

ONE

Introduction and overview

Chapter Contents

- Theory as testable explanation
- A critical approach to accounting research

A number of authors (e.g., Brownell, 1995, p. 2) describe accounting researchers as 'parasites' who prey on the work of others to generate their findings. The term may be an overstatement, but as with most rash generalisations it contains more than a germ of truth: accounting researchers have little theory of their own (they rely on economics, finance, psychology, sociology and organisational behaviour as their major sources); they have no methods of their own (they are all adapted from the natural and social sciences); and they have few instruments of their own (with many of these originating in or adapted from the organisational behaviour literature). Malmi and Granlund (2009) note that theories recognized as being 'real theories' by accounting researchers, are generally those that have been adapted from other disciplines. Merchant (quoted in Brownell, 1995, p. 140) even suggests that organisational behaviourists are much better at developing survey instruments than their accounting counterparts.

The overall aim of this book is to facilitate the conduct of applied research studies in accounting, and to do this we must recognise our reliance on work in other disciplines. To accomplish this aim, a number of subordinate objectives may be identified, all of which will contribute to the overall goal:

- an understanding of contemporary research ideas in accounting, so that readers can identify and define research problems and prepare strategies for their solution;
- an awareness of alternative research methods, to facilitate the selection of the most appropriate method for addressing particular research questions;
- an ability to review existing research and to offer critiques of articles published in refereed journals; and
- an appreciation of the ethical constraints on the conduct of accounting research.

Research in accounting is concerned with solving problems, investigating relationships and building a body of knowledge. Because we rely to such a great extent on prior research in the natural and social sciences to do so, this volume will take a similar

approach in leaning on work in other disciplines where it helps to inform accounting research.

Bennett (1991) identifies four basic levels of research:

- Description – concerned with the collection and reporting of data related to what is, or was, the case. This would include means and standard deviations of individual variables, and correlations between pairs of variables.
- Classification – still descriptive, but easing the reporting process, and highlighting similarities and clustering through grouping and classifying (e.g., through the familiar cross-tabulation facility in most basic statistical packages).
- Explanation – an attempt to make sense of observations by explaining the relationships observed and attributing causality based on some appropriate theory.
- Prediction – going beyond the understanding and explaining of the prior stage, to model observations in a way that allows testable predictions to be made of unknown events.

We return to this structure in Chapter 5 when discussing alternative quantitative methods, but an early distinction between ‘explanation’ and ‘prediction’ is appropriate here, because, as in the natural sciences, we are able to make excellent predictions of accounting behaviour without the backing of a sound underpinning theory. Bankruptcy prediction modelling provides an excellent example. A number of researchers (e.g., Altman, 1968; Taffler, 1983; Agarwal and Taffler, 2007) have developed models that have proved very successful in identifying ‘distressed’ companies – those companies that will fail in the short term. These models are statistically excellent but the theory underpinning their content, in terms of the ratios to be used and the variables they represent, is extremely weak; the essential problem is that such theories as we have (e.g., Wilcox, 1971; Blum, 1974; Myers, 1977; Scott, 1981) do not generate very good predictive models!

Good research generates the sound evidence needed to overturn or revise existing theories. These assertions will, in turn, yield to revised theories based on better evidence, so that healthy competition between rival ideas will lead to better explanations and more reliable predictions. Two major processes of reasoning, ‘deductive’ (theory to observation) and ‘inductive’ (observation to theory), are important for theory construction and observation testing. Inductive reasoning starts with specific observations (data) from which theories can be generated; a generalisable pattern may emerge from further observations and repeated testing for compliance. The natural sciences, for example astronomy, provide numerous examples of inductive reasoning, thus Hawking (1998) provides a number of fascinating examples of theories revised, or still in question, with implications for the progress of accounting research. However, he notes that generalisations made on the basis of induction can never be regarded as ‘certain’, since just one contrary instance can cause them to be overturned:

