Positive accounting theory is claimed to be explanatory of accounting practice; a scientific, empirical, economics-based theory. These claims are examined severally, and in significant respects are found to be untenable. Vast tracts of accounting practice remain unexplained, even unnoticed. That scientific enterprise is directed to the development of technical norms or prescriptions is set aside. The association between the ideas of economics and accounting extends, at the hands of the theory’s exponents, not to the corpus of well-established economic notions, but only to tangential elements of economics. While purporting to shun prescription, the exponents of the theory, both actively and by implication, endorse conventional accounting which is heavily dependent on normative propositions. The “theory” is marred by oversights, inconsistencies and paradoxes. The implicit hope that the methodology followed ‘will provide a useful positive accounting theory’ (Watts and Zimmerman, 1986, p. 362) entails that its proponents have offered no such theory.

Key words: Accounting theory; Scientific method; Theory.
are portrayed or bypassed by positive accounting protagonists. It will be contended, among other things, that many of the claims made by or for leaders of the movement could be made, equally if not more properly, by or for others; that some such claims are unjustified; and that some ideas advanced as novel are long-standing ideas in new verbal garb. It is necessary therefore to distinguish the movement from the persons and the work of others. It, and as convenient shorthand its devotees, will be referred to as the PA cult.

The PA cult appears to have emerged from work in financial economics in the University of Chicago in the mid-1960s and from work on the nature of the firm in the University of Rochester about the same time. That these origins were beyond the mainstream of work in accounting is of more than passing interest. The study, teaching and practice of accounting had been passing through a period of external and internal criticism and self-doubt. The mood could perhaps be dispelled by a new look — a seemingly respectable verbiage, an apparently sophisticated research apparatus, a nodding familiarity with the concerns of scientists and economists. The verbal adornments of the cult — 'positive', 'empirical', 'scientific', 'economics-based' and so on — its rituals, its congregations, its sanctions and its cohesion, drew a galaxy of followers into orbit about the Chicago–Rochester axis.

Attention can be given here only to a modest portion of the output of the cult, to what appear to be the key elements of its doctrines, hypotheses and procedures. The principal reference will be to Watts and Zimmerman (hereinafter W&Z; 1986), which appears to be a compendious exposition of the work of the cult, but there will be modest references to other work of the same genre.
influence the course of judgments, and hence of stock prices. So the conclusion of B&B was commonplace. The underlying presumption of the argument, however, was that the firm, as a separate entity, is to be disregarded. What is then of interest is the incomes and rates of return of investors, and that has to do with changes in stock market prices and amounts of declared dividends. That is all; and that is the focus of attention of the project.

But the critics of conventional practice were concerned with differences between the disclosed income of the firm and the empirically determinate income of the firm. The firm was regarded by them as a distinct entity having assets (just as investors have as assets the shares they hold), the net amount of which is variable (just as the market value of an investor's shares is variable). The firm was deemed to have an income consisting of cash increments and accrued changes in the prices of assets (as in the case of investors; B&B, p. 162), and a rate of return, being the ratio of the income to the opening investment stated at market prices (as in the case of investors). Given the availability of such information on the rates of return to the firm and to investors, investors could compare the two rates, compare either or both of those rates with returns to other investors and to other firms, and draw inferences affecting their judgments, their willingness to buy or sell and the prices at which they might do so. This schema, it is apparent, gives investors a richer supply of information pertinent to their actions than the PA schema. And the premise that the firm is a distinct entity is empirically defensible, whereas the PA presumption is counterfactual.

If any attention had been given to the firm as such and to features of the released income information, several things might have been apparent. First, under conventional accounting firms do not calculate incomes and rates off return in the manner of the B&B calculation of the return on a given stock. The anomaly deserved attention, but received none. The amounts of declared dividends depend on the amounts represented from time to time as divisible surpluses. To be consistent with the B&B calculation, divisible surpluses should be represented by empirically determined incomes and prior surpluses; under conventional accounting they are not. Under conventional accounting for firms, there are made available to investors, not only income figures, but also balance sheets representing the dated amounts and compositions of assets and debts. That information provides inputs to judgments on the solvency and the financial flexibility of firms and on the riskiness of asset portfolios, all of which are potential modifiers of the prices at which rational investors may otherwise be willing to buy or sell. Of course, they would not be serviceable modifiers if calculated otherwise than by the B&B method for the dated amounts of the assets of investors; but under conventional accounting they are calculated by quite different methods. The B&B exercise disregards the whole of this, both the anomalies mentioned and the function of periodical information on dated financial positions. That is the inevitable consequence of taking no notice of accounting, and only notice of what momentarily hits the stock market in respect of each security considered severally.

To evaluate the conventional income numbers of firms by recourse to the incomes of investors is logically impossible, however sophisticated the procedure used may
be; the two are not *in pari materia*. And to ignore the substantial differences in the modes of calculating those incomes is egregious oversight. Given these solecisms, notice may be taken of some subsidiary twists, for the PA literature is liberally supplied with examples of verbal legerdemain.

Note, first, the reference to income numbers. The products of accounting processes had long been described as measurements, improperly so described according to some of the critics. B&B seem to have acknowledged implicitly the impropriety of the description; but the reference to 'numbers' also implicitly severs the association of the products of accounting from the observables they are expected to depict. B&B did not deny that conventional income numbers 'lack substantive meaning', have no empirical referent to which the term 'income' can properly be applied. As has occurred so often in later work of the PA cult, such awkward contentions are just set aside — no proviso, no comment.

Second, what could be intended by 'further empirical testing'? The authors whose views B&B questioned were concerned with the failure to produce money amounts that corresponded with empirical phenomena, and were for that reason (a) unintelligible to recipients and (b) unserviceable as indicators of past and prospective real world relationships. 'Further empirical testing' would then mean further testing for correspondence and intelligibility. But 'further' was diversionary. B&B were altogether unconcerned with correspondence and intelligibility.

Third, 'useful' is clearly misused in the conclusion that disclosed earnings numbers are useful. It might be claimed that the published income numbers were used. Lacking a hammer, one might use the heel of a shoe, but it would be a gross overstatement to say that a shoe is *useful* as a hammer, and ludicrous to go to the shoe store when one wants a hammer. Lacking anything better, the market is (or, operators in it are) obliged to use whatever is called 'income' by those on whom they rely; but that does not mean that the number is *useful* as indicating or representing income. Whether the indifference of B&B to the distinction between used and useful is careless or tendentious the reader may judge.

Fourth, it is arguable that 'information content' is also misused. There are scores of events, or bits of information about events, that may influence the price of a stock at any point of time. The release of an income number is only one of them. At best, then, the release of such numbers is tenuously related to shifts in stock prices. To ascribe 'information content' to messages thus tenuously related is credulous in the extreme; and a misuse of a term having an entirely different and proper use. A message relates to some matter of which knowledge is available to the issuer. It can be said to have information content to the extent that it represents or corresponds with that matter, whether or not it brings about a response by any recipient. A response is the outcome of a complex evaluative process, of states and stimuli and needs and preferences, at a stated time. To ascribe information content to one of the inputs on the basis of an observed response is gross oversimplification. And it puts B&B on very slippery ground. Beaver on one occasion (1968) pointed out that the stock exchange trading volume statistics indicated that, on average, 'If corporation X has 10 million shares outstanding, during a normal week 50,000 shares will be traded, with an expected volume of 66,667 shares during
the earnings report week' (p. 75). Thus the shares traded on any earnings announce-
ment day would amount, on average, to less than 0.15 per cent of a company's
outstanding shares. It seems precarious indeed to base the sweeping conclusion
of B&B on reactions in respect of such tiny proportions of outstanding stock. At
face value, the volume figures suggest that the earnings release had no information
content in respect of holders of 99.85 per cent of an issue!

