Comment

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COMMENT

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This reply to Gerking's (1979) discussion of "reconciliation" in the construction of input-output tables was written reluctantly. I feel that protracted debate over essentially irreconcilable positions can be at best marginally useful, so I will limit the present discussion to issues raised by Gerking's rejoinder to my 1976 comments.

Gerking's most recent effort and his earlier paper were concerned with the reconciliation of row and column input-output coefficients. What my associates and I did (1970), was to reconcile "rows only" and "columns only" estimates of input-output *transactions.* This distinction is not mere nitpicking. When a research team constructs a transactions table it has the advantage of working with secondary-source control totals which provide sectoral estimates of primary inputs, sales to final demand, and total gross outputs (outlays). These estimates help establish the bounds of total interindustry transactions. The latter, in turn, help evaluate the "reasonableness" of individual coefficients. Gerking's approach was to calculate unbounded row and column coefficients from sample survey data, then reconcile them using a minimum variance criterion.

An understanding of the difference between the two approaches is essential for clarification of the points discussed in the first section of Gerking's 1979 paper: (1) "will column sums of MVR coefficients equal unity?" and (2) the systematic incorporation of what Gerking calls *a priori* information in his procedure.[1]

Gerking concedes that, in general, the MVR coefficients will not sum to unity — an essential condition in all input-output models. This problem, he feels, can be handled by changing the MVR criterion to one which will "minimize the variance of the *sum* of all *reconciled* regional coefficients (emphasis added)...." Gerking calls this new approach the "constrained MVR (CMVR)" and believes it is superior to the MVR. However, the object in input-output

[1] In customary reconciliation methods, including the one my associates and I used, it is probably more correct to talk about the use of *a fortiori* information.

modeling is not to derive sums of coefficients, it is to derive individual coefficients. It would be possible to have a "correct" sum of a column or row — if this sum could actually be known — in which all of the individual coefficients were completely wrong.

In Gerking's method "all establishments in a given sector were assumed to have identical Leontief type production functions," and "the transactions data used to estimate the parameters... were assumed to be subject to random measurement errors." My associates and I knew that the establishments in many sector samples would not have identical production functions. In some cases their production functions were not even remotely alike. This is the aggregation problem in its rawest form. To the best of my knowledge no one has devised any technique, stochastic or deterministic, for dealing with this problem. To make the tables comprehensive we were forced to aggregate unlike establishments. We knew, therefore, that there were more than "random errors" in our transactions data, and the coefficients derived from them. In some cases we had to aggregate to avoid disclosure, but in other cases — even at the 3-digit SIC level — we knew that establishments belonging to the same "industry" used markedly different sets of inputs and produced different outputs. To the best of my knowledge no one has devised a mathematical or econometric technique for dealing with this problem.

I see no point in further belaboring the question: Are input-output models deterministic or stochastic? My own view — which I think is the conventional one — is that they are deterministic, although they are anything but error free. Gerking is correct when he says that "the statistical significance of those traditionally estimated coefficients can never be assessed regardless of the sample sizes on which they are based." This is true of every *operational* input-output model I have ever seen. Income and product accounts — to select only one other simple economic "model" — are also subject to measurement and random error. Ex post estimates of GNP are not presented in probability terms, however.

Gerking maintains that my comparison of his coefficients and those in the West Virginia study are "misleading" and that a West Virginia coefficient is "in at least one case inconsistent with the data" that my associates and I collected. It is true that I compared the column sums of Gerking's coefficients with those given in our book, but this comparison should not have misled anyone. The complete tables were available in published form. However, a comparison limited to the three sectors (14, 16, and 32) for which Gerking had partial survey data (presented in his 1976 article) indicates that Gerking's coefficients are only 11, 20, and 53 percent of those derived in the West Virginia study.

The alleged "inconsistency" is easily explained. As we made abundantly clear (Miernyk, *et al.*, 1970) we tried to minimize, but could not eliminate, judgmental adjustments. Survey data were used to construct "first round" purchase and sales tables. Checks were made with industry experts after reconciliation. When an industry expert told us that certain cell entries were too low (high) — *and could provide supporting evidence* — we made an adjustment on the basis of this judgment.[2] There are a number of cases in which the reported transactions, and the coefficients derived from them, could not have been calculated from the survey data only.

Gerking concludes by stating that his primary interest is "in reducing... arbitrary or *ad hoc* judgments... in obtaining or adjusting regional coefficient

[2] We tried to contact more than one industry expert for those sectors where we thought the survey data were particularly weak.

estimates." This laudable interest is shared by everyone I know who is working with regional or interregional input-output models. It will be interesting to see if the methods he has discussed will actually be used in practical situations.

