# A NOTE ON METHODOLOGICAL PARALLELS BETWEEN ACCOUNTING AND ECONOMICS

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# A NOTE ON METHODOLOGICAL PARALLELS BETWEEN ACCOUNTING AND ECONOMICS

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Scholarly interest in the metatheoretical views advanced by Watts and Zimmerman (1978; 1979; 1986) in their theory of positive accounting has resulted in a lively and ongoing debate among theoretical accountants on the nature and importance of methodology in accounting research. A central theme in the arguments put forward by Watts and Zimmerman (1986, 2) resides in the adoption of a specific and controversial notion of the role of theory in accounting science:

"The objective of accounting theory is to explain and predict accounting practice... *Explanation* means providing reasons for observed practice... [Whereas] prediction of accounting practice means that the theory predicts unobserved accounting phenomena...", (original emphasis).

This view of the purpose of theory is drawn directly from the allied discipline of economics, and more particularly from Friedman's (1953) instrumentalist arguments surrounding the appropriate methodological status of economic theory (Watts and Zimmerman 1986, 8). The explicit acknowledgement of the centrality of instrumentalism to positive accounting theory has stimulated some commentators to draw direct comparisons between Watts and Zimmerman, or what some have termed the "Rochester school of accounting" (Jensen 1976, 11), and the Chicago school of economics (Sterling 1990, 101-106).

This paper seeks to extend the comparison advanced by Sterling (1990) further. In essence, we argue that the entire methodological debate initiated by Watts and Zimmerman mirrors a similar debate conducted in economics. Moreover, we postulate that the evolution of the debate among accounting theorists is likely to parallel that followed in economics to a significant degree. Section 1 attempts to identify briefly some of the more important similarities in the respective methodological debates in accounting and economics in order to establish a historical pattern. On the basis of this purported

sequential parallel between methodological argumentation in accounting and economics, section II seeks to examine some of the implications for accounting practice likely to follow the adoption of an accounting variant of the economic "rhetoric of inquiry" position advanced by McCloskey (1983; 1986; 1988). The paper ends with a short conclusion.

### I. METHODOLOGICAL DEBATE IN ACCOUNTING AND ECONOMICS

Gerard (1990) has provided a useful taxonomy of the major methodological perspectives that have been deployed in the ongoing methodological debate among economists. It should be emphasized that this taxonomy includes only stylized versions of those approaches broadly advocated as descriptive or prescriptive of actual scientific practice in mainstream or neoclassical economics (Caldwell 1984).<sup>1</sup> Nevertheless, it appears sufficiently comprehensive for our present purposes.

Gerrard (1990) has identified six main methodological approaches to orthodox economic research. Firstly, a priorism or deductivism that centers around the notion that "...economic science is a system of *a priori* truths, a product of pure reason, an exact science reaching laws as universal as those of mathematics, a purely axiomatic discipline, a system of pure deductions from a series of postulates, not open to any verification or refutation on the ground of experience" (Machlup 1955, 5). This position has generally been advanced as underlying the Austrian school of economics, which has eschewed econometric and other statistical techniques for evaluating theoretical hypotheses. A tentative parallel appears to exist in the debate in accounting on appropriate methodology. Dollery and Jackson (1990), for instance, have argued for the adoption of

<sup>&</sup>lt;sup>1</sup> It excludes, for instance, both holism and dialectical materialism, which provide methodological vehicles for institutionalism and marxism respectively.

methodological dualism and the consequent categorization of accounting research as part of the "sciences of human action" as envisaged by von Mises (1949, 9).

Secondly, Gerrard (1990, 200) classifies empiricism as an important methodological position in economics, where "empiricism is used...to embrace methodologies in which empirical testing is seen to have a role in determining the truth-status of theories". Empiricisim in this broad sense subsumes most forms of subjecting theoretical propositions to some kind of evidential testing, and includes (radical, dogmatic, and pragmatic) falsificationism (Popper 1959) and confirmationism, although not instrumentalism (Gerrard 1990, 202). The extensive debate among economists on the desirability and applicability of empiricism generally, and falsificationism specifically (Blaug, 1980), is clearly mirrored in a similar discussion among scholars of accounting. Christenson (1983), for example, has argued forcefully for adherence to Popperian falsificationism in the evaluation of theories of accounting, and has been attacked by Hines (1988).

