Rewriting The Discovery of Grounded Theory After 25 Years?

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Barney Glaser and Anselm Strauss published their account of the methodological approach and practices they followed in their study of dying in health institutions (1964, 1965a, 1965b) as The Discovery of Grounded Theory in 1967. The authors wrote this text at a time when sociological researchers largely were concerned with testing hypotheses derived from the work of a few specialized theorists. Framed as a polemic against this verificalional enterprise, The Discovery of Grounded Theory was written with several specific purposes in mind: to encourage researchers to use their intellectual imagination and creativity to develop theories relating to their areas of inquiry, to suggest methods for doing so, to offer criteria to evaluate the worth of discovered—as distinct from tested—theory, and to propose an alternate rhetoric, that of generation, to balance out the rhetoric of verification in journal articles and monographs. This volume is now a touchstone (Stinchcombe, 1982) for scholars conducting qualitative research in organization studies.

Although they shared this common research experience and purpose in writing the book, the authors came to their common work trained in quite different research traditions. Glaser trained at Columbia University, with its tradition of formal theorizing and verificational quantitative methods, and Strauss came out of the University of Chicago, with its strong tradition in field observation, intensive interviewing, and pragmatic theorizing.

Strangely, more than 30 years into their relationship, a controversy apparently has surfaced over the procedures involved in the research method they published in 1967 and have continued to practice and to rewrite to the present (see Corbin & Strauss, 1990; Glaser, 1978, 1992; Strauss, 1987; Strauss & Corbin, 1990, 1995).

According to Stern (1994), who was trained by Glaser, two methodological schools now exist in the grounded theory approach, the Straussian (after Anselm Strauss) and the Glaserian (after Barney Glaser). Dissension about the method and arguments for the presence of “two schools” first surfaced in Glaser’s 1992 methodological Basics of Grounded Theory Analysis. This text is crafted as a rebuttal to Strauss and Corbin’s (1990) Basics of Qualitative Research, which Glaser claims erroneously rewrites the method they cooriginated. Glaser offers his volume to those who wish to conduct grounded theory research as a rewritten and “corrected version of Strauss’ book” (1992, p. 3). He insists that those researchers who follow Strauss and Corbin’s guidance will develop “forced conceptual descriptions” as opposed to “grounded theories.” For his part, Strauss acknowledges that there are differences in specific terminology and even in specific procedures between himself and his cooriginator. However, he contends that both “express an identical stance toward qualitative analysis and suggest the same basic procedures” (1990, p. 8).
In this essay, I am concerned with the *rewritings* of *The Discovery of Grounded Theory*. The recent publications by each of the original authors constitute rewrites of the grounded theory approach. Moreover, when organizational studies researchers make use of the approach and methods as originally specified by Glaser and Strauss, writing them into their research publications, they also are rewriting *The Discovery of Grounded Theory*. In examining these rewrites of the work, I am, of course, also offering my own rewriting, not only of that text but also of all the works that inform this essay.

I begin by presenting a “cliff notes” rendition of the central features and operational procedures for building grounded theories as laid out in the original publication. Following this, I consider the controversy expressed in Glaser’s and in Strauss’ recent rewrites of the approach. My writing of the controversy demonstrates that I understand it to reflect differences in each author’s understanding of the role of researchers vis-à-vis the phenomena they study. Specifically, Strauss locates agency for theory development in human researchers, whereas Glaser confers agency on neutral methods and data. These differences are reflected in the disparate stands each takes on the sources on which researchers may draw to develop theoretical insights and on the practices that ensure complexity in theoretical frameworks. I conclude by questioning organizational theorists’ rewriting of the grounded theory approach.