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REPLY

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Miernyk has raised three issues in connection with my paper: (1) the CMVR method is suspect because the object in input-output modelling is to obtain individual, rather than sums of, coefficients; (2) all establishments in a given sector were assumed to have identical Leontief type production functions; and (3) input-output models are deterministic rather than stochastic, and therefore, methods such as CMVR are of limited interest. In this reply, I respond briefly to each of his points in an effort to show that Miernyk's charges are a bit overzealous. Specifically, the major flaws in Miernyk's remarks lie in his reluctance to apply his criticisms of my proposed reconciliation procedures to his own estimation methods and his failure to distinguish between the statistical properties of an input-output transactions table and those of an input-output model.

Miernyk's first criticism may stem from the use of different objective functions in the MVR and CMVR procedures. While CMVR minimizes the variance of the sum of all reconciled coefficients (subject to certain constraints), MVR minimizes the variance of reconciled coefficients one coefficient at a time. Both procedures provide estimates of individual, rather than sums of, coefficients. However, the estimates provided by CMVR are superior to those provided by MVR for at least two reasons. First, CMVR makes use of row and column constraints that are ignored by the MVR approach. Second, the CMVR objective function explicitly recognizes that the covariance between individual reconciled coefficients may not be zero. In fact, MVR allows for non-zero covariances only between "rows only" and "columns only" estimates of a single regional coefficient. On the other hand, if row and column constraints are ignored and if all coefficient estimates are independent, then MVR and CMVR would yield identical reconciled estimates. This point can be established by reconsidering Equation 12 in Gerking (1979), setting $\lambda_i = \mu_j = 0$ for all i and j, differentiating with respect to the q_{ij}, and then comparing the result to Equation 3.27 in Gerking (1976).

Of course, even in the most general case where non-zero covariances are permitted and constraints are explicitly recognized, Miernyk is correct in suggesting that all of the coefficient estimates produced by CMVR may be completely wrong. But then, this is true of *any* estimation technique, stochastic or otherwise. The only way out of this situation is to know the true values of the regional coefficients with certainty. However, in that case, no data collection or estimation would be necessary.

Miernyk stated that he and his associates "... knew that the establishments in many sector samples would not have identical production functions." I agree that the assumption of identical production functions is rather strong. Yet, this assumption is made in using any input-output model — whether it is made explicitly as in CMVR or implicitly as in traditional procedures. Individual macro coefficient estimates will always be weighted averages of their unequal underlying macro counterparts. Similarly, although Miernyk knew "that there were more than random errors" in his transactions data, his procedure for handling the aggregation problem has no advantage over mine on that score.

Finally, Miernyk insists that even though input-output models are "anything but error free," they are deterministic, rather than stochastic, in nature. In my view, this position represents a fundamental disregard for the difference between an input-output transaction table and an input-output table of technical or regional coefficients. A transactions table, like a set of income and product accounts, is constructed exclusively on the basis of deterministic identities, contains only accounting information, and is not a model of anything. Armed only with the information in these accounts, forecasts of variables such as total sectoral outputs, income, or GNP are simply not possible. These forecasts can only be obtained when the accounting information is employed in conjunction with an economic model such as an input-output model. In this case, the model comes into existence only after making the behavioral assumption that sectoral production functions are of the Leontief type. Once this assumption is in force, then the production parameters have an economic interpretation and the problem of how best to estimate them presents itself. Since, as Miernyk suggests, the transactions data "... are also subject to measurement and *random* [emphasis mine] error," the estimates of the production coefficients will be random as well. Therefore, the view that input-output models are deterministic contains a logical inconsistency and is absolutely untenable. Furthermore, the idea that input-output models are deterministic can hardly be regarded as "conventional" since it is far from being universally shared. Stochastic elements in the estimation of input-output coefficients have been explicitly recognized by authors including Briggs (1957), Hurwicz (1955), Klein (1974, pp. 341-342), Quandt (1958 and 1959), and Rasmussen (1956, pp. 45-47).

I would like to end this exchange with a question: why should empirical input-output *models* be estimated differently from other economic models? In virtually every branch of economics, models, even those constructed from accounting data, are subjected to statistical tests for "goodness of fit" prior to their use in applications such as forecasting. Yet objections are raised when methods to estimate technical or regional coefficients are proposed that would form the basis for such tests. The essential problem is one of choosing the best method from available alternatives in order to estimate input-output production coefficients while at the same time providing an appropriate test of the model. Miernyk's insistence that input-output models are deterministic is, in my view, a giant step in the wrong direction.

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