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The social scientific turn in UK financial accounting research: a philosophical and sociological analysis

Brian A. Rutherford*

Abstract — The demise of the classical programme of financial accounting research is generally represented as a progressive development. This paper argues that the academy's abandonment of classical methods was justified neither by the fruitfulness of post-classical programmes nor by their incontestable epistemological superiority. Rather, what occurred was a turn to mainstream social science, reflecting sociological characteristics of the UK financial accounting research community. The paper concludes with a call for a revival of the classical programme.

Keywords: classical accounting research; epistemology; history of accounting research; sociology of accounting research

1. Introduction

In the opening words of an introductory text on the subject, 'there is nearly universal agreement that science [that is, natural science] is a progressive discipline' (Losee, 2004: 1). Depictions of the history of financial accounting research, too, are typically cast in progressive terms, often taking it for granted that the discipline is a social science, at any rate in its scholarly mode (Beattie, 2005: 87; Riahi-Belkaoui, 2004: 40).

In this paper I suggest a different perspective. I argue that the move from 'classical approaches to addressing questions' and 'traditional normative research' (Zeff, 1989: 171) to contemporary programmes, such as neo-empiricism (Henderson et al., 1992) and the various 'ways of seeing' (Roslender, 1996: 542) embodied in radical research, marks a rupture in the discipline. That the UK financial accounting research community now draws its methods almost exclusively from the mainstream social sciences, and that 'academics have largely disengaged from traditional normative theorising in relation to financial statements' (Beattie, 2005: 93), is not in dispute, but I argue that financial accounting research is social scientific *tout court*, not because it was always so, and moving progressively towards its current level of sophistication, but because the academy broke with

its past and made a turn to mainstream social science.¹

If we follow Reiter and Williams (2002: 575) in taking progress in scholarship to be 'defined as innovation and relevance', the adoption of the methods of mainstream social science would be progressive if the bounty of innovative and relevant findings accruing from the new approaches substantially exceeded that from classical research in the phase before its abandonment. Alternatively, the move might be seen as progressive if the epistemological position of the classical programme had been demonstrated to be fundamentally flawed.² However, I argue that neither of these conditions is satisfied. It is important to emphasise that I am not arguing that contemporary programmes are invalid in their own terms but only that their results do not justify the abandonment of the classical programme – that its abandonment did not represent a progressive move but rather a turn to new forms of inquiry.

If it was not a progressive move, why did the turn to mainstream social science come about?

¹ It could reasonably be argued that, since financial accounting is a form of social practice and financial accounting research, and probably even financial accounting practice, should aspire to be scientific, financial accounting is a social science. Classical accounting research certainly borrowed – and adapted for its own use – ideas from economics. The argument of this paper relates to the adoption by financial accounting researchers of the methods of the mainstream social sciences (particularly economics and sociology) rather than merely the importation of their ideas modified for the particular demands of research into financial reporting systems. In the interests of brevity, however, I do not always include the term 'mainstream'.

² It is not suggested that epistemology is a matter of choice or that research should be defended by reference to epistemology but only that fundamental flaws in the epistemological position of a programme would constitute grounds for moving on.

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Discussing the development of one branch of neo-empiricism in the USA, Whitley (1988: 631) has argued that ‘doubts about the epistemological and practical value of “positive accounting theory” lead to an alternative, sociological, explanation of its growth and institutionalisation’. I suggest that doubts about the progressive nature of the transition to contemporary modes of research in general argue for a sociological explanation of this wider phenomenon and I sketch out the beginnings of such an explanation.

The paper’s contribution to the literature is fourfold. First, it argues that two decades of UK neo-empirical research since the turn to social science have not resulted in the refinement of classical theory that Whittington (1986) held out hope for. Rather, Chambers (1990) and Sterling (1990) were right that neo-empiricism represented an abandonment of the classical programme’s objects of inquiry. Further, work on the new objects of inquiry has achieved limited success. Second, it draws attention to the radical programme’s similar concern with objects of inquiry other than those of the classical programme – and its equally limited success. Third, it argues that the turn to social science requires a sociological explanation and begins the development of such an explanation. Finally, in the light of the circumstances in which the classical programme was abandoned, it calls for a revival of the programme.

The paper follows the lines of the argument set out above. The next section examines the orthodoxy of the transition to contemporary modes of research, demonstrating its claim to progressiveness. The following sections discuss the fruitfulness of the two major contemporary programmes and the epistemological position of the classical programme. Next, the turn to mainstream social science is plotted in terms of the objects of inquiry for financial accounting research. A sociological account of the forces that brought the turn about is then sketched. The final section discusses the implications of the arguments for the future of scholarly financial accounting research.

My primary focus is on the UK research community. However, it is impossible to disentangle epistemological and sociological developments in the UK from those in the USA because of the intimate relationship between the two communities (Beattie, 2002: 95; Whittington, 1981a: 24). This is especially so in the case of neo-empiricism, where there is a strong tendency for the UK to follow the USA – indeed to ‘replicate ... the thrust of US studies’ (Beattie, 2005: 97). Sociologically, the case of the UK is perhaps of more interest, partly because

neo-empiricism was resisted for longer and partly because the UK has adopted a more epistemologically tolerant attitude, while nonetheless fully embracing the social scientific turn.

2. The orthodoxy of the transition to contemporary modes of research

Beattie’s (2002) widely drawn outline of the history of financial accounting research exemplifies the orthodoxy of the transition to contemporary programmes. It focuses on the UK context (Ryan et al., 2002: 1) while reflecting the powerful influence of the USA. Where it overlaps with Whittington’s earlier historical sketch (Whittington, 1986) it follows much the same approach.

Beattie begins by explaining that ‘financial accounting research has gone through a number of distinct phases’ (2002: 94). Early phases involved ‘attempting to generalise about the principles underlying observed practices’ (2002: 100), followed by the search for ‘a measure of “true income”’ (2002: 101). She then suggests that ‘these different approaches began to converge after the Second World War’ (2002: 100). This represents a claim of ‘progress as incorporation’ (Losee, 2004: 5). As a result, and here she calls on Nelson’s much-employed encomium, ‘the decade of the 1960s was a golden age in the history of a priori research in accounting’ (Nelson, 1973: 4).³ The next phase was stimulated by a development in accounting practice, namely increased attention to standard setting, which triggered a search for ‘ways of selecting among the alternative income determination models which had been developed by a priori accounting researchers’ (Beattie, 2002: 102). The approach favoured during this phase was decision usefulness: shareholders were held to need information about future cash flows and ‘the financial statements with the greatest predictive ability would best meet the information needs of shareholders, and choices between alternative accounting methods could be based on an assessment of their effects on predictive ability’ (2002: 102).

Beattie’s characterisation of the adoption of decision usefulness has a progressive tone: it ‘succeeded in moving accounting research away from the search for “true income”’ (2002: 101). Elsewhere (Beattie, 2005) she has described the

³ An a priori concept, statement or judgment is one held prior to experience. Nelson explicitly disclaimed credit for introducing the phrase ‘a priori research’ into the debate; this was done, apparently, by the organisers of the conference to which he was contributing, who did not define it but provided some examples (Nelson, 1973: 3), namely works by Chambers (1966), Edwards and Bell (1961), Sterling (1970) and Ijiri (1967).

rejection of 'true income' theory in terms which suggest that it was the result of anomalies arising under the theory:

'Gradually, however, the limitations of the income model became increasingly apparent. It was unable to offer guidance on the appropriate accounting treatment of thorny accounting issues such as pensions and goodwill' (2005: 88).⁴

The elimination of anomalies is a common test of progressiveness in natural science (Losee, 2004: 157). Whittington's earlier outline also suggests that the transition from the golden age was progressive:

'There was disillusionment with the grand a priori theorising of the 1960s ... It was felt that the way to resolve such a debate was by resort to empirical evidence, to establish which theories had the most realistic assumptions or the greatest capacity to predict observed events' (Whittington, 1986: 24).

While the 1960s may have marked the high point of a priori research in the USA,⁵ a priori income theory continued to flourish in the UK into the 1970s. Indeed, it was 'central to the UK research effort' (Beattie, 2002: 98) during this period of rapid growth in the British academic accounting community. Whittington's 1981 survey of the British contribution to income theory traces its development through the 1970s, explicitly limiting the account to a priori theorising (Whittington, 1981a: 1), and concludes that, 'it seems likely that a flow measure, or measures, analogous to income will play an important role in the *theory* and practice of financial accounting for a considerable time to come' (1981a: 24, emphasis added).

Unfortunately decision usefulness 'did not succeed in providing the logical basis for accounting choices which its advocates had hoped for' (Beattie, 2002: 102). Beattie nonetheless credits it with progressive consequences because it 'stimulated two principal types of empirical study' (2002: 103) which constitute the next phase of research. These continue to be undertaken today, and one, market-based research, constitutes the largest area of 'conventional' financial accounting research in the UK today (Beattie, 2005, Table 2).⁶ Moreover, contemporary UK researchers have largely with-

drawn from traditional financial accounting theorising (Beattie, 2005: 93). Hence it would seem reasonable to characterise this phase as the beginning of modern research in financial accounting.

One approach described by Beattie is the 'positive accounting theory' associated mainly with the USA (2002: 106–109). This is 'a particularly extreme form of empiricism' whose 'advocates ... distance themselves as far as possible from the normative methodologies of the a priori theorists' (106). Although Beattie treats it separately from market-based research, the work of this school is strongly bound up in the emergence of the new wave of empiricism and Whittington (1986: 25–27) explicitly includes it within a broader classification of empirical research referred to in this paper as neo-empiricism (Henderson et al., 1992). The neo-empiricist programme emerging in the USA in the late 1960s rapidly achieved hegemonic status in that country (Reiter and Williams, 2002) and orthodox accounts of US historical developments (Beaver, 1998; Scott, 2003; Wells, 1976) naturally regard the move as progression in the form of revolutionary overthrow (Losee, 2004: 157).

