Federal Reserve Bank of Cleveland Working Paper Series

Childhood Exposure to Violence and Nurturing Relationships: The Long-Run Effects on Black Men

Dionissi Aliprantis and Kristen N. Tauber

Working Paper No. 23-16

July 2023

Suggested citation: Aliprantis, Dionissi and Kristen N. Tauber. 2023. "Childhood Exposure to Violence and Nurturing Relationships: The Long-Run Effects on Black Men." Working Paper No. 23-16. Federal Reserve Bank of Cleveland. https://doi.org/10.26509/frbc-wp-202316.

Federal Reserve Bank of Cleveland Working Paper Series
ISSN: 2573-7953

Childhood Exposure to Violence and Nurturing Relationships: The Long-Run Effects on Black Men

Dionissi Aliprantis* Kristen Tauber**

July 6, 2023

Abstract: Black men who witnessed a shooting before turning 12 have household earnings as adults 31 percent lower than those who did not. We present evidence that this gap is causal and is most likely the result of toxic stress; it is not mediated by incarceration and is constant across neighborhood socioeconomic status. Turning to mechanisms related to toxic stress, we study exposure to violence and nurturing relationships during adolescence. Item-anchored indexes synthesize variables on these treatments better than summing positive responses, Item Response Theory, or Principal Components, which all perform similarly. Providing adolescents with nurturing relationships is almost as beneficial as preventing their exposure to violence.

Keywords: Interpersonal Violence, Code of the Street, Toxic Stress, Nurturing Relationship, Race, Neighborhood Effect

JEL Classification Codes: H40, I38, J15, J24, R23

*: Federal Reserve Bank of Cleveland, +1(216)579-3021, *dionissi.aliprantis@clev.frb.org*.

**: New York University, *knt2029@nyu.edu*.

We thank Stephanie Tulley, Clint Carter, and Jennifer Cassidy-Gilbert for assistance accessing the data used in this paper. For helpful comments we thank Christina Bethell, Dan Black, Daniel Carroll, Steven Durlauf, Bruce Fallick, Andrew Garner, Hal Martin, Matt Masten, Lock Reynolds, Lowell Taylor, as well as seminar participants at the Cleveland Fed and the Paris School of Economics 2023 Workshop on Neighborhoods and Local Interactions. This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS. The opinions expressed are those of the authors and do not necessarily represent the views of the Federal Reserve Bank of Cleveland or the Board of Governors of the Federal Reserve System.

1 Introduction

Navigating a violent environment is psychologically costly. These costs limit the well-being and human capital accumulation of children.

Violence is particularly consequential for Black males in the United States because of the disproportionate rate at which Black children experience violent environments.[1] More than a quarter of Black men witnessed a shooting as a child, four times the rate of their white peers (Graham (2018)). Young Black males are killed at eight times the rate of their white peers.[2]

Short-run effects are a clear measure of the costs of Black males' exposure to violence. Black males who witnessed a shooting in their childhoods are twice as likely to engage in violence themselves at age 15 and 13 percentage points more likely to drop out of high school (Aliprantis (2017b); Bingenheimer et al. (2005)). We also know that Black students' academic performance declines after violent crime near their schools (Torrats-Espinosa (2020); Casey et al. (2018)) and in their neighborhoods (Sharkey et al. (2014)), with the latter effect mediated by school safety (Laurito et al. (2019)).

This paper studies the long-run effects of early exposure to violence on the outcomes of Black men using the National Longitudinal Survey of Youth 1997 (NLSY97). The first contribution of this paper is a treatment effect analysis of exposure to violence during childhood (before age 12). We document large gaps in adult outcomes between Black men who witnessed a shooting during their childhoods and those who did not. We give these gaps a causal interpretation using treatment effect models that allow for selection on observed characteristics and find that the majority of the gaps remain after conducting propensity score matching. For example, when we focus on the area of common support, those exposed to violence during childhood have household earnings that are 27 percent lower in their late 30s. This gap is reduced to 26 percent when controlling for observed characteristics.

Since selection into exposure to violence might be driven by unobserved variables, we assess the importance of this type of selection. We use recent advances in Masten et al. (Forthcoming) that compare the uncertainty under assumed levels of selection on unobservables to the selection implied by treating observed characteristics as if they were unobserved. Effects on shorter-run outcomes like educational attainment and violent behavior at age 15 are highly robust to selection on unobserved characteristics. Effects on long-run outcomes such as household earnings are reasonably robust to selection on unobserved characteristics, with some of the most robust effects being those on very harmful long-run outcomes such as a lack of individual earnings.

While this paper focuses on the outcomes of Black males, we do consider the robustness of

[1] Anderson (1999)'s classic ethnography concludes that "Of all the problems besetting the poor inner-city black community, none is more pressing than that of interpersonal violence and aggression" (p 32).

[2] Using data from NCHS (2021), Appendix Figure 1 shows that between 1977 and 2019, the average annual ratios of Black to white homicide rates for 15-24 and 25-34 year-old males were 8.1 and 8.4. Homicides with Black victims are cleared at lower rates than homicides with white victims (Fagan and Geller (2018)); such racial differences in interactions with the criminal justice system appear necessary to explain racial disparities in homicide rates (O'Flaherty and Sethi (2010)).

one treatment effect across race and ethnicity. We find that regardless of their race or ethnicity, adolescent males are equally likely to engage in violent behavior at age 15 after conditioning on childhood exposure to violence. We show that this remains true even after controlling for the observed characteristics of the adolescents. These findings are notable for their clear rebuke of Murray (2021)'s appeal to inherent differences between racial groups for explaining group differences in violent behavior.

By the process of elimination, our investigation of mechanisms suggests that the effects of childhood exposure to violence are driven by the stress and trauma resulting from the exposure itself. The vast majority of the gap in household earnings between those exposed and not exposed in childhood remains after conditioning on ever being incarcerated, neighborhood socioeconomic status, or exposure to gang activity.[3]

The second contribution of this paper is an analysis of how nurturing relationships moderate the effects of exposure to violence. The literature on child development has found that nurturing relationships are an antidote to the toxic stress generated by Adverse Childhood Experiences (ACEs) such as parental abuse, parental neglect, or household dysfunction (Shonkoff and Garner (2012); Garner and Yogman (2021)).[4] Additionally, nurturing relationships have been shown to support youth development even in the absence of ACEs (Bethell et al. (2019a), Bethell et al. (2019b)). Our analysis of the interaction between exposure to violence and nurturing relationships is focused on adolescence because the NLSY97 has a wide variety of related variables during this time period.

Before analyzing the effects of adolescent experiences, we must first confront a measurement question that arises due to the breadth of variables available in the NLSY97. How should one synthesize 11 variables on exposure to violence and 31 variables related to nurturing relationships? One could simply add up the positive responses to these variables to create indexes analogous to the original ACE score (Felitti et al. (1998)). Given the variation in positive response rates across variables, one might suspect that such an equal weighting of variables would be less informative than methods like Item Response Theory (IRT) or Principal Components (PC) that essentially weight each variable according to how well it explains the overall variation in the group of variables. We also consider an item-anchored scale, which weights variables according to how well they predict a later outcome (Cunha et al. (2010); Bond and Lang (2018); Nielsen (2022)).

An item-anchored index significantly outperforms the other indexes in our application. In contrast, indexes created by IRT or PC predict future outcomes no better than an index created as the sum of positive responses. This finding may be surprising in light of the sensitivity of results in education to the scale with which tests are measured (Bond and Lang (2013); Nielsen (2015); Agostinelli and Wiswall (2016); Cunha et al. (2021)). However, this finding is consistent with evidence showing that a frailty index summing adverse health indicators predicts health dynamics over

[3]While these results are also consistent with gaps in outcomes driven by permanent unobserved heterogeneity, the evidence we consider in Section 2.5.1 weighs heavily against this explanation.

[4]Garner and Yogman (2021) define toxic stress as the "wide array of biological changes that occur at the molecular, cellular, and behavioral levels when there is prolonged or significant adversity in the absence of mitigating social-emotional buffers." These biological changes are referred to as "toxic" because they are often maladaptive and health harming. Section 3.1 provides further discussion.

the life cycle as well as the first principal component of the indicators (Hosseini et al. (2022)). In light of these results, our analysis uses binary treatment variables for exposure to violence and nurturing relationships based on the medians of each item-anchored index. Following Imbens (2015), we estimate potential outcomes under a selection on observables assumption in which treatment-group-specific regressions are estimated.

We find that providing nurturing relationships to adolescents is almost as beneficial as shielding them from violence. Consider an adolescent Black male exposed to high levels of violence and low levels of nurturing relationships. Household earnings when aged 34-38 would increase by \$17,000 if such an adolescent were exposed to low levels of violence ($p = 0.01$). If such an adolescent were provided with high levels of nurturing relationships, his household earnings would increase by \$11,000 ($p = 0.04$). Protection from violence and providing nurturing relationships are not substitutes: the household earnings of our example adolescent would increase by \$32,000 ($p = 0.00$) on average if both of these treatments were improved.

We estimate positive effects of nurturing relationships at all levels of exposure to violence. This is consistent with recent results indicating that nurturing relationships are necessary for positive youth development even in the absence of adversity (Bethell et al. (2019a), Bethell et al. (2019b)). This is also notable when considering issues related to selection. In our analysis of the effects of childhood exposure to violence, we present a range of evidence that our estimates are not driven by selection. We present much less evidence on this assumption in our analysis of the effects of adolescent exposure to violence. This is because the assumption that a 16-year-old is randomly exposed to violence conditional on observed demographic characteristics is less plausible than the assumption that a 10-year-old is randomly exposed.[5] And this is why it is notable that those exposed to violence are so positively affected by nurturing relationships. Even if adolescent exposure to violence were driven by selection to an important extent, nurturing relationships would still be effective for each selected subpopulation.

We take three main policy implications from our findings. First, because their adult outcomes are so negatively affected by childhood exposure to violence, Black males' long-run outcomes are likely to be improved by reducing overall violence and/or providing youth with safe places. Second, the magnitude of our effects indicate that adolescence as a time period can matter in profound ways for adult outcomes (Chang et al. (2023), Wodtke et al. (2016)). And third, providing students with nurturing relationships appears to be a highly effective mechanism for improving adult outcomes.[6] Our results contribute to the evidence on the potential for leveraging nurturing relationships as the driving mechanism in interventions supporting parents (Olds (2002), Gertler et al.

[5]We do present one notable piece of evidence weighing in favor of a selection on observables assumption. We find similar results for the effects of non-violent adversity and nurturing relationships, with non-violent adversity from an unemployed parent or the death of a parent or sibling more plausibly random than exposure to violence.

[6]In our study, the effects of nurturing relationships are mainly driven by parents because the NLSY97 has richer variables on these relationships than others. Since similar results have been found in data sets containing richer information on these other relationships (Bethell et al. (2019a); Pierre et al. (2020); Kraft et al. (2023)), we therefore expect that providing nurturing relationships beyond parents to have additional beneficial effects on Black men's outcomes.

(2014), Cunha et al. (2022)) and in effective tutoring, mentoring, and community-building programs targeting both children and adolescents (Kraft and Falken (2021), Oreopoulos et al. (2017), Lavecchia et al. (2020), Guryan et al. (2023)).

2 Effects of Childhood Exposure to Violence

2.1 Model and Identification

Let $D_i \in \{0,1\}$ be an indicator for individual i's exposure to violence. For the outcome Y_i, we are interested in characterizing the potential outcomes $Y_i(D)$ in terms of treatment effects such as the average treatment effect and the average effect of treatment on the treated,

$$\triangle^{ATE} \equiv \mathbb{E}[Y(1) - Y(0)] \quad \text{and} \quad \triangle^{ATT} \equiv \mathbb{E}[Y(1) - Y(0)|D_i = 1]$$

We denote a vector of observed characteristics of individual i as $W_i \in \mathbb{R}^{d_w}$ with support $\mathcal{W} = \text{supp}(W)$. We follow Masten et al. (Forthcoming) and consider three approaches to identification based on adopting various assumptions about selection into treatment.

$$Y(0), Y(1) \perp\!\!\!\perp D \qquad \text{(Random Selection)}$$

$$Y(0), Y(1) \perp\!\!\!\perp D \,|\, W \qquad \text{(Selection on Observables)}$$

$$\sup_{y_d \in \text{supp}(Y(D)|W=w)} \Big| \mathbb{P}(D=1|Y(D)=y(d), W=w) - \mathbb{P}(D=1|W=w) \Big| \leq c \qquad \forall\, w \in \mathcal{W}. \qquad \text{(Selection on } c\text{-Dependent Unobservables)}$$

To aid in assessing assumptions of c-dependence, we will examine the distribution of estimated leave-one-out changes in propensity scores. Denote dimension k of W as W_k and define the propensity score and leave-out-variable-k propensity score, respectively, as

$$\pi(w) = \pi((w_{-k}, w_k)) = \mathbb{P}(D=1|W=(w_{-k}, w_k))$$
$$\pi(w_{-k}) = \mathbb{P}(D=1|W_{-k}=w_{-k}).$$

These variables allow us to define the leave-one-out change in propensity score as

$$\triangle_k \equiv \big|\, \pi(w) - \pi(w_{-k}) \,\big|.$$

We will also sometimes adopt and examine the assumption

$$\pi(w) \in (0,1) \ \ \forall\, w \in \mathcal{W}. \qquad \text{(Common Support)}$$

2.2 Data for Treatment Effect Analysis

The primary sample used in our analysis is from the National Longitudinal Survey of Youth 1997 (NLSY97). Here we provide an overview of our data work, with a greater level of detail provided in Appendix B.

We focus our analysis on the subsample of non-Hispanic Black males, and sometimes also consider the subsample comprising non-Hispanic white males. We measure our treatment variable, exposure to violence, based on whether a respondent reports having seen someone shot or shot at. One survey question asks about this exposure prior to age 12 and another question asks about exposure between the ages of 12 and 18. We refer to these variables as childhood exposure (ages 0-11) and adolescent exposure (ages 12-18).

Some W variables we use from the NLSY97 are mother's educational attainment at the time of the first survey, household structure at the time of the first survey (two parents (both biological); two parents (one biological); single parent; grandparent(s); or other), and parental income at the time of the first survey. Parental income includes income from labor (earnings) and business, but also interest income, income from Aid to Families with Dependent Children (AFDC) benefits, or income from pensions, Social Security, or insurance.

We consider a few short-run outcomes. We follow Aliprantis (2017b) and define an indicator for engaging in violent behavior at a given age as having carried a gun in the past year, attacked or assaulted someone, or belonged to a gang. We also study the percentile score for the Armed Services Vocational Aptitude Battery (ASVAB) created by NLS staff based on the results of a computer-adaptive test taken by respondents in survey wave 1. The percentile score summarizes results on the four domains of Mathematical Knowledge, Arithmetic Reasoning, Word Knowledge, and Paragraph Comprehension.

We measure long-run outcomes using results from the 2017 and 2019 waves of the survey. Sometimes we will report results in terms of the year of the survey wave, the year to which the survey variable pertains, or in terms of the average age of respondents for a given survey wave. For example, respondents are aged 35-39 at the 2019 survey wave, so results might be reported for the average age of 37. Since respondents are asked about labor market outcomes in the year before the survey, these outcomes might be reported for ages 34-38. All earnings and income variables are inflated to 2018 dollars using the US Bureau of Economic Analysis' Gross Domestic Product Implicit Price Deflator, downloaded from the St. Louis Fed's FRED website. Weekly hours worked is equal to the total annual hours worked at all civilian jobs during the year divided by 52. We measure depression using the self-reported variable that asks how often the respondent has experienced depression in the last month. An indicator for a respondent ever being incarcerated by the time of the 2019 survey wave is measured using the created variables indicating whether the respondent was incarcerated at any point in the past. We follow Aliprantis and Chen (2016) and define deceased (or missing) using the variable recording the reason for non-interviews.

2.3 Descriptive Statistics

Tables 1 and 2 present summary statistics of the variables used in the treatment effect analysis. The first notable result in Table 1 is the massive gap in Black and white boys' exposure to violence. Over a quarter of Black boys reported seeing someone shot or shot at before age 12, with 7 percent of white boys reporting the same. By age 18, cumulatively 47 (16) percent of Black (white) adolescents have been exposed to this violence. These results are consistent with the exposure to shooting reported in the National Survey of Children's Exposure to Violence conducted in 2011 (Finkelhor et al. (2015)).[7]

Table 1: Summary Statistics of the NLSY97, Percentages (Unless Otherwise Noted)

	Means for Males	
Variable	Black	White
Treatment D: Seen Shot		
Childhood	26	7
Adolescence	31	11
Child. or Adolescence	47	16
Observable Characteristics W		
Mother's Ed		
Not Determined	9	11
Dropout	20	8
GED	6	4
HS Grad	48	48
AA	8	11
BA	9	17
Parent(s)' Income in 1996		
Mean (Thousands of 2018 $s)	39	71
HH Structure		
Two Parent (Both Bio)	26	60
Two Parent (One Bio)	14	17
Single Parent	50	21
Grandparent(s)	6	1
Other	4	1

Note: See text for variable descriptions.

Table 2: Summary Statistics of the NLSY97, Percentages (Unless Otherwise Noted)

	Means for Males	
Variable	Black	White
Outcomes Y		
Violent Behavior Age 15	22	18
Violent Behavior Age 21	14	10
ASVAB Percentile	26	56
HS Grad by 26	61	78
BA by 26	9	24
HH Earnings in 2018 (Thousands of 2018 $s)	51	95
Earnings in 2018 (Thousands of 2018 $s)	37	68
0 Earnings in 2018	22	9
Hours in 2018 (Weekly Avg)	33	39
Ever Incarcerated by 2019	26	12
Depressed in 2017	14	11
Obese in 2019	39	33
Smoked in 2015	37	36
Deceased by 2019	5	3
Deceased or Missing by 2019	9	5

Note: See text for variable descriptions.

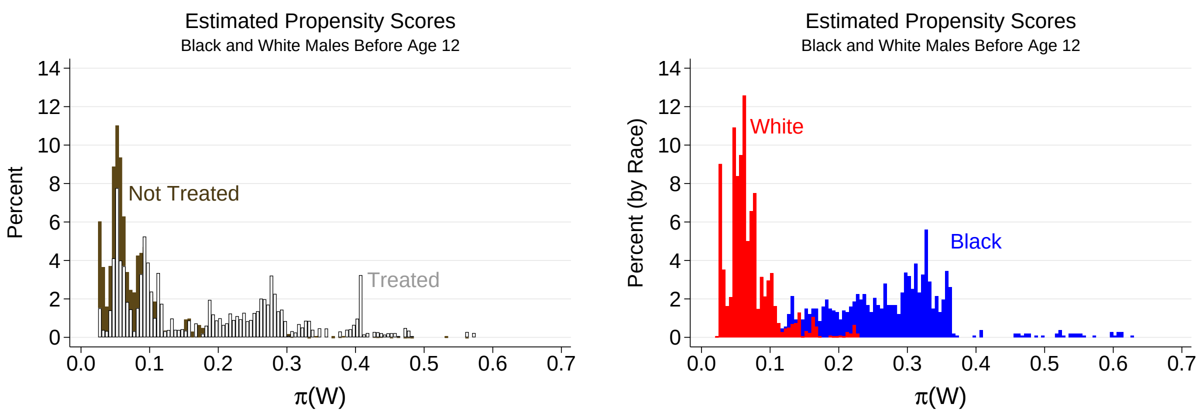
The second notable result in Table 1 is that Black males have less advantageous family backgrounds. Their mothers have lower educational attainment, their parents' income is lower, and they are much less likely to reside in a two-parent household than their white peers. We use a fine partition of household structure because exposure to violence is different in meaningful ways within coarser classifications, such as classifying together all two-parent households (Appendix Table 1).

