Positive Economics and the Role of Econometrics

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I. Introduction
This paper is part of a larger study of the way in which economists have adopted the language and tools of econometrics as an important element of our rhetoric. The paper examines the nature of the 1960s' embrace of econometric techniques and the sources of the concomitant expectation that econometrics would provide an empirical base similar in content to that of the hard sciences. Two major influences were undoubtedly Milton Friedman's 1953 essay 'The methodology of positive economics' and Richard Lipsey's 1963 text, An Introduction to Positive Economics. Taken together these two economists informed (and converted) most students of economics (and hence the next generation of professionals) to a so-called scientific view of their subject; firmly establishing the label 'positive economics' on both sides of the Atlantic.

The time was ripe for the popularisation of econometrics: Haavelmo (1944) had proposed that the methods of classical statistical inference could be used to test economic hypotheses and quantify the theoretical parameters of economic models, and computing facilities had become widely accessible. By the mid 1960s the lessons for the next generation of economists were learnt: economics should be regarded as a quantified science and economic knowledge should be expressed in a form making it amenable to testing.

The optimism of the period was unashamed and was marked by confident attempts to determine the correctness/falsity of theory. Yet it would be wrong to see these developments of the post-war period as smooth or uniform. There were major debates about the purpose and intended consequences of econometric testing. To place this in context, briefly consider the history of empirical analysis in economics.

II. History of Empirical Analysis
Empirical analysis in economics can be traced back at least as far as the sixteenth century when the 'political arithmeticians' analysed problems such as taxation and international trade with quantitative information. However, econometrics, as we currently understand the term, is of much more recent origin, and is marked by the foundation of the Econometric Society in the 1930s; although, at that time, econometricians were relatively few in number. The early works of these econometricians were essentially responsive reactions to problems in applied economics and were about the data-theory gap. For example, there was prominent work on both business cycles and demand analysis. Economists looked to empirical work to help define the theoretical notion of the business cycle because the contribution of theory was incomplete - the data - theory gap was large. In demand analysis, the role of empirical work was very different - the data theory gap was relatively well-defined and the econometricians' task was to offer some understanding of the correspondence between theory and data. One of the most significant correspondence problems was, of course, identification. In all this work data was in some ways a source of ideas - the theory development role of econometrics: but that role was downgraded given the rise of Haavelmo's (1944) probabilistic revolution in econometrics.

Haavelmo argued that theoretical propositions could and should be formulated in the context of a well-defined statistical model. This approach seemed to promise that the methods of classical statistical inference could provide a substitute for the experimental method; and could therefore be used both to test economic hypotheses and quantify the theoretical parameters of economic models. Herein lies a key to the spread of interest in econometric techniques throughout the economics profession. Until this stage it had been relatively easy for the sceptics to dismiss much of the econometric work: early applications of correlation analysis to economic time series had raised the problem of spurious correlation (see Yule, 1926); and work on macroeconomic models at the Cowles Commission seemed to be bogged down with the problem of identification. Yet The Probability Approach in Econometrics set rules of experimental design and gave a formalism to econometric investigations which had widespread appeal.

But this celebrated work would not have been enough on its own: Haavelmo was undoubtedly a facilitator but so was Friedman and so was Lipsey. Both these economists were popularists and led most students of economics to a so-called scientific view...
of their subject. “Friedman’s essay is part of the intellectual equipment of most American economists and its arguments come readily to their lips: (McCloskey, 1985, p.9); and on the other side of the Atlantic there can be few economists born after 1940 who have not purchased at least one edition of Lipsey’s positive economics text. Both economists appealed to the dominant philosophy of scientific positivism; and in both cases the writing was prescriptive - economists were told what to do.

In a logically positivistic science the ideal procedures are that one should first develop a rigorous (preferably) axiomatic logical statement of a particular theory. Then one should carry out empirical work to: (i) estimate important parameters of theory; (ii) test the hypothetico-deductive parameters against data not used in estimating these parameters; and (iii) test the ‘truth’ of contingent statements of the theory. In practice of course this ideal must often be seriously compromised and when we talk about positive economics we are really talking about positivism as it is practised not necessarily as it is preached.

