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Beyond Ghetto Schooling: The Problem of Concentrated Advantage in a New Understanding of Educational Inequality[1]

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Abstract:
This study seeks to discern from where in the uneven distribution of neighborhood resources social class provides its most significant contribution to educational inequality. Using hierarchical linear modeling the author synthesizes 81 estimates to ascertain whether the strength of the relationship between a neighborhood's socioeconomic status and education differs according to a study's consideration of the percentage of higher or lower income residents. The conditional analysis reveals no significant effects of the percentage of low income neighbors on education outcomes after controlling for differences in the model specifications within the primary studies. The effects of concentrated advantage are significant and persist after considering aspects of study quality and a sub sample of estimates from models that consider both measures of neighborhood socioeconomic status. The multivariate analysis estimates the effect-sizes for each educational indicator and finds no significant effects for low socioeconomic status while finding a stronger association for the percentage of higher income neighbors. The paper concludes with a discussion of educational inequality and its primary determinant, concentrated advantage.

KEYWORDS: Neighborhoods, Education, Inequality, Meta-Analysis

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INTRODUCTION

An understanding of the relationship between context and unequal academic outcomes requires us to acknowledge that vital resources are unevenly distributed among neighborhoods and schools, and to discern from where in the uneven distribution they provide their greatest contribution to educational inequality. Such a determination is important given the recent growth in the economic segregation of neighborhoods and schools (Massey and Fischer 2003), and that trend's possible relationship with the uncertain academic success of less advantaged children in urban environments (Noguera 2003; O'Connor 2000). These concerns encourage us to reflect on a growing body of research in "neighborhood effects" that seeks knowledge about the nature of the contextual determinants of inequality and attempts to draw conclusions about the corresponding behavior of individuals. Before turning my attention to the synthesis of this literature I explore several theoretical models to identify and describe the relationship between lower and higher levels of contextual resources, educational performance and inequality.

REVIEW OF LITERATURE

The Ghetto Effect

The idea that individual and contextual level poverty depresses the educational performance of youth and exacerbates educational inequality has motivated research efforts for decades. Over the years several models have emerged that attempt to identify and explain how the behaviors of lower income individuals undermine their educational development and that of their co-located peers. Decades ago Lewis (1969) argued low income individuals within areas of less social, political and economic organization might develop a culture of poverty in which their norms and behaviors become inconsistent with social mobility. Once developed such behaviors are intergenerationally transmitted from parents to children and are less affected by improvements to the structure of opportunity.

Other perspectives argue the effects of the economic structure are to blame, alluding to the cultural consequences of urban joblessness brought on by demand side changes in the labor market. The lower academic performance of urban youth is thought to originate in how families adapted culturally to limited opportunity, isolation and concentrated poverty (MacLeod 1987; Wilson 1987, 1996; Jargowsky 1997) rather than from an intergenerational transmission of subculture. The resulting "ghetto related"[2] behaviors appear very similar to those described in the culture of poverty model, the only difference being in the structural-cultural model low income behaviors are supposedly amenable to greater opportunity within the economic structure.

Ghetto behavior not only poses consequences for the individual but also for others in the vicinity that may be influenced by such behavior. For instance role modeling and mentoring relationships may explain the low aspirations of youth within areas of high unemployment as a consequence of their frequent observation of and interaction with unemployed adults (Wilson 1996). A similar contagion model, the peer epidemic model is thought to lead to higher rates early school departure (Crane 1991). After finding larger increases in dropout rates as the percentage of higher status residents drops below 5 percent, Crane (1991) concludes that ghettos experience epidemics of social problems that are spread through peer effects much like "infectious diseases" (p. 1227). Contagion models such as these are used to illustrate how prevalent joblessness and family level poverty within an area could cause other troublesome social indicators to occur at a higher rate than what can be accounted for by considering individual level characteristics (Jencks and Mayer 1990).

[2] Here I note Wilson's distinction between "ghetto related" and "ghetto specific" behaviors in that the behavior does not have to be observed "in" the ghetto in order for its quality to be "of" the ghetto. We may consequently observe ghetto behavior in varying degrees across neighborhoods that differ in their socioeconomic makeup.

These perspectives suggest that the relationship of concentrated poverty in neighborhoods and schools to limited educational success is strong primarily because the ghetto behaviors it inspires are simply not the ones that lead to learning or are rewarded by schools with academic credentials. Hence eliminating educational inequality ultimately requires a change in the behaviors of the poor and their concentration in neighborhoods and schools.

Alternative Hypotheses: The Importance of Individuals and Institutions

There are perspectives within research that are more suspicious about the link between the behaviors of the disadvantaged within low income environments and educational inequality. Liebow's (1967) early challenge to the culture of poverty thesis provided a different explanation that relies more on individual experience than contagion theory or intergenerational transmissions of culture. Each of Liebow's study participants went out and individually experienced failure within a structure of limited opportunity; this failure being followed by a culture of coping and rationalization rather than preceded by a culture of poverty or ghetto related culture. A parallel process may also exist in education as children, being less prepared but nonetheless eager to learn, enter school, experience educational difficulty, and in response develop limited aspirations and other unsuccessful dispositions toward education (see Clark 1965:132-133). In this explanation, the impact of the prevalence of other lower performing children in the educational setting is considered secondary to their personal experiences. If these contentions are true, a relationship between neighborhood poverty and educational performance should be trivial after considering individual level attributes.

Research has also put forth institutional models that argue the actions of some schools unfortunately resemble ghetto behavior more than that of the low income students they serve.

Urban schools in particular have been characterized as having low expectations for students, a low regard for low income parents and a professional culture inconsistent with normative conceptions of schooling (Clark 1965; Anyon 1997; O'Connor 2000; Noguera 2003). Often the identification of these dysfunctional school qualities are coupled with a claim that the same transformation of the urban economic landscape aiding the spread of urban vice also has severely constrained the ability of schools to achieve meaningful reform and meet the needs of their growing low income student population (Kantor and Brenzel 1992; Anyon 1997, 2005; Orr 1999; Tobier 2000). The importance of institutions as catalysts of inequality is not an easy one to understand however. The question lingers about the degree in which the local populating and financing of schools and the endogenous staffing decisions render an institutional effect a mere proxy for a neighborhood's. In the end, poorly functioning schools might influence learning so much that neighborhood studies will find a negative relationship between low income populations and achievement even in the presence of positive dispositions toward learning among students.

