

Towards an Empirically Valid Economics

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The movement to replace neoclassical theory with an alternative body of theory, one that can place economics on a scientific footing, has now entered its third, and decisive, stage.

The first stage, extending almost from the moment neoclassical theory emerged in the 1870's down through the immediate post World War II period, or for nearly 100 years, was concerned with pointing out the fallacies of that approach. Marx, Veblen, Keynes and others all contributed in important ways to the ensuing critique (Seligman, 1962). Still, the most striking blows to the logic of the system were struck by Piero Sraffa, first in his 1926 article on increasing returns and then, even more tellingly, in his 1960 book, Production of Commodities by Means of Commodities, and by Joan Robinson in her 1953 article, "The Production Function and the Theory of Capital," which marked the onset of the Cambridge controversy in capital theory.

Today, neoclassical theory stands totally discredited on intellectual grounds - whether the theory takes the form of Marshallian partial equilibrium analysis, a neo-Walrasian general equilibrium model, the microeconomic half of the "neoclassical synthesis" or a Clarkian steady-state growth model. Whatever the form, neoclassical theory can be shown to be either logically flawed or empirically irrelevant. No less important, the inability of the theory to suggest effective policies for dealing with the world's economic problems - whether they be the widespread unemployment of the 1930's or the double-digit inflation of the 1970's - have shown neoclassical economics to be politically as well as intellectually bankrupt.

The second stage in the shift of economics away from the neoclassical paradigm, extending from the publication in 1956 of Joan Robinson's great work of synthesis, The Accumulation of Capital, down to the present, has been taken up with the task of developing a coherent alternative. This effort has drawn on the three major dissident traditions in economics - Marxian, institutionalist and Keynesian. Still, the core of the new approach that has emerged during this period is distinctly post-Keynesian (and post-Marxian), combining as it does the growth dynamics of Roy Harrod, the production theory of Wassily Leontief, the value theory of Sraffa and the distribution and pricing models of Michal Kalecki - along with the monetary ideas of Keynes himself - into a single coherent view of a capitalist economy that is expanding unevenly over time (Eichner and Kregel, 1975, Eichner, 1979b).

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Although much work still remains to be done in filling in the details, the broad outlines of the new theoretical approach are clear: It encompasses production rather than just distribution, income effects rather than just substitution effects, a monetarized rather than just a barter economy. The key to the dynamics of the system is the rate of technical progress and the accompanying process of accumulation, which in turn determines both the distribution of income and, in conjunction with the growth of real wages, the set of relative prices. In each of the areas touched upon by this alternative approach - production, distribution, pricing, labor, tax incidence, international trade, natural resources and money - the conclusions are strikingly different from those of neoclassical theory. It is the economics of a real economy, moving forward in historical time, with the system almost certain to be "out-of-equilibrium." (Eichner, 1979b)

Still, the specification of this alternative theoretical system is not enough. To be considered scientific, a theory must satisfy not only the logical test of coherence but also at least three additional tests, all empirical in nature. These are 1) the correspondence test--that the conclusions which follow from the theory are confirmed by what can be observed in the real world; 2) the comprehensiveness test--that the theory is able to encompass all the known facts pertaining to the class of phenomena under study, and 3) the parsimony test--that all the elements used in constructing the theory, including any assumptions, are indispensable in accounting for what can be observed empirically. The third stage in the development of an alternative to the dominant neoclassical theory, the stage only just begun, will be occupied with applying all of these empirical tests to the new theoretical approach. But, even these tests may not be in sufficient to guard against serious error. It is necessary, in the case of social science theory, that one additional test be applied. This is the praxis test--that the policies based on the theory, when implemented, lead to the results predicted by the theory. (Eichner, 1984, 1985b). Unless this third stage can be carried out, there remains the danger that the alternative theoretical system, no matter how great its triumph in other respects, will simply lead to the establishment of a new orthodoxy, one that will be no more successful than its neoclassical predecessor in placing economics on a scientific footing. To understand this last point, one needs to examine the role of empirical research in relation to science. Only then can one fully appreciate why the neoclassical paradigm has become intellectually bankrupt and what role empirical research must play in the further development of economics as a science. to be "out-of-equilibrium." (Eichner, 1979b)