The teenage outcomes in Table 2 show that Black adolescents are more likely to engage in violent behavior and have much lower test scores than their white peers. For the adult outcomes in Table 2, Black men have lower earnings at ages 34-38 than their white peers (in 2018), and are more likely to be depressed or deceased. The cumulative risk of ever being incarcerated in Table 2 is in line with the estimates for Black males in Table 1 of Western and Wildeman (2009), noting that our sample was born in 1980-1984 and at the time of the 2019 survey they were aged 35-39.

[7] For example, when the NLSY97 data are weighted to be representative of the national population, they broadly match the statistics for ages 14-17 reported in Table 5 in Finkelhor et al. (2015).

The cumulative risk for white males in our sample is considerably higher than the estimates in Western and Wildeman (2009).

There are several additional variables available in the NLSY97 that we do not include in **W** because they look to us like "bad controls" (Angrist and Pischke (2009)), or variables that due to their time of measurement might have been affected by the treatment. This includes a range of questions on personality traits that are asked in the NLSY97 survey waves of 2002 and later. This also includes variables like neighborhood characteristics, which are first observed in the NLSY97 after childhood exposure.



(a) Estimated Together

(b) Estimated Separately

Figure 1: Propensity Scores of Childhood Exposure by Sample
Note: The left panel shows estimated propensity scores for childhood exposure to violence, shown separately for those treated (exposed to violence) and those not treated (not exposed to violence). The propensity scores in the left panel are estimated on the full sample of Black and white males. The right panel shows propensity scores estimated separately on the Black and white subsamples, and shows estimates separately by both treatment status and race.

Results from propensity score matching explain why we focus our analysis on Black males. If we were to estimate propensity scores for childhood exposure to violence on the sample of non-Hispanic Black and white males, where we included the **W** variables and an indicator for race, we would find a result similar to that in Bingenheimer et al. (2005). Figure 1a shows that in the combined Black and white sample, those exposed to violence appear to be different on observed characteristics than those not exposed. This suggests problems for matching on observed characteristics (Heckman et al. (1998)). However, when we estimate propensity scores for childhood exposure to violence by race, Figure 1b shows that race is the variable separating those with a high likelihood of exposure and those unlikely to be exposed. Expected exposure to violence is completely different by race, even taking into account parental income, mother's educational attainment, and household structure. Appendix B.1.2 shows these results in greater detail.

2.4 Treatment Effect Estimation Results

We impose common support on our sample by dropping respondents with estimated propensity scores either below the 5th percentile of the untreated distribution or above the 95th percentile of the treated distribution. Figure 2 shows that in the left tail of the distribution of propensity scores, this drops a group of respondents who are not treated for which there are few observationally similar treated respondents. In the right tail, this drops a group of respondents who are treated for which there are few observationally similar untreated respondents.

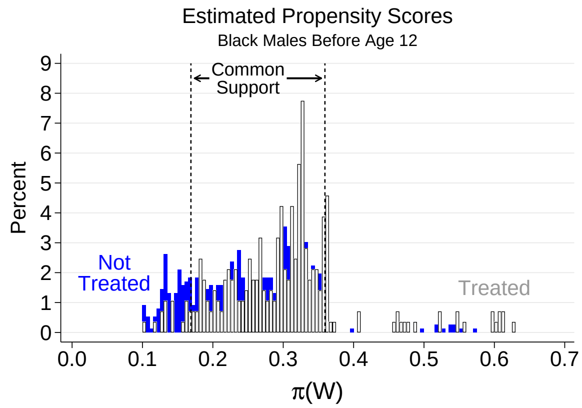


Figure 2: Imposing Common Support Using Propensity Scores of Childhood Exposure
Note: This figure shows the distributions of propensity score estimates for childhood exposure to violence by exposure. The vertical lines show the boundaries of common support, set as the 95th percentile of the exposed (ie, treated) sample and the 5th percentile of the sample not exposed to violence.

We begin our analysis by estimating the treatment effects of exposure to violence in childhood and adolescence on later life outcomes. The results are reported in Table 3. The first and second columns show control means and effects under the assumption of random selection. The third column shows effects assuming selection on observables where matching is achieved through entropy balancing (Hainmueller (2012); Zhao and Percival (2016)).

We find that exposure to violence has large and significant impacts on behavior, educational attainment, labor market outcomes, and health. Additionally, the treatment effects from childhood exposure are often greater than those from adolescent exposure.

First, we examine violent behavior at ages 15 and 21. Respondents who saw someone shot or shot at before the age of 12 are 20 percentage points more likely to engage in violent behavior at age 15. Similarly, respondents who saw someone shot or shot at between the ages of 12 and 18 are

14 percentage points more likely to engage in violent behavior at age 21.

Table 3: Treatment Effects of Exposure to Violence on Black Males

	Effects by Assumption about Selection into Treatment					
	Childhood Exposure			Adolescent Exposure		
	Random		on Obs.	Random		on Obs.
Outcome	C. Mean	Effect	Entr. Bal.	C. Mean	Effect	Entr. Bal.
Violent Behavior at Age 15 (%)	17	20 [0.00]	20 [0.00]			
ASVAB Pctl	25	–5 [0.00]	–5 [0.01]			
Violent Behavior at Age 21 (%)				9	15 [0.00]	14 [0.00]
HS Grad by 26 (%)	63	–16 [0.00]	–15 [0.00]	64	–13 [0.00]	–13 [0.00]
BA by 26 (%)	7	–2 [0.25]	–2 [0.26]	8	–4 [0.06]	–4 [0.02]
HH Earnings in 2018 ($1,000s)	48	–13 [0.00]	–12 [0.00]	49	–12 [0.00]	–12 [0.00]
Ind. Earnings in 2018 ($1,000s)	34	–7 [0.02]	–7 [0.02]	34	–7 [0.03]	–7 [0.01]
0 Earnings in 2018 (%)	20	9 [0.02]	9 [0.03]	21	5 [0.17]	6 [0.10]
Weekly Hours in 2018	33	–5 [0.03]	–5 [0.04]	33	–4 [0.10]	–4 [0.10]
Ever Incarcerated by 2019 (%)	26	8 [0.02]	8 [0.03]	21	21 [0.00]	22 [0.00]
Smoked in 2015 (%)	35	5 [0.22]	5 [0.25]	35	7 [0.07]	6 [0.13]
Deceased by 2019 (%)	5	3 [0.13]	3 [0.17]	4	3 [0.05]	3 [0.06]
Deceased or missing by 2019 (%)	8	5 [0.02]	5 [0.04]	7	3 [0.10]	4 [0.11]

Note: Childhood (adolescent) exposure to violence is seeing someone shot or shot at when aged 11 or younger (aged 12 to 18). "On Obs." is "On Observables," "C. Mean" is "Control Mean," and "Entr. Bal." is "Entropy Balancing." "GAD" is Generalized Anxiety Disorder Scale and "CESD" is Center for Epidemiologic Studies Depression Scale. The sample is Black males in the NLSY97. Values in brackets are the p-values associated with each coefficient being different from zero.

The estimated treatment effect of childhood exposure on obtaining a high school diploma by age 26 is –15 percentage points. Respondents who were exposed to violence in adolescence are 13 percentage points less likely to obtain a high school diploma. The impact on earning at least a bachelor's degree is smaller for both measures of exposure. Respondents who were exposed in childhood were 3 to 5 percent less likely to obtain a degree, and those who were exposed in adolescence were 4 percentage points less likely to obtain a degree.

Next, we examine labor market outcomes in 2018. The effects of exposure in childhood are similar to the effects of exposure in adolescence. Respondents who were exposed in childhood earned \$7,000 less in 2018. Those who were exposed in adolescence also earned \$7,000 less. Childhood exposure made respondents 9 percentage points more likely to have zero earnings in 2018, whereas adolescent exposure did not result in statistically significant differences. Respondents who saw someone shot or shot at before the age of 12 also worked 5 hours less per week in 2018.

We find that exposure to violence has a large and significant impact on incarceration. In 2019, those who had been exposed to violence in childhood were 8 percentage points more likely to have been incarcerated at least once. Adolescent exposure increased the likelihood of having been incarcerated by 22 percentage points.

2.5 Robustness

2.5.1 Robustness to Conditional c-Dependence

We follow Masten et al. (Forthcoming) to examine the robustness of the treatment effects we estimated under an assumption of selection on observables to violations of that assumption.[8] Specifically, we consider cases of selection on unobservables in the form of c-dependence. We use Masten et al. (Forthcoming)'s *tesensitivity* package in Stata to estimate breakdown frontiers for each outcome we investigate. Each breakdown frontier we consider is the maximum value of c for which c-dependence implies that the sign of the treatment effect's Manski (1990) bounds are of the same sign as the estimated treatment effect. Thus, the breakdown points for each outcome, $c^*(Y)$, allow us to judge the robustness of our estimates by quantifying the weakest assumption about selection on unobservables under which the estimated treatment effect keeps its sign.

While the breakdown points for each outcome, $c^*(Y)$, give us a means of judging the robustness of our estimates, this judgment remains subjective. Masten et al. (Forthcoming) suggest an approach, broadly following the approaches in Altonji et al. (2005) and Oster (2019), of comparing breakdown points representing a scalar summary of selection on unobservables with measures of the degree of selection on observables. For this purpose Masten et al. (Forthcoming) focus on the distribution of selection on observables when one variable is omitted from the estimation of the propensity score, defining Δ_{ki} as the change in individual i's propensity scores when the single observable variable w_k is left out of the specification of the propensity score.

Figure 3 shows the robustness of effects on short- and long-run outcomes. These figures compare the breakdown points for outcomes with the distributions of changes in leave-one-out propensity score estimates, where the leave-one-out distributions are calculated for specific samples. The further right a breakdown point is in the CDFs in these figures, the more robust a treatment effect

[8] Masten et al. (Forthcoming) themselves build on a body of work in Masten and Poirier (2020), Masten and Poirier (2018), and Horowitz and Manski (1995); Appendix A of Masten and Poirier (2020) provides a discussion of this literature.

estimate is. Respondents are included in the short-run sample if their BA attainment is observed, and they are included in the long-run sample if their earnings in 2018 are observed.[9] The solid lines show the leave-one-out changes in propensity scores variable by variable, and the dashed lines show the changes when a group of variables is omitted.

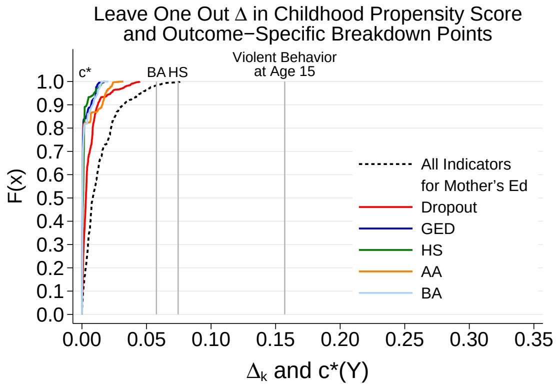
Looking at short-run outcomes in Figures 3a, 3c, and 3e, we find that the effects of childhood exposure to violence are robust for educational attainment and extremely robust for engaging in violent behavior at age 15. The least robust educational outcome, attaining a BA, is robust at a cutoff of the 90th percentile for mother's educational attainment and parental income and at the 75th percentile for household structure. A notable result from the distributions of leave-one-out propensity scores is that changes are greater when omitting a group of variables than any single variable, where the former case would seem to be most analogous to a variable being unobserved. Another notable result, shown in Figures 3c and 3d, is that the distribution of leave-one-out changes in propensity scores has an extremely long right tail for household structure.

Looking at long-run outcomes in Figure 3, we find that the effects are slightly less robust than are the short-run outcomes. Household earnings are reasonably robust; the breakdown point is outside the support of nearly all individual variables, and around either the 80th or 90th percentile for all groups of variables. The most robust result is for 0 individual earnings, which is beyond the 90th percentile for all groups of variables. This is notable given the importance of non-work (Thompson (2021); Aguiar et al. (2021)). When compared to the National Supported Work (NSW) demonstration example considered in Masten et al. (Forthcoming), these results are somewhere between those of their experimental and observational samples, arguably closer to the results in the experimental sample (See Figure 7 in Appendix C.).

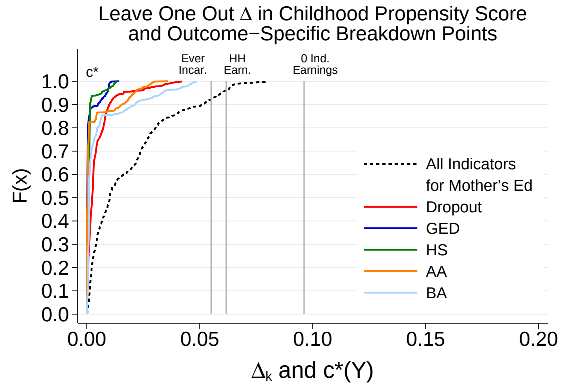
The effect of exposure to violence at age 11 or earlier on one's own violent behavior at age 15 is one of the estimates that is most robust to selection on unobserved characteristics. This robustness is evidence that the correlation between childhood exposure and violent behavior is more likely the result of pre-emptive violence driven by fear (O'Flaherty and Sethi (2019)) than youth who are inherently more violent selecting into both exposure and behavior. This robustness also suggests revisiting the similar controversial result in Bingenheimer et al. (2005). Bingenheimer et al. (2005) found in a survey of 12- and 15-year-old youths in Chicago that 23 percent of respondents had been shot or shot at, or had seen someone else shot or shot at, in the previous 12 months. In their estimated propensity score matching model, this exposure to violence doubled the probability that

[9] This is a slight variation from the approach in the *tesensitivity* package. The package computes outcome-specific breakdown points using the subsample of units observed for each outcome, which are the breakdown points we use here. However, the package also computes the distribution of Δ_{ki} for each outcome separately using the outcome-specific subsample. We compare the distribution of Δ_{ki} estimated on one sample to the breakdown points estimated for several specific outcomes to facilitate the comparison of breakdown points across multiple outcomes. Also note that all of these calculations are made on the sample with common support discussed earlier.

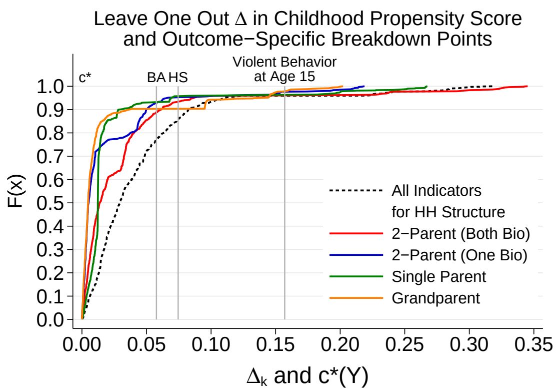
an adolescent would engage in violence over the next 2 years.[10]



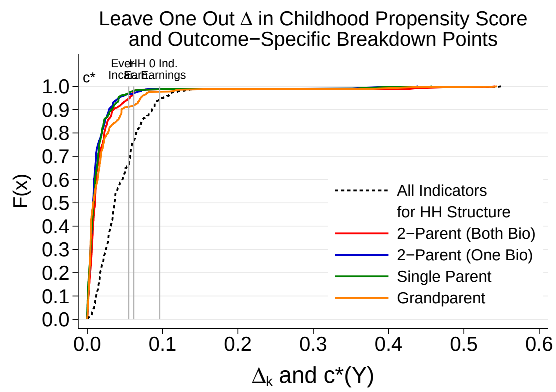
(a) Mother's Educational Attainment



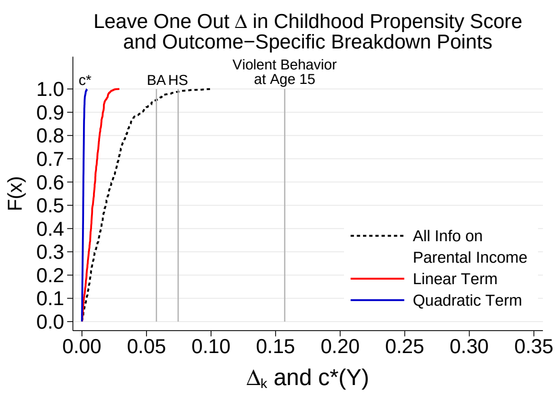
(b) Mother's Educational Attainment



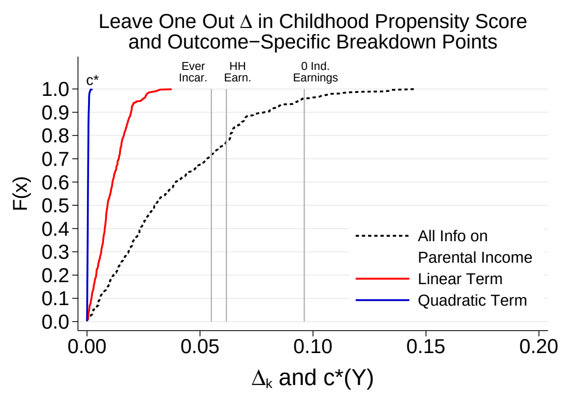
(c) Household Structure



(d) Household Structure



(e) Parental Income

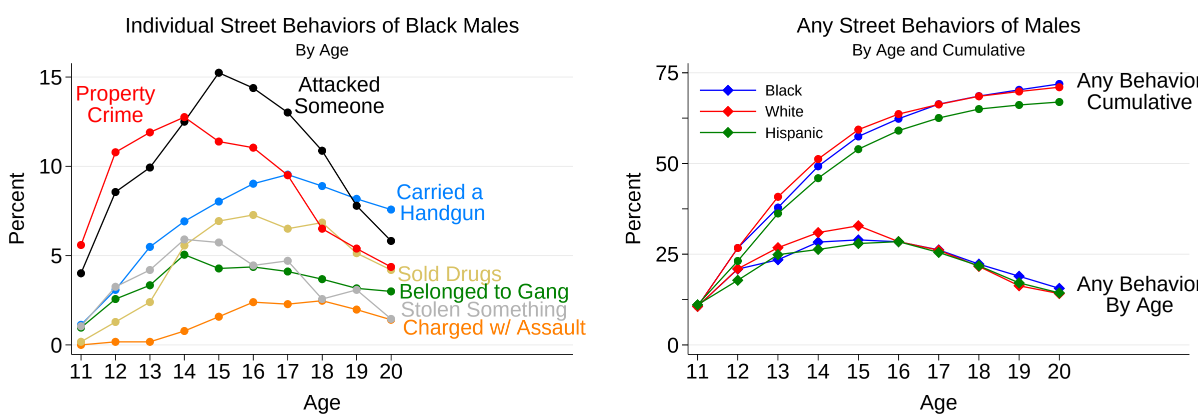


(f) Parental Income

Figure 3: Breakdown Points and Changes in Childhood Exposure Propensity Scores
Note: In these figures vertical lines represent outcome-specific breakdown points, which are described in the main text. The CDFs represent the individual-level distribution of changes in the propensity score when one variable or set of indicator variables is removed from the estimation of the propensity score. In the left panel, holding a high school diploma ("HS") or attaining a BA ("BA") are measured when the respondent is aged 26. In the right panel, individual earnings ("Ind. Earnings") and household earnings ("HH Earnings") are measured in the 2019 wave of the survey regarding their values in 2018, when respondents were aged 34-38. "Ever Incar." is an indicator for whether a respondent was ever incarcerated by the time of the 2019 survey. See the text for more details on each variable.

[10] Aliprantis (2017b) subsequently found similar results in the NLSY97.