In addition to alternative explanations that highlight the importance of the economic structure's impact on institutions and individual experiences, others question the magnitude or perhaps the existence of geographically specific macroeconomic influences. The selection model for instance highlights the inability of other perspectives to ensure the concentration of low income families in neighborhoods and schools *causes* a higher prevalence of educational failure (Manski 1993, Duncan and Raudenbush 2001; Durlauf 2001). Instead higher rates of educational failure may result as a greater number of affordable rents within some neighborhoods gather individuals who, due to intrinsic or family factors, are predisposed to lower incomes and performance in school. A corollary concern is that individuals might exhibit the same behavior no matter where they reside or are educated. This selection model acknowledges that individuals have exogenous qualities that are less dependent on the neighborhood structure and the behavior of others. In this scenario few social class effects would exist after considering the socioeconomic status of the individual or family.

A New Hypothesis: The Effects of Concentrated Advantage

During the time when public interest was directed toward the striking growth in concentrated poverty, those rates were being exceeded by growth in the neighborhood and regional concentration of higher income populations (Benabou 1996; Massey 1996; Massey and Fischer 2003). Only recently has attention been given to the likelihood that educational inequality is fueled as much or perhaps more by the assistance concentrated advantage provides higher income populations in securing education outcomes greater than what can be purchased by their individual incomes. This possibility is depicted in many of the same or slightly modified models used to describe poverty effects. For instance, whereas Lewis proposed a culture of poverty as a means of intergenerationally transmitting low achievement, Lundberg and Startz (2000) suggest social capital is a means of transmitting human capital from adults to youth and ultimately leads to the intergenerational reproduction of the average educational level of the neighborhood. The authors imply that the influence of social capital in neighborhoods should be noticeable after considering individual and institutional factors.

Likewise, the institutional model can be employed to demonstrate how schools benefit advantaged populations as much as they under-serve low income populations. Higher per pupil expenditures (Anyon 2005; Carey 2004; Liu 2006; Bifulco 2005) more highly trained teachers (Condition of Education 2004) and a greater number and variety of accelerated placement courses (Condition of Education 2005) are but a few of the benefits available to individuals that are educated within a high income area. Even within high income districts, there is evidence that schools with larger proportions of advantaged students receive higher quality resources (Brantlinger 2003; Liu 2006). Subsequently schools may enhance further the relationship of proximity capital to education outcomes.

Applying the Evidence

Despite the extensive exploration of poverty effects, the research findings often produce evidence that alludes to an alternative explanation of achievement disparities. For example, during the time in which the level of concentrated poverty within urban neighborhoods doubled, low income populations and African Americans in particular experienced the greatest cognitive gains relative to advantaged populations in the history of the National Assessment of Educational Progress (Jencks 1991; Jencks and Phillips 1998). Modest decreases in concentrated neighborhood poverty since the 1990s have ironically coincided with a slower to barely detectable rate of test-score convergence (Lee, Grigg, and Dion 2007). Just as hard to explain is the lack of relative progress for higher income students who theoretically should have benefited from the greater concentration of higher incomes in their communities coinciding with the concentration of poverty in others. These trends imply that either schools or the varied social classes they serve are somewhat resilient to fluctuations in concentrated socioeconomic status. They also imply that research may find a positive relationship between contextual poverty and education outcomes within neighborhoods that have effective schools.

In addition, a review of neighborhood effects research in education often fails to find strong relationships between poverty and education outcomes. For example, an analysis similar to Crane's (1991) failed to support his findings (Clark 1992). Clark's analysis of data from the largest SMSAs revealed no non-linear increases in dropping out as the percentage of high status residents dropped to extremely low levels for any ethnic/racial subgroup. The inconsistency of study findings exist across studies that examine the relation of neighborhood low socioeconomic status (SES) with early school departure (Ensminger et al.1996; Harding 2003), attainment (Duncan 1994; Rivkin 1994) and test-scores (Chase et al. 1997; Halpern-Felsher et al. 1997; Pebley et al. 2003).

While there is some agreement that the effect-sizes of concentrated economic advantage are among the larger effect-sizes (Massey 2001, Levanthal and Brooks Gunn 2000, Johnson 2003), there is still a great deal of inconsistency among the effects. While some studies report significant high SES effects (Halpern-Felsher et al. 1997; Chase-Lansdale 1997; Ainsworth 2002) others do not (Ensminger et al.1996; Foster and McLanahan 1996; Lopez-Turley 2002) while a few studies report a negative impact on the likelihood African Americans will graduate from high school. The latter finding implies neighborhood affluence may have varying effects for subgroups, including detrimental ones (Ginther, Haveman, and Wolfe 2000; Johnson 2005).

Understanding the Relative Effects of High and Low Socioeconomic Status to Inequality

The diversity of findings complicates any conclusion we would like to draw from research about the relative strength and contributions of high and low levels of contextual resources to educational inequality. That being said, one cannot ignore the fact that high income effects occupy a normative space in our thinking, so much so that we rarely think of contagious "good" behavior or liken a high prevalence of positive social outcomes to an epidemic spread like "infectious disease." Less frequently we are led to believe the greater achievement of higher income groups is in part a product of their economic segregation. A casual review of the literature supports this normative perspective with a healthy focus on poverty effects that would have the reader believe the primary contributor to educational inequality is concentrated disadvantage when a systematic review of this research may reveal a different conclusion.

The joint consideration of high and low socioeconomic contextual effects is not entirely common practice in research leading to additional questions. Recall Crane's finding of epidemics of dropping out among the poor for instance. His explanation for the prevalence of early school departure among the poor used the percentage of higher income residents as the measure of neighborhood socioeconomic status, not the percentage of low income neighbors, leaving open the question of whether the nonlinear association with problematic behavior that emerges when extremely low levels of high income residents are reached would have resulted if the percentage of low income neighbors was used instead. In this case, the connection of neighborhood variation in socioeconomic status to the social processes responsible for the outcomes remains tenuous since it is not known whether the nonlinearity results from the reduction of contagious successful behavior associated with the economically better off or the "infectious" spread of problematic behavior among the low income. Moreover, including a measure of neighborhood poverty within the modeling of high SES effects might have produced very different results and strengthened the study's construct validity.

