NEIGHBORHOOD DISADVANTAGE AND SCHOOL DROPOUT:

A MULTILEVEL ANALYSIS OF MEDIATING CONTEXTS

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ABSTRACT OF THE DISSERTATION

Neighborhood Disadvantage and School Dropout:

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Prior research has shown that children's residence in high poverty neighborhoods increases their risk of high school dropout. However, the mechanisms through which neighborhood socio-economic disadvantage exerts influence on educational attainment are poorly understood. The current study uses nationally representative survey data from the National Longitudinal Study of Adolescent Health (Add Health) to estimate the extent to which school, neighborhood, and peer group contexts mediate the effect of neighborhood disadvantage on high school dropout. The conceptual framework adopted integrates theoretical frameworks of school dropout with theories of social isolation, social organization, and resource-based theories of neighborhood effects. Mediating contexts examined include the resources, disciplinary policies, and structural characteristics of the schools youth attend; multiple dimensions of social organization in the neighborhoods youth reside; and the degree to which youths' closest friends are emotionally, behaviorally, and cognitively disengaged from school.

Few of the hypothesized school, neighborhood and peer group contexts are found to substantially mediate the effect of neighborhood disadvantage on high school dropout. Holding all mediating contexts constant, neighborhood disadvantage remains strongly

associated with school dropout, including in school-fixed models, which compare youth who attend the same school but reside in different neighborhoods. Contrary to theory, neighborhood social cohesion and informal social control are higher in poor neighborhoods and not associated with school dropout. Peer group school disengagement varies little across neighborhood context after adjusting for individual and family socio-economic characteristics. Neighborhood-level intergenerational closure – the extent to which parents in the neighborhood remain in communication with the parents of their children's friends – is substantially lower in poor neighborhoods, explaining around 25% of the neighborhood disadvantage effect on school dropout. However, the effect of neighborhood intergenerational closure is less pronounced for African American and Hispanic youth. Considerable variation in direct and indirect effects of neighborhood disadvantage is also observed across demographic sub-groups.

Findings suggest that the socio-economic composition of local residential contexts influences secondary educational outcomes independent of the schools youth attend. Moreover, results challenge the universal applicability of traditional theoretical models of neighborhood effects, which assert that the poor educational performance of youth from poor neighborhoods is best explained by disorganized community environments and deviant youth sub-cultures. Given these findings, public policy that reverses rising socio-economic residential segregation patterns is strongly recommended. Future research should more closely examine how heterogeneity within and between school, neighborhood, and peer group contexts interact to undermine the educational attainment of youth from disadvantaged socio-economic backgrounds.

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1. Introduction

1.1 Background and significance

The residential contexts in which children live and grow shape their schooling experiences and achievements. Where children live largely determines the schools they attend, the formal and informal institutions to which they belong, and the relationships and social interactions through which they learn and develop. In the U.S., rising residential segregation by income (Bishaw, 2014; Fry & Taylor, 2012) has coincided with rising economic and educational inequalities (G. J. Duncan & Murnane, 2011). These trends are likely to be reinforcing; children who grow up in poor neighborhoods experience a variety of academic disadvantages (Leventhal & Brooks-Gunn, 2000; Sastry, 2012). The increased risk of high school dropout is perhaps the most detrimental consequence experienced by youth who reside in communities of concentrated disadvantage. For the individual, failure to obtain a high school diploma severely constrains economic opportunity (Ashenfelter, Harmon, & Oosterbeek, 1999; Carneiro, Heckman, & Vytlacil, 2011), while increasing risk for lifelong health problems (Conti, Heckman, & Urzua, 2010; Lleras-Muney, 2005), and involvement with the criminal justice system (Pettit & Western, 2004; Wakefield & Uggen, 2010). These individual consequences not only translate into large costs to society (Belfield & Levin, 2007), but also reinforce structural inequalities that block individuals and communities from upward social and economic mobility.

Theoretical perspectives that seek to explain how and why students fail to earn a high school diploma emphasize dropping out as a process involving gradual and sustained withdrawal from school institutions (Rumberger, 2011; Wang & Fredricks,

2013), often referred to as school disengagement (Appleton, Christenson, & Furlong, 2008; Fredricks, Blumenfeld, Friedel, & Paris, 2006). Consistent with the social work profession's emphasis on the person-in-environment perspective (Chalmers, 2011; Green & McDermott, 2010), the current study examines environmental factors believed to influence the school disengagement processes that ultimately manifest in dropping out. In particular, the neighborhoods where youth reside are the primary contexts under study. The characteristics of schools and peer groups are also examined as mechanisms through which neighborhood contexts exert influence.

Policy makers and researchers have long theorized that living in poor neighborhoods interferes with positive youth development and undermines high school completion (Brooks-Gunn, Duncan, & Aber, 1997b; Jencks & Mayer, 1990). For over two decades, scholarly research using national data and a variety of methodological approaches has consistently confirmed that students from socio-economically disadvantaged neighborhoods (e.g., high poverty rates) are less likely to graduate from high school than comparable socio-economically positioned students from more advantaged neighborhoods (Brooks-Gunn, Duncan, Klebanov, & Sealand, 1993; Crane, 1991; Crowder & South, 2011; Harding, 2003; Wodtke, Harding, & Elwert, 2011). These findings have underscored the importance of developing broader and deeper understandings of structural disadvantages that extend beyond individual families. As the evidence suggests, children's educational prospects are not only affected by the socio-economic status of their family, but so too by the socio-economic status of their neighbors.

To date, the translation of these findings into practice and policy has been limited. Targeted mobility programs that relocate poor families out of poor neighborhoods are rarely seen as a viable large scale policy solution (Sampson, 2012). Instead, policy and practice efforts focused on ameliorating the variety of ecological hardships experienced by children who grow up in disadvantaged neighborhoods are likely to improve and sustain prospects of disadvantaged youth. Unfortunately, little is known about how or why the socio-economic characteristics of neighboring residents impact educational outcomes. For decades, scholars have underscored the need to uncover specific mechanisms of neighborhood effects that go beyond mere aggregation of the residents' socio-economic characteristics (Harding, Gennetian, Winship, Sanbonmatsu, & Kling, 2011; Jencks & Mayer, 1990; Sampson, Morenoff, & Gannon-Rowley, 2002; Sharkey & Faber, 2014a; Small & Newman, 2001). Identifying these mechanisms may help reveal specific contexts to which policy and practice intervention can be targeted.

A variety of theoretical models have been proposed. First, children from poor neighborhoods may be denied access to high quality institutional resources; poor quality schools attended by students from poor neighborhoods may contribute to disparities in school completion. The ecological contexts emphasized by these resource-based models of neighborhood effects are schools. Second, concentrated poverty may undermine the social cohesion of neighbors and their ability to work collectively to solve problems and informally regulate youth behavior. The ecological contexts emphasized by these social organization based theories of neighborhood effects are neighborhoods. Third, youth who grow up in poor neighborhoods may be isolated from institutions and social networks that shape local cultural norms about the importance of educational and occupational

attainment. The ecological contexts emphasized by these social isolation or epidemic models of neighborhood effects are peer groups.

The current study systematically examines each of these contexts, estimating the extent to which relevant school, neighborhood and peer group characteristics contribute to the effect of neighborhood poverty on school dropout. The current study does not endeavor to obtain precise causal estimates of neighborhood poverty, but rather presents a comprehensive ecological portrait of the theoretically derived constructs hypothesized to link poor neighborhoods to poor educational outcomes. More specifically, the current study includes five specific research objectives described as follows:

1. *Replicate prior research that establishes a positive relationship between neighborhood socio-economic disadvantage and school dropout.*
2. *Estimate the extent to which schools characteristics mediate the relationship between neighborhood socio-economic disadvantage and high school dropout.* School characteristics include school resources, strictness of disciplinary policies, structural and organizational features, and student body composition.
3. *Estimate the extent to which neighborhood characteristics mediate the relationship between neighborhood socio-economic disadvantage and high school dropout.* Neighborhood characteristics include measures of social organization – social cohesion, informal social control, intergenerational closure – racial/ethnic diversity, and residential instability.
4. *Estimate the extent to which peer group characteristics mediate the relationship between neighborhood socio-economic disadvantage and high school dropout.* Peer group characteristics include measures of school disengagement among friends of youth.
5. *Explore heterogeneity in the direct effects of neighborhood socio-economic disadvantage on school dropout and the indirect effects exerted through school, neighborhood, and peer group characteristics.* Analyses are stratified by youths' gender, parental education, race/ethnicity, and grade.

Most broadly, findings are expected to improve understanding about the ways in which disadvantaged neighborhoods undermine educational attainment. More specifically, findings are expected to inform policy and practice that seek to target

specific ecological contexts for intervention. The current chapter describes school dropout as a social problem, focusing on the origins, prevalence, and consequences of the phenomenon. The individual process of dropping out and the institutional factors that set in motion and reinforce these processes are reviewed. Finally, residential context, the focus of the current study, is highlighted as an important area for further study. The chapter concludes with a brief discussion about the ways in which neighborhood contexts may influence educational outcomes.

1.2 School dropout as a social problem

1.2.1 *Origins and national goals*

By the early 1960s, the failure of American youth to graduate from high school emerged as a national social problem. Often described as the "dropout crisis" or "social dynamite," high school withdrawal suddenly commanded the attention of educators, researchers, and policy makers (Kett, 1995, p. 282), even as graduation rates continued to rise to unprecedented historical levels that exceeded all other industrialized nations (Goldin, 1998). The social construction of the dropout problem therefore reflected a new age-specific norm, where secondary education joined elementary education as an ordinary expectation (Dorn, 1993, pp. 354-356). As more students graduated, those who left high school without a diploma were increasingly perceived as deviant (Dorn, 1996).The new social problem was exacerbated by a larger perceived social and political upheaval that permeated American society (Ravitch, 2001, pp. 383-393). In large part, the dropout problem became a medium to express anxiety about a wide range of perceived social ills, including juvenile delinquency, unemployment, and urban poverty (Dorn, 1996).

The new unquestioned primacy of the high school diploma has continued to be reinforced by economic and technological advances and heightened geopolitical competition. Policy makers have long worried about automation in the labor force brought about by new technologies and the elimination of unskilled jobs for future dropouts (Dorn, 1996; Rumberger, 2011). Early research supported the popular belief that the high school diploma was an increasingly necessary prerequisite to maintain a home, support a family, and live independently in the United States (Barclay & Doll, 2001). Priorities of educational systems followed labor market demands; as requisite skill levels for many jobs continued to rise, more expectations were placed on schools to train, socialize, and graduate an increasing number of students (Sherraden, 1986).

Technological advancements and economic globalization further underscored the link between universal secondary educational attainment and the nation's future (Ravitch, 2001; Reese, 2005). The highly influential 1983 report *A Nation at Risk* famously warned that educational mediocrity threatened American's political and economic security. While universal secondary educational attainment was previously viewed as economically inefficient (Kett, 1995), the increasing demand for an educated and skilled labor force forged a new consensus that has held to the present day: high school completion for all is more than a socially democratic ideal; it is a political and economic imperative (Reese, 1999, 2005).

More than a half a century after the emergence of the "dropout crisis", the problem of high school dropout remains a national policy priority. In 1990, the U.S. Department of Education formally declared a steep reduction in school dropout a national educational goal (National Education Goals Panel, 1995). Title I of the No Child Left

Behind Act (NCLB) mandates evaluation of dropout and graduation rates in yearly progress reports for all high schools (Linn, Baker, & Betebenner, 2002). Moreover, research and policy makers continue to advocate for stronger accountability of schools to prioritize school completion rates (Dorn, 2003; Losen, 2004; McNeil, Coppola, Radigan, & Heilig, 2008). Recognizing the important connection between educational attainment and health, the Department of Health and Human Services has also declared raising on-time high school graduation an important policy objective (U.S. Department of Health and Human Services, 2013).

1.2.2 *Consequences*

Much empirical research has reinforced historical concerns about the future well-being of high school dropouts. Overall, individuals who dropout confront poor economic prospects throughout their lifetimes and are substantially more likely than graduates to engage in crime, receive public assistance, bear children outside of marriage, experience civic disengagement, suffer from poor health, and have shorter life spans[1]. Because many factors are likely to influence educational attainment and adult outcomes, these associations may overestimate the true effects of dropping out. However, rigorous evaluation using twin-studies and natural experiments, strongly suggest that the economic benefits of a high school education are real (Ashenfelter et al., 1999; Card, 1999; Carneiro et al., 2011; Leigh & Ryan, 2008; Staneka, Iaconoa, & McGuea, 2011). As compared to high school graduates with no post-secondary education, high school dropouts fair substantially worse in all labor market outcomes, regardless of race/ethnicity or gender (U.S. Department of Education, 2011a, 2013a). They are

[1] See Rumburger (2011, pp. 86-129) for a recent review of the associations between school dropout and the economic, crime, health, family formation, and civic engagement outcomes during adulthood. The discussion that follows is largely influenced by the contents therein.

substantially less likely to participate in the labor market; if in the labor market, they are less likely to be employed; if employed, they are less likely to earn a living wage. The economic disadvantages associated with dropping out of high school also appear to be increasing over time, likely because of the reduction in low-skilled, well-paying jobs (U.S. Department of Education, 2011a, 2013a).

The link between school dropout and criminal justice involvement is also widely documented (Pettit & Western, 2004; Wakefield & Uggen, 2010). According to official reports, two in three of those incarcerated in state prisons are high school dropouts (U.S. Department of Justice, 2003). These figures are compounded by racial inequalities in the criminal justice system: 60% of African American males who dropout of high school can be expected to serve a prison sentence at some point in their lives (Pettit & Western, 2004). Studies also suggest that much of these associations are causal. Using education compulsory school laws as an instrument to estimate the returns to education, Lochner and Moretti (2004) provide among the strongest evidence to date that obtaining a school degree greatly reduces the likelihood of crime, arrest, and incarceration. Moreover, reviews of adult correctional programs that help inmates obtain a high school diploma show a sharp reduction in criminal recidivism, more so than any other rehabilitative program (Jensen & Reed, 2007).

Research also indicates that the failure to receive a high school diploma is extremely detrimental for long-term health. The relationship between educational attainment and health is considered among the most well-established findings in social science research (Conti et al., 2010). Those with the lowest levels of educational attainment are most likely to experience chronic illness and early mortality. These

differences persist even after accounting for a wide variety of relevant socio-economic and health behaviors and use of compulsory education laws as instrumental variables (Lleras-Muney, 2005).

1.2.3 *Prevalence*

It is not clear the extent to which national policy goals to reduce school dropout have been met. National estimates of high school attainment are complicated by methodological limitations, varying conceptualizations of the phenomenon, and different measurement approaches. Graduation and dropout rates have long been a source of controversy and debate (see Miao & Haney, 2004; Swanson & Chaplin, 2003; Tyler & Lofstrom, 2009; Warren, 2005; Warren & Halpern-Manners, 2007). Most often, dropout is described as a *status* – the lack of a high school credential among those not enrolled in school. However, dropout can also be understood as an *event* – the official withdrawal from school or the consecutive absence from school over a sustained time period, regardless of current status. The most frequently cited figure from the National Center for Education Statistics (NCES) reports status dropout rates as the percent of the 16- to 24-years old population who are not enrolled in school and have not earned a high school degree (U.S. Department of Education, 2013a, p. 128).[2] This cross-sectional approach suggests the prevalence of status dropout declined from 12 percent in 1990 to 7 percent in 2011. Alternatively, the rate of event dropout is calculated by NCES as the percent of high school students who left high school between the beginning of one school year and the beginning of the next without earning a degree. These annual rates have also declined

[2] All national NCES statistics are produced using data from Current Population Survey (CPS) and American Community Survey (ACS).

substantially over the past decades to a low of around 3.5 percent (U.S. Department of Education, 2009, 2013b).

However, these rates can be misleading; there are several important limitations of the widely cited NCES dropout estimates. First, they are cross-sectional measures. Many students currently enrolled in school (who do not meet the definition of status dropout) will dropout. Alternatively, many persons not currently enrolled in school who lack a high school degree (who meet the definition of status dropout) will return to complete their education. Second, exclusion of the institutionalized population, particularly those incarcerated, underestimates these national dropout rates (Heckman & LaFontaine, 2010). Third, an increasing number of young adults who leave school earn a General Educational Development (GED) credential (U.S. Department of Education, 2011b). Official NCES statistics do not count GED recipients as dropouts; however, research has long shown that the GED is essentially "nonequivalent" (Cameron & Heckman, 1993); recipients fare substantially worse than traditional graduates in labor market outcomes and post-secondary education attainment (Boesel, Alsalam, & Smith, 1998; Tyler, 2003; Tyler & Lofstrom, 2010). Researchers have attempted to correct for these limitations by adopting a unified measurement approach using multiple longitudinal datasets and excluding GED recipients as graduates (see Heckman & LaFontaine, 2010). These findings are much less promising than official NCES reports suggest and indicate that graduation rates have not improved over the last fifty years. As many as one in five students continue to dropout from high school (Heckman & LaFontaine, 2010).

1.3 Dropping out as an individual process

While the prevalence of dropout is measured based on a current status or observable event, the ontology of dropout is more often understood by theorists as an unfolding process of progressive academic withdrawal from school institutions (Rumberger, 2011, pp. 58, 148). A comprehensive understanding of this process is necessary to better understand how ecological contexts such as neighborhoods shape and constrain student educational opportunities. Often set in motion early in a student's academic career (Alexander, Entwisle, & Kabbani, 2001), the many potential manifestations of withdrawal that ultimately culminate in leaving school early are generally referred to as school disengagement (J.D. Finn & Zimmer, 2012; Fredricks, Blumenfeld, & Paris, 2004; Jimerson, Campos, & Greif, 2003). Despite great variation in the conceptualization and measurement of school engagement (see Appleton et al., 2008), the construct broadly encompasses dimensions of behavior (e.g., appropriate participation in school activities), cognition (e.g., effort and persistence in learning), and emotion (e.g., positive affective responses to school institutions) (Fredricks et al., 2006; Fredricks et al., 2004).

Nearly all theoretical perspectives on school dropout have incorporated constructs related to multiple dimensions of school (dis)engagement. For example, Finn's (1989) widely cited frustration-self-esteem model highlights how unsuccessful school outcomes reinforce low self-esteem, undermining academic effort (i.e., cognitive disengagement) and escalating in-school problem behavior (i.e., behavioral disengagement). Alternatively, the participant-identification model emphasizes how active participation in school activities (i.e., behavioral engagement) promotes successful school outcomes and

development of a sense of belonging to and valuing of school institutions (i.e., emotional engagement) (Finn, 1989). Similarly, the framework proposed by Wehlage and colleagues (1989) claims that dropout is caused by lack of *school membership* (e.g., low levels of social ties and commitment to the value or legitimacy of school institutions) and *educational engagement* (e.g., experience of the intrinsic and extrinsic rewards of the academic curriculum), each corresponding to components of emotional and cognitive school disengagement, respectively. These two dimensions are also comparable to the key constructs of *social (dis)integration* and *academic (dis)integration* proposed by Tinto (1975, 1987). Altogether, theory specific to school attrition has consistently viewed multiple forms of commitment to and involvement in academic institutions as more than mere proximate determinates of future withdrawal. These emotional, cognitive and behavioral manifestations of disengagement are conceptualized as part of the very dropout process itself, often self-reinforcing and inseparable from the final occurrence of school disenrollment.

1.4 Neighborhoods and the dropout process

1.4.1 *Review of institutional factors*

Multiple measures of school disengagement are not only highly predictive of future dropout (Wang & Fredricks, 2013), they are also presumed responsive to variations in the environment and therefore malleable to policy and practice intervention (Appleton et al., 2008; Christenson & Thurlow, 2004; Fredricks et al., 2006). Uncovering these underlying causal determinates of school withdrawal is widely acknowledged as complex and multifaceted. Students may become disengaged and dropout for many different reasons, as supported by in-depth qualitative inquiry and the wide variation of

explanations provided by students who recently dropped out (Bradley & Renzulli, 2011; Dalton, Glennie, & Ingels, 2009; LeCompte & Dworkin, 1991; Smyth & Hattam, 2004).

However, theoretical perspectives that underline the contexts in which students and adolescents develop help clarify how environmental influences may activate (or reverse) school withdrawal processes. Such integrated frameworks commonly adopt an ecological and developmental perspective modeled after Bronfenbrenner (1979) in which interactions between family, peer group, work, school, and community systems work together to either support or undermine school completion (see Jozefowicz-Simbeni, 2008; Rosenthal, 1998). A fully developed framework with respect to school dropout is illustrated by the widely cited conceptual model proposed by Rumberger and Lim (2008): institutional factors, such as families, schools, and communities (i.e., residential and neighborhood contexts) influence individual factors, such as students' background, attitudes, behaviors, and ultimately their educational performance and attainment.[3] The peers with whom students affiliate have also been highlighted as an important ecological context determinative of school completion (Battin-Pearson et al., 2000; French & Conrad, 2001; Jozefowicz-Simbeni, 2008; Rosenthal, 1998; Staff & Kreager, 2008). In addition, institutional factors – communities, schools, peer groups, and families – are understood as dynamic, directly and indirectly interacting with each other to impact all individual factors, including dropout (Rumberger & Lim, 2008). This complex and recursive ecological set of interactions creates challenges for hypothesis formation and empirical testing. A more precisely articulated conceptual framework is required in order

[3] While peer and employment characteristics have been previously characterized as environmental domains (see Jozefowicz-Simbeni, 2008), the model proposed by Rumberger and Lim (2008) includes peer affiliation and employment within the domain of student behaviors.

to better illuminate the influences of ecological context on the process of high school withdrawal.

1.4.2 *Residential context as a critical institutional factor*

Supported by ample theoretical and empirical support, the current study conceptualizes neighborhood residence as a critical, and in many ways, exogenous ecological context that sets in motion processes of school disengagement and school withdrawal. Neighborhood residence largely shapes and often directly determines the contexts of the remaining "ecological systems" (Jozefowicz-Simbeni, 2008) or "institutional factors" (Rumberger, 2011), all of which are influential on school dropout. In regard to school contexts, selection into public schools is usually determined by geographical residence; nearly three fourths of all U.S. students attend an assigned public school (U.S. Department of Education, 2013a). Even among children afforded the opportunity to attend private schools, geographical residence largely determines the private schools from which to select. Therefore, school contexts can in large part be understood as a mechanism through which neighborhood residence impacts youth outcomes. Second, the peers to whom youth are exposed and with whom they interact and form relationships are also largely determined by the residential surroundings in which they grow up. Because youth disproportionately come in contact with peers from their surrounding neighborhoods and schools, the effects of peers is often conceptualized as an indirect influence of residential context (Crane, 1991; Gephart, 1997; Small & Newman, 2001). Finally, the family context of children is also shaped by place. The formation, functioning, and economic well-being of families have all been theorized as intermediate mechanisms through which disadvantaged residential contexts undermine

educational achievement (Brooks-Gunn et al., 1997b; Browning, Leventhal, & Brooks-Gunn, 2005; Leventhal & Brooks-Gunn, 2000) and reinforce persistent structural inequalities across generations (Sharkey, 2013; Sharkey & Elwert, 2011). Integration of these perspectives compels a conceptual framework where residential context represents more than just another distinct institutional factor or ecological system. Instead, the effects of neighborhoods work through many other institutions and systems, expanding or constraining the types of social, economic, and educational experiences determinative of school success.

1.4.3 *Neighborhood disadvantage and contextual mechanisms*

A wide variety of neighborhood features have been hypothesized to affect developmental outcomes of children and adolescents, including their educational achievement and attainment. The most widely studied neighborhood feature is neighborhood disadvantage, often referred to as neighborhood poverty, concentrated disadvantage, or neighborhood socio-economic disadvantage (used interchangeably hereafter). Neighborhood disadvantage is commonly operationalized as the percent of neighborhood residents who live in poverty or an aggregate measure of correlated economic characteristics. While compositional characteristics of neighborhoods are generally understood by scholars as imperfect proxies for a host of dynamic neighborhood processes, aggregate measures of socio-economic characteristics have historically dominated empirical inquiry (George C Galster, 2012; Sampson, 2012; Small & Newman, 2001). This line of scholarship was largely motivated by the rise in poverty concentration starting around 1970. Demographers observed sharp increases in both the number of neighborhoods with high rates of poverty and the proportion of low-income

families who resided in poor neighborhoods (Jargowsky, 1997; Quillian, 1999), which has continued over time (Fry & Taylor, 2012).

In response to these trends, sociologists and demographers hypothesized that spatial concentration of poverty restricted individual opportunity of poor families and exacerbated social and economic inequality (Jencks & Mayer, 1990). The empirical work that followed has generally supported this hypothesis. A consensus emerged that residence in high-poverty neighborhoods during childhood and adolescence was related to lower educational and occupational achievement (Brooks-Gunn, Duncan, & Aber, 1997a; Brooks-Gunn et al., 1997b; Brooks-Gunn et al., 1993; Crane, 1991; Leventhal & Brooks-Gunn, 2000). In addition, more recent scholarship offers stronger support and more methodologically rigorous estimates for making statements about causality (George C. Galster, Marcotte, Mandell, Wolman, & Augustine, 2007; Harding, 2003; Sharkey, 2012; Sharkey & Elwert, 2011; Wodtke et al., 2011). Therefore, scholarly work has long focused on explaining why the economic characteristics of neighboring residents are important determinates of individual educational outcomes. Such theory is commonly discussed and presented within the context of research on poverty, economic inequality, and racial disparities, particularly within urban settings (e.g., see Jencks & Mayer, 1990; Leventhal & Brooks-Gunn, 2000; Leventhal, Brooks-Gunn, & Kamerman, 1997; Sampson & Sharkey, 2008; Sharkey, 2008; Small & Newman, 2001). Uncovering the mechanisms through which concentrated economic disadvantage undermines the educational attainment of disadvantaged youth has the potential to inform and target policy intervention.

As will be reviewed in the following chapter, theories on neighborhood effects are multidimensional and seemingly dissimilar perspectives often share fundamental premises or complement each other within a larger causal framework. The earliest categorization of theoretical models was offered by Jencks and Mayer (1990) in *The Social Consequences of Growing Up in a Poor Neighborhood*. The authors identified three "schools of thought" about how poor neighborhoods negatively impact youth development. First, *epidemic* models point to the influence of neighboring peers and assume youth attitudes and behaviors conform to local peer group norms. Second, *collective socialization* models focus on non-parental adults in the neighborhoods as influential role models and enforcers of social control. Third, *institutional* models suggest that teachers, police offers and other adult authority figures respond to youth according to their neighborhood affiliation, discriminating against those who live in poor neighborhoods.

Other scholars have described different ways of thinking about the impacts of living in poor neighborhoods. Small and Newman (2001) describe the three models proposed by Jencks and Mayer (1990) as "socialization" models because they focus on how neighborhood residence shapes the nature of one's relationships and social interactions; where one lives, effectively determines how one is socialized. "Instrumental" models, however, focus on how individual agency is limited by the neighborhood environment (Small & Newman, 2001). For example, theorists have argued that adults in poor neighborhoods are spatially isolated from employment opportunities, disconnected from social networks of employed people (W. J. Wilson, 1987), and deprived of local institutional resources, such as quality schools, daycare

centers, and other social services (Brooks-Gunn et al., 1997b). Scholars have also maintained that racial discrimination undermines the ability of poor, predominately African American communities from forging political alliances necessary for community enrichment (Massey & Denton, 1993). Collectively, these structural constraints undermine positive child and adolescent development, including educational attainment.

The following chapter establishes a specific framework for the current study by categorizing these models into three distinct theoretical perspectives. The categorization includes: (1) *resource deprivation*, which emphasizes unequal access to quality institutional resources, particularly schools; (2) *social organization*, which emphasizes diminished neighborhood social cohesion and means of informal social control, particularly control of youth behavior; and (3) *social isolation*, which emphasizes the emergence and social transmission of alternative cultural norms that devalue educational achievement, particularly within peer groups. This theoretical typology has been adopted previously to discuss how neighborhood poverty impacts high school completion (Wodtke et al., 2011). Similar "classes" of neighborhood mechanisms are also categorized by Sampson et al. (2002) in their seminal theoretical review on neighborhood research. Whether through means of socialization or structural constraint, each of the theoretical mechanisms described is hypothesized to exert independent explanatory effects on school dropout; collectively, they are hypothesized to link the economic characteristics of neighborhoods with individual prospects of school completion.

These theories are reviewed in greater depth and theoretical constructs applicable to each perspective are delineated. Next, methodological challenges estimating neighborhood effects are discussed and relevant findings and approaches used to study

educational attainment are reviewed, including heterogeneity in neighborhood effects. Finally, prior studies that have examined mechanisms through which neighborhood disadvantage impacts educational achievement and attainment are reviewed.

2. Conceptual Framework and Prior Literature

2.1 Theoretical perspectives on mechanisms of neighborhood disadvantage

2.1.1 *Theories of resource deprivation*

Theories of resource deprivation argue that neighborhoods vary in the concentration and quality of educationally supportive institutions, such as schools, daycare centers, civic organizations, health services, and voluntary youth-serving organizations. More specifically, children and adolescents from poor neighborhoods are deprived access to these institutional resources at the same level or quality as their counterparts from more economically advantaged neighborhoods. This perspective is best described by and widely accredited to the scholarly work of Brooks-Gunn and fellow colleagues (1997a, 1997b). Within this volume of research, schools are described among the most vital institutions through which neighborhoods exert influence on youth educational outcomes (Halpern-Felsher et al., 1997, pp. 180-181). This perspective compliments longstanding sociological theory about educational systems as sites of social reproduction (Collins, 2009). Rather than serving as popularly purported institutions of equal opportunity and social mobility, scholars have long claimed that unequal schooling experiences systematically maintain and often reinforce social inequality across generations (Bourdieu & Passeron, 1977; Bowles & Gintis, 1976; Jencks, 1972). Historically, policy makers and scholars have long claimed that if high quality primary and secondary schooling were to be afforded to those most disadvantaged, socio-economic disparities in educational achievement and attainment could be narrowed considerably or closed completely (Dobbie, Fryer, & Fryer, 2011; Dorn, 1996).

Theories of resource deprivation therefore assume: (1) children who grow up in poor neighborhoods also attend lower quality schools: and (2) school quality promotes positive educational outcomes for youth, including high school completion. If both these claims were substantiated, scholars would be compelled to integrate both the effects of neighborhoods and schools into a more unified body of theory, acknowledging that much of the effect of growing up in a poor neighborhood that is commonly attributed to deprived neighborhood environments may actually be explained by deprivation of quality schools. Therefore, children who grow up in poor neighborhoods may be best served through policies that work to directly enhance the quality of their schools.

Regarding the first assumption, no analysis of national data can be found that systematically compares measures of school quality attended by children according to the socio-economic characteristics of their neighborhood residence. Given that school enrollment and funding are both largely determined by geography, this relationship usually goes assumed in both statement of theory and motivation for empirical analysis (e.g., Balfanz & Legters, 2004; Leventhal & Brooks-Gunn, 2000; Wodtke et al., 2011). On the other hand, there is substantial support for the second assumption – that structural characteristics of schools are related to individual educational outcomes, including school dropout. One important descriptive clue is large variation in dropout rates across schools. Like within neighborhoods, school dropout is highly concentrated within select schools; these schools are popularly described by scholars and policy makers as "dropout factories" (Balfanz & Legters, 2004; A. Duncan, 2012; Tucci, 2009; Zehr, 2010). Despite a gradual decline in the number of schools with high dropout rates, a large proportion of the total dropouts each year still come from a small proportion of the total high schools;

around one in ten students attend schools where dropping out remains the norm, including as many as one in four black students and one in six Hispanic students (Balfanz, Bridgeland, Bruce, & Fox, 2013). Analysis of data from longitudinal studies also suggests that schools are important predictors of school dropout. Some estimates indicate that the school one attends explains nearly a quarter of the variability in one's likelihood to graduate high school (Li, 2007; Rumberger & Palardy, 2004).

Prior literature suggests that many characteristics of schools amenable to policy intervention are protective against school dropout. These school features are generally categorized as resources (e.g., financial expenditures, teacher qualifications, academic curriculums, provision of supplemental services), structures (e.g., size, organization), or policies and practices (e.g., school disciplinary policies, relations among students and teachers) (Rumberger, 2011, pp. 193-199). Findings are mixed with respect to the effect of structural characteristics, such as school size and organization, on school dropout. For example, "small school reforms" were motivated by positive student outcomes observed within select small sized schools, reversing much of the historical trend in expanding high school size (Gardner, Ritblatt, & Beatty, 2000; Vander Ark, 2002; Wasley & Lear, 2001). However, not only do schools experience challenges when downsizing (Raywid, Schmerler, Phillips, & Smith, 2003), findings on the importance of school size have been mixed. Some studies confirm that risk of dropout increases consistently with school size (Werblow & Duesbery, 2009); net of other school features, many studies have found no association (Bryk & Thum, 1989; R. B. McNeal, Jr., 1997; Van Dorn, Bowen, & Blau, 2006); while others suggest medium sized schools may be most protective (V. E. Lee & Burkam, 2003). School type, another widely studied structural characteristic, has also

produced inconsistent findings. As compared to public schools, private high schools (particularly Catholic schools) were long held to better individual prospects of school completion (Coleman & Hoffer, 1987; Rumberger & Thomas, 2000). On the other hand, research also points to no differences between public and private schools in the likelihood of dropout when other school-level variables are held constant (i.e., school composition, resources, policies, practices) (V. E. Lee & Burkam, 2003).

The effect of school resources, policies, and practices are substantiated by more consistent research findings. For example, financial resources (e.g., school district expenditures), lower student-to-teacher ratios, and higher quality teachers (e.g. teachers' experience, credentials, and salaries) have been found to be negatively associated with school (V. E. Lee & Burkam, 2003; Li, 2007; Loeb & Page, 2000; R. B. McNeal, 1997; Pirog & Magee, 1997; Rumberger & Thomas, 2000). Scholars have also highlighted school policies and practices as important determinates of successful school completion. Ethnographic work in poor urban communities has suggested that rather than students dropping out, schools work to effectively "push out" the most disadvantaged students (Fine, 1985, 1986, 1991). These practices are believed to have been exacerbated within under-performing schools due to accountability reforms that prioritize average standardized test scores of retained students over student retention and school completion (Allensworth, 2005; Dorn, 2003; Lipman, 2004). While some research has shown that strict disciplinary policies protect students from school dropout (Babcock, 2009), other empirical work strongly suggests that "zero tolerance" disciplinary policies that rely heavily on suspension and expulsion, more prevalent in schools located in poor urban communities, increase risk of dropout by further disengaging students from school

systems (T. Lee, Cornell, Gregory, & Fan, 2011; Noguera, 2003a, 2003b; Skiba & Knesting, 2002). On the other hand, when school practices are able to foster positive relations between students and teachers, and teachers are empowered with more decision making authority over academic curriculums and student discipline, pedagogical theory and empirical inquiry suggest that students are more likely to complete high school (Bradley & Renzulli, 2011; Croninger & Lee, 2001; Fall & Roberts, 2012; Rumberger & Palardy, 2005b).

2.1.2 *Theories of social organization*

Theories of social organization broadly assert that social integration and informal systems of social control are necessary for neighborhoods to realize common goals and solve chronic problems. Structural conditions, such as high poverty, residential mobility, and ethnic heterogeneity, are believed to undermine social organization, which in turn influences social outcomes over and above the characteristics of individual residents (Kubrin & Weitzer, 2003). Contemporary perspectives do not presume that poor neighborhoods are uniformly "disorganized" (Sampson, 2012); however, measures of social organization remain understood as important mechanisms that partially mediate the effects of neighborhood poverty on social outcomes, particularly behavior in violation of social norms, such as juvenile delinquency and crime (Kubrin & Weitzer, 2003; Sampson et al., 2002).

The theory described as social *dis*organization was introduced by Shaw and McKay (1942) after discovering that high rates of juvenile delinquency within Chicago neighborhoods persisted even during periods of racial and ethnic change. The authors concluded that distinct social structures (i.e., levels of disorganization) led to the

intergenerational transmission of crime within neighborhoods. Later sociologists have continued to theorize neighborhood-level variables that constitute levels of disorganization. Two constructs have been highlighted: disrupted or weakened systems of friendship, kinship, and acquaintanceship networks; and the inability to informally maintain social controls and regulate deviant behavior (Bursik, 1988; Kornhauser, 1978; Meier, 1982; Stark, 1987). Sampson and colleagues have built upon and refined these constructs. The theory of collective efficacy that emerged joins two related processes: *social cohesion* (the "collectivity" part of the concept) and *shared expectations for control* (the "efficacy" part of the concept) (Sampson, 2012, pp. 161-162). The theoretical construct departs from previous measures of disorganization in that it does not presume dense, intimate, or strong social ties to be necessary conditions for social integration. However, like previous conceptualizations, collective efficacy recognizes informal social control as a communal process causally related to variations in individual behaviors observed across neighborhoods. Collective efficacy theory operationalizes informal social control as neighbors' willingness to respond to social problems, particularly intervening when confronted with youth misbehavior (e.g., when youth get into fights, loiter, or skip school). Shared expectations of adult intervention serve as an informal system of social control that disincentivizes youth misbehavior, delinquency, and crime (Sampson, 2012).

In addition to the collective efficacy of neighborhoods, another related theoretical construct also underscores the importance of the capacity of neighborhoods to informally regulate and control youth behavior. Intergenerational closure, a construct often presented as complimentary (Harding, 2011) to theories of social organization, stems from social

capital theory (Coleman, 1990) and is understood as the extent to which parents know and remain in contact with the parents of their children's friends. When networks of parents experience closure, common expectations and norms can be enforced through use of sanctions and rewards; however, when closure is absent, parents are less able to guide and constrain their children's behavior (Coleman, 1990; Coleman & Hoffer, 1987). While intergenerational closure is often studied as a form of social capital unique to individual families (e.g., Carbonaro, 1998), scholars have also operationalized the construct to extend to community environments, such as within schools (Morgan & Sørensen, 1999) and neighborhoods (Harding, 2009, 2011). Other studies have even measured the concept as an indicator of neighborhood collective efficacy (Browning et al., 2005). Like collective efficacy, the role of intergenerational closure is therefore consistent with theories of social organization in that informal regulation and control of youth behavior is expected to translate into improved social and educational outcomes.

While theories of social organization have their historical roots in the study of crime, the ecological constructs that emerged have been widely applied across disciplines to study the effects on health, development, and educational outcomes of children and adolescents. Moreover, given the strong link between juvenile delinquency and school dropout (Battin-Pearson et al., 2000; Ou, Mersky, Reynolds, & Kohler, 2007; Suh, Suh, & Houston, 2007; Sweeten, 2006), neighborhood mechanisms of informal social control that reduce deviant or anti-social behavior of youth may also be expected to increase prospects of school completion (Crowder & South, 2011; Harding, 2009; Wodtke et al., 2011). Collectively, two hypotheses derived from theories of social organization can be made about neighborhood disadvantage and educational attainment. First, poor

neighborhoods are more likely to be characterized by low levels of collective efficacyand diminished intergenerational closure. Second, these indicators of weakened social organization undermine positive social and educational outcomes for youth, including high school completion. If these claims were jointly substantiated by empirical evidence, the educational prospects of children from poor neighborhoods would likely be improved through policy supports to poor communities that bolster social cohesion and informal social control and strengthen connections between parents.

Regarding the first premise, there is some empirical evidence to demonstrate that poor neighborhoods are less likely to possess the protective social structures outlined by theories of social organization. At least within select urban areas, neighborhood poverty is related to lower collective efficacy; longitudinal analyses suggest a "feedback loop" where these two neighborhood characteristics reinforce each other over time (Sampson, 2012). The extent to which intergenerational closure may be related to neighborhood structural characteristics is less apparent. However, theorists and ethnographic study do point to tightly interconnected social networks within very poor neighborhoods (Pattillo, 1998; St. Jean, 2008; Venkatesh, 1997; W. J. Wilson, 1996). While such social ties are not necessarily protective in all contexts, the density of the ties described suggests that social cohesion and intergenerational closure in some poor neighborhoods may actually rival or exceed more affluent neighborhoods.