We add three points that help to interpret the results in Bingenheimer et al. (2005) and Aliprantis (2017b). First, the propensity score estimates shown earlier in Figure 1 show that the lack of overlap would be an issue if the effects on the full sample of males were being estimated. This seems to be exacerbated by the extremely rich set of covariates collected in their survey, which was a part of the Project on Human Development in Chicago Neighborhoods (PHDCN).[11] Our results estimated on the subsample of Black males are robust to this concern. Second, the results above on c-dependence indicate that if a common cause were generating childhood exposure to violence and engaging in violence at age 15, that cause would need to drive selection into exposure in a way that is stronger than parental income, mother's educational attainment, or household structure. Third, Figure 4 presents evidence from the NLSY97 on street behaviors by age, which are behaviors we would expect to increase an individual's chances for exposure to violence, with the "street" label coming from Anderson (1999). Figure 4b shows that these behaviors are increasing during the early teenage years, so that most of this activity happens in adolescence, not during the time of childhood exposure to violence, when respondents were aged 11 or younger.[12] Yet as shown in Table 2, adolescent exposure to violence is only 5 percentage points higher than childhood exposure for Black males, and the majority of respondents exposed to violence during adolescence were not exposed during childhood. This evidence suggests that childhood exposure to violence is driven more by the environment selecting the individual than vice versa. The relative weakness of adopting a selection on observables assumption for childhood exposure to violence is the reason for our focus on the effects of childhood exposure throughout this analysis.



(a) Street Behaviors by Age

(b) Street Behaviors by Age and Race/Ethnicity

Figure 4: Street Behaviors by Age

Note: The left panel shows the percent of Black males engaged in specific behaviors at a given age. The right panel shows the percent of males who engaged in any of these behaviors at a given age by race and ethnicity, where the Black and white groups are both non-Hispanic. The right panel also shows by age the cumulative percent of males within each racial or ethnic group who engaged in at least one of these behaviors.

[11] See Sampson (2012) for a description of the PHDCN.

[12] Appendix D displays each specific street behavior by age and race/ethnicity, where we find that Black males are more likely to have attacked someone or belonged to a gang, but white males are more likely to have committed a property crime or sold drugs.

2.5.2 Robustness across Race and Ethnicity

One argument present in the current public discourse is that Black and white Americans face equal opportunities, but Black people are inherently less capable of taking advantage of those opportunities, either because they have less mental capacity (Herrnstein and Murray (1996)) or are inherently more prone to violence (Murray (2021)).

Here we consider the evidence from the NLSY97 on violent behavior by race. Figure 5 shows that there are indeed significant group differences in outcomes, consistent with the empirical fact pointed out by Murray (2021).

Figure 6 presents evidence that these differences can be explained by similar human beings facing different environments. The left panel shows that adolescent males of different racial and ethnic groups are equally likely to engage in violent behavior conditional on whether they are exposed to violence as children. The light blue column on the left shows that conditional on not experiencing childhood exposure to violence, 17 percent of adolescent males who are Black engage in violence at age 15. The dark blue column shows that conditional on exposure to violence during childhood, there is a doubling to 35 percent of Black adolescent males engaging in violence at age 15. The red bars and green bars show that a nearly identical pattern holds, respectively, for the white and Hispanic groups.

Figure 6b presents evidence that these differences are not driven by respondents' household structure, parental income, or mother's educational attainment. Using OLS regressions that condition on these variables for each exposure and race/ethnicity group, predicted violent behavior does not change.

This evidence is a stark rebuke to the claims in Murray (2021) that inherent racial differences are the explanation for differences in violent behavior. The idea that members of socially defined racial categories possess unalterable traits and abilities (O'Flaherty (2015)) distracts from the overwhelming mechanisms generating racial differences in outcomes; Dumornay et al. (2023) present another recent example and Goldberger and Manski (1995) and Manski (2011) present related discussions. Acknowledging that statistics on racial differences have played such an important role in justifying the malicious treatment of Black people would help contemporary researchers avoid repeating the history of motivated reasoning on racial essentialism.[13]

[13]For examples of this use of statistics, see Zuberi (2001), Chapter 6 of Washington (2006), or O'Flaherty (2016). For a summary of what has inspired motivated reasoning on race, consider the writing of historian of slavery David Brion Davis that throughout history "slaves, regardless of origin or ethnicity, were seen to carry the marks of childlike and animalistic inferiority later ascribed to such supposedly inferior peoples as Australians and sub-Saharan Africans. ... Throughout the ancient Euro-Asian world as well as in the preconquest Western Hemisphere, slaves were commonly marked off by identifying symbols or icons, such as brands, tattoos, collars, hairstyles, or clothing. Clearly such emblems would have been less necessary if all slaves had shared distinctive physical characteristics that quickly differentiated them from all nonslaves. ... long before the eighteenth-century invention of 'race' as a way of classifying humankind, a different phenotype or physical appearance made the dehumanization of enslavement much easier" (Davis (2006), p. 53).

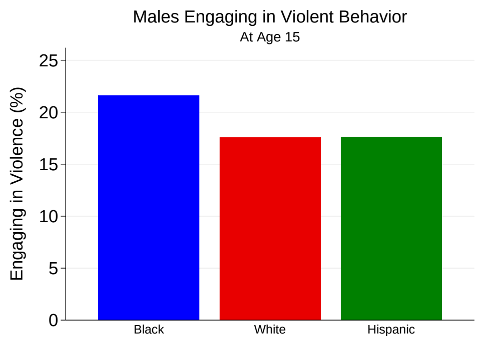
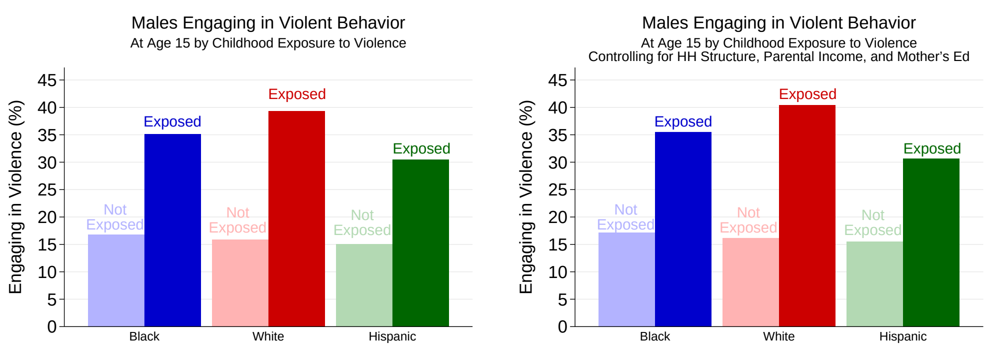


Figure 5: Violent Behavior by Race and Ethnicity
Note: This figure shows the percent of 15-year-old males who engaged in violent behavior. A respondent engages in violent behavior by attacking someone with the intention of seriously hurting them, being charged with an assault, carrying a handgun, or belonging to a gang. Black and white groups both exclude Hispanic respondents, and the Hispanic group includes all races.



(a) By Race

(b) By Race, Controlling for Observables

Figure 6: Violent Behavior by Race and Exposure to Violence
Note: Childhood exposure to violence is an indicator for whether the respondent reported seeing someone shot or shot at before age 12. See the note to Figure 5 for more details.

2.6 Mechanisms

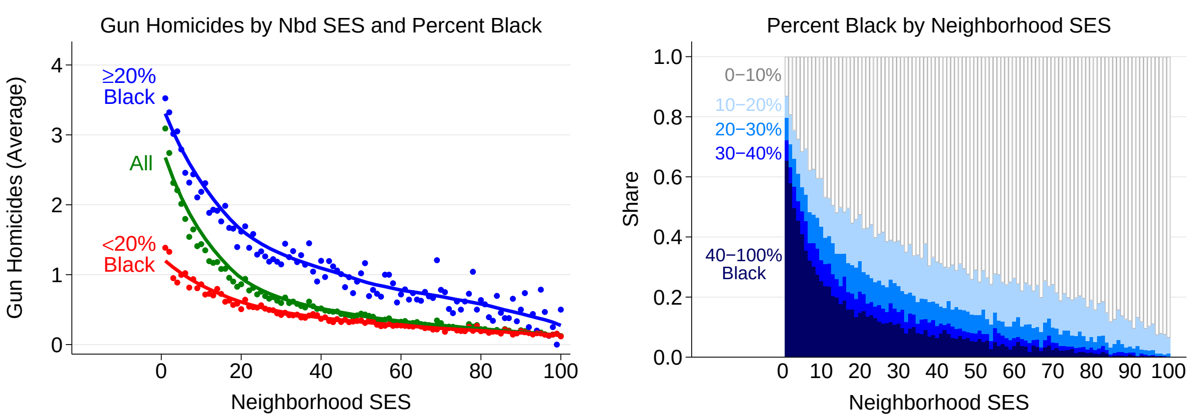
2.6.1 Are Effects of Exposure to Violence Capturing Overall Neighborhood Effects?

Our analysis has interpreted the treatment effects of exposure to violence as being caused by the exposure itself. But an alternative possibility is that witnessing a shooting during childhood indicates that a respondent grew up in a neighborhood that negatively affected their outcomes through other mechanisms. In this case, the effects we observe could ultimately be caused by the respondent's exposure to the broader neighborhood context rather than his exposure to violence (Aizer (2009); Perry et al. (2015)).

Here we investigate the possibility that the effects of exposure to violence reflect the effects of more general neighborhood conditions, motivated by the strong correlation between exposure to violence and neighborhood socioeconomic characteristics. To measure the key neighborhood characteristics thought to affect residents, we first calculate a tract-level ranking of neighborhood socioeconomic status (SES) following Aliprantis (2017a). The neighborhood SES measure is the percentile ranking of the first principal component of a tract's national rankings on six socioeconomic characteristics. The six characteristics used to calculate neighborhood SES are the poverty rate, the share of adults 25+ with a high school diploma, the share of adults 25+ with a BA, the Employment to Population Ratio for adults 16+, the labor force participation rate for adults 16+, and the share of families with children under 18 with only a mother or father present.

To understand the relationship between neighborhood SES and exposure to violence, we first use neighborhood SES estimated on the 2014-2018 ACS together with geolocated data on gun homicides from 2013 to 2018 from the Gun Violence Archive (GVA). The GVA is an independent organization that does research and collects data on gun violence in the United States with the goal of providing detailed and accessible data on all gun-related injuries, deaths, and crimes. The GVA collects and verifies daily data from over 7,500 sources that include local and state police, media, data aggregates, government, and other sources. Geolocations allow us to match incidents to Census tracts.

There is a relationship between neighborhood SES and neighborhood gun violence that is strongest in the lowest SES neighborhoods. Figure 7a displays local linear regressions of the expected number of gun homicides in a tract as a function of its SES. Focusing on the green line, which presents estimates for all tracts, we see that in the top half of tracts there is a linear, decreasing incidence of gun violence as SES increases. In tracts in the top half of neighborhood SES, the vast majority experienced 0 or 1 gun homicides over the observed time period. As neighborhood SES decreases, though, there is an inflection in the relationship to a more strongly negative relationship. The majority of the lowest SES neighborhoods experienced 2 or more gun homicides.



(a)

(b)

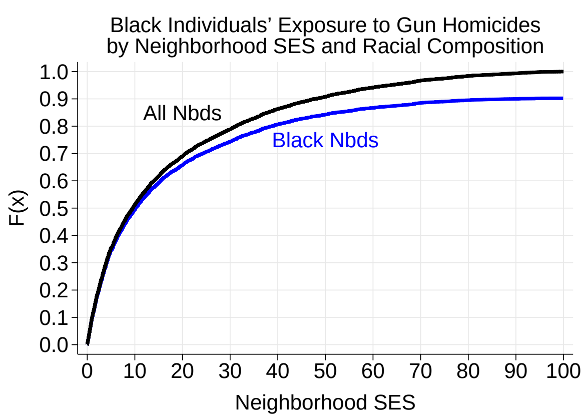
Figure 7: Neighborhood Gun Homicides, Socioeconomic Status, and Racial Composition
Note: The left panel displays local linear regressions showing the mean number of gun homicides as a function of a Census tract's neighborhood SES. Tract-level gun homicides are measured for 2013 to 2018 using data from the Gun Violence Archive and neighborhood SES is constructed using the 2014-2018 American Community Survey (ACS); see the main text for details on these variables. The green line shows this relationship for all tracts, while the blue and red lines show this relationship for tracts in which, respectively, more and less than 20 percent of residents are Black. The right panel shows the mean percent of tract residents who are Black by percentiles of neighborhood SES.

There is an important interaction between gun homicides, neighborhood SES, and racial composition. This is evident from the blue line in Figure 7a, which shows that there is a level effect with Black neighborhoods experiencing higher homicide rates even conditional on neighborhood SES (Cheon et al. (2020)). Figure 7b provides additional evidence on the non-linear relationship between neighborhood SES, gun homicides, and the share of Black residents. There appear to be highly non-linear effects of neighborhood SES when living in neighborhoods with racialized concentrated poverty, consistent with previous findings in Aliprantis and Richter (2020) and Weinberg et al. (2004). We note here the race-specific aspects of neighborhood-level public safety, both in terms of neighborhood sorting (Aliprantis et al. (2022)) and public policy (Sylvera (2023)), and provide further analysis of GVA data in Appendix E.

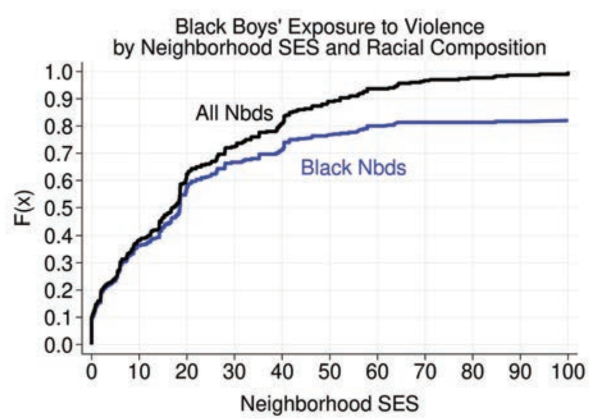
Black Americans' exposure to violence is highly concentrated in the lowest SES neighborhoods. We first show this fact using the GVA data on gun homicides before showing this fact with the NLSY97 data on witnessing a shooting. We use the GVA to create a tract-level measure of Black Americans' exposure to gun homicides,

$$\text{exposure to gun homicides in tract } j = \#\text{ of Black residents in tract } j \times \#\text{ of gun homicides in tract } j.$$

Figure 8a shows CDFs of Black Americans' exposure to gun homicides by their tract's SES ranking. Half of Black individuals' exposure to gun homicides is experienced in the bottom decile of neighborhood SES. An additional 20 and 10 percent of exposures are added, respectively, in the second and third deciles.



(a) GVA and ACS Data



(b) NLSY97 and ACS Data

Figure 8: Black Americans' Exposure to Violence by Neighborhood SES
Note: The left panel shows the cumulative exposure of Black residents to gun homicides in their Census tract by their tract's neighborhood SES ranking. The right panel shows the cumulative exposure of Black males in the NLSY97 to witnessing a shooting during their childhood by their initial tract's neighborhood SES ranking.

A very similar pattern holds when using shootings witnessed in the NLSY97 or gun homicides in the GVA as our measure of exposure to violence. This consistency is important because there are many ways of measuring exposure to violence (Bancalari et al. (2022)). Figure 8b plots CDFs of Black respondents' reporting witnessing a shooting by their tract's SES ranking in the first wave of the NLSY97 (See Appendix B.1 for data on the calculation of neighborhood SES in 1997.). The CDFs show that 40 percent of the Black male respondents in the NLSY97 who witnessed a shooting lived in the first decile of neighborhood SES. About 20 percent more of the shootings were witnessed by someone living in the second decile of neighborhood SES, and by the 30th percentile of neighborhood SES about three-quarters of shootings had been witnessed.

We note three key differences between the measures of exposure to violence in Figures 8a and 8b. First, the GVA is for all Black individuals, while the NLSY97 measure is only for Black boys aged 11 or younger. Second, the GVA measures both the intensive and extensive margins of exposure, while the NLSY97 variable only measures the extensive margin. Third, the GVA measures exposure at the same time residents are recorded in tracts in the ACS, while the exposure measured in the NLSY97 occurred when respondents were aged 0-11 and their tract of residence in 1997 was measured at the time of their first interview, when the respondents were primarily aged 12-16.[14]

Given the high concentration of Black men's childhood exposure to violence in neighborhoods with the lowest SES, it would seem plausible that the treatment effects estimated earlier in the paper are driven by neighborhood conditions other than violence. Figure 9 shows, however, that the effects of Black men's childhood exposure to violence are independent of neighborhood SES. Regardless of their childhood neighborhood's SES ranking, the household earnings of Black men in their late 30s who were exposed to violence as children are considerably lower than those of the men who were not exposed to violence during their childhood.

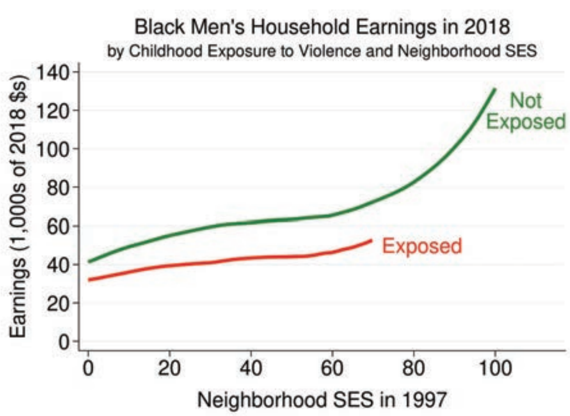


Figure 9: Black Men's Household Earnings by Exposure to Violence
Note: This figure shows local linear regressions of household earnings in 2018 on neighborhood SES in 1997 for Black men who did, and did not, witness a shooting during their childhood.

Table 4: Regression Coefficients with and without Decile of Childhood Neighborhood SES

Dependent Variable	With	Without
HH Earnings	–17.4	–16.8
(Thousands of 2018 $s)	(4.6)	(4.5)
HS	–17.6	–16.5
	(3.2)	(3.2)
BA	–5.6	–4.9
	(1.9)	(1.9)
Incar.	10.5	10.3
	(2.9)	(2.9)

Note: Regression coefficients represent percentages unless otherwise noted.

Table 4 expands on this analysis to present regression results and to include additional outcomes.

[14]Six percent were either 17 or 18 when first interviewed.

We see that the coefficients on childhood exposure barely change when the regressions include indicators for deciles of neighborhood SES. This is true for household earnings, attainment of a high school diploma or BA, and ever being incarcerated by 2019.

2.6.2 Are Effects of Exposure to Violence Mediated by Incarceration?

Incarceration is critical for understanding the labor market outcomes of Black men in recent decades (Bayer and Charles (2018); Neal and Rick (2014)). A single spell of incarceration has a permanent lifetime effect that flattens the earnings of young men, Black or white (Neelakantan et al. (2022)). Given this evidence, together with the evidence in Table 3 that childhood exposure increases incarceration rates, we might expect that the effects of childhood exposure to violence on labor market outcomes are mediated through incarceration.

Perhaps surprisingly, then, Table 5 and Figure 10 show that incarceration does not mediate the effects of exposure to violence on adult household earnings. Table 5 displays regression results that those exposed to violence during their childhood (ie, before age 12) had household earnings in 2018 that were $17,000 lower than those who were not exposed to violence. This gap only shrinks by 13 percent, to $15,000, when conditioning on ever experiencing a spell of incarceration. Figure 10 shows that for the never incarcerated group, shown in green, the gap in earnings between exposure groups grows steadily as the cohort ages. The gap is smaller for those who have experienced incarceration, shown in red, and later years might be interpreted as statistical noise. However, the dip and rebound in respondents' late 20s reflects the state of the labor market during the Great Recession, and the mid-20s pre-Great Recession gap between exposure groups indicates that earnings truly are higher for those not exposed.

