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Source: *Annual Review of Sociology*, 2001, Vol. 27 (2001), pp. 23-45

Published by: Annual Reviews

Stable URL: <https://www.jstor.org/stable/2678613>

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# URBAN POVERTY AFTER *THE TRULY DISADVANTAGED*: The Rediscovery of the Family, the Neighborhood, and Culture

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**Key Words** inner city, neighborhood effects, underclass, culture, birthrates

■ **Abstract** In what follows we critically assess a selection of the works on urban poverty that followed the publication of WJ Wilson's *The Truly Disadvantaged* (1987), with a particular focus on the family, the neighborhood, and culture. We frame our discussion by assessing the broad explanations of the increased concentration of poverty in urban neighborhoods characteristic of the 1970s and 1980s. Then, in the section on the family, we address the rising out-of-wedlock and disproportionately high teenage birthrates of poor urban women. Next, we critique the literature on neighborhood effects. Finally, in the discussion of culture, we examine critically the new efforts at complementing structural explanations with cultural accounts. We conclude by calling for more comparative, cross-regional, and historical studies, broader conceptions of urban poverty, and a greater focus on Latinos and other ethnic groups.

## INTRODUCTION

The most important publication in urban poverty over the past twenty-five years has been WJ Wilson's *The Truly Disadvantaged* (1987; henceforth, *TTD*). The hundreds of books and articles on urban poverty that followed the book's publication all responded in some way to the hypotheses it advanced. In what follows, we examine critically a selection of this literature, focusing on three topics—the family, the neighborhood, and culture—that we find especially important. Indeed, researchers have recently examined critical questions on these three topics that either lay dormant during the 1970s and 1980s or had not been addressed systematically at all. Given space limitations, our coverage must be selective, emphasizing the issues we find to be most pressing. We must also ignore other important topics in urban poverty—such as immigration, ethnicity, and politics. The latter,

however, are covered extensively in several recent reviews (Waters & Eschbach 1995, Walton 1993, Marks 1991, Teitz & Chapple 1998).

## THE NEW URBAN POVERTY

*The Truly Disadvantaged* argues that, since 1970, structural changes in the economy, such as the shift from manufacturing to service industries and the departure of low-skilled jobs from the urban centers, increased black joblessness in central city ghettos (Wilson 1987, 1991a, 1996). The inner cities also suffered from the flight of middle- and working-class blacks who took advantage of affirmative action and fair housing laws to relocate to higher-income urban neighborhoods and the suburbs. As working families departed and the nonworking families stayed behind, inner-city neighborhoods became mired in concentrated poverty. The result, Wilson argues, was a new “underclass” of single-parent families, welfare dependency, joblessness, and overall increased “social pathologies” (1987:viii).

Most sociologists agree that (a) urban poverty changed over the 1970s and 1980s and that (b) it became more concentrated. (What happened over the 1990s will be known when Census 2000 data become available.) But there are marked differences in how sociologists think about these two issues, reflecting conceptual arguments over the dynamics of urban poverty. We discuss these differences below to frame our discussion of the family, the neighborhood, and culture.

### Recent Urban Poverty as a New Phenomenon

Wilson and others argue that changes in the inner city produced a new, distinct, and growing phenomenon: “the underclass” (Kasarda 1989, Wacquant & Wilson 1989, Wilson 1987, Massey & Denton 1993; see also Marks 1991, Lawson 1992, Mincy 1994, Jargowsky & Bane 1990). This class is characterized by its geographic concentration, its social isolation from the middle class, and its joblessness. Indeed, it is these characteristics, Wilson argues, that distinguish the underclass as a new entity.

Jencks (1991, 1992) counters that the idea of a new class contributes nothing to our understanding of the urban poor, and he suggests that “the underclass” is simply a new term for what was known in the 1960s as “the lower class.” He believes, furthermore, that the term gives the mistaken impression that all urban problems are worsening, and he argues instead for a focus on the proportion of individuals experiencing specific social problems, such as joblessness and dropping out of school.<sup>1</sup>

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<sup>1</sup>This is a version of the classic debate in stratification between the “class approach,” which posits that society is composed of distinct classes, and the “gradational approach,” which posits that society is composed not of classes but individuals who vary in terms of income, occupation, and status (Grusky 1994).

At the time of Jencks's writing, male joblessness had risen and out-of-wedlock births had increased (and still continue to increase), but dropout rates and reading and mathematics skills had improved (and continue to improve, though only slightly in the case of reading and math) (Jencks 1991, Jencks & Peterson 1991, US Census Bureau 1999). In addition, the rate of black male joblessness, high throughout the 1980s, declined over the 1990s as labor markets tightened (Freeman & Rodgers 1999). The notion of an underclass, Jencks argues, masks the diversity of these trends.

At the onset of this debate, several researchers grappled with whether the underclass was a separate class (Marks 1991, Lawson 1992, Mincy 1994, Jargowsky & Bane 1990), but over time both the term and that debate have gradually been abandoned. Given the negative implications of the term, Wilson officially abandoned it in favor of "the ghetto poor" (Wilson 1991b), a change that also represented a subtle shift from a designation of classes to one of individuals. The only component of the underclass still debated is the neighborhood, which we discuss at length later. Many sociologists were convinced by Jencks's (1991) argument for the individual-centered approach (an approach perfected over decades by status-attainment researchers) because it allows for a relatively parsimonious discernment of which urban problems are worsening and which are not. Yet some worried about the dangers of the approach; a too strict individual-centered perspective assumes unproblematically that other units of analysis such as classes, networks, or places are unimportant.

