MORE GROUNDED THEORY METHODOLOGY:
A Reader

Edited by

Barney G. Glaser, Ph. D.

Sociology Press

looking at social psychological issues in a context which addresses macro structural issues. For a further discussion of analyzing stressful life events in a theoretical context which takes macro issues into account, see Gerhardt U. Coping and social action: theoretical reconstruction of the life-event approach. *Social. Hlth Illn.* 1, 195-225-1979.

42. Lubkin I.M. *Chronic Illness: Impact and Interventions.* Jones & Bartlett, Boston, MA, 1986.

43. Locker D. *Disability and Disadvantage: The consequences of Chronic Illness.* Tavistock, London, 1983.

44. Charmaz K. Intensive interviewing. Unpublished manuscript, 1986.

45. Gordon R.L. *Interviewing.* Dorsey Press, Homewood, IL, 1980.

46. Mishler E.G. *Research Interviewing: Context and Narrative.* Harvard University Press, Cambridge, MA, 1986.

47. Memo-writing comes close to what teachers of writing call pre-writing or free-writing, although memos are focused on a category or code. See, for example, Elbow P. *Writing with Power.* Oxford University Press, New York, 1981. The analyst may write anything in the memo without the constraints of evaluation, or of audiences. Memo-writing helps to reduce writer's block and also helps to bring the fluidity, imagery, and rhythm of spoken language into the work.

48. Becker H.S. *Writing for Social Scientists.* University of Chicago Press, Chicago, IL, 1986.

49. For a way of presenting data on interviews and respondent accounts which specifies the amount, if any, of rendering by the researcher, see Gerhardt U. and Brisekorn-Zinke M. The normalization of hemodialysis at home. In *Research in the Sociology of Health Care* (Edited by Roth J.A. and Ruzek S.). Vol. 4, pp. 271-317. JAI Press, Greenwich, CT 1986.

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THE GROUNDED THEORY METHOD:

An Explication and Interpretation

Kathy Charmaz, Ph.D.

Publication of Glaser and Strauss' pioneering book The Discovery of Grounded Theory (1967), provided a strong intellectual rationale for using qualitative research to develop theoretical analyses. The authors were protesting against a methodological climate in which qualitative research typically was viewed as only a helpful preliminary to the ''real'' methodologies of quantitative research (see, for examples, Hyman et al. 1954; Lazarsfeld 1944; and Stouffer 1962). In addition to providing a powerful rhetoric for qualitative analysis per se, in the Discovery book Glaser and Strauss also began articulating research strategies to codify the analytic process throughout the research project. In the decade that followed, other qualitative researchers who held different perspectives, notably Douglas (1976), Johnson (1975), Katz (this volume), Lofland (1971), and Lofland and Lofland (in press), and Schatzman and Strauss (1973) contributed to the growing literature on collecting and rendering qualitative materials.

Both the assumptions and analytic methods of grounded theory have been criticized by some qualitative researchers on a number of counts. Lofland and Lofland (in press), for example, suggest that grounded theorists fail to give proper attention both to data collection techniques and to the quality of the gathered materials. From Katz's (this volume) perspective, discovery and verification are inseparable and the grounded theory contrast between them in some sense perpetuates the notion that qualitative research is preliminary. These criticisms misinterpret the aims and methods of grounded theory. Unfortunately, several features of the grounded theory method have contributed to such misinterpretation. First, the language of the grounded theory method relies on terms commonly used in quantitative research and, I believe, this language lags behind actual development of the method. To illustrate, the terms such as coding, comparison groups, and theoretical sampling reflect the language of quantitative research and often elicit images of logical deductive quantitative procedures.[1] Second, the method arises from and, to date, relies on Chicago school sociology, which, as Rock suggested (1979), depended heavily on an oral tradition implicitly transmitted to students. I view grounded theory similarly as a practice learned largely through apprenticeship. Although Glaser's (1978) work is a critical step

forward in explicating the oral tradition in grounded theory, the work contains many tacit assumptions and speaks most directly to students who worked closely with him or Strauss.

In response to these criticisms and misunderstandings of the grounded theory method, I aim to: (1) explicate key analytic procedures and assumptions often left implicit in earlier statements; (2) offer interpretations which suggest varying approaches to the method; and (3) provide substantive applications of the method to illustrate how it can be used during the analytic process. Because I aim to explicate and interpret the method, I draw heavily on approaches developed by Glaser (1978). Throughout the discussion, I will provide examples and illustrations from my own past and current research using this method.

A PRELIMINARY STATEMENT OF THE GROUNDED THEORY METHOD

The grounded theory method stresses discovery and theory development rather than logical deductive reasoning which relies on prior theoretical frameworks. These two aspects of the method lead the grounded theorist to certain distinctive strategies. First, data collection and analysis proceed simultaneously (see Glaser and Strauss 1967). Since grounded theorists intend to construct theory from the data itself, they need to work with solid, rich data that can be used to elicit thorough development of analytic issues (see Lofland and Lofland, in press). Grounded theorists shape their data collection from their analytic interpretations and discoveries, and therefore, sharpen their observations. Additionally, they check and fill out emerging ideas by collecting further data. These strategies serve to strengthen both the quality of the data and the ideas developed from it.

Second, both the processes and products of research are shaped from the data rather than from preconceived logically deduced theoretical frameworks (see, for example, Biernacki, forthcoming; Broadhead, in press; and Wiener 1981). Grounded theorists rely heavily on studying their data and reading in other fields during the initial stages of research (see Glaser 1978). They do not rely directly on the literature to shape their ideas, since they believe that they should develop their own analyses independently. From the grounded theory perspective, researchers who pour their data into someone else's theoretical framework or substantive analysis add little innovation and also may perpetuate ideas that could be further refined,

transcended, or discarded.

Third, grounded theorists do not follow the traditional quantitative canons of verification. They do, however, check their developing ideas with further specific observations, make systematic comparisons between observations, and, often, take their research beyond the confines of one topic, setting, or issue. Perhaps because they make systematic efforts to check and refine emerging categories, their efforts may be confused with traditional verification. From the grounded theory perspective, the method does not preclude verification by other types of researchers; it merely indicates a division of labor.

Fourth, not only do grounded theorists study *process*, they assume that making theoretical sense of social life is itself a process. As such, theoretical analyses may be transcended by further work either by the original or a later theorist by bringing more and different questions to the data (see Glaser 1978). In keeping with their foundations in pragmatism, then, grounded theorists aim to develop fresh theoretical interpretations of the data rather than explicitly aim for any final or complete interpretation of it (see Schwatrz and Jacobs 1979).

CODING

Coding, the initial phase of the analytic method, is simply the process of *categorizing* and *sorting* data. Codes then serve as shorthand devices to *label, separate, compile,* and *organize* data. Codes range from simple, concrete, and topical categories to more general, abstract conceptual categories for an emerging theory.[2] In qualitative coding, researchers develop codes out of their field notes, interviews, case histories, or other collected materials (these could include diaries by participants, journals, interactional maps, historical documents, and so forth). Examples of codes I have used in my studies of chronic illness include self-esteem, sources of support, discovering illness, defining limitations, transitory self-pity, identifying moment, and identity questioning. These codes range from lesser to greater complexity as the analytic process proceeds.

Codes may be treated as conceptual categories when they are developed analytically. This means the researcher defines them carefully, delineates their properties, explicates their causes, demonstrates the conditions under which they operate, and spells out their consequences. A descriptive category such as defining illness applies to the substantive area studied. A theoretical category such as transforming identity, in contrast, is part of a

theoretical scheme, and may be applied across diverse substantive areas.

Codes serve to summarize, synthesize, and sort many observations made of the data. By providing the pivotal link between the data collection and its conceptual rendering, coding becomes the fundamental means of developing the analysis. Hence, the categorizing and sorting inherent in coding are more than simply assigning subject headings or topics to data. Researchers use codes to pull together and categorize a series of otherwise discrete events, statements, and observations which they identify in the data.[3] Researchers make the codes fit the data, rather than forcing the data into codes. By doing so, they gain a clearer rendering of the materials and greater accuracy. When reading the data, grounded theorists ask: "What do I see going on here?" To illustrate, when reading a number of interviews with widows who had been housewives, I noted that these women repeatedly mentioned the pressures to establish social and economic independence that they confronted due their husbands' deaths. Although they later enjoyed their new pursuits, they initially were fearful and reluctant to begin independent lives. Here, I coined the term "forced independence" to code their experience into a more abstract conceptual category that described and analyzed the widows' experience (Charmaz 1980a).

Grounded theorists code for *processes* that are fundamental in ongoing social life. These processes may be at organizational or social psychological levels, depending on the researcher's training and interests. To find these processes, grounded theorists carefully scrutinize participants' statements and actions for patterns, inconsistencies, contradictions, and intended and unintended consequences. The initial questions they ask are: What are people doing? What is happening? (See Glaser 1978). What the researcher views the participants as doing may not be the same as what participants claim to do. For example, in a treatment unit, participants may claim that their actions are devoted fundamentally to treating patients, yet the researcher may decide an intense jockeying for power underlies their actions, and thus, is the more significant process to pursue.

