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variables—precisely the case examined by Gerking—he finds that moments of the finite-sample distribution for the TSLS estimator exist only up to the number of overidentifying restrictions. In the context of equations (1) all structural equations are exactly identified. It follows that none of the moments of this distribution exist. One may obtain parameter estimates, but associated tests of significance are simply not meaningful. The empirical results established by Gerking must be questioned on these grounds.

It should also be recognized that any estimator used to obtain structural coefficients in this model must ensure that both the input and the output identities are satisfied. When coefficient estimates are obtained they must be such that implied interindustry flows ($\hat{Z}_{ij} = \hat{\alpha}_{ij}X_j$) are consistent with the equality of gross output and gross outlay. Without this constraint, comparative static results based on input-output coefficients are not meaningful.

Conclusion

We have attempted to demonstrate a number of serious issues that must be addressed before the application of stochastic estimation techniques to input-output models will have the potential of offering meaningful results. Any analyst who has faced the difficult task of empirical work in this area would applaud the intention of Gerking's paper. We must move in the direction of establishing estimation methods that minimize the significance of individual judgments. However, due to the nature of the information required by the model and the paucity of available data, individual judgments are not easily eliminated.

REFERENCES

Bacharach, Michael, *Biproportional Matrices and Input-Output Change* (Cambridge: Cambridge University Press, 1970).

Czamanski, Stanislaw, and Emil Malizia, "Applicability and Limitations in the Use of National Input-Output Tables for Regional Studies," *Papers, Regional Science Association* 23 (1969), 65–77.

Gerking, Shelby D., "Input-Output as a Simple Econometric Model," this REVIEW 58 (May 1976a), 274–282.

———, *Estimation of Stochastic Input-Output Models* (Hingham, Mass.: Martinus Nijhoff Publishing, 1976b).

———, "Reconciling 'Rows Only' and 'Columns Only' Coefficients in an Input-Output Model," *International Regional Science Review* 1 (1976c), 30–46.

Isard, Walter, and Thomas W. Langford, *Regional Input-Output Study* (Cambridge, MA: MIT Press, 1971).

Miernyk, William H., "Sampling Techniques in Making Regional Industry Forecasts," in A. P. Carter and A. Brody (eds.), *Contributions to Input-Output Analysis* (Amsterdam: North-Holland Publishing Co., 1970), 305–321.

Miernyk, William H., et. al., *Simulating Regional Economic Development* (Lexington, MA: D.C. Heath & Co., 1970).

Richardson, David H., "The Exact Distribution of a Structural Coefficient Estimator," *Journal of the American Statistical Association* 63 (1968), 1214–1226.

INPUT-OUTPUT AS A SIMPLE ECONOMETRIC MODEL: REPLY

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I. Introduction

Brown and Giarratani (BG) (1979) have directed three criticisms at my previous work on estimating the structural parameters of input-output models: (1) aspects of the distribution of stochastic disturbances have not been adequately explored, (2) stochastic methods are unsuitable for making parameter estimates due to the uniqueness of input-output models, and (3) two-stage least squares (2SLS) produces estimates that neither make use of a priori information such as row and column constraints nor possess small sample moments when applied to just-identified equations. The discussion to follow, which briefly addresses each of these alleged difficulties, will in no way deny their existence. Instead, the objective of this reply is to challenge BG's rather overstated conclusion that the ". . . application of stochastic techniques [is] particularly difficult, if not impossible. . . ." (BG, 1979). More specifically, section II indicates that BG's criticism regarding my lack of attention to constraints and a priori information has been recognized and addressed elsewhere. Section III then examines the heteroskedasticity problems that may, in part, characterize the distribution of disturbances, while section IV considers the problem of moments. Finally, section V, which contains some concluding comments, emphasizes the necessity of choosing an estimator based upon the relative strengths and shortcomings of various alternatives, a point that BG curiously neglect to recognize.

