Recent Methodological Developments in the Quantification of Economic History

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> The methodological hallmarks of the new economic history are its emphasis on measurement and its recognition of the intimate relationship between measurement and theory. (Robert W. FOGEL.)

In this paper we attempt to acquaint the historian with recent developments in "quantitative economic history". We begin with a discussion of the nature of contemporary economic theory and follow this with an examination of some aspects of statistical inference. Some recent important applications of theoretical-quantitative methods are discussed to illustrate the potential of this approach. The paper concludes with some comments on the significance of the new approaches to "traditional" historical studies.

I. — THE NATURE AND USE OF ECONOMIC MODELS.

Because economic theory plays a significant role in contemporary studies in economic history, it is desirable to outline some salient aspects of modern economic theory, and to provide illustrations of its use. [1]

A. THE ROLE OF ABSTRACTION.

While the deductive nature of economic theory has long been recognized, the past two decades have witnessed an emphasis upon clarification and formalization of its logical structure. Economic theory begins with a "model", an abstraction from reality composed of a set of axioms or postulates containing key terms such as "consumer", "firm", "resource", and "product". These axioms must satisfy certain properties in order to be amenable to logical analysis (for example, it is important that they

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1 An excellent discussion of economic methodology, much of which will be of interest to the non-economist, is presented in the elegant second essay of Tjalling C. KOOPMANS, *Three Essays on the State of Economic Science* (New York, 1957), especially pp. 129-150.

be non-contradictory).[2] From these postulates (which might be called "primary" hypotheses), theorems ("extended" hypotheses) can be derived with the aid of logical or mathematical analysis. The theorems which can be deduced from the original axiom set must be logically true if the axioms meet the properties alluded to, and if the deduction process is free of logical error. Unfortunately for the economist, the *logical* truth of these theorems need not imply anything about their "real-world" significance. While it is clear that the "realism" of the underlying axioms is related to the relevance of derived hypotheses to actual economic phenomena, the role of realism in the construction of testable theories involves complex logical and epistemological problems whose detailed examination is beyond the scope of this paper. Nevertheless, the historian's concern about the "unrealism" of economic theory is sufficiently widespread to warrant a brief discussion of the methodology of economics.

A particular methodological position, usually associated with the "Chicago School", has been advocated by Milton Friedman,[3] who contends that, to an appreciable degree, concern over the "realism" of economic axioms is simply immaterial. He argues that "a theory *cannot* be tested by comparing its 'assumptions' directly with 'reality' ",[4] but only by the extent to which its *predictions* succeed. This position has been criticized by those who, with P. A. Samuelson,[5] regard "realism" (inasmuch as it is consistent with tractability) to be a desirable feature of axiom systems.[6] Much of the controversy raised by Friedman's argument has recently been analyzed by Bear and Orr.[7] Using standard techniques of logical analysis they conclude that

[2] Karl R. Popper, *The Logic of Scientific Discovery* (New York, 1961), pp. 71-72, presents a summary of these conditions. Further terms may be included in the process of deduction to supplement those incorporated in the "fundamental" postulates. In our subsequent discussion we will have occasion to interpret a model in terms of a system of equations where these are intended to depict the behaviour of aggregates of firms and individuals.

[3] Milton Friedman, the first essay in *Essays in Positive Economics* (Chicago, 1966), pp. 3-43. This essay is not narrowly addressed to the economist, and should be of interest to anyone concerned with the methodological problems of the social sciences.

[4] *Ibid.*, p. 41. (Our emphasis.)

[5] See, for example, P. A. Samuelson's contribution to a discussion of methodological issues in *American Economic Review*, LIII (1963), 231-236, and "Professor Samuelson on Theory and Realism : Reply", *American Economic Review*, LV (1965), 1164-1172.

[6] One simple, but basic, criticism which has been levied against the Friedman methodology is that since a proposition implies itself, the axioms themselves are a part of the set of their implications, and therefore should not be exempted from empirical testing (Koopmans, *Three Essays on the State of Economic Science*, p. 139).

[7] D. V. T. Bear and Daniel Orr, "Logic and Expediency in Economic Theorizing", *Journal of Political Economy*, 75 (1967), 188-196.

disregard for the truth of specific antecedent conditions... removes the possibility of testing the theory from which the conclusions are drawn. A scientist is concerned with *how* things happen, not only with *what* happens, and the Friedman methodology makes it impossible effectively to pursue that concern. [8]

Bear and Orr appear to be in substantial agreement with Popper, whose views, as they point out, have had an appreciable impact upon attitudes towards the construction and testing of theories. Popper views *falsifiability* as a key element in theory construction, and stresses the inappropriateness of a preoccupation with the predictive ability of theories. [9] In economics, Samuelson was an early proponent of this type of approach, which by now has become well established. In one of his most influential works, the goal of the theorist was conceived to lie in the elucidation of "operationally meaningful theorems", defined as hypotheses about empirical data which could, at least conceivably, be refuted. [10]

While there is not the same agreement concerning the appropriate extent of "realism", no economist denies the need for *abstraction* from the almost overwhelming complexity of economic reality. Abstraction, which is a form of "unrealism", is clearly necessary to make theorizing tractable, and to the extent that Friedman stresses this aspect of theory construction, no economist would cavil. As Bear and Orr point out, the "trade-off" between tractability and realism involves, basically, rather pragmatic considerations :

any features in the environment under study that are abstracted are treated "as if" unimportant; ...adoption of [this] procedure does not imply indorsement of the position that the truth of the assumptions is irrelevant; rather, it implies a concern to get on with the generation of testable prediction statements. [11]

8 *Ibid.*, p. 191.

9 POPPER, *The Logic of Scientific Discovery*; the passages, pp. 27-48 and 78-92, discuss the role of "falsifiability" in testing theories. His essay "Three Views Concerning Human Knowledge", reprinted in Karl. R. POPPER, *Conjectures and Refutations* (New York, 1963), pp. 107-114 especially, presents a critique of "instrumentalism" (the undue preoccupation with the [apparent] predictive ability of theories).

Throughout his writings, the emphasis is upon the search for *refutation* of theories (as opposed to verification, which he regards as impossible and hence meaningless) and upon the *tentative* nature of the "acceptance" conferred by absence of refutation.

10 Paul Anthony SAMUELSON, *Foundations of Economic Analysis* (Cambridge, Mass., 1947), p. 4.

11 BEAR and ORR, "Logic and Expediency in Economic Theorizing", p. 195. The idea of a "prediction statement" in this context is similar to that employed by R. L. Basmann, which we will have occasion to examine in Section II.

Our aim, so far, has been to present the historian with a sketch of some methodological aspects of contemporary economic theory. We conclude with Koopmans' summing-up of the meaning of the axiomatic approach and of the role of model-building :

> neither are the postulates of economic theory entirely self-evident, nor are the implications of various sets of postulates readily tested by observation... It is desirable that we arrange and record our logical deductions in such a manner that any particular conclusion or observationally refutable implication can be traced to the postulates on which it rests... We should look upon economic theory as a sequence of conceptual *models* that seek to express in simplified form different aspects of an always more complicated reality... The study of the simpler models is protected from the reproach of unreality by the consideration that these models may be prototypes of more realistic, but also more complicated, subsequent models... Unless rigor follows along to consolidate the gains in realism, we shall not know which conclusions or recommendations depend on which postulates, and which postulates depend for their validity on which verifications of their implications by accumulated experience.[12]

B. The Use of Economic Models to Generate Qualitative Predictions.

In order to attach concreteness to our remarks on methodology and especially in order to provide some necessary background for our discussion of the use of statistical inference, we will present a few illustrative economic models. These models, while too simple to be of much practical use, have considerable heuristic appeal since they bear a distinct family resemblance to analytical models which are in widespread use.