The way in which Beattie (2002) contrasts golden age theorists and modern empiricists has distinctly progressive overtones: 'the a priori researchers conducted no formal testing of their deductively derived conclusions ... the empirical researchers, however, formally tested their hypotheses' (2002: 106), a description which, read in the context of orthodox accounts of scientific method, would certainly indicate an advance. Further, in Beattie's view empiricism has been 'very productive' (2002: 112) and she concludes that contemporary conventional accounting research 'is now a more balanced combination of theory and empirical analysis', representing 'a new level of maturity' (2002: 112). Her account of conventional financial accounting research concludes by welcoming an 'important recent trend' which has seen 'the coming together of, arguably the best of' several perspectives (progress as incorporation), namely a 'return to the measurement perspective' as exemplified by residual income valuation modelling (2002: 109).

Beattie also discusses the principal contemporary alternative to neo-empiricism, which I will refer to as the radical accounting research programme.⁷ This responds to 'calls for researchers to study the

⁴ The title of this article tells us that its subject is the UK's contribution to 'moving the financial accounting research front forward', an apparently progressive claim.

⁵ Although one of the most distinguished was Australian, most of the theorists involved were American (Gaffikin, 1988).

⁶ The term 'conventional' is used in the source to exclude 'research grounded in critical perspectives' (Beattie, 2005: 86, note 1).

⁷ Beattie mainly uses the terms 'interdisciplinary perspectives' and 'critical accounting research' (2002: 110, for example) although she does also refer to 'radical' (110) studies. Other advocates of the approach use the term 'radical' (see, for example, Chua, 1986). I have adopted a single 'catch-all' term merely in the interests of brevity.

social nature of accounting and, in particular, to examine the role of financial reporting in its economic, social and political contexts' rather than base research on 'an acceptance of the existing social order' (2002: 110). She claims here, too, to identify 'a new level of maturity' (2002: 112).

Beattie and Davie (2006) provide a review of the literature on the development of accounting thought (covering practice as well as scholarly research), stretching from the invention of double-entry bookkeeping to the present day; they examine two Kuhnian studies of the transition from classical accounting research (Cushing, 1989; Mouck, 1993) and place a progressive gloss on their findings, against the interpretation of the original authors (see the end of Section 5). Surveys of accounting research aimed at undergraduates, even in the USA, take a more ambivalent line about the move to contemporary modes of research than sources, such as Beattie (2002), directed at postgraduates and academics. Probably the best known, that by Riahi-Belkaoui (2004), borrows Ritzer's model of sociology as a 'multiple-paradigm science' and includes among the paradigms 'striving for acceptance' (Riahi-Belkaoui, 2004: 336) the true income paradigm and the even earlier anthropological/inductive paradigm. A survey aimed at the UK (Deegan and Unerman, 2006), argues that 'there is a role for prescription' (2006: 14) and discusses a number of theories from the golden age; indeed, it implies that the inductivist approach remains valid (2006: 7). Presumably undergraduate texts find it necessary to treat classical approaches as valid so as to square their account with the continuing use of such approaches in accounting practice, including standard-setting.

3. The fruitfulness of contemporary programmes

Beattie (2002) concludes that two modes of research now 'dominate the literature' (2002: 112), the empirically-based programmes of neo-empiricism and behavioural accounting and a radical programme. Her survey of contemporary 'conventional' (i.e. non-radical) British research shows that market-based accounting research 'dominates' the field (Beattie 2005: 92). She categorises just over a quarter of studies as within this programme (Table 2) but a number of her other categories, including disclosure (20%), accounting choice (5%), earnings management (4%), and economic consequences (3%), are essentially neo-empirical (see Parker, 2007: 42). Accordingly, consideration of the fruitfulness of contemporary programmes

will, for reasons of space, be limited to neo-empiricism and the radical programme.

3.1. *The neo-empirical programme*

In 1991, as the momentum of the move away from classical research was becoming irresistible in the UK, a number of leading neo-empiricists in the USA met under the auspices of the American Accounting Association (AAA) to produce a report on what they saw as the 'widespread sense among accounting researchers and practitioners that academic accounting, particularly on the research level, currently faces a serious crisis' (Demski et al., 1991: 1).⁸ Since neo-empiricism had dominated research in the USA since the 1970s, this crisis was a crisis of neo-empiricism. Among the symptoms of crisis the group identified were that, whereas 'most academic research areas are characterized by cycles of significant innovations ... innovations in accounting research are practically non-existent'; and that, 'despite considerable research effort, it does not seem that we are any closer now than we were 20–30 years ago to addressing the fundamental issues in accounting, such as the optimal choice of accounting standards and the optimal structure of accounting institutions' (1991: 1–2).

Though neo-empiricism may lack cycles of innovation, it is possible to discern cycles of a different sort. A characteristic of the programme is its apparent potential for the development of an integrated and cohesive theory (Peasnell, 1981: 107–108). It is thus unsurprising that highly regarded neo-empiricist scholars have devoted considerable time to producing periodic 'stock-taking' reviews; such reviews have been published at intervals of roughly a decade since the 1960s.⁹ They reveal a cycle of optimism about the prospect of innovation from one source being replaced by criticism once its ideas are found wanting – accompanied by equally buoyant optimism about another potential source.

The earliest stock-taking was a contribution to the conference that received Nelson's 'Golden Age' paper. Hakansson (1973: 160) found empirical research 'still in its infancy' and devoted his concluding section to a discussion of predictive ability, which he thought offered 'great promise'

⁸ I am indebted to Professor Paul Williams for supplying me with a copy of the report.

⁹ Like the programme itself, the surveys are dominated by US work, but since this work leads the way, the reviews have significance beyond the USA and are thus employed in this paper. Most of the reviews cited by Beattie's UK-oriented surveys of methods and studies (Beattie, 2002, 2005) are likewise of US origin. A review covering British findings is discussed towards the end of this section.

(1973: 160). In 1982 the *Journal of Accounting Research* published a series of literature reviews: two dealt with empirical research in financial reporting, one wide-ranging (Ball and Foster, 1982), the other (Lev and Ohlson, 1982) devoted specifically to what was now called market-based research. Predictive ability fell within the remit of Ball and Foster (1982). They described work in this area as having achieved ‘limited progress to date’ (1982: 215) and argued that ‘the most articulated paradigm guiding the empirical research surveyed ... is associated with the “stewardship-contract monitoring” literature.’ But ‘the results of empirical exercises guided by this paradigm [had] not been impressive’ (1982: 191). Like Hakansson (1973) a decade earlier, their explanation for this disappointing state is that it ‘reflects the early stage of the development of this paradigm’ (1982: 191).

The survey of market-based research concluded that:

‘early studies appearing to indicate investor rationality have given way to discomfiting findings. It is now clear that the existence of some investor irrationality cannot be precluded ... When the scope of inquiry is extended to include effects on management compensation and contractual arrangements, it appears that almost all accounting changes can have real effects ... Research on all these important issues is in its infancy, and results are far from conclusive’ (Lev and Ohlson, 1982: 250).

There was, however, one bright note: ‘for the stock market consequences of accounting regulation, it is comforting to conclude that ... results appear to be consistent and even conclusive’ (1982: 250).

The next survey was presented at another Illinois conference. In connection with the comment on accounting regulation, quoted immediately above, one author wrote, ‘I disagree with the Lev-Ohlson conclusion ... because it is an overstatement of the results’ (Dopuch, 1989: 49). The review of capital markets studies reported that few had been published since 1985 (Bernard, 1989: 73) and the editor’s summary of the review concluded that it showed that, ‘in spite of several decades of serious research, it is clear that we still know very little about the complex workings of modern capital markets’ (Frecka, 1989: 14). Another paper (Abdel-khalik et al., 1989) concluded as follows:

‘First, evidence about the income-smoothing hypothesis continues to elude researchers ...

Second, corporations and accounting firms do lobby for their preferred accounting standards ... [but a]gain, hard evidence is difficult to obtain ... Third, the role of contractual arrangements in affecting the choice of accounting methods and accounting accruals has been brought to the forefront ... [but t]he state-of-art in this area has progressed somewhat slowly’ (1989: 175).

It is not difficult to see why the AAA group reached the conclusion it did in 1991. But have things improved since? The most recent general survey occupies two full volumes of the *Journal of Accounting & Economics*. A review of capital markets research by Kothari (2001) offers no overall evaluation but his discussant identifies market efficiency as the ‘watershed issue’ (Lee, 2001, emphasis suppressed) and, on this, Kothari concludes that, although ‘we do not observe systematic, large differences in the prices of firms employing different accounting methods’ there is evidence that, ‘over long horizons differences in accounting methods produce measurable differences in risk-adjusted stock returns. Whether these abnormal returns suggest a modest degree of market inefficiency or they are a manifestation of the problems in accurately measuring long-horizon price performance is unresolved’ (2001: 199). This conclusion is very close to that reported by Lev and Ohlson (1982) 20 years earlier.

A second paper deals with accounting choice (Fields et al., 2001). Its authors ‘conclude that there has been, at best, modest progress in understanding the motivations for and consequences of accounting choice, with the rate of progress slowing in the last decade (Francis, 2001: 317). A third is devoted to value-relevance (Holthausen and Watts, 2001). According to Beattie:

‘The association between accounting numbers and equity market values ... received renewed interest during the 1990s, under the label “value relevance”. There is often a suggestion in the literature that such studies can help standard-setters by indicating the “usefulness” of various accounting numbers’ (2002: 105).