In any case, the method used biased the inquiry, and therefore the conclusions.
It automatically excludes companies that were not among those listed over the
full period of years examined. It thus excludes all companies that may have collapsed
or been liquidated, during that period or at other times; and collapses and failures
and the investigations that follow them are the richest source of evidence of deliberate,
and inadvertent but no less objectionable, misrepresentation, in short, evidence
of misleading information content. Where the methods used with that effect are
used almost universally, their effects in any particular case are a matter of degree,
not of kind.

B&B either overlooked or deliberately set aside the fact that statements that
do not correspond with what they purport to represent can only have misinformation
or disinformation content. They do not acquire meaning, in the sense of Canning
and others, or in the sense pertinent in practical affairs, simply by being published.
A falsification does not become a reality, a lie does not become a truth, a fiction
does not become a fact, by its utterance.

The usefulness and information content conclusions must be regarded as
overreaching, not proven. The inquiry was not, of course, an inquiry into any
facet of accounting at all. It was not a further test of prior contentions. It was
not a test or an evaluation of conventional income numbers. The paper's title
misrepresents what the paper does. It could have been written, indeed, without
any knowledge whatever of accounting. The exercise looks rather like the product
of people who have newly learned a method of analysis looking for some way
of applying it, rather than of people confronted with a problem looking for some
appropriate means of solving it.

It is noteworthy that neither Ball nor Brown had contributed anything of substance
on the particular rules and devices that have been advanced, analysed and criticized
in the professional and academic literature. Nor did they pay any attention to
the vast natural history of practice and its consequences that has provided
practitioners and academics with empirical evidence of the anomalous nature of
so much that was done and taught. Sufficient for them to hurl Jovian bolts from
high Olympus on those menials who strove with such difficulties as vague and
inapt terminology, inconsistent prescription, fallacious argument and the observable
inequities associated with demonstrably false financial statements. An upsurge of
concern with these very infelicities was the proximate spur to the emergence during
the 1960s and 1970s of the APB and the FASB in the U.S. and of similar organizations
elsewhere. But the contentions that conventional accounting, in spite of alleged
flaws, was useful, provided information content and did not mislead the market
cut right across the drift of the evidence and the urge for reform. All was right
with the accounting world after all! Or, if there were flaws they were innocuous,
for the market could, by some occult divination, 'see through' (W&Z, 1986, p. 99) the fictions and idiosyncrasies of accounting practice.

It was a short step to the position that all differences in precept or practice were justifiable, and that the business of inquiry in accounting was to discover what sets of conditions justified which sets of rules. If one obscures or suppresses the difference between knowledgeable reasoning and ex post rationalization, and describes the total package as theory, the ground is laid for the depiction of theory as excuse. Thus did W&Z (1979). Using theory 'as a generic term for the existing accounting literature' (p. 273) was a careless or a cunning way of bringing under one rubric work that was as different as the aprioristic and the empirical, or as rationalization and reasoning. The usage enabled them to lump all together under the summary description 'predominantly prescriptive'. The 'excuses' paper describes itself as 'not normative or prescriptive', as if (since there is no other apparent reason for the remark) to distinguish it sharply from prior work. But the paper is not in the same vein or of the same kind as that prior work; it is concerned not with the substance of rules but with the role of theory in determining accounting practice or accounting standards. The setting is there laid for the subsequent use of 'positive theory of accounting' for what should properly be called some sort of theory of standard setting — for accounting and standard setting are quite different processes.

ACCOUNTING?

The B&B paper had been rejected by The Accounting Review on the ground that 'it was not an accounting manuscript' (Dyckman and Zeff, 1984, p. 242). Whether or not some set of ideas or rules falls, or should fall, within the domain of accounting may seem to be a pointless question. It is not. There is a basic similarity among methods of disciplined inquiry in all kinds of fields, but there are also critical differences. Arithmetic differs from bookkeeping; pathology from epidemiology; economics from administration; financial economics from accounting. Each item of these pairs has its own characteristic set of concerns, problems and problem-solving procedures. Each item may benefit from interchanges with its other. But its orderly development and practice are disciplined only to the extent that those expert in it keep its function and functioning under critical scrutiny. If no attempt is made to delimit the field, attention and effort are apt to be squandered on irrelevancies that masquerade as the proper business of accounting. That possibility lurks in the background of the PA doctrine.

Given the training in finance of the forerunners of the PA cult, said W&Z (1986), 'it was natural for them to concentrate on explaining and predicting security price behavior and not on explaining and predicting accounting practice' (p. 16). They made use of the efficient market hypothesis (EMH) and the capital asset pricing model (CAPM), knowledge of which is 'still crucial to current researchers and those wanting to understand and perform accounting research', according to W&Z (p. 16). The claim is unproven except for some special types of research in which the PA cult engages. But have these borrowed devices anything to do with accounting research?
As Ball (1972) saw the EMH: 'The efficiency of the market is defined as the speed with which equilibrium is reached after the release of some set of data' (p. 2). It's a matter of speed of ingestion, not of the quality of the information released. In the absence of anything to the contrary in the literature, it holds of data that are true and false, substantiable and fabricated, financial and non-financial. It has nothing to do with explaining and predicting the specifics of accounting practice.

Neither has the CAPM. It is based on the assumptions — perfect markets; rational, risk-averse, maximizing investors; costless access by all to information; and homogeneous expectations (e.g., W&Z, 1986, p. 23). Given this roster of assumptions, 'in essence, under the multiperiod version of the CAPM, the market value of the firm is the discounted expected future cash flows' (p. 27). In the real world, in which none of the severe assumptions holds, to accept a calculated DCF as a market value would be plain error, and to use 'market value' of DCF in the passage cited is to confuse an imagined number with a number having, in principle, an empirical referent. So to confuse an imaginary with an observable is quite at odds with the view (which the PA cult claims to endorse) that reliable knowledge subsists in what can be shown to be the case. But, whatever it may have to do with the analysis of financial markets, the CAPM has not been shown to have anything to do with explaining and predicting accounting practices.

What, then, is to be taken as the domain of accounting and the subject matter of accounting theory? The quite general term 'positive accounting theory' implies that there is to be expounded a theory that deals with accounting generally. 'The objective of accounting theory is to explain and predict accounting practice' (W&Z, 1986, p. 2). W&Z affirmed that their concept of accounting theory is 'broader and has a different focus from that given in financial accounting texts' (p. 1). But broader does not mean narrower. It is to be expected, therefore, that the scope of positive accounting theory would embrace the substance, if not the particulars, of other expositions. In fact it does not.

In any developed community there may be hundreds of thousands of business firms that keep accounts. Of these suppose that 100,000 are corporations; and that of the corporations no more than 2 or 3 per cent have securities traded on stock exchanges. The studies of the PA cult cover only such corporations and only a select and much smaller proportion than that. In effect, the accounting of over 99 per cent of all firms is, by intent, excluded from notice and consideration. What, then, will serve to explain and predict accounting practice in their case? Certainly no inference from stock prices can do so. Further, there is not just a small number of varieties of accounting practice among the 1 per cent of firms noticed. Standard textbooks and manuals embrace a plurality of variously combinable rules for almost every item that appears in a set of accounts. Almost certainly the periodical accounts of every firm are *sui generis*, unique. The PA literature deals with only a very small part of all this diversity.

