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Jack Katz¹

Abstract

There is unexamined potential for developing and testing rival causal explanations in the type of data that participant observation is best suited to create: descriptions of in situ social interaction crafted from the participants' perspectives. By intensively examining a single ethnography, we can see how multiple predictions can be derived from and tested with field notes, how numerous strategies are available for demonstrating the patterns of nonoccurrence which causal propositions imply, how qualitative data can be analyzed to negate researcher behavior as an alternative causal explanation, and how folk counterfactuals can add to the evidentiary strength of an ethnographic study. Explicating the potential of field notes for causal explanation may be of interest to methodologists who seek a common logic for guiding and evaluating quantitative and qualitative research, to ethnographic fieldworkers who aim at connecting micro- to macro-social processes, to researchers who use an analogous logic of explanation when working with other forms of qualitative data, and to comparative-analytic sociologists who wish to form concepts and develop theory in conformity with an understanding that social life consists of social

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interaction processes that may be captured most directly by ethnographic fieldwork.

Keywords

ethnography, participant observation, analytic induction, race and ethnic relations, qualitative and quantitative methodology, causal analysis

It is often a challenge for ethnographers to tailor their causal arguments to variation in the data they collect. Ethnographers frequently offer their fieldwork as demonstrating the “mechanism” of a causal relationship which has its origins in areas of social life that reach beyond the time and place of participant observations. For example, many urban ethnographers refer to historical and quantitative evidence of the effects of “deindustrialization” (Anderson 1992:240, citing Kasarda 1989 and others; Wacquant 2003, p. x, with citations to Wacquant and Wilson 1989:18, 44, 46) in order to frame and add significance to texts that consist primarily of participant observation conducted in low income, African American settings, all within the “post-Fordist” stage of history. Original fieldwork is offered to show differences in how individuals respond to a common contemporary environment, but no ethnographic evidence is offered to describe variations of either cause or effect over historical time.

Some ethnographers use original fieldwork data to show variations in both cause and effect but do not acknowledge alternative explanations, much less develop evidence that would allow for ruling them out. An example is Hochschild’s (1983:183) well-known description of the contrasting mobilization of male and female identities in bill collecting and flight attendant work. Claims of deleterious “results” from gendered power differences at the job site were bolstered by quoting from a sex therapist “who had treated some fifty flight attendants.” But the biographical origin of damage could not be documented with the researcher’s original observations of training programs and interviews about on-the-job work experiences. Left unaddressed was the possibility that the damage was due to gendered differences in popular culture and socialization that predated labor market entry. Perhaps gender-stratified work stimulated collective awareness of shared problems, which facilitated referrals to therapy, which in turn built the personal confidence to address preexisting, societal-rooted problems (possibilities noted by Smith-Lovin 1998).

In other well-received ethnographies, the theorized cause is described with field notes, but a significant part of the theorized effects run beyond the researcher's time in the field. Khan's (2011) recent ethnography of the acquisition of habitus in a prestigious private high school relies for its significance on theories that predict long-term beneficial effects from the possession of social and cultural capital. The ethnographer draws on field notes to show variations in the course of student careers that depict the class formation part of the explanation: Adolescents from varied ethnic and socioeconomic backgrounds develop a common consciousness and competency in manifesting a new form of elite culture, one that does not reflect birth entitlement but privilege, a casually displayed implication that a right to superiority is grounded in hard-tested merit. Still, prep school students typically anticipate college and postgraduate education; any distinctive effects of prep school socialization on material status will not appear until 10 to 15 years later, far beyond the reach of the ethnographer's project. The ethnographer can report the current high status of alumni, but graduates from past cohorts may not represent the historically novel version of privilege that the author theorizes; and in any case, his observation of prep school life cannot distinguish between the contributions of advantages that may have been conferred on alumni by school socialization as opposed to those possessed before they entered the prestigious institution.

The frequent failure to acknowledge the misfit between causal claim and the variations in fieldwork evidence may suggest that explicit causal analysis, including the testing of significant rival explanations, must be left to other methodologies. But a rich array of strategies may be used to develop causal explanations and negate alternatives, even without reaching beyond the limits of the data a fieldworker can personally gather. First, causal implications may be made explicit much in the way that is common in the practice of other social research methods: an author can specify the various data patterns that should be found if the proffered explanation is true and then systematically examine field notes to specify whether and how they fit the theory's expectations. To the extent that the implied patterns are nonoccurrences, in the sense of events or behaviors that the explanation predicts should not be found in the data, ethnographers face unique challenges, at least when, as is usually the case, they did not gather data by following a design for random or probabilistic sampling. But there are many helpful strategies for analyzing sets of ethnographic field notes in ways that enhance confidence in claims of nonoccurrence.

Ethnographers may also counter alternative causal explanations that point to the researcher's behavior. Should the reader assume that the described

patterns are ongoing when the researcher is not present? Because ethnographers vary their behavior in the field more or less continuously in unplanned ways, they are likely to find it alien to use the quantitative researchers' language to argue against the claim that their interventions confused their findings. But ethnographers can discover a range of alternative strategies which may effectively lay to rest suspicions that they imposed the patterns they found through the ways they gathered their data. The ethnographer's strategies against the alternative explanation of researcher bias or "reactivity" may be summarized as ways of building triangular interactions among subjects, readers, and themselves.

Even "counterfactual" reasoning has fruitful analogies in application to sets of qualitative field notes. Quantitative researchers prize counterfactual reasoning for enhancing the probative value of what they (in language likely to mislead ethnographers unaware of subfield nomenclatures) call "observational" (as opposed to experimental) research. But counterfactual reasoning may also be introduced by the participant observing ethnographer. Further, ethnographers may detect counterfactual reasoning naturally introduced by the people studied, that is, scenes of social life in which members, as part of their everyday behavior, in effect "match" a person on the scene with people of another category, momentarily treating that person "as if" he or she had a different demographic identity. Such folk counterfactuals offer participant observing fieldworkers especially probative data resources. They provide a form of "control" insofar as the ethnographer can describe how a situated interaction progresses while biographical and ecological background features remain constant.

In order to reveal the methodological strengths that are potential in an ethnography, it is essential to highlight the qualities that make research specifically ethnographic. These include descriptions of contextual nuances in expressive behavior and propositions describing web-like relationships among interactions that, for members, form the natural context of action. In order to appreciate nuanced expressions and how actors take context into account when forming their behavior, it is helpful to study a particular text intensively.

This article concentrates on McDermott's (2006) *Working-Class White*. McDermott studied interracial and intraracial interactions by working in convenience stores in two ecologically contrasting settings, one in an Atlanta neighborhood where whites were the minority and many nonwhite immigrants were from Africa, another in a Boston neighborhood where American-born blacks were the minority and immigrant blacks came from the English, Spanish, and French-speaking West Indies.

The current analysis does not offer an overall assessment of the quality of McDermott's study, nor does it endorse the validity of her substantive claims. For illustrating some methodological points, the weakness of a text's evidence can be a resource. The objective here is to exploit her study to clarify challenges and solutions for basing causal explanations on ethnographic evidence.

For this purpose, her text is exceptional in several regards. The monograph is relatively brief and simple in design, and yet it develops causal ideas that are tested against both sharp sociogeographic ecological contrasts and complex demographic variations, including racial categories that go beyond a black and white binary. The text is based on a dissertation investigating questions and conducted at sites that have been widely appealing and practically accessible to novice ethnographers in sociology for decades. The data she gathers and analyzes include census, historical, participant observation, and interview evidence, a variety that facilitates exploring a range of methodological questions. In addition, for an ethnographic monograph, her text is unusually explicit as an exercise in causal explanation, being formally comparative in design, directly connected to Herbert Blumer's classic interactionist explanation of race prejudice, and readily evaluated in comparison with rival explanations that are based on macro-historical and survey research methodologies.

McDermott's text also invites considering rival causal views and questions about how evidence might distinguish among them, because the interactions she studies are so fraught in contemporary social life. Readers are likely to appreciate that they already have their own data resources for testing her explanations. She aims to explain variations in race as a dynamic, multi-sided, pervasive, and emotionally explosive theme invoked and provoked in everyday interaction. Her readers are likely to come to her text with a wealth of experiences in which the presence of race in interaction has been contested. They may have wondered whether they were the target of racism. They may have heard a racial accent imitated in a seemingly playful manner or a racial insult launched ironically and then have witnessed controversy over the gravity of the action. Having noted who sits with and apart from whom, readers may have wondered whether the pattern indicates racial likes and dislikes. While observing others confronting accusations that their conduct was motivated by racial sentiments, readers may have perceived apology or resentful silence.

