This unique and popular text, now in a sixth edition, clearly and succinctly guides Accounting students and researchers in their understanding and conducting of research from conception to completion, across a wide range of research methods, including quantitative, qualitative and mixed-methods research.

This latest edition provides new and extended coverage, including the role and impact of social media, big data analytics, data mining, and emerging and disruptive technologies, such as Blockchain. There is also a renewed focus on the role of ethics in Accounting research.

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Malcolm Smith was Foundation Professor of Accounting and Dean (Research) at the University of South Australia.

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RESEARCH METHODS in ACCOUNTING

SIXTH EDITION

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Contents

List of Figures

List of Tables

About the Author

Malcolm Smith has held professorial positions in accounting at universities in the UK (Leicester, Sheffield) and Australia (Darwin, Perth, Adelaide) and visiting/adjunct appointments to universities in the UK, Australia and Malaysia. He was Foundation Professor of Accounting and Dean (Research) at the University of South Australia.

Malcolm's publication and supervision record shows him to have been amongst the most prolific accounting researchers in Australia, with an output which includes 16 books, 13 book chapters and over 150 journal publications. These articles include some in the top-tier of accounting literature, most notably: *Journal of Business, Finance and Accounting; Accounting and Business Research; British Accounting Review; Journal of Business Ethics*; *Accounting and Finance*; *Accounting, Auditing & Accountability Journal*; and *Journal of International Financial Management and Accounting.* Two of his publications have received 'best article' awards from the Financial and Management Accounting Committee of the International Federation of Accountants in New York, based on global comparisons of contributions to the management accounting literature.

Malcolm's research efforts have focused on three major themes, in each of which he has an international reputation:

- Financial communication: concerned with the analysis and interpretation of corporate narratives.
- Strategic management accounting: concerned with the diffusion of accounting innovations and their implementation in the field.
- Failure prediction: concerned with the use of financial and non-financial information to identify distress and vulnerability.

Malcolm has received numerous awards for teaching and supervision, and has supervised over 20 doctoral candidates to completion, for most of whom English was a second language.

Preface

The data explosion of the last few years from social media and 'big' data, along with the impact of disruptive technologies, like blockchain and machine learning, have changed the face of accounting research. While building on those features that proved to be so successful in the previous editions of this text, this first post-pandemic edition gives due recognition to the revolution that is taking place with respect to both data sources and analytical methods. The aim of the book remains to provide an insider's view of the research process, by focusing on the actual choices made in the conduct of accounting research projects. In doing so we emphasise the importance of planning and preparation, in order to have a realistic perception of what might go wrong, so that appropriate corrective action can be taken. The focus remains on experimental methods, the use of archival data, survey and field study methods; we do not address issues of finance, capital markets research or stock-price-related accounting research on the fringes of finance. The reactions of readers to the first five editions of the book have been most encouraging, but they have highlighted the acknowledged weaknesses of earlier editions, most notably with regard to 'theoretical foundations', 'qualitative methods', 'ethics in research' and 'content analysis'; new sections have been added to this text to address these weaknesses along with new and expanded sections devoted to disruptive technologies, blockchain, social media, big data analytics, data mining, impression management and readability. New examples have been added together with updates to the text throughout.

Most other texts in this area remain long, over-theoretical and not particularly user-friendly. This book deliberately plays down philosophical considerations to adopt a practical approach that takes the reader from the initiation of the research idea right through to the publication of the research findings. The intended readership is wide, embracing instructors, doctoral candidates and academics starting, or re-starting, their research careers, but the focus is firmly on research students. Although the examples are mainly accounting-based, much of the material has been sourced from the management, organisational, psychological and information systems literatures and so will also be relevant to more general business applications. The practical examples employed are usually UK- or Australia-based, these being the two countries in which I have extensive experience of teaching, research, supervision and examining, but the principles should normally adapt easily to alternative environments.

An early distinction between 'methods' and 'methodologies' in research is essential because the two are so often confused, or else used interchangeably. Research methods are concerned with the technical issues associated with the conduct of research; research

methodology is concerned with the philosophies associated with the choice of research method. After Chapter 1 this book is almost exclusively concerned with the former.

Chapter 2 examines the research idea, and the documentary sources that might help to generate research topics and aid their development. A number of examples, many from non-accounting environments, are used to illustrate the research sequence, and to examine research that is seeking either to improve outcomes, to explain improved outcomes through new theory or to examine the improvement process itself. Theoretical frameworks and research models are used extensively here to help the reader to picture the key variables and relationships underlying their research, and to facilitate their generating a respectable research proposal.

Theory is the focus of Chapter 3, on the basis that 'good research is founded on good theory'. The chapter addresses the sources of the theory widely applied in accounting research, but mostly drawn from other disciplines. In the space available it cannot hope to be all-embracing, but does give an indication of the diversity of what is available, as well as directions for likely future developments. This chapter is expanded to provide increased recognition to non-economic theories, and to reflect the growing importance in accounting of diffusion theory and actor-network theory, among others. Chapter 4 addresses the increasingly important ethical considerations that underpin the conduct of 'credible' and 'relevant' accounting research, and the subsequent publication of research findings. It highlights the confusion that is still apparent among many academics as to what constitutes 'acceptable' deception and unethical conduct, and uses examples to specify the necessary guidelines for good practice. The importance of ethical research practices is reflected in the early positioning of this chapter, so that it still precedes both *data collection* and *data analysis* in this new edition.

Recognition of the importance of theory, reliability and validity as desirable characteristics of accounting research, lead, in Chapter 5, to a consideration of the issues of data collection and management, with a renewed emphasis on online sources, social media and data mining. This is followed in Chapter 6 by a discussion of data analysis and hypothesis testing; this chapter remains unashamedly quantitative in nature, and considers panel data analysis, partial least squares and structural equation modelling as well as the growing impact of big data analytics. Chapter 7 has also been expanded to address further aspects of the analysis of qualitative data and to reflect the increasing importance of mixed methods research and content analysis in accounting.

Chapters 8 to 11 are devoted, respectively, to the core forms of accounting research: experimental, survey-based, qualitative and archival. Numerous examples are used to demonstrate the relative advantages of alternative methods so that researchers can both make an informed choice and justify their preferred approach. Research can be based on quantitative or qualitative methods, and both should be equally acceptable as long as the most appropriate method has been chosen. The majority of the readers of this text will likely be higher-degree by research candidates (Master's or PhD) so Chapter 12 is devoted to supervisor–candidate relationships, highlighting the mutual responsibilities of all parties to the supervision process, from the outset right up to the outcome of the examination itself.

Publication is the natural target output of the research process, and Chapter 13 addresses the complexity of the publication process. In doing so it recognises that we are working in a dynamic environment; what was once acceptable in accounting research is no longer so because of a more appropriate emphasis on research ethics; what is publishable, at all or in specific journals, changes too, both with the passage of time and the retirement of particular journal editors. Many journals have remained conservative in the type of research they will publish, often on the grounds that it is difficult to demonstrate that 'new' methods constitute 'good' research in the same way as the traditional methods, because of problems with replication. However, there is ample evidence that this situation is changing rapidly – most notably in the surge of research papers devoted to readability, content analysis and data mining applications now appearing in top US journals. The emphasis on journal and university rankings, and associated funding systems based on the quality of publications, provide fresh difficulties. The provision of 'acceptable journals' listings by many universities, and the prohibition of publication elsewhere, perpetuate the position of the well-established journals, while making it extremely difficult for the editors of other journals to attract quality submissions. The opportunities for innovative new journals are also severely diminished in such circumstances. The generally negative impact of such publication exercises is documented in detail in Chapter 13.

