

# Ethnography

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## The role of theory in ethnographic research

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**ABSTRACT** ■ Scholars, including urban poverty researchers, have not seriously debated the important issues that Loïc Wacquant raised in his controversial review of books by Elijah Anderson, Mitchell Duneier, and Katherine Newman concerning the disconnect between theory and ethnographic research. Despite the tone of Wacquant's review, we feel that he made a contribution in raising important issues about the role of theory in ethnography. The responses to his review that address this issue, especially those by Anderson and Duneier, are also important because they help to broaden our understanding of how theory is used in ethnographic research. What we take from this exchange is that good ethnography is theory driven, and is likely to be much more reflective of inductive theoretical insights than those that are purely deductive. Moreover, we show that in some ethnographic studies the theoretical insights are neither strictly deductive nor inductive, but represent a combination of both.

**KEY WORDS** ■ ethnography, discovery, validation, deductive theory, inductive theory, ghetto, urban poverty

The ethnographic method examines behavior that takes place within specific social situations, including behavior that is shaped and constrained by these situations, and people's understanding and interpretation of their experiences. A full appreciation of ethnography's contribution to social

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science would entail a critical examination of its methodology within both the context of discovery and the context of validation. In the logic of social scientific inquiry, the former is concerned with the way in which fruitful concepts, hypotheses, and theories are discovered, while the latter is concerned with the evaluation of the products of social science and with making the evaluative criteria explicit.

In recent years, ethnography has been discussed within both contexts. In the context of discovery, it is generally acknowledged that ethnography enables social scientists to uncover relationships that have not been explicitly spelled out in theoretical formulations. These discoveries often lead to the formation of hypotheses that provide direction for further research involving either smaller ethnographic studies or quantitative studies with larger and more representative samples. In the context of validation, ethnographic data can be used to test, advance, or explain empirical assumptions. Ideally, these assumptions are derived from theoretical arguments. In the absence of theory, however, the use of ethnography in the context of validation is problematic.

We emphasize this distinction because a number of people have maintained that the best way to integrate ethnographic and quantitative research is to use ethnography to generate hypotheses that could be tested with quantitative data. The major objection to using ethnographic research in the context of validation is the inherent difficulty of generating a sample representative of a larger population. However, this problem can be resolved by theoretical sampling, which is crucial to theory testing and very appropriate for ethnographic research: the selection of natural cases that include the necessary conditions for the application of theoretical arguments that steer the research and are used to interpret the findings (Willer, 1967). Consider, for example, a researcher attempting to test William Julius Wilson's theory of the social transformation of the inner city, which includes a number of key hypotheses on the effects of living in highly concentrated poverty areas (Wilson, 1987). One of these hypotheses states that individuals living in extreme poverty areas are much less likely to be tied into the job information network than those living in marginal poverty areas. This hypothesis could be tested by a participant observer who selects one neighborhood that represents an extreme poverty area and another that represents a marginal poverty area and observes patterns of work-related interactions in each neighborhood over an extended period. Some scholars may question the degree of rigor involved in testing such a hypothesis with participant observation techniques, but this approach is clearly consistent with the logic of validation, and it represents a deductive process whereby prior theoretically derived arguments are tested with ethnographic data.

Theory plays an inductive role in ethnographic research as well. Here theoretical insights inform the interpretation of data uncovered in the

context of discovery. In the process the ethnographer integrates new empirical findings with theoretical arguments not by testing prior theoretically driven hypotheses but in using his or her theoretical knowledge to make sense of the data uncovered in the field research.

Finally, some ethnographic studies that incorporate theory into research are neither purely deductive nor inductive, but combine elements of both. These studies start out with a deductive theory and end up generating theoretical arguments in an inductive process that integrates old theoretically derived ideas with new and unanticipated theoretical arguments based on data uncovered in the field research.

We examine the varying role of theory in ethnographic studies by critically reflecting on issues raised in Loïc Wacquant's review essay in the *American Journal of Sociology*, 'Scrutinizing the Street: Poverty, Morality, and the Pitfalls of Urban Ethnography' (Wacquant, 2002). This essay generated a firestorm of controversy because of Wacquant's harsh critique of the widely read urban ethnographies by Elijah Anderson (1999), Mitchell Duneier (1999), and Katherine Newman (1999).