Regarding the second premise, evidence generally supports that neighborhood collective efficacy protects against crime, juvenile delinquency, and youth behavioral problems, such as aggression, substance use and risky sexual behavior (Browning, Burrington, Leventhal, & Brooks-Gunn, 2008; Browning, Dietz, & Feinberg, 2004;

Erickson, Harrison, Cook, Cousineau, & Adlaf, 2012; Morenoff, Sampson, & Raudenbush, 2001; Odgers et al., 2009; Sampson, Raudenbush, & Earls, 1997). Neighborhood collective efficacy is also associated with physical health and psychological well-being (Browning & Cagney, 2002; Cohen, Finch, Bower, & Sastry, 2006), both related to high school completion (Daniel et al., 2006; Farahati, Marcotte, & Wilcox-Gök, 2003; Roebuck, French, & Dennis, 2004). The construct of collective efficacy has been adapted to study teacher and school environments (Adams & Forsyth, 2006; Goddard, Hoy, & Hoy, 2004) and similar measures of neighborhood social cohesion and informal social control are shown to support educational behaviors and achievement (Bowen, Bowen, & Ware, 2002; Nash, 2002). However, no study to date has provided a focused examination of neighborhood collective efficacy on school dropout. The intergenerational closure of individual parents does appear to decrease their children's dropout risk (Carbonaro, 1998); however findings are inconclusive when measured as the average level of family intergenerational closure within neighborhoods (Harding, 2009, 2011).

2.1.3 *Theories of social isolation*

Theories of social isolation claim that children and adolescents from poor neighborhoods are spatially isolated from supportive social networks that uphold norms about the importance of educational and occupational attainment. Variants of this perspective are often described as collective socialization (Gephart, 1997; Jencks & Mayer, 1990), contagion, or epidemic models of neighborhood effects (Crane, 1991). Collectively, these perspectives are hereafter described as theories of social isolation because they share the presumption that values, aspirations, and behavioral norms

systematically vary across neighborhoods and are reinforced through social ties and interactions. They also uphold the importance of social capital and shared social norms within social structures (Coleman, 1988, pp. 116-117; 1990; Loury, 1987). Theories of social isolation do *not* claim, however, that children from poor neighborhoods are isolated from any social relations; rather, they are presumed isolated from normative social relations supportive of educational and occupational achievement. As a result, alternative or deviant subcultures distinct from "mainstream" or middle-class cultures emerge to create and reinforce place-based inequalities (Anderson, 1999; Massey & Denton, 1993; W. J. Wilson, 1987).

The concept of social isolation was first advanced by W. J. Wilson (1987) in *The Truly Disadvantaged.* Wilson described the hazards of living primarily among neighbors of deep social and economic disadvantage. He emphasized that children from poor neighborhoods often lack "conventional" non-parental mentors to encourage success in school through structuring norms modeled by educated working adults. Building upon Wilson's work, Massey and Denton (1993) also highlighted that oppositional attitudes and behaviors emerge and are reinforced through peer relationships. Structural antecedents are not ignored; they remain central to the thesis of social isolation and related perspectives. In particular, institutional racism (Massey & Denton, 1993) and mass unemployment brought about by urban deindustrialization are underscored (W. J. Wilson, 1996). In response to constraint and isolation, "oppositional" and "ghetto specific" cultural repertories are believed to emerge, reinforcing structural disadvantages (Massey & Denton, 1993; W. J. Wilson, 1987, 1996).

While a host of counter-cultural norms are described, with a particular emphasis on violence and delinquency, the attitudinal and behavioral constructs most relevant to the study of school dropout are those that include dimensions of school disengagement. As discussed previously, sustained disengagement from school institutions is theorized to be part of the dropout process and a strong empirical predictor of school withdrawal (Appleton et al., 2008; Fall & Roberts, 2012; Fredricks et al., 2006; Rumberger & Lim, 2008). As it relates to school dropout, the following hypothesis may be derived from theories of social isolation: (1) youth in poor neighborhoods are more likely to be surrounded and affiliated by peers who are disengaged from school; and (2) the disengagement from school of surrounding and affiliated peers impacts the educational outcomes of youth, including high school completion.

Much ethnographic and theoretical work similarly describe the emergence of an "underclass" (Marks, 1991) in response to structural constraint. For example, Willis (1977) described British working-class boys reject meteoritic ideology of schools and foster an alternative culture in which opposition to school authority – not academic achievement – was of primary importance to peer status. Ethnographic work in U.S. urban communities has drawn similar conclusions about peer group norms in low-income neighborhoods and school failure (Anderson, 1999; MacLeod, 1987). Scholars have also theorized that oppositional peer cultures exacerbate racial disparities in educational attainment; some black students and other "involuntary minorities" reject academic achievement in order to command peer respect and out of fear of "acting white" (Downey, 2008; Fordham & Ogbu, 1986; Ogbu, 2004).

However, theories of social isolation and related cultural perspectives on place-based poverty have been subject to much critique and nuanced reinterpretation. As described by more recent scholarships evaluating research on culture and poverty (Small, Harding, & Lamont, 2010), both ethnographic and survey research has suggested that poor and minority families (both children and their parents) often profess strong educational aspirations and traditional views about the importance of educational achievement (Ainsworth-Darnell & Downey, 1998; A. L. Harris, 2006; Newman, 2009; Young, 2006). As a result, both Small (2004) and Harding (2011) reject the assumption of a single, cohesive sub-culture of poor neighborhoods; instead, they propose that poor neighborhoods are better characterized by cultural heterogeneity and the emergence of conflicting "constraint-and-possibility" cultural frames (Small et al., 2010). For example, Harding (2011) shows that adolescents who live in neighborhoods with greater diversity in college goals are less likely to follow through with their stated expectation to attend college. Collectively, this scholarship highlights the potential limitation and explanatory power of theories of social isolation.

The link between peer context and school dropout appears to be less theoretically controversial and more empirically supported. As children transition into adolescence, peer groups become increasingly influential on motivation, engagement, and achievement in school (Jozefowicz-Simbeni, 2008; A. M. Ryan, 2000) – all strong determinates of school completion. Research has generally found that student academic outcomes are affected by the academic behaviors and achievement of their peers (see reviews by Epple & Romano, 2011; Sacerdote, 2011). Youth who are affiliated with schoolmates and peer groups characterized by higher levels of school disengagement and delinquent behaviors

are consistently shown to be at higher risk of school dropout (Battin-Pearson et al., 2000; Carbonaro, 1998; French & Conrad, 2001; Neild, Stoner-Eby, & Furstenberg, 2008; South, Baumer, & Lutz, 2003; Staff & Kreager, 2008). Therefore, while the nature of variation in peer contexts across neighborhoods may be ambiguous, the contribution of peer contexts on school dropout is generally uncontested.

2.2 Empirical literature on neighborhood disadvantage and school dropout

2.2.1 *Methodological challenges*

Prior research pertaining to neighborhoods and educational attainment has primarily sought to produce causal estimates of neighborhood disadvantage. More specifically, these studies examine differences in educational attainment of children and young adults who resided in varying levels of poor and non-poor neighborhoods during their childhood and adolescence. However, before reviewing the relevant empirical literature, it is necessary to briefly discuss some of the most noteworthy methodological challenges in this pursuit. The first question is simply how to define and measure a neighborhood. Three related issues are involved in the definition of neighborhoods: conceptualization; boundary definition, and measurement of features (Small & Newman, 2001). Neighborhoods may be understood as non-geographical systems, such as social and institutional networks in which membership and commitment varies along a continuum (Chaskin, 1997). However, the empirical literature has more consistently conceptualized neighborhoods as spatial units, often nested within larger, sometimes overlapping communities and regions. Residents and institutions with distinct social characteristics reside within these geographical boundaries (Sampson, 2012; Sampson et al., 2002; Small & Newman, 2001). Mostly because of data limitations, neighborhood

geography is commonly defined according to Census Tracts boundaries – small, relatively permanent statistical subdivisions of counties generally following visible and identifiable features[4] - or other Census-based or administrative boundaries, such as U.S. Postal ZIP codes or PUMA boundaries. Debates over the appropriate geographical definition of the neighborhood are increasingly less common; recent scholars call for a flexible approach to definition most appropriate to the neighborhood-level phenomena or residential context under study (Sampson, 2013; Sharkey & Faber, 2014a). Measurement of neighborhood features is also an ongoing empirical challenge. Socio-economic compositional characteristics can be easily obtained from Census data; however, data on latent characteristics of neighborhoods, such as cultural norms and informal social processes, are rarely available and not easily estimated from secondary data sources. As a result, the mechanisms through which compositional characteristics such as neighborhood poverty, affect individual well-being often go unobserved (George C Galster, 2012; Sampson, 2012; Sampson et al., 2002; Sharkey & Faber, 2014a).

Beyond measurement, valid estimation of neighborhood effects in observational study is made difficult by several sources of bias. Like other environmental contexts, whether by choice or structural constraint, individuals and families are not randomly distributed across neighborhoods. As such, the characteristics of individuals, families, and other environmental contexts vary considerably across place and between poor and affluent neighborhoods. Without appropriately accounting for this variation, simple comparisons of youth outcomes in poor and affluent neighborhoods is likely to

[4] Census Tracts are updated prior to each decennial census and generally have a population size between 1,200 and 8,000 people, with an optimum size of 4,000 people. Boundaries are delineated with the intent to be maintained and compared across time. Census tracts occasionally are split due to population growth or merged due to population decline. Refer to U.S. Department of Commerce (2013) for more information regarding Census Tract definitions.

inappropriately attribute influence of neighborhood poverty; other sources of variation, not directly influenced by neighborhood poverty, may actually be responsible for differential outcomes observed. In observational studies generally, this confounding source of variation is often referred to as selection or "omitted variable" bias, which has long remained a major methodological critique of neighborhood research, overall (G. J. Duncan & Raudenbush, 1999; Ellen & Turner, 1997; George C. Galster, 2008; Sampson et al., 2002; Winship & Morgan, 1999). On the other hand, observable characteristics of youth, parents, and families (e.g., individual stress, parenting practices, family formation, economic opportunity, household income), all which reinforce each other over generations, are hypothesized to be directly impacted by the neighborhood environment, as well (Sharkey, 2008, 2013). Simply "controlling for" or "holding constant" these variables may underestimate true neighborhood effects that operate indirectly through the observed time-varying intervening variables. Scholars studying neighborhoods have described this problem as "included variable" bias (Sampson, 2012), also referred to as "over adjustment" or "unnecessary adjustment" bias within the epidemiology literature (Schisterman, Cole, & Platt, 2009).

2.2.2 *Effects on school dropout in experimental studies*

Several social experiments have been designed with the intention of overcoming the challenges of causal inference from observational study. These studies are best described as housing mobility programs because they involved examining the effects of families (usually poor families) moving out of poor neighborhoods (usually public housing projects). The first such social experiment was the Gautreaux Program administered by the nonprofit Leadership Council for Metropolitan Open Communities in

Chicago in 1981. Low-income black families receiving public assistance were assigned to different city (predominantly poor and black) or suburban (less poor and predominately white) locations in a quasi-random manner through distribution of Section 8 housing vouchers (Rosenbaum & DeLuca, 2008).[5] Early follow-up studies suggested that the school grades of children who moved to more affluent suburban neighborhoods fared worse because they experienced difficulties adapting to higher expectations in suburban schools and widely perceived racial discrimination (Rosenbaum, Kulieke, & Rubinowitz, 1988). However, by the time children became young adults, only 5 percent of the suburban movers had dropped out of high school, compared to 20 percent of those who stayed in predominantly poor city neighborhoods (Rosenbaum, 1995). Post-secondary education and employment outcomes were also substantially better for those who moved into more affluent suburbs during their childhood (Rosenbaum, 1995; Rubinowitz & Rosenbaum, 2000). While the magnitude of these comparisons was striking, many scholars were wary to draw causal inferences because mobility into affluent neighborhoods was not truly random (Clampet-Lundquist & Massey, 2008; Sobel, 2006).

Therefore, the Gautreaux study served as a motivation for the larger scale and more methodologically rigorous experimental mobility study – the US Department of Housing and Urban Development (HUD) Moving to Opportunity (MTO) demonstration (U.S. Department of Housing and Urban Development, 1996). Families from five major U.S. cities living in public housing were randomly assigned vouchers that facilitated

[5] Apartment availability was determined by housing agents who were unrelated to client interests. Units were offered to clients based on availability and client waitlist position. Few clients refused offers. Therefore, client characteristics and preferences were deemed marginally related to their mobility destination.

residential moves into lower poverty neighborhoods.[6] Unlike the Gautreaux study, however, the results have largely been mixed and appear to run counter to theory on mobility out of high-poverty urban neighborhoods. While long-term gains in adult health and subjective well-being were evident (Ludwig et al., 2012; Ludwig et al., 2011), no consistent impacts on children's educational outcomes were detectable (Ludwig et al., 2013). Early evaluations pointed to improved developmental outcomes for children who moved into lower-poverty neighborhoods, including higher test scores, better health and fewer behavioral problems (Ladd & Ludwig, 1997; Ludwig, Duncan, & Hirschfield, 2001). However, the beneficial educational effects for female youth (and surprising adverse effects for male youth) observed four to seven years following MTO-assisted moves (Kling, Liebman, & Katz, 2007) appear to have dissipated completely in later follow-up studies (Ludwig et al., 2013). Given the careful design of the MTO study, the null results came as a surprise to many policy experts and has led to divides in the social sciences about how to think about and empirically assess neighborhood effects (Clampet-Lundquist & Massey, 2008; Sampson, 2012).

However, while experimental studies such as the MTO demonstration address the central problem of selection bias, cautious interpretation of findings are warranted. Some scholars have critiqued the problem of "spillover" effects in which social interaction between the control and experimental groups may have biased estimates of treatment (Sobel, 2006). Others have critiqued the "narrow slice of the population" to which the

[6] The study operated in five cities - Baltimore, Boston, Chicago, Los Angeles, and New York. The sample consisted of families living in U.S. public housing projects and were randomly assigned into three groups: (1) the control group received no assistance but their eligibility for public housing continued; (2) a comparison group was offered traditional Section 8 housing vouchers; (3) the experimental group was provided Section 8 housing vouchers restricted to 1 year in a census tract with a poverty rate below 10 percent.

study can be generalized – very poor families with children living in public housing and in neighborhoods with high levels of poverty (Sampson, 2012). Finally, the actual MTO-"treatment dosage" – the change in neighborhood poverty– was substantially smaller than movers in the Gautreaux study or as compared to magnitudes estimated in observational studies. MTO-movers relocated in predominately racially segregated neighborhoods only marginally less poor than compassion groups; movers were also observed to return to more economically disadvantaged neighborhoods over time, gradually approaching the conditions of controls (Clampet-Lundquist & Massey, 2008; Ludwig et al., 2012; Sampson, 2008). The intervention has therefore been described as a short-term (and perhaps mild) "shock," rather than a long-term escape from high-poverty neighborhoods (Sampson & Sharkey, 2008; Sharkey & Faber, 2014a). As articulated by Sampson (2012), findings from the MTO study do not answer the basic research question of whether, how, or in what context neighborhood poverty impacts individual outcomes, but instead answers the very specific and narrow policy question:

> Does the offer of a housing voucher only redeemable in a lower-poverty neighborhood affect the later outcomes of the extreme poor... and those who have grown up in poverty, and may have already experienced its developmental effects? (Sampson, 2012, p. 265)

Despite earlier findings, the most recent research indicates that long-term positive outcomes can be detected among those who were relocated to less poor neighborhoods as young children. Chetty, Hendren, and Katz (2015) present new evidence using tax returns and earnings of children who participated in the MTO experiment. By their mid-twenties, children whose families took up an experimental voucher to move to a lower-poverty area when they were less than 13 years old were less likely to live in poor neighborhoods as adults, less likely to live in single parent families, and earned 31% (around $3,500)

more in annual income than the control group. These findings are consistent with research showing that long-term outcomes are influenced largely by the duration of exposure to neighborhood environments during childhood (Chetty & Hendren, 2015; Crowder & South, 2011; Wodtke et al., 2011)

2.2.3 *Effects on school dropout in observational studies*

Observational studies have taken advantage of nationally representative samples and wide variation in neighborhood conditions experienced across families and within families over time. Compared to experimental studies, observational studies have stronger external validity and can measure a variety of "dosage effects," including duration and intensity of exposure throughout the life course. Early research supported the hypothesis that adolescents living in the most disadvantaged neighborhoods experienced sharp increases in the risk of dropping out from school (Brooks-Gunn et al., 1993; Crane, 1991; Datcher, 1982; Jencks & Mayer, 1990).[7] Brooks-Gunn and colleagues (1993) first demonstrated that the income of neighborhood residents was strongly related to a wide variety of childhood, adolescence, and young adult outcomes, including school leaving, even after controlling for family socio-economic characteristics. Also, relationships between neighborhood socio-economic characteristics and child outcomes seemed to persist across different geographical boundaries.[8]

Later research built upon these tentative findings. The most comprehensive early series of studies was conducted over eight years by an interdisciplinary group of scholars who comprised the Working Group on Communities and Neighborhoods, Family

[7] Refer to Jencks and Mayer (1990) for a thorough literature review of neighborhood effects on young adult outcomes prior to 1990.

[8] Datcher, 1982 first used ZIP codes as geographical boundaries; Crane (1991) defined neighborhoods as PUMS; Brooks-Gunn and colleagues (1993) tested models with both census tracts and ZIP codes.

Processes, and Individual Development. Using data from multiple longitudinal studies, their volume of empirical work (Brooks-Gunn et al., 1997a, 1997b) estimated the effect of five dimensions of neighborhood variation on a wide variety of outcomes experienced at different time periods during childhood and adolescence. Collectively, findings were broadly consistent with the central hypotheses advanced by Wilson (1987), as articulated in a critique by Massey (1998): neighborhoods exerted influence during childhood and adolescence, and the spatial concentration of both poverty and affluence were important correlates of educational outcomes. Several of the analyses also included innovative analytic approaches to mitigate selection biases. For example, use of instrumental variables suggested that the magnitudes of neighborhood effects on cognitive abilities and educational attainment were even larger than those produced by simple multivariate regression techniques (G. J. Duncan, Connell, & Klebanov, 1997).[9] With respect to school dropout specifically, findings were also robust to family fixed-effects models that compared outcomes of siblings who experienced different neighborhood conditions during childhood, controlling for time-varying socio-economic household characteristics (Aaronson, 1997, 1998) . While both approaches produced larger standard errors, the magnitude of neighborhood coefficients was equal to or greater than those previously estimated using standard OLS techniques.[10] These findings cautiously suggested that the omitted variable bias may be less problematic than had been assumed. If anything, there

[9] The characteristics of neighborhoods in which parents lived following children's departure from their homes were used as instrumental variables. These variables were strongly correlated with neighborhood characteristics of children during adolescence but hypothesized to be weakly correlated with unobserved confounding parental characteristics during childhood.

[10] More recently, George C. Galster et al. (2007) used coincident county-level data as identifying instruments for census tract poverty rates. The effects of neighborhood poverty on school dropout were similar to estimates obtained using multivariate OLS regression; however, standard errors were larger and estimates were not statistically significant at conventional levels. The authors also identified a variety of methodological limitations with this instrumental variable.

was evidence of a small downward bias from over inclusion of mediating family characteristics.

The relationship between neighborhood socio-economic characteristics and students' academic outcomes, including school dropout, has been observed across a wide range of studies that followed (see reviews by Leventhal & Brooks-Gunn, 2000; Sastry, 2012). In regard to school dropout specifically, subsequent studies have continued to build upon more rigorous causal estimation techniques using national longitudinal data provided by the Panel Study for Income Dynamics (PSID). For example, (Harding (2003)) used propensity score matching techniques, grouping neighborhoods into low, average, and high poverty concentrations (treatments). These results suggested that children who lived in different neighborhoods but who were otherwise identical on observed socio-economic factors at age 10 experienced markedly different outcomes. Those in higher-poverty neighborhoods experienced nearly twice the odds of dropout as their matched counterparts in lower-poverty neighborhoods. After a series of sensitivity analyses, the author concludes that selection biases would have to be "unreasonably strong" to account for such large "treatment" effects (Harding, 2003, p. 676).

More recent studies have examined neighborhood residence in temporal perspectives, estimating cumulative exposure to economically disadvantaged neighborhoods throughout childhood and adolescence. Using a discrete-time event history-model, Crowder and South (2003) estimated the effect of neighborhood disadvantage on the annual hazard of dropout from ages 14 to 19 years. Again, neighborhood socio-economic disadvantage was positively associated with school dropout. Crowder and South (2011) also showed that the relationship between

neighborhood poverty and school dropout was approximately 30% stronger when estimated as sustained exposure (i.e., from birth to age 18) than when estimated using point-in-time measures of neighborhood characteristics (i.e., when child was 14 years of age). These larger effects also persisted after controlling for economic characteristics of bordering neighborhoods. Wodtke et al. (2011) also estimated cumulative exposure to economically distressed neighborhoods. In order to overcome limitations of conventional regression methods that "control away" indirect pathways of neighborhood effects (e.g. household income), the authors use marginal structural models and inverse probability treatment (IPT) weighting (J. M. Robins, 1999; J. M. Robins, Hernán, & Brumback, 2000). This approach does not require the assumption that time-varying socio-economic confounders included in the models are not affected by past treatment (i.e., prior neighborhood exposure). The estimates produced were substantially larger than reported in previous studies; sustained exposure to disadvantaged neighborhoods was associated with a 60% to 80% decrease in the odds of high school graduation. Similar analytical approaches have also uncovered that economically distressed neighborhoods exert much stronger influences on other processes that are highly associated with school dropout than had been estimated previously, including teen parenthood (Wodtke, 2013) and cognitive abilities (Sampson, Sharkey, & Raudenbush, 2008; Sharkey & Elwert, 2011).

2.2.4 *Heterogeneity in the effects on school dropout*

Early scholarship on neighbored poverty hypothesized that the effects of concentrated disadvantage are likely to vary based on individual characteristics (Jencks & Mayer, 1990). This perspective has been underscored by more contemporary scholars as it relates to residential context more broadly (Harding, 2011; Sharkey & Faber, 2014a).

Much of the literature has focused on four moderating demographic characteristics of children and adolescents: age, gender, race/ethnicity, and socio-economic status. As described by Sharkey and Faber (2014a), study of differential neighborhood effects on educational outcomes across these population subsets has been mostly exploratory, rarely grounded in a strong theoretical basis, and has not resulted in a clear pattern across analyses. With respect to school dropout, two studies have found that younger adolescents were more affected by neighborhood disadvantage than older students (Crowder & South, 2003; Owens, 2010).[11] The effect of neighborhood poverty was also found stronger for white females than for white males, but stronger for black males than for black females (Crowder & South, 2003). In a separate analysis of students from all backgrounds, the effect of neighborhood poverty was substantially more pronounced for males than for females (Harding, 2009). The study by Wodtke et al. (2011) points to a stronger effect for black children than for nonblack children. Crowder and South (2003) also show that white children from low-income families and black children from single-parent homes are most affected by neighborhood poverty. Most recently, socio-economic residential segregation at the metropolitan-area level has been shown to decrease rates of high school graduation for adolescents from poor backgrounds, but has no effect for students from non-poor backgrounds (Quillian, 2014). Collectively, these findings generally support the idea that neighborhood disadvantage interacts with individual disadvantage to exacerbate risk (Crowder & South, 2003; W. J. Wilson, 1987, 1996). Males, blacks, and low-income students are also substantially more likely to dropout than females, whites, and high-income students (Rumberger, 2011). While the evidence is not

[11] The stronger relationship between neighborhood poverty and school dropout observed for younger adolescents in the study by Crowder and South (2003) was not observed for black adolescents.

strong, these students may potentially be more vulnerable to the effects of neighborhood poverty on school dropout.

2.2.5 *Contextual mechanisms*

Despite a wide range of innovative experimental and observational studies that have rigorously examined the effects of neighborhood poverty, few studies have attempted to unravel the structural mechanisms through which neighborhood poverty exerts influence on educational outcomes. In part, this may be due to additional methodological challenges in identifying two chains of causal inference – the causal effect of neighborhood poverty on structural mechanisms and the causal effect of structural mechanisms on educational outcomes. As the prior review of empirical literature demonstrates, researches are confronted with substantial challenges in providing causal estimates of neighborhood poverty on educational outcomes alone.

The single study that systematically examines the relationship between neighborhood poverty, a host of theoretical mechanisms, and school dropout was conducted by South et al. (2003). These authors analyzed longitudinal data from the National Survey of Children (NSC), a three wave nationally representative survey of U.S. children ages 7 to 11 when first interviewed in 1976 (Zill, Furstenberg, Peterson, & Moore, 1990). Like prior observational studies, multivariate regression methods estimate the relationship between the neighborhood poverty and school dropout, controlling for baseline socio-economic characteristics of youth and their families. A variety of individual variables are subsequently entered in regression models and changes in the magnitude of the neighborhood poverty coefficient are examined.

Using these methods, South and colleagues (2003) conclude that approximately one third of the observed relationship between neighborhood poverty and school dropout can be attributed to adolescent reports of their peers' educational behaviors.[12] A smaller proportion of this relationship was explained by youth's lower educational aspirations and higher rates of residential mobility in poor neighborhoods. Contrary to the authors' hypotheses derived from theory, other constructs did not affect the relationship between neighborhood poverty and school dropout, including youth' delinquent behavior, their sense of attachment to school, and their parents' social control and monitoring of their behavior. Together, these findings lend support for theories of social isolation and collective socialization; poorer educational behaviors of peers reported by individuals from poorer neighborhoods appear to be central to the neighborhood poverty-dropout relationship. Theoretical constructs related to theories of social (dis)organization, such as parental control and monitoring of youth behavior, did not mediate the relationship as expected.

This study is marked by several substantial methodological limitations, however. First, the study includes no measures of school resources or other school contexts, an important structural mechanism highlighted by theory (Brooks-Gunn et al., 1997b; W. J. Wilson, 1987). Second, measures of neighborhood environments are also excluded from the analyses. Theoretical constructs related to theories of social (dis)organization are measured at the individual/family-, not neighborhood-level, represented by parental social control and monitoring. Third, mediation is tested by comparing regression

[12] In the logit regression model predicting school dropout, the logit coefficient for the neighborhood disadvantage index was 0.060 ($p<.05$), controlling for baseline socio-economic characteristics of families. After including the peers' educational performance index, the neighborhood disadvantage index reduced to 0.042, which was no longer statistically significant at conventional levels ([0.060-0.042])/ 0.060 =0.30) (South et al., 2003, pp. 19-20).

coefficients from nested non-linear probability models, an analytical approach widely criticized (Breen, Karlson, & Holm, 2013; Karlson, Holm, & Breen, 2012).[13] Four, multilevel analytic models were not used because of insufficient clustering of respondents within both neighborhoods and schools. Finally, educational behaviors of peers, the mechanisms highlighted in the study's conclusions, are reported by the study's subjects, not self-reported by their actual peers. Such reports are likely subject to considerable measurement bias, particularly reports of peers' future educational plans (intentions of going to college) and past educational achievement (grand point average). Collectively, these limitations compel a very cautious interpretation of the findings and result in a largely incomplete examination of theoretically relevant mechanisms.

Given the strong link between educational achievement and attainment, related literature on educational achievement may also be informative to the current line of inquiry. Using data from the National Educational Longitudinal Study (NELS) of 1988 Ainsworth (2002) examines the influence of neighborhood socio-economic compositional characteristics and the mechanisms that mediate their associations with students' standardized academic test scores. The author measure a variety of theoretical mechanisms: respondents' school related behaviors and attitudes (time spent on homework, educational expectations); peer group culture (number of friends who dropped out); social networks (intergenerational closure of respondents' parent); perception of occupational opportunity; and school atmosphere (teacher morale, teachers' ease of motivating students, students' prioritization of learning). Collectively, these mediating variables accounted for approximately 40% of the effect of neighborhood

[13] A discussion about testing mediation using non-linear probability models appears in the following chapter.

socio-economic characteristics on students' academic test scores. Students' time spent on homework and their educational expectations contributed to this relationship most substantially. Like the prior study discussed, several noteworthy limitations compel cautious interpretation of findings. Again, there was insufficient clustering of respondents within neighborhoods to conduct multilevel analytic models. Second, financial and human resources of schools were omitted from the analysis. Third, the time students spent on homework, their educational expectations and intergenerational closure, the mediating variables highlighted in the study's findings, were all measured at the individual-level; no neighborhood-level estimates of mediating social or structural processes were measured. As a result, all variation observed for individual and parental characteristics hypothesized as mediating variables is assumed to be attributed to the socio-economic characteristics of neighborhoods. This assumption is likely to overestimate the true mediating effects of these variables. Finally, context of peer groups was measured based only on the number of the respondent's friends who dropped out of school (reported by the respondent). While this measure is likely subject to less bias than respondents' reports of their friends' academic achievement, as used by South et al. (2003), it is a limited indicator of peer context. Characteristics of respondents' peers who are enrolled in school also impact developmental and educational outcomes (Epple & Romano, 2011; Staff & Kreager, 2008).

Another related study by Harding (2009) examined the relationship between neighborhood disadvantage, neighborhood violence, and school dropout. The study uses data from the National Longitudinal Study of Adolescent Health (Add Health) (K.M. Harris et al., 2009); the analyses model for the clustering of adolescents within both

neighborhoods and schools; and the measures include a variety of neighborhood- and school-level contexts. Neighborhood violence is operationalized based on within-neighborhood aggregate reports of respondents' exposure to violence, including subjective measures of their perceived safety. The analyses suggest that a substantial proportion of the relationship between neighborhood poverty and school dropout is explained by neighborhood violence (approximately 44% for males).[14] The focus of this paper was to estimate the causal effects of neighborhood violence, including the indirect effects presumably exerted through neighborhood poverty. The mediating effects of other theoretical mechanisms were not examined, although many were included as control variables. These covariates included neighborhood-level measures of intergenerational closure and social cohesion, and measures of school size, organization, cumulative dropout rate, and percent of students in a college preparatory program.[15] Surprisingly, with the exception of school organization,[16] these variables were not related to school dropout at conventional levels of statistical significance. However, all models reported include measures of both individual and neighborhood-level violence, proximate outcomes of neighborhood social disorganization (Bursik, 1988; Kornhauser, 1978; Meier, 1982; Stark, 1987). The author interprets his own findings to suggest that neighborhood social organization may be even more important than previously thought, concluding that "spillover" effects or "collateral damages" of social disorganization (on school dropout) operate largely through neighborhood violence. However, the extent to

[14] For females, neighborhood violence explained 90% of the effect of neighborhood poverty on school dropout; however, the main effect of neighborhood poverty on school dropout for females was not statistically significant at conventional levels (z-statistic=1.86).
[15] Measures of neighborhood disorder were not measured; instead, reports of number of drug users and dealers in the neighborhood, a common indicator of neighborhood disorder, was included in the composite index of neighborhood violence.
[16] Enrollment in Catholic schools, as compared to public schools, was related to decreased risk of school dropout.

which measures of neighborhood poverty contribute to neighborhood social disorganization, which operates through neighborhood violence to affect school dropout, was not assessed. Moreover, the mediating effects of peer and school contexts were not examined.

Collectively, several tentative conclusions can be drawn from the studies reviewed. First, differential peer group contexts appear to explain some of the disparities in school dropout observed between students from advantaged and disadvantaged neighborhoods (South et al., 2003). When studying academic achievement, a strong determinate of school dropout (Rumberger & Lim, 2008), some of the effects of neighborhood socio-economic composition also appear to be explained by lower levels of academic effort and educational expectations observed among students in poor neighborhoods (Ainsworth, 2002). However, it is unclear the extent to which contextual mechanisms (i.e., school structures, neighborhood organization, and peer group contexts) operate through neighborhood poverty to depress individual academic effort and expectation. Finally, neighborhood violence also appears to explain much of the neighborhood poverty-school dropout relationship (Harding, 2009). Assuming much of neighborhood violence can be attributed to neighborhood social disorganization, these findings also lend support to theories of social (dis)organization.

2.3 Substantive contribution of current study

The review of the literature demonstrates that no study has used nationally representative data to systematically examine a broad range of theoretical mechanisms operating at multiple ecological contexts through which neighborhood disadvantage is hypothesized to impact school dropout. Therefore, the primary research objective of the

current study is to use national longitudinal data to estimate the extent to which relevant school, neighborhood, and peer group contexts mediate the effect of neighborhood disadvantage on school dropout. All mediating contexts, carefully selected and measured in accordance with prior theory, are examined within a common analytic framework. Heterogeneity in the effects of neighborhood disadvantage and mediating contexts are also examined. Supplemental analyses test the sensitivity of findings to alternative theoretical assumptions and analytic approaches. Methodological approaches are described in the following chapter.

3. **Methods**

3.1 Data

3.1.1 *Overview of Add Health data*

The current study uses data from the National Longitudinal Study of Adolescent Health (Add Health). Add Health is a nationally representative longitudinal study of 7th- through 12th-grade students in the United States in 1994 and 1995. Four waves of data collection span 14 years into young adulthood. The study provides a rich description of subjects' health, education and well-being, including the neighborhood, schools, and peer contexts in which they were embedded. Initiated in response to a 1993 Congressional mandate and funded by 24 federal agencies and foundations, Add Health is considered the "largest most comprehensive longitudinal survey of adolescents ever undertaken" (K.M. Harris et al., 2009).

Data from Add Health is most suitable for the current study. First, Add Health is the only nationally representative longitudinal study that provides school identifiers, neighborhood identifiers, and sufficient saturation of respondents within both contexts. Add Health's school-based sampling design results in large clustering of respondents within schools; large samples of respondents sampled within each school also results in sufficient clustering of respondents within neighborhoods (Census tracts), which makes possible reliable multilevel analytical models. Descriptions of school contexts are made available from school administrator reports and federal administrative data. Sufficient clustering of respondents within neighborhoods allows reliable assessment of neighborhood latent variables through aggregation of survey responses (Raudenbush &

Sampson, 1999), an approach that substantially mitigates measurement bias.[17] Finally, detailed peer contexts can be constructed by leveraging Add Health's friendship network data in which self-reported data from students can be linked to respondents who selected them as friends.

Alternative nationally representative data were considered, including data from the Panel Study of Income Dynamics (PSID). Many relevant studies previously discussed have relied on data from PSID to estimate neighborhood determinates of educational attainment (e.g., Crowder & South, 2003, 2011; Sharkey, 2012; Vartanian & Gleason, 1999). The longitudinal panel design of PSID measures repeated changes in both neighborhood residence and academic achievement throughout the life course. This time-series data has shown to be particularly advantageous for causal estimations of cumulative exposure to neighborhood poverty on high school graduation (Wodtke et al., 2011) and for important determinates of high school graduation, such as cognitive ability (Sharkey & Elwert, 2011) and teen parenthood (Wodtke, 2013). However, the sampling design results in insufficient clustering of subjects within both schools and neighborhoods. As a result, neither multilevel analytic models nor valid estimation of neighborhood social processes are possible. Moreover, the characteristics of peers with whom subjects associate are unobserved. Therefore, the PSID is not especially useful for examining mediating contexts through which neighborhood socio-economic disadvantage exerts influence.

[17] The alternative measurement approach involves use of individual subjects' reports on their own neighborhood environment. However, unobserved individual characteristics, which may be correlated with the outcome of interest, are likely to shape how individuals perceive and describe their neighborhoods; this bias is often referred to as shared source bias (Brendgen et al., 2011; Browning et al., 2008; Towers et al., 2000).

3.1.2 *Description of survey components*

Each wave of data collection in the Add Health Study includes one or more interview components. Data collection for Wave I took place between 1994 and 1995 when research subjects were 12-17 years of age. This initial baseline wave began with an in-school questionnaire administered to more than 90,000 students in grades 7 through 12. The questionnaire measured school contexts, friendship networks, school activities, expectations, and health conditions. Following the In-school survey, a sub-sample of nearly 20,800 students participated in a more extensive 90 minute In-home adolescent interview using audio-CASI (audio-computer assisted self interview), technology shown to enhance self-reporting quality of sensitive and illegal information (C. F. Turner et al., 1998). A 30-minute op-scan interviewer-assisted interview was also administered to parents, usually the resident mother (85%), in which data were gathered on socio-economic characteristics, parent-adolescent relationships, parent's familiarity with the adolescent's friends and friends of friends, and neighborhood characteristics. Approximately one year following, nearly 15,000 adolescents and their parents were re-interviewed in their homes at Wave II. The content of the In-home adolescent survey at Wave II was nearly identical to the In-home adolescent survey at the previous wave.

During the In-school and In-home survey, adolescents identified up to five best male friends and five best female friends from their schools' rosters. Detailed friendship-network data can be constructed by linking these friendship nominations to responses from the In-school questionnaire completed by nominated friends. Spatial data were also collected on the location of households using hand-held Global Positioning System (GPS) devices or recording actual addresses. More than 25,000 contextual attributes at

multiple spatial units were created and merged with Wave I and Wave II survey data to describe the community and neighborhood contexts in which adolescents were embedded.[18] A self-administered 30-minute survey of schools attended by adolescents was also completed by school administrators at both Waves I and II. Information on school resources, policies, and other contexts were obtained from the school administrator surveys, aggregated responses of in-school student surveys, and merged data from the Common Core of Data and Private School Survey.[19]

Wave III and IV measured multiple domains of subjects' transition into adulthood, including educational attainment and higher education, labor market participation, relationships, parenting, civic participation and community involvement. Data collection for Wave III occurred between 2001 and 2002 when subjects were approximately 18 to 26 years of age, 6-8 years following the baseline interview. Respondents and their romantic partners (if present) each participated in a separate 90-minute In-home young adult interview. Approximately 6 years later, 12-14 years following baseline when subjects were 24-32 years of age, respondents were re-interviewed at Wave IV in their homes. At both later waves survey data were collected using a 90-minute computer assisted instrument, where more sensitive questionnaire sections were self-administered and less sensitive sections were administered with the assistance of an interviewer. The 93% locate rate and 80% response rate at Wave IV reflect a substantial improvement from Wave III and are comparable to national studies

[18] Data from the U.S. Census, the Centers for Disease Control and Prevention, the National Center for Health Statistics, the Federal Bureau of Investigation, and the National Council of Churches were collected. Depending on the source and contextual variable, contextual data is available for the U.S. Census tract, county, or state in which adolescents reside.

[19] The Common Core of Data (CCD) and Private School Survey (PSS) are programs of the U.S. Department of Education's National Center for Education Statistics that collects fiscal and non-fiscal data about all public schools, private schools, public school districts and state education agencies in the United States.

with significantly shorter intervals between interviews and substantially exceeds other national studies with comparable interview intervals (Kathleen Mullan Harris, 2012).

3.1.3 *Sampling design*

Add Health adopted a school-based sampling design in which the primary sampling frame was schools derived from the Quality Education Database (QED). Eighty (80) high schools were randomly selected with probability proportional to school size within probability stratus by region, urbanicity, school type (public, private, parochial), racial/ethnic composition, and student enrollment.[20] Approximately 30 percent of the schools originally selected that did not agree to participate in the study were replaced by schools comparable according to the stratified sampling characteristics.[21] This sample serves as a nationally representative probability sample of U.S. high schools during the 1994-1995 school year.

School administrators were also asked to identify junior high or middle schools expected to provide at least 5 students to the entering high school class. For each selected high school, one of such "feeder schools" was randomly selected with probability proportional to its student contribution to the selected high school. Because the grade range of some schools included 7th or 8th grades, 20 high schools were selected as their own feeder school. Moreover, 4 high schools had no eligible feeder school because entering students previously attended a wide diversity of junior high and middle schools. In total, 144 middle, junior high, and high schools were selected and participated in the study.

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[20] High schools were defined as schools with an 11th grade and more than 30 students.