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One pitfall is to insist that the theory cannot be developed except in either a purely formal way, that is, deductively, or, alternatively, in a fully comprehensive manner based on the historical method. This constraint on the construction of theories not only hampers creativity but, even more deleteriously, is likely to keep, truth hidden. The error to which economists are particularly prone is to insist that a theory cannot be developed except formally. Indeed, this is part of what Wiles (1979-80), following Schumpeter, has termed the Ricardian vice. The fallacy here is in equating what may not be logical with what is illogical.

Certain empirically observable phenomena may not be deducible from simpler propositions, assumptions or other observable phenomena. This, it turns out, is a common result of moving from one resolution level to another, e.g., from physical atoms to chemical molecules. It does not mean the phenomena are illogical. It only means that they cannot be arrived at in a logical manner--or at least not by using the system of logic, mathematical or otherwise which one has selected as the tool of analysis. There are many phenomena in economics which are non-logical by this definition but still readily observable. They include the interdependent behavior of price leaders-price followers in oligopolistic industries, the commitment of workers to the quality of whatever good or service they are providing and the unemployment created by excess private savings relative to investment. One may be able subsequently to "rationalize" the observable result, that is, to show it is not illogical within a certain theoretical framework, but one cannot obtain the result deductively as a necessary conclusion. Economics as a discipline has been greatly impoverished over the years by its insistence that all phenomena be explained by purely formal, that is, mathematical models. As a consequence, the real world of the non-logical but observable has escaped it.

No less crippling is the insistence that a theory be fully comprehensive, in the sense of accounting for every known piece of evidence. This involves the fallacy that nothing is explained unless everything is explained at once. Economists, as a group, have not been prone to this error. They have been more than willing to develop simplified models which abstract from the complexity of observable reality--even if they have then been reluctant to validate those models empirically. Still, there is a hint of this fallacy in the argument by some economists that an equilibrium, to be a true equilibrium, must be a general one with all markets clearing simultaneously. Even though the models based on this line of argument are not meant to explain any observable phenomena, and indeed are anti-empirical and thus anti-scientific in spirit for precisely that reason, still the antecedent question of whether every market throughout the economy must be shown to have cleared before the analysis of any one of those markets can be said to be complete needs to be addressed. The argument, it should be understood, is but a variation on the theme that nothing is explained unless everything is explained at once. Of course, if a disequilibrium in one market were to have a significant impact on another market, that would be another matter. But then the issue would be the empirical one of determining the impact of the one market on the other--not a matter of a priori insistence, whatever the evidence may be.

A further pitfall to avoid at the theory construction stage is that of developing a line of argument which, by the very nature of the factors identified, cannot be validated empirically--or which is at such variance with observable reality that it is certain not to pass any of the necessary empirical tests once the theory has been fully worked out. To fall into this trap is to waste everyone's time, including one's own. Unfortunately, this is precisely the trap into which the majority of economists have fallen repeatedly throughout the post World War II period, beginning with the diversion created by the emphasis on neoclassical growth models and continuing today with the excitement over "rational expectations" models. The former, based on the metaphysical concept of a "marginal productivity" associated with "capital," has little prospect of ever being validated empirically. The initial encouraging results from econometric tests were simply the result of an unchanging labor share of national income and the specification of a Cobb-Dougllass production function; the same evidence is consistent with almost any other model of economic growth (Shaikh, 1974, 1980; Davenport, 1981). As for the rational expectations models, it is hard to take seriously an argument based on both a one-commodity economy and price-adjusting Walrasian markets. The one-commodity assumption suggests the absence of any role for money while the Walrasian nature of the markets implies the absence of an industrial sector. How a model with features such as these can be expected to meet the praxis test, let alone any other empirical test, is a mystery.

This further pitfall, then, is an especially important one for economists to avoid and it requires that they adhere to a few simple rules.