Although Wacquant's long review essay raised a number of important issues concerning the role of theory in ethnographic research, his arguments failed to engender a serious discussion among scholars, including urban poverty researchers. Unfortunately, the dismissive tone of his review and his inflammatory arguments distracted many readers from focusing on his most fundamental point – the separation of ethnography from theory.

While Wacquant raises a wide-ranging set of issues in his review essay, we focus only on the arguments concerning the role of theory in ethnographic research. We examine and critically assess both Wacquant's critique of the disjunction of ethnography from theory and the authors' respective rejoinders, raise a few critical issues about inductive and deductive theory in ethnographic research, and reflect on problems when ethnography is devoid of theory.

### **Wacquant's view of the disjunction of ethnography from theory**

Wacquant points to several important shortcomings that he sees as common across these three studies and reflective of a troubling pattern in recent urban ethnographic research. The three ethnographies are marked, he argues, by an inappropriate disconnect between theory and observation, which can lead to different, but equally troubling, analytical weaknesses in ethnographic research: the researcher may lack critical distance from the subjects and merely report, without analyzing, their perspectives; the analyst may shape observations to fit preconceived notions and make claims that are not well supported by the data; or the research may be organized

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according to the terms of public discourse around certain social issues, instead of applying the analytical lens of social science. Wacquant suggests that the poor fit between theory and observation in these studies facilitates the 'naïve acceptance of ordinary categories of perception as categories of analysis', as moralistic accounts replace social analysis (Wacquant, 2002: 1470).

Wacquant argues that if theoretical control is not exercised at each step in the process of designing and implementing an ethnographic study – as is true of every other method of social observation and analysis – ordinary notions based on common sense 'fill the gap and steer crucial decisions on how to characterize, parse, and depict the object at hand'. Distinctive ethnography and powerful theory are complementary, not antithetical, and 'the best strategy to strengthen the former is to bolster the latter' (Wacquant, 2002: 1524).

In *Code of the Street* (1999), Elijah Anderson conducts ethnographic research to examine the social order of a community in inner-city Philadelphia. He argues that activity and behavior in the neighborhood are characterized by one of two 'codes'. The 'code of the street' places highest value on interpersonal respect and makes regular use of the threat of physical violence as a means of self-assertion. While outsiders commonly stereotype all inner-city residents as acting in accordance with this code, many residents in fact follow the 'decent' code, which affirms middle-class values, personal responsibility, and participation in the mainstream economy instead of illegitimate activities. In the context of these conflicting codes, community residents develop the ability to switch between codes to adapt to different situations.

Wacquant presents several specific criticisms related to the role of theory in *Code of the Street*. He criticizes Anderson for approaching the division between 'street' and 'decent' as substantive and fixed social categories in the community and inappropriately accepting what Wacquant suggests are 'folk concepts' as analytical categories. Specifically, he charges:

... transmuting folk notions that residents use to make sense of their everyday world into mutually exclusive populations prevents Anderson from analyzing the dynamic contest of categorization out of which the distinction between 'street' and 'decent' arises and how this contest affects individual conduct and group formation. For it leaves unexamined the social mechanisms and paths whereby different persons drift toward this or that end of the spectrum, and what facilitates or hinders their sliding alongside it. (Wacquant, 2002: 1488)

Next, Wacquant argues that because Anderson fails to employ a theoretical framework that incorporates the stratification of social positions within the community, he cannot relate the moral distinctions of the 'street' and

‘decent’ codes to a system of internal social stratification. Rather than representing exclusive groups defined by a moral or cultural orientation, the street–decent division may actually describe groups who occupy different social positions with respect to the mainstream economy and make use of the ‘street’ and ‘decent’ cultural orientations to attach meaning to their divergent standings.

He also criticizes Anderson for not providing a theoretical explanation of how a code should be understood as influencing social action. Wacquant asks:

But what exactly is a code, where does the ‘code of the street’ come from and how does it actually generate particular behaviors? One would expect that Anderson’s book would elucidate these issues, but the more one reads the more muddled they seem to become . . . If the code is both a cultural template that molds behavior and that behavior itself, the argument becomes circular. (Wacquant, 2002: 1491)

Here, as with the discussion of urban ethnography more generally, Wacquant points to the limited ability of ethnographic research to provide adequate explanations of social mechanisms if it is insufficiently integrated with theoretical concerns.

