

Talk is Cheap: Ethnography and the Attitudinal Fallacy¹

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Talk is Cheap: Ethnography and the Attitudinal Fallacy

This paper examines the methodological implications of the fact that what people say is often a poor predictor of what they do. We argue that many interview and survey researchers routinely conflate self-reports with behavior and assume a consistency between attitudes and action. We call this erroneous inference of situated behavior from verbal accounts the *attitudinal fallacy*. Though interviewing and ethnography are often lumped together as “qualitative methods,” by juxtaposing studies of “culture in action” based on verbal accounts with ethnographic investigations, we show that the latter routinely attempts to explain the “attitude-behavior problem” while the former regularly ignores it. Because meaning and action are collectively negotiated and context-dependent, we contend that self-reports of attitudes and behaviors are of limited value in explaining what people actually do because they are overly individualistic and abstracted from lived experience.

Ethnography and the Attitudinal Fallacy

In the book *Sentiments and Acts*, Deutscher, Pestello, and Pestello (1992: 15) argue that there are three methods “through which social scientists can seek to understand human behavior: (1) We can observe it in process; (2) we can view written or otherwise artifactual records of people; and (3) we can ask questions and listen to their answers.” The authors go on to note the irony that sociologists prefer “the verbal approach” even though “it is actual behavior in which we are more often interested” (ibid.). Based on the assumption that verbalized attitudes (or sentiments) are manifestations of “tendencies to act,” sociologists routinely proceed to draw conclusions about people’s behaviors based on what they tell us.

Yet we are only justified in inferring situated behavior from verbal accounts if what people say is consistent with what they do. In everyday life, people know that this is often not the case: it is a truism that “talk is cheap” and that “actions speak louder than words;” parents command their children to do as they say, not as they do; and we are exhorted to put our money where our mouth is, practice what we preach, and walk the walk—not merely talk the talk. C. Wright Mills (1940a: 329) long ago recognized the wisdom of these popular idioms, calling the disparity between “talk and action” the “central methodological problem of the social sciences.”

Subsequent psychological research on attitude-behavior consistency (ABC) has repeatedly demonstrated that people’s verbal responses at time 1 are often unrelated to their observed behavior at time 2 (Wicker 1969; Gross and Niman 1975; Schuman and Johnson 1976). Moreover, social psychologists have long argued that the attitudes and dispositions of people might be poor predictors of their behavior when compared to the situations they are in (Ross and Nisbett 1991). Not only is it the case that people commonly act in ways that are inconsistent with their expressed attitudes, they also routinely provide inaccurate accounts of their past activities. Given these findings, one might expect that contemporary sociologists would be wary of using

methods that privilege verbal accounts if they aim to explain social behavior. However, many sociologists simply ignore the ABC problem and make little effort to validate the assumption that the attitudes they measure are associated with the behaviors they are interested in (see Pager and Quillian 2005). In so doing, they commit what we call the *attitudinal fallacy*—the error of inferring situated behavior from verbal accounts.

To illustrate the attitudinal fallacy and explore its methodological implications, we take the sociology of culture as our reference point. We focus on this field for two reasons. First, cultural sociology is a popular research area in which a variety of scholars have attempted to map the relationship between sentiments and acts using different methodological approaches (e.g., Swidler 1986; Vaisey 2009). Second, we believe that the approaches and debates that typify cultural sociology—e.g., its emphasis on meaning-making (Lamont 1992), cognition (DiMaggio 1997), frames (Young 2004), and schemas (Vaisey 2009)—are indicative of scholarly trends in other areas of the discipline (e.g., Brubaker et al. 2006; Edin 2000). By comparing account-based studies of “culture in action” with ethnographic studies, we show how the latter routinely attempts to explain the ABC problem while the former regularly disregards the problem.

Because ethnography is often conflated with interviewing under the rubric *qualitative methods*, and because space constraints preclude a thorough review of the vast array of survey methods, we focus our critique of account-based studies on interviews. However, we also analyze Stephen Vaisey’s (2009) use of forced-choice questionnaires to study “culture in action” in order to show how even the best-intentioned survey research is prone to the attitudinal fallacy.

The ABC problem implies a clear distinction between methods that observe behaviors and methods that observe verbal responses. Under such a classification, ethnography becomes more closely aligned with experiments and audits, and interviews find greater affinity with

surveys. Given ethnography's central concern with understanding the actor's point of view, we do not mean to imply that survey- and interview-based research on attitudes ought to be abandoned, or that accounts are totally unrelated to behavior. As Alison Pugh points out, interviews in particular are valuable for "their capacity to excavate and interpret emotions, which serve to animate, situate and connect... levels of consciousness" (2013: 44). Interviews can also be valuable for helping to reconstruct the temporal and narrative structure of events that have already occurred (e.g., Erikson 1976), or practices that the researcher cannot observe (Katz 1999).² But we do argue that, since the link between talk and action is variable and problematic, scholars who base their claims on accounts must either be clear that they are analyzing not what people do, but rather what people say they think and do, or they must demonstrate that accounts of action of the kind they are analyzing have been shown to be predictive of observed behavior (e.g., polling and voting). To reiterate, our premise is not that sentiments are meaningless, but that they cannot be taken as proxies for action without positive evidence of their predictive capacity.

This suggests the need for many account-driven studies to develop a more explicit rationale for their research, one that acknowledges their oft-limited capacity to draw conclusions about social action and that spells out precisely what it is that self-reports can tell us about the social world. While such caveats, cautions, and efforts to triangulate data were once a customary part of the methodological conversation (e.g., Dean and Whyte 1958; Merton 1940), contemporary sociology seems to tolerate scholarship that habitually infers situated behavior from verbal accounts without even token justification. Indeed, Duneier (2007: 36-7) notes the increasing use of "quotation-driven studies" in poverty research to "discover the reasons that

² Additionally, by conducting interviews during in situ observations, we can gain a window into the tacit knowledge that is put to work in interaction (Faulkner and Becker 2009).

people did things in their lives;” and Pager and Quillian (2005) observe that almost all studies of racial attitudes conflate accounts and action. Anthropologists too have criticized the ways in which attitudes (accessed through interviews) result in “the production of highly individualized, socially decontextualized talk” (Ortner 2003: 16).

Building on the interactionist premise that signification and action are collectively negotiated and context-dependent (Blumer 1969), meaning that they “belong” to situations as much as individuals, our analysis shows that self-reports of attitudes and behaviors are of limited value in explaining what people actually do because they are overly psychological and abstracted from lived experience. What people say or do is contingent on their “definition of the situation;” we thereby argue that the study of situated face-to-face encounters—what Goffman (1983) called the “interaction order”—is a key component of explaining social action and how it relates to accounts. As Becker and Geer (1957: 28) remarked long ago, ethnography gives us more information about social action than data gathered by other methods because it directly observes behavior, the events which precede and follow behavior, and explanations of a behavior’s meanings by “participants and spectators, before, during, and after its occurrence.” Thus, a powerful warrant for ethnography (Katz 1997)—and for allied methods that study *in situ* behavior (e.g., Pager and Quillian 2005)—is that it provides a systematic way of dealing with the problems created by the attitudinal fallacy.