The *assumptions* that participants hold provide a fertile field for coding. Seeking to discover, identify, and ask questions about these assumptions keeps the researcher thinking critically and defining what is implicit in the data. The researcher then defines how participants act upon their assumptions in the specific setting, which, of course, helps the researcher convert topics into processes. Further, rather than viewing the participants'

assumptions as truth itself, the researcher gains some distance on his or her materials. In this way too the researcher avoids overimmersion, which may lead to taking over the views of participants as one's own. For example, a medical sociologist who uncritically accepts the practitioners' discourse of meaning may shape his or her research around terms such as "coping," "stress," and "stress-reduction" without looking at their underlying assumptions.

When looking for processes, the researcher must also ask: What kind of events are at issue here? How are they constructed? What do these events mean? By looking for major processes, researchers delineate how events are related to each other. In a particular study, a researcher may identify several major processes. If so, then grounded theorists code for all of them and may decide later which ones to pursue. Importantly, a grounded theorist sticks with his or her interpretations of the data and follows leads from them, even when they lead to surprising new research problems.

At times, researchers readily identify basic processes, for example, when they are so visible, stark, and direct that even a naive researcher quickly defines them. But other major processes remain much more implicit and covert. Those which are tacitly shared but remain unspoken sometimes are difficult to pull out. This is particularly the case when participants themselves cannot articulate the assumptions and meanings that they, in fact, hold and act upon. In an earlier project on a rehabilitation institution for the physically disabled, I discovered that middle-class staff held markedly different conceptions of time than most of the lower-class patients whom they served. Professional staff held a linear progressive view of time, with realization of goals in the future. Yet they worked with patients who generally held a cyclic view of time, situated in the present. For the patients, time moved from present to present, from crisis to crisis. Repeatedly, staff became frustrated by these patients' failure to use time in the institution to work on the small incremental gains toward a distant goal that staff viewed as both medically and personally appropriate. Instead, patients simply passed time, waited for change, or killed time with unsanctioned pursuits until discharged (Calkins 1970).

Initial Coding

Coding is a two-phase process: an initial searching phase precedes a later phase of *focused* coding (Glaser 1978). In the initial phase, researchers look for what they can define and discover in the data. They then look for leads, ideas, and issues in the data themselves. Glaser (1978) advocates line by

line coding to gain a full theoretical accounting of the data. This prompts the researcher to look at the data with a theoretical eye from the start and actively encourages playing with and developing ideas.

Although every researcher brings to his or her research general preconceptions founded in expertise, theory, method, and experience, using the grounded theory method necessitates that the researcher look at the data from as many vantage points as possible. At this point, the rule for the researcher to follow is: *study your emerging data.* At first, the data may appear to be a mass of confusing, unrelated accounts. But by studying and coding (often I code the same materials several times just after collecting them), the researcher begins to create order.[4] If researchers think that the data suggest more questions than they can answer, then they need to collect more data while simultaneously coding them. Sometimes, neither the data actually collected nor the researcher's emerging ideas are related to the original research objectives or topics. In this case, the researcher either continues with the material on hand or finds more appropriate sources of data for the original topic. In my chronic illness interviews, for instance, I had not anticipated covering either self-pity or social support. Yet both these topics were repeated themes so I followed up on them.

Several further questions from my experience may help. First, I attend to the general context, central participants and their roles, timing and structuring of events, and the relative emphasis participants place on various issues in the data.[5] I also look for connections between individuals' special situations and problems and their interpretations of their experience.[6] For example the problems of leading independent lives become magnified for young adults with serious chronic illnesses who seek simultaneously to develop intimate relationships and to prepare themselves for jobs.

Second, I construct codes to note what participants lack, gloss over, or ignore, as well as what they stress. For example, I note the kinds of information patients possessed or lacked about their illnesses when they first were diagnosed. Also, since I am interested in time perspective, I note the lack of awareness of time when respondents tell me that they did not think about time at all as well as other respondents' descriptions of an intensified awareness of time. On a more concrete level, I code for the absence of attention and assistance from intimates when it is observed, implied, or reported, besides coding the detailed accounts of other patients who had available intimates to visit and help.

Third, I scrutinize the data for in vivo codes. Research participants sometimes describe their experiences with imagery and power that far transcend their individual situations. One young diabetic described himself as trying to become "super-normal," an experience that many newly and/our younger chronically ill persons shared. Later, I took the term super-normal identity and raised it to a conceptual level to treat analytically (Charmaz 1973). Many of the chronically ill talked about the significance of others "being there." Subsequently, I took the term "being there" as a code and devised subcodes to pull out its underlying meanings and assumptions.

Fourth, I try to identify succinctly the process that the data indicates. Here the onus is on the researcher to identify, through coding, what the data mean. For example, some respondents who said they had been "depressed" or "felt bad about myself" described these feelings in ways that were strikingly similar to those who explicitly defined their feelings as self-pity (Charmaz (1980a). Comparing bits of data with other data for their similarities and differences helps enormously in developing codes. For example, both "negative" feelings such as self-pity and elusive topics such as experiencing time sometimes prove to be difficult subjects for a respondent to address. So, I compare responses to help me identify what is implicit in one set of data but explicit in another. Then, I may decide to return to earlier respondents with more detailed queries.[7]

Examples of Initial Coding

In the following examples, I show the kind of diversity and number of codes developed in initial coding when the researcher pursues as many diverse avenues as he or she can create. The interview statements below are made by persons with different chronic illnesses. My study centers on experiences of time and self of the chronically ill.

Code: Self-perception; Awareness of difference; Identifying self through ill health; Comparing health to others'.

Interview Statement: *A 29-year-old man with renal failure was discussing his high school years, and events that occurred long before he was diagnosed.*

...I knew I was different. I caught colds very easily and my resistance was very low, and so I knew that generally speaking my health wasn't as good as everybody else's, but I tried to do all the things that everybody else was doing.

Code: Normalizing context of illness; Self esteem: feelings of failure, failure of self; Reality contradicts idealized experience.

Interview Statement: A 29-year-old woman with colitis was recounting her first episode of illness.

...I was under a great deal of stress as a result of all this bouncing around and trying to get a job and trying not to have to go home to my parents and admit that I had failed. [I] failed at life. I had left college, and left there saying, "Gee, I can do it on my own," so I was trying this exciting existence I read about and there was something wrong; I had all this pain. I didn't know what to do about it.

Code: Self in retrospect; Self-esteem; Outcome of timed struggle; Improving self-esteem as treatment goal.

Interview Statement: A 54-year-old woman who had cancer and currently had a crippling collagen disease was explaining her view on why she had had a recurrence of cancer.

...When I look back on my second bout of cancer, I was not feeling good about myself and the whole struggle of the last three years put me into X (cancer institute) to try to get me to feel better about myself.

Focused Coding

Focused coding is the second, selective and conceptual, phase of the coding process. In focused coding, the researcher takes a limited set of codes that were developed in the initial phase and applies them to large amounts of data. The process is selective because researcher has already weeded through the materials to develop a useful set of categories. It is conceptual because the codes employed raise the sorting of data to an analytic level rather than one that is used to summarize large amounts of information.

Focused coding forces the researcher to develop categories rather than simply to label topics. Categories may be taken either from the natural language of the participants (an in vivo code) or from the researcher's analytic interest. For example, I took the term self-pity and treated it as a category (Charmaz 1980a). Then I defined it by analyzing the data systematically. I developed another category out of my analytic interests: "identifying moments." This was not part of the natural language of my respondents. Instead, it reflected my categorization of those moments when

participants instantly defined clear meanings about their present identities. Since I was generally interested in relationships between time and identity, I looked for material in the data which illuminated connections between the two. I had heard a number of accounts of moments when identity was at issue before I created a category that reflected the described events.

The purpose of focused coding is to build and clarify a category by examining all the data it covers and variations from it. Frequently, this means going back through the data and resifting it in relation to the newly devised category. New categories may subsume earlier materials that were left uncoded or were coded in different ways.

Researchers also use focused coding to break up the category. They develop subcategories which explicate and exhaust the more general category. I broke my category of "identifying moment" into the rather obvious subcategories of positive and negative identifying moments and coded for them (after I witnessed moments when ill persons were identified positively). Properties must be identified for the categories developed through focused coding. The properties define the category, delineate its characteristics, and demonstrate the conditions when it develops. For example, a major property of "identifying moment" is the immediate, direct social identification one interactant confers upon another.

After developing their set of focused codes, the researchers may use knowledge of the literature to expand and clarify the codes and to sensitize themselves to ways of exploring the emerging analysis. Pretend, for example, a group of organizational researchers in a hospital find that nurse participants show much concern about "professionalism" but assume that everyone shares their implied meanings of it. The researchers need to discover precisely which meanings these nurses and other professionals hold. After collecting first-hand data, they may use the literature to compare meanings attributed to the term and the criteria invoked to indicate it with their data. The range of meanings of "professionalism" include: maintaining an objective distant attitude, realizing high-quality craftsmanship with criteria set by members of the occupation themselves, and claiming a high status while simultaneously dissociating from those who cannot also claim it. In this instance, researchers need to portray the meanings of the term held in the setting they study. Hence, they use the literature to help outline and compare these meanings rather than to force them into "correct" interpretations. In other instances, the literature can be used as direct data for focused coding. In both cases, the researcher uses the literature as a

source of *questions* and *comparisons* rather than as a measure of truth.