In *Sidewalk* (1999), Mitchell Duneier conducts an ethnographic study of street vendors in New York City to analyze the interplay of moral behavior, public space, and safety. Wacquant argues that Duneier was not led by an interest in addressing specific sociological questions in pursuing his research. Instead, he suggests that Duneier projected his interests in morality onto a site to which he had access, without consideration of its theoretical significance. Suspecting that Duneier is motivated to portray his informants in an unrealistically positive light, Wacquant charges that he ‘takes the statements of his informants at face value and conflates “vocabularies of motives” with social mechanisms, the reasons invoked by vendors to make sense of their actions with the causes that actually govern them’ (Wacquant, 2002: 1480–1). Like Anderson, Duneier is accused of accepting the perspectives of informants uncritically and as self-evident explanations of their behavior, which in Wacquant’s view does not pass muster as acceptable grounds on which to base sociological explanations.

Duneier does engage more explicitly in theoretically informed debates when he seeks to assess theories that link physical and social disorder to safety concerns in public spaces. Here, however, Wacquant faults him for arguing that the street vendors actually enhance safety on the street, since the claim is not substantiated by data presented in the study.

In *No Shame in My Game*, Katherine Newman (1999) presents ethnographic research on low-wage, service sector workers in Harlem against the backdrop of high rates of inner-city joblessness. Despite dim labor market

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prospects for inner-city workers, Newman documents the experiences of those residents who do not turn to the street economy, but instead remain committed to the challenging pursuit of an economic livelihood in formal employment.

In Wacquant's view, Newman places disproportionate emphasis on the moral orientation of these workers in explaining their continued engagement with the formal economy, even though they have limited opportunities for earning decent wages or advancing to higher positions. Wacquant criticizes Newman for attributing their actions to their commitment to an outlook that values a 'mainstream' work ethic and self-sufficiency over idleness or dependency, and in the process inappropriately praising their virtuousness and heroism.

Wacquant's critique of Newman is rooted in his view of the disjuncture between theory and ethnography in her study. First, he argues that she employs an atheoretical conception of 'culture' in its essentialist variant, as in a 'mainstream culture' that values work and individualism, which has limited explanatory power. This notion gives rise to a normative judgment of her subjects and leads Newman to associate their behavior with their cultural or moral orientation, without incorporating other theoretically informed explanations of their actions. Wacquant argues that pointing to the 'character, resolve, and responsibility' of the workers is not engaging in sociological analysis, but making a moralistic pronouncement about the strategies they pursue in their particular circumstances.

Second, he suggests that Newman makes use of a limited theoretical conception of choices and constraints and how they influence her subjects. In Wacquant's view, by distinguishing ghetto residents who work in the mainstream economy and explaining their actions as the result of decisions they made based on a 'mainstream' work ethic, Newman's analysis is based on a faulty assumption of the freedom of ghetto residents to choose between positions in and outside of the formal labor market. He argues, 'Couching these alternative paths in (and out of) the local socioeconomic structure in terms of individual volition and discretion thwarts the analysis of the mechanisms and conditions under which differently positioned youth follow this or that circuit and with what consequences' (Wacquant, 2002: 1512).

Given the breadth of Wacquant's criticisms, the authors raise numerous objections in defense of their work beyond the specific concerns about theory. In keeping with the focus of this discussion, we take account only those points in the authors' rejoinders to Wacquant that respond to his arguments about the role of theory in ethnography.

Anderson most directly engages with Wacquant's criticisms about the disconnect between theory and observation in *Code of the Street*. In the context of the failure of mainstream institutions to establish civil order in inner-city communities, Anderson argues, residents have had to develop

informal strategies that rely on 'street justice' and are oriented around the 'street code'. The breakdown of 'civil law' contributes to the observed high rates of urban violence, exacerbated by the structural conditions of institutionalized racism, joblessness, and alienation.

Anderson fundamentally rejects what he interprets as Wacquant's 'peculiar view of the role of social theory in ethnographic work. His view demands that the ethnographer begin with a rigid commitment to a theory. The ethnographer must then subordinate the cultural complexity he or she finds in the field to that theory' (Anderson, 2002: 1534). Anderson argues that Wacquant is wedded to a 'top-down' approach to ethnography that stubbornly views micro-level actions as necessarily reflective of macro-level structural factors and the organization of power in society. In contrast, Anderson describes his own, more inductive conception of ethnographic research. An ethnographer should 'be familiar with and edified by the various sociological theories at hand' and use his sociological knowledge, in combination with local knowledge gained from the site, to formulate the analytical questions that guide the research. The ethnographer then uses cases gleaned from field notes to test and refine his hypotheses in an effort to build a general argument.