A large part of the textbook and discursive literature, and of practice, is concerned with balance sheets, their contents and the inferences that may or may not be drawn from them. Without a balance sheet it is impossible to derive for any firm indicators of solvency, the riskiness of asset and debt compositions and debt to
equity relationships, financial flexibility, the financial feasibility of optional operations and the rate of return — all of which have some bearing on assessments of the firm's performance and prospects, and therefore on the market prices of its securities. Most of these crucial functions of the products of accounting, and what they imply, are completely disregarded.

Having found it necessary to explain at some length the nature of the firm (e.g., W&Z, 1986, p. 194), an occasion was introduced for considering accounting as a means of coordinating the internal use of resources with the object of maximizing profits and regulating liquidity and solvency. But the potential of accounting for service in this way has scarcely been noticed. Ball (1985, p. 7) has treated the products of conventional internal accounting as quasi-prices; but as they bear no relation to the prices which the firm must confront from time to time in external dealings, they can be of no value as coordinative or regulative devices.

The purview of Whittred & Zimmer (hereinafter WHZ; 1988) is likewise highly selective, limited to external financial reporting by companies having publicly traded securities. WHZ said they were 'more interested in the application [i.e., the selection] of accounting techniques than in the techniques per se' (p. xiii). They said they would investigate the perspective of shareholders and creditors on the choice of technique (though those parties have little effective say, and little understanding, in the matter), and the determinants of management's choice of techniques — why managers and others reveal strong preferences for one accounting method over another. To understand these things is to 'understand the fundamental nature of the accounting process' (p. xiv). But what fundamental proposition dictates or implies that there are or shall be options open to choice? How many options? In respect of what? May a given amount of money spent, or on hand, be represented by a variety of optional amounts? Is it the function of accounting to represent any observable event or object? If so, why, and how? And if not, why not? To have answers to such questions as these is preliminary, surely, to the offering of any grounds for managerial or other choice of techniques. But the PA literature asks no such questions.

In substance, then, the PA cult uses 'accounting' only of some phases of the accounting of firms having publicly traded securities. W&Z (1979, p. 300) advanced the hypothesis that 'much of accounting theory is the product of government intervention and that accounting theory satisfies the demand for excuses'. As noted already, they used 'theory' simply to describe the accounting literature. Even on that score a vast amount of the accounting literature was and is unrelated to government intervention. And, if 'theory' is used in a more exacting sense, a great deal of the discursive literature to which they alluded concerns itself with matters more fundamental and general than public reporting. The PA cult leaves the whole of that territory unnoticed and untouched.

**POSITIVE? NORMATIVE?**

Inventing names, attaching labels, classifying and pigeonholing are all useful devices in their place. But too often they are made to stand in place of careful analysis
and disciplined thought. The disputatious and the merely captious resort to them to advance or dismiss, to promote or put down, to set up or demolish argument, rather than to distinguish with care between what they consider meritorious and what objectionable. So it has been with 'positive' and 'normative'. 'Normative' has been widely used pejoratively, even though occasionally it is granted (e.g., W&Z, 1986, p. 9) that normative propositions are not unimportant. As positive research and positive theory are advanced as the proper way of going about the scientific study of accounting, it is necessary to be clear about the meanings of normative, positive and scientific.

Scientific inquiry does indeed make great use of propositions cast in the positive form: 'it is the case that A'. They are used as descriptions, as premises of reasoning and as the object of tests of the outcome of reasoning. However, that usage rests on underlying propositions that express ideals, judgments or values, or norms — propositions such as: observation is preferable to uninformed imagination or speculation as a basis of reliable knowledge; or, more reliable knowledge on any matter is preferable to less. The PA cult cannot object to such value-laden or normative propositions as these, for its avowed preference is an expression of such a proposition.

Next, scientific knowledge is simultaneously positive and normative, or descriptive and prescriptive. If it is true that gases and liquids expand on the application of heat or the reduction of pressure, it is true that one, anyone and everyone, must apply heat or reduce pressure to bring about an expansion of gases and liquids. The former proposition in positive form and the latter in prescriptive form are logically and practically equivalent. Scientists and others have never been queasy about affirming the intimate association between (positive) knowledge and the advancement of the ideals and aims of men. '...only by a knowledge of what is can we make any practical programs of what ought to be' (Lundberg, 1942, p. 54). The theoretical and the experimental natural sciences 'both lead us to the formulation of practical technological rules stating what we cannot do' (Popper, 1972, p. 342). The greater part of the scientific enterprise is directed, not to the tolerance or preservation of inherited knowledge and devices, but to the improvement or supercession of both. Improvement, betterment, inevitably entails a departure from what is, or was previously believed to be, the case. Since the utterances of the PA cult (e.g., W&Z, 1986, p. 11) contemplate the possibility at any time of the emergence of better theories or better models, it cannot be supposed that positive accounting theory entails unswerving allegiance to what is at any time believed to be the case. But the PA cult does, implicitly and explicitly, champion conventional accounting against its critics, the so-called normatives.

There is, however, one class of normative propositions that is to be rejected, that class for which there can be adduced no greater support than the authority of the utterer. An instance may be the AAA (1948) dictum: there should be no departure from the cost basis to reflect the assets of an enterprise at amounts higher than unassigned costs. This dictum lies at the root of the conventional practice that the PA cult condones or endorses. But beneath some of the precepts of that style of practice, there do lie some propositions that are not essentially moral or
authoritarian imperatives. The positive propositions and the reasoning underlying precepts may not always be apparent, or adjacent; but in many cases they are imaginable or discernible. The premises may be false or the reasoning fallacious, or both; a precept is then rejected, but for reasons other than its normative form. Thus, the AAA precept mentioned above is widely rejected. But, given that 'normative' is and has been used pejoratively, it is easier to use it of propositions from which one dissents than to try to discover the positive propositions on which the normative rests. That seems to have been the principal use of the term in the accounting literature of the past twenty years.

The object of the positive-normative distinction has not been to distinguish the 'good' from the 'bad', but to distinguish the testable from the untestable. '...is and ought belong to different worlds, so that sentences which are constructed with is usually have a verifiable meaning, but sentences constructed with ought never have' (Bronowski, 1964, p. 61). The purport of such observations as this is that only empirically supportable (positive) propositions govern what men may confidently believe to be possible experiences, and grounds, therefore, for serviceable precepts.

The frenetic urge to distance themselves from their antecedents impelled the PA cult to shun prescription. '...theory as we describe it yields no prescriptions for accounting practice... It is designed to explain and predict which firms will use a particular method of valuing assets, but it says nothing as to which method a firm should use' (W&Z, 1986, p. 7). This is curious. A theory is commonly understood to be a systematic body of propositions yielding an affirmation that, under all or stated conditions, certain phenomena are associated (as cause or antecedent) by specified means with other phenomena (as effect or consequent). An accounting theory, then, would affirm that, if a specified consequent is to be brought about, a specified antecedent is to be subjected to specified processes. However, W&Z (1986) held that the consequent, which they called the objective, cannot be specified in general terms; 'the decision on the objective is subjective' (p. 7). Which means that there can be no theory in the sense described above. If the consequent cannot be specified in general terms, there is no way of explaining or predicting, by recourse to the so-called theory, which firms will use which basis of valuation, or any other of a set of optional rules or procedures. Explanation and prediction turn on the possibility of applying to any given set of antecedents some generalization in the nature of laws or rules that leads to or entails a consequent. In the case of constructed devices such as accounts and accounting, the consequent is the performance of a specified function or functions. There can be no interest whatever in explaining and predicting if no such function can be specified.