Contributions to the profession by academic accountants are generally not well regarded, either by one's colleagues or by government bodies providing funding based on publications performance, even though, arguably, the education of the potential employers of our students might be seen as an important part of our jobs. In this context it is no coincidence that we continue to witness a widening of the relevance gap between 'research' and 'practice' during a period when positive accounting research has all but supplanted normative research.

This gap between research and practice has been amply illustrated over recent months by the interventionist policies introduced to address issues with the COVID-19 pandemic and with climate change. Both provide examples of our failure to match theory with data, so that we are unable to build adequate *causal* models for such as lockdowns, social distancing, global warming etc. Our models based on correlations and flawed assumptions only serve to emphasise the importance of our message from Chapters 3 and 5, in that good research needs good theory.

This book aims to provide a treatment of research methods that will be of use to both accounting practitioners and those contemplating the conduct of research projects. The explosion in the availability and importance of online sources and methods to accounting research is further reflected in this volume, and students will find many references to online sources now regarded as essential components of the research journey.

Space restrictions mean that we cannot hope to tackle all details of the application of different research methods, or the associated intricacies of complex quantitative methods. But the book should encourage candidates to become more reflective, and to keep asking themselves 'why?' when they make a specific research choice. This is an exciting time for accounting research, and if this book causes one paper to be published that would otherwise have gone unpublished, then all will have been worthwhile.

Acknowledgements

This sixth edition could not have been completed without the help of numerous colleagues, most notably Zubaidah Ismail and Saiyidi Mat Roni of Edith Cowan University, Glen Lehman and Basil Tucker, University of South Australia and doctoral colleagues from Universiti Teknologi Mara, Malaysia. However, as is normal, all errors and omissions remain the responsibility of the author.

ONE

Introduction and Overview

Chapter Contents

A number of accounting researchers, including the late Peter Brownell, have described their fellow researchers as 'parasites' who prey on the work of others to generate their findings. The comment may have been delivered 'tongue-in-cheek' and to be regarded as a gross 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 recognised as being 'real theories' by accounting researchers are generally those that have been adapted from other disciplines. As academics, we rightly refer regularly to the research literatures of other disciplines, but this book will demonstrate how accounting research can adopt a unique perspective towards human activity.

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. It is thus important that we do not confine our reading to the accounting literature when the finance, management, information systems and organisational literatures may be taking more innovative positions. The journals *Strategic Management Journal*, *Journal of Information Systems* and *Organizational Research Methods* are particularly noteworthy in this respect.

We can specify 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, classifying and cross-tabulation.
- **Explanation** – an attempt to make sense of observations by explaining the patterns and 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 6 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 – both of which are needed to affirm existing properties – produces a plausible explanation of behaviour, 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 Chapters 3 and 10); 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, Hines (1988), in financial accounting, and Parker (2012) 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.

- **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 early example of this approach.
- **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 early example of this approach.

Table 1.1, inspired by Connole (1993: 37), summarises the differences with each of these approaches.

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

		Positivist	Interpretive	Critical
1	*What is the approach modelled on?*	Classical investigation founded in the physical sciences.	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.
2	*What does it assume about reality?*	Reality is unitary and it can only be understood by empirical and analytic methods, i.e. the scientific approach.	There are multiple realities which require multiple methods for understanding them.	There are multiple realities which are made problematic through distorted communication.
3	*What is the foundation of data?*	Disciplined rules for observation.	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.

		Positivist	Interpretive	Critical
4	*How is observation done?*	Through clear and unambiguous rules which are not modified by the setting and are totally independent of it.	Through the social, linguistic and cognitive skills of the researcher.	Interpretive methods, plus critical self-reflection concerning the grounds of observation.
5	*What is generated?*	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.	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.
6	*What interests are inherent?*	Prediction and control, technically exploitable knowledge, and explanation.	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.
7	*What values are inherent?*	Science and scientific knowledge are inherently value-neutral.	Science and scientific knowledge both have to be interpreted in terms of values they represent.	Science and knowledge are never value-neutral: they always represent certain interests.

In a similar vein, Malsch and Salterio (2016: 5) modify the Power and Gendron (2015) structure which highlights the distinguishing characteristics of positivist and interpretive approaches to fieldwork, as follows:

	Positivist	Interpretive
Research aims	Empirical generalisation	Empirical examination of cause-and-effect global concerns
Focus	Breadth	Depth
Analytical emphasis	Cause-and-effect	Complexity of human behaviour
Explanation	Prediction	In-depth understanding
Viewpoint	Reality is external	Reality is socially constructed
Objectivity	Researcher and research	Subjective interpretation process objective throughout
Flexibility	Flexibility constrained	Flexibility paramount by objectivity
Writing style	Concise	Reflects real-world complexity

In view of the increasing importance of 'communications'-related papers in the accounting literature (as reflected by research concerning financial reporting, voluntary disclosure, content analysis, readability, impression management, whistle-blowing, etc.)

it is appropriate to extend these conceptual frameworks to include that developed by Merkl-Davies and Brennan (2017) for accounting communications, and designed to address narrative reports, press releases and websites. They specify eight traditions of communication research which can be used to specify illustrative examples from the accounting literature to identify future research opportunities:

1 **Mathematical tradition** – concerned with message transmission and following Shannon and Weaver's (1949) communication theory (e.g. Barton and Mercer, 2005).
2 **Socio-psychological tradition** – concerned with information processing by corporate actors and their audiences, following attribution theory or accountability theory (e.g. Merkl-Davies et al., 2011).
3 **Cybernetic/systems-oriented tradition** – concerned with interdependence, following organisational information theories (e.g. Brennan and Merkl-Davies, 2018) characterised as legitimacy theory, stakeholder theory or institutional theory.
4 **Semiotic tradition** – concerned with signals and symbols in communications (e.g. Crowther et al., 2006).
5 **Rhetorical tradition** – concerned with argument and persuasion in accounting communication (e.g. Brennan and Merkl-Davies, 2014; Higgins and Walker, 2012).
6 **Phenomenological tradition** – concerned with interpretation of symbolic meaning (e.g. Prasad and Mir, 2002).
7 **Socio-cultural tradition** – concerned with values and norms and often inspiring the use of legitimacy theory (e.g. Erkama and Vaara, 2010) or reflecting Goffman's (1959) theory of impression management, as in Parker and Warren (2017).
8 **Critical tradition** – concerned with power dynamics and conflict and often reflecting Marxism or critical theory (e.g. Tregidga et al., 2014). But the seminal works in this tradition remain those of Russel Craig and Joel Amernic (e.g. Craig and Amernic, 2004).

Merkl-Davies and Brennan also devise a heuristic to view similarities and differences of approach, and note that current research is dominated by the first three of these traditions, being essentially one-directional and typical of corporate reporting. They identify future avenues for research in terms of overlooked aspects, under-used perspectives and 'problematisation' (see Sandberg and Alvesson, 2011) to challenge assumptions and establish linkages between alternative perspectives (e.g. Hoque et al., 2013).

These linkages are explored by Brennan and Merkl-Davies (2018) who develop the concept of 'connectivity' from the communications literature to examine the effectiveness of financial communications with stakeholders. They identify three characteristics of connectivity: textual (the ability to connect different sections of the text); inter-textual (the ability to connect text from different time periods) and relational (the ability to connect firms with their audiences). They examine how social media and digital communication can improve connectivity through the dissemination of a coherent and consistent message.

