ECONOMETRICS AND POLICY MAKING IN THE UNITED STATES

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Policy formulation involves decision making under uncertainty. Ecouomitrics involves decision making under uncertainty. As the policy maker and the econometrician circle each other and idteract, both are playing a gampler's game, but the stakes are much higher for the former.

It is the theme of this paper that the rules of the econometrician's game often limit its usefulness for the policy maker. I, of course, approach the problem bearing all of the biases of a policy official. As will become apparent later,I am not a professional econometrician, and I have not done the many hours of simulations of different econometric models necessary to document the majority of conjectures put forward in this paper. However, whether the conjectures are right or wrong, I hope that I shall at least identify problems that warrant further thought and investigation.

I shall divide this paper into two segments-one dealing with macro-economic problems; the other dealing with the microeconomics of taxes and subsidies.

A. Macro-Policy

The macro policy maker's problem can, in turn, be divided into three parts. First, he must forecast the future. Second, having speculated regarding the forces that will be stabilizing or destabilizing in the future in the absence of any policy change, he must desing the appropriate policy response which, in ftscal policy making, involves predicting the economic impact of changes in Government spending and taxation. Third, the legislature must be persuaded of the wisdom of the policy official's view. While the third problem is often the most difficult, especially in the American context of a separate Executive and Legislative Branch, I shall ignore it in this paper.

In theory, one would hope that the econometric models which win the forecasting game do so because their structure provides a superior representation of

how the economy actually operates. Then the models that are accurate forecasters of the major aggregates such as GNP, employment, and inflation for given policies should be equally adept at forecasting the impact of policy changes on those variables. Unfortunately, after two years of intently watchinh the performance of the major American, large-scale econometric models, I have been forced to conclude that in practice, «It ain't necessarily so».

To understand what has happened, I think it necessary to spend a brief time describing to this European audience the environment in which the major large-scale American models have been developed. Econometric model building is a very expensive business. If the U.S. had relied on only public and foundation support to finance model building activity there would be fewer large-scale models in existence and those that did exist would be updated at less frequent intervals. American large-scale models have proliferated, in large part, because they can sell their services to willing clients in business and Government. This development has provided a generous source of financial support to an intellectually exciting activity. As an ex-scholar, I can hardly complain when a basically scholarly aclivity reccives generous financial support, and on balance, I think that the effect of the market place on model building has been beneficial to economics in general and to large-scale model building in particular. However, it has not been without some cost to the intellectual appeal of the models.

Whether we like it or not, economists are often judged by non-economists in business and Government by our ability to predict the future. Even those economists who are wise enough to refrain from this mystical activity are often tarred by the same brush that is applied to their less cautious colleagues. In the field of large-scale model building, the competition to predict the future takes on many of the characteristics of a sporting event. Scores ore kept; prizes are awarded; and to those model builders who sell their services lo clients, the prizes can be enormous.

In the U.S., the score is kept by a variety of business publications that generally have a round-up shortly after New Years showing how the various prognosticators did in the prior year. However, the score is kept in a way has to seem peculiar to any economist. A model that produced a good GNP or unemployment prediction is lauded, even though the prediction may have been based on policy assumptions and prepictions of exogeuous variabie that were totally erroneous.

In my view several distortions have resulted from the concentration of developing models which predict variables such as GNP rather than the effect of policy changes on those variables. In describing these distortions I admittedly enter the world of conjecture and to some extent I generalize, since not all of the large-scale models have beeu developed in the same way.

Obviously when we describe large-scale model building we are still describing a primitive art. The forecasts of such models have not been very good, and indeed, like our companion in the sin of forecasting, the weatherman, we often find our most sophisticated techniques outperformed by naive rules. For example, predicting

that tomorrow's weather will be identical to today's gives one a pretty good record over the long run. Similarly, predicting that mext year's interest rates or GNP growth will be identical to this year's is not a bad approach to forecasting if you want to minimize your average error. However, in both meteorology and economics, this approach has the rather distinct disadvantage that it misses all of the turning points.

While the naive approach to forecasting is clearly deficient, I suspect that there is a vestige of this approach embodied in most large-scale econometric models in that many equations contain a large number of lagged variables, the coefficients of which often suggest that tomorrow's values of many variables will be similar to today's. The use of complicated lag structures may facilitate good fits for the eguation over the long run, but they make it difficult for the models to identify turning points, and over the last U.S. business cycle there was a tendency to understate the size of the cycle, both underpredicting the extent of the dectine and the speed of the initial stages of the recovery.

