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**Conditions for Progressive Research Programs in
'Qualitative' Sociology**

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Abstract

I assess certain ‘qualitative’ approaches in sociology as *technologies* of research in order to determine what ‘technical’ aspects (rather than ‘social’ conditions) are necessary for engendering progressive research programs. Although hugely influential and thriving, symbolic interactionism, Goffmanian analysis and ethnomethodology have failed to become high consensus and rapid progress research programs. On the other hand, conversation analysis has been quite successful, but not without tradeoffs. They have focused on the task of explication at the cost of foreclosing other levels of meanings sociologists are interested in.

Introduction

The natural sciences are characterized as “high-consensus, rapid discovery” intellectual endeavors while the social sciences are plagued by controversies and a slow moving research front (Collins 1994). There are unsettled debates about whether sociology is in a preparadigmatic state that will eventually transition into a high consensus ‘normal science’ or whether it is condemned to be irrevocably polymorphous and multiparadigmatic (see Bell 1979; Ritzer 1980). Debating sociology’s paradigmatic status *globally*—whether one thinks sociology is currently undergoing preparadigmatic birth pangs, stuck in multiparadigmatic anarchy, or enjoying a form of healthy pluralism—glosses over the internal heterogeneity of its distinctive approaches and communities. Certain perspectives in sociology crystallize into productive empirical research programs that accumulate findings and advances theory. Others are embroiled in metatheoretical controversies (even internally, amongst its own practitioners) such that it is unclear *how* empirical findings are cumulative, or *when* theory is advanced.

It is not the lack of quantification that is the problem. Variables-based analyses and mathematicization are neither necessary nor sufficient conditions for the creation of cumulative research programs. But if even such highly formalized ‘quantitative’ sociologies have trouble becoming high consensus, rapid progress research programs, there seems to be even less hope for ‘qualitative’ sociologies engaged in fuzzier tasks like interpreting the meanings of social actions. Interpretative sociologies capture some form of meaning, and this type of analytic task is argued to require a categorically different logic of inquiry from causal analysis.¹ Interpretation seems

¹ See Weber (1968); Geertz (1973); Ricoeur (1979: 87)

impervious to systematization and lacks objective criteria of assessment. There are attempts to formalize criteria of interpretive adequacy², but there is little clarity or consensus about them. The interpretative task is therefore a significant fault line that continues to demarcate much of the social sciences from the natural sciences.

Despite such odds, one such ‘qualitative’ approach—conversation analysis (CA)—appears to have succeeded in creating a high consensus, rapid progress research program. CA has been proclaimed to be “the most sustained and coherent research program that has developed out of ethnomethodology” (Lynch, 1993). It has been hailed as a rare example of a research program in sociology that is closer to ‘normal science’ (Law & Lodge 1984: 283). Other ‘qualitative’ approaches such as symbolic interactionism, and even ethnomethodology from which conversation analysis was derived, have lower consensus and slower moving research fronts.

In this paper, I consider the following ‘qualitative’ approaches in sociology—symbolic interactionism (SI), Goffman, ethnomethodology (EM) and conversation analysis (CA)—to unearth the logic of inquiry and technical qualities of these approaches so as to better understand why CA has become a high consensus, rapid discovery research program while the others have not. Examining these approaches will provide some indications to help answer the more general question: What technical conditions are necessary for constructing high consensus and rapid discovery research programs? If many of these qualitative or interpretive approaches are not progressive research programs, is it possible and desirable to transform them into ones that are?

The Components of Research Programs

Collins (1994) argues that the social sciences are not and will not become high-consensus, rapid discovery intellectual enterprises. This is not due to the lack of 1) empiricism; 2) quantitative precision and mathematicization; or 3) the experimental method; all of which the social sciences have. Instead, the reason is that the social sciences lack what he calls a ‘genealogy of research technologies’, whose manipulation can reliably produce new phenomena and engender a rapid moving research front. This technology is “a machinery that produces some new observable phenomenon (thus giving material for theorizing), plus the capacity that by physically tinkering with the machinery, modifying or recombining its parts or applying it to new uses, it produces still further phenomena for study. It is a *tinkerable research technology* that gives rise to a stream of discoveries” (Collins 1994: 171 emphasis my own).

² See Weber (1968) and Geertz (1973) for example.

We typically think that research ‘technology’ includes only the methodologies that excavate data and the theoretical apparatus used to analyze them. These are merely the most visible components of the technology as they are the most explicitly formulated. The actual technology used includes a variety of other components: metatheoretical foundations, orienting sensibilities, working strategies, methodological directives, sensitizing concepts, analytic tricks and heuristics; together with the set of unit theories and concrete methods that we are more consciously aware of. Analysts may be more or less aware of such components, and vary in the explicitness with which they discuss these dimensions of their research technology. Nonetheless such tacit knowledge plays a crucial role in scientific research (see Polanyi 1962; Schon 1983). The actual ‘research technology’ therefore consists of the *whole* research program, which includes the array of tacit orienting strategies and sensitivities that are just as crucial as the specific unit theories or concrete methods that are made explicit. Having theories and methods itself is not enough to do research, just as state of the art machinery in a workshop will not be enough for us to build things if we lack the tacit understandings of how to use these tools skillfully, and for the right purposes.

We should therefore consider research programs in the social sciences to be made up of at least 4 main components³: 1) metatheoretical foundations; 2) the logic of inquiry; 3) an integrated auxiliary belt of unit theories⁴; and 4) exemplary studies. At the ‘hard core’ of research programs are metatheoretical foundations and this can be made up of fundamental ontological and epistemological assumptions, as well as metamethodological commitments.⁵ Such foundations, assumption and commitments are empirically untestable and are impervious to simple refutation. Surrounding the hard core is the auxiliary belt of empirically testable and conditionalizable unit theories that can improve in scope and precision.⁶ Productive research programs have a set of such interrelated unit theories and empirical testing can lead to cumulative findings. Lakatos’ distinction between the hard core and the auxiliary belt has allowed us to

³ This delineation takes off from the work of Imre Lakatos (1970), but has been tailored for the discussion of sociological approaches.

⁴ I take the term ‘unit theory’ from Wagner & Berger (1985) in their application of Lakatos’s (1970) criteria (of progressiveness) to assess theoretical growth in sociology.

⁵ An example of a metamethodological commitments are methodological individualism, methodological collectivism and methodological situationism. The interactionist perspectives operate according to what Knorr Cetina (1981) calls ‘methodological situationism’

⁶ See Walker and Cohen (1985) on conditionalization—the process of specifying scope conditions as a means of theory advancement. Basically, it involves specifying the conditions under which the theory works and conditions under which it does not.

identify key differences in the research technology we use, but he does not specify how we translate such metatheoretical foundations into specific and empirically researchable theories. In between the metatheoretical foundation and the specific testable set of theories is the logic of inquiry that transforms abstract metatheoretical commitments into an empirically researchable project. What is included here can range from less explicit orienting *sensibilities* to clear methodological *directives*. We can think of this level of activities as *working strategies* that may include analytic tricks, heuristics, sensitizing concepts and a repertoire of mechanisms that can help in research. Finally, research programs also typically include exemplary studies that contribute to specific empirical findings (adds to unit theory) or demonstrate the analytic payoffs of the methodological sensibilities of the program (reinforce the working strategy). Such exemplary studies serve as models and reference points for researchers and critics alike.

Some examples of relatively well formed research programs in sociology include the new institutionalism in organizational sociology (Powell & DiMaggio 1991), the production of culture perspective (Peterson & Anand 2004), expectation states processes in social psychology (Wagner & Berger 1985), and conversation analysis (Heritage & Atkinson 1984).

