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Don't Argue with the Members

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Abstract Mel Pollner regularly cautioned researchers not to argue with the members of settings under consideration. He warned against substituting the researcher's meaning for the meanings of those being studied. This article discusses facets of the caution as they relate to the research process. Seemingly simple, the tenet is nuanced in application. The article adds to the nuance by distinguishing what is called the "replacement" of meaning with the "displacement" of meaning, providing a way of understanding what members could mean if the contexts and settings of their accounts were taken into consideration.

"Don't argue with the members."

Mel Pollner regularly offered this tenet in relation to sociological observation and commentary. Deceptively simple, yet fraught with possibilities, it harbors considerable analytic force. We first heard Mel's counsel years ago when one of the authors (Holstein) was enthusiastically recounting experiences from fieldwork in a mental health court. Having observed hours of courtroom interaction that apparently contradicted courtroom personnel's claims that the law was being conscientiously applied, Holstein was excited about the prospect of writing about the sociological irony that the "law-in-practice" was not delivering "justice" as the "law-on-the-

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books" prescribed. In his genial way, Mel considered the irony, appreciated it for what it might have been, then cautioned: "Don't argue with the members."

In this instance, first and foremost, Mel was instructing a neophyte in the basics of "ethnomethodological indifference." This fundamental stance vis-à-vis everyday social encounters precludes the characterization of members as deficient, pathological, or irrational, but opens space for seeing and hearing how local orderliness is indigenously organized through practices of mundane reason (Pollner 1987). In his way, Pollner was recapitulating his version of Garfinkel and Sacks' (1970) admonition that ethnomethodological studies must seek "to describe member's accounts of formal structures wherever and by whomever they are done, while abstaining from all judgments of their adequacy, value, importance, necessity, practicality, success, or consequentiality" (p. 345). Putting it simply, Mel was advising not to let the sociological proclivity for invidious comparisons obscure the artful "worlding" practices unfolding before one's very eyes (see Pollner 1987).

That, however, is not the only message we've taken from the tenet. A few years later, one of us (Gubrium) presented a research seminar at UCLA, where Pollner was in the audience. In the lively discussion following the presentation, graduate students and faculty members offered the usual critical comments and raised a variety of analytic challenges. At one point, someone suggested that Gubrium may have overlooked the "possible hidden meaning," as she put it, embedded in one of the conversational extracts he presented. The possible hidden meaning in this instance involved the alleged real motives behind what was being said. Pollner eventually jumped in, responded supportively, but also advised her that one "shouldn't argue with the members," challenging the empirical grounding of the suggestion. Once again, Mel was showing his respect for member's practices by cautioning not to put words (or intentions) in members' mouths, so to speak.

On still another occasion, following the daily proceedings at a research conference, we took a long, pleasant summer evening walk with Mel. Mel the "vaudevillian" kept us laughing, of course, but our stroll was interlaced with cogent questions and supportive advice regarding our research program. He appreciated the new directions we were taking, but, again bringing a humorous, light touch to the matter, he cautioned that we needed to take care not to slip inadvertently into arguments with the members lest their everyday wisdom and practical competence be overshadowed by our concerns. It was vintage Mel: smart, persuasive, yet open-minded, and standing fast to ethnomethodology's cultivated respect for mundane reason.

Like many others, we've long appreciated the usefulness of this position and the ways Mel recommended taking account of it. As ethnomethodologically-informed sociologists[1], we've heeded Mel's advice—often implicitly—but we've also tried to explore and expand its implications. In various ways, we've avoided arguments with the members in our own research but we have pursued ramifications of the tenet that are not immediately evident. We've asked ourselves, what does it mean concretely not to argue with the members, given the varied ways sociologists investigate and

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[1] Pollner himself has been called a "protoethnomethodologist" (Lynch 1993), implying a reluctance to abandon concern for standpoint and method as cornerstones of reasonable analysis in favor of a more radical postanalytic version of ethnomethodology (e.g. Garfinkel 2002).

describe the social world? Turning the question around, we've asked, what does it mean to argue with the members—to take exception to, or to ignore, what they say about themselves, others, and the world they live in? We've mused on the analytic products of such arguments. In particular, we've long been concerned with the contextual (indexical) construction of meaning and action and were systematically and comparatively considering how members themselves relate to context, opportunities, and resources for making meaning. We have, in a sense, looked behind member's backs, but in a fashion that respectfully asks "Where are they coming from?" without denigrating local demands for accountability. In the following pages, we first explore how the admonition to avoid arguing with members shaped Mel's own work. We then illustrate what happens when arguments break out. Finally, we turn to ways in which the tenet has been borrowed and honored, but also adapted, in service to other forms of analysis.