Both traditional forecasters and the models understated the depth of the recent recession and it resulted in the policy reaction coming somewhat too late. If policy makers had believed the typical forecast of a shallow recovery they probably would have overreacted in the other direction, but luckily this did not occur.

Currentiy, the models provide what I consider to be a dangerous signai to policy makers. They suggest that the rate of inflation would not be increased greatly by expansionary fiscal or monetary policies. The models may, of course, be correct, but it is my strong view that they are not and the result is due to the ctructural features discussed above, that is to say, the important role of lagged variables which dampen the effect of any policy shock on expectations and on the price level.

The desire to produce good forecasts of aggregates has had another bothersome inpact on the models, but it is very difficult to determine the nature of the biases that it introduces. I am referring to the habit of constantly modifying intercept terms ane other coefficients with add and subtract factors to make the results more «reasonable». I admit that I would never accept the results of any large econometric model without modifying them judgementally, but the process is sometimes carried out to such extremes that there may not be much difference between the old-fashioned fudgemental forecast and those provided by the seemingly «modern» econometric model. The model becomes primarily a device for keeping judgments logically consistent. That is to say, it makes the product side of the GNP accounts add up to the income side ; it makes savings equal investment ; and it insures that something like a judgmental employment forecast does not imply a ridiculous productivity forecast. To me, models are most useful forecasting when used this way and I in no way wish to demean the judgmental approach. Given the primitive state of the models, it is essential to making reasonable forecasts. However, I do feel uneasy when a base model with modified intercept and other terms is then used to simulate the effcct of a policy change. Onc of many reasons for this uneasiness is that the models do not typically impose the

same sort of logic on the analysis of policy changes that they impose on forecasts of various components of the GNP accounts. While all macro models force saving to equal investment (unless a residual is forecast explicitly), I know of no major American model that contains a Government budget constraint, i.e., forces a larger deficil to be financed by issues of debt or money.

Given the structual problems outlined above, it is not surprising that different large-scale modelc provide very different policy guidance. Recently, my staff simulated the impact of a hypothetical $ 20 billion cut in the fiscal 1978 budget *. This amounts to less than 5 percent of expected outlays and it should be noted that to make such a cut in 1978 it was necessary to assume some minor reductions in fiscal 1977. The simulations were performed in the Data Resources and Chase Econometrics models. In both models, different sorts of Government expenditures have different impacts, but the same composition of cuts was assumed in all of the simulations-about two-thirds of the cuts occuring in grant and transfer programs and the most of the remainder occured in purchases of goods and services.

In both models, a budget cut results in an endogenous monetary response. In other words, both interest rates and the rate of growth of the money supply decline. If this is allowed to happen, the models provided remarkably similar results. In the Data Resources model money GNP is lowered $29.2 billion below the control solution by the fourth guarter of 1978 while in the Chase model the reduction is $28.6 billion. Unfortunately, the similarity between the two models ends at that point. If money supply (M-1) growth is forced to remain at its control level, the fall in 1978 : 4 GNP is lowered to $ 16.3 billion in the Data Resources model but only to $ 26.3 billion in Chase. In other words, money is very much more powerful in the Data Resources model than it is in Ghase. If the rate of growth of the money supply is increased by one percentage point over the conrtol solution, the budget cut is more than offset in Data Resources with GNP rising $ 16.8 billion in 1978 : 4. In Chase it still falls by $ 19.5 billion. The resulting discrepancy between the two models is $ 36·3 billion in this experiment or over 1-1/2 percent of the expected GNP. While this may not seem like a major quantitative difference, the qualitative distrinction is of extreme importance to the policy maker. In one model, monetary policy is extremely powerful and a reasonable change in the mix of monetary and fiscal policy results in an increase in GNP. In the other model this same change in the mix of policy results in a decrease in GNP.

