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Racial Variation in the Effect of Incarceration on Neighborhood Attainment

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Abstract

Each year, more than 700,000 convicted offenders are released from prison and reenter neighborhoods across the country. Prior studies have found that minority ex-inmates tend to reside in more disadvantaged neighborhoods than do white ex-inmates. However, because these studies do not control for pre-prison neighborhood conditions, we do not know how much (if any) of this racial variation is due to arrest and incarceration, or if these observed findings simply reflect existing racial residential inequality. Using a nationally representative dataset that tracks individuals over time, we find that only whites live in significantly more disadvantaged neighborhoods after prison than prior to prison. Blacks and Hispanics do not, nor do all groups (whites, blacks, and Hispanics) as a whole live in worse neighborhoods after prison. We attribute this racial variation in the effect of incarceration to the high degree of racial neighborhood inequality in the United States: because white offenders generally come from much better neighborhoods, they have much more to lose from a prison spell. In addition to advancing our understanding of the social consequences of the expansion of the prison population, these findings demonstrate the importance of controlling for pre-prison characteristics when investigating the effects of incarceration on residential outcomes.

Keywords

felon class, incarceration effects, neighborhood attainment, prison boom, racial inequality

The U.S. prison population has quadrupled since the mid-1970s, leaving the United States with the highest incarceration rate in the world (Raphael 2007). This dramatic expansion reflects one of the largest policy experiments of the twentieth century (Spelman 2000), and researchers and policymakers are just beginning to understand the impact this experiment has had on U.S. society. Because imprisonment rates are higher for African Americans and Hispanics than for whites, it is reasonable to assume that rising imprisonment has contributed to existing racial inequalities in U.S. society (Wakefield and Uggen 2010).

Prior studies most often conclude that imprisonment has in fact disproportionately disadvantaged minority ex-inmates, their families, and their communities. To start, the incarceration rate for blacks is over six times larger than the rate for whites, and incarceration has

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become an increasingly common part of the life course, especially for black males with low levels of education (Pettit and Western 2004). Disproportionate incarceration plays a role in racial variation in earnings (Lyons and Pettit 2011; Western 2002; but see Apel and Sweeten 2010 for a more complex picture) as well as certain aspects of health (Massoglia 2008b). Additionally, felon disenfranchisement, or the restriction of voting rights among ex-offenders, disproportionately affects African Americans, and this has had major implications for state and federal elections (Manza and Uggen 2006). Finally, because of large racial discrepancies in incarceration rates, black children are actually more likely to have an incarcerated *mother* than white children are to have an incarcerated *father* (Western and Wildeman 2009).

Recent research suggests that minority ex-inmates may also be disadvantaged in another critical life domain—the residential environment (Hipp, Turner, and Jannetta 2010; Morenoff, Harding, and Cooter 2009). That is, racial and ethnic minority ex-inmates live in poorer and more disadvantaged neighborhoods after prison as compared to white ex-inmates. These studies are limited, however, by their inability to account for neighborhood of origin. This is a key piece of information because the neighborhood of origin for the typical minority prisoner is likely much worse, socioeconomically, than the neighborhood of origin for the typical white prisoner. This is almost certainly the case given widespread racial residential inequality. In 1980 (the year after our longitudinal dataset began), for example, the *average* (median) minority in U.S. urban areas lived in a neighborhood where the poverty rate was as high as, or higher than, the rate where all but 10 percent of urban whites lived (calculated from U.S. Census data for the 53,138 census tracts in U.S. metropolitan areas [see Firebaugh and Farrell 2012]). In other words, fully 9 of 10 whites in 1980 lived in neighborhoods with lower poverty than the neighborhood where the *typical* nonwhite lived.

Given the magnitude of the neighborhood racial divide (Farrell and Firebaugh 2011; Timberlake 2002), it is reasonable to assume that whites will generally have more to lose than minorities from a spell of confinement. It is also the case that incarceration is much more unusual in white communities than in black communities. In Chicago, for example, Sampson (2012) found the incarceration removal rate was about 40 times greater in the black community with the highest rate compared to the highest-rate white community. Because neighborhoods where incarceration is unusual are less likely to welcome their straying members, whites might be less inclined than African Americans to return to their neighborhood of origin. Whether this disinclination will typically result in a move to a poorer neighborhood is an open question.

Although it is clear that blacks reside in the poorest neighborhoods after prison (Hipp, Turner, and Jannetta 2010; Morenoff et al. 2009), we do not know whether this reflects an incarceration effect or existing racial residential inequalities. So the time is ripe for a study of the effect of incarceration on residential attainment that controls for these important preexisting differences in neighborhood quality. Specifically, we ask: After accounting for neighborhood of origin, what is the effect of incarceration on residential attainment, and does it vary by race? To answer these questions, we use a unique nationally representative longitudinal dataset that allows us to track individuals as they transition between prisons and communities across roughly 30 years. By bridging two literatures that, to this point, have been largely separate, our findings also have important implications for research on locational attainment (Crowder, Pais, and South 2012; Crowder and South 2008; Sampson and Sharkey 2008) and on the social consequences of mass incarceration (Pager 2003; Western 2002; Wildeman, Schnittker, and Turney 2012).

The article is organized as follows. We first describe the types of neighborhoods where ex-inmates are known to reside. We then review the respective literatures on residential attainment and on the consequences of incarceration, developing testable hypotheses

on incarceration's impact on neighborhood quality. After briefly noting contributions of the present study, we include an extended discussion of our data and methods. The key here is our use of fixed-effects models, which enable us to estimate the impact of incarceration while accounting for prisoners' neighborhoods of origin. We find that incarceration has a negative effect on neighborhood attainment only for whites, and not for minority ex-inmates or even ex-inmates as a whole. After examining this effect further with a series of robustness tests, we conclude by noting the importance of a deeper and more nuanced understanding of the lasting effects of incarceration on America's growing felon class.

EX-INMATES' NEIGHBORHOODS

Our examination of incarceration's residential consequences focuses on neighborhood disadvantage as an indicator of neighborhood quality. As a group, individuals with a history of incarceration live in less desirable neighborhoods than do individuals without a history of incarceration. The best evidence of this comes from the Returning Home Project, in which researchers tracked released offenders across several metropolitan areas (La Vigne, Kachnowski, et al. 2003; La Vigne, Mamalian, et al. 2003; Visher and Farell 2005). For instance, more than half of the released inmates followed in Chicago settled in just 7 of the 77 total community areas (aggregates of tracts that reflect neighborhoods); these seven areas were typified by high rates of poverty and disadvantage (Visher and Farrell 2005). Furthermore, in an analysis of California paroles, Hipp, Turner, and Jannetta (2010) found that tracts that housed at least one parolee were more disadvantaged and less residentially stable than tracts with no parolees.

Little is known, however, about the processes that channel ex-inmates into these disadvantaged neighborhood environments. Do inmates come from and simply return to the same disadvantaged neighborhoods upon release? Or do prisons push released offenders into more disadvantaged neighborhoods? This gap in our knowledge is notable for several reasons. First, the sheer magnitude of mass incarceration is hard to ignore (Uggen, Manza, and Thompson 2006), with a population approximately the size of Boston, MA, now being released from prison each year. Successful reentry of a stigmatized population of this size depends largely on where ex-inmates settle. There is evidence, for example, that post-prison neighborhood environment affects recidivism (Hipp, Petersilia, and Turner 2010; Kirk 2009; Kubrin and Stewart 2006). This evidence, combined with more general evidence that life is shaped by one's residence (Leventhal and Brooks-Gunn 2000; Sampson 2012; Sampson, Morenoff, and Gannon-Rowley 2002), suggests the importance of ascertaining ex-inmates' residential destinations. Indeed, given the large racial disparities in confinement, it is possible that growth in the prison population has important implications for racial inequalities across a number of dimensions tied to neighborhood context (e.g., health and labor market outcomes) as an outgrowth of its (presumed) effect on neighborhood attainment itself.

