

**A Symposium Sponsored By  
The Federal Reserve Bank of Kansas City**

**MODELING  
AGRICULTURE  
FOR POLICY  
ANALYSIS  
IN THE 1980s**





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FOR POLICY ANALYSIS  
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Federal Reserve Bank of Kansas City  
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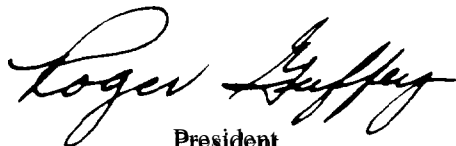
## Foreword

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Agricultural modeling was selected as the subject of our Bank's fourth symposium on major public policy issues for a number of reasons. Agricultural policy issues, in both the public and the private sectors, have become increasingly complex — and increasingly intertwined with other economic and political issues. In the years ahead, these issues will be of considerable importance and urgency to farmers and to nonfarmers alike. Yet the methodology used to support decisionmaking in these areas has not kept pace with the emerging issues. I trust that the proceedings of this symposium will identify these shortfalls in policy analysis methodology and will contribute to proposed solutions.

We feel fortunate to have assembled a distinguished group of participants on this program. We are equally pleased that those who attended this event were closely associated with policymaking and senior-level policy support at a broad range of both public and private institutions across the United States and Canada.

The proceedings were edited by Marvin Duncan, assistant vice president and economist with the Federal Reserve Bank of Kansas City, with assistance from Ann Laing Adair, assistant economist with the Bank.



**President**

Federal Reserve Bank of Kansas City



## The Contributors

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Before joining Schnittker Associates as a senior associate, **Lynn M. Daft** served as associate director of the Domestic Policy Staff at the White House and as a principal staff adviser to the President. He has done research in areas ranging from analysis of the impact of U.S. policy on world trade to evaluating options for welfare reform. Other government positions include service as principal analyst in the Congressional Budget Office and as senior economist with the Office of Management and Budget.

**Kenneth R. Farrell**, is a senior fellow and director of the Food and Agricultural Policy Program, Resources for the Future. Washington, D.C. Before joining Resources for the Future. Dr. Farrell was administrator for the Economics and Statistics Service of the U.S. Department of Agriculture. He has served as associate director of the Giannini Foundation of Agricultural Economics at the University of California-Berkeley, where he coordinated the university's extension programs. He is a former director and president of the American Agricultural Economics Association and was recently elected a fellow of the association.

**Richard L. Feltner**, consultant and former president, Federal Intermediate Credit Bank of Louisville, has for the last several years been deeply involved in the financial and marketing challenges that face U.S. agriculture. Before being named president of the Louisville FICB in 1977, he was the U.S. Department of Agriculture's assistant secretary for marketing and consumer services, Dr. Feltner, currently a consultant, has broad experience in rural development research, agricultural trade, and domestic food policy. and

is a contributor to the book, *Contemporary Agricultural Marketing*.

Before assuming his present position as professor of agricultural economics at the University of Maryland, **Bruce L. Gardner** served as professor of agricultural economics at Texas A&M University and as senior staff economist on the President's Council of Economic Advisers. He began his academic career with North Carolina State University, after receiving his **Ph.D.** from the University of Chicago. Dr. Gardner has authored or co-authored three books in the past two years.

**Dale E. Hathaway** is a principal member of Consultants International Group, Washington, D.C. Years of direct contact and negotiations with business and government leaders in the United States, Eastern and Western Europe, China, Japan and Latin America have given Dale Hathaway an in-depth knowledge of key economies around the world. A former under-secretary of agriculture, he has served as director and research administrator of the International Food Policy Research Institute and with the International Division of the Ford Foundation. Dr. **Hathaway** has been chairman and a professor in the Department of Agricultural Economics at Michigan State University.

As director of the Center for Agricultural and Rural Development and Charles F. Curtiss Distinguished Professor, Iowa State University, **Earl O. Heady** has won the American Agricultural Economics Association award for outstanding research 10 times since 1949. He has also served as adviser to the U.S. Agency for International Development, the U.S. State Department World Food Program, and the UN's Food and Agriculture Organization. His work in public service and consulting has taken him to 42 foreign countries. He is permanent chairman of the East-West Seminars for Agricultural Economists and is a fellow of the American Agricultural Economists Association and four other professional organizations.

**R. J. Hildreth** serves as managing director of the Farm Foundation. Before joining the Farm Foundation in 1962, Dr. Hildreth was assistant director of the Texas Agricultural Experiment Station. His public service includes appointments to the Joint Council for Food and Agricultural Sciences in the U.S. Department of Agriculture, and to the National Center for Voluntary Action. He is a past

president and director of the American Agricultural Economics Association.

**D. Gale Johnson** is chairman of the Department of Economics and Eliakim **Hastings** Moore Distinguished Service Professor, University of Chicago. Dr. Johnson has held a variety of positions in domestic and international public service, including the National Advisory Commission on Food and Fiber, the Trilateral Commission, the President's Task Force on Foreign Economic Assistance, and a stint as agricultural adviser to the Special Representative for Trade Negotiations. He has been vice president and president of the American Agricultural Economics Association and is currently a fellow of the association.

**Stanley R. Johnson**, professor of economics and agricultural economics, University of Missouri-Columbia, joined the economics faculty at UMC in 1964, and was chairman of the Economics Department from 1972 to 1974. He has also been on the faculty of the University of Connecticut and has held visiting professorships with the University of California, Purdue University, and the University of Georgia. His special research interests are econometrics and control theory.

**Richard E. Just**, professor of agricultural and resource economics, University of California-Berkeley, has received the Quality of Research Discovery Award from the American Agricultural Economics Association as well as the Outstanding Published Research Award from the Western Agricultural Economics Association, of which he is currently president. Dr. Just joined the faculty of the University of California-Berkeley after earning his **Ph.D.** there in 1972.

**William E. Kibler** has nearly **30** years of experience in tabulating and processing American agricultural data. Within the USDA's Statistical Reporting Service, he has headed the Research and Development Branch, the Standards and Research Division, and the Survey Division, and he is current deputy administrator for statistics in the Economics and Statistics **Service**. He has been a member of the USDA's graduate school faculty in mathematics and statistics for 10 years.

**Lawrence R. Klein** is Chairman of the Professional Board, Wharton EFA, Inc., and 1980 Nobel Laureate in Economics. A long and distinguished career of economic research and writing began when he received his **Ph.D.** from the Massachusetts Institute of Technology. Dr. Klein won the 1980 Nobel Prize for his pioneering work in the use of econometric models in business forecasting. He is a past president and distinguished fellow of the American Economics Association, and has served as correspondent and editor of several financial and economic journals.

Before starting his career in public service, **William G. Leshner** conducted research and taught at **Cornell** University. He has served as an agricultural legislative assistant for Sen. Richard **G. Lugar (R.-Ind.)**. Dr. Leshner became an economist for the U.S. Senate Committee on Agriculture, Nutrition, and Forestry in 1978, and was sworn in as assistant secretary for economics in the U.S. Department of Agriculture in April 1981.

Before joining Deere & Co. as director of market economics, **Dean McKee** was chief of the Production Resource Branch of the U.S. Department of Agriculture's Economic Research Service. He had earlier held several positions in the ERS, which he joined after serving as assistant professor of agricultural economics at Michigan State University.

A researcher, writer, administrator, and adviser, **Don Paarlberg** has divided his professional life between academic work at Purdue University, where he is professor emeritus, and his service in government. He has been Assistant to the President, assistant secretary of agriculture, and director of agricultural economics at USDA. He has written many books and articles, including *American Farm Policy* and *Farm and Food Policy: Issues of the 1980s*. Dr. Paarlberg has served as economic adviser to four secretaries of agriculture during his long government career.

**John B. Penson, Jr.** is professor of agricultural economics, Texas **A&M** University. When he received his **Ph.D.** from the University of Illinois, his thesis won a national award from the American Agricultural Economics Association, and three other



books he has co-authored have been published in the past two years. He has been a consultant to the governments of Israel and Syria and to the state of Alaska. A prolific contributor to economic journals, Dr. Penson was a visiting scholar at the Federal Reserve Bank of Kansas City during 1980-81.

**Gordon C. Rausser**, professor and head of the Department of Agricultural and Resource Economics, University of California-Berkeley, has received high honors as a researcher and has served as editor of three economics and statistical journals. He has taught at the University of California-Davis, Harvard University, Iowa State University, and the University of California-Berkeley, and was a visiting professor at Hebrew University and Ben Gurion University. Dr. Rausser has served with the Ford Foundation in Argentina, the U.S. Department of Agriculture, the World Bank, and the Federal Trade Commission.

Before taking his position as professor and head of the Department of Agricultural and Applied Economics at the University of Minnesota, **G. Edward Schuh** served as deputy under-secretary for international affairs and commodity programs at the U.S. Department of Agriculture. He held academic positions in agricultural economics at Purdue University from 1959 to 1978, during which time he also served on President Ford's Council of Economic Advisers and worked with the Ford Foundation in Brazil. Dr. Schuh has extensive international experience in Latin America and India. He is president and a fellow of the American Agricultural Economics Association.

**Luther Tweeten**, Regents Professor of Agricultural Economics, Oklahoma State University, has done a great deal of research and writing in the areas of economic development, human resources, and public policy for agriculture. He has written or co-authored four books, including *Foundations of Farm Policy*. He is a former director and immediate past president of the American Agricultural Economics Association, and in 1972, he was honored by the Western Agricultural Economics Association for "outstanding published research in agricultural economics." Dr. Tweeten has served as a consultant to the Congressional Office of Technology Assessment and as a director of Bread for the World.



# The Value of Models in Policy Analysis

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*L.R. Klein*

## **A Model as a Simplification of Reality**

There is no single model of an economic system. In general, a model is a simplified approximation of reality, and there must surely be many such approximations. Therefore, we have large and small models, real and nominal models, sector and aggregative models, dynamic and static models, long- and short-run models, and so on. The model being used at any one time is undoubtedly chosen, in part at least, according to the objectives for its use. Some models are very general in design, in order to be available for a variety of applications, but no economic model, in a very practical sense, stands apart from its end use. Special purpose models, to the extent that they can be made available, are the best for difficult problems.

Among the many classes of models, I am going to be concerned, in this paper, exclusively with econometric models. Accounting models, mathematical programming models, systems-dynamic models, general equilibrium models and other types are not going to be considered or implicitly assumed. I shall work exclusively in this essay with mainstream econometric models, typified by those of Wharton Econometric Forecasting Associates, Data Resources, Inc., the Federal Reserve Model, the Michigan Model, and similar systems.

These mainstream models are used in many ways, the most visible of which is in forecasting the macro economy or significant parts of it. The forecasting application is important and must continue to occupy a great deal of the model builder/operator's time, but surely the largest single use of econometric models is for study of economic alternatives. This is how they are best used in the policy

process.

Once a model has been specified, i.e., given a parametric structure, and estimated on the basis of available data, it is ready for application. The most important single tool for use of a model is analysis. Whether it is a pure forecast simulation or a hypothetical policy simulation or a stylized scenario, it is always a simulation of some kind that underlies any application of the system.

The mathematics, statistics, and numerical analysis of simulation are straightforward. A simulation is a *solution* of an economic model. This solution is an integral (in finite terms, usually) of a dynamic system, starting from fixed initial conditions. The generating of solutions is at the base of using models in the policy process.

### Formal Political Economy

The variables of an econometric model can be classified in a variety of ways, but the most revealing classifications are into:

- endogenous variables
- exogenous variables
- target variables
- instrument variables

Endogenous variables are variables that are generated, or explained, by the model. They are the objectives of model building.

Exogenous variables are external to the system. They have impact on the endogenous variables, but there is no feedback from the economy (or the model of it) to the exogenous variables. Other expressions for these same two classes of variables — endogenous and exogenous — are jointly *dependent* variables (endogenous) and *independent* (exogenous) variables. The independent variables "drive" the model, apart from initial conditions and functional form.

For purposes of policy analysis, the other split is very helpful. The concepts of targets and instruments are due to J. Tinbergen.<sup>1</sup> A target is a policy-set value (or group of values) for an endogenous variable. Four percent inflation, low (4.0 percent) unemployment, high (4.5 to 5.0 percent) growth, budget balance, a strong dollar, and other pertinent magnitudes are target objectives for public

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<sup>1</sup> J. Tinbergen, *Economic Policy: Principles and Design*, Amsterdam North-Holland, 1956.

authorities who need to try to reach certain goals for the economy.

Not all endogenous variables are targets — only those with a deep meaning and commitment for the policy maker. At the macro level, comprehension, appreciation, meaningfulness for the electorate, and manageability are criteria that limit the number of targets, certainly fewer than ten magnitudes, and possibly no more than five are practical limits at the present time. If there are hundreds or thousands of endogenous variables, it is clear that a tiny minority of such variables are used as targets at any one time. The remaining hundreds are not ineffectual; they are simply having a passive transition phase.

By the same token, not all exogenous variables are *instruments*. They are controllable magnitudes that are set by public authorities in order to achieve certain results. Among the thousands of exogenous variables in economic systems only a few (fewer than ten) are selected for policy control purposes. Most exogenous variables are not terribly concerned with contemporary policy control, in order to achieve stated aims, or targets.

In the formal design of an econometric model system for policy analysis we note that there are two types of endogenous variables — targets and other — and that there are two types of exogenous variables — instruments and other. In abstract terms we write:

$$F(y_t^t, y_{t-1}^t, \dots, x_t^t, x_{t-1}^t, \dots, z_t^t, z_{t-1}^t, \dots, w_t^t, w_{t-1}^t, \dots, \theta^t) = e_t$$

$$F = \begin{matrix} f_1 \\ f_2 \\ \cdot \\ f_n \end{matrix}$$

$y_t$  is a column of target endogenous variables at time  $t$ .

$$y_t = \begin{matrix} y_{1t} \\ y_{2t} \\ \cdot \\ y_{n_1t} \end{matrix}$$

$x_t$  is a column of non-targeted endogenous variables at time  $t$ .

$$x_t = \begin{matrix} x_{1t} \\ x_{2t} \\ \cdot \\ x_{n_2t} \end{matrix} \qquad n_1 + n_2 = n$$

$z$ , is a column of instrumental exogenous variables at time  $t$

$$z_t = \begin{matrix} z_{1t} \\ z_{2t} \\ \vdots \\ z_{m_1t} \end{matrix}$$

$w$ , is a column of non-instrumental exogenous variables at time  $t$ .

$$w_t = \begin{matrix} w_{1t} \\ w_{2t} \\ \vdots \\ w_{m_2t} \end{matrix} \quad m_1 + m_2 = m$$

$e$ , is a column of random variables.

$$e_t = \begin{matrix} e_{1t} \\ e_{2t} \\ \vdots \\ e_{nt} \end{matrix}$$

$\theta$  is a column of parameters.

$$\theta = \begin{matrix} \theta_1 \\ \theta_2 \\ \vdots \\ \theta_r \end{matrix}$$

The formal approach is clear enough. The parameters  $\theta$  are estimated from historical sample data. They are denoted  $\hat{\theta}$ . Given  $\hat{\theta}$ , initial conditions — lag values of  $x_t, y_t, z_t, w_t$  — and values of exogenous variables over a projection or solution period, estimate  $y_t$  and  $x_t$ . This is a dynamic solution. using lags as initial conditions but generating values of  $y_t$  and  $\mathbf{x}$ , as carryover initial conditions for the next period of solution. It is a non-stochastic simulation if  $e_t$  is put at its mean (zero) value or at some a priori non-zero value. If the values of  $e_t$  used in the simulation are drawn by a random process we obtain a stochastic simulation.

In the first instance, a *baseline* solution is computed. This would be with standard or best judgmental values for the exogenous variables. When it comes to policy analysis, however, we estimate deviations from the baseline simulation by changing exogenous inputs or by changing parameters of the system, if they are policy

controlled.

Policy has goals; these are expressed by the **target** values  $y$ . The policy maker attempts to hit these targets by changing values for  $z$ . If there are equal numbers of elements in  $y$ , and  $z$ , then the econometrician simply reclassifies the two. Target values become exogenous, because they are given by the policymaker. Instruments become endogenous, because they are to be computed for the policymaker.

This simple *inversion* of the simulation problem is not generally possible when the number of targets exceeds the number of instruments. We would then try to come "as close as possible," in some well defined sense, to the target values by judicious choice of instruments. The procedures for doing this fell under the heading of *optimal economic policy methods* or *optimal control* theory, as that subject is known in the engineering literature.

- Although some elements of the exogenous vector,  $w$ , are not controllable as instruments, the policymaker can try to become aware of various alternative consequences of changed values by altering the inputs for  $w$ , and computing corresponding estimates of the solution. Possible responses to oil price shocks or harvest failures are typical examples of policy simulation in preparation for adverse circumstances.

One way to use models in the policy process would be to follow the techniques of optimal control and allow in a probability sense for error by using the extensions of the methods, known as stochastic control. Another approach, by far the most prevalent, is to proceed by search and experimentation. We have learned to overcome the most serious computational problems in the application of control theory methods to large scale economic systems, consisting of hundreds or even thousands, of equations. Yet there is a feeling that public authorities are not yet ready for the automatic approach of control theory and prefer to proceed with models, among other devices, by search and experimentation.

Alternative assignments of values to the elements of  $z$ , and, in some cases, to  $\hat{\theta}$  with simulation of each set of values gives the policy analyst a large menu of possible economic developments from which to choose. Also, *scenario* analysis of different choices for the elements of  $w$ , together with choices for  $z$ , and  $\hat{\theta}$ , enable one to think in an analytical way about possible alternative futures. When policymakers find combinations of input values that lead to

desirable model solutions, they choose the configuration that they like. In actual practice, models will not be used alone in this **search/experimentation** mode, but will be combined with informational analyses from other sources, but model results are almost certain to be one of the most serious sources of information in reaching ultimate policy conclusions.

It is useful to think how agricultural models fit into this frame of analysis. A model of the agricultural sector is like a model of any other major part of the economy. In the abstract, it is an equation system, dependent on endogenous and exogenous variables, with both targets and policy instruments. They are also dynamic and stochastic equation systems.

There are, however, a few distinctive features about an agricultural sector model that are worth noting in relation to its applicability for policy analysis. First, it is a sector model and in that respect is an incomplete system when looked upon from a substantive point of view. In the United States, agriculture is an important sector, but it does not dominate the economy as it does in other countries, mainly large developing countries where population pressure imposes a burden on available food supplies. To a large extent, agriculture depends on the industrial economy in the United States and not vice versa, but agriculture does play a major role in determining a most sensitive component of the price level. It is also a major supportive factor in our net trade position; and it is important for regional politico-economic patterns. Either agriculture can be modeled as a satellite system with linkages to the non-agricultural base of the economy, with some degree of feedback, or agriculture can be modeled as one among several distinctive sectors in a large multi-sectoral system held together by some such device as an input-output system. The disadvantage of this latter approach is that it limits the amount of agricultural detail that can be included in an already large system of a few thousand equations. In a stand-alone mode, a complete agricultural model like the Wharton Model of *the* Agricultural Sector would have as many as 388 equations by itself. This would be the type of satellite system that would be used with linkages to the nonagricultural sector if the first approach is to be used.

The second distinctive aspect of agricultural model specification is the incorporation of a major uncertainty factor caused by the influence of weather variation. Agricultural supply responds to



price and other economic factors in a systematic way, but it is also strongly affected by natural growing conditions, the most volatile of which is weather. General climate, crop disease (or health), and other natural factors have significant effects but such weather variables as rainfall, soil moisture, wind, temperature, storm, and similar phenomena are all highly relevant.

While the application of fertilizer, insecticide, and irrigation are all man-made decisions that attempt to modify or change natural factors, many of the effects of weather, climate, and other natural factors cannot be dealt with by human decisions. The  $z$  and  $w$  variables both occur in agricultural sector models. The  $z$  variables are the input levels of fertilizer, insecticide, and irrigation, but the natural factors are  $w$  variables. They cannot be controlled effectively. At one time, it appeared that cloud seeding might enable man to have a significant impact on rainfall, but an effective degree of control is not visible in the near future. The distinctive features of agricultural sector models can be succinctly described in terms of the relative variance of the  $z$  and  $w$  variables. As compared with model structure for other sectors of the economy, the relative variance of  $w$  relative to that of  $z$  is large.

If we cannot control important  $w$  variables, what can we do about them? First, it is important, at the estimation stage of model building to have the best attainable values for the quantitative effects of  $w$  variables, even if they cannot be controlled. This is so because we need to know how much to expect from  $w$  variation, and we do not want to bias the estimated effects of the other variables. Within the realm of scientific modeling, econometric models of all *types*, whether agricultural or other, have comparatively large noise-to-signal ratios, and we have no more control over "noise" than over the  $w$  variables of an agricultural sector model. The difference between the two kinds of variables is that  $w$  variables are directly measurable, while the noise variables are not. The latter are generated by the laws of probability (assumed), while the generating process of  $w$  variables may or may not be known.

In the most favorable case, the laws governing the  $w$  variables are the subject of investigation of another branch of science, either meteorology or climatology. Short run weather factors are estimated by meteorologists for the economist. While, in principle, we can use meteorological estimates of rainfall, temperature, and other weather indicators, the trouble is that they are useable in terms of

degree of accuracy, over only a very brief horizon. Short term meteorological forecasts, of a few days have use and accuracy that are similar to those found in projections from economic models, but month-ahead or year-ahead weather projections are very unreliable.

The usual way of taking this aspect of uncertainty into account in applications of economic models is to prepare, first, an economic projection on the basis of normal weather patterns and then to consider deviations above and below normal. It is possible that meteorological data could be used to estimate probabilities of departure from normal; in this way an expected projection could be made, as from

$$P_0 \hat{Y} + \sum_{i=1}^n P_i^- \hat{Y}_i^- + \sum_{i=1}^n P_i^+ \hat{Y}_i^+$$

where  $\hat{Y}$  is the solution of the economic model using normal inputs, occurring with a relative frequency or probability of  $P_0$ ;  $\hat{Y}_i^-$  is the solution for the  $i$ -th level input below normal, occurring with relative frequency  $P_i^-$ ; and  $\hat{Y}_i^+$  is the solution for the  $i$ -th level input above normal, occurring with relative frequency  $P_i^+$ .

In the calculation of standard error of forecast from a linear model we construct a quadratic form in terms of departures of exogenous variables from their average values, the weights (coefficients) being covariances of the estimated coefficients. We could add a quadratic form to that having as coefficients the covariance of exogenous variables — in this case, the meteorological variables.

By drawing on the expertise of meteorology, and combining that with economic interrelationships, we can use models in a way that takes account, in a quantitative sense, of the uncertainty involved even though we cannot make a precise point estimate of the variable representing the uncertain magnitude.

### Some Examples of Policy Analysis

The discussion thus far has been quite general. It is time to take a look at some specific examples of what is meant by policy analysis, using an economic model. I shall begin with a macro analysis of the most relevant and discussed national issues contained in President Reagan's economic program. There are four main categories of action that have significant impact on exogenous variables of a model, in this case the Wharton Quarterly Model of the U.S. Economy.

1. Increases in defense spending.
2. Reductions in non-defense (federal) spending.
3. Reductions in personal federal taxes in three installments (10/1/81, 7/1/82, 7/1/83). Guideline lives for industrial capital are also shortened, for tax purposes.
4. Monetary policy is to be kept restrictive, in order to achieve specific targets for **M1-B** and **M2** growth.

Each of these policy assumptions has been factored into the Wharton Model for latest projections; some of the assumptions are statutory and some are our own interpretations of budgetary or stated commitments.

*Defense Spending.* Increases in military compensation of **14.4**, **8.9**, and **7.9** percent are introduced on October 1 of **1981**, **1982**, and **1983**, together with corresponding civilian raises of **4.8**, **7.0**, and **7.0** percent, respectively. By the middle of fiscal **1982**, military manpower is assumed to increase by **50,000** persons and by another **25,000** afterwards. For FY **1982**, the defense spending total is **\$172.8** billion, representing an increment of about **17** percent in nominal terms and about **7.0** percent in real terms. For **1983**, the real growth is increased to about **9.0** percent.

*Non-Defense Spending.* For goods and services, this figure is put at **\$77.4** billion for FY **1982**. This total includes pay increases of **4.8** percent, **7.0** percent, and **7.0** percent at the start of the next three fiscal years. Also, purchases of **250,000** barrels per day for the strategic petroleum reserve are included. In real terms, spending for goods and services is practically unchanged or falling slightly for the next year. In **1983**, there are significant real cutbacks of some **9** percent. This allows nominal increases of about **7.0** percent in FY **1982**, but hardly any change in **1983**. Transfer payments depend on the level of economic performance. We have assumed that the administration's targets for foodstuffs, medicare, and other programs will prevail. Also, interest costs will depend on behavior and results in financial markets. In total, the Wharton budget assumptions for FY **1982** come to **\$715** billion, while the administration's estimate is **\$705** billion. In FY **1983**, the Wharton total is **\$788** billion.

*Taxation.* Personal taxes have been reduced, in line with the administration's program (approved by Congress) for a reduction of **5** percent in rates on October **1981**, followed by **10** percent reductions on July 1, **1982**, and July 1, **1983**. The Wharton forecast also

allowed for the reduction of the maximum rate on investment income, the elimination of the marriage penalty, the deductions for income earned abroad, and the new deductions on estates and gifts. Some other minor tax reductions were also factored into the forecast.

The reduction in guideline lives for corporate depreciation allowances has been estimated at about **40** percent, effective January 1, **1981**. Some miscellaneous indirect taxes have been increased.

Monetary Policy. The guidelines of the administration made known publicly are simply to show restraint in expansion of money supply and to follow monetarist practices, i.e., to hold monetary aggregates to target ranges, while letting interest rates follow a course determined by market supplies and demands for funds. In a more formal sense, the Federal Reserve System has fixed guideline limits for **M1-B** and **M2**. For **M1-B** (adjusted for NOW and ATS accounts) the target range is **3.5-6** percent, and for **M2** it is **6-9**. The main instrument for control in the Wharton Model is nonborrowed reserves. This variable is fixed to a path that produces a solution for **M1-B** growth between 5 and 6 percent on average in **1981-83**, and **M2** growth between 8 and 11 percent. The later drifts above the range at the end of the solution path in late **1982** and **1983**. Nevertheless, we judge this as an overall restrained monetary policy.

The principal policy assumptions for the projection of the model being discussed are covered under the four heading listed above. There are two other important assumptions that must be dealt with in order to plan these political assumptions in the context of a meaningful result. These two exogenous areas are energy and agriculture. With respect to energy, the main assumption is that OPEC will make no price increases during the second half of **1981**. During **1982**, prices are increased **quarterly** at annual rates of 10 percent. During **1983**, this figure is raised to **11** percent.

The assumptions about weather, plantings, and main crop yields (wheat, corn, soybeans), lead to increases of the food CPI of 8 percent for **1981**, **9.6** percent for **1982**, and **9.4** percent for **1983**.<sup>2</sup>

Given these policy and other exogenous assumptions for the next three years, how do we interpret the outcome and the success of the

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2. The excellent crop reports (mid-August) for the United States would probably lower these estimates of the food CPI, especially in 1982, back to the estimate of food price inflation prevailing in 1981.

policies? In general, the Wharton Model estimates that the administration will move toward most of the targets that it has set, namely lower inflation, stronger growth, and lower interest rates. These are only some of the main targets. But it does not appear, from the Wharton calculations, that it will achieve one other important target — a balanced budget by 1984.

TABLE I

Wharton Model Forecasts and Administration Targets

	1980		1981		1982		1983
	(observed)	Model	Adminis- tration	Model	Adminis- tration	Model	Adminis- tration
Change in real GNP (%)	-0.16	2.3	2.6	3.1	3.4	4.4	5.0
Change in GNP deflator (%)	9.0	8.8	9.6	8.4	8.0	8.1	7.0
Treasury bill rate (%)	11.4	15.2	13.6	15.4	10.5	12.7	7.5
Deficit (fiscal year, \$billions)	54.9	54.3	55.6	80.0	42.5	98.6	22.9

Source: The Wharton Model forecast of July 29, 1981 and the Mid Session Review of the U.S. Government

The Review was released prior to the report of the 2nd quarter GNP data of July 20

1980 was a recessionary year, and the Wharton forecast is for a continuing recovery during 1981-1983. The administration also looks for a recovery, but one that is considerably stronger than the Wharton estimate. Similarly, they look for a better inflation performance (after a worse estimate for 1981) and much lower interest rates. The Wharton Model, however, sees a basic contradiction in the administration position, and this is a main use of models: to examine internal consistency. The model estimates that interest rates will be higher as a consequence of the internal deficit and the restrictive monetary policy. Since interest costs are now more than \$70 billion for the federal government, this is an item that can knock deficit estimates askew. Other aspects are higher transfers and reduced revenues associated with a softer real economy. These are the reasons why the model gives a message to policy makers that their plans will not achieve their targets.

In order to avoid the range of \$100 billion deficits what policies might the authorities undertake?

- They could rescind part of the three year tax cut program.
- They could make more expenditure cuts in the budget, defense or non-defense.
- They could increase indirect taxes.
- They could adopt an easier monetary policy, with lower interest rates.

Each of these policies could make significant contributions to lowering the deficits. It would undoubtedly take some combination of all together in order to account for some \$100 billion of estimated deficit, but it is a matter of quantitative magnitude — how much of a rescaling in the tax cut program, how much in expenditure cuts, etc.?

If the entire tax reduction plan for individuals were to be eliminated — in other words, if tax provisions that prevailed prior to August 13th were kept in place — the budget balance target would be met, but at the expense of higher unemployment and slower real GNP growth; therein lies the contradictory nature of the policy program, as estimated by the model.

Some of the individual options have been examined one at a time, in model simulations. The results are:

	Deficit Reduction (NIPA basis)
Rescind the 1983 round of personal tax cuts	\$30.0 billion
Tax gasoline by \$0.50/gal.	\$45.0
Easier money (reduce short rates 100-150 basis pts.)	\$ 9.0

These have not been estimated on a cumulative basis, and they all have differential impacts on other performance variables, but they do indicate the magnitude of the problem and the amounts that would be left for additional spending cuts if that were to be the residual item to make up the shortfall in achieving budget balance.

All these forecasts, including the **baseline** cases (both of the model and of the policymakers) are subject to error: therefore, one should not try to aim for pinpoint precision in policy formation. It should be pointed out, however, that projections in which a policy-induced simulation is compared with a baseline simulation are likely to benefit from error cancellation; i.e., the errors are correlated between two solutions being compared. This makes for better precision in comparative policy evaluation than in absolute forecasting.

Models have been used in more specific policy analysis than in this example of overall macro management of the economy. International models, comprising separate models for individual countries, have been simulated together, in project LINK, to study oil **interruptions**, oil pricing, and harvest failures, as well as general policy coordination among countries. By contrast, a specific policy appli-

cation of models that is more related to the interests of this conference is a case worked out for U.S. agriculture, using the Wharton Agricultural Model, together with the Wharton Quarterly Model of the economy as a whole. The case to be considered is one of "parity pricing," which became a national issue in the spring of 1978.

During 1977 favorable crops in the United States and elsewhere contributed to low inflation rates but also to relatively poor farm income. Costs continued to rise for farmers, and they lobbied for full parity pricing of agricultural products in 1978, by setting targets at projected parity levels of October 1978 on a 1910-14 base. Increases from that date were to be based on changes in production costs. The figures under discussion are outlined in Table 2.

TABLE 2  
Prevailing Market Prices and Parity Projections,  
May 24, 1978  
(dollars per bushel)

	Prevailing Prices May 24, 1981	Projected Parity Price (October, 1978)	Parity Projected by Production Cost Increases (Oct. 1979)
Wheat	3.24	5.17	5.36
Corn	2.67	3.62	3.75
Soybeans	7.08	8.55	8.85

The effect of these parity price projections on the national and agricultural economy were estimated by joint simulation of two Wharton Models.'

The Wharton Agricultural Model was simulated, using inputs from the Wharton Quarterly Model (general inflation, national income performance, world trade, and related magnitudes). The agricultural model solution also used the parity price values for 19 commodities (16 others, in addition to those important ones in table 2). The results were so different from previous solutions, of the agricultural model that the Wharton Quarterly Model, had to be re-solved, with the higher food prices, changed trade values, and related magnitudes. National economic variables were then fed back into the agricultural model for a new solution. The iteration process

3. In the policy context, these kinds of simulation results were used by Dr. Dean Chen in his testimony before the Senate Committee on Agriculture, Nutrition, and Forestry, March 2, 1978.

was halted at this stage, because, in a practical sense, convergence was attained. Table 3 shows the results of two simulations for 1979, with and without full parity pricing.

The parity requests were not granted. This model scenario showed that it would have been quite inflationary and very expensive to the federal government — more expensive than a \$20 billion tax cut that would eventually serve a much broader segment of the national population. Agriculture would have suffered significantly. The political choice was unacceptable, and full parity was rejected.

**TABLE 3**  
Parity Pricing Estimates of the Wharton Agricultural  
Model and Baseline Forecasts, 1979

	Parity Pricing	Baseline Forecast
Index of prices received by farmers, (1981-14 = 100)	731.6	462.1
Consumer Price Index for food (1967 = 100)	239.5	211.5
Net Farm Income (\$ billion)	69.8	21.9
Wheat (bu. million)		
domestic disappearance	766.3	806.3
exports	959.9	1127.3
Corn* (bu., million)		
domestic disappearance	4544.3	4486.6
exports	1579.7	1728.0
Soybeans* (bu., million)		
domestic disappearance	937.9	966.4
exports	458.1	597.5
Cattle and calves on feed (head, million)	11.5	11.8
Pig crop (head, million)	43.2	44.8
GNP (\$1972, billion)	1443.2	1455.7
GNP deflator (1972 = 100)	163.6	159.7
*Crop year estimates, 1978-79.		

### Overall Assessment

It could be argued plausibly that the examples cited could have been adequately dealt with by non econometric methods. That is undoubtedly true, but some kind of model, explicit or implicit would be needed to reach intelligent conclusions. All such policies



have quantitative dimensions, and it is not adequate to argue for or against them on purely qualitative grounds.

The main point about such calculations and supporting argument from an econometric modeling viewpoint is that they are but two of many, many such analyses that can be produced through the medium of model simulation. These are, by now, fairly standardized computations, and models are analyzed on virtually a daily basis for alternative consideration of different inputs. An entire technique and methodology are thus available for ready use, and the results retain a high degree of internal consistency.

It may well be asked, how good are the model findings in policy analysis? This issue was raised in connection with appraisal of the contemporary macro estimates for the administration's policies. In the case of parity pricing, the proposal is so unusual that it may be rejected immediately on the basis of figures that are widely different from those deemed acceptable, in which case model analysis might tend to be superfluous. In the case of closer correspondence between two alternatives, accuracy analysis of the underlying model is a highly relevant issue.

If policies that are analyzed through model comparisons are not adopted, it is difficult to determine whether or not the analysis is correct because there is no observational material on performance for policies that are not adopted. Similarly, even if policies are adopted after model analysis, it is not possible to assess the full extent of accuracy because it is not known where the economy (or parts of it) would have been in the absence of policy. Our problem is that we are not working in an experimental science and we use only non-experimental information for either estimation or testing of analysis. We have data only on what actually happened and not on the alternatives that are relevant for the comparison.

We do, however, recognize failure, in an absolute sense. We recognize that when President Johnson's tax surcharge and expenditure control act of 1968 was finally adopted, the model analyses that predicted a significant fall in inflation as a consequence of the restrictive legislation were in error. Similarly, when the oil embargo was imposed by OPEC in late 1973, the model analyses that predicted a rise in inflation from about 5 to about 8 percent were in error. They should have predicted a rise to about 12 percent.

In both cases, however, Wharton analyses were quite correct in their assessment of movements in real output. The recession **begin-**

ning in **1969** and **1973** were estimated in advance by the model; so partial validations were made, if not for the underestimation of inflation. At this point, I do not propose to go into detail about why models underestimated inflation in **1968** and **1973** or whether they did a better job than that of other methods. The main issue before us now is how to assess model performance, in general, for purpose of policy analysis, and my point of view, is that model validation in forecasting is *all* that we have to go on, in a concrete sense. Credibility in model performance must be built up on the basis of **the** ex ante forecast record. From the experiences of singular occasions, the ability of a model to forecast cannot be determined with any substantial degree of confidence. Any one, two, or three replications of success could be a chance event — luck. But if a model is used over and over again in repeated attempts at forecasting, a statistical distribution of successes and failures, with quantitative magnitudes of error, can be constructed. A poorly specified model — indeed, an incorrect model — will not perform well in repeated circumstances. The Wharton Quarterly Model, for example, has **been** projected and tested every quarter since **1963**. That is a long record. The model has undergone changes, as data and economic reasoning have changed over this period. In addition, there have **been** personnel changes over the years, but continuity far outweighs change, and I do believe that an appraisal of the Wharton Model as an instrument for policy analysis should be based on this 18-year, 22-quarter fund of experience. Other Wharton models — Annual Model for medium term analysis, the Agricultural Model, the World Model, the Philadelphia Model, the Mexican Model', the Brazilian Model, and others, should be similarly judged, but by fewer data points, for error measurement.

As forecasting devices, the Wharton Model and similar mainstream econometric models have stood the test of time. In the repeated investigations of Stephen McNees of the Boston Federal Reserve we find substance for the conclusion:<sup>4</sup>

The forecasts examined above must be considered "good" until other forecasters document that it was possible to have produced systematically more accurate predictions.

McNees' monitoring of the Wharton forecasts, together with

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4. Stephen K. McNees. "The Forecasting Record for the 1970s," *New England Economic Review*, September/October, 1979, pp. 33-53

those of other organizations that regularly make such projections and policy analyses with models; indicate the following sizes of errors, measured in growth rates cumulated over four quarters,

GNP price deflator	1.5 percent
Real GNP	1.4 "
Employment	1.0 "
Consumer expenditures (non-durables)	1.3 "
Consumer expenditures (durables)	6.1 "
Residential investment	8.1 "
Business fixed investment	3.7 "
Money supply (M1)	1.8 "

time span: 1971:I-1979:II

Accuracy is not uniform; there are definite grounds for improvement, but these are tested procedures that have been carefully followed. It is this repetitive testing that provides a basis for relying on model methods for policy analysis.

There was a brief period, 1973:III-1975:II, when price projections understated inflation, but an excursion into energy economics which suddenly became relevant at the new price relatives that were established in 1973-74 soon corrected this deficiency.

These model forecasts were not pure, in the sense that equation adjustments were introduced in order to cope with data revisions, temporary behavioral shifts, and statutory changes. But the adjustments made to the models in order to line up starting values over a forecast horizon were kept intact for policy analyses. Thus, if a system were adjusted for shifts in the above factors in order to achieve better forecasts, it can be presumed that the same adjustments should prevail for policy analysis, too.

Dr. McNees found a standard of comparison for model based forecasts in the average judgmental forecasts collected from a panel of members of the American Statistical Association. In general he concludes that the judgmental forecasts are no worse than the econometric model forecasts. I would claim that as the horizon lengthens, the Wharton forecasts tend to be a bit better than the ASA average. But if model forecasts are at least as good as the best of alternatives, they can exploit their comparative advantage of being ready for quick and frequent analysis of policy alternatives. The ASA judgmental forecasts are not available on the basis of policy alternatives.

Another standard of comparison is the estimated error implied in

revision of the NIPA figures by the agency that compiles them — first as preliminary approximations and later as benchmark revisions based on later, more complete data. The *ex ante* predictions from models are as close to the final figures as are the early estimates of the Bureau of Economic Analysis of the Department of Commerce. This, in my opinion attests too to the validity and credibility of model forecasts.

The record for predictive testing of agricultural sector models is just being compiled, and we do not have the good sample size that Dr. McNees uses for national models. In a recent paper of the Giannini Foundation by Gordon C. Rausser and Richard E. Just, price forecasting accuracy has been examined for econometric models of the agricultural sector.<sup>5</sup> In this case, the standard is futures market quotations. Agricultural models generate hundreds of variables — incomes, production, stocks, plantings, and other relevant variables besides price — but the Rausser-Just paper is confined to price forecasting in a limited number of markets.

The authors conclude that futures markets seem to be very good forecasters in comparison with models, but as I scan their tables, in the pre-publication research paper, it seems to me that the Wharton Agricultural Model does very well, too except in one market, namely the soybean complex. Their tests initially covered only December 1976 through December 1978. That was the period just months after the launching of the Wharton Agricultural Model. The model operators, under the direction of Dr. Dean Chen, have made considerable improvements in their ability to handle soybean markets since that time. In the Rausser-Just tables, the Wharton Model, in four quarter forecasts are much better than futures quotations in forecasting corn, hogs, and cattle. They are slightly better in cotton, and about equal in wheat. I would call this excellent performance of a sort that would lead me to want to use the model for policy analysis.

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5 R E Just and G Rausser. "Commodity Price Forecasting with Large Scale Econometric Models and the Futures Market," *American Journal of Agricultural Economics*, 63 (1981).pp. 197-208

## Commentary

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*Richard L. Feltner*

Mr. Klein has done an excellent job of setting the stage for this entire conference. His review has brought us to a common ground regarding the general nature and development of econometric models as well as some straightforward observations on their current and potential value and shortcomings.

My comments here are made from a user or decisionmaker point of view. I prefer to use the broader term "decisionmaker" rather than policymaker. Only some decisionmakers are policymakers, but all decisionmakers have reason to at least consider the possible use of econometric models.