The third methodological school identified by Gerrard (1990, 203) consists of instrumentalism. Instrumentalism in economics is closely associated with Friedman (1953, 7/8), who argued that "the ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed", and "...theory is to be judged by its predictive power for the class of phenomena which it is intended to 'explain'". According to this instrumentalist view of the nature of "scientific" theory, it is evident that the predictive capacity of theory forms the major criterion for acceptance or rejection of theory, rather than its "explanatory" power or the "realism" of its assumptions. The resultant debate surrounding the validity of an instrumentalist approach to economic theory has engendered a substantial literature and continues to the present (Caldwell

1984). As we pointed out earlier, Watts and Zimmerman's (1978; 1979; 1986) positive accounting theory explicity and self-consciously adopts instrumentalism as the methodological vehicle for adjudging accounting practice and theory. Moreover, and in common with economics, this ignited a vigorous debate on methodology in accounting that remains unsettled. The striking nature of the parallel between accounting and economics in this particular regard is noteworthy. Indeed, the famous interchange on the "F-Twist" (F for Friedman) and "S-Twist" (S for Samuelson) (Blaug, 1980) is reflected closely in Sterling's (1990, 105) characterization of his own position as "DP" (or non-Chicago school) and that of Watts and Zimmerman as "TP" (or Chicago school)!

A fourth view of methodology in economics revolves around what Gerrard (1990, 205) terms "economics as structure," by which he means metatheoretical descriptive explanations of the hierarchical structure of economics and changes to this structure. Possibly the two most important theories contained in this category are Kuhn's (1970) methodology of scientific revolutions, which centers on the idea of constellations of thought or paradigms within which normal science pursues puzzle-solving activities until anomalies arise generating a crisis which foreshadows paradigm switching, and Lakatos' (1978) methodology of scientific research programs, which holds that the structure of science consists in a hard core of basic assumptions surrounded by a protective belt of auxiliary hypotheses open to empirical testing. A good deal of debate has occurred among economists on the ability of these metatheories to explain the development of economic theory satisfactorily (Caldwell 1984). Once again, it is possible to identify a clear parallel with accounting. Mouck (1990), for instance, in a pathbreaking paper has attempted to demonstrate the advantages of a Lakatosian approach over its Popperian rival. Mouck (1990, 238) concludes thus:

"Lakatos' methodology of scientific research programs is superior to Popper's falsificationist methodology not only because it is descriptively

accurate regarding the historical practice of science, but also because it embodies a higher level of methodological tolerance. Positive accounting theory can be shown to be a 'progressive' (e.g. 'scientific') research program under Lakatos' methodology even though it must be judged 'unscientific' under the criteria of Popper's falsificationist methodology."

Given the controversial nature of this conclusion, it would not appear unwarranted to anticipate further discussion of Mouck's (1990) argument by accounting scholars.

In the fifth place, McCloskey (1983; 1986; 1988) has advanced the idea that economic discourse is essentially couched in terms of dimly understood persuasive metaphor that may best be unveiled by the application of the tools of literary criticism. Gerrard (1990, 208) notes that the "...basic proposition [of this rhetorical approach] is that economics is a social activity in which practitioners seek to persuade an audience by means of rhetorical devices." An important implication of this rhetorical approach to the question of methodology resides in the argument that economists should not seek guidelines from the philosophy of science or even other disciplines but rather continue "conversation" with each other in accordance with implicit rules of discourse already developed within economic discourse, but not yet understood. McCloskey (1983, 515) argues as follows:

"A rhetorical cure for such disabilities would reject philosophy as a guide to science, or would reject at least a philosophy that pretended to legislate the knowable. The cure would not throw away the illuminating implication unexpected crucial experiment, the regression, the These too persuade reasonable scholars. Nonunexpectedly falsified. argument is the necessary alternative to narrow argument only if one accepts the dichotomies of modernism. The cure would merely recognize the good health of economics, disguised now under the neurotic inhibitions of an artificial methodology of Science."

Although as yet we are unaware of the application of the rhetorical approach to accounting research, if earlier parallels between methodological debate in accounting and economics continue, then it may be anticipated with some

confidence that accounting theorists will examine accounting research in a rhetorical light.

Finally, and of much less interest in the present context, Gerrard (1990, 212) identifies a broad sociological approach to methodology, and emphasizes that "the notion that the acquisition of knowledge, in common with other forms of human behavior, is socially and historically contingent is a longstanding one, deriving much of its impetus from the writings of the German historical school." It is evident that this view of the evolution of economics virtually ignores cognitive elements by emphasizing its ideological and social dimensions.