It is important to emphasize at the outset that the observed association between incarceration and neighborhood attainment does not necessarily reflect a causal relationship. Ex-inmates are not a random sample of U.S. adults. Compared to the rest of the U.S. adult population, a prisoner is more likely to be male, young, poor, unemployed, a racial or ethnic minority, and have a low level of education (Western 2006). Many of these characteristics, especially socioeconomic characteristics and race/ethnicity, are also correlated with residence in disadvantaged neighborhoods (Logan and Alba 1993; South and Crowder 1997). Quite possibly, then, any association between incarceration and neighborhood quality would disappear if we controlled for such individual-level characteristics.

Moreover, ex-inmates are more likely to reside in disadvantaged neighborhoods *before*

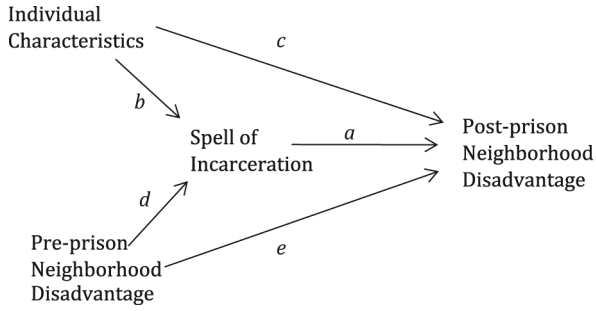

Figure 1. Incarceration Model of Neighborhood Disadvantage

prison (La Vigne, Kachnowski, et al. 2003; La Vigne, Mamalian, et al. 2003; Visher and Farrell 2005), and are likely to return to the same, or equally disadvantaged, neighborhoods after prison. Unless we know where convicted offenders resided prior to prison, it is impossible to determine if the post-release residential conditions they face represent the causal effect of incarceration or simply a reproduction of the neighborhood disadvantage they faced prior to prison. Controlling for individual characteristics alone is thus insufficient to determine incarceration's effect on neighborhood attainment.

Figure 1 makes this point graphically. We want to estimate path *a,* the effect of incarceration on neighborhood disadvantage. Ex-inmates differ in important ways from those who have never been incarcerated: they tend to be poorer and less educated (path *b*), and they tend to come from more disadvantaged neighborhoods before prison (path *d*). Unless we control for these differences between ex-inmates and others, the effect of incarceration on post-prison neighborhood disadvantage will be confounded with effects of individual traits and pre-prison neighborhood conditions. Prior studies of incarceration effects have focused on disentangling causal effects of incarceration from causal effects of individual characteristics (path *c*), but have largely ignored the effect of neighborhood context prior to incarceration, even though residential context before prison is almost certain to have an independent effect on residential context after prison (path *e*). In this respect, failure to account for neighborhood of origin opens questions about how omitted variables might bias our estimates of incarceration's impact on neighborhood characteristics.

We depart from prior studies on incarceration and neighborhood outcomes by employing a modeling strategy that accounts for both individual traits and neighborhood of origin prior to prison. By utilizing a combination of individual data from the 1979 National Longitudinal Survey of Youth (NLSY79) and contextual (tract-level) data from the U.S. Census, our results provide more reliable estimates of the causal effect of incarceration on neighborhood attainment than previously available. Before describing these results, we first draw on existing literatures on neighborhood attainment and on the consequences of incarceration to explicitly outline our research hypotheses.

RESEARCH HYPOTHESES

Residential location is an established marker of social standing (Logan 1978; Logan and Alba 1993), so it is not surprising that Americans are willing to pay more for residence in more desirable neighborhoods. The question of how households sort themselves (or are sorted) into neighborhoods of varying quality is the subject of a longstanding and extensive research literature (Alba et al. 1999; Crowder et al. 2012; Crowder and South 2008; Sampson and Sharkey 2008).

Although incarceration is rarely considered in studies of neighborhood attainment, there are a number of reasons to expect that incarceration affects neighborhood attainment patterns. For example, as a form of

coercive mobility, incarceration, at least temporarily, forcibly removes individuals from their communities (Clear et al. 2003). Upon release, ex-inmates might experience constrained residential options stemming either directly or indirectly from their spell of incarceration. For instance, inmates suffer from fractured social ties and an increased likelihood of divorce, meaning residences prior to prison may not be available upon release (Massoglia, Remster, and King 2011). Furthermore, as noted earlier, incarceration can limit employment opportunities (Pager 2003) and depress wages (Western 2002), which means ex-inmates often lack the socioeconomic resources necessary for residence in desirable neighborhoods. Finally, their status as a socially marginalized group suggests that ex-inmates might be explicitly targeted and excluded from some neighborhoods or communities (Beckett and Herbert 2010).

Furthermore, for the nearly 80 percent of prisoners released on parole supervision (National Research Council 2007), the close monitoring of ex-inmate living arrangements may create additional barriers to finding adequate and stable housing (Petersilia 2003; Travis 2005). Correctional agencies often require preapproval of housing choices, and in many respects housing discrimination against former inmates is now legally sanctioned. Some ex-inmates—notably sex offenders (Hipp, Turner, and Jannetta 2010; Socia 2011; Zgoba, Levenson, and McKee 2009), but increasingly other offenders as well (Beckett and Herbert 2010)—are restricted from living in certain parts of a city. Individuals convicted of drug crimes can be banned from public housing, which, ironically, is specifically intended to provide assistance to those most in need of housing (see Geller and Curtis 2011). Moreover, as an economically marginalized subgroup, ex-inmates often lack the resources to establish private residences. They may also encounter commercial rental agencies that simply refuse to rent to them. Faced with such overt discrimination and increasing legal restrictions, many ex-inmates may have few options outside the most disadvantaged neighborhoods.

We expect the combined effects of legal, financial, and institutional barriers to securing housing will restrict ex-inmates' residential options *more than if they had not gone to prison*. Thus our first hypothesis:

> *Hypothesis 1:* Controlling for neighborhood of origin and other determinants of residential location, ex-inmates will tend to reside in more disadvantaged neighborhoods following release from prison.

Note that Hypothesis 1 applies to the ex-inmate population as a whole, but considerable racial disparities in patterns of residential attainment and rates of incarceration may complicate this general expectation. In particular, African Americans traditionally do not achieve residence in the same quality neighborhoods as comparable whites (Logan and Alba 1993), with high-SES blacks typically falling short of even low-SES whites (Rosenbaum and Friedman 2006). Furthermore, incarceration is becoming so commonplace among black males that it now often constitutes a distinct phase in the life course. At current rates, approximately 60 percent of black males without a high school degree will experience a spell of imprisonment at some point in the life course (Pettit and Western 2004). Coupled with high rates of racial residential segregation, the male incarceration rate in some inner-city areas approaches 25 percent (Lynch and Sabol 2004).

Given these racial disparities in neighborhood attainment and exposure to incarceration, it is reasonable to ask if the consequences of imprisonment will be greater for individual whites or for individual racial/ethnic minorities. On one hand, Morenoff and colleagues (2009:20) find that "black parolees return to census tracts that are one standard deviation more disadvantaged than whites." Similarly, Hipp, Turner, and Jannetta (2010:574) find that African American parolees "move into neighborhoods with more concentrated disadvantage, more residential instability and more racial/ethnic heterogeneity than white parolees." Although both studies reach the same conclusion, they do so without information on

neighborhood of origin. Given extensive racial residential inequality, it may be that incarceration does little to actually change the neighborhood trajectories of minority ex-inmates. Whites, on the other hand, have more to lose given their advantaged starting points, so the effect of incarceration might be more pronounced for them.

In the end, there are compelling reasons to suggest racial variation in the effect of incarceration on neighborhood quality, yet the direction of this effect has not been clearly established. This leads to our second hypothesis, which has not been previously tested with data accounting for neighborhood of origin:

> *Hypothesis 2:* The incarceration effect on neighborhood attainment will differ for African Americans, Hispanics, and whites.

