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THE IRONY OF "THE GOLDEN AGE" OF ACCOUNTING METHODOLOGY

Abstract: Developments in accounting methodology during the 1960s are contrasted with concurrent developments in philosophy of science. The 1960s was a decade characterized by the widespread adoption of "the scientific method" in accounting methodology. The same decade was characterized by the degeneration of any semblance of consensus among philosophers of science regarding the nature of scientific inquiry. The irony of these incongruous but simultaneous developments is highlighted with the intent of weakening the current atmosphere of uncritical reverence for science and "the scientific method" in accounting research. A more contemporary (and more open) view of science — the postempiricist view — also is discussed.

Ruth Hines recently has expressed concern about dogmatic tendencies in accounting research; tendencies which are linked to "an unwarranted reverence for science and 'the scientific method'" [1988, pp. 660-61]. Reverence for "the scientific method" can be traced to developments in the 1960s — the decade that has been dubbed "The Golden Age" of accounting methodology [Graffikin, 1988]. The 1960s also has been referred to as the "Decade of Awakening" [Dyckman and Zeff, 1984, p. 233] — the decade during which accounting researchers awoke to the scientific method. This is highly ironic because the 1960s was the decade that saw the deterioration of any semblance of consensus among philosophers of science. Just as accounting researchers were discovering "*the* scientific method" philosophers of science were witnessing its disintegration. The aim of this article is to undermine the atmosphere of dogmatism noted by Hines by highlighting the irony of the Golden Age of accounting methodology and calling attention to a more contemporary (more open) view of science that has been referred to as the postempiricist view.

THE 1960s: A DECADE OF AWAKENING FOR ACCOUNTING METHODOLOGY

There is no question that the 1960s represent a watershed in accounting research. Gaffikin [1987] has argued that, with few

exceptions, pre-1960 accounting research was philosophically and methodologically unsophisticated. Researchers provided descriptive catalogues of existing practices and attempted to uncover rules for improving accounting practices.

Post-1960s accounting research is a radically different story. The mainstream journals reflect an increasing obsession with empirical research that is presumed to be in accordance with the principles of scientific inquiry. The journals are loaded with mathematical model-building, hypotheses testing, esoteric statistical techniques, and so forth. And even though the articles reflect considerable theoretical diversity, they suggest widespread agreement regarding scientific methodology. In fact, Chua [1986] has argued that

> . . . accounting research has been guided by a dominant . . . set of assumptions. There has been one general scientific world-view, one primary discipline matrix. And accounting researchers, as a community of scientists, have shared and continue to share a constellation of beliefs, values, and techniques. These beliefs circumscribe definitions of "worthwhile problems" and "acceptable scientific evidence." [p. 602]

The "scientific" world-view of mainstream accounting researchers, according to Chua, is grounded in a belief that "reality" exists independently of the human subject. Theories, in the mainstream view, "are put forward as attempts to discover a knowable, objective reality" [Chua, 1986, p. 606]. And since objective reality is taken to be separate from theoretical constructs, "Accounting researchers believe in the empirical testability of scientific theories" [Chua, 1986, p. 607].

This dominant "scientific" world-view has its roots in the literature of the 1960s. These roots have recently been traced by Gaffikin in his 1988 article, "Legacy of the Golden Age". He argues that, "despite the different research methods employed, because the ontological and epistemological presuppositions are the same, the methodological underpinnings have remained fairly constant" [1988, p. 16]. And he maintains that these methodological underpinnings are primarily due to the influence of four researchers — Chambers, Mattessich, Devine and Sterling.

These writers were well versed in philosophy of science and were anxious to extend the scientific method to accounting thought. Chambers, for instance, writes in a "Working Paper" for The Academy of Accounting Historians:

> By 1954 I believed it necessary, at least for myself, to set down the way in which a theory of accounting should be developed. In none of the important works on accounting was there a treatment of methodology. There was no pattern to follow except that of the well-developed sciences. And writers on accounting were following no pattern. My principal formal guides were Cohen and Nagel's *An Introduction to Logic and Scientific Method,* Larrabee's *Reliable Knowledge* and Robbins' *The Nature and Significance of Economic Science*. . . . I wrote "Blueprint for a Theory of Accounting" in 1955 and two other pieces shortly afterward in response to some criticism. I returned to the matter in the early sixties because no material change had occurred in the way in which accountants dealt with the construction and validation of their ideas [p. 8].

Devine emphasizes, "that measurement is a process that requires extremely high levels of abstraction" [1966, p. 14]. And he references Milton Friedman's "Essay on the Methodology of Positive Economics" in his discussion of the appraisal of abstractions. "The prospects for appraising such abstractions by the 'realism' of their components instead of by the relationship of their output to goals and need is dim indeed (See Friedman [7])" [Devine, 1966, p. 14]. This is apparently a reference to Friedman's notorious claim about the irrelevance of the realism of assumptions and his emphasis on predictive capability. In the same article Devine claims that, "The common core of scientific methods is the interworking of observation and deduction, and it should be clear that one can construct a predictive social theory only in conjunction with empirical and behavioral assumptions" [p. 26].