The aversion to prescription, or anything that looks like it, may have stemmed from misunderstanding of constraints in the natural and physical sciences. In those fields it is of no consequence, and therefore improper, to describe any phenomenon as good or bad, preferable or not preferable. A phenomenon is simply what it is. But in human and social affairs that embargo does not hold. A chair, a rule, a theory may be good or bad, and therefore preferred or not, according as it performs well its intended function. The aversion may also have stemmed from the widespread
misuse of ‘objective’ in the cited context. Devices have functions; people have objectives. Scientific generalizations and knowledge relate to functions, not to the personal and contingent aims and intentions of people who put that knowledge to use. By confusing the two terms, the quite proper rejection, as subjective, of the idiosyncratic objectives of people is improperly extended to debar the drawing of universal or general functional inferences.

EMPIRICAL?

In one sense, empirical means based on observation or experiment. Observation has two roles in the context of inquiry — to prompt generalizations that may serve as premises in deductive schemes, and to provide means of testing conclusions or inferences. Conclusions that are consistent with the wider range of observables are preferred, as having the greater explanatory or predictive power.

The critics of conventional accounting, mentioned in an earlier section, have taken as evidence of its misleading nature a wide range of observables. They included the widespread and frequently augmented and varied provisions of legislation and regulations enjoining the publication of true or fair representations of financial positions and incomes; like provisions proscribing the publication of misleading financial statements; writs and litigation alleging publication of false and misleading information; complaints by legislators, administrators, tribunals, analysts, financial reporters, accountants and academics, of the misleading nature of published accounts; long-concealed frauds and unexpected failures and bonanzas. These are overt manifestations of deceit or technical solecisms. As well, there is internal evidence, known or knowable among accountants, of the fictions and logical fallacies that underlie many common practices. The inference that investors and others are misled is thus well-supported. It could no doubt be shown that not a year passes without shareholders and creditors of some publicly noticeable companies suffering losses which, on the basis of published financial statements, they could not have expected to suffer.

The argument of at least some of the critics runs thus. Between successive dates of publication of corporate financial information, investors respond as they think fit to firm-specific, industry-wide and nationwide events and information on them. Only at and after those publication dates, however, is it possible to adjust their prior judgments in the light of the only source of potentially reliable firm-specific financial information. Prior guesswork and speculation are modified by observable, empirically determined magnitudes — ideally! In practice the correspondence between what is the case and what is represented to be the case may, and does by the very method of its derivation, fall far short of the ideal.

The PA cult takes no account of the above-mentioned observables. It is held on the contrary that managers, through the choice of accounting methods and of what shall be disclosed, ‘cannot systematically mislead the stock market’ (W&Z, 1986, p. 159). All the observables mentioned are thus, by implication, tokens only of costly and fruitless misapprehension! The complainants were all wrong; only the PA cult is right. The conclusion purports to rest on the EMH, the CAPM
and observable stock price movements. However, the EMH and the CAPM are highly abstract constructs, devised for the abstract discussion of markets as wholes, not for the explanation or analysis of the actions of individual investors in a less than abstract world. In an efficient market, it is said (W&Z, 1986), the price of a stock 'is such that investors on average earn a risk-adjusted market rate of return... In other words in a semistrong efficient market, the market is not systematically misled by accounting earnings'(p. 21). This is no more than an assertion that things turn out to be how they turn out to be. And since, in the analyses of the PA cult, there is no input representing an empirically determinable net income, nothing can be deduced regarding the quality of reported accounting earnings. The contention of the PA cult is simply an example of that predilection for dealing with stock market prices rather than accounting practices which, as noted earlier, W&Z found so disarmingly excusable.

A note on observables of a different kind is perhaps apposite at this point. It is well known that we see (or perceive) what we are conditioned to see (or perceive). It takes a particular commitment to the eradication of error, a scientific commitment, to discriminate between what appears to be the case and what is the case. W&Z (1986) wrote of 'the natural desire of proponents of a new set of theories and methodology... to demolish the existing paradigm's theories' (p. 16). The natural desire puts special strains on scientific commitment; it may override that commitment altogether. If old theories must be demolished, so be it. But demolishing straw men in the guise of demolishing theories is something else.

The monopolistic hypothesis, attributed by Ball (1972) to the critics of conventional accounting, is such a straw man. Quoting some passages from Simpson, Spacek and Chambers, severally, Ball said: 'Presumably, accountants possess a monopolistic influence over the data used by the market, since it is assumed [by the parties cited] that either competing data sources do not exist or (if they exist) they are not used' (p. 3). The sources cited neither said nor implied any such thing. Their authors were dealing, not with 'data' in general, but with accounting information. In an earlier paragraph of this paper the market's access to non-accounting information was granted; all kinds of information may be taken as hints or clues to movements in a firm's income. In respect of such data, accountants have no monopoly. But in respect of firm-specific income information, firms and their accountants do have a monopoly. To give fabricated rather than empirically determinable information under cover of that monopoly is just as misleading as it would be to give fabricated stock prices in place of observed prices. Neither Ball nor anyone else has shown that there are sources of that information other than the income report. The switch by Ball from firm-specific income information to 'data' is either an instance of seeing in the sources quoted what was expected or wished to be seen, or carelessness of an equally indefensible kind.

In due course, the idea as enunciated by Ball came to be endorsed by W&Z (1986). They added a twist of their own, but of the same kind. 'It was asserted [by the critics] that earnings should measure changes in the value of the firm, so stock prices could be good signals for the allocation of resources' (p. 20). No source of the assertion was given. But the literature carries abundant expressions
of the view that an earnings amount neither does nor should be supposed to measure changes in the value of a firm; earnings can only represent the change in the net assets of the firm. The W&Z twist is misconstruction or misrepresentation or careless imputation.

ECONOMICS-BASED?

Much of the work of the PA cult is described as ‘the economics-based empirical literature in accounting’ (W&Z, 1986, p. 1). Many have claimed to base their contributions to accounting discourse on economic ideas or doctrines. It is of some consequence to establish the sense in which PA theory is economics-based. The theory, we are told, is ‘based on and is an outgrowth of two economic-based theories: the theory of the firm and the theory of (government) regulation’ (W&Z, 1986, p. 353).

Use is made of a theory of the firm which regards it, not as an individual entity, but as a nexus of contracts between self-interested parties (p. 194). These contracts would specify limits to the powers of each participating contractor, and the manner in which the product (e.g., net income) of the firm is to be divided amongst them. Protection of self-interest would require that the manager, because of his unique position to influence the division of the product, would be subject to the monitoring of performance by or on behalf of each of the other classes of participant. The characteristic features of such a firm are thus contracting and monitoring. Therein lies a demand for accounting and auditing (W&Z, 1986, p. 196).

The participants in the firm thus postulated include managers themselves, debenture and stock holders, lessors, labour and trade creditors, and suppliers and customers. Since there is no single focal entity, every one of these classes, under the self-interest assumption, would have contracts with every other class, and each class would want to monitor performance under every contract against the possibility of collusive arrangements prejudicial to its own interest. A direct consequence of all this is that the terms of contracts and the elements of the monitoring device shall include a clear specification of the product in which the several parties are to share, and shall be equally intelligible to all parties. We are told that ‘positive propositions are concerned with how the world works. They take the form “If A, then B”, and are refutable’ (W&Z, 1986, p. 8). The above consequence may then be set up thus: if the terms of contracts and the mode of accounting clearly specify the product that comes to be divided and are equally intelligible to all parties, then all parties have equal and equally fair means of judging the extent to which their interests are, or have been, served. Monitoring can only mean what it seems to mean if the monitoring device fairly represents what has been done and what has occurred under the direction of the manager as agent. A false account will not do.