**A CLIFF NOTES REWRITING OF THE DISCOVERY OF GROUNDED THEORY: RECURSIVE THEORY-DRIVEN ANALYSIS AND SAMPLING**

The grounded theory method outlined by Glaser and Strauss in 1967 results in a conceptual framework that has been inductively derived from “directly” gathered (Mintzberg, 1984) “naturalistic” data (Turner, 1983). A grounded theory has a number of characteristics: It must *closely fit* the substantive area studied, be *understandable* to and *usable* by those in the situation studied, and be sufficiently *complex* to account for a great deal of variation in the domain examined.

A recursive, process-oriented, analytic procedure is the key feature of the research method they developed. As Glaser and Strauss (1967) pointed out, “Most hypotheses and concepts not only come from the data, but are systematically worked out in relation to the data during the course of the research” (p. 6). In practice, the conceptually distinct research activities of data collection, coding, and interpretation overlap to a significant degree: “They should blur and intertwine continually, from the beginning of an investigation to its end” (1967, p. 43). Figure 1 depicts the recursive overlap of data collection, coding, and interpretation that characterizes this approach.

In terms of procedural guidance, *The Discovery of Grounded Theory* put forward two key analytic operations that occur in tandem: making constant comparisons and theoretical sampling. As soon as researchers start forming provisional categories or abstractions from the data, comparison begins. As an incident or data observation is coded into a category, it is simultaneously compared with other incidents in that category. This comparison challenges and further explicates the properties of the initial concepts or categories with more data. At the same time, as Figure 1 indicates, the initial coding activity directs researchers toward more data collection. In this *theoretical sampling*, the researcher decides which additional data (events, activities, populations, etc.) are relevant to explicate and develop all properties of the evolving conceptual categories. Thus the grounded theory approach requires not only that data and theory be constantly compared and contrasted during data collection and analysis but also that the materializing theory drives ongoing data collection. As the categories take shape, an additional comparison process is added in which new data observations are compared with the developing properties of the emerging framework. These two comparative processes and further sampling continue until data gathering and comparative analysis yield no new examples and properties of a conceptual category. This is the point of *theoretical saturation*; it assumes that the researcher has deliber-
ately tested the limits of a category through theoretical sampling and constant comparison.

For example, Turner (1981) describes the development of one conceptual category that was originally created with the following ungainly provisional label:

Acceptance of partial view of problem obscuring wider view. Or/confusion of one factor with another (synecdoche?).

By comparing other incidents in his data set and by gathering additional data for more and different kinds of examples, this initial concept evolved into the category *decoy phenomena*. It was finally defined in this way:

Paying attention to some well defined problem or danger distracts attention from a still dangerous but ill structured problem in the background. (Turner, 1978, p. 60)

*Decoy phenomena* later became part of the conceptual infrastructure of a comprehensive theory about how industrial disasters occur (Turner, 1978). Such categories or codes and their properties, then, are the basic building blocks of a grounded theory. As they are developed, the same recursive, theory-driven, comparative processes are used to surface the links and relationships among the categories to construct a complete theoretical framework.

In 1967, Glaser and Strauss offered a general model for constructing new theory and some foundational research operations. Subsequent works on grounded theory methodology by both authors (Corbin & Strauss, 1990; Glaser, 1978; Strauss, 1987; Strauss & Corbin, 1990) provide more detail and codification of the analytic operations researchers need to perform to arrive at their discoveries. Specifically, the authors put forward coding paradigms that facilitate development of conceptual complexity and density in the resulting theories. Turner’s work also provides useful guidance on and examples of grounded theorizing within the domain of organizational studies (Martin & Turner, 1986; Reeves & Turner, 1972; Turner, 1981, 1983).

