My Experiences with Nonlinear Dynamic Models in Economics[*]

by

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I. Introduction and General Considerations

It is a great honor to present the keynote address at this March 2001 meeting of the Society for Nonlinear Dynamics in Economics. I thank James Ramsey for inviting me to join you and for the opportunity to share my ideas with you.

Since the field of nonlinear dynamics is so broad, it is impractical to attempt to discuss the entire field. Thus, I have decided to limit my discussion to a description of my experiences with nonlinear dynamic models in some of my past research projects. As will be seen, in each instance the nonlinear properties of models were critical in achieving the goals of these research projects. The first project, undertaken in the late 1950's jointly with James Crutchfield involved the economic evaluation of the world's most famous marine conservation program, the International Pacific Halibut Conservation Program. The second project, performed in conjunction with a group at the Battelle Memorial Institute, had as its objective to ascertain the effects on regional economic growth of proposed dam construction by the U.S. Army Corps of Engineers on the Susquehanna River. The third project involved an evaluation of the Federal Reserve-MIT-PENN model of the U.S. economy, done as a consultant to the group, including Franco Modigliani, Albert Ando and others who built the model. Last, I shall comment on my past and current efforts to construct a Marshallian macroeconomic model. As will be seen, in this work nonlinearities play an important role in model building.

To focus the discussion, it is relevant to discuss the goals of modeling and make some general remarks regarding the process of model building, a rather controversial topic that is not well treated in most economics, econometrics and statistics texts. Generally, the goals of modeling in any area of science involve producing models that perform well in (1) explaining the past, (2) predicting future and as yet unobserved data or outcomes and (3) solving policy problems. Of course, it may be difficult to achieve all three goals in one model building project. However, it is well known that models that fit well or perhaps fit too well and involve many parameters do not usually perform well in prediction, a criterion that is of the utmost importance. Further, we may have an empirical forecasting model that forecasts well but does not explain outcomes. Here the challenge is to develop a theory that explains why the forecasting model works well and perhaps leads to improvements in it. Last, models that work well in explaining the past and in prediction sometimes have to be elaborated to deal with the analysis of the effects of changing policies since policy changes may involve changes in a model's properties, as noted by my colleague Robert Lucas and many others.

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In constructing models, there is the issue of simplicity versus complexity. Many, including myself, believe that it is fruitful to start simply and complicate if necessary. That is, it is recommended that an initial, sophisticatedly simple model be formulated and tested in terms of explaining past data and in forecasting or predicting new data. If the model is successful in meeting these tests, it can be put into use. If not, usually the initial model can be modified or elaborated to improve performance. This approach is consistent with the Jeffreys-Wrinch "Simplicity Postulate," namely that simple models will probably work better than complicated models. Also, it is consistent with advice given in industry, namely KISS, that stands for "Keep It Simple Stupid." However, since some simple models are stupid, I changed the interpretation of KISS to "Keep It Sophisticatedly Simple." There are many sophisticatedly simple models that are useful and work fairly well, namely, $s = 1/2gt^2$, $E = mc^2$, PV=RT, the laws of demand and supply, etc. However, it is difficult to find a complicated model in any area of science that works well in explanation and prediction. Indeed, I have challenged many audiences over the years to tell me about a successful complicated model in economics or any other field of science and have not yet heard about a single one. It appears that there is a great need for much more emphasis on sophisticated simplicity in model construction, as emphasized in the structural econometric modeling, time series analysis (SEMTSA) approach advocated by Franz Palm and myself.

Some who disagree with the approach that involves starting simply and complicating if necessary suggest that we start with a general "encompassing" model and test downward to get a model that is satisfactory. A problem with this approach is that there are many, many general models and if the wrong one is chosen, obviously what follows will be unsatisfactory. Further, it is not clear that this approach, which has been practiced for decades, has produced any models that perform well in explanation, prediction and policy making.

Naturally, the issue of defining the simplicity or complexity of a model is a relevant issue. In this regard, Jeffreys has suggested the following measure of the complexity of a differential equation: The complexity, denoted by C, is given by C = O + D + S, where O = order of the equation, D = degree of the equation and S= sum of the absolute values of the parameters after setting one coefficient equal to one. According to Jeffreys, this measure C will order all the laws of physics that are expressed as differential equations with respect to complexity. Note that one does not just count the number of parameters in order to measure complexity. The absolute values of the parameters play a role as well as whether the equation is linear or nonlinear. Indeed a nonlinear differential equation involving a few terms can be less complex than a high order linear differential equation according to Jeffreys' measure. For further consideration of Jeffreys' measure of complexity including generalizations to cover stochastic difference equations, e.g. autoregressive (AR), vector autogressive models (VARs), etc. and error terms' distributions, see my article in the recently published Cambridge U. Press monograph, Simplicity, Inference and Modeling (Keeping It Sophisticatedly Simple), edited by H. Kuezenkamp, M. McAleer and myself. Also in this volume, the views on simplicity and complexity of a number of Economics Nobel Prize winners and workers from many fields are presented and it appears that many like to KISS.