But the reviewers’ conclusion, arrived at after this ‘renewed interest’, was that, ‘while the ... value-relevance literature is large, its contribution to standard setting seems modest’ (Holthausen and Watts, 2001: 63).

The usefulness of its output to accounting regulators has always represented an important measure

of fruitfulness for neo-empiricists.¹⁰ The discussants of the 2001 review on value relevance did not agree with its conclusion, arguing that ‘the value relevance literature provides fruitful insights for standard-setting’ (Barth et al., 2001: 98). In the light of this opinion, it is illuminating to examine a paper published by one of those discussants, Barth (2000), setting out, just a little before the review paper, what she saw as the ‘implications for financial reporting’ of valuation-based accounting research. She addresses four topics. On the first she concludes, ‘what is left to learn about fair value accounting? Much’ (2000: 21). On cash flow versus accruals she again concludes that, ‘there is much left to learn’ (2000: 23). On the third issue, she reports that:

‘Research to date on issues related to recognition versus disclosure is rather limited. It is difficult to obtain clear insights on this issue from empirical research because there are virtually always confounding effects ... What is left to learn? Even more than for fair value accounting or cash flows versus accruals’ (2000: 24).

Finally ‘research indicates that measuring harmonisation is difficult ... What is left to learn? Almost everything we would like to know’ (2000: 25–26). Given her views, we can take it that Barth chose these topics specifically to give a favourable impression of the merits of the literature.

By the turn of the century, the cycle of optimism and disillusionment can be discerned within sub-areas of the literature. For example, recall that in 2002 Beattie considered that residual income valuation modelling represented an important recent development combining the best of several approaches (see Section 2). She particularly singled out Ohlson’s ‘clean surplus’ approach (Beattie, 2002: 109). Yet, at more or less the time her paper was being drafted, Kothari’s review was pointing out that the way it is constructed ‘renders the Feltham-Ohlson model devoid of any accounting content ... because the model does not offer any guidance or predictions about firms’ choice of accounting methods or properties of accounting standards’ (Kothari, 2001: 177). A more recent survey specifically devoted to accounting-based valuation models reported that,

‘Some empirical researchers have become disenchanted with [residual income, insofar as it deals

with reported accounting numbers rather than forecasts], pointing to the apparent misspecification of the linear information dynamics of the ... models ... Further, attempts to modify [them] have not improved the valuation relation performance of models ... Finally, every attempt to date to resolve the other information conundrum by substituting observables for unobservables has led to the realization that omitted variables remain’ (Richardson and Tinaikar, 2004: 246).

Again, the section of Holthausen and Watts’ review (2001: 64–66) offering suggestions for future research devotes more space to conservatism than any other topic, specifically citing studies by Basu (1997) and Ball et al. (2000). Yet a recent review of work on conservatism provides evidence that results from a range of studies, including the two papers cited by Holthausen and Watts, ‘are attributable to biased test statistics rather than to conservatism’ (Dietrich et al., 2007: 96).

The sole non-US survey (Dumontier and Raffournier, 2002) cited by Beattie (2005) covers the whole of Europe but as there is evidence of a ‘British hegemony over Europe-based accounting research’ (Cámara et al., 1999: 473) it seems likely that its findings largely reflect the state of the British literature. Its authors conclude that the studies surveyed ‘provide little evidence useful to standard-setting bodies for the assessment of accounting standards or to managers in forming disclosure strategies to communicate effectively with investors’ (Dumontier and Raffournier, 2002: 145).

On any realistic appraisal, then, reasons to be cautious about the reliability of neo-empiricism’s innovations and the relevance of its findings to financial reporting were apparent from the early 1980s.¹¹ A decade later, as we saw at the beginning of this section, even distinguished proponents of the programme were expressing their doubts. Subsequent contributions to the literature give little reason to revise an attitude of caution.

3.2. *The radical programme*

Interdisciplinary, interpretive, critical or radical accounting researchers subscribe to a number of different ‘world views’ (Chua, 1986) or ‘ways of seeing’ (Roslender, 1996: 540) and, indeed, disagree about which ‘ways of seeing’ qualify for which titles (Roslender and Dillard, 2003: 326; Ryan et al., 2002: 41–44). Although radical

¹⁰ Members and staff of the US Financial Accounting Standards Board have from time to time expressed the view that research actually produced has been of limited relevance to their task and called for classical research: see, for example Leisenring and Johnson (1994) and Van Riper (1994: 52–53).

¹¹ The programme has attracted extensive epistemological critique (Reiter and Williams, 2002: 592, cite 21 studies, adding that this represents ‘by no means an exhaustive list’); five critiques appeared as early as 1982 and 1983, and one of these was by British authors (Lowe et al., 1983).

researchers often claim to be excluded from the mainstream of accounting research¹² in fact, as Beattie points out (2005: 86, note 1), “critical accounting” is increasingly entering the mainstream outside the US’.

Roslender and Dillard (2003) trace the emergence in the 1970s of a radical accounting project, drawing on the critical sociology of Marxist theory and the work of the Frankfurt School, and its displacement in the mid-1980s by a Foucauldian perspective (see also Gendron and Baker, 2005). Non-Foucauldian approaches survive but earlier influences have been joined, and are now dominated, by postmodernist and post-structuralist perspectives (Roslender, 1996: 541). In its heterogeneity, radical accounting research simply resembles, of course, the wider domain of social theory (Ritzer and Smart, 2001: 4). Ritzer, who introduced the notion of sociology as a ‘multiple-paradigm science’ (1975), argues that sociology has now become even more diverse: ‘the result is a wide open theoretical world, one that is so unrestricted and contested that it borders on, if it has not already descended into, chaos’ (Ritzer and Goodman, 2004: A16–17). Radical accounting researchers even disagree about what response the existence of a multiplicity of perspectives should provoke. Is the appropriate attitude, ‘tolerance, willingness to listen, and respect for alternative views’ as advocated by Merino (1998: 603), or should proponents of one school slug it out against all-comers, an approach certainly practised by, for example, Tinker (2005)?

The variegated – perhaps chaotic – nature of the radical programme makes it difficult to judge its results in the round. There is an abundant literature critical of every aspect of the social theorising on which the radical programme draws, from its most general epistemology (see, for example, Nagel, 2001) to the detailed content of its individual positions; for critiques broadly sympathetic to the radical project, see, for example, Best and Kellner (1991) and Sarup (1993). Perhaps the most fundamental challenge is offered by Nagel (2001: 15): ‘It is usually a good strategy to ask whether a general claim about truth or meaning applies to itself’. It is by applying this test to the work of Foucault and his disciples, the dominant school within radical accounting research from the mid-1980s according to Roslender and Dillard (2003), that Sanbonmatsu (2004) is able to write in the following terms:

¹²For example, Roslender and Dillard (2003: 326) say that, ‘from the outset, research perspectives beyond that of neo-classical economics have been, and continue to be, viewed unfavourably by those who constitute the gatekeepers of accounting knowledge’.

‘Foucault’s refusal of the *Lebenswelt* – the quotidian world of meaning ... reduces the first-order meanings of human civilization – including its *sophia*, its wisdom – to the status of shadows on a wall ... [T]he only autonomous “being” not duped into mistaking the shadows of “experience” for experience, is the archaeologist [i.e. the theorist] herself ... “Experience” only becomes intelligible, *real*, when a genealogist like Foucault or Scott or Spivak is on hand to observe it’ (2004: 111–13).

Gendron and Baker (2005) use a Foucauldian ‘sociology of translation’ approach to model the dissemination of Foucauldian ideas within accounting research, arguing that ‘academic endeavours are subject to tension between originality (differentiation) and imitational conformity’ (2005: 536), with the adoption of a Foucauldian perspective by a few scholars representing the initial act of originality. But the tension between innovation and imitation might equally well be cast in terms of a purely imitative adoption of any original perspective from social theory and attempts to expand and enrich it by examining accountancy itself. Thus, when Macintosh (2002) concludes his book-length review of ‘poststructuralist positions’ on accountancy with the insight that,

‘Nietzsche wrote, “‘Truth’ is therefore not something there, that might be found or discovered – but something that must be created and that gives a name to a process, or rather to a will to overcome that has in itself no end ... It is a word for the ‘will to power.’” Could it be, then, that accounts which are deemed to be “true and fair” or “presented fairly” are also only words for the will to power?’ (2002: 134, original citation omitted),

he is claiming nothing more than that, supposing Nietzsche to be right about the world at large, his (Nietzsche’s) characterisation holds true for accountancy.

Armstrong (1994: 38) puts the point thus: it ‘has been typical of much Foucauldian accounting research’ that it proceeds ‘only outwards from Foucault’s concepts and insights, rather than back into them after an encounter with empirical data’. Commenting more generally on the radical programme, Humphrey (2001: 93) talks of ‘this privileging of social theories ... which never saw such theories being altered by their exposure to accounting’ and goes on to ask ‘how many [studies] are needed before it can be accepted that accounting is socially constructed, paradoxical, bound up with

power relations and has unintended consequences? Of course, studies that demonstrate that the insights of social theory apply to accountancy have some use, though they suffer the same flaws, if flaws there be, of their root theory, a point demonstrated in the context of Foucauldian accounting research by Armstrong (1994: 28–29).