Finally, although the general population typically sees a progression of upward mobility over the course of a housing career (see Lee and Hall 2009), there are plausible reasons to expect incarceration will disrupt this general trend. First, the safety net available to ex-inmates (in the form of post-prison supervision) is likely short-lived. As time passes, any assistance provided to released offenders is either reduced or removed, leaving ex-inmates increasingly on their own to secure housing and employment. Second, limited empirical research on employment and earnings suggests the consequences of incarceration tend to persist or even intensify over time. For instance, Pettit and Lyons (2007), using data on Washington State ex-inmates, find that initial increases in employment after prison were followed by steep declines that eventually fell below pre-incarceration levels. Later work shows that labor market penalties also accrue over time (Pettit and Lyons 2009).

Indeed, part of what makes incarceration so detrimental is its notoriously sticky nature (Braman 2004). Descriptive and interview accounts of ex-inmates suggest that the stigma associated with incarceration can become a master status (Uggen, Manza, and Behrens 2004). The increasing use of incarceration, combined with post-release restrictions placed on returning offenders, has created deeper and longer-lasting distinctions between "us" and "them" (Travis 2002). Consequently, as Travis (2002:19) notes, "punishment for the original offense is no longer enough; one's debt to society is never paid." Ex-inmates are well aware of the challenges they face in the reentry process (Maruna 2001), and many ex-felons consider "their status as felons to be a scarlet letter, leaving them permanently marked or 'branded'" (Uggen et al. 2004:283). In summary, the problems ex-inmates face are, on average, likely to intensify rather than ease over time. This leads to our final hypothesis:

> *Hypothesis 3:* Neighborhood environments will tend to worsen over the years after release from prison for white, Hispanic, and African American ex-inmates.

STUDY CONTRIBUTIONS

In testing these hypotheses, we advance the existing literatures on neighborhood attainment and incarceration effects in several ways. Prior studies on the relationship between incarceration and neighborhood attainment are largely descriptive, with geographically and temporally limited data or few controls (Hipp, Turner, and Jannetta 2010; Visher and Farrell 2005). Indeed, one shortcoming of the Returning Home Project (which documents ex-inmate concentration within clusters of urban neighborhoods) is that it tells us relatively little about whether the parolees were in fact "returning home." Our study employs a nationally representative longitudinal dataset that allows us to examine how the relationship between incarceration and neighborhood attainment plays out across a period of almost 30 years.

Our data also allow us to create residential histories both before and after incarceration, which is a critical feature because, as we will show, conclusions regarding racial variation in incarceration's impact on neighborhood attainment depend on whether one controls for neighborhood context before prison.

Moreover, by examining within-person change, we can treat ex-inmates as their own controls, thereby eliminating omitted variables bias due to unmeasured individual traits that are stable over time for individuals but differ across individuals (Firebaugh 2008). If we find, consistent with Hypothesis 1, that incarceration channels individuals into more disadvantaged neighborhoods, and if disadvantaged neighborhoods are incubators for criminal behavior (Morenoff, Sampson, and Raudenbush 2001; Sampson and Groves 1989; Warner and Pierce 1993), then a potentially important dynamic of the crime/disadvantage relationship becomes the churning of individuals between prisons and communities. That finding would be in line with the conclusion that some neighborhoods have become trapped in a cycle where crime and disadvantage mutually reinforce one another (see Hipp, Turner, and Jannetta 2010).

Finally, our focus on neighborhoods may provide a useful theoretical and organizational template for future research on the consequences of incarceration. As an organizing unit of society, neighborhoods have implications for a wide range of outcomes, including a number of outcomes of interest to scholars studying the consequences of incarceration. Why, for example, does incarceration affect health (Schnittker and John 2007), limit employment opportunities (Pager 2003), and depress wages (Western 2002) independent of human capital characteristics? One plausible answer lies in ex-inmates' geographic concentration in disadvantaged neighborhoods, which may be a mechanism that helps researchers account for adverse incarceration effects over a variety of outcomes. Our analysis thus puts forth a framework that may help scholars better understand the diverse consequences of incarceration.

DATA AND METHODS

Data

Our analysis uses data from the 1979 National Longitudinal Survey of Youth (NLSY79), a data collection that began in 1979 with a group of 12,686 individuals between the ages of 14 and 22 years. Respondents were interviewed yearly from 1979 to 1994 and biennially since 1994; data collection is ongoing. The 1979 start date is ideal for our purposes, as it corresponds roughly with the onset of the prison boom in the United States. We track respondents over the entire range of the data collection period.[1]

After a review for scientific merit, the Bureau of Labor Statistics (BLS) granted our research team access to restricted NLSY79 data that identify respondents' geographic locations (state, county, and census tract) at each wave of data collection.[2] This access allowed us to construct individual residential histories that cover almost three decades. Consistent with other empirical work, we treat census tracts as proxies for neighborhoods (e.g., Jargowsky 1997; Logan, Stults, and Farley 2004; Massey, Gross, and Shibuya 1994; Quillian 2002; Wilkes and Iceland 2004). This allows us to merge the rich individual-level data in the NLSY79 with characteristics of respondents' neighborhoods (e.g., rates of neighborhood poverty and unemployment) across all waves of data.[3] This combination of individual and neighborhood data covering almost 30 years provides us with arguably the best dataset to date for examining individual and neighborhood characteristics before and after prison.[4]

Because some census tracts were reconfigured during the course of the NLSY79, we use tract data from the Neighborhood Change Database (NCDB), which was compiled through a collaboration of GeoLytics and the Urban Institute (GeoLytics 2006). The NCDB standardizes census tract boundaries from earlier years (e.g., 1980 and 1990) to 2000 tract boundaries. This allows us to maintain the same geographic areas across the entire period of inquiry.[5] Respondent neighborhood identifiers were also standardized to the 2000 tract boundaries, and we used linear interpolation to estimate census tract characteristics in non-census years. This is a standard approach in the residential attainment literature

(Crowder and South 2008; Crowder, South, and Chavez 2006; Quillian 1999), but we examine the implications of this strategy in our sensitivity analyses (see Table 4).

Measurement

Incarceration and ex-inmate status. In the NLSY79, incarceration status is measured yearly as an indicator of residential location. Because incarceration status is derived at the time of the interview, prison sentences are observed across time with certainty (Western 2002). Only respondents housed in state or federal prisons are interviewed while confined, meaning that short-term stays in local jails are not captured. Past research has used this residence item to estimate the effect of incarceration on outcomes such as earnings (Western 2002), health (Massoglia 2008a; Schnittker and John 2007), and divorce (Lopoo and Western 2005; Massoglia et al. 2011).

Because our focus of inquiry is life after release, we created two post-confinement measures. The first, called *ExInmate,* is a dummy variable coded 1 for all waves after a respondent leaves prison. Because this measure is coded zero before imprisonment and one after release, it captures the average effect of a prison spell. The second measure, called *TimeOut,* is a count of the number of survey waves since a respondent was last interviewed in prison. This measure speaks to our third hypothesis, which specifies that neighborhood environments will tend to worsen for ex-inmates over the years after their release from prison. A total of 628 respondents were interviewed in, and subsequently left, prison over the three decades of observation in the NLSY79.

Certain restrictions are necessary to enable clean estimates of incarceration effects. First, because female ex-inmates often differ in important respects from male ex-inmates, we dropped the small number of female ex-inmates from our sample.[6] Additionally, we do not include observation years in which respondents were actually incarcerated. If our analysis included offenders while they were incarcerated, we would be including data on the neighborhood characteristics where prisons were physically located, which would clearly bias our results. Finally, we limit our baseline analyses to a respondent's first spell of incarceration and reentry (we relax this restriction in our robustness checks). If a respondent returned to prison a second time, we removed subsequent observations from analyses. Although the neighborhood implications of offenders churning in and out of prison are important, that is not our focus here. These data restrictions leave us with a total of 558 ex-inmates (288 blacks, 166 whites, and 104 Hispanics) whom we are able to follow for an average of 5.5 survey waves following release from prison.