Microeconomic theory deals with "rational" decision making by basic economic units, which are firms, consumers, and resource holders. From postulates which depict the nature of the decision process of these idealized units[13] certain important (and refutable) results can be derived. In the theory of consumer behaviour an important derived relationship is that between the quantity demanded of a commodity on the one hand, and prices and income on the other. It is possible to make qualitative inferences about the nature of this derived relationship. It can, for example, be unambiguously deduced that if the price of a particular commodity should rise, all other prices and real income constant (for present purposes, "real income" can be taken to mean income in constant dollars), the quantity demanded of the commodity will fall. Once such

12 Koopmans, *Three Essays on the State of Economic Science*, pp. 142-143.

13 In the case of the rational consumer, for example, one postulate is that choices are consistent in the sense of being "transitive".

a relationship has been derived for each demander of the product, aggregation of these individual relationships will yield a market relationship (although complex problems can arise in attempts to achieve consistency between individual and aggregate relationships). We can depict the market demand relationship, algebraically, by (1). Through an analogous process, we can deduce that under certain circumstances the supply relationships for individual firms can be aggregated to yield an industry supply function, represented by (2). Here q^d denotes the quantity demanded and q^s the quantity supplied, per period of time, p denotes the price of the commodity ("deflated" by some overall price index whose precise nature is not relevant here), y represents real income per period, and w represents the systematic effect of weather on supply. Equation (3) is an "equilibrium condition", and indicates that in order for the system to be in equilibrium the quantities supplied and demanded must be equal. We can define "equilibrium" for our purposes as a state of affairs where the variables in the model are not in the process of change. That is, it is a state where the quantities that sellers wish to sell (at specified prices, as indicated by the supply equation) are equal to what in fact they do sell, and where the quantities that demanders wish to buy (at specified prices and real income, as indicated by the demand equation), they do in fact buy.

$$q^d = \alpha + \beta p + \gamma y \tag{1}$$

$$q^s = \delta + \varepsilon p + \theta w \tag{2}$$

$$q^s = q^d \tag{3}$$

Our two *behavioural* relationships, (1) and (2), are said to be *linear* in the variables (price, quantity, weather, and income).[14] The Greek letters represent the parameters of our supply and demand functions. These parameters (or constants) are presumed to have values (which generally are unknown), but it is possible to proceed rather far *without* such data. Of course, for certain applications of a model, numerical

[14] In order for an equation to be linear in the variables, these must enter only in the "first degree". That is, the exponent of a variable must be unity, which excludes terms such as p^2, p^3 or $\sqrt{y}$. Moreover, the variables must enter in an additive fashion. Thus, while the exponents in $q^d = \alpha p y$ are all unity, the equation is not linear because p and y enter multiplicatively rather than additively.

We should emphasize that microeconomic theory generally provides little guide as to which functional form is most appropriate. Quite frequently linear forms are convenient and useful approximations to non-linear ones. Because linearity is also convenient for expository purposes, we will use it extensively in our discussion.

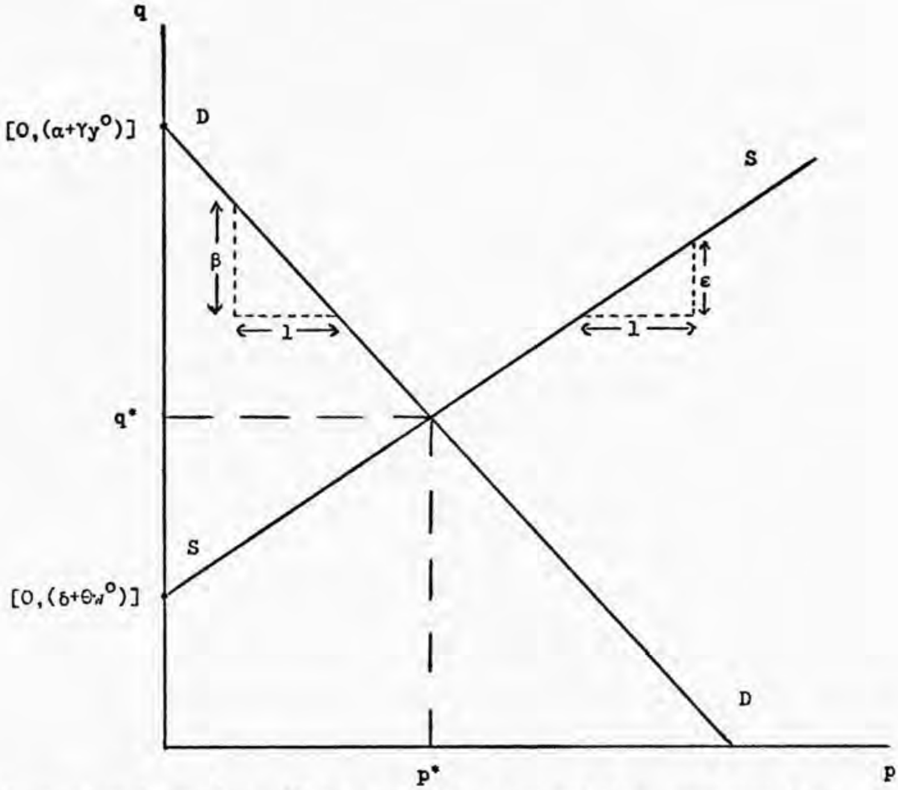
parameter values are required, but this raises the question of statistical estimation, which we defer to Section II.

As we indicated earlier, it is possible to infer that when price increases, other prices and real income remaining constant, the quantity demanded by the individual will fall. Abstracting from certain complications which are not relevant here, this will also be true in the aggregate, implying that β (which represents the change in q per unit change in p, with income constant) should be negative. Moreover, for many commodities we would expect that with a rise in income, everything else constant, the quantity demanded will rise. This means that γ (which represents the change in q per unit change in y, with prices constant) should be positive. In the course of deriving our supply function, we can deduce that for each firm the quantity supplied will increase with an increase in price. Again, we would expect this to be true for the industry as a whole, and consequently we expect ε to be positive. We will assume θ to be positive (if w represents inches of rainfall, for example, then a positive θ means that with prices constant, an increase in rainfall will result in an increase in the quantity supplied). Finally, since negative prices and quantities have no economic meaning in this context, we will constrain them to be non-negative. These *a priori* restrictions on our model can be summarized respectively, with a compact notation.

$$(4) \qquad \beta < 0, \quad \gamma > 0, \quad \varepsilon > 0, \quad \theta > 0, \quad p \geq 0, \quad q \geq 0$$

The nature of the model depicted by (1) — (4) can also be illustrated with the familiar supply-demand diagram of Figure 1. We are in fact depicting cross-sectional relationships between price and quantity, with the other variables (weather and income) held constant in the background. In order to draw these graphs it is necessary to assume specific values for y and w, and these are shown by w° and y°. We might note that we could equally well have chosen to put price on the vertical axis, but its horizontal representation is more useful heuristically. The geometric interpretation of our parameters as slopes or intercepts is clear from the diagram, and the intersection corresponds to the algebraic solution of the simultaneous equations represented in (1) — (3) with asterisks denoting equilibrium values. A complete geometric interpretation of our demand and supply equations would necessitate drawing planes in three-dimensional space. Consequently, to allow us to use the simpler diagram of Figure 1, we have suppressed the other two variables,

Figure 1



income and weather. Their presence is nonetheless apparent from the two intercept terms.

We must distinguish between two classes of economic variables. "Endogenous" variables are those whose values are determined by the model, while "exogenous" variables are those which are determined outside the model. Exogenous variables may be non-economic in nature or they may be economic variables which are assumed to be determined by forces other than those depicted by the model. A model is said to be a "partial" one if, as in our example, some economic variables are left "unexplained" (income and weather, above), but even the most general economic models are partial in a broader sense :

> economists are investigating what is at most a large subset of the complete set of equations that describe the social behaviour of man and the laws of the physical environment.[15]

[15] Franklin M. FISHER, "Selective Estimation and the Dilemma of Objectivity", in *A Priori Information and Time Series Analysis* (Amsterdam, 1962), p. 5.

