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Qualitative Research in Accounting & Management

**The role and status of qualitative
methods in contemporary
accounting research**

Guest Editors: Sven Modell and
Christopher Humphrey



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Guest Editors

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GUEST EDITORIAL

Balancing acts in qualitative accounting research

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Abstract

Purpose – The purpose of this paper is to reflect on the motivation for this special issue and its contributions.

Design/methodology/approach – This paper is a conceptual editorial piece which discusses current methodological tendencies in qualitative accounting research.

Findings – The paper argues that qualitative research involves some balancing acts between, on the one hand, pragmatic and aesthetic aspects and, on the other, the necessary steps for establishing trust in a particular piece of research. The authors explicate how this is manifest in the contributions to this special issue.

Originality/value – This paper highlights the need for greater attention to the many pragmatic aspects associated with qualitative accounting research rarely accounted for in previous texts on the topic.

Keywords Research methods, Qualitative research, Accounting, Accounting research

Paper type Viewpoint

Though this be madness, yet there is method in't (an aside by Polonius, speaking in *Hamlet*, Act II Scene ii).

You cannot go on “explaining away” for ever: you will find that you have explained explanation itself away. You cannot go on “seeing through” things for ever. The whole point of seeing through something is to see something through it [...] If you see through everything, then everything is transparent. But a wholly transparent world is an invisible world. To see through all things is the same as not to see (Lewis, 1947, p. 91).

Introduction

It is possible to think of situations where further explanation is just not possible. For instance, having to respond to the 15 question from a young, inquisitive, child as to why something is like it is. “Why do you like this?” I like it because it is nice. “But why is it nice?” Because of the graceful movement. “But why do things move in that way and why is it graceful?” They do because they do and it is graceful because it just is! The inappropriateness of insistent questioning and the myopia that it can reflect is classically illustrated in the following scene from Woody Allen’s 1972 film *Play it again Sam*, where Allen (playing a character whose first name is Allen) is looking at a Jackson Pollock painting in an art gallery and addresses a woman standing next to him looking at the same picture:



Allen: That's quite a lovely Jackson Pollock isn't it?

Woman: Yes, it is.

Allen: What does it say to you?

Woman: It restates the negativeness of the universe. The hideous lonely emptiness of existence. Nothingness. The predicament of man forced to live in a barren, godless eternity like a tiny flame flickering in an immense void with nothing but waste, horror, and degradation, forming a useless, bleak straitjacket in a black, absurd cosmos.

Allen: What are you doing Saturday night?

Woman: Committing suicide.

Allen: What about Friday night?

Art and music are nice examples as to whether things and people are there to be appreciated for what they portray or represent and the emotions that they stimulate. It may be for the artisan or the technician to ask how a picture was painted in the way it was or why a chord progression was used (and they may certainly get added satisfaction from knowing how to play the complicated parts of a Mozart piano concerto). As an artist, however, can you imagine which conversation you would prefer to have – one with someone personally moved by your painting or a young representative from the “Painting by numbers” company that your agent has commissioned to reproduce your picture on a 15 × 10 grid of numbered squares? Yet, it is interesting to note that in the field of art history, attention in recent years has been devoted to the practical side of the craft, particularly with respect to how artists obtained their colours, as opposed to how they used them (Ball, 2002).

A related pattern of development is noticeable in the shifting nature of methodological debate in the field of qualitative accounting research. In contrast to detailed prescriptive schemes with a highly technical emphasis (McKinnon, 1988; Atkinson and Shaffir, 1998), recent advances have attempted to place such research in more of a “real life” context, stressing the inherently flexible and pragmatic aspects seldom accounted for in texts on this topic (Humphrey and Lee, 2004; Ahrens and Chapman, 2006). While not denying the need for rigour and meticulousness the latter works have added an important dimension to earlier discussions of qualitative research methods in accounting by enhancing our understanding of how research “gets done” through exposure to lived, rather than text-book, accounts of the research process. Parallel to these advances, reflections on research practice remind us that the dividing lines between various qualitative (and indeed quantitative) research traditions might have been over-drawn (Kakkuri-Knuuttila *et al.*, 2008; Modell, 2005, 2007), while also illuminating the broad scope of what is currently understood as “good” qualitative research in accounting (Ahrens *et al.*, 2007).

What is at stake in much of this debate is the need to balance between the creative, and perhaps even aesthetic, aspects of research and the need to establish (or to provide the means for establishing) a sufficient level of trust and confidence in research findings. An excessive emphasis on the former may undermine trust while a one-sided pre-occupation with the latter could possibly cloud consideration of novel, but important, research issues and questions. According to Power (1997), an audit society is one that has lost the ability to trust and needs to start learning “to trust in trust”.

This is a compelling conclusion when it can be seen that a loss of trust in the safeness of society leads to controls, checks, safeguards, monitoring, precautions and a whole host of other measures that end up further constraining and limiting what we (and our children) can do and how we learn (and teach others). The problem with a conclusion of needing to trust in trust, however, is that it naturally evokes questions as to what is meant by trust. What are we entrusting when we trust in trust? In the context of an audit society, Power suggests the need for “a kind of intuitive auditing and mental accounting” (p. 137). From a research perspective, central here is the need to retain (or regain) a sense of belief in emotions, to know when things feel right and when something is appealing. To know when something works and when it does not. There is a real danger that we get bogged down by a lack of confidence in expressing what we know to be the case rather than any inherent lack of knowledge. Just think how many times you hear people comment that they know things do not feel right but that they cannot say what exactly is wrong. Or how fantastic something is, even though they cannot put such sentiments properly into words. Or how they feel that they have done enough field work, even though they know there are more people who could be interviewed or surveyed.

Our desire to compile this special issue and the resulting papers published here reflect ongoing efforts in qualitative accounting researcher to master this balancing act. While qualitative accounting research has come of age over the past 30 years, there still remains much to discuss. Collectively, the papers in this issue provide some illuminating reflections on the practice and impact of qualitative accounting research and should certainly serve to stimulate further thinking on the part of those studying and researching accounting practice. In this editorial, we elaborate on our personal motivation for editing this special issue, comment briefly on each of the three contributions and provide some closing reflections and thoughts for the future.

The motivation for the special issue

Accounting itself has proved to be a useful disciplinary focus for consideration of the practice of qualitative research, not least because it has almost become, by definition, interdisciplinary in its theoretical framing and the methods deemed to be consistent with such framing. This spirit has infused discussions over qualitative methods with a broad range of perspectives and reflections on the value of particular methodological standpoints. Arguments are bolstered or challenged on relative understandings of different fields of social science and the derivation or historical emergence of certain research approaches and/or perspectives. However, the views of the future of qualitative research in accounting diverge. Some commentators envisage a relatively peaceful co-existence of different paradigms (Locke and Lowe, 2008). Others have raised concerns over the hegemonizing tendencies of the quantitatively orientated “mainstream” (Ahrens *et al.*, 2007; Hopwood, 2007), particularly in terms of areas where our knowledge of practice remains very limited (for a recent discussion in the field of auditing research, see Humphrey, 2008).

What is possible to note is the increasing emphasis that is being placed on method and the specification, classification and categorisation of method. It is often said that research proposals are rejected because of being poorly specified in terms of chosen research methods. There are lively debates over the distinction between method and methodology or even over the classification of research approaches and the meaning,

for instance, of terms like “interpretive” research in accounting (Ahrens *et al.*, 2007). For some, definition is crucial and illuminating in terms of revealing contradictions and inconsistencies in methodological positioning. For others, it is unnecessary and constraining. Good practice tips proliferate although sometimes these are based less on actual research experience but rather represent (and reinforce) various research practice myths. There is also a danger that they focus on the obvious and the easy to recommend as compared to the more intractable issues and worrying undercurrents that more often characterise the realities of day-to-day research life. Additionally, “tips/suggestions” can easily transform into “stipulations”. Reporting on methods used in papers is required to be detailed and, ideally, separately sectioned. If interviews were conducted, it is important to know when and with whom and for how long. Likewise, it is necessary to demonstrate how the interviews were coded. Cited quotations need to be attributed to individuals (who, if not named, need to be individually coded). The way in which themes were constructed needs to be explained in detail and linked to particular interviews.

At its extreme, there can appear to be an overriding desire for double-checking – a belief in auditability wherein the research that has been undertaken can almost be reconstructed and redone by the reader (who essentially is able to step inside the research project and see if he/she would have come up with the same points and conclusions). A softer categorisation would point to an increased emphasis on assurance and credibility, with the ruling assumption being that the reader will feel more comfortable and trusting in the research if the researcher states clearly, plainly and in detail what was done and why it was done in the chosen way. A case of method not so much dominating but, at least, needing to be more visible.

Typical of what might be referred to as the “methodisation” of accounting research is the doctoral student who informs you that they have done their literature review, summarised their theoretical perspective, done their interviews, even coded them but they just need to develop their research themes and main findings/ideas – and want help with this as they are not sure what is coming out of their work. If the focus on method works to the detriment of the development of ideas this is a real problem. But it does appear to be an increasingly common event, particularly with the rise of computerised textual analysis packages, as there are things that technology will mechanically help you to do without too much serious thinking (although coding advocates will always emphasise that poorly thought through coding is pointless and that carefully planned coding can really help in terms of the organisation of both thoughts and subsequent writing-up).

There is a further risk that advocates of qualitative research may end up deterring people from doing such research – indeed, it can be ironic in that complaints that not enough people are doing qualitative research are invariably accompanied by acknowledgements of the sheer difficulties associated with such research. It takes “special skills” and persistence, if not luck, on the part of the researcher. It is often said to be too hard to gain access (although this is increasingly a myth that experienced researchers try to expose), tough to write-up (not only in terms of convincing themes and contributions to knowledge but also in terms of using good English) and a long struggle in terms of getting things published, particularly if the case is constrained to a country that does not figure highly in a supposed hierarchy of suitable and attractive national data sources. We are not claiming that qualitative research is easy but it is

important to be sensitive to the dangers of making it appear too difficult – and, by default, making quantitative research seem relatively easier to undertake.

The big question that an emphasis on method begs is whether as a result of such methodological description the work assumes a more reliable and believable status. Can you verify that the methods have been applied in the way claimed? Is there a need for the methods section to be audited and verified? Is the final paper that gets published really explaining the chronological way in which the research project was undertaken or is it an *ex post* rationalisation – allowing theoretical perspectives recommended by reviewers to be incorporated into the paper even though such perspectives did not explicitly inform or directly drive the way in which interviews were conducted?

Usually, debates do not get to such extremes, stopping short by invoking notions of reasonableness and pragmatism. But there is a danger that we are becoming over-obsessed with method and vulnerable to the standard claim of talking more about research than doing it. This “methodisation” of qualitative accounting research can also have knock-on effects in the way it partitions different elements, aspects and attributes of the research process. It can serve to establish mental barriers to entry (by emphasising how much methodological literature has to be assimilated before you venture out on any practical research assignment). It can overly dichotomise qualitative and quantitative research even though there are accounting journals that emphasise routinely their openness to research of whatever methodological form. Putting method before issue can prevent important questions being asked and/or addressed. It also runs the risk of creating enclaves or camps of researchers into which those with alternative methodological leanings are excluded or not welcomed – and, even creating a further, unintended, consequence in terms of reinforcing the feared marginality of qualitative research.

The contributions to the special issue

In editing a special journal issue on qualitative research methods, we were obviously very sensitive to concerns such as those outlined in the foregoing but were also aware of the scope for further discussion on the application of research methods and the tensions faced by researchers as they seek to accommodate, respond to or influence today’s accounting research arena. We were keen to encourage debate on the processes by which qualitative research is and/or should be evaluated and the choices made by researchers in structuring and reporting on their research. We were interested in knowing of experiences of accounting researchers who have utilised or experimented with novel means of qualitative analysis or adapted more standard qualitative research methods in specific accounting contexts. The call for papers made it clear that we welcomed applications and demonstrations of novel means of qualitative analysis in accounting research, as well as broadly based assessments of more established qualitative approaches. The three contributions to this special issue illuminate various aspects of this broad ambition.

The first article by Jane Baxter and Wai Fong Chua draws attention to the aesthetics associated with how various types of qualitative research convince the readership. This important, but not easily codified, aspect has largely been neglected in previous discussions on the appropriateness of various methods in the accounting literature. Drawing on Golden-Biddle and Locke (1993), Baxter and Chua demonstrate how different forms of literary styling contribute to imbue qualitative research with a sense

of authenticity, plausibility and criticality. While these aspects may be seen as broad signifiers of the adequacy (or validity) of a particular piece of research, Baxter and Chua carefully delineate how the production of valid research findings is not reducible to mere technicalities of following a pre-defined set of tightly specified criteria as tends to be that case in much quantitative research in the positivist “mainstream”. In doing so, they emphasise the fluid nature of the relationship between the author/researcher and the audience of research. What constitutes convincing research findings is continuously re-constituted and negotiated in a particular social context. While such a view is in line with recent, post-modernist critiques of the notion of validity as a question of establishing stable epistemological guarantees (Lather, 1993; Koro-Ljungberg, 2004) it has largely eluded methodological debates evolving in the accounting literature.

While Baxter and Chua’s piece emphasises the considerable degree of freedom afforded to accounting scholars engaging in qualitative research, the second article by Bruce Gurd cautions against excessive flexibility in terms of how such research is classified. This is especially problematic if the flexibility required for qualitative research to flourish leads to obvious inconsistencies with the established research approaches being mobilised to legitimise such research. Using grounded theory as an example, Gurd critiques the extant accounting literature espousing this approach for failing to follow some of its basic canons. Few researchers, he argues, have fully explicated how their studies entail the four basic issues of coding and theory building, iteration between data collection and analysis, theoretical sampling and comparative analysis. These observations parallel similar criticisms of the use of grounded theory in the wider organization and management literature (Suddaby, 2006). Gurd sees this as especially worrying given the claims to prominence recently ascribed to grounded theory in qualitative accounting research. His critique certainly cautions against potential tendencies among researchers to unreflectively submit to intellectual “bandwagon effects” as different research approaches gain in popularity.

What is clear from Gurd’s review is that the relative freedom afforded to practitioners of qualitative research needs to be accompanied by a certain element of responsibility and respect for the intellectual roots of various research approaches. An attempt to advance the discussion on this topic is presented in the third article by Ali Elharidy, Brian Nicholson and Robert Scapens. Positioning grounded theory in relation to the broader tradition of interpretive research in accounting, this paper engages with the vibrant methodological debate recently emerging in the latter research genre. Recognising the somewhat ambiguous epistemological framing of earlier articulations of grounded theory, Elharidy *et al.* advance some guidelines prescribing how it may be applied in accounting research while staying true to the core premises of interpretive research. While this may entail some deviations from what is commonly perceived as standard grounded theory principles, they illustrate how this may be justified from particular epistemological vantage points as researchers try to make sense of everyday accounting practices. This testifies to the nearly inevitable choices of a pragmatic nature constantly facing practitioners of qualitative research.

Concluding remarks

In concluding this editorial, it is worth reflecting on some of the messages emerging from the papers in the special issue as a collective body of work – not just as a way of emphasising their overall contribution but also in terms of providing some directional

guidance with regard to future development in this field of inquiry. As noted earlier, methodological debate is always vulnerable to the criticism that it is better to do some empirical research rather than talking about how it might be done or legitimating or defending the ways in which it has been done (or not done). In the qualitative accounting research arena, there is the added risk that such debate serves to construct barriers rather than breaking them down. There can be an uneasy tension or balance between genuinely encouraging open, forward thinking discussion and using debate to defend established ways and methods and issue reminders of the order of things. Qualitative accounting research can be especially vulnerable here as such debates can often amount to qualitative researchers talking to each other and in complex ways that tend to exclude others or privilege those with most experience in the area. It can also serve to construct barriers that may not have been there to such a degree in the past by overstating the difference between different research methods and/or methodologies and associated determining characteristics or characterisations of such research. At its worst, it can generate debate which takes us not that far by getting too pre-occupied with labels, elements and definitional matters and spending relatively little time on the substantive linkages between research methods and research findings.

What is particularly nice about the papers in this special issue is that while certainly individually not having the same message, collectively, they serve to reinforce the sheer freedom for action and scope for choice in the accounting research arena. Whatever approach we choose, it is not that hard to find support for our actions – although, the corollary to this is that it is also not hard to anticipate that someone, somewhere will not appreciate what we have done and think we could have done it better! A cynic might say that what is most important is that the latter people are not the referees for your paper when you submit it to a journal! The management strategist would probably say just do your research well! We would suggest that it is important to think carefully about what we are doing when planning, undertaking and writing up our research, know what is expected of us in the particular research domain in which we are working or the ways in which we can convince others that what we are doing, while novel, is certainly worthy of merit. While highlighting the significance of both “truth” and aesthetics, an emphasis on the literary and factual forms of “convincingness” (Baxter and Chua, this issue) still has certain parallels with earlier methodological papers in the field of qualitative accounting research which sought to emphasise the overall methodological significance of notions of scholarship (Mills, 1993; Humphrey and Scapens, 1996). As Baxter and Chua imply, the greatest methodological weakness in accounting is the unthinking researcher.