- **BIG BANG VERSUS STEADY STATE.** From the late 1940s to the mid 1960s, two competing theories were prominent in offering alternative explanations of the origins of the universe. The ‘Big Bang’ theory recognised a singular event as causing an ever-expanding universe

in which matter (notably galaxies) becomes continuously more widely dispersed. The 'Steady State' theory, attributed to Bondi, Gold and Hoyle, on the other hand, suggested that matter was continuously being created to fill the gaps between existing galaxies. They argued that the universe had no beginning, and had been forever expanding, with new matter being created out of apparently empty space. The Steady State theory importantly provided testable hypotheses in suggesting that the universe should look the same at all times and from wherever it was viewed. But surveys of radio waves in the early 1960s showed that sources were more numerous in the past, and that there were many more weak (distant) sources than strong (close) ones. Further, microwave radiation studies in 1965 demonstrated that the universe did not have a common density – it had been much denser in the past. These observations provided disconfirmations of the Steady State theory, causing its abandonment.

- **NEWTON'S LAWS OF PHYSICS.** New theory emerges when a new observation arises which does not correspond with existing theory. Once the technology permitted accurate observations of the planet Mercury to be made, it was clear that there were small differences between its observed motion and that expected under Newton's Theory of Gravity. Einstein's general theory of relativity matched the observed motions of the planet in a manner that Newton's theory did not, providing confirmation for the new theory.
- **THE WAVE THEORY OF LIGHT.** We can attempt to explain the behaviour of light in terms of its being composed of either 'waves' or 'particles'. Each view produces a plausible explanation of behaviour – both of which are needed to affirm existing properties – but they are incompatible explanations which cannot exist simultaneously. New theories are required (possibly those associated with parallel universes) for a complete understanding of the incompatibility.

Deductive reasoning, on the other hand, starts with the theory and proceeds to generate specific predictions which follow from its application. The predictions can be verified, or otherwise, from subsequent observation. For example, in his seminal paper, Healy (1985) used agency theory to develop a bonus hypothesis which could be substantially verified through observations of how managers manipulated their accounting earnings to optimise their short-term bonus performance.

However, such a strict division of reasoning processes is not always helpful because interdependencies almost always exist: induction will usually imply some knowledge of theory in order to select the data to be observed (a common criticism of grounded theory advanced in Chapter 9); deduction will be dependent on the selection of the initial hypotheses for testing.

Even without such problems, the scientific position of 'objective measurement' has come under repeated attack, in both natural and social sciences, because the act of observation is itself 'theory-laden' and influenced by the motives and preferences of the observer. For example, Hopwood (1987), in management accounting, and Hines (1988), in financial accounting, argue that accounting helps to create the 'facts' that it is supposedly reporting. More radical approaches (e.g., Tinker and Niemark, 1987) suggest that accounting distorts practice in a systematic manner. Such concerns have aided the development of new approaches: an interpretive perspective and a critical perspective.

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- **AN INTERPRETIVE PERSPECTIVE** – From an interpretive perspective, human actions are the result of external influences. These actions have both intentions and reflections, and take place within a structure of rules which binds the participants. The task of the researcher goes beyond measurement to developing an understanding of the situation. To do this effectively, active participation, rather than detached observation, may be required. Since the ‘action’ may be interpreted ambiguously when taken out of context, this perspective places the fundamental emphasis on the understanding of the process. In an accounting context, Arrington and Francis (1989) provide an example, while Willmott (2008) gives an excellent review of alternative approaches.
- **A CRITICAL PERSPECTIVE** – The critical approach expands on the scope of the interpretive approach by focusing on the ownership of knowledge and the associated social, economic and political implications. An empirical approach is criticised on the grounds that the research process is value-laden, and that the acquisition of knowledge provides the opportunity to oppress those being researched. In an accounting context, Tinker (1980) provides an example of this approach.

Table 1.1 summarises the differences in research assumptions, process and outcomes associated with each of these three major approaches.

