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Do Neighborhood Effects on Low-Income Minority Children Depend on Their Age? Evidence from a Public Housing Natural Experiment

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Abstract

We analyze data from a natural experiment involving Denver public housing that quasi-randomly assigns low-income Latino and African American youth to neighborhoods. ITT and TOT models reveal substantial effects of neighborhood socioeconomic status, ethnicity and safety domains on youth and young adult educational, employment and fertility outcomes. Effects are contingent on when a youth was first assigned to public housing and the neighborhood characteristic in question. Benefits from neighbors of higher occupational prestige are stronger if a child begins experiencing them at a younger age, whereas negative consequences of neighborhood crime are only manifested for teens. Neighborhood effect sizes apparently depend on the interaction among exposure duration, disruption effects of mobility, and developmental stage-specific differences in vulnerability to the given neighborhood effect mechanism operative. Our results hold powerful and provocative implications for where assisted housing should be developed and how applicants should be assigned to neighborhoods.

Keywords

housing policy; neighborhood effects; public housing; natural experiments; neighborhoods

Introduction: Housing Policy and the Debate over Neighborhood Effects

For over two decades federal housing policy has been crucially shaped by the notion of "the geography of opportunity."[1] This framing focuses on the dramatic increase in geographic inequalities in socioeconomic, environmental and institutional domains as both a reflection of and, more importantly, a significant contributor to rising inequalities in opportunities and outcomes across individuals (Drier, Mollenkopf, and Swanstrom, 2004; Galster and Sharkey,

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[1]For parsimony we do not present results for interactions with dummy variables denoting assignment to DHA under age 13. They support they main conclusions reported using the continuous age interactions and are available upon request.

2016). It challenges housing policymakers to address an uncomfortable question. To what degree have past and present subsidized housing programs contributed to creating and maintaining the spatial concentration of disadvantaged populations, thereby constraining their opportunities? The unambiguous answer provided by past research has been: “a great deal” (see Vale, 2013, Goetz, 2013 and the review in Galster, 2013).

Perhaps the most politically potent early evidence that the answer to this question mattered was provided by the Gautreaux program. Responding to a court-ordered plan for desegregating the notoriously concentrated Chicago Housing Authority (CHA), the Gautreaux program developed scattered site, small-scale public housing dwellings and provided mobility counseling assisting former CHA residents receiving Section 8 rental vouchers to move to suburban locations. James Rosenbaum and colleagues conducted a series of evaluation studies and found that both children and adults moving to the suburbs through the Gautreaux program fared much better in numerous socioeconomic domains than those who did not (see the review by Rosenbaum, 1995).

In recognition of both the importance of spatial context and its past programmatic shortcomings in creating an urban geography of more equal opportunities, the U.S. Department of Housing and Urban Development (HUD) under the Clinton administration undertook a comprehensive series of initiatives beginning in 1993. Disadvantaged neighborhoods were targeted for job-creation and revitalization activities through the Empowerment Zone program (see U.S. Department of Housing and Urban Development, 1997). Dozens of longstanding fair housing suits against HUD and local housing authorities were settled expeditiously with Gautreaux-like remedies (Popkin et al., 2003). The HOPE VI program was begun, aiming to replace severely distressed public housing complexes with new, mixed-income/tenure developments (Chaskin and Joseph, 2016). The Regional Opportunity Counseling demonstration was initiated to aid voucher-holders move more widely over a metropolitan area to find the places of highest opportunity (Varady and Walker, 2000). And, of course, the Moving To Opportunity (MTO) demonstration was launched to gather broader evidence about what everyone already knew based on the Gautreaux studies: neighborhoods mattered a great deal in shaping outcomes for low-income, minority households (Briggs, Popkin and Goering, 2010). The MTO research design randomly assigned residents of distressed public housing in five cities who volunteered to participate in one of three groups: (1) controls who got no voucher and stayed in public housing in disadvantaged neighborhoods; (2) recipients of rental vouchers with no restrictions; and (3) recipients of rental vouchers and relocation assistance who had to move to census tracts with less than 10 percent poverty rates and remain for at least a year.

There was only one problem: MTO did not find what was expected, thereby challenging the empirical foundation upon which all the foregoing initiatives were built. Investigations using MTO data revealed some positive impacts on the experimental group’s mental and physical health, safety, and self-assessed well-being, but failed to find substantial short- or long-term context effects on teen or adult educational or labor market outcomes (Orr et al., 2003; Sanbonmatsu et al., 2011; Ludwig, 2012). Because its unique, random assignment design has conventionally been seen as the preferred method for measuring program impacts, the

results of MTO have been interpreted by some as proof that neighborhoods matter little for socioeconomic outcomes in America (Smolensky, 2007).

Unsurprisingly given its policy relevance and surrounding publicity, there has been substantial debate over the power of MTO as an unambiguous test of spatial context effects (cf. Clampet-Lundquist and Massey, 2008; Sampson, 2008; Burdick-Will et al., 2010; Briggs, Popkin and Goering, 2010; Briggs et al., 2008, 2011; Sanbonmatsu et al. 2011; Ludwig, 2012). The debate has focused on five domains. First, although MTO randomly assigned participants to treatment groups, it neither randomly assigned characteristics of neighborhoods initially occupied by voucher-holders (except maximum poverty rates for the experimental group) nor characteristics of neighborhoods in which participants in all three groups moved subsequently. Thus, there remains considerable question about the degree to which geographic selection on unobservables persists. Second, MTO may not have created adequate duration of exposure to neighborhood conditions by any group at any location to observe much treatment effect. Third, MTO overlooked the potentially long-lasting and indelible developmental effects upon adult experimental group participants who spent their childhoods in disadvantaged neighborhoods. Fourth, it appears that even experimental MTO movers rarely moved out of predominantly African American-occupied neighborhoods near those of concentrated disadvantage and achieved only modest changes in school quality and job accessibility. Thus, they may not have experienced sizable enhancements in their geographic opportunity structures. For all these reasons, MTO may not have provided definitive evidence about the potential effects on low-income families from prolonged residence in multiply-advantaged neighborhoods, its theoretical promise and conventional wisdom notwithstanding.

The disappointment in housing policy circles surrounding the results of MTO has recently be assuaged by Chetty, Hendren and Katz (2015). They analyzed the subset of MTO experimental children who moved to low-poverty neighborhoods before they were age 13[2] and observed that they subsequently exhibited as young adults significantly higher earnings, better chances of attending college and lower rates of single parenthood than either experimental group children who moved after age 13 or children from the other MTO treatment groups. By contrast, effects on those who moved during ages 13-18 were mainly nil or even negative. The authors interpret their findings as consistent with a model of benefits from living in good neighborhoods directly related to duration of exposure and inversely related to the disruption effects from moving; for younger children the former dominates but for older ones the latter does.

As provocative and important as the Chetty, Hendren and Katz (2015) research is, it raises more questions for housing policymakers than it provides answers. What was it about the young children's experiences in low-poverty neighborhoods that resulted in their eventual

[2]In 2000, the rough midpoint of our sample youths' lives. African Americans represented only 11 percent of the overall Denver population, whereas Latinos comprised 32 percent. Ethnic residential segregation during the period of our study was lower than national averages for both Latinos and African Americans (Iceland, Weinberg and Steinmetz, 2002). Denver has a unified city-county government, and thus has much less geographic variation in local fiscal capacity and public services than in the other sites. All of these distinctions imply that Denver offers quite different opportunity structures, local cultural norms, public expectations and institutional supports than in the typical U.S. metropolitan area.

income gains as young adults? Are stronger neighborhood effects for younger children also observed across a wider range of outcomes than those emerging during young adulthood? Are similar effects on children observed for families participating in site-based assisted housing programs instead of using vouchers?

Our study answers these questions for a sample of young adult, low-income African Americans and Latinos who spent a portion of their childhood in Denver (CO) public housing. We quantify how their outcomes as children and young adults (in the domains of employment, education, fertility, exposure to violence) are related to particular facets (socioeconomic, demographic, ethnic, safety) of their childhood neighborhood and their age of exposure. Data come primarily from retrospective family history surveys of Denver Housing Authority (DHA) households who lived in public housing scattered throughout the City and County of Denver, Colorado. Because DHA household allocation mimics random assignment to neighborhood, this program represents a natural experiment for overcoming geographic selection bias. We employ characteristics of neighborhoods first offered to families as instrumental variables to control for any subsequent geographic selection by families after initial offer.

Our research questions are:

1. What are the Intent-to-Treat (ITT) and Treatment-on-Treated (TOT) effects of childhood neighborhood socioeconomic, demographic, ethnic and safety characteristics on young adult employment, education and fertility outcomes?[3]
2. What are the ITT and TOT effects of childhood neighborhood socioeconomic, demographic, ethnic and safety characteristics on youth education and exposure to violence outcomes observed before adulthood?
3. Are the effects different depending on whether the child was younger than 13 when first assigned by DHA to the neighborhood?
4. Does the answer to Q3 depend on which outcome is being considered?

We find that our results for young adult employment, education and fertility outcomes broadly replicate those observed by Chetty, Hendren and Katz (2015) insofar as neighborhood effects appear stronger when children are assigned to DHA dwellings at younger ages. However, neighborhood effects on youths' education and exposure to violence outcomes observed before adulthood are generally larger for youths who are first assigned as adolescents. For both sets of outcomes, it is the neighborhood's occupational prestige, Latino composition, and safety that prove most powerful. A primary (though not exclusive) vehicle through which neighborhood affects young adult outcomes is through secondary school performance and educational attainment.

[3]The term was introduced in published form in a special issue of *Housing Policy Debate* in 1995 (Galster and Hornberg, 1995). In that issue, Galster and Killen (1995) first formally conceptualized this concept, which has recently been modified and extended by Galster and Sharkey (2016). Besides the articles in that special issue, other seminal analyses related to this theme were published in Briggs (1995). For a recent discussion of the normative foundations of this concept, see Dawkins (forthcoming).

