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CAUSALITY AND CAUSAL LAWS IN ECONOMICS

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MRG WORKING PAPER #M8801

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ABSTRACT

After presenting and discussing H. Feigl's definition of causality and the general properties of causal laws in science, an explanation of why research in the area of "causality testing" in the last two decades has not produced many, if any, causal laws in economics. Then a leading study is reviewed which illustrates the author's preferred methodological approach and which yielded many fruitful research results. It is concluded that more studies of this kind will help to produce more causal laws in economics.

Causality and Causal Laws in Economics

by

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1. Introduction

Many scientific workers use the concept of causality in their work, as indicated by Stigler (1949, p.3), Simon (1953) and others. Since the concept is widely used, it is important to have a fruitful and operational definition of it. In Zellner (1979, 1984), I discussed many aspects of Feigl's (1953) definition of causality and Nelson (1979) and Sims (1979) provided thoughtful comments on my discussion. Feigl's (1953, p.408) definition, "predictability according to a law or set of laws" was put forward to summarize philosophers' past work on the definition of causality. See also Conway et al. (1984) and several of the papers in this issue for additional discussion of the Feigl definition. Further, there is no doubt but that establishing causality exists with a high degree of confidence, for example establishing that smoking causes cancer, is a very important matter, scientifically and practically. That is, causal laws are very useful in explaining past data and experience and in providing reliable predictions of as yet unobserved data and experience, be

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they the results of controlled or uncontrolled changes in initial conditions. Since these considerations are of utmost importance for the further development of economic science, the main thrusts of the present paper will be the presentation of additional clarifying thoughts on the definition and verification of causality and on how causal laws are produced with the hope that a more complete understanding of these issues will be helpful in guiding research to produce more causal laws in economics.

The plan of the paper is as follows. In Section 2, selected aspects of Feigl's definition of causality and causal laws will be discussed to clarify points about them which are relevant for material presented in following sections. Section 3 briefly describes and critiques several approaches which have been put forward to produce causal laws in economics. In Section 4, a particular study is considered which illustrates what the author believes is a very fruitful approach to the production of causal laws in economics. Section 5 presents a summary and some concluding remarks.