## II. THE RHETORIC OF INQUIRY AS METHODOLOGY

McCloskey (1986) argues that economists presently approach economic practice and theory from an ambiguous and dischotomised methodological perspective. More specifically, a distinction may be drawn between an "official" or "explicit" approach, where the economics profession believes that "good" theory is developed and employed in accordance with methodological principles derived from best practice in the history and philosophy of science, and an "unofficial" or "implicit" approach that eschews the application of rules in favor of pragmatism in economic research. In sum, although economists as a whole acknowledge and endorse the governance of prescriptive methodological rules in the development of their discipline, actual practice in economics belies adherence to any strict methodological code. McCloskey (1986) characterizes the "official" or "explicit" methodological viewpoint in economics as "modernist," by which he means broadly positivist. Some idea of the flavor attached to the term "modernism" is provided by McCloskey (1986, 5):

"Modernism gleams diamond-hard from many facets, and the word can be fully defined only in use. But in a preliminary way it can be said to be, as the literary critic Wayne Booth has put it, the notion that we only know what we cannot doubt and cannot really know what we can merely assent to. It is the attitude that the only real knowledge is, in common parlance, 'scientific,' that is, knowledge tested by certain kinds of rigorous

skepticism. Philosophically speaking, modernism is the program of Descartes, regnant in philosophy since the seventeenth century, to build knowledge on a foundation of radical doubt."

McCloskey argues further that modernism in economics is best exemplified by Friedman's (1953) famous statement of instrumentalism, which formed the cornerstone not only of the Chicago school of economics but also, as we have seen, Watts and Zimmerman's (1978; 1979; 1986) theory of positive accounting or the Rochester school of accounting. In this regard, McCloskey (1986, 9) notes that "Friedman's essay is the central document of modernism in economics and deserves respectful review."

McCloskey (1986) presents a strong case against modernism in economics and elsewhere, including presumably accounting. Firstly, he argues that modernism is now discredited in philosophy, since "...philosophical arguments for it have long been known to be unpersuasive" (McCloskey, 1986, 12). Secondly, falsificationism, as an important plank of "officially" sanctioned modernist methodology, is not only untenable in economics but excludes a range of useful techniques (including simulation) employed as a matter of routine research practice. McCloskey (1986, 14) puts the argument in terms of simulation exercises:

"This is why simulation - trying out scientific arguments on paper to see if they are powerful enough - is important in economics and similar fields. Simulation is affirmative, not falsifying, asking whether one can make a case for such-and-such, not whether one can prove it wrong. It tests systems, not isolated hypotheses, and affirms a framework in which to test them. It tests the reasonableness of affirmation, not the possibility of doubt."

Thirdly, prediction as a purportedly central characteristic of modernist economics (Friedman 1953) is simply not possible. McCloskey (1986) argues that strict adherence to modernist methodology not only cannot occur in economics, but hampers the development of economic theory. His argument runs thus (McCloskey, 1986, 16):

"Modernism promises knowledge free from doubt, free from metaphysics, morals, and personal conviction. What it is able to deliver renames as scientific methodology the scientist's, and especially the economic scientists', metaphysics, morals, and personal convictions. It cannot deliver what it promises. Probably it should not. One suspects, as have many who have thought about the matter in recent years, that scientific knowledge is not so very different from other knowledge."

Fourthly, and most importantly, the strongest objection raised against modernism in economics lies in the idea that, as with all other *prescriptive* methodological positions, it confines and restricts the acquisition of new knowledge by laying down rules today for unforeseen and unforeseeable future events. Indeed, McCloskey (1986) argues that it is fortunate that actual "unofficial" research practice in economics has not followed these "official" modernist methodological restrictions on research practice. The essence of McCloskey's (1983, 489) argument against modernism or any other prescriptive methodology in economics is as follows:

"The greater objection is simply that modernism is a method. It sets up laws of argument drawn from an ideal science or the underlying history of science or the essence of knowledge. The claim is that the philosopher of science can tell what makes for good, useful, fruitful, progressive science. He knows this so confidently that he can limit arguments that worthy scientists make spontaneously, casting out some as unscientific, or at best placing them firmly in the 'context of discovery'. The philosopher undertakes to second-guess the scientific community. In economics the claim of methodological legislation is that the legislator is not merely expert in all branches of economic knowledge within sound of his proclamations but expert in all possible future economics, limiting the growth of economics now in order to make if fit a philosopher's idea of the ultimate good."

Given his persuasive case against modernism and other prescriptive methodologies in economics and allied disciplines, the rhetoric of inquiry modus operandi advanced by McCloskey (1983; 1986; 1988) is open to charges of methodological "anarchy" along the lines of criticism directed at Feyerabend (1975). But this misses the point. Surely the rhetoric of inquiry merely legitimizes existing actual practice in economics, and adds the analytical tools

of literary criticism as a means of engendering an understanding of the nature of actual or "unofficial" research practice.