Theory and Evidence on Neighborhood Effects and their Age Contingency

It is beyond the scope of this paper to review in detail the voluminous, multi-disciplinary literature on neighborhood effects; instead we cite the conclusions of key recent review articles (Sastry, 2012; Sharkey and Faber, 2014, Galster and Sharkey, 2016). There clearly are several plausible mechanisms through which the neighborhood could affect a variety of outcomes for youth and young adults. These include forces within social interactive, environmental, geographical, and institutional domains (Jencks and Mayer, 1990; Duncan, Brooks-Gunn and Aber; 1997; Leventhal and Brooks-Gunn, 2000). In the most exhaustive typology to date, Galster (2012) identified 15 distinct potential mechanisms of neighborhood effects.

Whatever the underlying causal process(es) involved, estimating the unbiased magnitude of such effects is confounded by numerous methodological challenges; for an extensive discussion, see Galster (2008). Perhaps the most contentious aspect in this realm of scholarship, however, is the issue of geographic selection bias (Manski 1995, 2000; Duncan et al. 1997; Dietz 2002). The central issue is that individuals being studied (or their parents) likely have unmeasured motivations, behaviors, and skills related to their own (and/or their children's) socioeconomic prospects and move from and to certain types of places as a consequence of these unobserved characteristics. Any observed relationship between geographic conditions and outcomes for adults or their offspring may therefore be biased because of this systematic spatial selection process. Skeptics may rightly argue that what is being measured is simply another impact of (unmeasured) individual attributes, not the impact of the space in which the individual resides.

Several methodological approaches have convincingly met the challenge of geographic selection effects, including random assignment experiments, quasi-random natural experiments, instrumental variables, fixed-effects, differencing, sibling comparisons, inverse probability of treatment weighting and propensity score matching. After reviewing the relatively sparse set of studies employing these techniques, Galster and Sharkey (2016) concluded that the preponderance of evidence supported the existence of neighborhood effects in the realms of cognitive and behavioral development, educational performance and attainment, mental and physical health, criminality and labor force participation and earnings, despite the aforementioned generally contrary findings from MTO.

There has been considerably less scholarship devoted to the issue of whether neighborhood effects on youth are contingent upon age of exposure. Though there have been longstanding hypotheses that effects depended on the developmental stage of children (Ellen and Turner, 1997; Leventhal and Brooks-Gunn, 2000), two recent theoretical framings prove especially useful in establishing an expectation of age-contingency. Harding et al. (2011) conceptualize the power of neighborhood forces being determined by a multiplicative function of neighborhood characteristics, the timing and duration of individuals' exposure to them, and individuals' vulnerability to their influences. Galster (2012) conceptualizes neighborhood effects using a complementary, pharmacological metaphor of "dosage and response;" the power of the response will depend upon (among other things) the ingredients in and intensity of the dose, the timing and consistency with which it is applied, the ambient state of the

individual, and the presence of synergistic risk factors and protective factors. These frameworks suggest that age will influence the power of the neighborhood effect to the degree that age shapes duration of exposure, vulnerability, and compounding or countervailing influences.

Unfortunately, none of these three elements are likely related to age in the same fashion, hence the net relationship may be ambiguous. For example, younger children may be more biologically sensitive to certain environmental contaminants, but their parents may systematically be more effective in protecting them because of this, perhaps by limiting their time outdoors. Adolescents may potentially be more influenced by neighborhood peers than younger children, yet if they travel long distances to a school outside of their neighborhood their time of exposure to such peers may be minimal. These predictions of ambiguity are consistent with Sharkey and Faber's (2014) review, which noted that studies have come to opposite conclusions about when in a child's life neighborhood context is more important. They conclude that age effects are likely to be contingent on the specific mechanism of neighborhood effect at work.

This paper contributes to this extant literature in two important ways. First, it employs a natural experiment that (as we demonstrate below) provides a quasi-random assignment of neighborhood "treatments" to public housing applicants. This skirts the challenge of geographic selection bias and permits valid causal inferences from our ITT and TOT measures. Second, it demonstrates the degree to which the relationship between age of child when administered the neighborhood treatment and strength of subsequent outcomes depends both on the particular contextual characteristic(s) and outcome involved.

The Natural Experiment Involving Public Housing in Denver and Our Analytical Approach

Overview

In addition to its large-scale, conventional public housing developments, the DHA has operated since 1969 an unusual program providing approximately 1,500 low-income families with opportunities to live in scattered-site, single-family and small-scale, multi-family public housing units. These DHA-operated 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.[4]

As with any public housing program, applicants to DHA qualified for housing appropriate for the size and composition of their households if they earned less than 80 percent of the household-adjusted median family income for Denver; were comprised of U.S. citizens or eligible immigrants; and met DHA requirements regarding good housekeeping, no criminal activity, substance abuse, or sexual offending histories [5] From 1987 onwards, as a DHA dwelling (in either conventional or scattered-site developments) became vacant it was

[4]The authors provide no theoretical justification for the age 13 bifurcation. We employ the same cutoff in our study so our findings can be compared to theirs, but also note that this age has conventionally been used by developmental psychologists as the onset of early adolescence (Booth and Crouter, 2001).

offered to the household at the top of the common public housing waiting list having a family size and composition appropriate for the number of bedrooms in the vacant unit. If the household did not accept this unit they were offered the next similarly sized unit that became available (typically after a wait of 13 months). If the household did not accept this second unit they dropped to the bottom of the queue, creating a wait of an additional year or more.

Although there are clearly quasi-random aspects to this DHA housing allocation process, there is also some room for geographic selection on the part of households on the DHA waiting list because of their right to refuse the first offer. Indeed, our independent evaluation of DHA records showed that: 69.5 percent accepted the dwelling first offered by DHA, 18.8 percent accepted their second offer; 7.9 percent ended up rejecting both offers and taking a third offer later (after returning to the bottom of the wait list); 3.8 percent rejected three or more offers before being placed. Since offered dwellings were sometimes in the same neighborhood, we calculated that 75.5 percent of the tenants ultimately occupied a DHA unit in the first neighborhood offered. This means that roughly a quarter of DHA households clearly exercised some neighborhood selection different from the first neighborhood offered.

Furthermore, the quasi-randomness of the initial DHA assignment may erode over time if residents selectively leave their original locations while others stay. First, DHA households could voluntarily transfer between scattered-site and conventional public housing developments, though this occurred rarely.[6] Second, a substantial part of our study's information comes from households no longer residing in DHA housing, and their subsequent neighborhoods were likely not randomly chosen.[7] A third potential source of post-assignment selection relates to those who did *not* move out of their original DHA housing for an extended period. Perhaps their unwillingness or inability to move out of DHA was related to some unobservable parental characteristics that also may be connected to youth and young adult outcomes.

The aforementioned potentials for geographic selection in the natural experiment led us to adopt an identification strategy that employs the neighborhood characteristics associated with the dwelling *first offered by the DHA* to a household when it rose to the top of the waiting list. We would argue that the offered neighborhood characteristics provide valid instruments for the actual neighborhood characteristics experienced by sample youth. There is no plausible reason why the offer of a neighborhood should: (1) influence a youth's outcomes other than through its influence on actual neighborhood context experienced; or (2) be related to unobservable characteristics of the household related to both its neighborhood preferences and outcomes for its children. We would further argue that the offered neighborhood characteristics likely provide strong instruments for the actual neighborhood characteristics experienced by youth because (as noted above) three-quarters

[5]Though we adopt the conventional terminology about "treatment," we would not wish our human subjects to be seen as ciphers in a dosage-treatment regimen instead of efficacious agents with complex lives woven into neighborhood networks and conditions of varying density and salience.

[6]DHA dwellings are located in nearly 60 percent of the 137 tracts in the City and County of Denver.

[7]Over the period from which we drew our sample, housing authorities like DHA were given considerable latitude in specifying local preferences for which households received priority on the wait list, such as those who have been recently rendered homeless or displaced by fires or natural disasters, the elderly or disabled, or victims of domestic violence.

of households ended up residing in the neighborhood first offered and a majority of them were still residing there when we interviewed them to gather information about their children's outcomes.

A complimentary way to describe our approach is in terms of an "intent to treat" analysis. DHA could be viewed as offering to the waiting list household a "treatment" consisting of a bundle of neighborhood attributes. Most accept this treatment but some hold out for another neighborhood or dwelling they deem preferable to that originally offered. Regardless, our identification strategy can be thought of as a way to evaluate the impacts on minority youths of DHA's public housing program having both conventional and scattered-site components.

Generality

The use of natural experiments inevitably raises questions about the generality of results. We believe that our findings can fairly be generalized to low-income, Latino and African American 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 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 oft-cited MTO-based scholarly studies noted above. Finally, we acknowledge that the ethnic composition of DHA's public housing residents (and our analysis sample) differs from the national norm. Among DHA residents in 2005, 26% were non-Hispanic white, 23% non-Hispanic black, 43% Hispanic, 7% Asian and 1% Native American and other; the corresponding figures for public housing in the U.S. were: 32%, 43%, 22%, 2% and 1% (U.S. Department of Housing and Urban Development, n.d.)

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 (conducted in person for about 20 percent of the sample who had no landline phones) that collected retrospective and current information about the household, adults and children. Each household's residential mobility history was obtained so it could be associated with neighborhood developmental contexts 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 quasi-random assignment process came into operation); and (4) Latino or African American 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 a check or gift card. We estimate a participation rate of 56.5 percent, with virtually all non-participation due to our inability to locate the household; less than 6 percent refused to participate once contacted. Our team successfully completed 710 interviews with the parents or primary caregivers of eligible households whose surveys subsequently passed our rigorous data

verification and reliability processes. Details of sampling, participation rates, and profiles of eligible and participating households are available from the authors. The 425 youth analyzed in this study were (current or past) members of 248 interviewed households who had reached age 18 or older by the time of our survey and had resided in DHA housing during or before the particular outcome being investigated. Because these youth ranged in age from 18 to 35 years at time of survey, the period during which they first occupied DHA housing varied from 1987 to 2005.

