Recidivism in context: A meta-analysis of neighborhood concentrated disadvantage and repeat offending

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ABSTRACT

The relationship between macro-level concentrated disadvantage and crime is well established. Recent research has assessed whether macro-level concentrated disadvantage is similarly linked to individual-level recidivism, yielding mixed results. These equivocal results raise methodological concerns and questions as to the theoretical underpinnings of this relationship. To build consensus regarding the relation between concentrated disadvantage and recidivism, this study meta-analytically synthesized prior research (k = 32), and tested the degree to which study and sample characteristics explain variation in effects across studies. We find little support for concentrated disadvantage as a risk factor for recidivism after studies adjust for individual-level risk markers and factors (pooled log OR = 0.03, p = 0.07). We also, however, find effects vary by the age group studied and type of recidivism measured, with significant effects for juveniles and arrests/revocations. In turn, concentrated disadvantage should not be summarily dismissed as irrelevant to recidivism. Ultimately, the overrepresentation of disadvantaged neighborhoods among the justice involved—and the overrepresentation of the justice involved in disadvantaged neighborhoods—requires further research that is both empirically tenable and theoretically informative.

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1. INTRODUCTION

Recidivism poses a significant challenge to reducing incarceration rates in the United States. Best estimates suggest that five out of every six formerly incarcerated persons will fail community supervision, be rearrested, or reincarcerated within nine years of release from prison (Alper & Durose, 2018). As such, decarceration efforts depend on research that identifies and directs interventions that target risk factors for recidivism.

Most recidivism research focuses on testing individual-level characteristics as potential risk factors for reoffending. This work identifies demographic markers (i.e., age, gender, and race) and other individual characteristics (e.g., history of offending, anti-social thinking and personality traits, and anti-social peers) as predictors of recidivism (for a full discussion, see Bonta & Andrews, 2016). These demographic and psychosocial factors fairly consistently predict the likelihood that a once adjudicated person will offend again.

In recent years, scholars have questioned the reductionist nature of recidivism research's focus on individual-level risk factors. Kubrin and Stewart (2006) lead this charge, indicting prior scholarship for its "neglect of neighborhood" contexts in recidivism studies. Their analysis of 4,630 released prisoners in Oregon indicated that neighborhood-level factors predicted recidivism, above and beyond other established individual-level risk factors. The study marked an ecological turn in the study of repeat offending (Jacobs et al., 2020). Within this paradigm shift, no construct has received more attention than concentrated disadvantage: the socio-structural and economic conditions that demarcate "truly disadvantaged" neighborhoods (Wilson, 2012).

Though the ecological turn has garnered considerable interest in the connection between concentrated disadvantage and recidivism, no empirical consensus exists regarding this relationship. Reviews of the literature indicate tests of the concentrated disadvantage-recidivism relationship yield mixed results (see, e.g., author; Jacobs et al., 2020). Further, when associations are statistically significant, they are typically small. These small effects and mixed results may stem from several factors that could lead to variation and weaken effects across studies, including threats to measurement and internal validity, and/or other factors related to study design, sample, and measurement. In the remainder of this paper, we seek to clarify the concentrated disadvantage-recidivism relationship and to identify factors that may explain variation in effects across studies. We begin by providing background on concentrated disadvantage as a construct and its operationalization, and then unpack several plausible explanations for its small and variable effects across recidivism studies.

1.1. Conceptualizing and operationalizing concentrated disadvantage

Though relatively new to the study of recidivism, criminologists and sociologists in the macrosocial tradition have had longstanding interest in the relationship between neighborhood conditions and crime (see e.g., Burgess & Bogue, 1964; Sampson et al., 2018; Shaw & McKay, 1942). Within this tradition, features of what researchers now call concentrated disadvantage have been a focus. For example, Chicago School sociologists of the early 20th century identified poverty and neighborhood immigrant population among key factors in shaping delinquent behavior among youth (e.g., Shaw & McKay, 1942). Their theory of social disorganization is now among the most common in research on the concentrated disadvantage-recidivism relationship.

In the latter part of the 20th Century, Wilson (2012) developed a theory of spatial mismatch to explain the transformation of urban, Black communities from thriving, mixed-class communities to high-poverty communities, with few social or economic resources and increased crime. Building from Wilson's concept of "concentration effects," subsequent researchers operationalize concentrated disadvantage by combining several features of these communities—i.e., poverty, joblessness, receipt of public benefits, single female headed households, and sometimes the proportion of residents who are Black—into a single, multidimensional measure. Other neighborhood features are theoretically relevant (e.g., organizational and institutional resources, like schools, social services, and healthcare), but they are not typically included in measures of concentrated disadvantage, since they are not readily gleaned from Census data or what data is available is at a different spatial unit than a typical neighborhood.

Empirically, concentrated disadvantage tends to correlate with crime and incarceration (Hsieh & Pugh, 1993; Kurgan, 2013; Simes, 2018; for a review, see Pratt & Cullen, 2005). In one of the more recent and comprehensive studies on this relationship, Chamberlain and Hipp (2015) examined the effect of disadvantage on violent and property crime in 79 cities. After adjusting for other structural factors (e.g., residential stability, percent occupied units, relative deprivation, and racial composition), they found neighborhood concentrated disadvantage was a consistent, moderate predictor of violent crime and a weak, but also consistent, predictor of property crime. Pratt and Cullen's (2005) meta-analysis (k = 214) of "macro" influences on crime, also provides firm support for this relationship: factors related to concentrated disadvantage were the strongest and most consistent predictors of crime.

1.2. The theoretical and empirical relation between concentrated disadvantage and recidivism

Just as concentrated disadvantage is related to crime rates, it is plausibly related to recidivism. Incarcerated people are likely to come from and return to disadvantaged neighborhoods (Clear, 2009; Harding et al., 2013), where a trifecta of forces convene to enhance recidivism risk. First, typically low on social and economic capital (Clear, 2009), people in reentry face numerous challenges to getting on their feet and avoiding recidivism-- and have access to few resources with which to overcome challenges. Consequently, upon release, formerly incarcerated persons need to lean on neighborhood resources, whether that is through local organizations such as shelters, food banks, temporary employment agencies, or local employment networks (Fleisher et al., 2001; Wallace, 2015). However, one of the defining characteristics of a neighborhood with concentrated disadvantage is that residents are isolated from licit employment and associated social networks, as well as neighborhood-level resources. It is this social and economic deprivation that motivates most research on the concentrated disadvantage-recidivism relationship (see, e.g., Hipp et al., 2010; Kubrin & Stewart, 2006).

Concentrated *reentry* also weakens the ability of disadvantaged neighborhoods to respond to the needs of formerly incarcerated persons (Clear, 2009; Harding et al., 2013; La Vigne et al., 2003; Travis & Waul, 2003). Disadvantaged neighborhoods are not only overrepresented among incarcerated persons; incarcerated persons are also overrepresented within disadvantaged neighborhoods. In such contexts, residents—whether or not they are previously incarcerated—are competing for scarce resources (Chamberlain & Wallace, 2016), such as affordable housing, employment, or local resources from organizations (Wallace, 2015). In turn, individuals lacking in resources fare poorly within these neighborhood conditions.

Lastly, surveillance practices in disadvantaged communities may affect reoffending. Policing is not purely a tool for crime control as conflict theories of social control argue (see Liska, 1992; Tittle, 2018). Police surveillance of sub-criminal behavior is heightened in disadvantaged neighborhoods (Fagan & Davies, 2000; Sampson, 1986; Sampson & Loeffler, 2010). In New York, for example, Fagan and Davies (2000) found that for every percent increase in residents' reliance on public benefits, a neighborhood's number of police stops more than doubled, even after accounting for crime rates. As a result, they noted, "evidence suggests that policing is not about disorderly spaces, nor about improving quality of life, but about policing poor people in poor places" (p. 457; see also, Smith, 1986). In the context of such "broken windows" policing, low-level disorderly behavior and serious crimes are indiscriminately targeted, and arrests result in relatively few convictions (Fagan & Davies, 2000). Given arrests reflect both surveillance and criminal behavior, it is unsurprising then that some studies find no association between concentrated disadvantage and self-reported criminal behavior, but do find a relationship between concentrated disadvantage and arrests (see, e.g., Kling et al., 2005). Ultimately, through propinquity, increased criminal justice contact among individuals is likely to occur in disadvantaged neighborhoods and may account for enhanced recidivism risk, especially when recidivism is defined as arrest for low-level offenses.

Together, individual need, taxed informal and formal support systems, and increased surveillance, and relatedly deprivation, social capital, and social control orientations, provide reasonable theoretical rationales for the connection between disadvantage and recidivism; empirically, though this relationship is less clear. To date, no systematic review of research on the concentrated disadvantage-recidivism relationship among adults exists. One systematic review of ecological risk factors for recidivism among juveniles provided mixed support for the concentrated disadvantage-recidivism relationship (Jacobs et al., 2020). This study found that each standard deviation increase in concentrated disadvantage was associated with an increased risk of re-arrest by 9% (pooled OR = 1.09, p = .01, k = 15). However, results also indicated half of studies (17 of k = 36) that tested the relationship between concentrated disadvantage and reoffending more broadly (i.e., including other measures of recidivism) found statistically non-significant effects. Why, especially in light of the theoretical connection between characteristics of concentrated disadvantage and reoffending, might empirical tests yield equivocal results?

1.3. Challenges to assessing the concentrated disadvantage-recidivism relationship

Several factors, as summarized below, make empirical analysis of the concentrated disadvantage-recidivism relationship challenging and may account for mixed results.