Mattessich, in his 1964 book *Accounting and Analytical Methods,* criticizes the current state of both accounting theory and accounting pedagogy. "Accounting theory," he says, "has developed a body of knowledge which is of a dogmatic rather than scientific-hypothetical character and which serves with satisfaction only purposes of a legalistic nature" [p. 4]. And he chastises academic accounting for its over-emphasis on technical aspects of existing practice. "It leaves the student at a loss when it comes to expressing accounting theory in terms of modern logic, epistemology, and quantitative analysis" [Mattessich, p. 4]. "The accountant's dilemma," Mattessich suggests, "is not merely a problem of memorizing some formulas or learning new mathematical tricks, it is a problem of transition

from the dogmatic thinking of the jurisprudent to the behavioral-analytical thinking of the scientist" [p. x].

Finally, Gaffikin cites Sterling's *Theory of the Measurement of Enterprise Income* as one of the major works of the decade, noting that although it was not published until 1970 it was written ten years previously and portions of it were reflected in Sterling's other published works in the 1960s. Gaffikin also notes that Sterling's views on accounting theory are very similar to those of Chambers. Both Chambers and Sterling advocate the adoption of accounting based on exit-prices because these are empirically observable data.

Graffikin emphasizes the methodological similarities among Chambers, Devine, Mattessich and Sterling, but there are also significant differences; most notably with respect to Devine's pragmatic orientation. Compared with Sterling, for instance, Devine is much more circumspect regarding the nature of facts, truth, and the potential for scientific accounting. For Sterling, "Scientific knowledge is intended to refer to real things in the real world" [1976, p. 83], and competing hypotheses are empirically tested to find out "which is most in harmony with the facts of observation" [1976, p. 83]. An "empirical test," for Sterling, "simply means that one looks at real things in the real world to find out what is true" [1976, p. 83]. Accordingly, he maintains that accounting is in need of a redefinition: "we must define it [accounting] as a process of keeping track of real things in the real world" [1976, p. 85]. His candidate for an empirical base for accounting is, of course, ext values — "they are useful to a great many decisions . . . [and] they are subject to empirical test, we will be able to resolve disputes about them" [1976, p. 87].

Devine, on the other hand, maintains that "the facts of a case are determined by objectives" [1985, Vol. V, p. 57]. And as Arrington points out, "for Devine and the pragmatists, [truth is] something that a community finds useful to believe, and useful for definite assignable reasons that have to do with ways in which problems can be solved and life can be changed" [p. 139]. Also, for Devine and the pragmatists, if seems doubtful that science can find "any universal principles that are 'basic' to all cultures for eternity" [1985, Vol. III, p. 14]. And it is understandable that Devine would seem to suggest, in the words of one reviewer, "that it will not be possible to develop a global set of accounting principles" [Anton, et. al., p. 413].

Neither is it possible, from Devine's perspective, for accountants (or even scientists) to eliminate values from their work:

"everything of consequence done by accountants has ethical content in the sense that their decisions help or harm various individuals" [1985, Vol. V, p. 5]. And in response to Chambers' claim that, "Our inquiry, like that of economics, 'is entirely neutral between ends' . . . " [quoted by Devine, 1985, Vol. III, p. 39], Devine responds as follows: "Advocacy of neutrality is . . . insidious. It is an offspring of the discredited doctrine of observing *the* (!) facts. . . . Facts are interpretations relevant to a viewpoint" [1985, Vol. III, p. 40].

But regardless of differences, Chambers, Devine, Mattessich and Sterling were major influences on the new directions taken in accounting methodology and research. Their scientifically-oriented works on accounting theory, however, were not the only influences that made the 1960s a watershed decade for accounting research. As Whitley [1986 & 1988] has pointed out, social, political and economic events as well as institutional developments all played a role in transforming academic accounting research, and helped pave the way for the increasingly dominant emphasis on quantitative ("scientific") accounting research. Following the Soviet Union's successful launching of Sputknik, the U.S. in the late fifties and early sixties was pervaded by a sense of urgency to expand scientific and mathematical training. And following the successful employment of scientific research and operations research methods in World War II, there was widespread belief "that 'science' could be applied to managerial and business problems and scientific research into these problems should be supported" [Whitley, 1986, p. 171]. More concisely, the Ford and Carnegie Foundations had both published reports in 1959 encouraging "an expansion of 'scientific' research in U.S. business schools"; reports which were subsequently backed up by "substantial grants and publishing opportunities" [Whitley, 1988, p. 641]. In short, the stage was set for the emergence of a community of accounting researchers who shared a commitment to empirical ("scientific") research.