Two keys to the future success of the actual use of models in the formulation of policy or their influence on day-to-day operating decisions are: (1) further developments in both methodology and variable definition and (2) much greater understanding and acceptance on the part of decisionmakers regarding the use of models.

Decisionmakers usually can be found at any point in one of three states regarding their acceptance and/or use of econometric model results.

(1) Total antipathy or refusal to consider them.

(2) Conditional acceptance, in which there is the use of a reasoned and balanced mix of model results, intuition, and personal judgment.

(3) Blind faith in the model results and decisions made accordingly, with complete abdication of responsibility to use one's own thinking mechanism.

Individual decisionmakers can and do move back and forth among these three categories based on (a) their past and current educational exposure to models and (b) their personal experience

with model-generated or -assisted **decisionmaking**. Given there is reason to believe (and I am among those who do) that models **should** have and, do have an increasing role to play in decisionmaking, modelers, **have** a challenge to, **help** move as many decisionmakers into **the** conditional acceptance category as possible.

Perhaps Klein's most important point is that: "a model is a **simplified** approximation of reality." Models **are**, after all, just models. They are tools in the decisionmaking process, not the decisions **themselves**.

Regarding the use of model results, the need to inject intuition and personal judgment cannot be over emphasized. At the same time, there is a need to have more decisionmaker input into model development, particularly in definition of variables. Not only can this result in more useful variables and, hence, more useful model results, but decisionmakers will have more confidence in the results.

**Models** to date have been appreciably better for predicting general trends than absolute values. The smoother the trend the better the prediction.

In general, the higher the level of aggregation the better the predictive ability. National models perform better than regional models, **and** regional models perform better than state models. And, for a variety of reason, there is considerable variation in the predictive ability of similarly constructed **regional** models. For example, models used by **FarmBank** Services to predict interest rates, demand for agricultural credit, and loan volume enjoy considerably better predictive ability in some of the Farm Credit System's twelve districts than in others. Such inter-district variations as seasonality of loan, demand, concentration of loan size, and historical growth trends **are** some of the factors accounting for differences in the reliability of model results among districts. The obvious implication is the need to tailor models to the level of aggregation, geographic area, or economic sector which will produce satisfactorily precise results.

I would emphasize again the need for decisionmakers to have a thorough understanding of both the opportunities and the limitations of econometric models when used as decisionmaking tools. Only in this way can they make a judgment as to **whether** they can justify the cost of using them. **Also, such** an understanding **is essential** in order for decisionmakers to have meaningful input into the process of

model improvement over time. Hopefully, the results of this symposium, if properly disseminated and absorbed, will be a boost in this regard.





# The Emerging Policy Issues Affecting Agriculture

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*Don Paarlberg*

I begin by stating the assumptions on which this paper is based, knowing that no prognosis can be better than its premises, unless, of course, compensating error is at work.

*Weather will be average.* That is, growing conditions during the 1980s will be similar to those of the past several decades, both in central tendency and departures therefrom. Long-range weather forecasting and predictions of climatic change are not yet sufficiently accurate to be a significant factor in decisionmaking. A person who predicts better or worse weather in the 1980s than during the 1960s and 1970s is telling us more about his state of mind than he is about the weather.

*Agricultural technology will continue to advance*, much as it has during the past 20 years. I discount the widely held view that new agricultural knowledge is lagging. Agricultural productivity — that is, output per unit of input — shows irregular advance during the past century at a rate generally between 1 and 2 percent per year. More institutions are involving themselves in agricultural research: the non-land-grant universities, new agencies of the federal government, and the international research network. We are entitled to believe that some of this research will pay off, as has been true of research in the past.

*The real gross national product will increase*, though at a slower rate than during the past two decades. Performance of the general economy will be handicapped by a variety of problems: inflation with its misallocation of resources; declining competitiveness of the American economy as compared with those of other countries; and a diminution in the work ethic. Severe depression is not foreseen. During the past half-century we have developed so many tools to

forestall depression that we have institutionalized its opposite; inflation.

*Inflation will continue.* The virus is in the bloodstream; it is throughout the system, built into expectations, a component of almost every long-term contract. An effort to totally purge inflation and inflationary expectations would be accompanied by unemployment and recession so severe as to be unacceptable, economically and politically. Efforts to slow the rate of inflation will continue to be made and are laudable. But we should not pray too hard that the rate of inflation be brought to zero; an answer to such a prayer would be embarrassing to the supplicant. With inflationary anticipations written into everything from wage contracts to the price of farmland, a stable price level would be a relative deflation. We know from the experience of the 1930s how disastrous that is. I assume that the rate of inflation will be held to one digit, not two.

*Most of the liberal trade gains made since 1934 will be retained.* This will be in spite of increasing protectionist sentiment, and with the exception of certain industries such as steel and automobiles. The demands of third world countries to get into our markets with their sugar, beef, vegetable oils, textiles, and other manufactured products will increase, and we may accede to them in some degree. The use of trade as a diplomatic weapon is assumed not to be an important component of our policies during the decade ahead.

*Major war will be averted.* I assume that there will be wars of liberation, civil wars, and various uprisings, but that the great powers will succeed in avoiding direct confrontation. No doubt there is an element of optimism as well as analysis in this prognosis; with modern weapons, major war is too horrible a prospect to contemplate. One dares hope that our leaders will realize this and work out their differences at the negotiating table.

*Disillusion will grow regarding the ability of the government to solve economic and social problems.* Evidence is that during the past several decades there has been a gradually growing feeling that government is wasteful, in the hands of incompetent people, run for a few big interests, and that it can't regularly be trusted to do what is right. This feeling is confirmed by the Center for Political Studies of the Institute for Social Research at the University of Michigan, among others. It is witnessed by actions of the electorate and the Congress during the past year. The assumption I make is that a trend of this nature, now evidently established, will run for some addi-

tional time before it is checked.

***Farms that are large enough to be efficient and that are well-run will prosper.*** The reverse will be true for farms that are too small or are poorly managed. In other words, I foresee neither distress so general as to put efficient farms into a condition of persistent loss nor a situation so prosperous as to make profitable farms that are inefficient. I assume that we will increasingly recognize the enormous variation from farm to farm and will slough off the error of thinking that the average represents all the individuals that make up the average.

These are my assumptions. They underlie the choice of policy issues I lay before you.

First I consider the national and international setting within which agricultural policy issues will emerge.

Primary concern will focus on three related subjects which have been at the heart of domestic policy for decades: inflation, employment, and economic growth. These three have certain compatibilities and certain antagonisms. During recent decades the differences were resolved in favor of stimulating employment and growth. These policies led in time to inflation and then to what is called stagflation. Since January we have embarked on a new course called supply-side economics. The intent of this new course is to achieve economic growth and increased employment *without* inflation. The strategy is to reduce government outlays, cut taxes, exercise strong discipline over the money supply, and cut back on government regulation. The country and the Congress have given support to the administration in the pursuit of this new policy. The consequences cannot now be accurately anticipated. Presumably we will adhere to it at least for a time. The previous stimulative policies appear sufficiently discredited so that we are unlikely to return to them quickly.

Spirited debate on overall economic policy is certain during the decade ahead. The prospect is that we will have a lesser role for government than we have been accustomed to. The public appears to have lost its enthusiasm for governmental fine-tuning of the economy; the experts seem not to have known what octave we were in.

Increasingly the United States is involved in world trade. This is nowhere more clear than in agriculture. Exports now take one-

fourth of our total production, and imports constitute one-eighth of our food consumption. Issues focused on international trade will be:

- How much protection and for which industries?
- What degree of self-sufficiency for such products as petroleum and sugar?
- How to relate domestic prices to the fluctuations of international markets? Or how to dissociate them?
- To what degree should trade be supportive of our diplomatic initiatives?
- What roles respectively for trade and aid in our relations with third world countries?

Debates on national economic policy and international trade will provide the setting within which agricultural issues will be fought out. Quite possibly, these issues will be more important than those that appear to be strictly agricultural.

Clearly, econometric models that will be useful to agriculture will have to incorporate their agricultural variables within a larger context, including national and international forces. The agricultural sector is not autonomous and cannot be treated as if it were. The linkages and interactions involving agricultural and non-agricultural sectors are complex. With Keynesian theory unable to explain our situation — unused resources in an inflationary setting — the use of conventional Keynesian coefficients in our econometric models is inappropriate. If there is anything to the Laffer curve, which I believe there is, economic relationships are curvilinear rather than linear. Where are we on the curve? Modeling with curvilinear and joint relationships is infinitely more difficult than modeling with variables that are linear and additive. All of this poses an enormous challenge to the model-builders.

I now list six agricultural areas within which major issues are likely to be debated during the decade ahead. In doing so I have chosen not the issues that the farm people would like to see considered but rather those that seem to me to have the greatest likelihood of arising.

### **Commodity Programs**

Sometime during the past 15 years we crossed a watershed of farm policy so far as the big commodity programs are concerned. From 1933 to about 1964, we gradually increased the role of government in the pricing and production of corn, wheat, cotton,

and other major crops. Since that time the role of government has gradually been reduced, with favorable results. The new farm bill, the Agricultural Act of 1981, will prove to be an additional step toward market orientation. The presence of a few holdouts, like tobacco, does not overrule the general conclusion that a major change has occurred.

Reasons for the change have included evidence that the programs were losing markets, their regressive nature, their inflationary impact on retail food prices, their high budgetary cost, growing dislike for centralized decisionmaking, and the declining political power of the farm lobby. I see nothing in the picture during the coming decade that is likely to fundamentally change the recent social, economic, and political environment within which these programs have come to operate. Thus, the proponents of the big commodity programs are likely to be working within an overall climate of disfavor. Such commodity programs as we will have are more likely to emphasize price stability than price enhancement. They are likely to be symmetrical, with restraints on both price increases and decreases, rather than symmetrical as in **the past**, with floors but no ceilings. The commodity programs, which held center stage for 40 years, as the big feature in the farm policy theater, will become but one of a number of acts in a variety show.

## **Resources**

While the commodity programs retreat in importance as farm policy issues, resource questions will advance. A scarcity syndrome has arisen; the present and prospective mindset is that our resources are limited and that we must protect them. There is of course truth to this perception. Among the items in the natural endowment that are in limited supply are such agriculturally important resources as farmland, water, timber, recreational sites, and wildlife.

Farm people have underestimated the strength of public conviction regarding environmental matters. There has been the feeling among farmers that the ecological movement was a fad and that a new administration which is dubious about governmental regulation will return us to things as they were a decade or two ago. This seems to me unlikely.

The conservation of our soil resources will be an issue of growing importance. Conservation efforts of the past have been in part subverted. The Soil Conservation and Allotment Act of 1936 was a

facade for supply management. The Agricultural Conservation Program became a device for passing out government checks. Under the guise of conservation, government financial assistance was provided to drain wetlands and pump irreplaceable water from underground supplies. These policies and programs are either already superseded or are on the defensive. In the decade ahead, the public will demand value received for dollars spent on conservation. Preservation of prime farmland and erosion control are likely to be important farm policy issues. Not all the facts are clearly established on these subjects, and the ones that have been ascertained are not generally accepted. Is urbanization a serious threat to our agricultural capability? How serious are our soil losses? The policy questions, as well as the questions of fact, are very difficult: what are the respective roles of the individual landowner and his government? As to government, what could best be done respectively at the local, state and national levels? How much can be done by research and education on the one hand and by government on the other? What can best be accomplished by incentives and what by regulations? What is the legitimate public interest in privately owned land? Can we quantify the social costs and benefits of alternative forms of land use?

As to agricultural use of water, the facts are no clearer and the issues no easier than for land, particularly in the West. Generally, agriculture pays less for irrigation water than other users, and far less than the cost of supplying it. An effort to have agriculture pay the full water cost would convert large areas from irrigation to dry-land farming. What is the public stake in continuance of communities based on irrigated agriculture? Very likely we will see increased efforts to limit the amount of water an individual landowner can pump from the supply that underlies his own and his neighbor's land, as we do with oil. There is much work to do, including work of a theoretical nature. How price and manage and asset that is valuable, diminishing, and irreplaceable?

## **Energy**

The real cost of energy will increase during the years ahead. This will affect the cost of fuel, fertilizer, and pesticide. It will affect transportation costs and thus the location of production for bulky farm products produced far from market. It will affect production costs in areas dependent on pumped irrigation water.

If present legislation on gasohol is effectuated, it will result by 1990 in converting to alcohol fuel the corn from 15 to 23 million acres of land. This is equal to about one-fourth of the land presently committed to corn.

Certainly there will be major confrontations on the gasohol program. Favoring it are corn farmers and agribusiness concerns. Opposed, actually and potentially, are livestock producers, soybean growers, the wheat mill-feed people, exporters, consumers of meat, milk, and eggs, conservationists, producers of rival fuels, and taxpayers. This program has not yet begun to bite. If and when it does, it will be a bruising battle.

Are farmers to have preference over other users of fuel? Will energy be apportioned out by some allocation board or will the market be allowed to operate? These will be policy issues of the 1980s.

### **Consumer Issues**

Food safety appears to be an issue that has crested. But there will be debates on it in the years ahead. Legislation put on the books during the zeal of the 1970s remains there, to be enforced, removed, or ignored. Whichever one of these courses we pursue, or whatever combination of them, some groups will take offense.

Feeding programs such as direct donation, food stamps, and school lunches also seem to have passed their peak. They are likely to subside but not to disappear. At what magnitude are they to level off? How to distinguish between the unfortunate and the indolent? Or should we distinguish? By what agency will these programs be operated. Should assistance-in-cash replace assistance-in-kind? We can expect to re-hear familiar arguments on these issues.

Consumer attacks on agribusiness can be expected to continue, particularly with inflation and rising food prices. On occasion farmers will join in the attack. This is a perennial issue, with great political potency.

If retail food prices rise we can expect to hear consumer demands for direct price controls. And there may be proposals to limit exports in order to keep more food at home, an attempt to lower prices by increasing the domestic supply. There will be advocates of a cheap food policy during the 1980s, as one would expect at a time of inflation, with 97 percent of the people nonfarmers. The lineup on this issue is predictable — consumers vs. farmers.

The consumer movement has made substantial gains. It is locked into a power position with legislation, bureaus, and appropriations. In government, these are the certificates of longevity. There are **33** federal agencies with responsibility for consumer activities. These include approximately 400 bureaus and subagencies operating more than a thousand consumer-oriented programs.

Farmers still are inclined to think of the consumer as the adversary rather than as the customer. This feeling is abating somewhat but is still potent. Consumers have won a place on the agenda committee that determines farm and food policy. They are not to be dislodged from that position. How they are to comport themselves in their newly won role and how farmers will adjust to the necessary sharing of the policymaking prerogative are questions of policy portent for the years ahead.

## Structure

Agriculture is being transformed from its traditional status into something resembling an industrial enterprise. We note fewer and larger farms, greater specialization, more purchased inputs, more absentee owners, greater use of credit, more vertical integration, more contract farming, and fewer central markets. It is becoming harder and harder for a young man to begin farming unless he inherits a farm or marries into ownership. This is contrary to the agrarian tradition, which holds that farming opportunities should be readily accessible. Some people look with misgiving on the trends and wish to slow, halt, or reverse them. Former Secretary of Agriculture Bob **Bergland** launched a nationwide debate on this subject and, as almost his last act in office, issued a report on it entitled ***Time To Choose***. The report cited a number of government programs that serve to speed up the trends toward industrialization of agriculture: taxation, credit, commodity programs, research, and extension. He supported modification of these to take out the bias toward larger farms.

Apparently the new administration thought this was *not* the time to choose. No new copies of the report have been printed, and at eight months of age it is already a collector's item.

Will the structure issue subside? I think not. My view is that concern about structural change in agriculture is sufficiently widespread so that the issue cannot be adjourned. I think the agricultural establishment, to which the issue is an embarrassment, will attempt



to avoid confrontation on the subject, and resist it in subtle ways. But the Experiment Stations and the Extension Service will be under steady pressure to reduce their present tilt in favor of the larger and better-off farms. The regressive nature of the commodity programs will be to some extent redressed. Credit subsidies for well-to-do farmers will be squeezed down. Some of these things are already happening in low-key fashion.

This is all part of a century-old policy issue that has surfaced variously. During the 1930s it took the form of the Farm Security Administration. In the 1940s came Goldschmidt's study of Arvin and Dinuba. During the 1950s there was the Rural Development Program. In the 1960s came the report, "The People Left Behind." During the 1970s we had "Hard Tomatoes." Now we have the Structure Report. The issue will not go away.

### **People on the Fringe**

Agriculture's input into farm and food policy has for many years been shaped by operators of the large commercial farms. These have mostly been white and male. This leaves out small farmers, part-time farmers, hired farm workers, women, and ethnic groups including blacks, Chicanos, and native Americans. These left-out people have been on the fringe of policymaking and are demanding an enlarged role. That demand is likely to be heard during the decade ahead, coming in part from these people themselves and in part from their well-meaning sponsors among labor organizations, consumers, and church groups.

The 1900s have been aptly called "the Century of the Common Man." There are still two decades to go in this century, and still some common people who do not share fully in the rights and responsibilities of American citizenship. This will continue to be an issue. Hired farm labor will demand rights and programs comparable with those obtained by non-farm labor. Minority ethnic groups and females will challenge the white male tradition that has long characterized agriculture. A broadening of the base for the determination of farm and food policy is underway. There will be resistance. This will be mostly low-key, with occasional flareups, as we have seen in the effort of hired farm workers to win collective bargaining rights.

If I am right, most of the farm and food policy issues likely to be on the agenda for the 1980s will be placed there by nonfarm people.

Such has been the case now for some 15 years, though those of us in agriculture have been reluctant to admit it.

The new situation will call for a new policy strategy. It will have to be a defensive strategy, appropriate for a team that has lost the initiative. Challenges will have to be met. In years past the strategy for meeting challenges was to ridicule proposals that appeared preposterous, to ignore those that were thought to be faddish, to confront the challengers when there was thought to be the power to win, and to try to co-opt those that could not be overcome. The string of victories won by this strategy had been almost unbroken for a hundred years.

With the new and prospective situation, farm policymakers will have to consider alternatives of a different sort: de-escalate issues on which loss seems likely; find common ground with groups formerly considered adversaries; engage in tradeoffs when a net gain seems possible; reserve available strength for battles on issues that are of central importance and on which victory seems possible. If modeling techniques will help in the assessment of issues and in the development of appropriate strategy, I am an enthusiastic supporter.

## Commentary

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*Lynn Daft*

In forecasting policy issues a decade in advance there is only one real certainty — that you will be wrong. The relevant question therefore becomes: how wrong? In the case of Don Paarlberg's paper, I believe the answer is "not very." He has done his usual exemplary job. It is a thoughtful treatment, grounded in the pragmatism that comes from a unique combination of rich experience and an open mind. Spread throughout is Paarlberg's gentle humor, reminding us not to take ourselves or our bag of tools too seriously.

Much of what I have to say about Paarlberg's paper is by way of supplementing or qualifying it rather than criticizing it. Before turning to these comments, however, let me offer a brief reminder of the present condition of American agriculture. I believe it helps give the greater meaning to the comments that follow. Five or six characteristics stand out prominently.

- ***Approaching equilibrium.*** After experiencing a prolonged period of excess capacity, there is mounting evidence that U.S. agriculture is nearing a state of equilibrium. Acreage diversion measures are used less frequently and to a lesser degree. The movement of labor out of agriculture has fallen to a fraction of its earlier rate. Carryover stocks, though continuing to fluctuate from year-to-year, are more nearly in line with market needs.
- ***A dualistic structure.*** Farm units are gravitating toward both ends of the size spectrum. At the small end of the scale are a large number of units that have been called "farm residences." Although they engage in some form of agricultural production, as a group they are loosely tied to the agricultural economy and independent of it as a source of livelihood. At the other end are

the large commercial farm units that comprise the mainstream of commercial agriculture and account for most U.S. farm production. The interesting feature of this development is that two quite different worlds are being formed — one based almost exclusively on non-economic values; the other based on a combination of traditional agrarian values and economic aims, but with primary emphasis on the latter.

● ***Large-scale specialization.*** This is probably the hallmark of U.S. agriculture. Through growth in unit size and the adoption of labor-saving technology, our farms have been able to realize most economies of scale. Despite their large size, however, the opportunity for further growth and the demands for higher levels of technical sophistication continue to press. Also, the associated capital requirements have become so large as to be a constraint on the entry of new units.

● ***Increased economic instability.*** Variability in farm prices and farm income has risen sharply since the mid-1950s. Variability in the index of prices received by farmers for all products more than doubled between 1955-63 and 1964-71; between 1964-71 and 1972-78, this variability more than doubled again.

● ***Internationalization of U.S. agriculture.*** In the early 1950s, U.S. agriculture supplied the rest of the world with about 2 percent of its agricultural products. It now supplies about 11 percent, and the U.S. share is rising. While U.S. agriculture policy once was fashioned with only passing attention to international trade implications, that is no longer the case.

Let us now turn to Paarlberg's paper, beginning with his three assumptions. There are three or four points to be made about these assumptions.

First, I believe that there is too much certainty implied. As a statement of central tendency or a most likely condition, I believe Paarlberg's assumptions are reasonably close to the mark. But there is one thing we can be very sure of, as I said at the beginning: there will be surprises along the way, and they will probably influence policy in major ways. Some will come from natural sources; some will be man-made. This is important because public policy is in large measure a creature of circumstance and the events that give rise to circumstance.

One does not have to go far into history to find examples of what I mean — Watts, Vietnam, OPEC, Watergate, the Iranian hostages,

Three Mile Island, Afghanistan, Solidarity. Two features of this type of "surprise" are significant: (1) the events are highly unpredictable and (2) many are of foreign origin and carry with them international implications.

A second related point refers not to variation around a central tendency but to mistakes in judging the central tendency. As I said before, I find Paarlberg's list of assumptions reasonably close to the mark. But what if we are wrong? What if agricultural technology begins to lag seriously? What if growing conditions are not similar to those of the past several decades or well-run farms do not prosper? For policy purposes, "what if" questions will continue to be a very important part of analysis.

In this regard, I would give greater prominence to the policy implications of tighter commodity markets than Paarlberg does. Many signs point in this direction, some of them noted in Paarlberg's paper — nearing equilibrium of resource use, increased competition for land and water, growing export markets, an uncertain trend in productivity growth. At the least, I would expect to see policies that prepare for this possibility.

One last point regarding Paarlberg's assumptions — this one regarding the role of government. He points out that the public attitude toward government in general is critical and becoming more so. That is certainly true. Yet it is more complicated than this suggests. Attitudes toward specific government programs and services tell quite a different story. The same University of Michigan research paper that Paarlberg cites also finds that a majority of Americans believe that the government is spending *too little* on health programs, education programs, defense, and protection of the environment. Likewise, there is strong support for the core transfer payment programs, which are rapidly becoming the dominant element in the federal budget. Political rhetoric aside, I find little reason to believe that the role of government will change materially. To the extent there are significant changes, I suspect they will have more to do with transfers of responsibility among levels of government than with a reduced public sector.

I concur with Paarlberg's emphasis on national economic policy. More than anything else, this will shape and limit agricultural policy. This has been the case for the past several years, and it will continue into the foreseeable future.

Paarlberg lists six areas within which he predicts major agricul-

tural issues will be debated. While I agree that we can expect policy activity in all these areas, let me suggest a variation or two on Paarlberg's themes. The major commodity programs are clearly on the defensive, as Paarlberg says. Yet much depends on his initial assumption of prosperity for the larger, more efficiently managed farming operations. Given the cash flow problems that can result from cost-price squeezes of the type experienced in recent years, and the political pressures that follow, program changes can be expected to follow an irregular path.

Paarlberg says that food safety and food assistance programs, both of which he includes under consumer programs, have peaked. He also attributes substantial gains to the consumer movement and finds it operating from a position of significant influence. I have a somewhat different view. Although food safety will not necessarily be wrapped in the controversy that it has in the past, I do not see it receding. If anything, the health and nutritional implications of diet will gain more attention.

Neither am I inclined to think that the food assistance programs have passed their peak. This is not to say that there will not be changes designed to reduce abuse, streamline administration, and target the programs more directly on the poor. Eventually we might even see the largest of these programs, the food stamp program, cashed-out in a general welfare reform. But until these programs are replaced with a comparable level of assistance in a different form, I do not expect to see them become significantly less important.

And while it is true that the so-called consumer movement is more firmly established now than it was, say, a decade ago, that is not saying much. Its position is still precarious. It is weakly organized and thinly staffed.

I agree with Paarlberg that the structure issue will not go away; yet I see it taking a **different form** in the future. Rather than a defense of the agrarian tradition, I foresee attention focused on the implications of a changing structure for economic efficiency and industry competitiveness.

A final word about governmental institutions and their use of economic analysis in the policy process. Contrary to the impression one often gets from press accounts of political trades and bureaucratic infighting, it has been my experience that economic analysis is a far more important determinant of the outcome of most policy decisions than is generally understood. That's the good news. The

bad news is that as a broader array of interests becomes involved in agricultural policymaking, the decision process becomes more difficult to manage and the opportunity for error increases. This too will be a part of the future policy landscape.

To summarize, **Paarlberg** has provided us with an insightful preview of emerging policy issues. With due allowance for the unexpected, it offers a highly useful guide to the future.





## The Interface Between Policymakers and Model Builders

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*Dale E. Hathaway*

I have been asked to speak today on the subject of the interface between policymakers and modelers. Since the world interface is somewhat vague, I will attempt to translate so that people may understand what I am going to try to discuss. Basically, I want to talk about the problems that concern policymakers and what modelers might do to help improve policy decisions.

Since I have been both a policymaker and a model builder, more recently in the role of policymaker, I would like to outline some of the issues that I believe are important as perceived by policymakers, and some of the implications that these have for people who model policy options.

By definition, policymakers are individuals who reach their office, either in the executive or legislative branch, via the electoral process, or who are appointed by those who did. However much the individual policy maker may claim that the prime objective in his life is "good" policy, the realities are that he will be constantly reminded of the fact that he works for someone whose immediate objective, whether stated or not, is continuing in the office that he now holds. The simplest and crudest way to make this point is to say that no policymaker can escape the reality that all the decisions at the policy level have a political impact, and that impact must be considered in the decision process.

This reality of the politics of policymaking puts the policymakers and the decision process into a context which is not always well understood by modelers. But even when it is understood, it makes the problem of modeling policy alternatives extremely complex. Let me comment on several issues politics introduce into policy decision.

## **The Time Problem**

Everyone has commented that the time allowed policymakers tends to be substantially shorter than is desirable for decisionmaking, and furthermore, it makes modeling very difficult. It is very little consolation for an agricultural policymaker interested in expanding exports and improving farm prices to have a model builder tell him that sometime over the next five years the export market for farm products will be strong and lead to a boom in farm exports. The policymaker's problem is that he has to deal this day, this month, and this year with people who expect certain things to happen and, therefore, while it is desirable to look at the longer-run situation, it means very little if things are going very badly in the here and now.

To cite a current example, I believe almost any model of world supply and demand for grains during the next several years would suggest that a wheat and feedgrain set-aside in 1982 is a bad policy. But a wheat set-aside has been decided on short-run budget and price considerations. I believe that this phenomenon of policy making is called "the draining the swamp." It is one that plagues almost every U.S. administration and causes U.S. policymakers to look upon parliamentary governments with envy some days.

## **The Path is as Important as the Destination**

Similar to, but not exactly the same as, the issue of time is the issue of means. There is a practical political limit to the direction a policymaker can take in order to reach an objective. In most policy situations the ability to adopt and maintain a policy with a given objective depends not only on whether the objective itself appears rational and will have an outcome which is desirable, but also on whether or not the route to the policy objective is tenable. I could cite personal experiences and numerous observations in which government officials started out with policy goals that probably would have produced desirable results had they been allowed to pursue them. However, various political groups created so much difficulty over the chosen route that officials found the path was untenable and were forced to abandon the policy. In other words, for policymakers (unlike renting trucks to move) getting there is not half the fun. In fact, the question generally is can we survive the trip even if we want to get there?

### Who Gains and Who Loses?

Another major problem is that policymakers are rarely asked to choose between two Pareto better policies. Indeed, those policies which would benefit everyone are so obvious that they are decided and put forth by the professional bureaucrats. The decisions that are left to the policymakers are almost by definition decisions that require some to benefit while others suffer. To the decisionmaker, the question of who will benefit and who will suffer becomes almost as important as the question of what the policy will accomplish. The most skillful policymaker/politician attempts to convince the public that everyone will benefit and no one will suffer because of his policy actions. This attempt, however, tends to succeed in political campaigns more than it does in actual governance as the current administration, the last one, and the one before that can attest in varying degrees.

### **The Problem of Misplaced Preciseness**

Models are designed to predict with varying accuracy what will occur if certain variables are influenced. From the policymaker's point of view, degrees of accuracy may not be the most important element by which policies are judged. As an illustration, let me indicate some examples specifically relating to agriculture.

When I first accepted my position in the Department of Agriculture I was somewhat skeptical and critical that the Foreign Agricultural Service had consistently underestimated the projected growth in exports at the beginning of each crop year. My colleagues and I continually chided them to be more precise in their export estimates and to project increases more consistent with the expected actual increases. In retrospect, I think the Foreign Agricultural Service was aware of something that I only recognized later, that the accuracy of the direction of the increase is far more important than the preciseness of the actual projection. In other words, it is not a terrible error for a policymaker to predict farm exports or prices will rise and then to find that the increase was substantially greater than he had predicted. It is, however, both bad form and bad politics to have consistently predicted that there would be a rise in exports or a rise in farm prices and find that exports or prices are falling. The public tends to remember the direction and not the magnitude.

The importance of this phenomenon cannot be underestimated and should be obvious to people who merely observe the current

news. We have seen in recent months an administration claiming great victory for declining inflation rates even though lower rates were enough to have removed presidents from office in the recent past. The same has been true regarding interest rates. A decline in interest rates to levels only previously charged by loan sharks is now considered a major victory for the new economic policy.

### **The Tidal Wave Problem**

All policymakers recognize, in quiet moments, that in our economic system they have control over a few variables. What they often fail to recognize is how easily all the other variables may swamp the effect of the policy variable they control. In other words, what good is your tide table if your boat is swamped by a tidal wave?

For an example, in the spring of 1980, the USDA was trying to stabilize grain markets in the aftermath of the Russian embargo. We had only loans, reserve rules, and purchases as policy variables. We used them, but the results were far from satisfying to farm producers. The problem, however, was not our ineffective use of the available policies; it was that those policies were swamped by the sudden jump in interest rates and other factors outside the control of agricultural policymakers. The reality did not change the perception, however, that our policies were inadequate.

The current administration is experiencing this phenomenon now. Auto import controls have not improved auto sales, budget cuts have not reduced the budget deficit, and farm exports have lagged earlier expectations despite an emphasis on expanding exports. They are now learning that all individual policies can be quickly and easily overwhelmed by other economic and political events.

The concerns I have just outlined tend to result in a series of short-run policies that appear to be unrelated to any administration's stated long-run objectives. They produce conservative administrations that use price controls, short-supply embargoes, and finally, cuts in defense spending; and liberal administrations that decontrol, cut budgets, and refuse to use production controls and trade restrictions to protect constituent groups.

### **Politics of Policymaking in the 1980s**

Now let me translate these political issues that so greatly influence the policymakers into the context of the 1980s. First, despite

the overwhelming vote in 1980, there is no indication that the **volatility** of the electorate has ended. Because of it, no president has **served** two terms in two decades, and in the same period an unprecedented number of Congressional seats have turned over — in both directions. The number of politicians who view their position as "safe" is very small and likely to remain so. Therefore, they will focus more and continued attention on the time and path problems involved in the policy process.

This fact, in turn, makes who gains and who loses an important daily issue. In a political world of single-issue groups, their money and their support depends upon how their interests are treated in every policy decision. In farm policy this is especially difficult because the policymaker cannot deal with each commodity in a vacuum, as each commodity group wishes to be treated.

Because most of the public obtains its perception of how policies are working via 30-second capsules on TV and radio, the issue of "misplaced preciseness," or the importance of the overall accuracy of the projected policy outcome, will continue to be significant. The electronic media report direction, not magnitude, and say nothing about either the reason for change or what is likely to happen next.

Finally, in the area of agricultural commodity policy, the tidal wave effect is a dominant force. The money and credit system both for agricultural producers and for participants in the agricultural commodity markets is now fully integrated into a national and international market for money and credit.

In my view, policymakers will continue to have the concerns I have outlined; indeed they may become stronger rather than weaker. Until and unless model builders can understand and address these concerns, there will continue to be a gap between policymakers and model builders. Models with single-objective functions no longer suffice.

### **How Can Model Builders and Policymakers Function?**

How can modelers function in this environment? Basically, I think that policymakers and model builders need the answers to two sets of questions. The first is, "What are the possible ways of getting from here to there, and what will be the impact on various groups or sectors using different paths?" Second, "What can go wrong, what difference will it make, and how likely is it to happen?"

Part of the problem with this approach is that policymakers do not often ask these questions and, what is worse, are not appreciative of the answers when they are given. Moreover, government officials are generally reluctant to discuss the realities of short-term constraints, and often they are not even aware of them. They tend to ask the model builders for answers within a prestrained philosophical framework. The model builder provides answers even further constrained by the limits of his data and models. The decisionmaker is disappointed, upset, and looks for other advice when events intervene which were outside the framework of his question, and the modeler is frustrated to find the policymaker taking actions which are based upon incomplete and/or inaccurate judgments on issues he could have addressed.

Let me cite an example of the kind of problem that is involved. For three years the U.S. government, and the European Community for a longer period, has wrestled with the high cost of dairy price support programs. Let us assume that the political realities in both situations prohibit abolishing the support programs. The policy assumption, and therefore the question generally asked by policymakers, is how much should the support level be reduced to minimize the high budget cost.

A better approach to the issue might be: (1) Why is the program generating increasing costs? (2) What is the nature of the production or consumption situation which significantly affects costs? (3) Which policy variables will really change anything? (4) Will changes in consumption, production technology, or marketing technology swamp any politically tolerable changes in the policy variable?

Let me close with a fable about the interaction of model builders and policymakers. Once upon a time there was an agency in Washington. That agency had computers, and any agency that has computers obviously builds models. This agency, therefore, hired some model builders and they built a model of the feed-livestock economy of another country. The model was designed to predict the amount that the other country would import under certain conditions relating to internal crop production, livestock numbers, etc. Then the greatest of all policymakers became very upset with the other country, and thus, was looking for sanctions that would punish the other country. Therefore, he said to the agency that had the model builders, "Please tell me what would happen if I stopped all United

States exports to that country." The model builders did not have a model that was designed to answer that question, so they gimmicked up the model that was designed to predict imports and came up with a conclusion that the other country would have a 25 percent decline in livestock output if the United States stopped exports to the other country.

Unfortunately, the top policymaker believed this, and therefore he stopped exports to the other country. He was surprised and dismayed to find that the other country did not have a 25 percent decline in livestock output. He never asked why someone didn't tell him that the model was giving him a bad estimate.

All fables are supposed to have a moral. Well, there may be several to this one. One could gather from this story: Never trust an estimate from the agency. A lot of people had come to that conclusion without ever having seen the feed-livestock model. A second conclusion is that no model builder should be asked a question without asking what can go wrong, and how likely it is to occur. However, the most important conclusion coming from this little fable is that one who builds models should never accept an answer that clearly defies common sense.





# Capturing the Linkages Between Agriculture and the Domestic Economy

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*John B. Penson, Jr.*

There are signs of an increasing recognition among agricultural economists that agriculture is a fully integrated partner in the nation's economy and should be treated as such when modeling aggregate outcomes in this sector. While early attempts to model outcomes in agriculture ignored many of the major linkages between this sector and the general economy, a move has been underway for several years now to explicitly account for many of these linkages in one fashion or another. These efforts generally can be differentiated by their recognition of the transmission mechanisms through which events outside the sector affect agriculture and by the timing of agriculture's effects on the rest of the domestic economy.

My assignment today is to discuss the interface between agriculture and the domestic economy and the importance of endogenizing these linkages when modeling events in agriculture. More specifically, I shall (1) briefly review the major interdependencies between agriculture and the rest of the economy, (2) review the mechanisms through which these interdependencies are transmitted, (3) identify the particular channels through which government actions directly affect agricultural outcomes, (4) discuss a few specification issues that affect the size and timing of "feedback effects" in the economy, and (5) assess the value of modeling the linkages between agriculture and the domestic economy in a fully simultaneous fashion.

## **Sources of Interdependency**

Two sectors in an economy are said to be interdependent if they rely directly on each other — or indirectly through a third sector — for the supply of a particular good or service used in their production processes. Thus, if agriculture both supplies inputs to, and

purchases products from, another sector, these sectors are said to be interdependent. The interdependencies between agriculture and the rest of this nation's economy essentially can be grouped into two categories: (1) Those dependencies that others have upon agriculture, and (2) those dependencies which agriculture has upon others.

### *Dependency of Others on Agriculture*

Perhaps the most obvious example of a dependency that others have upon agriculture in the domestic economy is the dependency of a growing population upon food and fiber products. For example, the processing and distribution sectors in the domestic economy are dependent on the supply of raw agricultural products as an input to their business operations. Today, these sectors serve as an important intermediary between agriculture and consumers, who are increasingly demanding more highly processed foods. Rural commercial banks and thrift institutions, as well as those nonfinancial firms that supply physical goods and services to farmers and their families, are also dependent upon a growing and prosperous agriculture. Finally, agriculture plays an important role in the U.S. balance of trade by partially offsetting the trade deficit in nonagricultural products.

### *Dependency of Agriculture on Others*

Agriculture has historically been rather self-sufficient, producing many of its input needs and financing much of its growth with internal equity capital. Over the post-World War II period, however, agriculture has become much more dependent on the manufactured production inputs supplied by other production sectors in the economy. One example is energy. Not only does agriculture need energy in its production process, but it also needs such inputs as fertilizer and chemicals which are also highly dependent on energy for their production.

The dependence on the goods and services supplied by other sectors is not limited to physical goods. For example, the percentage of annual farm business capital accumulation financed with external capital has increased dramatically over the post-World War II period (Penson). Off-farm employment also has become an increasingly important source of funds in financing additions to investment portfolios for specific groups of farm families. Thus, the growing dependency agriculture has upon the health of the general economy shows up in financial and nonagricultural labor markets as

well as in manufactured production input markets. Finally, agriculture is dependent upon viable demand for its products and upon the government sector in periods of physical and economic emergencies.

As a result of these dependencies on other sectors, agriculture today is increasingly subject to events taking place elsewhere in the economy. For example, an increase in export demand for agricultural products — for whatever reason — bids up both the price of these products and domestic farm incomes. The uncertainties associated with these markets, however, often translate into greater variability in domestic agricultural product prices and exposure to business risk for farmers. The cost, availability, and technology of the goods and services supplied to agriculture also have an effect on net incomes in agriculture and on the expansion of this sector's productive capacity. The costs of these goods and services, while rising over the post-World War II period, have been much easier to predict than agricultural product prices. One area where this has not been true is obviously energy. Another is the loan funds market, where the difficulty of forecasting their future cost of loanable funds has led many lenders to adopt instruments and policies that allow them to lower their exposure to interest rate risk. For borrowers, however, this may mean lower net incomes in periods of rising interest rates and increased exposure to financial risk.

With these generalizations in mind, I would like to initially focus on the channels through which events elsewhere in the domestic economy are transmitted to agriculture. Ed Schuh will be examining how events outside the domestic economy affect agriculture.

### **Transmission Mechanisms**

There is a wide variety of mechanisms through which events elsewhere in the domestic economy are transmitted through to agriculture. I have grouped these transmission mechanisms into two groups: (1) those mechanisms which transmit the indirect effects that events elsewhere in the domestic economy have upon agriculture, and (2) those mechanisms which transmit government actions that have a direct effect upon agriculture.<sup>1</sup>

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<sup>1</sup> In the short run, both prices and quantities transmit the effects events outside agriculture have upon this sector. This point will be emphasized at specific points in this section.

### *Indirect Effects of Nonagricultural Events*

In focusing on this group of transmission mechanisms, we are interested in the opposite side of those markets in which agriculture participates, or in those supply-related factors which affect markets where agriculture buys goods and services and in those demand-related factors which affect markets where agriculture sells goods and services. For example, the relative prices for other goods and services and the level of real disposable income have an effect on the prices farmers receive for their products in the raw agricultural products market. Farmers, of course, can eliminate much of the uncertainty about the effects these factors will have on the prices they receive by entering into a production contract or hedging their market position in the futures market.

In the manufactured inputs market, the prices manufacturers pay for their own inputs and their manufacturing capacity can affect both the availability of manufactured farm inputs and the prices farmers must pay when acquiring these inputs. In the case of durable inputs like farm tractors, an increase in the purchase price of the input will be just one of many factors influencing the implicit rental price to agricultural producers. Other factors include the effective ordinary income and capital gains income tax rates, tax depreciation rates, and the cost of debt and equity capital.<sup>2</sup> Previous studies by Hall and Jorgenson, Coen, and Penson, Romain, and Hughes show how an increase in the rental price of capital will decrease desired stocks of durable inputs.

The wage rates farmers pay for hired labor services will be affected by such factors as wage rates paid to comparably skilled workers in other sectors of the economy as well as strike actions taken by hired farm laborers.

