

## Reinventing grounded theory: some questions about theory, ground and discovery

Thomas, Gary; James, D

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# **REINVENTING GROUNDED THEORY: SOME QUESTIONS ABOUT THEORY, GROUND AND DISCOVERY**

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**Gary Thomas      and      David James**

School of Education

Faculty of Education

University of Birmingham

University of the West of England

Birmingham B15 2TT

Bristol

0121 414 4836

mob: 0770 347 9052

[g.thomas.3@bham.ac.uk](mailto:g.thomas.3@bham.ac.uk)

# REINVENTING GROUNDED THEORY: SOME QUESTIONS ABOUT THEORY, GROUND AND DISCOVERY

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ABSTRACT: Grounded theory's popularity persists after three decades of broad-ranging critique. We discuss here three problematic notions – “theory,” “ground” and “discovery” – which linger in the continuing use and development of grounded theory procedures. We argue that far from providing the epistemic security promised by grounded theory, these notions – embodied in continuing reinventions of grounded theory – constrain and distort qualitative inquiry. We argue that what is contrived is not in fact theory in any meaningful sense, that “ground” is a misnomer when talking about interpretation and that what ultimately materializes following grounded theory procedures is less like discovery and more akin to invention. The procedures admittedly provide signposts for qualitative inquirers, but educational researchers should be wary, for the significance of interpretation, narrative and reflection can be undermined in the procedures of grounded theory.

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# REINVENTING GROUNDED THEORY: SOME QUESTIONS ABOUT THEORY, GROUND AND DISCOVERY

Grounded theory was developed and established nearly forty years ago by Barney Glaser and Anselm Strauss. Their 1967 book *The Discovery of Grounded Theory* laid out a set of procedures for the generation of theory from empirical data. There can be little doubt that it has been a major – perhaps *the* major – contributor to the acceptance of the legitimacy of qualitative methods in applied social research.

The origins and context of this seminal work are important. It was conceived at time when symbolic interactionism was suffering a decline, partly due to what some saw as its celebration of liberal individualism, but principally due to pincer-like pressure from, as Alvesson and Sköldbberg (2000) put it, “the ‘hard’ methods such as statistical method and structural functionalism on the one hand, and competition from the ‘soft’ side in the form of ethnomethodology ... on the other” (p. 13). Seen historically, grounded theory represented a resolution of different epistemological positions<sup>1</sup> and a solution to a broader problem about perceptions of the status of qualitatively based knowledge in the social sciences.

Four decades on, grounded theory continues to be used in a wide range of research settings, and is especially highly regarded as a method of social analysis in fields such as education and health studies<sup>2</sup>. Despite much critique, it continues to enjoy great kudos amongst educators, to the extent that its use can still seemingly validate the publication of a study’s findings (see, for example, Harry et al, 2005). Strauss and Corbin (1997) summarize the contemporary status of grounded theory when they say that grounded theory’s methods are “... now among the most influential and widely used modes of carrying out qualitative research when generating theory is the researcher’s principal aim” (p. vii).

There can be little doubt that Strauss and Corbin are accurate when they make this large assertion. As Miller and Fredericks (1999) put it, the grounded theory approach has become the “paradigm of choice” for qualitative researchers in education and other disciplines. And as Denzin (1994, p. 508) has noted, “The grounded theory perspective is the most widely used qualitative interpretive framework in the social sciences today.”

Grounded theory, and other techniques of analysis in qualitative inquiry, are bound to be popular, because they meet a need. For while qualitative inquiry is absolutely valid, it is difficult to do. In education it may involve talking – as naturally as possible – with students, parents, teachers; it may entail taking part, watching and listening, in schools and other environments. But when all this is

done, what comes next? Such ways of doing research can lead to a floating feeling, a lack of direction. What does one do with one's data? Surely one can't just talk about it. Grounded theory offers a solution: a set of procedures, and a means of generating theory. As such, it has become widely used and its reputation as an accessible and thoroughly explained method in qualitative inquiry has grown and grown.

The purpose of this article is to challenge the continuing legitimacy of grounded theory and the lofty place its methods have come to hold in social and institutional analysis. We dispute grounded theory's status as theory, and the assertion that it can be 'discovered'; we contest its claim to be consistent with the tenets of qualitative inquiry, and we question its claims to produce better predictive and explanatory outcomes than other methods.

Certain of these criticisms develop out of the work of others, and it is necessary first to acknowledge existing critiques of grounded theory and to note our points of similarity and difference with them. In brief, these criticisms centre on three broad themes: first, that grounded theory oversimplifies complex meanings and interrelationships in data; second, that it constrains analysis, putting the cart (procedure) before the horse (interpretation), and third that it depends upon inappropriate models of induction and asserts from them equally inappropriate claims to explanation and prediction. Let us look at some of the critical literature which exemplifies this debate, and indicate both our points of agreement and suggested foci for development.

Layder (1993) identifies four limitations. First, he says, a grounded theory approach has the effect of highlighting the immediately apparent and observable at the expense of attending to the interweaving of structural features of social situations with activities. Second, he argues that the focus on the "here and now of everyday encounters" limits the concept of power that is possible in the approach. Third, he contends that the development of theory in a grounded theory approach should be more *guided* by data than *limited* by it. And last, he suggests that the insistence that grounded theory should be recognizable to the people studied (encapsulated in the notions of "fit" and "relevance") places unhelpful constraints on analysis because it rules out features and interpretations which they could not be expected to have considered.

Others, such as Haig (1995), have queried grounded theory's reliance on a naïve model of scientific induction, inappropriate to the tenets of qualitative inquiry. Haig, together with others such as Miller and Fredericks (1999) point to what one might call the "everydayness" of inductive reasoning. The thrust of their argument is that this common, everyday induction is better described as "inference to

the best explanation” or what C.S. Peirce called “abduction.” This critique, and in particular the distinction drawn between looser and tighter kinds of induction, is important to this article and it is expanded in more detail later.

Robrecht (1995) notes that the elaboration of sampling procedures by Strauss and Corbin (which include instructions for techniques for open sampling, relational and variational sampling and discriminate sampling) divert attention from the data toward techniques and procedures. As Robrecht puts it, these elaborations encourage researchers “to *look for data* rather than *look at data*” (p. 171, original emphases). She recommends an alternative approach: that of Leonard Schatzman (1991), which seeks to extend the natural analytic process of everyday thinking by systematically examining different concepts that might be used to summarize events. The emphasis on that natural analytic process is a significant one and is a theme that will be rehearsed throughout this article.

A comprehensive list of criticisms is made by Dey (1999), who presents a host of ambiguities, confusions and unanswered questions. For example, he illustrates how tensions in the original formulation of the approach are revealed in the gulf that developed between the originators (see Glaser, 1992), such that the role of verification remains a “puzzle” and the role of prior theory is unclear. Despite this critique, though, Dey maintains a surprising degree of optimism – even faith – in the approach. He sees grounded theory as capable of development and as offering a “middle way” between ideographic and nomothetic theorizing.

Charmaz has over two decades been a developer of new forms of grounded theory (e.g. Charmaz, 1988; 2000) and her voice has been lucid in enabling its evolution. Some years ago, she offered a development of grounded theory which depended on an endorsement of five principles which kept faith with “the form and logic” of the Glaser and Strauss approach. Those five principles were: the structuring of inquiry; the simultaneity of data collection and analysis; the generation of new theory rather than the verification of existing theory; the refinement and exhaustion of conceptual categories through theoretical sampling, and the direction to “more abstract analytic levels” (1988, p. 125). More recently she has asserted that “grounded theory can bridge traditional positivistic methods with interpretative methods” (Charmaz, 1995, p. 30). She rejects the claims to disinterestedness and objectivity present in earlier versions of grounded theory, noting that “The myth of silent authorship is false but reassuring” (Charmaz and Mitchell, 1996, p. 299). She distinguishes between “objectivist” and “constructivist” grounded theory, aligning the former with “...awkward scientific terms and clumsy categories” (Charmaz, 2000, p. 525). The result may be “...an overly complex architecture that obscures experience” (ibid.). She offers a “simplified, constructivist version of grounded theory” (p.

514); “Emphases on action and process and, from my constructivist point of view, meaning and emergence within symbolic interactionism complement grounded theory.” (p. 513)

These latter are helpful developments in the move to open qualitative inquiry, faithful to the thinking which gave rise to it and continues to inspire it. However, we challenge the need to call these forms of qualitative inquiry “grounded theory” and we suggest that in so doing – in continuing to want to call them “grounded theory” with the corollary of the continued adherence to its precepts and techniques – there proceed two unwelcome sequelae. First, the claims of qualitative inquiry to be taken seriously in its own right (without the use of special method) are undermined, and second, unwelcome constraints on open, creative interpretation are imposed. We discuss this in more detail toward the end of this article.

