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ABSTRACT

Why does parenting look so different across children? While some children are disciplined with time-outs, others face physical punishment. Some children are expected to become doctors, and others are encouraged to pursue their own dreams. Such variation may reflect differences in the families to which children belong. Alternatively, parenting could be influenced by the neighborhoods in which children are raised. For instance, a parent may be more strict in a dangerous neighborhood as a method of protection. In this paper, I disentangle family and neighborhood effects and estimate bounds on their contributions to variation in parenting. The challenge in doing so lies in the presence of residential sorting. People choose where to live based on their background, and that gives rise to a correlation between family and neighborhood characteristics. I overcome this barrier to identification with a methodology that leverages sibling and neighbor covariances. This approach accommodates residential sorting but has the strict data requirement of observations for both siblings and neighbors. I fulfill these requirements with a dataset that, to my knowledge, has not yet been used by economists: the Project on Human Development in Chicago Neighborhoods. My findings indicate an average lower bound of 45% for family effects and an average upper bound of 2% for neighborhood effects across a broad set of parenting measurements. I bolster this analysis with a series of heterogeneity and robustness checks. First, I investigate whether neighborhoods matter in the extremes, such as areas characterized by high homicide or economic mobility rates. Second, I employ an alternative mover design to examine how parenting changes with residential relocation. Across all specifications, neighborhood effects are not a substantial driver of the observed variation in parenting. Instead, it is family that matters.

1

INTRODUCTION

Anyone who has ever stood in line at the grocery store knows that parents parent differently. Some will say no to the Snickers bar and offer an explanation, reminding their child about the importance of dental health. Others say no, "Because I said so," and may even accompany the one-size-fits-all explanation with a slap. Yet others will let the candy slide along with everything else dropped into the cart.

Parents make myriad decisions that govern their children's lives. Many of these choices dictate consumption, but developmental psychologists have long argued the importance of the broader nature of interactions between parents and children. For example, parents choose a level of warmth and affection, they set rules and expectations, and they take disciplinary actions. Although there is evidence that such parenting choices impact human capital development, little is known about how these decisions are made.

This paper examines the origins of parenting. I explore the extent to which observed variation in parenting can be explained by neighborhood and family effects. To do so, I use the sibling and neighbor corvariance methodology of Duncan et al. [2000], Page and Solon [2003], and Oreopoulos [2003] to identify an upper bound on neighborhood effects and a lower bound on the causal contribution of family effects.

Neighborhood effects are defined as the determinants of parenting shared by neighbors and invariant across families. For example, parenting may be a way to moderate the impact of local school quality or crime rates on the child's development.[1]

Alternatively, there may be direct neighborhood effects on parenting. Developmental

1. An extensive literature tests for neighborhood effects on child development and future labor market outcomes. The results are sensitive to specification. For reviews, see Durlauf [2014], Galster [2012] and Mogstad and Torsvik [2023]. Recent work using mover designs, such as Chetty et al. [2018] and Chetty and Hendren [2018], find evidence of neighborhood effects. Other work using the sibling and neighbor covariance methodology, such as Duncan et al. [2000], Page and Solon [2003], and Oreopoulos [2003], find no role of neighborhood effects.

psychologists have argued that different parenting choices engender different characteristics of the child, such as obedience, risk-taking propensity, and independence.[2] To the extent that there are differences across neighborhoods in the returns to different personality traits, parents may adapt their style accordingly.[3] As a last example, the costs of parenting may be localized. This could be through the diffusion process of information on how to parent, role-modeling, or the social stigma attached to certain behaviors. For instance, parents may be less inclined to employ corporal punishment in areas where such practices are viewed as child abuse and therefore more costly to implement.

Family effects are similarly defined as the determinants of parenting shared by siblings and invariant across neighborhoods. One example of a family effect may be the resources and constraints of the family that determine which parenting choices are available and the returns. For example, the returns to homework assistance may be increasing in the parent's own education. Family effects may also reflect preferences. A parent may want to raise their child as their own parents raised them, or be as different as possible. As a last example, family effects may reflect the information that a parent has on the returns to different parenting behaviors, such as through their education levels.

There is a growing economics literature that provides structural models of parenting. These models describe parenting as an optimized choice given the technology of skill formation and the constraints of the family, and they reflect the possibility of both family and neighborhood effects on parenting. Little empirical work has been done to complement this literature, and there is no paper that quantifies the overall contributions of family and neighborhood effects on parenting behaviors.[4] The key barrier has been the identification

2. See Steinberg and Blatt-Eisengart [2006] and Pinquart [2017].

3. For instance, Acemoglu [2022] suggests that obedience may be rewarded in industrial areas and penalized in regions characterized by high levels of entrepreneurship. In his model, a parent strategically interacts with their child to shape their personality to be more favorable to local labor markets.

4. Despite the lack of evidence on the origins of parenting, there has been broad policy interest in encouraging improvements to parenting. For example, the Maternal, Infant, and Early Childhood Home Visiting Program was created under the Affordable Care Act, and provided $1.5 billion in funding to expand

challenge of residential sorting. People choose where to live based on their background, and this engenders a correlation between family and neighborhood characteristics. The resulting identification challenge is the question of whether parents of a particular background are more likely to parent in a particular way because of who they are or where they live.

In this paper I overcome the identification challenge by using an identification strategy that allows for residential sorting on unobserved characteristics but at the cost of strong data requirements. The identification strategy was first proposed by Duncan et al. [2000] and Page and Solon [2003] to study the determinants of wages and educational attainment. The intuition for the strategy is simple. Consider a set of neighboring children. These children live in the same neighborhood and are likely to come from similar families, by positive residential sorting. The covariance in parenting between these children reflects the variance of the shared neighborhood effect and a positive covariance between the similar family effects. If we divide the neighbor covariance by overall variance in parenting, we therefore recover an upper bound for the share of variance in parenting explained by variance in neighborhood effects. We can similarly bound family effects with sibling covariances. Siblings live in the same neighborhood and share the same family. If we take the covariance between siblings, deduct the covariance between neighbors, and divide by the overall variance in parenting, we recover a lower bound for family effects.

This research design is attractive in that it invokes minimal assumptions and allows for residential sorting on unobserved characteristics; however, it requires a dataset that has measurements of parenting behavior for a large sample of siblings and neighbors. These requirements are not met by the standard datasets used to study household behavior, such as The National Longitudinal Study of Adolescent to Adult Health (Add Health), National Longitudinal Survey of Youth (NLSY), or Panel Study of Income Dynamics (PSID).[5] In this

evidence-based home visitation programs that role model best-practices for parenting.

5. Add Health, NLSY and PSID are each nationally-representative samples that do not explicitly cluster at the neighborhood level. The Add Health survey samples at the school level, the NLSY samples at the

paper, I introduce an alternative dataset that, to my knowledge, has not yet been leveraged by economists. The Project on Human Development in Chicago Neighborhoods (PHDCN) is a longitudinal survey dataset designed by a team of sociologists and developmental psychologists to study human development in a neighborhood context. The survey includes a wide-range of questions on parenting behaviors deemed important for developmental outcomes. The first wave samples families in over 80 neighborhoods in Chicago and includes child-specific questions so that siblings are distinct observations.

The breadth of survey questions on parenting allows me to measure parenting in a number of ways. I follow two of the predominant measurement approaches in the development psychology literature. First I examine several observed parenting practices, such as whether a child is allowed in public unsupervised. Under this approach I use both self-reported answers to survey questions and interviewer observations. Second I examine latent parenting styles that I recover by applying logistic principal component analysis to the observed measures of parenting behavior.

The results of this analysis are clear. No matter how I measure parenting, the upper bound on variance in parenting explained by neighborhood effects is small. On average, the upper bound on neighborhood effects is 2% of the observed variation in parenting.[6] Instead, my results show that family is key. On average, family effects explain a lower bound of 45% of the observed variation in parenting. Furthermore, in Section 5 I identify an additional lower bound on the share of variance in parenting explained by the error term that captures, for example, the idiosyncratic characteristics of the child. The error term is important and explains, at a minimum, 50% of the variation in observed parenting.

While neighborhoods are not important for explaining overall variation in parenting, it

metropolitan area level, and the PSID at the family level. While each of these samples does contain some neighbors, the PHDCN intentionally clusters at the neighborhood level.

6. This is a weighted average across the measurements of parenting where the weights are inverse standard errors.

is possible that certain neighborhoods have large effects for the people who live in them.[7] For example, developmental psychologists have argued that parents living in high-crime neighborhoods are more likely to exhibit corporal punishment so that their children become more obedient and less vulnerable to neighborhood crime.[8] To examine the possibility that neighborhoods have causal effects on parenting in extreme places, such as areas characterized by high or low homicide or economic mobility rates, I classify neighborhoods using data from the Chicago Police Department and the Opportunity Atlas. If neighborhoods have important causal effects in places with extreme levels of crime or mobility, then the neighbor covariance would be bigger in these areas. Yet they continue to be small relative to overall variance in parenting in these areas.

To further test the result that neighborhood effects are not important for parenting, I consider an alternative mover design. Under this design I isolate the set of families that move, and I examine the extent to which parenting changes with moves. This approach replaces the identifying assumption of positive sorting and instead assumes moves are exogenous to unobserved determinants of parenting. I again find no evidence of neighborhood effects.

The outline of the rest of the paper is as follows. Section 2 reviews the relevant literature on parenting. Section 3 describes the data sources, measurement of key outcome variables, and descriptive statistics. Section 4 establishes the presence of residential sorting, which is the key identification barrier. Section 5 presents the main research design of sibling and neighbor covariances. Section 6 discusses the estimation strategy and results. Section 7 explores heterogeneity and Section 8 estimates neighborhood effects using an alternative mover design. I conclude in Section 9.

7. To see this, recall that the R-squared of a variable may be small even though its coefficient may be sizable. For example, the effect of having a twin on family size is large, but twinning explains little of the variation in family size across families.

8. See, for instance, Horn et al. [2004].

2

LITERATURE REVIEW

In this section I review the literature on defining and measuring parenting, the evidence on the impact of parenting on human capital development, and the economic modeling of parenting.

2.1 Definition of parenting

There are several approaches to measuring parenting in the developmental psychology literature. The first is to examine observed parenting practices, such as whether the child is allowed outside unsupervised.[1] A second approach is to combine measurements of related parenting behaviors to uncover measurements of latent parenting dimensions. For example, whether a child is allowed outside unsupervised and whether the parent checks homework both speak to the dimension of control. Finally, the third approach is to uncover clusters of parenting behavior across multiple parenting dimensions which are interpreted as latent parenting styles.[2] The workhorse taxonomy of parenting styles first appeared in Baumrind [1967] and was extended by Maccoby and Martin [1983].[3] In this classification, parenting styles are defined by the interaction of the parenting dimensions of how demanding or controlling a parent is and how responsive or warm they are, as shown in the table below.

1. Darling and Steinberg [1993] and Kuppens and Ceulemans [2019].

2. Vandeleur et al. [2007] and Darling and Steinberg [1993].

3. The Baumrind classification, though old, continues to be the predominant classification system for parenting and has been documented in various contexts across time and place. See Steinberg [2015] for an overview.

Table 2.1: Parenting styles

		Responsive & Warm	
		Low	***High***
Demanding & Controlling	***Low***	Neglectful	Permissive
	High	Authoritarian	Authoritative

NOTE: In this table I describe the four parenting styles developed by Baumrind [1967] and Maccoby and Martin [1983].

A parent that exhibits high levels of both dimensions is classified as being authoritative, which is generally understood to be the style most favorable for human development.[4] The authoritative parent enforces rules, but they are affectionate and offer explanations to their child. A parent that is highly demanding but not responsive to the child is classified as being authoritarian, which is characterized by harsh punishment, minimal explanations, and coldness. A parent that is highly responsive and not demanding is permissive, and a parent that is low on both dimensions is neglectful.

A fifth parenting style, No-nonsense parenting, is characterized by high demandingness and high responsiveness, but it is distinct from authoritative parenting in that it involves more monitoring and harsher control than typical of authoritative parents.[5] This No-nonsense style, in particular, has been described as a strategic reaction to harmful neighborhood effects.[6]

In this paper, I use the first approach of observed parenting practices and include each survey question on parenting as an outcome variable. I also use the third approach and measure 5 parenting style indexes that correspond to Authoritarian, Authoritative, Neglectful, Persmissive and No-nonsense. I omit the second approach, which reduces a set of related

4. Baumrind [1991], Lamborn et al. [1991] and Steinberg et al. [1994].

5. See, for instance, Murray et al. [2001], Mason et al. [1996], Brody et al. [1999], and Simons et al. [2013].

6. See, for instance, Hill et al. [2007], Duncan and Rodgers [1988], McLoyd [1998] and Brody and Flor [1998].

parenting practices into parenting dimensions, as the data do not have a sufficient number of similar questions to make this approach valuable.

2.2 Impact of parenting on human capital development

To my knowledge, there has not been an empirical paper with plausibly exogenous variation in parenting to establish estimates of the causal effects of parenting on human capital development; however, there is a body of theoretical work that makes a neurological argument based on the importance of experience for brain development. In the first few years of life, an infant forms billions of connections between her neurons to foster the electrical transmission that allows the brain to operate.[7] Importantly, the brain functions better not when there are more connections, but when the connections are efficiently pruned.[8] This process of pruning unfolds throughout childhood and adolescence and is driven by experience.[9] With every experience, the resulting flow of electricity between specific neurons strengthens their connection. Importantly, given the age of brain plasticity, parents play a role in dictating what these experiences will be.

This neurological argument for the importance of parenting on human capital development is supported by correlations documented by developmental psychologists between parenting and developmental outcomes of the child. For example, Authoritative parents have children that are more self-reliant, have fewer behavioral problems, and do better in school as adults.[10] Yet they are also more likely to take risks and get in trouble as adolescents. The children of Authoritarian parents have lower self-esteem, less self-reliance, less

7. Steinberg [2015].

8. A typical infant has roughly twice as many connections at age 1 as she does during adulthood. See, Steinberg [2015].

9. Steinberg [2015].

10. Kuppens and Ceulemans [2019].

persistence, and do worse as adults.[11] They are also less likely to take risks and get in trouble as adolescents.

This illustrates an example of the potential economic trade-offs to parenting. Authoritative parenting, relative to Authoritarian, may increase the odds of success as an adult, but at the cost of safety during adolescence. How a parent resolves this trade-off is likely to reflect family and neighborhood characteristics.

2.3 Economic models of parenting

Economists have long recognized parenting as a choice problem well-suited for economic analysis. For instance, Becker [1981] modeled parents as making rational choices to maximize their utility given the cost and benefits of parenting options. The most closely related model for this paper is that of Doepke and Zilibotti [2017]. They develop a model of the choice of Baumrind [1967] parenting styles, where parenting styles are operationalized as the decision of whether and how to resolve disagreements with the child. A parent that does not interfere with their child's choices is Permissive; A parent that directly restricts their child's choices is Authoritarian; and a parent that attempts to influence the child's preferences is Authoritative.[12] In this model the choice of parenting is a function of the technology of skill formation,[13] the neighborhood environment, and other resources of the family.[14] Doepke and Zilibotti [2019] provide empirical evidence of a relationship between the choice of parenting

11. Pinquart [2017].

12. Note that Baumrind [1967] included only three parenting styles. Her framework was updated by Maccoby and Martin [1983] to include the Neglectful parenting style and I further include the No-nonsense style of Horn et al. [2004] in my analysis.

13. See Cunha and Heckman [2007], Cunha and Heckman [2008], and Cunha et al. [2010] for work characterizing the human capital production function. Note that while cognitive skills are formed in the early years of childhood, non-cognitive skills are both plastic throughout childhood and adolescence and important determinants of wages. See, Heckman et al. [2006], and Heckman et al. [2013].

14. For a related discussion of how family endowments, in general, shape the child's future labor market outcomes, see the reviews by Heckman and Mosso [2014] and Attanasio [2015].

and environmental characteristics, such as inequality.[15] Recent work by Agostinelli et al. [2020b] applies the model of Doepke and Zilibotti [2017] to model parenting as a function of neighborhood peer quality, which they operationalize as classmates.

There are alternative models in this literature.[16] Weinberg [2001] also models parenting as a tool to resolve disagreement with the child and Authoritarian parenting is the method of forcing a child's compliance. In their model, compliance under an Authoritative parenting regime would require providing the child additional monetary incentives. They use this model to explain the pattern in which the prevalence of corporal punishment decreases with parental income. There are alternative concepts of parenting in this literature. One example is Lizzeri and Siniscalchi [2008] who model parenting as the decision of whether to let your child learn from experience, which comes at the cost of mistakes, or to make choices for your child, which comes at the cost of the child not acquiring wisdom.

These papers articulate mechanisms through which family and neighborhood effects might impact parenting. The contribution of this paper is empirical causal evidence on the overall importance of family effects and neighborhood effects in explaining observed variation in parenting.

15. Doepke and Zilibotti [2019] establish a correlation between inequality over time and more intensive parenting. They interpret this pattern as reflecting the strategic response of parenting to inequality.

16. For a comprehensive review of the economics of parenting, see Doepke et al. [2019].

3

DATA, MEASUREMENT, AND DESCRIPTIVE STATISTICS

In this section I introduce the data, discuss the construction of key variables, and review descriptive statistics.

3.1 Data

The data on parenting, family background, and neighborhood location are reported in the Longitudinal Cohort Study (LCS) of the Project on Human Development in Chicago Neighborhoods (PHDCN). The PHDCN is a large-scale study directed by the Harvard School of Public Health to examine child development in a neighborhood context.[1] The LCS component is a longitudinal survey of approximately 6,000 children and their primary caregivers in Chicago during the mid-1990s to early 2000s. Families are interviewed in three waves spaced approximately 2 years apart. Neighborhoods in the PHDCN are constructed by the survey designers to be similar and adjacent sets of 2-3 Census Tracts. Each neighborhood is classified by the racial composition and socioeconomic status of the area, and neighborhoods are sampled from each strata of the intersection of the two neighborhood classifications. For example, medium SES and 70%+ white. Within each neighborhood, the surveyors sampled a set of blocks and attempted to interview all families living on the block. At the onset of the survey, all children were classified into cohorts based on their age at wave 1. Within a family, children in the 0, 3, 6, 9, 12, 15 or 18 age cohorts were included in the interview so that we have some, but not all, siblings observations.[2] In waves 2 and 3, the same children

1. The Principal Investigator is Felton J. Earls, M.D., Harvard Medical School (Emeritus), and the Scientific Directors are Jeanne Brooks-Gunn, Ph.D., Teachers College, Center for the Study of Children and Families, Columbia University; Stephen Raudenbush, Ed.D., Department of Sociology, The University of Chicago; Robert J. Sampson, Ph.D., Department of Sociology, Harvard University.

2. Note that cohort 0 is children that were in utero during wave 1. In wave 1, parents were asked about behaviors likely to affect those children in utero.

were included and continued to be labeled by their wave 1 age cohort. If a family moved, the surveyors would attempt to conduct the follow-up interviews, even if the move was out of the original sample of neighborhoods.

There are three characteristics of the LCS data that unlock the identification strategy of this paper. First, there is a wide set of survey questions on parenting as deemed important by the developmental psychologists who wrote the survey instruments. Second, the surveyors interviewed sets of siblings. Third, there are a large number of families sampled within each neighborhood.

I supplement the LCS data with incident-level data on homicides from the Chicago Police Department (CPD) that are reported at the street level. Lastly, I merge in estimates from the Opportunity Atlas of economic mobility at the Census Tract level.

3.2 Measurement of parenting

I measure parenting using two of the predominant approaches in developmental psychology discussed in Section 2, observed parenting practices and latent parenting styles. To implement the first approach of observed parenting practices, I isolate the set of child-specific survey questions in the PHDCN LCS. This includes both self-reported parenting behaviors and behaviors observed by the interviewer. For example, I see both whether the parent reports frequent physical punishment and whether the interviewer observes the parent physically punishing the child. Since I will be leveraging the panel structure of the dataset, I next restrict the survey questions to the subset that appears in all three waves. Finally, I restrict the set of survey questions to those that are asked of at least two cohorts since siblings are rarely in the same age cohort. I am left with 18 survey questions that are each described in the Appendix A.

To implement the second measurement approach, latent parenting styles, I apply logistic principal components analysis to reduce the dimensionality of the data into 5 principal components.[3] I find that the recovered principal components validate the 5 parenting styles described in Section 2. I label each principal component with the relevant parenting style and interpret the score of each principal component as the measurement of the intensity of the parenting style. See Appendix A for details on this process.

I adjust each of the 23 resulting outcome variables to account for age and gender profiles of the child as well as learning effects that operate through birth order. I do this to be able to better compare siblings and neighbors of different ages and genders. Take, for instance, a pair of siblings where one is 9 years old and the other is 15 years old. Instead of comparing the raw parenting behaviors, such as whether they are allowed outside, I compare the parenting behaviors relative to norms for that age, gender and birth order.[4] To account for differences in the wording of questions and answer choices across survey waves, I also include wave fixed-effects.[5]

3.3 Measurement of neighborhood characteristics

To estimate neighborhood homicide rates, I aggregate the incident level CPD data to the Census Tract level and calculate the homicide rate using census data on 1990 population levels. I estimate the homicide rate for each PHDCN LCS neighborhood, which contains 2-3 Census Tracts, as the population weighted average of the Census Tract level homicide rate. I then rank all neighborhoods in Chicago, include ones not in the PHDCN sample, and I classify neighborhoods as being low or high crime based on whether the neighborhood falls in the bottom or top 25 percentile of homicide rates across all neighborhoods in Chicago.

To estimate neighborhood economic mobility rates, I calculate the population-weighted

3. I set the number of clusters to match the 5 parenting styles in the developmental psychology literature.

4. I regress each outcome variable on a quadratic function of the age of the child and dummy variables for the sex of the child and whether the child is the first-born. The PHDCN does not track birth order beyond the first born.

5. For binary outcome variables, I estimate a logit model and recover the response residuals. For continuous parenting variables I estimate a linear model with Ordinary Least Squares and recover the residuals.

average of the Census Tract level predicted income from the Opportunity Atlas. I use the measurement of predicted income that is the estimate of household income at age 35 for children of low income parents, all races, and all genders.[6] I then rank all neighborhoods in Chicago, including ones not in the PHDCN sample, and I classify low-opportunity neighborhoods as those in the bottom 25 percentile and high-opportunity neighborhood as those in the top 25 percentile.[7]

3.4 Descriptive statistics

In Table 3.1, I characterize the sample of children that have at least one non-missing value across the set of 23 outcome variables.[8] Attrition in the sample shows up in the decreasing number of observations across each wave. Since new families are not added over time, the mean age of interviewed children increases and the share of first-borns decreases as children age out of the sample. In Table 3.1 also report on characteristics of the parent. Finally I report on the neighborhood characteristics.[9]

6. Low income is defined as being in the bottom 25th percentile. These estimates are based on data for children born between 1978 and 1983.

7. The threshold for the bottom quartile is $20,418 and the threshold for the top quartile is $33,545.

8. Some individuals drop out of the survey over time and some questions do not appear in all three waves. There are also data completion issues within each wave, as not all questions are asked across birth cohorts. As a result, the sample differs across outcome variables.

9. Since I classify neighborhoods based on the distribution of all neighborhoods in Chicago, the share of neighborhoods classified in the top or bottom 25th percentile are not mechanically 5%.

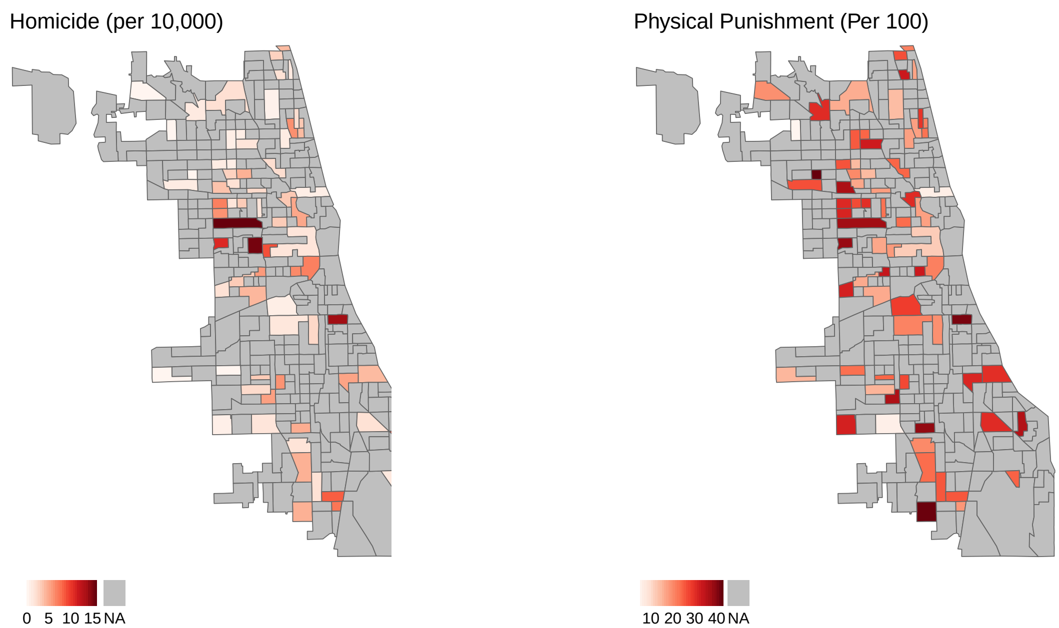
Table 3.1: Descriptive statistics for children, parents, and neighborhoods in the PHDCN LCS Sample

	Waves		
	1	**2**	**3**
	1994-97	**1997-99**	**2000-01**
Observations	4,177	3,486	2,164
Characteristics of Child			
Age	8.1	8.6	10.0
	(4.2)	(5.0)	(4.1)
Female	50%	50%	50%
First-born	42%	40%	37%
Number of siblings	2.4	NA	NA
	(2.0)		
Characteristics of Parents			
Age	35.7	37.1	38.8
	(8.5)	(8.6)	(8.2)
Female	92%	94%	96%
Single mom	28%	25%	25%
Ethnicity			
White	16%	17%	18%
Black	34%	28%	18%
Hispanic	46%	45%	44%
Other	4%	10%	9%
Education			
BA +	9%	12%	11%
Employment			
Employed	52%	60%	65%
Total HH Income			
LT 10k	22%	18%	13%
10-20k	21%	18%	15%
20-30k	19%	18%	18%
30-40k	14%	13%	14%
40-50k	9%	10%	12%
MT 50k	16%	23%	29%
Characteristics of Neighborhood			
Opportunity			
Top quartile	14%	16%	18%
Bottom quartile	12%	10%	11%
Homicide Rate			
Top quartile	22%	16%	21%
Bottom quartile	13%	19%	20%

NOTE: In this table I report average characteristics of children, parents (primary caregivers), and neighborhoods in the PHDCN LCS sample. Data on the characteristics of the child and parents are from the PHDCN LCS. Characteristics of the neighborhood are calculated from case-level homicide data provided by the Chicago Police Department and tract-level estimates of economic mobility from the Opportunity Atlas. An NA indicates the characteristic was not reported. Standard deviations are reported in parenthesis.

