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Abstract

Quantitative segregation research focuses almost exclusively on the spatial sorting of demographic groups. This research largely ignores the structural characteristics of neighborhoods – such as crime, job accessibility, and school quality – that likely help determine important household outcomes. This paper summarizes the research on segregation, neighborhood effects, and concentrated disadvantage, and argues that we should pay more attention to neighborhood structural characteristics, and that the data increasingly exist to include measures of spatial segregation and neighborhood opportunity. The paper concludes with a brief empirical justification for the inclusion of data on neighborhood violence and a discussion on policy applications.

Keywords

neighborhood effects, neighborhood opportunity, neighborhood violence, segregation, spatial analysis

1 Introduction

Research on segregation and neighborhood stratification has typically had three areas of focus. First, this research focuses almost exclusively on the sorting of households by income and race. Second, when related research does branch out beyond issues of race and class – such as the research on neighborhood effects – the focus is on negative attributes of neighborhoods and cities, as opposed to opportunities. Finally, segregation and neighborhood research has been much more focused on the attributes of those that live around ‘you’, rather than on the spatial sorting of structural characteristics that shape opportunities for individuals and families.

The purpose of this critical review is to demonstrate a need to shift the focus of segregation and neighborhood-based research in three ways. First, I propose that such research adds richness to the analysis by focusing on multiple dimensions of segregation and stratification – effectively

moving beyond race and class. Second, I propose that among these additional dimensions, we add structural characteristics of neighborhoods and communities that provide (or constrict) *opportunity* for neighborhood residents. Finally, I argue that we articulate this research in a positive framework – that is, we focus on assets and opportunities, in contrast to past conceptions of concentrated poverty and disadvantage.

In discussing additional characteristics to incorporate, I propose an inquiry into the geography of neighborhood opportunity that includes neighborhood-level indicators of job access, school quality, and crime (as a start) to be systematically included in measures of

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neighborhood opportunity. Further, we need to know more about how these opportunities affect household outcomes. I also argue in this paper that we need to study the larger city and metro-level forces that drive these levels of stratification, because that is where the policy decisions (housing, land use, economic development) are made.

This is a vital time to re-examine how we study and address issues of neighborhood opportunity. The Great Recession upended the economic landscape and exposed the widening gulf between rich and poor households. Additionally, a focus on neighborhood opportunity is spurred by recent advances in data availability and our knowledge about neighborhood effects. Data on crime, school quality, and job accessibility are now available in the USA at the census tract level, and there is overwhelming evidence that neighborhood attributes such as violence can strongly limit life chances (Sampson, 2012; Sharkey, 2010; Sharkey et al., 2012). Finally, public policy—specifically US housing policy—is more attuned to the notion that structural neighborhood opportunities shape individual outcomes.

The paper is organized as follows. First, I summarize the literature on racial and income segregation, focusing on measurement, causes, and the role of public policy. In this section, I pay special attention to the critiques by cultural and ethnic geography scholars about the form and use of such analyses. While this is not explicitly a paper about segregation as it is classically understood, the spatial concentration of opportunity and disadvantage are strongly tied to segregation and the research on it. Second, I discuss the consequences of segregation—consequences that speak directly to the need to focus on the spatial clustering of variables other than income and race. Third, I summarize the literature on concentrated disadvantage that explicitly focuses on neighborhood characteristics other than income and race. Fourth, I provide a way forward, toward conceptualizing a

geography of neighborhood opportunity; and, using existing data, I provide a brief discussion of potential alternatives that take into account structural features of neighborhood opportunity. Finally, I conclude with a discussion on a practical application of such measures via housing research and policy.

Segregation research has been stuck for decades on income and race. Households do not make housing decisions based on such a simple set of neighborhood characteristics, and their life trajectories are not determined by them. Research repeatedly suggests neighborhood effects are complex. What is important is opportunity—having the assets and amenities at ‘your’ disposal to improve outcomes throughout the life course, such as safe streets, quality schools, and access to jobs. Segregation research needs to expand its scope and develop tools for analyzing the concentration of these opportunities and their effects on people and places.

II Toward an articulation of the segregation literature

No review can encompass all of the strands of research on segregation. In fact, the goal of this paper is not to explain segregation but to put forth an argument that geographers and spatial scientists focus on a wider breadth of neighborhood attributes that shape opportunities (and disadvantages). I therefore limit the scope of this review in two ways. First, I focus on segregation studies in the USA, which largely examine residential location patterns by race and income, and often focus on the central city/suburb dichotomy that looks very different in urban areas outside of the USA, particularly in other Western countries.¹ Second, and relatedly, the studies under examination here are overwhelmingly quantitative. There is a lot of important work in qualitative analysis that has shed light on the mechanisms of segregation, especially the daily life challenges experienced by

households coping with the structural features of poverty. However, for the purposes of this review, where I aim to highlight the potentials in rearticulating this literature, many of the possible ways forward center on incorporating additional quantitative data. It is my hope that additional quantitative data can add a richness to our understanding of segregation that complements the qualitative literature on this topic.

The earliest studies of segregation came from the Chicago School of sociology, beginning in the 1920s with Park and Burgess (Park and Burgess, 1925; Wong et al., 2007). These studies gave birth to the human ecology approach, where the effect of the urban environment on human development became a central focus. The concentration of crime was also central to the research by Park and Burgess and other Chicago School sociologists, including Shaw and McKay (1942), who pioneered social disorganization theory. The voluminous sociology research on neighborhood stratification and residential segregation patterns all have roots in these foundational studies.

Notably, geographers have not only contributed a great deal of research on race and segregation,² but they have also been critical of the ways in which race and space have been studied in and out of the discipline. These critiques contend that race is not appropriately central to the discipline of geography (Deskins and Speil, 1971; Horvath et al., 1969; Mitchell and Smith, 1990), and that even when race is explicitly examined, the ways race is treated and defined within the discipline are problematic (Gregson, 1993; Kobayashi and Peake, 2000; Pulido, 2002).

Early work on segregation by social geographers was heavily influenced by the quantitatively oriented Chicago School. Examples include Joe T. Darden's research in Detroit, Pittsburgh, and other US cities that examined the competing and complementary roles of race and class in explaining residential segregation patterns (Allen and Turner, 2009; Darden, 1973, 1986; Darden et al., 1997; Darden and

Kamel, 2000). Such research has also tied segregation processes to topics such as class formation (Harris, 1984) and cultural assimilation (Peach, 1999). In addition to these connections, this literature more explicitly emphasizes how race affects real-world human interactions and experiences and the complexities of racial and ethnic identity that are minimized in quantitative research (Nayak, 2006).

