Parental Involvement and Neighborhood Quality: Evidence from Public Housing Closures in Chicago

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Abstract

This paper studies whether, and how, parents respond to changing neighborhood quality through parental involvement. Empirical measurement of this response is complicated by neighborhood selection: through their residential location choices, parents have some control over neighborhood quality. To deal with neighborhood selection, I make use of the mass closures of public housing projects in Chicago. Households displaced by these closures relocated into surrounding neighborhoods, which is associated with decreased neighborhood quality in those receiving neighborhoods. These incoming relocations are used to provide variation in neighborhood quality, which is plausibly exogenous to the residential choices of receiving families. The empirical strategy used accounts the potential anticipation of incoming relocations by receiving parents, as well as the potential selection of destination neighborhoods by displaced households. Using neighborhood crime rate as a measure of neighborhood quality, I find that parents tend to increase parental involvement when neighborhood quality falls. In contrast, the effects on child behavior are much smaller in magnitude. Together, these two findings suggest that parents compensate for declining neighborhood quality by increasing parental involvement. Additionally, I find interesting heterogeneity in neighborhood quality effects across families. Most notably, parents with low initial involvement make further reinforcing decreases to parental involvement when neighborhood quality declines. Relating this result to a model of parental involvement decisions, I make inferences about the relationship between parents and neighborhoods as productive inputs for child development. Overall, these results suggest that existing estimates of neighborhood effects may underestimate the direct impact of neighborhoods on children, and that different parental responses may explain how neighborhood change has different impacts on children. Finally, these results provide evidence on the spillover effects of public housing closures on parents living in receiving neighborhoods, and characterize neighborhood effects on parental involvement for families who have not selected into low-income public housing.

Keywords: Parental involvement, neighborhood effects, public housing

JEL Classification: I38, J13, R23

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1 Introduction

Parenting behavior is likely to be a key contributing factor towards the healthy development of positive later-life outcomes in children (Amato and Rivera, 1999; Avvisati et al., 2014; Fryer Jr et al., 2015). For this and other reasons, within the field of demographic economics, there is substantial research interest in studying parenting behavior. Parenting behavior varies substantially from parent to parent (Guryan et al., 2008). Additionally, for a given parent, parenting behavior is responsive to changing information and parental incentives (Gelber and Isen, 2013; Avvisati et al., 2014; Fryer Jr et al., 2015; Bergman, 2015). Such variation in parenting behavior has been linked to differences in child behavior and outcomes (Cunha et al., 2010; Pavan, 2013). Depending on the specific context, these changes may either reinforce or offset existing disparities between children.

Little is known about how parenting behavior responds to changes in the quality of the residential neighborhood. A large part of the research on neighborhood effects has focused on effects on children, and relatively little is known about the corresponding effects on parents’ behavior. Studying the Moving To Opportunity program, Katz et al. (2005) examine the effects of voucher-induced relocation out of high-poverty urban neighborhood. They find a decrease in monitoring of child safety, which is an aspect of parental care. Using the same experiment, Kling et al. (2007) find a decrease in the stress level of adults, which suggests that residential neighborhoods affect parental incentives substantially. On the other hand, studies a similar group of families - those displaced by low-income public housing demolitions - and finds no impact on adult labor force participation. Despite this finding, it is still possible that parental care changes in response to neighborhood quality, if the substitution occurs with leisure time instead of work.

In this paper, I consider how parental involvement responds to changes in neighborhood quality. Parental involvement refers to the time and effort a parent expends on the care, discipline and supervision of the child. This definition encompasses a wider range of parental behaviors than has been previously investigated. This paper attempts to answer the following questions: Do parents respond significantly to changing neighborhood quality through parental involvement? If so, what direction does this response take? A priori, it is unclear whether the changes will reinforce or offset the impact on children. The answer to this question depends on whether parents and neighborhoods are complements and substitutes, but also on how neighborhood quality affects parental incentives directly. Finally, do the answers to the above depend on parental characteristics,

1Pop-Eleches and Urquiola (2013) find evidence that parenting behavior response to changes in a different aspect of the external environment: school quality.
especially those associated with relative advantage/disadvantage? If different parents respond differently to the same change in neighborhood quality, this might imply an unequal impact of neighborhood change on children living in the same neighborhood. I first set up a model of parental involvement choice that shows explicitly the various channels by which neighborhood quality leads to response in parental involvement.

When attempting to measure the causal effect of neighborhood quality on parental involvement, the main difficulty lies in the endogenous choice of neighborhoods by parents. A parent who chooses good neighborhoods for her child is likely to be one who exercises a high level of involvement. One straightforward way to address this problem is to use fixed-effects analysis with panel data, identifying the causal effect from changes in parental involvement in response to changes in neighborhood quality. However, this approach fails to account for parents endogenously choosing to move out of or remain in a given neighborhood, in response to changing neighborhood quality. For example, a parent who loses a job may become depressed, leading to lower parental involvement. Simultaneously, the inability to pay rent may result in the family moving to a lower quality neighborhood. Such factors will generate a spurious correlation between parental involvement and neighborhood quality.

To address this problem, I utilize a series of shocks to neighborhood composition, caused by the mandated closure and demolition of high-rise public housing projects in Chicago. Residents from closed housing projects were dispersed among various neighborhoods in Chicago, changing the composition of those destination neighborhoods. I study the effect of these neighborhood changes on a particular group of families: those whose neighborhoods received an influx of relocations coming from closed projects. Henceforth, I address these families as receiving families, and their neighborhoods as receiving neighborhoods. These receiving families were by no means the only group affected by the public housing closures: families living close to a housing project that was closed down also experienced a change in neighborhood quality, and so did the families displaced by housing closures. The decision to restrict the analysis to receiving neighborhoods was made for two reasons: first, the effect of relocations on receiving neighborhoods is simpler to understand: the only effect was to change neighborhood composition. In contrast, closure neighborhoods were affected by the eviction and demolition process, the change in neighborhood composition, as well as the subsequent redevelopment of the public-housing site. These changes could have affected neighborhood quality in different, and perhaps conflicting, ways.

A second reason is that the data used in this study contains no displaced families, and only a small number

\footnote{Figure A1 provides a graphical description of the affected families.}
of families in closure neighborhoods. This data comes from the Project for Human Development in Chicago Neighborhoods (PHDCN). It contains longitudinal data on parental involvement and residential neighborhoods, for families living outside of project-based public housing. Using restricted neighborhood identifiers, I link the PHDCN with neighborhood measures of crime rate and relocations received.

To deal with the endogeneity problem, relocations received by a neighborhood are used to provide variation in neighborhood quality, which is plausibly exogenous to the residential choices of receiving families. This is achieved using two separate research designs. In the first, I use the response of parental involvement to relocations received by the family’s current neighborhood. The causal effect is identified under the assumption parents choose neighborhood location for the current year without fully knowing the level of relocations received in the previous year. While parents may have some expectations over the number of relocations received, I assume that the included neighborhood/time controls are sufficient statistics for that parent’s expectations. In that case, the net variation in relocations last year identify the causal effect. In the second research design, I instead use the parent’s response to relocations received by the family’s initial neighborhood. This research design exploits the fact that the initial neighborhood cannot be changed by the parent’s subsequent residential location choices. To assess the plausibility of both strategies, I examine the neighborhood choice behavior of these receiving parents, and I verify that the assumptions are not rejected by the data. I also address a secondary endogeneity concern: that displaced families may have chosen neighborhoods based on parental involvement trends. This concern is addressed through the use of a shift-share instrument, which is arguably exogenous to the decision of these displaced families.

I find that the overall intensity of parental involvement increases when neighborhood quality falls, which suggests that parents are making compensatory changes to parental involvement. Compared to a neighborhood that received zero relocations, parents in a neighborhood receiving the mean yearly level of relocations increased parental involvement by approximately 0.1 of a standard deviation.\(^3\) Using crime rate as a proxy for neighborhood quality, I find that a one standard deviation increase in the crime rate (decrease in neighborhood quality) is associated with a quarter standard deviation increase in parental involvement. These effects are substantial: the latter effect is around half of the gap in parental involvement, between college educated parents and less educated ones. In contrast to the significant findings for parental involvement, the evidence for effects on child behavior is weaker and less consistent. Putting these two findings together, it appears that, on average, parents are compensating for the negative shocks to neighborhood quality on

\(^3\)The measure of parental involvement has been standardized by age group of the child, so the increase in parental involvement is measured relative to other parents with children of the same age.
their children, by increasing parental involvement.

I explore heterogeneity in the response of parental involvement. Most notably, I find that the response of parental involvement to incoming relocations depends strongly on the initial level of parental involvement. Parents with high initial involvement subsequently raise parental involvement in response to relocations received. In contrast, for parents with the lowest initial involvement, the direction of the response is reversed: these parents make reinforcing decreases to parental involvement instead. I relate these findings to a model of parental involvement, where parents differ in their altruism levels. I find that the empirical findings are best explained by parental and neighborhood inputs being productive substitutes, which in turn suggests that, when policies targeting child environment and parental engagement are jointly implemented, the effectiveness of each individual policy will be somewhat reduced.

These findings provide more evidence that parents are important mediators of external influences on their children. The magnitude of the response suggests that parental involvement is a major channel through which residential neighborhoods affect families. Additionally, these findings also relate to the study of neighborhood effects on child development and later-life outcomes. On this question, the evidence is mixed: using randomly-assigned housing vouchers, Kling et al. (2007) find substantial effects of moving to a better neighborhood on educational and other outcomes, for girls but not for boys. At the same time, ? find more consistent positive impacts on adult incomes of exposed children, as ?. Furthermore, the duration and age of exposure to neighborhood influences also seems to play a major role (Wodtke et al., 2011). However, in all of these studies (experimental ones included), parental behavior is not held constant. Assuming that parental involvement is productive in developing good outcomes in the child, then my results suggest that the direct impact of neighborhoods quality on child outcomes - holding parental involvement constant - could be more positive than previously thought.

Additionally, as estimates of the effect of relocations, these findings also reveal the impact of the public housing closures on an unexpected group of individuals (parents in receiving neighborhoods). Other studies have documented the effects of public housing closures on displaced families (Jacob, 2004; ?), and more generally, the effects of housing interventions on disadvantaged families (Andersson et al., 2016; Jacob et al., 2015). Moreover, the spillover effects of public housing closures have been documented by Aliprantis and Hartley (2015); Sandler (2012), who find that relocations affect crime rate in receiving neighborhoods. This study extends that work by showing that these neighborhood-level changes had significant impacts on

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4 Cunha et al. (2010); Pavan (2013)
behavior within the family. The findings suggest that, even if children in receiving neighborhoods were not severely affected by incoming relocations, this may have come at a significant cost to their parents in terms of effort and stress. If these parents are seen as a vulnerable population, especially those living in already disadvantaged neighborhoods, then it may be important to devise policy responses to mitigate the impact of relocations on them.

This study is closely related to Patacchini and Zenou (2011) (henceforth referred to as PZ), who investigate whether neighborhood and parental inputs behave like complements or substitutes.\(^5\) PZ do not observe longitudinal changes in neighborhood quality or parental involvement, instead, their identifying variation comes from assignment of council housing in the United Kingdom. Their overall findings are very different from those in this study: where I find that parental involvement decreases when neighborhood quality improves, they find the opposite effect. Aside from the differences in sample, the opposite findings could also be generated by the timing of measurement: in PZ, parental involvement and neighborhood quality are measured 6 years apart, while this study uses concurrent measures of neighborhood quality and parental involvement. Finally, PZ study a sample that has selected into public housing. To my knowledge, this study is the first to estimate neighborhood quality effects on parenting behavior, using a non-public housing sample.

The rest of the paper proceeds as follows. Section 2 provides some theoretical foundations for the response of parental involvement to neighborhood quality. Section 3 describes the closures and relocations in more detail. Section 4 lays out the empirical strategy utilizing these relocations. Next, Section 5 describes the data used, paying special attention to the construction of the parental involvement variable. The results are reported in Section 6. Lastly, Section 7 concludes.

2 Expected Response of Parental Involvement to Neighborhood Quality

Several channels have been proposed for the effect of neighborhood quality of parental involvement. In this section, I summarize these channels using a simple model of parental utility. Abstracting from family structure, consider one-parent, one-child families living in a neighborhood with neighborhood quality \(q\). This measure of neighborhood quality captures influential behavior of children and adults within the neighbor-

\(^5\)PZ also consider the contribution of both these factors to educational attainment of the child, while this paper is limited to concurrent measurements of behavior.
hood, as well as the quality of amenities and institutions in the neighborhood. When considering influences of child behavior and development, one particularly relevant institution is the school (Sacerdote et al., 2011): in this paper, the school environment in nearby schools is considered as one aspect of neighborhood quality. Taking the level of neighborhood quality as given, parents choose the optimal level of parental involvement $y$. A high level of parental involvement contributes towards the development of good child behavior $b$, but exercising parental involvement requires costly effort $c$. Neighborhood quality affects parental decisions through child behavior, as well as parental effort cost.

\[ U_i(b, y, q) = u(b(y, q)) - c(y, q) \quad (1) \]

The object of interest in this model is the response of optimal parental involvement to changes in neighborhood quality, which is given by $\partial y^* / \partial q$. Assuming an interior solution to the problem, this derivative can be expressed as (2). Under the additional assumption that the solution is stable, the denominator of (2) is negative. Hence, the direction in which parental involvement changes is determined by the sum of three terms in the numerator. These terms correspond channels proposed in the existing research literature.

\[ \frac{\partial y^*}{\partial q} = \frac{-u''b_yb_q + u'b_yq - c_yq}{u''b_yb_y + u'b_yq - c_{yy}} \quad (2) \]

The first term, $u''b_yb_q$, captures the effect of neighborhood quality on the marginal utility that parents derive from child behavior. The influence of neighborhood quality on child behavior has been extensively modeled: most models suggest a positive relationship between neighborhood quality and child behavior. This positive relationship can be transmitted through differences in institutional quality, changing adult influences, or changing peer influences from other children (Jencks and Mayer, 1990). Empirically, estimates of peer effects support the hypothesis of a positive relationship, for a range of risky child/youth behaviors. In turn, different levels of child behavior are likely to incentivize parental involvement to different extents. For example, the presence of parental expectations and aspirations (Fan and Chen, 2001) for child educational attainment suggests that that marginal utility of improved child behavior is decreasing: as it becomes more likely that the child achieves the target level of education, the marginal utility from improving child behavior goes down.

