

“[T]here are known knowns ... things we know that we know”

Some reflections on the nature and practice of interpretive accounting research

[T]here are known knowns; there are things we know that we know.

There are known unknowns; that is to say there are things that, we now know we don't know.

But there are also unknown unknowns – there are things we do not know we don't know.

(United States Secretary of Defense, Donald Rumsfeld, 2002)

1. Introduction

One of the authors of this article previously worked at a university where he was told by a colleague, that if he really wanted changes to happen in his department, he should first talk to the dean to introduce his ideas and then to his deputy if he also wanted to obtain support from the university board. Although the dean was officially the person who should contact and negotiate with the board about such issues, and also saw himself as the one in charge of departmental changes, he apparently was not as well respected by the board as his deputy in regard to the facilitation of significant organizational change.

Now suppose that this university were included in a research project, and both the dean and the deputy would be interviewed by a researcher about recent significant departmental change. It would not be unlikely (but of course far from certain) for both the dean and the deputy to tell the interviewer(s) that the dean implemented departmental change on his own without consulting colleagues (Alvesson, 2003, 2010; De Loo and Lowe, 2012).

We claim that this brief vignette, set in a university department, illustrates that *any* research situation may be far from a ‘normal’ situation in which everyone is inclined to provide ‘actual’ or ‘factual’ information. In interpretive research, such as in accounting, the use of conversations, narratives and stories from ‘the field’ often provides the objects around which representations occur (Ahrens and Chapman, 2006). Yet it is the framing of these objects that highlight a major issue of concern for researchers. Putnam (1978), for instance, argues that the objects of interest within the social sciences are a ‘structured mess’ around which multiple realities and constructions can be attached (see also Herbst, 1970). Sayer (1992), however, conceives this problem differently,

suggesting that it is the "... internality of social science to its object which makes the latter susceptible to change by the former" (p. 234). He continues this debate, arguing that conventionally, the "... basic aims of social science are taken for granted as the development of a 'scientific' objective, propositional knowledge which provides a coherent description and explanation of the way the social world is" (p. 233). These perspectives are not easily reconciled.

When conducting interpretive research in accounting, Boland and Pondy (1983) note the central role of focusing "... on action in organizational settings ...", the importance of accepting that the "... perspectives of interest are those of the individual actors", and that such "... research must be interpretive and recognise the symbolic use of accounting in ordering and giving meaning to the individual's experience" (p. 226). The authors further argue that the "... researcher must step out of the actor's frame of reference (...) [in order that] the actor's purely subjective interpretation ... be transcended" (p. 227; see also Llewellyn, 1993; Silverman, 1998) in the analysis and writing up of the research. What this latter suggestion means exactly, and how this is to be done, remains something of a mystery¹. We claim, following Lillis (2008), that this is often taken for granted or left unexplained when research is conducted and discussed (see also Ahrens and Chapman, 2006; Parker, 2012; Vaivio, 2008).

The intention of this paper is to reinforce the crucial link between the making of research stories and the interpretation that is necessarily involved during their making (and also during their reading²). We claim that in the social sciences, research generally tries to develop understandings about (groups of) subjects and the environments (containing other subjects and objects) in which they live, engage, and evolve (see also Ahrens, 2008). We argue that it needs to be recognized that methodology, the orientation of the research, and (especially) the researcher are always intimately bound up with method and the research process, and that these elements cannot meaningfully be studied in isolation (Chua, 1986). Although this may seem straightforward, we believe that these issues are often overlooked when researchers engage in what has become classified as 'interpretive accounting research' (IAR). We conjecture that this may have happened because considering such issues could make the research appear insufficiently rigorous (see also Lillis, 2008; Parker, 2012;

¹ Although in the next section, we will see that there are links with interpretive sociology that may be explored. However, we will also show that we find some of these links arguable at best.

² The latter will, however, not be the focus of this paper, as this would, we believe, require a paper of its own.

Silverman, 1997). In this paper, we claim that such considerations are unwarranted and potentially misleading, as they are not aligned with what such research can achieve (Armstrong, 2008; Deetz, 1996).

We are especially concerned about two central aspects of the IAR debate and how interpretive research has been portrayed in some recent prominent contributions to the accounting literature:

1. The idea that IAR must produce a coherent body of research (Ahrens *et al.*, 2008, Lukka and Modell, 2010). We assess the negative and improbable nature of creating such a body of knowledge. In contrast, we will indicate why adopting a less homogeneous view of research knowledge is both realistic and still to be valued.
2. The complex interrelationship between methods and methodology (and ontology and epistemology). We assert that the failure to sufficiently consider this interrelationship has resulted in the conflation of methods and methodology, and in an inappropriate mixing of a number of the key elements of interpretive research that are described later in this paper. We will argue that this has brought about improbable claims of what IAR can achieve.

This paper will proceed as follows. In section 2 we will focus on interpretive sociology and discuss the ways in which some of the associated authors claim they can get insight into human interaction and behavior in organizations. We then move to the management accounting literature in section 3, and try to trace some of the debates on interpretive management accounting research, and IAR in particular, which have taken place between (roughly) 1980 and 2012. This leads to a discussion that focuses strongly on Ahrens and Chapman (2006) in section 4. Section 5 summarizes our main findings, while section 6 sets out what we personally see as viable avenues for engaging in interpretive management accounting research.

2. Influences in the underpinnings of interpretive research and IAR

a. IAR and the influence of David Silverman

It has become popular, when describing the interpretive nature of management accounting research (Ahrens and Chapman, 2006; Lukka and Modell, 2010), to rely on Silverman (1993, 1997; see also 1998, 2011). Silverman posits that in order for qualitative research in the social

sciences to be called ‘rigorous’, criteria for validation and verification have to be upheld. He advocates a pragmatic stance when conducting research (Silverman, 1997), and argues that he strives for a ‘minimalist aesthetic’ in the research act, focusing on clarity and rigor in its conduct, with a ‘passionate commitment’ to ‘beauty and truth’ (see *ibid.*, p. 240/244 for more detailed statements about this) – even though what constitutes ‘beauty’ remains somewhat unclear.

Silverman does appear to suggest that a focus on research method can help to enhance the quality and rigor of someone’s research, using, for instance, conversation or discourse analysis techniques (Benwell and Stokoe, 2006). By so doing, a “... kind of social science which seeks to make practical interventions which might make social institutions more efficient or effective” can be advocated (Silverman, 1997, p. 246). Silverman’s argument seems to be that rigor in research can chiefly be reached by a focus on the meticulous application of research methods, and by the effectuation of the research outcomes through their correspondence to what ‘really’ goes on when people interact. Silverman (1997, 1998, 2011) proposes analyzing ‘naturally occurring data’ to realize this: data that are somehow recorded as they occur in real time³. This he finds the most viable way in which statements about human interaction can be made through qualitative research, as it allows the analysis of what people in organizations do, instead of what they say they do.

Silverman (1993, 2011) notes however, that when conducting interviews, it is debatable whether such high ‘quality’ information can be collected. Silverman (1998) argues that interviews can only provide “... decontextualized accounts of meanings [that] are *very limited guides to the complexities of ... [human] interaction*” (p.19, emphasis added), that they “... neglect a great deal about how people interact... [and] *are primarily concerned with the environment around the phenomenon rather than the phenomenon itself*” (1998, p.11, emphasis added)⁴. He proposes using observational research methods and recording techniques instead, like videotaping (Silverman, 2011). It is asserted that this will lead to the collection of ‘naturally occurring data’. Analyzing data that have thus been collected succinctly is sufficient for ‘rigorous’ research outcomes to come about.

³ In doing so, Silverman appears to neglect that what becomes identified as ‘data’ is determined by the researcher.

⁴ In spite of his strong reservations, Silverman has continued using interviews himself (Seale and Silverman, 1997) and describes this method extensively in his books, partly due to its popularity (Silverman, 2011). In the latter publication, it is claimed that something can be gained from interviews anyway when they are carefully coded and meticulously analyzed – but more suitable approaches may be available (see also Silverman, 1997).

When considering the brief vignette introduced at the start of this paper, one might, therefore, conjecture that if naturally occurring data had been collected and analyzed (for instance, by examining one or more conversations occurring between the dean, the deputy, and the university board) alternative interpretations might have ‘presented themselves’. It would then seem quite likely that someone (possibly a researcher) might have concluded that the deputy facilitated departmental change rather than the dean. Although Silverman (1997) acknowledges the researcher’s role in the conduct and writing up of a research project, he takes the view that it is not very useful to go into great lengths analyzing it, since while this may lead to interesting discussions among researchers, this is likely not to extend to other readers. That in the process of collecting, analyzing and interpreting ‘naturally’ occurring data, numerous assumptions are made by the researcher(s) that make the offering of ‘glimpses’ of ‘goings on’ between interacting individuals arguable at best (Søberg, 2005), is something that Silverman seemingly does not perceive as a problem.