1. *Concentrated disadvantage's restricted distribution among the justice-involved.* Establishing risk factors for recidivism requires that, at a minimum, risk and recidivism covary (Monahan & Skeem, 2013). Concentrated disadvantage, as discussed above, is a fairly consistent predictor of differences in crime rates at a macro-level, where neighborhood disadvantage varies greatly across units. However, research also indicates that the effect of concentrated disadvantage on crime rates is diminished in samples with "restricted distributions" (McNulty, 2001). This lack of variation has important implications for analyses. The effect of disadvantage on violent crime rates is substantially weaker at higher ends of the disadvantage distribution, which produces smaller effects of concentrated disadvantage for Black neighborhoods than for White neighborhoods (where extreme

disadvantage is uncommon). In a similar sense, justice-involved samples do not hail from a range of neighborhoods. Thus, the restricted distribution of concentrated disadvantage among justice-involved samples—for whom disadvantage is likely to be high and constant— may weaken the ability to detect associations between concentrated disadvantage and recidivism.

2. *Surveillance may confound the concentrated disadvantage-recidivism relationship.* Recidivism is a product of behavior *and* surveillance (Jacobs et al., 2020) and, as noted above, police surveillance and interaction with civilians vary depending on neighborhood disadvantage. Thus, the non-random nature of police surveillance may confound the relationship between concentrated disadvantage and recidivism. In other words, in some neighborhoods, police surveillance impacts contact with the criminal justice system to such an extent that it becomes challenging to distinguish between recidivism due to concentrated disadvantage or recidivism due to hyper-surveillance. For this reason, surveillance may threaten the internal validity of studies that test the relationship between concentrated disadvantage and recidivism. Such confounding may be especially problematic when recidivism measures include minor offenses or violations, or are drawn from revocation or arrest data. These measures are more reflective of surveillance than recidivism measures that focus on violent or other serious offenses, or that draw on reconviction or reincarceration data (Blumstein & Larson, 1971; Harris et al., 2011).

3. *Concentrated disadvantage may lack conceptual equivalence across sub-populations.* As noted above, Wilson (2012) developed the concept of concentration effects in relation to the unique social and economic features of largely homogenous Black, poor, isolated, urban communities. If, as Wilson and colleagues make clear, concentrated disadvantage functions differently by neighborhood racial dynamics, studies that test effects among mixed-race samples may produce weak effects because the construct may not fully capture disadvantage for people in reentry who are members of other ethnoracial groups (Sampson et al., 1995, 2018). It is likely that some dimensions of concentrated

disadvantage (e.g., median income, poverty rate) bare relevance across groups and contexts, as research on the concentrated disadvantage-crime relationship demonstrates (see above). However, even modest threats to concentrated disadvantage's conceptual equivalence across groups and contexts may tip effects from significant to non-significant for the outcome of recidivism, where macro-level effects on individual behavior are already likely to be small.

4. *Sample characteristics may alter the effect of concentrated disadvantage on recidivism.* Not all residents respond in the same way to their contexts, making differential neighborhood effects common. For example, results from the Moving to Opportunities experiment indicated that moving from a high disadvantage neighborhood to a low disadvantage neighborhood had different effects on youth, depending on their age and gender (Kling et al., 2005; Leventhal & Brooks-Gunn, 2003; Sanbonmatsu et al., 2011; Sciandra et al., 2013). Movement affected different outcomes at different ages and, in general, provided more benefits to girls than boys or mothers.

5. *Study design features may alter the effect of concentrated disadvantage on recidivism.* Neighborhood effects vary by characteristics of study design (Sampson et al., 2002; Sharkey & Faber, 2014). Much attention has been afforded to how geographic units are defined and measured in multilevel studies (see, e.g., Hipp, 2007; Peterson & Krivo, 2005). Some effects may be detectable at smaller (e.g., blocks, neighborhoods) or larger (e.g., zip code, County) geographic units, a reality that may account for small and non-significant estimates in studies that arbitrarily choose a geographic unit of measurement. Here, effects may be muted when measured at the county or region level because concentrated disadvantage is likely to vary substantially within large areas. A county's overall level of disadvantage may not unilaterally reflect disadvantage for all neighborhoods within that county, including those where former offenders reside. Over-control for individual-level factors in neighborhood effects studies may reduce effects in some studies (Sampson et al., 2002). This occurs when researchers control for factors that may actually mediate the relationship between disadvantage

and recidivism (e.g., unemployment) in an effort to establish the effect of neighborhood characteristics above and beyond individual-level characteristics.

In sum, the effect of concentrated disadvantage on recidivism may vary not because of disadvantage's irrelevance (or relevance), but instead due to homogeneity in disadvantage's variance, threats to content and internal validity, and sample or other study features. Meta-analysis, and especially meta-regression techniques, are expressly useful for assessing this possibility.

1.4. Current Study

This paper aims to clarify the concentrated disadvantage-recidivism relationship. To achieve this aim, we conducted a systematic review of existing research and answer two questions: (1) to what degree, if at all, does concentrated disadvantage predict recidivism? And, (2) does the effect of concentrated disadvantage on recidivism depend on sample or study characteristics? Concentrated disadvantage and recidivism are theoretically linked, but empirically establishing this relationship is challenging. As such, we hypothesize that the pooled effect of concentrated disadvantage on recidivism will be statistically significant, though it is also likely to be small. We also anticipate that sample and study related factors will alter the effect of concentrated disadvantage on recidivism. Results from meta-analysis and meta-regression analyses below test these hypotheses, providing a much-needed synthesis of evidence to date on the concentrated disadvantage-recidivism relationship.

2. METHODS

We conducted a systematic review and meta-analysis of existing studies that test the relationship between concentrated disadvantage and recidivism. In doing so, we followed standards set forth in the Preferred Reporting Items for Systematic Reviews and Meta-analyses (PRISMA; Moher et al., 2015), and supplemented the PRISMA procedures with substantive guidance from Murray et al. (2009). Below we describe our inclusion criteria, search strategy, study selection, critical appraisal, measures, and the analytic strategy.

[Insert Figure 1]

2.1. Inclusion criteria

Meeting our inclusion criteria, studies in the meta-analysis were: (1) conducted in the United States; (2) published between 1980 and April 2019; (3) included concentrated disadvantage as a predictor variable (see below); (4) included an outcome measure of recidivism (see below); (5) included a follow-up of six months or greater; (6) used a quantitative design; and, (7) provided an odds ratio or log odds ratio for the effect of concentrated disadvantage. We defined concentrated disadvantage as a multi-dimensional characteristic of residential contexts that captures the synergy of social and economic factors representative of "truly disadvantaged" communities (Wilson, 2012; see also, Sampson et al., 1995). In turn, studies were only included if their definition was multi-dimensional and included economic and social elements. We defined recidivism as a criminal offense following prior adjudication, as indicated by revocations, rearrests, reconvictions, or re-incarcerations.

2.2. Search strategy

We conducted a search of the peer reviewed literature and grey literature, including conference abstracts, government reports, dissertations, and book chapters. A librarian trained in systematic review methods (author) developed the search strategy and compiled the responses. For a full description of search terms and procedures see author (year).

2.3. Study selection and review

We used DistillerSR (Evidence Partners, Ottawa, Canada) to manage citations and in the review process. In phase one, two reviewers (author and author) reviewed titles and abstracts with conflicts resolved through consensus and discussion with the principal investigator (author). In phase two, the two reviewers reviewed the full texts of studies that met criteria in phase one. Conflicts in the full text phase were resolved through review by the principal investigator and team discussion. In phase three, the two reviewers used a standardized form in Qualtrics (Qualtrics, Provo, UT) to extract data on the study sample, geographic unit of analysis, statistical approach, concentrated disadvantage, recidivism, controls, and results. Finally, we appraised study quality (see below).

2.4. Quality appraisal

We used the methodological quality checklist created by Murray and colleagues (2009) to assess and score the quality of included studies. We adapted the framework in two ways. We include an indicator that accounts for the spatial nature of the analyses, scoring multilevel studies higher if they contain adequate sample sizes (McCoach, 2019). We also scored studies lower if they rely solely on administrative data collected retrospectively, as this constrains analyses in ways that primary and prospectively collected data do not (for a full overview, see author, date). Ultimately, the appraisal provided descriptive information on study design features, and ensured a base-level of quality and comparability across studies (see Table 1).

[Insert Table 1]

2.5. Measures

2.5.1. Outcome variable: Effect size estimates

In meta-analysis, the outcome can be thought of as the effect of interest (Pratt et al., 2016). Here, the outcome of interest is the effect of concentrated disadvantage on recidivism. Studies that test this relationship attempt to reduce potential spuriousness by adjusting for individual-level markers and risk factors that increase recidivism risk (e.g., age, gender, race, and criminal history; Bonta & Andrews, 2016), and may confound the concentrated disadvantage-recidivism relationship. Studies typically reported adjusted log odds ratios from multivariate logit models (the most common analytic approach used in prior studies).[i] Thus, we operationalize the outcome as effect size estimates (log odds ratios) for the association between concentrated disadvantage and recidivism, above and beyond established recidivism risk markers and factors.

2.5.2. Effect moderators

Based on our prior discussion of factors that may explain variability in the effect of concentrated disadvantage on recidivism between studies, we examined several potential moderators of this relationship, including those related to sample and study design (see Table 2). To assess variation in concentrated disadvantage's effects by sample characteristics, we measured age, gender, and race. We operationalized *age* as a nominal variable with two categories, juvenile and adult. We measured *gender* as the percent of the sample that was female. We measured *race* as the percent of the sample that was White.[ii]

[Insert Table 2]

To test variation in concentrated disadvantage's effects by study characteristics, we assessed concentrated disadvantage's operationalization, recidivism's operationalization, geographic unit, and control variables. We coded *racialized concentrated disadvantage* (included % Black = 1 or a race-specific measure[iii]) to assess if inclusion of race in the study's construction of concentrated disadvantage affected its association with recidivism. We operationalized *geographic unit* as a two-category nominal variable, including small units (neighborhood, Census block group, Census Tract, and zip code) and large units (county or region).[iv] As a potential indicator of over-control, we measured *control variables* as the total number of covariates included in the models from which we extracted coefficients for concentrated disadvantage. To assess variation in effects related to study outcomes, we coded *evidence type* as a nominal variable with two categories (revocation/arrest and conviction/incarceration) and *offense type* as a nominal variable with three categories (drug, property, and violent).