Our *Denver Child Study* survey collected information on a variety of parental/caregiver ("caregiver" hereafter[8]) and household characteristics that we employed as controls; these are listed in Table 1.[9] Covariates included: whether the caregiver was born outside of the U.S., whether both parents were cohabiting, whether the caregiver had attained a high school diploma or higher, whether the caregiver was disabled (by DHA standards), the annual average number of household moves the child had experienced and the natural logarithm of household income. Our survey also asked questions that permitted us to measure a series of stressful household events from which we created a "household economic stressors index" (scaled 0-5). Caregivers were asked whether and when they experienced any of the following events for each residence: a. Unemployed a month or more?; b. Have a major illness or injury?; c. Have too little money to buy enough food for your family?; d. Have your electricity, gas, or phone service cut off?; or e. Get evicted from your home? This index was incremented by one for each of the above circumstances experienced by the household.[10] The survey also asked questions that permitted us to estimate whether the caregiver had excessively drunk alcohol or used marijuana or other drugs since becoming a caregiver; details are available from the authors.[11]

Caregiver and household characteristics for our sample youth as portrayed in Table 1 clearly reflect their disadvantaged circumstances. Households' mean income was $13,902 and they faced on average 1.09 incidents of acute financial distress while in their current residence. Over one-third of caregivers had no high school diploma, 11 percent were disabled and 17 percent had a history of alcohol or drug use while raising children. Twelve percent of caregivers were not born in the U.S. and 34 percent were married or partnered. Notably, this is an unusually stable sample of low-income youth, moving only once every four years, on average, undoubtedly attributable to their duration in public housing.

The *Denver Child Study* survey asked caregivers to supply information about all their children with whom they had lived in DHA public housing for at least one year and we

[8]Of the post-1986 vintage tenants residing in conventional public housing developments at the time of the Denver Child Study interviews, 99 percent were originally placed in such; only one percent moved in from dispersed housing. Of the post-1986 vintage tenants residing in dispersed housing at that time, 94 percent were originally placed in such; six percent moved in from the conventional developments. Moreover, an unknown number of these transfers were involuntary, required by regulations after changes in family size or composition.

[9]Slightly more than one-third of all caregivers interviewed in our study were former DHA residents. The primary reason for leaving DHA was improving economic circumstances that rendered the family ineligible for public housing and/or allowed it to secure preferable accommodations in the private market. A sizable share of households exited DHA involuntarily due to lease violations.

[10]We employ the more general term caregiver because some sample youth were living with a relative other than a biological parent.

[11]The time when covariates are measured varies according to outcome; those in Table 1 correspond to the onset of dropping out of school (or age 18 if never). For our youth outcomes (last GPA, dropping out, witnessing violence) all covariates are measured at the same time as the onset of the outcome, or age 18 if the outcome never occurred. For all our young adult outcomes (school, work, out-of-wedlock child bearing) all covariates are measured as averages during youth ages 15-17.

restrict our analysis sample to children who lived in DHA for two or more years. The individual characteristics we analyzed as control variables and their youth and young adult outcomes we analyze are listed in the upper portion of Table 1. Our sample consists of 53 percent Latinos and 46 percent African Americans, with both groups evenly split by gender. On average, each individual had 1.56 siblings and 41 percent represented first-born children.

Outcomes Analyzed for Young Adults and Youth

Caregivers in the *Denver Child Study* were asked a variety of retrospective questions about several outcomes occurring over the course of childhood and into young adulthood. We employed the technique of asking whether certain events occurred and then asking follow-up questions regarding ages at first and last occurrence. Further, we facilitated respondent recall at time of interview by using an event history or life calendar in order to reference key life events. We then used caregiver reports to these questions to estimate the prevalence of a given outcome of as well as to determine the average age of onset for each. We recognize the potential shortcomings of these retrospective, proxy reported data. These shortcomings, our and previous researchers' efforts to minimize their effects, and evidence about the reliability of our survey are discussed in Appendix B (intended for online version).

In this study we analyze three socioeconomic outcomes for young adults that roughly correspond to domains analyzed by Chetty, Hendren and Katz (2015): post-secondary schooling, employment and fertility. The *Denver Child Study* survey asked caregivers, "Since turning 18, has _[youth]_ *primarily* been working full-time, working part-time, not working but attending school, or neither working nor attending school?" We combined both full- and part-time work for the analysis and established the last option as the excluded reference category. In our sample, 14 percent primarily attended school and 70 percent primarily worked since turning age 18 (see Table 2). Our survey also asked caregivers, "Since turning 18, has _[youth]_ever fathered or given birth to a child out of wedlock?" Thirty-seven percent of our sampled youth had.[12]

We also analyze three outcomes for youths occurring before age 18 that, though important in their own right, also have intuitive appeal as contributors to socioeconomic outcomes as young adults: educational performance and exposure to violence.[13] We employ two measures of secondary school performance: leaving school before a diploma was earned, and grade point average at last school attended. These measures are commonly employed in major U.S. population surveys such as the Panel Study of Income Dynamics and, as in both our study and PSID, are assessed by the caregiver survey respondents. Grades were assessed from caregiver responses to the survey question, "What sort of grades did [youth] usually earn during high school?[14] Mostly As? Mostly As and Bs? Mostly Bs?…Mostly Ds and Fs?" The median response was "Mostly Bs and Cs." We converted these categories into their

[12]Previous research has indicated that acute economic shock to a household can have seriously disruptive effects on adolescents' mental and physical health than can impair a variety of outcomes over the longer term (Shonkoff et al., 2012).

[13]In preliminary models we experimented with a more covariates, including measures of fertility and employment history, family size, and whether the household had health insurance. We also assessed caregiver gender, but since virtually all were female this was not included as a covariate. Given our relatively modest sample sizes we omitted from our final models covariates that never proved statistically significant in preliminary trials.

[14]Though out-of-wedlock births were observed in our sample beginning at age 15, 90 percent occurred between ages 18-24.

standard grade-point equivalents ("Mostly As" =4.0, "Mostly As and Bs" =3.5, etc.)[15]; the mean grade point average during the last secondary school attended was 2.56. Dropping out was defined as the survey respondent saying regarding the youth's current status in school that s/he "was no longer in school because s/he had dropped out before getting a diploma." Thirty-one percent of sample youth were so designated, with the mean age of those dropping out being 16 years old at the time.[16] Exposure to violence was operationalized as a dummy variable based on responses to the survey questions, "Have any of your children witnessed violence in or around the neighborhood? [if yes] How old was [child] when s/he first witnessed violence in or around the neighborhood?" Thirty one percent of our sample had witnessed violence in their neighborhoods before turning 18.

We emphasize that we analyze for each of these three youth outcomes only sampled youth who evinced the given outcome *after their families began residing in DHA public housing*. Although this ends up producing different analysis sample sizes, the temporal sequence requirement is necessary for valid causal inference.

Collection of Neighborhood Indicators

We obtained neighborhood data from two sources. The first source was the decennial U.S. Census, where we used census tract geographic scales to define neighborhoods. Because the period during which sample youth outcomes were analyzed ranged from the 1980s to 2000s, we drew upon information from the 1980, 1990 and 2000 censuses. We employed the *Neighborhood Change Data Base* (a Geolytics proprietary product) for this information because it adjusts data to account for potential changes in tract boundaries between decennial censuses. For estimates of non-census year data, we used linear interpolation.[17] We gathered indicators that have been widely employed in prior social science research on neighborhood effects, including percentages of: female-headed households, families below the poverty line, unemployed adults, Latino population, foreign-born population, homes that are renter-occupied, and the occupational distribution of employed residents.

We reduced the number of these indicators by the construction of two composite measures. First, we computed a "neighborhood social vulnerability index:" the (equally weighted) sum of census tract percentages of poor residents, unemployed workers, renter households, and female household heads. This index construction was supported by a principal components analysis that consistently across the 1970-2000 censuses in Denver produced a single component based on the roughly equally weighted sum of the above four collinear variables. This index had a possible range from 0-400. Second, we computed a neighborhood occupational prestige score, based on the 1989 General Social Survey prestige score by occupation (Davis et al., 1991) weighted by the observed proportional distribution of occupations of employees in the census tract. The range varied from a minimum possible score of 29.44 (when all resident employees are laborers) and a maximum possible score of 62.24 (when all are in managerial-professional occupations).

[15]For a review of theory and evidence related to this relationship, see Santiago and Galster (2014).
[16]For those who dropped out in middle school we employed grades reported by caregiver for middle school.
[17]We recognize that GPA is not a true ratio-level of measurement. In a supplementary test, we retained the original ordinal categories of responses and employed ordered probit models. The results were not substantially different from those produced by the simpler OLS models reported here.

The second source of neighborhood information was the Denver-based Piton Foundation's *Neighborhood Facts Database*, which provided small area-based, annually measured information culled from Denver public safety administrative databases that are not provided by the Census. We employed violent crimes and property crimes reported to police, standardized per 1,000 population. These data are aggregated by the Piton Foundation 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.

Given our retrospective longitudinal study design, some comments are in order regarding the timing of when the aforementioned neighborhood characteristics were measured. When employed as measures of context contemporaneous with the three youth outcomes investigated, they were measured as the same calendar year as the particular event occurred (such as dropping out), or when the youth turned age 18 if the event never occurred. When employed in the three adult outcome models, they were measured as averages experienced during ages 15-17. When employed as treatments in the ITT models and instruments for the TOT models (explained below), the neighborhood variables were measured for the calendar year during which DHA made the first dwelling offer to the youth's family.

To get a sense of the sorts of places where our sample resided as youths, we present descriptive statistics of their neighborhoods in Table 3. This table underscores the substantial variation in the values of our neighborhood indicators that gives our study unusual power, and provides implicit testimony to the geographic scope of DHA's scattered-site program. The data show that our average youth resided at the time of dropping out of high school (or age 18 if never) in census tracts where: mean social vulnerability index was 120 (ranging from 27-287) and occupational prestige scale was 37 (ranging from 30 to 47). Census tracts were generally diverse along ethnic and nativity lines, though with large variations: mean 51 percent Latino (ranging from 1-96) and mean 22 percent foreign-born residents (ranging from 0-71). In our sampled youths' Piton neighborhoods, mean violent crime rates were 11 per thousand population (ranging from 1-46) and mean property crime rates were 60 per thousand population (ranging from 10-391).

A comparison of the above with the mean characteristics of neighborhoods associated with the dwelling first offered by DHA to the household of the youth (second panel Table 3) indicates that offered neighborhoods were disadvantaged on several dimensions: higher rates of social vulnerability, violent crime and property crime. The mean Latino composition was comparable, but the first-offered neighborhoods had a lower mean percentage of immigrants. The variation in most first-offered neighborhood indicators was smaller. All these differences would be expected, given that a substantial number of our sampled households experienced improved economic circumstances (noted above) that allowed or required (due to lost eligibility) them to move out of DHA housing, often to a wider range of typically less-disadvantaged neighborhoods.