It may, indeed, be questionable whether the root theories do actually apply to accountancy. Armstrong points out that claims to offer analyses of accounting systems as Foucauldian disciplinary regimes, such as the famous study of standard costing by Miller and O’Leary (1987: 31), collapse the distinction between ‘moulding the actual details of individual conduct’, a central concern for Foucault, and the potential offered by both management and financial accounting for leaving methods unspecified, provided desired results are achieved. Again, attempts to employ the Foucauldian notion of the ‘disciplinary society’, ‘basically a two-epoch model’ (Armstrong, 1994: 34), to explain relatively small-scale accounting change, as in Hopwood’s equally famous study (1987), stretch the concept in a way that, Armstrong concludes, does nothing to ‘illuminate the question of whether a consideration of accounting as power-knowledge has anything to contribute to an understanding of accounting change’ (Armstrong, 1994: 34).

According to Armstrong (1994: 38), one of the more ingenious Foucauldian studies in financial accounting is that by Hoskin and Macve (1986). This argues that double-entry bookkeeping was translated from various ‘gridding’ practices in early 13th century scholasticism, such as the numbering of paragraphs within texts. Yet the establishment of a genealogical connection depends on demonstrating that the practices have more in common than others excluded from consideration, which Hoskin and Macve do not do (Armstrong, 1994: 49); although they do demonstrate that the group supposedly implicated in the extension of the practices to accountancy was exposed to the alleged prototypes, the evidence that its members perceived the various practices as similar is only circumstantial (Armstrong, 1994: 49). In any event such a biographically-focused approach appears inconsistent with Foucault’s own decentring of the subject.

Some social theorists proceed by ‘advocating “what *ought* to be out there” instead of uncovering “what *is* out there”’, so that ‘sociological theorising becomes “a mode of altering reality, not by the direct application of energy to objects, but by the creation of discourse which changes reality through the mediation of thought and action”’ (Zhao, 2001:

391, quoting Bitzer, 1968). In this case, ‘the success of theorising is marked by the actualisation of what is advocated rather than by the verification of what is uncovered’ (Zhao, 2001: 391). This approach is, naturally, to be found within the radical accounting programme, with some taking it to be the defining characteristic of its truly ‘critical’ branch, now to be known as ‘enabling accounting’ (Broadbent et al., 1997), and finding themselves ‘troubled by the motivations of those who wish to be identified as critical accountants but who have no inclination to be associated with the political dimension of the project’ (Roslender and Dillard, 2003: 327). For those who ‘regard “reporting” as the *sine qua non* of accounting’ (Tinker, 1999: 646), the challenge is to find new financial statements that will secure the desired social change. Some have risen to this challenge, but the results generally appear banal. The finale of Macintosh’s book, cited earlier, is a proposal for what he calls ‘heteroglossic accounting’, following Bakhtin’s conception of the heteroglossic novel, which ‘gives equal weight to the voices of both the characters and the author’ (Macintosh, 2002: 129). Its aim is to ‘produce a report that allows the various “voices” currently embedded, but muffled[,] in the monologic report to “speak”’ (2002: 131). Macintosh’s example of such a report records the results of oil and gas operations using all four well-known accounting policies advocated for use within the sector, that is, immediate write-off, full costing, successful efforts and reserve recognition (2002: 131–132). But such a report can be thought of as an example of multi-column reporting, as advocated by Professor Edward Stamp (1981) a quarter of a century ago. Lest Stamp be thought a closet postmodernist, it should be pointed out that the UK profession advocated research on multi-column reporting in 1975 (ASSC, 1975: para. 7.40). Macintosh’s proposal thus seems to achieve by radical theorisation what can perfectly well be arrived at by conventional means.¹³

The paucity and banality of the proposals offered by radical theorists accepting the challenge in the terms set out above is, for other radical theorists, simply a consequence of the technocratic and reductionist nature of those terms (Tinker, 1999: 646 and note 6). Even apparently quite fundamentally grounded platforms for change are vulnerable to the charge of reductionism. As a case in point, we can examine Lodh and Gaffikin’s (1997) response to Laughlin’s (1987) advocacy of a Habermasian framework to develop a critique of accounting

¹³ For further examples, see Fleischman and Radcliffe (2003: 16) and Armstrong (1994: 46).

systems in an organisational context utilising insights obtained by researchers. In Lodh and Gaffikin's view, Habermasian "sociation principles" cannot be reduced to the methodological corollary at the level of an individual researcher' (Lodh and Gaffikin, 1997: 455) and thus the prospect of changing an accounting system within the Habermasian framework is postponed until appropriate sociation principles are manifest in the accounting world, or, possible, in the world at large.

How far has enabling accounting advanced? Not very far, according to Roslender and Dillard (2003: 342): even within the 'research community', apparently, 'there are many fields within the mainstream in which the critical accounting project is still largely unknown'.

Given the nature of the radical programme, this evaluation of its fruitfulness has necessarily concentrated on the robustness of its theoretical insights. It may well be that the chaos observed by Ritzer and Goodman (2004)¹⁴ is also the 'new level of maturity' which Beattie (2002: 112) infers from criticism from within the programme that it has 'lost its way because it is divided on methodological, philosophical and ideological lines'.¹⁵ Ritzer and Goodman ask what theorists can accomplish in such an environment: 'one thing they clearly cannot do is "advance" a theoretical perspective. Such a notion is based on the dubious idea of the existence of a well-defined theoretical perspective' (2004: A20). But theorists 'can gain new insights into the social world and can create new theoretical ideas' (2004: A20). It seems, though, that the incremental bounty of the radical accounting programme, its innovative and relevant insights into the nature of accountancy – beyond the insight that accountancy shares the character of other social practices, whatever this may be – has so far been limited. What we have is a shifting sea of possibilities, all contestable and endlessly contested, a field, like its parent discipline, 'almost constantly in flux' (Ritzer and Smart, 2001: 4).

4. The epistemology of the classical programme

4.1. The Illinois critique

Nelson's (1973) essay is often cited, in the UK¹⁶ as well as the USA, as early evidence of fundamental

dissatisfaction with classical research. His view of the flaw in the programme was that,

'if research is defined as the statement of hypotheses and the testing of hypotheses, a priori research might be called semi-research, because it is concerned with the statement of hypotheses on how accounting should be done, without the testing of these hypotheses' (1973: 3–4).

This critique appears to embrace the position, known as hypothetico-deductivism, generally associated with Karl Popper, whose work, *The Logic of Scientific Discovery*, was first published in English in 1959 (Jarvie, 2005: 821).

Hakansson's (1973) contribution to the same conference offers a rather different role for empiricism in financial accounting research. He is clear that accounting theorisation involves a normative dimension: 'we have worried and do worry more about what accountants should do rather than what they do do' (141).¹⁷ For Hakansson,

'empirical research is ... essential ... in determining the descriptive fit of the *premises* which underlie normative theories, and hence in satisfying one of the requirements (the other being the impeccability of the logic) for acceptance of a normative theory in making the "best" choice' (1973: 141).

His survey of empirical research reflects this view, being structured in terms of the various premises that individual studies can be considered to test. A commentary on Nelson's paper by Larson (1973) also took issue with Nelson's view. According to Larson, 'the significance of a priori research to the development of accounting is absolute' on the grounds that 'accounting is very largely a process of number assignment ... and ... analytical or a priori research plays a primal role in the evaluation of alternative number assignments' (1973: 29).

4.2. The nature of a priori justification

Epistemologically, the term 'a priori', 'typically connotes a kind of knowledge or justification that does not depend on evidence, or warrant, from

¹⁴ See the quotation earlier in this section.

¹⁵ See Section 2. The second quotation is drawn by Beattie from an unpublished version of Roslender and Dillard (2003); the phrase does not appear as such in the published version.

¹⁶ For example, by Whittington (1986), in the comments from which the quotation in Section 2 is drawn.

¹⁷ Classifications of financial accounting research frequently use 'a priori' and 'normative' interchangeably. For example Deegan and Unerman (2006) agree with Beattie (2002) that there is a research programme exemplified by the work of Chambers (especially 1966), Edwards and Bell (especially 1961), and Sterling (especially 1970), but refer to it as 'normative' (121–166) when Beattie uses 'a priori' (Beattie, 2002). Beattie's survey of British financial accounting research (2005: 93) uses 'normative' and 'a priori' as implied synonyms in describing the same type of research.

sensory experience' (Moser, 2005: 1). Such knowledge is acquired 'using only [one's] powers of reasoning' (O'Brien, 2006: 25). If those who attached the label to the work of the golden age theorists intended to consign its achievements to oblivion, they could hardly have chosen more astutely. The following text is extracted from an AS level primer in epistemology:

'The fascination with a priori truths ... has a long philosophical history. To regard such knowledge as having a privileged status, and to hold it as a benchmark for all other knowledge claims, is one of the main features of rationalism ... [But] the rationalist project appears to have failed. The dream of grounding any substantial knowledge of the world on the back of the absolute certainties of reason could not be realised' (Cardinal et al., 2004: 43 and 70).

Or, to quote from a work by one of the leading representatives of the school of logical positivism, originally published in 1936, 'to say that a proposition is true a priori is to say that it is a tautology. And tautologies, though they may serve to guide us in our empirical search for knowledge, do not in themselves contain any information about any matter of fact' (Ayer, 1990: 52).

