Barry Herman*

ON MUDDLED METHODS AND THEIR MEANING[1]

Recently, an article appeared which is a superb example of what we might call advocacy econometrics.[2] The article, by Dale Jorgenson and Calvin Siebert [4], has already crept into some graduate course reading lists, at least at the University of Michigan, and thus, by some, is considered important. It seems, however, that there are serious methodological problems in the article. Most important, the authors seem to be in error when they claim their work contributes to the empirical testing of the neo-classical theory of the firm. It is intended to demonstrate these points below as well as to suggest some reasons for the present healthly state of the neo-classical theory of the firm.

Advocacy Econometrics

At Columbia University's Spring 1966 Commencement exercises, former President Grayson Kirk is reported to have extolled the beauties and benefits of "value-free" science. Nevertheless, it is open to question whether any social science can be "objective". With respect to economics, Mike Zweig, in a very provocative paper [9], pointed out a number of value-ladden aspects of price theory. A similar critique may be attempted for econometrics. Here, however, we shall merely assert that our values contribute to our biases and help determine what theories we think up or believe to be true. In effect, we begin where Zweig ended. That is, we shall concentrate on the nonobjectivity.

In science, theories are never proved true or false, but are rejected or not rejected. Thus, economics as a science relies very heavily on those empirical specialists who attempt to convince us to reject or not. Let it first be noted the econometrician's task is fundamentally difficult, requiring everything from the ability to organize anarchic information into useful categories, thus forming data, to the ability to use this data to evaluate theories in some meaningful way. If economics is "science", econometrics is "art"--there is judgement at every step.[3]

One major problem in the evaluation of theories is how to operationalize them for testing. We are all aware, I am sure, of the psychologist's dilemma, confounding intelligence with I.Q. scores, as well as our own problem of discovering

*Graduate Student, Department of Economics, University of Michigan.

the empirical equivalent of the elusive Platonic ideal, "capital". If one wants to compare theories, one should not want the outcome determined before even getting to the data. For example, one wants to make sure he is indeed comparing different theories, rather than different operational forms of the same theory. In this way we are led to study the "art" of making a theory operational.

Jorgenson and Siebert set out to compare various theories of investment behavior in a microeconomic setting. They concluded that the neo-classical theory of the firm is the best available description of investment behavior for the specific firms studied, such as General Motors, Goodyear, General Electric, Standard Oil of New Jersey, IBM, and so on. In addition, although clothed in academic humility, there was a note of implicit jubilation in their closing paragraphs on emerging victorious over the insurgents and on restoring peace to Camelot.

> We conclude that the objections to the neo-classical theory of the firm as a basis for the theory of investment behavior...are ill-founded. The appeal to a broader view of entrepreneurial objectives is not supported by evidence from econometric studies of cost and production functions or from studies of investment behavior. ([4], p. 709)

It appears that the authors view their study, at least in part, as a comparison of the neo-classical idealization of the firm against a "broader" view in which the firm is characterized as a complex organization in a complex environment. But do they actually do this? We turn to their work to show they do not.

Jorgenson and Siebert accept as common to the "theories" to be tested that they can be formulated as flexible accelerator models in which a firm's net investment is proportional to the difference between its actual capital stock and its desired capital stock (defined in some way). This formulation is equivalent to a distributed lag model in which actual capital stock is a function of past and present desired capital stock. The "theories" are to differ, then, in what they suppose are the determinants of desired capital stock. Let us then turn to these "theories".

The "theory" that did worst in their tests presumes that desired capital stock is determined by "the flow of internal funds available for investment" ([4], p. 685); i.e., realized profits (specifically, retained earnings plus depreciation) are assumed to be a proxy for expected profits from investment. Now, profits should be relevant to investment decisions, but not necessarily to desired capital stock. Profit may operate as a constraint on investment spending if the firm relies primarily on internal funding; but to suggest that firms take profits as a proxy for expected profits is to oversimplify to the point of assuming that businesses are stupid. On the other hand, if econmetricians assume firms act as if current profits are what are used to forecast expected profits in an analysis of real firms studied as individual units, then it would seem econometricians act "as if" econometricians are stupid. However, the only way liquidity considerations, as a separate hypothesis, can be forced into Jorgenson and Siebert's flexible accelerator framework is to have these determine desired capital stock. In fact, the authors switch back and forth claiming the liquidity considerations imply, on the one hand, an expected profits hypothesis ([4], p. 685) and, on the other, a cost of capital hypothesis ([4], same page, same paragraph).