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II. Constraints and A Priori Information

BG correctly argue that, regardless of the estimation method selected, estimates of the parameters of an input-output model should obey both the sales and the purchases identities and incorporate available a priori information. As an example of how a priori information might be useful, they observe that the variance of the disturbance term ($\theta_{ij}(r)$) in

$$Z_{ij}(r) = \alpha_{ij}X_j(r) + \theta_{ij}(r) \quad (1)$$

may not be constant across establishments (r) due to differences in size, product mix, capital vintage, and accounting practices. I readily admit that these valid points were not given adequate attention in my earlier work (see especially the three references cited by BG) on input-output estimation methods. However, in response to the criticisms of both Miernyk (1976) and BG, I have attempted to determine how both constraints and a priori information may be utilized in conjunction with the previously proposed estimation methods.[1] Since my results on this subject are published elsewhere (Gerking, 1979), there is little reason to describe them in detail here. Nevertheless, the two main features of this paper, which addresses the question of how to reconcile "rows only" and "columns only" estimates of input-output coefficients, should at least be indicated. First, while the paper does not purport to discuss all types of a priori input-output information, the case of heteroskedasticity in $\theta_{ij}(r)$ arising from accounting practices that differ across r is considered at length. This problem, which BG mentioned in their comment, is shown to have a solution in terms of a straightforward generalization of the 2SLS estimation procedure. Second, a method is suggested for obtaining a minimum variance linear combination of the "rows only" and "columns only" estimates, subject to the constraints imposed by the purchases and sales identities. In the case of independent coefficient estimates, this method is conceptually simple and easy to implement. However, if the coefficient estimates have nonzero covariances, as will generally be true, there will be a significant, though not intractable, increase in computational burden especially in input-output models with a large number of sectors.

III. Heteroskedasticity

BG offer two further reasons to explain why the disturbances in equation (1) should not be expected to exhibit homoskedasticity: (1) for a given sector, the production coefficients (α_{ij}) may not be constant across establishments and (2) the transactions data must be adjusted for trade and transport margins as well as for secondary products.[2] While both of these factors are potentially relevant, the first is especially interesting because it suggests problems in addition to heteroskedasticity. If α_{ij} is interpreted as a regional, rather than as a technical, coefficient, then differences across establishments in the division between in-region and out-of-region purchases would cause this coefficient to vary across establishments as well. Zellner (1962) has shown that this coefficient variation would, in general, cause two problems.[3] First, the usual regression estimator of the disturbance variance is biased upward. This occurs as a direct consequence of the spatial heteroskedasticity in $\theta_{ij}(r)$ that BG mention. Second, a regression estimate of the single (macro) α_{ij} would be a weighted average of the underlying establishment specific (micro) coefficients. In this situation, the macro coefficient would be a biased and inconsistent estimator of the micro coefficients.

Unfortunately, these problems are quite difficult to detect and to adjust for in a practical situation as both the bias and the disturbance variance are functions of the true and unknown values of the establishment specific regional coefficients. However, in order to establish that either problem is important enough to deny ". . . the validity of stochastic methods that assume constant parameters across establishments, . . ." (BG, 1979), BG must do more than simply raise the issue. Instead, there are at least three reasons why they should have demonstrated, either theoretically or by example, that the estimates obtained by ignoring their criticism could be truly misleading. First, varying regional coefficients across establishments in a given sector will cause problems for *any* macro estimator, stochastic or otherwise. Second, coefficient variation across observations has probably occurred in virtually every regression equation ever estimated. Third, after examining the data from 29 sectors of the West Virginia input-output model, I am convinced that (1) the heteroskedasticity problem is nearly always present to some extent and (2) blanket statements cannot be made regarding its source. Nevertheless, in a great many of the estimating equations from these sectors, there was a strong positive correlation between the absolute values of the measured residuals and estab-

[1] BG made me aware of their views on this subject both by providing previous versions of their comment and through private communications.

[2] This last reason, BG argue, also supports the contention that $\theta_{ij}(r)$ may not be independent of $(\theta)_{hk}(r)$ for at least some $i \neq h$ and $j \neq k$. However, rather than causing serious estimation problems, this point may simply affect the choice of an estimator for obtaining the "rows only" and "columns only" coefficients. For example, 3SLS may be preferred to 2SLS on efficiency grounds. Nevertheless, the interdependence of disturbances across equations would certainly increase the computational burden associated with reconciling the two types of coefficients.