IIIa. Positivism Compromised: Friedman

Despite a superficial appeal to Popperian falsificationism, what Friedman talked about was ‘confidence in’ and ‘evidence for’ a hypothesis. He refers to testing as if its function was to verify which is distinctly un-Popperian; although he seemed well aware that factual evidence can never prove a hypothesis, only fail to disprove (e.g. Friedman, 1953, pp.9,12,23 and 28). Nevertheless, the undoubted strength of Friedman’s optimism in his approach can be seen from his argument that disagreements about policy prescriptions grow out of differences in the perceived results of different actions. He has stated that such differences could be eliminated by the progress of economic research. Consider the following:

“There is not...a one-to-one relation between policy conclusions and the conclusions of positive economics: if there were, there would be no separate normative science. Two individuals may agree on the consequences of a particular piece of legislation. One may regard them as desirable on balance and so favour the legislation; the other, as undesirable and so oppose the legislation....I venture the judgement...[that] differences about economic policy...derive predominantly from different predictions about the economic consequences of taking action - differences that in principle can be eliminated by the progress of positive economics...Progress in positive economics will require not only the testing and elaboration of existing hypotheses but also the construction of new hypotheses (Friedman (1953), pp. 5 and 42, emphasis added).

At the methodological level, Friedman is most associated with the maxim that: knowledge of how or why a model works is unimportant if, in practice, it does work. He used ‘work’ to refer to a model’s ‘success’ with respect to statistical criteria and, in this context often emphasized predictive power. Yet this approach runs close to ‘naive operationalism’, somewhat removed from the procedures of logical positivism.

IIIb. Positivism Compromised: Lipsey

As for Lipsey, what he had to say was properly a product of the methodological seminar group at the London School of Economics, M^1 (Methodology, Measurement and Testing). Founded in 1957, the group sought to present economic knowledge in falsifiable form and sought to argue that theory alone cannot be a sufficient basis for policy conclusions. Interestingly, testing was not part of the original concept, the main emphasis being on economics becoming a quantified subject: it was only after learning of Popper’s ideas that testing assumed prominence.

Lipsey’s text is almost evangelical in style. The book is prefaced by a lengthy quotation from Beveridge in which economists are criticized for not testing theories by facts. Lipsey’s concern was a general methodological issue of how to judge the ‘correctness’ of economic theories and he sought to teach a new generation of economics students that they should look for answers in the empirical rather than the a priori direction. For example, he writes ‘the theory of demand and price can have few applications to the real world without some empirical observations of quantitative magnitudes [sic]’ (Lipsey, 1963, p. 161).

Lipsey’s first edition proposes the label ‘positive economic science’ as a description of the method proposed; and Lipsey is clear that positive economics ‘deals with statements that could conceivably be shown to be wrong (i.e. falsified) by actual observations of the world...[it must be]...at least possible to imagine factual evidence which could show them to be wrong’ (Lipsey, 1963, p.5). Several Parts of the book end with a chapter criticizing received theory, presenting the problems of measurement and the difficulties for specifying the empirically testable implications of theories. In these chapters Lipsey was often led to conclude
that the theory being considered either did not yield a large number of testable predictions; or that it yielded few implications about questions of interest; or that testing it seemed to be extremely difficult.

IV. The 1960s’ Optimism

Nevertheless, by the mid 1960s, the lessons for the next generation of students were set: economics should be regarded as a quantified science and economic knowledge should be expressed in a form making it amenable to testing. This emphasis was particularly appealing for a profession already fascinated by Samuelson’s (1947) Foundations which stressed the role of ‘meaningful hypotheses’—propositions capable of refutation by reference to empirical facts. Moreover, both Friedman and Lipsey had shown how to combine theory with quantification.

However, the differences between the views propounded on each side of the Atlantic should not be ignored: for example, Friedman was criticized by members of the M-T group for saying nothing about the criteria for a good test while stressing testing almost to the exclusion of any other sort of examination. Furthermore, he was charged with complacency in that he ‘far too readily accepts hypotheses as tested’ (Archibald, 1959, p. 63). This was a reference to Friedman’s observed acceptance of a great deal of economic theory without actually offering any evidence of their having been tested (Klappholz and Agassi, 1959, pp. 65-9). By the early 1960s, the M-T view of Friedman’s 1953 essay was that it implied an unacceptable degree of naive instrumentalism (see for example the debate in the Review of Economic Studies: Archibald, 1961; Stigler, 1963; Friedman, 1963; and Archibald, 1963).