A 1D that we would like to add in this editorial to methodological debate is the importance of remembering that research is meant to be a fulfilling activity. There should not only be a sense of beauty in research findings but also a sense of fun and excitement in undertaking research. The enjoyment of being a researcher is in real danger of being lost or buried in the evaluatory and auditable pressures associated with today’s academic arena – wherein people talk more and more of citations, publication counts and journal (departmental, and even individual) star ratings than what is being written and found out through research. Debates on research methods could be helped by being a little less serious and a little less antagonistic. Socially constructed worlds can be as much about understanding and appreciating other intellectual positions as they are about protecting or bolstering your own. If it is

possible to find support for a wide variety of methodological positions, maybe we should be spending more time considering the findings emerging from different studies (whether using traditional or novel methodological approaches) rather than the way in which they have been undertaken (or how, in general terms, they should be undertaken). One of the key messages to come out of texts like the *Real Life Guide to Accounting Research* (Humphrey and Lee, 2004) is that the practical task of undertaking research is full of unpredictable twists and turns, surprises, moments of sheer genius, acts of innate pragmatism and, every so often, manifold disasters. Research practice is not as neat, complicated, rational, serious, or as dichotomised as standard methodological discussions and debate can appear to make it.

In this regard, even as editors of this special issue, we would certainly be of the view that a journal like *Qualitative Research in Accounting & Management* should be more about reporting the results of qualitative research studies rather than writing about how to do qualitative research. That said, there remain some important methodological reflections that we are still very keen to encourage. We still know very little of the influence on accounting research of national research contexts, traditions and cultures – especially those in non-first English speaking and developing countries. It would be useful to see more reflections on the tensions between such factors and the standards of evaluation dominating the international accounting research community and established accounting journals. In particular, to what extent (in what way and through what processes) are research traditions changing and what implications is this having for certain forms of research, conceptualisations and levels of understanding of accounting practice and modes of career advancement. We are certainly aware of countries where traditions of national accounting excellence and modes of research investigation are either being “sacrificed” or “improved” (depending on your outlook) in the pursuit of publications in “leading” international journals. It remains an open question as to whether such developments are things that researchers are able and willing to write about but they certainly incorporate interesting methodological issues and standpoints.

Finally, we feel there is still much to be gained from considerations of, and attempts to secure, the possibilities of mixed methods research, especially where this entails attempts to straddle established paradigmatic boundaries (Modell, 2007). For all the supposedly technical complexity of modern-day risk management practices in the financial services sector and the rise of financial econometrics, today’s credit-crunch and string of banking collapses is one very timely practical reminder of the potential gains to be had from studying a highly quantitative arena from a qualitative perspective. If scientific principles and cultural factors can be applied to the study of colour (Ball, 2002), there is much yet to be done through the development and mixing of accounting research methods, ideas, environments and researchers themselves.

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The field researcher as author-writer

The field
researcher as
author-writer

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Abstract

Purpose – The purpose of this paper is to introduce the literary authority of qualitative management accounting field research (QMAFR) and its interconnectedness with the scientific authority of this form of research.

Design/methodology/approach – The paper adopts a non-positivist perspective on the writing/authoring of QMAFR. The paper illustrates its arguments by analysing how the field is written/authoring in two well-known examples of qualitative management accounting research, using Golden-Biddle and Locke's framework as a way of initiating an understanding of how field research attains its "convincingness".

Findings – The paper finds that these two examples of QMAFR attain their convincingness by authoring a strong sense of authenticity and plausibility, adopting writing strategies that signal the authority of the researcher and their figuration of the "facts".

Research limitations/implications – The paper argues for a more aesthetically informed consideration of the "goodness" of non-positivist QMAFR, arguing that its scientific and aesthetic forms of authority are ultimately intertwined.

Practical implications – This paper has practical implications for informing the ways in which QMAFR is read and written, arguing for greater experimentation in terms of its narration.

Originality/value – The value of this paper lies in its recognition of the authorial and aesthetic nature of QMAFR, as well as its potential to encourage debate, reflection and changed practices within the community of scholars interested in this form of research.

Keywords Qualitative research, Management accounting, Narratives, Accounting research

Paper type Research paper

The end

Excessive concern, which in practice usually means any concern at all, with how ethnographic texts are constructed *seems* like an unhealthy self-absorption – time-wasting at best, hypochondriacal at worst (Geertz, 1988, p. 1, emphasis added).

It is a little unconventional to start at the end, but this paper is about an "end" – the end of the "literary innocence" (Geertz, 1988, p. 24) of qualitative management accounting field research (QMAFR). For too long we have been writing the field without giving active consideration to the authorial accomplishments informing our research[1]. However, as readers can never re-visit and re-experience the field in ways that the researchers were able – and given that researchers can never fully represent the field (Quattrone, 2006) – our judgments about the quality of QMAFR turn, in great part, on how the field has been narrated. In short, authorial accomplishments are

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integral to the constitution of QMAFR. But how do we author the field, and what implicit norms are emerging in this regard?

These are not easy questions to answer as only intermittent attention has been paid to the textuality of accounting research (Arrington, 2004; Lowe, 2004; Lukka and Kasanen, 1995; Power, 1991; Quattrone, 2004). Moreover, much of the debate which has taken place has done so within the framework of a realist, scientific epistemology (Madill *et al.*, 2000), in which the authorial accomplishments of accounting field researchers are inevitably constituted by and assessed in terms of their representational “reliability” and “validity” (Atkinson and Shaffir, 1998; McKinnon, 1998). And whilst the authoring of such “scientific maxims” may be appropriate within the context of so-called mainstream accounting research in which these norms have been “institutionalised” (Abernethy *et al.*, 1999, p. 24), much QMAFR does not ascribe to the realist epistemology of the mainstream (Ahrens *et al.*, 2007; Chua, 1986). Indeed, the type of QMAFR that we will concentrate on in this paper is positioned as an antidote and alternative (Baxter and Chua, 2003; Baxter *et al.*, 2007) to the mainstream and its epistemic assumptions (Dent, 1991; Chua, 1995). Drawing on post-positivist notions of epistemology in which the reflexive and interpretive nature of research is acknowledged (Hammersley, 1983; Fanzosi, 1998; Latour, 2005; Law, 2004; Quattrone, 2004), the notion of authoring the field in ways that “reliably” and “validly” mirror practice is questioned and challenged by constructivist and critical perspectives on field research (Hammersley, 1983; Madill *et al.*, 2000; Maxwell, 1992; Stiles, 1993). This, nonetheless, raises the issue as to how such research, including QMAFR, achieves its narrative “convincingness” (Maxwell, 1992).

Accordingly, there has been a growing stream of debate within the broader social sciences as to how qualitative research is rhetorically constituted (Clifford and Marcus, 1986) and, correspondingly, how such textual accomplishments may be assessed. As a result, various “assessment criteria” have been proposed, with one approach focusing on retaining but re-working notions of reliability and validity within the framework of qualitative research (Maxwell, 1992; Hammersley, 1983), and another aiming to institute different criteria – such as “trustworthiness” (Stiles, 1993), “methodological rigour”, “interpretive rigour” (Fossey *et al.*, 2002), and “convincingness” (Golden-Biddle and Locke, 1993), for example – with a view to outlining a literary aesthetic for considering the narrative constitution of field research.

It is the purpose of this paper to acknowledge and characterise the role of the field researcher as author-writer, and to consider the ways in which his or her authorial accomplishments are constituted within the context of the contemporary (management accounting) research community. Moreover, we will aim to demonstrate the textual strategies embedded in published QMAFR by deconstructing two examples of such research. We conclude by recognising the literary authority of field research and its interconnectedness with scientific authority also.

Now let us go back to the beginning.

Writing the field

[...] the characteristic literary figure of our age is a bastard type, the “author-writer”: the professional intellectual caught between wanting to create a bewitching verbal structure, to enter what he [Barthes] calls the “theater of language,” and wanting to communicate facts and ideas, to merchandise information; and indulging fitfully the one desire or the other (Geertz, 1988, p. 20).

Qualitative field research is about writing. While the most common impressions of field research tend to centre on the collection of “naturalistic” data, writing is central to field research (Rose, 1990). Perhaps, it is so obvious that even researchers experienced in the field have failed to recognise the pervasiveness and importance of writing (Geertz, 1988). Yet writing is everywhere in field research. Field researchers write all the time: we write project proposals to clarify our thinking and to persuade others as to the viability of our work; we write to gain access to the field; we write to secure funding for our research; as observers we write notes in the field; as dutiful researchers we write field notes during our time-outs from the field; as reflexive researchers we write analytical memos to theorise the field (Strauss, 1987; Strauss and Corbin, 1990); and as honest scholars we begin writing up when we have retreated from the field. On reflection, therefore, it is not so far-flung to associate the field researcher with “writerly” activities. Writing is embedded throughout the entire process of doing QMAFR.

But field research is not just about writing, the “writings down” and the “writings up” (Atkinson, 1990; Watson, 1990)[2]. There is an authorial or creative and imaginative aspect to field research as well. That is, we author the field (Baxter and Chua, 1998; Clifford, 1986; Marcus and Cushman, 1982) by fashioning “fictions”, “translations” or “inscriptions” of practice (Bates, 1997; Clifford, 1986; Denzin, 1989; Latour, 1987), interpreting and emplotting the data we have written/collected. On reflection, the authorial function in field research should be quite apparent to those who have tried it. Why do we have so much trouble when it comes to “writing up”? Why does “the sea of data”, which once seemed so comforting, now seem so confronting? In writing the field we acknowledge that the field does not come with an immanent form of coherence; there are only “disconnected” (Hammersley, 1983, p. 24) activities which the authorial imagination configures into a convincing account of the field (Agar and Hobbs, 1982; White, 1978; Iggers, 1997). As such, field work is an essentially textual enterprise (Atkinson, 1992; Bates, 1997; Jeffcutt, 1994; Rose, 1990; Watson, 1990), combining both writerly and authorial activities. This has led Geertz (cited above) to characterise the field researcher as an “author-writer” (1988, p. 20).

Whilst van Maanen (1988) has indicated that this sentiment may result in positivist colleagues exclaiming, “The bastards are making it up!” (van Maanen, 1988, p. 134), such an accusation about field research is incorrect. Field research is “made” by field researchers. Generally, however, it is not “made up” (Clifford, 1986, p. 6; van Maanen, 1988, p. 134)[3]. Writing the field is not about the mere seduction of the written word. Even when “literary authority” replaces “scientific authority” (Fabian, 1992), field research embodies truth claims, albeit of a more “fallibalistic” and “limited” type (Hammersley, 1983, p. 27). There are referents to our writings of the field – certain things happened, certain things were said[4]. It is just that truth becomes a (potentially polyvocal and even multiplicitous) reflexive, authorial accomplishment (Law, 2004; Slack, 1996). As Bates (1997) indicates, truth is about both the style and content of field research. In short, what we know about the field is constrained and enabled by what it is that we can write about and the ways in which we can and do write (Atkinson, 1992; Denzin, 1989; Worden, 1998)[5]. It is more appropriate, we believe, to describe qualitative field work as “fictual” and not as fictional. But how is a convincing literary aesthetic being achieved in QMAFR?

Writing “convincing” field research

“Being There” authorially, palpably on the page, is in any case as difficult a trick to bring off as “being there” personally [. . .] The advantage of shifting at least part of our attention from the fascinations of field work, which have held us so long in thrall, to those of writing is not only that this difficulty will become more clearly understood, but also that we will learn to read with a more percipient eye (Geertz, 1988, p. 24).

As author-writers we aspire to produce “well-made” texts (White, 1987, p. 177), but this is no easy task. Each field researcher, sooner or later, must confront their community of active and critical readership: thesis supervisors and examiners, editors, reviewers, conference participants, colleagues, and so-on. And every individual that reads qualitative research formulates their own aesthetic response to our writings of the field (Iser, 1978); an author-writer can never be “the absolute ruler” of the “imaginary kingdom” that her or his readers inhabit (Clive, 1989, p. 34)[6]. Nonetheless, we would like to confirm that reports of the death of the field researcher as author-writer are exaggerated. Field researchers are readers too, reflexively assuming the role of this other during their crafting of the field (Atkinson, 1990). As a consequence, the author-writer invokes selected rhetorical practices which, whilst re-producing communal norms for “good writing”, also guide and prestructure a reader’s reaction to his or her construction of the field (Iser, 1978, p. 21)[7]. In short, we aim to write “convincing” texts (Arrington, 2004; Bates, 1997; Geertz, 1988; Golden-Biddle and Locke, 1993; Lowe, 2004; Jeffcutt, 1994) that will persuade readers that our stories are credible and truthful. How then is convincingness authored? Moreover, as Quattrone (2006) indicates, such a question is of political importance to the field also: the ways in which we constitute convincingness shapes our responsibilities to the “others” whom we narrate[8].

For the purposes of this paper, we will confine our discussion to an illustrative but well-known characterisation of the “convincingness” of field research which aims to provide a way of framing the authored constitution of the field from a post-positivist and narratological perspective. As such, we rely on the work of Golden-Biddle and Locke (1993, 2007) for this purpose[9]. These authors characterise “convincingness” in terms of three dimensions, which they refer to as authenticity, plausibility, and criticality (Golden-Biddle and Locke, 1993)[10]. The first of these dimensions – authenticity – refers to the authoring of the so-called “been there” quality of field research (Geertz, 1988). A convincing text will provide some form of written assurance as to both the field researcher’s presence in and understanding of the field (Golden-Biddle and Locke, 1993). Authenticity is about the inscription of ethnographic authority (Marcus and Cushman, 1982), which often involves some form of “calculative” reckoning narrating the number of days/months/years spent in the field, the number and type of informants and the quantum of data collected (Briers and Chua, 2001). Plausibility, the second dimension, is concerned with whether (or not) our renditions of the field make sense. Does an account of the field seem credible, given what readers know about their world? Is a field report coherent when assessed in terms of its structure (or genre) and its disciplinary context[11]? A text that persuades in terms of its plausibility has attained a form of literary authority referred to as *vraisemblance* (Atkinson, 1990, p. 39). Criticality, the third dimension of “convincingness”, is concerned with the imaginative possibilities that field research may provoke (Golden-Biddle and Locke, 1993). Can readers configure a larger and more enduring theoretical referent in the field (White, 1987)? Is the general well embedded and articulated in our accounts of the

local (Ahrens and Chapman, 2006)? Do our accounts of the field “speak to our human and organizational conditions of existence in ways that we find useful and desirable” (Clegg, 2006, p. 861)? Field research is ultimately convincing because it also possesses an “allegorical register” (Clifford, 1986, p. 103; White, 1987, p. 172). It is the textual figuration of these three forms of “convincingness” which gives qualitative field research its “look of truth” (Geertz, 1988, p. 3).

It is our intention in the remainder of this paper to illustrate how “convincingness” has been fashioned in QMAFR. We examine actual writing practices which have been employed in extant research, thereby developing, detailing and illustrating the arguments of critical accounting commentators, such as Arrington (2004) and Lowe (2004), who have acknowledged the centrality of convincingness in writing QMAFR and the construction of its truth claims. We do so with the aim of promoting a more acute writing and reading of our discipline, as well as challenging our preconceptions of the authority of research narratives. As Atkinson (1990, p. 176) states:

The recognition of the textual conventions of [field research], then, does not rob it of its referential value, nor does it relegate it to a second division of non-sciences. If we comprehend how our understandings of the world are fashioned and conveyed, then we need not fear that self-understanding. Rather than detracting from our scholarly endeavours, an understanding of our textual practices can only strengthen the critical reflection of a mature discipline.

Deconstructing the field

Must we always murder the interesting story in service of an “objective” format? (Agar, 1996, p. 4).

In this section of the paper we have chosen two examples of QMAFR to “deconstruct” in terms of their “convincingness”[12]. The papers to be discussed are Preston (1987) and Miller and O’Leary (1997). These particular papers were selected for a number of reasons[13]. First, both papers were written by respected accounting researchers whose work is regarded in high esteem by peers – that is, these are examples of the work of accomplished researchers. Second, both papers have been published in what are regarded as “tier one” accounting research journals (*Accounting, Organizations and Society* and *Journal of Accounting Research*, respectively) which are argued to publish work of the highest standing (Chan *et al.*, 2007). Third, the two papers arguably introduce an element of variety, given that they have been authored for two ostensibly different readerships – one European (Preston, 1987) and the other North American (Miller and O’Leary, 1997). There is sufficient debate within the accounting literature to indicate that different research values and practices distinguish the North American and more European-oriented management accounting research communities (Dillard, 2007; Merchant, 2007). Fourth and somewhat self-indulgently, focusing on these two papers also allows us to explore our own initial aesthetic reaction to these two papers. Why do we (and continue to) enjoy reading the paper by Preston[14]? Yet why are we more indifferent towards this particular publication by Miller and O’Leary? Our “deconstruction” of these two papers is followed by some reflections on the authoring of QMAFR.

Preston – writing “Johnny-on-the-spot”

Preston’s (1987) seminal management accounting field study examines the ways in which managers from a plastics division of a large English manufacturing

organisation were implicated in a process described as “informing”[15]. As our discussion outlines, Preston’s writing strategies are convincing primarily because he has authored a very strong sense of “Johnny-on-the-spot”[16].

Authenticity. So what textual practices does Preston employ to establish the authenticity of his field research? The authenticity of Preston’s work is secured, in part, by the ways in which he writes his “ethnographic presence” (Fetterman, 1989, p. 166). Preston’s account testifies to his intimate “local knowledge” (Geertz, 1983) of every-day life in the plastics division. He weaves a number of “symbolic markers” (Marcus and Cushman, 1982, p. 33) throughout his text, embedding signature words and phrases in his account so that readers become familiar with idiomatic terms such as “getting genned up” (p. 523), “stroke rates” (p. 524) and “butcher books” (p. 538). Likewise, Preston demonstrates his presence by indicating the access which he secured to the minutiae of organisational life; we see that Preston ventured into the back-stage (Goffman, 1959) regions of the plastic division, reporting that certain events occurred during lunch breaks (p. 526), for example. He reports “confidential” utterances (p. 531) and moments of acute embarrassment (p. 534) too.