It is interesting to note that Silverman (1998) positions himself as an interpretive sociologist. In interpretive sociology (Schutz, 1972; see also Lee and Lings, 2008), it is often argued that experiences are labeled as such and are infused with subjective meanings through the self-interpretation of the person who lives them. At the time that they occur, experiences do not represent distinct events. They are arranged as such retrospectively. The associated meanings may then be used prospectively for interpretation purposes in the research process or indeed in everyday life. However, an individual’s interpretations are not accessible to others, as this would imply becoming the other completely. What often happens in a research setting (and in many other situations in everyday life) is that “[w]hen I become aware of a segment of your lived experience, I arrange what I see in my own meaning-context. But meanwhile you have arranged it in yours. Thus I am always interpreting your lived experiences from my own standpoint ... *everything I know about your conscious life is really based on my knowledge of my own lived experience*” (Schutz, 1972, p. 106, emphasis in original).

Knowledge about the other is created when someone interacts with the other simultaneously (for instance, in a conversation) or quasi-simultaneously (for instance, by reading personal notes). This will be riddled with assumptions about the other based on one’s own interpretive schemas

(Hacking, 1999). Furthermore, the resulting insights are expressed in language, which is always value-laden (Bernstein, 1983; Johnson, 1995; Llewelyn, 1993). Schutz (1972), nevertheless, asserts that it may be possible to achieve a ‘genuine understanding’ of the other. For this to happen, attention should be directed to “... what is really going on in the other person’s mind, grasping those things of which the external manifestations are mere indications” (p. 113). This, he thinks, could lead to finding objective meanings:

“... the already constituted meaning-context of the thing produced, whose actual production we meanwhile disregard ... Objective meaning ... consists only in a meaning-context within the mind of the interpreter, whereas subjective meaning refers beyond it to a meaning-context in the mind of the producer. A subjective meaning-context, then, is present if what is given in an objective meaning-context was created as a meaning-context by a Thou on its own part” (ibid., p. 134, emphasis in original).

Schutz (1972) sees social science as uncovering objective meaning-contexts constructed out of subjective meaning-contexts, there being a social reality beyond direct experience that a researcher can try to apprehend. In order to do so, tapping directly into lived experiences may be insightful, moving to ‘genuine understandings’ from there. Chua (1986) rightfully notes that Schutz, by following this conceptualization of the social, sticks to a positivist notion of verification, and assumes that there exists an orderly, given world of objective meanings ultimately guiding human interaction, and thereby, the associated subjective meanings. However, an objective meaning-context must still be a subjective meaning-context of some kind, for a researcher cannot rise above his or her standpoints and remain encapsulated by them at the same time (De Loo and Lowe, 2011). A researcher ought not to think that he or she should be able to distinguish the one from the others. But this is exactly what Silverman (2011) relies on.

Silverman (1998) identifies with Schutz when describing his own research position, but then dismisses him as being too technical. He wants to call for more pragmatism when conducting research (see also Bryman, 1988, 2006). Silverman’s emphasis on taking in as much as possible of the available data when analyzing them after they have been recorded may be seen as an instance of trying to move to ‘genuine understandings’ by allowing for as much verification through highly detailed data analysis procedures. However, it should be kept in mind that even so, we are still studying the other from our own standpoint. Being a researcher and/or author often implies

positioning oneself to some extent ‘above interpretations’ or ‘above debates’, in this case through the selection of a particular research method, mode of analysis, and the subsequent writing up of the research. Yet, thereby, someone engages in the very same interpretations and debates at the time that he or she is trying to rise above them (Ahrens and Dent, 1998; Alvesson and Sköldberg, 2000; Bernstein, 1983). Separating the two is very difficult – to say the least (see also De Loo and Lowe, 2012). This is something we would like to elaborate upon next.

b. Ethnography and the status of the author

The entanglement (Geertz, 1988; Weick, 1995; see also Barad, 2003) of the researcher in the research act, which we highlighted in the previous section, is something that we believe cannot be circumvented. It makes all research inherently subjective (Armstrong, 2008). Discussions about the role of the author as researcher in the research act are, however, not new, having been conducted extensively in the ethnographic literature among others (Power, 1991; Marcus and Fischer, 1986; see also Geertz, 1988). As an exemplar or ‘ideal type’ of interpretive research, we would like to briefly address various stances and outcomes of these discussions below. We will subsequently use these stances and outcomes and contrast them to Silverman’s (1993, 1998, 2011).

Ethnographers have been faced with severe questions about the authenticity of their research. Doubts have been raised on: the scientific nature of the ethnographic enterprise, the role and authority of the ethnographer, the adoption of new styles of representing ethnographic research, and the authenticity of storytelling (Kelemen and Rumens, 2008; Myers, 2009; see also Armstrong, 2008)⁵.

Ethnography is concerned with the close-up study of a social group in its cultural setting (Power, 1991). Such studies provide a lens to observe the day-to-day practices of inter-organisational activity. Because of this attention with “... how society gets put together ...” (Garfinkel, 1974, p. 16), ethnography is a process of analytical description and interpretation by an observer constructing meanings of how a group of people order their life. It is descriptive in that it is an

⁵Lyotard (1984) may be considered one of the frontrunners of the abovementioned view. His criticism of established ‘meta-narratives’ encouraged researchers to seek new modes of expression and implicitly provided impetus for the ethnographic approach, with its close scrutiny of everyday life (see also Myers, 2009). We will further discuss this aspect below.

observation of social practices, and of the features and characteristics of certain phenomena. It is explanatory in that it seeks to explore those features and the social meanings that generate them (Smith, 2003). The contribution from ethnographic insights is through the reconstruction of phenomena, with issues of interest being highlighted that may illuminate different perspectives for public dialogue (Hammersley, 1992). Public dialogue allows for the narration or exposition of observations, and hence has the potential to generate conclusions that may "... point out the way for future policies, rather than scientific generalizations that may be of little use at the coal-face" (Burns, 1994, p. 12). The focus is upon analysis as a process of question discovery and on the richness, uniqueness and contextuality of what researchers call 'data'. Burns (1994) acknowledges that in these circumstances "[m]ass representation is not the purpose; there is no uniformity of nature; statistical data sanitized by experimental control are not representations of life" (p. 326). The concern is with uniqueness or authenticity, not a scientific search for validation, verification, conformity and/or replication.

Geertz (1988) states that whether in science or art, the author is irremediably present in the research process and cannot be hidden behind methods of enquiry. Geertz espouses a conscious view of the role of the author in producing a believable text, in much the same way many postmodern ethnographers call for the author to be as clear as possible in revealing their role in the construction of the text (Crapanzano, 1980; Marcus and Fischer, 1986). The latter authors argue that research texts should not be based on the researcher's 'understanding' but on a 'dialogue' between the researcher and the native, recognizing the integral part both participants play in an ethnographic study (Marcus and Fischer, 1986, for similar ideas in accounting see Armstrong, 2008; Quattrone, 2009).

c. Interpretations, damned interpretations and the research act

Somewhat surprisingly, and in contrast to the authors mentioned in the previous section, Silverman (1993, 1998, 2011) uses terminology that is commensurate with positivism when setting out his point of view on the research act (Kalekin-Fishman, 2001; see also ten Have, 2008), but decides to adopt and adapt their definition as he goes (Seale and Silverman, 1997). For example, expressions like validity, verification, reliability and generalizability are all present in his work but would not be supported by the writers we addressed in the previous section in the way Silverman uses them.

Positivism draws its appeal, in part, from its determination to ignore "... the ontological discontinuity between natural and social phenomena [and] leave its representations unreflexive and unproblematical" (Knights, 1995, p. 248). Although Silverman (1998) states that he does not regard himself as a positivist, and acknowledges that there is no generally accepted 'doctrine' supporting qualitative research, he does choose to adopt positivist epithets, just as, for instance, Lukka and Modell (2010) do in their discussion of the validation of interpretive research in management accounting. In the case of Silverman (1998), this suggests that in essence, he believes that unreflexive and unproblematical representations of research findings can be made. If not, his use of these expressions is at the very least misleading (see also Kalekin-Fishman, 2001).

As stated, Silverman (1998, 2011) suggests that in the (interpretive) research act, a lot of time needs to be spent on meticulously transcribing and analyzing data. It seems from this that he sees an attention to detail, as a way to (partially) achieve unproblematical research outcomes similar to those sought by Schutz (1972). We disagree with such a view. Methods do not contain some neutral observational language with which observations made by a researcher are unencumbered by his or her assumptions and values. He or she is an epistemic subject by definition (Johnson, 1995). Acknowledging this makes Silverman's (1997) adherence to 'truth', which is to be reached through verification and validation by a succinct application of research methods unattainable.

We also believe that Silverman's quest for more pragmatism in conducting research, which we have previously pointed out, will not help in this, as the choice of research methods is not independent of the worldview someone has. After all, in pragmatism, such views are present as well. Lee and Lings (2008) state that "... pragmatism is not an 'anything goes' philosophy ... It is not a license to avoid engaging with wider theories or rigorous methodologies" (p. 33). The variety of philosophical traditions called 'pragmatism' all involve forms of empiricism, in which the existence of an external reality is accepted, but no mirroring of this reality is assumed to be possible (Mounce, 1997). Stated differently, reality cannot be represented, and 'pure' knowledge cannot be obtained. There is always something added by researchers and/or their objects and/or subjects when they conduct research, as research is communal and involves inquiry and interpretation (Rorty, 1979). Research outcomes are co-created by those involved (including the researcher[s]) and the (take on) reality they are working in (Lee and Lings, 2008). What may count as 'knowledge' is, therefore, a matter of social convention and language. On top of this, research

progress is fallible by default. This is a distinct epistemological outlook that is shared by pragmatists (Mounce, 1997). Note that there is nothing ‘pragmatic’ about such a stance.