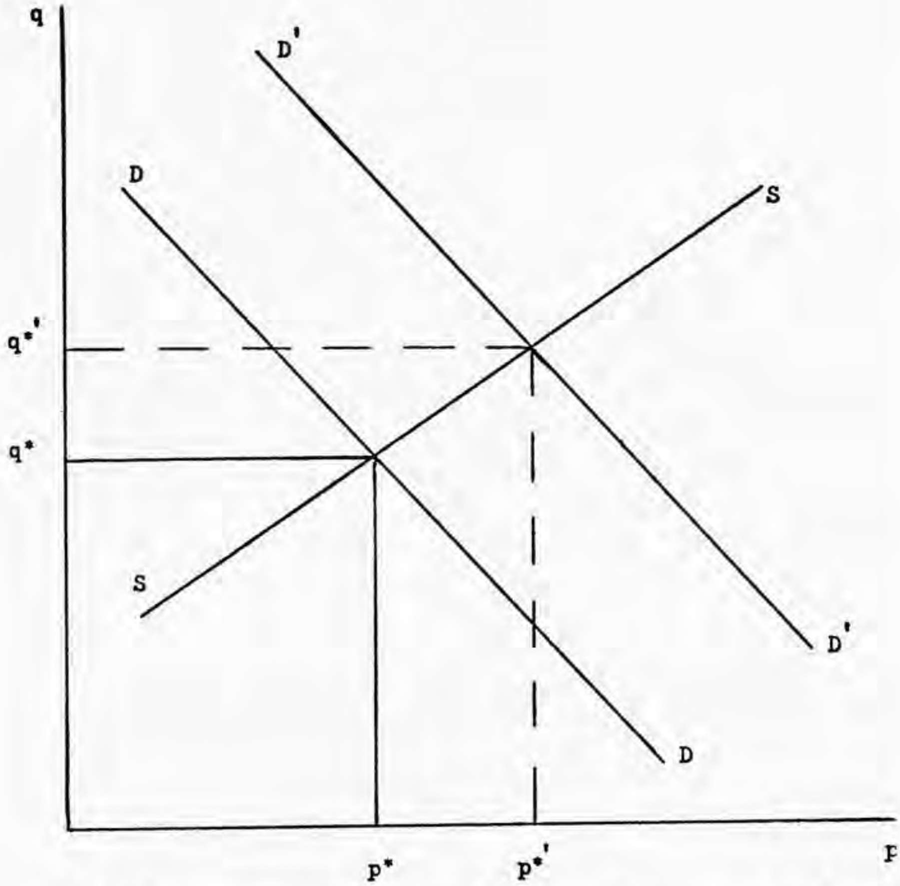
In our supply-demand model we have assumed that activity in the market in question affects price and quantity, but not income or weather, which are assumed to be determined elsewhere. Thus price and quantity here are endogenous while weather and income are exogenous.[16]

Let us now suppose that there is an increase in an exogenous variable. Suppose, for instance, that income increases. It is possible to demonstrate that both p^* and q^* will increase as a result. This is a *prediction* generated by our model, and it is important to recognize that such predictions are *qualitative* in nature. While the direction of effect can be inferred, the magnitudes involved depend upon the parameter values and, of course, upon the values of the exogenous variables. It is necessary, however, to impose *a priori* restrictions upon the signs of the parameters in order for qualitative predictions to emerge. This type of analysis, wherein one equilibrium position is compared with another (the latter resulting from the shift in an exogenous variable or in a parameter) is called "comparative statics".

Once again the use of the supply-demand diagram will facilitate an understanding of the processes involved. If income were to increase, the demand curve would shift rightward to a position such as that depicted by D'D' in Figure 2, resulting in a higher equilibrium price ($p^{*\prime}$) and in a higher equilibrium quantity ($q^{*\prime}$). Similarly, a decrease in w would shift the supply curve rightwards, resulting in a higher equilibrium price but in a *lower* equilibrium quantity. That is, given our interpretation of w, a decrease in rainfall decreases supply (that is, shifts the whole curve rightward), resulting in a lower equilibrium value of q and a higher p. We could also analyze, geometrically, the implications of varying a parameter. Thus, if for example the slope of the supply curve were to increase, this curve would rotate to the left about its intercept on the vertical axis, resulting in a higher equilibrium q and a lower equilibrium price (as compared with the initial equilibrium values). When the number of endogenous variables become larger than two the analysis is usually more readily done with non-geometric methods.

16 A model of the type illustrated is known as a "simultaneous model". These are comprised of a set of simultaneous equations, where, given values for the parameters and the exogenous variables, those of the endogenous variables emerge simultaneously. A system of *linear* simultaneous equations has a unique solution if the equations are independent (none is derivable from the rest) and consistent (no equation contradicts another), and if the number of equations equals that of the unknowns (the endogenous variables in the present context).

Figure 2



Not all comparative statics is conducted in the context of models of markets or industries, let alone with models of the individual consumer or firm. Much of this type of analysis is, in fact, directed towards the determination of aggregates : totals (or averages) of national income, output, employment, investment, and so forth. Among the relations which enter into such models are "consumption functions" (relationships between consumer spending and personal income), investment functions, and equilibrium conditions analogous to (3). The analysis of the determination of these aggregates is the province of *macroeconomic theory* and this too is amenable to the methods of comparative statics when, for example, we wish to contrast the levels of national employment before and after a change in some variable such as government spending.

By now the historian will have probably begun to be concerned about the absence of a role for *time*. Static theory is, by definition, timeless, and entails the examination of alternative equilibrium positions, without regard to the "time paths" of economic variables as they make the transition from one equilibrium value to another. It lies within the domain of *economic dynamics* to analyze these time paths. Certain salient features of dynamic processes can be illustrated with a dynamic model drawn from elementary macroeconomic theory. The first expression (5) states that current consumption expenditures (c_t) depend upon income in the previous period[17] (y_{t-1}). The next expression (6) *defines* current income as the sum of current consumption and non-consumption expenditures. For expository convenience we will assume that non-consumption spending (z) is entirely exogenous (given by mainly non-economic factors such as the "psychology of investors"), and we will also assume that this variable is constant over time and hence can be represented without need of a time subscript (that is, $z = z_t = z_{t-1}$, and so forth).

(5) $$c_t = \alpha + \beta y_{t-1}$$

(6) $$c_t + z_t = y_t$$

Some very simple algebraic steps will now permit the completion of our analysis. From the "income identity" (a definitional relationship) shown by (6) it follows that $c_t = y_t - z$, and if we substitute this for c_t in (5), we obtain an equation in current and lagged income, as shown by (7).

(7) $$y_t - \beta y_{t-1} = \alpha + z$$

This can be written more compactly by replacing $\alpha + z$ with the single symbol k, in which case it becomes

(8) $$y_t - \beta y_{t-1} = k$$

This relationship is an example of a first-order linear *difference equation*, whose properties can be ascertained by standard analytical methods.[18]

[17] For example, if t is 1922, then c_{t-1} represents consumption expenditure in 1921. For facility of exposition, we will use "current period" or "current" as synonymous with (an unspecified) period t.