Last, it is important to appreciate that simplicity or complexity is a relative term, relative to the current stage of development of a field. For example, in the early stage of the development of a field, a simple model or hypothesis might be that everything is random unless shown otherwise. Or, as in financial economics years ago, the benchmark model for stock prices was a random walk model. On the other hand, when a field is well developed what is a sophisticatedly simple model will not be a random walk model but a model that takes account of what is known in the field. As an example, the Shrödinger wave equation, a partial differential equation does not appear to be a very simple model. However, as pointed out by Jeffreys, it was the simplest partial differential equation that is consistent with what was known at the time. Thus simplicity and complexity are relative terms that depend on the state of knowledge in a field as Einstein understood when he advised researchers to make their models as simple as possible but no simpler.

Thus it is seen that model formulation is a rather difficult area. Some advocate combining ideas from a broad range of fields in model formulation guided by an esthetic sense. Others point to the key role played by unusual facts in stimulating thought processes to produce new models to explain them. Similarly with respect to Thomas Huxley's "ugly facts", that is facts that contradict current theories' and models' implications, they prompt new lines of thought that many times lead to new theories and models. In order to promote progress, I have urged that empirical workers and others concentrate on producing more unusual and ugly facts rather than dull, usual facts. Further, some find that using optimization schemes, e.g. profit, utility or entropy maximization or cost minimization schemes, is valuable in producing new models. See below for some examples of this in connection with learning models in statistics and econometrics. Last, as is evident, model formulation is an iterative, sequential process that involves sequential testing and revisions, often employing computer simulation experiments, hypothesis and predictive testing and on line tests of performance. In my opinion, there is a great need for more formal and useful model formulation techniques and principles.

Finally, there is the issue of how to use data to implement models, the problem of statistical inference. In this connection there are several well known alternative approaches and work has been underway for many years to evaluate them. It is interesting to note that given a model for the observations and its parameters, represented by a joint probability density function, $p(y,q)$ it is possible to represent the information in this joint density relative to uniform measure by $\int\int p(y,q)\log p(y,q)dydq$
$= \int I(q)p(q)dq + \int p(q)\log p(q)dq$
where $p(y,q) = p(q)f(y|q)$ has been employed and
$p(q)$ is a prior density, $f(y|q)$ is the data density and
$I(q) = \int f(y|q)\log f(y|q)dy$ is the information in the data density, $f(y|q)$. This measure of information is just the negative entropy relative to uniform measure, or, more simply , the expected log height of the density. Thus the total information is equal to the average information in the data density plus the information in the prior density for the parameters. If we subtract the information in the prior density from the total information in the joint density, we obtain "the information provided by the experiment" namely, $\int I(q)p(q)dq$. This is one

measure that is useful in appraising the information content of experiments and in designing experiments.

Also, there is the question of how the prior density, $p(q)$ is formulated. There are several well known procedures for formulating prior densities, one of which is to choose the form of the prior, perhaps subject to side conditions or constraints so as to maximize the difference between the information in the data density and the information in the prior, that is the difference of the two terms on the right hand side of the expression for the total information provided by an experiment that I denote by $G(p) = \int I(q)p(q)dq - \int p(q)\log p(q)dq$. The solution to this problem subject to just the condition that p be a proper density is $p^* = c\exp\{I(q)\}$, where c is a normalizing constant. Also, this problem has been solved subject to other side conditions to provide informative, invariant prior densities in a formal reproducible manner; for some explicit solutions, see my 1997 book cited below.

Given that we have a prior density and a density for the observations, it is relevant to ask how they can be combined. Usually they are combined using Bayes' theorem to provide a posterior density for the parameters that incorporates all the sample and prior information. While there are proofs of Bayes' theorem in the literature, as with all proofs they are conditioned on certain assumptions. Several years ago, it occurred to me that it would be useful to have another approach for producing Bayes' theorem and variants of the traditional Bayesian learning model. Given the information in two inputs, say a prior density and a data density, as introduced above, we have two outputs, a "post data" density for the parameters, $g(q|D)$ where D denotes the given sample and prior information inputs and a marginal density for the observations, $h(y|D)$. Now we choose g to minimize the difference between the output information, the information in g and h, minus the information in the inputs, the prior density and the data density since we wish the output information to be as close as possible to the input information. Explicitly the criterion function is:

$$\Delta(g) = \int g\log g dq + \int g\log h dq - \int g\log f(y|q)dq - \int g\log p\, dq = \int g\log[g/[p\, f/h]dq$$

