of different groups.

This reappraisal has generated a host of papers and international attention. Examples for the U.K., the U.S. and Australia include Akerlof and Main (1980, 1981, 1983), Bowers (1980), Bowers and Horvath (1984), Clark and Summers (1979,1980,1982a,b), Frank (1978), Hall (1982), Kaitz(1970), Lancaster (1979), Main (1981, 1982a, b), Nickell (1979a, b; 1980), Salant (1977) and Trivedi and Baker (1982a, b,c). Canadian examples are provided by Beach and Kaliski (1983), Fienberg and Stasny (1983), Hasan and de Broucker (1982a, b, c) and the studies they cite. Associated with this literature, we can detect a shift from the concentration on unemployment rates to new interests in turnover, the incidence of unemployment, frequency and duration of unemployment spells, probabilities of re-employment, and interrupted spells.

The new view of unemployment, as described, for example, by Feldstein (1973, 1976), suggests that its nature has changed fundamentally. It has been affected by an environmental transformation so that the unemployment rate, as conventionally measured, is no longer a stable indicator of closeness to capacity or of economic hardship. This criticism of the indicator is distinct from demographic transformation that we noted in an earlier section of this chapter and the techniques stemming from it differ from the re-weighting schemes that we cited there. Rather attention has shifted to means of modelling "gross flows" in the labour market. With a basic list of "states" (in the labour force, unemployed, employed part-time, employed full-time), these flows are persistence in or movement between successive states. A "spell" is a persistent stay within a given state so much of labour-market dynamics can be expressed in terms of states and spells. Unfortunately, just as we saw a mis-matching of theoretical horizons and sampling intervals for time-series in the last section, we can now distinguish between the time-frames for data from periodic cross-sectional (or rotation-group) samples and the irregular intervals between changes in state actually experienced. This mis-matching is not likely to be resolved by adding retrospective questions to surveys since responses on duration in spells are often inaccurate. In support of this, Bowers and Horvath (p. 148) indicate considerable inconsistency in longitudinal microdata from the Current Population Survey (CPS) concerning duration of individual unemployment. They conclude that it is "inescapable" that reported unemployment duration is greater, on average, than one would expect. This fallibility is additional to that associated with sample selectivity, truncated samples or rotation bias.

If unemployment is largely voluntary, turnover may be high.

and spells of unemployment brief. Rather than looking at unemployment rates, we might prefer to look at alternative indicators such as the average duration of unemployment. Validation of this new view clearly requires detailed exploration of labour-market dynamics beyond those commonly considered prior to the emergence of this view. Given the economic and political significance of unemployment rates as indicators of the need for corrective actions, alternative interpretations of the relative magnitude of the rates must receive adequate attention. This may explain the magnitude of the response to the advocacy of Feldstein and others, as revealed by the contributions in the list that we have provided. Cain (1980, p. 7) summarizes the situation. "It may... be said that if the concept of unemployment were to lose its status as an operationally valid measures of labor market conditions, then the teaching and application of macroeconomic theory, econometric estimation of macroeconomic models, and a large part of labour economics would need to undergo major revisions."

Three elements of labour-market dynamics are indicated in an explanatory pamphlet issued by the U.S. Bureau of Labor Statistics in 1983. These involve turnover, duration and unemployment experience in the year rather than the shorter sample interval (that determines the unemployment rate). First, the large degree of turnover is revealed by the fact that, in non-recessionary periods, about half of the U.S. unemployed in a given month are not unemployed in the following month according to CPS responses. Second, the time sequence of estimated average duration of unemployment is not stable. It was about 10 weeks in periods of economic prosperity but rose to about 18 weeks during the 1981-1982 U.S. recession. Finally, the number of persons who are estimated to experience some unemployment in a year is between $2\frac{1}{2}$ to $3\frac{1}{2}$ times the number revealed by monthly statistics. Clearly all of these affect our interpretation of the appropriateness of the (monthly) unemployment rate as an indicator of economic hardship or as an indicator of labour-market conditions ("tightness", closeness to capacity, or similar notion).

Since our primary interest here concerns the econometrics of gross flows rather than the substantial issues of economic policy, we leave discussion of the latter to Hasan and de Broucker and turn to technical issues raised by the use of data for spells and duration. The intrusion of quasi-structural and supplemental probabilistic models can be illustrated with the treatment by Nickell of unemployment incidence and expected duration for members of particular groups. Then a non-stationarity problem for hazard-function approaches to modelling can be identified using the comments of Beach and Kaliski and of Trivedi and Baker. Both of these components in our demonstration of the softness of econometrics stem from attempts to overcome the absence of appropriate information by using instrumental assumptions.

Nickell uses a logit model for estimating the probability of unemployment. If an individual is characterized by a number of recorded attributes x_1, x_2, ... and so on, the probability of being unemployed may be assumed given by the quasi-structural relations

$$(15)\qquad p(x) = 1/(1+e^{-m})$$

$$(16)\qquad m = b_0 + b_1x_1 + b_2x_2 + \ldots$$

where b_0, b_1, b_2 represent unknown weights. If a random sample of J unemployed and K employed persons is collected, its likelihood function is given by

$$(17)\qquad L(b) = \prod_j p(\underline{x}_j) \prod_k [1-p(\underline{x}_k)]$$

where $\underline{x}_j$ and $\underline{x}_k$ represent recorded attributes. Estimation of the unknown weights from this likelihood yields both the estimated incidence and group unemployment rates (for average values of attributes). A similar technique can be used to determine estimates of duration in unemployment. Suppose s measures an individual's present duration so far, then we could specify the conditional probability of his leaving unemployment in a given period as a function of s and other personal attributes:

$$(18)\qquad q(x,s) = 1/(1+e^{-n})$$

$$(19)\qquad n = a_0 + a_1s + a_2s^2 + \sum_i b_ix_i .$$

The expected duration for an individual or group with attribute levels x_0 is then estimated by

$$(20)\qquad d(x_0) = \sum_{s=1}^{\infty} s\hat{q}(x_0,s) \prod_{v=0}^{s-1} [1-\hat{q}(x_0,v)].$$

This form of model is quite common. Its acceptability stems from simplicity and the absence of more direct evidence.