It should be noted however that the idea that no researcher can rise above his or her interpretive schemas, when taken to the extreme, would imply that there are no good reasons for preferring one observation or interpretation to another (Feyerabend, 1975; also see Chua, 1986). Every interpretation would be valuable in its own right, and as adequate as any other. Sayer (1984) suggests that such a stance would indeed be extreme, and puts the view that despite someone’s encapsulation by his or her interpretive schemas, such schemas do possess more or less practically adequate beliefs or assumptions about ‘the world’. They provide socially shaped agendas for human (inter)action (see also Hacking, 1999; Lukka and Modell, 2010). People will modify their interpretive schemas when they feel that the expectations they sustain are sometimes, or more frequently, not met. When this happens, engaging in reflexive practices can open up a debate between, for instance, researchers about how their observations and interpretations come about, from which shared interpretations might follow (Alvesson and Sköldbberg, 2000; Johnson and Duberley, 1993). In that case, one observation or interpretation would no longer be as good as another. Stated differently, in case this happens, it would be the plausibility and/or trustworthiness of someone’s observational claims that matters, not their validity or (mode of) verification (Humphrey and Scapens, 1996; Lukka and Modell, 2010; De Loo and Lowe, 2011; Scapens, 1992). Differences or disagreements between interpretations may then constitute interesting avenues for further research. Examples of how knowledge may be created in such settings have been provided by van Helden *et al.* (2010), Lowe (2001), Pickering (1995) and Knorr-Cetina (1999).

If someone decides to engage in reflexive practices, together with others, “... it is important to consider what kind of ... self-reflection might form a basis ...” for this (Johnson, 1995, p. 487). Kilduff and Mehra (1997) address five aspects, that they call ‘problematics’, which we believe could help researchers in determining their own position in such a debate. Kilduff and Mehra think these ‘problematics’ are not often covered in (organization) studies for they do not conform to generally accepted publication standards. The authors point out which views on the ‘problematics’ would typically be held by postmodern researchers. We will, however, describe them more generally, without singling out specific positions, as postmodern notions are not at the heart of this paper.

The ‘problematics’ that Kilduff and Mehra distinguish are⁶:

- The plausibility of meta-narratives (Lyotard, 1984). Do overall conceptual schemes, which find their way in ‘grand theories’ describing how ‘the world works’, exist or not (Llewellyn, 2003)? If they exist, under which circumstances can they be established?
- The relation between truth and fiction (Weick, 1995; see also Latour, 2005; Knorr Cetina, 1999). Is truth fixed, or rather, dependent on social conventions, context, and/or language?
- The relation between object and subject (Clifford and Marcus, 1986; see also Ahrens, 2008). Is someone willing to view scientific work as involving interpretation, which is laden with conventions and taken-for-granted assumptions? If so, (how) can such conventions and assumptions be scrutinized?
- The effect of writing style on how research endeavors are perceived (Riessman, 1993; see also De Loo and Lowe, 2012; Latour, 2005). How far is one willing to accept that aesthetical considerations can affect the value of the empirical work that is done, and how it is viewed by others? The possibilities of progress in science (Kuhn, 1970; see also Flyvbjerg, 2001). Is empirical generalizability feasible or is this, rather, a myth, as more research only leads to more questions?

Kilduff and Mehra (1997) claim that the implicit or explicit stances researchers take on the ‘problematics’ influence the way they conduct and relate to research. These stances are, however, hardly ever discussed or written about. We feel that opening up these debates (perhaps in a way similar to Ahrens *et al.*, 2008) could foster better understandings about what research might achieve, given that researchers are actively involved in the research process. As stated, the reason why we have dwelled so extensively on Silverman (1993, 1997, 1998, 2011) is that he is sometimes addressed in discussions on the nature, conduct and future of interpretive research in management accounting. But if we have serious doubts on the way Silverman tends to frame research and the role of the researcher therein, what does this mean for our take on the way in which interpretive research in management accounting is perceived in the related literature? This we would like to assess in the following two sections.

⁶Alvesson (2003) notes that Silverman, on almost all of Kilduff and Mehra’s (1997) ‘problematics’, tends to emphasize “... ideals such as accumulation of knowledge, objectivity, the possibility of and very precise demands for representation, a clear writing style, and the possibility of generalization” (Alvesson, 2003, p. 17).

3. Management accounting

Hopwood (1983) and Scapens and Arnold (1986) were among the first authors who stated, some thirty years ago, that accounting-as-practice was hardly ever researched, the emphasis then being on economically based theories of business conduct. They alleged that those theories had not been very useful in understanding and predicting management accounting practices and outcomes (see also Scapens, 2006). Scapens and Arnold (1986) note that:

“... the general nature of management accounting in practice does display a neoclassical influence ... This influence has introduced into management accounting both the methodology and political philosophy of neoclassical economics – neither of which is unproblematic. Alternative political philosophies are available and the methodological issues are far from settled” (p. 96/99).

The authors assert that a valuable way to proceed would be to study the behaviour of individuals and organizations and the ways this affects and is affected by management accounting (see also pp. 98-99 of their paper), among others by using case studies (Scapens, 1990, 1992). Since then, it seems that substantial “... knowledge of particular aspects of, or perspectives on, accounting practice” (Ahrens *et al.*, 2008, p. 842) has been accumulated, by researchers following up on such calls (Parker, 2012; see also Merchant and van der Stede, 2006).

Much of the research conducted in this vein has been termed ‘interpretive’ (Ahrens *et al.*, 2008; Boland and Pondy, 1983; Chua, 1986; Tomkins and Groves, 1983; Parker, 2012), which at the very least suggests a specific position of the researcher in the research act (Boland and Pondy, 1983; Chua, 1986). Although in the previous sections we mostly referred to sociological literature when discussing what such research might achieve, and how researchers are engaged in the (re)production of its results and practices, the stances and views found in the accounting literature are not dissimilar from those we noted in the sociological literature. Yet, the implications that have been drawn from these discussions seem to be somewhat different.

Following Lowe (2008), we conjecture that there have been several ‘rounds’ of debates in the accounting literature on the issues we have noted above: one dating back to (roughly) the

mid-1980s to mid-1990s, and a second, more recent one, starting in the mid-2000s up until now. We would like to address both rounds below, solely looking at the issues that we deem to be relevant to our discussion, and then focus on particular manifestations of the second round in the remainder of this paper. We will however take the liberty to include references from outside the accounting domain and from different time periods in our discussion of the two rounds, if we feel this enhances the flow of the argument.

a. Round one: beliefs, values and transitory stories

There is considerable diversity in what may count as interpretive (management) accounting research (Ahrens *et al.*, 2008; Lukka and Modell, 2010). One of the fundamental distinguishing features seems to be the nature of the status accorded by the individual researcher to knowledge (Chua, 1986; Hopper and Powell, 1985; Tomkins and Groves, 1983; see also Burchell *et al.*, 1980; Hopwood, 1983), when he or she tries to get an impression of the meaning and roles of accounting information in organizations (Burchell *et al.*, 1980). Interpretive researchers typically concern themselves with the legitimacy and veracity of their research accounts and stories, but as interpretivists they are by default able only to offer their accounts to the community of scholars to further ‘interpret’ as they feel appropriate. Their work must naturally be about plausibility in the eyes of the reader (Covaleski, Dirsmith, Heian and Samuel, 1998). This is of course something that any researcher *could* do, but interpretive researchers *should* consciously devote attention to this, since the ‘facts’ are in the hands of readers and/or later users of the accounts produced (Latour, 1997, 2005; Lowe 2005). Chua (1986) remarks that “... *the aim of the interpretive scientist is to enrich people’s understanding of the meanings of their [own] actions*, thus increasing the possibility of mutual communication and influence. By showing what people are doing, it makes it possible ... to apprehend a new language and form of life” (p. 615, emphasis added).

But how is such attention to understanding and plausibility to be brought to the fore when research is used to come to terms with how the behaviour of individuals and groups is affected by or affects accounting practices? Boland and Pondy (1983) argue for a hermeneutic approach in doing so. They endorse the use of case studies to understand accounting as a lived experience, grounded in the social process of an organization and its environment. They assert that there is no single meaning attached to management accounting information, and the way in which it is shaped and applied in an organization. Meanings are likely to be diverse and may also be distinctive, and it is

up to the researcher to determine which meaning(s) he or she prefers in creating his or her own understanding of what has been witnessed and/or collected. For this to happen, Boland and Pundy think that a researcher should ‘transcend’ individual interpretations (and the associated meanings) somehow. Their view can be seen as a modification of what Schutz (1972) proposes. Schutz argues that when engaging in such ‘practices of transcendence’, only one interpretation can ultimately emerge: an objective one with specific and undeniable meanings. Llewellyn (1993), following Boland and Pundy (1983), supports a hermeneutic approach to field research in general, and makes the point that “... researchers can legitimately offer accounts of events which transcend the understandings of agents themselves” (p. 241).