On minimizing this last expression with respect to the choice of g subject to it being a proper density, the solution obtained via a calculus of variations approach or by noting the second line is in the form of the non-negative Jeffreys-Kullbach-Leibler distance measure is to take g = g* = $p\, f/h$. Thus the solution is precisely in the form of the traditional Bayesian learning model, that is Bayes' Theorem. Also, when g is taken in this form, $\Delta(g^*) = 0$, that is the input information = the output information and thus the process is 100% efficient in the sense that all the input information is conserved and none lost. In the recent literature variants of the above problem have been formulated and solved, e.g. inputting just a data density and no prior or weighting the prior and data density information measures differently, etc. Further, solutions to multi-period learning problems in which the output of one period is input to the next along with new data information have been formulated and solved. Thus, some rather simple, nonlinear measures along with an optimization approach can be used to produce optimal learning models. For more details, see my recent paper, "Information Processing and Bayesian Analysis," cited in the references.

Having raised some issues regarding the formulation of models, learning, etc. in these introductory remarks, I shall now turn to describe several past projects in which I have participated that involved formulation and use of nonlinear, dynamic models.

II. Evaluation of a Marine Conservation Program

In the late 1950s, James Crutchfield and I at the U. of Washington in Seattle spent several years evaluating the performance of the International Pacific Halibut Conservation Program. This conservation program was established under a treaty between Canada and the U.S. in the 1920s to save the Pacific halibut from extinction. Marine biologists in Canada and the U.S. had found that the Pacific halibut population was declining during the early decades of the 20th century and proposed that a catch limit or quota be imposed on the fishery to allow the population to grow so that at a future time there would be a large halibut population in the Pacific. Also, with a large halibut population, there would be a large annual natural increment that could be caught and consumed while preserving the population. Remarkably, these policies were implemented with the result that the Pacific halibut population grew over the years and the annual catch in the 1950s was about 50% larger than in the 1920s. In contrast, in the Atlantic halibut fishery that did not have a conservation program, the halibut population was decimated and the Atlantic halibut fishery disappeared.

Fundamental in the thinking of the marine biologists who designed the Pacific halibut conservation program was the following, well known nonlinear differential equation for the halibut population, denoted by N = N(t) with t = time:

$$dN/dt = rN(1 - N/N_e) \tag{1}$$

where r and N_e are positive parameters with the latter being the equilibrium population. That is, from phase diagram considerations, it is the case that dN/dt=0 for $N = N_e$ and this equilibrium is a stable equilibrium. Note too, the maximal value of the natural increment, dN/dt is associated with $N = N_e/2$, the so-called famous “maximum sustained yield” population value. This was the target value for the population for the marine biologists who designed the conservation program and who did not take account of economic considerations regarding common property rights, failure to charge economic rent, etc., in part because the theory had not been well developed in the early decades of the 20th century. Note too that the solution to the differential equation in (1) is the well known S-shaped logistic function for N(t).

While the nonlinear differential equation in (1) provides a first approximation to the behavior of many biological populations, some pointed out that instead of approaching the limiting population monotonically, as implied by the above equation, there are usually overshoots and oscillations, some rather violent about the limiting equilibrium value. Walter J. Cunningham in his excellent book, Introduction to Non-Linear Analysis, McGraw-Hill, 1958, recognized this phenomenon and analyzed it within the context of the following non-linear mixed differential-difference equation:

$$dN/dt = rN[1 - N(t-q)/N_e] \tag{2}$$

where q is, for example, a birth gestation lag. As Cunningham showed, solutions to equation (2) can exhibit various oscillatory properties. It was indeed surprising to me to learn that such a simple modification to equation (1) could result in such different properties of solutions. Also, it has been recognized that a discrete version of (1), $N_t - N_{t-1} = rN_{t-1}(1 - N_{t-1} / N_e)$ is in a form of a "chaotic" model that has a rich range of possible solutions, some oscillatory with most unusual properties. See, e.g. P.B. Kahn, Mathematical Models for Scientists and Engineers, Wiley, 1990, Ch. 16 for plots of solutions to this last nonlinear difference equation as parameter values and initial values are varied. More varied solutions are of course possible in stochastic versions of the above models as well as in stochastic predator-prey models.

