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Pragmatic Reflections on a Conversation About Grounded Theory in Management and Organization Studies

Karen Locke[1]

Abstract

This commentary offers reflections on the conversation about grounded theory in management and organization studies. It highlights the institutional context in which we are having this conversation, noting its consequences for grounded theory practice. It also raises questions about the definition and boundaries of grounded theory, including the role of theory in the analytic process, and it argues for a pragmatic consideration of its research practices.

Keywords

grounded theory, qualitative research, qualitative research, philosophy of science

Those of us in the management and organization studies community who practice research in what I'll characterize as outside the hypothetico-deductive tradition (theory building) are, relative to that tradition, a minority and generally loosely coupled group. For that reason alone, I think it is useful for that community to talk about what we do in our research and how we do it. And the represented conversations in which Isabelle Walsh, Judith Holton, Lotte Bailyn, Walter Fernandez, Natalia Levina and Barney Glaser share their perspectives and research practice experiences take a step forward in doing that relative to grounded theory.

Taking About Grounded Theory in Management and Organization Studies Today: How Did We Get Here?

I'd like to frame my response to the perspectives they offer and the experiences they describe of engaging with grounded theory in organization studies by first drawing attention to the particular institutional context of this symposium to help readers, especially young scholars beginning their

[1]College of William and Mary, Mason School of Business Administration, Williamsburg, VA, USA

Corresponding Author:
Karen Locke, College of William and Mary, Mason School of Business Administration, Williamsburg, VA 23817, USA.
Email: karen.locke@mason.wm.edu

research careers, understand the broader situation in which they may be being introduced to grounded theory and in which this conversation, at this time, is taking place. C. Wright Mills's (1959) emphasis on the relationship between personal troubles and public problems connects individual biography with larger historical and social concerns and provides a heuristic to help us relate individuals' personal experiences of engaging grounded theory with organization studies' broader institutional context as it relates to grounded theory. Two features of the context are relevant: the development of grounded theory methods discourse, including the broad transdisciplinary diffusion of grounded theory research practice, and the increasing differentiation of management and organization studies as a profession.

As the symposium participants indicate, it has been close to 50 years since *grounded theory* became a term of art in methodological discourse, and in that time period, grounded theorizing has been central to the methodological practice and intellectual contributions of scholars across a broad range of disciplines. Beginning in sociology and the health sciences, the analytic practices associated with the approach have been elaborated, expanded, and differentiated by the *Discovery* authors themselves, the students they trained and mentored, their students, and as well, researchers who did not study with the originators or the students they taught. They have all developed their articulations and enactments of grounded theory practices through their own intellectual and research biographies shaped by new eras, new research questions, and new ideas on the ontological and epistemological foundations of inquiry. For example, one of the early adopters of the grounded theory approach to organization studies, Barry Turner, an industrial sociologist, offered his articulations of grounded theorizing clarified through his use of the approach in his studies of the organizational characteristics of small batch production facilities and of the preconditions for organizational disasters (e.g., Turner, 1981, 1983). And so the pattern has repeated itself across the disciplines in which the approach has been taken up. The corpus of methodological texts on grounded theory is therefore, overwhelming; an indicator—writing this commentary, I searched *grounded theory* under the "books" category at Amazon.com, and it generated 16,971 hits!

In approximately the same time period, management and organization studies has migrated to business schools where it has been increasingly bounded off from its reference disciplines, for example, psychology, sociology, anthropology, economics, and associated research methodologies (Adler, 2009; Hinings & Greenwood, 2002; Locke, 2011; Scott, 2004). The social disciplines themselves have increasingly differentiated, forming themselves into separate departments with little cross-disciplinary interaction. We are now more isolated from the disciplinary homes that inform our intellectual and research practices. Today, professional and especially, business schools have become the chief location for the training and development of management researchers. In these schools, the institutional resources available to help our novice researchers develop their research practice in non–hypothetico-deductive forms of research is unevenly distributed.