Kuhn (1970) suggests that researchers are concerned with problem-solving within a single framework of widely accepted beliefs, values, assumptions and techniques. This shared framework, or view of the world, he termed a paradigm, so that a ‘paradigm shift’ corresponds with some revolution where the existing framework and theories can no longer cope with the volume of disconfirming evidence. Kuhn neatly illustrates such a shift by reference to a simple psychology experiment:

Subjects viewed cards from a deck. The deck included some unusual cards, including black hearts and red spades, but the subjects were not informed in advance about their presence. Initially the subjects saw only ‘hearts’ and ‘spades’, because they believed that only ‘red hearts’ and ‘black spades’ existed; only with repeated viewing did they grasp that these cards were not typical of a normal deck. Then they could recognise the cards that existed rather than the ones they were expecting.

In accounting research, the parallels might be the paradigm shifts associated with the ideas introduced by Ball and Brown (1968) and the difficulty they had in getting a paper published which questioned the existing paradigm by showing a link between stock prices and accounting earnings, through the abnormal performance index. A similar, though perhaps less radical, movement is associated with Watts and Zimmerman (1978) and their popularisation of agency theory in an accounting environment.

What is inescapable is that we are dealing with people, and in the research community that means individuals with their own agenda and with reputations to build and protect. The natural sciences are littered with character assassinations of individuals and their work, by others who have been less than willing to accept the impact of new findings on their own fiefdoms.

Table 1.1 Three alternative approaches (adapted from Connole, 1993, p. 37)

Positivist	Interpretive	Critical
<p>1. <i>What is the approach modelled on?</i> Classical investigation founded in the physical sciences.</p>	Historical, literary and existential studies in which the subjective understandings of subjects are significant.	Marxist and interpretive studies which focus on the insights and judgements of the subjects.
<p>2. <i>What does it assume about reality?</i> Reality is unitary and it can only be understood by empirical and analytic methods, i.e., the scientific approach.</p>	There are multiple realities which require multiple methods for understanding them.	There are multiple realities which are made problematic through distorted communication.
<p>3. <i>What is the foundation of data?</i> Disciplined rules for observation.</p>	Meanings are the basis of data: meaning precedes logic and fact.	Meanings are found in language and social behaviour and they precede logic and fact.
<p>4. <i>How is observation done?</i> Through clear and unambiguous rules which are not modified by the setting and are totally independent of it.</p>	Through the social, linguistic and cognitive skills of the researcher.	Interpretive methods, plus critical self-reflection concerning the grounds of observation.
<p>5. <i>What is generated?</i> Evidence and generalisable laws which are not affected by contexts and have nothing to do with the way in which they were discovered in the first place. Objectivity depends upon the removal of error and bias which is related specifically to the logic of observation and measurement.</p>	Knowledge which is dependent on the process of discovery. The integrity of the findings depends upon the quality of the social, linguistic and cognitive skills of the researcher in the production of data analyses and conclusions.	Knowledge which falls within the interpretive framework, but which also serves the purposes of assisting personal liberation and understanding, and emancipation from forces constraining the rational independence of individuals.
<p>6. <i>What interests are inherent?</i> Prediction and control, technically exploitable knowledge, and explanation.</p>	Understanding at the level of ordinary language and action. Discovering the meanings and beliefs underlying the actions of others.	Interpretive interests and those which underlie other forms of inquiry. Radically improving human existence. Practical and public involvement in knowledge formation and use.
<p>7. <i>What values are inherent?</i> Science and scientific knowledge are inherently value-neutral.</p>	Science and scientific knowledge have both to be interpreted in terms of values they represent.	Science and knowledge are never value-neutral: they always represent certain interests.