Since equation (2) above and elaborations of it had not been studied empirically with data for the Pacific halibut fishery, we, along with many others, employed equation (1) along with demand, supply and equilibrium equations to produce a first approximation model of the fishery. This model is capable of explaining not only the behavior of the fish population, N, but also that of the economic markets for fish. One such simple model presented in our study is:

(3) Demand: $X^d = a_1 p + a_o$

(4) Supply: $X^s = b_1 p + b_2 N + b_0$

(5) Market Equilib.: $X = X^d = X^s$

(6) Biolog. Constraint: $dN / dt = rN(1 - N / N_e) - X$

On substituting $X = X^d = X^s$ in (3) and (4) and then eliminating the price variable, p, it is possible to express X as a linear function of N which when substituted in (6) results in the following nonlinear differential equation:

$$dN / dt + k_1 N^2 + k_2 N + k_o = 0 \tag{7}$$

The phase diagram associated with (7) indicates that there are two stationary solutions one of them, N_1, being unstable and the other, $N_2 > N_1$, stable. Having these two particular solutions, the general solution to (7) is in the form $N = [N_2 - N_1 K \exp\{-k_1 (N_2 - N_1)t\}] / [1 - K \exp\{-k_1 (N_2 - N_1)t\}]$, where K is a constant of integration. Since $N_2 > N_1$ and $k_1 > 0$, as t grows $N \to N_2$, the stable solution. Further, if income Y is introduced in the demand equation and T, technological change in the supply equation, with the assumption that both grow linearly in time, on solving the system for the differential equation for N it is found that it takes the following form:

$$dN / dt + k_1 N^2 + k_2 N + d_o + d_1 t = 0 \tag{8}$$

Surprisingly, the approximate solution to (8), given by Cunningham (1958, pp. 250-253) and included in our monograph involves a term that can give rise to an oscillatory solution. Thus use of relatively simple demand, supply, market equilibrium and biological constraint models leads to a rather rich range of possible solutions.

Further, in more recent work, an entry and exit equation for fishing vessels has been added to the model along with the number of boats in operation included in the supply equation. As the models become more detailed, there usually are problems in getting explicit, analytical solutions and thus the need for good simulation programs that can be used to study properties of solutions under a variety of initial conditions, parameter values, etc. Also, alternative conservation policies can be evaluated in simulation experiments.

With respect to various solutions to the models used in the literature and in our study, we pointed out that under conditions of very strong demand for halibut, it may be that the solution to the above model will be to have the population of halibut, N(t) approach zero, a condition encountered in the Atlantic halibut fishery. That is, the market solution, whether rent for the use of the resource is being charged or not may be to take a very large catch and run down the stock of halibut. Whether or not it is socially desirable to run down the stock of halibut is of course a basic issue. In the case of the Pacific halibut fishery, marine biologists and others convinced the Canadian and U. S. governments to modify the market solution by establishing annual catch limits. The idea was to cut back the catch in the short run so as to permit the halibut population to grow to the maximum sustained yield value, denoted by $N_e/2$ above and to scoop off the large natural increment in future years. Indeed, with the institution of the conservation program in the 1920s, the annual catch rose by about 50% from the 1920s to the 1950s. However there was an unforeseen development, namely with every one rushing out to get their portion of the annual quota, the fishing season shortened from about ten months to less than two months. Such a change meant that fresh halibut was not available for much of the year and most of the catch was frozen and stored to meet consumer demand, thereby raising costs somewhat and perhaps affecting quality. To help solve this problem, we recommended that the conservation authorities auction off the right to fish for halibut at the beginning of each season. In this way, a person can buy the right to fish for say, 10,000 pounds and exert this right at anytime of the year. In recent years the Canadian and U.S. governments have instituted such auctions with the results that the season has lengthened considerably helping to lower production costs and the availability of fresh halibut has increased considerably. Also, both governments now have an extra source of revenue with which to finance research and other worthy activities. Further, the quota and fees paid for the right to fish help to limit entry and the latter are in part a payment for the use of the common property resource, the fish.

As is clear from this brief review of a conservation program, nonlinear dynamic economic resource models, some more disaggregated than those presented above, played and are playing a key role in formulating conservation policies and analyzing past, present and future developments in the Pacific halibut fishery and in many other natural resource industries.

III. The Susquehanna River Basin Study

In this study, I consulted with a group of researchers at the Battelle Memorial Institute in Columbus, Ohio who were involved in building a regional economic model to appraise the possible effects of proposed dam construction on the Susquehanna River on the economic growth of the region. The U. S. Army Corps of Engineers had proposed such dam construction and contracted with a Washington, D.C. research group to appraise the effects of proposed dam construction on growth of the region. This D.C. research group made mechanical trend extrapolations showing that the proposed dam construction would have large positive effects on the region's growth over the years. The research effort at Battelle was undertaken to appraise these results and to provide a more secure basis for drawing conclusions about the effects of dam construction on regional economic growth. In this effort, a nonlinear simulation model of the Susquehanna River Basin economy was built and simulated with no dams being built, a moderate set of dams being built and with many dams being built. The conclusion reached as a result of these simulation experiments was that aside from the expansionary effects of constructing the dams, there would be little long run economic effects of dam construction on regional economic growth, a conclusion quite at variance with that of the Washington, D.C. research group working for the Corps of Engineers. See Hamilton et al, Systems Simulation for Regional Analysis: An Application to River Basin Planning, Cambridge, MA: MIT Press, 1969 for further information about the model and methodology employed.