Working in hermeneutic circles necessarily requires that as researchers we accept our role in the crafting of the research story (Bernstein, 1983). The researcher as subject influences the object of his or her research through the research act and necessarily produces an ‘intrinsically transitory’ story as a result. This story is not only transitory but partial as a consequence of the “... unique engagement of the frames of reference of the researcher and the researched” (Llewellyn, 1993, p. 247). Llewellyn argues that the major difference between interpretive and positivist research is that, contrary to the latter, “... interpretive research will attempt, in some way, to address [the] ...connections between subject and object and between meanings and practices” (p. 233). Johnson (1995) adds that when such research is conducted, any notion of correspondence of the research outcomes with some truth that is ‘out there’ is let go of. Related to this, Scapens (1992) notes that “... the [interpretive] research process ... involves the *interpretation* of observed social practices. The social world itself is created by individual social actors making sense of their day-to-day activity, and researchers add a second level of ‘sense-making’ in their attempts to understand the experiences of social actors” (p. 371, emphasis in original). Although one could argue that a third level of sense-making occurs when researchers engage with ‘the field’, namely in the mere act of observation itself (preceding the second level of sense-making mentioned above, which solely focuses on the understanding of the observations made)⁷, this quote does indicate that interpretative research, from the outset, bears an extensive imprint of the beliefs, values, and interpretation schemas of the researcher(s). These, therefore, also need to be acknowledged from the outset. Humphrey and Scapens (1996) assert that, consequently, the persuasiveness of the arguments proposed by a researcher and his or her reflections on them may matter the most in assessing the

⁷ However, it must be acknowledged that these levels cannot be so easily disentangled.

value of a particular piece of research. Personal criticism (by the researcher, and possibly also others) on how his or her results have been obtained may be more helpful in assessing the research's contribution than considerations about validation or justification.

Chua (1986) asserts that connections between subjects and objects, and meanings and practices can judiciously be made when agreement is found between a researcher's interpretations and the common-sense interpretations of those who have been studied. Lee and Lings (2008) state more broadly that: "[p]resenting your work in a way which assures readers that you have used appropriate strategies and methods, as well as recognising and explaining your own part in creating the interpretation (remember reflexivity⁸) should help to enhance perceptions of the rigour of your work" (p. 68, footnote added). Therefore, holding an affiliation with interpretive research does not mean that acknowledging specific effects of socially produced structures, roles and rules must be eschewed in (the writing-up of) such research. In very general terms, we assert that in interpretive research, beliefs about the constitution of knowledge do not have to do with the hardness of the 'facts' or structures but with how they are constituted, through social process, and how they are open to change through human agency, thereby sustaining the fundamentally constructed nature of social 'facts' (Armstrong, 2008; Chua, 1986; Cooper, 1983; Hopper and Powell, 1985; Scapens, 1992). A researcher's authority is formed by providing interpretations of the field sensitized by his or her engagement with the research context and its participants - the 'contact zone' (Ahrens and Chapman, 2006). By so doing, the author might be regarded as claiming a superior position in providing interpretations that transcend that of the participants in his or her research (Bernstein, 1983; De Loo and Lowe, 2012). However, it is hard to see what a researcher might else do, apart from writing down his or her observations without trying to combine them in some way (Hammersley, 1992). Even then, interpretations of interpretations will be present (see also Scapens, 1992). This is simply inevitable when research is conducted. The only way to place a different view on the result of research of this nature would be to assume that the insights gained from research produce *concrete* facts. If this is to be the outcome, any association with interpretive research would have to be disavowed (Jackson and Carter, 1993; see also Burrell and Morgan, 1979; Chua, 1986).

⁸ We have discussed this briefly in section 2.

In sum, one of the main points in the early discussions on interpretive research in the management accounting literature that is relevant to our discussion concerns the idea that some consideration ought to be given to the role of the author in the research process. This could certainly mean an awareness of the need to acknowledge in the research story the researcher's concerns and assumptions. Research accounts do not provide access to the facts, or some objective reality, but present an opportunity for understanding through interpretation. All interpretations that may be achieved are tentative and inherently multiple. Rather than revealing facts 'as they are', particular views on and of day-to-day activities, which are subjective and mediated by a multitude of factors, among which are the research process and the researcher him- or herself, can thus be provided. At best, local, context and time-specific accounts and interpretations of how people interact in (preselected) social settings can be provided, possibly leading to useable knowledge for those involved in the research act and readers of the ensuing results (Merchant and van der Stede, 2006; see also Covalleski and Dirsmith, 1990; Jönsson and Macintosh, 1997; Lukka and Modell, 2010). Instead of focusing on the validity or generalizability of research findings it might then be worthwhile to discuss issues of transferability:(how far) can the knowledge developed in an interpretive research project be used in other contexts?⁹

b. Round two: positioning data, progress and moving away from interpretive sociology

For a period of some 20 years interpretive research was apparently 'simply' carried out, and field studies in accounting and management accounting, more particularly, significantly increased in number (Ahrens *et al.*, 2008; Merchant and van der Stede, 2006). Criticism then arose about the way in which such empirical research was being conducted and the breadth and 'reliability' of the conclusions reached (Zimmerman, 2001; but see also Hopwood, 2002; Lillis, 2008; Lukka and Mouritsen, 2002).

In the early literature (which we associate with 'round one') interpretive research was commonly proposed as an alternative to the prevailing neoclassical influences in accounting research. Often the approach was couched as the most legitimate framework from which to study accounting-as-practice and in practice (Scapens and Arnold, 1986). More recently, discussions have used a different language tending to juxtapose a 'quantitative research tradition' and a

⁹ We are indebted to Professor Bob Scapens for this useful insight.

‘qualitative research tradition’, with positivistic research being regarded as an exemplar of the former and field research based largely on interviews and case study as an exemplar of the latter. For instance, Parker (2012) states that over time, an influential ‘qualitative tradition’ in management accounting research has emerged which counterbalances an already existing and extensive ‘quantitative tradition’ in such research. The ‘qualitative tradition’ focuses on “... the understanding and critiquing of management and accounting processes, as well as having the ability to address the concerns of practitioners and policymakers” (p. 54) (see also Merchant and van der Stede, 2006; Vaivio, 2008). The extant literature is, however, somewhat more complex than this, with several authors arguing that the two approaches can be mixed (Baxter and Chua, 2003; Marginson and Ogden, 2005; Richardson, 2012), while others claim that this is almost impossible (Vaivio, 2008; Parker, 2012). This ‘dichotomy’ has been said to stem from two corresponding ‘camps’ or ‘tribes’ of accounting researchers (Ahrens *et al.*, 2008; Lukka, 2010; Parker, 2012), who hardly intermingle and cannot easily exchange standpoints and results due to differences in research approach and language used (see also Merchant, 2010).

Malmi (2010) and Vaivio (2008) find such descriptions and juxtaposes detrimental. They argue that this situation has, at least partially, been caused by differences in paradigmatic viewpoints between researchers, which may be resolved through more openness between various research traditions and their proponents (see also Merchant, 2010; Modell, 2009, 2010; Willmott, 1993), or by setting aside paradigmatic differences altogether and engaging in more pragmatic research¹⁰ (Malmi and Granlund, 2009; see also Lukka and Modell, 2010; Silverman, 1998). However, Armstrong (2008) notes that such views downplay the “... recognition of the tendential contradiction between the very concept of an interpretive epistemology and the expectation that its findings should be cumulative” (p. 869) - an aspect that we have stressed in the previous section. Such a ‘tendential contradiction’ is, however, not so much present in positivistic research, where propositions of goings on can be judged against an objective social reality (Chua, 1986), the researcher being the main judge through his or her appeal to method, coupled with an attentive attitude. This attitude carries certain conventions, assumptions and beliefs that support the idea of the progress of science and the building of a cumulative knowledge base as a matter of ‘fact’ – and also faith (Lee and Lings, 2008).

¹⁰The paradigmatic connotations of which are subsequently overlooked.

Kakkuri-Knuuttilla *et al.* (2008) assert that much of the extant interpretive management accounting literature appears unaware of the fundamental assumptions on which it is based. The authors associate a naïve subjectivism to interpretive accounting researchers by suggesting that “... in management accounting research, it is often routinely assumed that interpretive studies ... are based on subjectivism only” (p. 267). Kakkuri-Knuuttilla *et al.* support their contention with a reference to Ahrens and Chapman (2006), who they believe incessantly emphasize the subjectivist nature of interpretive management accounting research. Some have argued that positivistic research is inherently objective and field research, on which much interpretive management accounting research is based, inherently subjective - but objectifiable (Ahrens, 2008; Ahrens *et al.*, 2008; Kakkuri-Knuuttilla *et al.*, 2008). Therefore, it has been claimed that interpretive (management) accounting research combines both objective and subjective elements (Ahrens, 2008; Kakkuri-Knuuttilla *et al.*, 2008; Lukka and Modell, 2010). Some researchers have, consequently, put the view that interpretive management accounting research, like research from the ‘quantitative tradition’, has the ‘power’ to show or predict ‘what works’ in organizations (Malmi and Granlund, 2009; Merchant and van der Stede, 2006). Thereby, working practices might become more effective (Malmi and Granlund, 2009; see also Silverman, 1998).

