Preface

This book deals with five of the most fundamental problems in applied economics:

- How to estimate consumer preferences;
- How to estimate producer's production functions or any of the dual representations of technology;
- How to aggregate over economic agents;
- How to determine whether an economic aggregate *exists* that has the usual microeconomic properties;
- How to aggregate over commodities.

The first two problems are indeed fundamental. Preferences determine consumer demand and labor supply functions while production functions determine output supply and input demand functions. Virtually all important problems in applied economics depend on the accurate determination of these functions and the elasticities of demand and supply that they generate. Parts 1 and 2 of this volume deal extensively with these first two problems.

The third fundamental problem is an aggregation problem; namely, the aggregation over agents problem. In the household context, we ask how can we best aggregate over consumer demand functions and what properties will the resulting market demand functions possess. Chapter 1 can be viewed as a contribution to this literature. In the producer context, we similarly ask how can we best aggregate over individual firm supply and demand functions in order to obtain the corresponding market supply and demand functions. As Barnett and Zhou observe in Chapter 16, a result in Debreu (1959, p. 45) can be used to prove the existence of an aggregate technology that satisfies all the usual firm microeconomic properties, provided that there is price taking behavior over all producers in the industrial aggregate and provided that there are no fixed inputs.[1]

The fourth problem listed above is also of some importance to applied economists. There are millions of goods and services that are used in a modern

[1] Bliss (1975, p. 146) also noted that if all producers are competitive profit maximizers and face the same prices, then the group of producers can be treated as if they were a single producer subject to the sum of the individual production sets. This result can be traced back to May (1946) and Pu (1946). Thus aggregation over producers will be easier to justify if annual data are used instead of monthly or quarterly data, since many inputs are likely to be fixed in the short run.

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economy. Even with powerful computers, it is impossible to estimate flexible preferences or production functions once the number of goods in the model exceeds 100 or so.[2] However, if utility or production functions satisfy *separability conditions*,[3] then the number of parameters required to represent accurately these functions can shrink quite dramatically. Moreover, a large number of economic models simply assume various separability conditions. For example, many consumer models assume that labor supply or leisure demand is separable from the demands for goods and services. Virtually all intertemporal consumer models assume some type of separability between consumption in the present period vs. consumption in future periods. Hence, it is very useful to have at our disposal some econometric techniques that enable us to test for the existence of separable aggregates. However, Blackorby et al. (1977) showed that testing for separable aggregates in a flexible functional form context is not a trivial problem. The innovative work of Professor Barnett and his co-authors on this topic may be found in Chapters 11, 12, 15, and 16 of this volume.[4]

The fifth fundamental problem in applied economics is also an aggregation problem; given that we plan to aggregate over a number of commodities, what is the "best" way of forming this aggregate? This is the *index number problem.* Professor Barnett and his co-authors have done a considerable amount of work in this area as well; see the monograph edited by Barnett and Serletis (2000). Several of the chapters in the present volume draw on this earlier research on the index number problem and extend this research. In particular, Chapter 16 looks at the problem of constructing commodity aggregates when there is uncertainty or risk in the model.

Let us return to the first two fundamental problems in applied economics listed above. The chapters in sections 1.3, 1.4 and 2.2 of this volume deal with virtually all of the problems that arise when it is attempted to estimate utility or production functions (or their dual representations) in a flexible way. The theory of flexible functional forms started with the contributions of Diewert (1971) and Christensen et al. (1971, 1973, 1975), who introduced the Generalized Leontief and Translog functional forms into the economics literature. These functional forms are capable of providing second-order approximations to any twice continuously

[2] I have had some success estimating the Diewert and Wales (1988a) semiflexible normalized quadratic functional form for 50 or so goods in the production context but I have not gone beyond this limit.

[3] See Blackorby et al. (1978) for the best systematic exposition of separability concepts and their role in economics.

[4] I have also worked in this area of research: see Diewert and Parkan (1985) for a nonparametric approach and Diewert and Wales (1995) for an econometric approach. This econometric approach is used as a building block in Chapter 16 of the present volume.