Neighborhood disadvantage. Place of residence is an important predictor of individual well-being, and neighborhood disadvantage specifically has been implicated in a range of outcomes including prospects for successful reentry (Kubrin and Stewart 2006). We measure neighborhood disadvantage using census tract characteristics identified as important aspects of neighborhood socioeconomic disadvantage: poverty, joblessness, female-headed families, and receipt of public assistance (Crowder and South 2003; Krivo and Peterson 2000; Krivo, Peterson, and Kuhl 2009; South and Crowder 1999). We measure poverty as the percent of all census tract residents living below the poverty line, joblessness as the percent of all tract residents age 16 to 65 years who are unemployed or out of the labor market, female-headed families as the percent of all tract families headed by a woman, and reliance on public assistance as the percent of tract households receiving public assistance income (all measures are based on census tract data from the U.S. Census via the NCDB).[7]

For our analysis we created a disadvantage index score for every census tract by first standardizing and then summing the measures at each wave. Because we are summing standardized scores, the result is an index

with a mean of roughly zero at each time point. Higher scores on this scale reflect residence in a more disadvantaged neighborhood, so a positive coefficient for our post-prison measures would indicate that ex-inmates tend to live in *worse* neighborhoods following prison. The neighborhood disadvantage index is highly reliable, with an average alpha over .90 across the 23 survey waves.

Control variables. Our models also account for a range of relevant time-varying individual characteristics. With access to census tract locators for each respondent in each survey wave, we created a measure of the number of inter-tract moves a respondent made throughout the survey period. Additionally, because there may be something unique about individuals who move frequently, we created a squared term for this measure (Hipp, Petersilia, and Turner 2010). We measure educational attainment using number of years of school completed. Our measure of poverty status at the individual level is a dummy variable, coded 1 for respondents whose family income was at or below the federally established poverty level, given family size, for a specific year. We also include dummy variables coded 1 if respondents owned a home, if they resided in public housing at the time of the interview, if they reported being married at the time of the interview, and if they reported being employed at the time of the interview. Finally, our measure of number of residential children is based on household rosters.

We also control for respondent age and age-squared to account for the fact that not all prisoners are at the same point in the life cycle when they leave prison. Although prisoners tend to be young, there is variation in the age of admission to and, more importantly, release from prison. We account for age to capture any difference it may have in determining neighborhood destinations. Finally, to account for any unobserved period effects, we include dummy variables for each survey wave (with wave 1 omitted as a reference category). Time-invariant characteristics, such as criminal propensity and race, are accounted for through the use of fixed-effects models, and thus are not explicitly included as covariates in the models.

Analysis Strategy: Fixed-Effects Estimation

Prior estimates of the effect of incarceration on neighborhood attainment are suspect because, as noted earlier, they are based on analyses that do not control for pre-prison neighborhood environments. To address this issue, we use fixed-effects models that allow us to examine change in our dependent variable (neighborhood disadvantage) for individuals over time. Given the longitudinal nature of the NLSY79, fixed-effects models are optimal because they eliminate bias from the effects of unmeasured stable person-specific characteristics (Allison 2005). By focusing on within-person change, fixed-effects models can remove effects of time-stable characteristics of the neighborhood where a person previously resided, because those characteristics are constant over time for a given individual. By alleviating such likely sources of spuriousness, fixed-effects models generally provide a conservative test of the effect of interest (Allison 2005; Guo and VanWey 1999; Halaby 2004), which in this case is incarceration's effect on neighborhood attainment.

Formally, the fixed-effects models that we estimate can be expressed as follows:

$$Y_{it} = \alpha_{t1} + \mu_{i1} + \beta_1 ExInmate_{it} + \boldsymbol{\gamma_1} X_{\boldsymbol{it}} + \varepsilon_{\boldsymbol{it}} \quad (1)$$

$$Y_{it} = \alpha_{t2} + \mu_{i2} + \beta_2 TimeOut_{it} + \boldsymbol{\gamma_2} X_{\boldsymbol{it}} + \varepsilon_{\boldsymbol{it}} \quad (2)$$

Here Y_{it} is the disadvantage score for the census tract where respondent i resides at time t, and $\boldsymbol{X_{it}}$ is a vector of control variables described above. Equation 1 captures the average effect of incarceration (*ExInmate*). Then, to determine if the ex-inmate effect intensifies or weakens over time, in Equation 2 we model the ex-inmate effect as continuous (*TimeOut*) rather than as a dichotomy (see

Osgood 2010).[8] The α_t are fitted constants that capture unaccounted for period effects, where *t* indexes the *t = 1, 2, ..., T* surveys in the data. We used dummy variables to estimate the α_t (wave 1 of the survey is the reference). Likewise, the μ_i are person-specific fitted constants that make the equations fixed-effects models by eliminating the effects of constant individual traits that are not accounted for by other variables in the model. Pre-prison residential locations differ across inmates, yet are fixed over time for any given inmate, as captured by the μ_i. Critically, the fixed-effects model estimates incarceration's effect on neighborhood attainment based on the change in neighborhood quality after prison for each ex-inmate relative to his neighborhood environment prior to prison (average change, in the case of *ExInmate*). As such, the model alleviates bias from the likely link between pre- and post-prison neighborhood disadvantage (path *e* in Figure 1).

The parameter β_1 represents the effect of incarceration on neighborhood disadvantage. The key aspect of the β_1 parameter is that it represents a *change* in neighborhood conditions after prison. The parameter β_2, by contrast, bears on whether the incarceration effect intensifies over time. If, as we expect, $\beta_1 > 0$ (that is, ex-inmates tend to live in more disadvantaged neighborhoods, other things equal), then $\beta_2 > 0$ indicates that the incarceration effect worsens over time, in line with Hypothesis 3, whereas $\beta_2 < 0$ indicates a diminishing of the effect over time.

To estimate the fixed-effects model, we first transformed the data into person-period observations. We used only waves in which a respondent had complete information and, as discussed earlier, we restricted our sample of ex-inmates in several important ways, resulting in a final analytic sample of 185,962 person-observations. Following Allison (2005), our fixed-effects model allows for variation in neighborhood disadvantage across time by including α_t, thus capturing unaccounted for period effects (such as macroeconomic secular trends reflecting changing neighborhood conditions for everyone). Finally, because observations using panel data are not independent, we estimated robust standard errors by clustering observations within respondents.

RESULTS

Descriptive Results

Table 1 displays descriptive statistics for all variables, in the person-observation format. In interpreting these summary statistics, one should keep in mind that they represent an average across all waves of data collection, rather than a cross-sectional snapshot from any single point in time. Racial variation in our ex-inmate group reflects racial disparities in the general U.S. correctional population and the oversample of African Americans in the NLSY79 data. Of the 2,629 valid observations of inmates after release, approximately one-half are African American, 30 percent are white, and 20 percent are Hispanic. Our descriptive statistics also reflect well-known racial variation in residential attainment, with white respondents residing, on average, in the best neighborhoods, black respondents residing in the most disadvantaged neighborhoods, and Hispanic respondents in-between. Our data further reflect racial variation in many of our control measures. As Table 1 shows, whites have higher levels of educational attainment, homeownership, and employment. Conversely, African Americans are most likely to report residence in public housing and to have incomes below the poverty line.

