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Book Review: Being Barney Glaser

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Grounded Theory: the philosophy, method, and work of Barney Glaser. Vivian B. Martin & Astrid Gynnild (Eds). 2011. Boca Raton. BrownWalker Press.

I was a little taken aback when Astrid Gynnild asked me to review this work for *Grounded Theory Review*. As I explained, I have been impressed by a lot of what Glaser and Strauss and Glaser writing alone have said to me about sociological research, I teach methodology at masters and doctoral levels and always recommend these works to my students, encouraging them, where appropriate, to adopt some of the more familiar strategies of this approach—let the data speak, theoretically sample, write memos, conceptualise, in particular. I will, however, not allow them to say that what they are doing is grounded theory and nor do I claim that that is what I do; I may have been impressed, even inspired by Glaser's work, but what I do is other than it. So I'm not sure that I am qualified to review the book. Nevertheless, I agreed, only to be disconcerted by the announcement of audience in the introduction by Gynnild & Martin:

> It is our hope that grounded theorists at all stages of competence will find something useful to incorporate into their grounded theory practice. Much is said here about the desire to get good information into the hands of minus mentors, but the book is also for the many skilled GT researchers around the globe who are searching for more insights, inspiration, and ideas to move on with their own GT projects (p. 11).

It would appear that I am not even included as a reader, yet I am now asked to address people who certainly are. Well, here goes.

Firstly, I think Gynnild and Martin have left me out in error. There is a great deal in this collection for the non-specialist in grounded theory starting with the discussion and illustration of mentoring in the introduction and in the first section of the book, "Teaching grounded theory," in particular. Indeed, the editors have deployed a grounded theory approach to the analysis of their own collection. In their introduction, Gynnild and Martin present the outcome of this analysis, revealing that the main concern in this book is "mentoring a method" — the title of the introduction—"through cultivating competence of grounded theory networks over extended periods of time" (p. 3). If a practice—any practice—is to have coherence in its practical application, then that coherence will, in part, at least, be characterised by what I (Dowling, 2009) call "low discursive saturation," whereby its principles are not available within language, cannot be validly codified in books, though we may try. As Guthrie and Lowe put it in their chapter giving advice to students and their supervisors:

> Have you ever read a book which aims to teach you how to ski, surf, ride a horse? None, no matter how well written, can mimic what it is really like to feel the full range of these real experiences as they are lived (p. 154).

Neither do you learn to ride a horse or do research—at least, not well—without a mentor. This is an important lesson for all educators and, in particular, for the supervisors of dissertations to take mentoring seriously, whatever their approach to research. Of course, mentoring is not the only responsibility that a supervisor has in respect of their students, particularly where the students are drawing productively on the supervisor's own work. Examiners will want to be assured that that work has credibility within the relevant field and will expect to see citations of published work. The supervisor, in other words, has a responsibility to their students to publish. So I do not go along with Guthrie and Lowe's contrasting of the bad supervisor as a PRAT—one who prioritises publishing, research,

administration and teaching in that order—with a good supervisor as TRAP. Each of these domains of activity is vital both for the supervisor's career and in their responsibility to their students; it should not be a matter of prioritising—amusing as these acronyms are—but of engaging in each activity with integrity.

The ultimate impossibility of acquiring classic grounded theory skills textually notwithstanding, the non-specialist can still learn a great deal about grounded theory and, in particular, about the distinctiveness of classic grounded theory from this collection. A recurring theme is the well-known interdiction on doing a literature review in advance of data collection and analysis. Now I had previously understood this to be based solely on the need to avoid pre-conceptualisation and forcing. Indeed, this is re-stated by Glaser himself in his chapter on formal grounded theory (although I take the point that it applies to substantive, but not to formal grounded theory). I have tended to stand against this argument, not least because we generally should approach the literature critically. I feel I now have now been presented—particularly clearly in the chapter by Simmons, in that by McCallin, Nathaniel and Andrews and in Gynnild's interview with Glaser—with what seems to me to be a far more persuasive case. This concerns the fundamental intention of classic grounded theory to access that which is considered to be most important by the participants in the setting of the research and, through analysis, their latent patterns. This being the case, an advance literature review is not advisable because one cannot know in advance what literature will be relevant. I still disagree, because I approach research as more of a transaction between researcher—as a student of research literature—and researched and do not see a preliminary literature review as a contract; it has to be redone anyway at the completion of analysis. What is important is to allow the data to speak and not to presume that it will speak in the language of the literature.