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6These include smoking (Nakajima, 2007), alcohol, other drug use, dropping out of school (Gaviria and Raphael, 2001), absenteeism, disciplinary problems (Imberman et al., 2012), and crime Case and Katz (1991).
The second term, $u'b_yq$, captures the effect of neighborhood quality on the productivity of parental involvement. The sign of this term depends on whether parental and neighborhood inputs are substitutes or complements in the production of child behavior. A priori, it is unclear which way neighborhood quality and parental involvement are related. Empirical evidence on this matter is also limited. By comparing families across schools with different resources, Hoffmann and Dufur (2008) find evidence that school resources serve as substitutes for parental involvement. However, their estimates are associative in the sense that selection into schools is not accounted for.

The final term, $c_yq$, captures the effect of neighborhood quality on parental effort cost. This term captures the effect of neighborhood quality on adult well-being, which in turn affects the parent’s ability to exercise parental involvement. For example, the Moving to Opportunity experiment found a reduction in adult stress level from an increase in neighborhood quality (Leventhal and Brooks-Gunn, 2003; Kling et al., 2007). A reduced stress level might, in turn, leave parents better able to care for their children. Parents may also have been affected through their expectations: Green et al. (2007) suggest that parental understanding of their role in child education, parental beliefs about the effectiveness of involvement, and parental understanding of external expectations are influential in driving different parental involvement decisions. Any changes in these would alter the marginal cost of parental involvement.\footnote{Avvisati et al. (2011) present a thorough review of the literature regarding the effects and determinants of parental involvement, in the context of school quality.}

3 Public Housing Closures and Relocations in Chicago

The closure of low-income public housing, and in particular the resulting relocations by displaced households, provide the key source of variation in neighborhood quality. In this section, I summarize the historical background of these events. In addition, I describe the measures of closures and relocation that are used in this study. Finally, I also present some findings showing that the arrival of relocations in a particular neighborhood was associated with a decrease in neighborhood quality, as measured by the neighborhood crime rate.

3.1 Historical Background

In the 1990s, public housing in Chicago was notorious for a host of social ills, and the Chicago Housing Authority (CHA) was described as the “most troubled in the nation” (Terry, 1995). “Unemployment at CHA developments ran as high as 90 percent... residents were at least twice as likely to be the victims of...
serious crime as other Chicagoans,” (Chicago Tribune, 2002), and an estimated 95 percent of the resident population were on some other kind of public assistance (The Economist, 1998). The first closures were in response to building-specific issues, rather than as part of a co-ordinated plan (Jacob, 2004). For example, four blocks of Cabrini-Green were closed and subsequently demolished due to a high-profile crime incident: a child was killed by a stray bullet fired from one of the blocks (Austen, 2012). Another reason for closing was poor/deteriorating building conditions, e.g. a building-wide loss of heat in the winter, linked to shoddy construction (Garza, 1999). The scale of the problem prompted a federal takeover by the Department of Housing and Urban Development in 1995 (Terry, 1995). Shortly after, federal housing policy changes required viability testing of public housing blocks below a 90 percent occupancy, and around 18000 of Chicago’s units failed this test (The Economist, 1998). At this point, plans were made for the widespread demolition of low-income housing in Chicago. This effort was aided by new federal legislation (HOPE VI), which funded the conversion of low-income housing to mixed income developments (Popkin et al., 2000). Ultimately, the CHA proposed the Plan for Transformation in order to carry out these objectives (CHA, 2011), and was returned from federal control to carry out this plan. Since the beginning of the 1990s, at least 20000 units have been demolished by the authorities, which constitutes over half of the 1995 stock of public housing. (Bennett et al., 2015).

While the troubled history of Chicago public housing dates back to the 1970s (Popkin et al., 2000), the exact timing and volume of the studied closures was difficult to predict. One reason for this is the regulatory situation and the circumstances of the closings. The majority of closures during the study period (1994-2002) occurred before the year 2000: at this point, the closures were largely based on emergent issues and not an overall redevelopment plan. In addition, the federal takeover of CHA came largely as a surprise to the public (Terry, 1995), and the transition added to the uncertainty of the situation. While the last group of closures in the study period occur after the launch of the Plan for Transformation, they nonetheless occurred at the very start of its implementation.

A second difficulty in anticipating closures and relocations comes from the numerous challenges to planned closings from public housing residents. It was common for residents of public housing to unite in opposition to redevelopment plans, sometimes across housing projects: in the case of Cabrini-Green, a Local Advisory Council challenged parts of the proposed demolition plan, which resulted in a lawsuit (Bennett et al., 2015). The uncertainty generated by these challenges is sizeable: the Lathrop Homes were part of the Plan for Transformation, and again ordered demolished in 2006, but as of 2012, they were still standing, although
almost completely vacant (Moser, 2012). Even though plans for demolition were eventually made public, these legal and administrative challenges resulted in significant delays (Hunt, 2009), meaning that the timing of events did not coincide with statements by the housing authorities.

Residents in affected public housing blocks were relocated in two ways. First, they were relocated to private housing, and were offered Section 8 housing assistance. Alternatively, they were relocated to other public housing projects (Bennett et al., 2015). As a consequence, the destination neighborhoods of displaced households should be thought of as an intentional choice, made either by the displaced household or the public housing authority. At the same time, these choices are likely to have been constrained. For displaced households, constraints include the spatial concentration of voucher housing (Oakley and Burchfield, 2009), discrimination from potential landlords, rushed relocations and insufficient information about destination neighborhoods (Thompson, 2006). Quantitative evidence in support of this is provided by Desmond and Shollenberger (2015), who find that involuntarily displaced renters in Milwaukee tend to settle in poorer-quality neighborhoods than those choosing to leave. For the public housing authority, the major limiting factor in resettlement is likely to have been the long wait-lists for housing at other low-income public housing projects.

### 3.2 Data on Closures and Relocations

In this study, the definition of a neighborhood is chosen to match the neighborhood identifiers in the data on parental involvement.\(^8\) This neighborhood unit is known as a **Neighborhood Cluster** (NC): a group of adjacent, relatively homogeneous census tracts. The average NC was about 2.5 census tracts in size, and contained around 8000 people. Henceforth, the terms **neighborhood** and **Neighborhood Cluster** (NC) will be used interchangeably.

Data on CHA closures is available at the address level,\(^9\) and contains the yearly number of housing units (apartments) closed. This is aggregated to the NC level by first assigning addresses to census tracts, and then aggregating census tracts into neighborhood clusters. For each neighborhood cluster, I observe the yearly number of units closed. Hence, there is variation in the timing and volume of closures. The left panel of Figure 2 shows this variation. Slightly over 10000 units were closed during the sample period, and the volume of closures fluctuates considerably. Table 4 shows that, as expected, these closures occurred in neighborhoods which were poorer, contained more minorities, and exhibited a higher unemployment rate.

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\(^8\)This data, the PHDCN, is described in Section 5.

\(^9\)This data was gathered partly by Brian Jacobs, and partly by Daniel Hartley.
compared to the average neighborhood, and even compared to the average neighborhood containing public housing.

Data on relocations comes from a separate survey of ex-public housing residents - it contains the residential location of these households in 2008. There are two limitations to this data. First, the measure of relocations does not vary across time. Secondly, the geographical unit of measurement is a community area, which is a larger area than the neighborhood cluster. While this level of geographical aggregation is large, the community areas correspond to well-known, distinct neighborhoods in Chicago. Hence, these geographical designations still match socially accepted notions of neighborhood boundaries.

I do not observe actual yearly flows of relocations into a receiving neighborhood. Instead, I construct a shift-share measure of yearly relocations to a neighborhood as follows: First, from the survey of ex-public housing residents, I calculate the share of total relocations going to each neighborhood. Since the survey uses a larger geographical scale (community area), I assign to each neighborhood within the community area its population-weighted share of relocated households. This assumes that, within a particular community area, the distribution of relocations into small neighborhoods was uniform. The relocation shares are described in the right panel of Figure 2. The distribution contains many zeros: neighborhoods receiving zero relocations tended to be relatively rich. At the same time, the distribution has a long right tail: conditional on receiving a non-zero share of relocations, the average level of relocations was around 3 percent of year 1994 neighborhood population. For comparison, across a 10 year time span (1990-2000) the mean percentage change in neighborhood population, in absolute terms, was roughly 12 percent.

Next, I define the measure of relocations $r$ to neighborhood $j$ in year $t$ as the product of yearly closures in all other neighborhoods $c_{-jt}$, and the neighborhood’s share of relocations $s_j$ (3).

$$ r_{jt} = s_j c_{-jt} $$

This measure is used in all subsequent descriptive and regression analysis. It is an inaccurate measure of the actual flow of relocations from closed public housing into neighborhood $j$, which is not observed. However, as constructed, the measure addresses some of the concerns about the endogenous selection of displaced households into receiving neighborhoods. Hence, relative to more accurate measures of relocations, the constructed measure used here is preferred for the purposes of estimating neighborhood quality effects on parental involvement. What these concerns are, and exactly how the measure addresses them, are discussed.

\(^{10}\)There are 80 community areas covering Chicago, compared with 343 NCs.
later, in Section 4. The remainder of this section discusses how relocations affected receiving neighborhood quality.

3.3 Effect of Relocations on Neighborhood Quality

The best available measure of neighborhood quality is given by the neighborhood crime rate, because it is measured at a high (yearly) frequency and at a high level of geographical detail. As a result, existing research has considered the effect of relocations on this measure of neighborhood quality. Using precise block-level information on destination neighborhoods of displaced households, Aliprantis and Hartley (2015) find significant increases on crime rates for most major types of crime. The average magnitude of such effects varies from 3 to 30 percent of base year rates, depending on the specific type of crime. Similarly, using tract level relocations data, Popkin et al. (2012) find increases ranging from 0.7 to 0.9 percent in violent/property/gun crime, associated with 1 relocated household received per 1000 existing residents.

Using more detailed measures of relocations received, the previous studies mentioned above find a positive association between the arrival of relocations in a neighborhood and its subsequent crime rate. Since the measure of relocations used in this paper is relatively less precise, I verify that the effects on neighborhood crime rate are qualitatively similar. The analysis uses the event study framework (4). I regress neighborhood crime rate $v_{jt}$ on lead, current, and lagged values of relocations. Neighborhood fixed effects $F_j$ and year effects $G_t$ are included as controls. If relocations to the neighborhood increase crime rate, the coefficient on lagged relocations should be positive. On the other hand, if displaced households were simply relocating to households where the crime rate were increasing (decreasing), then the coefficients on leads of relocations should be positive (negative).

$$ v_{jt} = \sum_{k=-3}^{3} \rho_k r_{jt-k} + F_j + G_t + \varepsilon_{jt} \quad (4) $$

Figure 4 presents the coefficients $\rho_k$ from (4). The coefficients show that there is little evidence for an increase in crime rate prior to incoming relocations, which allays concerns that increasing crime rate in a neighborhood may be driving higher relocations, rather than the other way round. Relocations are also associated with an increase in the crime rate 1 to 2 years after the event. This finding is consistent with the notion that incoming relocations decrease neighborhood quality, and furthermore, the effect appears to fade out after 2 years. Relative to Aliprantis and Hartley (2015), the magnitude of the coefficient is lower: converting the coefficient into units they use, I find that one relocation is associated with an increase of
roughly 2.5% in the neighborhood crime rate. Since the geographical definition of a neighborhood in this study is larger, the lower magnitude is not surprising.

Based on these results, it is likely that incoming relocations reduced neighborhood quality in receiving neighborhoods. Aside from crime rate, it is highly likely that other dimensions of neighborhood quality were also affected. These other effects are difficult to document: other aspects of neighborhood quality are not as well-measured as crime rate. Nevertheless, it is unlikely that any of these dimensions were significantly improved by incoming relocations, and hence it overall neighborhood quality is likely to have decreased. In the next section, I discuss how the decrease in neighborhood quality due to relocations can be used to identify the effect of neighborhood quality on parental involvement.

4 Empirical Strategy

To arrive at an estimating equation for the effect of neighborhood quality on parental involvement, I specialize the model from Section 2. I proceed by assuming particular functional forms for child behavior, parental effort cost, and utility from child behavior. Suppose that, in (1), child behavior is given by (5). As before, b denotes child behavior and y denotes parental involvement, and both are now indexed by family i and year t. Neighborhood quality q is instead indexed by neighborhood ji(t) and year t. Neighborhood quality q is instead indexed by neighborhood ji(t) and year t. Note that ji(t) is written to denote that neighborhood location is a choice made by families, for a given year. The parameter αp governs the relationship between neighborhood quality and parental involvement as inputs in the production of good child outcomes: it is positive (negative) if neighborhoods and parents are complements (substitutes). Meanwhile, Fpi denotes unobserved, fixed characteristics influencing the marginal productivity of parental involvement, for family i, and εpi[t] represents an idiosyncratic shock to productivity.

\[ b_{it} = (\alpha_p q_{ji(t),t} + F_{pi} + \epsilon_{pi[t]} ) y_{it} \] (5)

Now, parental effort costs take the quadratic structure given in (6). The notation in this equation is similar to that for child behavior. αc now denotes the effect of neighborhood quality on the effort cost of parents, and analogous statements hold for Fci and εci[t].

\[ c_{it} = (\alpha_c q_{ji(t),t} + F_{ci} + \epsilon_{ci[t]} ) y_{it} - y_{it}^2 \] (6)
Finally, I assume that the utility of child behavior is linear: \( u(b) = \omega_i b \). The parameter \( \omega_i \) increases the marginal benefit of improving child behavior, relative to the cost. Hence, it captures the effect of parental altruism, for example. Differences in \( \omega_i \) will become important later, when studying the heterogeneity of neighborhood quality effects. For now, I assume that \( \omega_i = \omega \), which is common across all families.

With the chosen production, cost, and utility functions, the parental decision problem is given by (7). The resulting decision rule for parental involvement is linear (8). In (8), the parameter of interest is \( \alpha = \omega \alpha_p - \alpha^c \).

This parameter describes the behavioral response of parental involvement to changes in neighborhood quality, which is driven both by changing productivity as well as changing parental effort cost. Similarly, \( F_i \) and \( \varepsilon_{it} \) can also be interpreted as the aggregate of productivity and effort cost effects. Without resorting to an underlying utility model, (8) may be viewed as an approximation to the true decision rule. However, the derivation of (8) clarifies the interpretation of the coefficient \( \alpha \).

\[
U_{ijt} = \left( \omega \alpha_p - \alpha^c \right) q_{j(i,t),t} + \left( \omega F_p^i - F^c_i \right) + \left( \omega \varepsilon_{it}^p - \varepsilon_i^c \right) \right) y_{it} - y_{it}^2 \tag{7}
\]
\[
y_{it} = \alpha q_{j(i,t),t} + F_i + \varepsilon_{it} \tag{8}
\]

The empirical objective is simply to estimate (8) and recover the parameter of interest \( \alpha \). I now describe the difficulty posed by the neighborhood choice of parents.