2.6. Analytic strategy

We meta-analytically summarized the effect of concentrated disadvantage on recidivism and used meta-regression to test factors that may explain variation in this effect across studies. First, we addressed the complex data structure representing the parameter and variance estimates used in our models. This complexity arises because some studies conducted multiple tests and reported multiple coefficients. Depending on the nature of the repeated analyses (e.g., whether independent samples are included or multiple outcomes are tested), including these estimates as independent could inappropriately assign more weight to studies with multiple analyses and inflate the precision of parameter estimates (Borenstein et al., 2009). As such, we handled the reporting of multiple coefficients in accordance with recommendations from Borenstein and colleagues (2009), and the number of analyses for some tests of moderation differ from the meta-analysis (depending on how the moderator was handled in each study).[v] For example, our assessment of offense type as a moderator is based off of a sub-sample of studies that tested concentrated disadvantage across different crime types.

Next, to meta-analyze these data, we used random effects models with the metafor package in R (Viechtbauer, 2010). The random effects model estimates the average effect size from a hypothetical population of studies and total effect heterogeneity, under the assumption that effect heterogeneity is due both to sampling error and the fact that studies are conducted across a larger universe of populations (DerSimonian & Laird, 1986; Higgins & Green, 2008). Heterogeneity (τ^2) was assessed using the restricted maximum-likelihood estimation method and effect sizes were weighted via inverse probability weighting ($w_i = 1/(v_i + \tau^2)$). The single-level random effects model calculates the average effect of concentrated disadvantage, while accounting for heterogeneity at the sample level (i.e., sampling error). The multilevel model also accounts for heterogeneity at the study (i.e., heterogeneity between studies) and data source levels (i.e., heterogeneity between studies grouped by data source). The multi-level model avoids falsely constraining heterogeneity, which could result from dependence between analyses that share sampling frames, design features, or other features. We assessed the fit of single-, two-, and three-level models for each analysis and provide coefficients for the best fitting model, according to likelihood ratio tests (LRT).

We conducted a series of random effects meta-regression models to test the degree to which sample and study characteristics explain variance in the effect of concentrated disadvantage on recidivism across studies (i.e., were effect moderators). Some models include only a sub-sample of relevant studies because some studies did not provide information on the moderator tested. As with our model for the pooled estimate of concentrated disadvantage, we used likelihood ratio tests to guide our selection of the appropriate meta-regression model— single or multilevel.

Finally, we assessed publication bias and influential studies across all models. We used Egger's test, including sample size as a moderator in the multilevel model, to assess publication bias. We calculated Cook's distance and dfbeta coefficients to assess influential studies. Where influential studies were noted, analyses were conducted with and without those studies. We describe any cases where inclusion of these studies notably altered effects.

3. RESULTS

Results from random effects meta-analysis and meta-regressions are presented below. To answer our research questions, results estimate the overall (i.e., pooled) effect of concentrated disadvantage on recidivism and identify sample and study-related moderators of this effect. To contextualize these results, we begin by describing studies that assess the concentrated disadvantage-recidivism relationship.

3.5. Study characteristics

We drew data from a total of 30 studies, with 32 samples, and 17 related data sources (see Table 3). The average sample size was 18,518 (range 323-105,573). Most samples were of adults (N=18), and were mostly male (79%) and non-White (52%). Measures of recidivism were close to evenly split between arrest/revocation (N=17) and reconviction/incarceration (N=15). Ten analyses measure concentrated disadvantage at the county or regional level, with the remaining majority (N=22) measuring at the neighborhood, Census block group, Census tract, or zip code level. Finally, measures of concentrated disadvantage most often included dimensions of poverty, public assistance receipt, single parent household representation, unemployment, and income; less often, they included high school completion or used a race-inclusive measure of concentrated disadvantage (see Figure 2).

[Insert Figure 2]

4.2 What, if any, effect does concentrated disadvantage have on recidivism?

We used a three-level, random effects model to estimate the pooled effect of concentrated disadvantage across 32 analyses (see Fig. 3). The random effects account for potential variation due to sample-, study-, and data source-related heterogeneity. Based on this model, the average effect of concentrated disadvantage on recidivism is statistically non-significant (log OR = 0.03, p = 0.07).[vi]

The lack of a significant pooled effect for concentrated disadvantage may be due to meaningful variation in effects between groups of studies. Variation in coefficients across analyses is beyond what we would expect due to sampling error (Cochran's Q = 146.47, p < 0.01), and a substantial proportion of concentrated disadvantage's effect heterogeneity (τ^2 = 0.01) is caused by something other than sampling variance (i.e., I^2 = 82.63; Higgins et al., 2003). The largest portion (56.25%) of this heterogeneity exists at the data source-level and may reflect differences in concentrated disadvantage's effect on recidivism across populations and geographies. It also indicates the pooled effect may be diluted by studies that share sample or design (e.g., more men, larger geographic units) features that may weaken disadvantage's effect on recidivism. We now test whether these features alter the effect.

[Insert Figure 3]

4.3 Does the effect of concentrated disadvantage on recidivism depend on sample or study characteristics?

To test potential moderators of the concentrated disadvantage-recidivism relationship, we turn to results from a series of meta-regressions that test whether concentrated disadvantage's effect is conditional on certain sample and study related factors. For sample-related characteristics, we tested the degree to which age group of the sample (juvenile vs. adult), gender composition of the sample (percent female), and racial composition of the sample (percent White) moderated the effect of concentrated disadvantage on recidivism. We find gender (i.e., percent female; log OR = -0.001, p = 0.68) and race (i.e., percent White; log OR = 0.0004, p = 0.48) show no signs of moderating the effect of concentrated disadvantage (k = 32).

Of sample-related characteristics, only age presents as a moderator of concentrated disadvantage's effect on recidivism. Entering age as a moderator improves model fit, when compared to the model without a moderator described in Section 4.2 (LRT = 4.21, p = 0.04). Concentrated disadvantage has a stronger effect among juveniles than adults (log OR = 0.08, p = 0.05, k = 32; see

Fig. 4).[vii] In stratified analyses, each standard deviation increase in concentrated disadvantage yields a 9% increase in the risk of recidivism for juveniles (log OR = 0.09, $p = 0.02$, $k = 14$), but shows no sign of increasing risk among adults (log OR = 0.01, $p = .78$, $k = 18$). There is some evidence of potential publication bias, with the rank correlation test for funnel plot asymmetry yielding significant results ($\tau = 0.42$, $p = 0.04$). However, a Rosenthal file drawer analysis suggests 1,625 studies with null results would need to be published to make the effect of concentrated disadvantage on juveniles non-significant, making publication bias of limited concern.

[Insert Fig. 4]

As for study features, we used three-level meta-regression models to test effect variation by geographic unit, race-inclusive measure of concentrated disadvantage (i.e., whether race was included in the measure), potential over-control (i.e., total number of control variables adjusted in analyses), and the definition of recidivism (i.e., evidence and offense type). Of these predictors, using a race inclusive measure of concentrated disadvantage (log OR = -0.01, $p = 0.62$) and potential over-control (log OR = 0.003, $p = 0.14$) were not statistically significant predictors of the variation between studies ($k = 32$). Geographic unit approached statistical significance – studies that measured concentrated disadvantage in small to moderate units of analysis (i.e., block group, tract, or zip code) had, on average, stronger associations between concentrated disadvantage and recidivism than large units of analysis (i.e., county, region), but this effect bordered on significance at the .05 level (log OR = 0.08, $p = 0.05$, $k = 32$).[viii]

One factor that varied between studies that *did* explain concentrated disadvantage's effect heterogeneity was recidivism's operationalization. To test the degree to which recidivism's operationalization alters the effect of concentrated disadvantage, we examined variation in concentrated disadvantage's effect by the type of evidence used to measure recidivism (i.e., arrest/revocation verses conviction/incarceration) and by offense type (i.e., drug, property, or violent crime). We used a three-level meta-regression model, which included evidence type as an effect moderator, to test our hypothesis that concentrated disadvantage is more strongly associated with recidivism when recidivism is based on arrest/revocation than when recidivism is based on conviction/incarceration.

We find evidence type moderates the relationship between concentrated disadvantage and recidivism; when evidence type is included in analyses as a moderator, model fit is improved (LRT = 10.37, $p < 0.001$). Concentrated disadvantage has a lower effect when recidivism is based on conviction/incarceration than when based on revocations/arrests (log OR = -0.09, $p < 0.001$, k = 32; see Fig. 5). Examining the pooled effect of concentrated disadvantage for studies with revocations and arrests as outcomes, each standard deviation increase in concentrated disadvantage is associated with a 8% increase in the risk of recidivism (log OR = 0.08, $p = 0.002$; k = 17). Meanwhile, there is no statistically significant association between concentrated disadvantage and recidivism in studies that base recidivism on conviction or incarceration (log OR = -0.01, $p = 0.55$; k = 15). We found no evidence of publication bias in these stratified analyses.

