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DISCUSSION: IMPACT OF FEDERAL FISCAL-MONETARY POLICY ON FARM STRUCTURE

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I am going to separate my remarks on Luther Tweeten's paper into two categories, those on the empirical content of the paper and those on its conceptual content. I shall address the empirical part of the paper first, then discuss the conceptual part, and close with some challenges facing agricultural economists interested in macroeconomic issues.

EMPIRICAL ISSUES

The bulk of Tweeten's empirical analysis seems to be an attempt to settle a dispute with Bruce Gardner on the magnitude of the secondary effects of fiscal-monetary policy, via inflation, on real farm prices. This section ends with a call for an unbiased judge to decide whose results are more plausible. Being a colleague of Gardner who happens to think Tweeten might be right (at least in the short run) certainly does not qualify me as an unbiased judge. Biased or not, however, I am willing to say that neither gentleman's arguments are very convincing.

Let me explain. Tweeten presents us with a series of equations involving several permutations of arguments to conclude that the prices farmers pay for their inputs are not neutral to inflation. He then uses these results in conjunction with estimates from another study to infer that the short run elasticity of the parity ratio with respect to the GNP deflator is approximately 1.3. Although I have several questions about his econometric methodology, my most obvious question is why not use the parity ratio as a dependent variable in the model? Certainly, one can view the parity ratio as an appropriately normalized input price; such a conceptualization is compatible with the input supply framework developed by Tweeten.

I have reestimated Tweeten's model as described in his Table 1 with the parity ratio replacing prices paid by farmers as the dependent variable; the results are reported in Table 1. My data are not exactly consistent with those of Tweeten, but they are close. All data were derived from *Business Statistics* on an annual basis for the period 1948-80, hence our time periods do not match exactly. Variables are defined as follows: PAR is the parity ratio (1910-14 used as a base), PG is the implicit GNP deflator (base year 1972), and PC is the consumer price index (base year 1967).

All equations were estimated in log-log form, and, of course, all coefficients on the independent variables can be interpreted as elasticities. Now to the results. In both the PG and PC equations the short-run elasticity is not significantly different from unity; thus, it is impossible to infer from these equations that there is a short-run effect of inflation. However, in both instances the long-run multipliers are negative, implying that inflation depresses agricultural prices relative to nonagricultural prices. This I find hard to believe.

One might, therefore, ask what the problem is. My answer is that I am not sure equations such as these and those in Tweeten's (or Gardner's) paper are all that informative. Others might say that I am not estimating an input supply model of the same form as Tweeten since I am effectively dealing with a normalized price, the denominator of which should be explicitly modeled by another structural equation. My response is that in Tweeten's Table 1 we no longer have input supply equations as he suggests. Instead we have ad hoc reduced-form equations for prices paid by farmers, since

Table 1. OLS Estimates U.S. Annual Data (1948-80).

Regressors	Dependent Variable	
	PAR _a	PAR _b
Constant	2.36668 (.711238)	2.54491 (.720987)
Lagged dependent variable	.621546 (.110809)	.618251 (.105830)
PG	.995163 (.577041)	
PGL	-1.16783 (.625423)	
PC		.901861 (.462721)
PCL		-1.09958 (.517542)
R ²	.852130	.84220

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in these equations Tweeten has dropped all of the other independent variables, *including the input quantity*, from the input supply equation. An equation without an input quantity in it is not interpretable as a supply equation, but it may be interpreted as a reduced-form equation from an unspecified structure. Thus, my model can be construed as a reduced-form equation in the very same sense as Tweeten's model.

The flaw lies, however, with the specification of models of this sort. To my mind, they beg the question. Rather than directly examining the impact of actual fiscal-monetary variates on the agricultural sector, they regress one jointly dependent variable (prices in agriculture) upon other jointly dependent variables (prices not in agriculture, interest rates) as if it were legitimate to assume the casual direction only runs one way. I do not believe this, and there exists a large literature (e.g., Cooper and Lawrence) suggesting that commodity prices have a significant impact on the rest of the economy.

