

Final Report

**Magnitudes and Mechanisms of Neighborhood Impacts on
Children: Analyzing a Natural Experiment in Denver**

John D. and Catherine T. Mac Arthur Foundation

Grant #08-92652-000-HCD

Principal Investigators:

Dr. George C. Galster, Wayne State University, Detroit

Dr. Anna M. Santiago, Case Western Reserve University, Cleveland

Grant period: November 26, 2008-Dec. 31. 2011

January 31, 2012

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PART 1: Overview of Project

We aim to: (1) quantify precisely how various conditions in the surrounding neighborhood affect independently a variety of outcomes for low-income children who have resided in public housing for a substantial period of time during childhood; and (2) probe causal paths and mechanisms relative to how neighborhood conditions or environments might cause these outcomes.

The subjects in our research are the children of current and former residents of the Denver (CO) Housing Authority (DHA) who are either Black or Latino. In addition to its large-scale, conventional public housing developments, DHA has operated since 1969 a *Dispersed Housing Program*, providing low-income families (current N=1,500) with opportunities to live in scattered-site, single-family and small-scale, multi-family units. These units are located in a wide range of neighborhoods throughout the City and County of Denver. Because the initial assignment of households on the DHA waiting list to either dispersed or conventional public housing developments mimics a random process, this program represents an *unusual natural experiment holding great potential* for overcoming methodological challenges in accurately measuring neighborhood effects on low-income children, inasmuch as it overcomes household location selection bias.

Prior to receiving support from the MacArthur Foundation, we collected both quantitative and qualitative data, employing a mixed-methods research strategy. We

acquired information from: (1) telephone surveys with over 706 current and former DHA tenants whose 1,766 children were the appropriate ages when they lived in DHA housing; and (2) U.S. census and local Denver (Piton Foundation) administrative databases related to the characteristics of neighborhoods. The first source provides retrospective information on *a battery of child outcome variables* constituting our focus: health, education, employment, marriage and childbearing, social skills and behaviors, and exposure to violence. It also provides respondents the opportunity to discuss, in a qualitative format, the means by which neighborhood environment may have influenced their children. The second source provides an unusually wide variety of neighborhood indicators measured at two spatial scales (census tract, and two-tract “neighborhoods”), so that *neighborhood context can be richly operationalized in our statistical models*.

Our two primary research questions are:

- ***The Magnitudes of Neighborhood Effects:*** For children who spent a considerable period during ages 0-18 living in DHA public housing, are there statistically and economically significant differences in their outcomes in six domains (health, education, employment, behavioral, demographic and exposure to violence) that can be attributed to differences in their neighborhoods (controlling for family and individual characteristics)?
- ***The Mechanism(s) of Neighborhood Effects:*** If there are neighborhood-based differences in child outcomes, what appear to be the salient mechanisms and causal paths operative in the neighborhood environment, and do these mechanisms and paths vary by outcome domain, neighborhood spatial scale, and child developmental stage, ethnicity or gender?

PART 2: Accomplishments during Grant Period

Quality Assurance Procedures for Our Analytical Databases

We spent considerable effort during the grant period verifying, cleaning, and reconciling the household survey data and the neighborhood indicators. In addition, we did painstaking checks of the new child-year panel database. This is the huge machine-readable data file (N=7,622 variables) that contains information we have gathered from the retrospective household surveys, administrative data, and the neighborhood

indicators that we have appended to each year of a child's life. For each child there are variables associated with: (1) fixed personal characteristics (e.g., gender, ethnicity), (2) fixed family characteristics (e.g., parental education, age, nationality); (3) annually varying personal characteristics (e.g., age, accumulated education); (4) annually varying family characteristics (e.g., # siblings, marital status, financial situation); (5) annually varying neighborhood characteristics (at both census tract and Piton neighborhood scales); and (6) annually varying behaviors, health issues, educational characteristics, and other outcomes. We also aggregated information from child-years into child developmental stage units of analysis for future use in most of our statistical work.

Frankly, this data verification step proved more challenging and time-consuming than expected. Nevertheless, we thought it crucial for the long-term veracity and validity of our research findings that requisite resources be devoted to this data cleaning-verification task, even if it ultimately meant that our production of "final results" during the grant period was more modest than we had hoped.

Publications and Presentations on Analytical Issues and Preliminary Findings

Our strategy in using the MacArthur grant resources involved developing our research effort methodically, establishing a firm analytical foundation for our efforts. We pursued this strategy in two ways. First, we thoroughly reviewed the literature on the mechanisms of neighborhood effects, synthesized this material into a new organizational framework, and critiqued what new research was still required. We were invited to present this work at an Economic and Social Research Council (UK) Conference on Neighbourhood Effects, held at St. Andrews University in February, 2010. The presentation and subsequent drafts of the paper have been enthusiastically reviewed and widely cited in the scholarly literature. The work (see Appendix 1) has just appeared as a book chapter:

Galster, G. (2012). The mechanism(s) of neighbourhood effects: Theory, evidence, and policy implications. Pp. 23-56 in van Ham, M., Manley, D., Bailey, N., Simpson, L., and Maclennan, D., eds. *Neighbourhood effects research: New perspectives*. Dordrecht, NL: Springer.

The second aspect of our strategy involved soliciting input from scholarly audiences across a wide range of disciplines (economics, sociology, geography, social

work, planning; psychology, health sciences) and nations (U.S., U.K., Sweden, Netherlands, Australia, Turkey). Inasmuch as the use of “natural experiments” to evaluate neighborhood effects is in its infancy, we wanted to ascertain the reaction of the scholarly community to our fundamental research design using a retrospective survey of DHA households, and some of the preliminary evidence that came out of that design. As part of this strategy we made 14 presentations.

- *“What Does Neighborhood Mean for the Development for Low-Income Children: Exploiting a Natural Experiment in Denver,”* Dept. of Geography and Urban Planning, University of Amsterdam, NL (January, 2009); Depts. of Geography and Sociology, Utrecht University, NL (January, 2009); Institute for Housing and Mobility Studies, Uppsala University, SW (February, 2009).
- *“Does Neighborhood Affect the Development of Low-Income, Minority Children? New Evidence from a Natural Experiment in Denver.”* Annual Meetings of the Urban Affairs Association (March, 2009); Annual Meetings of the Michigan Academy of Sciences (March, 2009).
- *“New Evidence on Neighborhood Effects.”* Urban Research Center, University of Western Sydney-Parramatta (April, 2009); University of Western Sydney-Bankstown, AU (April, 2009).
- *“Neighborhood Poverty and the Development of Low-Income, Minority Children: Preliminary Evidence from a Natural Experiment in Denver.”* Skillman Foundation, Good Neighborhoods Learning Partnership. (October, 2009); Annual Meetings of the Association for Public Policy Analysis and Management (November, 2009); Annual Meetings of the ENHR, Istanbul, TK (July, 2010).
- *“Challenges in Quantifying Neighborhood Effects.”* Dept. of Geography, Utrecht University NL. (November, 2009).
- *“Challenges of Estimating Unbiased Neighborhood Effects When Using Natural Experiments: The Case of Denver Public Housing.”* Quantitative Methodology Program Seminar, University of Michigan, Ann Arbor (January, 2010).

- *“Exploring the Potential of a Natural Experiment in Denver to Investigate Neighborhood Effects on Health.”* Wayne State University, Institute of Environmental Health Sciences (February, 2010).
- *“The Challenges to Estimating the Independent, Causal Effect of Neighborhoods on Child Developmental Outcomes: Potential Responses Using Evidence from a Natural Experiment in Denver.”* Mandel School of Applied Social Sciences, Case Western Reserve University, Cleveland OH (February, 2011).

As we had hoped, these initial presentations solicited enthusiastic support but also a variety of constructive suggestions on which we have followed up. Specifically, our audiences told us that they wanted: (1) more information about and formal reliability tests of the numerous summary indices we developed; (2) tests of the degree to which the assignment of DHA applicants to neighborhoods indeed mimicked a random process; and (3) more direct (i.e., not inferential based on statistical models) information about mechanisms of neighborhood effects. We responded to the first two requests by developing two technical papers that demonstrated both points. These documents are included here as Appendices 2 and 3. They are also posted on the Case Western Reserve Mandel School Website (<http://povertycenter.case.edu/>). We responded to the third request by fielding a set of in-depth interviews with study participant parents and their older children, which is described in the next section.

Interviews with Parents and their Children about Neighborhood Effect

Mechanisms

We developed field protocols and semi-structured interview guides, conducted pre-tests, recruited subjects, and completed 84 semi-structured interviews with a sample of 34 study participants and 50 of their young adult children in 2010. In 2011, these interviews were completely transcribed and each transcript was checked for completeness in a three-step verification process. In addition, all Spanish language interviews were translated into English and back-translated into Spanish per established protocols. We are currently analyzing these data to probe in depth our second research question about the mechanisms of neighborhood effects. As noted below, our preliminary review of these interviews has already surfaced important themes and

quotes which, in turn, are being used to enrich the interpretations of our statistical findings.

Conference Papers and Presentations on Findings

Having established a firm, “peer-reviewed” analytical foundation for our work, we have proceeded to conduct multivariate statistical investigations in the following topical areas: (1) educational outcomes; (2) young adult labor force outcomes; (3) exposure to violence; (4) teen childbearing and fathering; (5) behaviors; and (6) parental perceptions of neighborhood mechanisms influencing child outcomes.

In the realm of *education outcomes* we examined dropout behavior of low-income Black and Latino youth. We found that an index of perceived neighborhood problems, occupational prestige and foreign-born shares strongly predicted dropping out for the full sample, though the particulars were contextualized by gender and ethnicity. Neighborhood effects appeared stronger when measured concurrently with high school, instead of middle school. Neighborhood poverty rate generally proved a weak, incomplete measure of neighborhood context, although sustained exposure to low-poverty environments provided gains to black youth, suggesting that results from the MTO demonstration were not general. The paper is attached as Appendix 2.

We have also investigated a wide variety of education-related outcomes and behaviors for elementary through high school aged students (e.g., grades and honor roll attainment; expectations about educational attainment; participation in school clubs, activities or sports; participation in gifted and talented programs; participation in special education; school desertion behaviors; school detentions, suspensions and expulsions). Our initial findings suggest that increases in the share of foreign born in the neighborhood and higher socioeconomic status in terms of educational attainment and occupational prestige serve as protective factors increasing the odds of participation in school clubs and activities at the elementary and middle school levels as well as lowering the odds of being enrolled in special education classes in middle school. Higher levels of neighborhood deprivation are associated with increasing risk of school detention, suspension or expulsion during elementary school. Greater social disorder increases the risk of skipping school among high schoolers. Further, high levels of neighborhood violence decrease the odds that high school students will engage in school sports programs.

To date, we have made six presentations of our educational outcomes findings at scholarly conferences and invited lectures at universities:

- “Neighborhood Effects on High School Performance:” APPAM meetings, Boston (Nov. 2010); Urban Affairs Association meetings, New Orleans (March, 2011); European Network for Housing Research meetings, Toulouse, FR (July, 2011); University of Southern California, Los Angeles (August, 2011); ACSP conference, Salt Lake City (October, 2011).
- “Neighborhood Effects on the Educational Outcomes of Latino and African American across Childhood.” Annual Meetings of the Society for Social Work and Research, Washington DC (January, 2012).

In the realm of *labor market outcomes*, we found that those who experienced during late adolescence above-average degrees of social capital within above-average occupational prestige neighborhoods evinced substantially higher probabilities of working or attending post-secondary education as young adults. This relationship: (1) appeared stronger when neighborhood context was measured during late adolescence compared to early adolescence; (2) was robust for men and women; and (3) was stronger for Latinos than Blacks. The paper is attached as Appendix 3. We made three presentations of our findings at scholarly conferences:

- “Does Neighborhood Context Affect the Labor Force Outcomes for Poor Minority Teens?” Annual Meetings of the Urban Affairs Association, Honolulu, (March, 2010); Wayne State University, Merrill Palmer Skillman Institute (March, 2010); Annual Meetings of the Association for Policy Analysis and Management (November, 2011).

Our research to date on *exposure to violence* seeks to identify the characteristics of neighborhoods that make them most dangerous for children and youth in terms of their chances of being exposed to violence during childhood as either witnesses or victims. Among our low-income, minority children in Denver, nearly one out of three children had witnessed neighborhood violence during childhood; nearly one out of 10 children had been childhood victims. The median age of first witnessing neighborhood violence was 12 years; the median age at first victimization was 13 years. By age 18,

only 56% of the entire sample had not witnessed neighborhood violence. Noteworthy is the statistically significant, accelerated risk of exposure for African American males. By age 18, only 47% of African American males had not witnessed violence, compared to 49% of African American females, 52% of Latino males, and 59% of Latina females. This accelerated risk of exposure to violence is also noted among youth who resided in DHA for a majority of their childhood and particularly for African American males. By age 18 less than 20% of African American males in the majority DHA sample had not witnessed violence, compared to 27% of African American females, 52% of Latino males, and 43% of Latina females.

When examining the prevalence of childhood victimization, we found that 86% of the entire sample had not been victims. However, we found that African American males continued to experience an accelerated risk of victimization during childhood. By age 18, only 75% of African American males had not been victims of neighborhood violence, compared to 91% of African American females, 81% of Latino males, and 91% of Latina females. This accelerated risk of victimization is also noted among youth who resided in DHA for a majority of their childhood and particularly for African American males. Although 81% of all majority DHA youth reached adulthood without being victimized by neighborhood violence, less than 48% of African American males had not been victimized, compared to 84% of African American females, 80% of Latino males, and 89% of Latina females.

Social disorganization and concentrated disadvantage increased the odds of exposure to violence in childhood as well as shortened the time to first exposure. Social capital appears to reduce the risk of neighborhood exposure to violence and delays the onset of such exposure in *some* neighborhood contexts. Further, these preliminary findings suggest that official violent crime statistics are insignificant predictors of exposure to violence – perhaps because they are underreported or are measured for too large a geographical area to capture more localized behaviors. Compared to African American males, there is a lower risk of violence exposure for all females regardless of ethnicity as well as for Latino males. These results have led us to focus our attention on the roles that parents might play in reducing such exposure, particularly in terms of parental monitoring.

To date, we have completed three scholarly presentations discussing these outcomes related to exposure to violence:

- “The Contexts Shaping Childhood Exposure to Violence in Schools and Neighborhoods.” Annual Meetings of the Society for the Study of Social Problems (August, 2010).
- “You Can’t Live in This World Without a Fight: The Contexts Shaping Childhood Exposure to Neighborhood Violence.” Population Studies Center 50th Anniversary Reunion, University of Michigan, Ann Arbor MI (October, 2011); .Annual Program Meeting of the Council on Social Work Education, Atlanta (October, 2011).

Neighborhood contexts shaping *teen childbearing and fathering* is the topic of one dissertation (Jessica Lucero, Wayne State University, May 2012) and several conference presentations and the attached manuscript (see Appendix 4):

- “Neighborhood Risk and Protective Factors for Teenage Childbearing and Fathering among Latino and African American Youth: An Examination of the Magnitude and Timing of Neighborhood Effects.” Annual Meetings of the Society for the Study of Social Problems, New Orleans, LA (August, 2011).
- “Neighborhood Risk and Protective Factors for Teenage Childbearing and Fathering among Minorities.” Annual Program Meetings of the Council on Social Work Education, Atlanta (October, 2011).
- “Neighborhood Risk and Protective Factors for Teenage Childbearing and Fathering Among Minority Youth: An Examination of the Timing and Nonlinearities of Neighborhood Effects.” Annual Meetings of the Society for Social Work and Research, Washington DC (January, 2011).

In this line of our research, we are examining the extent to which neighborhood effects on teenage childbearing/fathering operate differentially for Black and Latino youth as well as estimating how these effects may vary according to the timing and duration of neighborhood exposure. One in five teens in the sample had birthed or fathered a child before age 20. The only neighborhood factor increasing the risk of teenage childbearing/fathering among Black youth was an increase in the level of perceived

social capital during middle school, perhaps reflecting ties with undesirable role models (e.g., other adults or youth who were teenage parents). In contrast, increasing fractions of foreign-born neighbors, which may be a proxy for increased levels of parental monitoring, served as the primary neighborhood protective factor reducing the risk of bearing or fathering of children among Black teens.

For Latino youth, the significant neighborhood risk factors associated with teen childbearing/fathering were residential instability and perceived neighborhood social disorder. Further, the presence of negative peers in the neighborhood during high school was also associated with a significant increase in the odds of teenage childbearing/fathering.

We have begun to analyze the relationship between neighborhood contexts on *the onset of five risky childhood behaviors*: running away from home; alcohol use; smoking, substance abuse; and the use of violent or aggressive behavior. To date, initial study findings on these risky behaviors were presented at one conference:

- “The Effects of Living in the ‘Hood on Child and Adolescent Behavior.” Annual Meetings of the Urban Affairs Association, New Orleans, LA (March, 2011).

Preliminary findings based on parental/caregiver reports suggest that 9% of the youth in the sample had run away before the age of 18. Nearly one in seven had started drinking before age 21 (the legal age in Colorado). Approximately 12% started smoking during childhood. Slightly more than 11% started using marijuana. More than one out of ten engaged in violent or aggressive behavior. Our analyses suggest that relatively few neighborhood conditions were statistically significant predictors of onset of these risky behaviors during childhood. Each point increase in the neighborhood social disorder index was associated with a 38% higher odds of starting to smoke; a 21% higher odds of running away from home; and a 15% higher odds of starting to use marijuana. Collective efficacy was a protective factor reducing the odds of starting to drink during childhood. Each additional point increase in the collective efficacy scale was associated with a 30% reduction in the odds of starting to drink during childhood. For both boys and girls, greater neighborhood social disorganization was associated with higher odds of smoking or running away from home. Greater collective efficacy reduced the odds of drinking among boys. None of the neighborhood context variables appear to predict the onset of violent or aggressive behavior during childhood. Finally, there were no significant

neighborhood predictors of gender differences in the onset of marijuana use.

To begin to address our second question about the mechanisms of neighborhood effect, we have utilized qualitative data from our survey data that summarizes *parental perceptions of neighborhood mechanisms influencing child outcomes*. To date, this line of analysis has produced two conference presentations:

- “Parental Perceptions of Neighborhood Influence on Latino and African American Adolescents.” Annual Meetings of the Society for Social Work and Research, Tampa, FL (January, 2011).
- “Parental Perceptions of Neighborhood Influence on Latino and African American Child Outcomes.” Annual Meetings of the Society for the Study of Social Problems, New Orleans, LA (August, 2011).

According to parents/caregivers in the study, the most salient positive neighborhood mechanisms affecting childhood outcomes were local institutional resources (e.g., neighborhood recreation centers, after school programs) and the presence of local social networks and sources of support (e.g., positive adult role models, neighbors they can trust). Most salient negative neighborhood mechanisms were exposure to violence. Starting in elementary school, parents and caregivers also acknowledge the role of social contagion (peer influences) on child outcomes. Between 15 and 20% of parents/caregivers reported that their neighborhood contexts had no effect on their children – these percentages remained fairly consistent across developmental stages. Moreover, between one in six and almost one in four parents/caregivers reported that there were no neighborhood effects because of their ability to buffer their children from any potentially negative influences. Our preliminary analyses suggest significant differences in perceptions of neighborhood effects mechanisms by ethnicity, nativity status, disability status, educational attainment and parental efficacy. The dominant conclusion from our analysis thus far is that, in the eyes of low-income parents, effects that flow from social relationships within neighborhoods are much more important than effects from external forces.

Finally, a selection of our results were reported at an international roundtable called by the French Housing Ministry in an effort to guide forthcoming policies related to

the social mixing of their public housing estates:

- “Recent Evidence on Neighborhood Effects,” Centre Analyses Strategique, French Housing Ministry, Paris (November, 2011).

Papers in Progress

We are currently working on several papers assessing neighborhood effects on the physical and mental health outcomes of children as well as on the birth outcomes of low-income, minority infants. The first paper will investigate the extent to which asthma (affecting 22% of the children in the study) and cognitive disorders, such as autism, ADD or ADHD, developmental or learning disabilities, affecting 14% of the children, can be attributed to differences in concurrent, lagged, or cumulative neighborhood environments. In the second paper, neighborhood correlates associated with the use of mental health services will be examined. Our final paper will consider the extent to which pre-term birth and health problems at birth among low-income, minority infants are statistically related to the environmental, social and economic conditions in the neighborhoods where their mothers lived. All three of these papers will be presented at the Annual Meetings of the Urban Affairs Association in April 2012.

- “Neighborhood Effects on the Use of Mental Health Services by Latino and African American Children.” Paper to be presented at the Annual Meetings of the Urban Affairs Association, Pittsburgh PA.
- “Estimating the Effects of Neighborhood on the Physical Health Outcomes of Latino and African American Children.” Paper to be presented at the Annual Meetings of the Urban Affairs Association, Pittsburgh PA.
- “Neighborhood Risk and Protective Factors Influencing the Birth Outcomes of Low-Income Latino and African American Infants.” Paper to be presented at the Annual Meetings of the Urban Affairs Association, Pittsburgh PA.

APPENDIX 1.

Galster, G. (2012). The mechanism(s) of neighbourhood effects: Theory, evidence, and policy implications. Pp. 23-56 in van Ham, M., Manley, D., Bailey, N., Simpson, L., and Maclennan, D., eds. *Neighbourhood effects research: New perspectives*. Dordrecht, NL: Springer.

The Mechanism(s) of Neighbourhood Effects: Theory, Evidence, and Policy Implications

George C. Galster

In van Ham M., Manley D., Bailey N., Simpson L., Maclennan D. (eds) *Neighbourhood Effects Research: New Perspectives* (pp. 23-56). Dordrecht, NL: Springer, 2012.

Introduction

Although there has been a burgeoning literature on quantifying the relationship between various aspects of the residential environment and numerous outcomes for individual adults and children residing in that environment, comparably less attention has been given to uncovering empirically the causal mechanisms that yield these relationships. There have been many discussions of the potential causal connections between neighbourhood context and individual behavioural and health outcomes; see especially Jencks and Mayer (1990), Gephart (1997), Ellen and Turner (1997), Wandersman and Nation (1998), Friedrichs (1998), Green and Ottoson (1999), Atkinson and Kintrea (2001), Booth and Crouter (2001), Small and Newman (2001), Sampson (2001), Ellen, Mijanovich and Dillman (2001), Haurin, Dietz and Weinberg (2002), Sampson, Morenoff, and Gannon-Rowley (2002), Ellen and Turner (2003), Ioannides and Loury (2004), Pinkster (2008), and Phibbs (2009). Though often in these works the listings of potential mechanisms differ in labelling and categorizations, there is a broad consensus about how the underlying causal paths are thought to operate in theory. Unfortunately, there are few tentative conclusions, let alone any consensus, about which mechanisms demonstrate the strongest empirical support. The following quotes are illustrative. "In general research findings...are too scant to draw any firm conclusions about the potential pathways through which neighbourhood effects may be transmitted..." (Leventhal & Brooks-Gunn, 2000: 322). "The causal pathways that underlie hypotheses about the effects of neighbourhood social factors are often not explicit...This clearly is an important next step for understanding the relationship between neighbourhood and health." (Pickett & Pearl, 2001: 117, 120). "One important question is *how* a less advantaged neighbourhood increases the risk of low birth weight and of children developing behavioural problems...Understanding of the causal chains in both of these areas is...incomplete." (Sellström & Bremberg, 2006: 553).

Given this lack of scholarly consensus, my purposes in this paper are four-fold: (1) offer a comprehensive listing of 15 potential causal pathways between neighbourhood context and individual behavioural and health outcomes,

which synthesizes both sociological and epidemiological perspectives; (2) provide a new conceptualization of dimensions of neighbourhood effect mechanisms that uses a pharmacological analogy to clarify the empirical challenges of this field of enquiry; (3) provide an updated, international review of empirical studies related to neighbourhood effect mechanisms; and (4) draw provisional conclusions about the dominant neighbourhood effect mechanisms operating and implications from this review for scholarship and public policy. The paper is organized as follows. I begin with an overview of the numerous possible neighbourhood effect mechanisms that have been hypothesized, and group them into four categories. Next I will examine a variety of issues that render the identification of neighbourhood causal mechanisms particularly challenging for social scientists, and the two methodological approaches that have been adopted thus far. These issues are brought into clear relief by use of a pharmacological metaphor: *dosage-response*. I will then synthesize the international evidence related to the four categories of mechanisms, each in its own subsection, in an effort to assess the state of empirical scholarship and offer some provisional conclusions. Finally, I close the paper by drawing implications for future scholarship and public policy.

How Might Neighbourhood Effects Transpire?

Prior scholarly works addressing this question have been distinctly segregated, with social scientists focusing on behavioural outcomes and epidemiologists focusing on health outcomes. However, within each subset there is broad theoretical agreement about potential causal pathways of neighbourhood effects. I therefore will list these mechanisms and describe them only briefly here. My synthesis of these disparate literatures suggests that fifteen (15) distinctive linkages have been advanced. I think it is most useful to group these 15 mechanisms of neighbourhood effects under four broad rubrics: social interactive; environmental; geographical; and institutional.¹

Social-Interactive Mechanisms

This set of mechanisms refers to social processes endogenous to neighbourhoods. These processes include:

¹ By contrast, Manski (1995) groups them into “endogenous,” “exogenous,” and “correlated” categories. Ellen and Turner (1997) group them into five categories: concentration, location, socialization, physical, and services. Leventhal and Brooks-Gunn (2000) use the rubrics “institutional resources,” “relationships,” and “norms/collective efficacy.”

- *Social Contagion:* Behaviours, aspirations, and attitudes may be changed by contact with peers who are neighbours. Under certain conditions these changes can take on contagion dynamics that are akin to “epidemics.”
- *Collective Socialization:* Individuals may be encouraged to conform to local social norms conveyed by neighbourhood role models and other social pressures. This socialization effect is characterized by a minimum threshold or critical mass being achieved before a norm can produce noticeable consequences for others in the neighbourhood.
- *Social Networks:* Individuals may be influenced by the interpersonal communication of information and resources of various kinds transmitted through neighbours. These networks can involve either “strong ties” and/or “weak ties.”
- *Social cohesion and control:* The degree of neighbourhood social disorder and its converse, “collective efficacy” (Sampson, Morenoff, and Earls, 1999), may influence a variety of behaviours and psychological reactions of residents.
- *Competition:* Under the premise that certain local resources are limited and not pure public goods, this mechanism posits that groups within the neighbourhood will compete for these resources amongst themselves. Because the outcome is a zero-sum game, residents’ access to these resources (and their resulting opportunities) may be influenced by the ultimate success of their group in “winning” this competition.
- *Relative Deprivation:* This mechanism suggests that residents who have achieved some socioeconomic success will be a source of disamenities for their less-well off neighbours. The latter, it is argued, will view the successful with envy and/or will make them perceive their own relative inferiority as a source of dissatisfaction.
- *Parental Mediation:* The neighbourhood may affect (through any of the mechanisms listed under all categories here) parents’ physical and mental health, stress, coping skills, sense of efficacy, behaviours, and material resources. All of these, in turn, may affect the home environment in which children are raised.

Environmental Mechanisms

Environmental mechanisms refer to natural and human-made attributes of the local space that may affect directly the mental and/or physical health of residents without affecting their behaviours. As in the case of social-interactive mechanism, the environmental category can also assume distinct forms:

- *Exposure to Violence:* If people sense that their property or person is in danger they may suffer psychological and physical responses that may

impair their functioning or sensed well-being. These consequences are likely to be even more pronounced if the person has been victimized.

- *Physical Surroundings:* Decayed physical conditions of the built environment (e.g., deteriorated structures and public infrastructure, litter, graffiti) may impart psychological effects on residents, such as a sense of powerlessness. Noise may create stress and inhibit decision-making through a process of “environmental overload” (Bell et al., 1996).
- *Toxic Exposure:* People may be exposed to unhealthy levels of air-, soil-, and/or water-borne pollutants because of the current and historical land uses and other ecological conditions in the neighbourhood.

Geographical Mechanisms

Geographic mechanisms refer to aspects of spaces that may affect residents' life courses yet do not arise within the neighbourhood but rather purely because of the neighbourhood's location relative to larger-scale political and economic forces such as:

- *Spatial Mismatch:* Certain neighbourhoods may have little accessibility (in either spatial proximity or as mediated by transportation networks) to job opportunities appropriate to the skills of their residents, thereby restricting their employment opportunities.
- *Public Services:* Some neighbourhoods may be located within local political jurisdictions that offer inferior public services and facilities because of their limited tax base resources, incompetence, corruption, or other operational challenges. These, in turn, may adversely affect the personal development and educational opportunities of residents.

Institutional Mechanisms

The last category of mechanisms involves actions by those typically not residing in the given neighbourhood who control important institutional resources located there and/or points of interface between neighbourhood residents and vital markets:

- *Stigmatization:* Neighbourhoods may be stigmatized on the basis of public stereotypes held by powerful institutional or private actors about its current residents. In other cases this may occur regardless of the neighbourhood's current population because of its history, environmental or topographical disamenities, style, scale and type of dwellings, or condition of their commercial districts and public spaces. Such stigma may

reduce the opportunities and perceptions of residents of stigmatized areas in a variety of ways, such as job opportunities and self-esteem.

- *Local Institutional Resources:* Some neighbourhoods may have access to few and/or high-quality private, non-profit, or public institutions and organizations, such as benevolent charities, day care facilities, schools, and medical clinics. The lack of same may adversely affect the personal development opportunities of residents.
- *Local Market Actors:* There may be substantial spatial variations in the prevalence of certain private market actors that may encourage or discourage certain behaviours by neighbourhood residents, such as liquor stores, fresh food markets, fast food restaurants, and illegal drug markets.

Conceptual Issues in Uncovering and Measuring Mechanism(s) of Neighbourhood Effects

I begin this discussion with the premise that the ultimate goal of social science is to not only identify which mechanisms are responsible for creating a designated effect on residents but to ascertain *quantitatively their relative contributions* to the outcome of interest. For the purposes of this discussion it is useful to employ a pharmacological metaphor: “dosage-response.” There is substantial empirical evidence that several sorts of variables measuring neighbourhood-level indicators are correlated with a variety of behavioural and health outcomes for children, youth, and adults; for reviews see: Haveman and Wolfe (1994), Duncan, Brooks-Gunn and Aber (1997), Van Kempen (1997), Gephart (1997), Ellen and Turner (1997), Friedrichs (1998); Leventhal and Brooks-Gunn (2000), Booth and Crouter (2001), Atkinson et al. (2001), Ellen, Mijanovich and Dillman (2001), Pickett and Pearl (2001), Haurin, Dietz and Weinberg (2002), Dietz (2002), Sampson, Morenoff, and Gannon-Rowley (2002), Musterd (2002), Friedrichs, Galster and Musterd (2003), Kawachi and Berkman (2003), Galster (2005), Sellström and Bremberg (2006), and Schaefer-McDaniel et al. (2009). The question here is “Why?” I find it revealing to employ a pharmacological metaphor here and frame the questions as follows: What about this “dose of neighbourhood” might be *causing* the observed individual “response?” The challenges in answering this deceptively simple question are legion, and my purpose here is to present some of the major ones.² If we are to deeply understand why aspects of the neighbourhood context affect residents we ultimately must answer 17 questions arrayed under three overarching rubrics regarding the *composition, administration, and response to the neighbourhood dosage*.

² Note that this discussion is related to but distinct from the question of how to accurately measure the magnitude of this dosage-response relationship, about which I wrote in Galster (2008).

The Composition of the Neighbourhood Dosage

- *What are the “active ingredients” that constitute the dosage?* What is it about this space in terms of internal social interactions, environmental conditions, geographic attributes, and reactions of external institutional drivers that is (are) the causal agent(s) and how can it (they) be measured precisely? If neighbourhood is a multi-dimensional package of causal attributes, as is likely, each part of the package will need to be identified and measured directly.

