Special: An Interview with Kathy Charmaz:
On Constructing Grounded Theory

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Kathleen C. Charmaz completed her dissertation at the University of California at San Francisco under the supervision of the late Anselm Strauss, and is now Professor in the Department of Sociology at Sonoma State University. She served as the editor for *Symbolic Interaction* between 1999-2003, amidst her many other commitments to numerous academic journals and professional organizations. Dr. Charmaz's areas of expertise span the sociology of health and illness, death and dying, as well as qualitative methods, grounded theory, the study of academic writing, and a host of other topics, which she has often presented as guest lectures and keynote addresses the world over. Recently, Professor Charmaz has written extensively about grounded theory, challenging the objectivist leanings of the traditional model, and, by drawing on her roots in symbolic interactionism, extending the breadth and potential of grounded theory in a number of exciting ways. The culmination of this work can be seen in her recently published book *Constructing Grounded Theory: A Practical Guide Through Qualitative Analysis* (2006).

The following interview aims to explore Dr. Charmaz's most recent views on grounded theory, discuss her new book, and consider some of the debates and challenges to her theory over the years from people like Barney Glaser. The interview took place between Professor Charmaz and myself by phone on July 25, 2006. The interview was tape-recorded, transcribed and then edited to enhance the flow of the discussion, and to eliminate some extraneous material. The interview explores Dr. Charmaz's early intellectual development and engagement with grounded theory, considers the epistemology of social constructivism, tackles practical issues about doing grounded theory and, finally, presents her views on the recent fragmentation of symbolic interactionism and its future in the discipline of sociology.

Professor Charmaz is the author of *The Social Reality of Death* (1980), *Good Days, Bad Days: The Self in Chronic Illness and Time* (1991), and more recently *Constructing Grounded Theory: A Practical Guide Through Qualitative Analysis* (2006). Kathy also co-edited with Jaber Gubrium *Aging, the Self, and Community* (1992), with Glennys Howarth and Allen Kellehear *The Unknown Country: Death in Australia, Britain, and the U.S.A.* (1997), with Debora Paterniti *Health, Illness and Healing: Society, Social Context, and Self* (1999), and has forthcoming a volume co-edited with Antony Bryant called *Handbook of Grounded Theory* (2007). Dr. Charmaz has published numerous articles in journals such as *Symbolic Interaction, Qualitative Inquiry, Qualitative Health Research, Sociological Perspectives, Sociological Research and Methods, Journal of Contemporary Ethnography, Teaching Sociology, Sociology of Health and Illness, Social Problems, Studies in Symbolic Interaction, and Contemporary Sociology.* Professor Charmaz has also contributed numerous

chapters to various edited volumes in her field that span a range of theoretical, methodological, and substantive areas.

Tony Puddephatt: I recently had a chance to read your new (2006) book *Constructing Grounded Theory: A Practical Guide Through Qualitative Analysis*, and I very much enjoyed it. It was a good book for me to read, as I am going to begin a grounded theory project of sorts in the near future. It gave me a lot of new ideas, and I caught myself making errors in my past research endeavours as well. I found it very informative at a number of levels. I also found it very accessible to undergrads. The book is organized very well for a course, as it begins with a nice general introduction, proceeds into data collection techniques, different analytic coding strategies, and the importance of memos in theory construction. There is a very original discussion of theoretical sampling, and a very astute discussion of different ways of theorizing in the field. Finally, you have included practical sections on the craft of writing, and finally, the value of reflecting back on the work after much of it has been accomplished. This is not a dry cookbook styled approach, but rather represents your own unique take on grounded theory methods, and has a lot to offer professional researchers as well as those who are new to this evolving craft. I really did enjoy this book, and I would recommend it to anyone who is new to qualitative analysis, or who is looking for ideas on how to improve their use of grounded theory in practice.

Kathy Charmaz: I am glad that you found it useful. I work hard at making my work accessible. The book has a lot of new material on theoretical sampling, theory, and writing the results that are fairly original. It is a fairly straightforward read. I have made it fairly accessible for advanced undergraduates as well as more senior people. I have some original statements about how to construct arguments effectively, as well as delaying the literature review, rather than ignoring it, as a number of people do. My criteria for grounded theory is broader in this book. I think the most important criterion for a good grounded researcher is credibility. A number of grounded theory studies are too quick, in my view. So, striving for credibility in research is something the book really emphasizes.

TP: In the preface of *Constructing Grounded Theory* (Charmaz, 2006), you mention the influence of Barney Glaser and Anselm Strauss early on, as well as some of your mentors in your doctoral studies at University of California, San Francisco, such as Fred Davis, Virginia Olesen, and Leonard Schatzman. I wonder if you could talk a little bit about how these people may have influenced you in your early encounters with grounded theory?