2. Discussion of Causality and Causal Laws

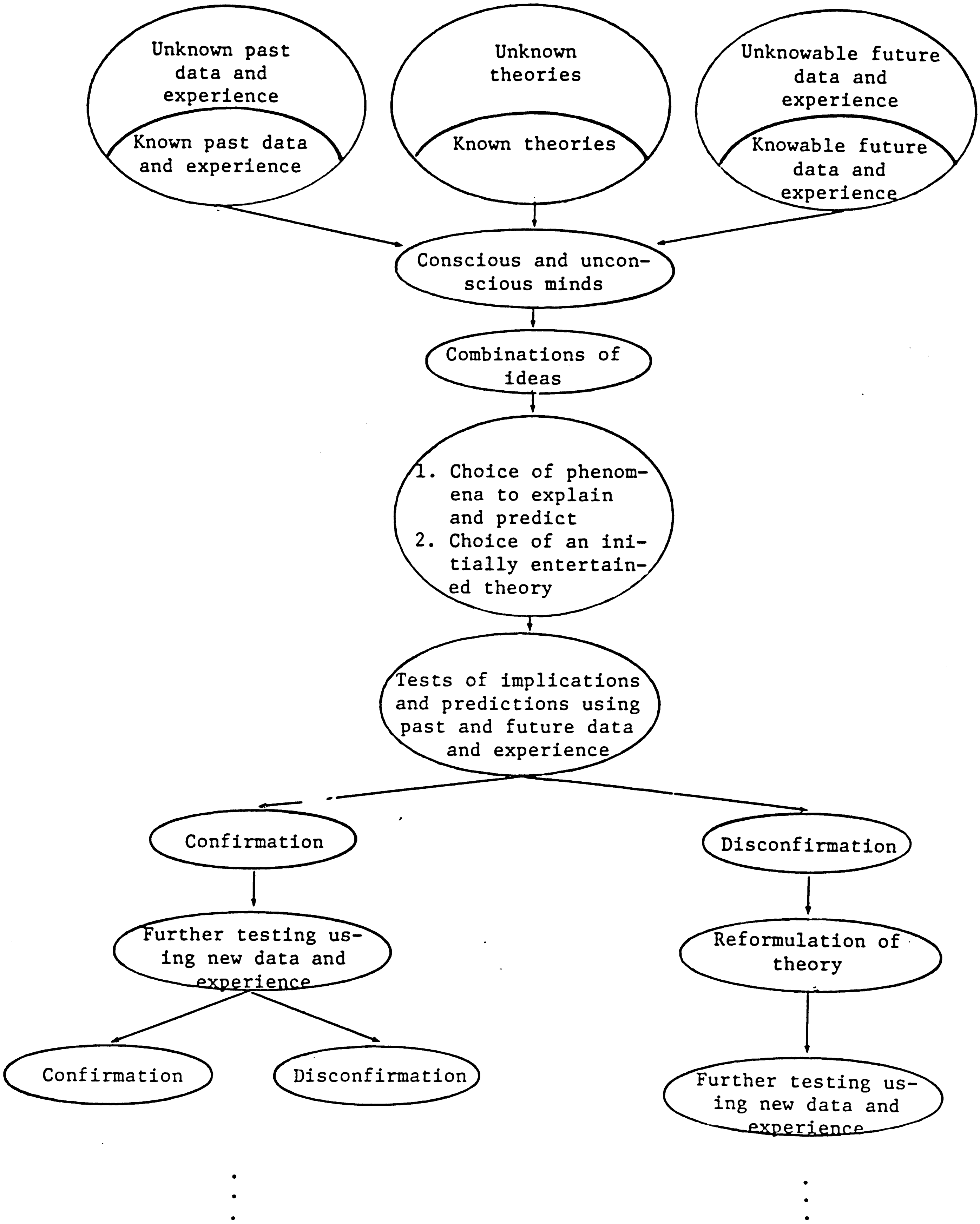
In this section, further discussion of Feigl's definition of causality and of causal laws will be provided. Feigl's simple and sophisticatedly deep definition of causality is "predictability according to a law or set of laws." This definition is such that it is applicable in all areas of science including economics. Note that predictability means confirmed predictability not merely potential predictability. More will be said below about confirmation procedures. Further, the definition does not place any severe restrictions on the forms of laws. The laws may be deterministic or stochastic, qualitative or quantitative, micro or macro and involve simultaneous and/or non-simultaneous relations, discrete or continuous time, contiguous and/or non-contiguous effects, controlled and/or non-controlled effects, and chronologically and/or non-chronologically ordered effects. Simply put, the definition does not place any a priori restrictions on the forms of laws other than the mild restrictions that their structure not involve logical or mathematical contradictions and that they be capable of yielding explanations of past data and verifiable predictions of as yet unobserved data. These requirements are in agreement with Jeffreys' (1967, p.8) rules for induction.

In Zellner (1979, 1984), Jeffreys' rules were related to issues in the previous literature on causality. Also, in Conway et al. (1984) and Swamy and von zur Muehlen's paper in the present issue, attention is given to inductive procedures used in previous analyses which violate Jeffreys' rules 1 and 2 involving the requirement of logical consistency. Such violations render these inductive procedures scientifically unacceptable. Simply put, an analysis or theory that involves logical and/or mathematical inconsistencies is not acceptable as a scientific explanation of observed phenomena. In addition to the restriction of logical and mathematical consistency, the forms of laws must be such that they are capable of explaining past data and experience and yield verifiable predictions of future data and experience. In economics, some theories are put forward which explain no past data and yield no verifiable predictions. Such "theories" cannot achieve the status of laws because they do not meet the requirements that a law must actually explain past data and experience and actually perform well in prediction. Further, empirical regularities, for example the observed constancy of labor's share or of the long run average propensity to save, etc., while very interesting and valuable, are obviously not laws in the sense described above. Similarly, mechanical statistical regressions or autoregressions, be they univariate or multivariate, no matter how successful they are in forecasting, are not causal laws since they generally do not provide understanding and explanation and often involve confusing association or correlation with causality. Subject matter theory and principles are needed to provide understanding and explanation as recognized by many in age-old discussions of correlation and causality. Finally, logically consistent and sufficient mathematical economic theorems do not qualify to be termed laws unless it can be shown that they actually explain a wide range of past data and experience and yield good predictions over a broad range of data and experience.