Both neighborhoods first offered to and experienced during adolescence by our sample can also be compared to those in the City and County of Denver as a whole (bottom panel of Table 3). As would be expected, the average neighborhood in Denver is advantaged in

aspects related to socioeconomic status and safety, and has lower percentages of ethnic minorities and foreign-born residents.

Finally, when employing as diverse a set of neighborhood indicators as we do here, the potential of multicollinearity must be raised. Indeed, our neighborhood indicators (either experienced by sample youths as adolescents or first offered by DHA to their families) are typically correlated to a statistically significant degree. Nevertheless, our tests revealed that none evinced a worrisome Variance Inflation Factor (above 5) in our models.[18] We thus have confidence that our approach to "unbundling" neighborhood indicators has the advantage of potentially revealing which dimensions are most predictive without adding confounding econometric difficulties.

Creation of Analytical Databases

We spent considerable effort cleaning, reconciling and augmenting the survey data. When our internal 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 several internal DHA administrative databases as well as via several automated or Internet-based search engines or directories.[19] Respondent data obtained from the DHA database were updated using several DHA internal databases to obtain current contact information.

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. We were able to link 92 percent of the residential locations listed by respondents.

Analytical Approach

In overview, our analytical approach takes DHA's offer of a public housing unit in a particular neighborhood as an environmental "treatment" that is exogenous from the perspective of our sampled youth. The Intent-to-Treat (ITT) effect is thus estimated by directly inserting the characteristics of the first-offered neighborhood into a model predicting a given youth or young adult outcome. The ITT effect is likely of salient interest to housing policymakers, given that it acknowledges that not all families accept this treatment (though three-quarters of our sample families did) and others who do accept it do not necessarily continuously reside there throughout the relevant child-years. By contrast, the Treatment-on-

[18]Dropping out and grades were correlated at −.35.

[19]We acknowledge that nonlinear changes between census years can create random measurement error for the within-decade neighborhood indicator values, though note that such errors are likely to bias the statistical significance of measured neighborhood effects towards zero.

Treated (TOT) effect is calculated by inserting an instrumental variable (IV) estimate of the neighborhood actually experienced by youth into a model predicting a given youth or young adult outcome. In this manner we can obtain an unbiased estimate of the true "neighborhood effect." Next we consider in more detail our instrumentation strategy and an evaluation of our instrument variables.

Instrumentation Strategy

As explained above and in detail in Appendix A, the natural experiment in Denver did not preclude the possibility of some geographic selection by households occurring either at the point of initially occupying a DHA dwelling and/or subsequently. Thus, to provide the most convincing evidence of a causal neighborhood effect we instrumented for the actual neighborhood characteristics experienced by sample youth. Our core strategy involved a classic two-stage least squares procedure whereby each of the neighborhood indicators was regressed first on all exogenous youth, household and caregiver variables shown in Table 1 and a set of excluded, identifying instruments. Our primary identifying instruments were the six neighborhood indicators associated with the dwelling first offered by DHA to the applicant at the top of the waiting list, as shown in Table 3. We supplemented these with a series of dummy variables denoting the calendar year in which this offer was made, under the supposition that these would capture some of the longer term trends in Denver neighborhoods overall, such as increasing shares of Latino and foreign-born residents.[20] We would argue that these instruments are valid because there is no plausible reason why the offer of a neighborhood or the year of such offer should: (1) influence youths' outcomes other than through their relationship with actual neighborhood context experienced; or (2) be related to unobservable caregiver characteristics related both to their neighborhood preferences and their children's outcomes.

Results for our first-stage OLS regressions are presented in Appendix Table A1. The models perform well (R-squares ranging from .24-.37), suggesting strong instruments. Most encouragingly, the most statistically significant predictor for each neighborhood indicator measured during adolescent onset[21] was its corresponding indicator measured at time of first offer (all at $p<.01$ or better). Other offered neighborhood characteristics, particularly social vulnerability and percentage of foreign-born residents, were also frequently predictive across multiple neighborhood indicators. By contrast, year of offer and household variables in Table 1 were rarely predictive.

Tests for the Quasi-Randomness of DHA Neighborhood Offers

Given our choice of instrumentation strategy it is imperative that we assess the degree to which quasi-random neighborhood offers were made by DHA. Following conventional procedures for conducting balancing tests (e.g., Jacob, 2004; Damm, 2014), we used regression to assess whether there were any non-zero relationships between individual

[20]This criterion led us to eliminate the percentage of African-American residents as a neighborhood indicator in preliminary analyses.
[21]Residential location information was cross-verified using an array of automated search engines (Anchor, Intellius, and Lexis-Nexis) as well as several Internet-based people search and telephone directories (e.g., Anywho). These additional search engines identified deceased residents (N=80 or 1.9 percent) as well as all known addresses for former DHA residents. In an attempt to reach former DHA residents, we sent letters soliciting study participation to all known addresses.

household characteristics and neighborhood characteristics first offered by DHA that could be indicative of selection, controlling for DHA allocation criteria (i.e., disability status and number of bedrooms for which the family qualifies). Specifcally, we employed OLS to estimate the statistical associations between 26 household and six offered neighborhood characteristics. A quasi-random assignment would be reflected in coefficients approximating zero.

Results are presented and discussed in detail in Appendix A (intended as online); we summarize conclusions here. The clear violation of quasi-randomness was the consistently strong significance of the race-ethnicity of the DHA tenant in predicting almost every feature of the offered neighborhood. Further analyses using balancing tests estimated separately for Latino and African American strata supported the notion of quasi-random assignment conditioned on ethnicity. Moreover, additional evidence Presented in Appendix A indicates that the DHA process produced an assignment of neighborhood characteristics that were uncorrelated with *unobserved* DHA applicant characteristics. First, only 4 percent of the associated coefficients for 13 characteristics of caregivers that were virtually impossible to observe by DHA (though we observed in our survey) were significantly different from zero using conventional standards, a figure easily incurred by chance. Not surprisingly, all but one F test on the “unobserved” set of individual characteristics failed to reject the null hypothesis of uniformly zero coefficients. Second, as detailed in Appendix A, we conducted an experiment that took as given the particular *combinations* of these 13 characteristics not observed by DHA evinced for each sample caregiver, repeatedly allocated this bundle randomly across DHA locations in Monte Carlo fashion, and estimated the pairwise correlations (and bootstrapped standard errors) of the individual and neighborhood characteristics that resulted in this hypothetical scenario. Comparison of the actual pairwise correlations to these randomly generated reference points again was consistent with quasi-randomness.

We thus have confidence that the DHA dwelling offer process sufficiently limited the possibility that any characteristics of the caregiver we could not observe were so strongly correlated with both youths’ outcomes and the neighborhood in which they resided that it would confound our causal interpretation of neighborhood effects. Nevertheless, to the extent that quasi-randomness of assignment is likely contingent on ethnicity, we will control for ethnicity in our models.

Models

Given that our outcomes are measured in different ways, it is appropriate that we measure both ITT and TOT effects with different statistical models. In the case of grade point average, we employ OLS.[22] In the case of dichotomous outcomes (dropping out, witnessing violence, having a child out of wedlock), we employ logistic regression. In the case of primarily attending school or working (instead of neither), we employ multinomial logit. In all models we report robust standard errors to account for clustering of youth in families.[23]

[22]As a test of our exclusion restriction we included calendar year of assignment in our second-stage regressions but they never proved statistically significant.

We test for whether neighborhood effects depend on age in two ways. First, we interact each neighborhood indicator by a variable measuring the age when the youth first occupied a DHA dwelling. Second, we create a dummy variable indicating whether the youth first occupied a DHA dwelling when younger than 13 years (the cutoff used by Chetty, Hendren and Katz, 2015) and interact this with each neighborhood indicator.[24] Both specifications convey distinct information. The former better captures potential age contingencies that change linearly across childhood; the latter is more appropriate for those changing discontinuously at the onset of the early adolescent developmental stage.

Results

Core Results

Estimated models for our three young adult outcomes are reported in Table 4. Results for covariates indicate that, compared to African American males. African American females are more likely to be primarily attending school and Latino males are more likely working. Higher household income during high school also is associated with higher odds of working. More household stressors and residential moves during high school years, and a caregiver with no high school diploma yield substantially higher odds of the young adult having a child out of wedlock. Females of both ethnic groups are more likely to have a child out of wedlock than males, though this is partially due to parental reporting errors.

Neighborhood effects are of more interest here. As expected, TOT effects are substantial and almost always larger in magnitude than ITT effects, of which only one is even marginally statistically significant ($p<.10$). Wald chi-square tests reject ($p<.02$ or better) the hypothesis that the coefficients of the neighborhood variables jointly equal zero for the TOT estimates (see the bottom of Table 4). Young adults raised during high school in a neighborhood of one-point higher occupational prestige have 35 percent higher odds of working and 27 percent lower odds of having a child out of wedlock. A percentage-point higher rate of Latino neighbors lowers the odds of having a child out of wedlock by three percent. Unexpectedly, one-point higher social vulnerability index also yields slightly lower odds of having a child out of wedlock.

Models for youth educational and exposure to violence outcomes are presented in Table 5. African American females outperformed African American males in terms of GPA. . African American females were also less likely than African American males to leave school without a diploma; so were youth whose caregivers were immigrants and were not disabled. Latino males and especially females were much less likely to witness violence than African Americans of either gender; the same was true for youth who moved more during childhood and whose caregiver was single.

[23]The particular statistics shown in Appendix Table A1 apply to the high school dropout outcome but are representative of all such first stage results. Recognize that such first-stages were used to generate a distinct set of IVs for each of our six outcomes, given the dependent variables in each were measured at a different point of onset.

[24]In a supplementary test, we retained the original ordinal grade categories of caregiver responses and employed ordered probit models. The results (available from the authors) were not substantially different from those produced by the simpler OLS models reported here.

As in the case for young adult outcomes, ITT effects on youth outcomes here are not as powerful in magnitude nor as statistically significant (either individually or jointly) as TOT effects. TOT estimates indicate that a youth being raised in a neighborhood with a one-point higher occupational prestige score has 30 percent lower odds of witnessing violence in the neighborhood. A youth with one percentage-point higher Latino neighbors can expect a .02 boost in GPA and a five percent reduction in the odds of dropping out. Each percentage-point increase in foreign-born neighbors lowers GPA by .04, however.