This seems to be an incontestable consequence of the posited nature of the firm, and an unchallengeable basis of a practical precept. But the PA cult admits neither. Indeed, the description of some of the contracts belies the idea of contracting with monitored performance. WHZ (1988) said that the accounting method to be used
may be specified in the contract, but in view of the expense of negotiation, 'contracting parties are more likely to leave the majority of such choices to management in full knowledge of the fact that management may act opportunistically, that is, choose a method that the parties would not have considered "reasonable" had it been on the negotiating agenda' (p. 22). If the latter step is the more likely, monitoring becomes fanciful, or rather, farcical; for then there is no contract covering the matter that would serve as grounds of a test of performance! The PA literature is, in fact, barren of a single example of a contract specifying fully the methods of accounting to be used. The exercise of Leftwich (1983), intended to show that methods are specified in debt contracts, illustrated very limited auxiliary provisions affecting only a fraction of the items that enter into the calculation of net assets and income. In effect, the PA cult does not push to any conclusion pertinent to accounting the assumptions made about the firm and its participants. And the whole contracting–monitoring edifice, as the PA cult sees it, collapses for lack or looseness of contracts.

It seems not unlikely that the PA cult has entirely misread Coase's (1937) theory of the emergence of firms. Coase observed that contracting between individual collaborators would be costly. Interposing a firm as an entity would reduce the costs of contracting, cross-contracting and monitoring. But given that firms exist, the postulated costly contracting is dispensed with. The costly contracting postulated by the PA cult has to do, it seems, either with the dismissal of the firm as an entity, or with the cost of negotiating contracts when a vast array of optional accounting methods gives every party an opportunity for demanding the use of his own choice of methods. But the former is inconsistent with what is observable, and the latter is a pseudo-problem, arising only because the PA cult grant the legitimacy of all possible rules in a circumstance that demands one set of commonly understandable rules. To imagine even a football game or a poker game in which the players may unilaterally make and change the scoring rules during play is impossible.

The nexus of contracts idea has been described by Fama (1980) as 'the striking insight of Alchian & Demsetz (1972) and Jensen & Meckling (1976)' (p. 289). But a similar idea, much closer to Coase's theory of the emergence of firms, was introduced to the accounting literature two decades earlier. From clues given in Barnard (1938) and Simon (1949), that idea was adumbrated in Chambers (1952) and amplified in Chambers (1967, p. 16). In the latter the firm was described as a temporary coalition of participants in unstable equilibrium.

There are some noteworthy differences between the PA nexus and the coalition hypotheses. Under coalition, the firm is regarded as a distinct entity, which it is at law in the case of corporations, and which it is for all practical purposes in commerce generally; under PA nexus, the firm is not an individual entity (Jensen and Meckling, 1976, p. 311; W&Z, 1986, p. 194). Under coalition, managers are regarded as agents of the firm, which they are in law in the case of corporations; under PA nexus, managers are treated as agents of the stockholders (Jensen and Meckling, p. 309; W&Z, Ch. 8). Under coalition, the firm through its agent is a party to all contracts and understandings with participants, including managers.
themselves as principal contractors; under PA nexus, there are no contracts with the firm, and no contracts with managers that cover separately their roles as independent contractor with the firm and as agent of the firm; there are contracts only between stockholders and managers as agents of stockholders. Under coalition, all parties can contract for, or otherwise discover, the pay-offs from the firm and from alternative engagements, and can from time to time renegotiate or sever association in the light of that discovery; under PA nexus, there is provision for monitoring within the firm, but scant attention to pay-offs in alternative employments, and no regard, therefore, for the uniformity or comparability of information on pay-offs which would secure informed choice between employment options.

On these and other grounds, the unstable coalition hypothesis is consistent with more observables (empirical features) of firms (and, incidentally, it is closer to the 1980 schema of Fama) than the PA nexus hypothesis. And it has been employed more fruitfully. The coalition hypothesis entails an accounting that would 'be neutral with respect to the wishes and claims of participants' (Chambers, 1952 p. 217) and equally comprehensible to all actual and prospective participants. These are countervailing features to the otherwise unconstrained use of the privileged information accessible to managers who play two conflicting roles. The PA nexus hypothesis justifies substantially the same inferences in respect of actual participants at any time, though the PA cult declines to draw them; but disregard of the comparative uses of information, especially where the mobility of factors is an element of the business system, robs the inference of its potential generality.

The second economics-based strand of argument on which the PA cult claims to rely is the 'economic theory of regulation [which] conceptualizes the political process as a competition among self-interested individuals for wealth transfers' (W&Z, 1986, pp. 224, 354). At best, this notion may serve as part explanation of some political behaviour at the margin of public tolerance. But, taking it as it is, the political process is made to appear to be substantially the same as the processes of competition in market economics. It's just a matter of comparative costs. The discussion of costs, however, is limited to inter-party costs under monitoring, and some generalizations on the relative costs of private and public determination of accounting rules. The unforeseeable and involuntary costs of losers under failures, liquidations and bankruptcies that have been associated with maladroit and venal accounting are passed off: 'On average, losses in bankruptcies do not cause a below market rate of return' (W&Z, 1986, p. 21). And the enormous costs of private negotiation and of the common standard-setting apparatus under multi-optional accounting are unnoticed, though there is much publicly known and readily discoverable about their scale.

The adopted 'economic theory of regulation' overrides a serviceable distinction between economics and politics. In general, economics is concerned with wealth and politics with power. The two become linked when the power of some prejudices the interests in wealth of others. Hence the centuries-old laws on weights and measures and against misrepresentation of the quality of merchandise. The earliest steps in the regulation of published financial information (say, the U.K. Joint Stock Companies Act of 1844, and the U.S. Securities Act of 1933) arose out of experience
of the losses of creditors and shareholders through the fraud and deceit of promoters and managers having privileged access to firm-specific financial information. Those steps were taken to curb the power (through knowledge) of those whose dual roles would enable them to benefit themselves and to defeat the reasonable expectations of others. That is a proper function of legislation in communities which prefer fair dealing among their members to the unrestrained pursuit of self-interest. The CAPM, by assuming equal access to costless information, implicitly acknowledges the possibility of privileged access, but the assumptions simply dismiss it from consideration. The PA cult 'has not pursued' the possibility of inequitable consequences 'because it is impossible to define an optimality criterion of "fairness" that is acceptable to everyone' (W&Z, 1986, p. 163).

What is thus set aside is not only the prime reason for legislative interest; it is also an intrinsic element of the contracting-monitoring schema. It is inconsistent with the rationality assumption to suppose that, in face-to-face negotiation, contracting parties would assent to non-factual representations of the product to be divided. They would insist on a fair game, a true, empirically determined account of the product. There is no ground for abandoning that expectation when contracting becomes institutionalized instead of face to face. It is said that competition for information underlies the EMH (W&Z, 1986, p. 20); it impels some to seek and others to supply information beyond what is contained in accounting reports. However, firm-specific information is not subject to and open to betterment under the force of competition. Its producers are natural monopolists, and its consumers are compulsory consumers. Competition as a discipline in that setting is a misapprehension, not even a myth.