The requirement that a law actually perform well in explanation and prediction over a broad range of conditions cannot be emphasized too strongly. For example, this requirement rules out theories based on impossible experiments or on data that can never be produced. Little confidence would be accorded to a predictive conclusion if it related to the results of an impossible experiment or an impossible set of conditions. Perhaps the best that can be said about such a predictive conclusion is, "How interesting!" Similarly, a theory set forth to explain a unique event may or may not be empirically valid in accord with the precept that no empirically testable proposition is ruled out on a priori grounds alone. However, with just one data point, the occurrence of an unusual event, it is clear that confirmation of a proferred theory is very tenuous indeed--see Keynes (1921, p.297ff) for further analysis of unique events and causality.

To summarize elements of the above discussion and other considerations as succinctly as possible, the schematic in Fig. 1 is useful. In the figure, it is indicated that the conscious and unconscious minds interact, as well described by Hadamard (1945), Einhorn and Hogarth (1982), Hogarth (1986), and Klayman and Ha (1987), to produce ideas and combinations

Fig. 1 Schematic of Elements Involved in Work to Produce Causal Laws



of ideas using as inputs at least (1) observed or known past data and experience, (2) a space of known theories and (3) future knowable data and experience.

Initially, the conscious mind, aware of a subset of past experience and data and of a subset of theories, decides the general nature or design of an investigation. For example, many times it is very fruitful to attempt to produce an explanation or theory or extend an existing theory to explain unusual facts such as empirical regularities, anomalous empirical findings, predictive errors of existing models, facts regarding the behavior of unusual groups or unusual historical periods, and the like. Also, in an area of investigation where there is little in the way of dependable theory, fruitful "null models" can involve the assumption that "all variation is random" or the assumption that "there is no effect."

When the general objective of an investigation is determined, the next problem involves getting an appropriate theory or model that is capable of explaining the phenomenon under investigation and yielding predictions. Here hard work, a breadth of empirical and theoretical knowledge, consideration of many possible combinations of ideas, luck, and a subtle interaction between the conscious and unconscious minds all play a role. Further, it is probably the case that focussing attention on sophisticatedly simple models and theories is worthwhile in accord with Ockham's Razor, the

Principle of Parsimony and the Jeffreys-Wrinch Simplicity Postulate. For example if the data, n pairs of observations on two variables, y and x fall exactly on a straight line, assuming a linear model seems natural and useful even though there are an infinity of non-linear models that can also fit the data exactly, e.g. $y = \alpha + \beta x + f(x)(x_1-x)(x_2-x)\ldots(x_n-x)$, where $f(x)$ is finite for $x = x_i$, $i = 1,2,\ldots,n$. Thus for any choice of $f(x)$, the model will fit exactly. The choice of the simplest model, the linear model, suggested by considerations of simplicity, is of course not a final decision. Over the range of the observed x_i's, it may be completely adequate in explanation and prediction. However outside this range of data, new theories may suggest a need for a nonlinear relation which may be approximately linear over the earlier range of the x_i's.

When the mind suggests a logically consistent model or theory to explain what has to be explained, the next step is to show that the suggested model or theory actually does explain what it purports to explain by empirical investigations using appropriate data. Further, additional implications of the model can be deduced and checked using appropriate data. To check certain implications, it is often necessary to collect new data, a very important role for empirical research. Finally, and very important in engendering further confidence in the new model or theory, it must yield a broad range of verifiable predictions about as yet unobserved experiences. If empirical research validates these predictions, then confidence in the theory increases. Continued successful explanations and predictions will elevate the theory to the status of a law and posited relations in the law would then have the status of causal relations which hold with a high degree of confidence or with high probability if the latter is regarded as a measure of the former.