[18] The behaviour of income over time depends upon the values of β and z. The extent to which *a priori* information on these is available to the economist determines the extent to which he can make inferences concerning the time path of the economic variable in question. One possibility is that y "explodes", changing without bound with the passage of time; another is that y "dampens" towards some stable (equilibrium) value; yet another possibility is that y alternates, lying above an equilibrium value during one period and

For our purposes the important point is that once a value for income in some period (t-l) is specified, we can readily obtain the value of income for the next period (period t), provided we know the values of β and z. A similar application of the equation allows us to substitute the value of income in period t to obtain the value for t + 1, and so forth. In other words we can obtain the time path of y once we know its initial value, regardless of which period is chosen as the "initial" one.[19]

Economic theory can sometimes be fruitfully combined with simple statistical techniques (possibly involving only descriptive measures such as averages) to generate and test *quantitative* predictions from a model. An excellent illustration of the fruitful use of this approach is the study by Paul David[20] on the diffusion of the mechanical reaper in United States agriculture in the 1850's. Although the reaper had been invented in the 1830's, its widespread adoption occurred only in the mid-1850's. As David points out, traditional accounts of this time lag and the sudden surge of adoption stress the importance of the rise in world wheat prices during the decade preceding the Civil War, or imply that the shift into small-grain farming in the old Northwest involved a move to terrain which necessitated mechanization; it has even been suggested that the American farmer was inclined to increase acreage under cultivation regardless of profitability calculations. David developed two distinct, although not mutually exclusive, hypotheses : first, that the primary causal mechanism was the rise in the world price of grain; secondly, that mechanization was caused by a downward shift in the price of reapers relative to labour costs (as a result of competing demands for labour in activities such as railroad and urban construction). With the use of

below that value in the succeeding period. Even if the exact values of β and z are not known, it is possible to infer whether the time path of y falls into one or another of the above categories if we know something about the nature of the *range* in which β and z fall. For example, it would be useful to know that β is greater than or equal to zero but less than one, and to some degree, at least, these restrictions can be deduced from the basic economic assumptions of the model.

[19] Considerably more complex dynamic models are commonly used in economic analysis. Even "purely theoretical" models, which are intended to yield qualitative predictions, usually involve an appreciably greater degree of complexity than does our heuristic model, and the economy-wide *quantitative* models used for forecasting or policy analysis involve a large number of equations with a concomitantly large number of endogenous and exogenous variables. We will have occasion to refer to such models later in another context.

[20] Paul A. DAVID, "The Mechanization of Reaping in the Ante-Bellum Midwest", in *Industrialization in Two Systems : Essays in Honor of Alexander Gerschenkron*, ed. Henry Rosovsky (New York, 1966), pp. 3-39. This is one of the most impressive of several recent papers on the general theme of technological change and productivity increase in the nineteenth century American economy.

economic cost curve concepts, the notion of a "representative" farm, and the concept of a "threshold" farm size (defined as that acreage sown to small grains beyond which it would increase profits to mechanize reaping), David deduced the implication of the first hypothesis to be an increase in the acreage sown, and that of the second to be a lowering of the "threshold" size to the initial "optimal" size. On the basis of some simplifying assumptions he was able to develop, as a measurable concept, the notion of a "threshold function",[21] which related acreage in small grains to the ratio of reaper price to labour costs incurred from harvesting with the older technique. In a short period from 1849-53 to 1854-57, the disparity between "threshold" size and average acreage was drastically reduced, with some two-thirds of the reduction attributable to a decreased "threshold" size (resulting from the shift in relative input prices), and about one-third attributable to increased acreage in small grains (resulting from the rise in world grain prices). Thus, while both causal mechanisms were operative, the change in relative factor prices apparently had the more pronounced effect on the widespread adoption of the mechanical reaper.

One of the branches of economics most frequently applied to topics in economic history has been capital theory, particularly the concept of economic (as compared with accounting) profitability as measured by the "internal rate of return". Beginning with the pioneering study of slavery by Conrad and Meyer,[22] the rate of return concept has been skillfully employed in assessing the contribution to United States economic development of such diverse innovations as railroads,[23] clipper ships,[24] and hybrid corn.[25]

21 David's use of economic theory to overcome data limitations is an excellent example of the increased efficiency of data use made possible by quantitative techniques. See *ibid.*, Appendix, pp. 28-39, and discussion of this point in R. W. Fogel, "The New Economic History : Its Findings and Methods", *Economic History Review*, Second Series, XIX (1966), 653.

22 Alfred H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante-bellum South", *Journal of Political Economy*, 66 (1958), 95-130. Reprinted in Conrad and Meyer, *The Economics of Slavery* (Chicago, 1964), pp. 43-92.

For our purposes, we can define the "internal rate of return" as the ratio of the time stream of net earnings of a capital good to the original cost of the good.

23 Robert W. Fogel, *The Union Pacific Railroad : A Case in Premature Enterprise* (Baltimore, 1960) and *Railroads and American Economic Growth : Essays in Econometric History* (Baltimore, 1964); Albert Fishlow, *American Railroads and the Transformation of the Ante-Bellum Economy* (Cambridge, Mass., 1965).

24 R. Evans, Jr., " 'Without Regard for Cost' : The Return on Clipper Ships", *Journal of Political Economy*, 72 (1964), 32-43.

25 Zvi Griliches, "Research Costs and Social Returns : Hybrid Corn and Related Innovations", *Journal of Political Economy*, 66 (1958), 419-431.

Conrad and Meyer, for example, addressed themselves to the controversy over the profitability of slavery in the United States South before 1860. The economic basis of the employment and ownership of slaves had received considerable attention from "traditional" historians. Despite this attention, however, no consensus had been reached on the key issue of whether the slave system was inefficient and hence economically unprofitable (relative to alternative investments) and likely therefore to decline even without abolition, or whether instead the system remained profitable to 1860. The dispute had never been satisfactorily resolved because the arguments on each side were founded only on *impressions* of the profitability of slavery gleaned from contemporary literary sources and plantation records. Conrad and Meyer employed capital theory and elementary statistical manipulations to determine that, in fact, slavery was *profitable* in the ante-bellum South. That is, the return on slaves as a form of capital was comparable with returns on other assets.

The work of Robert W. Fogel on the Union Pacific Railroad involved a major reinterpretation of its financial history and developmental impact. The general interpretation, in American historiography, of the cause of the financial problems of the Union Pacific had centered around the charge that the promoters of the railroad were guilty of profiteering during its construction, and that their profiteering weakened it financially to an extent that rendered bankruptcy inevitable. Fogel first re-examined the charge of "profiteering" and, given his estimate of "justifiable" profits to the promoters, concluded that these charges were much exaggerated. Moreover, the financial difficulties of the railroad were found to result primarily from its financial structure and particularly from its large bonded debt, and in part from the government's provision of financial assistance in the form of bonds. Secondly, and more significantly, the financial problems of the Union Pacific were found to have disguised the *economic* profitability of the project, compared with its failure according to *accounting* concepts of profitability. Fogel's calculation of the "private rate of return", computed from data on costs of construction, demonstrated that the project was *ex post* very successful by economic criteria and could have been built by private enterprise unaided. However, under prevailing economic conditions, the Railroad probably would not have been constructed without government assistance. In such circumstances, the government's decision to intervene appears economically rational, judged by the high "social rate of return" (about 30 per cent between 1870-1879) attaching to the project. [26]

Fogel's *Railroads and American Economic Growth : Essays in Econometric History* is widely regarded as an outstanding example of the "new" economic history. Fogel was expressly concerned in this work to examine systematically the undisputed acceptance in American historiography of the "axiom of indispensability" [27] — the implicit assumption that the nineteenth century United States economy lacked a viable transport alternative to the railroad. The contribution of the railway to the American economy in 1890 was the "social savings" [28] estimated to accrue from the shipment of freight via the interregional and intraregional railroads. Railroad "social savings" amounted to approximately 4.7 per cent of Gross National Product in 1890. [29] Fogel points out that, although the railroad was the most efficient form of transportation open to the economy, the data failed to

> establish a *causal* relationship between the railroad and either the regional reorganization of trade, or the change in the structure of output, or the rise in per capita income, or the various other strategic changes that characterized the American economy of the last century... [and the evidence

[26] On occasion, "private rate of return" is used synonymously with "internal rate of return". The numerator in the "social rate of return" calculation is equal to the net earnings of the railroad plus the "unpaid benefits" — "the increase in national income brought about by the road but which failed to be reflected in the company's net earnings" (FOGEL, *The Union Pacific Railroad*, p. 98). The economic theory of rent formed the basis for estimating the "unpaid benefits" which accrued from the opening up of lands through which the railway was constructed.