However the results are essentially soft because of the absence of any inherent justification for the structural specifications for (15), (16), (18) and (19) either at a given period or through successive periods. We obtain estimates of interesting dynamic features but the qualities of these estimates cannot be determined outside the narrow framework of a conditional "truth" of the specifications. This is clearly an area of exploratory econometrics rather than one of confirmation. Excellent accounts of alternative specifications for estimating duration are provided by Trivedi and Baker. All share the arbitrariness of Nickell's simple model, its structural softness and exploratory form.

Given the variability of actual duration that we have already cited for the U.S. (and present elsewhere, of course), the assumption of constant weights in these specifications is difficult to accept without qualification. Nonstationarity, invariance and parametric instability seem inevitable. This is recognized by Beach and Kaliski (p. 258) when they conclude that "one cannot get rid of all assumptions of invariance over time simply by changing from assuming a constant escape rate to estimating a 'hazard function' relating escape probability to the duration of unemployment." The implication for research is that its models may reflect crude approximations to steady-state formulations even if these are being explicitly discounted by individual authors. Appendix 2 of Trivedi and Baker (1982c) explores the relationship between some methods of estimation and steady state models, including those of Main, Salant, Kaitz and Frank. The overall picture is one of confusing complexity and, ~~perhaps~~, the major conclusion that emerges is that all procedures are soft both as theoretical formulations and as generators of empirical evidence on the features of spells and duration. Thus the fallibility of our data resources has not been overcome by recourse to supplemental modelling techniques. Many of the problems of dealing with changes in states, or changes in stocks with aggregation, persist to confuse interpretation of empirical findings.

Soft Concepts

In a major address to the American Economic Association, Leontief (1971, pp. 1-3) took aim at some undesirable features of economic theory and empirical research as he perceived them. He pointed to the "uncritical enthusiasm" for mathematical formulation that "tends often to conceal the ephemeral substantive content of the argument behind the formidable front of algebraic signs" and was severe on the failures of economists to consider the empirical validity of their assumptions. The impracticality of the "massive and sophisticated" statistical machinery of econometrics was noted and the preoccupation of economists with imaginary or hypothetical variables given suitable approbation. "In too many instances sophisticated statistical analysis is performed on a set of data whose exact meaning and validity are unknown to the author." A major hazard for linking the specification of econometric models to economic theory is explicit in this criticism. Soft theoretical concepts, so easy to introduce, need to be augmented before they imply particular measurements or before they can be embedded in any of the ideal models used for statistical analysis. Leontief is simply repeating the views expressed by Clark (1947) in the quotation cited in Chapter Three. Abstract symbols do not automatically eliminate loose thinking. Often they fail to attract the clarification needed for them to be compatible with any system of measurement.

Economic theory contains a host of concepts associated with terms such as potential, expected, normal, natural, underlying, permanent and so on. The occurrence of these qualifiers is almost always a sign of imprecision as regards measurement. Nor does the use of these terms exhaust areas of softness in theoretical concepts. One feature of a vibrant discipline is the transformation and evolution of its concepts. Thus it should not be surprising if economists choose to work with "fuzzy" concepts before they receive suitable definition. On the other hand, it may be surprising to find persisting softness in the determination of major economic variables such as the user cost of durable assets, unemployment, liquidity, capacity, labour-market slack and income or of economic turning points as in the stages of business cycles in economic activity. Indeed economists might even reveal a predilection for using soft concepts in excess of that found in some of the older disciplines.

Clearly imprecision in the basic elements of theory will have an impact on the specification, interpretation and appraisal of econometric models. It also affects the consideration of economic policy formulation, which can involve the soft concepts too. We have already noted the difficulties experienced in the choice of U.S. monetary aggregates for targetting ranges, the reappraisal of productivity indicators (and hence of capacity or potential concept)at the Economic Council of Canada, the search for appropriate weights with which to make measures of unemployment more compatible with intertemporal comparisons, and many more policy-related illustrations can be found. For example, the Department of Finance of the Federal Government began using the "underlying" rate of inflation in the 1981 issue of its Economic Review (ch. 3).

The relationship between concepts and actions may be quite complex especially when measurements for particular concepts reach "unacceptable" levels. The exchange between Taylor and Boreham in the discussion of the paper by Deville and Malinvaud (1983, pp. 354-355) provide an interesting illustration. When the U.K. government chose to change its definition of measured unemployment, its action created a disturbing controversy in which issues of measurement and political expediency became intermingled. Were the data amended because of flaws in their measurement, the availability of superior administrative methods, or because the values actually recorded were embarassing? This controversy could only arise when the underlying concept is soft although obviously other conditions are also relevant.

The implications of soft concepts for econometricians seem clear. We shall draw on developments in three areas of economics to clarify some of them. Our discussion involves the periodization for business cycles, prices and unemployment. The first area was conventionally treated with NBER methods and ad hoc rules until Mintz (1969-1974) and Bry and Boschan (1971) developed computer-based approaches to partially replace the earlier ones indicated in Burns and Mitchell (1946). The apparent conflict between NBER approaches and structural econometric models meant that there were two distinct attitudes to data and their processing. Since the diffusion indices and similar measurements are primarily dependent on directions of change and not on interval scales, they may be more robust to conceptual uncertainties and shifts than measurements used in econometric equations. The softness of traditional NBER methods for dating cycles is evident while that of structural methods is not. Rather the inadequate recognition of the fallibility of measured indicators obscures this softness. Indeed consideration of robustness might raise the relative status of the traditional methods. It is also worth recalling the discussion by Burns and Mitchell (pp. 3-5) of the definition of the business cycle. They list a series of issues raised by the definition and argue that each clause in their tentative choice "suggests hard questions, some of which raise doubts about the validity of the concept itself". This softness inevitably leads to empirical difficulties irrespective of what method is adopted for determining the change-points in cycles. Which indicators should we look at? How should we "prepare" them? How should we combine them? How do we establish significant change? Responses to such questions probably fail any simple tests of objectivity.