I shall close my discussion of macro models by referring to a current American phenomenon that is virtually impossible to handle in any economists' model. That is that there seems to have been a recent qualitative change in the psychology of the American consumer and businessman. The horrors of the 1974 and 1975 experience are seared in their memory. I have the strong impression and it is only

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* The U.S. Government's 1978 fiscal year covers the period October 1977 through September 1978.

an impression-that many Americans attach a fairly high probability to the events of 1974 and 1975 recurring in the near future, even though I think that most economists would attach a fairly low probability to the unusual coincidence of extreme events that led to the severity of the recent business cycle. However, the savings, inventory, and investment behavior that we are now observing is extremely cautious and there seems to be an immediate overreaction to each wiggle in the economic statistics. This makes life very difficult for the policy maker, because any significant change in policy is likely to provoke its own psychological overreaction which makes the resoults of that policy very hard to predict. For example, if there is a general belief that a more expansionary fiscal or monetary policy is inflationary, it will indeed show up in the inflation rate very quickly making it difficult to have an impact on real economic growth. Hopefully, this is a temporary phenomenon and expectations will become more stable as the recovery proceeds. However, while this mood of hyper-caution continues, it means that we must look with suspicion on the results of the traditional macro models.

The sort of problem described above, is, of course, related to the more formal work of the rational expectations theorists such as Muth ([3]), Sargent and Wallace ([5]), and Sargent ([4]). If expectations are indeed formulated rationally, and if economic actors are continually learning more about the economy so that the structure within which expectations are formed is constantly evolving, the world becomes very difficult to analyze for the econometrician and the policy maker.

B. Micro Policy

In micro economics the disagreement between various econometric models is generally less serious than the disagreement in macro econometrics. For example, I can think of no difference that is as serious as those between the monetarist and Keynesian approaches to macro econometric model building. There are, of course, some important differences. For example, the theories of micro investment behavior as proposed by Jorgenson ([2]) on the one hand and Eisner and Nadiri ([1]) on the other, have important policy implications in that the former believes that tax and depreciation changes can have a significant impact on investment decisions whereas the latter is skeptical about the impact of such changes. However, even Jorgenson suggests long time lags before the tax changes have an impact, and therefore, although the theories differ significantly, they both lead to the policy conslusion that the investment tax credit is not a very useful instrument for attaining short-rum stabilization goals.

While the differences in micro-econometrics may not be as serious as those in macro-econometrics, it is, of course, necessary to point out that it does not take much of a range in the estimate of a supply or demand clasticity to provide very different benefit-cost calculations for different tax-subsidy programs. However,

rather than belabor that point I would like to turn to a different policy problem in the use of micro-econometrics that I never noticed before I became a policy official and that I do not believe has received any attention from academic economists.

In elementary economics classes, the typical partial equilibrium analysis of a subisdy uses a diagram such as that in Figure 1. In this illustration a subsidy of

