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From Total Institution to Status Bloodbath: Goffman as a Comparative Researcher and Grounded Theorist

Abstract Erving Goffman has not only had a huge impact upon his home discipline of sociology but may be its most cited, as well as best known member ever. Nevertheless, Goffman continues to be heavily criticized by his fellow sociologists for his allegedly undecipherable and sloppy methods for conducting empirical research and constructing and accumulating general theory. We demonstrate here that while some of his specific practices were intuitive and unorthodox, and perhaps not always available to Goffman himself through self-reflection, they often included systematic empirical comparisons and generalizations, and grounded, general theory. It is tragic that he apparently died lamenting that these common characteristics of his work have been so little recognized (Berger 1986). He encouraged Verhoeven (1993), among others, to rectify this, and hopefully our own efforts here have contributed to this important, but unfinished business.

Keywords Goffman; Sociology; Comparative Methods; Grounded Theory; General Theory

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Erving Goffman is one of sociology's best-known practitioners. He has often been credited with superior powers of observation and analysis, producing "aha!" experiences in laypeople, beginning undergraduates, and mature sociologists alike. Lofland (1984) wrote this about his legacy for sociologists:

Goffman was the master coiner of exactly the apt concept, the champion selector of the quintessential label for the once dimly perceived, but henceforth crystallized reality. He has peppered our language with words and phrases that have new meanings, some of which have become part of our taken-for-granted worlds, and we no longer even associate them with Goffman. Impression management, total institution, stigma, mystification, encounter, interaction ritual, and presentation of self are among the most widely circulated. (p. 10)

This statement alludes to the great breadth of Goffman's impact within sociology itself. Thus, whereas the presentation of self and impression management in general and of stigmas in particular refer mainly to "micro" social psychological phenomena, interaction ritual and total institutions allude to sociologists' quintessential concern with "macro" social organization or structure. Nevertheless, while Goffman argued for studying naturally occurring social phenomena with relatively unobtrusive methods like participant observation, psychologists have easily employed and corroborated Goffman's claims in studies of the Internet, as well as experiments in laboratories (Brown 1998; Link and Phelan 2001; Ellison, Heino, and Gibbs 2006). Furthermore, historians and political scientists have used Goff-

man's late works (1986 [1974]) to explain how social movements "frame" themselves and their environments, choose strategies, win over allies in their struggles against authorities, and handle success and failure (McAdam, McCarthy, and Zald 1996; Benford and Snow 2000). Similarly, Goffman's earlier claims that total institutions "mortify" inmates' selves are alleged to have influenced psychiatry and contributed to the "de-institutionalization" of mental patients (Peele et al. 2000).

This is an impressive legacy, but Goffman and his methods for doing research and constructing theory have been much debated and criticized.

Thus, as an observer of social reality, Goffman has variously been characterized as biased, incomplete, or not particularly interested in precise empirical observations in the first place. With regard to bias, in his early critique, Gouldner (1971) suggested that Goffman's social position as a member of the new middle class and his adherence to Blumer's symbolic interactionism seriously affected what he saw and studied, leading him to presume that face-to-face interaction (hereafter FTFI) is episodic rather than heavily constrained by wider and longer social organization, and that humans are naturally Machiavellian self promoters and manipulators of others. Later, Weinstein (1994), a psychiatrist, claimed that Goffman's allegations about the mortification of mental patients' selves were tainted because he sided with patients against staff and was anti-psychiatry, leading him to ignore studies with contradictory findings, including surveys reporting that a majority of patients have been positive about their hospital stays.

As for incompleteness, others, like Lofland (1980) and Smith (2006), have pointed out that while Goffman usually referred to his own fieldwork studies as “ethnographies,” they do not meet the criteria for them; that is, complete accounts of entire, unique cultures. Instead, Goffman provided no such published accounts of any of his three, first-hand fieldwork studies (Shetland Isle, Central Hospital, and gambling in Las Vegas), preferring instead to mix seemingly scattered observations from them with those of other researchers and even laypeople and writers of fiction. Understandably, the latter procedure has raised concern about the reliability of such observations, and Goffman’s failure to explicate the criteria for how he selected these various observations to begin with has done the same for their generalizability (Verhoeven 1993:341).

These criticisms presume that Goffman had pretensions to be directly empirical in the logical positivist sense, but some critics have expressed doubts about this and pointed to his frequent use of others’ observations as evidence. Although Lofland (1980:31) excavated a number of empirical generalizations from Goffman’s writings, he concluded that Goffman, “performs a kind of abstract ethnography organized around his own concepts rather than around those of a particular set of ‘natives.’”

Williams (1988) characterizes the above critiques as a “critical orthodoxy”: those who make them presume that the epistemological realism and empiricism of the physical sciences are appropriate for the social sciences, and then find Goffman’s own methods wanting. Nevertheless, many of these critics recognize the intrinsic worth of the products of

his work, and therefore give him the benefit of the doubt. However, Williams argues that “normal” physical science is not in fact appropriate for the social sciences.

One of the principle differences is that social scientists share with laypeople consensually agreed upon observations about social reality that are not, and perhaps need not be, in question. Instead, the more important task is to better understand those relatively unvarying facts through comparing different concepts and metaphors, and accumulating an increasingly “thicker” and more convincing description. So, rather than a liability, Williams treats Goffman’s apparent epistemological naiveté and failure to justify his own methods as normal science and his ethnographies of concepts as blessings. Although they do not go this far, Cahill, Fine, and Grant (1995:611) also argue that Goffman was not attempting to offer his own ethnographic descriptions, “[r]ather, his primary concern was to develop general theories of interactional politics and the organizational construction of persons.” Therefore, criticisms of his empirical methods, such as his “unsystematic and non-participatory style of observation,” are misplaced.

Despite their differences on Goffman’s epistemology and empiricism or lack of it, Williams agreed with Lofland that Goffman relied heavily upon concepts, and also noted Goffman’s use of Kenneth Burke’s “perspective by incongruity.” There, as Lofland (1980:27) put it, “by looking at the familiar through a new set of concepts the taken for granted becomes problematic.” Since then other interpreters, like Burns (1992), Manning (1992), and Branaman (1997),

have followed these lines, but Manning (1992:55) in particular has argued that Goffman used concepts and metaphors like theater/dramaturgy, games/strategy, and ritual/ceremony in close conjunction with variations in empirical reality itself. Specifically, Goffman deductively “tested” provisional concepts and metaphors against that reality and then inductively modified or replaced them when he discovered they did not fit well.

Although the latter interpreters go farther than Williams in seeing Goffman as an empiricist and logical positivist, with the possible exception of Branaman, they, too, portray Goffman as having provided few empirical comparisons, generalizations, causal explanations, and general theory along the lines of normal physical science. However, still other interpreters disagree. Indeed, no less authorities than Glaser and Strauss (1967:139) wrote that Goffman, too, was a “grounded theorist,” although they were not sure how.

Much later Davis (1997:372-373) was still maintaining that Goffman was a rigorous inductivist, so much of one that he never used his own theories deductively. If this were true, it might explain why he seldom cited his own previous work. Interestingly enough, Berger (1986:xii-xiii) claims, Goffman eventually became disappointed that even his admirers did not see his legacy as one of distinctive general theory, and deliberately tried to rectify this in *Frame Analysis* (1986 [1974]). However, there, Goffman (1986:1-13) maintained that (a) his explicit focus was only upon the way individuals’ structure their internal perceptions about social reality, and (b) this was not what he usually did. Furthermore, the the-

ories Goffman claims to have built upon there are phenomenologies of individuals’ experience rather than explanations for social organization itself.

The above sets of issues—Goffman’s self-reflexiveness and objectivity or partisanship as an empirical observer; the degree to which, and how, he was a systematic comparative methodologist and grounded theorist and proceeded inductively versus deductively—are those we attempt to resolve in the rest of this paper.

