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Abstract

In critical accounting research an understanding is that theories are looming large in developing accounting theory. Although there has been a wide range of critical studies conducted using alternative theoretical stances and strategies, little is known about the processes of researching accounting-in-action within the critical studies in accounting research by informing a particular theory of interest. An implicit understanding of this research tradition is that "ethnography" can be used as a methodical discretion for field works. But it cannot be assumed that ethnographic studies in accounting research are based on some homogeneous set, rather they are conducted using differing theoretical perspectives - some are positivistic and others are not. This paper aims at reflecting on a theoretical framework in developing accounting theory as well as making sense of the doing of research on accounting-in-action, particularly using the works informed by Bruno Latour and his colleagues.

Introduction


One of the main foci of these critical studies in accounting research is striving for a more self-reflexive and contextualised accounting literature which recognises the interconnections between society, history, organisations, accounting theory and practice, to an extent not previously contemplated. Not only are critical accounting researchers critical of conventional accounting theory and practice of the "functionalist" (cf Burrell and Morgan 1979), they also are critical of the alternative approaches which they have advanced, that is, they do not consider all these approaches as some homogeneous set (see Laughlin and Lowe 1990, p.35).

Despite the diverse range of expressions as to the theoretical underpinning necessary for understanding and relating accounting theory to practice amongst the critical accounting researchers, a common feeling for accounting research is that the 'theoretical considerations have loomed large' in developing accounting theory as well as the doing of research. In this sense, embarking on a major study is dependent upon the way in which the researcher (a) resolves the theoretical and epistemological disputes in the 'doing of research' and (b) relates this to the analyses at the action-orientation level, that is, at the level of empirical investigation'.
Although much has dealt with the meta-theoretical linkage of how accounting research is related to practice, very little is known about the processes of researching accounting-in-action within these critical studies in accounting research. This paper aims to further develop an understanding for such processes.

The organisation of the paper is as follows. The paper begins with questioning whether 'field study' can be used as a common banner in researching accounting-in-action. The paper questions whether there exist differences in the application and usage of the 'case study' method in accounting research; some are positivistic and others are not. The paper then addresses 'ethnography' as a 'methodical discretion' for field research. Despite entering and settling various riddles of such a methodical discretion - ethnography, the paper then attempts to draw attention to some of the recent non-positivistic traditions of 'ethnography' writings and their theoretical approaches with special reference to the work of Bruno Latour and his colleagues (Latour 1987, Callon et al, 1986), which investigates "science-in-action". The paper then reflects on the research processes and the theoretical significance thereof specific to a study recently conducted in a large multinational company (BHP-SPPD)\(^1\) concerning the development of integrated cost management and other business systems using the SAP\(^2\) system.

**Is Field Study a Common Banner in Action Research?**

There are various nomenclatures for "field study". It is used synonymously to refer to a style of investigation such as "field work", 'qualitative method', 'interpretive method', 'case study', and 'ethnography'. Despite the existence of the different labels, the epistemological foundation of a field study approach "has traditionally been associated with social anthropologists whose 'field' consisted of a small-scale society where it was possible to do 'research' by living and working among the people" (Burgess 1982, 1) or, the "peculiar practice of representing the social reality of others through the analysis of one's own experience in the world of these other" (Van Maanen 1988, ix). These views suggest that the main instrument in field study research is the researcher himself or herself, who has to learn the local languages (if necessary), live among the people and participate in their activities over a relatively long period of time.

Burgess (1989) argues that field research cannot be fitted into a linear model of steps or stages, for a field researcher has to cope with a variety of social situations, perspectives and problems. Doing field research, therefore, according to Burgess, is not merely the use of a set of uniform techniques but depends on a complex interaction between the research problem, the researcher and those who are researched. To him it is on this basis that a researcher is an active decision-maker who decides on the most appropriate conceptual and methodological tools that can be used to collect and analyse

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\(^1\) SPPD stands for Slab & Plate Product Division and BHP stands for Broken Hills Proprietory Ltd. BHP-SPPD is major steel division which produces a range of steel products, the plant is located at Port Kembla near Wollongong, Australia.

\(^2\) SAP stands for Software, Applications, Products Ltd, a German based commercial software developer for a wide range of data processing and business applications.
data. "Field research is concerned with research processes as well as research methods. Field research methodologists, therefore, have focused on issues involved in starting research, gaining access, selecting informants, and handling ethical problems as well as collecting, analysing and reporting data" (Burgess 1989, 6).

Schatzman and Strauss (1973, 14) argue that "field method is more like an umbrella of activity beneath which any technique may be used for gaining the desired information, and for processes of thinking about this information". Field study research covers a diverse range of methods, strategies and tactics (Burgess 1982). It is the researcher who has to consider ways in which different methods can be used in collecting data and then to address a variety of theoretical and substantive research issues. The literature on the methodical prescriptions of field study is vast; there is extensive literature for conducting field study, such as how to obtain a 'way in' or get access, how to go about field research, and so on. Although much has been written about how to conduct field study, relatively little is available in the accounting arena on how to represent the outcome of a field study addressing a particular (non-positivist) theoretical perspective.

Burgess (1982) argues that the theoretical framework is of paramount importance, as this will influence the questions that are posed and the data collected by the field researcher. He further argues that the data that is gathered by the field researcher is shaped by the themes that emerge during the investigation. This is possible if researchers consider themselves as "travellers" rather than 'tourists' (see Preston and Mouck 1993). To be a traveller, an essential element is a training in sociology and anthropology. Preston and Mouck (1993) argue that the researchers as travellers enter into the research site with very little understanding about its reality.