The result over the past few years has been a sort of amalgam of the two approaches. Most empirical studies now implicitly follow a model of society as a collective of individuals (not classes), but individuals whose neighborhood of residence is important. This middle ground provides the important benefit that individuals are much easier to measure than classes (see, e.g., Wright 1985) and that neighborhoods can be measured by means of the census tract. However, it has left sociologists with little motivation to think theoretically about the relationship between neighborhoods and people, pushing researchers instead to focus on the methodological problems of using neighborhoods to explain individual variation (see section on neighborhoods). It has also, finally, left unresolved the question of whether recent urban poverty constitutes a sociologically new phenomenon.

## The Increased Concentration of Poverty

One of Wilson's arguments has provoked little disagreement, that urban poverty became more concentrated over the 1970s and 1980s. Jargowsky (1994, 1997), who defines a high-poverty neighborhood as one in which 40% of the population is poor, confirms that both neighborhood poverty (the number of such neighborhoods) and the concentration of the poor (the proportion of all poor people living in such neighborhoods) increased between 1970 and 1990. The question is, Why? Three principal explanations have been offered (Quillian 1999; see also Teitz & Chapple 1998).

One is the black middle-class flight model discussed above (Wilson 1987), which blames the departure of working blacks from the inner city (Wilson 1996, Jargowsky & Bane 1991, Jargowsky 1997). A second is the residential segregation model, which argues that fair housing laws were so poorly enforced that they did little to dismantle the racial divide in housing; this lack of enforcement, combined with the growth of black poverty in cities, led to the increase in neighborhoods with high concentrations of poor black people (Massey & Denton 1987, 1993, Denton & Massey 1988, Massey et al 1994). The third is the departure of low-skilled jobs model, which, borrowing from Kain's (1968) spatial mismatch thesis, argues that manufacturing jobs moved from inner-city neighborhoods, especially during the 1970s, increasing joblessness among black men and thus increasing the neighborhoods' concentration of poverty (Wilson 1987, Kasarda 1989, Weicher 1990, Kain 1992, Jencks & Mayer 1990a).

A great deal of the literature pits these explanations against one another as if they were mutually exclusive when in fact they are not. Quillian (1999) explains this cogently in a recent paper that ostensibly adjudicates between Wilson's black middle-class flight and Massey & Denton's segregation explanation. Relying on census-matched longitudinal evidence, Quillian shows that over the 1970s and 1980s nonpoor blacks were moving rapidly into white neighborhoods, as Wilson argued. But he also finds that racial segregation is a more important explanation than middle-class black out-migration of the present concentration of poverty, as Massey & Denton argued. Although segregation is a better account of contemporary concentrations of poverty, it cannot explain the increase in poverty concentration over the 1970s and 1980s because during that time the levels of segregation did not increase—in fact, they declined slightly (Massey & Denton 1993:64, Farley & Frey 1994). Quillian (1999) suggests, therefore, that middle-class flight correctly explains the movement of blacks over time; residential segregation strongly accounts for the concentration of poverty in a cross-section of neighborhoods. In general, however, few studies employ the requisite longitudinal data to assess these explanations; thus, although the three of them appear to contain a grain of truth, none of them has been overwhelmingly supported.

These accounts, along with much of the post-*TTD* work on urban poverty, are also weakened by several important limitations. Three are particularly salient: (a) their focus on dense cities in the midwest and northeast; (b) their almost exclusive focus on African-Americans; and (c) their relative neglect of the influx of new immigrants to the inner cities (see Weicher 1991, Jargowsky & Bane 1991, Waldinger & Bozorgmehr 1996, Moore & Pinderhughes 1993, Waters 1999, Waters & Eschbach 1995, Ong et al 1994). The major perspectives described above would be of little help in explaining the changes taking place over the past 30 years in Los Angeles, Houston, or Miami, cities not only outside the northeast-midwest belt but also populated by high proportions of nonblack poor people, many of whom are immigrants (see e.g., Waldinger & Bozorgmehr 1996). [In a recent review prepared for this journal, Waters & Eschbach (1995) cover the literature on immigration and other ethnic groups extensively.] During the discussions that

follow, we note several points on which the literature is held back by this undue focus on African-Americans in the northeast and midwest.

## FAMILY STRUCTURE

Recent studies of poor urban families have focused on family management and parenting practices (Furstenberg et al 1999) and the causes and consequences of teenage and out-of-wedlock births (McLanahan & Sandefur 1994, Geronimus 1991, Geronimus & Korenman 1992, 1993, Hoffman et al 1993a, 1993b, Cherlin 1992, Luker 1996, Kaplan 1997). Here we dwell on the debates over causal explanations, which we consider the most pressing.

Wilson (1987) argues that much of the increase in urban poverty reflects the sharp increase in the proportion of female-headed families among blacks. He correctly points out that black women are marrying at lower rates and never-married black women are having children at a higher rate than are married black women. The main concern of this literature has been to explain why, and several researchers either produced new explanations or revisited old ones.