Several mechanisms transmit the effects that events outside agriculture have upon the interest rates farmers either pay loans or

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2 Penson, Romain, and Hughes have expanded the specification of the implicit rental price of capital originally advanced by Hall and Jorgenson and by Coen to explicitly account for the cost associated with the capital structure specifically followed by farmers. This implicit rental price,  $X$ , is given by  $X = (QP/(1-F)) \cdot (A-C-T(G/(G+P)))/(1-T) + ((Z-TRW)/(1-T))$  where  $Q$  is the purchase price of the asset,  $P$  is the real after-tax cost of equity capital,  $F$  is the present value of the stream of capacity depreciation of the asset,  $A$  is the fraction of the purchase price financed with equity capital,  $C$  is the investment tax credit rate,  $T$  is the ordinary income tax rate,  $G$  is the tax depreciation rate,  $Z$  is the value of the periodic loan payment,  $R$  is the real rate of interest on debt capital, and  $W =$

receive on financial assets. The yields on securities offered by financial intermediaries and non-financial businesses, for example, will be affected by the demand for loanable funds and outside capital by these firms, except in those cases where yields are constrained by existing regulations. The cost and availability of loanable funds and the risk and returns on alternative uses of funds available to specific private financial intermediaries will affect the interest rates farmers must pay for loan funds. Both the yields on financial assets and the interest rates on loan funds will affect the weighted average cost of capital relevant to farmers, their implicit rental price of capital, the desired level and balance of their total investment portfolio, and their desired capital structure. Finally, the attractiveness of alternative uses of funds and the availability of funds to finance leasing operations will affect the cost and availability of lease-financing services to farmers.

This list of transmission mechanisms identifies many of the major — but certainly not all of the influences originating outside the sector that affect prices and quantities in markets where agriculture participates. In general, any variable representing an event occurring elsewhere in the economy which has an impact on (1) the supply of production inputs, financial assets, and loan funds to agriculture, or (2) the demand for raw agricultural products, represents a transmission mechanism through which events — regardless of where they originate — are made known to agriculture.

### *Direct Effects of Governmental Actions*

There are a variety of governmental actions that can directly affect the performance and growth of agriculture and the economic well-being of its participants. The monetary policy actions taken by the Federal Reserve System to meet its stated objectives, of course, affect all sectors of the economy through the cost and availability of money and credit and the purchasing power of current savings and wealth. While certainly of major importance, the transmission mechanisms through which monetary policy influence agriculture have already been largely covered. So have the effect that fiscal policy has upon other sectors of the economy. I am interested here in identifying those governmental actions that (1) support raw agricultural products prices and/or influence production, (2) affect the implicit rental price of capital and the desired portfolio balance in agriculture, and (3) affect the cost of estate transfers and the retire-

ment planning of farm families.

Actions that support raw agricultural product prices and/or influence agricultural output include (1) the CCC nonrecourse loan program and the provision of deficiency payments to producers of specific crops, (2) acreage allotments and set-aside provisions for certain crops, and (3) government purchases of agricultural products for defense, school lunches, and foreign food aid programs. These actions affect the level of farm income realized by farmers, their exposure to risk, the value of their farm assets, credit reserves, and contingent liabilities, and the growth of their firms. The Federal Crop Insurance Program — with its recently adopted all-risk features — provides farmers with the opportunity to further reduce their exposure to risk. Other governmental actions important to specific groups of farmers include the subsidized federal loan programs which make this sector a lender of last resort as well as a source of low-interest loans in periods of natural disasters and economic emergencies. These lending programs affect the cost and availability of loan funds to farmers, the ownership and control of agriculture, and the value of existing farm assets.

Actions that directly affect the implicit rental price of capital for farmers and the portfolio balance struck in agriculture include the fiscal policies of governments at the federal, state, and local levels. For example, the cost recovery deductions and limited expensing allowed under the recently passed Economic Recovery Tax Act of 1981 directly affect the implicit rental price of capital. By speeding up the rate at which the cost of personal tangible and real property can be recovered, the Reagan administration hopes to lower the implicit rental price of capital, increase retained earnings, and stimulate investment. Because the accelerated cost recovery system and other features of this new act are extended to all the production sectors in the economy rather than just to agriculture, events in other sectors (including government's need for funds) may further increase the cost of capital, and thus at least partially reduce the otherwise expected benefits from this action in agriculture. For this reason, both the direct and indirect effects of this and similar actions must be reflected in the implicit rental price of capital. Other fiscal policy actions are also transmitted directly to agriculture as well as indirectly through events elsewhere in the economy. These include investment tax credit; federal, state, and local ordinary income tax rates; the effective capital gains income tax rate; the definition of

what constitutes tax deductible expenses; and state and local property taxes. Following the line of thought expressed above for tax depreciation allowances, expansionary taxation policies reflected through these transmission mechanisms will lower the implicit rental price of capital and stimulate investment demand. As before, however, increases in the demand for external capital by firms in other sectors of the economy as well as by government may result in higher costs of capital if not its availability, and thus somewhat offset the desired expansionary effects of the government actions.

The final category of government actions discussed in this paper are those actions which affect the cost of estate transfers and planning for retirement income. The nature of federal estate and gift taxes and state inheritance taxes can affect the demand for loan funds by heirs of illiquid estates, the capital structure of their firms, and the supply of land for sale. For example, the Economic Recovery Tax Act of 1981 will no doubt substantially change the estate planning strategies of many individuals by increasing the amount of tax-free life-time gifts and estate transfers to their heirs. It also provides for the use valuation of real property if certain requirements are met, and raises the annual gift tax exclusion to \$10,000 per recipient (\$20,000 for both spouses). Because most firms in agriculture are sole proprietorships, these mechanisms are potentially much more important than they would be in sectors characterized by vastly held corporations. In addition, legislated retirement programs for self-employed individuals like farmers enables them to postpone the recognition of a portion of their current income for taxation while still earning a return on these funds. These funds are eventually recognized for tax purposes later when they are disbursed during the farmer's retirement years. The Economic Recovery Tax Act of 1981 also affects retirement planning in several ways. For example, the maximum contribution to individual retirement accounts has been increased. These and similar programs have an effect on the composition of investment portfolios in agriculture and the growth of the sector.

### **Other Specification Issues**

To assess the effects that events elsewhere in the domestic economy have upon agriculture, researchers should strive to incorporate the specific transmission mechanisms through which these events are relayed to agriculture when specifying their models. In this

section, I would like to address two additional specification issues that have a bearing on the size and timing of feedback effects in the economy.

### *Transmission Lags*

The first issue is the timing of the transmission of events elsewhere in the economy. Gordon, in an excellent survey article on the transmission of output fluctuations through prices, argues that this adjustment process is gradual in nature. Uncertainty about prices arises from the fact that markets do not clear instantaneously. During this period of disequilibrium, farmers will form expectations about prices based not only upon past prices, but on other information rationally thought to affect future prices. Thus, models which incorporate current prices when explaining outcomes in agriculture most assuredly will do poorly in forecasting future outcomes. The adaptive expectations hypothesis, used for many years, is being replaced by a variety of rational expectation hypotheses based upon the initial work of Muth in his landmark *Econometrica* article. For example, the work by Lucas, Sargent, and others — critically examined by Gordon — suggests that at least part of the forecast errors incurred by macroeconomic modelers in the past has been a result of how they modeled producer and consumer expectations.

### *Capacity Depreciation*

Considerable space has been devoted in the economic journals to the measurement of capital stocks and flows. Yet many macroeconomic modelers continue to assume that capital wears out in a geometric decay fashion because of the relative ease of employing this assumption. Coen has shown, however, that structures in the manufacturing sector wear out in a "one-hoss shay" capacity depreciation pattern much like the decline in the capacity of a light bulb. Coen — as did Griliches before him — also seriously questioned the wisdom of using the geometric decay capacity depreciation pattern when measuring stocks and flows of equipment. In fact, both have shown that the one-hoss shay and straight line patterns do a better job of capturing the factors underlying investment behavior when it comes to equipment than does the frequently used geometric decay pattern.

Penson, Hughes, and Nelson have shown that the choice of capacity depreciation pattern can have a significant effect on the



productive value of the existing stock of tractors on farms from one year to the next. Penson, Romain, and Hughes have also shown that this choice will affect the time series data on the implicit rental price of capital, the lagged capital stock, and the lagged dependent variable used in econometric investigations of net investment behavior, as well as the derived partial production elasticities in aggregate production functions. In short, adoption of the geometric decay capacity depreciation pattern when measuring the productive value of capital, in a world characterized by much smaller annual losses of capacity in the early stages of an asset's service life, will understate the productive capacity of agriculture, overstates its productivity, and bias econometric investigations of aggregate investment behavior in this sector. Importantly, similar measurement practices in other sectors of the economy will also affect forecasts of agricultural outcomes through their effects on many of the transmission mechanisms identified earlier in this paper.

### **Classification of Existing Models**

Agricultural sector models can be categorized according to the manner in which they recognize the linkages between agriculture and the rest of the general economy. Three such generations are described in this section.

#### ***First Generation Models***

First generation models view agriculture as a separate entity. Agriculture in these stand-alone models is influenced by relatively few macroeconomic variables. Three variables normally chosen are consumer disposable income, interest rates, and a particular broadly based implicit price deflator. Disturbances originating in agriculture, however, are assumed to have no impact on the rest of the domestic economy in first generation models, no matter how long the length of the forecast horizon.

Representatives of first generation models include the aggregative income and wealth (AIW) simulator developed by Penson, the Polysim simulator reported by Ray and Richardson, the capital and credit simulation model developed by Melichar, the agricultural sector modeling of Duloy and Norton, the national crop response model maintained by the USDA during the 1960s and early 1970s, and the sector simulation model reported recently by Schutzer, Roberts, Heady and Gunjal to name a few. Single equation models

and market equilibrium models focusing on a particular agricultural commodity or group of commodities like the Yeh model of the supply and demand for raw agricultural products can also be classified as first generation models because of their stand-alone nature.

First generation models focusing on the agricultural sector generally omit many of the transmission mechanisms through which events in other sectors of the domestic economy are relayed to agriculture. Investment functions, for example, generally fail to include some of the arguments contained in the implicit rental price of capital discussed earlier. In fact, one particular model forecasting capital flows in agriculture omitted any references to the cost of capital.

### *Second Generation Models*

Second generation models are those which forecast events in agriculture in a recursive fashion. An economy-wide macroeconomic model is first used to forecast a set of macroeconomic variables which appear in the agricultural sector equations. This information is then used to solve the agricultural sector equations. Finally, the solution values for a selected number of agricultural variables are fed back to the macroeconomic model and the macroeconomic solved again. No attempt is made to iterate this feedback loop in search of a set of a general equilibrium prices and quantities. Thus, while agriculture has an impact on the general economy in these models, the impact is delayed one period.

Representatives of second generation models include the Wharton Agricultural Model as reported by Chen and the Federal Reserve-MIT-Penn econometric model. These econometric models generally focus on flows of funds (but not capital flows), with net farm income being the only measure of the economic well-being of participants in agriculture. While these agricultural sector models are linked with macroeconomic models of the U.S. economy, these linkages are recursive rather than fully simultaneous in nature. An interesting twist to this recursive linkage is offered by the Federal Reserve-MIT-Penn model, where current agricultural product prices are explained by current nonagricultural product prices.

Because these models generally ignore capital stocks and flows in agriculture as well as the composition of farmers' investment portfolio, such transmission mechanisms as the implicit rental price of capital and market interest rates and yields are excluded from the

agricultural equations. One exception is the use of average interest rates to determine current interest expenses. Interestingly enough, some second generation models which project interest expenses in agriculture so that they can project net farm income do so without projecting period-to-period fluctuations in farm debt outstanding.

### ***Third Generation Models***

The linkages between agriculture and the general economy have been discussed in a number of invited papers and discussions presented at American Agricultural Economics Association (AAEA) meetings by King, Popkin, Roop and Zeitner, Johnson, Just, Penson and Hughes, and Gardner. Just, for example, summarized his concerns regarding partial equilibrium analyses by concluding that "both general and agricultural forecasters may benefit by pooling their models" (p. 137). Johnson criticized models designed to capture the linkages between agriculture and the rest of the general economy by recursively linking agricultural sector models to established macroeconomic models, concluding that "there must be more to the connection between economic sectors of the economy" (p. 134).

In response to calls for endogenization of the linkages between agriculture and the rest of the general economy, several econometric models of the U.S. economy determine agricultural outcomes simultaneously with outcomes in other sectors have been developed in the last few years. The first model, discussed in a contributed paper presented by Shei and Thompson at the 1979 AAEA meetings, was extremely aggregate, capturing the entire economy in fewer than 40 equations. Lamm later reported an even more aggregate model of the U.S. economy which included only 28 equations. By condensing the coverage of the economy to such a small number of equations, both models mask many useful economic relationships. For example, there are only three inputs to agricultural production in the Lamm model: (1) the real annual capital flow in agriculture, (2) the size of the agricultural labor force, and (3) time. While other issues about the specification of this function can be raised, certainly further disaggregation can be justified to at least capture the input substitution brought about by the changing relative costs of fuel and capital.

Prentice has recently developed a macroeconomic model which consists of more than 100 equations. This model thus provides more

detailed information than those reported by Shei and Thompson and by Larnrn. This model lacks any reference to credit markets, however. Because farmers finance a large share of their expenditures with external capital, the linkages between agriculture and the financial markets in the economy should be endogenized.

A fourth multi-sector macroeconomic model containing a fully simultaneous agricultural sector was recently reported by Hughes and Penson. Their model captures the linkages between (1) agriculture and the suppliers of manufactured production inputs, (2) agricultural output, wholesale purchases of food items, and the final consumption of agricultural goods at the retail level, (3) agriculture and the U.S. balance of trade and exchange rates, (4) agriculture and the government sector, and (5) agriculture and the nation's financial markets.

This model contains eight economic transactors: farm operator families, hired labor families, nonoperator landlords, nonfarm production units, other domestic consumers, governments, financial intermediaries, and the rest of the world. While there are too many goods in the model to list individually, they essentially can be classified as either physical goods or financial obligations. Physical goods in the model include primary inputs (land, labor, and crude petroleum), secondary inputs (equipment and structures, other manufactured production inputs, and raw agricultural products), and final consumption goods (consumer durables, food, and other consumer goods and services). Financial obligations include bank deposits, bonds, equities, and debt. Supplies and demands for each of these goods and services converge to a set of general equilibrium prices and quantities in the Hughes-Penson model.

In addition to these econometric representatives, the general equilibrium modeling efforts of Plessner and Heady, the linear programming input-output modeling of Penn, McCarl, Brink, and Irwin, and the quadratic programming input-output modeling of Harrington, Penson and Fulton, Penson and Webb, and Talpaz and Penson, have all led to models that can be classified as third generation models. The linkages between agriculture and the general economy in each of these models are treated in a fully simultaneous fashion, although only the intermediate demand for goods and services is treated in a fully simultaneous fashion in linear programming input-output models.

## **The Value of Endogenization**

In an invited paper at the recent AAEA meetings, Gardner, while recognizing the need to simultaneously account for the interrelationships between agriculture and the rest of the economy, concluded that it is "preferable to use the macroeconomist's model for the economy-wide variables, and **sectoral** models with deflated prices for agricultural variables" (p. 16). Gardner is essentially asking whether it is really worth the effort to solve agricultural and **non**-agricultural outcomes simultaneously. This is a valid question since it takes considerably more time and money to develop third generation models. The value of endogenizing the linkages between agriculture and the rest of the economy can be addressed in terms of its effect on forecast errors for agricultural and nonagricultural variables of questions the model can address.

### ***Lower Forecast Error***

Information provided by economic models should improve the likelihood of making correct decisions. Obviously no model will do a perfect job of forecasting the impact of a given decision. Yet we should strive to minimize errors in forecasting economic outcomes in agriculture as well as the rest of the economy. Hughes and **Penson** recently used their general equilibrium model of the U.S. economy which emphasizes agriculture to forecast events five years into the future with all three model configurations. A forecast time period of 1971-1975 was chosen because it represented a time of unusually high variability in agricultural outcomes. If it is important to capture the linkages between agriculture and the rest of the economy, the early 1970s should demonstrate the benefits of this endeavor. The Russian wheat deal and the first OPEC oil embargo occurred during this time, contributing to a highly **volatile** set of prices paid and received by farmers.

Because individuals differ in their ability to see the interdependencies among large numbers of exogenous variables, the quality of a model's forecast is not merely a function of its specification and estimation. The artistry of accounting for the relationships among exogenous variables may be the most important aspect of forecasting. Hughes and **Penson** used actual observations for the forecasts, using the exogenous variables when forecasting with representatives of each modeling generation.

The mean absolute percentage forecast errors (MAPE) calculated over the entire five-year forecast period declined in almost every case as endogenization of nonagricultural events was increased. In some cases, the reduction in forecast errors was substantial. For example, the MAPE for constant-dollar gross farm income in third generation models over the entire forecast horizon was about 40 percent less than that associated with the first and second generation models. The value of **endogenizing** agriculture on the MAPE for nonagricultural variables was most evident in the aggregate price indices. The MAPE for the consumer price index showed a decrease of almost a full percentage point. The MAPE for gross national product, however, was not appreciably affected.

Their results also suggest that the value of endogenization increases as one forecasts further into the future. While the third generation model provided some minor improvements in forecasting one year into the future for some variables, percentage errors for most variables were roughly the same until the fourth and fifth years of the forecast period, where the third generation model achieved substantially lower percentage errors. As feedback between different sectors is more fully incorporated into the model, more of the constraints on the activities of the decisionmaker are captured, making the model's forecasts more realistic.

Finally, Hughes and Penson found that the move from a second generation to a third generation model resulted in a much greater improvement in forecast errors than the move from a first generation to a second generation model. Only marginal improvements were found in the MAPE between the first and second generation models. Most of the reductions came in the move to the third generation model. One caveat to these conclusions should be mentioned at this point, however. The model used by Hughes and Penson was an annual model, while most second generation models are solved quarterly (i.e., estimated using quarterly observations of seasonally adjusted information expressed at an annual rate). Since the feedback from agriculture to the rest of the economy is more frequent, it may be argued that their recursiveness is less of a limitation than suggested by the results reported by Hughes and Penson. This empirical question definitely merits further investigation, however.

### *Scope & Analysis*

Third generation models are obviously going to be in a better

position to respond to a broader range of questions than first generation models. However, it is probably not clear to many just how restrictive the set of questions is that can be responsibly answered by first and second generation models. The lack of feedback between agriculture and the rest of the economy embodied in first and second generation models strictly prohibits them from addressing certain questions that might substantially alter general economic outcomes. For example, Hughes and Penson reviewed the impact a major drought would have on the financial condition of farmers, as well as the rest of the domestic economy. To begin with, a major drought would mean increased prices of farm products, which in turn would mean (1) increases in the relative price of food in domestic retail and export markets, (2) decreases in the purchases of nonfood consumer goods since the demand for food is inelastic, (3) increases in government expenditures (disaster payments to farmers), (4) decreases in the value of the dollar in foreign exchange markets, and (5) inflationary pressures if the money supply is increased to finance a growing deficit. Higher inflation rates in future periods would lead to increases in the costs of farm inputs, nominal interest rates, and unrealized capital gains on farmland.

First generation models would not capture these feedback effects. Second generation models would miss much of the impact that higher current food prices would have on current price levels by overlooking the impact this change has on consumers' decisions to purchase other goods and services.

## Conclusions

Agricultural economists who hope to successfully model agriculture in the increasingly integrated economy of the 1980s will have to expand the scope at their models. Multi-sector, fully simultaneous macroeconomic models deserve further consideration as a means of addressing the issues confronting agriculture in the 1980s. This paper suggests that the benefits of taking a disaggregated view of the national economy are both measurable and substantial.

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## Commentary

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*Dean E. McKee*

I am approaching my assignment in this symposium from the viewpoint of a user of models of agriculture rather than that of a designer of models. Further, my viewpoint is that of an economist employed in the private sector in an industry engaged in supplying capital inputs to agriculture which must deal with an uncertain future in rather specific quantitative terms in supporting the business planning function. Long-term capital commitments involved specific decisions with respect to time, geographic location, size of the facility, and the equipment included within it. Short-term manufacturing production decisions are explicit with respect to the volume and specifications of the products to be produced and the timing of their production. The resulting purchase orders state in precise terms the specifications and volumes of materials required and delivery dates.

Reasonably reliable assessments of the most likely future course of economic events are required to establish the multitude of quantitative parameters involved in implementing such decisions. In an increasingly complex, volatile and changing economic environment, the penalty for errors in anticipating future economic developments can be severe, if not disastrous. By the same token, the rewards of correctly anticipating the future course of the economy can be substantial.

Since foresight in anticipating the future course of economic events yet falls far short of perfection, there exists the need for the capability to quickly re-evaluate future prospects when it becomes apparent that actual events deviate from forecasts either due to fundamental errors inherent in the forecast or as a result of the occurrence of new or unanticipated developments that require modi-

fication of plans within the limits of existing commitments. Both the cost of carrying through the analysis and the rapidity with which one can assimilate and respond to new information become important considerations. Timeliness of the analyses become a particularly important attribute.

There is a growing appreciation within the private sector of the usefulness of mathematical models in developing forecasts of business prospects, by allowing one to more fully and systematically take into account the increasingly complex set of interacting forces that impact the sector of the economy within which a firm operates. There is an increasing desire to have access to appropriately specified models as an analytical tool to enable one to more fully and more quickly evaluate the possible future implications of economic events as they occur. It is my impression, however, that this application of models is less fully developed within the agribusiness sector of the economy than it is in many other sectors.

While the end objectives of the application of models in the private sector may be somewhat different than the end objective of the application of models to policy analysis, the specification requirements for either application should be little different. In policy analysis the end objective is to evaluate the implications of a range of policy alternatives for the purpose of providing insight as to the most appropriate choice of policy mix. In the private sector, the end objective is to develop a forecast of economic conditions that impact a firm's future business prospects, usually under a given set of policies, or if policies are altered to determine the impact of a changed policy upon a firm's future business prospects. In either application the relevant and significant linkages between the agricultural sector and other sectors of the economy, as well as the relevant policy variables and their linkages to the economy, need to be specified in the model if reliable interpretations are to be made.

As Professor **Penson** has particularly emphasized, the growing interdependency between agriculture and the remainder of the economy has tightened the linkages between the agricultural sector and other economic sectors. Increasingly, agriculture must compete with the other sectors of the economy for resources used in common: energy, water, capital, land, labor, metals, and chemicals. To a similar but probably lesser degree, because of the fundamental nature of food, agriculture must compete with other economic sectors for the consumer's discretionary spending. Professor **Penson**

is correct in stating that the usefulness and reliability of models of agriculture are limited by their failure to reflect their linkages in their structure. His solution is the obvious one of including in models of the overall economy a more fully specified agricultural sector, thereby permitting the reflection of the simultaneous feedback between the agricultural sector and other economic sectors to which it has a linkage. This a direction in the development of models that is desired and to be encouraged.

However, Professor **Penson's** solution has the equally obvious disadvantage of requiring larger and more complex models which are costly to develop and maintain and cumbersome to use. Advances in computer technology have over time enhanced the ability to effectively manage large-scale models, and I suspect that such advances will continue to be made in the future, so this may not be a serious limitation.

A more serious limitation may be the availability of the appropriate data to permit the identification and quantification of the relevant linkages. Unfortunately, most of our economic data-gathering system was not designed with models in mind. As a result, model builders must make do with the data that happens to be available. Caution does need to be exercised that the specification of models does not outpace the capability of the available data to support them. Theoretically, elegant models based upon inadequate data can be just as misleading as a model that is not specified in sufficient detail. There is risk of discrediting a useful analytical approach by claiming more for a model than can be reasonably delivered. Professor **Penson** does, however, provide some evidence that does suggest that significant improvement in the quality of forecasts can be achieved by fuller specification of the agricultural sector within a model of the general economy, to more fully reflect the feedback among the sectors.

In discussing the modeling of investment behavior, Professor **Penson** points out the potential bias introduced into the model by employing the commonly used assumption that capital wears out in a geometric decay fashion. His example illustrates the care that must be taken in the uses of simplifying assumptions. In our own efforts to forecast farm machinery demand, we have had limited success in trying to relate current equipment purchases to the stock of equipment on farms, or with efforts to estimate a replacement cycle. We have had consistently better results by relating current

purchases to measures which reflect current economic conditions surrounding agriculture, with appropriate lags to conditions in preceding periods. We find the investment behavior of farmers with respect to machinery purchases to be more a function of current economic condition than of the stock of machines on farms. Farmers tend to be quite flexible in their machinery replacement patterns in an agricultural market that is as mature and highly mechanized as that of the United States. Farmers can readily defer their equipment purchases, when economic conditions are difficult, as they are at the present time, in the face of weak income, declining commodity price, and extremely high interest rates.

You might be interested in a brief description of our approach to the use of models in our efforts to forecast the near-term demand for farm equipment. We do make use of a fairly extensive model of the U.S. agricultural sector to develop a forecast of economic conditions expected to prevail over a two-year period into the future. The model is quarterly. A macroeconomic model, also quarterly, is used to establish a forecast of the relevant general economic variables required to produce the agricultural forecast.

The model of the agricultural sector does not include relationships that model the farm equipment demand. The farm equipment demand models are instead simple, single equation models that are solved separate from the agricultural model but are based upon output from the agricultural as well as the macroeconomics model. The forecasts are updated each quarter. It has also been our practice to re-estimate our machinery demand models each quarter to take maximum advantage of the latest available information on actual retail sales of machinery and the relevant economic variables. We also find this practice advisable as a means of monitoring the stability of our forecasting models.

Our approach may not appeal to the model purist because the whole complex is not neatly brought together into a single model. There is of course room for improvement, and we are continually making refinements as we learn more about the behavior of our markets. However, the point is that the approach we have taken, while not without problems, has tended to serve our needs quite well. In reviewing past forecasts to determine where we went astray, we have generally found that our principal source of error has been the failure to anticipate sudden and unexpected events such as the imposition of a grain embargo, a massive drought, or the spread of

the southern corn blight throughout a major portion of the Corn Belt.





## The Foreign Trade Linkages

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*G. Edward Schuh*

Historically, most efforts to model the U.S. agricultural sector or its individual subcomponents have assumed a closed economy. Exports or imports were either ignored, treated as exogenous and added to demand or supply through identities, or explained with very simple, naive models.

During the 1950s and 1960s, such approaches in most cases did little violence to reality. With a few exceptions, trade was in fact relatively unimportant. Exports were important for only a few commodities, and such imports as we had did not compete directly with domestic products since they consisted largely of tropical products. Moreover, such competition as there might have been was excluded by trade barriers designed to protect domestic commodity policies.

Ignoring the trade sector is no longer realistic, however. During the 1970s, U.S. agriculture effectively became part of a world agricultural economy. Agricultural exports burgeoned, with the result that today approximately 30 percent of cash marketings are attributed to export sales, and the output of slightly more than one out of every three acres of cropland is sold abroad. For individual commodities, these percentages are even greater. We export roughly 60 percent of our production of wheat and cotton, 40-50 percent of our soybeans, and some 30 percent of our corn and tobacco. These are important commodities in the agricultural sector. Moreover, many of the shocks to these sectors come from the trade sector. To ignore trade is to ignore an important set of factors affecting the agricultural sector.

This situation is further complicated by the fact that exports of some commodities that are important to U.S. agriculture dominate

the international trade in those commodities. For example, the U.S. exports 70 percent of all the corn, 70-80 percent of all the soybeans, and 40 percent of all the wheat that move in world trade. The small-country assumption is hardly appropriate under these circumstances, so one cannot take international prices as exogenous in these cases.

Treating international prices as endogenous greatly complicates modeling efforts. One has to understand import demand and export supply from other countries, and this requires that one understand the agriculture and market conditions in those countries. Moreover, trade flows are complex, with a great deal of product differentiation and multiple flows among countries. In addition, government policy becomes a major factor affecting trade flows and if we ultimately want to push back to the identification of underlying casual factors, we must understand why governments do what they do.

Finally, commodity markets can no longer be understood in isolation of capital markets, either domestically or internationally. Although rather badly neglected until recently, shifts in real exchange rates can be a major factor influencing trade flows. Changes in real exchange rates took place even when nominal exchange rates were fixed. What has complicated this picture is that international capital markets have become increasingly important and increasingly well integrated over the last decade. In the context of a flexible exchange rate regime, these well integrated capital markets cause monetary policies to impact on agriculture in very different ways than with a fixed exchange rate system in which capital markets are either highly segmented or atrophied. Moreover, under the post-Bretton Woods system, monetary policy and conditions in capital markets can exert important influences on commodity markets.

The remainder of my paper is divided into three parts. The first part is a brief review of the alternative approaches that have been taken to modeling agricultural trade. Here I draw on an excellent paper by Robert Thompson.<sup>1</sup> The second part discusses the monetary aspects of agricultural trade and briefly reviews the state of knowledge of these phenomena. The third part makes some suggestions for directions that trade modeling efforts might take if we are

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1. "A Survey of Recent U S Developments in International Agricultural Trade Models." *bibliography and literature, Agriculture #21*, ERS. USDA. September 1981.

to develop realistic models of agricultural trade.

### **Approaches to Agricultural Trade Modeling**

There was a large increase in agricultural trade modeling in the 1970s, a reflection of the growing importance of trade to U.S. agriculture. The models which have evolved can be classified into basically two groups. In one group are the two-region models which involved essentially econometrically estimated export demand equations. The second group consists of multiple-region models and includes (1) nonspatial price equilibrium models, (2) spatial price equilibrium models, and (3) trade flow and market share models.

Let me attempt to characterize such models and make a brief assessment of their value for policy purposes.

#### ***Two-Region Models***

In two-region models all countries of the world are divided into two groups: the one of interest (e.g., the United States) and all others. Two-region models are basically agricultural sector models that are open to international trade. They contain explicit import demand or export supply relations and linkages between the domestic and world market prices to reflect the simultaneous determination of domestic supply, utilization, and price with those in the rest of the world.

Such open-economy models constitute a significant part of the agricultural trade research to date and have been used extensively for U.S. trade policy analysis. However, as Thompson notes, such models are not trade models in the strictest sense since they do not account for source-to-destination trade flows.

An import-demand or export-supply equation is nothing more than an excess demand or excess supply equation. Hence, it is the domestic demand curve minus the domestic supply curve, or vice versa, whichever the case may be. For the export-supply equation, the domestic-demand and supply curves are relevant. For the import-demand equation, it is the demand and supply conditions in the foreign country that are relevant. In the case of import demand, there would be one such equation for each country. This suggests how complex a structural model might be if it were to reflect any degree of country detail.

Two approaches can be used to obtain estimates of the parameters of such equations. The first is to estimate them directly. The second

is to calculate them by means of Yntema's formula,<sup>2</sup> which provides estimates of import-demand and export-supply elasticities as a weighted sum of the domestic demand and supply elasticities. The weights are relative shares of imports and exports in relation to domestic consumption and production.

Thompson surveys the various studies that have attempted to estimate these parameters. Quite a number of attempts have been made, both with single-commodity and multicommodity models. However, these studies have reached little consensus on the underlying or "true" elasticities, and considerable controversy still prevails, for example, over whether the foreign import demand for U.S. agricultural exports is price-elastic or price-inelastic. From a policy standpoint, this is an important issue, of course.

The reasons for the lack of consensus on the basic parameters are fairly obvious once one remembers how an "ideal" model might be specified and compares it with equations whose parameters are actually estimated. Thompson summarizes the points very well. First, an equation representing the excess demand of the rest of the world represents in effect the net effect of all supply and demand adjustments in all other trading countries. If the countries participating in trade change and their respective import demand elasticities are different, then the elasticity obtained would be quite sensitive to the time period used for the study.

Second, exchange rates, tariffs, subsidies, and transportation costs should be taken into account. The effect of changes in these factors is to **either shift** or rotate the excess demand schedule faced by an individual country. These factors do change from time to time. But when an aggregate relationship is used for purposes of estimation, there is no way to take account of such shifts. Hence, aggregation problems are quite serious.

Third, most models treat only one commodity at a time and ignore important linkages and interrelationships. They also tend to use OLS estimation procedures. Hence, the parameter estimates are subject to both specification and simultaneous equations bias. Moreover, most variables are probably measured with substantial error, and this introduces additional bias.

Finally, all shifters of the domestic supply and demand schedules

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2. Yntema, T.O.. *A Mathematical Reformatron of the General Theory of International Trade*. Chicago. University of Chicago Press, 1932.

in all other trading nations would be variables in a correctly specified import demand equation for U.S. exports. These shifters are typically omitted from the estimated equation, with specification bias again the likely result. Moreover, it simply may be asking too much to expect that a single import demand equation could adequately reflect the myriad forces which sift it from year to year. Put somewhat differently, one is faced with a rather serious identification problem, and the models used to date have had very weak identification power.

In conclusion, popular as the two-region models have been, they have really contributed little to our understanding of the interrelationships between the U.S. and world markets. Perhaps a more important point is that even if acceptable parameters estimates were available, their only value would be in analyzing domestic farm policies and U.S. trade policy. This is because it is impossible to tell how to change the import demand function in response to a policy change in any individual foreign country. Yet such changes in policy are the coin of the realm. Moreover, since policies in one country may tend to respond to changes in another, policy models for the U.S. really need the additional detail.

### ***Nonspatial Price Equilibrium Models***

Nonspatial price equilibrium models are the simplest multiple-region models one can have. They explicitly treat the interrelations among trading regions by assuming that the world market price is determined simultaneously by the supply-demand balance in all trading regions in such a way that the global market clears. The models are comprised of systems of equations which may be solved by various techniques. The model solution gives the world market clearing **price(s)** and net trade of each region trading in the world market. However, it provides no information on source-destination trade flows.

There are three classes of nonspatial price equilibrium models, with each class differing in the nature of the price linkages among the trading region. One class assumes the existence of one global market-clearing price (often the U.S. domestic or export price) at which all international transactions occur. In the second class the commodity prices in all but one region in the model are linked through transportation costs to the price in the *n*th region, which is often the United States. The third class is made up of models which

link prices through transport costs pairwise along the principal historical trade flows. This produces a web of price linkage equations. Although this class of models introduces a spatial pattern of prices, it differentiates itself from spatial equilibrium models in that it generates only the net trade position of each trading region, while the latter generates source-destination trade flows.

Nonspatial price equilibrium models can and often do include considerable detail on domestic markets. In addition, such models can also easily reflect tariff policies, although as Thompson notes, in practice they tend to have a free-trade bias.

The focus of these models is on the interrelationships among the trading regions. To be useful for this purpose, the models must reflect the structure of the markets of the regions linked through trade. This includes not only the structure of internal demand and supply, but also government policy behavior and the competitive structure of the industry.

Most models of this kind have contained internal supply and demand schedules of the trading regions. However, some have contained only an export-supply or import-demand schedule for each region.

Thompson observes that past research using such models has put much more emphasis on model specification and solution technique than on the empirical content. He finds that few validation statistics are reported, and that little attempt has been made to assess whether parameter estimates are realistic in light of the phenomena being modeled.

A major difficulty in doing research on such models is the availability of data. Obtaining data that are consistent across countries is a major challenge. No single organization now has the responsibility for doing this. Obtaining information on country policies is equally difficult, especially when most such information is in a foreign language. It may be that awareness of these data problems is one of the reasons why so little attention has been given to the empirical content of these models.

Thompson notes that researchers developing such models have tended to neglect the relatively large number of agricultural sector models that are available and that could be used as building blocks. The IIASA world model of agriculture is the only case in which considerable effort has been invested in developing satisfactory country models as elements of the nonspatial price equilibrium

models.

Another deficiency of such models is that they have failed to take account of trade interventions in a realistic way. Tariff barriers in particular can be easily introduced into a simultaneous equations model by means of the price linkage equations. But many nontariff barriers can also be introduced. Given the extent of such trade interventions, the failure to take account of them can only result in models that are of little value for policy purposes.

Government policy decisions are another element that have been neglected in such models. Considerable evidence has now accumulated that such decisions are not exogenous to the commodity markets.<sup>3</sup> It is not difficult to endogenize policy variables by including policy reaction functions or price transmission equations. Given the importance of government in most countries and the instability of government policies, the failure to endogenize this sector must be considered a serious deficiency of such models.

Two other variables that are usually treated as exogenous and which may need to be treated endogenously are freight rates and currency exchange rates. In years of unusually large volumes of trade, freight rates are clearly not determined exogenously. Little work has been done, however, to understand this important sector of the trade economy.

Similarly, Cheng and Chambers and Just have shown that the U.S. dollar exchange rate has been sensitive to changes in the value of agricultural exports.<sup>4</sup> This suggests that the exchange rate should also be made endogenous to the agricultural trade sector. This issue will be taken up below.

A final comment on such models has to do with the homogeneity assumption that is usually made. Grennes, Johnson and Thursby<sup>5</sup> found that in the case of wheat there was little correlation among

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3 For a survey and analysis of the literature on endogenizing government behavior, see Gordon C. Rausser, Erik Lichtenberg, and Ralph Lattimore, "Developments in Theory and Empirical Applications of Endogenous Government Behavior," in Gordon C. Rausser, ed., *New Directions in Econometric Modeling and Forecasting in U.S. Agriculture*, Amsterdam, North-Holland Publishers, forthcoming.

4 G. D. C. Cheng, "A Study of the Impact of U. S. Grain Exports on the Exchange Rate and the Domestic Price Level." Ph. D. Thesis, University of Notre Dame, South Bend, Indiana, 1976, and R. G. Chambers and R. E. Just, "An Investigation of the Effects of Monetary Factors on Agriculture," forthcoming in *Journal of Monetary Economics*.

5 T. Grennes, P. J. Johnson, and M. Thursby, "Some Evidence on the Nebulous Law of One Price," paper presented at the annual meetings of the Southern Economics Association, Washington, D. C., November 1978.

prices of the same commodity in different countries. Only as they narrowed the specification of the product did the correlations increase until the prices of the export goods for two different countries become almost perfectly correlated. This suggests that even quite narrowly defined agricultural products are in fact aggregates of different goods.

In conclusion, nonspatial price equilibrium models have made several contributions to understanding the interrelations among trading regions. In particular, they have helped us understand the extent to which world market shocks get transmitted through policy reaction functions or price transmission equations. The failure to give more attention to the empirical content of the models has limited their value for policy purposes, however. In particular, the failure to give more attention to trade distortions is somewhat paradoxical given the importance and significance of such distortions and the relative ease with which they can be introduced in the models.

### *Spatial Price Equilibrium Models*

Spatial equilibrium models are the most common class of agricultural trade models, particularly for comparative statistical analysis of the effects of a change in policy. These models are distinguished from the previous two classes in that they endogenize trade flows and market shares. They are structured in a manner consistent with spatial equilibrium theory, with the result that prices are directly linked only between those pairs of countries which actually trade with each other.

The data requirements for a spatial price equilibrium model are identical to those for a nonspatial price equilibrium model. Both require internal supply and demand schedules or an export-supply or import-demand schedule for each trading region, documentation on the levels of all policy variables, exchange rates, and a matrix of transportation costs.

The fundamental difference between spatial and nonspatial price equilibrium models is in the solution technique used. Most spatial models have been linear and solved by quadratic programming.<sup>6</sup> However, the disadvantage of linear equations has been overcome

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6. Other techniques include specifying the problem as a classical transportation problem and the use of linear and reactive programming; models with nonlinear demand equations have also been used.



by separable programming, Bender's decomposition, and nonlinear solvers.

An advantage in using quadratic programming is the facility with which policies can be introduced. Tariff barriers can be introduced in these models in basically the same way as in nonspatial price equilibrium models. Moreover, quantitative restrictions to trade can be introduced directly as linear inequality constraints in the constraint set of the QP problem. This is easier than using "if" statements in iterative solution techniques for systems of nonlinear equations.

One of the principal arguments for use of spatial over nonspatial models is that the former generate trade flows and market shares, variables which are of interest to some users of the models. In practice, however, this advantage has been illusory, in large part because the spatial models have not explained real world trade flows very well. This in turn is probably due to the fact that inadequate attention has been given to the empirical realism of the models.

As an example, the spatial equilibrium model assumes perfect certainty, yet the real world is characterized by risk and uncertainty. Risk behavior could be reflected in trade models in the same way that **Hazell** and Scandizzo<sup>7</sup> have introduced it into agricultural sector models. Yet Thompson could not find any attempt to use such a procedure.

Some users of trade policy analyses need information on the time path of adjustment of supply, disappearance, and price. Modeling work so far has done little along this line, although a number of different approaches might be used to generate such information. Moreover, if storage costs were included as the cost of carrying wheat from one year to the next, insight could be provided into the issue of optimum reserve stocks.

Another deficiency of spatial equilibrium trade models is their assumption that all trading countries behave perfectly competitively. The objective function could be altered to make every region trade on its marginal import cost or marginal export revenue schedule. However, such an approach would probably reflect inadequately the differences in market structure among trading regions.

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7. P. B. R. Hazell and P. L. Scandizzo, "Competitive Demand Structures Under Risk in Agricultural Linear Programming Models," *American Journal of Agricultural Economics* 56(1974), pp. 235-244.

Other aspects of the empirical deficiencies of such models includes simultaneous equations and specification bias in the structural elements of the models. The empirical results from the model can be no better than the empirical input to the model. Until more attention is given to these details, and to the detail of policy interventions, these models will provide poor replicas of real world phenomena.

### ***Trade Flows and Market Share Models***

The motivation for developing trade flow and market share models was the failure of spatial price equilibrium models to adequately account for trade flows and the lack of empirical support for the law of one price in world agricultural markets. As noted earlier, commodities are not perfectly homogenous. Moreover, both importers and exporters may want to diversify their sources and markets, respectively, due to market uncertainty or for historical and political reasons.