There has been a plethora of accounts now available in accounting research, of the methodological prescriptions of "field studies" (cf Ryan et al. 1992; Ferreria and Merchant 1992; Booth 1991; Kaplan 1983, 1986). These researchers also use the term "case studies" to refer to "field studies" with the exception of Robert Kaplan whose preferred label is case study. Few of these studies recognise that their accounts of methodological prescriptions have anthropological or epistemological foundations. Using Pratt's (1992) rhetoric - 'contact zone', Preston and Mouck (1993, 5) argue that "the case study (research) like the tourist site becomes a kind of 'contact zone' between two research cultures (paradigms)". They further argue that "the case study researcher's home lies in the realist ontological view of accounting and organisational phenomena and rests upon the principles of empiricist natural science research" (Preston and Mouck 1993, 6). There emerges a tense set of relationships within which they call a 'contact zone' of the two research 'cultures', that is, positivistic case study and ethnography.

According to Preston and Mouck (1993), certain groups of accounting researchers (ie, Kaplan 1983, 1984, 1986; Ferreria and Merchant 1992) deliberately (ignorantly may be a proper word) reconstitute case study research as another form of positivist research, without synthesising the epistemological foundations or roots of case study research. They (Preston and Mouck 1993)
suggest an analogy, 'those who subscribe to these views on science [case study research] feel more or less at home'. Booth (1991, 128) argues that "positivistic 'case study' researchers tend to view 'case studies' as either illustrative of 'good' accounting practices or exploratory in that they provide a more grounded basis for future model building and hypothesis formation". This view flows from the unquestioning acceptance of neoclassical economics based assumptions about the role of accounting in organisations (Scapens 1990). But, according to Preston and Mouck (1993), ethnographic studies are premised upon a very different set of methodological, epistemological and ontological assumptions.

Most, if not all, accounting academics who suggest that the case study(ies) is (are) another form of research, have ignored its founders' discipline - anthropology. Anthropologists or ethnographers have spent over a century accounting for it, not only linking its modality - 'ethnography' (either classical or new form) - to theoretical and epistemological underpinnings but also to a range of representational styles, vocabularies, and rhetoric. Despite the fragmented diversity amongst anthropologists (both classical and new) however, "what seems to define the centre in this eclectic time is the ongoing experimentation with semilitary genre of anthropological discourse - the ethnography - which is where the locus of intellectual energy in the discipline now seems to be" (Marcus 1986, 5). Thus, in the following, attention is drawn towards this discourse on 'ethnography'. However, the intention here is not to analyse or answer the various riddles of differing sectional interests within anthropology about ethnography. Rather, a major interest is to draw attention to some contemporary (non-positivistic) 'ethnographic' works, especially that of Latour (1987) and Latour and Woolgar (1979) and some of the works of their colleagues.

**Ethnography - A Methodical Discretion**

Within anthropology, in recent times, ethnographic field-work and writing have become the most lively current arena for theoretical discussion and innovation. "Ethnography's concern is with description, and present efforts to make ethnographic writing more sensitive to its broader political, historical, and philosophical implications place anthropology at the vortex of debate about the problem of representing society in contemporary discourses." (Marcus 1986, vii)

There are many critical views of 'ethnography' amongst ethnographers. Selecting any authority on ethnography is a difficult job because there exists such diversity in ethnographical writings and theoretical frameworks. However, before drawing attention to the works of technoscientists, that is - the work of Bruno Latour and others, 3 an understanding of 'ethnography' by Clifford Geertz, who is considered to be modern but with some remnants of a "past fashion" ethnographer, is advanced. The consideration of the work of Geertz and his account of ethnography is to point out that there

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3 A selection of the works of Latour and his colleagues for enhancing a reflective understanding of the doing of research on accounting-in-action is undertaken based on the recent claims introduced by accounting researchers (cf Robson 1991, 1992; Preston et al 1992, Boomfield et al 1992, Chua 1993)
may exist tension amongst ethnographers (both modern and post-modern) in respect of what might constitute "ethnography".

*An Understanding of Ethnography by Geertz*

In *Works and Lives: The Anthropologist as Author*, Geertz (1989, p.133) writes, "[i]ndeed, the very right to write - to write ethnography - seems at risk". Geertz (1989, p.16) argues that "[e]thnographers need to convince us not merely that they themselves have truly "been there", but (as they do, if rather less obviously) that had we been there we should have seen what they saw, felt what they felt, concluded what they concluded". He argues that "getting themselves [ethnography writers] into their text (that is, representationally into their text) may be as difficult for ethnographers as getting themselves into the culture (that is, imaginatively into the culture)" (p.17).

Geertz writes that "[t]he transformation, partly juridical, partly ideological, partly real, of the people anthropologists mostly write about, from colonial subjects to sovereign citizens, has altered entirely the moral context within which the ethnographical acts takes place" (1989, p.132). He argues further that "[o]ne of the major assumptions upon which anthropological writing rested only until yesterday, that its subjects and its audience were not only separable but morally disconnected, that the first were to be described but not addressed, the second informed but not implicated, has fairly well dissolved" (p.132). Thus, he goes on to argue that "the moral foundations of ethnography have been shaken by the decolonization on the *Being There* side, its epistemological foundations have been shaken by a general loss of faith in received stories about the nature of representation, ethnography or any other, on the *Being Here* side" [italics are added] (p.135).

In a chapter "Being There", Geertz begins his book, *Works and Lives: The Anthropologist as Author*, with the following paragraphs:

The illusion that ethnography is a matter of sorting strange and irregular facts into familiar and orderly categories - this is magic, that is technology - has long since been exploded. What is instead, however, is less clear. That it might be a kind of writing, putting things to paper, has now and then occurred to those engaged in producing it, consuming it, or both...

What a proper ethnographer ought properly to be doing is going out to places, coming back with information about how people live there, and making that information available to the professional community in practical form, not lounging about in libraries reflecting on literary questions. Excessive concern, which in practice usually means any concern at all, with how ethnographic texts are constructed seems like an unhealthy self-absorption - time-wasting at best, hypochondriacal at worst...

Another objection, here coming mostly from the consumer side, is that anthropological texts are not worth such delicate attention... Good anthropological texts are plain texts, unpretending. They neither invite literary-critical close reading nor reward it.