Their diverging approaches to the use of theory in ethnographic research is evident in the discussion of Wacquant's claim that Anderson fails to account for internal social stratification in the inner city to explain the emergence and salience of the street-decent dichotomy and instead accepts these 'folk concepts' as analytical categories. Wacquant argues:

Had he started from a systematic map of social differentiation inside the ghetto, he would have found that what he depicts as the coexistence of two codes that seem to float up above the social structure is in fact a division between inner city residents that corresponds to their varying social positions, particularly with regard to their relationship to mainstream institutions. (Wacquant, 2002: 1500)

In this view, a theoretical framework that accounts for internal social stratification would inform an interpretation of the ethnographic data and point to an underlying mechanism or social process that would not otherwise be evident by drawing only from what the researcher finds in the field. Elaborating on his inductive approach, Anderson responds: 'I did not start with a map' of social differentiation in the ghetto 'because there *is* no map and none is required. I found what I found by doing field work and involving myself in the lives of people living the reality of the inner city every day' (Anderson, 2002: 1543).

In his response to Wacquant's critique, Duneier rejects the central criticism that sociological concerns did not shape his research questions and choice of field site. He explains, 'A core issue of my agenda was to

understand the ways in which “moral” behavior and “decency” are and are not constructed within settings seemingly unfavorable to such behavior’ (Duneier, 2002: 1551). Duneier also describes *Sidewalk* as engaging with theories about public space and safety popularized by Jane Jacobs (1961), who believed that increased activity corresponds with more ‘eyes on the street’, which serves a regulating function that militates against antisocial behavior.

Related to Anderson’s explanation of his inductive approach, Duneier explains, ‘My approach is not strictly inductive or deductive: I engage a variety of theoretical/ sociological questions, some of which I brought to the site from the beginning, some of which I discovered through various routes as I worked in the site’ (Duneier, 2002: 1566).

Like Anderson, Duneier criticizes Wacquant’s application of theory in ethnography as deterministic and overly committed to macro-level explanations without leaving adequate room for micro-level specificity and nuance. Duneier argues:

The ethnographer who allows theory to dominate data and who twists perception to cover the facts makes a farce out of otherwise careful work. There is a middle ground: to try to grasp the connections between individual lives and the macro-forces at every turn, while acknowledging one’s uncertainty when one cannot be sure how those forces come to bear on individual lives. (Duneier, 1999: 334; quoted in Duneier, 2002: 1572)

With regard to the role of theory, Katherine Newman rejects Wacquant’s characterization of her book, *No Shame in My Game* (1999), as primarily a journalistic glorification of the working poor that sings praise to their mainstream cultural values, while seemingly distinguishing them as more deserving than their peers who are not involved in the mainstream economy as low-wage workers. Newman explains that by focusing on the working poor in the inner city, she engages with earlier theories of the urban poor that emphasized their detachment from the labor force, their disconnection from ‘mainstream’ behavior, and the lack of role models that promote ‘mainstream’ values. ‘The fundamental purpose of *No Shame in My Game*’, Newman writes, ‘was to hold these conclusions up to the light of ethnographic research among low-wage workers and unemployed job seekers in Harlem ... The genesis then of *No Shame* was a confrontation with the central tenets of underclass theory’ (Newman, 2002: 1578).

Newman justifies the selection of places of employment as her field site as locations where the middle class and the urban working class interact. Among the specific claims she rebuts is Wacquant’s argument that she attributes workers’ decision to engage in formal employment exclusively to the strength of their values without taking into account material conditions that influence their course of action. While Wacquant criticizes her for

simply replacing the stereotype of an idle ghetto resident dependent on state support with that of a virtuous low-wage worker, Newman offers a defense of looking beyond academic debates to public discourse on issues of urban poverty. She argues, 'Wacquant can rail that moral judgment has no place in sociology, or that books like mine should not address the moral portraits that exist about the poor, but that is just head-in-the-sand thinking', since popular discussions of the urban poor are rife with negative moral judgment (Newman, 2002: 1591).