The Administration of the Neighbourhood Dosage

- *Frequency: How often is the dosage administered?* For example, does a particular form of social interaction occur only rarely or (as in the case of air pollution) is the exposure occurring during each inhalation?
- *Duration: How long does the dosage continue, once begun?* Certain social interactions can vary dramatically in their length, whereas the dosage of unresponsive public services and non-existent facilities can be omnipresent.
- *Intensity: What is the size of the dosage?* How concentrated are the toxins? How weak are the local services? In the case of social interactive causes, the answers to the frequency, duration and intensity questions will be related to the amount of time that the individual spends in the neighbourhood and outside of the home in “routine activities.”
- *Consistency: Is the same dosage being applied each time it is administered?* Do pollutants or the threat of victimization vary daily based on meteorological conditions or time of day?
- *Trajectory: Is the frequency, duration, and/or intensity of dosage growing, declining, or staying constant over time for the resident in question?* Do the individuals in a rising trajectory context evince fewer effects because they get more “immune” or evince more effects because their resistance is “weathered?”
- *Spatial Extent: Over what scale does the dosage remain constant?* How rapidly does the frequency, duration, intensity and/or consistency of dosage decay when the subject travels away from the residence? Do any of these gradients vary according to the direction of movement away from the residence?
- *Passivity: Does the dosage require any action by residents (cognitive or physical) to take effect?* I.e., do residents need to engage in any activities or behaviours, or even be cognizant of the forces operating upon them for the effect to transpire? In the case of endogenous local social interactions,

the answer is probably yes, but not in the case of the other categories of mechanisms.

- *Mediation: Is the dosage received directly or indirectly by the resident in question?* For example, neighbourhood influences on children may be mediated by parents who are directly affected by the neighbourhood.

The Neighbourhood Dosage-Response Relationship

- *Thresholds: Is the relationship between variation in any dimension of dosage administration and the response nonlinear?* Are there critical points at which marginal changes in the dosage have non-marginal effects?
- *Timing: Does the response to the dosage occur immediately, after a substantial lag, or only after cumulative administration?* For example, you might become stigmatized as soon as you move into a certain neighbourhood, but eroded health due to lack of local recreational facilities may not show up until an extended period has elapsed.
- *Durability: Does the response to the dosage persist indefinitely or decay over time slowly or quickly?* The developmental damage done by lead poisoning is, for illustration, indelible.
- *Generality: Are there many predictable responses to the given dosage administration, or only one?* Peers may influence a wide variety of adolescent behaviours, whereas certain environmental toxins may have rather narrowly defined health impacts.
- *Universality: Is the relationship between variation in any dimension of dosage administration and the particular response similar across children's developmental stages, demographic groups, or socioeconomic groups?* The same dosage of neighbourhood may yield different responses depending on the developmental or socioeconomic status of those exposed.
- *Interactions: Are dosages of other intra- or extra-neighbourhood treatments also being administered that intensify the given dosage's expected response?* Different dimensions of neighbourhood may not be additive but multiplicative.
- *Antidotes: Are dosages of other intra- or extra-neighbourhood treatments also being administered that counteract the given dosage's expected response?* For example, efforts to improve residents' health by building new clinics and outreach facilities in the neighbourhood may apparently founder if environmental pollution in the area gets worse.
- *Buffers: Are people, their families, and/or their communities responding to the dosage in ways that counteract its expected response?* Because residents individually and collectively potentially have agency they may

engage in compensatory behaviours that offset negative neighbourhood effects, such as when parents keep their children in the home when certain violent youngsters are using the local playground.

Past Investigative Responses and their Limitations

There are two broad sorts of approaches that social scientists have employed in an attempt to answer the above questions and uncover the dominant neighbourhood effect mechanisms at work: (1) field-interview studies of people's social relations and networks within neighbourhoods and non-residents' opinions about neighbourhoods, involving both quantitative and qualitative analyses of the data collected thereby; and (2) multivariate statistical studies estimating models of how various neighbourhood indicators are correlated with a variety of individual outcomes for children, youth, and adults. Field-interview studies try directly to observe potential mechanisms. In this vein, there have been numerous sociological and anthropological investigations, but they are often limited in their ability to discern the relative contributions of alternative causes because of their qualitative nature and their typical focus on only one set of mechanisms to the exclusion of others. Nevertheless, several have been revealing and remarkably consistent in their findings that allow us to rule out certain potential causes. Moreover, this style of investigation is more appropriate for probing many of the questions noted above, such as active ingredients, passivity, mediation, and buffering of dosages.

The multivariate statistical approach tries to draw inferences about neighbourhood effect mechanisms from the statistical patterns observed. It has its own challenges, akin to a physician making a differential diagnosis on the basis of a patient's symptoms and only a partial, poorly measured medical history. One inferential notion that has been used is that if particular sorts of descriptors of a neighbourhood prove more statistically and economically significant predictors of resident outcomes they may hint at which underlying process is dominant. For example, if the variable "percentage of poor residents in the neighbourhood" was not to prove significant but the variable "percentage of affluent residents in the neighbourhood" was to in a regression predicting outcomes for low-income residents, it would suggest that a positive social externality from the affluent group like role modelling, not a negative social externality from the poor group like peer effects, was present. Unfortunately, an overview of the research record typically does not produce such unambiguous results for coefficients. Moreover, most of this statistical literature is of little help to us here because it does not disaggregate findings by economic or demographic group (though see Galster, Andersson and Musterd, forthcoming). For example, how is one to interpret the finding from a regression model estimated over youth from all income groups that there is a negative correlation between the percentage of poor households in the neighbourhood and an individual's chances of dropping out from high school? One cannot make the deduction that non-poor youth are positively influencing poor youth through

role modelling. A second inferential notion often employed draws upon the assumption that different types of neighbourhood social externalities yield distinctive functional forms for the relationship between the percentage of disadvantaged and/or advantaged residents in a neighbourhood and the amount of externality being generated. For example, collective social norms and social control likely come into play only after a threshold scale of the population group thought to be generating this effect has been achieved in the neighbourhood. This logic can be used to draw out implications for underlying mechanisms of neighbourhood effects if the statistical procedures used to investigate the relationship between neighbourhood indicator(s) and individual outcome permit the estimation of non-linear relationships. Unfortunately, few extant empirical studies test for non-linear relationships between neighbourhood indicators and various individual outcomes. Moreover, even if thresholds and other distinctive non-linearities are observed, it need not uniquely identify only one causal mechanism.

In the review that follows I will organize the presentation in subsections corresponding to the foregoing mechanisms of neighbourhood interaction,³ bringing to bear evidence from the two approaches as relevant. Before turning to this empirical evidence, however, I note as preface that no definitive, comprehensive study of neighbourhood effect mechanisms exists; none examines more than one or two of the above questions for an array of potential causal mechanisms. Indeed, most of the questions to my knowledge have not been addressed explicitly in the theoretical or empirical literature. Thus, most empirical conclusions regarding neighbourhood effect mechanisms should be treated as provisional at best.

Evidence on Social-Interactive Mechanisms of Neighbourhood Effects

Social Contagion and Collective Socialization

There have been numerous studies that have examined in detail the social relationships of youth from disadvantaged neighbourhoods. They have identified links between deviant peer group influences and adolescents' grade point average (Darling & Steinberg, 1997), mental health (Simons et al., 1996), anti-social behaviour, school attainment, and substance abuse (Dubow, Edwards & Ippolito, 1997). One of the most notable, because of its sophisticated efforts to avoid statistical bias, is Case and Katz's (1991) investigation of youth in low-income Boston neighbourhoods. They find that neighbourhood peer influences among low-income youth are strong predictors of a variety of negative behaviours, including

³ I combine the competition and relative deprivation mechanisms because, to my knowledge, there is little extant statistical evidence that can distinguish between them.

crime, substance abuse, and lack of labour force participation. For more supportive evidence on the importance of role models and peer effects in disadvantaged neighbourhoods, see Sinclair et al. (1994); Briggs (1997a); South and Baumer (2000), Ginther, Haveman and Wolfe (2000), South (2001), and Oberwittler (2004). To me this body of U.S. work suggests that negative social externalities are often being generated through peer effects and role models among disadvantaged young neighbours.⁴

However, the extent to which such negative socialization would be diminished, or replaced by positive socialization, were more higher-income youth to be present is unclear. Rosenbaum (1991, 1995, et al., 2002) has provided a series of studies related to black families living in public housing in concentrated poverty neighbourhoods who were assisted (with rental vouchers and counselling) in finding apartments in majority white-occupied neighbourhoods of Chicago and its suburbs as part of a court-ordered remedy for the *Gautreaux* public housing discrimination suit. Though he provides one of the most optimistic portraits of the benefits that such moves can provide to black adults and their children, he does not find a great deal of social interchange or networking between these new in-movers and the original residents. Rosenbaum (1991) concludes by stressing instead the importance of role models and social norms in middle class suburban environments for generating positive outcomes for those participating in the *Gautreaux* Program. However, this optimistic conclusion has been challenged by recent qualitative case studies revealing limited role modelling between upper-income and lower-income blacks in gentrifying neighbourhoods (Boyd, 2008; Freeman, 2006; Hyra, 2008).

The threshold notion embedded in both the social contagion and collective socialization (norm) mechanisms potentially allows them to be identified by regression-based studies that allow for non-linear relationships between the measure of neighbourhood. My (Galster, 2002) review of the U.S. literature (Vartanian 1999a, 1999b; Weinberg, Reagan & Yankow, 2004) suggests that the independent impacts of neighbourhood poverty rates in encouraging negative outcomes for individuals like crime, school leaving, and duration of poverty spells appear to be nil unless the neighbourhood exceeds about 20 percent poverty, whereupon the externality effects grow rapidly until the neighbourhood reaches approximately 40 percent poverty; subsequent increases in the poverty population appear to have no marginal external effect. Analogously, the independent impacts of neighbourhood poverty rates in discouraging positive behaviours like working appear to be nil unless the neighbourhood exceeds about 15 percent poverty, whereupon the effects grow rapidly until the neighbourhood reaches roughly 30 percent poverty;

⁴ However, it is not definitive about the extent to which such negative socialization is general across races. Turley (2003) probes beyond her discovery of overall positive correlations between median family income of neighborhood and youths' behavioral and psychological test scores to see whether there were interaction effects with proxies for number of peer interactions and time spent in neighborhood. She found such strong interaction effects for white but not black youths in her sample, and concluded "differences in neighborhood socializing may explain why neighborhood income affects black and white children differently" (2003: 70).

subsequent increases in poverty appear to have no marginal effect. This evidence supports the social contagion and/or collective socialization processes.

As far as non-linear relationships between individual outcomes and neighbourhood percentages of affluent residents, the work of Crane (1991), Duncan et al. (1997), and Chase-Lansdale et al. (1997) is relevant. Unfortunately, though they all suggest the existence of a threshold of neighbourhood affluence they differ on where this occurs. Crane's (1991) analysis finds strong evidence of epidemic-like effects on both secondary school leaving and teenage childbearing of the share of affluent (professional-managerial occupation) neighbours. For whites and blacks there is a threshold at five percent affluent neighbours, below which dropout rates skyrocket; for blacks not living in large cities there is another threshold at 20 percent, above which affluent neighbours cease having a positive impact. These thresholds are more dramatic for black males than females. A similar threshold at low percent affluent neighbours is observed for both black and white teen women's childbearing, especially in large cities. Crane (1991: 1234, 1241) interpreted these findings as consistent with intra-neighbourhood social interactions, but was unable to distinguish whether the high-status neighbours created an endogenous effect (such as serving as positive role models) or a correlated effect (such as bringing resources that made local institutions and services better). Duncan et al. (1997) find a different sort of nonlinear neighbourhood effect for educational attainment and the percentage of affluent neighbours. Here the threshold does not seem to occur at a small percentage of affluent, as in Crane's study.⁵ The positive effect of the latter becomes dramatically stronger when the percentage exceeds the national mean for the neighbourhood (for black men and women, and white women). Chase-Lansdale et al. (1997) examine how the percentage of affluent neighbours relates to a variety of intellectual and behavioural development test scores for youth. They find, controlling for family influences, that the percentage of affluent neighbours is positively associated with higher intellectual functioning scores for black children and female children only when the percentage exceeds the 25th percentile and is less than the 75th percentile; for other children the effect is linear. Both the Duncan et al. (1997) and Chase-Lansdale et al. (1997) findings support the notion of collective social norms taking hold only after a substantial share of the affluent group is present in the neighbourhood.⁶

Most Western European evidence related to potential non-linear neighbourhood effects focuses on labour market outcomes as they relate to per-

⁵ Duncan et al. (1997) did not explicitly test for a threshold at a below-average percentage of affluent, however.

⁶ Turley (2003) analyzes behavioral and psychological test scores for youth as measured in a special supplement of the PSID. She relates these scores to the median family income of the census tract, so one cannot be certain whether the relationship is being generated by share of affluent or share of poor. She tests for non-linearities by employing a quadratic version of neighborhood income variable and finds that its coefficient is statistically significant and negative for the self-esteem outcome, implying that improving the economic environment of youth has a much greater psychological impact for those initially in disadvantaged neighborhood circumstances. Unfortunately, quadratic specifications are not precise in identifying thresholds

centages of disadvantaged neighbours. Here the findings are inconsistent in the extreme. Several studies did not observe any strong nonlinear relationships. Ostendorf, Musterd, and de Vos (2001) compared “income-mixed” neighbourhoods in Amsterdam with “homogeneous” ones, to ascertain whether this aspect of neighbourhood was related to an individual’s chances of living in poverty. Bolster et al. (2004) compared one-, five- and ten-year income growth trajectories of British individuals living at the beginning of the period amid different degrees of disadvantage (measured by a composite index). Finally, McCulloch’s (2001) multi-level analysis of British Household Panel Study data also failed to identify any strong non-linearities between a ward-level index of disadvantage and such outcomes as employment status, current financial situation, financial expectations, health status, or receipt of social support. Musterd, Ostendorf and de Vos (2003) related the proportion of neighbouring households on social benefits to the chances of Dutch individuals’ being employed consistently or not during the 1990s. Over a vast variation in neighbourhoods they found no relationship. Though arguably some non-linearities were evinced at the extreme values of neighbourhood conditions, they involved few neighbourhoods.

Other studies detected non-linear relationships, but of highly inconsistent natures. Buck’s (2001) analysis of British Household Panel Study data (but, unlike McCulloch, using unemployment rate as the neighbourhood characteristic) identified substantial non-linearities with the probability of not starting work and the probability of not escaping from poverty, which suggested that the worst results for individuals occurred when the share of neighbourhood residents unemployed exceeded 23-24 percent (i.e., the highest five percent of all wards). Diametrically opposed results were generated by Musterd and Andersson (2006), who analyzed the Swedish national register database for the three largest metropolitan areas in Sweden to ascertain the relationship between the odds that an individual remained unemployed in both 1995 and 1999 and the percentage of unemployed residents in their neighbourhood in 1995. They (like Buck) found a strong positive relationship until the neighbourhood percentage unemployed exceeded 16%; thereafter there appeared to be no further marginal impact (instead of increasing marginal impact, as per Buck). Van der Klaauw and van Ours (2003) found, using data from Rotterdam (NL) administrative records, that the neighbourhood unemployment rate had no statistically significant negative impact on the probability of exiting welfare into work for Dutch job losers or school leavers until it surpassed 11 percent, though there were no neighbourhood effect for non-Dutch job losers.

Only two studies using Western European data have investigated the potential nonlinear effects of affluent neighbours. Kauppinen (2004) used categorical variables to delineate neighbourhood affluence in Helsinki and, like Duncan et al. (1997), found that only in neighbourhoods with above-average educational levels does the neighbourhood seem to make a difference in individuals’ post-secondary level of educational attainment.

Galster et al. (2008) study the effects of both disadvantaged and advantaged neighbours on individual earnings of adults using Swedish urban data. In the case of men who were not employed full time, it was the neighbourhood with the

highest possible share of *middle-income neighbours* that was most conducive to their earning more. The fact that even a few low-income neighbours eroded these benefits suggested to the authors that a negative role modelling or peer effect was transpiring here. Replacing middle-income with high-income neighbours also had negative impacts on these less-advantaged males, implying that the former provided positive role models and/or resource rich networks but the latter did not, perhaps because the social distance between the groups was too great for social interactions. The collective socialization model of interaction was not supported by their findings, because no minimum threshold of low-income neighbours was observed past which their negative impacts began and because such would imply no distinctions between shares of middle- and high-income neighbours under the assumption that both provided comparable norms and social controls.

In sum, this Western European evidence on non-linear neighbourhood effects is so inconsistent that no clear implications can be drawn regarding social contagion and collective socialization causal mechanisms. Nevertheless, it is fair to say that it does not appear to evince non-linear neighbourhood effects similar to those more consistently appearing in the U.S.-based research.

Social Networks

Tiggs, Brown and Greene (1998) investigate the social networks of blacks in U.S. urban areas. They find that, controlling for personal income, those in areas of concentrated poverty typically are more isolated within their households; they have fewer close external ties, especially with those who are employed or well-educated. . These findings replicate those of Fernandez and Harris (1992), who also found that the volume, breadth and depth of social relationships in poor neighbourhoods were especially attenuated for black females. Coupled with consistent evidence that job-seekers in U.S. high-poverty areas rely upon neighbours for potential employment information, and the situation appears ripe for neighbourhood effects in disadvantaged U.S. places working through resource-poor social networks.

Two statistical studies provide further support to the hypothesis that the "social network" mechanism of neighbourhood effect has veracity when it comes to finding employment in the U.S. Bertrand, Luttmer and Mullainathan (2000) consider the impact of local social networks on welfare participation. They find welfare participation was enhanced not only by geographic proximity to others on welfare, but especially if these proximate others on welfare spoke the individual's language. Bayer, Ross and Topa (2004) examine the degree to which people who live on the same census block also tend to work on the same census block. They find that individuals indeed interact very locally when exchanging information about jobs, even when controlling for personal characteristics. However, given the typical high degree of class and race segregation in American neighbourhoods it is not clear how much of the observed local social networks span across groups. In-

deed, consistent with sociological field evidence above, Bayer, Ross and Topa (2004) find that interactions are stronger when pairs of individuals are more likely to interact because of common education.

Evidence also suggests that the social networks established in disadvantaged U.S. neighbourhoods may be so influential that they are difficult to break even after moving away. Briggs (1998) examined the social networks of black and Hispanic youth who participated in a court-ordered, scattered-site public housing desegregation program in Yonkers (NY) during the 1990s. He found few differences in the network diversity or types of aid provided by networks comparing youth who moved to developments in white, middle-class neighbourhoods in Yonkers compared to those who remained in traditional public housing in poor, segregated neighbourhoods. The former group did not leverage any benefits of living in more affluent and racially diverse areas, and their social ties typically remained within the common race-class confines of their scattered-site developments. Popkin, Harris and Cunningham (2002) and Rosenbaum, Harris and Denton (2003) found that families participating in the Moving To Opportunity demonstration in Chicago were likely to maintain close social ties with their former, poverty-stricken neighbourhoods even after they moved a considerable distance away to low-poverty neighbourhoods. More than half indicated that their social networks were located someplace other than their new neighbourhood.

A complementary view is provided by U.S.-based field studies, which consistently show that the social interaction among members of different economic groups is quite limited, even within the same neighbourhood or housing complex. Members of the lower-status group often do not take advantage of proximity to broaden their “weak ties” and enhance the resource-producing potential of their networks, instead often restricting their networks to nearby members of their own group. Schill (1997) investigated relationships between different classes of residents living in a newly modernized public housing complex in Chicago that intentionally tried to mix employed, moderate-income households amid unemployed, poor households. Few social ties developed between the groups in the development. Similar conclusions were reached by Clampet-Lundquist (2004) in her study of residents displaced from a revitalized Philadelphia public housing development and Kleit (2001a, 2001b, 2002, 2005, Kleit & Carnegie, 2009) in a series of mixed-income housing developments in Maryland and Washington. Several European-based studies have probed this topic as part of restructuring of social housing estates (Atkinson & Kintrea, 1998; Jupp, 1999; Van Beckhoven & Van Kempen, 2003; Duyvendak, Kleinhans & Veldboer, 2000) or post-war neighbourhoods (Blokland-Potters, 1998; Pinkster, 2008) and reached similar conclusions.⁷

Several multivariate studies based on European data contribute as well to our understanding of neighbourhood networks. Buck (2001) uses British Household Panel Study data to ascertain a positive relationship between the probability that individuals have no close friends employed and neighbourhood unemploy-

⁷ See review in Kleinhans (2004).

ment rates or disadvantage index scores. When coupled with the aforementioned positive relationship Buck observed between these neighbourhood indicators and an individual's probability of not starting work and remaining in poverty, the totality of results are supportive of the importance of local job information networks as a mechanism of transmitting a neighbourhood effect. Farwick (2004) finds that Turkish individuals' contacts with native Germans declines rapidly once the percentage of Turks in the apartment complex exceeds 20 percent. In turn, this lack of contact increases Turks' chances of having an unstable employment history. Galster et al. (2008, 2009) show that Swedish individuals with a weaker labour market position apparently benefited more from middle-income than high-income neighbours, consistent with the view that the resource-enhanced job information networks provided by better-off neighbours was only influential if the class divide ("social distance") was not too extreme. Pinkster's (2008) study of networks in deprived neighbourhoods in The Hague (NL) discovered that localized social ties helped low-income residents in the short-term find jobs but over the longer-term locked them in to these dead-end options and adversely affected their work ethic and expectations. Pinkster suggested that one possible explanation for these effects was that processes of social control limited residents' ability and willingness to interact with residents in the other groups and to look for opportunities outside of the neighbourhood (Pinkster, 2008).

Social Cohesion and Control

The importance of social control has been emphasized in a number of studies by Sampson and his colleagues (Sampson, 1992; Sampson & Groves, 1989; Sampson, Raudenbush & Earls, 1997; Morenoff, Sampson & Raudenbush 2001). To understand the effects of disadvantaged neighbourhoods, they argue, one must understand their degree of social organization, which entails the context of community norms, values and structures enveloping residents' behaviours (what he has labelled "collective efficacy"). Sampson's work has empirically demonstrated that disorder and lack of social cohesion are associated with greater incidence of mental distress and criminality in neighbourhoods (see the review in Sampson, Morenoff & Gannon-Rowley, 2002).

In this regard there is a good deal of trans-Atlantic commonality of findings related to crime outcomes. Hirshfield and Bowers (1997) identify a strong relationship between neighbourhood social control and assault and robbery in their study using Merseyside (England) data. Veysey and Messner (1999) examine British Crime Survey data and find that unsupervised peer groups and weak organizational participation in the neighbourhood was associated with greater victimization. Markowitz et al.'s (2001) analysis of British Crime Survey data showed that neighbourhood cohesion mediated some, though not all, of the neighbourhood socio-economic status effects on burglary.

There also has been suggestive work in both North American and Western European contexts demonstrating that social control and disorder potentially have effects on a wider array of outcomes. Aneshensel and Sucoff (1996) find that neighbourhood social cohesion explains a large portion of the relationship between neighbourhood socioeconomic status and adolescent depression. Kohen et al. (2002) find in Canada that neighbourhood disorder is negatively related and neighbourhood cohesion is positively related to children's verbal ability, and that neighbourhood cohesion (though not disorder) is negatively associated with child behavioural problems. Steptoe and Feldman (2001) surveyed London adults and found that the effect of neighbourhood socio-economic status on individual psychological distress was mediated by social cohesion and informal control. Blasius and Friedrichs (2004) also found in Koln (Germany) that collective efficacy was a valid construct that was correlated with several individual outcomes.

Finally, Galster and Santiago (2006) provide a unique perspective on the issue by asking low-income parents what they thought the main mechanisms of neighbourhood effects upon their children were. The dominant plurality (24%) cited lack of norms and collective efficacy. By contrast, peers (12%), exposure to violence (11%), and institutional resources (3%) were cited much less often. Of interest, one-third reported that their neighbourhoods had no effect either because their children were too young or that they were able to buffer the impacts.

Competition and Relative Deprivation

Though the U.S statistical evidence (already cited) overwhelmingly suggests that affluent residents convey positive externalities to their less-well of neighbours, there is at least one dissenting study: Ginther, Haveman and Wolfe's (2000) analysis of U.S. high school graduation probabilities and total years of education attained. For the white subsample (only) they found that a larger percentage of high-income neighbours was negatively related to graduation probabilities, while a larger percentage of low-income families was positively related to educational attainment. The qualitative evidence from the U.S. is less clear, with some case studies indicating that upper-income gentrifiers can sometimes mobilize and compete in ways that can work to the detriment of the original, lower-income residents; cf: Hyra (2008), Boyd (2008) and Freeman (2006). The importance of these effects is, of course, impossible to quantify from these qualitative works.

The statistical evidence on the effect of affluent neighbours on less-fortunate ones is decidedly more mixed in Europe, with a non-trivial literature indicating that effects are negative. A hint of a social conflict-type of neighbourhood mechanism is embodied in the finding by Sampson and Groves (1989) in Britain that neighbourhood ethnic heterogeneity was associated with more unsupervised peer groups and lack of participation in local organizations. McCulloch's (2001) analysis of British data finds that disadvantaged women are more likely to experi-

ence a variety of negative outcomes if they live in affluent areas, indicative of relative deprivation or competition mechanisms. This is consistent with two other British studies that found that health issues for poor individuals were more problematic when they lived in more affluent areas (Duncan & Jones, 1995; Shouls et al., 1996). Finally, Oberwittler (2007) observed that German adolescents living in households receiving welfare recipients scored substantially higher on an index of relative deprivation when they resided in neighbourhoods with the lowest overall welfare receipt rates. Finally, I note the Atkinson and Kintrea (2004) qualitative study of key informant opinions in Glasgow, in which some espoused the relative deprivation consequence of extreme social mixing within neighbourhoods.

It is less clear whether this potential relative deprivation effect in Europe extends to outcomes related to education. Kauppinen (2004) reports a strongly positive influence of affluent neighbours on educational achievement of individuals in Helsinki. Gibbons (2002) used the British National Child Development Study to examine the relationship between educational levels of neighbourhood experienced during adolescence and educational attainments by age 33. He found that, controlling for parental and school characteristics, the neighbourhood percentage of highly educated adults was strongly positively correlated with the probability that the children would be highly educated as young adults, and negatively correlated with the probability that they would fail to obtain any credentials, and that these relationships persisted similarly for various groups of children stratified by early childhood test scores. However, the marginal gains from more educated neighbours clearly attenuated within the highest quartile of neighbourhoods. Indeed, for children living in social housing the probability of not gaining any credentials was slightly *greater* in the most-educated quartile of neighbourhoods than in more modestly educated ones.

Parental Mediation

Few would argue that parents' mental and physical health, coping skills, sensed efficacy, irritability, parenting styles, and socio-psycho-economic resources loom large in how children develop. Thus, if any of the above elements are seriously affected by the neighbourhood (by whatever causal path), child outcomes are likely to be affected, though in this case the neighbourhood effect for children is indirect (Klebanov et al., 1997; Spencer, 2001). For example, as I will explore in the following section, certain neighbourhoods generate much higher exposures to stress-generating events for parents that, in turn, has been shown to adversely affect children (Elder et al., 1995; Linares et al., 2001). Such neighbourhoods may also vary, however, in their degrees of social support networks that might serve to defuse the negative effects of stress. As another example, parenting styles related to responsiveness/warmth and harshness/control have been observed to vary across aspects of neighbourhood disadvantage (Klebanov et al., 1994; Earls, McGuire & Shay, 1994; Simons et al., 1996; Briggs, 1997a). Such variations, in turn, have

been related to, among other outcomes, adolescent boys' psychological distress (Simons et al., 1996). Finally, riskier neighbourhoods have been linked to lower-quality home learning environments on many dimensions, resulting in lower reading abilities, verbal skills, and internalizing behaviour scores (Greenberg et al., 1999).

Evidence on Environmental Mechanisms of Neighbourhood Effects

In the U.S. it is clear that exposure to violence has reached epidemic proportions for low-income, minority youths (Martinez & Richter, 1993; Richter & Martinez, 1993; Aneshensel & Sucoff, 1996). The Yonkers (NY) Family and Community Survey and Moving To Opportunity demonstration have provided strong support for the perceived importance of this factor, since safety concerns were cited as a prime reason for participating in these programs by most public housing families (Briggs, 1997b; Goering & Feins, 2003). One of the most significant results of the Moving To Opportunity demonstration was the substantial reductions in stress and other psychological benefits accrued by parents and children who moved from dangerous, high-poverty neighbourhoods to safer ones (Katz, Kling & Liebman, 2000; Goering & Feins, 2003). Other work also has demonstrated that youths and adults who have been exposed to violence as witnesses or victims suffer increased stress and declines in mental health (Aneshensel & Sucoff, 1996; Martinez & Richter, 1993; Ceballos et al., 2001; Hagan et al., 2001). Exacerbated stress, in turn, can produce a variety of unhealthy stress-reduction behaviours such as smoking (Ganz, 2000) and over the long term can reduce the efficacy of the body's immune system (Geronimus, 1992). Exposure to violence has also been linked to higher risks of pregnancy (Linares et al., 2001), poorer pregnancy outcomes and low birth weight (Zapata et al., 1992), poorer educational outcomes (Hagan et al., 2001; Lord & Mahoney, 2007), more aggressive behaviours (Linares et al., 2001; Guerra, Huesmann & Spindler, 2003), and reduced social cognition (Guerra, Huesmann & Spindler, 2003), though some of these effects appear substantially mediated by the stress levels of parents (Linares et al., 2001).

Several aspects of the physical environment of the neighbourhood have been probed for their potential health impacts. A major proponent of the physical decay dimension is Ross et al. (2001), whose work suggests that prolonged exposure to a badly deteriorated environment weakens residents' sense of efficacy. A variant on this approach is the "broken windows" hypothesis in criminology, which suggests that physical symbols are strongly correlated with deviant and criminal behaviours in the neighbourhood (Kelling & Wilson, 1982). It is less clear whether it is the decay that creates an effect in its own right, however, or whether it merely serves as proxy for lack of collective efficacy. Clearer links to health have been identified for another physical aspect of the environment: noise

(Stansfeld, Haynes & Brown, 2000; Schell & Denham, 2003; Van Os, 2004). Others have argued that the physical design of neighbourhoods (presence of sidewalks, local land use mixes, cul-de-sacs, etc.) can affect the amount of exercise that residents get, which in turn affects obesity rates and other health outcomes (Lopez, Russell & Hynes, 2006), though the body of empirical evidence is small thus far. Results from the Moving To Opportunity demonstration found, however, that those moving from disadvantaged to low-poverty neighbourhoods had reduced rates of obesity, which supports the view that some (unspecified) physical feature(s) of the neighbourhood environment were at play (Goering & Feins, 2003).

As for toxic exposure to environmental pollutants, there is a large body of U.S.-based literature that documents a common pattern whereby lower-income and minority-occupied neighbourhoods are exposed to higher concentrations of air-, water-, and soil-borne pollutants (Anderton et al., 1994; Bullard, 1994; Hamilton, 1995; Vrijheid, 2000; Perlin, Wong & Sexton, 2001; Kawachi & Berkman, 2003; Ash & Fetter (2004), Litt, Tran & Burke, 2009; Saha, 2009). In turn, air pollutants have been linked in many international epidemiological studies to lower life expectancy, higher infant and adult mortality risks, more hospital visits, poorer birth outcomes, and asthma (McConnochie et al., 1999; Brunekreef & Holgate, 2002; Ritz, et al., 2002; Clancy et al., 2002; McConnell et al., 2002; Kawachi & Berkman, 2003; Chay & Greenstone, 2003; Neidell, 2004; Currie & Neidell, 2005; Brook, 2008; Hassing et al., 2009). Proximity to hazardous waste (“brown-field”) sites has been linked to higher rates of mortality from cancer and other diseases (Litt, Tran & Burke, 2009). Potential physiological mechanisms by which pollution can create health risks have been elucidated by Holguin (2008) and Mills et al., (2009). All of these studies can be challenged on one or more methodological grounds, however (Vrijheid, 2000). These include failure to control for many confounding personal factors, lack of precision in the local-area estimates of pollution concentrations, high sampling variability due to the small number of toxic waste sites, and potential selection bias where unobserved personal characteristics affect both their exposure to pollutants and their health outcomes. For fuller critical review, discussion and evaluation of this vast research literature on pollution and health, see Bernstein et al. (2004), Stillerman et al. (2008), Ren and Tong (2008), Chen, Goldberg, and Villeneuve (2008), and Clougherty et al. (2009).

The one area where the health effects of exposure to environmental toxins seem incontrovertible is in the realm of lead poisoning. It has been shown that even small amounts of lead poisoning (typically produced by residue from deteriorated lead-based paint formerly used in homes) can produce harms to infants (Reyes, 2005). Lead poisoning also harms the mental development, IQ, and behaviours of older children (Needleman & Gastsonis, 1991; Pocock et al., 1994).

