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Notes on the Nature and Development of General Theories

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There is a remarkable absence of discussion in sociology about the nature of (grounded) theories, how to generate them, and go about verifying them (witness the recent Handbook of Qualitative Research). Most theories are substantive. Differences among types of theories along various dimensions are discussed. A stand is taken against the usual oversimplified hierarchization of them by "levels," using an analysis of how theories appear in Science articles. For social scientists, this use constitutes models to be learned from, but not slavishly followed. Extended illustration is given from the author's research on the control of information, including guarding secrets, giving misleading information, engaging in suspicion searches, and the like. From this research, several innovative procedures are discussed relative to developing, checking, and linking general theories, both with each other and with substantive theories.

This article addresses the issue of (grounded) general theory in sociology, but especially that designed for interactionist interests and problems.¹ The discussion will range over differences among types and dimensions of theories, the formulation and use of general theories by natural scientists as something to be learned from but not slavishly followed, and an emphasis along with an extended illustration of an interactionist-style mode of formulation that uses the constant comparison mode typical of grounded theory methodology. Some comments are made about the illustrative material apposite to procedures relative to developing, checking, and linking general theories, both with each other and with substantive theories.

ELEPHANTS ARE DIFFERENT TO DIFFERENT PEOPLE

Wilson and Pilcer and Snack stood before the . . . elephant.

Wilson said, "What is its name? Is it from Asia or Africa? Who feeds it? Is it a he or a she? . . . If it dies how much will another cost?"

Pilcer didn't have any questions; he was murmuring . . . "It's a house by itself . . . the architect of those legs was a workman, by God."

Snack looked up and down and . . . said to himself, "He's a tough son-of-a-gun outside. . . . I'll bet he's as strong as a copper-riveted boiler inside."

They didn't put up any arguments. They didn't throw anything in each other's faces. Three men saw the elephant three ways. And let it go at that. They didn't spoil a sunny Sunday afternoon; "Sunday comes only once a week," they told each other.

Neither Carl Sandburg's moral—tolerant, pragmatic, midwestern—nor any postmodernist one is what I will stress here. Rather, I would emphasize that three perspectives resulted in three types of description of the same elephant. These descriptions are not theories, for they do not entail explicit explanations of *why* the elephant appeared as x or y or z. Although I hasten to admit that undoubtedly theories in social science mean different things to different people.

Given the title of tonight's talk, the question is What does Strauss think a theory is—or more specifically, what does he think he's going to tell us about general theories? Let the talk itself allow that to emerge. But at least initially I can declare myself, quoting with approval a blunt statement from a somewhat kindred spirit, Diane Vaughan (1992), "Positivism's recent fall from grace notwithstanding, I think theory development and theory testing are central to sociology" (p. 181). My talk, then, is about some features of so-called general theory, as someone of my pragmatist, Chicago-style interactionism sees those features.

I begin with a remark about the recently published *Handbook of Qualitative Research* (Denzin & Lincoln, 1994), destined to be much read and studied. It has some 600 pages, about 50 authors, and 36 articles. It may not fully represent all approaches to qualitative research, but surely—or, perhaps, alas—it must be somewhat representative. What are the images and functions of theory in the book?

A scrutiny of its index is a greatly disappointing experience. A handful of passing references to theory are made, many to very general notions like theoretical perspective and theoretical frameworks. Only two sections deal specifically with theory: one in the article on grounded theory and the other in one about the Nudist computer program, which has been somewhat influenced by grounded theory.

In one place, the editors refer to Van Maanen: "Theories are now to be read in narrative terms, as in 'Tales in the Field'" (p. 21). (Really, must we all believe this?) Nearby, they assert that "the search for general narratives will be replaced by more local, small-scale theories fitted to specific problems and specific situations" (p. 21). These sentences are in Denzin and Lincoln's introduction to the handbook; thereafter, one can see various authors writing about and sometimes struggling with the meanings of terms like *paradigm*, *epistemology*, and *interpretive frameworks*. But they do not deal directly with the nature of theories.

We can quickly grant that some authors do refer to actual work involving theories, and presumably most accept the usefulness of theories. Still others have rejected the very concept of theory, at least in the range of forms normally

accepted by scientists and even in the amended versions of “postpositivists.” (I put that last term in quotes—it’s a classification meant to include sociologists like me.)