We should note two issues. First, these are in fact two separate phenomena: the teen birthrate and the out-of-wedlock birthrate. Since most births to teenage mothers occur out of wedlock, many sociologists have addressed the two problems as one, but they are distinct. Only the out-of-wedlock birthrate is rising rapidly, though the teen birthrate is disproportionately higher among blacks and Latinas than among whites. Second, most of these researchers focus not on poor women in general but on black women (but see Luker 1996, Vega 1990, Dietrich 1996). This bias results in part from a legitimate demographic concern: the rate of births to unmarried mothers among blacks in 1997 was a disturbing 69.1%; their rate of births to teens was 22.5%, double that of whites (US Census Bureau 1999). But the out-of-wedlock and teen birthrates among Latinas were not far behind (40.9% and 17.3%, respectively), while the out-of-wedlock birthrate is growing faster among whites than any other race (US Census Bureau 1999). Thus, the literature we discuss below tends to present a skewed picture of the problem, a picture that should be corrected with more studies on Latinas and whites, to say nothing of other ethnic groups.

### Out-of-Wedlock Births

The recent literature has produced three new (or revisited) explanations for the increasing proportion of out-of-wedlock births among poor urban (mostly black) women. One is the marriageable-male-pool explanation, which posits that declining economic opportunities in the inner cities reduced the pool of black men with steady jobs, leaving black women with fewer attractive mates (Wilson 1987). Another is the slavery explanation, revived recently by Patterson (1998), which contends that the institution of slavery produced lasting effects on relationships

between black men and women, including the tendency of black women to bear children out of wedlock. A third explanation argues that widespread cultural changes throughout American society are to blame, since out-of-wedlock births have increased among women of all races (Jencks 1992, Cherlin 1992; see also Luker 1996).

None of the explanations has accumulated enough evidence to be overwhelmingly convincing. Proposed partly in response to Murray's (1984) argument that federal welfare policies were to blame, the marriageable-male-pool hypothesis is the most empirically substantiated, though support has been mixed (Rolison 1992, Stokes & Chevan 1996, Hess 1990, Lerman 1989, Mare & Winship 1991). Moreover, since the mid-1980s, when the hypothesis was advanced, the jobless rate among black men has plummeted; if the explanation is correct, this shift should have increased the marriage rates among poor urban blacks. But no one, as far as we can tell, has tested for this.

The other two explanations have received relatively little empirical attention, partly because good data to test them are more difficult to obtain. Patterson's revival of the slavery hypothesis is an attempt to counter the work of Gutman (1976), whose study of ante-bellum census records convinced many sociologists that the effects of slavery on the black family were negligible (see also Preston et al 1992). One of the main problems with the slavery explanation is the impossibility of calculating a metric that can associate slavery with a certain percent decline in blacks' likelihood of marriage. This tends to frustrate researchers who expect precise estimates, thus weakening the reception of the work. Most sociologists sympathetic to the theory support it partly, but suggest that other factors matter as well (Cherlin 1992). A proponent of the widespread cultural change argument, Jencks (1992:134) notes that "during the 1960s . . . [society] moved from thinking that society ought to discourage extramarital sex, and especially out-of-wedlock births, to thinking that such efforts were an unwarranted infringement on personal liberty." Yet it is hard to ascertain whether the shift in cultural attitudes led to across-the-board changes in out-of-wedlock births, or whether the increase in those births led to changes in public attitudes. We know of no tests that ascertain which way the causal arrow points.

## Teenage Births

Sociologists have produced three new (or revisited) explanations for the high rate of teenage births among the poor in urban centers. One is the inner-city culture explanation, which contends that "street" families in poor (usually black) urban neighborhoods share a cultural code whereby early sexual activity (despite its risks) improves girls' reputation among peers (Anderson 1994, 1991, 1999). Another is the "weathering" explanation, which posits that deterioration in the health of poor women leads to premature aging and therefore transforms early childbearing from a risky strategy to a rational response to objective conditions. Older black mothers have higher infant mortality rates; hence, the explanation goes, having children



at an early age makes sense (Geronimus 1991, 1996, Geronimus & Bound 1990, Geronimus et al 1996, 1999a,b). Finally, a few researchers advance what Kaplan (1997) labels the “poverty of relationships” explanation, which states that women have children as teens to make up for the unsatisfying relationships they sustain with teachers, mothers, fathers, and boyfriends (also Dietrich 1998). In Kaplan’s words (1997:181), the black teen mothers “had babies because they were isolated from society and unwanted by everyone around them.”

The three explanations have been examined with mixed results. Elements of Anderson’s thesis find some support (Luker 1996:138–39), but Dietrich (1998:47) finds that the Chicana girls she studies value virginity above all, and that it is virginity, not promiscuity, that assures them a favorable reputation. With respect to the weathering hypothesis, there was some debate early on over the quality of the data (Furstenberg 1991, 1992, Geronimus 1991), but recently Geronimus and her colleagues have accumulated convincing evidence that mortality rates are higher among African-Americans in general, the infants of older blacks, and the black urban poor (Geronimus et al 1996, 1999a,b). Yet it remains unclear whether black female teens are aware of the infant mortality differential between themselves and adult black women, and whether this knowledge motivates them to bear children early (see Furstenberg 1991). Thus, the weathering hypothesis is at a critical juncture where the demographic evidence must be complemented by interview data. Kaplan’s explanation is a refinement of a general theory that links women’s poverty to both their loneliness and a yearning for social status (see also Dietrich 1998, Stack 1974).

In her comprehensive treatment of the issue, Luker (1996:134–174) advances a multi-causal explanation that attributes disproportionately high birthrates of minority teens to their poverty, their limited life choices, their ineffectiveness (for various reasons) with respect to contraception, and the difficult negotiations around sex that take place with their male partners. She also presents enough evidence to suggest that each of these explanations is plausible. This multi-factor approach constructively avoids what we call the trap of false adjudication: the inclination to pit explanations against each other that are not necessarily contradictory.