Judith A. Holton, in her chapter, reports an Alvin Gouldner anecdote, recounted by Glaser, about a student, interested in risk-taking behaviour among steeplejacks. The student had been frustrated by his difficulty in getting the steeplejacks to talk about risk. At one point, the student saw them drawing straws for the allocation not of potential risk, but of vantage points for window peeping. The study, originally on risk-taking, subsequently became a study in strategic positioning. Not, for me, an argument for avoiding the literature, but for keeping an open mind. Nevertheless, where the transactional aspect of research is being minimised, I will (almost) concede the point about preliminary literature reviews. I would not, though, go quite as far as Guthrie and Lowe in suggesting that, if university protocols insist on a preliminary literature review then the student should produce "a logically plausible (but quite irrelevant)" (p. 61) review: if your university or supervisor does not understand or permit the approach to which you are committed, then find another university/supervisor.

The second section of the book consists of six chapters on "doing grounded theory." The first three of these appear to me to provide practical advice on specific data collection strategies and could all be used as stand alone pieces on these strategies; I will certainly recommend them to students on my masters programme. Helen Scott's chapter on "Conducting grounded theory interviews online" addresses an important development in data collection settings and discusses some of the issues in a grounded theory motivated way; how do we cope with lies, for example. Lisbeth Nilsson discusses the use of video recordings in an approach that generally advises against audio recording interviews. Again, this is a useful piece for any researcher intending to make use of video and again there is discussion of grounded theory methodology, in particular, the interdiction on recording. Nilsson reports that Glaser had advised her that the rule would not apply in her case— working with "people who have profound cognitive disabilities [that] means having to learn a whole new system of communication where meaning is primarily conveyed through behaviors not words" (p. 103)—and that video recording was appropriate. Now, again, I take a different view from Glaser on audio recording interviews. For me, using the recorder enhances my ability to focus close attention on what the interviewee is saying and field notes can be used for preliminary analysis, which can begin with the beginning of the

interview. I see transcription as part of the process of analysis itself, getting to know the data, as well as enabling a form of re-visiting that relies less on memory and its inevitably uncontrolled recontextualisations. I'm sure that those affiliating to the classic grounded theory method have heard these arguments before, but I do not seek to establish counter legislation, merely to mark my position. The chapter by Cheri Hernandez again takes on the recording issue in her discussion of the use of focus groups. Here, recording becomes necessary because, for example, this method is likely to involve a team of researchers rather than just one and because of the extended period of time involved in focus group discussions. Transcription of group discussion will, of course, take a great deal longer than is the case for one on one.

Michael K. Thomas introduces another issue that may seem controversial to some classic grounded theorists, the use of qualitative research software. In his chapter on formal grounded theory, Glaser asserts that this cannot be done on a computer, though I cannot recall mention of computers in relation to substantive grounded theory. In any event, the computer does not do the analysis for you, it simply enables storage, annotation, and retrieval in ways that should help the grounded theorist—whether doing substantive or formal grounded theory—to conceptualise more efficiently and, just possibly, more effectively. Again, Thomas provides some insightful discussion and helpful advice in a chapter that could, again, stand alone on the reading list of a methods programme.

Mark S. Rosenbaum—whose chapter is in the book's fourth section—also argues a case for the use of computers in grounded theory research, this time for the deployment of structural equation modelling in the verification of grounded theories. The chapter includes descriptions of various kinds of theory models that might arise out of a grounded theory study and that can be verified by theoretical triangulation using the Amos software that Rosenbaum introduces. The discussion is interesting, though presumably Glaser would argue that, if the grounded theory study has been done properly in the first place then it shouldn't need verifying.

The chapter by Hans Thulesius (back to the second section of the book) discusses his work in developing the seminal study, *Awareness of Dying*. There is not a great deal on grounded theory *per se* in this chapter, but the chapter is interesting in its own right. So too is the chapter by Massimiliano Tarozzi in which he discusses his translation of *The Discovery of Grounded Theory* into Italian. Tarozzi reveals some of the ways in which the process and problems of translation can enhance the understanding of a method. I found both of these chapters fascinating.