We take issue here with the conciliatory nature of the critical discourse about grounded theory. Dey seeks his “middle way.” Layder seeks an “amended” grounded theory with a broader scope, ending up with a “realist approach” or “adaptive theory” (Layder, 1993; 1998) – which looks (at least to us) very much like grounded theory. Likewise, Haig looks for ways around the critique he outlines in an adaptation of grounded theory. Charmaz proceeds with a constructivist version of grounded theory which “takes a middle ground between postmodernism and positivism” (2000, p. 510). Unlike these critics, though, we cannot see how there can be a “middle way” or new versions. For us, the problems with grounded theory preclude any possibility of modification or retrenchment.

Our arguments about the pretensions of grounded theory in education centre on a number of interrelated issues concerning the three key words in the seminal text: “theory,” “discovery” and, underpinning both, the notion of “ground.” In examining these we shall draw upon the concerns of other critics – for instance, about claims to prediction – but attempt to integrate the concerns of those critics in what we consider to be some problematic issues concerning theory and discovery. It is the sidestepping of these major concerns that leads to continued attempts to re-clothe and revitalize grounded theory. Underpinning both of these concerns about theory and discovery are considerations about ground – which we see as a metaphor for some implicit assumptions about the mind of the researcher and about social reality, what we can know about it, and how we can arrive at that knowledge. “Ground,” with its intimations of solidity and fixity, simply does not mix with “construction,” with its contrasting intimations of the tenuous, the mutable, the interpreted.

## Theory

### *Why is grounded theory “theory?”*

“Theory” has not formed a major focus in the critique of grounded theory. Yet the claim for grounded theory actually to be *theory* raises some questions about what it is to be theory, what is demanded and expected of theory, and why people expect their methods-for-making-sense to be called “theory.”

In the preface of *The Discovery of Grounded Theory*, Glaser and Strauss justify the project of their book by noting that the interpretative sociologists of the Chicago tradition are associated with “an unintegrated presentation of theory” (p. vii). At the very outset, then, something interesting emerges about these authors’ views concerning knowledge and how it should be pursued. This can be summarized roughly as follows: qualitative research is valid, appropriate and necessary, but it does itself no favours by having no methodology to speak of, and by being unable to demonstrate how it develops theory. The latter – the development of theory – seems to be viewed as the *sine qua non* of serious inquiry.

Given the widespread use of grounded theory as a method of qualitative inquiry, it is important to recognize the character of the theory that Glaser, Strauss and their successors (for example, Charmaz, *op cit*) have consistently said they are seeking here. Much of the importance of theory development for them concerns notions of induction: theory is taken to be part of the canonical apparatus which allow us to make inferences and proffer explanations based on inductive processes.

Deep water is being traversed here. The issue of what theory might be in qualitative inquiry is a tricky one (see for example Woods, 1992), since the word “theory” has taken a wide variety of meanings, loose and tight, particularly in educational discourse. It can mean systems of evolving explanation, personal reflection, orienting principle, epistemological presupposition, developed argument, craft knowledge, and more (see, for example, Scheffler, 1967; Chambers, 1992; Thomas, 1997). It is not, of course, only in education that one finds these definitional conundrums aired. In sociological discourse, widely differing conceptions on the nature of theory are to be found, even in the same volume (compare, for example – all in the same book – Martindale, 1979; Cicourel, 1979; Snizek, 1979).

Given the plurality of view on this issue of theory, it is not surprising that Stanley Fish (1989) rejects the meaningfulness of any of this discourse. He contends that such discourse about theory in the academy is not really about theory at all, but rather about “theory talk” – that is, “any form of talk



that has acquired cachet and prestige” (1989, pp. 14-15). He surely has a point and it is necessary here to try to draw on some of this definitional discussion, since – in the invocation of theory and the cachet it brings with it for inquiry – important issues about finding out, discovery, explanation and prediction are at stake. Are the theoretical claims of grounded theory valid? Why should grounded theorists want, in “discovering” something, to call that which is discovered “theory?”

Miller and Fredericks (1999, p. 539) offer some help here. They say that it is possible to see grounded theory in three main ways: i) as an approach in “the logic of discovery;” ii) as being either “accommodationist or predictivist;” iii) as a variant of “inference to the best explanation.” This is a useful setting of scene, though we would prefer for simplicity’s sake to reframe their taxonomy thus: theory can, broadly speaking, be seen as being about a) inspiration involving patterning or accommodation (see, for example, Kaplan, 1964, p. 332, for a discussion of patterning); b) explanation and prediction. In its former, looser, sense it is principally about bringing ideas together, while in its latter, tighter form it adheres to positivist and functionalist expectations about explanation. The use of the word “theory” as though it described one inductionist process camouflages the confusion occurring here, and it enables further confusions to occur about the purposes of qualitative inquiry.

The “theoretical” notion in grounded theory, in other words, conflates and confuses two processes in inquiry. It conjoins the spark to inspiration – exemplified in Köhler’s apes’ “Ah Ha!” (Köhler, 1925) or Archimedes’s “Eureka!” – with the predictive function of theory in the natural sciences and in functionalism. For describing what happens in qualitative research, the use of the term “theory” only confuses what is going on. The former type – involving tacit patterning, interpretation and inspiration – is really a vernacular employment of the term “theory” (“I have a theory why my geraniums are dying”), and is part of everyday reasoning and Schatzman’s (1991, p. 304) “common interpretive acts.” The latter is about generalization following systematic and extensive data collection, and the testing of the generalization for the purposes of verification or falsification.

Thus, to the question of Miller and Fredericks (1999) “How does grounded theory explain?” we would answer, “It doesn’t,” because grounded theory procedures are a scion of qualitative inquiry, and qualitative inquiry is about interpretation. It is about – using Ricoeur’s (1970, p. 33) well known distinction – understanding before explanation. But on both counts, the former and the latter, theory as discovery and theory as part of an explanatory exercise, grounded theory fails to live up to its proponents’ expectations. It fails because it promises too much – because it is unsatisfied with “mere” understanding.

We explore the claims to “discovery” below (under *Discovery or invention?*), but it is perhaps worth noting here some concern about the linking of theory and discovery when discovery is taken to mean creativity or finding out. In grounded theory one sees signposts to investigatory avenues borrowed from natural scientific endeavour, but one should note here that natural scientists, whose notionally inductive-predictive theory is that commended by grounded theorists to the present day, hold far less firmly nowadays to the procedural guyropes they once cherished. Beck, a Nobel prize winning physicist noted that “The mechanics of discovery are not known ... I think that the creative process is so closely tied in with the emotional structure of an individual ... that ... it is a poor subject for generalization” (cited by Wright Mills 1970, p. 69). Thought actually moves forward, Feyerabend (1993) asserted, by “a maze of interactions ... by accidents and conjunctures and curious juxtapositions of events.” Similarly, Einstein noted that the creative scientist must be an “unscrupulous opportunist” and that the essence of science is the seeking “in whatever manner is suitable, a simplified and lucid image of the world ... There is no logical path, but only intuition” (cited in Holton, 1995, p. 168). In other words, Feyerabend concludes, “the only principle that does not inhibit progress is *anything goes*” (1993, p. 14).

One must be careful, in other words, that in creating something called “theory” (together with a set of procedural accompaniments for finding it) one does not inhibit rather than liberate discovery. One must be careful that fertility is not sacrificed to orderliness. Theory, if Einstein is right, does not give birth to discovery: it patterns, systematizes and tidies cognitive leaps, having painstakingly employed the “inference tickets” of Ryle (of which more below); it cannot act as a vehicle for creativity.

Theory in a loose, vernacular sense – to go back to “I have a theory why my geraniums are dying” – is indeed about conjecture, and, if you like, about creativity. But this is far from what happens when theory is used scientifically – in natural or social science, a matter which Cicourel (1979) recognizes in his distinction between the “weak theory” associated with the inductive assumptions of field research, and “strong theory.” Charmaz (1995, p. 28) also implicitly recognizes a distinction of this kind in her acknowledgement that grounded theory represents what she calls “middle range theories to explain behavior and processes.” The “explain” is the problematic word here. “Middle range,” “weak,” “protoscientific” or vernacular theories do not explain anything, since the sophisticated “inference ticket” procedures (described in the following section) are not present to enable it. Instead, they help us to understand. Understanding is a no less worthy ambition and there is a paradox in grounded theorists’ continuing strivings for explanation.

