

## FIFTY YEARS OF MEASUREMENT: A CAMBRIDGE VIEW

A Review of Ernst R. Berndt and Jack E. Triplett (eds.), *Fifty Years of Economic Measurement: The Jubilee of the Conference on Research in Income and Wealth*, Studies in Income and Wealth, Volume 54, University of Chicago Press, Chicago and London, 1990.

### I. INTRODUCTION

*Fifty Years of Economic Measurement*, edited by Ernst Berndt and Jack Triplett, commemorates 50 years of distinguished work at the National Bureau of Economic Research (NBER) Conference on Research in Income and Wealth.<sup>1</sup> The contributions contained in the volume, which cover a range of empirical issues, are firmly based on the conceptual foundations of orthodox neoclassical economics and their implications for measurement. Our evaluation is from an alternative perspective, one which uses as its starting point the development of empirical work at the Cambridge Department of Applied Economics (DAE). The Cambridge tradition was, and is, sceptical of the relevance and explanatory power of much of neoclassical economics. It is a tradition which emphasises, rather than downplays, real world complexities and which stresses the conceptual and practical limitations of empirical analysis.

### 2. ECONOMIC MEASUREMENT—SOME CAMBRIDGE CONTRIBUTIONS

The focus of applied economics at Cambridge in the post-war years has been the DAE which started in 1945 with Richard Stone as its first Director. Keynes (who gets a rather poor press directly and indirectly in the book under review, see, for example, Boskin's arguments on p. 160) was extremely significant in its founding. Cambridge economics has always been a broad church and it could be even argued that the Directors of the DAE have been patriarchs drawn from different denominations. It has had four Directors so far—Richard Stone (1945–55); Brian Reddaway (1955–70); Wynne Godley (1970–1987) and now David

*Note:* We are most grateful to Lars Osberg for detailed and helpful comments on the draft version of this paper.

<sup>1</sup>A most amusing section of the volume is the report of the speeches at the Luncheon in honour of the founding fathers (there do not seem to have been any mothers)—Roy Blough, Solomon Fabricant, Milton Friedman, Robert Nathan and Carl Shoup. Friedman could not make lunch but his message is included. He was an early research assistant of the "natural patron saint of the conference," Simon Kuznets. Fabricant remembers Friedman as "the first secretary of the conference and the first editor of the conference volume . . . a remarkably good editor, the best [he had] ever encountered . . . if [Friedman] had only stuck with that he might have amounted to something!" (p. 10).

Newbery (1988 ).<sup>2</sup> Despite their different approaches to economics they all have in common the desire to see the completion of thorough applied work, not only in order to explain and predict, but also in order to make contributions to policy, either immediately or ultimately. Richard Stone (who died in 1991) has given us his credo in, for example, his splendid set of essays, *Mathematics in the Social Sciences* (1966). He used mathematics in order to express theory rigorously and precisely and in a form in which it could be tested by quantitative methods, many of which he and his colleagues developed, just as they did the theory itself. Stone started from Keynes's work in macroeconomic theory and, of course, was a pioneer in developing the Keynesian framework of national accounts. His microeconomic theory had Marshallian roots but he very quickly put his own unique stamp on the developments with which he was associated. This view permeated not only his own work but also the work of the officers of the DAE while he was Director and then, when he became P.D. Leake Professor of Accounting in 1955, in the Growth Project group. The latter continued into the 1980s, led first by Stone and J.A.C. Brown, and then, after Brown had gone to Bristol and Stone had retired, by Terry Barker.

Brian Reddaway's period as Director was marked by an extremely down-to-earth approach to problem-solving. He emphasised the importance of understanding the strengths *and* weaknesses and limitations of data and the framing of questions in a manner which allowed the data to throw light on the answers. On the whole he encouraged the use of techniques which were less technical than those which were applied in the Stone era, and afterwards. Reddaway himself liked to be given problems to solve—two of the most famous reports during his period were *The Effects of U.K. Direct Investment Overseas* (1968) and *The Effects of the Selective Employment Tax* (1973). The SET itself was the brainchild of Nicholas Kaldor who was a teaching officer of the Faculty of Economics and Politics (of which the DAE is an integral part) and who regularly had research projects in the DAE (one of the reasons for setting it up in the first place was to make this procedure possible).