Thus, it seems the "liquidity theory" does not represent the "broader" view. The liquidity theory might have been put forward as a programmed decision rule used by firms such that the hypothesis would fall out of the behavioral theory of the firm, except no advocate of the theory, I think, would put business fixed investment into the programmable decision category. If anything, this "liquidity theory" can be construed as a very simple, "testable," operational form of neo-classical profit maximazation.

A second "theory" which did measurably better on the authors' tests takes the market value of the firm as the proxy for expected profitability from investment.[4] This argument has some grounding in price theory where market value is said to be determined by the present value of expected future profit flows. But, again, this formulation is an operationalization of neo-classical theory and not some "broader" view.

The third "theory", which did about as well as the second, is the flexible accelerator proper; desired capital stock is to be proportional to current output. In this "theory", firms are said to want to maintain either some desired constant capital/output ratio or capacity utilization rate, or are said to use output or sales as a proxy for profitability of new investment. In the latter case this "theory" is again not a "broader" view, and in the former this "theory" is usually presented as a rough and ready, ad hoc, aggregate relation between investment and output with its derivation from any price theory usually implicit.[5] In either case, where is the "broader" view?

Finally, Jorgenson and Siebert present their version of the neo-classical theory based on a maximization of net worth (discounted net revenue) objective function from which is derived the first order condition that the marginal product of capital should equal the rental rate on capital (or "user cost") divided by the price of output. All that the authors wanted from the first order condition is an elaborate expression for user cost of capital which involves an investment goods price index, the rate of replacement, a cost-of-capital term, the corporate income tax rate and "the proportion of depreciation at replacement cost deductible from income for tax purposes" ([4], p. 695).[6] This user cost, ct, is related to desired capital stock, K_t^* through the elasticity of output, α, which is constant for Cobb-Douglas production functions, but not in general. Thus,

$$K_t^* = \alpha \frac{p_t Q_t}{c_t}$$

where ptQt is the value of output. This complicated and subtle formulation is to do battle with, for example, functions of the form

$$K_t^* = \beta Q_t$$

for the accelerator model. It would seem the effects on desired capital picked up by the accelerator model through Q_t might also be picked up by the neo-classical formulation since Q_t appears there as well.

The main thrust of my argument is that Jorgenson and Siebert are not comparing different theories but rather alternate formulations of the same theory. Their results, that the most complete formulation does best in their tests, should not be surprising if there is any predictive value at all in the neo-classical theory. Further, if the formulations above are supposed to represent operationalizations of "broader" theories, they also conform to operationalizations of neo-classical theory. How then are we to interpret tests rejecting these formulations?

In addition, not only are they not testing different theories, but because the only firms they studied were fifteen of the largest American giants, they blow their whole bit by writing, "Of course, considerable care should be taken in extrapolating the results of the present study to small firms..." ([4], p. 692).[7] Thus, G.M. is "shown" to operate as a neo-classical firm but perhaps the dealers on the Chicago grain exchange do not.

It may well be the case that the authors were not initially concerned with the theories behind the models they used, but were merely concerned with the theories behind the models which had previously been used with varying and contradictory success by a host of authors. This seems implicit throughout their paper until their conclusion where they relate their work to the challenges posed to the theory of the firm made within the professional establishment. In other words, the relevance of their work to the theory of the firm may have been only an afterthought, but an important one.

The importance of this may be seen in a statement by Herbert Simon with which Jorgenson and Siebert disagree. They quote Simon,

> I should like to emphasize strongly that neither the classical theory of the firm, nor any of the amendments to it or substitutes for it that have been proposed [written in 1962] have had any substantial amount of empirical testing. ([4], p. 709; see also [7].)