The trade flow and market share models are a response to these problems, and focus on explaining the elements of the trade flow matrix. The various approaches used include mechanical procedures which transform the trade flow matrices from one year to the next without regard for price, econometric models designed to explain one or more elements of the trade flow matrix, and modifications of the spatial equilibrium models in which the elasticity of substitution among sources of supply is less than infinite in each importing region. The latter includes the so-called Armington approach to trade modeling.

The mechanical techniques, of course, lack normative content and can offer little guidance for policy formulation. Typical of these are the use of derived transition matrices. A second technique is the constant market share approach, which assumes that each exporter's market share is constant through time unless something happens which alters that exporter's competitiveness. A given country's export growth is then decomposed into various components, much as time series data are decomposed by mechanical procedures.

A related approach to studying trade flows is through probabilistic trade models. Still another approach is to use Markov models to predict market shares. This technique follows Telser's approach<sup>8</sup> to

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8. L. G. Telser, "The Demand for Branded Goods as Estimated from Consumer Panel Data," *Review of Economics and Statistics* 44(1962), pp. 300-24.

analyzing domestic demand for branded goods and does bring prices into the explanation.

The second class of trade flow and market share models implicitly or explicitly assumes that the U.S. exports of the respective commodities of interest are not perfect substitutes for exports from other countries in each importing country. Perhaps the most common application has been to estimate equations which explain the shipments from a given exporter to each foreign destination. These are usually represented as regional import demand equations for the given country's exports. Another approach has been to estimate a total import demand equation for each importing region and separate market share equations for the U.S. and other exporters.

The assumption that importers differentiate among goods by country of origin implies that the elasticity of substitution between countries of origin is less than infinite. Armington<sup>9</sup> has developed the theory for a class of trade models in which goods are differentiated by country of origin. In this approach it is assumed that the utility function is weakly separable so that the consumer's decision process may be viewed as occurring in two stages. The total quantity of a commodity to be imported is first determined, and then this quantity is allocated among the competing suppliers. The model is simplified by assuming that the total quantity of the product imported is a constant elasticity of substitution (CES) index of the quantities imported from the respective countries of origin. Given these assumptions, the cross-price elasticities between all pairs of countries of origin can be calculated from estimates of only the overall price elasticity of import demand and the (assumed constant) import elasticity of substitution and data on import shares. The cross elasticities, therefore, need not be estimated directly. The constant and identical assumptions for the elasticity of substitution can be relaxed, of course. This requires a multistage decision process instead of Armington's two-stage process.

By way of evaluation, the Markov approach is the only one of the mechanical approaches to analyzing trade flows which has explicit theoretical foundations. The models which seek to explain individual elements of the trade flow matrix suffer from the same specification and estimation problems as the import demand equations in

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9. P. S. Armington, "A Theory of Demand for Products Distinguished by Place of Production," *IMF Staff Papers*, 16(1969), pp. 159-178.

two-region models, since they tend to be specified as import demand equations.

The Armington approach to trade modeling, by explicitly introducing elasticities of substitution, can generate trade flows between **all** pairs of trading countries in solution. This represents a significant generalization of the spatial equilibrium model.

Thompson notes that the model also gives much smoother changes in trade flows in response to shocks than does the spatial equilibrium approach. He also points out that there is a logical inconsistency between assuming a commodity is differentiated by country of origin and then assuming that the same constant elasticity of substitution applies between all pairs of exporters in all import markets. Recent work has been directed to relaxing this assumption.

In conclusion, work on the trade flow and market share models has been the frontier of agricultural trade modeling in the past decade. Many of the approaches appear to account for the observed variation in trade flows more adequately than do spatial equilibrium models. Nevertheless, the theoretical foundation for several of the approaches is weak and few of the models include much policy or institutional content. Finally, the empirical content of the models also tends to be weak due to inadequate and incomplete data, specification errors, and choice of an inappropriate estimation.

### **The Monetary Aspects of Agricultural Trade**

The growing internationalization of agricultural commodity markets in the 1970s was a major factor influencing the modeling of the agricultural sector for policy purposes. Perhaps of equal importance was the shift from a system of fixed exchange rates to a system of flexible exchange rates and the growth and increased integration of the international capital **markets**.<sup>10</sup> In this section I want to briefly review this last set of developments and discuss the implications for modeling agricultural trade.

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10. The Bretton-Woods system of **fixed** exchange rates began to break down in 1968 when the world for all practical purposes went off the last semblance of gold standard. The **culmination** of the change occurred in 1973 when the U.S. floated the dollar. For a more **comprehensive** treatment of the monetary aspects, see G. Edward Schuh, Chris Hodges, and Dave Orden, "Monetary Aspects of International **Agricultural** Trade," Department of Agricultural and Applied Economics, **University** of Minnesota, December 1980 (mimeographed).

### Flexible Exchange Rates and Agricultural Trade

The shift from a system of fixed exchange rates to what is essentially a system of flexible exchange rates had two important consequences for agriculture. First, it permitted underlying comparative advantages to reveal themselves to a greater extent than they had under the fixed exchange rate system." In the case of the U.S., this accounted for an important part of the expansion of agricultural exports during the 1970s, since the value of the dollar experienced a significant decline with the shift to floating exchange rates.

An important aspect of the realignment of exchange rates was the large change that took place in the value of gold.<sup>12</sup> This change had at least two important effects. First, countries that held gold as part of their reserves experienced both an increase in the value of their international reserves for transactions purposes and a rather sizeable wealth effect, with both effects determined by how much gold they held. Second, the Soviet Union is a major producer of gold and sells it as the means of paying for its agricultural imports. The rise in the value of gold, coincidental with the decline in the value of the dollar, constituted a favorable shift in the terms of trade for the Soviet Union that undoubtedly contributed to its dramatic shift to external sources of supply for grain. A comprehensive modeling of this development would need to take into account the monetary phenomenon, per se, the change in terms of trade, and the response of policymakers in the Soviet Union to external economic conditions.

The second effect of the shift from fixed to flexible exchange rates was that, in the presence of well-integrated international capital markets, it altered significantly the way that monetary policy affected agriculture.<sup>13</sup> With a fixed exchange rate regime and seg-

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11. For a discussion of this phenomenon in the case of U.S. agriculture, see G. Edward Schuh, "The Exchange Rate and U.S. Agriculture," *American Journal of Agricultural Economics*, February 1974, 56(1), pp. 1-13.

12. The value of gold in dollar terms increased from \$35 an ounce to almost \$800 an ounce before declining to its present range of approximately \$400.

13. For more details on this set of issues, see G. Edward Schuh, "Income and Stability Implications of Monetary, Fiscal, Trade, and Economic Control Policies," *Farm and Food Policy Symposium*, South Dakota State University, 1977, and G. Edward Schuh, "Floating Exchange Rates, International Interdependence, and Agricultural Policy," presented at the 17th International Conference of the International Association of Agricultural Economists, in Banff, Alberta, September 3-12, 1979.

mented or poorly developed commodity markets, monetary policy affected U.S. agriculture largely through its impact on the intersectoral labor market. The secular outmigration of labor from agriculture, an integral component of economic development, has been quite sensitive to the level of unemployment in the general economy. With monetary policy reflected in differing levels of cyclical unemployment, the welfare of farm people and of course the level of farm output were influenced by monetary policy through its influence on the labor market. In terms of the secular adjustment of resources out of agriculture, this phenomenon was quite important and has been well recognized in the literature. In terms of short-term fluctuations in commodity markets, it was relatively unimportant, especially in light of the large stocks in government hands during most of the period in which these conditions prevailed.

The shift to floating exchange rates, in the presence of well-integrated international capital markets, significantly changed the impact of changes in monetary policy on U.S. agriculture. These changes were compounded by the decline in stocks in government hands, which had they continued might have attenuated at least some of the consequences.

The change in how monetary policy affects agriculture comes about because changes in monetary policy are reflected in changes in the value of the dollar. A tight monetary policy, other things being equal, leads to a rise in the value of the dollar and a decline in the competitiveness of the export sector in international markets. An easy monetary policy, on the other hand, leads to a decline in the value of the dollar and increased competitiveness. To put it simply, the trade sectors bear the adjustment of changes in monetary policy, and trade is now important to agriculture.<sup>14</sup>

Two points are worth noting in this context. First, the prices of both paper and real commodities can take place without actual changes in capital or commodity flows. In fact, one would generally expect the prices to change in the short run and then flows of capital and/or commodities to take place as time permitted adjustments to take place. In terms of model specification, we generally are concerned about both the changes in relative prices and in commodity flows.

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<sup>14</sup> It should be noted that the adjustments are borne by both the export sectors and the import-competing sectors. Our interest here, of course, is with the export-competing sectors.

The second point to note is that a number of developments converged to significantly change the conditions of commodity markets during the 1970s. It was this convergence of factors that made for such dramatic changes in these markets. For example, the shift to a floating exchange rate regime meant that foreign demand became more unstable than under the fixed exchange rate regime that prevailed earlier. This instability occurred at the same time that trade became relatively more important to U.S. agriculture. Similarly, international capital markets were growing rapidly and becoming increasingly well integrated at about the same time that we shifted from a fixed to a flexible exchange rate regime.

Finally, my discussant, Gale Johnson, has called our attention to the role of trade impediments as a factor causing instability in international commodity markets. With those impediments to market adjustment, it is no wonder that the monetary instability of the 1970s has led to such instability in commodity markets. This instability has been compounded by the virtual elimination of large government stocks which, whatever their liabilities and negative consequences, might have contributed some stability to the markets were they to be managed in an appropriate way.

### *The New Exchange Rate System*

Based on the above considerations, a proper modeling of international commodity markets now requires that exchange rates be taken into account. Some background on that system is therefore essential for proper modeling. What we now have is very much of a mixed system, with a considerable — although declining — degree of government management of the float. Both of these factors complicate the modeling of the exchange markets.

The mixed nature of the system is reflected in the tendency to bloc floating. Individual countries tie their currency to certain key currencies such as the U.S. dollar, the French franc, or the British pound sterling. To the extent that these key currencies float against each other and against other currencies, the currencies tied to them also float. Hence, in 1978 some 80 percent of trade took place across markets in which floating exchange rates prevailed, even though only 38 of the 133 member countries of the IMF, plus Switzerland as a nonmember country, had freely floating exchange rates. The important point, of course, is that such bloc floating gives rise to important third-country exchange rate effects that

generally do not receive the attention they deserve in either the modeling or the analysis of commodity markets.

### *An Increasingly Well-Integrated International Capital Market*

The third significant development affecting agricultural commodity markets is the growth and increased integration of the international capital markets. This development has been little noted by agricultural economists. But as noted above, its implications for modeling commodity markets is quite great. The international capital markets have become important links among individual economies and a means of transmitting the effects of government policies from one country to another. They have also become an important source of shocks to individual commodity markets.

In the immediate post-World War II period, international capital markets were almost nonexistent. Such capital flows as there were were either on a government-to-government basis, often on **concessional** terms, or they were surreptitious shifts of funds to circumvent regulations or to flee oppressive governments.

As confidence grew in the international system that emerged in the aftermath of World War II, and as trade grew at a rate requiring increased amounts of liquidity, an international credit system gradually evolved, with an ever larger participation of the private and public banking systems. Perhaps the most significant institutional innovation was the emergence of a Eurodollar market. This latter transformed itself into a more broadly based Eurocurrency market. An Asian currency market has emerged more recently. The volume of credit and capital that flows in these markets is now huge — the volume of credit outstanding in the Eurocurrency market now approaching **\$1 trillion** alone. Less-developed countries and centrally planned economies alike make use of it, and capital flows on concessional terms have dwindled to insignificance in a relative sense.

Perhaps the most significant aspect of this market is the lack of government regulation and distortion. The Eurocurrency market, for example, is almost completely beyond the pale of government regulation, despite the tight control exercised by governments whose currencies are represented in these markets over both their domestic capital and credit markets and over their respective commodity markets. The lack of government regulation suggests that these markets may be relatively efficient. **Harberger's** imaginative



attempt to look at the efficiency of this market<sup>15</sup> suggests that it may, in fact, be relatively efficient. This has obvious importance for commodity markets, for the modeling of commodity markets, and for government commodity policy. We will return to these factors below.

### Modeling Exchange Rate *Effects*<sup>16</sup>

Perhaps the first published discussion of how to model exchange rate effects on U.S. agriculture appeared with the exchange between Vellianitis-Fidas and myself.<sup>17</sup> This discussion focused on whether it was the domestic demand and supply elasticities that were relevant or the import demand and export supply elasticities. V-F emphasized the former; I stressed the latter. The appropriateness of the econometric procedures used by V-F to test for the effects of changes in the exchange rate also came under review.

A year after this exchange, Kost<sup>18</sup> also took exception to the view that "the exchange rate is an important structural variable" and suggested that such conclusions were "at their worst, wrong, or at their best, quite misleading as to the magnitude of the effects we can expect in agriculture when the exchange rate changes." To support his contention, Kost introduced a two-country, one-commodity, free-trade equilibrium model. Using graphical analysis to "derive" excess supply and import demand curves for a "trade sector" from the underlying supply and demand curves in each country, Kost introduced devaluation by the exporting country as a rescaling of the price axis of the importing country. Subsequent supply and demand adjustments in the importing country were then assumed to be reflected in the trade sector in a rightward shift in import demand along an unchanged excess supply curve.

Kost concluded from his graphs that "the apparent shift in the supply and demand curves in the importing country, and the result-

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15. A. C. Harberger, "Perspectives in Capital and Technology in Less Developed Countries," M. J. Artis and A. R. Nobay, ed., *Contemporary Economic Analysis*, London, 1978.

16 This section draws on Schuh, Hodges, and Orden

17 Vellianitis-Fidas, "The Exchange Rate and U.S. Agriculture. Comment," *American Journal of Agricultural Economics* 57(November 1975). pp. 691-95, and G. Edward Schuh, "The Exchange Rate and Agriculture Reply," *American Journal of Agricultural Economics* 57(November 1975). pp. 696-700.

18 William Kost, "Effects of an Exchange Rate Change on Agricultural Trade," *American Economics Research* 28(3) (July 1975), pp. 99-106.

ing shift in the import demand curve, each equal the percentage change in the exchange rate," which he argued was an upper ceiling on the price change (assuming excess supply is perfectly inelastic) or the quantity change (assuming excess supply is perfectly elastic) that can occur in response to devaluation.

In an appendix, Kost derived expressions for elasticity of excess supply in the exporting country,  $E_{ES} = \frac{e_S Q_S - e_D Q_D}{Q_S - Q_D}$ , and of im-

port demand,  $E_{ID} = \frac{e_D Q_D - e_S Q_S}{Q_D - Q_S}$ , where RHS elasticities and

quantities in each expression refer to the exporting and importing country's economy. Then, observing that the elasticity of both supply and demand is low for agricultural products in the U.S., Kost allowed that within the narrow limits suggested by his model one would expect that devaluation would have a greater price impact than quantity impact on agricultural goods. But, to reemphasize the point, Kost's principal conclusion was that the proportional increase in price or quantity of traded goods in response to a devaluation was restricted to being less than or equal to the percent of devaluation. Further, Kost argued that trade restrictions such as the EEC variable levy would insulate importers' domestic markets from changes in world prices and hence reduce the shift of the import demand curve, further lessening possible trade impacts of devaluation. Kost concluded, "In summary, we can only expect a small impact on agricultural trade as a result of a change in exchange rate."

Despite the apparent weaknesses in these arguments, Kost's paper captures much of the essence of later discussions on modeling the effects of the exchange rate. One of the first to respond to Kost was **Bredahl**.<sup>19</sup> In particular, Bredahl argued that within the two-country, one-good model, there was no basis for concluding that the proportional change in quantity traded was constrained by the percentage devaluation. Again using linear supply and demand curves, Bredahl developed expressions for the elasticity of exporters price and quantity traded with respect to exchange rate:

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19. Maury E. Bredahl, "Effects of Currency Adjustments Given Free Trade, Trade Restrictions, and Cross-Commodity Effects," Department of Agricultural and Applied Economics, Staff Paper p. 76-35, University of Minnesota, November 1976.

$$E_{p,r} = - \frac{1}{1 - E_{ES}} \frac{1}{E_{ED}}$$

$$E_{Q,r} = (E_{p,r}) (E_{ES})$$

Clearly, in his model,  $-1 \leq E_{p,r} \leq 0$ , constraining change in price in response to devaluation, as Kost suggested. But  $E_{Q,r}$  has no a priori lower bound. Noting that  $E_{ES}$  may be greater than one even if exporters' domestic supply and demand are inelastic, **Bredahl** rejected Kost's earlier result.

An obvious empirical issue in modeling exchange rate effects is the size of the foreign import demand for U.S. agricultural exports. **Tweeten**<sup>20</sup> had derived an expression for this foreign excess demand as follows:

$$E_{ED} = \sum_i \left[ e_{Di} e_{pi} \frac{Q_{Di}}{Q_x} - e_{Si} e_{pi} \frac{Q_{Si}}{Q_x} \right]$$

where  $i = 1 \dots n$  is a country index,  $e_{Di}$ ,  $e_{Si}$ ,  $Q_{Di}$  and  $Q_{Si}$  are elasticities of demand and supply and quantities demanded and supplied in the  $i$ th country, and  $Q_x$  is the quantity of U.S. exports. The term  $e_{pi}$  (referred to as the "elasticity of price transmission") measures the responsiveness of price in country  $i$  to changes in the U.S. price. Based on this expression and assuming free world trade, **Tweeten** initially estimated  $E_{ED} = 15.9$ , but he reasoned that trade restrictions reduced this value significantly to something on the order of  $E_{ED} = 6.3$ . This estimate has been widely used by those who argue that changes in the exchange rate have had a significant effect on the agricultural sector.

**Johnson**<sup>21</sup> disagreed with **Tweeten's** algebraic expression for  $E_{ED}$  but arrived at a similar estimate by his own techniques. This exchange points up an important problem that has arisen in the empirical work. Estimates of the export supply elasticity and the import demand elasticity that are built up from direct estimates of the more

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20. Luther G. **Tweeten**, "The Demand for U.S. Farm Output," *Food Research Institute Studies*, 7(1967), pp. 343-59.

21. Paul R. **Johnson**, "The Elasticity of Foreign Demand for U.S. Agricultural Products," *American Journal of Agricultural Economics* 59(1977), pp. 735-36.

basic elasticities suggest rather large excess demand and supply elasticities. However, direct estimates of the responsiveness of trade to changes in prices generally find a very low response.<sup>22</sup> At least part of this disparity is due to the identification problem in dealing with the trade sector. For the most part, models which make direct estimates of the elasticities are quite simple and probably not capable of identifying the underlying parameters.

Bredahl, Meyers, and Collins<sup>21</sup> returned to the controversy to assert that this discrepancy between derived and directly estimated elasticities is explained by restrictions on trade that insulate important agricultural markets so that the  $e_{pi}$  approaches zero in many cases. With  $e_{pi} = 0$ , a change in world price or a currency devaluation by an exporter would have no effect on domestic markets in the  $i$ -th country, and no effect on  $E_{ED}$ . After reviewing government policies of major importers of U.S. corn, sorghum, wheat, soybeans, and cotton', Bredahl, Meyers, and Collins assign an implied  $e_{pi}$  in each case. Elasticities of excess demand calculated on the basis of these  $e_{pi}$  ranged from  $-.47$  for soybeans to  $-2.36$  for sorghum, compared to  $-1.12$  for soybeans to  $-5.50$  for wheat under the assumption that  $e_{pi} = 1$  for all countries and all goods. The authors concluded that the estimates of the elasticity of excess demand put forth by Tweeten and Johnson are simply not "in line with what is known about a world with insulated agricultural markets."

Applied to the argument over the expected consequence of a change in exchange rates, the results of Bredahl, Meyers, and Collins underscore the variety of effects among countries and commodities that might be expected in response to a specific change in the exchange rate.

For all its utility in clarifying the relationships among countries, the two-country, one-commodity model examined by Kost and Bredahl, and often utilized implicitly in empirical work, is still a rather simple and perhaps excessively abstract representation of the real world. Chamber and Just<sup>24</sup> suggest a more complete two-country

22. For example, see Robert N. Stern, *Price Elasticities in International Trade: A Compilation and Annotated Bibliography*, London: MacMillan Press, 1978.

23. Maury E. Bredahl, William H. Meyers, and Keith J. Collins, "The Elasticity of Foreign Demand for U.S. Agricultural Products: The Importance of the Price Transmission Elasticity," *American Journal of Agricultural Economics* 61(1), February 1979, pp. 58-63.

24. Robert G. Chambers and Richard E. Just, "A Critique of Exchange Rate Treatment in Agricultural Trade Models," *American Journal of Agricultural Economics* 61(2) (May 1979), pp. 249-257.

model in which excess demand for goods in the importing country is a function of a commodity price and income expressed in the importer's currency, while excess supply of goods from the exporting country is a function of the same  $n$  goods expressed in exporter's currency. Following **Bredahl's** approach (totally differentiating demand and supply equations at equilibrium), Chambers and Just derive an expression for the proportional change in exporter currency price of the  $i$ -th good ( $p_i$ ) resulting from a given percentage change in the exchange rate:

$$\tilde{E}_{p_i,r} = E_{p_i,r} - \frac{\sum_{\substack{j=1 \\ j \neq i}}^n E_{p_j,r} \left[ e_{D_j} - e_{S_j} \right] - \left[ w_i + e_{D_i} \right]}{e_{D_i} + \sum_{\substack{j=1 \\ j \neq i}}^n e_{S_j}}$$

where  $E_{p_i,r}$  = the elasticity of price  $p_i$  with respect to the exchange rate (now interpreted as a partial elasticity)

$E_{p_j,r}$  = the elasticity of the  $j$ -th cross price (in exporter's currency) with respect to the exchange rate

$e_{D_j}$  = elasticity of excess demand (importer's for the  $j$ -th good

$e_{S_j}$  = elasticity of excess supply (exporter's for the  $j$ -th good

$w_i$  = income elasticity of excess demand for good  $i$ .

Under an assumption of homogeneity,  $e_{D_i} \div \sum e_{S_j} = e_{D_i} \cdot e_{S_i} < 0$ , so the numerator in the expression  $\tilde{E}_{p_i,r}$  is negative. While lacking the elegance of ease of interpretation, in case of the denominator being negative,  $\tilde{E}_{p_i,r}$  is larger (in absolute value) than  $E_{p_i,r}$ . In particular, Chambers and Just concluded that there is no basis to claim a priori that  $-1 \leq \tilde{E}_{p_i,r} \leq 0$ . By implication, earlier empirical studies specifying demand or supply as a function of own good price are thus in their view not capable of measuring the true effects of changes in the exchange rate on commodity prices. In contrast, Chambers and Just suggest that if a simple model is to be used the exchange rate should be included as a separate regressor. They cite

studies of wheat exports by Fletcher, Just, and Schmitz<sup>25</sup> and of corn exports by Meelke and de Gorter<sup>26</sup> in which exchange rates were found to be significant variables explaining U.S. exports.

Chambers' and Just's results imply that exchange rate effects can only be measured in a general equilibrium context. This point was underscored in an exchange between Grennes, Johnson, and Thursby (GJT)<sup>27</sup> and Chambers and Just<sup>28</sup> in the AJAE. An earlier study by GJT (AJAE, 1977)<sup>29</sup> had distinguished wheat by country of origin, and demand equations had included all wheat prices, and, exogenously, prices of other grains. Chambers and Just had criticized the GJT model as equivalent to the simpler one-good model developed by Bredahl. GJT responded that "failure to incorporate price of related products" was not a shortcoming of their approach. Chambers' and Just's reply was to emphasize that "the exchange rate must be given greater flexibility in a trade model than can be allowed by tying its effects to those of wheat and possibly corn prices, or indeed, to any small group of commodities."

An important strength of the 1977 paper by Grennes, Johnson, and Thursby was that they did use a model which permitted cross-elasticity effects among countries. However, rather than to attempt estimates of these cross-elasticities, they assumed them to be a rather low 0.3. Unfortunately, they then concluded that the effect of a change in the exchange rate would be quite low, apparently not aware that they had assumed a low effect by the assumption they had made. At a minimum, sensitivity analysis would have been appropriate.

In reviewing this literature, it appears in hindsight that the re-

25. Stanley M. Fletcher, Richard E. Just, and Andrew Schmitz, "The Impact of Exchange Rates and Other Factors on North American Wheat Export Demand," *World Food Crisis: Issues and Policy Alternatives*, Gordon C. Rausser, ed., Amsterdam: North Holland Publishing Co.

26. Karl D. Meelke and Hendry de Gorter, "A Quarterly Econometric Model of the North American Feed Grain Industry," paper presented to Economics Branch, Agriculture Canada, Ottawa, April 27, 1977.

27. Thomas Grennes, Paul R. Johnson, and Marie Thursby, "A Critique of Exchange Rate Treatment in Agricultural Trade Models: Comment," *American Journal of Agricultural Economics* (May 1980), pp. 249-51.

28. Robert G. Chambers and Richard E. Just, "A Critique of Exchange Rate Treatment in Agricultural Trade Models: Reply," *American Journal of Agricultural Economics* (May 1980), pp. 255-59.

29. Thomas Grennes, Paul R. Johnson, and Marie Thursby, "Devaluation, Foreign Controls, and Domestic Wheat Prices," *American Journal of Agricultural Economics* 59 (1977), pp. 619-27.

search has perhaps been cast in too narrow a context. This narrowness is probably due to the focus upon changes in the quantity demanded that are expected to result from changes in the exchange rate. As Kost showed, the change in exchange rate is a shift phenomenon whereby the excess demand curve moves along the excess supply curve in response to exporter changes in exchange rates. Therefore, it is not enough to say that prices change along a curve (FJT, BMC); rather, one must say that prices change in response to shifts in excess demand and excess supply curves. Furthermore, these shifts in excess demand and excess supply reflect a number of important and often subtle variables which are affected by changes in the exchange rate.

Bredahl's 1976 paper reveals the importance of including excess supply in the analysis. Chambers and Just (CJ) directed the discussion away from own-price relationships and included cross-price effects in their excess supply and excess demand functions. However, the models still remain incomplete. While Chambers and Just included the cross-price effects, they neglected to focus upon input prices as important shifters of domestic supply and the effect changes in the exchange rate would have on these prices. Finally, it is important to consider the cross-country effects of exchange rate changes. The excess demand for U.S. agricultural products is composed of demand and supply in both importing and exporting countries. Therefore, substitution among exporters can occur and should be considered in a multi-country model. Greenshields considered this factor in determining Japanese demand for U.S. grain and soybean exports where a U.S. devaluation caused Japan to substitute U.S. wheat for Australian wheat. Considering these factors, the following specification for a trade model equilibrium would be necessary:

Rest-of-World Excess Demand = Country j Excess Supply

$$\sum_{i=1}^n \left[ D_i(eP_j, P_i, M) - S_i(eP_j, P_i) \right] = S_j(P_j r_j) - D_j(P_j M)$$

$D_i$  = Foreign demand in country i

$S_i$  = Foreign supply in country i

$P_j$  = Price vector of related goods in demand and supply which are traded

$P_i$  = Price vector of non-traded related goods in demand and supply in country i

$r_j$  = Intermediate good and input prices in country  $j$ 's supply function

$M$  = Income

$e$  = Exchange rate

The large number of shift variables discussed above presents econometric problems, but their inclusion prevents biases resulting from incorrect specification. The justification for their inclusion lies in the fact that the exchange rate is pervasive and directly affects all traded goods.

By now, the suitability of a general equilibrium treatment of this question should be apparent. The inclusion of input prices, intermediate goods, substitute goods and competitive goods in the excess supply function suggests the need for a multi-sector general equilibrium treatment. A more fundamental rationale for employing a general equilibrium approach is its usefulness in measuring changes in the terms of trade. The discussion to this date has centered upon the impact of exchange rate changes on absolute agricultural prices. This by itself is a poor measure of agriculture's gain or loss from exchange rate changes when changes in other sector prices, non-traded agricultural good prices or the general price level may actually turn the domestic terms of trade against agriculture.

A simple four-sector matrix suggests how different sectoral prices can be compared.

	<u>Traded</u>	<u>Non-traded</u>
Agriculture	$P_{at}$	$P_{an}$
Manufactured	$P_{mt}$	$P_{mn}$

A devaluation will increase  $P_{at}$  and  $P_{mt}$ , but the net impact upon agriculture remains ambiguous due to impacts upon  $P_{an}$  and  $P_{mn}$  and the secondary effects transmitted through input prices, intermediate goods, incomes, and demand shifts.

The literature contains many treatments of inter-sectoral linkages such as Dornbusch (1973), Mundell (1961), and McKinnon (1963).<sup>30</sup> Dornbusch applied a monetary approach to the theory of

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30 R. Dornbusch, "Devaluation Money and Non-traded Goods." *American Economic Review* 63 (December 1973), pp. 871-80. R. A. Mundell, "A Theory of Optimum Currency Areas," *American Economic Review* 51 (September 1961), pp. 657-65, and R. I. McKinnon, "Optimum Currency Areas." *American Economic Review* 52 (September 1963), pp. 717-725.



devaluation and concluded that non-traded goods in the devaluing country will suffer a price fall. This is largely due to the shift in the hoarding schedule caused by the devaluation.

### *The Money Supply and Commodity Prices*

Shei<sup>31</sup> made an attempt to estimate the effects of the 1971 and 1973 devaluations of the dollar in a general equilibrium framework. He specified a general equilibrium econometric model of the U.S. economy and estimated the parameters for this model. The model treated both the real and monetary sectors, with sufficient disaggregation that the important simultaneities of the agricultural sector with the rest of the economy could be reflected.

The estimates of the structural equations were used to simulate the effects of a unilateral devaluation of the U.S. dollar, a once-and-for-all increase in the stock supply of the domestic component of the monetary base, and an exogenous shock to the system such as a crop failure in the rest of the world which shifts the export demand for U.S. crops upward. The simulations were based on observed levels of each of these changes in 1973.

These experiments suggested that dollar devaluations had a significant effect on U.S. crop exports and domestic and export prices in the early 1970s. However, the observed monetary expansion explained a larger part of the price changes than the dollar devaluation. Simulations of the shifting export demand for U.S. crops as a result of the 1972 crop failure in the rest of the world explained a relatively small amount of the observed changes in the early 1970s.

Barnett<sup>32</sup> followed up on Shei's work by examining the effects of both domestic and international liquidity on agricultural prices. His interests were in particular to determine whether international liquidity had a significant effect on the prices of commodities traded internationally, and whether these monetary variables have had an effect on the ratio of agricultural prices to prices in the rest of the economy.

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31. Shun-YI Shei, "The Exchange Rate and U.S. Agricultural Product Markets: A General Equilibrium Approach." unpublished Ph.D. thesis, Purdue University, 1978

32. Richard C. Barnett, "The Relationship Between Domestic and International Liquidity and Nominal Agricultural Prices: A Time Series Analysis," unpublished master's thesis, Purdue University, December 1980.

The empirical evidence suggested that both domestic and international monetary expansion had a significant effect on domestic agricultural and food prices in the United States and in the world in general during the 1970s. Monetary expansion also appears to have had an influence on the observed change in the ratio of U.S. agricultural prices to nonagricultural prices during this period. His empirical evidence also suggested that money is causal to agricultural prices, with little or no feedback.

### **Some Suggestions for Future Modeling Efforts**

This review of the monetary aspects of agricultural trade suggests that future modeling efforts have to deal with models that are a great deal more comprehensive than those used in the past. Treating the trade sector as a simple extension of the domestic agricultural sector as a general approach is not likely to have a very high payoff.

Instead, commodity markets need to be linked directly to monetary aggregates, both domestic and international. This is a tall order, especially in light of the more general effects of monetary policy and capital markets. One need only recognize that with privately held stocks, changes in the money markets have significant effects on the holding of stocks and in turn on commodity prices. The growing deregulation of the U.S. credit and banking system makes the entire agricultural sector much more sensitive to changes in monetary and fiscal policy. The failure to take account of this in our modeling efforts can only lead to a lack of realism in the models and poor prediction and forecasting models.

Two implications immediately follow from this. The first is that models of the agricultural sector really have to be components of general equilibrium models of the economy. There seems no other route to go, despite our desire for simplicity and for models that can be used in a low cost way.

The second implication is that viewing commodity markets in the traditional context of flows is unsatisfactory. Once one introduces monetary phenomena, one has to view commodity stocks as assets on a par with monetary assets and other capital instruments. The observed shifting of funds back and forth between commodity and capital markets is just too great to be ignored any longer.

In modeling exchange rate phenomena, greater attention needs to be given to both cross-country effects among exporters and to the supply response in import-demanding countries. The importance of

cross-country effects was referred to above. The supply response in import-demanding countries was discussed only implicitly. The point, of course, is that exchange rate realignments that are passed on to domestic economies affect the quantity supplied as well as the quantity demanded.

To understand the U.S. agricultural trade sector a great deal more effort needs to be devoted to understanding the agricultural sector of other countries. Models designed to do this will have to be structured as comprehensively and in as sophisticated a way as those for the U.S. agricultural sector. That means that available sector models for the most part will only be starting points. They need to be cast in general equilibrium models of their respective economics.

Much more attention also needs to be given to trade distortions and government interventions. As Thompson notes, many of the trade models have had an exaggerated free market bias to them. Government intervention in trade is significant and pervasive. It needs to be taken into account in developing sound models for policy analysis.

Greater attention also needs to be given to the role of governments in commodity markets. The evidence we have on the responsiveness of policy to changing economic conditions also suggests that government can no longer be treated as exogenous, but must be treated as endogenous to the economy. Moreover, it isn't just the **U.S.** government that needs to be understood; the behavior of governments in other countries is equally as important.

Finally, a great deal more effort needs to be directed to developing appropriate data series and information on government policy and interventions. Moreover, this information needs to be organized and pooled in such a way that it can be made available to modelers and trade researchers.

### **Concluding Comments**

Most models of the U.S. agricultural sector have been specified in a partial equilibrium context and have had fairly weak and inadequately specified linkages to the rest of the economy. The internationalization of **U.S.** agriculture, together with the shift to a floating exchange rate regime and the emergence of a well-integrated international capital market, cause the continued use of such an approach to be of dubious value. U.S. agriculture can only be understood in the context of the world agricultural economy of

which it is a part. Moreover, world agriculture can only be understood in the context of a general equilibrium model that takes account of monetary and fiscal phenomena. It would be nice if the world were simpler. But it really isn't.

## Commentary

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*D .Gale Johnson*

My good friend Ed Schuh has left me little to say. He has presented a first rate summary of the existing state of the art of agricultural trade modeling. He then goes on to indicate the major issues that are left unresolved or where very great difficulties exist in implementing what we know should be implemented. He concludes with some useful and important suggestions for guiding future modeling efforts.

### **Money Matters**

I am in agreement with Schuh in giving emphasis to the need for greater attention to monetary phenomena in modeling international trade. As Schuh notes, with floating exchange rates, monetary policy has a number of effects, depending of course upon the nature of that policy. A tight money policy results in capital inflows and a rise in the value of the dollar, while an easy money policy results in capital outflows and a decline in the value of the dollar. Schuh then argues that a tight money policy is responsible for a decline in the competitiveness of the export sector. This is the case in the short run, but it is not so obvious that long-run effects of a tight money policy on exports will be so adverse. **If** carried on long **enough**, the reduction in the rate of inflation and the increase in capital inflow could result in changes that will have a positive effect upon the export sector. These might include increased investment in research and development, greater investments in the farm supply sector, and improvements in the transport and marketing system for agricultural products. While not directly relevant to agriculture, the experience of both Germany and Switzerland during the 1970s indicates that if pursued consistently over an extended period of time, a tight money

policy and a moderate rate of inflation do not inhibit export industries. Trade surpluses were generated in both countries as their exchange rates increased relative to the dollar and almost all other currencies except for the yen. I mention the long-run effects of monetary policies on export sectors to indicate that we cannot move directly from the short-run effects to those that might prevail in the long run. I do not mean to imply that Schuh so stated; I do want to indicate that the long-run effects of monetary policies may be both different and more complex than the short-run effects.

### **Quantities vs. Prices**

I have grave doubts, as does Schuh, about our capacity to project or predict trade flows and about how important it is to project such flows except as reflecting trade policies of either the exporting or importing nations. In fact, I suspect that one of the most important reasons we have had such poor luck in projecting trade flows, either *ex post* or *ex ante*, is to be found in the Grennes, Paul Johnson, and Thursby analysis of price differences among grades and qualities of what is commonly called the same commodity, such as wheat, rice, or cotton. What may actually be of more interest than trade flows of wheat may be understanding why the relative prices of different grades of wheat vary from time to time. For example, in 1977 the average prices of the following types and grades of wheat in Rotterdam were: U.S. No. 2 Hard Winter, 13.5 percent protein, \$113 per (metric) ton; U.S. No. 2 Dark Northern Spring, ordinary protein, \$126 per ton; and No. 1 Canadian Western Red Spring, 13½ percent protein, \$133. In March 1979 the prices per ton were, respectively, \$165, \$164, and \$164. Trade flows or market shares, as measured in terms of quantity, were certainly different between the two time periods. But I'm not sure what relevance trade flows, as such, have to understanding the incomes of farmers, our trade balances, or the size of stocks at any given time.

Trade flows can be affected by governmental policies, as we saw during the 1960s when the U.S. and Canada were willing to hold large stocks of wheat, while Australia and Argentina were quite willing to accept the price stabilizing effects of those stocks and to sell whatever grain was available. Clearly when trade flows are not determined by competition, it may be possible to derive meaningful information from the trade flows. It may also be possible to use specific departures from competition in estimating trade flows.

Though even in this case I believe the price effects are more important than the matrix of commodity movements.

### **Governmental Policies**

What I shall now say is not intended as criticism of trade modeling but has the purpose of indicating how complex it is to derive empirically valid or relevant models when governmental interventions are involved. Perhaps the most striking cases in recent times were the policy changes that occurred in the Soviet Union in 1963 and again in 1971, the first in response to a poor crop that threatened the human consumption of grain and the second in response to consumer demand for livestock products. The earlier change resulted in grain imports rather than imposing the potential threat of hunger or famine, as had occurred in 1947, in the early 1930s, and in the early 1920s. A poor grain crop in 1965 resulted in the same response, namely significant grain imports. In both 1963 and 1965 the grain shortfalls resulted in reductions in the animal feed supply and subsequent reductions in the livestock herd and meat availability.

In 1970 or 1971 a decision was made to significantly increase the amount of grain for feed even at the expense of a reduction in grain exports or at the cost of actual grain imports. The decision made in 1970 or 1971 — implementation apparently occurred in 1971 — has had a great impact upon trade flows. Could this change have been anticipated in time to have permitted its effects to be reflected in trade models? I think not, though I hasten to add that this is not the fault of the trade models since the policy change was generally detected only a year or two after the fact. In such areas, the Soviet Union has been able to maintain a monopsonistic advantage through secrecy.

Other policy modifications of major significance may be briefly noted. One was the decision of the Japanese government, made in the late 1950s, to encourage or permit the production of livestock products. This meant a shift away from rice as the principal source of calories. It also meant that during the first half of the 1960s, grain imports increased by 150 percent and then nearly doubled again in the next decade.

The People's Republic of China during the past two decades has made a series of decisions affecting grain imports. One was the decision to import grain in 1961 for the first time since liberation;

grain imports generally increased until the early 1970s and then were sharply reduced by 1975 and 1976, though there is absolutely no evidence that domestic supplies were more adequate in those years than in the years of larger imports. But with generally improving per capita grain production, grain imports more than trebled in 1977 compared to 1976 and more than doubled in the next three years. Could these changes have been predicted *ex ante*? I doubt it.

In this discussion of policy changes that have made a difference, I should not exclude the major U.S. policy changes with respect to price supports and governmentally held stocks that have had a very great influence upon the amount of international trade in several products, in trade flows, in international prices, and in the U.S. role in international trade. The gradual transition from the relatively high price supports of the 1950s to the low price supports by the end of the 1960s, and continuing throughout the 1970s, had enormous consequences. It is possible that the policy changes in the Soviet Union, Japan, and China could not have occurred or been implemented without the change in U.S. farm price and export policy. But it may be equally true to say that the U.S. policies would not have changed if the other policy changes had not occurred.



# Alternative Designs For Policy Models Of The Agricultural Sector

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*Stanley R. Johnson*

## **Introduction**

Policy models are representations of systems formulated for the purpose of anticipating and evaluating outcomes of decisions that influence the functioning of the system. For static models, the decisions are introduced by selecting a set of values for the policy instruments under the control of the decisionmaker. The dynamic counterpart is a decision rule or strategy for determining values of the policy instruments based on external and system outcomes. Outcomes for the system are determined by its structure and the environment in which it functions. This environment can be specified by levels of conditioning factors in static contexts or time dimensioned structures for dynamic models.

The evaluation of policy models is on the surface a simple matter, Criteria ultimately employed are correspondences of system outcomes to those projected on the basis of the policy model. When the correspondence is close, the policy model is given a positive evaluation. The opposite is of course true as there is a lack of correspondence. Clearly, the term "close" requires additional definition. For example, policy models may suggest outcomes of actions which are inconsistent with those observed, not because of the model representation, but because of a failure to correctly anticipate the environment within which the system is functioning. Persistence of such results would, however, reflect on the model representation, indicating that the part of the environment responsible for the difficulty should be endogenized. Models resting upon environmental assumptions that can not be accurately projected or verified are of little value as decision-making aids. Thus, measures of predictive

accuracy must take into account the uncertainty about the environment faced by the decisionmaker.