Cultural geographers responded by contesting the extent to which race can be accurately categorized and mapped (Nayak, 2011). Geographers have made important contributions to the study of segregation and its effects through this challenge to the accuracy of racial and ethnic identities, in addition to pushing for a focus on how residential segregation relates to social differentiation (Gregson, 1993). In the UK, others have acknowledged the lack of focus on income segregation in the geography literature (Mohan, 2000).

Cultural geographers have pinpointed at least two problems with the specification and use of racial and ethnic identities in quantitative research on segregation. First, racial and ethnic identities are fluid and socially constructed (Gregson, 1993; Jacobs, 1994; Nayak, 2006). Such complications make it difficult to quantitatively describe and assess spatial geographies of race, and argue for more qualitatively-oriented approaches. Further, the construction of racial labels (that racial segregation studies depend upon) has a checkered past, developed historically in the pursuit of differentiation and social control (Gilroy, 1998). Kobayashi and Peake (2000) emphasize the imperialist development of geography (in the 19th century) as a discipline founded on 'difference and hierarchy'. Despite this, geography has contributed a great deal to studies of race, whether via the more quantitative research that focuses on racial differences or the work traditionally within the scope of cultural geography that examines racial issues through the lens of critical race studies (Pulido, 2002).

However, some authors have been critical of the different ways in which whites and other racial groups are treated in studies of race and place (within geography and beyond). Bonnett (1997) discusses the erasure of the white racial category from racial studies. Specifically, Bonnett argues, geography studies of race fail to ‘subject Whiteness to historical or geographic scrutiny’, while non-white groups are the singular focus of these inquiries. Bonnett argues that this is a form of privileging, ‘for it removes Whiteness from the debatable “racial” categories, placing it outside history and geography and onto the essentialist terrain of unchangeable nature’. This treatment of white racial groups implies that it is a more definite and legitimate category, despite the radical changes that the definition of ‘white’ has undergone over time (Leonardo, 2002; Roediger, 1999). The ramifications for segregation research are that studies of race and place focus much more on the concentration of non-white groups than on the concentration of whites. The growing concentration of wealth in the USA is spawning added attention to the concentration of the affluent (Reardon and Bischoff, 2011); however, it is difficult to find research that focuses on the concentration of whites that does not involve the rural poor (Lichter and Johnson, 2007).

While the research I discuss in this paper fits squarely in the mold of quantitative social geography and spatial sociology, the new directions I advocate respond to the critiques found in cultural geography discussed earlier. Given the limitations of treating race (and/or poverty) as constructs that define an individual or a place, I argue that we need to collect and utilize a larger set of variables to measure the consequences of segregation more precisely. While more precise data do not directly address the power relations of data definition or collection, by adding this richer context in the pursuit of measuring neighborhood opportunity I hope that quantitative researchers can better define what living in segregated neighborhoods means

for individuals in real life. Incorporating a wider array of neighborhood indicators into quantitative measures of segregation is a way to add richness to our discussions of spatial stratification that I hope can build on the depth of inquiry found in qualitative research.

I Segregation measurement

Although a thorough review of measurement issues in quantitative segregation research is beyond the scope of this paper, it is important to summarize the key methodological frameworks and advances to this point.³ Duncan and Duncan (1955) developed the first widely used measure of segregation – the index of dissimilarity, which is the differential distribution of two groups (e.g. white/non-white) among areal units such as census tracts. Massey and Denton (1988) conceived of segregation as occurring and measurable along five dimensions: evenness (reflecting the representation of two groups within small area units), exposure (the extent to which the minority group shares residential space with the majority group), clustering (the extent to which minority group enclaves are spatially proximate to one another), concentration (the extent to which the minority group occupies a small geographic space), and centralization (whether the minority group is located near the urban core). Segregation measures still largely fall into one of these five categories.

In recent decades, a number of researchers have emphasized the limitations of these measures as they do not explicitly take into account spatial relationships between small area units and also do not account for the fluidity and limited meaning of small area boundaries (in addition to the limited meaning of social constructs such as race). Reardon and O’Sullivan (2004) termed the former of these the ‘checkerboard problem’ – i.e. aspatial measures of segregation fail to deal with how census tracts may be sorted across urban space. The latter problem is widely described (Reardon and O’Sullivan,

2004; Wong, 1997; Wong et al., 2007) as the Modifiable Areal Unit Problem (MAUP). The issue with the MAUP is that results of analyses conducted at different spatial scales may differ substantially. Reardon and O'Sullivan (2004) argued for segregation measures that utilize point-specific data. They further argued that distinctions between evenness and clustering (and concentration and centralization for that matter) are arbitrary unless the area boundaries have specific social meaning, which is often not the case for census tracts. As a result, Reardon and O'Sullivan maintained, we truly require only two dimensions of segregation: spatial evenness and spatial exposure.

Particularly germane to this paper, we can further split segregation measures and related research into two domains – race and income – that dominate the literature. The trends in segregation by race and income differ somewhat by how they are measured, but some basic trends over the last few decades are relatively easy to determine. On the one hand, segregation by race peaked in most US metropolitan areas in 1970. The declines were steady through the 1990s, and thus far look to be levelling off in the 2000s (Logan and Stults, 2011). Concentrated poverty and income segregation, on the other hand, increased from 1970 to 1990, decreased by the 2000 census, and then increased through the 2000s (Bischoff and Reardon, 2013).

2 Causes of racial and income segregation: The central role of public policy

Research on the causes of income and racial segregation is extensive. Basic theories of urban spatial structure (Alonso, 1964; Mills, 1967; Muth, 1961) suggest that segregation by income could occur purely via economic forces. However, segregation levels in the USA are almost certainly higher due to public policy decisions. Some of these decisions are ostensibly race neutral, such as the proliferation of suburban communities that create relatively homogenous

jurisdictions through Tiebout sorting (in which households self-select a preferred bundle of taxes and public services), and relatedly, the government-subsidized suburbanization of housing and jobs. But many public policy decisions are more explicitly race-related, such as discrimination by governments and discrimination by private actors (real estate agents, loan officers, buyers and sellers) to which government has often turned a blind eye.