Staying on theory as creativity for a moment, the problem comes for grounded theory (or indeed almost any theory in the social sciences) in claiming some status for one's *theory* ("middle range," "weak" or "protoscientific") as an instrument which works beyond the level of one's everyday patterning generalization and practical syllogism – beyond Polanyi's (1958) *personal knowledge* or the "everyday knowledge" of Schutz and Luckmann (1974). For *theory* to be worth something, it must involve something more than these everyday patternings and tacit heuristic exercises, for such patternings and such exercises are what human beings do all the time *par excellence*. As Schatzman (1991) notes, we are all the time using "common interpretive acts" (p. 304) and unselfconsciously using these to help us order and comprehend the world. We all see links, discover patterns, make generalizations, create explanatory propositions – weak theory, if you like – all the time, emerging out of our experience, and this is all "empirical." The problems come when too much is claimed for it, simply because it is empirical; problems come in distinguishing generalization from over-generalization, narrative from induction. As Bertrand Russell (1956) notes on theorizing and induction, the person who theorizes that unsupported bodies in air fall, in fact "has merely generalised, and is liable to be refuted by balloons, butterflies and aeroplanes" (p. 91). Alasdair MacIntyre (1981, p. 91) suggests that social science's generalisations are not generalisations in any meaningful sense – that is to say, 'they are not only not genuinely of the form "For all x and some y if x has property  $\Phi$  then y has property  $\Psi$ " but we cannot say of them in any precise way under what conditions they hold'. Devotees of grounded theory have yet to make a case that their kind of theory possesses characteristics of induction in the way that natural scientists' theories may.

The epistemological weapons of "creative theory," in other words, have to offer more than natural everyday analytic skill, yet one cannot be confident that they will ever do such. Indeed, as Geertz (1975) says, the marriage of subjectivism to formalism which such expectations for theory involve, often result in sterile debate about whether "analyses ... in the form of taxonomies, paradigms, tables, trees, and other ingenuities ... are merely clever simulations" (p. 11). What more, in other words, does any of this kind of theorizing give over and above our everyday theorizing? How does grounded theory stand above the patterning of our everyday lives and everyday experience? One has to caution against the possibility that, as Miller and Fredericks (*ibid*) put it "... grounded theory is basically a way of making an inductive argument dressed up in a new label" (p. 548).

### *Theory: riding two horses*

Before looking in detail in the next section at claims to induction one needs to examine a more general respect for theory in qualitative research – a respect to which seekers of grounded theory appear to be prey. Such an examination exposes for scrutiny a kind of reasoning which leads to the belief that theory is essential to qualitative and ethnographic endeavour. The source of this is a confusion noted

by Mouzelis (1995): it is rooted in a blurring of the distinction we make above about inspiration/patterning versus explanation/prediction. He too points out that there are essentially two kinds of theory used by social scientists and that the refusal to distinguish between the two (or, more likely, the neglect to make the distinction) "... has social scientists talking at cross purposes" (p. 2). The two kinds of theory are: i) theory as tools for thinking or, as Nadel (1957) put it, tools "which serve to *map out* the problem area" (p. 1), and ii) theory as a set of statements telling us something new about the social world and which can be proved or disproved by empirical investigation. Mouzelis proceeds to note that the separation is similar to that made by Althusser when he set out to distinguish between theory as a tool/means, which he termed "Generalities II," and theory as a provisional end-product: "Generalities III" (Althusser, 1969, pp. 183-90). A similar distinction is drawn by Bourdieu, who says "... I never set out to 'do theory' or to 'construct a theory' *per se* ... There is no doubt a theory in my work, or, better, a set of *thinking tools* visible through the results they yield." (Bourdieu, in Wacquant, 1989, p. 50, emphasis in original; and see Grenfell and James, 1998, pp. 152-158)

What certain of the theorists of qualitative inquiry want to do is to ride two horses at once. They want, borrowing Mouzelis's (1995) words, to avoid "unfashionable functionalist vocabulary ... while retaining its fundamental logic – with the result that crypto-functionalist elements and related distinctions are clandestinely reintroduced into their writings" (p. 7). The problems come in the linking of theory – with its intimations of regularity and generality – to induction. But, as Hans-Georg Gadamer (1975) suggests, "... one has not grasped the nature of the human sciences if one measures them by the yardstick of an increasing knowledge of regularity. The experience of the socio-historical world cannot be raised to a science by the inductive procedure of the natural sciences" (p. 6). Or, as Howard Becker (1993) puts it, more bluntly, "Who are we kidding with all this science talk?" (p. 218).

The essential problem here, then, is in the conflating of the expectations of one kind of theory with another. The problem is claiming that one set of natural processes of understanding and patterning with the claims to explanation found in natural science. The problem is with "crypto-functionalism." But one might add in parenthesis here that the odd thing about grounded theorists' exposition is that there is nothing at all "crypto" or clandestine about the reintroduction of the functionalist elements. Far from being in any way hidden, the functionalist elements are central: they are written in big red letters. Grounded theory is functionalism incarnate. Far from being embarrassed about it, Glaser and Strauss make much of grounded theory's reputed success in providing "relevant predictions, explanations, interpretations and applications" (1967, p. 1). And their successors make claims that are scarcely weaker.

Stirred in with all this “science talk” – indeed, central to it for both for Glaser and Strauss and for newer exponents such as Charmaz (1995, p. 27) – is the linking of theory with inductive reasoning. It is this to which we now turn.

### *Theory and the inductive reasoning claim*

As we have noted, proponents of grounded theory make much of the place of grounded theorizing in the repertoire of inductive inquiry (as distinct from what they call “logically deduced” theory). What proponents of grounded theory are claiming here is that grounded theory, once established, will occupy a privileged epistemic place. The problem, they say, with work which depends on “logically deduced” theory is that the cart comes before the horse: a previously contrived theory is confirmed with evidence. The spark to action in that way of going about things comes from *previously devised* theory – then researchers, untrained in the ways of inductive inquiry, go out and merely *verify* their theories through the collection of data. This, grounded theorists claim, is the wrong way round. Consistent with the tenets of inductive reasoning, grounded theorists dispassionately gather the data and then pull the strand cleanly from it. The theory emerges from the data.

Their confidence about the legitimacy and usefulness of the separation of one kind of thinking and inquiry from another – inductive from deductive – is perhaps a product of the time in which pioneers Glaser and Strauss were writing, but the reliance on the importance of the inductive continues in more recent expositions (for example, Charmaz, 1988; Strauss and Corbin, 1998). With the continuing prominence given to this inductive notion, it is worth spending a moment or two on the genealogy of thinking about the “correctness” of inquiry methods, for this separation of correct from incorrect, of inductive from deductive, was symptomatic of a more general tendency in the social sciences in the 1960s and 70s to assume that there was a Right Way of thinking and of inquiry. For example, many psychologists of the 1960s and 70s were, like Glaser and Strauss, confident about inductive method when they celebrated the psychological offer to education. The distinguished behavioural psychologist Stanley Bijou (1970) confidently held up empirico-inductive inquiry (as distinct from the more suspect hypothetico-deductive inquiry) in telling the world “What psychology has to offer education – now” (p. 65). Inductive analysis is held up with pride, almost as a badge of office for the serious social analyst.

But people tend to talk with less certainty now about the relative benefits of one kind of method over another. Even back then, though, a third of a century ago, more reflective social scientists were wondering whether indeed notions like induction were in fact at all useful in the human arena. Even then, for example, respected psychologist Sigmund Koch (1964) could say:

In every period of our history we psychologists have looked to external sources in the scholarly culture – especially natural science and the philosophy of science – for our sense of direction. And typically we have embraced policies long out of date in those very sources ... (pp. 4-5)

A realization has been dawning that the processes of discovery are not as discrete and separable as the use of terms like “inductive theory” and “logically induced” imply, something about which Popper (1989) wrote so forcefully. Popper asserted that science advances not by induction but by a process of “conjectures and refutations.” It is imagination and creativity, not induction, that generates real scientific theories, Popper suggested, which is how Einstein could study the universe and change physics with little more than a piece of chalk. As Popper (*ibid*) put it “The belief that we use induction is simply a mistake. It is a kind of optical illusion.”