In the Godley period the emphasis was very much on forecasting and short-term policy proposals, as well as on longer-term problems of structural change in a more competitive world. Godley's own group pioneered the study of macroeconomic policy within a consistent set of stock and flow relationships, reflecting in some ways and probably unconsciously, Marshall's approach to the analysis of the long period. Finally, in the Newbery era, there has been great interest in the problems of restructuring in Eastern Europe and Russia as well as work on the environment, privatisation and various microeconomic problems, the explanation of which is required, together with the design of policies which draw on the pragmatic application of received theory, often tailor-made, to the problems in hand. The last was always a feature of the theoretical aspects of the DAE's contributions. Spanning the four periods of the different Directors has been the encouragement of projects in economic history. Seminal work by Phyllis Deane,

<sup>2</sup>Alan Hughes more than held the fort as a most capable Acting Director in the interim. He is now Director of the Cambridge University Centre for Business Research, an interdisciplinary organisation which undertakes research into business behaviour and performance.

Brian Mitchell, Charles Feinstein and Robin Matthews often originated as DAE projects.

The Cambridge approach to applied economics, as nurtured by the different Directors of the DAE, stresses the limitations of much of orthodox neoclassical theory, however elegant, in explaining economic phenomena in the real world. Instead, it emphasises the importance of relevance in economics, incorporating the lessons of history, the institutional context and prevailing social and political conditions. Theory and measurement are thus mutually interdependent as robust empirical analysis is dependent on relevant theory, which in turn depends on reliable observations. Cambridge advances in theoretical<sup>3</sup> and applied economics have, therefore, gone hand-in-hand. Furthermore, techniques have never been allowed to obscure the analysis—the medium is not the message.

### 3. ECONOMIC MEASUREMENT: SOME BASIC PRINCIPLES

Apart from celebrating 50 years of existence (if not uniqueness), a principal aim of *Fifty Years of Economic Measurement* is to provide a series of comprehensive survey articles on the state of the art in measurement which will be of value to graduate students in particular and to the profession generally. Accordingly there are chapters on productivity and economic growth (Dale W. Jorgenson), the measurement of capital (Charles R. Hulten), issues in the measurement and interpretation of saving and wealth (Michael L. Boskin), two papers on hedonic price indexes (one by Zvi Griliches, the other by Jack E. Triplett), the measurement of construction prices (Paul E. Pieper), data difficulties in labor economics (Daniel S. Hamermesh), demands for data induced by environmental policy (Clifford S. Russell and V. Kerry Smith) and measuring the tax burden (B. K. Atrostic and James R. Nunns), written, as can be seen, by acknowledged experts in their respective fields. The chapters are clearly and authoritatively written, comprehensive and useful. They are complemented by excellent discussion papers by other experts, taking up particular points raised, sometimes in praise, sometimes critical, sometimes extending.

#### *The Contribution of Theory*

A feature of virtually all of the chapters in the volume is the complacency of mainstream economists. The large body of economists working outside, or not solely within, the neoclassical paradigm either get no mention at all, or are summarily and contemptuously dismissed. Thus those economists sceptical of the aggregation of capital goods,<sup>4</sup> or labor economists working within an institutionalist

<sup>3</sup>The other tradition which influences our assessment is an amalgam of the work of the Classical Political Economists (including Marx) with the work of Keynes, Michal Kalecki and Piero Sraffa. These were amongst the principal influences on modern Cambridge economists such as Joan Robinson, Richard Kahn and Nicholas Kaldor who, of course, made outstandingly original contributions of their own (see Hamouda and Harcourt, 1988 and 1992).