Since the grounded theory approach heavily emphasizes process, the categories developed are not treated separately as single topics; rather, grounded theorists weave them together into a *processual analysis* through which they can abstract and explicate experience. Thus, returning to my earlier example, defining self-pity through data analysis was just the first step. After categorizing types of self-pity and its social sources, I then developed the processual categories of becoming immersed in self-pity and reversing self-pity which were vivid when I directed questions toward them, but only implicitly related in the early data before I systematically explored these areas.

Focused coding helps the researcher to outline a framework that preserves the complexities of everyday life. By showing relationships between categories in ways that explain the issues and events studied, focused coding helps to provide the groundwork for developing explanations and predictions.

When researchers begin to question their data analytically, they are beginning to use it, rather than simply relate to an audience. For example, when organizational researchers investigate a topic such as staff turnovers, they would first define exactly what is meant by the term and cite the conditions under which such turnovers occur. Then they would use their data and their knowledge of the situation to help them determine which leads to follow up from there. Under which structural conditions do turnovers increase? Under which do they decrease? How do supervisors view turnovers? How do staff view them? What effects, if any, do they have on staff and client morale? Are there any subtle properties of turnovers that have direct effects on other parts of the organizational structure? Do supervisors change supervisory styles after a run of turnovers? What are the consequences of turnovers? What are the consequences of turnovers for direct client service? Are turnovers the "real" organizational issue or are they reflections of something else? (See Katz's essay, which follows.)

Many, if not most, researchers do develop or adopt "families" of codes that shape their emerging analyses (Glaser 1978). Among them are those that specify process, causation, degree, dimension, type, or a particular type of ordering such as structural, temporal, or generality (see Glaser 1978). By becoming aware of the elements of the code family invoked, one can raise

more questions in research and become a better critic of other research works.

A final comment is in order. When the data are rich and full, the researcher may mine the information repeatedly for diverse foci (see Glaser and Strauss 1965, 1968). What may have been implicit to the researcher becomes explicit when he or she reexamines the data with new focused codes. For example, my interests in the chronically ill were primarily directed to issues concerning identity and time. Although I amassed considerable data concerning social support or its absence, I initially did not look at this topic systematically. As I began to study support and recognized its relationship to trust, I also realized that betrayal was a crucial code for understanding the experience of one group of chronically ill persons (Charmaz 1982). Then I categorized types of betrayal and their consequences. In short, the researcher may engage in focused coding of the same data multiple times as he or she identifies new questions to put to it.

Examples of Focused Coding

In the following examples, I provide several focused codes with their corresponding data. The codes show the selective nature of focused coding.

Code: Identifying moment; Critical failure of self

Interview Statement: *A young woman who had had a serious flare-up of colitis recalled:*

...During this time I was under constant care by an intern who later thought I should see a different psychiatrist when I got out of the hospital because he thought I was coming on sexually to him and the odd thing about that was that I found him not sexually attractive at all - that was sort of an interesting twist to that thing. I mean when you are not in a very good place to be told that you have failed with your psychiatrist is like the parting blow. You know it was awful.

Code: Relation of interactional sources of self-pity and self-blame

Interview Statement: *A young woman with intensive experience in undergoing bureaucratic evaluations responded to my questions about how she felt about being scrutinized.*

...All I can do is dissolve in tears - there's nothing I can do. I just get immobilized - you just sort of reach a point, you can't improve, can't remedy the situation, and you're told you aren't in the right category for

getting the services you need and can't get for yourself. It makes me madder and madder at myself for being in the situation in the first place.

Code: Negative identifying moment

Interview Statement: *The following observations were made during an interview with a retired college professor and his wife, both of whom had chronic illness.*

... I asked, "Did you keep up with professional work after you retired?" He said: "I used to teach extension courses but with the budget and the governor, there isn't any money for extension courses." She [his wife] cut in [to me], "Andrei used to be an extremely successful speaker; partly his enthusiasm; partly his articulateness, but with the speech problems, he can't do it..." [He, slowly and painfully] "The schools don't have any money ... I can't speak very well.

I felt desperately sorry for him at this point. Whether or not both factors were at play at the point when they stopped calling him for extension teaching, this was a terrible moment for him when she said it. Regardless of the real reason, at this precise moment knowing what she thought of his deteriorating competence was critical to him. Participating in this short sequence was like watching someone who was observing his own identity crumbling away - it was painful both for him and for me, although I got the impression that she was so caught up in her perceptions of accuracy that she actually didn't see how it defaced him... Acknowledging that he can't speak very well was said like an admission of guilt or inferiority that was previously hidden from view.[8]

MEMO WRITING

Memos are written elaborations of ideas about the data and the coded categories. Memos represent the development of codes from which they are derived. An intermediate step between coding and writing the first draft of the analysis, memo writing then connects the barebones analytic framework that coding provides with the polished ideas developed in the finished draft. By making memos systematically while coding, the researcher fills out and builds the categories. Thus, the researcher constructs the form and substance toward a finished piece of work and develops the depth and scope of the materials.

Through memo writing the questions developed in coding are put into analytic context. The memo tells what the code is about; it raises the code to a category to be treated analytically. To differentiate between descriptive and analytic categories, consider the topic "friends." Descriptive treatment might focus on the link between friends, and their shared activities. Analytic treatment, in contrast, might focus on the implicit criteria for qualifying to be a "friend," the rhetorical uses of the term, the conditions for elevating someone from an acquaintance to a friend, the converse conditions for reducing a friend to an acquaintance or former friend, the mutual obligations necessary to sustain friendship, and the consequences of friendship for other relationships and activities. When treating the topic analytically, the researcher likely generates a set of categories which are more abstract than the original topic, and yet explicate underlying assumptions and processes.

Memo writing takes place throughout the research process starting with the first interviews or observations. These early memos shape aspects of subsequent data collection; they point to areas the researcher could explore further. They also encourage the researcher both to play with ideas and to make early assessments about which ideas to develop. Additionally, early memos provide concrete sources for comparison with materials gathered later. By writing memos throughout the research process, researchers avoid being paralyzed by mountains of unanalyzed data and immobilized by the prospect of needing to complete final papers and reports. As a crucial correction to such problems, writing memos throughout the research process sharpens and directs data collection and coding.

Since it fosters a theoretical rendering of the data, memo writing is a useful strategy at various levels of theoretical development. Some grounded theorists construct many short memos on diverse categories. They gradually build up levels of abstraction. Others write fewer memos but work at a more abstract, comprehensive level from the start. Although each reflects a working style, novices frequently discover that writing many memos helps to expand their theoretical grasp of the materials, keeps their analyses flexible, and provides sharper, clearer guidelines for data collection. Also, developing memos through rewriting gives the novice practice in systematically raising the analytic level of the ideas. A developed memo may become a whole section of a paper since it renders and synthesizes part of the data.

Writing is only one part of the grounded theorist's work with memos. Sorting and integrating memos follows memo writing. These two steps may themselves spark new ideas which, in turn, lead to more memos.

Writing Initial Memos

The first step in writing memos is to take codes and treat them as topics or categories. At the beginning of the memo, the author should title it and describe what it is about. If the grounded theorist already has a precise definition of the category, he or she provides it. If the category is concrete and visible, the researcher likely constructs a precise and immediate definition. If the memo is about some more abstract or ambiguous category - such as transforming identities - then the researcher may develop a precise definition later in the analysis. However, at this point, researchers should explore ideas during the memo-writing process. By keeping work flexible, the researcher may create more innovative and denser (many ideas integrated together) pieces of work.

When a category explicates a major pattern, grounded theorists stop and cite the conditions under which it operates and when it varies. What are the structural conditions giving rise to increased turnovers? What are the structural conditions under which a policy about turnovers is articulated or is reorganized? When writing memos, grounded theorists sometimes discover that they define new patterns and ideas that do not initially tie into their coded topic or category. Even when these connections are not apparent, they pursue the idea anyway but put the memo aside and reexamine it after finishing several other memos. (The ideas may make sense in another section of the work.)

Grounded theorists also explain how the code is related to other previously developed categories and codes. Spelling out the connections between categories assists in creating an integrated "whole," helps to reduce rambling, and aids in identifying implicit links, all of which tighten the work considerably.

Whenever writing a memo, researchers describe and discuss the category by delineating its properties as they are reflected in the data the category represents, or at least note the page and date of the properties in the data so quick retrieval is possible later (Glaser 1978).

Grounded theorists make comparisons explicit through memo writing. They often compare several observations in order to demonstrate the existence of the category they are talking about. For example, I composed a stack of accounts of feelings about illness when I was developing my material on self-pity to separate what constituted self-pity from other responses.

As more data accumulate, grounded theorists refine the earlier memos to account for greater variation, to gain a firmer grasp of the general context, and to understand the specific conditions under which the category works. By this time, the grounded theorist may also understand when the category changes and what its consequences are. For example, by examining the accounts of many patients, I was able to outline what contributed to moving away from self-pity, as well as the consequences of remaining immersed in it.

Sorting Memos

Sorting memos simply means putting those that elucidate the same category together in order to clarify its dimensions and to distinguish it from other categories. By going through accumulated memos and sorting them, researchers gain insight into what the core variables, key phrases in a process, or major issues are in the research.

When analyzing a process, the researcher quickly sorts the memos into phases of that process. Sometimes researchers discover that they actually have several issues or processes that can be covered separately. In that case, sorting keeps the researcher from muddling categories that are logically, if not experientially, distant. Conversely, an important dimension of sorting is to increase analytic precision in handling experientially mixed and muddy categories. In my work on chronic illness, for example, I analyzed the sources of loss of self (Charmaz, forthcoming). Living a restricted life results in loss of self. So does being devalued. Yet several people voluntarily restrict their lives to avoid devaluation. The categories are not entirely distinct, hence sorting helps to provide an analytic handle for communicating the categories in writing.