[Insert Figure 5]

Next, we limited our sample to studies that tested the effect of concentrated disadvantage across specific offense types. Here, we found that a two-level random effects model, with level-1 representing sample-related variance and level-2 representing study-related variance, was superior to the three-level model (LRT = 10.56, $p < 0.01$). We used the two-level model to test our hypothesis that offense type would moderate the effect of concentrated disadvantage on recidivism, with stronger effects for relatively low-level offenses (i.e., property and drug offenses).

We find offense type is a statistically significant moderator of the concentrated disadvantage-recidivism relationship, with the moderation model fitting the data relatively well (LRT = 10.56, p = < 0.01). However, counter to our hypothesis, concentrated disadvantage has a stronger risk enhancing effect on violent offending than relatively low-level offenses (where reference category is drug crime, log OR Violent = 0.12, $p < 0.01$, k = 24). Limiting analyses to those that measure recidivism as a violent offense and using a single-level random effects model (as there was no detectable study or source variance), we find each standard deviation increase in concentrated disadvantage is associated with a 9% increase in the risk of violent recidivism (log OR = 0.09, $p < 0.001$) and no evidence of publication bias. Together, these results indicate concentrated disadvantage has a relatively *weak* effect on recidivism when a data source relatively less susceptible to surveillance (i.e., conviction and incarceration) is used to measure recidivism, but concentrated disadvantage has a relatively *strong* effect on recidivism for offenses that are relatively less susceptible to surveillance effects (i.e., violent crimes).

4.3.1. Post-hoc analysis: The abovementioned seemingly contradictory finding may be due to the fact that operationalizing recidivism requires the combination of offense and evidence type. Thus, we visualize effects by evidence and offense type combined. Figures 6 illustrates clear variation by these combined recidivism types. Visual inspection of these effects suggests concentrated disadvantage has stronger effects on drug arrests and violent convictions, and weaker effects on property arrests, violent arrests, drug convictions, and property convictions. We explored the differences between these effects using a meta-regression model, entering the interaction between offense and evidence type as a moderator of the concentrated disadvantage-recidivism relationship. Since there was no estimated study or data source variance, we used a single-level model.

Results indicated that compared to the random effect meta-analysis model (with no moderator), the meta-regression model (including the interaction between offense and evidence type as a moderator) improved model fit (LRT = 32.82, $p < 0.01$). All offense-evidence categories (property arrests, violent arrests, drug convictions, and property convictions) except violent convictions, had statistically significant, smaller effects than drug arrests (the reference group). Calculating the pooled estimate across offense-evidence types, we found notably larger pooled estimates for concentrated disadvantage's effect on drug arrests (log OR = 0.17, $p < 0.01$) and violent reconvictions (log OR = 0.10, $p < 0.001$), than other offense-evidence types (log OR = -0.07 – 0.01; see Fig. 7 for a visualization of this interaction). These estimates are only exploratory and should be interpreted cautiously, as the analysis represents 24 effects but only 6 studies.

[Insert Figures 6 & 7]

4. DISCUSSION

The role of macro-level factors in shaping recidivism outcomes is no longer neglected. The macro-level factor of concentrated disadvantage best reflects this surge in interest, having been tested in dozens of studies in the past two decades. In this paper, we aggregated results from these studies to assess the overall strength of the concentrated disadvantage-recidivism relationship. We found that concentrated disadvantage did not have a statistically significant pooled effect on recidivism, after models adjust for other recidivism risk markers and factors. However, we also found that the relationship between concentrated disadvantage and recidivism depends on sample and study-related factors, specifically the age group sampled and how recidivism was operationalized; for juveniles and some types of recidivism, concentrated disadvantage does predict recidivism. In the remainder of this discussion, we review study findings and situate them within relevant theory and research. We also provide recommendations for future research and theory building as it relates to the concentrated disadvantage-recidivism relationship. We begin by discussing key limitations.

This study has three limitations worthy of note. First, because meta-analyses reflect the aggregation of previous studies, they also reflect the limitations of those studies. Here, studies included in analyses were longitudinal in nature and coefficients for concentrated disadvantage's effect on recidivism were adjusted by established risk factors for recidivism. In turn, studies were designed such that they could speak to concentrated disadvantage as a risk factor for recidivism, but not a causal risk factor (see Monahan & Skeem, 2013). The lack of experimental studies in this area precludes our ability to make causal assertions regarding the concentrated disadvantage-recidivism relationship. Second, some potential moderators of the concentrated disadvantage-recidivism relationship were not testable across all studies. In one case, this translated into relatively few studies contributing information for moderators (i.e., only two studies examined arrests across offense types), which weakens confidence in the stability of estimates. Third, each study included a unique set of control variables. It is possible then that differences in effects could be rooted in differential inclusion of control variables, though we took steps to mitigate and assess this potential. All studies shared an essential covariate profile (i.e., at a minimum, demographics and an indicator of criminal risk), which increased confidence we were comparing apples to apples in assessing coefficients. We also tested whether the number of control variables included in models moderated concentrated disadvantage's effect on recidivism, and found no evidence of such an effect.

With these limitations in mind, we turn to our findings. Our first aim was to answer the question: does concentrated disadvantage predict recidivism? We hypothesized that concentrated disadvantage would have a small but statistically significant effect on recidivism. We tested this hypothesis with a multi-level model that included random effects for sample, study, and data source related heterogeneity across 32 coefficients from 30 studies. We found the pooled effect of concentrated disadvantage on recidivism, after prior adjustment for covariates, was statistically non-significant.

The pooled effect of concentrated disadvantage on recidivism may be undetectable because concentrated disadvantage provides no predictive utility above and beyond other risk markers and factors for recidivism. It is also possible that concentrated disadvantage does have predictive utility, but that this utility is difficult to detect. Such analyses require variability in both predictor and outcome. Because justice-involved persons reflect a sub-sample of the population who disproportionately come from disadvantaged residential contexts, concentrated disadvantage's variance is likely constrained for this group. The restricted distribution of concentrated disadvantage in justice-involved samples may hinder the detection of effects, just as it weakens the effect of concentrated disadvantage on crime in Black neighborhoods (see McNulty, 2001). Still, our results indicate the effect of concentrated disadvantage on recidivism varied across studies. Thus, constrained variation does not unilaterally diminish effects.

Concentrated disadvantage's effect on recidivism may be significant under certain conditions. We assessed this possibility by answering our second research question: what, if any, sample or study-related factors explain effect heterogeneity across studies? To answer this question, we used a series of meta-regression models. Each model regressed the effect of concentrated disadvantage on potential sample (i.e., race, gender, and age composition) and study related moderators (i.e., concentrated disadvantage's operationalization, geographic unit, number of control variables, and recidivism's operationalization). Results indicated that the geographic unit of analysis bordered on significance with a p-value of 0.05. Thus, in line with prior research (Oberwittler & Wikström, 2009; Smith et al., 2000), relatively small geographic units (neighborhood, block group, tract, or zip code) may better capture concentrated disadvantage than larger units (county, region). We also found age group, evidence type, and offense type were statistically significant moderators of the concentrated disadvantage-recidivism relationship. We found no evidence that any other factor tested— sample race or gender composition, using a racialized measure of concentrated disadvantage, or the number of control variables— moderated the effect of concentrated disadvantage on recidivism.

Together, these results address several abovementioned challenges and provide four clarifications regarding the concentrated disadvantage-recidivism relationship. First, problems related to conceptual equivalence likely play a minimal role in shaping the concentrated-disadvantage relationship. Since concentrated disadvantage's operationalization is rooted in theorizing related to Black, urban communities worst hit by industrial decline (Wilson, 2012), we suggested that concentrated disadvantage may not be operationalized in a manner that is relevant across racial groups and geographies—i.e., concentrated disadvantage may have weak conceptual equivalence. This lack of conceptual equivalence could explain the overall weak effect of concentrated disadvantage on recidivism. Yet, we find neither the percent of the sample White nor a race-inclusive measure explain variation in concentrated disadvantage's effect on recidivism, suggesting that conceptual equivalence is not a primary concern in assessing the concentrated disadvantage-recidivism relationship.

Second, null effects may result from over-control (Sampson et al., 2002). Macro-level variables are typically weakly associated with measures of individual-level behavior. This is particularly true when variables are included in the model that may mediate the relationship between the macro variable and the individual behavior. All studies included in this meta-analysis included, at a minimum, adjustments for participant's demographic markers and criminal risk (e.g., criminal history as a proxy for risk or a risk assessment score). We found that variation in the number of control variables beyond these standard controls failed to moderate the relationship between concentrated disadvantage and recidivism. Still, even these standard controls, particularly race (which will likely vary between geographic units, unlike gender and age) and criminal risk (which may be a product of prior exposure to concentrated disadvantage), may amount to over-control.

Third, just as contextual effects are not always equally distributed among residents, concentrated disadvantage does not equally affect recidivism across all justice-involved groups. Here, we found concentrated disadvantage's effect on recidivism was statistically significant for juvenile but not adult samples. This suggests that concentrated disadvantage's effect on recidivism attenuates with age. Such attenuation could be due to a unique vulnerability to disadvantage for youth, constrained mobility of youth that increases exposure to conditions in their neighborhoods (i.e., the uncertain geographic context problem; Kwan, 2012), or greater heterogeneity in neighborhoods and recidivism among youth.