### 4.1 Description of Endogeneity Problem

Naively, in the presence of panel data, (8) can be estimated using a fixed-effects regression, so that the changes parental involvement in response to changes in neighborhood quality identify \( \alpha \). This procedure will give unbiased estimates of \( \alpha \) as long as \( \varepsilon_{it} \) is uncorrelated with \( q_{j(i,t),t} \). In itself, highly-involved parents sorting into different neighborhoods than less-involved parents is not a problem. Suppose that parents optimize parental involvement according to (8) in two periods, \( t \) and \( t' > t \). Additionally, suppose that between \( t \) and \( t' \), parents move randomly between neighborhoods, or, alternatively, that no family moves. In that case, the fixed effect \( F_i \) will address sorting into initial neighborhoods, and (8) can be estimated in a straightforward manner.

The neighborhood selection problem arises when \( \varepsilon_{it} \) contains factors influencing the parent’s residential
choice of neighborhoods with different quality. Let \( j' = j(i, t') \) denote the parents choice of neighborhood in period \( t' \). The problem mentioned above suggests that \( q_{jt'} \) is likely to be correlated with \( \varepsilon_{it'} \). For example, consider a hidden factor - parental health - which changes over time. A parent who becomes ill will likely be less motivated to exercise parental involvement in the future. Simultaneously, due to medical expenses incurred, she may also be unable to pay rent in the current neighborhood, and hence be forced to move to a lower-quality neighborhood. If parents observe the shock \( \varepsilon_{it'} \) before \( j' \) is chosen, the value of this shock may to influence their preferences for neighborhood quality. If the parental involvement shock \( \varepsilon_{it'} \) affects the expected utility of living in a good neighborhood as opposed to a bad one, the neighborhood quality of \( j' \) will be correlated with this shock, through the parent’s choice of neighborhood.

Many models of neighborhood selection will generate the kind of correlation mentioned above. I consider one of these to formalize the intuition. Suppose that, in period \( t \), parents choose \( j' \) according to (9). In selecting neighborhood \( j' \), parents value neighborhood influence on the child through \( U_{ijt} \), and also incur some other benefits/costs from neighborhood residence, which are captured in the function \( W(\cdot) \). This function contains the parental involvement shock \( \varepsilon_{it} \), to reflect the idea that common factors shift parental involvement preferences and the parent’s willingness or ability to reside in certain neighborhoods. Because there may be uncertainty about the \( t' \) values of neighborhood quality or the parental involvement shock, parents maximize expected utility, conditional on their information set \( \mathcal{I}_{it} \).

\[
\begin{align*}
  j' &\equiv j(i, t') = \operatorname{argmax}_{j \in \mathcal{J}} E[U_{ijt'} | \mathcal{I}_{it}] + E[W(F_i, q_{jt'}, \varepsilon_{it'}) | \mathcal{I}_{it}] + \nu_{ijt} \quad (9)
\end{align*}
\]

Suppose that, when choosing neighborhoods, parents know \( \varepsilon_{it'} \) and can perfectly forecast \( q_{jt'} \) at the point of choosing neighborhood \( j' \). Because of the quadratic form chosen, \( U_{ijt} \) is constant in neighborhood quality, so this term drops out of the decision problem.\(^{11}\) As a result, the conditional expectation of neighborhood quality in the chosen neighborhood is given by (10). If \( W_{qe} \) is nonzero, for two arbitrary values of neighborhood quality \( q \) and \( \overline{q} \), the difference \( W(F_i, q, \varepsilon_{it'}) - W(F_i, \overline{q}, \varepsilon_{it'}) \) will depend on the value of \( \varepsilon_{it'} \). As a result, the expected value of \( q_{jt'} \) will be dependent on the value of \( \varepsilon_{it'} \). For arbitrary distributions of shocks \( (\varepsilon, \nu) \), it is likely that \( q_{jt'} \) and \( \varepsilon_{it'} \) will also be correlated.

\[
E \left[ q_{jt'} | j' \text{ chosen} \right] = E \left[ q_{jt'} | W(F_i, q_{jt'}, \varepsilon_{it'}) - W(F_i, q_{kt'}, \varepsilon_{it'}) > \nu_{ij't'} - \nu_{ikt}, \forall k \in \mathcal{J} \right] \quad (10)
\]

\(^{11}\)If \( U_{ijt} \) was chosen to be a different function, then this term would also have to be accounted for but would not change the main argument.
To explicitly show the potential correlation between neighborhood quality and the parental involvement shock, consider the case where \( W(F, q, \varepsilon) = (F + \varepsilon)q \). Then, because \( \varepsilon_{it'} \) is assumed to be mean-zero, the covariance between \( q_{it'} \) and \( \varepsilon_{it'} \) is given by \((11)\). In general, the covariance is non-zero. Furthermore, if the shocks \( \varepsilon \) and \( \nu \) are mutually independent, then the covariance is positive. This result is intuitive: by construction, a high parental involvement shock also increases the marginal benefit (relative to cost) that parents derive from living in a high-quality neighborhood. Hence, parental residential choices should reflect that preference.

\[
\text{Cov} \left[ q_{it'}, \varepsilon_{it'} \mid j' \text{ chosen} \right] = E \left[ q_{it'} \varepsilon_{it'} \mid q_{it'} \varepsilon_{it'} > q_{kt'} \varepsilon_{it'} + \nu_{ikt} - \nu_{ij't}, \forall k \in J \right] \tag{11}
\]

According \((10)\) or \((11)\), estimating \((8)\) using a fixed effects regression is likely to result in biased estimates of \( \alpha \). I mention three important details about the nature of this bias. First, families who move and families who stay in the same neighborhood are both selected samples. Revisiting the example of a parent who contracts a serious illness, instead of being forced to move to a worse neighborhood, that parent might instead be forced to stay in a declining neighborhood. In the counterfactual, had that parent been healthy, she might have searched for and found a house in a better neighborhood. Hence, the effect of neighborhood quality on parental involvement cannot be consistently estimated on either sub-sample. The second detail concerns the interpretation of the parental involvement shock, \( \varepsilon_{it'} \). Although parental health has been used as an example of a confounding factor, it can also be a channel through which neighborhood quality effects operate. For example, if an increase in secondhand smoke in the neighborhood affects the parents health, this corresponds to a direct effect on parental incentives, as described in Section 2. Hence, if declining neighborhood quality causes poorer parental health, this is not an endogeneity concern but merely part of the neighborhood quality effect \( \alpha \). The endogeneity only comes from idiosyncratic health shocks, not attributable to neighborhood quality, which affect whether/where a parent moves in the next period. In other words, it is incorrect to equate \( \varepsilon_{it'} \) with parental health: the former only contains a particular component of the latter. Finally, the same bias will arise even if parents do not perfectly forecast neighborhood quality or the parental involvement shock in \( t' \), as long as parents have some informed expectations about these two variables.

The rest of this section proceeds in the following manner: I first present two research designs to address this endogeneity problem. I explain the assumptions under which each research design identifies the effect of relocations on neighborhoods, in a manner that excludes the channel of neighborhood selection. In discussing each of these strategies, I assume that relocations themselves are not responsive to changes in neighborhood
quality. Following that discussion, I then account for nature of relocations as choices made by displaced households or the public housing authority, which may in fact be responsive to those changes. Finally, I address the difficulty of measuring neighborhood quality, which is likely to be multidimensional and contain many unobserved factors. I discuss the implications of using crime rate as the measure of neighborhood quality, which is the approach taken here.

4.2 Research Design 1: Relocations to Current Neighborhood

Recall that, in (8), the correlation between \( q_{j't'} \) and \( \varepsilon_{it'} \) arises only because \( \varepsilon_{it'} \) affects the choice of neighborhood \( j' \). Hence, if actual neighborhood quality experienced by the family at \( t' \) is different from anticipated neighborhood quality when \( j' \) was chosen, parents can only respond to the difference by adjusting parental involvement. This assumes that, firstly, parents cannot affect neighborhood quality (e.g. through neighborhood watch organizations) and, secondly, parents cannot immediately move out of a neighborhood in response. The direction and magnitude of this response will depend on the parameter of interest \( \alpha \), but not on neighborhood selection.

Because relocations from closed housing projects are associated with an increase in receiving neighborhood crime rate, it seems reasonable to use these relocations as a negative shock to neighborhood quality. Taking an extreme assumption, suppose that, when choosing their \( t' \) neighborhoods, parents expected that no public housing closures would occur at all. Then, because some closures actually did occur, parents in receiving neighborhoods encountered lower neighborhood quality than they expected. Hence, relocations to the family’s current neighborhood would be uncorrelated with the neighborhood selection decisions of the family, and could be used as an instrument for neighborhood quality.

The problem with the above argument is that relocations are likely to have been anticipated by families in receiving neighborhoods. The geographical pattern of relocations was predictable; furthermore, news of impending closures may have spread quickly, allowing families to anticipate the influx of relocations. Making another extreme assumption, if parents perfectly anticipated the level of relocations to each neighborhood when choosing their \( t' \) neighborhoods, relocations would be useless to solve the endogeneity problem.

I make the following assumption instead: relocations to the family’s current neighborhood in the last year \( r_{j(i,t),t-1} \) are uncorrelated with the family’s neighborhood selection, conditional on fixed difference between neighborhoods \( G_j \), city-wide time effects \( H_t \), and cumulative prior relocations \( \sum_{\tau=1994}^{t-2} r_{j\tau} \). Formally, this assumption is given by (12). Under this assumption, relocations to the current neighborhood in the previous
year are a valid instrument for neighborhood quality, conditional on the listed variables.

\[
E \left[ r_{j,t-1} \varepsilon_{it} \bigg| G_{j,t}, H_{t}, \sum_{\tau=1994}^{t-2} r_{j,\tau} \right] \tag{12}
\]

The intuition behind this strategy can be explained by rewriting (8) as (13), such that neighborhood quality is decomposed into two parts. The first part is the parent’s expectation of neighborhood quality, conditional on \( I_{it} \), which denotes the information available to parents while they were deciding on their neighborhood of residence. The second is the deviation of realized neighborhood quality from this expectation. In words, (12) makes the following assumption: when committing to live in a particular neighborhood \( j \) in year \( t \), parents do not observe the level of relocations to that neighborhood, in the previous year. Instead, they make neighborhood choices with some expected level in mind. Furthermore, these expectations are sufficiently described by fixed differences across neighborhoods, city-wide trends, and the neighborhood’s history of cumulative relocations (up to the year before last). If parents’ expected level of relocations last year is a linear combination of these controls, then the variation in actual relocations received last year, net of these controls, will identify \( \alpha \).

\[
y_{it} = \alpha E \left[ q_{j(i,t),t} | I_{it} \right] + \alpha \left( q_{j(i,t),t} - E \left[ q_{j(i,t),t} | I_{it} \right] \right) + F_{i} + \varepsilon_{it} \tag{13}
\]

Consider an illustrative example: a parent moves to a particular neighborhood \( j \), without knowing how relocations in last year will affect neighborhood quality this year. Because \( j \) is a low-income neighborhood, the parent may expect many relocations to have arrived (high \( G_{j} \)). Also, she may have heard news that many closures happened last year (high \( H_{t} \)). Finally, she also realizes how many relocations in total settled in this neighborhood, as of two years before. Based on this information, she signs the lease on an apartment at the beginning of the year, only to realize that actual neighborhood quality is different from what she expected, due to the actual level of relocations last year. Because this parent has already signed the lease, she can only respond to this difference through adjusting her level of parental involvement.

### 4.3 Research Design 2: Relocations to Initial Neighborhood

The second way of dealing with neighborhood selection is to use the effect of relocations received by the family’s initial neighborhood, regardless of whether the family still lives in that neighborhood. Let \( t_{1} \) be the first year a family is observed in the data. The key assumption used is given by (14). It states that relocations
to the initial neighborhood are uncorrelated with the idiosyncratic residential preferences of a parent. In other words, having lived in a neighborhood with many previous relocations should not, on average, alter parental preferences for living in a high quality neighborhood.\footnote{This does not imply that relocations should not affect a parent’s residential decisions. If a parent prefers high quality neighborhoods, she may move out of a neighborhood receiving high relocations, because those relocations decreased neighborhood quality. However, the strength of her preference for high quality neighborhoods should not change, in expectation.}

\[ E\left[r_{j(1,t_1),t-k}e_{it}\right] = 0, \; k > 0 \tag{14} \]

Again, the intuition for this strategy can be explained by rewriting (8), this time as (15), which splits the change in neighborhood quality into two terms. The first term, \( q_{j(i,t),t'} \), reflects the neighborhood quality in the initial neighborhood. Conditional on an initial neighborhood, this is identical for all families, whether they moved out or stayed put. Hence, by definition, it is uncorrelated with neighborhood preference shocks \( e \) received by these families. The second term, \( q_{j(i,t),t} - q_{j(i,t_1),t} \), captures the difference in neighborhood quality between the chosen neighborhood and the initial neighborhood. The correlation with neighborhood selection enters solely through this term.

\[ y_{it} = \alpha q_{j(i,t_1),t} + \alpha \left( q_{j(i,t),t} - q_{j(i,t_1),t} \right) + F_i + e_{it} \tag{15} \]

As this decomposition suggests, one way to address the endogeneity problem is to isolate a component of variation across time in initial neighborhood quality, which is uncorrelated with the difference between chosen and initial neighborhood quality. Relocations received by the initial neighborhood are likely to change neighborhood quality in the initial neighborhood. However, it is also necessary that they do not affect the difference between chosen and initial neighborhood quality. This would hold under the previously-stated assumption that preferences for neighborhood quality are not affected by relocations received.

### 4.4 Neighborhood Selection by Displaced Households

The two strategies described above address the endogeneity caused by neighborhood selection of receiving families. There is another potential source of endogeneity: the neighborhood choices of displaced households. Displaced households from public housing may have chosen where to move to based on trends in neighborhood quality, or even based on average levels of parental involvement in that destination neighborhood.\footnote{Alternatively, instead of displaced households doing the selecting, the public housing authority may have relocated residents to other public-housing projects in a manner correlated with parental involvement in those neighborhoods. The proposed solution deals with both these forms of selection.}
example, if displaced households favored destination neighborhoods where average parental involvement was increasing, neighborhoods were average parental involvement was increasing would receive higher levels of relocations. This form of reverse causation would generate a spurious positive estimate for neighborhood quality effects on receiving parents.