The problem of designing policy models as decision aids is the subject of the present paper. Three aspects of model design are of concern. The appropriate, or in some sense, optimal model design is first a function of the decisions or actions that the output is to support. That is, models should be designed to include as outcomes those variables used by the decisionmaker to evaluate the performance of the system. Also, they must encompass the policy instruments under the control of the decisionmaker. That is, the models must provide a structure whereby the settings of the instruments can be related to the values of the outcome or performance variables.

A second aspect of model design has been anticipated by the comments on evaluation. It is the scope of the model. Models must incorporate sufficient structure to permit the analysis of decision rules within a construct that has predictive integrity. Many of the statistical models employed to support policy analysis for the agricultural sector have not been sensitive to this question of scope. The performance of the models as decision aids has broken down because, although resolution within the models has been good, their predictions depended upon environmental variables that could both be accurately projected. In short, the scope of the models has been chosen without carefully evaluating the potential for implementing decisions based on their outcomes.

The final aspect of model design concerns the fact that models are approximations of systems. This must be if models are to contribute to decision making by pointing to key effects, responses or behaviors. Since models are approximations, means should be developed for adapting them to the systems, i.e., keeping the models current. This adapting or tuning of models is thus an important aspect of design, albeit one that has not received adequate attention by economists. Concerns with scope and the inclusion of relevant policy instruments and performance variables argue for larger and more comprehensive models. Limitations imposed by costs of information, computation and understanding of the structures favor smaller and less extensive models. The idea of models as approximations can be seen as an approach to achieving a workable compromise in this framework. Interestingly, the better the approximation the more successful a model can be at offsetting these two concerns.

Models should provide local, adaptable approximations to the

systems they are designed to represent. It follows that the major design issues for model development are: (1) structuring and specializing the approximations and (2) adjusting the approximations so they are localized to represent the systems as currently positioned for policy making. The advantage of viewing modeling in this framework is the simplicity it provides for model specification and specialization or estimation.

The paper begins with a review of the experiences of economists in developing and applying policy models. Although brief, the review serves to identify prominent themes in the evolution of model design and to tie these themes to the evaluation of designs in an approximation framework. These themes receive a more synthesized treatment in the subsequent section on promising developments in model design. The next two sections address the major design issues of the paper, approximate models and procedures for making these approximations appropriately local. A final section raises the evaluation and design issues again but within a context emphasizing the approximate nature of models.

### **Aggregate Models, Their Changing Structures and Designs**

The record of economics in developing policy models for decision making purposes at the sector and more aggregate levels is anything but distinguished. A number of reviews of the performance of various sector and economy-wide models have come to this conclusion through one avenue or another, e.g., Cromarty and Myers (1975), Crowder (1972), Cooper (1974), Fair (1981), Fox (1973), Haitovsky and Wallace (1972), Hendry (1980), Lucas (1976), Patinkin (1976), Popkin (1975), Rausser and Just (1980) and Tweeten (1975). In general, these and other evaluative studies indicate that performances of economic models have not met the claims of their architects or the anticipations of policy makers. It has been suggested that this failure is due to one or more of the following: insufficient theory, inadequate supply side representations and linkages to the economy at large, failure to update, structural change, unfortunate choices of underlying theory, and poor implementation of the results in policy contexts. Whatever the case, it is clear that economic models are not having a large impact on sector and national level policy decisions. Moreover, not many economists would suggest in good conscience that they should. This is a particularly unfortunate state of affairs in view of the substantial

investments in modeling technology, the computer capability for developing and specializing models, and model specifications.

This section reviews model design with respect to four criteria. The purpose is to demonstrate how economists attempting to adapt poorly performing models for use in policy contexts have evolved different methods and practices. These methods and practices are important for more general questions in model design. By tracing their evolution, common themes will become apparent. These themes point to increased recognition that if policy models are to be useful to decisionmakers, designs that reflect the approximate nature of models will have to come to play more directly.

### **Specification**

Model specifications are, of course, motivated by the policy questions at hand as well as hypotheses or theories about the functioning of the system to which the policy is directed. The discussion of the evolution of model specifications and modeling methods begins with the observations by Koopmans (1947) on the measurement without theory. According to Koopmans, measurement of economic relationships is ultimately dependent upon untestable hypotheses about the system. Moreover, he observed that measurements can not be useful unless the untestable propositions on which they depend are specified so that those using the results will understand their underlying restrictions.

Based on this argument and associated statistical developments, simultaneous equations estimation for economic policy models became popular in the 1950s. A number of advances in method followed from this emphasis on model specification and estimation. These related to appropriate estimators for systems of simultaneous equations and useful results on the applicability of such specifications for capturing causality as hypothesized by the underlying economic theory given the sampling time frames available in the secondary data (Judge, 1977).

Parallel to these developments were advances by agricultural economists and economists which emanated from an input-output conception of the economy (Heady and Egbert, 1959, Heady and Egbert, 1964, Leontief, 1971). These were called normative models because of the optimization rules implicit in their specification and application and focused more heavily on the production function for the economy or sector than on demand side representations. That is,

the model specifications emphasized production functions and decisions made around these technical relationships as important for the functioning of economic systems. This was in contrast to market equilibrium models specified at the sector level (e.g., Hildreth and Jarrett, 1955) and the prevailing Keynesian theories at more aggregated levels. For these models the simultaneous equations approach had a major impact.

By the late 1960s, the success record of market equilibrium and input-output models in forecasting and policy analysis had begun to raise important questions. Additionally, more pragmatic approaches to model development had emerged under the general rubric of systems analysis and simulation (Johnson and Rausser, 1977). The more *ad hoc*, unstructured systems approaches to model development were troublesome for those familiar with more traditional modeling technologies. They were extremely pragmatic. Searching and pretesting for specifications and synthesizing representations of systems were recommended as strategies for model development (Forrester, 1961, Manetsch, et al., 1971).

This adaptive systems approach for model specification forced important adjustments in traditional views on model building. Systems analysis and simulation brought a more pragmatic treatment of economic phenomena and a clearer recognition of the limitations of aggregate model specifications rationalized largely on microeconomic theory. The current more comprehensive and approximate model specifications emerged at least in part as a response by economic modelers to the systems and simulation methods.

Presently, the modeling technology for specifying representations for sector and economy-wide policy analysis is more eclectic. That is, the models have specifications that tend to incorporate essential features of the three approaches; market equilibrium, input-output, and systems methods. Models are market oriented and simultaneous, reflecting equilibrium price determination. They also include restrictions based on production function concepts or input-output information. Finally, simplified representations, largely motivated by the systems analysis and simulation methods, are widely used in bridging between important performance variables and policy instruments. Thus, the general technology for specifying models and policy analyses involves elements of the three approaches identified as influencing specification conventions over the past thirty years.

### *Statistical Method*

Parameters of econometric models specified for policy purposes are typically estimated using statistics which combine sample and other information sources. Early in the 1950s and 1960s, the information used for estimating these parameters was largely sample data, albeit, generated in nonexperimental contexts (Wold, 1969). Although the limitations for applying classical statistical methods in these data were well known, the parameters for statistical models were routinely estimated using these methods.

The pragmatic approaches to identifying parameters in systems analysis and simulation, along with requirements for estimating large scale aggregate models, have resulted in far more flexible approaches to parameter estimation. Prior restrictions and information from different sources now can be systematically incorporated to produce parameter estimates superior to those based on the sample data alone (Judge, et al., 1980). For example, the Bayesian and mixed estimation methods commonplace in the modern applied work are highly flexible with respect to the types of information that can be accommodated to generate parameter estimates (Zellner, 1971, 1979).

The use of Bayesian and mixed estimation methods and different information sources have also served to encourage the development of more flexible norms for evaluating the parameter quality. These are associated with the biased estimation work that occurred in the 1970s and the attention to consequences of pretest estimation. In the former case, trade offs have been recognized between information somewhat inconsistent with the underlying model hypotheses but still capable of providing useful input on parameter values.

The pretest results are particularly important for applied work and, in fact, can be viewed as a response by the statisticians and econometricians to data intensive modeling methods. These results show that the examination of the sample data as a basis of improving model specifications is a highly questionable practice. Claims about the reliability of the parameter estimates for the models, whether econometric or optimizing in nature, based on pretesting are likely greatly over-optimistic. The widespread use of pretesting and the reporting of statistical results for policy models as if such pretesting had not occurred is perhaps one source of the present disillusionment with economic policy models.

There is great pressure on modelers to develop constructs that can

predict well within the sample data. The theory is, as a rule, not sufficient to support the specification of models that will produce such predictive accuracy. As a consequence, curve fitting occurs; essentially pretesting in the sample data. Then, results are reported as if the parameters were estimated without pretesting. In such circumstances, the information content of models is grossly over-represented by standard statistics; percent of explained variation, standard deviations, and the like.

In summary, there have been important changes in the types of estimation methods employed for policy analysis models. These changes have served to clarify the limitations of approaches which involve data mining. In general, they show in a somewhat different way, the argument Koopmans (1947) made long ago regarding measurement without theory. That is, unless something is assumed about the model, it takes a great deal of sample data to generate results that will produce predictions or policy analysis constructs that have much information content.

### *Model Scope*

Motivations for expanding the scope of policy analysis models already have been reviewed briefly. It is important that models be sufficiently comprehensive to be predictive on the basis of exogenous or environmental variables that in turn can be reliably projected. Also, representations must be of sufficient size to allow performance variables and linkages to policy instruments to be appropriately modeled. This, together with the fact that researchers are inclined and frequently required to develop models for multiple purposes, along with increased computer technology has resulted in models of increasing scope or scale.

Presently, it is not surprising to find models of the agricultural sector and the economy that include hundreds of equations. That is, the economic constructs are developed which generate as endogenous variables hundreds of characteristics for the system or sector under study. Since these large, nonlinear models can be solved and monitored with modern computer software, they are operationally feasible. They are, as well, attractive to decisionmakers because of the array of performance variables on the systems that can be generated.

Questions remain, however, on whether or not these large constructs are sufficiently well understood to be useful in policy anal-

ysis. Also, there are reservations about their forecasting performance. The theory to support large scale models obviously must be developed from multiple and perhaps inconsistent behavioral hypotheses and institutional assumptions. This can lead to pretesting and curve fitting as a means of obtaining "reasonable" structures. The result is models which are impressive in terms of dimension but all too frequently unable to deliver acceptable predictive performance. The prevalence of "wrong signs" in large scale econometric models used in forecasting and policy contexts would be quite surprising to individuals unfamiliar with practices that currently exist in the field.

### *Approximation Methods*

The evolution of approximation methods is the most interesting for the design issues. It is common to make fortuitous choices of sample time periods in estimating parameters for policy models. The result is a model which forecasts well within the sample period and has appropriate signs for the important economic variables. Aggregate annual data are now available for most sector level and economy-wide models from about 1945, constituting a large number of potential sample observations. There is considerable fancy footwork amongst researchers regarding choice of sample period. Frequently, there is little recognition that these choices are essential because their models are approximations. The choice of the sample period is in fact a method of localizing the approximation.

Other more direct methods have been developed for accommodating the evolution of systems are calibration, variational parameters, updating, and disequilibrium modeling. In calibrating, large scale econometric models specifications are adjusted so forecasts are exact for the final sample period. Projections into the future can thus be based on a more "accurate" representation of the system. The model has been localized on the basis of an estimated structure and the most current sample values for the predetermined variables.

Variational parameters were advanced largely as methods for reflecting structural change. That is, when model performance became poor, it was recommended that variational parameters be introduced to accommodate the movement of the model from one regime or structure to another (Rausser, Mundlak, and Johnson, 1981). In this way, the structural change can be endogenized. Unfortunately, the applied experience with variational parameters



gives evidence of the limited basis for advancing hypotheses on structural change. Thus, although variational parameters specifications and estimation methods are attractive from an esoteric statistical viewpoint, their impact in applied modeling work has been limited. If a *priori* information exists to permit the representation of structural change by variational parameters specifications, then it should be included in the original specification.

Updating is a different method of adapting models. Updating methods which came to economic modeling in the 1970s were filtering techniques (Kalman, 1960). These filtering techniques provide for efficient linear updating of model parameters. Unfortunately, they do not give guidance on weighting the more recent sample observations. Only if the new sample information happens to be consistent with the structure for the policy exercise in question, can the updating techniques be useful (Sanchez and Johnson, 1981). Thus, the approach of relocalizing approximations through updating methods borrowed largely from engineering has not brought the often advertised benefits. Updating methods are no more than computationally advantageous ways of obtaining the least squares parameter estimates that result from adding linear stochastic restrictions to the existing sample data.

Finally, the disequilibrium methods in econometrics should be mentioned. These methods are essentially ways of statistically closing among model representations. Several possible regimes are specified. Based on parametric assumptions, the estimation process selects regimes most consistent with the sample data (Richard, 1980). Predictions then can be made using variables assumed to condition the regime as in the case of varying parameters specifications. For localization of models viewed as falling into sets of regimes, these disequilibrium methods can be useful. Of course, availability of the prior information on the regimes is the crucial factor for these methods.

### **Promising Developments in Model Design**

The models presently available for policy analysis for the agricultural sector and for the economy are surprisingly homogeneous in design. With some exceptions, e.g., CARD (Huang, Weisz, and Heady, 1980) and POLYSIM (Ray and Richardson, 1978), the models are econometric simultaneous equations constructs. For example, the USDA (Baumes and Meyer, 1979), Chase, DRI, and

Wharton models of the agricultural sector are nonlinear equation systems, at least in part simultaneous. Similar designs are used for the existing economy-wide models, e.g., Fed-MIT (DeLeeuw and Gamlich, 1968), Wharton (McCarthy, 1972), DRI (Eckstein et al., 1976). These economy-wide models have been reviewed recently for performance by Klein and Burmeister (1975).

Most of these large scale models are estimated with single equation methods that do not reflect simultaneity or mean square error norms. Specifications have evolved from substantial curve fitting and attempts to incorporate poorly rationalized theoretical constructs. Updating methods are *ad hoc* and opportunistic and, in general, not advanced on a strong *a priori* base whether in terms of the model specification or the process generating the disturbances. Finally, the results from the models are difficult to communicate to individuals at policy levels. This is because the structures are complex and simplified representations are not available. Still, however, there are encouraging developments.

### *Model Specification*

Developments in model specification have occurred more as a consequence of applying existing model representations than revelations in the theory. At the aggregate level, theoretical developments of practical import have been rather slow in coming. While there has been important work on micro foundations of macro relations, aggregation problems, available sampling time frames, and problems of approximating the equilibrium conditions at aggregate levels, have made it difficult to incorporate the results into model specifications. Instead, the directions in model specification have occurred more in response feedback from users. That is, users have required that models accommodate expanded sets of policy variables. Also, the forecasts obtained from the models have been under question. These two concerns and the problem of communicating the complex structures have led to interesting changes in model specification.

The first of these changes involves the willingness of the econometric modelers to incorporate judgmental information. Judgmental information systematically obtained and introduced into model specifications and estimation processes is particularly important for guidelines on design. Methods for eliciting judgments on parameters and troublesome variables are well developed (Hampton, 1973,

Hendrickson, 1972, Hogarth, 1975, Savage, 1971, and Winkler, 1967, 1969, 1971). Also, mixed estimation methods can be used to incorporate this information in particular estimation or forecasting contexts (Johnson and Rausser, 1981).

The use of judgmental input is advantageous because the processors which generate this information are typically far more adaptive than those represented in the econometric model. Judgments are influenced by information bases different than the data bases on which estimated models reside. Also, judgmental input is processed with a far more adaptive structures. Thus, in areas where judgmental input is used, more simplified models can be utilized. Model specifications do not have to be stretched beyond the theory to encompass events that can be represented by judgmental input.

A second change in model specification involves rationality. In general, rationality hypotheses can improve the behavioral consistency of the model specifications. Expectations variables are important arguments in many economic relationships. Expectations have been introduced by various methods ranging from lagged relationships to observed data (Nerlove, 1972, Gardner, 1976). The rationality hypothesis states that expectations must be generated from a structure consistent with that implied by the model for the corresponding endogenous variable (Muth, 1961, Simon, 1979).

Methods for incorporating rational expectations in large scale econometric models have received increased attention (Chavas and Johnson, 1981, Grossman, 1977, McCafferty and Driskill, 1980, and Taylor, 1977). The upshot of the developments on expectations incorporation and their existence in nonlinear structures shows that the impact of rationality is highly dependent on the ability to project the environmental or exogenous factors. Thus, through attempts to improve specification, a direct link has been made to the basic conceptual problem of formulating models sufficient in scope that the environmental variables can be accurately projected. Where this is not true, rationality may imply expectations determined by structures are not the same as those implied by the model structure (Chavas and Johnson, 1981). These observations suggest that model specifications should be no more complex than can be justified by the resolution possibilities for the environmental factors.

Still another change with implications for model scope has followed from forecasting problems (Feldstein, 1971, and Fair, 1980). Analyses of the forecasting potential for econometric models have

concentrated on the uncertainty transmitted from the environmental variables. Again, the emphasis is for models sufficient in scope to accommodate the uncertain environmental variables. Unless these uncertain variables can be incorporated, rationality does not require complex specifications (Simon, 1979). This more realistic approach to forecasting performance adds to arguments against unstructured, opportunistic methods for specification and application of econometric models.

Finally, model specification has been influenced by examining the information content of the sample. The implication is to modify model specifications, localizing them so that they do not demand information not available in the sample. The information content of the sample is determined by the implicit experimental design. By examining the implicit experimental design, two important results for model specification can be obtained. First, the viability of the existing structure for accurate prediction, ignoring the uncertainty associated with the environmental variables, can be determined (Sanchez and Johnson, 1981, Guttman, 1971, Kiefer, 1958, MacRae, 1977, Covey-Crump and Silvey, 1970, Silvey, 1969, and Wynn, 1970). By examining the implicit design matrix, the possibility of the specification for generating reliable forecasts or predictions or identifying reliably effects of policy instruments on the performance variables can be determined. Second, decisions can be made on the value of extra-sample information for particular policy situations. This result is especially useful in the case of incorporating judgmental input (Johnson and Rausser, 1981).

To summarize, these developments in aggregate model specification appear to have been triggered largely by more carefully assessing the potential of samples for providing reliable parameter estimates given the model specification. Encouragingly, changes in specification have been prompted by attempts to improve on the behavioral consistency of models. Finally, the forecasting potential of the models has been more realistically examined. It would be fair to say that the full effects of these changes have not been felt. This is especially clear if one sees the model specification process as one obtaining a usefully local approximation.

### *Adjusting Approximations*

As already mentioned, the most common method for adjusting model approximations is through the use of calibration methods.

That is, adjusting the model so that the forecast is accurate within the last sample period and then making projections into the future. Alternative ways of adjusting the approximations are, however, becoming available. Composite forecasting can be seen as a method of adjusting the model approximation (Johnson and Rausser, 1981, Falconer and Sivesind, 1977, Granger and Newbold, 1977, and Bessler and Brandt, 1979). This is observed by considering the econometric model as a fixed parameter construct based on existing sample data and the model from which the alternative forecast is generated, perhaps a judgment or futures market outcome, as one with variable parameters and an adaptive specification. By using the composite forecast, a more adaptive or adjusted approximation is available. That is, the fixed approximation represented by the econometric model is augmented with input from a more adaptive forecasting process.

Approximation methods are also becoming better developed for nonlinear representations. Economists are recognizing that it is necessary to approximate the aggregate, nonlinear models for use of their results in policy analysis and forecasting. Evidence of this is contained in improved results on the identification of impact multipliers for the conditioning variables (e.g., Brissimis and Gill, 1978, Chow, 1975, Fair, 1980, Sowe, 1973). For nonlinear and complex representations, the identification of these effects and the study of their behavior can be viewed as a way of localizing model results. These localized versions of the models can then be moved or altered depending upon the state in which the system is observed. Using these approximation methods, seemingly complex model structures can be represented in a communicable form.

Also, interestingly, these approximation methods and the ways of applying them indicate approaches for developing far more direct local model specifications. That is, one is led to ask, what is the gain that occurs as a result of estimating complex nonlinear models if the information from them must be summarized utilizing local approximations? Could not the local approximation simply be used to represent the system at large? If so, then model designs could be much more flexible; linear approximations localized based upon the observed levels of the environmental variables for the system.

## Approximate Specifications

The results from the above observations on present model specifications and the general framework for model design advanced in this piece, are relatively straightforward. At the aggregate level at least, economists are working with models that are false or approximations to true systems (Leamer, 1978). The theory, given the level of aggregation, can do little more than suggest a causal structure and the arguments of functions. Clearly, the economic theory of aggregate models is, in general, not sufficient to suggest specifications of functional form.

This observation on the richness of the theory relative to the applied problems of modelers has far-reaching implications. Specifically, if we follow through, using linear specifications (in the absence of a basis for more complex ones), the result is approximations that are extremely local. Again, the locality of these approximations has led researchers to engage in curve fitting as a basis for generalizing the representations. This curve fitting mines the sample data and, in most cases, produces specifications that are highly specialized to the nonexperimentally generated design matrix. If the problem of localization for structures is deferred to other methods or approaches and the economic models are specified conservatively *vis-a-vis* the theory, relatively uncomplicated representations are implied.

The recommendation, therefore, is that the models not be aggressive in theoretical content. Only well developed theoretical and empirical results should be introduced in the specification process. The concern for evaluating specifications should be more with respect to signs than with prediction. Predictability concerns can be left to the localizing mechanisms. In short, an understandable and limited local approximation can be specified. It is not necessary to complicate this specification to improve the predictive performance of the model. The predictive performance as the model moves from one locality to another can be assured by proper use of localization methods.

Implications of these observations are for models with less demanding constructs. These models are more easily understood by policymakers and not unimportantly, by the researchers themselves. With more simple models it is possible to trace effects of perverse signs and to use specifications that can generate quantitative information that is more consistent with the theory. Lastly, the substance

of the theory supporting the specification can be communicated to those who utilize the results.

## Localization

Suppose now that an approximate model has been specified. This model is conservative with respect to the theory and will likely produce estimates which are not impressive in terms of traditional validation schemes and the ability to predict outside the sample data. The localization or adaptive schemes for making the model predictable are generated outside the economic model specification. Two approaches for making these adjustments are discussed in this section. One involves the combining of models. The other is a direct reestimation scheme.

### *Combining the Models*

When estimating structural equations in econometric models, a number of diagnostics are usually applied. One purpose of these diagnostics is to test for patterns in residuals. Where patterns are evident and can be reflected using autoregressive moving average processes, these patterns are frequently incorporated in estimating the structural equations. The estimated structure is then solved for a reduced form and forecasts are made.

The problem with this process is that autoregressive moving averages in the residuals also can occur across equations. It is well-known that the forecasts from models with such autoregressive specifications should include the information on the process generating the residuals. Thus, the forecasts from econometric models where autoregressive moving averages are suspected or detected in the residuals should contain the information from this *a priori* specification as well as from the structure.

There are two methods for incorporating this information on the process generating the residuals. These are illustrated in Figure 1. The first is structural and by comparison generates a restricted reduced form specifications for the residual processes. The economic structure is specified and estimated. At the same time, a multivariate process is estimated for the residuals on the structural equations. Then a solution for the reduced form is made with the implications of the *a priori*; perhaps exclusion restrictions, imposed on the reduced form residual process.

A second method is simply to estimate the structure, perhaps with

the incorporation of important autoregressive moving average effects for specific equations. Then a solution is made for the reduced form implied by the economic structure. An unrestricted estimator of the autoregressive moving average process on the reduced form residuals is then estimated. Forecasts then involve two components. The first component is from the restricted reduced form estimator for the systematic component of the model. The second is the unrestricted reduced form estimator for the residuals.

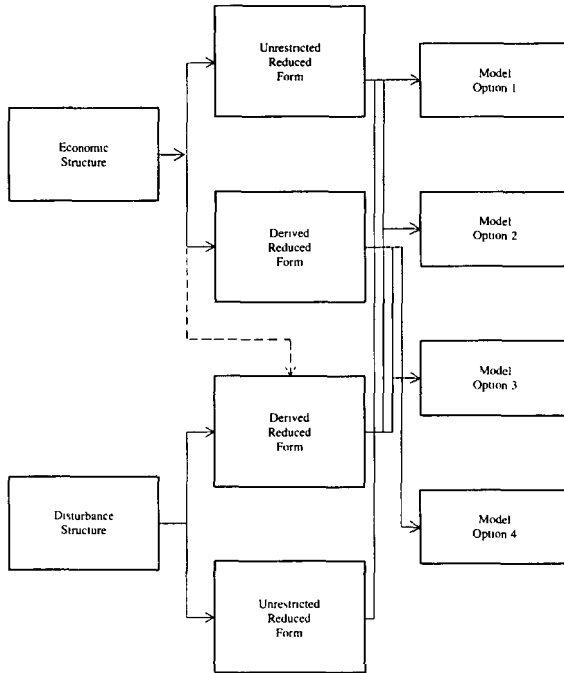


FIGURE 1. Combining Economic and Time Series Models.

These combined models can be viewed as methods for localizing approximate structures. The localization factors are derived from a fixed parameter representation of the residual process. That is, structural parameters are estimated from the time series data. Then either in a restricted reduced form or an unrestricted reduced form mode, the fixed parameter specifications for the autoregressive moving average processes on the residuals are estimated. Finally, the system is localized in a time series context by adjusting the values for the endogenous variables to reflect the information con-



tained in the residual process. Standard methods of calibration can, in fact, be rationalized based on this approach (Johnson and Rausser, 1981a).

The advantage of the residual process based approach to the localization of econometric models is that no violation of the theory is necessary to generate models with satisfactory predictive content. Autoregressive moving average processes, abstracting from computational difficulties associated with the multivariate specification, can be used to approximate series on endogenous variables until they include only white noise (Box and Jenkins, 1976). The same is, of course, true for the residuals based on the observed values of the endogenous variables as compared to the forecasts or projected values based on the estimates of these variables generated from the structural model. Thus, the forecasting accuracy of the model is not at issue. Instead, the issue is the partitioning of the process for generating the forecast as between a specification motivated by economic theory and a specification which is motivated by the desire to appropriately localize the model.\*

### ***Re-Estimation***

The advance of computer technology has made the problem of calculating parameter estimates for econometric models or systems of equations comparatively inexpensive. Parameters of models are easily recalculated on different data series or interestingly, the choice of different loss functions. This has led econometricians to begin to think of localized or sufficient linear approximations to systems. Approximations are made depending on the state in which the system resides, usually determined by the values of the environmental variables. Although work in this area is at a beginning stage, there are results that suggest the general thrust of the research (Gourieroux and Monfort, 1980, Hendry, 1980a, Monfort, 1975, Richard, 1980, White, 1980, and White, 1979).

The theory for these approximation approaches is, in general, straightforward. That is, one can think of an adapted approximation to a complex system, Figure 2. Sample information is summarized to specialize this approximation so that it generates the most useful

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\*To be sure, this approach can be rationalized in a varying parameters context, broadly conceived. The separation in this instance is motivated by the concern with separating the theoretically derived structure from one which has as its objective improving predictive performance for applied purposes.

forecasts. For example, forecasts might depend most heavily on the experience with the system in states similar to the one in which policy analysis is to be conducted. One method for choosing this approximation is to pick between regimes. Another is to let the sample data adjust the parameter values depending upon weights generated from the implicit design norm.

Suppose an implicit design matrix is reflected in a norm that makes it possible for the parameter estimates to be specialized to the forecast or policy problem at hand. The specification can be accomplished by specifying the position of the system (in terms of values of the conditioning variables) in which the policy exercise or forecast is to occur. Then, estimation of the parameters can proceed with the sample data and a loss function related to the distance of the observations (or another measure of locality) from the point to which the information on the model is to be focused.

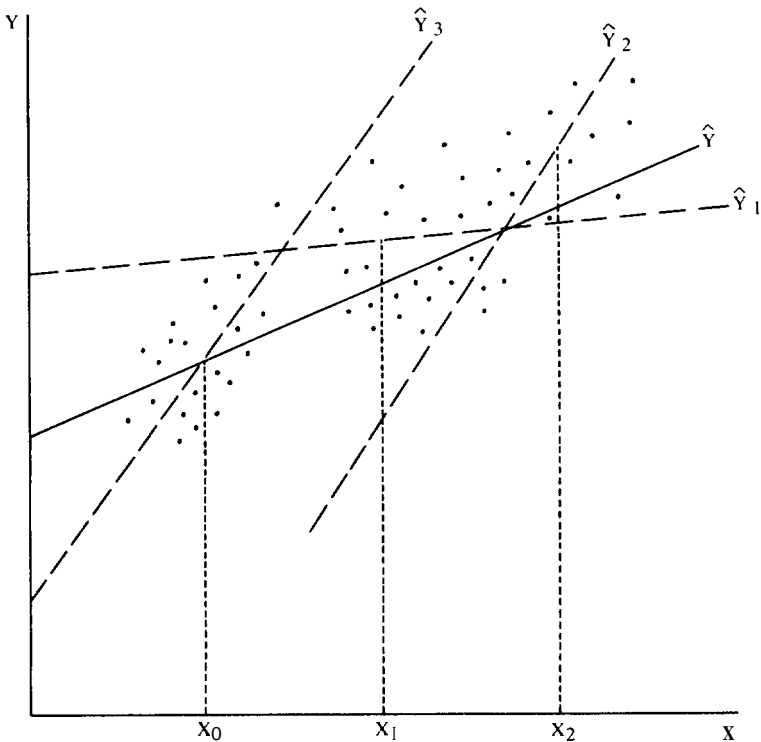


FIGURE 2. Localizing Parameter Estimation by Weighting Sample Data.

Using this approach, the experiences in the sample period most consistent with the exercise to which the model is to be put are weighted most heavily for calculation of the parameter values. Thus, different estimates of the parameters are implied by different forecasting problems and different policy analysis requirements. The intent is to move the local approximation of the complex system so that it is more accurate given the sample information available.

Straightforward computational procedures can be developed for this localization process. Values of the standard errors for the parameters computed for localized models provide an idea of the **reliability** of the results for specific policy analyses. That is, the estimated standard errors provide information on the uncertainty about the parameter estimates given the region in which the model is localized. For example, if there is a great deal of information in the sample about the system in the state or locality to be studied, then the parameter estimates will be highly reliable. The converse is true if the experience residing in the sample is thin. Thus, an approach of calibrating, or localizing the models is available by recalculating the parameters. This direct method makes the localization process apply to parameter estimation." That is, the estimation process is one which implicitly incorporates an hypothesis about localizing the model.

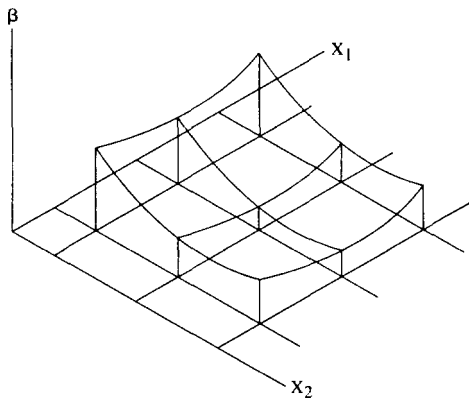


FIGURE 3. Response Surface in Estimated Parameter Values Based on Design Point Weights.

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\*It may be of interest to view this process in a varying parameters context. Instead of adding *a priori* information about the economic factors responsible for the change, parameters are altered by *a priori* information on the weighting of the sample observations.

### ***Re-Estimation and Combined Models***

Examination of the residuals from the re-estimated model for the sample data is readily accomplished. These residuals can be examined using the same kinds of time series processes as discussed for combined models. Based on the examination of these residuals, as illustrated in Figure 3, it can be determined whether the localization provided by the re-estimation at each of the sample points has removed sufficient variation. Also, since the localization does not have to be at the sample points, experimental designs in the underlying variables can be examined for impacts on parameter estimates. However, the results of the localized parameter estimates calculated for these experimental design points can not be compared to the sample data. The problem is, of course, that the residuals do not exist. Thus, residuals can be calculated based on the results of the localized estimators where the localization occurs at sample points.

Alternatively, the experimental design can be specified in the exogenous variables. The experimental design can be suggested by the forecasting and policy uses intended for the model. Then, the parameters at the design points can be estimated. The values of these parameters then can be examined as they vary over the design space. Methods for examining the parameter estimates as they vary over the design space can employ be the same autoregressive moving average processes suggested for analysis of the residuals. The stability of the parameter estimates can be analyzed. Alternatively, the estimated structures showing how the parameters vary over the design space can be used for adjusting the parameter values of the model depending on the situation to be studied.

### ***Evaluation and Alternative Designs***

The major points of the discussion of alternative designs and model performance are the separation of the processes of specifying a model which is appropriate based on the existing theory and the achievement of satisfactory predictive content for applied policy analyses. This separation may appear unconventional given that the ultimate validity of models is determined by predictive content. On the other hand, the conclusions are consistent with those common in research methodology (Popper, 1968 as compared to Kuhn, 1970). One should identify on the basis of theory, models which have the greatest predictive performance. But predictive performance not

being absolute is relative to that available for other models or theories. Once a specification choice has been made on the basis of relative predictive performance, the model can be fitted for use in applied situations.

Applications of policy models require high levels of predictive performance. If the theory is weak, then specifications with no economic content will have to be heavily relied upon to achieve adequate predictability. Autoregressive moving average processes are a logical supplemental choice in this connection. Using autoregressive moving average models perhaps combined with re-estimation of parameters for the economic model based on the implicit design matrix, specifications with various theoretical bases can be fitted for use in practical contexts. The choice criterion in model design is based on the relative amount of the variation which can explain with the economic or conceptual component of the structure. Nothing is changed relative to the traditional way of viewing model design. It is simply that the question of theoretical validity of the models is determined by relative predictability and the use of the models is governed by ultimate predictability.

With the approach emphasizing relative predictability in model design, econometricians and other modelers can escape traps associated with the mining of sample data, curve fitting, and the use of constructs which employ calibration or adjustment mechanisms that are indefensible to the ultimate users. There is justification for discerning among different theoretical model specifications and for approaches to localizing these models. The paper has had as its objective separating the localization from the theoretical specification questions and recommending approaches for the latter of these processes. It has been argued that by not recognizing this distinction, all too often econometricians and other modelers have moved into self-defeating approaches to model design. They have developed models which are not useful for predictive purposes and of little value to policymakers because the structures are so complex and so far removed from explainable theory that even the econometricians themselves do not believe them.

Finally, a comment is in order based on several older econometric pieces. What this reference shows is that, although we have developed substantial computational capability and information about the quality of estimates for fixed parameter structures, little has been done to better rationalize processes for dealing with approximate

specifications in realistic contexts (Keynes, 1939, Patinkin, 1976, Orcutt, 1952 and Schumpeter, 1933). The proposed approximation-localization approach and the associated discussion of model design provide an intuitive way out of the theory-predictive content dilemma, making it possible for economists to apply good theory to realistic problems with the prospect of providing useful results for policy.

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## Commentary

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*Earl O. Heady*

Stan Johnson's paper focuses largely on aspects of alternative statistical designs for policy models, rather than alternative designs of models or alternative modeling approaches. His paper has as its major objectives the explanation of different theoretical specifications of models and methods of localizing these models. His emphasis is almost entirely on statistically or econometrically estimated models. He also mentions that the subject of the paper is designing policy models as decision aids.

He has provided a very good synthesis of reasons why econometric models may not always have provided accurate forecasts or served efficiently for policy decisions. His main interest in the paper is in model specifications which simultaneously have theoretical justification and predictive accuracy. He properly emphasizes that the theory reflecting the specification is sometimes incomplete or nonexistent. He mentions that the performance of the models as decision aids has sometimes broken down because, although the internal resolution of the models has been good, predictions have partly depended on environmental variables which cannot be accurately projected. His suggestion for solving this problem is to endogenize these variables and have them predicted along with the rest of the system.

Other major modifications which he sees as necessary to improve the functioning of policy models include maintaining a current data base or set of observations and continuous model revisions. He states that optimal model design should have a configuration conforming with the decisions or actions that the output is to support. That is, variables should be included whose magnitudes will be determined as part of the system and by which the system can be

evaluated by the decisionmaker. The model also should include the variables representing instruments to be controlled by the decision maker. He states that the structure should permit the analysis of decision rules in a construct that has predictive integrity. Since especially statistical or econometric models, are approximations of systems, they should reflect responses or behaviors of the appropriate system.

He mentions that the record of policy models for predicting and guiding decisions has not been good for either economy-wide models or sector models. Although this statement may well apply to economy-wide models, I am not as pessimistic for some agricultural sector models. Too, not all agricultural models are for predictive purposes. But to the extent they have been inefficient in the respects mentioned, the reason may be more that in the past too many persons have been concerned with building "one night stand" models as an end in themselves. That is the model per se has been an end rather than the means of prediction and decision aids. We have quite a trail of models, especially in the graduate schools, where the analyst built a model, estimated its parameters then abandoned it to go on and build another model which also was subsequently abandoned. This approach was in keeping with earlier research in agricultural economics where the analyst completed a discrete study, published a research bulletin or journal article from it, "wrapped up thus," and moved on to a completely different discrete study. In this early process of modeling, the theoretical appeal of the specification was frequently the purpose of the model activity and the existence of "wrong signs," the inability to predict well even within the sample of observations and related deficiencies were given little weight. The goal was to be a modeler, rather than to assist decisions through models. Improvement in models is more likely to come about when they are used continuously, kept updated, and repeatedly respecified to meet changes in data, economic environments, and experienced model deficiencies. Perhaps the commercialization of models (i.e., the use of ongoing models to generate predictions which are sold to clients) will best fulfill this role in the long run. This would seem to be a necessity if existing or new commercial models are to endure and a market for their services is to be maintained.

There is opportunity and need for public institutions to build and maintain more ongoing models for similar decision purposes for

public policy. Once this goal is attained, I believe that some of the deficiencies that have been identified for previous models will be more readily overcome. Of course, some able analysts prefer not to engage in such a continuous activity since it does not involve going on to something new and different and may seem that they are performing a service in the manner of an extension specialist or a business economist. But I believe the criticism of Stan Johnson and some other economist has been that econometric models have not sufficiently or efficiently provided the services needed by decision-makers. Hence, I can see no reason why stigma should attach to individuals, institutions or firms who stay with their models and continue to use them while they are updated in terms of observations and respecified to provide more meaningful results. Some modeling efforts seem to follow cycles paralleling those of agricultural surpluses. A half a dozen years back, we had several people simultaneously working grain storage or buffer stock models. But we have little if any continuing work because each person published his results and went on to something else. After all, one wouldn't be in style if one persisted in perfecting his original model so it would be more useful in the next phase of the cycle. Modeling is an ongoing process, not a discrete activity. Many people are unwilling to follow the continuous respecification, updating, and estimating process because it does not bring notoriety and promotion. A young assistant professor probably would have a difficult time getting promotions, if he only kept up a well specified and updated model for continuous use.

I am not sure where Stan Johnson's discussion nets out with respect to complexities and completeness of the specifications and their underpinning theory. He seems to emphasize refinement in theoretical and specification aspects of models. However, he also suggests that complexity be avoided to the extent that decisionmakers or users can understand the model. I am not sure that the latter should be a tight requirement. The communications of the results or outputs of a model so that they can be understood and used by a decision or policymaker generally need not *interfer* with the construction of a model which is theoretically sound and generates dependable predictions. It seems to me that in most cases, the specification and estimation of a model is an activity differing from the interpretation and explanation of the results to users. I am sure that quite a large number of models, if not the majority, have been

developed with an audience only of academicians in mind and with little expectation of real world use by policymakers or decisionmakers. It often is these models where the developer is more concerned with the theoretical and mathematical sophistication of the specifications than with the quantitative results and whether they give wrong signs, exploding paths, etc. But where the analyst does develop and apply a model for actual use in prediction and thus for use by policy and decisionmakers, he should develop a model of the complexity needed to perform useful and dependable predictions. Then, as a separate step, he or others should translate these results for use by the appropriate policymaker. It will be only an exceptional case where an assistant secretary of agriculture, a state secretary of agriculture, or an administrator in SCS will want to know the internal structure or understand the statistical techniques used in generating quantities *reflecting* the future under different scenarios or policies.

Because this is true and because the user will have to depend on the integrity of the modeler and interpreter, it is more important that dependable models, regardless of their complexity, be used, rather than resort to oversimplicity to an extent that all users can understand the model. For the good of users, the dependability of the model is more important than its simplicity. However, since it will probably remain that many users will not understand the theory, mathematics, statistics, and basic validity of the model (including its consistence with theory and the real world), the model builder needs to be trustworthy in the sense that the predictions he propagates are dependable.