For many, the only exposure to grounded theory they receive is through a qualitative module in a survey course or as one of "five approaches" of qualitative inquiry (e.g., Creswell, 2012) in a single qualitative research course. These are a far cry from the long sequences of courses, for example, on "Discovering Social Reality," offered by the Social and Behavioral Science Department at the University of California San Francisco when Barney Glaser and Anselm Strauss were on faculty (Covan, 2007; Star 2007). In addition to not having deep access to courses, novice researchers in organization studies do not have access to a bench of faculty with whom they apprentice in the development of their methodological practice nor to a community of practitioners with whom they can share their ongoing research projects and engage with on their research understandings, challenges, and choices.

In this context, it is not surprising that young researchers should receive limited schooling in the approach and encounter arbitrary injunctions and proscriptions about grounded theorizing research practices, as for example, Isabelle Walsh indicates was part of her biography. Teaching

workshops, I have met PhD students whose faculty supervisors (who have not conducted any form of discovery or theory building research) make prescriptive statements about how their dissertation research should and shouldn't be structured if they are pursing grounded theorizing. Similarly, reviewing manuscripts for some of management and organization studies' flagship journals, I have seen similar arbitrary prescriptive statements (by both submitting authors and colleague reviewers), for example, that a study is grounded theory because it draws on interviews as a primary data source.

The symposium participants do the management and organization studies community a service first in reaching through boundaries to connect the Academy of Management to one of the *Discovery* authors, Barney Glaser, and second, in debunking unnecessary and arbitrary proscriptions such as, "you cannot use numerical data in a grounded theory study." All the panelists make this important point, and Bailyn and Levina's comments contextualized in particular research projects and the questions being pursued are helpful in bringing to life how numeric data might be profitably used. Bringing forward the possibility of drawing on numeric data and analyses is not only timely given that we now live in a "big data" world as Levina highlights, it is also timely in the face of broadening recognition of the value of "bottom up" data-driven approaches by researchers whose research biographies are consistent with hypothetico-deductivism (see Colquitt & Zapata-Phelan, 2007; Hambrick, 2007; Locke, 2007; Spector, Rogelberg, Ryan, Schmitt, & Zedeck, 2014). The recent establishment of a new mainstream management and organization studies publication, *Academy of Management Discoveries*, focused on "phenomenon-driven research" is further encouragement (Van de Ven, 2015, p. 1). The discussion is also important given an institutional context where there is relatively thin training in and understanding of the approach to raise young scholars' awareness that the representations of grounded theory featured in the methods sections of our field's journals should be read with a critical eye.

While I found the symposium members' comments around data forms important and useful, I have concerns in a number of areas that novice researchers should be alert to.

Grounded Theorizing Is Broader Than That Represented, and There Are Disagreements About Its Boundaries

While it is important given the size and range of grounded methods discourse to connect organizational researchers to foundational texts, the symposium represents grounded theory's intertextual field in overly narrow terms, constituting its methodological literature primarily though Glaser's corpus and individuals working in the information systems field. I understand that this represents the biographies of the symposium members and that space is always a constraint. At the same time, I believe it does aspiring organization studies scholars interested in investigating grounded theory a disservice by not pointing to the development and expansion of grounded theory practice as it has evolved and been variously elaborated over 50 years of research experience and intellectual engagement. Walsh notes that there are different streams of grounded theorizing but only in general terms. Notably missing are breadcrumbs which point to specific continuations and elaborations of the approach articulated by practicing scholars who studied with Glaser and Strauss at UCSF. An exception: In her brief historical overview of grounded theory, Judith Holton does point to Strauss and Corbin's (1990) articulation of grounded theory procedures, though suggesting that it belonged in the category of "qualitative" rather than "grounded theory" research. The history and evolution of the approach is, though, considerably richer, more complex, and as Holton's exclusion of Strauss and Corbin's work from the grounded theory category indicates, it is more contentious. The grounded theory section of my bookcase thus includes texts by first- and second-generation grounded theory methodologists. I learned from them all, as I also learned from Barry Turner's methodological statements.

Given the number of methodological statements, as a pragmatic starting point, readers interested in taking a first bite at a fuller picture of the grounded theory arena might consult *Developing Grounded Theory: The Second Generation* (Morse et al., 2009) compiled by six grounded theory practitioners and methodologists who apprenticed with Glaser and Strauss, went on to significant academic careers in sociology and health sciences, and embody the distinctions and tussles that have evolved in the domain. The text includes the ''voices'' of Phyllis Noerager Stern, Janice Morse, Barbara Bowker, and Juliet Corbin from the field of health sciences and Kathy Charmaz and Adele Clarke from sociology. They offer their perspectives on the history of grounded theory including the role they played in extending it. Able to compare and contrast perspectives within this second generation, novice researchers can then make better informed choices about how to navigate and locate themselves within the sizeable grounded theory domain.