In the context of discrimination in residential preferences, Clark (1991) and Schelling (1971) have done important work on the dynamics of racial segregation, and are chiefly concerned with how white neighborhoods became black. Schelling demonstrated that a little bit of discrimination can go a long way in creating sharply segregated neighborhoods. Essentially, if the preferences of white residents of a neighborhood differ, the dynamics will drive the proportion black successively over each individual's maximum tolerance and the neighborhood will ultimately become a black residential area. Their findings shed light on how widespread, overt racism is not necessary to explain the stark racial segregation that exists in most US cities. Further, this highlights the vital role of limited government efficacy in combating discrimination – US governments have largely chosen not to intervene when mundane residential preferences result in widespread segregation.

Tacit government support for discrimination was once substantial, and has had lasting effects. This support diminished formally throughout the 20th century as a result of case law and federal policy – e.g. the Supreme Court's ruling against racial covenants in *Shelley v. Kraemer* in 1948, and via 1968's Fair Housing Act. However, due to the permanence of the built environment, the relatively slow pace of neighborhood change, and the urban decay that flowed from such discriminatory practices, credible arguments suggest that the damage from urban legacies such as government-backed redlining and the approval of restrictive racial covenants and

racial violence and intimidation have long outlasted these practices in the form of our current segregated living patterns (Galster, 2012; Massey and Denton, 1993; Sampson, 2012; Sharkey, 2013; Sugrue, 1996; Wilson, 1987). As if this legacy of discrimination were not enough, racial discrimination persists in household residential housing preferences (Charles, 2005; Clark, 1992; Galster, 1988) and in actions by real estate agents and other actors in the housing search process, and various levels of government have limited power, resources, and will to combat these actions (Munnell et al., 1996; Turner and Ross, 2005; Yinger, 1997).

Surveying the evidence, Dawkins (2004) concluded that racial preferences and housing market discrimination play a strong role in creating our segregated living environments. Clark (1988, 1992) has repeatedly emphasized the role of residential choice, particularly focusing on racial preferences in an era when immigration has complicated the typical black/white segregation patterns of US cities (see also Clark et al., 2014). Geographers have written extensively on the segregation of different immigrant groups in the USA and other Western countries (Allen and Turner, 1996, 2012; Li, 1998; Peach, 1999; Price, 2012; Wright et al., 2005).

Others have explained the persistence of racial and economic segregation via the city/suburban dichotomy in US metropolitan areas. This is the literature on 'white flight' or the extent to which white and higher income households moved from central cities due to low-quality schools, high crime, minority presence, high taxes, homelessness, and other indicators of urban decay. Early research found evidence for the likelihood that the concentration of low-income households (Bradford and Kelejian, 1973; Grubb, 1982) or minority households (Mills and Price, 1984) had an effect on suburbanization and white flight. Later research focusing on crime offered conflicting accounts – Cullen and Levitt (1999) concluded that crime

was a strong driver of population loss in central cities, whereas Ellen and O'Regan (2010) disagreed, using a similar estimation strategy and more recent data.

More recent research further illustrates the heavy hand of public policy in the persistence of segregation. The interstate highway system radically reduced the cost of commuting and empirical work shows that highways indeed increased suburban populations at the cost of central city ones (Baum-Snow, 2007). Suburban zoning restrictions allowed higher-income households to exclude lower-income ones from living in their jurisdictions – restrictions that continue to this day. Pendall (2000b) found evidence that low-density zoning significantly reduces the amount of rental housing in a city and increases racial segregation in a metropolitan area. Massey and Rothwell (2009) found the same for racial segregation and segregation by income (Rothwell and Massey, 2010). And although rental housing subsidies comprise a very small proportion (roughly 1% to 3%) of the rental housing stock, households living in public housing and receiving other subsidies have been overwhelmingly low-income and subsidized housing has been located in overwhelmingly low-income and high-minority neighborhoods in many US cities (Hirsch, 1983; Massey and Kanaiaupuni, 1993; Popkin et al., 2000; Schill and Wachter, 1995).

In sum, there is rich and rigorous research on the causes of segregation. This research presents these causes as being many and complex, but a relatively coherent story can be told that reflects the mechanisms in many – though not all – US metropolitan areas. Market forces played a big role in spurring 20th-century suburbanization and segregation, where higher income households were able to afford larger plots of land with newer suburban housing within localities with public finance characteristics that were attractive to those households. These market forces were exacerbated by discriminatory government policies, including suburban zoning

restrictions, the concentration of public housing, and legal and semi-legal segregating actions such as restrictive covenants, redlining, discrimination, and racial intimidation. Additionally, the falling cost of transportation (due in part to government subsidies) facilitated the suburbanization of residential and employment locations. In the present time, these segregation patterns persist due to the permanence of the built environment and legacy of neighborhood distress, the intergenerational transmission of neighborhood disadvantage (Sharkey, 2013), continued discrimination by households and housing market actors, exclusionary zoning, and widening income inequality (Reardon and Bischoff, 2011). The strong role of public policy in the establishment and persistence of segregation by income and race behooves us to properly measure these phenomena and identify potential ways to mitigate their effects.

III The consequences of segregation⁴

Not only are segregation and neighborhood stratification often the result of public policy, but important outcomes such as employment, schooling, and health may also be affected by living in segregated environments. Much of the research on these mechanisms focuses on neighborhood effects – the notion that where households live helps determine their life outcomes.

Although the surge in interest in neighborhood research⁵ owes a great debt to William Julius Wilson's *The Truly Disadvantaged* (1987), it is worth revisiting Chicago's Gautreaux housing program that preceded Wilson's landmark book. The Gautreaux program was created in Chicago in 1976 as a result of a series of lawsuits against the Chicago Housing Authority (CHA) and HUD. Gautreaux offered black families in CHA housing the opportunity to move to mostly white neighborhoods (Rubinowitz and Rosenbaum, 2000). The program moved more than 7000 families between 1976

and 1998 (Keels et al., 2005). Participants' neighborhoods improved across several domains, but perhaps most striking are the baseline levels of crime and violence in the neighborhoods they left behind. Before moving to the suburbs, nearly half of Gautreaux participants 'told of dangerous and frightening incidents that occurred regularly on the streets of their inner-city neighborhoods' (Rubinowitz and Rosenbaum, 2000). Criminal victimization rates were twice as high among Chicago public housing tenants than in the city as a whole. Keels et al. (2005) estimated that violent crime rates in Gautreaux participants' original neighborhoods were three times higher than those in Chicago.