The basic trouble with the second strand of the PA argument is that it begins with differences assumed to be inevitable. Six categories of parties are represented as having different interests in accounting reports (W&Z, 1986, p. 2), and the arguments, proposals and preferences aired in the press and in submissions to standard-setting bodies exemplify the variety and inconsistencies in what is demanded. But all of this ignores the common elements of interest to all parties — the aggregate available spending powers, the solvencies and the incomes of firms. As in personal affairs, only reliable and up-to-date information on these provides the ground for informed judgment, negotiation, and appraisal of performance and prospects, and regulates the behaviour of all but the wilfully improvident. No amount of politically engineered prescription can serve fairly the parties whose collaboration constitutes the firm unless it is of that kind.

It is widely overlooked (and not only by the PA cult) that there is a significant difference between regulation in accounting and in other fields. Regulation in other fields does not concern itself with technical knowledge and procedures; it presupposes the possession by practitioners of knowledge, based on scientific and practical grounds, of demonstrated and agreed merit. In accounting the greater part of what is considered as regulation deals with detailed procedures by fiat, a solution which accountants and their professional organizations have fostered, rather than rely on disciplined observation and reasoning. But, when science will serve, to depend on the waywardness of political processes is folly.
POSITIVE ACCOUNTING THEORY

The extent to which the study of accounting is advanced by recourse to economics is, in any case, open to question. Accounting is concerned with the financial features of individual firms. Economics is concerned with the economic features of aggregative behaviour, disregarding many of the specific states and relationships which are of significance to individual firms. Accounting is concerned with the communication of constructed firm-specific information; economics is concerned almost exclusively with market mechanisms as generators and communicators of raw, unprocessed information. Accounting is concerned with the wealth and income of persons and firms; economics with the aggregate wealth and income of whole communities. The generic terms, wealth and income, have significantly different meanings in the two fields, except when economists make specific reference to firms as such. Accounting is concerned with the minutiae of experienced events; much of economics is concerned with the construction of analytical models which strip down to abstract form the complexity of real-world events. These differences make the literature of aggregative behaviour — economics and financial economics — an inapt source or model for the analysis of accounting, and provide some explanation of the infrequency (and the not uncommon inaccuracy) of references made to accounting in the literature of economics. Economics and accounting are arguably complementary (Chambers 1964–5), but they are certainly not the same in kind.

Accounting has long and widely been described as measurement and communication. Without limiting the 'fields' on which the analysis of accounting may draw, these appear to be the most fruitful of pertinent ideas. The Foreword of W&Z (1986) spoke of 'accounting, broadly conceived as the measurement and communication of economic information relevant to decision makers'; but the book deals with neither measurement nor communication; the two words do not even appear in the index.

THEORY?

Theory is valued for its capacity to explain recurrent or invariant relationships between phenomena. To theorize about objects, events or relationships that are occasional, accidental or dependent on personal taste is pointless; there is no accounting for tastes; there can be no serviceable generalizations about matters of taste. Self-interest is a matter of taste. The term is used of an unspecified but virtually unlimited package of preferences, of varying urgencies or intensities and changing composition. Those preferences, furthermore, may be egocentric or altruistic in a variety of senses. Such an amorphous package of preferences may be used by any person to justify or rationalize his behaviour, whether it is unique or commonplace, constant or variant. But no independent observer can affirm that an observed person acted in an observed manner out of self-interest; only the observed can make such an affirmation. The affirmation is not independently testable, and can therefore yield no scientifically acceptable explanation of the observed behaviour, no generalization that is empirically supportable. Yet the irrefutable premise, self-interest, is the basic and most commonly used 'explanation' of the behaviours noticed in the literature of the PA cult! As for prediction, it is impossible to predict behaviour
that is 'explained' by recourse to idiosyncratic and unpredictably variable packages of self-interests.

Given a mass of data on observed accounting practices, one might ask why, for the valuation of inventories, some firms apply the lower of cost and market rule to items, some to inventory classes and some to aggregate inventories; why some use LIFO cost and some use FIFO cost; and why some use unmodified cost, replacement cost, net realizable value and so on. (Simply to ask why some firms use LIFO and some FIFO, as do W&Z, 1986, p. 2, leaves a vast range of observables unnoticed.) And beyond inventories, a host of other items are open to differential quantification. There can be no single general explanation that will account positively for these diverse usages. But there can be rationalizations. Some may claim that LIFO is 'income-reducing', and that that is a good thing. That may be a good excuse, but it is no explanation, for LIFO is 'income-increasing' when prices are falling. There must be a whole battery of such rationalizations to justify all the valuation rules mentioned. But a series of rationalizations relating severally to specific segments of a population is not a theory.

The general, and for that reason the more interesting, question is: why do so many different rules having different effects coexist? Or, alternatively: why do so many rules coexist which produce figures having no uniformly dated empirical referents? Or, again: why do so many rules coexist which quantify different properties of objects and events when those quantities are to be aggregated and otherwise related as if their components were qualitatively similar? Or, again: why are there so many rules that yield or are based on long out-of-date information, when contracting and monitoring are pointless unless the information is up-to-date? Each of these questions deals with a large class of observables, and each can be answered by recourse to one or more empirically supportable generalizations.

In choosing to deal with selected fragments of the observables, the PA cult has mistaken justification of particulars (i.e., excuses) for explanation by way of testable generalizations. It has not even attempted to do what theories are expected to do, namely, to explain large clusters of observables by economical recourse to testable general hypotheses. Some consequences of its aversion to setting up general hypotheses have been noticed. The nexus of contracts and rationality notions provide grounds for an up-to-date, reliable and generally intelligible accounting. But the consequence of articulating that hypothesis would look too much like a norm, and norms are taboo. So the hypothesis is not articulated; by default, a woolly and feckless monitoring, that tolerates the production of idiosyncratic, cryptographic and self-serving misinformation, is endorsed.

Anyone who attempts to establish good reasons for patently unrealistic and unreasonable processes and products is bound to find it difficult. And that is how it has turned out for the PA cult. Many of the empirical studies reported yield contradictory inferences; almost every one of them has had to be hedged about with ad hoc caveats or apologetics (W&Z, 1986, passim). That exploratory work may be like this is understandable. Of physics is has been said: 'The physics of undergraduate textbooks is 90% true; the contents of the primary research journals of physics is 90% false' (Ziman, 1978, p. 40). For even greater reason than for
POSITIVE ACCOUNTING THEORY

physics, that might hold of research papers in accounting. But it is odd that so much that is inconclusive should be included in a book that claims (p. 352) to represent the state of the art of the PA cult. Blame has been laid (e.g., W&Z, 1986, p. 357) on the lack of ‘rich’ theories in the fields on which it has relied; but no-one is obliged to use poor tools. The cult seems to be so enamoured of its own work and ‘methodology’ that it tirelessly boasts of its promise, disregards or denigrates the product and style of its precursors and takes scant, if any, notice of its own critics. It was the fate of Narcissus to perish from such devoted self-admiration. Or, if one disdains the lesson of the classical myth, perhaps an observation of a modern scientist deserves better: ‘Beware of the man of one method or one instrument, either experimental or theoretical. He tends to become method oriented rather than problem oriented; the method oriented man is shackled’ (Platt, 1964, p. 351).