To be clear, we are *not* claiming that ethnography should be the gold standard for all sociological research. The method’s limitations have been well documented (e.g., generalizability beyond those actually studied, difficulty accounting for social structure); and different methods are suited for answering different questions (e.g., the study of macro changes in birth, death, and fertility rates is best left to demographers). The relationship between saying

and doing is one methodological challenge among many in the social sciences. We argue that ethnography is one of the most effective methods for addressing this challenge, but we do not thereby argue that ethnography is the best method for *all* methodological challenges. Instead, we hope this paper helps scholars be clearer about what kinds of claims their methods can (and cannot) support.

We *are* claiming that, given that (1) one of sociology's central concerns is explaining social action, and (2) people's accounts often differ from their actions, ethnography is in many instances a more useful method than interviews and surveys because it observes behavior directly and uncovers discrepancies between saying and doing. While ethnography may still fall short of offering definitive answers to *why* people do what they do, at the very least it tells us *what* people actually do and is better able infer to motives than research based wholly on verbal data.

To the extent that cultural sociologists in general, and account-driven researchers in particular, do not make claims about social action and why people do what they do, then they are exempt from the central criticism of this paper. However, we do explore an important caveat to this exemption: account-based studies that explicitly claim to only be interested in meaning making but that either (1) make implicit assumptions that what people say is related to what they do, or (2) are taken up by other researchers as evidence for behavioral claims.

THE ATTITUDE-BEHAVIOR CONSISTENCY PROBLEM

In a well-known study carried out in the 1930s, Richard LaPiere traveled around the United States with a young Chinese couple in order to ascertain whether businesses would discriminate against them. Out of 251 visits to hotels, restaurants, and campgrounds, only once were they

denied service. Yet when LaPiere called these same proprietors six months later to ask if they would allow a Chinese guest, only one responded “yes.” How could it be that almost everyone allowed the Chinese guests to stay, but when asked almost everyone told LaPiere they could not? This inconsistency led LaPiere (1934: 237) to argue, “If social attitudes are to be conceptualized as partially integrated habit sets which will become operative under specific circumstances and lead to a particular pattern of adjustment *they must, in the main, be derived from a study of human beings behaving in actual social situations.*”

In two book-length reviews that the authors say were inspired by LaPiere, Irwin Deutscher (1973) and his colleagues (Deutscher et al. 1992) elaborate on the problematic relationship between actors’ *sentiments* and their *acts* and lament the general failure of sociologists to employ methods that delineate the relationship between the two. In the decades following the publication of LaPiere’s landmark study, they find that only a handful of sociologists took the ABC problem—and its implications for methods—seriously. One of them was Robert Merton, who observed—like LaPiere—that ostensibly “incompatible judgments” regarding racial attitudes are often made by the same person, and that “Northerners” may treat blacks “less favorably” than they talk about them while “Southerners” may treat blacks more favorably than they talk about them. Merton critiqued the logic of surveys because they relied upon the “dubious rationalist assumption” of “internal consistency” both across an individual’s range of attitudes and between one’s opinions and overt behaviors (1940: 20-22).

Dean and Whyte argued (1958: 34) that it is wrong to assume that “there is invariably some basic underlying attitude or opinion that a person is firmly committed to, i.e., his real belief.” It is incorrect, therefore, to believe “that if we can just develop shrewd enough interviewing techniques, we can make him ‘spill the beans’ and reveal what this basic attitude

really is” (ibid.). They give the example of a young housewife who, in an interview, subscribes to careful budgeting of the family finances but who subsequently buys a dress “which is out of line with her financial plan” when out on a shopping trip with a close friend “with whom she feels a good deal of social competition” (p. 35). It is not meaningful, the authors contend, to say that her behavior invalidates her opinion; nor does it make sense to ask what her “real” attitudes toward budgeting are. The lesson is that attitude-behavior discrepancies cannot be reduced to dissembling or measurement error. Rather, we should consider both stated opinions and behavior to be “*highly situational*,” and the interview “must be seen as just ONE of many situations in which an informant may reveal subjective data” (ibid.). With interviews and surveys, Dean and Whyte conclude, “we are merely getting the informant’s picture of the world as he sees it” and “as he is willing to pass it on to us *in this particular interview situation*” (p. 34). For example, when Dean’s observations of factory workers (1958: 40) revealed that they exaggerated when asked, “How often do you attend union meetings?,” he reasoned that the respondents did “not really answer *this* question” but instead answered—largely unconsciously—the question, “How often do you *picture* yourself attending union meetings?” or “How often is it *appropriate* for you to attend union meetings?” So, while verbal reports can help illuminate the actor’s “orientations to interaction” (Dean 1958: 44), Dean and Whyte caution that we must triangulate them with other sources of data [e.g., observations] before making inferences about social action.³

It is notable that a number of Dean and Whyte’s contemporaries advised the same methodological prescription, indicating that sociologists were at one time more diffident about making behavioral claims with verbal data. Warriner (1958: 165), for instance, observed that

³ More recently, Desmond has made a similar point when noting that asking people about the support they receive when they are evicted is not indicative of the aid actually extended to them. “Statements about social support reveal more about the public persona one wishes to maintain than about the nature of the support itself” (2012: 1303).

residents of a small rural community in Kansas publicly affirmed an “official morality” that drinking alcohol is wrong even though many of them drank moderately in the home. Rather than dismiss their accounts as lying, Warriner argued that this ideological expression was “true” in the context of their orientation toward the community—members desired to make their public behavior conform to the sanctioned norms of the collective. The town even voted to remain officially “dry,” a move Warriner saw as functioning to maintain group solidarity. Warriner argued that both the private consumption of alcohol and the public proscription of drinking were “relevant and appropriate” to the distinct contexts (what Goffman called “back stage” and “front stage”) in which these behaviors manifested. And, echoing Dean and Whyte, he concluded that the relationship between verbalized sentiments and social action can only be understood by examining actors *in situ* and in relation to collectives. This argument dovetails with Wicker’s (1969: 76) classic review of the relationship between attitudes and behaviors in psychology, in which he found, “little evidence to support the postulated existence of stable, underlying attitudes within the individual which influence both his verbal expressions and his actions.” It is for this reason that Becker and Geer (1957: 28) refer to ethnography as “the most complete form of the sociological datum” and chide interviewers for making assumptions about the relationship between accounts and actions without observing behavior. From interviews alone we cannot know what actually happens in interaction, but only what people think about situations and how they feel about them. Even the researcher’s ability to access that social reality can be problematic, as the lack of context can make it hard to for the interviewer to verify that she and the interviewee have achieved intersubjectivity.

It should be noted that competent ethnographers also gather verbal accounts, but the inferences that they draw about the social significance of such accounts come from situating

them in relation to observed interactions that occur before and after the interview. Without an intimate understanding of context, even the best-intentioned interviewer can misunderstand the meaning of actors' words and accounts. This leads Becker and Geer to conclude that "the process of inference involved in interpreting interviews should always be made explicit and checked, when possible, against what can be discovered through observation" (p. 31). Where this is not possible, as is often the case, "conclusions should be limited to those matters the data directly describe" (ibid.).