Supportive technology also was available. The 1960s was a decade of rapidly expanding computer availability and computer-generated data; developments which greatly extended the possibilities for statistical work. And other developments of the 1960s, most notably the emergence of the efficient markets literature in economics and the capital asset pricing model in finance, further accelerated the pace of empirical research in accounting [Dyckman and Zeff, p. 236].

On all fronts the 1960s was, in the words of Dyckman and Zeff, "a pivotal decade" for accounting research [p. 236]: "In the literature of accounting research, the 1960s was the Decade of Awakening" [p. 233]. The American Accounting Association initiated a series of Studies in Accounting Research. Stanford University, the University of Chicago, and the University of Kansas initiated conferences and symposia focusing on empirical research and methodology. And in 1966 the *Journal of Accounting Research* began publishing "a series of annual Supplements that were devoted almost exclusively to the empirical research papers presented at Chicago's Conference on Empirical Research in Accounting" [Dyckman and Zeff, p. 269].

It is ironic, however, that accounting researchers were awakening to the scientific method of inquiry just as events in philosophy of science were raising doubts about the validity of any exclusive approach to inquiry. In 1965, the same year that saw "the first university-sponsored conference dedicated wholly to accounting research" [Dyckman and Zeff, p. 234], an international symposium on philosophy of science was held in London to explore the challenges presented by Thomas Kuhn's *The Structure of Scientific Revolutions* (originally published in 1962). Papers presented by some of the worlds leading philosophers of science were later published in a volume entitled *Criticism and the Growth of Knowledge*. Philosophy of science has not been the same since. Indeed, as economic methodologist Douglas Hands recently pointed out, philosophy of science "has undergone a major upheaval during the last twenty years. The so-called 'received view' of the preceding epoch is dead" [Hands, 1984, p. 116].

THE 1960S: A DECADE OF TURMOIL IN PHILOSOPHY OF SCIENCE

Modern Western society has tended toward the notion that the only valid truth claims are those resulting from the scientific process. And until the last twenty-five years or so, philosophers of science considered it their duty to provide prescriptions for scientific practice, and to provide philosophical explanations for why the truth claims of science are epistemologically valid. The following is a very brief sketch of the dominant view in pre-1960s philosophy of science. Based on empirical observations, scientists formulate general laws via a process of induction. The general laws must satisfy both logical and empirical conditions of adequacy. They must be logically necessary for deduction of the initially observed data, and they must be capable of empiri-

cal testing. The adequacy (or truth) of such laws is judged on the basis of their ability to predict the phenomena under consideration.

Sir Karl Popper, in his classic work, *The Logic of Scientific Discovery* (originally published in 1934), rejected this view of scientific method because of its reliance on induction. A logical deduction is complete in and of itself. An inductive inference, however, can never be complete in and of itself because it has to be based on limited experience and future experience may (in a logical sense) contradict any inductive inference. It is thus impossible to conclusively prove the truth of any theory. Popper therefore turned to falsification as a basis for philosophy of science with the idea that, if theories are repeatedly subjected to attempts at falsification, then scientific knowledge can, at least, grow ever closer to the truth as false theories are rejected.

Popper's falsificationism has been very influential in that it is often cited as *the* legitimate basis for scientific methodology. Unfortunately Popper also has been widely misunderstood. Naive empiricists (including mainstream accounting researchers) have assumed Popper to be arguing as follows: whereas no amount of confirmatory empirical evidence can conclusively prove the truth of a theory, it can be conclusively disproven by contradictory empirical evidence. But Popper expressly denied any such claim. "In point of fact, no conclusive disproof of a theory can ever be produced. . . . If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are" [Popper, 1968, p. 50].

What can be established with logical conclusiveness is the consistency or inconsistency of a set of propositions. Thus, for an empirical science the relevant propositions can be sub-divided into theoretical propositions and observational propositions in such a way that their logical consistency or inconsistency can be readily determined. In Popper's falsificationist philosophy of science, a theory is considered to be "falsified" when a contradictory observation statement is accepted [1968, p. 86].

An initial problem for this sort of falsificationism, if it claims to be both logical and empirical is that it is not possible to deduce observational propositions from pure experience. Any observational proposition, Popper points out,

> . . .[goes] far beyond what can be known with certainty 'on the basis of immediate experience'. . . . Every description uses universal names (or symbols, or ideas); every statement has the character of a

> theory, of a hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the universals which appear in it cannot be correlated with any specific sense-experience. (An 'immediate experience' is only once 'immediately given'; it is unique.) By the word 'glass', for example, we denote physical bodies which exhibit a certain law-like behavior, and the same holds for the word 'water'. Universals cannot be reduced to classes of experiences; they cannot be 'constituted' [1968, pp. 94-95].

Furthermore, any proposition which reports sensory experience must rely on some theory of perception, and of course no theory of perception can ever be conclusively proven true because of the problem of induction. And any observations that rely on instruments (microscopes or telescopes, for instance) rely on additional theories (a theory of optics). In short, there is no realm of non-theoretical facts against which theories can be tested.

