

RE-GROUNDING GROUNDED THEORY**ANTONY BRYANT, Leeds Metropolitan University***School of Information Management, LEEDS LS6 3QS, UK. Email: a.bryant@lmu.ac.uk*

So one day I was sitting in a meeting about the new global trade regime, NAFTA and GATT and the World Trade Organization. The more I listened, the more I began to simmer inside. "This is a huge new system people are inventing!" I said to myself. "They haven't the slightest idea how it will behave," myself said back to me. (Meadows, 1997, stress added)

ABSTRACT

IS researchers are now far more likely to consider using qualitative approaches than may have been the case a few years ago. Publication outlets such as JITTA, Information & Organization, and IFIP Working Group 8.2 have helped to establish a firm basis for non-quantitative IS research. One method that is gaining increasing popularity is the Grounded Theory Method originated by Glaser and Strauss. There are some profound problems with this approach; in particular the unproblematic conceptualization of data, and a level of methodological flexibility that can degenerate into methodological indifference and result in superficial and ambiguous conclusions.

This paper argues that the method is not indelibly stamped with these failings and inconsistencies; although they are indeed failings, despite the views of many users of the method. If these faults are remedied, however, the method is particularly suited to IS research, particularly where it proceeds from an anti-positivist orientation that sees truth as socially constructed and sustained, and where representation is viewed as a distributed, systems phenomenon.

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INTRODUCTION

Discussion of research philosophies and methods within IS has increasingly grappled with the choice between and relative merits of quantitative and qualitative approaches. In part this has come about as one aspect of the gradual conceptual dismantling of the positivist or scientific hegemony. This has occurred as part of the agenda to increase the visibility of *intepretivist* approaches; but it has also been part of the move to promote a more *critical* research orientation. Some versions of the critical programme seek to offer a more extensive and profound questioning of the entire scientific¹ enterprise, usually linked to the wider post-modernist perspective. This involves a comprehensive dismantling of scientism in any form, and in some variants denies any sort of privileged knowledge claim for science.

IS has had to accommodate to these various trends, and there are distinctions within the domain that parallel these wider philosophical differences. To an extent the divisions parallel those which distinguish different conceptions of the two words *information* and *system*. Some see the systems approach as an alternative to the scientific one; for others it is an orientation that enhances and complements the scientific approach both methodologically and epistemologically; but this is not the key division. The most profound demarcation concerns the *ontological status* of any system: To what extent it can be said to exist. In his recent summary of the systems approach, Ackoff (1999) uses the example of a car. There is the unmistakable sense in which the car exists, and so it might be supposed that it is similarly obvious that the car-as-system

CONTRIBUTION

This paper should be seen as part of a general move towards the retrieval of the Grounded Theory Method [GTM] from its positivist origins, and its unsystematic use by many researchers who often use GTM to mask their own - or their discipline's - methodological confusions.

In arguing for the importance of approaches such as GTM, the paper seeks to clarify some central general methodological issues, which are of particular concern to IS researchers and practitioners. It should also be seen as a contribution to the debate between the positivists and interpretivists, illustrating how many of the suppositions of the former fail to engage with philosophical developments over the past 40 years. The reader should be persuaded that using a methodology is not a matter of choosing an approach from some methodological supermarket, but something that has far wider and deeper ramifications.

exists. But whereas the physical extent of the car is tangible and measureable, the same cannot be said for the car-as-system – for instance, does it include the driver? Does it include the road on which the car is standing or moving? Now consider the healthcare system – another example used by Ackoff. Where does this begin and end? What does it include – and hence exclude? To what extent does it exist?

Some aspects of this ontological concern are raised by Flood in his recent work (1999), where he argues that there are two central issues for ‘systemic thinking’; *boundary setting* – ‘yielding a viewpoint that is both relevant and on a manageable scale’ (p 70); and ‘*who is to judge that any one viewpoint is relevant*’ (p 70 – stress in the original). A similar sentiment is offered by Meadows in the quote at the start of this paper.

This issue is critical for IS. Information *systems* exist, but if the system is more than the technology – the stuff you can kick – what is this ‘more,’ and where is the boundary? If it is not an obvious and tangible boundary, then to what extent is its existence dependent on certain actors acknowledging its existence? There are no simple answers to these questions; and the detailed arguments are not an issue in this paper. What is important,

¹ Scientism can be defined as ‘science’s belief in itself,’ or as Putnam states, people being ‘too realistic about physics and too subjectivistic about ethics’ –quoted in Brandom, 2000

however, is to recognize that the sorts of responses IS researchers and theorists might give if prompted – or forced - to deal with such questions would indicate a great deal about the assumptions in their investigations. Does their work emanate from determinist and mechanistic foundations? Do they have a tendency towards voluntarist and idealist positions? Are they using structuration or structuralist tenets, without consciously realizing it?

Flood's position is that the strength of systems thinking lies in its concern with emergent aspects, as opposed to reductionist analytic ways of thinking. His overview of some of the key systems thinkers distinguishes those who genuinely uphold and contribute to this strength from those who continue with a deterministic bent within a systems context. He singles out Senge's influential work (1990), pointing to Senge's tendency to develop 'an uncritical choice of boundary and a lack of awareness of the ethical nature of boundary judgements' (Flood, 1999, p 71). Coupled with a failure to address the issues of 'knowledge-power and social transformation' (p 72), this results in Senge offering a view of systems that amounts to a 'reductionist prescription.' Although Flood does not then state that Senge's work undermines the systems approach, he implies this and so offers a damaging critique of perhaps the best known *systems* book of the last 20 years.

Flood finds a far more acceptable and rigorous systems perspective in the work of C West Churchman (1984), whom he quotes approvingly – e.g. 'systemic thinking begins when you see through the eyes of another' (Flood, 1999, p 69). Using Churchman's ethically-minded approach together with insights from complexity theory, Flood is concerned to counter any form of determinism - or voluntarism - by centring his arguments around three paradoxes.

'We will not struggle to manage over things – we will manage within the unmanageable.

We will not battle to organise the totality – we will organise within the unorganisable.