While critics are right to point out that every method has its strengths and weaknesses and produces "different kinds of data designed to answer different kinds of questions" (Trow 1957: 33), it is apparent that many survey and interview researchers today insist on trying to answer questions whose scope exceeds their data. Pager and Quillian's (2005) review of racial discrimination studies makes clear that contemporary survey and interview researchers routinely treat accounts as reasonable approximations, or predictors, of action; and the fact that these authors still refer to LaPiere's research in 2005 as the benchmark study of the ABC problem speaks volumes about sociology's "abandonment" of this issue (p. 361). While psychologists take ABC more seriously (for a recent review, see Glasman and Albarracin 2006), most of their findings are restricted to the lab. Though the ABC is as high as .85 in studies of voting behavior (Schuman and Johnson 1976), Pager and Quillian point to meta-analyses showing that there is often no relationship between reported attitudes and *in situ* behaviors (Deutscher 1966; Wicker 1969), and that "no simple formula" can be employed to help us specify when attitudes and behaviors are highly correlated.

Like LaPiere's research, Pager and Quillian's experimental audit study profoundly illuminates the potential disconnect between sentiments and acts. They found that employers

expressed a far greater willingness to hire black male ex-offenders when asked in a vignette survey than in the real-world scenario when these men actually applied for jobs. Pager and Quillian note, “Measures from surveys often are accepted as an adequate proxy for behaviors, with little effort to validate this assumption” (p. 255). Had Pager and Quillian relied on survey data to estimate the likelihood of an employer calling back a black ex-offender they would have found it to be 61.7%. But in their audit study the callback rate was 14.7%. In short, their vignette questions were useless in predicting the actual behavior of their participants; they further demonstrate that even surveys that use an experimental design to control for social desirability bias may not allow us to make strong inferences about the actual level of hiring discrimination.

Although vignette studies were designed to address the ABC problem by simulating real-world scenarios, Pager and Quillian find that “hearing a description of a hypothetical black offender is quite different from seeing a young black man approach one’s business in search of employment” (p. 371). “Even if the employer genuinely believes that she or he would hire the applicant described in the abstract vignette, when confronted with the situation in real life, the contingencies of the hiring process may render hypothetical scenarios irrelevant” (ibid.). Recalling Becker and Geer, Pager and Quillian contend that, at most, surveys can “tell us something useful” about how respondents think about issues like hiring practices. It seems that certain sentiments and acts—whether discriminatory (Pager and Quillian 2005), accommodating (LaPiere 1934), or otherwise—may commonly be activated only through *in situ* interactions, making it difficult or impossible to capture them through self-reports. The difference between self-reports and situated behavior, Pager and Quillian conclude, “represents a meaningful discrepancy between two legitimate realities” whose resolution “represents an important focus of sociological investigation in its own right” (p. 372).

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Pager and Quillian's study reaffirms the forgotten lesson of LaPiere and his contemporaries: that what we say and what we do are strongly influenced by situational factors. This implies, we posit, that the recent spate of interview studies aiming to capture the "cognitive frames" that allegedly shape action (e.g., Young 2010), along with survey innovations designed to better measure respondents' "real" behavioral dispositions through vignettes and other indirect measures (e.g., Vaisey 2009), miss the mark. To escape the attitudinal fallacy, we must study interaction. Blumer argued that, in constructing lines of action, "the individual is engaged in defining what confronts him in his situation," and that how one "acts overtly is formed primarily by what he takes into account and how he molds what he takes into account." The "deficiencies" of most sociological paradigms, including those that reason from attitudes to behavior, "lie in the failure to accommodate this intervening process of constructing the act" (Quoted in Deutscher et al. 1993: 45). Attitudes and action are collectively negotiated and context-dependent, meaning that they "belong" to interactions as much as to individuals (Warner and DeFleur 1969).

Given that people "have *situated moral and other meanings* for many different types of situations and feel relatively little need to relate the situations to each other via abstract meanings" (Douglas 1970: 385), the methodological implication is that the direct observation of face-to-face encounters in "natural settings"—a *sui generis* social reality that Goffman (1983) called the "interaction order"—is often the most effective technique for understanding social action and its relationship to accounts. After reviewing a number of field studies and experiments, Deutscher et al. (1993: 153) conclude that "a considerable proportion of the variance in human behavior can be explained by efforts (conscious or unconscious) on the part of people to bring their sentiments and acts into line, not with each other, but with what they perceive to be the sentiments and acts of others in the immediate situation." Because interviews

and surveys are often designed to elicit people's most private opinions and to avoid "contamination" by context effects, they rarely offer opportunities for documenting "real expressions of attitude and overt behavior"—i.e., the sentiments and acts that people actually express in everyday social situations (Deutscher et al. 1993: 93). In the next section, we explore how account-based approaches to "culture in action" compel researchers to take self-reports at face value and commit the attitudinal fallacy; we then counterpose these approaches with ethnography, which enables researchers to systematically observe social action and build sociological explanations that can potentially account for discrepancies between saying and doing.

CULTURE IN ACTION AND THE ATTITUDINAL FALLACY

The methodological contradiction that we observe in cultural sociology echoes the field of sociology in general: many of the questions of interest are about action and social relations, but the methods deployed do not observe situated behavior and are individualistic. This problem seems to stem, at least in part, from a widespread assumption in the discipline that people's actions are guided by a durable set of schemas, frames, or repertoires and that verbal methods are the best—perhaps even the only—way to access these cognitive processes. In order to illustrate the fallacious logic of this premise, we analyze how a variety of cultural sociologists have modeled the relationship between accounts and action—particularly by focusing on the individual as the unit of analysis and assuming that culture operates through cognition (and is thereby revealed through interviews). We then show how Stephen Vaisey's (2009) attempt to circumvent the ABC problem in studies of "culture in action" by augmenting interviews with forced-choice surveys still commits the attitudinal fallacy by relying upon reports of activity to

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infer situated social action. We conclude that most studies of “culture in action” lack meaningful data on behavior and produce explanations that are, ironically, highly individualistic by virtue of studying individuals in isolation rather than in social relations.

Action as Frames and Repertoires: Tool Kit Theory, Culture as Cognition, and Interview Data

Ann Swidler (1986) helped make culture one of the most significant concepts in sociology. Her groundbreaking work conceived of culture as a “tool kit,” wherein culture is a set of “resources that can be put to strategic use” (DiMaggio 1997: 265) by actors as a means to solve the question of how to act in the world. Swidler contends, “Culture influences action not by providing the ultimate values toward which action is oriented, but by shaping a repertoire or ‘tool kit’ of habits, skills, and styles from which people construct ‘strategies of action’” (p. 273; cf. Mills 1940b).

Swidler’s original articulation of the “tool kit” echoes the concept of habitus in emphasizing patterned behavioral tendencies (habits, skills, and styles), yet most scholars who have taken up this theoretical framework have ignored Swidler’s early attention to behavior and instead focused on the “cognitive” dimensions (e.g., Lamont 1992; Young 2004). The result has been part of a larger “cognitive turn” in sociology, particularly in the sociology of culture (see Brubaker et al. 2006; DiMaggio 1997; Harding 2007; Zerubavel 1997). Such an approach “rejects the assumption that culture generates values that drive social action” (Kaufman 2004: 340) and instead argues that “culture organizes thinking by providing the frames and schemas that individuals and their groups use to process and translate their environment” (Fine and Fields 2008: 136). This development was instrumental in guiding sociology away from the Weberian/Parsonian emphasis on “value orientations” and toward an emphasis on how actors

make and narrate meaning (see Lamont 2000; Swidler 2001) through the employment of readily available “collective representations” (cf. Alexander and Smith 2002).