As regards construction of the model to evaluate the possible effects of dam construction on regional economic growth, in the very early stages of the project, it was considered very important to describe how dam construction might possibly affect a region's economic growth. Some of the items included on the list were the following: (1) Dams might provide water for agricultural irrigation during dry seasons of the year and help lower the price of water to attract heavy water-using industries to the region. . (2) Water from behind the dams might be released during low flow periods to help deal with pollution problems on the river that affect firms' and others' location decisions. (3) Dams could produce hydroelectric power and help solve power bottleneck problems, lower the price of power and thus attract firms to the region. (4) The artificial lakes behind the dams might attract tourists to the area for boating, swimming and other recreational activities. These and other possible effects of dam construction were considered at length. For example, with respect to (1), it was pointed out that the river flows from upstate New York through Pennsylvania and Maryland into the Chesapeake Bay. In this region, annual rainfall is about 40 inches a year and much agriculture flourishes without the need to obtain water from dams for irrigation. Also, the price of water in the region is practically zero and thus there is no need to have dams to lower the price of water to attract heavy water using firms to the region. As regards point (2), there are some polluted portions of the river, mainly downstream from a few cities that dump raw sewerage in the river. This causes a pollution problem, particularly during low flow periods of the year. However, this problem can be addressed by the construction of some sewerage treatment plants without the need for expensive dam construction. With respect to power, the region already has hydro-power and other types of power plants and the price of electricity is quite reasonable. It's hard to believe that power bottlenecks and a high price of electricity are causing firms not to locate in the region. Finally, with respect to point (4) that the dam construction might stimulate the tourist industry, it was pointed out that there are many lakes and large tourist industries in the Pocono Mountains of Pennsylvania and the Catskill Mountains of New York.

The above are examples of the considerations that conditioned the way in which the model was formulated. In this connection, the following quotation indicates some of the issues involved:

> "In the Susquehanna modeling work, which extended over approximately a five-year period, a key issue was whether to pursue a Forrester "industrial dynamics" approach or an econometric modeling approach. After long discussions, it was decided to attempt to synthesize elements of both approaches in producing the Susquehanna model. Forrester and his colleagues, including Pugh and Roberts, who were engaged in our project, were correct in criticizing econometricians for their relative lack of attention to determining the functional forms of relations, lag structures and dynamic properties of models. On the other hand, the industrial dynamic approach in the mid-1960s did not involve much economic theory and use of data to estimate and test models with appropriate statistical procedures. In our modeling work, we attempted to use the best elements of both approaches to the extent that data constraints permitted." (quotation from Zellner, *Basic Issues in Econometrics*, U. of Chicago Press, 1984, p. xi. See also J. Forrester, "Information Sources for Modeling the National Economy" and my discussion of his paper in the *Journal.of the American Statistical Association*, 75 (1980), 555-569.)

To address the above issues, it was necessary to build a detailed model of the region, one that included demographic, economic and water sectors. Since possible river pollution problems are local in nature and the model was to produce measures of river flow and measures of pollution, namely dissolved oxygen sag curves for metropolitan areas along the river, it was important to model sub-regions, usually two or three counties along the river, of the entire region. For each sub-region, demographic, economic and water sectors were constructed. The demographic sector included age specific birth rates, death rates, marriage rates, labor force participation rates, and migration rates. The economic sector involved distinguishing heavy water using industries from others as well as relating sub-regional and national employment growth by industry with comparative cost conditions affecting firms' decisions to locate in the sub-region. Thus the model provided plenty of opportunity for dam construction to affect regional growth. However, as mentioned above, the model did not provide evidence for much stimulus to growth over and above the initial impact of dam construction whether a small, moderate or very large set of dams was constructed.

To illustrate how the model performed in simulation experiments, in one experiment which involved running the model for a fifteen year future period, it was noted that unemployment in the Pennsylvania Wilkes-Barre-Scranton sub-region, a mining region, fell to about the frictional unemployment rate, very low for this notoriously high unemployment region. We were very surprised by this outcome and studied the model's output very closely to determine what produced it. It turned out that as the years went by in the model, miners were aging and retiring, thus helping to alleviate the unemployment problem. Further, the migration equations of the model operated to cause young entrants into the labor force who had trouble finding employment to migrate out of the sub-region, another factor helping to alleviate the traditional unemployment problem. Last, with many unemployed workers in the sub-region and lower wage costs, the model's equations operated to have more plants locate in the region, another factor offsetting unemployment in the sub-region. It took about 10 to 15 years for these processes to offset the initial high unemployment in the sub-region and introduced us to relatively simple long run dynamic effects that explained the reduction of unemployment in this particular sub-region. These intensive simulation experiments with the model helped to appraise its performance and to improve formulations when needed. Out of this experience came our advice to "simulate, simulate, simulate" in the construction of models and to check simulation results against actual outcomes as much as possible.