Still, what does a grounded theorist make of this absence of writing here about the concept or nature of theory? In part this reflects a usual practice among qualitative researchers. We have a long tradition of describing how data (materials) are discovered, noted, and elicited but are little interested in *analytic* scrutiny of how these are interpreted. (Classically, this situation is captured by the experienced researcher’s pithy remark: “Make like me—but don’t ask me how!”)

But let us move on to the issue of theory. I think of theory as a type—among others—of explanation. Its characteristics include conceptualization—there can’t be theory without concepts. These are explicitly stated and worked with. They become linked and systematically so. They are in some sense testable—in what sense, let us leave for commentary later. And as in my long illustration later, they are grounded in a very complicated interplay between a discoverer, observer, or elicitor of materials, and whoever does those things when analyzing the materials.

There are also different kinds of theories, a subject that I will also soon address. But first, a few words about description. One can hardly describe anything without at least implicit theory—some degree of speculation or at least imagination or interpretation about what is being described, and perhaps explanation about its forms. There are at least implicit concepts embedded in the very language of any description.

As we know, description is one of the predominant modes of reporting research in social science. Description is necessary for creating, challenging, and supporting theories. This can be seen clearly in the practices of natural scientists. If, for instance, you scan the journal *Science*, you will see theory in a variety of disciplines being built, qualified, and elaborated through expected or accidental observations—that is, descriptions. But descriptions are not enough. Description is rendered more useful by systematic conversion into theoretical specification. Although description as such is certainly useful, it just does not by itself constitute explicit theory.

Now consider the distinction that’s customarily made between different “levels” of theory. Barney Glaser and I have made such a distinction, referring to substantive and formal theories (cf. Glaser & Strauss, 1967, 1970).² Other social scientists often do the same, or they distinguish between general theories and theories that are more local—as in the sentence quoted earlier from Denzin and Lincoln. But I want to broaden these simple, sometimes useful, but ultimately much too simple kinds of distinction. I suggest the matter is less one of levels than of the dimensional location of a given theory and an understanding of what a specifying of location implies for inquiry.

Here is a list, probably not exhaustive, of such dimensions. Think of them in terms of “degrees of.”

abstraction (generality of conception)
 scope (number of substantive areas studied)
 range (extent of relevance—i.e., types of groups, organizations, and societies)
 specificity (of detail of grounding; of theoretical sampling expectations)
 conceptual complexity (development and linkage of concepts—density; linkage
 with [an]other theory or theories)
 applicability (relevance—i.e., extent and range—to aspects of “the real world”)

With these dimensions in mind, one can at least crudely locate any given theory. Much grand sociological theory is very abstract, broad in scope, variable in range, and fairly weak in both aspects of specificity (certainly in their grounding). Whereas ethnographies generally are referred to as substantive, these are very variable in terms of some dimensions. Also there are analytic inductive theories, such as those about opiate addiction (Lindesmith, 1947), embezzlement (Cressey, 1953), and socialization into marijuana smoking (Becker, 1953). These are about particular phenomena and allegedly operative wherever the phenomenon is found. Yet conceptual complexity, both of conceptual and theoretical linkages, is minimal; scope is limited, and perhaps, as in Lindesmith's bold and plausible theory, it's virtually impossible to test the theory in a nonauthoritarian society.

To know and specify location is important if only to know what a theory actually pertains to and what might be its strengths and limitations. We need to know location also if we are ever to link theories together more systematically, for, if you believe in some kind of science, that is an additional benefit from having theories. The alternatives are the impossible dreams of developing one grand theory or settling for local—and sometimes very local—theories. To link theories, we may not need to locate them precisely, but doing this lends clarity and probably efficacy to such endeavors.

As a background for my discussion of general theory, consider the question: *What* does theorizing do for natural science, and *how*? But why look at science and scientists for cues about our own enterprise? A personal anecdote may be illuminating here. On my bookshelves there is an old copy of Cohen and Nagel's (1934) once famous *An Introduction to Logic and Scientific Method*, from my college days. The logic was never more than a fun game for me, but the section on scientific method fired my fantasies of myself as a scientist. Nagel's main message was the mutual interplay between theory and data. Recent studies in the sociology of science reveal that the constructing of data and theories, and so their interplay, is a very complex activity. Relevant to this activity are various social structures, social worlds, and interaction. But, rather than review here the writings of sociologists (like my former students, Clarke, Fujimura, Star, and Gerson, and others like LaTour and Griesemer), I will make a few observations related directly to my talk. You can check out these observations if you have not already made them.