Intuitively, culture as cognition makes a lot of sense. But, problematically, the cognitive-tool kit approach explains and analyzes social processes strictly at the individual level; relationships are secondary. Though the cognitive turn helps move us toward Durkheim’s collective representations, the emphasis is on how individual actors use these representations and not, say, on how such representations might be collectively constituted (cf. Eliasoph and Lichterman 2003). “Culture in action” – as it has been deployed in the broader literature -- is an individual activity wherein actors deploy an *a priori*, externally constituted culture to endow their myriad life experiences with meaning. Those parts of Swidler’s toolkit theory that scholars have drawn upon tend to ignore behavior and instead focus on the claim that, “Culture is used by actors” (Swidler 1986: 284); and in “settled” cultural periods, “People naturally ‘know’ how to act” (p. 280). The problem with this notion is that, as Garfinkel (1967) noted, situated action is never simply guided by *sense already made* but rather is always a process of *sense in the making*,⁴ and Mead (1934) observed long ago that the meaning we grant to activity is often collectively negotiated through *interaction*. Indeed, our own critique of the psychological bias of such a model is inspired in part by scholarly developments made by Swidler herself, who has criticized the tendency in cultural sociology to gather individual-level data (Swidler 2008, Watkins and Swidler 2009).

The neglect of relationships in the tool kit/cognition model, and its emphasis on individual “sense-making,” is intimately connected to the method its proponents have usually utilized: the interview. If we subscribe to the idea that culture is a set of frames, representations,

⁴ We take this phrasing from a helpful anonymous reviewer.

and repertoires that actors draw on to build “strategies of action,” then it seems fitting to interview actors in order to see how they invoke culture to account for their behavior and define the boundaries of group membership (Lamont 2000). One warrant for interviews, then, is grounded in W.I. and D.S. Thomas’s (1928) deeply sociological dictum, “If men define situations as real, they are real in their consequences.” By assessing actors’ life histories, stated beliefs, and folk theories of social structure, interviews offer a window into the normative and cognitive frames that actors use to explain their actions and anchor their identity (see Gerson 2009; Swidler 2001; and Young 2004). In doing so, however, this method reifies the erroneous assumption that “culture in action” can be explained at the individual level. More problematic, the method is hard-pressed to determine how the frames and opinions that people emphasize to an interviewer influence their everyday action—or whether people do what they say they do.⁵

As detailed above, there is evidence that many attitude-behavior discrepancies are not the result of measurement error, but rather because actors often have little difficulty professing worldviews that seem inconsistent with their actions (Deutscher et al. 1992). In the sociology of culture, particularly in research that emphasizes the “talk” of actors, these findings should give us serious pause. It seems that there are many times in which situations that people define as real to a researcher are not manifested in consequential behavior. Thus, it is problematic when scholars surmise that the frames actors invoke during interviews reflect durable, “real” beliefs that effect particular actions. A caveat to the Thomas dictum which we believe verbal methods gloss is that “interpretive frames are seldom constant or context-free; rather, the definition of the situation is often worked out *in situ* and through interaction” (Khan and Jerolmack 2013: 17). As

⁵ Another potential pitfall of interviews is that they may force people to provide an opinion on matters they have thought little about or to which they are not committed. Analyses that treat each respondent as a case risk giving such flippant responses equal weight or validity as opinions that are well thought-out or to which one is strongly committed.

Duneier (2007: 36) points out, many interviewers “write as if there is a clear correspondence between confident statements by subjects and reality;” and they “increasingly use interviews...to discover motivation.” Indeed, the conflation of sentiments and acts lies at the heart of some of the most well-known interview-based studies of culture.

In the landmark book *Habits of the Heart*, for example, the authors rely primarily on interview data to argue that America’s culture of individualism is eroding people’s commitment to community (Bellah et al. 1985). The interviewees’ accounts vividly illustrate the authors’ assertion that Americans commonly justify their lifestyle choices through appeals to personal preferences rather than drawing on “any wider framework of purpose or belief“ (p. 6). However, the authors repeatedly imply—in the absence of observational data—that the interviewees’ accounts are not mere justifications but serve as their guides for action. Because an interviewer finds that “Brian’s” explanation of his newfound commitment to family rests on a “fragile [moral] foundation,” we are told that his “commitments themselves are precarious” (p. 8); because the tenants’ rights activist “Wayne” cannot elaborate on “the ways in which scarce goods should be distributed in a complex society,” we are led to believe that his commitment to social justice may waver (p. 19); and because “Margaret” claims to value the “freedom to be left alone,” the authors conclude that she is destined for a life of “being alone” (p. 23). In each of these cases, we are provided with no empirical data that suggest that any of these cognitive frameworks are actually reflected in social action. In short, motives of action are implied rather than demonstrated; and behavioral consequences of attitudes are imagined rather than observed.

In *Money, Morals, and Manners*, Michèle Lamont (1992)—who is far more cognizant of the attitudinal fallacy than most within this tradition—argues that inferential leaps between observed discursive claims and implied outcomes are not so troubling because speech acts are in

themselves behaviors. Lamont writes, “because symbolic boundaries are often primarily enacted at the discursive level... the data gathered in interviews can be interpreted as behavioral data” (p. 18). This is a useful and important clarification—speech *is* a form of behavior. However, this does not mean that any other kind of behavior can thereby be inferred from such speech. While we do not doubt that the discursive symbolic boundaries that Lamont identifies in her French and American subjects are indeed the most salient (e.g., that “the French in general are clearly less money-oriented than Americans” (p. 65)), there is scant evidence that these symbolic boundaries are defended or enacted beyond their verbal articulation. For instance, research shows that Americans over-report church attendance by about two-fold but that no such bias exists for Europeans (Brenner 2011). This suggests that Americans have a deep desirability to identify as religious persons, but that their actions are not in accordance with their sentiments. While this thesis supports, and perhaps helps explain, Lamont’s findings that morality is a more resonant cultural frame for the Americans in her sample than the French, it also forces us to recognize that the importance of moral frameworks of speech does not necessarily mean that people act in ways consistent with that speech. If we look to the example of Lapiere (1934)—where the symbolic racial boundary against allowing non-whites to stay at a hotel was clearly articulated but not enacted—or Pager and Quillian (2005)—where the symbolic anti-racist boundary was espoused but racist actions were taken, then we have very good reasons to be concerned about whether speech behaviors serve as the basis of other actions.

While Lamont carefully avoids making explicit claims about situated behavior in *Money, Morals, and Manners* and her next book *The Dignity of Working Men* (2000), her theoretical supposition that “symbolic boundaries take on a constraining character and pattern social interaction in important ways” once they become “widely agreed upon” implies that studying

symbolic boundaries matters because of the insights it ultimately gives us into how people sort themselves into groups that exhibit “stable behavioral patterns of association” (Lamont and Molnár 2002: 168-9). Many scholars who cite her work on symbolic boundaries have certainly presumed this connection (e.g., Emirbayer and Mische 1998; Peterson and Kern 1996). Yet in order to understand whether and how symbolic boundaries relate to social action, we must see how they are enacted in everyday life—not merely catalogue their presence in interviews.