Gibson (2017) uses the strategic management literature to identify four different approaches to research of relevance to the accounting literature:

Single method/single source – much the most dominant approach, with a single method of analysis being directed to one data set

Single method/multiple sources – data from multiple sources (e.g. archival and interviews) being fed into a single means of analysis

Mixed methods/single source – one source (e.g. the same firm) supplies both quantitative and qualitative data for analysis

Mixed methods/multiple sources – many groups are interviewed, say, to provide comparative data analysed with a variety of quantitative/qualitative tools.

She highlights the very small proportion of papers adopting a mixed methods approach, but notes that the prior dominance of quantitative findings is beginning to decline as qualitative research becomes more acceptable to the leading US accounting journals.

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 (1970: 64) 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.

Bloomfield et al. (2016) note how their five-step classification of the goals of positive accounting research fit neatly into Kuhn's *paradigm* construct:

1 Identify key constructs and specify theory as a relationship between these constructs.
2 Identify variables and analyse associations in order to model causal relationships.
3 Link observations to the causes of specified theory and seek confirmation on the accuracy of predictions.
4 Generalise associations by questioning how relevant and persuasive the evidence is, and
5 Seek to further build theory by placing the results in context and establishing how useful and important the predictions are.

In accounting research the parallels might be the paradigm shifts associated with the ideas introduced by Ball and Brown (1968) (as reported in Ball and Brown, 2014) 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.

Sir Humphrey Appleby, in Lynn and Jay (1987: 258), outlines the four stages of the process necessary to discredit an unwelcome report. The parallels between the fictitious Department of Administrative Affairs 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: 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: 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 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: 59) 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 impact 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). Kent and Ung (2003) find support for the Walker/Tsalta position finding no correlation, while Aljifri and Hussainey (2007) observe a negative correlation. Clearly more empirical work is required to clarify the nature and direction of causation, but comparable recent studies suggest a bi-directional relationship typical of the incidence of simultaneity and associated endogeneity problems when estimating relationships (see Chapter 6).

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 (TAR)* and *Journal of Accounting Research* (*JAR*) – in which they identify problems in the design and analysis of 27 of the studies!). Similarly, Dowd (2004: 510) notes the publication in the respected tier of economics literature of papers with nonsensical assumptions. More recently, Burks et al. (2019) highlight frequent errors of interpretation of interaction terms in studies involving linear regressions, while Larson et al. (2018) is critical of the questionable approaches to measurement employed in much of the literature devoted to accounting accruals.

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 and 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 in either further theory development or 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 data sets 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, a data mining exploration or a consultancy project, none of which would normally be gracing the pages of a respected 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 for 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 that are driving the research. If any of these is itself 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 the theory, the literature, the available data 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.

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 increasing 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. The most appropriate statistical tests should have been employed, but Butler et al. (2017) warn of the common use of 'p-hacking' – the selection of a test most likely to yield significant findings. 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, 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 chose 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 of this volume.

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 are new, and this 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 be of only 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 reason for 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 data set 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. Sullivan, 1984; Cushing and Loebbecke, 1986). 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). This appears to be evidence of the practice of HARKing (see Chapter 4 and Butler et al., 2017) in that the hypotheses appear to have been posited after data analysis rather than prior to data collection.

6 **Motivating literatures.** Relatively few articles, noted above (i.e. Sullivan, Kinney, Cushing and Loebbecke, 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:

- 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 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 data set 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 Australia (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 the 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 data sets – 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 were looking at potential suitors for Arthur Andersen, say, then the analysis suggests that Ernst and Young would provide 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.

Evans et al. (2015) provide a useful guide to help new researchers in adopting a critical approach to improving the quality of their work, so that they can target top journals. They detail important points to consider during each of five stages of a project – from identifying the initial topic to drawing conclusions from the findings of the completed study, as follows:

1 **Research question.** Specify the precise research question.
2 **Theory.** Use theory to develop a conceptual framework capable of distinguishing alternative explanations.
3 **Contribution.** Specify how this study is interesting and extends the existing accounting literature.
4 **Research design and analysis.** Match the research design with the purpose of the study by demonstrating which of archival, experimental, field, survey or interview studies is the most appropriate in this context.
5 **Interpretation of results and conclusions.** Describe, test and justify your findings relative to those of previous studies. Relate conclusions back to the original research question to discuss contribution and findings in a realistic manner.

At the beginning of the process, Maines et al. (2006) provide help by discussing how we might develop better research questions, while at the end Ittner (2014) and van der Stede (2014) evaluate their causal inferences to draw appropriate conclusions, recognising that it may not be possible to eliminate the possibility of competing theories or explanations, in which case they recommend that such doubts should be aired at this stage.

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 cooperative 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 that will eventually result in the publication of our research findings.

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TWO

Developing the Research Idea

Chapter Contents

We recognise in Chapter 1 that research processes are usually not simple, systematic or clean, because research rarely proceeds exactly to plan. However, this should not deter us from planning thoroughly in the first place to specify how, in an ideal world, we would like the research to be conducted.

The research sequence

Figure 2.1 specifies a typical research sequence described as a series of stages we would expect to progress through in most forms of accounting research, when moving from original idea to eventual publication.

The following chapters of this book address each of these stages and detail the constraints we might anticipate.

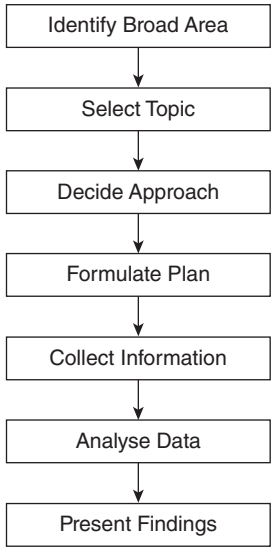

Figure 2.1 The research sequence

1 **Identify broad area.** Narrow the focus from 'accounting' in general to a stream associated with one topic area (e.g. management accounting).
2 **Select topic.** Specification of a sub-area to provide a tighter focus, and one for which supervision capacity is available, though further modification may subsequently be necessary.
3 **Decide approach.** Early thoughts regarding the approach to be adopted will revolve around the resources available, and in particular, access to the necessary data sources. A more precise specification of research methods to be adopted must wait until the literature review has been conducted and theoretical foundations and outline hypotheses have been established.
4 **Formulate plan.** Milestones and targets should be established at the outset so that it is clear how the research will progress over an extended period. This is particularly important for part-time researchers who may be contemplating study over six or seven years. This plan should include target conferences where preliminary findings may be presented, especially where deadlines are important to the candidate. The commencement period can often cause anxiety because of the perceived need to make swift progress. It cannot be emphasised enough how important an extended period of reading is to ensure that effort is not wasted performing experiments or surveys which subsequently emerge as being unnecessary or fatally flawed.

5 **Collect information.** Other than in data-mining exercises, data collection can only safely proceed when we recognise exactly what we want to know, and for what purpose. The planning stage should highlight the period over which we want to collect data; in most cases this will effectively preclude non-archival longitudinal studies, partly because it takes too long to collect data and partly because of the increased vulnerability associated with extended site access. We may require access to commercial databases; if these are an essential requirement then permissions should be sought immediately.
6 **Analyse data.** Methods of data analysis and software requirements should be apparent early in the research process.
7 **Present findings.** Preliminary findings will normally be presented at university workshops and seminars, and then at specialist conferences. These provide the precursor to publication in the refereed literature, which may take place before completion of any associated doctoral dissertation.