Evidence on Geographical Mechanisms of Neighbourhood Effects

Numerous rigorous empirical studies have investigated the issue of racial differentials in accessibility to work (the “spatial mismatch” hypothesis) in the U.S. context (for reviews see: Kain, 1992). This literature generally suggests that mismatch can be an important aspect of spatial opportunity differentials in at least some metropolitan areas. Ethnographies (Sullivan, 1989; Newman, 1999) have shown that low-income youths can benefit greatly from part-time employment (by gaining resources, adult supervision, and routinized schedules), yet these neighbourhoods typically have few such jobs (Newman, 1999; Wilson, 1997). Evaluations of the Gautreaux program in Chicago showed that low-income black youths moving to the suburbs were more likely to hold jobs and earn more than their counterparts who stayed within the city (Rosenbaum, 1995). Nevertheless, there is considerable statistical evidence that this spatial mismatch is of less importance to economic outcomes than the social-interactive dimensions of neighbourhoods (Weinberg, Reagan & Yankow, 2004; Dawkins, Shen & Sanchez, 2005). Spatial mismatch typically is not seen as major issue in Europe, perhaps because of lower levels of ethnic and income segregation, less concentration of worksites, and more comprehensive public transportation systems (Gobillon, Selod & Zenou, 2007). Nevertheless, the few studies have come to divergent conclusions (cf. Dujardin & Gofette-Nagot, 2007; and Gobillon, Magnac & Selod, forthcoming).

By contrast, what little evidence exists on the mechanism of neighbourhood stigmatization tends to be idiosyncratic, qualitative, and (with one exception) hard to evaluate or quantify. Nevertheless, considerable case study evidence suggests that place-based stigmatization is an often occurring process in Western Europe. The work of Wacquant (1993), Power (1997), Taylor (1998), Atkinson and Kintrea (1998), Dean and Hastings (2000), Hastings and Dean (2003), and Martin and Watkinson (2003) is noteworthy. This body of work does not, of course, help us to quantify the degree to which neighbourhood stigmatization diminishes the life-chances of residents or restricts the various public or private resources or institutions flowing into these areas. To my knowledge, only one study has attempted statistically to relate measured perceptions of key actors about neighbourhoods to socioeconomic or demographic indicators measured in those places. Permentier, Bolt and van Ham (2007) asked households and real estate agents to evaluate on multiple grounds a variety of neighbourhoods in their city of Utrecht (NL) in which they did not live. They found that neighbourhood reputations were significantly correlated with their socio-economic characteristics, while their physical and functional features were of less importance. Unfortunately, these authors did not test for threshold points where the perceptions dramatically changed in response to neighbourhood social mix. Perhaps even more crucially, it is unclear the degree to which the reputation of a long-stigmatized neighbourhood can change as a consequence of more advantaged households being added to the

social mix (Cole et al., 1997; Pawson, Karryn & MacIntosh, 2000; Beekman et al., 2001).

Evidence on Institutional Mechanisms of Neighbourhood Effects

Many U.S.-based studies have documented the vast differences in both public and private institutional resources serving different neighbourhoods (e.g., Condrón & Roscigno, 2003; Lankford, Loeb & Wyckoff, 2002). Though there has been considerable debate on this subject, the current consensus seems to be that measurable educational resources are strongly correlated with several aspects of student performance in both the U.S. (Jargowsky & Komi, 2010) and the U.K. (Bramley, Evans, & Noble, 2005). Although the evidence linking these geographic differences to various outcomes for children has been subject to challenge (e.g., Morenoff et al., 2001, Popkin et al., 2002), there is increasing evidentiary prominence of some institutions, such as the public schools, serving as important mediators of neighbourhood context (Teitler & Weiss, 1996). Moreover, it is clear that many parents believe that a paucity of local resources can adversely affect their children (Galster & Santiago, 2006; Phibbs, 2009) and often try to compensate for this lack by seeking them from outside of their neighbourhoods (Jarrett, 1997).

There is also substantial evidence from the U.S. regarding the large spatial variations in many sorts of market actors whose proximity may affect health-related behaviours of neighbourhood residents. Several studies, for example, have documented distinctive race and class patterns in supermarket food store locations (Morland et al., 2002; Block, Scribner, & Disavow, 2004; Zenk et al., 2005) and others have done the same for dietary habits (Diez-Roux et al., 1999). As another illustration, in his study of Latino and Black youth moving from concentrated poverty neighbourhoods in Yonkers, Briggs (1997b) finds that they had much less access to liquor stores in their non-poverty destinations and that their reported alcohol usage was lowered. Quantifying a convincing causal link between such contextual variations and individual's diets and consumption patterns and, ultimately, health, has proven more challenging, however; see Gallagher (2006, 2007) and Morland et al. (2002).

In the Western European context the effect of institutional and public service differentials across space are probably less severe, given that these welfare states have a more centralized funding mechanism and often try to provide compensatory services to disadvantaged neighbourhoods (Powell & Boyne, 2001). However, Atkinson and Kintrea (2001), Buck (2001), and Hastings (2007, 2009b) offer several more subtle mechanisms about how such effects may be imparted nevertheless: (1) low expectations by residents of disadvantaged places create self-fulfilling prophecies; (2) inter-neighbourhood competition for scarce public services, skilled employees, and facilities; (3) "rationing" of public services in ways that are insufficient to equally meet needs in different locales; and (4) direct place-based discrimination by institutional actors controlling allocations of resources.

Hastings (2009a) provides a comprehensive conceptual framework of a variety of endogenous relationships of relevance here.

The Western European evidence on these points is suggestive but hardly definitive. Lupton (2004) finds that schools in disadvantaged UK districts have a more difficult time attracting highly qualified, experienced teachers. Some studies have found that teachers in disadvantaged UK neighbourhoods expect less from their students (Atkinson & Kintrea, 2001; Gilborn & Youdell, 2000). Hastings' (2009a) case studies of 12 UK neighbourhoods suggest qualitatively that environmental service provision fails to compensate for higher levels of need in certain neighbourhoods, thereby setting in motion a mutually-reinforcing downward spiral of reactions by residents and service providers alike.

A Provisional Synthesis Regarding Evidence on Neighbourhood Effect Mechanisms

What does the foregoing evidence suggest about the importance of various neighbourhood effect mechanisms in the U.S and Western Europe, when all is said and done? With the mandatory caveat that firm conclusions are elusive here given the state of scholarship and the complexity of the topic, my evaluation provisionally suggests the following.⁸

First, in both the U.S. and Western Europe high concentrations of poverty or socially disadvantaged households (which typically are heavily Hispanic- and especially black-occupied neighbourhoods in the U.S. and immigrant-occupied neighbourhoods in Western Europe) have been consistently empirically linked to weaker cohesion and structures of informal social controls in their neighbourhoods. This situation produces, in turn, negative consequences like increased youth delinquency, criminality, and mental distress, although this mechanism has not yet been linked to other important outcomes like labour market performance. However, in both U.S. and Western European research the aforementioned concentrations of poverty or disadvantage retain their relationship with a variety of child and adult outcomes even after intra-neighbourhood levels of social control and cohesion are taken into account. Clearly, more than this mechanism is at work.

Second, the fact that neighbourhood poverty rates in the U.S. appear consistently related to a range of outcomes in a non-linear, threshold-like fashion further suggests that the social contagion (peers) and/or the collective socialization (roles models, norms) forms of causal linkages are transpiring. There may also be some selectivity involved, as some socially weaker groups in the U.S. seem more

⁸ I recognize that practitioners who deal directly with deprived neighborhoods hold divergent and conflicting opinions about which neighborhood effect mechanisms are most important (Atkinson & Kintrea, 2004). The same can be said of low-income minority parents (Galster & Santiago, 2006).

vulnerable to these contexts than stronger ones. I do not believe that the evidence can clearly distinguish the respective contributions made by the latter two alternatives.⁹ Unfortunately, with highly inconsistent evidence regarding non-linearities of neighbourhood impacts in the Western European evidence, there is no certainty about the relative importance of such processes there.

Third, in the U.S. the presence of affluent neighbours appears to provide positive externalities to their less-well off neighbours, seemingly working social controls and collective socialization. Social networks and peer influences between the affluent and the poor, by contrast, do not appear as important in this vein. The outcomes for individuals that are most strongly related to affluent neighbours seem to be different than those most strongly related to disadvantaged neighbours. There is consistent U.S. empirical evidence to suggest thresholds here as well, though the precise threshold is unclear and likely varies by outcome being considered. The Western European evidence is much less definitive, and indeed inconsistent, in all these aforementioned regards. Finally, most U.S. and Western European evidence indicates that the influence on vulnerable individuals of advantaged neighbours is smaller in absolute value than the influence of disadvantaged neighbours, whatever the mechanism(s) at play.

Fourth, in U.S. neighbourhood contexts there is little evidence suggesting that the competition or relative deprivation mechanisms are operating in a meaningful way. The same cannot be said of Western European evidence, however, where the preponderance suggests that mixing of low- and high-income groups results in little benefit or even harms for those who are most disadvantaged.

Fifth, a large number of U.S. studies have consistently found that there is relatively little social networking between lower-income and higher-income households or children in the same neighbourhood, and this lack is compounded if there are also racial differences involved. Thus, there is little to support the version of neighbourhood effects that advantaged neighbours create valuable “weak ties” for disadvantaged ones. I could identify no Western European evidence on this point.

Sixth, local environmental differences appear substantial and likely produce important differentials in mental and physical and mental health on both sides of the Atlantic. There are huge differences in exposure to violence across U.S. neighbourhoods and this undoubtedly produces important and durable psychological consequences for children and adults that, in turn, likely have numerous but hard-to-quantify other effects. Exposure to environmental pollutants and (especially in the U.S.) to violence undoubtedly produces significant consequences for the health of children, youths and adults, though evidence on the links for many toxins besides lead is often sketchy. The longer-term consequences of these health impacts on subsequent educational outcomes, behaviours, and economic outcomes have not been adequately explored, however.

⁹ After their review, Leventhal and Brooks-Gunn (2000) similarly concluded that the strongest support seems to be for the combined role of norms, collective efficacy (informal social controls), and peers as major neighborhood influences on adolescent behaviors.

Seventh, geographic disparities related to differential accessibility to work and quality public services (especially education) are likely more severe in the U.S. than in Western Europe. At least in the U.S. context, these mechanisms likely play a non-trivial role in explaining labour force and educational outcomes.

Eighth, institutional processes involving place-based stigmatization, local institutional quality and behaviour, and local market actors likely exist but quantification of their spatial variations have not been accomplished in a way that permits generalizations in either the U.S. or Western Europe. Moreover, convincing statistical models of the relationship between measured variations in these potential causal mechanisms and a wide range of behavioural or health outcomes have not been completed.

Finally, there is probably a substantial, indirect effect on children and youth than transpires through the combined effects of the social-interactive, environmental, geographic, and institutional dimensions of the neighbourhood context on their parents. This mediation of neighbourhood effects through parents is likely to affect a broad range of outcomes for their offspring, though there have been no attempts to measure comprehensively such effects.

Implications for Scholarship and Policy

Advancing Scholarship on Neighbourhood Effect Mechanisms

I return once again to a theme that introduced this paper and that echoed throughout: given the complexity of the topic there is simply far too little scholarship to make many claims about which causal links dominate for which outcomes for which people in which national contexts. I recognize that calling for “additional research” is a shop-worn conclusion for an academic paper; nevertheless, it remains unusually valid and significant in this case.

How might such additional research proceed? Given that both qualitative and quantitative approaches have different inherent strengths and limitations here, I would argue for mixed-methods strategies, ideally embedded within the same study design so the same populations, local neighbourhoods and overarching contexts can be held constant. Given the likelihood that many causal mechanisms may act cumulatively and with some durability of impact, there is a need for studies that explore residential histories and patterns of exposure to a wide variety of community conditions, not just current exposure to a narrow palette of neighbourhood measures (Rauh, Andrews, & Garfinkel, 2001). Because there is such a wide range of potential mechanisms, quantitative studies should not satisfy themselves with easily accessed census indicators for neighbourhoods, but should strive to obtain: (1) administrative data about neighbourhood conditions (e.g., crime, low birth weight rates, child maltreatment rates); (2) data about local institutions, fa-

cilities, and schools (both their existence and quality); (3) pollution measures at a fine-grained spatial scale. In addition, I urge moving beyond distal proxies for causal processes and more efforts to collect social process data from community surveys and systematic social observations (Leventhal & Brooks-Gunn, 2000; Sampson, Morenoff & Gannon-Rowley, 2002). Finally, there is a need for datasets that measure the amount of time spent and routines of activity in the neighbourhood and the degree to which social interaction patterns are concentrated in the neighbourhood (South, 2001; Sampson, 2001). Of course, these studies must also collect detailed information about family circumstances to accurately develop controls or, possibly, measures of parental mediation of neighbourhood impacts on children.

Though these data requirements represent an intimidating menu, there is one emerging study that offers unprecedented breadth in this realm. My Wayne State University School of Social Work colleague, Professor Anna Santiago, and I are now completing final data cleaning procedures on information gathered from a natural experiment in Denver, CO. The research aims to quantify how and why a variety of outcomes (health, education, employment, behavioural and demographic) for low-income, Black and Latino children and youth residing in Denver public housing for a substantial period are statistically related to conditions in the neighbourhoods in which they were raised. Data analyzed come from surveys we conducted with 765 current and former residents of the Denver (CO) Housing Authority (DHA) whose 1,995 children met study eligibility criteria. For decades, DHA has operated public housing located units throughout the City and County of Denver. Because the initial allocation of households on the DHA waiting list to units mimics a random assignment to a wide range of neighbourhood environments, this program represents a natural experiment for overcoming parental location selection bias in estimating neighbourhood effects. We have gathered life histories for all participating children and their families, relating a wide range of outcomes to individual developmental stages. To this residential history we have merged time-coincident data from: (1) census tract indicators of socioeconomic, demographic and housing characteristics; (2) administrative data on crime, low birth weight rates, and school quality; (3) survey-based, parental-identified measures of local institutions and facilities; (4) survey-based, parental-assessed social disorder, collective efficacy, and social networks. We hope to soon add information on air quality and location of hazardous waste sites. In addition, we will return to the field in the future with follow-up interviews with children in our sample who have become young adults, to query them about their perceptions of neighbourhood effect mechanisms, parental buffering attempts at same, etc.

Implications for Public Policy

Obtaining a clearer understanding of the pathways through which neighbourhoods exert their effects is crucial for public policy formulations in at least three

major domains: health, employment, and housing. Put bluntly, it is risky for policy-makers to naively observe a correlation between neighbourhood indicators and individual outcomes of interest and design programmatic strategies as if neighbourhood were a “black box.” At best, inefficiencies and, at worse, negative unforeseen consequences, are all-too-likely to follow in these circumstances.

In the health domain, it is obvious from an epidemiological perspective that understanding causal pathways is of “critical importance in determining how [public health] interventions should be designed” (Sellström & Bremberg, 2006:553). In the employment domain, distinctive programmatic implications follow from alternative conclusions about why some able-bodied are not employed. Perhaps they: (1) lack information networks about job opportunities; (2) try to apply for work but are turned away by employers who stigmatize their neighbourhoods; (3) try to find work but cannot access jobs due to local transport inadequacies; (4) do not try to work because of negative neighbourhood peer influences; or (5) are too sick to work because of severe local pollution levels. In the housing domain, the current Western European fascination with “social mix” strategies (Galster, 2007a, 2007b) could be helpfully guided by definitive explorations about what processes are thought to follow from social mix: social-interactive, geographic, and/or institutional? (Joseph, Chaskin, & Webber, 2006; Joseph, 2006) If it were to prove the case that, for instance, social networks among the various neighbouring classes were the dominant mechanism of positive influence, urban design strategies designed to maximize interpersonal contacts and “community-building” activities within the mixed estates would be recommended. On the other hand, if mixing served only to remove the former external stigmatization of residents, such micro-level social processes could well be ignored by policy-makers. Finally, there are some implications that overarch particular policy domains. For example, if it were to prove the case that the vast portion of neighbourhood impacts on children occurred indirectly through mediation of parents, then it would follow that interventions designed to minimize negative neighbourhood effects should target parents, even if the ultimate goal is child development.

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Appendix 2.

Neighborhood Effects on Latino and Black Youths' High School Dropout Behavior: Evidence from a Natural Experiment in Denver

Jackie M. Cutsinger, PhD*

George C. Galster, PhD**

Anna M. Santiago, PhD***

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*Center for Urban Studies, Wayne State University; **Department of Urban Studies and Planning, Wayne State University; ***Mandel School of Applied Social Sciences, Case Western Reserve University

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ABSTRACT

We quantify the relationships between unusually comprehensive measures of neighborhood context and the high school dropout rates of low-income Black and Latino youth, employing data from a natural experiment involving scattered-site public housing in Denver, CO to minimize geographic selection bias. We find that an index of perceived neighborhood problems, occupational prestige and proportion foreign-born strongly predict dropping out for the full sample, though the particulars are contextualized by gender and ethnicity. Further, neighborhood effects appear stronger when measured concurrently with high school. While neighborhood poverty rate generally was a weak, incomplete measure of neighborhood context, sustained exposure to low-poverty environments provided gains to Black youth, suggesting that results from the MTO demonstration are not generalizable.

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Improving the educational outcomes for low-income, minority youth has been a longstanding goal of U.S. educational policy. Though the importance of home and school environments in shaping educational outcomes is undeniable, increasing social scientific attention has been devoted to investigating the degree to which neighborhood context also exerts a substantial, independent influence. Unfortunately, consensual conclusions have yet to emerge from this literature due to hotly contested methodological debates; for recent reviews, see Galster (2008), and Harding et al (2011). The two core challenges are that (1) typically there is” little variation in the types of schools that low-income minority children attend...therefore we do not often get the chance to observe how a more advantaged environment might affect their life ;” and (2) “families choose neighborhoods...they are not randomly distributed across social settings” (DeLuca and Dayton 2009: 458).

We aim to overcome these challenges by leveraging a natural experiment involving the Denver (CO) Housing Authority (DHA), which since 1969 has operated public housing units located in a wide range of neighborhoods throughout the City and County of Denver. Because the initial assignment of households on the DHA waiting list to dwellings (and, thus, to neighborhoods) mimics a random process, this program represents an unusual opportunity for reducing parental location selection bias and observing the unusual combination of poor youths raised in good neighborhoods.

In this study we analyze data from administrative sources and data we have collected from telephone surveys with Black or Latino current and former

DHA tenants whose children were the appropriate ages when they lived in DHA housing. Our surveys provide retrospective information on a battery of youth outcomes and family characteristics. We focus in this paper on high school dropout rates.

Our primary research question involves identifying the magnitude of context effects as operationalized by several objective and subjective neighborhood indicators:

For Black and Latino youth age 15 and older who spent some of their childhood living in DHA housing, are there statistically and economically significant differences in their odds of dropping out of high school attributable to differences in their concurrent or lagged neighborhood environments, all else equal?

From the perspective of educational policy, we are investigating the degree to which an assisted housing policy can produce sufficient changes in the developmental context of low-income minority youth to yield substantial educational payoffs.

Our work advances this literature in four ways. First, because parents of our sampled children were quasi-randomly assigned to neighborhoods, the challenge of parental geographic selection bias is overcome. Second, we evaluate an unprecedented variety of measures of neighborhood environment, both objective and subjective, measured at different spatial scales. Third, we are one of the few studies to examine neighborhood impacts on the educational

outcomes of Latino youth. Fourth, we probe the timing of neighborhood effects by examining concurrent and lagged exposures.

The rest of our paper is organized as follows. The first section provides the theoretical bases for how neighborhoods might affect youths' educational outcomes. The second section discusses methodological challenges confronting those who would estimate the magnitude of neighborhood effects—especially the salient issue of parental geographic selection bias—and how prior research has tried to meet this challenge. The third section describes our natural experiment, study sample, survey, and neighborhood indicators forming the basis of our empirical work. The fourth section presents our analytical approach. The fifth section reports our findings and our explorations involving stratifications and alternative neighborhood indicators. The sixth section discusses implications of our results for measurement, causal mechanisms, heterogeneity of neighborhood effects, and education policy. The last section concludes and provides suggestions for future research.

How Neighborhoods Might Affect Youth Educational Outcomes

Our theoretical framework for studying links between neighborhood contexts and children and youths' outcomes draws most heavily from accepted ecological models of human development, which emphasize the need to examine developmental context. This perspective sees children's development being shaped by the proximal (e.g., family) as well as distal (e.g., neighborhood) contexts in which children live and interact (e.g., Bronfenbrenner and Morris,

1998). Neighborhood context may affect children through a variety of causal mechanisms that can occur either through social, institutional, or biological processes; for extended discussions see Jencks and Mayer (1990), Duncan, Connell and Klebanov (1997), Gephart (1997), Sampson (2001), Dietz (2002), Sampson, Morenoff, and Gannon-Rowley (2002), Odis (2007), Harding et al. (2011) and Galster (2011). The potential mechanisms relevant for educational outcomes include: peer influences, socialization and social control, violence and social disorder, institutional resources, accessibility, stigmatization, environmental health, and parental mediation. Because these mechanisms are well-known, we describe them only briefly:

Peer Influences: Youth may develop and modify attitudes, values, behaviors and expectations about school as a result of interactions with neighborhood peers (Anderson, 1990; Case and Katz, 1991). These peer effects may be transmitted among youth in a contagion-like fashion (Crane, 1991; South, Baumer and Lutz, 2003).

Socialization and Social Control: Youths' attitudes, values, behaviors and expectations about school may be shaped by neighborhood adult role models and norms enforced by the community or local culture (Wilson, 1987; Klebanov, Brooks-Gunn and Duncan, 1994; Connell and Halpern-Felsher, 1997).

Violence and Social Disorder: Exposure to neighborhood violence may lead to adverse physical responses (like ill health from stress), psychological responses (like post-traumatic stress syndrome) and inhibitions to speech

communication, all of which impede school performance (Massey, 2001; Sampson, Sharkey and Raudenbush, 2008).

Institutional Resources: Public and private institutions controlling services and facilities (especially schools, after-school tutoring, etc.) vary in their quantity and quality on the basis of neighbourhood context, thereby differentially affecting youths' perceptions of the value of educational attainment (Furstenburg and Hughes, 1997; Entwisle et al. 1997). Neighborhoods may also affect the socioeconomic and behavioral composition of local schools to the extent that they determine attendance zones, thereby shaping the peer influences to which teens will be exposed in the classroom (Feinstein and Symons, 1999; Hoxby, 2001; Lavy, Silva, and Weinhardt, 2009).

Accessibility: Neighborhoods offer different access to employment information and work sites themselves, due both to geographical proximity and public transportation disparities. This may lead youth from areas with poor job access to undervalue educational attainment since they see little prospective value from it (Anderson, 1999).

Stigmatization: Prospective employers may negatively evaluate job applicants from certain locales based on the bad reputation of the place. This, perhaps in combination with accessibility, may lead youth from these areas to undervalue educational attainment (Bauder, 2001).

Environment and Health: Neighborhood-based variations in exposure to ambient noise, toxins, lead, or other pollutants can affect mental and behavioral development and the severity of school absences due to asthma and other

diseases, thereby affecting student achievement (Aneshensel and Sucoff, 1996; Acevedo-Garcia et al. 2003).

Parental Mediation: Variants of the foregoing neighborhood mechanisms may also affect the physical and mental health, attitudes, behaviors, and resources of parents. These indirect neighborhood effects may be transmitted to children inasmuch as they affect the parents' willingness and ability to assist, monitor, and enrich their children's educational experiences (Connell and Halpern-Felsher, 1997; Furstenberg et al, 1999; Leventhal and Brooks-Gunn, 2000; Oh, 2005) or deter other behaviors that interfere with their educational achievement (Bellair and Roscigno, 2000).

Though there is a smattering of evidence to support several of the neighborhood effect mechanisms above, there is no consensus on which mechanism(s) may dominate for educational outcomes. Indeed, this remains a critical realm of future research (Harding et al. 2011; Galster, 2011).

Measuring the Independent, Causal Effect of Neighborhoods:

Challenges and Responses in the Empirical Literature

An educational outcome of interest (O) observed at time t for individual youth i residing in neighborhood j in a particular metropolitan area can be expressed:

$$O_{it} = \alpha + \beta[C_{it}] + \gamma[C_i] + \phi[UC_{it}] + \theta[UC_i] + \delta[P_{it}] + \zeta[P_i] + \lambda[UP_{it}] + \psi[UP_i] + \theta[N_{jt}] + \varnothing[UN_{jt}] + \varepsilon \quad [1]$$

where:[C_t] = observed characteristics of youth that can vary over time

(e.g., past trauma, number of siblings in the home)

[C] = observed characteristics of youth that do not vary over time

(e.g., race, year and country of birth)

[UC_t] = unobserved characteristics of youth that can vary over time

(e.g., psychological states, interpersonal relationships)

[UC] = unobserved characteristics of youth that do not vary over time

(e.g., genetic makeup, pre-natal experiences)

[P_t] = observed characteristics of youth's parent(s) that can vary over time

(e.g., marital status, income)

[P] = observed characteristics of youth's parent(s) that do not vary over time

(e.g., race, year and country of birth)

[UP_t] = unobserved characteristics of youth's parent(s) that can vary over time

(e.g., psychological states, interpersonal relationships, self-efficacy)

[UP] = unobserved characteristics of youth's parent(s) that do not vary over time

(e.g., genetic makeup, childhood experiences)

[N_t] = observed characteristics of neighborhood where youth resides

during t, and perhaps during prior periods (e.g., poverty rate)

[UN_{j,t}] = unobserved characteristics of neighborhood where youth resides

during t, and perhaps during prior periods (e.g., educational norms)

ε = a random error term with statistical properties discussed below

i = individual youth; j = neighborhood; t = time period

All Greek letters represent parameters to be estimated.

The central empirical challenges facing analysts attempting to measure neighborhood effects accurately (i.e., get a precise, unbiased measure of θ) can be seen through the framework provided by equation [1]. What is the appropriate geographic scale(s) that defines [N]? What are the appropriate characteristics to measure when operationalizing [N], and does this depend on the educational outcome in question? What is the intensity and duration of youth i 's exposure to [N]? Does [N] affect O immediately, with a lag, or cumulatively? Does this depend on the educational outcome in question? Do the above answers depend on the developmental stage, gender, and/or ethnicity of the youth? How can we comprehensively operationalize and measure the key components of [C], [C_{*t*}], [P], and [P_{*t*}]? Given that we cannot, by definition, operationalize and measure [UC], [UC_{*t*}], [UP], and [UP_{*t*}], what can be done to minimize bias in estimated θ from these omitted variables that may be strongly associated with parents' neighborhood selection process?

No studies have systematically tested and answered all of these questions satisfactorily; for a fuller discussion see Galster (2008, 2012). However, a large body of multivariate statistical studies on the relationship between neighborhood context and individual children's educational outcomes has provided a wide variety of alternative answers to the first five questions, though their lack of

comparability on multiple dimensions renders a consensus non-existent.¹ Most of the U.S.-based work has used census tracts as the scale of neighborhood, though some work used zip codes (e.g., Datcher 1982; South, Baumer and Lutz, 2003). It has been universal to use indicators of neighborhood socio-economic status (rates of poverty, female headship, welfare receipt, educational and occupational attainment, ethnic composition) as proxies for the underlying social processes; no studies have operationalized the violence, health, institutional accessibility or stigmatization mechanisms.² Most of this work has focused on the experience of neighborhood context as a child or adolescent providing a roughly contemporaneous educational consequence, though Aaronson (1998), Galster et al. (2007) and Wodtke, Harding and Elwert (2011) model cumulative contexts, and Kauppinen (2007) and Sampson, Sharkey and Raudenbush (2008) model both contemporaneous and lagged contexts. Several studies have confirmed that there were substantially different (although inconsistent) neighborhood context relationships with educational outcomes depending on the gender and race of the youths, though Latino ethnicity has not been probed distinctly (Crane, 1991; Duncan, 1994; Connell et al. 1995; Ensminger, Lamkin and Jacobson, 1996; Aaronson, 1998; Vartanian and Gleason, 1999; South, Baumer, and Lutz, 2003; Goering and Feins (2003); Ceballo, McLoyd and Toyokawa, 2004; Jargowsky and El Komi, 2011). Many studies have employed surveys that permit the statistical control of a wide range of parental and youth

¹ We do not consider here several studies that examine city-wide or metropolitan-wide interracial differences in educational outcomes as a function of racial segregation in the schools or neighborhoods. For a recent example, see Card and Rothstein (2007).

² To our knowledge, the only other educational effects study to employ subjective assessments of the neighborhood was by Ceballo, McLoyd and Toyokawa (2004).

characteristics, though no survey can avoid the problem of unobservables, of course. The vast majority of U.S.-based studies in this field³ have reported nontrivial partial correlations between various measures of the socioeconomic composition of neighborhood residents and several measures of educational performance (typically high school dropout rates, less often standardized test scores, school values and effort, or college enrollment rates) for one or more gender-race groups of youth; cf. Datcher (1982), Corcoran et al. (1990), Crane (1991), Garner and Raudenbush (1991), Clark (1992), Duncan (1994), Duncan, Brooks-Gunn, and Klebanov 1994, Connell et al. (1995), Chase-Lansdale and Gordon 1996; Ensminger, Lamkin and Jacobson (1996), Chase-Lansdale et al. 1997, Aaronson (1998), Vartanian and Gleason (1999), Kohen et al. (2002), Ainsworth (2002), South, Baumer and Lutz (2003), Harding (2003), Ceballo, McLoyd and Toyokawa (2004), Galster et al. (2007), Sampson, Sharkey and Raudenbush (2008), and Jargowsky and El Komi (2011).

The causal interpretation of the neighbourhood-educational outcome relationships measured by many of these studies is subject to challenge, however, due to potential geographic selection bias. The basic issue is that parents may have certain (unmeasured) behaviors, values, and skills related to their children's education and move to certain types of neighborhoods as a consequence of these same attributes. Any observed statistical relationship between their children's neighborhood conditions and educational outcomes may therefore be biased because of this systematic spatial selection process, *even if*

³ For an exception, see Plotnick and Hoffman (1995). The European-based evidence is considerably more mixed in conclusions; cf. Kauppinen (2007) and Nicoletti and Rabe (2010).

all the parents' and children's observable characteristics are controlled (Manski 1995, 2000; Duncan et al. 1997; Duncan and Raudenbush 1999, Dietz 2002).⁴

There have been several types of methodological responses to this selection / omitted unobserved variables challenge in the education-neighborhood effects literature:

Propensity Score Matching: Outcomes for those receiving some neighborhood “treatment” are compared to those for a synthetic sample of non-treated matched via a probability model of their similar likelihoods of being treated (Harding, 2003)

Sibling Models Based on Longitudinal Data: The biases from unobserved, time-invariant parental characteristics are eliminated by measuring differences in educational outcomes and neighborhood experiences between two siblings (e.g., Aaronson, 1998).

Fixed-Effect Models Based on Longitudinal Data: Unobserved, time-invariant characteristics of individuals that may affect both neighborhood selection and educational outcomes are measured by dummy variables (e.g., Jargowsky and El Komi, 2011).

Instrumental Variables for Neighborhood Characteristics: Proxy variables for neighborhood characteristics are devised that only vary according to attributes

⁴ [UC] and [UC_i] may also be correlated with [N] and thereby produce bias if, for example, parents who observe certain characteristics in their children make particular residential choices as a result, either to exploit or to minimize the consequences from these characteristics. Fundamentally it does not matter if the unobserved characteristics affecting O in [1] originate with the parent or the youth; if they are correlated with [N] a bias will result. The direction of the bias has been the subject of debate, with Jencks and Mayer (1990) and Tienda (1991) arguing that measures of neighborhood impacts are biased upwards, and Brooks-Gunn, Duncan, and Aber (1997) arguing the opposite. Gennetian, Ludwig, and Sanbonmatsu (2011) show that these biases can be substantial enough to seriously distort conclusions about the magnitude and direction of neighborhood effects.

exogenous to the individual youth and their parents (e.g., Duncan, Connell, and Klebanov, 1997; Galster et al. 2007).

Natural Quasi-Experiments: Observations are produced by idiosyncratic interventions, such as the Gautreaux and Yonkers public housing desegregation programs (Rosenbaum, 1995; DeLuca et al. 2010), public housing revitalization programs (Jacob, 2004; Clampet-Lundquist, 2007) or inclusionary zoning (Schwartz, 2010) that create exogenous variation in neighborhood environments for assisted tenants.⁵

Random Assignment Experiments: Data are produced by an experimental design whereby households are randomly assigned to different neighborhoods, such as the Moving To Opportunity (MTO) demonstration (Ludwig, Ladd and Duncan, 2001;; Kling, Liebman and Katz, 2007; Ludwig et al. 2008).