Sir Humphrey Appleby, in Lynn and Jay (1987), outlines the four stages of the process necessary to discredit an unwelcome report. The parallels between the fictitious Department of Public Administration and academia are uncomfortable, where unwelcome findings might arise from academic competitors:

- 1 **REFUSE TO ACCEPT THE FINDINGS** on the basis that they could be misinterpreted, and that a wider and more detailed study is required.
- 2 **DISCREDIT THE EVIDENCE** on the basis that it is inconclusive and the figures are open to other interpretations, or that the findings are contradictory and leave important questions unanswered.
- 3 **UNDERMINE THE RECOMMENDATIONS** because they say nothing new, and provide insufficient information on which to draw valid conclusions.
- 4 **DISCREDIT THE RESEARCHER** by questioning his or her integrity, competence and methods employed.

We thus have doubts about the researchers, their research questions, their research methods, the means of data collection and analysis, and the validity of the interpretation and recommendations – all issues to which we will return.

Theory as testable explanation

Faced with a set of diverse observations, we can establish a set of tentative explanations which help to make sense of the diversity. Such explanations constitute theory. In any set of circumstances, there will usually be multiple theories available to explain the observations. The systematic collection of further data allows for the testing of the alternative theories so that we can establish which of the existing theories best explains the facts. A layman's perspective of 'theory' is cynically expressed in Michael Crichton's, *The Lost World*, as: 'A theory is nothing more than a substitute for experience put forward by someone who does not know what they are talking about' (1995, p. 67).

The data collection itself allows only a descriptive approach (e.g., means, standard deviations, ranges, correlations); we cannot attempt to attribute causation in any meaningful way without recourse to an explanatory theory. We are always looking for another theory which may fit better, so that, as Popper (1959, p. 104) suggests, a 'genuine test of a theory is an attempt to falsify it or refute it'. We look for disconfirmations rather than confirmations.

In the short term, this may not be successful. In accounting, we witness the frequent and numerous 'anomalies' to which the Efficient Markets Hypothesis (EMH) is subject, but we have no other widely accepted theory of the manner in which stock prices react to the availability of relevant information.

Popper's suggestions are very attractive in providing a powerful empirical methodology for subjecting theories to attempts to refute them. However, this

position is not always ideal because the process of ‘observation’ in itself may be fallible. Thus, Hawking (1998) reports Heisenberg’s Uncertainty Principle:

If we are to predict the future position and speed of any particle, then we require accurate measurement of both its present position and current speed. Heisenberg did this in 1926 by shining light on a particle, and observing the resultant scattering of light in order to reveal its position. However, to determine the position of the particle accurately an amount of light needed to be used which changed the speed of the particle in an unpredictable way: the more accurately he tried to measure position, the less accurate was the measurement of speed!

The Uncertainty Principle has wide implications for research conducted in any environment, where it is impossible to measure the size and speed of a particle without altering all other characteristics in the process of measurement. We have a parallel situation in accounting research where the actions of the participants in ethnographic, experimental, survey or fieldwork impacts on the outcomes of the measurement process.

Three fundamental criteria exist to judge whether theory fits observation:

- 1 **CO-VARIATION** – even where no causality exists, we would expect the two variables to move together so that a high degree of correlation exists between the two variables. Where there is no co-variation it will be difficult to establish a causal link.
- 2 **CAUSE PRIOR TO EFFECT** – if a causal link is to be established, then the ‘causal event’ should occur before the ‘effect event’. The sequence of events can therefore help to establish an explanatory direction.
- 3 **ABSENCE OF PLAUSIBLE RIVAL HYPOTHESES** – the third rule seeks to eliminate alternative explanations of the events as being implausible. This may only be possible in the present, because future researchers may develop competing explanations of the events from a re-analysis of the data.

Consider, for example, the voluntary disclosure of information in corporate reports and analyst following (i.e., the number of analysts examining the performance and reporting on the disclosures of large companies). There is a relationship between these two variables – they co-vary: the volume of voluntary disclosures and the number of analysts reporting move together. But which is causing which? Rival hypotheses suggest that:

- a companies are supplying more information voluntarily to the market to signal their intentions and reputation, attracting the attention of more investment analysis;
- b investment analysts are focusing their attention on particular companies and demanding more information and more detailed disclosures.