In short, readers of McDermott's book are likely to be inclined and well prepared to invoke their own situated interactions as a resource for interrogating her claims about how race takes on causal force in interaction. This

resource for criticism is not inevitable in ethnography. It has been absent from anthropological ethnography when texts are based on fieldwork in sites inaccessible to most readers. In sociology, the warrant for ethnographic research often has been social distance between the site studied and those familiar to readers (Katz 1997, 2012). In both cases, the reader is severely limited in drawing on his or her experience to test the ethnographer's claims.

McDermott does not herself offer extensive methodological reflections on how her data may be used for testing causal explanation. As is usually the case in ethnographies, she focuses on matters of substance. Still, it is through a process reminiscent of analytic induction that McDermott arrives at Blumer's "status threat" explanation of racial denigration.

A sense of vulnerability arises for all her subjects, but in a context and in terms specific to the racial/ethnic population in which they are classified by self and other. Vulnerability arises for whites seen as left behind within historically changing local racial ecologies in which upwardly mobile blacks as well as whites move away; for blacks, from being identified with "the ghetto"; for black immigrants, from being miscast and subject to the pejorative characterizations of historical American racism; for African Americans, from what they perceive as immigrants' postures of superiority. All are at risk of criminal victimization and racial interpretations often enter their apprehensions. Additional vulnerabilities arise in the aftermath of erotic/romantic relationships, when an ex-partner attempts to use racial interpretation to make sense of earlier excitement and subsequent disappointment. McDermott operates more or less in the "recursive" (Tavorly and Timmermans 2014:71) style of analytic induction. With expressions of racial perspective as the explanandum, what she detects as expressions about race and what she means by her explanans, status threat, become progressively detailed as she works "status threat" through characterizations of the racial denigration of whites in the Atlanta area, of African Americans in Boston, of African immigrants in Atlanta and Haitians in Boston, and of "ghetto" blacks as seen by working and middle-class blacks in Atlanta.

I first present a table that explicates the empirical predictions that may be derived from McDermott's text. I next distinguish various strategies for establishing patterns of nonoccurrence, outline multiple strategies for refuting suspicions that the researcher's behavior caused patterns that are erroneously attributed to members, and then argue the utility of assessing the evidentiary value in ethnographic research with folk and analyst-created counterfactuals. After examining the ways a reader may test the fit of her data and her substantive claims, I show that McDermott's theory, which is based on descriptions of situated social action, can be tested further in research

based on other forms of data. Throughout the analysis, I develop the arguments that the evidentiary logic exemplified in McDermott's participant observation study may be summarized as a version of analytic induction, that a similar logic is implicitly used by qualitative researchers who work with interview and recorded data, and that the explication of causal logic presented in a fieldwork-based study therefore should be more broadly applicable in qualitative research. Overall, I argue that while analytic induction is exclusively applicable to qualitative research, the use of analytic induction in an ethnographic case study builds evidentiary strengths through a methodology that is fundamentally identical to the methodology of quantitative research (cf. Robinson 1951; Turner 1953).

The Multiple Data Patterns Implied by an Ethnography's Causal Claims

In the first chapter in which she reviews situated interactions, McDermott argues that whites in the Atlanta area she studied will be denigrated by virtue of presumptions that there must be something wrong with them to live in this majority black area, while whites in the majority white Boston area she studied will be seen as presumptively respectable. McDermott finds that whites in both sites had "virtually identical demographic profiles" (at p. 17). Given the history of race relations, the presumption is that whites living in a predominately black working class/poor area in the South must somehow be deficient. As the status of whites in each area is general knowledge, the implication is that the moral status of whites in each area will be understood similarly by local whites as well as blacks.

The author does not specify the data patterns that would support or disconfirm her analysis. But clearly a version of causal explanation is being advanced. It should therefore be possible to set up an assessment of her theory by formally stating the patterns to be expected in her descriptions of situated action.

McDermott's approach is to link ecological factors as causal and racialized expressions as effects, but the relationship should not necessarily be understood as deterministic. Qualitative sociologists are often more comfortable with what might be characterized as retrodiction. Retrodiction makes predictions of what will have happened, should a given type of behavior be observed; the prediction is not of what a member will do but of what an analyst inspecting the history of a given type of event will find in its past (see Katz 2001). Read in this way, which is consistent with the symbolic interactionist tradition she invokes with Blumer's theory, McDermott's claim is not

that differences in ecological structures create forces that push local residents to express racial views differently but that if a racial expression is made, the actor will have taken account of the ecological context through anticipating how others present would understand his or her expression. Through an interaction process by which locals relate situational dynamics to area demographics and history, racial expressions will “reflect” ecology.

For purposes of this methodological discussion, I have simplified the racial expressions McDermott describes into positive and negative categories. I also initially simplify the complex ethnic/racial/national origin differences in her two sites by using a binary conception of whites and blacks. Her theory of status vulnerability as shaped differently in her two ecologically contrasting sites implies at least seven distinct patterns of racial expression.

In order to decide how to fit particular descriptions of social interaction into the various cells of Table 1, some theoretical decisions about race as a social phenomenon are required.¹ Is a given expression about a given person relevant to one or two cells? When individuals orient to another person as “white,” an implicit racial contrast about blacks (or some other ethnicity/race) may be in play, and vice versa, but even so, positive statements about one population set may or may not convey negative sentiments about another. McDermott argues that patterns of hiring blacks in the Atlanta store at once support divergent moral implications about the two racially defined populations, that is, that the almost exclusive hiring of blacks is evidence both of positive views of blacks and of negative views of whites. But the same double counting cannot be assumed about every racial expression. If her theory is correct and probative, we should find many instances that fit cells 2 and 5, although not necessarily with the same frequency, and similarly we would not be surprised to find an imbalance in the cases filling cells 3 and 8.

Other cells should be empty. Cell 1 is critical: if we observe positive indications about whites in the Atlanta site, McDermott faces powerfully contradictory evidence. But it is less obvious what to expect about cell 6, which collects negative indications about Atlanta-area blacks. Such expressions would be more or less damaging to her claims, depending on whether her theory is given a strong versus a weak reading. Racial differences might be thought to be more strongly present in a site if positive indications are exclusively directed toward one population and negative indications are exclusively about another. But a racialized culture may also exist in a more modest form. For example, members may animate racial themes by making both negative and positive indications about one population set while only making negative indications about the other. Or the converse may be true: the social life in a given site may consist of exclusively positive indications

Table 1. Moral Imputations (Expected) Shown.^a

	Atlanta Context: Minority White		Boston Context: Minority Black	
	Positive	Negative	Positive	Negative
About Whites	1 (Empty) established by multiple logics	2 (Saturated) direct and inferred ↙ ↗ ↘ ↖ ↙ ↗	3 (Saturated) direct and inferred	4 [^] (Empty) negative case "As if" ↗
About Blacks	5 (Saturated) ↘ ↖ ↙ ↗ ↘ ↖ "As if" direct and inferred	6 (Unspecified) ↗ denigration by immigrant resisted	7 ↗ ↖ ↗ (Empty) ↗ exceptional details explain instance found	8 (Saturated) direct and inferred

^aViews by either blacks or whites; racial categorization is by subjects.
[^]: A negative (falsifying) case left unresolved.
 ↗↘: counterfactuals: what happens in one site is offered to explain what does not happen in another.
 "Unspecified": Theory does not imply absence or presence of pattern.

about one racial category but positive and negative indications about another.

In the data reported in McDermott’s text, there is sparse evidence to fill cell 1: She reports few positive indications about whites in the Atlanta site. In Atlanta, negative indications about whites (cell 2) are abundant, as are positive indications about blacks (cell 5 is saturated). Given a relaxed reading, her theory can survive some negative indications about blacks, although they suggest that whites in Atlanta are not stigmatized as strongly as would be the case in a more disciplined racist culture that only demeaned whites. Indeed, in several chapters, there is much evidence of negative views of blacks in Atlanta, by blacks and by whites. Cell 6 is thus left more or less indeterminate: if we find some instances, but fewer than we find for cells

2 and 8 (which predicts negative expressions about blacks in the Boston site), her theory remains intact.