While it is necessitated by data limitations, since actual relocation flows are not observed, the constructed shift-share measure of relocations is useful in addressing this particular endogeneity concern. It serves as an instrument for actual relocations, since it is likely to be uncorrelated with the neighborhood choice of displaced households. By construction, the measure $r_{jt}$ isolates a particular component of actual relocations to a neighborhood in a given year. Denoting actual relocations as $r_{Ajt}$, the measure of relocations used here can be related to actual relocations as shown by (16). First, actual relocations can be re-written as the product of $s_{jt}$, the share of yearly closures going to neighborhood $j$, and the total number of yearly closures $c_t$. This product can be further decomposed into the sum of three terms: the first term is the measure of relocations used. This term is neighborhood $j$’s average relocation share across years (denoted $s_j$) multiplied by the number of closures in year $t$, occurring in all neighborhoods except for $j$ (denoted $c_{j-}$).

$$r_{Ajt} = s_j c_t - (s_j - s_j) c_{j-} + s_{jt} c_{jt}$$ (16)

Without explicitly modeling the choice problem of displaced households, I now argue that the measured component of relocations $r_{jt}$ is plausibly unaffected by the preferences of displaced households for unobserved neighborhood factors. First, the number of closures occurring in a given year is mostly determined by administrative factors and unanticipated, building-specific events, which are unlikely to be correlated with the neighborhood choice of displaced households. Furthermore, any effect of unobserved neighborhood factors on the level of closures in the same neighborhood are also excluded. Second, the average share of relocations $s_j$ is interpreted as the influence of fixed differences across neighborhood, which affect the accessibility of the neighborhood to displaced households. Examples of such factors include the age/quality of homes in the neighborhood, as well as the convenience of public transport. Again, these factors are unlikely to be affected by the choices made by displaced households. If displaced families relocated preferentially to one neighborhood more in a given year than the next, due to fluctuations in parental involvement, these changes are excluded from the measure of relocations.

One problem with the above argument is that the empirical measure of $s_j$ from a concurrent time period
as the rest of the study. This means that any persistent trends in parental involvement will be captured in $s_j$. To address this concern, many studies (Autor and Duggan, 2003; Aizer, 2010; Saks and Wozniak, 2011) use historical measures of share components $^{14}$ Since historical relocations data is not available, I pursue an alternative approach: I generate predicted relocation shares $h_j$ using the neighborhood fraction black, measured in 1990 ($^{18}$). This procedure exploits the fact that the destination neighborhood choice of displaced families was severely constrained, and that, among other obstacles, these families faced racial discrimination when attempting to move to white neighborhoods (Oakley and Burchfield, 2009). Hence, the historical minority composition is positively correlated with the observed relocation shares. In addition, unless black residents chose neighborhoods anticipating future trends in unobserved neighborhood characteristics, historical minority composition is uncorrelated with those trends. In the empirical results, I present estimates using both observed relocation shares and the predicted ones ($^{19}$).

$$s_j = X_{j,1990}^i \beta_N + \epsilon_{Njt} \quad (17)$$
$$h_j = \hat{s}_j = b_{j,1990}^i \hat{\beta}_N \quad (18)$$
$$r_{Hjt} = h_j c_{-jt} \quad (19)$$

### 4.5 Crime Rate vs. Neighborhood Quality

In the preceding discussion, neighborhood quality is treated as a known variable. However, in reality, neighborhood quality is unobserved, and the neighborhood crime rate is used as a measure of neighborhood quality. I discuss the implications of doing so.

The most straightforward implication is that the estimates of $\alpha$ should not be interpreted as the causal effect of crime on parental involvement. Instead, they also capture the effect of other aspects of neighborhood quality that were altered by the relocations. Suppose that true neighborhood quality is the sum of crime rate (with a coefficient of $-1$) and an unobserved measure $\xi_{jt}$, ($^{20}$).

$$q_{jt} = -v_{jt} + \xi_{jt} \quad (20)$$

$^{14}$Bound and Holzer (2000) use a measure of shares closer to the one used here. They use the average of the share component at the beginning and at the end of the study period.
For discussion purposes, suppose that $\xi_{jt}$ represents school quality. If one unit of relocations raises crime rate and lowers school quality by $\theta_v$ and $\theta_\xi$ respectively, then the reduction in true neighborhood quality is the sum of these two: $\theta_v + \theta_\xi$. Hence, under the likely assumption that relocations change crime rate and school quality in different directions, the change in the crime rate alone will potentially understate the true change in neighborhood quality. In turn, estimates of the effect of neighborhood quality on parental involvement will be tend to be overstated in magnitude.

Suppose now, without loss of generality, that (20) is scaled such that $v_{jt}$ has standard deviation of 1. Now, consider the effect of relocations on standardized neighborhood quality. This is given by $\theta_q = (\theta_v + \theta_\xi) / \sqrt{1 + \sigma_\xi^2 - 2\rho_{v\xi}\sigma_\xi}$, where $\rho_{v\xi}$ denotes the correlation between crime and school quality. It is likely that the correlation is negative, so attention is restricted to this case. The relation between $\theta_q$ and $\theta_v$ is given by (21). In particular, in the case where $\rho_{v\xi}$ is negative, $\sqrt{1 + \sigma_\xi^2 - 2\rho_{v\xi}\sigma_\xi} - 1$ is less than $\sigma_\xi$. Then, under the additional assumption that $\theta_\xi / \sigma_\xi$ is approximately equal to $\theta_v$, (21) shows that $\theta_q$ will be understated by $\theta_v$. This last condition has the following interpretation: $\theta_\xi / \sigma_\xi$ is the effect of relocations on standard deviation units of school quality. As long as this effect is of roughly the same size as the effect of relocations on standard deviation units of crime rate, using crime rate in place of true neighborhood quality will not cause IV estimates to be overstated in magnitude. In fact, under all these assumptions, the estimates of the effect of neighborhood quality on parental involvement would be even more significant if the true measure of neighborhood quality were used. This reasoning provides some motivation for considering standard deviation units of crime rate, when estimating the effects on parental involvement.

$$\theta_q \geq \theta_v \iff \frac{\theta_\xi}{\sqrt{1 + \sigma_\xi^2 - 2\rho_{v\xi}\sigma_\xi} - 1} \geq \theta_v$$ (21)

Alternatively, instead of dealing with the unknown dimensions of neighborhood quality, it is possible to directly estimate the effect of relocations on parental involvement. These estimates are useful in establishing the direction of the effect of neighborhood quality. From section 3.3, it is probable that relocations decreased neighborhood quality. If this assumption is accepted, then the effect of relocations on parental involvement has the opposite sign as the effect of neighborhood quality on parental involvement.

Furthermore, if the effect of relocations on parental involvement differs with certain family characteristics, it is also likely that the effect of neighborhood quality differs as well. The latter claim requires the effect of relocations on neighborhood quality is uniform for families with different characteristics. On one hand, this
holds trivially since neighborhood averages like the crime rate are, by definition, identical for all families in the neighborhood. On the other hand, if the neighborhood is not the relevant reference group, the effect of relocations on the quality of the actual reference group could be different. For example, if the increase in criminal activity due to relocations only targeted the poorest families in the neighborhood, the true effect of neighborhood quality on rich families might be negligible. Without better information on social interactions at the sub-neighborhood level, it is difficult to explore this possibility. Hence, in the analysis of heterogeneous effects I assume that the effect of relocations on neighborhood quality was uniform for all families in the neighborhood, and I study the heterogeneous effects of relocations.

5 Data Description

This section describes the main source of data used in this study: the Project For Human Development in Chicago Neighborhoods (PHDCN). While this data has been widely used in the broader research literature on social science (Morenoff et al., 2001; Sampson et al., 1997), it is relatively unfamiliar within the specific field of economics. Within this section, special attention is paid to the construction of the parental involvement variable, based on the available measures of parenting behavior contained in the PHDCN data.

The main subjects of the PHDCN data are children/adolescents aged 0-18 years. In addition, the self-reported Primary Caregivers of these children were also surveyed. Henceforth, the terms parent and primary caregiver will be used interchangeably. For the purposes of this paper, the term family will refer to a primary caregiver-child pair, and the term parent and primary caregiver will be used interchangeably. These families were tracked as they moved across neighborhoods. By design, PHDCN sampling was restricted to 80 neighborhoods during Wave 1 (1994-1997). I refer to these 80 neighborhood clusters as initial neighborhoods.

The sample attrition rate is rather high: roughly 10 percent of the sample is lost in wave 2, and 20 percent in wave 3. However, the attrition is not strongly correlated with key family characteristics. Furthermore, attrition does not appear to be highly correlated with initial neighborhood. Figure 1 shows sample retention by mean income of the initial neighborhood. While respondents from poorer initial neighborhoods are slightly more likely to drop out of the sample, the difference in retention rate from the bottom to the top neighborhood quartile is at most 5 percentage points.

15The PHDCN also surveyed some young adults (aged 18 and above) who were living independently within the sampled neighborhoods. Because these young adults were not living with parents, they are excluded from this study.

16This refers to a neighborhood cluster (NC), which was defined in 3.
5.1 Measurement of Parental Involvement

The measure of parental involvement in this study is based on the features of the survey instrument: the Home Observation for Measurement of the Environment (HOME) inventory of the PHDCN. This battery of questions has been used in the National Longitudinal Study of Youth (NLSY) Child and Young Adult Survey, and is repeated in the PHDCN with slight modifications. The purpose of the HOME inventory is to measure the quality of the child’s home and family life. To this end, the HOME inventory gathered information about family practices regarding interaction with the child (e.g. conversations, outings) and discipline (e.g. TV usage, curfews). Most of the questions in the HOME inventory were of a binary nature. I aggregate these questions to create a single measure of parental involvement using principal components analysis. The first principal component is taken as the main indicator of overall parental involvement - I call it the HOME score. The first principal component is chosen because it offers an intuitive interpretation as a measure of overall intensity: the weights on individual behavior measures are uniformly positive. In other words, an increase in any individual parenting behavior will cause an increase in the HOME score.

The principal components analysis described above is done separately for each child age group. This is motivated by the varying number and type of questions in the HOME inventory, for children of different ages. For example, parents of toddlers are not asked about imposing curfews on their children. As a consequence, HOME scores are not directly comparable across age groups. For ease of comparison within age groups, the HOME scores are standardized by age group. The standardized HOME score thus measures parental involvement relative to other parents in the sample who have a child of similar age.

This constructed HOME score bears many similarities to the measures of parental input in the NLSY, although, again, it is not directly comparable. The NLSY version of this score has been used as a measure of parental behavior: Todd and Wolpin (2007) use it as a measure of home inputs in the production of cognitive function in children. At the same time, many other measures of parental involvement have been used. (Patacchini and Zenou, 2011) use the frequency of reading to the child as the measure of parental involvement, which covers a narrower scope of parenting behavior than the HOME score. On the other hand, (Guryan et al., 2008) have detailed measures of hours spent on each parenting activities. Compared to their

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17 The exact behavior measures in each subset are listed in Table A1.
18 The signs of these weights naturally depend on which direction of parenting behavior is defined as “more”. For most behaviors, this choice is unambiguous. However, in the area of physical punishment or losing temper, I have chosen to define avoidance of such harsh behaviors as the higher level of parenting. In support of this choice, in multiple studies in psychology, corporal punishment has been associated with increased externalizing behavior in children. See Gershoff (2002). In robustness checks, I show that the main results are not sensitive to the exclusion of parental disciplinary behavior.
19 In sensitivity analysis, I also explore an alternative treatment of the parental involvement variable: an index which weights each measure equally. In regressions of this variable, I include age of the child as a control.
measure, the HOME score used here is less sensitive to changes along the intensive margin of one particular parenting behavior: for example, it is able to capture the presence or absence of supervised homework time, but not an increase in the hours of supervised homework time.

5.2 Sample Restrictions

I place the following restrictions on the sample: First, I exclude any families that were surveyed only once. In this paper, I rely on the changes within the same family, across time. As a result, families that were only observed once are not useful for this purpose. Second, I exclude all families which left Chicago, at any point during the survey period: these families were not administered the full survey. As a result, there are several problems with missing information for these families. Finally, I exclude from the sample any changes in identity of the primary caregiver. Changes in the identity of the primary caregiver are likely to result in a change in residential neighborhood of the child; simultaneously, the different family structure is likely to result in changes in parental involvement as well. By excluding such families, any significant neighborhood quality effects found are less likely to be attributed to major changes in family structure.

After imposing these restrictions, the sample comprises roughly 2500 parent-child pairs (Table 1), each pair interviewed up to three times in the period 1994-2002. While some of these pairs are actually the same parent with multiple children, this information is not used in this study. Thus, every parent-child interaction is treated as a distinct observation.

5.3 Descriptive Statistics - Families

Table 2 shows some basic statistics about the PHDCN sample. The number of black primary caregivers in the sample is rather high (0.34) but in line with the racial composition of urban Chicago. Roughly 40 percent of the primary caregivers are employed, which is low but not surprising considering their caregiving status.

In addition to sample means, Table 2 also displays some evidence of neighborhood selection. In terms of initial characteristics, households who left their initial neighborhood during the study period appear to be more disadvantaged than households who did not: the primary caregiver is more likely to be unemployed, earns less, and exercises less parental involvement. At the same time, the children perform worse on the Wide Range Achievement Test (WRAT), and are more likely to exhibit problematic behaviors. The table

\footnote{Suppose a child has the same primary caregiver reporting for waves 1 and 2, and a different one in wave 3. In that case, waves 1 and 2 are retained and wave 3 is discarded.}

\footnote{This restrictions may be a source of concern if they are endogenous to neighborhood quality, for example if unsafe neighborhoods lead parents to send children to live full-time with grandparents. I also present results including changes in primary caregiver identity, and the effects are very similar.}
describes two kinds of problematic behavior: internalizing and externalizing. These terms are used widely in child psychology: internalizing refers to a group of withdrawn, inhibited, anxious or depressed behaviors. In contrast, externalizing refers to behavior that acts negatively on the external environment, including disruptive, hyperactive, and aggressive behaviors. Comparing families which were first observed in an affluent (above median income) neighborhood with those from poorer neighborhoods, a similar pattern emerges. Finally, a comparison of families with a high school graduate parent against those with a less-educated one tells essentially the same story.

Table 4 shows basic statistics about the initial neighborhoods of these sample respondents. Based on data from the 1990 census, neighborhoods that are in the study are a reasonably representative sample of neighborhoods in Chicago, in terms of average income, minority composition, and unemployment rate. As Column 5 of Table 4 shows, eight PHDCN neighborhoods contained a CHA public housing project. Five of those neighborhoods experienced a mandated closure. Since each initial neighborhood contains around 30 usable subjects, only about 150 respondents in the sample experienced the effects of a closure - too few for meaningful results on the effects of closures. For this reason, I focus on the effects of relocations instead of the effects of closures.

6 Empirical Findings

6.1 Exclusion Restrictions

Before presenting the main results, I present evidence that the exclusion restrictions imposed by each research design are not violated. While suggestive, these findings cannot conclusively show that the exclusion restrictions hold: it is possible that neighborhood choice along some untested (or unobserved) dimension violates the exclusion restriction. Nevertheless, these findings are presented in the spirit of a balancing test, to show that the assumptions made are not inconsistent with observable neighborhood choice behavior.