The existing empirical evidence is less than convincing: Lang and Lundholm (1996) find (a); but Walker and Tsalta (2001) provide only weak evidence for (a), but stronger evidence to support (b). Clearly, more empirical work is required to clarify the nature and direction of causation.

A critical approach to accounting research

Researchers must demonstrate a healthy scepticism towards both their own findings and those of other researchers. They must adopt a critical posture, questioning everything that they read until sufficient evidence has been provided for them to be satisfied with the quality of the outcomes. The development of critical appraisal skills is a fundamental requirement in researchers, so that they can distinguish between good and bad research, and clearly identify flaws of argument, methodology and analysis.

Honest and transparent reporting of research practice is an ethical duty of those participating. Researchers should report everything that they did, why they did it, and how they did it. If they have doubts about any stage of the procedure, then these should be stated, along with their likely implications and what, if anything, has been done to overcome these doubts. Where researchers have been 'economical with the truth', this is usually apparent in their papers and is often an indicator of bad research.

Students frequently struggle initially when they are asked to critique published articles. They are often in awe of the reputation of the authors, or doubt whether they are able to offer sensible criticism of papers which, after all, have already undergone editorial scrutiny and double-blind review. Despite the above, some flawed papers do get published, and these are not always in lower-tier journals (see Hartmann and Moers, 1999, for their critique of 28 papers on contingency analysis in three top accounting journals – *Accounting Organizations and Society* (AOS), *The Accounting Review* (AR) and *Journal of Accounting Research* (JAR) – in which they identify problems in the design and analysis of 27 of the studies!). Similarly, Dowd (2004, p. 510) notes the publication, in the respected tier of economics literature, of papers with nonsensical assumptions. With appropriate guidelines as to the right questions to ask, students can quickly develop some confidence in their ability to spot flaws and omissions. For example, Abernethy et al. (1999) provide a stimulating critique of the three subsequent papers in the same, outstanding, edition of the journal *Accounting & Finance*.

We would usually want to address the following:

- 1 **WHY IS THIS ARTICLE INTERESTING/IMPORTANT?** The paper must offer some new insights which constitute a contribution to knowledge. These insights should be non-trivial, so that they can be embraced either in further theory development or in recommendations for improvement.
- 2 **ARE THE OUTCOMES IMPORTANT?** Effectively, does the paper pass the 'so what' test? Will anyone be interested in the outcomes of this research, or will it have any implications for future practice? Would the scope of the research be well-regarded by competitive grant authorities? This has important implications for those papers which produce 'negative' findings, that is, they test reasonable hypotheses based on the research literature, but their datasets fail to support any of the expectations. These findings still make a contribution in that they demonstrate that findings from elsewhere (often other disciplines) do not hold in accounting, but their negativity may restrict their publication opportunities.