To minimize redundancy, I will analyze how the field notes McDermott includes in her text do or do not fit the various cells in the course of discussing other methodological issues. Because it is not the purpose here to assess her substantive arguments, it is not necessary to put numbers in the cells. Nor do I argue that ethnographers should commit themselves to such counting operations. Still, it should be useful for ethnographers to follow the tradition long familiar in methodological reflections on causal theories of formalizing anticipated data patterns (see the classic model in Stinchcombe 1968). If, after examining field notes, there is no evidence for an expected cell, or abundant evidence of phenomena that fit a cell which should remain empty, it may be appropriate to make a further search for evidence or fundamentally rethink the theory.

Proving Nonevents

Cells 1, 4, and 7 represent a critical part of the universal logic of causal explanation, that the explanation must include predictions of lesser, alternate, or nonevents. When an ethnographic proposition implies manifest actions, it is relatively straightforward to inspect a data set, compile confirming instances, and assess whether within the corpus as a whole the pattern is significant, at least in a qualitative sense. But the prediction of a nonoccurrence would seem unverifiable or at least never confidently claimed. If, for example, McDermott's fieldwork data set on the Boston site shows no evidence of the denigration of whites, as predicted by an empty cell 4, perhaps such expressions occurred outside her (white) presence.

The expectation that cell 1 will be empty follows from McDermott's claim that whites have a categorically denigrated status in the Atlanta research site. It would be consistent with the claim that whites are presumptively not hired. Giving someone a job is the gold standard of acceptance, not only because money is involved. A job offer is immediately consequential for the hiring agent. If the employer will work alongside the employee, a job offer will be a personally involved decision. And if it lasts beyond initial probationary periods, a job is likely to be consequential for the employer for an extended period of time. Employment patterns are more profound indicators of racial attitudes than are momentary responses to a questionnaire, offhand comments in daily life, or marks checked on a ballot. But still, that whites are not hired could be consistent with the irrelevance of race to the decision makers. The challenge for the researcher is to establish not just that something

does not show up in her data but that the nonoccurrence was the product of purposive social action.

McDermott in effect puts on trial a charge of employment discrimination. After asserting that employment in the Atlanta area convenience store usually goes to blacks, McDermott rules out explanations alternative to discrimination for the nonemployment of whites, one being that they do not apply. She describes an instance in which Stephanie, a white manager, does not accept a white male applicant for a clerk position, noting that it is “reasonable to surmise . . . race had something to do with the decision . . .” (at p. 46).

Clearly the evidentiary value of this instance is weak. It is consistent with the proposition asserting negative stereotypes of whites in the area, but McDermott offers no basis for ruling out that nonracial factors may have been involved, and indeed the fact that Stephanie and McDermott are white indicates that the store’s white owners are not consistently prejudiced. The incident is less convincing as evidence of negative views of whites (cell 2) than as evidence of a lack of positive indications about whites (cell 1): an opportunity for hiring a white was passed over.

Similarly, other data passages are at best consistent with the claim of negative views of whites at the Atlanta site. Stephanie remarks that a dismissed white woman had rotted teeth, which is a “telltale sign of ‘being on the pipe’” (at p. 47). The incident points to an interpretation consistent with the theory of the book, but the reader finds a triple weakness. There is no specific reference to race in the manager’s remarks; even to the extent that race prejudices entered into the firing, the act reveals a prior hiring of a white; and Stephanie remains white and employed.

McDermott needs ways to argue the patterned empirical reality of certain nonevents, beyond citing explicit statements by members to that effect. An empirical generalization about a nondoing may be supported by constructing the researcher’s demonstrated ability to have acquired evidence of the nonevent, should it have occurred, by folk-defined exceptions that show the rule, by actions that emerged within the course of fieldwork and reveal what was repressed earlier, and by applying a counterfactual logic to given moments of socially situated action.

Building the Presumption That the Researcher Would Have Observed Events, Had They Occurred

McDermott constructs an image of having developed the ability to perceive positive indications about whites in the Atlanta site, such as positive statements about whites in general or instances of hiring whites, should they have

occurred. First, she is herself white: if her racial identity had any impact on her ability to detect positive statements toward whites, it is reasonable to surmise that it enhanced rather than diminished that ability. More importantly, she describes the length and intimacy of her involvement as a worker in the Atlanta area store during a six-month period. Although we are not told how many hiring decisions she might have known, her discussion of interactions with co-employees indicates that there were many.

McDermott adds to the gravitas the reader may impute to her investigative capacities by noting that the “patterns I identify and analyze are based on thousands of naturalistic observations I made while conducting my research” (at p. 3). Assertions that a text is based on thousands of pages of notes and fieldwork extending over months or years are common rhetorical gestures in ethnographies. They should be understood as an implicit claim of authority, not only for the bulk of presumably supporting collected data that cannot as a practical matter appear in the text but for the paradoxical “finding” that certain types of theory-pregnant behaviors did not occur. While McDermott may have been privy only to a small number of hiring decisions in which whites might have been hired but were not, there was a massively recurrent possibility that recollections of hiring before her time and positive statements about the character of local whites could have surfaced.

McDermott grounds her authority as a witness of what did not occur through building up a self-portrait as a researcher. Between the lines of her descriptions of situated interactions, the reader sees her close relations with coworkers, how she aggressively interjects herself into tense interactions with customers, and how she becomes trusted for the disclosure of such delicate personal matters as sexual fantasies. Throughout the text, McDermott’s reporting of intimate involvement in everyday situations builds up authority for her assertion that the absence of positive indications about whites reflects systematic social understandings in and around the Atlanta store.

Thus, when she provides a rare passage in which members on the scene directly make positive indications about local whites, the moment is not disconfirming. A few days after she began work, a white customer tells her, “. . . good to see you working here . . . a lot of white people have applied to work here, and they haven’t hired any of them” (p. 45). Although we are not given details as to why we should believe that this person has the experience and self-interests that would make his testimony creditable, the passage appears to be an exceptional positive gesture towards whites.

Exceptions That Indicate the Rule

This last incident illustrates a second logic for empirically establishing empty cells, the use of exceptional occurrences that indicate that nonoccurrences are the rule. An indication that a subject perceives an exception is stronger evidence than a self-serving declaration by the researcher. A folk or subject-defined exception helps establish an empirical grounding for empty cell 7, an absence in the Boston study site of positive indications about the moral character of African Americans. An example is the statement by an older white woman that a black teacher and a black “lawn doctor” (landscaper) “took care of their property . . . when they own their own property they’re good, but when they’re renters . . . they wreck the places” (at p. 53).

The evidentiary value of exceptions that prove rules is at some point self-contradictory. Accordingly, when McDermott makes use of events that show positive indications about whites in Atlanta (cell 1), she usually shows why we should not expect to find many. That she was hired might be seen as undermining the claim that whites are not viewed positively in the Atlanta site, but she explains that she was hired only after she established her image as an unusual job applicant, overqualified due to her education and with interests that separated her from the local white applicant pool: “. . . only after I explicitly stated that I desired a ‘racially diverse’ workplace did I obtain an interview . . .” (p. 45)

Emergence Over Time Indicates What Did Not Happen Before

A third logic for establishing a pattern of nonevents exploits the retrospective significance of events that appear in the researcher’s fieldwork experience only after a period of immersion. As might be expected on any job, McDermott’s acceptance by coemployees increases over time. By the end of the book, we are reading about interactions in which her Atlanta area coworkers are describing their preferred sexual partners by race and are denigrating “ghetto” blacks.² Such instances of camaraderie cast white McDermott as morally competent to sympathize with stratifying actions that denigrate others. And they indicate the lack of presumptive moral competency in earlier stages of the relationship.³

In a long-term participant observation study, retrospective revelations of what previously was hidden will be especially valuable evidence. They not only help solve the problem of proving nonevents, they bolster the warrant for the study. Indeed, if an extended stint of participant observation does not reveal what had been invisible to the researcher early in the fieldwork, how

justify a long immersion in the field? The greater the social distance between what outsiders and insiders know of an area of social life, the more the study is needed (Katz 1997).