But perhaps the most intense objection, coming from all quarters, and indeed rather to intellectual life these days, is that concentrating our gaze on the ways in which knowledge claims are advanced undermines our capacity to take any of those claims seriously. Somehow, attention to such matters as imagery, metaphor, phraseology, or voice is supposed to lead to a corrosive relativism in which everything is but a more or less clever expression of opinion. Ethnography becomes, it is said, a mere game of words, as poems and novels are supposed to be. Exposing how the thing is done is to suggest that, like the lady sawed in half, it isn't done at all. (1989, pp.1-2)
Geertz (1989) is also careful to state that "these above views are unreasonable, because they are not based on the experience of threats present and actual, or even looming, but on the imagining of possible ones that might occur were everything to be suddenly otherwise than it now is" (p2).

To Geertz, "[a]nthropologists are possessed of the idea that the central methodological issues involved in ethnographic description have to do with the mechanics of knowledge - the legitimacy of "empathy", "insight", and the like forms of cognition; the verifiability of internalist accounts of other peoples' thoughts and feelings; the ontological status of culture. Accordingly, they have traced their difficulties in constructing such descriptions to the problematic of field work rather than to those of discourse. If the relation between observer and observed (rapport) can be managed, the relation between author and text (signature) will follow - it is thought - of itself." (Geertz, 1989, pp9-10)

Geertz is also sceptical about defining ethnography in terms of techniques such as establishing rapports, transcribing texts, taking genealogies, mapping fields, keeping diaries. Geertz argues that it is not these things - techniques and received procedures - that define the enterprise of anthropological ethnography. Rather, what defines it, as argued by Geertz (1973, p6), is the kind of intellectual effort to come up with "thick description". According to Geertz, it is by representing this "thick description" that an ethnographer enables his/her audience to appreciate the art and rhetoric, the art and tactical creativity of actors in their various manipulations.

Geertz argues that

...ethnographies tend to look at least as much like romances as they do like lab reports (though, as with our mule, not really like either), two questions, or perhaps the same one doubly asked, immediately pose themselves: (1) How is the "author-function" made manifest in the text? (2) Just what is it - beyond the obvious tautology, "a work" - that the author authors? The first question, call it that of signature, is a matter of the construction of a writerly identity. The second, call it that of discourse, is a matter of developing a way of putting things - a vocabulary, a rhetoric, a pattern of argument - that is connected to that identity in such a way that it seems to come from it as a remark from a mind. (1989, pp 8-9)

Despite the above views by Geertz, there is a wide range of interpretive research which uses 'ethnography' as a methodical discretion in conducting and representing (writing up) 'field work'. Such studies are based on differing theoretical perspectives. An example of such an account is that by the technoscientists, especially that of the work of Bruno Latour and his colleagues. It is claimed that the most promising contributions of the works of Bruno Latour and his colleagues are not only concerned with the methods for production of passive human agents but are also concerned with the methods for 'reduction of discretion' - that is, 'interpretive flexibility' (cf. Law, 1986). As intended, therefore, in the following, attention will be drawn to some of the work of Latour (and his colleagues) who has attempted to investigate 'science-in-action' (Latour, 1987), and has shaken not only the classical position of the ethnographer by writing "critical ethnography" but has also advanced some theoretical and methodological underpinnings for studying 'science-in-action'.

* Although the adjective 'critical' to critical ethnography has been coined by Power (1991) in the accounting literature, it is Chua (1993) who introduces the terminology to a Latourian literature. Perhaps an implicit objective of this introduction is to distinguish other forms of...
Technoscientists' Approaches to Studying 'Science in Action'

The term 'technoscience' is coined by Latour to emphasise his association with, and convergence on, a sociological tradition known as 'science and technology studies'. The most prominent authors within this area of 'science and technology studies' include: Donald Mackenzie, Judy Wajeman, Steve Woolgar, Trevor Pinch, Wieber Bijker, Thomas Hughes, John Law, Michel Callon and Bruno Latour.

"There is a wide range of studies of technology and its relationship to society" (Preston et al, 1992, 563). However, the work of Latour (1987) and Latour and Woolgar (1979), in particular their use of rhetorical vocabularies and 'reduction of discretion' in conducting and representing a 'field work' on 'laboratory life', is worth considering. Because these works, and that of Callon (1986) and Law (1986) and their colleagues at the Ecole Des Mines in Paris, have not only shifted the direction of imagining the shaping of 'science (facts) and technology (artefacts) per se, but also have advanced various rhetoric, 'language sets' and approaches including the concept of 'actor-networks' or 'sociology of translation', and various 'methodical discretions' in investigating and representing 'science-in-action' as opposed to 'ready made science'.

Latour (1987, 29) argues that "the construction of facts and machines is a collective process". "This collectivity is a network of actors, each playing a part in the unfolding of events" (Cockburn 1992, 33). In other words, it can be argued that through a constitutive process this collectivity treats every process of action-orientation as a learning process, thus allowing for a reproduction of the 'lifeworld' in a more conscious way and becomes enabling. Obviously, such a constitutive process is dependent on the stock of knowledge that the collectivity (actor-networks') possess at particular points in time and space. It is from this cultural background that the participants supply themselves with the requisite impetus for communicating amongst allies with interpretations as they come to an understanding about something [both facts (science) and artefacts (technology)] in the world.

The technoscientists' focus on science is from two different orientations - the 'technological determinism' and the 'social', which they relate with two different models, that is, "the diffusion model" and "the translation model". To focus on these orientations, Latour (1987, 4) draws on two distinctive types of science one of which is 'ready made science' and the other is 'science in the making' - which he refers to as a 'two-faced Janus' of science. This is reflective of his comparison between the diffusion model and the translation model. Latour argues:

It appears that power is not something one can possess - indeed it must be treated as a consequence rather than as a cause of action. In order to explore this paradox a diffusion model of power in which a successful command moves under an impetus given it from a central source is contrasted with

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*ethnography*. That is, to establishing a non-positivist methodical discretion, which should provide a much more interpretive flexibility in representing the others (organisation of social life) manipulations as well as should play a distinct role (in regard to tactics, tribulations, tools, rhetoric, etc.) in representing contextual reality in a self-reflexive manner. The term is used here with such an aim.