[27] The "axiom of indispensability" is discussed in FOGEL, *Railroads and American Economic Growth*, pp. 1-16. The "axiom" received perhaps its finest enunciation in an article by Leland H. JENKS, "Railroads as an Economic Force in American Development", *Journal of Economic History*, IV (1944), 1-20.

[28] "Social savings" is defined as "the difference between the actual level of national income in 1890 and the level of national income that would have prevailed if the economy had made the most efficient possible adjustment to the absence of the... railroad" (FOGEL, *Railroads and American Economic Growth*, p. 20). For a discussion of the concept, see pp. 20-29 and 52-58.

Here, Fogel has made use of a "counterfactual conditional", that is, a comparison between what actually happened and what *would* most likely have occurred in the absence of the development of the railroad. In order to determine what *would* have happened, the economic historian must deduce the "counterfactual" with the aid of a theoretical model. As Fogel has suggested, one difference between the "old" and "new" economic history lies not in the frequent use of "counterfactual" propositions in the "new", but in the extent to which such propositions, implicit in more traditional studies, have been made explicit and tested empirically. See R. W. FOGEL, "The New Economic History : Its Findings and Methods", pp. 653-656, and A. H. CONRAD and J. R. MEYER, "Economic Theory, Statistical Inference and Economic History", in *The Economics of Slavery*, pp. 23-24.

[29] FOGEL, *Railroads and American Economic Growth*, p. 223. Albert Fishlow suggests that a more plausible estimate, based on extrapolation to 1890 of his results for the pre-1860 period, would be in the range of 10 to 15 per cent of Gross National Product. See FISHLOW, *American Railroads*, p. 61.

failed even to] establish the weaker proposition that railroads were a *necessary* condition for these developments.[30]

This brief survey has overlooked many significant recent contributions to American economic history.[31] We have selected certain studies which particularly emphasize the role of economic theory in formulating empirically testable hypotheses. However, these studies are essentially non-econometric. Some writers, notably Fogel, Conrad and Meyer, and Jeffrey Williamson have employed methods of econometric analysis. In the next section we outline the nature of econometric models and consider their significance for the analysis of historical phenomena.

II. — APPLICATION OF ECONOMETRIC TECHNIQUES.

Much of econometric analysis deals with the behaviour of economic variables over time, and it is natural that its relevance to the study of economic history should have been apparent to econometricians who are not primarily concerned with historiography. Thus, the eminent econometrician Lawrence Klein writes that

> since the data used in econometrics are obtained by observing actual economic processes, we may conclude that econometrics is a way of studying history — a very systematic way... The econometrician tries to piece together the fundamental aspects of economic behaviour by looking at the interrelationships of the quantitative magnitudes generated historically, and then tries further to extrapolate past behaviour... even without... extrapolation...

Marc Nerlove, "Railroads and American Economic Growth", *Journal of Economic History*, XXVI (1966), 112-115, takes Fogel to task: "instead of asking whether the contribution of railroads to American economic growth was great or small *in toto* and absolutely, we should ask whether it was great or small at the margin and relatively. That is to say, we should ask whether the marginal social return... was greater, equal or less as compared to the marginal social return on other forms of investment" (*ibid.*, p. 112). Nevertheless, Nerlove's calculations (based upon admittedly shaky data) confirm Fogel's conclusions regarding the impact of railroads. (In any event, in correspondence with Nerlove, Fogel has questioned the use of the marginal concept as the sole criterion of "contribution" in this context.)

[30] Fogel, *Railroads and American Economic Growth*, p. 15. (Our emphasis.) In fact, Fogel demonstrated that adaptation of the economy to a non-rail transportation technology was feasible. The loss of the railroad in 1890 would have lowered the area of commercial agricultural land by 24 per cent. An improved internal navigation system would have reduced the loss to 7 per cent of 1890 acreage; allowing for improvement of country roads as well, only 4 per cent of 1890 commercial agricultural acreage would have been withdrawn from cultivation in the absence of the railroad (*ibid.*, pp. 91-110).

[31] Among them are Lance Davis' studies of the evolution of the American capital market, William Parker's examination of productivity change in grain production, Robert Gallman's estimates of commodity output and Gross National Product in the nineteenth century, the work of Fogel and Fishlow on interregional trade, and Douglass North's application of the staples approach to American development patterns before 1860.

econometrics is interesting as a mere study of the past to show how quantitative magnitudes interacted at that time.[32]

A. Estimation of Econometric Models.

The essence of the formulation of econometric models lies in the incorporation of random components into relationships obtained from economic theory.[33] The nature of the problem facing the econometrician can be illustrated through use of the supply-demand model given by (1) — (3). The subscripts simply denote values at time t, and the absence of lagged values implies that the model is static. If we assume that the market is in equilibrium ($q^s = q^d = q$) the equations become

$$q_t = \alpha + \beta p_t + \gamma y_t + \mu_t \tag{9}$$

$$q_t = \delta + \varepsilon p_t + \theta w_t + v_t \tag{10}$$

Both "structural equations"[34] now contain a new variable, the *stochastic component* of the relationship (μ_t in (9) and v_t in (10)). The stochastic variable is a catch-all, and is incorporated to take account of the fact that our hypotheses exclude many equations and variables which (hopefully) have only a random impact upon our variables. Moreover, to the extent that there might be a "fundamentally" random component in socio-economic behaviour, this too is assumed encompassed in the random term. In the demand equation, the *exact* (systematic) component of the relationship is represented by $\alpha + \beta p_t + \gamma y_t$ and by $\delta + \varepsilon p_t + \theta w_t$ in the supply equation.

In the case of the demand relationship (9), for example, we assume that within the underlying universe from which our data have been "drawn" there is a functional relationship between quantity demanded, price and income. Linear forms are often convenient for statistical

32 Lawrence R. Klein, *A Textbook of Econometrics* (Evanston, Illinois, 1953), p. 2. Klein is careful to stress that econometric extrapolation is not "mechanical". An "explanatory" mechanism is incorporated into the model, and thus the use of the model to forecast future activity is not a simple projection of trends.

33 A. H. Conrad and J. R. Meyer, "Economic Theory, Statistical Inference, and Economic History", and "Statistical Inference and Historical Explanation", in *The Economics of Slavery*, pp. 3-40, present a very interesting discussion of the role of statistical inference in historical analysis.

34 These equations depict the postulated "structure" of the model, where "structure" is defined by the functional forms of the equations (linear in this case), and by all the available *a priori* information concerning parameter values. In our model, for instance, one piece of such information is that the coefficient of the income variable in the supply equation is zero. In other words income is absent from (10). Similarly, the coefficient of the weather variable is zero in (9).

reasons and, as we noted earlier, they can frequently be used to approximate non-linear relationships. In addition to assumptions concerning the functional form of the equations and the choice of variables, it is now necessary to make certain assumptions concerning the random term.[35]

One of the chief tasks of econometrics is to provide methods by which the parameters of the structural equations can be *estimated.* In our model these are (α, β, γ, δ, ε, θ) and certain parameters of the probability distributions of μ and ν.[36] There is not scope here for a discussion of the rationale of alternative estimation techniques,[37] and so let us simply accept that the results of one of these methods yielded the following :

(11) $$q_t = 50 - 1.5\, p_t + 0.50\, y_t$$

This result indicates that *in a specific sample of data* a unit increase in price was associated on the average with a 1.5 unit decrease in quantity demanded (assuming income constant), and that a unit increase in income (assuming price constant) led to an increase in quantity demanded of 0.50 units on the average. The significance of such a finding lies partly in that it provides us with an indicator of the "degree of responsiveness" of demand to variables such as price and income.[38] Moreover, a comparison of results obtained from different time spans might yield interesting results on the extent to which consumption patterns have changed over time (say, with industrialization and urbanization). Estimation methods also allow the computation of measures of the "goodness of fit" of an equation to the data. Certain of these measures require parameter estimates for their use and interpretation. While the matter is rather complex, we can say that in some sense the better the goodness of fit the

[35] Statistical tests can, under some circumstances, serve as indicators of the extent to which these assumptions are met.