The authenticity of Preston’s field work is buttressed by his characterisation of the extent of his “relationship with the field” (Golden-Biddle and Locke, 1993, p. 603). The first words that the reader encounters in his paper, “Based on a year long participant observation study . . .”, are very persuasive indeed. The reader is further assured by Preston’s claims that he was present at the research site “four days a week” (p. 522). Moreover, the reader is reminded of Preston’s presence in the field by his use of the personal pronoun on occasions (“When I asked the Factory Managers . . .”, p. 523) and references to his presence by the informants (“To be perfectly honest Alistair . . .”, p. 523)[17]. Also by allowing informants to speak (for example, “Peter Travers explains the process:”, p. 526), Preston further provides evidence as to his research’s authenticity[18].

Likewise, the authenticity of Preston’s field work is also informed by the character and quality of the data that he collected. Other than the time-intensiveness of the overall data collection process, claims that multiple sources of data were collected (documents, interviews and participants observation) comprise a further form of textual assurance for the critical reader, alluding to a process of triangulation (Yin, 2003). Similarly, claims that data were gathered *in situ* and in real-time (“talked with them informally on the shop floor”, p. 522) serve to reinforce Preston’s “presence”[19].

Finally, the authenticity of Preston’s research is affirmed by the ways in which he was genuine to the field experience, changing the direction of his research and setting aside his predispositions:

The initial focus of the research was to study the design, implementation and use of the computerised production information system. This focus shifted, as will become clear, to a study of how the managers informed themselves and each other (Preston, 1987, p. 522).

Plausibility. What is it about the way in which Preston writes that makes his field research sensible and believable? First, Preston implicitly appeals to his readers’ stocks of knowledge. As world-weary accountants we know that this sort of “funny-business” goes on in conjunction with the operation of formal information systems. Preston’s research is not far-fetched or fanciful – this is real life, not a fairytale. Second, in the event that a naïve reader has not been drafted by the nature of Preston’s empirical data, there are more

conventional indications as to the plausibility of his account. Preston (1987, p. 538) states that this is an area of management accounting practice that “has largely gone unresearched” (so do not be alarmed, dear reader, at what you have read ...). Third, Preston further persuades by providing corroborating and “strong evidence” (p. 537) from other disciplines. His practice of writing alternating empirical and theoretical sections in the narrative also serves to more closely link the local to the disciplinary. In short, plausibility is strongly influenced by practices of intertextuality – both personal and disciplinary.

Criticality. Does Preston, the field researcher as author-writer, encourage his readers to envision new possibilities for management accounting practice and research? Most probably. He establishes a degree of criticality in his text by summoning the standard rhetoric of an “implications” section (pp. 537-9). Preston signposts the significance of his work – even if it is somewhat pessimistic in its presage (“Despite all attempts to design more timely, detailed and accurate information systems...”, p. 537). Additionally, he alludes to possibilities for future management accounting research (“What is required now, to advance our understanding and design of information systems...”, p. 539). Nonetheless, it is the novel and engaging quality of Preston’s empirical data that remains with readers, although his text may be read as an enduring allegory for the social construction of accounting work.

Miller and O’Leary – writing “degree zero”

Miller and O’Leary’s (1997) field study describes how the adoption of flexible manufacturing practices at Caterpillar Inc. became embedded in a new set of capital budgeting practices described as “investment bundling”[20]. Overall, we contend that Miller and O’Leary’s field research convinces through its “degree zero” writerly style – a sparse, unembellished and quasi-scientific mode of textualisation[21].

Authenticity. How then is authenticity achieved in Miller and O’Leary’s field work? Miller and O’Leary clearly indicate to us that they were “there” by nominating the location of their research site (“This paper provides a descriptive and qualitative study of how capital budgeting practices at Caterpillar Inc...”, p. 257), identifying the particular plants in which the research was conducted (“plants in Aurora and Decatur, Illinois...”, p. 263), and thanking a number of Caterpillar’s employees by name in the acknowledgement section. Authenticity is conferred through textual practices enabling authentication of their research. As an extension of this, Miller and O’Leary also demonstrate their “presence” through the occasional use of emic terminology, such as “bundle monitors”, p. 261) and “concept reviews” (p. 262).

Miller and O’Leary are also convincing because of the narration of the extended nature of their study: “our study covered June 1990 to May 1994” (p. 258). They reinforce this by cataloguing their data collection activities. Not only do they state the types of data that were collected, but also they quantify the nature of their data:

We used four research methods: interviews, analysis of internal documents, observation of manufacturing processes, and study of public record.

Thirty-three semistructured interviews were held with 29 persons identified as key participants in managing the firm’s transition to modern manufacture. These included one group president, three vice presidents, three business-unit directors and ten assistant directors, seven other managers, and five shop-floor workers (Miller and O’Leary, 1997, pp. 258-9).

This “count” is convincing because it conveys the utilisation of multiple sources of data, indicates an impressive number of interviews, testifies to the extensiveness of their access to all echelons of Caterpillar Inc., and reveals the care and exactitude informing their research. A sense of precision is integral to the authenticity of this study.

Plausibility. What makes their field research seem sensible and believable? Miller and O’Leary have achieved plausibility by adopting the conventions of technical, scientific texts – their field research seem plausible “in terms of more orthodox research standards by adopting the latter’s form and devices” (Golden-Biddle and Locke, 1993, p. 605). But how have they achieved this clinical patina? First, Miller and O’Leary’s work is consistent with the structural expectations of a science report (Hammersley, 1998). There are sections dealing with the introduction, method, description, analysis and conclusions. Second, they adopt a realist (van Maanen, 1988; Madill *et al.*, 2000) style of writing. Their text is descriptive, economical, and unadorned. Their narrative is highly controlled by their authorial voice (Hatch, 1996). Their text also resonates with the metaphors of the physical sciences, such as “flow”, “velocity” (p. 261), “synchronous” (p. 265) and “technologies” (p. 266). The process of anonymously referencing supporting interview data by a date and position (“Interview with a factory superintendent, September 24, 1991”, p. 265), rather than by a real or assumed name, is likewise suggestive of the classificatory schemata of scientific activity. Furthermore, the visualist element of their text (pp. 264-6) conforms to the representational tradition of technical drawing. In all, Miller and O’Leary push for and achieve plausibility by assuming the artifice of objectivity[22], [23].

Criticality. But is Miller and O’Leary’s field study fecund with reflective and imaginative possibilities? Their style is matter-of-fact, their conclusions are mundane (“Our findings support Milgrom and Roberts”, p. 270), and the implications of their research are populist (“new capital budgeting *mechanisms* are needed”, p. 270). A “scientific” rather than “allegorical register” (Clifford, 1986) dominates this particular text by Miller and O’Leary[24].

“Convincingly” different or similar?

Whilst both Preston (1987) and Miller and O’Leary (1997) have authored published exemplars of QMAFR based on extended periods of data collection in the field, they have done so in different yet similar ways. In terms of these apparent differences, it may be stated that Preston’s writing is much more dialogic-looking than Miller and O’Leary’s. Preston has infused his narrative with voices from the field, liberally citing from various interviews with organisational participants. In comparison, Miller and O’Leary’s writing is monologic in its style. As such, the metered precision of Miller and O’Leary’s prose remains uninterrupted by the more profane and lusty utterances of everyday (rather than privileged academic) voices in the way that Preston’s text has allowed. Preston’s text is also less predictable in terms of its structure, vacillating between sections concerned with the field and then more general theoretical debates. Miller and O’Leary’s work, in comparison, unfolds quite predictably – dictated by the linear sequence expected of a science report. Nevertheless, it needs also to be acknowledged that both Preston and Miller and O’Leary seek to write the “truth” and to demonstrate the authority of their presence in the field. To this end, both Preston and Miller and O’Leary author the field by referencing its artefacts and idioms to

“stack” (Latour, 1987) their crafting of its coherence. The “convincingness” of both papers involves constructing a nexus between “the facts” and their narrations of the field. But as Latour (2005, p. 127) puts it, Miller and O’Leary have associated the narration of truth with a more bland and objective approach, whereas Preston has foregone some of the trappings of science-like writing in his quest for convincingness that is rooted in the shared (between the author and the reader) of the everyday social world[25], [26].

A research method “with a future”

But as the reader will presumably gather from the text, I will not be able to read or write ethnography in quite the same way anymore (van Maanen, 1988, p. xiii).

Given the issues raised in our discussion of the Preston (1987) and Miller and O’Leary (1997) papers, it is our intention, at this point, to consider these issues within the broader context of the writing QMAFR and the challenges that such a consideration presents for the field researcher as author-writer in our discipline.

The first issue raised by our discussion is to consider critically the ways in which we write QMAFR. Whilst QMAFR has emerged, in part, to mobilise non-positivist epistemologies in accounting research (Chua, 1986), it may be contended, to varying degrees, that the narration of QMAFR continues to pay homage to authorial strategies institutionalising mainstream forms of scientific authority in the field (Armstrong, 2007). Even though some exemplars of field research dispense with the ostensible trappings of the “science report” (for example, the statement of research questions/hypotheses and the inclusion of separate data and analyses sections) (Covaleski and Dirsmith, 1986; Preston, 1987; Mouritsen, 1999; Quattrone and Hopper, 2001), such writing conventions are still adopted to confer disciplinary “respectability” (Atkinson and Shaffir, 1998, p. 43) on QMAFR (Miller and O’Leary, 1997; Malina and Selto, 2001). Likewise, the emphasis placed on triangulation through the use of multiple forms of data collection and the increasingly exaggerated cataloguing of researcher engagement with the field (Briers and Chua, 2001; Vaivio, 1999), speak also to a lingering desire (perhaps, prudently) to establish the credibility of qualitative data in quasi-positivistic terms, invoking the “bankable guarantees” (Law, 2004, p. 9) of more mainstream research.

If it is accepted, however, that notions such as objectivity and reliability attain their workability within a naïve, scientific, realist type of framework (Madill *et al.*, 2000), and that other types of “good” research are possible (Latour, 2005; Law, 2004; Quattrone, 2006), then it may be justifiably asked why QMAFR enshrines vestiges of positivist forms of research. We need to remind ourselves that social constructivist QMAFR (Mouritsen, 1999) aims to articulate and understand a range of diverse perspectives informing accounting practice (Madill *et al.*, 20001; Maxwell, 1992). Likewise, more critical forms of QMAFR (Knights and Collinson, 1987) are positioned within a world view aiming to facilitate political awareness and action (Madill *et al.*, 2000). This then suggests a need to shift the narratological emphasis in QMAFR towards the “honouring” of alternative perspectives on accounting practice and/or a capacity to facilitate change and growth in readers, research participants and the research community (Stiles, 1993). As Hammersley (1983) comments, we then become less concerned with reliability and validity (as conventionally defined) and more concerned

with the aesthetics and political/instrumental effects of research narratives. The challenge, therefore, is to find a voice (or voices) which better express a non-positivist episteme.

One implication of this is that there should be some collective preparedness to innovate in relation to the writing of QMAFR, treating texts as experiments (some of which may fail) in characterising the complexity, fluidity and diversity of the field (Latour, 2005). Overall, the management accounting field researcher as author-writer is not very adventurous. There is little diversity in the ways that we write research (Pinch *et al.*, 1989)[27], even though a number of experimental research genres are being promoted more broadly within the social sciences for writing the field[28], [29]. Tinkering with alternative forms of writing in QMAFR may enable scholars to write about things differently and to write about different things. One particular form of authorial experimentation which may be worth further consideration within QMAFR, given its concern for characterising and understanding multiple perspectives (and even multiple realities) (Law, 2004) and their potential growth/change implications, concerns the narration of more “dispersed” (Marcus and Cushman, 1982, p. 43) forms of authority in field research which explore the border and intermingling of indigenous and authoritative field work (Gubrium and Holstein, 1999; Law, 2004)[30]. As Marcus and Cushman have pointed out, qualitative research is dominated by texts which are “domesticated” by controlling author-writers. Even when different voices from the field are permitted to speak (through the use of verbatim quotations, for instance)[31], these voices become a medium to narrate and reinforce the authority of the author-writer and the implicit conventions of academic writing which enable researchers to assert “their” claims as “facts”. This has led more radical commentators to lament the subversion of lay forms of knowledge in the writing of the field (Aguinaldo, 2004; Law, 2004), prompting an opportunity for reflection on the political implications of extant authorial practices in relation to QMAFR. Both social constructivist and more critical field researchers may benefit from acknowledging how our textual practices may foreclose the constitution of knowledge(s) (Aguinaldo, 2004) about accounting practices[32].

Another area for reflection concerns the importance of theoretical resonances in our writings of the field (Ahrens and Chapman, 2006). How is a “secondary” or “allegorical” referent embedded in field research (Clifford, 1986; White, 1987)? How does the here and now connect to other times and places? How may the field point to larger understandings of the interweaving of organisations, machines and accounting technologies? As such, recognition of the convincingness of field research, especially its criticality (Golden-Biddle and Locke, 1993), encourages a more active consideration of the non-literal implications of our writings of the field, but in ways that are not limited to factually- and statistically-bound notions of external validity. In short, we are arguing for QMAFR which connects the ethnographic (Atkinson, 1990) and sociological (Mills, 1959) forms of imagination. In doing so, we would argue, it is possible to constitute the “beef” (Zimmerman, 2001, p. 412) and rigour of QMAFR. Rigour, as such, is not something which can be “purchased with techniques” (McGrath quoted in Maxwell, 1992, p. 281; Eisenhardt, 1991) but through the development of a capacity to consider what it is that we have learned about accounting and organisational/societal functioning as a result of QMAFR[33].

This leads us inevitably to a consideration of how it is that we evaluate the “goodness” of QMAFR. If we choose also to recognise a literary, rather than

exclusively scientific form of authority for this type of research, we begin to acknowledge the limitations of espoused ways of evaluating field research. Take the argument offered by Atkinson and Shaffir (1998, p. 60), for example, in which it is proposed that management accounting field research should be evaluated on the basis of two “standards”, namely “validity” and “reliability”[34]. Validity and reliability are only two forms of rhetorical assurance about the goodness of research – and then only particular (science-like) types of convincingness[35]. We need to consider a far broader range of acceptable convincing strategies. As our consideration of Preston (1987) and Miller and O’Leary’s (1997) work suggests, there are “house norms” (van Maanen, 1988, p. 27) for writing and authoring field research – even if we are unable, at this point in time, to find an appropriate register to articulate them. As Geertz (1988, p. 6) states:

In discovering how, in this monograph or that article, such an impression is created, we shall discover, at the same time, the criteria by which to judge them. As the criticism of fiction and poetry grows best out of an imaginative engagement with fiction and poetry themselves, not out of imported notions about what they should be, the criticism of anthropological writing [...] ought to grow out of a similar engagement with it, not out of preconceptions of what it must look like to qualify as science.

Following Geertz (quoted above), accepting evaluative criteria emerging from, and being informed by the figurative dimensions of our writings of the field, arguably encourages and perpetuates our authorial imagination. Given that QMAFR is rarely conducted with a view to producing standardised results that may be replicated by another researcher, but is rather undertaken with a view to producing a compelling and illuminating narrative (Wainwright, 1997), a desire for immutable a priori “standards” in management accounting field research may be misplaced. As such, Marshall’s (1985, p. 357) warning is both timely and appropriate for those seeking the comfort of standardisation in QMAFR:

Rigidly trustworthy research may be limited by the methods devised to ensure trustworthiness. It could make qualitative researchers into nothing more than objective observers and coding specialists. It could lead to premature coding, forcing data within theoretical framework, closing off alternate conceptualizations and precluding discovery of hidden, secret, unrecognized, subtle, “unimportant” data, connections, and processes.

Notwithstanding, there is a need for further debate about the goodness, or otherwise, of the writing/authorial practices constituting QMAFR. And the challenge is to consider how it is that we accredit well-made and truthful stories of the field. In doing so, however, it is necessary to confront the aesthetic (Iser, 1978) dimensions of field research. Basically some researchers write the field in more competent and persuasive ways; we are drawn into their crafting of the field, transported to other imaginary spaces. In short, there is an intrinsic aesthetic dimension to field research which needs to be acknowledged, considered and discussed within the context of QMAFR.

But how might this discussion about “good” writing in field research proceed if we abandon notions of “rigidly trustworthy research” (Marshall, 1985 cited above) and a desire to imitate the writing style of “hard sciences” (Latour, 2005, p. 125), focusing instead on the “criticality” or the imaginative and aesthetic pathways to truths in QMAFR? Arguably, corresponding debates within the social sciences (Latour, 2005; Law, 2004) provide germane points with which management accounting field

researchers may usefully engage. In relation to this, Law (2004) argues that “good” research sustains the heart, imagination and the mind through its “messiness”. Rather than using messiness as a metaphor for poorly executed and written research, Law invokes this term as a way of encouraging researchers to address “multiplicities” as well as “singularities” (p. 82) in their narrations of the field. Criticality, as conceived from this viewpoint, is constituted by narratives highlighting the multiple realities which coexist in the field, rather than privileging a prevailing doxa. He provides illustrations of how, for example, “scientific”/geological explanations of “Ayers Rock” in Australia can reside with indigenous Aboriginal accounts of “Uluru” (2004, Chapter, 7)[36]. Law further contends that imaginative research not only characterises such multiple ontologies but also captures the changing or “shape shifting” (Law, 2004, p. 122) forms of these realities, creating fluid spaces to be apprehended and enjoyed, such as those found in the Aboriginal Dreamtime legends which invite chronic reinterpretation, development and elaboration[37].