Indeed, had the criteria of modernism always been applied to economic theory, the outcome would have been nothing short of disastrous. The Keynesian revolution in economic thought provides a leading example. It is well documented that most key propositions in Keynesian economics were only subjected to statistical analysis in the 1950's, or some fifteen years after they had been accepted by a large number of economists. Moreover, despite repeated failure in econometric testing the Keynesian notions of the accelerator and liquidity traps were fully incorporated into conventional macroeconomics by the early 1960's. Similarly, it can hardly be argued that cliometrics, mathematical general equilibrium theory, the new theory of the firm, social choice theory, and many other recent advances in economics have arisen through empirical anomaly.

#### III. CONCLUDING REMARKS

We have attempted to demonstrate a parallel in the progression of methodological debate in accounting and economics. If this parallel continues, then the analogue of modernism in accounting represented by the theory of positive accounting advanced by Watts and Zimmerman (1978; 1979; 1986) is likely to be challenged by an accounting variant of the rhetoric of inquiry. The removal of authoritarian methodological prescriptions from accounting researchers may well serve the same function as in economics; that is, legitimize actual existing practice at the cost of demythologizing idealized "official" practice. Where research into accounting practice does not lend itself to the "official" or "modernist" viewpoint, we could see the removal of the word "mere" from the phrase "mere descriptive research."

#### REFERENCES

- Blaug, M. (1980), <u>The Methodology of Economics: or, How Economists Explain</u>, Cambridge: Cambridge University Press.
- Caldwell, B. J. (ed.) (1984), <u>Appraisal and Criticism in Economics</u>, Boston: Allen and Unwin.
- Christenson, C. (1983), "The Methodology of Positive Accounting," <u>Accounting</u> <u>Review</u>, January, pp. 1-22.
- Dollery, B. E. and Jackson, C. W. (1990), "A Critical Comment on the Theory of Positive Accounting," unpublished mimeo, School of Accounting, University of Southern California.
- Feyerabend, P. (1975), <u>Against Method: Outline of an Anarchistic Theory of</u> Knowledge, London: Verso.
- Friedman, M. (1953), "The Methodology of Positive Economics," in M. Friedman, <u>Essays in Positive Economics</u>, Chicago: University of Chicago Press.
- Gerrard, B. (1990), "On Matters Methodological in Economics," <u>Journal of</u> <u>Economics Surveys</u>, Vol. 4(2), pp. 197-219.
- Jensen, M. C. (1976), "Reflections on the State of Accounting Research and the Regulation of Accounting," <u>Stanford Lectures in Accounting 1976</u>, Graduate School of Business, Stanford University.
- Hines, R. D. (1988), "Popper's Methodology of Falsificationism and Accounting Research," <u>Accounting Review</u>, October, pp. 657-662.
- Klamer, A., McCloskey, D. N. and Solow, R. M. (eds.) (1988), <u>The Consequence of</u> <u>Economic Rhetoric</u>, Cambridge: Cambridge University Press.
- Kuhn, T. (1970), <u>The Structure of Scientific Revolutions</u>, Chicago: University of Chicago Press.
- Lakatos, I. (1978), <u>The Methodology of Scientific Research Programs</u>, Cambridge: Cambridge University Press.
- Machlup, F. (1955), "The Problem of Verification in Economics," <u>Southern</u> <u>Economic Journal</u>, Vol. 22(1), pp. 1-21.
- McCloskey, D. M. (1983), "The Rhetoric of Economics," <u>Journal of Economic</u> <u>Literature</u>, Vol. 21(2), pp. 481-517.
- (1985), <u>The Rhetoric of Economics</u>, Madison: University of Wisconsin Press.
- Mouck, T. (1990), "Positive Accounting Theory as a Lakatosian Research Programme," <u>Accounting and Business Research</u>, Vol. 20(79), pp. 231-239.

Popper, K. (1959), The Logic of Scientific Discovery, London: Harper and Row.

Sterling, R. R. (1990, "Positive Accounting: An Assessment," <u>Abacus</u>, Vol. 26(3), pp. 97-135.

Von Mises, L. (1990), <u>Human Action</u>, New Haven: Yale University Press.

Watts, R. L. and Zimmerman, J. L. (1978), "Toward a Positive Theory of the Determination of Accounting Standards," <u>Accounting Review</u>, January, pp. 112-134.

(1979), "The Demand for and Supply of Accounting Theories: The Market for Excuses," <u>Accounting Review</u>, April, pp. 273-305.

\_\_\_\_\_ (1986), <u>Positive Accounting Theory</u>, New York: Prentice Hall.