The research sequence above can easily be massaged to provide the elements of a traditional positivist approach, as illustrated in Figure 2.2. Again, this approach assumes the presence of specific conditions:

- The specification of a priori hypotheses – formulated on the basis of theory and literature before any data are collected or fieldwork contemplated
- The specification of a priori criteria – to measure the acceptability of the hypotheses, most commonly in the form of standard statistical tests
- The isolation and control of the variables to be investigated – determination of which variable(s) will be treated as dependent, which will be independent (explanatory) and which will be held constant, matched or ignored, and
- The verification of the methods for measuring the variables – specification of which variables can be measured directly, and those which will require the use of proxy variables, or measurement instruments of some form.

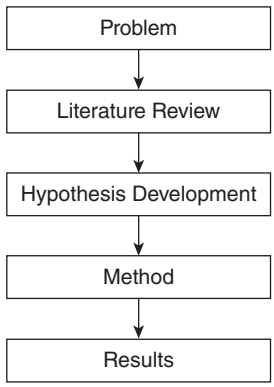

Figure 2.2 The positivist approach

We have to acknowledge, however, that there is no single method that necessarily applies to research in all situations. Thus, while the positivist tradition remains the most prominent in the accounting literature, non-positivist approaches have become increasingly acceptable. (Even so, some of the top US journals are moving slowly in their attitude to non-positivist approaches; Baker and Bettner (1997) and Parker (2012) among others, observe that most of the top journals are devoid of interpretive and critical research studies.) However, management-oriented investigations of change (e.g. the implementation of accounting innovations) may be particularly unsuited to a scientific approach. Where people are involved and multiple variables are beyond the control of the researchers, including management's own motivation and agenda, scientific approaches are of questionable validity. Checkland (1981: 316) observes that 'attempts to apply scientific methodology to real world, essentially social problems, have been responsible for the limited success of management science'.

Thus, we can stay within our original 'research sequence' framework but extend out beyond the positivist approach. Figure 2.3 illustrates the range of possibilities. As we move from top to bottom in the figure, we move from the traditional positivist approaches (archival and experimental studies) through field studies towards a case-based approach typically associated with ethnographic studies. This movement corresponds with an increase in the number of uncontrolled variables, with our increasing inability to formulate testable hypotheses, and with the increasing prominence of the 'human' element.

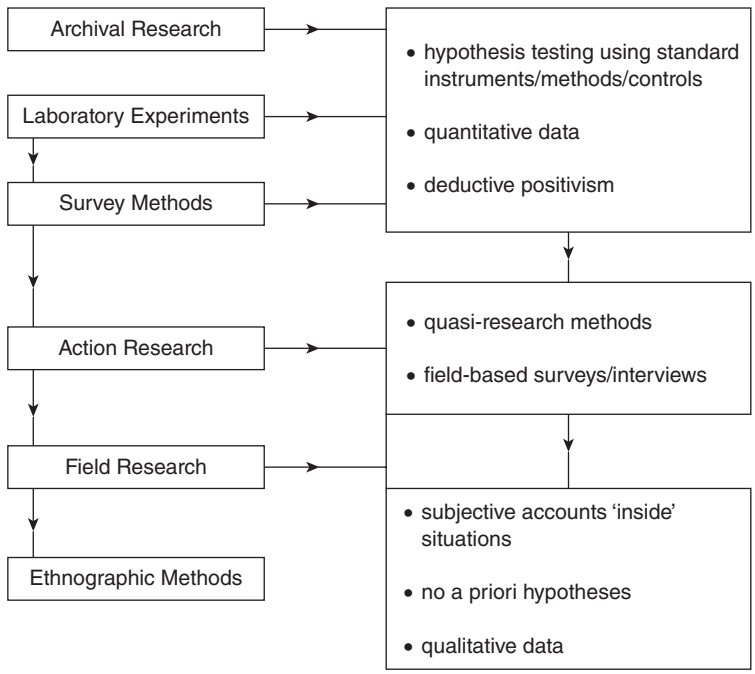

Figure 2.3 Alternative research methods

Emergence of the research topic

We should begin by choosing a research topic which is of interest both to the researcher and the supervisor, where the project is contributing to a doctoral qualification. The topic should generate enthusiasm in the researcher at the outset, otherwise he or she is unlikely to last the course of a protracted period of study in which motivation is bound to wane, even temporarily. The source of the topic can be from anywhere, but is most commonly:

- A problem at work with potentially wider implications
- A problem or application spotted in the newspaper, from television or during a web-search
- A conference presentation revealing the directions being explored by other researchers
- Working papers and completed theses elsewhere – the contents of which are usually at least two years away from publication
- Textbooks – particularly in management-related areas – which are a consistent source of untested theories
- Review articles, analysis of the literature in a particular area, to reveal the current boundaries of knowledge and a potential research agenda. The *Journal of Accounting Literature* is a particularly useful source in this regard (e.g. Jones and Shoemaker, 1994; Khlif and Chalmers, 2015; Gepp et al., 2018; Lee and Xiao, 2018), as are *Journal of Accounting and Economics* (e.g. DeFond and Zhang, 2014) and *Accounting Organizations and Society* (e.g. Langfield-Smith, 1997; Hartmann and Moers, 1999; Ahrens and Chapman, 2006; Gerdin and Greve, 2008; Martin, 2020)
- Review monographs (e.g. Ashton and Ashton, 1995, on information processing; Brownell, 1995, on management accounting; Trotman, 1996, on auditing) are still helpful, as is Foster's (1986) excellent book, which unfortunately has never been updated beyond this second edition, and
- Refereed journal articles, particularly the final sections, revealing flaws in existing research, gaps in our knowledge and research opportunities. This may, directly or indirectly, establish a future research agenda in the topic area, which the current authors may not be pursuing. For those students seeking inspiration my first destination would be the pages of *Accounting Horizons* and *Journal of Emerging Technologies in Accounting*!

The ideas will rarely emerge, therefore, from a 'spark' of original thought. More likely, the thought development will have emerged elsewhere, with the originator having either discarded his or her ideas or not seen their full value. It may fit an 'added-value' concept in the same way as a successful innovation may be remote from the inventor. This may be in the form of relating two concepts from different disciplines, in a manner that provides an application opportunity in the accounting environment. Here the 'success' is in the publishing of research findings in a respectable journal. To do so we will inevitably be building on the work of others.

The common element in each of the above approaches is 'reading' – hence the common advice given to doctoral candidates of 'reading, reading and yet more reading' to know an area and spot the opportunities. Candidates usually have a much greater commitment to a topic if they have developed it themselves, yet many find idea-generation an extremely difficult process. Thus, it is not uncommon for the supervisor to be the source of the research idea, because active and experienced researchers usually have far more ideas than they are capable of exploring by themselves. As Gill and Johnson (2010) observe, topic selection can be risky if left entirely in the hands of the candidate; the chosen topic may prove to be too small, too large, or simply not feasible in the time frame (especially for longitudinal studies). There may need to be a trade-off between the ownership/commitment associated with a student-selected topic and the practicability/timeliness expected of a supervisor-directed preference. Commonly, candidates will contemplate studies that involve the implementation of an accounting change, in order to monitor the change process and the resulting impact on financial outcomes. Such a model is rarely feasible because, apart from the access problems, involvement and data collection will be necessary over a period usually extending beyond that permitted within a standard candidature. Sandberg and Alvesson (2011) detail techniques that might be adopted in order to identify broad research topics from which specific research questions might be refined. They highlight 'gap-spotting' as a technique which helps to identify areas of research that have been overlooked or dealt with cursorily in the past ('neglect spotting') and perspectives or traditions that have been underused ('application spotting'). They also recommend 'problematisation', a term they use to recommend the challenging of assumptions and the establishment of linkages between alternative perspectives and theories.