[21] Comparable schools were selected according to school size, school type, level of urbanization, percent white, grade span, percent black, census region, and census division.

All students who attended the selected schools and who were present on the single day of survey administration were eligible to participate in the In-school survey. No makeup day was conducted for students absent from school on the day of administration. Exactly one school administrator from each school also completed the school administrator survey for all schools. The selection of the In-home Wave I sample, which served as the basis for all subsequent longitudinal follow-up interviews, adopted a stratified probability sampling design. The sampling population consisted of all 7th through 12th grade students enrolled in each school. The sampling frame was obtained from the union of students on school rosters and students not on rosters who completed in-school questionnaires.[22] A core sample was formed by randomly selecting students within each grade by gender strata (approximately 200 adolescents from each pair of schools and 17 students from each stratum). This core sample of approximately 12,100 serves broadly as a nationally representative sample of adolescents in grades 7 through 12 during the 1994-1995 school year.

In addition to the core sample, a number of supplemental samples were included in the study, drawn from adolescent responses in the In-school interview. For example, all enrolled students in 16 schools were selected for In-home interviews in order to make possible reliable analysis of social networks. Those selected from these 16 schools who were not selected for the core sample are referred to as the saturated school oversample. Disabled students were also oversampled - students who self-reported on the in-school questionnaire that they had physical disabilities involving the use of their limbs. Additional oversamples of blacks from well-educated families, Chinese, Cuban, and

[22] Students who did not participate in the in-school survey (e.g., absent from school or refused) were also eligible for selection into subsequent-home components.

Puerto Rican students were also drawn. Finally, the genetic sample includes an oversample of adolescent pairs living in the same household, including identical twins, fraternal twins, full siblings, half siblings, and unrelated pairs.

3.2 Multilevel structure and units of analyses

All analyses in the current study are conducted at the individual-level unit of analyses – individual youth sampled in the Add Health Wave I In-home study. However, three ecological contexts are examined: (1) individual-level contexts; (2) neighborhood-level contexts; and (3) school-level contexts. Individual-level contexts include environments that vary across subjects, such as family, household, and peer-group characteristics. Neighborhood-level contexts include characteristics associated with youth's Census tract of residence at the Wave I In-home survey. School-level contexts include characteristics of schools associated with youth at the Wave I In-home survey.

As originally structured, the Add Health sampling design involves a small degree of cross-classification due to the selection of middle/"feeder" schools. More specifically, students may reside in the same neighborhood (Census tract) but attend different schools only if the neighborhood contains both middle and high school students. In such cases, younger students may attend the sampled middle school, while older students may attend the sampled high school. However, middle schools were purposely selected based on the probability that graduating students would later attend the sampled high school. Therefore, following previously cited research using Add Health data (e.g., Harding, 2009; Owens, 2010), the current study associates middle school students attending middle/"feeder" schools with the respective "sister" high school, resulting in hierarchically structured multilevel data (no cross-classification): youth clustered within

neighborhoods and neighborhoods clustered within high schools. This approach improves model estimation efficiency by eliminating the small degree of "artificial" cross-classification produced only by variation in youth age. More importantly, however, this approach ensures consistent comparison of school contexts (i.e., high school contexts are compared to high school contexts only). Unless otherwise noted, all "school contexts" refer to the contexts of the 80 sampled high schools. As described in more detail in Section 3.5.7, supplemental analyses include stratification of the full sample by grade at baseline and assign characteristics of the school attended at baseline to all students, including middle school students (Grades 7-8). Also, note: 20 sampled schools attended by 21% of youth in the analytic sample include both high school and middle schools grades (7-8); these high schools serve as their own middle/"feeder" school.[23] Moreover, no middle/"feeder" schools were sampled for 4 high schools attended by youth in the analytic sample.

3.3 Analytic sample

The analytic sample is based on the Wave I In-home sample, which formed the longitudinal sample interviewed in follow-up waves (N=20,745). Around 82% of the Wave I In-home sample is included in the current study. The remaining 18% of youth were excluded from the current study because they did meet the following baseline and follow-up inclusion criteria.

[23] An indicator variable for whether high schools attended by youth include both high school and middle schools grades is included in multivariate models. Refer to Section 3.4 for description of variable.

3.3.1 *Baseline inclusion criteria*

A geo-coded Census tract identifier and valid Census tract data was required in order to assess the extent to which youths' neighborhoods' were socio-economically disadvantaged, the primary explanatory variable of interest. Therefore, the first baseline inclusion criterion was defined as youth with valid Census tract data at the Wave I In-home survey. Youth for whom their Census tract of residence could not be determined, or whose Census tract of residence contained no valid Census tract data were excluded (N=198, <1%). The second baseline inclusion criterion was defined as youth who were attending the school sampled for the Add Health study at the Wave I In-home survey. In order to appropriately model the temporal ordering of explanatory variables, youth were excluded if they were not enrolled in any school at baseline (N=404, 2%), which includes youth who completed or withdrew from school between the time of the In-school and In-home surveys. Youth who were attending a school different than the original sampled school at baseline were also excluded (N=441, 2%), which includes youth who changed schools between the time of the In-school and In-home surveys.

3.3.2 *Follow-up inclusion criteria*

A valid educational history report was required in order to determine youth's school dropout status. Therefore, the follow-up inclusion criterion was defined as youth who reported their educational history at the Wave III or Wave IV In-home survey. Youth were excluded if they did not participate in either follow-up waves (N=2,738, 13%) or if their school dropout status could not be determined due to non-response or inconsistent educational history reports (N=33, <1%). Only 12 youth (<1%) were

excluded because they were currently attending high school at Wave III and did not participate in Wave IV.

3.3.3 *Description of included and excluded subjects*

Youth who met all inclusion criteria and therefore comprise the current study's analytic sample are disproportionately advantaged in regard to basic socio-demographic characteristics. For those included, the average household income at Wave I is around $46,000 (in 1994 dollars), around $3,000 higher than those excluded. Parents of youth included are also more likely to have earned a college degree than those excluded (24% and 21%, respectively). A small majority of youth in the analytic sample is white, non-Hispanic (54%), however a small majority of youth excluded from the analytic sample is Hispanic or non-white (46% are white, non-Hispanic). Finally, older youth at baseline were more likely to be excluded from the study; the average youth in the analytic sample was 16.1 years of age at the Wave I In-home survey, compared to 16.4 years for those failed to meet inclusion criteria. Most of the differences between the included and excluded sample are explained by lower socio-economic status of youth who were excluded due to attrition at Wave III and IV. However, youth who were excluded at baseline because they were not enrolled in any school at the Wave I In-home survey are particularly disadvantaged in regard to household income ($36,000) and parental college education (14%). Refer to Appendix A for a description of the characteristics of youth by each of the inclusion and exclusion criteria described.

3.4 Measures

The following section describes the measurement of all analytic variables examined in the study. Appendix B outlines these measurements in table form and

provides information on the exact wording of survey questions, methods of construction, and the quantity of imputed missing data.

3.4.1 *School dropout*

School dropout, the outcome variable of the current study, is a dichotomous individual-level variable, which indicates whether the youth failed to earn a high school diploma. Youth who did not report having earned a high school diploma at follow-up waves were defined as having dropped out of high school. Youth who report having earned a high school equivalency degree (GED) were also defined as having dropped out. While many widely cited demographic statistics and some academic studies count GED recipients as high school graduates, it is generally more common among researchers studying secondary educational attainment to treat all those who withdrew from high school prior to earning a diploma as high school dropouts (see review byRumberger & Lim, 2008). As discussed previously; post-secondary education and labor market outcomes of GED recipients are more comparable to those with no high school credential than for those whose highest education is a high school diploma (Cameron & Heckman, 1993; Tyler, 2003; Tyler & Lofstrom, 2010). Defining GED recipients as school dropouts is also consistent with the conceptualization of school dropout as an event rather than a status; regardless of later educational attainment, students with a GED, by definition, dropped out of high school having failed to graduate with a diploma.

For all youth who participated in the Wave IV In-home survey (88% of analytic sample; N=14,885), school dropout status was determined by their response to the Wave IV In-home survey question: “What is your high school graduation status?” Response options included: (1) finished high school with diploma; (2) earned a high school

equivalence degree (GED); (3) earned a certificate of attendance or a certificate of completion; (4) did not receive a high school diploma, equivalence degree (GED), or other certificate. Youth who reported having finished high school with a diploma (response option 1) were coded: School dropout=0; youth who did not report earning a diploma (response options 2, 3, and 4) were coded: School dropout=1. School dropout status was deemed unreliable and coded missing if the respondent reported a high school diploma on the above survey question but less than a high school diploma on the subsequent survey question, which asked about the highest educational degree attained (<1% of responses).

Data from the Wave III In-home survey were used to determine school dropout status only for those youth who did not participate in the Wave IV In-home survey due to attrition or whose Wave IV dropout status was deemed unreliable (12% of analytic sample). Note: only a small number of youth were currently enrolled in high school at Wave III and not interviewed at Wave IV (<1%; N=12); these youth were excluded from the study because their dropout status was yet to be determined. During the Wave III In-home survey, youth completed a self-administered questionnaire where they were instructed to mark all educational degrees earned to date, including a "high school diploma". Youth who marked having earned a high school diploma were coded: School dropout=0; youth who did not mark this degree were coded: School dropout=1. School dropout status was deemed unreliable and coded missing if both a "high school diploma" and "high school equivalence degree (GED)" were marked (<1% of responses).

3.4.2 *Neighborhood disadvantage*

Neighborhood disadvantage, the primary explanatory variable in the current study, is a neighborhood-level continuous variable, which indicates the extent to which youths' neighborhood residence at baseline is populated by residents of socio-economic and demographic disadvantage. Neighborhood residence corresponds to youth's Census tract of residence during the Wave I In-home interview, conducted in the spring or summer of 1994. All compositional characteristics on Census tracts are obtained from the 1990 U.S. Census and Census tract boundaries are consistent with 1990 Census tract boundary definitions. Data on Census tracts were obtained from Add Health restricted-use datasets and merged to individual respondents. The following Census tract characteristics comprise the index of neighborhood disadvantage.

(1) Male unemployment rate;

(2) Proportion of persons with income below poverty line;

(3) Proportion of households headed by females with own children ages 18 years and younger;

(4) Proportion persons ages 25 years and older with no high school diploma or equivalency;

(5) Proportion of persons ages 25 years and older with a college degree (reverse coded); and

(6) Proportion of employed persons in a managerial or professional occupation (reverse coded)

Each of the above Census tract characteristics described above are standardized across the unique sample of Census tracts resided by youth in the analytic sample; the neighborhood disadvantage index is measured as the average of the above standardized Census tract characteristics. All Census tract characteristics are highly correlated (α =0.88), load on a single factor in principle factors factor analyses, and are identical to

measures adopted in related neighborhood research using Add Health data (see Harding 2009, 2011). Similar Census tract characteristics have also been used to measure neighborhood disadvantage; (e.g., Crowder & South, 2003, 2011; Sharkey, 2012; Vartanian & Gleason, 1999; Wodtke, 2013; Wodtke et al., 2011). Unlike some prior studies (e.g., Brooks-Gunn et al., 1997a, 1997b), the proposed index intentionally excludes measures of neighborhood racial composition. Scholars have critiqued this approach because socio-economic disadvantage and racial composition are held as theoretically distinct neighborhood characteristics which differently exert influence on social outcomes (Massey, 1998; Small & Newman, 2001).

3.4.3 *Socio-demographic control variables*

All multivariate models that estimate the effect of neighborhood disadvantage on school dropout control for a variety of individual-level socio-demographic characteristics of youth, parents, and households. These characteristics are selected and defined to correspond as closely as possible to youth, parent, and household covariates used in prior research that provide comparable estimates (Crowder & South, 2003, 2011; Harding, 2003, 2009, 2011; South et al., 2003; Wodtke et al., 2011). Using data from youth and parent reports from Wave I In-home surveys, the following socio-demographic characteristics are measured:

(1) *Youth race/ethnicity* is a categorical variable based on youths' reports and coded: 1=White, non-Hispanic (Reference category); 2=Black, non-Hispanic; 3=Hispanic, any race; 4=Other race, non-Hispanic.

(2) *Youth male gender* is a binary variable based on youths' reports and coded: 0=Female; 1=Male.

(3) *Youth low birth weight* is a binary variable based on parents' reports and coded: 0=Normal birth weight; 1=Low birth weight (less than 88 ounces, equivalent to approximately 5.5 pounds or 2,500 grams).

(4) *Mother age at youth's birth* is a continuous variable and derived from youths' and mothers' reported date of birth.

(5) *Parent immigrant* is a binary variable based on parents' reports and coded: 0=U.S. born; 1=(Either) residential parent born outside the U.S.

(6) *Parent highest education* is a categorical variable based on parents' reports of his or her highest educational degree obtained and coded: 1=No high school diploma; 2=High school diploma; 3=Some college or vocational/technical school; 4=College degree or higher.[24]

(7) *Household employment* is a binary variable based on parents' reports and coded: 0=No residential parent currently employed; 1=Any residential parent currently employed.[25]

(8) *Household family structure* is a categorical variable based on parents' reports and coded: 1=Youth lives with married biological parents (reference category); 1=Youth lives with single biological mother; 3=Youth lives in other arrangement (includes step-parent families and non-parental caregivers).

(9) *Household size* is a continuous variable that represents the number of persons who youth report reside in the household.

(10) *Household welfare receipt* is a binary variable based on parents' reports and coded: 0=Did not receive AFDC or Food Stamps last month; 1=Received AFDC or Food Stamps last month.

(11) *Household moved residence past year* is a binary variable based on youths' reports and coded: 0=Did not move residence in past year; 1=moved residence in past year.

(12) *Household income (log)* is a continuous variable based on parents' reports of annual income and transformed from income coded in thousands + 1 into its natural log.

Around 13% of parents of youth in the analytic sample did not participate in the Wave I In-home parent survey. For these observations, youths' reports on parent and household characteristics were used if available. Refer to Appendix B for a more detailed description of alternative reports from youth. Finally, all multivariate models also control

[24] Parent educational attainment is coded to "No high school diploma" if the parent reported receiving a GED.

[25] Residential parents include cohabiting spouses of youths' parental respondents, which my or may not be the youths' biological parents.

for the following two characteristics of youth, which relate to the study's sampling design and sample inclusion criteria:

(1) *Youth grade at baseline* is a categorical variable based on youths' reports and coded: 1=Grade 7 (reference category); 2=Grade 8; 3=Grade 9; 4=Grade 10; 5=Grade 11; 6=Grade 12.

(2) *Youth school dropout reported at Wave III* is a binary variable based on youths' reports and coded: 0=Youth school dropout reported at Wave IV (reference category); 1=Youth school dropout reported at Wave III.

3.4.4 *School resources, disciplinary policies, structure, and organization*

School resources are measured at the school-level based on school administrator reports from the Wave I School Administrator survey and merged contextual data from the Common Core of Data (CCD) and Private School Survey (PSS). Five indicators of school resources are measured as follows:

(1) *Pupil-to-teacher ratio* is a continuous variable computed as the number of enrolled students divided by the number of employed teachers, as reported by 1994 CCD and PSS federal administrative data.[26]

(2) *Teachers with Master's degree* is a continuous variable based on the percent of full-time teachers who hold a Master's degree or higher, as reported by school administrators on the Wave I School Administrator survey.

(3) *Teachers >5 years at school* is a continuous variable based on the percent of full-time teachers who have been employed at the school for five years or more, as reported by school administrators on the Wave I School Administrator survey.

(4) *Teachers <1 year at school* is a continuous variable based on the percent of full-time teachers who have been employed at the school less than one year, as reported by school administrators on the Wave I School Administrator survey.

(5) *School based health and social services* is a continuous variable based on the number of health and social services provided to youth in school, as

[26] Add Health also includes school administrator reports of the "average classroom size". This measure was excluded from the current study because reports were highly correlated with the school pupil-per teacher ratio (α=0.71). Administrative data from CCD and PSS on the pupil-per-teacher ratio was deemed a more reliable measure of the quantity of teacher resources.

reported by school administrators on the Wave I School Administrator survey.

School disciplinary policies are measured at the school-level based on school administrator reports from the Wave I School Administrator survey. School administers were asked to report what "happens to a student who is caught" engaging in 12 different forms of misconduct on the first and second occurrence (24 total questions).[27] Response options included: 0=Verbal warning; 1=Minor action; 2=In-school suspension; 3=Out-of-school suspension; 4=Expulsion. All 24 survey items were included in a series of exploratory factor analysis. Three conceptually distinct factors emerged from these analyses: school disciplinary policy response to (1) violent/aggressive misconduct; (2) first time drug/alcohol use or possession in school; and (3) repeated major misconduct; refer to Appendix C for rotated factor loadings of individual items. Other items were aggregated into a fourth index that represents policy responses to more common infractions that do not pose immediate safety concerns, described as minor misconduct. The following school disciplinary policy strictness indices are measured as follows:

(1) *Violent/aggressive misconduct disciplinary policy strictness* is a continuous variable comprised of school policy responses to fighting with another student (first and second time); injuring another student (first and second time); and verbally abusing a teacher (first and second time) (α=0.81).[28]

(2) *First time drug/alcohol misconduct disciplinary policy strictness* is a continuous variable comprised of school policy responses to possessing alcohol (first time); possessing an illegal drug (first time); drinking alcohol at school (first time); and using an illegal drug at school (first time) (α=0.85).

[27] The following survey items were excluded from indices due to poor factor loadings on exploratory factor analyses and conceptual inconsistency with other indices: "Possessing alcohol (second time)"; "Possessing a weapon (first time)"; "Drinking alcohol at school (first time)"; and "Physically injuring a teacher (first time)".

[28] Cronbach's alpha coefficients for school disciplinary policy indices are computed among school administrator reports on all schools (both high schools and their middle/"feeder" schools) attended by youth in the analytic sample at Wave I In-home survey (N=130).

(3) *Repeated major misconduct disciplinary policy strictness* is a continuous variable comprised of school policy responses to possessing a weapon (second time); physically injuring a teacher (second time); possessing an illegal drug (second time); using an illegal drug at school (second time) (α=0.83).

(4) *Minor misconduct disciplinary policy strictn*ess is a continuous variable comprised of school policy responses to cheating (first and second time), smoking (first and second time), and stealing (first and second time) (α=0.60).

All school disciplinary policy strictness indices are computed as the average of the standardized policy responses.

Finally, the following series of structural and compositional characteristics of schools are measured as follows:

(1) *School size* is a continuous variable measured as the total number of students enrolled in the school, as reported by the school roster.

(2) *School includes middle school* is a binary variable that indicates whether the school includes middle schools grades (Grades 7-8).

(3) *School organization* is a categorical variable that indicates whether the school is classified according to the National Center for Educational Statistics (NCES) as: (1) Public (reference category); (2) Private Catholic; or (3) Private non-Catholic.

(4) *School urbanicity* is a categorical variable that indicates whether the school is classified according to the National Center for Educational Statistics (NCES) as: (1) Urban; (2) Suburban; or (3) Rural.

(5) *School socio-economic disadvantage* is a continuous variable that represents the extent to which the school is attended by youth form disadvantaged socio-economic backgrounds and parallels the indicators used to construct the neighborhood socio-economic disadvantage index. The school socio-economic disadvantage index is computed as the average of the following standardized variables: (1) average adjusted household income; the proportion of students from households (2) with no employed parents; (3) headed by a single female parent; (4) headed by parents with no high school diploma; (5) headed by parents with a college degree (reverse coded); and (6) headed by parents employed in a professional or managerial occupation (reverse coded). All data used to compute socio-economic disadvantage of schools are based on aggregate survey responses from the Wave I In-home survey.

(6) *School racial/ethnic composition* is a series of continuous variables that represent the proportion of students who identify as: (1) White, non-Hispanic; (2) Black, non-Hispanic; (3) Hispanic, any race; and (4) Other race, non-Hispanic. All data used to compute school racial/ethnic composition variables are based on aggregate survey responses from the Wave I In-school survey. The proportion of White, non-Hispanic students serves as the reference variable in all multivariate models.

3.4.5 *Neighborhood social organization*

Three dimensions of neighborhood social organization are measured at the neighborhood-level based on aggregate survey responses from Wave I In-home surveys: (1) neighborhood social cohesion; (2) neighborhood informal social control; and (3) neighborhood intergenerational closure. All three measures are constructed using "ecometric methods" proposed by Raudenbush and Sampson (1999) to assess ecological settings from survey-based assessments. Previously cited studies on neighborhood social organization adopt similar measurement approaches (e.g., Browning et al., 2008; Browning et al., 2005; Sampson & Raudenbush, 1999; Sampson et al., 1997), including studies using Add Health data (see Harding, 2009, 2011).

Indices are represented as the sum of the constant and neighborhood-specific random effect, also known as the empirical Bayes estimate, obtained from hierarchy nested multilevel models predicting individual survey responses; Appendix D shows regression coefficients, standard errors, and variance components obtained from these models. This approach adjusts for the severity of item indicators, variation in respondent characteristics across neighborhoods, and reliability of estimates, which is largely a function of the number of respondents per neighborhood. Models include responses from all respondents who participated in the Wave I In-home survey, including youth and their parents who were excluded from the analytic sample due to attrition or other exclusion

criteria. These measures were constructed using the following survey items and analytic models:

(1) *Neighborhood social cohesion* is a continuous variable comprised of youth responses to the statements: (1) You know most of the people in your neighborhood; (2) In the past month, you have stopped on the street to talk with someone who lives in your neighborhood; and (3) People in this neighborhood look out for each other (α=0.60). Response options include 0=False; 1=True. A three-level logit model predicts affirmative responses to these statements; items are clustered within subjects, and subjects are clustered within neighborhoods.

(2) *Neighborhood informal social control* is a continuous variable comprised of parent responses to the survey questions: (1) If you saw a neighbor's child getting into trouble, would you tell your neighbor about it? and (2) If a neighbor saw your child getting into trouble, would your neighbor tell you about it? (α=0.59). Response options include 0=Definitely would not; 1=Probably would not; 2=Might; 3=Probably would; 4=Definitely would. A three-level linear regression model predicts response to these questions; items are clustered within subjects, and subjects are clustered within neighborhoods.

(3) *Neighborhood intergenerational closure* is a continuous variable comprised of parent responses to a single survey question. Parents were asked to report the number of parents of their child's friends they have talked to in the past four weeks. Their response was censored at 6 or more parents (selected by around 10 percent of the sample). A two-level linear regression model predicts number of parents; subjects are clustered within neighborhoods.

The survey items used to construct neighborhood indices for social cohesion and informal social control are conceptually similar to items later developed to measure the social cohesion/mutual trust and shared expectations of informal social control dimensions of collective efficacy, respectively (Sampson, Morenoff, & Felton, 1999; Sampson et al., 1997).[29] [30] Scholars using Add Health data have adopted similar

[29] The following five items developed by Sampson (1997) to measure the social cohesion and mutual trust dimensions of collective efficacy: (1) "People around here are willing to help their neighbors."; (2) "This is a close-knit neighborhood."; (3) "People in this neighborhood can be trusted; (4) People in this neighbored generally don't get along with each other."; and (5) "People in this neighborhood do not share the same values."

measurement approaches to the ones in the current study. For example, Harding (2009, 2011) constructed a social cohesion index using identical survey items and methodological approaches. However, Harding (2009) combined questions on informal social control (e.g., likelihood that neighbors would tell child's parents if child was getting into trouble) with the question on intergenerational closure (e.g., how often parents in the neighborhood talk to their children's friends' parents) into an aggregate index of intergenerational closure, presumably because all three items address communication between adults about children. As described, the current study disaggregates these items into separate constructs. Unlike the indicator for intergenerational closure, the indicators for informal social control address neighborhood-level social regulation of deviant behavior, consistent with the core meaning and function of informal social control as described by collective efficacy theory (Sampson, 2012; Sampson et al., 1997).[31] Second, intergenerational closure has more consistently been defined and measured as communication between parents within youths' own peer groups (i.e., the parents of youth's friends; Carbonaro, 1998; Coleman, 1988; Morgan & Sørensen, 1999) rather than as communication between parents and neighborhood residents, many of whom may be of different generations and not parents. Finally, item

[30] The following five items developed by Sampson (1997) measure the shared expectations of informal social control dimension of collective efficacy. Respondents are asked how likely it is that neighbors would "do something" if: (1) children were skipping school and hanging out on a street corner; (2) children were spray painting graffiti on a local building; (3) children were showing disrespect to an adult; (4) a fight broke out in front of their house; and (5) the fire station closest to home was threatened with budget cuts.

[31] Other studies have conceptualized these items more broadly as "collective monitoring" (Wickrama & Bryant, 2003; Wickrama & Noh, 2010) . Collective efficacy scales commonly ask the respondent to rate how likely it is that their neighbor would "do something," rather than the questions in Add Health, which ask how likely it is that respondents and their neighbors would inform the parent of the child.

analyses indicated that the three Add Health items discussed measure two distinct constructs.[32]

In addition to neighborhood social processes, two compositional characteristics of neighborhoods are measured that theory has hypothesized reflect or are related to neighborhood social organization: residential instability and racial/ethnic diversity (Bowen et al., 2002; Bursik, 1988; Kubrin & Weitzer, 2003). The following Census tract characteristics are measured using 1990 Census data obtained from Add Health restricted-use datasets and merged to individual respondents.

(1) *Racial diversity* is a continuous variable computed using Simpson's Interaction Index (Reardon & Firebaugh, 2002) as follows:

$$Racial\ diversity = w(1-w) + b(1-b) + a(1-a) + o(1-o)$$

where *w*=proportion of persons white, *b*=proportion of persons black, *a*=proportion of persons Asian, *o*=proportion of persons other race.

(2) *Hispanic ethnic diversity* is a continuous variable measured as the proportion of persons who are Hispanic.

(3) *Residential instability* is a continuous variable measured as the percent of housing units that have housed the same household for less than five years.

3.4.6 *Peer group school disengagement*

Three dimensions of peer group school disengagement are measured at the individual-level by linking youth friendship nominations to their friends' survey responses: (1) behavioral; (2) emotional; and (3) cognitive. These dimensions are consistent with prior theoretical and empirical work on school disengagement and school dropout (Fredricks et al., 2006; Fredricks et al., 2004). All data on school disengagement of youth's peers are obtained from Wave I In-school surveys, completed by over 90,000 youth who attended schools sampled at Wave I. Friendship nominations of youth are

[32] The scales developed to measure informal social control and intergenerational closure are weakly correlated in analyses of unique neighborhood within the full analytic sample (Pearson *r* correlation coefficient=0.07).

obtained from In-school and In-home surveys at Wave I. During both surveys, youth were asked to identify up to five of their closest male and five of their closest female friends.[33] All measures of subject-specific peer group variables are represented by mean values of youths' nominated friends.

The behavioral dimension of school engagement refers to students' level of participation in school, including their school attendance and involvement in extracurricular activities. Second, the emotional dimension of school disengagement refers to students' feelings of belonging and connectedness to the school institution. Third, the cognitive dimension of school engagements refers to students' level of effort and persistence in learning. Collectively, the extent to which youths' friends are disengaged from school are measured by the following variables:

(1) *Peer group truancy* is a continuous variable based on the frequency that youths' nominated friends skip school without an excuse. Response options are coded: 0=Never; 1=Once or twice; 2=Once a month or less; 3=2 or 3 days a month; 4=Once or twice a week: 5=3 to 5 days a week; 6=Nearly every day.

(2) *Peer group extracurricular disengagement* is a continuous variable measured as the proportion of youths' nominated friends who participate in no clubs, organizations, or teams within their school.

(3) *Peer group emotional school disengagement is a continuous variable* based on how strongly youths' nominated friends agree to the following statements: I feel close to people at this school; feel like I am a part of this school; and I am happy to be at this school. Response options are coded: 0=Strongly disagree; 1=Disagree; 2=Neither agree nor disagree; 3=Agree; 4=Strongly agree. All items are strongly correlated (α=0.81).

(4) *Peer group cognitive school disengagement is a continuous variable* based on how hard youths' nominated friends report trying to do their school work well. Response options are coded: 0=I try very hard to do my best; 1=I try hard enough, but not as hard as I could; 2=I don't try very hard; 3=I never try at all.

[33]At the Wave I In-home survey, youth from only a sub-sample of schools were asked to identify 5 male and 5 female friends (around 20%). The remaining youth were asked to identify only 1 male and 1 female friend.

Data on youths' peers were successfully linked to 79% of youth in the analytic sample. On average, youth with valid peer group data nominated 7.1 friends, 1.4 of whom did not complete the In-school survey, and 0.7 of whom did not attend their school. Therefore, all measures of peer group school disengagement for youth with valid data are based on an average of 5 unique reports from youths' nominated friends who attended their schools. Measures of peer group school disengagement for the remaining 21% of the sample with missing peer group data were multiply imputed (see Section 3.5.8). Around two thirds (66%) of youth with missing peer group data did not participate in the In-school survey when all youth were asked to nominated 5 male and 5 female friends, and nearly half (48%) were only asked to nominate 1 male and 1 female friend during the In-home survey. On average, youth with missing peer group data nominated only 4 friends; around 2 of those nominated did not attend their school, and 2 attended their school but did not complete the In-school survey or did not respond to relevant survey questions on school disengagement. Note: given the relatively large amount of imputed data on youths' peer groups, supplemental multivariate analyses are carried out on the sample with no missing peer group data (see Section 3.5.7).

Because only students who attended a school sampled by Add Health were eligible to complete the In-school survey, all measures of peer group school disengagement reflect youths' in-school peer groups only. Given these limitations, characteristics of nominated friends who do not attend youths' school are not observed in the current study. As a result, the set of measures on youths' peer groups also includes a measure for the proportion of youths' nominated friends who do not attend their school (or associated "sister" school) and whose school disengagement characteristics therefore

go unobserved. This variable controls for the quality of peer group data in multivariate models, but also serves as a meaningful indicator of the nature of youths' peer group, which may have important implications for youths' own school engagement and school completion.

3.5 Analysis

3.5.1 *Descriptive analyses*

Descriptive analyses begin by presenting univariate statistics of all analytic variables for the full analytic sample, including the indicator variables used in the construction of indices. The mean, standard deviation, and range of all variables are shown. Next, two series of bivariate analyses estimate the associations between neighborhood disadvantage, school dropout, and the school-, neighborhood-, and peer group-level variables hypothesized to mediate the relationship between neighborhood disadvantage and school dropout. Consistent with subsequent multivariate analyses, all univariate and bivariate analyses are conducted at the individual-level.

The first series of bivariate analyses estimate the relationship between neighborhood disadvantage and school dropout for the full analytic sample and by youths' race/ethnicity, parental education, and grade at baseline. Pearson *r* correlation coefficients are calculated to assess the relationship between the neighborhood disadvantage index (measured as a continuous variable) and school dropout; independent sample t-tests are used to assess statistical significance of this linear relationship. Mean dropout rates are estimated for each sample and by quartile of neighborhood disadvantage within each sample; F-tests are used to assess statistical significance of mean group differences.

Second, a series of bivariate analyses estimate the relationship between neighborhood disadvantage and contexts hypothesized to mediate the relationship between neighborhood disadvantage and school dropout. Again, Pearson *r* correlation coefficients are calculated to assess the relationship between the neighborhood disadvantage index (measured as a continuous variable) and the hypothesized mediating context; independent sample t-tests are used to assess statistical significance of these linear relationships. Mean values for each context are also estimated within each neighborhood disadvantage quartile; F-tests are used to assess statistical significance of mean group differences.

3.5.2 *Multivariate analyses, partially specified*

Multivariate analyses begin by replicating previous studies that have shown a positive relationship between neighborhood socio-economic disadvantage and school dropout, net of individual- and family-level socio-demographic controls. These analyses seek to estimate the extent to which growing up in a disadvantaged neighborhood increases risk of dropping out of high school among socio-demographically comparable youth. The probability of school dropout is modeled as a function of neighborhood disadvantage and individual- and family-level socio-economic control variables using a logistic function [$f(x) = 1 / (1 + e^{-x})$] with school- and neighborhood-level random intercepts. Following composite equation notation for multilevel models suggested by Rabe-Hesketh and Skrondal (2012) the three-level hierarchically nested baseline model is expressed as:

$$\Pr(Y = 1) = logit^{-1}(\gamma_{000} + \gamma_1 X_{jk} + \gamma_2 C_{ijk} + \zeta_{0jk} + \zeta_{00k} + \varepsilon_{ijk}) \qquad (1)$$

where individuals are indexed with *i*, who are nested within neighborhoods (Census tracts) as indexed with *j*, which are nested within schools as indexed with *k;* γ_{000} represents the fixed-intercept; X_{jk} represents the neighborhood-level measures of neighborhood socio-economic disadvantage, the primary explanatory variable of interest; C_{ijk} represents a set of individual socio-economic control variables (and γ_2 is a vector of these coefficients); ζ_{0jk} represents the neighborhood-level random intercept; ζ_{00k} represents the school-level random intercept ; and ε_{ijk} represents the individual-level idiosyncratic random error term. All models are fitted using Maximum Likelihood (ML) estimation obtained from the multilevel mixed-effects logistic regression command in Stata 13.0 (*melogit*). The structure of the covariance matrix is specified as independent (zero correlation between school random effects and neighborhood random effects).

3.5.3 *Multivariate analyses, fully specified*

The fully specified model includes all contextual variables at all three ecological levels – school, neighborhood, and individual/peer group – and is expressed as follows:

$$\Pr(Y = 1) = logit^{-1}(\gamma_{000} + \gamma_1 X_{jk} + \gamma_2 C_{ijk} + \gamma_3 S_k + \gamma_4 N_{jk} + \gamma_4 P_{ijk} + \zeta_{0jk} + \zeta_{00k} + \varepsilon_{ijk}) \quad (2)$$

where S_k represents the set of school-level contextual variables (school-level resources, structural and compositional characteristics, and school disciplinary policies; and γ_3 is a vector these coefficients); N_{jk} represents the set of neighborhood-level contextual variables (neighborhood-level compositional characteristics, social cohesion, informal social control, and intergenerational closure; and γ_4 is a vector of these coefficients); and P_{ijk} represents the set of individual-level measures of peer groups (peer group school

disengagement, including the proportion of youth's nominated friends who do not attend youth's school; and $\gamma_5$is a vector of these coefficients).

The observable characteristics of schools included in Equation 2 are imperfect measures or mere proxies for institutional capacities determinative of successful high school completion. Add Health's school-based sampling design results in large samples of youth attending the same high school but residing in different neighborhoods characterized by varying levels of socio-economic disadvantage. Therefore, the final models predict school dropout probability with fixed-effects for high schools, necessarily excluding observable characteristics of schools. This final specification serves to identify the contribution of unobservable sources of school-level variation not estimated in prior models. Unlike prior analyses in the literature, the direct effects of the neighborhood environment can therefore be interpreted holding school environments constant. The school fixed-effects model can be expressed as follows:

$$\Pr(Y = 1) = logit^{-1}(\gamma_{00} + \gamma_1 X_j + \gamma_2 C_{ij} + \gamma_3 N_j + \gamma_4 P_{ij} + \varphi_k + \varepsilon_{ij}) \quad (3)$$

where φ_k denotes school fixed-effects. For efficiency, neighborhood-level random intercepts are excluded from Equation 3 because preliminary analyses indicated that neighborhood-level variance components approached zero and were not statistically significant according to Likelihood ratio tests. Standard errors for all coefficients are adjusted using Huber–White robust standard errors for clustering within neighborhoods.

The models expressed in Equation 2 and 3 also include quadratic exponential terms for all school-, neighborhood-, and peer group mediating variables. This approach protects against potential non-linear relationships – both between the independent and mediating variable, and the mediating and outcome variable – that may mask important

indirect effects of neighborhood disadvantage on school dropout. As discussed previously, prior theory, particularly "contagion" or epidemic models of neighborhood effects (Gephart, 1997; Jencks & Mayer, 1990), support a more agnostic approach to functional form, as opposed to a constant assumption of linearity. Moreover, bivariate associations revealed many important non-linear relationships between neighborhood disadvantage and the mediating variables examined in the current study (presented subsequently).

3.5.4 *Decomposition analyses*

The multivariate analytic models above all estimate the added contribution of neighborhood disadvantage, holding constant individual-level socio-demographic and contextual variables. When estimating the extent to which the effect of an independent variable on a dependent variable can be explained by some third variable (a variable hypothesized to mediate or confound this relationship), it is common to simply compare the change in the independent variable coefficient after adding the third variable(s) to the model. The percent reduction in the coefficient from one nested model to another is then deemed to represent the percent to which the third variable(s) mediate or confound the relationship between the independent and dependent variable. For example, this approach was used by South et al. (2003) to decompose the relationship between neighborhood poverty and school dropout. For the current study, this approach would involve comparing the change in the conditional effect of the neighborhood disadvantage coefficient (γ_1) from Equation 1 ($\gamma_1^{restricted}$) to Equation 2 (γ_1^{full}). The indirect effect (effect of *X* on *Y*, mediated through *Z*) can be described as:

$$Total\ Effect - \ Direct\ Effect\ = \gamma_1^{restricted} - \gamma_1^{full} \quad (4)$$

However, this "difference in coefficients" method assumes normal distribution of the dependent variable, an assumption violated in non-linear probability models, such as logit models used in the current study. Unlike linear models, the idiosyncratic error term variance of non-linear probability logit models is fixed to $\pi^2/3$, which causes the variance of the underlying latent variable (often referred to as y*) to differ across models. As new regressors are added to models, differential variation of the latent dependent variable may change magnitudes of previously included regressors, even when regressors are uncorrelated due to such "rescaling" (Cramer, 2003; Long & Freese, 2005). Therefore, coefficients from differentially specified models are not comparable and $\gamma_1^{restricted} - \gamma_1^{full}$ cannot be attributed to the contribution of added variables (Breen et al., 2013; Karlson et al., 2012).

In order to accurately estimate the extent to which hypothesized contextual variables affect the relationship between neighborhood disadvantage and school dropout, the current study uses the Karlson/Holm/Breen (KHB) method for estimating and interpreting total, direct, and indirect effects (Breen et al., 2013). This approach compares the full model with a reduced model that substitutes the *Z* mediating variable with the residuals from a separate regression of *Z* variables on *X*. Because the decomposition is based on a single logit model for the binary outcome, the scale and the fit of the error to the assumed distribution of y* (the underlying latent dependent variable) is held constant.

More specifically, the direct effect of neighborhood disadvantage on school dropout is represented as the logit coefficient of the neighborhood disadvantage scale in

the fully specified model predicting school dropout shown in Equations 2 and 3, where neighborhood disadvantage scale is standardized to a mean of 0 and standard deviation of 1, denoted by γ_1^{full}. Following notation suggested by Breen et al., (2013) this direct effect is referred to as $\beta(YX \cdot ZC)$, which stands for the beta coefficient of *X* (neighborhood disadvantage) when regressing school dropout (*Y*) on *X*, controlling for all mediating contextual variables (*Z*) and individual-level socio-demographic controls (*C*). The indirect effect for given *n* mediating variable is computed as follows:

$$\text{Indirect effect } Z_n = \beta(YZ_n \cdot XC) \times \theta(Z_n \cdot XC) \quad (5)$$

where $\beta(YZ \cdot XC)$ represents the logit coefficient of the mediating variable *Z* in the fully specified model predicting school dropout shown in Equations 2 and 3; and $\theta(Z_n \cdot XC)$ represents the beta coefficient of neighborhood disadvantage in a separate linear regression model predicting mediating variable Z_n, controlling for individual-level socio-demographic characteristics (C). These linear models can be expressed as follows:

$$Z_n = \gamma_0 + \gamma_1 X + \gamma_2 C + \varepsilon_i \quad (6)$$

where Z_n represents given school, neighborhood, or peer group mediating variable of interest. When regressing school- and neighborhood-level contextual variables, standard errors are adjusted for clustering within schools and neighborhoods, respectively.