We can draw on the descriptive statistics for a preliminary examination of ex-inmate neighborhood conditions; Figure 2 plots disadvantage scores broken down by ex-inmate status and race/ethnicity. Because we use a standardized index, the zero point on the *x*-axis reflects the sample mean, with scores above zero reflecting higher-than-average levels of disadvantage. Two findings stand out. First, there are striking racial disparities in neighborhood attainment, with blacks and Hispanics who have never served time in prison living, on average, in more disadvantaged neighborhoods

Table 1. Descriptive Statistics Based on Person-Observations, by Race/Ethnicity

	Full Sample		Whites		Blacks		Hispanics	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Dependent Variable								
Neighborhood disadvantage (z-scores)	–.01	.88	–.37	.55	.60	1.05	.20	.85
% in poverty	.16	.12	.11	.08	.23	.14	.19	.13
% unemployed/out of labor market	.44	.13	.40	.11	.49	.15	.47	.13
% female-headed families	.26	.17	.19	.11	.40	.19	.25	.13
% households on public assistance	.10	.09	.06	.05	.15	.11	.13	.10
Focal Independent Variables								
Ex-inmate status[a]	2629	1.41%	780	.76%	1342	2.64%	507	1.55%
Time out of prison[b]	5.82	4.61	6.48	5.15	5.36	4.20	6.04	4.67
Control Variables								
Age	29.27	8.88	28.88	8.78	29.81	8.99	29.63	8.95
$(Age)^2$	935.53	558.84	911.28	550.60	969.72	568.02	958.15	564.71
Number of moves	2.36	2.41	2.30	2.33	2.33	2.48	2.60	2.54
$(Number\ of\ moves)^2$	11.39	20.18	10.72	19.07	11.58	20.96	13.21	22.12
Educational attainment (years)	12.50	2.44	12.79	2.47	12.35	2.14	11.81	2.61
Family poverty status	.16	.37	.11	.31	.26	.44	.19	.39
Homeownership	.32	.47	.39	.49	.20	.40	.29	.45
Public housing residence	.05	.21	.02	.13	.10	.30	.05	.21
Marital status	.42	.49	.49	.50	.27	.44	.45	.50
Number of children	.83	1.13	.76	1.06	.86	1.17	1.03	1.26
Employment status	.71	.45	.75	.43	.65	.48	.69	.46
Person observations	185,961		102,313		50,883		32,765	

Note: SD is standard deviation.
[a](*N*) for ex-inmate status, reported figures represent number and percent of ex-inmate status observations.
[b]Time out of prison mean and standard deviation for ex-inmates only.

than whites who have been in prison. Second, in initial support of Hypothesis 1, there appears to be a detrimental effect of incarceration. That is, whites, blacks, and Hispanics who have served time in prison generally live in more disadvantaged neighborhood environments than do individuals who have not (the differences are statistically significant in each case). To determine if these observed bivariate relationships between incarceration and neighborhood disadvantage are driven by the incarceration experience—rather than individual characteristics or pre-prison neighborhood conditions—we turn to results from our fixed-effects models.

Fixed-Effects Results

We organize the discussion of our fixed-effects results as follows. We first assess effects of ex-inmate status (Hypothesis 1) and time out of prison (Hypothesis 3) on neighborhood disadvantage for all ex-inmates collectively. Then, to assess Hypothesis 2 and provide additional evidence for Hypothesis 3, we present parallel models for white, African American, and Hispanic respondents.

Table 2 reports results predicting levels of neighborhood disadvantage for all NLYS79 respondents. Estimates for the control variables are consistent with a well-established

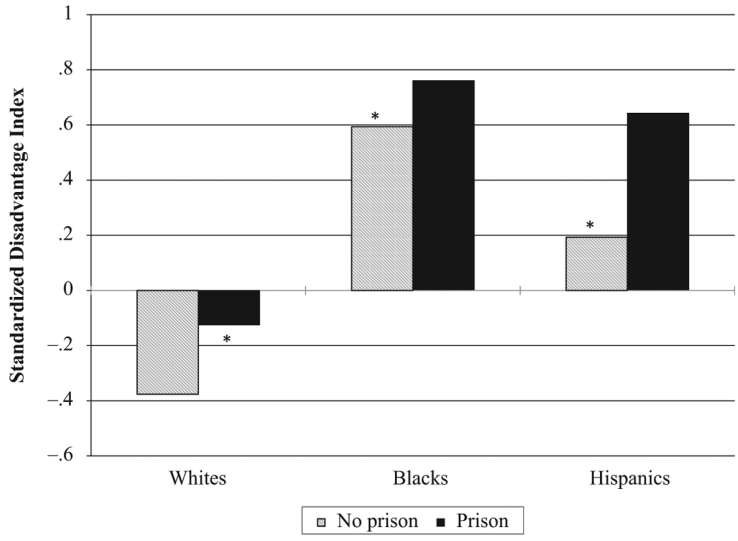

Figure 2. Neighborhood Disadvantage by Race and Ex-inmate Status, OLS Specification
Note: $^*p < .05$, two-tailed test.

research tradition on neighborhood attainment. For instance, individuals in poverty and those who live in public housing are more likely to live in disadvantaged neighborhoods (positive coefficients represent an increase in disadvantage). In contrast, individuals who are married, employed, and homeowners are less likely to live in disadvantaged neighborhoods. Because these findings are consistent with previous studies, they lend support to our data and approach.

We are primarily interested, however, in the effects of our two incarceration measures, which are examined separately in Model 1 (*ExInmate*) and Model 2 (*TimeOut*). Recall that, because they are based on a fixed-effects model, the incarceration effects reported in Table 2 are net of the effects of disadvantage levels in neighborhood of origin.

The findings provide little support for either Hypothesis 1 or Hypothesis 3. After accounting for time-varying individual characteristics through control variables and time-invariant individual characteristics through the fixed-effects design, the coefficients for our incarceration variables fall short of statistical significance. The coefficient for *ex-inmate status* in Model 1—which represents incarceration's average impact across all post-incarceration survey waves—is in the expected direction, but it is not statistically significant. We see a similar pattern for time since release (Model 2). There is thus insufficient evidence to conclude that ex-inmates on the whole reside in more disadvantaged neighborhoods following prison, compared with the types of neighborhoods they resided in before prison.

Although coefficients for the two key variables fail to attain statistical significance, the results in Table 2 are still noteworthy because we controlled for effects of ex-inmates' prior neighborhood environment. Previous research, by failing to measure pre-prison neighborhood conditions, may have overestimated incarceration's impact on neighborhood disadvantage. Furthermore, because results in Table 2 are for all respondents, they may mask important racial variation in the relationship between incarceration and neighborhood disadvantage. From Figure 2 we know that, in each of the three groups, ex-inmates live in more disadvantaged neighborhoods than their never-incarcerated counterparts. To determine if this association reflects a causal effect of incarceration for any subgroup, we estimate race-specific fixed-effects regression models.

Table 2. Fixed-Effects Regression Models Predicting Neighborhood Disadvantage

	Model 1	Model 2
Incarceration Measures		
Ex-inmate status	.031	
	(.039)	
Time out of prison		.006
		(.006)
Controls		
Age	.003	.003
	(.008)	(.008)
$(Age)^2$ x 100	−.019*	−.019*
	(.008)	(.008)
Number of moves	−.068***	−.068***
	(.005)	(.005)
$(Number\ of\ moves)^2$	.004***	.004***
	(.000)	(.000)
Educational attainment (years)	−.009**	−.009**
	(.003)	(.003)
Family poverty status (1 = poor)	.082***	.082***
	(.006)	(.006)
Homeownership (1 = homeowner)	−.046***	−.046***
	(.007)	(.007)
Public housing residence (1 = public housing)	.197***	.197***
	(.018)	(.018)
Marital status (1 = married)	−.063***	−.063***
	(.007)	(.007)
Number of children	−.012**	−.012**
	(.004)	(.004)
Employment status (1 = employed)	−.038***	−.038***
	(.005)	(.005)
Constant	.083	.083
	(.119)	(.119)

Data: National Longitudinal Survey of Youth (1979) years 1979 to 2008.
Note: Sample size: 185,961 person observations; 12,241 persons. All models include dummy variables for survey wave (wave 1 omitted). Robust standard errors are in parentheses.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

These results (Table 3) indicate significant racial variation in the effect of incarceration on neighborhood attainment. Specifically, results indicate that incarceration has a significant impact on neighborhood disadvantage only for white ex-inmates, and is unrelated to neighborhood attainment for either blacks or Hispanics. This is notable for at least two reasons. First, it suggests that the association between incarceration and neighborhood disadvantage observed in Figure 2 is—for blacks and Hispanics but not for whites—attributable to the individual traits or pre-prison neighborhood histories of the ex-inmates themselves. Second, it suggests that the nonsignificant effect of incarceration on neighborhood disadvantage for all ex-inmates collectively (Table 2) masks the significant effect of incarceration for whites.