These issues support my interest in several questions about neighborhood composition studies; primary among them asks which type of socioeconomic concentration has the strongest significant impact on education outcomes. Answers to this question can take several hypothetical forms as it concerns educational inequality. In the first hypothesis, there is no difference in the high and low SES concentration effect-sizes because the effects of both fail to retain significance after considering individual level factors. Subsequently there is no evidence of environmental contributions to educational inequality. Hypothesis II finds significant effects for both high and low SES concentrations but no difference in their magnitude. In hypothesis III the negative low SES concentration effect is larger than the positive high SES effect supporting the conclusion that its stronger association with education outcomes makes it the leading contributor to educational inequality. Hypothesis IV finds a large effect for concentrated advantage over concentrated disadvantage implying the inflation of education outcomes among the well off is the primary contextual determinant of inequality. Finally hypothesis V finds a positive effect of low SES concentration, possibly due to the intervening effect of educational institutions serving low income populations. Depending on whether the high SES concentration effect is insignificant, smaller than, equal to or larger than the low SES concentration effect we could find that socioeconomic composition effects reduce, leave unchanged or add to the educational inequality existing at the individual level.

Meta-Analysis and Validity in Neighborhood Studies

Questions of this type are frequently followed by some sort of systematic evaluation of a body of research. My interest in understanding the relative influence of a context's high or low socioeconomic composition in terms of its contribution to education outcomes and educational inequality is served best by a quantitative synthesis of research findings or meta-analysis. Meta-analysis pools the results of a body of research that tests the same conceptual hypothesis and presents an overall effect-size estimate (Cooper and Hedges 1994; Hedges and Olkin 1985). An effect-size estimate provides the researcher with the evidence useful to generalize across an otherwise complicated body of literature.

A meta-analysis however has a difficult time avoiding some of the shortcomings of the existing studies that are included in the synthesis. There are many reasons to be suspicious of research that seeks to measure the influence of the socioeconomic context. First, these studies are largely correlational ones and therefore cannot avoid the threat of selection bias. There is a possibility that the individual level factors typically unobserved in neighborhood research could account for much of the variation we would otherwise attribute to neighborhood characteristics (Ginther, Haveman and Wolfe 2000). Concern has also been expressed about the construct validity of research that relies on census tract information to represent an area's socioeconomic status when it is unclear if those census tracts mirror the true boundaries of neighborhoods and capture the social processes that distinguish neighborhoods in definition from other geographic areas (Raudenbush and Sampson 1999). One may ask why attempt a meta-analysis of such a troublesome literature?

My interest in the relative influence of two socioeconomic indicators addresses some of these concerns in that I place an emphasis on the internal validity of studies. Of interest in matters of internal validity are the circumstances under which research data were collected and the trustworthiness of conclusions made about the specific subjects observed in the setting. In this meta-analysis the estimates are in fact the specific subjects observed and the analytical actions taken by the researchers in the individual study setting, including the specification of the model, affect the information those estimates provide and the trustworthiness of conclusions made about them. That is not to say that external validity is not important, however, we lessen the focus on and therefore the problems associated with generalizing back to human populations in that a meta-analysis generalizes back to the population of studies from which the effect-sizes were extracted. Hence I seek to make statements about the research in this area in what it says about the relative importance of the neighborhood factors to an understanding of educational inequality.

A concern with relative effects within a meta-analytic framework permits me to make a few assumptions. Since the unit of analysis is the study, any misidentification of neighborhoods, selection bias and differences in model specification will be constant within the studies from which we extract two measures of neighborhood socioeconomic status for comparison. Nonetheless, since some of these less than ideal features of the literature vary across studies I investigate a few secondary questions concerning the model specifications of the primary studies. These questions ask whether the difference in the two concentration effect-sizes varies according to 1) the inclusion of both measures of neighborhood SES; 2) the inclusion of more stringent controls; and 3) the selection of different education outcomes in the primary models. The analysis subsequently disaggregates the effects according to the type of education outcome, incorporates a measure of study quality, and explores any differences that may emerge between the primary sample and a sub-sample of effect-sizes from models that consider both socioeconomic measures. Since effect-sizes are nested within studies, I use Hierarchical Linear Modeling (HLM) to generate an overall effect-size, estimate the variance components, and consider characteristics of the study models to explain variation in the effect-sizes.

Identifying and Selecting Studies

Studies were identified and selected through three processes. The awareness of quantitative studies in this area allowed the author to search bibliographies, references and citations to locate comparable studies. Informal networks and contacts with researchers in the field also produced a number of studies that were "in press," disseminated at academic conferences or available as reports from research organizations. Last, computerized databases such as Wilson, ERIC, EconLit, and PsycLIT were searched using the keywords *neighborhood*, *community*, *urban*, *concentration*, *spatial*, *composition*, *education*, *achievement*, *learning*, *test-scores*, *attainment* and *dropping out* in various combinations. This approach produced several hundred studies but few relevant ones that had not been identified through our initial efforts. The literature search revealed 74 studies that contained effect-size estimates of neighborhood influences on education outcomes.

Selection of the Predictors and Outcomes

After identifying the studies, I constructed a set of conditions to guide the selection of the studies and their correlational estimates. These criteria for the selection of studies considered 1) whether neighborhood measures of high and low SES were present 2) which education outcomes were most numerous, 3) the number of samples and estimates available, 4) the inclusion of relevant statistical information, 5) whether the data were collected in the United States and 6) whether the inclusion of study parameters violated an assumption of independence among the estimates. With these criteria I reviewed the population of studies for the sampling of estimates that are relevant to the conceptual hypotheses I wish to explore.