Before we begin, however, we offer some words of caution. First, we note that Pollner did not necessarily imply that researchers actually argued with members, presenting contrary opinions in face-to-face interaction with them. The tenet referred more generally to the analyst's practice of taking exception to or "debunking" mundane reason and everyday wisdom as part of the analysis of members' actions. We also note that just what constitutes being a "member" must remain problematic to some extent, because the idea of membership itself is continuously constructed in interaction and is subject to interaction's going concerns. What is clear is that membership—who is or isn't a member or actor in the immediate scheme of things—is part of the dynamics of social interaction, not something figured separately. This needs to be kept in view lest interactively unfolding senses of membership be replaced by other sources of assignment such as official definitions or structural and cultural designations (Francis and Hester 2004, pp. 1–3, 205–206, 210–211).

Heeding His Own Advice

"Don't argue with the members" has both analytic and empirical implications. Some of Pollner's earliest published work implicitly brings the tenet into play through his critique of Howard Becker's version of labeling theory (Becker 1963). Generations of sociologists recognize Becker's paradigm-building pronouncement that "The deviant is one to whom that label has been successfully applied; deviant behavior is behavior that people so label" (p. 9).[2] While this observation has clear ethnomethodological resonance, Becker goes on to juxtapose and implicitly critique what he calls the "common-sense premise" that there is something inherently deviant (pathological) about people or acts that violate social rules. Becker then offers a four-fold typology of deviant behavior (p. 20), including "conforming," the "falsely accused," the "secret deviant", and the "pure deviant."

Pollner (1978, 1987) recognized at least two ways in which the typology was arguing with the members. The first was obvious. In critically juxtaposing a

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[2] While Becker's *Outsiders* (1963) is widely cited as the inspiration for the development of labeling theory, Edwin Lemert (1951) and John Kitsuse (1962) had published earlier work featuring the same argument, as Becker himself admits.

common-sense (mundane) definition of labeling with his analytic version, Becker implicitly denigrated the common-sense version. His argument suggested a flawed members' version of deviance as compared to the more astute outsider's analytic prescription, without carefully considering the common-sense model for its mundane sensibility and utility. Becker's foray was not the ethnomethodological indifference that would encourage a more disinterested or appreciative view of members' practices, a stance that was characteristic of Pollner's own work.

Perhaps more importantly, Pollner noted that Becker believed he could spot deviance better than could members themselves. Pollner pointed out that Becker's four-fold typology explicitly violated Becker's own definition of deviance. The categories of falsely accused and secret deviant, Pollner noted, are conceptual anomalies—theoretical non sequiturs—since both rest on the premise that deviance inheres in something other than the act of labeling, and can thus be misidentified. If deviance is behavior so labeled, for example, it is impossible to have "secret" deviance, since no label has been applied. Similarly, there should be no distinction between the falsely accused and the pure deviant, since, by Becker's definition, both have been labeled deviant. From Pollner's perspective, Becker famous four-fold typology of deviance (Becker 1963, p. 20) could be reduced to a two-category typology: 1) "Deviant"—persons/acts responded to as deviant and 2) "Not Deviant"—persons/acts not responded to as deviants (Pollner 1987, p. 98).