KC: I went to that program in 1968, and Glaser and Strauss's (1967) book *Discovery of Grounded Theory* had hardly been out in print. I had read it before I had enrolled, but I was more interested in the kind of work they did – qualitative sociology. They of course had a long history of work in that area, so that was definitely something that fascinated me. I was also quite interested in Fred Davis' work as well, and Egon Bittner, an ethnomethodologist. He was very bright, and had done some interesting work as a younger person. He actually left the program before we all came, but he had been associated with it early on. So there were several people whose work I knew pretty well before going, but not grounded theory per se, more qualitative research.

TP: Was there a course on qualitative methods you took in graduate school?

KC: Yes, I had a course on qualitative methods that I took from Sherri Cavan at San Francisco State University in my masters program. One of the reasons I was interested in that program was the qualitative emphasis. So we had an introductory field methods course with Leonard Schatzman, and a course that discussed qualitative works and theoretical works with Anselm Strauss, in our first quarter. And then we had six quarters of grounded theory with Barney Glaser, and I did work separately with Anselm. I did one reading course with Virginia Olesen, on professional socialization. She was writing on nurses and the nursing profession, she is an expert in organizations and occupations. The program was very small at that time. We [the graduate students] were a very small group. We had a very heavy intensive qualitative and grounded theory emphasis. Most of our doctoral program was concerned with qualitative research in some form or another. Many people specialized in urban sociology. I specialized in medical sociology and aging. I think these were the three major areas of specialty, in addition to social psychology and qualitative methods.

TP: Were you always doing qualitative research, or did you shift gears at some point?

KC: I was always doing it. I would have become a medical anthropologist had I not gone on into a doctoral program that allowed qualitative research. I was primarily interested in those programs that had faculty that were interested in that. A lot of them were Erving Goffman's students. I was interested in University of California, Santa Barbara, University of California, Davis to some extent, University of British Columbia, and possibly the University of Minnesota.

TP: Was there a big crisis of legitimacy for doing qualitative research at the time? How marginal was the approach in the wider discipline?

KC: It had become marginal to the discipline, yet you had this circle of elites, like Erving Goffman, Anselm Strauss, Herbert Blumer, and to some extent some of these folks commanded considerable respect within their departments. I guess what I am really referring to is the Chicago School network. Those were the kinds of schools I was really interested in. I would say a place like University of Texas would have been mostly quantitative. Like, Chicago has become quite quantitative itself. And Berkeley as well, once Goffman left. Blumer was marginalized, it's kind of interesting. He created a department at Berkeley that was rated first in the nation for a number of years. But the people he hired then marginalized him.

TP: How did you come upon Chicago style work to begin with?

KC: Well, I took a course on deviant behaviour at San Francisco State University, which I just found fascinating. And you just ran into the Chicago School writings. All the stuff that was being done on qualitative methods was directly or indirectly coming from the Chicago School. That is of course until the ethnomethodologists got started. And of course Garfinkel's (1967) book, *Studies in Ethnomethodology*, really spurred that on.

TP: Which contemporary thinkers have shaped your thoughts on grounded theory more recently, in the last 5-10 years?

KC: Well certainly I do have that grounded theory emphasis that stems from Barney Glaser. I think I have the fluidity from Anselm Strauss, which is probably as much pragmatism and symbolic interactionism as it is grounded theory. I have been influenced by loads of people. Conversations with Susan Leigh Star and Adele Clarke, who were in a writing group I was in during the 1980s. My co-editor Antony Bryant is certainly one major source of influence. I think one major influence comes from outside grounded theory. You know, the symbolic interactionists, as well as other qualitative researchers who probably wouldn't want to claim a grounded theory identity as clearly as I do. Certainly all of these people have had their influence, and, as I mention in the preface of the book, early on, I was influenced by relativity theory from the physical theorists, cultural anthropologists who were discovering cultural relativity, as well as sociologists who were asking questions about the philosophy of science and the philosophy of social science.

TP: Did the research experience for your major study *Good Days, Bad Days* (Charmaz, 1991) influence your view of grounded theory?

KC: It did, definitely. It took me a very long time to write that. It was an extension of the study I did for my dissertation. I did 55 interviews for the dissertation, and Anselm and Barney were somewhat cavalier about data collection. Barney did not believe in using tape-recorders, Anselm was not that dead set against them, but…

TP: What did he not like about using tape-recorders?