Such research can allegedly help build a bridge between ‘theory’ and ‘practice’ (Baldvinsdottir *et al.*, 2010; Malmi and Granlund, 2009; see also van de Ven and Johnson, 2006 and Merchant and van der Stede, 2006). The position adopted by researchers supporting this stance is close to that of Silverman (1997, 1998), who supports similar aims for qualitative research, as we have seen in the previous section ¹¹. Therefore, not surprisingly, Silverman is invoked directly by many management accounting researchers including Ahrens and Chapman (2006) and Lukka and Modell (2010), and used to sustain a line of thinking that resembles that described above. For example, Lukka and Modell (2010)¹² derive a specific conceptualization of ‘validation’ for interpretive research in management accounting, arguing that Silverman has done something similar for qualitative research. In contrast, we have argued that using positivist epithets to describe interpretive research practices and aims is misleading. Other authors also insist that the current debates in the management accounting literature are too simplistic, and that the

¹¹ Qualitative research, however, is not necessarily the same as interpretive research. This we will discuss in the following section.

¹² We will examine Ahrens and Chapman (2006) more closely later.

terminology used is confusing (Armstrong, 2008; Lillis, 2008; Richardson, 2012). For instance, debates about the ‘quantitative’ and the ‘qualitative tradition’ in management accounting research are often called ‘methodological’ (Ahrens, 2008). While there are certainly methodological issues at stake, such issues cannot be considered in isolation from the position of the researcher in the research act and his or her own beliefs, values and interpretive schemas, as the debates in ‘round one’ eloquently illustrated (Chua, 1986).

All research is inherently subjective (Armstrong, 2008; De Loo and Lowe, 2011), and paradigmatic standpoints can never be put aside when the research act is discussed – there are no other foundations to base someone’s approach to research on otherwise (Feyerabend, 1975). Stated differently, we feel that discussions about the conduct and possibilities of interpretive management accounting research are currently often being carried out at an inappropriate level of analysis (see also Lillis, 2008). If interpretive research in management accounting indeed “... tends to entail highly context- and time-specific analyses of how people communicate and act in a particular social setting” (Lukka and Modell, 2010, p. 464; see also Ahrens, 2008), in which “... the inter-subjective, rather than strictly subjective, nature of meanings” (Lukka and Modell, 2010, p. 465) is emphasized, this has huge consequences for what such research can sensibly achieve. We would like to illustrate these consequences by looking at, in our view, a telling example where a discussion in the management accounting literature is carried out at an inappropriate level of analysis: the equal treatment of interpretive and qualitative research (Ahrens and Chapman, 2006, 2007).

4. Interpretive vis-à-vis qualitative accounting research

Right at the start of Ahrens and Chapman (2006), qualitative and positivistic researchers (conducting field studies) are pitched against one another and presented as two different groups with different views on what constitutes ‘good’ field research (in management accounting), even though, according to the authors, “... qualitative and positivistic field studies are systematic articulations of sets of statements that can variously relate to explaining, predicting and prescribing social phenomena” (p. 826). In footnote 1 of their paper, the authors state that they:

“... draw on Silverman’s (1993) usage of the term qualitative in relation to methodology, which, in the management accounting literature, has, with minor variations, also been referred to as naturalistic, holistic, interpretive, and phenomenological. It stands in contrast to a positivistic approach to research” (p. 819).

This same statement is repeated on p. 822 of their paper. Thus, Ahrens and Chapman (2006) claim that by and large, ‘interpretive’ and ‘qualitative’ research mean the same and can and have been used interchangeably in the management accounting literature (see also Ahrens, 2008). Ahrens and Chapman (2007) seems to have been written with this idea in mind, for whereas in their 2006 paper the expressions ‘qualitative’, ‘qualitative research’ or ‘qualitative researcher(s)’ are used repeatedly, they are not used at all in the 2007 paper – the focus now being on ‘interpretive studies of accounting’ (p. 2). In later publications, for instance Ahrens *et al.* (2008) and Ahrens (2008), the expression ‘interpretive accounting research’ (IAR) emerges suddenly. Unfortunately, the authors in the ‘polyphonic debate’ (Ahrens *et al.*, 2008), fail to provide a definition of IAR that they all sustain. It is, we assert, likely that footnote 1 of Ahrens and Chapman (2006) formed the basis of this shift from the label ‘qualitative’ to that of ‘interpretive’ accounting research.

Apparently, Ahrens and Chapman (2006) endorse the idea that researchers are actively involved in the research act, as they state that “[d]efining qualitative field studies with reference to qualitative methodology allows us to focus on the qualitative researchers’ strategies in the pursuit of knowledge, rather than simply the tools that they commonly use” (p. 822). In so doing, researchers “... work from the assumption that the field is an emergent social reality open to diverse interpretations of its participants and observers (and not an objective reality suitable for positivistic inquiry) and that this social reality can be studied ...” (p. 829; see also Chua, 1986)¹³. This diversity of interpretations that the researchers are confronted with needs to be overcome, because “... nuanced descriptions of the phenomena themselves, the processes which define them, and the (changing) contexts in which they are situated” (Ahrens and Chapman, 2006, p. 834) have to be provided. The qualitative researcher “... has done well when she has developed a convincing

¹³ While Ahrens and Chapman (2006, p. 819) appear to draw authority for their remarks from Chua (1986), she is referring explicitly to ‘interpretive’ epistemology, and not to qualitative research in a more general sense.

account of the ways in which meanings and purposes relate to patterns of activity¹⁴” (p. 834, footnote added).

There are a number of ontological, epistemological and methodological claims made, in the quotes we have reproduced in the previous paragraph, that we find mutually incompatible. They are also not in line with footnote 1 of the Ahrens and Chapman (2006) paper. Indeed, we have difficulties with the footnote itself. These aspects will be developed further below.

a. Method and methodology

Armstrong (2008) notes, in passing, that Ahrens and Chapman (2006) apply some of their terminology carelessly when they use ‘qualitative research’ when they probably mean ‘interpretive research’¹⁵. Nevertheless, Ahrens (2008), like Ahrens and Chapman (2006), stresses that the two can be used interchangeably¹⁶ “...to characterize a study’s methodology ... The label “interpretive” ... gives a broad indication that social reality is emergent and subjectively created yet (successively) objectified in social intercourse” (p. 296). For this interpretation to be correct, the label ‘interpretive’ would have to describe a primarily ontological view, but not also a methodological view, as ontologies and methodologies are only loosely coupled (Guba, 1990). Lee and Lings (2008) note in distinguishing methodology that “... it is important to never confuse *interpretive* with *qualitative*, as is often done. Simply saying a piece of research is qualitative does not imply it is also interpretive, and conversely saying you are an interpretive researcher is not the same as saying you use only qualitative data. Never make that mistake ...” (p. 65, emphasis in original; see also Deetz, 1996; Walsham, 1995, 2006).

Interestingly, Ahrens and Chapman (2006) address a similar issue¹⁷ when they argue that “[j]ust as statistical methods may be used in qualitative field research, positivistic studies may rely on interviews” (p. 823). Expressions such as these lead us to believe that Ahrens and Chapman ultimately conflate methods and methodologies. Richardson (2012) observes that this is a simplification of the debate because:

¹⁴ Interestingly, such patterns of activity are the focus of the Ahrens and Chapman (2007) study.

¹⁵ Also refer to the postscript of this paper in relation to this.

¹⁶ While we accept that some texts on research methodology interchange these terms this may be excused because of the readers to whom such books are aimed. In a complex contribution to a sophisticated literature the use of such terminology must be much more circumspect.

¹⁷ That is, when the authors’ opinion that ‘qualitative’ and ‘interpretive’ research are very similar is accepted.

“... [i]t is important, in particular, to recognize that qualitative methods are just that – methods. These methods are consistent with a number of philosophical positions ... and it is troubling to see qualitative methods (or quantitative methods for that matter) ... as if they were based on a homogeneous set of assumptions. This rhetoric fuels a division between qualitative and quantitative research in accounting which is neither empirically valid nor productive ...” (p. 84; see also Myers, 2009).

We agree with Richardson, and it is illuminating to see that in ‘round one’ of the debate in the management accounting literature such simplifications were absent, and more nuanced discussions seemed to be a feature of the literature (Chua, 1986; Cooper, 1983; Hopper and Powell, 1985; Scapens, 1992).

As we have seen, Ahrens and Chapman (2006, p. 822) initially do, separate methods from methodology (at the very start of their paper), using Silverman (1993) as their main source of reference. If this is done, it is not correct to consider the adjective ‘qualitative’ to belong to methodologies, as for instance Ahrens (2008) appears to believe. ‘Qualitative’ is most appropriately used to describe research methods (Richardson, 2012; see also De Loo and Lowe, 2011).

b. Being ‘interpretive’

Even though many researchers will accept that all research involves interpretation (Armstrong 2008; De Loo and Lowe, 2011), the adjective ‘interpretive’ is commonly used to describe a specific philosophical position (Chua, 1986). These different uses of the label ‘interpretive’ ought not to be mixed. Ahrens and Chapman (2006) refer to Chua (1986) when setting out their point of view on qualitative field research in management accounting, and Ahrens (2008) quotes her extensively to illustrate that interpretive management accounting research contains both subjective and objective elements.