In all, the new literature on both teen and out-of-wedlock birthrates shows some promising leads, but much more empirical work remains to be done, especially on other ethnic groups. This research is critical because the higher these birthrates are among a poor population, the higher the chances its members will experience long-term poverty.

## THE NEIGHBORHOOD

Perhaps no single question in urban inequality has produced more research than whether neighborhood poverty affects the life chances of the poor. Wilson (1987) argues that the concentration of poverty results in the isolation of the poor from the middle class and its corresponding role models, resources, and job networks; more generally, he argues that being poor in a mixed-income neighborhood is



less damaging than being poor in a high poverty neighborhood. Concentration effects increase the likelihood of being unemployed, dropping out of school, taking up crime, and becoming pregnant out of wedlock. A large body of empirical research has tested for neighborhood effects on unemployment (Vartanian 1999, Elliott 1999), dropping out of school (Crane 1991), crime (Sampson & Groves 1989), out-of-wedlock births (Crane 1991, Anderson 1991, 1999, South & Crowder 1999), and cognitive development (Brooks-Gunn et al 1997a, 1997b). The body of research is large enough to require its own separate review, and indeed, several of them exist (Jencks & Mayer 1990b, Gephart 1997). Instead of repeating the work of these reviewers, we (a) discuss the most important methodological problems with measuring whether neighborhoods have these hypothesized effects; (b) report the latest findings on whether neighborhood poverty affects life chances; and (c) examine what we argue is the most pressing unresolved question with respect to neighborhood effects: how they work.

Much of the literature on neighborhood effects has been methodological, and with good reason. It is extremely difficult to test the hypothesis that, everything else being equal, an individual living under any particular neighborhood condition is worse off than in the absence of that condition. Several problems are related to this difficulty, such as the need for longitudinal data, the challenge of disentangling neighborhood from school effects, and the possibility of nonlinear effects (for extended discussions, see Duncan et al 1997, Tienda 1991, Jencks & Mayer 1990b). But two problems are particularly important.

First, people are not randomly distributed across neighborhoods. People live in neighborhoods as a result of both observable and unobservable characteristics that may themselves, independently of neighborhoods, affect life outcomes. For example, parents with little education are more likely to live in poor neighborhoods, and they are also more likely to have children who drop out of high school. By neglecting to control for the parents' low educational attainment, researchers may overstate the impact of living in a poor neighborhood. Most published studies of neighborhood effects deal with this question in a perfunctory fashion, adding a small number of controls for parental education and income. There are exceptions, such as Duncan et al (1997), who control for a battery of typically unmeasured variables; Duncan et al (1997) and Cutler & Glaser (1997), who make use of instrumental variables; and Rosenbaum & Popkin (1991), who, via the Gautreaux program, approximate a randomized experiment (see also Spencer et al 1997). But the bottom line is that most neighborhood studies are unable to make causal links and can only point to strong associations.

Second, how do we define and measure neighborhoods? The problem involves three interrelated issues: conceptualizing neighborhoods, drawing their geographic boundaries, and determining which neighborhood characteristics should be used to measure disadvantage. Most sociologists conceptualize neighborhoods in terms of informal relationships or social networks among persons living in a geographic space; thus, when we use the term "neighborhood" we tend to mean "community" (Wellman 1988, Chaskin 1997, Sampson 1999). But geographic location

and social networks are separate and distinct attributes that may have different effects on individuals. Failing to account for this and for the many possible ways neighborhoods may be defined will result in an increasingly muddled discourse on the effects of neighborhoods on people. A few recent scholars have proposed that we conceive of neighborhoods in terms of several separate and complementary dimensions, such as (a) a social space, (b) a set of relationships, (c) a set of institutions, and (d) a symbolic unit (Chaskin 1997); or that we think of neighborhoods alternatively as (a) sites, (b) perceptions, (c) networks, and (d) cultures (Burton et al 1997). These works, though still in their conceptual infancy, push us toward the important task of developing greater clarity over what is meant by neighborhoods.

Even if we sharpen our thinking about neighborhoods in terms of nongeographic concepts, we still have to determine their geographic boundaries if we want to test whether they matter for poverty outcomes. This task is not straightforward. Most sociologists resort to the census tract, but, depending on how we think neighborhoods matter, census tracts may be woefully inadequate proxies. For example, the perceptions of local residents regarding the boundaries of their neighborhoods may be important determinants of how the neighborhoods affect them; in that case, census-tract operationalizations will be of little use. Some scholars suggest replacing census tracts with the smaller block groups (a practice common in the fields of demography and public health), which allow for a narrower geographic area of socialization (C Jencks, personal communication). A few recent studies (Sampson et al 1997, Sampson & Groves 1989) employ “neighborhood clusters” or “localities,” which are neighborhood boundaries drawn by researchers explicitly for the purpose of studying neighborhood effects. The advantage of these clusters is that they are often drawn with an eye to local perceptions about what constitutes the end of one neighborhood and the beginning of another. The disadvantage is that the more accurately they reflect local perceptions of neighborhood boundaries, the more costly and time-consuming it is to draw them.