When using relocations to the current neighborhood, the required restriction is that, net of controls imposed in (12), neighborhood choice by sample families is uncorrelated with relocations to that neighborhood in the previous year. I focus on the choice to exit the initial neighborhood. For this analysis only, I restrict the sample to families with a complete yearly history of residential locations. For this restricted sample,

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22 The description of internalizing and externalizing behaviors is paraphrased from Liu (2004).

23 In baseline analysis, I retain families in closure neighborhoods. These families were still affected by relocations to their neighborhood. In robustness checks, I show that the main results are unaffected by excluding all families who ever resided in a neighborhood containing a public housing project.
information on the timing of residential moves is precise. The regression equation (22) takes the form of a
parametric (exponential) survival time regression. The outcome variable is a binary variable for never having
left the initial neighborhood. $\lambda_{it}$ always contains $r_{j(i,t_i),t}$, to measure the effect of last year’s relocations on
the probability of remaining in the initial neighborhood. Additionally, initial parental involvement $y_{i,t_1}$ is
included as a baseline control. I investigate how the effect of relocations on survival probability changes as
more controls are added.

$$Pr(T_{i,\text{surv}}^i > t) = \exp(\lambda_{it} t)$$

where $T_{i,\text{surv}}^i = \max \tau \text{ s.t. } j(i, \tau) = j(i, t_i)$

Table 5 shows these results. Column 1 reports the effect of last year’s relocations on the survival probability
without any controls. Relocations to the initial neighborhood reduce the chance that the parent remains in
that neighborhood. The negative effect on survival probability is expected: if relocations make the neigh-
borhood less desirable, then parents in neighborhoods experiencing a high volume of relocations will choose
to move out sooner. Column 2 includes neighborhood fixed effects, year effects, and cumulative prior re-
locations. With these controls, the effect of last year’s relocations becomes small and insignificant. This
provides support for the exclusion restriction assumed in (12): conditional on the controls imposed, reloca-
tions received by the family’s current neighborhood do not make families more likely to move out. Column 3
adds an interaction between initial involvement and relocations, to study if more involved parents moved out
faster in response to relocations. The insignificant coefficient suggests that, conditional on the same set of
controls, the is no evidence for such behavior. Based on these observable measures of neighborhood choice,
the exclusion restriction for the first strategy is plausible.

The second research design instead requires that prior relocations to the initial neighborhood are uncorreled
with the difference between chosen and initial neighborhood quality, in the current period. I test this
restriction directly, by regressing the difference in neighborhood crime rates (chosen neighborhood - initial
neighborhood) on lagged values of relocations to the initial neighborhood. The results of this regression,
with different sets of controls, are presented in Table 5, Columns 4 to 6. No coefficient is statistically
significant, and the magnitudes of all coefficients are uniformly small. In estimating these coefficients, both
the dependent variable and the measure of yearly relocations have been standardized. Hence, the coefficient
sizes imply that, for a standard deviation increase in prior relocations, crime rate in the chosen neighborhood increases by less than three hundredths of a standard deviation, relative to the initial neighborhood. Based on this, I conclude that the effects are negligible, and that the exclusion restriction for the second strategy seems reasonable.

6.2 Effects on Parental Involvement

In estimating the effects of relocations to the current neighborhood, I use the regression equation specified by (23). When it comes to the effects of relocations to the initial neighborhood, the regression equation is given by (24) instead. Controls in $X_{it}$ are parental marital status (including a category for single but cohabiting) and parental employment. Any family characteristic that does not vary over time is accounted for by subject-level fixed effects $F_i$.

\[ y_{it} = \rho r_{j(i,t),t-1} + \kappa \sum_{\tau=1994}^{t-2} r_{j(i,t),\tau} + X'_{it} \beta + F_{j} + G_{j} + H_{t} + \varepsilon_{it} \]  

(23)

\[ y_{it} = \sum_{k=1}^{2} \rho_k r_{j(i,t),t-k} + \rho_3 \sum_{\tau=1994}^{t-3} r_{j(i,t_1),\tau} + F_{j} + \phi_{1} t + \phi_{2} t^2 + \varepsilon_{it} \]  

(24)

Table 6 presents the estimated effect of relocations received on parental involvement. The estimates show that parents in receiving neighborhoods increased their parental involvement in response to incoming relocations. Across all specifications, the effect of relocations last year on parental involvement this year is positive and significant.\(^{24}\) When using relocations to the initial neighborhood instead of the current neighborhood, the coefficient increases but only slightly. Furthermore, controlling for the concurrent measurement of observed relocation shares (by using predicted shares based on 1990 neighborhood fraction black) does not change the coefficients appreciably.

One question that may be raised is whether these responses are short-lived or sustained. I investigate this question using relocations to the initial neighborhood. Using relocations to the current neighborhood is less reliable, because older lags of relocations are correlated with neighborhood selection behavior of receiving families. Using relocations to the initial neighborhood, the response to relocations two years ago is even larger than the response to last year’s relocations, suggesting that the effects are not just constrained to a

\(^{24}\)In this and subsequent analysis, standard errors have been clustered by community area. This choice is motivated by how relocation shares are reported at the community area level.
single year. When looking at the effect of cumulative relocations to the initial neighborhood (Column 5), a slightly larger coefficient is obtained.

The coefficients in Table 6 display the effect of 100 relocations. To interpret the magnitudes of these effects, the bottom panel of Table 6 contains means and standard deviations of relocation variables. During the years 1994-2002, the average neighborhood received around 30 households over 8 years. For this number of relocations received, the response of parental involvement is roughly 0.4 standard deviations.\textsuperscript{25} To reiterate, since parental involvement has been standardized by age group of the child, these effects mean that this change is relative to other parents with a child of similar age.

The results in Table 6 suggest that parents compensate for reduced neighborhood quality by increasing parental involvement. To confirm this claim, and to better describe the magnitude of the effects, I now present IV estimates of the effect of neighborhood quality on parental involvement, where crime rate is used as the measure of neighborhood quality. The baseline regression specification is given by (25). Relocations to the initial neighborhood are the focus here because the other strategy lacks power in the first stage.

\begin{align}
y_{it} &= \alpha \hat{q}_{jt} + X_{it}'\beta + F_i + \phi t + \varepsilon_{it} \\
q_{j(i,t)t} &= \theta r_{j(i,t_1),t-1} + F_{j(i,t_1)} + \psi t + \nu_{jt}
\end{align}

These IV estimates are shown in Table 7. A decrease in neighborhood quality, measured by an increase in the crime rate, is associated with increased parental involvement. Relative to the OLS estimates (Column 1), which do not account for neighborhood selection by receiving families, the baseline IV estimates are larger (Column 2). This finding is consistent with expected neighborhood selection behavior: parents who experience increasing motivation to care for their children are also likely to move to better neighborhoods. Columns 2 to 4 of Table 7 vary the instrument for neighborhood quality: using either observed relocation shares and historical fraction black as share components. To understand the magnitude of these estimates, I calculate the response of parental involvement to a one standard deviation increase in crime rate. Because crime rate varies widely between neighborhoods, and families rarely move to neighborhoods with drastically different crime rates, I use the within-neighborhood standard deviation of crime rate as a basis for comparison. For this variation in crime rate (17 crimes per 1000 residents), the estimates in Columns 2 to 4 suggest a effect size of 0.25 to 0.35 of a standard deviation. A response of this size is considerable: for comparison,\textsuperscript{25} This is for a coefficient of 1.3, which is in the middle of the range of estimated coefficients.
the gap in parental involvement between college education parents and less-educated parents is roughly 0.5 standard deviations.

In comparison to previous results, the results obtained here are very similar in spirit to those of Pop-Eleches and Urquiola (2013). Using a regression discontinuity design, they find that an increase in school quality results in a reduction in parental effort. Similarly, Katz et al. (2005) find that parents reduce monitoring of their children after an experimentally induced relocation to a better neighborhood, albeit for a different population - residents of low income public housing. On the other hand, these findings sharply contrast with those of Patacchini and Zenou (2011) (PZ). They find positive effects of neighborhood quality on parental involvement, a relationship they refer to as “cultural complementarity”. Compared to theirs, the results presented here are indicative of “cultural substitution”. Aside from differences in the sample - the sample in PZ is restricted to residents of council housing in the UK during the 1970s - one possible reason for the discrepancy is the timing of variable measurement. In PZ, parental involvement variables are measured when the child was 7 years old, but the reference neighborhood was measured when the child was 16 years old. During the intervening period, changes in the level of parental involvement made in response to changes in neighborhood quality (or even changes in neighborhood)\footnote{It is not reported if the sample is restricted to individuals who lived in the same neighborhood at these two points, or if the information is even available.} are not observed by PZ. In contrast, in this study, it is precisely those changes over time (e.g. parental involvement at age 7 vs. parental involvement at age 16) which identify neighborhood effects. Another issue with the timing is that neighborhood quality is measured after parental involvement, meaning that the interpretation of the effects is unclear. It is possible that, in PZ, initial levels of parental involvement are affecting neighborhood quality through selection into public housing, rather than the other way round.

6.3 Robustness

I examine the sensitivity of the above findings in Table 8. These robustness specifications modify the baseline analysis for the effects of relocations (Table 6, Columns 3 and 4). Mostly, the coefficients remain positive and significant. Column 1 of Table 8 changes the dependent variable to an additive index, instead of using the principal component. Column 2 restricts the sample to only child families, since the choice of parents with multiple children is not explicitly modeled, and the effects obtained here are larger. Column 3 discards any family who ever lived in a neighborhood containing a public housing project, for the reason that those projects might have closed, meaning that these families were affected by both closures and relocations.
Column 4 restricts the sample to families who never moved out of the initial neighborhood. If the exclusion restriction of each empirical strategy holds, the coefficients obtained should be identical for households who moved as it is for households who did not. The resulting estimates are slightly larger than the baseline, however, there is a large overlap in the confidence intervals.\textsuperscript{27}

Finally, Columns 5 and 6 alter the sample by respectively discarding any parents who changed marital status, and including any families where the primary caregiver changed. While there is a loss of significant for one coefficient in Column 5 (which may be due to the reduction in sample size), the significance of the estimates is mostly preserved.

### 6.4 Subsets of Parenting Behavior

I now consider what behaviors drive the increase in parental involvement. Due to their binary nature, individual behavior measures do not provide sufficient power. Hence, I consider two subsets of parenting behavior: interaction and discipline. Interaction contains behavior measures concerning routine daily life in the home, as well as any activities that the parent organizes for the child.\textsuperscript{28} Discipline covers the setting and enforcement of rules, both about homework, as well as chores and behavior with friends. Together, these two subsets cover all the parenting behavior measures in the HOME score. The effect of relocations on these subsets of parenting behavior is captured in two ways: first, the contribution of each subset to the HOME score is assessed. This is done by creating a weighted sum of behavior measures, using the weights derived from the principal components analysis in the creation of the HOME scores. Second, each subset of parenting behavior is considered separately, ignoring behavior from the other subset. This is done by extracting the first principal component of each subset. One additional group of parental behavior is considered: housekeeping. This group of behavior is excluded from the HOME score, since it is less clearly associated with parental care towards the child.\textsuperscript{29}

The effects of relocations on these subsets of parental behavior are presented in Table 10. Considering the contributions of each subset towards the overall measure, the bulk of the response comes from increases in parental interaction with children, as opposed to discipline. Hence, it appears that most of the increase in parental care comes from engaging with the child in activities or daily routines, instead of through

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\textsuperscript{27}The sample for households who moved is smaller, and the effect sizes obtained are similar but insignificant.

\textsuperscript{28}An example of the former is whether the parent engages in conversation daily with the child. An example of the latter is whether the child visits with family/friends regularly 2-3 times a month.

\textsuperscript{29}If housekeeping behavior is included in the HOME score, the weights on these behavior measures are very small, suggesting that housekeeping behavior is uncorrelated with the kind of parenting behavior that is included in the HOME score. Consequently, the main results for are not affected if housekeeping behavior is included in the measure for parental involvement.
disciplinary practices. When considering the first principal component within each subset, the results for discipline become inconsistent across specifications, however, the results for interaction remain positive and significant.

Additionally, the response of housekeeping behavior to relocations is relatively small and mostly insignificant. This works against the following explanation for the findings documented here: that parents in a declining neighborhood face lowered incentives to leave the house, due to rising crime rate, deteriorating neighborhood amenities or other causes. Under this explanation, parental involvement would increase as a result of parents spending more time at home. However, this explanation would also suggest an increase in housekeeping behavior as well, which is inconsistent with the findings.

6.5 Effects on Child Behavior

I estimate effects for child behavior, replacing the dependent variable with variables describing child behavior. These variables are the Wide Ranging Achievement Test (WRAT), as well as scores for internalizing and externalizing behavior. The estimated effects contain both the direct effect of neighborhoods on children and the indirect effect of changing parental involvement. These estimates are shown in Table 9. In contrast to the estimates for parental involvement, the estimates for child behavior have inconsistent sign across specifications. There is a significant, positive association between relocations received and child WRAT, for one specification but not the other. In contrast, there is a positive association between relocations received and externalizing problems, again for only one specification. Overall, the size of these effects are smaller than the effects on parental involvement. The biggest response in the family to declining neighborhood quality appears to have come from parents and not from children.

Together with these findings, the significant effects for parental involvement suggest that parents could be increasing involvement to compensate for deterioration in neighborhood quality. These compensating changes could be offsetting the effect of changing neighborhood conditions, so that the net effect on the children is very small. This reasoning assumes that an increase in parental involvement is beneficial towards producing good child behavior. This assumption is not tested in this paper, since the causal effect of parental involvement on child behavior is not identified. However, there is a body of research which suggests the positive effect of increased parental involvement on child behavior. Fryer Jr et al. (2015) find that incentivizing parental involvement leads to increased child achievement in preschool, within an experimental setting. In a similar study, Avvisati et al. (2014) find positive effects on student behavior and achievement.

30 These variables are introduced in Section 5.3.
These experimental findings is supported by a range of observational studies, which find a positive association between parental involvement and student achievement (Jeynes, 2007). Admittedly, the parental involvement variables featured in these other studies are restricted to academic matters (e.g. time spent in parent-teacher conferences), while the parental involvement variables measured here have a wider scope. However, Amato and Rivera (1999) finds that increased paternal involvement is associated with reduced behavioral problems in children, for a general definition of parental involvement that is similar to the one used here.

The findings for child behavior are insufficient to resolve the endogeneity between parenting and child behavior. In particular, the insignificant coefficients could be the result of small direct effects of neighborhoods on children, or, alternatively, of compensating changes in parental involvement. This ambiguity illustrates the limitations of existing research on neighborhood effects on children: even in experimental designs, the empirical object of interest is the aggregate of two conceptually distinct effects: the direct effect of neighborhoods on children and the mediating effect of altered parental involvement on children. These findings highlight the importance of considering parental mediation, if the research objective is to isolate the direct impact of neighborhoods on children. Furthermore, under the likely assumption that parental involvement positively affects child behavior, then, based on the findings in this paper, the direct effect of neighborhoods on children is likely to be larger (more positive) than existing estimates suggest.