<sup>4</sup>Even when the capital theory controversies are mentioned (see Hulten, p. 119) the basic criticism is misunderstood. The principal critique related to the meaning of capital rather than its measurement and this was associated with a different view of the accumulation and distribution processes in modern capitalist economies (see Harcourt and Whittington, 1990, pp. 199–206).

framework, are largely ignored. Jorgenson (p. 24) even goes so far as to attribute the discovery of reswitching, or at least the initiation of the controversy that surrounded it, to Paul Samuelson in 1962—which is a bit rich when it is remembered that Samuelson relegated it to a footnote far on into his article.<sup>5</sup> The point we wish to make is that these surveys are workmanlike efforts *if* initially it is accepted that the main processes in economic life are captured in a basically Fisherian world of intertemporal consumption, saving and investment behaviour (or even a simple extension of J. B. Clark's theoretical vision to applied work) so that all the statistical data which is used may be taken as reliable quantitative expressions of the concepts of this theoretical approach. Solow (1974, p. 121) put this very well in his exchange with Anwar Shaikh concerning the humbug production function: "It merely shows how one goes about interpreting given time series if one starts by *assuming* that they were generated from a production function and that the competitive marginal-product relations apply" (italics in original). *If* this is accepted, it is possible to assess how good the statistical methods are for getting reliable measures of the orders of magnitude associated with the variables and the parameters of the assumed interrelationships between them over time and place. What is missing is any discussion of whether there are ways of testing whether the statistics thrown up in the data may have been generated by entirely different economic processes to those modelled.<sup>6</sup> (Also, the theoretical approach taken imposes conceptual meanings on the variables in the theoretical relationships. This, in turn, raises the question whether the available statistics are suitable for matching, even at several removes, the theoretical concepts. Applied economists, even those in this volume, often forget to ask if the conceptual categories which define the measurement of statistics *are* the appropriate ones for modelling the underlying economic processes.) That all this should be so after 20 years of the Western World lurching from one crisis to another (having been preceded by the Golden Age of Capitalism which on the face of it, was the outcome of the view of "economic processes" least likely to be considered or accepted by the authors of these surveys) is an index of the extraordinary hegemony that characterises the mainstream practitioners in the U.S.—and, no doubt, elsewhere as well.

Again, in their seemingly comprehensive, historically detailed and critical survey of measuring the tax burden, Atrostic and Nunns make no mention (p. 344) of John Burbidge's (1974) critique of the neoclassical theory of tax incidence as set out in, for example, Mieszkowski (1969), to whom they *do* refer. (Burbidge's critique was contained in his prize winning Ph.D dissertation on theories of taxation incidence.) Nor do they refer to Burbidge's paper (with the late Athanasios

<sup>5</sup>Samuelson was attempting to provide a rationale for Solow's theoretical work on neoclassical growth models and his pioneering empirical work on technical progress using the same simple models. Both of these approaches were criticised by Joan Robinson, Kaldor and Sraffa. Robinson and Kaldor were simultaneously developing the Classical-Marxian *cum* Keynesian-Kaleckian approach to growth and distribution, while Sraffa was attempting to revive the conceptual framework of Classical Political Economy and Marx.

<sup>6</sup>We recognise that this is much easier said than done, see, for example the rather rueful comments on this, and other issues, in Pesaran and Smith (1992). They cautiously conclude that "it seems unlikely that economic theories can be tested. [Yet] . . . within an agreed procedure for inference it may be possible to judge whether the conditional predictions of a particular model . . . do in fact match the data better than those of a rival model" (p. 17).

(Tom) Asimakopulos), “The Short-Period Incidence of Taxation” which was published in *The Economic Journal* in June 1974. Asimakopulos and Burbidge criticised the (then) “recent work on the incidence of taxes” for being “largely carried out on the basis of neoclassical assumptions [in pre-Keynesian models] . . . [R]eal wages are determined in the labour markets . . . full employment [is] automatically achieved through price and wage flexibility. Investment is . . . determined by saving out of full employment income” (1974, p. 267). As a result in the short period the legal and economic incidence of taxes coincide. Asimakopulos and Burbidge obtain different results by using a model from an alternative framework which is based on the work of Keynes and Michal Kalecki. One of its appraisers, Carl Shoup, could have told them of the existence of Burbidge’s dissertation and even the most died-in-the-wool Chicago person would have at least have heard of the *The Economic Journal*.