- 3 **WHAT MOTIVATES THE AUTHORS TO WRITE THIS ARTICLE NOW?** The paper may be clearly addressing issues of contemporary concern; on the other hand, it may be addressing more historical issues and/or be using 'old' data. If we have the latter, we may be dealing with an old paper recently recycled, or a paper which has been through multiple iterations at several different journals before being deemed 'publishable'.
- 4 **WHAT IS THE RESEARCH PROBLEM/QUESTION?** We are looking for a clear statement of the problem very early on in the paper, so that its objectives are readily apparent. If we reach page 11, say, of the paper without a clear idea of its direction, or any sort of research model, then perhaps the authors need to readdress the fundamental purpose of the research.
- 5 **WHAT THEORY OR THEORETICAL FRAMEWORK UNDERPINS THE RESEARCH?** Without some theoretical foundations, we have a problem-solving exercise or a consultancy project, neither of which should be gracing the pages of a refereed journal. There must be some theoretical justification for the question being addressed and the research approach adopted. Theory will often not come first in the research process – it will frequently be preceded by an interesting idea or a perplexing observation. But we require some theoretical explanation for the relationships under investigation before we have the basics of a refereed journal article. Observed deficiencies in this area usually fall into one of four categories:
- the underlying theory is either non-existent or extremely thin;
 - the theoretical context is there but appears to have been tacked on as an afterthought – usually at the beginning of the paper and often written by a co-author. Examination of writing styles suggests that we frequently do not have a seamless divide between 'theory' and 'conduct of research';
 - the theoretical arguments are unconvincing, so that there are competing theories that may reasonably have been adopted in the paper but have been overlooked;
 - a sound theoretical framework but findings which are totally at odds with theory. Apparently, a competing theory may be more appropriate, although this is unknown to the authors at the time.
- 6 **WHAT ARE THE KEY MOTIVATING LITERATURES ON WHICH THE STUDY DEPENDS?** There will normally be a small number of seminal pieces of literature which are driving the research. If any of these are themselves unreliable, it may cast doubt on the state of the foundations on which the paper is based. If one of the papers is an unpublished conference or working paper from several years before, then alarm bells ring to question why that piece has not itself been published in the refereed literature. If key seminal pieces of literature have been overlooked, then again, the integrity of the findings is reduced.
- 7 **WHICH RESEARCH METHOD HAS BEEN CHOSEN?** There should be a justification for the chosen method, and a clear preference over alternatives. The method should be consistent with both theory and literature and, ideally, prior empirical studies in the field will have adopted similar methods. Most importantly, we want to see a research method that has evolved rather than one that has been selected first, even before the research question has been fully developed. The use of survey methods should always be questioned in this way since, frequently, they seem to have been selected without explanation of the elimination of alternatives. Ideally, we should be able to trace through the emergence of abstract concepts, from theory, through their operationalisation and measurement, so that any hypotheses are entirely consistent with both theory and literature.

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- 8 **HOW HAS THE SAMPLE BEEN SELECTED?** Details on sample selection are often sketchy in many articles, perhaps because the authors feel vulnerable about the procedures adopted. Sometimes (see, for example, Young, 1996; and Van der Stede et al., 2005) the actual sample size employed is omitted, as is the response rate. Both omissions should be regarded as bad news. It is usually clear that scientific methods have not been adopted (unfortunately, far too commonly in accounting research) where there is an over-reliance on convenience samples. What may be apparent is an attempt by the authors to obfuscate in this regard, to overlook detail and try to create an impression that the sample selection is more systematic than it has actually been.
- 9 **HOW HAVE QUESTIONS OF VALIDITY BEEN ADDRESSED?** Choice of research method should address issues of validity. Where experimental methods have been employed, we would anticipate questions of internal validity to be paramount; where field studies are involved, we would expect issues of external validity to be addressed. For survey methods, we would anticipate the focus to be more on the reliability of the test instrument and the rigour of the subsequent statistical analysis, rather than on validity issues.
- 10 **HOW HAVE THE RESULTS BEEN ANALYSED?** We want to see the simplest analysis of the results consistent with the relationships being explored. We do not wish to see unnecessary complexity; this will make the paper less readable and tend to mask the findings and their significance. On the other hand, most academic accountants are only 'amateur' statisticians; if the level of their analysis is inadequate, then they may need to bring in a statistician as co-author (evidenced by the number of 'quant jocks' appearing as third or fourth authors on accounting papers to satisfy the reviewers). Importantly, we do not wish to see the method of analysis driving the study. In just the same way as the research method should not precede the research question, then neither should the method of analysis. For example, I recall a paper of my own (M. Smith, 1992) presented at a conference but never published. It attempted to show the advantages of using multidimensional scaling (MDS, then a little-used technique in the accounting literature) for problem-solving, but the journal referees rightly observed that the method was inappropriately sophisticated for a relatively simple research question. MDS was abandoned, simpler methods instituted and the revised paper eventually published as Smith (1996).
- 11 **ARE THE CONCLUSIONS AND RECOMMENDATIONS CONSISTENT WITH THE FINDINGS?** Effectively, does the paper hold together? Is the title appropriate? Do the abstract and introduction lead us to expect what we find at the end of the paper? In many papers, the final sections are the weakest and may not do justice to the breadth of the research conducted. We look for explanations, limitations and a future research agenda.