Age Interaction Models

Models for young adult outcomes that permit neighborhood effects to vary continuously with age of assignment to DHA housing are presented in Table 6; for brevity these and all forthcoming Tables suppress covariate parameters.[25] What becomes immediately apparent in examining Table 6 is that all of our observed neighborhood effects on young adult outcomes are contingent upon age of youth assignment. Specifically, all correspond with the Chetty, Hendren and Katz (2015) finding that assignment at younger ages produces a stronger effect in the long run. Overall, the models in Table 6 reveal many more statistically significant neighborhood indicators (especially in the ITT estimates) whose predictive power was apparently obscured in the core model because of its failure to account for age contingency.

Neighborhood occupational prestige offers the clearest illustration of a consistent contributor to all three young adult outcomes whose power diminishes the older the child at time of assignment to DHA housing. The TOT effects from Table 6 show that a child first assigned DHA housing at age one moving into a neighborhood with one-point higher occupational prestige score will have as a young adult 53 percent higher odds of attending school, 52 percent higher odds of working and 35 percent lower odds of having a child out of wedlock[26] than a comparable child. If, however, the child were to move into the same neighborhood at age 15 the respective changes in these odds would decline to 23, 37 and 27 percent.

Results for the foreign-born composition and violent crime indicators exhibit such extreme age interactions that the implied *direction* (not just magnitude) of effect changes at some point during childhood. In the case of ITT effects, a child first assigned DHA housing at age one moving into a neighborhood with one percentage-point higher foreign-born population will have as a young adult 20 percent lower odds of attending school, 18 percent lower odds of working and one percent lower odds of having a child out of wedlock than a comparable child. These predictions all switch to higher odds of these outcomes, however, if the child is assigned at ages 7, 10, 12 (or later), respectively. A child first assigned DHA housing at age one moving into a neighborhood with one percentage-point higher rate of violent crime will

[25]We do not need to worry about clustering at the neighborhood level here because sampled youth who lived in the same neighborhood during their respective secondary school years typically experienced different values for all the neighborhood indicators because they were experiencing such for different calendar years (ranging from the late 1980s to the late 2000s). There thus is no *commonly experienced* "higher spatial scale" as is typically the case in hierarchical data structures when all values of the variable(s) at the higher scale are identical for all observations at the lower scale. As a check, however, we estimated our models with robust standard errors based on clustering at the neighborhood level and found the standard errors to be much smaller than those generated by clustering at the family level. We chose the more conservative approach to report here.

[26]Our sample sizes were too small to permit stratification by age less than 13 and 13-18 years.

have 13 percent higher odds of attending school and 12 percent higher odds of working as a young adult[27] than a comparable child. These predictions all switch to the expected lower odds of these outcomes, however, if the child is assigned at age 11 or later.[28]

Results for age interaction models of youth outcomes are presented in Table 7. They indicate some variations in neighborhood effects depending on the age of first assignment into DHA, though these variations are less precisely measured than in the models of young adult outcomes. Nevertheless, the results counter the conclusions of Chetty, Hendren and Katz (2015). Higher property crime rates and lower percentages of Latino neighbors are associated with progressively lower secondary school grades the older students are when they are assigned to DHA housing.

Youth Outcomes in Young Adult Outcome Models

Given that we have found strong neighborhood effects on both youth secondary school outcomes and on young adult educational, employment and fertility outcomes, it is natural to enquire about the degree to which neighborhood effects on the latter are mediated by the former. To explore this we added our three youth outcome variables as predictors in the core and age interaction models of young adult outcomes; results are shown in Table 8.

Two conclusions immediately emerge from Table 8. First, the youth outcomes measured strongly affect the young adult outcomes measured. Second, neighborhood effects on young adult outcomes are only partially mediated by their effect on measured youth outcomes. More concretely, leaving school without a diploma is an immensely strong predictor of less likelihood of post-secondary education and more likelihood of having a child out of wedlock. Having higher grades in secondary school predicts greater odds of primarily engaging in post-secondary education or working. None of these findings are unusual. What is unexpected is the lack of any apparent negative consequences of witnessing violence prior to age 18 for the outcomes measured.

Despite the aforementioned strong explanatory roles played by grades and dropping out, our core neighborhood predictors—occupational prestige, percentages of Latino and foreign-born residents— maintain substantial and statistically significant power in the models. Moreover, the previously observed patterns of age interactions persist (see the bottom panel of Table 8). All of these findings clearly suggest that these aspects of neighborhoods have broader impacts on young adults than merely affecting their performance and attainments in secondary school or exposure to violence as youths.

Discussion

Inside the Black Box of Neighborhood

A shortcoming of the MTO experiment was its uncontrolled heterogeneity of treatment. Not only were all three comparison groups allowed to move anywhere during the demonstration (the experimental group after one year of residence in a low-poverty neighborhood), but the

[27]Though we note this interaction is only significant at $p<.10$.
[28]Though we note this interaction is only significant at $p<.10$.

wide variations in the constituent characteristics associated with their neighborhoods were not controlled. The offer of a neighborhood with under 10 percent poverty rates thus remains a “black box” in quantitative studies based on MTO data. By contrast, our natural experiment permits some quantitative analyses of what “active ingredients” are at work in a “good neighborhood treatment.” Though we cannot pinpoint specific causal linkages given the multiplicity of causal mechanisms (Galster, 2012), we offer suggestive explanations in the neighborhood indicator domains of socioeconomic status and ethnicity. These explanations are fully consistent with the growing consensus that neighborhood effects operate primarily through developmental effects on children (cf. Sampson, 2008; Chetty, Hendren and Katz, 2015), though they still may have some direct impacts on adults (Tach et al., 2016).

Being raised in a neighborhood where workers exhibit higher occupational prestige clearly provides benefits to low-income Latino and African American youth regardless of age of assignment, though younger children benefit more. These benefits first take the form of reduced odds of dropping out of school before earning a diploma, which, in turn, greatly reduces the odds of neither primarily working nor attending school as young adults, or having a child outside of marriage. But these effects on young adult outcomes are not completely mediated by secondary school attainment. These results have intuitive appeal from the perspective of local networks and socialization forces related to pro-educational, pro-employment and delayed child bearing behaviors. Neighborhoods that surround their children with higher-prestige workers likely expose them to norms and role models that encourage economic success, and provide greater access to networks of information about post-secondary school and employment opportunities, prerequisites and payoffs, and the skills needed to take advantage of them. These interpretations are consistent with those produced by qualitative research on both the MTO and Gautreaux programs. Some low-income, minority MTO caregivers in low-poverty (presumably, higher prestige than originally occupied) neighborhoods stressed during interviews the value of adult role modeling of work habits for their children and the “soft skill” enhancement that improved their employment prospects (Briggs, Popkin and Goering, 2010; Briggs et al, 2011). This mimics results from Gautreaux that suggested how positive role models and higher economic expectations in advantaged neighborhoods positively influenced lower-income African American youth in-movers (Rosenbaum, 1991; Rosenbaum, DeLuca and Tuck, 2005).

Regardless of when assigned, low-income Latino and African American youths’ exposure to more Latino neighbors results in better grades (for older children), reduced odds of dropping out of school and having a child outside of marriage, but also reduced odds of undertaking post-secondary education. We think these findings are understandable when seen through the lens of the protective forces of collective socialization and social controls within the Latino community. Strong (often gendered) mores prohibiting sexual risk-taking and stressing educational attainment have been observed within the Latino community (Erikson, 1998). These same norms may so emphasize the importance of working as a young adult, however, that they might lower the propensity to seek higher education. Latino adults within neighborhoods may provide a significant service for all youths who reside there by more

actively monitoring their public behaviors as well as those occurring in Latino homes, thereby broadly promoting more favorable educational and fertility outcomes.[29]

Why Age-Contingent Effects?

We have demonstrated the strong degree to which the relationship between age of child when administered the neighborhood treatment and strength of subsequent outcomes depends both on the particular contextual characteristic(s) and outcome involved, consistent with the position of Sharkey and Faber (2014) that age effects are contingent on the specific mechanism of neighborhood effect at work.

This discussion should be contrasted to the Chetty, Hendren and Katz (2015) explanation for their age-contingent findings. They posited a heuristic model combining a linear function between strength of the positive effect and duration of exposure to a low-poverty neighborhood and a discontinuous, negative "disruption effect" associated with the fracturing of social networks and short-term adaptation problems caused by moving residential locations, which is positively associated with age. When children move at a younger age they have adequate time for the former effect to dominate in producing net favorable adult outcomes; when they move as teens the latter negative impact dominates. Our findings suggest that this framing is too simplistic. We do not deny the importance of well-established moving disruption effects (Wood et al., 1993; South et al., 2007); indeed our own results indicate more moving during childhood has deleterious consequences for nonmarital childbearing (Table 4). Rather, we challenge the notion of a generalized linear function between exposure duration and effect size, instead arguing that developmental stage-specific differences in vulnerability to the given neighborhood effect mechanism must also be superimposed, consistent with the formulations of Harding et al. (2011) and Galster (2012).

The simple function posited by Chetty, Hendren and Katz (2015) is perfectly consistent with our findings concerning occupational prestige and Latino composition, which we argued above were likely produced by a range of social-interactive processes and institutional contexts that take a considerable period of time to exert themselves in cumulative fashion. By contrast, the aforementioned evidence that many neighborhood indicators have weaker effects at younger ages of assignment (or even change the direction of their effect during childhood) is inconsistent with this formulation. The case of neighborhood safety is particularly illustrative. The deleterious impacts of neighborhood property crime (on grades and staying in school until a diploma is obtained) and violent crime (on young adult education and work) are clearly much stronger when youths are assigned to DHA housing as teens.[30] This result is consistent with a causal mechanism that renders teens especially vulnerable, even after short durations of exposure. We see several such plausible mechanisms related to safety that could have a rapid, disparate impact upon teens but not younger children. In neighborhoods with more crime, there may be more opportunities for

[29]Robustness tests using interactions with dummies indicating assignment at age below 13 confirmed that the relationship between violent crime and these outcomes at young ages was positive.