As noted in the first criterion, studies had to hold a number of predictor and outcome variables in common to synthesize. For example, a synthesis of low socioeconomic effects on test score performance requires that both variables are employed within a significant number of studies. After coding the studies according to the variable definitions, the *percent of low income neighbors* and *percent of higher income neighbors* were the most common neighborhood socioeconomic characteristics.[3] The literature produced a number of education outcomes, the most numerous being *school measures, test scores, attainment* and *dropping out*. Each of these measures poses unique implications with regard to neighborhood studies. Ecological perspectives for instance (Bronfenbrenner 1984; Wilson 1987) suggest neighborhoods bring their effects to bear on youth development and behavior over time and therefore may be most evident in measures of attainment and the outcomes of adolescents over younger children (Halpern-Felsher et al. 1997). However, due to residential mobility and its tendency to be higher among less advantaged urban populations, and the structure of schooling which requires the majority of students to change educational environments at least once during their matriculation, measures of attainment rarely reflect the influence of just one neighborhood or school. Outcomes such as grade point average (GPA), self-reported grades and class rank are highly contextualized; a course grade of A- may correspond to a B- in a comparable course within a more rigorous school. The use of these highly contextualized indicators may be the least useful in understanding variation in learning across contexts. Test-scores have received much empirical attention due to their standardization. Unfortunately, test-scores if taken early in a youth's development may not capture the full predictive capacity of neighborhoods, and if taken later are likely to reflect the influence of more than one neighborhood or school. Taken together, these conditions do not identify one outcome unequivocally as a superior measure of contextual effects. Studies that did not contain these outcomes were eliminated from consideration reducing the number of studies to 34.

[3] Joblessness and neighborhood deprivation measures were common within the literature but were less suitable for this analysis for a few reasons. First, one's employment status is less stable and more likely to change over time than one's income. Deprivation composites are less ideal because their definition typically include non economic factors which range from gender and renter status (Catsambis and Beveridge 2002) to measures of industrial presence and the prevalence of teenage meeting places in the neighborhood (Spencer, McDermott, Burton and Kochman 1997).

Selection of the Estimates and Assumptions of Independence

Including more than one estimate from the same final sample or sub-sample, whether calculated within the same study or across several studies, violates the assumption of independence among the estimates. To comply with our last criterion and avoid any subsequent serial auto correlation among the estimates I decided to give preference to estimates with the most stringent model specifications. In studies that produce several estimates from models with different combinations of covariates, preference was given to the estimates of “full models” as opposed to models with fewer covariates. As a result all effect-sizes in this synthesis were estimated with individual and family-level controls in the primary studies. Second, since a larger number of independent estimates strengthens the study, I gave preference to the effect-sizes from disaggregated over aggregated data. In studies where it remained unclear which coefficient to include, multiple coefficients were averaged to produce a single effect-size estimate. Safeguarding the assumption of independence put the final sample size at 13 primary studies, 48 data samples and 81 sample estimates. The inclusion of multiple estimates from studies with multiple samples and from analyses of disaggregated data allowed the number of sample estimates to far exceed the number of studies without weakening the independence of the estimates. Despite the seemingly low number of studies, the figures presented in Table 1 show the selected studies represent a combined sample size of 142,036 individuals for the high SES analysis and 53,683 for the low SES analysis. Furthermore the estimate *n* is sufficient considering well regarded research syntheses have been conducted with a similar number of studies and samples (Cooper et al. 1996; Raudenbush and Bryk 2002) and fewer estimates (Raudenbush 1984; Raudenbush 1988; Kalaian and Raudenbush 1996).

[insert Table 1 near here]

Definition of Variables

Frequently the variables involved in meta-analysis are in fact composites due to the varying variable definitions used in the primary studies. *Percent of Low SES neighbors* for instance is defined in the primary studies as the percent making below $10,000; percent receiving welfare; and the percent living in poverty. Measures of *percent of higher SES neighbors* are also composites and include the percent of families making above $30,000 for older studies and $50,000 for a more recent one; percent holding white collar occupations; absolute income of the top economic quintile, and the percent holding professional and managerial positions.

The univariate analysis produces one estimate for the effect of a neighborhood factor across all four outcomes. I refer to this composite indicator simply as *education outcomes*. The multivariate analysis reports estimates for *attainment*, *school leaving* and *test-scores* while using *school measures* as the reference. Since not all of these studies will contain all four outcomes, these dummies are coded 1 for present and 0 for absent. *Test-scores* and *school measures* should be viewed as composites due to the multiple test-types and indicators of academic performance used within and across the primary studies. The use of a composite indicator as an outcome presents some benefits as well as limitations. The differing variable definitions across studies limits the practical interpretability of the findings; the effect magnitudes presented in the synthesis cannot be used to determine how grades or test-score points correspond to standard deviation unit increases or decreases. Though interesting, such information is not central to our research question. Composites do however increase the external validity and generalizability of the findings in allowing us to include all of the relevant studies.

All of the primary studies employ numerous control variables. Over 90 percent of the estimates come from models that account for the age, gender, socioeconomic status and race of the child. Less prevalent were measures of the family structure and the educational level of the parent(s) which were accounted for in approximately 58 and 65 percent of the sample estimates, respectively. In contrast, only a little over 7 percent of these estimates include school level measures, a fact that precludes the relative assessment of effect-sizes among studies that do and do not consider schools and how might their consideration alter the relative impact of the two neighborhood socioeconomic factors.

Though additional covariates exist in the primary studies that are less relevant to this study, their unequal inclusion among the study models may influence the relative balance of the SES effects we observe. To assess the possibility, I have elected to consider the stringency of the model specifications within the primary studies by constructing a moderating variable, *study quality*. Caution requires I echo the suspicion that the inclusion of seemingly less relevant covariates within the primary study models may have suppressed the original estimates included in this synthesis (Ginther et al. 2000; Sampson, Morenoff and Gannon-Rowley 2002). Thus the consideration of the study quality variable is unlikely to determine to what extent a finding of smaller effect-sizes among studies with more numerous controls reflect this suppression or the inclusion of more appropriate controls. This awareness does not seriously jeopardize the merit of considering model specifications as a measure of study quality however. For example, a finding of larger effect-sizes for studies with more controls would challenge the threat of suppression. Nonetheless, I present the analysis of the full sample with and without the study quality variable.