Grounded theorists sort for both the content of the memos and the ordering of them. The ordering of the memos, which forms the core of the paper, often reflects the ordering of experiences the data represent. The ordering may be explicitly grounded in the data as the researcher discusses steps in a process such as recruiting new workers. Or it may be implicitly grounded through the researcher's own sense of logic. For example, the organizational researcher might order memos on supervising by sorting for its properties, when it varies, its implications for morale, and its significance for getting the actual work done.

Integrating Memos

Sorting the memos helps to prepare for their subsequent integration. By

integrating the memos the researcher reveals the relationships between categories. Such integration does not always occur spontaneously; often the researcher has to demonstrate the integration explicitly. Although analyses of processes sort and integrate readily into phases, other analyses require the imposition of a logical order. After writing, sorting, and integrating memos, I sometimes share them with interested respondents to see how my analysis fits with their experiences and views (see Huber 1973).

In the following two memos, I treat the category "identifying moment." The first memo is an initial description of the category as I first developed it when working on my dissertation. The second memo refines and extends the earlier materials; it also takes into account substantial further ·data collection. In the second memo, I include raw data to illustrate the analytic points. That memo appears in almost identical form in the published paper (see Charmaz 1980a).

An Initial Memo on "Identifying Moments"[9]

Identifying moments, in which the individual is treated in ways which designate new definitions of who he really is, may be captured and dramatized in the person's mind. When the disparity is great between prior valuations of self and present treatment such as of being a person worthy of *respect* and the entire procedure is characterized by *disrespect*, from the long wait to being shunted around and having one's identity questioned and categorized, conditions exist for these individuals to feel that they are losing control of their selves and the form of their existence.

Further, identifying moments when the individual is being defined and categorized may instantaneously flash images of the future and heretofore *unforseen identity*. Consider the impact on the unsuspecting individual who hopes to remobilize later to be told the only category into which he fits is that for the "totally disabled."

A Later Memo on "Identifying Moments"

It became clear to me that how a particular chronically ill person was identified by others sometimes became revealed to them in the course of a moment's encounter or interaction. These moments gave the ill individual new reflections of self, often revealing that he (or she) is not the person he felt he was. Hence, within the course of a few moments, someone's self-image may be radically called into question.

Moments that call into question previously held definitions of self may be

identified as either negative or positive, although data describing negative moments are much more extensive (identifying moments may also reconfirm assumptions about self, although these are less likely to be recounted since they are not problematic).

Negative identifying moments are those shrouded in embarrassment and devaluation. They often lead to self-pity and self-blame: self-pity because of the implications of the definitions of the other; self-blame because of being in the situation in the first place. One woman described a demeaning encounter with a social service agency when in the course of a moment, she saw herself as being defined as someone not worth helping. She said,

> All I can do is dissolve in tears - there's nothing I can do. I just get immobilized - you sort of reach a point, you can't improve, can't remedy the situation, and you're told you aren't in the right category for getting the services you need and can't get for yourself. It makes me madder and madder at myself for being in the situation in the first place.

Negative identifying moments that occur in intimate relations are likely to be even more devastating. If ill persons can no longer claim preferred identities in other worlds in the present, although they may have possessed extraordinary identities in the past, they may feel that no recourse exists but to accept the identity thrust upon them since it was defined by those who know them most intimately.

(The observation of the elderly professor and his wife which occurs on pages 119-120 directly follows.)

THEORETICAL SAMPLING

Theoretical sampling means sampling aimed toward the development of the emerging theory (Glaser 1978; Glaser and Strauss 1967). As researchers analyze their materials and develop theoretical categories, they frequently discover that they need to sample more data to elaborate a category. Because researchers only develop theoretical categories through the analytic process, they do not know in advance what they will be sampling. Thus, theoretical sampling differs from the kind of selective initial sampling most qualitative researchers engage in as they set criteria for their research problem (see Schatzman and Strauss 1973).

As an inductive technique, theoretical sampling exemplifies the inductive logic of the grounded theory approach. Since grounded theorists systemati-

cally build their theoretical frameworks out of their observations, theoretical sampling is part of the progressive stages of analysis. It becomes necessary to use theoretical sampling when the analyst's present data do not exhaust the theoretical category the researcher is developing. At this point, then, more data are needed to fill out, saturate, and exhaust the category. Subsequently, the researcher samples whichever groups or events will provide the relevant material for the category. Comparison groups are chosen only for their theoretical relevance in theoretical sampling (see Glaser and Strauss 1967). Since I focus upon the chronically ill, I return to them when I use theoretical sampling. The theoretical category gains more scope, however, if the researcher chooses other comparative groups.

The need for theoretical sampling means that the conceptual categories that were inductively constructed have become sufficiently developed and abstract that the researcher can construct specific questions about them. Theoretical sampling then becomes a means for checking out hunches and raising specific questions. Furthermore, it provides a way to check the scope as well as the depth of a category.

CONCLUSION

The above explication of the grounded theory approach derives from the original methods that Glaser and Strauss (1967) and Glaser (1978) have delineated. Although I attempt to be faithful to the form and logic of their approach, over the years I have developed my own style of using grounded theory. Each researcher who adopts the approach likely develops his or her own variations of technique.

Basically, however, any researcher who claims to use the grounded theory approach endorses the following fundamental strategies. First, discovering and analyzing social and social psychological processes structures inquiry. Second, data collection and analysis phases of research proceed simultaneously. Third, analytic processes prompt discovery and theory development rather than verification of preexisting theories. Fourth, theoretical sampling refines, elaborates, and exhausts conceptual categories. And last, systematic application of grounded theory analytic methods progressively leads to more abstract analytic levels.

Although I have outlined how to do substantive analysis using a grounded theory approach, analysis need not remain at the substantive

level. By taking the analysis to higher levels of abstraction and conceptual integration, grounded theory methods provide the means to develop formal theories (Glaser and Strauss 1971; Strauss 1978). To do so, the grounded theorist takes the comparative methods further. After developing conceptual categories, he or she refines and reworks the emerging theory by comparing concept with concept. Developing formal theories necessitates sampling a variety of different situational contexts and groups in which the concept applies. That way the theorist analyzes the boundaries and applications of the developing theoretical framework. To date, however, the grounded theory approach has been used primarily to develop rich substantive analyses. A theoretical analysis at the substantive level, though more modest in scope and power than formal theory, gives the analyst tools for explaining his or her data as well as tools for making predictions.

REFERENCES

1. No doubt this reflects Glaser's rigorous quantitative methodological training at Columbia. To date, the language of grounded theory is largely the language Glaser adopted.

2. Qualitative coding is not the same as quantitative coding. The term itself provides a case in point in which the language may obscure meaning and method. Quantitative coding requires preconceived, logically deduced codes into which the data are placed. Qualitative coding, in contrast, means creating categories from interpretation of the data. Rather than relying on preconceived categories and standardized procedures, qualitative coding has its own distinctive structure, logic and purpose (see Glaser and Strauss 1967, Glaser 1978).

3. Glaser and Strauss (1967) imply that the data speak for themselves. They don't. Since researchers pose questions to the data, the codes they develop directly reflect the questions posed. Similarly, Glaser and Strauss often seem to take a partly objectivist view of the researcher's role. While they encourage researchers to build on their prior experience, they

Theory and Verification in Sociology (New Jersey: Bedminster Press, 1963).

14 Zetterberg, *Social Theory, op. cit.,* pp. 188-189.

15 *Ibid.,* p.139. This dictum is based on the idea: "The crucial act here is to deduce a solution to a problem from a set of theoretical principles." Theoretical principles refer to laws of formal theories.

16 At a lower level of generality, in much consulting done by sociologists to industrial firms, hospitals, social agencies, and the like, what is usually offered by the sociologists is "understanding," based upon an amalgam of facts intuitively rendered by references to formal theory and some loosely integrated substantive theory developed through contact with a given substantive area over the years. (Sometimes this is abetted, as in consumer research, by relatively primitive but useful analyses of data gathered for specific purposes of consultation.) Providing that the amalgam makes "sense" to the client and that he can see how to use it, then the consultation is worthwhile. Conversely, no matter how useful the sociologist may think his offering is, if the client cannot "see" it then he will not find the consultation very useful. See also Zetterberg, *op cit.,* Chapter 2.

17 Elements of "material culture" should not be neglected in development of substantive theory. Gouldner suggests they are the "forgotten man of social research": *op. cit.,* p. 97.

18 On the lay referral system see Eliot Freidson, *Patients' Views of Medical Practice* (New York: Russell Sage Foundation, 1961), Part Two.

19 *The Education of Sociologists in the United States* (New York: Russell Sage Foundation, 1963), p. 19.

20 See "Social Science and Social Control" in Joseph Ratner (ed.), *Intelligence in the Modern World, John Dewey's Philosophy* (New York: Modern Library, 1939), pp. 949-954.