Evaluating the ‘Technical’ Properties of Research Programs

Instead of taking research programs as a technical-evaluative reference point, some sociologists have used research programs as a descriptive-analytic concept to study intellectual change. Tiryakian (1979, 1986) translated Lakatosian research programs into a social *unit of analysis*, to allow students of intellectual change to examine processes that occur at the ‘school’ level instead of taking individual figures or following major ideas (see also collection of essays in Monk 1986). He regards such identifiable schools as consisting of an actual, socio-historical community of professional scholars, brought together by a charismatic intellectual leader (Tiryakian 1986: 418). This unit of analysis is then used to analyze the intellectual developments of different disciplines (See Monk 1986). For example, Tiryakian (1986) examines the history of sociology as a succession of different hegemonic schools.

Even though the ‘social’ properties of research programs are relevant for examining scientific and intellectual change (cultural and institutional context, charismatic leaders, organizational bases, career trajectories, reward structures, communicative channels, invisible colleges and networks etc), my attention will be focused on the ‘technical’ properties of these programs. The task here is not intellectual history or sociological explanation of scientific or disciplinary developments, but a rational assessment of the progressiveness of research programs.

In other words, the goal is an evaluation of the practical usefulness of the technology that we make use of for research.

It is tempting to make use of uni-dimensional metrics such as ‘falsifiability’ with which to pass judgment on the scientific status of theories (see Popper 1963: 33-39). Unfortunately, such uni-dimensional metrics are inadequate criteria *by themselves* because they may encourage practices we do not want to allow into the orbit of science while discriminating against practices that we *do* want to admit. While psychoanalysis and Marxism are unfalsifiable by Popperian standards, these traditions are still regarded as useful technologies of research, despite their weaknesses. It is therefore too hasty to discard such traditions merely because they cannot be falsified. To correct for such hastiness, Lakatos (1970) suggests a ‘sophisticated falsificationism’ that prescribes the appraisal of a *series* of theories and theoretical *adjustments* instead of judging single theories. Research programs are therefore judged holistically instead of according to one simple and universal criterion. According to Lakatos (1970: 119), research programs are progressive if they generate novel findings, especially those that are undreamt of, or would have been contradictory to previous or rival programs. Such progressive programs make use of anomalies or negative cases productively to test theories or specify the scope conditions of those theories.⁷ On the other hand, programs are degenerative if theories are fabricated in order to accommodate known facts (Lakatos 1970).

It is the whole apparatus of the research program—including all its interrelated components—that constitute the technology sociologists use for research. Like other forms of technology, it is not enough to merely have the tools; we need to know the right task to use them for; and also how to use them skillfully. Therefore, to create social scientific knowledge requires not just the *tools* (methods, unit theories) but also the *skills* involved in using them (working strategies, analytic tricks, heuristics). We also learn such skills from others who are doing the same type of work (exemplary studies). The social scientific technology of research is therefore a holistic enterprise that includes concrete tools, explicit skills and tacit sensibilities. This research technology is ‘tinkerable’ because its various components can be adjusted. Sometimes we work on shaping the tools; sometimes we hone our skills and strategies. At times we make sure that we use the right tools for the right task; but at other times we improvise those tools for performing tasks that it was never intended for. Whatever adjustments we make to the technology have to be assessed in terms of whether it helps achieve our research goals. Through this process, we acquire a holistic and practical sensibility about research similar to a craftsman’s sense of his practice.

⁷ See Emigh (1997) on the use of negative cases to extend and test theory in sociological research

With the different dimensions of research programs specified, and a discussion of the complex but desirable task of evaluating the technical properties of research programs, we can now turn to an assessment of the various approaches or perspectives in ‘qualitative’ sociology.

Symbolic interactionism

Fine (1993) claims that in the early decades of its development, the central themes of SI were lucid, identifiable, and the perspective hung together because of the writings of Herbert Blumer, who proclaimed that:

“Symbolic interactionism rests in the last analysis on three simple premises. The first premise is that human beings act toward things on the basis of the meanings that the things have for them...The second premise is that the meaning of such things is derived from, or arises out of, the social interaction that one has with one’s fellows. The third premise is that these meanings are handled in, and modified through, an interpretive process used by the person in dealing with the things he encounters” (Blumer 1969: 2).

Because students of SI trained in a variety of approaches and only a few received ‘pure’ interactionist training, the core beliefs of interactionism became muddled (Fine 1993: 63-4). With the death of Blumer, SI became “intellectually promiscuous” and fractured (Fine 1993: 64). There is disagreement on how many and what the different schools of symbolic interactionism are. Contributors to the *Handbook of Symbolic Interactionism* themselves agree that “detailing the major schools or varieties of symbolic interactionism is no easy matter. Disagreement abounds” (Herman-Kinney & Reynolds 2003: 85). Some have identified the following strands of symbolic interactionism: structural, phenomenological, semiotic, behaviorist, postmodern, Simmelian, dramaturgical, Marxist, Weberian and feminist (Fine 1990: 120-1). To this proliferation of types, Maines (2001: xiv) protests that “it is not at all clear what these designations mean in and of themselves, but I am certain that they are nowhere close to being “schools” of thought that are somehow distinguishable from one another and that represent divergent paradigmatic approaches” (Maines 2001: xiv). Reviews of SI typically acknowledge the fragmentation.⁸

SI lost its coherence precisely because of its success. The central ideas of SI have become so widely accepted and incorporated that it has lost its distinctiveness. Atkinson and Housley (2003: 144) argue that “we are all interactionists now” but fail to recognize this fact. Interactionist

⁸ When reviewers lay out the domains of symbolic interactionists research, they tend to sound like items in a laundry list. For example, Fine (1993) details 6 domains of research: 1) social coordination theory; 2) emotion work and experience; 3) social constructionism; 4) the creation of selves 5) macrointeractionism; 6) policy relevant interactionism. This seems to be merely a list because there is no larger coherent classificatory logic: some elements are included because of the scale of analysis (macrointeractionism); others because they have policy relevance; some seem to be united by a substantive domain (emotion, self); another seems to be a general perspective (constructivism).

insights are now so taken for granted that many are ‘unaware interactionists’ who use those ideas without declaring or making them explicit (Maines 2001: 16-25).⁹

1 Orienting principles of SI has not translated into generally relevant auxiliary belt of unit theories

The proclamations made by Blumer and others are really orienting principles or sensibilities, and understood as such by interactionists themselves. Maines (2002: xiii) explains that these are not theories, but a kind of “characterization or a series of declarations about human beings and what they do” (Maines 2001: xiii). SI is not a high consensus or rapidly cumulative program because these orienting strategies and principles have *not* been translated to an auxiliary belt of interconnected unit theories that are generally relevant for other practitioners to consider and test. Even though there is a proliferation of SI inspired empirical studies, they do not build an interconnected belt of unit-theories. Interactionists typically study particular social worlds—art worlds, scientific social worlds, particular communities, or deviant subcultures (See Becker 1982; Clarke & Gerson 1990). These studies help us understand the rich but localized social logic and meanings of activities undertaken in those worlds. We learn about relevant types of actors and activities, the career structures of new incumbents, the conventions of that social world, and how its meanings are constructed collectively as a joint activity of various relevant actors. These findings are certainly ‘theoretical’ but they are specific unit theories limited to that particular social world. While such findings are of interest to other interactionists, they are not incisively or decisively crucial for others. What interactionists learn about musicians can sensitize them to look out for conventions, reward structures, locally meaningful practices when examining doctors, but there is no integrated set of unit theories with general import for all interactionists. An experienced interactionist will therefore acquire very fine-tuned sensibilities and skills, but not have the benefit of a set of generally relevant unit theories that cumulatively increase in scope and precision.