The prescient sociologist Wright Mills (1970), also writing, it should be noted, at the same time as Bijou and Glaser and Strauss, quotes a number of Nobel Prize-winning physicists on the fluid and unbounded nature of thinking, even in that “hard” reductionist science. One of these says (in the gendered language of the time, for which, apologies) that “There is no scientific method as such, but the vital feature of the scientist’s procedure has been merely to do his utmost with his mind, *no holds barred*” [original emphasis] (Wright Mills 1970, p. 69).

It is worth stressing the continuation of this theme in scientists’ discussions of inductive method. The tone is one self-effacement rather than certainty. The renowned biologist Peter Medawar is specific in his doubt about the supposed benefits of induction, noting that “... the influence of inductivism ... has in the main been mischievous” (1982, p. 81) in its effects on social scientists. This is mainly because, he says, there is little real distinction in fact to be made between deductivism and inductivism. These words merely relate to “postures we choose to be seen in when the curtain goes up and the public sees us.” (*ibid*. p. 88). Diffidence is the hallmark of the modern natural scientist when it comes to reflection on method.

But such diffidence, such tentativeness, about ways of finding out is not for grounded theorists: induction is good, and it is bound together with prediction in their thesis. And it is important to remember that faith in these tenets continues (see Strauss and Corbin, 1997, 1998). Induction is good, evidently, because it enables one to derive theory which will, in turn, enable one to order and to predict.

Let us unravel the implications of this a little further. It all depends on what is meant by “predict” in our educational worlds, and whether the “theory” that emerges can predict any better than our everyday judgments, or better indeed than any formula that will – by good luck – accurately predict. Ptolemy constructed an intricate calculus, increasingly refined with all sorts of bells and whistles (“epicycles” and “equants”), for explaining and predicting the anomalous movement of the planets in what he assumed to be an earth-centred universe (see Russell, 1991). It was accurate enough – more accurate, indeed, than early Copernican calculations could manage – yet few would now be persuaded that the accuracy of his predictions added any veracity at all to his geocentric theory. Predictive accuracy does not make for explanatory veracity. On its own it’s not enough: it doesn’t provide what Ryle (1990) calls an inference ticket.

There is a difference between the noticing of an association and the confident issuing of an inference ticket. This difference is important, for we are invited to take grounded theory seriously precisely because *theory* – the jewel in epistemology’s crown – is being established. We are talking, remember, about theory’s power, according to Glaser and Strauss, in enabling “... predictions, explanations, interpretations and applications” and the claim to explanation is no less muted in today’s grounded theorists. We are not, in other words, talking about garden-variety noticing, or about everyday generalization.

The claim being advanced here is that grounded theory provides us with something more secure epistemologically than everyday noticing. It provides more, for example, than simply the modest “epistemic sieve” spoken of by Crews (1997) or the everyday “protoscientific” theory spoken about by Russell (1992).<sup>3</sup> In place of these weak contrivances, a power-tool is being proffered which works at a level above and beyond the fragile epistemic devices with which we fashion judgments and decisions in our everyday lives. The inferences provided by the theory are better than other inferences. It is worth taking a moment to consider this, since it is a profound and important assertion.

It rests implicitly for its legitimacy in associating itself with the power of other kinds of theory (notably the theory of the natural sciences) without providing an account of the provenance of the legitimacy of “grounded theory” itself. Where, in other words, are the “inference tickets” coming from in the supposed process of induction involved in grounded theorizing? This is a crucial issue, given the claims of grounded theory’s proponents.

As Ryle notes, natural scientists do have clear methods for establishing these inference tickets: “Bacteriologists do discover causal connexions between bacteria and diseases ... and so provide

themselves with inference tickets which enable them to infer from diseases to bacteria” (1990, p. 117). But Ryle goes on to point out that these inference tickets – pointing to causality and its direction – depend for their validity on the quality of the associated fact-finding and reasoning. Since Ryle’s death an example in just that field makes precisely his case. When the association was noted between the bacterium *Helicobacter pylori* and the presence of ulcers in patients with abdominal complaints, a wide range of additional work had to be undertaken to demonstrate that *H. pylori* was in fact the *cause* of the ulcer (see Blaser, 1996). It was perfectly possible, for example, that a weakening of the immune system brought on by the *prior* existence of an ulcer (the cause of which was uncertain) merely enabled adventitious colonization by *H. pylori*, rather than the ulcer itself being caused by *H. pylori*. The direction of causation, in other words, was still in doubt before the undertaking of considerable additional work to establish that direction – before the finger could be pointed at *H. pylori* as villain rather than merely freeloader. Ryle’s point is that the mere notion of association and the mere notion of inference, coupled with a metaphor such as “the rails of inference” adds almost a third dimension to what is really a narrative.

The association in itself, then, is insubstantial; the “inference ticket” has to be painstakingly enabled by the additional work of a broad research community who replicate, test alternative hypotheses and attempt to falsify. Only after these processes are complete do the grand ambitions of induction in natural science – namely, prediction and explanation – begin to be realizable.

The problem, says Ryle, is that functional differences between arguments and narratives are often obliterated in our everyday discourse. And here is the problem for grounded theory. Interpretations of the kind made in grounded theory research offer, on their own, no inference tickets – they enable no prediction or explanation, or at least no better prediction or explanation than any of us would make on the basis of our many years of experience of being human.

What such interpretations offer is merely a narrative. But the point is not to be apologetic about narrative in social analysis. Narrative can be argued to offer more in the way of enlightenment than putative theory, while forsaking its epistemic pretensions. By saying it is merely a narrative, we are saying that it is not a narrative *and* something else: rather, it is a narrative and nothing else. There’s no shame to be admitted in this. Nor does one assert that the ideographic constitutes an illegitimate kind of knowledge in educational inquiry. The particular and the narrative – the vignette, the portrait and the story – are valid and proper ways of doing educational inquiry. The legitimacy of this is at the root of Gadamer’s hermeneutics.<sup>4</sup>



Especially peculiar and, in our view, objectionable, is the claim that the professional methodologist's armoury contains a set of procedures and techniques (such as "saturation," and "theoretical sampling") that will transform an understanding of narratives or events into some kind of inductive "theory."<sup>5</sup> As Eisner reminds us, there is in much scientific and social scientific endeavour the highly problematic assumption that procedural objectivity (that is, the attempt to eliminate the scope for judgment) will lead us to ontological objectivity (that is, seeing things the way they *really* are). As he puts it, "... consensus achieved through procedural objectivity provides no purchase on reality. It merely demonstrates that people can agree" (Eisner, 1993, p. 53). In grounded theory, a set of "neutral" analytical procedures replace the "neutral" controls and treatments of the experimental situation in the hope that they will provide the same guaranteed route to an uncontaminated correspondence.

The mistake in much of the reasoning of those who propose grounded theory is to assume that qualitative inquiry can in any way share elements or end-points congruent with those of scientific inquiry. Not only is it to assume a continuing need for, to paraphrase Rorty (1979), *commensuration* rather than *conversation*, it is to import with that assumption all the methodological algorithms – of which theory is only one – of commensuration. It is strange that some social scientists should continue to be so attracted by these methodological charms while their "natural" scientific counterparts have become more reflective and less self-assured about the benefits of method. As we noted above, many of the latter question what uniquely is gained from induction and the extent to which it is separable from deduction, imagination, inspiration, insight or daydream in enabling shifts forward in what Ziman (1991) has uncomplicatedly called *Reliable Knowledge*. The result has been less arrogant conviction of late about method and more acceptance of the truth of Canguilhem's (1994, p. 41) assertion that scientific advance cannot be defined much more satisfactorily than the "elimination of the false by the true."

### *Theory, grounded theory and qualitative inquiry*

When one argues for the validity of qualitative inquiry one is arguing for a reinstatement of the validity of interpretation and understanding in a social world – and all educational worlds are inevitably social. That understanding is built out of what we, as people, make of others' – teachers', parents', children's – utterances, gestures and actions. They are built, then, out of what Oakeshott (1967) called

an inheritance of feelings, emotions, images, visions, thoughts, beliefs, ideas, understandings, intellectual and practical enterprises, languages, relationships, organizations, canons and maxims of conduct, procedures, rituals, skills, works of art, books, musical compositions, tools, artefacts and utensils – in short, what Dilthey called a *geistige Welt*. (p. 157)

Interpretations are built, in other words, out of what it is to be human. A method will not substitute for the essentials of this humanity; it will not enable one to substitute some formula for divining meaning which is not provided by the inheritance spoken of by Oakeshott. And why should one expect any method to provide such a surrogate? Whence emerged the urge to move beyond everyday understanding and discover some methodological proxy? The answer lies at least partly in the notion of *theory* in “grounded theory,” for in the search for such theory one can recognize a familiar melody line in social research. It is the search for explanatory phenomena which become accessible via a neutral observation language wherein the observer rises above the merely contingent and interpretable.