Inadvertently, perhaps, Becker imported features of the common-sense version of deviance into his own model, tacitly incorporating the notion of rule-breaking or "real" deviance, then positioning the analyst as the arbiter of "correct" labeling. As Pollner (1978, p. 270) put it, Becker conceived of the community's (members') role in the labeling process as resembling "an umpire calling balls and strikes" who sometimes got it wrong according to the outside arbiter (e.g., the unidentified secret deviant or the innocent who is falsely accused). Disregarding his own definition, Becker in effect was "arguing with members" that he (the analyst) could better identify deviance than they could—using their own mundane criteria. While Pollner's central argument revolved around the revelation that Becker had actually appropriated features of mundane labeling into his own sociological theory, Pollner was also displaying the consequences of implicitly arguing with the members by attempting to debunk everyday practices.

Mel tried to practice what he preached in his own work. In addition to his research documenting mundane reason in a traffic court (Pollner 1987), his collaboration with Lynn McDonald-Wikler (Pollner and McDonald-Wikler 1985), especially, offered a fascinating glimpse of member's reality-constructing practices. In their words, Pollner and McDonald-Wikler conducted a "case study of a family's attribution of competence to a severely retarded child."[3] Their field work took them into a professional, clinical environment where a variety of mental health specialists had conscientiously diagnosed 5 ½ year old Mary as severely retarded. Nevertheless, members of the Mary's family insisted that Mary was "normal" and was just putting on an act of being retarded. Pollner and McDonald-Wikler's analysis is a tour de force in appreciating members' every day, indigenous practices for sustaining the competence of a fellow member.

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[3] We use the term "retarded" in our discussion to reflect its usage in the original research. The term "retarded" was clinically appropriate at the time Pollner and McDonald-Wikler conducted their research.

The question of arguing with the members arises from this study in several ways. Most notably, while there was ample clinical evidence that family members were deluded about Mary's condition, Pollner and McDoanld-Wikler avoided arguments through the way they approached the idea of locally constructed interactional competence, family membership, and, in particular, the competence of the retarded child to be a regular family member. They did not argue for or against the child's actual competence based on their field material, but rather investigated how family members' themselves visibly accomplished a *sense* of the child's competence. They could have provided independent conversational and ethnographic evidence to confirm or disconfirm the clinical staff's designation of incompetent membership, but chose to set that aside to investigate how the family members assigned competency in their everyday reasoning about the matter. Doing otherwise would have questioned the capabilities of family members' to judge the child's abilities, thus putting the researchers in the position of arguing with the members (which is exactly the position the professionals involved were obliged to take).

McDonald-Wikler first brought the case to Pollner's attention because it had been the object of curiosity and scrutiny by the staff at the Neuropsychiatric Institute for quite some time. Her initial interest was to understand the intractability of Mary's family in the face of a diagnosis that had been thoroughly "worked up" and discussed before Pollner and McDonald-Wikler arrived on the scene. The diagnostic practices through which Mary's retardation or "incompetence" were no longer available for investigation, so Pollner and McDonald-Wikler turned their attention to how Mary's family members managed to vigorously contend that Mary was, indeed, quite normal. As Pollner and McDonald-Wikler explain in their article,

> Family members stated that Mary was a verbal and intelligent child who malingered and refused to speak in public in order to embarrass the family. Extensive clinical observation and examination revealed Mary to be severely retarded and unable to perform at anywhere near the level of competence claimed by her parents and two older sibs. (p. 242)

Note that what in family member's reasoning was unusual behavior was "arguably" viewed by the consultants as retardation. The consultants' applicable vocabulary was already geared to produce findings of pathology; the family's claim was destined to be arguable. This difference implicated all family members. Not only were the consultants primed to argue with the child's existential status in the family, but they also were set to argue with the family's own competence in accepting their diagnosis. The clinical report suggested the diagnosis of *folie à famille*—a medical framing of perceptual incompetence extending to all family members—was a distinct possibility. The clinical gaze was ceaseless, set to argue with any and all who questioned the assessment of the child (see Foucault 1965).

Bracketing Mary's competence, Pollner and McDonald-Wikler posed different questions, centered on members' mundane reasoning about their child's competence in the family context. The nature of their questions was clearly informed by their commitment to avoid arguing with the members. The researchers were interested in how family members' ordinary reasoning served to gloss the child's behavior as competent, specifically what ordinary rules guided the family's interpretations of the child's behavior. They focused on the locally reflexive organization of competence

with the family without invidiously comparing family practices to clinical standards. They asked, by what mundane interpretive means did this happen? How, as a matter of interpretive practice, did family members construct Mary as bizarre in public but as an otherwise normal child at home?