Three of the chapters in the fourth section—those by Glaser himself, Tom Andrews and Vivian B. Martin—concern the development of formal grounded theory. Andrews' and Martin's discussions of particular projects are helpful here as are the many illustrations in Glaser's chapter. Again, there are lessons or, at least, interesting points for debate here, not only for aspiring and actual grounded theorists, but for all researchers. Glaser, for example, warns against the tendency (most of us are guilty of it at some point) to "drift into logic-deductive speculation" that is "just 'super think' divorced from reality" (p. 274). He also notes that:

Rewriting substantive theory up a notch can sound like formal theory and gives formal theory implications but it is not FGT. At best it is a FGT waiting to happen by comparisons with new data and simply rides on the general implications of the core category. ...For example, a theory on becoming a nurse can be rewritten as ...a theory of becoming a professional by leaving out substantive words, or even becoming in general, an aspect of socialising. Or a theory of cautionary control among dentists can be rewritten, leaving out references to dentists as four general types of cautionary control. Or a theory of cultivating housewives for milk delivery accounts can be rewritten leaving out substantive reference to milkmen, as a theory of cultivating clients for profit or recreation. In short, by rewriting

leaving out the substantive attributions the researcher has raised the conceptual level of his [sic] work mechanically. He has not done the research to broaden the scope of his theory to the formal by conceptual comparative analysis of different substantive areas (pp. 274-5).

These cases are well made and, as I say, constitute important caveats for all researchers and not just grounded theorists. One additional point is worth mentioning. In this chapter Glaser points out that:

> People collect heaps of data thinking that is what research is, and then do not know what to do with it. They are often delighted that someone will or may do something with it (p. 263).

One needs, however, to take care here. Research Ethics Committees—at least in the UK— are tending to take a dim view of the use of data for purposes other than that for which it was collected unless the informed consent of those from whom the data was collected has been given for this additional use either originally or subsequently. The institutional scrutinising of research ethics in social and educational research has only really been an issue in the UK for about ten years, with research education rather lagging behind the bureaucracy. I hope that this will not unhelpfully reduce the availability of data for use such as the development of formal grounded theory.

Both grounded theorists and non-specialists will find the insights into Glaser's life, teaching, and early influences to be fascinating and helpful in appreciating what is specific about classic grounded theory; these insights are also, for me, an inspiration in dedication. There is something in pretty much all of the chapters as one might expect, I suppose, from a team who all studied under Glaser at one point or another in their careers. Key chapters in this respect for me, however, were those by Simmons, Charmaz—in her chapter presenting observations from students who studied with Glaser in the 1960s and 1970s— Holton—on early academic influences—the chapter, "Atmosphering for Conceptual Development," by Gynnild and especially, of course, Gynnild's interview with Glaser. Indeed, the interview seems to get to the heart of the matter. "Where did you get the inspiration from?"(p. 238), Gynnild asks, the reply: "Me. It's doing Barney" (ibid.), and later:

> You know what I'm thinking the core variable is? I know how wonderful it is to have one's self. I want to give people their sense of being themselves. (p. 251)

"Doing Barney," it seems, entails the application of the Golden Rule and hence the insistence on the crucial aspect that I mentioned above:

> ... classic grounded theorists begin with problems that are important to the people involved (McCallin, Nathaniel & Andrews, p. 78).

The rule penetrates Glaser's teaching, which enables Gynnild to reveal a resonance between it and Carl Rogers' person-centred theory that itself resonates with many of the anecdotes in the collection relating to learning grounded theory with Glaser and, indeed, with Anselm Strauss. The coherence that is suggested in Glaser's response to Gynnild's question is underscored by Evert Gummesson, who claims that, unlike many politicians, economists, lawyers, and physicians, Glaser "walks his talk:"

> Barney Glaser lives GT ... [He] became a world-renowned sociologist, but he used his scientific method to start a building company, a financial business, and a publishing house. And they are all successful. His windsurfing skills are the outcome of a GT study; his mini-GTs help him to quickly get to the point in all walks of life. Doing what he preaches, he personifies GT methodology (p. 230).

Indeed, Glaser seems to have adopted a similar approach in respect of determining and maintaining a healthy diet. This practical intention of classic grounded theory is nicely illustrated by Odis E. Simmons:

> ... in my study of the relationship between milkmen and their customers [..], I discovered the core category, "cultivating relationships." Prior to my sharing the concept with the milkmen from whom I had collected my data, they were unaware that they were cultivating relationships and that this was an essential part of their jobs. When I pointed it out, they immediately understood. And, with the informal theoretical foothold that I had provided them, they devised enhanced cultivating strategies and became even better at it. What had been latent easily became apparent and modifiable (p. 26).

Judith A. Holton describes Glaser's enthusiasm for the use-value of classic grounded theory:

> Glaser would continue to apply GT methodology to a wide range of studies including topics of everyday life interest such as contracting the building of a house and safe investing [..]. For Glaser, it was a natural evolution in applying and refining the methodology as practical sociology. In his writing and in seminars, Glaser continues to underscore the substantial power of GT and frequently advocates that this power deserves to be applied to those areas of life that matter most—relationships, parenting, careers, health and wellness, etc. (p. 216).