[36] Generally the only parameter to be estimated of the postulated probability distribution of a random term is its variance (a measure of its dispersion). The mean is assumed to be zero.

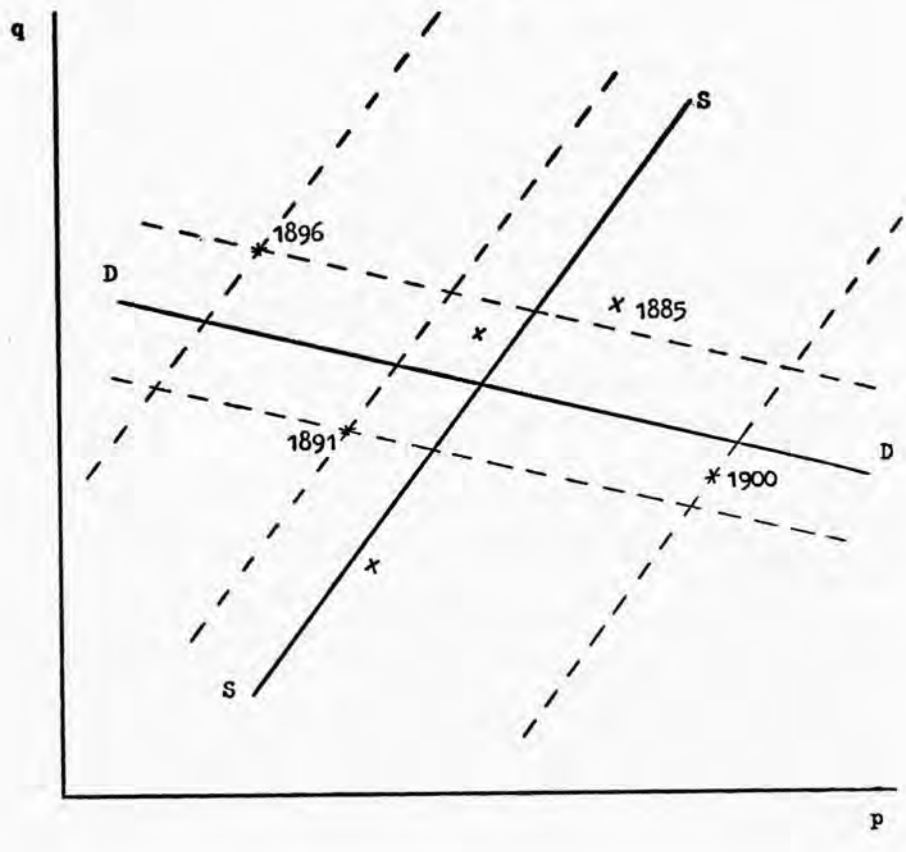
[37] The celebrated method of "classical least squares" is frequently employed but there are many circumstances where the validity of its use is questionable, particularly if the relationship is one involving causation in both directions within the equation rather than from the right to the left-hand side only. In such instances "simultaneous estimation" methods should generally be used.

[38] The way in which the units are measured is of no significance here. For example, quantity might be measured in millions of bushels, price by some index number, and income in thousands of 1939 dollars. However, to obtain a meaningful estimate of the degree of responsiveness it is necessary to work with a measure which is invariant with respect to units of measurement, and consequently some variant of the "elasticity" concept is generally employed.

greater the "explanatory power" of the equation (in terms of its ability to account for observed movements in the endogenous variables).

The new situation confronting us in (9) and (10), as compared with (1) — (3), is depicted graphically in Figure 3 where we have shown a "scatter" of observations on price and quantity. These observations are assumed to have been drawn over time (and to emphasize this we have arbitrarily attached years to some of the points). Moreover, to preclude the need for three-dimensional depictions, we have simply assumed that income and weather have not varied over the period in question. The absurdity of this assumption in most historical studies is obvious enough, but it enables us to concentrate on what, for the present, are more important issues.

Figure 3



We have shown the systematic part of the "true" demand and supply schedules which underlie our data. We cannot observe these and we wish to use our data to *estimate* them. With w and y assumed constant, any variations in the demand curves are a manifestation of changes in μ and ν, the random terms. The actual observations involve intersections of the schedules (which can be thought of, for heuristic purposes, as being subjected to "movements" resulting from changes in their random components). Clearly, if both curves "jump about" in equally volatile fashion, "disentangling" the equations would not be feasible. This is a manifestation of a complex problem known as "the identification problem", which logically precedes statistical estimation. We have arbitrarily presented a situation in Figure 3 where (as may be the case with certain agricultural commodities) supply exhibits appreciable random movements over time (perhaps as a result of weather conditions which are random in their nature and consequently not incorporated into w), while demand is relatively stable. Such a situation "traces out" a demand curve but does not allow us to estimate the supply curve. Only the former curve is said to be "identified".

Parameter estimates may also be used to *test hypotheses* concerning true parameter values. [39] To give a simple illustration from *a priori* economic theory, we know that the coefficient of the price variable (β) should be negative, and we obtained a value which is indeed negative. Does this imply that our *a priori* theory has not been refuted ? The answer is "no", for it is possible (that is, the probability is not zero) to obtain parameter estimates which are non-zero even though β is zero. The usual procedure in this type of hypothesis testing is to formulate the decision rule that, when the probability of obtaining values such as the one obtained from a universe where β is zero is "very small" (a probability, say of 0.01), then β is assumed to be non-zero. In our case this would mean that β is regarded as negative. This is, however, only a very tentative "acceptance" of our theory, for we have merely rejected the "null hypothesis" [40] that the true price effect is zero. In rough outline

[39] In order to test a hypothesis concerning β (for example that $\beta=0$) we need not only an estimate of β, but also an estimate of the variability (usually the standard deviation) of the distribution of estimates of β which arises from repeated (at least conceptually repeated) sampling.

[40] This term frequently arises in applications of statistical inference. For our purposes it suffices to define it as the hypothesis which we are concerned with testing. Because it often entails the absence of the effect in question (in our example an absence

the above procedure is that which is typically followed in the analysis of econometric models.[41]

B. Econometrics and History : Some Recent Methodological Developments.

Much of economic analysis is predicated upon the *ceteris paribus* clause, and econometrics is no exception. It is vitally important to be able to assume, validly, that certain "background conditions" remain essentially constant :

> because of our inability to perform controlled experiments ourselves in the socio-economic area, we are forced to select from the experiments performed for us by Nature those which are at least approximately controlled.[42]

It is at this juncture that the training and approach of the economic historian is crucial for the elucidation and critical examination of the nature of these background conditions. According to Basmann,

> to every explanatory economic model there corresponds a set of more or less definite background or external conditions that must be fulfilled in a given period of economic history if observations recorded for that period are to be deemed appropriate for predictive testing of that model. Those conditions, which must remain approximately constant over the period in question, are suggested in part by the structure of the model... the conceptualization of relevant background conditions is a task for which the economic historian *qua* historian is equipped by training, experience, and general point of view, at least potentially.[43]

This emphasis upon the derivation of "prediction-statements", let alone upon the role of the economic historian, "as peer [of the econo-

of a price effect) the adjective "null" is employed. When the null hypothesis is a contradiction of our *a priori* theory we are hopeful of being able to reject it. Implicitly, the null hypothesis is tested against some alternative (here the alternative hypothesis is $\beta<0$).