Members are therefore held together by sharing orienting strategies at the ‘hard core’ but not by a common auxiliary belt. Their skills may become more honed over time, but the technology closer to the empirical data fails to develop into generally relevant findings. An

⁹ For example, one might argue that Abbott’s (1988) study of the professional groups engaged in jurisdictional competition draws from symbolic interactionist ideas (albeit one that is historical and structural). His study examines the negotiation between different occupational groups, in order to explicate how those actions collectively define what the division of expert labor is in the American occupational structure. Abbott is unlikely to be an unaware interactionist, but merely takes such interactionist insight for granted. When researchers utilize these principles without explicitly stating them, they lose their ‘programmatic’ status.

interactionist may collect a set of mechanisms and identify some broad patterns that could be applied to different social worlds, but that is as far as it goes because the findings are still tied to the substantive context of its investigations—the particular social world under consideration.

2 Debates about strategies instead of testing unit theories

Interactionism has been criticized for being ahistorical, lacking in appreciation of larger social structures, and ignoring the workings of power (see Reynolds 1990: 135-157). In response, SI has attempted to correct for these criticisms by attempting to incorporate social structure (e.g. Maines 1977), and its proponents argue that a kind of ‘macrointeractionism’ is possible (e.g. Fine 1993). Such debates between SI and their critics operate at the level of orienting strategies and are rather unproductive because it degenerates into vague disputes about *relative emphasis*. One analytic trick that SI performs is to examine the *proximate* social situation whereby specific negotiations and strategic actions lead to the collectively defined outcome. Critics argue that this overemphasizes the capacity of actors to determine their situations and that they should really be examining the *distal* forces of broader structures and macro-contexts. Such debates are carried out as if it is possible to find some general answer to the question of whether the relative emphasis on the proximate social situation or on distal effects is ‘better’. The typical ‘solution’ to such debates is an allusion to some ‘healthy balance’ between both positions, but what constitutes the healthy balance is often left vague. When the object in question is about such working *strategies* or *sensibilities*, it is more productive to specify the conditions under which some strategy will be more suited for achieving some specified task. There can be no simple *and* general statements to be made about such analytic skills.¹⁰

3 Adjustments that undermine the coherence of the research program

i) First adjustment-attempting ‘synthesis’ by identifying shared traits

It is not just the lack of an integrated auxiliary belt or the disagreements about orienting strategies that poses problems; the ways in which adjustments to the program have been attempted may also defeat the possibility of a coherent research program. Practitioners tend to resort to identifying ‘shared characteristics’ as a strategy of integrating the different strands of SI. Mostly, this lowest common denominator approach creates empty observations that lack analytical

¹⁰ This is similar to debating the merits and limitations of all manners of skills, for example, parenting. It seems unproductive to assert one parenting style as globally better than another, or even to assert the vague allusion to a ‘healthy balance’ between extremes. Instead, we might try to specify the conditions under which a more authoritarian parenting style is more productive (and in what ways) and under what other conditions a more liberal parenting style is productive (and in what ways).

purpose and real content. For example, Meltzer, Petras and Reynolds (1975: 54-5) propose that the 4 varieties of SI (Chicago, Iowa, dramaturgical genre and ethnomethodology) “share the substantive view that human beings construct their realities in a process of interaction with other human beings” and also that all four accept “to some degree, the methodological necessity of ‘getting inside’ the reality of the actor in an effort to understand this reality as the actor does.” Other such characterizations list symbolic interactionism as sharing a concern for ‘interpretation, interaction and intersubjectivity’ (Prus, 1996). The similarities identified are devoid of content and create a semblance of shared attributes only because they are vague enough to encompass the different strands of interactionism. This adjustment is no more enlightening than saying that the hammer, screwdriver and saw are similar because they are all tools we grip with our hands.

ii) Second adjustment-adding other analytic tools to the program

Adjustments that attempt to incorporate all forms of components, analytic tools, repertoires of mechanisms etc may be detrimental because they may dilute the logic of inquiry. The explanatory payoffs of certain research program comes from *intentionally* underplaying or taking as equal a set of factors. For example, sociological new institutionalism “*defocalizes ‘actors’ on purpose*” (Jepperson, 2002: 230). By not taking individuals to be the fundamental units of society, it allows them to see how broad institutional frameworks construct actors and their interests. This also allows them to explain the homogeneity of nation states or organizations in the face of great differences in resources and traditions.¹¹ It makes little sense to suggest that new institutionalism should start to take actors seriously when it is the analytic trick of downplaying them that generated explanatory payoffs in the first place.

SI seems to be taking this road. It has tried to address its critics and incorporate aspects that were not originally part of its repertoire, for example, adding everything from cultural studies, critical theory, Parsonian theory, to child development psychology (see Fine 1993). Such corrections, or add-ons can contribute to the strength of a program by increasing the repertoire of analytic and explanatory tools. However, overzealous attempts to incorporate everything will only dilute the original payoffs of the program. We act as if the goal is to find a multi-purpose tool that can do everything—preferably a theoretical perspective that is better than all the others, or a set of generally relevant and constant cause explanations for as many phenomena as possible. However, anyone who has ever made use of a multi-purpose tool will understand that the attempt

¹¹ “The institutionalization of world models helps explain many puzzling features of contemporary national societies, such as structural isomorphism in the face of enormous differences in resources and traditions” (Meyer et al, 1997: 145).

to pack in too many features creates trade offs and makes the whole tool unwieldy and inefficient. This broad array of different approaches with different logics of inquiry have not been truly synthesized, but instead eclectically thrown together. It is not clear how these diverse resources and tools can ‘hang together’ as a coherent program. Such adjustments without a larger logical synthesis will only lead SI to further lose its distinctiveness and coherence. For whatever reasons (whether they are making a virtue out of necessity; or whether it is a sincere epistemological stance), some interactionists argue that the pluralism that has emerged is desirable. In response to the diversity of SI, Sandstrom and Fine (2003: 1052), propose that “this is how it should be” as the final word in the *Handbook of Symbolic Interactionism*.

Goffman

Unlike SI, Goffman’s approach can be seen as an attempt to transcend specific contexts or social worlds by creating a unified conceptual space that he calls the interaction order—the face-to-face, body-to-body, immediate situations that can be studied in and of itself. Goffman employs naturalistic observation and catalogues types of interactions and their consequences. But even as he catalogues the species of interactions, the lack of an overarching taxonomic logic results in the lack of any cumulative theory. He does not specify how different types of interactive actions are connected, or the conditions under which they are deployed. While he creates a unified conceptual space, and sets up the interaction order as a valid domain of inquiry, the contexts of interaction is not theoretically delimited. His empirical studies therefore excavate different orders of meanings and interactions without being able to create a connected and cumulative auxiliary belt of unit theories.

1 Establishing the ‘interaction order’ as a legitimate domain of research

Goffman attempts to demarcate a *unified conceptual space* by arguing that the interaction order is a social institution that can be studied in and of itself. He proposes that the ‘interaction order’ is a legitimate domain of study—environments whereby participants are physically in one another’s presence, and in face-to-face interaction (Goffman 1983). Being in circumstances where bodies are thrown together, our appearance and mannerisms become crucial and significant.¹² In such circumstances, individuals are inclined to manage their appearance and mannerisms because of coordinative functions and strategic purposes. Impression management serves a *coordinative function* because a working consensus is necessary in the division of definitional labor—to know

¹² As he explains, face to face interaction has a “promissory, evidential character” (Goffman 1983: 3).

in advance what a specific actor expects of others and what they expect of him. Impression management is also undertaken for *strategic purposes* because actors have interests in manipulating the outcome of those interactions.¹³ Actors desire to sustain a particular definition of the situation in the face of disruptions and challenges (Goffman, 1959: 254). Interaction is in this sense, political action.¹⁴

According to Goffman, even though interaction is socially situated (circumscribed in time and place), it is orderly and patterned. Interactions are institutionalized because different societies face the same contingencies of interaction, and therefore patterns are likely to emerge. In that sense, interactive norms, rules and regulations are not invented anew in each situation or behavioral setting. Interaction orders therefore extend beyond any single situation and Goffman was interested in abstracting such patterns of interaction. His efforts can be seen as an attempt to get at more generally relevant analyses that he felt SI failed to achieve. In fact, Goffman criticizes symbolic interaction for being “anti-systemic” because it does not show that there is organization, orderliness and pattern (see Verhoeven 1993: 334). Goffman argues that analysts “have to end up with a natural history of something, with phases, structures, patterns, or (they) haven’t said anything...haven’t analyzed it. And none of these things are really a part of symbolic interactionism” (interview in Verhoeven 1993: 335). The goal of analyses here is not merely to show complexity (as Goffman suggests that the symbolic interactionists are doing), but to identify patterns that could be used to simplify it. Presumably, if certain generic patterns can be identified, then they will be relevant to all who study interaction.