McCallin, Nathaniel and Andrews present this use-value rather more forcibly, classic grounded theory is, they argue:

> ..a unique theory-generating approach to understanding human experience. The moral imperative of research in the social sciences is to produce the best possible knowledge that can be used to positively affect those who require the services of a professional. So, there seems to be a valid moral justification for adherence to the tenets of classic grounded theory in disciplinary research. Furthermore, inadequate, skewed, misinformed, biased, or capriciously interpreted data and thoughtless, preconceived analysis of research data fails to attain the moral imperative central to disciplinary development. (pp. 78-9).

There is "a valid moral justification for adherence to the tenets of classic grounded theory in disciplinary research:" they seem, here, to be labelling not only the modification of classic grounded theory, but all other approaches to social research as immoral. This is a step too far for me and appears to be in some tension with Glaser's determination "to give people their sense of being themselves," "people" presumably including researchers. There are many legitimate justifications for social research, the production of directly useable results being just one. My own view of educational research, for example, is that it can provide bases for the interrogation of professional educational practice, but that it should not seek to direct that practice. It can generate new ways of looking at parts of the world that practitioners may recruit and recontextualise to enable them to develop their own practices. Glaser describes his own upbringing in a wealthy household with domestic servants. Many of the settings of social research include people who are rather less fortunate and a good deal of social research aims to reveal the systems and processes that constitute their oppression and this often requires going beyond the local areas of life of "relationships, parenting, careers, health and wellness, etc." I'm not saying that this cannot be done using classic grounded theory, simply that there are other legitimate approaches, including, perhaps, versions of modified grounded theory. McCallin, Nathaniel and Andrews are unduly simplifying the situation in a kind of purifying strategy: purifying is legitimate in terms of maintaining the specificity of classic grounded theory itself, but not as an attempt to pathologise alternatives.

Judith A. Holton's chapter considers the early influences on Glaser's academic thought and work, presenting a strong sense of Barney doing Barney in his engagement with some of the big names of American sociology, Paul F. Lazersfeld, Robert Merton and Hans Zetterberg amongst others. Holton quotes Glaser as recalling that "Throughout my whole training I resisted the efforts of both Lazersfeld and Merton to co-opt me to work for them…I had no time for them personally, just their ideas." (p. 209). These and other ideas from the field presumably contribute to what Glaser refers to as the theoretical sensitivity that enables him to recognise significant patterns in data. Theoretical sensitivity—if I've understood the concept appropriately—is going to be to a degree specific to the researcher and their intellectual history. I was a little bewildered, therefore, by the suggestion by Alvita K. Nathaniel that both Glaser and C.S. Peirce

> … believed that no matter where different investigations may begin, if they closely follow the method, their results will eventually converge toward the same result and that further study will tend to correct the results (p. 197).

If, indeed, theoretical sensitivity constitutes the individuality of the researcher in classic grounded theory (I'm sure someone will correct me if this is not the case), and if theoretical sensitivity develops with each project, then the different investigations may very well not "converge toward the same result." In his chapter on formal grounded theory, Glaser recalls, "the two dissertations I supervised on heart attacks lead two diametrically opposed core categories: cutting and super normalising" (p. 268), although he—the supervisor and so third researcher—articulates them, "heart attack victims ordered to cut back, if the attack is not severe, will super normalise to prove that they are still ok." (ibid.). It may be, then, that Nathaniel's attempt to "propose an extant, integrated, philosophical framework that fits the classic grounded theory method and undergirds its rigorous scientific processes" (p. 187) is somewhat forced, but why do it in the first place?

> Why is it important to identify the philosophical foundations of a research methodology? If carefully attended, the first principles, assumptions and beliefs of a given philosophy contribute the ontology and epistemology to a *methodology* and hold it together. This provides structure, logic and cohesion. Methodology carries through to the *method*, which includes practical steps of procedures such as data gathering coding, and analysis and also language, images, relationships, and meanings. Thus the philosophy's assumptions and beliefs imbue the day-to-day practical application of the method and its eventual product. This engenders research that is ethical, logical, truthful, and cohesive—earmarks of good scholarship. (p. 187)

It seems to me that this is less an argument than a sequence of assertions. It is reasonable to note that there are resonances between Peirce's pragmatism—the extant framework identified by Nathaniel—and the pragmatic claims of classic grounded theory, but I tend to empathise with Glaser himself when he asserts that "grounded theory is a-philosophical" (McCallin, Nathaniel & Andrews, p. 72) or as Simmons reports:

> Let the diehard constructivists and objectivists continue their rhetorical wrestle; for others, as Glaser tells his students, "just do the work!" (p. 27)