In his 2008 publication, Ahrens also acknowledges that Chua (1986) draws heavily on Schutz’s work. Chua (1986) says that she (and others) describe three ‘disciplinary matrices’- general approaches to be used to frame and study accounting. The approaches she distinguishes are denoted as ‘mainstream’, ‘interpretive’ and ‘critical’ alternatives to accounting thought. In the various tables in Chua’s paper, she depicts the key features of the three approaches. It is evident

that she discerns, to a varying degree and detail, ontological, epistemological, and methodological aspects in all three of them (*ibid.*, p. 605). She does not use the term ‘paradigm’ however, even though some authors seem to think this is what she actually depicts (see, for instance, Vosselman and van der Meer-Kooistra, 2012). This, perhaps, is done because the paper contains a critical discussion of Burrell and Morgan (1979), who use the term explicitly in their influential treatise. What Chua (1986) adds, to what are generally considered to be ‘paradigms’ (Guba, 1990), is how the relationship between theory and practice is perceived in the three alternatives. In the case of interpretive accounting research, Chua (1986) asserts that “[t]heory seeks only to explain action and to understand how social order is produced and reproduced” (p. 615).

Chua thus associates the adjective ‘interpretive’ with more than just methodology. Even though Ahrens and Chapman (2006) posit that ‘interpretive’ or ‘qualitative’ research is a matter of methodology, they briefly mention that this is related to a different set of ontological and epistemological standpoints than positivism (p. 822). However, this is not further discussed. Although Ahrens (2008) also states that ‘interpretive’ research in accounting is a matter of methodology, he goes on to address specific views on the nature of reality that are involved in such research – thereby framing the ontology that is used. In addition to this, Ahrens and Chapman (2006), as we have previously seen, discuss the involvement of the researcher in the qualitative or interpretive research process – this serves as an illustration of the epistemology they adhere to. We believe, in contradiction to the view Ahrens (and Chapman) appear to express, that interpretive and qualitative research are not the same and involve much more than chiefly methodological issues.

In addition, we believe that Silverman does not assert that interpretive and qualitative research can be used as synonyms (with minor variations). In fact, he is careful to discriminate between the two, albeit the terminology he uses may be somewhat confusing (Silverman, 1998). Like us, he uses the adjective ‘qualitative’ to depict a range of research methods (Silverman, 1993, 2011), but argues in addition that these methods cannot be discussed separately from someone’s research outlook and design. Silverman (1998) then posits the view that interpretive research (where ‘interpretive’ is defined similarly to our discussion of ‘interpretive sociology’ in section 2), often applies qualitative methods. He uses the expression ‘qualitative research’ almost exclusively to denote the latter (the application of qualitative methods), but at the same time uses ‘interpretive research’ to

discuss the ‘bigger picture’ in which qualitative methods (‘qualitative research’) typically operate. Although this terminology might confound someone, and could lead to the suggestion that the two expressions can be mixed, Silverman thus does make a distinction between the two. In fact, Silverman (1998), which refers to Silverman (1993), indicates that qualitative research (methods) cannot be criticized ‘as a whole’, there being a number of ‘research styles’,¹⁸ commensurate with such research (methods). Therefore, we suggest that Ahrens and Chapman (2006), in footnote 1 of their paper, may have interpreted Silverman somewhat cursorily.

There is, however, a specific aspect where we believe Ahrens and Chapman (2006) have followed Silverman (1993) very closely. Unfortunately, this has been followed by a number of other accounting researchers also (Ahrens, 2008; Ahrens *et al.*, 2008; Malmi and Granlund, 2009; Parker, 2012; Vaivio, 2008). This is the notion that interpretive research (in accounting or elsewhere) could and should somehow lead to a coherent body of knowledge (just like in positivistic research) (Ahrens *et al.*, 2008; Ahrens and Chapman, 2006; Malmi and Granlund, 2009). This view, we believe, is based upon an overestimation of researchers’ abilities. It is also not in line with the ontological and epistemological standpoints followed by many of the authors of ‘round one’.

Malmi and Granlund (2009) argue that management accounting research should “... provide explanations ... to answer questions of what kind of management accounting systems we should apply, how, in what circumstances, and how to change them” (p. 597). Interestingly, both authors were also engaged in the ‘polyphonic debate’: an exchange among fifteen management accounting researchers about the definition, (then) current state and future perspectives of IAR (Ahrens *et al.*, 2008). In this ‘polyphonic debate’, it is noted how difficult it is to reach an agreement on these issues, even though IAR is clearly identified as an alternative to positivist research, and should offer insights that can be juxtaposed to such research (see also Baxter and Chua, 2003). If this is not done, such research could appear insufficiently rigorous (Lillis, 2008; Parker, 2012; Silverman, 1997).

¹⁸ Silverman (1998) does not like the implications of the term ‘paradigms’, which he thinks makes research unnecessarily inflexible. Implicitly, he calls for loose couplings between ontological, epistemological and methodological standpoints in research. Such views are endorsed by Guba (1990) and De Loo and Lowe (2011) among others, and are also followed here.

Williams and Ravenscroft (2012) suggest, however, that the notion of positivist research¹⁹ being rigorous is a myth, as it is based on the assumption that accounting is quantifiable, the outcome of which can be compared to some physical state that is ‘out there’ to be unraveled. These authors claim that in economics, this assumption is fallible (see also Flyvbjerg, 2001). Calling someone’s research ‘rigorous’ may give credibility to someone’s outcomes, but is ultimately a matter of appearance and not a substantive quality of the research conducted. Note that similar discussions have been conducted about the quality of interpretive (accounting) research (De Loo and Lowe, 2011; Riessman 1993), with similar results. Juxtaposing insights from interpretive and positivist research may, consequently, be less compelling than it may appear at first.

A somewhat related view is expressed strongly by a number of writers. Lillis (2008) states that it is not very fruitful to frame discussions about (the value of) research in terms of whether one research ‘camp’ or research method is ‘better’ than the other (see also Richardson, 2012), while Armstrong (2008) alleges that it is not useful “... to think of interpretive research primarily in terms of its contribution to theory, if theory is understood in the sense of abstract propositions which will generalize across settings” (p. 876).

We contend that the abstract propositions Armstrong refers to constitute what is sometimes called a ‘body of knowledge’ (see also Ahrens *et al.*, 2008). Some researchers in the ‘polyphonic debate’ suggest that such a body of knowledge exists in IAR or can be built (*ibid.*, p. 857). Like Malmi and Granlund (2009), Merchant (2008) claims that researchers ought to look for the general applicability of their research findings. This assumes that the world ‘out there’ exists and is governed by general patterns and that their general applicability is waiting to be unraveled and can, consequently, be found by researchers through their engagement in the research act. Following Schutz (1972), Malmi and Granlund’s (2009) and Merchant’s (2008) view may seem understandable. Researchers, by being attentive and engaging in ‘deep’ probing, might uncover innate structures guiding human behavior (objective meaning-contexts). These structures may be temporary, and may differ between contexts, but they might be distilled nevertheless (Vaivio, 2008). This is a view which, as we have previously discussed in section 2, can also be found in

¹⁹ It should be noted that Williams and Ravenscroft use the expression ‘contemporary mainstream accounting research’ instead of positivist research. We acknowledge that there exists contemporary, non-mainstream positivistic accounting research, and suggest that there may be contemporary accounting research that is not positivist too. However, the implicit assumption in the Williams and Ravenscroft paper is that the two are essentially the same (see p. 7 and pp. 11-12 for details).

Silverman's work (1997, 1998, 2011) and, as argued earlier on in section 4, in Ahrens and Chapman (2006) and Ahrens (2008). We hope to have demonstrated, in the foregoing sections, that this view cannot be sustained when conducting interpretive research, no matter what some of its proponents proclaim.

In relation to this, we also find the notion of generalizability as a potential outcome of interpretive research problematic²⁰. Vaivio (2008), when reflecting on interpretive and qualitative research²¹, mentions that “[t]he desire to look beyond a particular study to discover something more “general” is to be applauded” (p. 78). He also asserts that “... qualitative [and interpretive] management accounting researchers can be prone to over-generalizing their findings. ... [S]ome researchers ... suggest that ... findings can be generalized ... to a larger organizational population. This inappropriate claim ... stands as ... [a] pitfall of qualitative management accounting research, which stems from a positivist or modernist conception of science” (*ibid.*, p.78). Vaivio thus seems to imply that there are two levels of generalization that researchers can apply: the ‘right’ level of generalization and ‘over-generalization’, induced by following ‘fallacious’ positivist or modernist notions (see also Modell, 2009). How to distinguish between these two levels is, however, unclear. De Loo and Lowe (2011) claim that distinctions such as these infuse a researcher with too much ‘power’, as it can never be ascertained if ‘general patterns’ or more ‘local’ ones have been found, or indeed if there is a ‘pattern’ operating in the first place (see also Ahrens and Dent, 1998). They can only be seen as assertions, (hopefully) based on a set of assumptions related to the research act and the role of the researcher(s) therein. Besides, as we have shown in section 3, the mere notion of ‘generalizability’ may oversell what research in the social sciences can achieve, as no researcher can stand outside his- or her frame of reference (De Loo and Lowe, 2011; Flyvbjerg, 2001; Kilduff and Mehra, 1997). We admit that making general claims looks appealing, and conforms to what is regularly done in journal publications across the management fields (Kilduff and Mehra, *ibid.*). However, we assert this goes against the trustworthiness of such research for the reasons just mentioned (Riesmann, 1993; see also Flyvbjerg, 2011).

When generalizability cannot be attained, the credibility of creating a coherent body of knowledge in social science research also vanishes, and thereby, Malmi and Granlund's (2009) and

²⁰ Note that we earlier referred to the ‘transferability of research findings’. Unfortunately, there is no precise agreed definition of generalizability, and yet it is very difficult to write about methodology without using the term.