Empiricists such as logical positivists regard all a priori truths as what Immanuel Kant called 'analytic' statements, in contrast to 'synthetic' truths, which 'do not simply depend on what our terms mean, but also on how the world happens to be' (O'Brien, 2006: 27). But Kant himself did not believe that all a priori truths are analytic. Included in his own list of synthetic a priori truths are those that underpin empirical investigation, such as the belief that events have causes (Morton, 2003: 46). Contemporary philosophy continues to embrace a wide range of views about the existence, nature and scope of a priori knowledge (BonJour, 2005; Devitt, 2005). To know that something that is red all over cannot be green all over is an a priori truth but not one that appears to be analytic. If it were so, the concept of 'red all over' would be reducible to 'not green all over' and 'not blue all over' and 'not yellow all over' etc. and this is implausible: it would seem that we can possess the concept 'red' without possessing the concepts of all other colours (O'Brien, 2006: 27). In the same way, mathematical truths can be argued to be synthetic: 'I can understand "12" without understanding " $(\sqrt{4}\sqrt{9})^2 \div 3$ "' (O'Brien, 2006: 28).

Among, as Peacocke (2005) charmingly puts it, 'friends of the a priori' (2005: 742), it is accepted that 'the range of propositions that are a priori is vast

and varied' (Peacocke, 2005: 745). Examples offered by Peacocke in defence of his claim of particular relevance to financial accounting theory include the principles of rational decision theory and 'much of economics' (2005: 745).¹⁸ Speaking of his full list, he says that 'it is often clear that a proposition is a priori, while the nature of the justification or entitlement for belief in the proposition remains unclear ... The identification of the full nature of the entitlement that sustains a priori knowledge, as opposed to its existence, is an open question in almost all the domains mentioned above' (2005: 746).

Logical positivism, with its strongly dismissive view of a priori knowledge, flourished in Europe before, and in the USA after, the second world war but was everywhere in decline by 1960, not least as a result of the work of W.V. Quine (Friedman, 2005). Quine, writing in the 1950s and 1960s, drew on the argument of the French physicist and philosopher of science, Pierre Duhem, that 'non-observation sentences face the tribunal of experience not singly but in groups' (Dancy, 1985: 92). Claims other than those which report nothing beyond the evidence of our senses, strictly interpreted, cannot be conclusively verified, or conclusively falsified, by observation alone; they will always be part of a more general theory and 'because of this we have a choice where to alter the theory when things go wrong at the observational level' (Dancy, 1985: 92). For Quine, apparently secure analytic truths such as that 'a woman giving birth to a child is its mother' are subject to revision on the basis of empirical evidence – such as that the child is the result of in vitro fertilisation of an ovum supplied by another woman (example from Everitt and Fisher, 1995; cited in O'Brien, 2006: 131). Even mathematics and logic are subject to change: '[r]evision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics' (Quine, 1953: 43).

4.3. *A priori justification and 'scientific rigour' in the golden age*

Though published in the 1960s, the grand theories of the golden age are associated with methodological developments which took place during the preceding five years (Gaffikin, 1988: 19). Gaffikin argues that much the most important contribution to this phase was made by Chambers in four articles, the first of which (1955) 'was indeed seminal'

¹⁸ For the argument that economists' methods are a priori but that they do not treat their basic assumptions as analytic, see Udehn (2003: 152) and Hausman (1992).

(Gaffikin, 1988: 17). Although, for Gaffikin, the ‘hallmark’ of the work was ‘the appeal to what was understood to be the application of scientific rigour to accounting research and theory construction’ (1988: 19), he nonetheless says of Chambers’ apparently seminal work that, ‘while alluding to a theory of scientific rigour, the argument can only be admitted as a set of tentative hypotheses based on a priori assumptions’ (1988: 18). In Gaffikin’s view, Chambers’ articles make it ‘increasingly clear that the formal methodology to which he was alluding was hypothetico-deductivism’ (1988: 18) so that ‘with hindsight it is apparent that these early works were leading to Chambers’ *opus magnum*, *Accounting, Evaluation and Economic Behaviour*’ (1988: 18). This was one of the studies offered to Nelson as an illustration of golden age theorising and in it Gaffikin finds further evidence to ‘reinforce the belief that [Chambers] employed hypothetico-deductivism’ (1988: 21).

Despite his claim to discern hypothetico-deductivism in the work, Gaffikin concedes that Chambers’

‘argument is analytically derived from basic assumptions (postulates) which he claims are derived from his observations of the business world, together with notions generally accepted in economic theory. This is the method Mattessich refers to as being postulational’ (1988: 21).

Gaffikin explains that a postulational work ‘relies on certain assumptions (axioms) from which conclusions are deduced’ (1988: 21) and treats the term as synonymous with ‘a priori’ (1988: 24). After examining all the works identified for Nelson as examples of a priori theorising (and others), Gaffikin summarises the ‘methodological soul-searching by accounting writers’ during the 1960s as concluding that ‘the prospect of the further development of accounting ideas was seen to lie in the application of the rigour of scientific method. This was found in the various guises of logical empiricism/positivism’ (1988: 23). He nonetheless goes on to list a variety of terms used to describe the methods involved, including ‘a priori’, ‘postulational’, ‘normative’ and ‘deductive’ explaining that, although ‘not all are accurate descriptions’, they do ‘relate to features of the methods’ (1988: 24).

It would appear, then, that the outcome of the golden age’s concern with epistemological and methodological issues was a commitment to scientific rigour and a consequent desire for empirical engagement which some are able to read as embracing logical empiricism/positivism, and per-

haps even hypothetico-deductivism (see also Mouck, 1989), but, at the same time, the continued use of methods generally associated with a priori theorising. This ambivalence may have made it easier for Nelson to create a presumption for the hypothetico-deductive method – a position which, as Gaffikin’s characterisation of the methods used by the golden age theorists demonstrates, cannot be derived directly from their work and, further, is not consistent with the philosophical use of the term ‘a priori’. It may also have contributed to the difficulty of defending golden age theorisation in its own terms.

But such a defence would have been possible. Ironically, perhaps, one could have been mounted from a proper interpretation of the term ‘a priori’. As we have seen in Section 4.1, Hakansson’s Illinois contribution took a rather different line from Nelson’s. For him, the model underpinning a financial reporting system has a normative character, unsusceptible to empirical testing. The contrast between normativity, in the sense employed by Hakansson, and empirical verification, need have less significance for an analysis of the role of accounting theory than it may appear to have at first sight. If I have a sore throat and visit my doctor and she tells me to take medicine X, she is employing an overarching normative theory (I *ought* to take the medicine) but one underpinned by: (a) observations of people like myself, further confirmed by my attendance at the surgery, that my objective is to be restored to good health; (b) empirical verification (as it happens, scientifically conducted) of the relationship between taking medicine X and the curing of sore throats; and (c) a range of other value judgments and observations of means-ends relations covering cost, unpleasantness of treatments, likelihood of success, and so forth.

Developments in rational decision theory and in related areas within economics¹⁹ and operational research taking place during the 1950s and 1960s (see, for example, Simon, 1956, 1965) exhibit a similar character to the example in the previous paragraph, though with a heavier emphasis on the logical linkages in the modelling (Mattessich, 2002: 187–188). On this view the non-empirical truths of the golden age’s grand theories take the form of synthetic a priori propositions. Hakansson is merely holding that the theories are derived in part from premises that are themselves susceptible to

¹⁹The role of a priori justification in economic theorisation had been debated for more than a century before the 1960s (Blaug, 1980: chapters 4–5), remained a live topic during the decade (see, for example, Stone, 1966), and remains so now (see, for example, Maki, 2002).

empirical testing – and should, indeed, be so tested.²⁰ An approach to financial accounting as a conditionally normative discipline has been advanced by Mattessich, who developed a structure ultimately termed Conditional-Normative Accounting Methodology (CoNAM, see Mattessich, 1995: ch. 11). Although initially proposed in his 1964 work, the structure appeared in complete form only during the 1970s (Archer, 1998: 310). It has elicited little interest, perhaps because of its emerging fully only after the social scientific turn in the USA and weaknesses in other areas (Archer, 1998).

An alternative formulation of the defence might have been offered from a Quinean position. Accounting theorisation, it could have been argued, occupies a position within the interior of the web of accounting knowledge, nearer in form to logic and mathematics than the experimental sciences. Recalcitrant experience, for example in the form of unstable prices, may require an adjustment to this interior structure and it was on this particular work that the golden age theorists were engaged.²¹

4.4. *The UK response to the Illinois critique*

In December 1979, nearly a decade after the Illinois conference, another took place in London to ‘review the current state of accounting research in the UK’ (Bromwich and Hopwood, 1981: xii). The principal paper devoted to financial accounting theorisation concerned income theory and was contributed by Whittington (1981a). As we have seen in Section 2, Whittington’s conclusion implies that a priori theorising is alive and well in the UK.

After a further seven years, Whittington provided an ‘overview’ of financial accounting theory (1986) in which he referred to Nelson’s 1973 paper and argued that,

‘There were and are two responses to this

²⁰ This position is compatible with the example of empirical testing offered by Chambers in the (1955) article referred to earlier in this section:

‘If one is confronted with what purports to be a theory of accounting, and finds that it deals with corporate accounting only, the existence in reality of unincorporated ventures will cast doubts on the validity of the propositions put forward’ (Chambers, 1955: 19).

²¹ Applying Nagel’s (2001) test (see Section 3.2) to Quine’s (1953) ‘web of belief’ invites the question, ‘if all our beliefs are revisable, is the belief that all our beliefs are revisable revisable?’ (Ritchie, 2008: 202). Whether this gives rise to a formal paradox is a matter of dispute within philosophy: for arguments that it does, see Katz (1998) and Elstein (2007); for the opposite position, see Adler (2003), Resnik and Orlandi (2003) and Colyvan (2006). A response in keeping with Quine’s location within the pragmatist tradition would be that, supposing the web of belief model to be the best that can be achieved, we should take it as good enough (Rescher, 2005: 747).

critique. The first is to narrow the area of disagreement by empirical research, rejecting theories whose assumptions or predictions are at variance with empirical evidence. This type of research has become increasingly popular during the past fifteen years or so ... The second response is to adopt an eclectic approach to income measurement, providing a variety of alternative measures ...’ (1986: 15).