**WRITING DIFFERENT IMAGES OF THE RESEARCHER: ACTIVELY PROVOKING VERSUS EXERCISING DISCIPLINED RESTRAINT**

In *Basics of Grounded Theory Analysis*, Glaser (1992) argues for innumerable substantive differences between his and Strauss’ rewritings of the discovery process associated with grounded theory. Reading the methodological treatises of both authors indicates that issues about method are tangled with issues about friendship, loyalty, and intellectual ownership. As far as the foundational analytic procedures sketched earlier, I find no differences between the two authors. However, they do write substantively different renditions of researchers’ relationships to the worlds they study, and this is expressed in the analytic process. Thus Strauss and Corbin’s (1990) rewriting expresses a very active, even provocative, role in which researchers essentially interrogate the data they gather to arrive at conceptual categories. Glaser not only finds such active provocation of the data unnecessary but also objects to it on the basis that it will contaminate the concepts that are formed. This provocative role violates Glaser’s restrained approach in which researchers maintain distance and independence from the phenomena they study. He insists that the analytic techniques offered by Strauss (1987) and Strauss and Corbin (1990) will preempt and obstruct understanding of the phenomena under study. He underscores this by subtitling his work *Emergence Vs. Forcing*.

The tenet expressed by Glaser—that researchers should ascribe agency to the neutral methods they passively execute, constructing, as it were, a one-way mirror through which the natural world might be revealed—is consistent with the verificationist tradition in which he was trained at Columbia. Bazerman (1988) refers to this as the active seeking of passive constraints—that is, researchers actively seek to prevent and minimize their impact on the data through methods that restrain their influence. As Glaser’s student emphasizes, reading data and comparing incident to incident and incident to category are the sufficient and necessary conditions for discovery: “The electricity arcs and the world is there to see and to take” (Stern, 1994, p. 218). Clearly, in this tradition, the natural world is “out there,” and with an appropriate method executed with discipline and restraint, it will embed itself in theory. This portrayal of researchers presented in Glaser’s 1992 publication is entirely consistent with the images scientists working in the positivist tradition present of themselves in their disciplinary writing (Bazerman, 1988; Knorr-Cetina, 1981; McCloskey, 1985; Selzer, 1993).

**Illustrative Expressions of Provocation and Restraint**

Consider this discussion of open coding (Glaser, 1978; Strauss, 1987; Strauss & Corbin, 1990), the pro-
cess by which data are initially conceptualized and categorized. Strauss and Corbin suggest that conceptualization is facilitated by asking numerous questions of the data. The observation of a woman who seems dressed too formally in a restaurant kitchen is analyzed in the following way: “You notice that she is intently looking around the kitchen area, a work site, YOU ASK YOURSELF, WHAT IS SHE DOING HERE? THEN YOU LABEL IT watching. Watching what? Kitchen work” (1990, p. 64, emphasis in original). For Strauss and Corbin, such provoking of the data is useful for “breaking down” and categorizing the data. They view researchers as interpreters of the data they study who can build good complex theories by actively “opening up” the data to discovery.

Glaser, on the other hand, finds the questioning to be unnecessary and corrupting. His comments reveal the importance of restraint in coming to discover the world:

Concepts will emerge as the analyst compares and codes. This is one of the first junctures the analyst must guard himself against: that of slipping into preconception instead of listening carefully to each incident in order to figure out what the research is truly a study of. (1992, p. 45)

The only questions appropriate for Glaser are those he outlines as “totally neutral” (1978, 1992). For example, he would only ask, “What category or what property of what category does this incident indicate?” These questions are, of course, appropriately constrained because the constant comparative method is sufficient and necessary to let the data speak. “Categories emerge upon comparison and properties emerge upon more comparison. And, that is all there is to it” (Glaser, 1992, p. 43).

Provocation Versus Restraint in Developing Insights

This disparate view of researchers and their relationship to the worlds they study results in a fundamentally different perspective on the use of various sources to assist in conceptualizing the data. For example, Strauss and Corbin (1990) allow for the potential of prior theory, nontechnical literature, and personal as well as professional experiences to help researchers gain insight into the data. Glaser’s objection to this, based on the need for passive constraint to allow only the world under study to shape theorizing, is to be anticipated: “The analyst should just not know as he approaches the data” (1992, p. 50).