[30]For deeper studies on the social capital built within Latino communities, see, e.g., Sanchez (1993), Klinenberg (2002), Ricourt and Danta (2003) and Dávila (2004).

and peer behaviors encouraging teen participation in risky, “street life” or even illegal activities at the expense of academic achievement.[31] Economic disincentives for such achievement may ensue if local illegal markets (such as those associated with the drug trade) seemingly offer more lucrative income-earning potential than legal labor force activities requiring superior educational credentials. Unsafe neighborhoods may also heighten teens’ risks of personal exposure to violence, identified by others as an important predictor of negative cognitive and educational outcomes (Sharkey and Sampson, 2010, 2015; Sharkey et al., 2012, 2014), though our measures apparently were incapable of identifying these relationships.

Implications for Housing Policy

Our results imply that good assisted housing policy can be good education and economic development policy as well. We have demonstrated in this Denver natural experiment that a well-conceived and operated scattered-site public housing program can yield significant improvements in secondary school, post-secondary school, employment and nonmarital fertility outcomes of low-income Latino and African American young adults (cf. Schwartz, 2010; Casciano and Massey, 2012). We see no reason why these results could not be generalized to other site-based assisted housing programs, and may well apply to tenant-based voucher assistance as well.

These gains could be realized in two, not-mutually exclusive ways: changing the spatial distribution of assisted housing units (i.e., their associated neighborhood characteristics) and changing the allocation of extant assisted dwellings to residents based on the ages of their children. First, as is obvious, policymakers should extend their efforts to develop more assisted housing in neighborhoods that have higher occupational prestige and lower crime rates and/or improve these indicators in the environs of existing assisted housing units. Such changes would provide long-term payoffs for all children residing there, regardless of their ages, based on the findings of our study.

Second, and perhaps more controversially, our results suggest that the age of children should play some role in determining which households on the assisted housing waiting lists are assigned to which neighborhoods, instead of the current “first-come/first-served” rule. Assuming that a local housing provider (authority, community development corporation, etc.) has dwellings located in a manner providing some variation in neighborhood characteristics, they should make allocation decisions in ways that would maximize the future prospects for children participating in their program to become self-sufficient adults. According to our results, this means assigning families with the youngest children to neighborhoods with the highest occupational prestige and families with teens to the safest neighborhoods. Although we have also found that the ethnic and nativity compositions of the neighborhood have effects upon children, we do not advocate employing them as assignment criteria since this will run afoul of fair housing legal and ethical considerations.

[31]Indeed, models employing interactions with dummy variables indicating assignment before age 13 (not shown) indicate no negative impacts of property or violent crime on youth or young adult outcomes if assignment occurs before age 13.

These implications for the assignment of assisted housing applicants offer subtle contrasts with analogous recommendations made by Chetty, Hendren and Katz regarding the voucher program (2015: 35-36). They argued for a policy of giving MTO-style experimental vouchers to families with the youngest children, since their research suggested that they would benefit the most from long-term residence in low-poverty neighborhoods whereas teens may actually be harmed by such moves. Our research by implication offers two amendments to this recommendation. Indeed, families with younger children should be assigned to lower poverty neighborhoods, but only if these places are associated with higher occupational status of those who are employed, not simply places with an absence of disadvantage (higher percentages female-headed households, renters, non-employed, and poor), which we have shown to be a distinct dimension of neighborhoods. Moreover, unlike Chetty, Hendren and Katz (2015), who imply that families with teens should not be moved, our findings suggest that teens living in high-crime neighborhoods should be helped to move to safer environments to lessen the considerable risks in their economic prospects they will face otherwise.

Caveats and Shortcomings

The obvious caveat is that our findings were produced in Denver, and thus may not be generalizable to metropolitan areas having different characteristics.[32] On a related point, Denver's Latino population primarily consists of those of Mexican ancestry; there may be different neighborhood effects generated in Cuban-, Dominican- and Puerto Rican-origin communities. The first shortcoming of our study is that, although we have unbundled neighborhood characteristics more than MTO-based studies, we do not have a comprehensive portrait of the neighborhood context. In particular, we have not investigated here the role of public and private institutional resources, schools, amenities, public services and the natural environment. We do not think that these omissions weaken our finding of some kind of context effect, but do constrain us in identifying the precise source of this effect. For example, our multiple findings of neighborhood socioeconomic characteristics affecting secondary school outcomes may have arisen due to their correlation with (unmeasured) school characteristics.[33]

Second, our investigation cannot distinguish how the influence of neighborhood contexts may vary by developmental stage and by duration and consistency of exposure. Our instrumentation strategy relied on identifying the exogenous variation in neighborhood context contemporaneously experienced when a youth outcome occurred. We can tell when this "treatment" was first offered by DHA but cannot not distinguish how long this environment had been actually experienced. Thus, our finding of an inverse relationship between power of neighborhood effect and age of assignment may be due to children being more vulnerable to context at a younger age (Sampson, Sharkey, and Raudenbush, 2008) or due to longer cumulative neighborhood exposures on average (Crowder and South, 2011; Wodtke, Harding and Elwert, 2011; Hedman et al., 2015).

[32]The importance of neighborhood and school peers in encouraging youth criminality has been convincingly demonstrated by Billings, Deming, and Ross (2016) with a natural experiment.

[33]For example, we find that neighborhood occupational prestige and Bifulco, Fletcher and Ross (2011) find that the share of students in a school cohort with college-educated mothers is inversely related to the odds of dropping out of high school.

Third, our samples were too small to estimate reliably whether the patterns of age-contingent neighborhood effects were also contingent upon the gender and/or ethnicity, as have been observed in other studies (e.g., Galster, Andersson and Musterd, 2010; Sharkey and Faber, 2014; Galster et al., 2015; Galster et al., 2016) or whether they manifested nonlinear relationships (Galster, 2014). Finally, we have not attempted to probe potential ways in which neighborhood context may affect young adult outcomes via youths' risky behaviors, nutrition, and health, which might reveal more about underlying causal mechanisms.

Conclusion

Our analysis of data from a natural experiment involving Denver public housing residents reveals substantial effects of neighborhood socioeconomic status, ethnicity and safety domains on a variety of youth and young adult educational, employment and fertility outcomes for a low-income Latino and African American population. These effects are highly contingent on when a youth was first exposed to a new neighborhood, though it is clearly not the case that effect sizes are always greater for earlier ages of exposure; it depends on the neighborhood characteristic. Benefits from neighbors of higher occupational prestige that manifest themselves later in life are stronger if a child begins experiencing them at a younger age. The negative consequences of neighborhood crime, on the other hand, are only manifested if the youth is exposed to them as a teen. A model of neighborhood effect size that depends on the interaction among exposure duration, disruption effects of residential mobility, and developmental stage-specific differences in vulnerability to the given neighborhood effect mechanism at work can explain these findings. Our results hold powerful and provocative implications for where assisted housing is developed and how applicants are assigned to neighborhoods.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Appendix

Appendix Table A1

First-Stage OLS Regression Results for Instrumentation

Exogeous predictors; measured at first offer	**Neighborhood Characteristics Measured at Time of Onset^**									
Individual Characteristics	**Social Vulnerability**		**Occupational Prestige**		**Latino Residents (%)**		**Foreign-Born (%)**		**Violent Crime R**	
Latina female	13.7	(8.46)	−1.34 *	(0.64)	19.1 ***	(3.86)	6.19 **	(2.13)	0.79	(0.97
Latino male	16.5 *	(7.65)	−1.63 *	(0.66)	20.8 ***	(3.66)	5.22 *	(2.02)	0.64	(0.85

Exogeous predictors; measured at first offer	Neighborhood Characteristics Measured at Time of Onset^									
Individual Characteristics	**Social Vulnerability**		**Occupational Prestige**		**Latino Residents (%)**		**Foreign-Born (%)**		**Violent Crime R**	
African American female	4.62	(6.13)	−0.54	(0.53)	1.96	(3.23)	1.65	(1.85)	1.42	(0.95
Child was firstborn	11.9*	(5.04)	−0.31	(0.36)	−1.19	(2.08)	−1.95	(1.25)	1.82**	(0.66
Household Characteristics										
# siblings	−0.084	(2.77)	−0.051	(0.19)	−0.75	(1.19)	−0.53	(0.73)	0.010	(0.33
PC immigrant	13.1	(9.37)	−0.28	(0.49)	4.87	(3.18)	1.36	(1.73)	0.76	(0.90
PC abuses substances	13.4	(7.00)	0.14	(0.60)	3.14	(3.55)	3.31	(2.23)	−0.60	(0.97
PC disabled	1.56	(12.2)	0.19	(0.84)	−9.82	(4.99)	−4.46	(2.37)	2.52	(1.53
PC HS diploma or higher	−9.14	(5.88)	−0.54	(0.46)	3.00	(2.77)	0.83	(1.69)	−0.77	(0.74
PC married or partnered	3.52	(5.68)	−0.65	(0.41)	−0.68	(2.70)	−1.61	(1.56)	0.26	(0.72
Household income (ln)	0.053	(0.68)	0.032	(0.048)	0.049	(0.30)	0.072	(0.17)	0.035	(0.08
Household stressors score	0.24*	(0.12)	−0.012	(0.010)	0.090	(0.11)	0.0030	(0.039)	0.038*	(0.01
# moves since birth	0.046	(0.18)	0.013	(0.024)	−0.19	(0.16)	−0.18	(0.11)	0.067	(0.04
Time of DHA First Offer										
offer1988	−1.13	(12.6)	−1.26	(1.00)	1.63	(5.76)	2.18	(3.24)	−1.31	(1.51
offer1989	19.8	(13.1)	−2.28*	(0.91)	7.63	(5.94)	2.24	(2.93)	4.17*	(1.95
offer1990	3.62	(10.1)	−0.89	(0.85)	5.00	(5.33)	4.98	(2.96)	−0.97	(1.60
offer1991	−11.1	(16.3)	−0.54	(0.98)	−3.47	(6.35)	1.23	(4.01)	−1.44	(1.80
offer1992	−5.63	(14.1)	−1.06	(0.97)	2.84	(5.92)	−1.13	(3.01)	−0.96	(1.52
offer1993	−12.2	(16.8)	1.19	(2.26)	−0.43	(16.6)	1.22	(7.68)	−1.53	(3.94
offer1994	−6.17	(14.7)	−0.18	(1.24)	−2.55	(6.26)	1.55	(3.61)	−0.54	(1.66
offer1995	−2.97	(15.3)	−0.38	(1.20)	3.02	(6.22)	1.88	(3.82)	−0.90	(2.02
offer1996	7.18	(14.2)	−1.39	(1.17)	7.65	(6.28)	5.21	(4.14)	−1.14	(1.90
offer1997	18.7	(11.5)	−2.75**	(0.88)	12.3*	(5.38)	3.38	(3.13)	−0.57	(1.42
offer1998	−2.33	(15.1)	−1.07	(0.97)	11.6	(7.15)	5.91	(3.91)	−0.77	(1.64
offer1999	−8.63	(14.4)	−0.22	(1.07)	−2.15	(8.74)	0.82	(4.82)	−3.16*	(1.51
offer2000	−15.7	(13.0)	−1.30	(0.98)	6.25	(6.59)	3.12	(4.05)	−2.88	(1.57
offer2001–03	−11.5	(14.3)	−1.75	(1.28)	15.2	(9.02)	7.48	(4.50)	0.094	(1.96
Neighborhood Characteristics										
Social Vulnerability	0.43***	(0.10)	0.00049	(0.0061)	0.048	(0.045)	0.020	(0.027)	0.020	(0.01
Occupational Prestige	0.91	(1.31)	0.38***	(0.098)	0.47	(0.60)	−0.41	(0.36)	0.15	(0.15
Latino Residents (%)	−0.34	(0.22)	0.0043	(0.020)	0.38**	(0.12)	−0.11	(0.064)	−0.0060	(0.02
Foreign-Born Residents (%)	1.27*	(0.56)	−0.075	(0.045)	0.50	(0.28)	0.77***	(0.16)	0.10	(0.07
Violent Crime Rate	0.14	(0.35)	−0.032	(0.028)	0.43*	(0.18)	0.15	(0.11)	0.20***	(0.05
Property Crime Rate	0.025	(0.12)	0.00030	(0.0074)	−0.044	(0.046)	−0.0038	(0.026)	0.0013	(0.01
Constant	−15.1	(57.4)	27.2***	(4.44)	−17.1	(28.0)	24.3	(16.3)	−7.36	(6.84
Observations^	393		393		393		393		348	
R-squared	0.28		0.24		0.37		0.25		0.31	
p < F stat	1.7e-13		7.8e-11		1.2e-23		2.0e-11		1.1e-18	