Fourth, surveillance is a factor that likely contributes to concentrated disadvantage's effect heterogeneity. Surveillance varies between neighborhoods depending on their level of disadvantage—with greater surveillance in disadvantaged contexts, where police contacts are especially elevated for sub-criminal activity (Fagan & Davies, 2000). In turn, we suggested concentrated disadvantage's effects would be greatest when measures of recidivism were prone to capturing surveillance and thus likely to capture the effect of disadvantage *and* surveillance. In support of this hypothesis, we found evidence type (arrest/revocation and conviction/incarceration) was a statistically significant moderator of the concentrated disadvantage-recidivism relationship; concentrated disadvantage had a statistically significant effect on arrests and revocations, but not convictions and incarcerations. However, we also found concentrated disadvantage had a statistically significant effect on violent crime, but not more surveillance-prone, drug offenses. Post-hoc analyses, which investigated evidence and offense types in tandem, shed some light on this counterintuitive finding and indicated that concentrated disadvantage actually had the greatest association with drug *arrests*. Though we could only test the evidence-offense type interaction among a handful of studies, relatively strong effects for surveillance-prone drug arrests provides tentative support for the role of surveillance in shaping the concentrated disadvantage-recidivism relationship.

Together, these findings have three important implications. Future research should (1) go beyond assessing the main effect of concentrated disadvantage on recidivism, net standard individual-level risk markers and factors, to identify the mechanisms through which concentrated disadvantage effects recidivism and moderators of these relationships; (2) explain the differential effect of concentrated disadvantage on juvenile samples by assessing whether an age-related contextual sensitivity exists, the effect for adults is weakened by the uncertain geographic context problem, or greater heterogeneity in disadvantage and/or recidivism exists in youth samples; and (3) investigate the connections between disadvantage, surveillance, and recidivism, while examining the potential relevance of conflict theories of social control alongside theories of social capital and deprivation in making sense of these relationships.

4.5. Conclusion

Existing research provides little evidence that concentrated disadvantage is a risk factor for recidivism across groups of formerly incarcerated persons and types of reoffending, after accounting for individual-level risk markers and factors. At face value, the overall non-significant effect of concentrated disadvantage on recidivism tempts the dismissal of concentrated disadvantage as a meaningful point of intervention for recidivism reduction. However, dismissing concentrated disadvantage in this manner ignores the fact that concentrated disadvantage is, in fact, a significant risk factor for some offenders (i.e., juveniles) and some forms of recidivism (e.g., drug arrests). Dismissing concentrated disadvantage also risks missing the forest for the sake of the trees. Though concentrated disadvantage may not consistently provide predictive utility above established recidivism risk factors, it would be foolish to argue that concentrated disadvantage is summarily irrelevant to recidivism when disadvantaged neighborhoods are so overrepresented among justice involved persons and justice involved persons are so overrepresented in disadvantaged neighborhoods. What these non-significant effects do promote is future theorizing and research that unpacks how disadvantage unfolds to affect specific groups of justice-involved persons and types of reoffending, and future studies that test interventions that target such mechanisms.

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*Wright, K. A., Turanovic, J. J., & Rodriguez, N. (2016). Racial inequality, ethnic inequality, and the system involvement of at-risk youth: Implications for the racial invariance and Latino paradox theses. *Justice Quarterly*, *33*(5), 863–889.

[i] Where estimates were provided in their exponentiated form (i.e., odds ratios) we converted them to the log scale.
[ii] Because all studies provided the percent White for their sample but some studies lumped all ethnoracial groups other than White into one category, it was not possible to extract data related to sample ethnoracial composition in a reliable manner using any indicator other than the percent White.
[iii] Some studies calculated concentrated disadvantage as a race-specific measure. In such cases, dimensions of concentrated disadvantage were calculated for White and Black residents, participants were then coded with the measure concordant to their race (see Mears et al., 2014; Reisig et al., 2007; Wang et al., 2010).
[iv] We could have also chosen to classify zip code along with units we classify as large geographic units (i.e., county- and regional-level units), given some consensus regarding the relative superiority of neighborhood-, block group-, and tract-level units over zip code-, county-, and regional-level units. However, we chose to group zip code along with small units because zip codes (with a mean of about 7,500 people) are typically much closer in population size to those we classify as small (e.g., tracts have a mean of about 5,300 people) than those we classify as large (e.g., counties have a mean of about 100,000 people; U.S. Census, 2010). Sensitivity analyses provide some support for this decision (see endnote ix).
[v] We handled the reporting of multiple coefficients as follows: In studies that reported an overall result for the entire sample, as well as results pertaining to sub-samples, we include the coefficient from the overall result in our meta-analysis and, where relevant, include sub-sample results in meta-regressions (e.g., race-specific analyses; see below). In cases where effects were from different samples (e.g., analyses stratified by race), each individual effect was included in the meta-analysis. Where studies did not report a parameter estimate for a holistic measure of recidivism and instead reported multiple effects for the same sample across different outcome measures (e.g., violent-, property-, or drug-related recidivism), we calculated a synthetic, combined effect and adjusted variance estimate across these dependent analyses (for a detailed discussion and specific equations, see Borenstein et al., 2009).
[vi] We noted larger studies tended to produce smaller effect sizes (log OR for log sample size = -0.03, p =0.02), evidencing potential publication bias, i.e., the tendency for large studies and small studies with significant findings to be published, while small studies with non-significant findings unlikely to be published. Specifically, since every magnitude of ten increase in sample size equates to a 2.3 change in the log sample size (log[1000] – log[100] = 2.30), going from a sample size of 10 to 100 results in an additive effect on the pooled log odds ratio of -0.07 (-0.03*2.30 = -0.07). Given the non-significant pooled effect, however, concern regarding publication bias, which would result in a positive distortion in pooled effect estimates, is somewhat moot.
[vii] Looking across all studies, age failed to reach statistical significance at the 0.05 level (log(OR)= 0.08, p = 0.05). However, inspection of Cook's distance and residual heterogeneity indicated the coefficient from McReynolds (2004) was an outlier. Upon removal of these outliers, juvenile status became a statistically significant predictor of the relationship between concentrated disadvantage and recidivism (log(OR) = 0.10, p = 0.02). Thus, we provide the more conservative coefficient, but interpret the effect as significant.
[viii] We conducted sensitivity analyses, recoding geographic unit such that smaller units reflected Block group and tract and larger units reflect zip code, county, and region, produced no evidence of effect moderation and non-significant results.

TABLE 1. Quality Assessment

Study Authors	1. Correlate Score (0 – 5) 1a	1b	1c	1d	1e	2. Design Adequacy for Spatial Data (0 -3)	3. Design Adequacy for Establishing Risk Factors (0 – 3)	4. Design Adequacy for Establish Causal Risk Factors (1 – 7)	Total
Baglivio et al. (2017)	1	1	1	0	0	1	2	5	11
Burden (2009)	1	1	0	0	0	2	2	5	11
Chamberlain (2016)	1	1	1	0	0	2	3	5	13
Clark (2016)	1	0	0	0	1	1	3	5	11
Craig (2019)	1	1	1	1	0	1	2	5	12
Craig et al. (2017)	1	1	1	1	0	1	2	5	12
Grunwald et al. (2010)	1	1	0	1	0	1	2	5	11
Headley (2017)	1	0	1	1	0	1	2	5	11
Huebner, et al. (2010)	1	1	0	0	0	0	2	5	9
Intravia et al. (2017)	0	0	1	1	0	1	2	5	10
Jeong (2011)	1	1	0	0	0	1	3	2	8
Kubrin et al (2007)	1	1	1	0	0	1	2	5	11
Kubrin et al. (2006)	1	1	1	0	0	2	2	5	12
LeBaron (2002)	0	1	0	0	0	1	2	5	9
Lockwood et al. (2015)	1	1	1	1	0	1	3	5	13
McNeeley (2018)	1	0	1	1	0	1	2	5	11
McReynolds (2004)	1	1	1	0	0	1	2	5	11
Mears et al. (2008)	1	1	1	0	0	2	2	5	12
Mears et al. (2014)	0	1	0	0	0	2	2	5	10
Orrick et al. (2011)	1	1	1	0	0	2	2	5	12
Reisig et al. (2007)	1	1	1	0	0	1	3	5	12
Tillyer et al. (2011)	1	1	1	0	0	1	2	5	11
Wang (2010)	1	1	1	0	0	2	2	5	12
Wang et al. (2014)	1	1	1	0	0	2	2	5	12
Wehrman (2010)	1	1	1	0	0	0	2	5	10
Wolff et al. (2015)	1	1	1	1	0	1	2	5	12
Wolff et al. (2016)	1	0	1	0	0	1	2	5	10
Wolff et al. (2017)	1	0	1	1	0	1	2	5	11
Wright (2014)	1	1	1	1	1	1	3	5	14
Wright (2016)	1	0	1	1	0	2	2	5	12

Notes: See author for a full description of quality assessment indicators and procedures.