Continued success in actual explanation and prediction raises confidence or reasonable belief in a theory. The degree of reasonable belief or probability associated with a theory reflects the degree to which the theory has performed successfully to date. As Jeffreys (1967), Burks (1977) and others indicate, posterior probabilities associated with alternative theories or models can be computed using Bayes' Theorem and measure degrees of confidence or reasonable belief. However, it must be realized that no theory, model or hypothesis is ever completely proved in a deductive sense. There is always room for improvement and/or the possibility that old laws may be replaced by new ones. While this is the case, the solid performance of old laws over the range of experience for which they were verified still remains. As an example, many engineers and physicists continue to use Newton's laws for low velocity phenomena even though Einstein's more general laws cover both low and high velocity phenomena.

One further consideration relating to Fig. 1 which seems particularly appropriate for economics is the following one. A researcher may consider explaining a complicated economic phenomenon by constructing a detailed and elaborate theory. However, it may be that data are not available for checking the explanatory and predictive aspects of the proposed theory. Given this state of affairs, it seems clear that the status of such a theory is provisional until the relevant data are produced and the theory's performance is checked using them. This is not to say that work leading to the production of such a detailed theory is without value. On the contrary, it may prompt the collection of new data appropriate for testing the new theory. On the other hand, if the required data to check the validity of the theory's explanations and predictions cannot ever be produced, then clearly the implications of the theory do not have scientific support and hence the theory's empirical validity is problematic. All of this seems obvious; yet there are many cases in which economic theorists strongly believe their theoretical constructs and econometricians believe their models to be "true" even though they have not been subjected to extensive or even much empirical verification. Sometimes we believe what we want to believe rather than what the information in the data justifies our believing.

3. Comments on Some Approaches to Establish Causal Laws

With all the work in the past two decades on definitions of causality and tests for causality, it may be asked, "How many new causal economic laws have been produced by this work?" I believe that an honest answer is, "not a single one." I shall try to indicate why this is the case in the present section.

First, with reference to Fig. 1, in some analyses an initially entertained model is not well-conceived in that it may be too broad or too narrow, to mention just two possibilities. Examples of models in economics which are usually "too broad" are unrestricted linear vector autoregressive (VAR) models and unrestricted multivariate linear autoregressive moving average (MVARMA) models. In Zellner (1979, 1984, 1985), it is pointed out that such models contain large numbers of parameters relative to the number of data points usually available and this means that parameter estimates will be imprecise, tests will not be very powerful and predictions will not be very precise--also see, e.g. Schwert (1979), Litterman (1986) and Runkle (1987). For example, in modeling a competitive industry, product price, prices of substitute and complementary products, income, consumers' liquid assets and debt, output, number of firms in operation, and factor prices are some of the variables that economic theory indicates are relevant. The above listing of variables indicates that roughly at least twelve or more variables are relevant in modeling a competitive industry. If a linear VAR model is posited for a 12×1 vector, $\underset{\sim}{z}_t$, of these variables, it would have the following form:

$$\underset{\sim}{z}_t = \underset{\sim}{\alpha} + A_1\underset{\sim}{z}_{t-1} + A_2\underset{\sim}{z}_{t-2} + \ldots + A_q\underset{\sim}{z}_{t-q} + \underset{\sim}{\varepsilon}_t \qquad t = 1,2,\ldots,T$$