2 Goffman’s logic of inquiry and method: cataloguing species of interactions within the interaction order

Goffman proposed naturalistic observation as the primary means of studying the interaction order. As he proclaimed in his presidential address to the ASA, “I believe that human social life is ours to study naturalistically, *sub specie aeternitatis*” (Goffman 1983: 17). He also clarified that he approaches research “as a naturalist would”, much like a “botanist” or other such “classificatory science” (see Verhoeven 1993: 329). He advocates that “the perspective of *natural*

¹³ Goffman argues that individuals will mobilize their activity in order to convey an impression to others so that they elicit desired responses: “It will be in his interests to control the conduct of others, especially their responsive treatment of him” (Goffman 1959: 3).

¹⁴ The definition of a situation is not merely a technical function for coordination. There is also a moral quality to interaction because we assign moral rights to value and treat persons in appropriate ways (Goffman 1959: 13). The norms and rules of interaction do not merely allow coordinative action to occur, but they are strategically managed so that it occurs in morally appropriate ways, and therefore, strategic behavior comes to play as people contest moral norms.

history is taken: unique outcomes are neglected in favor of such changes over time as are basic and common to the members of a social category, although occurring independently to each of them” (Goffman 1961: 211 emphasis my own).¹⁵ There are good justifications for this naturalistic approach because what an individual says may have little bearing on what he does in interaction (see Verhoeven 1993: 322). Goffman does not want to get at ‘doctrine’ or ‘ideology’ because that is not the domain of analysis he is examining (Verhoeven 1993: 325).

Just like natural scientists would catalogue the species in each ecological niche, Goffman offers us a catalogue of the species of interactions, interacting units, interactional strategies and consequences within the ‘interaction order’. Lofland (1980: 28) thinks that most fundamentally, Goffman is propounding large numbers of terms and concepts that denote some aspect of interpersonal conduct:

“The bulk of any given piece of Goffman’s work is taken up with more or less elaborate enumerations of types and kinds of things that can happen in a situation...Typically, at least three-quarters of the space in any given piece is devoted to laying out categories of kinds of sources of occurrences, forms of occurrences, kinds of consequences of occurrences, ways of preventing occurrences, and forms of dealing with occurrences” (Lofland 1980: 29 emphasis in original).

These are just some examples of the typologies he offers us:

- i) *Types of interactional entities: ambulatory units, contacts, conversational encounters, formal meetings etc* (Goffman 1983: 7).
- ii) *Types of responses to stigmatized identity: 1) repair; 2) master activities ordinarily closed off; 3) use for secondary gains* (Goffman 1963b)
- iii) *Types of strategies of passing* (Goffman 1963b: 73, *Stigma*)
- iv) *Types of copresence: gathering, situation and social occasion* (Goffman 1963a: 13-22, *Behavior in Public Places*).

“The main effort is clearly that of delineating categories of types and kinds of events, roles, rules, and so on. Each category is usually given one or more illustrations, sometimes drawing from a wide variety of contexts and historical periods, bringing together seemingly disparate incidents, activities and reflections into a single model of interest at the moment” (Lofland 1980: 30). As Goffman himself explains: “I collect lots of different things and force myself to try to find the formulation that’s relevant and consistent with it” (Verhoeven 1993: 341). Goffman also uses strategic data sites because the level of reality he gets at actually does not necessarily need to come from ethnographic sites or natural observations. Species of interactions can come from many sources. In *The Presentation of Self In Everyday Life*, Goffman

¹⁵ In *Moral Career of Mental Patient*

collects illustrations from novels, observations from monographs, and quotations etc besides his own fieldwork.

Goffman does not even call this approach ‘research’, and thinks it is more like ‘scholarship’. He explains:

“It’s taking concepts or variables which I think have some relevance, significance, and reviewing various kind of findings, or research, novels, all that sort of thing, in terms of them. So it is a sort of freewheeling, literary kind of thing...usually collect a large number of illustrations and think about them in a more systematic way...But that stuff is stuff one does when one isn’t doing participant observation, isn’t doing a natural study. I don’t know where that stuff goes. It’s theory, on some crude and vulgar level. It’s a claim that one has access to, or one can introduce some concepts or variables, or terms, that will have some utility. And one introduced them by showing how a wide variety of relatively different sorts of material can be discussed in terms of it. That’s, I suppose, one of the techniques that I employ, but I also employ that technique in actual ethnography” (Verhoeven 1993: 338-9).

3 Mere cataloguing does not create a generally relevant belt of unit-theories

Goffman’s “central pre-occupation is with sociological taxonomy” (Lofland 1980: 34). Lofland (1980: 30) reminds us that these are not statements to be tested but assertions that they are important considerations when examining the interaction order. A theory at least has to be “a set of substantively interrelated propositions which purport to specify the conditions under which some determinate phenomenon varies” but “it is the typical absence of such statements that deny the early Goffman’s analyses the status of theory” (Lofland 1980: 31). There is therefore little concern with “the underlying relations among the types” (Lofland 1980: 31) that would constitute theory. It is this neglect of the theoretical relationships with the types that fails to create an interrelated auxiliary belt.

While Goffman creates many insightful typologies, we do not know when, or what types of strategies will be deployed by who, and under what conditions. The scope and generalizability of such analyses is therefore questionable (even to Goffman himself). While he recognizes that the goal of analysis is to generalize to other settings, or to show continuities and make comparisons across settings, he admits that he has no grounds for describing the parameters, or the outside boundaries of the conduct. In other words, he has no sense of what the scope is, and what cases it applies to (Goffman, 1963). In an interview, Goffman says he hedges his bets by claiming that his observations are limited to the specific group he studied. However, that answer is just a verbal device he uses and “no one really knows the boundaries (of applicability)” (Verhoeven 1993: 325). He recognizes that it is difficult to know how far such analyses are generalizable, and even when he has a sense of it, it cannot easily be proven (Verhoeven 1993: 341). He is therefore understandably humble about his studies of social interaction. As he said, “from the perspective of the physical and biological sciences, human social life is only a small

irregular scab on the face of nature, not particularly amenable to deep systematic analysis” (Goffman 1983: 17). “We are just trying to get reasonable classifications, one or two useful concepts, way of touching on and describing processes and practices” (Goffman, in Verhoeven 1993: 328).¹⁶

As a result, despite his brilliant insights and treatises, Goffman’s approach has not resulted in a high consensus, rapid discovery research program. Like SI, scholars inspired by Goffman’s work may share sensibilities and strategies, but not a generally relevant auxiliary belt of theories. Despite the attempt to create a unified conceptual space, the ‘interaction order’ was still too theoretically broad to be able to render patterns of interaction generally relevant for those studying interaction in different contexts.