Popper's solution to these problems (and others) is to take a methodological decision; a decision to regard the supporting theories as "unproblematic background knowledge" — "Let h be the hypothesis to be tested; let e be the test statement (the evidence), and b the 'background knowledge', that is to say, all those things which we accept (tentatively) as unproblematic while we are testing the theory" [Popper, 1965, p. 390].

With due regard to the extent of the qualifications and methodological decisions entailed, Popper's falsificationism can be summarized as follows:

> According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival [Popper, 1968, p. 42].

Popper's falsificationist philosophy of science is significantly different from its predecessors which are often characterized as verificationist philosophy of science. Both, however, are prescriptivist philosophies of science which perpetuate the notion that scientific truth claims are epistemologically superior to those of folklore, art, literature, religion, metaphysics, etc. The epistemological virtues of science, according to both verificationists and falsificationists, rely on the notion that science is essentially characterized by empiricism and rationality.

The 1962 publication of Thomas Kuhn's popular and influential *The Structure of Scientific Revolutions* posed a challenge to Popperian falsificationism and kicked off what has come to be known as the 'growth of knowledge' movement in philosophy of science. Kuhn's work seemed to 'pull the rug from under' the claim that science is a rational enterprise. According to Kuhn, some of the most crucial aspects of scientific advance are determined by non-rational factors.

According to Kuhn, most scientific activity is carried out within an accepted theoretical framework which has been built upon past scientific achievements. The accepted theoretical framework reflects certain beliefs about the world, and it serves as a foundation for the articulation of problems that must be solved if the range of explanatory power is to be extended. Furthermore, even the methods of research that were used in the foundational achievements tend to be accepted as the legitimate methods, and thus perpetuated. All of this adds up to what Kuhn characterizes as paradigm-based research. The term "paradigm", in the broad sense, "stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community" [Kuhn, p. 175]. Paradigm-based research is what Kuhn refers to as "normal science". It is research aimed at the fleshing out and extension of the already accepted theoretical framework. Contradictory theories and viewpoints tend to be suppressed by the established scientific community. Kuhn claims that the research problems pursued tend to be those seen as holding the most promise for such fleshing-out and extension, and has likened the process to puzzle-solving.

In the process of carrying out normal science, scientists inevitably encounter discrepancies between the theoretical structure and nature. Such discrepancies can generate a crisis and spawn competing paradigms which challenge the dominant paradigm. Such crises and how they are resolved probably represent the most controversial aspect of Kuhn's ideas. They also represent the most fundamental challenge to Popper's concern with the rational growth of scientific knowledge. While Popper agrees that such periods exist and are, in fact, essential to scientific progress, he believes that scientists can and do rationally evaluate alternative paradigms. Kuhn, on the other hand, sees such choices as essentially extra-rational or, at least, strongly influenced by non-rational factors. He deliberately uses "revolution" as a metaphor because of parallels he sees between political and scientific change. He classifies as scientific revolutions, "those non-cumulative developmental episodes in which

an older paradigm is replaced in whole or in part by an incompatible new one" [Kuhn, p. 92].

Kuhn maintains that scientific revolutions are important stages in the growth of scientific knowledge that fall outside the logically controlled processes of normal science. He refers to a decision to adopt a new paradigm as a conversion experience that cannot be forced by logic. Since the conversion results in (or from?) a new way of seeing the world, Kuhn likens the process to a visual "gestalt" switch, or to a man who has put on inverting lenses. "Confronting the same constellation of objects as before and knowing that he does so, he nevertheless finds them transformed through and through in many of their details" [Kuhn, p. 122].

These are the kinds of arguments that have produced charges that Kuhn's ideas lead straight to relativism and irrationality. Kuhn also has been criticized for turning to social psychology for enlightenment regarding scientific method. In an attempt to maintain Kuhn's descriptive accuracy without resorting to social psychology, the late philosopher Imre Lakatos developed the "methodology of scientific research programmes" (MSRP); a descriptive philosophy of science which does not undermine the notion that science is thoroughly rational.

According to Lakatos, most of the significant series of theories in the growth of scientific knowledge are welded together into research programmes by a certain continuity of conceptual framework. Scientists working within a programme, tend to work as if they had agreed at an early stage on a set of methodological rules. The most basic "agreement" concerns the conceptual framework that will not be subject to rejection. Lakatos characterizes this as the "hard core" of the programme. Scientists working within the programme then use their "ingenuity to articulate or even invent 'auxiliary hypotheses,' which form a protective belt around this core. . . . It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core" [Lakatos, p. 133].

Lakatos downplays the instances of widespread abandonment of one research programme in favor of another, the sort of situation Kuhn describes as a religious sort of conversion. Lakatos claims that there can be objective reasons for rejecting one programme for another: "such an objective reason is provided by a rival research programme which explains the previous success of its rival and supersedes it by a further display of heuristic power" [Lakatos, p. 155].