where $\underset{\sim}{\alpha}$ is a 12×1 vector of intercept terms and the A_i, i = 1,2,...,q are 12×12 matrices of autoregressive coefficients. If it is assumed that the error vector has a zero mean, is serially uncorrelated and $E\underset{\sim}{\varepsilon}_t\underset{\sim}{\varepsilon}_t' = \Sigma$, a 12×12 positive definite symmetric matrix with (12)(13)/2 = 78 distinct parameters, the total number of parameters in the system is N = 12 + 144q + 78 and the total number of observations is NOBS = 12T. With, for example, q = 2 and T = 30, N = 378 and NOBS = 360. It is seen that with only two lags (q=2), the number of parameters, 378 exceeds the number of observations, 360. Thus there will be grave difficulties in implementing an unrestricted VAR approach in this instance. Also, demand, supply and entry (DSE) models for competitive industries are generally nonlinear in variables and parameters--see e.g. Veloce and Zellner (1985) and Zellner (1985) for an example involving three equations and about 20 parameters. Thus for the above reasons, and some others, a VAR approach to modeling a competitive industry is clearly one that involves entertaining too broad a model and a model that is probably inappropriate for the problem at hand. Similar arguments relate to linear MVARMA models of a competitive industry involving about twelve variables.

Many workers realize the difficulties with linear VAR and MVARMA models mentioned in the last paragraph. Some try to "solve" these problems by modeling just a few or even just two of the variables in $\tilde{z}_t$, say $\tilde{y}_t' = (z_{1t}, z_{2t})$, for example price and output. Trying to understand the workings of a competitive industry by just considering price and output is clearly absurd and will not yield dependable causal inferences no matter which "causality tests" are utilized. The problem is simply one of left-out, relevant variables. Thus a model involving two or a few of the elements of $\tilde{z}_t$ will not be satisfactory from a subject matter explanatory point of view and is an example of a model that is too narrow.

Observations similar to those made in the previous paragraphs are relevant for macroeconomic modeling. Unrestricted models in the linear VAR or MVARMA forms involving the number of variables suggested by macroeconomic theories, say about ten or more, are too broad relative to available data sets and thus yield imprecise inferences and little or no understanding of macroeconomic phenomena. Since this is the case, at best analyses using them can be regarded as "exploratory" and have not yet been capable of establishing causal laws. Even as an "exploratory" data analysis approach, the use of linear VAR and MVARMA models may not be entirely satisfactory. A more productive and perhaps traditional approach would be to study intensively the time series properties of individual variables before trying to combine them into a multivariate model. Contrary to this advice, "traditional" macroeconometric modelers have constructed models containing hundreds of non-linear stochastic difference equations. The mathematical, statistical and economic properties of these models are very difficult to establish. If these properties are not well understood, there is no secure basis for asserting that they can possibly be causal laws.

It is clear that in many instances workers have entertained models that are too broad or too narrow in their conception and this simple fact, I believe, has been mainly responsible for the failure to produce many or any new causal laws in economics in the past few decades by those working in the area of "testing causality" at the macro and micro levels. It appears necessary to formulate sophisticatedly simple initial models which imply strong, testable propositions relative to the data that we do have or can reasonably expect to have within a reasonable time.

Since the use of "sophisticatedly simple" models is strongly recommended, it is appropriate to say a few words about them in an effort to clarify what is meant by "sophisticatedly simple," a relative term. A model is "sophisticatedly simple" relative to other, presently known models, relative to presently available mathematical and statistical techniques and relative to available or potentially available data. If there are no effective models or theories available to explain a phenomenon, for example the variation of stock prices, a sophisticatedly simple initial hypothesis or model is that "all variation is random unless shown otherwise," a suggestion put forward by Jeffreys (1967). This stance has proven to be very fruitful in stock market research. For example, when several researchers concluded that automobile sales could be used to forecast the British stock market price index, Box and Newbold (1971) showed that the result obtained was produced by forcing an incorrect model on the data. By analyzing a broader model containing that used by the researchers and a random walk model as special cases, Box and Newbold were able to show that the data supported the random walk hypothesis. This is not to say that the random walk model is necessarily "true" but that it is a fruitful, sophisticatedly simple working hypothesis. Indeed, it has been the focus of much important empirical research which has uncovered departures from it, namely day-of-the-week, January, small firm, low frequency, and heteroskedasticity effects. These suprising and unusual effects or "anomalies" are now the concern of much theoretical and additional empirical research designed to explain them. Such research arising from the consideration of a relatively simple hypothesis and unusual facts is most productive in my opinion and may lead to causal laws regarding stock price behavior. This point of view is in accord with that of Jeffreys (1973) who has written:

> "The argument in this [his] book completely reverses the usual notion of causality. This started with some idea of inherent necessity. After Mach it was replaced by invariable succession, but in fact invariable successions are rare. We now see that science starts with the fact that variation is random and detecting in succession departures from randomness." (p.262)

When a model or models are on the scene which explain some phenomena, it is always useful to consider sophisticatedly simple modifications of them. The null hypothesis is, of course, that the modification is not needed, a hard-boiled, "show-me" stance that is very fruitful. The Pigou effect, a modification of the Keynesian macroeconomic model, and the rational expectations hypothesis (REH) represent important examples of the modification of existing theories and have led to propositions which have been tested empirically. Both the Pigou effect and the REH are examples of sophisticatedly simple, important hypotheses with far-reaching implications. Some simple hypotheses are not sophisticated since they may not be in forms which lend themselves to analysis using known or knowable mathematical and statistical techniques and for which no data are available or will ever be available to test them. Further, given that an area of study involves much detail, it is possible to formulate a detailed, sophisticatedly simple large model to capture detail rather than a large complicated model.

It is natural to ask whether simplicity or complexity of models can be quantified. One rough index of the complexity of differential equations, put forward by Jeffreys (1967), is stated as follows, "We could define the complexity of a differential equation, cleared of roots and fractions, by the sum of the order, the degree, and the absolute values of the coefficients." (p.47) This rule takes account of nonlinearity, the order of the equation and the number and absolute values of coefficients. The rule can also be applied to difference equations, for example the implied "final" equations for individual variables in a linear VAR model. Since these equations are high order autoregressive equations with many parameters, they are not very simple. While not perfect, the Jeffreys' index of complexity, discussed further in Jeffreys (1967, pp.46-49) is a useful first approximation.

Further, with respect to sophisticatedly simple versus complicated models, the stage of development of a field must be taken into account. What appears to be sophisticatedly simple at present may not have been considered to be so years ago. For example, Schrödinger's wave equation, a partial differential equation probably would not have been considered to be simple in the nineteenth century. But as

Jeffreys (1973) has shown, relative to alternative theories which were proposed since the nineteenth century, Schrödinger's equation is probably the simplest equation which can explain the relevant empirical phenomena.

In addition, many scientists believe that sophisticatedly simple models will perform better in explanation and prediction than complicated models. Jeffreys (1967) remarks, "... the simplest law is chosen because it is most likely to give correct predictions ...." (p.4) and "All we have to say is that simpler laws have the greater prior probabilities. This is what Wrinch and I called the simplicity postulate," (p.47) In Jeffreys (1973), the important role of simplicity in science is discussed with many illustrations drawn from the history of science. In economics, the Nobel Prize winners, Arrow, Friedman, Frisch, Schultz, Stigler, Tinbergen, and Tobin all appear to appreciate the value of sophisticated simplicity in their work. In the author's opinion, a preference for complicated models and methods in economics has been a strong negative factor impeding progress in producing more economic laws. For many years in talks and papers, I have issued the following challenge: Demonstrate that a complicated model in any area of science has performed well in explanation and prediction. To date, I have not heard of any. As regards large, complicated macroeconometric models' performance, Christ (1975) remarked, "... though the models

[Wharton; Data Resources, Inc.; Bureau of Economic Analysis; St. Louis; Fair; Liu-Hwa; Hickman-Coen; and U. of Michigan] forecast well over horizons of four to six quarters, they disagree so strongly about the effects of important monetary and fiscal policies that they cannot be considered reliable guides to such policy effects, until it can be determined which of them are wrong in this respect and which (if any) are right." (p.54) For more recent information regarding the forecasting performance of several large, complicated macroeconometric models and VAR models, see McNees (1986) who concluded in his last paragraph, "Just as conventional macroeconometric models may have been "oversold" in the 1960s and early 1970s, leading to disappointment and rejection in the late 1970s and 1980s, there is some danger that the VAR approach to modeling and forecasting may now be oversold as a superior substitute for the more traditional approaches ...." (p.15) Litterman (1986) also provides interesting results on the forecasting performance of his Bayesian VAR model and that of three traditional models (Chase, Wharton and DRI). Neither McNees (1986) nor Litterman (1986) considered the models' responses to monetary and fiscal policies and their assumed causal structures.