Counterfactuals Constructed by the Analyst

The Boston area convenience store that McDermott worked in was often dirty and disorganized. The Atlanta area store was carefully ordered and cleaned. Following Goffman's many leads, ethnographers frequently find that the status of a place is taken as indicating the moral character of the people found in them. McDermott reports the outrage of black employees in Atlanta when an African immigrant complained that the bathroom was dirty. Were that true the place would reflect poorly on them. When they respond soon after with talk among themselves that denigrates the customer and his immigrant kind, McDermott has strong evidence for the status threat theory that runs through her book.

But McDermott also uses counterfactual logic to give empirical support for cell 4, the expected pattern of no negative indications about the character of whites in the New England setting. (This analytic operation is indicated in the Table by the ascending arrows connecting cells 6 and 4.) She notes that in Boston "no aspersions were cast on the clerks . . . for being white yet 'working in a place like this'" (p. 50). Although the author does not use the phrase, the message is that "white trash," a staining of personal identity, is uniquely produced in Atlanta's ecology. For whites in Boston, trash in a convenience store has no polluting status as taboo, no power to stain the character of the people who treat it as part of their normal environment. Essentially the thinking here is counterfactual. What if such a scene had occurred in the Atlanta site? By describing the resistance to the immigrant's criticism in Atlanta, and by arguing what would have happened in Atlanta had the store been "dirty," McDermott highlights the nonaction of denigrating whites in the Boston site.

Building the Research Triangle to Rule Out Researcher Behavior as Causal

Experimental, historical and survey research commonly operate through a social organization that divides the roles of data gathering and analysis in time and assigns them to different people. When data gathering is relegated to others, threats to distortion predictably arise, a risk long recognized in work on statistics to detect and control for reliability. Ethnography's distinguishing feature is originality in the inscription of data, and while the data

gathering leading to ethnographic texts is sometimes delegated, frequently the writer/analyst is personally involved in the site under study. Some ethnographers have implied that employee resistance makes “hired hand” research especially vulnerable to the manipulation of data inscription (Roth 1966), but even when ethnographers work on their own, they too are often hired hands—someone is funding them—and more generally they are vulnerable to analogous suspicions that they shape the gathering and presentation of data to serve career interests. A famous example is the controversy over the representation of sex life in Samoa.⁴

Participant observer-ethnographers have several ways of arguing against the rival explanation that the associations represented in a text are artifacts of interactions among the researcher’s roles as data gatherer, data analyst, and author. Several strategies for arguing against “reactivity,” bias, or unreliability in the process of gathering and presenting data can be developed by promoting a triangular social interaction among reader, research subjects, and analyst. The general idea is to create a social system around the ethnography that breaks the author/researcher’s monopolization of access to subjects. This is essentially the same social control strategy that operates in other forms of research, quantitative or qualitative, where, to varying degrees of practicality, the statistical data sets, audiovisual recordings, or historical archives examined in a text are available to other researchers.

As a strategy for rejecting the hypothesis of researcher bias or reactivity in the data, the process of creating a researcher–subject–reader triangle has a prophylactic logic. When established, the triangle encourages confidence in the researcher’s claims without necessarily specifying the hypothesis which has been ruled out. The analogy is to statistical tests of coder reliability as often used in quantitative research. They rule out or calculate the extent of alternative causes without necessarily determining which coders gathered data unreliably or which of the responses, those obtained earlier or later in the course of an interview in which probing declined, were more accurate.

How a triangular structuring of reader–researcher–subject interaction may be promoted in ethnographic research is neither obvious nor simple as a practical matter. The key objective is to enhance the reader’s independent access to subjects. Even where concerns of confidentiality do not block sharing the participant observer’s data set with readers, opening public access to field notes is rarely an effective option. Many passages will be indecipherable because they were written originally in ways that presuppose the researcher’s already acquired contextual knowledge. Recorded field notes provide no

insurance against the nonrecording of inconvenient facts. And ethnographers know that many scenes come to life and are first recorded, or at least significantly elaborated and edited, only in the writing-up, when analysis has advanced significantly and observations previously considered irrelevant, and so never before described, suddenly are recalled as resources for theorizing. Indeed, in the working phenomenology of fieldwork-based ethnography, researchers often take the emergent appreciation of past observations as personally convincing evidence that their analysis has developed significant empirical power. Ethnography's theory is commonly tested in an eminently pragmatic way. The researcher says, first to himself or herself, then to the reader: "Look at the scene this way and you will find patterns that otherwise you would not see." For the ethnographer, the "cash value" of a theory includes the illumination of fieldwork conducted many years earlier.

But if analysis guides data gathering in the field, and if data gathering continues as new memories are elicited during the writing process, how may a reader find independent access to the data that the ethnographer claims are the basis of the analysis? How can a reader obtain a vantage point for seeing the scenes studied, such that he or she can assess what the ethnographer made of them? A wide range of solutions have emerged.

Publishing Field Notes

Early in the history of sociological ethnography, an expectation developed that original field documents would be made available to readers. Thomas and Znaniecki reproduced subjects' correspondence as well as witness accounts of crime events and various other primary sources in their volumes on *The Polish Peasant*. When ethnographers began writing up their own descriptions of scenes and in situ behavior, they were in effect treating descriptions made by police, home visitors from settlement houses, and journalists as amateurish and less trustworthy. Field notes began to be published in texts in part to show that research had become professionalized.

It may now seem indispensable to give the reader access to original field notes, but monographs still differ widely in the extent to which field notes are included. A common but unevenly embraced device is to quote or paraphrase field notes. By formally setting field notes off from analysis, the author immediately sets the reader in a critical stance to examine the relationship of analysis and evidence. Blumer's (1939) critique of *The Polish Peasant* essentially noted misfits of the authors' "wishes" theory with the data they published. Although the ethnographer-author remains in control of the

selection and editing process (cf. Clifford 1983; Van Maanen 2011), when field notes appear in the text, the author must anticipate critiques of the sort Blumer demonstrated. Any ethnographer can attest to the palpable character of the constraints on wishful theorizing which then emerge.

Identifying the Research Site

A powerful strategy for promoting the reader's practical ability to collect potentially falsifying data through independent access to the research site is to identify the site geographically and historically. That may be no simple matter. Making public the place of the study may require negotiating consent with subjects, a process which in the extreme can entail having subjects sign off on their representation in the text (Duneier and Carter 1999). But if the site is close to communities of researchers—a condition that, at least in their early decades, distinguished sociology's ethnographies from anthropology's—disclosure of the site creates a market for others to make verifying visits that can assuage readers' suspicions. Enforcing methodological integrity is subject to the same economics as other values: it may be only the most highly rewarded works that are in fact audited (e.g., Duneier 2004).

Conceptualizing the Explanandum as Ubiquitous

An ethnographer can promote the reader's independent access to potentially falsifying data by conceptualizing the explanandum as a ubiquitous phenomenon. Erving Goffman's oeuvre is extraordinarily weak methodologically in that he rarely identified where, of whom, and how he made his observations, but also extraordinarily strong in discovering forms of social life that readers had already come to know. By working at the level of granularity of everyday social life and analyzing what Diane Vaughan has felicitously called *Similarities* (Vaughan 1998, as cited in Zerubavel 2007:137), Goffman enfranchised his readers to make up their minds about the empirical validity of his analyses in the private voting booths of their own intimate experiences. His constantly comparative analysis cut across the divisions made by popular culture (e.g., between mental hospitals and summer camps, between cooling out suckers and firing employees), such that instances of the explanandum were likely to turn up in a social world to which the reader had some access, whether personal and direct or acquired through independent reading. The very lack of detail in his texts about how he did his research is a methodological strength. Readers can "test" his analysis on their own experiences,

without a travel allowance, without negotiating access, without following any demanding protocol.

Weighting the Evidentiary Value of Context-constrained Behavior

Another strategy for arguing against researcher behavior as causing patterns found in field notes was captured by Becker's (1958) weighting advice. The formal recommendation was to give one weight to observations made in the sole presence of the researcher and greater weight to observations of behavior undertaken in the presence of others also at the scene. Perhaps originally ironic as a ritualistic bow to statistical research, and followed mechanically by few if any subsequent researchers, the methodological point remains valuable. What members tell or show the ethnographer when only the two are present is more likely to be an effort by the subject to fashion an attractive image of self for the researcher, as compared to what members display when in the constraining presence of others whose concerns and interests remain after the researcher leaves. (On the application of related doubts to interview-based cultural sociology, see Jerolmack and Khan 2014).