Our authors respond,

> Simon ignores the entire econometric literature on cost and production funtions, all of which is based on the neoclassical theory of the firm. The evidence is so largely favorable to the theory that current empirical research emphasizes such technical questions as the appropriate form for the production function and the statistical specification for econometric models of production based on this theory. Simon's characterization of alternatives to the neoclassical theory of the firm is correct; this theory [sic] has not been subjected to substantial empirical testing. However, his characterization of the empirical evidence on the neoclassical theory is seriously incomplete.([4], p. 709.)

I have the impression they missed the point. To build statistical models more or less derived from the received theory of the firm and to fit these to data and to go no further is not to test a theory. We should require, at least, a comparison of the predictive value of the statistically fitted model with some naive model on <u>different</u> data from that used to derive the statistical model. But Jorgenson and Siebert run their tests on the same data used to fit their equations.

By the most common tenet of scientific methodology we never "accept" hypotheses. We either reject or refrain from rejecting them. Our confidence in our theories grows as the number of times we try to reject and fail mounts. However, to fit a curve is not an attempt to reject a theory. In other words, experimenting with various operational forms until a satisfactory formulation is found is not testing a theory. How many times do econometricians go to their work hoping to disconfirm a hypothesis derived from neo-classical arguments? To repeat Simon, has there been "any substantial amount of empirical testing"?

Competing in Economics

It seems we do not usually see tests of theories in economics unless someone is interested in debunking one theory to support another. Milton Friedman put forth his permanent income hypothesis and econometricians began trying to disprove it. Thus, for example, we have Ronald Bodkin's test [1] and Friedman's discussion claiming that the results which seemed to strongly reject the permanent income hypothesis really conformed with it, "properly" interpreted; i.e., that Bodkin had not made the correct transformation from theory to operational model. In other words we have the advocates of opposing theories fighting it out: one sets up a test which may be damaging, in which case the other tries to show the test illicit.

We can further illustrate this point with a paper by Eisner and Nadiri [2] which claims to refute Jorgenson's neo-classical investment model by showing the alpha in his equation (see above) is not constant empirically. That is, they claim to show, on the same data Jorgenson used previously, that the elasticity of substitution is closer to zero than to one (the Cobb-Douglas case for which alpha is constant) and that the elasticity of output with respect to capital (alpha) is indeed rather high which indicates the good results obtained by Jorgenson in earlier tests were due more to the accelerator term than acknowledged. Jorgenson and Siebert, in the paper discussed in the previous section, test the accelerator model against the "neo-classical" model on individual firm data and find, as mentioned, their model does better. I expect the other side will soon rebut and we shall be off on another round.

Because econometrics is so much an art, it seems advocacy econometrics is the only way any "progress" can be made.[8]

We can expect Friedman and Meiselman to run a test of Keynesian versus Quantity theories which vindicates the Quantity theory and we should not be surprised when an issue of the AER (September, 1965) is, for the most part, devoted to rebuttals questioning their technique and models. Econometrics is certainly not "objective". However, it should be clearly understood that I am not implying that there are attempts to defraud. Rather, our methods are influenced by our biases, perhaps with acumen sharpened in the heat of battle, perhaps not.

Barriers to Entry

We may now return to Herbert Simon's complaint: why are there few tests of alternate theories of the firm? In some respect these theories may not be any threat to neo-classical theory. Of the existing new theories, few attack neo-classical theory but rather treat problems about which neo-classical theory is ambiguous, e.g. oligopoly behavior. Thus, Fritz Machlup [5] calls for a love-in for the different theories, different theories for different problems. Also, even in that area of overlapping concern in which theories could be compared there is still the difficulty of operationalizing the new theories. Another problem, at least with the behavioral theory of the firm, is that hypotheses comparable to those of the neo-classical theory are not developed. The theories are in two different worlds. If a "political economy" theory of the firm should arise from our collective endeavors, it may well operate in a "third world" because the considerations with which we may deal will not be those allowed for in neo-classical theory.

This suggests, I think on a superficial level, why there is no econometric battle over the neo-classical theory: there is little direct conflict between theories and thus no advocates to do each other in. Jorgenson and Siebert's advocacy econometrics is really part of a family squabble within the neo-classical camp; factionalism if you will.