Unfortunately, there is no consensus about the unbiased magnitude of neighborhood effects on educational outcomes (Burdick-Will et al., 2010). The studies noted above that use the first four, non-experimental methods for overcoming selection bias find strong neighborhood effects on educational outcomes, with one exception.⁶ By contrast, the natural quasi-experiments provide mixed results; cf. Rosenbaum (1995), Jacobs (2004), Fauth, Leventhal, and Brooks-Gunn (2007), DeLuca et al. (2010) and Schwartz (2010). The only random-assignment experiment in the field suggests even smaller neighborhood effects. The initial gains in math and reading scores observed in the Baltimore

⁵ Often there is still room for some geographic selection by participants in these programs, however.

⁶ Jacob (2004) found no improvement in children's test scores when their families used rental vouchers to move from public housing being demolished in Chicago. However, Jacob also finds that there were little differences in the children's neighborhood environments or school systems pre- and post-move, so this hardly represents a robust study of neighborhood effects.

MTO site from youth whose families moved to low-poverty neighborhoods (Ludwig, Ladd and Duncan, 2001) seem to have dissipated after four years when all study sites were analyzed (Sanbonmatsu et al. 2006).⁷ However, MTO's findings have been challenged for many reasons (Sanbonmatsu et al. 2006; Clampet-Lundquist and Massey, 2008; Sampson, 2008; Briggs, Ferryman, Popkin, and Rendon; 2008, DeLuca and Dayton, 2009; Burdick-Will et al., 2010; Briggs, Popkin and Goering, 2010).

Thus, the existing literature has not provided definitive evidence about the potential benefits to low-income, Black and Latino children from sustained residence in neighborhoods that are advantaged on multiple dimensions. Our study hopes to contribute to a resolution of this empirical issue by leveraging a natural experiment related to the Housing Authority of the City and County of Denver (DHA).

The Natural Experiment Involving Public Housing in Denver

In addition to its large-scale, conventional public housing developments, DHA has operated since 1969 a program providing approximately 1,500 low-income families with opportunities to live in scattered-site, single-family and small-scale, multi-family units. These units are located in a wide range of neighborhoods throughout the congruent City and County of Denver, whereas the conventional developments are typically located in less-advantaged neighborhoods.

⁷ A few modest gains in reading scores persisted (Kling, Liebman and Katz, 2007).

From a research methodology standpoint, the most important feature of the DHA case is that, from 1987 onwards, applicants for all of their public housing programs were screened using the same evaluation standards and assigned to vacant housing corresponding to their family characteristics. We carefully reviewed this assignment process with DHA officials during several interviews prior and subsequent to data collection for the study. As applicants (who meet certain basic eligibility criteria) come to the top of the waiting list they are offered a vacant DHA unit (in either conventional or scattered-site programs) with a number of bedrooms appropriate for their family size and gender of children. If they do not accept this unit they are offered the next similarly sized unit that becomes available (typically after a nontrivial wait). If applicants do not accept this second unit they drop to the bottom of the queue, creating a wait of a year or more. DHA officials told us that relatively few applicants choose to reject both units offered.⁸ The initial assignment of households to a DHA unit (and, thus, neighborhood) therefore appears on its face to be a quasi-experimental design that mimics random assignment of household to neighborhood.

We tested this proposition in two ways. First, we explored the possibility of selection arising because the tenant can potentially choose between two DHA units that may be located in quite different neighborhoods. Our analyses indicated to the contrary that the vast majority of our survey respondents (74.2 percent) either expressed no geographic preferences for their DHA assignment or their stated preferences were not honored. Second, we

⁸ Our independent evaluation of DHA records showed that 88.3% of the applicants accepted their first or second offers; 7.9% ended up rejecting both offers and taking a third offer; 3.8% rejected three or more offers. Further, 75.5% of all applicants ended up in the neighborhood originally offered.

tested the DHA assignment process by analyzing the resulting matches of DHA client characteristics and neighborhood characteristics. Through Monte Carlo simulations we determined that the observed correlations between neighborhood characteristics and household characteristics that are typically unobserved in most databases (but revealed in our survey) were not significantly different from what would have been expected by chance. *[technical details of these tests are provided in a separate document for reviewers]* We thus have confidence that the natural experiment employed in our study indeed does remove the correlation between unobservable parental characteristics that potentially affect both initial DHA location and subsequent child outcomes.

However, the longer time passes since initial assignment two potential sources of geographic selection based on parental unobservables arise. First, DHA households can voluntarily transfer between dispersed housing and conventional public housing developments. This occurred rarely, however. Of the post-1986 vintage tenants now residing in conventional public housing developments, 99 percent were originally placed in such; only one percent moved in from dispersed housing. Of the post-1986 vintage tenants now residing in dispersed housing, 94 percent were originally placed in such; six percent moved in from the conventional developments.⁹

Second, a larger source of selection arises because a substantial part of our survey involved households no longer residing in DHA housing. Although their initial assignment to a neighborhood by DHA was effectively random, their

⁹ Moreover, an unknown number of these transfers were involuntary, required by regulations after changes in family size or composition.

subsequent move(s) out of DHA were likely not. In these cases, the children's cumulative contextual exposures will be a combination of randomly assigned and selectively (to some degree) assigned neighborhood characteristics (analogous to experimental households in MTO). To the extent that the former contexts are sufficient to rupture the correlation between unobservable parental characteristics affecting high school outcomes and neighborhood characteristics experienced by their children, our estimates of neighborhood effects will not be substantially biased. To investigate this assumption, we will replicate our analyses using only those youth who spent a majority of their high school years residing in DHA housing, i.e., a randomly assigned neighborhood. This replication is not without potential bias, however, since there may be selection on other unobservable characteristics that leads certain families to remain in their DHA dwelling for an extended period.

A further important feature of our natural experiment is the comparatively long exposures children in DHA households had to their assigned neighborhoods. Our sample had a 6-year mean (5 median) DHA residential duration, approximately twice as long as reported for the MTO experimental group (mean = 2.7 years; median = 3.3 years). Recent work by Wodtke et al. (2011) and Crowder and South (2011) stress the importance of taking into account the length of time children are exposed to particular neighborhood contexts, lest we underestimate the true effects that neighborhoods have on child outcomes.

The use of natural experiments inevitably raises questions about generality of results. We believe that our findings can fairly be generalized to low-income, Black and Latino families who apply for and remain on the waiting list long enough to obtain public housing. As such, it may not be fully generalizable to the population of minority families who obtain subsidized rental housing, and certainly may not be to the larger population of minority families who qualify for housing assistance. Nevertheless, it is similar to--yet considerably more general than--the populations forming the samples for the various Gautreaux-, Yonkers-, and MTO-based scholarly studies noted above.

Data Collection in Denver

Denver Child Study Survey of Current and Former DHA Households

We developed and fielded during 2006-2008 the Denver Child Study, a telephone survey (sometimes done in person for those who had no phones) that collected retrospective and current information about the household, adults and children. Detailed information related to multiple domains of outcomes was gathered for all eligible children associated with each household. Each household's residential mobility history was obtained so it could be associated with neighborhood developmental context for children. Study eligibility criteria were: (1) presence of children in the home between ages 0 and 18 years when they moved into DHA; (2) family remained in DHA housing for at least two years; and (3) family first entered DHA in 1987 or later (when DHA's current quasi-random assignment process came into operation); and (4) Black or Latino

ethnicity identified.

Attempts to recruit subjects for the study were made by mail and phone, in both English and Spanish when appropriate. Compensation for participation took the form of either cash or gift card. We estimate a participation rate of 56.5 percent, with most non-participation due to our inability to locate the household; less than 6 percent refused to participate once contacted. Our team successfully completed 809 interviews with the parents or primary caregivers of eligible households, though 736 were retained for analysis after eligibility verification. Details of sampling, participation rates, and profiles of eligible and participating households are available from the authors.

Youth analyzed as part of this study were (current or past) members of these 736 households who were age 15 or older and had attended at least one year of high school by the time of our survey (N= 668 with information on all variables used in multivariate analyses). As discussed above, we replicated analyses for a subsample of this group who had spent a majority of their high school years residing in DHA housing (N=514 with complete information).¹⁰

Our Denver Child Study survey collected information on a wide variety of parental/caregiver characteristics that we employed as controls; these are listed in Table 1. This included information about national origin, substance abuse history, education, marital status, and economic status. The survey also asked parents/caregivers to assess the strength and type of influence that various relatives and others had on the youth (these variables are scaled from -2 to 2,

¹⁰ 19 cases were excluded from the full sample and 13 cases were excluded in the DHA-in-High-School sample because they were identified as multivariate outliers using the Mahalanobis distance technique.

with -2 indicating very negative and 2 indicating very positive). The survey also asked questions that permitted us to compute scales of parental/caregiver depression (CES-D)¹¹, traditional parenting beliefs, and parenting efficacy; details are available from the authors.

The Denver Child Study survey asked parents/caregivers to supply information about all their children with whom they had lived in DHA public housing for at least one year. In this manner we collected detailed information about children's residential histories (including bouts of homelessness and living outside of the parental home), health, exposure to violence, behaviors and activities, marital/fertility histories, employment, and education. By merging information across children in a household we are able to discern numbers and characteristics of the given high school student's siblings. The youth characteristics we analyzed as control variables and their high school outcomes are listed in Table 1. These include demographic characteristics such as gender and race/ethnicity, the number of younger siblings present in the household while the child of interest was in high school, whether any siblings were in special education during that time, and whether any older siblings had dropped out of high school.

In this study we focus on dropping out of high school. Dropping out was defined as the parent/caregiver survey respondent saying the youth "was no longer in school because s/he had dropped out before getting a diploma." Nearly

¹¹ We operationalized two dummy variables indicating two different levels of parental depression at the time the survey was administered: clinical depression (score of 27 or higher on the CES-D scale) and borderline clinical depression (score between 16 and 26 on the CES-D).

20 percent of our high-school aged sample dropped out before graduating.¹²

A shortcoming of our survey was that it gathered limited information about children's schools. For this study we have information about the type and number of high school(s) attended and whether it was located in the student's neighborhood. During the period of our study, the Denver Public Schools operated an open enrollment system, permitting students greater latitude to enroll in schools of their choice that may not have been associated with the catchment area of their neighborhood.¹³ This has both advantages and disadvantages for our study. The disadvantage is that our inability to control for numerous characteristics of the high school attended clearly attenuates the explanatory power of our models and raises concerns about potential omitted variables bias (Jargowsky and Komi, 2011; Sykes and Musterd, 2011). The advantage is that open enrollment likely reduces the correlations between these omitted school characteristic variables and our included neighborhood indicator variables, thereby attenuating the severity of these potential omitted variables biases.

Collection of Neighborhood Indicators

We obtained a wide variety of neighborhood data from three sources. The first was the decennial U.S. Census, where we used census tract geographic scales from 1970, 1980, 1990 and 2000 censuses. We employed the *Neighborhood Change Data Base* (a Geolytics proprietary product) for this

¹² We counted youths as dropping out even if later they may have obtained a GED. School leaving may have occurred before high school.

¹³ Even though open enrollment was allowed, only about one-fifth of the children in our sample attended schools that were outside out the neighborhoods in which they resided.

information because it adjusts figures to account for potential changes in tract boundaries between decennial censuses. For estimates of non-census year data, we used linear interpolation or extrapolation. We gathered indicators that have been widely employed in prior research on neighborhood effects, including proportions of: female-headed households, poor families, unemployed adults, those with only elementary school education, those with college degrees, non-Hispanic black population, Hispanic population, foreign-born population, housing vacancy rate, homes built prior to 1939, homes that are owner-occupied, and an index of occupational prestige based on the observed distribution of occupations of employees.¹⁴

The second source was the Denver-based Piton Foundation's *Neighborhood Facts Database*, which provided small area-based, annually measured information culled from Denver administrative databases that are not provided by the Census.¹⁵ We employed rates of violent and property crimes per 1,000 population, confirmed cases of child abuse and neglect per 1,000 children, and number of licensed day care slots per 1,000 children. The Piton Foundation data are aggregated to 77 named areas consisting of two census tracts, on average, and thus are measured at a larger spatial scale than our census-based data. Moreover, most Piton series go back no earlier than 1990 and are available only for the City and County of Denver, which produced some shrinkage in our analysis sample.

¹⁴ The prestige index weights the proportions of each occupational group by its corresponding General Social Survey cardinal prestige score. A neighborhood with all professional and technical workers would score 60.5; one with all laborers would score 17.

¹⁵ The authors thank Terri Bailey and Matt Berry of the Piton Foundation for providing the raw data to us for this analysis.

The third source was subjective indicators based on responses of the households interviewed in our Denver Child Study. For each neighborhood in which they lived while they were raising children, we asked the parent/caregiver to respond to a battery of questions related to the location's assets and liabilities. From the responses we devised two indices of neighborhood social capital and social problems, a dichotomous measure of bad peer influences, and three dichotomous measures of whether their children had used parks / playgrounds, mentoring / counseling centers, and indoor recreation facilities located there. The social capital index (range from 0-6) was incremented by "one" for each of the following respondent descriptions of people in the neighborhood: could get together to solve neighborhood problems; would watch out for their children and property; knew them and their children by name; they and their children could look up to them; or could be counted on in times of trouble, and whether the respondent participated in any organizations located in the neighborhood (e.g., block clubs, tenant groups, religious organizations and the like). The neighborhood social problems index (range 0-5) was incremented by "one" for each of the following conditions: people selling drugs; gang activity; homes broken into by burglars; people being robbed or mugged; people getting beaten or raped. Both indices proved reliable; details are available from the authors.

Creation of Analytical Databases

We spent considerable effort cleaning, reconciling and augmenting the survey data. When our audits revealed inconsistencies or omissions in the responses, we attempted to contact respondents again and seek clarifications.

Information provided by respondents on their residential histories was cross-checked with residential location information contained in the DHA administrative databases and Lexis-Nexis files.

Once residential history information obtained on the survey was verified for accuracy, we geo-coded each address, using the U.S. Bureau of the Census' *American FactFinder* website utility. In cases where respondents could not recall specific addresses but only proximate cross-streets, we verified these locations using MapQuest and then identified the corresponding census tract using the aforementioned Census website showing tract boundaries. This procedure provided the census tract corresponding to each location in respondents' residential histories, which, in turn, permitted us to match each location to the aforementioned battery of neighborhood indicators for census tract neighborhoods. We were able to successfully link 92 percent of the residential locations listed by respondents.

All these data for households and neighborhoods were then transformed into the format of a *child-year unit of observation*. For each child-year there are variables associated with: (1) fixed child characteristics [C], (2) fixed parental characteristics [P]; (3) temporally varying child characteristics [C_t]; (4) temporally varying parental-household characteristics [P_t]; (5) temporally varying neighborhood characteristics [N_t]; (6) temporally varying outcomes [O_t]. We aggregated information from child-years 15-18 into a "high school stage" unit of analysis for use in our statistical work here, typically by averaging values for the relevant child-years.

Characteristics of Youth Analyzed, Their Households, and Their Neighborhoods

Table 1 shows that the full sample of 791 teens is comprised of slightly more females than males, with females making up just over 50 percent. The mean current age of the child at the time of the survey was 21 (the youngest children in the sample were 15, while the oldest was 36). The majority of the teens analyzed were of Latino ethnicity, comprising 56 percent of the sample, with the remainder Black. Of those important persons in the children's lives, mothers had the highest incidence of positive influence (mean = 1.66), followed by teachers (mean = 1.45). The mean number of schools attended during the high school stage was just over 1, and 82 percent of those most recently attended were public schools. Additionally, 80 percent of the high schools attended were in the child's neighborhood. On average, only a very small percentage of time was spent outside of the household by the children of interest. The mean number of younger siblings in the household during the high school stage was 1.2, while the mean number of siblings that had dropped out of high school was 0.13. Lastly, only about 5 percent of sampled youths' families were homeless while the youth was in high school.

When examining Table 1 to determine the parental characteristics associated with the high school students in our sample, one finds that 60 percent of parents had a high school diploma or more education. Nearly 13 percent of parents were born outside of the United States, typically in Mexico. Nearly 16 percent of parents were classified as borderline clinically depressed (scoring

between 16 and 26 on the aforementioned CES-D scale) and 8 percent were clinically depressed according to the CES-D scale (scoring 27 or higher). Nearly 8 percent of parents had a disability while the sampled youth was in high school and around 32 percent of parents were teens themselves when they gave birth to or fathered the sampled youth. Moreover, on average, parents spent about 30 percent of the sampled youths' high school years with a spouse or partner.

To get a sense of the sorts of places where our sampled teens resided during their high school years, we measured the mean characteristics of their neighborhoods for the relevant years. The descriptive statistics for the neighborhood characteristics we analyzed are portrayed in Table 2. This table indicates that, on average, in the census tract neighborhoods occupied by the high school youth in our sample only 14 percent of residents had obtained a college degree. The average census tract experienced by our sampled high schoolers had nearly equal proportions of renters and homeowners (with homeowners making up slightly more), a significant proportion of female-headed households (37 percent), a small proportion of Black population (15 percent), and a relatively high proportion of foreign born population (24 percent). As far as job prestige is concerned, where higher values on the index indicate more prestige, the neighborhoods occupied by our sampled children ranked relatively low on this index, averaging around 37 (the maximum value for this index in the sample was 47, while the minimum value was 31). Mean property crime in our sampled youths' Piton neighborhoods was 57 per thousand, while the corresponding violent crime mean was around 11 per thousand population. Child abuse rates

were on average around 10 per thousand population in Piton neighborhoods. Furthermore, there was an average of 10.5 subsidized day care slots in the Piton neighborhoods within which our sampled children resided. The youths' parents/caregivers assessed their neighborhoods as having roughly half of the potential six components of social capital and around 1.6 of the 5 potential components of the neighborhood social problems index, indicating both relatively low social capital and relatively low social disorder in youths' neighborhoods, on average. Nearly half of respondents reported that their children's peers were getting into trouble in the neighborhood during the child's high school experience. As far as using neighborhood resources goes, children in our sample utilized parks and playgrounds present in their neighborhoods most often (70 percent), while nearly 57 percent utilized indoor recreation centers. Only 28 percent utilized mentoring / counseling centers present in their neighborhoods.

Analytical Approach

Our dichotomous dropout model is estimated with Stata's maximum-likelihood logistic regression algorithm. Because our youth are clustered into families (N= 361 for the full sample and 279 for the majority-of-high school-in-DHA subsample), we use clustered robust standard errors.

We intentionally omit from these educational outcome equations any variables describing the youths' mental and physical health, exposure to violence, fertility, employment, or other behaviors, inasmuch as many of these may themselves be affected by neighborhood context. In this fashion we avoid

“over-controlling” and thus minimizing the apparent influence of neighborhood. We can therefore interpret our models as akin to yielding “reduced form” estimates of the degree to which neighborhood variables correlate with high school dropping out through unspecified intervening causal pathways.

Our core concurrent model operationalizes most neighborhood variables as *average* conditions experienced while the given youth was in high school (the exception being dichotomous variables: presence and use of local facilities and peers getting into trouble), and tests for relationships across all youths in the particular sample. Some census tract variables could not be included in the analyses because they were too collinear with included variables to meet tolerance standards. In all cases, the included variables in the models below are not multicollinear according to the standard criteria. We will experiment with stratifications according to gender and to ethnicity, and neighborhood context measured with a lag (i.e., during middle-school stage).

Results

Core Model

Estimated logit model parameters and odds ratios for concurrent neighborhood effects models are presented in Table 3, for both ever-in-DHA and currently-in-DHA high school samples. Overall, both models perform well, with pseudo-R squares in the .28-30 range and numerous statistically significant predictors including several neighborhood characteristics. Crucially, the parameter estimates for the neighborhood variables are virtually identical for both

samples (though standard errors are higher for the smaller sample), suggesting that there is little bias being introduced by the selection of households either out of DHA or their persistence in DHA since their initial quasi-random assignment.

Consider first the results for the control variables. Controlling for all the other youth, family, and neighborhood characteristics, we found no statistically significant differences in the odds of dropping out across low-income Denver minority youth distinguished by ethnicity and gender. The strongest youth predictors were time spent out of the household (such as in foster care) and having an older sibling who dropped out. For each percentage point increase in the amount of time the child spent outside of the household of the survey respondent, the youth's odds of dropping out increased by 3 percent. Having an older sibling dropping out more than doubled the youth's odds of dropping out. As far as family characteristics are concerned, parent's national origin and mental state were significantly related to dropping out. If the parent was born outside of the United States, the youth had 77 percent lower odds of dropping out of high school; the comparable figure for a parent having borderline depressive symptoms was 86. The youth also was less likely to drop out if the parent had higher efficacy scores and (surprisingly) if the parent had been a parent as a teen, though these relationships fall just sort of conventional p values in the smaller sample. The only school characteristic that proved predictive was whether the youth attended a public high school or not. Attending a non-public school reduced the odds of dropping out by 70 percent. Surprisingly, the number

of different high schools attended or whether the current high school was located in the neighborhood were not related to dropout behavior in our sample.

Of more relevance to this study, several neighborhood variables were statistically significant and strongly related to dropping out. The occupational and nativity composition of the neighborhood during high school years were crucial predictors of dropping out. One standard deviation-higher values for the prestige index and percentage foreign-born were associated with 69 percent and 58 percent lower odds ratios, respectively. These variables may be reflecting the impact of neighborhood adult role models and pro-education norms enforced by the community or local culture (Klebanov, Brooks-Gunn and Duncan, 1994; Connell and Halpern-Felsher, 1997). Immigrant parents (primarily from Mexico in our sample) may be stricter than other low-income minority parents when it comes to the education of their children and the expectations that they have of them, since acquiring superior education for their children may be a prime reason why they immigrated in the first place. The neighborhood problems scale was also substantively important; a standard deviation-higher value was associated with 66 percent higher odds of dropping out. This relationship is likely indicative of social disorder's corrosive effects on educational attainment. Youths who used local indoor recreation centers had 158 percent higher odds of dropping out, a result that upon further investigation we found primarily attributable to the experiences of Latino males. This finding may indicate that recreation centers in the Denver Latino community are havens for anti-education social norms or activities that effectively compete with scholarship for Latinos' attention.

Variations on the Core Model: Timing

We undertook several explorations to check the generality and robustness of the neighborhood effects observed in the models discussed above. First, we investigated the temporal pattern of exposures, given recent heightened attention to this dimension of neighborhood effects (Galster, 2011; Musterd, Galster and Andersson, forthcoming). We tested a lagged model where the neighborhood context variables were measured at the middle school stage. While this lagged timing model produced results with many significant neighborhood context variables, the stronger neighborhood effects (as far as both coefficient magnitude and levels of significance are concerned) were produced by the concurrent model. We recognize that some neighborhood contexts may produce even more durable impacts (Sampson, Sharkey, and Raudenbush, 2008) but these are not explored in this study.

Variations on the Core Model: Ethnic and Gender Differences

As our second exploration we stratified the core concurrent model, first according to gender, and then by ethnicity.¹⁶ For space considerations we provide summaries of main themes here that are robust across both samples; details are available from the authors. The stratifications reveal that only one of the foregoing neighborhood variables maintains statistically significant (and, as it turns out, roughly equal in magnitude) predictive power across all strata: percentage of foreign born. The remaining neighborhood relationships are highly differentiated by gender and ethnicity. Latino males have lower dropout rates in

¹⁶ In the full sample the Ns for these strata were as follows: 290 blacks, 378 Latinos, 329 males, 339 females.

neighborhoods with higher occupational prestige and an absence of indoor recreation centers. Black males, on the other hand, have lower dropout rates in neighborhoods with fewer social problems and higher percentages of households headed by a female. The presence of parks used by the youth emerged as a predictor of (higher) dropout rates for girls, with no definitive ethnic differences. Latinas, however, evince substantially lower dropout rates where neighborhood child abuse and neglect rates are lower.

Variations on the Core Model: Neighborhood Poverty

Our third exploration involved the use of percent of persons in poverty as the sole summary measure of neighborhood context.¹⁷ Concentrated poverty has conventionally been viewed as the single most-predictive indicator of environments yielding a host of negative outcomes for children and youth (Burdick-Will et al., 2010). Despite several alternative (including nonlinear) specifications of this continuous variable, it never proved a statistically significant predictor of dropping out, regardless of sample or ethnic / gender stratum. This suggests that (at least in Denver) poverty rate is a weak summary indicator of neighborhood context as it relates to high school performance outcomes, compared to the more comprehensive and nuanced measures we have reported above.

Next we experimented with a dummy variable indicating if, on average, during high school the youth was residing in a neighborhood with ten percent poverty rate or lower, the same standard applied to the initial assignments of the

¹⁷ Poverty was excluded from the prior models due to its multicollinearity with several other census tract variables.

experimental voucher group in the MTO demonstration. As with others, this measure of neighborhood poverty always proved statistically insignificant, *except* in the case of black youth, where it evinced a highly statistically significant relationship indicating that such a context was associated with a huge 94 percent lower odds of dropping out. Given that the vast majority of MTO experimental movers also were black but little to no gains in educational outcomes were observed, why might results differ?¹⁸ We admit there are many possibilities, but would forward differences in the duration of exposure to low-poverty neighborhoods as a prime candidate. For the MTO experimental group the mean was only 2.7 years; the comparable figure for our black youth sample living in a low-poverty neighborhood while attending high school was 7.0 years. Sustained exposure to low-poverty environments may be required before noticeable differences in dropout rates emerge for black youth.

Variations on the Core Model: Exposure to Violence

Our last exploration probes a theme of growing salience in the educational attainment-neighborhood-school literature: exposure to violence (Sampson, Sharkey and Raudenbush, 2008; Burdick-Will et al., 2010). Our measures of reported crimes never proved predictive, but we suspect that they are measured at too large a spatial scale—typically two census tracts—to be accurate measures of youth’s exposure to violence. Perhaps more likely is that our index

¹⁸ We recognize that MTO measured standardized achievement test scores, not dropout rates. We further recognize that in the two MTO sites with almost exclusively black participants and the highest concentrations of disadvantage, Chicago and Baltimore, MTO researchers did observe a positive effect of low-poverty neighborhoods on scores (Burdick-Will et al., 2010). Finally, we note that in our study we only observe 39 cases of black youth residing in low-poverty neighborhoods during high school.

of neighborhood problems based on respondent's assessments is serving as such a proxy.

We estimated variants of the core model employing variables based on respondent-provided measures of whether and when the youth was either victimized by or witnessed violence in the neighborhood or in school. The only such variable that ever proved predictive was whether the youth had *ever* been victimized by violence in the neighborhood; such victimization during high school itself was not predictive. Such an exposure was associated with a doubling of the odds of dropping out. Remarkably, the inclusion of this variable only reduced the magnitude of the neighborhood problems coefficient by 4 percent, suggesting that exposure to violence and social disorder may have distinctive contextual impacts involving different causal mechanisms.

Discussion

We think it particularly noteworthy that variables infrequently (if ever) employed in neighborhood effect statistical studies (due to their general unavailability) often proved robust predictors here, either in the full sample or for particular gender / ethnic strata. In particular, neighborhood context as measured by the neighborhood problem index, child abuse rate, indoor recreational facilities, parks, and victimization by neighborhood violence show important associations with dropping out. This implies that much prior work employing only census-based indicators may have overlooked important neighborhood effects because of their limited contextual measures.

It is also of importance that there is clear heterogeneity in neighborhood effects, as has been recently emphasized (Galster, Andersson and Musterd, 2010; Burdick-Will et al., 2010; Harding et al. 2011). Our findings indicate that particular dimensions of context matter differentially to youth from low-income families based on their gender and ethnicity. Several studies have confirmed that there were substantially different (although inconsistent) neighborhood context relationships with educational outcomes depending on the gender and race of the youths, though Latino ethnicity has not been probed distinctly (Crane, 1991; Duncan, 1994; Connell et al. 1995; Ensminger, Lamkin and Jacobson, 1996; Aaronson, 1998; Vartanian and Gleason, 1999; Ceballo, McLoyd and Toyokawa, 2004; Jargowsky and El Komi, 2011). They also indicate that different groups may have different reactions to the same neighborhood dimensions and/or may employ different strategies to buffer context. Few prior studies have considered neighborhood effects on Latinos; the oversight is unfortunate, as we have identified a distinctive and substantively important pattern of neighborhood effects for Latino high school dropout rates. Our findings also suggest that several causal mechanisms may be jointly producing neighborhood effects on dropping out, even when holding a particular gender / ethnic stratum constant.

One measure of neighborhood context proved robust across ethnicities and genders, however: percentage of foreign-born residents. In Denver a majority of this foreign-born group consists of Mexican-origin households. To probe the potential causal processes producing this observed association, we

conducted follow-up in-depth interviews with 34 selected adult participants in our phone survey and their 50 young adult children during the Fall 2010. The qualitative evidence from both parents and children interviewed suggested that our Latino parents typically promulgated pro-education values and closely monitored teen activities that might conflict with schooling. But recall we found that the impact of this contextual condition is only slightly weaker for Blacks than Latinos, suggesting that there is some broader externality effect that transcends intra-ethnic culture. Pro-schooling values might well spread from immigrant students to others in the neighborhood and/or classroom, of course. Moreover, immigrant households may have more adults (from multiple generations) and more adults who are not in the workforce, on average. This cadre of home-based adults may provide more opportunities for supervised study and recreation in homes for not only their own children but neighboring ones as well. As one of our teens expressed during a follow-up interview, *“Everybody was always at my house. My parents kept us busy... watching out and being careful, and not only my parents [did this].”* These adults might also supply enhanced collective efficacy that discourages teen loitering or other public activities that might compete with more pro-education ones.

Although we found that neighborhood conditions measured concurrently with attending high school were stronger predictors than lagged (middle school) contexts, we emphasize that this finding should not be generalized to early childhood neighborhood exposures. On the contrary, several scholars have argued that the impact of childhood neighborhood may produce lasting effects

that could show up in high school performance (Bouchard 1997; Duncan et al. 1998; Shonkoff and Phillips 2000; Slavin, Karweit, and Wasik 1993), and recent empirical work has substantiated how cumulative neighborhood exposures are strong predictors of dropping out (Crowder and South, 2011; Wodtke, Harding and Elwert, 2011). In this sense we have estimated here a “value-added” model of what difference concurrent neighborhood exposures during high school might make on children who already bring certain attributes and experiences into their high school years. As such, we are investigating only a segment of what may be even larger effects of neighborhood.

Our results hold important implications for education policy, i.e., assisted housing policy can be school policy as well. We have demonstrated in this natural experiment that a well-conceived and operated scattered-site public housing program can yield impressive improvements in educational outcomes for low-income Black and Latino teens (cf. Schwartz, 2010). Thus, we come to a more hopeful conclusion than that reached by DeLuca and Dayton (2009) in their recent review of housing mobility and school choice initiatives. Though acknowledging that assisted housing programs can bring about context changes, “it appears much more difficult to improve the educational outcomes of children” (2009: 478).

Conclusions, Caveats and Future Directions

Education policymakers have struggled with the daunting methodological challenges of obtaining unbiased estimates of the causal impact of neighborhood

on high school educational outcomes, due primarily to incomplete controls for selection biases and little variation in the environments experienced by low-income, minority children. An innovative public housing program instituted by the Denver Housing Authority provides a unique opportunity to explore this issue because the DHA mimics a random assignment to a wide range of neighborhood for families with children who apply for DHA housing.

We find that fewer neighborhood problems, higher neighborhood occupational prestige, and higher foreign-born shares generally predict less dropping out for low-income, minority teens, though the particulars are contextualized by gender and ethnicity. Neighborhood effects appear stronger when measured concurrently with high school than during middle school. Neighborhood poverty rate generally proves a weak, incomplete measure of neighborhood context, although sustained exposure to low-poverty environments provided gains to black youth, suggesting that educational “non-findings” from the MTO demonstration are not general.

The first shortcoming of our study is that we have virtually no measures of the school environment, which other work has identified as a primary conduit of neighborhood effects (Sykes and Musterd, 2011). We do not think that this biases our results for neighborhood insofar as there is an open, non-geographic enrollment system in Denver, although we cannot be certain. Further, we have not investigated the extent to which early childhood neighborhood environments may have had durable effects that manifest themselves in high school performance. Finally, we have not attempted to probe potential ways in which

neighborhood context may affect youth exposure to violence, behaviors, nutrition, and health, which might reveal more about underlying causal mechanisms.