Another important voice in the philosophy of science debate is that of Paul Feyerabend. Feyerabend, a self-declared anarchist, has laid out an outline of an anarchistic theory of knowledge in his celebrated work *Against Method.* He claims "that there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: anything goes" [Feyerabend, p. 28]. There are always, he claims, circumstances in which scientific progress is enhanced by disregarding, or even acting contrary to, any methodological maxim that has ever been developed.

Feyerabend considers Lakatos to have made an ingenious attempt at establishing methodological standards for scientific progress, but in the final analysis he concludes "there is no 'rationally' describable difference between Lakatos and myself . . . " [Feyerabend, pp. 186-187]. He points out that Lakatos' arguments favoring the granting of a "breathing space" for new theories and research programmes removes most of the objections he (Feyerabend) has formerly leveled at attempts to establish methodological standards. The main point on which Feyerabend bases his claim of "no 'rationally' describable difference" is that Lakatos' standards do not contain any rules that tell scientists what to do; nothing is ruled out.

Feyerabend claims that science does not deserve any special consideration or support in a free society. Western rationality itself, which science supposedly epitomizes, is only one tradition among many. It provdies one way of looking at the world, according to Feyerabend. But science, he says, has no legitimate claim to superiority over any other sort of knowledge.

SCIENTISM AND THE "FETISH OF EMPIRICAL RESEARCH": THE REAL LEGACY OF THE GOLDEN AGE?

Given that the 1960s was the decade when doubt and uncertainty about the nature of scientific research dominated discussion in philosophy of science it is indeed highly ironic that this was also the decade that finally brought "*the* scientific method" to accounting research. But the result has been more than ironic. A good argument has been made that the dogmatic tendencies currently being manifested in accounting research have resulted from first equating knowledge with "scientific knowledge," and secondly equating empirical with scientific.

In a recent *Abacus* article "Wisdom or Widgets", Dan Subotnik points out that the high academic esteem for the natural sciences, especially physics, has led to an attempt to

emulate the methods of physics. "When [research] takes place in disciplines outside the natural or physical sciences, but using the same techniques, it is assumed to be an extended application of 'scientific method' " [Subotnik, p. 96]. This, of course, is what Chambers, Mattessich, Devine and Sterling did for accounting — they introduced the techniques of the natural sciences.

I hasten to add that I am not suggesting that Chambers, Mattessich, Devine and Sterling held scientistic or dogmatic views. Far from it. In fact, Gaffikin points out that, "Having made his case for scientific method for research, Devine draws attention to weaknesses in it" [1988, p. 22]. And Sterling has argued eloquently for methodological tolerance [1971, pp. 1-6]. Subsequent researchers, however, have not followed suit; they have tended to accept "*the* scientific method" as an article of faith. The result has been aptly described as follows: "Science has given us a hammer — to borrow from an old adage — now all our problems look like nails" [Subotnik, p. 96].

Subotnik suggests that the appeal of "the scientific method" to the academic masses is largely due to a fear of taking positions that are vulnerable to criticism. The avoidance of vulnerability has become institutionalized in academia, and its perpetuation seems to be assured by the Ph.D. dissertation process. "We have an operating rule in academia, that in writing a dissertation one should continuously narrow one's vision. The common wisdom is that the supreme, if not the only, objective in a dissertation is to make a statement that is unassailable" [Subotnik, p. 104]. And in disciplines which have become enamored of "the scientific method" the avoidance of vulnerability has manifested itself in a penchant for "hard" or empirical research. I would argue further that the penchant for empirical research in accounting has been fed by the ready availability of empirical data in the form of securities prices, in conjunction with the ready availability of theories from economics and finance (the efficient markets hypothesis and the capital assets pricing model) which can be used to relate accounting numbers to securities prices.

In any case, the scientific ideals introduced into accounting by Chambers, Mattessich, Devine and Sterling in the 1960s have been adopted by subsequent academic accountants who are less familiar (or in many cases, totally unfamiliar) with philosophy of science. This has resulted in accounting research that is largely characterized by a scientistic attitude: "On the whole, our working image of science can be reduced to a single narrowly positivistic principle: Truth is to be found only

through application of empirical methods" [Subotnik, p. 97]. Subotnik suggests that we call this "the Principle of Quantitative Unassailability" [p. 97]. The Principle of Quantitative Unassailability, he claims, has tended to erase the distinctions between academic research and factory work.

> As "techniques" are increasingly refined for reducing the "scientific method" to a guarantee of empirical quantifiability, the pursuit of knowledge is turned into the production of research, and knowledge itself becomes a product or commodity. Once the benefits of the Quantitative Unassailability Principle becomes apparent — that is, foreclosure of debate over the importance or the integrity of the argument — it is only a matter of time until, for similar reasons, five articles necessarily become better than three. In other words, researchers over time came to superimpose another quantitative business paradigm upon their work product — factory output [Subotnik, pp. 99-100].