After moving, not only were participants less fearful of crime, but they experienced positive employment outcomes and their children had substantial schooling improvements. In particular, the suburban youth were much less likely than city youth to drop out of school, and suburban youth were more likely to be enrolled in college and more likely to be employed (Rosenbaum, 1995). However, Gautreaux participants were not randomly assigned, meaning selection bias limits the strength of causal conclusions scholars can make about neighborhoods and these important outcomes from this study.

In *The Truly Disadvantaged*, Wilson (1987) focused on the segregation of black households into jobless ghettos as the main factor for deteriorating social conditions (high rates of out-of-wedlock births, delinquency, high school dropouts, etc.). He argued that the disappearance of work in the low-skilled economy has been particularly devastating in US central cities. The neighborhood effects literature has largely been a result of attempting to specify the mechanisms through which concentrated poverty of the type that Wilson observed affects a host of other household outcomes.

Ellen and Turner (1997) took stock of the literature in the 10 years that elapsed since *The Truly Disadvantaged*. Presciently, this was at a point in which the Gautreaux program was

being supplanted in neighborhood and housing mobility research by the Moving to Opportunity for Fair Housing Demonstration (MTO). They concluded that empirical research generally confirms that neighborhood environment has an influence on important outcomes for children and adults, but efforts to identify which characteristics matter most and to quantify their importance have been inconclusive. Further, they noted that neighborhood effects are much less important than family characteristics, although there is typically a very high correlation between neighborhood and family characteristics.

The results from MTO speak to how neighborhood poverty affects a variety of household outcomes. Beginning in 1993 in Baltimore, Boston, Chicago, Los Angeles, and New York City, MTO was set up as a random experiment with three groups of public housing residents. The experimental group received housing vouchers to be used in neighborhoods where the poverty rates were 10% or less. A second group was provided housing vouchers that could be used anywhere, and a control group remained in public housing. The experimental nature of the MTO study allowed for a controlled estimation of neighborhood effects. In all, the impact of moving MTO households out of high-poverty, dangerous neighborhoods was less profound than many expected. Adults in the experimental group were no more likely to be employed at the first or second follow-up than the control and comparison groups, and being in the experimental group had no positive effects on children's schooling or employment outcomes. Children were also no less likely to engage in risky or criminal behaviors. The experimental group did experience statistically significant declines in adult obesity relative to the comparison groups, as was the case with mental health problems for female adolescent participants (Sanbonmatsu et al., 2011). Although it is clear that many participants found difficulty using vouchers in low-poverty

neighborhoods – a limitation that may contribute to the lackluster findings – MTO has called into question the importance of neighborhood factors in household outcomes.

Sharkey (2013) focuses on the role of inherited neighborhood disadvantage, arguing that much of the disparity between whites and African-Americans can be explained by the incredibly stark differences in neighborhoods that these different racial groups occupy. Using the Panel Study of Income Dynamics (PSID), Sharkey first sets out to illustrate these differences. For the cohort born between 1955 and 1970, only 4% of white households lived in relatively high-poverty neighborhoods – where the poverty rate was 20% or higher. For African-Americans born at the same time, that number was 15 times higher, or 62%. These differences barely changed in 30 years – for the 1985 to 2000 cohort, those numbers were 6% and 68%, respectively. In other words, the high-poverty neighborhood that is a typical place to live for African-Americans is almost unheard of for white Americans. Furthermore, these disparities hold when controlling for income differences between whites and blacks.

Sharkey linked living in a high poverty neighborhood to two key outcomes – inherited neighborhood disadvantage and economic mobility. Neighborhoods are largely inherited across generations: the correlation between the income level of parent and child neighborhoods is quite high (about 0.67). However, he also found that when white families live in a high-poverty neighborhood, it tends to be for a single generation, whereas whites tend to live in affluent neighborhoods for multiple generations. The opposite pathways are typical for African-American families – multigenerational exposure to neighborhood poverty is common and multigenerational exposure to affluent neighborhoods is rare.

The exposure to neighborhood disadvantage, Sharkey argues, is an important factor in understanding the distressingly limited outcomes

among the African-American population as a whole. Sharkey finds that the neighborhood poverty rate of a child explains a great deal of the income he or she earns as an adult, and also explains much of the economic mobility gap. The latter fact reflects the reality that black children are more likely than white ones to experience downward mobility (moving from a high-income category to a lower one) and black children are less likely to experience upward mobility (moving from a low family income category to a higher one).

Sharkey's conclusions have been re-affirmed by Chetty et al. (2014), who examined the geography of intergenerational mobility – looking at the regional scale rather than the neighborhood. They found that movement up and down the economic ladder across successive generations varies dramatically by metropolitan area. They described the USA as 'a collection of societies' – in some metro areas, economic mobility across generations is common, whereas elsewhere movement out of poverty is a rare event. Importantly, they find that the spatial concentration of particular demographic characteristics – such as college attendance and teenage birth rates – is strongly linked to rates of economic mobility.

Further, the persistence of racial and economic segregation is continuing to lead to substantial inequities in terms of public services that people consume and exposure to crime and violence. Higher quality services and other amenities are concentrated in particular areas within metropolitan areas, and these concentrations map onto patterns of economic and racial segregation. De la Roca, Ellen, and O'Regan (2014) used census data, a unique tract-level dataset on crime in 91 US cities (Peterson and Krivo, 2010), and geocoded school zone data to census blocks in order to estimate the effect of racial segregation on the exposure of different racial groups to low socioeconomic status neighbors, crime, and low-quality schools. They found substantial racial disparities in exposure

to disadvantaged neighborhoods – whites and Asians are much less likely to live in neighborhoods with negative features than blacks and Hispanics – and that these disparities are not fully explained by differences in income; they found 'that the average poor white person lived in a neighborhood with a lower violent crime rate than the average non-poor black person' (De la Roca et al., 2014: 143). Further, they found that metropolitan area segregation levels (i.e. dissimilarity and isolation indices between various racial groups and whites) are strong predictors of these racial gaps in exposure to all three domains of neighborhood disadvantage – neighbor socioeconomic characteristics, quality of the zoned school, and violent crime.