Further to these themes, Latour (2005) argues that “well written” (p. 124) research enables the “social” to be “reassembled” by the reader (p. 138). Conceived from Latour’s vantage point, imaginative criticality is imbued by research narratives which create a “circulating” (rather than static) entity (Latour, 2005, p. 128), being an account in which all actors (human and non-human) transform or mediate networks of practices in various ways. Latour (2005, p. 138) thereby contends that good research enables us, as readers, to reassemble how the world works – but in a way that enables non-singular forms of “world-making” (Law, 2004, p. 151) in which the scientific engages (and re-engages) with the personal and political through our aesthetic experiences. So, good research prompts an imagining and re-imagining of realities and possible futures.

Correspondingly, we argue that QMAFR is incapable of achieving a look of truth independently of this aesthetic dimension. That is, the writing of “facts” depends on the literary convincingness of QMAFR: our aesthetic response to a text’s authenticity, plausibility and criticality helps to persuade us as to its “facts”. Yet, concomitantly, scientific authority is integral to the aesthetic and literary dimension of QMAFR! As a discipline we submit to quasi-scientific modes of writing, privileging a controlling authorial voice that constitutes its authority through calculative types of reckoning of the field, as well as structural predictability in our report of it. As such, truthful and well-made narratives necessarily invoke both literary and factual forms of convincingness. It is, therefore, the basic contention of this paper that future debates about the direction and quality of post-positivist QMAFR must acknowledge the hybrid accomplishment on which its constitution rests, contesting a modernist division between “truth” and “aesthetics” (Law, 2004, p. 143). How does the author-writer intertwine mutually informing scientific and literary forms of authority in narrations of the field? Accordingly, this paper represents an initial contribution to a debate about a hybrid identity for QMAFR. But irrespective of the outcomes of any future engagements with this debate, we can state now (like van Maanen, cited above) that we will read QMAFR with a more percipient eye. Our appreciation of the strengths and limitations of QMAFR has been heightened by an awareness of this duality of textual practices informing its constitution.

The end

This paper has discussed the textual constitution of QMAFR in terms of authoring-writing practices. In particular, we have outlined ways in which QMAFR achieves its literary convincingness, illustrating how two well-known examples of management accounting field research were narrated to achieve this. We concluded by arguing that QMAFR is constituted by a hybrid accomplishment in which both scientific and literary forms of authority are textually instantiated.

But by showing how the “rabbits have been pulled out of hats”, we do not wish to rob field research of its magic and attraction. Rather we argue that a recognition of the field researcher as author-writer makes QMAFR even more compelling in its thrall. The imaginative possibilities offered by an acknowledgement of our craftings of the field contribute to a more reflexive and critically-informed future direction for QMAFR.

To summarize briefly: A white rabbit is pulled out of a top hat. Because it is an extremely large rabbit, the trick takes many billions of years. All mortals are born at the very tip of the rabbit’s fine hairs, where they are in a position to wonder at the impossibility of the trick. But as they grow older they work themselves ever deeper into the fur. And there they stay. They become so comfortable that they never risk crawling back up the fragile hairs again (Gaarder, 1994, p. 16).

Notes

1. This contrasts to other disciplines, such as organisational anthropology, where it is claimed that “there are probably more people writing about organizational ethnography than actually doing it” (Bates, 1997, p. 3).
2. The term “*ethno-graphy*” attests to the importance of writing in field research (Tyler, 1986, p. 128).
3. This is not to deny that the “made up” is not incorporated into more experimental forms of qualitative research (Pinch *et al.*, 1989).
4. To deny the organisational referent of field research is to transport this research method to a potentially parlous moral state. As Iggers (1997, p. 13) remarks, in the context of post-modern historical work, non-referential writing would enable the denial of significant events – such as the Holocaust, for example.
5. Further to this, Law (2004, p. 147) points out that certain realities are not readily condensed into written texts.
6. We depart from an extreme post-modernist position, hence our focus on the craft of the author.
7. We do not use the term “rhetorical” in a dismissive fashion to connote “trickery” or “ornamentation”. Rhetoric is the means by which we persuade our readers and rhetoric is a feature of *all* research (Watson, 1990, pp. 304-306).
8. See Quattrone (2006) for a discussion of the political/ethical dimensions of the role of the author-writer as “auctor” (one who exercises authority over the field).
9. We have made use of Golden-Biddle and Locke’s (1993) characterisation of convincingness so as to initiate a discussion about the authoring/writing of qualitative management accounting field research that articulates more generally with similar debates and concerns within the social sciences. In so doing, we do not wish to endorse a particular framework, or limit any ensuing debate to that which may be conducted within it. Indeed, whilst their work provides a useful starting point for such conversations, as the latter part of this paper indicates, Golden-Biddle and Locke’s work invites broader excursions into the literature in

order to more fully explore the argued aesthetic aspect of writing qualitative management accounting field research.

10. Golden-Biddle and Locke propose these three dimensions of “convincing” as alternatives for positivist forms of authority. They state, “when the interpretive perspective of science is adopted, as in much of the work based on ethnography, the generally-accepted standards and practices for writing and assessing the convincingness of this work become increasingly difficult to apply” (1993, p. 595).
11. Genre is a rhetorical structure which acts as a “sensitising device” (Atkinson, 1990, p. 8), creating certain expectations of the writer within the reader.
12. Deconstruction is a process that aims to make visible the assumptions informing our writings of the field (Feldman, 1995).
13. We do not wish to convey any sense of disingenuousness in terms of the selection of these papers for review.
14. Moreover, why do our research students also enjoy reading this paper so much?
15. The abstract of Preston (1987) is reproduced in Appendix 1.
16. The term “Johnny-on-the-spot” is used by van Maanen (1988, p. 66) to denote a style of writing, such as Preston’s (1987), which narrates a strong sense of presence in the field – being there as events are unfolding in real time.
17. There seems to be a general aversion to the use of personal pronouns in academic writing (Power, 1991). As Clive (1989, p. 25) states: “They use the editorial ‘we’, or, less frequently, they adopt the chaste disguise of ‘the writer’ . . . The reasons lie partly in modesty, partly in the assumption that the ever-beckoning, though illusory goal of ‘objectivity’ is somehow fostered by an impersonal mode of writing – perhaps mainly in the conviction that good taste dictates distance between the author and the reader”.
18. It may be argued, however, that these quotations have been used dramatically rather than dialogically (Jeffcutt, 1994, p. 251).
19. Preston, like other ethnographers, also writes in the “ethnographic present” (Fetterman, 1989, p. 16) – his story of the plastics division will be “kept alive”, along with his ethnographic authority.
20. A digest of the Miller and O’Leary (1997) paper has been included in Appendix 2.
21. The term “degree zero” is adopted by Atkinson (1990, p. 46) to refer to writing styles (such as that in this paper by Miller and O’Leary, 1997) which are written in a “scientific” way, making use of “objectivist prose” (Latour, 2005, p. 126).
22. This seems to be a reasonable writing strategy, given the “scientific” nature of the journal in which this field study was published.
23. As Latour (2005, p. 127) observes, an objective style does not necessarily make something “true”.
24. This is not to be read as a summation of other examples of Miller and O’Leary’s research.
25. This may explain, in part, why as readers we are more drawn towards Preston’s (1987) style.
26. It should also be noted that authoring convincingness is a more complex and institutionally-bound process than is portrayed in terms of these two “deconstructions”. If the institutionalised practices and values of North American and more-European oriented academic circles differ (as is suggested by debate within accounting (Dillard, 2007; Merchant, 2007)), then practices of convincingness will be constrained and enabled by the context in which researchers seek peer esteem through publication. As such, editorial and review practices will further perpetuate particular ways of authoring convincingness (and authors will bow to the real or perceived expectations of reviewers in a bid to secure

- publications – indicating the potentially fragile, flexible and co-produced practices constituting the author-writer in published accounting research).
27. As Pratt (1986, p. 33) has commented, “ethnographic writing tends to be surprisingly boring”.
 28. Consider the following list constructed by Ashmore (1989, p. 66): plays; limericks; parodies; parables; dialogues; parallel texts; lectures; encyclopaedias; and the press report.
 29. Some authors take this line of argument even further and suggest that non-written genres, such as driving, cooking, dancing and gardening and so-on, may provide other “allegorical methods” for crafting the field and its realities (Law, 2004, pp. 146-7).
 30. This raises issues more broadly as to how “polyphony” is to be achieved in QMAFR (if this is desired). Nonetheless, we believe that there is a role to be played by journal editors in encouraging research which provides different perspective/views on accounting practices – such as that which has emerged in relation to the “Caterpillar debate” (Arnold, 1999; Froud *et al.*, 1998; Miller and O’Leary, 1994).
 31. It may be argued also that the use of verbatim quotes harks back to a positivist desire for “data”.
 32. However, as Hammersley (1983) indicates, field researchers cannot hide behind the multivocal nature of dispersed accounts – as the authority of the researcher is essential to their assembly, publication and dissemination.
 33. Clegg (2006, p. 861) puts this nicely when he states, “Narratives make sense not simply by fetishizing certain techniques but because they also address existential dilemmas in meaningful ways”.
 34. Atkinson and Shaffir (1998) are not alone in believing that field work should be “valid” and “reliable” (Kirk and Miller, 1986).
 35. Indeed, this is a point that Golden-Biddle and Locke do not seem to recognise in their argument. Whilst they posit their three forms of “convincingness” as alternatives to such ways of writing and reading field research, the rhetorical categories that they create do, in fact, enable a reading of both interpretive and positivist field reports (although different writing strategies may be evident in these different approaches to writing the field). “Scientific” research is informed by rhetorical forms of convincing also. Researchers write statistical significance and so-on, as meaning is not inherent in scientific numbers (Maines, 1993).
 36. Uluru is the Aboriginal word for the landform that white settlers called Ayers Rock.
 37. For example, it is sated: “Dreaming stories vary throughout Australia and there are different versions on the same theme . . . There are stories about creation of sacred places, landforms, people, animals and plants, law and custom. It is a complex network of knowledge, faith and practices . . .” (<http://en.wikipedia.org/wiki/Dreamtime> (accessed 31 October 2007)).

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Appendix 1

The abstract summarising Preston's (1987) paper is reproduced below.

"Interactions and arrangements in the process of informing" by Alistair Preston

Based on a year long participant observation study, this paper attempts to explain how managers are informed or inform themselves. In contrast to the hierarchical systems model of organisations and behaviour typically adopted by information designers, a model of the social order is presented. Adopting a symbolic interactionists perspective, the way managers are informed is defined as a process of informing in which managers construct and maintain arrangements to inform each other and themselves. These arrangements to inform employ media of interactions, observations, personal record keeping and attending meetings. In addition to describing the process of informing, supported with qualitative data from the research setting, a number of implications of this perspective for information design are considered (Preston, 1987, p. 521).

Appendix 2

This appendix provides a digest of Miller and O'Leary's (1997) paper. As there is no abstract for this paper we have quoted directly from their paper to provide a summary (see below).

"Capital budgeting practices and complementarity relations in the transition to modern manufacture: a field-based analysis" by Peter Miller and Ted O'Leary

This paper provides a descriptive and qualitative study of how capital budgeting practices at Caterpillar Inc., were redesigned to accommodate a shift from mass production technologies to modern manufacturing systems characterised by flexibility (the ability to adjust quickly to changes in demand and product designs) and specialization (the outsourcing of production of peripheral commodities). We describe how the firm's capital budgeting practices shifted from considering incremental asset purchase proposals to considering proposals to purchase sets of diverse but mutually reinforcing assets ("investment bundles") [...] our study covered June 1990 to May 1994. We used four research methods: interviews, analysis of documents, observation of manufacturing processes, and the study of public record [...] Our findings support Milgrom and Roberts's [1990, 1995] concept of complementarity relations but suggest a more complex view of how they are identified and acted on at firm and plant levels [...] we believe our analysis suggests that if investments in modern manufacture are to achieve the synergies available from coordinating spending across diverse but mutually reinforcing types of assets, new capital budgeting mechanisms are needed. We identify three distinct features of these mechanisms: new frameworks for proposing investments to incorporate the diverse assets that may economize on complementarity relations; new mechanisms for authorizing capital investment that provide strategic and corporate direction and link it with plant-level discretion [...] and new ways of monitoring investments that go beyond conventional financial postaudits and that monitor implementation against appropriate financial and nonfinancial targets (Miller and O'Leary, 1997, pp. 257-8, 270-1).

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The field
researcher as
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Remaining consistent with method? An analysis of grounded theory research in accounting

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Abstract

Purpose – The paper revisits the intellectual roots of grounded theory and aims to analyze the consistency of the method used in grounded theory research in accounting. About 23 papers are identified and analysed.

Design/methodology/approach – The paper is an analytical review of the research literature. It uses four fundamental canons of grounded theory to analyze accounting research.

Findings – Some accounting researchers who have used the label “grounded theory” for their research have misunderstood or not applied the core canons of grounded theory established by Glaser and Strauss and developed with diversity in other disciplines. Most claim to follow the specific approach of Strauss and Corbin, but the published research shows limited explication of method.

Originality/value – Since Parker and Roffey in 1997, there has been no analysis and re-evaluation of the burgeoning academic accounting literature using grounded theory. While celebrating the growth of this research, the paper does raise concerns about the lack of consistency of grounded theory research in accounting with the central canons of grounded theory, and it provides some directions for future grounded theory research by encouraging accounting researchers who wish to use grounded theory to engage more strongly in understanding the method and providing transparent explanations of their data collection and analysis methods.

Keywords Accounting research, Accounting theory, Research methods

Paper type Research paper

1. Introduction

Smith (2003, p. 139) comments that: “Grounded theory has been increasingly adopted as the preferred qualitative approach in accounting field study environments”. This paper is the first review of the progress in accounting research using this dominant qualitative research approach since it was advocated more than a decade ago (Parker and Roffey, 1997; Lye *et al.*, 1997). My central argument is that accounting researchers appear to have mis-used the term and used it for a label for other forms of inductive inquiry. This builds on the same argument of Suddaby (2006) in the management literature where the *Academy of Management Journal* has made clear its policy of rejecting papers claiming to be “grounded theory” which use an “overly generic use of the term ‘grounded theory’ ” (Rynes in Suddaby, 2006, p. 633) and where “‘grounded theory’ is often used as a rhetorical sleight of hand” (Suddaby, 2006, p. 633). Although the same phenomena has been observed in other disciplines it is helpful for qualitative researchers in accounting to step back and re-evaluate the methods they are using.



There are two key strands to the paper. First, I follow the trajectory of the development of grounded theory research both within and outside of accounting research to re-focus on the core elements of grounded theory. Second, I wish to analyse critically whether accounting researchers have been consistent with the method. While welcoming a variety of epistemological stances; I am arguing that grounded theory is a method, not a methodology[1]; and that consistency with method is important for research. Where accounting researchers are using their own approach to inductive theory development, an alternative label than grounded theory might be helpful. Then researchers could carefully explain their approach.

The earliest published papers which use grounded theory go back to 1983 (Covaleski and Dirsmith, 1983). Smith's (2003) accounting research methods book contains a section on grounded theory – the first in this type of book. Despite the strong presence of grounded theory research in health and nursing fields (Chenitz and Swenson, 1986), Parker and Roffey identified only four papers in the accounting literature[2] which used a grounded theory method reflecting a past “bias against grounded theory” (Hopper and Powell, 1985, p. 455). In the last decade there has been much more research published using grounded theory. In particular, the analysis of the grounded theory papers in Table I shows that *Accounting, Auditing & Accountability Journal (AAAJ)* has become the most common preferred outlet for accounting researchers working within grounded theory.

This review comes at a time when there has been some re-evaluation of the interpretivist position in accounting research (Ahrens *et al.*, 2008, Willmott forthcoming in *Critical Perspectives in Accounting*). Kakkuri-Knuuttila *et al.* (2008) have reviewed the seminal piece by Dent (1991) to argue the impossibility of a pure subjectivist position. Although never mentioning or referencing grounded theory, they do argue for a social constructivist position – the foundation of grounded theory.

This paper takes a different turn. While the use of grounded theory in accounting research may be celebrated, there appear to have been problems in the application of grounded theory in accounting. The lack of adherence to generally agreed canons of the method may bring into question the quality of the research and cast doubt on the findings. It may well be a disservice to accounting research if grounded theory is used as a label to make the research seem more rigorous. In such cases, it would be more appropriate to describe the research method as inductive theory development or when appropriate describe it precisely as content analysis.

Grounded theory has been defined in its most general form as “the discovery of theory from data” (Glaser and Strauss, 1967, p. 1). Despite the sub-title of the 1967 book being “Strategies for qualitative research”, their lack of clarity of the “strategy” led to a flurry of books and articles explicating the method (Glaser, 1978; Strauss, 1987). However, in 1990[3], a rift became apparent in the grounded theory area when Strauss joined partnership with a nursing researcher, Corbin, to produce a more clearly defined system of grounded theory (Strauss and Corbin, 1990). Their definition of grounded theory was “a qualitative research method that uses a systematic set of procedures to develop an inductively derived grounded theory about a phenomenon” (Strauss and Corbin, 1990, p. 23). Glaser was angry about this perceived shift to a “new” more prescriptive “systematic” approach which forced the data rather than allowing theory to emerge (Glaser, 1992). He redefined grounded theory as “a general methodology of analysis linked with data collection that uses a systematically applied set of methods to generate an inductive theory about a substantive theory” (Glaser, 1992, p. 16).