Ethnomethodology

One reason why symbolic interactionist studies lack a generally relevant auxiliary belt is because it is notoriously difficult to achieve agreement between analysts when it comes to the interpretation of subjective meanings of actors. Ethnomethodology overcomes these problems not by formulating precise tools of interpretation, but by bypassing it altogether. Ethnomethodologists are not interested in the subjectively expressed meanings of social actions, but in the *objective-interactional* meaning of those actions. Whatever the expressive meanings of the actors, their utterances and actions can be understood in terms of the interactional function or purpose that they play in that local interactional system. Because the interactional purpose of those utterances and actions are made by actors for other actors, they are by their nature public. It is in this sense that these utterances and actions are ‘objective’—they are publicly observable and reportable. This move has significantly reduced the semantic variance of ethnomethodological analyses, and resulted in a higher level of practitioner consensus in terms of the concrete explications of interactions. But even though it solves the problem of consensus, it fails to achieve rapid progress because the interactional context is not delimited, so empirical studies proliferate without the benefit of a generally relevant belt of auxiliary unit theories.

¹⁶ “The issue is not to have to depend very much on being able to say for certain what went on in any particular case, but rather to be able to use particular cases to illuminate things that must go on in some cases but one isn’t absolutely sure in which cases it goes on. And all that ought to be presented tentatively, that is, not as confirmed findings but merely as suggestions. The proof always comes from whether individuals find it useful to use those concepts and terms and what they do” (Verhoeven 1980: 339).

1 Examining folk methods that create orderly social life

Husserl's phenomenological philosophy was meant as a correction of what he considered to be the crisis of western science. It reintroduced the significance of subjectivism to challenge the unclarified foundations and unquestioned presuppositions of the objectivism in western sciences (Gurwitsch 1966: 412, 417). Husserl sought to ground the sciences in the world of common experience, or the *Lebenswelt*. This philosophy was translated by Alfred Schutz into a sociological problem of examining how intersubjectivity is achieved in everyday life.¹⁷ This phenomenological sensibility has been taken up by Garfinkel to examine how ordinary members of the social world make use of practical methods and create objectivity as an ongoing accomplishment.¹⁸ Ethnomethodology therefore involves the study of "the body of common-sense knowledge and the range of procedures and considerations by means of which ordinary members of society make sense of, find their way about in, and act on the circumstances in which they find themselves" (Heritage 1984: 4). Simply put, EM investigates the folk methods that make social and organizational activity visible and accountable.¹⁹

Garfinkel (1996) begins from different foundational premises compared to what he terms the 'formal analysis' of other approaches in the social sciences. Such approaches presume that raw experience has no organization or order until it is apprehended through a stipulated analytic framework.²⁰ Furthermore, these analytic frameworks tend to emphasize macro structures which are given explanatory priority "because the big thing is considered more massive and stable than

¹⁷ How intersubjectivity is achieved is a practical and empirical issue (Goode 1994: 43), and by implication, not a philosophical one. Goode (1994: 43) poses the question in simple terms: "How do people start out with the feeling of being cut off from each other and together achieve the mutual feeling of being in touch with each other in the same world?" What 'working assumptions', 'common schemes of communication', 'congruent practical relevancies', do we have that help us mutually define things to do in the world? (Goode 1994: 43).

¹⁸ Ethnomethodology takes "the objective reality of social facts as an ongoing accomplishment of the concerted activities of daily life, with the ordinary, artful ways of that accomplishment being by members known, used, and taken for granted, is, for members doing sociology, a fundamental phenomenon" (Garfinkel 1967: vii)

¹⁹ "Ethnomethodological studies analyze everyday activities as member's methods for making those same activities visibly-rational-and-reportable-for-all-practical-purposes, i.e., "accountable," as organizations of commonplace everyday activities," and therefore, "analyzeable" (Garfinkel 1967: vii-viii). "Ethnomethodology's fundamental phenomenon and its standing technical preoccupation in its studies is to find, collect, specify, and make instructably observable the local endogenous production and natural accountability of...society's most ordinary organizational things in the world" (Garfinkel 1996: 6).

²⁰ Garfinkel argues that these other approaches assume "there is no order in the concreteness of things" and so they have to "respecify the sheer circumstantiality of ordinary activities so that order can be exhibited analytically" (Garfinkel 1996: 7). In other words, formal analysis requires an analytic framework to make sense of the raw stream of experience.

any of the little events that take place within it” (Lynch 1993: 30). In contrast, ethnomethodology regards raw experience as *already* orderly and intelligible (Maynard & Clayman 1991: 387). Garfinkel proclaims that “there is order in the most ordinary activities of everyday life in their full concreteness” (Garfinkel 1996: 7 emphasis in original). This orderly structure of the social world is an accomplishment of the accounting practices through and by which the social world is described, explained and thus achieved by the practical activities of actors.

2 Bypassing the problem of interpretation to get at objective meaning

Beyond merely claiming that everyday social reality is orderly, Garfinkel proves that it is in fact, objectively ordered. That is, the orderliness exists beyond our subjective interpretations of it. As Garfinkel (1996: 8) puts it, this order is “discoverable” and not “imaginable.” Unlike SI, EM is therefore not concerned with the interpretation of subjective expressive meanings. As Garfinkel (1996: 8) explains,

“Ethnomethodology is not in the business of interpreting signs. It is not an interpretive enterprise. Enacted local practices are not texts which symbolize “meanings” or events. They are in detail identical with themselves, and not representative of something else. The witnessably recurrent details of ordinary everyday practices constitute their own reality. They are studied in their unmediated details and not as signed enterprises.”

Zimmerman and Wieder (1971: 289) reiterate the same point:

“The ethnomethodologist is *not* concerned with providing causal explanations of observably regular, patterned, repetitive actions by some kind of analysis of the actor’s point of view. He is concerned with how members of society go about the task of *seeing*, *describing*, and *explaining* order in the world in which they live”

The type of meaning that ethnomethodology ‘uncovers’ is a very specific type of meaning—the objective level of meaning that is part of a trio of meanings that Mannheim (1952) takes pains to distinguish. Mannheim (1952: 49-51) identifies three analytically distinct kinds of meanings: 1) objective meaning 2) expressive meaning and 3) documentary meaning. *Objective meaning* can be “fully grasped without knowing anything about the ‘intentional acts’ of the individual ‘author’ of the product or manifestation” (Mannheim 1952: 45). *Expressive meaning* “cannot be divorced from the subject and his actual stream of experience, but acquires its fully individualized content only with reference to this ‘intimate’ universe...the interpretation of expressive meaning always involves the task of grasping it authentically—just as it was *meant* by the subject, just as it appeared to him when his consciousness was focused upon it” (Mannheim 1952: 46). *Documentary meaning* is meant to capture the global character, ethos or worldview. When a subject is grasped by its documentary meaning, it points to something beyond itself, here

not the intentional content, but the epitomized or ‘essential’ character. This is the perspective in which we grasp the elements that go to make up the global outlook of a creative individual or of an epoch (Mannheim 1952: 48).²¹

The documentary method of interpretation involves a circular hermeneutic logic whereby the whole is used to make sense of the parts, and the parts to make sense of the whole. Mannheim (1952: 70-5) considered the documentary method as a mode of interpretation that captures the *weltanschauung*, global outlook or ethos. Even though Mannheim wanted to establish the scientific validity of this method as an analyst’s method, Garfinkel (1967: 78-95) took this mode of interpretation to be a common folk method used by both professional as well as ordinary members of society. As a folk method, the documentary method of interpretation is a means of making sense of utterances and actions since these are necessarily indexical. Members actively invoke and construct the context of those utterances and actions to make sense of them, and in the process, actually constitute the meanings of those utterances and actions. Ethnomethodologists therefore regard the documentary method of interpretation as part of the repertoire of *folk methods* members use for sense-making (as data), and not as an *analytic method* they advocate for use by ethnomethodologists (see Zimmerman and Wieder 1971: 293).