KC: The same reasons he's got now. He thinks you learn to become a good note-taker, and that if you have a lot of detail, that just interrupts your thinking, and you can get side-tracked and derailed. But I can tell you that the interviews I did in the 1980s [after the dissertation] were just so much better. And of course I learned so much about interviewing. The materials that are available to a qualitative researcher now as opposed to the 1960s and 1970s are just enormously different. There is just so much good practical information and ideas that are available to a novice that just weren't there when I was a graduate student working on a PhD. So I was very aware that the quality of my data was so much better. I was also aware that there wasn't one single over-riding process necessarily. If you are familiar with Barney Glaser's early work, he argued that there is a basic social process in the setting or experience, or a basic social-psychological process. He really touted that. It can be a really useful way of organizing your material, and focusing on the things that are important in a setting, because I think that Glaser and Strauss were absolutely correct that a lot of people gathered a lot of data, but it was very general, and it didn't answer questions. At the time I was a graduate student, many people went into the field, like I did. I went and lived in an institution for six months and gathered a lot of data. But, it was thin, and I was in the field for far longer than Strauss would have recommended. His formula was one hour in the field, and three hours of writing notes. It was very different for me. I was in the field for eight hours and could spend a couple hours writing notes. Even as a young person, it was fairly strenuous. I have learned that there can be more than one social process. Barney Glaser, interestingly enough, has disavowed that notion that he had early on, that looking too hard for one thing forces the data into a process, which is exactly what other people who have tried it have said. It depends how you analyze things. There may be many processes that are operating within a setting.

TP: To try and reduce everything to one thing is forcing the data?

KC: Yes, you know, you may grab on to something and go with it. It may be great. But that is one of the things I learned in *Good Days, Bad Days* and it also kind of held me up, because I kept looking for the over-riding process. I did have one, which I never published. I called it "re-mobilizing" in my dissertation, but I don't like the term. Many people were trying to get themselves going, moving on, after a chronic illness, and I used that as one over-riding thesis, and wove in identity and time around those issues. I developed them in different ways in the book, which is also much more readable than my dissertation. I didn't like the word "re-mobilizing" for one thing, and I also saw so many more things going on. It wasn't a matter of re-mobilizing once at all, as it might be after having one heart attack. Rather, so many people I talked to had these intermittent illnesses that were quite devastating. There is a constant process of trying to get back to where you were, or where you felt you were able to go after a round of illness, and sometimes, each time would be a plateau down. And in some cases, after surgical procedures and medication, they would plateau up, which was really interesting. So people can go up or down over the years. You often have some of both.

TP: Interesting. Maybe we can move on more directly to issues surrounding constructivist grounded theory. What started some controversy is the chapter you wrote for the Denzin and Lincoln volume that juxtaposed constructivist with objectivist grounded theory (Charmaz, 2000). Basically, you position constructionist grounded theory as a useful middle ground between postmodernism and objectivism, in an effort to resolve the differences between the two camps, and offer a way forward. This position seems less strong in your more recent book on the topic. What is your position on grounded theory nowadays, and has it changed at all since the strong constructionist position you took on in the chapter you wrote in 2000?

KC: Well, I thought the distinction between objectivist and constructivist was a useful one, because a number of people wanted to use grounded theory, but they were very uncomfortable with the positivistic elements. The notion of discovering something in an external reality, the assumptions about truth, and a neutral, unbiased observer. I think one of the things that has happened over the last twenty years is the postmodernist movement has made some of the concerns that many of us had in the 1960s, and articulated them somewhat differently than we would, but nevertheless brought these concerns to the forefront. It is very hard to ignore the position of the observer, as well as the issues around truth and accuracy. There are always tensions there, because truth can be local, relative, historically based, situational, and contextual. I agree with that. Now, the objectivist grounded theory, and Barney Glaser still maintains this, aims toward generalizing, simplifying, parsimonious statements, and universalizing in abstract terms so that it cuts across fields. I think that sociologists do try to offer something that is general and useful and that cuts across fields, but I think we always have to be aware of the points from which we are speaking on the kinds of data we are analyzing. I think the postmodernist movement has contributed to that. Of course, Norm Denzin's version of postmodernism goes much further in the kind of research and the kind of writing that people engage in. I certainly advocate accessible writing, as I think we have more responsibility to more publics, rather than fewer. I think most sociologists have not worked terribly hard on that, and tend to be opaque, academic, and abstruse, and that it something I try and

minimize in my empirical writing. In some of my writing in theory, when you are writing to a much smaller audience, I do need to use some of the theoretical terms associated with particular arguments, but I do try to use them as clearly as possible. So basically, I thought the objectivist/constructivist grounded theory chapter was a way of saving a method that could have been thrown out totally because people saw it as dated. Yet some of the basic guidelines, the type of coding, the type of memo-writing, and trying to make your memos more analytic, more theoretical, and then checking through theoretical sampling still hold. Those are very useful techniques, and they can be used from a variety of theoretical frames, or types of data.