There Is No World Divided Into Qualitative Research on One Side and Grounded Theory on the Other

The comments by Holton draw a strange map of the landscape of research that is not in the hypothetico-deductive tradition. Specifically, it appears to be separated into grounded theory and qualitative research, with the distinction being that whereas grounded theorizing admits all forms of data, the category qualitative research only admits nonnumeric data. This is not any research landscape I recognize. Indeed, this category-making activity regarding qualitative research is bewildering for at least three reasons. First, as has been emphasized for over 35 years, *qualitative* is an umbrella term that points to an enormously diverse field. In his 1979 introduction to *Administrative Science Quarterly*'s special issue on reclaiming qualitative research for organization studies, Van Maanen (1979) stated, ''The label qualitative methods has no precise meaning in any of the social sciences. It is at best an umbrella term covering an array of interpretive techniques'' (p. 520). And in the 35 years since that statement was made, the umbrella has only expanded. One point of evidence is the range of ''qualitative'' handbooks that have been published in the past decade; these include handbooks on action research, case-based methods, indigenous methods, critical management studies, discourse analysis, ethnography, feminist research, and narrative methods—and grounded theory (Locke, 2011). While we're stuck with the qualitative label because it is part of everyday methodological discourse, we should not pretend that it describes a homogenous model of research practice against which others can be compared. It doesn't.

Second, for almost 30 years, it has been repeatedly argued that the distinction between qualitative and quantitative data is problematic—a point that Lotte Bailyn underscores in her commentary. Taking type of data as distinguishing feature of qualitative work renders the latter unstable as a category (Agar, 1999; Allwood, 2012; Bryman, 1984, 1995, Hammersley, 1992, 1996). It doesn't hold up. Any category whether it is developed through open coding or through cluster analysis has both a qualitative and quantitative aspect—its ''what-ness'' has to be specified and its degree of ''much-ness'' taken into account (Allwood, 2012; Sandelowski, Voils, & Knafl, 2009). Thus, separation by numeric and nonnumeric data makes little sense; it neither holds up conceptually nor in practice. For example, Agar (1999) notes, ''I have never done an ethnography where I have not used numbers'' (p. 685). Third, when qualitative research points to inquiry constituted within a logic of discovery, wherein as researchers we work to develop theory we don't know in advance from data we will identify during the process (Agar, 1999, 2004; Locke, 2011), grounded theory falls under the same umbrella as many other approaches prosecuted by organization scholars who may structure their inquiry around case studies or ethnography or narrative approaches, and so on, though of course, there are differences at the level of research practices. Attempting to distinguish ''classic'' grounded theorizing by contrasting it with qualitative research doesn't illuminate either.

The Role of Theory Is More Complicated

A final issue I'd like to take up relates to the role of theory, especially as it relates to research practice. Specifically, I'd like to consider the strong proscriptive statements made by Holton and Glaser (respectively) that researchers should prosecute our research "free of preconceived ideas based on personal or professional research interests or theoretical frameworks drawn from extant theory" and researchers can "go into the field, not knowing anything and come up with theories that have many practical implications." Of course, the notion of letting new concepts emerge from data uncontaminated by extant theory has historically been definitive of grounded theorizing, yet given that the untenability of naïve empiricism and the theory-ladenness of observation has been well established (Bendassolli, 2013; Gherardi & Turner, 1987; Hanson, 1958; Kelle, 2005; Laudan, 1977), I was surprised to still encounter such strong proscriptive language against admitting existing theoretical elements into the analytic process. In light of the consensus that "one could not pick up rocks without some sort of theory to guide them" (Van Maanen, 2011, p. 222), I felt like I had stepped back in time.