Finally, what characteristics should we employ to measure disadvantage (Gephart 1997, Mincy 1994, Elliot et al 1996)? There are many possibilities: neighborhood poverty, segregation, the unemployment rate, and the level of educational attainment. Consequently, some researchers have combined these measures into composite “disadvantage” or “risk factor” indexes that encompass race, class, and other variables (e.g., Brooks-Gunn et al 1997a,b, Duncan & Aber 1997, South & Crowder 1999). These indexes have the advantage of statistical parsimony, especially since many of these variables tend to be correlated. Yet indexing makes replication cumbersome, especially when the index employs survey data. Furthermore, that solution does not help us discern which neighborhood characteristics affect people and which do not. The substantively important question is whether neighborhood unemployment or racial homogeneity or resource-deprivation affects life chances. A composite index that lumps all of these variables obscures which factors are creating the effect (see Massey 1998).

Methodological difficulties notwithstanding, several major recent studies have collected copious and increasingly sophisticated data that suggest neighborhoods matter with respect to certain variables. In an early release of the findings from the Gautreaux program, Rosenbaum & Popkin (1991) report that low-income black families who moved from public housing to the suburbs were more likely to be employed than similar families who remained in the inner city. Brooks-Gunn et al (1997a,b,c), employing data from the Working Group on Communities and Neighborhoods, Family Processes, and Individual Development, find that neighborhood conditions are often predictors of children's development, that the effect is strongest during early childhood and late adolescence, and that affluent or middle-class neighborhoods increase children's development and improve adolescents' achievement. After interviewing 500 families in Philadelphia, Furstenberg et al (1999) uncovered the fact that neighborhood conditions affect parents' family management practices (e.g., the more dangerous the neighborhood, the more restrictive the parents), but not achievement among early adolescents. Relying on data compiled in Chicago, Denver, and Philadelphia by the Neighborhood Project, Elliott et al (1996) find that neighborhood disadvantage affects successful behavioral development and delinquency among adolescents, but only via the mediating effect of informal social control (a concept encompassing many of the variables of social organization and collective efficacy, which we discuss below). Using data from the Multi-City Study of Urban Inequality, Tigges et al (1998) find that neighborhood poverty significantly increases social isolation and decreases access to resources. The ambitious Moving to Opportunity study is preparing early findings at the time of this writing. We should reiterate that most of these studies, though they may point to strong correlations, cannot make causal statements. Still, if these studies are any indication, future, more sophisticated work will probably show that (a) neighborhoods affect life chances during early childhood and late adolescence, that (b) most neighborhood effects are not as strong as family effects, and that (c) social networks, which sometimes are linked to neighborhoods but often transcend them, are critical.

## The Mechanisms Behind Neighborhood Effects

How does neighborhood poverty produce its negative effect? Surprisingly few studies have tackled this question seriously, although many researchers have argued that we need to do so (Jencks & Mayer 1990b, Tienda 1991, Furstenberg & Hughes 1997). Based on the works that have addressed this question (Wilson 1987, 1996, Jencks & Mayer 1990b, Massey & Denton 1993), we identify two general categories of models: socialization mechanisms, which describe how neighborhoods socialize those who grow up in them, and instrumental mechanisms, which describe how individual agency is limited by neighborhood conditions.<sup>2</sup>

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<sup>2</sup>Jencks & Mayer (1990a) propose a categorization based on whether advantaged or disadvantaged neighbors are beneficial or detrimental to a person's life chances. Our categorization is based on the type of effect the mechanism is purported to have.

Socialization mechanisms tend to conceive of individuals as (relatively passive) recipients of powerful socializing forces, suggesting that neighborhoods mold those who grow up in them into certain behavioral patterns. For this reason, these mechanisms tend to focus on children and adolescents. There are six socialization mechanisms. The epidemic model (Jencks & Mayer 1990b, Wilson 1987) argues that when many of a child's neighborhood peers engage in a certain type of behavior, the child will be socialized into engaging in such behavior. The collective socialization model (Jencks & Mayer 1990b, Wilson 1987) argues that having a scarcity of successful role models in their neighborhood makes children less likely to envision success for themselves (see also Cutler & Glaeser 1997, Newman 1999). The institutional model (Jencks & Mayer 1990b) argues that non-resident adults (such as teachers and police officers) attached to institutions in the neighborhood will treat young people worse if the neighborhood is poor (thus either teaching them poorly or treating them as criminals). A fourth, the linguistic isolation model, refers to the socialization of African-American children in poor, segregated neighborhoods (Massey & Denton 1993, also Labov & Harris 1986). It argues that black children under such circumstances become isolated from Standard American English, absorb only Black English Vernacular, and therefore do poorly in school and when interviewing for jobs. The fifth, relative deprivation, model argues that poor children will be worse off in rich than in poor neighborhoods (Jencks & Mayer 1990b). Because people judge their economic position by comparing themselves to those around them, poor children will develop more unfavorable opinions of themselves the richer the neighborhoods they live in, resorting (in many cases) to deviance as a maladaptive response. The sixth is the oppositional culture model (Massey & Denton 1993 and Jencks & Mayer 1990b call this the cultural conflict model), which argues that either segregation or neighborhood poverty causes residents to develop a culture opposed to mainstream norms and values.