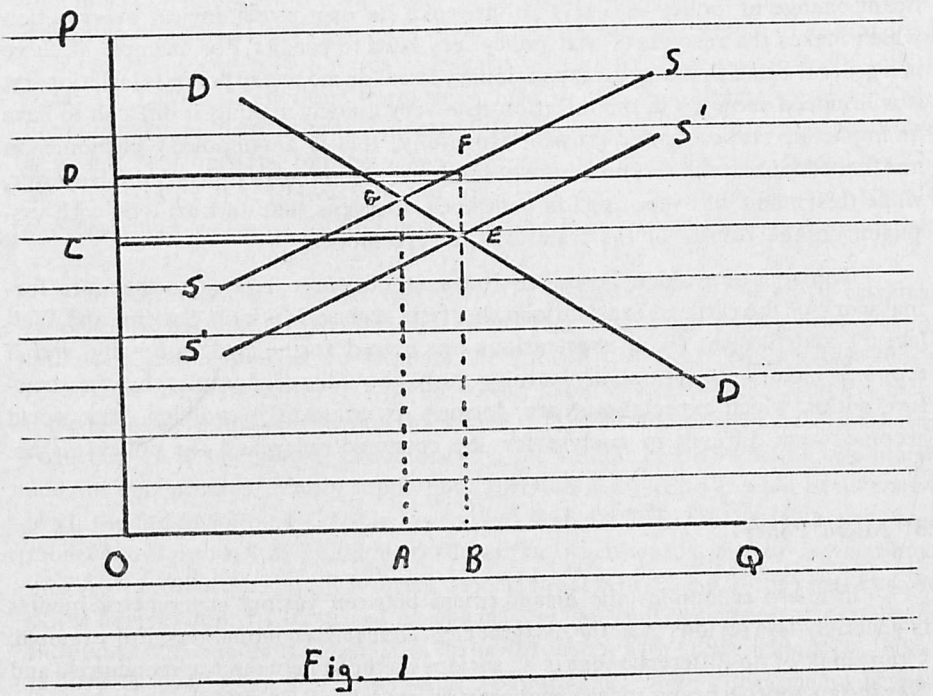

Fig. 1

CD is provided to the seller of good or a service and as a result the equilibrium amount of the good or service sold rises from OA to OB. Using econometric estimates of the supply and demand curves, one can readily compute the increase AB for any given subsidy.

However, it is important to note that the subsidy program described by Figure 1 is, in effect, an entitlement program in that suppliers receive a subsidy for every unit that they can sell. Many subsidy programs are of this type, but many, at least in the U.S., are not. For example, the typical American mortgage subsidy program does not offer interest subsidies on all mortgages offered. Instead, a limited dollar appropriation is passed and when that appropriation is exhausted

no more subsidies are offered. I call this type of program a rationed subsidy as opposed to the entitlement subsidy described by Figure 1.

Estimating the impact of a rationed subsidy is a nightmare for the econometrician. When the appropriation is only sufficient to subsidize a small portion of the level of activity represented by OB in Figure 1, it becomes necessary to ask what portion of the subsidy affects truly marginal decisions in the range AB and what portion represents a pure windfall on units that would have been supplied in the absence of the subsidy. Assuming for the moment that the appropriation is not even sufficient to cover the subsidy on a number of units equivalent to AB, two extreme results can be identified. At one extreme all of the subsidy can accrue to infra marginal units in which case the subsidy is pure windfall and no extra production results, or all of the subsidy can be assumed to affect marginal decisions in which case the increase in production is equal to the number of subsidized units.

The actual result is likely to end up between these two extremes. The outcome will frequently depend on the nature of the rationing mechanism which is usually designed with the intent of focusing the subsidy on marginal decisions. Unfortunately, it is virtually impossible to ration perfectly, and indeed, most rationing devices are extremely crude.

In our analysis of such policy issues, we have handled this problem by using the subsidy amount as an independent variable in a regression to see if any impact on housing starts can be identified.* One cannot place great faith in the results because our mortgage subsidies tend to be turned on and off over the business cycle and each time that they are reconstituted they tend to have slightly different structures which imply different incentives and different rationing mechanisms. Therefore, even if an average impact can be identified, there is no guarantee that the impact of a slightly redesigned program will be similar to that appearing in a regression equation.

Conclusions

This paper has tended to stress the failings of econometrics as applied to policy making. While economics, in general, and econometrics, in particular, have a long way to go before policy makers can rely on them with confidence, it is important to end on a note of optimism and to stress the enormous contribution that economists and econometricians have made to policy analysis. Although economists are sometimes ridiculed in public because of the apparent constant disagreement among them, the actual range of disagreement among economists on economic issues is typically very much smaller than the range of disagreement among the legislators that they serve. The science of economics-and I do not hesitate

*See Utt (6).

to call it a science-possesses extremely powerful analytic tools, and when the results of their application can be effectively communicated to policy makers, they can have an immense impact on policy discussions.

I can provide one example from my own experience. Three years ago housing construction in the U.S. was entering its severest decline in the post World War II period. Naturally, a variety of housing subsidies was proposed and many were implemented. In the initial stages of this process, it was the strong belief of most legislators that every housing unit that was subsidized would not have been constructed in the absence of the subsidy. We worked constantly to show them that this was not so and that regardless of the subsidy design, they had to expect a large share of the subsidy to go to infra marginal units that would have been constructed anyway. The fact that economists disagreed on the quantitative magnitude of the implied windfall transfer was troublesome, but not nearly as important as the fact that all economists had to agree that it was significantly greater than zero. Once this point was successfully communicated it had a large impact on the discussion and led to a much more sensible set of policy recommendations.

This is only one of a number of examples of cases in which the ideas of economists have had a beneficial impact. As Lord Keynes said, «The world is ruled by little else.» He also noted that the result was not always beneficial, but my own personal prejudice suggests the good economists' conclusion that, on balance, our marginal social product is at least equal to our salaries.

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