If you look closely at the language and procedures used and reported in various articles published by the journal called *Science*, you should see some of the following.

- There are a variety of terms referring to theory or theoretical-related items: theory, theoretical models, models, theoretical frameworks, paradigms. It's not always clear to an outsider why one is used rather than another. Are they used somewhat interchangeably?
- Theories are not all of the same generality. They differ along the dimensions mentioned earlier: abstraction, scope, and so on.
- One or another of these theories are used, and sometimes constructed, in conjunction with the situational specifics of the tasks at hand. (As Clarke & Fujimura, 1992, say, "things, attributes and elements are *in the situation*" [p. 17; emphasis in original.]) This certainly includes theories and theoretical-related items and elements. These are situation bound and instrumental.
- The origins of theories also are diverse, for they may be borrowed from other disciplines and from other areas of research within the same discipline, or constructed *in situ*.
- Their uses are also varied, depending on how they are constructed or incorporated into the ongoing research. Various they are used as probable explanations or to support negative findings, generate relevant calculations, and so on. A theory of considerable abstractness and scope may be developed or adopted to give overall coherence to a lot of hypotheses, facts, and partial theories.
- In other words, different theoretical elements or items are used and not necessarily with the same kinds of data—that is, different in type, importance, amount, or certainty.
- Theory construction and verification also use diverse kinds of technologies and procedures whose sources may lie inside or outside of the area of research or of the discipline itself. (The sociology of science tells us that the assumptions in borrowed technologies and procedures may be disregarded or not known by the borrower—"blackboxed"—who uses them anyhow if they seem useful for the problems to be immediately solved [cf. Clarke & Fujimura, 1992, for several references].)
- In the theorizing, a fair amount of imagination is reflected and even unabashed speculation. Not anything goes, because these operate within the constraints of *some* current knowledge; yet rigor seems reserved for verification—that is, for the creating, eliciting, and discovering of evidence. I also include "discovering" because, of course, some data may be found unexpectedly. If its relevance to some theory is recognized, sooner or later (later, as with penicillin), then these data become evidence.

My comments, you may notice, have touched on data as well as theories. Each of these reciprocal components of research inquiry seem not to be homogeneous in nature, and they certainly are related to each other in no single way.

So the \$64 question for us is this: For our own research, are all of those cues irrelevant or can something be learned from them? Learned for what? Both

for our frequently overly simplistic views of natural science and perhaps simplistic rejection of the possibilities of a humanistically grounded social science. Criticism by some social scientists of positivism never did, and still does not, rest on understanding this complexity of natural science models. On the other hand, interactionists like me must join their social science aspirations with humanitarian considerations. But that's a topic for quite another talk!

Next, consider the nature of general theories. These seem much different than our sociological grand theories, however useful the latter may be, in that those lack some of the crucial dimensions noted earlier. By contrast, if a natural science theory embodies to a considerable degree all or most of those dimensions, then I would think it a rather general theory. For instance, there is an astrophysical phenomenon termed *opacity* (Rogers & Iglesias, 1994). Theory about opacity not only has all the features of theory just itemized, but it includes interlocked subtheories in combination with mixtures of data, and its multiplicity of procedures reflects high scores on each of the dimensions. I am struck also by an accompanying extensive social organization that offers immense resources—financial, organizational, and theoretical, as well as experimental and technological. So the cumulative development of theory is often very rapid. Perhaps we cannot in social science do nearly as well—or perhaps too you think such an attempt is irrelevant to our work. I would agree with the first reaction. I just do not think the second represents more than an extreme antisocial position—an excessively exclusive embracing of the humanities, vital as they are to our understanding of social life.

Now in the remaining minutes I am more interested in showing how a “grounded theorist” like me might go about developing a general theory that includes, to a fair extent, several of the dimensions. In other words, the focus will be on procedures and on the process of developing a theory, rather than on the finished product. (It is far from finished anyhow.)