Table 5: Household Earnings

Independent Variable	Coefficient in Earnings Regression		
Childhood Exposure	−17.4 [0.00]		−15.2 [0.00]
Ever Incarcerated		−33.8 [0.00]	−33.3 [0.00]
R^2	0.02	0.06	0.07

Note: This table reports coefficients from regressions where the dependent variable is Black men's household earnings in 2018 and the independent variables are dummies for childhood exposure to violence alone (first column), ever being incarcerated alone (second column), or both (third column). Values in brackets are the p-values associated with each coefficient being different from zero.

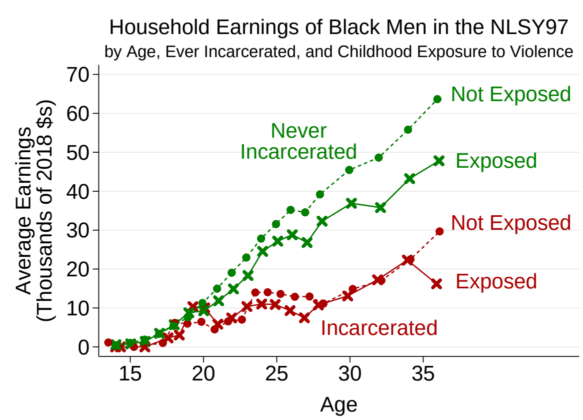


Figure 10: Household Earnings by Age, Childhood Exposure to Violence, and Incarceration
Note: This figure shows the means of Black men's household earnings by the average age in the wave of the NLSY97 for each of four groups. Those groups are the four combinations of the binary indicators for childhood exposure to violence and ever being incarcerated.

Appendix F shows similar results for both individual earnings and for the probability of earning zero dollars in a given year, with the gap between exposure groups again clearer for the never incarcerated group.

2.6.3 Are Effects of Exposure to Violence Capturing Gang Activity?

We know that children could be exposed to violence through the crime created by gangs (Bruhn (2021); Monteiro and Rocha (2017)), as well as the police response in trying to reduce gang behavior (Wagner (2021)). A natural question, then, is whether our estimated effects of exposure to violence capture the effects of exposure to gang activity. In addition to exposure to violence, the NLSY97 also asks questions about exposure to gang activity. We have data on how many respondents reported living in a neighborhood with gangs by the age of 18, the percent of the respondent's peers who they report were in gangs in 1997, and whether the respondent had siblings or close friends who were in gangs from 1997 to 2005.

Exposure to violence and exposure to gang activity appear to be distinct experiences for children. Figure 11 shows that while there is a correlation between seeing someone shot and peers in a gang, among those who saw someone shot, the majority still report that few peers belong to a gang.

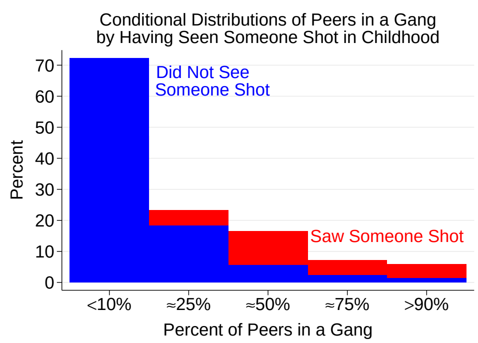


Figure 11: Exposure to Gang Activity by Exposure to Violence
Note: This figure shows the percent of peers that respondents reported belonging to a gang conditional on whether the respondent reported childhood exposure to violence.

Table 6: Gangs and Childhood Exposure to Violence

Outcome	Ref. Mean	Seen Shot	Peers in Gangs ≈ 25%	≈ 50%	≈ 75%	> 90%
Violent at 15 (%)	16	17	–1	–0	4	21
		[0.00]	[0.99]	[0.99]	[0.42]	[0.00]
HS Diploma (%)	67	–16	6	–3	–11	–19
		[0.00]	[0.54]	[0.54]	[0.06]	[0.00]
BA (%)	12	–5	0	–5	–6	–10
		[0.02]	[0.08]	[0.08]	[0.09]	[0.01]
Incarcerated (%)	21	9	1	5	5	16
		[0.00]	[0.20]	[0.20]	[0.32]	[0.01]
Earnings ($1,000s)	43	–10	–2	–12	–11	–20
		[0.01]	[0.01]	[0.01]	[0.11]	[0.01]
HH Earnings ($1,000s)	61	–15	–4	–18	–18	–29
		[0.00]	[0.01]	[0.01]	[0.05]	[0.00]

Note: This table reports coefficients from regressions of outcomes on indicators for childhood exposure to violence and reported percent of peers in a gang. The reference group is Black men who were not exposed to violence during childhood and who reported that less than 10 percent of their peers were in a gang in the 1997 wave of the NLSY97. Values in brackets are the p-values associated with each coefficient being different from zero.

Exposure to violence and exposure to gang activity also appear to have distinct effects for children. Table 6 shows the results of multivariate regressions of outcomes on an indicator childhood exposure to violence and indicators for the percentage of peers reported to belong to a gang. The reference group in these regressions is the group of Black men who did not see someone shot and who reported that less than 10 percent of their peers belonged to a gang. For all outcomes, the correlation with seeing someone shot during childhood does not disappear when conditioning on the percent of peers in a gang. For most outcomes, seeing someone shot during childhood is correlated at a similar magnitude as reporting that between 75 and >90 percent of peers belong to a gang. The transition from <10 to approximately 25 percent of peers belonging to a gang does very little to most outcomes, while the transition to 50 percent of peers belonging to a gang tends to result in a large negative change in outcomes. Outcomes tend to be monotonically improving as the percentage of peers in a gang decreases.

3 Effects of Adolescent Exposure to Violence and Nurturing Relationships

3.1 Toxic Stress and Nurturing Relationships

By the process of elimination, the results of Section 2 point to trauma as being the primary mechanism through which childhood exposure to violence has long-run effects on the outcomes of Black men. The effects of witnessing a shooting before age 12 do not appear to be driven by selection on observables (Section 2.4) or selection on unobservables (Section 2.5.1). Nor do the effects of this childhood exposure to violence appear to represent broader neighborhood effects (Section 2.6.1) or to be mediated by incarceration (Section 2.6.2).

The importance of psychological costs from exposure to violence is consistent with qualitative evidence on the mental costs of ensuring one's personal security in a violent environment. For example, Tack and Small (2017) find that in neighborhoods with high levels of violence, attempts to manage that violence are the primary driver of elementary school children's interpersonal relationships, including their friendship formation. Ta-Nehisi Coates recalls that during his childhood "each day, fully one-third of my brain was concerned with... securing the body. ... I think I was always, somehow, aware of the price. I think I somehow knew that that third of my brain should have been concerned with more beautiful things" (Coates (2015), p 24).

The importance of psychological costs from exposure to violence is also consistent with the large literature on toxic stress responses. This literature originated in the study of Adverse Childhood Experiences (ACEs, Felitti et al. (1998)), which found that childhood exposures to abuse, neglect, and household dysfunction were correlated with long-run outcomes experienced many years later in adulthood.[15,16] It is important that this literature has studied stress responses rather than the stressors themselves. While the same stressor can lead to subjective, idiosyncratic responses, the stress response itself can be measured in objective, biological terms.

[15] Findings on ACEs parallel those from the literatures on *in utero* nutrition (Barker et al. (1989); Almond and Currie (2011)), early childhood deprivation (Mackes et al. (2020); Tottenham et al. (2010)), and early childhood education (García et al. (2021); Bailey et al. (2021)). In addition to the literature reviews cited in the text, see Tough (2018) for discussions of several strands of related literature.

[16] Many researchers, such as Rajan et al. (2019) and Finkelhor et al. (2013), advocate for exposure to violence to be classified as a type of ACE. Similar to other ACEs, exposure to violence is associated with symptoms of trauma, post-traumatic stress, and diminished long-run health outcomes (Turner et al. (2021); Thompson and Massat (2005); Ford and Browning (2014)).

The National Scientific Council on the Developing Child (2014) places stress responses in three broad categories. Shonkoff and Garner (2012) describe these categories in terms of stressors and the social and emotional buffers to which individuals have access. Brief and mild stressors can lead to positive stress responses when youth are guided by a caring and responsive adult. Even longer and more severe stressors can lead to tolerable stress responses where stress response systems return to their baseline, as long as youth are guided by a caring and responsive adult. Toxic stress responses are generated by frequent or prolonged exposure to severe stressors in the absence of social or emotional buffers. Such stress responses are labeled "toxic" because they can disrupt brain development and other organ and metabolic systems.

In consideration of the findings in Section 2 and the literature on toxic stress, we turn our attention to nurturing relationships. Garner and Saul (2018) summarize the findings from the aforementioned literature as follows: "From a neuroscience perspective, then, what is the antidote to early childhood adversity and toxic stress? It is safe, stable, and nurturing relationships" (p 46). The reason, as noted in Garner and Yogman (2021), is that nurturing relationships "turn off the body's stress machinery in a timely manner" (p 2), before this machinery can generate biological changes that are maladaptive and health harming over the long run. Moreover, recent evidence indicates that nurturing relationships are important for youth development whether or not toxic stress and adversity are present (Bethell et al. (2019a), Bethell et al. (2019b)).

3.2 Measuring Exposure to Violence and Nurturing Relationships

The following analysis quantifies how combinations of violent stressors and social/emotional buffers during adolescence lead to long-run outcomes for Black men. The reason for this focus is that the NLSY97 does not have a great deal of information on exposure to violence and Nurturing Relationships (NRs) during respondents' early childhood years beyond the variables studied in Section 2. However, the NLSY97 does contain a rich set of variables on exposure to violence and Nurturing Relationships (NRs) during respondents' adolescence. These variables are displayed in Table 7.

How should we go about synthesizing the information from these variables? For the sake of exposition the following discussion is focused on exposure to violence. Many related studies create an index or score that is simply the sum of having each type of specific experience. For example, if a measure of exposure to violence V^j is experienced by individual i, then $V_i^j = 1$, otherwise $V_i^j = 0$. An index using the sum of measured variables, analogous to the ACE score, would be

$$\theta_i^{Sum} = \sum_{j=1}^{J} V_i^j.$$

Alternatively, one might consider using Item Response Theory (IRT) to estimate the value of a latent index θ_i^V most likely to produce an individual's response pattern to the variables $V_1, \ldots, V_J$. In this case, we would assume that each item V_i^j is associated with parameters (α_j, β_j) and an

Table 7: Measures of Adolescent Exposure to Violence and Nurturing Relationships in the NLSY97

Exposure to Violence	**Nurturing Relationships**
saw someone shot or shot at[1]	about both the resident mother and father, whether[2]
had home broken into[1]	each is residing with the respondent
victim of repeated bullying[1]	respondent thinks highly of them
victim of a violent crime[1]	respondent thinks they want to be like them
siblings or friends were in a gang[1]	respondent really enjoys spending time with them
percent of peers belong to gang[2]	they often criticize the respondent or their ideas
got into a physical fight at school[2]	respondent thinks they are supportive
something of value stolen at school[2]	they often help the respondent
threatened to be hurt at school[2]	they blame the respondent for their problems
felt unsafe at school[2]	they often cancel plans with the respondent
days/week typically hear gunshots[2]	they know a lot about the respondent's friends
	they know the parents of the respondent's friends
	they know details when respondent not at home
	they often praise the respondent
	whether school's teachers are[2]
	interested in the students
	good
	whether other students get in the way of learning[2]
	percent of peers who[2]
	cut class or skip school
	plan to go to college

Note: 1 indicates variable is measured between ages 12 and 18 (over multiple waves of the NLSY97 survey).
2 indicates variable is measured in wave 1 of the NLSY97 survey, asked only of those respondents aged 14 and younger at the time of the interview.
Violent crime includes physical or sexual assault, robbery, or arson.
Questions about percentages of peers allow for responses in five discrete bins (less than 10 percent; approximately 25, 50, or 75 percent; or more than 90 percent).

error distribution ϵ_i^j governing positive responses as

$$V_i^j = \begin{cases} 1 & \text{if } \alpha_j(\theta_i^{IRT} - \beta_j) - \epsilon_i^j \geq 0 \\ 0 & \text{if } \alpha_j(\theta_i^{IRT} - \beta_j) - \epsilon_i^j < 0. \end{cases}$$

Likewise, we might also consider using Principal Components (PC) Analysis to estimate the location θ_i^{PC} on the line explaining the most variation in the responses to the J questions about exposure to violence. Finally, we might follow Nielsen (2022) and anchor items to a later outcome like high school graduation $Y_i \in \{0, 1\}$, estimating a regression of the form

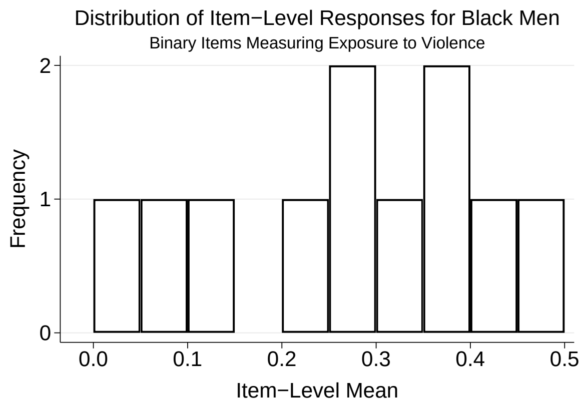
$$Y_i = \beta^1 V_i^1 + \cdots + \beta^J V_i^J + \epsilon_i$$

via Ordinary Least Squares (OLS) to obtain

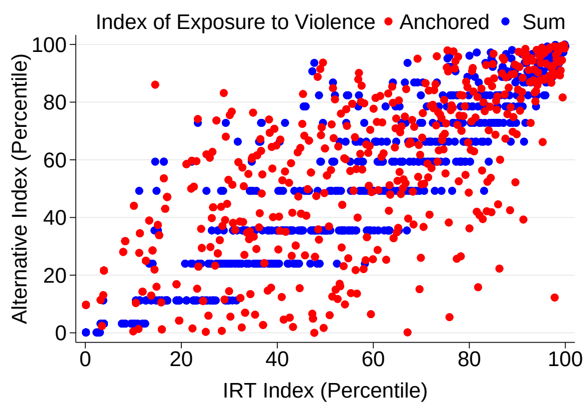
$$\theta_i^{Anchored} = \mathbb{E}[Y | V_i^1, \ldots, V_i^J] = \beta^{1,OLS} V_i^1 + \cdots + \beta^{J,OLS} V_i^J.$$

We could think of the Sum index as giving all questions the same weight, and the IRT, PC, and Anchored approaches as ways of weighting some questions more than others in the creation of the index. These latter approaches are appealing if we think that specific items are more informative about exposure to violence than others. The unequal weighting approaches should be particularly appealing in cases when there is variation in the positive response rates to questions. Figure 12a shows that this is indeed the case after converting the positive responses for all specific items to binary variables.[17] The items measuring exposure to violence have positive response rates that are nearly uniformly distributed between 0 and 50 percent.

Figure 12b shows the disagreement between how the various indexes rank the exposure of individuals. The percentile of the IRT index is represented on the x-axis.[18] On the y-axis in red is the anchored index and in blue is the index that is simply the sum of positive responses.[19] The index anchored to attaining a high school diploma by age 26 has somewhat greater disagreement with the IRT index than does the sum index, and we can also see the discrete nature of the sum index.



(a) The Raw Data



(b) Indexes Created from the Data

Figure 12: Indexes of Exposure to Violence

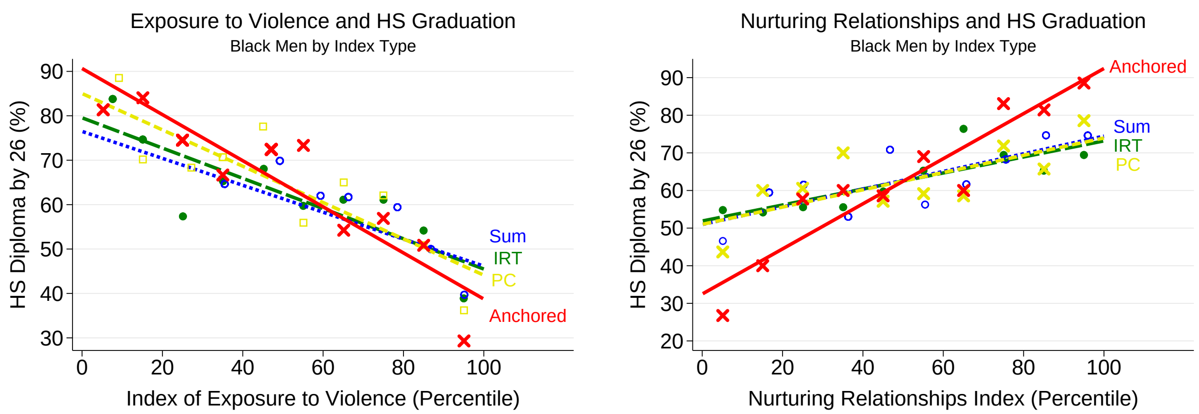
Note: The left panel shows the positive response rates to the specific variables used in the construction of the exposure to violence indexes. The right panel shows the joint distributions of several of these indexes.

Given the range of positive response rates across items, together with their disagreement, we might expect these indexes to predict outcomes differently. However, the indexes perform almost identically irregardless of whether they weight all items equally (Sum) or choose weights to explain the variation among items (IRT, PC). Figure 13a shows that among the indexes of exposure to violence, the Sum and IRT indexes perform very similarly, with a slightly steeper slope for the PC index. Figure 13b shows that among the Nurturing Relationships Indexes, the Sum, IRT, and PC indexes perform identically. Appendix I shows that this general pattern holds among other outcomes, including those not targeted by the Anchored index: the Sum, IRT, and PC indexes perform almost identically for the NR index; and the PC index generates slightly steeper slopes for the index of exposure to violence.

[17]For example, the item "How many days per week do you typically hear gunshots in your neighborhood" is converted into an indicator for typically hearing gunshots at least 1 day per week.

[18]Details of the IRT estimation, including robustness to alternative distributional assumptions, are available in Appendix H.

[19]Multi-valued responses are handled in the anchored index using quadratic terms and in the sum index simply using all possible values.



(a) HS Graduation by Indexes of Exposure to Violence (b) HS Graduation by Nurturing Relationships Indexes

Figure 13: Indexes of Treatments and High School Graduation
Note: These figures show binned scatterplots and best fit lines of high school graduation rates as a function of the percentile of each index of exposure to violence (left panel) and nurturing relationships (right panel).

While the Sum, IRT, and PC indexes all perform similarly, the Anchored index outperforms the other indexes. Figure 13 shows that the anchored index has a steeper relationship with outcomes than do any of the other indexes. This is especially clear for the NR index shown in Figure 13b, and Appendix I shows that this remains true for additional outcomes, including those not used in the anchoring.[20] The key distinction between the weights chosen by the Anchored index and those chosen by the IRT or PC indexes is that the Anchored index weights items by their ability to predict future outcomes rather than their ability to predict other items.

Two comparisons are natural for these findings. Most notably, several results in education have been shown to be sensitive to the scale with which tests are measured (Bond and Lang (2013); Nielsen (2015); Agostinelli and Wiswall (2016); Cunha et al. (2021)). This would seem to suggest that the IRT or PC indexes would outperform the sum index. However, consistent with our findings, Hosseini et al. (2022) show that an equally weighted frailty index of health performs similarly to a PC-weighted index in predicting health outcomes.[21] Understanding when the relative strengths of each index apply to specific empirical contexts is an important direction for future research.