TABLE 2. Included studies

Study	N	% White	% Female	Recidivism measure	Geography	Race-inclusive CD Measure	Number of control variables	Age group	Tested effects by multiple offense types?	Tested effects by race/ethnicity?
Baglivio et al (2017)	12302	0.33	0.14	A/R	Small	No	8	Juvenile	No	No
Burden (2009)	18013	0.31	0.11	C/I	Large	Yes	10	Adult	No	No
Chamberlain et al (2016)	31191	0.28	0.08	C/I	Small	No	17	Adult	No	No
Clark (2016)	4357	0.53	0.11	A/R	Small	Yes	24	Adult	No	No
Craig et al. (2017)	25461	0.39	0.23	A/R	Small	No	7	Juvenile	No	No
Craig et al. (2019)	25461	0.38	0.23	A/R	Small	No	14	Juvenile	No	No
Grunwald et al (2010)	7061	0.11	0.00	A/R	Small	No	14	Juvenile	Yes	No
Headley (2017)	3077	0.84	0.06	C/I	Small	No	8	Adult	No	No
Huebner et al (2010)	519	0.64	1.00	C/I	Small	Yes	17	Adult	No	No
Intravia et al (2017)	24791	0.35	0.23	A/R	Small	Yes	17	Juvenile	No	No
Jeong (2011)	782	0.4	0.38	A/R	Small	No	27	Juvenile	No	No
Kubrin & Stewart (2006)	4630	0.68	0.25	A/R	Small	No	14	Adult	No	No
Kubrin et al (2007)	4630	0.68	0.25	A/R	Small	No	14	Adult	No	No
LeBaron (2002)	323	NA	0.00	A/R	Small	No	5	Juvenile	No	No
Lockwood & Harris (2015)	5517	0.12	0.00	A/R	Small	No	15	Juvenile	Yes	No
McNeeley (2018)	3923	0.52	1.00	A/R	Small	Yes	21	Adult	No	No
McReynolds (2004)	914	0.19	0.20	A/R	Small	No	8	Juvenile	No	No
Mears et al (2008)	49420	0.36	0.00	C/I	Large	No	6	Adult	Yes	No
Mears et al (2014)[W]	374	1.00	0.00	C/I	Large	No	11	Adult	Yes	Yes
Mears et al (2014)[B]	773	0.00	0.00	C/I	Large	No	11	Adult	Yes	Yes
Orrick et al (2011)	49420	0.37	0.00	C/I	Large	No	12	Adult	Yes	No
Resig et al (2007)	34868	0.38	0.00	C/I	Large	No	10	Adult	No	No
Tillyer & Vose (2011)	5027	0.86	0.19	C/I	Large	No	7	Adult	No	No
Wang et al (2010)[W]	8648	1.00	0.00	C/I	Large	Yes	10	Adult	Yes	Yes
Wang et al (2010)[B]	13272	0.00	0.00	C/I	Large	Yes	10	Adult	Yes	Yes
Wang et al (2014)	54359	0.37	0.09	C/I	Large	No	14	Adult	No	No

Wehrman (2008)	1548	0.30	0.14	C/I	Small	No	8	Adult	No	No
Wolff et al (2015)	105573	0.42	0.31	C/I	Small	No	17	Juvenile	No	No
Wolff et al (2016)	26960	0.38	0.23	A/R	Small	No	18	Juvenile	No	No
Wolff et al (2017)	13096	0.42	0.32	A/R	Small	No	25	Juvenile	No	No
Wright & Rodriguez (2014)	12660	1.00	0.39	A/R	Small	No	19	Juvenile	No	No
Wright et al (2016)	13138	0.42	0.38	A/R	Small	No	16	Juvenile	No	No

Notes: A/R = Arrest or revocation; C/I = Conviction or incarceration; Where [B] and [W] are indicated, the study performed analyses separately for [B] Black samples and [W] White samples

Figure 1. PRISMA Flow Chart

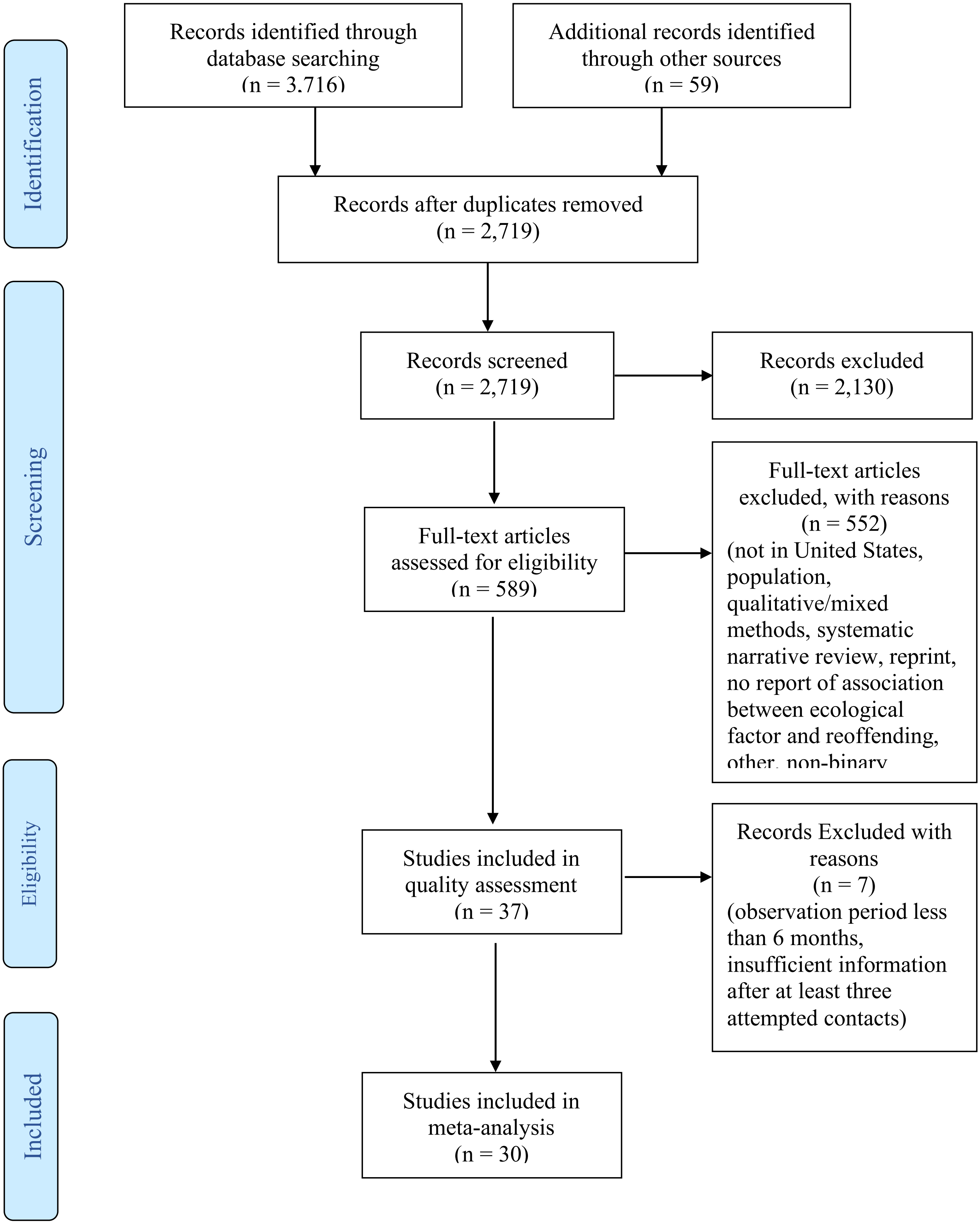
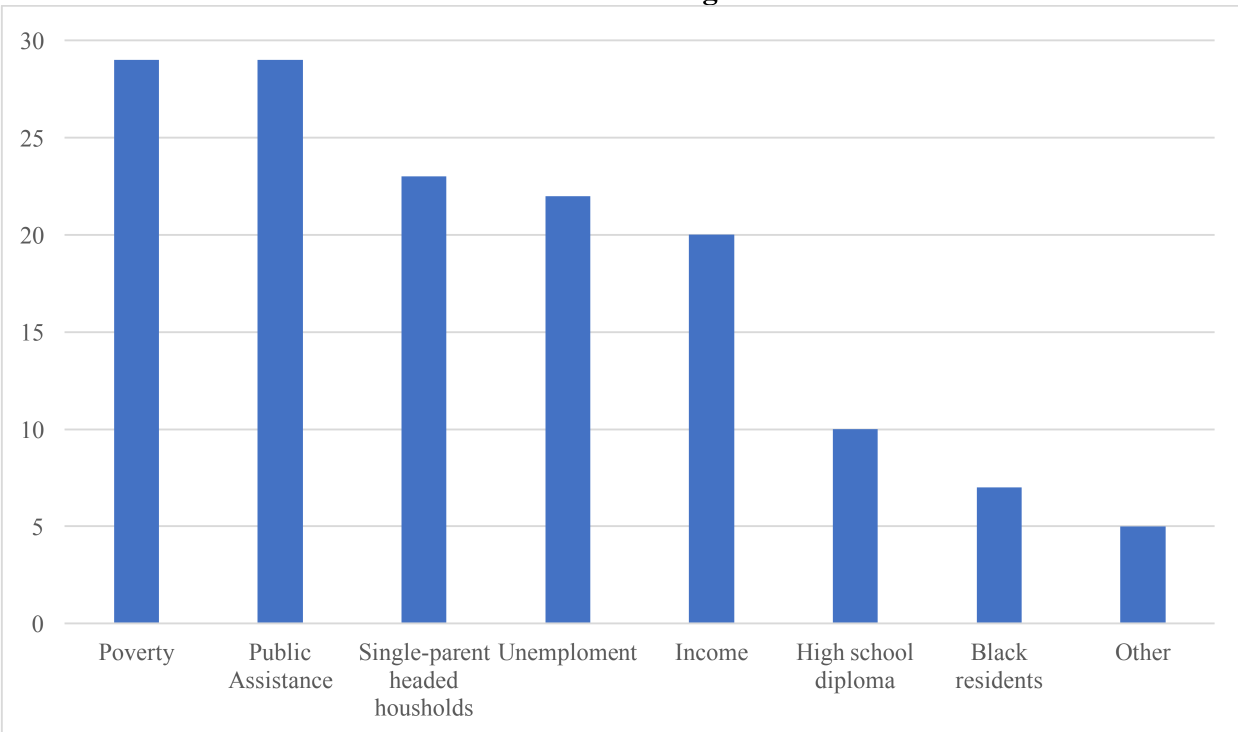
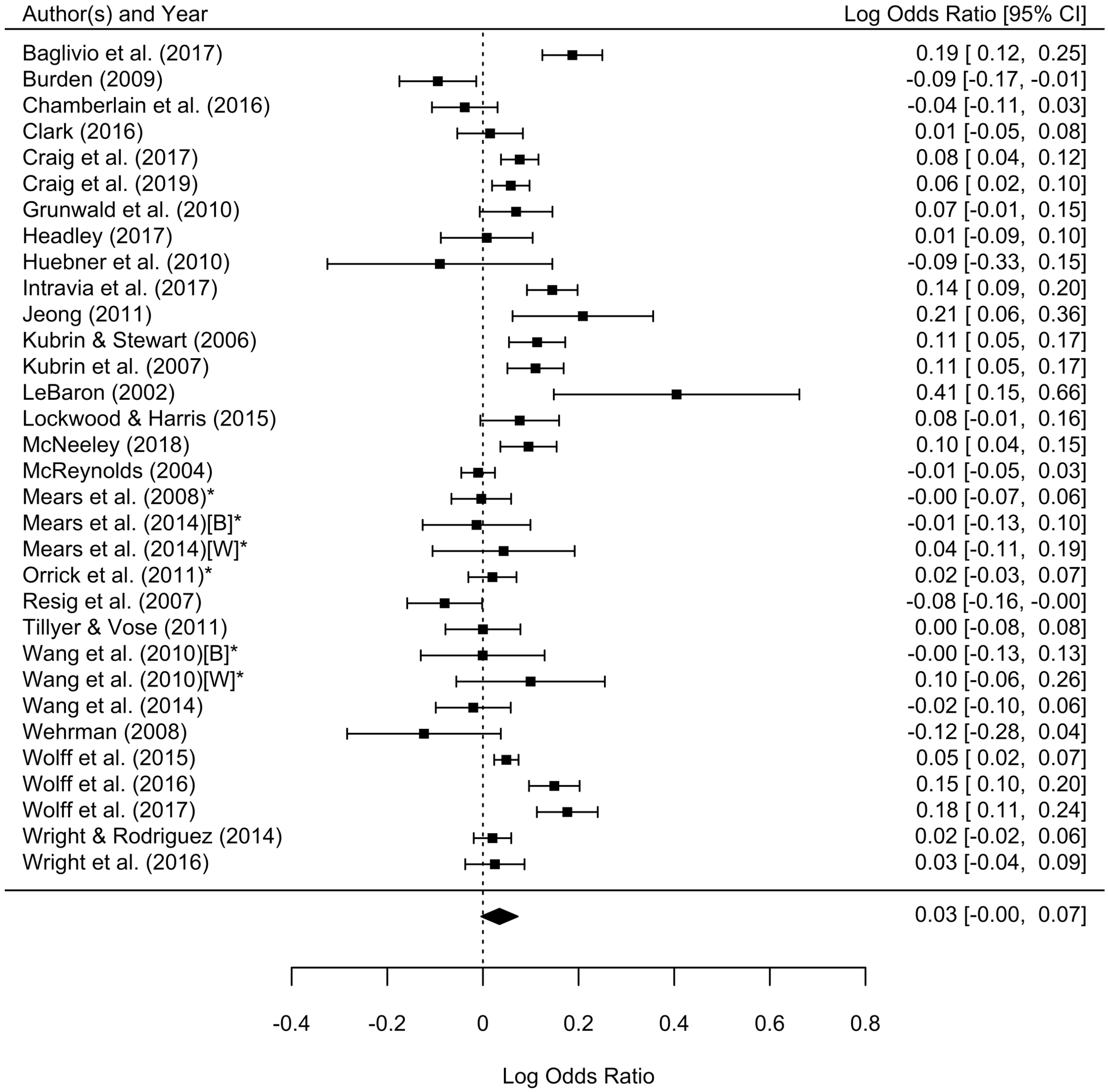