It would be wrong, of course, to imply that there is complete harmony and no discord within the mainstream boundaries themselves. Jorgenson’s chapter, for example, continues his long crusade to show that the approach to the measurement of the contributions to the growth of productivity and economic growth generally by, for example, Solow and Denison is basically wrong-headed; that by properly measuring productive inputs, which means correctly allowing for their “improved” quality over time, nothing (much) is (or should be) left for “explanation” by “the measure of our ignorance,” which soon the contribution of technical progress came to be called. Even Jorgenson and Griliches, though they are well-known collaborators, can fall out over fundamentals. Ernst Berndt, the discussant of Hulten’s chapter on “The Measurement of Capital,” tells a delightful story of a clash between these two giants, plus one other, Larry Lau, who played the role of Adam Smith’s impartial spectator.

*As a young economist just out of graduate school, I once had a privilege of listening to an exchange among three very wise men . . . —Dale Jorgenson, Zvi Griliches, and Larry Lau. Based on his recently completed research . . . Dale Jorgenson provocatively summarised his findings by saying . . . [in effect], “I do not believe value added exists”. Looking towards Dale’s bookshelf containing works by John Kendrick, Jack Faucett, Ed Denison, and others, Zvi Griliches scratched his beard and responded, “Of course value added exists. There’s a whole set of value added measures on that bookshelf.” And Larry Lau smiled. (p. 152)*

Discord within the neoclassical paradigm is also to be found in the work on tax incidence surveyed by Atrostic and Nunns, where they draw attention to a crude misspecification error within a mainstream framework. It concerns the illegitimate combination of a long-period substitution process between capital and labor, modelled by CES functions, with a short-period non-substitution process in the use of intermediate products, modelled by a Leontief fixed-coefficient matrix. As their discussant, Martin H. David, comments, “This weakness can and should be remedied” (p. 414).

Throughout the volume it is evident that the underlying models employed are predominantly supply side—as though (if we may be completely old-fashioned) Harrod’s natural rate has been the guiding spirit of the processes which have

generated the statistics to be used, the raw data, rather than them being the outcome of an interplay between the warranted rate and deviations from it and the consequent feedback effects of these on the size of the natural rate, the sorts of processes analysed so effectively and illuminatingly over the years by John Cornwall, (see, for example, Cornwall, 1972, 1977 and 1990). A typical example is on p. 162 where Boskin argues as if saving is the necessary condition for investment, providing the necessary funds for the latter to occur. But for most of the period covered by his statistics, saving was created by investment which itself was constrained, first, by the “animal spirits” of American and foreign business people—their confidence, basic drive, and initiative and so on—and secondly, by their access to funds from financial intermediaries at home and abroad. Saving itself is a decision not to spend—period. It is only when the economy is fully employed that, for *extra* investment to occur, extra saving is needed to release resources for the production of the investment goods to occur. Even then extra finance is necessary as well. In any other situation a free lunch is usually possible—we may have extra investment and extra consumption at the same time.

Of course Boskin may well be right that, given the restructuring needs of the U.S. economy, even at full employment the saving ratio (however defined) may be too low to release the resources to increase capacity and stimulate structural change. If this is what he means when he is discussing the genesis of saving and its purposes and meanings in the United States he should say so. As it stands, it is a confused discussion as it is unclear whether the model of the economy which he has in mind is capacity or demand constrained. It may be that the U.S. has followed the disastrous British path where the long-term lack of investment has reduced the growth potential of the economy so that the effective constraints on production are the capital stock and the balance of payments rather than the workforce. If so, the inflation barrier is met long before the workforce is fully employed. Thus a higher domestic saving rate is a necessary, although not sufficient, condition to break the bottleneck to allow a higher level of activity and (sustainable) rate of growth to be achieved. Also required are mechanisms to channel the released resources into productive purposes—the British experience suggests that the market cannot achieve this alone.