^ Using HS drop-out outcome;
* p <0.05;
** p <0.01;
*** p <0.001; robust std. errors adjusted for clustering in households shown in parentheses

Biographies

George Galster is the Clarence Hilberry Professor of Urban Affairs in the Department of Urban Studies and Planning at Wayne State University. His research has focused on urban neighborhoods and housing markets, exploring why they change and how they change the people who live within them. This has resulted in over 145 peer-reviewed articles, 34 book chapters and eight books. In 2016 he was awarded the "Contributions to the Field of Urban Affairs" prize by the Urban Affairs Association. He earned his Ph.D. in Economics from M.I.T.

Anna Maria Santiago is a Professor of Social Work at Michigan State University. Her research is concerned with identifying the ways in which contemporary housing policies can be used to reduce the social and economic disparities experienced by vulnerable families and children residing in U.S. urban areas. Her publications include numerous peer-reviewed articles, book chapters, and reports. She earned her Ph.D. in Urban Social Institutions from the University of Wisconsin-Milwaukee.

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Table 1

Descriptive Statistics of Individual Youth and their Households

Individual Characteristics	N	Mean	Std Dev	Min	Max
Latina Female	425	0.26	0.44	0	1
Latino Male	425	0.27	0.45	0	1
Black Female	425	0.23	0.42	0	1
Black Male (omitted category)	425	0.24	0.43	0	1
Child Was Firstborn	425	0.41	0.49	0	1
Household / PC Characteristics ^					
# of Siblings in Household	425	1.56	1.38	0	7
PC Immigrant	425	0.12	0.32	0	1
PC Abuses Substances	425	0.17	0.38	0	1
PC Had a Disability	425	0.11	0.32	0	1
PC Has HS Diploma or More	425	0.65	0.48	0	1
PC Married or Partnered	425	0.34	0.47	0	1
Household Income (ln)	425	6.77	4.50	0	11.11
Household Stressors Scale	425	1.09	1.10	0	5.00
Mean Annual # of Moves	425	0.25	0.14	0	0.89

^ Measured at time of onset of dropping out or age 18; PC = parent or caregiver

Table 2

Descriptive Statistics of Youth and Young Adult Outcomes

Young Adult Outcomes (age 18+)	**Mean**	**Std Dev**	**Min**	**Max**
Primarily Attended School[#]	0.136	0.343	0	1
Primarily Employed Full- or Part-time[#]	0.696	0.461	0	1
Had Out of Wedlock Child pre-age 25	0.367	0.483	0	1
Youth Outcomes (pre-age 18)				
GPA in Last Secondary School Attended	2.586	0.888	1	4
High School Drop Out (No Diploma)	0.313	0.464	0	1
Witnessed Violence in Neighborhood	0.313	0.465	0	1

[#]primary activity post-age 18; neither working nor attending school = reference

Note: Statistics shown are for samples used in ITT analyses

Table 3

Descriptive Statistics for Youths‘ Neighborhoods

Neighborhood Characteristics					
At Time of Onset (or age 18 if never)^	**N**	**Mean**	**Std Dev**	**Min**	**Max**
Social Vulnerability	390	119.6	53.4	26.5	286.7
Occupational Prestige	390	36.9	3.3	30.1	47.4
Latino Residents (%)	390	50.6	23.0	1.0	96.1
Foreign-Born Residents (%)	390	22.3	12.0	0.3	70.7
Violent Crime Rate	346	10.7	7.4	1.0	46.1
Property Crime Rate	346	59.9	41.8	9.9	390.7
At Time of First Offer					
Social Vulnerability	425	159.5	57.0	18.8	284.4
Occupational Prestige	425	34.8	3.2	30.3	46.9
Latino Residents (%)	425	52.8	19.4	4.6	84.7
Foreign-Born Residents (%)	425	13.8	6.7	1.9	36.6
Violent Crime Rate	425	14.9	10.9	0.8	51.1
Property Crime Rate	425	98.2	54.4	2.2	294.4
Estimated by Instrumental Variables					
Social Vulnerability	385	118.6	29.6	21.5	198.4
Occupational Prestige	385	36.9	1.7	33.3	43.7
Latino Residents (%)	385	50.6	14.5	6.6	85.2
Foreign-Born Residents (%)	385	22.5	5.6	10.7	39.8
Violent Crime Rate	341	10.5	3.9	0.9	23.6
Property Crime Rate	341	58.6	20.0	6.2	141.8
Denver County, 2000					
Social Vulnerability		96.7	42.0		
Occupational Prestige		41.0	4.4		
Latino Residents (%)		28.9	24.6		
Foreign-Born Residents (%)		15.8	10.8		

Neighborhood Characteristics					
At Time of Onset (or age 18 if never)^	**N**	**Mean**	**Std Dev**	**Min**	**Max**
Violent Crime Rate		5.6	6.7		
Property Crime Rate		46.8	43.6		

^ These data apply to the drop-out sample

Table 4

ITT and TOT Effects of Neighborhood (odds ratios) on Young Adult Outcomes

	Attend School[#]		Employed FT or PT[#]		Out of Wedlock Child	
	ITT	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Neighborhood Characteristics[^^]						
Social Vulnerability	0.999	1.012	0.994	0.987	0.992*	0.973***
Occupational Prestige	0.970	1.156	1.069	1.353**	0.884***	0.725***
Latino Residents (%)	0.976	0.958	0.995	0.980	0.992	0.966**
Foreign-Born Residents (%)	1.020	1.110*	0.968	1.032	1.022	1.051
Violent Crime Rate	1.020	1.062	1.017	1.070	1.005	1.045
Property Crime Rate	1.001	0.991	1.002	1.007	1.003	1.017
Individual Characteristics						
Latina Female	2.661	3.871	1.611	3.009	1.954*	2.559**
Latino Male	0.912	1.468	1.352	3.460*	0.918	1.375
Black Female	3.199**	4.466**	1.495	2.590	2.181***	1.548
Child Was Firstborn	1.428	1.323	1.344	1.484	0.981	1.157
PC & Household Characteristics[^]						
# of Siblings in Household	0.842	0.856	1.068	1.137	0.989	0.948
PC Immigrant	1.689	1.793	1.173	1.121	0.512*	0.742
PC Abuses Substances	2.859	4.212*	1.893	3.521*	0.552*	0.743
PC Had a Disability	0.393	0.395	0.763	0.544	1.058	0.888
PC Has HS Diploma or More	1.499	1.827	1.088	1.186	0.526***	0.393***
PC Married or Partnered	0.667	0.627	1.246	1.473	0.864	0.833
Household Income (ln)	1.042	1.000	1.101**	1.069*	1.006	1.043
Household Stressors Scale	1.337*	1.224	1.055	1.024	1.71*	2.080**
Mean Annual # of Moves	0.671	0.253	7.282	3.141	6.437**	6.071***
Observations	358	316	358	316	414	368

	Attend School#		Employed FT or PT#		Out of Wedlock Child	
	ITT	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Pseudo R-squared	0.091	0.110	0.091	0.110	0.083	0.104
Wald Chi-squared	57.40	63.55	57.40	63.55	38.07	42.19
prob > Chi-squared	0.023	0.006	0.023	0.006	0.006	0.002
Chi-squared (neigh'd vars. only)	14.62	26.47	14.62	26.47	9.36	15.18
prob > Chi-squared	0.264	0.009	0.264	0.009	0.154	0.019

[*] $p < 0.10$;

[**] $p < 0.05$;

[***] $p < 0.01$; based on robust std. errors adjusted for clustering in households

[#] primary activity post-age 18; neither working nor attending school = reference

[^] measured as average during high school years; PC = parent or caregiver

[^^] ITT uses first-offered neighborhood; TOT uses IV for neighborhood experienced; see Table A1