In constructing the variable *study quality,* I first reviewed the model specifications of the primary studies. The review revealed a total of 186 different variables across the primary studies. I organized these variables with regard to their unit of analysis (individual, family demographics and process, school and neighborhood structure and process). Next I organized the variables within each unit of analysis according to common social science constructs. For example within the unit of analysis "family demographics," the variables parents' education; father's education; mother's education; and no knowledge of mother's and father's education were grouped and coded as indicators of "parents' education." Other constructs within the family demographics heading are family origin, family size, adult presence, parental health and parent's age. The organization of the primary study variables yielded 52 constructs.

The next step in the assessment of study quality included the coding of the estimates according to the specifications of the models from which they were extracted. A study quality score was tallied for each estimate by allocating a point for each of the 52 constructs present in the primary study model(s). The study quality score ranges from 6 to 25 with a mean of 9.822. We use this mean to dichotomize the distribution of low SES and higher SES effect-sizes: a study quality score of 0 is entered for those estimates that fall below the study quality mean while those that exceed the mean are coded 1.

Method and Analytical Procedure

Statistical Conversions: Computations of Half-Standardized Coefficients, *d*.

Most of the studies report the effect-size estimate as a regression coefficient, sometimes in both the metric coefficient (unstandardized), β^m, and standardized coefficient $\beta^* = \beta^m\sigma_x/\sigma_y$ (where the sample estimate of β^m is denoted as B^m, the estimate of β^* as B^* and the estimate of

δ as *d*). Due to the conceptual focus given here to socioeconomic status, I have elected to report the effect-size estimates in their half-standardized form ($\delta = \beta^m / \sigma_y$). In questions that concern socioeconomic status, it is better to report half-standardized estimates than to report β^*, primarily because with β^*, communities with a restricted range of SES will generate a smaller effect-size than communities with a larger range in SES even if the actual effect-sizes are the same. In this analysis $\delta = \Delta y$ for each 1% increase in *X*. Because none of the studies report half-standardized values, the statistical information from each study was used to calculate half-standardized coefficients.

The computation of half-standardized estimates is applied uniformly across all estimates except where the amount of statistical information available within a particular study was limited. Some studies did not report standard errors. Fortunately these estimates were designated as significant in the primary studies and subsequently assigned a standard error approximately half ($\beta/1.96$) of the estimate value. Standard deviations that were absent within the primary studies were imputed from another study that relied on an identical data source. While imputing information about data from other studies is not ideal, the practice is common in meta-analysis. There is no reason to believe imputing statistical information will lead to important differences in the effect-size estimate; should one occur its impact will be constant across the two socioeconomic effect-sizes taken from each study since the imputation occurs at the study level. The decision to impute missing values occurred infrequently (in less than 9 percent of the sample estimates) and was preferable to the bias and limited external validity that would result from eliminating the estimates from consideration. I will revisit this issue in the discussion of the strengths and weaknesses of the analysis. The half-standardized coefficient was computed as $d = B^* / \sigma_x$ based on the reasoning that $B^*/\sigma_x = (B^m \sigma_x / \sigma_y)/\sigma_x = B^m/\sigma_y = d$. The statistical conversions complete the data set. Table 2 displays the means and standard deviations of the effect-sizes and moderating variables.[4]

[insert Table 2 near here]

Hierarchical Linear Modeling (HLM)

As Raudenbush and Bryk (2002) point out, it is "natural to apply hierarchical linear models to meta-analytic data because such data are hierarchically structured," (p. 206) that is, effect-sizes are nested within studies. HLM attends to hypothesis testing by 1) generating an overall effect-size; 2) estimating the variance of the effect-size parameters (as distinct from the variance of the effect size estimates); and, 3) explaining variation in the effect-size parameters by considering subject and study characteristics in linear models. I consider a specific study that estimates the effect of a neighborhood level variable on educational indicators, denoted as, δ. Suppose not a similar effect is estimated in each of many studies, indexed by *j*. My aim is to compare these estimates in a meta-analysis of the *j* studies. In the analysis, I test the null hypothesis, H_0: $\gamma_s = 0$ which implies that the effect, γs of study characteristic W_s on a particular effect-size is zero. These procedures will be applied to a univariate analysis where we specify an unconditional model for the full sample and a sub-sample of estimates (taken from models that include both neighborhood socioeconomic measures) and a conditional model that considers study quality. In the multivariate mixed linear meta-analysis I disaggregate the effect-sizes according to our four educational outcomes, using school measures as the reference.

[4] The correlation of the socioeconomic effect-sizes with the study quality and education outcomes variables did not exceed .213. The highest correlation among the education outcomes was between school measures and test scores at -.556 within the analysis of Low SES effect-sizes and -.535 in the analysis of higher SES effect-sizes. These are acceptable upper bounds of correlation.

Estimation

Unequal sample sizes would compromise the interpretability of the meta-analysis results. Since each d_j, has valuable and unique properties, weights were constructed to ensure the appropriate consideration of effect-size estimates (Raudenbush and Bryk 2002). In viewing each d_j as an independent, unbiased estimator of δ_j with variance Δ_j, the precision of d_j is defined as the reciprocal of its variance: precision (d_j) = Δ_j^{-1}. These weights are represented in the construction of a dummy variable for *P* parameters δ_{1j}, δ_{2j}, δ_{3j}, and δ_{4j}.

Results

Tables 3 through 6 present the findings of the univariate and multivariate analysis. Table 3 presents an unconditional analysis which specifies estimates of the grand mean effect-size γ_0 and level-2 variance of the effect-size τ for the percentage of low SES neighbors in Model 1 and the percentage of higher SES neighbors in Model 2. In the analysis of low SES effects the effect-size, $\hat{\gamma}_0$ = -.0271 is of a moderate magnitude and implies that among the studies considering this relationship we can expect a decrease of nearly .0271 standard deviation units in education outcomes for every unit increase in the percentage of lower SES neighbors. The estimate is insignificant however at p = .449. The estimate of between-study variance, $\hat{\tau}$ = .00132 and corresponding standard deviation of .03634 imply there is a sizable amount of variation between the studies in the effect-size: a study with an effect-size one standard deviation above the mean would yield δ_j = .0092, while a study with an outcome one standard deviation below the mean would yield δ_j = -.0635. The corresponding significance level (p = >.500) suggest the inconsistency in study findings could be due to chance.