But this theoretical debate sits somewhat independent of the applied work of the period, particularly in the UK where we see an extreme concern with model estimation as distinct from model evaluation. For example, typical of the early published work of the M-T group is Lipsey’s well-known reconsideration of Phillips’ work on the relation between the unemployment rate, the rate of change of unemployment and the rate of change of money wage rates (Lipsey, 1960). Amongst Lipsey’s objectives was to determine the proportion of the variance in money wage rates that is associated with the two unemployment variables. It is of course well known that theoretical justification for the Phillips curve was undertaken only after the empirical phenomenon was published and this was Lipsey’s other objective in the 1960 paper. It was consistent with Lipsey’s stated ideal order of proceeding: first, the researcher outlines the phenomena which require explanation; second, a model is developed to rationalize the available data; and then further implications or out of sample predictions are tested (see Lipsey, 1960; and Lipsey and Brechling, 1963).

At this time Sagan was something of a lone voice at the LSE in keeping the main focus of econometrics on critical testing of hypotheses (Sagan, 1964). Although, on the other side of the Atlantic there was the enthusiastic adoption of significance tests by the new economic historians, ‘climetricians’, of the 1960s. Here there were strong statements about refutation for example in the Introduction to the Penguin Readings on the New Economic History Peter Temin wrote that “much of the work of the new economic history can be seen as a refutation of previous generalizations about growth, as is illustrated by the first four sections of this collection”. (Temin, 1973).

Notwithstanding the above, much of the optimism of the period related to the ability to uncover empirical regularities, good examples being the Phillips curve the consumption function and the aggregate demand for money function. Yet one strand of this literature sought evidence on ‘fundamental ratios’ in economics: this was the objective of the Klein and Kosobud (1961) paper ‘Great Ratios in Economics’. The paper opens with the words ‘Economists frequently base their reasoning on key ratios between variables. If these ratios are in the nature of fundamental parameters, simplifications of theory may result’. The paper studies five ‘celebrated ratios of economics’:

- the savings-income ratio;
- the capital-output ratio;
- labor’s share of income;
- the income velocity of circulation; and
- the capital-labor ratio.

Klein and Kosobud put these ratios to empirical test stressing that ‘standards must be high, and stability or plainly systematic variation in ratios must be found in order to enhance their usefulness for the construction of theories’ (op. cit., p. 173). Underlining their study is the optimism that empirical investigation could resolve the question of whether these ratios were fundamental parameters. The conclusions are strong:

...the savings ratio is not a constant, but is on a declining trend’ (p.178).
‘Our estimate [for the capital-output ratio]...shows a significant downward trend’(pp.179-80).
‘A formal calculation of the trend in this ratio yields...that there is no trend in [the ratio defining labor’s share of income] (pp.182 3).
‘[T]here is a noticable trend in [the velocity of circulation]’ (p. 185).
‘Our series show a steady upward growth in [the capital-labor ratio]’ (p. 187).

Significantly, some of these conclusions ran counter to the empirical findings available at that time. For example, the declining savings ratio was to be contrasted with the findings of studies by Kuznets (1942) and Goldsmith (1955, 1956) which presented evidence of constancy in the ratio. While it is interesting simply to note this, the example is particularly apposite for an appreciation of the way in which some belief in fundamental ratios (or more generally relationships) continued to pervade the profession. When, in the 1970s, it seemed that the received relationship between savings and income had broken down, a great deal of research effort was directed to the search for the definitive statement on the nature of the relationship. Similarly, the episode of ‘Missing Money’ prompted research effort on the demand for money function, much of which was based on the premise that a stable relationship was there to be found. The seeming unwillingness of some members of the profession to accept that some of the ‘Great Relationships’ may not hold has of itself been a strong impetus to empirical research, and indeed economists have learned much from such analyses. No economist would today run a regression of a simple Kynesian consumption function, any more than they would argue that Keynes’s absolute income hypothesis has nothing to say. Economists have learned to take notice of applied econometric studies and one might argue that they would be misled if they were ever to ignore them.

V. Difficulties with the ‘Positive Approach’ to Economics

But the path to this understanding has not been smooth and certainly it has not evolved directly from the ‘positive approach’. The positive approach (whether propounded by Friedman or Lipsey) was found to be seriously wanting for economic practice. Three main difficulties were identified:

(i) Many aspects of economic theory do not imply either strong quantitative or qualitative predictions.

(ii) Economics is made up of interlinked propositions: thus the main hypothesis is insulated from testing by the range of ancillary hypotheses necessitated in making it testable.