The second response is, of course, a way of handling the alleged indeterminacy of theory in practice, not a prescription for theoretical advance. While the first is formulated in a way that is even-handed between hypothetico-deductivism (the testing of predictions) and postulationalism (the testing of assumptions), its focus on empiricism reflects the decline in a priori work then occurring in the UK.

5. The turn to mainstream social science

Financial accounting research’s turn to mainstream social science can be plotted by reference to its ‘objects of inquiry’, using the term in its Kantian sense to denote,

‘the way in which phenomena in the social domain (people, behaviour, actions) become “objects” of ... inquiry by being classified in certain ways, in specific contexts, and under some descriptive conditions’ (Montuschi, 2003: 20).

According to Nelson (1973: 15), the hypothesis underpinning a financial reporting system is that,

‘the proposed financial reporting will cause the decision-maker to make decisions which will enable him [sic] to reach the goals that have been set, or at least result in more progress towards these goals than would exist if alternative measurement systems were to be used.’

Though it might be possible to test a system empirically, for example along the lines envisaged by Nelson, using the measurements resulting from the system as part of the test (but not, of course, to measure outcomes such as investor wealth, the independent variable), the system itself does not generate predictions that can be tested. Further, testing the hypothesis in any rigorous way would be profoundly difficult for a number of reasons including the strict unavailability of the comparator and the highly complex interrelationships between the various economic and social factors involved, which make applying the *ceteris paribus* condition extremely difficult (Archer, 1998: 311–314), a point made by Chambers himself in one of the papers

alluded to earlier (Chambers, 1960: 38–39). So scientifically rigorous empirical work on financial reporting systems was never going to be easy.

Both Nelson (1973: 18) and Hakansson (1973) (see Section 3.1) identified predictive ability as a promising approach to the sort of empirical work they were calling for. Predictive ability studies respond directly to what are taken to be investors' ultimate information needs, namely forecasts of future events that will affect their wealth, typically dividends or the corporate cash flows available to pay them. They attempt to identify superior financial reporting methods on the basis of their usefulness in facilitating the generation of such information. In so doing they retain the classical programme's focus on the traditional objects of inquiry of financial accounting research, namely financial reporting systems as systems, while enabling researchers to bring statistically more sophisticated methods to bear on their work. As Beattie (2002: 102) reports, the first wave of empirical work to follow the Illinois critique did indeed focus on predictive ability. But, in 1979, probably the first survey of empirical research in financial accounting directed at a UK audience was reporting that predictive ability studies, in both the USA and the UK, were 'extremely thin on the ground' (Peasnell, 1981: 113).²² Its author drew attention to the size of the challenge presented in designing tests of predictive ability: (a) predictions require a forecasting model as well as data and any test of predictive ability will be a joint test of both; (b) realistic data from financial reporting systems other than that currently employed in statutory reporting will be difficult to obtain; and (c) testing over a long time horizon is required (1981: 112–114).

One response to the limited success of the approach, in the face of problems recognised as very challenging (for example, by Carsberg et al., 1977: 421–422), would have been to redouble the effort expended on addressing those problems. Another would have been to seek alternative ways of supplying empirical validation, such as adopting the structure suggested by Hakansson (see Section 4.1 and later this section). Yet another would have been to return to classical methods, mounting a more robust defence of the programme, perhaps following up the avenues suggested in Section 4. What actually happened was rather different.

In Whittington's view, a key factor implicated in the growth of empiricism from the 1970s was the

influence of the Chicago (or Rochester) school (Whittington, 1986: 24–25), that is, so-called positive accounting theory. The epistemological position of the Rochester school combines Popperian falsificationalism and Friedmanite instrumentalism (Watts and Zimmerman, 1986: 7–12). In their search for tightly-defined hypotheses that could be tested rigorously by statistically sophisticated methods (in the case of the Rochester School, the methods of quantitative economics), neo-empiricists switched their attention from financial reporting systems as systems, to any objects of inquiry identifiable within the domain of financial reporting that might prove amenable to such testing. Such objects included the behaviour of those involved in the reporting process and accounting numbers considered as independent entities associated only loosely, if at all, with the systems that generate them. Positivists, and other neo-empiricists, often test hypotheses involving the inputs or outputs of financial reporting systems: for example, they tested whether 'managers selected accounting methods opportunistically to enhance their own wealth' (Beattie, 2002: 107) and 'the way in which the stock market, through share prices, reacts to different types of accounting information' (Beattie, 2002: 104). But these tests generally treat the financial reporting system itself as a black box, of relevance only in providing outputs or a framework within which choices are made. As Barth's (2000) paper ultimately concedes, very little is learnt about which outputs and choices would be appropriate (see Parker, 2007: 42). The positivist's – and, to a considerable extent, the neo-empiricist's – approach to hypothesis formulation, considered as a practical activity, is that of the pure social scientist: questions asked are driven by the scope for rigorous testing rather than the level of interest necessarily attaching to the answers. As Zeff memorably put it, studies are 'the result of methods in search of questions, rather than questions in search of methods' (Zeff, 1983: 134; see also Zeff, 1989).

There is widespread agreement that the paper by Ball and Brown (1968) represents a watershed in the development of financial accounting research (Beattie, 2002: 104; Watts and Zimmerman, 1986: 15; Whittington, 1986: 25). Watts and Zimmerman's well-known text on positive accounting theory identifies the paper (perhaps claims it) as marking the beginning of the literature they survey (1986: 15) and spends eight pages describing it (40–47). But Hakansson's (1973: 146) stock-taking of empirical work for the Illinois conference, which explicitly viewed the role of such work as being to test premises underlying financial reporting theory

²² Several of the most frequently cited papers on predictive ability – including probably the best-known British example, that by Carsberg et al. (1977) – merely describe and argue for the approach rather than providing an application of it.

as then constituted, included the Ball and Brown study, classified as a test of an ‘assumption bearing on the question *what*: relevance (assets, claims and changes in these)’ (1973: 144, emphasis supplied). A number of other studies are also common to the surveys by Hakansson and by Watts and Zimmerman. The dividing line between the two is not based on the design of individual studies, which can thus be claimed by both sides. On the surface, this enables positivism to be represented as ‘merely an extension of the general direction of accounting research since the late 1960s’ (Gaffikin, 1988: 31). But the division does reflect a major difference in the underlying epistemological stance of the surveys. In one, individual studies are regarded as contributing on a piecemeal basis to the overall project of testing financial reporting systems as systems; in the other, each study delivers a new individual insight into the world of financial reporting. In the latter case, as we have seen, the objects of inquiry thus became the phenomena captured by the empirics. By the mid-1980s in the UK – somewhat earlier in the USA – the former view was defunct. The predictive ability criterion had been written off as a failure before the major research programme called for by Carsberg et al. (1977) had properly got under way and the studies of Ball and Brown and others had been successfully claimed for positivism.

Beattie (2002) describes the move to positive accounting theory as ‘a reaction to the *excessive* a priori theorizing in financial accounting in previous decades’ (2002: 106; emphasis added). However, by the time the calls for strong-form positivism were at full throttle, financial accounting research had, for example via predictive ability studies and Hakansson’s scheme, begun to engage with empirical methods of validation. Although Whittington (1986: 24–25) characterises neo-empiricists as motivated in part by the desire to resolve the debate between competing golden age theorists, very few serious attempts so to do were in fact made; once empiricists had given up on predictive ability, attempts at testing financial reporting systems as systems were simply abandoned. Yet support for positivism’s epistemological underpinning was far from secure. Falsificationalism was, from the early 1970s onwards, coming to be largely rejected in favour of more sophisticated approaches, on the grounds that individual instances of adverse evidence need not (and in practice do not necessarily) lead to rejection of a theory, as opposed to acceptance of some other explanation such as imperfections in observation (Benton and Craib, 2001: 58–63). Friedmanite instrumentalism, which

holds that in testing theoretical models, ‘the *only* question is which sort of model results in more successful predictions’ (Hollis, 1994: 54), and that the realism or otherwise of assumptions in the model is irrelevant, has always been highly controversial, even among positive economists (Hausman, 1994: 39–40; Hollis, 1994: 53–56).²³ As Mouck (1989: 90) points out, ‘it is ironic . . . 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.’

One important difference between the American and UK scene is the place of radical accounting research. In the USA, strongly positivist variants of empiricism continue to hold an overwhelmingly dominant position (Reiter, 1998). While a ‘strongly positivist’ version of neo-empiricism remains the ‘predominant’ research tradition in the UK (Beattie, 2002: 112), the British academy has found substantial space for interpretive and critical approaches (Beattie, 2002: 212). The development of the radical programme can be linked to that of positivism in a number of ways. The positivist project predated radical accounting research and, by redefining the objects of inquiry of accounting research, opened up the ground for other perspectives drawn from social science and sanctioned the move away from focusing on financial reporting systems as systems. Further, positivism provided a position against which to react (Roslender and Dillard, 2003: 327): many expositions of radical developments in accounting thought, including possibly the most frequently cited, Chua’s 1986 paper in the *Accounting Review*, are set out as a series of contrasts with what she calls the ‘mainstream’ (Chua, 1986: 611) perspective, and Merino (1998: 604) says firmly: ‘I view the rejection of modernist epistemology, modelled on 17th century Newtonian physics and its rule bound methodology, as the unifying factor in accounting critical [sic] research’.