Be that as it may, we can now suggest why there is no conflict of theories. Interupt a working economist and ask, "what is a firm?" and repeat the question two days later when he is working on a different problem and see if you get the same answer. The concept is elusive. When concerned with methodology, which is often the case in rebutting critics of "marginalism", economists treat the neo-classical "firm" as a total fiction, "a mental construct helping to explain how one gets from the cause to the effect " in discussing market behavior (Machlup [5], p. 9). In the Jorgenson and Siebert paper the neo-classical firm is the real flesh and bones corporation: G.M., I.B.M., etc. Should their results have proved less attractive to their view, someone would have written sagaciously that the firm is an abstraction and the theory certainly not meant to describe the behavior of individual units like G.M. This is not duplicity because we have not derived rules to determine when a firm is a firm and when not.[9] This is confusion.

Note, however, one additional point. If Machlup-Friedman "Platonists" predominate, economics comes to be defined as the study of markets (not even of behavior in markets) rather than the study of getting and spending, or as Samuelson's text puts it, determining "what, how and for whom". The area of economic analysis is then severely circumscribed; certain questions can not be handled (income distribution, imperialism, the military-industrial complex and other aspects of corporate structure such as conglomerates, etc.). In particular, there is then no room for political economy in economics. We are defined as belonging in another (presumedly empty?) box. Yes, ladies and gentlemen, I fear we are to be banished, suspended, expelled, expunged or ignored (depending on what year one sits in (or sat in) at the University of Chicago.

In sum, I have tried to indicate the advocacy nature of econometrics and to suggest why neo-classical theory is as healthy as it is. It needs a healthy challenge.

Footnotes

1. The marginal product of this paper has been increased measurably by discussions with Professors W.H. Locke Anderson and Sidney Winter. Professor Winter has especially made his imprint on the final sections of the paper.

2. Apologies to Rick Wolff for bastardizing the title of his paper in the 1968 URPE Conference Papers [8].

3. This applies also to the statistical tests used by most econometricians which, as any Bayesian will argue for hours, are essentially arbitrary.

4. Actually, here the authors also treat this theory as if firms desired to maintain a constant ration between capital stock and market value of the firm [4], p. 695), but

this seems absurd and not derived from any arguments.

5. Development economists have had an especially good time showing how constant aggregate capital/output ratios can only be derived from micro-theory under the most extreme and absurd assumptions. See, for example, the demolition in Appendix 3 of Gunnar Myrdal's Asian Drama[6].

6. See also [3], pp. 248-9, for a derivation of this expression.

7. Since this statement appears early in the article in their data section they perhaps forgot it when warming to their grand conclusion.

8. It should also be noted that the advocacy metaphor is incomplete because there is no rule making and conflict resolving bar before which we advocate -- only advocates and jury.

9. This point is Professor Sidney Winter's.

References

1. R. Bodkin, "Windfall Income and Consumption", and comment by Milton Friedman, in I. Friend and R. Jones, eds., Proceedings of the Conference on Consumption and Saving, Vol. II (Philadelphia, 1960), 175-187, 191-206.

2. R. Eisner and M.I. Nadiri, "Investment Behavior and Neo-Classical Theory", Review of Economics and Statistics, L, No. 3, (August, 1968). 369-382.

3. D. Jorgenson, "Capital Theory and Investment Behavior", American Economic Review: Papers and Proceedings, LIII, No. 2, (May, 1963), 247-259.

4. D. Jorgenson and C. Siebert, "A Comparison of Alternate Theories of Corporate Investment Behavior", American Economic Review, LVIII, No. 4, (September, 1968), 681-712.

5. F. Machlup, "Theories of the Firm: Marginalist, Behavioral, Managerial", American Economic Review, LVII, No. 1, (March, 1967), 1-33.

6. G. Myrdal, Asian Drama, 3 vols., (New York, 1968).

7. H. Simon, "New Developments in the Theory of the Firm",

American Economic Review: Papers and Proceedings, LII, No. 2, (May, 1962), 1-15.

8. R. Wolff, "Advocacy Economics", Conference Papers of the Union for Radical Political Economics, (Ann Arbor, 1968), 178-181.

9. M. Zweig, "New Left Critique of Economics", Conference Papers of the Union for Radical Political Economics, (Ann Arbor, 1968), 46-52.