We define our treatment using the Anchored index as a result of its outperformance of the other indexes. Denoting percentile p of the distribution of random variable X as $\pi_p(X)$, we define discrete treatment variables as:

$$D^V = \begin{cases} \text{Low} & \text{if} \quad \theta^V \geq \pi_{50}(\theta^V) \\ \text{High} & \text{if} \quad \theta^V < \pi_{50}(\theta^V), \end{cases} \qquad \text{and} \qquad D^{NR} = \begin{cases} \text{Low} & \text{if} \quad \theta^{NR} < \pi_{50}(\theta^{NR}) \\ \text{High} & \text{if} \quad \theta^{NR} \geq \pi_{50}(\theta^{NR}). \end{cases}$$

[20] These slopes might be even steeper if one were to account for the measurement error in test scores (Nielsen (2022); Williams (2019)).

[21] Aliprantis et al. (2023b) show that there is considerable disagreement in neighborhood rankings based on the outcomes of current versus previous residents, and that much of this disagreement can be explained by changes in contemporaneous residents over time.

Our discretized treatment variables order respondents by their exposure to violence and nurturing relationships. Table 8 shows that as we move from the low to high levels of our exposure to violence treatment, the percent of respondents who reported seeing someone shot at between 12 and 18 increases from 11 to 40 percent. All of the specific variables measuring exposure to violence used in the estimation follow this pattern, to such an extent that our discrete treatment captures important variation in the measures of exposure to violence available in the NLSY97.

Table 8: Means of Specific Violent Experiences for Adolescent Black Males by Level of Treatment D^V

	D^V	
Specific Measure of Exposure	Low	High
saw someone shot or shot at (%)	11	40
had home broken into (%)	12	14
victim of repeated bullying (%)	4	10
victim of a violent crime (%)	0	9
siblings or friends were in a gang (%)	32	65
% of peers belong to gang	14	32
Hear gunshots in nbd (days/week)	0.5	1.6
at school:		
got into a physical fight (frequency)	0.1	1.5
something of value stolen (frequency)	0.5	0.9
threatened to be hurt (frequency)	0.5	1.0
felt unsafe (%)	4	9

Note: D^V is our created binary treatment measuring exposure to violence.

Table 9 shows that as we move from the low to high levels of our nurturing relationships treatment, the percent of respondents who report thinking highly of their father increases from 61 to 86 percent. Only 52 percent of respondents in the low NR treatment report that their father often praises them, compared with 69 percent of respondents in the high NR treatment. Just as we saw with the exposure to violence treatment, nearly all of the specific variables measuring NRs used in the estimation follow the desired pattern, with the discrete treatment capturing important variation in the measures of NRs available in the NLSY97.

Table 9: Means of Specific Nurturing Relationships Questions for Adolescent Black Males by Level of Treatment D^{NR}

	Mother D^{NR}		Father D^{NR}	
Specific Measure of Parental NRs	Low	High	Low	High
Residing with respondent (%)	90	98	45	66
respondent thinks highly of them (%)	70	92	61	86
respondent thinks they want to be like them (%)	52	63	40	64
respondent really enjoys spending time with them (%)	79	85	63	80
they often praise the respondent (%)	70	80	52	69
they often help the respondent (%)	81	80	61	65
they know a lot about the respondent's friends (%)	51	40	25	29
they know the parents of the respondent's friends (%)	31	33	16	31
they know details when respondent not at home (%)	54	74	35	51
they know the respondent's teachers (%)	61	72	39	47
they often criticize the respondent or their ideas (%)	25	8	16	16
they blame the respondent for their problems (%)	9	3	3	6
they often cancel plans with the respondent (%)	9	3	13	7
	D^{NR}			
Specific Measure of Non-Parental NRs	Low	High		
teachers care about the students (%)	74	89		
teachers are interested in the students (%)	75	93		
% of peers who plan to go to college	59	63		
% of peers who cut class	33	27		
peers disrupt learning (%)	38	32		

Note: D^{NR} is our created binary treatment measuring Nurturing Relationships.

3.3 Potential Outcomes and Causal Effects

We estimate potential outcomes as functions of exposure to violence and nurturing relationships as $\mathbb{E}[Y(D^V, D^{NR})]$ where each treatment is the binary variable defined in the previous section. We estimate these potential outcomes under an assumption of selection on observables

$$D^V, D^{NR} \perp\!\!\!\perp Y(D^V, D^{NR}) \,|\, W. \tag{1}$$

We follow Imbens (2015) in implementing the selection on observables assumption in Equation 1 by estimating

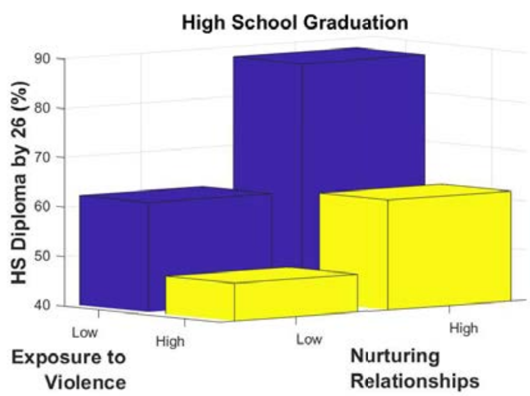
$$\mathbb{E}[Y|W, D^V, D^{NR}]$$

via OLS with separate coefficients for each subgroup of D^V and D^{NR}. We then estimate potential outcomes $\mathbb{E}[Y^{D^V,D^{NR}}]$ as

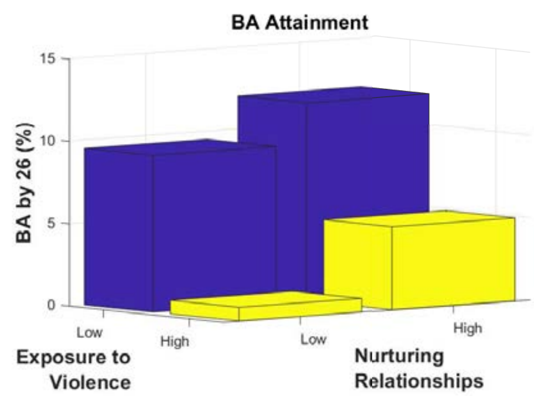
$$\mathbb{E}[Y^{LL}] = \mathbb{E}[\widehat{\beta}^{LL}_{OLS} W].$$

The potential outcomes in the other treatment combinations $\mathbb{E}[Y^{LH}]$, $\mathbb{E}[Y^{HL}]$, and $\mathbb{E}[Y^{HH}]$ are estimated analogously.

Figure 14 shows the estimated potential outcomes for educational attainment by age 26. In Figure 14a we can see a large drop-off in high school graduation rates as exposure to violence increases. Likewise, at all levels of exposure to violence, increasing NRs has large positive effects on graduation rates. Similar patterns obtain for BA attainment as shown in Figure 14b.



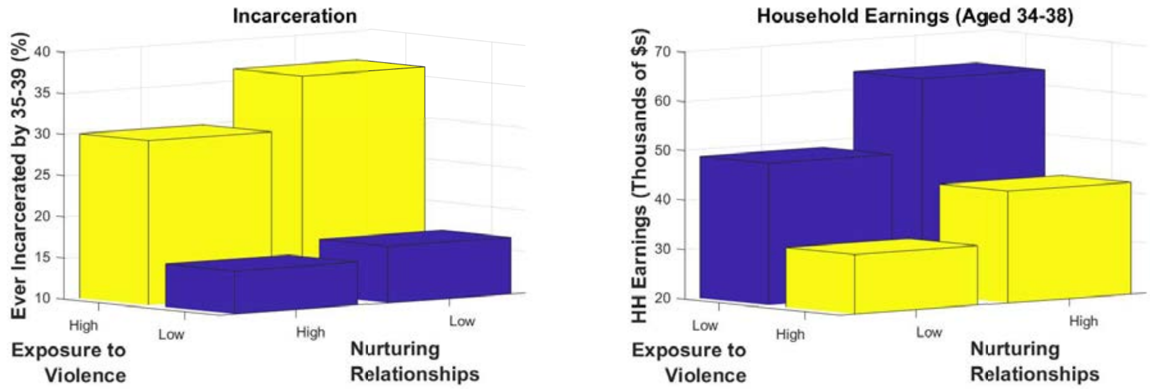
(a) High School



(b) BA

Figure 14: Potential Outcomes for Educational Attainment

Figure 15 shows that there are large effects of adolescent exposure to violence and nurturing relationships on the adult outcomes of Black men. Figure 15a shows a massive difference in the incarceration rates of those exposed to high versus low levels of violence in their adolescence. In terms of incarceration, the benefits from nurturing relationships accrue to those who were exposed to high levels of violence. Figure 15b shows that Black men's household earnings in their late 30s is highest when they were exposed to low levels of violence and high levels of nurturing relationships during their adolescence. In terms of household earnings, the benefits of nurturing relationships are experienced by those exposed to both high and low levels of violence, although the greatest benefits are to those exposed to low levels of violence.



(a) Incarceration

(b) Household Earnings

Figure 15: Potential Outcomes for Incarceration and Household Earnings

Table 10 provides the numerical values of the treatment effects implied by these potential outcomes along with one-sided p-values of these effects being different from zero obtained from 1,000 bootstrap replications of the estimation of both the item-anchored index and the potential outcomes. These effects are economically large and statistically significant.

Table 10: Effects of Changing Treatments

	Given High Exposure to Violence and Low Nurturing Relationships		
	$\downarrow D^V$	$\uparrow D^{NR}$	Both
HS Diploma	14.5	14.5	40.3
(% by 26)	[0.00]	[0.00]	[0.00]
BA Attainment	8.7	4.2	11.2
(% by 26)	[0.03]	[0.12]	[0.00]
Household Earnings	16.6	10.5	31.6
(1,000s of 2018 $s)	[0.01]	[0.04]	[0.00]
Ever Incarcerated	–19.6	–6.5	–21.3
(% by 2019)	[0.00]	[0.00]	[0.00]

Note: The p-values of one-sided tests for each coefficient being different from 0 are reported in brackets [ ] and are obtained from 1,000 bootstrap replications. ACE is "Adverse Childhood Experience" and NR is "Nurturing Relationship." See text for variable descriptions.

The results highlight the importance of nurturing relationships in several ways. One feature of nurturing relationships is that they improve outcomes at all levels of exposure to violence. For example, providing an adolescent Blak male with high levels of nurturing relationships would increase their adult household earnings by \$11,000 when exposed to high levels of violence and \$15,000 when exposed to low levels of violence. Another feature is that nurturing relationships are not only substitutes for shielding adolescents from violence, but complementary. For example, providing adolescent Black males with high levels of nurturing relationships or shielding them from high levels of exposure to violence would increase their high school graduation rates by 15 percentage points, from a base of 48 percent. Improving both of these treatments at the same time would increase high school graduation rates by 40 percentage points. We note that these large improvements in outcomes are broadly in line with the magnitudes of changes in flourishing found in Bethell et al. (2019b).

Two broad considerations related to selection into adolescent exposure to violence are relevant for interpreting the estimation results. First, Appendix G presents results that suggest that a selection on observables assumption may be reasonable for adolescent exposure to violence. We find similar results for the effects of non-violent adversity and nurturing relationships, with non-violent adversity from an unemployed parent or the death of a parent or sibling more plausibly random than exposure to violence. Second, it is notable that those exposed to violence are very positively affected by nurturing relationships. Even if adolescent exposure to violence were driven by selection to an important extent, nurturing relationships would still be effective for each selected subpopulation.

3.4 Potential Outcomes and a Policymaker's Decision Problem

We now use the potential outcomes from the previous section as inputs into a policymaker's decision problem by supposing that a program Z would lead some share of compliers to change the treatment they receive from low to high nurturing relationships or to change from high to low exposure to violence. Assuming that potential outcomes are uniform across compliers, always-takers, never-takers, and defiers, we can use the potential outcomes estimated in the previous section to calculate the benefit an intervention would have assuming a specific share of compliers.

Table 11 presents estimates of the benefits of programs accruing from long-run effects on Black men. Table 12 presents the estimated costs of programs that might provide adolescent Black males with nurturing relationships, safety, or both. Even at low compliance rates, and even focusing only on benefits directly accruing from the effects on Black men, the costs are outweighed by the benefits of scaling programs like either Boys and Girls Clubs for all 12-18 Black males or else wrap-around services provided via a dedicated family support specialist serving each K-12 Title I school (modeled after the Say Yes Cleveland). When considering nurturing relationships and safety as a place-based policy, it is worth noting that related programs would seem to have increasing marginal returns to investment. This contrasts with dispersing participants through housing mobility programs that would, at some point, seem to have decreasing marginal returns to spending (Agostinelli et al. (2020)).

Table 11: Annual Program Benefits Accruing from Black Males' Participation

	Compliers	Benefit of Providing: NRs	Safety	Both
Ind. Earnings				
	10%	\$3.7B	\$5.2B	\$12.0B
	25%	\$9.1B	\$13.1B	\$29.9B
Incarceration				
	10%	\$1.4B	\$5.4B	\$9.4B
	25%	\$3.4B	\$13.6B	\$23.5B

Note: Providing NRs means an intervention alters D^{NR} from 0 to 1. Providing safety means an intervention alters D^{V} from 1 to 0. Increased individual earnings are calculated for the population of prime age Black men, or those aged 25-54, in five year windows using an age-earnings profile estimated on the Black males in the National Longitudinal Survey of Youth 1979 (1979) following the assumptions adopted in Aliprantis et al. (2023a). Decreased costs of incarceration are calculated assuming one spell of 6 months randomly timed before age 40 for Black men at the Federal Register's estimate of an average of \$108 per day using data from fiscal year 2019.

Table 12: Annual Program Costs

Program	Program/Study	Cost
Boys and Girls Clubs*	Boys & Girls Clubs (2023)	\$2.2B
Big Brothers/Big Sisters*	Alfonso et al. (2019)	\$3.0B
Wrap-Around Services*	Say Yes Cleveland	\$5.2B
School-Wide Tutoring*	Kraft and Falken (2021)	\$5-\$16B
Summer/After-School	American Rescue Plan	\$6B
High-Dosage Tutoring*	Guryan et al. (2023)	\$9.5-11.7B
Student Supports*	Oreopoulos et al. (2017)	\$19.0B

Note: * indicates all students in Title I K-12 Schools. * indicates Black males aged 12-18 in the 2020 Census. Cost estimates in Oreopoulos et al. (2017) are in 2018 dollars.

4 Conclusion

In an unsafe environment, ensuring one's physical security can dominate one's life. This is true for Black males growing up in unsafe areas in the US, a phenomenon that has been described in academic studies (Anderson (1999); Tack and Small (2017)) and personal memoirs (Coates (2015); Canada (1995)).

This paper made contributions to the literature on exposure to violence by showing the magnitude of long-run effects on Black men in the US. We found that seeing someone shot or shot at when aged 11 or younger was associated with 31 percent lower household earnings in the late 30s. We found that when viewed as a causal effect, this gap is unlikely to be driven by sorting on observables or unobservables. When we investigated mechanisms, we found that the effects of childhood exposure are distinct from those of growing up in a low SES neighborhood, as the gap in household earnings from childhood exposure to violence is constant across neighborhood socioeconomic status (SES). This result is somewhat surprising given the concentration of exposure to violence in the neighborhoods with the lowest SES. We also found that incarceration is not a major mediator of exposure to violence, and that the reported exposure to violence is distinct from exposure to gang behavior. Collectively, these results indirectly implicate the trauma and toxic stress response from childhood exposure to violence as the main mechanism through which exposure to violence affects long-run outcomes.

Guided by the literature on Adverse Childhood Experiences (ACEs) and toxic stress responses, we went on to study how nurturing relationships moderate the effects of exposure to violence in adolescence. The NLSY97 has a wide range of variables measuring these treatments, so we first investigate how to best synthesize these variables. We found that simply summing the positive responses to all variables predicts later outcomes just as well as indexes created by Item Response Theory (IRT) or the first Principal Component (PC) of the variables. In contrast, indexes based on item-anchored scales predict outcomes better than indexes based on summing, IRT, or PC. A strength of our analysis is that with the rich set of variables available in the NLSY97, we were able to estimate potential outcomes under a selection on observables assumption.

Our findings on exposure to violence and nurturing relationships during adolescence have a clear implication: nurturing relationships are a lever capable of supporting positive long-run outcomes, even for adolescents. Our results are located at the intersection of the literature on neighborhood effects and the role of family and peers in child development. Neighborhoods matter because of the people with whom children and adolescents interact in them. Relationships matter because of the way they help children and adolescents navigate those interactions. Our results point to the strength of these mechanisms, operating both independently and together.

References

Agostinelli, Francesco, Matthias Doepke, Giuseppe Sorrenti, and Fabrizio Zilibotti (2020). "It takes a village: the economics of parenting with neighborhood and peer effects." Working paper 27050, National Bureau of Economic Research. doi:10.3386/w27050.

Agostinelli, Francesco and Matthew Wiswall (2016). "Identification of dynamic latent factor models: The implications of re-normalization in a model of child development." Working paper 22441, National Bureau of Economic Research. doi:10.3386/w22441.

Aguiar, Mark, Mark Bils, Kerwin Kofi Charles, and Erik Hurst (2021). "Leisure luxuries and the labor supply of young men." *Journal of Political Economy*, 129(2), pp. 337–382. doi:10.1086/711916.

Aizer, Anna (2009). "Neighborhood violence and urban youth." In Jonathan Gruber, editor, *The Problems of Disadvantaged Youth: An Economic Perspective*, pp. 275–307. University of Chicago Press. URL https://www.nber.org/books-and-chapters/problems-disadvantaged-youth-economic-perspective/ne

Alfonso, Y. Natalia, Sarah Lindstrom Johnson, Tina Cheng, Vanya Jones, Leticia Ryan, Joel Fein, and David Bishai (2019). "A marginal cost analysis of a Big Brothers Big Sisters of America youth mentoring program: new evidence using statistical analysis." *Children and Youth Services Review*, 101, pp. 23–32. doi:10.1016/j.childyouth.2019.03.002.

Aliprantis, Dionissi (2017a). "Assessing the evidence on neighborhood effects from Moving to Opportunity." *Empirical Economics*, 52(3), pp. 925–954. doi:10.1007/s00181-016-1186-1.

Aliprantis, Dionissi (2017b). "Human capital in the inner city." *Empirical Economics*, 53(3), pp. 1125–1169. doi:10.1007/s00181-016-1160-y.

Aliprantis, Dionissi, Daniel Carroll, and Eric Young (2022). "What explains neighborhood sorting by income and race?" *Journal of Urban Economics*. doi:10.1016/j.jue.2022.103508.

Aliprantis, Dionissi, Daniel Carroll, and Eric Young (2023a). "The dynamics of the racial wealth gap." *Mimeo, Federal Reserve Bank of Cleveland*.

Aliprantis, Dionissi and Anne Chen (2016). "The consequences of exposure to violence during early childhood." *Economic Commentary*, (2016-03). doi:10.26509/frbc-ec-201603.

Aliprantis, Dionissi, Hal Martin, and Kristen Tauber (2023b). "What determines the success of housing mobility programs?" *Mimeo, FRB Cleveland*.

Aliprantis, Dionissi and Francisca G.-C. Richter (2020). "Evidence of neighborhood effects from Moving to Opportunity: LATEs of neighborhood quality." *The Review of Economics and Statis-tics*, 102(4), pp. 633–647. doi:10.1162/rest_a_00933.

Almond, Douglas and Janet Currie (2011). "Killing me softly: The Fetal Origins Hypothesis." *Journal of Economic Perspectives*, 25(3), pp. 153–172. doi:10.1257/jep.25.3.153.