(iii) ‘Refutation’ is difficult because hypotheses are probabilistic and errors (of rejecting a true hypothesis and of not rejecting a false hypothesis) are always possible. Formally, refutation requires the rejection of a theory if one is confronted with contrary evidence; however it is difficult to know what proportion of such incidences are required before the theory is rejected.

These factors contributed to the view that econometrics could not result in the rejection of many (possibly any) hypotheses; and led to the view that the emphasis should turn to estimation of the parameter values of economic theory and comparability through predictive performance.

Indeed Lipsey formally acknowledged the difficulty of translating falsifiability into practice: in the notes introducing the second edition of his text: ‘I have abandoned the...notion...of refutation and have gone over to a statistical view of testing that...all we can hope to do is to discover...the balance of probabilities between competing hypotheses’ (Lipsey, 1966, p. xx). Lipsey had become sensitive to the problem of errors of observation (both omitted factors and errors of observation, but he stressed the latter) stressing that errors ‘may always be present’ (Lipsey, op. cit., pp. xx and 51); moreover, he argued that because all empirical hypotheses are really probabilistic, the hypotheses necessarily admit of exceptions and therefore one cannot absolutely prohibit anything. Lipsey concluded that stochastic propositions are not strictly refutable (see p. 51).

Archibald endorsed this in his writings and advocated scientific comparison: the probable truth or falsity of a statement should be compared with that of another statement by appeal to observation, that is reference to facts. Thus empirical investigation of a theory required only that it be potentially comparable with pre-existing rivals, with constructed rivals or with the null hypothesis and in every case by appeal to facts. Archibald pointed out that $R^2$ is a comparative measure: ‘It tells us how much better our fitted relationship predicts the independent variable than does its own mean’ (Archibald, op. cit., p. 292). Perhaps the most well known example of this style of scientific comparison is the comparison of Keynesian theory with monetarist theory carried out in Friedman and Meiselman (1963). Yet, as is well known, this study settled nothing, provoked much controversy and led to a debate on the comparability of reduced form models.

The vision of resolving disputes was lost in the difficulties of carrying out the exercises and was soon to be replaced by an emphasis on verification and forecasting: provided the ‘evidence is consistent with the hypothesis’, the econometrician was content that the exercise was both satisfactory and complete; provided the forecasts were not ‘widely out of line’ with the actual outcomes econometricians were deemed to be doing a useful job. This was an important development stage in the use of econometrics.
Is the Evidence Consistent with the Hypothesis?

Economists placed a high degree of confidence in classical regression analysis, using the concept of statistical significance to establish the nature of economic relationships. By the early 1970s, strong statements were made like 'the evidence in favor of the existence of a stable relationship between the aggregate demand for real balances and a few variables is overwhelming' (Laidler, 1971, p. 91) and 'there is an overwhelming body of evidence in favor of the proposition that the demand for money is negatively related to the rate of interest' (Laidler, 1977, p. 130).

It seems that economists believed that they could establish whether a variable was worth including in a regression by noting the number of standard errors the estimated coefficient lies distant from zero; they could establish whether a relationship was stable through time by reference to the newly established Chow test (1966): their faith in significance tests was paramount. When faced with 'poorer performance' of these equations attention was turned to reformulate them to capture short-term dynamics, (albeit in an ad hoc way): the confidence remained as partial adjustment mechanisms, adaptive and extrapolative expectations formation became standard components of both the empirical and the theoretical models.

This form of equation remained popular into the 1970s despite the 'ad hominy' and despite the fact that econometric investigations had already revealed a disconcerting lack of stability for fitted coefficients (e.g. the 'Missing Money' debate).

The Forecasting Performance

The other main development in the use of econometrics was the creation of large-scale macroeconomic models. Econometricians were optimistic that they could build simultaneous equations models of an economy and, with the increased availability of macroeconomic data, use them both to test hypotheses and provide conditional forecasts (useful for policymakers and any other body willing to pay for them). In some ways this was a direct development of the Cowles Commission approach to econometrics. The move was successful in that most economists (and policy makers) attributed the macroeconomic stability of the 1960s to the successful application of economists' work by governments; moreover, the issues of economic policymaking taught to undergraduates were couched in this framework. The model builders responded to the needs (demands?) of policy makers by carrying out simulation exercises that identified potential consequences of changing exogenous variables like policy instruments. The ad hoc description of dynamic adjustment previously referred to on single equation estimation also became a characteristic of these models. Indeed, as the decade progressed there was an increasing reliance on ad hoc amendments to estimating equations to satisfy the empirical standards by which they were judged and to yield acceptable dynamic predictions: while the textbooks continued the rhetoric of the Cowles Commission framework, the builders of large models were divorcing themselves from it by making pragmatic adjustments to their so-called structural equations.