> Our analysis of the available materials has been instigated by the following questions: How does the family do it? What sorts of skills, practices, and strategies are utilized to create and then [reflexively] "discover" Mary's competence? . . . The yield of our analysis has been a set of [interpretive] practices by which family members created Mary's "perfection." (p. 244)

Pollner and McDonald-Wikler identified six interpretive techniques, the everyday applications of which constructed and sustained Mary as a normal child with the ability to account for her conduct. These included "framing" whatever Mary did in the context of the home as normal; "postscripting" whatever Mary did after the fact as reasonably motivated; and "putting words" into Mary's mouth. If, from a clinical perspective, the application of such techniques might be construed as part of the folly of the family, the family itself might have been deemed pathological. But adhering to the tenet of not arguing with the members, the interpretive practices that the researchers unveiled stood as evidence of local reality-constructing practices that served to normalize Mary's actions and identity within the family environment.

This study eventually proved problematic from an epistemological point of view. Pollner and McDonald-Wikler might have inserted themselves into the local debate over the child's competence, but instead chose to examine dispassionately how evidence in this debate was accountably produced by one side (the family's). When the researchers submitted the article for peer review and publication, reviewers raised questions that harkened the "ontological gerrymandering" (Woolgar and Pawluch 1985) controversy that was brewing at the time (personal communication). The researchers were chided for taking an authoritative position vis-à-vis the "real reality" of Mary's situation. The criticism, however, was ill-conceived, considering the fact that Pollner and McDonald-Wikler remained agnostic regarding the issue of whether clinical diagnoses or family interpretive practices were more in touch with a "real" reality. As the researchers put it, their analytic approach undertook "close examination of the artful, minute, and continuous work through which what might be characterized as 'myth,' 'distortion', or 'delusion' from outside the family is rendered a reality for them on the inside" (p. 242). They carefully circumscribed the field of analysis—the inner interactional workings of the family—and avoided arguing about "real realities" with both family members and outsiders to the family scene. They left open the possibility that they might just as well have examined the clinical practices of the psychiatric diagnosticians for the ways in which they mundanely produced evidence of Mary's retardation. As Pollner and McDonald-Wikler noted, "The social constructionist attitude does not provide privileged exemptions for 'expert' or 'scientific' constructions of reality. The tacit practices by which clinicians develop, coordinate, use, and defend their versions of reality are as amenable to analysis as those of the families they study" (p. 242). Pollner and McDonald-Wikler had simply chosen to "bracket" one set of actors and practices as topics for analysis while remaining indifferent to other—competing—actors and

practices. They neither argued with the family members under study nor the clinicians, in whose practices they remained temporarily disinterested.

When Arguments Erupt

While Pollner tried to adhere to his own admonition, sociologists of the latter half of the 20th century often took the opposite tack. The proclivity to unmask or debunk everyday practices led to frequent arguments with the members, both tacit and occasionally overt. This is evident, for example, in a longstanding debate about the relative value of participant observation versus interviewing for uncovering the actual meaning of behavior. One of the earliest statements came from William Foote Whyte (1943), who touted the value of participant observation over survey interviews in the Introduction to his seminal ethnography, *Street Corner Society: The Social Structure of an Italian Slum*. Writing about the need for intimate knowledge of social life if we are to understand members' actions, Whyte complained about the "limited body of information" that surveys had generated about Cornerville, his pseudonym for the Boston slum he studied in the 1930s. He was annoyed by inadequacy of the information that members provided through survey interviews, implicitly suggesting that what members reveal by way of surveys is less authentic or revealing than information collected through participant observation. The complaint implicated the survey interview method as much as the information provided by the members being interviewed.

A decade later, in a now-classic statement, Howard Becker and Blanche Geer (1957) also championed the use of participant observation. In doing so, however, they highlighted an additional, but rather uncommon, way to argue with the members. According to Becker and Geer, interviewing shortchanges the everyday realities of meaning by taking members at their word rather than considering their actions. Interview reports, they argued, may be filled with falsehoods and exaggerations. Actions, observable as field events (what people actually do, not what they say or report about it), reveal better what members are up to. The authors are unmistakably clear about this.