GROUNDED THEORY METHODOLOGY AS A RESOURCE FOR DOING ETHNOMETHODO-LOGY

Stuart C. Haddan and Marilyn Lester

INTRODUCTION

This paper will explore the potential of grounded theory methodology for generating systematic ethnomethodological theory. Unlike previous attempts at linking ethnomethodology to other sociologies (cf. Denzin, 1970; Handel, 1974; Molotch and Lester, 1973, 1974; Sallach, 1973) which were done only at the programmatic level, the link we seek to effect is an empirically formulated one, arising from actually using grounded theory to study an ethnomethodological topic, i.e., the practices which people employ to "talk" their identities. There is a second distinction between our work and that of others who have sought to articulate ethnomethodological concerns with other sociologies. The antecedent attempts at linkages were between what most of us would take to be competing sociological perspectives, with differing assumptive bases and differing problematics, which result in divergent conceptions of social order, of interaction, etc., such that both ethnomethodology and the other sociology being addressed lose their recognizable characters. Our work leaves the ethnomethodological concern with members' fact-creating and sense-making practices (at several levels of analysis(in tack, and rather seeks to provide a methodology by which these concerns can be explored more systematically and in greater detail.

From our standpoint, ethnomethodologists have paid less than warranted attention to analytic procedures and methods of generating ethnomethodological theory, at least in their *published* works. If discussed at all, such usually consists of a very general statement. We suspect that at least in part this condition has arisen because an explication of those methods is seen to constitute just another "account" with all the properties of accounts so far elucidated in those same studies. In other terms, there is a hesitation to discuss analytic procedures due to ethnomethodological critiques of same. To that position we respond that, "yes, as members we are largely condemned to the same kind of interpretive work that we seek to analyze."

However, ignoring a discussion of methods does not eradicate the similarity

between ours and members interpretive work, but it does tend to mystify the "doing" of ethnomethodology for others. Within this spirit, we see a focus on methodology and formulating a systematic one at that, may help to advance the enterprise. In proposing and explicating a coherent, systematic methodology, ethnomethodological topics will be available for study without having to either intuit the analytic procedures, resort to logical elaboration of preconceived categories, or be situated in the California Sun. Hoosiers as well as students of the California groups will be able to conduct ethnomethodological inquiry.

This need to explicate analytic procedures, whether they be from grounded theory or other analytic modes is clearly demonstrated in a question which appeared on a Ph.D. preliminary examination in a major university. Ethnomethodology was seen as an important topic which students should know something about, but there were no ethnomethodologists to conduct training or to write an answerable question. The question went like this:

Because of his training in ethnomethodology, a researcher plans to employ the documentary method of interpretation for interpreting his observations. In the context of this problem (e.g., examining the societal impact of a norm-oriented social movement), just what is the documentary method? *In what ways is it superior or inferior to conventional modes of analysis using literal observation?*

We would hope that explicating whatever methods ethnomethodologists use may eradicate such inanity. In this regard, we do not advance grounded theory as *the only* useful methodology for exploring ethnomethodological-informed topics. We do not intend to take issue with phenomenological modes of discovery, nor with procedures developed by Garfinkel, Cicourel or the conversational analysts. However, we do see grounded theory as a useful and codifiable program of research for extending our knowledge of members interpretive practices. Further, effective theoretical integration of work where other analytic procedures are employed may be established with a grounded approach.

There is a further reason to consider such a program. At present, our studies often result in "rediscoveries" of the "discovered"(e.g., "here's the documentary method again, the accomplishment of social facts, etc., in a different substantive arena,") followed by several citations of previous studies. Rediscovery is in contradistinction to *discovering* other practices of members (perhaps less generic ones), to *elaborating* those (as well as previously discovered ones), to finding *conditions* under which a practice might take varying forms, to discovering the "work" that the practice might take varying forms, to discovering the "work" that the practice accomplishes (otherwise know as "consequences"), and perhaps most importantly, to *integrating* what often appears to others to be a set of isolated concepts or propositions. An analytic mode of the sort proposed by grounded theory will facilitate these latter endeavors.

In order to illustrate our position via grounded theory, the remainder of this paper will follow a somewhat circuitous route. First, we will briefly characterize the aims of grounded theory methodology as a theory-generating enterprise. Second, using our own study on "talking identities" as "data", we will show how the set of analytic procedures can generate ethnomethodological theory. Finally, we will return to a formal discussion of what we take to be the contribution of grounded theory methodology to ethnomethodology.

Time does not permit a full exposition of our analysis of identity production since our concern here is with the general link between grounded theory and ethnomethodology per se. Thus, we will only refer to that empirical work as it helps to illustrate the point we are trying to elucidate. For this reason, we have made the more complete analysis of identifying available before this session (1975).

A PRIMER IN GROUNDED THEORY

The Goals

The aim of grounded theory is the systematic generation of social theory accomplished through a sequence of analytic procedures. By "systematic generation of social theory" we mean several things. First, as a package, grounded theory constitutes a research program which takes one from the initial collection of data through analysis and write-up, although it provides for some variation at each step in the program. Second, at each stage of the research, theoretical concepts are developed in response to the data, but are never left as a set of isolated conceptual ideas. Indeed, no concept is left unlinked; *integration* of concepts is a primary concern in conducting grounded research. Ideally, what results is a multi-level conceptualization of a *basic social process*, (or core process), including the conditions under which the process occurs (or under which its form might vary), the properties of the process, and its

consequences for other interactional work. Third, and partly as a consequence of the above, cumulative development of theory is facilitated under the same program.

We have made reference to the notion of "basic social process." The idea connotes generic phenomena, analogous to Garfinkel and Sacks' conceptualization of "formal structures" (1970) and to ethnomethodological concerns more generally. Thus the interest of grounded theory is not in exhaustive description of a specific unit of analysis, but in the trans-unit character of the process.

The trans-unit property of analysis is derived from what Glaser and Strauss (1967) call the *constant comparative method* which entails several analytic procedures. Theory is seen as and every-developing entity, where data and conceptualization are constantly being compared so as not to fall prey to logically elaborating categories in a deductive manner with concomitant loss of linkages to available data. Instead of exhaustively collecting data in one substantive area, data is theoretically sampled. That is the analyst jointly collects, codes and analyzes data which informs what data to collect next, with the goal of refining and elaborating an emerging conceptual scheme. It is likely that several sets of data from different "areas" in the conventional sense will be sampled to varying degrees, depending on the stage of the research and the analytic level desired. This variation incumbent in theoretical sampling will be explicated in more detail when we discuss the procedures entailed in the research program.

As characterized thus far, it should be apparent that grounded theory departs from other analytic modes. For example, many empirical studies
1. attach an explanation and interpretation of findings at the end which are logically deduced rather than being generated by the data. Another similar
2 technique is referred to as "exampling" where the researcher finds
3 illustrations of theory that was formulated in an apriori way. Other studies serve only a verification rather than a theory-generating function, or
4. basically describe rather than conceptualize phenomena. On the other hand it is often suggested that analytic induction is synonymous with grounded theory. While it is true that analytic induction is concerned with generating many categories and hypotheses about a specific population or conceptual unit through comparative procedures, grounded theory differs substantially in at least two major respects. First, grounded theory analysis in *any particular study* has a process as its focus which moves beyond the fairly truncated concern of analytic induction with causality into the multi-variate

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realm of explicit exposition of conditions, consequences, dimensions, strategies, and the like. Hence there is a significant increase in breadth of purpose. Second, since analytic induction attempts to ascertain universality of propositions there is a required consideration of all available data (the search for the negative case) in a particular substantive concern. Grounded theory on the other hand, makes no claim to either exhaustive logical or descriptive coverage of any one substantive unit. Rather, the task through this latter research mode is to theoretically saturate with available data. Additional development elaboration, or refinement of the resultant theoretical scheme can be accomplished as more data (i.e., different conditions) are brought to bear by others in formalizing the theory. Since the theoretical scheme is "grounded" and not dependent on a singular substantive concern or type of data one would be hard pressed to "falsify" the model although it can certainly be modified.

DOING GROUNDED THEORY

In this second part of our adumbrated primer on grounded theory, we discursively outline the procedures for conducting a grounded analysis, using our own research on "talking identities" to illustrate the research process.[1]

A. *The Starting Point: Data Collection and Sociological Problem (maybe)* In the fall, Hadden, some graduate students and Lester formed a seminar in grounded theory. While there were two preliminary classes on grounded theory per se, Hadden wanted the class to "learn by doing," the usual technique for teaching the method. At the end of the first class, he asked if anyone had some data that the seminar could analyze. One student, interested in ethnomusicology, said she had been reading some interviews with jazz musicians that she found "interesting". In the beginning of the second class, transcripts of interview with several musicians were disseminated to wit. The first data collection phase was complete for us. (Clearly one possible variation at this stage is collection of primary rather than secondary data.)

This might appear to be a peculiar way to begin a study. We had not formulated a sociological problem, no hypotheses to test, no paradigmatic frameworks, no "check" (as yet) on validity and reliability of the data. Although one might begin with a general phenomenon of interest in grounded theory, the relevant core process is still not preconceived. Core processes and their properties are developed in response to the data. It is thus the opposite of logical deduction. Although Hadden wanted the data

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to be initially interesting in some way to some member, that was as far as preconceptions went. As to the data itself, one of the tenants of grounded theory is that there is no "bad data", although some will be easier to work with, yield different levels of analysis, etc. First, the notion of "bad data" is usually defined with respect to the problem at hand, hypotheses, etc., and since those are absent, the data will yield information on *some* process. Second, the constant comparative method takes no one data set as definitive with respect to process analysis, only as plausibly suggestive.