These latter shortcomings will be addressed in future work.

Finally, our results clearly suggest that education policymakers should be cognizant of neighborhood as an important developmental context. Finishing high school is clearly influenced by more than personal or family characteristics of low-income, minority youth, though which aspect of the larger context matter appear quite gender- and ethnic-specific. The daunting policy challenge is encouraging neighborhood environments that can be more developmentally friendly to all these strata, though our results imply that well-designed assisted housing can provide a key component of this strategy.

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Table 1.
Descriptive Statistics of Parental and Youth Characteristics in Analysis
Sample

VARIABLE	FULL SAMPLE			DHA-in-HS SAMPLE		
	N	MEAN	STD DEV	N	MEAN	STD DEV
Child's Gender (Female = 1)	791	0.506	0.500	543	0.486	0.500
Child's Age at time of Survey (Minimum 15 for inclusion)	791	20.960	4.753	543	21.785	4.920
Child's Race/Ethnicity (Black = 1; Latino = 2)	791	1.556	0.497	543	1.613	0.487
Hispanic Females	791	0.276	0.447	543	0.289	0.454
Hispanic Males	791	0.281	0.450	543	0.324	0.468
Black Females	791	0.230	0.421	543	0.197	0.398
Black Males (excluded category in analyses)	791	0.214	0.410	543	0.190	0.392
Percentage of HS years Child spent outside the Household	791	5.815	19.102	543	3.039	11.464
Number of Schools attended during High School	766	1.313	0.614	527	1.306	0.601
Child's High School was Not a Public School (1 = yes)	791	0.178	0.383	543	0.123	0.329
Child's High School was in the Neighborhood (1 = yes)	791	0.796	0.403	543	0.790	0.408
Number of Younger Siblings in the Household during High School	791	1.226	1.288	543	1.413	1.374
Number of Siblings that Dropped Out	791	0.131	0.380	543	0.125	0.368
Family was Homeless while Child was in High School (1 = yes)	791	0.054	0.227	543	0.053	0.225
Household Stressors Scale	777	2.696	1.735	543	2.812	1.676
Influence of Father (-2 = very negative, 2 = very positive)	791	-0.014	1.347	543	0.035	1.313
Influence of Mother (-2 = very negative, 2 = very positive)	791	1.655	0.787	543	1.656	0.774
Influence of Other Male (-2 = very negative, 2 = very positive)	791	0.996	0.998	543	0.974	0.998
Influence of Teacher (-2 = very negative, 2 = very positive)	791	1.450	0.749	543	1.475	0.738
Influence of School Counselor (-2 = very negative, 2 = very positive)	791	1.115	0.863	543	1.090	0.887
<i>Child Dropped Out of High School (1 = yes)</i>	791	0.192	0.394	543	0.195	0.397
Parenting Efficacy Scale	791	16.853	3.315	543	16.764	3.412
Parenting Beliefs Scale	791	20.827	3.625	543	20.862	3.545
Percentage of HS years Parent had a Spouse or Partner	791	31.100	42.134	543	31.031	42.323
Parent had a Disability during Child's High School years (1 = yes)	791	0.077	0.267	543	0.070	0.255
Parent was a Teen when Child was Born (1 = yes)	791	0.321	0.467	543	0.330	0.471
Parent Born outside of the U.S. (1 = yes)	791	0.126	0.333	543	0.147	0.355
Parent Alcohol and Drug Use Scale	791	1.690	2.768	543	1.617	2.634
Parent is Clinically Depressed (1 = yes)	791	0.082	0.275	543	0.087	0.281
Parent is Borderline Clinically Depressed (1 = yes)	791	0.155	0.363	543	0.144	0.351
Parent has High School Education or Higher (1 = yes)	791	0.602	0.490	543	0.584	0.493
NOTE:						
<i>Italicized variable is the Dependent Variable in analyses.</i>						

Source: *Denver Child Study* survey data

Table 2.
Descriptive Statistics of Neighborhood Characteristics in Analysis Sample

VARIABLE	FULL SAMPLE			DHA-in-HS SAMPLE		
	N	MEAN	STD DEV	N	MEAN	STD DEV
Avg Percentage of Population in Neigh'd with College Degree	764	14.081	11.563	543	13.373	11.794
Avg Percentage Female-Headed Households in Neigh'd	764	37.494	15.912	543	39.201	17.077
Avg Percentage Owner-Occupied Homes in Neigh'd	764	51.822	19.321	543	49.289	19.293
Avg Percentage Households in Neigh'd that Moved in Past Year	764	24.895	8.374	543	25.061	7.942
Avg Percentage Black Households in the Neigh'd	764	15.100	18.291	543	12.732	15.839
Avg Percentage Foreign-Born Population in the Neigh'd	764	23.683	11.774	543	23.956	11.679
Avg Occupational Prestige in the Neigh'd	764	36.952	3.128	543	36.740	3.227
Avg Confirmed Cases of Child Abuse or Neglect in the Neigh'd	709	9.951	5.778	537	10.126	5.839
Avg Number of Licensed Subsidized Day Care Slots in the Neigh'd	709	10.490	8.805	535	10.416	8.415
Avg Number of Property Crimes in the Neigh'd**	718	57.353	33.074	543	60.109	34.139
Avg Number of Violent Crimes in the Neigh'd**	718	10.685	6.531	543	11.073	6.724
Avg Neighborhood Social Capital Scale	777	3.270	1.783	543	3.291	1.732
Avg Neighborhood Social Problems Scale	777	1.563	1.640	543	1.735	1.713
Presence of Peers Getting into Trouble in the Neigh'd (1 = yes)	791	0.469	0.499	543	0.490	0.500
Presence of Park/Playground in the Neigh'd that Child Used (1 = yes)	791	0.703	0.457	543	0.738	0.440
Presence of Indoor Recreation Cntr in the Neigh'd that Child Used (1 = yes)	791	0.566	0.496	543	0.595	0.491
Presence of Mentoring Center in the Neigh'd that Child Used (1 = yes)	791	0.277	0.448	543	0.284	0.451
NOTE:						
** Crime rates are per 1000 population.						

Sources: U.S. Bureau of the Census/Geolytics *Neighborhood Change Database*; Piton Foundation *Neighborhood Facts Database*; *Denver Child Study*, respectively

Table 3.
Parameter Estimates of Core Model of High School Dropout Behavior for Black and Latino Youth

VARIABLE	FULL SAMPLE				DHA-in-HS SAMPLE			
	COEFF	ROBUST STD ERR	ODDS RATIO	ROBUST STD ERR	COEFF	ROBUST STD ERR	ODDS RATIO	ROBUST STD ERR
Number of Observations	668				514			
Wald Chi-Square (43 DF)	151.07**				116.06**			
Log pseudolikelihood	-229.34				-168.09			
Pseudo R-square	0.281				0.304			
Hispanic Female	-0.622	0.461	0.537	0.248	-0.936	0.571	0.392	0.224
Hispanic Male	0.275	0.430	1.317	0.566	-0.044	0.510	0.957	0.488
Black Female	-0.500	0.411	0.606	0.249	-0.615	0.556	0.541	0.300
Percentage of HS years Outside HH	0.0325**	0.010	1.033	0.010	2.927*	1.224	18.668	22.848
Number of Schools attended during HS	0.003	0.197	1.003	0.198	0.150	0.239	1.161	0.278
HS was Not a Public School	-1.192**	0.385	0.304	0.117	-1.490*	0.738	0.225	0.166
HS was in the Neighborhood	0.621	0.387	1.861	0.720	0.283	0.436	1.327	0.578
Number of Younger Siblings	0.128	0.103	1.137	0.117	0.032	0.122	1.033	0.126
Number of Siblings who Dropped Out	1.159**	0.374	3.188	1.193	1.871**	0.367	6.496	2.384
Family was Homeless during HS	-0.782	0.658	0.458	0.301	-0.340	0.553	0.712	0.394
Avg Household Stressors Scale	-0.078	0.083	0.925	0.077	-0.123	0.098	0.884	0.087
Parenting Efficacy Scale	-0.093**	0.035	0.911	0.032	-0.066	0.042	0.936	0.039
Parenting Beliefs Scale	0.012	0.035	1.012	0.036	0.071	0.042	1.074	0.045
Percentage of HS years with 2 Parent HH	0.005	0.003	1.005	0.003	0.350	0.361	1.419	0.512
Parent had Disability during HS	0.941	0.516	2.563	1.322	0.946	0.591	2.575	1.522
Parent was a Teen when Child was Born	-0.664*	0.291	0.515	0.150	-0.611	0.351	0.543	0.191
Parent was Born Outside of the US	-1.482**	0.540	0.227	0.123	-1.118*	0.542	0.327	0.177
Parent Alcohol and Drug Scale	0.070	0.041	1.072	0.044	0.083	0.055	1.087	0.060
Parent was Clinically Depressed	0.272	0.484	1.312	0.635	0.704	0.549	2.022	1.110
Parent was Borderline Clinically Depressed	0.84*	0.356	2.316	0.824	0.811*	0.399	2.249	0.897
Parent had HS Education or Higher	-0.377	0.261	0.686	0.179	-0.619	0.318	0.538	0.171
Father's Influence	-0.061	0.099	0.941	0.094	-0.006	0.123	0.994	0.122
Mother's Influence	0.348	0.229	1.417	0.324	0.267	0.281	1.306	0.367
Other Male's Influence	-0.068	0.138	0.935	0.129	0.026	0.169	1.027	0.174
Teacher's Influence	-0.223	0.191	0.800	0.153	-0.269	0.230	0.764	0.176
Counselor's Influence	-0.006	0.182	0.994	0.181	0.081	0.208	1.085	0.225
Child Used Park during HS	0.254	0.329	1.289	0.424	0.315	0.396	1.371	0.543
Child Used Rec Center during HS	0.948**	0.318	2.582	0.821	0.895*	0.369	2.447	0.902
Child Used Mentoring during HS	-0.551	0.360	0.576	0.208	-0.674	0.409	0.510	0.208
Avg Neigh'd Social Capital Scale during HS	-0.060	0.092	0.941	0.087	-0.121	0.103	0.886	0.091
Peers got into Trouble during HS	-0.432	0.328	0.649	0.213	-0.453	0.407	0.636	0.259
Avg Neigh'd Social Problems Scale during HS	0.262**	0.096	1.299	0.124	0.266*	0.126	1.305	0.164
Avg College Educated Pop in Neigh'd during HS	0.021	0.026	1.021	0.026	0.036	0.031	1.036	0.032
Avg Female-Headed HHs in Neigh'd during HS	-0.037*	0.018	0.964	0.017	-0.041	0.023	0.960	0.022
Avg Homeowners in Neigh'd during HS	-0.005	0.015	0.995	0.015	-0.013	0.022	0.987	0.022
Avg Recent In-Movers in Neigh'd during HS	0.005	0.021	1.005	0.021	-0.008	0.029	0.992	0.029
Avg Percent Black HHs in Neigh'd during HS	-0.009	0.009	0.991	0.009	-0.019	0.013	0.982	0.012
Avg Job Prestige in Neigh'd during HS	-0.240*	0.093	0.787	0.073	-0.336**	0.112	0.715	0.080
Avg Foreign-Born HHs in Neigh'd during HS	-0.056**	0.018	0.946	0.017	-0.062*	0.024	0.940	0.023
Avg Child Abuse Cases in Neigh'd during HS	0.056	0.034	1.058	0.036	0.074	0.039	1.076	0.042
Avg Subsidized Day Care Slots in Neigh'd during HS	0.023	0.014	1.024	0.015	0.038	0.023	1.039	0.024
Avg Property Crime Rate in Neigh'd during HS	0.001	0.008	1.001	0.008	0.002	0.007	1.002	0.007
Avg Violent Crime Rate in Neigh'd during HS	-0.061	0.046	0.941	0.043	-0.072	0.047	0.930	0.044
Constant	9.640	3.222	15369.0	49516.5	12.813	4.389	366969.9	1610460.0

NOTE:
** indicates p<.01; * indicates p<.05 (two-tailed test).

Appendix 3.

The Effects of Adolescent Neighborhood Context on Early Adult Labor Market Outcomes for Low-Income, Black and Latino Teens

**George Galster
Wayne State University**

**Jackie Cutsinger
Wayne State University**

**Anna Santiago
Case Western Reserve University**

**Rebecca Grace
The Urban Institute**

**Presented at Association for Public Policy Analysis and Management meetings
Washington, DC November, 2011**

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ABSTRACT

The research aims to quantify how a variety of labor market outcomes during ages 18 to 21 (worked full or part-time, not working but in school, neither working nor in school) for low-income, black and Latino teens residing in Denver public housing for a substantial period are statistically related to conditions in the neighborhoods in which they were raised.

Data analyzed come from surveys of 736 current and former residents of the Denver (CO) Housing Authority (DHA) whose 1,859 children met study eligibility criteria. For decades, DHA has operated public housing located units throughout the City and County of Denver. Because the initial allocation of households on the DHA waiting list to units mimics a random assignment to a wide range of neighborhood environments, this program represents a natural experiment for overcoming parental location selection bias in estimating neighborhood effects.

We found that those who experienced during late adolescence above-average degrees of social capital within above-average occupational prestige neighborhoods evinced substantially higher probabilities of working or attending post-secondary education as young adults. This relationship: (1) appeared stronger when neighborhood context was measured during late adolescence compared to early adolescence; (2) was robust for men and women; and (3) was stronger for Latinos than Blacks.

Introduction

One of the most widely debated issues in contemporary social science is the degree to which neighborhood context exerts a substantial, independent influence on the life courses of children, youth, and adults. As often framed in public policy debates in the U.S., the issue is the degree to which low-income, minority families living in neighborhoods that are not predominantly low-income and minority-occupied will have enhanced opportunities for economic independence and superior developmental environments for their children.

Empirical investigations of this issue have comprised a rapidly expanding literature, yet consensual conclusions have foundered on hotly contested methodological debates. For reviews, see Gephart (1997), van Kempen (1997), Friedrichs (1998), Leventhal and Brooks-Gunn (2000), Sampson, Morenoff, and Gannon-Rowley (2002), Friedrichs, Galster and Musterd (2003), Ellen and Turner (2003), Galster (2003, 2005, 2008), and Harding et al. (2011). We aim to contribute to this literature through research on the teenage children of current and former residents of the Denver (CO) Housing Authority (DHA) who are either Black or Latino. DHA has operated since 1969 public housing units located in a wide range of neighborhoods throughout the City and County of Denver. Because the initial assignment of households on the DHA waiting list to dwellings (and, thus, to neighborhoods) mimics a random process, this program represents an unusual natural experiment holding great potential for accurately measuring neighborhood effects.

In this study we analyze data from administrative sources and data we have collected from telephone surveys with current and former DHA tenants, which provide retrospective information on a battery of youth outcomes and family characteristics. We focus in this paper on labor market outcomes during teenage years.

Our primary research question in this paper involves identifying the magnitude of context effects as operationalized by several neighborhood indicators:

For Black and Latino teens who spent a considerable period during childhood living in DHA public housing, are there statistically and economically significant differences in their labor market outcomes during age 18-25 (productive activity, welfare receipt) that can be associated with differences in the neighborhood environments to which they were exposed as teens?

Our work advances this literature in four important ways. First, because parents of our sampled children were quasi-randomly assigned to neighborhoods, the challenge of parental geographic selection bias is overcome. Second, we evaluate an unprecedented variety of measures of neighborhood environment, both objective and subjective. Third, we are one of the few studies to examine neighborhood impacts on labor market outcomes of a large sample of low-income Latino youth. Fourth, we probe the timing of neighborhood effects by examining exposures during both late adolescence (high school) and early adolescence (middle school) developmental stages.

The rest of our paper is organized as follows. The first section sketches the theoretical bases for how neighborhoods might affect teens' labor force outcomes. The second section discusses methodological challenges confronting those who would estimate the magnitude of neighborhood effects—especially the salient issue of household geographic selection bias—and how prior research has tried to meet this challenge. The third section describes our study sample, survey, and neighborhood indicators forming the basis of our empirical work. The fourth section presents our analytical approach. The fifth section reports our findings. The last section concludes, provides caveats, and offers policy implications of the research.

In brief, we find that those who experienced during late adolescence above-average degrees of social capital within above-average occupational prestige neighborhoods evinced substantially higher probabilities of working or attending post-secondary education as young adults. This relationship: (1) appeared stronger when neighborhood context was measured during late adolescence compared to early adolescence; (2) was robust for men and women; and (3) was stronger for Latinos than Blacks.

How Neighborhoods Might Affect Teens' Labor Force Outcomes

Neighborhood effects may transpire through a variety of causal mechanisms that can occur either through social interactions within the neighborhood, biological processes within the neighborhood, and/or by actions of others located outside of the neighborhood; for extended discussions see Jencks and Mayer (1990), Duncan, Connell and Klebanov (1997), Gephart (1997), Friedrichs (1998), Sampson (2001), Dietz (2002), Sampson, Morenoff, and Gannon-Rowley (2002), Ioannides and Loury (2004), Harding

et al. (2011), Briggs et al. (2011), and Galster (2011). The potential intra-neighborhood social mechanisms include socialization and social control (norms, peers, and role models), networks, social disorder, exposure to violence, and competition. The potential intra-neighborhood biological mechanisms involve environmental exposures. The potential extra-neighborhood mechanisms are stigmatization, institutional resources, and accessibility. Because these mechanisms are well-known, we describe them only briefly:

- *Socialization*: Teens develop attitudes, values, behaviors and expectations about school and work as a result of interactions with neighborhood peers, role models. Some types of collective socialization may reinforce normatively these developments, while other types (perhaps arising within kin or cultural groups) may operate in offsetting fashion.
- *Networks*: Teens may gain different amounts of information about skill-enhancing and employment opportunities, depending on the degree to which they rely on local social networks and the resources these networks can access.
- *Social Disorder*: Teens may be able to take advantage of a different range of skill-enhancing and employment opportunities, depending on the degree to which they feel secure leaving their homes and traversing their neighborhoods.
- *Exposure to Violence*: Exposure to neighborhood violence may lead to adverse physical responses (like ill health from stress), psychological responses (like post-traumatic stress syndrome) and inhibitions to speech communication, all of which may impede labor market performance.
- *Competition*: Teens may intensify their labor force efforts in a neighborhood context of greater economic competition and status-seeking.
- *Stigmatization*: Prospective employers may evaluate teen job applicants from certain locales based on the reputation of the place, a version of “statistical discrimination.”
- *Environment and Health*: Neighborhood-based variations in exposure to ambient noise, toxins, lead, or other pollutants can affect mental and behavioral development and the severity of asthma and other diseases, thereby affecting labor market performance.
- *Institutional Resources*: Public and private institutions controlling important services and facilities vary their quantity and quality on the basis of neighborhood

context, thereby differentially affecting teens' opportunities to acquire and use human capital.

- *Accessibility*: Neighborhoods offer different degrees of access to employment information and work sites themselves, due both to geographical proximity and public transportation disparities.

While current scholarship is not decisive, it suggests that several intra- and extra-neighborhood mechanisms above may be operative (Van Kempen, 1997; Dietz, 2002; Sampson, Morenoff and Gannon-Rowley, 2002; Ellen and Turner, 2003; Galster, 2012) and that different mechanisms may have varying salience across different groups (Burdick-Will et al, 2010; Galster, Andersson and Musterd, 2010; Clampet-Lundquist et al. 2011).

Measuring the Effect of Neighborhoods: Challenges and Responses in the Empirical Literature

In simplified terms, a labor market outcome of interest (O) observed at time t for individual teen i residing in neighborhood j in metropolitan area k can be expressed:

$$O_{it} = \alpha + \beta[C_{it}] + \gamma[C_i] + \phi[UC_{it}] + \partial[UC_i] + \delta[P_{it}] + \zeta[P_i] + \lambda[UP_{it}] + \psi[UP_i] + \theta[N_{jt}] + \mu[M_{kt}] + \varepsilon \quad [1]$$

where:

[C_{it}] = observed characteristics of teen that can vary over time
(e.g., past trauma, number of siblings in the home)

[C] = observed characteristics of teen that do not vary over time
(e.g., race, year and country of birth)

[UC_{it}] = unobserved characteristics of teen that can vary over time
(e.g., psychological states, interpersonal relationships)

[UC] = unobserved characteristics of teen that do not vary over time
(e.g., pre-natal experiences)

[P_{it}] = observed characteristics of teen's parent(s) that can vary over time
(e.g., marital status, income)

[P] = observed characteristics of teen's parent(s) that do not vary over time

(e.g., race, year and country of birth)

[UP_i] = unobserved characteristics of teen's parent(s) that can vary over time

(e.g., psychological states, interpersonal relationships, self-efficacy)

[UP] = unobserved characteristics of teen's parent(s) that do not vary over time

(e.g., childhood experiences)

[N_i] = observed characteristics of neighborhood where individual teen resides during t, and perhaps during prior periods (e.g., poverty rate)

[M_i] = observed characteristics of metropolitan area in which individual teen resides during t (e.g., overall unemployment rate)

ε = a random error term with statistical properties discussed below

i = individual teen

j = neighborhood

k = metropolitan area

t = time period (typically a year, though perhaps a developmental stage)

All Greek letters represent parameters to be estimated through some sort of multivariate statistical technique.

The central empirical challenges facing analysts attempting to measure neighborhood effects accurately (i.e., get a precise, unbiased measure of θ) can be seen through the framework of equation [1]:

- What is the appropriate geographic scale(s) that defines [N], and does this depend on developmental stage of child?
- What are the appropriate characteristics to measure when operationalizing [N], and does this depend on developmental stage of child?
- What is the intensity and duration of child i's exposure to [N]? Does [N] affect O immediately, with a lag, or cumulatively?
- How can we comprehensively operationalize and measure the key components of [C], [C_i], [P], and [P_i]?
- Given that one cannot operationalize and measure [UP] and [UP_i], what can be done to minimize bias in estimated θ from these omitted variables that may be strongly associated with parents' neighborhood selection process?

Few (if any) studies have answered all of these questions satisfactorily; for a fuller discussion see Galster (2008). Most of the methodological attention has been paid, however, to the last question: geographic selection variable bias (Ginther, Haveman and Wolfe 2000). The issue is that adults likely have (unmeasured) motivations, behaviors, and skills related to their own and their children's economic prospects and move from and to certain types of neighborhoods as a consequence of these unobserved characteristics. Any observed relationship between their neighborhood conditions and economic outcomes for adults or teens may therefore be biased because of this systematic spatial selection process, *even if all parental observable characteristics are controlled* (Manski 1995, 2000; Duncan et al. 1997; Duncan and Raudenbush 1999, Dietz 2002).¹

There have been several types of methodological responses to this challenge in studies focusing on labor market outcomes:

- *Difference Models Based on Longitudinal Data:* The biases from unobserved, time-invariant parental and teen characteristics are eliminated by measuring differences between two periods (Bolster et al., 2007; Galster et al., 2008; Musterd et al., 2008; Van Ham and Manley, 2009; Galster, Andersson, and Musterd, 2010).
- *Fixed Effect Models Based on Longitudinal Data:* Unobserved, time-invariant characteristics of individuals that may lead to both neighborhood selection and labor force outcomes are measured by individual dummy variables (Weinberg, Reagan and Yankow, 2004; Musterd, Galster and Andersson, forthcoming).
- *Instrumental Variables for Neighborhood Characteristics:* Proxy variables for neighborhood characteristics are devised that only vary according to attributes exogenous to the individual (Galster et al., 2007; Kling, Liebman and Katz, 2007; Ludwig et al., 2008; Cutler, Glaeser and Vigdor, 2008).
- *Residents of Same Block:* If there is little sorting on unobservables at the census block level, then networks among very localized neighbors should be free of geographic selection bias (Bayer, Ross and Topa, 2008)

¹ The direction of the bias has been the subject of debate, with Jencks and Mayer (1990) and Tienda (1991) arguing that neighborhood impacts are biased upwards, and Brooks-Gunn, Duncan, and Aber (1997) arguing the opposite. Gennetian, Ludwig, and Sanbonmatsu (2011) show that these biases can be substantial enough to seriously distort conclusions about the magnitude and direction of neighborhood effects.

- *Natural Quasi-Experiments*: Data are produced from idiosyncratic public policy initiatives (typically involving subsidized housing) that create exogenous variation in neighborhood environments for tenants (Rosenbaum, 1995; Rosenbaum, Reynolds and DeLuca, 2002; DeLuca et al., 2010; Briggs, 1997, 1998; Oreopolis, 2003; Edin, Fredricksson and Aslund, 2003; Aslund and Fredricksson, 2005; Propper et al., 2007).
- *Random Assignment Experiments*: Data are produced by an experimental design whereby households are randomly assigned to different neighborhoods, such as the Moving To Opportunity (MTO) demonstration (Ludwig, Duncan, and Pinkston, 2000; Orr et al., 2003; Kling, Liebman and Katz 2007; Ludwig et al., 2008).

Unfortunately, there is no consensus about neighborhood effects on labor market outcomes. Using one of the aforementioned econometric techniques on observational datasets, Weinberg, Reagan and Yankow (2004); Bolster et al. (2007), Dawkins, Shen and Sanchez (2005), Cutler, Glaeser and Vigdor (2008), Galster et al. (2008), Bayer, Ross and Topa (2008), Galster, Andersson, and Musterd (2010), Musterd, Galster and Andersson (forthcoming) argue that their evidence suggests nontrivial neighborhood effects on labor market outcomes, whereas Propper et al. (2007) and van Ham and Manley (2009) suggest selection dominates. Rosenbaum (1995), Edin, Fredricksson and Aslund (2003), Aslund and Fredricksson (2005), and DeLuca et al. (2010) find evidence of neighborhood effects on labor market outcomes in their analyses of natural experiments, but Oreopolis (2003) does not. The MTO has been seen in some quarters as the “gold standard” method, so the null labor market findings of Ludwig, Duncan, and Pinkston (2000), Orr et al. (2003), Liebman and Katz (2007) and Ludwig et al. (2008) have been seen as definitive in some circles (e.g., Smolensky, 2007: 1016).

Such a sweeping conclusion is unsupportable, however, for at least seven reasons (Sanbonmatsu et al. 2006; Clampet-Lundquist and Massey, 2008; Sampson, 2008; Briggs, Ferryman, Popkin, and Rendon; 2008, DeLuca and Dayton, 2009; Burdick-Will et al., 2010; Briggs, Popkin and Goering, 2010; Briggs et al., 2011). First, although MTO *starts* with random assignment, thereafter it does not control the assignment of neighborhood characteristics to members of *any* of the three comparison groups who move, and thus does not purge the relationship between neighborhood characteristics and unmeasured individual characteristics. For illustration, 85 percent of the experimental group moved subsequent to their initially assigned, low-poverty location.

As Gennetian, Ludwig, and Sanbonmatsu (2011) explain, this selection within MTO can be addressed through instrumental variables techniques. But the adjustment remains imprecise, because the neighborhood poverty instrument is measured either at a single point in the observed family's trajectory of moves or represents the average across this trajectory. Second, MTO fails to ensure adequate duration of exposure of children to new neighborhood conditions. Some neighborhood processes are likely to yield outcomes only after long-term exposure, yet the extreme mobility of the experimental group obviated the chance for these affects to appear. Third, MTO fails to consider the potentially durable developmental effects upon youths who spent their childhoods in disadvantaged neighborhoods *before* their families participated in the MTO experimental group. If the cumulative impacts of such environments created some durable disadvantages for these youth, their relatively short exposure to low-poverty environments due to MTO would have been insufficient to observe many changes in their outcomes in these domains. Fourth, MTO experimental families rarely lived in any but predominantly black-occupied neighborhoods located near those of concentrated disadvantage and thus may not have experienced sizable enhancements in neighborhood context. Poverty rates under 10 percent proved a myopic indicator of opportunity structure. Fifth, the treatment effects may have been relatively weak. Few MTO experimental families became deeply socially embedded in their host communities, thus damping potential socialization and network effects. Many actually moved farther from expanding, low-wage job concentrations, thus reducing their access to work. Much of the early years of the experiment were characterized by expansive labor markets and new welfare reforms that may have swamped the MTO effect. Sixth, roughly a fifth of the experimental families had such severe personal barriers to becoming employed that they would be unlikely to benefit from any relocation scheme. Seventh, Latino families represented too small a share of MTO recipients to be analyzed distinctly from African-American families. Indeed, MTO shares this myopia with virtually all the scholarly literature regarding the fastest-growing, disproportionately low-income minority group in the U.S.

Thus, despite its gilded reputation, MTO has not provided definitive evidence about the potential economic benefits to low-income, Black and Latino youth from *sustained residence* in neighborhoods that are advantaged on *multiple dimensions*. Our study hopes to contribute to a resolution of this vital empirical issue by exploiting a

natural experiment related to the Housing Authority of the City and County of Denver (DHA).

The Natural Experiment Involving Public Housing in Denver

In addition to its large-scale, conventional public housing developments, DHA has operated since 1969 what it calls its “Dispersed Housing Program,” providing approximately 1,500 low-income families with opportunities to live in scattered-site, single-family and small-scale, multi-family units. These dispersed units are located in a wide range of neighborhoods throughout the congruent City and County of Denver, whereas the conventional developments are typically located in less-advantaged neighborhoods; see Figure 1 for an example of the geographic distribution of our DHA household sample by neighborhood poverty rates.

[figure 1 about here--map of first DHA locations assigned]

From a research methodology standpoint, the most important feature of the DHA case is that, from 1987 onwards, applicants for all of their public housing programs were screened using the same evaluation standards and assigned to vacant housing corresponding to their family characteristics. We carefully reviewed this assignment process with DHA officials during several interviews prior and subsequent to data collection for the study. As applicants (who meet certain basic eligibility criteria) come to the top of the waiting list they are offered a vacant DHA unit (in either conventional or scattered-site programs) with a number of bedrooms appropriate for their family size and gender of children. If they do not accept this unit they are offered the next similarly sized unit that becomes available (typically after a nontrivial wait). If applicants do not accept this second unit they drop to the bottom of the queue, creating a wait of a year or more. DHA officials told us that relatively few applicants choose to reject both units offered.² The initial assignment of households to a DHA unit (and, thus, neighborhood) therefore appears on its face to be a quasi-experimental design that mimics random assignment of household to neighborhood.

We test this proposition in two ways. First, we explore the possibility of selection arising because the tenant can potentially choose between two DHA units that may be

² Our independent evaluation of DHA records showed that 88.3% of the applicants accepted their first or second offers; 7.9% ended up rejecting both offers and taking a third offer; 3.8% rejected three or more offers. Further, 75.5% of all applicants ended up in the neighborhood originally offered.

located in quite different neighborhoods. Before their initial assignment to a DHA dwelling, clients were asked by DHA whether they had any geographic location preferences. DHA administrative data show that 42.5 percent of the clients in our sample did not articulate any locational preference, approximately one-third expressed general geographical areas (i.e., Southwest Denver) while the remaining 23.5 percent provided responses that ranged from specific addresses to specific DHA developments (i.e., North Lincoln Campus of Learners). In order to assess whether those who stated a preference were assigned to a housing unit in their specified area, a number of different approaches were taken. For those who specified a particular address, we checked to see if that address was the DHA unit to which the client was initially assigned. For those who specified a preference for a particular DHA development, we used the unit number reported by DHA (which has an abbreviation of the development embedded in it) to assess whether the initial DHA unit was located within that development. For those who specified a preference for a particular neighborhood, we relied on our survey data to determine whether the original DHA unit was in the specified neighborhood. Lastly, initially assigned DHA units were mapped to identify where within the Denver metropolitan area they were located for those who specified a preference for a particular part of the metro area. Once these assessments were made, we were able to calculate frequencies and percentages for those who specified a geographic preference and got it (N=190; 25.8 percent) and those who specified a geographic preference but didn't get a housing unit that met that preference (N=233; 31.7 percent). Undoubtedly, if the vast majority of households in our sample had a strong geographic preference and were granted this preference by the DHA assignment process, one would challenge the process as one producing a quasi-random assignment. However, our analyses indicate to the contrary that the vast majority of the respondents to our survey (74.2 percent) were either cases where there was no geographic preference articulated, or where the client's stated preference was not honored. Since we are unable to ascertain the geographic location of all potential DHA unit vacancies that arose during the times that each client was assigned to their initial unit, we are unable to perform any formal statistical tests to determine whether the frequencies we obtained for those who were assigned their expressed preference were any different than what would be expected by chance.