IV Beyond income and race: Measuring neighborhood disadvantage

In *The Truly Disadvantaged*, Wilson (1987) popularized the term 'the underclass' to describe areas with a high prevalence of: poverty, out-of-wedlock births, black male unemployment, crime, and poor schools. In much of the research thereafter, the term 'underclass' was interchangeably used to describe areas with high levels of many or all of these features and – more controversially – the people that lived within them. Ricketts and Sawhill (1988) operationalized a formal definition of underclass neighborhoods as those where census tracts were one standard deviation above the US mean on each of the following indicators: high school dropouts, male unemployment, welfare recipients, and female-headed households. Using this definition, Ricketts and Sawhill estimated that about 1% of the US population lived in such neighborhoods as of the 1980 census. About 60% of this population was black, and 44% were in poverty. Although the underclass measure as operationalized was somewhat arbitrary, it provided useful information that deviated from simple measures of neighborhood poverty – the

underclass census tracts and high-poverty tracts overlapped but were not the same ones – nearly 40% of the underclass tracts were not high-poverty ones. Additionally, an update of this analysis from Ricketts and Mincy (1990) showed just how dramatic the growth in these tracts was during the 1970s – using 1970 data, there are only one-quarter as many underclass tracts.

After the unexpected decline in concentrated poverty that occurred during the 1990s, Jargowsky and Yang (2006) updated the Ricketts and Sawhill (1988) analysis using data from the 2000 census. The population living in underclass neighborhoods had essentially remained the same during the 1980s. During the 1990s, Jargowsky and Yang found that the number of tracts meeting at least three of the four underclass thresholds fell, with the exception of female-headed households. Accordingly, the number of underclass tracts and people living within them fell, resulting in a 36% drop in the number of people living in underclass tracts.

Kasarda (1993) modified Ricketts and Sawhill's (1988) definition to estimate the trends from 1970 to 1990 in the population living in what he termed 'distressed tracts'. He dropped the high school dropout indicator used by Ricketts and Sawhill and put the percent of poverty in its place for a definition of neighborhood distress. For severely distressed tracts, he added high school dropouts as a fifth indicator. Like Ricketts and Sawhill, he considered a tract to be distressed if it surpassed one deviation above the national mean on all four of these indicators (and all five indicators for severe distress).

Sampson, Raudenbush, and Earls (1997) conducted a factor analysis to create a measure of concentrated disadvantage. They then used this measure in a widely cited study on the role of collective efficacy in preventing disadvantaged neighborhoods from becoming violent places. Their measure focused on six census tract characteristics: welfare receipt, poverty, unemployment, female-headed households, percentage

black, and density of children. Sampson, Sharkey, and Raudenbush (2008) later utilized this measure to study the effects of concentrated disadvantage on children's verbal ability.

Until recently, all of the focus has been on neighborhood *disadvantage*. A recent paper from the Urban Institute (Turner et al., 2011) framed this issue in an assets rather than deficits perspective, identifying aspects of neighborhood opportunity, rather than underclass or distress. Another way in which this research differs from previous concepts of neighborhood quality or disadvantage is in its inclusion of a structural neighborhood characteristic – job density – in addition to the demographic characteristics of neighbors. Three of the indicators are essentially the flip-side of the underclass and distress measures defined by Ricketts and Sawhill (1988) and Kasarda (1993), respectively, with different thresholds. The resulting measure includes thresholds for work participation, income, college completion, percent white, and job density. The goal for Turner et al. was to examine the extent to which MTO participants were able to access higher opportunity neighborhoods.

McClure (2011) further advocated for a more complex measurement of neighborhood opportunity: 'the development of an opportunity index should examine the potential for improved educational attainment, greater safety from crime, a higher probability of obtaining gainful employment, as well as finding a good quality dwelling unit at an affordable rent' (McClure, 2011: 10). McClure used a factor analysis to produce a neighborhood opportunity index with the goal of narrowing the list of variables (or factors) that explain the majority of the variation in the initial variables – possibly due to the high level of correlation between the various constructs. Using this factor analysis, he recommended that an analysis of neighborhood opportunity should include: the incidence and level of poverty, educational attainment, employment rates, employment accessibility (a structural opportunity factor), race, and the presence of

other assisted households. He further suggested that the measure should be employed at the block group level where possible. McClure notes that missing from this measure is school quality and crime rates (additional structural opportunity characteristics), due to a lack of data availability.

1 The consequences of neighborhood disadvantage

The measures proposed by McClure (2011) and Turner et al. (2011) include employment accessibility, which is a much-studied neighborhood attribute via the ‘spatial mismatch’ literature on whether the lack of proximity to employment leads to poor job outcomes for low-income and minority job seekers. On this question, prior research is relatively inconclusive. Stoll (1999) found that blacks and Latinos live in areas of Los Angeles with poor job growth and that this results in their spending more time and effort to find work. Also in Los Angeles, Ong and Blumenberg (1998) found that the job-poor neighborhoods lived in by welfare recipients made it less likely that they would find work. In contrast, Cervero, Sandoval, and Landis (2002) found no relation of regional job accessibility to employment outcomes for welfare recipients in Alameda County, California – a finding echoed by Sanchez, Shen, and Peng (2004), who looked at Temporary Assistance for Needy Families recipients in six US cities. Finally, research on MTO cast some doubt on the importance of employment accessibility, given that no employment impacts were found from living closer to potential employment opportunities (De Souza Briggs et al., 2010; Kling et al., 2007).

Robert Sampson’s Project on Human Development in Chicago’s Neighborhoods (PHDCN) has provided evidence that challenges the MTO results on neighborhood effects. The PHDCN has collected an extensive set of neighborhood structural characteristics since the early 1990s

to identify their effects on household outcomes. Sampson’s most recent book, *Great American City: Chicago and the Enduring Neighborhood Effect* (Sampson, 2012), summarized the body of work by Sampson and colleagues through the PHDCN. He argued that the obsession over tackling the selection bias problem in neighborhood research distracts from the fact that neighborhood selection is in itself an important contextual process that is affected by the neighborhood characteristics of individuals. In other words, households that select disadvantaged neighborhoods often do so *because* they have greater familiarity with such neighborhoods, and that imperfect selection process is yet another negative result of living in disadvantaged neighborhoods. In this way, neighborhoods select people.