Table I.
An analysis of a sample
of grounded theory
research in
accounting

	Grounded theory approach	(1) Explanation of coding and theory building process	(2) Iterative process of data collection and analysis	(3) Theoretical sampling	(4) Comparative analysis
Covaleski and Dirsmith (1983)	Glaser and Strauss (1967)	No	Yes	Not stated	Yes
Covaleski and Dirsmith (1984)	Glaser and Strauss (1967)	No	Not stated	Not stated	Not stated
Czarniawska-Joerges (1988)	Glaser and Strauss (1967)	No	Not stated clearly but appears not	Not stated	Yes
Gibbins <i>et al.</i> (1990)	Glaser and Strauss	No	No	No	No
Innes <i>et al.</i> (1996)	Strauss and Corbin	Yes	Not stated	Not stated	Not stated
Norris <i>et al.</i> (1996)	Strauss and Corbin	Yes	No	No	Not stated
Slagmulder (1997)	Strauss and Corbin	Yes	No	Not stated	Yes
Baxter and Birkett (1998)	Strauss (1987)	Yes	No	No	Not stated
Abdul-Rahman and Goddard (1998)	Strauss and Corbin	Limited	Not stated	Not stated	Not stated
Barker (1998)	Glaser and Strauss	None	Not stated	No	No
Greenhalgh (2000)	Orton (1997)	Yes	Yes	No	Yes
Lightbody (2000)	Strauss and Corbin	Limited	No, except "dialogue"	No	Not stated
Hines <i>et al.</i> (2001)	Strauss and Corbin	Yes	Not clear, although by inference no	No	Yes
Parker (2001)	Strauss and Corbin	Limited	No	Not stated	No
Parker (2002)	Strauss and Corbin	Limited	No	Not stated	No
Norris (2002)	Strauss and Corbin	Yes	No, but acknowledged	Not stated	No
Parker (2003)	Strauss and Corbin	Limited	No	Not stated	No
Goddard (2004)	Strauss and Corbin	Yes	Not stated	Not stated but by inference no	Yes
Wickramasinghe <i>et al.</i> (2004)	Strauss and Corbin	No	No	No	No
Goddard (2005)	Strauss and Corbin	Yes	Not stated	Not clear	Yes
Holland (2005)	Locke	Yes	Yes	Yes	Yes
Lye <i>et al.</i> (2005)	Strauss and Corbin	Yes	Yes	No	Yes
Goddard and Assad (2006)	Strauss and Corbin	Extensive	Not clear, although implication is yes	Not clear	Yes
Solomon and Solomon (2006)	Strauss and Corbin	Yes	Limited	No	Yes

Any method evolves; grounded theory has changed. Its diffusion started from the nursing research community as Glaser and Strauss both took up academic positions for lengthy periods in the nursing faculty of the University of California – San Francisco. They trained a large number of grounded theorists in nursing leading to hundreds of published papers using grounded theory method (Benoiel, 1996). The writings of Glaser and Strauss and a range of conferences diffused the grounded theory method to scholars in other disciplines. Although the grounded theory method has evolved, there are some central canons that relate to the method used. This paper provides an overview of the changes in the grounded theory approach over the last two decades. It does this to re-focus grounded theory accounting research which has tended to lose contact with the central canons of the method.

The take-up of grounded theory method in accounting has been more limited than in nursing. This is partly because grounded theory has been identified as a “craft” (Martin and Turner, 1986) passed on through an oral tradition and mentoring (Stern, 1994). Accounting researchers are therefore potentially guilty of having learnt “it from a book or, more likely a book chapter or, with no reference from which to work, decided to manipulate the data in any old way and name it grounded theory” (Stern, 1994, p. 213). The concern has been expressed that as grounded theory becomes more popular it will be abused as a label to cover content analysis (Morse, 1994) and general qualitative approaches, lacking clear conceptual foundations and “imprecise application” (Annells, 1996, p. 391). Sometimes in accounting research the method has been labelled as grounded theory, when on closer inspection it appears to be content analysis.

There has been a significant contribution of inductive theory development in accounting with a growing body of papers producing insights which are more novel and richer than that obtained by using an existing body of theory. Exploring the contribution of this research is not the purpose of this paper. One example is Parker’s (2001) model of reactive planning which provides an explanation of the antecedents and the ultimate results of this behaviour in “negotiated financial control”. Another example is Abdul-Rhman and Goddard’s (1998) new understanding of accountability in two Islamic religious councils. They use an inductive approach to argue that the sacred-secular divide discussed in the analysis of Christian institutions does not apply in an Islamic context.

While arguing for consistency with method, I am not arguing for constraining methodology. The two English-language books on grounded theory in the business and management literature (Locke, 2001; Goulding, 2002) both investigate methodological pluralism while clearly differentiating grounded theory from other forms of qualitative inquiry.

In the next section, the evolving perspectives of grounded theory are discussed. By explaining the various schools of thought, I am providing a basis for future accounting researchers to be clearer about which version of grounded theory they are using and to clarify their own ontological and epistemological stances. The Section 3 provides the evidence that grounded theory in accounting research has not been consistent with the method. The following section produces an argument that even if method was followed consistently there would still be issues with the use of grounded theory; issues which need to be addressed. The paper concludes with possibilities for improving grounded theory research in accounting.

2. The changing perspectives of grounded theory

Grounded theory is “rooted” in symbolic interactionism (Stern, 1994, p. 215; Bowers, 1988; Chenitz and Swenson, 1986; Lye *et al.*, 1997; Locke, 2001). The researcher “hopes to construct what the interactants see as their social reality” (Stern, 1994, p. 215). Some grounded theorist researchers wish to move beyond the symbolic interactionist view and develop more critical approaches to grounded theory research (Annells, 1996).

Some confusion in grounded theory relates to the variety of methodological stances and methods which are labelled as grounded theory. The grounded theory approach cannot be expected to be rigid and prescriptive, although there must be some central tenets of the approach. In a collection of readings, edited by Glaser, it is noted: “Although I attempt to be faithful to the form and logic of their approach, over the years I have developed my own style of using grounded theory” (Charmaz, 1994, p. 160) or as May expresses it “. . . we . . . have come across the general forms of the process we have come to call grounded theory . . . and they are different in some fairly interesting ways” (quoted in Morse, 1994, p. 211). In the nursing area alone, there were at least 146 refereed articles labelled “grounded theory” in the short period between 1990 and 1994 but of these only 33 could strictly be classified as grounded theory (Benoliel, 1996). A number of methods are building out of the grounded theory method developed by Glaser and Strauss (1967), but “a child once launched is very much subject to a combination of its origins and the evolving contingencies of life. Can it be otherwise with a methodology?” (Strauss and Corbin, 1994, p. 283).

A comprehensive analysis of the grounded theory literature suggests four particular schools of grounded theory. First is Glaser’s emergent approach, best reflected in Glaser (1978). The second comes from Strauss’ collaboration with Corbin which produced a much more structured approach (Strauss and Corbin, 1990). The third is the development of the approach by Strauss before his collaboration with Corbin (Strauss, 1987). Papers continue to be published which rely on earlier development of grounded theory by Strauss who adhere to his general approach but do not agree with the Strauss and Corbin (1990) approach. Finally, there is the dimensional analysis of Schatzman (Schatzman, 1991; Kools *et al.*, 1996).

Many authors would not see the second and third as different schools – Strauss and Corbin’s approach being an extension of the diversion of thought between Glaser and Strauss. Glaser, of course, saw Strauss’ co-authored work with Corbin as abandoning all the previous agenda. This produced one of the most vitriolic attacks in academic history in Glaser’s (1992) little red book.

Various efforts have been made to compare the differences between different grounded theory approaches, but primarily focusing on the divergence between Glaser and Strauss (Stern, 1994; Parker and Roffey, 1997; Lye *et al.*, 1997). Some of the analyses concentrate purely on differences of method rather than methodology (Parker and Roffey, 1997). Heath and Cowley (2004) analyse the differences in relation to the greater deductivism in Strauss and Corbin, with their different use of the literature and a different approach to coding.

It has been argued that historically the grounded theory research endeavour was formulated from a post-positivist stance (Annells, 1996). Evidence for this belief is particularly found in the writings of Glaser (1992) who at times assumes an objective reality, “true meaning” (p. 55), in which “scientific facts” (p. 30) can be developed by

an objective researcher. The work of Strauss and others appears to stress a more relativistic approach to ontology and epistemology (Annells, 1996), by accepting a “reality that cannot be known, but is always interpreted” (Strauss and Corbin, 1990, p. 22) and the subjectivity of the researcher who is never “neutral, detached and objective” (Bowers, 1988, p. 43). Strauss and Corbin appear to have moved grounded theory to a more interpretivist (Parker and Roffey, 1997) or constructivist (Annells, 1996) stance.

In contrast with this seemingly more relativistic stance, the work of Strauss and Corbin has provided a more rigid procedure. Glaser (1992) argues that this forces data into a model, and thereby cuts off the development of interpretations by the constraining of theory. He remains wedded to the belief in “emergence” and that creativity comes from memoing and constantly developing concepts that fit all of the data and are changed to meet each new data. At no stage is the data forced to meet a concept. Heath and Cowley (2004) reasonably claim that Strauss and Corbin have moved from pure induction to deduction and verification. For this reason it has been seen as “a densely codified structured operation” (Stern, 1994, p. 220), rule bound and formulaic (Melia, 1996).

Indeed, this produces an interesting conundrum. While Glaser appears to argue for a more objectivist-realist ontology he advocates a relatively unstructured method, and resists the codification found in Strauss and Corbin. Strauss and Corbin, on the other hand, while ascribing to a more subjectivist position than Glaser, are much more willing to adopt a highly prescriptive and structured method.

The approach of Strauss and Corbin (1990) specifically includes the contextual issues beyond the actors’ views. However, grounded theory researchers who do not accept the Strauss and Corbin paradigm would still argue that the researcher should go beyond the experience of the actors. For example, the idea of “marginality” is recommended (Bowers, 1988); where the researcher attempts to keep one foot outside of the research environment. There may be a belief that researchers using grounded theory can move into post-modernist or critical research, but that relies on shaking off its roots in symbolic interactionism and its claims to theory construction (Annells, 1996).

It would not be surprising that the more structured approach of Strauss and Corbin (1990) would appeal to accounting researchers because of the attractiveness of its precise procedure and structure to researchers brought up with the order of conceptual frameworks, and with backgrounds in quantitative methods. However, the adoption of Strauss and Corbin’s approach may restrict the creativity of theory building where the “procedures are getting in the way” (Melia, 1996, p. 376).

Accounting researchers have often adopted Strauss and Corbin’s approach, but an even stronger research tradition might arise if accounting researchers explored more thoroughly the underlying assumptions of the method they are working with. One of the weaknesses of Parker and Roffey’s (1997) argument is that they minimise the differences between Glaser and Strauss, rather than carefully distinguishing them as is found in the broader grounded theory literature (Heath and Cowley, 2004; Stern, 1994; Melia, 1996).

Corbin and Strauss (1990) proposed 11 canons of grounded theory method. Some of these are not critical (e.g. that grounded theorists need not work alone), and some are contested. Starting with these 11, I have used the writings in the method of grounded theory and the only two books on grounded theory in business and management (Glaser and Strauss, 1967; Glaser, 1978; Stern, 1980; Strauss, 1987; Bowers, 1988; Glaser, 1992; Corbin and Strauss, 1990; Goulding, 2002; Locke, 2001; Suddaby, 2006) to arrive at four key canons that are uncontested.

The first of these is that grounded theory is an iterative process of data collection and analysis. Corbin and Strauss (1990) make this the first of their “canons and procedures”. This was fundamental to Glaser and Strauss’ (1967) approach and was picked up by Parker and Roffey (1997). A researcher who collects all of their data and then starts to analyse it is not using the grounded theory method. In true grounded theory “data collection and analysis proceed simultaneously” (Charmaz, 1994, p. 96). As Glaser and Strauss (1967, p. 71) expressed it: “Research aimed at discovering theory . . . requires that all three procedures (data collection, coding and analysis) go on simultaneously to the fullest extent possible”. “Grounded theory is about the simultaneous collection and analysis of data” (Goulding, 2002, p. 25).

The second core canon is theoretical sampling; the fourth of Corbin and Strauss’ “canons and procedures”. Data gathering is driven by the theory. The concept of theoretical sampling is critical to all approaches to grounded theory – for without it the grounded theorist argues there can be no closure in theory building. Theoretical sampling involves seeking out comparison groups as the theory is developing (Locke, 2001; Bowers, 1988) and collecting new data based on emerging categories. Grounded theorists often seek disconfirming cases which may contradict parts of the present theory development and hence enrich theory development. It is only when all contradictory points have been elicited that the researcher can come to some limited finality in the process.

The third is the constant comparative method. Strauss and Corbin (1990, p. 67) explain it as the “identification of variations in the patterns to be found in the data”. Glaser sees it more in terms of comparing differences and similarities to integrate categories and their properties. The comparison process happens throughout the grounded theory process so that the theory becomes richer.

A fourth canon is the explanation of coding and the theory building process. Critical to the acceptance of qualitative research is the willingness of the researcher to “articulate as fully as possible the processes associated with data analysis” (Bryman and Burgess, 1994, p. 224). Morse (1994, p. 24) expresses the concern that in grounded theory she observes: “The neglect or inability of qualitative researchers to make explicit the cognitive struggle of model or theory construction . . .”. Given that coding and theory generation is an area of clear difference between the Glasserians and the Straussians, researchers could be expected to explain their approach to coding and theory development.

I have been unable to find a single paper on grounded theory method which disagrees with these four central tenets of the method. I have not found an accounting researcher who disputes them, even though they have may not have been followed in accounting research. The next section explores the level of consistency with method in accounting research.

3. A review of studies in accounting

I argue that when reading the grounded theory accounting research readers are entitled to ask whether the researchers are really using grounded theory or whether they are using an inductive approach which has been labelled as grounded theory, “a nod in the direction of general direction of grounded theory and then a progression to a generalised qualitative analysis” (Melia, 1996, p. 376). Then the reader needs sufficient detail of the method to make a judgment whether it is sound grounded theory. It is not my purpose to speculate why researchers have chosen to use a particular approach to “grounded theory” in the context of their research.

The four canons (Corbin and Strauss, 1990) established earlier in the paper have been applied to the established body of accounting research which uses grounded theory. In Table I I have analyzed grounded theory papers in the top 20 accounting journals using the most recent research of the top 20 journals reviewed by Lowe and Locke (2004)[4]. My sample was to the end of 2006, adding some papers from *British Accounting Review* which is not in the top 20 but has been willing to publish grounded theory research. I have also used Google Scholar in January 2007 in order to pick up grounded theory papers published as book chapters. Search terms were “grounded theory” and “Strauss”, which picked up all papers using grounded theory and referencing either the older versions of Strauss’ work or Strauss and Corbin. The limitations of the sample are that it excludes papers from the non-English language accounting literature and research published as book chapters.

Looking at the first column, grounded theory approach, there seems to be a trend that the earliest published studies referenced Glaser and Strauss (1967) whereas nearly all of the recent studies are built on Strauss and Corbin (1990). The more functionalist approach of Strauss and Corbin (1990) probably fits more comfortably with accounting researchers. It tends to lead the researcher in a step by step approach.

An explanation for the form of “grounded theory” being used in accounting research may be a form of academic tribalism (Becher, 1989). Of these papers, nine appear in *AAAJ*, two more were presented at one of the APIRA conferences supported by *AAAJ* and two more were authored by the editor of *AAAJ* although published in other journals. It may be that this is a community of scholars who are supportive of the Strauss and Corbin approach which is perpetuated amongst them. This “tribe” acts outside the ambit of the broader “tribe” of grounded theorists and appears unconnected with them.

The next column explores the level of explanation of coding and theory building. Observable in these papers is that many researchers do not explain their process of theory building. Yet transparency should be a key criterion for publishing qualitative research (Suddaby, 2006; Johnson *et al.*, 2006; Meyrick, 2006). A careful reading of the 24 papers in the sample and their antecedent conference or working papers shows that it is not uncommon for detailed explanation of the process of theory generation to be deleted in the process of development to a final journal publication. Bruggeman and Slagmulder (1995) was originally in the above table as it is known to be grounded theory research because of the previous working papers and conference papers (Greenhalgh, 2000) – but after the review process its final form in the accounting literature there is no mention of grounded theory. Baxter and Birkett (1998, p. 7) claimed their work to be ethnography and a “form of ‘grounded theory’”. Three accounts are developed to answer three different research questions and core categories are developed by integration of the *in vivo* categories and sociological constructs. In its latter iteration (Baxter *et al.*, 2004), with the addition of Chua as an author, all mention of grounded theory is gone.

It must be acknowledged that some accounting researchers do give detailed explanation of the process of data analysis and theory building (Norris *et al.*, 1996; Innes *et al.*, 1996). Norris *et al.* (1996) does not follow the iterative process of data collection and analysis as interviews were completed before data analysis commenced but we do know the method used and can choose to accept it as credible or not. Although Lye *et al.* (2005) did use iterative data collection and analysis there is no evidence that theoretical sampling was used – the sample appears to have been

selected before any interviewing was started. Lye *et al.* (2005) also demonstrates an approach to theory building where one section of text can only have one theoretical concept. These are built up into more macro concepts. Although they obtained closure, this model building process seems a departure from the tradition of grounded theory. Following the full grounded theory method may have produced a richer theory where more theoretical dimensions were identified. It is not that the explanations are unsupported but the possible richness that was lost.

An analysis of the last three columns, which analyse the consistency with the central canons of grounded theory, produces a result similar to the results in the management literature where Suddaby (2006, p. 640) claims about submissions to the *Academy of Management Journal* that: "... the term 'grounded theory' was interpreted to mean 'anything goes'". Suddaby suggests that such contributions to the literature should be rejected by reviewers. An analysis of the 24 "grounded theory" papers suggests that many do not fit the observed canons of grounded theory and hence do not appear to be grounded theory.