Whereas socialization models explain how neighborhood environments socialize individuals, instrumental models focus on how individual agency is limited by neighborhood environment. Here, the mechanisms tend to focus on adults, rather than children and adolescents. The most prominent of these is the networks isolation model,<sup>3</sup> which argues that being in a poor, or extensively unemployed, neighborhood will disconnect individuals from social networks of employed people, making it difficult for them to obtain information about job opportunities (Wilson 1987, 1996, Elliott 1999, Tigges et al 1998). The resource model argues that poor neighborhoods, deprived of institutional resources such as schools, churches, recreational areas, and daycare centers, make it difficult for parents to raise their children effectively (Wilson 1987, Brooks-Gunn et al 1997a,b). (See Jencks & Mayer 1990b for a variant of this model.) The final one is the limitation of

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<sup>3</sup>Wilson uses the term "social isolation" to encompass the combined effects of job network isolation, role models, peers, and resource deprivation. We believe that disentangling them into their separate components allows for more systematic future work.

political alliances model. Massey & Denton (1993), focusing not on neighborhood poverty but on neighborhood segregation, argue that blacks have a difficult time developing political alliances across racial lines because, in conditions of segregation, no neighborhood-specific benefits accrued to blacks will accrue to members of other races. Consequently, they are unable to attract the public resources that will undergird decent schools, playgrounds, and business investment.

Most of these models have received little or no theoretical or empirical attention (Newman 1992; but see Rankin & Quane 2000, Fernandez & Harris 1992, Huckfeldt 1983, Elliott 1999, Tigges et al 1998). Most of them require observational and interview data that students of urban poverty have not collected systematically. Students have also spent little time thinking through these explanations, some of which rely on tenuous assumptions about how much time people spend in their neighborhoods, how much they interact with their neighbors, and how attitudes and values develop (see Wellman 1988, 1999, Fischer 1982).

There is one more model of how neighborhood effects work, one that focuses on the effects not on individuals but on neighborhood crime. Building on the foundation laid by the works of Shaw & McKay (1942), Sampson and his colleagues argue that a major cause of delinquency is social disorganization or the lack of collective efficacy (Sampson 1988, 1999, Sampson & Groves 1989, Sampson & Raudenbush 1999, Sampson & Wilson 1995, Sampson et al 1997). In empirical papers, both terms have referred to (a) the density of social networks in a neighborhood, (b) the extent of neighbors' involvement in voluntary associations, and (c) the degree to which neighbors are willing to supervise the young and intervene in social situations for the collective good; the term collective efficacy has tended to refer in greater degree to the latter attribute. Sampson and his colleagues have collected a wide array of evidence to demonstrate that neighborhoods with a high level of social organization and collective efficacy have lower crime rates, regardless of their poverty level. Since poor neighborhoods tend to be lower on these factors, their crime rates tend to be high.

Yet like most neighborhood studies the work is unable to make strong causal statements. Furthermore, Patillo-McCoy (1998, 1999) finds in recent work that social organization does not always lead to lower crime. On the contrary, in the black middle-class neighborhood she studied, dense, organized internal networks between gang and nongang residents make for highly organized, powerful drug-dealers. Most of the dealers grew up in the neighborhood, so residents, reluctant to see their nephews, cousins, and grandchildren in jail, fail to seek the police enforcement the neighborhood could use. A more general problem with the social organization literature is that, despite its impressive cross-sectional evidence, it has produced little on how social organization is generated in a neighborhood and how it changes over time (but see Gregory 1998).

The literature on neighborhood effects has produced some of the most fruitful, and in some ways most sophisticated, recent work in urban poverty. Much of this work has been methodological of necessity. But this narrow focus has shifted attention away from the important questions of how we should think about

neighborhoods and by what mechanisms they affect the people who live in them. This neglect makes many of the studies on neighborhood effects unsatisfying, both because they often fail to account for the methodological problems that make them unreliable and because often the most they can tell us is that census tracts are highly correlated with certain social problems. In the words of Tienda (1991:258), “[b]efore encouraging further statistical modeling to capture neighborhood effects, more conceptual groundwork is needed to specify . . . the exposure, selection, and feedback effects that define how neighborhoods shape the behavior of the poor.”

## CULTURE

The rigid distinction between structural and cultural explanations has begun to loosen, and many sociologists now employ both types of explanations in their accounts of urban poverty (Anderson 1999, Massey & Denton 1993). Wilson (1987, 1996), generally known as a structuralist, argues that, though the lack of jobs was the ultimate cause behind the inner-city destitution, cultural and behavioral patterns perpetuate the conditions of the poor. Because this resurgence is still in its infancy, much of the new work is a revision or revisiting of theories developed during the late 1960s and early 1970s. Below we cover this work, focusing on inner-city or underclass culture and on the approaches from the sociology of culture recently being applied to urban poverty.

### Inner-City or Underclass Culture

An overarching issue in this new literature has been whether there is an inner-city or underclass culture that, whatever its cause, perpetuates inequality (see Marks 1991, Gould 1999, Lawson 1992, Mincy 1994, Wilson 1996). The issue requires sociologists to examine (a) how they define culture, (b) whether there is a relatively homogenous culture of the inner city, and (c) whether this inner-city culture, if it exists, is significantly different from other forms of American culture. The first question has tormented both sociologists and anthropologists for decades, and there is no reason to believe we will ever arrive at a consensus. The discourse, however, appears to have shifted away from the narrow definitions of culture as values or norms characteristic of conservative accounts. Most sociologists today who think about this issue follow (at least nominally) Swidler’s (1986:273) definition of culture as a “‘tool kit’ of habits, skills, and lifestyles from which people construct ‘strategies of action.’” Beyond this, however, students of urban poverty still treat culture somewhat simplistically.