Once the germ of an idea is forthcoming it must be worked over to see if it really constitutes 'research'. For example, are we sure that it is more than a consultancy project? Is it more than a trivial problem with no wider implications? Is it more than a replication of something someone else has done before, or done in a different industry or different country? If we are happy in this regard, then several other questions emerge:

- Is the project 'doable' in a reasonable time frame (e.g. the period of candidature)?
- Will the project fit the NIRD acronym (usually attributed to Rashad Abdel-Khalik during his tenure as editor of *The Accounting Review*) in that it is New, Interesting, Replicable and Defendable? Unsurprisingly, the acronym fits a positivist outlook since replicability may be impossible to guarantee in field study settings.
- Will the data required be readily available? If site visits are required, will access be available over a sufficient time period and to a sufficient depth? This last scenario is of great practical concern and difficult to control. Young and Selto (1993) and Lapsley (2004: 184) each report on studies whose depth was seriously curtailed because management changed its mind and restricted the access to personnel due to be interviewed. Worse than that, there are numerous cases of change of company ownership during the data collection period, resulting in further site access being permanently denied. Thorough examination of a small sample of firms over an extended period also constitutes a high-risk project, since the host firms can withdraw their collaboration at any time.

Once the general topic area has been determined, it may be refined by formal methods (e.g. brainstorming, attribute listing) to identify possible fruitful directions and potentially interesting relationships, and to eliminate blind alleys. Diagrammatic aids, particularly whiteboards, are very helpful at this stage for mapping ideas, variables, relationships and processes.

The research proposal

Attention to the fundamental requirements of the refereed literature will allow the researcher to produce an outline research proposal, one that is continually revised during the reading period and may nevertheless have to be revised further during the conduct of the research itself, due to unforeseen circumstances. A typical research proposal will include the following elements:

- **Title** – what you are trying to do
- **Abstract** – the problem, objectives and expected outcomes
- **Issues** – why they are interesting and important
- **Objectives** – how the study relates to the problem
- **Literature** – review of relevant, themed publications
- **Method** – the how and why of the process
- **Benefits** – the anticipated outcomes that make this study worthwhile.

Kinney (2019) describes a practical approach to the development of the research proposal where key questions are asked and answered right at the outset. A working **title** is important to clarify the topic, especially if external grant income is being sought to support the project. However, the final title is rarely the original one and there are plenty of opportunities to make changes.

The **abstract** is an important departure at this stage, because it allows the researcher to speculate on what the outcomes of the research might be, should everything go according to plan. The abstract can be ambitious initially, but will require revision (perhaps radical revision) as problems and constraints emerge in the research process.

The contribution of the paper and the way that it aims to address important **issues** in a systematic manner are fundamental to its success. Internal consistency and overall coherence should ensure that the **objectives** and the intended approach are appropriate.

The outline **literature** review may be incomplete but it must nonetheless identify the key motivating literatures and theories. Research candidates must recognise that the literature review is a constantly moveable feast and something that will be added to right up to the final presentation of the research findings. One of the major deficiencies of both papers and dissertations is that they frequently overlook the most recent relevant publications: it can be a heart-stopping moment when one is about to submit the thesis, and the latest website update of 'online first' pre-issue journal papers appears to report the outcomes of a research project very similar to one's own! At the very least this new paper

must be cited. A common complaint from inexperienced researchers is that there is 'no literature' available. If this is true, it may mean that the projected topic may be too trivial to consider. More likely, however, is that the literature review should drill down further and search on different keywords. That there is a dearth of recent literature on a topic may foreshadow problems. For example, papers on 'decision-making heuristics' were common in the late 1970s and early 1980s, and papers on 'group decision-making' common in the mid-1980s, but progress in both of these research areas has slowed, and publications are relatively rare because the psychological theories underlying the research have not developed sufficiently to facilitate new approaches. In a similar manner, we have witnessed the demise of normative accounting research since the early 1970s, it being increasingly supplanted by research based on positive accounting theory. With respect to apparently new projects, for example, research into 'social media'-type topics, students must recognise that social media is just a new form of disclosure, and that their review must address the implementation of prior forms of disclosure innovation. Rogers' (2003) diffusion of innovations theory would normally provide an excellent starting point for theorising such projects.

Discussion of **method** and **theory** should address the alternatives available in order to demonstrate that the preferred choice is the most appropriate. The proposal should also echo the **benefits** of the research, in particular its contribution to knowledge and the potential implications for business practice.

The research proposal would normally form a central feature of any application for ethics approval, and must therefore demonstrate the value of the research, the integrity of the methods employed and the extent of the involvement of human participants. Students are often unsure as to the length and detail required in a research proposal, and there may be restrictions imposed by the academic institution as to length. But focus is the essential element – it should be clear what the objective of the research is, and what theories and literatures are driving it. Where longer proposals are permitted, students may also see the proposal as providing a much-abridged first draft of Chapters 1, 2 and 3 of a subsequent thesis! Evans et al. (2015) provide a self-assessment guideline for authors and students to improve the quality of their research and to anticipate the questions of examiners and reviewers.

Conceptual frameworks

A valuable part of the initial planning process is the development of a conceptual representation of the research project. This can help to clarify the important relationships (and the need for supporting theory), the explanatory and intervening (mediating or moderating) variables, as well as the demonstration of causation.

The inductive and deductive approaches identified in Chapter 1 provide an objective alternative to the conduct of research, but neither allows the opportunity for human interaction: the inductive approach is where new theory is developed on the basis of fresh

observations (as is most commonly the case in hard sciences, like astronomy); and the deductive approach is where theory provides the basis for the testing of empirical observations (and which is the most common form of positivist accounting research). The deductive approach is suitable in a highly structured environment, involving the empirical testing of theoretical models, so that its reliability is dependent on the integrity of quantitative and statistical methods. However, the causal relationships explored rely on an internal logic and take no account of the human relationships present. The application of the inductive approach in the accounting environment necessitates a variation to the traditional model, such as that provided by Kolb's experiential learning cycle (Kolb et al., 1979: 38), which is illustrated in Figure 2.4. Recognition of the importance of internal processes and human relationships to the inductive approach allows for the existence of human subjectivity without distorting research findings, even though they may be qualitative and not replicable. Where human relationships are central to an understanding of accounting behaviour the approach exploits the subjective environment.

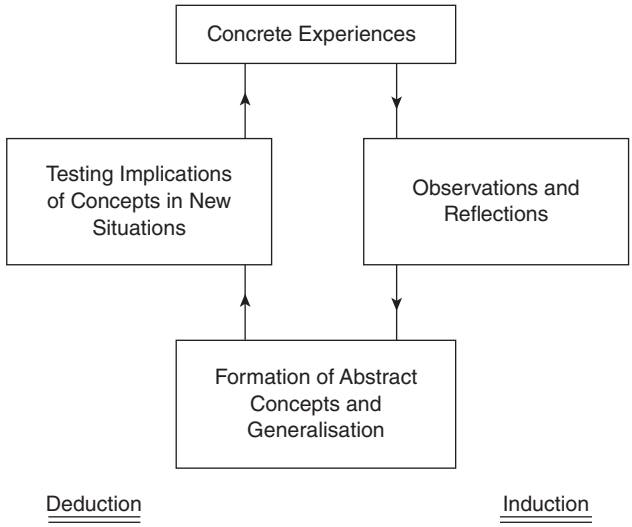

Figure 2.4 Kolb's learning cycle

Although both models provide opportunities in accounting research, the deductive approach offers greater possibilities for the implementation of scientific methods, since it facilitates arguably more reliable measurement and control. Grounded theory (discussed in more detail in Chapters 3 and 10) illustrates the potential for inductive methods in accounting research.