One of Stan's major concerns is in model designs which do not have predictive integrity because certain variables of the economic environment cannot be accurately projected. Hence, model projections may "go wild" or "blow up." His solution is to endogenize these variables so that their values are determined within the system. This suggestion is fine for variables that can be so handled. But there are many which cannot be adequately handled in this manner (for example, those relating to wage agreements, price indexing, grain embargoes, OPEC pricing policy, and many others which have been mentioned at this conference). However, I have no objection to exercising a bit of judgement and allowing these exogenous variables to take on alternative values (i.e., alternative scenarios) with prediction of other variables generated accordingly. Then the *mode-*

ler and user may decide on the most likely alternative and use the corresponding set of model outputs. Too, there is no reason why the user or policymaker's judgement of the future values of exogenous variables should not be used in the analysis. There is great need for greater interaction between developers and users as the model is developed and applied — especially as it is applied. In the 1980 RCA analysis, we made 69 solutions of a national model. In five of these, we made our own assumptions about the future levels of a set of exogenous variables. In the other 65 solutions, the users (an interagency coordinating committee within the U.S. Department of Agriculture) provided their estimates of levels of exogenous variables and relationships such as exports, irrigation technology, public expenditure as in agricultural research, etc. They then could review outcomes under a range of scenarios which they helped devise and on which they were as well informed as the modelers. Also, they could use their own subjective probabilities, which were undoubtedly as good as those of the modelers, in selecting the most likely future.

While the reasons for, or uses in, building models are numerous, there are five major ones. One reason for building models is to generate theses which will serve for degree purposes of graduate students while allowing them exercise in applying statistics, econometrics, and economic theory. Another is to generate materials for a journal article where it is a presentation of the model per se, rather than predictions from it used for decisions, which is the objective. These first two are fairly commonplace reasons for models and usually result in one night stand models which are seldom used again. A third major reason is to provide short-term forecasts of commodity prices, grain inventories, and other trading information for policy and other decision uses. A fourth major reason is to provide estimates of the effects of alternative policies, technological changes and market conditions on agricultural structure including such items as numbers, size, and distribution of farms, employment, capital purchases and inventories, farm income, etc.

To the extent that they are used for the latter two uses, the models will be mainly positive or predictive in nature. However, a fifth major use may call for a model which is more normative in character. This is the case of an imposition on agriculture of a policy or set of circumstances which have never previously been experienced and thus cannot be reflected in time series or sample data. It also is the

case when we want to know the resource or production potentials of agriculture in a manner which cannot be estimated through a model predicted from time series data. For example, in recent years, the nation has had before it questions of whether its resources were sufficient to invoke controls and conservation measures which would improve the environment and maintain soil productivity at selected levels. Generally, a programming or simulation model is necessary for such purposes simply because there is no time series basis for estimating coefficients of water runoff, soil loss, and the like for different land classes, crop systems, and cultural practices.

However, while normative models may be needed to assess these potentials, we also may need predictive modules to estimate price and related outcomes if these potentials in environmental improvement, soil conservation, and productivity maintenance were attained. Increasingly, policy will have to concern itself more with issues of this nature. Hence, there also is a need to be concerned with more than the predictive ability of econometric models. A much greater mix of positive and normative models may be necessary for major agricultural policy issues of the future.

Unfortunately, Stan Johnson did not concern himself with other than positive models. (I consider an input-output model to be more descriptive or positive in character than normative). To answer many future policy questions, we will have to apply models where the coefficients of variables are derived from technological knowledge, simulated methods and other means than statistical production from time series data. In too many of the papers of this conference, it has been assumed that there is only one type of model — one estimated statistically or econometrically from time series data. The issue in modeling for policy purposes must focus more broadly than on econometric models alone. They represent one tool from the modeling kit. What we need to be concerned with is the whole set of quantitative tools and the one (or the combination) which is best for analysis of a particular policy question or problem. The set includes econometric, linear and nonlinear programming, input-output, systems simulation, and operations research generally. Often the task is that of combining two or more of these quantitative methods into a model suitable to the purposes. Discussions which treat modeling as if it concerned only econometric, time series constructs, as in the case of Bruce Gardner at this conference, are too narrow and inflexible. While we keep ongoing time-series econometric models,



one of the greatest user demands from our model set over the last 15 years has been for national-interregional programming models.

At Iowa State University we have developed a large set of models in order to be able to analyze the impacts on agriculture of a wide range of changes in agricultural policies, market and export conditions, technological and structural changes, and previously unexperienced conditions such as fuel alcohol production, "forced conservation programs," reallocation and revised pricing of water, and others.

These analyses require both positive and normative type models. Normative type models are used especially where regional differentials are desired and the phenomena of concern has not been experienced previously. They are used to evaluate potentials in production, alternative use of resources, environmental conditions, water reallocations, etc., and especially to provide interregional interrelationships relating to various land classes, soil loss and environmental impacts. At one extreme in this normative set, we have a model which incorporates 223 producing regions, 12 land classes in each, 25 market and export regions, and endogenous transportation sub-model, endogenous crop and livestock production activities, endogenously determined livestock rations, and soil erosion rates for each crop for each tillage method and each conservation practice on each soil type of each region.

This general model can be restructured so that production regions or land classes can be aggregated into a smaller number, so that livestock production and ration composition can be either endogenous or exogenous, etc. It generates results for national, state, producing regions, river basins, watersheds, land classes, and other entities. It can trace the relationship of a change in an irrigated region in Oklahoma on a **dryland** farming region of Washington. It can be used to estimate potential supply price effects of a range of policy, market, resource use, and technology situations. It has been used in all major national assessments of agricultural resource use and technology over the past 15 years.

As a modification, demand functions have been incorporated as a positive feature, while supply remains are expressed normatively through the programming proponent. These quadratic programming modifications allow simultaneous (nonsequential) analyses not only of the potentials of programs, technologies, environmental or conservation restraints, and resource changes or supplies, but also of

the possible price impacts if these potentials were realized. These models provide great detail by region, commodity, resources, and other facets of agriculture.

At another extreme we have long used and extended an econometric, recursive simulation model. It has not emphasized short-run commodity price forecasts but has major focus on estimating, at the national level, the longer-run effects of changes in policies, market conditions, technological change, factor prices, and similar variables and our farm structure generally — including numbers and sizes of farms, resource demand and input use, farm income, capital use and farm expenses, and similar variables.

These projections are based, as is typically true for statistical or positive-type models, on relationships of the past. This model is structured on an annual sequential basis since we are interested in tracing out the impact of such changes over a fairly extensive time period such as 10 or 20 years. Quarterly commodity prices or similar variables have little importance in these uses. While we are in the process of regionalizing this econometric recursive model and adding more simultaneity, we have also developed linked or hybrid models of the national econometric model and the interregional programming model. These hybrid models also are recursive, with the econometric component determining prices when fed to the programming model where commodity supplies and resource demands are generated for the next period, etc., for subsequent periods. Again the hybrid model provides, for all of the nation's production regions, a normative analysis of potential should we try to produce as much food as possible, lower soil loss to specified levels, attain specified levels of environmental improvement, reallocate or price water on the basis of its marginal productivity, etc. But it also, in terms of its positive or econometric component, allows examination of the potential equilibrium price and impacts of these changes.

As a variant of this linked model, we have developed a regional model which has promise for several purposes. The econometric structure is retained for commodity demands, equilibrium price determination and commodity supplies outside the region of particular analysis. Supply in the region of special analysis is generated through a programming **submodel** and is added to the supply for the rest of the nation as predicted from the econometric model. The model is mainly normative for the problems being analyzed in the

particular region, but more or less of a positive nature for the rest of the nation. This model was built at the request of the International Institute of Systems Analysis but promises to have considerable use potential in U.S. regional analyses.

Of course, various modifications and respecifications of the above model set can be and have been made to study specific problems. Modifications include specific models to analyze energy supply and price effects, energy production potentials in agriculture, alternative water pricing systems, and tradeoffs in soil conservation, production costs, energy use, and exports.

The problems of agriculture are so heterogeneous and the quantities which need to be analyzed, either to forecast short-term economic outcomes or analyze long-run potentials, are so various that no single model form can meet all of these needs. Hence, there continues to be justification for further development of a variety of models which can help answer problems related to different facets of the agricultural economy.



# Principles of Policy Modeling in Agriculture

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*Gordon C. Rausser and Richard E. Just*

## **Introduction**

The domestic and world economies of food and agriculture have become increasingly complex over the last decade due to economic instability, government administrative instability, inflation, foreign price and trade regulation, along with money supply and credit manipulation. From resource utilization at the agricultural production level, all the way to final consumption of food, a variety of economic, political, and technological forces have continued to evolve with pronounced structural implications.

To deal with this apparent complexity of the agriculture and food sector in policy formulation, models have long been viewed as a potentially valuable aid to the evaluation and selection of policy strategies. Models can be employed to generate quantitative forecasts and to evaluate the effects of alternative decisions or strategies under the direct control of policymakers. In essence, models can offer a framework for conducting laboratory experiments, without directly influencing the agricultural and food economy. They also potentially offer a basis for sharpening the judgments of analysts and policymakers alike.

Many models of the food and agricultural sector have been constructed. Some have been constructed for descriptive purposes, some for explanatory or causal purposes, some for exploratory purposes, some for forecasting purposes, and others for the express purpose of decision analysis. The latter group of models, of course, is of direct interest in policy formulation. Such models require at a minimum (a) the performance or target variables considered important by the policymakers, (b) the instruments or policies available to

policymakers, and (c) a set of behavioral, identity, and physical relationships which link (a) and (b). This group of models is, indeed, the most demanding; the development of useful decision-making models for dynamic stochastic systems of the type represented by the agriculture and food economy requires the construction of conditional policy forecasts. In many situations, the construction of forecasting frameworks will also require the development of descriptive as well as explanatory models. To ascertain the effect of alternative policies in terms of performance measures, causal relationships between the decision variables and relevant performance measures must be captured.

An examination of the anatomy of policy models provides the basis for an assessment that the potential for such efforts is largely unrealized. By examining the elements of policy models in terms of their conceptualization, specification, estimation, and use, the unfulfilled promise of modeling as an aid in support of policy analysis begins to take shape. While the anticipated costs of policy modeling have been incurred (and often exceeded) over the past few decades, the anticipated benefits have not yet emerged. This observation is, of course, not new. Reasons such as insufficient model validation, insufficient linkage and feedback relationships, and insufficient communication between model analysts and policymakers have been advanced for the failure of quantitative models to attain their promise. This paper argues, however, that the reasons underlying this failure run deeper and span a broader set of issues.

Architects of policy models have too often followed the principles of model formulation that are generally appropriate for other purposes of models — descriptive, explanatory, causal, exploratory, or forecasting purposes (Rausser and Hochman). A close examination of problems arising in the use of quantitative models in policy formulation or decision analysis suggests the need for a set of principles to emphasize the tradeoffs that must be considered in the construction and use of agricultural policy models. The assessment of tradeoffs for descriptive, explanatory, or forecasting models differ measurably from such assessments for policy models. This paper attempts to develop such a set of principles or a code of conduct specifically relevant to modeling for policy decision analysis. Ten basic principles, along with a number of subprinciples, are identified. The ten basic principles and associated tradeoffs that are justified and discussed through the course of this paper are

as follows:

1. The purposes and goals of policy models should be explicitly defined at the outset with a view to the policy decisions that will be evaluated.
2. The experimental role of policy models should be exploited.
3. Post-Bayesian analysis should guide the design, estimation, and use of policy models.
4. Policy models should be designed to accommodate and track structural change.
5. The degree of imposed theoretical structure in policy model specification should depend on the amount of historical information.
6. General equilibrium rather than partial equilibrium relationships should be emphasized in the structure of a policy model.
7. Policy modeling must provide for the use of intuition, both in model development and in updating; strong intuition should override causal implications of coincidental data in model development.
8. Use of greater weight on more recent data in policy model estimation should be seriously considered.
9. General purpose data sets rather than general purpose models should be emphasized.
10. Policies should be formulated with an appropriate degree of learning in mind.

### Principles of Policy Model Use

*Principle 1: The purposes and goals of policy models should be explicitly defined at the outset with a view to the policy decisions that will be evaluated.*

What decisions or policies is the model designed to influence? Who will use the model? For whom is the output information intended? Consequently, what information must the model provide to the users? What input variables shall be used to test alternative and environmental assumptions? How often will the model be used? How timely must the input information be? The answers to these questions are crucial. They define the model operationally; in turn, they become the marching orders for the model architect to implement.

There are far too many models that have been constructed by an ambitious analyst that are well specified technically but have not addressed these questions. As a result, the models contain elaborate but irrelevant detail. Far too frequently, researchers construct policy models under the following premise: "that the goal of economic modeling is to provide *helpful information to decisionmakers* that will *improve the likelihood of their making a correct choice* when confronted with *a set of possible actions unknown to the researcher during the construction of the model*" (Hughes and Penson). This perspective places the researcher in a world of uncertainty, gambling with odds heavily stacked against success.

To illustrate the importance of model purpose, consider the effect or the design of policies to influence the structure and control of agricultural production. As noted by Gardner, agricultural economists have made little progress in determining the distributional effects of price support, acreage set-aside, deficiency payment, and public stockholding policies. One possible reason for this observation is that most models concentrate on output markets; and, certainly, the vast majority of agricultural sector models address only these markets. However, to measure the distributional impacts of various policies both qualitatively and quantitatively, we are forced to deal squarely with dynamic interactions, feedback, and linkage effects as well as equity and efficiency effects. This general observation leads to the following subprinciple.

***Subprinciple 1.1: For multidimensional policy problems with noncomparable objectives, the analyst and policymaker should examine alternative weights or equity schemes.***

In the case of many agricultural policy problems, we are faced with multiple objectives, including such loosely defined measures as increased income of farmers, increased consumer welfare, improved distribution of income, self-sufficiency; price stability, improvement in the balance of payments, decreased public expenditures, stable flow of supply, and the like. It has been recognized on both normative and positive grounds that criterion functions based only on efficiency are inappropriate in many operational applications. The work of Stigler and Peltzman highlights the growing disenchantment with the economic efficiency objective and points out that the political process is inconsistent with dichotomous treatment of resource allocation and wealth distribution.



In the face of multiple concerns, the continued use of single-attribute, objective-criterion functions will result in analyses which often fail to address actual policy problems. Hence, multiple objectives must be considered. The definition of a multidimensional objective function neither creates nor resolves conflicts associated with policy issues; instead it identifies them. The identification of conflicts is, of course, an important first step in resolution. Most of the recent advancements on the specification, identification, and assessment of multidimensional objective functions are summarized by Keeney and Raiffa.

Since unique single-attribute objective criteria are often not appropriate for policy analysis, one approach is to determine the effects of alternative policies on each objective and then allow the political process to select among the alternatives. Policy model experimentation with alternative weights can provide some important information for this process. In a "normative" or prescriptive setting, the Keeney and Raiffa multi-attribute utility function approach can be used, while, in a more "positive" setting, revealed preference has been employed to determine weights associated with various objectives. In any event, as Steiner (p. 31) argued some years ago, "we now accept in principle that the choice of the weights is itself an important dimension of the public interest."

In a revealed preference framework, Rausser and Freebairn argue that the importance of the bargaining process and the resulting compromises between different political groups, the range of preferences of these groups, and the lack of an explicitly stated, unambiguous value consensus suggest construction of several criterion functions. They argue that these functions should reflect the extreme viewpoints and preferences of various decisionmakers actively involved in the policymaking process as well as the preference sets lying between these extremes. A parametric treatment of the resulting set of preferences will provide decisionmakers with rational policy outcomes, conditional on the representation of policy preferences. Thus, the results obtained from such an approach should contribute to the efficiency of the bargaining process and in reaching a consensus, they should serve each policymaker individually, and they should serve to make quantitative analysis based on historical data effective for many policymakers even though the composition

of a legislative body and/or "appointed" policymakers might change.<sup>1</sup>

*Subprinciple 1.2: The distributional effects of agriculture and food policies can be seriously examined only through their indirect effects on asset markets.*

Of course, if distributional issues are not under examination, a model need not have the complexity associated with asset markets. However, if such issues are crucial, the general equilibrium effects on these asset markets are exactly what must be examined. In three conceptual papers (Rausser, Zilberman, and Just; Just, Zilberman, and Rausser; and Hochman et al.), it has been demonstrated how input flow and asset stocks can be altered indirectly by changes in both **sectoral** and general economic policies. For example, a sampling of the implications of these theoretical frameworks under partial participation are as follows:

- An increase in deficiency payments and/or a reduction in acreage set-aside requirements leads to increased concentration, measured by the average land size of active farms.
- An increase in deficiency payments and/or a reduction in acreage set-aside requirements encourages the adoption of output-increasing technologies and discourages the adoption of cost-reducing technologies.
- Restrictive monetary policy tends to reduce the ratio of land prices to rental rates and to encourage participation in voluntary government programs.
- Higher rates of exemption on capital gains for tax purposes and escalations in the general tax structure increase the ratio of land prices to rental rates and encourage inflationary land price spirals.

Without the explicit consideration of the indirect effects of sec-

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<sup>1</sup> It should be noted that the revealed preference approach imposes rather restrictive assumptions. The mathematical form of the criterion function must be specified, the constraint structure must be empiricized, and rationality is assumed. Given this structure, past policy actions can be utilized to infer the weights or tradeoffs among alternative objectives. Rausser, Lichtenberg, and Lattimore have developed an integrative framework which blends a number of frameworks that have appeared in the literature. This framework presumes that there is a set of relevant criteria functions. Elements of this set differ in terms of alternative weighting or equity structures. As the policymakers change over time and power shifts occur in the composition of legislative bodies, weights across various performance measures change.

toral and general equilibrium policies on asset markets in agricultural systems, it would not have been possible to derive the above implications. It is, indeed, important to be alerted to such potential effects in the selection of actual policies. For example, a desire to increase farmers' income by reducing output could lead to an increase in the relative rental/land price ratio, thus reducing the shadow price of credit and making new investments more attractive. The resulting adoption of new technology, especially output-increasing technology, can make various policy mixes of target prices, loan rates, and acreage set-asides in the short run quite different from the long run.

***Principle 2: The experimental role of policy models should be exploited.***

In essence, policy models offer a framework for conducting laboratory experiments without directly influencing the system. Since these experiments can be conducted with a model rather than the real system, mistakes that may result in costly consequences can be avoided. This experimental perspective forces analysts or others interested in a particular system to be precise about their perceptions and to examine possible inconsistencies in those perceptions.

Experimentation with policy models has often been inhibited because of inability to solve complex dynamic stochastic systems. However, the development of a number of methods over the past decade facilitate the experimentation of the sort envisaged here. They can be categorized anywhere from analytical to analytical simulation to ad hoc simulation methods. All of these methods are faced with a problem of multiple local optima. Analysts frequently deal with these problems by employing incomplete or partial multiple-objective criterion functions. The limitation of such partial analysis is that superior solutions often lie in "inferior" regions. Given the limitations of operating with complete, as well as incomplete, multiple-objective criterion functions, analysts should attempt to generate alternative weightings or trade-off relationships in accordance with Subprinciple 1.1. One set of weights could reflect the power and strength of various interest groups.

Most policy models are structured to investigate specific policy instruments. The emphasis on the experimental role of policy models requires, however, more originality in the selection of policies that are evaluated. For example, the results from policy models

for predetermined instruments should be used in part to gauge the design of other policies not previously considered.

To facilitate originality in the policies selected for evaluation, econometric methods, operations research, systems analysis, and simulation should not be viewed as mutually exclusive approaches. The use of multiple approaches is often more desirable (Brill) to develop, evaluate, and elaborate alternative solutions. It increases the likelihood of tailoring available algorithms to provide significant insights rather than just answers. With this perspective, policymakers and analysts are not wedded to the first design, and there are implicit incentives to pursue other distinct alternatives. In this environment, artificial intelligence and heuristic methods will prove particularly worthwhile. Thus, the answer-seeking mentality is avoided, and learning and inductive inference is highlighted.<sup>2</sup>

***Subprinciple 2.1: Potential users must be involved in the process of model design and development.***

One effective means of facilitating the effective use of policy models and the explicit definition of the goals of a policy model at the outset of the model development phase is to involve the policymakers or users of the model results in the development process from the very start. As noted in a study by McKinsey & Co., Inc., in the late 1960s, one of the principal factors explaining the failure of a large number of private corporation planning and decision models is the lack of user involvement in the development process. Of the 36 large corporations surveyed in this study, the report concluded that the neglect of user involvement is, indeed, costly.

There can be little doubt that users should play an important role in the determination of the objectives for the modeling effort. When designing the model, substantial attention should be paid to users' perceptions of the environment under examination. In general, we

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2 To facilitate learning and inductive inference, analysts investigating various policy issues in agricultural systems will have to develop an **expertise in experimental design** and response surface procedures. Relevant experimental designs must be sequential (Anderson) and squarely address "**policy improvement**" **algorithms**. Such **algorithms involving sequential designs typically begin with** an extensive search via **simple** exploratory experiments which converge toward some peak (or valley) of the surface and then switch to an intensive search as the optimum is approached. To implement such **sequential experiments and policy-improvement methods**, the appropriate response surfaces must be constructed. Fortunately, an excellent survey is available for analysts to familiarize themselves with response-service investigations from the **standpoint** of sequential analysis and optimal designs (Chernoff).

tend to trust and use something we have had a hand in developing; it is difficult to develop confidence in something we must accept on faith. Equally important, the involvement of users during development enhances their understanding and decreases the educational effort required after the model is constructed. Obviously, involvement of the ultimate users must be managed judiciously, given their perceptions about the opportunity cost of their time. If the ultimate users cannot allocate time for such efforts, then at a minimum their trusted deputies should be assigned the task.

*Subprinciple 2.2: Development of policy models must be treated as a process, as opposed to just the creation of the product.*

Unfortunately, this is a subprinciple that often fails to guide the actual construction and use of policy models. The product approach is the more usual situation; its goal is to create a working model, and those involved in the construction find it difficult to see beyond that stage in their efforts. For the process approach, the creation of the model is an important step along the way toward using the model to affect policy analysis favorably. The longer run view of the process approach fosters a give-and-take relationship between the analyst and user in model design, and improvements that usually continue beyond the first implementation. It assists everyone involved in the process of model construction to behave nonmyopically and to consider how the model will be used in the future and how the organization is likely to respond to its use. The process approach anticipates the need for education and organizational change to effectively utilize the model for policy evaluations.

## Principles of Policy Model Specification

*Principle 3: Post-Bayesian analysis should guide the design, estimation, and use of policy models.*

As argued by Faden and Rausser, neither the "Bayesian" nor the "classical" school of thought on the foundation of statistics is adequate. Thus, the nature and purpose of the current statistical foundations need to be reexamined. An adequate theory should be compatible with the way science develops. Moreover, the conceptual base should be consistent with the way in which we casually accumulate knowledge in everyday life. It should also be "axiomatically" satisfying.

The Bayesian approach to statistical inference and knowledge accumulation would, in fact, be correct if analysts and policymakers had unlimited and costless information-processing capacity. A rigorous Bayesian would need superhuman abilities — a perfect and infinite memory, perfect deductive powers including faultless and instantaneous calculating ability, and the wherewithal to understand questions of arbitrary complexity. Hence, due to human limitations, more or less serious departures from the strict Bayesian approach are warranted. In particular, the cost of information collection, processing, and interpretation should be recognized.

Formally, the post-Bayesian criterion for inference is to minimize expected loss or costs. It is, therefore, consistent with the general framework of decision theory; inferences are "Bayes" decisions with respect to some prior distribution. However, the criterion stresses two major cost categories that do not appear in the early work of Wald or his successors. The first cost is associated with complexity, namely, those costs that emanate from information processing: constructing models, gathering and storing data, solving models, communicating results, and the like. The second cost component is associated with inaccuracy. Hence, the approach explicitly evaluates the tradeoff between accuracy and complexity. In essence, the benefits and costs associated with alternative policy models dictate strategy in their construction and use.

According to Powell, the complexity of a model is measured by such characteristics as a number of equations in a model, the nonlinearity of a model, and number of "families" to which the equations belong. Similarly, *ceteris paribus*, deterministic models are simpler than stochastic models, static models are simpler than dynamic models, and lump-parameter models are simpler than distributed-parameter models. In general, complexity rises with the number of free parameters. Complexity of a policy model is not measured simply by model size or the number of endogenous variables.

To indicate how complexity costs can be assessed, consider the problem of alternative regression models aimed at, say, predicting a certain variable of interest. Complexity costs generally rise with the number of explanatory variables. Here, cost may take the form of money, time, resources, or effort used in model development and analysis. Certain aspects of cost rise linearly with the number of variables (e.g., tabulating the data); some go up quadratically (for

example, printing the covariance matrix); some rise cubically (e.g., inverting the moment matrix). These are not the only costs, but they suggest that a cubic polynomial in the number of variables may be one possible representation of complexity costs.

In addition, differences in complexity costs of observation also result from sample survey design, sequential analysis, and other data selection criteria. Thus, even tractable models differ considerably in complexity. The most radical consequences of incorporating complexity costs — or, equivalently, the value of simplicity — results from evaluating the relative costs of such alternative models (Faden and Rausser).

The second important cost component is associated with inaccuracy. The more accurate a model is, the more benefit is accrued from employing it to resolve various policy issues. Or, in other words, there is a cost associated with inaccuracy. The cost of an inaccurate model depends on how it is used. That is, for models used as guides in making decisions, inaccuracy tends to degrade the quality of the decision. This implies that, to assess the cost of forecast inaccuracy, one must embed the model in a more complete policy framework. There are several ways of making this embedding, each generally leading to a different inaccuracy cost function. There is no absolute "metric" for inaccuracy.'

***Subprinciple 3.1. Alternative model specifications for the same problem imply different decompositions of systematic and nonsystematic components.***

The balancing of inaccuracy with complexity is particularly crucial in the selection of explanatory variables. Somehow, a selection of "significant" explanatory variables (or "appropriate" policy variables) must be made from a large pool of variables, and the proper estimates or settings must be made for each. The post-Bayesian approach makes this selection in a structured fashion that involves the weighting of alternative costs and avoids the inappropriate tests that are inherent from conventional statistics.

To illustrate the implementation of Subprinciple 3.1, consider the case of supply response for some of the major feed grains where weather conditions are important. Owing to complexity costs, the

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3 Various metrics of Inaccuracy are outlined by Faden and Rausser. Briefly, these measures are based on departures from the ideal pattern of Bayesian inference

coefficients on weather variables in an estimation context may be set to zero. For feeder calf supply, range conditions, indeed, play a role; nevertheless, they are sometimes excluded as an explanatory variable because of complexity costs associated with data acquisition, the increased ability to identify other coefficients and the inability to forecast weather. Such potential explanatory variables are subsumed in the error process. To the extent that movements in these variables can be represented by autoregressive, moving average processes, their influence on endogenous variables of interest can be ferreted out through time series representations of the error or disturbance terms. Moreover, if the purpose of constructing a policy model is to evaluate, say, alternative feed grain reserve policies vs. meat import quotas, the explanatory variable which must appear in systematic components (variables whose coefficients assume values other than zero) vs. nonsystematic components (disturbance terms) may differ among policy evaluation problems.

One of the major problems with conventional policy models that have been constructed to date emanates from their failure to recognize complexity costs and, thus, the need to balance those costs against the cost of inaccuracy resulting from abstraction. Incorporation of these costs leads to what we have characterized as the post-Bayesian approach and requires a reexamination of procedures of model construction. Admittedly, however, because accurate estimates of complexity and inaccuracy costs are not possible, post-Bayesian procedures must often be implemented with crude estimates of such costs. Nevertheless, for a number of illustrative applications (see Faden and Rausser), it is possible to use very crude estimates of these costs to motivate procedures that should prove to be superior to conventional treatments.

***Principle 4: Policy models should be designed to accommodate and track structural change.***

By their very nature, models are abstractions involving simplifications imposed by available data, research time, and budget as well as by the desire to achieve tractable results. Such simplifications and abstractions often result in misspecifications which, in turn, influence the accuracy of conditional probability distributions. As demonstrated in Rausser, Mundlak, and Johnson, the effects of such misspecifications can be countered by introducing appropriate parameter-variation structures which may be theoretically or empiri-



cally based. The most important types of misspecifications that arise in the construction of policy models include omitted variables, proxy variables, aggregate data, and simplified functional forms.

In addition to the misspecification rationale for varying parameter formulations, economic theory can be advanced to justify their potential relevance. In many situations, the very nature of economic theory leads to relationships that change over time. For example, Lucas has argued that the constant parameter formulation is inconsistent with economic theory. He notes that a change in policy will cause a change in the environment facing decisionmakers; under the assumption of rational decisionmaking, this will result in shifts in the equations representing their behavior.

One of the better examples of the points raised by Lucas occurred as a result of the U.S. economic stabilization program during the period 1971-1974. Price ceilings were imposed on red meats at the end of March 1973. When combined with the biological nature of various red-meat animals, these ceilings led to distorted and clouded price signals which resulted in strategic errors on the part of numerous decisionmakers. Thus, the signals led to instability in the expectation-formation patterns of decisionmakers along the vertical commodity chain in beef, pork, and poultry. During that period, the cattle cycle, which was poised for a sizable liquidation, was substantially altered. In fact, for a short time, price ceilings appeared to become the expected prices of producers. As a result, the liquidation phase was curtailed, resulting in larger supplies, substantially lower prices, and significant negative margins. Hence, the price ceilings had the immediate effect of a substantial shift in price expectations which, in turn, had drastic implications for dynamic supply responses, ultimate market realizations, and cattle inventories. A model which includes a particular price expectation formation pattern as part of its maintained hypothesis would thus be subject to structural change.

In essence, this principle recognizes that it is important to distinguish between the "local approximation" accuracy and the "global approximation" accuracy of a model structure. In attempts to achieve global-approximation accuracy with abstract models, there is no choice but to operate with specifications that readily admit structural change. The importance of this principle has been illustrated on numerous occasions during the last decade. For example, models based on data bases up to 1972 fail to account for the

significant linkages with the international economy, especially the significant movement in the exchange rates and the integration of international capital markets during the balance of the 1970s (see Schuh and Chambers and Just). Models that fail to track and accommodate these significant changes will fail to achieve sufficient credibility and thus will not be seriously entertained by policymakers. Similarly, in the late 1970s and early 1980s, linkages with the general economy (especially with interest rates reflecting monetary and fiscal policies) apparently forced a shift from one local approximation to another. During the 1980s, models which fail to accommodate structural changes that result from significant movements in interest rates (via their effect on exchange rates, export demand, stockholding behavior, and investment) will fail many credibility tests.

The issue of accuracy is particularly important when the structural model representation is nonlinear in the variable space. In agricultural systems that address dynamic, linked, and feedback relationships, model representations often involve simultaneous interactions of large systems. For nonlinear representations in these model forms, it is not possible to obtain a unique reduced form. In computing the necessary derivatives to obtain this form, issues of approximation and round-off problems naturally arise. More importantly, it is not possible to derive reliability statistics for highly nonlinear models. Analysts operating with such models often "sweep under the rug" the problem of measuring the variability (or risk) associated with the various policies that are under examination. It is shown in Rausser, Mundlak, and Johnson that these problems frequently can be avoided by specifying models that are linear in the variable space but are, in essence, nonlinear in the parameter space. This requires the specification of models in which the parameter effects are not constant but are treated as time-varying and random. The approach allows forecasts of probability distributions, conditional on alternative policy actions, to be generated for particular points in the parameter space. This approach also simplifies the validation and verification procedures, especially the derivation of dynamic properties.<sup>4</sup>

4 This approach is entirely consistent with post-Bayesian principle 3. From an operational standpoint, the relevant issue is whether or not the explicit recognition of varying parameters will improve accuracy and implementation benefits which outweigh their additional complexities. For most agricultural policy problems, these formulations are more likely to capture the enduring characteristics of the processes under examination.

*Principle 5: The degree of imposed theoretical structure in policy model specification should depend on the amount of historical information*

The proper degree of imposed structure, as well as the extent of accommodation for structural change, depends upon whether the model is used to evaluate policies for which there is much prior experience or little or no experience. The latter situation would arise in evaluating new institutional designs. In other words, a greater amount of prior experience on the effects of a particular policy allows greater accuracy in estimation with less imposed ad hoc structure. However, more specification is needed if new policy controls or instruments are under examination in order to allow parameter identification. In some instances, highly structured programming models may be the only possibility for evaluating policies for which no prior observations are available; if prior observations are available, a less structured model may be more appropriate and may provide a better level of flexibility in ascertaining from observed data the effects of alternative policy instruments.

Where sufficient data are available, reasonable fits are often obtained with the econometric approach. But, even under these circumstances, predictions often quickly go off course as explanatory forces move outside the range of data used in the sample period for estimation. Some of the main approaches to combat this problem have involved adding further structural specification such as theoretical restrictions based on consumer utility theory or producer profit maximization. Some of these approaches are based on a neoclassical theory which entails full flexibility at least as an approximation. But the cost of such flexibility can be that the numerous resulting parameters may not be identifiable when few observations on a given situation are available. This problem is mitigated to some extent by making further ad hoc assumptions with respect to functional forms of preferences and technologies; but this approach leads to costs of inaccuracy associated with erroneous ad hoc assumptions.

At the other extreme, programming models can make more efficient use of data in estimating input-output coefficients and resource availability when only one or a few observations are available, but

very poor predictions of producer behavior are often obtained from programming models. This is apparently due principally to three sources of inaccuracy. First, producers' objective criteria may differ from that used in the programming model; second, farmers' subjective distribution of prices and yields may be different from that reflected in the programming model; and, third, the linearity of a programming model may be inappropriate. All three of these problems result from using extreme ad hoc assumptions rather than providing the flexibility to allow inference from observed data. The results of programming models in predicting farmer responses are often less satisfactory than those of econometric models in situations where both are applicable (that is, where sufficient historical data are available on the policy controls of interest). Thus, the appropriate degree of ad hoc structure depends crucially on the availability of data reflecting the observed effects of relevant policy controls.

Moreover, the fact that U.S. agricultural policy change is often a mixture of both institutional change and policy instrument change further suggests that policy model specification can, in some cases, be enhanced by a proper blend of the two seemingly very different approaches. An effective merger of the conventional econometric and programming approaches centers on the distinction between discrete (qualitative) and continuous (quantitative) choices. Institutional choices or selection of particular policy instruments correspond to qualitative choices, while changes in policy instruments correspond to quantitative choices. Programming formulation can easily handle the former, while conventional econometric models focus on the latter. Moreover, inequality constraints found in programming models are not admitted in conventional econometric formulations. However, both discrete and continuous choices and inequality constraints can be admitted in behavioral models estimated by qualitative econometrics methods; thus, some of the recent developments in qualitative econometrics offer promise for achieving a proper blend. Two recent papers which survey and apply these methods are Chambers and Just (1981) and Rausser and Riboud (1981).

***Subprinciple 5.1: The number of variables employed to reflect policy instruments is crucial in interpretation of historical data.***

Government policies are often changed from time to time in a way that seemingly involves a switch to a new set of policy instru-

ments. For example, U.S. wheat was regulated by price supports and strict allotments with marketing quotas in 1950 and from 1954 through 1963; by price supports alone in 1951 through 1953; by voluntary allotments, diversion requirements, and price supports in 1964 through 1970; and by set-asides with target prices and deficiency payments in the 1970s. Furthermore, the set-aside program has at times required cross-compliance and in other times not. With this frequent revision of the set of policy instruments, there has sometimes been only a very small number of years in which the effects of a given set of policy instruments could be observed. If each of these sets of policy instruments is treated as independent, then the information that can be gained through historical observation of their impacts is extremely limited.

Econometric purposes, for example, are greatly facilitated if ways can be found to represent alternative instruments as different levels of the same set of instruments. In this way, both degrees of freedom can be saved in estimation, and more information can be gained by comparison of the effects of alternative policy regimes. For example, in moving from a policy period with strict allotments to one of voluntary allotments, one would expect that those farmers that continued to participate would behave in much the same way as when allotments are strictly imposed. Similarly, one would expect those farmers who do not participate to behave much like they would when no allotment program was exercised. By making this minimal assumption, one can reduce the number of variables needed to reflect the alternative policy regimes in an econometric model (Just 1974).

Similarly, the roles of diversion requirements and set-aside requirements are quite similar as are the roles of wheat certificates and deficiency payments. By appropriately considering the similarity of these controls from one policy regime to another, one can often gain more information on the effects of policy instruments from historical data. These considerations also lead to greater simplicity in policy models and, thus, the complexity costs can be reduced accordingly. In reducing the number of variables representing policy instruments, however, one must bear in mind the approximations that are introduced. In this context, the earlier comments on the degree of imposed ad hoc structure may be reiterated.

*Subprinciple 5.2: Summary variables rather than representative variables should be emphasized in policy models.*

A common practice in econometric application has been to consider as many variables in model construction as may seem intuitively important but then to prune that set of variables based on their apparent econometric importance. In doing so, variables may be excluded which intuition implies should clearly play a role. A justification for this practice usually goes as follows: (1) either the variables are truly unimportant or do not play a role, or (2) they are sufficiently closely related to variables that are retained in the model that multicollinearity prevents estimating a separate coefficient. Thus, a similar multicollinearity is assumed to persist in the forecast period. When intuition is sufficient, a more appropriate practice would be to construct summary variables which include the effects of perhaps several colinear variables. This is particularly true in policy modeling where distinct changes in policy controls may cause collinearities observed in a sample period to cease.

Many models have made use of price indices along this line to represent the effects of many exogenous prices. However, relatively few models make use of price indices including several endogenous prices. Similarly, relatively few models use quantity indices which embody the effects of several quantity variables which may be too highly related to be included separately in an econometric model.

The case of estimating meat demand prior to 1970 may serve to illustrate the importance of this principle. In data generated prior to 1970, the prices of beef, pork, and poultry all tended to move together so that the resulting multicollinearity prevented estimation of commodity-specific cross-elasticities. As a result, many modelers tended to exclude all but one of the "cross" prices so that, for example, beef demand would not be sensitive to pork prices, etc. Many of these models, however, performed poorly in forecasting the events of the 1970s because the huge feed-price increases caused a change in the relationship among livestock prices. For example, hogs began to sell at a premium relative to beef cattle. These events thus led to failure of the models which had followed the practice of excluding colinear variables. Alternatively, if summary variables had been used to include the prices of all commodities which intuition clearly dictated were important, then the associated models might have been able to predict the associated consequences of high

feed prices, at least to some extent. Thus, if summary variables are used rather than excluding variables which are clearly important, then a model may not flounder as soon as some existing causal multicollinearity ceases to hold. Of course, these arguments are also consistent with the need for constant consideration of model revisions and the importance of subjective information in model development and data interpretation.

***Subprinciple 5.3: Functional flexibility and alternative distributed lag structures must be evaluated constantly as more information is obtained.***

This subprinciple simply recognizes that all maintained hypotheses must remain tentative. In other words, various elements of conventional maintained hypotheses must be relaxed and reevaluated as the modeling process continues. The imposed structure must be constantly reassessed. In essence, to the extent possible, the imposed structure should be in a fluid state.

***Subprinciple 5.4: Relative rather than absolute specifications enhance policy model longevity and degrees of freedom in estimation.***

In the infancy of econometric modeling, the objective of policy modelers was to determine a linear relationship between two or more variables in nominal form. Further experience, however, particularly in inflationary times, suggested that models tended to lose their tracking ability after sufficient inflation when variables were used in nominal form. In response to this problem, prices began to be used in relative or deflated form for econometric modeling purposes. This specification was justified by the fact that economic theory under certainty implies that both producers and consumers respond directly to changes in relative prices rather than changes in nominal prices. But the imposition of such specifications is debatable since economic theory under risk implies that decisionmakers may respond to nominal prices as well as absolute prices. Nevertheless, the use of relative or deflated prices for econometric purposes has persisted because experience with deflated price models has tended to dominate nominal price models, particularly in postsample periods.

One may question, however, whether this use of relative vs. absolute specifications has been carried far enough. The practice of

deflating prices by some general price index has become quite common (although it is not clear that use of a general price index in the denominator of a price relative always outperforms the use of a price of a closely related good). But the use of quantity relatives in policy models is a much less common practice. The use of quantity relatives, as well as price relatives, can often better facilitate comparisons both across time periods and economic units (decisionmakers, counties, states, countries, etc.) and often reduces the number of coefficients that must be estimated. In addition, when alternative policies are actually evaluated, relative measures ("ratios" or "differences") will simplify the comparisons.

By specification in terms of relatives, models often turn out to be independent of units of measurement and are thus formulated in terms of the basic conceptual unit of economic measurement — elasticities (quantity as well as price elasticities). In this context, the estimated structure of the model is likely to have greater longevity of application. This has been borne out by experience with respect to the use of price relatives. When all prices tend to increase together with inflation, the use of price relatives removes the effects of inflation on several prices in order to increase comparability across time periods. However, in a growing economy, all quantities also tend to increase together with the expansion of the economy. Thus, the use of quantity relatives should also tend to increase comparability of several quantities across time periods in a growing economy. The same considerations for both prices and quantities also make sense in comparing across economies (counties, states, countries, etc.) and also appear to offer even greater advantages in the context of cross-section data where units of measurement may not be comparable or where general price levels or economy sizes may greatly differ.

Experience in some preliminary work on the effects of the International Sugar Agreement may serve to illustrate this point in the context of time series data. In data over only a 10-year period from 1970 to 1980, the size of the world sugar market in terms of production and consumption increased from around 70 million metric tons to around 90 million metric tons. A change in stock levels of, for example, 5 million metric tons is often more crucial in a market with 70 million metric tons of consumption than in a market with 90 million metric tons of consumption. To reflect this difference, a model stated in terms of quantity relatives is more effective.