### *The Scope of Measurement*

In his 1984 Nobel Memorial Lecture Richard Stone (1986, p. 5) noted that “the three pillars on which an analysis of society ought to rest are studies of economic, socio-demographic and environmental phenomena.” In many ways this reflects the Cambridge tradition in empirical work which analyses economic processes within a broad framework. The thrust of this volume is narrower in scope and although it would, of course, be churlish to expect a collection of essays to comprehensively cover all areas, there are a number of notable omissions. There is little discussion of the immense problems in measuring activity in the service sector<sup>7</sup> and the area of constructing reliable historical data is largely

<sup>7</sup>Nevertheless the NBER Conference on Research in Income and Wealth has made important advances in this area, see, for example, Fuchs (1969) and, more recently, Griliches (1992).

ignored. Of the remaining two pillars of Stone's measurement edifice, socio-demographic phenomena are not directly considered but the issue of data and environmental policy is addressed in the chapter by Russell and Smith.

In discussing Griliches' chapter, Robert E. Lipsey comments:

*“as we become more interested in the output and productivity of the service sector, and if we are more skeptical about the official measures, as I am, we will be compelled to think more seriously about the meaning of output and input in service industries and about the relationships between service industry inputs and outputs . . . I suspect that there is more to be learned about the mysteries in recent productivity developments along these lines than in pursuing that picture of the continuous process plant producing a single output from labor and capital inputs.”* (p. 205.)

The service sector is by far the largest in developed western economies but, if British experience is a typical yardstick to go by, we remain seriously ignorant of its development and growth. Indicators such as the volume of labor input and deflated wage bill measures provide little guidance to changes in activities producing increasingly sophisticated outputs, where product differentiation and customisation are particularly important. The British Central Statistical Office observed that “there is at present no satisfactory treatment of quality changes in the service industries” (CSO, 1985, p. 40)<sup>8</sup>—perhaps a prima facie case for the use of hedonic price indexes, so thoroughly discussed by both Triplett and Griliches in this volume.

The lack of historical national income data is an important oversight. Recent extensions and revisions to historical data have done much to enlighten debates in economic history.<sup>9</sup> Furthermore, a rigorous evaluation of historical phenomena is necessary to understand the processes of economic growth and development. Many current economic problems are well-rooted in historical processes. In the British case, understanding slow and erratic growth, poor trade performance, short-termism, lack of investment in skills and so on, demands a thorough analysis of British economic history.

An important chapter in the volume is by Russell and Smith on “Demand for Data and Analysis Induced by Environmental Policy.” They identify clearly the large gaps in the data, gaps which need to be filled in order to fulfill the needs of the emerging environmental policy agenda. They observe that “When compared with the effort and experience devoted to the conventional topics considered under the auspices of the Conference on Income and Wealth, the record of empirical

<sup>8</sup>Of perhaps greater concern in the British data is the subjective treatment of the public and private sectors. For education and health services, “the indicators used for the public sector elements cover employment and capital consumption . . . In the absence of suitable alternatives, the private sector elements are also covered by arbitrary series or employment indicators, *with arbitrary adjustments for changes in output per head*” (CSO, 1985, p. 44, italics added). So an arbitrary productivity term is added to the private sector element, but not the public sector. There can be no justification for such a biased approach; data should be treated in a consistent manner and not according to dogma. By and large, within the confines of their theoretical perspective, the authors in this volume have done just that.

<sup>9</sup>The Conference on Research in Income and Wealth has made important contributions in this area, for example, NBER (1960), NBER (1966) and Engerman and Gallman (1986).

analysis of public policies for the management of environmental resources is quite limited” (p. 322). Russell and Smith identify four areas of immediate policy concern: environmental risk, air quality, water quality and stock pollutants and global climate change. “The great need here is for data-gathering and model-building efforts to reflect the demands for policy analysis. Identifying the need is a great deal easier than meeting it, for the required interaction has all the difficulties of interdisciplinary research” (p. 323). This must surely be the case in an area dominated by the moral issue of whether the current generation has the right to irreversibly damage the environment that will be inherited by future generations.