The second way we test the DHA assignment process is by analyzing the matches of DHA client characteristics and neighborhood characteristics. The key issue

is whether DHA's assignment of public housing tenants to neighborhoods effectively removes the correlation between unobservable parental characteristics that potentially affect both location chosen and subsequent youth educational outcomes. We investigated this by examining the degree to which a variety of characteristics of parents in our sample that typically are not observed in neighborhood effect studies were correlated with multiple characteristics of their neighborhoods at the time of initial assignment by DHA. In particular, we considered 14 typically unobserved characteristics of parents (listed in Table 1) and 12 conventionally used characteristics of census tracts (percentages of: female-headed households, poor families, unemployed adults, those with only elementary school education, those with college degrees, those with professional-technical occupations, non-Hispanic black population, Hispanic population, foreign-born population, housing vacancy rate, homes built prior to 1939, homes that are owner-occupied). For each of three family sizes of DHA tenants (zero or one child; two children; three or more children) we calculated the Pearsonian correlation between each of 14 parental characteristics and the 12 neighborhood characteristics observed when the DHA first assigned our sample households to their DHA units. We stratify by family size because there is a distinct geographic pattern in Denver of where public housing units of various bedroom configurations are located.

[Table 1 about here: results of simulation]

If the DHA assignment process produced the equivalent of a random assignment of a particular family size across neighborhood characteristics, we would expect to observe no greater number of correlation coefficients between household and neighborhood characteristics significantly different from zero than would have been expected by chance. To test this we conducted Monte Carlo simulations in which each sample household was, indeed, randomly assigned to one of the DHA units with its associated bundle of neighborhood characteristics that we observed whenever the initial assignment of household in our study actually occurred.³ We used 10,000 repetitions of these simulations to produce bootstrapped standard errors and distributions of how many correlations significantly different from zero would be expected by chance.

The results are reported in Table 1. The parental characteristics are listed in the rows and the three family-size strata in the columns. The cells show for how many of the 12 possible neighborhood characteristics the initial DHA assignment produced a

³ The programming and execution of these simulations was conducted by Dr. Albert Anderson of PDQ Inc., whose contribution we gratefully acknowledge.

correlation with the given parental characteristic that was significantly different from zero at the 5% level (two-tailed test); the actual correlation coefficient and the neighborhood characteristic involved are reported in these cases. The table shows that for families with no or one child and families with two children, only 9 of a possible 168 correlations were statistically different from zero; the corresponding figure for families with three or more children was 13. Our simulations showed that in over 98% and 95% of the cases, respectively, a larger number of statistically significant correlations were produced randomly. This strongly indicated that the actual correlations we observed from initial DHA allocations of tenants to neighborhoods were consistent with those that would have been generated by a process of pure random assignment. We thus have confidence that the natural experiment employed in our study indeed does remove the correlation between unobservable parental characteristics that potentially affect both initial DHA location and subsequent outcomes for children and youth.

However, the longer time passes since initial assignment two potential sources of geographic selection based on parental unobservables arise. First, DHA households can voluntarily transfer between dispersed housing and conventional public housing developments. This occurred rarely, however. Of the post-1986 vintage tenants now residing in conventional public housing developments, 99 percent were originally placed in such; only one percent moved in from dispersed housing. Of the post-1986 vintage tenants now residing in dispersed housing, 94 percent were originally placed in such; six percent moved in from the conventional developments.⁴

Second, a larger source of selection arises because a substantial part of our survey involved households no longer residing in DHA housing. Although their initial assignment to a neighborhood by DHA was effectively random, their subsequent move(s) out of DHA were likely not. In these cases, the children's cumulative contextual exposures will be a combination of randomly assigned and selectively (to some degree) assigned neighborhood characteristics (analogous to experimental households in MTO). To the extent that the former contexts are sufficient to rupture the correlation between unobservable parental characteristics affecting high school outcomes and neighborhood characteristics experienced by their children, our estimates of neighborhood effects will not be substantially biased. To investigate this assumption, we will replicate our analyses using only those youth who spent a majority of their high school years residing

⁴ Moreover, an unknown number of these transfers were involuntary, required by regulations after changes in family size or composition.

in DHA housing, i.e., a randomly assigned neighborhood. This replication is not without potential bias, however, since there may be selection on other unobservable characteristics that leads certain families to remain in their DHA dwelling for an extended period.

A further important feature of our natural experiment is the comparatively long exposures children in DHA households had to their assigned neighborhoods. Our sample had a 6-year mean (5 median) DHA residential duration, approximately twice as long as reported for the MTO experimental group (mean = 2.7 years; median = 3.3 years).

The use of natural experiments inevitably raises questions about generality of results. We believe that our findings can fairly be generalized to low-income, Black and Latino families who apply for and remain on the waiting list long enough to obtain public housing. As such, it may not be fully generalizable to the population of minority families who obtain subsidized rental housing, and certainly may not be to the larger population of minority families who qualify for housing assistance. Nevertheless, it is similar to--yet considerably more general than--the populations forming the samples for the various Gautreaux-, Yonkers-, and MTO-based scholarly studies noted above.

Data Collection in Denver

Denver Child Study Survey of Current and Former DHA Households

We developed and fielded during 2006-2008 the Denver Child Study, a telephone survey (sometimes done in person for those who had no phones) that collected retrospective and current information about the household, adults and children. Detailed information related to multiple domains of outcomes was gathered for all eligible children associated with each household. Each household's residential mobility history was obtained so it could be associated with neighborhood developmental context for children. Study eligibility criteria were: (1) presence of children in the home between ages 0 and 18 years when they moved into DHA; (2) family remained in DHA housing for at least two years; and (3) family first entered DHA in 1987 or later (when DHA's current quasi-random assignment process came into operation); and (4) Black or Latino ethnicity identified.

Attempts to recruit subjects for the study were made by mail and phone, in both English and Spanish when appropriate. Compensation for participation took the form of

either cash or gift card. We estimate a participation rate of 56.5 percent, with most-non-participation due to our inability to locate the household; less than 6 percent refused to participate once contacted. Our team successfully completed 736 interviews with the parents or primary caregivers of eligible households that were retained for analysis after quality control. Details of sampling, participation rates, and profiles of eligible and participating households are available from the authors.

Youth analyzed as part of this study were (current or past) members of these 736 households who were ages 18-25 by the time of our survey (N=421 with information on all variables used in multivariate analyses). As discussed above, we replicated analyses for a subsample of this group who had spent a majority of their high school years residing in DHA housing (N= 264, with complete information).⁵

Our Denver Child Study survey collected information on a wide variety of parental/caregiver and household characteristics that we employed as controls; these are listed in Table 2. This included conventional information about caregiver's national origin, education, disabilities, economic status, and marital, fertility, substance abuse, and employment histories. The survey also asked parents/caregivers to assess the strength and type of influence that they and significant others had on the youth (these variables are scaled from -2 to 2, with -2 indicating very negative and 2 indicating very positive). The survey also asked questions that permitted us to compute reliable scales of parental/caregiver depression (CES-D)⁶, traditional parenting style, and parenting efficacy; details are available from the authors. We also were able to measure a series of household events (like eviction, inability to pay bills) from which we created a "household economic stressors index." We also measured the number of additional workers in the household during the child's high school stage (besides the parent who responded to the survey and the child), the number of younger siblings, and any periods of homelessness.

As far as parent and household characteristics for young adults between the ages of 18 and 25 are concerned, slightly less than 10 percent of parents were disabled while the sampled children were in high school (in the full and DHA-in-HS samples).

⁵ Two cases were deleted from the full sample and three cases were deleted from the DHA-in-HS sample because they were identified as multivariate outliers using the Mahalanobis distance technique. These were cases that exceeded the chi-square critical value of 74.75 with 41 degrees of freedom.

⁶ We use two dummy variables for our regression analyses indicating whether the respondent was clinically depressed (score of 27 or higher on the CES-D scale) or exhibited sub-clinical depression (score between 16 and 26 on the CES-D scale).

Around 24 percent of respondents were teen parents, while around 60 percent had a high school diploma or more education. Parents surveyed worked an average of 21 hours per week while sampled children were in high school. The average number of younger siblings present in households was around 1.3. Concerning the influence that certain family members had on sampled children, fathers had the most negative influence (with a mean in the full sample of -0.047), while mothers had the most positive influence (with a mean in the full sample of 1.688).

[Table 2 about here—youth & family descriptive stats]

Characteristics of Young Adults Analyzed and Their Labor Market Outcomes

The Denver Child Study survey asked parents/caregivers to supply information about all their children with whom they had lived in DHA public housing for at least one year. In this manner we collected detailed information about residential histories (including living outside of the parental home), health, exposure to violence, behaviors and activities, marital/fertility histories, education and (for older children), labor market outcomes during early adulthood. The youth characteristics we analyzed as control variables and their labor market outcomes analyzed are listed in Table 2. These controls include demographic characteristics such as gender and ethnicity, whether the individual had any severe and permanent cognitive-neurological functioning problems (such as mental retardation), and the percentage of years the individual spent outside of the household during high school (such as in foster care or juvenile detention). Finally, we controlled for the individual's age (and, implicitly, for the sorts of macroeconomic prospects faced at that time) by specifying a series of "vintage" dummy variables corresponding to period when the individual turned 18 years old.

In this study we focus on two labor market outcomes for young adults aged 18-25. The first is "*productive activity*," which we define as being employed (either part- or full-time) or enrolled in post-secondary education/training. This outcome was specified on the basis of the parent/caregiver survey respondent's mutually exclusive categorical response to the question, "Since turning 18, has [youth] primarily been working full-time, working part-time, not working but attending school, or neither working nor attending school?" The second was "*received public assistance*," assessed by the response to the question, "Since turning 18, has [youth] received public assistance (during ages 18-25)?" These two outcomes were not mutually exclusive. In the full sample, 76 percent were productive and 12 percent received public assistance between

the ages of 18 and 25. In the sample which resided in DHA during the majority of their high school years, 74 percent were productive and 8 percent received public assistance between the ages of 18 and 25.

Characteristics of Neighborhoods Experienced During Teenage Years

We obtained a wide variety of neighborhood data from two sources; see Table 3. The first was the decennial U.S. Census, where we used census tract geographic scales from 1970, 1980, 1990 and 2000 censuses. We employed the *Neighborhood Change Data Base* (a Geolytics proprietary product) for this information because it adjusts figures to account for potential changes in tract boundaries between decennial censuses. For estimates of non-census year data, we used linear interpolation or extrapolation. We gathered indicators that have been widely employed in prior research on neighborhood effects, including percentages of: female-headed households, poor families, unemployed adults, those with college degrees, non-Hispanic black population, Hispanic population, foreign-born population, and home owners, and an index of occupational prestige based on the observed distribution of occupations of employees.⁷

The second source of neighborhood information was subjective indicators based on responses of the households interviewed in our Denver Child Study.⁸ For each neighborhood in which they lived while they were raising children, we asked the parent/caregiver to respond to a battery of questions related to the location's assets and liabilities. From the responses we devised two indices of neighborhood social capital and social disorder problems, a dichotomous measure of bad peer influences, and three dichotomous measures of whether their children had used parks / playgrounds, mentoring / counseling centers, and indoor recreation facilities located there. The social capital index (range 0-6) was incremented by "one" for each of the following respondent descriptions of people in the neighborhood: could get together to solve neighborhood problems; would watch out for their children and property; knew them and their children by name; they and their children could look up to them; or could be counted on in times of trouble, and whether the respondent participated in any organizations located in the neighborhood (e.g., block clubs, tenant groups, religious organizations and the like).

⁷ The prestige index weights the proportions of each occupational group by its corresponding General Social Survey cardinal prestige score. A neighborhood with all professional and technical workers would score 60.5; one with all laborers would score 17.

⁸ Recent research has shown that such subjective information based on resident's perceptions of neighborhoods provide important additional explanatory power in modeling a variety of economic outcomes (Furtado, 2011).

The social disorder index (range 0-5) was incremented by “one” for each of the following conditions: people selling drugs; gang activity; homes broken into by burglars; people being robbed or mugged; people getting beaten or raped. Both indices proved reliable; details are available from the authors.

We experimented with interactions between the neighborhood social capital index and: neighborhood occupational prestige, unemployment rates and college-educated rates. The economic impact of strong connections with neighbors should not be independent of resources that these neighbors can command and potentially transfer, the norms they promulgate, and the role models they provide. The interaction with occupational prestige proved most efficacious and will be reported henceforth.

[Table 3 about here-neighborhood characteristics descriptives]

These neighborhood indicators serve as proxies for the aforementioned causal processes as follows:

- *Socialization*: occupational prestige, college-educated rate, percentage foreign-born population, teens in trouble (re: values, role models and peers); social capital index (re: degree to which local socialization forces will be influential)
- *Networks*: social capital index (re: local connections); occupational prestige; unemployment rate; college-educated rate (re: quality of resources local networks provide); interactions between social capital and the others
- *Social Disorder*: home ownership rate, rate of moving in during prior year (re: social control and collective efficacy); neighborhood problems index
- *Exposure to Violence*: neighborhood problems index
- *Competition*: occupational prestige
- *Stigmatization*: occupational prestige; unemployment rate; college-educated rate, percentage non-white population, percentage foreign-born population
- *Institutional Resources*: parks; indoor recreation centers; counseling & mentoring centers

Creation of Analytical Databases

We spent considerable effort cleaning, reconciling and augmenting the survey data. When our audits revealed inconsistencies or omissions in the responses, we attempted to contact respondents again and seek clarifications. Information provided by respondents on their residential histories was cross-checked with residential location

information contained in the DHA administrative databases and Lexis-Nexis files.

Once residential history information obtained on the survey was verified for accuracy, we geo-coded each address, using the U.S. Bureau of the Census' *American FactFinder* website utility. In cases where respondents could not recall specific addresses but only proximate cross-streets, we verified these locations using MapQuest and then identified the corresponding census tract using the aforementioned Census website showing tract boundaries. This procedure provided the census tract corresponding to each location in respondents' residential histories, which, in turn, permitted us to match each location to the aforementioned battery of neighborhood indicators for census tract neighborhoods. We were able to successfully link 92 percent of the residential locations listed by respondents.

All these data for households and neighborhoods were then transformed into the format of a *child-year unit of observation*. For each child-year there are variables associated with: (1) fixed child characteristics [C], (2) fixed parental characteristics [P]; (3) temporally varying child characteristics [C_t]; (4) temporally varying parental-household characteristics [P_t]; (5) temporally varying neighborhood characteristics [N_t]; (6) temporally varying outcomes [O_t]. We aggregated information from child-years 15-18 into a "high school stage" unit of analysis for use in our statistical work here, typically by averaging values for the relevant child-years; for our timing analysis we did a similar process for child-years 11-14, a "middle school stage."

Analytical Approach

Our dichotomous models are estimated with Stata's maximum-likelihood logistic regression algorithm. Because our youth are clustered into families (N=276 for the full sample and 185 for the majority-of-high school-in-DHA subsample for the public assistance outcome and N=276 for the full sample and 189 for the majority-of-high-school-in-DHA sample for the productive outcome), we use clustered robust standard errors.

We intentionally omit from these labor market outcome equations any variables describing the youths' mental and physical health, exposure to violence, fertility, educational attainments, or other behaviors, inasmuch as many of these may themselves be affected by neighborhood context. In this fashion we avoid "over-controlling" and thus minimizing the apparent influence of neighborhood. We can

therefore interpret our models as akin to yielding “reduced form” estimates of the degree to which neighborhood variables correlate with young adult labor market outcomes, both directly and indirectly through intervening causal pathways.

Our core concurrent model operationalizes most neighborhood variables as *average* conditions experienced while the given youth was in high school (the exception being dichotomous variables: presence and use of local facilities and peers getting into trouble), and tests for relationships across all youths in the particular sample. Some census tract indicators could not be included in the analyses because they were too collinear with included variables to meet tolerance standards. In all cases, the included variables in the models reported below are not multicollinear according to the standard criteria. We will experiment with stratifications according to gender and to ethnicity, and neighborhood context measured with a lag (i.e., during middle-school stage).

Results

Core Model

Estimated logit model parameters and odds ratios for concurrent neighborhood effects models are presented in Tables 4 and 5, for both ever-in-DHA and currently-in-DHA high school samples. Table 4 reports results from the model investigating productive work/school outcomes between the ages of 18 and 25. Table 5 reports results from the model predicting public assistance use between the ages of 18 and 25. Overall, the models perform well (pseudo-R squares in the .20-.28 range for productive outcomes and .35-.52 range for welfare) and numerous statistically significant predictors including several neighborhood characteristics. Crucially, the parameter estimates for the neighborhood variables are virtually identical for both samples (though standard errors are higher for the smaller sample), suggesting that there is little bias being introduced by the selection of households either out of DHA or their persistence in DHA since their initial quasi-random assignment.

[Table 4 about here]

Consider first the results for the control variables in the models predicting productive outcomes. In both the full sample and the DHA-in-High-School sample there were several consistent predictors of a productive work or school results between the ages of 18 to 25: turning age 18 in 2003, parenting beliefs scale, parent having sub-clinical depression, and mother’s influence. Young adults coming of age in 2003 were

significantly less likely to have a productive labor market outcome. In both samples, these individuals had about 83 percent lower odds of having a productive outcome than those in the reference category (youths who came of age prior to 1997). When parenting styles are one standard deviation more traditional, their children have about 45 percent higher odds of having a positive labor market outcome. Surprisingly, when respondents were assessed as having sub-clinical depression at the time of the survey, their children were significantly more likely to have a productive work outcome than those with non-depressed parents. This positive association may be a spurious one, however, since the timing of this productive activity may be totally unrelated to the caregiver's state of mental health at the time of the survey. A mother being assessed as a more positive influence was associated with productive labor force outcomes. For the full sample, youths with a unit-higher mother's positive influence had 66 percent higher odds of having productive outcomes, while the corresponding figure in the DHA-in-HS sample was 153 percent.

[Table 5 about here]

Next, consider the results for the control variables in the models predicting a public assistance outcome. For this outcome there were only two variables that were significant across both samples: cognitive/neurological disability and mother's influence. The relationships between these variables and the dependent variable were similar in direction between the two samples, but the magnitude of the relationships was stronger for both variables in the DHA-in-HS sample. In the full sample, youths with a cognitive or neurological disorder had 369 percent higher odds of receiving public assistance between the ages of 18 and 25, but those youths in the DHA-in-HS sample with the same condition remarkably had 2216 percent higher odds of receiving public assistance. A mother's positive influence had a negative relationship with receiving public assistance in both models. In the DHA-in-HS sample, a unit increase in the mother's positive influence decreased the odds of receiving public assistance by 87 percent, while in the full sample the reduction in odds was 63 percent. It also should be noted that females of both ethnicities were much more likely to have received public assistance, although the parameter estimates in the DHA-in-HS sample fell just short of the five percent significance threshold.

More central to this paper are the results for the neighborhood context variables. We found that the neighborhood social capital index, the neighborhood occupational prestige index, and their interaction to be consistently most predictive. In the productive

early adult outcome model, this trio of variables proved to be the only neighborhood context variables that were consistently statistically significant in both samples and, encouragingly, of virtually identical magnitudes in both samples as well. The signs and magnitudes of these three coefficients collectively indicate that the marginal effect on the probability of a productive outcome associated with increases in social capital will be positive when prestige is greater than 37.5, slightly higher than its sample mean of 37. Analogously, the marginal effect on the probability of a productive outcome associated with increases in neighborhood prestige will be positive if social capital is greater than 3.75, about a third of a standard deviation above the sample mean of 3.2. These relationships are substantial in magnitude, as the following illustrates for hypothetical individuals possessing sample mean values for all variables (except for neighborhood social capital and prestige). Were they to live in a neighborhood with one standard deviation *above* the mean level of prestige and with one standard deviation *below* the mean level of social capital, their predicted probability of a productive outcome as a young adult would be .66. In a context where the relative-to-mean values of these variables were reversed, the predicted probability would be .69. If *both* prestige and social capital were one standard deviation above their respective means this probability would be .80. If both were two standard deviations above their respective means this probability would be .89, 17 percent greater than the observed sample mean.

In the case of welfare usage outcome, far fewer neighborhood variables proved statistically significant in either sample, and none were consistently significant in both samples. An analogous pattern of interaction effects between prestige and social capital did, however, appear for the DHA-in-HS sample (with all signs reversed compared to the productive outcome model, as is appropriate for this negative labor market outcome). Remarkably, signs and magnitudes of these three coefficients collectively indicate that the region where the marginal effect on the probability of a welfare outcome associated with increases in both social capital and prestige will be negative is virtually identical to that calculated above for productive outcomes.

Variations on the Core Model: Timing

We undertook several explorations to check the generality and robustness of the neighborhood effects observed in the models discussed above. First, we investigated the temporal pattern of exposures, given recent heightened attention to this dimension of neighborhood effects (Galster, 2012; Musterd, Galster and Andersson, forthcoming).

We tested a lagged model where the neighborhood context variables were measured at the middle school stage. While this lagged timing model produced results with many significant neighborhood context variables, the stronger neighborhood effects (as far as both coefficient magnitude and levels of significance are concerned) were produced by the concurrent model. We recognize that some neighborhood contexts experienced earlier in childhood may produce durable impacts (Sampson, Sharkey, and Raudenbush, 2008) but these are not explored in this study.

Variations on the Core Model: Ethnic and Gender Differences

As our second exploration we stratified the core models, first according to gender, and then by ethnicity.⁹ For space considerations we provide summaries of main themes here that are robust across both samples; details are available from the authors. The aforementioned interaction patterns for neighborhood social capital and prestige predicting productive outcomes was replicated for both young adult men and women in our sample. The statistical significance and magnitudes of estimates were virtually the same for both genders. Such was not the case, however, in the case of ethnic stratifications. Latinos strongly evinced the aforementioned patterns but none emerged in a statistically significant way for Blacks in our sample. No noteworthy inter-strata differences in results for neighborhood variables emerged when the model of welfare usage was estimated.

Discussion

Our findings suggest that the degree to which a household is strongly embedded in the local neighborhood—in combination with the occupational prestige of the employees in that neighborhood—influences adolescents in ways that soon appear as their young adult behaviors related to work, post-secondary education, and welfare. These results have intuitive appeal from the perspective of local networks, norms and role models related to these behaviors. Strong ties to high prestige neighborhoods likely mean that adolescents are being intensely exposed to norms and role models that encourage work and post-secondary education, and/or to networks of information about

⁹ In the full sample the Ns for these strata were as follows. For the productive outcome: 192 African Americans, 237 Hispanics, 213 Males, and 216 Females. For the public assistance outcome: 185 African Americans, 214 Hispanics, 158 Males, and 213 Females.

these productive opportunities. By contrast, strong household ties to low-prestige neighborhoods may subsequently produce less productive behaviors because the norms and role models to which adolescents are being exposed are less supportive of productive activities and their neighborhood networks may provide fewer resources assisting in these activities.

These results are consistent with those produced by recent research on both the MTO and Gautreaux programs. Though MTO revealed little economic impact on adults who moved to low-poverty (presumably, higher prestige) neighborhoods, subsequent qualitative analysis revealed that this likely was because they often did not activate meaningful ties or possess meaningful structures for developing such ties in their new neighborhoods; moreover, they maintained pre-established (resource-poor) networks in their low-prestige former neighborhoods (Briggs, Popkin and Goering, 2010; Briggs et al, 2011). However, some parents in advantaged neighborhoods stressed the value of adult role modeling of work habits for their teens and the “soft skill” enhancement that improved their employment prospects. This mimics results from Gautreaux that showed how higher economic expectations in advantaged neighborhoods positively influenced lower-income teen in-movers (Rosenbaum, DeLuca and Tuck, 2005). Thus, our results align with those from MTO and Gautreaux in the implication that moving low-income minority households to more advantaged neighborhoods may be successful in improving labor force (and educational) opportunities for teens, especially if residence there can be sustained and social capital built.

Conclusions, Caveats, and Future Directions

Social scientists have struggled with the daunting methodological challenges of obtaining unbiased estimates of the impact of neighborhood experienced while a teen on young adult labor market outcomes, due primarily to parental geographic selection. An innovative public housing program instituted by the Denver Housing Authority provides a unique opportunity to explore this issue because the DHA mimics a random assignment to neighborhood for families with children who apply for DHA housing. Specifically, for two samples of young adults who lived in DHA housing (one during high school and the other at any time through age 18) we investigated how late-adolescent neighborhood context related to labor market performance during ages 18-25. We found that those who experienced during late adolescence above-average degrees of social capital within

above-average occupational prestige neighborhoods evinced substantially higher probabilities of working or attending post-secondary education as young adults. This relationship: (1) appeared stronger when neighborhood contest was measured during late adolescence compared to early adolescence; (2) was robust for men and women; and (3) stronger for Latinos than Blacks.

We urge caution in interpreting these results, inasmuch as these models make several simplifying assumptions about neighborhood effects. First, we measure average neighborhood conditions experienced during a developmental stage, thus potentially obscuring more extreme conditions that might be present during a few years. Second, we do not investigate the potential durable impacts of early childhood neighborhood environments. Third, we have not explored neighborhood indicators related to environmental pollution or job access. Fourth, though our neighborhood measures are comparatively comprehensive, they do not provide direct measures of the causal processes that may link the distal environment to individual behaviors.

In a similar vein, we have not attempted to probe here potential ways in which neighborhood context may affect teens' exposure to violence, behaviors, nutrition, health, and schooling, which might reveal more about underlying causal mechanisms between the relationships we have observed between teen neighborhood and young adult labor force outcomes. These latter shortcomings will be addressed in future work, both through structural equation modeling and qualitative investigations involving in-depth interviews with teens and their parents who have participated in our survey earlier.

Finally, our results clearly suggest that policymakers should be cognizant of neighborhood as an important developmental context. Early adult performance in the labor market is clearly influenced by more than personal or family characteristics of low-income, minority youth. The daunting policy challenge is encouraging not only access to neighborhood environments that can be more developmentally friendly to such youth, but the development of social capital in these places. Our research suggests that not only location in advantaged places but the social attachment of low-income, minority families who reside there will influence the economic advancement of our least-advantaged youth.

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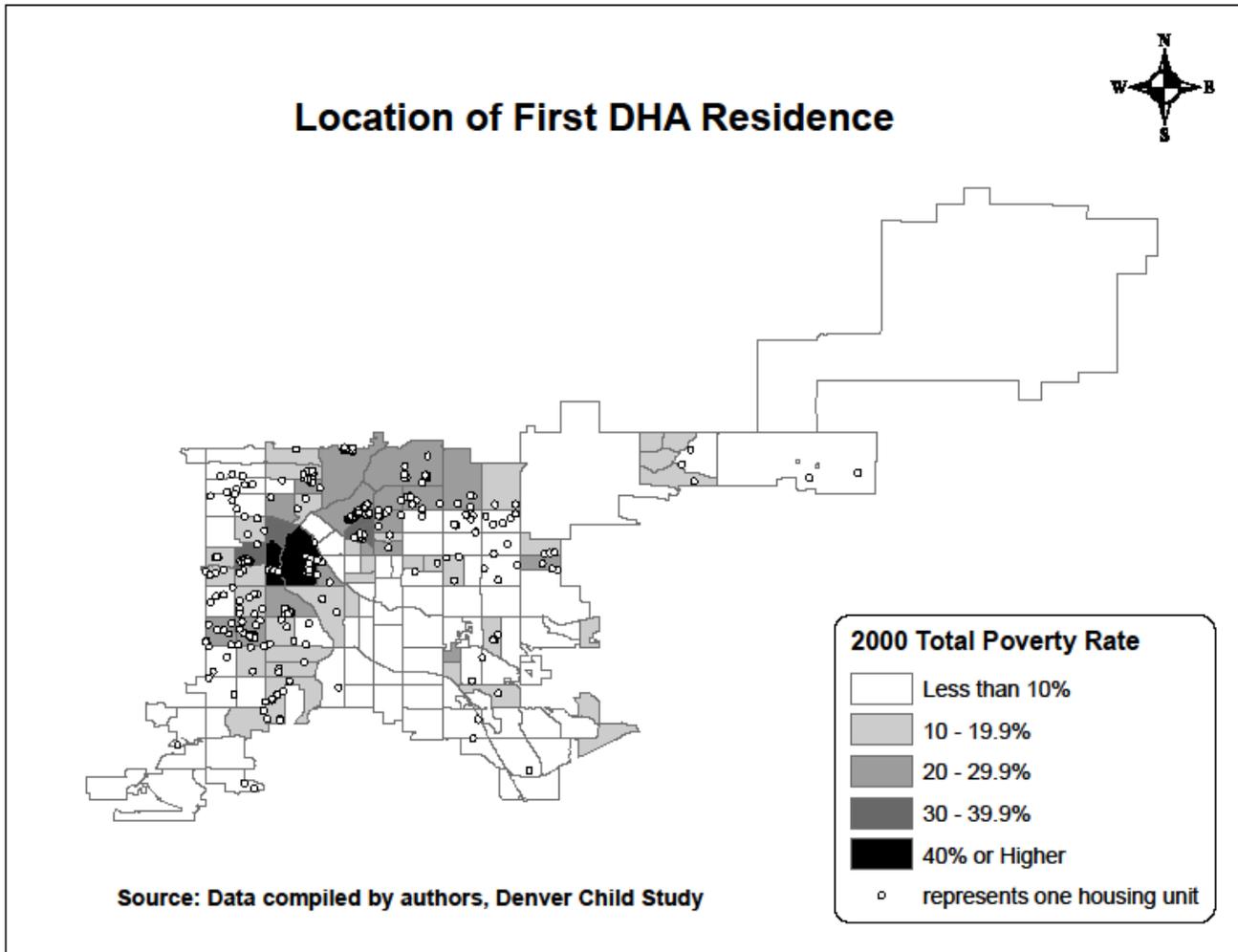
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Figure 1: Locations of Sample Household's First Denver Housing Authority Dwelling



Sources: Denver Housing Authority, Piton Foundation *Neighborhood Facts* database, map by authors

Table 1.
Simulation Results: # of Statistically Significant Correlations between Typically Unobserved Household Characteristics and Neighborhood Characteristics

Household Characteristic	Families with 0-1 Child	Families with 2 Children	Families with 3+ Children
Ever not enough food for family while reside in this location	0	1 (%black=.14)	0
Ever unable to pay all bills while reside in this location	2 (%foreign-born = .13; %vacant = -.16)	2 (%elem. school ed. = -.17; %vacant = -.14)	1 (%vacant = -.12)
Frequency of alcohol use since becoming parent	2 (%unemployed = -.16; %owner =.13)	0	1 (%black = -.09)
Frequency of marijuana use since becoming parent	1 (%black = .17)	0	0
Frequency of drug use since becoming parent	1 (%black = .13)	0	0
Ever seen psychologist, psychiatrist or counselor	0	0	0
Did your parents ever live in public housing when you were growing up	1 (%female heads = .22)	0	1 (%foreign born = -.18)
Did your parents ever own their home when you were growing up	0	4 (%elem. school = .26; %college = -.26; %prof. / tech.= -.32; %own = .20)	0
Born in U.S.	2 (%college = -.16; %prof. / tech. = -.15)	0	0
Primary language is Spanish	0	0	0
Father of child always lived in home while child growing up	0	0	5 (%female heads = -.11; %elementary school = -.10; %poor = -.10; %own = .09; %pre-1939 homes= -.12)
Parental depression (CESD) scale	0	1 (%Hispanic = .13)	2 (%elem. school = .13; %Hispanic=.13)
Parental self-efficacy scale	0	0	0
Parental beliefs & practices scale	0	1 (%Hispanic = -.21)	3 (%college = -.09; %prof. / tech. = -.10; %black = -.12)

Source: authors' calculations based on Monte Carlo simulations of Denver Child Study survey data

Table 2.
Descriptive Statistics of Sampled Families and Young Adults being Analyzed