Apart from Mintz's efforts, we should note two other econometric developments which are affected by soft concepts. The use of clustering algorithms and Chernoff's faces (as by Meyer at the NBER) to date cycles is a straightforward extension of the traditional methods. It shares their evident softness especially as the choice of facial configurations and clustering metrics is known to be highly subjective. The second development is associated with the new equilibrium business-cycle theories of Lucas and others. It stresses the use of stochastic difference equations and the commonality of some cyclical frequencies. Sargent (1979, pp. 254-256) provides an excellent description of how econometric models can be used in this line of development. He illustrates pairwise coherence among the unemployment rate, real GNP and output per manhour at low business-cycle frequencies for example. The presence of soft concepts means that these calculations are distorted by noise.

Turning to prices, we can find a variety of major problems. The consumer price index provides the most common framework for their discussion and successive BLS Commissioners in the U.S. have felt compelled to speak out and to issue reports on the challenges and potential amendments of the CPI. A typical report is <u>Problems in Measuring Consumer Prices</u> (BLS-697, 1983) while standard issues involve the treatment of prices for durables (especially for owner-occupied housing and automobiles), taxes, differential experiences for population groups, substitution, seasonality, quality controls due to response patterns and sample size, and the comparability with other price indices. Nor is discussion restricted to governmental bureaucrats, statisticians and academics. In May 1982, for example, the president of the Canadian Bankers Association told a committee of the Canadian Senate that the prevailing rate of the CPI at 12 per cent was quite incompatible with the corresponding rates for other economic indicators, which were in the range of about 7 or 8 per cent. A few months earlier, the President of the Treasury Board had issued a similar warning about the possible shortcomings of the Canadian CPI.

Some aspects of attendant problems are revealed by Blanciforti and Galvin (1984), Cagan and Moore (1981), Callahan (1981), Gillingham (1983), Gillingham and Lane (1982), Lane and Sommers (1984), Mitchell (1980), Rymes (1979) and Triplett (1980, 1981) and in many other papers. Blanciforti and Galvin consider new approaches of driving a user cost for automobile services for inclusion in the overall index. (The user-cost framework had already been thoroughly explored for homeownership by Gallingham so that the BLS has begun a process of amending the homeownership component in the U.S. CPI.) One of their conclusions is worth repeating. "[Estimated] user cost functions are extremely volatile, reflecting weaknesses in the determination of changes in the market value of a durable over time, specifically with respect to depreciation and capital gains." The issues connected with housing are also prominent in the other papers cited. Triplett (1981) considers the reconciliation of the CPI and the implicit price deflator for personal consumer expenditures from National Income and Product Accounts so that he must address the issue of substitution and its impact on the interpretation of fixed-weight indices. Callahan (p. 12), on the other hand, compares different measures of the rate of underlying inflation. Again one of his comments is worth repeating. "The widespread usage of the term underlying rate of inflation would imply a consensus of understanding. It is cited so often without any explanation that we dare not ask what it means for fear of showing our ignorance. In truth, the presumption that the meaning of underlying rate of inflation has become common knowledge is only half correct..... We now go from the world of conceptual unanimity to widespread disagreement on the appropriate measurement of the rate. There is almost a one-to-one correspondence between the number of economists who have addressed this topic and the number of different measures proposed." With this backdrop, it is difficult to see how we can avoid the placing of complex qualifications on any fitted relationship that includes the CPI or an alternative price index as a measure of an explanatory factor.

The situation is no better with labour-force statistics. In 1962, the President's Committee to Appraise Employment and Unemployment Statistics (the Gordon Committee) suggested that the CPS include special efforts to identify "discouraged" workers although it recommended the unemployment rate ought not to include them. By March 1984, the Metropolitan Toronto Social Planning Council was able to report unemployment rates of 16.1 per cent, about 40 per cent higher than those stemming from Statistics Canada. The difference was due to the disparities in treatments of discouraged workers. Even the practical definition of discouragement is contentious. The choice of the Gordon Committee was, for example, rejected by the report of the more recent National Commission, which indicated a preference for a narrower definition, but much more will be heard on this question before the matter is settled. Until then, the problem of "hidden" unemployment due to discouragement will continue to damage the integrity of published unemployment rates.

Similar disparities arise when we consider partitions of existing rates, as when attempting to identify "voluntary" or "structural" components in different decompositions. For employment, we can point to the definitional softness for "marginal attachment", "full employment" and "subemployment". Further information is provided in Chiswick (1980), Clogg (1979), Fellner (1978), Fortin and Newton (1982), Stein (1980), Thirlwall (1983), and Trivedi and Baker (1982d). One consequence of this imprecision is experimentation with different measures for given econometric equations. Fortin and Newton (p. 262) provide a simple illustration for modifications of the unemployment rate to approximate labour market tightness. With variants of a basic equation for wage-determination, they include or exclude a number of different modified rates (sometimes singly, sometimes collectively) and judge their corresponding fits in terms of what they term t-statistics and standard errors for the equations. This amounts to repeated use of confirmatory procedures in an exploratory situation. The impossibility of deriving accurate inferential statistics for their method is a consequence of the softness. All of their diagnostics have invalid probabilistic bases. That this type of experimentation is common does not remove its adequacies. Only a refinement of the theoretical constructs to include specification of their measurement will suffice.