In short, what started as a move toward more scientific accounting research, has largely degenerated to scientism and dogmatism. Many of the most prominent accounting researchers have developed an attitude that theirs is the only legitimate form of research. Watts and Zimmerman provide the most conspicuous case in point. In their 1986 book *Positive Accounting Theory* they pretentiously announce that, "Throughout this book, we use science's concept of theory (positive theory)" [p. 338]. And they denigrate research efforts that fall outside their own variety of economics-based empirical research (what they call positive research). The demand for other types of accounting literature, according to them, can be thought of as "the demand for excuses" [p. 339]. This attitude also has been fostered by editors of some of the leading journals. For instance, it has been reported that Nicolas Dopuch, as long-time editor of the *Journal of Accounting Research*, has commented that "he sought to kill 'the traditional form of normative theorizing' " [Gaffikin, 1988, p. 24]. This sort of reverence for empirical ("scientific") research naturally filters down to hiring practices in academia. Subotnik notes that, "I myself was once told at the outset of a job interview: 'This is a statistically oriented department. We look for people who can complement [or was it "compliment"?] our work' " [p. 102].

These scientistic tendencies in current accounting research are certainly cause for concern. Such concern has been succinctly expressed by Stephen Zeff in his departing *Accounting*

Review editorial [1983]; concern about the consequences of narrowness and overspecialization in accounting research. He suggests that "the 'wave of rigor' that has engulfed the accounting literature since the 1960s has led to a lesser inclination to tackle big questions" [p. 133]. In fact, "it [often] seems that manuscripts are the result of methods in search of questions, rather than questions in search of methods" [p. 134]. More specifically, Zeff is concerned that the over-emphasis on empirical research may eventually result in the complete elimination of historical scholarship in accounting [p. 134].

There is, however, in my opinion, good basis for optimism regarding a reversal of this scientistic trend — there is evidence of a "reawakening" in accounting research. I am referring primarily to the increasing stream of articles on methodology. The last three or four years have witnessed articles tracing the history of methodological perspectives [Gaffikin, 1987, 1988]; articles criticizing the established, or prominent, methodological views [Lehman & Tinker, 1987; Whitley, 1988; Whittington, 1987; Hines, 1988; Subotnik, 1988; Tinker, 1988]; and articles exploring new methodological perspectives [Chua, 1986, 1986a; Cooper & Hopper, 1987; Hopper et. al, 1987; Hopwood, 1987; Laughlin, 1987; Lavorie, 1987; Richardson, 1987; Morgan, 1988; Arrington & Francis, 1989]. This is a very encouraging trend. Methodological debate is the natural enemy of the dogmatic, scientistic attitude. Methodological debate opens up other ways of viewing the world and knowledge of the world.

But in addition to the anti-dogmatic virtues of methodological discussion, it is especially encouraging to note that the discussion is introducing accounting researchers to a radically different view of science than the outmoded positivistic/empiricist conception they have received from mainstream accounting "methodologists." Accounting researchers are being exposed to what Richard Bernstein and other philosophers refer to as "postempiricist philosophy and history of science" [Bernstein, p. 22].

THE POSTEMPIRICIST VIEW OF SCIENCE: THE LEGACY OF THE GROWTH-OF-KNOWLEDGE DEBATE

Bernstein has noted that traditional empiricist philosophy of science assumes such things as the following: experience is objective and testable; the language of science is exact, formalizable, and literal; meanings are separate from facts; etc. [p. 32]. But largely as a result of the "growth of knowledge" debate, the component elements of the traditional empiricist view are

almost universally considered to have been discredited and a new, postempiricist view of science has emerged. From the postempiricist perspective: scientific theories are ways of interpreting nature; facts are, to a significant degree, constituted by theory; the language of science is inescapably metaphorical and inexact; meanings are generated by the community of inquirers and are understood by theoretical coherence rather than by correspondence with facts; and so forth [Bernstein, p. 33]. The most salient feature of postempiricist philosophy and history of science is, according to Bernstein, its "recovery of the hermeneutical dimension of science". The hermeneutical dimension can be explicated very succinctly with reference to the debate over rationality.

Bernstein has noted that three books published within four years of each other posed unique and profound implications regarding the nature of knowledge and rationality. Kuhn's *The Structure of Scientific Revolutions* (1962) was essentially concerned with natural science. It touched off a storm of controversy primarily because it was perceived as calling into question the rationality of science. In claiming that competing paradigms may be incommensurable, that proponents of competing paradigms are functioning in diffrent "worlds", and that the switch from one paradigm to another is comparable to a religious conversion or a gestalt switch, Kuhn was seen as denying the possibility of a philosophy of science which offers explicit and fixed criteria for decisions involving theory choice — "his critics took him to be challenging the very rationality and objectivity of science" [Bernstein, p. 23].