In what follows, we argue that Goffman’s epistemology was surprisingly conventional, and cannot be used to support claims that his objectives differed so much from mainstream sociologists that his work should be evaluated on different criteria. By both personal and theoretical inclination, Goffman probably did give participants’ own experiences much less weight than he should have, yet there is considerable evidence supporting his emphasis in relative terms. Furthermore, although many of his specific methodological strategies were unorthodox, they have now often been acknowledged as “best practices” for increasing the validity and reliability of qualitative research.

With a few exceptions, Goffman himself was not very helpful on the problem of how he selected and compared observations and made empirical and theoretical generalizations. Nevertheless, one can detect practices and substantive explanations that go far beyond the formal concepts and metaphors most of his interpreters have stopped at. Here, too, Goffman may sometimes have un-self-reflectively allowed concepts and explanations he had adapted

from past sociologists' work to bias his own, yet we follow Huber (1973) in arguing that this problem is unavoidable and has usually been inadequately addressed by proponents of "emergent" or "grounded theory." As he did with his observational methods, Goffman also lessened the negative impact of his a priori deductions by often making his presumptions and qualifications for his empirical and explanatory generalizations explicit, so that others can test their validity. On balance, when one does the latter, as well as takes into account his methodological acuity more generally, the aforementioned "aha!" nature of his work becomes more understandable.

Was Goffman a Reconceptualizer, Not a Researcher?

In the interview with Verhoeven (1993 [1980]:327), Goffman did characterize himself as "unsophisticated" on epistemological matters, yet immediately added that he was also "conservative and old-fashioned." He had read Parsons' *The Structure of Social Action* (1968 [1938]) early on and found his rendering of the "epistemological realism" of Weber and Durkheim appealing. There is an objective social reality "out there," which can be observed and analyzed relatively directly and objectively, yet has not already been fully revealed to us in our everyday experiences.

Clearly, how we perceive it and give it meaning affects how we and others react to it, and Goffman made considerable use of Thomas' (1923) "definition of the situation" and "theorem" (defining a situation as real is likely to heavily affect its reality), and presumably also Mead's (1934) and Blumer's

(1969) emphasis upon the importance of meaning. The latter seems clear in Goffman's *The Presentation of Self in Everyday Life [PS]* (1959:9-14), where he frequently used the terms "definition of the situation" and "working consensus." Nevertheless, Goffman told Verhoeven (1993:327, 323-324), "So I don't take a radical, evaluational, subjectivist view. I'm not an ethnomethodologist by any means," and strongly resisted Verhoeven's suggestion that he might be a social constructionist. We are all social constructionists to some degree, Goffman countered, but because of unforeseen and unintended consequences and norms and roles that predate and impinge upon FTF encounters, their nature and course cannot be reduced to shared definitions of the situation and narrow social contracts (also see: Goffman 1963a:7-8, 104-106; 1983:5-6.)

If there is any doubt that Goffman aspired to be a realist and empiricist, one need only closely examine his answer when Verhoeven (1993) asked him,

[V:] ... can I formulate it in this way, that you have ... a hypothesis, and then you look at society through different examples to find a confirmation of this particular hypothesis.

[G:] I guess I would go along with that except I wouldn't use the term *hypothesis*. I think that's rather optimistic ... I [do] try to draw the lot [of concepts that I employ] to see whether they survive after being thought about or tested or applied or used. But, I think it's very much an exploratory, tentative undertaking. (pp. 327-328)

Admittedly, the latter process includes conceptual exercise (thinking about), but also operations on

what one presumes is a relatively objective reality also recognizable by others (testing, applying, using). In fact, later in the interview, Goffman (Verhoeven 1993:338) made a point of portraying the former as only "scholarship," as opposed to true "research," "participant observation in the main, some sort of deduction from one's data."

[G:] I still believe, that given what one studies, one can come up with something that wasn't in one's head, but was in the data, within limits. Otherwise there wouldn't be much reason to continue in the business except as a livelihood. It would just be a question of who could paint a picture that would sell. (Verhoeven 1993:340)

Returning to the debates with which we began, we can see that Goffman was neither naïve nor radical in his epistemology. He considered himself a direct, empirical researcher and logical positivist. He was not simply doing ethnographies of others' and his own concepts, as Lofland (1980) had complained, but later appears to have changed his mind about (Lofland 1984:11), and Williams (1988) had praised him for, albeit with reservations about how to develop better non-directly-empirical criteria by which to evaluate the worth of Goffman's products.

Observational Validity, Reliability, and Generalizability

In the earlier talk "On Fieldwork," Goffman (1989 [1974]:125-127) told his graduate student audience that he preferred direct, participant observations of people's everyday actions and interactions. Goffman often defended this choice on two grounds. First,

he was primarily interested in naturally occurring rather than experimentally or otherwise "staged" interaction, and he wanted to employ methods of observation which are least obtrusive and likely to alter the interaction studied. Second, he felt that most of the time, most people are circumspect and reticent about expressing what they actually believe and feel, and present and defend their beliefs and feelings, as well as overt actions in idealistic and self-serving ways. Clearly, Goffman had good reasons for this stance.

For one thing, the Hawthorne researchers (Roethlisberger and Dickson 1964) had suggested that the mere knowledge they were being observed may have affected the workers studied and the results of the research. Furthermore, understandably, the men in the Bank Wiring Room were initially suspicious of the researcher-observer and very circumspect in his presence because they (rightly) thought he was allied with management; whereas the women in the Relay Assembly Test Room may have felt they were expected to produce more, and done so to ingratiate themselves with the researchers, as well as management. After Goffman's own early research many others have demonstrated the existence of "effects" for both "experimenters"/researchers and "subjects"/the observed, including self-fulfilling and self-negating prophecies (Cahill, Fine, and Grant 1995; Meeker and Leik 1995).

For another thing, that researchers and the researched would affect each other is a logical consequence of the interactionist theories that emerged at Chicago, and being circumspect in the presence of others and ingratiating oneself with them are

central assumptions of Goffman's own theory of impression management. In her critique of symbolic interactionist traditions, Huber (1973) worried that such researchers would be overly influenced by powerful informants and their biases and overly influential upon relatively powerless ones. Nor, as Becker (2003) had suggested, would siding with "underdogs" absolve researchers of their responsibility for validity and reliability. Either way, to the extent that researchers rely heavily upon the views of those they study, they are in danger of "going native" and losing their objectivity. In fact, Silverman (1989a; 1989b; 2005) has directly demonstrated how much one can bias results if one relies upon informants' own views to the extent that qualitative sociologists, as well as cultural anthropologists and oral historians typically do.

In their review, Adler and Adler (1998:89) identify the unobtrusiveness of direct observational methods and the relatively less likelihood of researcher and informer effects as their greatest strengths. However, following Blumer (1969), Denzin (1989:19-21) had questioned the validity of Goffman's direct methods precisely because they allegedly neglect the subjective experiences of those studied. In addition to the latter, Adler and Adler (1998:87-88) referred to the more general problems of validity and reliability when one usually has only one observer and one case study, "without statistical analyses to confirm the significance of observed patterns or trends." Nevertheless, they followed Denzin in suggesting other strategies to increase validity and reliability.

Earlier, Denzin (1978) had suggested that the limitations of any one method can be lessened by "tri-

angulating" it with several others, including, of course, accessing the subjective experiences of the observed. However, in addition to the latter ([b] in the list below), the Adlers (1998:89-90), who, like Goffman, are primarily interested in direct observation, suggest other means as well: (a) using multiple observers; (c) presenting results and conclusions in ways that third-party social scientists and other readers can recognize as similar to their own personal observations and experiences about everyday life; (d) looking for negative cases, and (presumably) showing either that they do not exist, can be systematically accounted for by one's own theory, or/and qualifying one's claims and theory accordingly; and, relatedly, (e) showing that one's observations hold up in other case studies of other settings and/or times.