If ethnomethodology is involved in any ‘interpretation’ at all, it is the ‘interpretation’ of the objective meaning of member’s actions. The meanings of these actions are ‘objective’ because they are publicly observable and reportable, and thus unproblematically demonstrated and made evident to others. Using Mannheim’s distinctions, we can more clearly see that EM is really getting at a kind of objective meaning (of actions within some interactional system), and this may include the examination of the documentary method used by everyday folk.

3 Discarding explanation and interpretation for explication

Ethnomethodology involves a logic of ‘explication’ quite unlike ‘interpretation’ or ‘explanation’. It is possible to distinguish between explanation (*eklarern*) and interpretation (*verstehen*) as categorically different enterprises with distinctive logics of operation. The agenda of a positivist social science is explanation, which involves the search for correlations and aspires towards generalizable or predictive laws of social behavior. In other words, the principal task of explanation is to locate *causality*. Interpretation on the other hand seeks to understand the

²¹ Mannheim uses the example of his friend giving some money to a beggar as illustration. The objective meaning of that act is ‘assistance’; the expressive meaning of the act depends on his friend’s intentions, and it is to express sympathy or generosity. Mannheim suggests that the documentary meaning of the action is ‘hypocrisy’, and that is his ‘synoptic appraisal’ that is then used to make sense of the particular actions of his friend.

relevant meanings with which social actors actively use to create and make sense of their social worlds. The principal task of interpretation is therefore to decipher or uncover *meaning*. In short, explanation locates causality, and interpretation deciphers meaning.²²

Even though explication is arguably a variant of both interpretation *and* explanation, it is worthwhile to make an analytical difference between the three for heuristic purposes. Explication occupies a unique position in the sense that it straddles between explanation and interpretation, acting as a handmaiden to the explanatory task of locating causality as well as the interpretive task of deciphering meaning. Explication is a type of systemic or formal analysis that is designed to *construct* or *uncover* the mechanisms and dynamics that underlie the operation of the phenomena in question. The ‘mechanisms approach’ in the social sciences makes use of this explicatory logic (Hedstrom & Swedberg 1998; Hedstrom 2005). This explicatory logic is also close to what Blaikie (1993) calls ‘retroduction’ (neither induction nor deduction), whereby models of the mechanisms and processes of the phenomena in question are constructed and then tested through observations. To the extent that they throw light on the black box of social interaction, we could say that interactionist approaches are engaged in an explicatory task—interaction as a ‘mechanism’ that mediates and transforms various inputs (actors, situations, resources) into observable outputs (*outcomes* of social interaction such as the division of labor, the distribution of rewards, or the definition of the situation).

The difference between EM and other ‘interpretive’ methods is that one ‘captures’ reality while the other ‘represents’ it. EM gets at ‘objective meaning’ but interpretation requires the researcher (employing a documentary method of interpretation) to transform reality into a representation. Depending on his interests, an ethnographer may choose to paint a picture of reality that highlights certain features while downplaying others. Interpretive sociologists are interested in the interpretation of subjective expressive meaning (what does it mean to be a drug dealer, musician, scientist).²³ The actor’s intentions are typically important to them so that they can ‘give voice’ to these subjects. In contrast, ethnomethodologists are mainly concerned with explication and bypass the messy task of interpretation. As Zimmerman and Wieder (1971: 295)

²² Weber makes such an analytical distinction between interpretation and explanation. He argues that *causal* factors that affect social action do not affect the logic of interpreting those actions *meaningfully*: “The recognition of the causal significance of (various factors that underlie meaningful action) *would naturally not in the least* alter the specific task of sociological analysis or that of any other sciences of action, which is the *interpretation of action in terms of its subjective meaning*” (emphases my own, Weber, 1968: 25).

²³ Or they may be interested to do meta-interpretations of the broad cultural significance of a category of practices (eg the cultural significance of religion in a secular world; the meaning of work in postindustrial society etc).

put it, EM is not concerned with how situations are defined, but how persons go about seeing and proposing a definition of the situation. Unlike other mechanisms-based approaches, ethnomethodology does not ‘construct’ but instead ‘discovers’ what is inside the black box of social interaction. Because of the metatheoretical assumption that there is a *common* (public, observable and reportable) objective reality that is *orderly*, they can engage in naturalistic observation to excavate this orderliness.

What ethnomethodologists are really interested in, is the objective meaning of actions made within the local interactional system. Whatever the expressive meanings of the actors, their utterances and actions can be understood in terms of the *interactional function* or *purpose* that they play *in that local interactional system*. Utterances are examined not in terms of what the author’s intend to express, but what they are meant to do. For example, convicts who tell researchers about the convict’s code is not merely describing the norms that exist in their world, but also *doing* boundary-work by drawing social distinctions between convicts and researchers (Wieder 1974).²⁴ Because the interactional purpose of those utterances and actions are made by actors for other actors, they are made to be public and observable and understandable by other actors. It is in this sense that these utterances and actions are ‘objective’—they are publicly observable and reportable. Ironically, even though the phenomena EM gets at is publicly available and observable, “society hides from its members its activities of organization and thus leads them to see its features as determinate and independent objects” (Garfinkle 1969: 182). EM makes visible these constitutive practices that are for the most part, invisible to ordinary members of society engaged in these activities.

Bypassing interpretation and delimiting the analytic task to explication helps to create greater consensus because the focus is on the most objective and public level of meaning. This does not mean that this particular level of meaning is the most analytically crucial or causally significant, but that focusing analytic attention to it creates a high level of consensus.²⁵

²⁴ Taking the lead from ordinary language philosophers, EM examines ‘how to do things with words’ but extends this to actions as well, to examine ‘how to do things with actions’. The ‘thing’ that is being done is an interactional achievement. For EM, this interactional achievement is not theoretically pre-defined, but left to whatever is of interest to members in local contexts. Extraneous meanings therefore creep in to the pure interactional achievement. CA is capable of retaining the ‘pure’ interactional system.

²⁵ Focusing solely on explication neglects other levels of meanings and other analytic tasks. Gubrium & Holstein (1997: 107) argue that “ethnomethodology risks reality’s melting into representation as it focuses on the *hows* of reality construction at the expense of the *whats* of lived experience.” In a discussion of Wieder’s (1974) EM study of the convict’s code in a halfway house: “Because the *whats* of a particular code, say, as opposed to other codes, are technically undistinguished, the sociological message can become more about member’s methods than about a concrete code (this particular code) as an object of

4 Context or ‘interactional system’ not delimited

Even though greater consensus was achieved by EM, and rapid discoveries made in many different specific studies, there was no accumulation of knowledge through an interrelated belt of unit theories because the context of investigation had not been delimited. The problem with explicatory research programs is that they tend to be highly contingent upon the local substantive context where the action is being carried out. Garfinkel (1967) constructed breaching experiments to demonstrate that there *are* in fact common sets of background assumptions that we share, and explicated some of the common folk methods (e.g., ad hocing) actors use to make sense of the world. Those studies were designed to demonstrate the payoffs of the ethnomethodological sensibility—the skill of explicating the objective interactional meanings that contribute to the achievement of social reality. The empirical studies spawned from those insights are extremely diverse. Ethnomethodologists have catalogued the ordinary methods of assessing organizational effectiveness (Anspach 1991); examined how deaf-blind children experience music (Goode 1994: 34); or how formal protocols are used as a resource to attack professional competence (Lynch 2002). It is difficult for EM to become a cumulative research program because of the lack of an auxiliary belt of testable theories that all ethnomethodologists have an interest in working on. What they share is an insightful methodological sensibility, but this sensibility does not translate into a set of standard analytic mechanisms that can work in many different contexts, or any generally relevant unit-theories. Empirical studies in EM are therefore highly contextual—they end up studying *whatever* folk methods and practical activities are invoked in that local context.