Altonji, Joseph G, Todd E Elder, and Christopher R Taber (2005). "Selection on observed and unobserved variables: assessing the effectiveness of Catholic schools." *Journal of Political Economy*, 113(1), pp. 151–184. doi:10.1086/426036.

Anderson, Elijah (1999). *Code of the street: decency, violence, and the moral life of the inner city*. W. W. Norton & Company.

Angrist, Joshua D. and Jörn-Steffen Pischke (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.

Bailey, Martha J., Shuqiao Sun, and Brenden Timpe (2021). "Prep school for poor kids: the long-run impacts of Head Start on human capital and economic self-sufficiency." *American Economic Review*, 111(12), pp. 3963–4001. doi:10.1257/aer.20181801.

Bancalari, P., M. Sommer, and S. Rajan (2022). "Youth exposure to endemic community gun violence: a systematic review." *Adolescent Research Review*, 7, pp. 383–417. doi: 10.1007/s40894-022-00178-5.

Barker, David J. P., Clive Osmond, P. D. Winter, Barrie Margetts, and Shirley J. Simmonds (1989). "Weight in infancy and death from ischaemic heart disease." *The Lancet*, 334(8663), pp. 577–580. doi:10.1016/S0140-6736(89)90710-1.

Bayer, Patrick and Kerwin Kofi Charles (2018). "Divergent paths: a new perspective on earnings differences between Black and white men since 1940." *The Quarterly Journal of Economics*, 133(3), pp. 1459–1501. doi:10.1093/qje/qjy003.

Bethell, Christina, Jennifer Jones, Narangerel Gombojav, Jeff Linkenbach, and Robert Sege (2019a). "Positive Childhood Experiences and adult mental and relational health in a statewide sample: Associations across Adverse Childhood Experiences levels." *JAMA Pediatrics*, 173(11), pp. e193,007–e193,007. doi:10.1001/jamapediatrics.2019.3007.

Bethell, Christina D., Narangerel Gombojav, and Robert C. Whitaker (2019b). "Family resilience and connection promote flourishing among US children, even amid adversity." *Health Affairs*, 38(5), pp. 729–737. doi:10.1377/hlthaff.2018.05425.

Bingenheimer, Jeffrey B., Robert T. Brennan, and Felton J. Earls (2005). "Firearm violence exposure and serious violent behavior." *Science*, 308, pp. 1323–1326. doi:10.1126/science.1110096.

Bond, Timothy N and Kevin Lang (2013). "The evolution of the Black-White test score gap in grades K–3: the fragility of results." *Review of Economics and Statistics*, 95(5), pp. 1468–1479. doi:10.1162/REST_a_00370.

Bond, Timothy N. and Kevin Lang (2018). "The Black–white education scaled test-score gap in grades K-7." *Journal of Human Resources*, 53(4), pp. 891–917. doi:10.3368/jhr.53.4.0916.8242R.

Boys & Girls Clubs (2023). *2021 Annual Report*. Boys & Girls Clubs of America, Atlanta, GA. URL https://www.bgca.org/-/media/Documents/AboutUs/2023/2021_Annual_Report_web.ashx.

Bruhn, Jesse (2021). "Competition in the black market: estimating the causal effect of gangs in Chicago." *Mimeo, Brown University*.

Canada, Geoffrey (1995). *Fist stick knife gun: a personal history of violence in america*. Beacon Press.

Casey, Marcus, Jeffrey C. Schiman, and Maciej Wachala (2018). "Local violence, academic performance, and school accountability." *AEA Papers and Proceedings*, 108, pp. 213–216. doi:10.1257/pandp.20181109.

Chang, Yoosoon, Steven N. Durlauf, Seunghee Lee, and Joon Y. Park (2023). "A trajectories-based approach to measuring intergenerational mobility." *NBER Working Paper 31020*. doi:10.3386/w31020.

Cheon, Chaeyoung, Yuzhou Lin, David J. Harding, Wei Wang, and Dylan S. Small (2020). "Neighborhood racial composition and gun homicides." *JAMA Network Open*, 3(11), pp. e2027,591–e2027,591. doi:10.1001/jamanetworkopen.2020.27591.

Coates, Ta-Nehisi (2015). *Between the World and Me*. One World Press.

Cunha, Flávio, Irma Elo, and Jennifer Culhane (2022). "Maternal subjective expectations about the technology of skill formation predict investments in children one year later." *Journal of Econometrics*, 231(1), pp. 3–32. doi:10.1016/j.jeconom.2020.07.044. Annals Issue: Subjective Expectations Probabilities in Economics.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach (2010). "Estimating the technology of cognitive and noncognitive skill formation." *Econometrica*, 78(3), pp. 883–931. doi:10.3982/ECTA6551.

Cunha, Flavio, Eric Nielsen, and Benjamin Williams (2021). "The econometrics of early childhood human capital and investments." *Annual Review of Economics*, 13(1), pp. 487–513. doi:10.1146/annurev-economics-080217-053409.

Davis, David Brion (2006). *Inhuman Bondage: The Rise and Fall of Slavery in the New World*. Oxford University Press.

Dumornay, N. M., L. A. M. Lebois, K. J. Ressler, and N. G. Harnett (2023). "Racial disparities in adversity during childhood and the false appearance of race-related differences in brain structure." *American Journal of Psychiatry*, 180(2), pp. 127–138. doi:10.1176/appi.ajp.21090961.

Fagan, J. and A. Geller (2018). "Police, race, and the production of capital homicides." *Berkeley Journal of Criminal Law*, 23(3), pp. 261–313. URL https://www.bjcl.org/assets/files/23.3-Fagan-Geller.pdf.

Felitti, Vincent J., Robert F. Anda, Dale Nordenberg, David F. Williamson, Alison M. Spitz, Valerie Edwards, and James S. Marks (1998). "Relationship of childhood abuse and household dysfunction to many of the leading causes of death in adults: The Adverse Childhood Experiences (ACE) Study." *American Journal of Preventive Medicine*, 14(4), pp. 245–258. doi:10.1016/S0749-3797(98)00017-8.

Finkelhor, David, Anne Shattuck, Heather Turner, and Sherry Hamby (2013). "Improving the Adverse Childhood Experiences study scale." *JAMA Pediatrics*, 167(1), pp. 70–75. doi:10.1001/jamapediatrics.2013.420.

Finkelhor, David, Heather A. Turner, Anne Shattuck, and Sherry L. Hamby (2015). "Prevalence of childhood exposure to violence, crime, and abuse: results from the National Survey of Children's Exposure to Violence." *JAMA Pediatrics*, 169(8), pp. 746–754. doi:10.1001/jamapediatrics.2015.0676.

Ford, Jodi L. and Christopher R. Browning (2014). "Effects of exposure to violence with a weapon during adolescence on adult hypertension." *Annals of Epidemiology*, 24(3), pp. 193–198. doi:10.1016/j.annepidem.2013.12.004.

García, Jorge Luis, James J. Heckman, and Victor Ronda (2021). “The lasting effects of early childhood education on promoting the skills and social mobility of disadvantaged African Americans.” *NBER Working Paper 29057*. doi:10.3386/w29057.

Garner, Andrew S. and Robert A. Saul (2018). *Thinking Developmentally: Nurturing Wellness in Childhood to Promote Lifelong Health*. American Academy of Pediatrics. doi:10.1542/9781610021531.

Garner, Andrew S. and Michael Yogman (2021). “Preventing childhood toxic stress: partnering with families and communities to promote relational health.” *Pediatrics*, 148(2). doi:10.1542/peds.2021-052582.

Gertler, Paul, James Heckman, Rodrigo Pinto, Arianna Zanolini, Christel Vermeersch, Susan Walker, Susan M. Chang, and Sally Grantham-McGregor (2014). “Labor market returns to an early childhood stimulation intervention in Jamaica.” *Science*, 344(6187), pp. 998–1001. doi:10.1126/science.1251178.

Goldberger, Arthur S. and Charles F. Manski (1995). “Review Article: *The Bell Curve* by Herrnstein and Murray.” URL https://www.jstor.org/stable/2729026.

Graham, Bryan S. (2018). “Identifying and estimating neighborhood effects.” *Journal of Economic Literature*, 56(2), pp. 450–500. doi:10.1257/jel.20160854.

Guryan, Jonathan, Jens Ludwig, Monica P. Bhatt, Philip J. Cook, Jonathan M. V. Davis, Kenneth Dodge, George Farkas, Jr. Fryer, Roland G., Susan Mayer, Harold Pollack, Laurence Steinberg, and Greg Stoddard (2023). “Not too late: Improving academic outcomes among adolescents.” *American Economic Review*, 113(3), pp. 738–765. doi:10.1257/aer.20210434.

Hainmueller, Jens (2012). “Entropy balancing for causal effects: a multivariate reweighting method to produce balanced samples in observational studies.” *Political Analysis*, 20(1), pp. 25–46. doi:10.1093/pan/mpr025.

Heckman, James, Hidehiko Ichimura, Jeffrey Smith, and Petra Todd (1998). “Characterizing selection bias using experimental data.” *Econometrica*, 66(5), pp. 1017–1098. doi:10.2307/2999630.

Herrnstein, Richard J. and Charles Murray (1996). *The Bell Curve: Intelligence and Class Structure in American Life*. Free Press Paperbacks.

Horowitz, Joel L. and Charles F. Manski (1995). “Identification and robustness with contaminated and corrupted data.” *Econometrica*, pp. 281–302. doi:10.2307/2951627.

Hosseini, Roozbeh, Karen A. Kopecky, and Kai Zhao (2022). “The evolution of health over the life cycle.” *Review of Economic Dynamics*, 45, pp. 237–263. doi:10.1016/j.red.2021.07.001.

Imbens, Guido W. (2015). “Matching methods in practice: three examples.” *Journal of Human Resources*, 50(2), pp. 373–419. doi:10.3368/jhr.50.2.373.

Kraft, Matthew A., Alexander J. Bolves, and Noelle M. Hurd (2023). "How informal mentoring by teachers, counselors, and coaches supports students' long-run academic success." *NBER Working Paper 31257*. doi:10.3386/w31257.

Kraft, Matthew A. and Grace T. Falken (2021). "A blueprint for scaling tutoring and mentoring across public schools." *AERA Open*, 7, pp. 1–21. doi:10.1177/23328584211042858.

Laurito, Agustina, Johanna Lacoe, Amy Ellen Schwartz, Patrick Sharkey, and Ingrid Gould Ellen (2019). "School climate and the impact of neighborhood crime on test scores." *RSF: The Russell Sage Foundation Journal of the Social Sciences*, 5(2), pp. 141–166. doi:10.7758/rsf.2019.5.2.08.

Lavecchia, Adam M., Philip Oreopoulos, and Robert S. Brown (2020). "Long-run effects from comprehensive student support: Evidence from Pathways to Education." *American Economic Review: Insights*, 2(2), pp. 209–224. doi:10.1257/aeri.20190114.

Mackes, Nuria K., Dennis Golm, Sagari Sarkar, Robert Kumsta, Michael Rutter, Graeme Fairchild, Mitul A. Mehta, and Edmund J. S. Sonuga-Barke (2020). "Early childhood deprivation is associated with alterations in adult brain structure despite subsequent environmental enrichment." *Proceedings of the National Academy of Sciences*, 117(1), pp. 641–649. doi:10.1073/pnas.1911264116.

Manski, Charles F. (1990). "Nonparametric bounds on treatment effects." *American Economic Review*, 80(2), pp. 319–323. URL https://www.jstor.org/stable/2006592.

Manski, Charles F. (2011). "Genes, eyeglasses, and social policy." *Journal of Economic Perspectives*, 25(4), pp. 83–94. doi:10.1257/jep.25.4.83.

Masten, Matthew A. and Alexandre Poirier (2018). "Identification of treatment effects under conditional partial independence." *Econometrica*, 86(1), pp. 317–351. doi:10.3982/ECTA14481.

Masten, Matthew A. and Alexandre Poirier (2020). "Inference on breakdown frontiers." *Quantitative Economics*, 11(1), pp. 41–111. doi:10.3982/QE1288.

Masten, Matthew A., Alexandre Poirier, and Linqi Zhang (Forthcoming). "Assessing sensitivity to unconfoundedness: estimation and inference." *Journal of Business & Economic Statistics*. doi:10.1080/07350015.2023.2183212.

Monteiro, Joana and Rudi Rocha (2017). "Drug battles and school achievement: evidence from Rio de Janeiro's favelas." *The Review of Economics and Statistics*, 99(2), pp. 213–228. doi: 10.1162/REST_a_00628.

Murray, Charles (2021). *Facing Reality: Two Truths about Race in America*. Encounter Books.

NCHS (2021). *Health, United States*. National Center for Health Statistics, Washington, DC. URL https://www.cdc.gov/nchs/hus/data-finder.htm.

Neal, Derek and Armin Rick (2014). "The prison boom & the lack of Black progress after Smith & Welch." *Mimeo, University of Chicago*.

Neelakantan, Urvi, Grey Gordon, John Jones, and Kartik Athreya (2022). "Incarceration, employment, and earnings: dynamics and differences." *Mimeo, Richmond Fed*.

Nielsen, Eric Reed (2015). "Achievement estimates and deviations from cardinal comparability." *Mimeo, Federal Reserve Board of Governors*.

Nielsen, Eric Reed (2022). "Test questions, economic outcomes, and inequality." *Mimeo, Federal Reserve Board of Governors*.

O'Flaherty, Brendan (2015). *The Economics of Race in the United States*. Harvard University Press.

O'Flaherty, Brendan (2016). *Race may be pseudo-science, but economists ignore it at their peril*. Institute for New Economic Thinking, Detroit, MI, INET Conference on the Economics of Race edition.

O'Flaherty, Brendan and Rajiv Sethi (2010). "Homicide in black and white." *Journal of Urban Economics*, 68(3), pp. 215–230. doi:10.1016/j.jue.2010.06.001.

O'Flaherty, Brendan and Rajiv Sethi (2019). *Shadows of Doubt: Stereotypes, Crime, and the Pursuit of Justice*. Harvard University Press.

Olds, D. L. (2002). "Prenatal and infancy home visiting by nurses: from randomized trials to community replication." *Prevention Science*, 3, pp. 153–172. doi:10.1023/A:1019990432161.

Oreopoulos, Philip, Robert S. Brown, and Adam M. Lavecchia (2017). "Pathways to Education: an integrated approach to helping at-risk high school students." *Journal of Political Economy*, 125(4), pp. 947–984. doi:10.1086/692713.

Oster, Emily (2019). "Unobservable selection and coefficient stability: theory and evidence." *Journal of Business & Economic Statistics*, 37(2), pp. 187–204. doi:10.1080/07350015.2016.1227711.

Perry, Danielle M., Karen M. Tabb, and Ruby Mendenhall (2015). "Examining the effects of urban neighborhoods on the mental health of adolescent African American males: A qualitative systematic review." *Journal of Negro Education*, 84(3), pp. 254–268. doi:10.7709/jnegroeducation.84.3.0254.

Pierre, C. L., A. Burnside, and N. K. Gaylord-Harden (2020). "A longitudinal examination of community violence exposure, school belongingness, and mental health among African-American adolescent males." *School Mental Health*, 12, pp. 388–399. doi:10.1007/s12310-020-09359-w.

Rajan, Sonali, Charles C. Branas, Dawn Myers, and Nina Agrawal (2019). "Youth exposure to violence involving a gun: evidence for adverse childhood experience classification." *Journal of Behavioral Medicine*, 42, pp. 646–657. doi:10.1007/s10865-019-00053-0.

Sampson, Robert J. (2012). *Great American City: Chicago and the Enduring Neighborhood Effect*. The University of Chicago Press.

Sharkey, Patrick, Amy Ellen Schwartz, Ingrid Gould Ellen, and Johanna Lacoe (2014). "High stakes in the classroom, high stakes on the street: the effects of community violence on student's standardized test performance." *Sociological Science*, 1, pp. 199–220. doi:10.15195/v1.a14.

Shonkoff, Jack P. and Andrew S. Garner (2012). "The lifelong effects of early childhood adversity and toxic stress." *Pediatrics*, 129(1), pp. e232–e246. doi:10.1542/peds.2011-2663.

Sylvera, Craig (2023). "Black mayors, officers, and arrests." *Mimeo, Cleveland Fed*.

Tack, Anjanette M. Chan and Mario L. Small (2017). "Making friends in violent neighborhoods: strategies among elementary school children." *Sociological Science*, 4(10), pp. 224–248. doi:10.15195/v4.a10.

The National Scientific Council on the Developing Child (2014). "Excessive stress disrupts the architecture of the developing brain." Working paper 3, Harvard University. URL https://developingchild.harvard.edu/wp-content/uploads/2005/05/Stress_Disrupts_Architecture_

Thompson, Owen (2021). "Human capital and Black-white earnings gaps, 1966-2017." *NBER Working Paper*. doi:10.3386/w28586.

Thompson, Theodre and Carol Rippey Massat (2005). "Experiences of violence, post-traumatic stress, academic achievement and behavior problems of urban African-American children." *Child and Adolescent Social Work Journal*, 22, pp. 367–393. doi:10.1007/s10560-005-0018-5.

Torrats-Espinosa, Gerard (2020). "Crime and inequality in academic achievement across school districts in the United States." *Demography*, 57(1), pp. 123–145. doi:10.1007/s13524-019-00850-x.

Tottenham, Nim, Todd A. Hare, Brian T. Quinn, Thomas W. McCarry, Marcella Nurse, Tara Gilhooly, Alexander Millner, Adriana Galvan, Matthew C. Davidson, Inge-Marie Eigsti, et al. (2010). "Prolonged institutional rearing is associated with atypically large amygdala volume and difficulties in emotion regulation." *Developmental science*, 13(1), pp. 46–61. doi:10.1111/j.1467-7687.2009.00852.x.

Tough, Paul (2018). *Helping Children Succeed*. Mariner Books.

Turner, Heather A., David Finkelhor, and Megan Henly (2021). "Exposure to family and friend homicide in a nationally representative sample of youth." *Journal of Interpersonal Violence*, 36(7-8), pp. NP4413–NP4442. doi:10.1177/0886260518787200. PMID: 29998751.

Wagner, Jessica (2021). "The effects of student exposure to proactive policing: evidence from Los Angeles gang injunctions." *Mimeo, University of Toronto.*

Washington, Harriet A. (2006). *Medical Apartheid: The Dark History of Medical Experimentation on Black Americans from Colonial Times to the Present.* Harlem Moon.

Weinberg, Bruce A., Patricia B. Reagan, and Jeffrey J. Yankow (2004). "Do neighborhoods affect hours worked? Evidence from longitudinal data." *Journal of Labor Economics*, 22(4), pp. 891–924. doi:10.1086/423158.

Western, Bruce and Christopher Wildeman (2009). "The Black family and mass incarceration." *The ANNALS of the American Academy of Political and Social Science*, 621(1), pp. 221–242. doi:10.1177/0002716208324850.

Williams, Benjamin (2019). "Controlling for ability using test scores." *Journal of Applied Econometrics*, 34(4), pp. 547–565. doi:10.1002/jae.2683.

Wodtke, Geoffrey T., Felix Elwert, and David J. Harding (2016). "Neighborhood effect heterogeneity by family income and developmental period." *American Journal of Sociology*, 121(4), pp. 1168–1222. doi:10.1086/684137.

Zhao, Qingyuan and Daniel Percival (2016). "Entropy balancing is doubly robust." *Journal of Causal Inference*, 5(1). doi:10.1515/jci-2016-0010.

Zuberi, Tukufu (2001). *Thicker Than Blood: How Racial Statistics Lie.* University of Minnesota Press.