Note that Becker's advice is, in effect, to value role differentiation in the research process. In a one-to-one interview, the data that an ethnographer describes are tightly bound to his or her role as an analyst. The researcher must more or less constantly anticipate the pause that will come at the end of the interviewee's turn in speaking. That pause will occasion a need for the researcher to ask the next question. Anticipating a return of interaction initiative to himself or herself, the researcher will reflect on self-as-analyst even as the stream of expression from the member is flowing. In contrast, when the researcher is observing as a veritable fly on the wall, he or she will often have to put off analytic reflections in order to follow what is going on, because members will more likely be acting on the basis of understandings that they need not make explicit to each other. When the researcher thus differentiates the activities of data acquisition and analysis, he or she empirically diminishes the conflict of interest inherent in ethnographic interviewing.

Field note Quality

Techniques for producing high-quality field notes indirectly but effectively promote the segregation of data-inscribing and data-analytic functions.

(The leading guide is Emerson et al. 2012.) These include recommendations for:

- writing jottings, a practice that aides the careful separation of quoted, paraphrased, and generalized description and thereby avoids the need to invent dialogue in order to convey authenticity;
- describing scenes by sustaining the point of view of given members instead of jumping from one participant/touchstone of observation to another, the latter implying an omniscient comprehension of a coherent narrative that obscures the possible presence of multiple and inconsistent realities;
- using quotation marks and indented text to set verbatim and paraphrased data off from analysis; and
- sequentially describing practical activities that recur in the place under study.

Correctly describing the sequential structure of social life requires close attention and reliably produces discoveries independent of the researcher's prior sense of theoretical significance. (See, e.g., Frake 1980, which reveals a culture through meticulous descriptions of how people enter its houses.) McDermott "grounds" her data not simply on claims of having been there but by noting how provocations to racialized expression emerge in the practical ordering of clerks' work. In one instance, a clerk's perception of racial prejudice by a customer was manifested not by anything the clerk said but by the clerk's action of pushing the customer's credit card to McDermott, so that she would finish the transaction's narrative. In another instance, the familiar sequential narrative for getting gas—pulling up, pumping, paying—set up a silent drama in which tension mounted as a customer moved ever closer to driving off without paying. McDermott understood that her status at the work site was vulnerable and that she either had to run the risk of being accused of racial prejudice or of being seen as weak by fellow employees. The moment captures an association of status threat and reflections on racial attitude.

When field notes appear crafted, they indicate that the ethnographer has honored data inscription in its own right. In addition to aesthetic appeal and beyond conferring verisimilitude, crafted data draw the reader into the experience of members, independent of the author's analysis. Induced to resituate himself or herself into a sequentially framed dilemma, the reader is drawn to contemplate how he or she would respond, to compare his or her own

experiences, and thus to expand the tests to which the proffered explanation is subject.

Demonstrating Local Competency

Describing the sequential structuring of local practices is one feature of a more general methodological achievement, demonstrating local competency. Creating local competency is especially valuable for showing how ongoing social structure is reflected in members' fleeting sentiments rather than being an idea projected from the author's theory commitments.

When ethnographers offer descriptions of social interaction as evidence for causes framed as social structures, they run into a dilemma in one of the two forms. Most commonly, the explanans remains constant, while the explanandum emerges/rises or disappears/falls. The explanans will then seem irrelevant or lacking in causal power. If both explanans and explanandum emerge and disappear together, the explanans may appear to be micro processual, not macro or meso structural in nature.

In McDermott's effort to explain expressions of race, the question becomes: How can the explanans, which is one's own racial identity, socio-economic status in local neighborhood ecology, immigration status, or some interaction among the three, change in the course of emerging and vanishing expressions about race? Her warrant for using the methods of participant observation is the understanding that expressions about race are not constant but are situated phenomena. If so, must not their explanation lie in fleeting, locally situated factors? As expressions about race are not constant, how can they be the product of social "structure"?

McDermott's evidence shows both horns of the dilemma. In the bulk of her textual passages, the explanans—characterizations of "status threat" based on descriptions of the different statuses of blacks and whites in the local ecology—is visible but the explanandum—racial attitudes—is not; or vice versa. For example, a racial attitude will be expressed, or arguably implied, but without indications in the described action that the status of the participants in the local area's racial makeup was in anyone's awareness at the time. In the Boston site, when a white customer observes that her son's toy has been picked up by the child of a black customer, she responds "that's his toy" in a curt, cold manner. The event arguably fits cell 8. But where is the evidence of the relevance of the "context" whose effects are being argued? The reader will feel at the mercy of the author's theoretical interests. She selects emotionally provocative events but finesses rather than demonstrates their causal relevance.

Intraracial moral stratification is in abundant evidence in the Atlanta site, where American-born black employees denigrate African-born customers and area residents. Many of the insults emerge in the absence of their targets and with no apparent immediate provocation. But in several passages, the insults (explanandum) either respond to or recall previous situated status threats to the denigrators (explanans). I paraphrase from different incidents:

An African man complains that the car wash is not operating, and after the worker goes into a cold night to check, and the man has left, she utters to co-workers: "Africans are so rude!...they think they are better than us" (p. 86). As a recurrent topic in informal talk, the American born employees depict African beauty salon workers as "strange" and barbaric. On one occasion a situational provocation is evident. A convenience store worker recalls being chastised by a salon worker for spanking her own child and puts down Africans for "'just letting [their children] run around wild.'" (p. 87)

In these incidents, racial or ethnonational denigration emerges in direct response to a status threat and disappears from expression as situational tensions are resolved. As cause and effect appear closely linked over the course of sequential situated action, the relationship gives rise to the interpretation that members invoke demographic characterizations specifically to make sense of face-to-face humiliation. Commenting on the statement that Africans let their children run "wild," McDermott notes that Mary Waters' research in the Boston area showed that foreign-born blacks from the West Indies have a reputation among American-born blacks as *too* severe in family discipline. Perhaps the causal rule is, when one feels cut, or cut off by another, in order to resolve anger, invoke whatever demographic features are at hand (e.g., Katz 1999:58-59). Because the people we study so fervently but also so flexibly believe in the causal force of demographic patterns, ethnographers may not, at least not by understanding that social ecology deterministically produces moralized racial or ethnonational differentiation. A nondeterministic, mediated understanding would hold that virtually all members understand that all other locals share an awareness of certain prominent features of local ecology, but local ecology does not appear in expressive conduct except when useful in transforming one's situational identity in a constructive way, for example, by shifting from a position of moral vulnerability to righteous indignation.

There is no quick answer to the dilemma of linking structural cause and situational effect. Indeed, the need for a complex answer is itself a strong warrant for participant observation research. An effective response comes

from no specific passage but through situated descriptions that show the link between structure and situated conduct being made through the social phenomenology in the scenes of action. McDermott captures the challenge by noting a difference between “situated” action and “situational” action (at pp. 14-15), the latter distinctively reflecting the intersubjective relevance of context in the construction and receipt of an expression.

Demonstrating that the ecology in which a situation develops and the demographics of participants are relevant to members will often require that the analyst bring out what was implicit to members. Explicit expressions about race are less common in the interactions McDermott studies than are actions arguably making inferences about race. Still more common are actions of alliance and separation, conduct that constructs fluctuating tensions by widening interpersonal distances and moving a relationship toward greater intimacy. The link of structure and situated expression lives in the sentimental underpinnings that are produced and registered through embodied action: how far people stand from each other; whether they finish each other’s sentences or seem deaf to each other; laughs that accept a remark as ironic versus “stand offish” regards that suggest the remark was offensive. Only on occasion will there be cathartic moments which, in their eruptive character, index what had been vividly experienced but not articulated in language.

The ability of an ethnographer to convey situational themes in situated data is enhanced if they can describe what is alternatively referred to as *habitus*, the embodiment of behavior, emotional tones of interaction, or the phenomenology of situated action. The last includes the sensual/sensible ways that individuals live the transcendent themes in their lives (see the time dimensions analyzed in Emirbayer and Mische 1998; Tavory and Eliasoph 2013). The ethnographer must convince readers that her subjects, in their manner of perceiving and responding to others in a situation, maintain their awareness of transcending features of their life, such as racial identities in a caste-like system, patterns of local ethnic demography, and historical status as left behind, even as they shape fluctuating selves in each situation’s here and now.