Conversation Analysis

Unlike EM, conversation analysts delimited the interactional system to the immediate interactional system of talk. Because of this analytical move, they could accumulate a set of findings that are generally relevant to its practitioners. What was crucial was beginning with mundane, ordinary conversations so as to accumulate a set of primordial mechanisms of talk, and then use these as the baseline for analyzing the special conditions of institutional talk. This has proven to be extremely successful at creating a high consensus, rapid discovery and cumulative research program. CA has been proclaimed to be “the most sustained and coherent research program that has developed out of ethnomethodology” (Lynch, 1993) and hailed as a rare example of an approach in sociology that is closer to ‘normal science’ (Law & Lodge 1984: 283).

experience... Viewed in this way, Wieder’s analysis becomes morally empty and might as well have been the study of *any* kind of rule use, in the halfway house or any place else” (Gubrium & Holstein 1997: 107).

1 CA is just like EM, but delimits the interactional system to talk-in-interaction

Inspired by the investigative initiatives of Garfinkel and Goffman, CA was articulated by Harvey Sacks in association with his collaborators, Emanuel Schegloff and Gail Jefferson (Atkinson & Heritage 1984: 1) Derived from EM foundations, CA is like EM in many aspects:

1) It presumes the orderliness of social interaction. As Heritage (2001: 52) explains, “social interaction is orderly on an individual, action-by-action, case-by-case, level” and that “order must be found in the naturally occurring materials of interaction, rather than materials fabricated through experimental procedures or role plays.”

2) CA bypasses the problems of interpretation and so the conversational practices are “basically independent of the motivational, psychological or sociological characteristics of individuals: the institution of interaction largely antedates the characteristics of those who staff it” (Heritage 2001: 52).

3) CA operates with a predominantly explicatory logic. “The central goal of conversation analytic research is the description and explication of the competences that ordinary speakers use and rely on in participating in intelligible, socially organized interaction” Heritage & Atkinson (1984: 1).

If no rapid discovery research program emerged from either Goffman or Garfinkel, what properties does CA have that is different? Even though both EM and CA make sense of the objective interactional meaning of actions by locating that action within an interactional context or ‘system’, CA delimits that context to the interactional system of talk. EM examines the objective-interactional meaning of actions or utterances in some local substantive context, e.g., ‘telling the code’ could be a kind of *boundary-work* (Weider 1974); invoking (the lack of) formal protocols is a way of *attacking* normative propriety of actual behavior (Lynch 2002). Therefore, specific actions or utterances are examined not for their expressed meanings, but for the interactional meaning, or the interactive function it achieves within that interactional context. For ethnomethodologists, the context is pretty much left to the analyst to identify and define. CA on the other hand, begins by delimiting that context to the immediate interactional system of talk. This interactional system is made up of turns and sequences. As Heritage (1984: 245 emphasis added) puts it, CA is “primarily concerned with the ways in which utterances accomplish particular actions by virtue of their placement and participation within *sequences of actions*. It is *sequences and turns-within-sequences* which are thus the *primary units of analysis*.” At its most basic, “conversation analytic research into the sequence is based on the notion that in a variety of ways the production of some current conversational action proposes a *local, here-and-now*

‘*definition of the situation*’ to which subsequent talk will be oriented” (Heritage 1984: 245 emphasis added).

This is why Gurbium & Holstein (1997: 126-7) suggest that the empirical horizons of the term ‘context’ are important to consider.

“Conversation analysts tend to limit their interest in context to what is immediately and discursively apparent within conversational exchanges. The parameters of the contextual are defined by what is ongoingly available, or being built up, in conversation. Ethnographers, in contrast, traditionally view context as more scenically and culturally inclusive, extending to matters not readily evident in conversational exchanges per se (e.g., performance rituals, institutional missions, and broad cultural understandings).”

2 CA as a primitive natural science collecting raw data from mundane conversations

Harvey Sacks set out to construct a nascent behavioral science that was based substantively, and not only analogically, on the existing natural sciences (Lynch 1993: 204). Sacks thought of conversation analysis as a primitive natural science concerned with studying actual, observable social activities. The assumption was that the social world was already well ordered, and that this order was visible. He wanted to establish what he called “a foundation for a natural observational science of sociology” (Sacks, 1989: 212). Because there is a tendency for sociological concepts and generalizations to gloss over or idealize the specifics of what they depict, they have a “vague and indeterminate relationship with any specific set of events” that inhibits the development of sociology as a cumulative body of knowledge (Heritage 1984: 234). Sacks’ response to this problem was to “develop a method of analysis which would keep a grip on the primary data of the world” (Heritage 1984: 235). He felt that sociology needed to handle the details of actual events formally and “be informative about them in the direct ways in which primitive sciences tend to be informative, that is, that anyone else can go and see whether what was said is so” (Sacks 1984: 26).

Through the recording of conversations, CA manages to capture ‘raw data’ that allows for detailed public scrutiny that minimizes the influence of personal preconceptions or analytical biases (Heritage 1984: 238). Such “original data are neither idealized nor constrained by a specific research design or by reference to some particular theory or hypothesis” (Heritage 1984: 238), and “every effort is made to render empirical analyses answerable to the specific details of research materials and...to avoid their idealization” (Heritage 1984: 243). The expressive level of meaning, that is, the author’s intentions is bypassed in order to examine the objective-interactional meaning. As Heritage (1984: 243) puts it, “there is a strong bias against *a priori* speculation about the orientations and motives of speakers and in favor of detailed examination of conversationalists’ actual actions” (Heritage 1984: 243).

3 Expansion of analysis to institutional talk

The raw data that is collected is mined from ordinary, mundane conversation. Mundane conversation is regarded as primordial, while talk in specific institutional contexts as special variations on this foundation.²⁶ CA has

“a commitment to the study of ordinary conversation as a domain which has substantive priority over other forms of interaction such as, for example, the rituals of public events or more specialized activities such as court hearings or business meetings. The initial body of CA research focused entirely on ordinary conversation, and even when drawing from data, such as group therapy or emergency telephone calls, its practitioners focused on what was ‘ordinary’ rather than what was ‘institutional’ or otherwise exceptional about them” (Heritage 2002: 53).

The reason why CA has obtained a set of generalizable statements is because it has been presumed and demonstrated that all cases of ordinary talk can be studied without exception. Once a set of basic CA observations have been made, these understandings can extend the project to special cases. We might argue that a set of primordial and thus universal mechanisms of talk have been uncovered by basic CA, and these may be modified when participants engage in institutional talk. The decision to begin with mundane conversation is therefore strategic. They may not have been able to unearth such universally relevant mechanisms if they began with the more complex institutional talk. As Heritage (1984: 240) explains, “comparative analysis with mundane interaction is essential if the ‘special features’ of interaction in particular institutional contexts are to receive adequate specification and understanding.”

Basic conversation analytic studies take conversational interaction as a topic in its own right. These conversation analytic studies describe how people take turns at talk in ordinary conversation and negotiate overlaps and interruptions; how gaze and body posture are related to talk; how grammatical form and discourse particles are related to turn-taking and other interactional issues (Heritage, 1997). Beyond the conversational order of interaction there are also ‘social and institutional orders in interaction’ such as the social worlds of medicine, law and the classrooms whereby their reality is ‘talked into being’ in interaction (Heritage, 1984). This reality however, is not confined to talk, but exists in and as document, buildings and legal arrangements. The CA study of institutional talk is concerned with “how these institutional realities are evoked, manipulated and even transformed in interaction” (Heritage, 1997). CA has been applied to legal proceedings, doctor-patient interaction, calls to emergency services, news interviews and classroom interaction (Heritage 2001: 54).