An example is Barker's (1998) research which does use an inductive approach, but it is not based on Glaser and Strauss. Barker commenced with a pre-determined model and went out and interviewed fund managers and others using a ranking scale of items as part of the data collection. This is not consistent with grounded theory canons. Suddaby (2006) argues that when theoretical sampling and saturation are not used, the researcher may well withdraw early from the field and produce "simplistic output". In contrast to Barker, a paper in the same journal (Holland, 2005) shows genuine engagement with the canons of grounded theory and is firmly based in that tradition. Similarly to Barker, Wickramasinghe *et al.* (2004) uses the term grounded theory but their research appears to be a rich ethnography, unconnected with grounded theory. The reader can only speculate that the label "grounded theory" has been used as a form of legitimation of the research.

The canons of grounded theory are abandoned by researchers who collect a range of interviews and documents and then seek to build theory using what is called "grounded theory". The iterative approach to data collection and analysis, accompanied by theoretical sampling and the constant comparative method, is so central to all of the grounded theory literature (Suddaby, 2006) that it is difficult to understand how researchers would publish papers claiming to be "grounded theory" while not following the method. Grounded theorists in the field are continually seeking out disconfirming cases and constantly comparing to develop new theoretical codes. *Ex post* data analysis is not grounded theory. The paper by Gibbins *et al.* (1990) is another case of using a traditional sampling plan instead of shaping "... their data collection from their analytic interpretations and discoveries" (Charmaz, 1994, p. 96). In contrast Covaleski and Dirsmith (1983, p. 324) follow the grounded theory tradition: "... as data were collected, coded, analyzed and related to the emerging theory in an interactive fashion".

Covaleski and Dirsmith provide an exemplar of authors who focus on the method used rather than using the label of grounded theory method. Covaleski and Dirsmith (1988) appear to have many of the characteristics of grounded theory. The outcomes used from one interview were used in another so that an iterative process of theory development was used. However, no process of theoretical sampling was used. The authors do not use the grounded theory label at all but suggest that it is "qualitative, naturalistic research" and explain thoroughly the research approach used, rather than

a passing mention of grounded theory. They are concerned with the issue of validity, and specifically whether their prior conceptions have moulded their thinking rather than the views of the 110 interviewees. They believe that they represented the “intensity” of the views of the respondents but leave it to future research to corroborate their findings.

The analysis from the table suggests that research using grounded theory method in accounting is not remaining consistent with method. It would be preferable if the label “grounded theory” was not used for work which is inductive but does not follow the central canons of the method (Suddaby, 2006). Suddaby argues that such work should be rejected from publication at least until the authors clarify their method. Grounded theory and qualitative methods are not the same thing, yet as Wilson (2002, p. 484) observes, there are writers who use the terms as if they were interchangeable when they “ought to know better”. The grounded theory label provides no legitimacy in itself and distracts researchers from being more precise about their methods of data analysis.

Stern demonstrates that this problem arises because of poor mentoring and learning grounded theory from a book. Perhaps, it is time that accounting researchers went back to the tradition of grounded theory, which has been kept alive in the health sciences, and rediscover grounded theory. Locke (2001) notes that most management theorists cite no more than Glaser and Strauss (1967) and Strauss and Corbin (1990) and ignore the rich discourse about grounded theory.

On a more constructive note, an example, although incomplete, of good grounded theory write-up in the accounting literature is Greenhalgh (2000). It is one of the few grounded theory studies in accounting which unambiguously states that data were collected and analysed and a new interview protocol developed. The author uses a form of grounded theory where the researcher explicates their prior biases and knowledge and does not pretend to possess a tabula rasa. It is unusual in that there appear to be only two interviewees and the benefits of theoretical sampling are lost.

A recent addition to the accounting literature (Efferin and Hopper, 2007) provides a possible exemplar. The method is explored in a footnote for the reader who needs stronger confirmation. The authors are honest in admitting that they have not used grounded theory method but have decided to use the techniques of data analysis developed within grounded theory.

Even if all of the problems of the lack of consistency with method were addressed there are still issues relating to grounded theory being the dominant research method for qualitative research in accounting. These are explained in the next section.

4. Issues with the continuing use of the grounded theory approach

Although grounded theory has been put forward as a useful technique for “filling a potential gap between positivist and critical theory research methodologies” (Parker and Roffey, 1997) there are reasons to believe that this is not an all embracing approach for interpretivist researchers. Qualitative research methods abound with approaches to methodology and method that may be much more appropriate for much interpretive research rather than continuing to use grounded theory.

Fundamental to the adoption of grounded theory is the need to match the method with the ontological and epistemological beliefs of the researcher (Annells, 1996). The very nature of the Strauss and Corbin paradigm model may make it attractive

to positivist researchers who are attracted to a highly procedural approach, with little empathy for the philosophical tradition from which it has been derived. Researchers whose background is in phenomenology or ethnography can distort grounded theory and carry over their previous beliefs (Stern, 1994; Benoliel, 1996; Suddaby, 2006).

Grounded theorists have made a point that as the origins of grounded theory are in sociology rather than psychology, the research “questions” addressed by grounded theory are questions of process, and therefore is more appropriate to “how” questions, rather than the social psychological questions of “why”. Like any method, grounded theory needs to match the research question.

Another issue is that grounded theory is more likely to be useful in new situations or gaining a new point of view in familiar settings (Stern, 1980) but may not be applicable to already well-developed areas of knowledge. For example, it is unlikely to be useful in investigating participation in budgeting where the variables are robust and the methodology is well established. It would be more useful for example to explore the nature of the participation process in team-based cultures. Stern (1994) argues that trying to use grounded theory universally adds mediocre research to the literature.

In some cases there may be a problem in the use of grounded theory in accounting because of the use of the idea of an “incident”. This is satisfactory in the development of a grounded theory in the areas of sociology from which it was derived (Glaser and Strauss, 1967) but may not always be useful in the management area where the research may be seeking opinions, attitudes and reflections on a system or direction. There may not be “incidents” to comparatively compare. This is particularly important where the research becomes more longitudinal and reflective, rather than a review of social practice at a single point in time. It is acknowledged that there may be cases where the use of an incident is very important such as the break-off of a merger and acquisition and the flow on to corporate governance or an incident of a financial loss leading to management accounting system changes.

A further concern has been well expressed by Layder (1993) that the beliefs and attitudes of the actors in the organisation cannot be divorced from the social structures and historical forces in which those actors move. Yet the actors cannot be conscious of all of the social forces. The insights of the researcher may “transcend or differ from the insights of the researched” (Llewellyn, 1993, p. 239), because the researcher may draw upon the social in a way in which the researched may not. Layder (1993) attempts a form of grounded theory which more explicitly deals with context, setting and the self than just the situated activity.

The critical researcher will reflect that the grounded theorist is trying to take “a fresh start, open to the emergent” (Glaser, 1992, p. 15), a stance which is impossible. Rather than being open to emergent theory, the researcher may be limited in what they observe by prior theoretical stances and biases. The solution for the grounded theorist is to write a reflexive account, acknowledging as many pre-conceived positions and changes of positions as a result of the research process; for “The purpose of field work is not to strip ourselves of biases, for that is an illusory goal, nor to celebrate those biases . . . but rather to discover and change our biases through interaction with others” (Burawoy, 1991, p. 4). In the end the research is an account of the process under study. Other researchers with other priors will observe, record and analyse quite differently

and therefore their “insights are inevitably subjective because no knowledge is generated distinct from the observer whose reasoning and experiential powers are not uniform or determined” (Laughlin, 1995, p. 71).

5. Conclusion

Over time, grounded theorists in all disciplines have diversified their approaches. The concern of this paper is that accounting researchers appear to continue to use the label grounded theory for research which does not follow the central canons of grounded theory. They are not alone, but whereas in other disciplines there has been warnings sounded to try and reduce this, there appears to have been little action in the accounting literature.

It seems essential that accounting researchers recognise that grounded theory has developed into several different forms. This paper will help accounting researchers clarify which tradition of grounded theory they are working in. However, as a research community we must also be aware of what grounded theory is not (Suddaby, 2006). Whereas other disciplines have allowed for evolution of the grounded theory method, they have not been willing to accept as grounded theory research which is not consistent with the core canons. This does not appear to have been the case in accounting.

Accounting researchers may continue to borrow methodology and method from other fields of social science. But if they wish to transform it or not act consistently with the original method they need to devise new labels. To use the label “grounded theory” to legitimate a variety of approaches to qualitative data analysis is not helpful to developing our own research traditions. Accounting researchers need to both design their research consistent with a research approach and write it up to explicate their methods.

Transparency is a critical criterion for good qualitative research and therefore published papers should contain enough specifics that the reader has confidence in the method. A general allusion to grounded theory is insufficient. Some explication of the method used is necessary. This would include the approach to sampling, to coding and to theory building.

There also needs to be much more consciousness of the inherent weaknesses of the grounded theory approach, and a willingness to be open to attempt to overcome them. Layder (1993) has provided a template for including context, setting and self into the research, as well as situated activity, to provide a grounded theory which implicitly includes history and context. Greenhalgh (2000) has provided an example of being reflective about the inherent biases that the grounded theorist brings to the research. Grounded theory research needs to be written up in a way that the researcher uses their memos to explain how the theory developed in their mind – given that “emergence” is such a difficult idea.

Until accounting researchers build on the foundations of grounded theory and are willing to write up their method, they can hardly expect the community of scholars to take their work seriously. “Make-it-up-as-you-go-along” grounded theory will never advance accounting knowledge, not because the outcomes are not potentially thought provoking and insightful, but because the confusion in method may undermine the credibility of the argument advanced.

The rich insights to be gained from grounded theory take considerable research effort for “... the construction of such theory construction is inevitably much longer

than a more functionalist approach” (Abdul-Rahman and Goddard, 1998, p. 200). Wilson (2002) notes the impact of this on PhD candidates in particular. This effort may constrain the volume of grounded theory research in accounting as pressure increases for greater publications from academics.

Notes

1. Here I follow the definition offered by Llewellyn (1993) that methodology reflects the ontological and epistemological beliefs of the researcher, while method concerns the specific research practice.
2. Two of these papers (Covaleski and Dirsmith, 1986, 1988) are recognised by their authors, Covaleski and Dirsmith, as ethnographic and not grounded theory and one of their grounded theory papers is not cited by Parker and Roffey (Covaleski and Dirsmith, 1984).
3. Stern (1994) suggests that these differences always existed. The partnership of Strauss with Corbin made them all the more apparent.
4. These journals are: *The Accounting Review*; *Journal of Accounting Research*; *Journal of Accounting and Economics*; *Accounting, Organizations and Society*; *Contemporary Accounting Research*; *Auditing: A Journal of Practice & Theory*; *Accounting and Business Research*; *Journal of Business Finance & Accounting*; *Accounting, Auditing & Accountability Journal*; *Journal of Management Accounting Research*; *Management Accounting Research*; *Review of Accounting Studies*; *Critical Perspectives on Accounting*; *Journal of Accounting & Public Policy*; *Journal of Accounting Literature*; *Accounting Business and Financial History*; *Behavioral Research in Accounting*; *Journal of International Financial Management and Accounting*; *Abacus*; *Financial Accountability & Management*.

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Using grounded theory in interpretive management accounting research

Using GT
in IMAR

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Abstract

Purpose – The aim of this paper is to assess and explain the role of grounded theory (GT) in interpretive management accounting research (IMAR) and seeks to answer the question: can interpretive researchers use GT? And if so, how?

Design/methodology/approach – This is a theoretical paper that attempts to investigate how researchers can use GT in relation to their ontological stance, methodological position and research methods.

Findings – The paper suggests that GT offers a balance between the expediency of the research findings, thereby allowing researchers freedom to interpret management accounting practices, and the development of rigorous theory from IMAR.

Research limitations/implications – The paper provides an analysis of GT from an interpretive perspective and, clearly, there are other research perspectives which could have been discussed.

Practical implications – GT can be a powerful tool that researchers could use to collect and analyse empirical data. However, researchers need to align GT with the broader paradigm they adopt when researching social phenomena. The paper provides some general guidelines for IMARs who want to use GT in their research.

Originality/value – This paper shows that GT can offer interpretive researchers a way of balancing the need to develop theory, which is grounded in everyday practices, and the recognition that the research process is inherently subjective. However, it is argued that in interpretive research GT cannot provide a simple “recipe book” which, if followed rigorously, will result in a high-quality research (i.e. valid, reliable and unbiased). Nevertheless, the guidelines provide a way for IMARs, who use GT to improve the quality of their research findings.

Keywords Accounting research, Research methods, Management accounting

Paper type Research paper

Introduction

This paper explores the use of grounded theory (GT) in interpretive management accounting research (IMAR). GT has been used by management accounting researchers

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in various research settings in order to provide insights into the complexities of accounting practices (Parker and Roffey, 1997; Parker, 2001, 2002; Goddard, 2004). A key advantage of using GT was suggested by Goulding (2002, p. 41): “the main thrust of [grounded theory] was to bridge the gap between theoretically ‘uninformed’ empirical research and empirically ‘uninformed’ theory by grounding theory in data”. If GT is used by interpretive researchers it can encourage greater creativity, interaction with data and a strong commitment to theory development from everyday practices. However, Gurd (2008) questions the way management accounting researchers have used the GT to inform their analysis. He expresses dissatisfaction with researchers who seem to use GT simply to legitimate their findings. Nevertheless, as we argue later, we see an important role for GT in IMAR. In this paper, we will discuss how GT can assist IMARs who apply it in the collection and analysis of their data. Also, we will offer some guidance about how GT can be aligned with the essential features of IMAR.

Before discussing the use of GT, it will be important first of all to establish what we understand as interpretive research and IMAR, in particular. Recently, published debates on IMAR have highlighted the potential for further developments in the field and have celebrated its diversity and pluralism (Ahrens, 2008; Ahrens *et al.*, n.d.; Armstrong, n.d.; Kakkuri-Knuuttila *et al.*, 2008b; Parker, n.d.; Scapens, n.d.; Willmott, n.d.). At the same time, these debates have witnessed calls for some integration of the various findings of IMAR to provide a more coherent body of knowledge. In line with this debate, we acknowledge that IMAR is not a homogenous and stable paradigm. Rather, multiple and diverse positions are adopted by IMARs, and many aspects of their claims and findings are quite controversial.

In this paper, we argue that IMARs need to be both faithful to “the data” and open to the complexities of the context. Part of the solution to the problems of using GT in IMAR, as we will discuss later, may be simply to remind ourselves of the essential features of IMAR and to examine the accumulated knowledge in the area. In terms of GT, there appears a preference amongst accounting researchers for Strauss and Corbin’s (1990, 1998) version of the method, as a guide for their data collection and analysis. However, this carries the risk of becoming overly focused on the methods and procedures, which could come to be seen as a sort of “recipe book”, leading to a neglect of the substance of the phenomenon being studied. Seen as an “artefact” from a functional paradigm, these methods and procedures might create a belief that, so long as they are closely followed, reality will eventually be found. Consequently, we may be deceived into assuming that we will reach the all important “saturation” point in our research when we faithfully follow the recipe. Thus, we need to re-examine the suitability of GT for IMAR and to see how we can use the methods of GT to guide our research in a way which is consistent with the underpinnings of the interpretive approach. Specifically, the question we seek to address in this paper is:

RQ1. Can interpretive researchers use GT? And if so, how?

The remainder of the paper is organised as follows: after this introduction, we outline what, for the purpose of this paper, we regard as IMAR. We bring together insights from recent debates on the nature and future of IMAR and discuss some of its central features. In the subsequent section, we focus on the main features of GT and this will enable us to focus on those aspects of the method which make it potentially suitable for IMAR. We also highlight some points of divergence between different approaches to GT,

and indicate their implications for the use of GT in IMAR. This leads onto the following section in which we will explicate the relationship between interpretive research and GT. This section discusses how GT can contribute to theory development in IMAR, and then in the following section we suggest some general guidelines to help IMARs who wish to use GT to inform their data collection and analysis. Our conclusions are presented in the final section of the paper.

IMAR: common features

Recent papers have renewed the debate about the current state and future direction of interpretive research: in particular see the forthcoming papers in “Critical perspectives on accounting” (Ahrens *et al.* (n.d.) and the various associated comments) and the exchange in *Accounting, Organizations and Society* between Kakkuri-Knuuttila *et al.* (2008a, b) and Ahrens (2008). In a comment on Ahrens *et al.* (n.d.) and Scapens (n.d.) acknowledges that there is no clear way forward unless two key concerns are resolved: IMAR needs to have some relevance to practitioners, and to be firmly grounded in theoretical understandings which extend our existing knowledge. Although the various contributors to the debates about IMAR agree that interpretive research covers a vast and diverse range of research, we detect some common features which are useful for the purposes of this paper. Table I sets out these common features and they are then discussed in the remainder of this section. Having identified these common features of IMAR, later in the paper we will explain how GT can be used to inform such research.

IMAR is interested in studying real world practices, decisions and settings, with the objective of analysing, interpreting and understanding them: thereby identifying solutions to pragmatic problems. Its focus is the everyday life of organisations as they exist “on the ground”; rather than exploring abstract problems and providing artificial solutions, “sitting at a distance” and using some remote lens held by a “detached” researcher. As such, IMAR is a part of the naturalistic philosophy of science which aims to study practices as they are, not as they should be (Hopper and Powell, 1985; Kakkuri-Knuuttila *et al.*, 2008b).