> The most complete form of the sociological datum, after all, is the form in which the participant observer gathers it: An observation of some social event, the events that precede and follow it, and explanations of its meaning by participants and spectators, before, during, and after its occurrence. Such a datum gives us more information about the event under study than data gathered by any other sociological method. Participant observation can thus provide us with a yardstick against which to measure the completeness of data gathered in other ways, a model which can serve to let us know what orders of information escape us when we use other methods. (p. 28)

As Becker and Geer proceed to illustrate and valorize participant observation, they explain that the astute observer is better able to decipher social situations than members themselves. On one occasion, this admittedly invited

an actual argument with members. Becker et al. (1961) had conducted extensive field research on student culture in a medical school. As Becker and Geer note, the method of participant observation put them in the position to know better than members (in this case, the medical students) what might have transpired interactionally between medical students and their supervising resident physicians. According to one of the researchers' fieldnotes, "Before I joined the group, several of the students told me that the residents were "mean," "nasty," "bitchy", and so on, and had backed these characterizations up with evidence of particular actions" (p. 31). The researchers went on to argue that their observations told a different story. In this instance, the researchers actually argued in person with the medical students, a flagrant violation of Pollner's tenet.

> After I began participating daily with the student on his service, a number of incidents made it clear that the situation was not quite like this. Finally, the matter came completely into the open. I was present when one of the residents suggested a technique that might have prevented a minor relapse in a patient assigned to one of the students. . . . This student reported to several other students that the resident had "chewed him out" for failing to use this technique: "What the hell business has he got chewing me out about that for? No one ever told me I was supposed to do it that way." I interrupted to say, "He didn't really chew you out. I thought he was pretty decent about it." Another student said, "Any time they say anything at all to us I consider it a chewing out. Any time they say anything about how we did things, they are chewing us out, no matter how God damn nice they are about it." (p. 31)

As Paul Atkinson and Amanda Coffey (2002) point out in their chapter of the *Handbook of Interviewing* revisiting Becker and Geer's article, the latter were aware of the socially structured character of complaints and acknowledged the sociological relevance of "inaccurate" member claims and counterclaims in relationships of authority. The problem is that Becker and Geer were not interested in analytically naturalizing or topicalizing the inaccuracies. This would have turned the inaccuracies analytically into functional, even correct claims of a sort under the prevailing circumstances. Becker and Geer discounted the students actions (the claims, in this case) because they judged them to be false, but in doing so, overlooked an analytic opportunity to consider the claims as part of students' locally situated mundane reasoning and identity formation. They failed to take up the question of what the purpose of such claims might be in the circumstances. This would have turned the analytic table, so to speak, away from an argument with the members into an examination of student accounts rendered in particular contexts of accountability. Instead, Becker and Geer replaced the accounts with what they, as observers, claimed to know better from their fieldwork and left it at that.

Accounts, of course, are as discernible in interviews as they are through participant observation. Acknowledging this immediately levels the methodological playing field and devalorizes participant observation. As Pollner would undoubtedly remind us in this case, what members do with words in interviews is as genuine and scientifically valuable as what they do with words in more "natural" settings. Both

are fields of empirical material, neither of which warrants arguing with the members.[4]

In light of Becker and Geer's argument with the medical students, it is interesting to note that the new oral history may lean towards Pollner's stance regarding arguments over the truth of matters at hand. Interviews with living members of historical scenes, for example, are not evaluated exclusively in terms of their accuracy, but are considered in terms of what members do with words when they offer accounts of experience (see Cándida Smith 2002). Alessandro Portelli's (1991) essay "The Death of Luigi Trastulli: Memory and the Event" is instructive. Portelli takes interviewees at their word and analyzes accounts of steel worker Luigi Trastulli's death following a walkout in Terni, Italy on March 17, 1949 when laborers left a factory to protest the signing of the North Atlantic Treaty by the Italian government. Richard Cándida Smith (2002) describes Portelli's approach to working-class residents' interview accounts of the event.