First, I want to sketch several logical possibilities for developing a general theory. There are probably more, but I need to keep this presentation relatively simple. (Hearing these may be a little like hearing the music of Webern for the first time—evanescent, hard to remember; so don't labor over the list of items—just get the general idea.)

1. Begin with one or more researched substantive areas and then elaborate directly to a general theory. But do this without developing any corresponding substantive theories.
2. Begin with a substantive theory, then speculate about its wider general applications. Also, if possible, further develop your thoughts systematically into a general theory or general framework. (The speculative mode is still a popular one.)
3. Begin with a general theory (grounded or ungrounded in research), then develop grounded substantive theory of one or more substantive areas. (Diane Vaughan, 1992, calls this “theory elaboration.”)

4. Begin with a grounded general theory and then elaborate it by linking with another grounded general theory. Presumably one could do the same (but I think far less satisfactorily) by linking two ungrounded theories.
5. Begin with a grounded substantive theory and then develop it into a general theory by looking comparatively at multiple substantive areas. The data can be drawn from both technical and nontechnical literature. The theory of awareness context developed in *Awareness of Dying* (Glaser & Strauss, 1965) can be elaborated by looking at materials on con men, spies, government officials, court testimonies, and so on.
6. The same as just above, but also look at other substantive grounded theories that speak directly to your phenomena (like the relevance for a general theory of awareness context of Fred Davis's, 1961, article on disabled people's handling sociability interaction when with strangers).
7. The same as above, but elaborate this general theory still further by also relating to it another grounded general theory (like Goffman's, 1963, theoretical treatment of stigma concealment).
8. And a final spurious form, where no research whatever is involved: Begin with a grounded or ungrounded theory—apply it to one or more substantive areas for better understanding (and teaching about understanding) of the area or areas. (We do this in teaching, but also in our reading the newspaper or thinking about events in our own and others' lives.)

I turn finally to my own elaboration of the original substantive theory of awareness context, as developed by Barney Glaser and me (1965) in our book about people dying in hospitals. This example is meant to illustrate especially the fifth, sixth, and seventh modes sketched above. These are substantive to general theory via comparative analysis of multiple substantive areas, and linking with related substantive theories and with grounded general theories.

Consider first several events. A confidence man misleads a mark, then vanishes with his money. A young man tells his parents that he now feels secure enough to reveal to them that he is gay. Parents withhold from their son that he is an adopted child, but when he is a teenager he begins to suspect his status and confronts them. All those events reflect some sort of secret: kept, revealed, and lied about. Such information may also be discovered, whether suspected beforehand or stumbled upon.

Our sociological journals are full of descriptions and substantively focused analyses of such phenomena. (I will elaborate that point in a moment.) In fact, it would be impossible to imagine interaction—whether of persons, groups, or organizations—without such control of information. In other words, such phenomena are universal across the human species; even a society of saints would fall apart if everyone knew exactly what everyone else was thinking about, let alone thinking specifically about every feature of each other, and all of the time!

How now to develop a general theory about the universal and no doubt complex modes of controlling others' knowledge of information? Procedurally, it is absolutely necessary to abandon adherence solely to substantive

theory—that is, theory about information control in specified worlds and organizations—whether among confidence men or within gay circles, among spies, between intimates, or in government agencies adept at concealing or giving out misleading information. To develop a more general theory, we must seek and use data from a large variety of such areas. Each source is likely to add potentially useful concepts, conceptual properties, and conceptual relationships. Besides such conceptual specificity, such theory gets broadly extended in scope—that is, over many substantive areas and in range—over many types of groups and organizations. Also, it promises greater applicability in the “real world” of practical action.

Some of the relevant materials written about information control in the sociological journals are suggested by the titles of articles. Here are a few of them: “Undercover Deception,” “Making It by Faking It—Tarnished Goods and Services in the Marketplace,” “Undercover Drug Users’ Evasion Tactics,” “Learning Real Feelings: High Steel Ironworkers’ Reactions to Fear and Danger,” and “Going into the Closet with Science: Information Control Among Animal Experimenters.”