5 Only human actors, not the non-human actors.

6 There are inputs on this translation model from other 'technoscientists' as well, such as Michel Callon and others.
translation model in which such command, if it is successful, results from the actions of a chain of agents each of whom 'translates' it in accordance with his/her own projects. (1986, 264)

Latour relates this diffusion model to the 'inertia' principle of physics. He states, "according to the inertia principle token [objects] will move in the same direction as long as there is no obstacle" (267). According to Latour, within this 'diffusion model' there are three elements which spread objects through time and space: "the initial force that triggers the movements and which constitutes its only energy; the inertia that conserves this energy; and the medium through which the token circulates". In contrast to the diffusion model, Latour (1986) argues that in the 'translation model', first, 'there is no inertia account of the spread of a token'; and second, the 'social action' is seen as a continuously 'transformative' process. To support this, Latour expounds that:

When no one is there to take up the statement or the token then it simply stops... The initial force of the first in the chain is no more important than that of the second, or the fortieth, or the four hundredth person... If you want the token to move on you have to find fresh sources of energy all the time; you can never rest on what you did before, no more than rugby players can rest for the whole game after the first player has given the ball its first kick. (1986, 267)

Thirdly, he proclaims that the chains of actors are actively participating (performative rather than ostensive) in the shaping of facts and artefacts. He writes:

Each of the people in the chain is not simply resisting a force or transmitting it in the way they would in the diffusion model; rather, they are doing something essential for the existence and maintenance of the token. In other words, the chain is made of actors - not the patients - and since the token is in everyone's hands in turn, everyone shapes it according to their different projects. (Latour 1986, 268)

Law (1986, 17) argues that in the translation model the concept 'power' is treated as a composition, that is, "the composition of a set of actors who are temporarily enrolled in the schemes of the powerful and who accordingly lend their efforts to his/her project". One of the central leitmotifs of this translation model is that there is not a background, a determinant social structure that may be observed by social scientists. Rather, 'what may be observed are sets of different people trying to define the nature of social structure, and then trying to persuade others to subscribe to that definition' (Law 1986). Thus, this school (the constructivist school of science and technology studies) advances a methodological corollary that "social scientists should stop trying to determine the nature of social structure that they believe generates these conflicts, and instead treat the latter as data". In other words, "society is not seen as the referent of ostensive definition, but rather is seen as being performed through the various efforts to define it" (Law 1986, 18).

Latour (1987) suggests that we should study 'science in the making' or 'science-in-action' rather than 'ready made science'. In adopting Latour's (1987) approach, Preston et al (1992, 264) note that "Latour suggests that in order to better understand the nature of technology we should examine the processes involved in its fabrication. In this respect scientific facts and technical artefacts, for example machines, are not viewed as being part of a pre-existing natural order, simply waiting to be discovered by the people in academia and in the commercial world. Rather, they are the result of an elaborate process of fabrication." Thus, according to Latour, the "facts and artefacts (technology) continually changes shape and content as alliances are stitched together to achieve it" (Cockburn
1992). Latour, therefore, is of the opinion that 'science and technology' are socially constructed. This conforms with his (Latour 1987, 259) inculcation of principles, especially, his first and third principles, as follows:

First principle The fate of facts [science] and machines [technology] is in later users' hands; their qualities are thus a consequence, not a cause, of a collective action.

Third principle We are never confronted with science, technology and society, but with a gamut of weaker and stronger associations; thus understanding what facts and machines are is in the same task as understanding who people are.

Referring to the cybernetic literature, Latour (1987) introduces a concept called the 'black box' to refer to a piece of machinery or a set of commands whenever it is too complex. According to Latour (1987), to build a 'black box', whether this is a theory or a machine, it is necessary to enrol others so that they believe it, take it up and spread it. The control of the builder is therefore seldom absolute. "The new allies shape the idea or artefact to their own will - they do not so much transmit as translate it" (Cockburn 1992, 34). Thus, the concept of sociology of translation is determined

As mentioned earlier, there have been inputs into the concept of 'sociology of translation' or actor-network approach by other technoscientists such as Michel Callon and his colleagues (cf. Callon 1986, Callon et al. 1986). For example, according to Callon (1986), the 'actor-network' or 'translation' approach, as noted by Law:

... is based on the assumption that as actors struggle with one another they first determine their existence and then (if that existence is secured) define their characteristics. An actor that exists is thus one that is able to exert itself upon others. It attempts the latter by borrowing the force of others in a process that Callon calls 'translation'. This process involves four stages. First, an actor tries to make itself indispensable to others - to force them to come to it. Having done so - it moves to a second stage - called by Callon 'interessement' - in which it attempts to lock these others into place by coming between them and their alternatives. It is at this stage that discretion is removed and the actors so trapped become authorities in the sense defined by Barnes. The third step involves both the definition of the roles that are to be played by these 'authorities' and the way in which they are to relate to one another in the scheme devised by the principal actor. This process, which Callon calls enrolment, thus involves the generation of a network of passive agents that may, for all intents and purposes, be seen as forming part of the actor in question (hence the term 'actor-network'). Finally, the actors borrow the force of the passive agents that it has enrolled by turning itself into its spokesman and talking on their behalf. Callon calls this part of the process mobilisation...(1986, 15-16)

Latour argues that 'there are many methods for studying the fabrication of scientific facts and artefacts'. In the introduction of the volume Science In Action he states that:

... we will not try to analyse the final products, a computer, a nuclear plant, a cosmological theory, the shape of a double helix, a box of contraceptive pills, a model of economy, instead we will follow scientists and engineers at the times and at the places where they plan a nuclear plant, under a cosmological theory, modify the structure of a hormone for contraception, or desegregate figures used in a new model of economy... Instead of black boxing the technical aspects of science and then looking for social influences and biases, ... be there before the box closes and becomes black...