Table 5

ITT and TOT Effects of Neighborhood (odds ratios except for GPA) on Youth Outcomes

	Last School GPA^^^		High School Drop Out		Witnessed Violence	
	ITT	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Neighborhood Characteristics^^						
Social Vulnerability	0.002	0.001	0.996	0.993	0.997	0.988
Occupational Prestige	0.018	0.018	0.863**	0.857	0.839**	0.697**
Latino Residents (%)	0.004	0.023***	0.975**	0.950**	1.002	1.029
Foreign-Born Residents (%)	−0.004	−0.036***	1.016	1.052	1.024	0.969
Violent Crime Rate	0.002	0.002	0.986	0.917	0.981	0.989
Property Crime Rate	−0.002	−0.004	1.004	1.027**	1.009	1.021
Individual Characteristics						
Latina Female	0.205	−0.02	1.458	1.494	0.355**	0.164**
Latino Male	−0.013	−0.324*	1.568	1.946	0.495	0.216**
Black Female	0.343***	0.378***	0.460**	0.343**	1.420	1.475
Child Was Firstborn	−0.073	0.025	1.081	1.006	1.496	1.382
PC & Household Characteristics^						
# of Siblings in Household	0.043	0.047	1.186*	1.188	1.070	0.913
PC Immigrant	0.160	0.277	0.387**	0.250***	0.542	0.340
PC Abuses Substances	−0.100	0.009	0.791	0.611	0.810	1.113
PC Had a Disability	−0.258	−0.081	2.384**	2.365*	0.515	0.540
PC Has HS Diploma or More	0.085	0.111	0.704	0.621	1.426	1.354
PC Married or Partnered	−0.052	−0.037	1.478	1.695*	2.34**	2.459**
Household Income (ln)	−0.006	−0.001	0.981	0.973	0.992	0.975
Household Stressors Scale	−0.023	−0.068	0.965	1.039	1.362**	1.225
Mean Annual # of Moves	0.312	0.253	0.579	0.559	0.093**	0.106***
Constant	1.462	1.587	N/A	N/A	N/A	N/A

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	Last School GPA^^^		High School Drop Out		Witnessed Violence	
	ITT	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Observations	403		425		297	
R-squared (Pseudo for logit)	0.054	0.086	0.081	0.115	0.171	0.200
F (or Wald Chi-squared for logit)	1.28	2.10	35.55	38.7	40.99	38.32
prob > F (or Chi-squared)	0.196	0.006	0.012	0.005	0.000	0.005
F (or Chi-squared) (neigh'd vars.)	0.44	1.97	8.02	13.77	15.07	15.67
prob > F (or Chi-squared)	0.849	0.071	0.237	0.030	0.020	0.016

* $p < 0.10$;

** $p < 0.05$;

*** $p < 0.01$; based on robust std. errors adjusted for clustering in households

^ measured at first occurrence of outcome, or age 18 if never outcome; PC = parent or caregiver

^^ ITT uses first-offered neighborhood; TOT uses IV for neighborhood experienced; see Table A1

^^^ Estimated via OLS; all others via logit

Table 6

ITT and TOT Effects of Neighborhood (odds ratios) on Young Adult Outcomes Interactions with Age when First Resided in DHA Housing

	Last School GPA^^^		**High School Drop Out**		**Witnessed Violence**	
Neighborhood Characteristics w/Interaction with Age into DHA^^	**ITT**	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Social Vulnerability	−0.003	−0.002	1.007	1.023	1.007	0.993
*Age into DHA	0.000	0.000	0.999	0.997	1.000	1.001
Occupational Prestige	0.013	0.046	0.845***	0.842	0.856*	0.767
*Age into DHA	−0.001	−0.002	1.004	1.001	1.004	0.992
Latino Residents (%)	−0.011	0.003	1.003	0.960	1.001	0.990
*Age into DHA	0.001**	0.002	0.997	0.999	1.000	1.004
Foreign-Born Residents (%)	0.005	−0.032	0.992	0.967	1.103	0.950
*Age into DHA	−0.001	0.000	1.003	1.007	0.992	1.004
Violent Crime Rate	0.003	−0.022	0.971	1.019	1.020	1.132
*Age into DHA	−0.001	0.003	1.003	0.991	0.996	0.983
Property Crime Rate	0.005	0.009	0.994	0.977	1.010	1.015
*Age into DHA	−0.001**	−0.001	1.001	1.005*	0.999	0.999
R-squared (Pseudo for logit)	0.098	0.115	0.093	0.125	0.305	0.343
F (or Wald Chi-squared for logit)	2.42	2.59	46.84	47.04	58.44	64.51
prob > F (or Chi-squared)	0.000	0.000	0.005	0.005	0.000	0.000
F (or Chi-squared) (neigh'd vars.)	2.43	2.31	16.44	20.24	46.34	48.51
prob > F (or Chi-squared)	0.005	0.009	0.172	0.063	0.000	0.000

*p <0.10;

**p <0.05;

***p <0.01; based on robust std. errors adjusted for clustering in households

^^ITT uses first-offered neighborhood; TOT uses IV for neighborhood experienced; see Table A1

^^^Estimated via OLS; all others via logit All models include covariates as in Table 1

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Table 7

ITT and TOT Effects of Neighborhood (odds ratios except GPA) on Youth Outcomes; Interactions with Age when First Resided in DHA Housing

	Last School GPA^^^		High School Drop Out		Witnessed Violence	
Neighborhood Characteristics w/Interaction with Age into DHA^^	**ITT**	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Social Vulnerability	−0.003	−0.002	1.007	1.023	1.007	0.993
*Age into DHA	0.000	0.000	0.999	0.997	1.000	1.001
Occupational Prestige	0.013	0.046	0.845***	0.842	0.856*	0.767
*Age into DHA	−0.001	−0.002	1.004	1.001	1.004	0.992
Latino Residents (%)	−0.011	0.003	1.003	0.960	1.001	0.990
*Age into DHA	0.001**	0.002	0.997	0.999	1.000	1.004
Foreign-Born Residents (%)	0.005	−0.032	0.992	0.967	1.103	0.950
*Age into DHA	−0.001	0.000	1.003	1.007	0.992	1.004
Violent Crime Rate	0.003	−0.022	0.971	1.019	1.020	1.132
*Age into DHA	−0.001	0.003	1.003	0.991	0.996	0.983
Property Crime Rate	0.005	0.009	0.994	0.977	1.010	1.015
*Age into DHA	−0.001**	−0.001	1.001	1.005*	0.999	0.999
R-squared (Pseudo for logit)	0.098	0.115	0.093	0.125	0.305	0.343
F (or Wald Chi-squared for logit)	2.42	2.59	46.84	47.04	58.44	64.51
prob > F (or Chi-squared)	0.000	0.000	0.005	0.005	0.000	0.000
F (or Chi-squared) (neigh'd vars.)	2.43	2.31	16.44	20.24	46.34	48.51
prob > F (or Chi-squared)	0.005	0.009	0.172	0.063	0.000	0.000

*p <0.10;

**p <0.05;

***p <0.01; based on robust std. errors adjusted for clustering in households

^^ITT uses first-offered neighborhood; TOT uses IV for neighborhood experienced; see Table A1

^^^Estimated via OLS; all others via logit

All models include covariates as in Table 1

Table 8

ITT and TOT Effects of Neighborhood (odds ratios) on Young Adult Outcomes Including Youth Outcomes

	Attend School#		Employed FT or PT#		Out of Wedlock Child	
	ITT	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
Neighborhood Characteristics^^						
Social Vulnerability	0.999	1.024	0.996	0.994	0.991**	0.975***
Occupational Prestige	0.927	1.097	1.108	1.427**	0.917	0.756**
Latino Residents (%)	0.961	0.917**	0.990	0.972	1.001	0.982
Foreign-Born Residents (%)	1.025	1.191**	0.966	1.037	1.010	1.020
Violent Crime Rate	0.996	0.995	0.998	1.022	1.012	1.056
Property Crime Rate	1.005	0.997	1.002	1.010	1.004	1.012
Individual Characteristics						
Last School GPA	2.256**	3.443***	1.756**	2.093***	1.027	1.000
Dropped Out of HS	0.001***	0.001***	0.462*	0.523	3.907***	3.321***
Witnessed Violence	2.739*	2.136	2.766**	2.244*	0.798	0.887
	Attend School#		Employed FT or PT#		Out of Wedlock Child	
Neighborhood Characteristics w/Interaction with Age into DHA^^	ITT	TOT	ITT	TOT	ITT	TOT
Social Vulnerability	1.003	0.950	0.995	0.938*	1.000	0.984
*Age into DHA	0.999	1.009	1.000	1.006	0.999	0.999
Occupational Prestige	1.128	1.492	1.189	1.547**	0.854**	0.670***
*Age into DHA	0.978***	0.972**	0.991**	0.992	1.003	1.006
Latino Residents (%)	0.958	0.873**	0.995	0.964	0.979	0.991
*Age into DHA	1.000	1.004	0.999	0.999	1.002	0.999
Foreign-Born Residents (%)	0.758***	1.040	0.760***	0.916	1.125**	1.094
*Age into DHA	1.037***	1.019	1.025***	1.014	0.989***	0.993
Violent Crime Rate	1.107	1.143	1.125*	1.322	1.022	1.026

	Attend School[#]		Employed FT or PT[#]		Out of Wedlock Child	
	ITT	**TOT**	**ITT**	**TOT**	**ITT**	**TOT**
* Age into DHA	0.988	0.985	0.986*	0.972	0.999	1.003
Property Crime Rate	0.978	1.081	0.984	1.069	0.991	0.995
* Age into DHA	1.004	0.992	1.002	0.995	1.001	1.002
Individual Characteristics						
Last School GPA	2.169**	3.285***	1.545*	1.878**	1.020	1.014
Dropped Out of HS	0.001***	0.001***	0.369**	0.444*	4.027***	3.335***
Witnessed Violence	3.437**	2.529	3.298***	2.355*	0.857	1.017

*p <0.10;

**p <0.05;

***p <0.01; based on robust std. errors adjusted for clustering in households

#primary activity post-age 18; neither working nor attending school = reference

^^ ITT uses first-offered neighborhood; TOT uses IV for neighborhood experienced; see Table A1 All models include covariates as in Table 1