### Reflections on the debate concerning the separation of ethnography from theory

Sociologists generally concur with the view that theoretical issues ought to inform the interpretation of ethnographic data. Examples of theoretically sophisticated ethnography include Herbert Gans's *The Urban Villagers* (1962), Gerald Suttles's *The Social Order of the Slum* (1970), Ulf Hannerz's *Soulside* (1969), Michael Burawoy's *Manufacturing Consent* (1979), Paul Willis's *Learning to Labor* (1981), and Mario Small's *Villa Victoria* (2004). The issue is not whether but how theory is used in ethnographic studies. This is where the debate ought to begin and why we need to consider Wacquant's argument about the disconnect between recent urban ethnography and theory. If theory is to play a role in ethnography, should it be largely deductive, as Wacquant seems to suggest, or inductive, as reflected in Anderson's work? Or should theory in ethnographic research be both inductive and deductive, as suggested in the work of Duneier?

In his response to Wacquant's critique, Anderson makes a strong case for the inductive theoretical approach. Anderson states: 'the ethnographer should enter the field armed with a certain sociological sophistication'. For example, he points out that he has been influenced by theoretical work of scholars such as Simmel, Du Bois, Weber, Durkheim, Mead, Blumer, Goffman, and Becker. Armed with this theoretical knowledge, he enters the field with a theoretical perspective. As his fieldwork proceeds, this perspective helps him to 'formulate questions concerning the social organization of the subjects and their settings' (Anderson, 2002: 1536). Anderson argues persuasively that 'the most penetrating ethnographic questions often results from a fusion of concern that reflect both the ethnographers engagement of the social setting as well as his or her own sociological orientation' (Anderson, 2002: 1536–7).

Anderson's dismissive response to Wacquant's criticism that he fails to apply a systematic social map to understand how street-decent categories function in relation to the social positions of ghetto residents is less convincing. How does the arrangement of social positions in the community relate

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to the orientations that residents adopt? An answer to this question is important because it relates to what Wacquant means by theoretically informed research. Anderson maintains that the writings of classical theorists (Durkheim, Weber, Simmel, and others) informed his research, but this important orientation is implicit not explicit in the framing of his arguments in *Code of the Street*. Wacquant is not criticizing Anderson's theoretically informed approach to the material per se but, rather, the lack of explicit theoretical frameworks in his interpretation of the evidence. In particular, he has a problem with Anderson's reliance on folk concepts (i.e. street–decent) in his explanations. Anderson defends his use of folk concepts and incorrectly claims that Wacquant dismisses them. In fact, Wacquant says they can be descriptively useful, but he questions using them as the basis of analytical/theoretical explanations. Anderson uses 'street' and 'decent' inconsistently as categories in the book. They are better described as cultural frames through which the residents make sense of their world, their actions, and the behavior of others. Anderson most often describes 'street' and 'decent' as fluid categories between which the same person switches, yet in other cases he employs them as exclusive categories to which families belong. This inconsistency weakens the analytic strength of these concepts and leads Wacquant to raise questions about their theoretical import. Nonetheless, few scholars would deny that, with the use of these folk concepts, Anderson has produced some powerful new insights.

Indeed, one of the effects of living in neighborhoods that are racially segregated is exposure to group-specific cultural frames, worldviews, orientations, habits, styles of behavior, and particular skills that emanate from patterns of racial exclusion. Some scholars describe these as 'non-dominant forms of cultural capital' (Carter, 2003) that may not be conducive to facilitating social mobility in mainstream social institutions (Wilson, 2009). In Anderson's *Code of the Street*, some groups in the ghetto place a high value on 'street smarts', the actions and behavior that keep them safe in high-crime neighborhoods. In this milieu, it is wise to keep to yourself and avoid eye contact with strangers. This frame of mind may lead individuals to approach new situations with a certain level of mistrust or skepticism. While this approach is useful in certain contexts, the same orientation may inhibit the formation of valuable social ties in other settings. Moreover, this orientation may influence the racially tinged assessments of employers in job interviews and create perceptions that ghetto residents are undesirable job candidates (Kirschenman and Neckerman, 1991; Pager and Shepherd, 2008; Wilson, 1996).

Anderson's 'code of the street' is an informal but explicit set of rules developed to regulate violence and govern interpersonal public behavior in neighborhoods where residents view crime as high and police protection as low. The issue of respect is at the root of the code, argues Anderson. In a

context of limited opportunities for success and self-actualization, some ghetto residents, most notably young black males, develop alternative ways to gain respect that emphasize manly pride, ranging from talking the right way and wearing brand-name clothing with the 'right look' to asserting their power over others in the neighborhood. Anderson points out, however, that all the residents of these troubled neighborhoods are affected by the code of the street, especially young people who are drawn into this cultural frame in the schools as well as on the block and frequently adopt 'street' behavior as a form of self defense. As Anderson argues, 'the code of the street is actually a cultural adaptation to a profound lack of faith in the police and the judicial system – and in others who would champion one's personal security' (Anderson, 1999: 34).