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differentiable function (satisfying the appropriate regularity conditions) around any specified point. Thus these functional forms can provide first-order approximations to arbitrary supply or demand functions without imposing unwarranted *a priori* restrictions on elasticities of supply or demand. However, a problem with these functional forms soon became apparent: as the number of commodities in the model grew, it proved to be impossible to impose the correct curvature conditions on these functional forms without destroying the flexibility of the functional form.[5] This negative result led to search for a *parsimonious flexible functional form* where the correct curvature conditions would hold globally without destroying the flexibility of the functional form. There was also a search for functional forms that could do better than just achieving a second-order approximation. Several of the chapters in this volume contributed strongly to this literature; in particular, see Chapters 4–10 and 14–16. However, in my opinion, this literature has not been completely successful: Diewert and Wales (1993, 89–92) reviewed this literature and found problems with *all* of the models that had been suggested up to that point in time, with one exception. Diewert and Wales suggested that the normalized quadratic functional form[6] was the best parsimonious flexible functional form, since it was the only known parsimonious form where curvature conditions could be imposed globally without destroying the flexibility of the functional form. However, subsequent research has revealed a flaw with this form: the elasticities of derived demand or supply that this functional form generates will tend to have systematic *trends* in them in the time series context, trends that are an artifact of the functional form rather than reflecting the "truth".[7] This defect of the normalized quadratic functional form can readily be fixed in the producer context by making the functional form flexible at two points: one near the beginning of the sample period, and one near the end; see Diewert and Lawrence (2002, pp. 150–151). However, there is a significant cost in making this modification, since the resulting functional form is no longer parsimonious: it has double the number of parameters of the initial form.[8] Hence, it seems that all of the difficult issues involved in finding the best functional form for applied work in economics have still not been settled in a definitive manner.

In addition to the above general points about this volume, some specific comments on some of the chapters are made below.

[5] See Diewert and Wales (1987) on this point.

[6] For the properties and applications of this functional form, see Diewert and Wales (1987, 1988a,b, 1992, 1993).

[7] See Diewert and Lawrence (2002, pp. 149–150).

[8] Of course, this is to be expected since we achieve flexibility at two points instead of one point using the modified functional form.

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In Chapter 14, Barnett, Geweke, and Wolfe use Bayesian methods in order to aid in the estimation of a flexible functional form model.[9] In general, I do not favor Bayesian procedures. There is nothing logically wrong about a Bayesian estimation method, but these methods introduce an additional layer of complexity to the overall modeling exercise. In a non-Bayesian model, it is necessary to:

- Decide which variables are important and should be included in the model;
- Decide which variables are exogenous and which are endogenous;
- Decide on functional forms that relate the exogenous variables to the endogenous variables;
- Specify something about the error terms that are added to the estimating equations or equivalently, specify something about the conditional distributions of the dependent variables, given the exogenous variables.

In a Bayesian model, it is necessary to make an additional layer of assumptions: namely, it is necessary to assume that the parameters in the above "classical" model satisfy some *a priori* distributional assumptions, which must be chosen by the econometrician. But on what informational base will these distributional assumptions be based? Many applied economics problems tend to be one of a kind, making use of unique data set and thus the typical applied economist will have no idea of how to parameterize *a priori* the unknown parameters in the model. Thus this problem of specifying prior distributions simply adds another layer of uncertainty to the outcome of the modeling exercise, leading to the *nonreproducibility* of the results. Results are *reproducible* if the average competent applied econometrician, when given the relevant data set, would come up with much the same answer to the applied economics problem that motivated the model. This concept of reproducibility is much broader than the usual narrow one where the econometrician hands in his programs and spreadsheets at the end of the modeling exercise and tells the client that the results can readily be replicated.[10] It can be seen that the

[9] Bayesian techniques did not play a large role in this chapter since classical estimation techniques were used as well.