In summary, it has been emphasized that causality and causal laws involve actually successful explanation and prediction of a wide range of data and experience. Further, posterior probabilities, explicitly or perhaps implicitly evaluated can measure degrees of confidence that are associated with theories. When such probabilities are very high, reflecting much outstanding and broad-ranging performance in explanation and prediction, a theory can be termed a causal law. The production of such laws will be facilitated by consideration of sophisticatedly simple theories and models. Some attention was given to elaboration of the concept of "sophisticatedly simple." Complicated theories and models, which are too broad as well as theories and models which are too narrow, utilized by many, are not satisfactory in terms of fostering the production of new economic laws. Since this process is extremely important, a review of a leading study will be presented in the next Section.

4. Consideration of a Specific Study

In this Section, a brief review of Friedman's (1957) A Theory of the Consumption Function is provided to illustrate some of the general points made in the previous Sections of this paper.

Chapter I of Friedman's book is devoted to describing a general problem area, Keynes' (1936) consideration of consumer behavior and a serious conflict of evidence, namely, "Estimates of savings in the United States made by Kuznets for the period since 1899 revealed no rise in the percentage of income saved during the past half-century despite a substantial rise in real income" (pp.3-4) and Kuznets' findings indicate that the marginal propensity to consume is equal to the average and is "... decidedly higher than the marginal propensities that had been computed from either time series or budget data." (p.4, fn. omitted). Thus Kuznets' important empirical research produced unusual facts or anomalies which Friedman and others recognized required explanation, an excellent example of the vital role of measurement in economic research.

After mentioning earlier attempts by Brady and Friedman, Duesenberry, Modigliani and Tobin to explain the conflicting evidence, Friedman mentions the role of wealth in determining consumption and indicates that he will present "... yet another hypothesis to explain the observed relation between consumption expenditure and income." (p.6). He notes that his hypothesis (1) "... seems potentially more fruitful and in some measure more general than either the relative income hypothesis or the wealth-income hypothesis taken by itself. It incorporates fully the wealth-income effect and explains why the relative income hypothesis should be valid under special circumstances." (p.6) and (2) "The hypothesis follows directly from the currently accepted pure theory of consumer behavior, seems consistent with existing empirical evidence, and has observable implications capable of being contradicted by additional evidence." (p.6) In his book, Friedman analyzes a broad range of cross-section and time series data to show that they are consistent with his theory. Also, very importantly, he proposed eight additional tests of his theory involving readily available data and predicted the outcomes of these proposed tests, many of which have been performed subsequently--see Friedman (1957, pp.214-219.).

This overview of Friedman's work indicates that attention was focussed on explaining unusual empirical findings. A range of alternative theories was considered and some were nested in his theory based on "the currently accepted pure theory of consumer behavior," a two-period Fisherian indifference equation model in which a symmetric, homogeneous utility function $U(C_1,C_2)$ is maximized subject to a wealth constraint--see Friedman (1957, p.7ff). Use of this sophisticatedly simple model yielded many fruitful, testable propositions, including, among others, that current planned or permanent consumption is influenced by wealth or permanent income rather than just by current income, that the ratio of permanent consumption to permanent income is independent of wealth or permanent income and that reactions to changes in permanent income are very different from reactions to changes in transitory income.

Further, note that new unobservable, theoretical constructs, permanent and transitory income and permanent and transitory consumption are employed in Friedman's theory.