In explaining ghetto responses to chronic subordination, Anderson provides a powerful argument within the context of validation, regardless of his use of folk terms to describe some patterns of behavior. Indeed, it would be difficult to defend the view that 'the code of the street' cannot be seen as both a descriptive and analytic construct, as Anderson uses it, in the context of validation.

As we reflect on Anderson's arguments on the merits of inductive theory in ethnographic research, we cannot think of any notable ethnography that is not in some way inductive. Key aspects of ethnographic research, such as participant observation, are almost by definition inductive, as researchers try to make sense of new findings uncovered in the field. Moreover, a number of theoretically informed ethnographies reflect both a deductive and inductive approach. Two of the most explicit are William Julius Wilson and Richard Taub's *There Goes the Neighborhood* (2006) and Mario Small's *Villa Victoria* (2004). A crucial theoretical question that Wilson and Taub raised in their ethnographic study of four working-class and lower middle-class neighborhoods in Chicago is to what extent their findings were consistent with the assumptions of Albert Hirschman's (1970) theory of exit, voice, and loyalty. Their findings led them to augment these theoretical formulations to develop a broader theory of the social organization of neighborhoods that integrate propositions from Hirschman's theory with theoretical arguments, based on their data, that associate the strength of neighborhood social organization with neighborhood ethnic stability and change. In his excellent book *Villa Victoria*, Mario Small set out to examine theoretical assumptions about poor neighborhoods and social capital by conducting research in a poor Puerto Rican neighborhood in Boston. However, he ended up developing a more comprehensive theory of poverty and social capital based on his imaginative use and interpretation of ethnographic data he collected in the field.

While Small and Wilson and Taub's studies represent an explicit integration of the deductive and inductive methods, Mitchell Duneier's *Sidewalk*

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is a good example of a study that implicitly integrates inductive and deductive theoretical insights. As Duneier points out in his response to Wacquant, his approach is neither strictly deductive nor inductive. He describes his engagement with theories about public space and safety, including those of Jane Jacobs and James Q. Wilson. Duneier does not argue that theory is unimportant to him, as Wacquant seems to suggest; he says that Wacquant ignores the attention he does give to theory.

Using the concept 'diagnostic ethnography', Duneier points out that he engages in a variety of theoretical/sociological questions, some of which he discovered while conducting fieldwork. He states:

The idea of diagnostic ethnography is not that ethnography extracts a theory out of clinical data. Rather the idea is that the ethnographer comes to a site with the sociological equivalent of the doctor's medicine bag of diagnostic tools derived from already-existing sociological theory and uses these tools to generate a specific explanation of the 'symptoms' in the site. (Duneier, 2002: 1566)

Although Anderson and Duneier argue that their work is theoretically informed, they are both critical of what they perceive as Wacquant's view of the role of theory in ethnography. Both suggest that too much prior theory is conducive to interpretations that tend toward structural determinism. As Anderson put it, Wacquant's 'top-down' approach would lead the ethnographer to 'subordinate the cultural complexity he or she finds in the field to that theory' (Anderson, 2002: 1534).

It is not clear to us, however, why the theoretically informed research that Wacquant advocates should necessarily lead to rigid interpretations of the data through the lens of structural determinism. Wacquant seems to be arguing that ethnographic work should be theoretically informed, and his views are probably more consistent with a deductive theoretical approach. But nothing in his review essay suggests that he does not see the value of inductive theoretical insights, whereby theoretical knowledge aids in the interpretation of new findings uncovered in the field.

There are some pitfalls in using theory to guide research. Just as a strictly deductive approach could lead researchers to overlook important nuanced behavior not consistent with prior theoretical arguments, so too an inductive approach could result in an inappropriate use of certain theoretical insights to interpret new findings. Few scientific endeavors are free of errors. The extent to which work can withstand critical and prolonged scrutiny in the context of validation will be based in large measure on the researcher's creative insights in the discovery and integration of empirical findings and theoretical ideas.