[10] I became aware of the broad sense reproducibility problem in working with statistical agencies who have to produce a consumer price index. These agencies try very hard to produce indexes which are reproducible, based on best practice and can be explained to the public. Thus they try to base their index procedures on principles that are broadly acceptable so that if two such agencies suddenly switched personnel and all their computer programs were lost in the changeover, then nevertheless, the switched agencies would still produce much the same index in each country.

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Bayesian approach will tend to lead to a lack of reproducibility in the broad sense.[11]

There are a few more problem areas associated with the estimation of production and utility functions where the reproducibility issue again emerges. One of the major problem areas is concerned with the *endogeneity of the exogenous variables* or with exogenous variables that are measured with error, leading to correlation of the observed "exogenous" variables with the error terms in the regressions. I think that Professor Barnett and myself start out using similar methodologies; namely, represent preferences or technology using a dual function, differentiate this dual function in order to obtain derived demands or supplies (which are quantities) and then use these equations, which treat prices as exogenous, as the basis for estimating equations. Thus our regression equations tend to treat quantities as the endogenous variables and prices are regarded as exogenous. Thus we tend to estimate conditional mean functions for quantities, conditioning on prices. But, of course, as Professor Barnett recognizes throughout the volume, the prices which appear in our regression equations are constructed using imperfect index number techniques and are certain to have measurement error in them. Under these conditions, classical OLS estimation will generally lead to biased estimates of the parameters that characterize tastes or technology, which is not a good thing.

One solution to this problem is to use *instrumental variable estimation* in place of OLS and its system variants.[12] But there is a lack of reproducibility associated with the use of instrumental variable estimation in finite samples. Davidson and Mackinnon (1993, pp. 218–219) explain the problems associated with instrumental variable (IV) estimation methods as follows:

> The biggest problem with using IV procedures in practice is choosing the matrix of instruments **W**. Even though every valid set of instruments will yield consistent estimates, different choices will yield different estimates in any finite sample.... Thus, in practice, there are usually many reasonable ways to choose **W**. There are two conflicting objectives in the choice of **W**. On the one hand, we would like to obtain estimates that are as efficient as possible

[11] It could be argued that classical procedures are not immune to this type of criticism as well; after all, much of this volume is concerned with choosing a functional form for the underlying economic model and we have seen that this is a decision where there is not a lot of agreement. However, I am arguing that a Bayesian procedure will just make the reproducibility problem worse.

[12] Heckman (2000) provides a far ranging discussion and a very large and useful set of references dealing with this topic from different perspectives.

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asymptotically. On the other hand, we would like to obtain estimates that have as small a finite sample bias as possible. Unfortunately, these objectives turn out to conflict with each other.

Thus as was the case with the use of Bayesian estimation, *the use of instrumental variable estimation will generally lead to a lack of reproducibility.* Even after choosing exactly the same economic model to explain the same data set, applied economists, using instrumental variable estimation, will generally choose different sets of instruments, leading to different parameter estimates and an overall lack of reproducibility in their economic advice.

Instead of using instrumental variable estimation techniques to solve the problem of errors in the exogenous variables, we might turn to the *cointegration literature*, which at first glance, seems relevant to our problem where both price and quantity variables contain some form of measurement or approximation error. We will explain the approach, following the exposition in Davidson and Mackinnon (1993, p. 716), in the context of a one price and one quantity model. Thus let **x** and **y** be N-dimensional stochastic vectors[13] that are linked by a long run equilibrium relationship of the following form:

$$\eta_1 \mathbf{x} + \eta_2 \mathbf{y} = \eta_3 \mathbf{z} + \mathbf{v} \tag{1}$$

where **v** is a stationary error vector and **z** is an N-dimensional vector of nonstochastic explanatory variables and η_1, η_2, and η_3 are unknown parameters to be determined.[14] The two-dimensional vector $[\eta_1, \eta_2]$ is called a *cointegrating vector.* A simple partial equilibrium supply or demand model would fit into this framework, where we let **x** and **y** be a vector of prices and quantities, respectively. The applied econometrician is interested in determining the ratio of η_1 to η_2, since once this ratio is known, an elasticity of demand or supply can be calculated. Now Equation 1 can be divided by η_2 and η_1, respectively in order to obtain the following two regression equations:

$$\mathbf{y} = \alpha \mathbf{x} + \beta \mathbf{z} + \mathbf{v}/\eta_2, \tag{2}$$

$$\mathbf{x} = \gamma \mathbf{y} + \delta \mathbf{z} + \mathbf{v}/\eta_1, \tag{3}$$

where $\alpha \equiv -\eta_1/\eta_2, \gamma \equiv -\eta_2/\eta_1 = 1/\alpha, \beta \equiv \eta_3/\eta_2$ and $\delta \equiv \eta_3/\eta_1$. It can be verified that the regressions 2 and 3 do not satisfy the condition that the right-hand side explanatory vectors are uncorrelated with the error terms. Nevertheless, as Davidson and Mackinnon (1993, p. 717) point out, the simplest way to estimate a

[13] These two vectors are assumed to be integrated of order one; i.e., their first differences are stationary.
[14] We assume η_1 and η_2 are both nonzero.

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cointegrating vector is to estimate the two models 2 and 3 using OLS. The OLS estimators for α and γ (the structural parameters of most interest to the applied economist) are defined as follows:

$$\alpha^* \equiv [\mathbf{z}^T\mathbf{z}\mathbf{x}^T\mathbf{y} - \mathbf{x}^T\mathbf{z}\mathbf{z}^T\mathbf{y}]/[\mathbf{x}^T\mathbf{x}\mathbf{z}^T\mathbf{z} - \mathbf{x}^T\mathbf{z}\mathbf{z}^T\mathbf{x}] = \mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y}/\mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{x} \quad (4)$$

$$\gamma^* \equiv [\mathbf{z}^T\mathbf{z}\mathbf{y}^T\mathbf{x} - \mathbf{y}^T\mathbf{z}\mathbf{z}^T\mathbf{x}]/[\mathbf{y}^T\mathbf{y}\mathbf{z}^T\mathbf{z} - \mathbf{y}^T\mathbf{z}\mathbf{z}^T\mathbf{y}] = \mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y}/\mathbf{y}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y} \quad (5)$$

where $\mathbf{M}$ is the following rank 1 projection matrix:

$$\mathbf{M} \equiv \mathbf{z}(\mathbf{z}^T\mathbf{z})^{-1}\mathbf{z}^T. \quad (6)$$

Davidson and Mackinnon (1993, p. 718) note that the above estimates for α and $\gamma \equiv 1/\alpha$ are consistent under their assumptions. But this is a large sample property. In small samples, there will be a systematic inequality between the two ways of estimating α, as we will now show.

We assume that $\mathbf{x}$, $\mathbf{y}$, and $\mathbf{z}$ are linearly independent so that the denominators in Equations 4 and 5 will be positive. We also assume that $\mathbf{x}$ and $\mathbf{y}$ are correlated[15] in the subspace orthogonal to $\mathbf{M}$; i.e., assume that

$$\mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y} \neq 0. \quad (7)$$

The generalized Cauchy–Schwarz inequality[16] implies that

$$(\mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y})^2 < (\mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{x})^2(\mathbf{y}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y})^2. \quad (8)$$

Using Equations 4, 5, 7, and 8, it can be seen that the following inequalities hold between the two ways of estimating α :[17]

$$\mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y} > 0 \text{ implies } \alpha^* < 1/\gamma^*; \quad (9)$$

$$\mathbf{x}^T[\mathbf{I}_N - \mathbf{M}]\mathbf{y} < 0 \text{ implies } \alpha^* > 1/\gamma^*. \quad (10)$$

The results (Equations 9 and 10) are independent of the particular stochastic specification for the model defined by Equation 1; *these results depend only on the algebra pertaining to the two least squares regression models.* The point is this: if