This conflicts with the advice of some who recommend that science deal just with observables, an a priori restriction on the form of theories or models which cannot be justified. It may be that the introduction of too many unobservables can render models ineffective in explanation and prediction. However this is a matter to be determined empirically in terms of the quality of explanation and prediction of data and not on a priori grounds alone. That Friedman kept his model sophisticatedly simple by not introducing too many unobservable constructs is evident and also explicit in his consideration of the possible effects of uncertainty:

> "The introduction of uncertainty thus blurs the sharp lines of the above analysis, and suggests additional factors that may produce departures from the shape of the consumption function specified in (2.5). However, on the present level of analysis, there seems no way to judge whether these factors would tend to make consumption a larger or smaller fraction of wealth the higher the absolute level of wealth. Accordingly, this effect of uncertainty establishes no presumption against the shape assigned to the consumption function, and thus casts no shadow on the "simplicity" that recommends it." (p.15)

In this quotation, there is an emphasis on the hypothesis of "no effect" of uncertainty, unless shown otherwise, and on the consideration of simplicity, considerations mentioned in earlier sections of this paper.

Also, Friedman recognized the importance of tests of his theory using as much old and new data as possible and actually performed many tests. The nature and form of his theory indicated what tests to perform and how to perform them. Surely it is preferable to perform appropriate predictive tests implied by a theory rather than tests implied by some formal and perhaps artificial definition of causality which places important restrictions on the forms of theories and thus can lead to unsatisfactory results--see Zellner (1979, pp.40-42) for an explicit example in which this type of "causality testing" is applied to Friedman's theory and produces very unsatisfactory results.

While Friedman's work provides an excellent example of theory formulation and validation, as is to be expected, there are parts of the theory that are still controversial and have prompted much additional research. For example, some, including Hall (1978) have entertained multi-period stochastic optimization formulations of the theory in an effort to deal with the problem of uncertainty explicitly. Generally these works have utilized temporally separable utility functions in contrast to Friedman's non-separable two-period utility function. The assumption of temporal separability, while convenient from the point of view of obtaining mathematical solutions may not be satisfactory from an economic point of view and it is known that the properties of solutions can be very sensitive to departures from temporal separability--see e.g. Wan (1970). Others have attempted to replace Friedman's use of an adaptive expectations hypothesis with a rational expectations formulation. In this connection, users of the rational expectations approach often assume that economic agents know "the" true model's form and properties even when expert economists do not. Overlooking model uncertainty and use of inappropriate models may be reasons for the mixed results obtained in testing the rational expectations hypothesis--see e.g. Lovell (1986). Finally among others, Zellner (1960), Houthakker (1965), Bhalla (1979), and Zellner and Moulton (1986) have tested Friedman's proportionality hypothesis, namely that the ratio of permanent consumption to permanent income is independent of the level of permanent income. In this work, attempts are made to investigate whether the proportionality hypothesis breaks down at very low levels of permanent income since pushing theories to extremes usually provides strong tests and often evidence that theories have to be modified.

The works cited in the previous paragraph and many more that could be cited attest to the power, breadth and fruitfulness of Friedman's sophisticatedly simple theory. Also, by bringing a wide range of data to bear on the original model and variants of it, Friedman and other economists involved in this work have set a very good example for others who wish to establish new causal laws in economics.

5. Summary and Concluding Remarks

As in past writings, I have emphasized that Feigl's definition of causality is a fruitful and appropriate one for work in all areas of science. It does not place undue restrictions on the forms of causal laws and is operational as indicated by a discussion of research on Friedman's theory of consumption. Several aspects of theorizing in economics were discussed with the main conclusion that sophisticatedly simple models and theories are more fruitful to consider than complicated models and theories. Some unsophisticatedly narrow models and some very broad, complicated models were mentioned and deemed unsatisfactory for efforts to establish causal laws in economics. After commenting on various aspects of the term, "sophisticatedly simple," Friedman's work on a theory of the consumption function was reviewed to indicate how the general principles have actually been applied, have yielded a wealth of useful results and can serve as a model for other research in economics aimed at the production of new causal economic laws. It involved a wholesome combination of sophisticatedly simple economic theory, extensive empirical research to establish what the facts are, and relevant testing of the theory's implications and predictions employing a broad range of data. Additional research of this type is needed to produce more causal economic laws in economics.

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