²¹ Note that Vaivio (2008, p. 65) also conflates ‘qualitative’ and ‘interpretive’ research in his contribution.

Merchant's (2008) desire for certain types of results from management accounting studies. 'Grand theorizing' from interpretive management accounting research does not seem to be viable (Kilduff and Mehra, 1997). Most, if not all, that happens in the study of human behavior and interaction involves interpretation. Recognizing that such interpretation is entangled with all of our actions, writings, preconceptions, and the interpretation of others, often unwittingly so, suggests that whatever is come up with in the course of research and framed as a 'body of knowledge' can only be seen as being highly volatile, variable, and/or as pieces from separate analyses. Whenever one claims that such a 'body' has been constructed through interpretive research, this cannot have been done without making heroic assumptions - and having a lot of faith in one's own research powers. Once we no longer view positivist and interpretive research as stemming from opposing 'camps' that should lead to contrasting outcomes, the main aim being to show which approach 'works best' in practice, this could actually lead to a constructive dialogue between the researchers involved (Kilduff and Mehra, 1997). However, it would be in direct contrast to what some authors seem to endorse or deem possible (Ahrens *et al.*, 2008; Merchant 2010; Parker, 2012).

Sandelands (1998) asserts that since the 'world' is 'eternally' unfinished and incomplete it follows that it is better to have theories that are unfinished, possibly ambiguous, and alive with questions than finished, dead theories that presume to provide answers to questions which invalidate it, in the shaping of a 'body of knowledge'. Theories are, we believe, constructions generated by the subjectivity of human meanings (Bernstein, 1983). This is the distinguishing factor between the conceptualisation of theory and description, which merely provides summaries of observations (Weick, 1989, 1995a, 1995b). In ethnographic research, passing judgement on which practices are worthy of exploration and the interpretation and framing of these in terms of a 'theory' are a reflection of an ethnographer's observation of daily conventions. Thus, the values and ideology of the observer implicitly become part of the selection of views and insights for the production of an ethnographic account. As Geertz (1975) declares: "[t]he locus of study is not the object of study. Anthropologists don't study villages; they study in villages" (p. 22).

The subjective representations that result from such processes have been the object of censure by those preoccupied with the search for scientific 'truth', suggesting that such social facts are mere human fabrications. Yet, few would claim that (even) positivist methodologies have been entirely successful in eliminating bias from experimentation and research design (Bonoma, 1985). Smith

(1991), for instance, argues that while scientific inquiry claims to "... annihilate the scientist's viewpoint through the separation of the producer of the statement and the procedure whereby it is produced ..., [there is] no magic trick that may bypass the act of interpretation" (p. 147). Gouldner (1970) argues that: "... positivism premises that [the] self is treacherous and that, so long as it remains in contact with the information system, its primary effect is to bias or distort it" (p. 495). He goes on to claim that "... the assumption that the self can be sealed off from information systems is mythological" (*ibid.*). Indeed, this analogy is embraced by ethnographers, who argue that the human world can only be known in subjective human terms. Watson (1987), for instance, proposes that "... ethnographies are fictions in the sense that they are something made" (p. 37).

Similar notions, we believe, have to be respected in discussions on (interpretive) management accounting research. Willmott (1998; see also Knights, 1995) suggests that ethnomethodology, in common with positivism, has a tendency to seek to produce complete narratives. Critical theorists also are inclined to seek complete stories which accord with their prior theories (Willmott, 1998). Knights' (1995) recommends that researchers dispense with the illusion of neutrality that many academics seek to cultivate around their activities. We feel that the latter conception of building a 'body of knowledge' is commensurate with our view of research and the contribution we might claim for such academic endeavours. An interpretive view of the complexity of social organizations needs to reflect the differentiation and heterogeneity which we find in studying these contexts. A number of writers have discussed how greater modesty might be brought into the execution and reporting of research. In so doing, some authors emphasize the significance of narrative styles of writing (Czarniawska, 1995, 1998; Deetz, 1996; Polkinghorne, 1987; Putnam, 1996; Van Maanen, 1996) while others introduce aspects of multivocality (Linstead, 1993), or problematize the boundaries between theory and method (Burrell, 1997; Jacques, 1992). Ahrens (2008) puts the view that "[i]t is misleading to suggest that the achievements of interpretive management accounting research are the result of researchers saying one thing (insisting on the exclusivity of subjectivist research) and doing another (quietly smuggling objectivist findings into research papers)" (p. 296). As we hope to have illustrated in this section, it may just be that this is done (inadvertently) by some researchers who claim to be conducting interpretive research when they may be conducting positivist research while using qualitative methods (Armstrong, 2008).

5. Discussion: what we know and what we know we don't know

In the previous section we object to the way in which IAR has been constructed by deploying what we believe to be a more proximate representation of interpretive research. We do not accept the viability of the position described by Silverman (1993, 1997, 1998, 2011) and more particularly Ahrens (2008) and Ahrens and Chapman (2006). We do accept that this alternative portrayal of a research programme may be acceptable to others but it does not accord with the critical features of interpretive research that we have sketched. It might be acceptable to argue that such a programme could be considered to be qualitative. But it is not appropriate to refer to the programme as IAR since this implies that it is in some sense inclusive and this is patently not the case. All interpretive research in accounting is not definable in this way since as we have argued there would be swathes of interpretive research that would not accept and could not be made amenable to a cumulative body of knowledge (Ahrens, p. 863, in Ahrens *et al.*, 2008) about either management accounting practices or social structures... nor the imprecision in distinguishing methodology and method as, we suggest is, exhibited in some of the writings critiqued above.

There is a strong-form objection to IAR and a weak form objection. Much of what we have dealt with in this paper is of the former nature. In the earlier sections of the paper we have examined some of the fundamental theoretical objections to the way IAR has been constructed and suggested ways in which they are unconvincing. But there is, we believe also a powerful weak-form objection to the IAR that has been offered to the accounting research community (Ahrens, 2008; Ahrens *et al.*, 2008; Ahrens and Chapman, 2006, 2007). We accept that one researcher's methodology of choice cannot easily or effectively be overturned by sincere intellectual argument (a strong-form argument) but instead we believe that a weaker argument is possible to problematize IAR. Essentially this is that Ahrens and Chapman (2006) seem to rely on Silverman and others to argue that a general theory of IAR can be defined to encapsulate particular characteristics such as would enable the construction of coherent theory and the development of a homogeneous disciplinary knowledge base. We are not convinced that this is what Silverman has in mind in the writings we have discussed but we are certain that such criteria and conceptualizations of social affairs cannot be ascribed to other major tracts of interpretive sociology.

What we know (we know) is that however competent the theories of Silverman (or Schutz) are they cannot be said to produce an interpretive (or qualitative) methodology which somehow subsumes all others. The broad sweep that is characteristic of interpretive methodology is notable for its heterogeneity (Lee and Lings, 2008). Interpretive sociology is marked by its internal divisions and alternate theoretical frames. Its philosophical connections are also marked out by some of the great thinkers of the past two centuries (Laughlin, 1999). Other developments in post-structuralist and postmodern thought have also offered much to interpretive researchers (Kilduff and Mehra, 1997). These disparate philosophical perspectives do not offer a coherent theoretic framework but a cacophony of intellectual traditions. Maybe we should take more care in wishing for coherence and adherence to a poorly sketched out tradition such as set out in recent contributions under the IAR banner. We do know that IAR cannot be defined separately from interpretive sociology since this is where the origins of IAR clearly are. If we want to give part of this community a name it cannot be that of interpretive accounting research for if we fall for this then we insult the traditions from which it obviously draws.

While we may know that interviews (and other forms of data collection) provide only flawed data (De Loo and Lowe, 2012; Silverman, 1997), we must acknowledge their popularity among the qualitative methods available – not least among accounting researchers. Silverman (1997, 2011) certainly argues that interviews often provide only a superficial perspective on social practices. Some of the main promoters of IAR also appear to accept this lack of reliability but then subsequently seem to downplay it by writing of generalizability and the progressive building of knowledge (Ahrens *et al.*, 2008). For us it is this acceptance of fallibility that necessarily distinguishes interpretive sociology. We would reiterate that it is the very acceptance of the partial nature of social science research knowledge that is attractive to many of those who practice interpretive research (Armstrong, 2008; Flyvbjerg, 2001; see also the contribution by Mikes, p. 861 and Scheytt, p. 863, as quoted in Ahrens *et al.*, 2008). This does not mean that research findings cannot be categorized, grouped together, or summarized when conducting interpretive research, based on the former's trustworthiness in the eyes of the researchers and/or readers involved (however they wish to define this term), but it does mean that this can only result in, at best, highly conjectural, temporal groupings and/or summaries, which cannot be seen as a cumulative body of knowledge, in which research results neatly build on one another, thus

establishing overarching ‘patterns’ showing ‘what works’ in organizations (Malmi and Granlund, 2009).

In Table 1, we have tried to summarize the main arguments put forward in this paper. Please keep in mind that we acknowledge that the use of a table prohibits us from making fully nuanced statements about some of these arguments. However, we have tried to achieve this in the earlier sections.

Table 1 Overview of the main arguments expressed in this paper.