We need to move beyond such secondary analysis to analysis that focuses directly on the effect of various macroeconomic policies on the agricultural sector. While problems of multicollinearity and simultaneity make this an empirically difficult task, relatively simple and plausible approaches are being developed (e.g., Sims). If we only had to update our empirical tools, we would be in a relatively good position. Unfortunately, we have only vague ideas of the avenues by which fiscal-monetary policy affects agriculture. As agricultural economists, we must, therefore, develop consistent conceptual models of the interactions between agriculture and the rest of the economy before we plunge blindly ahead with questionable data analysis. But herein lies the real contribution of Tweeten's paper, for most of it is devoted to laying the groundwork for such a task.

Before turning to my comments on the conceptual portion of Tweeten's paper and my closing remarks, I would like to point out a few remaining areas where I disagree with Tweeten's empirical analysis. As I stated before, I see no reason, regardless of the size of calculated t-statistics, that allows one to drop input quantity from an input supply equation. Furthermore, low calculated "t-values" are not, as Tweeten suggests in his discussion of Table 1, evidence of exogeneity of input quantity in a simultaneous equations framework. This seems more like evidence of an inappropriate specification of the input supply relationship. Finally, when Tweeten discusses equations for agricultural price variables deflated by the GNP deflator, one must remember that regressing such variables on the GNP deflator involves an element of simultaneous equation bias.

CONCEPTUAL ISSUES

My remarks on Tweeten's conceptual analysis are brief, largely because I feel this part of the paper does an admirable job of providing a mainly heuristic but basically sensible analysis of the implications of various fiscal-monetary policy mixes on parts of the U.S.

farm sector. Although I have a few quibbles with this section, for example, no emphasis is given to the possible effects of high interest rates and tight money on storage and production decisions, by and large I find myself in agreement with much of his analysis. The direction taken is important and should be further pursued.

I would like to close my discussion with a few general remarks about our understanding of the effects of monetary and fiscal policy on agriculture as a profession. Although the subject area is not new (having roots in Schultz's classic work, *Agriculture in an Unstable Economy*), it is of much current interest. Unfortunately, it catches us at a time when we as a profession are very poorly prepared to deal with it. Although I am a relatively new member of the profession and thus may be in a poor position to judge, it seems to me that macroeconomics has recently been considered as a legitimate area of interest for agricultural economists. This bias or prejudice is reflected in the training of many of us who have received only a smattering of macroeconomic theory in our graduate curricula. It is my opinion that this neglect in training shows not only in the problems we consider but also in the way we research problems involving macroeconomic linkages with agriculture.

One element common to much of the current and recently completed research on these issues is our very incomplete way of viewing the problem. Rather than seeing agriculture as an integral part of a general economy, we tend to view agriculture as a sector affected by a larger "macroeconomy." The assumed causality in these models clearly runs from the "macroeconomy" to agriculture. Examples of such research include a good bit of my own published work, so I am as culpable as any in this regard. However, I feel that it is time for us as a profession to move beyond such research and try to integrate our research effort more closely with state of the art macroeconomic theory. After all, partial equilibrium models of the effects of general equilibrium phenomena are almost by definition a contradiction.

Another aspect of this problem is our tendency to look past the problem of the effect of macroeconomic policy on agriculture to such issues as the effect of inflation or recession on agriculture. While this is a legitimate and important area of research for agricultural economists, it has been perhaps the major area in which we have exhibited our ignorance of current developments in the general literature on macroeconomics. Hence, there are more than a few studies conducting research on the basis of dummy variable regressions where inflationary or other macroeconomic phenomena periods are arbitrarily designated, transformed into dummy variables, and then used as regressors (sometimes the only regressors) in explaining agricultural variables of interest. At best the implications of this type of results are limited. More frequently, however, one is reminded of the old saw all graduate students have heard at least once: "they don't call 'em dummy variables for nothing."

Developments in modern macroeconometrics, in-

cluding the vast regional expectations literature and the already mentioned developments by Christopher Sims in the area of vector autoregression modeling, highlight our naiveté in this regard. It is time agricultural

economists interested in such issues move forward to direct investigations of these matters and stop looking only at secondary effects with second-best techniques.

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