Given the inclusion of exponential terms for relevant mediating variables, each mediating context modeled in quadratic form produces two indirect effects: (1) the indirect effect for the linear term (*Z*); and (2) the indirect effect for the quadratic exponential term (Z^2). The sum of each of these indirect effects represents the total extent to which a given mediating context modeled in quadratic form mediates the relationship between neighborhood disadvantage and school dropout.

The total effect is computed as the sum of the direct effect and the indirect effects of all mediating (*Z*) variables, expressed as follows:

$$Total\ effect = \beta(YX \cdot ZC) + \sum_{n} (YZ_n \cdot XC) \times \theta(Z_n \cdot XC) \quad (7)$$

The total effect can also be interpreted as the rescaled effect of *X* on *Y* when all *Z* mediating variables are omitted from the full model or the effect of *X* on *Y* when all *Z* residuals from separate regressions of *Z* on *X* are included in the full model.

The ratio of the indirect effect of given Z_j contextual variable to the total effect represents the extent to which Z_j mediates, confounds, or "explains" the conditional association between neighborhood disadvantage and school dropout, holding constant individual-level socio-demographic characteristics. The ratio of all indirect effects to the total effect is referred to as the "grand indirect effect" (Breen et al., 2013). In the current study, these indirect effects are shown as percentages.

The approach described above is used to decompose the effect of neighborhood disadvantage on school dropout as estimated in fully specified models with school characteristics and school-level random intercepts (Equation 2) and school fixed-effects (Equation 3). The total indirect effect operating through the school fixed-effects specification is computed as the sum of the indirect effect of all school identifier dummy variables included in Equation 3.

3.5.5 *Estimating heterogeneity in neighborhood effects*

Heterogeneity in the direct effect of neighborhood disadvantage and the indirect effects operating through hypothesized mediating contexts are examined by replicating analyses within sub-groups according to youths' socio-demographic characteristics. School fixed-effect models are performed separately by: (1) youths' gender – male and

female; (2) youths' parental education – no high school diploma; high school diploma or some college; and college degree or higher; (3) youths' race/ethnicity – white, non-Hispanic; black, non-Hispanic; Hispanic, any race; and (4) youths' grade at baseline – Grades 7-8; and Grades 9-12. Unlike school fixed-effects models using logistic regression performed on the full sample and described in Equation 3, linear probability models (Ordinary Least Squares regression) are performed for sub-group and supplementary analyses, expressed as follows:

$$\Pr(Y = 1) = \gamma_{00} + \gamma_1 X_j + \gamma_2 C_{ij} + \gamma_3 N_j + \gamma_4 P_{ij} + \varphi_k + \varepsilon_i \quad (8)$$

As discussed previously in Section 3.2, all analyses assign middle school students at baseline to their associated community high school. However, analyses that stratify according to youths' grade at baseline assign the characteristics of middle schools to middle school students at baseline (Grades 7-8). Therefore, school effects can be interpreted as the effect of middle school characteristics for middle school students (Grades 7-8), and the effect of high school characteristics for high school students (Grades 9-12). In order to model variation in direct and indirect effects of observable characteristics of both middle and high schools, linear probability models with school random and fixed-effects are performed for both groups.[34]

Linear probability models are deemed more appropriate for sub-group analyses than logit models for two reasons. First, unlike linear probability models, logit model necessarily exclude youth from schools with no within-school variation in the outcome (all or no youth from the school receive a high school diploma). This exclusion is quite small for analyses based on the full sample (around 1.5% of youth), but becomes quite

[34] Three middle/"feeder" schools include grades 7-9 and their associated high schools include grades 10-12. Therefore, 145 youth from these 3 schools who were enrolled in 9th grade are excluded from the high school sample (Grades 9-12) because they were not enrolled in their community high school at baseline.

large for homogeneous sub-group analyses based on race/ethnicity and parental education. Excluding these observations is likely to bias estimates because school-specific fixed-effects are more likely to be perfectly collinear with the outcome when youth within a given sub-group attend schools that are: (1) relatively small in size; (2) attended by few youth within given sub-group (e.g., black youth who attend predominately white schools); or (3) exceptionally effective or ineffective at graduating students within given sub-group. Second, linear probability models allow non-biased comparison of coefficients across models within and between sub-groups. Decomposition analyses are conducted using the KHB method, which is also appropriate for linear models. The total indirect effect calculated using the KBH method for linear models is identical to the change in coefficient from the restricted to full model expressed in Equation 4 (Breen et al., 2013; Karlson et al., 2012)

3.5.6 *Weighting of respondents*

All analyses, including univariate and bivariate analyses, are carried out without weighting respondents according to probability of sample selection. This approach is motivated by several factors. First, prior research relevant to the current study that uses Add Health data also conduct and present unweighted multivariate analyses (e.g., Harding, 2009, 2011; Owens, 2010). Methodological consistency with prior studies allows for a reliable comparison of findings. Second, Add Health does not provide accurate sample weights for the current study's analytic sample – youth who participated in *either* Wave III or IV.[35] Moreover, no sampling weights are provided for 3.4% of the analytic sample who formed the genetic sample or whose demographic characteristics

[35] Add Health provides cross-sectional sample weights for respondents who participated in Wave III and IV, respectively. Longitudinal sample weights are also provided, but only for youth who participated in both Wave III and IV.

could not be determined. Note: these respondents were also included in previously cited research relevant to the current study. Third, unweighted regression estimates are unbiased, consistent and have smaller standard errors than weighted regression estimates, given that sampling weights are solely a function of independent variables in the current model (Winship & Radbill, 1994). Given the extensive set of explanatory variables in fully specified multivariate models, including covariates directly or indirectly related to oversampled sub-groups (e.g., race/ethnicity, immigrant status, parental education), the assumption that respondents' probability of sample selection is solely a function of independent variables is reasonable and supported by empirical testing. [36]

3.5.7 *Alternative specifications and sensitivity analyses*

The robustness of findings is tested through a series of alternative model specifications. First, primary analyses measure school dropout based on at Wave IV (when available) and Wave III (when Wave IV reports are not available). Findings may be biased due to inconsistent timing and methods of measuring the outcome. Moreover, there is a non-trivial proportion of inconsistency in reports among the sample who participated in both Wave III and IV (around 5%). Therefore, the fully specified linear probability model with school fixed-effects is replicated on the sample of youth who participated in Wave III and on the sample of youth who participated in Wave IV. For both analyses, school dropout is measured based on youths' report at the respective wave.

Second, reliance on unweighted analyses could potentially bias estimates if probability of sample selection is related to differential associations between the

[36] To test the assumption that sample weights are solely a function of independent variables, Wave IV cross-sectional grand sample were included as an explanatory variable in the fully specified school fixed-effects model expressed in Equation 3 (includes sample of respondents who participated in Wave IV). Conditional on all other explanatory variables, the sampling weights were not associated with school dropout.

constructs measured. Therefore, the fully specified linear probability regression model with school fixed-effects is replicated using the cross-sectional grand sample weights assigned to youth at Wave IV and III on youth who participated in Wave IV and III, respectively.

Third, omitted variable bias may be of particular concern when examining the mediating influence of subject-specific peer groups. Youth who are disengaged from school are likely to affiliate with peers who are also disengaged (Battin-Pearson et al., 2000; Staff & Kreager, 2008; Weerman, 2011); therefore, estimates of peer effects may be vulnerable to biases of self-selection. As compared to the neighborhoods where youth reside and the schools they attend, youth are plausibly afforded much greater choice in determining the peers with whom they affiliate. Therefore, the fully specified linear probability regression model with school fixed-effects is replicated controlling for youths' own levels of school disengagement (identical to measures constructed for peer groups).

Fourth, because characteristics of peer groups are based on youths' school friends only, peer group characteristics may also reflect the extent to which schools effectively engage students in school. More school resources may enhance school engagement for all students who attend the school, and therefore decrease peer group school disengagement for all respondents within a given school. Because school disengagement is generally viewed as a proximate cause of school dropout, peer group measures of school disengagement may underestimate the influence of school characteristics in a single multivariate analytic model. Therefore, the fully specified linear probability regression

model with school random and fixed-effects is also replicated excluding all measures of peer group characteristics.

Finally, 21% of the analytic sample has missing data on all measures of peer group school disengagement, either because respondents did not nominate a friend who attended their school (or associated "sister school) or because all nominated friends who attended their school did not complete the In-school survey (or refused to answer relevant questions on school disengagement). The missing at random assumption is less plausible for these cases than for other multiply imputed variables of interest with substantially less missing data due to item-specific non-response. Therefore, the fully specified linear probability regression model with school fixed-effects is replicated excluding youth with missing peer group data on all measures of peer group school disengagement.

3.5.8 *Imputation of missing data*

Due to the large number of observed characteristics analyzed in the current study, list wise deletion of observations with any missing information would undermine statistical power and external validity. Therefore, all youth who meet the study's inclusion criteria are retained in analyses through imputation of missing data.

A relatively small amount of missing data on high schools results from the school administrator who did not complete the Wave I School Administrator survey (attended by 1% of youth in the analytic sample) and 2 high schools for which CCD administrator data are not available (attended by 2% of youth in the analytic sample). These schools characteristics are imputed using a linear regression model on the full sample of schools associated with youth in the analytic sample (N=132, includes middle/"feeder" schools) with all school-level contextual variables as predictor variables.

For missing data on neighborhood, peer group, and individual-level socio-demographic characteristics, 20 complete datasets are created using multiple imputations by chained equations (MICE; Allison, 2001; Royston, 2004). Imputation model equations include all analytic variables discussed and analyzed in the current study with a school-fixed effects specification that corresponds to the current school attend by youth at baseline (N=132, includes middle/"feeder" schools). Models are estimated and imputed data sets are constructed using the *mi impute* chained routine in *Stata ME Version 13.0*. Continuous and binary variables are imputed using the predictive mean matching algorithm; parental education is imputed using ordered logit regression models; and youths' race/ethnicity and family structure are imputed using multinomial regression models.[37]

3.5.9 *Note about causal interpretation of direct and indirect effects*

The primary research objective and contribution of the current study involves a systematic series of decomposition analyses that examine theoretically derived ecological constructs hypothesized to be causally affected by neighborhood disadvantage and causally affect school dropout. As described by Breen et al. (2013), mediational analyses as proposed in the current study have long been criticized for lacking causal interpretation (Jo, 2008; J. M. Robins & Greenland, 1992; Sobel, 2008). Causal interpretation of mediational analysis requires two assumptions, described together as the sequential ignorability assumption (SIA) (Imai, Keele, & Tingley, 2010; Imai, Keele, & Yamamoto, 2010): (1) the predictor variable (i.e., neighborhood disadvantage) is

[37] Due to repeated convergence failures imputing ordinal and nominal data, multiple imputations was carried out in two steps. First, all binary and continuous variables were imputed with missing dummy variables for ordinal and nominal analytic variables included in the model. Next, ordinal and nominal analytic variables were imputed using all multiply imputed binary and continuous variables included in the model.

conditionally independent of unobservables, given background covariates (i.e., socio-economic control variables); and (2) the mediator variable (i.e., each school, neighborhood, and peer group context examined) is conditionally independent of unobservables, given background covariates and the explanatory variable (neighborhood socio-economic disadvantage). Given this assumption, Imai, Keele, and Tingley (2010) show that causal interpretation can be given to mediating effects and Breen et al. (2013) show that the analytical approach proposed in the current study allows for such interpretation.

All explanatory studies, including experimental studies, require assumptions, many of which cannot be tested. Given the extensive set of individual, family, school, and neighborhood control variables, the necessary assumptions for causal interpretation in the current study may not be unreasonable. Moreover, the temporal order of key variables is appropriate: neighborhood disadvantage is measured using 1990 Census data; mediating contexts are measured in 1994 (Wave I); and educational attainment is measured after 2001 (Wave III and IV).

However, causal interpretation in the current study is subject to many potential biases, including biases of omitted variables due to non-random distribution of residents across neighborhoods. Unlike causal interpretations in related observational studies, however, the proposed study involves two chains of causal inference, and therefore two sets of causal assumptions. Given this high bar of necessary rigor, the current study, therefore, does not claim to make confident assertions of causality. Instead, the current study aims to present a comprehensive ecological portrait of the theoretically derived constructs that link poor neighborhoods to poor educational outcomes. Measurement and

analytic approaches are carefully deduced from theory and reasonable attempts to minimize biases with respect to internal validity are carried out. Discussion and interpretation of findings also use the language of "mediation," "direct effects," and "indirect effects". However, such language is presented and should primarily be interpreted as statistical associations, not casual assertions. A rigorous examination of the extent to which these statistical associations are consistent with theory is intended to inform future research aimed at more rigorous explanatory purposes. Noteworthy limitations with respect to causal interpretation of specific findings are discussed in more detail in Section 5.2.

4. **Results**

4.1 Descriptive statistics of analytic variables

Table 1. Descriptive statistics of analytic variables presents the means, standard deviations, and ranges of all analytic variables, including indicator variables used to construct indices of school, neighborhood, and peer group contexts.[38] Consistent with subsequent multivariate models, all statistics are unweighted and presented at the individual-level (N=16,919).

4.1.1 *Description of school dropout and neighborhood disadvantage*

In total, 15% of youth failed to earn a high school diploma. This statistic is lower than national estimates cited previously for two reasons. First, the analytic sample does not include approximately 20% of the baseline sample excluded due to attrition or other criteria, whose socio-demographic characteristics places them at increased risk of school dropout (refer to Appendix A for comparisons). Second, the analytic sample includes only youth who were currently enrolled in school at baseline, which excludes youth who already dropped out. When subjects are assigned cross-sectional weights at the wave in which their school dropout status was observed, the nationally representative school dropout rate of the analytic sample is 17% (not shown). The average dropout rate of youth enrolled in grades 7-8 at baseline is approximately 21% (shown in Table 2). These higher estimates are in line with national estimates of school dropout reported previously (Heckman, 2010).

[38] All statistics in Table 1 are based on 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). However, because indicator variables that comprise indices are not entered into imputation models, descriptive statistics of indicator variables are representative of observations with non-missing data only. These variables are indicated by an asterisk (*) in Table 1.

On average, youth in the analytic sample reside in neighborhoods with a 15% poverty rate and 8% male unemployment rate. Approximately 18% of the households in youths' neighborhoods are headed by female headed households; 29% of adults ages 25 years and older have not obtained a high school degree, 23% have obtained a college degree, and 23% are employed in a professional or managerial occupation. The average standardized value of these six neighborhood contexts (the latter two reverse coded) represent the index used to measure neighborhood socio-economic disadvantage. As indicated by the ranges and standard deviations presented, significance variance in all neighborhood characteristics is observed across youth in the analytic sample.

4.1.2 *Description of youth, family, and household characteristics*

A majority of youth identify as White, non-Hispanic (54%), 22% identify as black, non-Hispanic, 16% identify as Hispanic (any race) and 8% identify as some other race (85% of whom identify as Asian). Around 8% of youth and 19% of their parents were born outside the U.S. The proportions of black and immigrant youth in the analytic sample are considerably higher than national estimates due to the oversampling of blacks from educated families and immigrant groups from Cuba, Puerto Rico, and China.

Because middle school students (Grades 7-8) from "feeder" schools were not sampled for all high schools, there are more youth in high school grades 9, 10, 11 and 12 (18%, 19%, 20%, and 16%, respectively) as compared to middle school grades 7 and 8 (14% and 13%, respectively). On average, mothers of youth were 25 years old when youth were born. Around 9 in 10 youth have at least one currently employed parent in the household (includes residential spouses of parents). The highest educational attainment of youths' parents is nearly evenly distributed across the constructed categories: 24%

obtained a college degree or higher; 28% attended some college or received an Associates college degree; 26% received a high school diploma only (excludes GED recipients); and 22% did not receive a high school diploma or any other higher educational degree. Nearly half of youth live with their married biological parents and approximately one in five live with their biological mother only. On average, youth live in households with 4-5 persons and $46,000 annual income (in 1994 dollars). Over the past year, approximately 15% households moved residence and 13% of households received some welfare assistance (AFDC or food stamps).

4.1.3 *Description of school contexts*

Most youth attend high schools that are public (93%), although 3% of youth attend Private, Catholic schools and 4% attend non-Catholic private schools. Because youth were sampled within schools, the mean socio-economic and racial/ethnic composition of schools is broadly consistent with the mean composition of youth in the analytic sample. On average, youth attend high schools where approximately 1,350 students are enrolled. The high school of around 1 in 5 youth is also attended by middle school students (Grades 7-8). Around 47% of teachers at youths' schools have a Master's degree or higher, 65% have been teaching at the current school for more than 5 years, and 10% have been teaching at the current school for less than 1 year. Youths' schools have an average pupil-per-teacher ratio of 19 and provide around 5 school-based health and social services to students.

Table 1 also presents the mean strictness of school disciplinary policy responses to various forms of student misconduct, where 0="Verbal warning," 1="Minor action," 2="In-school suspension," 3="Out-of-school suspension," and 4="Expulsion". The mean

disciplinary policy response indicates that out-of-school suspension is the norm for violent or aggressive misconduct and first-time drug/alcohol misconduct. However, school administrators are more likely to respond with an in-school suspension if a student verbally abuses a teacher for the first time (2.2 mean score) and more likely to respond with an expulsion if a student injures another student for the second time (3.4 mean score).

Less variation in disciplinary policies are observed for repeated major misconduct. In most schools, students are expelled after the second time they have used or possessed drugs at school, injured a teacher, or possessed a weapon at school; although some schools select an out-of-school suspension (mean scores range from 3.8 to 3.9). School administrators generally respond to student cheating with a verbal warning after the first occurrence (1.1 mean score) and an in-school suspension after the second occurrence (1.9 mean score). Students caught smoking at school are most likely to receive an in-school suspension the first time (2.1 mean score), but an out-of-school suspension the second time (2.6 mean score). Students who steal for the first time are generally suspended (mean score 2.8); after the second-time, this suspension is likely to be out-of-school and in some cases may result in expulsion (3.4 mean score).

4.1.4 *Description of neighborhood contexts*

Within the average neighborhood, a near majority of youth agree that they know most people in their neighborhood (47%). A large majority report having stopped to talk with someone in their neighborhood in the past month (78%) and agree that people in their neighborhood look out for one another (72%). Parents report that they are likely to inform their neighbor if they saw their neighbor's child getting into trouble (mean within-

neighborhood score of 3.3 on a Likert scale of agreement, where 3="Agree" and 4="Strongly agree"), but less likely to expect that their neighbors would tell them if their own child was getting into trouble (mean within-neighborhood score of 2.9). Parents within the average neighborhood report that they talked to around 2 other parents of friends of their own children in the past month.

Around 45% of housing units within the average neighborhood in which youth reside have been occupied by current household residents for less than five years. The mean racial composition of neighborhoods is predominantly white (73%), and approximately 17% black and 11% Hispanic. There are several related reasons why neighborhood racial/ethnic composition appears inconsistent with the racial composition of schools and youth in the analytic sample: (1) Census tract measures provided by Add Health do not disaggregate race and ethnicity (many Hispanic residents also identify as White); (2) the study disproportionately oversampled non-white respondents (highly educated blacks and immigrant groups); and (3) racial segregation patterns across neighborhoods vary by youth's race/ethnicity. White, non-Hispanic youth live in neighborhoods that are overwhelming white (around 90% on average), while black and Hispanic youth are more likely to reside in more racially diverse neighborhoods (not shown in Table 1).

4.1.5 *Description of peer contexts*

Youth generally nominate friends who report relatively low levels of truancy – skipping school without an excuse; the within-peer group mean score is 0.64 on a 0-6 ordinal scale where, where 0="Never" and 1= "Once or twice". Around 17% of nominated friends within youths' peer groups participate in no extracurricular school

activities. On average, youths' friends tend to "Agree" or "Neither agree nor disagree" to questions about feeling close to people at school, feeling like they are a part of school, and feeling happy at school (within-peer group mean score ranges from 1.3 to 1.4 on a 0-5 Likert scale, where 0="Strongly agree" and 5="Strongly disagree"). Most nominated friends of youth report making some effort to do well in school; the within-peer group mean score for cognitive school disengagement – the extent to which peers reporting trying hard to do well at school – is 0.75 on a 0-3 ordinal scale, where 0="I try very hard to do my best," 1="I try hard enough, but not as hard as I could".

These above characteristics of peer groups capture school disengagement among youths' friends who attend their school (or associated "sister" school) only. However, on average, around 25% of youths' friendship nominations are to peers who do not attend their school. Approximately 60% of youth nominated at least one friend who does not attend their school (not shown in Table 1). As discussed previously, the characteristics of these friends go unobserved in measures of peer group school disengagement and it is not possible to distinguish whether these friends attend a different school or no school at all; however, the absence of youths' friends from their own school is deemed important as it relates to youths' own school engagement and successful school completion.

4.2 Associations between neighborhood disadvantage and school dropout

Table 2 shows the bivariate associations between the neighborhood socio-economic disadvantage index and school dropout among the full analytic sample, and by youth gender, race/ethnicity, highest parental education, and grade at baseline. For each sample, the Pearson *r* correlation coefficient and school dropout rate within each neighborhood disadvantage quartile are presented. When neighborhood disadvantage quartiles are computed across the full sample, the highly unequal racial/ethnic and educational distribution results in small cell sizes and imprecise estimates of sub-sample dropout rates. For example, almost half of all black youth (47%), which includes an oversample of highly educated black youth, reside in the most disadvantaged quartile; around 43% of youth with college graduate parents reside in the most affluent quartile, while 43% of youth whose parents did not earn a high school diploma reside in the most disadvantaged quartile (not shown in Table 2). In order to more precisely depict the within-group neighborhood disadvantage school-dropout relationship, neighborhood disadvantage quartiles are computed within each sub-sample. Therefore, dropout rates within quartiles should not be compared across sub-samples.

For the full sample of youth, neighborhood disadvantage is strongly associated with school dropout (r=0.13, p<.001); youth who reside in the most disadvantaged neighborhoods (highest quartile) are more than twice as likely to dropout as those who reside in the most affluent neighborhoods (lowest quartile; 21% vs. 9%). The positive association between neighborhood disadvantage and school dropout appears linear when comparing rates across quartiles; one quartile increase in neighborhood disadvantage is associated with an increase in the school dropout rate of around 3 to 5 percentage points.

A positive linear association between neighborhood disadvantage and school dropout is observed for all sub-groups examined. The association is slightly stronger for boys (r=0.14, p<.001), as compared to girls (r=0.12, p<.001), and moderately stronger for White, non-Hispanic youth (r=0.15, p<.001), as compared to black, non-Hispanic (r=0.11, p<.001) and Hispanic youth (r=0.07, p<.001). Bivariate associations within parental education sub-groups are comparatively less pronounced than within the full sample because parental education is highly predictive of school dropout. The strongest relationship between neighborhood disadvantage and school dropout is observed for youth whose parents have earned a high school diploma only (r=0.11, p<.001). The weakest relationship is observed for youth whose parents did not earn a high school diploma (r=0.04, p<.05), who dropout at very high rates, whether they reside in the least disadvantaged neighborhood quartile (25%) or most disadvantaged neighborhood quartile (29%). As a result of the sampling design, observed dropout rates of middle school youth (Grades 7-8) at baseline are substantially higher than rates of high school youth (Grades 9-12) at baseline (21% and 14%, respectively). The relationship between neighborhood disadvantage and school dropout is also considerably stronger for middle school youth (r=0.17, p<.001) than for high school youth (r=0.11, p<.001).

4.3 Associations between neighborhood disadvantage and mediating contexts

Table 3 shows the extent to which the school, neighborhood, and peer group contexts hypothesized the mediate the relationship between neighborhood disadvantage and school dropout are associated with the neighborhood socio-economic disadvantage index. For each contextual variable, the Pearson r correlation coefficient (from the bivariate association with neighborhood disadvantage) and the mean contextual value by

neighborhood disadvantage quartile are presented. Consistent with forthcoming multivariate analyses, bivariate associations are presented at the individual-level, including school- and neighborhood-level contextual variables.

4.3.1 *High school contexts and neighborhood disadvantage*

Residence in disadvantage neighborhoods is associated with attendance at high schools with fewer teachers with Master's degrees (r=-0.10), teachers who have taught at the school for five years or more (r=-0.05), and school-based health and social services (r=-0.07). Unexpectedly, youth from disadvantaged neighborhoods attend schools with lower pupil-to-teacher ratios (r=-0.05) and fewer teachers who have taught at the school for less than one year (r=-0.16). However, after adjusting for clustering within high schools (N=80), none of these associations are statistically significant at conventional levels.

Youth who reside in more disadvantaged neighborhoods appear to attend high schools with more strict disciplinary policies in response to violent/aggressive misconduct (e.g., fighting with or injuring a student, verbally abusing a teacher; r=0.07) and less strict disciplinary policies in response to first time drug/alcohol misconduct (r=-0.07). However, as with measures of school resources, these associations are not statistically significant at conventional levels, nor do they appear to be linear. For example, for first-time drug/alcohol misconduct, youth who reside in the second most disadvantaged neighborhood quartile attend schools with the strictest policies, while youth in the most disadvantaged quartile attend schools with the most lenient policies (as compared to youth form the least disadvantaged quartiles). With respect to repeated major misconduct (e.g., second time injuring a teacher, possessing a weapon, or drug

use/possession), minimal differences are observed across neighborhood environments. The most pronounced association between neighborhood disadvantage and school disciplinary policies is observed for relatively minor and less dangerous forms of misconduct (e.g., cheating, smoking, and stealing r=.17, p<.10). Youth from the most advantaged neighborhood quartile attend high schools with substantially less strict disciplinary polices in response to these relatively minor infractions (0.3 standard deviations below mean) as compared to youth from other neighborhood quartiles (around 0.1 standard deviations above mean).

Structural and compositional characteristics of schools present more striking variation in school characteristics observed across neighborhoods. Youth from more disadvantaged neighborhoods are much more likely to attend public schools (r=0.03, p<.10; and therefore less likely to attend private schools) and much less likely to attend schools located in suburban settings (r=-0.09, p<.05; and therefore more likely to attend schools located in urban or rural settings). The neighborhood-level socio-economic disadvantage index is very strongly correlated with the comparable school-level socio-economic disadvantage index (r=0.58, p<.001). Students from disadvantaged neighborhoods are also more likely to attend high schools with fewer White, non-Hispanic students (r=-0.29, p<.001) and more Black, non-Hispanic students (r=0.32, p<.001).

4.3.2 *Neighborhood contexts and neighborhood disadvantage*

Large differences in levels of neighborhood social organization are observed between socio-economically advantaged and disadvantaged neighborhoods. Contrary to traditional theories of social organization, neighborhood disadvantage is associated with

markedly higher levels of social cohesion – youth interaction with and knowledge of neighborhood residents, including feelings of trust (r=0.16; p<.01) and more informal social control – parental expectations that child misbehavior in neighborhood would be reported to children's parents (r=0.19, p<.001). Mean levels of both measures increase with each quartile increase in neighborhood disadvantage. While disadvantaged neighborhoods appear to benefit from higher levels of social cohesion and informal social control, youth from more disadvantaged neighborhoods are exposed to substantially lower levels of neighborhood intergenerational closure – the extent to which parents in the neighborhood talk to the parents of their children's friends (r=-0.39, p<.001). The difference between average levels of neighborhood intergenerational closure among the most and least disadvantaged quartile approaches one full standard deviation (around 0.5 standard deviations below the mean for the most disadvantaged quartile, and 0.5 standard deviations above the mean for the least disadvantaged quartile).

Neighborhood disadvantage is also associated with more neighborhood racial diversity (r=0.22, p<.001); the neighborhood racial diversity index, which ranges from 0 to 75, is 75% larger for neighborhoods in the most disadvantaged quartile (33) as compared to the least disadvantaged quartile (19). More Hispanic ethnic diversity is also observed in disadvantaged neighborhoods (r=0.26, p<.001). Unexpectedly, neighborhood disadvantage is associated with less residential instability (r=-0.12, p<.01). Around 48% of housing units in the most advantaged quartile have resided in the current household for less than five years; mean residential instability is comparable across other disadvantaged quartiles (43-44%).

4.3.3 *Peer contexts and neighborhood disadvantage*

Finally, residence in disadvantage neighborhoods is related to all measures of peer group school disengagement except truancy (the frequency youths' friends report skipping school without an excuse). Friends of youth from the most disadvantaged neighborhood quartile report nearly identical levels of truancy as the friends of youth from the least disadvantaged neighborhood quartile (0.62 within peer group mean score on a 0-6 ordinal scale, where 0="Never" and 1= "Once or twice"). However, consistent with relevant theory, extracurricular school disengagement and emotional school disengagement for friends of youth who reside in more disadvantaged neighborhoods are higher than for friends of youth living in more advantaged neighborhoods (r=.06 and .08, p<.001, respectively). Around 14% of the friends of youth who reside in the most advantaged neighborhood quartile participate in no extracurricular activities, compared to 19% of friends of youth who reside in the most disadvantaged neighborhood quartile. Average within peer group scores of emotional school disengagement – the extent to which youth feel dissatisfied with school, not a part of school, and not close with people at school – for friends of youth from the most disadvantaged neighborhood quartile is 1.44 (on a 0-4 Likert scale of agreement, where 1="Disagree" and 2= "Neither disagree nor disagree"), compared to 1.31 for friends of youth from the least disadvantaged neighborhood quartile (a difference of 0.2 standard deviations).

Contrary to theories of social isolation, however, cognitive school disengagement – the extent to which youth report not trying hard to do their school work well– is lower among friends of youth from the most disadvantaged neighborhoods (r=-0.*05*, p<.001). Lower levels of peer group disengagement for youth form the most disadvantaged

neighborhood quartile appears to contribute most to the negative association. Average within peer group scores of cognitive school disengagement are similar across the first three neighborhood disadvantage quartiles (0.75-0.78 on a 0-3 ordinal scale, where 0="I try very hard to do my best" and 1="I try hard enough, but not as hard as I could"), but modestly lower for the most disadvantaged neighborhood quartile (0.70; a difference of 0.15 standard deviations as compared to the most advantaged quartile). Finally, the percent of youths' peer group who did not attend youths' school and whose school engagement characteristics are therefore unobserved is positively associated with neighborhood disadvantage (r=0.05, p<.001), considerably higher in the most disadvantaged neighborhood quartile (26%) than in other quartiles (22-23%).

4.4 Total effect of neighborhood disadvantage

Table 4 presents results from the multilevel random effects logistic regression of high school dropout on neighborhood disadvantage, conditional on individual-level socio-demographic characteristics. Exponentiated logit coefficients (odds ratios) and z-statistics of coefficients are shown for neighborhood disadvantage and select individual-level variables. Variance components and intraclass correlation coefficients are presented, which represent the proportion of total variance in high school dropout attributed to the school- and neighborhood-level, calculated using the linear threshold model for non-linear logit probability models.[39]

Model 1 includes no explanatory variables, generally referred to as the unconditional means model in multilevel modeling. Approximately 17% of the variance in school dropout is explained by neighborhood-level variation, 12 percentage points of

[39] Intraclass correlations are also estimated using linear probability models with random intercepts for neighborhoods and schools. Estimates are broadly consistent with the intraclass correlations presented using the linear threshold model for non-linear logit probability models.

which is attributed to school-level variation. Note: because neighborhoods are hierarchically nested within schools, all between-neighborhood variation includes between-school variation.

Model 2 includes the neighborhood socio-economic disadvantage index only; with no controls, one standard deviation increase in neighborhood disadvantage is associated with 40 percent higher odds of high school dropout, which represents around a 4 percentage point change in probability when translated into marginal effects or estimated using a comparably specified linear probability model. The neighborhood disadvantage index alone reduces the proportion of unobserved variation in school dropout attributed to the school- and neighborhood-level by around 30 and 24 percent, respectively.

Model 3 controls for youths' grade at baseline and the wave in which the outcome was reported. These variables are strong predictors of school dropout; however, they do not appear to bias the relationship between neighborhood disadvantage and school dropout. The neighborhood disadvantage effect and the neighborhood-level intraclass correlation coefficient remain virtually unchanged from the model previous.[40] While 7^{th}, 8^{th}, and 9^{th} grade students at baseline are at similar risk of dropout, each additional high school grade at baseline is associated with substantially lower risk. The odds of dropout for youth in 12^{th} grade at baseline are 77% less than for 7^{th} grade youth. Moreover, the odds of dropout for youth whose dropout status was measured at Wave III (did not participate in Wave IV) are 47% higher than for youth whose dropout status was measured at Wave IV (participated in Wave IV). These large effects can be attributed to

[40] The neighborhood disadvantage effect and the neighborhood-level intraclass correlation coefficient also remain virtually unchanged from the model previous when estimations are performed using comparable linear probability models.

the study's sampling design and follow-up procedures. Older youth at baseline are more positively selected because comparably aged youth who dropped out of high school are excluded by design. Future attrition at Wave IV is associated with modest family socio-demographic disadvantage (see Appendix A) and likely associated with unobserved propensity for educational failure.

Model 4 adds all youth, parent, and household socio-demographic control variables shown in Table 2. Holding constant these individual-level characteristics, one standard deviation increase in neighborhood disadvantage is associated with 18% higher odds of school dropout, which represents around a 1.5 percentage point change in probability when translated into marginal effects or estimated using a comparably specified linear probability model (not shown). This conditional association, the primary effect of interest in the current study, is similar to estimates obtained from related studies using Add Health data with comparable socio-demographic controls[41]. In subsequent decomposition analyses this conditional effect is rescaled (to account for added regressors) and referred to as the "total" effect of neighborhood disadvantage on school dropout.

Approximately 63% of the bivariate association between neighborhood disadvantage and school dropout estimated in Model 2 is explained (or "confounded," assuming all covariates are exogenous with neighborhood disadvantage) by the individual-level socio-demographic variables included in Model 4. This "confounding"

[41] The effect of neighborhood disadvantage in logit metric estimated in Model 5 is 0.16. Harding (2009) provided an estimate of 0.14 using the same neighborhood disadvantage index, controlling for similar socio-demographic controls and other school- and neighborhood-level characteristics. Owens (2012) provided an estimate of 0.11 using a similar neighborhood disadvantage index, controlling for similar socio-demographic controls and youths' cognitive abilities. Both Harding (2009) and Owens (2012) used data on the Wave III Add Health sample only.

effect is estimated using the KHB method (refer to Equations 5-7) and is higher than the 53% "naïve" percentage calculated by simply comparing the neighborhood disadvantage logit coefficients from Model 2 to 4, although similar to the 64% percentage calculated by comparing coefficients estimated using comparably specified linear probability models (not shown). The bivariate neighborhood disadvantage-school dropout relationship is most substantially attenuated by parental college education (29%), household income (19%), family structure (8%), household welfare receipt (7%) and mothers' age at youths' birth (5%) – all which are strongly predictive of school dropout and vary substantially across neighborhood context.[42] Refer to Appendix E for a complete decomposition analysis of all individual-level socio-demographic control variables.

4.5 Direct effect of neighborhood disadvantage and mediating variables

Table 5 presents results from the two fully-specified multivariate analytic models: (1) the three level logistic regression of high school dropout with high school- and neighborhood-level random intercepts as expressed in Equation 2 and shown in Model 1; and (2) the high school fixed-effects logistic regression of high school dropout as expressed in Equation 3 and shown in Model 2. Both models include all contextual variables, including relevant contexts modeled in quadratic form, and control for individual-level control variables. Model 1 includes the full sample of analytic youth, while Model 2 necessarily excludes the 255 youth from 103 neighborhoods and 2 schools

[42] The total percent contribution of the individual-level socio-demographic control variables discussed exceeds the total 63% contribution because some control variables moderately suppress the bivariate neighborhood disadvantage-dropout relationship. In particular, youths' black, non-Hispanic race/ethnicity suppresses the association by 11%; holding all other socio-demographic controls constant, black youth are predicted most likely to reside in disadvantaged neighborhoods but most likely to graduate from high school. Prior research has generally found similar rates of dropout between black and white, non-Hispanic youth after adjusting for socio-demographic characteristics. Lower rates of dropout for black youth estimated in the current study may be related to the oversampling of youth from highly educated families.

where no within-school variation in school dropout is observed (all youth reported high school diploma receipt). As in Table 4, exponentiated logit coefficients (odds ratios) and z-statistics of coefficients are shown for all variables presented. In order to ease interpretation and permit comparison of effect sizes across contexts, all variables except dummy variables are standardized to mean 0 and standard deviation 1.

4.5.1 *Direct effect of neighborhood disadvantage*

Net of all mediating contextual variables and individual-level socio-demographic controls, neighborhood disadvantage remains associated with school dropout in the school random-effects model shown in Model 1; one standard deviation increase in neighborhood disadvantage is associated with a 11% increase in odds of school dropout ($p<.01$). This conditional effect is referred to as the "direct" effect of neighborhood disadvantage on school dropout in subsequent decomposition analyses. The intraclass correlation coefficient, conditional on explanatory variables, indicates that 2.2% of the variation in school dropout can be attributed to the neighborhood-level, down from 17.1% in the unconditional model with no explanatory variables shown in Model 1 of Table 4. This reduction demonstrates that observed contexts and individual characteristics account for the large majority – around 87% – of the total between-neighborhood variance in school dropout.

The direct effect of neighborhood disadvantage is slightly larger in the school fixed-effects model shown in Table 2; one standard deviation increase in the neighborhood disadvantage is associated with a 12% increase in odds of school dropout ($p<.01$). Unlike in Model 1, Model 2 controls for all unobserved heterogeneity across schools by assigning a fixed intercept for each high school; coefficients of non-school

level predictor variables, including neighborhood disadvantage, can therefore be interpreted as the average effect when comparing youth who attend the same high school. However, the neighborhood disadvantage coefficient shown in Model 1 and 2 are not directly comparable for two reasons: (1) Model 2 excludes 2.5% of the analytic sample with no within-school variation in the outcome; and (2) coefficients of non-linear probability models are not comparable across models.

4.5.2 *Direct effect of school contexts*

The only measure of high school resources that is associated with school dropout at conventional levels of statistical significance in Model 1 is the proportion of teachers who have taught at the school for five years or more; one standard deviation increase in this measure is associated with a 15% reduction in the odds of school dropout. Several associations between school disciplinary policies and school dropout are observed, although the direction and function forms vary. There is a positive relationship between disciplinary response to students' first time drug or alcohol misconduct and risk of school dropout ($p<.10$); one standard deviation increase in policy strictness is associated with a 9% increase in the odds of school dropout. However, there is a negative relationship between disciplinary response to students' minor forms of misconduct (e.g., cheating, smoking, and stealing) and risk of school dropout; one standard deviation increase in policy strictness is associated with a 10% decrease in the odds of school dropout.

The statistically significant exponential term for violent/aggressive misconduct (e.g., fighting with or injuring students and verbally abusing teachers) indicates a non-linear association between policy strictness and school dropout. More strict policy is associated with less dropout, but the protective effects are diminishing and dissipate

completely around 1.8 standard deviations above the mean, the upper range of the standardized scale. In other words, very lenient school policy in response to violent/aggressive misconduct is related to more school dropout; however, no statistically or substantively significant effect is observed for marginal increases at other ranges.

All else equal, Model 1 also indicates that youth who attend private schools, particularly Catholic private schools ($p<.001$), and schools with a higher proportion of Hispanic students ($p<.10$) are more likely to receive a high school diploma. Moreover, youth who attend high schools that include middle school students (Grades 7-8) benefit from a 29% reduction in the odds of school dropout. Among all school contexts examined, however, the effect of school socio-economic disadvantage, which parallels the disadvantage index at the neighborhood-level, is the most pronounced and more than twice the neighborhood disadvantage effect size in logit metric. Holding constant all individual controls and examined contexts, including neighborhood disadvantage, one standard deviation increase in socio-economic disadvantage at the school-level is associated with a 22% increase in odds of school dropout.