[15] Strictly speaking, this correlation terminology is only accurate if $\mathbf{z}$ is a vector of ones.
[16] See Rao (1965, p. 43). Our assumption that $\mathbf{x}$, $\mathbf{y}$ and $\mathbf{z}$ are linearly independent implies that the strict inequality holds in Equation 8 rather than the corresponding weak inequality.
[17] The inequalities 9 and 10 generalize a related result due to Bartelsman (1995) where he set $\mathbf{z}$ equal to a vector of ones and considered only case 9. The above results can readily be generalized to the case where the vector $\mathbf{z}$ in Equation 1 is replaced by an N by K matrix of exogenous variables $\mathbf{Z}$, where $K + 1$ is less than N, and $\mathbf{x}$ and $\mathbf{y}$ and the columns of $\mathbf{Z}$ are linearly independent. In this case, the inequalities 9 and 10 are still valid but the old rank one projection matrix $\mathbf{M}$ defined by Equation 6 is now replaced by the rank K projection matrix, $\mathbf{M} \equiv \mathbf{Z}(\mathbf{Z}^T\mathbf{Z})^{-1}\mathbf{Z}^T$. The proof follows by using the Frisch, Waugh, and Lovell Theorem; see Davidson and Mackinnon (1993, pp. 19–21).

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we have a single linear regression with two jointly dependent variables and we decide to estimate the structural parameters in the model by running two conditional regressions, one where **y** is the dependent variable and one where **x** is the dependent variable,[18] and if we want a relatively large or small estimator for α (in order to please a client for example), then we can strategically choose to run either Equation 2 or 3 to achieve this objective. This is a very unsatisfactory state of affairs.[19] Again, there is a lack of reproducibility due to the possibility that different applied economists will choose to run different conditional regressions.

What is the solution to the problems that arise in regression analysis when prices and quantities are measured with error? The problem is not that solutions have not been suggested; rather it is that the applied economist has a huge array of possible solutions and a consensus has not emerged on what is *the* best practice technique. We have already noted that instrumental variable techniques[20] could be used. But we could also turn to a regression model which treats **x** and **y** in a symmetric manner; the literature that follows this approach dates back to Adcock (1878).[21] The literature on models that have errors in the variables also has many additional suggestions on how to deal with the problem.[22] Heckman (2000) reviews many additional techniques that could be used to address the problem. It would be useful for Professor Barnett to reflect on this problem and try to distill

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[18] Let **y** be a quantity vector and let **x** be the corresponding vector of prices. Then the regression 2 is a direct regression of price on quantity and generates a direct estimate, α^*, of the effects on quantity supplied or demanded of a change in price. The regression 3 generates an indirect estimate of the same effect, $1/\gamma^*$. In the case where we are estimating an input demand function or a consumer demand function, α^* and $1/\gamma^*$ will be negative and we will be in case 10. In the case where we are estimating an output supply function, α^* and $1/\gamma^*$ will be positive and we will be in case 9. In both cases, it can be seen that the absolute value of the direct estimate will generally be less than the absolute value of the indirect estimate; i.e., in both cases, we have $|\alpha^*| < 1/|\gamma^*|$. Thus own price elasticities estimated *directly* by regressing quantities on prices will generally be *less in magnitude* than when estimated *indirectly* by regressing prices on quantities. This is a very troublesome result since the direct and indirect estimates can be very different.

[19] Davidson and Mackinnon (1993, p. 721) also recognized that the fact that the different regressions corresponding to different normalizations of Equation 1 will give rise to different results in finite samples is somewhat troublesome: "The OLS estimates of η depend on which one of the y_is is treated as the regressand. Changing the regressand will, in finite samples, change the residual vector **v** and hence change the calculated values of any cointegration test statistics based on that vector. This is rather unfortunate, because there are already a great many possible test statistics. Thus for cointegration tests even more than for unit root tests, there are likely to be plenty of opportunities for different tests to yield conflicting inferences".

[20] For the history of the method and additional references to the literature, see Heckman (2000, p. 63).