Brad Sherman (1988), in talking about hermeneutics and the law, suggested that most nominally hermeneutic discussion is in fact characterized by a lack of understanding of hermeneutics; it offers “an unhermeneutical approach to hermeneutics” (p. 395). Our position here, to paraphrase him, is that the search for grounded theory is characterized by an unqualitative approach to qualitative inquiry. The search for the kind of theory hankered after by grounded theorists reveals a range of tacit assumptions and expectations which disclose grounded theory (and, indeed, many of the techniques of nominally qualitative inquiry) to be merely part of another foundationalist enterprise.

Followers of positivism, so reviled by the new theorists of the social sciences, could at least be said to have established methodological groundrules which were true to positivism’s tenets. Grounded theorists, by contrast, want it both ways. They want the comfortable feeling which comes from a denial of the arrogance of foundationalism and essentialism – and this is understandable enough. But they want this insignia of intellectual adulthood while clinging on to an epistemological security blanket – one woven from the associated notions that a) some clearer distillation of truth can be established about the particulars and generalities of social behaviour, and b) that this can be established using the cogs and levers of structured inquiry. Far from the notion that understanding is irreducible, their project is to step aside from the ineffable and rise above it to emerge with theory.

The significance of this extends beyond the status and acceptability of grounded theory. It stretches to the nature of educational inquiry itself, and its place in shaping educational discourse and the educational enterprise. For grounded theory is merely an *example* – it is not an exception to the rule – of a tacitly held desire to respond to those foundationalist impulses. In Derrida’s (1978) terms, the desire for grounded theory represents “a desire for centre in the constitution of structure” (p. 280). But “desire” here is probably too weak a word; this search for some predictive, explanatory theory is more like a need – one rooted somewhere deep in the recesses of the structuralist history of the social

sciences – for the security of an epistemic homeland. Perhaps more accurately, then, it is a need to manufacture the machinery for finding that homeland, that “centre.” It is a need for commensuration rather than conversation, for measurement instead of memory, or as Dallmayr (1992: 20) puts it, for “structure” in preference to “event.”

The question that won’t go away in all of this is why grounded theorists (and some other qualitative inquirers) want to call their mental constructions “theory.” For an answer to this question one has to return to a discussion of the persistent primacy of theory in the knowledge stakes.

Hammersley (1992) offers some useful insights on this persistence of theory. Drawing out the essentials of qualitative method, he notes that this method is about *discovering* the social world and producing, supposedly, *theoretical* descriptions. He notes that this concept of theoretical description is problematic in this context. This is because ethnography places emphasis on description, and descriptions can’t be theories. Descriptions can’t be theories, according to Hammersley, because, “Descriptions are about particulars ... whereas theories are about universals” (pp. 12-13). He proceeds to unpack some of the features of theoretical activity emerging from ethnographic enterprise but is frank in his failure to resolve the dilemma, concluding that “the goals of ethnographic analysis need rethinking” (p. 28). Further on, Hammersley admits that the “quasi-law-like character of social science theory” (p. 42) creates problems about which he sees no obvious resolution in the context of the constructionist tenets of ethnographic inquiry (see also Hammersley, 1985).

It is indeed a problem, as we noted above (*Why is grounded theory “theory?”*), and it is faced peculiarly starkly by grounded theory, whose protagonists are less self-conscious than Hammersley in their allegiance to the methods which in the past have conferred such distinction on positivistic inquiry and of which theory is a central element.

For Hammersley, the problem requires that the goals of ethnographic analysis are rethought in respect of theory. For us, though, the problem is different. It is not with qualitative inquiry’s goals and tenets – there’s nothing at all wrong with these – but with the search for a particular sort of theory: theory as the end-point of serious inquiry (see Thomas, 2002). The problems stem, in other words, from the notion of theory (in grounded theory or wherever) and what we mean by and expect from theory.

## Ground

### *Whence “emergence?” Commitment or distance?*

... one must remember that because **emergence** is the foundation of our approach to theory building, a researcher cannot enter an investigation with a list of preconceived concepts, a guiding theoretical framework, or a well thought out design. Concepts and design must be allowed to emerge from the data. (Strauss and Corbin, 1998, p. 34, original emphasis)

How are grounded theorists to quarantine themselves, as social selves, from the data they are analyzing and re-analyzing to enable “theory” to emerge? And how can they transcend this and move outside it to stand on neutral “ground?” When Strauss and Corbin (1998, p. 99) say “We know that we never can be completely free of our biases [so we must] ... acknowledge that these influence our thinking and then look for ways in which to break through or move beyond them,” how is this distance, this “beyond,” this “ground,” to be achieved? And why should one want to move to some uninflected beyond?

How is an interpreter, an ethnographer or grounded theorist to emerge with anything which is not merely reportage if that theorist is not using his or her own person to emerge with the “theory?” The question is not really “How is distance to be found?” but rather “What use is distance even if it can be found?” The credibility of much that is at the root of qualitative inquiry is at stake here: the issues concern the legitimacy of the immediate, and the immediately made social construction. Everything in such inquiry balances on the meanings that we as people read into the social encounters we make in life.

To think about these meanings and their relevance for the “moving beyond” claims of grounded theory, a reverse Schank and Abelson test is helpful. The Schank and Abelson test is one used by the cognoscenti of artificial intelligence to determine whether a machine is responding intelligently. A complex sentence such as “A woman went into a restaurant and ordered a hamburger; when the hamburger came she was very pleased with it; and as she left the restaurant she gave the waiter a large tip before paying her bill” would baffle a computer which was then posed with the question “Did the woman eat a hamburger?”<sup>6</sup> That is to say, it would baffle the computer unless the machine were to be fed a ludicrously long set of instructions incorporating vast amounts of knowledge – or unless, and this is the important bit, the computer had some means of hermeneutically “bracketing” (as Hirsch, 1976, p. 5, puts it) the information so that it was meaningful. It would baffle the computer unless the

computer was, in other words, to all intents and purposes human – in the sense that it contained all the sense apparatuses, accumulated knowledge and modes of response that make humans human.

Now consider the test in reverse. Consider human grounded theorists, rather than a computer, being asked to take the hamburger question as a Schank and Abelson test. Could they fail it? Or to put the case more starkly, consider the grounded theorist presented with the sentence (perhaps from a transcript): “The child approached the teacher with her book; the teacher shouted ‘No!’ and pushed the child outside the door.” Could the grounded theorist, presented with the question “Did the teacher act reasonably?” fail to answer the question? Not even with a superhuman feat of energy could grounded theorists detach themselves from their backgrounds, from their own sets of hermeneutic brackets, from all the knowledge, biases and prejudices they have about human behaviour, to deny interpretation and fail to answer the question in one way or another.

It should not, of course, be necessary to make this point, since one would assume that it is the position of the qualitative researcher: a starting point of such researchers is surely that meaning is constructed by the interpreter. The interrelationship between interpreter and interpretation is indissoluble; there is no ground, no hidden truth residing somewhere in the data ready to inscribe itself, just as there is no Lockean *tabula rasa* in the researcher waiting to be engraved. But – and it’s a major “but” – this is not the starting point of Glaser and Strauss and their successors. For while they say that one should not start a piece of research with preconceived theories, they proceed to say that “... it is presumptuous to assume that one begins to know the relevant categories and hypotheses until the ‘first days in the field,’ at least, are over” (p. 34). Subsequently, Glaser (in his differences with Strauss) described the naïve emptiness with which a researcher should enter the research scene as “... abstract wonderment” (1992, p. 22).

Yes, “abstract wonderment”. If the assumption of foreknowledge is taken to be presumptuous, it is nothing to the presumptuousness of assuming the empty, directionless, uninflected mind of “abstract wonderment.” For the latter – a mind which will fail any Schank and Abelson test – is a contradiction in terms. Nor is it as presumptuous as: “... we believe that grounded theory will be more successful than theories logically deduced from *a priori* assumptions” (Glaser and Strauss, 1975, p. 6). The problem is that *a priori* assumptions are uneliminable, and this fact – far from being a source of anguish – is what the qualitative researcher should expect: *a priori* assumptions are what make study a) worthwhile, and b) possible.