More importantly, how are researchers learning their discipline and their craft to interpret such doctrinaire statements in the context of their unfolding research projects? With what consequences? Abjuring theory until the conceptual framework is developed can have several less than desirable ramifications. First, in their efforts to comply with such strong statements and conduct their analysis as "blank slates," novice researchers may adopt a completely open approach to their analysis, crediting every word or every line of data documents with high significance. This can result in analytic exhaustion and failure to establish a research focus (Kelle, 2007). Second, and conversely, withholding theoretical notions can limit what researchers see happening in their data. For example, early in my career when my efforts (with my colleague Karen Golden-Biddle) to learn and understand the craft of writing for scholarly publication turned into a grounded theory research project theorizing the crafting of contribution (e.g., Locke & Golden-Biddle, 1997), reading rhetorical theory was critical in helping us move beyond a brute window pane view of writing, bringing into focus how we as humans "do things with words." With this broadened set of tools, we were able to "see" and code so much more happening in the texts that were our data. Rather than being an encumbrance to discernment, it expanded what we were able to perceive and theorize about what was happening in crafting contribution. As researchers, we are aware of when we are using theory in a determinate way and when we are using it in a tentative way as a tool for exploration; we can choose the latter use. (From this perspective I understand characterizations of theoretical sensitivity to hinge more on its function and use in context than to classifications of it as the theory which is outside the boundaries of the research project).

Finally, as Bailyn noted in her comments, our field strongly values contributing to theory. Theorizing what is happening in the data without taking into account how what one is finding relates to existing literature until after the emergent conceptual framework is established risks a possible waste of research energy and resources. As I learned with my dissertation, when a theoretical frame is independently developed, the literature to which it might contribute is not self-evident. More recently, I recall experiencing a sinking feeling when as a reviewer I found myself reading a manuscript presenting a beautifully grounded and elaborated theoretical framework; I could see all the careful thoughtful work that went into development of the core category. The problem, from the perspective of all three reviewers, was that there was nothing new. Furthermore, the fit between the presented data and the categories developed was so strong that as reviewers we could not discern a possible more theoretically fruitful adjustment to the researchers' analytic focus. The manuscript did not receive an offer to revise. The framework was likely very useful to the participants in the organizational setting studied to reflect on their own behavior, but it was less useful in making a contribution to theory.

Asking how what is emerging in the in-process categories might be the same as and different from what is established in the literature during the analytic process can help researchers both work toward clarifying their contribution and discern what is happening in their data that is different from what has already been said. Of course, this adds a further layer of complexity to the process. To be clear, I am not suggesting that theory has to be incorporated in this way, and I am not elevating theory at the expense of observations. It is appropriate to be wary of theory, and I agree that it can consume energies better devoted to finding out about what is happening in the world (Becker, 2000); what I am arguing for is a more pragmatic stance. Rather than making strong prescriptive statements about keeping theory out of the analytic process, I believe it would be more useful to explore the different ways in which theory and observations may relate during analysis and what the tradeoffs associated with each of the ways might entail.

In the end, there is a tension between abstract methodological steps and their translation on the ground in specific research projects (Suddaby, 2006). Recent developments in the philosophy of science make a case for articulation of research guidelines as strategic principles rather than as pre- or proscriptive rules to be mechanically followed (Garver, 2001; Hintikka, 1999, 2001). For example, ''maintain the primacy of observations'' might be such a principle as compared with, say, ''the literature must be avoided'' during analysis. As we develop our research craft, we need to interrogate and develop a feel for the strategic principles implied in our methods, including when and how prescriptions make sense for a given research project; I believe it would be useful if our conversations about methods invited doing so.

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Author Biography

Karen Locke is the W. Brooks George Professor of Business Administration at the College of William and Mary's school of business. She joined the faculty there in 1989 after earning her PhD in organizational behavior from Case Western Reserve University. Her work focuses on developing a sociology of knowledge in organizational studies and on the use of qualitative research for the investigation of organizational phenomena. Her work appears in journals such as *Academy of Management Journal*, *Organization Science*, *Journal of Organizational Behavior*, *Journal of Management Inquiry*, *Organizational Research Methods*, and *Studies in Organization, Culture and Society*. And, she has authored *Grounded Theory in Management Research* and coauthored *Composing Qualitative Research* (with Karen Golden-Biddle), both books published by Sage. She is a recipient of the Academy of Management's Robert McDonald Award for the Advancement of Organizational Research Methodology.