A Homicide

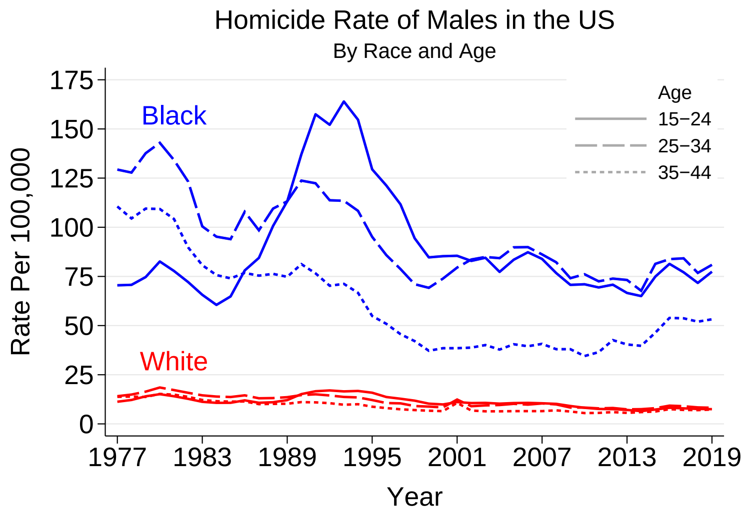


Figure 1: Homicide Rates by Race and Age
Note: This figure presents data from NCHS (2021).

B Additional Details on the Data

B.1 NLSY97

The primary sample used in our analysis is from the National Longitudinal Survey of Youth 1997 (NLSY97). We focus our analysis on non-Hispanic Black males, and in the main text we sometimes also consider a sample comprising non-Hispanic white males.

We measure long-run outcomes using results from the 2019 wave of the survey. Weekly hours worked is equal to the total annual hours worked at all civilian jobs during year Y-1 divided by 52. Earnings are equal to zero if the respondent says they did not receive income from a job, and equal to their estimate if they say they did. However, not all respondents are able to provide an estimate immediately, in which case they are subsequently prompted to select an income range. In these cases, earnings are equal to the midpoint of the range selected.

Educational attainment by age 26 is created using variables CV_HIGHEST_DEGREE_EVER for 1997 and CV_HIGHEST_DEGREE_EVER_EDT_Y for subsequent rounds. We split educational attainment into 4 groups, where a dropout is a respondent who does not have a high school diploma or GED; greater than or equal to a GED; greater than or equal to a high school diploma; and greater than or equal to a Bachelor's degree. Ever incarcerated is measured using the INCARC_STATUS_Y_M_XRND. The respondent is considered to have been incarcerated if at any point the status is positive. Marital status is measured using CV_MARSTAT_COLLAPSED_Y. The respondent is considered to have been married if at any point they report being married, separated, divorced, or widowed. We follow Aliprantis and Chen (2016) and define deceased (or missing) using the variable RNI_2019 (codes 80, 98, or 90). We follow Aliprantis (2017b) and define violent behavior at a given age as having carried a gun in the past year, attacked someone with the intent of seriously harming them, been charged with an assault, or belonged to a gang.

B.1.1 Earnings and Attrition

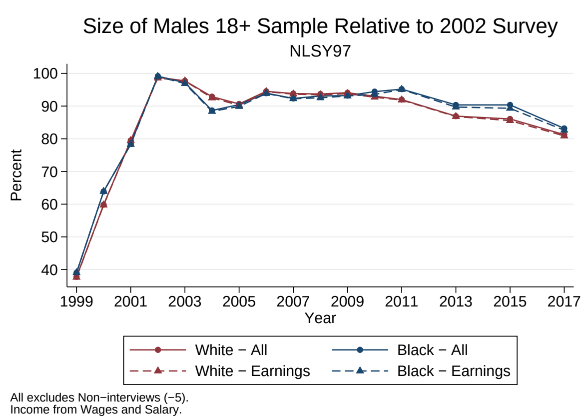
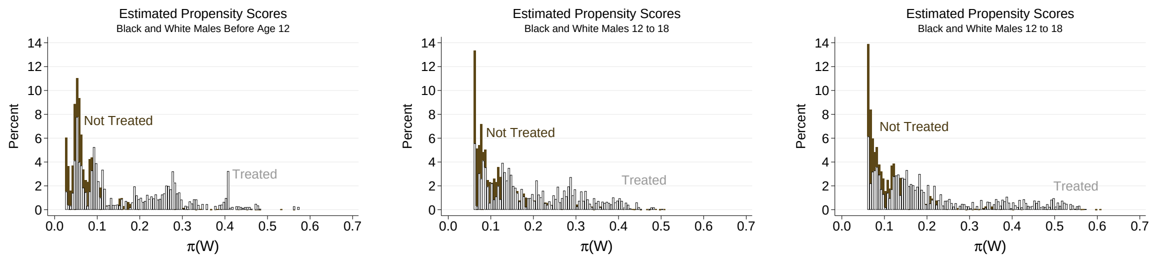


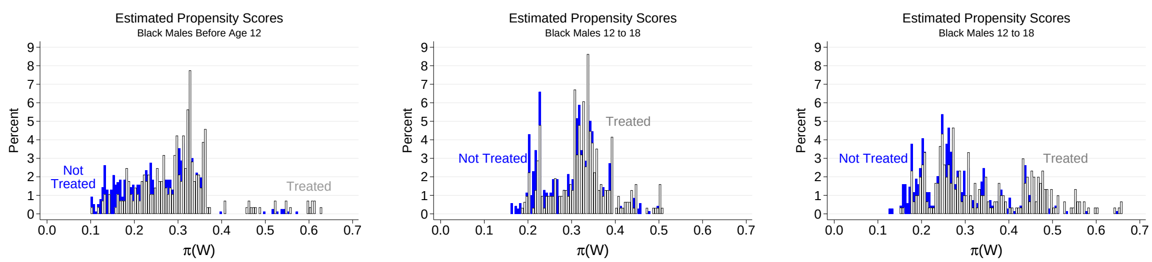
Figure 2: Attrition rate for Males 18+ relative to 2002 sample.
Note: We see that the attrition rate is similar for all races, and never falls below 80 percent. Years are one year before the survey year because we are analyzing earnings.

B.1.2 Exposure to Violence: Propensity Scores and Raw Probabilities



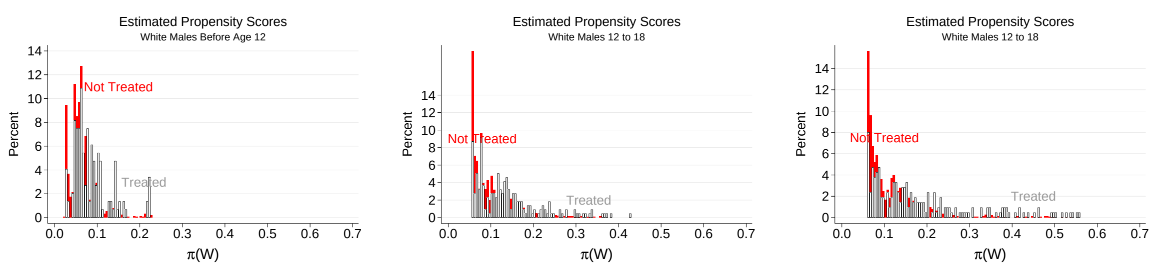
(a) **W** (b) **W** (c) **W** and Childhood Exposure

Figure 3: Propensity Scores of Black and White Males' Exposure to Violence by Covariates



(a) **W** (b) **W** (c) **W** and Childhood Exposure

Figure 4: Propensity Scores of Black Males' Exposure to Violence by Covariates



(a) **W** (b) **W** (c) **W** and Childhood Exposure

Figure 5: Propensity Scores of White Males' Exposure to Violence by Covariates

Table 1: Exposure to Violence
by Age, Race, and Household Structure

Household Structure	Black by Age 0-11	Black by Age 12-18	Black by Age 0-18	White by Age 0-11	White by Age 12-18	White by Age 0-18
Two Parent (Both Bio)	15	22	34	5	8	12
Two Parent (One Bio)	28	38	50	11	16	23
Single Parent	30	34	52	8	15	20
Grandparent	27	26	44	5	11	17
Other	45	33	63	5	16	20

Note: The categories here are formed from the NLSY97 categories as follows: The "Two Parent (Both Bio)" category includes "Both biological parents." The "Two Parent (One Bio)" category includes "Two parents, biological mother" and "Two parents, biological father." The "Single Parent" category includes "Biological mother only" and "Biological father only." The "Grandparent" category includes "No parents, grandparents." And the "Other" household structure includes "adoptive parent(s)," "foster parent(s)," "no parents, other relatives," and "anything else."

B.1.3 Calculating Neighborhood SES in 1997

Calculating neighborhood SES in 1997 is not straightforward, as this year is not the subject of a decennial Census or ACS. Therefore we calculate neighborhood SES for each tract in 1997 using Census data from a range of years downloaded from the National Historical Geographic Information system (NHGIS, Manson et al. (2017)). We first calculate neighborhood SES using the 2000 US Census and each 5-year American Community Surveys (ACS) from the years 2005-2009 until 2015-2019, interpolating data from 2010 to 2000 tract boundaries using the Longitudinal Tract Data Base (LTDB) when necessary.[22] We then estimate tract-level regressions of neighborhood SES on year and use the estimated regression to predict neighborhood SES in 1997.

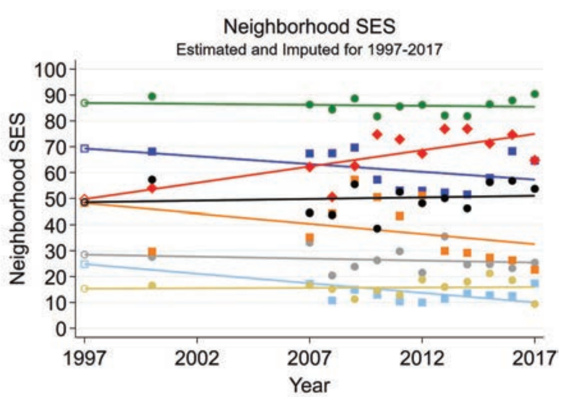
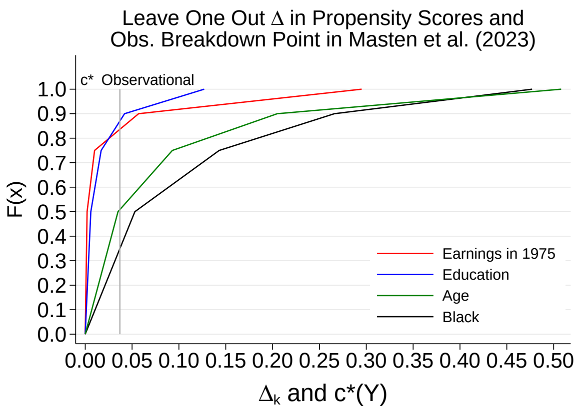


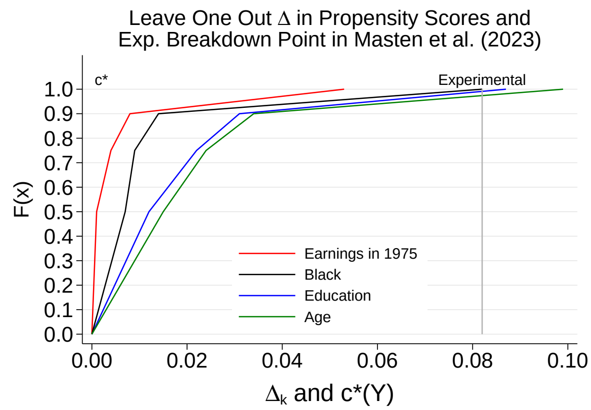
Figure 6: Calculating Neighborhood SES in 1997
Note: This figure shows estimated and imputed neighborhood SES by year for eight randomly chosen Census tracts. The solid markers denote estimates from the 2000 Census and the middle year of each 5-year American Community Survey. The hollow markers denote values imputed via tract-level regressions.

[22]See important details about the LTDB in Logan et al. (2014), Logan et al. (2016), and Logan et al. (2021).

C Robustness to c-Dependence in Masten et al. (2023)



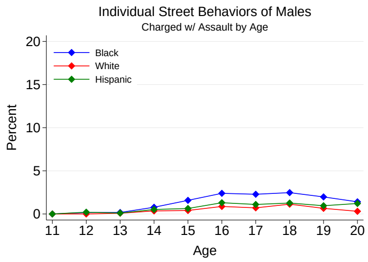
(a) Short-Run Outcomes



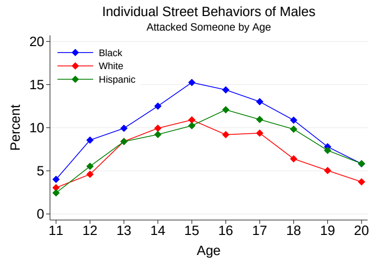
(b) Long-Run Outcomes

Figure 7: Breakdown Points and Changes in Propensity Scores from Masten et al. (2023)
Note: In both panels the Cumulative Distribution Functions (CDFs) of the distribution of leave-one-out propensity scores is constructed as a piecewise linear function with knots at the percentiles reported in Masten et al. (2023). In the left panel, the knots are taken from Table 5 and in the right panel the knots are taken from Table 3.

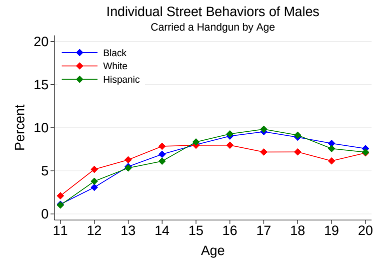
D Street Behavior of Males by Age and Race/Ethnicity



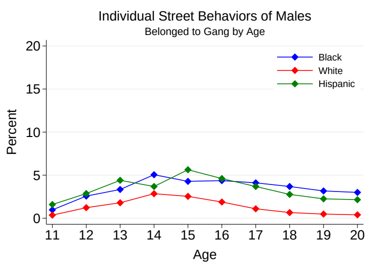
(a) Charged with Assault



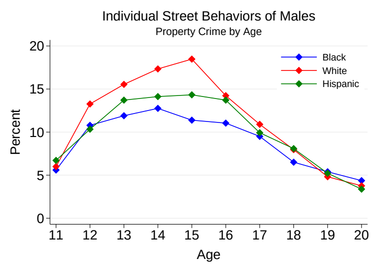
(b) Attacked Someone



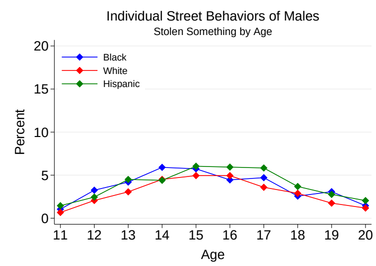
(c) Carried a Handgun



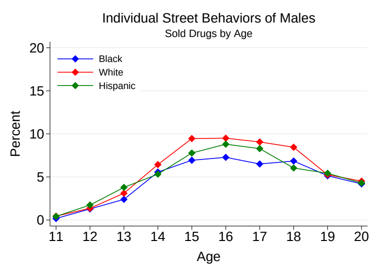
(d) Belonged to a Gang



(e) Committed a Property Crime



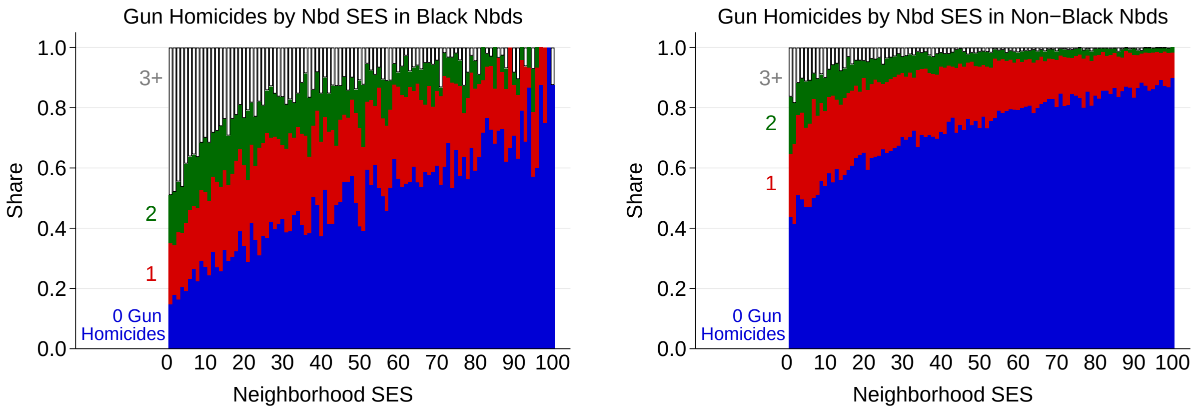
(f) Stolen Something Valuable



(g) Sold Drugs

Figure 8: Street Behaviors

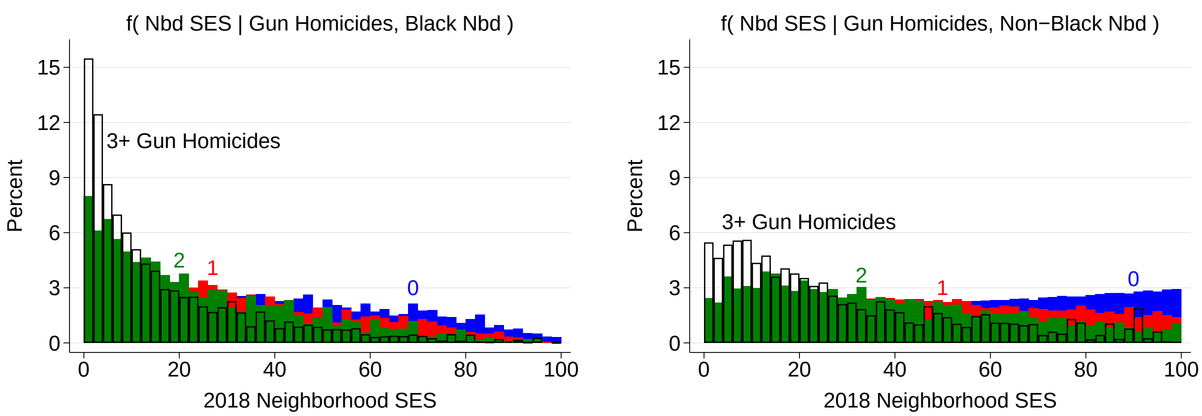
E Neighborhood Violence and Neighborhood SES



(a) Tracts ≥20% Black

(b) Tracts <20% Black

Figure 9: Gun Homicides by Neighborhood SES and Racial Composition
Note: These figures plot the tract-level distribution of number of gun homicides by percentile of neighborhood SES. The left panel displays tracts that are at least 20 percent Black and the right panel displays other tracts. The text describes the data on gun homicides from the Gun Violence Archive (GVA) and the data on tract-level characteristics from the American Community Survey (ACS).



(a) Tracts ≥20% Black

(b) Tracts <20% Black

Figure 10: Neighborhood SES by Number of Gun Homicides and Racial Composition
Note: These figures plot the distribution of neighborhood SES by the number of gun homicides. The left panel displays tracts that are at least 20 percent Black and the right panel displays other tracts. The text describes the data on gun homicides from the Gun Violence Archive (GVA) and the data on tract-level characteristics from the American Community Survey (ACS).