Although interview researchers routinely *say* they are primarily interested in “sense-making” and not action, many *do* wind up making inferences about situated behavior from accounts. *The Minds of Marginalized Black Men* (Young 2004) exemplifies such an approach. Alford Young, Jr. divides his 26 interviewees into three groups: those who report having had little, some, or a lot of social contact with people and institutions outside of their neighborhood. While Young claims upfront to only be interested in how his subjects articulate their worldviews, he winds up arguing, based on the “high social contact” group’s comparatively heightened ability to discuss race and class-based obstacles to success, that they have acquired forms of cultural capital through their diverse weak ties that they can use to formulate better “personal strategies” for getting ahead than those men in the “low social contact” group (p. 182, 194). This argument is speculative, as Young does not observe these men interacting with the labor market. In fact, there is reason to believe, given that all of the men claimed to be unemployed and to have few if any job prospects, that demonstrating a more complex understanding of structural inequality to an interviewer does not result in better social outcomes.

In addition to the problem of conflating sentiments and acts, interview-based studies such as Young’s are often forced to build claims on self-reports that cannot be verified. Young notes that, in response to a question about whether they possessed any skills, 18 of his participants

mentioned being able to perform some kind of manual labor. He concedes that they “did not have a lot to say about how they acquired their skills” (p. 174) and that “I had no grounds to actually find out what these men could or could not do in terms of hands-on work” (p. 242). However, the veracity of the men’s claims about their skills is actually central to Young’s argument, because he contends that what prevents them from gainful employment is not their ability but rather their lack of formal certification. Further, Young argues, this dilemma becomes part of the men’s stock of knowledge and shapes how they approach the labor market. These claims suffer from two critical problems: (1) the reader has no ability to judge what—if any—skills these men actually possess; and (2) even if the men possess said skills, the book contains no empirical evidence that these men are seeking—or being denied—work (cf. Liebow 1967).

Given that attitudes and accounts may not be tightly coupled with action, to what extent can interview data serve as a valid proxy for social action? The explanations generated by interview data are either *ex-post* or *ex-ante*—that is, they can only reveal actors’ justifications for actions that take place in unobserved contexts (the past or the future). What is the significance of culture—or any sense-making—to sociologists if it is manifested in individuals’ private thoughts or self-reports, but not in situated interaction? The primacy of *social* action in our discipline (Parsons 1937; Weber 1905) implies privileging phenomena that are verifiably constituted in everyday life rather than in “the conditions of sterility that are deliberately structured for the interview situation” (Deutscher et al. 1992: 93; see also Eliasoph and Lichterman 2003: 743). It seems that, in the end, many interviewers are not interested in verbalized frames or schemas per se either—rather, they consider actors’ sense-making and self-reported behaviors to be representative or predictive of social action. The problem is that, rather than trying to specify if and when this is the case, they commit the attitudinal fallacy by simply surmising that talk

accords with action. Yet it is only by sampling acts over time and across contexts, rather than sampling accounts from one-shot interviews, that we can understand how talk and action relate.

Action as Self-reports: Vaisey and Survey Data

While an extensive review of survey research on “culture in action” is beyond the scope of this paper, we touch upon Stephen Vaisey’s work here for two reasons: (1) like us, Vaisey recognizes the problem of relying upon interview-based accounts to infer social action; and (2) unlike many survey researchers, he makes an explicit attempt to address the ABC problem.

Vaisey (2009) argues that, because interviews only engage participants on the level of discursive consciousness, they cannot capture the unconscious dispositions, or implicit moral schemas, that may actually motivate action.⁶ Thus, interviews in which participants produce ad hoc justifications for their actions are inadequate for analyzing “culture in action.” We need a process-oriented method that is attuned to the ways that practical and discursive consciousness interact in everyday life. Perhaps counterintuitively, Vaisey turns to questionnaires for a solution.

Vaisey contends that “forced-choice surveys are better suited [than interviews] to the study of the culture-action link” because they “present the respondent with situations that are homologous with everyday decision-making processes” (2009: 1689). Because respondents use minimal cognitive effort to answer survey questions and instead rely on heuristics and intuition, Vaisey reasons, their answers tap into practical consciousness. He suggests that if talking about mental processes with an interviewer is akin to describing a criminal suspect to a sketch artist, then answering survey questions is like choosing a suspect out of a lineup.

⁶ Vaisey’s theory of culture harks back to normative orientations and demands that we take motivating values seriously again.

To demonstrate the superiority of forced-choice surveys, Vaisey turns to data from the National Study of Youth and Religion (NSYR). During interviews, Vaisey finds that the NSYR respondents sketched vague, even contradictory, logics in explaining how they make moral decisions. That is, interviewees did not appear to employ deliberative moral reasoning (Vaisey 2009: 1698). In comparison, Vaisey uses a survey question about “how would you decide” what is right or wrong to measure teenage respondents’ “most accessible or salient moral schema” (p. 1691). Vaisey then tests to see if he can predict self-reported behavior (three years later) on the basis of these moral schemas, and he finds that the “choice of moral script in 2002 is a very good overall predictor of behavior in 2005.” (p. 1703).⁷ Vaisey concludes that the “how would you decide” question taps into a tacit moral disposition that interviews miss, and that this disposition guides subsequent morally relevant behaviors.⁸ These results, he argues “suggest that fixed-response surveys play a vital role in inquiry about how meanings shape action” (p. 1705). Vaisey thereby attempts to make a direct link between sentiments and acts through survey data.

Yet while the self-reports of Vaisey’s survey respondents at T1 and T2 show high reliability, we have no measuring stick by which to gauge their validity. This is because surveys do not observe activity; they collect accounts of activity. Ironically, Vaisey critiques interview research for ignoring action, even though the only action his method allows him to observe is the

⁷ The behaviors examined include self-reports of alcohol and drug use, cheating on school assignments and cutting class, and keeping secrets from one’s parents.

⁸ We do not interpret these findings as evidence that interviewees can never articulate motives, or that forced choice surveys are necessarily a better option. The conditions between the NSYR survey and interview data are not the same. The survey asks a hypothetical question, “If you were unsure of what was right or wrong in a particular situation, how would you decide what to do?” The interview asks for justification of a real activity, “Has there ever been a time when you were unsure of what was right and wrong in a particular situation? How did you decide what to do?” Asking teenagers to justify their actions in a real situation that was morally ambiguous is not equivalent to asking them how they might act in an imagined scenario. It is not surprising that narrated justifications of action in a difficult, real-world scenario are vague compared to the clarity of imagined situations.

checking off of boxes—the attitudes and behaviors that he is studying are both gathered through the same longitudinal survey. It is only by ignoring all of the evidence that the connection between self-reports of behavior and actual behavior is variable and problematic that Vaisey can claim a major methodological improvement over interviews. Yet like the interviews he critiques, Vaisey’s approach unquestioningly treats self-reports of a specific behavior as tantamount to the actual behavior; and, ignoring LaPierre’s (1934) lesson about the situational character of behavioral intentions, Vaisey (2009: 1689) presumes that survey questions presenting respondents with “situations that are homologous with everyday decision-making processes” will elicit the “real” decision that they would actually make when confronted with such a situation in everyday life. Yet perhaps the thing that the consistency between respondents’ answers to questions at T1 and T2 can be said to most convincingly demonstrate is that the two data-gathering situations are homologous with each other.