We will not simply know things – but we will know of the unknowable.' (Flood, 1999, p 3)

Flood wants to stress that 'systemic thinking involves, in the first instance, drawing tentative boundaries around stakeholders, focusing primarily on clients, and raising issues and dilemmas relating to those clients.' (p 7)

The lesson from this for IS is that our research programme has to engage with the central ideas around *systems*. We have to find ways of clarifying and validating our boundary settings and the judgements we make regarding the relevance of differing perspectives. This involves taking account of social actors, acting in social contexts as key constituting factors – what can be termed *constituting constituents*. The research agenda must follow from this, and research methods must not simply aim at *counting* but at *accounting* (in the sense of giving an account of): Hence the importance of interpretive orientations to research, and of qualitative research methods, and of the Grounded Theory Method.

THE GROUNDED THEORY METHOD

There is always some confusion regarding the distinction between the grounded theory *method* and grounded theory itself. A grounded theory is the possible outcome of using the GT method. One might use the method without producing a grounded theory, and there are some who claim to use the method as part of an approach that does not seek to develop grounded theories. Since this paper is specifically concerned with the method, it scrupulously makes correct use of the phrase 'grounded theory method' (GTM) throughout – apart from the title! This may seem somewhat pedantic, but should avoid unnecessary confusion – something all too common in discussions of GTM.

There is now a more considered and well-argued basis for the use of qualitative research methods in IS as can be witnessed from recent contributions to many key IS journals and conference. Such contributions allow work to be done in the field – literally in the case of Klein and Myers' profound and influential paper (1999) – without researchers having to justify their research method from scratch or grant some level of obeisance to

scientism, positivism and quantitative methods.

The Grounded Theory Method (GTM) is now widely mentioned in the IS research literature. The method is still closely associated with its originators, Barney Glaser and Anselm Strauss who introduced and developed it in the 1960s. Until relatively recently, the method had something of a peripheral, if not pariah, status in many areas; but in recent years it has enjoyed a resurgence, and there is a growing body of literature that attests to its range of application in IS research. (See Myers, 2002)

Glaser and Strauss first published studies using the grounded theory method with their co-workers in the early 1960s (e.g. Glaser & Strauss, 1965; Strauss et al, 1964). In 1967 they published an extended exposition of the method, and this text of origin has remained the central reference point for all those working with GTM; having achieved something of a classic status in the general methods literature. (Classic in the sense that it is always cited, but not always understood or followed ... or read.)

The fact that GTM is still so intimately linked to its founders might be taken as evidence for its failure to move far beyond its origins. Comparison may be made with action research, which although owing a good deal to the early work of Kurt Lewin now stands on its own as a thriving and diverse research orientation. (A recent issue of *Information, Technology & People* was devoted to articles on Action Research, and not one of the contributions mentioned Lewin.)

The two originators of GTM came from contrasting backgrounds, each with a specific influence on the method. Strauss had worked in the Chicago School of social research. This was particularly noted for its stress on *qualitative* research methods, exemplified by the work of people such as Robert Park (1968), John Dewey (1991), G.H. Mead (1934) and Herbert Blumer (1937). Glaser on the other hand had worked at Columbia University with Paul Lazarsfeld (1955) who emphasized the importance of empirical research, but developed innovative ways in using

quantitative methods. (See Strauss & Corbin, 1998, pp 9-10)

The confluence of these influences was brought to bear on the project to offer 'strategies for qualitative research' or 'qualitative analysis.'

In speaking of qualitative analysis, we are referring not to the quantifying of qualitative data but rather to a nonmathematical process of interpretation, carried out for the purpose of discovering concepts and relationships in raw data and then organizing these into a theoretical explanatory scheme. (Strauss & Corbin, 1998, p11)

Glaser and Strauss were concerned with what they saw as an imbalance between theory *generation* and *verification*. 'Since verification has primacy on the current sociological scene, the desire to generate theory often becomes secondary, if not totally lost, in specific researches.' (Glaser & Strauss, 1967, p2) They open their book with the statement that although obtaining 'accurate facts' is important, 'we address ourselves to the equally important enterprise of *how the discovery of theory from data – systematically obtained and analyzed in social research – can be furthered*. (1967, p1 – stress in original)

They proposed a 'general method of comparative analysis.' 'The basic theme of our book is the discovery of theory from data systematically obtained from social research.' (p 2) This emanated from their concern with 'processes of research for generating theory. ... [as opposed to] theory generated by logical deduction from *a priori* assumptions.' (p 3) Although the two originators went their separate theoretical ways with regard to GTM in the 1980s and 1990s (see Smit & Bryant, 2000) the essential aspects of GTM as specified in most of the key sources do not appear to have altered significantly since the 1967 publication.

In the GTM literature it is assumed that verification was indeed prevalent in sociological theorizing at the time, although it is sometimes unclear whether this was indeed the case. Whatever the circumstances, Glaser and Strauss were intent on establishing a basis for qualitative research in the social sciences, as opposed to research that relied almost exclusively on quantitative – i.e. statistical –

methods. The targets of their criticism were not limited to such work, however, since they also pointed to the deficiencies in the work of more polemical social theorists; and they singled out C Wright Mills (1951), whom they accused of exhibiting ‘only little theoretical control’ (p 5). This sentiment is reiterated in Strauss and Corbin’s work in 1998 – ‘Theory derived from data is more likely to resemble the (sic) “reality” than is theory derived from putting together a series of concepts based on experience or solely through speculation (how one thinks things ought to work). Grounded theories, because they are drawn from data, are likely to offer insight, enhance understanding, and provide a meaningful guide to action.’ (p 12) (Note the way in which ‘speculation’ is seen to involve ‘ought’ rather than ‘might’: Also the quotes around the word reality, which is used unadorned elsewhere, although the authors do say that ‘only God can tell infallible humans the “real” nature of reality’ (p 4). If space permitted, it would be interesting to use GTM on this statement itself, looking at terms such as ‘experience,’ ‘likely,’ and ‘meaningful guide to action.’ A cynical observer might wonder from which data God emerged.)