In support of Hypothesis 2, the NLSY79 data show that incarceration's effect on neighborhood disadvantage does vary by race, but not necessarily in the way one might expect from the results of prior studies. Our results show that, after accounting for neighborhood of origin, it is whites, not African Americans or

Table 3. Fixed-Effects Regression Models Predicting Neighborhood Disadvantage, by Respondent Race/Ethnicity

	Whites		African Americans		Hispanics	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Incarceration Measures						
Ex-inmate status	.152**		–.010		.105	
	(.047)		(.061)		(.075)	
Time out of prison		.020***		<.001		.011
		(.005)		(.010)		(.012)
Controls						
Age	.026**	.027***	–.023	–.023	–.020	–.020
	(.008)	(.008)	(.019)	(.019)	(.018)	(.018)
$(Age)^2$ x 100	–.038***	–.038***	–.025	–.025	.026	.026
	(.008)	(.008)	(.020)	(.020)	(.019)	(.019)
Number of moves	–.030***	–.029**	–.116***	–.116***	–.090***	–.090***
	(.005)	(.005)	(.011)	(.011)	(.011)	(.011)
$(Number\ of\ moves)^2$	.002***	.002**	.007***	.007***	.006***	.006***
	(.001)	(.000)	(.001)	(.001)	(.001)	(.001)
Educational attainment (years)	–.011***	–.011***	–.029***	–.029***	.001	.001
	(.003)	(.003)	(.008)	(.008)	(.008)	(.008)
Family poverty status (1 = poor)	.070***	.070***	.065***	.065***	.083***	.083***
	(.007)	(.007)	(.013)	(.013)	(.013)	(.013)
Homeownership (1 = homeowner)	–.050***	–.050***	–.086***	–.085***	–.063***	–.064***
	(.007)	(.007)	(.018)	(.018)	(.017)	(.017)
Public housing residence (1 = public housing)	.031	.031	.234***	.234***	.228***	.228***
	(.018)	(.018)	(.026)	(.026)	(.043)	(.043)
Marital status (1 = married)	–.038***	–.038***	–.122***	–.122***	–.092***	–.093***
	(.007)	(.007)	(.019)	(.019)	(.017)	(.017)
Number of children	–.019***	–.019***	–.002	–.002	.000	.000
	(.004)	(.004)	(.009)	(.009)	(.009)	(.009)
Employment status (1 = employed)	–.027***	–.027***	–.056***	–.056***	–.015	–.015
	(.005)	(.005)	(.013)	(.013)	(.012)	(.012)
Constant	–.633***	–.636***	1.390***	1.390***	.480	.478
	(.121)	(.121)	(.297)	(.297)	(.278)	(.278)

Note: Sample size (person observations): Whites (102,313), African Americans (50,883), Hispanics (32,765). All models include dummy variables for survey wave (wave 1 as reference), National Longitudinal Survey of Youth (1979) for years 1979 to 2008. Robust standard errors are in parentheses. $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$ (two-tailed tests).

Hispanics, whose neighborhood environments are most affected by a prison spell. Based on our estimates, a prison sentence boosts the neighborhood disadvantage index score by more than one-fourth of a standard deviation for whites ($b = .152/.55$ [standard deviation for whites] = .28), but has no statistically significant effect on the index score for African Americans (Models 3 and 4 in Table 3) or Hispanics (Models 5 and 6). Also noteworthy, for whites, the magnitude of the effect of incarceration on neighborhood disadvantage (β = .152) is more than five times larger than the effect of employment (–.027), four times larger than the effect of marital status (β = –.038), three times larger than the effect of homeownership (β = –.050), and more than twice the size of the family poverty effect (β = .070).

Moreover, for white ex-inmates the adverse effect of incarceration appears to

increase over time (Model 2). Given our use of interpolated neighborhood-level data, we exercise caution in discussion of the effect of time since release, because this parameter could be sensitive to the linear imputation of the dependent variable (as we examine in Table 4). That said, however, results in Model 2 indicate that white ex-inmates' neighborhood environments worsen at an annual rate of .020 standard deviations on the disadvantage index, even after accounting for a host of controls and neighborhood of origin. This is a comparatively strong rate of change, as the effect of each year out of prison on neighborhood disadvantage is nearly twice that of an additional year of schooling.

In summary, results in Table 3 provide a more complete, and complex, picture of the incarceration–neighborhood disadvantage relationship than provided by previous research. After accounting for time-stable characteristics and pre-prison neighborhood context, we find no effect of incarceration on neighborhood disadvantage for either African Americans or Hispanics. In contrast, incarceration has a significant negative effect on neighborhood attainment for whites, and this penalty appears to intensify across time.

Coercive Mobility? Robustness and Sensitivity Analyses

Results in Table 3 indicate that incarceration constitutes a form of coercive downward mobility for whites. However, before accepting this finding at face value, we examine its consistency across a variety of alternative model specifications. Our sensitivity analyses focus on incarceration's effect for white respondents and are broadly organized around ensuring our results are robust in relation to (1) possible deterioration in neighborhood quality while individuals were incarcerated, (2) alternative comparison groups, (3) the possibility of sample "selection creep" (explained below), and (4) use of interpolated neighborhood data. Table 4 summarizes results of these sensitivity analyses, focusing on the effects of our two incarceration measures, *ExInmate* and *TimeOut*. In all cases, we compare results from the sensitivity test to results from Models 1 and 2 of Table 3, which are displayed in the top row of Table 4 (the baseline results).

Neighborhood deterioration. Two distinct processes could be driving our baseline results for white respondents: ex-inmates could be moving to more disadvantaged neighborhoods at release, or they could be returning to neighborhoods that deteriorated during their spell of confinement. Although findings reported in Tables 2 and 3 are adjusted for macro-secular neighborhood trends, we explore the issue further in Model 1 of Table 4 by removing ex-inmates who returned to and stayed in their former neighborhoods upon release (about 20 percent of all ex-inmates). Using the same controls from Table 3, results are virtually identical to the baseline results, suggesting that downward mobility among ex-inmates, rather than neighborhood deterioration, is driving our findings.

Alternative comparison groups. We further restrict the sample by removing everyone who did not move during the study period, regardless of incarceration history. The rationale is that movers and non-movers might differ on some unmeasured time-varying quality, and that our results might be a function of this unmeasured difference. Again, however, our results for respondent movers (Model 2 in Table 4) are virtually identical to findings for the baseline model.

Another possibility is that our results are biased because never-incarcerated individuals differ from ex-inmates in critical time-varying ways that we do not fully measure in our models. We examine this possibility in Model 3 of Table 4 by re-estimating our models after removing all respondents from the sample who have never served a prison term. Although the U.S. felon class continues to grow at a rapid pace, the fact remains that the majority of the NLSY79 respondents never go to prison, so this restriction reduces the white sample by over 98 percent. Despite the reduction in statistical power, coefficients and standard errors for this fairly homogenous

Table 4. Incarceration Effects for Whites: Sensitivity Analyses

	Ex-inmate Status	Time Since Release	Sample Size
Baseline Model From Table 3[a]	.152**	.020***	102,313
	(.047)	(.005)	
Issue: Neighborhood Deterioration			
1. Sample excludes ex-inmate non-movers	.156**	.019***	102,262
	(.049)	(.005)	
Issue: Comparison Group			
2. Sample excludes all non-movers	.157***	.021***	95,996
	(.047)	(.005)	
3. Sample includes inmates only	.120*	.020**	1,995
	(.053)	(.007)	
Issue: Selection Creep			
4. Ex-inmates with 1 to 3 spells of incarceration	.104*	.019***	102,552
	(.046)	(.005)	
5. Ex-inmates with 1 or 2 spells of incarceration	.114*	.018***	102,515
	(.046)	(.005)	
6. Ex-inmates with 1 spell (non-recidivists)	.241***	.023***	101,666
	(.054)	(.005)	
Issue: Use of Interpolated Data			
7. Sample includes only first two waves out	.126*		101,756
	(.059)		
8. 1980 to 1990 difference model (no interpolation)[b]	.261**		3,782
	(.098)		
9. 1990 to 2000 difference model (no interpolation)[b]	.397**		2,705
	(.115)		