In addition, interpretive research is rooted in hermeneutics (Llewellyn, 1993), which emphasises that actors compare, contrast and redefine subjective realities to reach one (or a few) substantial understanding(s) of a (re)constructed reality (Guba, 1990; Klein and Myers, 1999). Thus, it seeks to reconstruct the meanings and interactions between multiple subjectivities/realities. In this context, some management accounting researchers

Common features

Naturalistic	The aim is to study practice as it exists (as is) not as it should be
Hermeneutic	Interpretation and understanding are established by a focus on integrating various perspectives (e.g. individual, social, cultural and political)
Social construction	Understanding of everyday practices – an analysis of human actions and interactions
Eclecticism (or polycentrism)	Reflective use of multiple theories, research methods and disciplines
Explanation	A balance between subjectivity and theoretical relevance (emic and etic perspectives)
Diversity	Focus on “different” contexts, cultures, backgrounds, etc.

Table I.
Common features
of IMAR

(Kakkuri-Knuuttila *et al.*, 2008b) have argued that IMAR should incorporate elements of subjective interpretation (the emic perspective), as well as elements of objective understanding (the etic perspective). Although interpretive research is based on an inductive approach, which takes field data as the starting point for its analysis, its aim is to develop theories of accounting practices (Humphrey and Scapens, 1996). Although IMAR starts from the subjective meanings which actors ascribe to their everyday actions, it is nevertheless not an exclusively subjectivist approach. Interpretive research uses theory to provide explanations of human actions, via logical consistency and agreement with the actors' common sense interpretations (Ryan *et al.*, 2002, p. 42). Thus, IMAR seeks a balance between subjectivity and theoretical relevance (by combining elements of both the emic and etic perspectives).

The primary aim of interpretation is to explore individual and collective experiences in order to develop an holistic understanding of people's actions and interactions in the field. Hence, the aim is not the reconstruction of individual dispositions (as we cannot directly access another person's consciousness). Instead, the aim is the reconstruction of everyday experiences and actions of individuals who are entrenched within socially-patterned temporal practices (Meyer, 2006). Therefore, IMAR can be seen to be seeking to understand, explain and describe a "social reality that is emergent, subjectively created and objectified through human interactions" (Chua, 1986, p. 615). As such, interpretive research is concerned with a world which is socially constructed –, i.e. produced and reproduced through the actions and interactions of the members of that world (Orlikowski and Baroudi, 1991). In addition, IMAR is eclectic, as it draws on various research methods, theoretical frameworks and perspectives to provide better understandings or explanations of the substantive research phenomena. To achieve understanding, interpretive researchers study diversity –, i.e. they seek to build and extend knowledge by breaking away from traditional settings, and providing insights into "different" contexts, cultures and backgrounds; thereby producing knowledge in novel ways.

IMAR provides rich explanations of the changes in management processes, as well as comprehensive, contextually rooted interpretations of their interplay in wider contexts. In the forthcoming debate in CPA (mentioned earlier) Mennicken, for example, argued that IMAR needs to integrate the isolated "local" research findings by looking for links between studies which deal with similar issues in different contexts and from different perspectives (Ahrens *et al.*, n.d.). IMAR will only be able to achieve this aim if it can make cases "talk to each other" (Lukka and Kasanen, 1995), and theories will only be useful insofar as they can integrate findings and accumulate knowledge. However, such a view of theory development in IMAR does not mean that we have to "abandon reflexivity and vagueness as a research strategy" (see comments by Hansen and Grunlund in Ahrens *et al.*, n.d.). Thus, IMAR's contribution is in developing theories that listen to practitioners' voices and talk back to them (Scapens, n.d.). In addition, Ahrens and Chapman (2006) argue that there needs to be an ongoing reflection of data against different theories, with the aim of developing a fuller understanding (interpretation) of the phenomenon under study.

Although, we would like to see a greater integration of the individual research efforts of interpretive researchers, we do not see this as contradicting the notions of "eclecticism" and "diversity", which we referred to above[1]. On the one hand, eclecticism allows researchers the flexibility to seek understandings from the field by listening to multiple voices (including those of previous researchers or other cases).

On the other hand, theory is developed to enhance our understanding of practice, again by listening to these multiple voices – both in the literature and in practice. The aim of theorising should be to enhance our understanding of practices, rather than to “prove” some hypotheses derived from existing theories. Such an aim should encourage interpretative management accounting researchers to accumulate their findings and to produce a coherent body of theoretical knowledge that can advance understandings of practice and provide a basis for future studies.

To summarise, we would argue that although IMAR is not a homogenous paradigm, it collectively recognises that accounting comprises social actions and interactions and it understands the importance of the various voices (and multiple perspectives) in the field. As GT is an inductive approach, it has the potential to help interpretive researchers to develop theories of everyday management accounting practices. However, some writers, such as Goulding (1998), might argue that GT it is not appropriate for interpretive research as its language, use of coding and verification procedures seem to derive from a rather functionalist perspective. Whilst we acknowledge that there is a risk that GT may be seen as overly functionalist, we believe that it can be quite appropriate for IMAR provided we keep in mind the essential nature of interpretive research – and especially the common features of IMAR set out in Table I. In order to provide some guidance about how GT can be used appropriately in IMAR, the next section will discuss the main features of GT (as a research method[2]) and relate them to the common features of IMAR discussed above.

GT Approach

Main features

GT is a research method which seeks to generate theory from data that are systematically obtained and analysed. It has been defined in its most general form as “the discovery of theory from data” (Glaser and Strauss, 1967, p. 1). GT is consistent with IMAR in its emphasis on developing theory from data, the importance given to “local voices”, and its emphasis on explaining interactions between participants in the field. Over the years since its inception, GT has developed into two rather distinct approaches (Heath and Cowley, 2003). Goulding (1998, p. 52) observed that some GT researchers believe that Strauss has adapted his version of GT from the original concept of theoretical emergence and turned it into a densely codified set of procedures. For Glaser (GT’s co-creator), Strauss’s approach represents an “erosion” of what GT originally stood for and is responsible for the impression that GT uses a functionalist approach (Stern, 1994). However, from a social constructionist perspective, the use of GT involves a dialectical process and the outcome is “a social construction of the social constructions found and explicated in the data” (Charmaz, 2006, p. 1165).

Initially, GT was developed as a response to the highly abstract theorisations which were being used in sociological research at the time, and it can be seen as an attempt to encourage the growth of qualitative sociological research. It starts with a low level of prior theorisation and works through a highly structured approach to collect and analyse field data[3]. Despite its original creators now advocating rather different approaches, there is some common ground in what is generally known as GT; see Table II for the main features which comprise this common ground. For clarity of discussion, and due to limited space, our aim is not to list all of the features of GT, but to focus on those which characterise GT as a research method. As we are primarily

Main features	Explanation
<i>Data collection</i>	
Iterative process of data collection and analysis	In practice, data collection and analysis should be interlinked. Data are first collected and analysed, and then this should lead to further data collection and analysis; and so on until the research is complete and a theoretical understanding is reached. Glaser and Strauss (1967, p. 71) explain as follows: "Research aimed at discovering theory . . . requires that all three procedures (data collection, coding and analysis) go on simultaneously to the fullest extent possible"
Constant comparative method	Glaser sees this in terms of comparing differences and similarities so as to integrate categories and their properties. Strauss and Corbin (1990, p. 67) explain it as the "identification of variations in the patterns to be found in the data"
Theoretical sampling	Emerging theory dictates where the research will go next to collect data. Grounded theorists often seek disconfirming cases which could contradict parts of the emerging theory and hence enrich theory development
Processual	Longitudinal studies are important for GT so that the researcher can follow the unfolding events over a relatively long period of time and thereby gain an understanding of the phenomena being studied. The rich insights that can be gained from GT require considerable research effort, and theory construction tends to take longer than in a more functionalist approach
<i>Data analysis and interpretation</i>	
Coding	Coding the data is the fundamental analytic tool of GT. It is used to uncover the emerging theory from the field. However, the linkage between coding and theory development is an area of difference between the Glaserian and Straussian approaches, as will be discussed in the next sub-section (Gurd, 2008)
Identifying the core categories and theoretical saturation	Through the process of selective coding researchers can reconstruct the participants' stories and give them a voice "albeit in the context of their [the researchers'] own inevitable interpretations" (Strauss and Corbin, 1998, p. 281). However, this is another area where there are differences between the Glaserian and Straussian approaches. Glaser and Strauss approaches – as will be discussed below
Inductive theory development	With the focus on social interaction, theory is grounded in data obtained from interviews and observations, rather than by testing existing theory or simply describing the empirical phenomena. GT studies actors "in their normal everyday world, recognizing that they subjectively construct their own organizational realities . . . with an objective of developing rich descriptions and insights . . . and that have been observed in their naturally occurring context" (Parker, 2001, p. 323)
Theoretical sensitivity	According to Strauss and Corbin " <i>Theorising</i> is the act of <i>constructing</i> . . . from data an explanatory scheme that systematically integrates various concepts through statements of relationship" (1998, p. 25; emphasis in original), and theories themselves are "interpretations made from given perspectives as adopted or researched by researchers" (1998, p. 279). However, as will be discussed later, Glaser argues that researchers must enter the field with a "blank slate", whereas Strauss sees a role for prior theory
Memoing and diagramming	Strauss and Corbin (1998) suggest complex coding methods, including the use of memos and diagrams. However, they argue that to increase theoretical sensitivity these coding methods need to be used with a degree of flexibility and creativity (Mills <i>et al.</i> , 2006)
The measure of rigour	Reflexivity is emphasised in an inductive-deductive cycle of theory generation. However, many GT studies report rather loose collections of inductively generated insights which cannot be justified by any notion of rigour or evidence (Gurd, 2008). Rigour could be improved by requiring the researchers to explain their coding processes, theorisation, and conclusions (McCann and Clark, 2003)

Table II.
Main features of the GT

concerned in this paper with the use of GT as a research method, we will focus specifically on Strauss and Corbin's (1990, 1998) version of GT. This version of GT has been the favoured approach of researchers who have used GT in management accounting research in recent years (see Gurd's (2008) review). In subsequent sections references to GT will normally mean the Strauss and Corbin version, unless otherwise indicated. After a brief explanation (below) of the common ground as set out in Table II, we will describe (in the next sub-section) three significant differences between Glaser's and Strauss' approaches to GT, which have important implications for IMAR.

The main features set out in Table II characterise GT as an inductive, iterative, comparative and systematic method for data collection and analysis. GT starts with empirical data and, using a set of coding procedures, inductively develops theory to explain the data collected. In practice, researchers must be open to various perspectives (and voices) from the field and "go-back-and-forth" between their "theorising" and their data collection. As such, theorising in GT is an iterative process through which theories are developed by deriving propositions from the data, and then confronting these propositions with further data, leading to revised and/or new propositions, and then further data collection; and so on. The aim is not to "test" the emerging propositions, but to be open to new avenues and to be prepared for "surprises" in the field. In addition, theorising can be extended by collecting and comparing data from other contexts, settings and/or existing research (where available).

What distinguishes GT from other research methods is the systematic process for data collection and analysis; starting with data and progressively transforming it into refined theoretical concepts through three (main) processes of coding: open, axial and selective (Strauss and Corbin, 1998). As we will discuss below, some of these features make GT quite suitable for IMAR. However, IMARs need to be aware of the different approaches to GT, as misunderstandings can create potential dangers which could breach the core principles of IMAR (discussed above). These differences are discussed in the following section.

Different approaches to GT

In this section, we identify three differences between the Glaserian and Straussian versions of GT, insofar as they are important for understanding how GT can be used in IMAR. The basic argument is that IMARs need to be aware of the nature of the particular version of GT that they are using to build their grounded theories and to understand/interpret the field. The first difference relates to the approach taken to the generation of core research issues and to the emphasis given to theory induction versus theory verification. In other words, there is a difference between whether the core issues in the research become "visible" through the observations in the field or through the detailed process of coding leading to "theoretical saturation".

On the one hand, Glaser (1992) places great emphasis on the emergent nature of theory through the process of induction:

[...] [Through] researcher's knowledge, understanding and skill, which foster generation of categories [...] to relate them to hypotheses [taken to mean probability statements], and to further integrate the hypotheses [...] [However] grounded theory is not verificational [...] hypotheses need not be verified, validated or be more reliable.

To Glaser, theoretical saturation refers to a purely inductive (emergent) process; which should lead only to theory and not its verification (Corbin, 1998). On the other hand,

Strauss (1987) believes that a systematic process of theoretical development, through a rigorous coding process, can enable the researcher to verify the emerging theory and to conceptualise beyond the immediate field of study (Goulding, 2002). As interpretive researchers are seeking to develop theories of management accounting practice, the issues of theoretical sensitivity and saturation are clearly important. Consequently, interpretive researchers who use GT need to be clear in their understanding and description of the approach to GT that they are using to inform their data collection and analysis, and thereby to develop their theories. In a critical comment on Strauss and Corbin (1990) and Glaser (1992, pp. 2-3, emphasis added) wrote:

[...] piling up tons of fractured rules instead of cutting directly through to basic and underlying fundamental relevance [...] it is a logic that thwarts and frustrates the *discovery* of what is truly going on in the substantive area under study, and undermines grounded theory at every turn by preconceived *forcing* of the data.

As GT guides the process of data analysis, it should encourage a dialogue between the researcher and the data. From an interpretive perspective, a narrow notion of verification could encourage an undue focus on the “process” of theory development and an attempt to simply “tick the boxes” –, i.e. follow the method. Instead, the researcher needs to give careful consideration to, and to justify, the selection specific coding procedures. Simply following the prescribed method is likely to be seen as adopting a more functionalist approach and would be inconsistent with the key principles of IMAR discussed above. In IMAR there needs to be a careful justification of how the researcher has made sense of the data and how he/she has been able to understand what is going on in the field. As such, IMAR should be a reflective/reflexive exercise (Alvesson and Sköldbberg, 2000; Covaleski and Dirsmith, 1990; Quattrone, 2004), not a process of verification through the use of a defined set of procedures. For example, Covaleski and Dirsmith (1990) suggest that in developing their emergent theories of accounting, researchers should be constantly aware of their own assumptions and preconceptions. There should be a process of continuous questioning to avoid biases which could result in “channelling and directing research attention and creating or altering that which is observed” (p. 550). This need for reflexivity and reflection is one of the guidelines we propose later in the paper.

The second difference in the two approaches to GT relates to the use of the existing literature to guide the process of data collection and analysis. Whereas Glaser believes that the researcher should not review the literature prior to conducting the empirical part of the study, Strauss is more open about the use of existing literature. Glaser (1992, pp. 25-31) argues that:

[...] in GT there is no preconception of being too broad or global or too narrow at whatever stage [...] the emerging questions simply tap the variables that work whatever the field [...] in contrast the dictum in grounded theory research is: there is a need not to review any of the literature in the substantive area under study.

This “blank-slate” approach to data collection is intended to avoid the research being clouded by sub-conscious preconceptions about the field, unrecognised assumptions, and/or bias in interpreting the data. Strauss and Corbin (1998), however, acknowledge that the researcher is bound to be influenced by prior training, education, preferences, interests, etc. and that they can all be used to guide the research process and to focus on potentially relevant phenomena. So, for Strauss and Corbin the literature can be helpful

in various ways; such as opening up avenues for investigation, acting as another (secondary) source of data, and validating the observed findings. They argue that:

[...] We are asking researchers to set aside their knowledge and experience to form new interpretations about phenomena. Yet, in our everyday lives, we rely on knowledge and experience to provide the means for helping to understand [...]. [R]esearchers have learned that a state of complete objectivity is impossible and that in every piece of research there is an element of subjectivity (1998, p. 43).

At the core of both arguments is the basis of “understanding”. Whereas Strauss and Corbin see a role for the existing literature in the process of understanding the collected data; Glaser’s approach seeks to achieve understanding by focusing entirely on the observed practices of the participants and their interpretations of those practices (Suddaby, 2006). On the one hand, with its emphasis on emergent inductive theory, Glaser’s approach is one in which the researcher attempts to understand a particular phenomenon through the eyes and minds of the actors being researched, and the focus is on the subjectivity of the interpretation. On the other hand, Strauss and Corbin’s (1998) argument broadens the evidence that a researcher can use to understand the area being researched. Here, the researcher interprets and theorises based on continuous readings of the literature. That said, it should be noted that in both versions of GT, the use of prior literature is intended to illuminate the data collected and to add theoretical richness, rather than to impose a limited and narrow way of viewing data from the field. In addition, by consulting the existing literature, before entering the field, researchers can avoid “re-inventing the wheel” (Alvesson and Sköldbberg, 2000). As Parker and Roffey (1997, p. 224) indicate:

A grounded theory researcher’s decision to select a particular research project reflects the individual’s perspective on research, but the researcher should make strenuous efforts to avoid superimposing pre-existing theories on the data.

The third difference between the two approaches relates to whether GT is a research method or a methodology. In their definition of GT, Strauss and Corbin (1990, p. 24) explicitly describe it as “a qualitative research *method* that uses a systematic set of *procedures* to develop and inductively derive grounded theory about a phenomenon” (emphasis added). In contrast, Glaser (1992, p. 16) defines GT as “a general *methodology* of analysis linked with data collection and uses a systematically applied set of *methods* to generate an inductive theory about a substantive area” (emphasis added). Here, it is essential to distinguish between methodology and methods, and we share Ahrens and Chapman’s (2006, p. 822) view that:

The conflation of method with methodology means that ontological assumptions remain unrecognised as assumptions. We see the distinction between method and methodology and the theoretical potential that it affords for defining research questions and notions of research trustworthiness as central to much of the miscommunication between qualitative and positivistic researchers.