What unarguably moves across the temporally and geographically divided situations of each person’s life is the person’s body. (The seminal work here is Merleau-Ponty 1962 [1945], 1968.) To get off the horns of the dilemma of providing evidence to link constants in a structural explanans to an interaction-varying explanandum, the ethnographer enacts her subjects’ lived sensualities and sensibilities. McDermott becomes, or fails to become, our guide to local social life to the extent she shows us that she has become a

vehicle for registering and conveying the intersubjectively shared tenor, emotional ambience, and range of locally resonant action in her research sites. In short, she must show us that the crucible of the research situation has shaped her authorial self. Put in terms of the triangular relationship between subject, reader and researcher, the methodological objective is to detail the embodied understandings that must be acquired for an outsider like the reader to become a competent insider.

The Stickiness of the Ethnographer's Analytic Web

When they develop their findings into a web of propositions, ethnographers incidentally promote a triangular relationship with readers. Presenting such a web will often require a monographic canvass with more space than journal articles can provide. But even in a relatively short book like McDermott's, the result is to empower the reader to find falsifying passages by comparing different segments of her text.

Her analysis begins with the contrast framework that is simplified in Table 1, where the position of a racial group in local ecology sets up its status as respected or denigrated. As she covers perspectives on immigrants and on those who put one at risk through criminal activity or sexual disloyalty, her explanation becomes increasingly generalized and integrated around the theme of status threat. The more she attempts to explain, the more nuanced and elaborated becomes her explanation. New challenges then emerge.

For example, while Table 1 indicates that cell 4 should be empty, in the Boston-area neighborhood "I was told by two of my white male coworkers that I would never find a 'stable' man in Greenfield, that everyone was 'messed up' in some way." (p. 50). This view of whites as generally impaired in personal character should distinctively fit the Atlanta site. Even though McDermott does not label her methodology as analytic induction, the dynamic principle of that logic is apparent: A negative case should force a qualification if not a major revision of her explanation.

Researchers who offer qualitative data for evidence empower the reader to object by noting a single disconfirming case. But a single unresolved case is fatal only if one forgets the exclusively backward orientation for justifying ethnographic research. In quantitative research, the measure of explanatory power commonly is forward-looking. An image of perfect explanation is visible at the start; it is envisioned in statistical terms of truth that are independent of the substance studied; one ends by specifying how close the findings come to some abstractly defined goal of perfect explanation: how much of the variance is explained, how closely a statistic of association approaches

1, how different from a perfect inversion are the results for experimental and control group. In contrast, for ethnographers, the warrant of a research project is only specifiable retrospectively, as the distance travelled. But (like all forms of social research) an ethnography's contribution can be assessed by tracing how far the researcher progressed from the state of knowledge as represented in the preexisting literature. Ethnographic texts make their evidentiary case only by demonstrating how many steps the researcher has taken, not by specifying how close the evidence comes to an abstractly conceived form of proof.

From this perspective, falsifying cases, if rare, have mixed methodological implications. Their positive side is to demonstrate that the ethnographer has spun a web sufficiently elaborate and tight that the reader can use it independently to catch contradictory evidence. Of course, a web that captures too much inconsistency will fall. But to the extent that an ethnographer has presented data passages so that they raise myriad implications for interrelated theory propositions, the overall analysis will resist the charge of tautology that haunts qualitative research (cf. Turner 1953). And by empowering the reader to detect contradictions, the web-like structure of data implications in an ethnographic monograph completes the triangular interaction. The researcher has effectively given up a monopoly on access to the subjects, at least for purposes of testing the author's explanation.

Reconceptualizing the Theorist's Problems as Members' Problems

Another strategy for allaying the reader's suspicion that the researcher is producing patterns in the presented evidence in a self-serving manner is to pull the teeth of objections, not simply by acknowledging them but by establishing them empirically as members' problems, that is, interpretive challenges with which the people studied themselves struggle. In effect, the ethnographer flips inadequate evidence and ambiguities in central concepts into substantive matters, treating them as conundrums not uniquely plaguing the researcher but endemic to the study's site. When done effectively, the reader is not asked to accept the researcher's coding (see Cicourel 1964, on "measurement by fiat") but to see an analogous coding problem in his or her own social experience. Fuzziness is displaced from the analytical framework and relocated in the fabric of social life.

The most troublesome interpretive challenge that McDermott faces is likely to be known by all readers who live in the contemporary United States: knowing confidently when race is or is not implicated in an interaction. This uncertainty runs through the great bulk of her data. Instead of trying to force

the reader's agreement, McDermott frequently acknowledges doubts. Most effectively, she devotes a chapter to "perceptions" of prejudice. Instead of taking interpretive ambiguities as a challenge unique to a researcher of these scenes, she throws the problem back into the field and studies how members recognize and handle the paranoia, diffidence, and quickly shifting sensitivities in her research sites. Prejudice is transformed from a psychological state into a situationally emergent challenge for members' interpretation. In effect, the author gets out of the way and guides readers to struggle with members' interpretive challenges.

Folk Counterfactuals

Causal propositions predict what will and will not occur, whether prospectively, by stating what members will do, or just retrospectively, by claiming what analysts will find in the history of a phenomenon. Causal explanations also imply counterfactuals: what would have occurred, or, for retrodictive propositions, what would not have occurred, if the explanans had been in a different state. Counterfactuals make claims about phenomena that have not occurred and may not occur, phenomena that are not and may never become observable.

Counterfactuals are used in quantitative research as an analytic strategy to provide a form of control that data gathering cannot provide (Morgan and Winship 2007). Lacking sufficient variation in a data set to test certain reasonable rival hypotheses, the analyst seeks a form of control by performing a matching operation that imagines realities which did not and perhaps could not occur. An analogous procedure of using fiction to advance science is possible in ethnographic research. For McDermott's study, one can ask the following questions: All other things constant, what if a black person in one or the other site had been white, or vice versa? How would he or she have been treated?

One way that ethnographers can use counterfactual thinking to argue for an explanation specifying structural or contextual effects is to argue that a given behavior would have been different, had the context been different. In a previously discussed example, McDermott, as the analyst, introduces the alternative state. She observes the dirty state of the Boston-area convenience stores, considers what would have been imputed to workers had the Atlanta site been similarly unkempt, and takes the lack of taint imputed to white workers as evidence of the presumptive and deep-seated respectability of whiteness in the Boston context. Conversely, in the Atlanta site, McDermott observes that when blacks are fired, the reasons given are about specific

behaviors (theft, drug use), not reflections on race (p. 47); but when white workers are fired, the local white population is disparaged. She implies that different responses would have occurred had racial identities been different, but she presents no evidence that members of the scene shared her comparative, *ceteris paribus* thinking.

In other data passages, the matching is done in the first instance by the subjects who act as if a member of the scene had an identity they know he or she does not have. Control is provided by the temporal character of the data: much stays constant in the brief periods during which members revise their definition of the situation or of a person in it. In these folk counterfactuals, an interaction begins in a way that would fit one predicted empirical pattern, that is, would fill a given cell of the table used here to schematize the implications of McDermott's theory, and then through a change in the definition of the situation or an altered definition of the identities of the people in it, behavior emerges that fits a different cell. Such transformations are represented by arrow markings. Folk counterfactual analysis has the extra evidentiary value of doubling the confirming (or disconfirming) value in a given passage of observed social life. In application to a table of implied data patterns, a given field note passage fits two cells.

By exploiting folk counterfactuals, ethnographers can highlight the distinctive evidentiary potential of their methodology for capturing *in situ* process. In an ethnographic study, as opposed to the survey studies that are the basis for the typical counterfactual analysis, perceived scenic and individual features of identity often change during data gathering. Ethnographers can trace events over the stages of their formation and development, as opposed to basing analysis on characterizations that compress action sequences into their before and after states.