It seems to me to be entirely consistent with "doing Barney" to resist being subordinated to someone else's discourse, in this case, philosophy, elsewhere symbolic interactionism and so forth. I approach the situation in a different way, but arrive at the same conclusion. Philosophising generates metadiscourse in its relations with social research. This is often engaging, as it is here, but it is largely irrelevant to "doing the work"—and, apparently, to doing Barney—except insofar as it may contribute to, dare I say, theoretical sensitivity, in which case we need lots of different philosophies and not just one. One of the flaws in research methods teaching, at least in educational studies in the UK, is that programmes often begin with discussion of philosophy on the apparent grounds that one needs to sort

out one's epistemology and ontology before even beginning to think about "doing the work." I take a different view (see, for example, Dowling & Brown, 2010) and, so it would seem, does Barney Glaser. So I welcome the presence of philosophical discussion in this collection—especially that included in the chapter by Odis E. Simmons—but react against its attachment to the body of classic grounded theory as an unnecessary prosthesis.

A substantial amount—I hope not all—of this review has been, I suppose, descriptive rather than conceptual, but then I suppose that's the job of a reviewer in large part. I have a sense of the kind of distinction that Glaser is making between description and conceptualisation, but I don't like the use of these terms in this way: a description is always a conceptualisation of that which it describes, though often this may not have been taken very far. I refer to my own general approach as constructive description and, as I've mentioned above, I see my kind of analysis as a transaction between analyst and data. The analyst begins with a disposition or prejudice that has arisen from an engagement with largely conceptual literature to constitute an internal theoretical language. I conceive of analysis as the transaction between this internal language and the empirical, which may be data of any kind to generate an external language and an analysis in terms of this language. The external language accumulates to constitute the legacy of past analyses. My internal language encourages a sensitivity to action and on emergent alliances and oppositions. One of the components of my external language is a scheme that describes authority strategies, employing terms that are borrowed from Max Weber (though used in a way that is different from his). The scheme originated in the analysis of an institutional circular letter and proposes that the authority of an utterance or act may be attempted via a closing of the category of its author, or a closing of the category of the practice that contextualises the utterance or act, or a closing of both, or of neither. The result is the 2x2 schema in Figure 1.

Figure 1. Authority Strategies

Category of Author	Category of Practice	
	Open	Closed
Closed	*charismatic*	*traditional*
Open	*liberal*	*bureaucratic*

(Adapted from Dowling, 2009)

Re-visiting the collection that I have reviewed, all of these strategies can be found. Much of the book concerns the attempt to specify the classic grounded theory method, to mark out its distinctiveness from other approaches, to establish that whoever uses it should adopt its very specific procedures. These are bureaucratic strategies, specifying practice, but not the practitioner. At the same time, a number of chapters in the book make it quite clear that it is not possible to deploy the approach appropriately without an extended mentoring. This is a traditional authority strategy because it specifies both the practice—classic grounded theory—and the practitioner. Mentors must themselves have been mentored by a previously mentored grounded theorist and as the originator of the method was Barney Glaser. This establishes a necessary direct line of descent of all legitimate, classic grounded theorists from Glaser. This and the reported references back to him for the legitimation of particular decisions, not to mention the references to his quirky behaviour, in "atmosphering" and in waving at window cleaners during an interview, for example, constitute him as the charismatic author and ultimate arbiter of the method. Finally, the openness to the voices of others in being themselves, whether as learners of grounded theory or as participants in research settings, is a liberalising, a handing over of authority from the author of an act or utterance to its audience. I have no space to do this properly nor to defend the approach, though I will note that I justify my use of polarised categories and my rejection of the continuum by my contention that you cannot have a continuum unless you have a metric. Perhaps I can also note that, although I do look for patterns in data, I do not regard those

that I find as latent or as subjacent in respect to the practice or text. For me, all is surface and the patterns are the product of my deforming transaction with the data. A rather attractive illustration is offered by Jerome McGann's (2001) application of random Photoshop mutations to Rossetti's *The Blessed Damozel*, "revealing" (or producing?) hitherto unsuspected structure in the painting. McGann's own explanation of what he's achieving—his metadiscourse as opposed to mine—does constitute the structure as latent. Alternatively, then, (and also reported by McGann) there is Emily Dickinson's advice to try reading a poem backwards (I wonder what she meant exactly; perhaps it doesn't matter) if one is making little headway with a poem. In any event, this little ending is just a very quick holiday snap of Dowling doing Dowling. For the rest, in reading this book I was reminded, oddly, perhaps, of the film, *Being John Malkovich* (Spike Jonze, Dir.), with all of its puppetry imagery; it was fun, just for a little while, perhaps not doing, but being Barney Glaser.

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