With this approach, we found that a model may be stated in terms of fewer estimable coefficients without losing tracking power. Furthermore, we found that postsample predictability was improved through the use of quantity as well as price relatives.<sup>5</sup>

As a precaution in applying this principle, however, one must bear in mind complexity costs which may be related to certain nonlinearities that may be introduced into a system (depending upon functional forms). That is, if a model is stated in terms of price and quantity relatives involving several equations, then the use of any identity relating quantity variables may make the resulting system of equations nonlinear and, thus, associated complexity costs would increase. One way to avoid this problem is to specify quantity relatives so that denominators are exogenous variables. This is essentially the traditional approach that has been used with price relatives. In addition, if general equilibrium relationships (rather than partial equilibrium relationships) are estimated, then it may not be necessary to use groups of equations together with identities for policy impact purposes (see Principle 6). In this way, some of the complexity costs associated with the use of quantity relatives may also be outweighed by the associated benefits of accuracy and model longevity.

***Principle 6: General equilibrium rather than partial equilibrium relationships should be emphasized in the structure of a policy model.***

In the early days of econometric modeling, researchers attempted to estimate single-equation relationships describing supply or demand in a particular market. Following a traditional Marshallian approach, the supply or demand relationship was conditioned upon all of the determinants (*ceteris paribus* conditions) which were econometrically discernible. The problem with such simple models is that they reflect behavior only in the market in question and ignore possible repercussions of policy changes which may take place in other markets. Also, they ignore possible feedback effects in the market in question from repercussions in other markets. For example, when a price support is increased on a feed grain, one may

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5. To facilitate the merger of programming and econometric approaches, Rausser, Just, and Zilberman have presented some preliminary work on the microeconomic foundations of the merger.

obtain an estimate of the increase in feed-grain production based on a simple feed-grain supply equation. However, an increase in feed-grain prices may have substantial effects on livestock producers through higher feed prices, and the higher feed prices may lead to a reduced quantity demanded by the livestock sector. These effects, of course, could not be captured in a single-equation model.

In response to this problem, policy modelers began to add additional equations describing effects on other markets. The search for all of these effects has at times seemed endless as policy models have grown to hundreds of equations. Conceptually, these models are appealing since they allow for the feedback effects of repercussions in other markets. However, the cost has been high. Large, complex models require simultaneous solution techniques to assess the potential effects of policy changes. Also, a serious error in estimating an equation even in a market other than the one in which the policy changes are imposed can invalidate all of the results forthcoming from the model.

To exemplify the distinction between general and partial equilibrium approaches to policy modeling, consider the case where one wishes to model the beef-marketing sector to determine the effects of grain price policy and conceptualizes the problem (simplistically, for purposes of exposition) as follows. Consumers decide how much beef to consume,  $Q_b^d$ , based on the retail price of beef,  $P_b$ , and income,  $Y$ :

$$Q_b^d = Q_b^d(P_b, Y). \quad (1)$$

The beef-marketing industry (meat packers and retailers) decide how much beef to supply,  $Q_b^s$ , based on the retail price, the price they pay for fat cattle,  $P_f$ , and the wage rate of labor,  $P_L$ :

$$Q_b^s = Q_b^s(P_b, P_f, P_L). \quad (2)$$

The beef-marketing industry likewise decides how many fat cattle to buy,  $Q_f^d$ , based on the same prices:

$$Q_f^d = Q_f^d(P_f, P_b, P_L). \quad (3)$$

Feedlots decide how many fat cattle to sell,  $Q_f^s$ , and how many feeder calves to buy,  $Q_c^d$ , based on the price of fat cattle, the price of feeder calves,  $P_c$ , the price of grain,  $P_g$ , and the number of cattle placed on feed in a previous time period,  $N_{-1}$ :

$$Q_f^s = Q_f^s(P_f, P_c, P_g, N_{-1}) \quad (4)$$

$$Q_c^d = Q_c^d(P_c, P_f, P_g, N_{-1}). \quad (5)$$

Finally, cow-calf operators' supply of feeder calves,  $Q_c^s$ , depends on the price of feeder calves,  $P_c$ , and the price of hay,  $P_h$ :

$$Q_c^s = Q_c^s(P_c, P_h). \quad (6)$$

In addition, the system of supply and demand equations is closed by equilibrium relationships:

$$Q_b^d = Q_b^s, \quad Q_f^d = Q_f^s, \quad Q_c^d = Q_c^s.$$

Using the partial approach, the above six nonidentity equations would be estimated directly as specified. In the context of this system of equations, however, one can solve for general equilibrium specifications in each market. In doing so, one must keep clearly in mind the difference in true general equilibrium specifications and general equilibrium specifications in the context of a particular model specification. It is the latter possibility which offers advantages in policy modeling. In reality, the general equilibrium demand for beef may depend on factors underlying production conditions of many other commodities, influences on tastes and preferences for other goods, and a seemingly endless host of other factors. In the context of examining policies using the model above, however, the equilibrium effects obtained by solving the system of equations under several alternative policies (say, high grain prices and low grain prices) would not depend on such a wide array of factors; in point of fact, the effects could depend only on  $Y$ ,  $P_L$ ,  $P_g$ ,  $N_{-1}$ , and  $P_h$  (or such changes as have well-defined effects in the context of a market model — e.g., a tax or quota) since those are the only exogenous factors in the system.

Following the abstraction of reality set forth in the above system of equations, the general equilibrium demand and supply for beef at the retail level are of the form

$$Q_b^d = \bar{Q}_b^d(P_b, Y) \quad (7)$$

$$Q_b^s = \bar{Q}_b^s(P_b, P_L, P_g, N_{-1}, P_h). \quad (8)$$

respectively; the general equilibrium demand and supply of fat cattle are of the form

$$Q_f^d = \bar{Q}_f^d(P_f, Y, P_L) \quad (9)$$

$$Q_f^s = \bar{Q}_f^s(P_f, P_g, N_{-1}, P_h). \quad (10)$$

respectively; and the general equilibrium demand and supply feeder calves are respectively of the form

$$Q_c^d = \bar{Q}_c^d (P_c, Y, P_L, P_g, N_{-i}) \quad (11)$$

$$Q_c^s = \bar{Q}_c^s (P_c, P_h). \quad (12)$$

To clarify some of the advantages of estimating equations in the general equilibrium form, suppose one is attempting to determine the effects of a grain price policy (with explicit effects on grain price) on the market transactions of consumers of beef. Using the partial approach and assuming all equations are specified linearly with constant terms (for simplicity of exposition), one must estimate 24 coefficients in six equations, whereas using the general equilibrium approach would require estimation of only nine coefficients in two equations [equations (7) and (8)]. Estimation of equations (9)-(12) would not necessarily be required. Solving for equilibrium prices and quantities is thus much simpler in the latter case because of the reduced dimensions of the problem (therefore corresponding to the guidelines of Principle 3). Finally, Just, Hueth, and Schmitz (1982) show that examining policy objectives, such as consumer and producer surplus using equilibrium supply and demand relationships in a single market, attains the same results in theory as summing results over all relationships in a system of partial specifications. Hence, policy analysis can also be simplified greatly (although with loss of distributional detail on the producer side in this case) while making the results subject to errors of estimation in fewer parameters.

Admittedly, the model specified above is quite simple but, nevertheless, illustrates the advantages of the general equilibrium approach to specification, estimation, and policy analysis. In the context of any specification of a system of equations describing a number of markets, however, one can, in principle, solve for equilibrium supply and demand equations for a particular market which describe, say, equilibrium supply price, demand price, quantity demanded, and quantity supplied as a particular policy instrument (e.g., a price support, quota, subsidy, etc.) is altered in the market. In practice, these relationships may or may not be simple to estimate as illustrated above depending on the complexity of the complete model specification. If not, however, it is often practical to estimate semiequilibrium relationships which correspond to equilibrium specifications of submodels.

For example, in the above example one may be considering effects of grain price policy in a larger model which also describes behavior in the grain market according to the equations:

$$Q_g^d = Q_g^d(P_g, P_c, P_f, N_{-1}). \quad (13)$$

$$Q_g^s = Q_g^s(P_g, A_{-1}, I_g, P_n). \quad (14)$$

$$Q_n^d = Q_n^d(P_n, P_g, A_{-1}, I_g). \quad (15)$$

$$Q_n^s = Q_n^s(P_n, P_p). \quad (16)$$

where

$Q_g^d$  = quantity of grain demanded

$Q_g^s$  = quantity of grain supplied

$Q_n^d$  = quantity of nitrogen demanded for fertilizer

$Q_n^s$  = quantity of nitrogen supplied for fertilizer

$A_{-1}$  = acreage planted to grains in a previous time period

$I_g$  = inventory of grain

$P_n$  = price of nitrogen used for fertilizer

and

$P_p$  = price of petroleum.

In this case, the general equilibrium demand and supply of beef in the context of the entire model composed of equations (1)-(6) and (13)-(16) are

$$Q_b^d = \tilde{Q}_b^d(P_b, Y) \quad (17)$$

$$Q_b^s = \tilde{Q}_b^s(P_b, P_L, N_{-1}, P_h, A_{-1}, I_g, P_p). \quad (18)$$

respectively, whereas the equilibrium specification for the beef market in equations (7) and (8) is a semiequilibrium specification which considers only equilibrium adjustments in the beef-marketing sector for given grain price. If, because of complexity (too many coefficients to estimate in a single equation) equation (18) is impractical to estimate, then the entire model in equations (1)-(6) and (13)-(16) could be replaced by one containing several **semiequilibrium** relationships, e.g., equations (7) and (8) plus the following semiequilibrium representation of the grain market above:

$$Q_g^d = \bar{Q}_g^d(P_g, P_b, P_L, N_{-1}, P_h) \quad (19)$$

$$Q_g^s = \bar{Q}_g^s(P_g, A_{-1}, I_g, P_p). \quad (20)$$

Thus, the model is reduced from one with 10 nonidentity equations with 42 coefficients to one of four nonidentity equations with 20

coefficients (assuming linearity with constant terms) while still reflecting the same phenomena. The complexity of the empirical model is thus greatly reduced although the underlying conceptual model does not involve any greater degree of abstraction.

Alternatively, depending on the policy objective, one could examine general equilibrium specifications for a different market. For example, the general equilibrium specification of demand and supply for the grain market in the context of the overall model in (1)-(6) and (13)-(16) is

$$Q_g^d = \tilde{Q}_g^d (P_g, Y, P_L, N_{-1}, P_h)$$

$$Q_g^s = \tilde{Q}_g^s (P_g, A_{-1}, I_g, P_p).$$

respectively, and is apparently no more complex than the **semiequilibrium** equations in (19) and (20). As implied by the work of Just, Hueth, and Schmitz (1982), estimates of these equations are appropriate for examining aggregate welfare effects associated with any standard intervention in the grain market for the entire group of decisionmakers whose behavior is reflected by equations (1)-(6) and (13)-(16).

*Subprinciple 6.1: In policy model analysis, the emphasis should be on obtaining the most accurate conditional probability distributions for the relevant performance measures (after accounting for complexity costs).*

This subprinciple is consistent with and implied by the principles of the post-Bayesian approach. The criteria used in estimating a model often do not correspond appropriately to the policy goals of interest in predicting the effects of alternative policies. For example, in an econometric model, each of the equations is usually estimated with the criterion of minimizing the sum of squares of errors in a sample period. That is, in the feed grain/livestock case, one may minimize the errors in forecasting the quantity of feed grains produced given the level of a price support in one equation, minimize the errors in forecasting the quantity of feed grain consumed by livestock producers given the price of feed grains in other equations, etc. For policymaking purposes, however, one may be more concerned with the effects of the price support on the real income of feed-grain producers and livestock producers and consumers. Since the criterion in conventional estimation does not focus on accuracy in the latter forecasts, the value of the policy

model may be far less than is potentially possible.

As a possible means of overcoming these problems as well, greater emphasis on estimation of general equilibrium relationships rather than partial equilibrium relationships offers promise. Simulation and forecasting in a model with many partial equilibrium relationships allow errors to propagate through a system of equations upon solution of the model, whereas the statistics of fit in the criterion of estimation of a general equilibrium relationship are more directly applicable to the forecasting mode.

## **Principles of Information Use**

***Principle 7: Policy modeling must provide for the use of intuition, both in model development and updating; strong intuition should override causal implications of coincidental data in model development.***

Data use in policy models can never be allowed to become a substitute for sound, hard thinking about assumptions and alternative courses of action. To enhance the believability of policy models and their effective use by policymakers, new, potential local approximations must be continually investigated and evaluated. Prior information facilitates this investigation and evaluation. To accommodate structural change and track new and changing developments, the weighting of prior information must be revised constantly in policy models.

The relative weightings on prior information vs. sample information must depend upon the degree to which relevant policy instruments have been observed. When no prior experience (data) is available on the effects of particular policy instruments, even greater weights must be placed on intuition. New institutional designs involving discrete choices across alternative policy sets will lead to greater weight on intuition than will policy evaluations for instruments that have been applied under existing institutional designs. In this setting, the following subprinciple arises.

***Subprinciple 7.1: Ample opportunities should be given for judgmental inputs, especially those provided by commodity specialists.***

Subprinciple 7.1 suggests that the expertise and software must be developed for cost-effective interactions of policymakers and com-

modity specialists with the policy model. The basic premise for introducing information from commodity specialists into the analysis provided by policy models is given in Johnson and Rausser. To facilitate these interactions, experimentation with alternative information bases (various weightings across prior intuition and sample data) must be accomplished easily. Interactive software must be developed and maintained which allows policy scenarios to be developed both with and without the subjective input of commodity specialists. The sensitivity of such policy scenarios to the subjective input of commodity specialists should, indeed, be valuable for a number of purposes. To the extent that the information provided by commodity specialists is separable from other information sources for the constructed policy model, improved or more precise conditional policy distributions will be obtained for relevant performance measures.

***Principle 8: Use of greater weight on more recent data in policy model estimation should be seriously considered.***

The intuition of Principle 4 dictates that we are living in a world with constant structural change. We must accept the premise that models used for policy purposes are abstractions and approximations of reality. Thus, as the economy changes from time to time, one may find that not only should the structure used in the abstract model be changed but also, and perhaps more often, the models should be calibrated more closely to recent data. That is, to accommodate structural change and to track new and changing developments, the weighting of sample data must be revised constantly in updating policy models. In a world in which underlying forces change in an unpredictable way from time to time, this principle is formally supported by the results of Kalman filtering and adaptive stochastic control theory. In this framework, one does not view the world as having discrete structural changes between reasonably long periods of constant structure. Rather, structural change is viewed as a process which takes place constantly but with small and subjectively random increments. In this context, recent observations are far more valuable in predicting the future than are observations in the distant past although distant observations are still useful. Moreover, this consideration emphasizes the importance of continual maintenance and updating of policy models.

Principles 7 and 8, when combined with 3, 4, and 5, have some



direct implications for assessment of the tradeoffs between the use of information from (1) economic theory, e.g., homogeneity, symmetry conditions, etc., (2) nonsample information, such as expert judgment, (3) recent sample data, and (4) the entire sample. The assessment of these tradeoffs must be determined in large part by the purpose for which a policy model is constructed (Principles 1 and 2). In general, however, the credibility of policy models will be enhanced by giving the most serious considerations to (1), followed by (2), (3), and (4) in that order. This ordering follows from currently available data support systems and the "local approximating" nature of quantitative models.

***Subprinciple 8.1: Model maintenance and updating are continuous processes for which explicit expertise must be fostered.***

Maintenance and updating must take place not only for growth and continual quality enhancement of policy models but also to avoid deterioration of the information in a policy model. Again, these arguments underscore the importance of viewing development and use of policy models as a process and not as the creation of a product.

***Principle 9: General purpose data sets rather than general purpose models should be emphasized.***

The use of the post-Bayesian approach, the need for constant revision of the weighting of sample information vs. intuition in model specification, the need to incorporate summary variables in policy models, and the need to evaluate new and different policy problems from time to time all dictate the need for an all-purpose data set rather than an all-purpose model. Two of the greatest problems policy modeling has faced historically have been the extreme complexity needed in a model in order to be able to address a wide set of issues unforeseen at the time of model construction and the extreme costs imposed by this complexity in model development and use. As evidenced by the experience of the Forecast Support Group in the USDA, complex models take years to build. Such models can often not be brought to fruition before some of the pressing issues have passed. Furthermore, even though a model may be made very large and complex, it may still not include the appropriate focus to evaluate some policy issue which is unforeseen at the time of the model development.

An alternative approach is to develop small policy models with specific policy focus at the time that specific policy issues surface as suggested by Principle 1. In order to pursue this approach, however, models must be developed rapidly if they are to have any bearing on the current policy considerations. Rapid model development can be facilitated by the maintenance of an all-purpose data set. That is, one of the largest costs both in terms of money and time involved in model construction is the acquisition of data and development of a data-management system and appropriate software for estimation. With the existence and maintenance of an all-purpose data set, a data-management system, appropriate estimation software, and a portfolio of previously constructed specific purpose models, a policy analyst can sit down at a computer terminal and develop a model with specific focus on the issues at hand in a matter of a few days. This has been borne out by the authors' own experience in which a model of moderate complexity (34 equations with 52 variables) was developed in less than a week through the use of a general-purpose data set.<sup>6</sup>

The maintenance of an all-purpose data set is also important in facilitating the use of summary variables in policy model construction. That is, with the maintenance of an all-purpose data set, the means of constructing price or quantity indices as the need arises is available. Thus, a policy analyst is less likely to be forced to use only representative variables in policy model construction.

No matter how general a general purpose model is, questions always seem to arise that are beyond the scope of the model. Moreover, what some would define as general purpose models others would argue are specific purpose. The essential point, however, is that actions which result in increasingly more general purpose models place insufficient weight on complexity costs. In this regard, the experience of the U.S. Department of Agriculture policy modeling effort speaks for itself.

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6. The work by Feder, Just, and Ross on international lending policies of the World Bank also illustrates the preferred postsample predictability of a model with quantity relatives in the context of cross-section data.

***Subprinciple 9.1: The principles of post-Bayesian analysis are also appropriate in governing the design and maintenance of a general purpose data set.***

The design and maintenance of an all-purpose data set requires that some framework be developed to determine which variables should be initially included in such a data set and which variables should be added or deleted from a data set as additional experience is gained. Formally, these problems can be solved using the principle of preposterior analysis. That is, data base inclusions, augmentations, or deletions should be based upon intuition and judgment as well as experience in assessing the cost of maintenance vs. the potential policy modeling benefits. In the case of data set maintenance, however, these issues must be decided based on the entire collection of policy models and potential policy models rather than on the basis of a single policy model.

### **Principles of Policy Selection**

***Principle 10: Policies should be formulated with an appropriate degree of learning in mind.***

If policy models are to become an important source of information in policy selection, then, in some instances, the policies should be determined so that a greater amount of information can be ascertained from observation of their effects. Principle 10 is supported formally by adaptive control theory which places some emphasis on the value of experimenting with an economy. The cost of such experimentation may be more than recovered by the benefits of setting the policy controls taking into account the potential value of improved perceptions of the system under examination.

Principle 10 is also related to the earlier discussion on the form and shape of much of governmental intervention in the agricultural economy. The form of this intervention in effect has made policy modeling difficult. Moreover, policies resulting from such intervention have placed, as expected, little value on information that might be generated from quantitative models. However, the "tidal wave" effect and the importance of path vs. magnitude emphasized by Hathaway (this volume) can be effectively managed by effective implementation of Principle 10 and the following subprinciples.

***Subprinciple 10.1: Policy alterations should be imposed whenever possible by revising existing policy instruments rather than by determining a new set of policy instruments, subject to political feasibility.***

Currently, historical agricultural policies generally result in instruments which are imposed only if certain fixed barriers or trigger points are reached. For example, acreage allotments and price supports represent fixed quantity and price barriers; set-aside requirements are imposed depending on whether the Secretary of Agriculture determines that some theoretical trigger point has been crossed. With such policy instruments, the effects of various policy controls may be observed in some years and not in others. Hence, less information is gained than if policy instruments were effective in varying degrees over the complete sample record. Data generated from such policy regimes call for analysis by means of qualitative econometrics thus greatly increasing the complexity costs of analysis and reducing the value of information forthcoming.

***Subprinciple 10.2: Depending on administrative costs, policy instruments should be exercised in a smooth and continuous fashion conditioned on market conditions.***

Greater value of feedback information from policy modeling would result from the implementation of Subprinciple 10.2. For example, government price-supporting operations for, say, wheat could be carried out by means of government purchases of 1 million bushels of wheat for every 1 cent per bushel the market price is below some target price (or, conversely, selling 1 million bushels of stock for every 1 cent per bushel the market price is above the target price). Similarly, a 1 percent set-aside could be required for every 20 million bushels of wheat in government reserves. Such policies are generally more consistent with economic efficiency in contrast to the form of existing policy instruments which are conditioned on fixed barriers and trigger points. They have the additional benefit of reducing policy risk and allowing farmers to reduce allocative inefficiencies. In other words, farmers are more able under such policies to correctly anticipate government actions based on their own assessment of market conditions. Too often, analysts concentrate on instabilities and distortions in the private sector and offer policies which, when implemented, lead to instability of the political administration system. In essence, the risk faced by individual farmers is

transferred from economic markets to political markets.

As most agricultural policy instruments have been exercised historically, their effectiveness is largely dependent on market conditions. Thus, under many market regimes, no information is generated on the effects of the policy instruments. However, when policy instruments are exercised in a smooth and continuous fashion, governmental actions behave much as a demand or supply curve that can be observed at every time period. Thus, the information on the effects of policy instruments can be compiled with less empirical difficulty.'

## Conclusion

We have offered a number of principles that may be interpreted as rules or a code of conduct which will allow the potential for quantitative policy models to be realized. They emphasize the tradeoffs that should be examined as we move from more conventional models (those with descriptive, explanatory, or forecasting purposes) to operational and usable policy models.

In the final analysis, of course, major benefits from modeling public policy problems depend critically upon the sound judgment and experience of public decisionmakers and the analyst involved. Only through such judgment and experience will it prove possible to balance the value of simplicity with the value of accuracy. Given the appropriate balance, the principal benefits of quantitative modeling will be achieved. These benefits include: *inter alia*, forcing the users or public decisionmakers and the analyst to be precise about perceptions of the system they are attempting to influence and testing these perceptions with available evidence, providing structure to the analysis, extending the policymakers information processing ability, facilitating concept formation, providing cues and insights to policymakers, stimulating the collection, organization, and utilization of data, freeing the decisionmaker and analyst from a rigid mental posture, and becoming an effective tool for negotiation and bargaining and as a basis for persuasion.

The above benefits can accrue to policy models provided the obstacles to achieving such potential benefits are avoided — obstacles such as timeliness, solving the wrong problem or solving the

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7. For further elaboration of improved policy controls, see *lust*; and, for policy uncertainty, see Gardner et al., and Rausser and Stonehouse.

right problem too late, allowing improper expectations to form by not clearly delineating what the model can and cannot accomplish (the role of modeling efforts should always supplement rather than supplant the normal decision processes), and failure to differentiate the characteristics of the policymaker or user from the analyst (these are often very different types of people with different roles, responsibilities, expertise, cognitive style, etc.). The rules or principles advanced in this paper are an attempt to facilitate avoidance of the major obstacles in gaining the promised benefits of policy modeling efforts.

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## Commentary

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*Kenneth R. Farrell*

It is tempting to present a mini-paper of my own on the theme of this session, "Using Models in Policy Foundations." My temptation derives not from any major disagreement with the Rausser-Just paper but a desire to relate my personal views on the shortcomings and successes in application of quantitative economic/statistical models to policy formulation and program administration in the USDA in the past decade. However, I will dutifully resist the temptation and offer only a few perspectives drawn from my experience as a point of departure for my specific comments concerning the Rausser-Just paper.

1) There is innate suspicion if not distrust of formal economic models on the part of many policymakers. The derivation of that suspicion derives from multiple sources, e.g. poor quality and reliability of model estimates, poor specification of models (omission of variables important to policymaker), poor communication among policymakers and analysts, and the tendency of policymakers to be their own best economists.

2) Policy formulation is not generally a dispassionate, intellectually pure process — reason and logic do not always prevail. Further, the time horizon is usually short in the decisionmaking context of policy officials. Concern usually centers on the immediate future — it is a rarity to confront true policy watersheds. Formulation of most policies proceeds incrementally from present or recent past policies. The implications for policy models and modelers are important:

- Models must have a demonstrated capacity to produce reliable, plausible, conditional forecasts of policy-critical variables over short- or intermediate-run periods.
- Timely data must be readily accessible and the model must be

capable of being turned around quickly, sometimes overnight, to deal with successive iterations and specifications of policymakers. However elegant the model may be in formulation, it is of little value unless it can be turned around quickly.

3) Model output almost invariably requires translation for effective use by policymakers. The surest way to turn off a busy policy official is to dump the whole load. A succinct statement of the tradeoffs among sometimes competing policy target variables — budget outlays, income, employment, and prices, for example — is essential. Sometimes all that is usable or relevant may be a simple multiplier, price elasticity, or flexibility coefficient. Policy officials are primarily interested in the numbers on the left side of the decimal.

4) As Rausser-Just point out, no model or type of model will suffice for all purposes. For some purposes a simple, least squares multiple regression may be adequate to give the policymaker the parameters he needs to assess a particular policy option and the costs of taking, or not taking, a particular decision. In other cases a large **input-output** or simulation model will necessarily undergird the policy analysis.

5) Finally, the results of a model should not stand alone in presentation to policy officials. After all, models are conceptual abstractions of the organization and performance of complex economic institutions. While we have made great progress in our capacity to handle more variables, including policy variables, and in the design of increasingly complex systems of models, we can still only crudely approximate the behavior of individuals and institutions by means of various statistical/mathematical expressions. As the authors indicate, intuition, judgment, experience, and knowledge of institutions and markets must be coupled with model results.

Where does this leave us in context of the theme of this session — "Using Models in Policy Formulations"? Necessary but not sufficient would be an appropriate answer, I suppose.

Now, a few specific comments on the authors' 10 principles.

- I have no disagreement nor elaboration to offer with respect to Principles 1 (explicitly define purposes and goals of policy models), 2 (exploit the experimental role of policy models), or 7 (use intuition in model development and updating). The authors view policy modeling as a process, not the creation of

a product. To me it is both — the process should lead to useful, applied product, albeit a changing product over time.

- As regards Principle 3 (use post-Bayesian analysis in consideration of complexity and inaccuracy costs) I think I agree. I have seen little direct useful application of Bayesian principles in agricultural policy research and analysis. Therefore I must agree with post-Bayesian analysis. And also, possibly, pre-Bayesian analysis!
- With respect to Principle 4 (policy models should be designated to accommodate and track structural change) and Principle 5 (theoretical structure in the model specification should depend on the amount of historical information), I would merely add that the same principles apply to other types of models — descriptive, explanatory, forecasting, etc.
- One of the most important principles is No. 9 (general purpose data sets rather than general purpose models should be emphasized). I concur with the authors statement that, "Two of the greatest problems policy modeling has faced historically have been the extreme complexity needed in a model in order to be able to address a wide set of issues unforeseen at the time of model construction and the extreme costs imposed by the complexity in model development and use." As they properly state, "Rapid model development (small policy models) can be facilitated by the maintenance of an all-purpose data set." Generally, economists give far too little attention to the quality and suitability of the data used in their analyses. I agree further with the authors on the need for a carefully specified framework to guide development of data. Current data are inadequate in several respects for the types of policy research discussed by Raussler and Just. That problem is likely to become more severe considering the changing organization of agriculture and the conceptual deficiencies in existing data systems.

Although I agree with Principle 9, it is not clear that it is consistent with Principle 6 (emphasize general equilibrium rather than partial equilibrium relationships). Some clarification would be appropriate.

With respect to Principle 10 I can only sympathize with the authors' concerns — the form of government intervention in the agricultural economy has made policy modeling difficult. Ideally,

"policies should be formulated with an appropriate degree of learning in mind." But in the real world, I expect policymakers will continue to enact policies without reference as to whether they enhance learning by economists.

The Rausser-Just paper contains many useful insights and recommendations. I appreciate the opportunity to discuss it with you.

## Constraints on the Modeling of Agriculture and How They Might be Relaxed

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*Bruce Gardner*

A presumption of my title is that the modeling of agriculture is in some important sense constrained. I believe this presumption is correct, but that it is less obvious than might be supposed what the constraints are. Therefore, I will spend a good part of my time discussing the nature of the constraints of policy modeling before going on to discuss how they might be relaxed.

### **The Output of Models**

What is it that is constrained when modeling is constrained? What is the output that is not forthcoming? The output is quantitative conditional predictions. The relevant constraints reduce the accuracy of such predictions.

The output I am concerned with is not forecasts of the future and it is not advice in the normative sense. A paradigm of the output of policy modeling is the following: under the assumption that policy A is undertaken, the differential consequences for variables X, are generated. By "differential consequences" I mean, what difference the policy A makes in the X,; thus the output is like a regression coefficient. It is not a forecast of the future values of X,.

In the policy process such information is not the only, or perhaps even the main, valuable ingredient. Policymakers are also often interested in forecasts and in normative advice. Forecasts come from *experts*. For example, Schnittker (1981) sees for the 1980s a "shift to commodity shortages," hence rising real agricultural prices, and predicates his policy discussion on such a situation. This advice may be correct, but it is given not as a conclusion from a policy model but as the judgment of an expert.

Normative advice, on a professional basis, comes from "intellectuals," construed broadly. James Q. Wilson (p. 46) summarized a discussion of policy intellectuals as follows: "In short, what intellectuals chiefly bring to policy debates, and what chiefly accounts for their **influence**, is not knowledge but theory." This distinguishes intellectuals from experts (the intellectuals supplying theories while experts supply facts), but I want to go further and distinguish normative theories from positive theories (like regression models) and confine the output of **policy analysis** as discussed here to the latter. In this sense, our sights are set a bit low; we are discussing a task more humdrum than attempting prescience as an expert or providing leadership as a guru. Part of the reason for this is that such policy modeling, done well, is a scarce commodity. In working for both Ford's and Carter's Council of Economic Advisers, I found a notable lack of demand for anything I had to offer as an expert or an intellectual, but a great deal of demand for answers to questions of the form: if we do A, what will happen to X? Being also a bureaucrat, I soon found how to pass these questions on to others. In the end, I very seldom found answers in which one could have much confidence. Why? What makes these questions so intractable? This brings us back to the constraints on modeling.

Before moving on to discuss the constraints, I want to emphasize another aspect about the output, the form of the answers; namely, they must usually be quantitative to be of value. Consider an example: suppose it is proposed to subsidize U.S. exports of corn. From elementary economics we expect the U.S. price of corn to rise. It doesn't take an expensive modeling effort by a Ph.D. economist to draw this conclusion. The point of having professional-caliber policy research is to provide the best possible estimates of how **much** the corn price is expected to rise, and for **what period** of time. Moreover, in some instances we need quantitative estimates to answer seemingly qualitative questions. For example, will a corn export subsidy increase or decrease the acreage of other feed grains? To answer this question we need quantitative information: the relative magnitudes of the cross-elasticities of supply and demand between corn and other feed grains and estimates of acreage response in other feed grains to corn demand shifters. In short, policy research that is worth doing professionally today almost inevitably involves quantitative modeling of agriculture.

Returning to the theme of constraints, the issue is, what prevents the accuracy of quantitative if-then statements from being greater than it typically is? The preceding discussion is meant in part to convince you that this is essentially the same as asking how we may get better estimates of a regression coefficient in an econometric model. Let us now proceed to examine the topic in detail.

My first hypothesis is that the gaps in our knowledge today do *not* stem from a lack of appropriate econometric methods. The profession has come a long way from early studies such as Henry Schultz (1938) to recent work such as Chen (1977), Grennes, Johnson, and Thursby (1977), Burt, Woo, and Dudley (1980), Goodwin and Sheffrin (1980), or Gallagher et al (1981). Indeed, some of the most sophisticated approaches to estimation are being tested out currently, as was also the case with earlier advances, on agricultural commodity markets. Examples are work on rational expectations models tested on broilers (Huntzinger 1979), multiple time series analysis tested on 19th-century hogs (Box and Tiao 1977) or cattle (Nerlove, Grether, and Carvalho, 1979), and the application of dual theory to agricultural production (Lopez 1980, Brown and Christensen 1980).

As yet, none of these studies is capable of providing new answers to questions most important for policymaking. In fact, it is not even clear that recent sophistication has provided any real improvement in estimation of traditional parameters such as own supply and demand elasticities. For example, Chambers and Just (1980) criticize Grennes, Johnson, and Thursby (1977) for using an insufficiently general theoretical model, not allowing for enough cross-commodity price influences. Yet in their own empirical work, Chambers and Just (1981) omit cross-price effects in their export demand equations. Thus, they were not put off by their own theoretical strictures. Others could possibly have been, and if so, empirical work would probably have been hindered, not aided, by theoretical sophistication. Nonetheless, the work of Chambers and Just and others should ultimately prove helpful, precisely because of disputes such as theirs with Grennes, Johnson, and Thursby, which serve to sharpen the profession's collective thinking. Moreover, past theoretical advances in econometrics particularly, have enabled us to understand more fully the pitfalls (and sometimes the unanticipated virtues) of crude OLS estimating equations and classical significance tests, for example, in time series data. But while we are today

in a better position to avoid errors of inference than in the past, *i.e.*, to avoid accepting false answers as true, we are still faced with a disheartening lack of answers. To suppose otherwise is to succumb to an "illusion of technique."

Notwithstanding advances in analytical methods, there are few policy questions to which agricultural economists can give confident quantitative answers. One can give many reasons for such failure, but I believe the most generally constraining factors are, first, a pervasive lack of appropriate data; second, the limitations of economic theory; and, third, a general inefficiency in the mobilization of economic expertise in policy analysis. The lack of data is not only a matter that the appropriate surveys have not been made or that facts have remained unpublished, but more fundamentally that the course of events has not generated the states of affairs in which one may observe the relevant data. The limitation of theory is not that it is wrong but that so many policy questions involve issues to which theory is inapplicable.

The best way to explain my views on these constraints is by means of examples, to which I now turn.

### **The Farmer-Owned Reserve Program and Optimal Storage**

This program was labelled a success within a year of its introduction (U.S. Council of Economic Advisers). Since the program was intended to increase grain stocks and stabilize prices, success presumably means that stocks were increased and prices stabilized. I was involved with an effort to assess the effects of the FOR (U.S. General Accounting Office), in the course of which considerable effort was devoted to quantitative estimates of how much was added to stocks, and how much prices were stabilized, by the program. Of the many statistical tests attempted, some showed no significant effects and most only small effects. Certainly there was no basis for any strong conclusion of "success" in any meaningful sense. Undoubtedly, there are good *a priori* reasons to expect subsidy payments to storage to increase storage, and to expect increased storage to stabilize prices. But the empirical evidence, the value added of policy research as a professional activity, was too weak to support any firm judgment.

What were the operative constraints? Not a lack of appropriate theory, although as always, almost every analyst makes mistakes of some kind in applying theory to data. The basic problem is that



there were not enough experimental data available when policymakers wished to assess the program. The basic idea is to stabilize prices between large-crop and small-crop years by means of carryover stocks between crop years. But by definition we can observe only one crop, and one carryover per year. So we can't be sure how much of an observed change in stocks is attributable to the FOR program and how much to changes in other variables. In principle, econometric modeling could solve this problem by providing estimates of the effects of other variables, so that we can subtract out their effects and attribute the rest to the FOR program. Unfortunately, the errors in such models are too large to make this approach work convincingly. (For detailed discussion see U.S. General Accounting Office, Appendixes.) In the end, there seems no substitute for observing several years under the FOR for comparison with the pre-FOR (and possibly a post-FOR) period.

Storage of commodities for the purpose of price stabilization is an area where economists have been called upon to provide advice as to optimality in policy. That is, there is a demand not only for positive-economics models as just discussed, but also for normative models. Policymakers have long been inclined to the view that price stabilization is a good thing, without always being clear about what was good about it, or how much is best, or how one evaluates gains in price stability as against other good things. Here economic analysis has been weak, in my opinion, because of weaknesses of economists. The analytical models were led down an unfortunate path by the seminal work on price stabilization of **Massell** (1969). The basic weakness of Massell's approach is that it presumed to specify the social gains from price stabilization without incorporating storage costs, and private storage activity, into the model. In this **Massell** was followed by **Just** (1975) and by **Houck and Subotnik** (1976), the latter of whom were in turn severely criticized by **Helmberger and Weaver** (1977). However, **Helmberger and Weaver's** model was also fundamentally flawed, as spelled out by **Ippolito** (1979). An irony in this literature is that the theoretically appropriate normative model for optimal storage had been developed some 20 years earlier by **Gustafson** (1958), and was updated and developed in works such as **Stein** (1962) and **Pliska** (1973). The point here is not (only) to criticize **Massell**, **Just**, **Houck**, **Subotnik**, **Helmberger**, and **Weaver**, all of whom have done good work in agricultural economics. The general point is a limitation on modeling that derives from our

inevitable limitations as economists faced with quite tricky problems.

Moreover, theoretical optimality is not necessarily practical policy optimality, quite apart from political issues. It is a matter of limitations of knowledge to implement optimal policies. It is easily shown that price-band policies, as most international or national stabilization schemes recommend, are nonoptimal. But the optimal policy can be specified only after parameters such as the elasticity of product supply, the elasticity of demand (including demand for private stocks), the storage-cost function, and externalities associated with price instability are known. In the absence of such knowledge, much simpler storage policies may be optimal, such as a simple subsidy to private storage, because they are more robust in not being *far* suboptimal over the range of our ignorance.

Despite the problems with the Massell approach when extended to storage issues, this literature is important in showing that it is far from obvious that producers will gain from price stability. Although I don't know that his view is shared by many agricultural economists, it may be worth mentioning Cochrane's (1980) apparent misunderstanding of this type of analysis in a recent note. Cochrane seems to believe that the results obtained depend on a uselessly complicated and even frivolous view of how people think and behave. Actually, the question from the producers' side is simply whether they can expect to receive greater or less returns, on average, under stabilized or unstabilized prices. This indeed turns out to be a complicated issue, but the literature is valuable, if for no other reason, in demonstrating the questionable nature of the assumption that rational farmers would favor a stabilization program.

### **The Support Price of Milk**

The support price of milk has been controversial in recent years, and it is controversy on which econometric modeling should have something useful to say. On the normative side, the issues involve the social value of stabilization, which as just discussed is not such a straightforward issue as is sometimes supposed. On the positive-economics side, however, there are basic questions such as: if we raise the support price 10 percent, what will happen to supply and demand, i.e., what excess supply, if any, can we expect. We have models such as Heien (1977), as well as dairy sectors embedded in sectoral models such as Chen (1977). Yet such models seem not to

have nailed down the issues. There is still a fairly wide range of plausible estimates of elasticity of demand for milk, and the elasticity of supply remains largely guesswork.

What is the problem? What is the constraint on our knowledge here? While one can cite theoretical refinements such as risk response in supply analysis or the proper measurement of price expectations that farmers respond to, I believe that a low-brow response to the problem is appropriate — standard explanations of relative commodity and factor prices on the supply side and income, population, and exports on the demand side are the key elements. Nor is the constraint poor quality of data. Prices and quantities in the USDA statistics are not always accurate, as revisions in them and a brief study of their methods of generation make clear, but these data seem to me adequate to indicate the relative scarcity of milk and other products at any given time.

The real constraint, I think, is an insufficient quantity of data. In order to obtain a good feel for supply response to the price of milk, one needs to hold constant 4 or 5 variables at least, among them the price of concentrates, the price of forage, wage rates, the price of cattle, the size of the dairy herd, and its average age. But since these are constantly varying, isolation of the effect of the price of milk requires a substantial number of observations. Even if there were no random errors and we had a perfect linear specification with five variables, we would need six production periods to guarantee an identifiable estimate of supply elasticity. With random errors and uncertain specification, we need much more data in order to obtain an estimate that is at all reliable. But then we have to worry about structural change invalidating the model.

Further difficulties arise when the excess supply at alternative support prices must be estimated. This is the quantity that the government will have to absorb in CCC stocks. It depends not only on both the supply and demand elasticity, but also on shifts in supply and demand. The impossibility of forecasting these shifts can vitiate excess supply forecasts even if we know supply and demand elasticities without error.

One of the most irritating aspects of policy analysis is that an absence of knowledge does not inhibit the production of answers put forth with great confidence. There is a demand for answers, and the supply comes forth. An example for milk is an exchange between Tweeten (1979, p. 82) and Bjornson (1979). Tweeten, citing Man-

chester (1978), says: "Based on 1953-1973 experience, the long-run supply-demand balance is maintained with milk prices about 75 percent of parity." Bjornson takes strong exception to this statement, but cites no relevant evidence. The imitating factor is that since that time milk prices have in fact been supported at slightly more than 75 percent of parity (ostensibly at 80 percent, but in fact somewhat less) and it is becoming clear that this is well above the market-clearing price. The evidence is the large accumulation of CCC dairy stocks in 1980 and 1981. And this was not only predictable, but predicted.

For another example, return to the FOR program. Responding to GAO findings of quite small effects of this program, a farm journal reported the following response from USDA:

"USDA Undersecretary **Seeley** Lodwick disagrees with some key GAO conclusions. Without the reserve, the grain would have been held by nonproducers and prices would have been sharply lower and more unstable, Lodwick charges." The word "charges" is appropriate here, and should be read: "asserted without any supporting reason or evidence." The idea that 100 million bushels held off the market by nonproducers has a sharply different effect on prices than 100 million bushels by producers is a hypothesis that I find implausible. But it could be true. My point is that the appropriate analytical procedure is to try to marshal data and evidence, not to "charge" that one's hypothesis is correct. (I hasten to add that I mean this episode to illustrate a point, not to criticize Mr. Lodwick particularly. In fact, it is not unlikely that his views were not accurately or fully conveyed in the position attributed to him.)