In Figure 1 I display the map of neighborhoods sampled in the PHDCN LCS, and I give one example of the empirical relationship between neighborhood characteristics and parenting. In the first panel, I shade neighborhoods by the homicide rates and in the second panel I shade by the rates of physical punishment. Neighborhoods with high crime rates are neighborhoods where children experience high levels of physical punishment. However, neighborhoods with high crime rates are also neighborhoods with different types of families. In the next section, I describe a method to separately establish the causal impact of neighborhoods and families on parenting.

Figure 1: Map of neighborhoods sampled in PHDCN LCS and rates of homicide and physical punishment



Note: In this figure I display neighborhoods in Chicago as defined by the PHDCN LCS. A gray filling indicates that the neighborhood is not in the wave 1 sample. In the left panel, the shading corresponds to the average annual homicide rate in the two years prior to the start of wave 1, 1993-1994. In the right panel, the shading reflects the average rate of physical punishment as reported in wave 1 of the PHDCN LCS survey.

4

IDENTIFICATION CHALLENGE

In this section I provide evidence on the presence of residential sorting, the key identification challenge the research design accommodates. I first estimate the propensity score for each parenting outcome, such as whether the child is allowed outside unsupervised, as a function of family background.[1] I regress parenting on ethnicity, total household income, whether the parent has obtained a Bachelor's degree, whether the parent is a single mom, and a quadratic in the number of siblings.[2]

I then examine the extent to which the parenting propensity scores, PS, cluster within neighborhoods by estimating the variance decomposition between and within neighborhoods.

$$Var(PS) = \underbrace{Var(\mathbb{E}[PS|N])}_{Between} + \underbrace{\mathbb{E}[Var(PS|N)]}_{Within}$$

The between component recovers the variance in parenting, as predicted by family, that can be explained by differences between neighborhoods.[3] The larger the between share, the more that parenting, as predicted by family background, clusters at the neighborhood level.

In Figure 2, I plot the between component as a share of total variance. I first focus on observed parenting practices. The first 8 parenting variables reflect self-reported parenting behaviors. The remaining 10 reflect interviewer observations. Across these measurements 31% of the variation in parenting, as predicted by family background, is explained by differences between neighborhoods. In Figure 3, I plot the shares for the latent parenting styles, which are the principal component scores. Across these measurements, 26% of the variation is explained by differences between neighborhoods, though there is more variation across

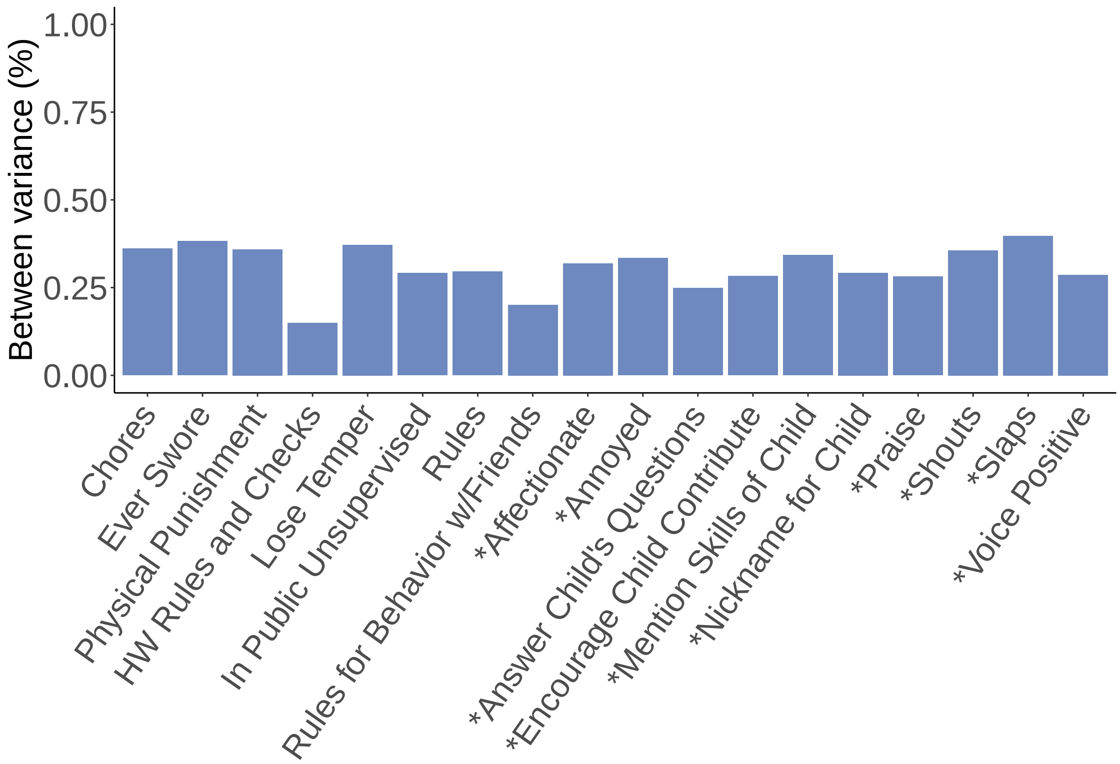
1. I use a logit model for binary parenting outcomes and OLS for continuous parenting outcomes.

2. Since some of these variables are only measured in wave 1, I restrict the data to wave 1 for this analysis.

3. The within component recovers the variance in parenting, as predicted by family, that can be explained by differences within neighborhoods.

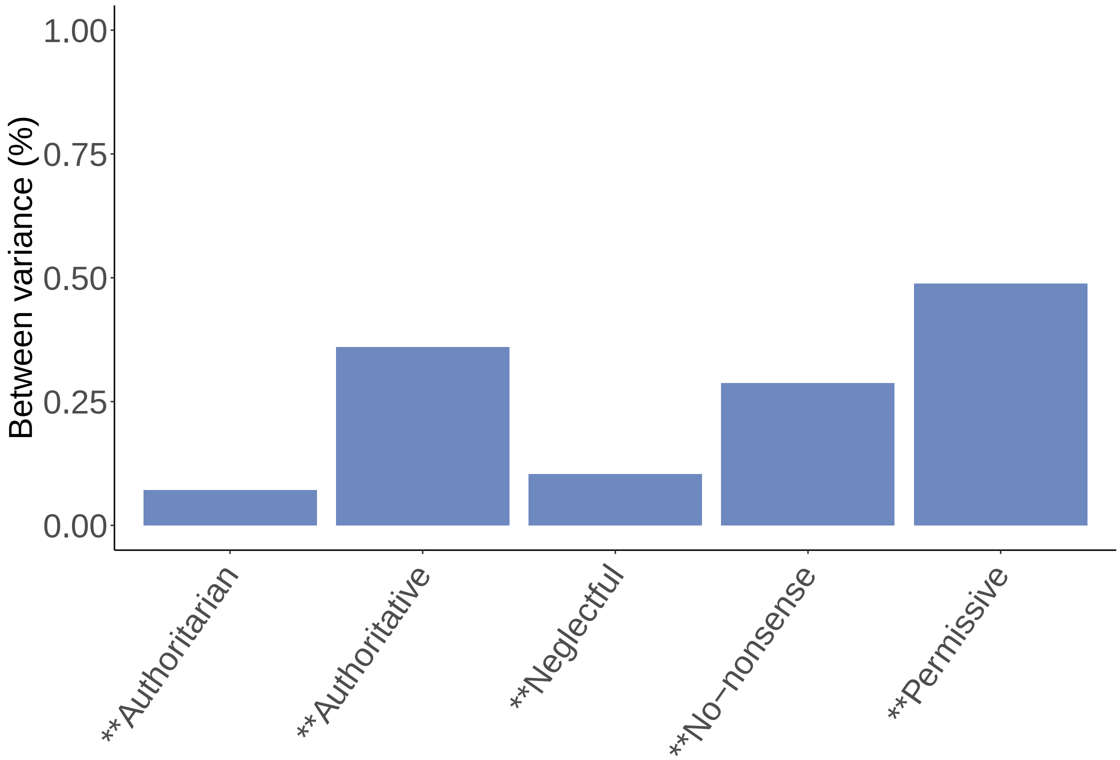
parenting outcome variables.

Figure 2: Share of variance in propensity scores for parenting explained by differences between neighborhoods: Observed parenting behaviors



NOTE: In this figure I report the share of variance in parenting propensity scores explained by differences between neighborhoods, $\frac{Var(\mathbb{E}[PS|N]),}{Var(PS)}$. Parenting propensity scores are estimated using data on the family background characteristics of ethnicity, total household income, whether the parent has at least a Bachelor's degree, whether the parent is a single mom, and a quadratic in the number of siblings. This figure reports the results for observed parenting, which includes self-reported and interviewer-observed parenting behaviors. An * denotes an interviewer observation.

Figure 3: Share of variance in propensity scores for parenting explained by differences between neighborhoods: Latent parenting styles



Note: In this figure I report the share of variance in parenting propensity scores explained by differences between neighborhoods, $\frac{Var(\mathbb{E}[PS|N]),}{Var(PS)}$. Parenting propensity scores are estimated using data on the family background characteristics of ethnicity, total household income, whether the parent has obtained a Bachelor's degree, whether the parent is a single mom, and a quadratic in the number of siblings. This figure reports the results for latent parenting styles, which are the scores for the 5 principal components recovered by logistic principal component analysis.

These figures show a high level of residential sorting in that family and neighborhood characteristics overlap in their predictive power of parenting. If any of the correlated family and neighborhood characteristics cause parenting, then residential sorting poses an identification challenge. While I could attempt to address this issue by controlling for observable characteristics, the strength of residential sorting on observable characteristics is suggestive of sorting on unobservables. Instead, I implement an identification strategy that allows for sorting on both observable and unobservable characteristics.

5

RESEARCH DESIGN

In this section, I introduce a model of parenting and demonstrate the identification challenge posed by residential sorting.

To articulate the parameters of interest, I present a regression model that links parenting outcome P for child i to her family membership, f, and her neighborhood membership, n.[1]

$$P_{ifn} = \alpha_{f(i)} + \beta_{n(i)} + u_{ifn}$$

The family fixed effect, $\alpha_{f(i)}$, captures the impact of being a child in family f. This family effect is invariant across siblings and neighborhoods, and it represents the shared impact of all observable and unobservable characteristics of the family that cause parenting. For example, it may reflect background characteristics of the parent, such as education, as well as characteristics of the broader family environment, such as the number of siblings. The neighborhood fixed effect, $\beta_{n(i)}$, similarly reflects the impact of being a child in neighborhood n, and it is invariant across neighbors. It reflects the shared impact of all observable and unobservable characteristics of the neighborhood, such as the crime level or school quality. The error term u_{ifn} captures all remaining idiosyncratic determinants of parenting, such as the characteristics of the child.

I follow Duncan et al. [2000] and assume a linear model. This is standard in the literature on neighborhood effects, but it excludes the possibility that the impact of a neighborhood on parenting depends on the family or child's characteristics and vice versa.[2] This assumption would be violated if, for instance, the impact of neighborhood crime levels on parenting

1. Note, in the empirical section, I specify the parent to be the primary caregiver. For the majority of children in my data, this is the biological mother. While there is a large body of work emphasizing the distinct roles of parents, these dynamics are beyond the scope of this paper.

2. See Durlauf [2014].

depends on the parent's race. I defend this assumption by testing for specific interactions between observable characteristics of the family or child and the overall neighborhood effects in Section 7.

I could estimate this model through Ordinary Least Squares (OLS) in two ways. The first approach would be to assume all components of the family and neighborhood effects are observed. I would then replace $\alpha_{f(i)}$ with $\alpha' X_{f(i)}$ and $\beta_{n(i)}$ with $\beta' Z_{n(i)}$, where α and β would be vectors of coefficients and X and Z would be vectors of observed characteristics. However, it would be unreasonable to assume away unobservable characteristics of family and neighborhood that determine parenting given that we do not have a good understanding of what those determinants are, and many of the candidates are hard to measure, such as parental preferences. If we restrict X and Z to the characteristics we can measure, then our coefficients would likely suffer from omitted variable bias. This is because families self-select into neighborhoods so that residential choices are likely to be endogenous to family background. Any unobserved relationship between neighborhood choice and unobserved heterogeneity in family background would be captured by u_{ifn} so that $\mathbb{E}[u_{ifn}|\alpha_{f(i)}, \beta_{n(i)}] \neq 0$.[3]

A second approach to estimating this model with OLS would be through the inclusion of fixed effects for each family and neighborhood.[4] This approach would be similar to recent and influential work, Chetty et al. [2018] and Chetty and Hendren [2018], estimating individual neighborhood effects through mover designs. The drawback to this approach is that there is large statistical uncertainty due to the limited mobility of families between neighborhoods.[5]

Instead I apply the research design of Duncan et al. [2000]. The key insight of Duncan

3. See Heckman [2001], Manski [1995], and Durlauf [2014] for excellent discussions of this selection bias and how it prohibits identification through linear regression.

4. Abowd et al. [1999].

5. Note, also, that this approach would require a stronger assumption on the exogeneity of moves, for instance, the Chetty et al. [2018] exposure design assumes that selection effects due to unobserved residential sorting are independent of child's age when the family moves.

et al. [2000] is that we can allow for sorting on unobservable determinants of parenting and still recover bounds on $Var(\alpha_f)$ and $Var(\beta_n)$ from observable data. These parameters tell us how much of the overall variation in parenting is due to variation in neighborhood effects and how much is due to variation in family effects. To recover these parameters, I impose two additional assumptions.

First, I assume $\mathbb{E}[u_{ifn}|\alpha_f, \beta_n] = 0$. Since α_f and β_n include the effect of unobservable characteristics, there are no omitted variables by construction. However, I am assuming away the existence of individual-specific effects that depend on family or neighborhood characteristics. This would be violated if, for instance, the child's susceptibility to peer-influence affects parenting differently in high versus low-crime areas. The second assumption is positive sorting. Under positive sorting, neighbors have similar family effects and the neighborhoods that promote a particular aspect of parenting behavior attract families that promote that same aspect.

Under these assumptions I can recover bounds on the share of variance in parenting caused by neighborhood and family effects.

$$\text{Neighborhood contribution: } \frac{Var(\beta_n)}{Var(P)} \tag{5.1}$$

$$\text{Family contribution: } \frac{Var(\alpha_f)}{Var(P)} \tag{5.2}$$

Notice that the denominators are observed, but the numerators are not. However, Duncan et al. [2000] show that an upper bound on $Var(\beta_n)$ is identified by the observable covariance in the outcome variable between neighbors. Similarly, a lower bound on $Var(\alpha_f)$ is identified by the covariance between siblings less the covariance between neighbors.

Though Duncan et al. [2000] studied different outcome variables, the intuition carries over and is simple. I have modeled parenting as a function of family, neighborhood, and idiosyncratic effects. This implies that the parenting behaviors of neighbors will be similar due to sharing not only the same neighborhood effect, but also similar family effects, by way of residential sorting. The neighbor covariance is thus an upper bound on neighbor effects. The parenting behavior toward siblings will also be similar as siblings share the same neighborhood effects and the same family effects. If we subtract the neighborhood covariance from the sibling covariance, we are subtracting the upper bound on neighborhood effects from the combination of neighbor and family effects. This leaves us with a lower bound on family effects.

To see this more formally, consider the covariance in parenting between two neighbors,

$$\eta := \mathrm{Cov}(P_{ifn}, P_{i'f'n}) = \overbrace{\mathrm{Var}(\beta_n)}^{Target} + \overbrace{\mathrm{Cov}(\alpha_f, \alpha_{f'})}^{Sorting>0} + \overbrace{2\mathrm{Cov}(\alpha_f, \beta_n)}^{Sorting>0}$$

The first term is the numerator of equation 5.1, the neighborhood contribution to variance in parenting. The second and third terms reflect sorting, which we assumed to be positive, so that the neighbor covariance identifies an upper bound for $\mathrm{Var}(\beta_n)$.

We can similarly identify a lower bound for the numerator of the family contribution, $\mathrm{Var}(\alpha_f)$, starting with the sibling covariance.

$$\Theta := \mathrm{Cov}(P_{ifn}, P_{i'fn}) = \overbrace{\mathrm{Var}(\alpha_f)}^{Target} + \mathrm{Var}(\beta_n) + \overbrace{2\mathrm{Cov}(\alpha_f, \beta_n)}^{Sorting>0}$$

Notice that Θ, on its own, is an upper bound for the family effects. To get a lower bound for family effects I subtract the neighbor covariance. This is the numerator of equation 5.2.

$$\Theta - \eta = \mathrm{Cov}(P_{ifn}, P_{i'fn}) - \mathrm{Cov}(P_{ifn}, P_{i'f'n}) = \overbrace{Var(\alpha_f)}^{Target} - \overbrace{Cov(\alpha_f, \alpha_{f'})}^{Sorting>0}$$

I also identify a lower bound on the role of idiosyncratic effects that reflect, for example, the behavior or characteristics of the child. Note that $1 = \frac{Var(\alpha_f)}{Var(P)} + \frac{Var(\beta_n)}{Var(P)} + \frac{Var(u_{ifn})}{Var(P)} +$

$2\frac{Cov(\alpha_f,\beta_n)}{Var(P)}$. If we replace $Var(\alpha_f)$ with the upper bound, θ, and $Var(\beta_n)$ with the upper bound η, we get the following relationship.[6]

$$1 - \frac{\Theta}{Var(P)} - \frac{\eta}{Var(P)} \leq \frac{Var(u_{ifn})}{Var(P)} + 2\frac{Cov(\alpha_f, \beta_n)}{Var(P)}$$

The left hand side is a lower bound for the combined effect of idiosyncrasies and sorting. However, notice that the covariance between neighbors serves also an upper bound on $2Cov(\alpha_f, \beta_n)$ so we can subtract it from both sides and recover a lower bound on idiosyncratic effects.

$$1 - \frac{\Theta}{Var(P)} - 2\frac{\eta}{Var(P)} \leq \frac{Var(u_{ifn})}{Var(P)}$$

In summary, we can bound neighborhood effects between 0 and η, we can bound family effects between $\theta - \eta$ and θ, and we can bound idiosyncratic effects between $1 - \frac{\Theta}{Var(P)} - 2\frac{\eta}{Var(P)}$ and 1.

6. Recall, $\theta - \eta$ is a lower bound, but θ is an upper bound for family effects

6

ESTIMATION

The PHDCN LCS dataset has an unbalanced panel structure where each child is observed up to three times across seven years. This feature provides an opportunity to measure a particular parenting outcome in numerous ways. One option is to include each observation, up to three per child, so as to maximize the sample size; however, the idiosyncratic characteristics of each child, as captured by the error term, are likely to be serially correlated. This approach would therefore likely violate the assumption that the data are drawn independently from an identical distribution (i.i.d), which is invoked when estimating population covariances from the sample. Instead, for the main specification, I estimate sibling and neighbor covariances using data from the single wave that contains the highest number of observations, wave one.

Let P_{ifn} denote a residualized parenting variable for child i in family f and neighborhood n.[1] Since P_{ifn} is residualized, it is centered at zero, so that I can estimate the sample variance σ^2 with the sample mean square.

$$\hat{\sigma}^2 = \frac{\sum_{n=1}^{N} \sum_{f=1}^{F_n} \sum_{i=1}^{I_f} P_{ifn}^2}{I}$$

In this notation, N is the total number of neighborhoods, F_n is the number of families in the neighborhood n and I_f is the number of siblings within the family f. I is the total number of children.

If each family had the same number of interviewed siblings, I would similarly estimate the sibling and neighbor covariances with sample counterparts. However, families and neighborhoods vary in size so that a more efficient approach is to weight families and neighbors in proportion to their size.[2] I follow the approach of Duncan et al. [2000], Page and Solon

1. I adjust each measurement of parenting by regressing it on a quadratic function of age, the sex of the child, and whether the child is first-born.

2. See Karlin et al. [1991] and Page and Solon [2003] for simulation results for different weighting schemes.

[2003], and Karlin et al. [1991], and I estimate the sibling covariance by first estimating the unweighted sibling covariance within a given family. To do this, I stack all pairs of observations for unique sibling pairs and take the average product. In notation, the family-specific covariance is estimated as $S_f := \sum_{i \neq i'} P_{ifn} P_{i'fn} / I_{ff}$, where I_{ff} is the unique set of pairs of siblings within family f. I then estimate the overall sibling correlation by averaging the family-specific estimates where I weight families in proportion to their size.[3]

$$\hat{\Theta} = \frac{\sum_{n=1}^{N} \sum_{f=1}^{F_n} W_f S_f}{\sum_{n=1}^{N} \sum_{f=1}^{F_n} W_f}$$

Each family is weighted by the square root of the number of unique sibling pairs, $W_f = \sqrt{I_{ff}}$.

To estimate the neighbor covariance, I use the same strategy and first estimate the sample covariance between the children in two neighboring families: $N_{ff'n} := \frac{\sum_{i=1}^{I_f} \sum_{i'=1}^{I_{f'}} P_{ifn} P_{i'f'n}}{I_{ff'n}}$ where $I_{ff'n}$ is the number of unique pairs of neighbors between family f and f' in neighborhood n. I then calculate an overall estimate of the neighbor correlation by calculating the weighted average across pairs of families and neighborhoods.

$$\hat{\eta} = \frac{\sum_{n=1}^{N} W_n \sum_{f \neq f'} W_{ff'n} N_{ff'n}}{\sum_{n=1}^{N} W_n \sum_{f \neq f'} W_{ff'n}}$$

I weight pairs of families within a neighborhood as the square root of the number of distinct neighbor pairs. $W_{ff'n} = \sqrt{I_{ff'n}}$, and I weight neighborhoods by the sum of contained weights, $W_n = \sum_{f \neq f'} W_{ff'n}$.

One complication of this estimation approach is that sibling covariances are estimated from a different sample than the variance. The sibling covariance is calculated from the set of children with interviewed siblings, whereas the variance is calculated from all children. In the Appendix B.3, I include a robustness check where I restrict all samples to be the same by

3. I depart from Page and Solon [2003] here in that they aggregate through neighborhoods and weight family covariance estimates differently across neighborhoods of different sizes.

estimating the neighbor covariance and variance using the set of children with interviewed siblings. This does not substantively affect the results.

To estimate standard errors, I bootstrap at the neighborhood level and run the full sequential estimation process 500 times, starting from residualizing the parenting variables. Notice that I do not bootstrap at the family level since the resulting sample would include repetitions of families as neighbors. This would defeat the research design as I would be calculating neighbor correlations from siblings.

I report my main results in Table B.1. Each row corresponds to a different measurement of parenting behavior. Column a reports the overall variance of the parenting behavior, columns b and c report the estimated neighbor and sibling covariances. I use these components to estimate the upper bound on neighborhood effects, in column d, and the lower bound on family effects, column e. The lower bound on the idiosyncratic component is reported in column f. For a clearer discussion, I represent the main results from column d and e graphically in Figure 4.

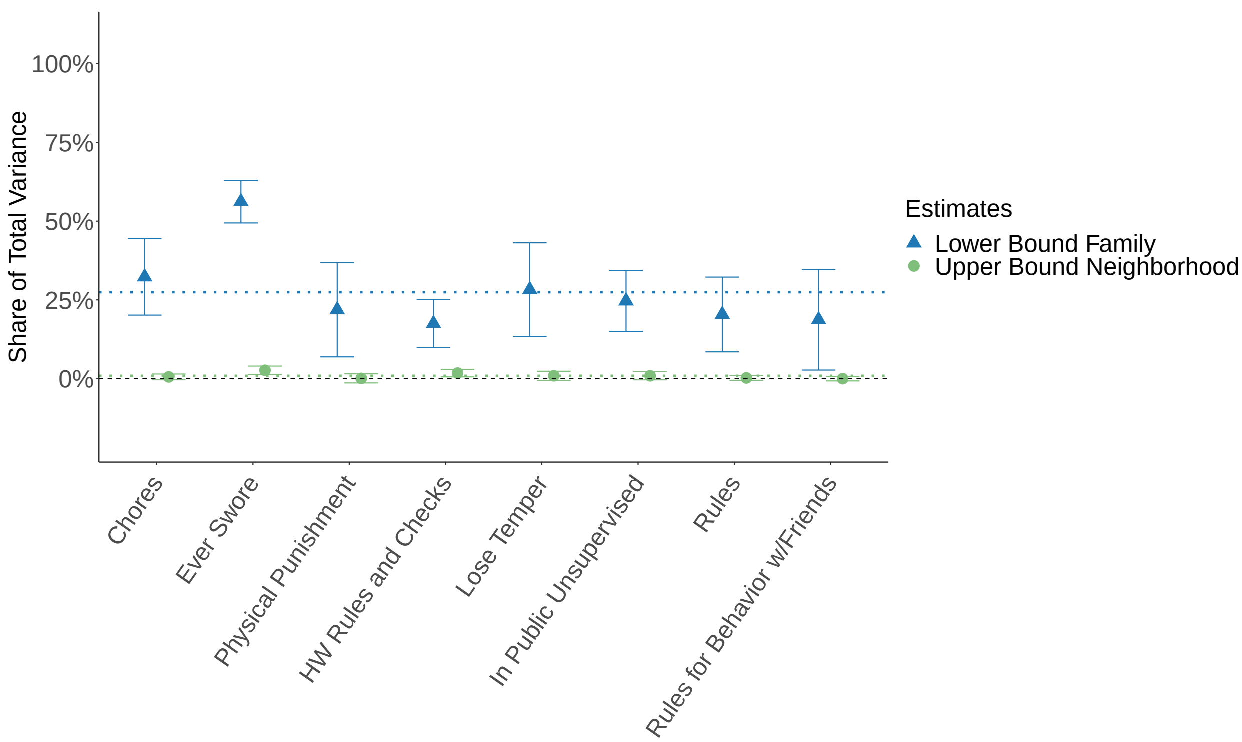
In Figure 4, I plot the upper bound on neighborhood effects and the lower bound on family effects for all self-reported parenting behaviors. Since the estimates of the upper bound are close to zero, with tight bounds, I can rule out the importance of neighborhood effects on variation in parenting.

This result is striking and remarkable. In Figure 1, I show that rates of high physical punishment vary across neighborhoods. Yet in Figure 4 I show that the differences between neighborhoods pale in comparison to the differences between families within a neighborhood. This result is not limited to physical punishment. Neighborhood effects are null across the wide range of self-reported parenting behaviors that cover the three hypothesized mechanisms for neighborhood effects. For example, homework rules and checks are likely to be a moderator to school quality, which varies drastically across Chicago neighborhoods. The stigma associated with physical punishment is likely to vary across neighborhoods as well.

Whether a child is allowed outside without supervision is connected to the development of independence, and that is likely to be rewarded differently across the local labor markets of Chicago. Yet neighborhood effects do not matter for explaining the variance of any of these parenting behaviors.

Instead it is family effects that matter. The estimated lower bound on family effects varies across parenting behaviors but is large, with an average of accounting for about 25% of the total observed variation. It's who your family is, not where you live that determines parenting.