### *Data and Data Manipulation*

The effectiveness of extending the scope of economic measurement will ultimately be constrained by the reliability of raw data and the careful construction of economic indicators. These factors are of increasing importance as economies grow, as this increases the fragmentation of markets—greater product differentiation and a wider range of skills and functions in labor markets—and products and services become more sophisticated.

A number of chapters in the volume seek to shed light on the difficulties of collecting and constructing reliable data. Hamermesh considers “Data Difficulties in Labor Economics.” He argues that some studies of labor market behaviour have been based on data that are inappropriately disaggregated, unrepresentative and uncharacteristic of current structures (p. 291). Furthermore, and counter to most of the other studies in the volume, he identifies the demand side of the labor market as an area of importance which is in need of improved data. He suggests the increased use of labor market surveys and the deployment of more resources towards the production of disaggregated data. “Rather than rely on inappropriate data, those of us interested in empirical research in labor economics outside the narrow and decreasingly fertile area of labor supply must adopt some of the sociologists’ willingness to generate new sets of data” (p. 291). This is an admirable objective although it would also be desirable to construct models of labor market behaviour that adopt some of the sociologists’ willingness not to rely on a narrow range of economic, mainly price, variables.

The importance of collaboration between government and academia in improving economic measurement is demonstrated in the chapter by Pieper. In “The Measurement of Construction Prices: Retrospect and Prospect,” he reviews the past 40 years of development of the price indexes used by the U.S. Bureau of Economic Analysis. He concludes that there is no single best method for deflating construction as each approach has its strengths and weaknesses. More tellingly, he observes that “progress in construction deflation has been made in the past when there has been interaction between government statisticians and the academic profession” (p. 260).

The resistance of statistical agencies to change is discussed in the chapter by Jack Triplett. He contributes a masterly survey of the use and abuse of, and conceptions and misconceptions about, hedonic price indexes—all you ever wanted to know but were too afraid to ask. He concludes that the conceptual issues have mostly been resolved and should no longer pose any barrier (p. 228).



“The data problems remain formidable” (p. 228) but the statistical agencies could crack them if they were convinced that the hedonic methods would improve price indices. They would be hard put to argue otherwise after Triplett—but, as indicated in the chapter by Russell and Smith, just because a cause is just does not mean it will triumph.

As Berndt, the discussant of Hulten’s chapter, notes, the author has written a useful and readable chapter on applied theory and practical issues in the measurement of capital. It remains true that precise, exact and rigorous definitions of capital (and profits) only exist in Golden Age conditions far removed from the actual world. Golden Ages were originally called so in order to indicate their mythical quality. Yet measurement has to be done if applied work is to get off the ground. Berndt makes the fundamental point that how you measure depends upon what you want the measure for. Like Hulten he distinguishes at least three possible uses in this context: to help to explain and predict investment in producer durables and non-residential structures; to help to measure productive capacity; and to help to measure *multifactor productivity growth*. Putting the question first clears the mind as to what exactly is needed (Reddaway would approve).

Hulten highlights the major difficulties associated with the measurement of capital, that much of the data comes from accountants’ procedures and is greatly affected by their conventions, which, in turn, have been considerably modified over the years for which the figures are reported as, for example, the profession (and their employers) have reacted more and more to living in a world of continuing (but changing rates of) inflation. Secondly, because markets for used capital goods are notoriously thin, the valuation of capital goods in use is an extremely tricky problem of indirect imputation. In tackling this problem, it is vital to be clear whether the problem being addressed concerns productive capacity, actual or potential, or the value of the quasi-rents expected to be received for the remaining lives of the assets, where the lives themselves are endogenous variables, the values of which have to be determined as well. One subtle point which Berndt emphasises is that mere passage of time is not necessarily an accurate measure of asset life unless constant utilisation is assumed. This is often an incorrect assumption, for example, car-owners reacted to oil price rises by using their cars far less. Hulten has a careful section in which he relates patterns of decline in the physical productivity of durable assets to the conventional methods used by accountants to reckon depreciation. These have wide-ranging implications not only for assessing potential productivity but also for making measures of profitability which mean “something.”