In summary, there are no apparent means of avoiding the complications of soft theoretical concepts for econometric modelling that requires interval scales and given structures. The complications are generally far from being trivial because alternative measures (associated with different treatments of a single theoretical concept) are often quite different in level and dynamic movements.

Final Comment

We began with the suggestion that the fallibility of economic indicators was an important element of soft econometrics even though this fallibility is often ignored or not generally realized. The substance of the sections that we have provided is far removed from the contents of most econometric textbooks and of a large body of research papers produced by economists. In many ways, the procedures adopted in practice should be seen as inadequate responses to the problems of fallible indicators. Instabilities in fitted equations and their sensitivity to sample coverage, modelling imprecisions due to temporal aggregation, experimentation with different measurements due to softness of theoretical constructs are major features of applied econometrics. However, as we have amply demonstrated, issues of measurement are far more extensive than even this list suggests. It seems appropriate to conclude that the consequences of fallibility would be much better dealt with if more attention were given to measurement as such. It is a vital aspect of econometrics.

Refrences

ADELMAN, I., and F.L. ADELMAN (1959), "The Dynamic Properties of the Klein-Goldberger Model", Econometrica, Vol. 27, No. 4, October, pp. 596-625.

ADVISORY COMMITTEE ON MONETARY STATISTICS (1976), Improving the Monetary Aggregates, Board of Governors of the Federal Reserve System, June.

ANTOS, J., W. MELLOW and J.E. TRIPLETT (1979), "What is a Current Equivalent to Unemployment Rates of the Past?", Monthly Labor Review, Vol. 102, No. 3, March, pp. 36-46.

AKERLOF, G., and B.G.M. MAIN (1980), "Unemployment Spells and Unemployment Experience", The American Economic Review, Vol. 70, No. 5 , December, pp. 885-893.

AKERLOF, G., and B.G.M. MAIN (1981), "An Experience-Weighted Measure of Employment and Unemployment Duration", The American Economic Review, Vol. 71, No. 5, December, pp. 10003-1011.

AKERLOF, G., and B.G.M. MAIN (1983), "Measures of Unemployment Duration as Guides to Research and Policy: Reply", The American Economic Review, Vol. 73, No. 5, December, pp. 1151-1152.

BAILAR, B.A. (1975), "The Effects of Rotation Group Bias on Estimates Fron Panel Surveys", Journal of the American Statistical Association, Vol. 70, pp. 23-29.

BARNETT, W.A. (1983a), "New Indices of Money Supply and the Flexible Laurent Demand System", Journal of Business and Economic Statistics, Vol. 1, No. 1, January, pp. 7-23.

BARNETT, W.A. (1983b), "Understanding the New Divisia Monetary Aggregates", Review of Public Data Use , Vol. 11, No. 4, December, pp. 349-355.

BARNETT, W.A. (1984), "Recent Monetary Policy and the Divisia Monetary Aggregates", The American Statistician, Vol. 38, No. 3, August, pp. 165-172.

BARNETT, W.A., E. OFFENBACHER and P. SPINDT (1981), "New Concepts of Aggregate Money", Journal of Finance, Vol. 36, May, pp. 497-506.

BARNETT, W.A., and P.A. SPINDT (1980a), The Velocity Behavior and Information Content of Divisia Monetary Aggregates, Special Studies Paper No. 141, Division of Research and Statistics, Federal Reserve Board, January.

BARNETT, W.A., and P.A. SPINDT (1980b), The Information Content of Divisia Monetary Quantity Indices, Special Studies Paper No. 146, Division of Research and Statistics, Federal Reserve Board, August.

BEACH, C.M., and S.F. KALISKI (1983), "Measuring the Duration of Unemployment from Gross Flow Data", Canadian Journal of Economics, Vol. 16, No. 2, May, pp. 258-263.

BIRD, R.C., M.J. DESAI, J.J. ENZLER and P.J. TAUBMAN (1965), "'Kuznets Cycles' in Growth Rates: The Meaning", International Economic Review, Vol. 6, No. 2, May, pp. 229-239.

BLANCIFORTI, L., and J.M. GALVIN (1984), New Approaches for Automobiles in the CPI, Paper presented at Annual Meeting of the American Statistical Association, Philadelphia.

BLUNDELL-WIGNALL, A., M. RONDONI and H. ZIEGELSCHMIDT (1984), The Demand for Money and Velocity in Major OECD Countries, Working Paper No. 13, OECD Economics and Statistics Department.

BOWERS, N. (1980), "Probing the Issues of Unemployment Duration", Monthly Labor Review, Vol. 103, pp. 23-32.

BRY, G., and C. BOSCHAN (1971), Cyclical Analysis of Time Series: Selected Procedures and Computer Programs (New York: NBER).

BUREAU OF LABOR STATISTICS, U.S. DEPARTMENT OF LABOR (1983), Workers, Jobs, and Statistics, Report 698.

BURNS, A.F., and W.C. MITCHELL (1946), Measuring Business Cycles (New York: NBER).

CAGAN, P., and G.H. MOORE (1981), "Some Proposals to Improve the Consumer Price Index", Monthly Labor Review, Vol. 104, September, pp. 20-25.

CAIN, G.G. (1979), "The Unemployment Rate as an Economic Indicator", Monthly Labor Review, Vol. 102, No. 3, March, pp. 24-35.

CAIN, G.G. (1980), "Labor Force Concepts and Definitions in View of Their Purposes", in NATIONAL COMMISSION ON EMPLOYMENT AND UNEMPLOYMENT STATISTICS, Concepts and Data Needs: Appendix Volume 1 (Washington, D.C.: NCEUS).