Over the course of her Atlanta research, McDermott was first taken as a white applicant. Then she altered her application to be seen as different from the typical white applicant, in part by indicating her desire specifically to work with blacks. Once on the job, and in what is perhaps her most luminous field note passage, she was denigrated specifically as a white by a customer whose scam she rebuffed, leading to her embrace by black coworkers who saw her aggressive, self-respecting response as grounds for recasting her as if she were of their kind. I paraphrase:

In the Atlanta site, a black prostitute demands change for a \$3 purchase, pretending to have given McDermott a \$10 bill. Two black cashiers look on as a shouting match ensues. McDermott insists on payment, threatens to call the

police, and then “leaned in toward the woman and shouted, ‘Get out of my face!’” The customer leaves, shouting back, “‘You’re not even worth it. A white person working in a place like this!’” Her black co-workers celebrate her as an exception to what McDermott characterizes as “a dominant stereotype of whites in this area . . . that whites are weak and submissive,” one saying “‘You can hang with me in my neighborhood anytime.’” (pp. 41-42)

Because the researcher was herself so intimately involved, her report of what happened may be taken as suspect, especially since it serves her analytic purpose. Such problems of reactivity are addressed elsewhere in this article. In order to illustrate the potential for counterfactual analysis in ethnography, I assume the factual validity of the account.

The incident begins as a cell 2 event and ends as a virtual cell 5 datum (as indicated by the line of descending arrows). By recounting her experiences chronologically, McDermott in effect holds constant a multitude of personal and contextual features and observes what happens as her identity is successively redefined. The passage is valuable perhaps for adding autobiographical color to the monograph but more importantly for indicating what would routinely happen in local social life, had she had a different racial identity.

It is particularly effective because, with the aide of counterfactual thinking, the incident conveys a rare moment of empirical/theoretical merger where the ethnographer pulls off something analogous to a quasi-experimental “now you don’t see them, now you do, now you don’t” demonstration of causal nexus: the explanans emerges and disappears virtually simultaneous with the emergence and disappearance of the explanandum. The customer’s status is threatened with a charge of attempted theft (explanans) and she responds with a racial insult directed toward the clerk (explanandum). The same action puts the clerk’s status at risk (explanans): will she be made a fool of or risk verbal or even physical assault? After the clerk responds aggressively and just after the customer leaves, coworkers embrace each other across race (explanandum) through fantasizing commonality of neighborhood (explanans).

This incident, in which McDermott was explicitly if playfully treated as black, indicates that racial identity is a theme of varying vibrancy in situated interaction. McDermott’s identity as white sometimes emerged from a background characteristic to become a focal feature in her interactions with her black coworkers in Atlanta, with immediate, theory-confirming effects on their expression of racial views. Late in the text McDermott is examining expressions of intraracial stratification. In a discussion of preferred sex

partners, a black co-employee details her preferences for blacks instead of whites. As McDermott presses for her reasoning, the researcher perceives that her identity as white suddenly becomes prominent, at which point her correspondent's line of talk abruptly ends. The researcher was first treated as if she were part of the in-group, then recast as an outsider. In the phenomenology of the moment, the two are engaged first in a fantasy world of shared or nonexistent racial identities, then in a sobering flash they are separated into the segregated racial categories that strangers conventionally apply to them. The shift from intimacy to distance is represented in Table 1 by arrows pointing from cell 5 to cell 2.

Putting aside questions about the strength of the evidence that the researcher's racial identity was catalytic in the interaction, we can see the incident as a kind of short-lived exception that proves the rule about the segregated cultures of intraracial stratification. If participant observing ethnographers create field notes that are sufficiently sensitive to the emergence and decline of identity features in interaction, they may record many such moments of situational transformation. By appreciating what members are doing as folk counterfactual thinking, the ethnographer can give such data passages double evidentiary weight, as indicating both what happens within and what does not happen across segregated identity lines.

Note also that this incident reconciles a description of structure and an appreciation that social life is processual in nature. Although introduced into methodological discussion by quantitative researchers, counterfactual analysis is invaluable to qualitative researchers. It enables the ethnographer to appreciate "structural" differences in biography and ecology, here racial identity and neighborhood demographics, as causally potent only and as made relevant in the organization of situated action.

Implications for Researchers Working With Other Forms of Data

Participant observation is especially capable of creating data that "situate" social action in practice, sequence, and context. Observing in situ, the researcher can describe how a behavior fits into the course of a practical activity. McDermott, for example, specifies where and how a racial comment emerges in the process of taking payment at a cash register, observing shoppers who may be shoplifting, or giving directions to the bathroom. A participant observer can also specify the sequential references of an action, noting how a given expression plays off of an immediate prior statement, glosses a scene that all have been observing silently, or resurrects an

experience that was shared on a past occasion and uses it to reframe the instant action. A fieldworker with an ongoing presence in a scene is also relatively well positioned to describe how an actor develops the meaning of a gesture or a statement by invoking context in a less-than-fully-explicit way, for example, through colloquial nods to neighborhood ecology or with ironic flourishes which assume that racial controversies in national culture are in the common awareness of all others who are present.

When ethnographers use situated data to describe variations in cause and effect, they rarely make use of studies that use historical and social organizational data. They might do more. If analysts attend to the data requirements at different levels of social organization, their studies need not run past each other. But it should be appreciated that in studies like McDermott's, where situated racial expressions are more varied than is the contrast in regional demographics, the explanation will inexorably become social psychological in nature because it is at the level of face-to-face interaction that the researcher can get the most traction on rival hypotheses.

The in situ descriptions created by participant observers directly facilitate a processual analysis of social life. Ethnographers often resist working with noun-like forms of data such as ethnic categories found in conversation transcripts or statistics describing the status, power, and interethnic attitudes of different population segments. But it is not inevitable to fall into what Bourdieu referred to as the errors of "substantialist" thinking. The character of data need not control the nature of theoretical conceptualization.

McDermott's theory of racial expression can be extended and improved by examining studies that problematize ascriptive identities with data which may be either more micro or more macro than hers. Wimmer, drawing on network and attitude survey interviews, Facebook ties, historical scholarship, and participant observation in diverse ethnonational sites, argues that racial and ethnic designations should be investigated as strategic boundary making. Some of his findings might bolster McDermott's "status vulnerability" explanation for racial expressions. He studied residents of working class areas in three Swiss cities. The neighborhoods date from the industrialization era and still carry stigmatized labels that reflect the disdain they received from the higher strata of Swiss society during their formation in the nineteenth century. An intensified form of a "local order" culture developed in response, residents defending their dignity by exaggerating Swiss emphases on punctuality, cleanliness, decency, and order in general. When "60s"-era culture came into neighborhood scenes, and later when new waves immigration developed, the "established" residents of various national descents extended their culture to distinguish themselves from segments of a range

of ethnic/national descent populations whom they saw as indecent, disorderly, and so on. While Wimmer does not develop a general social psychological theory, he directly rejects stating the cause of hostile ethnonational views as economic competition or as anti-immigrant animus. Something more like status insecurity in a localized cultural world is a better fit with his survey data, which describe “established” residents of Swiss, Italian, Turkish, and other national descents distinguishing themselves as threatened by indecent newcomers and disorderly youth of various ethnicities, religions, and national origins (Wimmer 2013:Chapter 5).

In a possible contrast, Brubaker’s study of how Hungarian ethnicity is constructed in Cluj, a city in Romania’s Transylvania, may be read as challenging the pathos in explanations that posit negative forces, such as threats, vulnerabilities, and fears, as behind negative racial/ethnic expressions (Brubaker 2006). Like whites in McDermott’s Atlanta site, Hungarians were the rulers in Transylvania until well into the twentieth century, over the course of which they shifted from a majority to a minority in Cluj. Brubaker produces an exceptionally elaborate description of the deployment of ethnic categories by self-identified Hungarians. He examines school enrollments, marriage patterns, sensational clashes over ethnic displays in public culture, and focus group conversations. As one reads how racial/ethnic identification is used as a means for everything from mating, creating moments of humor, refining ironic sensibilities, and grounding a vibrant associational life, it becomes difficult to argue that ethnic/racial boundary making is always fueled by perceived threat, especially given that the Hungarians in Cluj brush off denigration by Romanian politicians, including 12 years of constant insult by the local mayor, and even seem unperturbed by violent interethnic attacks in a town some 50 km away. It is perhaps critical that the Hungarian-speaking residents of Cluj have their status officially anchored by laws promulgated in Hungary to give rights to transborder Hungarian populations and that the whites that McDermott studied lost various forms of government underwriting of racial privilege in the wake of the civil rights movement.

