PREFACE TO THE THEORY OF MONETARY AGGREGATION

by W. Erwin Diewert,
University of British Columbia,
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W. T. Foster wrote the following lines as a start to his preface of Irving Fisher's classic study, *The Making of Index Numbers*:

> "To determine the pressure of steam, we do not take a popular vote; we consult a gauge. Concerning a patient's temperature, we do not ask for opinions: we read a thermometer. In economics, however, as in education, though the need for measurement is as great as in physics or in medicine, we have been guided in the past largely by opinions. In the future, we must substitute measurement. Toward this end, we must agree upon instruments of measurement. That is the subject of this book."

The above lines are also an appropriate introduction to the present book, edited by William Barnett and Apostolos Serletis. The present book is a collection of papers by Barnett and his co-authors (E. Offenbacher, P. Spindt, A. Serletis, M. Hinich, P. Yue, Y. Liu, M. Jensen, H. Xu, G. Zhou, D. Fisher, W. E. Weber, J. H. Hahm, M. Kirova, and M. Pasupathy). Each paper has better measurement as its central theme and hence this book follows in the tradition of Irving Fisher, who also tried to improve economic measurement. In what follows, when I refer to Barnett, this should be understood as a shorthand notation for Barnett and his co-authors, when appropriate.

Barnett's basic research program has been to integrate monetary theory into macroeconomics starting with microeconomic theory and then using index number and aggregation theory to go from microeconomics to macroeconomics. Barnett has also used modern econometric techniques to estimate demand and supply functions for money and test for the existence of various monetary aggregates. More specifically, some of the major theoretical contributions of Barnett, which appear in this book, are: (i) producer and consumer user costs for money are rigorously derived and used as the appropriate prices for monetary components; (ii) the insertion of real balances into neoclassical utility and production functions is rigorously justified using the work of Fischer, Feenstra, and others; (iii) when aggregating commodities, superlative index number formulae are used; (iv) flexible functional forms for utility and production functions are consistently used throughout the book; (v) modern developments in testing for the existence of weakly separable aggregates are used to test for the existence of various monetary aggregates; and (vi) the usual consumer and producer models are extended to include *risk* in a fundamental way. I would also like to note the contribution made in chapters 3 and 19 where Barnett points out that the existence of bank reserve requirements creates a regulatory wedge in the user cost of money. That is, the reserve requirement acts like a capital tax on the bank and thus the user cost of money will be different on the supply (or bank) side of the market compared to the demand side of the market. This point creates a tremendous difficulty for macro models or applied general equilibrium models: there is no unique price that can equilibrate the demand and supply of money!

In addition to the above theoretical contributions, Barnett compares the performance of his superlative indexes, which use monetary user costs, with simple sum monetary aggregates, which do not use user costs. In chapter 24, he notes that Milton Friedman predicted that a resurgence of inflation would inevitably follow the explosion that occurred in the simple sum aggregates for the U. S. from late 1982 to mid 1983. Friedman also predicted that once the inevitable inflation began, the Federal Reserve would tighten monetary policy in a manner that would produce a recession. However, on the very same day that Friedman made his prediction, Barnett went on the record with a dramatically different forecast based on his superlative Divisia monetary indexes (which showed no monetary explosion). In fact, Friedman's predicted inflation and subsequent recession did not occur.

It is also interesting to observe what happened during the immediately preceding period. The following quotation, taken from pages 581-582 of chapter 24, explains how the different measurement techniques led to very different numerical estimates of money supply growth and to the mistakes in policy between 1979 and 1982 that produced the recession of 1982:

> "As I reported in Barnett (1984), the growth rate of simple sum M2 during the period of the 'monetarist experiment' averaged 9.3%, while the growth rate of Divisia M2 during the period averaged 4.5%. Similarly, the growth rate of simple sum M3 during the period averaged 10%, while the growth rate of Divisia M3 during the period averaged 4.8%. This period followed double digit growth rates of all simple sum and Divisia monetary aggregates. In short, believers in simple sum monetary aggregation, who had been the advocates of the 'monetarist experiment', were put in the embarrassing position of witnessing an outcome (the subsequent recession) that was inconsistent with the intent of the prescribed policy and with the behavior of the simple sum aggregates during the period. This unwelcome and unexpected outcome rendered vulnerable those economists who advocated a policy based upon the assumption of a stable simple sum demand for money function.
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> Friedman's very visible forecast error on 26 September 1983 followed closely on the heels of the end of the monetarist experiment in August 1982 and the recession that it produced. The road buckled and collapsed below the monetarists and those who believed in stable simple sum demand for money functions. Those two associated groups have never recovered.
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> But the recession that followed the monetarist experiment was no surprise to anyone who had followed the Divisia monetary aggregates, since those aggregates indicated that a severe deflationary shock had occurred. To those who were using data based upon valid index number and aggregation theory, rather than the obsolete simple sum monetary aggregates, the road remained smooth—no bumps, no breaks. Nothing unexpected had happened."

The above quotation shows that measurement matters! It is a topic that is dear to my heart, having labored in the measurement field for some 25 years. Thus it is perhaps no surprise that I am very enthusiastic about the basic Barnett research program: there is a substantial overlap in our research agendas. I too have worked with user costs,

aggregation theory, flexible functional forms, tests for separability and superlative index numbers. In Diewert (1974), I derived a very simple user cost formula for non-interest bearing money, but I did not deal with interest bearing monetary assets and I did not deal adequately with the problem of converting nominal balances into real balances. The path breaking works of Fischer (1974), Samuelson and Sato (1984), and Feenstra (1986) on this tough problem were not yet available at that time. After this early attempt to integrate money into consumer theory, I never wrote another paper on this topic, although my former students—Donovan, Epstein, Feenstra, Hancock, and Kohli—have all made important contributions in this area of research. To further differentiate the research products of Barnett and Diewert, I note that, in addition to being the master of monetary user cost theory, Barnett has very substantial skills as an econometrician and macroeconomist—skills that I lack!

Barnett is very generous in this book about giving me credit for unifying the statistical (or test) approach to index number theory with the economic approach based on weakly separable aggregator functions. I would like to take this opportunity to point out that I was not the first to note the link of statistical agency index number formulae with functional forms for aggregator (utility or production) functions. In Diewert (1976; 116), I referred to Byushgens, Konüs, Frisch, Wald, Afriat, and Pollak as early pioneers in making this connection. However, these early researchers did not have the concept of a flexible functional form at their disposal, so they could not determine which exact index number formula might be "best" from the viewpoint of the approximation properties of the corresponding aggregator function. Barnett is well aware of this point but I do not want others to be confused about the nature of my contribution to the literature.

What is a possible future research agenda that might flow out of this book? It seems to me that there are a number of basic problems that need additional research.

- There is a need to examine more closely the problem of deriving the "right" price deflator for monetary balances. The "right" deflator depends on ones theory of how money enters the constraints of the consumer's and producer's constrained maximization problems. Moreover, the producer model of Fischer (1974) and the consumer model of Feenstra (1986) are both highly aggregated, and there is a need to generalize their deflator results to higher dimensionality models.
- Chapters 10, 11, 12 and 21 all deal with the extension of riskless consumer and producer models to situations where the consumer or producer make decisions under uncertainty. This is very innovative work, which I applaud, but these chapters use an expected utility approach. Starting with Allais (1953), various researchers, including for example, Machina (1982), Mehra and Prescott (1985) and Chew and Epstein (1989), have noted various paradoxes associated with the use of the expected utility approach. Using the state contingent commodity approach to choice under risk that was pioneered by Blackorby, Davidson and Donaldson (1977), Diewert (1993) tried to show that the expected utility framework led to a relatively inflexible class of functional forms to model preferences over uncertain alternatives. Diewert showed that a much more flexible class of functional forms can be obtained by moving to nonexpected utility models that are counterparts to the choice over lotteries models of the type pioneered by Dekel (1986), Chew (1989), Epstein and Zin (1990,1991), and Gul (1991). Epstein and Zin (1990), Epstein (1992), and Diewert (1993) (1995)

showed that these more flexible models can explain many of the choice under uncertainty paradoxes, including the equity premium puzzle of Mehra and Prescott (1985). Thus there is a need for the Barnett research agenda to be extended to a nonexpected utility approach. A related problem in this uncertainty area that needs further research is the problem of determining the firm's preference for risk utility function, given that the owners of the firm might have rather diverse risk preferences.