TP: How would you respond to Glaser's claim that as an objectivist, he is in fact aware that there could be researcher bias, and that this is important. Though, he argues in contrast to you that bias should be taken into account as a variable, and you should be able to analyze your own perspectives and feelings as just another piece of data that emerges, as a way to get at the underlying social conditions in the field (Glaser, 2002). What is your take on that argument, or effort?

KC: Well, first of all, I am not really interested in variable analysis. To me, that really smacks of positivism, and he is becoming more articulate about the fact that his version of grounded theory is a type of variable analysis. I would agree with him that the observer's standpoint is something to be taken into account. I don't think he does. But what I don't think he would agree with me about is that the very view you have as an observer shapes everything you see. And that means that values and facts are connected. Everything I have read of his assumes an authoritative, neutral, expert observer. Oddly enough, it also assumes you do not necessarily have to be "accurate." That sort of flies in the face of usual positivism, which aims towards accuracy, as he calls it "worrisome accuracy," and he accuses researchers of pursuing "worrisome accuracy," with the interview transcripts. He thinks you get bogged down with the details of it, and don't see the major processes and problems in the data. Whereas I think that the observer's standpoint is not an add-on, it is a way of seeing and I think you constantly have to be self-reflective about where you come from to have any conception of your own values, because the things that are most important to us are what we tend to take for granted.

TP: How would you respond to Glaser's (2002) claim that "Constructionism is used to legitimate forcing. It is like saying that if the researcher is going to be a part of constructing the data, then he or she might as well construct his own way."

KC: Well, I don't think so. I really disagree with that, because what you try to do is to understand as best you can, knowing that it always comes out of your own perspective, but you try to understand how the people that you are talking with or studying, construct the situation. I think grounded theory can be an enormous help with the checks to catch the kinds of constructions, and to have a sense of them. But it always comes from our perspective. Now I would argue that some of the things in the Glaserian model force the data. I think when you are not aware of your own starting points, and the situations that you come from, and the positions from which you stand, you tend to think that your view is the only view.

TP: So you can be forcing things implicitly, without being aware of it?

KC: Yes. I think one example of that is often people who are of Caucasian identity will say that they don't understand why people of colour are so concerned with identity, because they aren't concerned with identity. It is not a problem for them. And it is those things that are a problem for people, or that raise issues, that become a way in which they see nuanced interactions in a different way than the larger public. And I think that it is the poststructuralist turn that has made those kinds of concerns more apparent.

TP: Is there a way for a method to transcend the individual using the method? Or is standpoint epistemology an irreducible problem? Can we reduce the impact of the researcher's standpoint on the ensuing research and analysis through using the methods of grounded theory, such that the method is doing the investigating more so than the individual who carries in certain viewpoints?

KC: People often think that by using the method correctly, they are reducing the impact of the researcher, but I am not sure that is always true. Any study is situationally grounded in context and position, time, all of those sorts of things.

TP: Glaser (2002) gave the example of nurses collecting data on patients, and comparing their results in an effort to reduce each others' biases. They engage in a sort of team collective scrutiny, in a sort of Popperian fashion, in an effort to unmask the hidden subjectivities within the reports. Do you think strategies like this have any merit for the potential to reduce bias in studies?

KC: Well, if people are all seeing the same thing, I might say to them, well, you are all quite alike! (laughs).

TP: I also got the feel from reading the book, that in 2000 you had a much stronger constructionist position (Charmaz, 2000). But more recently, you are talking more in terms of pragmatism, analyzing action, making observations, considering nonhuman actors such as institutions and organizations, and focusing on things that go beyond mere human perspectives (e.g., Charmaz, 2005, 2006). I got more of a sense in the 2000 piece that you were not interested in trying to convey one true reality, but rather present a multitude of realities that exist within varying perspectives.

KC: Yes, I think I still do. I don't think I am that different from how I was in 2000. I try to draw fairly clear boundaries for the sake of analysis and comparison, and apparently that chapter has been quite useful to a number of people.

TP: Ok, how do you draw the boundaries?

KC: I made the boundaries rather firm between objectivist and constructivist grounded theory at that time so that people could discern some differences, and see the usefulness of grounded theory techniques for doing a different kind of research, and taking a somewhat different approach. I think it has been fairly effective in that sense because I think people are clearer now about where they place people, and where they place themselves. So for example there are a lot of positivist issues in Strauss and Corbin's (1998) *Basics of Qualitative Research*, and the third edition of that will be coming out. I have heard that Juliet has told people that her position doesn't differ that much from mine, and maybe it doesn't. But, the way that the book was written was quite positivist and objectivist in certain ways. It had all of that

technology to apply to the data, rather than emerging from what you are analyzing. It is a very different kind of conception. So basically I was trying to lay things out, and try to save grounded theory from being cast aside, because a lot of it had subsumed a lot of positivistic kinds of approaches.