We can therefore develop the model of the deductive process (see Figure 2.5) so that it corresponds with Popper's (1959) defining characteristics of scientific theory:

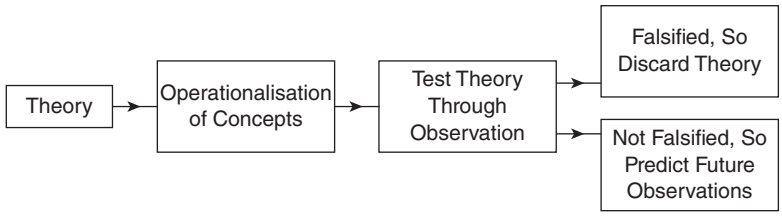

Figure 2.5 The deductive process

- The theory is capable of empirical testing.
- Scientists (and researchers) should make rigorous attempts at falsifying theory.
- Science advances as falsified propositions are left behind, leaving a core of theory still to be disproved.

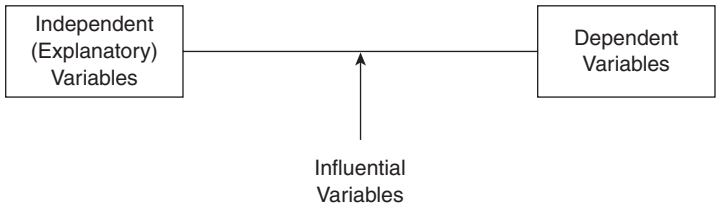

Figure 2.6 The conceptual schema

The basic conceptual schema of Figure 2.6 provides a powerful tool for the examination of causal relationships in a positivist environment. By establishing the key variables of interest, and the other potentially influential factors, we can form a better impression of the breadth of the problem.

The design problems associated with the identification and measurement of variables may mean that the basic schema needs to be modified according to Figure 2.7.

What is also apparent is that the simple conceptual schema of Figure 2.6 must be regarded very much as a preliminary picture which will quickly outlive its usefulness. While helpful to begin with, it must now be modified, and further detail added, to allow the further development of hypotheses and data collection methods. Chapter 3 focuses on these issues in the development of the important links between theory, literature, hypotheses and methods.

In Chapter 1 we acknowledged our debt to researchers in the natural and social sciences. It is now helpful to turn to the popular and practitioner literatures, which can provide highly readable explanations of complex situations, for an insight into the emergence of research ideas, the development of research questions and the airing of potential

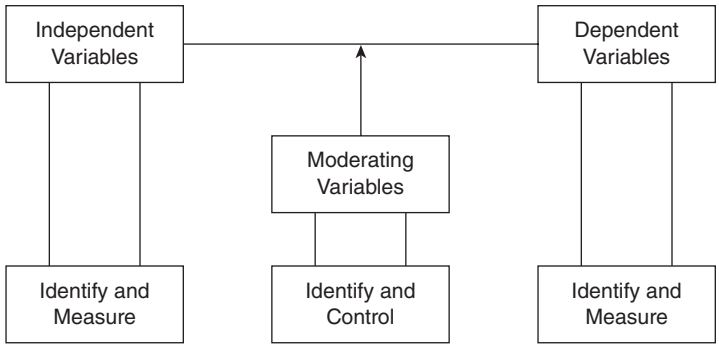

Figure 2.7 Measurement issues

solutions for testing, to illustrate the research sequence in practice. It is instructive to consider the different scope of original research in solving practical problems by examining the particular aspects of well-documented stories, some from non-accounting environments. Thus, we explore the development of new theory in a chemical environment in 'The structure of DNA'; the development and testing of alternative strategies to address a sporting issue in 'The Bradman problem'; the solution of apparently insuperable implementation issues to a problem where the 'answer' was well known in 'The longitude problem'; problems associated with qualitative archival research, as illustrated by reference to 'family tree' searches, and an example in Benford's law of an apparently trivial casual observation being turned into a powerful forensic tool.

The structure of DNA: the development of new theory

James D. Watson's (1968) *The Double Helix* (subsequently filmed as *Life Story*) provides a brilliant description of the exciting process of discovery in scientific research, even if the approach adopted is rather unorthodox. The development of theory and conceptual modelling, from systematic deductions based on the empirical findings of others, is conducted in a competitive environment where the ultimate prize for winning the 'race' is the Nobel Prize. (Watson, together with Francis Crick, both of Cambridge University, and Maurice Wilkins, of King's College, London, were awarded the 1962 Nobel Prize for Physiology or Medicine for their pioneering work during 1951–2.)

Sir Lawrence Bragg reflects on these achievements by researchers in his Cavendish Laboratory through a revealing preface to Watson's book, with implications for research ethics:

> He knows that a colleague has been working for years on a problem and has accumulated a mass of hard-won evidence, which has not yet been published because it is anticipated that success is just around the corner. He has seen this evidence and has good reason to believe that a method of attack which he can envisage, perhaps

> merely a new point of view, will lead straight to the solution. An offer of collaboration at this stage might well be regarded as trespass. Should he go ahead on his own? (Bragg, in Watson, 1968: vii)

That Watson and Crick, at the Cavendish, did proceed with a belated relationship – though collaboration is too strong a word – with Wilkins and Rosalind Franklin is a matter of history. The course of their investigations, and the factors leading to their success in developing a new theoretical model, have implications for all research. Essentially, they proceed to develop a model that fits all of the evidence currently available to them, and they await confirmation or disconfirmation of their framework from the empirical findings of others:

- The work of Linus Pauling in the USA on a helix formation for polypeptide chains suggested that DNA (deoxyribosenucleic acid) too had a helical structure. Pauling's early attempts at modelling though, without crystallographic evidence, had produced stereochemically impossible components. Early evidence from X-ray crystallographic diffraction presented by Wilkins also seemed to suggest a helical structure, but there was no evidence of whether a single, double or triple strand helical configuration was most appropriate. Crick and Watson apparently proceeded on the basis of an educated guess favouring the double helix because most things biological come in twos!
- Ernst Chargaff had produced vital evidence on the ratios of constituent bases, and particularly the equalities existing between adenine (A) = thymine (T), and guanine (G) = cytosine (C). The A–T, G–C flat hydrogen-bonded base pairs formed the core of the Crick and Watson structure, rather like a spiral staircase in which the bases form the steps.
- Important advice from a structural chemist colleague (who just happened to be sharing the same office with Crick and Watson) suggested that the normal textbook formulation of the A–T, G–C bases was incorrect and that they should work with an alternative 'keto' form. Without this important questioning of textbook content, and accepted knowledge, their structure would not have held together. The impact of this finding was that a given chain could contain both purines and pyrimidines (with the capacity to carry the genetic material for self-replication) and that the backbones of the chains should run in opposite directions.

Thus, Crick and Watson were able to construct a physical model comprising two intertwining helically coiled chains of nucleotides, right-handed and running in opposite directions, with complementary sequences of hydrogen-bonded bases. The resulting structure was stereochemically possible, and subsequent X-ray evidence from Franklin confirmed that the sugar-phosphate backbone was indeed on the outside of the molecule.

The more general implications for researchers are constant vigilance and a questioning attitude to the work of others and existing publications.

Reference

Watson, J.D. (1968) *The Double Helix*. New York: Penguin Books.

The Bradman problem: the development of new strategies

Test match cricket in the 1930s was dominated by a single, outstanding individual whose unique gifts of batsmanship threatened to change the way the game was played. Donald Bradman scored so many runs, and scored them so quickly, that he was likely to win a game almost single-handedly. Ashes test series, once so closely contested, threatened to be one-sided affairs, with Australia the perennial victor. In the 1930 England v. Australia test matches Bradman totalled a record 974 runs at an average of 139.14 per innings, and recorded separate scores of 334, 254 and 232. At the commencement of the 1932/33 series he averaged 112.29 in all test matches, and had already posted six scores in excess of 200 in only 24 completed innings.