The dictum in grounded theory research is: There is a need not to review any literature in the substantive area under study. This dictum is brought about by the concern to not contaminate, be constrained by, inhibit, stifle or otherwise impede the researcher’s effort to generate categories, their properties and theoretical codes. (Glaser, 1992, p. 31)

In other words, Glaser is advocating the position that the researcher should not bring any a priori knowledge to the research endeavor. This stance seems to rewrite the flexible orientation toward cultivating insight articulated in The Discovery of Grounded Theory. In 1967, Glaser and Strauss suggested it was possible for researchers to cultivate fruitful insights from many sources without compromising those suggested by the data. For, in the end, they would have to be worked out in relation to data. Indeed, phenomenology and hermeneutics suggest that this is a conceptual and practical impossibility.

Provocation Versus Restraint in the Elaboration of Analytic Practices

As indicated earlier, both Glaser and Strauss originally developed coding paradigms to support the evolution of conceptually complex theories. However, Strauss and Corbin (1990) elaborate a hierarchy of coding types and with their related forms of theoretical sampling. They insist that filling in such a structure will ensure conceptual density. Three forms of each are offered. During open coding, when researchers are forming their provisional categories, the authors suggest relatively indiscriminate data gathering or open sampling of those persons, places, and situations that will provide the best opportunities for collecting relevant data. During axial coding, when researchers are tracking the particular categories developed in open coding so as to identify their relationships to each other, Strauss and Corbin advise actively seeking out those situations that will provide opportunities to gather data about how the categories are interrelated. They term this variational and relational sampling. At the apex of the coding-sampling hierarchy is selective coding. Selective coding is carried out to validate the relationships identified during axial coding and to refine any categories that need further development. The additional data sampling, termed discriminate sampling, must be very focused and deliberate. At this point, researchers deliberately seek out opportunities for validating the developed theory’s story line and the proposed relationships between categories.
Although the differences between the coding and sampling practices articulated by each cooriginator are largely a matter of the degree to which they are codified, they are significant to Glaser. For him, these three forms of sampling constitute unnecessary and contaminating impositions on the analytic process. "Strauss's sampling is controlled by the evolving relevance of concepts, and relevance comes from testing out what is looked for, not what is emerging" (Glaser, 1992, p. 103). Thus, time and time and time again, Glaser charges Strauss (and Corbin) with promoting practices that prematurely anticipate the data and that interfere with the ability of the phenomena studied to inform discovery.

REWITING
THE DISCOVERY OF GROUNDED THEORY
IN ORGANIZATIONAL STUDIES

In the previous section, I have suggested that Glaser and Strauss, including the latter's coauthors, have been rewriting the role of researchers in the grounded theory approach. Glaser has been rewriting the role to emphasize the need for disciplined restraint so as to maintain the integrity and neutrality of the method that allows studied phenomena to inform theorizing; Strauss has pushed researchers to actively engage what they study and to systematically explore the full possibilities of their data. They are not alone: Management researchers also have been rewriting grounded theory method.

Reviewing the use of Glaser and Strauss (1967) in one organization studies outlet, the *Academy of Management Journal*, I made the following observations. The first concerns how extensively *The Discovery of Grounded Theory* is cited. Nineteen qualitative studies have been published in *AMJ* since 1976 when it was created as a separate journal. Sixteen of these studies cite the original grounded theory publication. (Of the three that do not, one offers no account of the research process, and the other two more recent articles use research approaches from literary criticism.)