To start our enquiry, we are going to begin from the simplest of all possible situations: when someone utters a statement, what happens when others believe it or don't believe it. Starting from the more general situation, we will be gradually led to more particular settings.

Throughout the volume Science In Action, Latour (1987) delivers seven rules for methods of studying 'science-in-action'. For example, two of these rules are as follows:
Rule 1  We study science *in action* and not ready made science or technology, to do so, we either arrive before the facts and machines are blackboxed or we follow the controversies that reopen them.

Rule 2  To determine the objectivity or subjectivity of a claim, the efficiency or perfection of a mechanism, we do not look for their *intrinsic* qualities but all the transformations they undergo *later* in the hands of others. (Latour 1987, 258)

Using Latour's rules for methods and his principles, there are attempts to relate accounting theory, practice and the doing of research. This is to what we will turn in the following. However, it should be mentioned that the above summary of a Latourian approach may not do justice to all his and the related works of his colleagues, but such an *eclecticism* can bring a rich research tradition in accounting research.

**Accounting Research, Ethnography and a Latourian Approach**

In the accounting literature, from a non-positivistic research point of view, there are attempts to use 'ethnography' as a 'method of discretion' in investigating and writing up (representing) micro level 'field works' (see Berry et al, 1985; Dent 1990; Laughlin 1988, Preston et al, 1992; Broadbent et al, 1991; Chua 1993). More recently, there have been attempts to write ethnography particularly using the works informed by Latour and his colleagues. It is claimed that the works informed by this authority portray a range of 'tactics, tools and tribulations' (Chua 1993), 'rules of methods', 'mode of representations' for investigating and representing the creations and fabrications of 'the doing of accounting' (Robson 1991, 1992; Preston et al, 1992; Chua 1994) at a micro-organisational context.

Robson (1991, 550) relates the concept of 'the sociology of translation' to understand the processes through which accounting and the social can be interrelated. In conceptualising and relating this translation model to the accounting changes, Robson (1991, 550) argues that the "translation will refer to the process through which often pre-existing accounting techniques, and their associated roles, are articulated discursively, in ways that construct individuals' and groups' *interest* in those techniques, and may subsequently provide motives for producing changes in accounting" [emphasis added]. According to Robson (1991, 566), the concept of translation can be seen as a construct for understanding the specific associations, connections or "positive" relations that are made between accounting and its social context. Thus, Robson (1991, 566) urges that "in examining accounting change, it is necessary to attend to the process through which particular accounting statements, calculations and techniques are subject to a translation into wider social, economic and political discourses not normally associated with the apparently neutral, technical discourse and practices of accounting".

Preston et al (1992) also use Latour's (1987) various concepts in carrying out and writing up the 'field-work' of the budgeting fabrication processes at the NHS (National Health Services) in the UK. They advocate that:

*Our investigation of the fabrication of budgets was particularly informed by three guidelines from Latour's [1987] rules of method. Firstly, we chose a controversial accounting and budgeting technology to facilitate the identification of alternative possibilities. Secondly, we mapped networks of resource,
support and use, both historically and across conventional organisational boundaries, in order to examine the multiplicity of people involved in the fabrication process. The third guideline was to attempt to be present in the fabrication process before the black box is closed and debates have died down. (Preston et al., 1992, 567)

In their conclusion, Preston et al (1992, 590) urge that "[j]ust as the study of science has moved into the laboratory (Latour and Woolgar, 1979) so it may be fruitful if students of accounting in action also examine the practices and discourse of management consultants, systems analysts, software engineers and designers, and accountants involved in fabricating accounting and budgeting systems".

Latour (1987) argues that "there is no way of tying together interested groups (people) unless 'things' are tied with them". To Latour, 'things' can be represented by such aspects as "machinations" and "inscriptions". In this sense, as advanced in Lodh (1994), it is argued that an organisational internal control system is no more than tying together people (employees) within and across various functional areas with "things" (namely, operations, machines and inscriptions). Not only can such tying be referred to as the interaction between 'machines' and 'people' but also as interactions of 'people with people' or 'things with things' or 'machines with machines'. Depending on the size and the environment in which the organisation operates such tying can vary from a simple to a very complex interaction (cf Porter 1985). At a very general level such a model can be presented as follows.

**Figure 1**

![Tying People and 'Things'](image)

Following this simple analogy, an accounting student in action may raise an interested question: How has the change management of an organisation attempted to tie up "things" (ie, machines and inscriptions) to the interested groups? For example, in a manufacturing concern such groups may include Accounting and Finance, Supply, Maintenance Engineering, Engineering, Human Resources, Information Technology, Health and Safety, and Production departments. There is a plausibility of many debates taking place to determine the shape, functions and costs of implementing any information system, including accounting systems, in such organisations. In addition, there are numerous "machinations" and "inscriptions" may be involved with installing and/or developing information technologies. Not only these machinations and inscriptions can be seen a part of the fabrication process, but also there is a need to understand people and change management issues in understanding accounting systems in an organisational context. At a general
level, accounting students in action, thus, can urge and develop many research questions by emphasising the idea that very little is known about the design and implementation of large, complex computer based integrated accounting and information systems.

Chua (1993) also applies the 'translation' or 'network' approach in writing a 'critical ethnography' of the fabricating processes in the implementation of case-mix DRG system (a cost accounting technology) at three public hospitals in Australia. Chua (1993, 9) raises the question: 'why use their [Latour and his colleagues] work as a signboard to write a piece of critical ethnography about the fabrication of accounting knowledge?'. In response she argues the following:

Firstly, the making up of new accounting numbers and the battle to secure their legitimacy may be seen as being similar in important respects to a scientific controversy. Like these controversies, the birth of an accounting may change the map of organizational 'reality', challenge existing work traditions, and unfold battle-like, with opposing supporters and detractors who are intent upon vanquishing each other.