Sampson termed his analytic solution ‘ecometrics’ – which is designed to treat social context explicitly as a unit of analysis through systematic measurement of neighborhood mechanisms. He utilizes field observations, housing data, crime and violence, residential mobility between neighborhoods, information on contacts between public officials and leaders in different neighborhoods (in order to measure communication between neighborhoods), administrative records, and even a letter-drop survey in order to measure social altruism and civic cooperation – key components of what he and his colleagues have termed ‘collective efficacy’ (Sampson et al., 1997). This method is best-suited to a grand project like PHDCN that is able to collect a broad scope of variables, but more data are available at small levels of geography than ever before. Furthermore, some countries, such as Sweden, have better individual-level data available to tie neighborhood opportunity measures to individual outcomes (Galster et al., 2008).

Using these extensive data on neighborhood domains and over multiple time periods, Sampson found substantial overlap between various measures of disadvantage and that neighborhood

disadvantage is very persistent over time. Neighborhoods with high violence are also the ones with low health indicators and poor collective efficacy. Further, disadvantaged neighborhoods tend to remain disadvantaged for decades – and neighborhood poverty is particularly persistent in neighborhoods with high African-American populations. Sampson's data allowed him to tie together the role of community social capital in protecting neighborhoods from becoming violent and disadvantaged.

Finally, more research from Sharkey and colleagues suggests that neighborhood violence is particularly influential on children's outcomes. Using data from the PHDCN Sharkey (2010) found strong evidence that local homicides affect children's performance on verbal and reading assessments taken shortly after the homicides occurred. He exploited the exogenous variation in the timing of the homicides in order to strengthen the causal linkages between violence and assessments. In another paper, Sharkey et al. (2012) found more evidence that geographically proximate homicides have a negative impact on several youth outcomes, including pre-academic cognitive skills such as impulse control, and vocabulary and math assessment scores. Further, they found that parents' mental health outcomes were negatively affected by local homicides.

The research on segregation suggests that the effects of highly concentrated poverty and racial stratification are real and meaningful. However, research by Sampson and others has made it clear that other measures of neighborhood disadvantage allow us to see more holistic neighborhood change processes and also observe different effects on households. Furthermore, there is new evidence that structural neighborhood characteristics such as crime and violence, job accessibility, and schools are vital in affecting household outcomes through processes that are related yet distinct from demographic characteristics such as race, income, employment, schooling, and household headship.

Sampson's research leaves several areas of focus for geographers and spatial social scientists. First, the PHDCN was concentrated in particular neighborhoods in Chicago. The data are available to implement multivariable spatial analyses across countries. We need geographers and spatial scientists to be at the forefront of producing neighborhood opportunity indicators that are easily replicable without such heroic efforts as the PHDCN. Second, Sampson and colleagues focus on neighborhood disadvantage, rather than opportunity. Research that allows policymakers to proactively identify high-opportunity neighborhoods is necessary so that we can focus on where households should live rather than where they should leave.

V Toward a geography of neighborhood opportunity

At this point, we have substantial information on the causes and consequences of segregation and neighborhood stratification. However, this information is overwhelmingly focused on race, ethnicity, and income as the sole measures of how individuals and households are sorted across urban space. Additional measures of neighborhood distress and opportunity have been added to this list, including educational attainment, employment, female-headed households, employment accessibility, and the density of children. However, it is vital that in examining geographies of neighborhoods, such measures include structural characteristics of neighborhoods that dramatically shape the day-to-day lives of low-income households. As a start, we need to include data on schools, crime, and job accessibility. And to do this, we need to collect more data – particularly on crime. Additionally, I argue that this research must be positively articulated. Neighborhood research should focus on assets and opportunities rather than disadvantage and distress.

The data now exist on schools (Horn et al., 2014) and employment accessibility (Lens,

2014). Neighborhood crime data have been collected before on a large scale, suggesting that collecting data on a large number of cities is feasible. From 1999 to 2001, Peterson and Krivo (2010) conducted the National Neighborhood Crime Study (NNCS), a nationally representative sample of crime data for 9593 census tracts in 91 US cities. The resulting public dataset includes an average of the major crime categories developed by the Federal Bureau of Investigation's Uniform Crime Report System over the entire three years for each census tract.

In subsequent research tract-level crime data has been used with increasing frequency, covering a variety of years and cities. Lens et al. (2011) collected neighborhood-level crime data for 10 US cities from one of three sources: directly from police department websites or data requests to the department (Austin, New York, and Seattle), from researchers who obtained these data from police departments (Chicago and Portland), and from the National Neighborhood Indicators Partnership (NNIP) (Cleveland, Denver, Indianapolis, Philadelphia, and Washington, DC). Mast and Wilson (2013) investigated the relationship between vouchers and crime in Charlotte, North Carolina, using data on property, violence, residential burglary, and street crimes from 2000 to 2009. Griffiths and Tita (2009) used tract-level data on homicides in Los Angeles to explore whether public housing is a 'hotbed' for crime. MacDonald, Hipp, and Gill (2013) also used tract-level crime data in Los Angeles to investigate the effects of immigrant concentration on crime. Hipp and Yates (2009) used tract-level data to study how returning parolees affect crime in Sacramento.

This breadth of research suggests that technology and a greater appreciation for data sharing among public agencies – including police – are helping to foster an era in which crime data are increasingly available at small levels of geography, including census tracts. Some of these data are publicly available via

municipal police department websites. However, for a systematic effort on the level of the NNCS, a sampling frame and extensive police department contacts will be necessary to obtain representative samples.

For a brief glimpse at why adding structural neighborhood characteristics might matter, I provide some preliminary analyses using crime. I first replicated Ricketts and Sawhill's (1988) measure of the underclass using data from the 2000 census. I then added crime data from Krivo and Peterson's NNCS to understand how strongly the classic measures of underclass are correlated to crime. Looking particularly at violent crime – where we have more evidence on impacts on household outcomes beyond public safety – there is a lot of overlap between violent crime and the underclass domains. Yet, as others have shown, including Sampson, Raudenbush, and Earls (1997), such demographic features of a neighborhood are not deterministic regarding violent crime rates. Table 1 displays the correlations between the violent crime rate and the four Ricketts and Sawhill (1988) measures (proportions of female-headed households, high school dropouts, on public assistance, and the male unemployment rate) in addition to two other common measures of neighborhood distress: the poverty rate and percent black.⁶ We see that the bivariate correlations range from 0.28 (high school dropouts) to 0.57 (percent poverty), and all but high school dropouts have a correlation coefficient with the violent crime rate as high as 0.47. The correlations between these variables and property crime rates are much lower – ranging from 0.12 to 0.22.