Another suggestion can be extrapolated from another of the Adlers' claims (1998:90): "[a]lthough direct observation may be marred by researcher biases, at least they are consistent and known." We do not believe that this is necessarily or even usually true. Yet, (f) to the extent that researchers are aware of their biases and do declare them and suggest ways they can be allowed for or their validity tested, then this, too, should increase the validity, reliability, and generalizability of their claims.

Not being aware and not declaring their biases and providing means for testing them was another complaint that Huber (1973) made against Mead, Blumer, Glaser and Strauss, and others who have stressed allowing theory to "emerge" only during rather than before the process of empirical research. Thinking one does not have "biases" and an already

existing theory, and not declaring them, contribute greatly to the aforementioned tendency to "go native." One's theory is still likely to bias one's results, but even if it does not do so directly, it and the absence of methods for allowing for one's own biases may mean not being able to resist biasing influence from others. A major problem with Mead and Blumer, Huber (1973:278-282) argued, was that their undeclared liberal, optimistic biases and theories made them not see or downplay inequalities in power. Whereas Weinstein's (1994) criticism of Goffman appears to have been that his biases were conscious and led him to deliberately ignore or deny contrary evidence, Gouldner's (1971) is like Huber's (1973); that is, Goffman's allegedly naïvely liberal and episodic, astructural conception of interaction is supposed to have limited his awareness and analysis of large-scale systematic inequalities.

With these criteria in mind, how do Goffman's apparent methods hold up?

(a) Only a Single Observer and Case Study?

Strictly speaking, Goffman did not use multiple observers and run validity and reliability "checks." He preferred to work alone and did not believe he would receive large research grants to study the phenomena he wanted to. Nor, one suspects, would he have wanted the responsibility and accountability that would have come with such grants. On the other hand, Goffman did frequently use other researchers' observations of similar conditions, events, and sequences of events (processes) alongside his own, and often presented these other researchers' observations in great detail, in their own words. Al-

though the events observed were not precisely the same ones Goffman himself had observed, demonstrating what many others have "seen," for example, "role distance," approaches both "convergent internal validity" (different others have recognized similar orders of events and explained them similarly) and "external validity" (similar orders have been recognized and explained in similar ways by different observers in different settings and times). These practices then legitimate Goffman's work, particularly when one considers the further problem of not being able to afford additional observers for one's own case study, or of hiring only young, untrained, and inexperienced ones.

The same logic applies to case studies: Goffman relied upon those of many other researchers besides his own. Thus, Goffman told Verhoeven (1993:340-341) that he had somehow managed to make valid and reliable conclusions about mental hospitals, despite having "by and large" studied only one of them. In fact, however, he read and reported the findings of a huge number of other cases studies, of schools and prisons, as well as mental hospitals, and those in Masters and PhD theses, as well as published reports.

As for the Adlers' concern that such settings and case studies are seldom chosen statistically, Goffman (Verhoeven 1993:339-340) had some interesting suggestions in his defense. These included his charge that all too many statistical samplers have not explored and identified the relevant populations enough before drawing their samples. More importantly, for him, the most relevant populations were neither settings nor individuals, but instances and

sequences of FTFI, which occur almost everywhere. Therefore, identifying the entire population would probably be impossible.

The general lesson here is that sampling should probably fit the interests and purposes of the researcher as much as the reverse. Furthermore, this is particularly true when, as may be the case with most qualitative researchers, one's initial purpose is to develop rather than test theory. As Eisenhardt (1989) has put it,

Such research relies on theoretical sampling (i.e., cases are chosen for theoretical, not statistical, reasons) ... While the cases may be chosen randomly, random selection is neither necessary, nor even preferable ... given the [small] number of cases which can be studied, it makes sense to choose cases such as extreme situations and polar types in which the process of interest is "transparently observable." (p. 537)

(b), (c) Participants' and Third Parties' Views

On these criteria Goffman's performance was mixed, but not a "write off" either. Although, as we have seen, Goffman had good reasons to weight others' points of view less than his own direct observations, his own descriptions and explanations relied considerably upon participants' shared "definitions of the situation," and the same is true for beliefs in the applicability and often justness of social norms and the trustworthiness or untrustworthiness of others. Therefore, Goffman probably should not have discounted and failed to directly study participants' own views as much as he did. Richard (1986) has argued this for the "next of relation" for inmates

of total institutions. Whereas Goffman implied that they almost always collude with institutional staff against patients, Richard demonstrates that this is not so. Yet, here, too, Goffman often attempted to compensate for not interviewing people by using indirect methods.

Interestingly, when he described these methods in "On Fieldwork," Goffman (1989:131) began with Cooley's "sympathetic introspection": using one's own experience of the situation to understand others. Thus, Goffman (1989) advised students,

Write [your field notes] as lushly as you can [without trying to defend what you have written to absent professional others whose roles you are nevertheless taking] ... as long as you put yourself into it, where you say, "I felt that" ... you've got to start by trusting yourself ... Now don't *just* write about yourself [or to too great a degree] but put yourself into situations that you write about so that later on you will see how to qualify what it is you've said. You say, "I felt that," "my feeling was," "I had a feeling that"—that kind of thing.¹ (p. 131)

What Goffman was suggesting here is that sympathetic introspection is one useful way of attempting to take the role of those one is studying, and therefore understand their experiences without having to actually ask them and initiate the biasing processes referred to earlier. By itself, of course, the latter strategy also risks privileging one's own response, and, when it is different from those of the people one observes, erroneously projecting it into others

¹ All but the first bracket have been added.

and biasing one's results. In fact, Mead (1964) had accused Cooley of projecting his own small-town middle class Christian perspective into others. Nevertheless, by discussing methods for accessing participants' own views, Goffman was obviously acknowledging the latter's importance. Furthermore, in that same passage, he did refer to taking into account "what they [participants] are saying" (Goffman 1989:131), and, one presumes, what they are thinking.

Of course, following Huber (1973), this by no means lets Goffman off the hook. After all, even when there is much overlap between the researcher's and participants' views, there remains the problem of which others and their views one has taken, and the possibility that overlaps have occurred through social influence rather than independent, accurate views of a relatively objective social reality.

With regard to the first problem, Weinstein (1994) criticized Goffman's claims about the mortification of patients' selves in mental hospitals not only on the grounds that he had neglected patients' own views, but that he neglected those of staff, and even admitted siding with patients against them. The latter is largely true (Goffman 1961a:x), but several important qualifications are in order.

One is that Goffman (1961a:54-57) offered several reasonable, methodological justifications for this. The reality of Central Hospital and other total institutions studied by others was that such settings tend to be divided along the lines of staff versus inmates—two competing "performance teams," in Goffman's terms—and if one is to gain good access

to inmates and their natural actions and views, one has to not be seen to be aligned with staff. Otherwise, patients would be as circumspect about revealing their experiences to the researcher as they are to staff. Goffman (1989:128) reiterated this in "On Fieldwork," and in his study of a narcotics prison hospital, Tittle (1972a) found that patients often did try to ingratiate themselves with staff in order to obtain an early release, by insincerely playing the role of the good, thankful, rehabilitated patient.

However, another qualification is that Goffman was by no means totally unsympathetic towards staff. This was true not only for young, idealistic psychiatrists who genuinely put therapy and rehabilitation before merely comfortable custody but custodians themselves, who, Goffman (1961a:78-82, 89-92) noted, were few in number relative to the patients at St. Elizabeth's (over 7000). In addition to having many needs and rights that required attention, patients were often distressed and difficult to manage.

Clearly, Goffman was highly critical of psychiatrists' propensities to reduce everything patients think and do to organic and psychogenic illnesses, and in addition to *Asylums*, other of his writings (1963a; 1971) were designed to counter such accounts with his own. Yet, Goffman (1971:373, 386) did not discount organic and psychogenic explanations altogether, and expressed sympathy for psychiatrists' dilemma of whether to side with patients and their stories or those of their responsible, "next of relation."