Wacquant's critique of Anderson and Duneier prompted the two scholars to make explicit the theoretical issues they believe are implicit in their

research. We see their responses as positive, not as a defensive argument in support of their works, but in informing the reader of the subtle but important role that theory plays in their research. We see their work as representing the logic of social inquiry, legitimately falling within both the context of discovery and the context of validation.

We have a different view of Newman's book, *No Shame in My Game* (1999). In response to Wacquant's critique about the theoretical basis of her study, Newman defends her research on the grounds that 'underclass theory' presented the urban poor as detached from the labor force, not subscribing to mainstream values, deviant, or otherwise outside the social norm. She explains that she then:

looked to see whether the unemployed were so separated from workers; whether welfare mothers and their kin were as far removed from the world of work as this theory asserted; whether role models were really absent from the ghetto; whether ghetto dwellers appreciate the role of education in later mobility; and whether 'mainstream' models of behavior had really disappeared with the exodus of more affluent families'. (Newman, 2002: 1578)

There are major problems with this argument. The objective of the 'underclass theory' to which she refers was to analyze the structural forces that produced conditions of concentrated poverty and joblessness and to examine the implications of these factors for inner-city residents. The arguments associated with 'underclass theory' did not simply stipulate that people were not working or engaged in behavior associated with work. Rather, the arguments attempted to provide theoretical explanations for why conditions in inner-city neighborhoods had changed over time and how social processes in those contexts resulted in increasing, though not universal, joblessness.

Simply describing supposedly contrary evidence showing that, indeed, many people in the inner city are working and subscribe to mainstream values does not refine or challenge this theory. Unlike Anderson and Duneier, Newman is not explaining the social mechanisms or processes underlying observed social phenomena. To be sure, media coverage, public discussion, and academic research transmuted these concepts from describing those who were economically marginalized by structural economic shifts to inappropriately characterizing entire communities primarily by their own attitudes and behaviors (Wilson, 1991). To the extent that dominant popular conceptions portray the urban poor as universally idle and rejecting mainstream values, then by documenting the working poor Newman is indeed countering those stereotypes. However, that is not the same as challenging an earlier theory which simultaneously recognize that a substantial proportion of the urban poor are regularly engaged in the urban labor market and puts forth arguments to explain certain changing conditions

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and social processes in the inner city that affect increasing concentrations of poverty and joblessness.

Newman argues that, contrary to Wacquant, sociology should indeed address moral portraits of the poor. The critique made by Wacquant, however, is that Newman merely presents a moralistic view of the working poor, rather than analyzing moral depictions of them. Indeed, several important studies analyze how moral constructions of the poor influence the formation of social policy (Katz, 1989; Skocpol, 1992). Newman suggests that challenging negative moral depictions of the poor is important, presumably to influence the discourse around policy reform. Public engagement with pressing political and social issues should not be discounted, especially given the troubling terms of public discourse around the urban poor. A contribution to policy discussion, however, should not be misleadingly conflated with contributing to a sociological explanation of the behavior and experiences of the poor. Newman's arguments seem more oriented toward the strategic framing of a political issue. We do not pass judgment on either objective, but rather highlight the distinction and note that they are not always overlapping. In short, Newman is neither adequately addressing theoretical issues consistent with the logic of inquiry nor making a theoretical contribution in an inductive or deductive sense to our understanding of the forces and processes that shape behavior and social outcomes.

**Conclusion**

We began this discussion by pointing out that scholars in the field have not seriously debated the important issues that Loïc Wacquant raised in his controversial review of books by Elijah Anderson, Mitchell Duneier, and Katherine Newman concerning the disconnect between theory and ethnographic research. Despite the tone of Wacquant's review, we feel that he made a contribution in raising important issues about the role of theory in ethnography. The responses to his review that address this issue, especially those by Anderson and Duneier, are also important because they help to broaden our understanding of how theory is used in ethnographic research, particularly the inductive theoretical approach. Ethnography has always been a vital part of the sociological undertaking. Scholars are beginning to appreciate its role in the context of validation; it is no longer relegated solely to the context of discovery.

What we take from this exchange is that good ethnography is theory driven. Given the nature of ethnographic research, it is likely to be much more reflective of inductive theoretical insights than those that are purely deductive. Moreover, in some ethnographic studies the theoretical insights

are neither strictly deductive nor inductive, but represent a combination of both. We might venture to say that the most creative ethnography reflects this synthesis.

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