B. *Substantive Coding.* The next phase in the research process is called substantive coding. In a "non-training" grounded theory production, this phase is usually initiated after 4-8 interviews; or, where data already exists en tot, a "sample" of about 20-30 pages of data is drawn. This coding entails reading the materials very judiciously and noting "what is happening" or what is being said in each line. (Ideally, for this purpose, "data" appears on one side of the page leaving room for coding on the other.) In doing this, one attempts to use the same code when the same process or phenomenon recurs: and conversely, does not employ the same code when seemingly diverse things are happening. Codes, at this point, stay very close to the data--i.e., are fairly literal reductions of the substantive matter although rudimentary conceptualizations are also noted as they are suggested to the analyst.

It might seem at this point, that grounded theory's nose gets rubbed in the ethnomethodological analysis of coding and its critique of coding-as-literal-description (cf. Cicourel, 1964; Garfinkel, 1967; Leiter, 1969). We want to take up some of the most salient issues in this regard. In some sense, ethnomethodology and especially grounded theory are slightly recast.

Glaser and Strauss (1967), followed by their students, saw the possibility in coding "what was really happening on each line" --i.e., "happenings" were factual states of affairs, although independently, particular happenings are but one index of a derived concept. After lengthy discussions between the authors of this paper, (which are not resolved "once and for all"), a different conclusion has been reached, rooted in some ethnomethodological insights into the status of coding as well as the authors' experience with using grounded theory. Description, and thus coding, do not stand as mirrors to the world, to the data here. While substantive coding is seen by other grounded theorists to merely "reduce the data," from our standpoint, what the data "really is" and what is "most important" in each segment

of data is context-sensitive in the most global sense. "What is really happening", is dependent on typification of "what is really important" in the occasional coding of taken-for-granted ideas held by the analyst which informs as to what stands as important, and on previous coding. To some significant degree, what is "really important" to code is an ongoing assembling activity on the part of the analyst. So, for example, what might have been seen as previously unnoteworthy (i.e., not necessary to code), might in light of later analysis, be retrospectively viewed as "important after all," or the reverse.

Given these features, coders, especially those not schooled in the problematics of ethnomethodology, and who thus analyze the data from a normative standpoint, invariably "fill in" assumed but unstated meanings. They take for granted that they can access and share the intentional schemes of subjects in the latter's interpretive work; i.e., there is the assumption of common understanding and suspension of knowledge of the indexical character of any account such that the content of subjects' accounts become the codable features.

As we began substantive coding in the fall with Lester promoting an ethnomethodological analysis of same, there was understanding of the features of coding just noted, but disagreement and later recasting of our enterprise as well. For example, insofar as possible substantive coding is totally grounded in the data--we attempt to prevent logical elaboration of codes from the data; so even if different coders see different things as "important" to code, each is rooted in the data. In the present case, the first statement by a musician in the data, as nearly all the data, was substantively coded by each member of the seminar in seemingly the same way: e.g., "About his recent musical changes, Coryell said..." was coded as "change in music." But it is also true that this seeming inter-coder reliability was *only* on the face of it. That is, when we moved to the next phase, theoretical coding, what "change in music" "really meant" was transformed by different analysts who were interested in the data in different ways. Thus, substantive codes do not have a self-evident status but are particulars to be assembled into "good gestalts." The connections between the particulars are to be grounded in the data, but here is more than one possible ensemble of codes. For the two of us, for example, those substantive codes became processual accomplishments, e.g., "changes in music" became "identifying change," while other people took the "change" as a factual product because their interests were essentially substantive and normative.

Here is where we effect a transformation of sorts in grounded theory. For Glaser and Strauss, "what is happening in each line" is an observable, literally describable and self-evident phenomenon. So "change in music" would ordinarily be seen as a "fact in the world" apart from its production by people in verbal interaction. As soon as it became clear that we could employ grounded theory for ethnomethodological concerns, the interests in substantive coding switched to people's interpretive practices for producing what others analyze as self-evident facts. In this transformation our concerns directly dovetail with those of other ethnomethodologists. The interest was in the formal structure of peoples interpretive work, rather than the content per se, which in effect, eradicates some of the problems with "filling in" work, assuming common intentionality, etc.

There is another way to look at the problem of coding. Except for the systematic nature of substantive coding in grounded theory, we ask how our procedures at this stage differ from the way in which ethnomethodologists go about discovering that a stretch of talk exhibits "this" or "that" property of talk, of accounts, etc. That is, for example, to use a protocol as reflexively tied to the phenomena being talked about entails, in its formal structure, the self-same practices--i.e., coding. When ethnomethodologists document their theoretical point with an illustration coding of the illustration as an instance of the theoretical phenomena is taking place. In every sense, coding *is* employing the documentary method of interpretation; and since that is everyone's mode of sense-making, all ethnomethodological inquiries employ the practice too, whether analyzed as such or not.

Since we are all condemned to the documentary method, of which coding is just an instance, there is every reason to introduce *systematic* explicit coding into ethnomethodological research, unless one wanted to postulate that systematic coding has a fundamentally different structure than intuitive unexplained coding!!! As it occurs in grounded theory, explicit and systematic coding leads to discovery of previously unanalyzed processes, their interrelationships, conditions under which they appear and consequences for social interaction. In short, this second step sets the stage for well-integrated and densified social theory.

C. *Initial Theoretical Development.* Once the first data are substantively coded, interest shifts to a more theoretical level. This does not mean that the grounded character of the analysis is laid by the wayside, or that we "intuit" theory. Rather, we start to ask some theoretical questions about

the data and substantive codes, beginning with "what is *the* or *a* core process contained in the data" to which the most substantive codes appear related?

Actually, in the present project, a core process was discerned before the substantive coding on the first data was competed (a fairly common occurrence). Independent of each other, Hadden saw a configuration in the substantive codes which he termed "locating": i.e., the musicians were seeking to produce verbal documents of who they are and, also produced various assemblings of those documents into gestalt-like phenomena. Simultaneously, Lester had conceived a seemingly more generic practice--*identifying*--that on initial inspection seemed to incorporate Hadden's "locating" as one of its major components. Lester saw segments of data that dealt not only with documenting who the musicians "are" (locating", but who they *were, how* they got to be located as musicians with various characteristics, and who they aspired to be. At this point, it is important to note that instead of taking identifying as our emergent process and seeking its dimensions, we could have focused on just the locating practice. Indeed most of the substantive codes in *this* data dealt with pronouncements of who the subjects are, with much less emphasis on who they were and aspired to be-- what later became our retrospecting strategies, respectively--with some modification.

Just as we transformed the substantive codes into products of ongoing documentary work by the musicians, so some students in the seminar found an entirely different process, and substantive codes came to mean different things for them. This is to suggest that the same data will yield information on more than one core process, at more than one analytic level, and the choice of which to pursue is a personal one. Rather than be disturbed by this rather personalistic criteria, we feel it is merely an explication of what goes on everywhere. To our way of thinking, to insist or even intimate that only one researchable problem is evident in a set of data is one of the ultimate vulgarities thrust on the social world by sociologists of several ilks.

Theoretically, we had a core process and a whole host of substantive codes which we thought were related to identifying, to people's practices for "talking identities". But as yet, we did not have the codes linked at a theoretical level. There were ideas, such as locating in the present and retrospecting identities. We asked further questions about Identifying--what makes up the process? What are its properties? Under what

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conditions does it occur? What are its consequences? But it was premature to discern variation or the relationship among the several features of Identifying. Thus, the next phase of the process was begun while still analyzing and trying to integrate substantive codes from the initial data on musicians' Identifying practices.

D. *Theoretical Sampling.* In grounded theory, the interest is not in a specific population unit. One would not be interested in musicians' identifying per se, nor in exhaustively describing musicians. Rather a population or conceptual unit is analyzed only in so far as it elucidates features of the emergent core process of interest. But of course, the process will in turn reflect back on and provide analysis of the unit. So it was that we wanted to sample other kinds of data in order to refine, elaborate, and perhaps to reformulate the configuration of properties, strategies conditions and consequences relevant for the generic process of talking identities. We collected and substantively coded ethnographic data on institutionalized alcoholics. This data was selected because we wanted more unstructured data in contrast to the magazine interview and a seemingly contrasting population as well.

Each of us substantively coded this data and were to come up with, and bring to the seminar, an initial schematic of features of the Identifying process. This data was rich in people's practices for articulating who they *were,* or so it seemed, as opposed to creating and assembling features of who they *are,* as in the case of the musicians. As well, the alcoholics talked future identities. The Identifying process was now tentatively dimensionalized into three sub-strategies: Locating, Retrospecting and Prospecting. Further, each for these strategies were analyzed for *its* constituent features. For example, people locate themselves in social networks of others. Actually they continually constitute themselves as members of different networks. And they further assemble features of those networks (i.e., establish networks as such) which elaborates their initial locating-in-a-network. Their production of -in-order-to motives renders their initial locating as sensible, rational and competent--an important way to Identify. In retrospecting, as we saw it at the time, people isolate "influencers", "inciting events", etc., in their autobiographies.