FIGURE 2. Dimensions of concentrated disadvantage measures



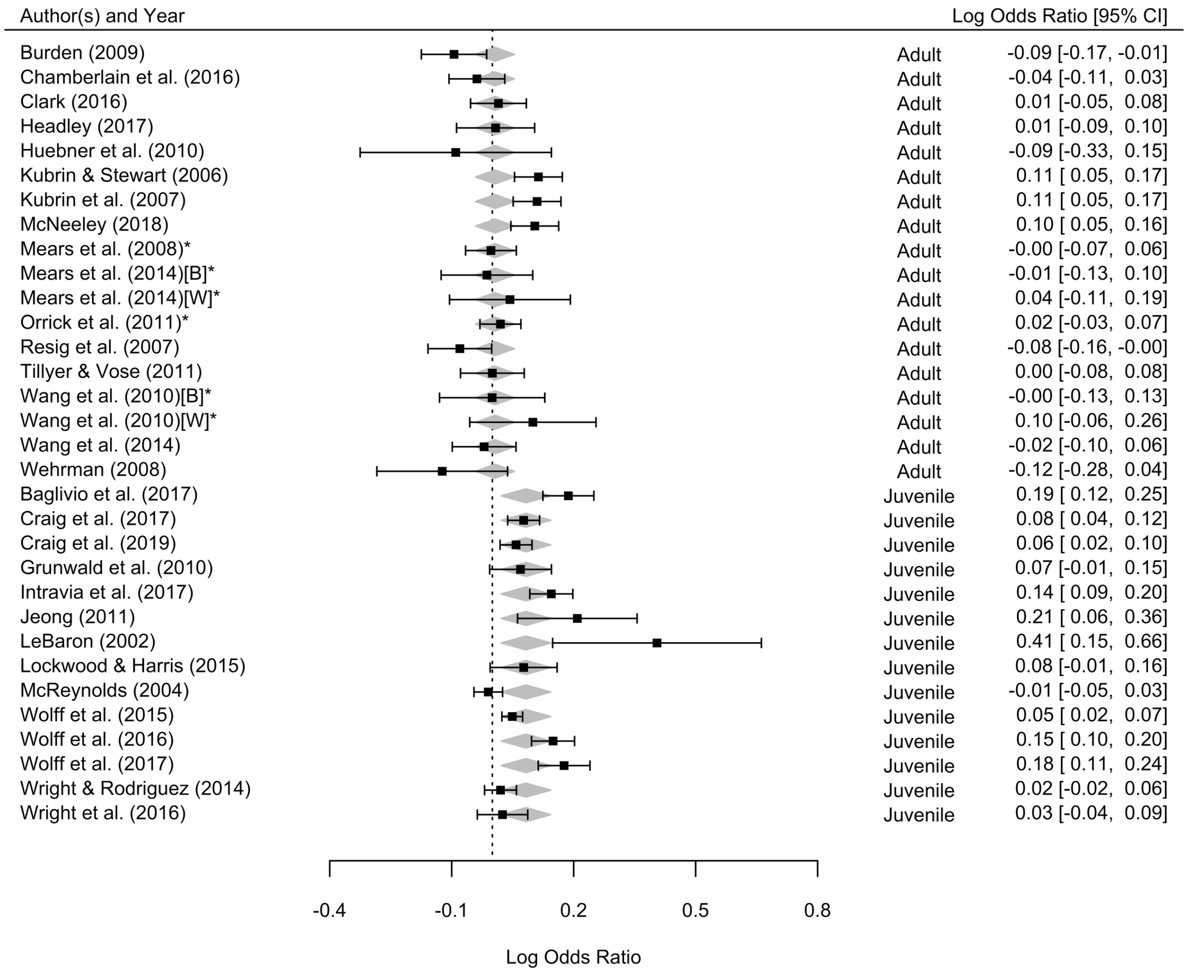
Note: Each construct represents a dimension included in the construction of concentrated disadvantage; frequency represents a count of studies.

FIGURE 3. Forest plot for effect of concentrated disadvantage on recidivism across all studies



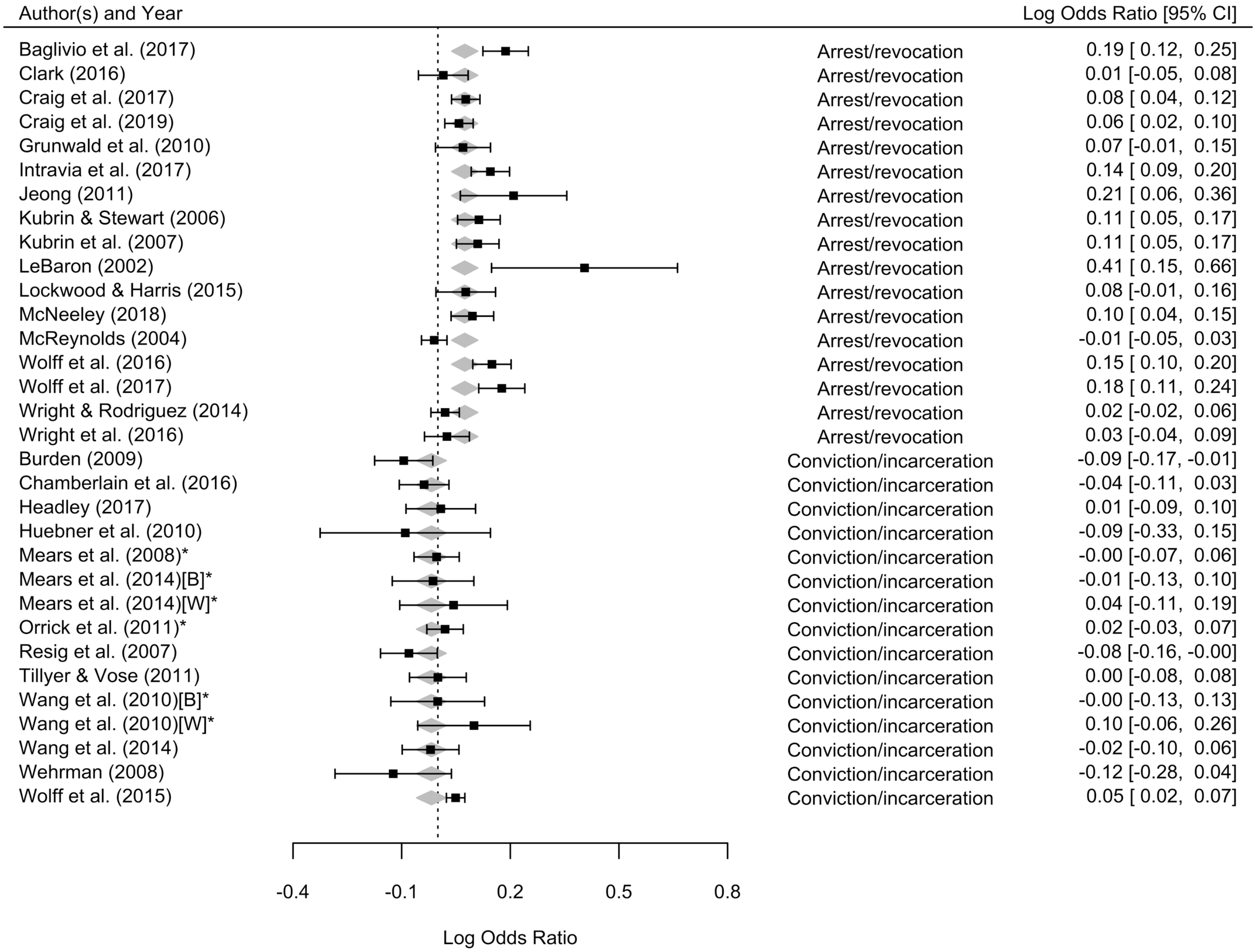
Notes: Coefficients presented from a three-level random effects model testing the pooled effect of concentrated disadvantage on recidivism across k = 32 analyses. Whiskers represent 95% confidence intervals. In analyses from the same study where two separate samples are represented, [B] = Black race and [W] = White race. Asterisks denote calculation of a synthetic beta (see Methods).