6.6 Heterogeneous Effects of Relocations

I consider whether the effect of relocations on parental involvement differ across different types of families. For this analysis, I present results using relocations to the current neighborhood, using observed relocation shares. These results are summarized in Table 11, and also plotted on various figures. The most important finding of this section is displayed in the right panel of Figure 5. There is a positive association between initial parental involvement and the response to incoming relocations. Parents with above-median initial involvement levels exhibit a positive, compensatory response. In contrast, parents from the lowest initial involvement quartile actually respond by reducing involvement, by about the same magnitude per relocation as the increase for parents with the highest initial involvement. The reversal of sign is especially significant: if there is variation in how relocations affected different subgroups within the neighborhood (e.g. if segregation means that children of low involvement parents were more exposed to negative peer influences), it may be argued that the families with the lowest parental involvement face a different neighborhood quality effect than other families. As long as all families encountered a decrease in neighborhood quality, the reversal of sign still indicates a switch from compensating to reinforcing behavior.
Since more involved parents tend to live in better neighborhoods, it may seem unclear whether the heterogeneous response of parental involvement is mainly driven by differences across neighborhoods, as opposed to differences between parents living in the same neighborhood. As the left half of Figure 5 shows, there is a positive association between the initial neighborhood income of the family with the subsequent response to relocations, which suggests that parents starting out in more affluent neighborhoods compensate more for the same amount of relocations. However, the differences between coefficients is much smaller, suggesting that a substantial part of the heterogeneous response is unaccounted for by sorting into initial neighborhoods. This hypothesis is verified directly, by repeating the analysis using initial involvement relative to the mean within the initial neighborhood: the results are very similar (Figure A2).

I discuss a few implications of this striking finding. First, a switch from reinforcing to compensating parental behavior has previously been documented, in a different context by Hsin (2012). She finds that low-SES mothers invest less time in a low birth weight child relative to a sibling with normal birth weight; in contrast, high-SES parents invest more.

Next, this finding implies that place-based policies to influence outcomes of children may have varied impacts, if parental mediation is not targeted as well. Specifically, low-involvement parents might magnify the effect of such policies through increased parental involvement, while high involvement parents diminish these effects. Hence, when designing policies to counteract the effects of worsening environmental influences on long-run child outcomes, policymakers might seek to target children receiving low levels of parental care. On the other hand, policies to improve the neighborhood environment are likely to be more effective on this same group of children, because of parental mediation.

As Figure 6 shows, the effects of CHA relocations on parental involvement do not differ significantly by age cohort or gender of the child. Looking at interactions by age cohort, the parental response for children in the age 0-3 cohort is much larger, in terms of the point estimate, although the standard errors are large. This difference can be explained in a few ways: The first explanation is that parental involvement is of decreasing effectiveness as children grow older. Similarly, it could also be the case that children are less susceptible to neighborhood influence after they reach a certain age. Finally, it is also possible that parents are directly affected by changing neighborhood conditions, through changing parental information or aspirations are affected, which drives the change in parental involvement. Comparing parental response across children of different sexes, no clear conclusions can be drawn.

Furthermore, Figure 7 shows that the response of parental involvement to relocations does not vary with
parental education and race. While, as established in the descriptive statistics, base levels of parental involvement differ substantially across parents of different education levels, the magnitude of the response appears to be very similar.

Furthermore, standard demographic characteristics do not perform well in predicting which parents make reinforcing adjustments to parental involvement. Such characteristics include age and gender of the child (Figure 6), as well as race and education level of the parent (Figure 7). Since information about parenting behavior is rarely collected, it can difficult to identify children who are doubly vulnerable to neighborhood decline. In turn, this means it is difficult to target policies to mitigate the impact of declining neighborhoods at these children in particular. To a limited extent, observable child behavior can be used as a predictor of the direction of parental mediation. As Figure 8 shows, children from the lowest quartile of cognitive test scores (WRAT) tend to have parents who make reinforcing responses to neighborhood quality change, in contrast to children with higher scores. However, the magnitude of the difference is much smaller than the interaction with initial involvement, which suggests that it would be preferable to target policies by parental involvement levels, if that information is available.

6.7 Are Parents and Neighborhoods Complements or Substitutes?

Finally, as an extension, I rationalize the heterogeneous response by initial parental involvement using the model outlined in Section 4. This exercise provides some insight into the mechanism driving parental involvement responses: specifically, whether parental involvement and neighborhood quality are substitutes or complements in producing good child outcomes. The finding of a compensating parental response on average, is consistent with a relationship of substitution. However, because neighborhood quality can also affect parental utility apart from productivity, this claim does not immediately follow. To analyze the implications of the heterogeneous response, (8) is rewritten here as (27), with one difference: I now allow for parental altruism parameter $\omega_i$ to vary across families. As stated in Section 4, this parameter also captures other differences in the opportunity cost of parental involvement.

$$y_{it} = (\omega_i \alpha^p + \alpha^c) q_{j(i,t),t} + (\omega_i F^p_i - F^c_i) + (\omega_i \epsilon^p_{it} - \epsilon^c_{it})$$ (27)

Recall that the parameter $\alpha$ can be decomposed as follows: $\alpha = \omega_i \alpha^p + \alpha^c$, where $\alpha^p$ denotes the effect of neighborhood quality on the marginal productivity of parental involvement, and $\alpha^c$ denotes the effect of neighborhood quality on the marginal effort cost instead. By assumption, $\alpha^p q_{j(i,t),t} + F^p_i$ is positive,
as a necessary condition for the marginal productivity of parental involvement to be positive. It follows immediately that, within a given neighborhood, parents with higher altruism have a higher initial level of involvement.

By inspection of (27), the cross-partial derivative of parental involvement with respect to neighborhood quality and parental altruism is equal to $\alpha_p$. The empirical analogue to this cross-partial derivative is the association between initial involvement and the response to changing neighborhood quality. Empirically, higher initial involvement is associated with a more positive response of parental involvement to incoming relocations. Bearing in mind that relocations decrease neighborhood quality, this finding suggests that higher initial involvement is associated with a more negative response of parental involvement to an increase in neighborhood quality. This then implies that $\alpha_p$ is negative: in other words, that parental involvement and neighborhood quality are substitute inputs.

The intuition behind this result is that greater parental altruism not only affects the initial level of behavior, but also affects how sensitive parents are to changes in the productivity of parental involvement, due to neighborhood quality. Because parents with high altruism seem to have a more negative/compensatory response to neighborhood quality, this implies that increased neighborhood quality decreases the marginal productivity of parental involvement.\(^{31}\)

The result suggests that policies to improve the external environment of the child and policies to foster parental engagement will, to a certain extent, “crowd out” the other. Such jointly-targeted policies are often implemented: one well-known case is the Harlem Children’s Zone. This finding suggests that, when implemented in conjunction with interventions targeting children, policies encouraging parental engagement may have smaller effectiveness than when implemented alone.\(^{32}\)

Relative to previous work by Patacchini and Zenou (2011), this study makes use of better data quality - specifically, panel data - to deliver the above result. In contrast to their model, in this study the production technology of child outcomes is not assumed to be one of substitution. Instead, substitution is delivered as a result, revealed by analyzing the interaction between initial involvement and the response to neighborhood decline. Because Patacchini and Zenou (2011) uses a single cross-section, the interaction cannot be estimated.

\(^{31}\) The result holds under more general functional form assumptions. See Section B for a brief discussion.

\(^{32}\) While the specific functional forms chosen for
7 Conclusion

In this paper, I have investigated how parental involvement changes in response to neighborhood quality. By using induced relocations as a natural experiment, I first verify that relocations induced by public housing closures cause neighborhood crime levels to increase in the subsequent year. This suggests the relocations had an adverse impact on neighborhood quality in the receiving neighborhoods. Following this, I find that a higher level of relocations causes parents to increase their level of involvement: a pattern which has not been previously documented or accounted for in studies of neighborhood effects on children. In addition, I find significant interaction effects between the relocations and basic demographic characteristics of the families. This suggests that the response of parental involvement to neighborhood quality differs meaningfully between families. The differing magnitude of parental response may result in different later-life outcomes in children, in response to the same neighborhood shock. Additionally, the interaction between initial levels of parental involvement and its response to neighborhood is informative on the latent structure of the decision problem faced by parents.

Further work is needed to incorporate child behavior into the model, in order to examine further the mechanisms affecting parental involvement, as well as its ultimate impact on children. One goal of such research would be to weight the relative contributions of neighborhood quality and parental involvement towards later-life outcomes in children. Patacchini and Zenou (2011) have made some steps in this direction, by estimating the effect of (randomly-assigned) neighborhood quality and parental investment towards child education, however, the analysis is limited by the single measure of neighborhood quality and of parental involvement.

Another promising direction is to incorporate neighborhood selection by parents in more detail. While, the shock to neighborhood quality cause by incoming relocations is unlikely to have influenced neighborhood exit in the short run, it is likely to have had some influence in the long run, as the signs of neighborhood decline became more obvious. Another question about neighborhood selection is where parents facing a declining neighborhood choose to move to, and how the new neighborhood compares to the old one. Finally, analyzing the simultaneous choice of neighborhoods and parental involvement would answer a new set of interesting questions. For example, are the parents who move also the ones who change their level of involvement? Also, do the changes in neighborhood quality brought on by moving reinforce the changes (if any) in parental involvement? These are the questions to be pursued in the future.
### Tables

#### Table 1: PHDCN Sample Size

<table>
<thead>
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<th>Overall</th>
<th>Wave 1</th>
<th>Wave 2</th>
<th>Wave 3</th>
<th>Total</th>
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<tbody>
<tr>
<td>All subjects</td>
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<td>4855</td>
<td>16421</td>
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<td>3507</td>
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<tr>
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<td>Toddler Sample (0-5 years)</td>
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<td>1337</td>
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<tr>
<td>Young Adults (18+ years)</td>
<td>633</td>
<td>506</td>
<td>984</td>
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<table>
<thead>
<tr>
<th>Overall</th>
<th>Wave 1</th>
<th>Wave 2</th>
<th>Wave 3</th>
<th>Total</th>
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<td>All subjects</td>
<td>75.0</td>
<td>48.8</td>
<td>34.8</td>
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<td>Consistent PC</td>
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<td>Toddler Sample (0-5 years)</td>
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<td>12.9</td>
<td>3.64</td>
<td>11.2</td>
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<tr>
<td>Young Adults (18+ years)</td>
<td>7.63</td>
<td>4.61</td>
<td>7.10</td>
<td>6.45</td>
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Table 2: Descriptive Statistics of PHDCN Families

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<th></th>
<th>Mean (sd)</th>
<th>Differences (p)</th>
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<tr>
<td></td>
<td>Wave 1</td>
<td>Move - Stay</td>
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<tr>
<td><strong>Primary Caregiver (PC):</strong></td>
<td></td>
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<tr>
<td>Employed</td>
<td>0.43 (0.50)</td>
<td>0.074 (0.00)</td>
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<td>Salary ($ 000)</td>
<td>21.1 (33.1)</td>
<td>-5.25 (0.00)</td>
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<tr>
<td>Black</td>
<td>0.35 (0.48)</td>
<td>0.11 (0.00)</td>
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<tr>
<td>PC age</td>
<td>37.0 (8.86)</td>
<td>-3.82 (0.00)</td>
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<tr>
<td>PC Female</td>
<td>0.93 (0.25)</td>
<td>0.027 (0.00)</td>
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<tr>
<td><strong>Parental Involvement:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sum, HOME Score Measures</td>
<td>12.6 (2.30)</td>
<td>-0.47 (0.00)</td>
</tr>
<tr>
<td><strong>Parenting Behavior Subsets:</strong></td>
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<td></td>
</tr>
<tr>
<td>Interaction</td>
<td>5.75 (2.92)</td>
<td>-0.063 (0.59)</td>
</tr>
<tr>
<td>Discipline</td>
<td>7.54 (3.09)</td>
<td>-0.59 (0.00)</td>
</tr>
<tr>
<td>Parental Involvement Measure</td>
<td>0.024 (0.98)</td>
<td>-0.10 (0.01)</td>
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<tr>
<td><strong>Child:</strong></td>
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<tr>
<td>Age</td>
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<td>-0.93 (0.00)</td>
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<tr>
<td>WRAT</td>
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<tr>
<td><strong>Behavioral Scores:</strong></td>
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<tr>
<td>- Internalizing</td>
<td>8.58 (7.40)</td>
<td>1.02 (0.00)</td>
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<tr>
<td>- Externalizing</td>
<td>9.64 (8.23)</td>
<td>2.14 (0.00)</td>
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<tr>
<td>Observations</td>
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</tr>
</tbody>
</table>

Sample excludes any subjects who left Chicago, changed PCs, as well as those whose PC changed marital status. Variables measured at Wave 1 of survey.

Rich - Poor Nbd.: whether wave 1 neighborhood was above/below median neighborhood income. Move - Stay: families which changed / did not change neighborhood when next interviewed. PC HSG - Dropout: families where the primary caregiver attained/did not attain a high school degree. Salary: treated as missing if PC is unemployed.