In his chapter on “Hedonic Price Indexes and the Measurement of Capital and Productivity: Some Historical Reflections”, Griliches disclaims any originality concerning hedonic price indexes. “What was impressive about . . . [Griliches, 1961] is that it took the idea seriously, did a lot more work with it and showed that something interesting can be accomplished [so having] a significant impact on the subsequent literature” (p. 186), itself huge. Griliches takes a splendidly pragmatic approach, arguing that what is being estimated by the use of hedonic price indexes and the like “is the locus of intersections of the demand curves of different consumers with varying tastes and the supply functions of different

producers with possibly varying technologies of production” (p. 189). It would be foolish therefore to expect to be able to push back further to the underlying utility and cost functions, especially, we might add, if they may not even be there to be found.

Very succinctly, Griliches sets out his objections to available capital measures, viz, that they are “over deflated and over depreciated, . . . items with different expected lives [are] . . . added together in [the] . . . wrong way, and . . . no allowance [is] . . . made for changes in the utilization of such capital” (p. 192). His criticism that the observed depreciation rate in secondhand markets contains a large obsolescence component induced by the rising quality of new machines is well taken as is his comment that, while it is a valid subtraction from the Present Value of a machine in current prices, it is not the right concept to be used in the construction of a constant quality notion of the flow of services from the existing capital stock in “constant prices.”

Griliches reports on the exchanges between Jorgenson and himself with Denison in 1972, in which they conceded to Denison the weakness of their treatment of utilization, with the result that their “explanation” of productivity growth shrank from 94 percent to 43 percent and with it also their “claim to “do it all” (without mirrors)” (p. 193). He still believes that they were right to “explain” rather than “just measure” and that this required expanding their framework further to allow for R and D and other expenditures, increasing returns to scale and other disequilibria! (p. 194).

Griliches makes a distinction between the amounts of the services of labor and capital goods actually used rather than just paid for (the latter involve expectations of sales and cost-minimisation proportions so that remunerations and expected marginal products match). He readily admits that many of the differences stem from “the failure of the assumption of perfect competition that is the basis for much of the standard productivity accounts” (p. 195). This makes life hard but, he argues, it is an admission of failure to switch over to a model of the world where market power is not evenly distributed but concentrated in the hands of a few decision-makers who have considerable discretion as to the prices they charge for their products (and, in labor markets, the prices paid or, sometimes, charged for services).

The comments which Griliches makes on the slow-down of U.S. productivity are extremely interesting. Though he said that it was the neglect of education, investment in research and economies of scale which hindered his work with Jorgenson, he argues that the recent slow-down is not due to a slow-down in technical progress but to misguided macroeconomic policies associated with the oil price shocks and fear of inflation—the “Germanisation” of the United States (and other countries) bourgeoisie. He argues that we should not use data from those years for long-term studies. What is happening inside the production possibilities frontier does not give us clues as to how (and why) the latter is moving out over time (p. 198). Again we have common sense and deep economic intuition associated with an inappropriate framework. We also have the implied acceptance of the legitimacy of distinguishing the factors responsible for the cycle from those responsible for the trend (and those responsible for their theoretical cousins, existence and stability), so ruling out the work of, for example, Richard Goodwin

and Kalecki on the indissolubility of trend and cycle and much modern work on path dependent equilibria (or even no equilibria at all).