Comparative analysis of these and other nonparticipant observation studies might expand the evidence for McDermott’s theory or force a revision. Perhaps status threat is only sometimes the cause of negative expressions about race, ethnicity, or national identity. A more comprehensive explanation might look to the self-reflexive meanings implicit in setting off others as racial or ethnic alter egos: in the first instance, gestures that put others at ethnoracial distance center the self within a putative contrast community, providing a collective grounding for subsequent actions that may have a range of positive emotional significances and practical consequences for the

labelers, whether those labeled are ever aware of or touched by the process. Even without the stimulus of sensed vulnerability, the attractions of self-centering may be at the originating core of boundary-making actions. In any case, it should be clear that McDermott's explanation of racial expressions can be further tested and refined by examining studies using forms of data other than situated interactions, albeit at the cost of working through complex shifts in analytical level and analogies that challenge the author to make wide-ranging geohistorical contrasts.

Sociologists are challenged to work in the increasingly differentiated research worlds within micro sociology. Qualitative researchers increasingly tend to work exclusively with different materials: participant observation field notes, or audiovisual recordings of naturally occurring social life, historical archives, or with snowball sampled interviews that are conducted to maximize serendipitous opportunities to learn something unprecedented. In each genre of qualitative work, it is typical to work implications for causal explanation through developing nuances in substantive arguments rather than through explicitly identifying expected data patterns and formally identifying and ruling out rival explanations. Yet a common logic appears to be at work across the qualitative genres, even as the commonality is obscured by inevitable craft differences in the relationship between the data gathering and data collection phases.

Perhaps the most concise way to state the common logic is with the language of analytic induction. The traditional statements of analytic induction describe an iterative, recursive process. One starts with an explicit explanation, then searches solely for negative or disconfirming cases, then strives for "perfect" explanations by redefining explanans or explanandum so that what had been a negative case becomes confirming or irrelevant to the theory's scope. The reformulation then redirects the search for contradictory evidence and so on.

Within the history of sociology's methodological literature, analytic induction was little more than a brief topic of debate in the 1950s.⁵ Its dismal fate as an inspiring rhetoric is due not necessarily to the supremacy of quantification in social science but to the fact that even as qualitative social research has flourished, analytic induction has been impractical as a moment-by-moment guide. The classic examples were interview studies. After each foray into data gathering, the researcher could reevaluate the explanation and then set sights on acquiring a negative case in the next interview (Angell 1936; Cressey 1953; Lindesmith 1947). Or at least texts were written to give the appearance that alternating phases of data gathering and analysis guided the study. But even in interview studies that have used

analytic induction, the researcher often finds that she must snowball rapidly from interview to interview for fear that leads and vouching introductions will dry up. If the interviews are recorded, transcription may not be completed before the date for the next interview. A common result is that analysis may not be feasible until the data set is already substantially complete (see, e.g., Vaughan 1986).

Perhaps because many forms of qualitative social research do not accommodate the description/analysis in two steps that analytic induction seems to require, the methodological strategy is rarely labeled as such even when it is used. If, for example, one wants to advance knowledge about assaults on masses of strangers made by one or two attackers without organizational sponsorship, it is efficient simultaneously to organize a research team to collect varied data on all the cases available to date and then to begin analysis. Because the cases are so few, correlational analysis is less compelling than treating each as a negative case. Willy-nilly, one ends up thinking about causal explanation as one might had analytic induction been adopted as an initial commitment. The product becomes an explanation of a form of social life, perhaps “rampage,” that had never before been identified as such in social research (Newman 2004).

Similarly, original recordings of naturally occurring social life may wisely be gathered before and independent of analytical operations. In order to minimize reactivity, a recording device may be left running without an operator at hand. Conversation analysis has thrived by exploiting such nonanalytically created corpuses of data. It has escaped attention that some of the products of conversation analysis are exemplary achievements of the “universal” explanations promised by analytic induction. As the analyst searches for negative cases in transcripts and recordings that were created in varied times and places, cross-cultural, cross-language phenomena that were never previously identified are gradually defined in nuanced ways and explained retroactively as predictable (e.g., if a “structurally provided repair” is observed, it will have occurred before or in “next” turn; Schegloff 1992).

In most ethnographies that are based on participant observation, the researcher finds it necessary to “immerse” himself or herself in the field in ways that frustrate structuring a study into alternating phases of data gathering and analysis. The fieldworker persists on site so as to become confident that events are understood correctly. In order to get access, a participant observer often must make ongoing time commitments to remain in the field. If the researcher takes a job on the site of the study, analysis usually must be put on hold: finding hours to write up field notes will often be all he or she can do practically. If during the analytic phase the researcher finds a negative

case that cannot be reconciled with the rest of the evidence by changing the definition of explanans or explanandum, it may be reasonable to investigate whether the scene was misunderstood or inaccurately written up. But it will often be impossible to return to the site, or if one can return, to assume that one is documenting the same phenomenon, especially if multiple people were interacting in the scene.

It is understandable that studies which essentially use its logic make no mention of analytic induction. Still, there are abundant indications of the hallmarks of analytic induction in these varied forms of inquiry: ongoing revision of explanandum and explanans; treating single cases as capable of forcing revision in theory; wariness about arguing from correlational patterns; developing concepts that depart both from definitions taken from popular culture and from the categories used to amass official statistics; arrival at gerund-like concepts that capture a kind of doing, a pattern of becoming, a liminal historical phase. These indicators suggest that there is a community of methodological interest under the increasingly segregated, craft-based divisions in qualitative sociology. While the preceding explication of methodological thinking is most relevant for analyzing ethnographies that are based primarily on situated field notes, it may be useful for revealing hidden methodological strengths in other forms of qualitative social research as well. And as the constant search is developing falsifiable causal explanations and testing reasonable alternative explanations, analytic induction should be appreciated as applying to participant observation field research the same logic that guides social research in general, whether qualitative or quantitative.

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Notes

1. In the mixed working class and poor areas of Atlanta she studied, a deprecatory view of whites “is held by most members of the community regardless of race or class.” In the Boston area, a low-income racially mixed area “with a tight labor market and a history of working-class consciousness, whiteness is more likely to function as a mark of superiority than of inferiority” (both quotes at p. 39).

- McDermott does not spell out the implications that in the Boston site, both blacks as well as whites will see whites as superior (cell 3), nor that both whites and blacks will demean blacks (cell 8). Her Boston area data are more severely limited to expressions by whites, so these ambiguities in the theory cannot be clearly resolved. She especially lacks data showing how Boston area blacks view blacks and whites.
2. Such denigration would fit cell 6 and might be disconfirming unless a “class” qualification is added to the analysis, which becomes refined to assert that whites in the local area of the Atlanta store are denigrated independent of class but an additional condition of low-class characterization is necessary for blacks to be denigrated.
 3. McDermott does not indicate the timing of her observations and interactions as clearly as is suggested by this paragraph. “Later in the text” does not necessarily mean later in the fieldwork. The point here should be understood as illustrating a logic of proof.
 4. Mead (1928) achieved fame when her book on the sex life in (American) Samoa appeared in the late 1920s. She was seen as making the case for cultural relativity, a basic warrant for anthropology, by arguing that for Samoan adolescents, sex was a matter of joyful promiscuity. By inference, she critiqued the contrasting moralistic constraints on the sex lives of Western adolescents: They were repressive for reasons not rooted in what society must inevitably demand of human nature. The study vaulted Mead to the status of an admired public intellectual and a respected academic. About 45 years later, Derek Freeman (1983, 1999) argued that she had been duped by the joking practices of her subjects, who were subject to sexual repression in ways that paralleled and in some ways went beyond what Western adolescents had to endure. Freeman had conducted his own field research in (Western) Samoa, about 20 years after Mead’s fieldwork. His argument essentially was that Mead was taken in by the attractions of sculpting a self-image as the bearer of liberating news, an image celebrated widely in her home society but for decades unknown in Samoa, and when known, rejected by Samoans. Freeman in turn was criticized by reviewers who argued that he neglected historical changes in the populations studied and was taken in by the siren call of a self-image as a heretic who was making his historic place in the annals of anthropology, deriving fame not from the significance of his own findings but from attacking Mead without sufficient empirical foundation. Shankman (2009).
 5. Analytic induction was introduced with a claim of “scientific” superiority to quantitative methodology (or “enumerative induction”) that was not only undiplomatic but ironically misguided (Znaniecki 1928, 1934). In the popular mind, statistical evidence is associated more closely with science, while qualitative evidence is associated with the humanities. As implied by Becker’s (2007) comparative analysis of different ways of “telling about society,” the differences in

methodology between qualitative social science and the humanities essentially result from differences in the materials examined.

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