Those disgraced “theories logically deduced from *a priori* assumptions” are no more or less sinister than the already existing hermeneutic brackets in the researcher’s head – the stuff of which interpretation is made. These also comprise “*a priori* assumptions” albeit that someone hasn’t taken the trouble to write them down in order to verify them. For why is the researcher there at all? There must be some assumption that the chosen topic is a worthy field for study. As Gadamer (1975) puts it: “... the meaning exists at the beginning of any ... research as well as at the end: as the choice of the theme to be investigated, the awakening of the desire to investigate, as the gaining of the new problematic” (p. 251). There can be little doubt that some process of verification – albeit implicit rather than explicit – is going on. This is well illustrated in Alvesson and Sköldbberg’s critical discussion of the nature of grounded theory. These authors use an extract from one of the most widely-used manuals on grounded theory (that is, Strauss 1987), in which Strauss offers a paragraph of fieldnotes from observations of nurses working in a cardiac recovery unit, and also gives a detailed introduction to the coding process. As Alvesson and Sköldbberg show, the example raises fundamental questions about the *implicit* meaning-making in which the researcher is engaged. For example, a focus on the nurse borrows from one idea in thinking about the division of labour (the degree of autonomy the nurse has in relation to specific tasks) yet completely ignores the gender dimensions of the situation. Furthermore, the things the researcher chooses to code produce an account that is highly technicized and is stripped of emotions. Alvesson and Sköldbberg note the effect of this, which is that “we have on the one hand a researcher and a nurse who are conscious but *lack* emotions, and on the other an unconscious patient who *possesses* strong emotions” (Alvesson and Sköldbberg, 2000, p. 26, original emphasis). These authors go so far as to suggest that a kind of “repression” or denial is at work here. Whether or not one agrees, grounded theory procedures begin to look like the neutralization of the uncomfortable in the way they can reduce the rich complexity of a situation to relatively safe external causal relationships.

### *Grounded theory and gift-wrapped meaning*

There is a central problem in the search for grounded theory. It is that there is no untethered spirit existing in the minds of researchers which will enable them neutrally and inertly to lay some cognitive framework over the data they collect to allow them to draw “theory” dispassionately from this data, this ground. These researchers are human beings who walk, talk to friends, tend their gardens, watch TV, read books, go to lectures. They have histories of friendships, relationships, of household life of one kind or another. They understand guile, happiness, sadness, envy, deceit, irony. Their heads are full of notions – notions about equality, justice, freedom, education, the future, hope, fraternity, charity, feeding the cat and parking the car. These are precisely the things that comprise and give structure to their mental lives. They are what make the drawing of themes from the data possible. They are not things which can be put to one side temporarily for the purpose of discovering grounded

theory. As Fish (1994) puts it, it is simply “zany” to assume that you can “in some way step back from, rise above, get to the side of your beliefs and convictions” (p. 295) in such a way that you will be able to survey the data unencumbered by the grip of those beliefs and that mental structure. What narcotic will enable the relaxation that will permit this? More importantly, what will be left when the narcotic has worked? For these things are the fabric of mental life and to imagine a mode of being devoid of them is to imagine nothingness.

To be fair to grounded theorists, they are not unique in their imaginings of epiphany coming *ex nihilo*. Such imaginings lie also at the root of the seemingly irrepressible search for ever-better method in qualitative research. It is a desire to have it both ways, to take on board the fortuitousness and uncertainty of a Gadamerian hermeneutics while cleaving to the attractive idea that there is some determinate and explicable social universe – some ground – waiting to be scrolled out.

The collateral assumption to scrolling out, of course, is that the scrolls, once scrolled out, can be deciphered. The hieroglyphics written on them can be transcribed and painstakingly translated using the tools of grounded theory. Beneath the code is a rich meaning ready for another to understand. The concealed assumptions lurking here in all of grounded theory’s processes and machinery are about language acting as a barrier to understanding. It is almost as though language is seen as a kind of inert conduit for some real, underlying meaning: the conduit has to be split open for meaning to escape. Indeed, the splitting open is made explicit in the word “fractured,” which is used by grounded theorists – for example by Strauss (1987), who says that “coding ... fractures the data, thus freeing the researcher from description and forcing interpretation to higher levels of abstraction” (p. 55). Researchers, with the tools of comparative analysis furnished by grounded theory, will break the cipher – they will be freed from description and mere narrative to discover the theory. But grounded theorists and other qualitative researchers are surely first and foremost human listeners – listeners who interpret more or less well on the basis of their experience of being human – on the quality of their shared knowledge, shared history and shared understandings. Listeners should surely be, in Wittgenstein’s words, “haunted by explanations” (Kenny, 1994, p. 221) if they have any sensibility about the frailty of knowledge which an understanding of qualitative inquiry brings them.

None of this is to say that the understanding of others is, as Berlin (1996) puts it, a “special act of magical divination not describable in the language of ordinary experience” (p. 24). Rather, it is to recognize that our everyday understanding is “a kind of *automatic integration of a very large number of data too fugitive and various* ... Our language is not meant to catch them” (*ibid.*, our emphasis). As anyone who has worked with a transcription of someone else’s interview tape would confirm, capturing others’ meaning

will depend on all kinds of unarticulated and half-articulated signs – the gaze that is a second too long, the gesture, the tone, the intonation, the word used in a special way, the language games. If these “fugitive data” are indeed processed without conscious deliberation, as cognitive science confirms is probable (see, for example, Schooler and Melcher, 1995; Claxton, 1997), how likely are they to be accessible via the incomparably clumsier textual weapons of grounded theory? Indeed, in the very notion of “thick description” and its distinction from “thin description” – a distinction borrowed from Gilbert Ryle by Geertz (1975) – lies the recognition that such weapons will not be successful in divining meaning. As Geertz goes on to point out, in interpreting meanings one cannot be a “cipher clerk” (1975, p. 9).

Even if one could, in other words, “fracture” the data, clean it up, map it to its barest neural components, one would be no closer to a definitive, transcendent understanding. Even if, as T.S. Eliot (1969) put it in *The Love Song of J. Alfred Prufrock* “... a magic lantern threw the nerves in patterns on a screen,” we should still be able to demur from some imposed interpretation. There is, in other words, no “ground” when interpretation is being spoken of. Fish (1989) notes that “candidates for the status or position of ‘ground’ have included God, the material or ‘brute act’ world, rationality in general and logic in particular, a neutral-observation language, the set of eternal values, and the free and independent self.” (p. 343). The problem with grounded theory is, to paraphrase Fish, that it is implicated in everything it claims to transcend.

## Discovery

### *Discovery – or invention?*

One ought to consider the significance of the word *discovery* in the methods of grounded theory. Discovery is at the heart of these methods, and there has been no dilution in this large claim since the early years of grounded theory. The choice of the word “discovery” says much about the epistemic project the authors imagine themselves to be leading, and they lead this project with no trace of false modesty. Indeed, Strauss and Corbin began a recent book (1998) with a long quotation from Galileo, which they suffixed with the words “... we, like Galileo, believe that we have an effective method of discovery” (p. 1). One needs to look in more detail at this claim to discovery, particularly when the claim is made solid, rather than metaphorical, with this remarkable self-comparison.

Discovery is a process of uncovering, revealing, disclosing that which is there. The assumption in the use of “discovery” is therefore that meaning is laid open for all to see following the application of some method of finding. Discovery is in no way synonymous with “tentatively suggesting.” The idea that a “theory” can be “discovered” therefore puts that theory a long way away from interpretation. In the opposition of the interpretative to the normative and the illuminative to the definitive, grounded



theory – proudly boasting a pedigree from qualitative inquiry’s stable – surely aspires to interpretation and illumination. The mere use of “discovery,” however, divulges expectations closer to the normative and the definitive – to a correspondence view of knowledge.

The fact that the word “discovery” is used in preference to “invention” (or “construction,” or even “generation”) is revealing. “Discovery” involves the disclosure of well hidden but already-existing phenomena; it concerns things which exist, albeit that they’re hard to find. By contrast, *invention* refers to things that exist only in an infinite universe of possibly existing things. Thus, the moons of Jupiter, the molecular structure of DNA and the tomb of Tutankhamen are discovered. On the other hand, the spinning-jenny, diesel engines, iambic pentameters and the telephone are invented.

When the word “discovery” is used, the presumption is therefore of the revelation of a solid, disclosable thing – an entity transcending interpretation. The quite explicit assumption of grounded theorists is that the paraphernalia and machinery of inquiry can reveal this entity with “analytic tools” (Strauss and Corbin, 1998, p. 87). Invention, by contrast, is a creative process in which one of myriad possible constructions is made out of the stuff – concrete or mental – available.