4.5.3 *Direct effect of neighborhood contexts*

Both Model 1 and Model 2 provide estimates for the direct effects of neighborhood contexts modeled in quadratic form. The only measure of neighborhood social organization that is predictive of school dropout, all else equal, is neighborhood-level intergenerational closure– the extents to which parents in the neighborhood talk to the parents of their children's friends; one standard deviation increase in the intergenerational closure scale is associated with a 12% reduction in the odds of school dropout when modeled linearly ($p<.001$). While imprecisely estimated, the exponential

term for intergenerational closure is negative (in logit metric), which indicates that marginal changes are most protective at higher ranges of intergenerational closure; the exponential term nearly approaches conventional levels of statistical significance in the school fixed-effects model shown in Model 2 (p=.11).

While neither the linear nor the quadratic Hispanic ethnic diversity terms are statistically significant in either model, the joint functional form is statistically significant in the school fixed-effects model shown in Model 2 (F=4.32, p<.05; not shown). In this model, one standard deviation increase in neighborhood Hispanic ethnic diversity is associated with a 19% increase in odds of school dropout for marginal changes at average levels of Hispanic ethnic diversity. However, this effect lessens as the proportion of Hispanic residents increase and diminishes completely at around 3 standard deviations above the mean (the 96 percentile in the analytic sample).

4.5.4 *Direct effect of peer group contexts*

All measures of peer groups are positively associated with school dropout in both the school random effects and school fixed-effects shown in Model 1 and 2, respectively. Odds ratios are nearly identical in both models (vary no more than one one-hundredth in exponentiated logit form). Regarding measures of peer group school disengagement, the proportion of youths' friends who do not participate in any extracurricular school activities is positively and lineally associated with youths' own school dropout; one standard deviation increase in extracurricular disengagement is associated with 19-20% higher odds of school dropout. Emotional school disengagement within peer groups is also positively and linearly associated with dropout, although less strongly; one standard deviation increase in emotional disengagement is associated with 10-11% higher odds of

school dropout. Non-linear effects of peer group truancy and cognitive disengagement are estimated, although in opposite functional forms. There are diminishing effects of peer group truancy on school dropout; effects are most pronounced for marginal change at low levels (i.e., the difference between no/low to moderate levels of truancy within peer groups). The opposite is true for peer group cognitive disengagement; effects are most pronounced for marginal change at high levels (i.e., the difference between moderate and high levels of cognitive disengagement within peer groups). The effect of associating with peers who report not trying to do well in school (cognitive disengagement) is relatively small in magnitude, as compared to the effect of associating with peers who report skipping school (truancy). One standard deviation increase in peer group truancy and cognitive disengagement is respectively associated with a 29% and 4% increase in the odds of school dropout for marginal changes at average disengagement levels.

4.6 Decomposition of the neighborhood disadvantage effect

Table 1Table 6 and Table 7 present statistics obtained from the decomposition of the effect of neighborhood disadvantage into direct and indirect effects operating through mediating variables. Table 6 decomposes the neighborhood disadvantage effect from the school random effects model expressed in Equation 2 and presented in Model 1 of Table 5; Table 7 decomposes the neighborhood disadvantage effect from the school fixed-effects model expressed in Equation 3 and presented in Model 2 of Table 5. For both tables, total, direct, and indirect effects are presented in logit metric and the percentage contribution is shown in parentheses.

The total effect shown, computed as the sum of all direct and indirect effects, represents the rescaled neighborhood disadvantage effect on school dropout excluding all

mediating (Z and Z^2). The direct effect shown represents the logit coefficient of neighborhood disadvantage from Table 5 – the effect of neighborhood disadvantage on school dropout, holding constant all control and mediating variables. For each mediating (Z and Z^2) variable, the $\beta(YZ \bullet XC)$ coefficients represent the logit coefficient from the fully specified model shown in Table 5. The $\Theta(ZX)$ coefficients for each mediating (Z and Z^2) variable represent the neighborhood disadvantage regression coefficient in a linear regression of the mediating variable, controlling for individual-level socio-demographic controls (i.e., the association between neighborhood disadvantage and the mediating variable conditional on controls). Interpretation of the $\Theta(ZX)$ coefficient for exponential (Z^2) terms is less intuitive, but because all contextual variables are mean centered, $\Theta(Z^2X \bullet C)$ can generally be interpreted as the association between neighborhood disadvantage and extreme values (i.e., very high or very low values). The indirect effect of neighborhood disadvantage operating through each mediating context is computed as the sum of the indirect effect of the linear (Z) and quadratic (Z^2) mediating variable. Refer to section 3.5.4 for further discussion on the KHB approach for decomposing total effects into direct and indirect effects.

4.6.1 *Decomposition from school random effects model*

Table 6 presents statistics obtained from the decomposition of the effect of neighborhood disadvantage into direct and indirect effects operating through mediating variables from the school random effects model expressed in Equation 2 and presented in Model 1 of Table 5. The total effect of neighborhood disadvantage on school dropout computed using the KHB method is 0.161 (in logit metric), nearly identical to the neighborhood disadvantage logit coefficient estimated in Model 4 of Table 4 (0.162),

controlling for individual-level socio-demographic characteristics but no mediating variables. The indirect effect (0.057) indicates that approximately 35% of the effect of neighborhood disadvantage on school dropout (conditional on controls) is explained by the observed characteristics of high schools, neighborhoods, and peer groups.

However, interpretation of the total indirect effect alone is misleading because this statistic includes contexts that both mediate (positive indirect effect) and suppress (negative indirect effect) the relationship between neighborhood disadvantage and school dropout. In particular, measures of school disciplinary policy strictness *suppress* the neighborhood disadvantage-school dropout relationship by around 35%. With the exception of policy in response to repeated major misconduct, residence in disadvantaged neighborhoods is related to attendance of high schools with disciplinary policies associated, with less school dropout than socio-demographically alike youth who reside in more advantaged neighborhoods.

In particular, youth who reside in disadvantaged neighborhoods are more likely to attend schools with more strict disciplinary policy in response to less serious infractions (e.g., cheating, smoking, and stealing); however, more strict disciplinary policy is associated with less school dropout, which suppresses the net effect of neighborhood disadvantage on school dropout by around 14%. The diminishing protective effect of strict disciplinary policy in response to violent/aggressive misconduct and the diminishing detrimental effect in response to first time drug and alcohol misconduct (as indicated by $\beta[YZ^2 \bullet XC]$ – the logit coefficients of exponential terms presented in Table 5) – also produce modest suppression effects of around 9 and 13 percent, respectively. For example, the violent/aggressive misconduct index is only protective against school

dropout at very low/lenient levels, and residence in disadvantage neighborhoods is associated with increased likelihood of attending a school with very low/lenient (or high/strict) policies (as indicated by $\Theta[Z^2X\bullet C]$ – the logit coefficient of the neighborhood disadvantage coefficient when regressing the disciplinary policy index-squared on neighborhood disadvantage and all individual-level socio-demographic controls, $p<.10$).

Collectively, all five measures of school resources – pupil-per-teacher ratio, three measures of teacher qualifications, and the number of school-based services – account for minimal indirect effects of neighborhood disadvantage on school dropout. The proportion of teachers who have taught at the school for five years or more, the only measure of school resources that is significantly associated with school dropout, is comparable for youth from advantaged and disadvantaged neighborhoods. In total, measures of school structure account for around 16% of neighborhood disadvantage-school dropout relationship. In particular, youth from disadvantaged neighborhoods are less likely to attend Private, Catholic schools (associated with less school dropout, $p<.001$) and more likely to attend schools located in rural settings (associated with more school dropout, although not at conventional levels of statistical significance), each which account for around 9% of the indirect effect of neighborhood disadvantage.

The school disadvantage index, which measures the extent to which the student body is socio-economically disadvantaged and is comparable in construction to the neighborhood-level disadvantage index, produces the largest indirect effect relative to all mediating variables examined. Approximately 55% of the neighborhood disadvantage-school dropout relationship can be explained by variation in observed school-level disadvantage. Youth from disadvantaged neighborhoods attend schools with more

disadvantaged students ($p<.001$), and attending schools comprised of disadvantaged students is strongly predictive of school dropout, all else equal ($p<.001$). Also, like the relationship between school disadvantage and school dropout (in logit metric), the relationship between neighborhood disadvantage and school disadvantage is almost entirely linear.

Next to school level socio-economic disadvantage, the second largest indirect effect is observed for neighborhood-level intergenerational closure (26%). Higher level of intergenerational closure are negatively associated with school dropout ($p<.001$) and neighborhood disadvantage ($p<.001$). As indicated by the $\Theta(Z^2X\bullet C)$ coefficient (-0.217, $p<.10$), the negative relationship between neighborhood disadvantage and school dropout grows stronger at higher levels of neighborhood disadvantage, and so too does the negative relationship between intergenerational closure and school dropout ($\beta[YZ\bullet XC]$=-0.032), which results in slightly larger indirect effects than when intergenerational closure is modeled linearly. While neighborhood disadvantage is strongly associated with higher levels of neighborhood social cohesion and informal social control, these constructs are not predictive of school dropout; therefore, they do not substantively affect the neighborhood disadvantage-school dropout relationship.

Because neighborhood disadvantage is related to lower rates of residential instability at very high levels of disadvantage, which is associated with slightly lower rates of school dropout, this neighborhood characteristic produces a small suppression effect of around 4%. Neither the racial diversity index nor the proportion of neighborhood residents who are Hispanic meaningfully alter the effect of neighborhood disadvantage on school dropout.

School disengagement of youths' friends mediate little to none of the conditional relationship between school disadvantage and school dropout. Bivariate comparisons indicated that youth from disadvantaged neighborhoods nominated friends with markedly lower levels of extracurricular and emotional school disengagement (see Table 3). However, variation in peer group observed across neighborhood contexts are explained almost entirely by youths' own individual-level socio-demographic characteristics – not the extent to which the neighborhood in which youth resides is disadvantaged. In general, friends of similarly socio-demographically positioned youth from dissimilarly advantaged neighborhoods are predicted to report similar extracurricular, emotional, and cognitive levels of engagement in school (as indicated by the $\Theta[ZX{\bullet}C]$ coefficients shown in Table 6). Truancy within peer groups is the single peer group measure associated with neighborhood disengagement net of individual controls; however, all else equal, youth from more disadvantaged neighborhoods befriend other youth who *less frequently* report skipping school without an excuse ($p<.001$). As a result, the effect of neighborhood disadvantage on school dropout is suppressed by around 4%.

While strongly predictive of school dropout, the proportion of nominated school friends who do not participate in any extracurricular activities and the proportion of total nominated friends who do not attend youths' school (and whose disengagement characteristics are therefore unobserved), does not substantively affect the neighborhood disadvantage-school dropout relationship. However, for both peer group measures, youth from more disadvantaged neighborhoods are more likely to nominate either a very large or very small proportion (e.g., all or none), as indicated by the statistically significant $\Theta(Z^2X{\bullet}C)$ coefficients ($p<.05$). These non-linear effects are relatively small, however,

and contribute to minimal indirect effects because both peer group measures are almost entirely linearly predictive of school dropout, as indicated by the non-statistically significant $\beta(Y^2Z \bullet XC)$ coefficients.

4.6.2 *Decomposition from school fixed-effects model*

Table 7 presents statistics obtained from the decomposition of the effect of neighborhood disadvantage into direct and indirect effects operating through mediating variables from the school fixed-effects model expressed in Equation 4 and presented in Model 2 of Table 5.The total effect of neighborhood disadvantage on school dropout computed using the KHB method is 0.122 (in logit metric). Note: this effect is notably smaller than when estimated in the school random effects model shown in Table 6, due in large part because the effect of neighborhood disadvantage, net of individual-level socio-demographic characteristics but no mediating variables, is smaller in magnitude when estimated without random intercepts for schools and neighborhoods (not shown). The indirect effect (0.006) indicates that only approximately 5% of the effect of neighborhood disadvantage on school dropout conditional on controls is explained by the school fixed-effects specification and observable characteristics of neighborhoods and peer groups.

As described previously, the total indirect effect of neighborhood disadvantage on school dropout operating through the high school fixed-effects specification is calculated as the sum of the indirect effect of the 77 high school dummy variables (78 schools minus 1 reference school) included in the fixed-effects model. Note: the indirect effect for each school fixed-effects dummy variable is not shown in Table 7. Contrary to the study's hypothesis, the school fixed-effects specification *suppresses* the conditional neighborhood disadvantage-school dropout relationship by around 16%. The negative

indirect effect indicates that youth from disadvantaged neighborhoods are *more* likely to attend high schools protective against dropout than socio-demographically alike youth from advantaged neighborhoods. Note: this "protective effect" refers not to the school's dropout rate, which is positively associated with neighborhood disadvantage, but to the average high school specific fixed-effect estimates from the fully specified multivariate logit model expressed in Equation 3 and shown in Model 2 of Table 5, conditional on all individual-level socio-demographic characteristics, and neighborhood and peer group variables. Although not directly comparable, the suppression effect is also consistent with the slightly larger direct effect of neighborhood disadvantage observed in the school fixed-effects specification (shown in Model 2 of Table 5), as compared to the school random effects specification (shown in Model 1 of Table 5).

As originally presented in Table 5, neighborhood social organization, neighborhood composition, and peer group characteristics are associated with school dropout in consistent directions, functional forms and effect sizes as compared to estimates obtained from the school random effects specification. Also, the $\Theta(ZX \bullet C)$ coefficients for (Z and Z^2) mediating variables are consistent with estimates obtained from the school random effects specification and differ only due to a slightly reduced sample (2.5%). However, because the school fixed-effects specification acts to modestly suppress the neighborhood disadvantage-school dropout relationship, the total effect is slightly smaller and the indirect effects of neighborhood and peer group variables are therefore slightly larger in percentage form. For example, neighborhood-level intergenerational closure mediates the neighborhood disadvantage-school dropout relationship by around 32% (as compared to 26% estimated in the decomposition of the

school random effects models). Residential instability and peer group truancy suppresses the neighborhood disadvantage-school dropout relationship by around 7 and 5%, respectively (as compared to 4% for both measures estimated in the decomposition of the school random effects models).

4.7 Heterogeneity effects of neighborhood disadvantage

The subsequent series of analyses presented in Tables 8-11 replicate multivariate analyses presented thus far but stratify the full analytic sample by youths' individual characteristics. As discussed previously, linear probability models are used for all sub-group analyses in order to permit comparison of effects across models and avoid large exclusions of youth from schools without any within-school sub-group variation in school dropout (refer to Equation 8 for model specification). For each sub-group analysis, regression coefficients are shown, which can be interpreted as changes in the probability of school dropout (z-statistics are shown in parentheses). Model A (presented in Tables 8-11) presents the effect of neighborhood disadvantage controlling for individual-level socio-demographic controls only. Model B (presented in Tables 8-11) adds the school fixed-effects specification and observable characteristics of neighborhoods and peer groups. A simple comparison of the neighborhood disadvantage coefficients from Model A to Model B indicates the extent to which added variables mediate the effect of neighborhood disadvantage on school dropout, conditional on controls. In Model B, the indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form (i.e., the sum of the indirect effect via Z and Z^2) are shown in brackets as percentages. Like previous models, indirect effects are calculated using the KHB method (refer to Equations 5-7).

In general, model stratification results in substantially reduced statistical power and less precise estimates; results are therefore presented and interpreted cautiously, with emphasis on direct effects of mediating variables and their respective contribution to the indirect effect of neighborhood disadvantage that differ substantially from estimates obtained from prior models estimated with the full analytic sample.

4.7.1 *Variation by youths' gender*

Table 8 presents results from the linear probability model with school fixed-effects for the full sample of youth and by youths' gender. For the full sample of youth, one standard deviation in neighborhood disadvantage is associated with a 1.1 percentage point increase in the probability of school dropout.[43] This effect is virtually unchanged in Model B, which includes school fixed-effects and all neighborhood and peer group characteristics (hereafter referred to as the "fully specified model"). In total, the school fixed-effects specification suppresses 6% of the conditional effect of neighborhood disadvantage. This suppression effect is smaller than when estimated in the decomposition of the logistic regression model shown in Model 2 of Table 5 and decomposed in Table 7. The modestly larger suppression effect observed previously (16%) is related to the exclusion of two schools with no within-school variation (N=255, 1.5%).[44] Regarding neighborhood and peer group mediating contexts, the estimates produced by the linear probability model are consistent with the estimates produced by the comparable non-linear probability model shown previously. In both models,

[43] The 1.1 percentage point change in probability differs from the 1.5 percentage point change referenced in the *Section 4.4* because the current model excludes random intercepts for schools and neighborhoods.
[44] The comparably specified linear probability models on the sample of 78 schools included in the logistic regression school fixed-effects model shown in Model 2 of Table 5 and decomposed in Table 7 produces nearly identical estimates of the extent to which the school fixed-effect specification suppresses the neighborhood disadvantage effect on school dropout (16%).

neighborhood intergenerational closure is the only contextual variable that contributes to a significant proportion of the neighborhood disadvantage-school dropout relationship. As observed previously, all measures of peer groups are predictive of school dropout and small suppression effects are estimated for truancy and extracurricular disengagement within peer groups.

The subsequent two models stratify the full sample by youths' gender. Consistent with prior studies, the effect of neighborhood disadvantage on school dropout estimated in Model A (no mediating variables) is stronger for males (0.013, $p<.001$) than for females (0.010, $p<.001$). For males, the mediating variables and school fixed-effects suppress this effect by 31%, while the mediating variables and school fixed-effects mediates this effect by 40% for girls. Much of the gender variation in indirect effects is explained by the indirect effects of the school fixed-effects specification (-23% for males and +20% for females). For both groups, the largest mediating effect for observable contexts is intergenerational closure (17% for males and 28% for females). Higher levels of neighborhood social cohesion and informal social control appear protective for males but detrimental to girls; however, neither effects are estimated precisely. The direct and indirect effects of peer groups are broadly similar for both males and females; although, males appear more vulnerable to the influence of truancy within peer groups, whereas girls appear more vulnerable to the influence of extracurricular school disengagement within peer groups.

4.7.2 *Variation by youths' parental education*

Table 9 presents results from linear probability models with school fixed-effects stratified by youths' parental education. The effect of neighborhood disadvantage on

school dropout shown in Model A is more pronounced for youth whose parents earned a high school diploma but not a college degree (0.013, p<.01; hereafter referred to as "high school diploma") than for youth whose parents earned no high school diploma (0.009) or earned a college degree (0.008, p<.05). However, the most apparent variation across sub-groups is related to the indirect effects shown in Model B. The effect of neighborhood disadvantage on school dropout is more than twice as large in the fully specified model for youth form the least educated families (0.020, p<.10), in large part because the school fixed-effect specification suppresses by neighborhood disadvantage-school dropout relationship by over 100%. For youth whose parents earned a high school diploma or college degree, the direct effect of neighborhood disadvantage is similar when school fixed-effects and contextual variables are added to the model, in large part due to less pronounced school fixed-effect suppression effects (34 and 37%, respectively).

Intergenerational closure exerts the strongest direct effects for the youth whose parents earned a high school diploma only; however, intergenerational closure is not associated with school dropout for youth whose parents did not earn a high school diploma. More neighborhood-level racial diversity is strongly protective against dropout for youth with the least educated parents (-0.044, p<.01), which suppresses the effect of neighborhood disadvantage on school dropout by 53% because disadvantaged neighborhoods are substantially more racially diverse; however, Hispanic ethnic diversity increases risk of dropout for this group (at marginal changes from low to moderate diversity, in particular), which mediates the effect of neighborhood disadvantage on school dropout by 27% because disadvantaged neighborhoods are more ethnically diverse. For youth from more educated families, racial diversity is associated with more

school dropout and mediates 27% of the neighborhood disadvantage-school dropout relationship, although these effects are imprecisely estimated.

Regarding measures of peer groups, extracurricular school disengagement is most predictive of school dropout for youth whose parents did not earn a high school diploma; truancy is most predictive for youth whose parents earned a high school diploma only; and cognitive school disengagement is most predictive for youth whose parents earned a college degree. Youth from the least educated families are most sensitive to the proportion of nominated friends not attending youths' school, which explains 12% of the neighborhood disadvantage-school dropout relationship. The small suppression effects for truancy and extracurricular disengagement observed in the full sample are most apparent for youth with the most educated parents (10% and 5%, respectively).

4.7.3 *Variation by youths' race/ethnicity*

Table 10 presents results from linear probability models with school fixed-effects stratified by youths' race/ethnicity. Unlike the bivariate association presented in the Table 2, the strongest effect of neighborhood disadvantage on school dropout in Model A is observed for Hispanic youth (0.015, $p<.10$); effects are nearly half the size for white and black youth (0.008, $p<.10$, and 0.007, respectively).

For white youth, the direct and indirect effects are quite similar to the estimates for the full sample of youth, except the peer group suppression effects for truancy and extracurricular disengagement are noticeably larger (17% and 12%, respectively). Also, neighborhood-level Hispanic ethnic diversity is protective against dropout for white youth (-0.030, $p<.05$), which suppresses the neighborhood disadvantage-school dropout

relationship by around 14% because disadvantaged neighborhoods are more ethnically diverse.

The indirect effects operating through the school fixed-effects specification produce the most notable differences across racial/ethnic groups. For black youth, the school fixed-effects mediate the neighborhood disadvantage-school dropout relationship by 58%; however, for Hispanic youth, the effect is suppressed by 65%. Unlike for white youth, intergenerational closure is not associated with school dropout for black and Hispanic youth, nor does it exert substantial indirect effects. Although not precisely estimated, very high levels of neighborhood-level social cohesion and very low levels of informal social control appear to increase risk of school dropout for black youth (the latter suppresses the neighborhood disadvantage-school dropout relationship by 21%). For Hispanic youth, neighborhood-level Hispanic ethnic diversity is non-lineally associated with school dropout. Hispanic youth from the least ethnically populated neighborhoods are least at risk; however, Hispanic youth from highly populated Hispanic neighborhood experience some protective benefit, as well, which collectively mediates the neighborhood disadvantage-school dropout relationship for Hispanic youth by 30%. For both black and Hispanic youth, the direct effects of peer group school disengagement are less pronounced than for white youth (than for black youth, especially) and do not substantially influence the effect of neighborhood disadvantage on school dropout.

4.7.4 *Variation by grade at baseline and school type*

All analyses presented thus far assign middle school students at baseline to their associated community high school. Table 11 replicates prior multivariate analyses, but stratifies the full analytic sample by grade at baseline and assigns the characteristics of

the youths' current school at baseline, rather than the community high school. Therefore, school effects can be interpreted as the effect of middle school characteristics for middle school students (Grades 7-8), and the effect of high school characteristics for high school students (Grades 9-12). Linear probability models with school random and fixed-effects are performed for both groups.[45] Like Tables 8-10, Model A presents the effect of neighborhood disadvantage controlling for individual-level socio-demographic controls only and Model B adds the school fixed-effects specification and observable characteristics of neighborhoods and peer groups. Unlike prior sub-group analyses, however, Model C of Table 11 presents results from the linear probability model with observable school, neighborhood, and peer group characteristics and random intercepts for school and neighborhoods.

As shown in Model A, the effect of neighborhood disadvantage on school dropout, net only of individual-level socio-demographic controls, is stronger for middle school students (0.017, $p<.05$) than for high school students (0.008, $p<.05$). However, the direct effect shown in Model B, which adds school fixed-effects and neighborhood and peer group characteristics, is comparable in magnitude for both middle school students (0.009) and high school students (0.010). For middle school students, around 47% of the total effect of neighborhood disadvantage is explained by the school fixed-effects and mediating variables; however, for high school students, the total effect is suppressed by around 24%. The school fixed-effects specification has no effect on the neighborhood disadvantage-school dropout relationship for high school students and suppresses the relationship by 7% for middle school students.

[45] Three middle/"feeder" schools include grades 7-9 and their associated high schools include grades 10-12. Therefore, 145 youth from these 3 schools who were enrolled in 9th grade are excluded from the high school sample (Grades 9-12) because they were not enrolled in their community high school at baseline.

Variation in indirect effects is explained mostly by neighborhood and peer group characteristics. For middle school students, the direct effect of intergenerational closure is around twice the size as estimated in comparably specified models for the fully sample (-0.022, $p<.05$) but substantially smaller (-0.005) and imprecisely estimated for high school students. Although not statistically significant at conventional levels, neighborhood social cohesion and informal social control are positively associated with school dropout for middle school students and mediate the neighborhood disadvantage school dropout relationship by 23% because these measures are substantially higher in disadvantaged neighborhoods. Collectively, all measures of neighborhood social organization mediate the effect of neighborhood disadvantage on school dropout by 51% for middle school students, but only 6% for high school students.

Peer group characteristics are similarly associated with school dropout for both middle and high school students; however, these measures mediate a small proportion of the total neighborhood disadvantage effect for middle school students and suppress a moderate proportion of the effect for high school students. This difference is most pronounced for peer group truancy. For both groups, one standard deviation increase in peer group truancy is associated with around a 3 percentage point probability increase in school dropout. However, conditional on socio-demographic characteristics, neighborhood disadvantage is associated with only slightly higher peer group truancy for middle school students and moderately lower peer group truancy for high school students, (not shown), which produces a 5% and -15% indirect effect, respectively. Also, peer group extracurricular school disengagement does not affect the neighborhood

disadvantage-school dropout relationship for middle school students, but suppresses the effect by 8% for high school students.

For high school students, direct effects of high school characteristics on school dropout and indirect effects of neighborhood disadvantage via these characteristics obtained from the school random effects model specification shown in Model C are generally more pronounced than the estimates for the full sample presented in Table 5 and 7. For example, high school socio-economic disadvantage mediates 83% of the neighborhood disadvantage-school dropout relationship and school disciplinary policies collectively suppress the neighborhood disadvantage effect by 66%. Non-linear effects are also more pronounced than estimates for the full sample. Risk of school dropout is highest when high schools report very strict or very lenient polices toward violent/aggressive misconduct; however, risk of school dropout is lowest when high schools report very strict or very lenient polices for to first time drug/alcohol misconduct.

For middle school students, direct effects of middle school characteristics on school dropout and indirect effects of neighborhood disadvantage via these characteristics differ from estimates for the full sample presented previously. For example, the direct effect of middle school socio-economic disadvantage on school dropout is substantially smaller, not statistically significant, and mediates the neighborhood disadvantage-school dropout relationship by only 15%. Unlike for high school students, middle school disciplinary policies are not protective against dropout for youth from disadvantaged neighborhoods. Risk of school dropout increases exponentially when middle schools adopt increasingly strict disciplinary policy for violent/aggressive misconduct. As a

result, this measure mediates the effect of neighborhood disadvantage on school dropout by 14%.

4.8 Alternative specifications and sensitivity analyses

4.8.1 *Alternative reports of school dropout*

Findings may be sensitive to alternative measurement approaches of school dropout for two reasons: (1) there is a non-trivial proportion of inconsistency in reports among the sample who participated in both Wave III and IV (around 5%); and (2) the sample of youth whose dropout status could not be determined at Wave IV are substantially more likely to report having dropped out at Wave III (refer to Table 4). Therefore, the linear probability model with school fixed-effects is replicated on the sample of youth who participated in Wave III and on the sample of youth who participated in Wave IV. For both analyses, school dropout is measured based on youths' report at the respective wave.

The extent to which the school fixed-effects specification suppresses the neighborhood disadvantage-school dropout relationship is moderately stronger using Wave III sample and reports (24%) as compared to using Wave IV sample and reports (7%); however, all other estimates are extremely similar and mirror the using the combined sample and combined report presented and discussed previously. Refer to Appendix F for both model estimates, including indirect effects.

4.8.2 *Nationally representative weights*

The fully specified linear probability regression model with school fixed-effects is also replicated using the cross-sectional grand sample weights assigned to youth at Wave IV and III on youth who participated in Wave IV and III, respectively. For both analyses,

results are broadly consistent with the comparable unweighted model specification for the full analytic sample, although standard errors of the neighborhood disadvantage coefficient in the fully specified model are considerably larger. When weights are applied, the direct effect of neighborhood intergenerational closure is around twice as protective as when estimated without weights (-0.024 and -0.023 on Wave III and IV weighted samples respectively, as compared to -0.010 estimated previously, $p<.001$). The extent to which school fixed-effects suppress the neighborhood disadvantage-school dropout relationship is also greater on the weighted samples (25% and 10% on the Wave III and IV weighted samples respectively, as compared to 6% estimated previously). Refer to Appendix G for model estimates of weighted analyses, including indirect effects.

4.8.3 *Self-selection into peer groups*

Omitted variable bias may be of particular concern when examining the mediating influence of subject-specific peer groups. Therefore, the fully specified linear probability regression model with school fixed-effects is replicated controlling for youths' own levels of school disengagement (identical to measures constructed for peer groups). Controlling for youths' own school disengagement reduces the magnitude of peer group effects by around 27-41% (for marginal changes at average levels of peer group school disengagement). However, effects are more pronounced when excluding the sample whose school disengagement variables were imputed (the 34% of youth who did not participate in the Wave I In-school survey). Also, the neighborhood disadvantage effect on school dropout is equally strong (or slightly stronger) when controlling for youths' school disengagement because youth from disadvantaged neighborhoods (like their peers with whom they affiliate) report equal (or slightly lower levels) of school disengagement

than socio-demographically comparable youth from more advantaged neighborhoods. One standard deviation increase in neighborhood disadvantage increases risk of school dropout by 1.1 percentage points ($p<.001$), controlling for youths' school disengagement, all socio-demographic controls, all neighborhood and peer group contextual variables, and school fixed-effects. Refer to Appendix H for model estimates that include youths' own levels of school disengagement, including indirect effects.

4.8.4 *Alternative estimates of school characteristics*

To the extent that school characteristics shape the overall level of school engagement within schools, inclusion of peer group school disengagement characteristics may underestimate the true effect of school characteristics in fully specified analytic models. Therefore, the fully specified linear probability regression model with school random and fixed-effects are replicated excluding all measures of peer groups. The school fixed-effect specification suppresses the neighborhood disadvantage school-dropout relationship by 36% when all measures of peer groups are excluded (as compared to 6% when all measures are included). As expected, the effects of observable school characteristics are generally more pronounced when peer groups measures are excluded. The proportion of teachers who have taught at the school for more than five years is slightly more protective than estimated previously ($p<.05$), and the proportion of teachers who have taught at the school for less than one year is now associated with increased risk of school dropout ($p<.05$). The latter effect is non-linear and diminishes at higher ranges ($p<.05$). Collectively, these measures mediate around 10% of the neighborhood-school dropout relationship. However, the proportion of teachers with Master's degree is associated with *higher* risk of school dropout, and because schools

attended by youth from disadvantaged neighborhoods have fewer teachers with Master's degrees, this measure suppresses the neighborhood-school dropout relationship by 8%. Also, larger schools are associated with lower risk of school dropout ($p<.05$). Regarding school disciplinary policies, more strict policy in response to violent/aggressive misconduct is linearly associated with less school dropout ($p<.10$), while policy in response to first time drug/alcohol misconduct non-linearly associated with school dropout (most protective when very lenient or very strict; $p<.10$). Refer to Appendix I for model estimates that exclude youths' peer group measures of school disengagement, including indirect effects.

4.8.5 *Missing reports of peer groups*

As discussed previously, peer group school disengagement data were multiply imputed for 21% of the analytic sample with missing data. Therefore, the fully specified linear probability regression model with school fixed-effects is replicated excluding youth with missing peer group data. The proportion of nominated friends who do not attend youths' school is less strongly correlated with school dropout when youth with missing peer group data are excluded, likely because youth with the highest proportions (100%) are necessarily excluded due to missing peer group data. However, direct effects of peer group school disengagement and indirect effect of neighborhood disadvantage via peer group school disengagement are consistent with comparably specified models for the full analytic sample with imputed data. Refer to Appendix J for model estimates that exclude youths with missing peer group data, including indirect effects.

5. **Discussion**

5.1 Overview of findings

Findings from the current study provide estimates on the extent to which three sets of ecological constructs mediate the effect of neighborhood disadvantage on school dropout. School, neighborhood, and peer group characteristics were included in decomposition analyses, mediating variables as hypothesized by resource deprivation, social organization, and social isolation theories of neighborhood effects, respectively. An overview of findings with respect to each of these contexts follows.

5.1.1 *Schools as a mediating context*

Resource deprivation theories of neighborhood effects claim that neighborhoods vary in the concentration and quality of educationally supportive institutions, such as schools. Schools have been described as vital institutions through which neighborhoods influence children's educational outcomes (Brooks-Gunn et al., 1997b; Halpern-Felsher et al., 1997, pp. 180-181), Moreover, harshly punitive disciplinary policies of schools that serve children from the most disadvantaged neighborhoods have been theorized to "push" disadvantaged students out of school (Fine, 1985, 1986). Collectively, these perspectives suggest that: (1) children who grow up in poor neighborhoods attend relatively low quality schools with harsh disciplinary policies: and (2) low school quality and harsh disciplinary policies increases risk of school failure, including school dropout.

Findings from the current study provide very limited support for these theoretical premises. Youth from the most disadvantaged neighborhoods are found to attend schools with lower teacher-to-pupil ratios, fewer teachers with Master's degrees, and fewer school-based health and social services. However, these school characteristics are not

predictive of school dropout in multivariate models that control for relevant characteristics and contexts. The proportion of teachers who have taught at the observed school for more than five years is protective against school dropout, although does not vary substantially across the neighborhood environment. As a result, measures of school resources do not substantively mediate the neighborhood disadvantage-school dropout relationship.

Second, findings indicate that youth from disadvantaged neighborhood do generally attend schools with more strict disciplinary policies, and more strict policies for violent and aggressive misconduct in middle schools explains 14% of the effect of neighborhood disadvantage on school dropout. However, more strict disciplinary policies in high schools are often found to be negatively associated with school dropout, especially policies in response to less dangerous infractions. As a result, stricter disciplinary policies of high schools appear to benefit youth from poor neighborhoods, and therefore suppress the effect of neighborhood disadvantage on school dropout.

Third, attending schools comprised of socio-demographically disadvantaged students substantially increase risk of school dropout – more so than the comparable indicator of concentrated disadvantage at the neighborhood level. Because youth from poor neighborhoods are more likely to attend schools other poor students, concentrated disadvantage at the school-level explains a large proportion of the effect of neighborhood-level disadvantage on school dropout (55%). However, the school environment mediates none of the relationship between neighborhood disadvantage and school dropout when all unobserved heterogeneity across schools is modeled using school fixed-effects specifications. The effect of neighborhood disadvantage on school

dropout is equally strong, or stronger, when comparing youth who attend the same school but reside in different neighborhoods (as specified by school fixed-effects). However, substantial variation in this indirect effect is observed across demographic sub-groups. In particular, the school environment suppresses the neighborhood effect by over 100% for youth of the least educated parents (no high school diploma); on the other hand, the school environment, consistent with general resource theories, mediates around 50% of the neighborhood disadvantage effect for African American youth.

5.1.2 *Neighborhood social organization as a mediating context*

Social organization theories of neighborhood effects claim that social integration and informal systems of social control are necessary for communities to realize common goals and solve chronic problems. While originally developed in the study of crime and juvenile delinquency (Sampson et al., 1997), social organization perspectives and related theoretical constructs have also been applied to the study of youth development more broadly (Browning & Cagney, 2002; Cohen et al., 2006), including educational outcomes (Adams & Forsyth, 2006; Bowen et al., 2002; Morgan & Sørensen, 1999; Nash, 2002). Collectively, these perspectives suggest that: (1) poor neighborhoods tend to be characterized by low levels of social cohesion, informal social control, and intergenerational closure: and (2) these measures of social organization protect against risk of school failure, such as school dropout.

Findings from the current study provide mixed support for these theoretical premises. First, contrary to traditional theory, disadvantaged neighborhoods in the current study are characterized by substantially higher levels of social cohesion (e.g., youth know and interact with neighborhood residents) and informal social control (e.g., neighbors are

likely to tell children's parents if children were getting into trouble). However, neighborhood social cohesion and informal social control are not predictive of school dropout in any of the fully specified multivariate models for any of the demographic sub-groups examined. Therefore, these neighborhood contexts do not substantially influence the neighborhood disadvantage effect on school dropout.

Second, neighborhood disadvantage is strongly associated with lower levels of neighborhood intergenerational closure (e.g., parents in the neighborhood talk to the parents of their children's friends). Moreover, neighborhood intergenerational closure is strongly associated with decreased risk of school dropout. Therefore, this neighborhood context mediates a substantial proportion (around 26-32%) of the neighborhood disadvantage effect on school dropout. However, the importance of neighborhood intergenerational closure is less pronounced or not detected for specific demographic sub-groups. In particular, intergenerational closure is not associated with school dropout for African American and Hispanic youth. Effects are also smaller and imprecisely estimated for youth with the least and most educated (college degree or higher) parents.

Third, compositional characteristics of neighborhoods closely associated with theories of social organization, including racial/ethnic heterogeneity and residential instability (e.g., percent of housing units resided by current households for less than 5 years), mediates little to none of the neighborhood disadvantage effect on school dropout among the full sample of youth. However, the extent to which these variables are associated with neighborhood disadvantage and school dropout varies substantially across demographic sub-groups. For example, youth with the least educated parents benefit from more racial diversity at the neighborhood level, which suppresses the effect

of neighborhood disadvantage on school dropout by 53%. On the other hand, neighborhood racial diversity is associated with higher rates of dropout for youth with the most educated parents, which mediates 27% of the effect of neighborhood disadvantage on school dropout. The effect of Hispanic ethnic diversity on school dropout and its relationship with neighborhood disadvantage also varies considerably in direction and functional form across race/ethnicity and parental education. Although not precisely estimated, more residential instability is generally associated with higher rates of dropout; however, among disadvantaged demographic sub-groups, poor neighborhoods tend to have slightly lower levels of residential instability. As a result, residential instability suppresses the effect of neighborhood disadvantage on school dropout for African Americans (49%) and youth with the least educated parents (19%).

5.1.3 *Peer groups as a mediating context*

Social isolation, collective socialization, and "contagion" or epidemic theories of neighborhood effects claim that children and adolescents from poor neighborhoods are spatially isolated from supportive social networks that uphold norms about the importance of educational and occupational attainment. As a result, and in response to structural constraint, alternative or "oppositional" subcultures distinct from "mainstream" or middle-class norms are believed to emerge, which become reinforced and internalized within peer groups over time (Anderson, 1999; Massey & Denton, 1993; W. J. Wilson, 1987, 1996). Collectively, these perspectives suggest that: (1) children from poor neighborhoods are more likely to affiliate with peers who devaluate educational achievement, and are therefore less engaged in school; and (2) affiliation with peers who are less engaged in school exacerbates risk of school failure, including school dropout.

Findings from the current study provide strong support for the second theoretical premise. Affiliation with school friends who are disengaged in school strongly predicts school dropout, including when youths' own levels of school disengagement are held constant. Youth appear especially sensitive to their friends' behavioral dimensions of school disengagement – their friends skip school without an excuse and do not participate in any school-based extracurricular activities. Males and adolescents with the least educated parents appear most susceptible to the influence of their friends' school disengagement. Adolescents with a higher proportion of friends who do not attend their school also experience substantially higher risk of school dropout.

However, findings from the current study provide little to no support for the first theoretical premise. The extent to which youths' neighborhoods are disadvantaged explains little to none of the variation in the school disengagement of school peers with whom youth affiliate. Descriptively, youth from disadvantaged neighborhoods do tend to affiliate with peers who are less likely to participate in school-based extracurricular activities and less likely to report that they feel satisfied with and emotionally connected to their school. Youth from disadvantaged neighborhoods also report a higher proportion of friends who do not attend their school. However, these differences are entirely explained by the disadvantaged characteristics of youths' families, not the disadvantaged characteristics of youths' neighborhood environment. Therefore, when these characteristics are included in multivariate analytic models that control for individual-level socio-demographic characteristics, the effect of neighborhood disadvantage on school dropout is unchanged.