The Adlers' suggestion of stimulating third parties to recognize one's own experiences as a researcher and those of the participants one has studied may

have come from Glaser and Strauss (1967:230), but Goffman (1959) himself claimed to have deliberately used this in the *PS*:

The illustrative materials used in this study are of mixed status: some are taken from respectable researchers where qualified generalizations are given concerning reliably recorded regularities; some are taken from informal memoirs written by colorful people; many fall in between. In addition, frequent use is made of a study of my own of a Shetland Island crofting (subsistence farming) community. The justification for this approach (as I take to be the justification for Simmel's also) is that the illustrations together fit into a coherent framework that ties together bits of experience the reader has already had and provides the student with a guide worth testing in case studies of institutional social life. (pp. xi-xii)

Again, consensus among observers of whatever type does not assure accurate information, but here, too, Goffman (1989) anticipated this problem:

Then there's [the matter of] what to do with information [that one has recorded in notes to oneself]. Jackie takes seriously what other people say. I don't give hardly any weight to what people say, but I try to triangulate what they are saying [and my own experiences?] with events.² (p. 131)

In other words, the validity and reliability of subjective experiences, the researcher's included, are judged against overt actions and sequences of events that other scientific observers would probably also

see and report on in relatively unbiased ways. While Goffman did not provide an example there, one could easily employ his greeting and parting rituals (individuals say "hello" and "goodbye" to each other), and "remedial interchanges" (someone is challenged for something they have said or done, s/he justifies or apologizes for their actions, and the challenger resumes interacting as they had before).

Given her criticisms of Mead for implying that one can experience social, as well as physical reality directly, Huber (1973:278) would probably still counter that such observations themselves are at least interpreted and perhaps stimulated in the first place by pre-existing concepts and theories rather than "pure" and unbiased. To follow up the above examples, Goffman was also inferring norms, desires to continue relationships, as well as lessen conflict that would threaten them, and so on. This was part of Huber's rationale for questioning the validity of "emergent" ("grounded") theory. Similarly, Silverman (1989a:38-39; 1989b:226-227) warned that overlaps among researchers' and participants' experiences of social reality and researchers' (seemingly) more direct observations of its more physical features (e.g., sequences of actions) may occur only because researchers and participants, and perhaps events themselves, are all organized by the same discourse. The events may be intelligible to both, but still misunderstood.

On the other hand, Huber (1973) did not deny either the existence or eventual discovery of an objective reality, and argued only that one offers and/or accepts well-established criteria for determining the validity and reliability of propositions about it. Sim-

ilarly, for all his seemingly relativist leanings rather than a continuous conundrum, Silverman (1989c:57) proposed a "cautious positivism" as a means of solving these problems. As we saw earlier, Goffman considered himself a realist and positivist.

(d), (e) Negative Cases and Confirmatory Comparisons

Goffman often appeared to be looking for negative cases, for how actions and sequences differ in different settings and circumstances, and then suggesting how his initial empirical generalizations needed to be qualified. Thus, whereas Gouldner (1971) had accused Goffman of imposing a middle class, Western view of human nature and interaction on his research, Goffman (1959:244-245), himself, cautioned readers of the *PS* that most of his material there came from "unsettled," relatively egalitarian "mass societies" like America, and that without further, independent case studies, one should be cautious even about characterizing such societies themselves as a whole, let alone automatically generalizing from them to more settled, less egalitarian ones.

This is presumably why Goffman used observations from his dissertation on Shetland Isle—a small, settled community with a single "laird" and several squires—as a baseline in the *PS*. Although long-term status relationships, close physical proximity, and familiarity apparently limited the scope and intensity of dramaturgy, there, too, residents appear to have engaged in it. Hence, he concluded, his observations and theory of impression management are fairly widely applicable. On the other hand, after comparing self-presentation and protection

by patients inside Central Hospital and outside, in pluralistic civil society, Goffman qualified the generalizability of the theory considerably. Because patients were not free to present themselves as they had on the outside, they were seldom able to protect their selves, which were then "mortified" (also see: Lofland 1980:42-43; Branaman 1997:lii-lviii). Nevertheless, as we shall see below, this generalization, too, was later qualified.

(f) Declaring, Testing, and Neutralizing "Bias"

Whereas Gouldner (1971) complained that Goffman was not aware of and did not declare his biases, Weinstein's (1994) point was that he both had and declared them, but then still allowed them to bias his research. We doubt that either claim is accurate. Like the rest of us, Goffman probably attempted to be "objective," but frequently failed; but just as often made firm presumptions (had "biases," if you will) that he then declared and either tested empirically himself or/and justified in ways that allow others to do so.

We have already seen hints of this in the comparisons between Shetland Isle and the United States in the *PS*, and these practices are more explicit in *Asylums*. There Goffman (1961a:x, 65-66, 152) warned readers from the beginning that he may have exaggerated how many and much patients' selves were mortified by incarceration because, as a middle class male, he took much personal distinctiveness and autonomy for granted, and may have experienced Central Hospital as more degrading than working class patients may have. This would then allow other researchers to question and test this possibility,

² All but the first bracket have been added.

by comparing the responses of different types of patients. Unfortunately, few researchers have done this. However, Tittle (1972b) did compare male and female patients in his narcotics hospital, and as one would expect, extrapolating from the logic of Goffman's hypothesis about class differences, women there were more likely to rely upon other inmates for support and views of themselves than men did, and less likely to express self derogation.

Besides such "biases," *Asylums* is replete with qualifications about which additional circumstances Goffman's empirical generalizations about mortification presupposed. Goffman (1961a:131) carefully noted that mortification occurs particularly in large, "closed" hospitals, where the vast majority of patients are there involuntarily, cut off from their previous lives on the outside. Unlike pluralistic civil society on the outside, such institutions are also "impermeable": work and family, class, race, and ethnic relationships are not segregated by time and space, preventing patients from avoiding different others and protecting their own selves (Goffman 1961a:119-123). Furthermore, mortification begins soon after incarceration, but then lessens as patients develop supportive relationships with each other and secondary psychic adjustments, which provide alternative definitions and evaluations for their hitherto mortified selves (Goffman 1961a:133, 146-149).

Unfortunately, most subsequent researchers have failed to take into account Goffman's own qualifications, what he took to indicate mortification, and/or the processes of self-presentation and protection that he believed come between those institutional circumstances and patients' private selves. Thus,

Peele and colleagues (2000) revisited "Central Hospital" (St. Elizabeth's) some twenty years later and reported less "institutionalization" than Goffman had. They did note that the hospital had become much smaller and that the majority of inmates were there voluntarily, yet, since they did not look for mortification, theirs is not a good test of Goffman's own claims.

Quirke, Lelliott, and Seale (2006) also reported less "institutionalization" in three small, acute care hospitals in London, England, and to their credit they attributed the alleged differences from Central and similar hospitals during Goffman's time to the openness and permeability of their own hospitals. Nevertheless, they, too, did not directly observe patients' presentation and protection of their psychics selves and mortification. Meanwhile, Tittle (1972b) did not control for whether patients in the narcotics hospital were there voluntarily; measured mortification with a self-administered, albeit relatively unobtrusive, instrument; and wrongly interpreted Goffman as having expected mortification to be greatest in the middle rather than the early stages of patients' hospital stay. Therefore, his own qualifications of Goffman's claims remain suspect.

How Was Goffman Comparative and Grounded?

Thus far we have felt reasonably confident about Goffman's methods because we have had Goffman's explicit qualifications for his claims for the mortification of selves in *Asylums* (1961a); his seemingly candid, detailed description of his observational methods in "On Fieldwork" (1989); and his straight-

forward account of his (surprisingly conventional) epistemology in the interview with Verhoeven (1993). Nevertheless, we are less sure of his methods for selecting and comparing observations and generalizing from them to theory. This is so not only because he seldom explicated them but because he steadfastly insisted that he did not know what they were, could not have excavated them even if he tried, and had he done so, readers would find that his methods were used inconsistently and may even have been contradictory (Goffman 1986:15; Verhoeven 1993:323, 340-341).