CALLAHAN, D.W. (1981), "Defining the Rate of Underlying Inflation", Monthly Labor Review, Vol. 104, September, pp. 16-19.

CHISWICK, B.R. (1980), "A Review of 'Counting the Labor Force', the Report of the National Commission on Employment and Unemployment Statistics", in (ed.) W. FELLNER, Contemporary Economic Problems 1980 (Washington, D.C.: American Enterprise Institute), pp. 263-300.

CLARK, K.B., and L.H. SUMMERS (1979), "Labor Market Dynamics and Unemployment: A Reconsideration", Brookings Papers on Economic Activity, No. 1, pp. 13-72.

CLARK, K.B., and L.H. SUMMERS (1980), "Unemployment Reconsidered", Harvard Business Review, Vol. 58, November-December, pp. 171-178.

CLARK, K.B., and L.H. SUMMERS (1982a), "Labor Force Participation: Timing and Persistence", Review of Economic Studies, Vol. 49, pp. 825-844.

CLARK, K.B., and L.H. SUMMERS (1982b), "Unemployment Insurance and Labor Market Transitions", in (ed.) M.N. BAILY, Workers, Jobs, and Inflation (Washington, D.C.: The Brooking Institution).

CLARKSON, K.W., and R.E. MEINERS (1977), "The Spurious Increase in the Unemployment Rate", Policy Review, Vol. 1, pp. 27-51.

CLARKSON, K.W., and R.E. MEINERS (1979), "Institutional Changes, Reported Unemployment, and Induced Institutional Changes", Carnegie-Rochester Conference Series on Public Policy, Vol. 10, pp. 205-235.

CLOGG, C.C. (1979), Measuring Underemployment (New York: Academic Press).

COCKERLINE, J.P., and J.D. MURRAY (1981a), A Comparison of Alternative Methods of Monetary Aggregations: Some Preliminary Evidence, Technical Paper 28, Bank of Canada.

COCKERLINE, J.P., and J.D. MURRAY (1981b), Superlative Monetary Aggregation: Some Preliminary Evidence, Paper presented at Annual Meeting of the Canadian Economic Association, Halifax.

COOK, T.Q. (1984), "The 1983 M1 Seasonal Factor Revisions: An Illustration of Problems that May Arise in Using Seasonally Adjusted Data for Policy Purposes", Economic Review, Federal Reserve Bank of Richmond, Vol. 70, No. 2, March-April, pp. 22-33.

DAGUM, E.B. (1982), "Revisions of Time Varying Seasonal Filters", Journal of Forecasting, Vol. 1, pp. 173-187.

DAGUM, E.B. (1983), "Spectral Properties of the Concurrent and Forecasting Seasonal Linear Filters of the X-11-ARIMA Method", The Canadian Journal of Statistics, Vol. 11, No. 1, pp. 73-90.

DEVILLE, J.-C., and E. MALINVAUD (1983), "Data Analysis in Official Socio-economic Statistics", Journal of the Royal Statistical Society, Series A, Vol. 146, Part 4, pp. 335-361.

DODD, E.L. (1939), "The Length of the Cycles Which Result From the Graduation of Chance Elements", The Annals of Mathematical Statistics, Vol. 10, pp. 254-264.

DODD, E.L. (1941), "The Problem of Assigning a Length to the Cycle to be Found in a Simple Moving Average and in a Double Moving Average of Chance Data", Econometrica, Vol. 9, No. 1, January, pp. 25-37.

DUNSMUIR, W., and P.M. ROBINSON (1981), "Parametric Estimators for Stationary Time Series With Missing Observations", Advances in Applied Probability, Vol. 13, pp. 129-146.

FELDSTEIN, M. (1976), "Temporary Layoffs in the Theory of Unemployment", Journal of Political Economy, Vol. 84, No. 5, October, pp. 937-957.

FELDSTEIN, M. (1973), "The Economics of the New Unemployment", The Public Interest, No. 33, pp. 3-42.

FELLNER, W. (1978), "Structural Problems Behind Our Measured Unemployment Rates", in (ed.) W. FELLNER, Contemporary Economic Problems 1978 (Washington, D.C.: American Enterprise Institute), pp. 83-112.

FIENBERG, S.E., and E.A. STASNY (1983), "Estimating Monthly Gross Flows in Labour Force Participation", Survey Methodology, Vol. 9, No. 1, pp. 77-102.

FISHMAN, G.S. (1969), Spectral Methods in Econometrics (Cambridge, Mass.: Harvard University Press), pp. 45-49.

FLAIM, P.O. (1979), "The Effect of Democratic Changes on the Nation's Jobless Rate", Monthly Labor Review, Vol. 102, No. 3, March, pp. 13-23.

FORTIN, P., and K. NEWTON (1982), "Labor Market Tightness and Wage Inflation in Canada", in (ed.) M.N. BAILY, Workers, Jobs, and Inflation (Washington, D.C.: The Brookings Institution), pp. 243-278.

FRANK, R. (1978), "How Long is a Spell of Unemployment?", Econometrica, Vol. 46, pp. 285-301.

GHANGURDE, P.D. (1982), "Rotation Group Bias in the LFS Estimates", Survey Methodology, Vol. 8, Nos. 1 and 2, pp. 86-101.

GILLINGHAM, R. (1983), "Measuring the Cost of Shelter for Homeowners: Theoretical and Empirical Considerations", Review of Economics and Statistics, Vol. 65, No. 2, May, pp. 254-265.

GILLINGHAM, R., and W. LANE (1982), "Changing the Treatment of Shelter Costs for Homeowners in the CPI", Monthly Labor Review, Vol. 105, June, pp. 9-14.