TP: So you set the categories of constructivist and objectivist up as ideal types? Such that no study is totally constructivist or totally objectivist?

KC: Yes, yes.

TP: If it is true that all studies carry with them both constructivist elements as well as objectivist elements, do you see these studies adopting contradictory epistemologies that might be problematic?

KC: Not necessarily, because I have always believed since the 1960s that things are objective because we define them as such. And, a core of people, a consensus among scholars, or a consensus among publics, define them as real. The publics who have the power to make their definitions stick are the ones whose definitions become real in their consequences. These are the definitions that are acted upon and often become reified. And, we change what we define as real. I think the classic example of how constructions of how things have changed are male and female behaviour. Over the last forty years, what is considered to be gender has really changed. So, that is a construction that people took as objective and real, and people changed, such that the definition has become much more contested. However, those things do become more concrete. That is why we can treat certain things as objective when we are studying them, things like social class, for example. As slippery as it is, we can make some assessments.

TP: So it is agreed upon by the collective that social class exists, therefore it is real in its consequences. As such, you can study things as though they are real?

KC: Yes, certain things are just, certain things are unjust based on people's definitions. So, from a constructivist view you would want to learn, if you were in this area, about how those constructions are made, and whose viewpoints stick, and to what extent.

TP: Can't we do better than a consensus theory of truth? Certainly the pragmatists tried to do better than that, and measured truth by its practical effects through action (which is materialist and goes beyond just social consensus). I am thinking about the philosophy espoused by Dewey, Peirce and Mead. Do you believe in this as another necessary
requisite for truth, or do you stay at the level of a social consensus model? I guess another way to put this is, do you see a tension between pragmatist philosophy, which is quite materialist in nature, and the modern offshoot of social constructivism, which tends to be more idealist? And which of these should we prefer?

KC: I come closer to the pragmatists here because I endorse Marx's statement, "Men make their own history but they do not make it as they please; they do not make it under self-selected circumstances, but under circumstances existing already, given and transmitted from the past." This materialist view of action shreds the extreme individualism and solipsism given in radical constructivist views. As for scientific

truths by consensus, this point does not deny the significance of power in science -- in part gained by what works -- in part by power and entrenched views, as Thomas Kuhn (1962) suggests.

TP: Glaser would argue however that there are multiple viewpoints, but they are all parts of one over-arching social reality and pattern of relations. Glaser (2002) writes "GT is a theory about a conceptualized latent pattern – e.g., cultivating, credentializing, covering, client control, ritual loss ceremonies… etc etc." Later he writes that "Conceptual reality DOES EXIST. For example, client control is real, cautionary control is real. These processes and a host of others discovered in grounded theory impinge on us every day." Do you disagree with this perspective that Glaser lays out?

KC: I think it is an improvement on what he was saying some years ago. Now he is saying that there are latent patterns, rather than explicit patterns that should be analyzed. To me, that is an improvement. He was saying in 1992 that you just ask your participants what is going on and they will tell you (Glaser, 1992). Well, not necessarily. Now he realizes that. I don't think that all the patterns that he is talking about are always as important as he thinks they are. I think they are important in certain contexts but not in others. I think one has to be very careful about generalizing them, whether you are a constructivist or an objectivist. So, I think the latent patterns are an improvement, but then you have to ask for whom are they latent, for whom are they explicit, for whom do they have meaning, for whom do they not? He is aiming for generality without necessarily working on variability. He is aiming to simplify, without seeing the social world as it is enacted in a more complex way, in my view.

TP: My problem with constructivism is that proponents will argue that there is not necessarily any objective reality, and that it is relative to the viewer, and that all is an interpretation. However, it implies that if you interview somebody, it is possible to accurately present that person's view. Isn't this an objectivistic claim, to imply you are able to present the truth about a view?

KC: I don't think constructivists do that. I think we try to aim for understanding the view of the people as well as we can, knowing that we are limited. Whereas I don't think the objectivists allow for that ambiguity in knowledge. Essentially, objectivists view data and their own view as unproblematic.

TP: Could you not equally have an interpretation of a viewpoint, much like you might hold an interpretation of a social structure, or a latent social pattern, or set of conditions? Why is one kind of data privileged over another?

KC: I think the viewpoints are always interpreted, even in fairly mundane work, because of the things you choose to include, how you frame the data. You always give it a certain interpretation. For example, when you have large interview excerpts from a person. You can take it right down to what people hear, too. One observer may hear somewhat different things, and that occurs quite commonly when people take notes. Different observers will hear different things, and of course one of the hazards of interview transcription, particularly when they are not analyzed by the same person who did the interview, is you can see the statement in a different way than someone who heard it. What comes across in print might not necessarily be the

same as something that came across in speech, but you try to do the best you can. My view on accuracy is you try to be faithful, but you always realize it is coming from a position, and it is an interpretation.

