

Constraints on the Modeling of Agriculture and How They Might be Relaxed

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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 than 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.¹ 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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