However, among youth with the least educated parents, the proportion of friends who do not attend the same school does mediate 12% of the effect of neighborhood disadvantage on school dropout. Also, all measures of peer group characteristics collectively mediate a small proportion (7-9%) of the neighborhood disadvantage-school dropout relationship for the youngest youth at baseline (Grades 7-8). On the other hand, among more demographically advantaged sub-groups, the current study finds that neighborhood disadvantage is associated with less peer group truancy and extracurricular disengagements. As a result, these observable characteristics of youths' school friends suppress the effect of school dropout for youth with college educated parents (15%) and White, non-Hispanic youth (29%).

5.1.4 *Direct effect of neighborhood disadvantage*

Collectively, findings from the current study suggest that the school, neighborhood, and peer group contexts examined explain only a small proportion of the association between neighborhood disadvantage and school dropout, conditional on socio-demographic characteristics of individual youth. As a result, multivariate analytic models indicate that the direct effect of neighborhood disadvantage on school dropout remains nearly equally strong, or stronger, when all hypothesized mediating contexts are held constant. For example, one standard deviation increase in neighborhood disadvantage is associated with a 1.1 percentage point increase in the likelihood of school dropout among demographically comparable youth with identical levels of school engagement, who attend the same school, whose school friends are equally engaged in school, and who live in neighborhoods with equal levels of social organization, racial/ethnic diversity, and residential stability (see Model A of Table 8 and Appendix

H). Moreover, these direct effects, net of all contexts examined, are most pronounced for those who are most at risk of school dropout, including youth with the least educated parents (2.0 percentage point increase), male youth (1.7 percentage point increase), and Hispanic youth (1.4 percentage point increase).

5.2 Interpretation of findings

The following section interprets the current study's findings with consideration to methodological limitations. Consistency with theory and prior literature are highlighted and discussed.

5.2.1 *General limitations*

Given the current study's observational design and methodological constraints, findings should not be interpreted as confident causal assertions. As previously discussed, measurement and analytic approaches are carefully deduced from theory and reasonable attempts to minimize biases with respect to internal validity are carried out. However, unlike an experimental study, subjects in the current study are not randomly distributed across school, neighborhood, and peer group contexts. Effects estimated in the current study and interpret below are therefore subject to biases of self-selection and omitted variables, which may overstate the true effect of context on school dropout. On the other hand, multivariate analyses that control for time-varying individual and contextual variables endogenous with residential context also subject estimates to biases of "included variables" or "over adjustment," which may understate or misidentify the true effect of context on school dropout.

While the temporal order of key variables is appropriate for meditational analyses, theory tested in the current study involves two chains of causal inference, and therefore

two chains of causal assumptions and potential biases. The conceptual framework of the current study hypothesizes that neighborhood socio-economic disadvantage influences specific school, neighborhood, and peer group contexts, and these context influence risk of school dropout. However, it is also likely that the mediating contexts examined may influence neighborhood socio-economic disadvantage and are endogenous with each other. For example, a high quality school district is likely to increase local area housing values and attract more affluent neighborhood residents. School policies may also influence other mediating contexts, such as peer group school disengagement and parental involvement. As discussed previously, while interpretation of findings use the language of "mediation," "direct effects," and "indirect effects," such language is presented and should primarily be interpreted as statistical associations, not confident causal assertions.

Second, the current study assumes that the mechanisms through which the socio-economic composition of neighborhoods impact school dropout work similarly across all residential contexts. Potential heterogeneity with respect to mediating contexts is not examined. For example, it is possible, perhaps likely, that the ways neighborhood disadvantage influences school dropout varies across urban, rural, and suburban areas, or across geographical regions of the country. Moreover, the relative importance of school, neighborhood, and peer group contexts may vary depending on the level of neighborhood disadvantage. The findings discussed above are interpreted below with consideration to these limitations.

5.2.2 *Interpreting effects of schools*

The current study finds that measures of school quality, as indicated by school resources, are weakly correlated with both school dropout and neighborhood disadvantage in multivariate analytic models that control for individual characteristics and other relevant contexts. Among all school characteristics observed, the socio-demographic composition of schools is the strongest predictor of school dropout. In many ways, these findings are broadly consistent with many other multivariate observational studies (Rumberger, 1995; Rumberger & Thomas, 2000), including the widely influential *Equality of Educational opportunity* report (Coleman et al., 1966); however, some studies find that compositional characteristics of school appear less important when structural, resource, and school practice variables are measured more precisely (V. E. Lee & Burkam, 2003; Rumberger & Palardy, 2005a) . Given measurement limitations of school quality, self-selection into schools, and institutionally ingrained segregation within schools, disentangling the effects attributable to school quality, school composition, and individual and family characteristics has long been a notorious methodological challenge (Orfield & Lee, 2005; Rumberger & Palardy, 2004). Moreover, the current study does not include important measures of school resources, such as financial expenditures and teacher's salaries, both of which have shown to be protective against school dropout in rigorous quantitative studies (Lochner & Moretti, 2004; Pirog & Magee, 1997). Given these measurement and methodological limitations, cautious interpretations of the relationships between neighborhood disadvantage, school resources, and school dropout estimated in the current study is warranted.

The current study also finds that strict disciplinary policies within high schools in response to some types of student misconduct are associated with lower rates of school dropout. This finding appears inconsistent with previously documented detrimental effects of suspension on successful school completion (Noltemeyer, Ward, & McLoughlin, 2015). Moreover, prior qualitative inquiry highlights the role of harsh and unfairly administered disciplinary policies within schools serving poor students (Fine, 1985, 1986). On the other hand, like the current study, prior research using Add Health data has also found that strict disciplinary policies may protect students against school dropout (Babcock, 2009).[46] These seemingly divergent findings warrant further consideration to the populations affected by school disciplinary policies.

First, the effect of punitive disciplinary policies may be different for those youth who are actually disciplined (or who may be unfairly targeted for discipline) than for the general population of students who attend the school. The current study examines the effect of the latter population, while prior research has generally studied the former. Second, relevant qualitative inquiry and policy discussion has generally focused on disciplinary policies and school dropout within very poor, inner-city schools (e.g., Fine, 1991). It is possible that harsh and unfairly administered disciplinary policies may indeed "push out" struggling youth who experience multiple dimensions of structural disadvantage (Fine, 1986), while marginal increases in disciplinary policy strictness effectively deter misbehavior among students from relatively less disadvantaged family backgrounds embedded within relatively less disadvantaged school and neighborhood contexts.

[46] Unlike the current study, Babcock (2009) measures school disciplinary policy strictness as one dimension, aggregating all 24 possible infractions into a single index.

In general, severely disadvantaged school environments, often labeled "dropout factories," often form the basis for many theoretical assumptions about the links between poverty, schools, and student outcomes. "Dropout factories" are usually defined as schools with a 40% dropout rate or higher (Balfanz et al., 2013; Balfanz & Legters, 2004; Tucci, 2009). However, only 1 of 80 high schools in the current study (attended by <1% of analytic sample) meets this definition and only 3 high schools have a dropout rate of 30% or more (attended by 2.4% of the analytic sample). Therefore, associations between neighborhood poverty, school environments, and school dropout estimated in the current study should be interpreted as marginal changes for the "average" adolescent population. It is possible, and perhaps likely, that school characteristics operate entirely differently for severely disadvantaged youth embedded in severely disadvantaged school and neighborhood contexts. This consideration is especially relevant when interpreting school disciplinary policy effects, but applies generally to all contexts examined and discussed in the current study.

Perhaps most importantly, the current study also finds that while concentrated disadvantage at the school-level mediates much of the effect of concentrated disadvantage at the neighborhood-level, school fixed-effects specifications, which eliminate all unobserved heterogeneity across schools, mediate none, or perhaps moderately suppress, the effect of neighborhood disadvantage on school dropout. These seemingly inconsistent findings merit more nuanced reflections about interactions between family-, neighborhood-, and school-level disadvantage. Three plausible explanations are presented as follows.

First, the schools attended by many youth from disadvantaged neighborhoods may actually work to ameliorate rather than reinforce their risk of school failure. These schools systems may respond by more effectively implementing educational services and leveraging additional financial and material resources from local, state, and federal governments. As mentioned, schools' financial resources and non-disciplinary school practices are important school characteristics that go unobserved in the current study.

Second, relative deprivation hypotheses, which studies using Add Health data have partially supported (Owens, 2010), propose that youths' higher socio-demographic positions relative to their school peers positively affects achievement. Schools have been described as "frog ponds," where being a "small frog" (e.g., poor youth from a disadvantaged neighborhood) in a "big pond" (e.g., school attended by affluent students) may actually exacerbate, rather than mitigate, risk of school failure (Davis, 1966; Espenshade, Hale, & Chung, 2005). Because adolescents tend to compare themselves within rather than across schools, socio-demographically disadvantaged students may develop negative academic self-concepts when attending schools with substantially more advantaged peers. More affluent schools may also be more competitive and enforce more demanding grading and graduation requirements. If true, socio-economic segregation within schools may to some degree benefit, or at least not harm, youth from the most disadvantaged neighborhoods. This explanation is consistent with the large suppression effect of over 100% via school fixed-effects specifications observed for youth with the least educated parents; if youth from the low socio-economic backgrounds who live in more affluent neighborhoods are disadvantaged by attending more affluent schools, the true effect of local neighborhood context would be underestimated when the school

context is unobserved. Moreover, initial sociological evidence for relative deprivation within schools was based on a sample of high school males (Davis, 1966). If males are more susceptible to competition within schools, this may also explain why school fixed-effects suppress the neighborhood disadvantage effect by 23% for males, but mediate the effect by 20% for females.

Third, parents may choose to live in a marginally less advantaged neighborhood in order for their children to attend a marginally higher quality school. This explanation involves a simple self-selection bias, although in the reverse direction usually associated with neighborhood self-selection. Neighborhood composition and school quality both influence housing costs and the decision making about residential selection for parents with young children (Chen & Lin, 2011; Kim, Horner, & Marans, 2005). Given limited resources, residential selection for families with school aged children therefore involves a tradeoff between desirable school and neighborhood characteristics. To the extent that some families select a marginally less advantaged neighborhood within a higher quality school district, while other families select a marginally more advantaged neighborhood within a lower quality school district, unobserved heterogeneity across schools would be expected to underestimate the effect of local residential context. Both school quality and parental motivation would therefore be important omitted variables when estimating associations between neighborhood composition and students' academic outcomes. The potential omitted variable bias of parental motivation is consistent with relatively large suppression effects for peer group school disengagement (i.e., lower levels of disengagement in more disadvantaged neighborhoods, controlling for individual socio-demographic characteristics) among those most able to select into schools and

neighborhoods of their choice (White, non-Hispanic youth and youth with college educated parents). Moreover, those most likely constrained with respect to residential choice, African Americans, are the only demographic sub-group for which substantial mediating effects, rather than suppression effects, are observed via school fixed-effects.

5.2.3 *Interpreting effects of neighborhood social organization*

Unlike traditional theories of social organization, poor neighborhoods resided by youth in the current study are characterized by substantially higher levels of social cohesion and informal social control. These findings are consistent with more contemporary theory and ethnographic study, which highlight tightly interconnected social networks within very poor neighborhoods (Pattillo, 1998; St. Jean, 2008; Venkatesh, 1997; W. J. Wilson, 1996). Nonetheless, in the current study, higher levels of social cohesion and informal social control experienced by youth from disadvantaged neighborhoods are not protective against school dropout. These findings are inconsistent with findings from prior studies that uncover protective effects on children's developmental and educational outcomes for similar dimensions of social organizing (Bowen et al., 2002; Browning et al., 2008; Cohen et al., 2006; Nash, 2002; Xue, Leventhal, Brooks-Gunn, & Earls, 2005). These inconsistencies may be explained by limitations of both the current and prior studies.

First, the measures of social cohesion and informal social control used in the current study are notably different than the "social cohesion/mutual trust" and "shared expectations of informal social control" dimensions of collective efficacy later developed by Sampson and collogues and now broadly used as a measurement instrument. Two of the three survey indicators for social cohesion in the current study focus on knowledge of

and interactions with neighborhood residents; however, the "social cohesion/mutual trust" dimension of collective efficacy is generally measured based on whether neighbors are likely to help one another, can be trusted, get along, and share the same values. Survey indicators for informal social control in the current study measure the likelihood that neighbors would inform a child's parent if a child was "getting into trouble"; however, the "shared expectations of informal social control" dimension of collective efficacy assesses the likelihood that neighbors would actually intervene and "do something" if they saw a child engage in explicitly defined behaviors detrimental to their general wellbeing and academic progress, such as "skipping school and hanging out on a street corner". Therefore, the survey indicators for social cohesion and informal social control used in the current study may not accurately or fully measure the extent to which neighborhoods are socially cohesive and effectively regulatory of youth misbehavior in ways that promote positive educational outcomes for youth.

On the other hand, most of the prior literature with respect to measures of neighborhood social organization and collective efficacy has examined effects on youth health and emotional development (e.g., Browning et al., 2008; Browning & Cagney, 2002; Xue et al., 2005) rather than educational attainment, the outcome in the current study. Moreover, studies focused on educational outcomes have generally relied on individual subjects' reports of their own neighborhood environment (Caughy, Nettles, & O'Campo, 2008; Nash, 2002), likely correlated with unobserved characteristics of individual respondents (Towers et al., 2000). Finally, the most methodologically rigorous studies with respect to internal validity, including those using data from the Project on Human Development in Chicago Neighborhoods (PHDCN), are usually carried out

within a single urban area and are therefore limited with respect to external validity. General findings from PHDCN in Chicago have been replicated in Stockholm, Sweden (Sampson & Wikström, 2008), and neighborhood collective efficacy has shown to be protective against children's problem behaviors among a national British sample of youth who reside in poor neighborhoods (Odgers et al., 2009). However, given the findings of the current study, it is also possible that neighborhood social cohesion and informal social control within a national U.S. sample do not substantially affect educational attainment outcomes, or potentially interact with other family and neighborhood characteristics to exert influence.

While the current study does not find protective effects of neighborhood social cohesion and informal social control, the related theoretical construct of neighborhood intergenerational closure does appear to substantially decrease risk of school dropout and explains a substantial proportion of the neighborhood disadvantage-school dropout relationship. This direct protective effect on school dropout is consistent with theory and empirical study (Carbonaro, 1998; Coleman, 1988). While Harding (2009) did not find any association with high school graduation, the measure of neighborhood intergenerational closure included the survey items used to measure informal social control in the current study; as discussed previously, these survey items are not internally consistent, and as presented in the current study, differentially associated with both neighborhood disadvantage and school dropout. However, neighborhood intergenerational closure exerts little, if any, protective influence on school dropout for youth with the least educated parents and African American and Hispanic youth. These

findings suggest that not all youth may be similarly influenced by neighborhood-level intergenerational closure.

5.2.4 *Interpreting effects of peer groups*

Findings from the current study generally find that youth from disadvantaged neighborhoods are no more likely to affiliate with academically disengaged peers than demographically comparable youth from more advantaged neighborhoods. These findings are consistent with prior ethnographic and survey research, which has shown that poor children and their parents often profess strong educational aspirations and traditional views about the importance of educational achievement (Ainsworth-Darnell & Downey, 1998; A. L. Harris, 2006; Newman, 2009; Young, 2006). However, findings do not support traditional social isolation theories of neighborhood effects, nor are they consistent with prior research focused on mediating effects of disadvantaged neighborhoods. For example, South and colleagues (2003) found that educational characteristics of peer groups explained around a third of the effect of neighborhood disadvantage on school dropout. In this study, however, measures of peer groups were based on subjects' retrospective reports at the age of 18-22 of their friends' educational characteristics when they were 16; characteristics included their friends' grades, expectations of college attendance, and the proportion of friends who dropped out of schools. These measures are subject to multiple forms of bias, including recall bias and lack of knowledge. Moreover, temporal ordering is inappropriate for all subjects who withdrew from school prior to the age of 16.

Several noteworthy methodological limitations also warrant consideration with respect to the current study's findings. First, all characteristics of youths' friends who do not attend their school or who did not participate in the In-school survey are unobserved.

These students are likely to be less engaged in school than other students. For example, many may not attend their friends' current school because they attend no school at all, and those who did not complete the In-school survey may have refused or been absent (there was no makeup day), which may also be indicators of school disengagement. However, the current study does control for the proportion of youths' friends who did not attend their school and whose characteristics are therefore unobserved, which accounts for a large proportion of missing data on youths' friends and is generally consistent across neighborhood environment, condition on individual characteristics. Unobserved peer characteristics may bias the estimates presented in the current study if unobserved peer characteristics systematically differ across neighborhood environment. For example, it is plausible that youth from affluent neighborhoods may have friends who do not attend their school for different reasons (e.g., their friends attend private schools) than youth from poor neighborhoods (e.g., their friends dropped out of school).

A more substantial limitation relates to Add Health's sampling design. As discussed previously, the analytic sample is only representative of youth who attend school at baseline. Given that both neighborhood disadvantage and peer group school disengagement are strong additive determinates of school dropout – assumptions strongly supported by the current study – risk of not attending school, and therefore probability of sample exclusion, is highest for youth from the most disadvantaged neighborhoods with the most academically disengaged friends. In other words, among a theoretical sample of all youth with academically disengaged friends, those from disadvantaged neighborhoods are least likely to be observed in the current study, biasing estimates of peer group school disengagement downward.

There is some evidence of this bias in the current study. Most obviously, peer group characteristics are found to slightly suppress the neighborhood disadvantage-school dropout relationship, whereas theory would signify the opposite effect. These suppression effects are most pronounced among the youth who attend high school at baseline– the grade cohorts most likely to experience sample exclusion due to school dropout – whereas no evidence of any suppression effects are observed among younger youth who attend middle school at baseline – the grade cohorts least likely to experience sample exclusion due to school dropout. Moreover, among middle school students at baseline, no suppression effects via peer groups are observed; neighborhood disadvantage is associated with slightly higher levels of peer group emotional school disengagement, which mediates the neighborhood effect on school dropout by 3%.[47]

With consideration to these limitations, the current study still provides moderately robust evidence to conclude that school disengagement within peer groups is neither the primary nor a substantial mechanism through which neighborhood disadvantage exerts influence on school dropout. Given the rich set of controls across multiple ecological contexts, potential biases associated with unobserved peer characteristics would need to be unusually large to explain the consistent null (and often negative) findings for observable peer characteristics across all demographic sub-groups. Negative associations between neighborhood disadvantage and peer group school disengagement are likely attributable to potential biases associated with sample inclusion. However, given that the dropout rate of youth enrolled in Grades 7-8 baseline is nearly identical to national estimates during this time period (21%;Heckman & LaFontaine, 2010), the sample

[47] Model C of Table 11 also indicates that peer group truancy mediates effect of neighborhood disadvantage on school dropout among middle school students by 4%. However, the association between neighborhood disadvantage and peer group truancy is not statistically significant (p=.88; not shown).

inclusion bias for these youth is likely minimal and the analytic models for middle school students can be interpreted confidently. These models indicate that neighborhood disadvantage is a weak predictor of peer group school disengagement and peer group characteristics explain little of the effect of neighborhood disadvantage on school dropout.

5.2.5 *Interpreting direct effect of neighborhood disadvantage*

The primary research objective of the current study was to decompose the associations between neighborhood disadvantage and school dropout, estimating the extent to which theoretically relevant constructs mediate or "explain away" the direct effect of neighborhood disadvantage on school dropout. As discussed previously, the current study did not endeavor to provide precise causal estimates of the effect of neighborhood disadvantage on school dropout. However, findings from the current study indicate that little of the main effect is "explained away" after adjusting for a rich series of variables strongly predictive of school dropout and measured at multiple ecological levels. In particular, the inclusion of school fixed-effects specification eliminates all biases of unobserved heterogeneity at the school-level, further isolating effects of neighborhood disadvantage to the local residential context. As a result, the findings presented contribute to growing evidence that local residential contexts during childhood and adolescence exert causal influence on important long-term outcomes, including educational attainment. Moreover, the current study provides persuasive evidence that this effect is not entirely attributable to or confounded by differential school environments across neighborhoods.

However, causal inference in the current study is clearly constrained by methodological limitations. Most obviously, individuals and families are not randomly distributed across neighborhoods; therefore, unobserved individual-level variations across neighborhoods may confound the effect of neighborhood disadvantage estimated in the current study. Most notably, due to data limitations, the current study does not include a direct measure of family wealth. Although, prior studies that have used similar controls and included home ownership as proxies for wealth find similarly convincing effects of neighborhood disadvantage on educational and occupational outcomes (Crowder & South, 2003; Sampson & Sharkey, 2008; Sharkey, 2012; South et al., 2003; Wodtke et al., 2011). On the other hand, limitations of the current study may be equally, or perhaps more, likely to underestimate the effect of neighborhood context. First, the current study measures neighborhood residence at only one point in time during adolescence (average age of 16); however, supported by prior time series observational studies (Crowder & South, 2011; Wodtke et al., 2011), the most recent experimental and quasi-experimental evidence indicates that neighborhood context exerts influence on long-term outcomes through cumulative exposure, perhaps especially during pre-adolescent years (Chetty & Hendren, 2015; Chetty et al., 2015). Finally, as discussed previously, the current study holds constant time-varying family-level variables that are sensitive to residential context and known to exert influence on education outcomes (e.g., parental income, employment). This "included variable" bias (Sampson, 2012) may therefore "control away" important causal effects operating through the neighborhood environment.

5.2.6 *Unobserved mediating contexts*

The current study observes three sets of mediating variables as hypothesized by resource deprivation, social organization, and social isolation theories of neighborhood effects. However, other unobserved contexts are likely to partially mediate the effect of neighborhood disadvantage on school dropout. Therefore, the direct effect of neighborhood disadvantage, net of all contexts observed, should be interpreted cautiously. In particular, much of this effect may be explained by two noteworthy constructs unobserved in the current study.

First, using a similar sample of Add Health respondents, Harding (2009) found that neighborhood violence mediates almost half of the neighborhood disadvantage effect on high school graduation for boys and most of the effect for girls. In this study, neighborhood violence was broadly conceptualized to include aggregate measures of perceived safety, prevalence of drugs, and respondents' experience of and exposure to violence generally (not necessarily within neighborhood of residence). The current study conceptualized individual- and neighborhood-level violence and crime as social outcomes, operating through and therefore largely endogenous with measures of social organization and other mediating constructs examined. However, independent of these contexts, youths' exposure to neighborhood violence is very likely to contribute to adverse educational outcomes of youth (Sharkey, Schwartz, Ellen, & Lacoe, 2014; Sharkey, Tirado-Strayer, Papachristos, & Raver, 2012), perhaps especially for girls (Miller, 2008; Zuberi, 2012).

Second, local pollution and environmental health hazards are not examined in the current study, but are likely to contribute to poor health and cognitive outcomes for youth

who live in poor neighborhoods (Crowder & Downey, 2010; Wodtke et al., 2011). Residents in poor neighborhoods are more likely to live in housing units with indoor toxins and pollutants, located near major highways and industrial manufacturing centers (O'Neill, McMichael, Schwartz, & Wartenberg, 2007; Rosenfeld, Rudd, Chew, Emmons, & Acevedo-García, 2010). Adverse health and cognitive outcomes associated with these environmental hazards are likely to interfere with academic performance, consistent school attendance, and successful school completion (Moonie, Sterling, Figgs, & Castro, 2006; Schwartz, 2006).

5.3 Implications of findings

5.3.1 *Social work policy and practice*

The current study finds that concentrated disadvantage at the neighborhood-level remains an important determinate of school completion even among adolescents who attend the same school. However, around 12% of the total variation in school dropout is explained by the schools that adolescents attend, and several observable school characteristics are predictive of school completion, including teachers' level of experience teaching at the school. Therefore, findings do not imply that schools are unimportant or that low performing schools in poor communities are not in need of additional policy supports. State and federal policy that directs additional resources and supports to schools in poor communities, including efforts to retain more experienced teachers, would be expected to ameliorate some of the added risk of school dropout experienced by youth living in poor neighborhoods.

However, the current study does suggest that school-based policy interventions are unlikely to eliminate all disparities in educational outcomes observed across

neighborhoods. Findings indicate that an added risk of school dropout will remain for youth from more disadvantaged neighborhoods relative to other students within the same school. As a result, policy interventions that target neighborhood contexts and seek to bolster formal and informal institutions within the most disadvantaged communities are also recommended. Findings from the current study suggest that the organization of and support for informal academically supportive networks of parents may be most needed within poor neighborhoods and most effective at reducing school dropout. Models to assist professional practitioners build these types of community partnerships should focus on community strengths, democratic collaboration, and empowerment (Bryan & Henry, 2012). Moreover, poor neighborhoods can generally be characterized by relatively high levels of social interaction and willingness to report children's misbehavior to parents. While these characteristics are not associated with high school completion in the current study, these strengths should be considered and incorporated as foundations for the design of community-based initiatives.

At the individual-level, dropout prevention programs evaluated in the literature are generally found to support school completion in a cost-effective manner (Hahn et al., 2015; Qu, Chattopadhyay, Hahn, & Force, 2015; S. J. Wilson, Tanner-Smith, Lipsey, Steinka-Fry, & Morrison, 2011). Interventions generally target youth deemed most at risk to dropout, typically based on indicators of school disengagement and absenteeism, and provide ongoing monitoring, mentorship, and individualized academic and social supports. However, the current study finds that residence in a poor neighborhood is a strong risk factor for school dropout, independent of youths' socio-economic status and levels of school engagement. Therefore, neighborhood characteristics should be included

as important criteria in determining youth who are most likely to benefit from targeted interventions.

Moreover, the current study finds that adolescents from poor neighborhoods and the peers with whom they affiliate are generally engaged in school at levels comparable to students from more affluent neighborhoods. To the extent that youth from poor neighborhoods befriend peers who are less engaged in school, it is because their friends feel disconnected from the school community and are less likely to involve themselves in school-based extracurricular activities; however, these differences are generally explained by individual socio-demographic characteristics, not the neighborhood environment. More stigmatizing dimensions of school disengagement, such as truancy and lack of effort in learning, are not associated with the neighborhood environment. These findings are clearly inconsistent with traditional "culture of poverty" models, generally criticized for "blaming the victim" (W. Ryan, 1976), but also diverge from more contemporary and nuanced theoretical conceptualizations of culture as a response to structural constraint (Massey & Denton, 1993; Small et al., 2010; Small & Newman, 2001; W. J. Wilson, 1987). Social work practitioners should be cognizant of these findings when working with adolescents from distressed neighborhoods and careful to avoid making undue assumptions about academic motivations based on neighborhood residence.

Finally, a principal finding of the current study is that concentrated neighborhood disadvantage exerts large negative effects on school dropout independent of contexts examined, suggesting that the adverse consequence of residing in a poor neighborhood may not be solely attributable to differential school environments, neighborhood social

processes, or peer group behaviors. As a result, limited policy supports seeking marginal changes within poor communities may be only marginally effective. Instead, increasing school completion among disadvantaged children may best be achieved through public policy that encourages the integration of families from diverse socio-economic backgrounds within communities. Many potential remedies to residential segregation have been proposed, including development of affordable housing within middleclass communities, large expansions to housing voucher programs that maximize residential choice, public advertising campaigns to change attitudes and social norms, and tax incentives for owner-occupied homes in distressed neighborhoods (Cashin, 2004; Danziger & Lin, 2000; McClure, 2008; M. A. Turner, 1998). However, even as cities and neighborhoods are constantly changing, the "durability" of neighborhood inequality has been historically resistant to change (Sampson, 2012). Overcoming placed-based inequality is likely to require sustained large-scale policy commitments to equity in the distribution of resources and opportunities. Towards this end, the long and persistent history of institutionalized racial discrimination in public policy, public assistance programs, and private real estate industries cannot be overlooked (Massey & Denton, 1993; Rugh, Albright, & Massey, 2015; Sharkey, 2013; Squires & Kubrin, 2006). Some recent policy development are encouraging, such as the Affirmatively Furthering Fair Housing (AFFHR) rule, which requires Housing of Urban Development (HUD) grantees to identify residential segregation patterns by race and class and develop plans to promote more integrated living patterns. Continued policy efforts to reduce persistently high rates of racial residential segregation, and to stem the rise in socio-economic residential segregation, can be expected to translate into greater equality in educational opportunity.

5.3.2 *Future research*

Findings from the current study imply that more scholarly inquiry and empirical research is needed on the potentially complex interactions between school, neighborhood, and peer group environments in order to better understand the mechanisms through which concentrated neighborhood disadvantage exerts influence on children's educational outcomes. Neither traditional theories of resource deprivation, which stress unequal school quality, nor traditional theories of social isolation, which stress alternative norms within peer groups, appear adequate to fully explain why children from poor neighborhoods fare worse than their peers from more affluent neighborhoods.

Two related theoretical perspectives may be helpful to inform related research seeking further understanding. First, relative deprivation theories consider children's position within a given environment relative to their peers, including within and between neighborhoods and schools (Davis, 1966; Jencks & Mayer, 1990; Owens, 2010). How and to what extent the demographic composition of social environments influence educational outcomes for children, if at all, may therefore largely depend on the unique demographic position of the individual child. Second, cultural heterogeneity theories of neighborhood effects consider the variability in local norms. Contemporary scholars have theorized that disadvantaged neighborhoods are best characterized by high levels of competing and conflicting cultural models, which include both "mainstream" or "middleclass" *and* "oppositional" and "ghetto specific" models (Harding, 2007; Small, 2004). Empirical research finds that adolescents from more culturally heterogeneous neighborhoods are less likely to follow through with their stated educational goals (Harding, 2011). Future conceptual models that seek to explain how concentrated

neighborhood disadvantage affects children's educational outcomes should build upon and extent traditional theories by integrating relative deprivation within schools and cultural heterogeneity within communities.

While the findings of the current study are ambiguous with respect to supporting traditional theories of neighborhood effects, given its limitations, they do not warrant disregard without more rigorous research. The current study examines the mediating role of school contexts attended by adolescences and levels of school engagement as reported by their friends. However, unequal access to and quality of institutions experienced earlier in childhood may be more important in explaining the effect of residential context on long-term educational outcomes. Major behavioral and academic risk factors for school dropout emerge well before middle and high school years (Alexander, Entwisle, & Horsey, 1997; Alexander et al., 2001). High quality early childhood and pre-school programs lead to long-term positive outcomes, including high school completion (Deming, 2009; G. J. Duncan & Magnuson, 2013; Ludwig & Miller, 2007; Magnuson & Shager, 2010), and the quality of primary schools is strongly correlated with upward social mobility within communities (Chetty et al., 2015). Therefore, future research should also examine the role of these community institutions known to support positive youth development, which are likely to vary in availability and quality across neighborhood environments.

Additionally, cultural explanations of placed-based inequality generally focus on the emergence and transmission of aggressive, oppositional, and rule-breaking norms (Anderson, 1999; MacLeod, 1987; Willis, 1977) and these attitudinal and behavioral constructs are extremely predictive of future school dropout (Battin-Pearson et al., 2000;

Ou et al., 2007; Suh et al., 2007; Sweeten, 2006). It is plausible that neighborhood variation in social norms within peer groups better explains residential disparities than neighborhood variation in academic norms. Therefore, future research should also examine the role of social dimensions within peer groups as mechanisms through which residential context influences educational outcomes, such as school dropout.

In regard to theories of social organization, more research is needed using population-based samples and reliable measurement techniques so that specific findings can be interpreted more confidently and generalized more broadly. Findings from the current study suggest that supportive neighborhood social processes among parents (i.e., intergenerational closure) may be more important for children's educational outcomes than other social processes among neighborhood residents (i.e., social cohesion, informal social control). Future research on neighborhood collective efficacy may therefore benefit from a specific focus on informal networks among school aged parents. However, these parental networks do not appear influential for African American and Hispanic youth in the current study. More generally, substantial variation in associations between neighborhood composition, neighborhood social processes, and school dropout are uncovered. Future research should therefore consider how individual and neighborhood factors interact with specific neighborhood social processes to influence educational outcomes.

The current study examines relationships between neighborhood disadvantage and relevant school, neighborhood and peer group contexts among a national sample of diverse neighborhoods. Due to highly segregated residential patterns in the U.S., the dissimilarly of many residential contexts may be too great to reach confident conclusions

about how neighborhood composition influences educational outcomes for all children in all types of neighborhoods. Future research should focus examination on these mechanisms within more homogenous populations and residential contexts.

Finally, given methodological limitations, causal interpretations of mediating contexts cannot be made confidently in the current study. Evidence in support of causal neighborhood effects continues to grow (Chetty & Hendren, 2015; Chetty et al., 2015). However, causal interpretations with respect to the mechanisms through which these placed-based effects exert influence are generally lacking. More must certainly be learned from experimental designs that include random assignment, and quasi-experimental designs that treat naturally occurring exogenous events as instrumental variables. However, observational studies using longitudinal data should also take advantage of changes observed over time within the contexts and institutions in which individuals are embedded. Recent methodological approaches that treat residence in changing neighborhoods as explanatory variables have made important contributions (see Leventhal & Brooks-Gunn, 2011; Sharkey, 2012). Similar approaches should be developed that consider interactions between changing neighborhood, school, and peer group environments. Finally, the methods adopted by future research on neighborhood contexts should be closely aligned with the specific theoretical mechanisms under study and consider variations across time, place, and persons (Sharkey & Faber, 2014b). Equipped with more appropriate theory and methods, future research may provide a more complete understanding of the mechanisms that link residential context and educational opportunity.

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Table 1. Descriptive statistics of analytic variables

	Mean	St. Dev.	Range
School dropout (%)	15		
Neighborhood socio-economic disadvantage			
Persons below poverty line (mean %) *	15	12	[1-86]
Males unemployed (mean %) *	8	5	[1-94]
Female headed households with children (mean %) *	18	12	[1-93]
Adults with no high school degree (mean %) *	29	14	[1-87]
Adults with college degree (mean %; reverse) *	23	13	[1-82]
Employed prof./mang. occupation (mean %; reverse) *	23	10	[2-75]
Individual, family, and household characteristics			
Youth race/ethnicity (%)			
White, non-Hispanic	54		
Black, non-Hispanic	22		
Hispanic, any race	16		
Other race, non-Hispanic	8		
Youth male gender (%)	48		
Youth low birth weight (%)	11		
Youth foreign-born (%)	8		
Youth grade at baseline (%)			
7th grade	14		
8th grade	13		
9th grade	18		
10th grade	20		
11th grade	19		
12th grade	16		
Mother age at youth's birth (years)	25.1	5.4	[12-53]
Parent foreign-born (%)	19		
Parent highest education (%)			
No high school diploma	22		
High school diploma only	26		
Some college/vocational	28		
College degree or higher	24		
Household employment (%)	90		
Household family structure (%)			
Married biological parents	48		
Single biological mother only	20		
Other living arrangement	31		
Household size (persons)	4.6	1.6	[1-21]
Household moved residence in past year (%)	15		
Household welfare receipt in past year (%)	13		
Household income ($1,000)	45.9	52.3	[0-999]
School characteristics			
Pupil-per-teacher ratio	18.8	4.4	[7-27]
Teachers with Master's degrees (mean %)	47	26	[0-95]
Teachers <1 year at school (mean %)	10	21	[0-98]
Teachers >5 years at school (mean %)	65	15	[0-99]
School-based health and social services	4.9	3.1	[0-14]

Violent/aggressive misconduct disciplinary strictness			
Fighting with another student, first occurrence *	2.8	0.5	[0-4]
Fighting with another student, second occurrence *	3.1	0.5	[1-4]
Injuring another student, first occurrence *	2.8	0.6	[1-4]
Injuring another student, second occurrence *	3.4	0.6	[1-4]
Verbally abusing a teacher, first occurrence *	2.2	0.9	[0-4]
Verbally abusing a teacher, second occurrence *	3.1	0.7	[1-4]
First time drug/alcohol misconduct disciplinary strictness			
Possessing alcohol, first occurrence *	2.9	0.6	[1-4]
Possessing an illegal drug, first occurrence *	3.1	0.6	[1-4]
Drinking alcohol at school, first occurrence *	3.0	0.5	[1-4]
Using an illegal drug at school, first occurrence *	3.2	0.5	[2-4]
Repeated major misconduct disciplinary strictness			
Possessing an illegal drug, second occurrence *	3.8	0.4	[3-4]
Possessing a weapon, second occurrence *	3.9	0.3	[3-4]
Using an illegal drug at school, second occurrence *	3.8	0.4	[3-4]
Physically injuring a teacher, second occurrence *	3.9	0.2	[3-4]
Minor misconduct disciplinary strictness			
Cheating, first occurrence *	1.1	0.7	[0-3]
Cheating, second occurrence *	1.9	0.6	[1-4]
Smoking at school, first occurrence *	2.1	0.9	[0-3]
Smoking at school, second occurrence *	2.6	0.7	[0-4]
Stealing, first occurrence *	2.8	0.5	[1-4]
Stealing, second occurrence *	3.4	0.5	[1-4]
Students enrolled	1372	826	[85-3546]
Includes middle school grades (%)	21		
Organization (%)			
Public school	93		
Private, Catholic school	3		
Private, non-Catholic school	4		
Urbanicity (%)			
Urban	29		
Suburban	54		
Rural	17		
Socio-economic disadvantage			
Size-adjusted household income ($1000) *	22.4	10.9	[9-90]
Parents unemployed (mean %) *	8	4	[0-27]
Female headed households (mean %) *	27	11	[0-63]
Parents with no high school diploma (mean %) *	16	12	[0-54]
Parents with college degree (mean %; reverse) *	32	16	[8-91]
Parents prof./mang. occupation (mean %; reverse) *	28	12	[0-79]
Racial/ethnic composition			
White, non-Hispanic (mean %)	53	35	[0-100]
Black, non-Hispanic (mean %)	22	25	[0-98]
Hispanic, any race (mean %)	16	22	[0-89]
Other race, non-Hispanic (mean %)	9	12	[0-64]
Neighborhood characteristics			
Social cohesion			
Know most people in neighborhood (mean %) *	47	22	[0-100]

Stopped on street to talk with someone (mean %) *	78	17	[0-100]
People look out for one another (mean %) *	72	18	[0-100]
Neighborhood informal social control			
Parent tells neighbor if child gets in trouble *	3.3	0.37	[0-4]
Neighbor tells parent if child gets in trouble *	2.9	0.41	[0-4]
Neighborhood intergenerational closure			
Number of parents of youth's friends talked to in past month *	2.0	0.9	[0-6]
Racial diversity			
White (mean %) *	73	29	[0-100]
Black (mean %) *	17	27	[0-100]
Asian (mean %) *	5	12	[0-84]
Other (mean %) *	5	9	[0-73]
Hispanic ethnic diversity (mean %)	11	20	[0-96]
Units housing current household <5 years (mean %)	45	12	[13-97]
Peer group characteristics			
Truancy			
Frequency skip school	0.6	0.9	[0-6]
Extracurricular school disengagement			
Participate in no extracurricular activities (mean %)	17	27	[0-100]
Emotional school disengagement			
Don't feel close to people at school *	1.32	0.7	[0-4]
Don't feel like a part of school *	1.33	0.8	[0-4]
Not happy to be at school *	1.38	0.8	[0-4]
Cognitive school disengagement			
Don't try hard to do well at school	0.7	0.4	[0-3]
Not in youth school (mean %)	24	29	[0-100]

Notes: Statistics are presented at the individual-level and based on 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). Indicator variables used to construct indices, which were not included in multiple imputation models, are indicated by an asterisk (*) and do not include multiply imputed values. School disciplinary policy indicator variables are constructed from administrator reports: 0="Verbal warning," 1="Minor action," 2="In-school suspension," 3="Out-of-school suspension," and 4="Expulsion". Neighborhood informal social control and peer group school disengagement indicator variables are constructed using the Likert scale of agreement: 0="Strongly disagree"; 1="Disagree"; 2="Neither disagree nor agree"; 3="Agree"; 4="Strongly agree". The intergenerational closure response is censored at 6 or more parents by the survey instrument. Peer group truancy is constructed from survey responses: 0="Never"; 1="Once or twice"; 2="Once a month or less"; 3="2 or 3 days a month"; 4="Once or twice a week"; 5="3 to 5 days a week"; 6="Nearly every day". Cognitive school disengagement is constructed from survey responses: 0="I try very hard to do my best"; 1="I try hard enough, but not as hard as I could"; 2="I don't try very hard"; 3="I never try at all".