Glaser and Strauss were not then seeking to privilege any specific agenda or political priorities for social research; but they were arguing that a non-quantitative form of research could still attain levels of rigour and validity that would stand comparison with well-established quantitative ones. At their time of writing (1960s), they characterized sociological research as heavily biased towards validating the grand theories of the sociological pantheon. The legacy of the grand social theorists - Marx, Weber, Durkheim - had generated a vast array of theories that were largely devoid of the validation drawn from empirical underpinnings. Thus students were encouraged to immerse themselves in these theories, and then to ‘test them in small ways’ (p 10). Some exceptions, particularly Robert Merton (1957) and Talcott Parsons (1937), had broken with this trend, generating their own grand theories. ‘But even these few have lacked methods for generating theory from data, or at any rate have not written about their methods. They have played “theoretical

capitalist” to the mass of “proletariat” testers.’ (p 10)

Ignoring the convoluted and questionable imagery, Glaser and Strauss were laying the foundation for GTM on the basis of two criticisms of social theory in 1960s. In the first place, they pointed to a gap between theory and substantiation, with grand theoretical expositions exceeding any empirical base; and, second, this hiatus is exacerbated by instructors and their researchers limiting their endeavours to supplying precisely this underpinning.

GTM was then put forward as a remedy to correct these deficiencies by generating new theories, but still using the criteria of rigour associated with existing – predominantly empirical and quantitative - work. Glaser and Strauss were quite explicit about this, indeed they stressed that the method must adhere to scientific rigour, and that the generating of sociological theory is the sole job of sociologists. Professionals and lay people ‘cannot generate sociological theory from their work. Only sociologists are trained to want it, to look for it, and to generate it.’ (pp 6-7).

This coincides with their position regarding the ‘interrelated jobs of theory in sociology’ (p 3) - ‘(1) to enable prediction and explanation of behaviour; (2) to be useful in theoretical advance in sociology; (3) to be usable in practical applications – prediction and explanation should be able to give the practitioner understanding and some control of situations; (4) to provide a perspective on behaviour – a stance to be taken toward data; and (5) to guide and provide a style for research on particular areas of behaviour’ (Glaser & Strauss, 1967, p 3).

All of these five aspects would be readily acceptable to even the most ‘verificationist,’ empirical sociologist. In fact the idea that research should yield a basis for prediction and control based on study of data is a fairly succinct summary of a scientific or positivist position.

This early statement of the method can be read as an argument that qualitative methods – in particular their own ‘general method of comparative analysis’ – can be as rigorous as quantitative ones. As a

consequence, research developed using GTM can make the same knowledge claims as those made by research findings generated by more traditional - verified - quantitative methods. It is, therefore, not surprising to see that Glaser later stated that ‘Grounded Theory does **not** represent a change in philosophy and scientific thought’ (1998, p 44). The principles behind GTM, and its characterization of scientific discovery are completely in tune with the standard positivist orthodoxy of the 1960s. This was perhaps readily understandable in 1967, when their work first appeared; but it is hardly credible to maintain such a view in the 1980s and 1990s without at least engaging with critiques of positivism.

GROUNDING THEORY METHOD AND POSITIVISM

A study of the work of Glaser and Strauss, and others quoted and cited in the central texts on GTM reveals an almost complete silence about any of the key developments in epistemology and philosophy of science in the last 30 years. One searches in vain for a reference to positivism or interpretivism, let alone any substantive discussion of Thomas Kuhn (1996), Karl Popper (1968), and the developing philosophical debates about their work.

Even now, these failings have not really been addressed in the context of GTM. Haig (1995) has pointed out that the method has been accused of a ‘naïve Baconian inductivism,’ although he offers no characterization of what this might be, nor does he specify any sources of this criticism. The problematic nature of the process of deriving general law-like statements from repeated observations – i.e. induction, presumably what Haig means by ‘inductivism’ - is not explained. Popper’s work in the 1960s, and Kuhn’s in the 1970s are only the most obvious and well cited examples of critiques of induction. Moreover, Haig reiterates the point that Glaser and Strauss wished to see GTM as a ‘general theory of scientific method,’ meeting ‘the accepted canons for doing good science.’ Haig does not see these factors as problematic, simply stating that ‘these methodological notions are not to be understood in a positivist sense.’ Although he

fails to point out why not; but his paper does at least hint at some unease within the GTM fold about its philosophical bases.

Positivism is founded on the assertion that knowledge of reality is only possible through observations; the ‘rule of phenomenalism,’ which asserts that experience provides the unique basis for knowledge. Furthermore these observations can and must be value-neutral, since judgements of value have no empirical content. Scientific knowledge is developed by gathering together repeated observations, so that general law-like generalizations can be made. This archetype of what constitutes valid knowledge should apply not only to the natural sciences, but to all other modes of enquiry.

Certainly there are some statements in the ‘standard’ GTM literature (attributable to Glaser and Strauss singly and collectively) that stress that the researcher’s own views do have some impact on the research process. But there is a far stronger, clearer and consistent thread from the late 1960s to the present that is readily identifiable with the characteristics mentioned above. The ‘rule of phenomenalism’ is central, and intimately linked to the key role that is played by ‘data’ in all GTM writings. Glaser and Strauss continually refer to the theory being ‘grounded in the data’; this is part of the mantra of the grounded theorist, and like a mantra it is continually chanted, but rarely questioned or examined. A few examples from early and late GTM publications will serve as illustrations.

- ‘[t]he basic theme in our book is the discovery of theory from data systematically obtained from social research’ (G&S, 1976, p 2);
- ‘Theory based on data can usually not be completely refuted by more data or replaced by another theory. Since it is too intimately linked to data, it is destined to last despite its inevitable modification and reformulation.’ (G&S, 1967, p 4);
- ‘... the generation of theory from such insights [sources other than data] must then be brought into relation to the data, or there is great danger that theory and empirical world will mismatch.’ (G&S, 1967, p 6);
- ‘The first step in gaining theoretical sensitivity is to enter the research setting