EMPIRICALLY BASED THEORY BEFORE THE PA TWIST

As far back as Pacioli accounting has been, and it still is, represented as a means of keeping property and debt, and income and expenditure, under periodical and continuous observation for the informed management of the financial affairs of persons and firms. Whether or not a person or firm is able to pay debts, can maintain a given level of consumption or trade, is adding to a capital stock, and so on, are matters of perennial concern on which the products of accounting may shed light. But will shed light only if the figures correspond with discoverable facts from time to time. In a nutshell: informed action at any time depends on the correspondence of the information with dated facts. This proposition, a present indicative not a normative proposition, runs through the literature in various guises — some clear, some garbled, some in imperative form — as truth in accounting, realism, reliability, timeliness and like notions. It runs also through other literatures concerned with well-informed behaviour. In the form given it expresses one aspect of how the world works. Any such singular proposition may be an element in an empirically based theory. Any extended series of such propositions and legitimate inferences from them may properly be styled an empirically based accounting theory.

Much of the literature prior to the rise of the PA cult was not cast in the form of indicative (positive) propositions and inferences from them. Much was in the nature of precepts or prescriptions. That form had its origin in the modest bookkeeping manuals of long ago, and more recently in the recommendations, opinions and rules by which professional organizations sought to give guidance to bewildered practitioners. But underlying many of those precepts there are refutable but unrefuted empirical propositions. To dismiss all that preceded the PA twist as simply normative expresses either carelessness or contempt. There has long been a discursive literature which concerned itself with fundamentals expressible as positive propositions.

Take Canning, temporally the first on the B&B hit list. Canning observed (1929, p. 319) that many economists were grieved to find that the products of accounting arose from the aggregation of money amounts representing vastly dissimilar features
of the phenomena under notice. The state of affairs under complaint, he said, does exist. It still exists. His condemnation of these 'statistical absurdities' was based on the proposition: 'If individual measures are to be merged by summation or otherwise, the individual things measured...must all be sensibly alike with respect to the property...under investigation' (p. 199). The 'must' is, of course, a logical imperative, not merely an opinion of Canning. To this as a major premise may be added the minor premise: 'In accounting, individual measures are merged by summation or otherwise', to yield the conclusion: 'In accounting, individual measures to be merged must be measures of the same property'. The appearance of the imperative may be circumvented, and what is analytical may be made empirical, by casting the major premise in the form: 'Sums of individual measures of different properties of things have no empirical counterpart' and varying the argument accordingly. A positive researcher, finding accounting practice universally at odds with this theorem, might seek to explain the anomaly. The PA cult takes no notice of it.

Sweeney (1936) likewise observed what he considered a serious flaw in practice, the assumed stability of the monetary unit. There was abundant evidence of its impropriety in the Germany of 1923 and elsewhere at other times. Another of Canning's propositions dealt with it: 'If measures are to be merged, the unit of measure must either be uniform throughout the measuring, or all units must be convertible into the unit in terms of which the measures are to be merged'. Canning and Sweeney were, in the respects mentioned, clearly aware of how the world works, and of the inconsistency with it of the processes of conventional accounting.

By contrast with these elements of a theory, Sterling's Theory and Measurement of Enterprise Income (1970) was far more extensive. Sterling focused on measurement, valuation and communications problems pertinent to accounting. His conclusions were based on five measurement propositions and five valuation propositions cast in the present indicative (or positive) form, and certain information and communication propositions which are or may be cast in the same form. That is to say, all the propositions advanced are testable, and the whole exercise may quite properly be called an empirically based theory.

Chambers' Accounting, Evaluation and Economic Behavior (1966) was foreshadowed by certain testable propositions cast in the positive form in a paper of 1955. The intention was to avoid all precepts or imperatives that might conceal value-laden assumptions. The introduction to the 1966 work specified that there would be derived from the universe of experience a number of testable statements which, when arranged systematically, would 'enable us to specify the kind of accounting which will perform the functions required of it...within the universe of experience' (p. 7). That work enunciated scores of propositions cast in testable form. The major part of the work dealt with the products of accounting as contributories to informed action. But as the style of accounting that emerged from the analysis differed in material respects from practice, the final chapter, again by recourse to testable propositions, offered an explanation of how conventional accounting came to be what it was. The whole work was in substance an explanation of what is the case. It presupposed some familiarity on the part of the reader with the observable disputes, the demonstrable anomalies and the recurrent
involuntary redistributions of wealth, of which the financial press gave (and still
gives) abundant evidence. It carefully avoided recourse to all such general notions,
functions or aims as economic efficiency, market efficiency and Pareto optimality,
and all such theoretical constructs as perfect markets, perfect competition and their
like, for every one of these is unrelated to the derivation of financial information
of serviceable quality in the world as it is. Readers who failed to notice the dominant
place of testable, refutable propositions in the work have erred in describing it
as normative. The transactions, the firms, the markets and the relationships
postulated are substantially consistent with the observables of ordinary commerce.

Sterling and Chambers found no special occasion to describe their work as positive,
as distinct from the prescriptive style of other material. They were simply doing,
and expected to be understood as doing, what investigators in the sciences and
other technologies do in the ordinary course of events — observe widely, posit
generalizations and draw operable and testable inferences. The hope that that way
of proceeding might be more widely adopted, because those exercises demonstrated
that what is done in disciplined inquiry elsewhere could be done in respect of
accounting, has not been realized. But there has emerged evidence (e.g., Chambers
et al., 1987) to the effect that some 90 per cent of over 5,000 business-related
respondents in six countries would determine wealth and financial position by
reference to resale prices of assets, a conclusion of Sterling and Chambers that,
incidentally, is consistent with the method used by B&B in calculating rates of
return. And there has continued to emerge evidence of the misleading nature of
conventional information, in the unexpected collapses and bankruptices of
apparently flourishing firms.

REVOLUTION OR DEBACLE?

Of the present dominance of the work and modus operandi of the PA cult there
can be no doubt. Its doctrines have, from the outset, been propagated with
proselytizing zeal and beguiling rhetoric. Its products have attracted prestigious
awards, engaging the emulation of others. It has spawned new journals only too
willing to publish work of its kind. Its devotees, rising to editorial and referential
posts, have shifted publication opportunities in its favour; it is the avowed policy
of some journals and the implicit policy of others to disregard submissions not
of its genre. Its products have captured the allegiance of whole schools in teaching
institutions across the breadth of English-speaking communities, and perhaps
elsewhere. As one may judge from journal acknowledgments, it is a most tightly
knit and closely inbred brotherhood.

All of that is fine for the PA cult. But what of the advancement of knowledge and
understanding? Peirce (1877/1958) had some apposite words on the fixing of belief:

Let an institution be created which shall have for its object to keep current doctrines
before the attention of the people, to reiterate them perpetually, and to teach them to
the young; having at the same time power to prevent contrary doctrines from being
taught, advocated or expressed. Let all possible causes of a change of mind be removed
This method of fixing belief he called the method of authority. One hundred years later, a physicist uttered a consequential caution: 'worldly wisdom teaches us to be a little suspicious of knowledge that emanates from a unique, compactly organized and self-regarding social institution, staffed by specially trained and thoroughly indoctrinated personnel using machinery of awesome size and complexity' (Ziman, 1978, p. 64).

Of course, power may be beneficent. Is there, then, at present or in the offing, a positive accounting theory that may be associated with the PA cult? Is there a specified set of assumptions or hypotheses, general in form and potential application, that can be shown to be consistent with much that is observable in the practice and context of accounting? It seems not. Having given a succinct description of 'a theory', it might have been expected that W&Z would show clearly in what respect PA theory corresponds with the description. There is no such demonstration. Rather, although there is much talk of 'the theory', the final sentence of W&Z (1986) expresses not an accomplishment, but a hope: 'we have confidence that the methodology... will produce a useful positive accounting theory' (emphasis added). Similarly, WHZ (1988, p. xiv) hope that their book starts to answer some of the questions they pose, though they brush aside a whole roster of questions and anomalous observables that a work of its kind might be expected to deal with. It is difficult to discern a single instance (if, indeed, there is one) of a finding of the PA cult that has added a proposition to the pre-existing corpus of accounting doctrine or knowledge. Monitoring is simply the old idea of stewardship under another name; the nexus of contracts and agency ideas are recycled notions of older vintage; not even 'positive' is novel in the field.