Secondly, Latour's sociology of translation does not begin with the simplistic, positivistic assumption that a particular science or technology (or set of accounting numbers) is rationally accepted because it more accurately represents reality...

Thirdly, the work of Latour and Callon draws attention to the persuasive power of non-human resources such as visual inscriptions, academic texts and 'centres of calculation' (Latour, 1988a). Papework such as formulae, graphs and charts are argued to possess many rhetorical advantages: they are mobile, immutable, recombining and are perceived to be built on many 'facts'. Most important of all, inscriptions make 'black boxes' visible. (Chua 1993, 9)

Despite the above views, Chua (1993, 10) argues that "[u]seful though the sociology of translation is, it is not without ambiguity or weakness". By considering some counter accounts of Shapin (1988) and Barnes (1981), she advances a critique of Latour's actor-networks approach namely that Latour (1987) did not see interests of actors (people) [which was related to Latour's fourth rule of methods,' that society should not be separate from science] as being a theoretically predetermined class structure of capitalistic societies. Preston et al (1992) advance a sceptical view of the absolute following of Latour's first rule of methods, that is - 'we should arrive before the facts and machines are blackboxed'. They proclaim that it is not possible to arrive before all the important events are impacted in the fabrication of facts and technology, "some judgement of the historical context is unavoidable".

In most of the works of Latour, it is implicit that to him 'power' is something like a capacity or effectiveness, which does not accommodate any other forms or meanings of 'power' - such as 'domination' or 'coercion' or so. In other words, his representation of 'power' has a lack of concern for accounting the 'intersubjective communicative subjectivity'; also, "there is an incomplete representation of the historical dimensions of power" (see Cockburn 1992, Clegg 1989).

Latour's rhetoric and vocabularies may facilitate us to redirect "the doing of research" towards "traces and inscriptions" of "the doing of accounting". Rix (1991, 3) contends that "collapsing

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7 Rule 4 Since the settlement of a controversy is the cause of society's stability, we cannot use society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enrol human and non-human resources.
humans and non-humans into each other aids in the reversal of forces, the inversion of orders of magnitude, and a corresponding alteration of scale, all so necessary to invest with some validity Latour's hopes for the possibility of reform in or of the social world". However, Rix (1991) advances the criticism of Latour that it is "more surely not the case that we can avoid or deny the importance of the ideological and rhetorical roles of those 'traces and inscriptions' which have been decided within large power structures by the powerful social forces which occupy and control them". He further argues that "Latour's abolition of the distinction between human and non-human is based on a reconstitution and blending, even merging, of the entities cohabiting in the social world". Schuster (1991, 18) argues that "Latour empties his explanatory space of 'contextual' forces and structures, and so he also thereby necessarily empties his key actors (Princes, entrepreneurs, innovators) of any internal socio-cognitive texture". Schuster further argues that "in the end, one is left with rational, clear sighted heroes who participate in inherently whiggish 'just-so' stories of triumph, in which everybody instantly recognises 'hard' facts, 'too large' costs, and 'too many' allies, and judges, acts and enrols accordingly". (p18)

In addition, Latour's model might assume that all contenders make essentially the same evaluations and judgements of the state of play in the agonistic moment or rhetorical situation. 'Latour is [also] limited in considering so many social foci' (Rix 1991). Some may argue that in applying a Latourian approach there is a possibility of reducing the actors' social world with merely just the 'traces and inscriptions', which may become closest to the canonical positive association. Therefore, why use Latour's concept? Is it for a 'methodical discretion' or mere techniques or his rhetoric? The question is not an easy one to answer, at least to answer in this short space and time. Nevertheless, Latour makes many shrewd observations. He is correct, for example, "in insisting that the status and fate of a fact as a fact is entirely in the hands of subsequent users; or in his observation that since facts and machines have no inertia, their stability over rounds of usage require explanation every bit as much as would their alternation, rejection or renegotiation" (Schuster 1991, 18).

Some may argue that Latour has no agenda for a research program on how to deal with the interrelationships within state, economy and society other than the micro laboratory situation. Schuster (1991, 24) argues that, to Latour, "state, economy and society are crystallised products of earlier successful passages of network building, so until we tell those stories we cannot mobilise the products in explaining things that came later".

Accounts of the Field Research Processes

Field research processes (or methods) are always contingent on the nature of the investigation. Methods in conducting a 'field work' vary depending on the researcher's time and space availability, personality, social historical class, ethnicity, gender and economic class, obtaining access; and the location of research site.

Booth (1991) argues that the description of the methods in any field study (he refers to 'case study') is difficult. Booth (1991, 139) further argues that 'while some issues can be addressed before the
study commences, others evolve or can only be addressed as the research progresses'. Similarly, Simon argues that:

There is never a single, standard, correct method of carrying out a piece of research. Do not wait to start your research until you find out the proper approach, because there are many ways to tackle a problem - some good, some bad, but probably several good ways. There is no single perfect design. A research method for a given problem is not like the solution to a problem in algebra. It is more like a recipe for beef stroganoff; there is no best recipe. (1969, 4)

However, the following two sections contains an account of how this study has been carried out. That is, how the researcher got a 'way in' to the rapport or researched organisation as well as the 'quasi-laboratory'? What was the involvement? How was the subject of investigation constructed while the researcher got a way in? How did serendipity patterns of the investigation processes influence the researcher in the construction of the research topic?

**Way In to the Quasi-Laboratory**

It was not a Latourian laboratory (Latour 1987) that was investigated. It was neither a project of the innovations of production technology nor the weaponry of a Machiavellian 'Prince' (Latour 1988). It was a project where men and women of a large steel division had been engaged in designing (or customising) and installing (implementing) a mainframe Integrated Business System (IBS) in order to fabricate their cost management and other information systems. In a way it can be called a 'quasi-laboratory' because there were many similarities as far as the processes of 'fact-building' in a laboratory (cf Latour 1987; Latour and Woolgar 1979) are concerned. The study also has similarities with the studies conducted by Preston et al (1992), Boomfield et al 1992 and Chua 1993.