Table 3: Comparison of PHDCN Families with Census Households

<table>
<thead>
<tr>
<th></th>
<th>(1) PHDCN</th>
<th>(2) Census</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frac. white</td>
<td>0.16</td>
<td>0.47</td>
</tr>
<tr>
<td>Frac. black</td>
<td>0.34</td>
<td>0.32</td>
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<tr>
<td>Frac. hispanic</td>
<td>0.46</td>
<td>0.31</td>
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<tr>
<td>working</td>
<td>0.50</td>
<td>0.89</td>
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<tr>
<td>Frac. on welfare</td>
<td>0.22</td>
<td>0.11</td>
</tr>
</tbody>
</table>
### Table 4: Neighborhood Characteristics

<table>
<thead>
<tr>
<th></th>
<th>(1) mean/sd</th>
<th>(2) mean/sd</th>
<th>(3) mean/sd</th>
<th>(4) mean/sd</th>
<th>(5) mean/sd</th>
<th>(6) mean/sd</th>
</tr>
</thead>
<tbody>
<tr>
<td>ln. family income</td>
<td>10.4</td>
<td>10.4</td>
<td>9.90</td>
<td>9.74</td>
<td>10.5</td>
<td>10.4</td>
</tr>
<tr>
<td>Frac. minority</td>
<td>0.56</td>
<td>0.54</td>
<td>0.75</td>
<td>0.84</td>
<td>0.52</td>
<td>0.56</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>0.14</td>
<td>0.12</td>
<td>0.27</td>
<td>0.32</td>
<td>0.15</td>
<td>0.18</td>
</tr>
<tr>
<td>Population</td>
<td>8121.7</td>
<td>9081.6</td>
<td>8276</td>
<td>7950.4</td>
<td>10304.3</td>
<td>11909.8</td>
</tr>
<tr>
<td>PHDCN observations</td>
<td>(2914.9)</td>
<td>(2966.0)</td>
<td>(3862.7)</td>
<td>(3912.2)</td>
<td>(4188.4)</td>
<td>(3781.6)</td>
</tr>
<tr>
<td>Relocates</td>
<td>5.36</td>
<td>4.54</td>
<td>5.62</td>
<td>4.00</td>
<td>2.45</td>
<td>2.66</td>
</tr>
<tr>
<td>Observations</td>
<td>343</td>
<td>83</td>
<td>32</td>
<td>22</td>
<td>8</td>
<td>5</td>
</tr>
</tbody>
</table>

### Table 5: Exclusion of Relocations from Neighborhood Selection

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Remaining in Initial Neighborhood</th>
<th>Crime Rate, Current - Initial Neighborhood.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) exp(coef.) (2) exp(coef.) (3) exp(coef.) (4) exp(coef.)</td>
<td>(5) coeff. (6) coeff. (7) coeff.</td>
</tr>
<tr>
<td>Relocations to Initial Neighborhood:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cumulative, t-1</td>
<td>1.550 (0.467)</td>
<td></td>
</tr>
<tr>
<td>t-1</td>
<td>0.871* (0.060)</td>
<td>1.073 (0.052)</td>
</tr>
<tr>
<td>x Parental Involvement</td>
<td>0.009 (0.036)</td>
<td></td>
</tr>
<tr>
<td>Neighborhood FE</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Subject FE</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Control for Year</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Cum. prior relocations</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>14672</td>
<td>14672</td>
</tr>
</tbody>
</table>

Notes:
- Coefficients describe effect of 1 standard deviation in relocations received (7.7 households).
- Columns 1 to 4: Exponentiated coefficients from parametric (exponential) survival time regression.
- Column 4 also controls for interaction between cumulative prior relocations and parental involvement.
- Regression sample excludes any child who changed parents (primary caregivers), as well as any child who left Chicago.
- All models control for parental employment and parental marital status, measured at baseline.
- Robust standard errors in parentheses.
### Table 6: Effect of Receiving Public Housing Relocations on Parental Involvement

<table>
<thead>
<tr>
<th>Relocations Received:</th>
<th>Observed Shares</th>
<th>Fraction Black</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Relocations Received:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>t-1</td>
<td>1.14*</td>
<td>1.43***</td>
</tr>
<tr>
<td>(0.55)</td>
<td>(0.41)</td>
<td>(0.51)</td>
</tr>
<tr>
<td>t-2</td>
<td>1.82*</td>
<td>1.49*</td>
</tr>
<tr>
<td>(0.70)</td>
<td>(0.66)</td>
<td></td>
</tr>
<tr>
<td>cumulative, t-1</td>
<td>2.51***</td>
<td></td>
</tr>
<tr>
<td>Neighborhood FE</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Year Effects</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Year Quadratic</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Cum. Prior Lags</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Relocations Received by Neighborhood:</td>
<td>Current</td>
<td>Initial</td>
</tr>
<tr>
<td>mean (s.d.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relocations / year</td>
<td>3.5</td>
<td>(7.7)</td>
</tr>
<tr>
<td>If reloc/yr &gt; 0</td>
<td>5.6</td>
<td>(9.0)</td>
</tr>
<tr>
<td>Cum. (’94-’02)</td>
<td>31.8</td>
<td>(40.6)</td>
</tr>
<tr>
<td>Observations</td>
<td>7481</td>
<td>7481</td>
</tr>
</tbody>
</table>

Notes:
- Effects take units of standard deviations per 100 households received.
- Dependent Variable: First principal component of 15-23 parenting behaviors, standardized by child age group.
- Neighborhood: refers to study-defined Neighborhood Cluster (343 Neighborhood Clusters in Chicago).
- Relocations constructed as product of Yearly Closures with either Observed (relocation) Shares, or Fraction Black.
- Yearly Closures: City-wide number of public housing apartments closed, excluding receiving neighborhood.
- Sample excludes families that left Chicago, or changed parent (Primary Caregiver).
- Common controls: Parental employment, parental marital/co-habitation status, subject fixed effect.
- Models with Fraction Black also control for interaction between Fraction Black and linear time trend.
- Standard errors clustered by initial community area of residence (77 community areas in Chicago).

### Table 7: IV Estimates: Effect of Neighborhood Quality on Parental Involvement

<table>
<thead>
<tr>
<th>Measure of Neighborhood Quality (default = Crime Rate)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Second Stage Coefficient</td>
<td>0.25+</td>
<td>1.40*</td>
<td>2.09*</td>
<td>1.50*</td>
<td>0.19*</td>
<td>2.07*</td>
<td>4.32*</td>
</tr>
<tr>
<td></td>
<td>(0.13)</td>
<td>(0.64)</td>
<td>(0.86)</td>
<td>(0.62)</td>
<td>(0.08)</td>
<td>(0.96)</td>
<td>(2.12)</td>
</tr>
<tr>
<td>S.D. of Regressor, Within-ndb.</td>
<td>0.17</td>
<td>0.17</td>
<td>0.17</td>
<td>0.17</td>
<td>1.50</td>
<td>0.13</td>
<td>0.06</td>
</tr>
</tbody>
</table>

#### Relocations Instrument for Neighborhood Quality

<table>
<thead>
<tr>
<th>None</th>
<th>Observed</th>
<th>Frac. Black</th>
<th>per Resident</th>
<th>Observed</th>
<th>Observed</th>
<th>Observed</th>
</tr>
</thead>
<tbody>
<tr>
<td>F-statistic, Instrument</td>
<td>15.30</td>
<td>10.43</td>
<td>23.08</td>
<td>7.20</td>
<td>16.17</td>
<td>5.94</td>
</tr>
<tr>
<td>Observations</td>
<td>7481</td>
<td>7481</td>
<td>7481</td>
<td>7481</td>
<td>7481</td>
<td>7481</td>
</tr>
</tbody>
</table>

Notes:
- Dependent Variable: First principal component of 15-23 parenting behaviors, standardized by child age group.
- Second Stage Coefficient for Crime Rate (Number Crimes) shows effect of 1 crime per 10 residents (100 crimes).
- Property/Personal: categories of crime expressed as crime rates.
- First Stage Coefficient shows effect of 1 relocation on crimes per 100 residents (crimes).
- Neighborhood: refers to study-defined Neighborhood Cluster (343 Neighborhood Clusters in Chicago).
- All instruments measured from initial neighborhood, neighborhood quality measured from current neighborhood.
- Observed: product of yearly public housing closures in previous year, and observed relocation shares.
- Frac. Black: product of yearly public housing closures in previous year, and neighborhood black share in 1990 per Resident: Observed measure, divided by neighborhood population in 1990.
- Controls: Parental employment, parental marital/co-habitation status, subject fixed effect.
- Standard errors clustered by initial neighborhood.
### Table 8: Robustness Checks: Effect of Relocations Received on Parental Involvement

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Index</td>
<td>Only Child</td>
<td>No Public Housing</td>
<td>Never Moved</td>
<td>Same Marital</td>
<td>Changed Parent</td>
</tr>
<tr>
<td>Current Neighborhood</td>
<td>Relocations, t-1</td>
<td>1.63***</td>
<td>2.30**</td>
<td>1.73**</td>
<td>1.62*</td>
<td>1.29+</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.46)</td>
<td>(0.74)</td>
<td>(0.64)</td>
<td>(0.79)</td>
<td>(0.70)</td>
</tr>
<tr>
<td></td>
<td>Relocations, t-2</td>
<td>1.11*</td>
<td>1.92***</td>
<td>1.33*</td>
<td>1.98***</td>
<td>2.18**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.49)</td>
<td>(0.52)</td>
<td>(0.68)</td>
<td>(0.53)</td>
<td>(0.60)</td>
</tr>
</tbody>
</table>

**Notes:**
- Effects take units of standard deviations per 100 households received.
- Index: HOME Score constructed as additive index, rather than as primary component.
- Only Child: Sample restricted to one-child families.
- No Public Housing: Sample restricted to families that never lived in neighborhood containing public housing project.
- Never Moved: Sample restricted to families that never moved during study.
- Same Marital: Sample excludes any parents who changed marital status.
- Changed Parent: Sample includes children whose primary caregiver changed.
- Each panel presents results from a different regression specification.
- All specifications control for parental employment, parental marital/co-habitation status, subject fixed effects.
- Current Neighborhood: Additional controls for year effects, neighborhood fixed effects, cumulative relocations up to t-2.
- Initial Neighborhood: Additional controls for quadratic in year, interaction between 1990 fraction black and time trend.
- Relocations constructed as product of Yearly Closures with either Observed (relocation) Shares, or Fraction Black Yearly Closures: City-wide number of public housing apartments closed, excluding receiving neighborhood.
- Share Component: Predicted fraction of all relocations (1999-2008) going to receiving neighborhood.
- Standard errors clustered by initial community area of residence (77 community areas in Chicago).

### Table 9: Effect of Relocations Received on Child Behavior

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>WRAT</td>
<td>Internalizing</td>
<td>Externalizing</td>
<td>WRAT</td>
<td>Internalizing</td>
<td>Externalizing</td>
</tr>
<tr>
<td>Relocations Received:</td>
<td>t-1</td>
<td>1.52+</td>
<td>-0.02</td>
<td>0.46</td>
<td>0.64</td>
<td>0.43</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.84)</td>
<td>(0.44)</td>
<td>(0.45)</td>
<td>(0.39)</td>
<td>(0.26)</td>
</tr>
<tr>
<td></td>
<td>t-2</td>
<td>0.42</td>
<td>0.55+</td>
<td>(0.39)</td>
<td>(0.33)</td>
<td>(0.47)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.39)</td>
<td>(0.33)</td>
<td>(0.47)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relocations to Neighborhood</td>
<td>Current</td>
<td>Current</td>
<td>Current</td>
<td>Initial</td>
<td>Initial</td>
<td>Initial</td>
</tr>
<tr>
<td>Observations</td>
<td>4914</td>
<td>7271</td>
<td>7271</td>
<td>4914</td>
<td>7271</td>
<td>7271</td>
</tr>
</tbody>
</table>

**Notes:**
- Effects take units of standard deviations per 100 households received.
- Dependent variables specified by column heading, and are standardized within child age group.
- WRAT: Wide Range Achievement Test.
- Internalizing: Problematic behavior index, associated with depression, withdrawal, etc.
- Externalizing: Problematic behavior index, associated with attention deficit, hyperaggression, etc.
- All specifications control for parental employment, parental marital/co-habitation status, subject fixed effects.
- Current Neighborhood: Additional controls for year effects, neighborhood fixed effects, cumulative relocations up to t-2.
- Initial Neighborhood: Additional controls for quadratic in year, interaction between 1990 fraction black and time trend.
- Relocations constructed as product of Yearly Closures with either Observed (relocation) Shares, or Fraction Black Yearly Closures: City-wide number of public housing apartments closed, excluding receiving neighborhood.
- Share Component: Predicted fraction of all relocations (1999-2008) going to receiving neighborhood.
- Using 1990 neighborhood fraction black.
- Sample excludes families that left Chicago, or changed parent (Primary Caregiver).
- Standard errors clustered by initial community area of residence (77 community areas in Chicago).
### Table 10: Effect of Relocations Received on Parenting Behavior Subsets

<table>
<thead>
<tr>
<th></th>
<th>HOME Score Contribution</th>
<th>1st Principal Component</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Interaction</td>
<td>(2) Discipline</td>
</tr>
<tr>
<td><strong>Relocations to Current Neighborhood</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>t-1</td>
<td>0.38 (0.26)</td>
<td>0.03 (0.12)</td>
</tr>
<tr>
<td>t-2</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Effects take units of standard deviations per 100 households received.

### Table 11: Heterogeneous Effects of Relocations Received on Parental Involvement

<table>
<thead>
<tr>
<th>Age Cohort</th>
<th>Child Gender</th>
<th>Parent Education</th>
<th>Neighborhood Income</th>
<th>Initial Involvement</th>
<th>Child WRAT</th>
</tr>
</thead>
<tbody>
<tr>
<td>0-3</td>
<td>Male</td>
<td>1.60**</td>
<td>No HS</td>
<td>0.40</td>
<td>1st Quartile</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.07 (0.93)</td>
</tr>
<tr>
<td>3-6</td>
<td>Female</td>
<td>0.72</td>
<td>Some HS</td>
<td>1.33+</td>
<td>2nd Quartile</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.19 (0.81)</td>
</tr>
<tr>
<td>6-9</td>
<td>Male</td>
<td>0.68</td>
<td>HS Degree</td>
<td>1.43+</td>
<td>3rd Quartile</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.43 (0.82)</td>
</tr>
<tr>
<td>9-12</td>
<td>Male</td>
<td>0.23</td>
<td>Post HS</td>
<td>1.00+</td>
<td>4th Quartile</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.00 (0.57)</td>
</tr>
<tr>
<td>12+</td>
<td>Male</td>
<td>1.48</td>
<td>College Degree</td>
<td>1.43</td>
<td>Continuous</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.43 (1.22)</td>
</tr>
</tbody>
</table>

Notes: Coefficients from interaction of listed characteristic with 100 relocations received last year. All characteristics measured at Wave 1. Controls: cumulative prior relocations (up to t-2) parental employment, subject fixed effect, neighborhood effect, year dummies. All characteristics measured at wave 1. Robust standard errors in parentheses. Equal?: shows p-value for test of joint equality of coefficients on all categories of characteristic. Years Attained / Continuous: show coefficients from (separate) regression of continuous variable interacted with 100 relocations received last year.
Figures

Figure 1: Sample Retention by Neighborhood Income Quartile and Survey Wave

Notes:
Retention rates measured as a percentage of wave 1 respondents.
Neighborhood income: 1990 average family income of initial neighborhood.
Figure 2: Closures and Relocations in Chicago

(a) Total Number of CHA Public Housing Units Closed by Year

(b) Histogram of Community Area Relocation Shares

Figure 3: Histogram of Relocations Received per Capita

Notes:
Figure 4: Effects of Relocation Leads/Lags on Neighborhood Crime Rate

Notes:
Sample: All neighborhoods (NCs) in Chicago, years 1992 to 2004.
Controls: Neighborhood fixed effect, year effect.
90% confidence intervals shown, calculated using robust standard errors.
Figure 5: Heterogeneous Effects of Relocations Received on Parental Involvement, by Initial Neighborhood / Initial Involvement

Notes:
Neighborhood income of Wave 1 neighborhood measured in 1990.
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to \( t - 2 \)) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-value displayed for test of joint equality across all categories.
Figure 6: Heterogeneous Effects of Relocations Received on Parental Involvement, by Child Age / Gender

Notes:
Age cohorts of child measured in Wave 1.
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to t – 2) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-values displayed for test of joint equality across all categories.
Figure 7: Heterogeneous Effects of Relocations Received on Parental Involvement, by Parent Education / Race

Notes:
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to $t-2$) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
$P$-values displayed for test of joint equality across all categories.
Figure 8: Heterogeneous Effects of Relocations Received on Parental Involvement, by Initial Child WRAT

Notes:
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
WRAT: Cognitive test score, measured in Wave 1. Controls: cumulative prior relocations (up to $t-2$) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-values displayed for test of joint equality across all categories.
Appendix A  Data Appendix

A.1 PHDCN Sampling

For the PHDCN, 847 census tracts in Chicago were collapsed into 343 neighborhood clusters to form neighborhood clusters: the unit of analysis for residential neighborhoods in this study. The process of aggregation was guided by the following principles: first, the tracts to be aggregated were to be spatially contiguous. Second, these tracts were to be relatively homogeneous along a set of key census variables. In addition, the boundaries of neighborhood clusters were to respect physical boundaries (e.g. railroads and highways) and conform to local definitions of neighborhoods.