Table 2. Bivariate associations between neighborhood disadvantage and school dropout

	Pearson *r* coef.	Percent Dropout					
		All	By Neighborhood Disadvantage Quartile				
			1 Q.	2 Q.	3 Q.	4 Q.	F-test
Full sample	0.13 ***	15	9	14	17	21	***
By youth gender							
Male	0.14 ***	17	10	16	19	25	***
Female	0.12 ***	14	7	12	14	18	***
By youth race/ethnicity							
White, non-Hispanic	0.15 ***	14	8	11	15	21	***
Black, non-Hispanic	0.11 ***	16	11	14	19	21	***
Hispanic, any race	0.07 ***	20	16	17	22	24	**
By highest parental education							
No high school diploma	0.04 *	27	26	26	28	29	
High school diploma only	0.11 ***	15	11	14	17	20	***
Some college/vocational	0.07 ***	12	10	11	13	15	***
College degree or higher	0.08 ***	6	3	5	7	8	***
By youth grade at baseline							
Middle school (7-8)	0.17 ***	21	12	17	23	30	***
High school (9-12)	0.11 ***	14	8	12	15	17	***

Notes: Statistics are presented at the individual-level and are based on 16,919 unique youth from 2,152 neighborhoods (there are no missing data on youths' neighborhood disadvantage or school dropout status). Youth with missing information for race/ethnicity (<1%) or parental education (1.5%) due to non-response are excluded from respective sub-group analyses. Neighborhood disadvantage quartiles correspond to quartiles constructed within each sub-sample. Statistically significant associations between neighborhood disadvantage and school dropout are indicated by p<.05*, .01**, .001***. Tests of statistical significance are calculated using t-tests for Pearson *r* correlation coefficients and F-tests for mean differences in neighborhood disadvantage quartiles. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors.

Table 3. Bivariate associations between neighborhood disadvantage and high school, neighborhood, and peer group contexts

	Pearson *r* coef.	By Neighborhood Disadvantage Quartile				
		1 Q.	2 Q.	3 Q.	4 Q.	F-test
School resources						
Pupil-per-teacher ratio	-0.05	18.6	19.6	19.0	18.1	
Teachers Masters degree (mean %)	-0.10	54	42	45	47	†
Teachers >5 years at school (mean %)	-0.05	67	64	64	66	
Teachers <1 year at school (mean %)	-0.16	15	8	9	9	
School-based services	-0.08	5.2	5.4	4.4	4.5	
School disciplinary policy strictness						
Violent/aggressive (z-score)	0.07	-0.05	0.02	0.03	0.00	
First time drug/alcohol (z-score)	-0.07	-0.04	0.04	0.20	-0.20	
Repeated major (z-score)	0.03	-0.11	0.07	0.01	0.05	
Other minor (z-score)	0.17 †	-0.33	0.10	0.09	0.14	
School structure and composition						
School size	0.00	1438	1325	1318	1406	
Includes middle school (%)	0.02	18	19	20	26	
Public school (%)	0.03 †	85	96	95	96	†
Private Catholic school (%)	-0.02	7	1	1	1	
Private non-Catholic school (%)	-0.02	8	2	3	3	
Urban school (%)	0.04	31	20	24	41	*
Suburban school (%)	-0.09 *	64	60	55	36	**
Rural school (%)	0.04	5	20	20	22	†
Socio-economic disadvantage (z-score)	0.58 ***	-0.52	-0.09	0.11	0.54	***
White, non-Hispanic (mean %)	-0.29 ***	61	63	53	36	***
Black, non-Hispanic (mean %)	0.32 **	15	14	24	34	**
Hispanic, any race (mean %)	0.15	13	14	15	23	
Other race, non-Hispanic (mean %)	-0.11	11	9	8	6	
Neighborhood characteristics						
Social cohesion (z-score)	0.16 ***	-0.30	0.03	0.10	0.17	**
Informal social control (z-score)	0.19 ***	-0.18	-0.11	0.06	0.24	**
Intergenerational closure (z-score)	-0.39 ***	0.45	0.11	-0.07	-0.49	***
Racial diversity index (0-75)	0.22 ***	18.8	21.8	26.4	33.0	***
Hispanic ethnicity (mean %)	0.26 ***	4	8	11	20	***
Residential instability (mean %)	-0.12 **	48	44	43	43	**
Peer group characteristics						
Truancy (0-6)	-0.01	0.62	0.65	0.65	0.62	
Extracurricular disengagement (mean %)	0.06 ***	14	16	18	19	***
Emotional school disengagement (0-4)	0.08 ***	1.31	1.38	1.40	1.44	***
Cognitive school disengagement (0-3)	-0.05 ***	0.75	0.78	0.76	0.71	***
Friends not in youth school (mean %)	0.05 ***	23	22	23	26	***

Notes: Statistics are presented at the individual-level and are based on 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). Response ranges are shown in parentheses for contextual variables constructed from ordinal scales. Neighborhood disadvantage quartiles correspond to quartiles constructed on the full sample. Statistically significant associations between neighborhood disadvantage and contextual variables are indicated by p<.10†, .05*, .01**, .001***. Tests of statistical significance are calculated using t-tests for Pearson r

correlation coefficients and F-tests for mean differences in neighborhood disadvantage quartiles. For school- and neighborhood-level contextual variables, standard errors are adjusted for clustering of youth within schools and neighborhoods, respectively, using Huber–White robust standard errors.

Table 4. Logistic regression models predicting school dropout, partially specified

	Model 1	Model 2	Model 3	Model 4
Neighborhood disadvantage		1.40 ***	1.41 ***	1.18 ***
		(10.12)	(9.97)	(4.55)
School grade at baseline (Ref=7th grade)				
8th grade			1.15 †	1.16 †
			(1.77)	(1.85)
9th grade			0.98	1.00
			(0.22)	(0.07)
10th grade			0.70 ***	0.70 ***
			(4.54)	(4.41)
11th grade			0.47 ***	0.48 ***
			(9.14)	(8.51)
12th grade			0.23 ***	0.24 ***
			(14.35)	(13.67)
Report of school dropout (Ref=Wave IV)				
School dropout reported at Wave III			1.47 ***	1.43 ***
			(5.92)	(5.20)
Covariates:				
Individual socio-demographic controls	No	No	No	Yes
Variance components				
School variance	0.472	0.342	0.339	0.195
Neighborhood variance within schools	0.209	0.106	0.112	0.049
Intraclass correlation coefficient (ICC)				
School-level	0.119	0.091	0.091	0.055
Neighborhood-level (includes schools)	0.171	0.120	0.121	0.069

Notes: Exponentiated logit coefficients (odds ratios) and z-statistics of coefficients are shown for neighborhood disadvantage and select individual-level variables. Analyses are based on the full analytic sample of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). Results are obtained from three-level hierarchically nested logistic regression models with random intercepts for schools and neighborhood; refer to Equation (1). Intraclass correlation coefficients are calculated using the linear threshold model method. The neighborhood disadvantage index is standardized to mean 0 and standard deviation 1. Individual socio-demographic controls include: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Refer to Appendix D for the decomposition analysis of the neighborhood disadvantage effect estimated in Model 4. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Table 5. Logistic regression models predicting school dropout, fully specified

	Model 1 School random effects		Model 2 School fixed-effects	
Neighborhood disadvantage	1.11 (2.50)	**	1.12 (2.83)	**
School resources				
Pupil-per-teacher ratio	1.00 (0.02)			
Pupil-per-teacher ratio-squared	1.01 (0.24)			
Teachers Masters degrees	1.05 (0.89)			
Teachers masters degrees-squared	1.01 (0.14)			
Teachers >5 years at school	0.85 (2.37)	*		
Teachers >5 years at school-squared	0.95 (1.22)			
Teachers <1 year at school	1.13 (1.11)			
Teachers <1 year at school-squared	0.98 (0.85)			
School-based services	1.02 (0.41)			
School-based services-squared	1.00 (0.02)			
School disciplinary policy strictness				
Violent/aggressive misconduct	0.92 (1.31)			
Violent/aggressive-squared	1.05 (2.01)	*		
First time drug/alcohol misconduct	1.09 (1.74)	†		
First time drug/alcohol-squared	0.97 (1.12)			
Repeated major misconduct	1.05 (0.51)			
Repeated major-squared	1.00 (0.02)			
Minor misconduct	0.90 (2.12)	*		
Minor-squared	1.00 (0.15)			
School structure and composition				
School size	0.93 (1.09)			
School-size squared	1.02 (0.41)			
School includes middle school	0.71 (2.25)	*		
Private, Catholic (Ref=Public)	0.37 (2.95)	**		
Private, non-Catholic (Ref=Public)	0.41 (2.34)	**		
Suburban school (Ref=Urban)	1.06 (0.55)			
Rural school (Ref=Urban)	1.26 (1.44)			
Socio-economic disadvantage	1.22 (2.47)	*		
Socio-economic disadvantage-squared	0.95 (1.27)			
Percent Black, non-Hispanic	0.94 (1.05)			
Percent Hispanic, any race	0.83 (1.83)	†		
Percent Other race/ethnicity	0.97 (0.48)			
Neighborhood characteristics				
Social cohesion	0.99 (0.34)		1.01 (0.19)	
Social cohesion-squared	1.01 (0.37)		1.02 (0.79)	
Informal social control	1.00 (0.00)		1.02 (0.39)	
Informal social control-squared	0.99 (0.30)		1.01 (0.56)	
Intergenerational closure	0.87 (3.25)	***	0.87 (3.12)	**
Intergenerational closure-squared	0.97 (1.13)		0.96 (1.62)	
Racial diversity	1.04 (0.58)		0.99 (0.18)	
Racial diversity-squared	0.97 (0.77)		1.00 (0.01)	

Hispanic ethnic diversity	1.11 (0.83)			1.19 (1.23)	
Hispanic ethnic diversity-squared	0.97 (0.96)			0.95 (1.35)	
Residential instability	1.06 (1.50)			1.04 (1.00)	
Residential instability-squared	0.99 (0.55)			1.00 (0.20)	
Peer group characteristics					
Truancy	1.29 (5.09)	***		1.29 (5.01)	***
Truancy-squared	0.96 (3.24)	**		0.96 (3.26)	**
Extracurricular disengagement	1.20 (3.59)	***		1.19 (3.42)	**
Extracurricular-squared	1.00 (0.07)			1.00 (0.14)	
Emotional school disengagement	1.10 (2.70)	**		1.11 (2.96)	**
Emotional-squared	1.00 (0.03)			1.00 (0.12)	
Cognitive school disengagement	1.04 (1.28)			1.05 (1.46)	
Cognitive-squared	1.03 (1.87)	†		1.03 (1.87)	†
Friends not in youth school	1.36 (7.60)	***		1.37 (7.64)	***
Friends not in youth school-squared	0.98 (1.01)			0.98 (1.00)	
N					
Youth	16,919			16,664	
Neighborhoods	2,152			2,049	
Schools	80			78	
Variance components					
Between schools	0.035				
Between neighborhoods within schools	0.039				
Intraclass correlation coefficient (ICC)					
School-level	0.010				
Neighborhood-level (includes schools)	0.022				

Notes: Exponentiated logit coefficients (odds ratios) and z-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The Model 1 sample includes the full analytic sample of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set); results are obtained from the three-level hierarchically nested logistic regression model with random intercepts for schools and neighborhood as expressed in Equation 2. Intraclass correlation coefficients are calculated using the linear threshold model method. The Model 2 sample excludes 255 youth from 103 neighborhoods and 2 schools where no within-school variation in school dropout is observed (all 255 youth reported high school diploma receipt); results are obtained from the high school fixed-effects logistic regression model as expressed in Equation 3. All variables except dummy variables are standardized to mean 0 and standard deviation 1. Both models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. For Model 2, standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Table 6. Decomposition of neighborhood disadvantage effect on school dropout with school and neighborhood random effects

	Effect [%]	β(YZ•XC)		Θ(ZX•C)	
		Z	Z-squared	Z	Z-squared
Total	0.161 [100]				
Direct, β(YX•Z)	0.104 [65]				
Indirect, β(YZ•X) × Θ(ZX)	0.057 [35]				
via School resources					
Pupil-per-teacher ratio	-0.001 [0]	-0.001	0.009	-0.103	-0.090
Teachers Masters degree	-0.002 [-1]	0.050	0.006	-0.048	0.000
Teachers >5 years at school	0.004 [2]	-0.168 *	-0.052	-0.024	0.008
Teachers <1 year at school	-0.001 [-1]	0.126	-0.022	-0.174	-0.955
School-based services	-0.001 [-1]	0.019	0.001 †	-0.043	0.005
via School disciplinary policies					
Violent/aggressive	-0.015 [-9]	-0.081	0.053 *	0.034	-0.227 †
First time drug/alcohol	-0.021 [-13]	0.083 †	-0.031	-0.097	0.434 **
Repeated major	0.002 [1]	0.051	0.001	0.046	-0.085 **
Minor	-0.022 [-14]	-0.104 *	-0.005	0.219 *	-0.209
via School structure and composition					
School size	0.011 [7]	-0.078	0.022	-0.068	0.277 *
Includes middle school	-0.010 [-6]	-0.338 *		0.031	
Private, Catholic	0.014 [9]	-0.988 **		-0.015	
Private, non-Catholic	0.000 [0]	-0.889 **		0.000	
Suburban school	-0.005 [-3]	0.062		-0.087 *	
Rural school	0.014 [9]	0.232		0.060 *	
Socio-economic disadvantage	0.089 [55]	0.195 ***	-0.054	0.445 ***	-0.041
Black, non-Hispanic	-0.009 [-6]	-0.066		0.141 *	
Hispanic, any race	-0.013 [-8]	-0.187 †		0.067 *	
Other race, non-Hispanic	0.005 [3]	-0.032		-0.142	
via Neighborhood characteristics					
Social cohesion	-0.001 [-1]	-0.014	0.008	0.154 **	0.095
Informal social control	-0.001 [-1]	0.000	-0.006	0.115 **	0.239 ***
Intergenerational closure	0.041 [26]	-0.143 **	-0.032	-0.243 ***	-0.217 †
Racial diversity	-0.001 [-1]	0.035	-0.035	0.078 *	0.118 **
Hispanic ethnic diversity	-0.001 [-1]	0.108	-0.033	0.216 ***	0.749 ***
Residential instability	-0.007 [-4]	0.063	-0.012	-0.161 ***	-0.251 ***
via Peer group characteristics					
Truancy	-0.006 [-4]	0.252 ***	-0.042 **	-0.043 ***	-0.111 **
Extracurricular disengagement	-0.003 [-2]	0.184 ***	0.001	-0.014	-0.054 *
Emotional school disengagement	0.000 [0]	0.092 **	0.000	0.005	-0.024
Cognitive school disengagement	-0.001 [0]	0.039	0.029 †	0.001	-0.023
Friends not in youth school	-0.001 [-1]	0.309 ***	-0.022	-0.002	0.037 *

Notes: Total, direct and indirect effects are reported in logit metric (percentage shown in brackets) obtained from the decomposition of the neighborhood disadvantage coefficient from the logistic regression of high school dropout with random effects for school and neighborhood intercepts; refer to Equation 2 for model specification. All coefficients and indirect effects are calculated using the KHB method (refer to Equations 5-7). The sample consists of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). All variables presented are standardized to mean 0

and standard deviation 1 except dummy variables for school organization and urbanicity. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Table 7. Decomposition of neighborhood disadvantage effect on school dropout with school fixed-effects

	Effect [%]	β(YZ•XC)		Θ(ZX•C)	
		Z	Z-squared	Z	Z-squared
Total	0.122 [100]				
Direct, β(YX•Z)	0.116 [95]				
Indirect, β(YZ•X) × Θ(ZX)	0.006 [5]				
via High school fixed-effects	-0.020 [-16]				
via Neighborhood characteristics					
Social cohesion	0.003 [3]	0.008	0.019	0.144 **	0.108
Informal social control	0.005 [4]	0.016	0.011	0.112 *	0.243 ***
Intergenerational closure	0.039 [32]	-0.138 **	-0.046 †	-0.227 ***	-0.161
Racial diversity	-0.001 [-1]	-0.011	-0.001	0.073 *	0.116 **
Hispanic ethnic diversity	-0.001 [-1]	0.172	-0.051	0.215 ***	0.748 ***
Residential instability	-0.008 [-7]	0.043	0.005	-0.164 ***	-0.259 ***
via Peer group characteristics					
Truancy	-0.007 [-5]	0.253 ***	-0.043 **	-0.046 ***	-0.113 **
Extracurricular disengagement	-0.003 [-3]	0.175 **	0.003	-0.019	-0.059 *
Emotional school disengagement	0.000 [0]	0.101 **	-0.002	0.004	-0.024
Cognitive school disengagement	-0.001 [0]	0.045	0.029 *	0.002	-0.024
Friends not in youth school	0.000 [0]	0.312 ***	-0.022	0.003	0.040 **

Notes: Total, direct and indirect effects are reported in logit metric (percentage shown in brackets) obtained from the decomposition of the neighborhood disadvantage coefficient from the logistic regression of high school dropout with school fixed-effects; refer to Equation 3 for model specification. All coefficients and indirect effects are calculated using the KHB method (refer to Equations 5-7). The sample consists of 338,380 observations across 20 imputed data sets (16,664 unique youth from 2,049 neighborhoods and 78 high schools per data set). All variables presented are standardized to mean 0 and standard deviation 1. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Table 8. Linear probability model decompositions of neighborhood disadvantage effect on high school dropout stratified by youths' gender

	Full sample		Male		Female	
Model A:						
Neighborhood disadvantage	0.011 (3.30) **		0.013 (2.63) **		0.010 (2.30) *	
Model B:						
Neighborhood disadvantage	0.011 (2.39) *		0.017 (2.39) *		0.006 (0.95)	
School fixed-effects	*Varies*	[-6]	*Varies*	[-23]	*Varies*	[20]
Neighborhood characteristics						
Social cohesion	0.000 (0.01)	[2]	-0.005 (0.70)	[-5]	0.003 (0.61)	[8]
Social cohesion-squared	0.002 (0.71)		0.001 (0.22)		0.002 (0.69)	
Informal social control	0.002 (0.38)	[5]	-0.005 (0.64)	[-4]	0.007 (1.31)	[14]
Informal social control-squared	0.001 (0.66)		0.000 (0.06)		0.003 (0.74)	
Intergenerational closure	-0.010 (2.11) *	[22]	-0.009 (1.18)	[17]	-0.012 (1.88) †	[28]
Intergenerational closure-squared	0.000 (0.16)		0.000 (0.01)		0.001 (0.30)	
Racial diversity	0.001 (0.15)	[3]	0.011 (1.04)	[12]	-0.008 (0.92)	[-3]
Racial diversity-squared	0.002 (0.45)		0.003 (0.44)		0.001 (0.23)	
Hispanic ethnic diversity	0.005 (0.34)	[-11]	-0.015 (0.67)	[-13]	0.022 (1.15)	[-10]
Hispanic ethnic diversity-squared	-0.003 (0.78)		0.002 (0.35)		-0.007 (1.41)	
Residential instability	0.002 (0.41)	[-4]	0.007 (1.11)	[4]	-0.004 (0.69)	[-15]
Residential instability-squared	0.001 (0.23)		-0.006 (1.82) †		0.008 (2.33) *	
Peer group characteristics						
Truancy	0.031 (5.03) ***	[-7]	0.042 (4.50) ***	[-9]	0.021 (2.86) **	[-5]
Truancy-squared	-0.005 (2.98) **		-0.008 (2.75) **		-0.003 (1.53)	
Extracurricular disengagement	0.017 (2.66) **	[-4]	0.013 (1.36)	[-5]	0.023 (2.87) **	[-2]
Extracurricular-squared	0.003 (1.28)		0.005 (1.34)		0.001 (0.21)	
Emotional school disengagement	0.011 (3.02) **	[0]	0.014 (2.65) **	[1]	0.008 (1.61)	[0]
Emotional-squared	0.001 (0.28)		0.001 (0.38)		0.000 (0.19)	
Cognitive school disengagement	0.005 (1.30)	[-1]	0.003 (0.43)	[0]	0.007 (1.54)	[-1]
Cognitive-squared	0.003 (1.66) †		0.004 (1.13)		0.003 (1.16)	
Friends not in youth school	0.031 (6.52) ***	[-1]	0.038 (5.11) ***	[-4]	0.027 (4.51) ***	[3]
Friends not in youth school-squared	0.001 (0.24)		0.000 (0.05)		0.001 (0.22)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "Full sample" model includes the full analytic sample of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). The "Male" model includes the sample of 162,820 observations across 20 imputed data sets (8,141 unique youth from 1,520 neighborhoods and 80 high schools per data set) who reported male gender. The "Female" model includes the sample of 175,560 observations across 20 imputed data sets (8,778 unique youth from 1,553 neighborhoods and 80 high schools per data set) who reported female gender. All results are obtained from the high school fixed-effects linear probability model as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Table 9. Linear probability model decompositions of neighborhood disadvantage effect on high school dropout stratified by parents' educational degree

	No high school diploma		High school diploma		College degree or more	
Model A:						
Neighborhood disadvantage	0.009 (1.07)		0.013 (3.10) **		0.008 (2.03) *	
Model B:						
Neighborhood disadvantage	0.020 (1.75) †		0.012 (2.01) *		0.010 (1.60)	
School fixed-effects	*Varies*	[-107]	*Varies*	[-34]	*Varies*	[-37]
Neighborhood characteristics						
Social cohesion	0.002 (0.13)	[5]	0.002 (0.29)	[1]	-0.004 (0.62)	[1]
Social cohesion-squared	0.002 (0.30)		-0.002 (0.53)		0.006 (1.34)	
Informal social control	0.003 (0.24)	[8]	-0.004 (0.65)	[-3]	-0.003 (0.47)	[-7]
Informal social control-squared	0.003 (0.37)		0.001 (0.23)		-0.001 (0.15)	
Intergenerational closure	-0.004 (0.31)	[6]	-0.015 (2.28) *	[23]	-0.006 (0.85)	[19]
Intergenerational closure-squared	0.002 (0.28)		0.000 (0.01)		0.001 (0.24)	
Racial diversity	-0.044 (2.48) **	[-53]	0.018 (1.92) †	[8]	0.015 (1.53)	[27]
Racial diversity-squared	0.007 (0.50)		-0.001 (0.09)		0.005 (0.69)	
Hispanic ethnic diversity	0.127 (3.89) ***	[27]	-0.039 (1.91) †	[1]	-0.004 (0.20)	[-1]
Hispanic ethnic diversity-squared	-0.058 (4.09) ***		0.009 (1.81) †		0.001 (0.23)	
Residential instability	0.007 (0.57)	[-19]	-0.001 (0.20)	[15]	0.005 (0.86)	[-2]
Residential instability-squared	0.009 (1.35)		-0.005 (1.77) †		-0.002 (0.40)	
Peer group characteristics						
Truancy	0.020 (1.23)	[1]	0.035 (4.67) ***	[-6]	0.013 (1.55)	[-10]
Truancy-squared	-0.004 (0.74)		-0.006 (2.98) **		0.001 (0.19)	
Extracurricular disengagement	0.030 (2.11) *	[-1]	0.016 (1.95) †	[-2]	0.006 (0.71)	[-5]
Extracurricular-squared	0.005 (0.63)		0.003 (0.72)		0.002 (0.64)	
Emotional school disengagement	0.011 (1.19)	[-3]	0.013 (2.75) **	[2]	0.003 (0.58)	[-1]
Emotional-squared	-0.001 (0.11)		0.000 (0.12)		0.002 (0.89)	
Cognitive school disengagement	0.007 (0.74)	[-1]	0.002 (0.43)	[-1]	0.011 (2.13) *	[3]
Cognitive-squared	0.003 (0.56)		0.004 (1.73) †		-0.001 (0.22)	
Friends not in youth school	0.048 (3.63) ***	[12]	0.033 (5.44) ***	[0]	0.012 (1.82) †	[-5]
Friends not in youth school-squared	0.001 (0.12)		0.001 (0.23)		-0.001 (0.16)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "No High School Diploma" model includes the sample of 71,520 observations across 20 imputed data sets (3,576 unique youth from 850 neighborhoods and 79 high schools per data set) whose parents' did not earn a high school diploma (includes GED recipients). The "High School Diploma" model includes the sample of 180,840 observations across 20 imputed data sets (9,042 unique youth from 1,496 neighborhoods and 80 high schools per data set) whose parents earned a high school diploma but not a college degree (includes parents who attended some college or went to vocational school). The "College degree or higher" model includes the sample of 80,840 observations across 20 imputed data sets (4,042 unique youth from 1,024 neighborhoods and 80 high schools per data set) whose parents earned a college degree or higher. All results are obtained from high school fixed-effects linear probability models as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001***.

Table 10. Linear probability model decompositions of neighborhood disadvantage effect on high school dropout stratified by youths' race/ethnicity

	White, non-Hispanic		Black, non-Hispanic		Hispanic, any race	
Model A:						
Neighborhood disadvantage	0.008 (1.81) †		0.007 (1.08)		0.015 (1.82) †	
Model B:						
Neighborhood disadvantage	0.010 (1.31)		0.006 (0.58)		0.014 (0.96)	
School fixed-effects	*Varies*	[-8]	*Varies*	[58]	*Varies*	[-65]
Neighborhood characteristics						
Social cohesion	0.000 (0.01)	[3]	0.017 (1.51)	[-1]	0.000 (0.04)	[0]
Social cohesion-squared	0.002 (0.53)		0.008 (1.37)		0.000 (0.03)	
Informal social control	0.000 (0.02)	[-1]	-0.009 (0.73)	[-21]	0.001 (0.07)	[8]
Informal social control-squared	0.000 (0.11)		-0.002 (0.39)		0.006 (0.89)	
Intergenerational closure	-0.016 (2.95) **	[30]	0.000 (0.01)	[12]	0.002 (0.14)	[-7]
Intergenerational closure-squared	0.006 (2.26) *		-0.004 (0.79)		0.002 (0.25)	
Racial diversity	0.009 (0.81)	[-1]	-0.009 (0.89)	[53]	0.022 (0.98)	[21]
Racial diversity-squared	-0.004 (0.80)		0.006 (0.76)		-0.013 (1.01)	
Hispanic ethnic diversity	-0.030 (2.06) *	[-14]	0.010 (0.42)	[-10]	0.035 (1.42)	[30]
Hispanic ethnic diversity-squared	0.002 (1.28)		-0.002 (0.37)		-0.049 (2.90) *	
Residential instability	0.006 (1.00)	[0]	0.012 (1.37)	[-49]	-0.007 (0.69)	[-5]
Residential instability-squared	-0.005 (1.39)		0.004 (0.63)		0.009 (1.22)	
Peer group characteristics						
Truancy	0.032 (4.77) ***	[-17]	0.015 (1.33)	[-5]	0.025 (1.45)	[-2]
Truancy-squared	-0.004 (2.07) *		-0.002 (0.61)		-0.006 (0.98)	
Extracurricular disengagement	0.025 (2.95) **	[-12]	0.003 (0.21)	[-9]	0.014 (0.93)	[5]
Extracurricular-squared	0.001 (0.35)		0.005 (0.75)		0.010 (1.10)	
Emotional school disengagement	0.010 (2.11) *	[2]	0.005 (0.55)	[-3]	0.015 (1.53)	[2]
Emotional-squared	0.000 (0.15)		0.003 (0.85)		-0.001 (0.17)	
Cognitive school disengagement	0.006 (1.59)	[-1]	0.001 (0.15)	[1]	0.004 (0.40)	[2]
Cognitive-squared	0.001 (0.33)		0.003 (0.50)		0.006 (1.16)	
Friends not in youth school	0.032 (4.84) ***	[-3]	0.019 (1.88) †	[-3]	0.039 (3.50) ***	[4]
Friends not in youth school-squared	-0.001 (0.17)		0.010 (1.44)		0.000 (0.01)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "White, non-Hispanic" model includes the sample of 180,200 observations across 20 imputed data sets (9,010 unique youth from 1,181 neighborhoods and 79 high schools per data set) who reported their race as "White" and did not report Hispanic ethnicity. The "Black, non-Hispanic" model includes the sample of 73,300 observations across 20 imputed data sets (3,665 unique youth from 871 neighborhoods and 68 high schools per data set) who reported their race as "Black" or "African American" and did not report Hispanic ethnicity. The "Hispanic, any race" model includes the sample of 55,180 observations across 20 imputed data sets (2,759 unique youth from 706 neighborhoods and 75 high schools per data set) who reported Hispanic ethnicity. All results are obtained from high school fixed-effects linear probability models as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001***.

Table 11. Linear probability model decompositions of neighborhood disadvantage effect on high school dropout stratified by youths' school and grade at baseline

	Middle School (Grades 7-8)		High School (Grades 9-12)	
Model A:				
Neighborhood disadvantage	0.017 (2.54) *		0.008 (2.32) *	
Model B: School fixed-effects				
Neighborhood disadvantage	0.009 (0.91)		0.010 (1.99) *	
School fixed-effects	*Varies*	[-7]	*Varies*	[0]
Neighborhood characteristics				
Social cohesion	0.007 (0.73)	[9]	-0.001 (0.24)	[0]
Social cohesion-squared	0.002 (0.32)		0.001 (0.50)	
Informal social control	0.011 (1.11)	[14]	-0.002 (0.40)	[-2]
Informal social control-squared	0.001 (0.15)		0.001 (0.46)	
Intergenerational closure	-0.022 (2.17) *	[28]	-0.005 (0.88)	[8]
Intergenerational closure-squared	0.007 (1.19)		-0.002 (0.59)	
Racial diversity	0.009 (0.69)	[-4]	-0.001 (0.08)	[3]
Racial diversity-squared	-0.010 (0.93)		0.003 (0.51)	
Hispanic ethnic diversity	0.054 (1.92)	[1]	-0.009 (0.54)	[-13]
Hispanic ethnic diversity-squared	-0.010 (1.96) *		0.001 (0.24)	
Residential instability	-0.003 (0.32)	[-3]	0.002 (0.41)	[-6]
Residential instability-squared	0.004 (0.74)		0.001 (0.37)	
Peer group characteristics				
Truancy	0.032 (3.04) **	[5]	0.030 (5.12) ***	[-15]
Truancy-squared	-0.006 (3.12) **		-0.005 (2.33) *	
Extracurricular disengagement	0.018 (1.43)	[0]	0.017 (2.70) **	[-8]
Extracurricular-squared	0.001 (0.31)		0.004 (1.48)	
Emotional school disengagement	0.012 (1.65) †	[3]	0.010 (2.39) *	[-1]
Emotional-squared	-0.001 (0.14)		0.001 (0.53)	
Cognitive school disengagement	0.012 (1.59)	[0]	0.002 (0.52)	[-1]
Cognitive-squared	0.004 (0.87)		0.004 (2.00) *	
Friends not in youth school	0.029 (2.65) **	[1]	0.034 (7.40) ***	[-1]
Friends not in youth school-squared	-0.002 (0.42)		0.001 (0.51)	
Model C: School random-effects				
Neighborhood disadvantage	0.009 (0.98)		0.009 (1.95) †	
School resources				
Pupil-per-teacher ratio	0.000 (0.03)	[5]	0.003 (0.52)	[-7]
Pupil-per-teacher ratio-squared	-0.009 (1.60)		0.003 (0.68)	
Teachers Masters degrees	0.003 (0.26)	[0]	0.006 (1.21)	[-3]
Teachers masters degrees-squared	-0.002 (0.20)		-0.002 (0.39)	
Teachers >5 years at school	-0.022 (2.30) *	[-19]	-0.009 (1.49)	[2]
Teachers >5 years at school-squared	-0.026 (3.19) **		-0.002 (0.64)	
Teachers <1 year at school	0.012 (0.84)	[-10]	0.002 (0.21)	[13]
Teachers <1 year at school-squared	-0.009 (1.64)		-0.001 (0.57)	
School-based services	-0.009 (0.92)	[3]	0.000 (0.05)	[0]
School-based services-squared	0.007 (1.33)		0.001 (0.29)	

School disciplinary policy strictness						
Violent/aggressive misconduct	0.031 (2.08)	*	[14]	-0.002 (0.44)		[-12]
Violent/aggressive-squared	0.012 (2.25)	*		0.005 (2.04)	*	
First time drug/alcohol misconduct	-0.005 (0.58)		[-1]	0.001 (0.27)		[-28]
First time drug/alcohol-squared	0.001 (0.08)			-0.005 (1.89)	†	
Repeated major misconduct	-0.011 (0.54)		[-2]	0.007 (0.72)		[2]
Repeated major-squared	0.003 (0.33)			-0.001 (0.22)		
Minor misconduct	-0.011 (1.16)		[0]	-0.011 (2.59)	**	[-26]
Minor-squared	-0.010 (1.36)			-0.001 (0.48)		
School structure and composition						
School size	-0.018 (1.55)		[7]	-0.007 (1.07)		[10]
School-size squared	0.002 (0.47)			0.001 (0.24)		
Includes middle/high school	-0.024 (0.83)		[-1]	-0.034 (2.44)	*	[-17]
Private, Catholic	-0.056 (1.11)		[5]	-0.063 (2.64)		[11]
Private, non-Catholic	0.000 (0.01)		[0]	-0.023 (0.68)		[0]
Suburban school	0.037 (1.58)		[-15]	-0.005 (0.43)		[6]
Rural school	0.048 (1.45)		[21]	0.012 (0.81)		[8]
Socio-economic disadvantage	0.006 (0.43)		[15]	0.015 (2.03)	*	[83]
Socio-econ. disadvantage-squared	0.006 (1.17)			0.000 (0.06)		
Percent Black, non-Hispanic	0.002 (0.19)		[-2]	0.003 (0.51)		[6]
Percent Hispanic, any race	-0.013 (0.93)		[-4]	-0.018 (1.63)		[-18]
Percent Other race/ethnicity	-0.016 (1.40)		[10]	0.003 (0.55)		[-7]
Neighborhood characteristics						
Social cohesion	-0.001 (0.07)		[-1]	-0.003 (0.61)		[-5]
Social cohesion-squared	-0.002 (0.45)			0.000 (0.18)		
Informal social control	0.001 (0.18)		[0]	-0.004 (0.94)		[-11]
Informal social control-squared	-0.002 (0.45)			-0.002 (0.92)		
Intergenerational closure	-0.021 (2.46)	*	[26]	-0.004 (0.88)		[16]
Intergenerational closure-squared	0.008 (1.49)			-0.001 (0.41)		
Racial diversity	-0.001 (0.09)		[-3]	0.005 (0.67)		[1]
Racial diversity-squared	-0.003 (0.31)			-0.002 (0.41)		
Percent Hispanic	0.032 (1.49)		[-4]	-0.015 (1.02)		[-9]
Percent Hispanic-squared	-0.007 (1.90)			0.004 (0.87)		
Residential instability	0.006 (0.70)		[-7]	0.004 (0.94)		[0]
Residential instability-squared	0.000 (0.03)			-0.002 (0.91)		
Peer group characteristics						
Truancy	0.031 (2.96)	**	[4]	0.031 (5.36)	***	[-16]
Truancy-squared	-0.006 (3.00)	***		-0.005 (2.31)	*	
Extracurricular disengagement	0.019 (1.58)		[0]	0.019 (3.03)	**	[-9]
Extracurricular-squared	0.001 (0.34)			0.004 (1.39)		
Emotional school disengagement	0.011 (1.46)		[3]	0.009 (2.16)	*	[-2]
Emotional-squared	0.000 (0.08)			0.001 (0.50)		
Cognitive school disengagement	0.011 (1.60)		[0]	0.002 (0.47)		[-2]
Cognitive-squared	0.003 (0.74)			0.004 (2.10)	*	
Friends not in youth school	0.031 (2.91)	**	[0]	0.033 (7.38)	***	[-2]
Friends not in youth school-squared	-0.003 (0.63)			0.002 (0.55)		

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are

calculated using the KHB method (refer to Equations 5-7). The "Middle School Grades 7-8" model includes the sample of 91,180 observations across 20 imputed data sets (4,559 unique youth from 955 neighborhoods and 73 middle schools per data set) who were in grades 7-8 at baseline and attending a middle school. The "High School Grades 9-12" model includes the sample of 244,260 observations across 20 imputed data sets (12,213 unique youth from 1,846 neighborhoods and 80 high schools per data set) who were in grades 9-12 at baseline and attending a high school. School characteristics correspond to the characteristics of the middle and high school that middle and high school students were attending at baseline, respectively. Model B presents results obtained from the high school fixed-effects linear probability model as expressed in Equation 8. Model C presents results obtained from the multilevel linear probability model with random intercepts for schools and neighborhoods. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Appendix A. Description of Wave I In-home sample by inclusion and exclusion criteria

	Sample		Socio-demographic characteristics			Youth age (years)	
	N	%	Income (1k)	College (%)	White (%)	Baseline	Follow-up
Wave I In-home sample (N=20,745)							
Included in sample	16,919	81.6	45.9	24	54	16.1	28.2
Excluded from sample (*Ref=Included*)	3,826	18.4	42.8 **	21 ***	46 ***	16.4 ***	-
Included in sample (N=, 16,919)							
School dropout measured at Wave IV	14,885	88.0	46.2	24	55	16.1	29.0
School dropout measured at Wave III (*Ref=IV*)	2,034	12.0	43.8 †	23	44 ***	16.3 ***	22.7 ***
Excluded from sample (N=3,826; *Ref=Included*)							
Baseline criteria not met							
No valid Wave I Census tract data	198	5.2	45.6	29	60	16.3 †	-
Not enrolled in school at baseline	404	10.6	36.2 **	14 ***	49 *	17.5 ***	-
Enrolled in non-sample school at baseline	441	11.5	42.2	22	42 ***	15.5 ***	-
Follow-up criteria not met							
Not in Wave III or IV	2,738	71.6	43.7 †	20 ***	45 ***	16.4 ***	-
No valid response at Wave III or IV	33	0.9	37.6	18	46	16.1	-
Not in Wave IV; high school at Wave III	12	0.3	46.0	17	58	14	-

Notes: Statistically significant differences in group means are indicated by p<.05*, .01**, .001*** and calculated using independent sample t-tests. The reference group for the "Excluded from analytic sample" and all associated sub-groups is the "Included in analytic sample" group. The reference group for the "School dropout status measured at Wave III" is the "School dropout status measured at Wave IV" group. "Income" refers to parent reported household income on the Wave I In-home survey coded in thousands. "College" refers to the proportion of youths' parents who earned a college degree or higher as reported by parents on the Wave I In-home survey. "White" refers to the proportion of youth who reported their race/ethnicity as White, non-Hispanic on the Wave I In-home survey. All estimates are based on multiply imputed data on 414,900 observations across 20 data sets.

Appendix B. Measurement description of analytic variables

Variable	Source, indicator variable(s), and coding	Missing data description
School dropout *Individual-level* (Binary)	*Wave IV In-home youth survey* "What is your high school graduation status?" Coding School dropout = 1 if "Earned a high school equivalence degree (GED)," or "Earned a certificate of attendance or certificate of completion," or "Did not receive a high school diploma, equivalence degree (GED), or certificate of attendance or certificate of completion"; else, 0 if "Finished high school with diploma" *Wave III In-home youth report* [if Wave IV report not available] "What degrees or diplomas have you received? Coding School dropout = 1 if "High school diploma" not marked; else, 0 if "high school diploma" marked.	No missing data.
Neighborhood disadvantage *Neighborhood-level* (Continuous)	*1990 U.S. Census on Census tract characteristics* (1) Male unemployment rate; (2) Proportion of persons with income below poverty line; (3) Proportion of households headed by females with own children ages 18 years and younger; (4) Proportion persons ages 25 years and older with no high school diploma or equivalency; (5) Proportion of persons ages 25 years and older with a college degree (reverse coded); (6) Proportion of employed persons in a managerial or professional occupation (reverse coded) Coding Index = average score of 6 standardized indicator variables	No missing data.