[21] In the applied mathematics and science literature, this symmetric regression approach is known as "total least squares"; see Golub and Van Loan (1980). See also Van Huffel and Vandewalle (1991) for an extensive review of this literature.

[22] See for example Madanski (1959).

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his knowledge of econometric theory and his extensive practical experience in the area of preference and technology estimation into a readable "cookbook" of best practice methods for doing the econometric estimation. Such a monograph or review article would be tremendously useful to the applied economics community, since forming a consensus on best practice would reduce the lack of reproducibility that current competing strands of econometric knowledge have generated for applied economists working in industry and government.

In my own work in this area of preference and technology estimation, I have tried to use prices as the exogenous variables as much as possible. From the viewpoint of economic theory, this seems appropriate in most cases, since our basic consumer and producer models assume that economic agents treat prices as exogenous signals and use these signals in their optimization problems in order to determine quantities demanded or supplied. But price and quantity data used in applied economics inevitably contain errors and this means that the usual conditions required to justify least squares estimation are not satisfied. However, it turns out that quantities are usually much more variable than prices.[23] Hence, it seems likely that the errors in the quantity aggregates are bigger than the errors in the corresponding price aggregates. This in turn suggests that biases due to the incorrect application of least squares methods will be lower using prices as the conditioning variables rather than using quantities. Hence, keeping in mind reproducibility considerations, I will probably continue to use least squares type estimation methods with prices as exogenous variables when estimating flexible functional forms. When Professor Barnett proposes the best practice alternative method of estimation, I will enthusiastically embrace it.

Chapter 16 is probably the most interesting and challenging chapter in the volume. This chapter by Barnett and Zhou presents an interesting intertemporal maximization problem for a financial firm using flexible functional forms but also taking the risk considerations that financial firms face into account. The authors also look at the implications of their model for the construction of index numbers in an uncertain context. The problem with this chapter is: there is a tremendous amount of interesting material presented all too briefly! There is a need for the authors to turn this chapter into a separate book, where all the various assumptions that go into the model could be discussed at greater length. In particular, given the intertemporal separability assumptions that the authors make, it would be useful to know how many of the implications of their complete model could be captured by a conditional one period model. I would also like to see a lengthier discussion of how expectations about future returns on the various asset classes were calculated. It would also be useful to have a more extended discussion on how best to choose

[23] On this point, see for example, Allen and Diewert (1981).

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the "correct" price deflator for financial variables, since this deflator plays a large role in the calculation of real quantities in the model. There are also some interesting econometric issues that arise when modeling economic behavior under risk since invariably, first order conditions involving a utility function crop up and usually the resulting estimating equations cannot be explicitly solved so that quantities are on the left-hand side and prices are on the right-hand side of the estimating equations. Usually, instrumental variable estimating techniques are used in this context but again, there is a bit of a worry about the reproducibility of the results in the small samples that the applied economist usually works with.[24] Finally, I note that the chapter uses the usual expected utility framework for modeling firm behavior. This is quite acceptable in such a pioneering work but it should be noted that the expected utility model has proven to be not quite flexible enough to *always* model economic behavior when there is risk.[25]

In Chapter 17, Professor Barnett observes that in the literature on modeling the estimation of tastes and technology, it has become common to impose curvature globally, but not monotonicity. He further points out, that in some cases, these models generate isoquants that have positive slopes at one or both ends of the isoquant. Thus he concludes:

> For decades, econometricians have been searching for a model that would permit flexibility and global regularity to be attained simultaneously with a parsimonious model having a finite number of parameters. In my opinion, availability of that capability has still not been demonstrated.

It will be useful to look at the capabilities of the normalized quadratic functional form to attain global regularity in the light of the above observations. I agree that this functional form, with the correct curvature conditions imposed globally, will frequently fail global monotonicity. But under what conditions will this matter to the applied economist? Consider for concreteness, a situation where we want to estimate a single output normalized quadratic cost function of the form $C(\mathbf{y}, \mathbf{p})$, where C is the cost function, $\mathbf{y}$ is output and $\mathbf{p}$ is a vector of input prices. Suppose that we use the following system of derived demand equations as the

[24] Estimation based on the method of moments is also subject to a lack of reproducibility in small samples. How many moments should be used in the estimation procedure? Are all the chosen moment equations equally important or should they be weighted somehow? And so on.