Let us now consider how this framework may be applied to a critique of a published piece. Naturally, I choose one of my own publications (Smith et al., 2001) for the treatment because a knowledge of the history of the development of the paper, from an insider's perspective, can be most instructive. Readers will be able to make the most of the subsequent discussion if they are first able to read a copy of the paper, and for this purpose the complete paper is reproduced as Appendix 2.

- 1 **INTERESTING NEW INSIGHTS:** The paper posits an interesting connection between (1) audit firm; (2) manner of conduct of the audit; and (3) classification of audit firms based on their procedures and culture. The paper also attempts to impose a global perspective by

employing findings from the USA, the UK and Australia. But neither the data nor the supporting literature is new, and it compromises the originality of the paper.

- 2 **IMPORTANCE:** The paper is important if it makes a contribution to knowledge. This may be a contribution to theory development or implications for business practice. If the paper can demonstrate a relationship between 'auditor' and the manner in which the audit has been conducted, then this makes a contribution, even though it may only be of historical relevance. Such a relationship is shown for 1987/88 data, but evidence is also presented to suggest that this relationship no longer holds. The absence of a current relationship suggests that the paper has no implications for current auditing practice. The reasons why a relationship between the audit firm and its propensity towards tolerance of particular accounting policies among its clients is by no means clear.
- 3 **MOTIVATION:** The timing of the paper is problematic. It is published in 2001 but uses data predominantly from 1987/88. There is a danger of its being regarded as a historical piece with little relevance to current practice. The authors justify the use of this dataset in that the Kinney classification, the target test of the paper, is based on data relating to the Big 8 group of accountants, with 1988 being the last year of existence of the Big 8 in Australia, prior to extensive merger activity in the sector. There is the suggestion, though, both from the paper itself and the references cited, that the data have been used primarily to generate failure prediction models for the Western Australian government (i.e., Houghton and Smith, 1991) and that the further use of this data in this paper may be incidental and opportunistic.
- 4 **PROBLEM STATEMENT:** The problem statement is quite clearly stated as:

Accounting Policy Changes = f {auditing firm}, where both sides of this equation are elaborated and measured for a large number of companies:
 - Accounting policy changes: discretionary/mandatory; income increasing, income reducing, neutral.
 - Auditing firm: by individual name, and by grouping according to classifications developed by Kinney (1986) and Moizer (1998).
 - A number of extraneous variables (notably firm size, financial performance and industry) are also examined to determine their impact.
- 5 **THEORETICAL FRAMEWORK:** This remains something of a problem with this paper, despite strenuous efforts to overcome omissions. The literature demonstrates that there are differences between auditors, and in the procedures that they adopted for audit in 1988 (i.e., Cushing and Loebekke, 1986; Sullivan, 1984). However, why these procedural differences between auditors translate into differing tolerances towards income-impacting accounting policy changes is unclear, and is largely attributable to unpublished anecdotes from practising auditors and the discussion arising in a single paper (Dirsmith and Haskins, 1991).
- 6 **MOTIVATING LITERATURES:** Relatively few articles, noted above (i.e., Sullivan, Kinney, Cushing and Loebekke, Dirsmith and Haskins) motivate this paper, while Terry Smith (1992) and Peter Moizer (1998) provide the opportunity for UK comparisons. The pivotal paper is Dirsmith and Haskins (1991), published after the conduct of the data collection; there is thus a strong suspicion that interesting findings have arisen from data mining operations in 1988, for which Kinney (1986) provides a conceptual framework, but that publication must wait for a suitable theory. There is very little other supporting literature, though self-citation by the authors is also revealing:

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- Houghton and Smith (1991) relates to failure prediction models constructed with the same data and is employed here to provide a measure of overall financial performance;
 - Smith (1998a) reports current UK findings linking auditor with attitude to accounting policy change;
 - Smith and Kestel (1999) update the present study with a time series analysis, but the results are apparently insufficiently interesting to constitute subsequent publication in a refereed journal;
 - Brown (1988) reports on the most appropriate means of conducting statistical tests with contingency tables.
- 7 **RESEARCH METHOD:** Archival methods are employed, since they are the only realistic alternative given the nature of the data: namely, historical, documentary, and covering many companies that are no longer in existence. The authors' access to a dataset comprising the population of Western Australian public companies is a considerable strength of the paper. Data collection is meticulous and involves checks for consistency both between individual researchers and for temporal validity.
- 8 **SAMPLE SELECTION:** The paper accesses the annual reports of all 463 publicly quoted companies in Western Australian (WA), so does not encounter any sampling issues other than a restriction on the nature of statistical tests that may be employed because of using a population rather than a sample from a normal distribution.
- 9 **VALIDITY ISSUES:** There are potential internal validity threats consequent upon the failure to consider competing theoretical explanations for the observations. The incidence of accounting policy change is apparently associated with auditing firm, but both the direction of causation for the relationship and alternative auditor motivations might be considered. The authors acknowledge the lack of external validity in the study – the applicability of the findings to other time periods and other datasets – in that conditions have changed so substantially since the data collection period, that the procedures adopted by all auditors are now very similar.
- 10 **ANALYSIS:** The fundamental analysis is relatively unsophisticated, involving the comparison of 'observed' and 'expected' frequencies through a chi-squared test. A variation on the traditional approach is introduced to take account of an ordering effect in the contingency tables, the power of the tests being increased with the use of Kendall's-tau. (A co-author with specialist statistical publications has been included to address testing issues, potentially in response to reviewer concerns on previous versions.) A comparative fundamental analysis for UK data (alluded to in Smith, 1998a) is apparently not possible, and further analysis is restricted to tertiary sources.
- 11 **CONCLUSIONS:** There are no formal conclusions or recommendations, rather a discussion of other interesting findings in related fields which may impact on the integrity of the outcomes. The findings of this study are linked to merger activity in the Big 8, showing a pattern with considerable similarities to past successes. The paper suggests that future merger activity in the sector may be influenced by the organisational culture aspects of the Kinney classification and the clustering of companies generated by Moizer (1998); thus, if we had been looking at potential suitors for Arthur Andersen, say, then the analysis suggests that Ernst & Young would have provided potentially the most successful alternative.

Such a critique is revealing, giving glimpses of a less-than-optimum approach adopted in the development of this particular paper. Data were collected for the specific purpose of generating failure prediction models for the WA government, and corporate monitoring of distressed enterprises (i.e., Houghton and Smith, 1991). The interesting auditor findings were generated at the same time, but there was no substantive theory to justify the observed relationship – and, consequently, no research paper. Only with the emergence of new theories (e.g., Dirsmith and Haskins, 1991), which might motivate the study, could further development towards a publishable paper proceed.

Clearly, research is not always simple, systematic and clean – despite the sanitised versions that we read in the published journals. The research process can be both chaotic and exciting, and very rarely proceeds exactly according to plan. Unfortunately, this impression is rarely created by what we read because published pieces usually have happy endings – positive findings and co-operative participants. For a more realistic version of events, we must rely on books like this, conference presentations and research workshops!

Armed with a critical and sceptical approach to the research of others, we can now start to develop the skills required to conduct competent research of our own, and commence a sequence which will eventually result in the publication of our research findings.

Further Reading

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