Why Goffman took this stance and whether he was sincere is an interesting problem in its own right. Interestingly enough, Becker (2003:660) reports that Goffman told him this was a deliberate strategy on his part; that if he were to make his methods explicit, his critics would insist that he always use and justify them, even when they were not appropriate. This is consistent with Goffman's own theory of impression management; that is, we avoid presenting cues and making impressions that others would then insist we abide by in order to make interaction predictable and sustainable, and to obey rules of considerateness (which prevent others from losing face and becoming embarrassed) and self-respect (consistency is necessary for predictability).

Obviously, we cannot know what Goffman was "really up to." On the other hand, Goffman did graciously suggest a resolution for our dilemma to Verhoeven (1993:322-323). To wit, because we should regard all sociologists' accounts of their methods as more impression management than reality, the best way to discover Goffman's methods for selecting,

comparing, and generalizing is to take the entire corpus of his written work as a population of events about what he actually did, and then draw our own conclusions.

In fact, we have already begun that process here by noting Goffman's juxtaposition of impression management in the civil societies of developed Anglo-American countries with less developed Shetland Isle in the *PS*, and the still more confined circumstances of traditional *Asylums*. In addition, *Stigma* (1963b) deals with intermediate circumstances where individuals' successful impression management is likely to be limited by the fact that others will already have formed an impression if one has a visible stigma, and one will be threatened and insecure about interacting with others if, for example, as an ex-mental patient, one worries that they will discover one has an invisible yet discreditable stigma (Goffman 1963a:42). Branaman (1997:lviii) alludes to the former, and Misztal (2001:317) points out that "Goffman's description of the process of 'passing for normals'" is "an application of impression management."

Meanwhile, the moderate constraints of relationships with family and friends, as well as larger ones with employers and political authorities can be contrasted with the much greater freedom at social parties, especially where a large proportion of those in attendance are strangers or only acquaintances (Goffman 1961b:78; 1963a:135-136, 170-171; 1971:207-208). There, rather than "keeping one's place," may be a "status blood bath"; a "free-for-all" competition for status that may include a great deal of impersonation and other forms of misrepresentation; rather

than the kind in a large, public asylum, where one is forced to interact with very different others and not permitted to escape.

In *Behavior in Public Places* (1963a:198-199) and *Relations in Public* (1971:206-207), Goffman wrote about still more anonymous and fluid settings and relationships, and differentiated among them according to how “loose” as opposed to “tight” they are in terms of the number of norms that apply to them, to whom they are most likely to apply, how serious deviance from them is likely to be regarded, and therefore also how punitive sanctions are likely to be. For example, task actions and “focused gatherings” are likely to require more continuous displays of involvement than expressive actions (“socio-emotional” in Parsons’ terms) and “unfocussed” gatherings. Furthermore, although all settings, tasks, and relationships are likely to have some opportunities and rights to display “role distance,” middle class members will usually have more (Goffman 1963a:46-47, 110, 127). However, depending upon the gender aspects of the norm and role, women may have either more or fewer opportunities and rights than men (Goffman 1963a:50-51, 206-207; 1971:207-208).

This type of dissection of Goffman’s writings is clearly useful, but it seldom goes beyond abstract concepts and “perspective by incongruity” (e.g., the “Free” versus “The Confined Self”). Exceptions have been Branaman’s (1997:lii-lviii) catalogue of the social and psychic processes Goffman used to explain the mortification of selves in *Asylums*, and Misztal (2001:312-14) having noted Goffman’s underlying substantive dimensions of normality and trust, of norms for establishing both, and of all three

for increasing the predictability and success of interaction and participants’ security.

Admittedly, concentrating upon specific substantive dimensions and explanations involves considerable digging and extrapolation, and may detract from the “thickness” or “richness” of Goffman’s explicit accounts. Nevertheless, as Silverman (2005:211) suggests, relying mainly upon rich description can be a serious liability when it comes to arriving at valid and parsimonious explanations, and in our case, it might mean never discovering Goffman’s comparative methods and substantive theory. With this in mind, let us closely re-examine some of Goffman’s major texts.

There are four passages in the *PS* where Goffman compares his observations about interaction on Shetland Isle with his own and others’ about “civil society” in more developed Anglo-American communities. In the first passage, he generalized that most performers use status and other symbols to present a positively idealized view of their performance and selves (Goffman 1959:35). “In fact, however, many classes of persons have had many different reasons for exercising systematic modesty and for underplaying any expressions of wealth, capacity, spiritual strength, or self-respect.” (Goffman 1959:38).

The case studies or other illustrations from which the latter is inferred are (1) “the ignorant, shiftless, happy-go-lucky manner which Negroes in the Southern States sometimes feel obliged to affect during interaction with whites” (Goffman 1959:25); (2) “American college girls did, and no doubt do,

play down their intelligence, skills, and determinateness when in the presence of datable boys” (Goffman 1959:48); (3a) Shetland Islanders’ grandfathers having not fixed up the appearance of their cottages to prevent lairds from increasing their rents; (3b) Americans in the Great Depression appearing poorer than they were when visited by relief inspectors; and (4) current Islanders who were no longer crofters (subsistence farmers), nevertheless dressing and otherwise appearing to be so. Although “the many different reasons” for such modesty are not stated, it is not difficult to infer what they are likely to have been: (1) avoiding physical and/or verbal abuse, losing one’s employment, and perhaps being lynched; (2) being disapproved of, ostracized, and perhaps not getting a husband; (3) paying higher rent, not getting or receiving less relief; and (4) being disapproved of and rejected by the few people available to work and socialize with.

In all of these cases, one has to interact with others upon whom one is dependent, at least to avoid unpleasant consequences, and who therefore have power over one.

In another passage, Goffman (1959:78-79) dealt with the more complex circumstances surrounding performance teams. Here, performers are interdependent and must coordinate their performances to make a favorable impression and otherwise influence third-party audiences upon whom they, too, are dependent; that is, their employers, customers, and/or competing performance teams. The generalization is that the performance is likely to express the characteristics of the task and team, not those of its members as individuals.

One set of observations comes from Goffman’s own case study of a medical ward, where interns taking over from their counterparts from the day or night before had to appear knowledgeable, competent, and make definite recommendations about patients, despite not having seen and diagnosed them before, and having only colleagues’ earlier charts to go on. Another set was from Shetland Isle. There, in the tourist hotel, the host and hostess managers presented themselves to guests as if they were middle class, while the local girls who were waitresses and maids presented themselves subserviently. However, outside that setting the latter came from higher status families, and even within the hotel, once guests were no longer present, the subservience of employees ceased. Two other observations were wives appearing more subordinate to their husbands when hosting dinner parties than they usually were, and white and black co-workers being more formal to each other when third party whites were present.

In the third passage, Goffman (1959:220) referred to which particular strategies for presenting themselves performers are likely to employ in general, and to deal with such dilemmas as keeping close to the facts about oneself to safeguard the show and one’s self versus idealizing oneself enough to make an especially favorable impression. There Goffman was still more elaborate and explicit about the sources of such strategies.

One category (in effect) has to do with incentives: “care will be great in situations where important consequences for the performer will occur as a result of his conduct. The job interview is a clear example”

(Goffman 1959:225). In addition, Goffman made much of the fact that the mere presence of others and interacting with them involves the risk of discrediting oneself and/or one's team, and losing various other benefits in the process. Nevertheless, opportunities to lessen such risks vary considerably by additional circumstances.

Thus, risks are less the shorter the time one is in front of and performing for the audience, and the more resources one has to manage a longer show. It is easier to maintain a show for guests for short periods of time, and even in Anglo-American communities, "only in the upper-middle and upper classes do we find the institution of the week-end guest" (Goffman 1959:142). However, in less affluent Shetland Isle, most crofters "felt they could sustain a middle class show for" only a tea or meal, and "many Islanders felt it only safe to perform for middle class audiences on the front porch, or, better still, in the community hall, where the efforts and responsibilities of the show could be shared by many teammates" (Goffman 1959:221-222).