- with as few predetermined ideas as possible – especially logically deducted (sic), a prior (sic) hypotheses. In this posture, the analyst is able to remain sensitive to the data by being able to record events and detect happenings without first having them filtered through and squared with pre-existing hypotheses and biases.’ (Glaser, 1978, pp 2-3);
- ‘A theory must be readily modifiable, based on ever-emerging notions from more data.’ Glaser, 1978, p 4);
 - ‘A researcher does not begin a project with a preconceived theory in mind (unless his or her purpose is to elaborate and extend existing theory). Rather, the researcher begins with an area of study and allows the theory to emerge from the data. (Strauss & Corbin, 1998, p 12);
 - ‘Creativity manifests itself in the ability of researchers to aptly name categories, ask stimulating questions, make comparisons, and extract an innovative, integrated, realistic scheme from masses of unorganized, raw data.’ (Strauss & Corbin, 1998, p 13)

Taken together this demonstrates a consistently positivist strand running through the GTM literature from the 1960s to the present. Data is seen as an unexceptional category; it is simply what is observed. Conclusions are based on accreting multiple observations, and then using these as the basis for deriving general, law-like statements - i.e. *induction* - in the process of *theory generation*. This may have been conceptually unremarkable in the 1960s; but given the developments in epistemology and philosophy of science since then, it is surprising that there has not been some extended justification particularly of the use of the concept of ‘data’ in the GTM literature in the last 30 years.

Glaser did later acknowledge that GTM is only ‘one theory on where theory may profitably come from and one method of how to obtain it’ (1978, p 3). This should not, however, detract from the inconsistencies within the method itself, and its general failure to engage in self-reflection, applying GTM to its own concepts and categories. For instance it still begs the question from which set of data does the theory about the origins of theory

emerge? (NB – Also it is unclear what the ‘it’ refers to at the end of the quoted section – it could be ‘theory,’ or ‘data.’)

Haig (1995) does try to engage with these issues in distinguishing between data and phenomena – ‘it is typically phenomena, not data, that our theories are constructed to explain and predict. ... grounded theories should be taken as grounded in phenomena, not data.’ Haig stresses that these ‘phenomena’ comprise a ‘varied ontological bag,’ and in general they are ‘not observable.’ But he fails to sustain his argument, and eventually undermines it completely with statements such as ‘some observations (phenomena) are encountered which are surprising,’ and ‘we come to notice that those observations (phenomena)’ So are phenomena observable or not? If they are, how are they distinct from data?

In the absence of any sustained critique from within GTM that defends the basic precepts from being labelled positivist and scientific, one can only turn to the consistent and repeated aspects of the method itself. Early and late writings on GTM stress that the method should give rise to theories – *grounded theories* - that meet certain criteria. They should have *fit* and *relevance*, they must *work* and be readily *modifiable* (Glaser, 1978, p 4). By fit, Glaser means that the categories of the theory must fit the data – whatever that might mean. ‘Since most of the categories of grounded theory are generated directly from the data, the criteria (sic) of fit is automatically met’ (p 4 – note the use of ‘most’ in this sentence).

When a grounded theory *works* it means that the theory can ‘explain what happened, predict what will happen, and interpret what is happening in the area of substantive or formal inquiry’ (Glaser, 1978, p 4). Glaser maintains that this is achieved through determining what is going on in the area of inquiry through systematic research. A theory will therefore work if its categories fit and if it is relevant to the ‘*action* of the area’ (Glaser, 1978, p 5).

In terms of the characterization of positivism given above, and on the basis of the extracts quoted, GTM appears indelibly

positivist. The entire GTM endeavour seems to be founded on phenomenism, guided by induction. The only possible conclusion that can be made from all this is that GTM developed from an epistemological position that was positivist, and that it has failed to justify or to shake off this inheritance. What the remainder of this paper will argue, however, is that this dubious legacy can be discarded, re-aligning the method as a crucial device for IS research and theoretical development.

In their later writings Glaser and Strauss parted company, with a good deal of acrimony. (See Smit & Bryant, 2000, for a more detailed account of these developments.) Without going into the intricacies of the dispute, the differences that emerged between Glaser and Strauss are symptomatic of the uneasy alliance of assumptions that lie at the heart of GTM: In particular the tension between the claims bound up with scientific status and empirical reality, and the attention to detail, context and meaning. This is not to attenuate the criticism of the method for its scientism and positivism, but it is to allow that there are some aspects of the method that do begin to take a more interpretive and dialogical view of the research process and any resulting knowledge claims. It is this aspect that is critical to IS, as will be explained later.

METHODOLOGICAL INADEQUACIES OF GTM

The weaknesses of GTM are not confined to the philosophical, they extend to the methodological. One of the reasons that GTM continues to be held at arm's length in some research communities is that all too often it is an excuse for evading methodological issues. People who claim to be using GTM often use this as a way of disguising their methodological incompetence or fragility – particularly if they lack clear objectives or have poorly developed research ideas.

Babchuk (1996) hints at this when contrasting Glaser's 'laissez-faire' approach with Strauss' detailed procedural minutiae. In his survey of 15 years of publications in the Adult Education field, he notes euphemistically that GTM has been used as an

'umbrella term' by a large number of researchers – by which he implies that they use the term to mean more or less anything they want. Babchuk offers examples that range from use of one aspect of GTM (constant comparison), through use of the method as one of several employed on a project, to careful efforts to follow the 'full complement of rules and dictates.' From which he concludes that '[O]ne begins to wonder if this diverse interpretation of grounded theory procedures is representative of the ingenuity of educators and their research designs or simply confusion over method.' He is equivocal in regard to this *flexibility* of GTM; is it a source of strength or weakness? (He concludes that the method is particularly suited to Adult Education - make of that what you will.) Robrecht (1995) arrives at a similar conclusion in her review of the use of GTM in management literature. She found that some authors cite Glaser and Strauss, but often with little to indicate that the method has been followed. At best this amounts to a 'selective rewriting' of GTM; and at worst, mention of GTM is used as a way of masking 'an *anything goes* approach' that is methodologically arbitrary and ultimately indefensible.

A large number of those professing to use GTM exhibit this laxity of interpretation of the method, often accompanied by a philosophical naiveté and confusion similar to that exhibited by Glaser and Strauss themselves. An example of this is provided in the paper by De Vreede et al (1999), and it is worth analyzing it in some detail. Their paper, published in a reputable journal, uses GTM in the context of a cross-cultural field study of group support systems [GSS]. The points that follow are not meant in any way to detract from the content and import of the paper. On the contrary, the paper illustrates the key point of the present argument that despite the weaknesses and evasions of GTM, the method is powerful and often indispensable for precisely this sort of investigation.