### *Use and Misuse*

The concluding chapter in the volume is a “Policy User Panel” which contains observations and comments by Charles L. Schultze, Rudolph G. Penner, Ian A. Stewart and a summary by Roger B. Porter. Schultze considers the problems of organising statistical material in the U.S. and recommends the creation of a chief statistical office to help coordinate the federal government’s statistical activities. Penner considers the problems of producing reliable preliminary data. He believes “that forecasting the past may occasionally mean actually ceasing the prediction of data that do more to confuse than enlighten” (p. 429). This strikes a painful cord in recent British economic history. In the mid 1970s initial estimates of the British fiscal deficit for 1976–7 were put at £11.2 billion. The perception of a major fiscal crisis led to the arrangement of an IMF loan, controls on public expenditure and the introduction of monetary targeting. For many observers the disastrous British monetarist experiment started here and not with the election of Mrs Thatcher in 1979. As for the fiscal deficit for 1976–7, it turned out to be a far more manageable £8.8 billion, more than 20 percent below the initial estimate; with more reliable initial figures, 15 years of economic mismanagement may have been avoided. Stewart’s contribution emphasises the need to extend measurement beyond purely economic issues. In some ways echoing the approach of Stone, he argues for an extension of national accounts to “portray aspects of society and policy issues, such as the health system, the education system, the work system, all of which have aspects about them which are beyond the narrowly economic and whose policy issues entails research issues that are certainly beyond the narrowly economic” (p. 433).

The policy users remain reasonably optimistic about the political manipulation of data. Schultze cites a remark attributed to Winston Churchill: “When I call for statistics about the rate of infant mortality, what I want is proof that fewer babies died when I was prime minister than when anyone else was prime minister” (pp. 422–3). Schultze does not, however, believe that policymakers behave this way. The view from this side of the Atlantic is less sanguine. Throughout the 1980s, the definition of the major British economic curse, unemployment, has been continuously revised, virtually always leading to lower estimates.<sup>10</sup> A former Government minister responsible for employment has acknowledged that his job seemed to be not to address the unemployment problem but to devise ways of reducing the figures (Clark, 1993). Similarly, British politicians continue to be highly selective in the use of economic data. When discussing economic *growth* they focus on the *level* of national income. Continually—at least prior to the present slump—we hear of a “record level of output” as if this is a

<sup>10</sup>Of the 30 changes made to the definitions of unemployment by the U.K. Department of Employment in the 1980s all but one reduced the jobless total. This raises the issue of the independence of statistical agencies. The British decentralised system lacks formal independence from government whereas countries such as Canada, Australia and Holland provide their statistical agencies with statutory protection from political pressure.

unique phenomenon. In fact since 1855, from when we have reliable figures, we could speak of a “record level of output” in five out of every six peacetime years. Since 1979, this has fallen to four out of six years. The reluctance to discuss growth rates perhaps reflects poor recent performance, particularly in comparison to the much berated period of Keynesian demand management in the 1950s and 60s. We can only hope that the bombardment, through the media, of the general public with statistics and indicators will raise economic literacy to the level where the “wheat” can be more easily distinguished from the “chaff”.

#### CONCLUSIONS

This volume is an articulate celebration of orthodox empirical economics. There is, however, little acknowledgement of the existence, let alone importance, of alternative approaches to applied work. Very occasionally the cracks in the facade appear. Hamermesh, for example, notes that “The cultural imperialism of American empirical economics should not blind us to the possibility that the structure that describes a relationship in the U.S. may not be representative of some (any?) other economies” (p. 274). The neoclassical approach is *a* way of doing economics, it is not *the* way. The Cambridge approach is one alternative; an approach, however imperfect, which tries always to place great emphasis on the complexities of the real world. Reliable measurement is dependent on relevant theoretical hypotheses. The neoclassical approach, displayed in this volume, provides clarity and internal consistency. An alternative Cambridge approach, sceptical of the ability of markets to clear, would more readily accept that individual and collective actions are affected by institutions and political and social forces. The resulting picture of the world that emerges may be less defined but also perhaps less distorted.

As we have been critical, perhaps harshly so, it seems appropriate to close by paying a warm tribute to the sheer professionalism, technical intelligence and sheer hard work contained in this volume. We may not recognise the world as depicted in many of the chapters; nevertheless, the editors are to be congratulated for bringing together such a splendid set of economists and so providing a volume which will more than ‘do’ until the next fifty years are up and we celebrate 100 years of economic measurement.<sup>11</sup>

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<sup>11</sup>British readers will recognise the significance of scoring 100.

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