Bradman was a phenomenon, some would say a 'freak', and curtailing his dominance became a pressing question for successive England captains. 'The Bradman problem' is capably detailed by Lawrence Le Quesne in his book *The Bodyline Controversy* (1983), and presents an intriguing research question, investigating a number of alternatives that might provide successful solutions.

1. Changing the playing conditions

Cricket was played on hard, fast wickets, largely true, though with the occasional unpredictable bounce. If bowlers did not take wickets in the first few overs, when the ball was still shiny and swinging, then they might have a lot of overs to bowl before they got a replacement ball, by which time 200 runs had been scored. Wickets were uncovered and exposed to the elements once a game had started and could become unplayable as a hot sun dried out a wet pitch; these 'sticky' wickets provided a possible solution to the problem because Bradman was nowhere near as prolific on bad wickets as many of his contemporaries were. However, captains could hardly rely on this occurrence to blunt Bradman's genius on a regular basis.

2. Changing the rules of the game

Test matches were timeless in the 1930s and played to a finish. Declarations were rare and slow play very common. Further, the leg-before-wicket (LBW) rule made it difficult for the batsman to get out in that way – he had to be caught in front of the stumps by a ball pitching in line, wicket-to-wicket. Life was difficult for bowlers, and a number of other batsmen (notably Ponsford for Australia and Hammond for England) regularly completed double and triple centuries, although none with the regularity, reliability or speed of Bradman.

Reduction of the specified playing time to five (or four) days would mitigate slow play and dull the impact of less talented batsmen content to occupy the crease, but would not affect Bradman – witness his 309 in a day against England at Leeds in 1930. However, timeless tests were outlawed before the end of the inter-war period. Similarly, the LBW

law was changed so that batsmen could be given out to a ball breaking back from outside the off stump and striking the pads in front of the wicket.

3. Changing the bowling

Bowlers had often sought to restrict the batsman by bowling outside of the leg stump (e.g. Hirst and Foster in the early 1900s), ostensibly to cut out off-side shots but also to restrict the on-drive through bowling just short of a length. An accurate bowler could therefore depress the scoring rate by keeping the batsman to deflected singles in the arc between the wicketkeeper and square leg. As a consequence the whole game is slowed to a snail's pace – a strategy still used today to curb an aggressive batsman's dominance. But Bradman was too good to fall for such tricks and used his agility, quick reflexes and nimble footwork to move to leg, outside the line of the ball, so giving himself room to hit through the off-side field. Such a strategy would have made less gifted batsmen vulnerable because it entailed the exposure of all three stumps to the bowler, but Bradman could execute the manoeuvre without undue risk.

4. Changing the field placings

An orthodox field will involve the placing of fieldsmen on both sides of the wicket, with a majority coincident with the bowler's planned line of attack. This means plenty of gaps in the field and, for inaccurate bowlers, plenty of scoring opportunities. Clearly, a preferred strategy would be to place all the fielders in a tightly confined space and have the bowler deliver a line and length that forces the batsman to play the ball there. This is a sound strategy for an accurate bowler – witness Laker's devastating use of a packed leg-trap to a turning ball in 1956 – but for a batsman of Bradman's ingenuity we now have a vacant off-side to be penetrated by inventive, unorthodox shots.

5. Introducing a 'bodyline' attack

While a combination of numbers (3) and (4) above provides a partial solution in restricting scoring opportunities, only impatience will induce eventual dismissal. For Bradman it may do neither. However, together they provide the basis for a potentially successful solution: 'leg theory' will dictate the line of delivery, but we need also to control the height of the delivery to induce the batsman to give chances from playing the ball in the air. Herein lies the Jardine–Larwood proposition, initiated by Douglas Jardine, England's captain in 1932, and executed brilliantly by Harold Larwood, the fastest and most accurate bowler of his time.

Extreme fast bowling does not provide even Bradman with the time to move outside the line consistently without risk. The introduction of a high proportion of short-pitched balls rearing towards the throat or the rib-cage of the batsman makes scoring without risk extremely difficult. Batsmen are likely to fend off the ball defensively – to be caught in the

leg-trap ring of close catchers. If they try to attack by hooking the ball over the in-field, they fall prey to a number of deep-set fielders on the leg-side and behind the wicket. Concentrating almost all the fielders in the arc between wicketkeeper and mid-wicket, five close to the bat and three close to the boundary, covers almost all the options. Scoring is restricted to risky options and bad balls.

This form of attack was 'successful' in that it resulted in a 4–1 series victory for England and also provided an appropriate solution to 'the Bradman problem' in that he scored only one century in the series, a total of only 396 runs at an average of just 56.57. He still tried to play in a cavalier fashion, moving outside the line to play tennis-like shots; the result was brilliant, hectic cameos that were over all too soon for his team's requirements.

However, the risks of bodily injury to the batsman from the bodyline solution were high, and its introduction was seen to be ungentlemanly and against the spirit of the game. Jardine and Larwood were never chosen to play against Australia again. The subsequent 1934 test series was very much a fence-building exercise, with the England bowling friendly and 'bouncer' free. Bradman was again unconstrained and scored 758 runs at an average of 94.75. The success of the leg theory solution generated further changes to the rules of the game, with a restriction on the number of fielders permitted on the leg-side behind the wicket and the number of short-pitched balls that were allowed to be bowled per over.

The more general implications for research are that there may be legal, moral, ethical or professional circumstances which prevent either the conduct of the research or the implementation of recommendations from the research findings.

Reference

Le Quesne, L. (1983) *The Bodyline Controversy*. London, Unwin.

The longitude problem: implementing solutions

The measurement of longitude at sea requires the accurate measurement of both the time at the current location and that at the Greenwich meridian, or some other similar base point. The time difference allows the calculation of geographical separation – since the 24-hour revolution of the Earth constitutes a 360-degree spin – so that a one-hour time difference constitutes 15 degrees of longitude, where one degree of longitude is equivalent to 68 miles on the Equator. The measurement of local time is not a problem, especially during hours of daylight, but in the absence of accurate timepieces knowledge of the corresponding time at the base point remains a considerable problem.

The consequences of being unable to measure longitude were serious and have been detailed by Dava Sobel in her book *Longitude* (1995). Shipwrecks and lost vessels were common, and piracy was facilitated by the need for ships to track across common lines of latitude on the 'trade routes' to maintain their position. The pendulum clocks of the 1660s, due to the work of Christiaan Huygens, had been used to demonstrate the possibility of measuring

longitude at sea with timepieces, but they were only helpful in favourable weather. So much so that Sir Isaac Newton (cited in Sobel, 1995: 52) was forced to admit in 1714:

> One [method] is by a watch to keep the time exactly. But by reason of the motion of the ship, the variation of heat and cold, wet and dry, and the difference of gravity in different latitudes, such a watch hath not been made.

Newton clearly had astronomical, or at least scientific, solutions in mind rather than mechanical ones, necessitating the consideration of alternative solutions.

1. Existing methods

These were largely confined to 'dead reckoning' and 'compass method' approaches. Dead reckoning required estimates to be made of the speed of the ship, in conjunction with the calculation of the effects of wind speed and currents. Its success relied on good seamanship, reliable maps and luck! Compass methods were concerned with comparisons between magnetic north and 'true' north as shown by the pole star. Relative positions allowed the estimation of longitude without the necessity of measuring time. However, compass needles were notoriously unreliable, with a great deal of variation for the same compass on successive voyages. This, coupled with variations in terrestrial magnetism, made readings highly dependent on the particular seas being traversed.