In the abstract the authors note that the study used a 'grounded theory approach ... to collect and analyze data.' 'From the data emerged a model of GSS acceptance in the cultures investigated that extends the Technology Acceptance Model (TAM).' This

is problematic in GTM terms since Glaser & Strauss (1967) explicitly distinguished between ‘grounded *generating* of theory’ and ‘grounded *modifying* of theory’ (1967, p 2 footnote, stress added). Strauss and Corbin some 30 years later do seem to allow modification, and they note that ‘a researcher might have some preconceived theory in mind ... [if the] purpose is to elaborate and extend existing theory’ (Strauss & Corbin, 1998, p 12). In this case, however, they stress that the theory does not emerge from the data. Glaser tackles the same issue when he discusses ‘modifiability’ (1978).

Despite this, the distinction between modifying a theory (grounded or not) and developing an existing theory remains an enigma. Neither category is ever analyzed in the GTM literature. De Vreede et al can hardly be held accountable for this confusion, since it lies at the heart of ‘classic’ GTM writings; but it does exemplify a group of keenly analytic researchers failing to apply insights to their own tools.

De Vreede et al offer their version of the mantra of GTM – ‘This approach [GTM] aims to develop inductively derived grounded theories about a phenomenon. A grounded theory is not built *a priori*; rather, it emerges during study as data collection, analysis, and theory development occur in parallel.’ (1999, p 205) What they mean by ‘in parallel’ is not clear; but in any case they almost immediately undermine the statement about emergence by noting that ‘data-collection activities may be guided by relevant existing theories’ (p 206).

They try to offer some explanation and justification for what they mean by being ‘guided by... relevant existing theories,’ arguing that it might be thought that ‘our data-collection efforts could have been prestructured using research domain relevant theories such as the cultural theories of Hofstede or the TAM.’ Given that they are obviously well aware of such theories, the authors reassure their readers that ‘[W]e decided not to do so in order to avoid a standard way of thinking about the phenomena observed.’ How they managed this feat of cognitive evasion is not clear, but their imagery is revealing as they develop the point – ‘Having too much *a priori* theoretical

guidance can block a researcher from seeing what is and is not really significant.’

Here is a succinctly positivist view of cognition. Other theories, known to the observer can simply be discarded, and assumptions can be reduced or perhaps dispensed with altogether. The phenomena can be observed from a totally neutral position by a dispassionate and passive observer. Cognitive reservoirs of previous experience and knowledge can be dammed, blocked or diverted - a good example of what Michael Reddy terms the *conduit metaphor* (see Reddy, 1993 and Bryant, 2001). The flow of ‘raw data’ can be turned on and off like a tap; and categories and theory emerge from this neutral, passive observational practice – ‘we closely examined all collected data, broke them into discrete parts, and labeled these parts.’

As a statement of GTM this is, unfortunately, unexceptional; as a statement about observation and theoretical insight it is naïve and highly misleading. Coupled with the idea that theory can be ‘inductively derived,’ it evades far too many issues and questions that have been central to philosophy of science and epistemology for at least the past 30 years. De Vreede et al give no explanation for what counts as ‘data’ in their work. They see no problem with ‘induction,’ despite the fact that it has been largely discredited in such a simplistic form. They imply that their observational role is largely passive, yet fail to explain how the data can be broken into ‘discrete parts’ and how ‘categories could be identified’ other than through conscious actions of the researchers themselves.²

² An earlier version of this paper (Bryant, 2002) was presented at HICSS35; and by chance one of the reviewers was de Vreede. Amongst his comments, he said that the original version of the paper made no mention of TAM, but that ‘[T]o get through the process’ (i.e. to get published) they had to link TAM to their findings - ‘we struggled quite a bit.’ Another reviewer made the point that although GTM may be imbued with the positivism of the 1960s, ‘many qualitative researchers in IS have to conduct and publish their research within the context of a positivist orthodoxy in North America ... and thus, the scientific (or perhaps scientific)

All of this raises a number of questions, without having to delve deeply into the philosophical intricacies involved. ‘How do categories emerge from passive observation of data?’; ‘Why are certain things held to be similar, and others held to be different?’; ‘Are such ideas historically or culturally distinctive?’ As soon as one does confront these queries, however, the limitations of GTM as expressed by many of its proponents become apparent. This is not to say that the philosophical bases of GTM ideas themselves are wrong – although I think that they are inadequate at best. But it is to point out that the assumptions about cognition and knowledge need extensive justification, and this is almost totally lacking in the GTM literature.

WHY BOTHER WITH GTM?

Given the foregoing discussion, why not simply jettison GTM in its entirety? The weaknesses of GTM are evident. The key statements about the method are steeped in a scientism that was already largely undermined in philosophical terms in the 1970s and 1980s. (Although it has not been undermined in practical terms, but that is another matter.) Ideas about ‘data,’ the ‘emergence of theory,’ and the essentially passive stance of the researcher to the research context and research resources are just not tenable. At the very least they have to be clarified and justified. Furthermore the entire assumptions about representation and discovery are at best questionable, and at worst unfounded and naïve.

But the basic principles about the *conduct* of research guided by GTM are another matter altogether. Here we have an extremely well-developed programme for conducting qualitative research. Many of the examples of actual research that Glaser and Strauss – in concert and separately - and others offer are exemplars of good qualitative

research practice. In other words we need to pay attention to **what they do**, rather than **what they say they do**. If researchers paid more attention to the substantive studies of Glaser, Strauss and their colleagues (Strauss et al, 1964; Glaser & Strauss, 1965), and less to their lengthy methodological disquisitions, perhaps the value of GTM would be more readily apparent.