Though optimism about the potential for a priori theorising (in effect, classical accounting research) survived in the UK into the late 1970s (Whittington, 1981a), the embrace of social science turned out to be as seductive in the UK as in the USA. Yet, as demonstrated in Section 4, epistemological positions to underpin classicism remained available and, as shown in Section 3, by the early 1980s, there was plentiful evidence that neo-empiricism was not yielding the abundance of results that had been

²³ Hakansson (1973: 158) explicitly rejected Friedman’s argument.

hoped for. Rather, the turn to mainstream social science represented the seeking out of social scientific methodology for its own sake.

As mentioned at the end of Section 2, both Cushing (1989) and Mouck (1993) examine the move to neo-empiricism in the USA using a Kuhnian framework. Cushing considers that the US crisis in financial accounting research in the 1970s parallels, not a Kuhnian paradigm shift, but a different effect noted by Kuhn, namely the desertion of science by scholars unable to tolerate crisis: 'contemporary academic accountants ... have in a fundamental sense deserted accounting' (p. 29). Beattie and Davie (2006) report Cushing's broad argument correctly but immediately follow this by stating that, 'thus, the domain of the accounting discipline *widened* and new theory groups formed' (p. 4; emphasis added). Widening of theory is a form of progressiveness but is hardly consistent with Cushing's own view: 'the discipline's leading scholars no longer display a paramount interest in the fundamental issues that distinguish accounting from other fields. This suggests that accounting's present crisis is not only severe, but possibly fatal to accounting as a viable branch of knowledge' (Cushing, 1989: 31). Again, Beattie and Davie suggest that Mouck's (1993) conclusion was that the transition was 'not a revolution ... but rather a normal science expansion of the economics paradigm' (pp. 4–5). They fail to point out that Mouck regarded this expansion as an incursion by economics into space formerly occupied by the, now-failing, classical research programme, which he actually argued to be in a pre-paradigmatic state.

6. The sociology of the turn to mainstream social science

A growing literature on the sociology of accounting research practice in the USA²⁴ is directed at explaining the emergence and continuing dominance of neo-empiricism, a dominance seen, at least implicitly, as achieved against the claims of radical research rather than the classical programme. This paper begins the search for a more complex sociological model of the British financial accounting academy that can account for the widespread adoption of neo-empiricism despite its fruitfulness having already come under question from its own practitioners in the USA; its continuing vitality in the face of limited fruitfulness; the emergence of a

substantial community of radical researchers and their absorption into the mainstream, again in the face of limited fruitfulness; and the demise of classical accounting research despite the evidently pluralistic nature of the British academy. Following Beattie (2005), potential influences will be classified as cognitive, external social, and internal social factors.

6.1. Cognitive factors

According to Whitley (1988: 641), the particularly strong 'belief in scientific knowledge as a crucial resource in maintaining and improving the social order' manifest in the USA, combined with the achievements of operational research and economics, encouraged an expansion in formal, mathematicised, hypothetico-deductive methods applied to managing organisations and these came to dominate the work of leading business schools from the 1950s onwards. Cognitively, the principal influence which enabled this general 'scientification' (Whitley, 1988: 642) to be extended to financial accounting research was accountancy's traditional use of marginalist economics to provide its theorisation, which made the adoption of economic theory generally difficult to resist. Other attractions in the US context were that neo-empiricism is compatible with a strongly capitalistic worldview (Reiter, 1998: 153–154) and utilises a 'ready made', pre-justified, methodology imported from finance, particularly comforting for an intellectually immature discipline (Gaffikin, 1988: 27) and especially one suffering from 'a professional inferiority complex' (Mattessich, 1995: 162; see also Hopwood, 2007: 1366).

Key features of neo-empiricism are the elegance of its methods, which is conceded even by its critics (Reiter, 1998: 152–153), and how little attention it gives to its epistemological stance and overarching theorisation. As Sterling (1990) points out, although Watts and Zimmerman's (1986) landmark exposition of positive accounting theory actually begins by stating, 'this book is about accounting theory' (p. 1), in fact only the first brief chapter (14 pages out of 362) is devoted to considering theory as such. Though demanding in the quantitative skills it requires, neo-empiricism is only lightly burdened with theoretical baggage, enabling those with the requisite technical skills to get rapidly down to work.

From a UK perspective, the importance of the rapid expansion of neo-empiricism in the USA is that it provided an attractive source of research questions and methods, ready made and now pre-justified twice over: Peasnell's 1981 survey

²⁴ Reiter (1998) cites seven studies, to which her own adds an eighth; see also Parker (2007) and Reiter and Williams (2002). For an alternative socially-based analysis of the demise of classical research, targeted principally at neo-empiricism, see Clarke et al. (1999).

described efficient markets research as ‘intellectually impressive’ twice within the same paragraph (1981: 107 and 108), giving reasons which included its ‘impressive economic-statistical methodology ... the like of which is not to be found elsewhere in accounting’ (1981: 108). Although the technical skills required by neo-empirical research are demanding, the nature of the programme offers extensive scope for using very similar research designs on new data (Daley, 1994: 44). This point is often made as a criticism but it actually offers significant opportunities for those with some quantitative skills but limited abilities and expertise in research design, especially given the broad similarity between UK and US financial systems.

Neo-empiricism was slow to get off the ground in the UK, partly, no doubt, because of the UK accounting academy’s relative shortage of quantitative skills (Peasnell, 1981: 119–120). The delay meant that the programme began growing significantly only after its epistemological and methodological weaknesses had begun to be exposed – Peasnell’s 1981 survey lists five ‘limitations’ (1981: 108–109). This must have helped in opening up the field to other theoretical perspectives. Because accounting departments in the UK are generally located in social science faculties rather than ‘stand alone’ business schools, radical ideas were not anathematised in the UK as in the USA. For entrants with a traditional background, radical theorising may have the appeal of exoticism, while those motivated to enter academic life by discomfort with practice may find its politics attractive. Each school within social theory offers a ready made, pre-justified approach for importation into financial accounting research. Each is, in its own terms, elegant and rigorous. As with neo-empiricism, a form of replication study can be undertaken, demonstrating that accountancy, like all other instruments of hegemony, works to exploit the subaltern classes, feed the will to power/truth, create textual meaning from nothingness, promote surveillance, discipline and punishment, and so on. As we have seen (Section 3.2), much radical accounting research appears to take this form.

6.2. External social factors

Beattie (2002) identifies a number of interactions between accounting practice and the theorisation of the classical programme but is able to draw only much more tenuous links between theory and practice since the social scientific turn. The only relevant developments in practice identified since the 1970s are cross-border harmonisation, the increasing importance of intangible assets and

greater concern with earnings management. Although these have focused attention on particular topics, Beattie does not suggest any intellectual advances resulting from them. Indeed, a major factor in explaining the character of the contemporary accounting academy in the UK is the almost total lack of any significant engagement between the accountancy profession and the scholarly community at the level of research (Arnold, 1989; Baxter, 1988; Dewing and Russell, 1998; Hopwood, 2007; Power, 2004; Whittington, 1986; Zeff, 1996).

Practitioners take little notice of post-classical research because it is perceived as having little relevance to their interests (Roslender and Dillard, 2003: 343; Whitley, 1988: 642–643).²⁵ Some commentators have argued that politically sophisticated practitioners are particularly comfortable about academic accountants pursuing post-classical research precisely because it does not present policy-relevant findings to those, such as regulators, who might be inclined to use them. Neo-empiricism deals with ‘imaginary worlds of economic equilibria where information is true and costless and everyone acts “rationally”’ and, consequently, ‘current practices and conventions are not ... threatened by this sort of research because it does not deal with them’ (Whitley, 1988: 642–643; see also Power, 2004: 377; Tinker, 1985: 205). Radical attacks on practice are regarded as too arcane to merit attention (Roslender and Dillard, 2003: 343). At the same time the weight of mathematicisation (in the case of neo-empiricism, Whitley, 1988: 641) or social theorisation (in the case of the radical programme) preserves post-classical work from informed criticism by the profession.

Even if uninterested in their results, practitioners might benignly aid academics by providing access to the sites on which accounting practice takes place but the level of such access is actually minimal, for sound business reasons (Bricker and Previts, 1990: 11–12; Howieson, 1996: 33; Kinney, 1989: 120–21). The production of financial statements is almost inevitably highly sensitive in any location about which there would be interesting things to be discovered and any data other than that actually produced (for example, using alternative accounting policies or measurement methods) would be expensive to prepare and also potentially highly sensitive (Peat Marwick Mitchell & Co., 1982: 89–90; Rutherford, 2007: 98). Poor access increases the

²⁵ Professional accountancy bodies do fund some academic research but the scale of funding is very modest by comparison with that available from sources favouring social scientific research (Wallace, 1997: 235–238).

attractiveness of programmes which do not require co-operation from practitioners.

6.3. *Internal social factors*

In 1986–1987, 66% of accounting staff in the older universities were professionally qualified (Wallace, 1997: 232–233) but between 1998 and 2003 only 30% of recruits to pre-1992 institutions held a CCAB qualification (Duff and Monk, 2006: 203). This decline in staff with a professional background, largely a reflection of increasing salary differentials between practice and academia (Brinn et al., 2001: 351; Wallace, 1997: 233) is linked by Otley (2002: 398) to a ‘dearth of policy- or business-oriented work’ in accounting (see also Beattie, 2005: 94). It reduces the number of researchers with advanced expertise in accounting techniques, familiarity with practical contexts, access to the tacit knowledge-base of accounting practice, credibility with practitioners as research subjects, and an interest in the problems which provide the material for classical accounting research.