Figure 4: Share of variance in parenting explained by family and neighborhood effects: Self-reported parenting behaviors



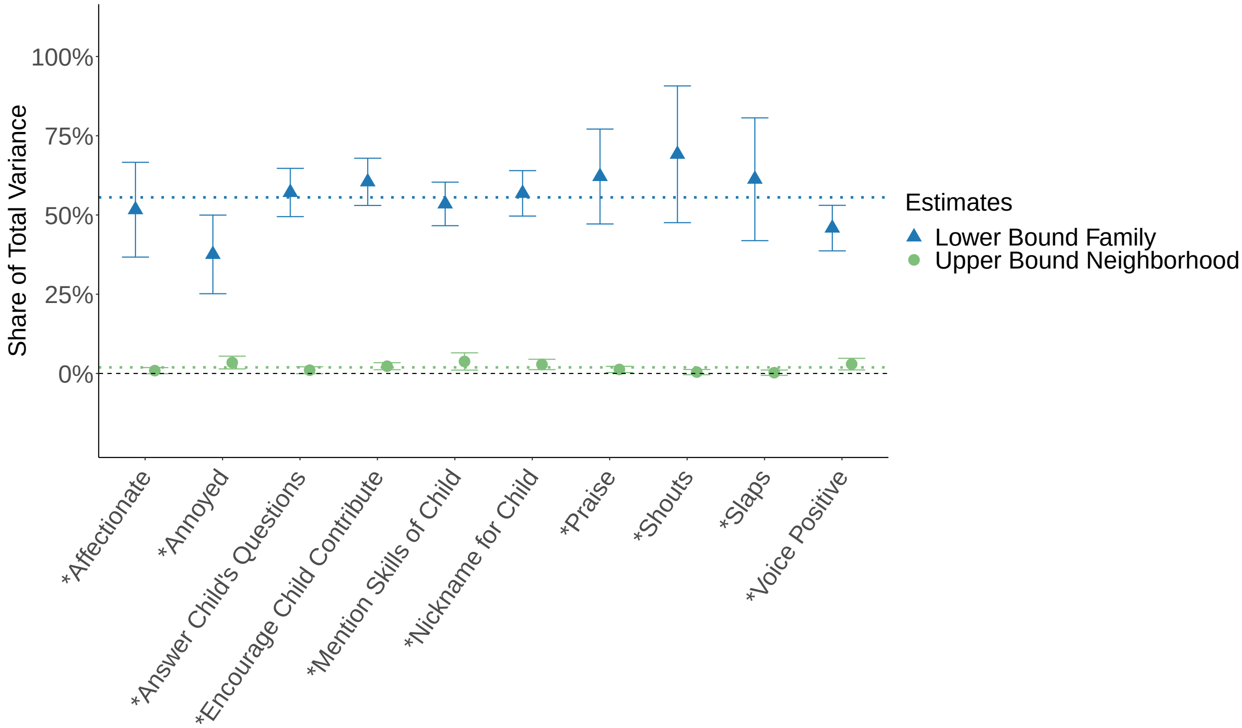
NOTE: In this figure I report the share of variance in parenting explained by causal family and neighborhood effects for self-reported parenting behaviors. The blue points represent the lower bound on family effects, and the green points represent the upper bound on neighborhood effects. The blue and green dashed lines reflects the respective weighted averages across self-reported parenting behaviors. I weight by statistical precision using the inverse of standard errors. The average lower bound on family effects is 27.5% and the average upper bound on neighborhood effects is 0.9%.

One issue with self-reports of parenting behavior is that there may be measurement error that artificially inflates the sibling correlation. For example, parents may have limited attention that makes them more likely to choose the same answer across siblings. Parents may also have a tendency to over and under exaggerate behavior, or they may have preferences on what to share with the interviewer. To address this issue, I hone in on the set of interviewer observations of parenting behavior.[4] For example, did the interviewer observe the parent slapping the child over the course of the interview. To the extent that the interviewer is better trained or incentivized to report on each child separately, this would ameliorate such measurement error.

In Figure 5, I report these estimates and find that neighborhood effects continue to be tightly bounded near zero. However, interviewer observations dramatically increase the lower bound for family effects. This is the opposite direction of what we would expect under the measurement error story where parents exaggerate the similarity between siblings. This suggests that honing in on interviewer observations may capture a different type of parenting behavior. The self-reported behavior captured in the previous figure included questions on behavior that would be hard for an interviewer to observe, especially in a short period of time. For example, the interviewers would be unlikely to reliably observe chores, homework rules, and the behavior of the parent when facing conflict with the child. While interviewers report if they observe the child being slapped, this variable may be a noisy measurement of whether the parent deploys corporal punishment in general if such behavior is triggered by infrequent conflict unlikely to be observed during the interview.

4. Notice this would not address the issue of the Hawthorne effect, where a parent changes their behavior in the presence of an interviewer.

Figure 5: Share of variance in parenting explained by neighborhood and family effects: Interviewer-observed parenting behaviors

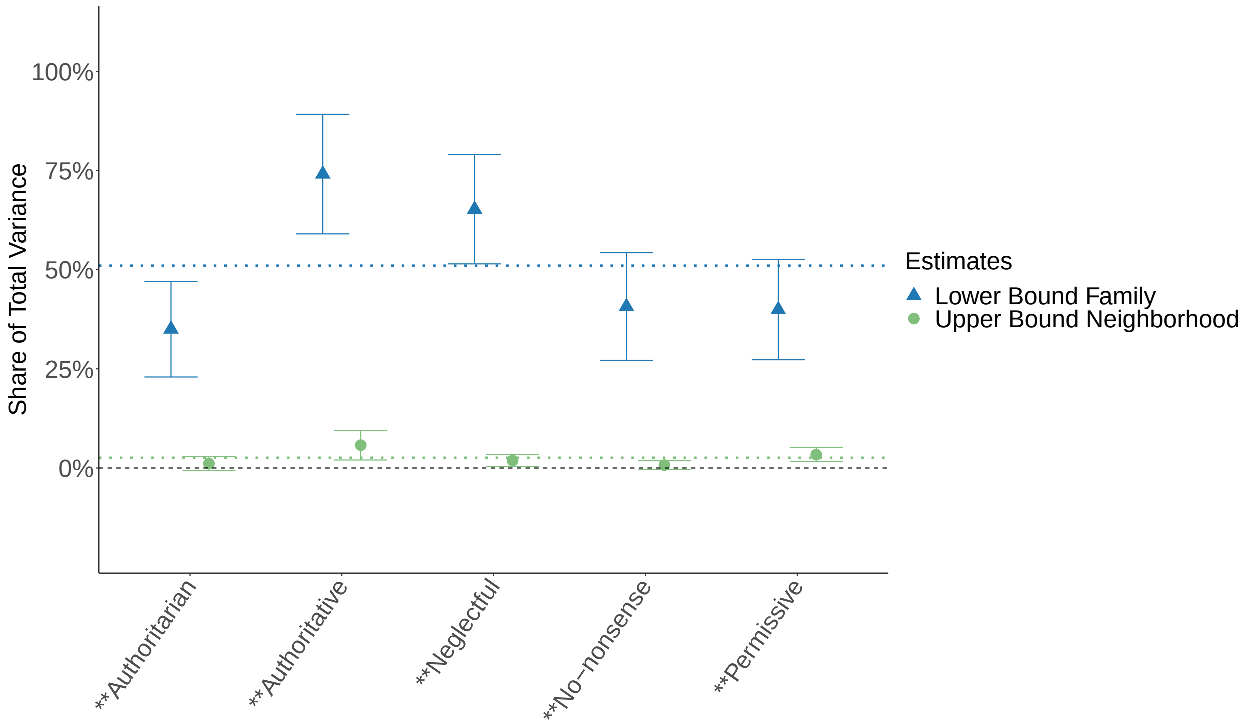


NOTE: In this figure I report the share of variance in parenting explained by causal family and neighborhood effects on interviewer-observed parenting behaviors. The blue points represent the lower bound on family effects, and the green points represent the upper bound on neighborhood effects. The blue and green dashed lines reflects the respective weighted averages across self-reported parenting behaviors. I weight by statistical precision using the inverse of standard errors. The average lower bound on family effects is 55.6% and the average upper bound on neighborhood effects is 1.9%.

The final set of parenting outcomes are for the latent parenting styles that I recover using logistic principal component analysis.[5] Across these measurements of parenting, I continue to find that neighborhoods are not important drivers of the observed variation in parenting. This holds true even for no-nonsense parenting, which has been described as a strategy that parents use in response to hostile environments.

5. See Appendix A for more details.

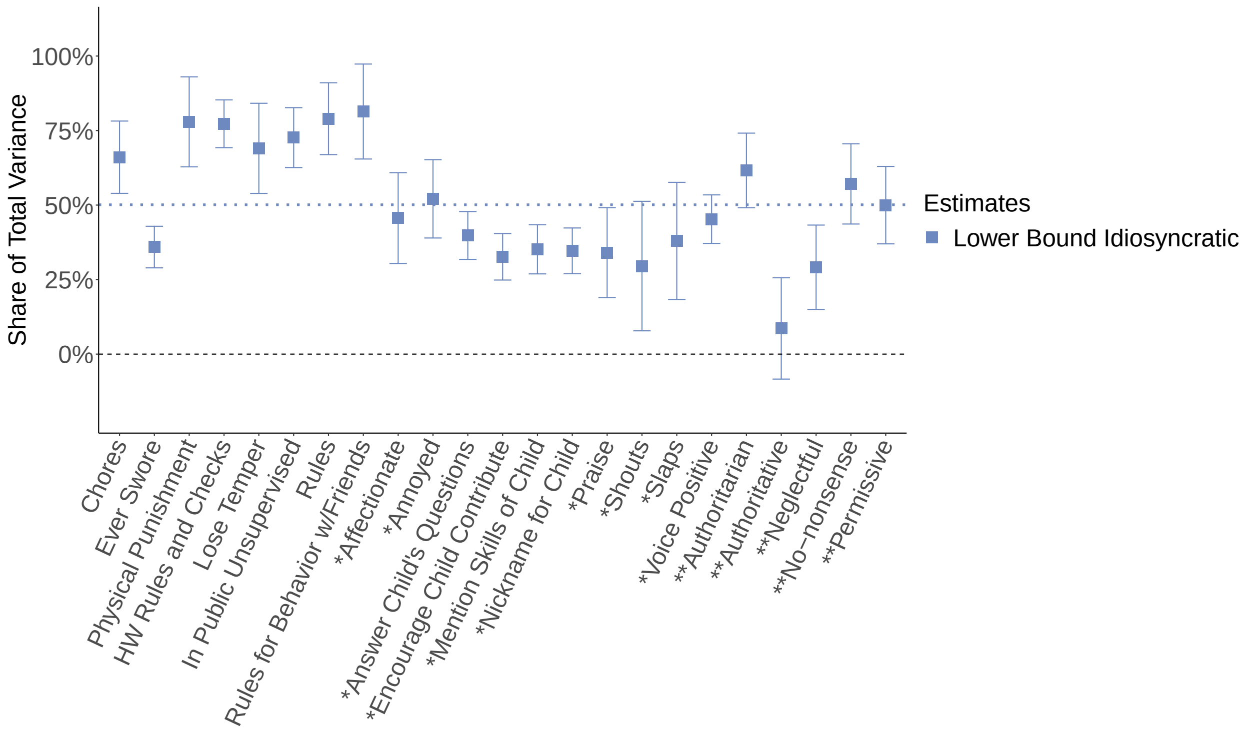
Figure 6: Share of variance in parenting explained by neighborhood and family effects: Latent parenting styles



NOTE: In this figure I report the share of variance in parenting explained by causal family and neighborhood effects on latent parenting styles. The blue points represent the lower bound on family effects, and the green points represent the upper bound on neighborhood effects. The blue and green dashed lines reflects the respective weighted averages across self-reported parenting behaviors. I weight by statistical precision using the inverse of standard errors. The average lower bound on family effects is 51% and the average upper bound on neighborhood effects is 2.6%.

In the final set of results, I look at the lower bound on epsilon across all parenting outcomes. Epsilon captures both idiosyncrasies of a particular child as well as interactions between neighborhood and family. These effects might include unobserved characteristics of the child such as motivation, behavioral problems, or quality of friends. This is not surprising, since many of the observed parenting behaviors, such as whether the parent exhibited high levels of physical punishment, is likely to be a response to the child's behavior. The lower bound on the impact of idiosyncrasies explains the majority of variation for self-reported parenting behaviors.[6] The lower bound of epsilon is less for interviewer-observed behaviors yet still large.

Figure 7: Share of variance in parenting explained by idiosyncratic component



NOTE: In this figure I report the share of variance in parenting explained by the idiosyncratic component on all parenting styles. The points represent the lower bound on the idiosyncratic component. The weighted average lower bound on the idiosyncratic effect is 50%, where I weight by statistical precision using the inverse of standard errors.

The upshot of the main specification is that neighborhood effects are not important drivers of the observed variation in parenting, but family effects and idiosyncratic effects are.[7] In the next section, I test the strength of these results by considering a suite of heterogeneity analyses and robustness checks.

6. This would be more surprising if the outcome variables included questions of potential parenting behavior, such as would you hit your child if they were to fail a class or get in trouble with the law.

7. To strengthen confidence in these results, I repeat this exercise for test score outcomes which are more similar to the outcomes used in existing studies that rely on sibling and neighbor covarainces. See Figure 43 in Appendix B.4.

7

HETEROGENEITY

In the main analysis I find that neighborhood effects are not important drivers of variation in parenting. In this section, I run heterogeneity analyses to examine whether there is evidence of neighborhood effects in subsets of the population.

The strongest existing evidence for neighborhood effects on parenting is in the sociology and developmental psychology literature examining parenting in high-crime areas. Harsh parenting, in particular, has been described as a strategic response of black families to dangerous environments through the effect on the child's obedience levels.[1] While such parenting may be costly for adult outcomes, parents may prioritize the more immediate return on safety. Motivated by this argument, I examine whether there is evidence of neighborhood effects within subsets of the PHDCN LCS sample defined by characteristics of the neighborhood or family.

7.1 Characteristics of neighborhoods

I first examine whether there is evidence of neighborhood effects in subsets of neighborhoods characterized by high levels of crime. I augment the PHDCN LCS data with Chicago Police Department data and classify neighborhoods using the average annual homicide rate in the two years prior to the start of interviews, 1993-1994. I define high-crime neighborhoods as those in the top 25 percentile of all Chicago neighborhoods, including those not in the sample.

In Figure 8 I report the results for the subset of self-reported parenting.[2] The point estimates for neighbor correlations are all less than 5%. The sample size is reduced after

1. See, for instance, Coll and Pachter [2002], Brody and Flor [1998], and Horn et al. [2004].

2. In the appendix I include figures for interviewer observations and latent parenting styles.

restricting to observations in high-crime neighborhoods, so the standard errors are larger. However, the upper bound on neighborhood effects is still statistically indistinguishable from zero for all but 1 measurement of parenting.

This result is evidence against the theory that neighborhood crime causes harsh parenting. Even within the most dangerous neighborhoods, there is much more variation across families than within. This suggests that moving families from bad neighborhoods will not alter parenting behaviors on average.

In Figure 9 I examine the parenting in exceptionally safe neighborhoods, where I define safe neighborhoods to be those in the bottom quartile of homicide rates. Perhaps parents spend more time with neighbors in safer neighborhoods, which might enable the role-modeling effects that could drive a causal relationship between parenting and place. Again, I find the upper bound on neighborhood effects to be close to zero.

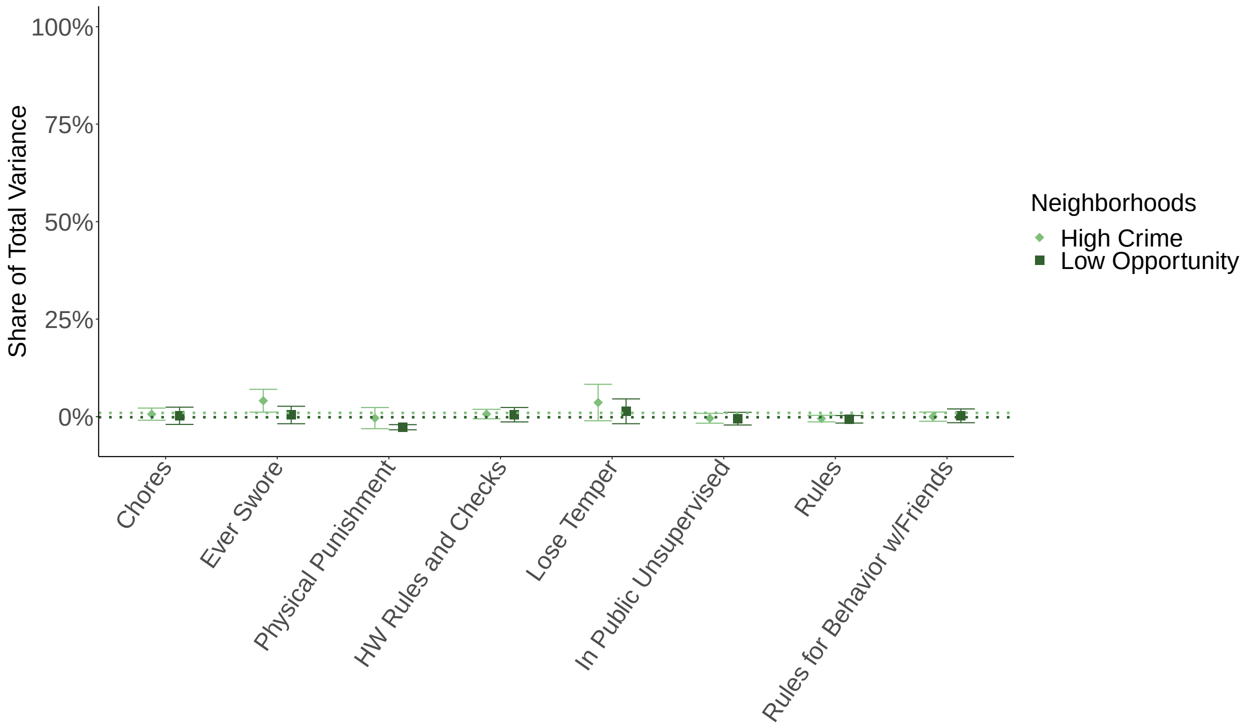
Next I classify neighborhoods based on their economic opportunity rates. As discussed in Section 1, parenting may be a way to moderate other neighborhood effects so that we might expect neighborhoods with exceptionally high or low neighborhood effects to exhibit strong neighborhood effects on parenting. High-opportunity areas are also those characterized by stronger social interactions across class lines.[3] This may foster role-modeling. To examine whether there are neighborhood effects in high-opportunity or low-opportunity areas, I augment my data with Opportunity Atlas data on average adult household income for children that grew up in the area.[4] I define low-opportunity neighborhoods as those in the bottom 25 percentile across all Chicago neighborhoods, and high-opportunity neighborhoods as being in the top 25 percentile. I report my results in Figures 8 and 9 and continue to find minimal neighborhood effects.[5]

3. Chetty et al. [2022].

4. I use estimates for children with low-income parents, defined as the bottom 25 percentile of the income distribution, and I use children of all races and genders.

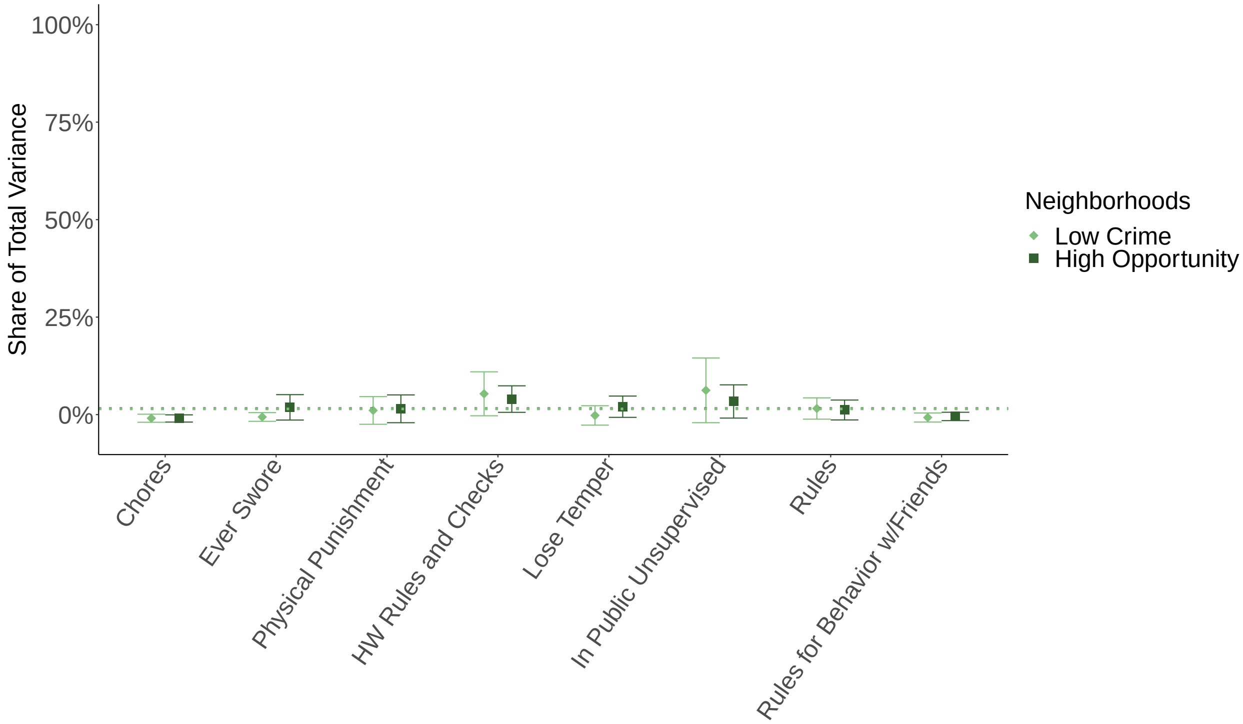
5. See Appendix B.2.1 for figures reporting on the interviewer observations and latent parenting styles along with a table of regression coefficients.

Figure 8: Share of variance in parenting explained by neighborhood effects in high-crime or low-opportunity neighborhoods: Self-reported parenting behaviors



Note: In this figure I report the share of variance in parenting explained by causal neighborhood effects on self-reported parenting behaviors in neighborhoods characterized by high homicide rates and low economic opportunity. The dashed lines reflect the respective weighted averages across self-reported parenting behaviors. I weight using the inverse of standard errors. The average upper bound on neighborhood effects for high crime areas is 1% and for low-opportunity is 0%.

Figure 9: Share of variance in parenting explained by neighborhood effects in low-crime or high-opportunity neighborhoods: Self-reported parenting behaviors



NOTE: In this figure I report the share of variance in parenting explained by causal neighborhood effects on self-reported parenting behaviors in neighborhoods characterized by low homicide rates and high economic opportunity. The dashed lines reflect the respective weighted averages across self-reported parenting behaviors. I weight using the inverse of standard errors. The average upper bound on neighborhood effects for low crime areas is 1% and for high-opportunity is 2%.

7.2 Characteristics of parents

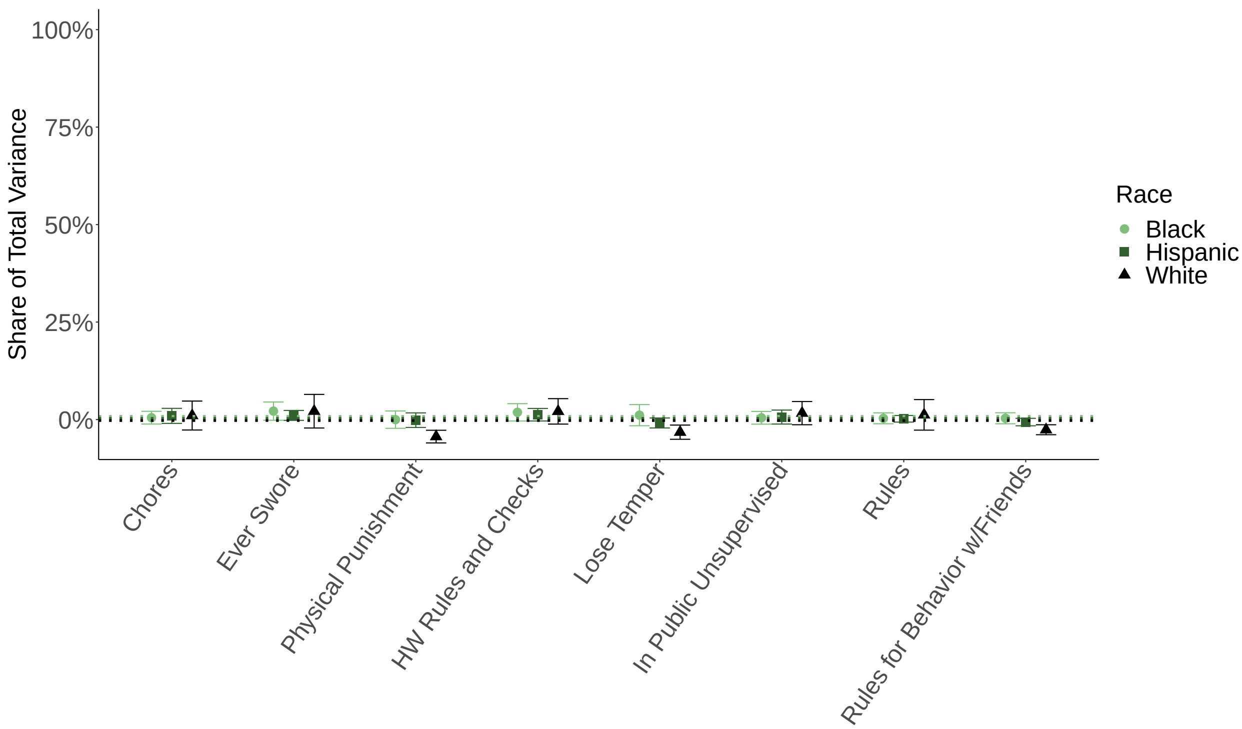
As described in Section 2, the literature has suggested that harsh parenting tactics, such as corporal punishment, and strict parenting styles, such as authoritarian and no-nonsense parenting, may be strategic reactions to dangerous environments specifically among black parents.[6] While the correlation with race could just reflect residential sorting, it's also possible that different racial groups have different exposures to the neighborhood and therefore

6. See, for instance, Coll and Pachter [2002], Brody and Flor [1998], and Horn et al. [2004].

react differently. In this section I test for neighborhood effects in subsets of the data defined by the race of the parent. This is a test of the linearity assumption imposed by the model.

In Figure 10 I show the neighborhood effects on self-reported parenting estimated for different racial subgroups. In the Appendix B.2.2 I report the same figure for interviewer observations and parenting styles. In Appendix A figures 28 and 29 I show that there are racial differences in parenting behaviors. However, the results here show that such differences are not accounted for by the causal impact of neighborhood effects.

Figure 10: Share of variance in parenting explained by neighborhood effects by race of parent: Self-reported parenting behaviors



Note: In this figure I report the share of variance in parenting explained by causal neighborhood effects on self-reported parenting behaviors by race of the parent. The dashed line reflects the respective weighted averages across self-reported parenting behaviors. I weight using the inverse of standard errors.

8

MOVER DESIGN

The key identifying assumption in the sibling and neighbor covariance research design is positive sorting. Positive sorting refers to a positive correlation between the family effects within a neighborhood and a positive correlation between the family and respective neighborhood effect. In other words, families that live together have a similar propensity to parent in a particular way, and they live in neighborhoods that encourage that same approach to parenting.