Even then, the forecasting performance of the relatively complex structural equation models was not particularly good, particularly for time horizons greater than six quarters and the emphasis on structural estimation was correspondingly weakened. Simpler forecasting mechanisms gained in standing: these were based on either 'reduced form' models (for example, the St. Louis model (see Anderson and Carlson, 1970) or the time-series characteristics of economic variables (using techniques developed by Box and Jenkins, 1970). The St. Louis model expressed the growth rate of nominal income as (separate) distributed lag functions of the growth rates of money and government expenditure. It was seductive: the model is easy to understand, has some intuitive appeal and its proponents promised a basis for assessing the relative significance of monetary and fiscal policies (to be inferred from the relative size of the estimated coefficients). The time-series analysts (using Box-Jenkins methods) promised a relatively cheap, straightforward mechanism for the generation of forecasts. Enthusiastic economist learned about autocorrelative structure (of economic data) and stationarity in time series. Of these two forms, it is probably the latter which has persisted the time-series models often out-performed the large econometric models and have served as the foundation of the now popular 'a-theoretic macroeconomics' associated with Christopher Sims (Sims, 1980).

VI. Disillusionment

Since the 'positive approach' first facilitated the embrace of econometric techniques, a keger had been learned - econometrics was no able to resolve economic dispute. Economists who had hoped for the 'ultimate econometric test' have been disappointed. One need look no further than the literature on the aggregate demand for money function to see how the 'answers' to unchanging, questions have changed markedly over the two decades since the 1960s' econometric work: Compar
Laidler's quote in the third edition of his text *The Demand for Money* with the two given earlier: "The fact is that a decade ago it was possible to be much more confident about the robustness of our knowledge of the demand-for-money function than it is now...Not the least important lesson that monetary economists have learned over the last decade...is that our knowledge [from studies of the demand for money]...though by no means non-existent, is fragile (Laidler, 1985, pp. 146 and 152).

There are many reasons for this but important is the point that the statistical tools that promised an end to economic disputes could never do so on their own...too much has been asked of them. This is neatly summed up in the accusation that some researchers confuse statistical significance and substantive significance. (Morrison and Henkel, 1970).

In economics it is of course the case that major developments are made without heavy reliance on significance tests: indeed McCloskey (1985) argues that while economists (especially in the 1960s) delighted in the rhetoric of significance tests they were not generally persuaded by the rhetoric. He points to the style in which we persuade students of the law of demand: we rely on introspection, analogy, utility theory, but not much on econometric results (see p. 58). The point is that economists (quite properly) do not rely exclusively on econometric evidence to form their understanding of economic behavior despite the fact that they may continue to present their subject within the 'positivist' tradition.

And herein lies an underlying theme on the perceived role of econometrics in economics, one which has led some observers of the economics literature to bemoan the profession's relative emphasis on theoretical and not applied work. Leontief's views are well-documented, for example in his Presidential Address to the American Economic Association, he emphasised that, in our academic community, empirical analysis gets a lower rating than formal mathematical reasoning (Leontief, 1971, p. 3). A similar view was expressed by Phelps Brown in his Presidential Address to the Royal Economic Society: "the more abstract, the more rigorous, the more general, so much the more distinguished...In economics at least those who devote themselves to the direct observation of attitudes and behaviour have commonly been regarded as playing in the 2nd XI (Phelps Brown, 1972, p. 9).

One reason put forward low regard for econometric work was the limited applicability of the techniques to the questions of interest. For example Worswick (1972) reproduced the remark that Samuelson once made about economists that: 'they are like highly trained athletes, who never run a race [Samuelson (1947)]' and argued that the remark could be fairly made about some econometricians who are 'engaged in...making a marvellous array of pretend-tools which would perform wonders if ever a set of facts should turn up in the right form' (Worswick, 1972, p. 79). In similar vein Phelps Brown noted that 'Those who have to bear the responsibility for policy...do not trust the systems fitted by econometricians to establish relations or coefficients on which that policy can be based' (Phelps Brown, 1972, p. 2).

These views still have some currency today and certainly there is some evidence that mainstream economics journals continue to devote more pages to the relatively abstract, theoretical material than to applied econometric work (see Morgan (1988) and Leontief (1982)). However, it would be wrong to infer that the economics profession as a whole has lost interest (or is it faith?) in applied econometric work: there is a strong confidence amongst econometricians that they have something to offer.