[25] See Diewert (1993, p. 405) for some references to the "paradoxes" literature. Diewert (1993, pp. 415–432, 1995b) shows how a relatively simple extension of the expected utility model can "explain" many of these paradoxes.

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system of estimating equations:

$$\mathbf{x}^t = \nabla_{\mathbf{p}} C(\mathbf{y}^t, \mathbf{p}^t) + \mathbf{e}^t; \qquad t = 1, 2, \ldots, T \tag{11}$$

where $\mathbf{x}^t$ is the positive input vector observed in period t, $\mathbf{y}^t$ is the corresponding observed output, $\mathbf{p}^t$ is the vector of observed period t input prices and $\mathbf{e}^t$ is a vector of period t error terms. If the fit in the system of estimating equations 11 is good, then monotonicity is very likely to hold over the convex hull of the set of sample prices. For most applications, this will be satisfactory. On the other hand, if the fit is poor, then monotonicity may well fail over part of the sample space. In this case, I agree with Professor Barnett that there is a problem. There is another way in which monotonicity could fail. Suppose that input data are not available but data, C^t, on observed costs in each period are available. Then in place of Equation 11, we could run the following single equation regression model:

$$C^t = C(\mathbf{y}^t, \mathbf{p}^t) + \mathbf{u}^t; \qquad t = 1, 2, \ldots, T \tag{12}$$

where $\mathbf{u}^t$ is a scalar error term. In this setup, monotonicity is very likely to fail, even over the sample region, due to multicollinearity problems. In this case, it will be necessary to impose monotonicity.

Thus Professor Barnett is right to caution us about the possible failure of monotonicity, but, keeping in mind the above limitations, I still lean towards the use of the normalized quadratic functional form with curvature imposed (but not monotonicity unless it becomes necessary to do this).

Section 4 of the volume is good fun. Professor Barnett and his co-authors take a look at the econometric problems involved in detecting whether economic variables exhibit chaotic behavior. I will not describe these chapters in detail except to say that I particularly enjoyed Chapter 26.

I conclude with some words of praise for Professor Barnett and his co-authors.[26] The chapters in this volume demonstrate a tremendous grasp of economic theory, applied mathematics, statistics and econometrics. The list of Journals where many of these chapters first appeared is very impressive.[27] All in all, this volume is a substantial contribution to the five problem areas in applied economics that were listed at the beginning of this Preface.

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[26] His co-authors are: Ping Chen, Seungmook Choi, A. Ronald Gallant, John Geweke, Jeong Ho Hahm, Melvin J. Hinich, Mark J. Jensen, Barry E. Jones, Jochen A. Jungeilges, Daniel T. Kaplan, Yul W. Lee, Travis D. Nesmith, Michael D. Wolfe, Piyu Yue, and Ge Zhou.

[27] These journals include the following ones: *Journal of Econometrics, Journal of Economic Behavior and Organization, Journal of the American Statistical Association, Journal of Political Economy, Federal Reserve Bank of St. Louis Review, Journal of Business and Economic Statistics, Econometrica, Advances in Econometrics, Economics Letters, Review of Economic Studies.*

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Where do we go from here? I have the following research wish list that I hope that Professor Barnett will undertake in the future:

- Write a lengthy survey paper that would give applied economists his vision of what the best practice techniques are in estimating preferences and technology.
- Write a book or monograph that would present the material in Chapter 16 in greater depth with more discussion of alternative approaches to modeling the financial firm's choice problems when there is uncertainty.
- Write a paper that would lay out the different options for a choice of a deflator for monetary variables.

I look forward to Professor Barnett's future contributions on the above topics.

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