A deterioration in research time, combined with pressures to increase output in quantitative terms (Gray et al., 2002; Humphrey et al., 1995; Parker and Guthrie, 2005; Puxty et al., 1994), has encouraged researchers to focus on the efficiency and effectiveness of research methodologies in delivering research products, primarily articles in learned journals. The methodologies of post-classical programmes offer a number of advantages. Availability of extensive, publicly accessible, data in machine-readable form, the potential for replication studies with limited methodological contribution, and the growth in cheap computational capacity, have greatly facilitated the production of neo-empirical articles (Whittington, 1981b: 131, 1986: 131) and made this form of research ‘low risk’ in that ‘validity of research outcomes ... becomes a technical matter rather than a conceptual or theoretical one’ (Whitley, 1988: 641–642). The radical programme can also enable articles to be produced reasonably speedily and with relatively low risk, for example by remaining within a narrow theoretical model with which the researcher is familiar for multiple projects, generating papers demonstrating that any particular theoretical position applies to accountancy, comparing details of two theoretical positions, and so on (Humphrey, 2001: 92–94; Wallace, 1997: 243). Neo-empirical research does not typically require access to unpublished data and this is also true of radical research conducted at the theoretical level or employing publicly available material as case studies. All in all, post-classical accounting

research methods are efficient and reliable at producing academic outputs compared to the traditional approach to research design in the classical programme, which is to start with a problem and search out methods to solve it (Zeff, 1989: 170).

Academic accounting in the UK has traditionally been located in social science faculties. The impact of this, primarily through the career and reward structures of institutions, combined, perhaps, with a feeling of inferiority in the face of the better established disciplines of mainstream social science (Wallace, 1997: 239), seems likely to be significant. Candidates for promotion generally have to compete with others, and will be judged by senior staff, from across the faculty. Research papers which resemble those of mainstream social science are likely to carry more weight (Wallace, 1997: 239).

Once classical research comes to be perceived as inferior, social control effects snowball. Editors seeking to improve the reputation of their journals may discourage acceptance of articles within the classical programme, if only at the margin of acceptability. Departments seeking to improve reputations and research assessment scores will prefer early career recruits who have the research training to begin immediately to publish in high quality journals – which is easier if undertaking post-classical work (Brinn et al., 2001: 350–351; Weetman, 1993) – and applicants with established careers who have published in such journals. Vice-chancellors seeking ‘research stars’ will look only at post-classical researchers.

7. Conclusion and implications

In the UK, the classical financial accounting research programme remained in good health into the 1980s, well after it had been eradicated in the USA. But American developments robbed the UK’s classical programme of a powerful source of intellectual nourishment while providing an alternative approach of great attractiveness. British pluralism enabled both neo-empiricism and radical research to flourish but the classical programme itself has died out, as a consequence, I argue, of sociological features of the academy. I have not set out to deny the legitimacy of post-classical research which has, indeed, had some successes, for example, in influencing the way the relationship between accounting numbers and share prices is characterised (neo-empiricism) and the development of social responsibility accounting (radicalism). I seek merely to demonstrate that, neither by its fruitfulness in generating innovative and relevant findings nor by incontestable epistemological superiority, does post-classical research justify the

elimination of the classical programme: contemporary research programmes mark a turn to mainstream social science rather than a progressive development. This turn took place, not directly in response to the call for empirical research by Nelson and others, but only as such of the initial responses to this call as continued to address the traditional objects of inquiry of financial accounting research (namely accounting systems as systems), and in particular, the predictive ability project, came to be abandoned. In the face of the difficulties which researchers using social scientific methods encountered in addressing the traditional objects of inquiry of financial accounting research, the accounting academy retained the methods and rejected the objects of inquiry.

The light cast on the turn to social science by this paper suggests that the financial accounting research community may have been premature in its abandonment of classicism – that there may be life yet in the programme and in the activity directed at the traditional objects of inquiry of accounting research that it permits. It might be objected that the classical programme in its heyday was no more fruitful than others have been since. Certainly the profession's deep suspicion of any intellectual approach to its discipline (Stamp and Marley, 1970: 113–114; Wallace, 1997: 233–235) places a limit on its absorption of research outputs. However, significant contributions have been made to accounting practice by the classical programme.

Among the academic advances with a significant impact on accounting practice identified by Baxter, in his monograph reflecting on the relationship between academic research and practical needs, was the development of current cost accounting (Baxter, 1988: 4). During the 1970s and 1980s, while the UK profession struggled to adapt traditional financial reporting to cope with substantial levels of inflation, there was a significant demand both for the outputs of previous classical research in the area and for further research conducted by classical methods. The models employed in the development of practical solutions to accounting under inflation were essentially the products of academic theorists working within the classical programme (Tweedie and Whittington, 1984: especially chapters 2–3). Additional developmental activity included both further a priori theorising responding to lacunae in the basic models, for example on the issue of physical versus monetary capital maintenance (see, for example, Tweedie and Whittington, 1985) and empirical work – largely carried out without heavy mathematicisation – to investigate the usefulness of

current cost accounting (see, for example, Carsberg and Page, 1984). These contributions became redundant as a result of 'the end of the current cost revolution' in the late 1980s (Tweedie and Whittington, 1997) but they demonstrate the capacity of classical work to contribute to accounting practice.

A second contribution came about as a result of the increasing importance in accounting standardisation of an approach based on a conceptual framework, dating, in the UK, from 1988 onwards (Rutherford, 2007: 261–264). The UK's framework is strongly derived from the original, US, project (Rutherford, 2000: 5), which, itself, drew heavily on classical theorising, as any account of the evolution of the project makes clear (Gore, 1992; Solomons, 1986; Storey and Storey, 1998). The move to a more conceptually rigorous approach to standardisation might have been expected to generate significant demand for classical accounting research and, indeed, UK standard-setters have called for research inputs from the academic community, though they generally complain that few are received (Rutherford, 2007: 113–114; Wallace, 1997: 245–246; Whittington, 1995). This is hardly surprising since, ironically, demand grew significant only as the classical programme was rapidly dying.

It has long been accepted that there is a schism in the USA between accounting practice and academic accounting research (Bricker and Previts, 1990); some commentators identify a similar schism in the UK (Baxter, 1988; Dewing and Russell, 1998; Wallace, 1997). Though by no means the only cause of this schism, the turn to mainstream social science is a major contributory factor.²⁶ The schism appears unique to anglophone countries – it is apparent in Australia (Howieson, 1996) but not in Germany (Power, 2004: 382) – and to accountancy. Adopting social scientific methods in some of its programmes has not resulted in the legal academy abandoning doctrinal legal studies and sociology departments embracing social theory nonetheless conduct research considered useful by social policy analysts.

Reopening space for classical research may help to mend the schism and enable the academy to make

²⁶ This point is made graphically by Sterling (1990: 117–118) in his withering comments on positive accounting theory. He cites one theorist's view that it is preferable to teach positive accounting theory because accounting methods change rapidly as accounting standards are revised: better for students to understand why companies choose particular accounting procedures. Would you, he asks, want your medical operation performed by a practitioner who had been trained, not in surgical procedures, but in why surgeons choose the procedures they do, perhaps to maximise their own utility or caught in the grip of their profession's will to truth?

a contribution to accounting practice once again, providing an intellectually rewarding pathway for researchers keen to engage with accounting systems as technical systems. As suggested above, the increasing importance of conceptually-based accounting standardisation, especially combined with the growing complexity of substantive topics, offers opportunities for classical research; indeed the discussion documents of standard-setters grow increasingly to resemble the normative theorising previously produced within the academic community (see, for example, IASB, 2005). Current economic developments suggest that recent price stability may be coming to an end, perhaps raising again demand for classical research in price change accounting.

In making a contribution to practice, academics will bring their traditional strengths of theoretical sophistication, multidisciplinary, rigour, disinterestedness and ability to take the long view, strengths which give them a comparative advantage over practitioners in theorisation and will, it is to be hoped, enable them to improve the overall social functioning of accountancy. Practitioners' traditional suspicion of intellectualism is reflected in some quarters in attitudes to conceptually-based standardisation but the approach seems fairly well embedded and may help to encourage at least leaders of the profession to be more open to theoretical debate.

Among the developments which a resurgence in the classical programme could usefully embrace are further exploration of its underlying epistemological position;²⁷ the (re)introduction of the methodology of classical research into the research training of new entrants to academia; and the establishment of one or more journals devoted to classical research. Research training might be provided, at least initially, by means of the coverage of accounting theory and the history of accounting thought and practice that, as Zeff (1989: 170) points out, was the traditional route to an appreciation of classical methods but has been squeezed out of the curricu-

lum by the demand for training in current practice at the undergraduate level and social science research methods at the postgraduate level. Perhaps funding for a journal devoted to classical accounting research might be available from a professional body or large professional firm.

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²⁷ One possible line of development here would draw on classical American pragmatist philosophy. Work in this direction was undertaken during the 1960s (Beams, 1969; Deinzer, 1965, 1966; Dopuch, 1962) but, not surprisingly, petered out with the demise of the classical programme. Pragmatism shares with postmodernism a rejection of the foundationalist project of 'knowing things as they really are' but, whereas 'postmodernism ... celebrates ... the fragmentary, the incoherent, the irrational, and the paradoxical', pragmatism 'focuses on ... "what works"' (Bix, 2003: 245; see also Putnam, 1995). Such an approach holds out the promise of enabling financial accounting theorists to synthesise aspects of classical and post-classical methods to achieve a higher level of awareness and understanding.

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