The panel data used in this study comes from the Longitudinal Cohort Survey of the PHDCN. Aside from this component, the PHDCN also contains a cross-sectional Community Survey and a Systematic Social Observation component, which characterized neighborhood block faces. These two components are not used in this paper.

The sampling of respondents to the Longitudinal Cohort Study was performed as follows: first, neighborhood clusters were stratified by racial composition and income level, forming 21 cells. Next, 80 neighborhood clusters were selected from the 343 in Chicago, using with sampling weights designed to produce equal representation across each cell. Within each of these 80 initial neighborhoods, census block groups were selected randomly, and residents living in those block groups were contacted for participation in the survey.

A.2 Neighborhood Crime Data

Data on neighborhood crime rates was originally gathered by D. Garth Taylor, and was generously shared by Michael Lens. These crime rates detail the total number of crimes reported within the census tract, per 1000 residents. These are available at a yearly frequency, and hence can capture the immediate change in neighborhood quality following incoming relocations. These crime rates are aggregated from the tract level to the neighborhood cluster level.
Appendix B Parental Involvement Choice under General Functional Forms

Consider a general utility function (28) for parental involvement choice. The assumption that utility is linear in behavior is without loss of generality: if \( b(y,q) \) were instead \( u(b(y,q)) \) for an increasing function \( y \), the first order condition would be given by \( \omega_i u_b(y,q) - c_y(y,q) = 0 \). This can be rewritten as \( \omega_i - \frac{c_y(y,q)}{u_b(y,q)} = 0 \). Hence, by redefining the cost \( c(y,q) \) as \( \int c(y,q)u_b(y,q)dy \), the maximization problem is unchanged.

\[
U_i = \omega_i b(y,q) - l(1 - s(q)y) \tag{28}
\]

The derivative of parental involvement with respect to neighborhood quality is given by (29). Because some parents respond in a compensating manner and others reinforce neighborhood change, this expression switches sign depending on \( \omega_i \).

\[
\frac{\partial y}{\partial q} = \frac{\omega_i b_{yq} - c_{yq}}{\omega_i b_{yy} - c_{yy}} \tag{29}
\]

The relevant cross-partial derivative describing this phenomenon is given by (30). It is the sum of two terms. The second term is related to the curvature of marginal productivity and marginal cost, and is difficult to sign. For now, I assume that this term is zero, and focus on the first term. Under standard assumptions (\( b_{yy} < 0 \) and \( c_{yy} > 0 \)), the negative association between initial involvement and response to neighborhood quality implies that either \( b_{yq} < 0 \) and/or \( c_{yq} \) < 0. Assume that \( b_{yq} \) and \( c_{yq} \) have consistent sign across all families. To establish a contradiction, suppose that \( b_{yq} > 0 \): in that case, \( c_{yq} \) must be negative. However, from (29), this implies that the response of parental involvement to neighborhood quality is always negative: this is at odds with the empirical findings. Ultimately, this implies that \( b_{yq} \) must be negative.

\[
\frac{\partial y}{\partial q\partial \omega_i} = \frac{b_{yq}c_{yy} - b_{yy}c_{yq}}{(\omega_i b_{yy} - c_{yy})^2} + \frac{b_y[\omega(b_{yyq} - b_{yqq}) - (c_{yyq} - c_{yqq})]}{(\omega_i b_{yy} - c_{yy})^3} \tag{30}
\]

If the second term in (30) is non-zero, then the above claim does not necessarily hold. Certain assumptions must be imposed before a conclusive statement can be made. One sufficient condition for the previous claim to hold is that \( b_{yyy} = b_{yyq} \) and, at the same time \( c_{yyq} = c_{yqq} \). This condition restricts the way neighborhood
quality affects the curvature of the productivity (cost) of parental involvement.

Supplementary Tables

Table A1: List of Raw Variables in HOME Score

<table>
<thead>
<tr>
<th>Home Environment</th>
<th>Discipline</th>
<th>Other</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age group 1</td>
<td>Age group 2</td>
<td>Age group 3</td>
</tr>
<tr>
<td>Home is not dark</td>
<td>Home is not dark</td>
<td>Home is not dark</td>
</tr>
<tr>
<td>Home is reasonably clean</td>
<td>Home is reasonably clean</td>
<td>Home is reasonably clean</td>
</tr>
<tr>
<td>Home is minimally cluttered</td>
<td>Home is minimally cluttered</td>
<td>Home is minimally cluttered</td>
</tr>
<tr>
<td>Play environment is safe (home or building for ages 36 mos +)</td>
<td>Play environment is safe (home or building for ages 36 mos +)</td>
<td>Play environment is safe (home or building for ages 36 mos +)</td>
</tr>
<tr>
<td>Home has at least 100 square feet/pace/person?</td>
<td>Home has at least 100 square feet/pace/person?</td>
<td>Home has at least 100 square feet/pace/person?</td>
</tr>
<tr>
<td>Home not too noisy from noise in house?</td>
<td>Home not too noisy from noise in house?</td>
<td>Home not too noisy from noise in house?</td>
</tr>
<tr>
<td>Home not too noisy from noise outside?</td>
<td>Home not too noisy from noise outside?</td>
<td>Home not too noisy from noise outside?</td>
</tr>
<tr>
<td>No signs of alcohol/illegal drug use?</td>
<td>No signs of alcohol/illegal drug use?</td>
<td>No signs of alcohol/illegal drug use?</td>
</tr>
<tr>
<td>Child eats meal with both mother and father/figure once a day or more</td>
<td>Child eats meal with both mother and father/figure once a day or more</td>
<td>Child eats meal with both mother and father/figure once a day or more</td>
</tr>
<tr>
<td>Child taken to museum in past year</td>
<td>Child taken to museum in past year</td>
<td>Child taken to museum in past year</td>
</tr>
<tr>
<td>Mom showed physical affection to child</td>
<td>Family visits with family or friends 2-3 times a month</td>
<td>Family visits with family or friends 2-3 times a month</td>
</tr>
<tr>
<td>Child receives lessons in sports/music/ent/dance/drama.org</td>
<td>Child receives lessons in sports/music/ent/dance/drama.org</td>
<td>Child receives lessons in sports/music/ent/dance/drama.org</td>
</tr>
<tr>
<td>Talk with child as works about house?</td>
<td>Mom encouraged child to contribute to conversation</td>
<td>Mom encouraged child to contribute to conversation</td>
</tr>
<tr>
<td>SP included in family activities</td>
<td>Mom answered child’s questions or requests verbally</td>
<td>Mom answered child’s questions or requests verbally</td>
</tr>
</tbody>
</table>

Notes:
Age group 1: Cohort 3 - Wave 1, Cohort 0 - Wave 2.
Age group 2: Cohort 6 - Wave 1, Cohort 3 - Wave 2, Cohort 0 - Wave 3.
Age group 3: Cohort 12, Cohort 9, Cohort 6 - Wave 2 & 3, Cohort 3 - Wave 3.
Shaded cells based on interviewer observation. Unshaded cells self-reported.
Exact text in questionnaire varies slightly across surveys.
Table A2: First Stage: Effect of Relocations Received on Neighborhood Crime

<table>
<thead>
<tr>
<th>Share Component</th>
<th>Concurrent</th>
<th>Historical</th>
</tr>
</thead>
<tbody>
<tr>
<td>Relocations Received:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>t-1</td>
<td>8.989*</td>
<td>8.426</td>
</tr>
<tr>
<td></td>
<td>(4.362)</td>
<td>(1007.6)</td>
</tr>
<tr>
<td>t-2</td>
<td>10.99+</td>
<td>9.033</td>
</tr>
<tr>
<td></td>
<td>(5.775)</td>
<td>(1127.9)</td>
</tr>
<tr>
<td>F statistic</td>
<td>1.588</td>
<td>0.996</td>
</tr>
<tr>
<td>Observations</td>
<td>5831</td>
<td>5831</td>
</tr>
</tbody>
</table>

Notes:
- Effects take units of standard deviations per 100 households received.
- Neighborhood: refers to study-defined Neighborhood Cluster (343 Neighborhood Clusters in Chicago).
- Relocations constructed as product of Shift Component and Share Component (Concurrent or Historical).
- Shift Component: City-wide number of public housing apartments closed, excluding receiving neighborhood.
- Historical Shares: Predicted value of Concurrent Share component, using 1990 neighborhood characteristics.

Table A3: Principal Component Weights of Parenting Behavior Measures in HOME Score

<table>
<thead>
<tr>
<th>Age Bracket</th>
<th>1</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>1+ family meal/day</td>
<td>-0.196</td>
<td>0.0551</td>
<td>0.213</td>
</tr>
<tr>
<td>Participates in child organization</td>
<td>0.0612</td>
<td>0.457</td>
<td>0.332</td>
</tr>
<tr>
<td>Brought child to museum, past year</td>
<td>-0.192</td>
<td>-0.570</td>
<td>0.213</td>
</tr>
<tr>
<td>2+ visits with family/friends per month</td>
<td>0.0701</td>
<td>0.505</td>
<td></td>
</tr>
<tr>
<td>Family Activities</td>
<td>-0.192</td>
<td>0.229</td>
<td></td>
</tr>
<tr>
<td>Owns 3 children’s books</td>
<td>0.547</td>
<td>0.313</td>
<td></td>
</tr>
<tr>
<td>Encourages child to read at home</td>
<td>0.290</td>
<td>0.477</td>
<td>0.745</td>
</tr>
<tr>
<td>3+ puzzles for sub</td>
<td>0.290</td>
<td>0.477</td>
<td>0.745</td>
</tr>
<tr>
<td>Encourages child to contribute</td>
<td>0.425</td>
<td>0.596</td>
<td>0.620</td>
</tr>
<tr>
<td>Answers child</td>
<td>0.396</td>
<td>0.341</td>
<td>0.873</td>
</tr>
<tr>
<td>Talks to child twice during visit</td>
<td>0.467</td>
<td>0.596</td>
<td></td>
</tr>
<tr>
<td>Introduces observer to child</td>
<td>0.290</td>
<td>0.477</td>
<td>0.745</td>
</tr>
<tr>
<td>Speaks positively to child</td>
<td>-0.0534</td>
<td>0.278</td>
<td></td>
</tr>
<tr>
<td>Praises child twice</td>
<td>0.547</td>
<td>0.313</td>
<td></td>
</tr>
<tr>
<td>Hugs child</td>
<td>1.054</td>
<td>0.405</td>
<td>-0.0351</td>
</tr>
<tr>
<td>Daily conversation</td>
<td>1.054</td>
<td>0.405</td>
<td>-0.0351</td>
</tr>
<tr>
<td>Physical punishment less than once/week</td>
<td>-0.182</td>
<td>0.120</td>
<td>0.161</td>
</tr>
<tr>
<td>Does not slap/spank child</td>
<td>-1.931</td>
<td>-2.807</td>
<td>-1.074</td>
</tr>
<tr>
<td>Lost temper with child less than twice, last week</td>
<td>0.0225</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Assigns chores to child</td>
<td>-1.586</td>
<td>0.141</td>
<td></td>
</tr>
<tr>
<td>Has rules for behavior with friends</td>
<td>0.205</td>
<td>0.205</td>
<td></td>
</tr>
<tr>
<td>Sets and enforces rules about homework</td>
<td>0.340</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Implements curfew, weekend nights</td>
<td>-0.162</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Supervises child in public</td>
<td>0.227</td>
<td></td>
<td></td>
</tr>
<tr>
<td>TV used judiciously</td>
<td>0.00926</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Observations: 1728 2251 5807

Notes:
- Age bracket 1: Cohort 3 - Wave 1, Cohort 0 - Wave 2.
- Age bracket 2: Cohort 6 - Wave 1, Cohort 3 - Wave 2, Cohort 0 - Wave 3.
- Age bracket 3: Cohort 12, Cohort 9, Cohort 6 - Waves 2 & 3, Cohort 3 - Wave 3.
Supplementary Figures

Figure A1: Definition of Closure (C) and Receiving (R) Neighborhood Populations

(a) Before Closures

(b) After Closures
Figure A2: Heterogeneous Effects of Relocations Received on Parental Involvement, by Initial Involvement Relative to Neighborhood

Notes:
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to $t-2$) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-values displayed for test of joint equality across all categories.
Figure A3: Heterogeneous Effects of Relocations Received on Child Externalizing Behavior, by Initial Neighborhood / Initial Involvement

Notes:
Neighborhood income of Wave 1 neighborhood measured in 1990.
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to t − 2) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-value displayed for test of joint equality across all categories.
Figure A4: Heterogeneous Effects of Relocations Received on Child Internalizing Behavior, by Initial Neighborhood / Initial Involvement

Notes:
Neighborhood income of Wave 1 neighborhood measured in 1990.
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to $t-2$) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-value displayed for test of joint equality across all categories.
Figure A5: Heterogeneous Effects of Relocations Received on Child WRAT, by Initial Neighborhood / Initial Involvement

Notes:
Neighborhood income of Wave 1 neighborhood measured in 1990.
Points denote coefficient on interaction between listed categorical variable and relocations received last year.
Controls: cumulative prior relocations (up to $t-2$) parental employment, subject fixed effect, neighborhood effect, year dummies.
Spikes indicate 90% confidence intervals.
P-value displayed for test of joint equality across all categories.


Chicago Tribune (Oct 13, 2002). On moving day, past gets packed away at Cabrini.


Wodtke, G. T., D. J. Harding, and F. Elwert (2011). Neighborhood effects in temporal perspective the impact