GODAMBE, V.P. (1955), "A Unified Theory of Sampling From Finite Populations", Journal of the Royal Statistical Society, Vol. 17, Series B, pp. 269-278.

GRANGER, C.W.J. (1964), Spectral Analysis of Economic Time Series (Princeton, N.J.: Princeton University Press), pp. 41-42.

HALL, R.E. (1982), "The Importance of Lifetime Jobs in the U.S. Economy", American Economic Review, Vol. 72, No. 4, September, pp. 716-724.

HARVEY, A.C. (1981), Time Series Models (Oxford: Phillip Allan), pp. 81-83.

HASAN, A., and P. de BROUCKER (1982a), An Assessment of the Turnover View of Labour Markets: Some Canadian Evidence Paper presented at Eastern Economic Association Meetings, Washington.

HASAN, A., and P. de BROUCKER (1982b), Turnover and Job Instability in Youth Labour Markets in Canada, Paper prepared for Experts Group Meeting on the Dynamics of Youth Unemployment, Paris.

HASAN. A., and P. de BROUCKER (1982c), "Duration and Concentration of Unemployment", Canadian Journal of Economics, Vol. 15, pp. 735-756.

HESTER, D. (1981), "Innovations and Monetary Control", Brookings Papers on Economic Activity, No. 1, pp. 141-189.

JUDD, J.P., and J.L. SCADDING (1982a), Financial Change and Monetary Targeting in the United States, Paper presented to Conference on Interest Rate Deregulation and Monetary Policy, Monterey, California.

JUDD, J.P., and J.L. SCADDING (1982b), "The Search for a Stable Demand Function: A Survey of the Post-1973 Literature", Journal of Economic Literature, Vol. 20, No. 3, September, pp. 993-1023.

KAITZ, H.B. (1970), "Analyzing the Length of Spells of Unemployment", Monthly Labor Review, Vol. 93, November, pp. 11-20.

KOTT, P.S. (1984), "A Superpopulation Theory Approach to the Design of Price Index Estimators With Small Sampling Biases", Journal of Business and Economic Statistics, Vol. 2, No. 1, January, pp. 83-90.

LANCASTER, T. (1979), "Econometric Methods for the Duration of Unemployment", Econometrica, Vol. 47, pp. 939-956.

LANE, W.F., and J.P. SOMMERS (1984), Improved Measures of Shelter Costs, Paper presented to Annual Meeting of the American Statistical Association, Philadelphia.

LEONTIEF, W. (1971), "Theoretical Assumptions and Nonobserved Facts", American Economic Review, Vol.61, No. 1, March, pp. 1-7.

LOEYS, J.G. (1984), "Market Views of Monetary Policy and Reactions to M1 Announcements", Business Review, Federal Reserve Bank of Philadelphia, March-April, pp. 9-17.

MAIN, B.G.M. (1981), "The Length of Employment and Unemployment in Great Britain", Scottish Journal of Political Economy, Vol. 28, June, pp. 146-164.

MAIN, B.G.M. (1982a), "Three Summary Measures of the Duration of Unemployment: A Note", Scottish Journal of Political Economy, Vol. 29, February, pp. 99-101.

MAIN, B.G.M. (1982b), "The Length of a Job in Great Britain", Economica, Vol. 49, No. 195, August.

MARAVALL, A., and D.A. PIERCE (1983), "Preliminary-Data Error and Monetary Aggregate Targeting", Journal of Business and Economic Statistics, Vol. 1, No. 3, July, pp. 179-186.

MARAVALL, A., and D.A. PIERCE (1984), The Transmission of Data Noise and Policy Noise in Modern Control, Special Studies Paper 184, Division of Research and Statistics, Federal Reserve Board.

MAYER, T. (1982), "Financial Innovation-The Conflict Between Micro and Macro Optimality", American Economic Review, Vol. 72, No. 2, May, pp. 29-34.

MEYER, J., and D. WEINBERG (1975a), "On the Classification of Economic Fluctuations", Explorations in Economic Research, Vol. 2, No. 2, pp. 167-202.

MINTZ, I. (1969), Dating Postwar Business Cycles: Methods and Their Application to Western Germany, 1950-67, Occasional Paper 107, NBER, New York.

MINTZ, I. (1974), "Dating United States Growth Cycles", Explorations in Economic Research, Summer, pp. 1-113.

MITCHELL, D.J.B. (1980), "Does the CPI Exaggerate or Understate Inflation?", Monthly Labor Review, Vol. 103, May, pp. 31-33.

MOULTON, E.J. (1938), "The Periodic Function Obtained By Repeated Accumulation of a Statistical Series", The American Mathematical Monthly, Vol. 45, pp. 583-586 and 105-106.

NICKELL, S.J. (1979a), "Estimating the Probability of Leaving Unemployment", Econometrica, Vol. 47, September, pp. 1249-1266.

NICKELL, S.J. (1979b), "The Effect of Unemployment and Related Benefits on the Duration of Unemployment", Economic Journal, Vol. 89, March, pp. 34-49.

NICKELL, S.J. (1980), "A Picture of Male Unemployment in Britain", Economic Journal, Vol. 90, December, pp. 776-794.

PERRY, G.L. (1970), "Changing Labor Markets and Inflation", Brookings Papers on Economic Activity, No. 3, pp. 411-441.

PERRY, G.L. (1971), "Inflation Versus Employment: The Worsening Tradeoff", Monthly Labor Review, No. 2, pp. 68-71.

PIERCE, D.A. (1983), "Seasonal Adjustment of the Monetary Aggregates: Summary of the Federal Reserve's Committee Report", Journal of Business and Economic Statistics, Vol. 1, No. 1, January, pp. 37-42.

ROWLEY, J.C.R., and P.K. TRIVEDI (1975), Econometrics of Investment (London: Wiley).