Let us put “invention” in the place of “discovery” in *The Discovery of Grounded Theory* so that it becomes *The Invention of Grounded Theory*. The reason it doesn’t sound quite so impressive is because “invention” (more consistently with the tenets of qualitative inquiry) implies one unique construction among a plethora of possible constructions. Inventions are mutable, not static. In fact, inventors are sure that their inventions will change and improve: Alexander Graham Bell, one imagines, did not assume that his telephone would be the last word in electronic communication. He did not claim to have *discovered* the telephone. Diffidence must surely be the *sine qua non* of the inventor. “Discovery,” on the other hand, implies little diffidence in one’s ontological assumptions.

Discovery implies a clean lineage from thing to thought and an uncomplicated correspondence between the two. The thought is merely a doppelganger for the thing. But in fact this correspondence can never really exist when it comes to the things of the mind. The historian Simon Schama (1996) makes this point in discussing the discovery of “wild” America: “The presumption was that the wilderness was out there, somewhere, in the western heart of America, awaiting discovery, and that it would be the antidote for the poisons of industrial society” (p. 7). But, of course, that wilderness was a fabrication, a dream spun of wishes; it was an invention dressed up as a discovery, or as Schama puts it “... the healing wilderness was as much the product of culture’s craving and culture’s framing as any

other imagined garden” (ibid.). The point is that notions, dreams, even grounded theories, are products of cultured minds. Unlike the moons of Jupiter, they are not discovered.

Researchers can, then, be justifiably inductive and analytical about what is assumed unproblematically to exist, what is already “there,” in the way that they cannot be analytical about that which is only possibly there and that which changes its shape depending on the knowledge, disposition and dreams of the listener or viewer. All the technical procedure and analytic tools of grounded theory therefore admirably fit the bill for “discovery” – but discovery is far from what happens in the process of divining meaning, and this surely ought to be a starting point for the qualitative researcher.

The choice of the word “discovery,” then, reveals much about the way that the proponents of grounded theory think about knowledge. But even more is revealed by the way that they knot together “discovery” with the idea of “ground.” Together, “discovery” and “ground” reveal the notion of capturing a truth somewhere out there, which is of course to say, nowhere near the seeker.

### **Developments in grounded theory: what do they offer?**

Some three decades have passed since the inception of grounded theory and while many continue to promote the methods in forms similar to the original (e.g. Strauss and Corbin, 1997, 1998), others, as we noted earlier, have offered significant developments. In these developments, though, as we shall try to indicate, there are persisting issues to be resolved. How do the tools of grounded theory, even in these new forms, make what occurs superior to everyday interpretation? How might the looser and more tentative forms of interpretation that go with “common interpretive acts” be inhibited or demoted if the methodological proxies of grounded theory persist? What does the continued use of grounded theory say about the legitimacy of qualitative inquiry? And what does a continuing insistence on the procedural signposts of grounded theory encourage people to ignore?

Let us look at a recent use of grounded theory and speculate on how it might have differed from another analysis of the same subject not benefiting from grounded theory. Clarke, an ex-student of Strauss (Clarke, 1998), undertakes an excellent analysis of the American reproductive sciences which, she reports, emerges out of grounded theory analysis. She notes, in a general description of grounded theory, that “the data are coded, and codes are densified and ultimately integrated into an analysis” (p. 278). However, her very brief description of the method occurs at the end of an appendix in a book of over 400 pages. In fact, although a generous attribution to grounded theory is made, it is impossible to see how the detailed analysis – depending on interviews, documentary data and “‘insider’ histories—accounts of events and discoveries, biographies and autobiographies, status reports and so on written by reproductive researchers” (p. 277) in any way develops out of the specifics of grounded theory. One

has to speculate (speculate, since no evidence of the use of codes or densification is offered) how the inductive process has differed from, for example, that which occurred in other first-rate analyses which have not employed grounded theory, such as the equally excellent *Labeling the Mentally Retarded*, written by Jane Mercer (1973) which likewise used unstructured interviews, community survey and official records to draw conclusions about the social nature of learning disability.

Indeed, as Becker (1996) points out, Mercer's classic about labelling occurred in the context of other iconic texts which also used a range of methods and analytical strategies. Becker gives as examples *Black Metropolis* (Drake and Cayton, 1945) and *Boys in White* (Becker et al, 1961) which drew upon and used data of broad provenance – interview, statistical, observational. These are studies where people have, as Geertz (1995, p. 20) puts it, worked "... *ad hoc* and *ad interim*."<sup>7</sup>

If one is freed from methodological constraint one is in turn freed to depend more on one's own experience, more on Dilthey's *geistige Welt* – on all the things of the mind in the world. Those things are available to us because we are all members of a species that commonly inhabits and shares the same universe and the experiences it offers, sharing understandings and sharing meaning-making. Their availability comes neither by dint of grounded theory nor any other prescription of method. In fact, looking back on iconic studies and how grounded theory *followed* (not preceded) them, the point is reinforced that much of the original impetus for the formulaic guidance of grounded theory came from the desire to gain the respect and trust of the "scientific" social scientists in the 1960s – to be able to talk about procedures, reliability, validity and so on. As Robrecht (1995) puts it of *Boys in White* (for which Strauss, incidentally, was one of the research team), "The lack of explicit methodological procedures served as a focal point for criticism from the general scientific community ... The challenge to those interested in qualitative methods, then, was to provide a more rigorous explanation of these methods" (p. 170). As Sanders (1995, p. 92) put it, the aim seems often to continue to counter positivist critique "through more-rigorous-than-thou instructions about how information should be acquired and pressed into an analytic mold." But there should surely be no need any longer to point to proxy indicators of a research program's validity. To want to do this, to want to summon up methods and instruments as if these were the insignia of some higher authority which offered an imprimatur of legitimacy, by implication relegates the more direct understandings and interpretations of qualitative researchers. What is the message being conveyed to the dissertation committees, granting agencies and tenure review boards if the shibboleths of positivism and functionalism are so doggedly grasped?

Steps in more helpful directions have been made in the thirty years since *The Discovery* was published. Lincoln and Guba (1985, p. 339), for example, draw the constant comparative method from grounded

theory with enthusiastic recommendations as to its value as a means for processing data, but with equally clear warnings as to the hollowness of the putative ends, namely prediction and explanation.

As we see it, the mere existence of the term “grounded theory,” with its key assumptions of “ground,” “theory” and “discovery” lures any newer forms of qualitative inquiry which cleave to its assumptions back into the same problematic territory. Earlier in this article, we briefly described Charmaz’s *constructivist* grounded theory. But what might be gained by seeking a revised grounded theory of this kind? Charmaz says, “In short, constructing constructivism means seeking meanings” (2000, p. 525) but it is unclear – in distinguishing this from other forms of seeking meaning – why this has to be called “grounded theory.” She proceeds: “My version of grounded theory fosters the researcher’s viewing the data afresh, again and again” (p. 526). Again, one needs to ask how this differs from the induction or abduction of our everyday sense, how it differs from using the tacit and spoken tools of normal sense-making, or of what Schatzman (1991, p. 304) calls “common interpretive acts” – of review, rehearsal, of talking about it with friends, of employing practical syllogism, recognition, evaluation, coming to a conclusion. What Charmaz is doing in her constructivist grounded theory sounds very much like the process of hard work and inspiration that combines to enable interpretation and new insights in any field, in the natural or social sciences, in the arts or humanities. It sounds like the need for, as Pólya (1990) suggests, working with the data and *thinking* “until you get a bright idea” (p. 172). One wonders what the grounded theoretical ingredients in Charmaz’s new form of grounded theory contribute in addition to this. Moreover, one wonders whether they may in fact *inhibit* rather than *enable* “common interpretive acts” as Schatzman (1991) indicates was indeed the case with many of his students who notionally employed grounded theory.

And what right does anyone have to proffer a better, a superior rendition based on the benefits evidently to be gained by some proxy for direct experience and interpretation? Perhaps because of her allegiance to the tenets of grounded theory, Charmaz seems to want in her most recent version to continue to interpose an additional element in the process of understanding. She does this in rendering through writing. The constructivist grounded theorist “... tries to balance theoretical interpretation with an evocative aesthetic” (p. 526). She says: “I attempt to evoke the experiential feeling through how I render it in writing.” Although this elevation of the process of writing following the use of grounded theory procedures takes research and the reporting of research to a place many miles from the origins of grounded theory, it does nothing to challenge the notion of some expository formula.