Equally important are the questions of whether it is sensible to think of inner-city culture as internally homogeneous, and in what ways this culture differs from middle-class or mainstream culture. Most scholars do not view inner-city culture as completely homogeneous, but much of the literature, focused largely on African-Americans, ignores the diversity created by the influence of Latinos, Asians, and



West Indians living in inner cities and large metropolitan areas (see Waters & Eschbach 1995, Moore & Pinderhughes 1993, Bourgois 1995, Waters 1999). To the extent that contemporary scholars see inner-city culture as heterogeneous, they usually follow the lead of the seminal works of Hannerz (1969), Rainwater (1970), and Valentine (1968), who tended to describe inner-city culture as composed of both “ghetto-specific” and “mainstream” (in Hannerz’ 1969 terms) forms of behavior. Anderson (1999), e.g., in a study of inter-personal relations in inner-city Philadelphia, finds what he calls both “street” and “decent” families living in the same urban neighborhoods.

Categorizations such as street culture, however, can easily slip into compendia of all undesirable cultural traits, leaving massive conceptual and empirical gaps. This issue is so critical that an example is worth discussing. Consider Massey & Denton’s (1993) recent application of the oppositional culture thesis, which posits that black inner-city culture is not only different but also directly and self-consciously opposed to the norms and values of the white middle-class. The term originates in the work of Ogbu, who presented evidence that black adolescents in a predominantly black high school devalued schoolwork out of a fear of being called braniacs; Ogbu argued that this devaluation was the result of their rejection of white culture (Fordham & Ogbu 1986; Ogbu’s thesis has been challenged recently).<sup>4</sup> Massey & Denton (1993), whose intent is to provide a mechanism for how neighborhood segregation affects life chances, argue that black people in segregated neighborhoods develop their culture as an oppositional stance against “white” cultural traits. Consider the following passage:

[B]lack street culture . . . [legitimizes] certain behaviors . . . that . . . [are] held in contempt by white society. . . . If whites speak Standard American English, succeed in school, work hard at routine jobs, marry, and support their children, then to be “black” requires one to speak Black English, do poorly in school, denigrate conventional employment, shun marriage, and raise children outside of marriage. To do otherwise would be to “act white.” (Massey & Denton 1993:167–68)

The implication of this passage is that poor urban blacks—not just the teens in Ogbu’s schools but also adults—shun work because whites work; shun marriage because white women marry; and speak Black English vernacular because whites speak Standard English. No evidence is produced in support of what seems, on its face, an implausible set of conclusions. Indeed, there is ample empirical

<sup>4</sup>See Carter (1999), Cook & Ludwig (1998), and Ainsworth-Darnell & Downey (1998), most of whom find that black students are no more likely than white students to rebel against schoolwork. Indeed, the same phenomenon is widely acknowledged in ethnographic studies of white students (e.g., Willis 1977, Kinney 1993). The fact that most of us, regardless of race, can recall either rebelliousness or the fear of being called “braniacs” or “nerds” should raise suspicions about an obligatory link between race and opposition to school among teens. On the other hand, we should point out that the presence of “oppositional cultures” across racial groups does not mean that the consequences of adopting that stance is the same for teens of all races.

evidence that these characterizations are either misguided or incorrect (on work, see Newman 1999, Petterson 1997, Duneier 1992, 1999; on marital attitudes, see Wilson 1996:98–105, Anderson 1991, 1999:Ch. 5; on the historical origins of Black English Vernacular, see Dillard 1972; on attitudes toward achievement in education, see Carter 1999).<sup>5</sup> Massey & Denton (1993) have produced some of the most sophisticated and trenchant work in urban poverty, and their general arguments about the importance of persistent segregation (see above) are both cogent and well supported by the available evidence. Nonetheless, the application of the oppositional culture thesis to urban poverty is empirically unsubstantiated.

The overall literature on inner-city culture should address the specific ways this culture (or cultures) differs from mainstream or middle-class culture (or cultures). Any strict inner-city/mainstream or underclass/middle-class dichotomy faces the danger of degenerating into stereotypes. Indeed, several recent ethnographic works offer fresh perspectives and correctives on the differences, real or alleged, between inner-city and mainstream culture. Nightingale (1993) finds that the black boys he studies in inner-city Philadelphia not only receive a steady influx of American mainstream culture but in fact embody what the author considers its preeminent cultural values: violence, individualism, materialism, and consumerism. What is poignant about Nightingale's work is that it forces us to face not just sociology's stereotypes of the inner city, but also its romantic notions of what constitutes the mainstream. The perceptions of the middle-class we use to compare to the ghetto are generally impressionistic and selective ideals based on what we believe society should look like; these are poor heuristics for interpreting cultural practices in the inner city.

Pattillo-McCoy (1999) dismantles many of our perceptions of what constitutes the middle class in her study of a black middle-class neighborhood in Chicago. She finds that the black middle class is not isolated from the black lower class and that part of its daily struggle is to attain the crime-free character of many white middle-class neighborhoods. Another recent attack on stereotyping by sociologists is Duneier's (1992), which shows that many of the poorer black men of Chicago value work, responsibility, honesty, loyalty, and integrity as much as or more than the average middle-class person. Newman (1999), one of the authors of this review, finds that many of the poor residents of Harlem are not only willing but anxious to take no-benefit, minimum-wage jobs and that they believe in work, family, and

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<sup>5</sup>We should also note a problem with the basic logic of the thesis. In order for individuals to reject a set of cultural attitudes, they must have some exposure to it. To reject schoolwork, they must have been in school; to reject whites, they must have seen them, or at least heard about them. Thus, in order for Afro-Americans to develop an oppositional culture, they need to have come into contact with the culture they are trying to oppose. But the more segregated neighborhoods are, the *less* inter-racial contact there is, so the fewer the number of whites to associate with certain cultural traits and the fewer the cultural traits blacks would know to oppose. If a teenager sees almost no whites in her day-to-day life, then why should she associate any particular cultural traits with whites? In fact, the more logical proposition would be that blacks in predominantly white, not predominantly black, environments are likely to develop an oppositional identity.

responsibility. These works do not represent a coherent theory of how to think about inner-city culture, and they are limited in their generalizability because they often focus on one city or even one neighborhood. They provide important correctives to the predominant perceptions among sociologists and open the door to more nuanced and systematic theoretical work—work that may well uncover an inner-city culture that is not only heterogeneous but also contradictory and unsystematic.