First, the variables identified as either dependent variables or explanatory factors must be capable of being observed empirically in some manner. Thus the neoclassical theory of production, which is based on

substitution among inputs, should be replaced by fixed-technical-coefficient models like those of Leontief, Sraffa and von Neumann. The variable K , denoting the capital stock in real terms, is especially to be avoided. (Eichner, 1984, 1985b) The same rule requires that, when it comes to analyzing household behavior, the conventional indifference curves be replaced by empirically estimated income and price elasticities.

Second, the assumptions underlying the model, and therefore the conditions under which the model holds, should be identified at the outset. Ordinarily, this would suffice to protect against either of two additional sources of difficulty: one is that the conditions under which the model holds are unlikely to be encountered in practice so that the model is without relevance for the real world; the other is that the model will be applied in circumstances other than those under which the argument holds so that the wrong conclusions are drawn. Many economists have been persuaded by Friedman (1953), among others, that any assumption is permissible. Thus it is necessary to add two codicils to this second rule:

- a. If the assumptions underlying the model represent conditions unlikely ever to be realized or observed in practice, abandon the model as worthless.
- b. If the model holds only under certain conditions, do not use it to analyze a different set of conditions.

Unfortunately, there are more than enough examples of both strictures being violated in economics. Models which hold only under conditions unlikely ever to be observed in practice include not just the rational expectations ones but, indeed, the entire broader group of Hicks-Arrow-Debreau general equilibrium models. As for the models which have been applied to circumstances other than those under which they hold, these include all the models which rely on supply and demand curves to analyze pricing behavior in the industrial sector. The positively sloped supply curve--if not also the negatively sloped demand curve--applies only to the commodity sector of a modern market economy and even then only to the portion which is not controlled either by the government or by large industrial enterprises. This is why the excess-demand explanation of inflation has been so misleading. It assumes that the supply curves in the industrial sector are, for the most part, positively sloped.

Yet a third pitfall to avoid is that of extending or modifying a body of theory which, experience has shown, cannot be empirically validated. The most that can be hoped for is that the variant of the revised theory will not be immediately disconfirmed by the available evidence so that the proposition will, for the moment, satisfy the correspondence test (most likely because, as will be noted shortly, a weak form of the correspondence test has been applied). Whatever the evidence in support of the proposition may be, if it is logically linked to a more basic body of theory, one which, being inconsistent with other evidence, lacks empirical validity, the two together will still fail the comprehensiveness test. This is why the effort to improve neoclassical theory by adding more "realistic" features, such as money or fix-price markets, is an exercise in futility. Progress cannot be

made until all of economic theory's neoclassical features, and not just some, have been eradicated. Building upon a false argument, no matter how sound the extension or modification may itself be, will still leave the argument false. It is this methodological point which argues most tellingly for purging from economics all of the metaphysical constructs underlying the neoclassical approach and basing any future work in economics on the alternative body of theory. This advice, were it to be followed, would free empirical research in economics from the single greatest handicap under which it presently suffers, leading to a renaissance of the discipline as an intellectual activity. Before elaborating on that point, however, it is necessary to address the methodological principles of theory validation which are distinct from those of theory construction.

Once the task of formulating an addition to some existing body of theory has been accomplished so that it satisfies both the coherence and comprehensiveness criteria, the steps then be taken are narrowly circumscribed. The methodological principles which need to be followed at the theory validation stage permit little latitude. The proposition being advanced must first be contrasted with each of the alternative formulations that are possible (including, but not limited to, the null hypothesis that no such relationship exists). The available empirical evidence (including, but not limited to, quantitative data) must then be examined to see with which of the several alternatives the evidence is most consistent. Finally, if the proposition with which the available evidence is most consistent is not the one initially advanced, one needs to retreat to the theory construction stage and rework the entire argument. Just as there are pitfalls at the theory construction stage, so too there are pitfalls at the subsequent validation state.