	FULL SAMPLE			DHA in HS SAMPLE		
	N	MEAN	STD DEV	N	MEAN	STD DEV
<i>Parent & Family Characteristics:</i>						
Parenting Efficacy Scale	450	16.751	3.423	272	16.717	3.488
Parenting Beliefs Scale	450	20.722	3.538	272	20.699	3.352
Parent had a Disability while Child in HS	450	0.096	0.294	272	0.096	0.295
Child's Parent was a Teen Parent	450	0.251	0.434	272	0.235	0.425
Parent Drug & Alcohol Use Scale	450	1.771	2.877	272	1.610	2.468
Parent was Clinically Depressed at time of Survey	450	0.093	0.291	272	0.074	0.261
Parent was Borderline Depressed at time of Survey	450	0.160	0.367	272	0.151	0.358
Parent has HS or more Education	450	0.609	0.489	272	0.581	0.494
Avg Hours Parent Worked per Week while Child in HS	447	21.919	18.914	270	21.013	17.726
Family was Homeless while Child in HS	450	0.062	0.242	272	0.055	0.229
Avg Household Stressors during HS	449	2.596	1.670	272	2.750	1.612
Father's Influence	450	-0.047	1.345	272	0.085	1.307
Mother's Influence	450	1.689	0.723	272	1.724	0.666
Other Male's Influence	450	1.042	0.986	272	1.051	0.982
Number of Younger Siblings	450	1.278	1.330	272	1.493	1.445
Avg # Other Wage Earners in Household during HS	450	0.658	1.098	272	0.616	1.078
<i>Child Characteristics:</i>						
Hispanic Females	450	0.276	0.447	272	0.287	0.453
Black Males	450	0.216	0.412	272	0.176	0.382
Black Females	450	0.229	0.421	272	0.184	0.388
Percent HS years Child Spent Outside Household	450	7.833	23.204	272	2.574	9.738
Child has a Cognitive or Neurological Disorder	450	0.151	0.359	272	0.162	0.369
Vintage 1997 thru 1999	450	0.082	0.275	272	0.092	0.289
Vintage 2000	450	0.109	0.312	272	0.114	0.318
Vintage 2001	450	0.084	0.278	272	0.099	0.300
Vintage 2002	450	0.131	0.338	272	0.129	0.335
Vintage 2003	450	0.093	0.291	272	0.092	0.289
Vintage 2004	450	0.158	0.365	272	0.165	0.372
Vintage 2005	450	0.200	0.400	272	0.184	0.388
<i>Dependent Variables:</i>						
Child Received Public Assistance after Age 18	441	0.116	0.320	266	0.083	0.276
Child Worked or Attended School after Age 18	450	0.760	0.428	272	0.743	0.438

Table 3.
Descriptive Statistics of High School Neighborhoods Experienced by Young Adults being Analyzed

	FULL SAMPLE			DHA in HS SAMPLE		
	N	MEAN	STD DEV	N	MEAN	STD DEV
<i>Neighborhood Predictor Variables:</i>						
Child Used Neigh'd Park during HS	450	0.720	0.449	272	0.757	0.429
Child Used Neigh'd Rec Center during HS	450	0.584	0.493	272	0.629	0.484
Child Used Neigh'd Mentoring Center during HS	450	0.287	0.453	272	0.254	0.436
Avg Neigh'd Social Capital during HS	449	3.187	1.810	272	3.272	1.768
Neigh'd Peers Got into Trouble during HS	450	0.451	0.498	272	0.445	0.498
Avg Neigh'd Problems Scale during HS	449	1.433	1.600	272	1.664	1.705
Avg % Foreign Born present in Neigh'd during HS	432	23.733	10.415	272	24.370	10.463
Avg % Homeowners present in Neigh'd during HS	432	51.924	20.051	272	49.346	20.500
Avg % In-Movers within Past Year in Neigh'd during HS	432	24.663	8.307	272	24.655	7.863
Avg % Hispanic Population in Neigh'd during HS	432	52.125	20.492	272	56.891	19.422
Avg Job Prestige in Neigh'd during HS	432	36.949	2.978	272	36.760	3.205
Avg % Unemployment in Neigh'd during HS	432	8.909	4.794	272	9.551	5.091
Social Capital / Prestige Interaction	447	117.687	67.994	272	120.343	66.213

Table 4.
Logistic Regression Parameters for Productive Outcomes Models

	FULL SAMPLE				DHA in HS SAMPLE			
Number of Observations	429				270			
Wald Chi-Square (41 DF)	64.23*				74.91**			
Log pseudolikelihood	-188.56				-109.092			
Pseudo R-square	0.195				0.284			
Variable	Coeff.	Robust Std. Err.	Odds Ratio	Robust Std. Err.	Coeff.	Robust Std. Err.	Odds Ratio	Robust Std. Err.
Hispanic Female	0.587	0.380	1.799	0.684	0.360	0.542	1.433	0.777
Black Male	-0.175	0.438	0.840	0.368	-0.546	0.687	0.579	0.398
Black Female	-0.250	0.426	0.779	0.332	0.037	0.551	1.038	0.572
% Outside Household	-0.004	0.009	0.996	0.009	0.008	0.019	1.008	0.019
Cognitive/Neuro Disability	-0.893**	0.339	0.410	0.139	-0.879	0.476	0.415	0.198
Vintage 1997-1999	-0.923	0.666	0.397	0.265	0.064	0.961	1.066	1.024
Vintage 2000	-1.272*	0.603	0.280	0.169	-0.898	0.884	0.407	0.360
Vintage 2001	-0.937	0.696	0.392	0.273	-0.501	0.953	0.606	0.578
Vintage 2002	-1.279*	0.542	0.278	0.151	-1.130	0.876	0.323	0.283
Vintage 2003	-1.763**	0.590	0.171	0.101	-1.750*	0.855	0.174	0.149
Vintage 2004	-1.136*	0.574	0.321	0.184	-0.729	0.916	0.482	0.442
Vintage 2005	-0.543	0.528	0.581	0.307	0.138	0.906	1.148	1.040
Parenting Efficacy Scale	-0.003	0.059	0.997	0.058	-0.029	0.072	0.972	0.070
Parenting Beliefs Scale	0.119**	0.043	1.126	0.048	0.124*	0.061	1.132	0.069
Parent Disability during HS	-0.170	0.401	0.844	0.338	-0.393	0.603	0.675	0.407
Teen Parent	-0.501	0.339	0.606	0.205	-1.055*	0.494	0.348	0.172
Parent Drug/Alcohol Scale	-0.042	0.065	0.959	0.062	-0.093	0.105	0.911	0.095
Parent Clinical Depression	0.036	0.487	1.037	0.505	0.239	0.639	1.270	0.811
Parent Borderline Depression	2.102**	0.550	8.184	4.502	2.082**	0.740	8.024	5.940
Parent HS Educ or More	0.439	0.328	1.550	0.509	-0.168	0.426	0.845	0.360
Avg Parent Work Hours during HS	-0.007	0.009	0.993	0.009	0.007	0.011	1.007	0.011
Family Homeless in HS	-1.17*	0.521	0.310	0.162	-0.423	0.723	0.655	0.474
Avg Household Stressors Scale	-0.069	0.102	0.933	0.095	-0.171	0.131	0.843	0.111
Number Younger Siblings	0.134	0.128	1.143	0.146	0.249	0.162	1.283	0.208
Influence of Father	-0.009	0.105	0.991	0.104	-0.137	0.163	0.872	0.142
Influence of Mother	0.505*	0.232	1.657	0.385	0.927**	0.334	2.526	0.844
Influence of Other Male	0.35*	0.152	1.418	0.215	0.293	0.226	1.340	0.303
Avg Other Wage Earners	0.237	0.176	1.268	0.223	0.358	0.230	1.431	0.329
Child Use Neigh'd Park in HS	0.455	0.344	1.576	0.542	0.784	0.525	2.190	1.150
Child Use Neigh'd Rec Ctr in HS	0.132	0.302	1.141	0.344	-0.012	0.421	0.988	0.416
Child Use Neigh'd Mentoring in HS	0.786*	0.368	2.195	0.808	0.177	0.489	1.194	0.584
Avg Neigh'd Social Capital Scale	-2.952**	0.973	0.052	0.051	-3.234*	1.518	0.039	0.060
Peers in Trouble in Neigh'd	0.865*	0.382	2.375	0.908	0.804	0.571	2.235	1.276
Avg Neigh'd Problems Scale	-0.119	0.143	0.888	0.127	-0.072	0.194	0.931	0.181
Avg % Foreign Born	0.021	0.023	1.021	0.023	0.024	0.034	1.025	0.035
Avg % Homeowners	-0.003	0.011	0.997	0.011	-0.038*	0.018	0.963	0.018
Avg % Recent Movers	-0.023	0.028	0.977	0.027	-0.146**	0.047	0.864	0.041
Avg % Population Hispanic	-0.027	0.015	0.973	0.014	-0.036	0.022	0.965	0.021
Avg Job Prestige	-0.316**	0.105	0.729	0.077	-0.293	0.176	0.746	0.132
Avg % Unemployment	0.044	0.040	1.045	0.042	0.066	0.062	1.068	0.066
Social Capital / Prestige Interaction	0.076**	0.026	1.079	0.028	0.081*	0.041	1.085	0.044
Constant	11.130	4.749	68155.2	323692.6	15.293	7.680	4381636.0	33600000.0

Note: * denotes $p < .05$, ** denotes $p < .01$ (two-tailed test).

Table 5.
Logistic Regression Parameters for Public Assistance Outcome Models

	FULL SAMPLE				DHA in HS SAMPLE			
Number of Observations	421				264			
Wald Chi-Square (41 DF)	113.33**				90.78**			
Log pseudolikelihood	-95.114				-36.718			
Pseudo R-square	0.354				0.515			
Variable	Coeff.	Robust Std. Err.	Odds Ratio	Robust Std. Err.	Coeff.	Robust Std. Err.	Odds Ratio	Robust Std. Err.
Hispanic Female	1.976*	0.872	7.216	6.291	2.213	1.745	9.147	15.963
Black Male	0.705	1.084	2.023	2.193	-2.372	2.863	0.093	0.267
Black Female	3.073**	0.878	21.598	18.961	5.167	2.739	175.448	480.513
% Outside Household	-0.002	0.025	0.998	0.025	0.068	0.044	1.070	0.048
Cognitive/Neuro Disability	1.546**	0.464	4.691	2.179	3.142**	0.972	23.156	22.507
Vintage 1997-1999	1.595	1.128	4.928	5.558	0.337	1.472	1.400	2.061
Vintage 2000	0.886	1.102	2.425	2.673	-0.298	1.708	0.742	1.268
Vintage 2001	0.735	1.011	2.086	2.110	-2.502	1.868	0.082	0.153
Vintage 2002	1.549	0.887	4.708	4.176	-0.484	1.554	0.617	0.958
Vintage 2003	0.580	0.961	1.785	1.717	0.853	1.256	2.346	2.946
Vintage 2004	0.442	0.740	1.556	1.152	-0.368	1.264	0.692	0.875
Vintage 2005	0.238	0.692	1.269	0.878	-1.357	1.768	0.257	0.455
Parenting Efficacy Scale	-0.138	0.084	0.871	0.073	0.009	0.155	1.009	0.157
Parenting Beliefs Scale	0.043	0.075	1.044	0.078	0.066	0.144	1.068	0.154
Parent Disability during HS	0.389	0.734	1.476	1.083	-2.174*	1.097	0.114	0.125
Teen Parent	0.396	0.564	1.485	0.837	2.986	1.937	19.798	38.355
Parent Drug/Alcohol Scale	0.011	0.090	1.011	0.091	-0.088	0.212	0.915	0.194
Parent Clinical Depression	1.374	0.709	3.952	2.801	0.855	1.573	2.352	3.701
Parent Borderline Depression	-1.362*	0.638	0.256	0.163	-3.961	3.988	0.019	0.076
Parent HS Educ or More	0.376	0.608	1.456	0.885	-0.238	1.382	0.788	1.089
Avg Parent Work Hours during HS	0.009	0.013	1.009	0.013	0.034	0.033	1.035	0.034
Family Homeless in HS	1.017	0.582	2.766	1.610	1.377	1.793	3.962	7.104
Avg Household Stressors Scale	-0.188	0.147	0.828	0.122	0.191	0.450	1.211	0.545
Number Younger Siblings	-0.455**	0.162	0.634	0.103	-0.514	0.488	0.598	0.292
Influence of Father	-0.051	0.213	0.950	0.202	0.490	0.687	1.633	1.121
Influence of Mother	-1.007**	0.331	0.365	0.121	-2.064*	0.919	0.127	0.117
Influence of Other Male	-0.629*	0.271	0.533	0.144	-0.639	0.445	0.528	0.235
Avg Other Wage Earners	-0.395	0.316	0.674	0.213	0.558	0.471	1.747	0.824
Child Use Neigh'd Park in HS	-0.808	0.544	0.446	0.242	-2.532	1.561	0.080	0.124
Child Use Neigh'd Rec Ctr in HS	-0.040	0.498	0.961	0.479	3.03*	1.249	20.701	25.848
Child Use Neigh'd Mentoring in HS	-0.475	0.495	0.622	0.308	-2.470	1.498	0.085	0.127
Avg Neigh'd Social Capital Scale	0.734	2.011	2.083	4.189	10.255**	3.421	28421.44	97215.85
Peers in Trouble in Neigh'd	-0.496	0.561	0.609	0.342	-4.878	2.587	0.008	0.020
Avg Neigh'd Problems Scale	0.064	0.208	1.066	0.222	1.577	0.978	4.838	4.734
Avg % Foreign Born	-0.046	0.038	0.955	0.036	-0.144	0.183	0.866	0.158
Avg % Homeowners	-0.016	0.017	0.984	0.016	-0.019	0.039	0.981	0.039
Avg % Recent Movers	-0.015	0.042	0.985	0.041	0.058	0.119	1.059	0.126
Avg % Population Hispanic	0.035	0.024	1.036	0.025	0.141	0.147	1.151	0.169
Avg Job Prestige	0.085	0.276	1.088	0.300	1.078	0.616	2.940	1.811
Avg % Unemployment	-0.177*	0.090	0.837	0.075	-0.590	0.340	0.554	0.188
Social Capital / Prestige Interaction	-0.012	0.055	0.988	0.054	-0.268**	0.093	0.765	0.071
Constant	-2.208	10.877	0.110	1.196	-45.915	33.032	0.000	0.000

Note: * denotes $p < .05$, ** denotes $p < .01$ (two-tailed test).

Appendix 4

Neighborhood Risk and Protective Factors for Teenage Childbearing among Black and Latino Youth: An Examination of the Timing and Duration of Neighborhood Effects

Jessica L. Lucero, PhD Candidate, Wayne State University

Anna M. Santiago, PhD, Case Western Reserve University

George C. Galster, PhD, Wayne State University

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Background

Study Purpose

Among industrialized countries, the United States has consistently had one of the highest rates of teenage childbearing (Singh & Darroch, 2000). Recent statistics on teenage pregnancy and childbearing reported by the Guttmacher Institute (2010) indicate that despite a downward trend since their peak in 1990, pregnancy rates began to increase after 2006. Pregnancy rates and birth rates are nearly three times as high for Blacks and Latinos as they are for White females (see figure 1). The city and county of Denver has a birthrate nearly twice as high as the larger state's at 61.8 per 1000 (Colorado Youth Matter, 2010). Colorado is ranked 19th in the U.S. with a birth rate of 43 per 1000, slightly higher than the U.S. average of 40 per 1000 (Colorado Youth Matter, 2010). Although current research on the effects of teenage childbearing no longer supports the notion that bearing children in adolescence is a catastrophic event (Hoffman, 1998), there is still compelling reason to believe that teenage childbearing places mothers and their children at a disadvantage relative to their counterparts who delay childbearing (Ashcraft & Lange, 2006; Fletcher & Wolfe, 2009; Hoffman, 1998).

Since the 1990's there has been an upsurge in the literature on neighborhood effects—enough to constitute several systematic reviews (Booth & Crouter, 2001; Furstenberg, Cook, Eccles, Elder & Sameroff, 1999; Dietz, 2002; Leventhal & Brooks-Gunn, 2000; Galster, 2008; Sampson, Morenoff, & Gannon-Rowley, 2002). Despite a burgeoning literature that compellingly relates concentrated neighborhood disadvantage among other neighborhood effect mechanisms with deleterious outcomes for children and adolescents, there is only a small and emerging body of literature which comprehensively elucidates the varied, specific mechanisms and pathways by which neighborhoods influence individuals (Galster, 2012).

Teen fertility outcomes have been related to a number of structural neighborhood characteristics such as concentrated poverty, residential instability, neighborhood affluence, female employment, and racial/ethnic heterogeneity (Brewster, 1994; Browning, Leventhal, & Brooks-

Gunn, 2005; Crane, 1991; Hogan & Kitigawa, 1985). Unfortunately, many of the studies that have attempted to quantify the neighborhood-level risk factors for teenage childbearing have fallen short methodologically. It is the purpose of this study to overcome a number of these methodological shortcomings in an attempt to quantify more accurately the neighborhood risk and protective factors for teenage childbearing. Framed from an ecological perspective (which posits that children and adolescents are shaped by both proximal and distal influences) and informed by social disorganization and collective efficacy theories, this study investigates the neighborhood contexts associated with teenage childbearing and fathering for Latino and African American adolescents who resided in Denver public housing for a substantial period of time during their childhood. Specifically, we examine the extent to which teenage childbearing or fathering (occurring between the ages of 15 and 19) are statistically related to various conditions in the neighborhoods in which these youth were raised. The purpose of this study is twofold: (1) to determine if neighborhood effects on teenage childbearing/fathering operate differentially for Black and Latino youth; and (2) to estimate how these effects may vary according to the timing and duration of neighborhood exposure. Further, we hypothesize that (a) adolescents who lived in disadvantaged neighborhoods during middle school and high school will be more likely to bear or father children between the ages of 15 and 19 than those who lived in average or advantaged neighborhoods; and (b) adolescents who lived in neighborhoods with higher levels of social capital for the majority of middle school and high school will be less likely to bear or father children between the ages of 15 and 19.

Neighborhood Effects: Theory and Empirical Challenges

Neighborhood effects are generally understood to be the independently causal effect of neighborhood residence on individual outcomes. Dietz (2002) defines neighborhood effects as “community influences on individual social or economic outcomes (p. 539),” and describes pertinent outcomes such as labor force activity, child psychosocial and health outcomes, criminal behavior, and other socioeconomic phenomena. Neighborhood effects on teenage

childbearing may be direct (e.g., as concentrated neighborhood disadvantage increases, probability of teenage childbearing increases) or mediating (e.g., as neighborhood social capital increases, the effects of concentrated disadvantage on teenage childbearing are attenuated). There are several potential mechanisms through which neighborhood context may affect teenage childbearing and fathering. Galster (2012) delineates 15 major mechanisms by which neighborhoods impact individuals. These include social contagion, collective socialization, social networks, social cohesion and control, competition, relative deprivation, parental mediation, exposure to violence, physical surroundings, toxic exposure, spatial mismatch, public services, stigmatization, local institutional resources, and local market actors. All of these mechanisms can be broadly grouped into three categories: 1) social-interactive; 2) behavioral; and 3) geographic. This study draws from theories that would illuminate the social-interactive effect mechanisms (e.g. social cohesion and control, collective socialization, etc). We will discuss evidence of this later.

One of the most formidable methodological challenges that many “ecologically grounded” studies on teen pregnancy and childbearing face when trying to quantify the neighborhood’s causal impact is that of geographic selection bias. Essentially, geographic selection bias refers to the very real possibility that individuals may self-select into neighborhoods based on unmeasured personal characteristics that also affect the outcome being investigated. As a result, the independent effects of neighborhoods cannot be accurately estimated due to the lack of adequate control variables. For example, parents who closely monitor their teenage daughters’ behaviors may move to neighborhoods where they expect to find teen peers who will reinforce the behaviors they wish their daughters to emulate. Without knowing who such parents are, the investigator cannot be sure if the observed behaviors are a function of neighborhood peers or unmeasured, uncontrolled parental characteristics. The *Denver Child Study* offers an unparalleled opportunity to overcome this geographic selection bias by exploiting a natural experiment, as explained below.

Neighborhood Effects as Dosage-Response Relationship

The major methodological thrust of this study is to examine the timing and duration of neighborhood effects, an area of research that has only recently been given serious attention (Musterd, Galster and Andersson, forthcoming). Galster (2012) discusses neighborhood effects in terms of a pharmacological metaphor wherein neighborhood “dose” is related to individual “response.” Essentially this dosage/response considers the “active ingredients” of neighborhood dosage (i.e. social-interactive, institutional, or geographic mechanisms) and how this dosage might play out in terms of duration and intensity. In terms of duration, one consideration is how long the neighborhood dosage continues, and in terms of intensity, another consideration is the size of the neighborhood dosage. Both duration and intensity can be related to the response relationship by considering neighborhood thresholds and timing. For example, if the neighborhood dose of concentrated disadvantage exceeds a meaningful threshold, then one might consider the intensity of this dose to be greater than the intensity of another dose which does not exceed the threshold. In terms of timing, one might consider whether the response to the neighborhood dosage occurs immediately or in a lagged manner following the accumulation of neighborhood dosage. This issue of timing relates to duration of neighborhood exposure across developmental stages. For example, is it possible that a particular intensity of neighborhood dosage at an early developmental stage has a lagged effect on the individual outcome of teenage childbearing in a later developmental stage? Is it the accumulation of effects, or developmentally-specific exposure to neighborhood that matters? These questions are considered in the present study.

Evidence on Neighborhood Context and Teenage Childbearing and Fathering

The academic work on teenage childbearing has evolved from an exclusive focus on individual characteristics (Hofferth, 1987; Mott & Marsiglio, 1985; Moore and Waite, 1977), to considering family attributes (Miller, Benson, & Galbraith, 2001), to including more methodologically sound quasi-experimental studies of individual characteristics (Ashcraft &

Lange, 2006; Fletcher & Wolfe, 2009; Hoffman, Foster, & Furstenberg, 1993; Hoffman, 1998; Geronimus & Korenman, 1993; Hotz, McElroy, & Sanders, 2005), to finally considering the neighborhood context of teenage childbearing (Billy et al., 1994; Brewster et al., 1993; Brewster, 1994; Crane, 1991; Harding, 2003; Ku et al., 1993; Lauritsen, 1994; Plotnick and Hoffman, 1999; Ramirez-Valles et al., 1998; Small & Luster, 1994; Sucoff & Upchurch, 1998; Upchurch, Aneschensel, Sucoff, & Levy-Storms, 1999). Some of the prominent neighborhood risk factors for teenage childbearing that have been empirically identified include: neighborhood affluence (Crane, 1991), female employment (Brewster, 1994) and concentrated poverty (South & Crowder, 1999). Additionally, neighborhoods with high levels of residential instability, crime, and violence generally have higher rates of teenage pregnancy, early sexual initiation, and decreased contraceptive use (Billy, Brewster, & Grady, 1994; Miller et al., 2001; Upchurch, Aneschensel, Sucoff, & Levy-Storms, 1999). Concentrated disadvantage is generally defined as high levels of deprivation restricted to a given geographic space. This construct is often created by an index of neighborhood indicators including some variation of the following: rates of poverty, unemployment, female-headed households, minority households, children under age 18, and households on public assistance (Cohen, Farley, & Mason, 2003; MacDonald & Gover, 2005; Sampson, Raudenbush, & Earls, 1997; Xue, Leventhal, Brooks-Gunn, & Earls, 2005). While the associations between neighborhood deprivation and fertility behaviors are depicted within the literature, the actual mechanisms by which concentrated disadvantage impacts the likelihood of a teen bearing a child are less well known. Below we review a number of prominent studies, provide a methodological critique, and situate our work in the field.

Hogan and Kitigawa's (1985) study was the first prominent study to investigate neighborhood effects on teenage sexual behaviors and pregnancy rates. Their study examined the pregnancy rates and sexual behaviors of Black female adolescents between the ages of 13 and 19 (N=1,078). Their sample was drawn from Chicago neighborhoods, and key neighborhood variables were drawn from 1970 census tract data. Neighborhood predictors

included in their multivariate model included racial composition, median family income, proportion of families below the poverty line and sex ratio. They created a neighborhood index and then categorized neighborhoods by quartiles in terms of neighborhood quality. The authors found that youth living in the most disadvantaged neighborhoods had pregnancy rates 33% higher than those in middle and upper quartiles. Interestingly, when parental supervision was entered in the statistical model, the effect of neighborhood disadvantage on teen pregnancy was attenuated. Essentially, disadvantaged neighborhoods were comprised of parents with diminished parental supervision (possibly a result of neighborhood social disorder) and this lack of parental monitoring and supervision actually mediated the effect between neighborhood disadvantage and increased rates of teen pregnancy. While this study provided an important first step in quantifying neighborhood effects, it only considered Black adolescents, and suffered from geographic selection bias. Additionally, the mediating effect of parental monitoring could be seen as mutually causal (i.e. neighborhood social disorder may lead to weakened parental supervision, but weakened supervision when generalized to all parents in the neighborhood may also lead to greater social disorder).

Crane's (1991) seminal study on the epidemic theory of neighborhood effects on high school dropping out and teenage childbearing heavily informs the current study. Crane utilized 1970 Public Use Microdata Samples to examine the extent to which neighborhood effects are transmitted in a nonlinear manner. Using data for 44,466 Black and White adolescent females residing in urban and rural settings, this study was among the first to propose a "contagion model" of neighborhood effects wherein social problems are spread to others through peer influence and are thus considered contagious. He purports that if "the incidence of problems stays below a critical point, the frequency or prevalence of the problem tends to gravitate toward some relatively low-level equilibrium. But if the incidence surpasses a critical threshold, the process will spread explosively. In other words, an epidemic may occur, raising the incidence to an equilibrium at a much higher level" (p. 1227). His study identified the existence of

neighborhood thresholds associated with teen childbearing. Crane found that when the percent of high status (characterized by percent of employed persons in the neighborhood who held professional or managerial jobs) residents in the neighborhood fell below 3.5, there was a significant upswing in the probability of childbearing for Black and White teens. Additionally, these threshold effects were more pronounced for teens living in urban settings compared to rural settings.

Similar to Hogan and Kitigawa's (1985) study, Sucoff and Upchurch (1998) examined neighborhood effects on childbearing among Black adolescents in urban neighborhoods in Chicago. They found that neighborhood racial composition was the greatest predictor for teenage childbearing inasmuch as teens living in neighborhoods with higher proportions of Black residents were at greater risk of teenage pregnancy regardless of neighborhood socioeconomic status. Further, they found that Black teens living in predominantly segregated neighborhoods (whether impoverished or working class) had birthrates 1.5 times those who lived in racially mixed neighborhoods.

Another prominent study conducted by Plotnick and Hoffman (1999), utilized a fixed effects approach in order to deal with the issue of selection bias. Using data from the Panel Study of Income Dynamics, the authors examined childbearing among pairs of sisters in a nationally representative sample of adolescent females. They specified three models: 1) did not include control variables to represent gross neighborhood effects; 2) included individual and family controls to obtain net effects; and 3) used a fixed-effects approach to control for unobserved family characteristics. The authors found that key census indicators did not have any independent effects on childbearing outcomes. In an effort to understand the influence of selection bias, Harding (2003) used a new method for sensitivity analysis. Using this particular method, he concluded that unobserved factors and personal characteristics would have to be unreasonably strong in order to ameliorate the effects between neighborhoods and high school drop-out and teen pregnancy. The sensitivity analysis assessed the impact of selection bias on

neighborhood effects by comparing identical individuals (in terms of observable characteristics) who live in poor and nonpoor neighborhoods. Additionally, the study used a counterfactual model to compare the neighborhood effects on teenage childbearing and high school dropout among two groups of adolescents who were identically matched on personal characteristics at age 10 using propensity score matching. Using data from the Panel Study on Income Dynamics, Harding found that children who grew up in neighborhoods of concentrated disadvantage were more likely to drop out of high school and experience a teenage pregnancy than children identically matched on observed characteristics but who grew up in more advantaged neighborhoods.

There have been a number of studies reviewed here that have provided a solid framework for continuing this line of questioning, but all of the studies summarized above have been unable to overcome various methodological challenges inherent in neighborhood effects studies. The most glaring methodological weakness of most neighborhood effects studies is that of geographic selection bias. Plotnick and Hoffman (1999) and Harding (2003) both attempt to overcome the challenge of selection bias. However, the former fixed effects study falls short because it fails to address selection based on time-varying family characteristics, and the latter faces several weaknesses, the most apparent being the inadequacy of propensity score matching.

Other studies reviewed here suffer from a number of other methodological weaknesses. First, neighborhood measures were generally inadequate. Often neighborhood is represented by a short list of census indicators (Crane, 1991; Hogan & Kitigawa, 1985; Plotnick & Hoffman, 1999), and other aspects of neighborhood are neglected—especially social-interactive features of neighborhood as perceived by neighborhood residents. Another apparent weakness is the neglect to examine timing and duration of neighborhood exposure (and the potential lagged, cumulative, or contemporaneous effects that may result). For example, Plotnick and Hoffman

(1999) only examine the neighborhood contexts in which the youth in their study resided in between ages 16 and 18.

In an attempt to overcome some of these methodological limitations, this study utilizes an approach that measures appropriate neighborhood characteristics (using both census and survey self reports). Further, this study overcomes the challenge of selection bias by exploiting a natural experiment which mimics randomization. Finally, this study utilizes a comprehensive residential history that allows for an exploration of lagged or contemporaneous effects of neighborhood residence on teenage childbearing/fathering. In summary, our study advances our understanding of teenage childbearing and fathering by answering the following questions:

- (1) What neighborhood factors are associated with teenage childbearing and fathering?
- (2) Are these associations stronger when measured during middle school or high school ages?
- (3) Do these factors vary by gender, race/ethnicity?
- (4) Are there threshold effects for neighborhood disadvantage?
- (5) Do these thresholds operate differentially by gender, race/ethnicity?

Method

Denver Child Study Design

Data sources include (1) survey data from parent/caregivers; (2) administrative data from the U.S.Census Bureau and the Piton Foundation; and (3) 84 in-depth interviews with caregivers and their young adult children. The *Denver Child Study* is a large-scale, mixed-methods study of current and former residents of the Denver (CO) Housing Authority (DHA). Since 1987, DHA has been randomly assigning individuals on their waiting list to the first available housing unit which matches their family's physical needs (i.e., number of bedrooms, etc.). Through preliminary statistical simulations, this process has been found to mimic random assignment, and thus provides a unique opportunity to observe neighborhood effects in the

context of a natural experiment (Galster & Santiago, 2008). Between the years of 2006 and 2008, surveys were administered to 809 parent/caregivers (736 after quality control) and their 1,856 eligible children. Surveys were administered over the phone or in-person, and participants received \$60 for compensation. Beyond reporting household characteristics, perceptions of neighborhood quality, and personal characteristics, parent/caregivers provided retrospective information on all eligible children's health, behavior, education, exposure to violence, and employment. Additionally, complete residential histories of each eligible child's life were obtained beginning at time of birth.. Parent/caregivers provided physical addresses for all locations that their child(ren) lived in since the oldest eligible child's birth. Addresses were then geocoded, and through a complex linking process, Census and Piton neighborhood data for each year of each child's life were generated. These data provide a rare opportunity to comprehensively examine neighborhood exposure. In some cases there were only a couple of residential locations; however, there were up to nineteen residential locations for some families.

Study Sample

The unit of analysis for this study is the child, though family characteristics are controlled for in all of the statistical models. Because the outcome of interest for this study is teenage childbearing and fathering between the ages of 15 and 19, the full sample was significantly reduced to include only those children who were at least 15 years old at time of the survey. This exclusionary criteria provides a sample of N=708, wherein 52% are female and 55% are Latino. Forty-eight percent of the sample was between the ages of 15 and 19 at the time of the survey, while the remaining 52 % was between the ages of 20 and 36. One third of the sample was under the age of 18 at time of survey (but over the age of 15) and thus residential histories for these participants only extend to age 15, 16, or 17. The average proportion of time spent with two parents in the home was .37.

In terms of neighborhood disadvantage, this sample predominantly lived in disadvantaged to average neighborhoods (relative to the Denver metro area) during middle

school and high school. A greater proportion of Black youth tended to live in disadvantaged neighborhoods, and a greater proportion of Latino youth tended to live in average neighborhoods in middle school and high school. Less than 10% of both racial/ethnic groups lived in advantaged neighborhoods in middle school and high school (see Figure 2 for detail). Other pertinent sample characteristics are summarized in Table 1.

Study Measures

Dependent variable. The dependent variable for this study is teenage childbearing or fathering. Conceptually this refers to whether an adolescent gave birth or fathered a child during her or his teen years. Given that the majority of the literature focuses on childbearing between the ages of 15 and 19, this study will use this definition. Operationally, the outcome was measured dichotomously as 0=never birthed or fathered a child between the ages of 15 and 19 or 1=birthed or fathered a child between the ages of 15 and 19.