41 We have bypassed a mention of many technical problems. One is that of "good" estimators. Among many criteria of a "good" estimator is the concept of "unbiasedness". An unbiased estimator is one which has the property that, over repeated sampling from an unchanging population or universe, the sample estimates average out to the true population value. Quite frequently the choice of one or another estimation technique (classical least squares, say, opposed to some simultaneous method) hinges upon the estimator properties associated with the techniques. Another very important problem to which we have scarcely more than alluded is that of "identification".

42 Fisher, "Selective Estimation and the Dilemma of Objectivity", *A Priori Information and Time Series Analysis*, p. 5.

43 R. L. Basmann, "The Role of the Economic Historian in Predictive Testing of Proffered 'Economic Laws' ", *Explorations in Entrepreneurial History*, 2 (1965), 173.

These background conditions may involve the nature of government economic policy over the period in question, or changes in the socio-economic background which are difficult to assess except in a qualitative sense. The historian's contribution might, for example, involve expert knowledge of changes in legislation affecting economic activity, and an ability to assess qualitatively the implications of these changes for the stability of the relationships employed in the model.

metrician] in respect of criticism and assistance",[44] is not common among econometricians, and so it is worth examining Basmann's scheme in some detail.

In a small model of the United States economy which Basmann constructed for the period 1930-1959, one of the "reduced form"[45] equations can be represented by

(12) $$y_t = \alpha + \beta x_t + \gamma z_t$$

where y_t represents current United States gross national product, x_t represents current bank reserves, and z_t denotes current "autonomous" shifts in the investment equation (caused by, for instance, changes in business expectations).

From *a priori* assumptions concerning the values of the structural parameters and the parameters of the random terms, Basmann deduces that the probability cannot exceed one in a million that the coefficient of the bank reserves variable in (12) is less than zero. That is, in symbolic notation, his model implies

(13) $$P(\beta < 0) \leq 0.000001,$$

and he calls this result a "prediction-statement". Its negation implies that if the statement of background conditions is correct, then one or more of the model's underlying economic assumptions must be wrong.[46]

Basmann's primary aim is to test "economic laws" or hypotheses,[47] rather than to forecast economic activity, and the role of background conditions (and hence of the economic historian) in this testing is clear from the following :

44 *Ibid.*, p. 159.

45 A reduced form equation is obtained by solving the simultaneous model to obtain as many equations as there are endogenous variables, each with only exogenous or lagged endogenous variables on the right-hand side. Since no endogenous variables appear on the right-hand side, causation in each of *these* equations is from the right-hand to the left-hand side only, and this absence of "feedback" may allow the circumvention of awkward statistical problems.

46 In this model the background conditions are described in part by the time series of exogenous variables.

47 Basmann takes pains to allow for the possibility that for *forecasting* economic activity, as opposed to testing "laws", "the appropriateness of the foregoing [regression equation] ... does not rest on its being in good agreement with the premises of some explanatory economic model or other" (*ibid.*, p. 171). Nevertheless, he is critical of the methodological prescriptions of the Chicago School : "predictivists [he uses 'predictivism' as synonymous with Popper's 'instrumentalism'] seem to be content to remain ignorant of all but the most transparent testable consequences of the economic premises in their models ..." (*ibid.*, p. 184, note 18).

> *if the statement of background and initial conditions is warranted,* then the actual occurrence of the event described by [$\beta < 0$] disaffirms one or more (but perhaps only one!) of the economic premises from which the prediction-statement [(13)] is derived... the argument for disconfirmation is enthymematic; appeal is tacitly made to a statistical convention termed by Emile Borel *la loi unique du hasard*: extremely improbable events do not occur... if the event [($\beta < 0$)] actually occurs, then we agree to act as if the probability of its occurrence is greater than one chance in a million... that is to say, we act to modify one or more of the premises from which the prediction statement has been derived. [48]

It would be perfectly understandable for the historian to interject that while econometrics may be relevant to those preoccupied with "contemporary" data, as in Basmann's model, what of those who must deal with data predating the establishment of government statistical bureaux ? To answer, we would like to discuss a study probably unsurpassed from the point of view of sophisticated use of technical econometrics combined with an ingenious analysis of data sources. We refer to Franklin Fisher's analysis of the structure of the United Kingdom wheat market in the decades preceding World War I. [49]

The first part of the study will repay the attention of any historian faced with what may appear to be simple economic series. Among several time series, one was required for wheat prices in the United Kingdom. The "obvious" series to use, from the point of view of comprehensiveness and accessibility, turned out to be deficient in several ways. For instance, national price data had been formed from local price data without weighting local prices by local sales, with the result that high prices, at which relatively few sales were made, were over-represented in the national average. Fisher's statistical estimation method enabled him to circumvent this type of systematic error, but it could not cope with another similar error in the series, arising from the formation of an unweighted yearly price from weekly prices. [50] Fisher next managed to obtain government price data which was free from the latter distortion. This series extended back only to 1875, but he was able to use the series to *construct* a harvest-year price series prior to 1875 through application of a regression equation, which was estimated with data for the later period. This is an illuminating

48 *Ibid.*, p. 170.

49 Fisher, "The Quantitative Structure of the United Kingdom Wheat Market, 1867-1914", in *A Priori Information and Time Series Analysis*, pp. 61-92.

50 That is, the published data involved two "aggregation errors"; one resulted from improper aggregation over space (that is, over units spatially dispersed), and the other from improper aggregation over time.

example of the use of an econometric technique, not for estimation of a model, but for filling lacunae in data.[51]

The supply-demand model employed by Fisher bears a generic resemblance to ours, but it is much more complex and sophisticated. First, it is a dynamic model in the sense described in Section I, for it contains lagged values of some variables.[52] Second, there are appreciably more variables than in our example (six together with lagged versions of several of these), and four structural equations.

The equation system is composed, on the production side, of a production function which relates domestic wheat production to wheat acreage (and, through a random term, to weather effects), and of a short-run supply relation, wherein expected production is related to the previous period's price. The other two equations are more complicated. The demand equation relates current price to current domestic production, current net imports of wheat, current income, and to the inter-period change in wheat inventories. It is illustrated by (14), where the symbols describe, respectively, the variables mentioned.

$$p_t = \alpha + \beta q_t + \gamma m_t + \delta y_t + \varepsilon (s_t - s_{t-1}) + v_t \tag{14}$$

Finally, the change in inventories between two successive periods is regarded as dependent upon current price, the inter-period change in price, current domestic wheat output, current net imports of wheat, and upon the previous period's inventories. All of these relationships have random terms.

Much of Fisher's analysis is concerned with short-run reactions, and involves the use of estimation techniques to obtain parameter estimates and measures of goodness of fit. He concludes that his model, using post-1890 data, provides a tolerable explanation of the short-run behaviour of such variables as price and inventories.[53]

Fisher also examines the long-run behaviour of wheat imports and wheat demand. While his estimation of long-run reactions is more cursory than his short-run analysis, he feels able to make the tentative conclusion,

[51] Despite such ingenuity, the gaps in certain series (notably that for wheat inventories) were so obdurate that a substantial part of his analysis had to be confined to the period beginning in 1890.

[52] The model is an example of a "cob-web" supply-demand model. In these models the values of the endogenous variables are determined in sequence over time.

[53] Fisher notes that his findings are quite consistent with results on the demand for wheat flour for the period 1920-1938. Such divergences as did show up are those which would be anticipated on *a priori* grounds (*ibid.*, p. 85).

based upon the relationship between changes in wheat price and changes in wheat imports, that

> although in the long run free trade for Britain clearly did lead to substantially lower wheat prices, free trade or lower tariffs on wheat in other countries may have severely limited the effect of imports in stabilizing the British wheat market and in neutralizing the effects of poor harvests at home.[54]

C. Further Remarks on Causality, Measurability, and Probability.

Before we end our discussion of econometric methods we will add a few remarks on topics whose detailed examination lies beyond the scope of this paper.