> Their reconstructions of the past were factually wrong. Their accounts merged or scrambled events and at times referred to events that never occurred. In effect, their collective stories had created an alternative chronology that allowed them to maintain their own historical experience.
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> Portelli argues that chronological inaccuracy in the narrative helped the community maintain a sense of continuing to have a future and retaining the possibility of political resurgence during a time of retreat. . . . Portelli's analysis suggests that the community's ability to maintain identity rested on a utopian, historically inaccurate, but culturally effective myth of the past. The narratives kept alive an alternative future that preserved for several decades the possibility of independent, worker-based action, even if, for the most part, members of the community were actively participating in the reconstruction of Italian society around international markets. (pp. 720–21)

Cándida-Smith is not saying that Portelli ignores the inaccurate chronology, but rather considers what function the inaccuracy serves in the broader context of collective memory. The workers' narratives, despite (or because of) their inaccuracy, serve to sustain community identity, Portelli explains. The recontextualized inaccuracy becomes the focus of interest, not the ostensible inaccuracies themselves. Portelli does not aim to *replace* the inaccurate chronology with a more accurate one, but rather *displaces* the "inaccuracy" into a different context and asks what function is serves there, which is what Becker and Geer themselves might have asked about the operating "inaccuracy" of student complaints in the context of student culture. It's an important distinction, to which we return in the last section of the article.

Longstanding Reductionist Habits

Two longstanding analytic habits regularly lead social researchers to inadvertently argue with members. Both reduce members' actions to popular understandings about

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[4] The idea of the universally accountable nature of members' actions was still in its infancy when Becker and Geer wrote their article and they would be unfairly criticized for not taking this up (see Heritage 1984, chapter 6).

the substance of members' actions (see Silverman 2000). One habit views members' actions substantially as forms of inner life; the other views actions substantially as matters of culture or social structure, which were once called "social forces." As resources for interpreting members' actions, these habits may be useful as long as researchers don't replace mundane understandings with what is figured instead to be their "real" substance. This might amount to viewing actions as the result of, say, motivation or perception in the case of replacements with the substance of inner life. Similarly, the substance of culture or social structure—say norms or socioeconomic status—might be conjured up as the animating forces behind behavior.[5]

The first of these reductions was broached at the start of this article, in our recollection of a seminar participant's reference to the possible "hidden meaning" of comments in a conversational extract. By referring to unobservable psychological states and processes that might have been at work, the comment invited the use of inner states such as hidden motives and underlying feelings as analytic resources for understanding members' actions. It was an analytic reduction in the sense that "the interaction order" (Goffman 1983) was reduced to the vocabulary and related realities of inner life. (See C. Wright Mills' [1940] seminal article on what he called "vocabularies of motive" for a nonreductionist and topicalized approach to member accounts of intention.) It was Erving Goffman's view, with which Pollner would agree, that the interaction order has its own operating logic, replete with the contingencies of everyday life. To reduce it to inner life in this case eclipses a whole world of human experience that might otherwise be understood on its own terms. In doing so, the researcher implicitly argues with members' overt hearable statements and observable actions. Examples of this reduction are legion; it's what one would expect, given that members' actions in our society are commonly interpreted in terms of a vocabulary of intentions, thoughts, feelings, values, unconscious motives, identities, and similar elements of inner life.

The second reduction also is a popular source of replacement. In this case, the substance of members' actions is constructed in terms of a vocabulary of allegedly larger social forms, such as culture, society, class, and status. This reduction overshadows how members themselves use such constructs in interpreting their lives and actions. We can forestall such arguments, however, by concertedly referring to culture, class, and other macroscopic social forms as "vocabularies" rather than as agentic social forces. This means viewing the vocabularies as accounts and topicalizing them, rather than treating them as resources that trivialize ordinary usage and mundane reason.