It is uncontroversial that families with similar backgrounds tend to live together. It is more of a stretch to assume positive sorting between family and neighborhood effects. For example, a parent may have a preference to be affectionate, but they may be constrained to live in a dangerous area that inhibits parental affection, perhaps through higher levels of stress. In this case there would be a negative relationship between family effects and neighborhood effects on parenting.

In this section I consider an alternative research design, the mover design, that replaces the assumption of positive sorting. In the mover design, I take advantage of the panel structure of the PHDCN LCS and the fact that movers are followed. I estimate the causal effects of moving out of high-crime, low-crime, high-opportunity, or low-opportunity neighborhoods on parenting. In this research design, I replace the assumption of positive sorting and instead assume that moves are exogenous to unobserved changes in family circumstance that determine parenting. I discuss this assumption in greater detail below.

As in the previous section, I residualize parenting by regressing parenting measurements on a quadratic in the child's age, and indicator variables for the sex of the child and whether they are the oldest. Since I work with panel data in this research design, I also include the survey wave to control for shared differences across time, such as how a survey question is worded. I normalize the residualized parenting variables so that each measurement has a standard deviation of 1. Lastly, I first-difference the residualized measurements of parenting across survey waves.

I restrict my sample to the set of families that move during the PHDCN study period, and I restrict the observations to the periods capturing the moves. For instance, if a family moved in between waves 1 and wave 2 and remained in place for wave 3, I only include the first difference of wave 2 less wave 1. Since parents are instructed to report on their behavior in the previous year, I drop observations where the time at the current address is less than 6 months. I also drop observations where the parent changes.

In the first mover design, I classify neighborhoods by crime rates. In a given survey wave, a neighborhood is classified as being low (high) crime if the average annual homicide rate in the two years prior to the start of the wave is in the bottom (top) quartile.

I regress the change in parenting, Δ^P, on $Move$, a variable that takes the value 1 if the subject moved into a neighborhood of a particular type, the value -1 if the subject moves out of a neighborhood of a particular type, and the value 0 if the neighborhood type remains constant. Since moving is likely to be correlated with changes in aspects of family background that might cause parenting, I include controls for changes in education, marriage, employment and income status of the parent which are stored in the vector X.[1]

I estimate the following regression with Ordinary Least Squares.

$$\Delta^P_{iw} = \alpha + \beta Move_{iw} + \gamma' X_{iw} + \epsilon_{iw}$$

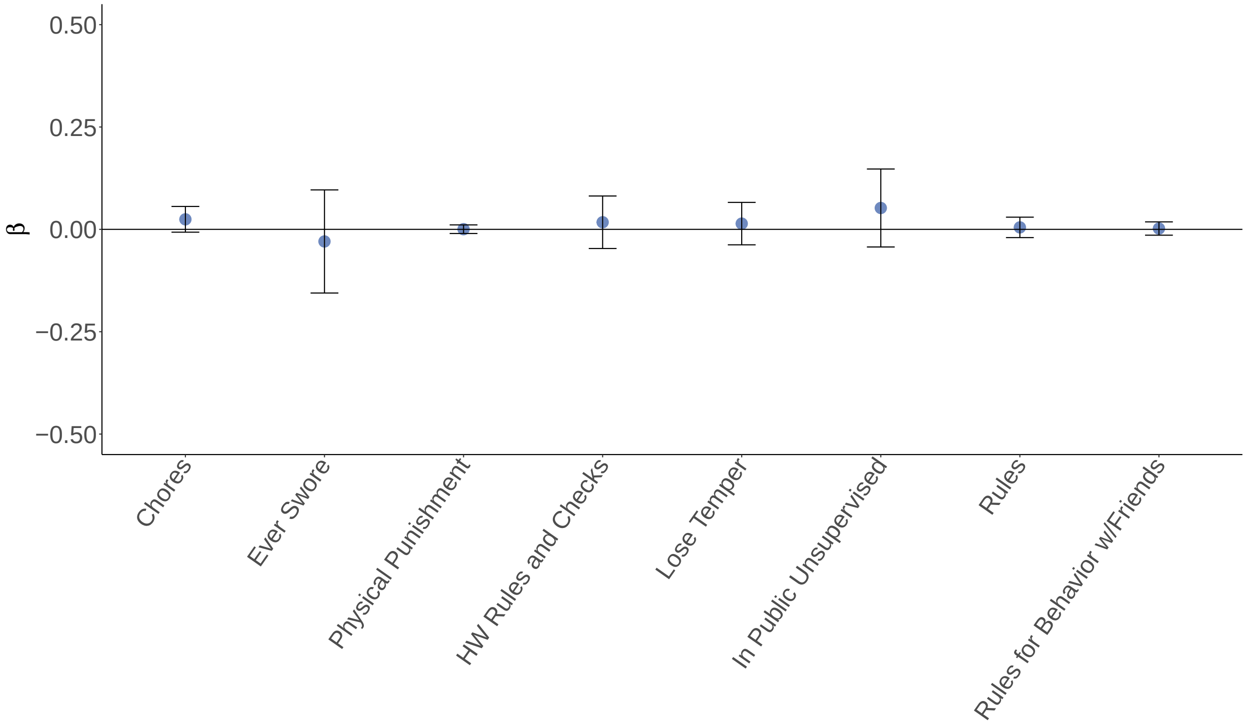
I replace the assumption of positive sorting with the assumption that all unobserved changes in family background are exogenous so that the coefficient on $Move$ measures the impact on parenting of moving between neighborhood types. This assumes away scenarios such as where a family moves due to a parent getting a better job with higher income and less stress. In

1. A change in education reflects crossing the threshold of obtaining a Bchelor's degree, and a change in income reflects the threshold of an income greater than $30,000.

this example, the unobserved change in stress would be correlated with the move to a better neighborhood and the change in parenting. While this assumption is strong, I recover results that are consistent with those uncovered in the sibling and neighbor correlation analysis.

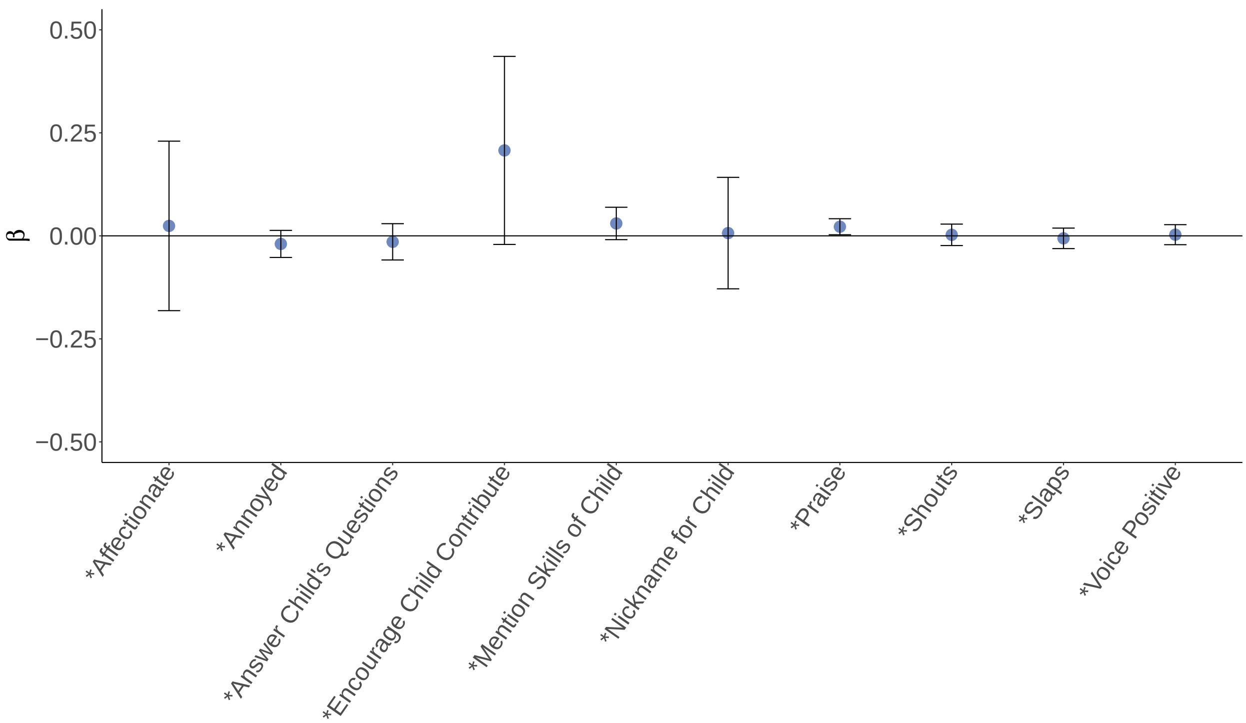
In Figure 11 I report coefficients for the set of self-reported parenting behaviors for moves to dangerous neighborhoods. Moving into a high-crime neighborhood is not statistically significant from zero across self-reported parenting behaviors. In Figure 12 I show that there is no statistically significant effects on interviewer-observed parenting behaviors either. When a family moves into a high-crime neighborhood, the parent does not adjust their parenting strategies. In Appendix B.5 figures 44 and 45 I report estimates for the impact of moving to a low-crime area where the results are, again, bound around zero.

Figure 11: Impact of moving to a dangerous neighborhood: Self-reported parenting behaviors



NOTE: This figure plots estimates of the causal impact of moving to a high-crime neighborhood on self-reported parenting. High-crime neighborhoods are those where the average annual homicide rate in the two years prior to the survey wave is in the top 25 percentile. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and fixed effects for the survey wave. I regress the standardized and residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

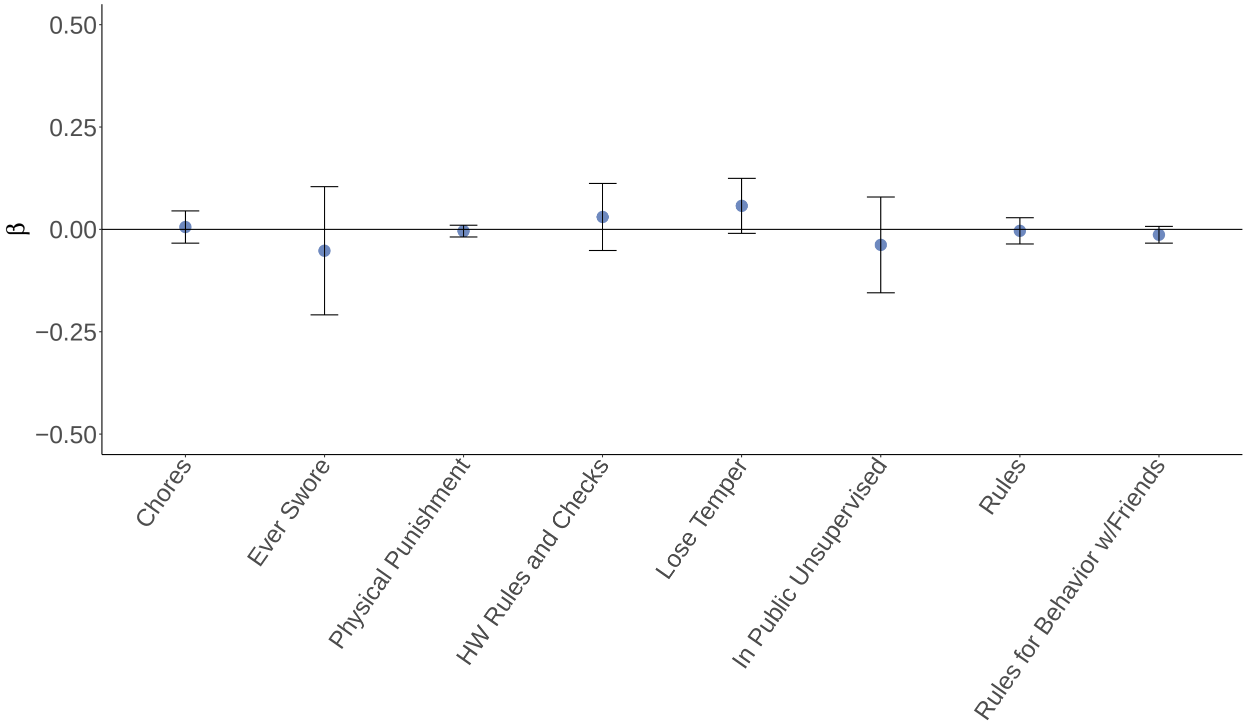
Figure 12: Impact of moving to a dangerous neighborhood: Interviewer-observed parenting behaviors



Note: This figure plots estimates of the causal impact of moving to a high-crime neighborhood on interviewer-observed parenting. High-crime neighborhoods are those where the average annual homicide rate in the two years prior to the survey wave is in the top 25 percentile. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and fixed effects for the survey wave. I regress the standardized and residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

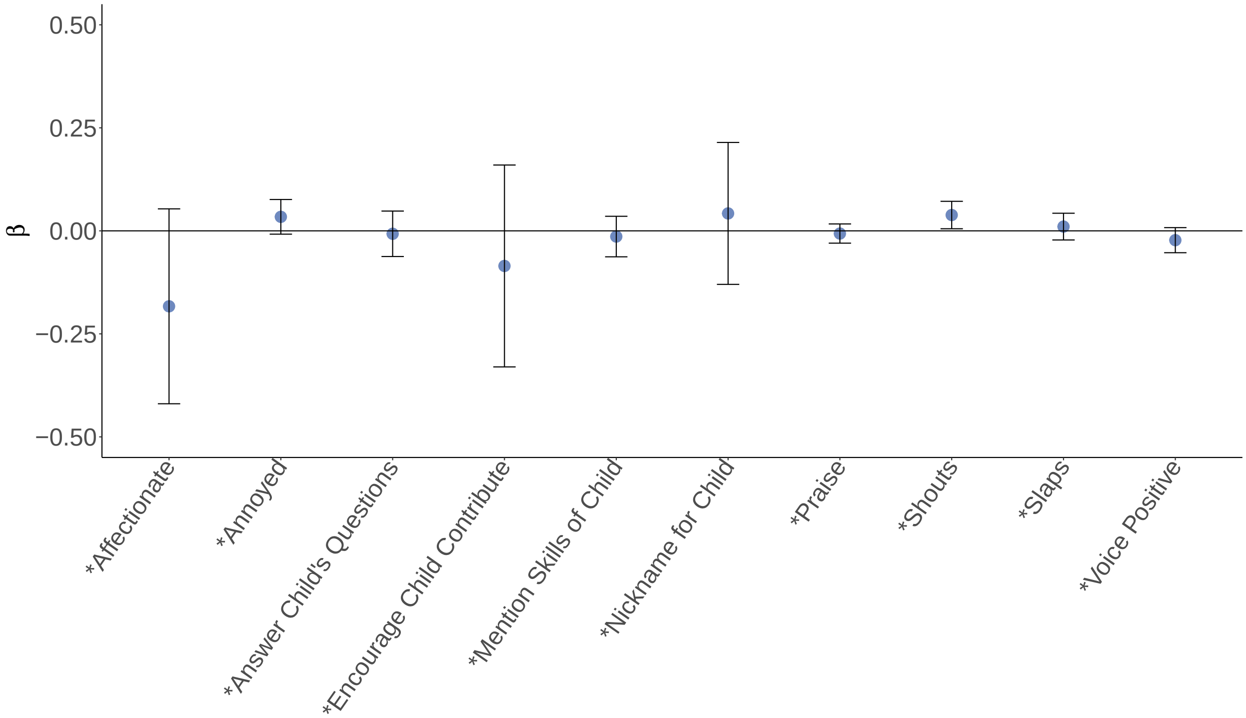
I repeat this analysis for moves into low opportunity areas and there continues to be no measured impact on parenting. In Appendix B.5 Figures 46 and 47 I report estimates for the impact of moving into high-opportunity areas where the results are, again, bound around zero.

Figure 13: Impact of moving to a low-opportunity neighborhood: Self-reported parenting behaviors



Note: This figure plots estimates of the causal impact of moving to a low-opportunity neighborhood on self-reported parenting. Low-opportunity neighborhoods are those where the expected adult income is in the bottom 25 percentile. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and fixed effects for the survey wave. I regress the standardized and residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

Figure 14: Impact of moving to a low-opportunity neighborhood: Interviewer-observed parenting behaviors



Note: This figure plots estimates of the causal impact of moving to a low-opportunity neighborhood on interviewer-observed parenting. Low-opportunity neighborhoods are those where the average expected adult income in the bottom 25 percentile. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and fixed effects for the survey wave. I regress the standardized and residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

One possible explanation for null effects is that people may move to neighborhoods that are similar to their origin neighborhood. Perhaps the moves in and out of the top or bottom percentile are on either side of the threshold. In Appendix Figure 49, I show that this is not the case. For example, moves to high-crime neighborhoods are three times as dangerous, on average, and moves to low crime neighborhoods are half as dangerous as the origin neighborhoods. In other words, the moves from which I identify treatment effects are between neighborhoods that are meaningfully different, yet parenting does not adjust.

9

CONCLUSION

This paper establishes some of the first empirical estimates of the causal origins of parenting. I impose a model where parenting is a linear function of family, neighborhood and idiosyncratic effects, and I assume that residential sorting is positive. Under these mild assumptions, I bound the contributions of each effect to observed variation in parenting. I find that, despite many theoretical arguments for neighborhood effects, neighborhoods are not important determinants of the observed variation in parenting. This result holds across a wide set of measurements of parenting that includes self-reported and interviewer-observed behaviors, as well as latent parenting styles. Instead it is family effects that matter. I test the robustness of this result by examining whether neighborhood effects are important in places characterized by extreme levels of homicide or economic mobility. I also conduct an alternative mover design where I replace the assumption of positive sorting with the assumption that moves are exogenous conditional on observed changes in family background. In both specifications I continue to find that neighborhood effects are not important determinants of variation in parenting.

These results are surprising. If neighborhoods have strong direct effects on human capital development, why do we not see parents moderating the effects? One possibility is that there is masked heterogeneity. For example, some parents might offset the consequences of poor school quality by increasing their levels of homework assistance. Alternatively, given the complementarity between investments in human capital formation, other parents may be discouraged from at-home investments due to the lower return.[1] If parents have a mixture of responses, then the net effect of neighborhoods on parenting may be zero. Future work should explore whether such heterogeneity exists and continue to examine whether there

1. See, for instance, Heckman and Mosso [2014] and Attanasio [2015] for reviews characterizing the human capital production function.

are important interactions between neighborhood effects and characteristics of the family or child. A second explanation for the lack of neighborhood effects on parenting may be that parents are constrained, either by information or resources, and unable to adapt to their environment. While these explanations are worth exploring, it is also worth continuing to explore whether neighborhoods do indeed have strong causal effects on child development.

Beyond enhancing our understanding of parental decision-making, this paper contributes insight into the determinants of earnings inequality. Parenting is likely to impact human capital development, and parenting behaviors exhibit significant disparities along socioeconomic lines.[2] Families with higher levels of education invest more time in their children, and the nature of their interactions align more closely with practices recommended by developmental psychologists.[3] These disparities in parenting are likely to have implications for the subsequent earnings distribution. The better we understand the origins of parenting, the more effectively we can address parenting gaps to encourage mobility. The insight of this paper is that it will take more than moving a family to address critical parenting gaps.

2. See Waldfogel and Washbrook [2011], Cunha et al. [2006] and Heckman and Masterov [2004] for evidence that parental engagement explains a large share of the skill gap between children of different socio-economic backgrounds.

3. For instance, high-SES parents are more likely to exhibit warmth, set clear rules and expectations, and be firm yet fair in consequences. These practices are associated with better adult outcomes. See, for instance, Kalil et al. [2012], Hill and Stafford [1974], Pinderhughes et al. [2000], Hart and Risley [1995], and Hoff [2003]. Higher SES parents are also more likely to spend more time with their children, see Guryan et al. [2008], Ramey and Ramey [2010], and Hurst [2010].

A

APPENDIX: ADDITIONAL FIGURES ON THE MEASUREMENT OF PARENTING AND DESCRIPTIVE STATISTICS

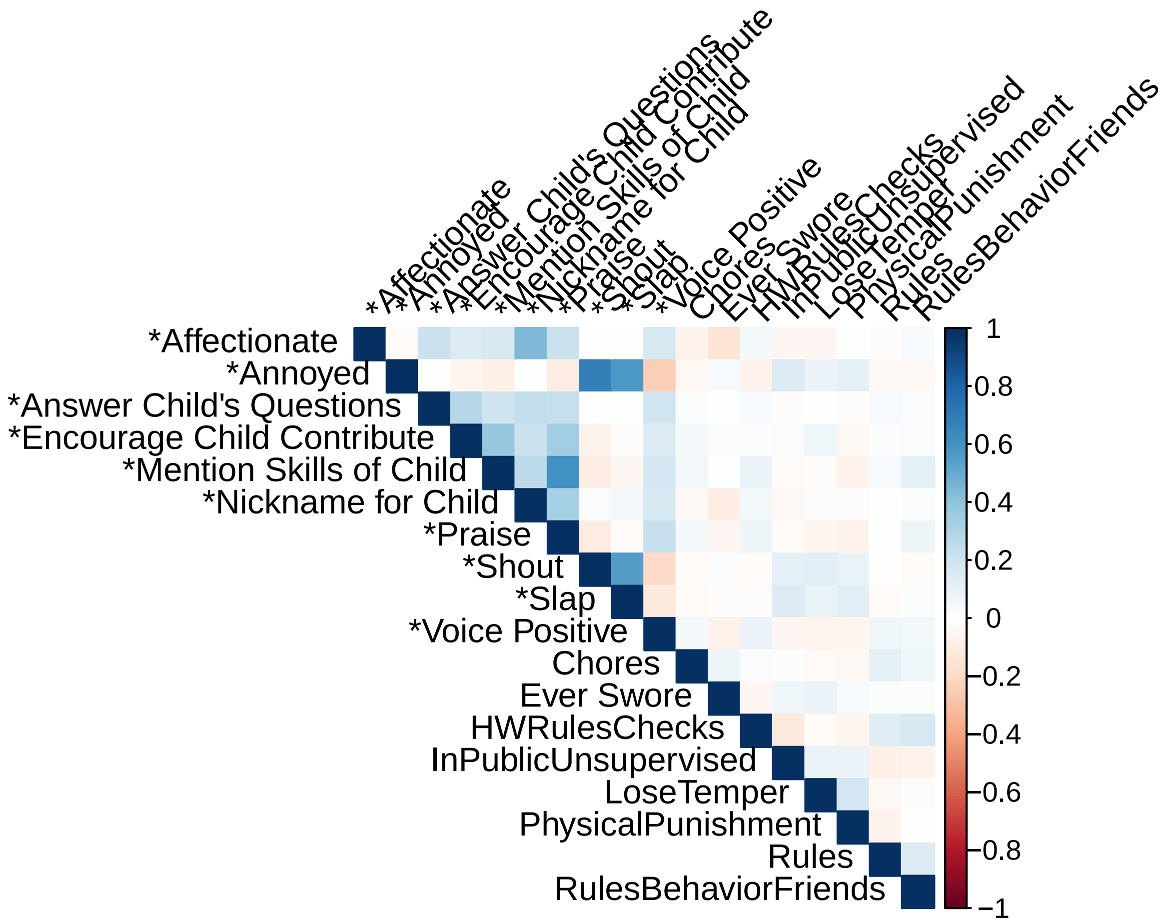
In this section I describe how I construct the latent parenting styles and I provide descriptive statistics on all measurements of parenting.

A.1 Constructing latent parenting style indexes

In Figure 15 I plot correlations between the 18 survey questions I use to construct the latent parenting style indexes. The color and saturation represents the strength of the correlation. As described in Section 3.2, I take a data-driven approach and use logistic Principal Component Analysis (PCA) to reduce the survey questions into a set of 5 latent parenting styles.[1]

1. Alternative unsupervised learning methods include K-means, which is ill-advised for binary data, K-modes, which is unattractive in that it uses constant weights across variables, and Latent Dirichlet Allocation (LDA) which is a hierarchical model most commonly used for topic modeling. While I used LDA in an early iteration of this work, the results were similar enough to logistic PCA that I solely use the more familiar methodology of logistic PCA.

Figure 15: Correlations between observed parenting behaviors.



Logistic PCA is PCA extended to binary data.[2] To articulate this method I use the probabilistic perspective of the standard PCA model.[3] I model the high dimensional data point of stacked parenting behaviors, p_i, as a noisy observation of a low-dimensional true

2. Collins et al. [2001]. Logistic PCA is less prone to over-fitting than alternative methods for binary data, such as logistic SVD. See Song et al. [2017] for a more detailed comparison.

3. Tipping and Bishop [1999].

data point θ_i.

$$p_i = \theta_i + \epsilon_i$$
$$\theta_i = \underbrace{\mu}_{\text{Offset}} + \overbrace{B}^{\text{coefficients}} \underbrace{a_i}_{\text{scores}}$$
$$\epsilon \sim \mathcal{N}(0, \sigma^2 I_j)$$

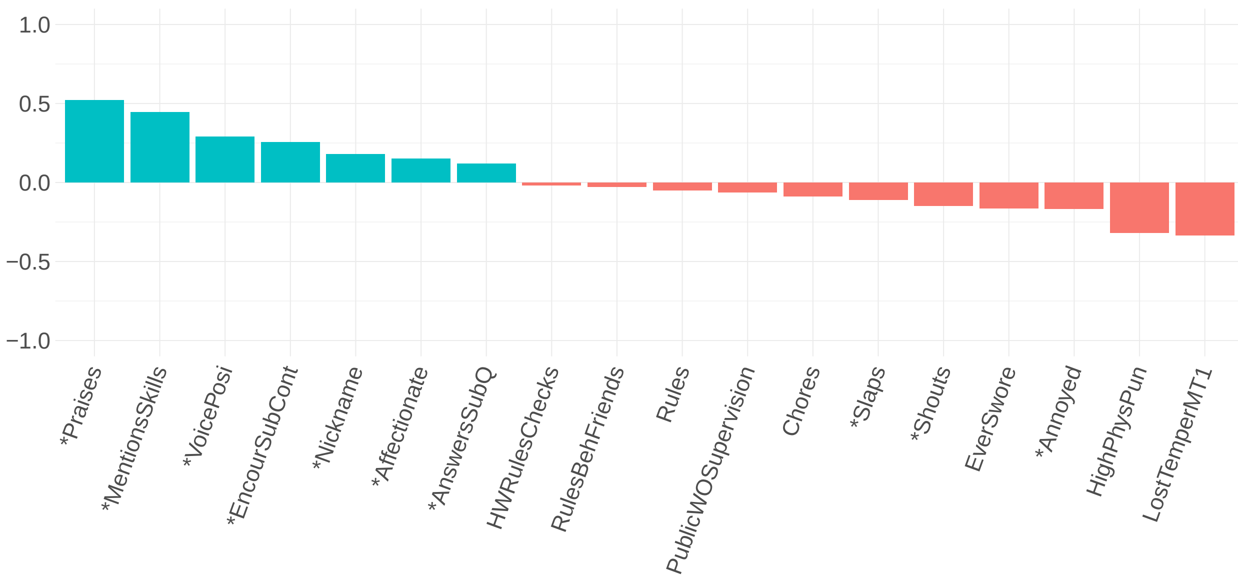
In standard PCA, the conditional distribution of p_i is the normal distribution.

$$\mathrm{p_i} \mid \mu, \mathrm{a}_{\mathbf{i}}, \mathrm{B} \sim \mathrm{N}\mu + \mathrm{Ba}_{\mathbf{i}}, \sigma^2 \mathrm{I_J}$$

Logistic PCA replaces the normal distribution with a Bernoulli distribution. I estimate logistic PCA in my data using the set of 18 parenting variables in all three waves of the survey. I replace missing values with the cohort and wave mean in instances where the share missing for a cohort and wave is not more than 75 percent. I then estimate logistic PCA for the subset of complete cases. I set the number of principal components to 5 to capture the 4 Baumrind parenting styles plus the no-nonsense parenting style.