To evaluate our model, our sponsors had us give day-long seminars on it at various places, including Washington, D.C., Cornell, Penn State, Drexel, and several other sites, many times with not only our sponsors and other interested parties in the audience but also our "competitors" from the Washington, D.C. research unit and elsewhere in the audience. At the end of the day, there were gala cocktail parties to help us celebrate when we had a good day and to help us forget when we had a bad day. Fortunately, many appreciated our study and efforts to arrive at a good answer to this important policy problem by considering it both heuristically and also in the context of a dynamic simulation model. As far as I know, the Corps of Engineers did not build its proposed set of dams.

IV. The Federal Reserve-MIT-PENN Model of the U. S. Economy

This large, quarterly model of the U.S. economy was constructed for the U.S. Federal Reserve System with the intent to use it in explaining past macroeconomic developments, prediction and policy-making. The version of the model that I was asked to consider included 171 equations, many of them nonlinear stochastic difference equations relating to the consumer, industrial, monetary and government sectors of the economy. See my article co-authored with Stephen Peck, "Simulation Experiments with a Quarterly Macroeconometric Model of the U.S. Economy," reprinted in A. Zellner, *Basic Issues in Econometrics*, U. of Chicago Press, 1984, for a more detailed description of this 171 equation model and the results of simulation experiments using it. My first reaction to the model was the question, "Does it have a unique solution?" The model was solved numerically by first linearizing it and then getting a solution to the linearized version. Whether such a solution is appropriate deserves study. Further, on getting the model up on the computer at Chicago and allowing students in my course to do experiments with it, a problem arose. The students had no difficulty in devising extremely interesting experiments but they had serious difficulties in explaining the outcomes of their simulation experiments. Just as we were confused, for a time, by the low unemployment rates in the mining region of the Susquehanna River Basin economy, the students had difficulties in interpreting the model's simulated outputs. It is no easy matter to understand the workings of a nonlinear dynamic model containing 171 equations.

To help promote understanding of the model and produce results that might lead to improvements in it, Stephen Peck and I did some rather direct simulation experiments with the model. In the first set of experiments, we decided to shock the model with small and large changes in two major policy variables, the money supply variable and the personal income tax rate variable. In these experiments, we wanted to determine whether the model's responses were "reasonable", whether they were approximately linear or nonlinear and symmetric or non-symmetric. In our article, Peck and I formulated operational measures of nonlinearity and asymmetry of responses to changes in a control variable, e.g. the money supply(M) or the federal personal income tax rate (T) and computed values of them for a variety of changes in M and T. For small changes, say a billion dollar change in M or a two percentage point change in T, most responses were symmetric except for that of the implicit price deflator for GNP. For this variable, the model-builders had built in some downward rigidity. For larger changes in these input variables, the degree of asymmetry increased for most variables. For rather large quarterly changes of 3 or 5 billion dollars in M, the model's algorithm failed to converge to a solution. We speculated in a footnote of our paper that with such large changes in M a nominal interest rate was forced to be negative and the program refused to compute the logarithm of a negative number. Thus these simple impulse response experiments revealed many interesting features of the model.

In addition, we decided to check the global properties of the model by putting it through a major depression. To create the depression, the input money variable, M = unborrowed reserves plus currency in the hands of the public was allowed to be fairly constant for 6 quarters, beginning in the first quarter of 1964 and then was moved downward by 5% per quarter for five quarters and for the remaining 10 quarters it was kept constant in value. This behavior of M did indeed produce a major depression with real GNP falling considerably and the unemployment rate jumping from about 5% to 12%. While these seemed to be fairly reasonable responses, the following rather surprising responses were noted: (1) The implicit deflator for real GNP barely turned down. (2) The AAA corporate bond rate and the treasury bill rate both moved up to about 15%. (3) The federal government's deficit and the net deficit of state and local governments moved up to astronomical levels. In response to the possibility that the large deficits may have in some way produced the very high 15% interest rates, we created another major depression in such a way as to avoid having government deficits rise very much with results similar to those presented above. In an effort to explain these anomalous great depression results, we resorted to a simple Hicksian IS-LM framework. In this framework, a large downward movement in M would cause the LM curve to shift upward to the left to intersect the IS curve at a low level of income and a high interest rate, just what was observed in our simulation experiment. Perhaps if the model had included real balance effects affecting the demands for durables, services, etc., the large downward change in M would have caused a downward shift in the IS curve to produce an even lower equilibrium value of income AND a lower rate of interest.