In Model 2, the effect-size depicts a positive association between the percentage of higher income neighbors and education outcomes. This effect-size is significant at the p = .018 level and of greater magnitude ($\hat{\gamma}_0$ = .0321) than the low SES estimate in Model 1. To put this finding in perspective, consider a 25 percent increase in the percentage of higher income residents is associated with a .80 standard deviation increase in the educational outcomes of residents in the area. The variability among these estimates is considerable and more than among the low SES effect-sizes ($\hat{\tau}$ = .00354, SD = .0595). The corresponding level of significance suggests the variation in the estimates is unlikely due to chance (p = .000).

[insert Table 3 near here]

In order to better understand the findings of the univariate analysis I question if the relationship between the two neighborhood socioeconomic factors and education outcomes differ according to the type of educational indicator. I subsequently have disaggregated the effect-sizes of the full sample in a multivariate analysis according to their measurement of a common educational indicator and report the findings in Table 4. Model 1 contains the findings of the analysis of low SES neighborhood effects where I estimate the effect-sizes (d_{j1}) and the parameters (test-scores, attainment, and dropping out). The grand mean estimate $\hat{\gamma}_{00}$, is modest but significant ($\hat{\gamma}_{00}$ = .0071, p = .004). The effect-size estimates for test-scores ($\hat{\gamma}_{10}$ = -.0315, p = .000), attainment ($\hat{\gamma}_{20}$ = -.0122 p = .025) and dropping out ($\hat{\gamma}_{30}$ = –.0215, p = .000) are much greater in magnitude than $\hat{\gamma}_{00}$ and negative. The significance levels of the moderators suggest that their estimated difference from the significant grand mean is unlikely due to chance. The negative valence of the moderators and their significant departure from the grand mean illuminates why the univariate analysis reported a negative and insignificant effect-size.

Model 2 within Table 4 details the findings of the analysis of the effects of higher income neighborhood composition on distinct education outcomes. In contrast to Model 1, the mean effect-size for the percentage of higher income residents is considerable and positive (γ_{00} =.0232, *p* = .076). The effect-sizes reported for test scores (γ_{10} = .0041, *p* = .894) and the estimate for dropping out ($\hat{\gamma}_{30}$ = -.0133, *p* = .670) are insignificantly different than $\hat{\gamma}_{00}$. Despite the negative estimate for dropping out, its contrast with γ_{00} leaves a small but nonetheless positive and unexpected relationship between the percentage of higher income residents and rates of premature school departure. The estimate of the attainment effect-size is -.0638 with a corresponding significance value, *p* = .036 suggesting that the attainment effect-sizes are significantly different than $\hat{\gamma}_{00.}$ Once again the variance components indicate there is a large amount of variation between studies in the effect-sizes that is unlikely due to chance (*p* = .000).

[insert Table 4 near here]

There is a possibility that other factors within the studies have influenced the findings presented in Table 3. In Table 5, I present two additional models to understand how the effect-sizes may differ according to the stringency of the model specifications from which the estimates were extracted. This conditional analysis presents a grand mean estimate, γ_0 and the moderator γ_1, study quality. In Model 1, we find the effect-size estimate for the percentage of low SES neighbors becomes larger and remains insignificant ($\hat{\gamma}_0$ = -.0353, *p* = .294). The study quality moderator variable is not significant; however the estimate is considerably larger indicating that studies with more covariates within the model specifications report negative relationships of greater magnitude between the percentage of low SES residents and education outcomes. The variance components of this model are very similar to those of the unconditional model reported in Table 3.

In contrast, Model 2 details the findings of the conditional analysis of higher SES concentration effects and finds the effect-size decreases in magnitude ($\hat{\gamma}_0$ = .0286) and somewhat in significance (p = .059) while the moderator variable remains insignificant ($\hat{\gamma}_1$ = .0208, p = .492). There is only slightly less variation among the effects in the conditional (τ = .00345) than the unconditional analysis (τ = .00354), the former explaining an additional 2.54 percent of the variation. The corresponding significance level (p = .000) suggest the inconsistency in study findings is not by chance. After considering variation in study quality the percentage of advantaged neighbors remains the only significant neighborhood level determinant of education outcomes within the studies.

[insert Table 5 near here]

There is a possibility that the estimates taken from models that contain only one of the two neighborhood socioeconomic composition measures are less accurate due to the unobserved influence of other neighborhood SES factors. Table 6 presents the unconditional analysis of a sub-sample containing the effect-sizes from models that estimate both measures of neighborhood socioeconomic status providing a direct test of the relative influence of each while controlling for the other. In Model 1, the effect-size for low SES effects on education outcomes is at least three times smaller than any of the previous low SES estimates, unexpectedly positive and significant

($\hat{\gamma}_0$ = .0073, p = .022). Given the full sample of low SES estimates yield larger associations, it appears the low SES effect-sizes estimated in the primary studies with measures of concentrated advantage are much smaller. Though conventional wisdom would lead one to question the positive impact of concentrated economic disadvantage, the finding alludes to the last of our hypotheses listed within the literature review that recognizes the ameliorating impact institutions can have on the learning outcomes of the less fortunate. The variance components find much less variability among the effect-sizes than in previous models (τ = .00006, p = .000). Model 1 has 95.5 percent less variation among the effect-sizes than found in the analysis of the full sample presented in Table 3.

Model 2 within Table 6 details the findings of the unconditional analysis of higher income effect-sizes. In contrast to the findings of the low SES sub-sample, the higher SES sub-sample produced a pooled effect-size that is slightly larger and more significant than found in the full sample (γ_0 = .0329, p = .006). In addition, the magnitude of the estimate is a little over 4.5 times greater than the low SES sub-sample effect-size. Hence across the primary studies, it appears that the effects of the percentage of higher SES residents is greater than that of low SES residents within models that simultaneously measure the influence of both socioeconomic populations. The measures of variability in the sub-sample also differ from those reported in the full sample. The estimate of between-study variance, $\hat{\tau}$, decreased from .00354 in the full sample to .00168 in the sub-sample and remained highly significant. This difference between the variance components of the two samples corresponds to a 52.54 percent difference in the variability of effect-sizes. In sum, the findings of the sub-sample suggest that even in the most hopeful case where concentrated disadvantage is associated with educational outcomes that exceed what we might expect given one's individual level characteristics (perhaps due to school reforms or targeted intervention in distressed areas), those associations are marginal compared to the greater effects of concentrated advantage.