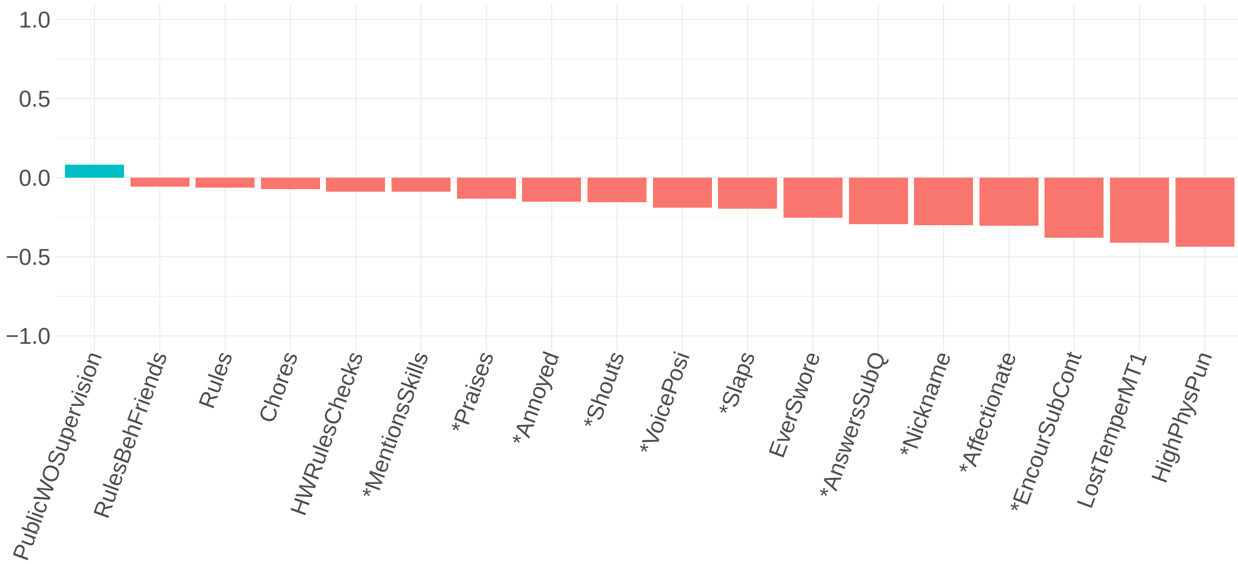
I map the recovered principal components to the parenting styles by reviewing the weights (loadings) assigned to each survey question. A large absolute value on a variable indicates that the question is important for the principal component, a positive weight means the parenting style index value (score) is higher when the question is answered positively. In Figures 16 through 20 I plot the loadings for each principal component and the linked parenting style.

Figure 16: Logistic PCA loadings on parenting behaviors for first principal component, interpreted as latent authoritative parenting style



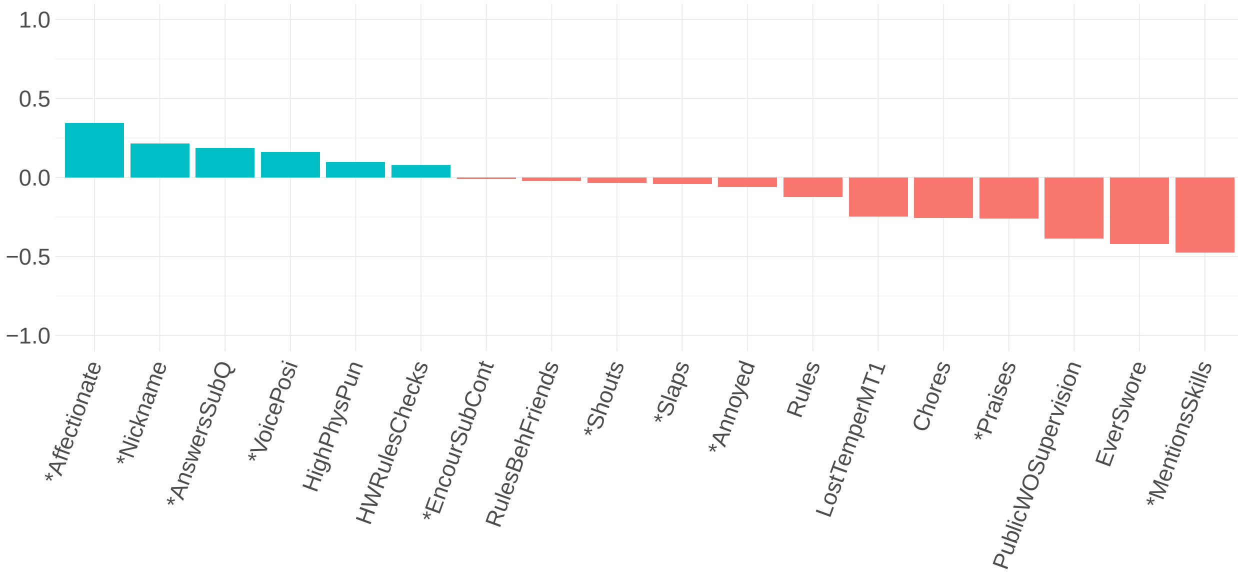
NOTE: - In this figure I plot the loadings on parenting behaviors for the first principal component recovered from logistic principal component analysis. I interpret this principal component as the authoritative parenting style as there are large positive weights parenting behaviors that speak to the dimensions of a parenting being demanding & controlling (encourages subject to contribute, mentions skills of the child) and responsive & warm (positive voice, nickname, affectionate). There are also negative weights on aspects of harsh punishment (high physical punishment and losing temper).

Figure 17: Logistic PCA loadings on parenting behaviors for second principal component, interpreted as latent neglectful parenting style



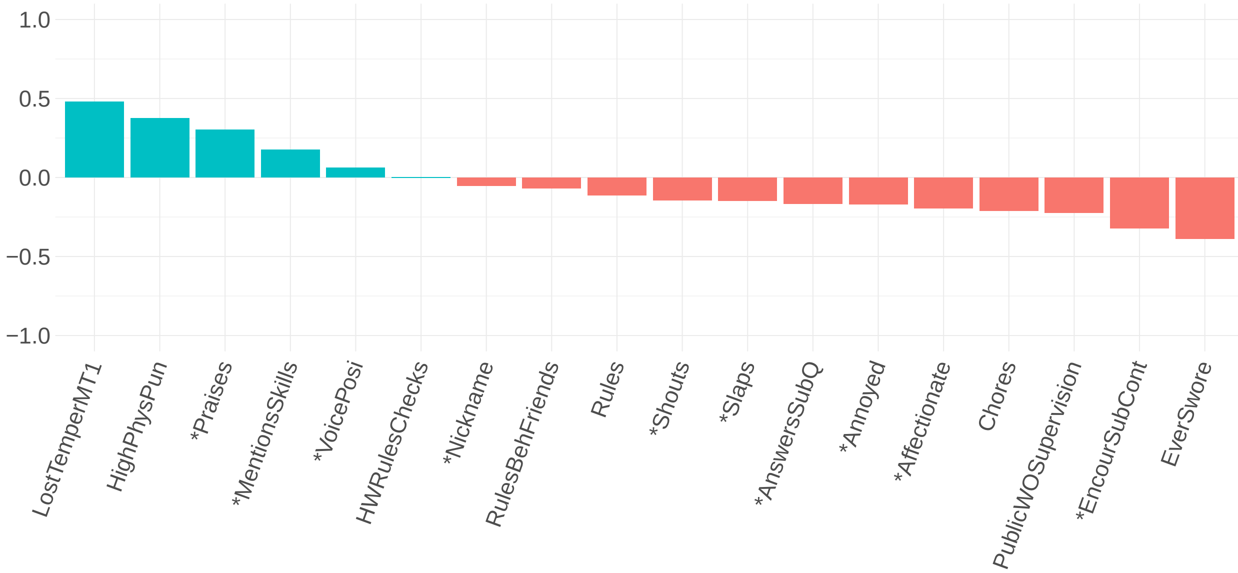
NOTE: - In this figure I plot the loadings on parenting behaviors for the second principal component recovered from logistic principal component analysis. I interpret this principal component as the neglectful parenting style as there are large negative weights on all parenting behaviors that speak to the dimensions of a parenting being demanding & controlling and responsive & warm.

Figure 18: Logistic PCA loadings on parenting behaviors for third principal component, interpreted as latent permissive parenting style



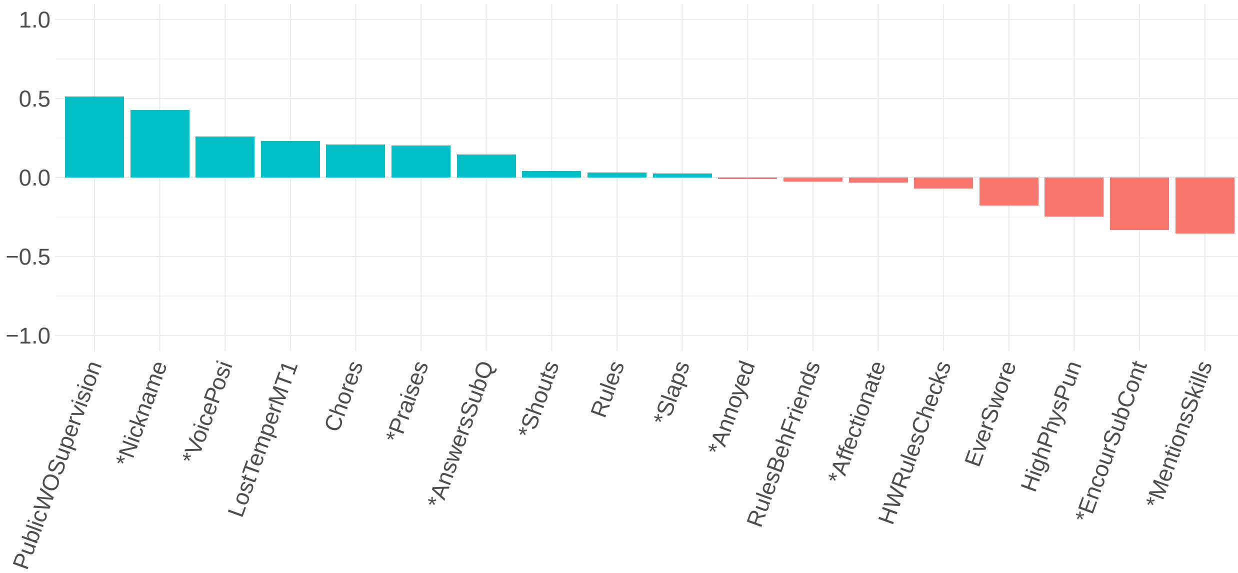
NOTE: - In this figure I plot the loadings on parenting behaviors for the third principal component recovered from logistic principal component analysis. I interpret this principal component as the permissive parenting style as there are large negative weights on parenting behaviors that speak to the dimensions of a parenting being demanding & controlling (mentions skill of the child, chores) and positive weights on the parenting behaviors that speak to the dimensions of a parent being responsive & warm (affectionate, nickname, answering the child's questions).

Figure 19: Logistic PCA loadings on parenting behaviors for the fourth principal component, interpreted as latent authoritarian parenting style



NOTE: - In this figure I plot the loadings on parenting behaviors for the fourth principal component recovered from logistic principal component analysis. I interpret this principal component as the authoritarian parenting style as there are large positive weights on parenting behaviors that speak to the dimensions of a parenting being demanding & controlling, including harsh punishment, (mentions skill of the child, high physical punishment) and negative weights on the parenting behaviors that speak to the dimensions of a parent being responsive & warm (affectionate, answering the child's questions).

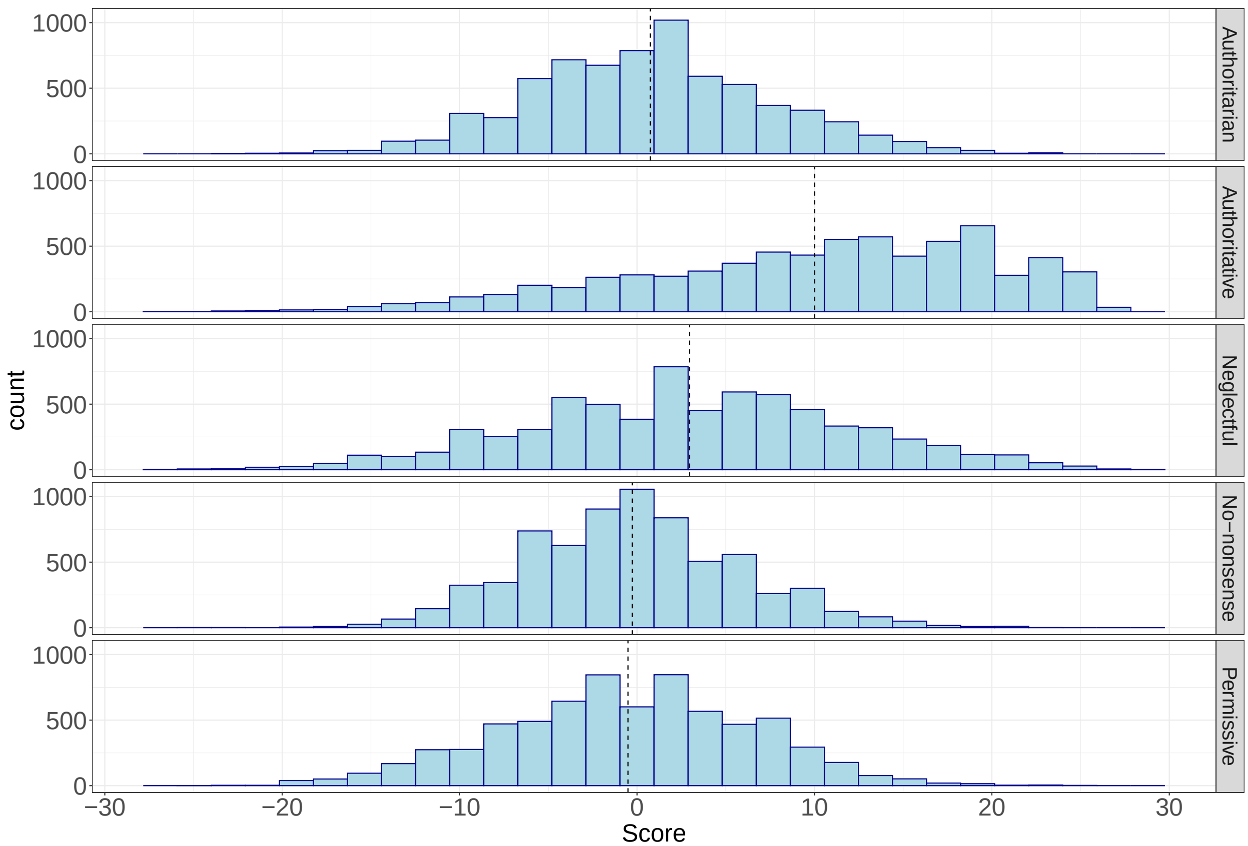
Figure 20: Logistic PCA loadings on parenting behaviors for the fifth principal component, interpreted as latent no-nonsense parenting style



NOTE: - In this figure I plot the loadings on parenting behaviors for the fifth principal component recovered from logistic principal component analysis. I interpret this principal component as the no-nonsense parenting style as there are large positive weights on parenting behaviors that speak to the dimensions of a parenting being demanding & controlling, including harsh punishment, (chores) and large weights on the parenting behaviors that speak to the dimensions of a parent being responsive & warm (nickname, positive voice). But there is harsher punishment than typical of an authoritative parent (losing temper).

The distribution of scores for each latent parenting style is displayed below.

Figure 21: Distribution of scores for each latent parenting style



NOTE: - In this figure I plot the distribution of scores across parent-child pairs for each principal component, which I interpret as indexes of latent parenting styles.

A.2 Additional descriptive statistics on parenting

In this section I provide descriptive statistics of parenting including age and gender profiles, correlations with neighborhood characteristics, and correlations with family characteristics.

A.2.1 Age and gender profiles

In the following set of figures I display how average parenting varies by the age and gender of the child. While age profiles appear, there is surprisingly little variation across gender.

Figure 22: Age profiles of self-reported parenting, by gender of child

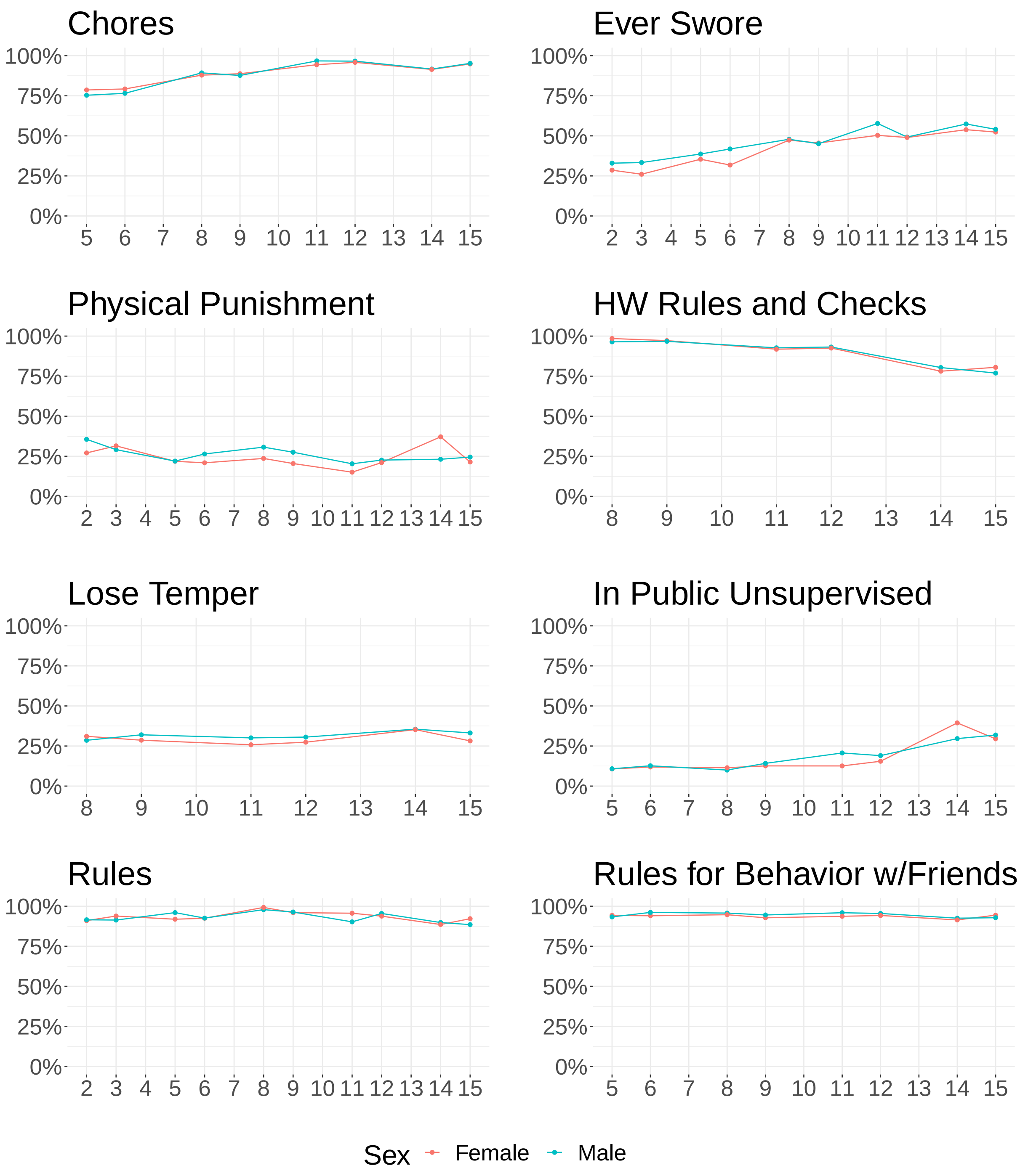
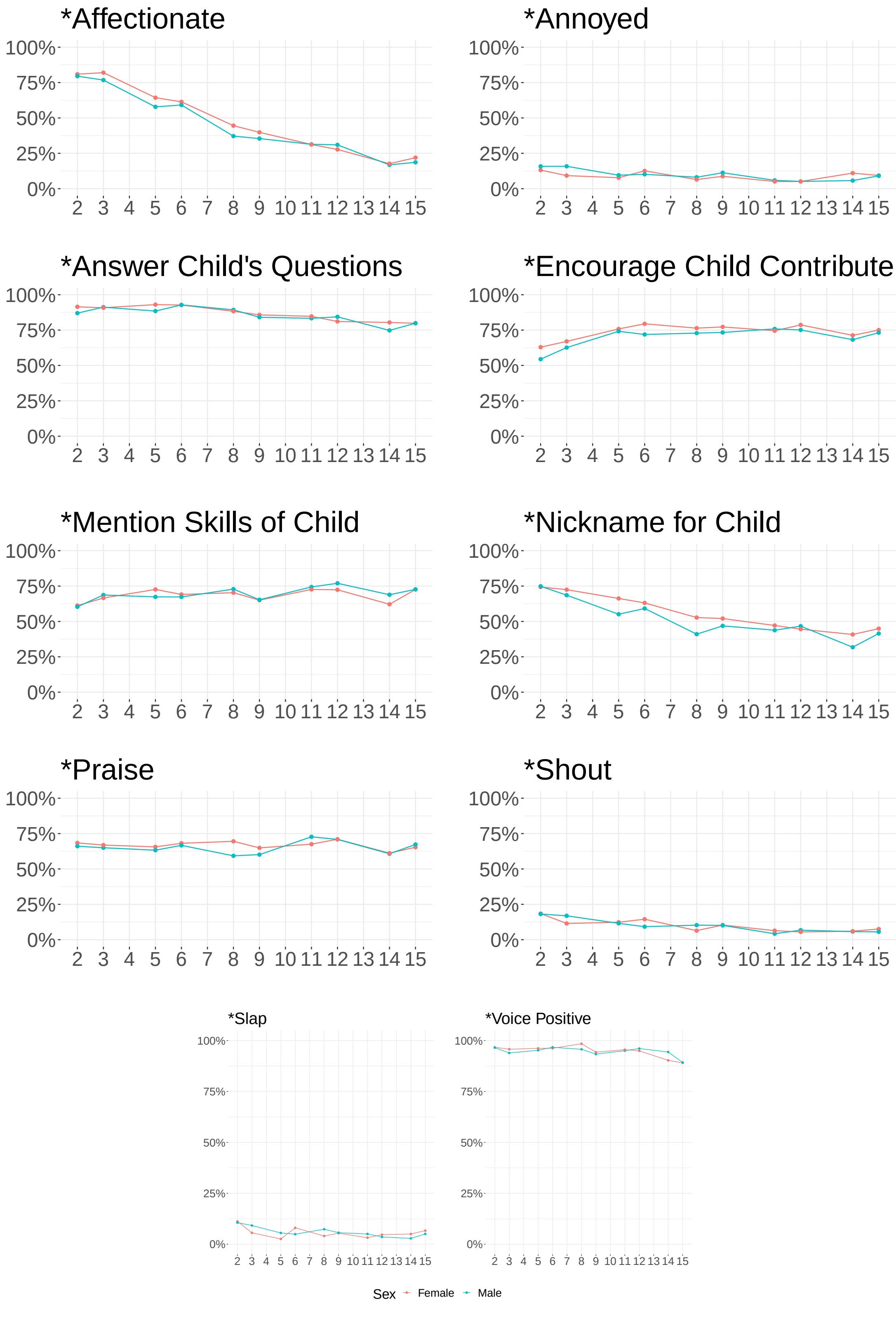


Figure 23: Age profiles of interviewer-observed parenting, by gender of child



A.2.2 Correlations with neighborhood characteristics

The following set of figures display how average parenting varies by neighborhood classification.