Consistent situations may indeed be likely to elicit consistent responses, but this is not evidence that such responses are consistent with activity outside that situation itself (recall our point about religious attendance from above). Further, maintaining the claim that culture is primarily “inside” of actors (e.g., unconscious moral dispositions) leads to a conceptual trap: either outcomes are observed and culture is the implied *explanans* (e.g., Weber [1905] 1992),⁹ or culture is observed or somehow measured and social action—the *explanandum*—is implied (e.g., Vaisey 2009). In short, these methods require that we either observe the antecedent of action and infer its outcome, or observe the justification of action and infer its motivation. Neither of these approaches is empirically sound, and both are tied to the methodological problem of taking individual-level talk as a proxy for situated action.

⁹ (e.g., Protestants are where capitalist action is, so it must be their religious values that drive their capitalist action).

ETHNOGRAPHY, INTERACTION, AND CULTURE

Theoretical-methodological approaches to explaining social action that rely upon data not sensitive to *in situ* relations are mired in “an image of culture”—and social life in general—“as located in individual experience” (Watkins and Swidler 2009: 162). Perhaps more troubling, they commit the attitudinal fallacy as a matter of course. We need methods that enable us to model social action as it is lived—collectively and through practice in “microsituations” (Fine and Fields 2008). This provides a powerful warrant (Katz 1997) for employing ethnography in the service of social analysis. Ethnography—or participant observation—is concerned with explaining when and how both discursive and tacit schemas/frames shape behavior; it observes meaning-making and action *in situ*; and it is explicitly process-oriented. Importantly, ethnography goes a step further. Rather than studying accounts and actions individualistically, ethnography situates them in an interactive context. Ethnography does not come close to solving all of our methodological challenges, nor can it be used to observe all social phenomena of interest. But because it observes people saying and doing things together over time and in natural settings (rather than in a single, contrived temporal moment), ethnography is one of the most effective tools for studying the relationship between meaning and action.¹⁰

¹⁰ Because our agenda is to use cultural sociology as a lens for examining the methodological implications of the ABC problem, and not to advance cultural theory, we will only mention briefly that ethnographers have made important contributions to explanations of “culture in action” by showing how “culture is defined, created, and transmitted through interaction” (Fine 1979: 733; see also Faulkner and Becker 2009; Fine and Fields 2008). Eliasoph and Lichterman (2003), for instance, develop the concept of “group style” to show how situated actors use and adapt “a broader cultural repertoire” (p. 740) to their “habitual styles of interacting” (p. 773). Specifically, they show how two groups of community activists used the “language of individualism” (the cultural repertoire) in drastically different ways (according to their group style) as they sought to make “public-spirited community involvement meaningful” (p. 736). Such ethnographies make a compelling case that it is difficult to understand culture in any meaningful way apart from the particular social situations in which it is instantiated.

Many ethnographers operate on the basic premise that firsthand observations of people acting and interacting in the course of their everyday lives can yield more valid data about behavior than self-reports. While ethnographers are susceptible to the very same problems with subjects' self-reports as other methods, they have a way of dealing with this challenge: situated behaviors themselves serve as a "validity check" against the reports of subjects. Ethnography can place actors' accounts in the context of what actors *do* on a day-to-day basis, when all the constraints of their lives are operative (see Becker 1996). Venkatesh (2002), for instance, describes how his extended time in the field made clear that many of the accounts and behaviors that he observed when he first entered a public housing project were disingenuous. Over time, his informants saw his research as a form of "hustling" similar to their own survival strategies. This led the gang members and community organizers to begin trading "true" information (i.e., information that he later empirically verified) for services that Venkatesh might provide, and to allow him to tag along as they engaged in high-stakes negotiations with each other and the police—situations where they were not in a position to freely posture for Venkatesh as they had previously done.

The ethnographer's evaluation of the validity of the saying and doing that she observes is not grounded in her own expertise or "objectivity," but rather in how others in the situation respond. Such punctilious observations under real world conditions give ethnographers a warrant for believing that the behaviors and meanings that they record are a close approximation of everyday "reality."¹¹ Thus, to continue Vaisey's earlier analogy, if "talking about our mental processes with an interviewer is like describing a criminal suspect to a sketch artist," and if

¹¹ Capturing the "ordinary world" and the actor's point of view are the paramount goals of ethnomethodology, which is skeptical of methods and claims that are abstracted from the lived experience of actors (Garfinkel 1967).

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“answering survey questions is like picking the suspect out of a lineup” (2009: 1689), exemplary ethnography is like gaining intimate entrée into a criminal’s social world, observing the planning and execution of the crime, and then seeing how the criminal makes sense of the crime after the fact. Indeed, for fieldworkers who study criminals (e.g., Bourgois 1996; Contreras 2012; Venkatesh 2008), ethnography is not *like* this—it *is* this. Rather than simulating action, ethnographers participate in and observe it. Many have internalized Deutscher’s (1973) prescription to ascertain sentiments and acts from the same population, under natural settings.

Ethnographers share some of Vaisey’s concerns about the limitations of interview data. Citing the exemplary research of Elliot Liebow (1967), Duneier (2007: 36) distances himself from approaches like those we critique by arguing that in-depth fieldwork highlights how talk and action are “dissimilar units that can only be understood in relation to one another.” Ethnographers are not content to take their participants’ attributions of cause and effect at face value. At its best, ethnography carefully adjudicates between what actors say—in interviews or in conversations with each other—and what they actually do in the contexts of their everyday lives; and it exploits such comparisons to uncover motivation. For example, Liebow’s (1967: 116) poor black male participants accounted for their broken marriages “in terms of their personal...unwillingness to adjust to the built-in demands of the marriage relationship;” in particular, drawing on an available cultural script, they portrayed themselves as “dogs” who could not remain monogamous. But Liebow challenges these “self-serving” accounts by placing them in context; his observations reveal that the men dissolved their marriages to avoid being confronted by their own failures as breadwinners and role models for their families. It is only through careful observation over time that Liebow was able to discover these discursive deceptions and understand the men’s motivations behind both their actions and their accounts.

Ethnography can also analyze how unconscious cognitive and behavioral dispositions may shape behavior. Additionally, ethnographers are able to explain how such dispositions arise from the accumulation of situated practices over time. Influenced by practice theories of sociology (Bourdieu 1984; Giddens 1984) and their phenomenological and pragmatist precursors (Garfinkel 1967; James 1907; Schutz 1974), ethnographers who aim to capture the lived experience of their participants routinely depict and deconstruct tacit and embodied knowledge (see Katz 1999). Through sustained participation in the lives of her subjects, the ethnographer can actually witness and even experience the formation and/or activation of dispositions or schemas (for compelling ethnographic depictions of habitus and schema, see Wacquant 2004 and Brubaker et al. 2006, respectively). For example, only by becoming a forest firefighter was Matthew Desmond (2007) able to dispel the myth that these men are drawn to the occupation because of its ostensible thrills and status. Rather, Desmond demonstrates that the men's rural, working-class roots preconditioned their bodies and minds to be ready and willing to perform the unglamorous, back-breaking labor of the forest. Their lifetime of experience working with their hands and learning to be self-reliant in the wilderness (e.g., hunting, camping) resulted in a primary "country masculine" habitus, which the US Forest Service then handily shaped—via institutional socialization—into the specific habitus of a wildland firefighter.