What one quickly observes about these articles is that aside from their substantive descriptions, their authors may develop or borrow bits of theory (say, from Goffman). These are meant to explain their descriptive materials about nondisclosing, suspecting, pretending, and so on. Metaphorically, we might picture these researchers as not so much like Sandburg’s three observers looking with different perspectives at an elephant, but rather like three blind men feeling different parts of the elephant and not recognizing someone else’s elephant as being related to their own.

Nevertheless, we can add to our procedural armamentarium by sometimes incorporating items from these substantive articles: their concepts, conceptual properties, and even their theoretical statements. Raw data from these studies, in the form of statements by interviewees or informants, can also be used. So can quotations taken from sociological, as well as other social science and historical, studies that are focused on quite other phenomena. To give a couple of instances, I have found invaluable (and fun to read, but that’s irrelevant) various books like Klapp’s (1964) *Symbolic Leaders*, Murray Davis’s (1973) study of intimacy, and Goffman’s (1963) *Stigma*.

We can legitimately use also—not for evidence, but for theoretical stimulation—the anecdotes and events in biographies, autobiographies, memoirs, novels, plays, and movies. For instance, many events in Richard Wright’s (1945) autobiographical *Native Son* can be useful for our purposes. So are those in Stevenson’s (1983) book about British spying operations.

For the next point, I return to my statement about using theoretical statements pertaining to nondisclosure in sociological studies. Although sometimes merely stimulating, occasionally they can be developed further by elaboration techniques.

In an early article, titled "Discovering New Theory from Previous Theory" (Strauss, 1970/1988), I did an extensive elaboration of Fred Davis's (1961) empirically grounded theory of deviancy disavowal. But it's possible to do another set of elaborations. This is because his theory embodies a sequence of contexts (conditions) that involves strategic control of information by a disabled person when in interaction with a normal person.

In that article, I elaborated Davis's theory of deviancy disavowal in many different directions, but not in terms of its implications for a theory of control of information with regard to its misrepresentation, nondisclosure, secrets, and so forth. Yet the interactional stages and associated strategies in Davis's explicit theory depend on a sequence of changes in what Barney Glaser and I (1965) earlier called "awareness contexts." However, in the deviancy disavowal article, although those contexts are descriptively clear, Davis gives no analysis of those awareness contextual components as such.

Now, only to illustrate how one goes about elaborating such a grounded theory as Davis's, consider the following. His theory is about how persons with visible disabilities go about controlling this potentially damaging information to themselves when in a brief initial encounter with a nondisabled stranger. Theoretical elaboration of such a theory takes every component and begins to hypothesize—speculate, if you prefer that term—about each if it were to be altered. Simultaneously, you raise questions stimulated by those imagined alterations.

Here are some examples. You say to yourself that Davis's nondisabled, "normal" person is apparently inexperienced in handling a disabled person. Then what would happen interactionally if this person were quite experienced—had a spouse who was disabled, or, to add another condition, if this person were a physical therapist? Or reverse the roles and make the disabled person an inexperienced, newly disabled one, while the other was a physical therapist. Or change the disabled feature so that it is invisible or is either self-defined as tremendously negative or only mildly so. What then would the interactions look like?

Or think of the issue of attention: say, if the disabled feature were really not noticed by the other because it was so minor or because the other's attention was lessened because of undue attention to himself? Assume he was intent on telling a story about something amusing or terrible that had just happened to himself? Or change the intention of the disabled person so that now he or she wishes to call attention to the feature, defining it as a positive attribute rather than a potentially negative one—indeed, making a proud statement of it?

One can, in imagination, also change conditions like the structural context within which the interaction occurs. Davis deliberately confined his study to interaction in sociable settings, but we could ask about what might happen to first meetings of strangers within work settings. We can play games also

so as to make the interaction perhaps more complex. Suppose the other actually bears an invisible negative bodily sign, a stigmata, and so is concerned with accidentally revealing this, while at the same time the visibly disabled person is concerned with interactionally overriding his or her own? Also, what is implicit in Davis's study is that the latter might actually suspect that the normal person is pretending not to notice the visible disability but is somewhat embarrassed at having noticed it. This is a different interactional situation than if mutual pretense were going on. Also, it is different than if the disabled person freely admitted to a disability but signaled this unimportant matter should be ignored. Of course, one can continue with this comparative analytic game by altering conditions like the numbers and kinds of other people within hearing distance or who actually participated in the interaction, and so on.