[3] Zellner obtained his results on the assumption that OLS is used as an estimation method. Analogous but asymptotic results can be obtained for 2SLS.

lishment size variables such as sales and wage and salary payments. For this reason, I chose to use the Goldfeld-Quandt (1965) test for heteroskedasticity and to treat this problem by deflating the variables in a given equation using a measure of establishment size. The previously mentioned positive correlation does not argue one way or another that other sources of heteroskedasticity are not present. However, in the absence of any evidence, the practical importance of the sources of heteroskedasticity that BG consider is a matter only of conjecture.

IV. The Distribution of Coefficient Estimates

BG also criticize the use of 2SLS in an input-output context. As Richardson (1968) has shown, this estimator does not possess finite moments of order greater than or equal to one in small samples when applied to just-identified equations containing two jointly dependent variables. BG, but not Richardson, conclude that this estimator "fails" because ". . . tests of significance are simply not meaningful" (BG, 1979). Here again, BG make a valid point but then proceed to overstate its importance. In particular, there are two reasons why the lack of moments for 2SLS estimates of equation (1) may not damage the case for stochastic estimators of the α_{ij} to the extent that BG suggest. First, even if BG's conclusion regarding the application of 2SLS to equation (1) is accepted, appropriate alternative estimation methods may be available. For example, OLS, which does yield estimates possessing finite moments in small samples, may actually be a better choice in certain situations than 2SLS. In fact, based presumably on the first-named author's experience with the West Virginia input-output study, they argue indirectly that such situations are likely to arise with regularity (BG, 1979). They state that measurement errors in establishment level observations on total sales may often be small relative to the measurement errors in the intersectoral flows variables. If this assertion is correct, then the interdependence between $X_j(r)$ and $\theta_{ij}(r)$ may not be sufficiently strong to warrant the use of any instrumental variable techniques.

Second, as Mariano and Sawa (1972, p. 162) have indicated, ". . . existence or nonexistence of moments alone can hardly be used as a conclusive basis for determining the merits or demerits of . . . various estimators." These authors, who showed that limited information maximum likelihood has no moments of any order, explicitly recognized one of the undesirable features of such estimators. That is, estimators without moments may have a tendency to give extreme outlyers more frequently than estimators for which moments do exist. However, among available estimators for a certain equation, one without moments may be most suitable. For example, consider a comparison of an estimator with a highly concentrated Cauchy distribution with another normally distributed estimator that has a large variance. Also, the small sample distribution of an estimator without moments may converge, with increases in sample size, to an asymptotic distribution that possesses both finite moments and very similar mathematical properties. In some cases, the really important difference between the small sample and large sample distribution of such an estimator may lie only in the heavier tails of the former. Such a relationship could explain why Monte Carlo studies often show a strong carry-over of asymptotic results to small sample settings in situations where the limits of the exact small sample moments are not the same as the moments of the limiting distribution.

V. A Concluding Comment

In this reply, I have attempted to show that although BG raise valid and potentially important questions regarding my treatment of matters such as row and column constraints, a priori information, heteroskedasticity, and the choice of estimators, their conclusions are stronger than can be justified. To this point, my approach has been structured by addressing each of the issues BG have raised *in seriatim*. However, a thread of inconsistency, that has not yet been addressed, pervades their entire comment. On the one hand, BG state that the stochastic estimation methods that I proposed are not yet sufficiently refined to yield meaningful results when applied to input-output models, but on the other, they never deny that any parameter estimates in such models have random properties. In addition, BG do not suggest alternative estimators that are capable of taking these random properties into account. Apparently, BG are bothered by the fact that stochastic estimators of the α_{ij} impose various statistical problems, are justified only by restrictive assumptions, and are, therefore, imperfect. Nevertheless, the same statements can be made regarding any estimator, including the traditional ratio estimator which, at least implicitly, appears to carry BG's recommendation. More specifically, this ratio estimator, when applied in an input-output context, is constructed so as to mask any random properties that may be present. With respect to estimating the α_{ij}, the essential problem at hand, then, is one of either choosing the best method from the menu of alternatives or else expanding the menu. Consequently, the ratio estimator would not be an appropriate choice, unless of course, the assumption that all of the $\theta_{ij} \equiv 0$ is somehow more defensible than the assumptions underlying its competitors.