Finally, one's familiarity with others is important, in its own right and in interaction with incentives and opportunities. On the one hand, if one is familiar with others (and interaction has been rewarding), one is likely to identify and sympathize with them, which will, in turn, foster trust, tact, and cooperation (Goffman 1959:230-232), and therefore also less risk to one's self and more relaxed interaction. The latter also occurs because the impression each has of the other is likely to depend upon past interactions much more than any new one (Goffman 1959:222). Increased security from familiarity seems to be the

logic behind this later comparison between Shetland Isle and America:

[W]ith settled inegalitarian status systems and strong religious orientations [presumably against presenting oneself falsely], individuals are sometimes less earnest about the whole civic drama than we are, and will cross social barriers with brief gestures that give more recognition to the man behind the mask than we might find permissible. (Goffman 1959:157)

Of course, this presumes that in (allegedly) unsettled, "mass" societies like the U.S., individuals are less likely to be familiar with most others and more likely to believe that they can present themselves more positively than is warranted by their class position and personal characteristics. If this is true, then the main determinant becomes opportunity rather than incentive. On the other hand, in those situations where one is familiar to and with others, one has less opportunity to misrepresent one's self and get away with it, and is less likely to attempt to do so (Goffman 1959:222).

Goffman then made other cross-cultural comparisons relevant to differences in familiarity. Specifically, in the United States, the combination of a lack of familiarity, the value on privacy, and rules specifying "civil inattention" among strangers means that individuals usually do not "intervene" to help others except in relatively unavoidable circumstances; whereas on Shetland Isle, the difficulty of avoiding others, the familiarity of others, and rules requiring one to help others even on trivial, solitary tasks meant that others could drop in on one unannounced, and that one could not refuse help even if

one felt it was an invasion of one's privacy and did not want it (Goffman 1959:230).

In his introductory and concluding passages for the *PS*, Goffman appears to have situated these relatively specific explanations within a more general theory. We not only seek information about another person and are circumspect about disclosing information about ourselves but do so in order to understand, predict, and influence him or her, and we "usually" do these for "quite practical reasons," especially "to call forth a desired response from him" (Goffman 1959:1). However, both from taking the roles of and understanding others and our knowledge that they are pursuing their own "enlightened" self interests and expecting us to abide by rules of, among others, considerateness and self-respect, we usually also pursue our own interests in an "enlightened" (tactful and considerate) way (Goffman 1959:249); depending, of course, upon particular incentives, opportunities, and familiarities.

Furthermore, the types of comparisons and general explanations related to them in the *PS* were continued in many subsequent writings. Thus, *Stigma* (1963b) relies mainly upon case studies and single observations by others rather than his own direct ones, yet Goffman's comparison point was impression management among "normals" in the *PS*.

Thus, when someone has a visible stigma, normals are unlikely to be familiar with it and them, and likely to have difficulty knowing what to expect and do themselves, and be fearful about what the stigmatized is likely to do. As a consequence, they will avoid interacting with the stigmatized or attempt to

be "civilly inattentive" to the stigma. If normals are instead associated and identified with the stigmatized, they may attempt not to be seen with them or to hide the stigma during interaction with outside audiences. However, family members are likely to be highly interdependent with the stigmatized and unable to avoid them, and will have more of an interest in protecting them, for their own, as well as the good of the stigmatized.

Meanwhile, the visibly stigmatized themselves will anticipate not being accepted by unfamiliar normals and therefore have an interest in avoiding interacting with them and associating with other stigmatized persons, as well as protective normals. Should the stigmatized have to interact with unfamiliar normals anyway, and especially when they have an invisible and potentially disclosable and discrediting stigma, "[t]he issue is not that of managing tension generated during social contacts, but rather that of managing information about his failing" (Goffman 1963b:42). On the other hand, the opportunity to do the latter may be absent, and there Goffman understandably mentioned the plight of the ex-mental patient. He or she is likely to fear that those who are familiar with his or her past as a patient will be uncomfortable, if not outright disapproving, but that those who are not familiar with his or her past will be threatened, disappointed, or patronizing should they find out. Either way, the stigmatized and interaction with normals will probably be tense.

We have already taken up many of the circumstances of mental patients themselves while in hospital, and mentioned some of Goffman's important comparisons and qualifications with regard to the

mortification of selves. Yet, Goffman also frequently compared the conditions for impression management and self-protection “inside” total institutions with those “outside” and “before” entering them. Furthermore, rather than only constraints upon self-presentation and protection in the abstract, his explanations and qualifications for mortification again rely heavily, and sometimes explicitly, on variations in incentives, opportunities, and patterns of familiarity.

Thus, whereas in civil society on the outside one’s dependence upon and obligations towards employers, public service dispensing officials, family, and even friends are likely to be considerable, they will be segregated in space and time, and therefore also limited (Goffman 1961a:36-37). However, when one enters the hospital, one becomes nearly totally dependent upon staff for positive feedback about one’s performance and self, as well as one’s physical and psychic needs, including meaningful activity that indirectly contributes to one’s social identity and sense of personal well-being (Goffman 1961a:6-10). Although this heavy dependence upon staff usually decreases as patients become familiar with other inmates and form supportive relationships with them, it returns in the period up to one’s hoped for and/or scheduled release, since actual release is likely to depend upon continued or renewed positive assessments by staff (Goffman 1961a:167-168).

Goffman made parallel comparisons for opportunities. Upon entry one is “stripped” of and prevented from displaying one’s usual physical and social symbols of one’s identity as a distinct, autonomous, competent, and contributing adult. These include

not just one’s full name and title (Goffman 1961a:20-21), but

[a] margin of self-selected expressive behavior—whether of antagonism, affection, or unconcern ... This evidence of one’s autonomy is weakened by such specific obligations as having to write one letter home a week, or having to refrain from expressing sullenness. It is further weakened when this margin of behavior is used as evidence concerning the state of one’s psychiatric, religious, or political conscience. (Goffman 1961a:43)

The latter interpretations are then recorded and stored by staff, and patients often worry that they will be used against them by third parties, as well as staff (Goffman 1961a:159). Contrary to the social, role, and audience segregation on the outside, patients are forced to be around and interact with others whom they would not normally meet and/or engage with. Such “contaminative exposure” can threaten and weaken one’s self-supporting identity (Goffman 1961a:28-31). The same is true for the substance of the interaction. For example, high status persons on the outside will be threatened by not being deferred to on the inside, while low status patients who must obey staff will be humiliated by not being “allowed a margin of face-saving reactive expression—sullenness, failure to offer the usual signs of deference,” and so on (Goffman 1961a:36).

As for familiarity, patients are likely to feel that their “next of relation,” their most significant other, has colluded with staff to get one into hospital and not visit frequently, and are therefore likely to feel abandoned and betrayed, such that their “next

of relation” is no longer so familiar with one’s own distress and not as supportive and trustworthy as they once may have been. Either way, unfamiliar others will now see one in various states of undress and physical, psychic, and social distress. Worse, one is forced to disclose the intimate details of one’s own self and distress, and likely to be neglected or punished if one does not. Meanwhile, if one does disclose them, their origins and meaning will be interpreted for one. Should one’s own view differ from staff’s it is likely to be dismissed or reinterpreted (see above), but should one concentrate upon one’s illness and “sad tales” to rationalize it and seek pity, others’ agreement that everything one does and feels is ill might reinforce this and hinder rehabilitation.

In fact, Goffman continued such comparisons and specific, as well as general explanations in much of the rest of his writings, which can also be organized accordingly. If one starts with a more abstract dimension of constraints upon self-presentation and protection, one has total institutions like traditional asylums at a pole of most constraint, with subsequent amounts of constraint decreasing as one proceeds to communities with relatively “mechanical solidarity” like Shetland Isle, production and service organizations in the civil societies of Anglo-American societies, then more “private” relationships and groups like family and friends. The latter tend to be more encompassing, familiar, and supportive, and therefore conducive to security about others, thus one’s own self-presentation. However, interdependence, familiarity, and many diffuse obligations also mean few opportunities to avoid others and change established impressions.