The simple understanding of being again moves aside for a proxy – admittedly a self-consciously mutable and personal one – but this proxy is made to stand superior to our other forms of

understanding. It is the imposition of an *enhanced* understanding that is troubling, and it occurs not just in grounded theory, not just in ethnography, but throughout the social sciences so willingly drawn upon by educators. Some time ago, Vernon (1964) made the same point about psychologists' methods and suggested that "the unsophisticated methods that we ordinarily use in understanding people in daily life" (p. vii) are likely to produce knowledge every bit as accurate and reliable as that coming from psychological tools, however sophisticated.

Clearly one needs to write to report one's findings, but the setting on a pedestal of the artefacts of the written form as though they provide aids to understanding and interpretation seems to want to take the process one stage too far – to offer an interpretation of interpretation. (Indeed, in emphasizing the processes involved in writing is the potential to magnify differences between observer and observed – a theme of much discourse on writing from Rousseau to Derrida, and well discussed by Norris, 1987, p. 97.) What Charmaz seems to be arguing for is a rediscovery of the symbolic interactionism that grounded theory sought to transcend. In so doing, she endorses a wholly appropriate and interpretivist form of constructivism. What is puzzling is her apparent need to tether this to grounded theory as a methodological label. While Charmaz's form of constructivist grounded theory moves grounded theory far from its inception point in the ideas of Glaser and Strauss, it still – in wanting to call itself grounded theory – adheres to grounded theory's core principles. That is to say it adheres to the notion of "ground" (the idea there is something beyond and underpinning) and the notion of "theory," that one can perform some supervening process which will interpret interpretation.

### **Concluding comment – grounded theory misses the best**

In formulating grounded theory procedures, Glaser and Strauss performed an invaluable task in bringing to general notice the powerful deliberations of the Chicago School: of argument about the validity of ethnographic, phenomenological, interpretative, hermeneutic – in other words, qualitative – inquiry. More than this, to those who called "Where is your epistemic collateral? Where is your theory?" they provided difficult-to-refute answers. They made a major contribution to making qualitative inquiry legitimate.

But all of that debate and proselytization was a product of its time. Our argument is that the success of grounded theory – its success in helping to validate qualitative inquiry – depended on something of a sleight of hand in reasoning about inquiry. Hindsight enables one to see the debate of the time in context, and to move on. The argument for qualitative research now stands in its own right. Continued allegiance to grounded theory procedures – or, strangely, loyalty simply to the term "grounded theory," unstitched from its procedures or putative ends – stunts and distorts the growth of qualitative inquiry.

There is no need continually to reinvent grounded theory. Our argument throughout this article has been that the starting points of qualitative inquiry are contradicted – and even undermined – by the aims, claims and methods of grounded theorists. But the most important practical issue, particularly for educators and educational researchers, is neither whether the algorithms of grounded theory are justified, nor whether they enable the induction, explanation and prediction that Glaser and Strauss (and others more recently) have claimed for them. The most important issue is rather about what is missed or dismissed if one continues to use grounded theory, and continues thereby to promote the epistemological and theoretical precepts embodied in its name.

To use grounded theory involves a rejection of simple understanding. It entails an explicit denial of what we know and our ways, as practitioners (and as human beings), of making sense. For grounded theory elevates a certain kind of thinking while it demotes and eschews other kinds of thinking and understanding. In its hankering after order – with its fracturing, its axial coding, its categories and sub-categories – it seeks to impose a certain kind of patterning, shape, and even rationality. Via such procedures it thereby relegates the original voice – the narrative – of both the respondent and the discussant in the research exercise. By the superimposition of method, and the ultimate production, supposedly, of theory, it implies a dismissal of the direct validity and import of people's accounts, such as those children who fail to adapt to the school system or fail to learn in that system. Earlier in this article we gave reasons for qualitative inquiry in the tenets of hermeneutics, but it should be remembered that another reason for favouring qualitative method lies in the foregrounding of different voices and the displacement of what Lyotard called “privileged speech communities” (see Haber, 1994, for a readable account of Lyotard's views). One should hear the narrative of those who have been taken to be, according to Foucault (1980), “naive ... low-ranking” (p. 82). But the implication accompanying grounded theorizing is that the knowledge of these others has in some way to be sanitized for it to become transparent to rational people. The narrative has to be broken, “fractured,” for sense to prevail.

Not only this, but grounded theory with its procedural machinery, also relegates the clear accounts of researchers themselves: students, teachers or other professionals. Although there is a new kind of constructivist grounded theory it is unclear what this can add to such accounts; indeed, it may even subtract from them, as users focus on method rather than voice. Examples of such clear narrative can be found in James Patrick's (1973) classic *A Glasgow Gang Observed*, or in the “fictional” narrative of classroom life given by Sconiers and Rosiek (2000). Here, narrative is told simply and clearly with no

pretence that by some methodological alchemy it will be transformed to something more secure in its epistemic status.

As Becker (1996, p. 70) asserts, there are no recipes for ways of doing social research; rather, one has to have “imagination and ... smell a good problem and find a good way to study it.” This sounds rather like Feyerabend’s “*anarchism*, while perhaps not the most attractive *political* philosophy, is certainly excellent medicine for *epistemology*” (1993, p. 9, original emphasis).

In none of the looser approaches just noted will be found the sort of elevation of a methodological prescription that one finds in grounded theory. More typical is an insistence that knowledge is everywhere, that all such knowledge is valid and that we should feel unconstrained in its collection, use and analysis. A preoccupation with method (and not just in grounded theory) makes for mirages of some kind of reliable knowing, and this in the end makes us almost more concerned with the method than the message. As Andreski (1972) suggests, “The overemphasis on methodology and techniques, as well as adulation of formulae and scientific-sounding terms, exemplify the common tendency ... to displace value from the end to the means” (pp. 108-9).

New researchers in education are likely to find the procedural rules of grounded theory attractive: the approach has an apparent resonance with particular folk models of scientific endeavour, and appears to proffer a map and a compass to navigate the open terrain of qualitative inquiry. Everyone, after all, likes to know the “right” way and the offer of such aids will surely be appealing. But if researchers do pick up and run with grounded theory, they risk losing the best of qualitative inquiry.

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## Notes

<sup>1</sup> As Charmaz has put it, grounded theory combined Glaser's "positivistic methodological training in quantitative research from Columbia University" with Strauss's Chicago school "pragmatist philosophical study of process, action and meaning" (Charmaz, 2000, p. 512).

<sup>2</sup> For example, Babchuk (1997) identifies a plethora of grounded theory investigations in education. These range over a vista of topics from the study of costs and benefits associated with participation in adult basic education programs (Mezirow, Darkenwald, & Knox, 1975), to academic change (Conrad, 1978), to middle school students' perceptions of factors helping the learning of science (Spector & Gibson, 1991), to reference group socialization of secondary school teachers (Gehrke, 1981), to teachers' perspectives on effective school leadership (Blase, 1987). Many more are identified by Babchuk. Clearly, grounded theory is viewed as a most accessible and appropriate – perhaps *the* most accessible and appropriate – way of doing qualitative research in education.

<sup>3</sup> Protoscientific theory emerges out of "... private and social experiences [and] human beings construct a set of generalisations, held together by seemingly theoretical postulates (such as 'an intention' and 'a belief') and inferences ('practical syllogisms'), and ... this has a kind of protoscientific structure" (Russell, 1992, p. 486.). The need for the distinction between this and other kinds of knowledge goes back a long way: Plato distinguished between *eikos* – what is likely, probable, reasonable – *epagôgê* induction, and *episteme*, meaning knowledge.

<sup>4</sup> As Gadamer (1975) notes, in arguing for the legitimacy of experience, the dignity of science depends on the "fundamental repeatability" (p. 311) of experience. In answering Bacon's opposition to "empty dialectical casuistry" (p. 313), he suggests that in studies of what it is to be human – in the "human sciences" – "the only scientific thing is *to recognise what is*" (p. 466, original emphasis). No method can help us achieve this recognition.

<sup>5</sup> Glaser, in his dispute with Strauss, is able to state that grounded theorising needs a series of steps "... none of which can be skipped if the analyst wishes to generate a quality theory" (1978, p. 16).

<sup>6</sup> This is a paraphrase of the use of the Schank and Abelson test used by John Searle (1982).

<sup>7</sup> In more recent, but related, work – looking at the overrepresentation of students of certain ethnicities in special education programmes – it is difficult to see the benefits of the grounded theoretical analysis of Harry et al (2005) over the non-grounded analysis of, say, Tomlinson (1982) or Benjamin (2003).