There is a final issue in the inner-city culture literature with which new research must contend. Some cultural theorists argued that, though the original cause of inner-city cultural patterns was the (structural) absence of jobs, this culture was somewhat self-perpetuating, so that people would find it difficult to start working even if conditions did improve. Wilson (1996, 1987), for instance, rejects Lewis's (1968) culture-of-poverty theory but argues that long-term unemployment generates a low self-efficacy among urban dwellers, making it difficult for them to take advantage of economic opportunities if and when these arise. But labor markets have tightened and joblessness among black men has dropped precipitously (Freeman & Rodgers 1999). This does not automatically refute the self-efficacy thesis, but it forces scholars to assess how widespread self-efficacy (or any cultural trait) is and, most importantly, how resistant it is to change.

### Cultural Approaches Recently Applied to Urban Poverty

A few approaches from the sociology of culture have been applied recently to issues in urban poverty. One of these is the boundary work approach (Lamont 1992, Lamont & Fournier 1992). The approach, with roots in the sociology of ethnicity (Barth 1969) and of science and knowledge (Gieryn 1995, Small 1999), is the topic of an important new collection of papers (Lamont 1999). Boundary-work, rather than a comprehensive theory, is a perspective by which sociologists examine relationships between individuals or groups not by studying their inherent characteristics but by analyzing the boundaries they draw between and among one another, such as when the working poor define themselves in opposition to the poor who do not work. The collection applies the perspective to the relationship among race, culture, and urban poverty; it examines boundary-work within the black community, black and white attitudes toward race, and attitudes among blacks toward urban street culture. Much research remains to be done to determine whether and how this boundary work has lasting effects on the perpetuation of urban poverty.

Finally, the work of Bourdieu (1977, Bourdieu & Wacquant 1992) has been touched by a number of students in urban poverty. Wilson (1996) hints at the notion of habitus and cites Bourdieu when he describes the disposition-forming effects of prolonged unemployment. McLeod (1995) employs it to analyze the differences between two groups of boys, one black, one white, who have different aspirations despite their similar structural conditions. Young (1999a, 1999b) finds that the young black men he studies have accumulated varieties of cultural capital, but not

enough of it to ensure their upward mobility. Nevertheless, most applications of Bourdieu's work to urban poverty do not engage Bourdieu's deeper theoretical questions—particularly the relationship among the concepts of habitus, capital, and field—that applications do in other fields, such as political sociology, (see, e.g., Brubaker 1996). If they did, they would do much to improve the quality of our thinking about whether Bourdieu has something to offer to studies of urban inequality.

## CONCLUSION

We have argued that some of the current work on the family, the neighborhood, and culture in urban poverty revisits ideas from the 1960s and early 1970s; some of it is empirically weak, and some of it is under-conceptualized. Yet we have also shown that much of it is provocative and promising. Throughout the paper we have signaled several important avenues for further study, but a few issues merit greater discussion. The most important is for mainstream work in urban poverty to take more seriously the greater and recent demographic changes taking place in cities, such as the growth of large metropolitan areas in the west and southwest (e.g., Los Angeles, Houston); the recent re-entry of the middle-class into certain cities (e.g., Boston, New York, San Francisco) and the accompanying tighter housing markets; the long-standing and rapidly increasing “digital divide” between the rich and poor; and the economic, cultural, and social dynamics generated by the increasing presence of Asian, Latino, and recent Eastern European immigrants. This work demands more comparative and historical research, studies that examine differences between nations, ethnic groups, cities, and geographic regions. Here, it is particularly pressing that students go beyond the easily available census data, which will always pull research toward its strengths: census tracts and black-white differences (the Latino data, e.g., are notoriously problematic).

Comparative work is also necessary on the work on birthrates. In particular, the quickly rising out-of-wedlock birthrate among white women and the rates among Latinas should be a greater preoccupation of mainstream urban poverty students, especially since Latinos are quickly becoming the largest minority (and already are in some cities). This work should also consider how the recent drop in unemployment affects marriage. Indeed, research on the working poor should come to the fore in light of recent welfare reform and the growth of the low-wage labor market. We noted many issues in the work on neighborhoods, but we should emphasize that the growing literature on social capital and networks, which we could not cover, should complement the neighborhood effects literature well (e.g., Wellman 1999). Finally, the cultural literature in urban poverty, still in its infancy (or early re-incarnation from the 1960s), is in dire need of conceptual work. In this vein, comparative and historical studies are particularly important to assess the origins and development of urban cultures. The existing ethnographies have provided critical insights, but because so few of them have been comparative, they

have not taken us far enough on the cultural front. If anything, these ethnographies, and the new work in urban poverty as a whole, has laid the groundwork for the important work to come.

## ACKNOWLEDGMENTS

Support for this paper was provided by a National Science Foundation Integrative Graduate Education and Research Traineeship Grant (NSF No. 9870661). In addition we thank Silvia Dominguez, Herbert Gans, Christopher Jencks, and James Quane for comments and criticisms.

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