Desmond's case illuminates one way that ethnography can uncover the hidden dispositions that may influence social action. "Ethnographers can give a temporal structure to data collection that may put them as close to inherently elusive causal contingencies as any research can" (Katz 2002: 459). By following their participants over time—often for several years—ethnographers can capture the development, transformation, and enactment of dispositions and competencies in their participants in a way other methods cannot (see Black

2009, Bourgois and Schonberg 2009). Careful ethnographers also “implicate observations in different places in order to specify the features in a given situation that were meaningful to and taken into account by the actor as contingencies of his or her conduct” (Katz 2002: 446). By accounting for variation through sampling across actors, times, and contexts the ethnographer tightens and strengthens her explanations.¹² For example, when Desmond (2007) encounters a less competent and ostracized firefighter who comes from a middle-class, urban background, he gains confidence in his argument of how the country masculine habitus shapes behavior.

Finally, and most importantly, the guiding principle behind most ethnography is that our social world is generated and maintained through interaction. Because interaction involves the coming together of lines of action of two or more individuals, the unit of analysis for many ethnographers is often the collective act (Becker 2004). One implication of this conception of social order as locally constituted (Blumer 1969; Fine 2003; Garfinkel 1967; Goffman 1983) is that whatever dispositions or repertoires of thought and action people may bring to a social situation or reveal to an interviewer, their *in situ* actions are guided and constrained by the “demand characteristics” of the situation itself (including the actions of others); the resultant collective activity—and the meaning of the activity—is often largely a product of negotiations among the participants but may not be what any of them had intended (Faulkner and Becker 2009; see also Fine and Fields 2008). Many ethnographies can be read as case studies of how dispositions, beliefs, order, and culture are made and remade through negotiating the situational demands of everyday face-to-face interaction. This project entails deconstructing the “taken-for-granted understandings people share which make concerted action possible” (Becker 1996: 61). These understandings often exist below the level of conscious awareness, perhaps only coming

¹² We are referring here to analytic induction, which uses negative cases to build and modify explanations until they can account for all of the relevant observed phenomena (Katz 2001: 465).

to the surface when interactions go awry or when social norms or group dynamics are in flux. Where do these sentiments come from? How do they motivate behavior? Why do they persist? What kinds of social action result? What are their consequences? These are the bread and butter questions that ethnographers often ask, and the answers go straight to the heart of understanding the relationship between accounts and action and explaining attitude-behavior discrepancies. Most significant, such explanations take the situated character of social action seriously by observing actors in the conditions of their lived lives rather than in conditions constructed by the researcher. Ethnographies show us how shared understandings and habits of practice emerge and are put to use to solve the mundane problems of collective action (Katz 2002). This is a pragmatist, situational conception of social action. Before we conclude, let us quickly take up two ethnographic cases that illustrate many of the points we have made.

In the book *Unequal Childhoods*, Annette Lareau (2003) examines how social class influences the lives of children. In interviews, she found that working-class and middle-class mothers “express[ed] beliefs that reflect a similar notion of ‘intensive mothering’ (p. 236).” Their harmonizing accounts, however, belied the divergent parenting styles that Lareau discovered through “naturalistic observation.” She finds two distinct “cultural logics,” defined as “sets of paired beliefs and actions” (p. 236-7), of child-rearing—logics that parents could not routinely articulate to interviewers because they were so taken-for-granted, but which manifested in “the rhythms of their everyday lives (p. 31).” Middle-class parents structured virtually every waking moment of their children’s time, attempting to foster their children’s talents by funneling them into organized sports, music lessons, and other extra-curricular activities. They also tended to reason and negotiate with their children rather than issue directives to them. Working-class

parents, on the other hand, gave their children more room to structure their own lives; yet they laid down firm rules and boundaries that clearly established parental authority.

Lareau finds that the two different cultural logics of parenting congeal in children, over time and through interaction, as two distinct dispositions: the middle-class children exhibit a sense of *entitlement*, while the working-class children exhibit a sense of *constraint* (p. 31). Lareau does not infer these dispositions from accounts—she witnesses them as practices enacted in routine face-to-face encounters: middle-class children are comfortable across a range of interactional settings and are assertive with authority figures like teachers and doctors, while working-class kids are more circumspect outside of their home or neighborhood and defer to authority figures. By watching how teachers, coaches, and other institutional actors routinely reward the middle-class children's entitled disposition, Lareau moves beyond speculation to empirically demonstrate the mechanism by which class cultures contribute to social inequality. Her observations of real-world scenarios allow her to build an explanation of the relationship between attitudes and actions, and examine their everyday consequences, that would be impossible with methods that simply gather accounts within a temporally bound moment.

While we have emphasized culture throughout this paper, our contention is that almost all careful ethnographies capture the situated and relational character of social life and construct analyses that untangle the relationship between sentiments and acts. Certainly, fieldwork alone does not guarantee that researchers will critically assess the accounts that subjects give. Some fieldworkers commit the attitudinal fallacy by treating accounts and action as equivalent forms of data, constructing “quote-driven” rather than “context-driven” ethnographies (Duneier 2007). But work like Alice Goffman's (2009) exemplifies the potential of ethnography to uncover and explain ABC discrepancies. Drawing on six years of fieldwork, she sets out to document how the

status of having a warrant out for one's arrest shapes the social life of young black men in an impoverished Philadelphia neighborhood. "The fact that some young men may be taken into custody if they encounter the authorities," Goffman writes, "is a background expectation of everyday interaction" (p. 344). Between 2002 and 2007, for instance, her participant "Mike" spent over three years in jail after repeatedly being picked up for minor infractions such as not paying court fees. Of the 139 weeks that he was not incarcerated, he spent 87 weeks on probation and 35 weeks with a warrant out for his arrest; and he attended court 51 times.

Goffman powerfully demonstrates how the "people, places, and relations that [the men] formerly relied on, and that are integral to maintaining a respectable identity, get redefined as paths to confinement" (p. 344) when they are wanted: police raid their parents' and girlfriends' homes, lie in wait at their place of employment to arrest them, and even handcuff them at the hospital while their girlfriends are in labor. The men's accounts reflect this observed reality. When Mike drew his girlfriend's ire by failing to show up to Parents' Day at his son's school, he pointed to his bench warrant as the reason: "Do she want me to get locked up? How I'm going to be there for my kids if I'm locked up?" Mike's friend "Steve" repeatedly pointed out how his warrant prevented him from working; and "Reggie" complained how his wanted status blocked him from getting a job, obtaining a driver's license, using the bank, and renting an apartment.