An illustration of the value of GTM in the IS literature is provided by Wanda Orlikowski’s article on CASE tools (1993). She uses GTM ‘because it allows a focus on contextual and processual elements as well as the action of key players associated with organizational change.’ Orlikowski offers a justification of GTM that refers to the secondary literature far more than the primary Glaser and/or Strauss sources. She argues that GTM has three key characteristics, it is ‘inductive, contextual, and processual,’ and this makes GTM ‘fit with the interpretive rather than the positivist orientation.’ This conclusion is questionable unless Orlikowski has a very unusual definition of ‘inductive.’ However, although she does offer some of the standard GTM mantra – ‘concepts that are suggested by the data,’ etc. – her use of the GTM approach seems to be one of ‘constant comparative analysis,’ using two case studies.

What distinguishes the paper (winner of MIS Quarterly ‘Best Paper’ for 1993) is its extensive detail, and the ways in which the differing accounts that she develops of the two case studies illustrate general and specific aspects of the respective experiences of CASE tool introduction. Her conclusions point to the importance for IS practice of consideration of people’s motives, beliefs and orientations in regard to their work environment and change in routines and expectations. Anyone reading the paper could not help but be impressed by the detail and analysis, yet one might wonder what makes the study an example of developing a ‘grounded theory.’

One aspect of Orlikowski’s paper is the way in which she ‘grounds’ her own use of GTM. She refers to Glaser & Strauss (1967), but not to any of their other writings; and she refers extensively to several papers by Turner. A study of Turner’s 1983 paper, together with Orlikowski’s, and Locke’s (1996) strengthens

language of GTM is VERY valuable to them.’ Perhaps this also explains the attraction of GTM for IS researchers in the 1990s and 2000s; although it does put IS some 30-40 years behind other disciplines.

the view that the method underlying GTM does indeed offer a distinctive and important basis for research. This is not simply restricted to sociology, as Glaser and Strauss first thought, but applicable to many other forms of research, and in particular to anything focused on people's actions and interpretations in organizational and other social contexts.

If the tendentious and misleading philosophical underpinnings of GTM are swept aside, the strengths of the approach can become apparent. To some extent, what the method highlights is a series of activities and considerations that ought to be primary for any form of research involving people. Strauss and Corbin offer a list of the 'characteristics of a grounded theorist' (Strauss & Corbin, 1998, p 7) – the ability to step back and critically analyze situations, recognition of the tendency toward bias, and the ability to think abstractly. Coupled with the ability to be flexible and open to helpful criticism; sensitivity to the words and actions of respondents; and a sense of absorption and devotion to the work process.

These are hardly specific to GTM, and would be highly regarded qualities in most researchers. What distinguishes GTM, however, is its insistence on engaging with the actors and their contexts; Turner offers a useful summation as follows;

This approach to qualitative data promotes the development of theoretical accounts which conform closely to the situations being observed, *so that the theory is likely to be intelligible to and usable by those in the situations observed*, and is open to comment and correction by them. ... The approach also directs the researcher immediately to the creative core of the research process and *facilitates the direct application of both the intellect and the imagination* to the demanding process of interpreting qualitative research data. It is worth noting that the quality of the final product arising from this kind of work is *more directly dependent upon the quality of the research worker's understanding of the phenomena under observation* than is the case with many other approaches to research. (Turner, 1983, pp 334-5, stress added)

Turner's characterization of GTM is far more persuasive than the refrain of 'theory emerging from the data.' Here we see research

as a process of engagement with actors-in-contexts; with the corollary that the researcher is also an actor-in-context(s) - an active, participating observer who must be wary of cognitive and theoretical arrogance. This implies that the process of research might not be one of discovering or establishing truth, but rather concerned with developing understanding and adequate models for specified purposes. Charmaz (2000) in her recent discussion of GTM makes many similar points, and concludes that 'we can adopt grounded theory strategies without embracing the positivist leanings of earlier proponents of grounded theory' (p 510).

The problem with GTM is that the method is offered in terms of both a qualitative, interpretive one, and a 'good, scientific' one. It is important that qualitative research should strive to be rigorous, but unfortunately the latter aspect of GTM has 'emerged' rather more strongly than the former, and it has done so in the guise of an idiosyncratic caricature of rigour, expressed in scientific terms. GTM writings are still predominantly couched in terms of an expert researcher dispassionately investigating a research domain. This obscures the point that research is an active engagement undertaken by researchers with their own assumptions, cultural backgrounds and predilections; and it is better to admit this and then seek to explicate the process as one of dialogue rather than as some form of dispassionate and detached analysis. The sections in italics in the extract from Turner exemplify the sources of strength and value in GTM.

In many regards it is surprising that what can be termed the hermeneutic or interpretive trend in GTM did not come to dominate the approach. Strauss came from a background influenced by symbolic interactionism, derived from the work of G.H. Mead and John Dewey. As Robrecht (1995) notes, the symbolic interactionist approach was founded on three premises; the actions of human beings are based on meanings that actors invoke as appropriate; meanings are derived from social interaction with others; meanings are dealt with/modified by people through interpretation and social experience. Yet these bases seem to have been effaced in

favour of claims for the method to be ‘good science,’ a tendency that was later exacerbated by the dispute between Glaser and Strauss. (Although Glaser is certainly justified in his criticism that Strauss & Corbin have sought to codify or systematize GTM to such an extent that it could be classified as one of the methods that were the main target of Glaser & Strauss in 1967.)

GTM AS A FUNDAMENTAL RESEARCH METHOD FOR IS

Where then does this leave the GTM approach? Why is it still an important consideration for IS research? Flood’s critique of Senge, and his lauding of Churchman, are based on the importance he gives to *boundary setting* and *judgement of relevance*. These, together with the issue of *ontological status* (the sense in which something actually exists), underline the importance of people’s perceptions in research activities. Research issues exist in the sense that some group of people agree on identifying and validating a particular topic – and thus identifying a boundary. Once these issues come to researchers’ attention, it becomes critical that some effort is expended in clarifying the nature of the topic, its boundaries, and its priorities. Consideration of such issues will necessitate attention being paid to the ontological bases of the topic. If the topic involves people, however, a further aspect of perception is involved, since the actors themselves will have perceptions that have to be taken into account; so attention must be given to the accounts of those involved: The constituting constituents.