First, although we have carefully avoided discussion of that perennial historiographical problem, the nature and role of "causation", we have made use of a particular concept of causality. Our approach is that of contemporary economics, and has been succinctly defined and analyzed by Herbert A. Simon :[55]

> the proposition that it is possible to discover associations among events that are, in fact, invariable [is but] ...a working rule to guide the activity of the scientist... the only "necessary" relationships among variables are the relationships of logical necessity that hold in the scientist's model of the world, and there is no guarantee that this model will continue to describe the world that is perceived.[56]

The question of the meaning of "cause" narrows

> down to the question of whether there is any meaning in the assertion that the relationship between two variables in a model is sometimes *asymmetrical* rather than symmetrical.[57]

His answer is in the affirmative, and emphasizes that causation need not be linked with the notion of a time sequence. Causation, as a logical concept, neither implies nor is implied by an ordering of events in time. This approach was followed in our illustrative supply-demand model, where changes in income or weather, the exogenous variables, produce changes in price and quantity, the endogenous variables, but where the direction of this relationship is not reversible. Changes in the exogenous variables are viewed as the "causes" of changes in the endogenous variables and, in this terminology, changes in the latter are the "effects" of

54 *Ibid.*, p. 91.

55 Herbert A. Simon, "Causal Order and Identifiability", in W. C. Hood and T. C. Koopmans, eds., *Studies in Econometric Method* (New York, 1953); reprinted in Daniel Lerner, ed., *Cause and Effect* (New York, 1965), pp. 157-189.

56 *Ibid.*, pp. 157-158.

57 *Ibid.*, p. 159. (Our emphasis.)

changes in the exogenous variables. In spite of this asymmetry of relationships, all variables are regarded as being *instantaneously* determined in our model. [58]

To conclude our discussion of causation, we emphasize that our approach is not to be construed as implying any position on questions such as "determinism versus freewill" in historical events.

The question of the measurability of historical phenomena has so far been viewed in the context of ordinary economic time series. We believe that much work remains to be done in the utilization of existing series and in the development and systematic analysis of new historical series. There remain some variables which cannot readily be encompassed by this type of quantification, perhaps as a result of their very nature, or perhaps as a result of overwhelming difficulties in compiling such series from historical documents. These difficulties can be overcome in certain cases, however, through the use of "dummy variables", variables whose values are dichotomized as zero or unity. A simple hypothetical illustration will serve to make the point. Suppose our purpose is to "explain" investment in agricultural mechanization in the United States in the nineteenth century. We might establish a hypothesis of the kind shown in (15) : y = investment in farm equipment, x_1 = farm income, x_2 = price of the services of equipment relative to the wages of farm labour, x_3 = existing stock of equipment, v = random term.

$$y_t = \alpha + \beta x_{1t} + \gamma x_{2t} + \delta x_{3t} + \varepsilon x_{4t} + v_t \qquad (15)$$

If, during the Civil War years, the underlying economic structure was subject to severe dislocations and distortions, this might be taken into account with the dummy variable x_4, defined to be zero for all of the war years (and perhaps the reconstruction years), and unity for all other years. By doing this we hope to isolate the effects of wartime disruptions from workings of the "normal" variables. Such a technique might be used in another context to assess the impact of political events. For instance, to isolate the effect of political parties on some variable, a

58 Some economists, notably Herman Wold, argue that econometric models by their very nature should be "recursive". That is, the values of the endogenous variables should be regarded as determined in a step-by-step or sequential fashion over time. However, even if variables actually do assume values in this fashion, it is probably the case that very often the lags involved are too short to be captured by the available data. That is, where a one-week lag is involved, and only monthly data are available, the workings of the lag are "lost" in the data.

separate dummy variable might be defined for each party (when the party was in office, the variable would have a value of unity, and when it was out of office a value of zero). [59]

We are not suggesting that some variables may not defy analysis, even by the use of scaling devices developed by psychologists and sociologists. We are merely stressing that *some* variables which appear unamenable to quantitative analysis, can nonetheless be incorporated into our format.

Finally, we have a few comments to offer on "probability", beyond our earlier discussion in the context of econometric methods. To some degree, the argument about the "uniqueness" of historical events can be effectively faced by appeal to our use of a random term. Inasmuch as "uniqueness" is a reflection of random forces, we feel that the econometric formulation stands up. Moreover, to the extent that this "uniqueness" is a manifestation of what Fisher calls "exogenous shocks", it is possible that the impact of these shocks might be captured by a dummy variable, for the exogenous shock might be regarded as simply an upward shift in the entire relationship. In the case of a more complex shift in parameters (a "structural break"), however, the problem becomes more acute. At any rate, the possibility of either an exogenous shock or a structural break serves to stress the need for what Fisher calls "selective estimation", the use of observations which, on *a priori* grounds, are believed to have been generated by a common mechanism. Some type of selective estimation is generally practised by econometricians and econometric historians, but not all would share Fisher's skepticism concerning the usefulness of classical hypothesis testing procedures under these circumstances. [60]

Fisher's approach really fits into the general category of Bayesian statistics, which relies upon the incorporation of *a priori* probabilities (which can be subjective in nature) to obtain probabilities attaching to

[59] The method of dummy variables has been widely used in conjunction with "cross-sectional" data. For example, if we had enough information on, say, educational levels of individual farmers at some point in time, we could define separate variables to incorporate the effects of education on the decision to purchase equipment. (One dummy variable would denote zero years of formal education, and subsequent dummy variables could incorporate varying degrees of education.)

[60] Fisher's goal is to obtain parameter estimates through careful examination of the apparent underlying mechanism which produced his observations. Because his procedures entail a careful combing of observations, certain ones being discarded because they arose from structures different from the one being studied, he feels that "[this] largely demolishes

"causal" hypotheses. There is not scope here for a discussion of the application of Bayesian concepts to historical analysis. The reader can find a most interesting non-technical discussion in Conrad and Meyer's study. [61]

III. — SOME CONCLUDING REMARKS.

We have endeavoured to describe the nature of the model-building approach through a few examples, and through examination of some recent applications. Our intention has been to demonstrate the potential fruitfulness of this approach rather than to proselytize for "econometric history". The use of systematic statistical methods is clearly not an "all or nothing" proposition, for these methods can be usefully employed in a subsidiary role in, at the least, the construction and compilation of data.

We have also tried to show that, to a certain extent, the impact of "shocks" which affect "regularities" in historical series can be captured by standard estimation methods. However, even if such occurrences cannot be incorporated into econometric models through these common procedures, it may be possible to work with sub-periods lying between these shocks. Moreover, estimation methods have been developed which regard the shocks as "signals" to which decision-makers (households, or firms, for instance) respond.

While the bulk of our discussion has centered upon economic variables and series, some of the methods described here can be used with socio-political concepts (possibly in conjunction with economic variables). Clearly there is scope for an extension of these approaches to social history.

We believe that the use of carefully formulated models, directed at empirical testing, will become increasingly important for the study of social and economic history. We also believe that the true potential of these methods is most likely to be realized if their users are well versed in "traditional" approaches, and if "traditional" historians have some understanding of the goals and methods of their model-building colleagues.

the received theory of tests of the null hypothesis type", which "[assume] that there is *no a priori* information available which can distinguish between subsets of the data" (FISHER, "Selective Estimation and the Dilemma of Objectivity", *A Priori Information and Time Series Analysis*, pp. 10, 17).

61 CONRAD and MEYER, "Statistical Inference and Historical Explanation", *The Economics of Slavery*, pp. 35-40.