Although these variables are moderately correlated with violent crime (and to an even lesser extent with property crime), it is clear that underclass neighborhoods and high-crime neighborhoods are not one and the same. Table 2 provides a cross-tabulation between 'underclass' tracts and tracts with violent crime rates more than one standard deviation above the mean violent crime rate. The vast majority of

Table 1. Tract-level correlation between violent crime rate and neighborhood distress variables.

	Correlation with Violent Crime Rate
%Female Headed	0.51
%HS Dropout	0.28
%Public Assistance	0.53
%Male Non-work	0.52
%Black	0.47
%Poverty	0.57

Sources: U.S. Census Bureau, 2000 SF3 Files; Peterson and Krivo (2010).

Table 2. Cross-tabulation between high violent crime tracts and 'underclass' tracts.

Underclass Tract	High Violent Crime Rate		
	No	Yes	Total
No	6230	678	6908
Yes	51	57	108
Total	6281	735	7016

Sources: U.S. Census Bureau, 2000 SF3 Files; Peterson and Krivo (2010).

census tracts (6230) are neither highly violent nor fit the definition of underclass. And since there are many more census tracts that are highly violent, most highly violent tracts are not 'underclass'. But it is interesting to look at the relatively small (108) number of underclass tracts. Those are almost evenly split between highly violent and not highly violent – nearly as many underclass neighborhoods are highly violent as those that are not. Underclass neighborhoods tend to have higher crime and vice versa, but underclass (and other neighborhood distress) measures are by no means a proxy for high levels of violence.

A simple regression model puts the limited predictive power of demographic variables on neighborhood crime rates in clearer perspective. Using the underclass variables and adding the poverty rate and share non-Hispanic black

Table 3. OLS regression model, dependent variable: Tract violent crime rate.

Variable	Coefficient
Intercept	-1.43*** (0.47)
%Female Headed	2.26* (1.24)
%HS Dropout	7.98*** (1.09)
%Public Assistance	8.31*** (2.36)
%Male Non-work	6.61*** (1.74)
%Non-Hispanic Black	8.28*** (0.67)
%Poverty	30.37*** (1.77)
N	6997
Adjusted R-Squared	0.38

Standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Sources: U.S. Census Bureau, 2000 SF3 Files; Peterson and Krivo (2010).

population (commonly used in indicators of neighborhood distress), I find that although all of these variables (with the exception of female-headed households) have very strong relationships with the violent crime rate, the full model only explains 38% of the variation in the violent crime rate. Again, these constructs and measures are related to the violent crime rates experienced by households in these communities, but they are in no way proxies for such an important neighborhood feature (see Table 3).

Given the strong evidence on the negative effects of violence on household outcomes, and the fact that our current measures of neighborhood quality and spatial segregation are inadequate proxies for neighborhood violence, we need to collect and analyze more data on neighborhood crime. As noted, crime data on several cities and years have already been collected and utilized for research purposes, suggesting that collecting neighborhood crime

data on a wide scale is feasible. Grouped with existing data on neighborhood distress and opportunity – schools, job access, and population demographics – these data may radically improve our understanding of the concentration of neighborhood distress and disadvantage.

An important limitation is the lack of neighborhood crime data in most suburban areas. There are thousands of suburban jurisdictions and police departments across the country, and it is simply not feasible to collect crime data from all of those areas. For a more comprehensive look at metropolitan areas, pilot projects in large MSAs would be a plausible start. For example, there are 88 cities in Los Angeles County, most of which are suburban. Further, many of these jurisdictions are policed by one agency – the LA County Sheriff's Department. Such an undertaking would improve our understanding of how the geography of neighborhood disadvantage is reflected in the suburbs. With the massive increases in suburban poverty in recent decades, it is vital that we better understand how these trends are affecting the ability of low-income households to live in high opportunity environments.

Once these data are obtained, researchers can examine the spatial segregation patterns of these indicators in isolation and in concert with one another, in order to bring more richness to neighborhood stratification analyses, and to clarify geographies of neighborhoods of opportunity. These new analyses should build on earlier research (e.g. Kasarda, 1993; McClure, 2011; Pendall, 2000a; Ricketts and Sawhill, 1988; Sampson et al., 1997; Turner et al., 2011), but with a new goal of measuring the concentration of neighborhood opportunity, and therefore focus on both the attributes of the population (poverty and unemployment rates, race and ethnicity) and the neighborhood structural environment and services (job accessibility, crime, and school quality). This work should also build on the extensive body of qualitative research, which has been documenting the

consequences of neighborhood stratification for decades (Kotlowitz, 1992; Wacquant, 1993).

VI Conclusion: Neighborhood opportunity and public policy

This paper emphasizes the importance of accurately measuring neighborhood opportunity – living among a set of assets and amenities that allow for positive individual and family outcomes throughout the life course. Segregation researchers have concentrated nearly exclusively on income and race, at the ignorance of a broader set of outcomes – such as safe streets, quality schools, and employment – that more accurately constitute neighborhood opportunity. Segregation researchers need to lend their considerable talents to measure and analyze the concentration of these opportunities and their effects on people and places.

Such research is vitally important for changing public policy. Much of the policy focus on segregation and neighborhood disadvantage has centered on housing, with good reason. A number of erroneous and unjust housing policies have contributed to concentrated poverty and racial segregation. In low-income housing policy, research has been conducted that describes the neighborhoods that these households occupy and evaluates various programs in their efficacy in allowing households access to better neighborhoods. In fact, the US Department of Housing and Urban Development (HUD) incentivizes local housing authorities to assist subsidy recipients to gain access to low-poverty neighborhoods, and the fragmented housing subsidy system has been seen as a way to deconcentrate poverty since the 1990s. HUD has been very active recently in promoting neighborhood opportunity, and has published a rule guiding and directing local jurisdictions in these efforts.

The vast majority of prior research describing the neighborhoods lived in by assisted households focuses on poverty rates. This research suggests that public housing has long

been concentrated in high-poverty neighborhoods, and some of the more recent housing subsidies, such as housing vouchers and Low Income Housing Tax Credits (LIHTCs), are in less impoverished areas (though more impoverished than the general population) (Devine et al., 2003; Goering et al., 1997; McClure, 2006; Pendall, 2000a; Wang et al., 2008; Wang and Varady, 2005).