Certainly economists do not rely exclusively upon econometric evidence to form their understanding of economic behavior. But nor is it the case that the confidence in econometrics as exhibited by the early positivists has been eroded by the experiences documented here. During the 1960s, many had hoped that econometrics would provide a sound 'scientific' foundation for economics, along the lines of the standard hard science model of, for example, physics. Much of that anticipation may be attributed to a misunderstanding of the methodology of economic enquiry, and especially to a misunderstanding of the methodological status of econometrics; the hope was certainly fuelled by the 'positivists'. That optimism was, and is, misplaced; however the appropriate response is not to discard econometrics, nor is it to erect econometrics as a separate discipline. What is needed is a proper understanding of the methodological status of econometrics within economics: the economics profession would do well to recall the experiences of economists exploring the use of econometric techniques in the 1960s and 1970s and take heed of the methodological debates of that time in which limitations of econometric techniques were intertwined with limitations of the positivist tradition.
Notes

* Parts of this paper first appeared in Darnell and Evans, The Limits of Econometrics (1990).
However, the general focus of the book is on the nature of scientific explanation using the framework of probability, rather different from the material presented here.

1. There had been an attempt to found an Econometrics Society in 1912 when Fisher was approached to be President: 12 people were needed to form the society but there was insufficient interest. It took until the 1930s to establish any strength of opinion within the profession that economic theory could be given empirical content; but even then, the optimism was restricted to relatively few economists.

2. ‘Testing’ here refers to predictive power (Friedman’s emphasis was placed on the capacity of the estimated relationship to predict the dependent variable): it does not refer to testing the null hypothesis $B = 0$ for example.

3. This set of procedures has a strong pedigree in the nineteenth-century literature of Mill: for example, ‘instead of deducing our conclusions by reasoning, and verifying them by observation, we in some cases begin by obtaining them provisionally from scientific experience, and afterwards connect them with the principles of human nature by a priori reasoning, which reasons are thus a real verification’ (Mill, A System of Logic quoted in Darnell, 1981, p. 148).

4. In the UK the personal savings rate was observed to rise in the 1970s, whereas in the USA the rate declined in 1975-6. On both sides of the Atlantic there were attempts to explain the changes in the savings ratio (see for example Coghlan and Jackson, 1979; Deaton, 1977; and Cagan, 1983, referred to in Hadjimatheou, 1987).

5. The first important set of difficulties relates to obtaining testable predictions from economic theory. For example, Archibald had found that the need to specify a number of subsidiary assumptions made it virtually impossible to get testable predictions from marginal productivity theory and the theory of imperfect competition. On the second set of difficulties, see the view expressed in McCloskey (1985) that ‘The doubting and falsifying method, enshrined in the official version of econometric method, is largely impractical’ (p. 14). On the third point, it should be noted that this was not new to Archibald: Popper had in fact answered it! For example, ‘since probability statements are not falsifiable, it must always be possible... to “explain” by probability estimates, any regularity we please’ (Popper, 1968, p. 197).

6. As early as 1966 Archibald had argued that rival theories should be compared in terms of their predictive success and gradually many economists and econometricians moved towards this more relativist position.

7. See for example Laidler (1971) who argues the case that ‘comparing the reduced forms of these two models is not a very useful test of their relative empirical relevance’ (p. 80). Archibald was not unaware of the potential for comparisons being inconclusive (1966, p. 295). Lipsey’s way of saying the same thing is: ‘The choice is not one between theory and observation but between better or worse theories to explain our observations’ (1966, p. 14). Then the problem to be faced is that theoretically defined variables can differ substantially from the available observed data; moreover the observed data may themselves be incorrect. Indeed published data are far from being objective facts against which theories are to be appraised thus striking at the foundation of ‘positivism’. This is a feature common to single equation work and the large-scale econometric models. In assessing eleven models of the US economy Christ (1975) wrote ‘[the models] disagree so strongly about the effects of important monetary

8. an fiscal policies that they cannot be considered reliable guides to such policy effects’ (p. 54).

9. See Darnell and Evans (1990) for an extended explanation of this misunderstanding of the methodological status of econometrics; and a counter position to the view that ‘Econometrics is now a fully fledged and distinct discipline’ (DeMarchi and Gilbert, 1989, p. 11).

References

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