The first pitfall to be avoided at the validation stage is failing to identify all the other arguments possible so that the proposition being advanced is not, in fact, tested. Indeed, this is one reason for skepticism about much of the empirical work being done along neoclassical lines. Seldom, if ever, is the proposition being advanced tested against alternatives even when those alternatives have long been a part of the economics literature. The usual practice is to test the proposition against the null hypothesis only - a notoriously weak test, one that engenders even less confidence when it is based on time series data. Since the serial correlation which occurs when time series data are used cannot be eliminated entirely, this type of test leads to an inflated R^2 which may give credence to an argument which, in fact, is false. Even if other types of data are used, however, a test against the null hypothesis, namely, that no such relationship exists, is a weak test.

To have greater confidence in the results, one must test a proposition, not just against the null hypothesis (as may be necessary initially) but also, against each of the alternative arguments for which there is similar supporting evidence. In this way, one is less likely to be left with several explanations which, though seemingly at odds with one another, are all equally consistent with the available evidence. One can instead focus on what is the crucial difference between any two of the explanations. This crucial difference is the result that one would expect to observe if the one

explanation were correct but not if the other were correct. Once that crucial difference has been identified, a more rigorous test of the proposition being advanced can then be devised. To apply the correspondence test in this more rigorous form, it is, of course, essential that each of the possible alternative arguments first be identified. Indeed, unless this is done, and the investigation then centered on what is the crucial difference between that proposition and any alternative explanation, the test will be flawed.

A second pitfall to be avoided at the theory validation stage is that of drawing too hard and fast a distinction between theoretical and empirical work. It is not just that a theory must be tested empirically, either to be accepted or rejected. It is also that, when the results obtained from an empirical study are different from those expected, the theory must be revised so that it will then be consistent with all the evidence, old as well as new. Unless this second step is taken, the steady accrual of against most, if not all, of the prevailing theories will continue--as increasingly the in economics today.

The necessary revision of theory in light of the empirical evidence is less likely to occur when, as case in economics, a sharp line is drawn between theoretical and empirical work. Theorists will continue to ignore empirical evidence, insisting that their formal proofs are sufficient, while those engaged in empirical research consider theory revision to be someone else's responsibility. This unfortunate situation will end only when economists begin, as a routine matter, to cross the line which separates theoretical and empirical work. Theory construction and empirical validation are not two separate activities to be pursued by two separate groups of economists. They are only separate steps in the same interactive process of making the theory and the available evidence consistent with one another.

The sharp line drawn in economics between theoretical and empirical work is unknown in the natural and biological sciences. In those fields, no reputable scholar would present a new theory without at least some supporting evidence. The need to integrate theoretical and empirical work, however, goes beyond a mere reference to the relevant literature, whether it be theorists citing supporting empirical studies or empirical investigators indicating the theoretical basis for the interpretation they have given their findings. The integration of the theoretical and the empirical needs to be a part of every economist's normal mode of working. Theoretical arguments are likely to remain naive about the real world as long as those making the arguments have little or no feel for the empirical evidence because they do not themselves work with data. And empirical studies are likely to be of little value as long as those carrying out the studies remain unconcerned about the underlying theoretical issues.

Indeed, this last point is illustrated by the great bulk of empirical work being done today which, rather than providing support for any of the key elements of neoclassical theory, merely use those theoretical constructs to draw certain conclusions from the evidence at hand. The conclusions are only as valid as the theoretical constructs upon which they are based--which

means, in light of what has already been said, that very little confidence can be placed in those empirical results. Virtually all the studies of economic growth based on the neoclassical growth model, along with virtually all the studies of the returns to education and training based on the human capital model, fall into this category. What the above argument suggests is that theorists need to be more directly involved in empirical research and empirical researchers need to be more directly concerned with the ongoing, cumulative development of theory. While economists are still likely to continue specializing in one or the type of work, depending on their particular bent, the two activities cannot be kept as separate as they have in the past--not if economics is to become a truly scientific endeavor. At the present critical stage in the development of economic theory, it is essential that an extensive program of empirical research be carried out to build an alternative which is more relevant to the real world and more useful to policy makers. It is this program of empirical research which constitutes the imminent research agenda for post-Keynesian and other economists.