Latour (1987, 2) asks "Where can we start a study of science and technology? The choice of a 'way in' crucially depends on good timing." We had been trying to gain access into a large organisation for more than a year where re-structuring or change process was underway. We had obtained information that a major change process was underway at a local steel manufacturing company (ie, BHP's Slab, Plate and Product Division - BHP-SPPD). Eventually, we were granted an appointment with a senior executive officer (SEO) at the commercial building premises of the researched organisation.

After introductions, the SEO handed over two 'draft' copies of strategic highlights on the project "Phoenix 21 Project - Stage I". These draft copies were labelled as "World Class Cost Management Strategy for the 90s". He then explained the strategic features of the SPPD's cost management systems and their on-going Phoenix 21 project (also known as SAP project). After explaining some strategic issues of the project for about an hour, the SEO asked us what could he do for us. "What sort of things are you looking for?"

In reply, the first author (hereinafter the researcher) said "I am trying to do some research on the area of management accounting and control systems". He also mentioned some other tentative areas to be investigated. All these statements basically were uttered to show his confidence that he had
some real intention to do research and was familiar with contemporary cost management issues. However, the SEO asked whether he wanted to be involved with their on-going Phoenix 21 project (ie, the quasi-laboratory). If so, he could arrange for that. The supervisor (the second author) supported the researcher by saying: "Aren't you looking for this?" This was said mainly to apprehend the idea that in conducting any investigation on contemporary change processes there is a need for a process oriented involvement. The SEO arranged for a future appointment. This was our initial way in to the organisation, followed by the way in to the "quasi-laboratory", that is, the Phoenix 21 project. However, it should be mentioned that the involvement with the quasi-laboratory continued for about two years.

Field Research Processes of this study

There were various modes of field research processes followed in this study. These processes include attending various meetings, review sessions and training courses to 'hands-on' and understanding the computer system; collecting a wide range of project design related documents, minutes, discussion papers and other materials; conducting interviews (both formal and informal) with various levels of officials including some officials outside the quasi-laboratory. For most of the involvement diaries were kept. Most of the interviews were tape recorded. Initially there were some interviews which were not tape recorded. All these interviews were written based on the notes taken during the interviews. These were conducted mainly to familiarise, maintain and develop further interactions in order to keep track of, and update, the fabricating processes. Interviews were conducted with a general range of questions prepared before interviews and conducted with a focus on particular key questions depending on the nature and works with which the interviewee was involved. The questions were not followed up in a fixed order, and issues raised by subjects were pursued. These interviews, in fact, supplemented a vast body of comments and information gleaned from the informal discussions. Extensive notes were also taken at the various interactions.

The role of the researcher was announced to the team members of the project from the start of his involvement in the quasi-laboratory by the project coordinator. This helped gain access to various facilities including documents and photocopying facilities and so on. The researcher also was provided with a desk and a special digital card to enter the quasi-laboratory. Initial 'familiarisation' (Booth 1991) of the 'actors' various manipulations on the fabricating cost management systems was gained including attending various review sessions and meetings and reading diverse range of internal documents. Above all, the researcher tried to understand the various fabricating processes in the quasi-laboratory by observing aspects such as: How did they initiate opening differing 'black boxes' (accounting or otherwise) then debate them? What did new allies do when they came in? When there were new displays of softwares or design related prototype seminars what did people ask? How would the new systems impact upon them? What did the system mean to them? And so on.
Moreover, initially the researcher had to search for 'key informants' (Tremblay 1982) to be interviewed. On the basis of the 'verbatim conversation' or information gleaned from meetings or informal chats with the project members, the selection of key informants to be interviewed was made. However, most of the interviews were conducted with the people who were attached to the project. It is an interdisciplinary project by nature where various actors from differing areas such as Finance and Planning, Supply, Production, Engineering, Maintenance Engineering and Human Resources have been involved in fabricating the IBS. Informal interviews and verbatim conversation with the various project members, and reading the conceptual, functional and other design documents also enabled the researcher to quickly understand the fabricating processes of the 'fact-builders'. Attending five weeks of formal training courses on some SAP's software modules enhanced his understanding of the various design related papers and documents and the 'actors-networks' (both human and non-human) within the quasi-laboratory. Without such computer hands on training courses it would have been a difficult task to understand the "machines" (software modules), let alone, follow up on the fabrication processes of the 'fact-builders' in the quasi-laboratory of this type including the "traces and inscriptions" of the "machines" (the SAP system).

Since the access of the researcher to the quasi-laboratory was well accepted, there were no obstacles to collecting the documents (with some exceptions). Various internal documents (that is, a range of design papers, occasional papers, project design manuals both current and historical, various booklets of differing initiatives of the fabricating CMS and other systems) were collected through various interactions with the various officials both inside and outside the quasi-laboratory. Sometimes extra copies of some of the design related documentations were specially made available to the researcher. There was no shortage of co-operation. All the members became friendly, cooperative, open, supportive, and seemed to genuinely value our interest in the investigation of the complex activities of the project. Sources of secondary information (historical data) included the BHP-Library, local news papers, published books, special monographs, memorandums and journals about the researched. Most of the secondary information about the software company (ie SAP International AG Ltd) was collected through personal interactions with the consultants and from their office in Chatswood, Sydney, Australia.