Individual variables. Parent/caregivers were asked to indicate the age at time of survey as well as the gender for each of their children on the survey. Child age is operationalized as age at time of survey in years. Gender is operationalized as a dichotomous variable, either male or female. Similarly ethnicity is operationalized as either Black or Latino. The small portion of parent/caregivers who indicated that the ethnicity of their child was something other than Black or Latino was excluded from the study sample.

School involvement represents the extent to which an adolescent participates in school-related activities. Operationally school involvement is defined as a dichotomous variable in each developmental stage (elementary, middle school, and high school) of yes or no to two separate survey items: (1) Did your child ever participate in clubs or activities in school?, and (2) Did your child ever participate in sports teams at school?. This variable was dummy coded for both middle school and high school developmental stages as 1=participated in clubs/activities and/or sports in developmental stage. School success was represented as having been on the honor roll during middle school or high school developmental stages.

Religious participation is conceptualized as the extent to which an adolescent attended religious services or activities. Operationally, this is measured with one survey question, “did your child ever attend religious services/activities?” This variable was dummy coded for both middle school and high school developmental stages as 1=attended religious services during developmental stage, 0=did not attend religious services in developmental stage.

Pubertal timing is conceptualized as the degree to which a child enters puberty later or earlier than usual. Operationally, this was measured using one survey item wherein parents were asked if their child(ren) was “early, on time, or late in reaching puberty?” Due to small cell counts, responses were dummy coded (for early and on time/late), and the reference group is ‘reached puberty on time/late.’

Family variables. Parent/caregiver depression is conceptually defined as the extent to which a parent/caregiver exhibited depressive symptomology in the past week at the time of survey. In order to assess depression, the Center for Epidemiologic Studies Depression scale (CES-D) was used (Radloff, 1977). The scale is based on 20 questions about the emotions a person has felt over the past week. Overall scores range from 0 to 60; with scores less than 16 indicating no depressive symptoms, 16 to 26 indicating sub-clinical depression and scores of 27 or higher indicating clinical depression (Santiago, Cutsinger and Galster, 2011). In this study, two dummy variables are used to indicate sub-clinical and clinical depression. The Cronbach’s alpha for the CES-D scale with this study population was .87 (Santiago, Cutsinger & Galster, 2011).

Household income was conceptually defined as the average annual dollar income reported by parent/caregivers during each developmental stage. Operationally, annual income was calculated by multiplying hourly wage rate by hours worked per week and weeks worked per year for each residential location. This, in turn, was matched to the appropriate child years and then household income was averaged across child years in a specific developmental stage.

Parent/caregiver educational attainment was estimated using self-reported highest degree earned at time of survey completion. This variable is represented by four dummy variables: GED, high school diploma, technical/certificate, and college degree. Less than a high school diploma is the reference category.

Parenting efficacy is conceptually defined as the extent to which a parent is confident in his or her ability to parent effectively. Operationally this is measured using a 10-item scale developed by Santiago which asks parents to rate their confidence in their parenting skills using a 3-point Likert scale. The scale ranges from 0 to 30 with higher scores indicating higher levels of parenting efficacy. The Cronbach's alpha for this scale was 0.87 (see Santiago, Cutsinger & Galster, 2011).

Parenting style is conceptually defined as the extent to which a parent/caregiver adheres to egalitarian or authoritarian parenting. This is assessed using a 5-item scale which asks parents to rate their beliefs about how children should be raised on a 5-point Likert scale ranging from 0=doesn't describe me to 4=describes me perfectly.

Residence in a two-parent household is defined as the proportion of time within each developmental stage that the child lived with two parents. Middle school (ages 12 through 14) had proportions ranging from .0, .33, .66, and 1.0. High school had proportions of .0, .25, .5, .75, and 1.0.

Maternal teen parent is defined as a child's mother having given birth to a child between the ages of 15 and 19. Sibling teen parent was defined similarly. Both maternal teen parent and sibling teen parent were dummy coded as 1=mother(sibling) gave birth between ages 15 and 19; 0=otherwise.

Key neighborhood predictors. Social capital is operationalized by a 6-item index that identifies the degree to which a parent/caregiver could rely on his or her neighbors in times of need. Items asked responded if there were people in the neighborhood (1) who could get together to solve neighborhood problems; (2) who would watch out for their children and

property; (3) who knew them and their children by name; (4) were adults who they and their children could look up to; or (5) were people they could count on in times of trouble. The sixth item asked if respondents were active in any organizations located in the neighborhood (e.g., block clubs, tenant groups, religious organizations and the like).

Scores range from 1 to 6, higher scores indicated greater levels of social capital. Average levels of social capital were calculated for middle school and high school developmental stages.

Parent/caregiver perceived neighborhood problems was defined by a 5-item index that measured the level of social disorder (i.e. selling drugs; gang activity; homes broken into by burglars; people being robbed or mugged; people getting beaten or raped) experienced within one's neighborhood. Responses were either yes or no for each of the five items, resulting in a range from 0 to 5, with higher scores indicating higher levels of social disorder. Average levels of neighborhood social disorder were calculated for middle school and high school developmental stages.

The presence of negative peer influences in the neighborhood was measured by one survey question that asked if "there are many teens who get into trouble" in the neighborhood. This was dummy coded as 1=yes; 0 otherwise.

Residential instability (percentage of homes moved out of in the census tract in the past year), foreign born (percentage of neighborhood residents born outside the U.S.) and children aged 5-17 (percentage of neighborhood residents who are between the ages of 5 and 17) were assessed with Census data linked to the neighborhood(s) in which children lived during both middle school and high school. Average percentages of residential instability, foreign born, and children aged 5-17 were calculated for middle school and high school developmental stages.

Due to high collinearity, neighborhood percentages of poverty, unemployment, proportion minority, female headship, and rental tenure were summed to create a neighborhood disadvantage index for each year of each child's life. In order to begin to examine potential thresholds for disadvantage, we computed a mean disadvantage index for the entire Denver

metro area using census data for the years that corresponded to the survey (between 1970 and 2008). Disadvantage indices for each child were then compared to metro means in the corresponding years, and if it was within one standard deviation of the mean, it was coded as 'average'. More than one standard deviation above the mean was coded as 'disadvantaged and more than one standard deviation below the mean was coded as 'advantaged.' Finally, in order to compute a disadvantage typology for both middle school and high school developmental stages, developmental stages were dummy coded as 'all or majority advantaged,' 'all or majority average,' or 'all or majority disadvantaged.'

Analysis

Using Stata's logistic regression with a clustered robust adjustment to address clustering at the family level, two models were specified. Both of these models were stratified by ethnicity.

1. $\text{logit}[O_i] = \beta[C_{iMS}] + \beta[C_i] + \beta[P_{iMS}] + \beta[P_i] + \beta[N_{jMS}]$
2. $\text{logit}[O_i] = \beta[C_{iHS}] + \beta[C_i] + \beta[P_{iHS}] + \beta[P_i] + \beta[N_{jHS}]$

Where:

[O_i] = outcome of interest (teenage childbearing or fathering between the ages of 15 to 19)

[C_i] = characteristics of youth that can vary over time (e.g., substance use, number of siblings in the home)

[C] = characteristics of youth that do not vary over time (e.g., race, gender, etc.)

[P_i] = characteristics of youth's parent(s) that can vary over time (e.g., marital status, income)

[P] = characteristics of youth's parent(s) that do not vary over time (e.g., race, nativity status)

[N_i] = characteristics of neighborhood where youth resides during time t (e.g., concentrated poverty, residential instability)

i = individual youth and their family

j = neighborhood

MS = developmental stage ages 12 thru 14

HS = developmental stage ages 15 thru 18

Results

Of the 708 youth in the study sample, 20% birthed or fathered a child before the age of 19 (n=145). Twenty seven percent of children had mothers who were teen parents, and 14% of children had older siblings who were teen parents. As the major aim of this study is to examine how neighborhood effects might operate differently for Black and Latino youth, we present the results separately for each racial/ethnic group for middle school and high school developmental stages. All four models performed well with pseudo R square values ranging from .20 to .27. There tended to be more significant neighborhood predictors in middle school than in high school, suggesting that neighborhood effects on teenage childbearing/fathering may be more lagged than contemporaneous. For details on individual- and family-level predictors, refer to Tables 4 and 5.

Black Youth in Middle School

For every one unit increase in average level of parent/caregiver perceived social capital, the odds of teenage childbearing/fathering increased by 28% ($p < .05$). Neighborhood composition of foreign born persons proved to be a protective factor in that increases in average percentages of foreign born in the neighborhood were associated with a 4% decrease in the odds of teenage childbearing/fathering ($p < .05$).

Latino Youth in Middle School

For Latino youth, the significant neighborhood predictors were residential instability and parent/caregiver perceived neighborhood problems. Increases in average percentages of residential instability were associated with a 6% increase in the odds of teenage childbearing/fathering ($p < .05$), and increases in average levels of parent/caregiver perceived neighborhood problems were associated with an 26% increase in the odds of teenage childbearing/fathering ($p < .10$).

Black Youth in High School

Similarly for Black youth middle school neighborhood exposure, increases in average percentages of foreign born in the neighborhood were associated with a 5% decrease in the odds of teenage childbearing/fathering ($p < .05$). No other neighborhood predictors were significant in model 3.

Latino Youth in High School

Parent/caregiver perceptions of neighborhood problems as a risk factor for Latino teenage childbearing/fathering endured as neighborhood risk factor. Increases in average levels of parent/caregiver perceived neighborhood problems were associated with a 44% increase in the odds of teenage childbearing/fathering ($p < .05$). Unlike middle school neighborhood exposure, parent/caregiver perceived negative teen peer influence in the neighborhood during high school was associated with a 60% increase in the odds of teenage childbearing/fathering ($p < .10$).

Even after controlling for a great number of individual and family variables, several neighborhood factors did influence one's propensity to bear or father children during adolescence.. Surprisingly, neighborhood disadvantage was not a significant predictor in any of the models. Social capital only served to be a significant neighborhood predictor for Black middle school neighborhood exposure, and rather than being a protective factor, it was a risk factor.

Discussion

It appears that by stratifying the sample by ethnicity, we see that neighborhood effects are transmitted differentially among Black and Latino youth. Additionally, results suggest that timing and duration of neighborhood exposure substantively matters in predicting the outcome. In terms of ethnic differences, percentage of foreign born in the neighborhood is an important protective factor in middle school and high school for Black youth only. The largest portion of foreign born in the Denver metro area originate from Central or South America (55.7%) with the majority of these folks coming from Mexico. It would logically follow that the strong protective

feature of foreign born neighborhoods that many of these Black youth were residing in was attributable to Mexican immigrant norms. It is possible that Latino immigrant norms of stricter parental monitoring actually extend to the neighborhood by creating a neighborhood social climate that emphasizes adherence to parental rules. Immigrant families often express a desire to provide a better life for their children as their primary reason for coming to the U.S. (Fugligni, 2006). Foreign born parents may strongly desire to raise children who follow a normative pathway to success in terms of education and employment, and thus their parenting both provides strong adult role models in the neighborhood, and even a sense of collective efficacy wherein adults may intervene when teens in their neighborhood are behaving in ways that would place their success at risk.

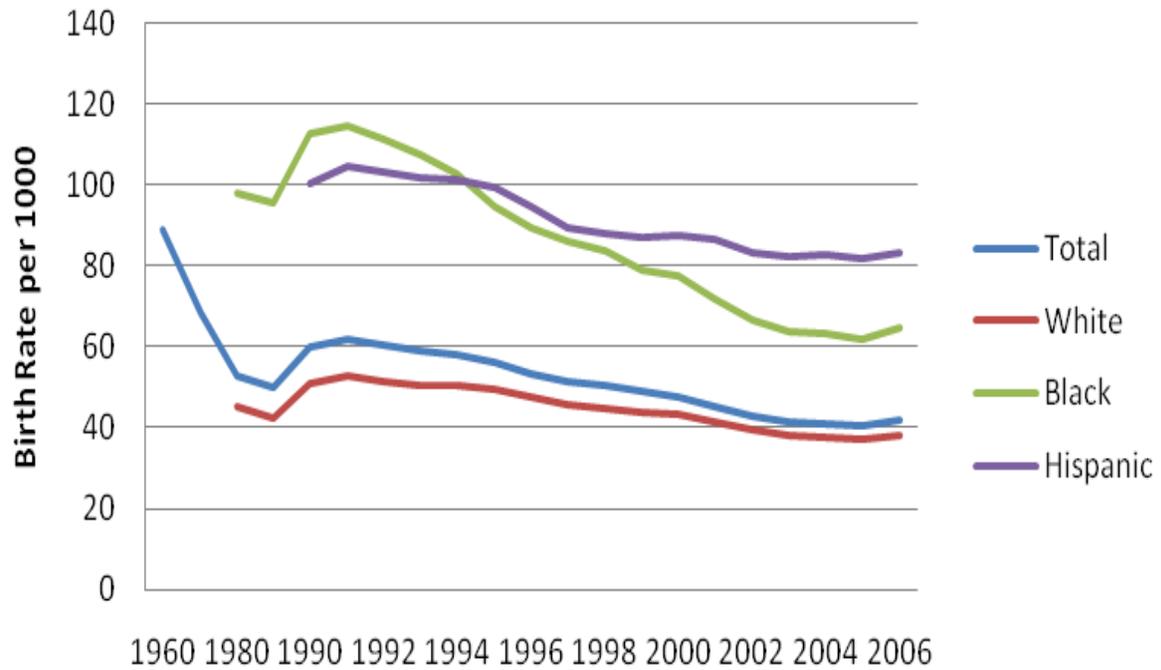
Parent/caregiver perceptions of neighborhood problems are important predictors in middle school and high school for Latino youth only. Preliminary qualitative results from our recent interviews of parent/child dyads and triads from the *Denver Child Study* suggest that study parent/caregivers felt as though their parenting could buffer the detrimental effects of the neighborhood, especially while their children were young. However, in both middle school and high school models for Latino youth, parental perceptions of neighborhood social disorder were significant predictors of the outcome. In essence, if a parent perceived there to be crime, drugs, and violence in their neighborhood, this increased a child's risk for teenage childbearing/fathering. It is possible that once a neighborhood reaches a certain critical perceived level of social disorder, a Latino parent no longer feels as efficacious in overcoming the negative effects of the neighborhood.

In terms of timing and duration, we see that there are both lagged and contemporaneous effects on teenage childbearing/fathering. Many studies only consider the neighborhood contexts at the point in time where one bears or fathers a child. We see here that it is useful to consider years leading up to the outcome as well. The models tested here utilize neighborhood averages to examine duration of neighborhood exposure. While this may potentially dilute the

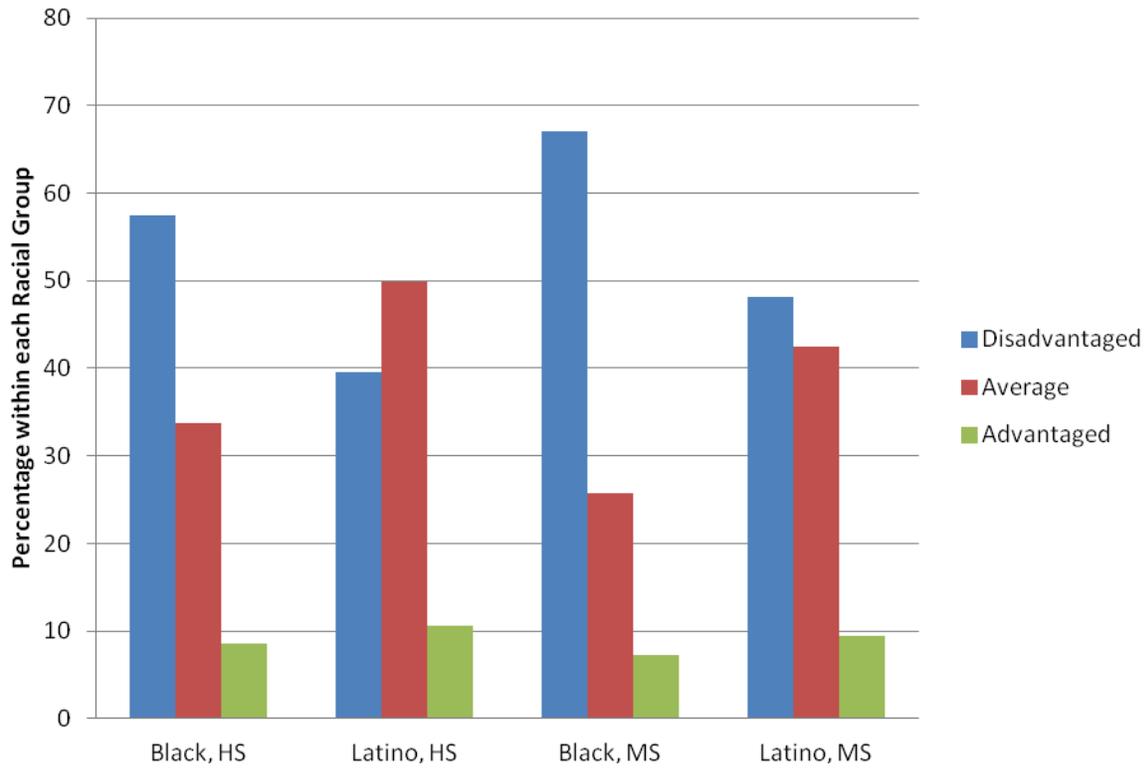
neighborhood effect, it provides a first step at examining how long-term (in this case during middle school and high school) exposure to neighborhood affects one's risk for teenage childbearing/fathering.

Practice and Policy Implications

Even after controlling for a great number of individual and family variables, neighborhood factors do influence one's propensity to bear or father children during teen years. We see that neighborhood effects operate differently for Black and Latino youth. Intervention efforts may need to be tailored to the specific needs of individual racial/ethnic groups. Further, in order to inform community based prevention/intervention strategies for teenage childbearing/fathering, it may be important to consider parental perceptions of neighborhood rather than only census indicators of neighborhood. This begs important questions, however. Are parental perceptions of neighborhoods accurate? If so, how can neighborhood conditions be changed? Future work with this data will examine how parental perceptions of neighborhood correlate with more objective measures of neighborhood conditions.

Figure 1. U.S. Birth Rates for Females ages 15-19

Source: www.childtrendsdatabank.org

Figure 2. Neighborhood Disadvantage Typologies in MS and HS

A greater portion of Black youth lived in disadvantaged neigh'ds in MS and HS than Latino youth. A greater portion of Latino youth tended to live in average neigh'ds than Black youth

Table 1. Descriptive Statistics of Youth and Parents in Analysis

Variable	Black Youth (n=327)				Latino Youth (n=416)			
	n (%)	Range	Mean	SD	n (%)	Range	Mean	SD
Teen Parent (dependent variable)	62 (19.0)				83 (20.0)			
Age		15-36	20.79	4.79		15-35	21.17	4.83
Gender								
Female	174 (53.2)				208 (50.0)			
Pubertal Timing (early)	59 (18.0)				32 (7.7)			
School honors	145 (44.3)				178 (42.8)			
School Involvement	159 (48.6)				185 (44.5)			
Religious Participation								
None	98 (30.0)				170 (40.9)			
Some	81 (24.8)				72 (17.3)			
All	148 (45.3)				174 (41.8)			
Sibling Teen Parent	42 (12.8)				66 (15.9)			
Mother Teen Parent	102 (31.2)				100 (24.0)			
Household Stressors		0-9	2.85	1.76		0-7	2.59	1.47
Household Income		1-66,353	14,216	12,798		1-41372	9,407	10,365
Parents' Education								
<HS	61 (18.7)				192 (46.2)			
GED	37 (11.3)				78 (18.8)			
HS Diploma	115 (35.2)				75 (18.0)			
Technical/Certificate	57 (17.4)				49 (11.8)			
College	57 (17.4)				22 (5.3)			
Proportion of Time in Two Parent Household		0-1	0.3	0.43		0-1	0.4	0.46
Parent Depression								
Borderline	53 (16.2)				61 (14.7)			

Clinical	35 (10.7)			31 (7.5)		
Parenting Style	7-20	16.44	3.49	6-20	16.89	3.38
Parenting Beliefs	7-25	20.2	3.69	5-25	21.3	3.31

Table 2. Descriptive Statistics of Neighborhood Characteristics in which Middle School Youth Lived

Variable	Black Youth				Latino Youth			
	%	Range	Mean	SD	%	Range	Mean	SD
Parental Perceptions of Neighborhood Problems		0-5	1.9	1.69		0-5	1.55	1.56
Social Capital		0-6	3.35	1.74		0-6	3.44	1.68
Presence of Peers Getting into Trouble (yes or no)	186 (56.9)				186 (44.7)			
Average % Foreign-Born Population in Neigh'd		0.26-66.5	19.1	10.4		3.4-71.5	20.7	10.5
Average % Households in Neigh'd that Moved in Past Year		9.7-56.9	27.5	8.6		6.9-45.8	25.9	7.5
Average % of children aged 5 to 17 in neigh'd		3.7-33.9	20.7	5.4		9.6-35.5	22.8	4.6
Neighborhood Disadvantage Typology								
Disadvantaged	219 (67.0)				200 (48.1)			
Average	84 (25.7)				177 (42.5)			
Advantaged	24 (7.3)				39 (9.4)			

Table 3. Descriptive Statistics of Neighborhood Characteristics in which High School Youth Lived

Variable	Black Youth				Latino Youth			
	%	Range	Mean	SD	%	Range	Mean	SD
Parental Perceptions of Neighborhood Problems		0-5	1.77	1.71		0-5	1.56	1.62
Social Capital		0-6	3.29	1.75		0-6	3.41	1.75
Presence of Peers Getting into Trouble (yes or no)	178 (54.8)				175 (44.3)			
Average % Foreign-Born Population in Neigh'd		0.26-68.6	22.4	11.6		4.1-53.0	24.4	12.3
Average % Households in Neigh'd that Moved in Past Year		4.6-57.9	26.7	8.8		4.6-47.4	24.3	8.4
Average % of children aged 5 to 17 in neigh'd		3.6-34.2	21.4	5.5		8.0-34.5	22.1	4.9
Neighborhood Disadvantage Typology								
Disadvantaged	187 (57.5)				156 (39.5)			
Average	110 (33.8)				197 (49.9)			
Advantaged	28 (8.6)				42 (10.6)			

Table 4. Logistic Regression Results for Middle School Youth

		Pseudo R ² =.26 Chi square=65.36 Std. Err. Adjusted for 153 clusters by family					Pseudo R ² =.20 Chi square=65.45 Std. Err. Adjusted for 188 clusters by family				
		Black Youth (n=284)					Latino Youth (n=340)				
Variable	Odds Ratio	Std. Error	z	95% CI		Odds Ratio	Std. Error	z	95% CI		
Age_16 ^a	0.09 *	0.12	-1.84	0.01	1.17	0.11 *	0.12	-2.12	0.02	0.69	
Age_17 ^a	0.08 **	0.07	-2.74	0.01	0.49	0.17 *	0.13	-2.42	0.04	0.72	
Age_18 ^a	0.11 *	0.11	-2.22	0.02	0.77	0.22 *	0.16	-2.10	0.05	0.90	
Age_19 ^a	1.19	0.63	0.33	0.42	3.38	1.33	0.62	0.61	0.53	3.34	
Gender ^b	6.32 **	2.71	4.30	2.73	14.62	3.94 **	1.31	4.12	2.05	7.58	
School Honors ^c	0.89	0.36	-0.29	0.40	1.97	1.22	0.40	0.59	0.64	2.32	
School Involvement ^d	1.36	0.61	0.69	0.56	3.30	0.61	0.21	-1.42	0.31	1.21	
Religious Participation (some) ^e	1.05	0.56	0.09	0.37	3.01	0.67	0.37	-0.73	0.23	1.96	
Religious Participation (all) ^e	1.20	0.53	0.42	0.51	2.84	1.68	0.67	1.29	0.76	3.69	
Mental Health ^f	2.77	4.05	0.70	0.16	48.52	0.88	1.02	-0.11	0.09	8.45	
Sibling Teen Parent ^g	0.49	0.28	-1.27	0.16	1.47	0.61	0.27	-1.14	0.26	1.44	
Pubertal Timing ^h	1.64	0.91	0.89	0.55	4.85	0.68	0.54	-0.49	0.14	3.26	
Household Stressors	0.95	0.15	-0.33	0.70	1.29	1.15	0.14	1.22	0.92	1.45	
Parenting Style	1.00	0.07	0.01	0.87	1.15	1.01	0.05	0.17	0.92	1.11	
Parenting Beliefs	0.96	0.06	-0.70	0.85	1.08	1.02	0.05	0.29	0.92	1.13	
Mother Teen Parent ⁱ	1.04	0.44	0.08	0.45	2.40	2.08 *	0.72	2.12	1.06	4.08	
GED ^j	3.13	2.21	1.61	0.78	12.52	0.57	0.26	-1.24	0.23	1.38	
HS Diploma ^j	0.63	0.39	-0.74	0.19	2.13	0.94	0.41	-0.14	0.40	2.20	
Technical/Certificate ^j	1.41	0.92	0.53	0.39	5.06	0.31 *	0.19	-1.96	0.10	1.00	
College ^j	1.41	1.01	0.48	0.35	5.73	0.53	0.38	-0.88	0.13	2.18	
Borderline Depression ^k	1.84	1.06	1.05	0.59	5.70	0.93	0.44	-0.15	0.37	2.37	
Clinical Depression ^k	2.75	1.88	1.47	0.72	10.53	2.72 †	1.54	1.77	0.90	8.24	

Two Parent Home	1.03	0.53	0.06	0.37	2.83	0.87	0.29	-0.42	0.45	1.67
Household Income	1.00	0.00	-0.37	1.00	1.00	1.00	0.00	0.40	1.00	1.00
Peers in Trouble ^l	1.01	0.52	0.02	0.37	2.79	0.82	0.35	-0.47	0.36	1.88
Social Capital	1.28 *	0.14	2.29	1.04	1.58	0.97	0.09	-0.35	0.80	1.17
Neigh'd Problems	1.19	0.19	1.09	0.87	1.63	1.26 †	0.17	1.77	0.97	1.64
Kids Age 5-17	0.97	0.04	-0.72	0.89	1.06	0.98	0.03	-0.66	0.91	1.05
Residential Instability	1.03	0.03	1.00	0.98	1.08	1.06 *	0.03	2.33	1.01	1.12
Foreign Born in Neigh'd	0.96 *	0.02	-1.98	0.91	1.00	0.98	0.02	-1.09	0.95	1.01
Disadvantaged ^m	0.44 †	0.20	-1.76	0.18	1.09	0.80	0.32	-0.54	0.37	1.76

*Indicates significance at the $p < .05$ level, **Indicates significance at the $p < .01$ level, †Indicates marginal significance at the $p < .10$;
^aReference category is age>19 at time of survey; ^bReference category is male; ^cReference category is never on honor roll in MS;
^dReference category is never involved in sports or clubs in HS; ^eReference category is never attended religious meetings in MS;
^fReference category is never had anxiety, depression or PTSD in MS; ^gReference category is did not have an older sibling who was a teen parent; ^hReference category is on time/late; ⁱReference category is mother was not a teen parent; ^jReference category is less than HS; ^kReference category is no depression; ^lReference category is no peers in neigh'd who got into trouble; ^mReference category is average/advantaged.

Table 5. Logistic Regression Results for High School Youth

Pseudo R ² =.23 Chi square=67.41 Std. Err. Adjusted for 152 clusters by family							Pseudo R ² =.27 Chi square=65.45 Std. Err. Adjusted for 181 clusters by family				
Black Youth (n=283)							Latino Youth (n=321)				
Variable	Odds Ratio	Std. Error	z	95% CI		Odds Ratio	Std. Error	Z	95% CI		
Age_16 ^a	0.08 *	0.10	-1.96	0.01	1.01	0.16 *	0.10	-2.76	0.06	0.71	
Age_17 ^a	0.06 **	0.06	-2.96	0.01	0.39	0.18 **	0.11	-2.80	0.05	0.60	
Age_18 ^a	0.26 †	0.20	-1.74	0.06	1.18	0.13 *	0.13	-2.01	0.02	0.95	
Age_19 ^a	1.15	0.61	0.26	0.41	3.24	1.00	0.46	0.00	0.40	2.48	
Gender ^b	4.25 **	1.71	3.59	1.93	9.36	6.13 **	2.41	4.61	2.84	13.25	
School Honors ^c	0.59	0.26	-1.21	0.25	1.38	1.80	0.68	1.57	0.86	3.76	
School Involvement ^d	0.73	0.33	-0.70	0.31	1.75	0.39 **	0.14	-2.58	0.19	0.80	
Religious Participation (some) ^e	0.53	0.28	-1.20	0.19	1.50	0.69	0.41	-0.62	0.22	2.21	
Religious Participation (all) ^e	0.87	0.40	-0.31	0.35	2.16	0.67	0.30	-0.88	0.27	1.63	
Mental Health ^f	2.05	3.03	0.49	0.11	36.88	0.23	0.21	-1.59	0.04	1.40	
Sibling Teen Parent ^g	0.43	0.24	-1.49	0.14	1.31	0.75	0.37	-0.59	0.29	1.95	
Pubertal Timing ^h	1.90	0.92	1.32	0.73	4.89	1.34	1.07	0.37	0.28	6.40	
Household Stressors	1.06	0.12	0.49	0.84	1.33	1.03	0.14	0.20	0.78	1.35	
Parenting Style	1.01	0.07	0.15	0.88	1.15	1.05	0.07	0.84	0.93	1.19	
Parenting Beliefs	1.01	0.07	0.15	0.89	1.15	0.96	0.05	-0.73	0.86	1.07	
Mother Teen Parent ⁱ	0.87	0.36	-0.33	0.39	1.95	2.01 *	0.73	1.92	0.99	4.10	
GED ^j	1.32	0.87	0.42	0.36	4.82	0.36 †	0.19	-1.91	0.13	1.03	
HS Diploma ^j	0.72	0.45	-0.53	0.21	2.44	0.47	0.23	-1.57	0.18	1.21	
Technical/Certificate ^j	0.90	0.61	-0.16	0.24	3.43	0.34 †	0.20	-1.87	0.11	1.06	
College ^j	1.28	0.87	0.36	0.34	4.85	1.00	0.72	0.00	0.24	4.14	
Borderline Depression ^k	1.61	0.92	0.82	0.52	4.95	0.92	0.54	-0.14	0.29	2.91	
Clinical Depression ^k	3.62 †	2.70	1.73	0.84	15.61	4.27 *	2.65	2.34	1.26	14.44	
Two Parent Home	0.84	0.41	-0.36	0.32	2.20	1.20	0.49	0.45	0.54	2.68	

Household Income	1.00	0.00	-1.54	1.00	1.00	1.00	0.00	-0.20	1.00	1.00	
Peers in Trouble ^l	0.77	0.37	-0.55	0.29	2.00	0.40	[†]	0.20	-1.82	0.15	1.07
Social Capital	0.94	0.12	-0.51	0.74	1.20	1.01		0.11	0.11	0.81	1.26
Neigh'd Problems	1.18	0.18	1.10	0.88	1.60	1.44	[†]	0.20	2.58	1.09	1.91
Kids Age 5-17	1.02	0.04	0.59	0.95	1.10	0.94		0.04	-1.60	0.87	1.01
Residential Instability	0.98	0.03	-0.58	0.92	1.04	1.01		0.03	0.25	0.96	1.06
Foreign Born in Neigh'd	0.95	*	0.02	-2.41	0.91	0.99		0.02	-0.59	0.95	1.03
Disadvantaged ^m	0.79	0.35	-0.55	0.33	1.86	0.51		0.24	-1.41	0.20	1.30

*Indicates significance at the $p < .05$ level, **Indicates significance at the $p < .01$ level, [†]Indicates marginal significance at the $p < .10$;
^aReference category is age > 19 at time of survey; ^bReference category is male; ^cReference category is never on honor roll in HS;
^dReference category is never involved in sports or clubs in HS; ^eReference category is never attended religious meetings in HS;
^fReference category is never had anxiety, depression or PTSD in HS; ^gReference category is did not have an older sibling who was a teen parent;
^hReference category is on time/late; ⁱReference category is mother was not a teen parent; ^jReference category is less than HS;
^kReference category is no depression; ^lReference category is no peers in neigh'd who got into trouble; ^mReference category is average/advantaged.

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