Youth race/ethnicity *Individual-level* (Categorical)	*Wave I In-home youth survey* "What is your race?" "Are you of Hispanic or Latino origin?" "Which one category best describes your racial background?" [if multiple race categories selected] Coding Youth race/ethnicity=1 if "White" and not of Hispanic or Latino, origin; else, 2 if "Black or African American" and not Hispanic or Latino; else, 3 if Hispanic or Latino (and any race); else, 4	77 youth have missing data (<1% of analytic sample) due to survey non-response. Missing data multiply imputed.
Youth male gender *Individual-level* (Binary)	*Wave I In-home youth survey* Confirmed by interviewer based on pre-loaded information. Coding Youth male gender=1 if "Male"; else 0	0 youth have missing data.
Youth low birth weight *Individual-level* (Binary)	*Wave I In-home parent survey* "What was (youth)'s birth weight? Coding Youth low birth weight=1 if less than 88 ounces, equivalent to approximately 5.5 pounds or 2,500 grams; else 0	3,147 youth have missing data (18% of analytic sample); 2,770 missing due to no parent survey participation; 377 missing due to survey non-response. Missing data multiply imputed.
Mother age at youth's birth *Individual-level* (Continuous)	*Wave I In-home youth and parent survey* Derived from date of birth of youth and biological mother (if parent survey respondent) Coding Mother age = date of birth of mother – date of birth of youth	4,577 mothers have missing data (27% of analytic sample); 2,770 missing due to no parent survey participation; 1,807 missing because parent survey respondent was not youth's biological mother or both youth's and mother's date of birth was unavailable.

Parent immigrant *Individual-level* (Binary)	*Wave I In-home parent survey* "Where you born in the U.S.?" Coding Parent immigrant=1 if not born in U.S; else, 0 *Wave I In-home youth survey* [if parent report not available] "Was (parent) born in the U.S.?" Coding Parent immigrant=1 if not born in U.S; else, 0	147 parents have missing data (<1% of analytic sample) due to survey non-response. Missing data multiply imputed.
Parent highest education *Individual-level* (Categorical)	*Wave I In-home parent survey* "How far did you go in school?" Coding Parent education=1 if "8th grade or less," or "more than 8th grade, but did not graduate from high school," or "went to a business, trade, or vocational school instead of high school," or "never went to school," or "completed a GED"; else, 2 if "high school graduate"; else, 3 if "went to a business, trade or vocational school after high school," or "went to college, but did not graduate"; else 4 if "graduated from a college or university" or "professional training beyond a 4-year college or university" *Wave I In-home youth survey* [if parent report not available] "How far in school did (parent) go?" Coding Same as above.	259 parents have missing data (2% of analytic sample) due to survey non-response. Missing data multiply imputed.

Household employment *Individual-level* (Binary)	*Wave I In-home parent survey* "Do you work outside the home?" "Does (spouse/cohabiting partner) work outside the home? Coding Household employment=1 if parent or spouse/cohabiting partner works outside the home; else, 0 *Wave I In-home youth survey* [if parent report not available] "Does (mother) work for pay?" "Does (father) work for pay?" Coding Household employment=1 if either parent who lives with youth works for pay; else, 0	141 households have missing data (<1% of analytic sample) due to survey non-response. Missing data multiply imputed.
Household family structure *Individual-level* (Categorical)	*Wave I In-home parent survey* "What is your current marital status?" "Is this (marriage-like cohabiting) relationship still going on?" Coding Household family structure=1 if parent is biological parent and married to youth's other biological parent; else, 2 if parent is biological mother and not in a marriage-like cohabiting relationship; else, 3	2,731 households have missing data (16% of analytic sample) due to no parent survey participation. Note: Households family structure could be determined from pre-load variables for some youth whose parent did not participate in the survey. Missing data multiply imputed. Youths' reports of whether both biological parents lived in the household were included in imputation models, although not used as substitute report because youth did not report on parents' marital status.
Household size *Individual-level* (Continuous)	*Wave I In-home youth survey* Coding Household size = number of persons reported on household roster	No missing data.

Household welfare receipt *Individual-level* (Binary)	*Wave I In-home parent survey* "Are you receiving public assistance, such as welfare?" "Is (spouse/cohabiting partner) receiving public assistance, such as welfare?" Coding Household welfare receipt=1 if parent or spouse/cohabiting partner is receiving public assistance; else, 0 *Wave I In-home youth survey* [if parent report not available] "Does (mother) receive public assistance, such as welfare?" "Does (father) receive public assistance, such as welfare? Coding Household welfare receipt=1 if either parent who lives with youth receives public assistance; else, 0	39 households have missing data (<1% of analytic sample) due to survey non-response. Missing data multiply imputed.
Household moved residence past year *Individual-level* (Binary)	*Wave I In-home youth survey* "How old were you when you moved here to your current residence?" Coding Household moved residence past year=1 if (current age – age of move) < 1; else, 0	53 households have missing data (<1% of analytic sample) due to survey non-response or missing information on youth's age. Missing data multiply imputed.
Household income (log) *Individual-level* (Continuous)	*Wave I In-home parent survey* "About how much total income, before taxes did your family receive in 1994? Include your own income, the income of everyone else in your household, and income from welfare benefits, dividends, and all other sources. Coding Household income (log) = ln((Household income/1000)+1)	4,170 households have missing data (25% of analytic sample); 2,770 missing due to no parent survey participation; 1,400 missing due to survey non-response. Missing data multiply imputed.
Pupil-to-teacher ratio *School-level* (Continuous)	*1994 Common Core Data and Private School Survey* Total number of enrolled students dived by total number of employed teachers. Coding Ratio = as entered	2 schools have missing data (2% of analytic sample). Values linearly imputed based on school administrators' report of the number of full time teachers divided by the number of students on school roster.

Teachers with Master's degree *School-level* (Continuous)	*Wave I School Administrator survey* "Approximately what percentage of your full-time classroom teachers hold Master's degrees or higher?" Coding Percent = as entered	1 of 80 schools have missing data (1% of analytic sample). Values linearly imputed based on observable school characteristics.
Teachers >5 years at school *School-level* (Continuous)	*Wave I School Administrator survey* "Approximately what percentage of your full-time classroom teachers have worked at your school for five years or more?" Coding Percent = as entered	1 of 80 schools have missing data (1% of analytic sample). Values linearly imputed based on observable school characteristics.
Teachers <1 years at school *School-level* (Continuous)	*Wave I School Administrator survey* "Approximately what percentage of your full-time classroom teachers are new? (i.e., began teaching at this school during the present school year)" Coding Percent = as entered	1 of 80 schools have missing data (1% of analytic sample). Values linearly imputed based on observable school characteristics.
School-based health and social services *School-level* (Continuous)	*Wave I School Administrator survey* "For each of the following health-related services, please indicate whether it is provided at your school…" (1) athletic physical; (2) non-athletic physical; (3) treatment for minor illness and injuries; (4) diagnostic screenings (e.g., sickle cell anemia, sexually transmitted diseases); (5) treatment for sexually transmitted diseases; (6) immunizations; (7) family planning counseling; (8) family planning services; (9) prenatal/postpartum health care; (10) drug awareness and resistance education program; (11) drug abuse program; (12) alcohol abuse program; (13) nutrition/weight loss program; (14) emotional counseling; (15) rape counseling program; (16) physical violence program (e.g., family violence, partner violence); (17) day care for children of currently enrolled students; and (18) physical fitness/recreation center. Coding Total number = number of health-related services indicated	1 of 80 schools have missing data (1% of analytic sample). Values linearly imputed based on observable school characteristics.

Violent/aggressive misconduct disciplinary policy strictness *School-level* (Continuous)	*Wave I School Administrator survey* "In your school, what happens to a student who is caught…?" (1) Fighting with another student, first occurrence; (2) fight with another student, second occurrence; (3) Injuring another student, first occurrence; (4) Injuring another student, second occurrence; (5) Verbally abusing a teacher, first occurrence; (6) Verbally abusing a teacher, second occurrence Coding Response options: 0="Verbal warning"; 1="Minor action"; 2="In-school suspension"; 4="Out-of-school suspension"; 5="Expulsion" Index = average score of 6 standardized indicator variables	1 of 80 high schools (1% of analytic sample) has missing data. Values linearly imputed based on observable school characteristics. School administrator "No policy" responses to specific forms of misconduct were coded to missing and linearly imputed based on all other school disciplinary policies.
First time drug/alcohol misconduct disciplinary policy strictness *School-level* (Continuous)	*Wave I School Administrator survey* "In your school, what happens to a student who is caught…?" (1) Possessing alcohol, first occurrence; (2) Possessing an illegal drug, first occurrence; (3) Drinking alcohol at school, first occurrence; (4) Using an illegal drug at school, first occurrence Coding Response options: 0="Verbal warning"; 1="Minor action"; 2="In-school suspension"; 4="Out-of-school suspension"; 5="Expulsion" Index = average score of 6 standardized indicator variables	1 of 80 high schools (1% of analytic sample) has missing data. Values linearly imputed based on observable school characteristics. School administrator "No policy" responses to specific forms of misconduct were coded to missing and linearly imputed based on all other school disciplinary policies.
Repeated major misconduct disciplinary policy strictness *School-level* (Continuous)	*Wave I School Administrator survey* "In your school, what happens to a student who is caught…?" (1) Possessing a weapon, second occurrence; 92) Physically injuring a teacher, second occurrence; 93) Possessing an illegal drug, second occurrence; 94) Using an illegal drug at school, second occurrence. Coding Response options: 0="Verbal warning"; 1="Minor action"; 2="In-school suspension"; 4="Out-of-school suspension"; 5="Expulsion" Index = average score of 6 standardized indicator variables	1 of 80 high schools (1% of analytic sample) has missing data. Values linearly imputed based on observable school characteristics. School administrator "No policy" responses to specific forms of misconduct were coded to missing and linearly imputed based on all other school disciplinary policies.

Minor misconduct disciplinary policy strictness *School-level* (Continuous)	*Wave I School Administrator survey* "In your school, what happens to a student who is caught…?" (1) Cheating, first occurrence; (2) Cheating, second occurrence; (3) Smoking, first occurrence; (4) Smoking, second occurrence; (5) Stealing, first occurrence; (6) Stealing, second occurrence Coding Response options: 0="Verbal warning"; 1="Minor action"; 2="In-school suspension"; 4="Out-of-school suspension"; 5="Expulsion" Index = average score of 6 standardized indicator variables	1 of 80 high schools (1% of analytic sample) has missing data. Values linearly imputed based on observable school characteristics. School administrator "No policy" responses to specific forms of misconduct were coded to missing and linearly imputed based on all other school disciplinary policies.
School size *School-level* (Continuous)	*School roster* Number of students on school roster at time of data collection Coding Size = as entered	No missing data.
School includes middle school *School-level* (Binary)	*School roster* Grades of students on school roster at time of data collection Coding School includes middle school=1 if school includes Grades 7 or 8; else, 0	No missing data.
School organization *School-level* (Categorical)	*National Center for Education Statistics (NCES)* Coding School organization=1 if "Public"; else, 2 if "Private, Catholic"; else, 3 if "Private, non-Catholic"	No missing data.
School urbanicity *School-level* (Categorical)	*National Center for Education Statistics (NCES)* Coding School organization=1 if "Rural"; else, 2 if "Suburban"; else, 3 if "Urban"	No missing data.

School socio-economic disadvantage *School-level* (Continuous)	*Wave I In-home part and youth report* Survey indicators include: (1) average adjusted household income; the proportion of students from households (2) with no employed parents; (3) headed by a single female parent; (4) headed by parents with no high school diploma; (5) headed by parents with a college degree (reverse coded); and (6) headed by parents employed in a professional or managerial occupation (reverse coded). Coding Index = average score of 6 standardized indicator variables	No missing data.
School racial/ethnic composition *School-level* (Continuous / multiple variables)	*Wave I In-school survey* Survey indicators include proportion of students who identify as: (1) White, non-Hispanic; (2) Black, non-Hispanic; (3) Hispanic, any race; (4) Other race, non-Hispanic Coding Composition = number of youth in school who identify as racial/ethnic category divided by total youth who responded to survey in school	No missing data.
Neighborhood social cohesion *Neighborhood-level* (Continuous)	*Wave I In-home youth survey* "Indicate whether each of the following statements is true for you." (1) You know most of the people in your neighborhood (2) In the past month, you have stopped on the street to talk with someone who lives in your neighborhood: (3) People in this neighborhood look out for each other Coding Response options: 0=False; 1=True Index = sum of constant and neighborhood-specific random effect (empirical Bayes estimate) from hierarchically nested multilevel logit model predicting affirmative item response. Refer to Appendix D for model estimates.	5 neighborhoods have missing data (<1% of analytic sample) due to survey non-response. Missing data multiply imputed.

Neighborhood informal social control *Neighborhood-level* (Continuous)	*Wave I In-home parent survey* 149 "If you saw a neighbor's child getting into trouble, would you tell your neighbor about it?" (2) "If a neighbor saw your child getting into trouble, would your neighbor tell you about it?" Coding Response options: 0="Definitely would not"; 1= "Probably would not"; 2="Might"; 3="Probably would"; 4="Definitely would". Index = sum of constant and neighborhood-specific random effect (empirical Bayes estimate) from hierarchically nested multilevel regression model predicting item response. Refer to Appendix D for model estimates.	149 neighborhoods have missing data (1% of analytic sample) due to no parent survey participation. Missing data multiply imputed.
Neighborhood intergenerational closure *Neighborhood-level* (Continuous)	*Wave I In-home parent survey* "Please think about all of (youth)'s friends. How many parents of (youth)'s friends have you talked to in the last four weeks" Coding Response options: 0-6 (censored at 6 or more) Index = sum of constant and neighborhood-specific random effect (empirical Bayes estimate) from hierarchically nested multilevel regression model predicting item response. Refer to Appendix D for model estimates.	149 neighborhoods have missing data (1% of analytic sample) due to no parent survey participation. Missing data multiply imputed.
Neighborhood racial diversity *Neighborhood-level* (Continuous)	*1990 U.S. Census on Census tract characteristics* (1) Proportion of persons white; (2) proportion of persons black; (3) proportion of persons Asian; (4) proportion of persons other race Coding Index = $w(1-w)+b(1-b)+a(1-a)+o(1-o)$ where *w*=proportion of persons white, *b*=proportion of persons black, *a*=proportion of persons Asian, *o*=proportion of persons other race.	No missing data.
Neighborhood Hispanic ethnic diversity *Neighborhood-level* (Continuous)	*1990 U.S. Census on Census tract characteristic* Proportion of persons Hispanic ethnicity Coding Index = proportion of persons Hispanic ethnicity	No missing data.

Neighborhood residential instability *Neighborhood-level* (Continuous)	*1990 U.S. Census on Census tract characteristic* Proportion of housing units that have housed the same household for the past five years (since 1994) Coding Index = 1 – proportion of housing units that have housed the same household for the past five years (since 1994)	No missing data.
Peer group school truancy *Individual-level* (Continuous)	*Wave I In-school survey* "During the past twelve months, how often did you skip school without an excuse?" Coding Response options: 0=Never; 1=Once or twice; 2=Once a month or less; 3=2 or 3 days a month; 4=Once or twice a week: 5=3 to 5 days a week; 6=Nearly every day Index = average of youths' friends' scores	3,541 youth have missing data (20% of analytic sample) due to no friendship nominations or no survey data available on nominated friends.
Peer group extracurricular disengagement *Individual-level* (Continuous)	*Wave I In-school survey* "Here is a list of clubs, organizations, and teams found at many schools. Darken the oval next to any of them that you are participating in this year or that you plan to participate in later in the school year." (1) French club; (2) German club; (3) Latin club; (4) Spanish club; (5) Book club; (6) Computer club; (7) Debate team; (8); Drama club; (9) Future Farmers of America; (10) History club; (11) Math club; (12) Science club; (13) Band; (14) Science club; (15) Cheerleading/dance team; (16) Chorus or choir; (17) Orchestra; (18) Orchestra; (19) Other club or organization; (20) Baseball/softball; (21) Basketball; (22) Field hokey; (23) Football; (24) Ice hokey: (25) Soccer; (26) Swimming; (27) Tennis; (28) Track; (29) Volleyball; (30) Wrestling; (31) Other sport; (32) Newspaper; (33) Honor society; (34) Student council; (35) Yearbook Coding Index = proportion of youths' nominated friends who did not indicate participation in any extracurricular activities	3,527 youth have missing data (20% of analytic sample) due to no friendship nominations or no survey data available on nominated friends.

Peer group emotional school disengagement *Individual-level* (Continuous)	*Wave I In-school survey* "How strongly do you agree or disagree with each of the following statements?" (1) I feel close to people at this school; (2) I feel like I am a part of this school; (3) I am happy to be at this school Coding Response options: 0=Strongly disagree; 1=Disagree; 2=Neither agree nor disagree; 3=Agree; 4=Strongly agree Friends' scores = average of 3 indicator variables Index = average of youths' friends' scores	3,447 youth have missing data (21% of analytic sample) due to no friendship nominations or no survey data available on nominated friends
Peer group cognitive school disengagement *Individual-level* (Continuous)	*Wave I In-school survey* "In general, how hard do you try to do your school work well? Coding Response options: 0=I try very hard to do my best; 1=I try hard enough, but not as hard as I could; 2=I don't try very hard; 3=I never try at all Index = average of youths' friends' scores	3,319 youth have missing data (20% of analytic sample) due to no friendship nominations or no survey data available on nominated friends
Peer group friends not in youth school *Individual-level* (Continuous)	*Wave I In-school and In-home youth survey* Coding Index = proportion of youths' nominated friends coded by Add Health "does not attend respondent's school or sister school".	383 youth have missing data (2% of analytic sample) due to no friendship nominations.

Appendix C. Rotated factor loadings on school disciplinary policy variables

	Factor 1	Factor 2	Factor 3
Violent/aggressive misconduct			
Fighting with another student, first occurrence	0.702		
Fighting with another student, second occurrence	0.592		
Injuring another student, first occurrence	0.712		
Injuring another student, second occurrence	0.718		
Verbally abusing a teacher, first occurrence	0.607		
Verbally abusing a teacher, second occurrence	0.617		
First time drug/alcohol misconduct			
Possessing alcohol, first occurrence		0.762	
Possessing an illegal drug, occurrence first		0.819	
Drinking alcohol at school, first occurrence		0.742	
Using an illegal drug at school, first occurrence		0.740	
Repeated major misconduct			
Possessing a weapon, second occurrence			0.786
Physically injuring a teacher, second occurrence			0.630
Possessing an illegal drug, second occurrence			0.807
Using an illegal drug at school, second occurrence			0.742

Notes: Principal factors factor analysis extracted 5 factors with Eigenvalues greater than 1. Rotated factor loadings are obtained using orthogonal varimax rotation method. Factors are displayed in order in which they were extracted and rotated factor loadings <0.4 are not shown. Sample includes all school administrator reports on schools (high schools and middle/"feeder" schools) attended by youth in analytic sample at Wave I In-home survey (N=130). Missing values due to item-specific non-response or "no policy" report are linearly imputed based on responses to all other policy questions. The following survey items are excluded due to poor loadings on initial exploratory factor analyses: "Possessing alcohol (second time)"; "Possessing a weapon (first time)"; "Drinking alcohol at school (first time)"; "Physically injuring a teacher (first time)"; "Theft of school property (first time)"; and "Theft of school property (second time)".

Appendix D. Multilevel regression models for neighborhood social organization measures

	Logit model	*OLS model*	*OLS model*
	Model 1 Informal Social Control	Model 2 Social Cohesion	Model 3 Intergenerational closure
Youth gender			
(Female)			
Male	0.299 ***	0.056 ***	-0.069 *
	(8.68)	(4.76)	(2.51)
Youth grade at baseline			
(Grade 7)			
Grade 8	-0.057	-0.023	0.000
	(0.83)	(1.04)	(0.01)
Grade 9	-0.091	-0.062 **	-0.137 **
	(1.43)	(2.91)	(2.75)
Grade 10	-0.281 ***	-0.085 ***	-0.325 ***
	(4.40)	(4.03)	(6.61)
Grade 11	-0.561 ***	-0.115 ***	-0.403 ***
	(8.80)	(5.39)	(8.07)
Grade 12	-0.598 ***	-0.116 ***	-0.350 ***
	(9.23)	(5.24)	(6.74)
Social cohesion survey items			
(Know most people in neighborhood)			
Stopped on street to talk with someone	0.636 ***		
	(22.43)		
People look out for one another	0.058 *		
	(2.16)		
Informal social control survey items			
(Parent tells neighbor if child gets in trouble)			
Neighbor tells parent if child gets in trouble		-0.365 ***	
		(47.44)	
Intergenerational closure survey item			
(Parents of youth's friends talked to in past month)			
Variance components			
Neighborhood	0.58	0.21	0.63
Respondent	2.79	0.58	1.78
Item	0.72	0.72	
N			
Neighborhoods	2,432	2,247	2,246
Respondents	20,438	17,461	17,461
Observations	60,965	34,729	17,461

Notes: Logit coefficients and z-statistics of coefficients are shown for all variables in Model 2; regression coefficients and t-statistics of coefficients are shown for all variables in Model 2 and 3. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Appendix E. Decomposition of neighborhood disadvantage effect on school dropout via youth, parent, and household characteristics

	Effect [%]	β(YZ•XC)	Θ(ZX•C)
Total	0.435 [100]		
Direct, β(YX•Z)	0.162 [37]		
Indirect, β(YZ•X) × Θ(ZX)	0.274 [63]		
via Youth characteristics			
Male gender	-0.004 [-1]	0.453 (0.048) ***	-0.010 (0.004) *
Low birth weight	-0.002 [0]	-0.151 (0.087) †	0.011 (0.003) ***
Foreign-born	-0.003 [-1]	-0.202 (0.109) †	0.013 (0.002) ***
Black, non-Hispanic	-0.050 [-11]	-0.389 (0.083) ***	0.129 (0.003) ***
Hispanic, any race	0.008 [2]	0.179 (0.090) *	0.045 (0.003) ***
Other race, non-Hispanic	0.001 [0]	-0.045 (0.124)	-0.017 (0.002) ***
8th grade	0.001 [0]	0.149 (0.080)	0.004 (0.003)
9th grade	0.000 [0]	0.005 (0.078)	-0.002 (0.003)
10th grade	0.000 [0]	-0.354 (0.080) ***	0.000 (0.003)
11th grade	0.003 [1]	-0.735 (0.086) ***	-0.004 (0.003)
12th grade	0.003 [1]	-1.439 (0.105) ***	-0.002 (0.003)
Wave III report of school dropout	0.002 [0]	0.354 (0.068) ***	0.005 (0.003) †
via Parent characteristics			
Mother age at youth's birth	0.021 [5]	-0.024 (0.007) ***	-0.866 (0.048) ***
Foreign-born	-0.005 [-1]	-0.240 (0.094) *	0.020 (0.003) ***
Any parent currently employed	0.009 [2]	-0.166 (0.075) *	-0.056 (0.002) ***
High school diploma	-0.014 [-3]	-0.499 (0.065) ***	0.027 (0.003) ***
Some college/vocational school	0.017 [4]	-0.701 (0.068) ***	-0.025 (0.003) ***
College degree or higher	0.128 [29]	-1.226 (0.092) ***	-0.105 (0.003) ***
via Household characteristics			
Single biological mother	0.022 [5]	0.353 (0.073)	0.063 (0.063) ***
Other living arrangement	0.014 [3]	0.529 (0.063)	0.026 (0.026) ***
Size	0.001 [0]	0.010 (0.024)	0.065 (0.065) ***
Moved residence in past year	0.004 [1]	0.470 (0.071)	0.010 (0.002) ***
Welfare receipt	0.032 [7]	0.341 (0.060)	0.093 (0.003) ***
Income (log)	0.085 [19]	-0.208 (0.038)	-0.407 (0.009) ***

Notes: Total, direct and indirect effects are reported in logit metric (percentage shown in brackets) obtained from the decomposition of the neighborhood disadvantage coefficient from the logistic regression of high school dropout with random effects for school and neighborhood intercepts; refer to Equation 2 for model specification. All coefficients and indirect effects are calculated using the KHB method (refer to Equations 5-7). The sample consists of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). All variables presented are standardized to mean 0 and standard deviation 1 except dichotomous and dummy variables. Statistically significant coefficients are indicated by p<.10†, .05*, .01**, .001***.

Appendix F. Linear probability model decomposition of neighborhood disadvantage effect on school dropout stratified by wave of school dropout report

	Wave IV Report		Wave III Report	
Model A:				
Neighborhood disadvantage	0.009 (2.57) **		0.009 (2.36) *	
Model B				
Neighborhood disadvantage	0.009 (1.81) †		0.008 (1.47)	
School fixed-effects	*Varies*	[-7]	*Varies*	[-24]
Neighborhood characteristics				
Social cohesion	0.000 (0.04)	[2]	0.005 (0.95)	[11]
Social cohesion-squared	0.002 (0.93)		0.003 (0.90)	
Informal social control	0.003 (0.65)	[7]	0.003 (0.62)	[6]
Informal social control-squared	0.001 (0.44)		0.001 (0.23)	
Intergenerational closure	-0.012 (2.42) *	[29]	-0.011 (2.08) *	[26]
Intergenerational closure-squared	0.002 (0.68)		0.002 (0.79)	
Racial diversity	0.003 (0.36)	[0]	0.003 (0.39)	[0]
Racial diversity-squared	-0.002 (0.35)		-0.002 (0.36)	
Hispanic ethnic diversity	0.009 (0.64)	[-14]	0.008 (0.44)	[3]
Hispanic ethnic diversity-squared	-0.004 (1.10)		-0.002 (0.38)	
Residential instability	0.002 (0.40)	[-1]	0.005 (1.05)	[-7]
Residential instability-squared	-0.001 (0.22)		-0.001 (0.32)	
Peer group characteristics				
Truancy	0.029 (4.69) ***	[-7]	0.031 (4.59) ***	[-7]
Truancy-squared	-0.005 (2.80) **		-0.005 (2.45) *	
Extracurricular disengagement	0.016 (2.56) *	[-4]	0.013 (1.81) †	[-4]
Extracurricular-squared	0.004 (1.44)		0.004 (1.38)	
Emotional school disengagement	0.009 (2.47) *	[0]	0.009 (2.10) *	[0]
Emotional-squared	0.000 (0.09)		0.001 (0.45)	
Cognitive school disengagement	0.006 (1.66) †	[-1]	0.006 (1.46)	[0]
Cognitive-squared	0.004 (1.70) †		0.001 (0.49)	
Friends not in youth school	0.034 (6.79) ***	[-2]	0.026 (4.58) ***	[4]
Friends not in youth school-squared	0.000 (0.02)		0.006 (1.70) †	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "Wave IV Report" model includes the sample of 297,700 observations across 20 imputed data sets (14,885 unique youth from 2,002 neighborhoods and 80 high schools per data set) who reported a school dropout status at Wave IV. The "Wave III Report" model includes the sample of 283,680 observations across 20 imputed data sets (14,184 unique youth from 1,934 neighborhoods and 80 high schools per data set) who reported a school dropout status at Wave III. All results are obtained from the high school fixed-effects linear probability model as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001***.

Appendix G. Weighted linear probability model decomposition of neighborhood disadvantage effect on school dropout stratified by wave of survey participation

	Wave IV Weighted Sample		Wave III Weighted Sample	
Model A:				
Neighborhood disadvantage	0.010 (2.06) *		0.011 (2.35) *	
Model B				
Neighborhood disadvantage	0.008 (1.04)		0.009 (1.17)	
School fixed-effects	*Varies*	[-25]	*Varies*	[-10]
Neighborhood characteristics				
Social cohesion	0.006 (0.89)	[9]	0.008 (1.22)	[11]
Social cohesion-squared	-0.001 (0.23)		-0.002 (0.46)	
Informal social control	-0.003 (0.50)	[-1]	-0.006 (0.90)	[12]
Informal social control-squared	0.003 (0.98)		-0.001 (0.40)	
Intergenerational closure	-0.024 (3.14) **	[61]	-0.020 (2.69) **	[48]
Intergenerational closure-squared	0.002 (0.66)		0.001 (0.21)	
Racial diversity	0.007 (0.50)	[2]	0.003 (0.24)	[5]
Racial diversity-squared	-0.004 (0.41)		0.002 (0.19)	
Hispanic ethnic diversity	-0.014 (0.43)	[-36]	-0.009 (0.30)	[-20]
Hispanic ethnic diversity-squared	-0.003 (0.34)		-0.001 (0.17)	
Residential instability	0.002 (0.35)	[9]	0.005 (0.69)	[-3]
Residential instability-squared	-0.005 (1.39)		-0.002 (0.61)	
Peer group characteristics				
Truancy	0.030 (3.28) **	[-2]	0.028 (3.03) ***	[-1]
Truancy-squared	-0.005 (1.95) †		-0.004 (1.60)	
Extracurricular disengagement	0.010 (1.08)	[-8]	0.006 (0.60)	[-8]
Extracurricular-squared	0.007 (1.74) †		0.008 (2.03) *	
Emotional school disengagement	0.009 (1.45)	[4]	0.011 (1.83) †	[3]
Emotional-squared	-0.003 (1.17)		-0.002 (0.58)	
Cognitive school disengagement	0.011 (1.87) †	[0]	0.010 (1.75) †	[0]
Cognitive-squared	0.003 (1.01)		0.001 (0.47)	
Friends not in youth school	0.030 (4.27) ***	[6]	0.026 (3.54) ***	[4]
Friends not in youth school-squared	0.004 (0.99)		0.004 (0.85)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "Wave IV Weighted Sample" model includes the sample of 287,140 observations across 20 imputed data sets (14,357 unique youth from 1,983 neighborhoods and 80 high schools per data set) who were assigned Wave IV cross-sectional grand sample weights. The "Wave III Weighted Sample" model includes the sample of 274,200 observations across 20 imputed data sets (13,710 unique youth from 1,918 neighborhoods and 80 high schools per data set) who were assigned Wave III cross-sectional grand sample weights. All results are obtained from the high school fixed-effects linear probability model as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001***.

Appendix H. Linear probability model decomposition of neighborhood disadvantage effect on school dropout controlling for youths' school disengagement

	Full sample Disengagement imputed		Restricted sample Disengaged not imputed	
Model A:				
Neighborhood disadvantage	0.014 (4.17) ***		0.015 (4.37) ***	
Model B				
Neighborhood disadvantage	0.011 (2.45) *		0.015 (3.17) **	
School fixed-effects	*Varies*	[8]	*Varies*	[-3]
Neighborhood characteristics				
Social cohesion	0.001 (0.30)	[2]	0.001 (0.29)	[3]
Social cohesion-squared	0.002 (0.68)		0.001 (0.36)	
Informal social control	0.000 (0.03)	[3]	0.000 (0.04)	[2]
Informal social control-squared	0.001 (0.66)		0.001 (0.47)	
Intergenerational closure	-0.008 (1.73) †	[14]	-0.010 (1.95) †	[14]
Intergenerational closure-squared	0.000 (0.21)		0.001 (0.38)	
Racial diversity	-0.002 (0.24)	[2]	-0.008 (1.04)	[-4]
Racial diversity-squared	0.004 (0.83)		0.000 (0.07)	
Hispanic ethnic diversity	0.002 (0.14)	[-8]	0.007 (0.46)	[-1]
Hispanic ethnic diversity-squared	-0.002 (0.51)		-0.002 (0.54)	
Residential instability	0.001 (0.28)	[-3]	-0.002 (0.40)	[-6]
Residential instability-squared	0.001 (0.41)		0.005 (1.64) †	
Peer group characteristics				
Truancy	0.020 (3.27) **	[-2]	0.024 (4.04) ***	[-2]
Truancy-squared	-0.005 (2.85) **		-0.005 (3.13) **	
Extracurricular disengagement	0.012 (1.90) †	[-1]	0.016 (2.48) *	[1]
Extracurricular-squared	0.003 (1.19)		-0.001 (0.20)	
Emotional school disengagement	0.007 (1.82) †	[0]	0.007 (1.73) †	[0]
Emotional-squared	0.000 (0.08)		0.000 (0.06)	
Cognitive school disengagement	0.003 (0.82)	[0]	0.000 (0.09)	[0]
Cognitive-squared	0.002 (1.02)		-0.001 (0.30)	
Friends not in youth school	0.021 (4.42) ***	[1]	0.017 (3.14) **	[0]
Friends not in youth school-squared	0.000 (0.09)		-0.002 (0.64)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "Full sample" Model includes the full analytic sample of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). The "Restricted sample" model includes the sample of 222,720 observations across 20 imputed data sets (11,136 unique youth from 1,720 neighborhoods and 80 high schools per data set) with valid school disengagement data from the In-school survey. All results are obtained from the high school fixed-effects linear probability model as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, sex, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001***.

Appendix I. Linear probability model decomposition of neighborhood disadvantage effect on high school dropout excluding peer group characteristics

	School random effects			School fixed-effects		
Neighborhood disadvantage	0.009 (2.12)	*		0.013 (2.75)	**	
School fixed-effects				*Varies*		[-36]
School resources						
Pupil-per-teacher ratio	-0.002 (0.22)		[1]			
Pupil-per-teacher ratio-squared	0.000 (0.07)					
Teachers Masters degrees	0.012 (1.94)	†	[-8]			
Teachers masters degrees-squared	0.000 (0.04)					
Teachers >5 years at school	-0.015 (1.98)	*	[6]			
Teachers >5 years at school-squared	-0.001 (0.30)					
Teachers <1 year at school	0.032 (2.38)	*	[4]			
Teachers <1 year at school-squared	-0.006 (2.18)	*				
School-based services	0.001 (0.14)		[0]			
School-based services-squared	0.002 (0.38)					
School disciplinary policy strictness						
Violent/aggressive misconduct	-0.013 (1.94)	†	[-11]			
Violent/aggressive-squared	0.003 (1.07)					
First time drug/alcohol misconduct	-0.002 (0.38)		[-14]			
First time drug/alcohol-squared	-0.006 (1.92)	†				
Repeated major misconduct	0.009 (0.80)		[2]			
Repeated major-squared	-0.001 (0.22)					
Minor misconduct	-0.011 (1.98)	*	[-8]			
Minor-squared	-0.002 (0.57)					
School structure and composition						
School size	-0.042 (2.59)	*	[-5]			
School-size squared	-0.007 (0.91)					
Includes middle/high school	-0.042 (2.59)		[-4]			
Private, Catholic	-0.074 (2.65)		[8]			
Private, non-Catholic	-0.052 (1.40)		[6]			
Suburban school	-0.008 (0.61)		[4]			
Rural school	0.003 (0.14)		[1]			
Socio-economic disadvantage	0.015 (1.87)	†	[55]			
Socio-econ. disadvantage-squared	0.003 (0.87)					
Percent Black, non-Hispanic	-0.002 (0.22)		[-5]			
Percent Hispanic, any race	-0.012 (1.06)		[-12]			
Percent Other race/ethnicity	0.002 (0.28)		[-1]			
Neighborhood characteristics						
Social cohesion	-0.002 (0.54)		[-2]	-0.001 (0.12)		[0]
Social cohesion-squared	0.000 (0.07)			0.001 (0.52)		
Informal social control	0.000 (0.11)		[-3]	0.001 (0.15)		[4]
Informal social control-squared	-0.001 (0.36)			0.001 (0.51)		
Intergenerational closure	-0.013 (2.88)	**	[33]	-0.011 (2.31)	*	[26]
Intergenerational closure-squared	0.001 (0.32)			0.000 (0.01)		
Racial diversity	0.006 (0.99)		[6]	0.001 (0.15)		[1]
Racial diversity-squared	-0.003 (0.63)			0.000 (0.02)		

Hispanic ethnic diversity	0.005 (0.33)	[-7]	0.011 (0.72)	[-6]
Hispanic ethnic diversity-squared	-0.003 (0.79)		-0.004 (1.02)	
Residential instability	0.004 (0.82)	[-2]	0.002 (0.54)	[-3]
Residential instability-squared	-0.001 (0.35)		0.000 (0.05)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). Both models include the full analytic sample of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). Results from the "School random effects" model are obtained from the multilevel linear probability model with random intercepts for schools and neighborhoods. Results from the "School fixed effects" model are obtained from the high school fixed-effects linear probability model as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001***.

Appendix J. Linear probability model decomposition of neighborhood disadvantage effect on school dropout excluding youth with missing peer group data

	Full sample Peer group imputed		Restricted sample Peer group not imputed	
Model A:				
Neighborhood disadvantage	0.014 (4.17) ***		0.010 (2.93) **	
Model B				
Neighborhood disadvantage	0.011 (2.39) *		0.011 (2.34) *	
School fixed-effects	*Varies*	[-6]	*Varies*	[-17]
Neighborhood characteristics				
Social cohesion	0.000 (0.01)	[2]	-0.001 (0.17)	[-1]
Social cohesion-squared	0.002 (0.71)		0.001 (0.40)	
Informal social control	0.002 (0.38)	[5]	0.002 (0.45)	[7]
Informal social control-squared	0.001 (0.66)		0.002 (0.86)	
Intergenerational closure	-0.010 (2.11) *	[22]	-0.012 (2.36) *	[24]
Intergenerational closure-squared	0.000 (0.16)		0.001 (0.51)	
Racial diversity	0.001 (0.15)	[3]	0.005 (0.65)	[5]
Racial diversity-squared	0.002 (0.45)		0.001 (0.26)	
Hispanic ethnic diversity	0.005 (0.34)	[-11]	0.014 (0.94)	[-12]
Hispanic ethnic diversity-squared	-0.003 (0.78)		-0.005 (1.32)	
Residential instability	0.002 (0.41)	[-4]	-0.003 (0.74)	[-3]
Residential instability-squared	0.001 (0.23)		0.003 (1.25)	
Peer group characteristics				
Truancy	0.031 (5.03) ***	[-7]	0.031 (5.87) ***	[-7]
Truancy-squared	-0.005 (2.98) **		-0.005 (3.52) *	
Extracurricular disengagement	0.017 (2.66) **	[-4]	0.020 (3.29) **	[-1]
Extracurricular-squared	0.003 (1.28)		0.002 (0.78)	
Emotional school disengagement	0.011 (3.02) **	[0]	0.010 (2.86) **	[-1]
Emotional-squared	0.001 (0.28)		0.001 (0.30)	
Cognitive school disengagement	0.005 (1.30)	[-1]	0.003 (0.82)	[-1]
Cognitive-squared	0.003 (1.66) †		0.003 (1.95) †	
Friends not in youth school	0.031 (6.52) ***	[-1]	0.018 (3.74) ***	[-3]
Friends not in youth school-squared	0.001 (0.24)		0.002 (0.79)	

Notes: Regression coefficients and t-statistics of coefficients are shown for neighborhood disadvantage and all contextual variables. The indirect effects of neighborhood disadvantage on school dropout via the contextual variables modeled in quadratic form are shown in brackets as percentages. Indirect effects are calculated using the KHB method (refer to Equations 5-7). The "Full sample" Model includes the full analytic sample of 338,380 observations across 20 imputed data sets (16,919 unique youth from 2,152 neighborhoods and 80 high schools per data set). The "Restricted sample" model includes the sample of 266,460 observations across 20 imputed data sets (13,323 unique youth from 1,795 neighborhoods and 79 high schools per data set) with valid peer group data. All results are obtained from the high school fixed-effects linear probability model as expressed in Equation 8. All variables are standardized to mean 0 and standard deviation 1. All models control for the following individual-level socio-demographic controls: youth race/ethnicity, gender, low birth weight, immigrant status; mother age at youth's birth; parental employment, education, and immigrant status; and household structure, size, residential instability, welfare receipt, and log of annual income. Standard errors are adjusted for clustering of youth within neighborhoods using Huber–White robust standard errors. Statistically significant coefficients are indicated p<.10†, .05*, .01**, .001*