W&Z (1979), as noted earlier, set up the existing accounting theory (strictly literature) as ventures in the production of excuses. It now seems that the PA cult has produced the excuse to outdo all excuses. The self-interest hypothesis entails that every device that any person or firm chooses to employ is justified, excused. It follows that all the logical and practical flaws that have occupied the attention of practitioners, academics, lawyers, administrators and others are excusable, and objections to them otiose; 'no one is fooled by accounting' is the slogan forever (e.g., W&Z, 1986, pp. 21, 343). There can no longer be legitimate criticism of the utterances of professional bodies or standards boards or legislatures, for what they have done and do is done for good self-serving reasons. And perhaps the accounting curriculum should be reduced to a two-word program: 'Please yourself'.

As for the advancement of knowledge, it sometimes seems as if the clock has been set back a thousand years. WHZ (1988) averred that, where there are state-contingent pay-offs to individual productive factors (which is universally the case in respect of some parties in corporate business), 'these states are invariably defined in terms of the firm's financial position or performance'. Then: 'We argue that traditional accounting serves this role — that the historical cost system provides a basis for financial contracting... In this sense, accounting, rather than being a
faithful representation of some underlying "true" (but unobservable) economic reality, becomes reality itself" (p. 84). Here is a pretty pass. We look back, with scorn or amusement, to primitive ages when men accepted figments of the imagination, myths and goblins as reliable knowledge. We suppose the scientific revolution to have disposed of that. But it is still with us. The foibles and fictions of conventional accounting become 'reality itself'. What is said to be, actually is, and the world must put up with it. Of such things Lerner (1960) wrote:

Whereas the function of research is to test social theory by observation of reality, the function of agitprop [agitation and propaganda] is to make reality appear to conform to the ideology. One does not consult the man who wears the shoe to learn where it pinches; one tells him that it must pinch to fit better later — or that it does not pinch at all. (p. 24)

The PA cult tirelessly counsels theorists and researchers to observe what is the case; but the practical world, in which what is the case is of urgent and ineluctable significance, is excused altogether from an equivalent admonition!

DOUBLE TALK AND PARADOX

The literature and practice of conventional accounting is a classic example of double think and double talk after the pattern described by Orwell (1955):

Double think means the power of holding two contradictory beliefs in one's mind simultaneously, and accepting both of them... The process has to be conscious, or it will not be carried out with precision, but it also has to be unconscious, or it would bring with it a feeling of falsity and guilt... to forget any fact that has become inconvenient, and then, when it becomes necessary again, to draw it back from oblivion for just so long as it is needed, to deny the existence of objective reality and all the while to take account of the reality one denies — all this is indispensably necessary. (p. 171).

Conventional accounting, it is claimed, yields true and fair information on financial position and results; what it produces has neither of those features. It claims to be based on the accrual principle, but plays fast and loose with that principle as it pleases. It claims to yield information pertinent to decision making, but yields information of a kind that no reasonable person would use. It claims to be realistic, but liberally uses fictions that misrepresent reality. It claims to be historical, but at many significant points relies on conjectures about the future. And so on.

The work of the PA cult is essentially similar. PA theory sets up individual self-interest as the dominant explanans of accounting practice; it also sets up a contracting-monitoring model of the firm in which self-interest is necessarily constrained. It purports to deal with accounting, but ignores large tracts of the territory; the only applications it deals with at any length are compensation and debt contracting. When convenient it claims to be able to identify the impact of a single cause when there are joint causes; and when convenient it denies the possibility. It purports to be positive, descriptive; but it endorses conventional
accounting which is observably and dominantly prescriptive. It claims to be scientific, but in many ways is non-scientific or anti-scientific; what could be more anti-scientific than the hubristic contention: 'not only is there no generally accepted accounting theory to justify accounting standards, there never will be one' (W&Z, 1979, p. 301)? It claims to be empirical, but the limited range of observables it notices are largely determined by the method used rather than the matter to be examined. The PA cult claims to present theory or a theory, but it actually presents an unsystematic set of propositions liberally buttressed by *ad hoc* addenda. Alluding to much work in psychology, Dunnette (1966) described it as

> 'folderol... tendencies to be fixated on theories, methods and points of view, conducting little studies with great precision, ... asking unimportant or irrelevant questions... coining new names for old concepts, fixation on methods and apparatus, seeking to prove rather than test theories, and myriad other methodological ceremonies conducted in the name of rigorous research.' (p. 343)

It's a caution deserving notice.

Conventional doctrine and the PA doctrine alike foster, not well-informed interaction in the practical and academic worlds respectively, but illusions. In moments of ease and relaxation the world is vastly entertained by illusionists; but no-one believes that the illusionist's assistant is really cut into halves, or disappears forever. In practical affairs, and in training for practical affairs, the world does not expect to be fed illusions when it needs facts. Unhappily, that is what it still must tolerate, and pay heavily for the privilege. For much of the PA literature provides, not a guide to the production of generally serviceable information, but a catalogue of the games people will play, and rationalize, when only they have power to make and change the rules.

Brown (1989) has claimed, with some pride: 'it is now hard to imagine any proposal to change U.S. accounting reporting requirements that will not have to run the gauntlet of a market-based test, as part of the routine evaluation of its economic consequences' (p. 207). But how well have generally accepted accounting principles fared, notwithstanding the test? 'The accounting standard setting process is in deep trouble, possibly in such deep trouble that our present structure is irretrievably lost to us. After seventeen years, the Financial Accounting Standards Board lies dead in the water beset by critics on all sides' (Burton and Sack, 1990, p. 117). 'The accounting profession is being battered again... the profession continues to be harried by litigation... public suspicion of its credibility, demands that the accounting standard setting process be reformed, and blame for every financial catastrophe' (Sommer, 1990, pp. 114-15) — this from the chairman of the Public Oversight Board of the SEC Practice Section of the American Institute of CPAs. These disastrous outcomes were all predictable from the contentions of the critics of generally accepted accounting principles twenty-five years ago. But Brown still contends that the grounds of criticism were, and still are, 'insufficient grounds for rejecting reports prepared under generally accepted accounting principles' (p. 204).

The whole PA venture in fact constitutes a colossal paradox. Its founding fathers claimed to put under inquiry the *income numbers of firms*; they and their successors
actually have had recourse to the imputed income numbers of investors. All inquiries using the procedures of B&B (1968) employ a rate of return consistent only with exit value or money equivalent accounting; notwithstanding that, the cult emerges as a champion of cost-based accounting. The cult professes to be averse to prescription; but it endorses, implicitly and explicitly, traditional cost-based accounting, the doctrines of which are full of unsupportable prescriptions. It lays the ground in the contracting–monitoring hypothesis for an up-to-date and intelligible accounting, but supports conventional accounting which has neither of those characteristics.

The greatest paradox of all is that such grossly paradoxical beliefs survive and flourish so luxuriantly in institutions dedicated to the cultivation of open inquiry and disciplined thought, and the elimination of the wayward paradox.

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