- There is a need to solve the problem raised in chapter 19 where the price of money on the supply side of the market is not equal to the corresponding price on the demand side. Actually, this problem is a special case of a wider problem, which may not have a satisfactory solution. The wider problem is this: if our macro model or applied general equilibrium model of the economy distinguishes more than one class of consumer or more than one class of producer (e.g., industries or firms are distinguished), then the index number commodity aggregates for the household and production sectors constructed by statistical agencies *will never match up*. In other words, the composition of aggregate "food" consumption by say, the elderly, will never be *precisely* equal to the composition of aggregate "food" consumption by say, single person working households. This means that the aggregate "food' equation for the economy will never add up precisely; i.e., the physical balancing of commodity supply and demand that input-output analysis attempts to do cannot be done *precisely*.

Before closing, I would like to discuss a few additional points that struck me as I read the manuscript.

- At times Barnett is somewhat critical of the monetary authorities for not adopting a user cost approach to the price of monetary services while he praises statistical agencies like the Bureau of Labor Statistics for producing consumer price indexes that are closer to the ideal indexes that economic theorists might prefer. However, while some statistical agencies may be willing to construct user costs for housing (or use a rental equivalence approach as the BLS does), most statistical agencies are just as opposed to constructing user costs for other consumer durables as the monetary authorities are opposed to constructing user costs for monetary components. Why is this? It is because statistical agencies feel that user costs are not *objective* or *reproducible*. In constructing a user cost, various choices have to be made about the appropriate depreciation rate, the appropriate interest rate, whether expected or ex post capital gains should be included, whether tax considerations should be included and so on. Since there is usually no single unambiguous choice for all of these components of a user cost, the agency is open to a charge of being nonobjective and of course, different statisticians will make different choices and so the resulting user cost will not be reproducible. Of course, as an economic theorist, I am not as worried about this lack of objectivity problem as the statistician since I believe that reasonably objective procedures could be worked out. In addition, it is worth observing that the greatest problems in measuring depreciation rates---the dependency upon usage rates, maintenance, and wear-and-tear---are not relevant to financial and monetary assets. However, it is important for theorists to recognize the concerns of the practitioners.
- This leads us to Barnett's interesting discussion on page 407 below on why government statistical agencies shy away from using econometrics in their procedures. Barnett points out that there are many possible econometric

specifications (both of functional forms and of stochastic specifications) that could be used to address a particular problem and there are many methods of statistical estimation (and of model selection). Thus statistical agencies will have difficulty in justifying an econometric model to persons untrained in econometrics. In other words, the use of econometrics these days is inherently *nonreproducible*: different econometricians will come up with different models (including functional forms, stochastic specification, model selection criterion, and method of estimation) and possibly, very different results. I believe that this *nonreproducibility* problem is even worse today than it was two decades ago due to the widespread use of the Generalized Method of Moments (GMM) method of estimation, which requires the researcher to choose a set of instrumental variables. As far as I can determine, there is no *objective* way for researchers to choose these instruments. In many cases, the choice of instruments will affect the results obtained, so GMM has just added to the *nonreproducibility* problems associated with the use of econometric techniques. Let me add here that I am not advocating throwing out econometrics; I am just pointing out that there is a problem out there (the lack of reproducibility problem) that the econometric literature has not adequately addressed.

- On page 567 and elsewhere, Barnett refers to the statistical or test approach to index number theory that was pioneered by Irving Fisher (1911,1922). Readers who might be interested in more recent work on the test or axiomatic approach to index number theory could refer to Diewert (1992) and Balk (1995).

To conclude, I note that Barnett and Serletis have nice introductions to each major section of the book, which will give the reader an overview of each section's content. For the reader who is not familiar with the Barnett approach, I recommend reading chapters 18, 23, 24 and 25 first. These chapters lay out much of the practical importance of the Barnett research philosophy and will serve to motivate further reading of the book.

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