As this last simulation exercise indicates, it is useful to have a simple model with which to interpret the output of simulation experiments and to understand the workings of the large complicated model. With this in mind, it was recommended that the model-builders develop a core model of say 10 equations and to compare the workings and forecasts of the core model with that of the large 171 equation model. After the large model was put into operation at the FRB in Washington, DC, it was run for many years and complaints were that it did not forecast well, that its outputs were not understandable, etc. Finally, the model was shut down and efforts have been instituted to produce an improved model. This experience apparently is not unique to the U.S. In many other countries, complicated, large- scale, nonlinear, complicated macroeconometric models have been closed down and efforts are underway to find models that work well in explanation, prediction and control. Further, it appears that many are seeking sophisticatedly simple solutions to these modeling problems rather than "Stanley Steamer" models that often break down and sometimes explode.

V. The Marshallian Macroeconomic Model

Since the mid-1980s, my colleagues and I have been pursuing the structural econometric, time series analysis (SEMTSA) approach in an effort to produce models of economies that perform well. As explained and illustrated in our past papers, we start by testing the components of models and then put the tested components together in a multivariate model that is subjected to forecasting, simulation experiment and other performance tests. In this way, we hope to iterate in on an adequate model. Note that we do not start from an "encompassing model," say a vector autoregression (VAR, i.e. very awful regression model) since, as we have shown in previous publications, such a model implies marginal processes for individual variables that are very high order autoregressive moving average processes. Further, if one has a VAR for seven variables with six lags on each, each equation of the model contains 6x7=42 input variables. With so many free parameters to estimate, it is not surprising that VARs have not performed well in forecasting according to results reviewed by Zarnowitz, McNees and others.

In our work, we started by modeling an important variable, annual real GDP. Initially, we entertained a benchmark AR(3) model for annual rates of growth of real GDP for 9 countries. It didn't take long to learn that the AR(3) model did not forecast very well since it missed all the turning points. At the top of the cycle, its forecasts would tend upward while the economy was moving downward while at the bottom, the AR(3) model would trend downward while the economy was trending upward. To alleviate this problem, I recalled that Burns and Mitchell in their classic work, Measuring Business Cycles, discovered that two variables, money and stock prices, tended to lead in the business cycle using data for the U.S., U.K. Germany and France going back to the 19th century. Thus we introduced two leading indicator variables, the lagged rates of change of real money and of real stock prices, in our AR(3) model and renamed the model, an autoregressive-leading indicator (ARLI) model. Using this simple ARLI model and variants of it, we reported reasonably good point and turning point forecasting results using data for 18 industrialized countries using data from the early 1950s to the early 1970s for fitting and data from the early 1970s to the late 1990s for one year ahead forecasting. For 211 turning point episodes, the various variants of the ARLI model forecasted about 70% or more of the downturns and upturns successfully. Also, the empirical root mean squared errors of forecast, while not as low as could be desired, are competitive with those of some large scale OECD econometric models.

Given that variants of our ARLI model worked reasonably well, we provided various economic theoretical derivations of it from special versions of (1) an aggregate demand and supply model, (2) a Hicksian IS-LM model by C. Hong and (3) a generalized real business cycle model by C. Min. While this compatibility with certain macroeconomic theoretical models was satisfying, in general we, along with many others, did not find these particular macroeconomic models completely satisfying. In particular, as Modigliani, Ando and others recognized, they do not take account of particular sectors that play an important role in affecting the dynamic properties of our economy. Further, in real business cycle models, there is the representative firm. What happens if the representative firm shuts down? In this and almost all other theoretical and empirical models, no allowance has been made for entry and exit of firms, an important mechanism that produces equilibrium in Marshall's competitive industry models. Also, empirically start-up and shut-down decisions are very important in affecting cyclical movements of many economies. And last, given the diverse behavior of the various sectors, say agriculture, mining, construction, durable goods, retail trade, etc., it appeared to me and many others that some disaggregation may be helpful in explaining and predicting outcomes. The critical issue was how to disaggregate.