ROWLEY, J.C.R., and D.A. WILTON (1977), The Determination of Wage-Change Relationships (Ottawa: Economic Council of Canada).

RYMES, T.K. (1979), "The Treatment of 'Home Ownership' in the CPI", Carleton Economic Papers, No. 79-03, March.

SALANT, S.W. (1977), "Search Theory and Duration Data: A Theory of Sorts", Quarterly Journal of Economics, Vol. 91, pp. 39-57.

SARGENT, T.J. (1979), Macroeconomic Theory (New York: Academic Press), pp. 254-256.

SIMPSON, T.D. (1984), "Changes in the Financial System: Implications for Monetary Policy", Brookings Papers on Economic Activity, No. 1, pp. 249-265.

SPINDT, P.A. (1985), Money is What Money Does: A Revealed Production Approach to Monetary Aggregation, Special Studies Paper No. 177, Division of Research and Statistics, Federal Reserve Board, June.

STEIN, R.L. (1980), "National Commission Recommends Changes in Labor Force Statistics", Monthly Labor Review, Vol. 103, No. 4, April, pp. 11-21.

THIRLWALL, A.P. (1983), "What Are Estimates of the Natural Rate of Unemployment Measuring?", Oxford Bulletin of Economics and Statistics, Vol. 45, No. 2, May, pp. 173-179.

TINSLEY, P.A., B. GARRETT and M.E. FRIAR (1978), The Measurement of Money Demand, Special Studies Paper No. 133, Division of Research and Statistics, Federal Reserve Board, October.

TRIPLETT, J.E. (1980), "Does the CPI Exaggerate or Understate Inflation? Some Observations", Monthly Labor Review, Vol. 103, May, pp. 33-35.

TRIPLETT, J.E. (1981), "Reconciling the CPI and the PCE Deflator", Monthly Labor Review, Vol. 104, September, pp. 3-15.

TRIVEDI, P.K., and G.M. BAKER (1982a), Estimation of the Average Length of an Unemployment Spell from Labour Force Experience Data, Working Paper No. 66, Research School of Social Sciences, The Australian National University.

TRIVEDI, P.K., and G.M. BAKER (1982b), Unemployment in Australia: Duration Experience and Recurrent Spells, Discussion Paper No. 45, Centre for Economic Policy Research, The Australian National University.

TRIVEDI, P.K., and G.M. BAKER (1982c), Methods for Estimating the Duration of Periods of Unemployment: A Comparative Study. Working Paper No. 76, Research School of Social Sciences, The Australian National University.

TRIVEDI, P.K., and G.M. BAKER (1982d), Equilibrium Unemployment in Australia: Concepts and Measurement, Discussion Paper No. 59, Centre for Economic Policy Research, The Australian National University.

WALD, A. (1939), "Long Cycles as a Result of Repeated Integration", The American Mathematical Monthly, Vol. 46, March, pp. 136-141.

WILKERSON, M. (1967), "The Statistical Structure of the Revised CPI", Monthly Labor Review, Vol. 62, pp. 899-914.

WOJNILOWER, A. (1980), "The Central Role of Credit Crunches in Recent Financial History", Brookings Papers on Economic Activity, No. 2, pp. 277-326.

CHAPTER FIVE

THE WIDER VISION

Econometrics is a mixture of economics, mathematics, statistics, computing and other elements. The relative contributions of these components vary as we shift from one research situation to another. However no single component can predominate without the nature of econometrics being unduly restricted. In earlier chapters, we have cited two alternative views. One view associated with Frisch, was put forward as Econometrica was launched a half-century ago. It stressed balance and the interaction of diverse interests. The second view emerged as the associates of the Cowles Commission sought to establish the primacy of a particular probability approach to structural estimation in econometrics. As expressed by Klein in 1971, this made the subject of econometrics "a special branch of mathematical statistics". Other components were not wholly dismissed but their significance was reduced as statistical inference became the major preoccupation. Yet the practical demands of real circumstances brought forth incompatibilities between this narrow view and the procedures actually adopted by econometricians. There occurred a pronounced discrepancy between what econometricians did and the theoretical frameworks that were cited in support of their activities. Eventually this gap was the stimulus for discussions of robustness, sequential modelling, "criticism" and model diagnostics which reflected exploratory themes rather than the confirmatory theme of econometrics as a branch of mathematical statistics. Extrapolation of such discussions seems to point to the re-emergence of the earlier view with its wider dimensions. It should be clear that our three essays assume the wider vision is the appropriate choice. Then much of the richness of econometric analyses can be identified with elements from outside the narrow confines of mathematical statistics. In addition, these confines have themselves been weakened by feedback from practical problems to theoretical modelling. Obvious examples are the revived interest in pre-test biases and the development of theories for dealing with the restricted domain of economic variables.

Such shifts in focus and discrepancies between theory and practice are not, of course, limited to econometrics. The parallel developments in statistics are evident in the presidential address of Cox (1981) to the Royal Statistical Society and in Moser's response to this address. They found a distinction between "hard" and "soft" essential in explaining the diversity of statisticians' activities. These qualifiers have also been used, as by Brouwer and Nijkamp (1982), in distinguishing among cardinal, ordinal and other scales for measurement. In our three essays, we have attempted to show their usefulness in econometrics too. Suppose the subject-matter of the narrow view of econometrics as a branch of mathematical statistics is termed "hard". Then the additional subject-matter that is included as we move to the wider view might be termed "soft". Hard econometrics is characterized by the firmness of its propositions while soft econometrics involves imprecision, subjectivity, flexibility and complexity. Soft econometrics might even include awareness that particular problems cannot be resolved. This realism enhances econometrics. It does not diminish econometrics even though criticism, dissent and diversity are more likely than the consensus found with hard mathematical propositions.