²⁶ “If ever there was an object of inquiry furnished internally with its own constitutive sense, with ‘its own terms’, with a defensible sense of its own reality, it is talk-in-interaction, and most centrally ordinary conversation” (Schegloff 1997: 171).

For example, in terms of explicating the formal structures of conversational interaction in the medical encounter, CA has been very enlightening. Studies have demonstrated how asymmetry in the medical consultation is a result of a collaborative achievement and not background structural factors (Gill 1988); how bad news is conveyed to patients by forecasting (instead of stalling or being blunt) results in better patient ‘realization’ of the news (Maynard 1996); how patients have to establish legitimate patienthood and ‘doctorability’ of their visit (Halkowski, forthcoming); how medical authority is challenged and struggled instead of a functionalist assumption that doctors have absolute control over the outcome of a medical encounter (Perakyla 1997). Such studies unveil the negotiation that goes on between doctors and patients, and the formal structures, stages and sequences of interaction that goes on in the encounter.

4 CA discovers instead of constructs mechanisms: delimiting the interactional context leads to cumulative cataloguing

We might argue that CA is involved in ‘discovering’ mechanisms instead of ‘constructing’ them.²⁷ CA gets at the objective-interactional meaning, where abstract (expressive or documentary) interpretive frames do not matter. Basic CA has been described by its proponents as a form of ‘objective hermeneutics’ (John Heritage) and ‘analytical description’ (Emanuel Schegloff)²⁸. Because basic CA discovers objective mechanisms, the extension of such findings to the analyses of institutional talk becomes possible. Both Goffman and Sacks modelled their work after the natural sciences, and both sought to catalogue species of interactional elements in the interaction order. The difference is that in CA, the interaction order is delimited to the interactional system of talk. Because of this, the cataloguing becomes a cumulative exercise since what is being collected in that abstract system of ordinary talk is more ‘immutably mobile’²⁹, and less context dependent. These structures and sequences of talk transcend different social worlds and interactional contexts.

²⁷ Although recordings of conversation are ‘perspectival’ in the sense that a selected bit of conversation is put under the microscope, this type of perspectival nature, is not likely to be consequential for the objectivism of CA.

²⁸ From personal correspondence

²⁹ Latour’s (1987) term, to describe how certain components of science acquire stability of meaning and become widely established.

Summary of argument

The central project of the symbolic interactionists—the interpretation of social meanings—is not systematizable. The perspective continued to inform ethnographic studies, excavate novel information about specific social worlds, and provide insight on specific social worlds, but this empirical work did not build up a cumulative and interrelated unit theoretical apparatus. Interactionists may tweak or clarify the program’s orienting strategies, and acquire more artful sensibilities of research, but contributions made at this level of the technology do less to help build a progressive research program with a rapidly moving research front.

Goffman’s theorization of the ‘interaction order’ is an attempt to mobilize analytic attention to the same conceptual space so that generally relevant findings may accumulate. If patterns and types of interactions may be identified, they should be of general relevance to all students of interaction, no matter what social world they examine. Goffman begins to do this by cataloguing interactionally relevant elements such as types of interactions or strategies within certain arenas of interaction (some ordinary, some stigmatized). However, the lack of an overarching taxonomic logic to organize those types resulted in the absence of any cumulative theory. It is unclear how one set of interactive strategies are connected to others, or the conditions under which some specific type of strategy is undertaken.

Ethnomethodology, unlike SI, bypasses problems of interpretation because it sets up itself to be primarily concerned with explication. In this way, an unusually high level of consensus is achieved because it is possible to examine the publicly displayed details of the concrete actions undertaken in those specific settings. The objective-interactional meanings of their phenomenon can be verified objectively without the semantic variance and polymorphism of interpretations. However, EM does not delimit the interactional context of their explications, and so these explicatory accounts (while verifiable and thus achieving high consensus), are not generally relevant to other practitioners working in other contexts. Since this is an explicatory program, and thus, concerned with ‘how’ questions, the answers are likely to be highly context-dependent and local. Ethnomethodologists did not manage to collect a repertoire of specific ‘mechanisms’ (sometimes true theories) that could be used in various contexts.

CA on the other hand, managed to accumulate such robust ‘mechanisms’ by delimiting the interactional system to talk. By examining mundane conversations, CA was able to accumulate a belt of auxiliary theories relevant for other practitioners because these were primordial aspects of talk generally prevalent in all forms of ordinary conversation. The research technology of CA is therefore a “tinkerable research technology” (Collins 1994: 171) that has

given rise to a stream of discoveries. Just because CA is a progressive research program does not mean that it is superior to the other 'qualitative' approaches.

Research as Craftwork, Research Programs as Technology

I am in no way suggesting that CA is the best 'qualitative' perspective because it is a high consensus and rapid progress research program. In order to achieve this, CA has delimited itself to a very specific task and logic of inquiry. It also bypassed the task of interpretation, and doing so, ignored getting at a level of reality and a set of meanings that sociologists also find useful to investigate. SI, Goffman and EM mainly provide us with methodological directives or sensitizing strategies. These are crucial contributions even if they do not translate to an integrated auxiliary belt. The goal here has not been to undermine these approaches in any way, but merely to figure out the conditions under which sociological approaches can generate standard puzzles to solve, and produce findings that are cumulative and interrelated. While it is ideal to have clarified metatheoretical foundations, a clear logic of inquiry, an integrated auxiliary belt of unit theories, and exemplary studies, many research programs continue to generate new and novel findings even without some of these components. For high consensus, rapid progress and cumulative programs to exist, these components of the technology are more likely to be present.

Evaluating sociological research programs is therefore an extraordinarily complex exercise because we are examining the whole research technology and not just the tools in themselves. It may be easier to pass judgment on some single specific unit theory, but to evaluate a whole program is a much more complicated, but worthwhile task. Sometimes it is not the tool itself, but the way or purposes for which the practitioner is using the tool that creates problems. These are equally crucial aspects and ought to be included in our assessment of the productivity of research programs. The concern here has been on the consideration of 'technical' properties of programs, e.g., whether the sensitizing strategy translates into unit theories. Such components of the program and their interrelations affect the functioning of the whole technology. We should be able to determine what adjustments to which component of the program is productive and what adjustments are degenerative. Not only should each individual component of the program be considered in terms of the role it plays in the program, but the whole program itself can be considered in the context of the intellectual field.

With this in mind, we can begin to assess adjustments to research programs more incisively. The bickering about orienting *strategies* are typically unproductive if it is discussed in the abstract, as if one could adjudicate which strategies are globally better. There are many attempts to 'synthesize' different research programs at the level of strategies without

consideration of the other components of the technology and how such integration actually affects these components. This is why many attempts to create a unified paradigm have largely been unsuccessful, despite methodological clarifications and periodical attempts to create a more unified, integrated approach for social scientific analysis (see for example Mitroff & Kilmann 1977; Wagner & Berger 1985; Liao 1990; Holmwood & Stewart 1994; Jasso 2004). The reason for this lack of success is because many of these attempts at synthesis, or paradigm building, try to integrate different approaches along only one of the dimensions of the whole technology. It is only by developing the connections between the different levels of programmatic activity that it becomes possible to create a high consensus, progressive approach. Instead of the less illuminating categories we typically use to make sense of these approaches—micro vs. macro; quantitative vs qualitative—it is therefore more productive to understand the logic of inquiry. By taking research programs as technologies with distinctive properties and payoffs, we can begin to adjudicate what tools are better for achieving what goals and with what tradeoffs. Then we will have acquired a sense of the craft.

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