On the surface, the men's accounts seem to explain their social situation: they say that being wanted prevents them from doing the things that "clean" people should do, and Goffman's observations show that wanted men are indeed arrested while trying to do those things. However, Goffman warns that, "when wanted men (or social analysts) imply that being wanted is the root cause of their inability to lead 'respectable' lives, they are stretching" (p. 351). Mike, the man who claimed to skip Parents' Day because of a warrant, "proudly attended" a parent-teacher

conference after paying his son's overdue school fees. Goffman shows that the warrant allowed Mike to avoid going to Parents' Day without admitting that he did not want to go because he could not pay the school fees. Similarly, Goffman's observations reveal that Mike and Reggie made no effort to get a job, apartment, or license even when they were in good standing with the authorities. She concludes that, "what they said amounted to reasonable 'half-truths' (Liebow 1967) that could account for their failures, both in their own minds and in the minds of others" (p. 353). Recognizing that she could not rely on the men's vocabularies of motives to explain their situated action (cf. Young 2004), Goffman used years of observation across multiple settings to build an explanation of their social situation that reconciles the saying/doing disparity: being wanted did help undermine "already tenuous attachments to family, work, and community" (p. 339), but the men also strategically used being wanted as a face-saving excuse for obligations that they would not have fulfilled anyway.

CONCLUSION

In this paper, we outlined what we call the *attitudinal fallacy*—the error of inferring situated behavior from verbal accounts. While sociologists in previous generations were somewhat attuned to the ABC problem (e.g., Dean 1958; Merton 1940), scholars today seem to routinely presume that advances in survey methodology and the analytical turn toward cognition and sense-making enable them to uncover durable and "real" attitudes—i.e., behavioral intentions—without analyzing or observing social action. The faultiness of this logic lies in the fact that both stated opinions and behavior are often "highly situational" (Dean and Whyte 1958: 35). We have used cultural sociology to show how the attitudinal fallacy is endemic in account-based studies; we might have drawn upon almost any area of sociology to make the same point. In this

conclusion, we outline two important methodological issues related to the attitudinal fallacy: (1) relying upon methods that privilege individual-level accounts; and (2) gathering data in a research setting and assuming its generalizability to unobserved situations.

Methodological instruments that rely upon individual-level accounts of behavior can remove the “social” from social action. They often imply that meaning is made by or within the individual, rather than between individuals, and they suggest that to understand activity we simply need to aggregate the accounts of representative actors. We have argued that this is a mistake; interaction is central to understanding activity. People do not make meaning or act alone—they do so in relation to other people and in particular situations. Therefore, research methods must be sensitive to the relational quality of social life (Emirbayer 1997). The individualist bias that is often inherent in survey and interview instruments is deeply tied to the attitudinal fallacy. When interviewers or survey researchers ask individuals to talk about a question of interest, they cannot assume that a subject’s talk is predictive of future action or an accurate account of previous behaviors—making this type of inference requires subscribing to the faulty psychological notion that one’s situated action is the result of internal, stable, and “real” attitudes and opinions (which we can access through rigorous questionnaires) rather than a product of situated, collective negotiation. Perhaps ironically, many social psychologists have argued that attitudes are poor predictors of action (Wicker 1969; Gross and Niman 1975; Schuman and Johnson 1976), referring to the tendency to overstate the role of individuals’ dispositions or attributes in explaining behavior and to undervalue situational determinants as the *fundamental attribution error* (Lee and Ross 1991). Our argument is not that we learn nothing by attempting to access people’s cognitive frames, sensibilities, or reflections—as we have noted, interview studies of meaning-making may well provide us with insights into the content of

people's inner self (Pugh 2013). We instead argue that such knowledge cannot be taken as an unambiguous representation of what people do when interacting with others. Obviously, social action is not the only phenomenon of sociological interest; but if action is of interest, then the best—and sometimes the only—way to know about it is to actually observe activity.

This points us to our second problematic: survey and interview observations are abstracted from the situations about which many claims are ultimately being made. We have argued that actions and their meanings are situationally contingent. Therefore, to gather information from actors in situations that are different from those that one seeks to make claims about requires scholars to show that their claims apply to such unobserved situations. Interviews and surveys can tell us a lot about people, including how they narrate and justify their attitudes and actions to a researcher. These data are not trivial. But if action is contingent upon situational conditions, then claims about how people act in such situations must be based either upon empirical evidence gathered within them or upon a demonstration—rather than an assertion—that the research situation is indeed analogous to the subjects' day-to-day lives.

We highlight the ethnographic method as one of the tools that social scientists can use to help them move beyond both of the above problems. This should not be read as an insinuation that ethnography solves all methodological problems, or that all social phenomena should be studied ethnographically. However, we do assert that there are important and distinct advantages of ethnography: rather than gather and then aggregate individual-level accounts, ethnography takes as its unit of analysis the “collective act,” wherein action is carried out conjointly with others. Further, the method recognizes that such conjoint action is embedded in social situations that are not entirely of actors' own making. This is certainly not to say that ethnography ignores the self-reports, narratives, frames, or boundaries that actors verbally enact. But, rather than take

them as indicative of behavioral intentions, ethnography systematically examines them in relation to real—i.e., situated—action. This intensive “sampling” of interactions over time and across contexts allows ethnographers to evaluate the veracity of actors’ claims and create sociological explanations that account for discrepancies between saying and doing.

Still, survey data (and to a lesser degree, interview data) have a quality that ethnography lacks: generalizability to a population. This is hardly trivial – one need only think of political polling to grasp its importance. Yet while survey and interview data prioritize the representativeness of individuals from whom they can aggregate information, it is often ambivalent to the representativeness of the situation within which information is gathered. As a result, though the individuals may be representative, the unique context of the interview/survey situation means that the findings produced cannot be assumed to be generalizable to everyday life—we can infer that individuals not sampled would provide a similar distribution of self-reports as those in the sample, but we cannot infer that these self-reports are indicative of how people will act under social situations not sampled. Ethnographic work prioritizes the observation of social action—including accounting—within the real world rather than a research context. Little effort is made to ensure that such data are “representative” in the sense that actors can stand in as an unbiased sample of the population. While this is indeed a limitation of the method, ethnography’s epistemology is different: it does not select representative samples of individuals, rather it samples everyday situations and interactions. Part of our argument is that such situations and interactions can provide more valid data about social behavior than even the most representative sample of individuals whose traits are then aggregated, particularly when such a sample is used to gather individual attitudes within a single research situation. Even when ethnographies fall short of explaining *why* people do what they do, they nonetheless provide a

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firsthand living record of *what* people actually do in their everyday lives. This seemingly trivial virtue becomes quite significant in light of the attitudinal fallacy.

If scholars seek to maintain that survey and interview methods provide greater generalizability than ethnography, we ask, “to what?” We have implied that the attitudinal fallacy means that these methods privilege reliability over validity. (Again, our critique applies mainly to the category of studies that infer social action from verbal data, which is something not all interview and survey research does). They can generalize about mental states that are only occasionally predictive of social activity, in situations that are important for researchers but not necessarily for subjects’ day-to-day lives. While survey and interview methods may offer a window into “attitudinal frames,” it is important that they also demonstrate, rather than assume, the impact of these frames upon activities in the world—lest they cede any rightful claim to explaining social action and circumscribe their concerns to how people account for their sentiments and acts in a contrived setting. Rather than ignore the ABC problem, it is paramount that such methods find a way through it.

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