TP: In a recent chapter in the 3rd Edition of the Denzin and Lincoln Handbook, you talk about using grounded theory for justice studies (Charmaz, 2005). You talk about making "invisible structures visible," as well as paying attention to systems, resources, hierarchies, policies as well as meanings and action, and so forth. Would a constructionist accuse you of reifying morality and social structures, in such a way that you are becoming the very sort of objectivist that you are warning against?

KC: It depends on how you study them, and what sorts of things you do. For example, I don't think you can just take "resources" as facts given to you by a report, or an official of an organization. You really have to analyze what stands as resources in whatever actions they are engaged in, and whatever they need to achieve. I know some ethnographers will look at that as well. What sort of activities is the person engaged in and what do they need to be engaged in, like, what does it take? How do they get what it takes, or compensate for it?

TP: What about the implication of latent social structures lying underneath human awareness and definition, that the actors operate around and have to negotiate, yet it exists outside of their consciousness?

KC: Yes, or it is taken for granted, and they don't articulate it. Well, I think any good sociologist is always looking for hidden assumptions. So, the notion of finding what is under the surface, what is tacit, what is liminal, and what certain positions tend to assume, is, I think, very congenial to a constructivist approach. I don't think that it is necessarily objectivist. To presuppose that there are certain structures would be objectivist. However, to take a look, and a rather deep look, and try to find things that are latent, would be quite consistent with social constructionism and grounded theory.

TP: On page 58 of your book *Constructing Grounded Theory* (Charmaz, 2006), you talk about discovering themes that a respondent makes explicit that were previously implicit. At this point, you state that now that the researcher "understands" what is going on now, they should then go back over their data again in an effort to re-read previous data in this new light. Is this sort of assumption about discovering implicit relationships or concepts in the data another slipping into objectivist assumptions, about the ability to discover an objective reality?

KC: Not necessarily. It can mean that you now define issues, views or actions that you had not defined before. This "understanding" arises from experience and how you conceptualize it and act toward it. The interaction that you have with research participants and subsequently with your codes and categories results in defining new leads to pursue empirically and analytically. As a result, you may go deeper into interpretive inquiry.

TP: I very much enjoyed the section in *Constructing Grounded Theory* that discusses the topic of theoretical sampling (Charmaz, 2006). I don't think that I have seen the topic discussed in that way before, that the researcher is sampling concepts, not people along some set of demographic categories.

KC: The argument has been made but no, I don't think the argument has been presented before in the way I render it.

TP: You say early on that the constructionist view assumes that you walk into the research with a host of assumptions and perspectives that cannot be exorcised fully. The researcher cannot pretend to be a blank slate, and that you should try to be aware of your own assumptions going in. However, on the other hand, you say what is more in line with traditional grounded theory such as that proffered by Glaser and Strauss (1967), in that you should leave the literature review until after most or all of the research has been completed. I wonder if as you are theoretically sampling, and you are seeing things emerge in the data, and if the data does not have to be contained in human beings per se, but rather concepts, then would it be a good idea to sample for theoretical concepts that might be parallel by going through the library?

KC: Well, I have always argued that the library can give you great sources of comparison, and materials to sample. And as I state at the beginning of the book, I think that the notion that we go in to our studies with a blank slate is impossible. You haven't been a very good student if you're totally a blank slate in your area. But you can go in with an open mind. I think the concept that Karen Henwood and her partner Nick Pidgeon offer of "theoretical agnosticism" is really useful, in that you try to look at things theoretically, knowing a range of theories, and think about which one might speak to the data that you have (Henwood and Pidgeon, 2003). So I am getting quite close to Barney Glaser's conception of theoretical codes here, but those can be forced upon the data as well. So, the notion of "theoretical agnosticism" is quite good, because you go in with that doubt, and you look for some kind of interesting category or finding, which is consistent with abduction, or abductive reasoning. Then you explore, and re-check how that category holds up. I think that is really interesting. So, yes, I think with theoretical sampling, you need to have some real categories before you sample. I just read something the other day, where the author argued that representational sampling is theoretical sampling. Well, no, it is not. That is just population sampling. For example, figuring that you would want more men than women in your initial sample for studying a topic that might differentially affect men. That is a good idea, but that is not theoretical sampling. Theoretical sampling is all about the concepts and the categories you are developing, and the testing that you do of them.