GTM offers a range of methods, techniques and exemplars of ways in which researchers can ensure that they begin to take account of actors’ perceptions and actions. Baszanger & Dodier (1997) capture this in their discussion of ethnographic approaches, which they characterize as empirical; non-codified, remaining open, and grounded in observed phenomena. They discuss each of these separately, but in general their position can be summarized by their statement that –

[T]he principle of openness to what cannot *a priori* be pre-codified results in the basic tension underlying *in situ* studies. The flexibility required by this openness conflicts

with the need to maintain at least a minimum of method in the conduct of the study, that is, a certain guide for the behaviour *both of the fieldworker and the people observed*. (p 9, stress added)

Here is a far more acute and philosophically defensible basis for GTM – and kindred ethnographic approaches. Baszanger & Dodier note that such approaches are prone to a methodological problem whereby ‘the moment at which data are integrated into a whole occurs at an unknown, almost mysterious point of the process’ (p 14). This is a more honest view than that of theories *emerging* from the *data*. Yet they are firm in their conviction that methods such as GTM – together with the earlier Chicago tradition, and sociological pragmatics – offer a profound basis for research that ‘is no longer concerned with the search for references shared by actors ... [but one which] aims to take stock of the dynamic relationship between the real activities of individuals within the framework of complex, normative references which are related to the situation and are not unified’ (p 17).

They use Glaser and Strauss’ definition of GTM, a method of ‘constant comparison ...consisting of accumulating a series of individual cases, of analyzing them as a combination between different logics of action that coexist not only in the field under consideration, but even within these individuals or during their encounters’ (p 17). The aim of such methods is *generalization* rather than *totalization*, with the objective being to produce ‘a combinative inventory of possible situations.’ (Note the tentative view of the nature of research and knowledge.)

This interpretation of GTM, by two French writers, seems far clearer and more defensible than those offered even by Glaser and Strauss themselves. It echoes Charmaz’ argument and reinforces Turner’s interpretation of GTM that stresses that the outcome of research has to be ‘understandable and enlightening to individuals who have some familiarity with the social phenomena under investigation, either as participants or as *lay* observers.’ This can be achieved using GTM since it treats the accounts of members painstakingly and seriously, hence the

importance that all GTM research gives to the activity of *coding*: Something that Glaser and Strauss agree on, despite any of their mutual misgivings.

The methods and tools outlined by Glaser and Strauss provide guidance for researchers who seek to follow Churchman's precept that 'systemic thinking begins when you see through the eyes of another.' Many other researchers who have used GTM come to a similar conclusion in advocating GTM as an important part of a collaborative approach involving conceptualization from both the informant's and researcher's perspectives – e.g. Fitzgerald & Howcroft, 1998, Ellis, 1993, Bartlett et al, 1997.

The real mystery about GTM is why these aspects of the method have remained subjugated to those evoked by the mantra of 'theory emerging from the data'; and why the early statement that sociological theory is the exclusive estate of sociologists has remained largely unchallenged in the GTM literature. To date GTM has been widely misused; often as a catch-all that can be evoked as a justification for methodological inadequacies, or a qualitative loin-cloth to fool the gatekeepers of the academies. But this should not be allowed to detract from its strengths, and in particular its value for IS research.

Perhaps one of the reasons why GTM has had a particularly troubled relationship with IS practice and research is that issues around 'data,' and representation have proved enormously troublesome within the IS domain. The debates often revolve around the relationships between terms such as 'data,' 'information,' and more recently, 'knowledge,' now often joined by 'insight' and 'wisdom.' Many of those contributing to these discussions base their ideas on what has been termed the 'chemical engineering' model where *data* is the raw material from which *information* is extracted. This is a restatement of phenomenalism, since it implies that data exists waiting to be captured and processed. If there is any rationale for continuing to use the term *data*, then it is only in the sense of 'something that is stored in objects' - both inanimate and animate. Books, records, accounts, computer systems, CDs, disks and the like can be thought of as 'containing data';

but then so too do trees, plants, rocks, animals and people. Human beings do not, however, *extract* information from this raw material. As soon as humans turn their attention to any object, we are immediately in the realm of **meaning**. If data can be said to exist at all, data is the stuff of which by definition human beings are unaware.

People cannot engage directly in anything to do with data. Scanning a book into a computer is a data process; someone trying to read it - and make sense of it - involves information; because it inevitably involves meaning. Carbon-based entities are information-oriented; silicon-based ones are data-oriented. Information comes about because animate entities - particularly human beings - construct **meaning** and exchange ideas in order to exist as social beings and interact. Meaning construction is a key activity in all human processes. GTM, stripped of its scientific veneer, is oriented precisely towards this aspect of existence, and that is why it is such an important device for IS practise.

A large amount of misunderstanding about the nature of the process of constructing meaning emanates from the metaphorical imagery in which such discussions are couched. This has been explained elsewhere, particularly by Schön (1993) and Reddy (1993); and has been specifically applied to the field of information systems and software engineering (Bryant, 2000 and 2001).

The dominance of what Reddy terms the *conduit metaphor*, leads to the presumption that *information flows around a system from source to target*; from sender to receiver. (Exemplified by De Vreede et al implying that cognition is something that can be turned off and on like a tap.)

- (1) 'language functions like a conduit, transferring thoughts bodily from one person to another;
- (2) in writing and speaking, people insert their thoughts or feelings in the words;
- (3) words accomplish the transfer by containing the thoughts or feelings and conveying them to others; and
- (4) in listening or reading, people extract the thoughts and feelings once again from the words.' (Reddy, 1993)

This allocates primacy to the action of sending, and implies that receiving is a relatively passive process, at the most calling upon the repertoire of actions required 'merely' for extracting or decoding. This metaphor also obscures the point that what is sent is a series of *signals*; information is created in devising the message and in interpreting it. Reddy distinguishes between the signals and the selection processes that occur *at both ends of the process*. The thing that is sent is not the message but the signal; the message is what the sender wanted to communicate, and which may or may not correspond to the message derived by the receiver. Sending and receiving each require action and interpretation. This also applies to observation in the sense that the observer has to put effort into interpreting what is seen; nothing *emerges* without this activity.