The imminent research agenda

The emergence of an alternative body of theory (whether labeled post-Keynesian, Sraffian, Marxian or institutionalist) should prove a veritable boon to empirical research, both quantitative and historical. What empirical researchers have observed in the real world--whether it be mark-up pricing, fixed technical coefficients of production, dual labor markets or credit rationing--will then no longer seem puzzling. One of the first items on the research agenda is thus to go back over the body of empirical work already accumulated to see whether most of the unsatisfactory results previously reported are not actually a confirmation of the alternative theoretical approach. In this way, a good deal of the earlier empirical work can be given the recognition it deserves, and a new generation of researchers encouraged to follow up on those leads. Post-Keynesian theory, in particular, suggests a large number of testable hypotheses; specifically, the importance of fixed investment in determining the growth of productivity and aggregate output, the effect which macroeconomic variables have on the distribution of income, the relationship between the growth of money wages and inflation, and the endogenous nature of the money supply. While some evidence on each of these points is already in hand, a systematic test of these and the many other relationships suggested by post-Keynesian theory is needed.

In pursuing this research agenda, the two separate parts of post-Keynesian theory must be recognized. There is the long-period analysis, based on a comparison of alternative steady-state expansion paths, and there is the short-period analysis, intended to explain the actual movement of economic systems through historical time. While it might be thought that, since steady-state rates of economic expansion can never actually be observed, only the second of these two parts lends itself to empirical validation, this is not the case. Whether mathematically or otherwise derived, the propositions that follow from a long-period analysis, no less than those that follow from a short-period analysis, can be tested empirically. Although steady-state rates of expansions may never actually

be observed, the rate of growth of the economy over time, once any cyclical fluctuations have been taken into account, may serve as a reasonable approximation--at least reasonable enough to demonstrate the validity of the post-Keynesian long-period propositions.

This means that the long-period theory can be tested in either of two ways: 1) by comparing the growth experience among a group of similarly developed countries, such as the OECD nations, or 2) by comparing the growth experience of the same country over different time intervals. While there are serious limitations to either approach, the two together should provide a sufficient basis for testing empirically the key points derived from a post-Keynesian long-period analysis. Cornwall (1977) and Wilson (1981) have already demonstrated how the first approach can be used to good effect, while Garrett (1981) has shown the use that can be made of the second approach (see also, Semmler, 1984; Ochoa, 1984).

It is, however, in testing the implications of the short-period model that empirical research based on post-Keynesian theory is most likely to flourish. Several elements of that short-period model, such as the fixed technical coefficients of production and the mark-up basis for price formation, have already been extensively tested empirically--with results that are almost, without exception, favorable to those arguments. Recently Moore (1979, 1981) has begun to provide evidence as to the endogeneity of the money supply. Still, there are certain elements, such as the disequilibrium properties of the model and the exogenous nature of the money wage rate, which have yet to be tested explicitly. Moreover, the model as a whole needs to be empirically validated. For this reason, as well as others, the construction of large-scale macroeconomic models is likely to become the focal point for empirical research based on post-Keynesian theory and, in particular, for efforts to validate the essential features of the post-Keynesian short-period analysis.

One of those other reasons has to do with the difficulty of testing a theory in economics. While statistical analysis can compensate somewhat for the near inability to carry out controlled laboratory experiments, econometric studies are not without their limitations. Because of serial correlation and other problem encountered, especially when the study must be based, as it often needs to be, on time series data, it is all too easy to reject the null hypothesis--even when the proposition being tested is a false one. Experienced econometricians have learned to distrust the R^2 and similar statistics. They have also learned to distrust the results based on any singler equation model. While a multi-equation model may be just as invalid as any single-equation model, it at least must bring out more of the underlying relationships--and thus the likelihood is greater that, if incorrectly specified, a multi-equation model will fail the several empirical tests. Errors, rather than being concealed in aggregates, will tend to be compounded as the simulation of the model proceeds so that those errors will, by the end of the simulation exercise, be more readily apparent. One can therefore have greater confidence in a multi-equation model that is consistent with the historical data, especially if the test is based on a simulation exercise, than one can have in a single-equation model. It is this logic for developing multi-equation models which argues

for constructing a model for the economy as a whole, this being the ultimate limit, within a nationalistic framework, on the type of multi-equation model that can be developed.