TP: So, say that after an initial foray in the field, you find that there is a theme surrounding status and identity. Is it acceptable at that point to go to the library and sample a bunch of literature on status and identity, in an effort to discover elements of that concept that might help to enrich your further investigation on the ground, for example, the sorts of things you look for? Could sampling concepts in the library on a topic help to increase the researcher's theoretical sensitivity?

KC: Well, I would say that as often as you can, you should go back to the field, and test out your notions, before you go to the library. I mean sometimes you are not going to find what you are looking for at the library anyway. I'll give you an example, when I was doing my research and I discovered that time really collapsed when people got much better. I was really embarrassed to go back and ask people about that experience, because it is so esoteric, and there is no language to consider how we think about time. We just don't have the language yet to articulate it for the most

part, particularly in the public language. So I went back and I talked to several people, when this one woman said to me "Yeah, like, it collapsed – like a wink!" She understood immediately what I was getting at, so it was really interesting. I don't think I would have found it in the literature. I did see that in Thomas Mann's (1973) novel, *The Magic Mountain*, which has a lot about time in it, but I hadn't seen it in a sociological or social-psychological literature on time at the point when I was doing the research.

TP: Well, if you had read G.H. Mead's (1932) theory of time, do you think that would have helped you to ask certain types of questions in the field, and look for certain kinds of information that would be helpful, and perhaps make it more theoretically rich?

KC: For people who can move in between theories and literature, and then back to the data with that slight scepticism, as well as interest and curiosity, then yes. However, there is always that issue of people trying to just apply it. I think that Glaser rather over-reacts to that, assuming that students will just apply theories within the literature to their own observations. But, I think it is possible to move back and forth. I don't think too many people have that theoretical playfulness, which is something to develop.

TP: I think that is a problem with lots of grounded-theory styled research projects. It can become overly descriptive, there is not a lot of theoretical material, it is often too obvious in its results, and there is the danger of people re-inventing the wheel without realizing it. What is your take on trying to get studies more theoretically informed and rich?

KC: Well that is really capturing my interest now. I think that throughout the discipline of sociology, we need to have more qualitative work be more theoretically informed, and have more theoretical implications. We need to move beyond description. Description is nice and it is fine, but I think we can do more than that.

TP: What would you say to someone like Joan Huber (1973), who would argue that any kind of sampling that is emergent is inherently biased by the whims of the researcher, who purposefully selects cases who are supporting the particular concept he or she is working with? I.e. the researcher, through emergent sampling, will simply find whatever he or she is looking for in the field, and ignore that which disconfirms the theory? Further than this, the lack of a systematic random sample means that the results cannot be trusted, as there is no way to assess generalizability to the greater population. Other empiricists might argue that the only real way to test a theory for sure is to see if it holds within a systematic sample that would allow for generalizable tests. Thus, with no sample that can be used for generalization, and no transparency of the research process that could be scrutinized by sceptics, grounded theory is not a scientific approach. What is your answer to these kinds of charges?

KC: I think it is a very traditional charge from quantitatively oriented scholars who critique qualitative research. I think critiques like this miss the strengths that grounded theory offers, and the kind of depth it can achieve. If you are able to administer some kind of transparent test that you can give to everyone, then it is probably going to be fairly descriptive and fairly limited, and fairly a-theoretical. That is not where my interests lie. The notion of emergence is, to me, one of the major

strengths of qualitative research in its entirety, and a definite strength in grounded theory, so I would really disagree with that. I think that people often apply a criterion that is useful for other types of research, onto qualitative research, for which it is not useful. There are other criteria that are much more important.

TP: John Goldthorpe (2000) similarly charges that qualitative researchers believe that they are immune from having to sample rigorously, but they are no more excused from the need to sample systematically than quantitative researchers. We use the notion of theoretical saturation as grounds for an adequate sample, but he would say that this is inadequate, and just because we are qualitative researchers, we are not necessarily off the hook on the issue of sampling systematically.

KC: Yes, I would definitely say that, rather strongly. I have said in many places that I think theoretical saturation is something that people proclaim. I think in the glossary I state that it is not that I support it necessarily, but I try to adhere as much to the working definitions that are around as I can. I mention this problem in my chapter on theoretical sampling, where I argue that this concept is often just rhetoric, used to legitimate a very weak sample.

TP: Goldthorpe would say that we should be adopting research strategies that span numerous researchers collecting data in numerous cities, using traditional statistical sampling methods in an effort to increase the generalizability of results.