REFERENCES

Brown, Douglas, and Frank Giarratani, "Input-Output as a Simple Econometric Model: A Comment," this REVIEW 61 (Nov. 1979).

Gerking, Shelby D., "Reconciling Reconciliation Procedures in Input-Output Analysis," *International Regional Science Review* 4 (Spring 1979).

Goldfeld, Stephen, and Richard Quandt, "Some Tests for Homoskedasticity," *Journal of the American Statistical Association* 60 (Sept. 1965), 539–547.

Mariano, R. S., and T. Sawa, "The Exact Finite Sample Distribution of the Limited Information Maximum Likelihood Estimator in the Case of Two Endogenous Variables," *Journal of the American Statistical Association* 67 (Mar. 1972), 159–163.

Miernyk, William et al., *Simulating Regional Economic Development* (Lexington, Mass.: D.C. Heath, 1970).

———, "Comments on Recent Developments in Regional Input-Output Analysis," *International Regional Science Review* 1 (Fall 1976), 47–55.

Richardson, D. H., "The Exact Distribution of a Structural Coefficient Estimator," *Journal of the American Statistical Association* 63 (Dec. 1968), 1214–1226.

Zellner, Arnold, "Estimation of Cross-Section Relations: Analysis of a Common Specification Error," *Metroeconomica* 14 (1962), 111–117.

AN EMPIRICAL TEST OF THE LOCK-IN EFFECT OF THE CAPITAL GAINS TAX

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The aim of this note is to present a simple model of the decision to sell an asset and to estimate the lock-in effect of the capital-gains tax in the stock market.[1] The main conclusion reached is that high-income investors sacrifice an annual return of approximately 1½% of the value of their stock as a result of the lock-in effect. For low-income investors the effect is weaker.

The approach used here is to assume that an investor with locked-in assets forgoes part of the expected gross rate of return that he could get in the market. Hence if the lock-in effect is significant, the expected gross rate of return should be lower, the larger the fraction of capital gains embodied in the asset. This approach may be viewed as a testable modification of that of Holt and Shelton (1962).

I. The Model

Let R be the expected return on a share, and let R_a be the expected return of an alternative share. (The alternative share is that with the highest rate of return which is substitutable for the share in the portfolio. It may have different characteristics from the latter; in that case we should interpret R_a as the rate of return adjusted for differences in other characteristics.) A switch from the portfolio share to its alternative occurs if

$$R < R_a - C, \tag{1}$$

where C is the equivalent, in terms of the rate of return, of the transaction cost of the switch.

From (1) we can derive the expected rate of return on a share that continues to be held, R_h, and the expected rate of return on a share that is sold, R_s. Formally,

$$R_h = \{R | R > R_a - C\} \tag{2}$$

$$R_s = \{R | R < R_a - C\}. \tag{3}$$

Assume that R and R_a are random variables drawn from given distributions. Then R_h and R_s are decreasing functions of C. The transaction cost, C, includes the capital-gains tax; hence it is a function of the capital gains embodied in the share.[2] Since capital gains are correlated with the holding period we may expect C to be positively correlated with the holding period. Hence R_h and R_s are decreasing functions of the holding period.

The expected return on a share that is held for n periods can be written as[3]

$$S/P_{|n} = R_s(C_n) \prod_{j=1}^{n-1} R_h(C_j), \tag{4}$$

where S is the (expected) selling price of the share, P is the purchase price, and C_j is the transaction cost in period j. The expected return on a share which is held $n - 1$ periods is, similarly,

$$S/P_{|n-1} = R_s(C_{n-1}) \prod_{j=1}^{n-2} R_h(C_j). \tag{5}$$

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[1] For a theoretical and empirical discussion of the lock-in effect see Bailey (1969), Diamond (1975), Feldstein and Yitzhaki (1978), and Holt and Shelton (1962).

[2] The transaction cost is also a function of the asset holder's age. Data limitations do not allow us to test the effect of age.

[3] To avoid a complex notation we use R to stand for either "rate" or "1 + rate," as appropriate. The results are of course presented in percentage terms.