We also began to discern some conditions under which one or another Identifying strategy is prominent. So, for example, Identifying can occur as an explicit oriented-to topic in conversation, or it can occur as a by-product or tangential feature to other interactional work. And Identifying will take

rather different forms under these conditions. As well we noted a general property of Identifying at this time--identities are a continual ongoing assembling process, where on one occasion for one purpose at hand, with one sequential organization to talk, one identity will be assembled; another in a different setting; and perhaps conjoint rendering on still other occasions. In a similar vein, social networks, significant others, etc., are also continually assembled and reassembled dependent on the nature of the Identifying occasion at hand (cf. Hadden and Lester, 1975), for a more complete analysis).

An integrated conceptualization was emerging, but the constant comparative method dictated yet further sampling; as the strategies for Identifying and their features were still considered very tentative. Some features of Identifying didn't fit and we suspected that we did not have all the relevant variation on the process and its constituent features. Hence, five more data sets were sampled. There was an ethnography of a speech colloquium, transcripts of interviews with mothers about children, with a waitress, and the like. This time only data possibly pertinent to Identifying was substantively coded.

These five data sets reinformed the conceptualization and added properties, conditions, consequences, etc.--i.e., densified the core process. So for example, we had initially conceived the three primary strategies for Identifying--Locating, Retrospecting and Prospecting--as defined along a temporal dimension. Locating was present Identifying, retrospecting was Identifying the past, where it was assumed that when retrospecting as past occurred, it would have been preceded by present locating. But in the new data, some of the subjects were Identifying a past, "e.g., I was a waitress...", but it had the same structure to it as locating *except* for the temporal dimension. And what we had called retrospecting before had "all along" an implicit *connotation* that it was tied to present locating by the actor. That is, the musicians and alcoholics "talked the past" as filling in work for the previous locating activity. The identifying work of the waitress was of a different sort than the retrospecting of the alcoholics: she didn't tie having-been-a-waitress to any account of present identity. As a result of this variation the locating code lost the temporal dimension as its defining characteristic. Locating can assemble both present and past identities and it is the *connection* that the identifier makes between temporal periods that differentiates locating and retrospecting. Thus, for example when an alcoholic assembles features of that located status and then proceeds to pronounce

biographical explanations for that location, both locating and retrospecting occur. But when locating is done in present and/or past, with no oriented-to tie between them, both are instances of locating.

In this reformulating stage, another important feature emerged. The retrospecting of the musicians differed significantly from that of the alcoholics. In essence, the musicians retrospected only by describing "how" the past helped them get to where they were; and in general they located much more than they retrospected. But the alcoholics were explicitly providing the "why"-- explaining their locating as their major identifying mode. The result was that retrospecting was dimensionalized into two forms--explicating which connotes describing work, and warranting, which provides explicit "because motives." Aside from just describing that variation, a condition was found to set the stage for engaging in one form of retrospecting or the other--assumption on the part of the identifier about what she and the hearer take as the valued or de-valued character of the location. When ego assumes that alter sees the locating as relatively valued, only explicating will occur. When there is the assumption of a devalued status warranting work takes place.

One last data set, received several months later, filled out this variation. There had been concern about the nature of the data--relative to everyday conversation, all our data was structured. This concern derived from Sacks, et al. (1974) and West and Zimmerman (1974, 1975) explicit focus on "naturally occurring talk" where they suggest that it has a different structure than "programmed interaction". We were fortunate to gain access to some transcripts or more "routine talk". For us, it was the final check on our conceptualization. Virtually all the same practices, properties, conditions appeared in this last data set. Of great interest was the fact that the data confirmed our differentiation of explicating and warranting under the retrospecting code. We had been worried that perhaps the respective interviewers of the musicians and alcoholics had *elicited* the differential pattern--that the difference was due to the nature of the data and not an actual trans-unit condition. But we found the same pattern in naturally occurring talk. For example, two women met for the first time and were engaging in Identifying work. One woman said she lived in a sorority house, which is generally devalued on the university campus. The second speaker directly affirmed that assumption and both located (i.e., assembled) negative features of the sorority/fraternity networks. This was followed by the resident of a sorority house retrospecting in the warranting mode--i.e.,

"there was no other kind of housing available" when she needed a place to live. Other conversations in this data set focused around Identifying, manifested explicating as the major retrospecting mode, where the located status was relatively valued. Not only were our conditions for undertaking the two forms of retrospecting verified, but there also emerged the idea that the valued-or-devalued character of the located identity can be *accomplished* in the course of talk, rather than being brought to the setting and assumed as an operating feature, as in the case of the alcoholics' assumptions about the ethnographer's evaluation of them.

Several months prior to obtaining this conversational data, the last step in the research process was begun: memo writing.

D. *Memo writing, "et cetera"*. As the core process and its attendant features are being integrated and densified, the memowriting stage ensues, coterminous with continuing other phases of the research (e.g., theoretical sampling, continual refinement of scheme, etc.). In general, memos are written about the core process, its constituent features and their linkages. So, for example, we write memos on Identifying; what it means, noted its strategies, conditions, and consequences. That one long memo proliferated further memos in two directions: first we moved up generically to talk about the relationship between Identifying, ethnomethodology and grounded theory; second, and most importantly, for the empirical analysis, memos were written on each strategy, sub-strategy, condition, property, etc., until we had effectively exhausted what we deemed to be relevant for analyzing Identifying. I having to explicate codes, their meaning, and linkages, one can discern weak points in the analysis which can send the researcher back to the data for refinement, then to new memos once again. In order to avoid logically elaborating theoretical codes at this stage, we continually extracted illustrations of the code being discussed which may also lead to further refinement, when, for example, the process of writing a memo and articulating it with the data leads one to see a new property of a theoretical code, which generates a new memo and so on. Ideally, memos, theoretical scheme and data continually reinform each other until the best fitting configuration is obtained.[2]

Memos are then sorted. All memos dealing with the same code or set of codes are placed together. Sorting enables one to insure that she/he is not using two different codes for the same process, and conversely that the

same code is not in fact referring to different processes. As well, sorting memos within and between theoretical codes elucidates points at which linkages between codes are absent. Sorting can thus send one back to the data, to substantive and theoretical codes for re-analysis, refinement or reformulation. In the end, memos on the core process are linked to those on conditions, properties strategies and consequences, which results in a relatively integrated conceptual scheme.

With elaborate memos, writing for publication can consist primarily of cutting and pasting xerox of memos or parts therein, with an introduction and conclusion *appended*. One will notice that we have so far made no reference to the "literature in the field." If one were to start with that work, the grounded character of the research would recede and logical deduction would assume prominence. It is after one's own analysis is complete that the literature review occurs, and then typically, as in our case, it was literally appended to the beginning of our analysis such that the analysis of Identifying looked like the typical ASR paper even though the latter is usually a deductive enterprise, where the literature actually suggested the study. Were journals more sensitive to this type of work, it would be perfectly reasonable to tie the research into the literature at the end of the paper, for that is what actually happens in grounded theory, where the literature is treated as supplementary data, and in inductive research generally. But as Hadden would say, "SO IT GOES!!"

ETHNOMETHODOLOGY AND GROUNDED THEORY: THE POTENTIAL LINK

We have so far characterized the aims of grounded theory and traced the steps in its research programs they were used to generate a conceptualization of identity production. This research is, in fact, the first time that grounded theory has been used to conduct ethnomethodological inquiry. At this point, we want to discuss, in a more explicit fashion, what we take to be the potential of grounded theory for ethnomethodology.

Fundamental to grounded theory is the interest in basic social process--i.e., generic process. While this has usually meant either social structural or social psychological process in the ordinary sense in which those terms are employed, it is also amenable to ethnomethodological problems or what we might refer to as basic interprective practices. As such grounded theory methodology dovetails directly with ethnomethodology's paradigmatic interest in generic or formal structure of practical

actions. Actually, we believe grounded theory not only can be employed on a par with other research methodologies, but can *enhance* the analysis of ethnomethodology's problematics by rectifying what we see as two problems in the present state of the art. Insofar as many published and unpublished works are concerned, there has been a dual and polar tendency in the past several years to: (1) proliferate programmatic statements, and (2) discard the early interest in discovery and analysis of members' generic interpretive work and rather has rediscovered those practices in different research settings or among different population units.

Programmatic statements have been employed to assemble and reassemble the boundary lines of ethnomethodological inquiry, to suggest theoretical handles or the theoretic attitude to be assumed in undertaking research, and to assemble the "real ethnomethodologists". Were these statements used merely as suggestive of problems of study, of modes of inquiry, there would be no problem. But instead, they have been reified (usually not by the author) for the purposes of criticism (cf. Attawell, 1974); Carpenter, 1975) of our problematic, and/or they are elaborated through logical deduction into hypotheses to be tested. We end up in a modus operandi that has been heavily critiqued by ethnomethodologists themselves, i.e., logical deduction. Were more of us to adopt a grounded approach, programmatic statements, if they occurred at all, would attain a different status. They would be grounded in research, and hence, not really constitute programmatic statements at all, but perhaps function to link at a conceptual level, studies of different ethnomethodological analysis.

On the other hand, a good portion of the ethnomethodological research has moved to an interest in specific population units (e.g. school teachers, newsworkers) or conceptual units (e.g., deviance) rather than exploiting these units for purpose of generating previously seen but unnoticed interpretive practices (e.g., theoretically sampling), or densifying, elaborating and integrating previously discovered ones. So, for example, once Garfinkel elucidated the documentary method of interpretation, characterized some of its properties and consequences for the social world, more than one of us went on to show that it operates in the newsroom (Lester, 1974), among elementary school teachers (Leiter, 1970), and among cops (cf. Sanders, 1975). Each study essentially concluded that "this group's major fact-creating and sense-making practice was the documentary method of interpretation," which had most

of the same features as when it was first articulated, except in one case it was used to create evidence of crimes, in another to generate new stories, etc. In so far as this becomes a major mode of inquiry, there will be continual movement from discovery to a "rediscovery" approach and to unit analysis.