Everyday social settings routinely deploy inner lives or social forces as interpretive resources, taking us beyond research applications. Such commonplace reductions are the very stuff of a "form of life," as philosopher Ludwig Wittgenstein (1953) would put it. Their vocabularies are the way most of us reference the realities of life, so it's natural to turn to them to give substance to members' actions. The rub is that we *all* are members—sociologists included; the trick of the tenet for

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[5] Such replacements are versions of what ethnomethodologists refer to as confounding the *topics* of one's research with the *resources* for studying them (Zimmerman and Pollner 1970; Pollner 1987), that is, turning aspects of a proper topic of research—for example, what members "do" with inner life and culture or social structure—into an analytic toolkit for interpreting and assigning meaning to members' actions.

researchers is to recognize this in our analytic work and become disciplined about the distinction between topic and resource. Topicalizing members' everyday resources and practices requires attending to how members "do" experience, how they construct their inner lives and social worlds, among other possible domains of experience. Pollner's tenet reminds us of the importance of taking members' work seriously by applying a stance of ontological indifference in order to see how the mundane constructs what it is commonly reduced to.

Replacement Versus Displacement

As we have seen, arguments with members emerge when researchers replace or challenge members' actions or meanings with their own or other exogenous understandings. Arguments are not necessarily forthcoming when researchers *displace* mundane understandings to other contexts in order to gain broader understandings of the locally sensitive deftness of mundane reason. Replacement, as we have seen, might take the form of ascribing hidden meanings or cultural prescriptives to members' actions. In contrast, researchers would *not* be arguing with members if they tracked the displacement of members' actions—whether in talk, activity, or both—to alternate contexts, that might construct talk, behavior, and meanings in contrasting terms, rendering them substantively different. Such displacement allows researchers to view mundane reason and everyday practices in light of alternate social environments and practical purposes at hand. This renders everyday practices more visible by virtue of the possible contrasts that emerge across contexts, allowing for the description of situational differences.

Does Pollner's tenet allow us to capitalize analytically on displacement rather than replacement in order to bring contextual matters into view as topics in their own right, rather than treating the matters as everyday resources? We believe Mel would think this was possible. Beyond his characteristic open-mindedness, we also note his willingness to bracket particular aspects of experience in order to gain analytic purchase on other aspects of experience in his own research into *folie à famille*, Alcoholics Anonymous support groups, and traffic courts. While Pollner stood fast to ethnomethodology's cultivated respect for mundane reason, his empirical work indicated that he knew the importance of recognizing the varied situated realities of members' lives. Elsewhere, we have called these contextual parameters the *whats* of membership (see Gubrium and Holstein 1997; Holstein and Gubrium 2000). It is important to recognize that members' actions and concerns vary in discernable ways across time and social space. Members don't do the same things with words everywhere, but discernibly adapt how they use and apply them to the circumstances at hand. The self, for example, is discursively constructed—talked into being—in distinctly different ways under the auspices of an Alcoholics Anonymous support group than in other realms of everyday life (Pollner and Stein 2001; Holstein and Gubrium 2000).

Pollner recognized that the admonition not to argue with members had to be strategically but temporarily set aside in order to take account of the distribution of the forms of life to which the tenet otherwise applied. Forms of life are systems of usage which are shaped by circumstance as much as they are ways of forming

everyday realities; they are socially situated language games, as Wittgenstein viewed them (Wittgenstein 1953; see Holstein and Gubrium 2011). Accordingly, the *whats* of everyday life are as important as the *hows*. In documenting the *whats*, the *hows* must necessarily (but temporarily) be relegated to the analytic background, taking along with them the tenet about not arguing with the members.

The *hows* of everyday life—"the work whereby a world per se and the attendant concerns which derive from a world per se . . . are constructed and sustained" (Pollner 1987, p. 7)—have been ethnomethodology's central concern. But the displacements of diverse *whats* demand attention as well. For example, the *hows* of social interaction, such as the everyday interactional work of constructing the competence of a severely retarded child, as we saw earlier, wouldn't make much sense if we paid no attention to what was at stake in the matter. We need to account analytically for, among other things, the institutional context of the diagnosis of the child as retarded, the professional obligation to assess what the family called "bizarre," the family's sense that basically their child was normal, and the possible diagnosis of *folie à famille*. These *whats*, among many others, are highly consequential for those concerned.