The second observation involves the extent to which Glaser and Strauss' elaborations of the theory are drawn on. All 16 *AMJ*-published studies appeared at least 2 years after Glaser's 1978 elaboration of grounded theory in *Theoretical Sensitivity*, 11 appeared at least 2 years after the publication of *Qualitative Analysis for Social Scientists* (Strauss, 1987), and 3 appeared at least 2 years after Strauss and Corbin added *Basics of Qualitative Research* (1990) to the literature on grounded theory. Yet only one article cites any of these subsequent works. Taken together, these two observations suggest that, at least in *AMJ* publications, even though *The Discovery of Grounded Theory* is referenced extensively, the elaborations of the approach offered by the cooriginators and the controversy surrounding these developments are either deemed irrelevant or are unknown. Apparently, as management theorists, we have gone to other methodological sources (cf. Miles & Huberman, 1984; Yin, 1981) and to neighboring qualitative researchers (cf. Eisenhardt, 1989; Gersick, 1988; Harris & Sutton, 1986) for further analytic guidance, preferring not to keep up with developments in the methodological traditions that originally informed our methods and that we continue to cite.

The third observation concerns the way in which the original Glaser and Strauss (1967) has been cited. Most often (9 of the 16 studies), *The Discovery of Grounded Theory* was cited to underscore an iterative analytic process in which researchers repeatedly tested their emergent theoretical frameworks against their data. (It is not clear in these articles that theoretical sampling was included as part of the recursive process.) Second in frequency (7 of 16), the notion of theoretical saturation was invoked as the basis for deciding that the theoretical framework was complete. Third on the frequency ranking (3 of 16) was the use of *The Discovery of Grounded Theory* to distinguish the study as inductive and theory building.

Taken together, these observations on the way in which *The Discovery of Grounded Theory* is cited raise a number of other issues that are relevant to the current practice of grounded theory research and writing. The grounded theory approach is being selectively rewritten and, indeed, selectively drawn from in qualitative organizational research. Although one core aspect of the approach, the constant comparison of data and theory, is retained, another central element, theoretical sampling, is not discussed. It is essentially being written out of the analytic process. This smacks of an "anything goes" approach to research methods in which we may indiscriminately and arbitrarily pick and choose data-gathering techniques and analytic operations. For example, when we claim to be using a "grounded theory approach" in a study in which participants to be interviewed were "randomly" selected, as at least one study did, we are clearly confusing analytic traditions as well as procedures. Any study in which observations were randomly selected violates
one of the central analytic tenets and operational procedures of the grounded theory method.

Qualitative methods as a category comprises a huge umbrella of research activities and disciplinary traditions. How can we maintain coherent practices of inquiry if we are not careful in the use of specific approaches? When researchers report that a "grounded theory" approach was used, to take the example at hand, they need to have used its key operations and to provide information on how theoretical categories emerged, the basis on which theoretical sampling proceeded, and so on. Readers need to be able to evaluate the research based on the presence or absence of a full methodological report. Broad, unspecified claims—for example, the statement that "this research followed Glaser and Strauss (1967)," with no operational indicators or, indeed, with procedures that run counter to the method's specifications—cannot stand. This does not mean that we ought not combine various approaches and analytic operations. However, we need to be clear that we are doing so, ensure that the combined methods do not contradict one another, and explain the necessity of the combination to the specific study.

But what if we are only muddling our terms? What if we performed appropriate data-gathering and analytic operations but didn't use the conforming technical vocabulary? What are the consequences of muddling our terms in the reporting of the approaches and operations we carry out in the conduct of our inquiries? First, how can we as researchers talk about our processes of inquiry if we distort what those processes entail and misuse what we assume to be a common vocabulary? Such muddling delays the point at which we as researchers will have a repertoire of codified, accepted, and widely understood procedures by which to understand and evaluate each other's research processes. How else can we agree on what was done and talk about it?

Finally, given the dominance of The Discovery of Grounded Theory as a methodological citation in the research methods sections of these articles, what if we invoke the work only to add legitimacy to our studies? Where is the harm in a little innocent trading on the acknowledged adequacy and eminence of a methodological touchstone (Stinchcombe, 1982) even if it is not closely related to specific operational procedures carried out in the research? First, it contributes to the writing of research methods in a rhetoric of justifica-

REFERENCES