Methodology concerns the “set of spectacles” that determine the type of methods used for investigating the world (Laughlin, 1995); whereas methods are the specific techniques used to collect and/or analyse data. Treating GT as a methodology implies that it is a general philosophy about doing research, coupled with a set of methods which are fundamentally influenced by its ontological and epistemological assumptions.

Putting it another way, GT as a methodology transcends a simple categorisation of methods, and involves deeper assumptions about the philosophical basis of doing research. We agree with Strauss and Corbin's definition of GT as a research method, i.e. a technique that a researcher can follow in order to collect and analyse (qualitative and quantitative) data. However, the problem of confusing GT as a methodology and GT as a method is that it can limit attention to the procedures (i.e. method), rather than exploring the philosophical basis of the research (i.e. methodology). As a result, there is a danger that the focus of the researcher could be on how to verify the emerging codes, rather than on how to understand the nature of the phenomenon being studied. Therefore, interpretive researchers drawing on GT to guide their data collection and analysis should be consciously aware of the basic principles of their research approach, which we discussed above.

As a research method, GT can potentially be used in different methodologies, but the researcher needs to consider carefully how GT fits the underpinning ontological and epistemological assumptions. Seeing GT as a methodology, however, raises questions about the researcher's ontological and epistemological assumptions. Here, the researcher needs to use method(s) that is (are) consistent with the ontology/epistemology of GT. In interpretive research, GT (as a method) can be used to guide data collection and analysis. However, by definition, such research adopts an interpretive methodology and thus GT is used to identify subjective understandings, meanings, perceptions, behaviours, etc. of the participants (i.e. an emic analysis), as well as developing broader theory of management accounting practices (i.e. an etic analysis). In functionalist research, where reality is taken for granted as objective and independent of the researcher and the researched, GT as a method could be appealing as a seemingly rigorous way of collecting "objective data". In this sense, the danger is that GT is used simply as a set of objective procedures.

In summary, this section has discussed the key features of GT and the main differences between the two approaches. The aim now is to assess how well GT fits with IMAR. In the next section, we will integrate the main features of GT with the common features of IMAR in order to explore the potential of GT in IMAR. More importantly, this discussion will enable us to derive some general guidelines for using GT to inform IMAR.

GT in IMAR – opportunities and obstacles

Debates on how social reality emerges through subjective understandings and come to be objectified through interaction lie at the heart of IMAR (Ahrens, 2008). In addition, interpretive researchers play an active role in their interpretation of social phenomena. To explore areas where GT (as a research method) can contribute to (or can be used to produce) "good" interpretive research, this section brings together the features of GT (Table II) and IMAR (Table I). This discussion will then be followed by an analysis of three obstacles that we believe could potentially reduce the GT's contribution to IMAR. As indicted earlier, here GT will refer to the research method provided by the Strauss and Corbin approach.

In Table III, we outline the areas of common ground (or fit) between GT and IMAR. As can be seen in the table, GT fits with the naturalistic and hermeneutic principles of IMAR. It emphasises inductive theory development, involves processual studies, and takes multiple perspectives into account. It starts with data from the field and attempts

Features of IMAR	Corresponding GT features	The fit
Naturalistic	Inductive, processual, comparative	A focus on investigating real world phenomena from the viewpoints of the participants
Hermeneutic	Inductive, theoretical sensitivity, comparative, coding, core categories and saturation	The aim is to develop theoretical understandings of social phenomena by integrating the various perspectives of participants in the field
Social construction	Comparative, processual, theoretical sensitivity	Understanding of social reality is constructed by actors in the field and “tested” against existing knowledge
Diversity	Theoretical sampling, comparative, coding, core categories and saturation.	Importance of investigating different settings to create dense theory of the social practices
Eclecticism	Theoretical sampling, theoretical sensitivity, comparative, memoing and diagramming, rigour	GT draws on a wide range of perspectives, theories, and methods. As a research method, GT involves elements of induction (subjectivity), deduction (coding), and verification (comparative method)
Explanation	Inductive, theoretical sampling, theoretical sensitivity, core categories and saturation, memoing and diagramming, rigour	The aim is explanation which is achieved by reorganising empirical data to build an inductive theory that can explain everyday practices

Table III.
Comparison of the key features of IMAR and GT

to make sense of it by integrating various categories, perspectives, theories, and through the coding procedures derives core conceptual constructs which provide the key to understanding the area under investigation. These features embody both the emic and the etic perspectives. Furthermore, GT involves both induction (which relates to the way data is collected) and interpretation (which relates to how the data are understood). GT also includes an element of deduction, as the coding processes leads to theoretical understanding grounded in context.

As Meyer (2006) indicates, interpretation proceeds in two stages; first a “reconstructive stage” where the researcher reconstructs the world of the social actors, and second a “replicative stage” where the researcher explains that world using methods which follow rules to ensure the explanation is valid. GT fits the principle of social construction in IMAR by seeking to understand the world from the viewpoints of the individual actors. In order to make sense of the phenomenon being studied, GT can be used in different settings in order to examine the findings, challenge propositions and confirm the emerging theory. In this process, different theories and perspectives can be used to make sense of the world, and as a result GT fits the principles of eclecticism and diversity in IMAR.

As GT is used to build theory, it clearly fits the essential aim of IMAR, which is to produce theories of management accounting practice. However, as we argue later, the analysis stage of GT carries a risk that it may appear rather functionalist, especially if

the adherence to the method and procedures takes precedence over the basic principles of interpretive research. On one hand, Glaser (1992) stresses the subjective nature of the emerging (grounded) theory. On the other hand, Strauss and Corbin (1998) suggest a more systematic approach to the development of (grounded) theory. It might be argued that such a systematic approach should improve the analysis by highlighting the key themes, incidents and patterns in the field. If researchers see “following the procedures” as the guarantee of a valid analysis without making a reflexive interpretation of the data their first priority, the credibility of the research findings are likely to be adversely affected. However, Strauss and Corbin (1990, p. 19) were quite aware of this problem:

If the researcher simply follows the grounded theory procedures/canons without imagination or insight into what the data are reflecting – because he or she fails to see what they really indicate except in terms of trivial or well known phenomena – then the published findings fail on this criterion. Because there is interplay between researcher and data, no method, certainly not grounded theory, can ensure that the interplay will be creative.

Nevertheless, it might be tempting to use GT as a “recipe book”; assuming that adherence to the procedures will lead to well-developed theories. From an interpretive point of view, there is considerable danger in applying such “pseudo scientific” rationality in applying GT. For example, the researcher’s decision about whether saturation has been achieved is a crucial one. Suddaby (2006, p. 639) indicates that identifying when the point of saturation has been reached is often very difficult and requires a great deal of experience and tacit understanding on the part of the researcher. He points out that as GT research uses iteration and sets no explicit boundaries between data collection and analysis, saturation is not always obvious, even to experienced researchers. Also, Walsham (2006, p. 326) argues:

[...] it is essential that researchers are not misled to confuse process with outcome. So it is insufficient to say that I have applied the principles. It is essential to say here are my interesting results.

The above discussion shows that there is much to be gained by using GT in IMAR, but there are also potential dangers (or obstacles). These are summarised below:

- *Premature saturation* (too early closure) – saturation is the point at which there are no new concepts, categories, relationships, etc. emerging from the analysis (Strauss and Corbin, 1998). It is important not to rush to conclusions based on incomplete data collection and/or analysis. This could lead to artificial findings, insignificant (inconsequential) conclusions, and/or superficial theory. IMARs need to be open to alternative views and multiple perspectives, and to continue probing and questioning until no new evidence can be found.
- *Using a coding “recipe book”* – functional (or mechanical) application of GT’s methods and procedures. The role of any method (including GT) in IMAR is to assist researchers to understand/interpret the world. An emphasis on following the “recipe book” may be accepted (and indeed necessary) in functionalist research, but interpretive researchers have to try to understand the field from the perspective of the social actors, and then to theorise about it. In practice, interpretive researchers should use the methods and procedures of GT, as well as the emerging codes, in a flexible and creative way.

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- *Use of literature* (tunnel vision: see Suddaby, 2006) – too much influence from the literature can prevent new understandings emerging from the field. A novice researcher might be tempted to start the research by trying to confirm existing theory instead of discovering new knowledge (Heath and Cowley, 2003). One of the key advantages of interpretive research lies in its investigation of real world problems and its search for new solutions to these problems. This is achieved by listening to the multiple voices in the data, rather than searching for abstract ways (or universal laws) to generate conclusions. In other words, the literature should inform rather than prescribe how the researcher interacts with the field.

This section has argued that there is a case for using GT in IMAR. Interpretive researchers can make use of GT to inform their research, but they must also be aware of the potential dangers. One of the main advantages of GT is that it enables researchers to study actors in their everyday world. GT can help interpretive researchers to produce interpretations which are grounded in the data, and to bring together evidence collected from various settings. GT provides an iterative process which focuses attention on key issues and, potentially, facilitates the development of theoretical explanations of social phenomena. Because of its inductive approach to theory generation, GT offers a useful tool which guides the systematic collection and analysis of data, and assists in developing theories which are grounded in the data. A major difference between GT and other qualitative research methods is its concentration on theory development. GT aims to produce theory that is “conceptually dense” (Strauss and Corbin, 1998), and which offers a rich conceptualisation, as opposed to mere description. To exploit the potential of GT in IMAR, in the next section we suggest some guidelines to help IMARs who want to use GT.

Some general guidelines for using GT in IMAR

Based on the above discussion, we suggest the following guidelines for interpretive researchers who use GT:

- *Subjectivity of interpretation.* Attempt to understand the phenomenon in the terms that the actors in the field use to give meaning to it. Rather than testing “scientific” hypotheses, the researcher should become immersed in the field, working closely with the empirical data. When using GT, there should be a dialogue between a researcher and the voices in the data. Interpretation is a reflective/reflexive exercise, rather than a process of following established procedures. Theory is derived from data in a subjective process of construction, through which the researcher develops understandings which are firmly grounded in the data, and not simply through the imposition of explanations (based on theories) drawn from outside. This is a hermeneutic and dialectical approach, in which the researcher goes back and forth between data and interpretation, as well as using existing theories to make sense of such data.
- *Emergence.* GT is method designed to allow theory to emerge. It aims to establish theory which is useful in explaining the observed data. A key departure from a more functional approach is that there should be a continuing search for evidence which contradicts or disconfirms the emerging theory[4]. In addition, the theory must reflect a detailed awareness of the (local) context. The key notion is that researchers need to stay as close as possible to the field in order to

appreciate local settings and to be able to develop theories that reflect the local contexts. Finally, the researcher should follow the unfolding events over time in order to identify the linkages between events and outcomes. As such, there will usually be a longitudinal element in the use of GT in IMAR, both in the data collection and in the analysis.

- *Questioning.* In the course of building GT the researcher gains new knowledge, confronts self-biases, and modifies what he/she has acquired from existing theory. This is achieved by openness to the field, sensitivity to data and a willingness to modify initial preconceptions, assumptions, and interpretations as new evidence is collected. The reflexivity inherent in this process should be explicitly explained and illustrated in writing-up the research. This will increase the trustworthiness of the findings, by demonstrating that care has been taken in substantiating evidence, and thereby add credibility to the conclusions (Baxter and Chua, 2008).
- *Theory-building approach.* The aim of using GT in IMAR (or elsewhere) is to develop new theory. GT offers great opportunities for researchers to investigate the unknown, to improve their understanding and to contribute to existing knowledge. In areas where there has been little research, researchers have to start from the data, and GT offers them a way of gaining useful insights which can be extended to wider contexts. However, there may be less to be achieved in using GT in more well established areas where there is general agreement in the literature, and where existing knowledge could potentially bias the findings.

Conclusions

The aim of this paper has been to improve our understanding of whether interpretive researchers can use GT and if so, how. In response, we have explored the role of GT to IMAR and suggested some general guidelines to help interpretive researchers who want to use GT in IMAR. Part of the contribution of this paper, as we discussed in the introduction, has been to remind ourselves of the essential features of IMAR and the need to examine the accumulated knowledge in the area. By examining the underpinnings of GT and IMAR we have been able to consider the “fit”, as well as identifying some obstacles. In response to the critique of Gurd (2008), we have argued that GT can offer a valuable tool for interpretive researchers. Gurd criticises much of the prior GT research for a less than strict adherence to the principles of GT. However, as we have argued, using GT in a mechanical manner, i.e. as “recipe book”, could also represent a risk for IMAR.

From an interpretive perspective, there is danger in simply adopting a “pseudo-scientific” rationality in applying GT. From a functionalist perspective it might be acceptable to use GT as “recipe book” in order to validate research findings. But as we discussed, the mere adherence to GT’s procedures of data collection and analysis will not of itself guarantee valid results; this requires reflexivity on the part of the researcher. Interpretive research is a naturalistic endeavour, which seeks to understand everyday practices in their natural settings. It draws on and develops theory to explain observed phenomena, and to contribute back to the practice that it studies, as well as building on existing knowledge (Scapens, n.d.). In this respect, a key advantage of using GT in IMAR is that it offers a middle-way between empirically uninformed or abstract research (where researchers develop grand theories to make predictions about a supposedly objective reality) and theoretically uninformed practical research (that can result in trivial findings which cannot be extended elsewhere).

In IMAR, research using GT involves a dialogue between the researcher and the data, and as such it can encourage creativity, immersion in the data, and sensitivity to different perspectives. This means that researchers must clearly explain to the readers of their research papers how they acquired their data and how they reflected on their research findings. They also need to explain the processes used to analyse the data so as to convince the reader that their theorisations of the phenomena under study are credible. Thus, in IMAR, GT must be much more than a means of verifying propositions through the simple adherence to a set of procedures. Instead, in using GT the researcher must be self-reflexive and able to reflect on his/her assumptions and preconceptions on entering the field (including existing knowledge). In other words, the researcher must show a commitment to the data, act reflexively, and question what might otherwise be taken for granted.

Finally, we would encourage interpretive researcher to consider and experiment with using GT, in any of its alternative forms. In so doing they will, as we have outlined in this paper, need to give careful consideration to how their methodological assumptions relate to the research method(s) they choose.

Notes

1. We thank one of the reviewers for helping us to develop this argument.
2. Methods are specific techniques used to collect and/or analyse data. We discuss in later sections how treating GT as a methodology implies a general philosophy about doing research, coupled with a set of methods which are fundamentally influenced by its ontological and epistemological assumptions.
3. In this sense it corresponds to Laughlin's (1995) low/medium category of theorising (Llewellyn, 2003).
4. This runs counter to current practice in quantitative research where editors seem to accept only papers in which hypotheses have been supported, rather than where hypotheses have been rejected. We thank one of the reviewers for suggesting this point.

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Qualitative Research in Accounting & Management

Special issue on

Interventionist research

Guest Editors: Hanno Roberts, Norwegian School of Management BI, Norway, and Olle Westin, Orebro University, Sweden

Interventionist research is part of a family of methodological approaches to doing field research in accounting and management. Its distinguishing characteristic is that the researcher is directly involved in the real-time flow of events instead of observing at a distance or working with *ex post* facts. Interventionist research is grounded in action when refining, testing, illustrating or constructing theory (Jönsson and Lukka, 2005). It aims to uncover the “theory-in-use” rather than the “espoused theory” (Argyris *et al.*, 1985) held by the subjects and, as such, is part of the larger family of qualitative research methods.

As the name indicates, interventionist research means that the researcher directly intervenes in practice, usually as part of a mixed practitioner-academic team effort to change a current situation or solve a problem. Interventionist research implies an active engagement with reasoning according to local logic and practice rather than from a merely conceptual perspective. However, the practical involvement needs to lead to theoretical contributions in order to qualify as research instead of consulting.

Interventionist research tends to come in two flavours; weak intervention and strong intervention. Weak intervention involves a low level of participation by the researcher, for example, through questioning and dialogue with the organization’s members. Strong intervention involves a high level of participation, for example, through proposing alternative solutions and approaches to the organization’s members. In both cases, the interventionist researcher is part of a joint

team. As such, strong interventionist research can be considered alongside approaches such as action research, clinical research, constructive research and design-based research.

The purpose of this Special Issue is to provide a forum for the discussion of interventionist research in accounting and management. It aims to deepen existing theoretical insights, provide further empirical evidence of the (relatively scarce) use of interventionist research, and initiate the formulation of specific approaches and methods related to the use of interventionist research.

Topics of interest for the Special Issue include but are not limited to:

- **Deepening the epistemological foundations of interventionist research as a research methodology**
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- **Case-based descriptions of various forms of doing interventionist research in terms of methods and approaches used**
- **The combination of interventionist research with other qualitative and quantitative research designs and methods**
- **Personal reflections and (hi)stories of conducting interventionist research**



Call for papers

- **Specific approaches to identifying, formulating, and extracting theoretical contributions from the intervention process, including the subsequent communication of findings and contributions**
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Interventionist research is not limited to any particular area of accounting or management, or to any particular setting such as the public, private or not-for-profit sectors. Rather, as a research methodology, it can be of equal use to refine, test or construct theoretical contributions around introducing IT systems, improving performance measurement, integrating internal or external reporting, effecting organizational change, learning, educating organizational members, designing collective decision-making and participative leadership styles etc.

We particularly encourage submissions to this Special Issue that take a multidisciplinary approach, for example, by involving authors from different disciplines or by using a framework derived from other social sciences such as health care, design engineering, pedagogics or sociology. The fact that interventionist research is part of a larger family of action-oriented research methodologies allows for cross-pollination in terms of arguments, specific approaches, and

application contexts; all in order to avoid reinventing the wheel when it comes to the field of accounting and management.

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Qualitative Research in Accounting & Management

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