Four years earlier, Peter Winch had published *The Idea of a Social Science and Its Relation to Philosophy* (1958). It also had touched off a storm of controversy. Winch was essentially concerned with an analysis of the fundamental contrast between the natural and the social sciences at a time when, according to Bernstein, "The prevailing attitude . . . among professional social scientists was that their discipline was now on the secure path of becoming a genuine natural science of individuals in society, a natural science that differed in degree and not in kind from the rest of the natural sciences" [pp. 26-27]. But the basic point of congruence in the respective controversies over Winch's book and Kuhn's book concerned the concept of rationality. Winch had implied that different cultures may have incommensurable standards of rationality. And the implication was made more specific in a follow-up essay: "he used the figure of speech of 'our standards' and 'their standards' of rationality when

speaking of modern Western society and the 'primitive' society of the Azande" [Bernstein, pp. 27-28].

Bernstein points out that in both controversies the critics of Kuhn and Winch tended to focus on the problem of coming up with a universal standard of rationality. But this focus, Bernstein maintains, was off the mark. The real issue, he suggests, is more appropriately stated as follows:

> The vital issue here is really the question of what is involved in understanding, interpreting, and explaining alien societies (and not just their rationality or lack of rationality). How are we to do justice to the strangeness that we discover when we encounter alien types of activities, beliefs, rituals, institutions, and practices, without falsifying or distorting them? [p. 28].

And this is where Hans-Georg Gadamer's *Truth and Method* (originally published in German in 1960) enters the picture. Gadamer is the central figure in the contemporary hermeneutics movement, and hermeneutics is specifically concerned with the processes of interpretation and understanding.

Traditional hermeneutics focuses on the processes of interpreting and understanding texts from a different time, language, or culture. But contemporary philosophical hermeneutics as developed by Gadamer claims a much more universal applicability. Gadamer claims that all life experiences involve the processes of interpretation and understanding. And the relevance of Gadamer in the present discussion is that he denies the possibility of an objective rationality that is free of historical and cultural context, and offers a different, but non-relativistic, notion of reason and rationality.

Understanding, according to Gadamer, grows out of "experience" and always involves a "fusion of horizons". An "experience", as Gadamer uses the term, results from an encounter with a new situation or new development. And new situations, he contends, are never approached with a clean slate of outlook and expectations. We always have a perspective (a "horizon") that has been historically shaped by culture, tradition, and personal circumstances. And since new situations always involve an element of the unexpected, "experience" generates what Gadamer calls "a radical negativity" (the knowledge of not knowing) which creates an attitude of openness and allows us to "see" possibilities that we hadn't been open to before. We are thus changed as a result of the experience; we have a new understanding. "The experiencer", says Gadamer, "has ac-

quired a new horizon within which something can become an experience for him" [p. 317].

None of this takes place, however, without language. We are born into a linguistic environment; an environment in which "reality" has already been linguistically classified and ordered. We interact via language. Our concepts are linguistically shaped. And we think in language. As Bernstein puts it, "for him [Gadamer] the medium of all human horizons is linguistic . . ." [p. 144]. The linguistic role is most explicit in Gadamer's model of conversation which could be characterized as a model for the fusion of interpersonal horizons.

For two people who do not agree on some subject and who wish to achieve agreement, conversation holds the possibility of the desired agreement. True conversation, however, is only possible if both parties are willing to be open to the other's point of view. When both parties are open in this way, then the conversation is guided, in a sense, by the subject of the conversation. The matter under discussion, in this case, generates questions. On the other hand, if the parties are not open but only pretend to be (as in a debating contest), then the questions they pretend to have are false questions. Thus, according to Gadamer, "a question can be right or wrong, according as it reaches into the sphere of the truly open or fails to do so" [p. 327].

In the case of false questions, not only do they prohibit the issue at hand from being decided, but they stand in the way of discovering what Gadamer refers to as "truth". Truth, in the sense that Gadamer uses the term, refers to shared understanding and is caught up with the notion of community, as is illustrated in the following quote:

> Every conversation presupposes a common language, or, it creates a common language. Something is placed in the centre, as the Greeks said, which the partners to the dialogue both share, and concerning which they can exchange ideas with one another. Hence agreement concerning the object, which it is the purpose of the conversation to bring about, necessarily means that a common language must first be worked out in the conversation. This is not an external matter of simply adjusting our tools, nor is it even right to say that the partners adapt themselves to one another but, rather, in the successful conversation they both come under the influence of the truth of the object and are thus bound to one another in a new community [p. 341].

Bernstein suggests that the new, postempiricist view of science tends to incorporate a view of rationality that is very much in tune with Gadamer's model of conversation. The proponents of the new view deny the validity of any predetermined algorithmic scheme for evaluating hypotheses, theories, and arguments. Instead, they tend to accept what Bernstein has called "a dialogical model of rationality that stresses the practical, communal character of this rationality in which there is choice, deliberation, interpretation, judicious weighing and application of 'universal criteria,' and even rational disagreement about which criteria are relevant and most important" [p. 172].