Cilliers distinguishes between the 'mentalist' or 'functionalist' view of representation, and the 'connectionist' approach that treats representation as distributed (Cilliers, 1998, chapter 5). He refers to the work of Hilary Putnam since Putnam's early work provides the classic account of mentalism, while his later work offers a trenchant critique of his earlier position. Putnam specifically criticizes mentalism, describing it as 'the latest form taken by a more general tendency ... to think of concepts as scientifically describable ("psychologically real") entities in the mind or brain' (quoted in Cilliers, p 65). Mentalism is fallacious since meaning is 'holistic,' 'part of a normative notion,' 'dependent on environment,' and, Cilliers adds, 'historical.' Mentalism is based on a view of cognition as a process of 'extraction' of discrete items of data: Connectionism assumes that meaning is a property of a network of relationships, where the links between the nodes are as important as the nodes themselves.

Without going in to the details of Putnam's and Cilliers' respective positions, the general connectionist account severely undermines those who see cognition as a passive gathering of discrete 'data,' that can be accumulated and later classified into categories. It also impacts on the related position that characterizes communication as

an unmediated transfer of these data packets. Judgements are involved in deciding what constitutes 'data,' what constitutes 'similarity,' how categories and distinctions arise and are sustained. Furthermore, communication has to be treated as an active process, requiring effort by all involved.

As an example, consider the current trend in the field of IS and elsewhere to focus on knowledge and knowledge management. For Davenport and Prusak knowledge is defined as 'a fluid mix of framed experience, values, contextual information, and expert insight that provides a framework for evaluating and incorporating new experiences and information' (1998, p 5). Meehan (1999) in his brief survey of definitions of knowledge in the knowledge management literature, argues that definitions like Davenport and Prusak's are heavily slanted towards a 'technical-rational' view. Thus although Davenport and Prusak offer an initial definition of knowledge as a 'fluid mix,' a few pages after this, they state that '[N]ot only can *it* [knowledge] judge new situations and information in the light of what is already known, *it* judges and refines itself in responses to new situations and information' (p 11, quoted by Meehan, p 5, his stress added). Meehan rightly points to this as an example of knowledge as a disembodied object; and most definitely not a socially-constructed, human-centred process.

A similar objectification and disembodiment can be found in regard to the practices involved in developing information systems, particularly in specifying system requirements, where we often talk about 'requirements capture,' and use phrases such as 'get those requirements down on paper before we lose them,' or 'we've been trying to pin down that idea for ages.' (For a fuller account of this see Bryant, 2001.)

This tendency to objectify is not unique to IS. Indeed elsewhere I have argued that people seem generally and inherently drawn towards object abstractions rather than process ones – a tendency that can be referred to as *thinking* (Bryant, 2000). It is however, particularly acute as an issue for IS where there is a tendency to move from initial statements that afford significance to human

processes to methods and conclusions that are anything but human-centred and process-based.

IS researchers have to engage with issues concerned with the socially constructed nature of knowledge, adequacy of method, rigour of procedures, and extensibility of findings. GTM – in its remedied form – provides a useful barrier to such tendencies: Particularly if it keeps researchers anchored in the three premises that Robrecht traces from the interactionist perspective: Human actions are based on meanings that actors invoke as appropriate; meanings are derived from social interaction with others; meanings are dealt with/modified by people through interpretation and social experience.

Such postulates can provide a basis for undermining any inclination towards object-centred, mechanistic and technicist thinking. Furthermore if IS researchers take on board the insights of Turner, Charmaz, and others, then application of GTM can be seen to support moves that derive from ideas evoked by terms such as the ‘social construction of reality’ (Berger & Luckmann, 1971) and ‘structuration’ (Giddens, 1981). Here, in contrast to mechanistic and technical-rational views of information and knowledge, meaning is characterized as something that is continually constructed by social actors. Giddens’ theory of structuration distinguishes between *system* and *structure*. Social systems are ‘composed of patterns of relationships between actors or collectivities reproduced across time and space’ (p 26); whereas structures ‘have only a *virtual* existence.’ This existence has a dual nature - *the duality of structure* - in the sense that the structure ‘is both the medium and the outcome of the practices which constitute social systems’ (p 27). This is not to say that social actors do this in an arbitrary fashion: On the contrary we continually test and seek to confirm our own sets of meanings.

Meaning construction is a social activity, not an individual one. For the most part we do this all the time, without realizing that we are doing so. We only become conscious that we are doing this if someone draws our attention to it - as I am doing here: Or if something ‘goes wrong’; so that our

implied or assumed meanings or ascriptions fail to receive support from the context or events. Knowledge is then a human construct that arises from our actions as social beings producing and reproducing social systems against the capacities and constraints afforded by social structures. It also provides a resource for those actions.

All of this has extensive ramifications for IS. The IS research trajectory has to contend with the activities of knowledgeable social actors and their stocks of knowledge. Research and investigation cannot be undertaken on the assumption that people can simply be questioned, counted and processed; but neither can it be undertaken on the basis that they can simply be observed and recorded. The IS agenda has to involve engagement and collaborative construction, involving both the participants and the researchers. GTM presents an approach that directs researchers to consider any selected research context as problematic and non-obvious. A challenge that can only be met with a contribution from the social actors involved with the context, but which cannot be wholly or ultimately reliant on participants’ own accounts. The techniques the non-positivist practitioners of GTM apply and exemplify offer a range of heuristics and guidelines for the conduct of such research. If we can become aware of the philosophical contradictions and methodological inconsistencies of GTM, and move beyond them to a clearer view of the strengths it offers, then our own IS research will yield richer results, and some of the scepticism concerning GTM should start to dissipate.

We also have to engage with issues such as those raised by Flood - boundaries and judgements of relevance. As Baszanger & Dodier argue, GTM offers a basis for doing this, since it provides researchers with a series of exemplars for developing research with these objectives and constraints clearly in mind; but where the topic of research is non-codified and disparate, having to take people’s perceptions and beliefs into account. In this sense Turner is correct to note that ‘there is no orthodoxy in grounded theory, and I do not think that it is necessary or desirable that such an orthodoxy should develop.’ (p 347) He concurs with Glaser’s contention that GTM is

an approach that develops through activities be added *understanding, engaging,*
best 'expressed as a gerund: negotiating, *challenging and contesting.*
encountering, complaining': To which might

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