[insert Table 6 near here]

Discussion

I started this investigation by articulating several questions concerning the relative effects of the percentage of low income neighbors versus the percentage of higher income neighbors in relation to educational inequality. I speculated that given the qualities of the effects there were at least five ways to understand the low-higher SES relationship, including that 1) there is no difference in the high and low SES concentration effect-sizes because the effects of both fail to retain significance after considering individual level factors, 2) there are significant effects for both high and low SES concentrations but no difference in their magnitude, 3) the negative effect for low SES is larger than the positive one for high SES supporting the conclusion that its stronger association with education outcomes makes it the leading contributor to educational inequality, 4) the effect of concentrated advantage is larger than the one for concentrated disadvantage implying the inflation of education outcomes among the well off is the primary contextual determinant of inequality, and 5) the effects of low SES concentrations are positive possibly due to the intervening effect of educational institutions and targeted interventions serving low income populations. Depending on the balance of the low and higher SES concentration effects in the literature we could find that socioeconomic composition is associated with reductions, no change in or additions to the educational inequality existing at the individual level. This analysis sought to inform these questions and hypotheses by pooling the effects of neighborhood composition studies in education, considering aspects of study quality that may influence the findings, and also disaggregating those effect-sizes according to distinct education outcomes.

This study finds that investigations of the relationship between the presence of higher income neighbors and education outcomes produce effect-sizes that are greater in magnitude and significance than similar investigations of low SES residents. This result appeared consistent after disaggregating the effect-sizes according to the type of educational indicator, considering the stringency of the model specifications and examining the estimates of models that include both high and low neighborhood socioeconomic factors. In the case of the latter, the magnitude of the low SES effect-size diminished most significantly within models that included both neighborhood socioeconomic predictors. The higher SES effect-sizes of the full and sub-sample appeared essentially identical, implying that in this body research the effects of higher SES residents are somewhat resilient to the presence of lower income populations.

In relation to our hypotheses these findings suggest inasmuch as concentration effects are related to educational inequality, the studies find the spatial arrangement of families at the higher end of the economic distribution is more problematic than that of the lower. These findings are not as counterintuitive as they may appear. First, advantaged populations benefit from concentration effects more than other populations primarily because they have the resources to bring to bear on schooling and learning. There are a myriad of ways in which their concentration leads to advantages including better financed schools, higher property values and the educational arrangement advantaged communities secure within schools that ensures their children will receive the education of greatest value (Jencks and Mayer 1991; Brantlinger 2003). Second, the finding of no relationship between the percentage of low income residents and education does not deny the troublesome lower quality of those environments and their connection to educational behavior. Indeed the findings of the sub-sample in this analysis allow us to consider that it's the *absence* of advantaging concentration effects more than the *presence* of contagion ones that disadvantages poor communities. In short, it is more important to consider *who does not* live in places of concentrated poverty than *who does* in explanations of educational inequality. This observation is consistent with the concerns expressed by Goetz (2003) and Benabou (1993) regarding the creaming effects of redistributive policy. Both authors point out the consequences of individuals moving to more prosperous environments are greater for the communities left behind than for those being joined. These findings are also consistent with studies conducted in Helsinki (Kauppinen 2004) and the London metropolitan area (Gordon and Monastiriotis 2006), both finding that the individual-level inequality in educational outcomes resulted from the positive impacts of concentrated advantaged rather than the negative impact of economic segregation for the disadvantaged. Hence, positive externalities in places correspond to the existence of neighborhood capital whereas negative ones correspond to its absence.

The findings are not without limitations, many of which are common in meta-analysis and neighborhood studies. The lack of consideration given to schools in this body of research is a delimiting factor. Schools may provide independent effects that offset neighborhood influences or in contrast work to enhance the impact of presumably endogenous neighborhood influences. A better understanding of neighborhood-school relationships and their role in educational production can only be attained in research that considers both contexts. A second limitation is that publication bias may have also influenced the findings of this study. By reviewing submissions, editors and reviewers decide which studies are published and thereby determine the universe of studies from which to draw a data sample. It is possible that some studies were not published because their findings were similar to other studies already published or seemed inconsistent with the current direction of neighborhood research. Finally, it is possible some studies worthy of consideration were not included in this meta-analysis. Their omission can be explained in a number of ways. First, some studies focused on education outcomes that were not indicators of learning or attainment (e.g. discipline, engagement, course-taking) and therefore were not considered. Those studies await the formulation of other research questions and meta-analytic studies. Despite the rigorous efforts of the author, some studies may have been overlooked or made available during the latter stages of this work. Additionally, the omission of some standard errors and significance levels required I impute the parameters which may have an unforeseen affect on the results. Any impact from the imputations would more than likely artificially suppress the findings of both socioeconomic factors since the minimum level of statistical significance was used for those significant estimates missing standard errors.

There are nonetheless important implications for urban and educational studies awaiting consideration. First, the socioeconomic context of the relationships we seek to explore in research warrants consideration of multiple socioeconomic indices to avoid generating less accurate estimates. This approach will better support the relating of the findings to the theoretical explanations of how these outcomes develop. Second, theories that presume the educational performance of residents of poor communities is lower than what their individual characteristics would suggest need to be explained in light of these findings. Many of these theories reviewed here suggest a subculture shared among co-inhabitants is the culprit when it is just as likely underperformance results from what the poor do not or have not to share. Last, further consideration should be given to the relationship between concentrated advantage and social class disparities in research. As the demography of capital remains or becomes increasingly uneven, research should seek answers as to its effect on populations at both ends of the economic distribution. Then perhaps we can understand better the problem of concentrated advantage.