It must be noted that Bernstein does not claim that postempiricist philosophers of science were directly borrowing from hermeneutics. What he does maintain is that these philosophers, via their dialectical give and take concerning the nature of scientific inquiry, "have stressed those features of science . . . that are hermeneutical" [Bernstein, p. 33]. Most notably with respect to Kuhn, Bernstein suggests that he [Kuhn] was groping toward a hermeneutical view of rationality: "It is as if he has been searching for a proper model to express his awareness that such deliberation and choosing [among rival paradigms] are rational activities, but not the sort of rational activity that has been characterized as deductive proof or empirical verification or falsification" [p. 41].

In any case, it is obvious that the salient features of the postempiricist view, can appropriately be characterized as hermenueutical: the questions and problems that deserve attention emerge from social, cultural and historical circumstances; methods of inquiry and standards of judgement are shaped by the social practices of the community of scientists; and "truth" hinges on shared understanding.

It is also obvious that the postempiricist view of science is radically at odds with the dominant view of science among accounting researchers. And what is most interesting for the present discussion is the fact that many of the (presumably scientific) methodological views held by mainstream accounting researchers would be seen as unscientific from the postempiricist perspective. As Morgan [1988] has pointed out, "The idea that accountants represent reality 'as is' through the means of numbers that are objective and value free, has clouded the much more important insight that accountants are always engaged in interpreting a complex reality, partially, and in a way that is heavily weighted in favour of what the accountant is *able* to measure and *chooses* to measure . . . " [p. 480]. And as

Hines has suggested, the erroneous and widespread identification of statistical procedures with "the scientific method" serves to restrain criticalness and creativity in accounting research [p. 661]. Open discourse in accounting also has been restricted by the widespread notion of a logical gulf between positive and normative theories. "If one assumes (as many empiricists do) that theories can be divided into 'normative' and 'positive' frameworks, and that the verity of the latter can be established by merely consulting factual evidence, then the scrutiny of underlying values slips from explicit attention, returning covertly in the disguise of 'facts' to participate in deciding what passes as 'truth' " [Tinker, p. 183]. And making a similar point Arrington and Francis note that, "To deny the value-ladenness of one's theorizing is to deny responsibility for the consequences of one's theories" [p. 4].

Finally, a historical note with respect to Devine must be added. Although Devine was a major influence in bringing philosophy of science into accounting thought, there is evidence that he is not particularly happy with the outcome (at least as it currently stands). In the Preface to Volume V of his "Essays," Devine expresses a growing concern "over what appears to be a new parochialism in accounting research, i.e., a tendency to restrict research to the narrow confines of quantitative methods." And one could make a good argument that Devine's view of science is much more in tune with the postempiricist view than with the mainstream accounting view. In fact, Arrington's review of the "Essays" could be construed as such an argument. According to Arrington, Devine considered science to be essentially a way of expanding (rather than limiting or closing off) the discourse of accounting. And there can be no doubt that Devine's pioneering work regarding semiotics helped pave the way for the introduction of postempiricist views into accounting literature.

> Devine is fascinated with the role of language in constructing knowledge and meaning, and draws upon the early work in semiotics and what it might have to say to accountants. What he could not have foreseen is the way in which semiotics has been expanded to the point that, currently, the history of ideas is firmly grounded in the overriding importance of language in the construction of meaning. Contemporary work in hermeneutics, structuralism, and poststructuralism that is sweeping the human sciences is beginning to surface in accounting. This work owes a debt to Devine for being the first scholar to

position accounting firmly in the domain of language [Arrington, 1988, p. 139].

CONCLUSION

The juxtaposition of developments in accounting research with developments in philosophy of science reveal that "the Golden Age" of accounting methodology is caught up in a compound irony; an irony that functions on more than one level. The most basic irony is, of course, that accounting researchers were "awakened" to *the* scientific method during the same decade (the 1960s) that witnessed the disintegration of "the received view" of scientific methodology as a result of the "growth of knowledge" debate. The second level of irony has to do with the respective legacies of "the Golden Age" of accounting methodology and the growth of knowledge movement in philosophy of science. The legacy of "the Golden Age" seems to have been the enshrinement of a dogmatic reverence for a positivistic/empiricist research methodology and a research environment characterized as "methods in search of questions." The growth of knowledge movement, on the other hand, has essentially discredited the positivistic/empiricist methodology and cleared the way for the emergence of a hermeneutically-informed postempiricist view of science; a view which acknowledges the social role in the construction of "reality" and emphasizes the importance of replacing rigid pre-determined methodological rules with the give and take of "good conversation" in the resolution of methodological issues. The ultimate irony then is that the research methodology touted by some of the most prominent mainstream accounting researchers must be judged clearly "unscientific" from the postempiricist philosophy of science perspective.

Mainstream accounting researchers would do well to ponder the advice of an outsider: "Accountants can begin making themselves 'more scientific' by shedding their guilt for being normative or controversial, or for having unfalsifiable theories" [Lavoie, p. 582].

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