There is the further advantage that a model constructed to explain the behavior of the economy as a whole can be used to obtain future estimates of precisely those variables which are of the greatest interest not just to economists but to public officials and other lay people. These variables are the growth of real output, the aggregate price level and employment. The ability of the model to provide future estimates of these variables not only means that the model has potentially greater social usefulness but also that it can be subjected to a further empirical test. When the estimates relating to the future growth of real output, prices and employment turn out to be consistently wrong, even after unanticipated changes in policy are taken into account, there is good reason to reject the model as being invalid. By representing the economy in a large-scale econometric model, it is easier to determine if a body of theory, such as the post-Keynesian short-period analysis, can meet the correspondence test and, to the extent that policy is subsequently based on that model, the praxis test as well.

It is for all of these reasons that efforts are now under way in a number of countries to construct large-scale econometric models, based on what can broadly be termed post-Keynesian theory. A model of the U.S. economy is being constructed at the Center for Economic and Anthropogenic Research,¹ while a similar model for the U.K. economy is being constructed at Thames Polytechnic in London.² These are models which can be sharply distinguished from the more conventional Keynesian and monetarist models.³ The CEAR model is being developed so that not only the results but also the underlying data base will be available to other researchers. This is being done not just so others can reproduce, and thereby check, the results. It is also being done so that others will have a common base from which to start in pursuing empirical research of their own--especially empirical research which seeks to extend, in some important way, the same post-Keynesian model of the economy. Starting from such a common base is essential if the development of economics along post-Keynesian lines is to be cumulative.

This common base does not preclude the possibility that different versions of the same basic model will emerge as others join in the task of empirically validating post-Keynesian theory. Indeed, differences have already arisen in the way the several large-scale econometric models now being constructed are specified, and further differences can be expected to arise as other investigators attempt to enlarge upon or extend those models. It only means that, whatever the different versions, the models should not be at odds with one another on any fundamental point. If they are at odds, it is a sign that the program of empirical research has omitted an important step along the way. That step is to make sure that when two or more opposing arguments emerge within the same theoretical framework--each interpretation equally defensible in light of the available evidence--a concerted effort is then made to determine which argument has the greater empirical validity. Such a systematic attempt to reconcile, through further empirical research, any points of difference should be the third item on the

imminent research agenda. It is yet another way in which the new theoretical approach is likely to prove a boon to empirical research.

The research agenda just outlined should go far toward establishing economics as a scientifically-based discipline, with a body of theory which has been empirically validated and which can, at the same time, provide the understanding of economic phenomena needed to solve pressing social problems. If the theory can meet all the required empirical tests, then the emerging new paradigm in economics will have passed the third, and most critical, stage in its development. But even if that body of theory requires significant modification, it will, nonetheless, have served the important historical purpose of rescuing economics from the intellectual and political bankruptcy into which it has now sunk, demonstrating the more constructive lines that empirical research can take if fully supported by a more appropriate theoretical foundation. More importantly, economics will have finally become a science, with its theoretical core merely the starting point for further empirical research rather than a body of settled doctrine to be defended against the observations of the real world.

FOOTNOTES

¹See, for example, Eichner, 1979b; Forman and Eichner, 1981; Forman, Groves and Eichner, 1984.

²Arestis and Driver, 1984b, 1985-86.

³Earlier examples are Cornwall, 1972, and Sylos-Labini, 1974. A recent comparable model is the one constructed for Great Britain by the Applied Economics Group at Cambridge University. See Godley and Cripps, 1983; Arestis and Driver, 1984a; Cuthberston, 1979, ch. 3.

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