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TABLE 1: List of Studies, Study Characteristics, Study Quality Score and Primary Study Estimates [a]

First Author/Year	Data Set	Number of Samples	Avg. Study Quality Score	Low SES *d*	High SES *d*
Pebley 2003	Los Angeles	2	13.00	-.24	2.14
				-.25	3.09
Clark 1992	PUMS 1980	1	13.33	.0087	-.02
Duncan 1994	PSID	4	11.00	.057	.04
				.015	-.01
				.024	.03
				.027	.04
Rivkin 1994	HSB	4	11.00	-4.63	--
				6.01	--
				-3.44	--
				1.01	--
Ensminger 1996	Chicago	2	10.00	.005	.03
				-.000	.03
Chase-Lansdale 1997	IHDP	4	11.50	-.14	2.65
				-1.01	1.29
	NLSY	4	12.50	-1.54	.38
				-.76	-.98
				-.2333	2.31
				-.1466	-.20
Halpern-Felsher 1997	New York	4	7.60	-.16	-.41
	(Ages 8-11)			-.02	.15
				.27	-.25
				-.17	.03
	New York	4	7.60	.05	-.29
	(Ages 10 -16)			-.08	.08
				.05	-.05
				-.01	-.17
	Atlanta	2	10.60	-6.00	-.38
				-5.52	8.51
	NY/B/DC	4	8.60	.02	-.30
				.04	.02
				-.37	.11
				-.02	-.02
	New York	4	7.60	-.14	-.32
	(Ages 15 -20)			.12	.02
				.09	.02
				.05	-.21
Ladd 1997	Baltimore	1	6.00	-.1763	--
Crane 1991	PUMS 1970	4	9.00	--	.91
				--	.73
				--	.54
				--	.57
Ainsworth 2002	NELS	1	14.66	--	1.13
Lopez-Turley 2002	PSID-CDS	1	9.00	-.0616	-.01

Fischer 2004	Philadelphia	1	15.00	--	-8.67
Woolley 2006	SSP	1	16.00	.026	-.025

[a] Some *d*s are averages of multiple estimates within the primary studies.

TABLE 2: Definitions, Means and Standard Deviations of Effect-Sizes and Variables

Variables	Definition	N	Mean	Std. Deviation
High SES Effect-Size	Effect of High SES (*Percent professional or managerial, Percent of families with income > $30,000 and $50,000, Percent white collar and Absolute income of top quintile*) on Education Outcomes (*school GPA, years of schooling, and test scores*)	41	.0627	.13130
Test Scores	Outcome of effect-size is a math, reading, vocabulary or IQ test score (1 = yes, 0 = no)	41	.2927	.46065
Attainment	Outcome of effect-size is years of completed schooling (1 = yes, 0 = no)	41	.1463	.35784
Dropout	Outcome of effect-size is dropping out (1 = yes, 0 = no)	41	.1463	.35784
School Measures [a]	Outcome of effect-size is self-reported grades, self-reported GPA, student mobility, placement in Algebra, attendance and whether child is receiving psychological testing (1 = yes, 0 = no)	41	.4146	.49878
Study Quality	Number of constructs included in model specifications (above the mean = 1, below the mean = 0)	41	.5122	.50606
Low SES Effect-Size	Effect of Low SES *(Fraction neighbors with incomes < $10k, percent in poverty, relative disadvantage & percent receiving welfare, percent of children in poverty)* on Education Outcomes (*school GPA, years of schooling, and test scores*)	40	-.0074	.13415
Test Scores	As Previously Defined	40	.3000	.46410
Attainment	As Previously Defined	40	.1500	.36162
Dropout	As Previously Defined	40	.1250	.33493
School Measures	As Previously Defined	40	.4250	.49831
Study Quality	As Previously Defined	40	.5500	.50383

[a] All non-cognitive outcomes listed in the variable definition are part of an educational composite within the primary studies. All education composites within the primary studies include an indicator of achievement.

Table 3: Models of Neighborhood Low and High SES Effects on Education Outcomes

Fixed effects	Model 1 Effect	SE	t	df	Model 2 Effect	SE	t	df
Intercept, γ_0	-.02713	.0355	-0.765	39	.0321**	.0129	2.476	40

Random effects	Model 1 Variance Component	X^2	p-value	Model 2 Variance Component	X^2	p-value
	.00132	0.12241	>.500	.00354	2080.81554	.000

(***) $p < .001$
(**) $p < .05$
(*) $p < .10$

Table 4: Models of Neighborhood Low and High SES Effects with Moderators, Test Scores, Attainment, and Dropping out

Fixed effects	Model 1 Effect	SE	t	df	Model 2 Effect	SE	t	df
Intercept	.0071**	.0023	3.164	36	.0232*	.0127	1.821	37
Test Scores, γ_{10}	-.0315***	.0061	-5.186	36	.0041	.0302	0.135	37
Attainment, γ_{20}	-.0122**	.0052	-2.343	36	-.0638**	.0293	-2.173	37
Dropout, γ_{30}	-.0215***	.0043	-4.957	36	-.0133	.0309	-0.429	37

Random effects	Model 1 Variance Component	X^2	p-value	Model 2 Variance Component	X^2	p-value
	.00004	50.24877	.058	.00467	2937.67851	.000

(***) $p < .001$
(**) $p < .05$
(*) $p < .10$

Table 5: Models of Neighborhood Low and High SES Effects on Education Outcomes with Moderator Variable, Study Quality

	Model 1				Model 2			
Fixed effects	Effect	SE	t	df	Effect	SE	t	df
Intercept, γ_0	-0.0353	.0332	-1.065	38	.0286*	.0015	1.939	39
Study Quality, γ_1	-0.0969	.0663	-1.462	38	.0208	.0299	0.694	39

Random effects	Variance Component	X^2	p-value	Variance Component	X^2	p-value
	.00138	0.11252	>.500	.00345	2981.65055	.000

(***) $p < .001$
(**) $p < .05$
(*) $p < .10$

Table 6: Models of Neighborhood Low and High SES Effects on Education Outcomes, Sub-sample

	Model 1				Model 2			
Fixed effects	Effect	SE	t	df	Effect	SE	t	df
Intercept, γ_0	.0073**	.0030	2.398	34	.0329**	.0112	2.941	34

Random effects	Variance Component	X^2	p-value	Variance Component	X^2	p-value
	.00006	481.20832	.000	.00168	2079.22096	.000

(***) $p < .001$
(**) $p < .05$
(*) $p < .10$