Finding (Dis)Similarities for Positioning the Study

Like Latour's (1987) way in to the laboratory, the researcher's way in to the quasi-laboratory was also well-timed. At the time, there were many discursive conditions that prevailed in the quasi-laboratory, which can be matched with some of the leitmotifs of Latour's (1987) positioning tactics (or otherwise) of the 'fact-building' processes within the laboratory life. Also, there are similarities in our investigation processes in the quasi-laboratory to some of Latour's (1987) rules of methods. First, the researcher's way in to the quasi-laboratory matches with Latour's first rule of methods. That is, he arrived before the facts (science) and machines (technology) were 'blackboxed' in the 'quasi-laboratory' and before controversies died down and well before the implementation of the IBS systems at the researched organisation. Secondly, similar to Preston et al (1992), the researcher
mapped networks of resources, support and use, both historically and across conventional organisational boundaries, in order to examine the multiplicity of people involved in the fabrication process. Thirdly, he gathered various machinations and inscriptions of the systems to determine how they would tie together to become an integrated system.

In a way, our investigation also matches with the studies conducted by Boomfield et al (1992) and Chua (1993), where the researcher (the traveller) enters into a rapport with a very little understanding of its reality, as indicated earlier. However, in many respects the researcher's field work in the "quasi-laboratory" would be dissimilar to (or short of) Latour’s rules of methods. For instance, Latour's (1987) fifth rule of methods suggests that we should follow all the networks no matter how long it takes and how heterogeneous they are. But, in a project (ie, the quasi-laboratory) like the one we investigated it would be a difficult job practically for a single researcher, though not impossible, to attend all the meetings and note the utterances of the various players (actors-networks) as they opened 'black boxes' and so on. There were many overlapping meetings, diverse activities, of which one could only hope to get a general view with some specificity over a lengthy period of investigation. As well, Latour’s (1987) second rule of methods might not be appropriate in a study like this. This is because there is a need for some judgement about the fabrication process whether it is good or bad, or why some 'facts' and 'technology' (software modules) might have been accepted and others rejected.

It may be possible that in a Latourian laboratory (cf. Latour 1987) there was limited people-interaction. But in a 'quasi-laboratory' like the one we investigated, where the number of actors (both human and non-human) dealing with the design and implementation of the IBS was large, it would be difficult to interact with all the 'actors' (including the machines) involved in the laboratory. Moreover, as mentioned earlier, the project was interdisciplinary by nature, where various actors from differing functional areas such as Finance and Planning, Supply, Engineering, Maintenance Engineering and Human Resources had been engaged in designing and prototyping various standard SAP software modules for their own respective functional requirements.

Framing the Research Topic

Research phases provide strategic rationale for a particular research programme. In most research programmes, the specification or formulation of research problems/questions together with a literature review are seen to be a first and primary phase. Typically, whether or not it has to be the first phase, perhaps the importance of formulating significant research question(s) is (are) unavoidable. Moreover, one can argue whether problems formulated or posed by the researcher coincide with those of concern to the organisational practitioners in fabricating their facts and technology. Can researcher problem formulation be identical with practitioners' problems which can be seen as 'socially relevant problem(s)'? For whom are the research outcomes or results (stories or otherwise) to be staged or framed?
The pursuit of a scientifically significant research question(s) is (are) a never ending quest. A researcher's boundary and discovery of 'facts and technology' are limited to the extent to which they can investigate, where they can get access, and their cognitive limitations; even more so what they can represent from the collected 'empirics' by framing a particular theory of interest.

As mentioned earlier, the researcher had entered the 'quasi-laboratory' (ie, Phoenix 21 project) with very little understanding about what he would be seeing except his pre-understanding of the theoretical and methodological underpinnings that have been advanced so far. It was only after entering and spending some time in the quasi-laboratory, that he could pursue the final construction of his research topic: "Fabricating Cost Management and Other Systems in a Mainframe Integrated Business System Environment: A Critical Accounting Study". Of course, this has to be examined with the theoretical relevance and the way in which the researcher has conceptualised and advanced the methodological underpinnings (or otherwise). It can be argued that embarking on a major study is dependent upon the way in which the researcher (1) resolves the theoretical and epistemological disputes in the 'doing of research' and (2) relates that to the 'action-oriented' level - that is at the level of empirical investigation.

Epilogue

This paper was part of an on-going research project. At the time, the major tasks ahead for the researcher was to write up a critical ethnography of the collected "empirics" about the fabrication of accounting knowledge (and otherwise) via the quasi-laboratory (ie, SPPD's Phoenix 21 project), where three organisations, BHP-SPPD, SAP International AG and BHP-Information Technology were engaged in implementing an Integrated Business System (IBS) at BHP-SPPD using SAP commercial packages. At the researched organisation (BHP-SPPD) the initiative for fabricating this IBS started in the early 1990, and was an on-going project and was endorsed to 'go live' on 1 July 1994. The question remains as to when such an investigation process into the quasi-laboratory can be considered 'enough'. In this respect we can support Latour (1987, 7) who advanced an analogy "when enough is never enough" - that is, science does not know yet what should be considered as discovery of facts and technology. It is rather socially constructed.

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1 It should be reminded that this is just a header of the research topic. During the study a range of questions was generated and evolved. For example, what, how and why has contemporary accounting practice become purposeful, is being used and is to be used, including the means of so doing at a micro-organisational context? What discursive conditions can give rise to the possibilities of a certain technological change in a particular micro-organisational context? Or, why possibilities for change emerge? How did SPPD tie up the interested groups with the "Machinations and Inscriptions" in order to deploy the technology? Why has the emphasis been shifted to develop an IBS from a stand alone CMS? How did the fact-building tasks, including accounting, persuade? What were the struggles involved in such a fabrication? Why had such fact-building processes taken a long time? What are the consequences of such fabrication? What impact has such fabrication on the future roles of (management) accountants (or otherwise) within the researched organisation? What possible communicative (behavioural) effects do the various occupational groups (the users of the integrated system) have in sharing and managing information under the proposed system at the researched organisation? What constraints did the fact-builders face to build, implement and deploy the technology (the SAP system)? How can the technology (the proposed integrated CMS) shape and influence the "lifeworld" at SPPD?
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