In part, we suspect that it was the very success of the early work which lead to this focus on units per se as opposed to continuing to develop conceptions of formal structures. As properties of the documentary method and of accounts more generally were discovered, they were taken as exhaustive and became constituted as a paradigm for others to verify and rediscover in setting after setting, even though one of the major properties of these practices is that they are generic, e.g., transcends units. Ethnomethodological ideas became apriori assumptions about the data and were basically enumerated into testable hypotheses, though not usually related as such. This is not to say that there is anything intrinsically faulty about unit analysis. To be sure this approach is the standard fare in sociology, to wit, sociology of deviance, of organizations, etc. However, proliferating rediscoveries and unit analyses is not facilitative insofar as generic process is the desired goal.

Employing a grounded approach, one may, and an ethnomethodologist will, attend to the constitutive character of the data, to the "doing" as opposed to the product as such. However, preconceptions of what the data will yield ceases there. Because they are not serving primarily a verification function, substantive followed by theoretical coding and theoretical sampling of data can yield information on and lead to conceptualization of yet undiscovered or unstudied facets of interpretive work, or at least to elaboration, densification and integration of already-noted practices. Perhaps, as with the identifying process, these will be less generic than the global "reflexivity of accounts," and "the documentary method," etc., but nonetheless they constitute formal structures. In our study, these latter essential properties of accounts were of course also major properties of Identifying. Were we to have focused on the documentary method as the basic social process so our results might well have come to the same conclusion as other rediscoveries except for the variation in the conceptual unit of analysis. In employing grounded theory, we went on to capture the more specific process by which all identities are talked. We also suspect that we could have analyzed the documentary method via grounded theory and perhaps elaborated and

integrated its features beyond work already done.

Aside from being adaptable for generating conceptualization of variety of generic practices, we argue that employing a grounded analysis will result in a level of theoretical integration not so far achieved--i.e., will cement as well as analyze features of the paradigm, and will allow for cumulative development of ethnomethodological theory. This is perhaps particularly important at this point in time while ethnomethodologists are pursuing research in seemingly disparate directions, where there are differing problematic and thus differing features seemingly taken as resource. So, for example, to take the production of accounts-as-fact-creating-work as a problematic usually entails employing the social organization of talk as a resource, as pointed out by Carpenter (1975). Similarly, to take even higher level process, e.g., Identifying, status passage, or the social construction of social problems are typically seen to fall outside the domain of ethnomethodology altogether. Even though there is a fundamental concern with members' fact-creating and sense-making in these latter problems they are seen to take talk, accounts and their attendant properties as resource.

These variations can result in rather fruitless debates about the "real" ethnomethodology or about who constitute the "real" ethnomethodology or about who constitute the "real" ethnomethodologists. That is, conflict rather than complementarity, and integration of different level processes has generally characterized the "accounts" of our individual efforts. If carried to its logical conclusion, we will inevitably get lost in the regress problem. In so far as we take the distinction between topic and resource as the ultimate criterion for ethnomethodology, then when *any* social accomplishment of members is not rendered analytically in *any one study*, the study will be faulted. Taken to such extremes, ethnomethodology will come to constitute only the study of talk, until someone levels the critique that the conversational analysts are employing a fundamental accomplishment as their resource. Indeed, Mehan and Wood (1975) suggest just that, although they also suggest that the conversational analysts employ common sense knowledge as their resource.

Our conceptualization of the meaning and consequences of the differences amongst us takes a different turn. Such is rooted in taking our whole identifying scheme as data for analysis. That is, we asked questions about *that* process, analogous to those we asked about the original data. We wanted to understand the relationship of Identifying to

ethnomethodology, to its "good fit" with other ethnomethodological studies, etc. "What properties did Identifying share with analysis of other practical actions?"

With further data, Identifying self might well come to be seen as a variant of *Rendering* anything--other people, social organization and so on. That is, cursory inspection of our's and other's data and analyses suggest that locating, retrospecting and prospecting, so far seen as strategies for self production, are similarly members' practices for rendering other things as factual, patterned, sensible and the like, although the substrategies of those would vary. Thus, this process could be made even more generic. In so doing, Rendering would be analyzed for its properties, conditions, consequences and the like, i.e., would come to be an analyzed basic social process. Identifying people, would likely share all the features of the more general process of Rendering, but would have features in addition which are specific to "people production".

This is an instance of both vertical and horizontal integration. We could move up generically, through the method of constant comparison to analyze the whole Rendering process; and similarly move horizontally to take into account the variation encountered in Identifying people, Identifying self, Identifying objects removed in space and time, etc. In analyzing the most generic process, the characteristics of "rendering talk" would be analyzed, the forms that the documentary method takes in this work, and the reflexive character of "rendering accounts" with what is being rendered, etc. (We cannot at this time really suggest the properties of that process since our data was analyzed at a more substantive level.) However, in studies of the more substantive process--e.g., Identifying "self", "others", etc., the analysis would not rediscover the properties of the formal rendering process (unless new properties of this generic process were discovered in the course of this work), but would look at the form that these practices take--their properties, conditions and consequences. It is not the case that "talk" is being used as a mere resource, but that it has been analyzed in previous work, and here is the opportunity to build on that work and to dimensionalize the properties of the substantive Identifying practices. This work in turn would reflect back on and enrich the initial analysis of Rendering. One could as well work in the opposite direction and build from substantive to formal theory. In either case, Identifying self and others, etc., would be linked together, as well as to the overall Rendering process--the beginning of integration and cumulative

development of theory.

There are other possibilities, using the analysis of the more generic Rendering process. If we had analyzed that process, we could have moved in a different direction vertically. For example, Hadden's dissertation, *The Social Creation of Social Problems* (1973) can be seen to analyze, through grounded theory, the process of identifying social problems. That process included practices such as "alarming", that is seeking to produce the sense in which there is an urgent problem, which, in turn, consists of practices such as "pronouncing prevalence" of a problem, "providing indicators" of a problem and, more generally, doing production work to create the sense in which the problem has a stable, objective, transpersonal character. The analysis of the social creation of social problems can be seen to derive from what was previously unexplicated, and is now an explicit analysis of Identifying. This is to say that with an eye toward theoretical integration and cumulative development of theory, phenomena typically unanalyzed by ethnomethodologists will be made available for study and will be linked to other studies of ethnomethodological problems.

Much of the foregoing is at a relatively macroscopic level compared to many ethnomethodological works, but the potential for integration and cumulative development of theory does not start and stop at this level. Indeed, if it did so, our resource would be the more general phenomena of interpretive work. The general nature of interpretive work and its properties, etc.--e.g., the documentary method, the embedded character of accounts, the assembled nature of any phenomena--have and will undoubtedly continue to form problematic for research. But in so far as they are indeed the formal, i.e., transsituational structure of practical actions, they are also attendant properties of Identifying, of creating social problems, etc. By noting that foundation, less generic problems need not rediscover and reanalyze the formal structure of the more generic practices. That is, "Identifying" partakes of all the discovered features of accounts and practical actions in general and so our analysis focused about the more precise mechanisms by which identities are preferred. In analogous fashion, the work of ethnomethodologists concerned with members' practical actions in preferring anything could be seen to rest at least in part on the work of the conversational analysts, or at least analysis of the two topics would be mutually informative. While the latter are pursuing their problematic--the structure of conversational practice--those interested in higher level phenomena can

embed their work in the analysis of talk, and vice versa, without having to take the related problem up in detail, since it is not the topic of study per se. The analysis of accounts for example rests implicitly on features of talk that the conversational analysts have taken as topic, but have analyzable properties of its own. The latter should form the specific research problem, rather than having to undertake basically two studies, i.e., the social organization of talk and the production of accounts, nor apologize for using "talk" as a resource. Especially insofar as we want to disseminate our work to others, standard presentational practices and space limitations prevent the analysis of all the more general processes plus the specific problem under study.

We basically see ethnomethodological work varying in terms of the level of problem. Only to a minor degree, then, is there intrinsic conflict between analysis of conversational practice on the one hand and the social creation of social problems on the other. By virtue of the fact that these are different rather than competing levels, there is every reason to conceptualize the integrative potential of our enterprise. This will be effected by each of us explicitly attending at the level of analysis of our studies, nothing where they fit in the whole enterprise, rather than feeling obligated to include analysis of other levels, especially those higher on the continuum of generic verses substantive (or microscopic versus macroscopic) process. In short, with attention to these issues, we will be better able to pursue our individual problems of interest, at the same time seeing and articulating the integrated and cumulative character of our program as a whole.

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ENDNOTES

1 It is to be remembered that while grounded theory provides a research program, some variation can and does take place in the precise way in which each stage is undertaken.

2 It often happens as well, that in the process of writing one memo, an idea for another occurs. Following the grounded theory process, one will usually stop the first memo, start the second to get the idea on paper, then finish the first, etc. One can thus have several memos going at once which continues until all relevant codes have found their way into memo writing.