Exercising a concern for the *whats* is not arguing with the members. Rather, it is a concern with figuring the situated realities from different members' points of view and describing what everyone concerned is up against (or has available to them) in constructing the identity of the child in question. In practice, the *whats* reflexively inform the *hows*. Displacements differentially condition the work of constructing the child's identity. The consequences of the identity constructed cannot be understood from the interactional *hows* alone. The six interpretive techniques or *hows* that Pollner and McDonald-Wikler identified in their study could apply to any number of circumstances and, in that regard, do not exclusively apply to the normalization of retarded children. The personal and societal relevance of the *hows* must be discerned from the *whats* of the matter.

Conversely, the *whats* at issue, such as a family's concern, a neighbor's casual interest, or the professional responsibility of a clinical diagnostician were not fixed realities. They were subject to the identity work of members of various circumstances. Family members applied a vocabulary and definitional technology that constructed a normal child from within their "family circle," as it were. They didn't use a medical vocabulary or diagnostic technology, but, under different circumstances, they could have and, in a medicalized society like ours, they likely would have.

From Pollner and McDonald-Wikler's article, we have no way of knowing how the professionals involved applied a medical and diagnostic vocabulary to link related categories with the empirical particulars of Mary's case; this was largely accomplished before the case came to Pollner's attention. The researchers instead chose to focus on how the family constructed Mary's competence. But, in principle and given the opportunity, Pollner and McDonald-Wikler (or some other researchers) could have studied the social practices through which a medically-appropriate diagnosis was assembled. This would turn them to the narrative environment of the clinic, its predominant discourses, and the related local, professional, and disciplinary discursive resources for understanding and defining Mary's situation.

We do know from other studies of professional categorization processes that the development and application of *hows* in everyday life is multifaceted and artful. What results can be amazingly complicated and inventive (see Garfinkel 1967,

Buckholdt and Gubrium 1978). The *whats* of the matter are reflexively related to the *hows* in that substantive outcomes, such as being or not being normal or retarded, are not a straightforward matter of linking "facts" with available or preferred categories, since both the "facts" and the "categories" are subject to the interpretive contingencies of their construction. The contextual features of any setting are themselves locally constructed members' accomplishments. Thus the *hows* of the setting are reflexively implicated in *what* the setting comes to be.

The broad analytic challenge of displacement is to document the interplay between the *hows* and the *whats* of mundane reason. Systematically and dispassionately attending to both the *hows* then the *whats* (or vice versa), while recognizing that their reflexive interplay is the constitutive basis of the actions and circumstances under consideration, can safeguard against arguing with the members. In our own work, we have relied on a procedure we've called "analytic bracketing" (Gubrium and Holstein 1997, 2009; Holstein and Gubrium 2011) to deal with the reflexive relationship between the *hows* and the *whats* of everyday life. In this procedure, the researcher alternately orients to everyday realities as both the products of members' interpretive practices and as the substantive resources from which realities are reflexively constructed. At one moment, the researcher may be analytically uninterested in the circumstances of an everyday interactional setting in order to document their mundane constructions. In the next analytic move, he or she brackets mundane practices in order to describe the local availability, distribution, and/or regulation of resources and constraints for reality construction. The analysis of the constant interplay between the *hows* and *whats* of reality construction mirrors the lived interplay between constitutive social interaction and its immediate context, resources, restraints, and going concerns.

The strength of the tenet "don't argue with the members" is its ability to rein in the sociologist's tendency to look for members' meaning beyond mundane attributions and reasoning. This strength can be extended to other aspects of reflexively constituted reality. Pollner wrote about the way what he called "narrative maps" "pre-presented the dimensions and denizens of a social world" to neophyte members of Alcoholics Anonymous (Pollner and Stein 1996, p. 204). He took seriously the ways in which mundane processes of self construction were conditioned by organizational stocks of knowledge and institutionalized ways of talking selves into being (Pollner and Stein 2001). If Mel were still here to help coin a tenet to turn our analytic attention to the *whats* as much as to the *hows* of membership, he also could very well have advised "Don't forget the 'things' that could concern the members." Taken together, the two tenets keep us focused on the reflexive interplay between the *hows* and the *whats* of everyday realities, extending analysis across the substantive landscape of mundane reason.

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