Figure 24: Average self-reported parenting across neighborhood types: Crime

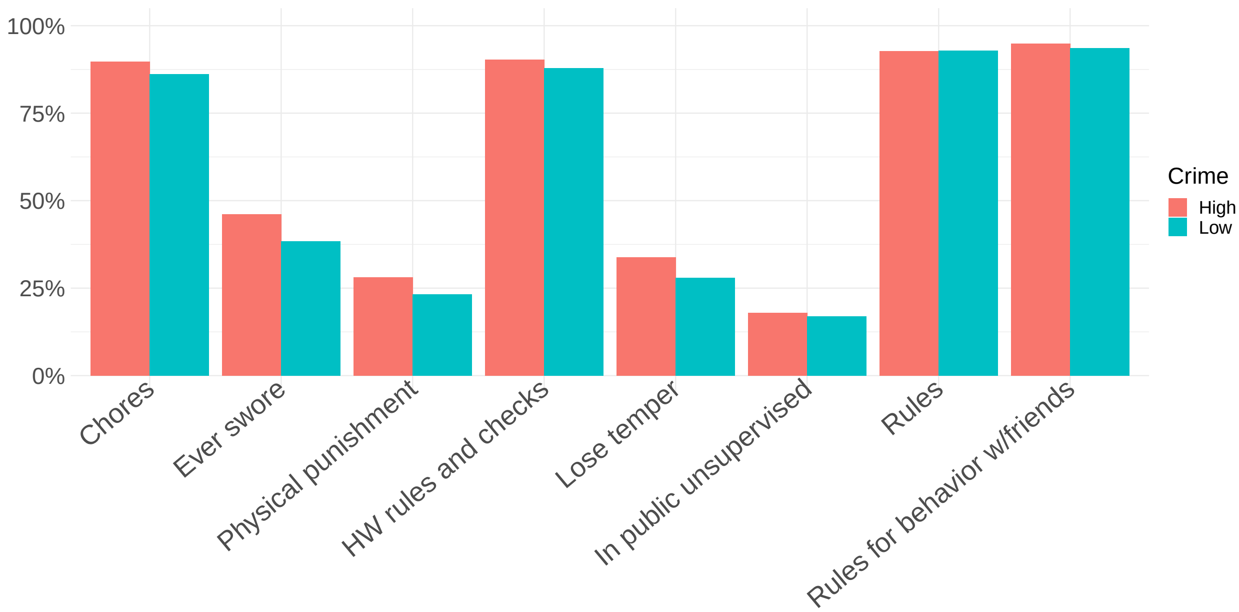


Figure 25: Average interviewer-observed parenting across neighborhood types: Crime

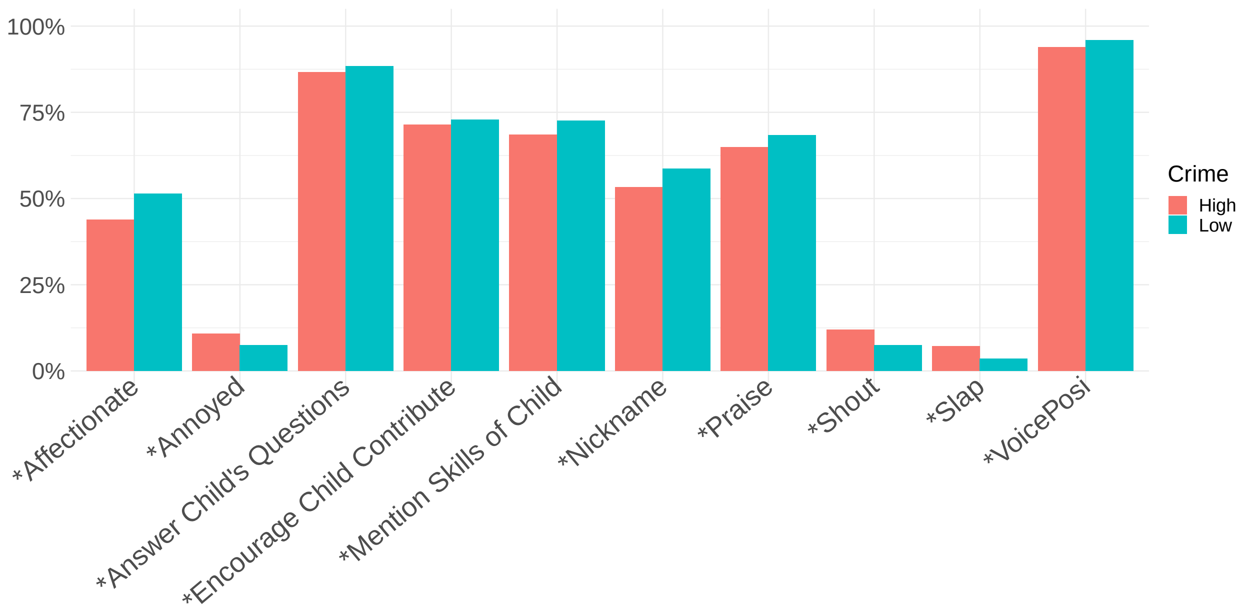


Figure 26: Average self-reported parenting across neighborhood types: Economic mobility

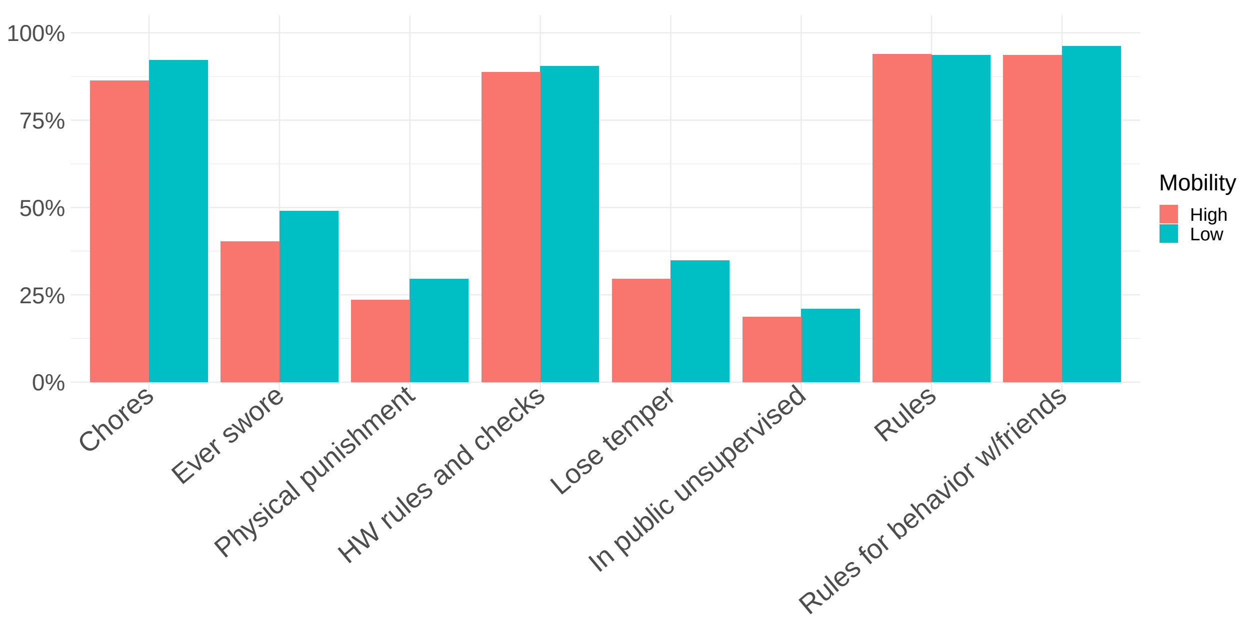
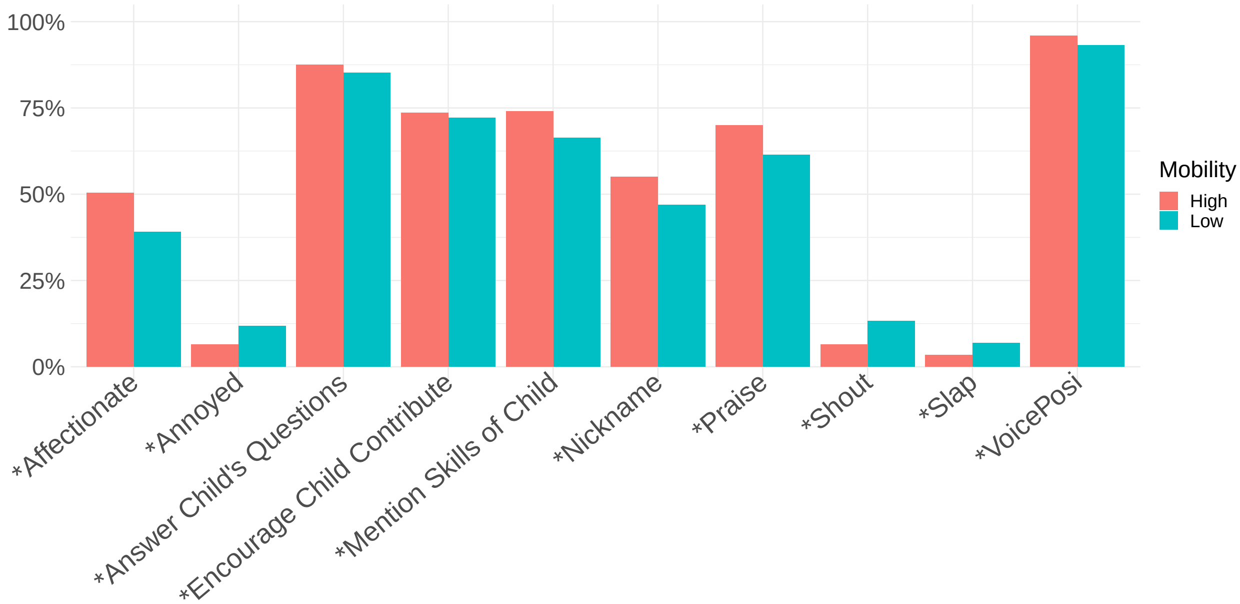


Figure 27: Average interviewer-observed parenting across neighborhood types: Economic mobility



A.2.3 Correlations with family characteristics

The following set of figures display how average parenting varies by the race, education, and income level of the parent.

Figure 28: Average self-reported parenting across family types: Race of parent

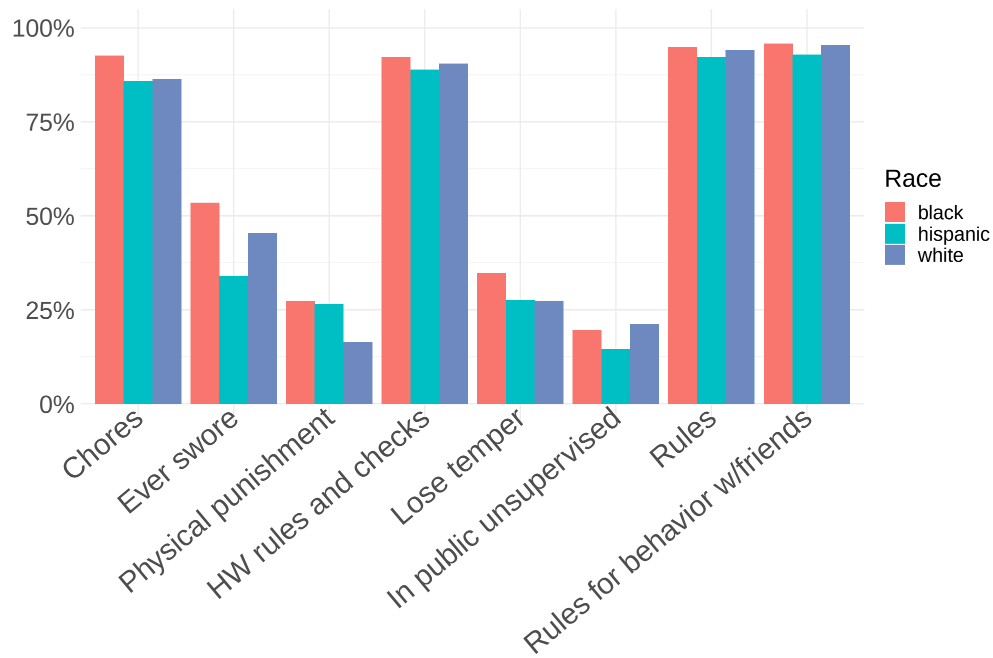


Figure 29: Average interviewer-observed parenting across family types: Race of parent

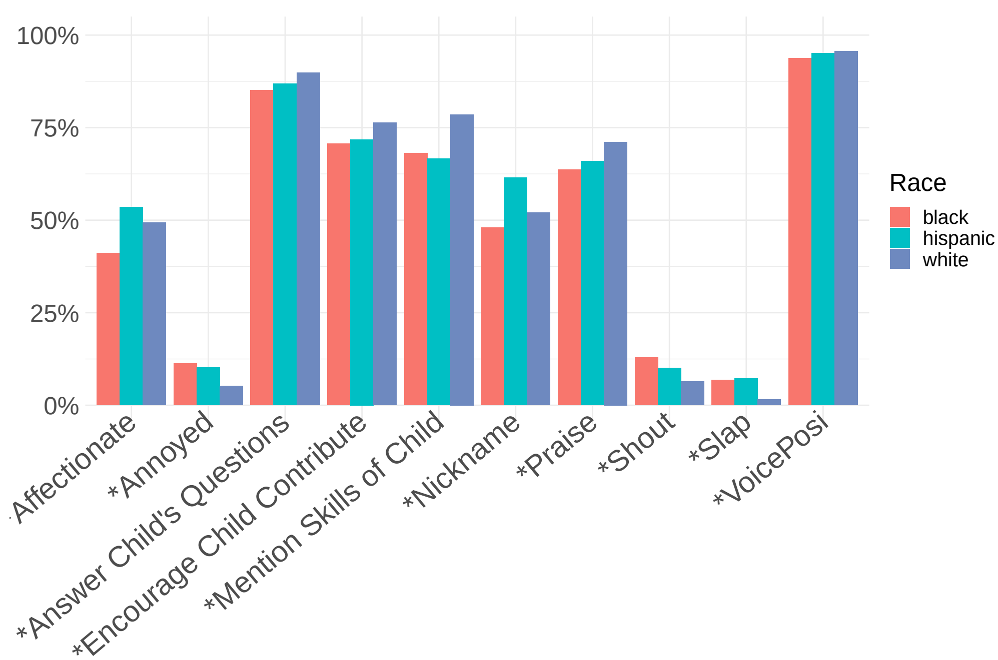


Figure 30: Average self-reported parenting across family types: Education of parent

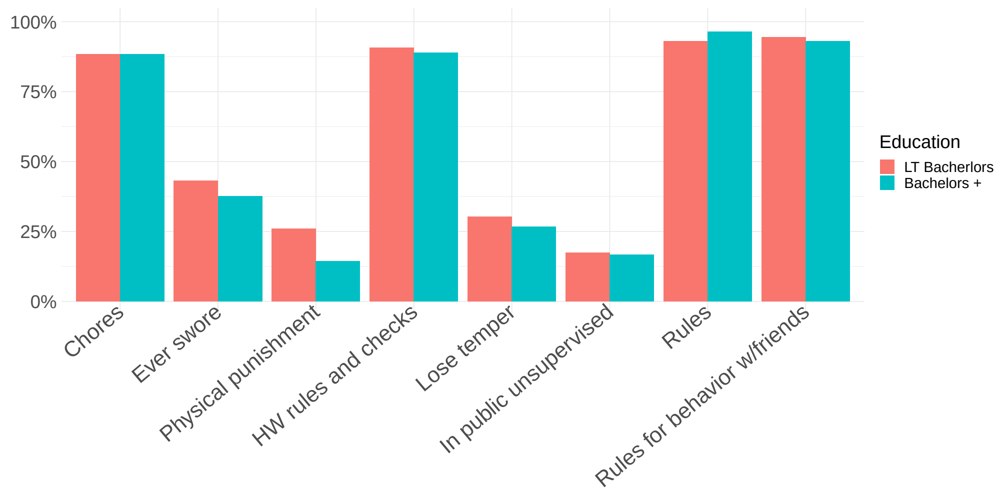


Figure 31: Average interviewer-observed parenting across family types: Education of parent

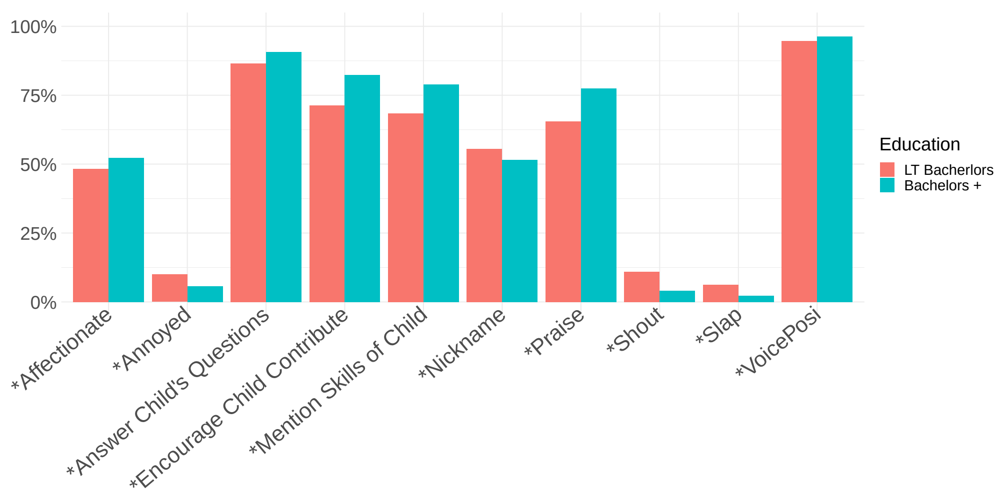


Figure 32: Average self-reported parenting across family types: Household income

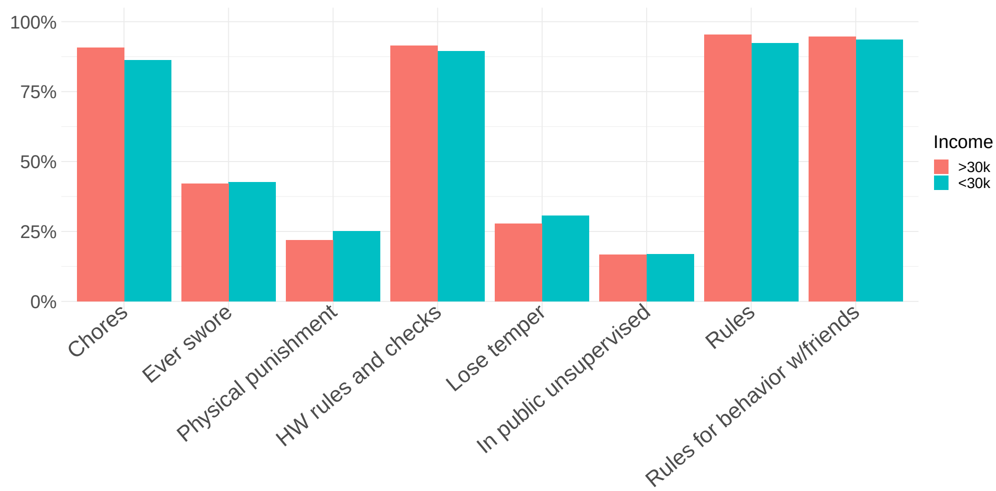
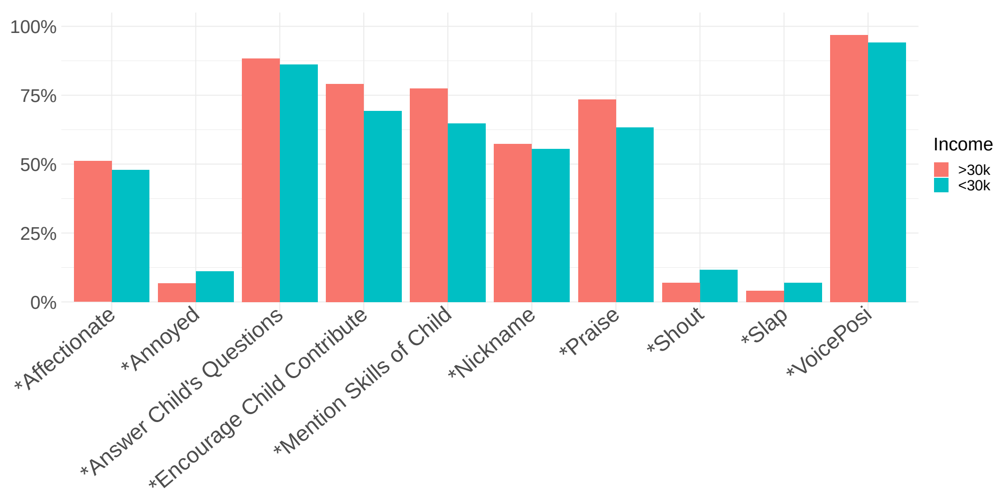


Figure 33: Average interviewer-observed parenting across family types: Household income



B

APPENDIX: ADDITIONAL TABLES AND FIGURES SUPPORTING ANALYSIS

In this section I display tables containing the point estimates and standard errors underlying the figures in the main body of the report. I also display additional robustness checks.

B.1 Main research design

In Table B.1 I report the underlying point estimates and standard errors to the figures in Section 6.

Table B.1: Point estimates underlying main analysis

	Variance	Neighbor Covariance	Sibling Covariance	Upper Bound Neighborhood Effects	Lower Bound Family Effects	Lower Bound Idiosyncratic Effects
	[a]	[b]	[c]	[d]	[e]	[f]
Chores	0.099***	0.001***	0.033***	0.005	0.323***	0.661***
	(0.005)	(0)	(0.006)	(0.005)	(0.061)	(0.061)
Ever Swore	0.235***	0.006***	0.138***	0.026***	0.562***	0.359***
	(0.003)	(0.002)	(0.008)	(0.007)	(0.034)	(0.035)
High Physical Punishment	0.082***	0***	0.018**	0.001	0.218***	0.779***
	(0.005)	(0.001)	(0.007)	(0.007)	(0.075)	(0.075)
HW Rules and Checks	0.186***	0.003***	0.036***	0.018**	0.175***	0.773***
	(0.005)	(0.001)	(0.007)	(0.006)	(0.038)	(0.04)
Lost Temper > 1 Past Week	0.21***	0.002	0.061***	0.009	0.283***	0.69***
	(0.004)	(0.002)	(0.016)	(0.007)	(0.074)	(0.076)
Public Without Supervision	0.138***	0.001***	0.035***	0.009	0.247***	0.726***
	(0.005)	(0.001)	(0.007)	(0.006)	(0.048)	(0.05)
Rules	0.062***	0	0.013***	0.002	0.204***	0.79***
	(0.004)	(0)	(0.004)	(0.004)	(0.059)	(0.06)
Rules for Behavior with Friends	0.055***	0	0.01*	0	0.187***	0.814***
	(0.004)	(0)	(0.005)	(0.004)	(0.08)	(0.08)
*Affectionate	0.086***	0.001***	0.045***	0.009	0.517***	0.456***
	(0.005)	(0)	(0.008)	(0.005)	(0.075)	(0.076)
*Annoyed	0.113***	0.004***	0.046***	0.034***	0.376***	0.521***
	(0.006)	(0.001)	(0.007)	(0.01)	(0.062)	(0.066)
*Answers Child's Questions	0.198***	0.002**	0.115***	0.01***	0.571***	0.398***
	(0.004)	(0.001)	(0.007)	(0.005)	(0.038)	(0.04)
*Encourage Child to Contribute	0.214***	0.005***	0.134***	0.023***	0.605***	0.326***
	(0.004)	(0.001)	(0.009)	(0.006)	(0.037)	(0.039)
*MentionsSkills	0.234***	0.009***	0.134***	0.038***	0.535***	0.352***
	(0.002)	(0.003)	(0.008)	(0.014)	(0.034)	(0.041)
*Nickname	0.223***	0.006***	0.133***	0.028***	0.568***	0.346***
	(0.004)	(0.002)	(0.008)	(0.008)	(0.036)	(0.038)
*Praises	0.09***	0.001***	0.057***	0.013***	0.621***	0.341***
	(0.005)	(0)	(0.008)	(0.005)	(0.075)	(0.075)
*Shouts	0.055***	0	0.039***	0.004	0.692***	0.295***
	(0.004)	(0)	(0.007)	(0.004)	(0.108)	(0.109)
*Slaps	0.049***	0	0.03***	0.002	0.613***	0.38***
	(0.004)	(0)	(0.005)	(0.004)	(0.097)	(0.098)
*Voice Positive	0.202***	0.006***	0.099***	0.03***	0.459***	0.453***
	(0.003)	(0.002)	(0.007)	(0.009)	(0.036)	(0.041)
**Authoritative	104.673***	6.029***	83.605***	0.058***	0.741***	0.086
	(3.386)	(1.964)	(8.758)	(0.019)	(0.075)	(0.085)
** Neglectful	73.955***	1.385***	49.634***	0.019**	0.652***	0.291***
	(2.542)	(0.555)	(5.976)	(0.008)	(0.069)	(0.071)
** Permissive	50.968***	1.716***	22.063***	0.034***	0.399***	0.5***
	(1.515)	(0.447)	(3.399)	(0.009)	(0.063)	(0.065)
** Authoritarian	43.63***	0.489	15.765***	0.011	0.35***	0.616***
	(1.679)	(0.394)	(2.765)	(0.009)	(0.06)	(0.063)
** No-nonsense	34.468***	0.251	14.285***	0.007	0.407***	0.571***
	(1.257)	(0.191)	(2.588)	(0.005)	(0.068)	(0.067)
Weighted Average				0.017	0.448	0.501

NOTE: *p<0.1, ** p< 0.05, *** p< 0.001.
Column [d] is equal to column [b]/[a]. Column [e] is equal to column [c]/[a] - [d]. Column [f] is equal to 1-[c]/[a]-2*[b]/[a].

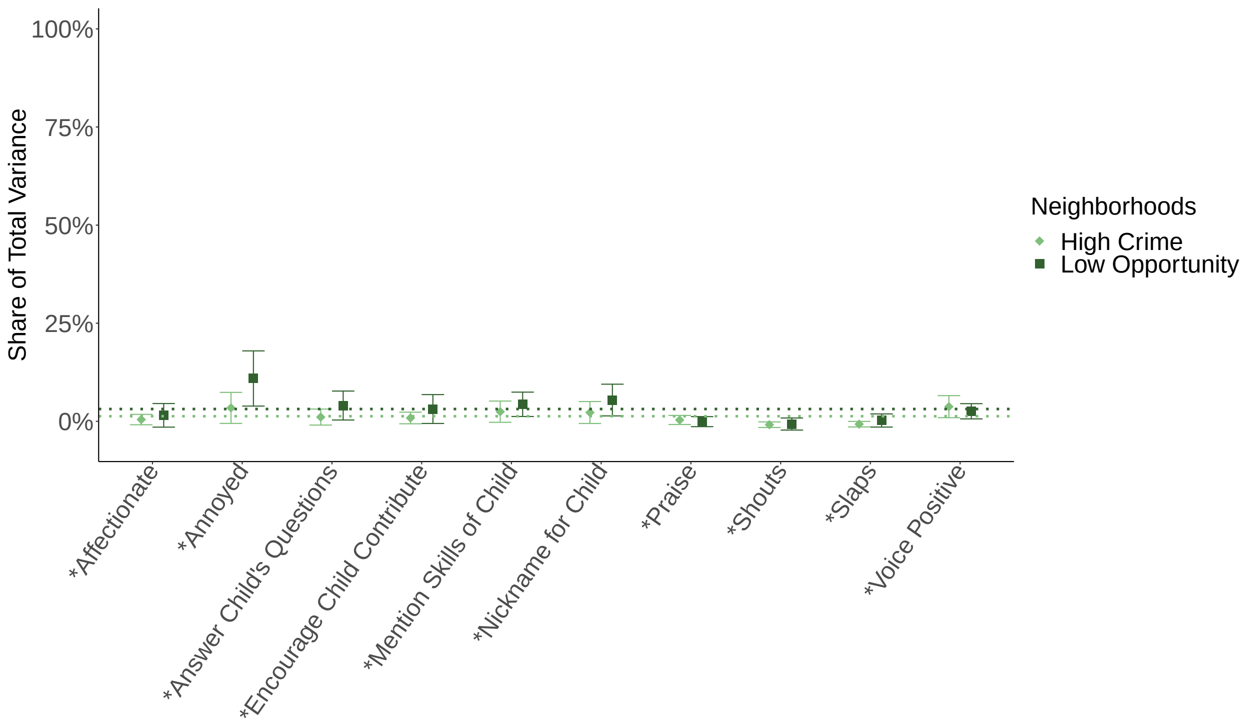
B.2 Heterogeneity analysis

In this section I display additional results on the upper bound on neighborhood effects across neighborhood and family characteristics.