[a]The baseline model is presented in Table 3, Models 1 and 2. Unless noted in the text, the models here include all individual-level controls listed in Table 3.
[b]Non-robust standard errors.
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

and conservative inmate-only sample are again consistent with those in the baseline model.[9] The stability in standard errors here is notable, especially given the reduction in analytic sample. Recall, however, that never-incarcerated respondents add no variance to our *ExInmate* or *TimeOut* measures, which will only vary for the incarcerated sample in a fixed-effects framework. As such, the standard errors (which are functions of variance) change very little when we remove the never-incarcerated portion of the sample.[10]

In short, across Models 1, 2, and 3 in Table 4, we tested the robustness of our results by removing, in succession, all residentially stable ex-inmates, all non-movers, and all non-inmates. Consistent with the baseline model, the new results indicate a statistically significant adverse effect of incarceration for whites. Furthermore, although not shown here, the results are also consistent for African American and Hispanic ex-inmates (that is, the effect is consistently nonsignificant).

Selection creep. Another concern is that our decision to remove respondents when they are re-incarcerated creates, over time, an increasingly peculiar group of ex-inmates. If individuals who experience multiple spells of incarceration are meaningfully different from individuals who never recidivate, this could create "selection creep" as the sample becomes increasingly selective.[11] We examine the implications of this decision by expanding

and contracting the number of ex-inmate observations. Recall that the baseline model includes observations for all ex-inmates before incarceration, and then again following release from prison until either the last wave of data collection or a second prison spell. In Models 4 and 5 we add post-prison data for up to the second and third times out of prison. Then, in Model 6 we reduce ex-inmate observations by estimating our models only among ex-inmates who never recidivate and thus eliminate selection creep by holding our analytic sample constant. We note two trends across these three model specifications. First, coefficients for ex-inmate status and time since release remain statistically significant. Second, the effect of ex-inmate status gets progressively larger with a more restrictive treatment of the ex-inmate sample. Because non-recidivists will (on average) have the most post-prison observations, this finding squares with our finding that the effect of incarceration intensifies the longer one is out of prison.

Interpolated neighborhood data. A final potential threat to our findings is our use of interpolated census data. Although this is a common approach in the residential attainment literature (Crowder and South 2008; Crowder et al. 2006; Quillian 1999), the concern is that linear interpolation of intercensal survey waves creates an artificial—and linear—pattern of change in the dependent variable. We examine the implications of this approach in the remaining models of Table 4. Because these models take various forms of first difference specifications, we are unable to estimate an effect for our measure of time out of prison. In Model 7 of Table 4 we make use of limited interpolated data, examining ex-inmates' neighborhood locations for only their first two survey waves following release. In Models 8 and 9 we drop all interpolated data by creating change scores for all variables of interest, based on their values at the different census collection points (1980 to 1990; 1990 to 2000). Results for these models are, once again, consistent with the baseline results.

In summary, across nine different empirical models, with varying model specifications, sample restrictions, control groups, and measurement characteristics, two facts never change. First, for whites the effect of incarceration is always adverse, and the coefficient is always statistically significant. Second, effects for African Americans and Hispanics never reach statistical significance (not shown). In the end, we see no evidence to reject the main findings advanced in Table 3. By employing a research design that accounts for neighborhood of origin, we find that incarceration's causal impact on neighborhood disadvantage is realized entirely for whites. We now consider the substantive and theoretical implications of our empirical findings.

A "More to Lose" Explanation

Our results suggest that whites have much more to lose with regard to neighborhood quality. That is, incarceration likely results in downward residential mobility for whites and no downward mobility for blacks because, in terms of neighborhood quality, whites have the most to lose, and blacks the least to lose. This explanation is plausible because disparities in pre-prison neighborhood environments for whites, Hispanics, and African Americans are massive: on average, blacks are .82 standard deviations above the mean on the standardized disadvantage scale, Hispanics are .62 standard deviations above the mean, and whites are .27 standard deviations *below* the mean (so whites and blacks differ by *more than one standard deviation*).

Indeed, if we replicate Figure 2, but this time using a fixed-effects specification to partial out the effect of pre-prison neighborhood context, we see in Figure 3 that incarceration does not create significant within-person change in neighborhood attainment for either African Americans or Hispanics. Note that this figure is based on a model that does not control for marital status, poverty, homeownership, education, and other individual characteristics that are predictive of neighborhood disadvantage. Even without taking important

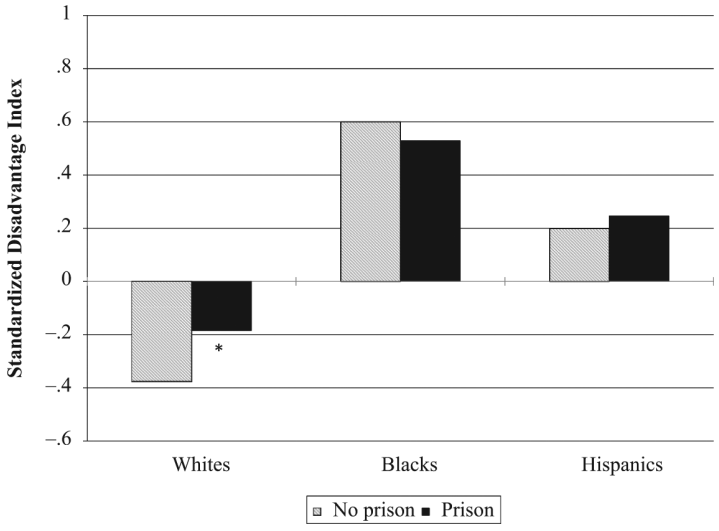

Figure 3. Neighborhood Disadvantage by Race and Ex-inmate Status, Fixed-Effects Specification
Note: $^*p < .05$, two-tailed test.

time-varying predictors of neighborhood attainment into account, we can effectively rule out incarceration as a predictor of neighborhood quality for minorities. White ex-inmates, on the other hand, live in significantly more disadvantaged neighborhoods following prison, over and above pre-prison neighborhood disadvantage.

Our finding that whites have more to lose from a spell of incarceration than do African Americans raises an important question: Why is the incarceration penalty not more severe for whites than for African Americans in other domains where whites are also more advantaged, such as wages (Western 2002)? The answer, we suspect, is that blacks and whites differ much more with regard to neighborhood environment than they do with regard to wages or employment. In 2008, for example, the difference in the average hourly wage for blacks and whites in the NLSY79 data was *less than one-third* of the overall standard deviation in wages. Contrast this with the racial difference in neighborhood disadvantage: as we noted earlier, the average black lives in a neighborhood that is *more than one* standard deviation higher on the disadvantage scale than the neighborhood where the average white lives. In short, the more there is to lose, the more the "more to lose" hypothesis pertains.

DISCUSSION AND CONCLUSIONS

Given the dramatic swelling of the ex-inmate population in the United States, understanding the lasting effects of incarceration on ex-inmates, their families, and their communities is critical. Most research on collateral consequences of incarceration focuses on individual and family outcomes. We know much less about incarceration's effect on residential outcomes such as neighborhood quality. In particular, we do not even know whether ex-inmates tend to reside in more disadvantaged neighborhoods after prison than they did before prison.