KC: Well, as I have said, striving for a representative sample is often something that I argue against, unless it is necessary for the particular problem you are studying. I think it is a fine place to start, and certainly admirable. If you know that whatever phenomena you are studying mostly affects men, then it is great to have a sample with more men than women, and if need be, more older men if they happen to be more affected. But for me, this is a starting point, not an ending. The chapter on theoretical sampling (Charmaz, 2006: 96-122) is interesting, because I interview Jane Hood, who I have always thought is a person who has always understood it. I go back to an earlier substantive grounded theory study that she did. It is very hard to find people who actually explicate their methods, and that was difficult. Jane Hood did a very small study in numbers, but very much in depth. I mean she gathered multiple forms of data about these people over a period of 6-7 years. I think her logic is quite interesting. It bothered one of my feminist reviewers, because she thought studying the decision to go back to work for married women was dated and sexist, and that it doesn't take into account lesbian couples - which it doesn't – it just focuses on married couples. Jane is pretty clear about that. But I think you'll find that interesting. I think it is interesting.

TP: What about attempts to try and make the data more public, so that people can see the transcripts and such, in an effort to make the data and research process more transparent?

KC: You can't do that in the United States, because in order to do the research at all, you promise confidentiality and anonymity. Some of my people were very concerned about having their whole stories revealed. The narrative approach emphasizes the person's story, and valorizes that story. Well, not everyone wants to become that recognizable. Even when you try and hide identities, and try and protect people from being known, just a few characteristics can identify a person to someone who knows

who you are talking about. And of course, some people can make wrong identifications. I presented a paper on chronic illness one time and a colleague from Illinois raises his hand and asks "Were you talking about my cousin Glenda?" (laughs).[i] And I said, "No, I wasn't interviewing your cousin." He said that he had identified one of the people in my paper, and said that it sounded exactly like Glenda, but it wasn't. In the United States, you really can't make everything transparent. The memos, the interview transcriptions particularly, are too identifiable. I can't even justifiably use whole interview transcripts for teaching purposes, because of possible breach of anonymity.

TP: Symbolic interactionism seems to be taking a number of different directions lately, which may have implications for how certain people approach their research within the tradition in the future. For example, David Maines (2001) wants to move the tradition towards the mainstream, and make it more simpatico with other approaches. Meanwhile, you have Norman Denzin (1997), who wants to move the tradition closer to cultural studies and postmodernism. What is your take on these trends?

KC: Well, I think what you pointed out is that the fragmentation is there. We do not speak with one voice. There are different trends occurring in symbolic interaction, different collectivities of people who centre around a particular type of approach. I think that David Maines (2001) is quite correct that symbolic interactionism in so many ways has become part of the mainstream, but that people don't know it. I think it is very important that your generation carry pragmatism and symbolic interaction forward, on into the future. I think it is important for people who are trained by symbolic interactionists to really impart that tradition into their writings, and into their students. When you have the background, when you've studied with people, like in my case, it was Anselm Strauss, it makes a difference on how you view the world and the take that you have on the readings in your own field. It affects your interpretation. I would much prefer that symbolic interaction have a steady pragmatist foundation, with people who have worked in that tradition, rather than having some hotshot from Harvard, twenty years hence, rediscover it in his or her own way, without having that background, without having the insights that come from the exposure. I realize that for a number of students they have had to learn much about symbolic interaction on their own, in part because there haven't been people in their departments who have come out of the perspective very strongly. In the chapter I wrote in the 3rd edition of the qualitative handbook (Charmaz, 2005), I advocate going back to the pragmatist tradition, and building on that pragmatist foundation. In particular, the interpretive aspects, as well as the democratic aspects. I think that as poststructuralists take symbolic interaction more towards cultural studies, they might well lose some of that foundation. That is a concern.

TP: What is your take on Ulmer and Wilson's (2003) argument to make use of statistical survey analysis in order to test out interactionist concepts?

KC: I agree with David Maines on that issue, that much of symbolic interactionism's writing have always been quantitative. Those who are qualitative don't always see it, because those quantitative folks are often not part of the *Society for the Study of Symbolic Interaction*, and they tend to publish in other journals. But, they may well use the background. I think that Ulmer and Wilson (2003) are quite right that there is room for survey research in this area. The two might be able to complement each

other rather than compete, in certain projects. Certainly, the move towards mixed methods would make interpretive symbolic interactionism much more congenial with objectivist survey research symbolic interactionism, in the same project. It could be quite useful. So, I am certainly not opposed to it by any means.

TP: So, you don't mind integrationist approaches?

KC: No, not necessarily. I am not an expert in those areas, and I don't want to do them, but I see the possible usefulness of symbolic interactionism for questionnaires and survey research, definitely. We would have a lot to offer survey research with our emphasis on meaning, and wording, and those sorts of things.

TP: Is there anything else you would like to say?

KC: I see great possibilities for grounded theory in the future. I hope that these possibilities are realized in sociology and beyond.

TP: Thanks very much for your time.

KC: Thanks so much.

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Endnotes

i   This name is a pseudonym to protect the identity of the case example.

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