B.2.1 Neighborhood characteristics

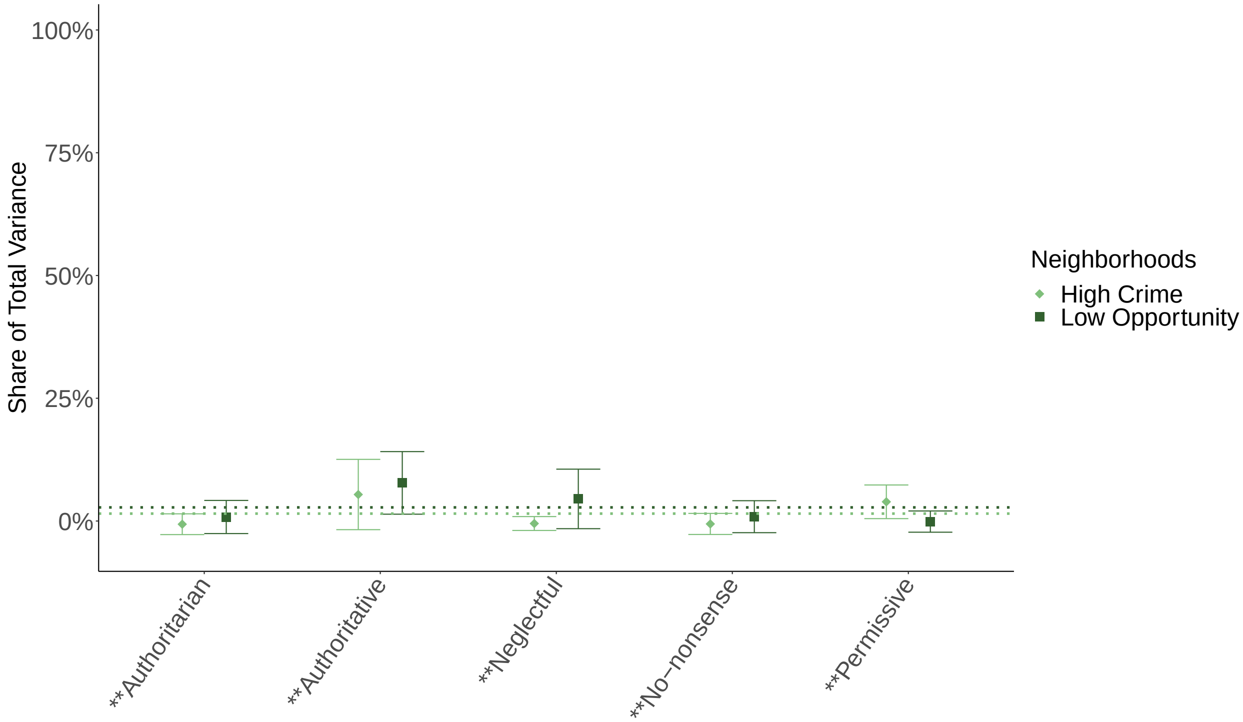
This section contains graphs for interviewer-observed parenting and latent parenting styles.

Figure 34: Share of variance in parenting explained by neighborhood effects in high-crime or low-opportunity neighborhoods: Interviewer-observed parenting behaviors



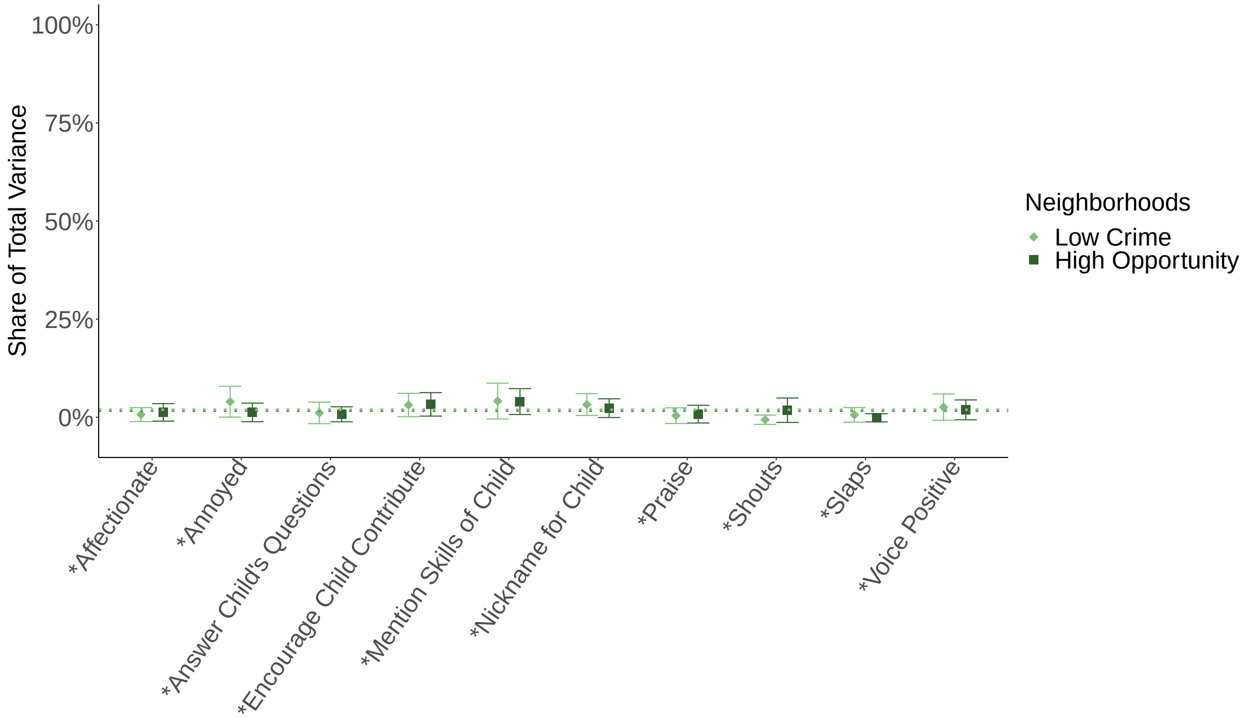
Note: In this figure I report the share of variance in parenting explained by causal neighborhood effects on interviewer-observed parenting behaviors in neighborhoods characterized by high homicide rates and low economic opportunity. The dashed lines reflects the respective weighted averages across interviewer-observed parenting behaviors. I weight by precision using the inverse of standard errors. The average upper bound on neighborhood effects for high crime areas is 1% and for low-opportunity is 3%.

Figure 35: Share of variance in parenting explained by neighborhood effects in high-crime or low-opportunity neighborhoods: Latent parenting styles



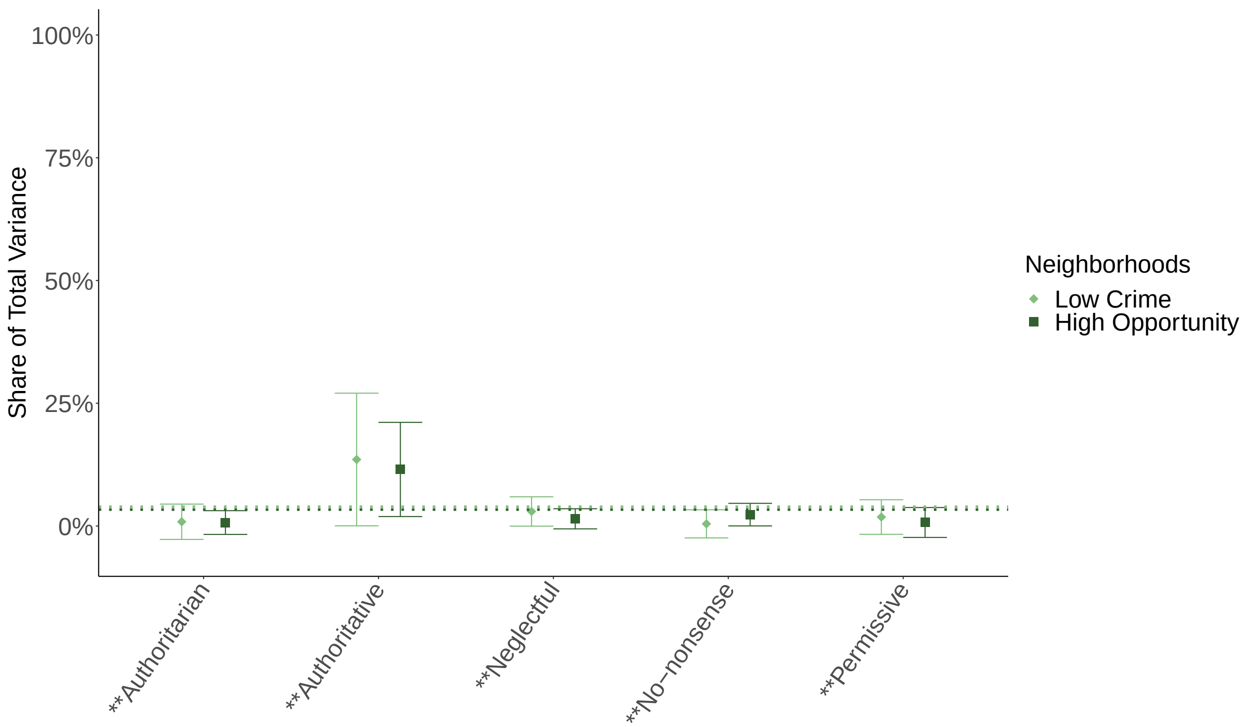
NOTE: In this figure I report the share of variance in parenting explained by causal neighborhood effects on latent parenting styles in neighborhoods characterized by high homicide rates and low economic opportunity. The dashed lines reflect the respective weighted averages across latent parenting styles. I weight by precision using the inverse of standard errors. The average upper bound on neighborhood effects for high crime areas is 2% and for low-opportunity is 3%.

Figure 36: Share of variance in parenting explained by neighborhood effects in low-crime or high-opportunity neighborhoods: Interviewer-observed parenting behaviors



NOTE: In this figure I report the share of variance in parenting explained by causal neighborhood effects on interviewer-observed parenting behaviors in neighborhoods characterized by low homicide rates and high economic opportunity. The dashed lines reflect the respective weighted averages across interviewer-observed parenting behaviors. I weight by precision using the inverse of standard errors. The average upper bound on neighborhood effects for low crime areas is 2% and for high-opportunity is 2%.

Figure 37: Share of variance in parenting explained by neighborhood effects in low-crime or high-opportunity neighborhoods: Latent parenting styles



NOTE: In this figure I report the share of variance in parenting explained by causal neighborhood effects on latent parenting styles in neighborhoods characterized by low homicide rates and high economic opportunity. The dashed lines reflect the respective weighted averages across latent parenting styles. I weight by precision using the inverse of standard errors. The average upper bound on neighborhood effects for low crime areas is 4% and for high-opportunity is 3%.

In Table B.2 I report the underlying point estimates and standard errors to the estimates of the upper bound on neighborhood effects displayed in the figures in Section 7 and this appendix.

Table B.2: Point estimates underlying heterogeneity analysis

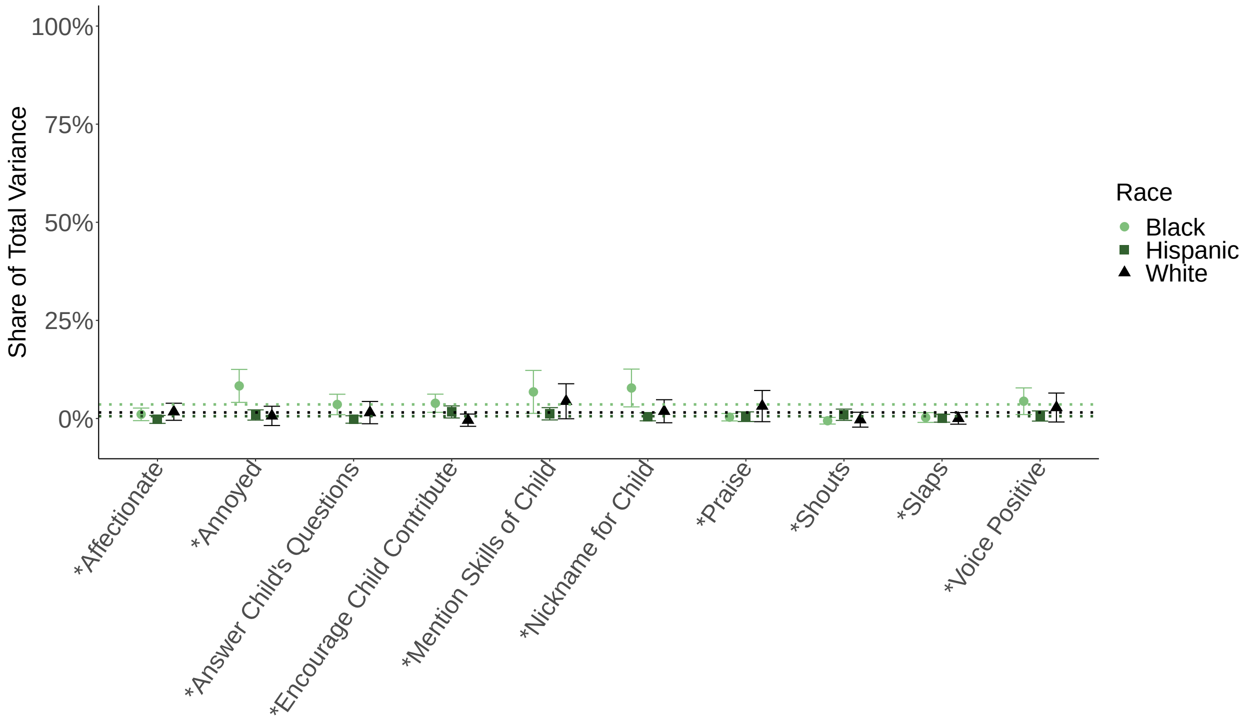
	Bad Neighborhoods		Good Neighborhoods		Race of Parent		
	High Crime	Low Opportunity	Low Crime	High Opportunity	Black	Hispanic	White
Chores	0.006	0.002	-0.009*	-0.01***	0.021	0.028	0.033
	(0.008)	(0.011)	(0.005)	(0.005)	(0.029)	(0.022)	(0.045)
Ever Swore	0.041***	0.004	-0.006	0.019	0.001	0.013	0.087**
	(0.015)	(0.011)	(0.006)	(0.016)	(0.023)	(0.026)	(0.043)
High Physical Punishment	-0.004	-0.027***	0.011	0.015	0.033	0.045	-0.063
	(0.014)	(0.003)	(0.018)	(0.018)	(0.068)	(0.039)	(0.056)
HW Rules and Checks	0.006	0.005	0.053*	0.04***	0.072**	0.01	-0.018
	(0.006)	(0.009)	(0.028)	(0.017)	(0.034)	(0.014)	(0.01)
Lost Temper > 1 Past Week	0.036**	0.014	-0.002	0.02**	0.009	-0.032	-0.053
	(0.023)	(0.016)	(0.012)	(0.014)	(0.041)	(0.04)	(0.037)
Public Without Supervision	-0.004	-0.005	0.062	0.034***	0.003	0.045	0.115
	(0.006)	(0.008)	(0.041)	(0.021)	(0.027)	(0.033)	(0.106)
Rules	-0.005	-0.007	0.016	0.012	0.006	0.029	-0.017
	(0.004)	(0.005)	(0.014)	(0.013)	(0.021)	(0.017)	(0.016)
Rules for Behavior with Friends	0	0.002	-0.007	-0.005	-0.022	0.021	-0.045**
	(0.006)	(0.009)	(0.006)	(0.005)	(0.035)	(0.02)	(0.019)
*Affectionate	0.004	0.015	0.007	0.012	0.01	-0.007	0.01
	(0.007)	(0.015)	(0.009)	(0.011)	(0.017)	(0.008)	(0.011)
*Annoyed	0.034	0.109***	0.04**	0.012	0.044	0.051**	-0.005
	(0.02)	(0.035)	(0.02)	(0.012)	(0.031)	(0.028)	(0.014)
*Answers Child's Questions	0.011	0.04**	0.011	0.008	-0.015	-0.01	0.029
	(0.01)	(0.018)	(0.014)	(0.01)	(0.019)	(0.012)	(0.023)
*Encourage Child to Contribute	0.009	0.031**	0.031**	0.033**	0.01	-0.006	0.006
	(0.007)	(0.018)	(0.015)	(0.015)	(0.019)	(0.014)	(0.015)
*MentionsSkills	0.025*	0.043***	0.041*	0.04***	0.002	0.007	0.097
	(0.014)	(0.016)	(0.023)	(0.017)	(0.016)	(0.018)	(0.078)
*Nickname	0.022	0.054***	0.032**	0.023**	0.021	-0.008	0.057*
	(0.014)	(0.02)	(0.014)	(0.012)	(0.021)	(0.014)	(0.033)
*Praises	0.003	-0.001	0.004	0.008	0.01	0.005	0.056***
	(0.006)	(0.006)	(0.01)	(0.011)	(0.024)	(0.016)	(0.021)
*Shouts	-0.009	-0.007	-0.006	0.018	0.037	0	-0.009
	(0.004)	(0.008)	(0.006)	(0.016)	(0.026)	(0.013)	(0.022)
*Slaps	-0.007	0.002	0.006	-0.001	-0.007	-0.019	0.008
	(0.004)	(0.008)	(0.009)	(0.005)	(0.015)	(0.01)	(0.017)
*Voice Positive	0.037**	0.026***	0.026	0.019**	-0.011	-0.029***	-0.017
	(0.014)	(0.01)	(0.017)	(0.013)	(0.016)	(0.009)	(0.029)
**Authoritative	0.054*	0.078***	0.135*	0.115***	0.021*	-0.018*	0.01
	(0.036)	(0.032)	(0.068)	(0.048)	(0.012)	(0.01)	(0.017)
** Neglectful	-0.005	0.045	0.03**	0.015	0.013	-0.014**	0.056
	(0.007)	(0.03)	(0.015)	(0.01)	(0.02)	(0.007)	(0.05)
** Permissive	0.039**	-0.001	0.018	0.007	0.037**	0.014	0.027
	(0.017)	(0.011)	(0.018)	(0.015)	(0.017)	(0.013)	(0.016)
** Authoritarian	-0.006	0.008	0.009	0.007	-0.013	0.013	-0.002
	(0.011)	(0.017)	(0.018)	(0.012)	(0.008)	(0.01)	(0.01)
** No-nonsense	-0.006	0.009	0.004	0.023**	0.013	0.012	-0.004
	(0.011)	(0.016)	(0.014)	(0.011)	(0.009)	(0.01)	(0.009)

NOTE: *p<0.1, ** p< 0.05, *** p< 0.001.

B.2.2 Family characteristics

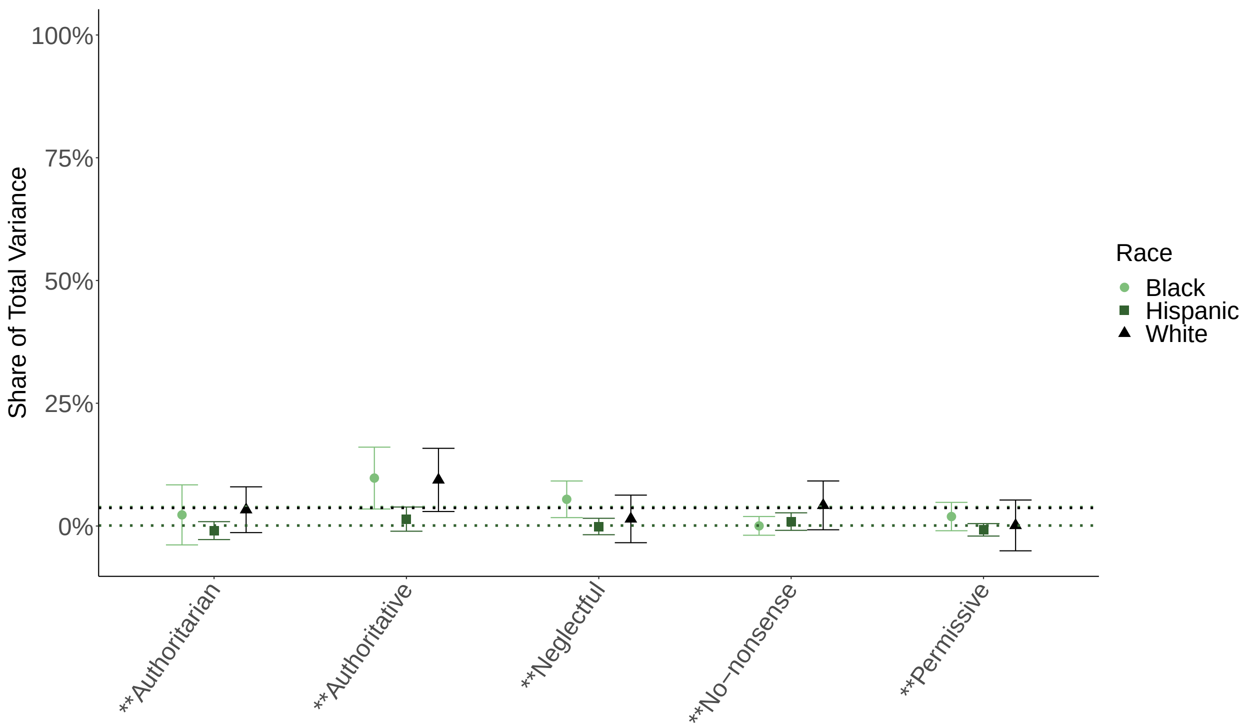
This section contains graphs for interviewer-observed parenting and latent parenting styles.

Figure 38: Share of variance in parenting explained by neighborhood effects by race of the parent: Interviewer-observed parenting behaviors



NOTE: In this figure I report the share of variance in parenting explained by causal neighborhood effects on interviewer-observed behaviors by race of the parent.

Figure 39: Share of variance in parenting explained by neighborhood effects by race of the parent: Latent parenting styles

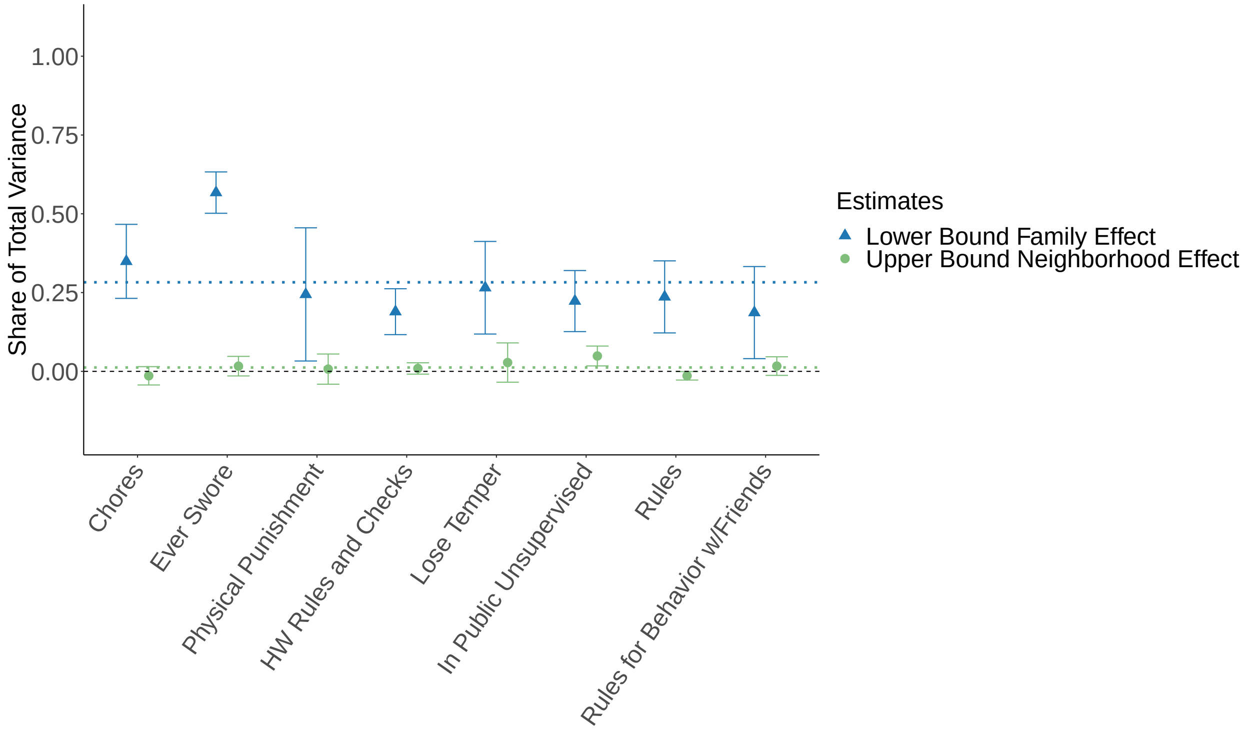


NOTE: In this figure I report the share of variance in parenting explained by causal neighborhood effects on latent parenting styles by race of the parent.

B.3 Restricted sample

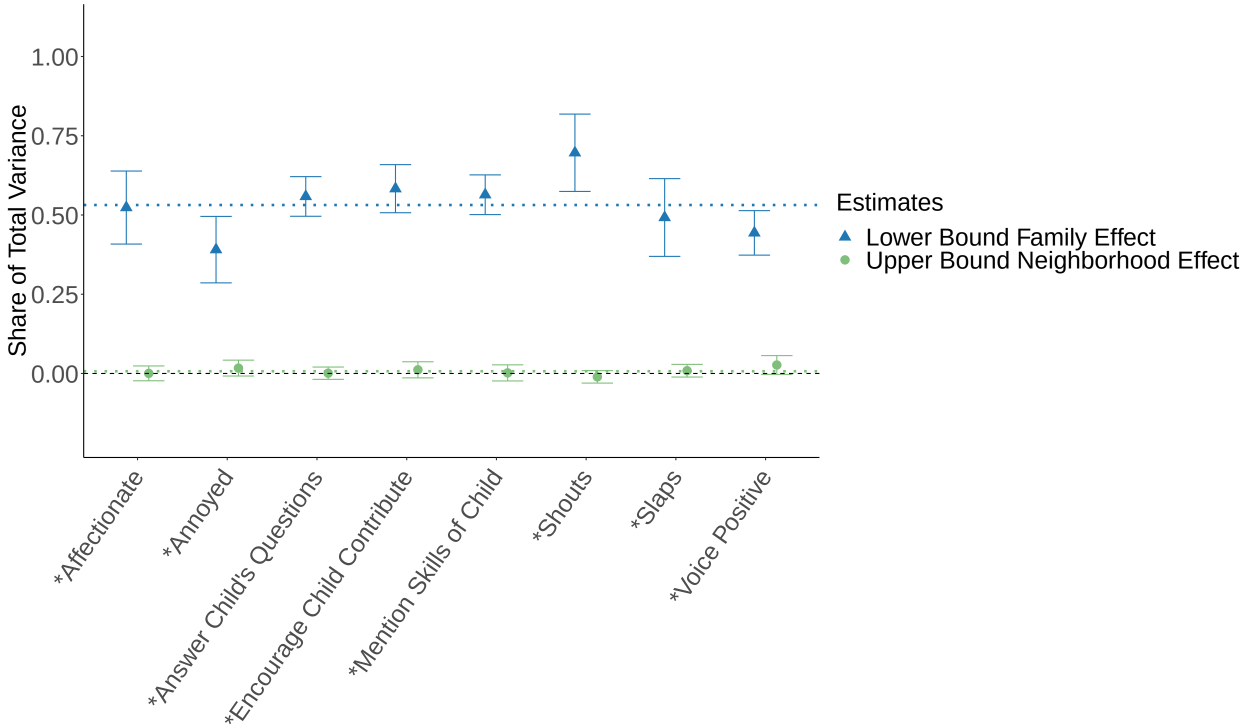
In Section 6 I discuss how the sibling covariance is estimated off a sub-sample of that used to estimate neighbor covariances and variances. In the graphs below I restrict the neighbor and covariance estimation to be based on the same sub-sample, that which was initially used to estimate sibling covariances. Standard errors increase but the key takeaways do not change.