One day while shaving, it occurred to me that it might be worthwhile to disaggregate by industrial sectors using Marshallian models for each sector, namely a supply function for output, a demand function for output AND an entry/exit equation for three endogenous variables, price of output, quantity of output and the number of firms in operation. Fortunately, Veloce and I had formulated just such a model for the Canadian furniture industry and fitted it to illustrate the importance of taking account of the variable, the number of firms in operation, N, that appears in the industry supply function on aggregating firms' supply functions. In this paper and in my 2000 Ryuzo Sato Conference paper that incorporated neutral and factor-augmenting technological change, it was assumed that firms are operating with identical Cobb-Douglas production functions and maximizing profits under competitive conditions to derive individual firms' supply functions which were aggregated to obtain the industry supply function. Then on adding a log-log consumer demand function for output and a partial adjustment equation for entry that includes allowance for a fixed cost of entry, our three equation model was complete given the input variables such as demand shifters, income, etc. and supply shifters such as the real wage rate, the real price of capital, etc. On solving the three equation model for the implied equation for real sales of the sector, S = S(t), it surprisingly turned out to be in the following form (see derivation in my Ryuzo Sato Conference paper):

$$(9) \qquad (1/S)dS/dt = (a+g)[1 - S/(a+g)F]$$

where a and F are parameters and g denotes a linear function of the rates of change of variables shifting the sector's demand and supply equations, e.g. real income, real money, the real wage rate, the real price of capital, technological change, etc. If in (9), g = 0 or g = constant, the nonlinear differential equation has a solution in the form of the logistic function for industry real sales, a form that has been utilized in many earlier studies without its being derived from a traditional competitive Marshallian industry model.
Also, if g is a given function of time, say g = g(t), equation (9) is in the form Bernouilli's differential equation. In general, g may change value through time due to changes in the rates of growth of the variables shifting demand and supply relations. That such a simple model for real sales emerged from our demand, supply and entry model was indeed a pleasant surprise.

To test (9) empirically, in recent work we have employed, among others, the following discrete approximation to (9):

(10) $(1-L)\log S_t = a_o + a_1 S_{t-1} + a_2 S_{t-2} + a_3 S_{t-3} + x_t' b + u_t$ t = 1,2,…, T

where L = lag operator such that $L^n z_t = z_{t-n}$, $x_t' = (1-L)(\log Y_t, \log M_{t-1}, \log w_t, \log RS_{t-1})$, $b' = (b_1, b_2, b_3, b_4)$, and u_t is an error term. Also, Y is real income, M real money balances, w the real wage rate and RS a real stock price index. Analyses of (10) and variants of it were carried forward for eleven sectors of the U.S. economy using data from the early 1950s to the late 1970s to fit equations and data from 1980-97 for one year ahead forecasting with estimates updated year by year. As explained in the recent paper by B. Chen and myself, we employed various estimation and forecasting techniques in these experiments including Bayesian shrinkage, seemingly unrelated regression, and also Bayesian minimum expected loss estimation and two stage least squares that allow for possible endogeneity of the income and wage rate variables in (10), as well as other estimation and forecasting techniques. In these experiments, we were particularly interested in determining whether it pays to disaggregate in terms of forecasting accuracy. That is, we used equations in the form of (10) to forecast each sector's one year ahead value of real sales and then added the forecasts so obtained to forecast the economy's total real sales and its growth rate. Such "disaggregated" forecasts were compared to forecasts derived from several models implemented with aggregate data, e.g. an AR(3) model, an ARLI model and a one sector Marshallian macroeconomic model (MMM). Using the aggregate data, it was found that the MMM model outperformed the AR(3) and ARLI models by a fairly wide margin. Also, it was found that using the MMM disaggregated models to forecast the components of the total and adding up such forecasts to get a forecast of the total and its rate of growth provided better forecasts than any of the aggregate models implemented with aggregate data.

For example, for the one year ahead forecasts of the annual rates of growth of real U.S. GDP, 1980-1997, the mean absolute errors, MAEs, ranged from 1.17 to 1.38 percentage points for the alternative approaches mentioned above applied to equation (10) above. For a benchmark AR(3) model applied to the annual aggregate data, it missed all the turning points and its MAE = 1.71 percentage points, about 24 to 46 percent larger than the MAEs for the disaggregate forecasts. A MAE of about 1.2 percentage points compares favorably with those reported by Zarnowitz for forecasts of the rate of growth of real U.S. GDP made by the U.S. Council of Economic Advisors and other forecasting groups. These results indicate that IT PAYS TO DISAGGREGATE when disaggregation is done in a sophisticatedly simple manner.

It is expected that improvements in our sector models for the highly variable agricultural, mining, construction and durable goods sectors, perhaps to incorporate Stephen Peck's impressive investment functions and some nonlinearities suggested in work by Franz Palm and his colleagues may improve our current forecasting results. Also, by summing factor demands over sectors and adding factor supply equations, and making allowance for the emergence of new sectors, a complete Marshallian model of the economy is obtained. It will be of great interest to study the trend, cyclical and policy properties of such a model analytically and by use of simulation experiments. Also, checking further its point and turning point forecasting performance will be of great interest. It will be a pleasure to keep you informed of future developments. Many thanks for your kind attention and your thoughtful questions and comments.

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