As had Park (1952:176), Goffman regarded friendships as less confining than families, but more impersonal encounters and relationships in public are still less so. In turn, between these two settings are private social parties, where there is often a mixture of strangers and mere acquaintances, as well as more established friends. There a lack of interdependence and much familiarity with many others often permit a great deal of impersonation; individuals can pretend to have much more social status, personal competence, and charisma than they normally do, to the point where there is a raucous—but exciting!—“status blood bath” (Goffman 1961b:78).

We could push the applicability of Goffman’s comparisons and generalizations about impression management along the dimensions of (inter)dependence, opportunity, and familiarity still further. For example, the very distinctions that Goffman used in the rest of his writings appear to presume them. To wit, encounters, engagements, and interchanges involve more interdependence and risk of harm, as well as opportunities for new gains than mere “co-presence” in public does. Unfamiliar strangers are likely to be less predictable and more difficult to adapt to and influence than familiar family and friends are, but as we have seen, with the latter, opportunities for more freedom and more favorable impressions may be low. “Unfocused gatherings” provide more opportunities than “focused” ones that entail more interdependence and require more cooperation, and individuals will have more freedom to “save” and “make” “face” while working than in how they perform their work role itself (their contribution to the collective “focus”), but there will still be rules about expressing “role distance,” as well as genuine involvement.

Again, Goffman seldom articulated the dimensions he used to select and compare his materials, and when he did discuss dimensions, he sometimes seemed to be only entertaining them provisionally, with his characteristic qualifications. For example,

In the study of groups, the distinction between primary and secondary and between private and public meeting places may indeed be significant, but in the study of gatherings, all occasions when two or more persons are present to one another can be fruitfully treated initially as a single class. (Goffman 1963a:9)

Similarly, when Goffman (1963a:198-215) discussed the looseness versus tightness of gatherings, he agreed “informal” versus “formal” is often useful, but then made many qualifications. In addition to the abovementioned ones about class and gender (and age), they included whether or not high status and powerful officials were present to enforce compliance with norms. For attendants in Central Hospital, this meant only wearing ties when they were in the administrative wing, and not smoking when giving patients their “meds” only when doctors and nurses were present. Furthermore, whereas, for example, Parsons’ “pattern variables” work in the same direction to produce “traditional” versus “modern” role relationships and societies, Goffman’s qualifying dimensions are “nested” within others, making claims at this high level of generality difficult.

These considerations mean that the dimensions and comparisons we suggested above are just that: hypotheses. Nevertheless, since Goffman so seldom made his underlying dimensions and the logic of his comparisons explicit, we have had to make

such hypotheses, and recall that we have done so with his own blessing. In addition, the three dimensions we have induced from his comparisons and conclusions make sense of the latter, across many of his written works. They have also allowed us to do something about which both Goffman and his admirers have despaired; that is, indicate how much of Goffman’s work can be organized by a general theory of impression management.

Finally, for present purposes, the substantive dimensions and explanations we have “teased” out of Goffman’s writings overlap considerably with those used by earlier sociologists whom he acknowledged had inspired him. Specifically, Goffman often employed Cooley’s and Mead’s theory of role taking and empathy (e.g., see: Scheff 2006 [chapter 3]), and in that theory, their frequency increases with interdependence and their accuracy with interpersonal familiarity, as well as a common “universal discourse” more generally (see: Mead 1934). Similarly, in Park’s and Hughes’ “human ecology,” independence and/or competing interests lead to avoiding others, whereas interdependence and/or unequal dependence lead to accommodation and cooperation or domination and exploitation, respectively. Goffman (1971:190) both referred to such explanations and used them for his own results.

Meanwhile, much of Durkheim’s and Parsons’ structural functionalism relies upon the distinction between mechanical and organic solidarity, which itself entails economic independence versus interdependence and competing versus complementary interests, and Goffman’s own tightness versus looseness implies the same intolerance or tol-

erance for individual differences. His distinctions between the “focus” of an encounter or relationship and the “face” of participants and focused versus unfocused gatherings mirror Parsons’ instrumental versus expressive actions and roles, just as Goffman’s tightness/looseness parallels Parsons’ diffuse versus specific obligations/rights.

On the other hand, despite this considerable support for our hypotheses on how Goffman went about making comparisons and arriving at substantive explanations, one certainly cannot reduce all of Goffman’s work to these particular dimensions and his theory of impression management. Rather, in addition to such analyses and theories at the level of “individuals” and their relatively primitive organization (e.g., accommodation, mainly through only a “working consensus”), Goffman proceeded at the level of “social facts”; of the unintended consequences of individuals’ and teams’ actions and their organization by norms within role relationships, often segregated from each other, and within sequences of events like rituals and ceremonies. Indeed, Goffman often treated the latter as more or less self-equilibrating.

Nevertheless, whereas the latter phenomena require different types of explanations from those at the more individual, less organized level, Goffman appears to have used the same formal methods for comparing and generalizing from his empirical materials. That is, one chooses and compares circumstances and events that are similar and contrasting in order to infer substantive, cause-and-effect relationships. Indeed, Goffman’s comparative methods at the level of social order can already be seen in his

use of Shetland Isle in the *PS*. Many Islanders would have preferred not to be helped by their neighbors and others, but rules specifying that one must help others, no matter how trivial the task, did not allow them the privacy they wanted. This was contrasted with the rule of civil inattention elsewhere, which may mean that people who want, as well as need help may not get it. In another comparison, Islanders could not maintain their privacy because norms allowed others to visit one’s home unannounced, without even knocking (Goffman 1959:227).

Such “social facts” and how they occur take up a large portion of the first chapter on Performances and the summary of empirical generalizations Goffman provided near the end (1959:65). Furthermore, they are clearly based on and/or “illustrated” by comparisons like those just mentioned. For example, at one point, he illustrated the general phenomenon of a “working consensus” by inducing the common structure in different relationships with opposite substantive norms: for friends to express familiarity, involvement, and affection, but for specialists and clients not to (Goffman 1959:10). Goffman then took up how, because of norms of considerateness and self-respect, unintended impressions and the entire “show” prevail (1959:12-13). Finally, with empirical illustrations, Goffman argued that as long as performers perform well, whether or not they are emotionally sincere, does not make much of a difference either (1959:17-21).

That Goffman was aware of his use of such comparative methods to study norms and roles is clear in *Relations in Public (RP)* (1971). There Goffman (1971:225-237) explicates them with regard to hand-holding,

a type of “tie-sign”; that is, how individuals ritualistically express to third-party audiences, as well as each other the nature of their relationship. Thus, one strategy for studying such signs “is to try to assemble all of the environments in which the particular practice is found and then attempt to uncover what those various contexts have in common. Here, the ‘meaning’ of the practice is whatever co-occurs with it” (Goffman 1971:226).

Having begun to learn about the meaning of hand-holding by looking at those who do it and those who don’t, we can go forward. Given the people who do it, we can ask when it is they do it, and when it is they don’t (when they might) ... [Where it is prohibited and] where hand-holding seems to be approved and even idealized ... What groups were first to start using it? ... How does a holder learn to hold hands? (Goffman 1971:228-232)

Having provisionally answered these questions, Goffman proceeded to suggest general explanations from and for his empirical generalizations: “[W]e can anticipate that certain functions can be performed that could not be conveniently performed otherwise [although in some cases, other practices may be equivalent and substitutable for the one in question]” (1971:234). Note that this explanation is structural functionalist along the lines of Durkheim and Parsons. The same is true for his comments about socialization:

[H]owever hand-holding is learned, it is likely that a wide range of other items in our ritual idiom is learned in the same way, and it is the connection between socialization techniques and this total

range of items that is interesting, not in particular how hand-holding itself is learned ... In the face of all these easy opportunities [to have very different meanings attributed to hand-holding], a single understanding effectively orients the various holders and the various witnesses to the holdings. (Goffman 1971:235-236)

Meanwhile, note that these allusions to the origins and persistence of the practice (why some groups were the first to use it, the requirement for and effectiveness of common understandings, and therefore the socialization of new members) also suggest the social Darwinism of Spencer and Park. The same is true for Goffman’s use of a statement by Spencer to begin *RP* (formal government evolved out of such more primitive, adaptive accommodations as only “working consensuses”), and this one: “It would be impossible to read effectively the social scene around oneself or to provide others a reading of it if one were not constrained by the same rules as the other participants regarding ritualized indications of alignment” (1971:237).