Figure 40: Share of variance in parenting explained by family and neighborhood effects: Self-reported parenting behaviors in restricted sample



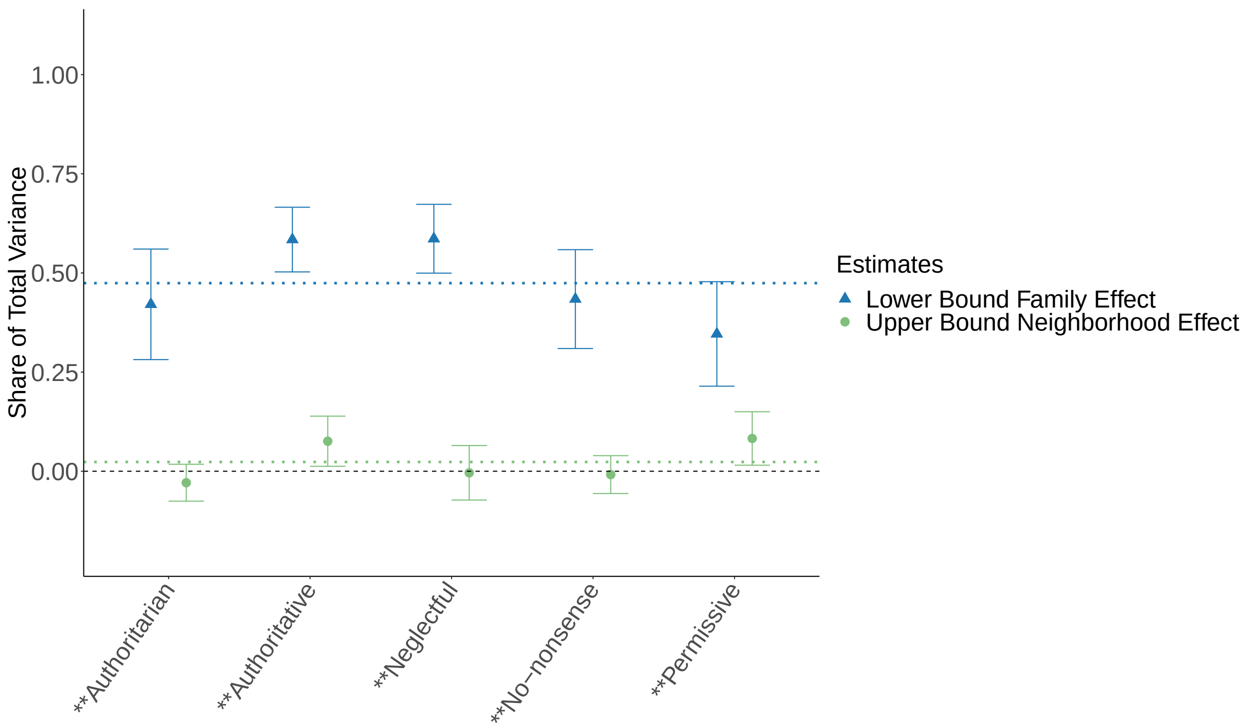
NOTE: This graph replicates Figure 4 restricting all estimates for self-reported parenting behaviors to be calculated from the same sample.

Figure 41: Share of variance in parenting explained by family and neighborhood effects: Interviewer-observed parenting behaviors in restricted sample



NOTE: This graph replicates Figure 5 restricting all estimates for interviewer-observed parenting behaviors to be calculated from the same sample.

Figure 42: Share of variance in parenting explained by family and neighborhood effects: Interviewer-observed parenting behaviors in restricted sample

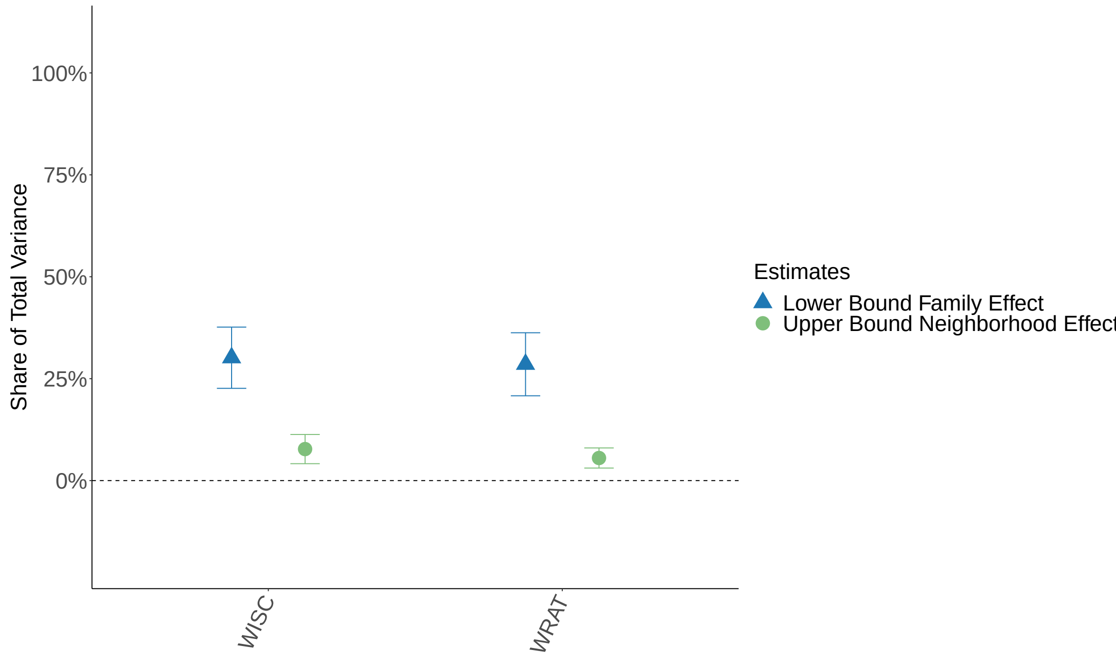


NOTE: This graph replicates Figure 6 restricting all estimates for latent parenting styles to be calculated from the same sample.

B.4 Additional outcomes

In the graphs below I run the sibling and neighbor covariance analysis for outcomes that are more comparable to the existing literature that uses sibling and neighbor covariances. An overview can be found in Mogstad and Torsvik [2023]. I look at two test scores that were administered as part of the PHDCN that capture the cognitive skills likely to predict schooling outcomes and adult earnings. The lower bound on neighborhood effects is statistically distinguishable from zero, yet small, which is consistent with the existing literature using the sibling and neighbor covariance methodology.

Figure 43: Share of variance in cognitive skills explained by family and neighborhood effects



NOTE: This graph replicates Figure 4 for outcome variables capturing the child's cognitive skills. The lower bound on family effects on WISC has a point estimate of 0.301 and standard error of 0.038. The upper bound on neighborhood effects on WISC has a point estimate of 0.077 and standard error of 0.012. The lower bound on family effects on WRAT has a point estimate of 0.285 and standard error of 0.039. The upper bound on neighborhood effects on WRAT has a point estimate of 0.056 and standard error of 0.012.

B.5 Mover design

In Table B.3 I report the underlying point estimates and standard errors to the figures in Section 8.

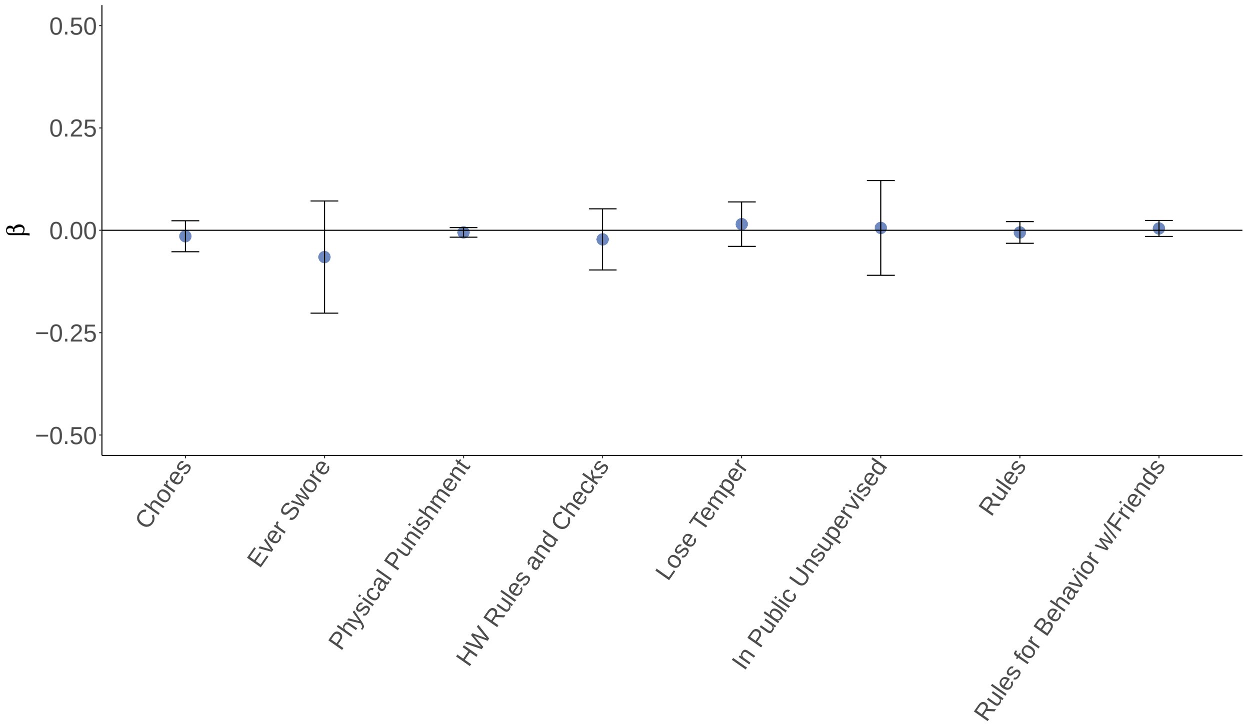
Table B.3: Point estimates underlying mover analysis

	Bad Neighborhoods		Good Neighborhoods	
	High Crime	Low Opportunity	Low Crime	High Opportunity
Chores	0.025	0.006	-0.015	0.009
	(0.016)	(0.020)	(0.019)	(0.021)
Ever Swore	-0.030	-0.052	-0.065	0.073
	(0.063)	(0.078)	(0.069)	(0.079)
High Physical Punishment	0.0004	-0.004	-0.005	-0.002
	(0.005)	(0.007)	(0.006)	(0.007)
HW Rules and Checks	0.017	0.030	-0.022	-0.069*
	(0.032)	(0.041)	(0.037)	(0.041)
Lost Temper > 1 Past Week	0.014	0.057*	0.015	0.046
	(0.026)	(0.034)	(0.027)	(0.031)
Public Without Supervision	0.052	-0.038	0.006	0.104
	(0.048)	(0.058)	(0.058)	(0.079)
Rules	0.005	-0.004	-0.005	-0.025
	(0.012)	(0.016)	(0.013)	(0.016)
Rules for Behavior with Friends	0.002	-0.013	0.004	-0.006
	(0.008)	(0.010)	(0.010)	(0.011)
*Affectionate	0.024	-0.183	0.068	(0.023)
	(0.103)	(0.118)	(0.108)	(0.104)
*Annoyed	-0.02	0.034	-0.005	-0.030
	(0.016)	(0.021)	(0.017)	((0.020)
*Answers Child's Questions	-0.015	-0.007	-0.001	-0.036
	(0.022)	(0.028)	(0.023)	(0.025)
*Encourage Child to Contribute	0.207*	-0.085	0.014	-0.158
	(0.114)	(0.122)	(0.113)	(0.110)
*MentionsSkills	0.030	-0.014	-0.014	-0.044*
	(0.020)	(0.025)	(0.020)	(0.024)
*Nickname	0.007	0.042	-0.095	-0.047
	(0.068)	(0.086)	(0.074)	(0.083)
*Praises	0.022**	-0.007	-0.018*	-0.018
	(0.010)	(0.012)	(0.010)	(0.011)
*Shouts	0.002	0.038**	0.001	-0.025*
	(0.013)	(0.017)	(0.013)	(0.015)
*Slaps	-0.006	0.010	0.004	-0.018
	(0.012)	(0.016)	(0.013)	(0.014)
*Voice Positive	0.003	-0.023	-0.013	-0.033**
	(0.012)	(0.015)	(0.013)	(0.014)
**Authoritative	0.277*	-0.309	-0.333**	-0.0138
	(0.146)	(0.199)	(0.170)	(0.194)
** Neglectful	-0.045	-0.177	0.087	-0.066
	(0.145)	(0.195)	(0.166)	(0.187)
** Permissive	-0.321**	-0.186	-0.013	-0.030
	(0.144)	(0.188)	(0.165)	(0.181)
** Authoritarian	0.017	0.302	0.268	0.053
	(0.158)	(0.213)	(0.183)	(0.204)
** No-nonsense	0.014	0.029	0.168	0.406*
	(0.166)	(0.218)	(0.194)	(0.210)

NOTE: *p<0.1, ** p< 0.05, *** p< 0.001.

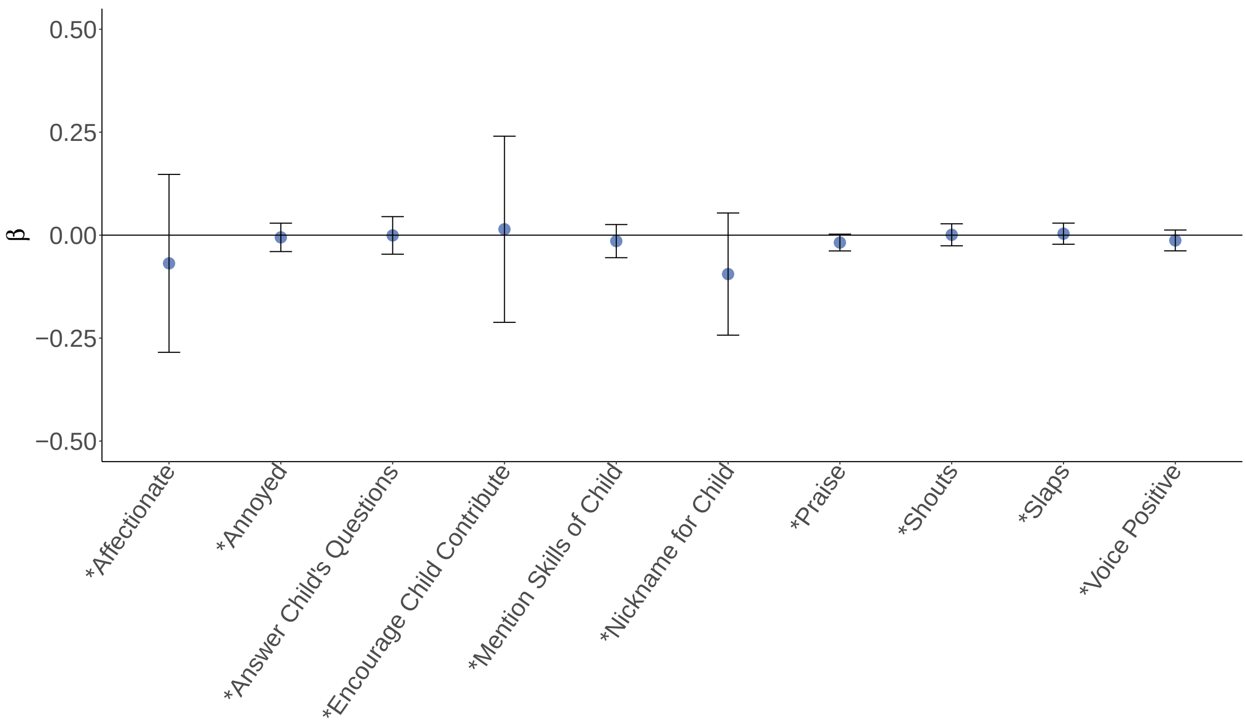
Below are figures for moves to low-crime and high-opportunity areas.

Figure 44: Impact of moving to a safe neighborhood on self-reported parenting



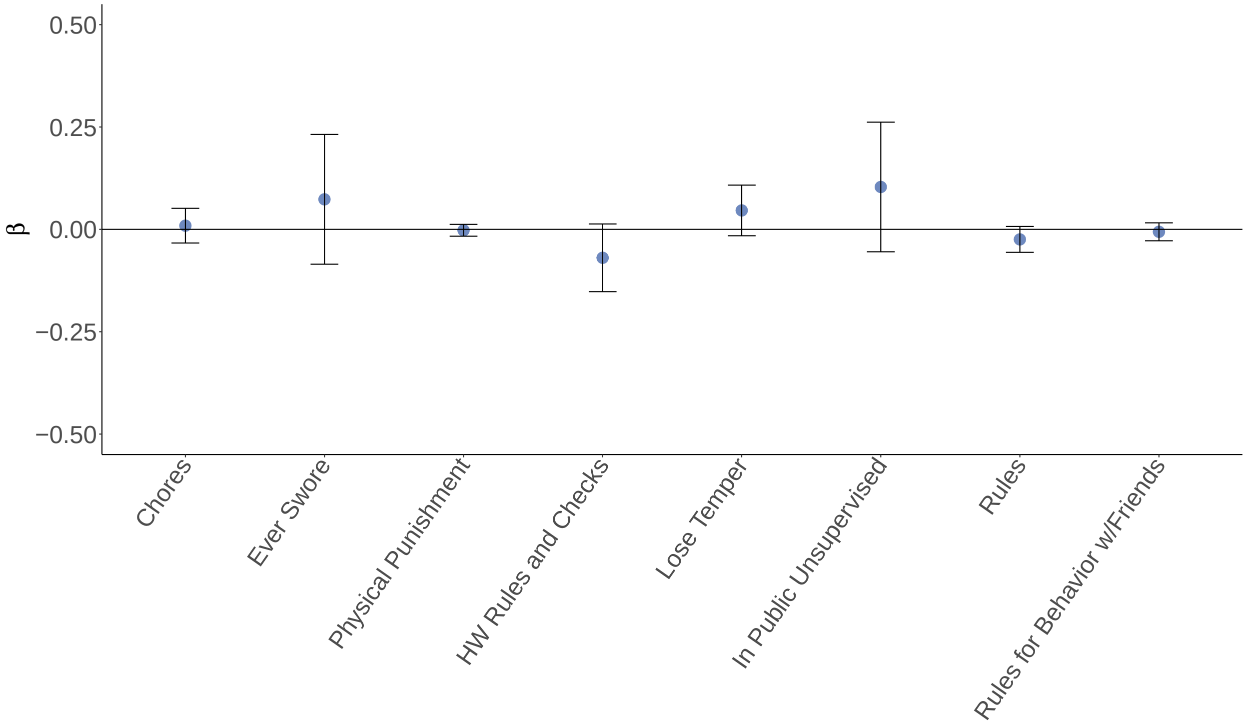
NOTE: This figure plots estimates of the causal impact of moving to a low-crime neighborhood on self-reported parenting. Low-crime neighborhoods are those where the average annual homicide rate in the two years prior to the survey wave is in the bottom 25 percentile. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and a fixed effects for the survey wave. I regress the residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

Figure 45: Impact of moving to a safe neighborhood on interviewer-observed parenting



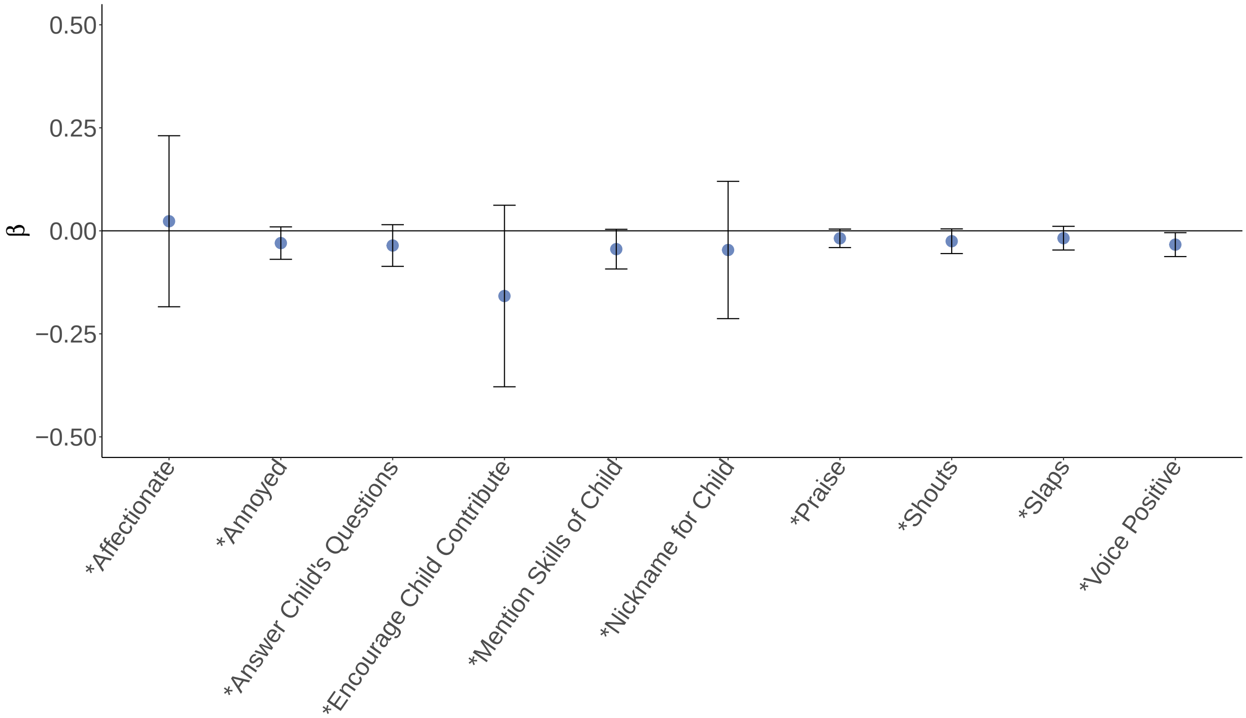
NOTE: This figure plots estimates of the causal impact of moving to a low-crime neighborhood on interviewer-observed parenting. Low-crime neighborhoods are those where the average annual homicide rate in the two years prior to the survey wave is in the bottom 25 percentile. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and a fixed effects for the survey wave. I regress the residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

Figure 46: Impact of moving to a high opportunity area on self-reported parenting



NOTE: This figure plots estimates of the causal impact of moving to a high-opportunity neighborhood on parenting. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and a fixed effects for the survey wave. I regress the residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

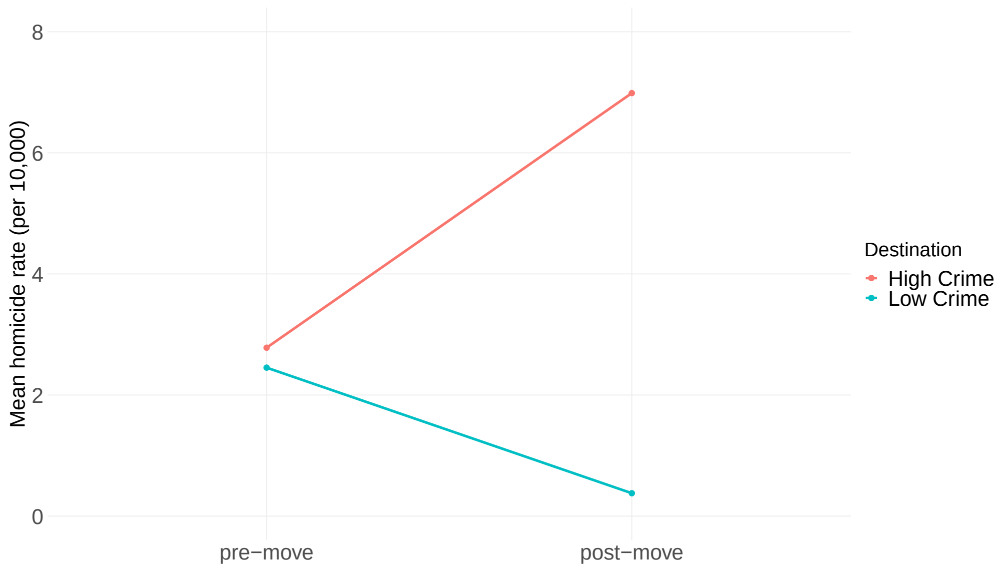
Figure 47: Impact of moving to a high opportunity area on interviewer-observed behavior



NOTE: This figure plots estimates of the causal impact of moving to a high-opportunity neighborhood on parenting. I adjust the measurements of parenting for the age, sex, and birth order of the child and the survey wave by regressing parenting on a quadratic function of age of the child, indicators for sex and being the oldest child, and a fixed effects for the survey wave. I regress the residualized parenting measurement on a set of indicator variables for whether the parent experienced a change in education, marriage, employment, or income status.

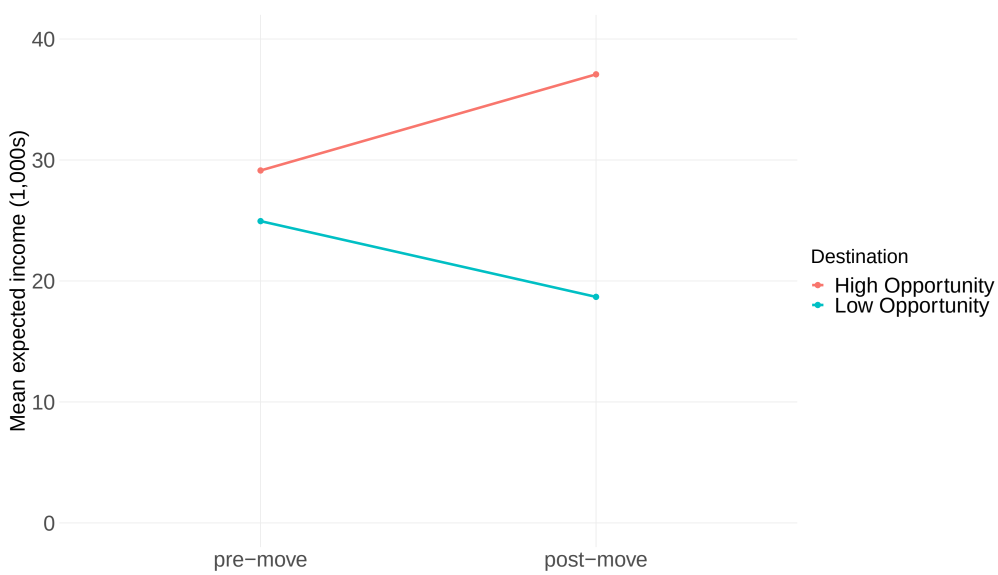
In the next set of figures I show how crime and economic opportunity changes with different move types.

Figure 48: Average homicide rates in origin and destination neighborhoods, by move type



NOTE: This figure plots the average annual crime rate in origin and destination neighborhoods for moves into high or low crime neighborhoods.

Figure 49: Average expected income in origin and destination neighborhoods, by move type



NOTE: This figure plots the mean income in origin and destination neighborhoods for moves into high or low opportunity neighborhoods.

The movers estimates are based on the following counts of move types in the data.

	In	Out	**Total**
Bad neighborhoods			
High Crime	174	213	**387**
Low Opportunity	124	98	**222**
Good neighborhoods			
Low Crime	220	127	**347**
High Opportunity	170	74	**244**

Table B.4: Counts of movers by move type

NOTE: This table reports on the move counts that underlie estimates in Section 8.

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