In short—note that this is an important sentence—elaborating an extant theory involves multiple and ultimately systematic comparisons of various conditional dimensions as we look then for various associated interactions, processes, strategies, and consequences. The advantage of doing this with theories, as over against mere quotations and other data, is that we are not just generating concepts, properties, and conceptual linkages, we are building on and incorporating extant grounded theories. These are theories like Davis's, or like Goffman's theory of stigma, Klapp's theoretical statements about celebrities and their images, or Murray Davis's work on intimacy. Thus their theories get linked with yours—and vice versa—although the latter endeavor is not your real concern or, at best, is secondary to it. The crucial point is that your own theory gets more complex, tied in with previous theorizing, and becomes increasingly cumulative.

How far does one go in this kind of imaginative hypothesizing—some might say purely logical—elaboration? When developing a substantive theory, too broad a set of comparisons done in this way tends to take researchers away from the data. But for developing a general theory, I would hazard there is far less danger of the comparisons being irrelevant. These imaginative comparisons lead via theoretical sampling to a broad scope of data that support or qualify or negate each speculation. Or you stumble on the data and now recognize their relevance. We stop when time, personal interest, and other resources run out. Then the baton passes to others to qualify, fill in the holes, and generally test a general theory's applicability to particular substantive areas. A humanistically oriented sociologist can also, for enjoyment or scholarly reasons, apply the theory to dramas and movies, seeing in terms of such a theory how the plot lines evolve and get spelled out.

This talk originally ended right here (finally!), except for its summary. But an additional issue should be mentioned, lest you think me blind to it. A general theory whose advocates claim it pertains to a set of universal phenomena is likely to be accused of ignoring culture, history, and societal

variation. Nothing of the sort! Such variation is precisely what one seeks to build into such a grounded general theory. This is what constant comparison and theoretical sampling are intended to do. Yet there's certainly a limit to the variation that a theory's originators can manage to build in; indeed, the intent is to have it guide further research into a variety of additional substantive areas and cultural and organizational settings. A generation or more of researchers can and should seek to qualify, modify, and elaborate the theory. Additionally, a general theory, one would hope, should stimulate development of many substantive theories and, simultaneously, help to link them. Or it should be integrated in a more embracing one.

When I was almost finished writing this talk, Adele Clarke asked me, "What kind of general theory are you going to advocate, what kind do we want?" In answer, I more or less summarized as follows. A general theory should have considerable scope, range, and conceptual complexity, and that means also that it will be fairly abstract and have a fair degree of specificity (and so encompass much variability). It necessarily gathers up and helps to integrate what previously have been discrete theories, and elements of theories, that bear on the phenomena you are focused upon. So this leads to increasing accumulation of this general theory and to the plausible knowledge it embodies. (As interactionists, we assume too that our theories, unlike those of some other people, deal centrally with social processes. I too have assumed this, so I have not particularly emphasized this point.)

I would hope too that it would be applicable to aspects of the world that we might wish to change—which it should because of its careful if always incomplete grounding. I believe the world is an enormously complex place and that general theory is, at best, an imperfectly plausible instrument for comprehending all that otherwise terrifying complexity. Yet as analysts it is our grave but also joyous responsibility to attempt this ordering, and a general theory is one of the best instruments for achieving this.

I rest my case.

NOTES

1. Except for its first paragraph, this article is identical with an invited address given at the Gregory Stone Symposium, SSSI, University of Illinois at Urbana on May 4, 1994.

2. Long after this paper was written, I discovered the recently published conference papers, *Formal Theory in Sociology* (Hage 1994). Authored by sociologists devoted, although with diverse views, to developing "formal theory" and to combating most sociologists' seeming indifference to that enterprise, there's a striking absence of any reference to symbolism and only a bare nod to interactionism via Blumer's overall position on theorizing. Jack Gibb's position (Hage 1994, pp. 90-104) is the most challenging in that he thinks of "natural" or common sense language as used in "discursive" modes of theorizing as the central block to sociologists ever reaching the promised land

of effective formal theory. (He even criticizes James Coleman's work that "bristles with equations" but is vitiated by his assumption that use of natural language is quite acceptable.) My article assumes, without then knowing Gibb's radical position, that he is dead wrong.

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