The three essays may appear critical of econometrics in some respects. Such criticism should not be misinterpreted. As in the research programmes of Box and others, which are now becoming common in the newer textbooks, criticism is not to be interpreted negatively. It is forward-looking and the means of identifying feasible directions for reducing imprecision or resolving shortcomings. In many places, we have found softness. This identification indicates where we might search for progress. There is no intention of using softness as an excuse for rejecting econometrics. To the contrary, softness means that econometrics becomes more interesting, more thought-provoking and more realistic. Flaws are easy to find and many will persist because we presently have no idea of how they could be eliminated. If their presence were sufficient for rejecting econometrics, we would have not bothered to write our three essays. The large number of references that are given throughout our discussion also suggests that many researchers are attempting to focus attention on soft elements in practical situations. Their efforts are not wholly successful but this is hardly surprising unless we ignore the inherent softness and fail to maintain the wider vision.

The complexity of this view of econometrics has implications for the qualities of persons involved with it. In 1948, Frisch outlined the responsibility of econometricians. "Econometrics is a powerful tool, but also a dangerous one. There are so many chances of abusing it, of doing more harm than good with it, that it should only be put into the hands of really first-rate men. Others should be absolutely discouraged from taking up econometrics." The wider view and softness require considerable knowledge. Econometric literacy ceases to be just an awareness of some ideal statistical models and basic manipulative techniques. Our comments in Chapter Two and Four reveal the need for institutional knowledge and for interaction with the providers of data. The essential familiarity with economic theory and computational methods should also be apparent. Clearly these demands limit the ranks of econometricians. Softness may not imply a restriction to first-rate men, as found in Frisch's comment, but it does require more than is involved with hard econometrics.

To illustrate the diversity of soft econometrics, three different topics were chosen as the bases for our three essays. These were the revival of the long wave, the demise of structural estimation and fallible indicators. In choosing the long wave, we were searching for an area in which the value of econometric analyses might appear slight. We found that such analyses have a limited role but a non-negligible one. Conventional regression methods may be used to explore particular topics but they need supplementing with less familiar procedures such as simulation and cluster determination. The efforts of Forrester, Nelson and Winter and others (including, somewhat surprisingly, Rostow) to simulate cyclical phenomena or to explore evolutionary models reveal the dramatic impact of computational advances on econometric practice. Clustering algorithms are valuable in finding significant groupings in time of inventions or innovations as reflected in weak data that are subject to considerable inaccuracies. Spectral analyses, the explorations of time series in the frequency domain, are handicapped by the long periodicity but they have been found effective in clarifying (perhaps tentatively) the presence of waves and the cyclical interaction of different economic variables. Our overwhelming impression is that there is, in fact, considerable scope for econometrics in long-wave research provided it is not restricted to hard elements.

The second topic was chosen because of its closeness to hard econometrics. Thus, at its heart, we find the structural notions of the simultaneous-equation model and the related concepts popularized by associates of the Cowles Commission. Taking this conventional framework, we detected many soft elements and argue that an historical perspective is a valuable means of understanding some current crises in econometric modelling. For example, the atheoretical VAR approach and the challenge of rational expectations are best considered in the light of earlier discussions of theoretical concepts such as exogeneity and autonomy. They should also be connected with the choice among different representations of economic systems and the co-movements found in economic time series. Again valuable insights on these matters may be obtained from early papers of Tinbergen, Orcutt and others. A significant aspect of this historical literature is their acceptance of our wider vision of econometrics rather than the narrower alternative.

Other facets of the issues raised in this second essay are concerned with experimentation and the interaction of time-series analyses and structural methods as in the SEM framework. We argued that, from the wider vision, it is sensible to treat the two distinct schools of experimentation (as represented by Forrester and Naylor on the one side and by Smith and Day on the other) as part of econometrics rather than as alternatives to it. This argument taken to an extreme would permit econometrics to include activities from which statistical inference is absent and data are far removed from cardinal indices. We also argue that time-series analyses can enhance structural specification so that they are complementary to the so-called structural methods. The critical comparison of the two approaches may then be seen as misdirected.

In the final essay, we recall that practical research is generally based on data so that softness in the measurement of economic indicators must surely lead to softness in the econometric methods with their use. Theoretical "default" is apparent in the treatment of time intervals, prior adjustment and inoperational definitions of theoretical concepts. We identify particular difficulties such as, for example, the source of indicators in sample surveys. Identification of these difficulties is essential if we are to aim for the development of better methods. Finding and elaborating the nature of important flaws is a first step in progress. It is quite inappropriate to overlook them and to resort to instrumental assumptions, such as those invoking white noise and other correlative properties, which are convenient for deriving mathematical propositions but severely distort the character of existing data. The problems of measurement can also be linked to a host of ad hoc procedures that have become popular in applied econometrics although incompatible with the bases of conventional statistical criteria they employ. An excellent example of this is given by the search for stable fitted regression lines over alternatives involving different measures of the same broad economic concept.

Finally, it is appropriate to ask where this apparent softness leaves econometrics. Our personal view is that the immediate future looks exciting as we come to terms with the reappraisal of familiar concepts and models and as we begin to address some of the troublesome details that we have indicated in the three essays. The broadening of interest from a preoccupation with mathematical statistics alone (while retaining a pronounced statistical contribution) is a major improvement.

References

BROUWER, F., and P. NIJKAMP (1982), Reduction Methods for Qualitative and Quantitative Data in Regional Analysis, Research Memorandum 21, Economics Faculty, Vrije Universiteit, Amsterdam.

COX, D.R. (1981), "Theory and General Principle in Statistics", Journal of the Royal Statistical Society, Series A, Vol. 144, Part 3, pp. 289-297.

FRISCH, R. (1948), "The Responsibility of Econometricians", Econometrica, Vol. 14, No. 1, January, pp. 1-14.