FIGURE 4. Forest plot for meta-regression test of age group as a moderator of the concentrated disadvantage-recidivism relationship



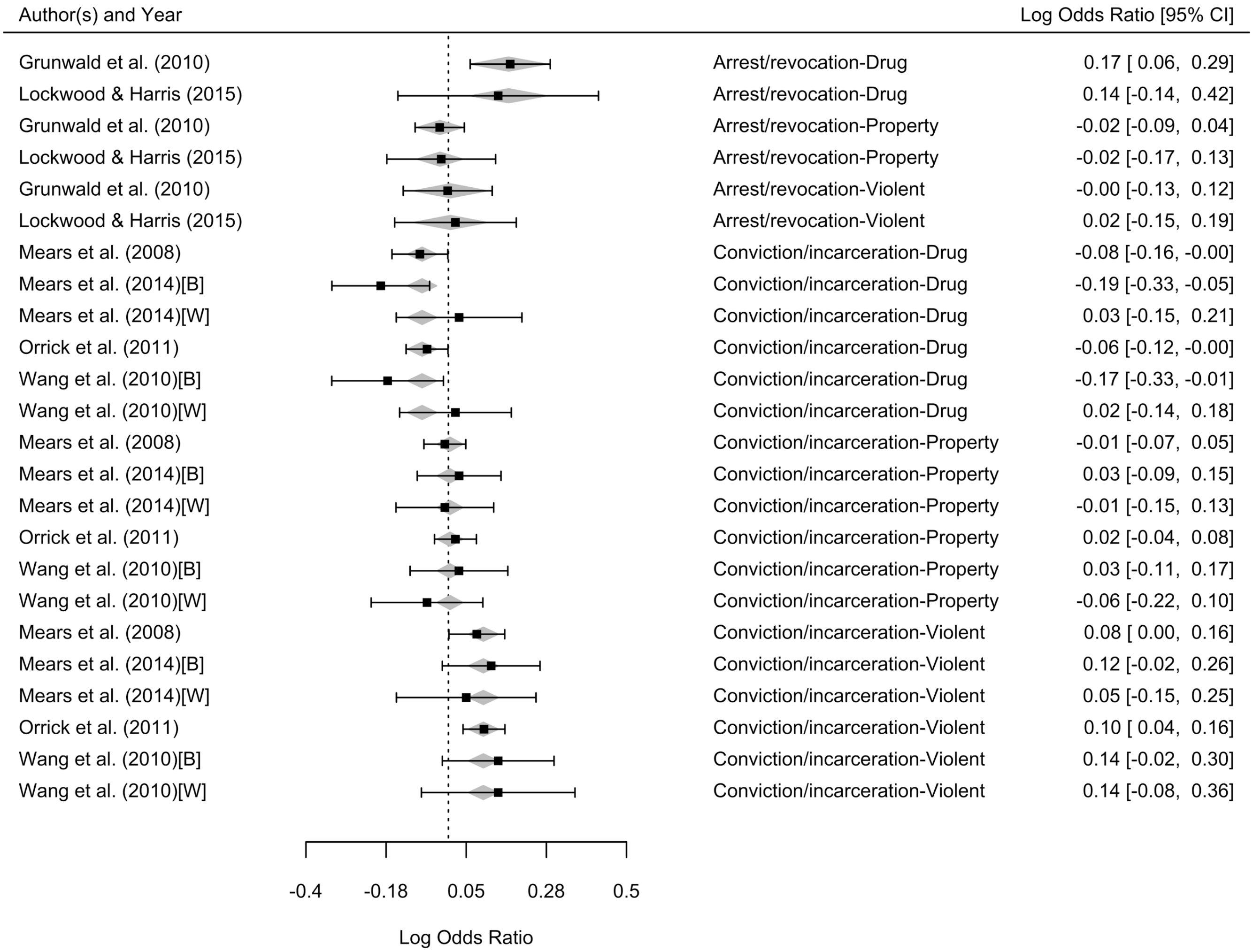
Notes: Coefficients presented from a three-level random effects model with age group entered as a moderator of the effect of concentrated disadvantage on recidivism (k = 32). Gray diamonds present the estimated pooled effect for adults and for juveniles. Whiskers represent 95% confidence intervals. In analyses from the same study where two separate samples are represented, [B] = Black race and [W] = White race. Asterisks denote calculation of a synthetic beta (see Methods).

FIGURE 5. Forest plot for a meta-regression test of evidence type as a moderator of the concentrated disadvantage-recidivism relationship



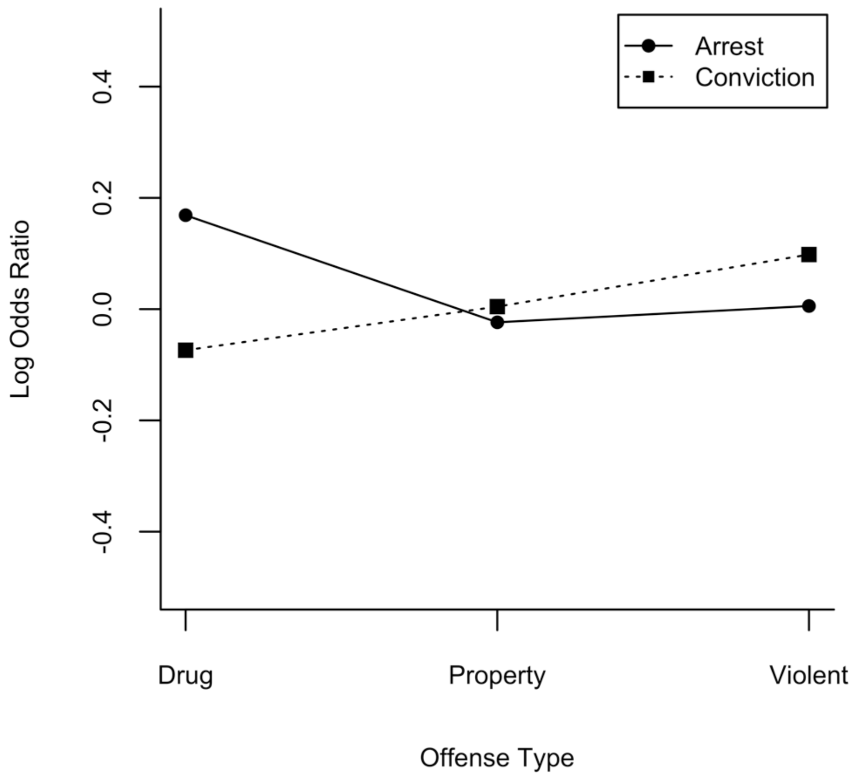
Notes: Coefficients presented from a three-level random effects model with evidence type entered as a moderator of the effect of concentrated disadvantage on recidivism (k = 32). Gray diamonds present the estimated pooled effect for Arrest/reconviction and for Conviction/incarceration. Whiskers represent 95% confidence intervals. In analyses from the same study where two separate samples are represented, [B] = Black race and [W] = White race. Asterisks denote calculation of a synthetic beta (see Methods).

FIGURE 6. Forest plot for meta-regression test of combined evidence-crime type as a moderator of the concentrated disadvantage-recidivism relationship



Notes: Coefficients presented from a single-level random effects model with evidence-crime type entered as a moderator of the effect of concentrated disadvantage on recidivism (k = 32). Gray diamonds present the estimated pooled effect for each evidence-crime type combination. Whiskers represent 95% confidence intervals. In analyses from the same study where two separate samples are represented, [B] = Black race and [W] = White race.

FIGURE 7. Pooled log odds ratios for concentrated disadvantage's effect on recidivism by recidivism and conviction type



Notes: Coefficients reflect those described and presented in Figure 6.