Such explanations are obviously highly general, but they are explanatory rather than only categorical and metaphorical, and Goffman clearly used them in close tandem with substantive empirical generalizations and explanations. Opportunities for and restrictions upon hand-holding are related not only to establishing and maintaining relationships in general, as most “tie-signs” are, but to the particular requirement of preventing incest and allowing bona-fide sexual partners to leave other considerations aside and become completely physically and emotionally involved (Goffman 1971:230-231).

Goffman’s writings are replete with favorable references to explicitly Darwinian explanations (1963a:43-44; 1969:13-14; 1971:xvi-xvii), but as he did with most theoretical traditions from which he borrowed, he added his own qualifications. A main one occurred when he praised animal ethnologists as a model for his own work, but then cautioned that they, too, often assume that all the ways animals relate continue to have survival value. It bears noting that most of Goffman’s main theoretical ancestors—Cooley and Mead, Park and Hughes, and Durkheim and Parsons—are united by some degree of social Darwinism. These links are taken up elsewhere (in progress).

Inductive Versus Deductive?

Having made our case that Goffman’s research was heavily comparative and his explanations heavily “grounded” in it, what can we conclude about whether and how much he proceeded inductively versus deductively?

Goffman told Verhoeven (1993:328) that, “as you say, my approach is largely inductive,” and we have seen several cases of this in the previous section: the systematic presentation of data and only afterwards empirical generalizations and explanations about performances in the *PS* and hand-holding in *RP*. Nevertheless, as we also noted there, when he provided general explanations for his empirical generalizations, he often appealed to established general theories from the past rather than constructed entirely new ones. Indeed, this is how Goffman (1959) prefaced the *PS* as a whole:

The perspective used in this report is that of the theatrical performance; the principles derived are dra-

maturgical ones ... In using this model, I will attempt not to make light of its obvious inadequacies ... The framework is presented in logical steps. (pp. xi-xii)

That Goffman was proceeding deductively, as well as inductively is also clear in his notorious statement near the end: “Now it should be admitted that this attempt to press a mere analogy so far was in part a rhetoric and a maneuver” (1959:254). He then outlined many of the ways in which everyday life is not staged to the degree that a play in a theater is. On the other hand, he then acknowledged that it remains a useful way for drawing out many of the central features of everyday social encounters: “The key factor in this structure is the maintenance of a single definition of the situation, this definition having to be expressed, and this expression sustained in the face of a multitude of potential disruptions” (Goffman 1959:254). Recall that this is also how Goffman closed his late account of hand-holding and other tie-signs.

In his Preface to the *PS*, Goffman (1959:xii) had told readers that, “The introduction is necessarily abstract and may be skipped,” but were one to skip the Introduction, one would underappreciate the general nature and importance of Goffman’s own theory of impression management, a theory that clearly guided not only *Stigma* and *Asylums*, as we have seen but one which Goffman continued to refer to in later works. As we also saw earlier, *Asylums* itself has heavily deductive elements. Goffman went into Central Hospital with certain expectations about what he would find, and what he reported on and how he qualified his generalizations involved the same hypothetico-deductive logic that we suspect underlies most scientific research and theorizing.

As we noted earlier, there have been tendencies to portray Goffman as either a pure inductivist (Davis 1997) or pure deductivist (Lofland 1980). Surely, however, such extreme interpretations are not warranted. Methodologically and formally, our own view seems closest to Phillip Manning's (1992:54-55) "spiral"; that is, Goffman both tested his concepts and metaphors against empirical reality and then qualified or rejected them accordingly. However, rather than stop at concepts and metaphors, we have followed Goffman and moved on to substantive empirical and explanatory generalizations.

We have emphasized the absolute importance of deduction from general substantive theory to fill important gaps in how Goffman has typically been interpreted. Nevertheless, we have also stressed how systematic, comparative, and genuinely empirical much of his work was. It is impossible to determine the exact balance or imbalance between after-the-fact induction and a priori, hypothetico-deduction in that work. All we have to go on are Goffman's writings and reflections, and given his theory of impression management and warning to Verhoeven, it is not surprising that physical and social scientists' reports of their research and conclusions have been found to be idealized performances, often competing team performances, rather than completely accurate accounts of what actually went on (Kuhn 1962; Kaplan 1964; Garfinkel 1967).

On the other hand, whereas the latter research helped spark a host of new epistemologies and methodologies opposed to positivism, we should be careful not to rush to judgment, let alone impose the latter upon Goffman. As we have seen, he had

good reasons for resisting these trends. Unquestionably, Blumer and Glaser and Strauss were correct to warn against going into the field with one's mind already made up by pre-existing notions of what one is likely to find and how to interpret it. In fact, this was Goffman's complaint about much European sociology (Verhoeven 1993:343-344). Yet, surely Huber (1973) is no less correct: None of us goes into the field as a "blank slate," and rather than deny that we have presuppositions and preferred theories, we should make them available to others so they can decide how warranted our conclusions are.

In this regard, Glaser and Strauss themselves were anything but strict inductivists. Although they followed Blumer in recommending that we initially go into the field with only sensitizing concepts, they suggested that we then set out to verify or qualify our conclusions in realist and positivist ways (Charmaz 1998:254-256). Furthermore, when one examines the specific examples from their own research on awareness contexts Glaser and Strauss (1967) used to illustrate their methods for grounding theory, one finds a considerable gap between their findings and general explanations. Curiously, the latter look suspiciously like those in mainstream "role theory" and structural functionalism. There is nothing wrong with this per se. As per Huber (1973), the problem is when one denies one has presuppositions, which then are "snuck in through the back door" and affect one's research and theorizing anyway. Goffman's refusal to be consistently "grounded" in the new, pure, politically correct meaning of this term probably helped him strike a reasonable balance between the above two, equally dangerous methodological errors.

General Conclusion: A Parting Ritual

We have attempted to go beyond previous interpretations to provide a reasonable account of how Goffman went about selecting and comparing his and others' observations, as well as how he claimed to, and probably did, make his own direct observations. Although many of Goffman's specific methods were unorthodox when he first used them, by now they have often become "best practices." We have also found wanting claims that Goffman was either not a true empirical researcher in the first place or an unusually "biased" one when he did do empirical research. Furthermore, although we agree that Goffman's observations, empirical generalizations, and explanations were often organized by formal classification schemes and abstract metaphors, we have made a case that he went much farther. In particular, both his classification schemes and his metaphors appear to have been linked to substantive dimensions of in/inter/dependence, incentives, constraints versus opportunities, and unfamiliarity

and familiarity, dimensions which had been used by Cooley and Mead to explain the frequency and accuracy of role taking, and Park and Hughes those of contact, competition, accommodation, and super/subordination.

Finally, attempts to present Goffman as an inductivist rather than a deductivist are clearly misrepresentations. Instead, unlike many others out of Chicago, Goffman often made his own "biases" and preferred explanations explicit, allowing others, as well as himself to verify or revise them, and to accumulate a body of well-supported, general, substantive theory. That so few of his interpreters recognized the existence and nature of the latter in his work was apparently a major disappointment for Goffman (Berger 1986:xii-xiii). Hopefully, we are now closer to acknowledging Goffman's contribution to general sociological theory, repairing his impression, and developing his legacy further by building upon that general theory, as well as his highly innovative methods.

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