Main tenet/issue	The IAR perspective (Ahrens, 2008; Ahrens <i>et al.</i>, 2008; Ahrens and Chapman, 2006)	Alternative perspective proposed in this paper
Interpretive ‘versus’ qualitative research	These authors conflate ‘interpretive’ and ‘qualitative’ when defining research methodology. We argue that using these labels interchangeably lacks precision when referring to the philosophical orientation adopted by the researcher. To subsume qualitative research within IAR introduces potentially serious confusions.	In the research literature it is common and helpful (though by no means universal) for researchers to refer to methods as qualitative or quantitative, while distinguishing methodology as interpretive on the one hand, and positivist on the other ²²
Issues related to being interpretive in the research act: - Distinguishing methods from methodology - Ontological and epistemological standpoints	There is a broad agreement between method and methodology. It appears that method and methodology are locked together in a symbiotic relation. The significance of assumptions regarding knowledge, and background assumptions on the research act (ontology and epistemology) are acknowledged, but are not deemed to be very important when engaging in interpretive research.	‘Disciplinary matrices’ (or paradigms) are critical to the research act. Methods follow in terms of the fit with the philosophical and paradigm choices/selections made by the researcher. The relationship between theory and practice is determined by understandings about knowledge from ‘disciplinary matrices’ (or paradigms).
Development of accounting theory for practice	Coherent knowledge production within a consistent framework is possible. An ability to produce ‘cumulative knowledge’, to generalize across settings and literatures is feasible and can be done. Results from various research traditions can be juxtaposed.	Generalizability is eschewed in most recognized ‘interpretive’ research traditions. ‘Building’ knowledge and/or refining theory cannot be reliably claimed or proven.

²²Although they have not been specifically addressed in this paper, we could imagine the use of ‘phenomenological’ and ‘functionalist’ methodologies instead of ‘interpretive’ and ‘positivist’ methodologies (respectively) in this text.

We have mainly used Ahrens and Chapman (2006) in this paper as an example to reinforce the link between the making of research stories and the interpretation they involve during their making. Being as specific as possible about such interpretations is the very least that researchers can do when they engage in the research act. Taking these issues for granted can have serious consequences for what someone's research can viably achieve – and ultimately, what accounting research as a whole can achieve (apart from achieving popularity among its proponents).

Related debates have been taking place in both the accounting and organization theory literature about more specific developments in interpretive research. One of these developments has involved the growing interest in practice theory and the importance of objects, both separately and as an element of practice theory (Ahrens and Chapman, 2007; Jørgensen and Messner, 2009; Orlikowski, 1996, 2007; Reckwitz, 2002; Schatzki, 1996, 2002, 2010). Another development which has been introduced from organization theory into accounting is an attempt to reposition institutional theory by associating 'supporting' theories which are aimed at bringing more (human) agency into the existing institutional theories (Kondra and Hinings, 1998; Lounsbury, 2007, 2008; Reay and Hinings, 2009). Within these theoretical debates there are significant differences of emphasis on the relative significance of human agency, objectualized agency, and some complex entangled or inseparable conception of these fundamental theoretical conundrums. In true interpretive research traditions such debates over what constitutes the 'social milieu' are not resolvable. We see virtue in developments of this kind since they offer some useful additions to areas where our theorizations and our theories are currently lacking. But these developments, despite their innovative nature and at times associated attempts to reconcile perhaps intractable theoretic differences, will not move us to a place where talking and working across 'disciplinary matrices' or paradigm boundaries becomes commonplace (see also Guba, 1990). Indeed, many of these advances in theory are likely to make such communication more difficult rather than less difficult, even though such communication is certainly possible and could lead to a form of agreement or conciliation between researchers and the research traditions they adhere to (Sunderlin, 2003; Willmott, 1993).

6. Concluding remarks

While we find some aspects of IAR as expressed by Ahrens (2008) and Ahrens *et al.* (2008) alluring and appealing (like the delineation of a community, building theory in particular subject areas, working toward a coherent theoretical base, and ‘building’ new knowledge), we do not accept that such objectives can be based on concepts and theory informed by interpretive sociology (or for that matter, the various strains of postmodern and post-structural thought to which accounting researchers have had recourse over the past 30 years). If we wish to build a more coherent research community we believe it is inevitable that we become more reflexive about the theories we employ and the interpretations we offer from our research experiences – and accept the consequences of this (De Loo and Lowe, 2012). Most importantly, we assert that we must be cognizant of the limitations and constraints of our theoretical, methodological and philosophical frameworks. While it may be possible to talk across (alleged) boundaries between different strands of research or ‘disciplinary matrices’ (Chua, 1986; Guba, 1990; Lukka and Modell, 2010; Lukka, 2010; Modell, 2009), that does not mean that philosophical and methodological commensurability can be assumed or that it should be ‘automatically’ embraced (De Loo and Lowe, 2011; Guba, 1990).

While we should seek to find areas of common ground across different ‘disciplinary matrices’ (or paradigms) that does not mean that some form of grand theoretical reconciliation is possible. Generalizability is clearly a problem in many interpretive research traditions. Finding the ‘holy grail’ of what might realize this has been abandoned in most of the social sciences in favor of an acceptance of the frail and unpredictable nature of human communities and systems of social arrangement (Flyvbjerg, 2001). In this paper we have sought to exemplify some of the latter views by discussing developments in the ethnographic literature.

One of the advantages of interpretive research is its heterogeneity and its broad acceptance of how little we can ever know about our social and material surroundings and how they interact to produce the environment in which we act. So what is encouraging about interpretive methodology is its humble rather than audacious predictive statements. Much interpretive methodology is careful to point out what we cannot know with certainty. In contrast, much positivist terminology hypes what we can know and predict and applies methodological restrictions to hide what cannot be known behind oftentimes convenient simplifying assumptions. There are of course good

reasons for conducting positivist or post-positivist research but these cannot be the same reasons and cannot be pursued in the same way within interpretivist methodology. The objectives of the two research traditions are far apart (DeLoo and Lowe, 2011) so that we cannot expect to build on prior knowledge to advance some grand scheme of understanding. It breaks with most of the philosophical underpinnings of interpretive research to suggest that we can even tell whether we know more or less than we did in prior times. We can of course suggest that we know different things and our stories may sometimes present statements in a relatively strong manner, that might seem convincing. But these statements are usually about what we know about a specific setting, 'field', 'contact zone', or 'site', and are not typically generalizable with any confidence across sites or across time.

What is possible will inevitably be limited. Perhaps one of the few viable questions one can ask when engaging in interpretive research is whether the research results can be used, in one way or another (for instance, as a source of inspiration), in another context. The notion of generalizability would then be replaced by a notion that focuses on the transferability of research findings, based on the researchers' and/or readers' own points of view. On top of this, engagement with other methodological traditions is time-consuming and intellectually demanding, which is why it is so infrequently achieved, or left at a superficial level. Researchers experience great difficulty in mixing broad research styles and approaches well (Grafton, Lillis and Mahama, 2011)²³. We firmly believe that accounting research may get into serious trouble if at times very complex concepts from other disciplines are combined into theoretical and/or methodological constellations, seemingly at will. This will be a significant risk when these 'constellations' are subsequently accepted as the way to proceed in academe without receiving much further probing from fellow researchers. Accounting researchers may then ultimately end up being able to talk only to other accounting researchers.

There appears to be no convincing evidence of a research community in the sense defined currently as IAR, which believes in and shares some common conceptual principles that could bind it into a substantial and coherent whole. The somewhat adventurous polyphonic debate in

²³ Some authors frame 'mixed methods research' in terms of combining quantitative and qualitative methods, whereas others frame it in terms of combining interpretive and positivist methodologies. These are quite different approaches (De Loo and Lowe, 2011). These differences are similar to what we argue about Ahrens and Chapman's (2006, 2007) work.

accounting did not achieve anything like a melodic harmony but demonstrated some of the notable conceptual differences among a group of *qualitative* accounting researchers (Armstrong, 2008; see also Lillis, 2008; Richardson, 2012). The debate produced, in spite of the claims of ‘adjudicators’, a cacophonous contribution able to exhibit only limited commonality. This heterogeneity, we assert, may in fact be one of the most important strengths of interpretive research. Many in the community of interpretive accounting researchers – including some of those represented in the polyphonic debate – appear to celebrate the differences across the broad philosophical and theoretical traditions to which they subscribe, and like seeing their contribution as being tentative and faceted. These researchers would not (have to) claim to be building cumulative knowledge nor producing generalizations of some kind, or deem this the way to go in future research.

Postscript

We have tried hard to reduce confusion in our paper but we must admit the inherent difficulty in maintaining a balance in the use of the terms ‘qualitative’ and ‘interpretive’. What we have tried to do is retain where possible the term as used by the original author. Often we do this by quoting them. The outcome is nevertheless problematic. In itself this is however part of our argument. Most authors, with whom we are familiar, use the term ‘qualitative’ as an umbrella descriptor to refer to the deployment of qualitative methods. ‘Interpretive’ is a more restrictive label, we believe to refer to a philosophical perspective. Interestingly, we would also note that, leaving aside journal titles, our references contain 16 instances of the use of ‘qualitative’ (often to ‘qualitative research’) while there are only 7 to ‘interpretive research’ – 5 of which relate to the IAR-debate in accounting.

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