2. Eclipse data

Eclipse data were thought to be potentially useful. Sonar and lunar eclipses provided opportunities if it was known when they were expected to be observed in other locations, but such occurrences were far too rare to provide a realistic navigational aid. Galileo had established that eclipses of the moons of Jupiter were extremely common and predictable, making them an accurate means of specifying longitude at specific land-based locations. However, movement aboard ship made this an impossible strategy for navigation, even when the night skies were clear.

3. Lunar distances methods

These involved measuring the distance between the moon and the sun, by day, and between the moon and stars at night. Such methods required detailed data on the track of the moon and the positions of individual stars so that the disappearance of particular stars behind the moon could be predicted. The complexities of the moon's orbit impeded progress in the prediction of the required measurements at different locations, and it was not until 1725 that Flamsteed's posthumous almanac of star positions was published. Even so, the available tables still meant it took four hours to calculate longitude (subsequently reduced to 30 minutes by Maskelyne's 1766 almanac).

The lunar distance method was therefore shown to be a theoretically possible means of accurately computing longitude, made more practicable by the invention of the quadrant (later sextant) in 1731 to measure elevations of, and distance between, moon and sun by day, and moon and stars by night. This permitted an estimate of time differences between a ship and known, fixed land locations. Even so, actual measurement proved impossible at times for a variety of reasons:

- Weather conditions can occasion fog or thick cloud cover.
- The moon is so close to the sun for about six days per month that it disappears from view.
- The moon is on the opposite side of the Earth from the sun for about 13 days per month.

John Harrison adopted a more direct solution to the problem, questioning the position of Newton and proceeding to build a succession of clocks that were shown to be accurate to fractions of a second per day. By eliminating problems of friction, he developed clocks that required no lubrication or cleaning. This, combined with the use of bi-metal strips of non-corrosive materials, overcame the problems of temperature change and rust. The choice of innovative balancing mechanisms also meant that the clocks were virtually unaffected by the most severe weather conditions.

By the time Harrison died in 1776 copies of his watch were still rare and expensive (in excess of £200) whereas a good sextant and set of lunar tables could be purchased for as little as £20. This considerable price difference meant that the 'lunar distance method' of calculation remained prominent until more affordable watches became available in the early 1800s through the sale of the mass-produced Arnold/Earnshaw pocket 'chronometers'.

Both timeliness and resource cost remain fundamental elements in the conduct of research projects and the implementation of their findings.

Reference

Sobel, D. (1995) *Longitude* London, Fourth Estate.

The scope of three examples above is very different, concerned respectively with the development of new theory, the development of workable solutions and the implementation of workable solutions. As we suspected, the research process is not simple, systematic or clean in any of the cases. What is common throughout are the pivotal roles played by 'theory' and 'validity': good theory produces good findings, and we are able to evaluate both the reliability and the validity of these findings through external reference. Chapter 3 examines theory in more detail, and expands the consideration of reliability and validity as desirable characteristics of accounting research.

Searching the family tree: qualitative archival research

One area of archival research available to all of us is the genealogical research necessary to establish reliable family trees. The elements of the search will provide examples of the most problematic issues that can arise in archival research – particularly in qualitative accounting research: missing data, errors, deliberate obfuscation, inconsistencies, ambiguities and alternative explanations. Triangulation of data sources may be necessary where problems persist in validating suspected relationships – in this case familial relationships.

Initial primary sources will usually be readily available in the form of documentary evidence: birth, marriage and death certificates. Where originals are not available then copies can be sourced (at a cost) from government sources – in the UK from the General Register Office of HM Passport Office at www.gro.gov.uk. Despite their pivotal nature the contents of these basic certificates might be misleading the further back we go – registration and certification only became mandatory in the UK in 1837. Thus, age at death reported by a 'next of kin' on a death certificate may be unreliable, as could the ages reported on wedding certificates, especially where minors were seeking to marry without parental consent. But the calendar dates on certificates will be reliable and vital to future research, providing additional data with respect to addresses, names of parents and occupations. Before 1837 we must rely on parish records of baptisms, weddings and burials; accessible records of these may be incomplete since they rely on local transcription. While wedding dates are reliable, the dates of baptisms and burials will necessarily not correspond precisely with those of births and deaths, with differences of several weeks not uncommon. Indeed many families chose to have several children baptised at the same time, meaning in some cases gaps of several years between birth and baptism. The Church of the Latter-day Saints provides the most comprehensive coverage of UK parish records.

Secondary data sources are available from a number of organisations with efficient search engines capable of tracing relationships back to the 1500s. The cheapest of these is available free through the FamilySearch facility of the Church of the Latter-day Saints (www.lds.org). Alternative, more sophisticated, sites are available via subscription and include Ancestry, My Heritage and The Genealogist, and these will provide access to both newspaper archives and, most importantly, census records. UK census records are currently accessible between 1841 and 1921, with no further records available until 2051. While the census provides an essential secondary source of historical information, it also provides examples of the potentially misleading nature of data from archival sources:

- **Transcription errors** – as we might also anticipate from interview data, since prior to 1911, census data were collected by a third party standing on the doorstep of the residence transcribing verbal responses as many of the residents could neither read nor write. Names were frequently misspelled and nicknames often recorded as valid forenames.

- **Errors of omission** – individuals may be left off the census record – particularly children who may be staying elsewhere on census night, and who are omitted there too.
- **Errors of accuracy** – perhaps deliberate in order to obfuscate age at marriage, or accidental because the respondents may not be sure of the ages of their children, especially in very large families.

Census data must therefore be treated with caution, especially with reference to dates and ages since they are often in error by several years. Repeated checks for the same family over consecutive census declarations may provide explanations. When searching for relatively unusual names the basic primary and secondary sources will generally get you back through several generations easily and reliably. With more common names (e.g. Smith) problems will arise – but these are not insuperable, they just necessitate a wider search of different family strands using 'clues' provided by the persistence of family forenames through generations and the common incorporation of a grandmother's surname as a forename. A major problem arises where brothers give their children identical names (surprisingly common in the 1800s); then a careful tracking of all families through the census will need to be backed up with documentary evidence from primary sources (particularly birth certificates) to ensure that we are indeed in the right place. Gaps in the documentation mean that alternative explanations must be sought – if not remarriage or death that may mean migration from the UK to the USA, Australia, Canada or New Zealand (extremely common since the 1850s); fortunately, ships' passenger lists are available online too, and often provide physical descriptions of the passengers.

The numerous ways in which errors can arise in such research is typical of that in qualitative archival research, making it essential that we take a rigorous approach, following Nisbett and Ross (1980) by ensuring that we do not ignore evidence contrary to our hypotheses, and following Kahneman and Tversky (1972) in ensuring that we do not draw conclusions based on insufficient evidence.

References

Kahneman, D. and Tversky, A. (1972) 'Availability: a heuristic for judging frequency and probability', *Cognitive Psychology* 5 (2): 677–95.

Nisbett, R. and Ross, L. (1980) *Human Inference: Strategies and Shortcomings of Social Judgment*. Englewood Cliffs, NJ: Prentice Hall.

The Holy Grail: experiments in the field

Field studies are noted in Chapter 10 for their real-world emphasis with a focus on external validity, while experimental research (see Chapter 8) focuses on research designs which emphasise internal validity in order to establish reliable cause-and-effect relationships. A combination of the two, where we seek to maximise internal validity in an