In terms of violence, we know that the participants in the three major housing mobility programs – Gautreaux, MTO, and HOPE VI – all lived in very dangerous environments prior to participation, and on average moved to substantially safer neighborhoods after enrolling in the program (Keels et al., 2005; Kingsley and Pettit, 2008; Popkin and Cove, 2007; Rubinowitz and Rosenbaum, 2000). Lens et al. (2011) represents the only study of the neighborhood crime conditions faced by housing subsidy participants across the country. They found that voucher households occupy much safer neighborhoods than housing built using Low Income Housing Tax Credits (LIHTC) and public housing, face similar crime rates as the broader population of renters below the poverty line, and live in higher crime neighborhoods than the population as a whole in those cities.

Horn, Ellen, and Schwartz (2014) linked data on housing subsidy recipients to school location and performance data in order to estimate the extent to which these households live in areas with high quality schools. Overall, they found that voucher households with children lived in areas near to schools with math proficiency rates that were 3% higher than public housing households with children. However, voucher households lived near worse performing schools than LIHTC, poor renters, all renters, and households in fair market rate (FMR) units. Horn et al.'s ability to link together a national dataset on school quality to housing subsidy locations presents a promising addition to potential measurement of geographies of opportunity at the neighborhood level.

McClure (2010) estimated the capacity in US metropolitan areas for housing assisted households in higher opportunity neighborhoods. Using measures of neighborhood opportunity devised in McClure (2011), he estimated the number of block groups that would accommodate housing voucher households should HUD prioritize the location of these households in such tracts of opportunity. He found that although 52% of US census block groups had poverty rates of 10% or below as of the 2000 census, only 28% (over 5 million) of the country's rental units below FMR were located in those block groups. That would be enough, however, to include all of the roughly 2.2 million housing vouchers. But when neighborhood opportunity is restricted to include the rest of McClure's (2011) attributes (less than 15% project-based housing; less than 4% housing vouchers; less than 20% adults not completing high school; less than 5% unemployment; minority population below 20%; and a negative growth in poverty), the number of rental units below FMR declines below 975,000 nationally, far below the 2.2 million needed.

Better measures of neighborhood opportunity and disadvantage would help researchers and policy makers to develop ways to allow low-income rental households to identify and access high-opportunity neighborhoods. A measure that combines the demographic attributes utilized by McClure (2010) and others with structural characteristics such as school quality, employment proximity, and crime would provide a much more informative assessment of neighborhood opportunity.

Just this year, HUD published a widely discussed rule on Affirmatively Furthering Fair Housing (AFFH), which has the potential to steer funding away from municipalities for not proactively assisting beneficiaries of HUD programming in accessing higher opportunity neighborhoods (Affirmatively Furthering Fair Housing, 2015). This rule comes on the heels of a Supreme Court decision in *Texas*

Department of Housing and Community Affairs v. Inclusive Communities Project Inc. that found the Texas agency in violation of the mandate to provide access to non-segregated communities (Bostic, 2015). One of the components of HUD's AFFH work is to provide local jurisdictions with information on where opportunity neighborhoods are located in their areas while requiring funded jurisdictions to submit plans on how they will comply with AFFH. Geographers and spatial scientists need to be at the forefront of efforts to produce and analyze spatial data on neighborhood opportunity and evaluate the effects of households living in and out of these neighborhoods.

The geography of opportunity should be utilized in research on low-income households and renters. These households are historically the most likely to live in neighborhoods that lack opportunities across several dimensions, because of affordability. Further, there is reason to believe that these households are less able to mitigate the effects of living in disadvantaged areas. For example, they are less able to afford transportation to find economic opportunities elsewhere or pay for private school tuition to provide better schooling for their children. Additionally, housing policy has long had a hand in shaping the geography of opportunity for low-income households, for better or worse. Federal Housing Administration-backed redlining, public housing construction in distressed areas, exclusionary zoning, and urban renewal are tragic examples of government actions that resulted in further concentration of low-income households in low-opportunity neighborhoods. The heavy hand of public policy in contributing to the concentration of neighborhood disadvantage and the inequities produced by such concentrations implore us properly to measure and to understand their causes and consequences and, further, to clarify the complexities of the geographies of disadvantage and opportunity.

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Notes

1. Importantly, this very brief characterization of non-US segregation patterns masks substantial heterogeneity across other Western countries. Recently, Musterd (2005) offered a comparative look at European and US segregation. Musterd concludes that segregation levels (by race and income) are lower in Western European cities than in the USA. However, he cautions that this is largely driven by the relatively extreme segregation of US blacks – other racial and ethnic groups in the USA are no more segregated than most groups in Western European cities. Further, the multiplicity of countries and ethnic groups in Europe results in striking variation across these European contexts. In terms of income segregation, Musterd provides evidence that the poor are substantially more segregated from the middle class in the USA than in Europe. The geography of segregation is also heterogeneous across European cities. Whereas the vast majority of non-white and low-income US metropolitan area residents reside in the central city or inner-ring suburbs, this pattern is found in some European cities but not others. In an examination of six European cities, Musterd, Ostendorf and Breebaart (1997) found that Frankfurt, Brussels, and Dusseldorf reflected the US spatial pattern, but lower-skilled immigrants in Paris and Amsterdam were located outside the city center. London reflected a mix of these two spatial patterns. Wacquant (1993) discussed the poverty and isolation of the Parisian urban periphery in his research on the banlieues of Paris.

2. Dwyer (1997) surveyed six major geography journals and found that, from 1911 to 1995, these journals had published 176 articles on African-Americans. This amounts to over two articles per year.
3. For a thorough discussion of segregation measurement see Iceland et al. (2002); Massey and Denton (1988); Reardon and O'Sullivan (2004); Reardon (2009).
4. This section (and the paper) focuses on the USA. For a good survey of segregation consequences with a global perspective, see Kaplan and Douzet (2011).
5. For extensive reviews, see Dietz (2002) Ellen and Turner (1997) Sampson et al. (2002).
6. Using the non-Hispanic black population as the sole racial indicator is an oversimplification, but that is what how race has been operationalized in the majority of prior research. As such, I use this variable in part to display its limitations.

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