By using nationally representative longitudinal data to examine within-person change in neighborhood attainment across time, we discovered that white ex-inmates live in significantly more disadvantaged neighborhoods after a prison spell than they did before. We

found no effect for neighborhood characteristics of ex-inmates as a group, or for African American or Hispanic ex-inmates. Additionally, and again for whites only, incarceration's adverse effect on neighborhood attainment intensifies during the years following release from prison.

What remains to be determined is whether the pre- and post-prison disparity for whites is a pure incarceration effect. The NLSY79 data are relatively limited in terms of measures of arrests and criminal convictions, so we cannot separate out effects of a criminal history from effects of incarceration, at least not directly. Would we see the same downward neighborhood trajectory for whites who are convicted of the same offenses but do not spend time in prison? The weight of the evidence suggests that the pre- and post-prison difference we observed for whites reflects primarily (although perhaps not entirely) the effect of a prison spell, not the effect of criminal offending or a criminal record. Incarceration automatically removes individuals from their neighborhoods; a criminal record does not. In our sample, among individuals uprooted from their neighborhoods by a prison spell, only one in five return to and remain in their pre-prison neighborhoods, and our sensitivity analyses suggest it is the nonreturnees (i.e., the movers) who account for the downward residential mobility among whites (Table 4). In other words, the causal chain appears to be: conviction → prison sentence → uprooted from current neighborhood → move to a new, more disadvantaged neighborhood upon release from prison.

What if conviction does not lead to a prison spell? The chain of events would be different. Because conviction itself does not necessarily, or even likely, uproot an individual from his neighborhood, rates of mobility will be dramatically lower. Among individuals who do choose to move, such a decision is more likely to be voluntary, and thus more likely to result in lateral or upward residential mobility. There is reason to believe, then, that conviction without incarceration will not lead to the downward residential mobility that we observe for incarcerated whites in this study. It remains for future research to verify our findings, and to collect data on offending and convictions as well, to determine how much (if any) of the pre- and post-prison difference is attributable to the effect of a criminal history independent of the effect of incarceration.

In addition to setting an agenda for future research, our results demonstrate the importance of accounting for neighborhood of origin when studying incarceration's effect on neighborhood attainment. Some research in other substantive areas (see Western's [2002] investigation of wages) has accounted for pre-prison conditions, but our study clearly demonstrates the empirical pitfalls of not accounting adequately for pre-prison context when investigating incarceration's effects generally. In addition, our finding of racial variation in incarceration's impact on neighborhood attainment provides further evidence that a spell of incarceration does not have universal effects across different demographic groups. Finally, given that recidivism rates are higher in disadvantaged areas (Hipp, Petersilia, and Turner 2010; Kubrin and Stewart 2006), our results illuminate a process—incarceration leading to downward mobility, at least for whites—that likely bears on the high rates of recidivism among ex-inmates.

By including the U.S. felon class—an expanding population that currently constitutes about 7 percent of the U.S. adult population (Uggen et al. 2006)—in the analysis of neighborhood attainment, this study also contributes to the literature on neighborhood sorting and attainment. Virtually all inmates are eventually released from prison, and each year more than 700,000 released offenders join more than 16 million current or former felons already residing in neighborhoods across the country. The penal system's stratifying effects are now well recognized in other areas, but they have not been fully incorporated into the literature on neighborhood attainment. Our findings here, along with those in recent related analyses (Clear 2007; Hipp, Turner, and Jannetta 2010; Kirk 2009), provide a starting point for an earnest investigation of incarceration's enduring effects on imprisoned

felons and on the neighborhoods where they reside after exiting the prison gates.

Our findings also have a number of policy implications. To say that incarceration tends to harm whites more than African Americans with respect to neighborhood attainment is not to say that incarceration effects always tend to be greater for whites or are always inconsequential for African Americans. Rather, we emphasize that there is substantial and meaningful racial variation in incarceration's effects across different life domains. In some cases incarceration apparently contributes to racial and ethnic inequalities (Lyons and Pettit 2011; Massoglia 2008b; Western 2002). In other cases, such as the results presented here, the incarceration effect is more pronounced for whites. There is evidence that this is also the case for mortality (Patterson 2010) and labeling effects on recidivism (Chiricos et al. 2007). Policymakers should be attentive to these differences in fashioning policies to temper the societal costs of mass incarceration.

We noted earlier that the steep rise in the prison population is largely policy-driven, rather than being tied to any dramatic increase in criminal activity. As such, reductions in the use of incarceration must also be driven by policy. Clearly a balance needs to be struck between public safety and the costs of incarceration. In a time when federal and state budgets are being strained, many observers have started to question the current balance, noting that increased public funds directed to the correctional system come at the expense of funds for education, health, or any number of other public goods and services (PEW 2008). Even if the prison boom has peaked,[12] the consequences of that boom will be felt for decades to come, as large numbers of prisoners are reintegrated into U.S. society. Results presented in this article provide a strong reminder of the need for effective policies concerning that reintegration process.

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Notes

1. Western (2002:532) notes that we can "be confident that the NLSY correctional residence item provides reasonable coverage of prison inmates," and that rates of incarceration captured by the NLSY79 approximate general incarceration trends reflected in official statistics (see Western's Figure 1, p. 531).
2. The restricted data are accessible only at the BLS offices in Washington, DC. This prohibits us from making our data available for replication. Interested researchers should first contact the BLS for data access (http://www.bls.gov/bls/blsresda.htm), at which point we could provide information on data management and replication.
3. Across the 23 data waves, we matched an average of 85 percent of respondents per wave, and this pattern was consistent across racial/ethnic groups. Matching of the ex-inmate sample was slightly more successful, with an average of 89 percent of ex-inmates matched at each wave.
4. A second NLSY study, the NLSY97, provides an alternative national dataset with information on criminal justice involvement. However, the NLSY79 is better suited for our purposes because it spans 18 more years than the NLSY97.
5. To supplement some incomplete tract coverage in 1980, we used a separate GeoLytics database that contains complete 1980 census tract information using the 2000 boundaries. We used this supplemental database for approximately 15 percent of all U.S. census tracts that were not covered in the 1980 NCDB. Furthermore, in cases where respondents are missing tract identifiers but lived in the same tract on either side of their missing interval (up to five waves), we imputed their tract locator for the missing years. This resulted in modest increases in sample size and did not change the pattern of results.
6. This approach is common in the incarceration-effects literature, especially given that the correctional population is over 90 percent male (Geller and Curtis 2011; Guerino, Harrison, and Sabol 2011; Lopoo and Western 2005; Massoglia et al. 2011; Pager 2003; Western 2002; Western and Beckett 1999; Wildeman 2010).
7. There is no standard, universally accepted definition of residential disadvantage. The measures we use are common indicators of disadvantage, but other research commonly incorporates percent

black into the construct (see Sampson, Raudenbush, and Earls 1997). However, we view residential segregation as theoretically different from neighborhood disadvantage, so we elected not to include percent black in our composite indicator. Including it in our measure does not change the pattern of results.

8. In models not shown, we examined whether the effect of time since release was nonlinear through the use of a squared term, but this coefficient never reached conventional levels of statistical significance.
9. The reduced sample size based on a model of white respondents with a history of incarceration (Table 4, Model 3) dramatically diminishes our statistical power and makes estimating age and period effects problematic. As such, Model 3 retains all individual-level controls displayed in Table 3 but omits controls for age and period. To ensure our results were not driven by some particularities of a given model specification, we re-estimated all models in Tables 2 and 3 with and without controls for age and period effects and results did not change.
10. Where we do see major differences here is in the standard errors of our control variables, which increase substantially. The never-incarcerated sample contributes most of the variance to those variables, so including the never-incarcerated in our baseline models provides us with more precise estimates of the controls (and thus better estimates of the incarceration variables).
11. We thank the reviewers and editors for this phrase, which succinctly captures the issue.
12. In 2010, the overall U.S. prison population declined for the first time since 1972 (Guerino et al. 2011). Signaling another possible emerging trend, the Supreme Court recently required the State of California to reduce their prison population by 30,000 inmates (*Brown v. Plata* 2011).

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