Despite my expressing irritation with the political element in coming to conclusions, it is only from an analytical point of view that one can be critical. From the political point of view, it is not the intention of the dairy program to find the market-clearing price; its intention is to improve the well-being of milk producers. Nonetheless, policymakers can increase the efficiency of redistribution, the more precisely they know the results of the alternatives that they must choose between.

Unfortunately, good analysis is difficult to detect in advance of the outcomes it predicts. In the milk case just cited, the evidence that long-run excess supply was zero at a support price of 75 percent of parity was not really very solid. The point of the earlier discussion about the dairy data is that this situation is unavoidable.

### Regulation of Land and Agricultural Production

Examples of issues in this area are: restrictions upon foreign ownership of U.S. farmland; restrictions upon conversion of prime farmland to nonfarm uses; restrictions upon farming practices, notably pesticide use and livestock waste disposal, for environmental reasons; and worker safety and food quality regulation. In these areas, economic modeling has contributed negligibly to policy formulation, as far as I can tell. What is the **constraint**? Here I believe it is a lack of basic data.

In the regulatory areas, modeling serves as an adjunct to **benefit/cost** analyses. The best-developed models provide information on the cost side. For example, if a pesticide is banned, what will be the consequences for farm prices and output? But the hard questions arise on the benefit side. Often the benefits are reduced probabilities of undesirable events such as killing fish or birds, or esthetic components of the environment, such as how it smells. The first problem in assessing these benefits is that we do not have the data with which to measure the value of avoiding unpleasant odors, or the relationship between rates of pesticide use and mortality of wild game. Obtaining this information is not something that can be done by economic modeling. It is a matter of sampling and experimentation.

However, some regulatory issues turn on, or are importantly affected by, consideration of the "structure of agriculture." This policy area concerns the desirability of fewer, larger farms and of the owner-manager-operator forms of organization as opposed to farming in which these functions are separated. The constraints on our ability to use economic modeling here are rather different. I will consider them by reference to the following issues.

### Regulation of Commodity Pricing

Recently the Packers and Stockyards Administration was a center of concern about the rapid expansion of Iowa Beef Packers, Inc. (IBP) at the expense of smaller scale rivals, particularly in the Northwest. What was the concern? It was the belief that IBP might drive all its competitors out of business and then exploit consumers by monopoly pricing or producers by monopsony pricing (or both). What have policy models to say about this? The concern is not a matter of well documented fact but is a matter of theory. In the industrial organization literature it is the theory of predatory pricing.

Industrial organization is known for its lack of quantitative modeling and well-specified econometric testing, and the theory of predatory pricing is one of the early casualties of increasing rigor in the field in the last 25 years or so. As a result, even the classic Standard Oil case has been reconsidered, with a general tendency to rehabilitate the view that low prices are good for consumers, even if offered by aspiring monopolists. Nonetheless, economic theory cannot yet provide a sure guide to policy in the sense that we cannot be as sure that unhindered entry by IBP anywhere would be good for consumers as we can be that repealing the Meat Import Act would be good for consumers.

G.E. **Brandow** began his survey of post-war policy work by saying: "Farm price and income policy is about an actual world, not an abstraction in which simple, homogeneous resources are frictionlessly allocated to production of want-satisfying goods, free of political influence or the clash of opposing value systems" (p. 209). And he concludes that productive work in farm policy should use "realistic if sometimes necessarily inelegant models" (p. 281). Economists necessarily deal with models and theories. Otherwise they would be only data-gatherers, historians, or journalists. And models are by definition abstractions. Dealing with abstractions places the economist with problems that **Brandow** sees as very serious — they are the constraints on modeling in his view. Accordingly, the natural step in removing the constraints is to develop models in which resources are not simple, not homogeneous, costly to allocate, and subject to political influence. Models which claim to incorporate these complications have in fact been developed and applied. Yet the models which have gone furthest from elegant abstraction to realistic detail have in my opinion been quite unhelpful in policy analysis. The view that I have come to is that undue addiction to simple neoclassical models is not an important constraint in policymaking today. It is true that the lack of relevant theory is a major constraint in assessing the "structure" of agriculture and regulatory and pricing issues in marketing. But this is not to say that the beginning of wisdom is to jettison the supply/demand models that work best in analyzing farm commodity markets once we move past the farm gate.

## Commodity Import Policy

The earlier mention of meat imports as a case where theory has definite predictions brings to mind the fact that it ain't quite so. One hears cattle interests argue that while free trade is in general a nice idea, steps to stabilize imports by restraining excessive foreign supplies of imported meat are necessary to insure a healthy U.S. cattle industry, and that unrestrained imports would drive U.S. cattlemen out of business, after which prices would be higher than ever. Thus, a theory is generated by which restraining imports increases the long-run well being of consumers as well as producers.

Another similarly dubious theory was put forth by soybean interests and presented to Congress by an assistant secretary of agriculture under the Ford administration in 1976, when palm oil imports were seen as a threat to U.S. producers of vegetable oils. The theory is that if imported palm oil drives down the price of U.S. soybean oil, U.S. crushers will have to cover their margins by charging higher soybean meal prices. This will increase the price of meat, so that consumers will be made worse off. Therefore, the argument goes, the welfare of both consumers and producers is best served by restricting palm oil imports.

The failure of analysis here is mainly a failure of will. Incoherent theories with no evidential support are asserted because they generate the conclusion that an agency wants to put forth for political reasons.

## Relaxing the Constraints

There are no easy ways to relax the constraints that have been discussed. Otherwise it would have been done already. I don't see any alternative to the slow process of continuing to invest in data, to accumulate experience, and to develop economic theory and testable hypotheses and to continually try to learn from analytical mistakes. But it may be worth commenting on three particular lines of strategy for relaxing constraints: simulation, experimentation in policy, and analytical shortcuts. These comments are even more subjective opinion than the earlier parts of this paper.

*Simulation* is meant in this discussion to refer to quantitative modeling without data.<sup>1</sup> It is the most popular thing for agricultural

1. The activity of extrapolating from econometric models by varying independent variables and calculating the results implied by previously estimated coefficients thereof is also known as simulation. The following objections do not apply to this activity.

economists to do when asked to answer a question on which a data-based answer is not possible to obtain. My view is that *simulation is almost never a preferred analytical tool in policy research*. Simulation is very helpful in some research contexts, particularly in seeking to understand the functioning of complex technical or mathematical systems. But it is hardly ever helpful in policy analysis. The reason is that when policy is involved, the issues in question are almost never principally ones of complex systems of interactions, but instead turn on unknown responses of human decisionmakers to policy options.

Let me elaborate. Simulation studies have been important in the development of econometric methods by permitting assessment of the practical consequences of departures from standard assumptions, and in the discovery of small-sample distributions of estimators whose properties cannot be derived analytically. Such Monte Carlo studies can generate many drawings from constructed error structures so that the consequences, say, of non-normal disturbances, can be assessed. In physical problems, simulation can also be helpful. For example, a mathematical model of stream flow or soil erosion might not be solvable analytically, but one can simulate a model that may provide an indication of how the physical system would work over a period of years.

But attempts to use programming models to yield information about the consequences of soil loss under alternative export scenarios or energy price scenarios seem to me quite dubious. The same is true of attempts to model the consequences of farm programs. Thus I have to say that I find studies such as the CARD reports on commodity programs of little use in policy analysis. The issues instead turn on the values of key parameters — the elasticity of demand for and supply of land, or of energy — on which simulation in my view can never provide a satisfactory alternative to observation of economic behavior as prices change through econometric modeling of some sort.

The kind of information that simulation can best provide is evidence that changes in a policy variable are likely to be of little consequence. For example, if the wheat release price in the FOR makes no appreciable difference in stocks or grain prices under a wide range of structural and behavioral parameter values, then we may be confident that no great harm, or good, will be caused by a change in the release price. Consequently, to me the most valuable



aspect of simulation is sensitivity analysis. But the baseline or point estimates of effects from even (or especially) the largest simulation models seems to me suspect.

It might be said that the problem being discussed here is constraints on simulation modeling. And since my subject is how to relax constraints, I should not be criticizing simulation models but suggesting how to remove constraints to improve such models. But my point about simulation models can be restated as follows: If we had the information about behavioral regularities and structural parameters necessary to make simulation models useful, then we wouldn't need a simulation model for policy analysis. Thus, I see simulation as a means of relaxing constraints on modeling as in most instances an informational bootstrap operation doomed to irrelevance.

Experimentation is not often proposed by policy researchers, but it is, I think, the means by which most of our lessons in policy research have been learned. We try out a policy and observe what happens.

In the late 1940s we had squadrons of agricultural economists preaching that high price supports would create more problems than they solved (American Farm Economics Association, 1945, 1947). The federal government eventually came to act in acceptance of this view. But it was only in a series of small steps, from the late 1950s through the Agricultural and Consumer Protection Act of 1973, that high price supports were abandoned. It seems likely that policy analysis, even though it was correct, had no role in this evaluation; the government learned to keep out of the manure pile, at least the deeper parts, only by getting in up to its knees.

Experimentation is not proposed in policy research by academics because it takes too long to obtain results — you will run right off the tenure track while waiting for results. But because this means of learning has been important in the past, we should consider how best to organize policy implementation in order to make the best experimental use of the policy.

Not that it is desirable or possible to try to mold policy itself to experimental purposes. The few policy experiments known to me, such as the income maintenance experiments of the 1960s, seem to have been very costly for the results achieved. What I have in mind is that initiatives in agricultural policy, which are constantly occurring in any case, be used consciously as sources of data for policy

analysis. This is where data collection can be a most useful means of relaxing constraints on analysis. From this point of view it would be very helpful if ASCS could collect information from program participants in addition to that necessary to administer the program. I hope that administrative separation of ASCS and ERS-SRS would not prove an obstacle to upgrading the data generated by farm program experience, but if it would, overcoming this obstacle should have high priority.

Analytical *shortcuts* are sometimes helpful in drawing inferences by indirect means. For example, in predicting the effects of a proposed import tariff, the relevant direct experience may be nil, but one may nonetheless use information about domestic demand and foreign supply elasticities to obtain roughly appropriate excess-demand price flexibilities for the imported product. Or the long-term consequences of a price-support regime in the U.S. are not observable, but a careful cross-country comparison of nations with different policy regimes might prove illuminating. Another example is that one can obtain information about the expected permanence of programs, and hence how farmers may be expected to react to them, by comparison of the rental and purchase prices of marketing quota under the tobacco program. Of course, one has to take these opportunities as one finds them, and there are no guarantees that they can be generated when needed. Nonetheless, a search for such shortcuts should be part of any program to relax the constraints in policy modeling.

### Summary and Conclusion

Four types of constraints have been discussed, falling in two broad categories: lack of appropriate data and limitations of analysis. They are:

- A. Lack of data
  1. Absence or low quality of economic statistics to model past economic events empirically
  2. Absence of past economic events that permit assessment of effects of proposed policy interventions
- B. Limitations of analysis
  3. Inability of economic theory to forecast answers to questions asked, or guide empirical work that will
  4. Failure to mobilize proper economic analysis in the political setting

It was argued that two further shortcomings of applied work in agricultural economics are less important for policy analysis today than the amount of effort devoted to them might suggest. These are insufficiently sophisticated econometric methods and unrealistically simple economic models.

Needless to say, these points are not rigorously established in this paper, but I have tried to develop reasons for the views expressed by considering several case studies of economic analysis in policy formation: the farmer-owned grain reserve (FOR), the dairy program, meat and palm oil imports, and several regulatory issues. Item 2 is of particular importance for assessing the FOR, and also bears on the analysis of dairy policy. Item 3 is of particular importance for regulatory issues such as forecasting the consequences of banning commodity options or restraining the expansion of Iowa Beef Packers. Problems with Item 4 arise with assessment of the FOR, the dairy program, and commodity imports.

In relaxing the constraints, Item 1 is relatively easy since all it requires is investment of money and talent (which shows how intractable the other three are). Item 2 often creates insuperable difficulties, but sometimes ingenious use of data can wring out more information from historical experience than might at first seem possible. Item 3 may yield to better theorizing, but it certainly isn't guaranteed by funding research projects. Item 4 could be viewed as most intractable of all, especially under the view that when the chips are down in the real policy process the participants do not want nor do they need policy analysis. But while this view has a grain of truth, it is incorrect in its implication that politics renders policy analysis completely impotent.

As for methods of relaxing constraints, the paper discussed simulation models, policy experiments, and analytical innovation, with skepticism about simulation but some hope for the latter two. In the context of policy experimentation, data collection becomes a key factor. Given the modest hopes for relaxing constraints along these lines, it is meet to return to what were claimed above to be relatively minor constraints — inadequacies of currently used econometric methods and standard economic models. At least economists have some reasonably clear and plausibly feasible ideas about what to do along these lines, for example as spelled out in the Rausser-Just, Johnson, and Klein papers at this conference.

It is surely preferable to make progress in modest ways rather than to persist in butting our heads against imposing stone walls. Nonetheless, it is probably a useful division of labor for some of us to go on butting just in case something unexpected turns up — either a surprisingly soft section of wall or a hard section of head.

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## Commentary

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*William E. Kibler*

The thoughts presented and the positions taken by Bruce Gardner on the nature of constraints for policy modeling are indeed refreshing. I believe a very high proportion of all discussions I have encountered on shortcomings or improvements that could be made in modeling have dealt with improved **reliability** of data or data for additional variables. For one who has spent considerable effort over the past three decades trying to supply new or better quality data to answer an ever-increasing number of policy issues, it's nice to see such questions as: Are there limitations on economic theory? Is there a general inefficiency in the mobilization of economic expertise? As well as: Is there a pervasive lack of appropriate data? . . . asked and discussed in this type of forum.

Since I'm not an expert on the limitations of economic theory or an authority on mobilization of economic expertise, I will direct most of my remarks to the questions on data. Gardner, I feel, is again on target when he considers that one of the key data ingredients might be that appropriate data series might be available, but events haven't generated enough experimental data to accurately assess impacts of the policy or program.

In his section on the Farmer Owned Reserve, for example, he points out that one's degrees of freedom are severely restricted because we can observe only one crop and one carryover each year. This prohibits the observation and measurement of changes in stocks until several years of pre- and post-FOR are observed. Although this requirement cannot be totally eliminated, perhaps it would be possible to reduce the number of years needed for evaluation by making fuller use of data that are available. I refer here to production forecasts for crops such as corn that have measures of

variability that can be expressed in terms of probabilities.<sup>1</sup> For the July 1 corn production forecast, chances were two out of three it would be between 6.47 and 7.76 billion bushels, and nine out of ten it would be between 6.00 and 8.23 billion bushels.<sup>2</sup> Such data clearly give some objective quantities for model testing that require no questionable assumptions. These data could also be provided for states or regions to give additional information or degrees of freedom for evaluation. Such data might also be a measure of the probability of various sized crops within a given crop year.

Part of my thrust here is to nudge my economist friends who were critical for many years about the lack of published measures of variability for crop forecasts. These have now been published monthly for four years and have performed exceedingly well statistically.<sup>3</sup> To my knowledge, they have not yet been used to any degree by economists in modeling. Another key role that such information or data reliability could play would be to measure how sensitive model outputs are to improved accuracy for independent data variables used in forecasting.

I read with interest Gardner's review of literature and comments on the support price for milk. Admittedly, we might need 30 to 40 production periods to estimate supply elasticity, and other variables, accurately in strictly economic and statistic terms. However, I observed a recent example of a more pragmatic approach used for determining whether the current price, based on parity, is above or below the market-clearing price or improving the well being of milk producers. Recently, while briefing Secretary Block in lockup on the July cattle report, items such as a 2 percent increase in total inventory and a 1 percent increase in milk cows were taken in stride. When the briefer mentioned a 267,000, or 6 percent, increase in milk replacement heifers, he interrupted to say the dairy industry didn't need a one of those 267,000 new heifers for 1982. The very next day in meeting with dairy operators he used that single number very effectively to defend the Department's proposed program for dairy prices. I think it's easy to discern the policy analysis the

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1 U.S. Department of Agriculture, Statistical Reporting Service, Crop Reporting Board, *Crop Production*, June 1977

2. U.S. Department of Agriculture, *Crop Production*, July 1981

3. William E. Kibler, "The Statistical Reporting Service System," annual meeting of the American Agricultural Economics Association, Clemson, S.C., July 1981



secretary did on the spot in this instance. One further example in this area: A dairy operator commented last week to a member of my staff that the 7.94-billion bushels corn crop forecast could be a major factor that could eventually destroy the dairy price support program. I don't have to lay out for you his non-optimal policy analysis on the support program. However, I find it difficult to justify that we as professional analysts don't have sufficient data to determine a price band with a spread of 4 to 6 percent that will achieve a long-run excess supply close to zero. The quality of our dairy statistics on production, consumption, and product use, particularly the historical series, is as good as any SRS produces. The failure here might be desire or will, as was concluded for commodity import policy. My point is that we may never reach theoretical optimality, but it is good to have that as an objective to work toward whether you are the secretary of agriculture, a dairy producer, or an agricultural economist.

I find myself in more disagreement with the author for issues related to regulation of land and agricultural production such as foreign ownership of U.S. farmland, restriction on uses of prime farmland for non-farm uses, or pesticide use. Admittedly, many of the reasons behind these issues may be more emotional than economic, but if that's the situation, we should be able to illustrate this as an emotional issue with some model.

Some examples for consideration might be an indirect approach, such as how much higher food and fiber costs are we willing to bear to ensure maintenance of prime farmland, avoidance of odors, or more recently enhanced treatment of animals. I'm not confident that a direct data collection approach of asking individuals to quantify the value of avoiding unpleasant odors or increased wildlife populations will ever yield very reliable data on benefits. Nor do I feel that we will ever be able to afford massive resources to establish accurate relationships between pesticide use and the mortality of wildlife. A better conclusion might be that it could be impossible to acquire appropriate basic data for such analysis. The task involves more than mere sampling and experimentation. Some interaction between the statistician and economist is an absolute must to reach some compromise agreement on what types of useful data might be collectable for this type analysis. Once this point is reached, it will be time to begin the sampling and experimentation. This interaction, I feel, is the key Gardner mentions in the ingenious use of data

series to glean more results from the same basic series.

I find it hard to disagree with the thesis advanced by the author that there are no easy ways to relax the constraints discussed. It will not only be a slow process requiring investments in data, accumulation of experience, theory development, and research, but one of developing priorities among other competing and closely related modeling activities such as measuring capacity and productivity.<sup>4</sup> I feel all of these activities share many of the same problems in shortages of data, conflicting views on the theoretical and conceptual approaches, and lack of effort on the part of both statisticians and economists.

Unfortunately, obtaining funding to acquire data solely for policy analysis is much more difficult than to acquire data that are useful to firms in making production and marketing decisions. Many contend that there are many sources of data for these purposes from administrative records that have not been tapped.

I don't share this view since it has been my experience that data collected for general administrative purposes have many weaknesses in terms of definition, concepts, timing, and detail when they are used for other analysis. I believe its capability to serve as a verification of analysis is where its strength lies. As Rausser and Just suggest, we might have to identify a few vital general purpose series and work to refine these and measure their reliability.

This brings to mind another point that I commend the author for making. I would like to see economists put more emphasis on establishing improved standards of quality for data used in modeling or other analysis. There are entirely too many data series included in modeling and analysis simply because it is the only source available. It will be difficult to establish exact standards that might apply to all series, but numbers based on fewer than 20 degrees of freedom, and sampling errors of 20 percent or larger, present problems for statisticians. I get much more criticism for refusing to publish data that don't meet these standards. I get few compliments for withholding data that fail to meet them.

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4. J. B. Penn, "Prospectives on Capacity Concepts, Measures, and Uses for the Food and Fiber System," *Proceedings of Workshop on Agricultural and Rural Data, Series A*, ERS and SRS, USDA, Washington, D.C., May 1977, and John B. Penson, Jr., and William E. Kibler, "A Critical Review of Alternative Approaches to Estimating Capacity and Capacity Utilization for the Food and Fiber System," *Proceedings of Workshop on Agricultural and Rural Data, Series A*, ERS and SRS, USDA, Washington, D.C., May 1977

I concur with the author's point that experimentation has been the chief factor that has had impact on policy development. The only problem we have is the slow learning curve we follow in these areas. As pointed out, the many initiatives implemented provide a multitude of opportunities for both data collection and analysis. However, the data needed for relaxing modeling constraints will not automatically flow from these programs. There must be careful identification of the types of data needed, relevant definition and concepts established and quality standards set. As yet we have not taken many steps to overcome many of these conceptual problems that Bonnen and others<sup>5</sup> have so effectively articulated.

The administrative separation of ASCS and SRS is really not an obstacle to upgrading data generated by the farm program experience. We have a free flow of data and information policy between the agencies. The more significant constraints are the basic recording units of ASCS records that are still generally based on a historic tract ownership concept, geographically oriented toward townships and counties. They currently carry about 8,000,000 records in their offices for our 2,300,000 farms. There are also problems with uniformity among counties and states in their record systems. I am happy to say the task of upgrading and bringing all department records into a more compatible base is being addressed and given high priority by Secretary Block.<sup>6</sup> There will be no easy quick answers or solutions. Public Law 96-511, enacted in December 1980, mandating a 25 percent reduction in the response burden for the private sector in providing basic data for public policy and program decisions, could be one of our most severe restrictions.<sup>7</sup> This even includes data on applications for benefits for such things as farm programs. This, enforced vigorously, coupled with very tight and reduced budgets that agencies will face during the next few years, will make it necessary for us to carefully set priorities and

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5. James T. Bonnen, "Our Obsolete Data Systems: New Directions and Opportunities," Presidential Address, Annual American Agricultural Economics Meeting, Columbus, Ohio, August 1975, and Charles H. Riemenschneider, "Economic Structure, Policy Discovery Mechanics and Informational Content and Nature of USDA Prices," Proceedings of Workshop on Agricultural and Rural Data, Series A, ERS and SRS, USDA, Washington, D.C., May 1977

6. U.S. Department of Agriculture, Office of the Secretary, *Information Resource Management Task Force*, Secretary's Memorandum 1044-1, September 1, 1981

7. Public Law 96-511, The Paperwork Reduction Act of 1980, Washington, D.C., December 1980.

standards for data collection that will ensure effective use of the resources available.

## Future Directions for Agricultural Policy Analysis

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*Luther Tweeten*

My task is to summarize briefly some highlights of the conference and suggest future directions for agricultural policy analysis. I define policy analysis here to include policy problems or issues and econometric models to address them. The term "econometric models" is used broadly herein to include not only those combining economic theory, statistics, and mathematics, but also models such as simulation and linear programming which contain no statistical component. After discussing issues of economics as a predictive science, including the institutional environment for econometric modeling, I relate modeling efforts to selected policy issues likely to be prominent in the 1980s.

### **Economics as a Predictive Science**

Economics has progressed from a science of classification and explanation to include prediction. The economy, like the weather, influences people's lives each day. The public's appetite seems to be insatiable for weather and economic forecasts, despite their frequent imperfections. George Meany said that economics is the only profession where a person can gain great eminence without ever being right. (Soviet economist Nikolai Kondratieff was an exception; he was executed for his long-term growth cycle theory which, to his misfortune, predicted an oscillating rather than a truncated future for capitalist economies.) This issue is not whether but how well and by what means economists predict the future.

Econometric models are here to stay in part because they have become to prediction what mathematics earlier had become to economic theory — a systematic way of dealing with complex situations while allowing scrutiny of assumptions and logical processes.

Yet many of the symposium participants seemed to agree with S. R. Johnson's conclusions, presumably based on his own perceptions as well as a dozen reviews he cited of the performance of various sector and economy-wide models, that "the record of economics in developing policy models for decisionmaking purposes at the sector and more aggregate levels is anything but distinguished; . . . performance of economic models has not met the claims by their architects or the anticipations of policymakers."

At least one conference participant expressed a different view. L. R. Klein stated that, "As forecasting devices, the Wharton Model and similar mainstream econometric models have stood the test of time." However, Stephen McNeese (reference 6) found that average judgmental forecasts of the economy collected from a panel of members of the American Statistical Association were no worse than mainstream econometric model forecasts. An evaluation by Richard Just and Gordon Rausser (4) revealed that forecasts of agricultural commodity prices were on the average no more accurate from econometric models than from the futures market. At the July 1981 meeting of the American Agricultural Economics Association, Cornelius (1) presented results showing that judgmental livestock price predictions by agricultural outlook specialists were as accurate as those from mainstream econometric models of private firms. The econometric models have advantages, however, in providing a rich and systematic source of forecasts on a wide range of economic outcomes, including alternative policy scenarios. Mainstream models are powerful educational devices that have enlarged the audience for econometric analysis. They have encouraged decision-makers to think in terms of what-if questions of sensitivity analysis, economic interactions, critical factors to monitor, and opportunities for further application of alternative types of econometric models. Furthermore, the mainstream econometric models have the advantage of continuity in contrast to what Earl Heady called the "one night stand" frequently characterizing policy models originating in universities, which are abandoned following completion of the thesis or journal articles of the graduate student who constructed the model.

### *The Institutional Setting*

Without doubt, a major innovation in the institutional environment of economic modeling is the emergence of the mainstream

econometric models of the Wharton type. The disadvantage of relying on such models alone is that they are built to respond to the short-term political horizon of the federal government, as noted by Dale Hathaway. The federal government can respond to pressures to increase national income and employment much more quickly by stimulating aggregate demand than by stimulating aggregate supply through incentives for savings, investment, and economic efficiency. Accordingly, emphasis in mainstream economic modeling is on demand-side rather than supply-side economics. The result of pursuing a series of short-term, demand-side expedients for an extended period is a chronically underachieving economy (14). The point is that the institutional framework for economic modeling sometimes must provide an environment for educating the public by answering questions policymakers are not asking. These questions, which need to become part of the national public dialogue, deal frequently with distributional and long-term economic impacts of current or potential policies. Economic models need to present a vision of what could be as well as what is or what will be.

A strong case can be made for a pluralistic institutional setting for econometric modeling. Modeling is an emerging science; some trial and error is unavoidable with an opportunity for the most successful systems to survive based on accuracy of predictions and other norms of performance. A mixed system offers advantages including checks and balances on each system. Universities are frequently in a position to innovate and exercise academic freedom in making sometimes unpopular results available to the public with a minimum of political interference. Basic research at universities on use of optimal control theories and procedures and of marginal utility of income to ascertain impacts of income redistribution offer potential for improved policy analysis, but bugs will have to be worked out before such approaches will be adopted by federal and private agencies providing day-to-day inputs to policymakers.<sup>1</sup>

The Economic Research Service (ERS) of the USDA has continuity and can direct considerable professional resources to data collection and monitoring and to analysis using models designed at ERS or elsewhere to respond to pressing policy issues raised by the execu-

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1. A case can be made for diverting resources from construction of new models to maintaining and improving existing models. But concentration of a few "good" models in a few locations also has drawbacks — there is no evidence that monopoly induces innovation.

tive or legislative branches of government. Private firms are in a position to provide continuity, quick turnaround, and results from complex and tested models for those who can afford to pay. The system is further enriched by models of other institutions such as Federal Reserve Banks.

The advantages of each contributor can sometimes be joined in cooperative efforts between, say, universities and the federal government. As part of a U.S. General Accounting Office survey, questionnaires were sent to developers of agricultural models asking, "Excluding your model, what model would you consider best for evaluating federal food policies on national and international levels?". The four most frequently mentioned models were POLISIM, the Iowa State Programming Models developed by Earl Heady and associates, NIRAP, and the USDA Cross Commodity Forecasting System. The observation of interest from an institutional perspective is that each of these models either originated in or received financial support from ERS.<sup>2</sup> Although at least three of the above models had origins in land grant universities, the models would have had less continuity and usefulness without USDA support. At issue is whether, in the face of budget cutbacks and personnel reductions, ERS can continue to provide the environment for data quantity and quality and for econometric model construction and maintenance; and whether universities and private companies can fill voids in the event of less ERS support.

### *Efforts to Improve Econometric Modeling*

Conference participants offered generous and sometimes conflicting observations concerning shortcomings of models and how to remedy them. S. R. Johnson expressed concern that complex models manipulated by "curve fitting" to obtain good *ex post* "predictions" in fact provide exaggerated estimates of statistical significance, narrowness of confidence intervals, and *ex ante* ability to predict.<sup>3</sup> He called for "relatively uncomplicated" models, not

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2. It is also notable that three of these models are not very complex. Of course, simple models are not necessarily small models. In the 1960s, ERS developed a large linear programming model (Aggregate Production Analysis System or APAS) to forecast farm commodity output in detail. Addition of complex recursive components and other constraints so complicated the model that it mercifully sank of its own weight.

3. One can obtain a perfect *ex post* fit simply by adding enough parameters. But such methods do not insure *ex ante* ability to predict.



"aggressive in theoretical content", and containing only well-developed theoretical specifications. Yet the reasoned imperatives cited by John **Penson** to make the national economy endogeneous and by **G. Edward Schuh** to make the international economy endogeneous in agricultural sector models translate into much expanded model specifications.

One is sorely tempted to side simultaneously with the conflicting counsel of Johnson, Schuh, and **Penson**. Small is beautiful; so is realism. But one must choose. Disputes over the magnitude of basic parameters, such as the price elasticity of U.S. export demand (see reference 8) and the tendency for a complex model to become a "black box" even to its architect, dictate that basic research on parameters and other aspects of specifications is required as we expand models. Construction of complex models guided only by how well equations "predict" the past often leaves little time for attention to the structural validity of coefficients. This lack of attention can lead to serious problems especially when attempts are made at structural analysis of the impact of untried policies or changes in the magnitude of one or a few explanatory variables. Of special importance is recognition of differences between short-term and long-term behavioral responses, a lack of which has generated some of the disagreement over the parameters magnitude, such as export demand elasticities. Time series may not contain the long-term response information needed to estimate long-run parameters and other approaches (2, 9) may be necessary. The issue is not trivial because the magnitude of the price elasticity of export demand is critical in determining the economic merits of policies such as a unilateral U.S. cartel in wheat or export subsidies.

Gordon Rausser and Richard Just, along with Dale Hathaway, stressed the advantages of involving policymakers in planning and designing models. There is no such thing as a truly general model, and it is very expensive to maintain a comprehensive model to respond to the wide range of questions posed by policymakers. Advances in computer hardware and software have reduced problems of managing large econometric models, but problems of specification as well as data remain. Maintaining capability to tailor-make special purpose models for responding to emerging policy questions is essential. Rausser and Just called for general purpose data *sets* rather than general purpose *models*. In this regard, the OASIS data base system recently made available through ERS is a

promising development for ready and widespread access to updated data. Communication of information on the conceptual basis and reliability of data has been inadequate when data were circulated in printed form. This problem could intensify as more data are disseminated electronically.

### **Future Policy Analysis**

Two basic approaches can be taken in determining directions for agricultural policy analysis. One is to begin with analytical models, describing their faults and how to alleviate them in applying models to policy issues. The second is to begin with policy issues likely to be prominent in the 1980s, then discuss how econometric modeling can be applied to help resolve the issues.

Because the worth of econometric models derives from the information they provide to help make decisions that improve the well-being of society, it is useful to examine future agricultural policy issues in the context of what econometric models have or can contribute to their understanding and resolution. Don Parrlberg set forth a policy agenda with which I have no major quarrel. However, I agree with Lynn Daft that some of his assumptions (such as the future economic environment for agriculture) are appropriate grist for the econometric modeling mill. In my judgment, the principal economic concerns for farmers in the 1980s are instability in prices and incomes, cash-low and cost-price pressures (already apparent in the early 1980s), and concentration of economic activity in fewer farm and nonfarm firms. Consumers are concerned with production capacity, resiliency of food output in response to changing conditions, and impacts of higher energy costs, cropland losses, world population growth, and other factors in the price and availability of food.

I address these agricultural policy issues under the topics of (1) supply-demand balance for farm commodities and attendant issues of inflation and terms of trade for agriculture, (2) instability in economic outcomes, (3) the structure of the economy, especially of the agricultural industry and the agribusiness firms from which farmers purchase inputs and to which they sell output, and (4) resource limitations, including land losses to erosion, urban development, and other factors.

I do not take sides in whether the issues are best addressed by first, second, or third generation models (the latter preferred by

John Penson) or by linear programming, simulation, or neoclassical positivistic models (the latter preferred by Bruce Gardner). Each of these approaches has advantages and disadvantages and there appears to be no substitute for case by case judgment in determining when and where each model is appropriate.

### *Supply-Demand Balance*

I have reviewed a half-dozen projections which without exception provide a glowing outlook for the farming economy in the 1980s. My own projections (12) are the least optimistic but also appear to be more favorable than the current situation warrants. Of course, the decade is only two years old, and subsequent years could validate the projections on the average. But if, as it now appears, models have gone wrong, what are the likely reasons? To examine possible reasons, we must look at the demand components (exports, inflation, and domestic population and income) and supply components (productivity and inflation) as well as at parameters.

First, considering demand, projections of domestic population and income are not a major source of projection error, in part because the income elasticity of demand is low for farm output.

Exports are an important component in their own right and also as part of the world supply-demand balance which is of humanitarian concern. A flurry of long-term projections seem to follow world food crises and some modelers confuse *need* with *effective demand* in world markets. All projections are plagued by unreliable data for developing countries and inability to deal with unpredictable weather and politics.

Estimates are also troubled by failure to account for interactions between international markets and monetary-fiscal policy. A policy of domestic monetary-fiscal restraint decreases the money supply and aggregate demand. The initial impact is to lower domestic prices relative to foreign prices of goods and services, thereby increasing exports, reducing imports and improving our trade balance. On the monetary side, higher interest rates cause dollars to flow into this country, improving the cash trade account while adding to the domestic money supply.

The improvement in balance of trade and financial reserves causes the value of the dollar to rise in international exchange, making our exports more expensive and imports less expensive, the reverse of the first round effects. General prices fluctuate through

the periods of stabilization and expansion called the inflation cycle, but what happens to farm prices paid and received, *ceteris paribus*? The demand for our agricultural exports is buffeted by these circumstances but the impacts have not been quantified.

Monetary restraint that is successful in slowing inflation reduces the cash-low squeeze (13) on the farming industry, but this benefit may be offset by loss of agricultural export markets as balance of payments improve and the value of the dollar rises in world markets. Expansionary macroeconomic policies produce results opposite those above.

The impact of national inflation on nominal demand and prices received by farmers for farm output is a major potential specification error on the demand side. If passthrough is low (a 1 percent increase in the general price level causes farm output prices to rise but by less than the increase in farm input prices), the immediate impact of inflation is to reduce directly the domestic terms of trade but improve the international terms of trade in U.S. farm products. While average estimates of inflation passthrough have been quantified (11), the extent of such passthrough has not been related to demand-pull, cost-push (e.g. from tight energy or food supplies), and wage-price inflation sources. It makes a great deal of difference to the farm sector if inflationary pressures come from a world food shortage rather than from the wage-price spiral.

The principal source of error is estimating intermediate and long-run prices and incomes for the farming industry appears to come from inaccurate estimates of shifts in supply rather than in demand. On the supply side, the specifications of productivity and inflation pose problems. Measures of productivity confound weather, capacity idled by government programs, and technology. Although weather cannot be forecast except in the short run, separation of the past effects of weather and technology on productivity would give helpful information on whether productivity gains are the result of unusually favorable weather or technology. Underestimation of productivity gains from technology in the 1980s may originate from the observed small real increases in resources for public research and extension which modelers expected to translate into low levels of productivity growth. Perhaps the lag between output and input will vindicate productivity projections in time, but an alternative explanation is that private domestic research and extension along with foreign imports of technology are having a larger impact than

expected. Specifications need to be improved but are severely hampered by lack of time series data on weather and private investment in farming technology.

Inflation is also a factor on the supply side, with inadequate accounting for the impact of the general price level on nominal supply at the farm level and hence on the ratio of prices received to prices paid by farmers. In short, the evidence suggests (11) that projections of real demand and supply (based on deflated price series) overestimate income by failure to account for the depressing effect of inflation on the ratio of prices received to prices paid by farmers.

### *Instability*

Instability is a perennial farm problem and may become more prominent with a declining federal support of commodity programs (including disaster payments), growing cash costs relative to receipts, and rising share of demand from volatile exports.

Introduction of producers' risk into economic policy models has significantly improved specifications. Just's research (3) indicates that output induced by greater economic security under commodity programs may have offset production controls. Li et al. (5) found strong support for Friedman's permanent income hypothesis with a given average farm income inducing more investment as the transitory component enlarges relative to the permanent component. It is premature to say that finding of the Just and Li et al. studies contradict one another because they do not deal with the same resources.

It has long been public policy to assist farmers by providing outlook information to improve decisions and increase economic efficiency of benefit to the firm and society. Forecasts from numerous sources, including mainstream econometric models and outlook specialists, tend to bunch together and consequently err somewhat uniformly when underlying conditions change. Although unforeseen and perhaps unforeseeable changes in weather and political decisions such as export embargoes or an OPEC oil price adjustment are factors, an emerging problem may be self-defeating forecast feedback. Some outlook specialists contend that enough producers took optimistic beef and pork price outlook seriously the last few years so that production increased and prices fell. If self-defeating forecast feedback is the source of error, it represents a new

challenge to specification of price prediction equations. Of course, part of the forecast error may come from more conventional shortcomings such as failure to account for broiler and pork supply in projecting beef prices. Also the income elasticity of demand for beef may have fallen with slow growth or decline in consumers' incomes.

### *Structure*

The issue of farm structure is now muted but will emerge from time to time. Economic modeling can provide helpful insight into the impact of federal monetary-fiscal policy — including income and estate taxes — on farm size, numbers, growth, and accessibility to new entrants.

The current rapid pace of industry mergers in an environment of passive federal antitrust efforts will renew farmers' and consumers' concerns over exploitation by input supply and output marketing and processing firms. Despite much rhetoric, we know comparatively little about the impact of such mergers and attendant industry concentration on economic efficiency or on farm and food prices. While evidence indicates that concentration of firms in the food marketing industry entails social costs (7), these costs need to be compared to costs from diseconomies associated with a more nearly atomistic structure of small firms. Economists are challenged to model the probable level and incidence of economic gains and losses from changes in the structure of marketing orders (e.g. termination of Class I premium and import restrictions and allowed use of reconstituted milk).

In short, considerable basic research will need to precede modeling of how the agribusiness economy operates with various degrees of concentration of industry.

### *Resource Constraints*

Much concern is apparent today over "exporting our rich topsoil" and "urban encroachment" into prime farmlands. Serious gaps exist in our knowledge of how erosion and urban encroachment reduce cropland and farm output either in the past or for the future. Earl Heady and his associates at Iowa State University provides estimates on the impact of energy and pesticide restrictions or price increases on the location and level of farm output. Economic modeling can help to ascertain the tradeoffs between mandatory controls

on land use and exports on the one hand versus, on the other hand, economic inducements for proper land use (e.g., use of property taxes, full-cost charges for rural services, etc.) and more research and extension investments.

A related issue is the production capacity of agriculture and its resilience in responding to emerging conditions (16). A central and imponderable concern has been cropland availability in response to economic conditions. Acres of cropland available of various quantities have been identified in national soils inventories. But we lack adequate positivistic estimates of cropland supply response to prices. Our models will not adequately predict production capacity or tradeoffs between various options to increase production without improved estimates of cropland supply response to price. The public concern over exporting topsoil through erosion of soils used to produce exports does not seem to recognize that an additional bushel of corn for export provides in theory the same utility to Americans as another bushel for domestic purposes.

Again, the point of importance in this discussion of econometric modeling of policy issues is that model builders must give careful attention to specifications, including economic theory, both in formulation of models and in interpretation of results.

## **Summary**

Econometric modeling now is an indispensable component of agricultural policy analysis. Contributors to the symposium recognized the problems of modeling both from the demand side (e.g., politicians tend to focus on short-term issues coincidental with their two-, four-, or six-year term of office) and from the supply side. On the supply side of modeling, participants appropriately emphasized problems of data and model specification more than the more narrowly quantitative concerns for statistical unbiasedness, consistency, and efficiency.

The institutional environment for policy modeling could have received more attention at the symposium. The growth of mainstream modeling in the private sector is a positive development but raises questions about the potential for crowding out modeling by universities and by the USDA which has the advantage of being close to data as well as to questions policymakers are asking. A need exists for some modeling institutions with independence to analyze with continuity policy issues of concern to the public at large. One

suggestion is for greater financial support for modeling centers from foundations or other somewhat politically neutral sources (see 10). Federal budget constraints that reduce quality and quantity of data and that interfere with continuity needed for improving successive generations of policy models are of continuing concern.

Agricultural issues of supply-demand balance, instability, structure, and resource limitations will be prominent in the 1980s. Modeling can provide basic information to help resolve the issues. But some very fundamental economic analyses relating to model structure and data are required as indicated in this paper. Some large models that perform reasonably well in predicting short-run economic outcomes lack structural integrity or contain distributed lag components that make them unsuitable for intermediate- and long-run predictions or for examining the impact of untried policies.

The diversity of econometric modeling efforts that has characterized the field since its inception has frequently been wasteful. A case can be made for fewer new models and for better maintenance of the old. No single approach to modeling can address the need for information and there appears to be no alternative to a case by case application of the best judgment possible in choosing analytical tools. Competition among model designs has merit as economists, policymakers, and the public sort out the best efforts based on ability to predict consequences of actual and prospective public policies rather than based on model size or sophistication.

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