F Incarceration Mediating Childhood Exposure for Earnings

Table 2: Individual Earnings in 2018

Variable	Independent		
Childhood	–11.7		–10.0
Exposure	[0.00]		[0.00]
Ever		–25.5	–25.1
Incarcerated		[0.00]	[0.00]
R^2	0.01	0.06	0.07

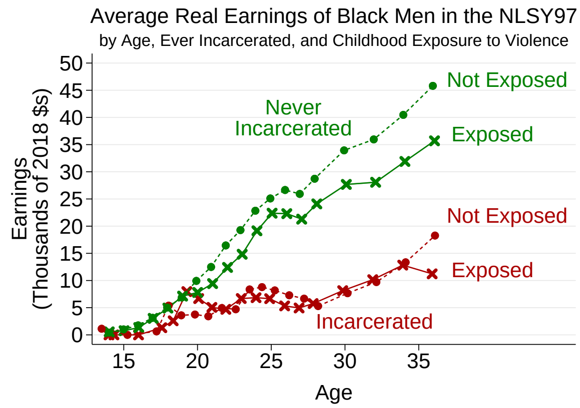


Figure 11: Individual Earnings

Table 3: Earning $0 in 2018

Variable	Independent		
Childhood	11.5		9.8
Exposure	(3.2)		(3.1)
Ever		25.2	24.8
Incarcerated		(3.1)	(3.1)
R^2	0.01	0.07	0.08

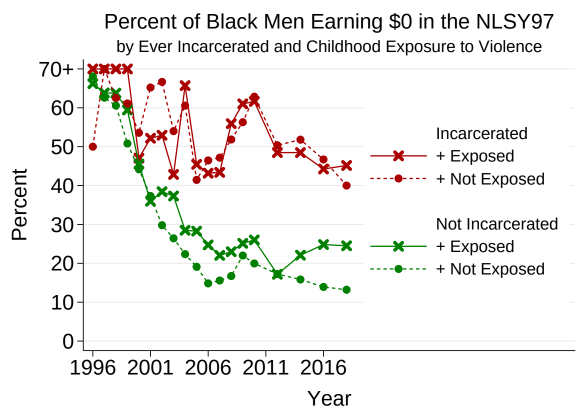


Figure 12: Earning $0

G Non-Violent Adversity

G.1 Measuring Non-Violent Adversity

We measure exposure to non-violent adversity using an indicator for whether a respondent experienced an incarcerated parent, homelessness, an unemployed parent, or the death of a parent or sibling. These variables are generally measured for the first five years of the survey, but not previously, meaning that depending on their age at the time of the first survey, we are able to measure respondents' experiences between the ages of 12 and 18 for the ages 13-18, 14-18, . . . , and 17-18. Table 4 shows that there is very little overlap in exposure to each variable capturing a form of non-violent adversity. This lack of overlap suggests that an IRT or PC model would not do a good job of summarizing the variation in these variables; these responses do not look as though they are the noisy responses determined by the same latent index. For this reason we define non-violent adversity as an indicator for having responded affirmatively to one of these experiences.

Table 4: Black Adolescents' Non-Violent Adversity, Ages 12-18

Specific Adversity	Percent	Cumul.
Incarcerated Parent	1.2	1.2
Homeless	1.6	2.8
Unemployed Parent	6.4	9.0
Death of parent or sibling	15.0	23.6
Any Non-Violent Adversity	23.6	23.6

Note: See text for variable descriptions.

The variables chosen for inclusion on our measure of non-violent adversity are distinct from those used to define Adverse Childhood Experiences (ACEs). These choices are partly driven by the NLSY97 data set. The NLSY97 includes few of the measures used in the original ACEs study (Felitti et al. (1998)). This may be partly due to the fact that unlike the retrospective design of the original ACEs study, the NLSY97 interviewed children while they were still living with their parents. Asking children about their parents' abuse (physical, sexual, or emotional); neglect (physical or emotional); or household dysfunction (intimate partner violence, mental illness, or substance abuse) would have likely resulted in uncooperative survey respondents.

The variables chosen for inclusion on our measure of non-violent adversity are also driven by the sample of Black males we are studying. While it is unsurprising, Table 5 shows that all of the variables included in the non-violent adversity indicator are highly predictive of long-run outcomes for both Black and white males. A more surprising set of results, related to the exclusion of divorce and family hospitalizations, is shown in Table 6. The table shows that for long-run outcomes of Black men, differences between those exposed to each measure of non-violent adversity and those not exposed are small and statistically insignificant. In contrast, for white men, differences are almost always large and statistically significant, in line with the results from Johnson and Reynolds (2013). These results add an important point to the discussion of what experiences should be included in a measure of adversity (Finkelhor et al. (2015)): The best measure is likely to be group specific.

Table 5: Adult Outcomes and Included Measures of Non-Violent Adversity

	Parent Incarcerated		Homeless		Parent Unemp.		Parent/Sib. Death	
Variable	Black	White	Black	White	Black	White	Black	White
HS	-47.1	-20.6	-13.5	-45.4	-8.8	-2.0	-10.1	-9.7
	[0.00]	[0.03]	[0.27]	[0.00	[0.16]	[0.64]	[0.02]	[0.00]
BA	-9.8	-15.7	-10.0	-27.3	-3.6	-8.8	-5.5	-10.7
	[0.25]	[0.13]	[0.18]	[0.00]	[0.35]	[0.07]	[0.04]	[0.00]
incar	32.3	15.2	36.9	6.3	8.4	-2.7	3.3	12.0
	[0.01]	[0.05]	[0.00]	[0.32]	[0.14]	[0.46]	[0.39]	[0.00]
earnings	-16.5	-34.5	-18.4	-22.5	-10.7	-11.1	-12.8	-13.8
	[0.32]	[0.03]	[0.21]	[0.12]	[0.11]	[0.17]	[0.00]	[0.02]

Note: This table reports coefficients from regressions of the outcome variables on an indicator for each measure of non-violent adversity. The p-values of tests for each coefficient being different from 0 are reported in braces [ ].

Table 6: Adult Outcomes and Omitted Measures of Non-Violent Adversity

	Divorce		Parent or Sibling in Hospital	
Outcome	Black	White	Black	White
HS by 26	3.8	–12.6	–5.9	–5.1
	[0.38]	[0.00]	[0.19]	[0.05]
BA by 26	1.2	–15.2	–1.9	–8.1
	[0.66]	[0.00]	[0.50]	[0.01]
Incar. by 2019	1.7	3.5	2.2	0.8
	[0.68]	[0.07]	[0.60]	[0.72]
Earnings in 2018	–2.1	–0.4	–1.3	–7.8
(1,000s of 2018 $s)	[0.64]	[0.93]	[0.79]	[0.10]

Note: This table reports coefficients from regressions of the outcome variables on an indicator for each measure of non-violent adversity. The p-values of tests for each coefficient being different from 0 are reported in braces [ ].

G.2 Potential Outcomes and Causal Effects

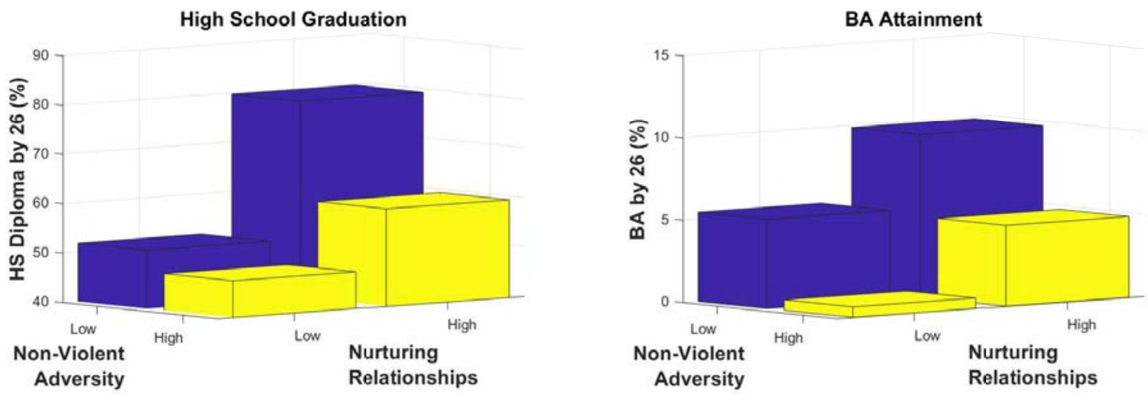


Figure 13: Potential Outcomes for Educational Attainment

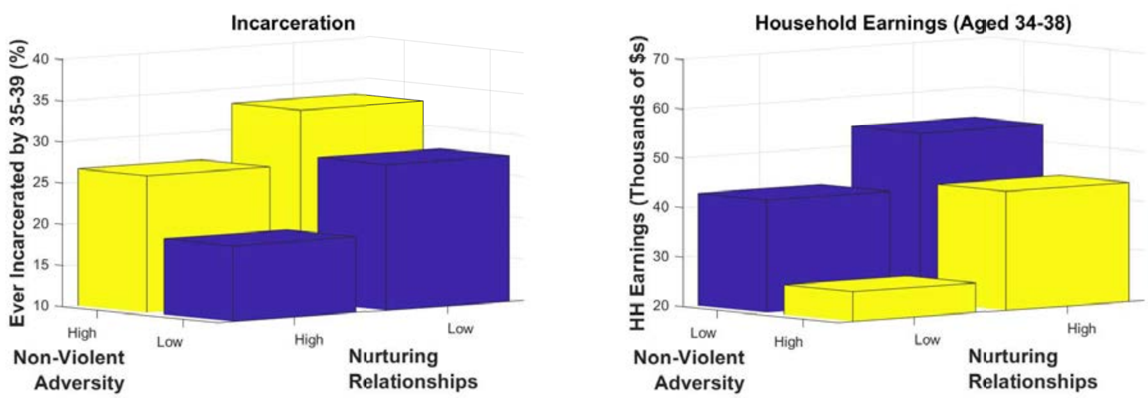


Figure 14: Potential Outcomes for Incarceration and Household Earnings

Table 7: Effects of Changing Treatments in Non-Violent and Nurturing Relation. Model

	Given High Non-Violent Adversity and Low Nurturing Relationships		
	$\downarrow D^{NV}$	$\uparrow D^{NR}$	Both
HS	4.4	12.3	32.3
	[0.06]	[0.00]	[0.00]
BA	4.8	4.3	9.2
	[0.03]	[0.20]	[0.00]
HH Earnings	16.7	18.1	28.0
	[0.03]	[0.11]	[0.00]
Incarceration	–5.6	–6.6	–14.0
	[0.07]	[0.11]	[0.00]

Note: The p-values of one-sided tests for each coefficient being different from 0 are reported in braces [ ] and are obtained from 1,000 bootstrap replications.

H Details on the Item Response Theory Estimation

H.1 The Likelihood Function

We estimate our Item Response Theory (IRT) model using the `irt` command in Stata. We study the robustness of distributional assumptions on the latent index by estimating the model in MATLAB. We infer the precise estimation routine in Stata using a combination of Stata (2021), de Ayala (2022), Raykov and Marcoulides (2018), and Harwell et al. (1988).

We observe $j \in \{1, 2, \ldots, J\}$ noisy measures V_i^j of a latent index θ_i^V representing each respondent's exposure to violence.[23] Our Item Response Theory (IRT) model assumes that each binary measure V_i^j for respondent i and measure $j = 1, \ldots, J$ is a function of the latent index as

$$V_i^j = \begin{cases} 1 & \text{if } \alpha_j(\theta_i^{PCE} - \beta_j) - \epsilon_i^j > 0 \\ 0 & \text{if } \alpha_j(\theta_i^{PCE} - \beta_j) - \epsilon_i^j \leq 0. \end{cases}$$

Assuming ϵ_i follows a type-1 extreme value distribution, then

$$Pr(V_i^j = 1|\alpha, \beta, \theta_i) = \text{logit}[\alpha_j(\theta_i^{PCE} - \beta_j)]$$

where the α_j term is often referred to as the discrimination of item j, the β_j term is typically referred to as the difficulty of item j, and the combined parameters α_j, β_j are often called the item parameters. If we further assume that the latent index follows a standard normal distribution, or $\theta_i \sim \Phi$, we can write each individual's contribution to the Likelihood as

$$\mathcal{L}_i(\alpha, \beta) = \int_{-\infty}^{\infty} Pr(V_i|\alpha, \beta, \theta_i) d\Phi(\theta_i) \tag{2}$$

where

$$Pr(V_i|\alpha, \beta, \theta_i) = \prod_{j=1}^{J} Pr(V_i^j = 1|\alpha, \beta, \theta_i)^{V_i^j} (1 - Pr(V_i^j = 1|\alpha, \beta, \theta_i))^{1-V_i^j},$$

Because θ_i is not observed, to calculate each individual's likelihood in Equation 3 one must use numerical integration, or numerical quadrature, as

$$\mathcal{L}_i(\alpha, \beta) = \sum_{q=1}^{Q} Pr(V_i|\alpha, \beta, \theta_q) \widehat{\varphi}(\theta_q) \tag{3}$$

where there are Q quadrature points and $\widehat{\varphi}(\theta_q)$ is the numerical probability mass function (pmf) approximating the standard normal distribution. This marginal likelihood is estimated in each iteration before parameters are found, which respectively represent the Expectation and Maximization steps of the EM algorithm. The resulting estimates of the item parameters $\widehat{\alpha}$ and $\widehat{\beta}$ are often referred to as the marginal maximum likelihood (MML) estimates (Raykov and Marcoulides

[23] The same procedure is used for nurturing relationships.

(2018)).

Once the MML item parameters are estimated, one can compute Empirical Bayes estimates of each individual's latent index as

$$\overline{\theta}_i = \int \frac{\theta Pr(V_i|\widehat{\alpha}, \widehat{\beta}, \theta)\varphi(\theta)}{Pr(V_i|\widehat{\alpha}, \widehat{\beta}, \theta)\varphi(\theta)} d\theta.$$

This binary IRT model can be generalized to ordered responses along the lines by which a logit model is extended to an ordered logit model. A given measure

$$V_i^j = \begin{cases} 1 & \text{if } \alpha_j \theta_i^V - \epsilon_i < C_1^j \\ 2 & \text{if } C_1^j \leq \alpha_j \theta_i^V - \epsilon_i < C_2^j \\ \vdots & \vdots \\ K & \text{if } \alpha_j \theta_i^V - \epsilon_i > C_{K-1}^j, \end{cases}$$

will have likelihood

$$\mathcal{L}_i(\alpha, \beta, C) = \int_{-\infty}^{\infty} Pr(V_i|\alpha, \beta, C, \theta_i) d\Phi(\theta_i)$$

where

$$Pr(V_i|\alpha, \beta, C, \theta_i) = \prod_{j=1}^{J} \left[\sum_{k=1}^{K} \mathbf{1}\{V_i^j = k\} Pr(V_i^j = k|\alpha, \beta, C, \theta_i) \right].$$

Thus the log-Likelihood is

$$\mathcal{LL}(\alpha, \beta, C) = ln[\prod_{i=1}^{N} \mathcal{L}_i(\alpha, \beta, C)] = \sum_{i=1}^{N} ln \left[\mathcal{L}_i(\alpha, \beta, C) \right].$$

H.2 Robustness to Distributional Assumptions

A key distributional assumption in the estimation of the IRT model is that the latent indexes follow standard normal distributions. Here we show that this distributional assumption has no implications for our discrete treatment; individuals will receive the same treatment label based on their percentile in the distribution of the estimated latent index.

We estimate the IRT model described above for exposure to violence under two distributional assumptions:

$$\theta^V \sim \begin{cases} \mathcal{N}(0, 1); \text{ and} \\ \text{U}[-5, 5]. \end{cases}$$

Figure 15 shows a scatter plot of the resulting estimates of the latent indexes, together with the terciles of the empirical distributions. What is evident from this plot is that the estimated θ_i^V is in the same tercile of the distribution nearly all of the time when it is estimated under the assumption of a normal or uniform distribution. This fact is quantified in Table 8. In our sample 97 percent of individuals are labeled with the same three-leveled treatment regardless of whether we assume

θ_i^V follows a normal or a uniform distribution. In our sample 1.4 percent of individuals are ranked higher under the assumption that θ_i^V follows a normal distribution and 1.5 percent of individuals are ranked higher under the assumption that θ_i^V follows a uniform distribution.

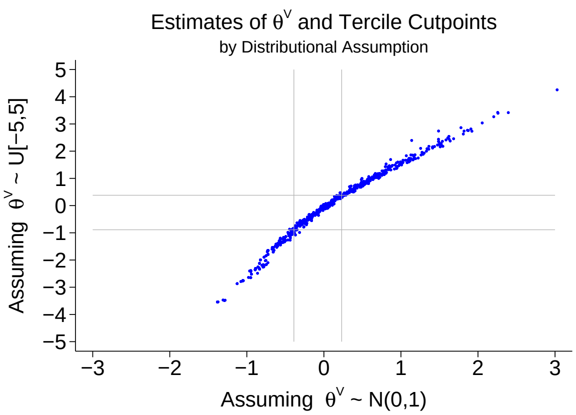


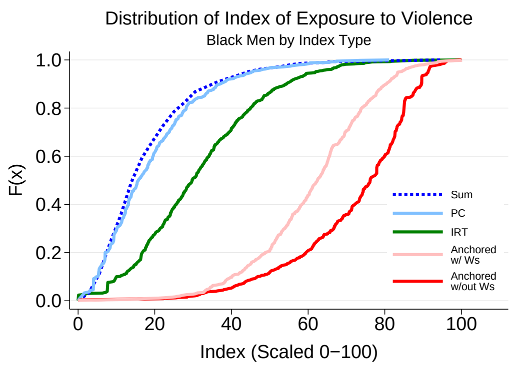
Figure 15: Estimated θ_i^V's by Dist. Assumption
Note: This figure shows the distribution of Empirical Bayes estimates of θ_i^V in the Item Response Theory model under the assumptions that $\theta_i^V \sim \mathcal{N}(0,1)$ and $\theta_i^V \sim \text{U}(-5,5)$. The grey vertical and horizontal lines display the terciles of each distribution, which can be used to define 3-level treatment variables analogous to the binary treatment variable used in the analysis in the main text.

Table 8: Difference in 3-Level Treatments by Distributional Assumption

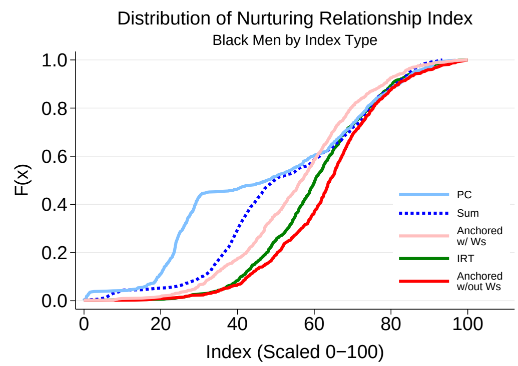
	Difference in 3-Level Treatments $D_U^V - D_N^V$		
	–1	0	1
Frequency	10	700	11
Percent	1.4	97.1	1.5

Note: This table reports the difference in a 3-level treatment estimated under the assumption that $\theta_i^V \sim \text{U}[-5,5]$, denoted by D_U^V, and a 3-level treatment estimated under the assumption that $\theta_i^V \sim \mathcal{N}(0,1)$, denoted by D_N^V.

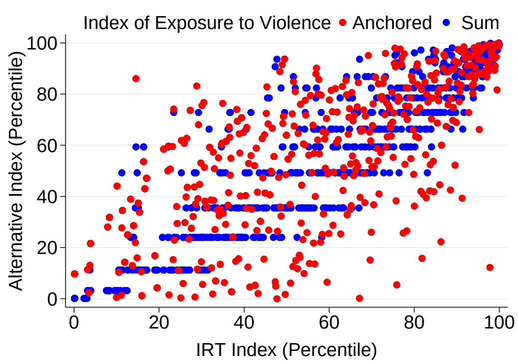
I Comparison of Approaches to Calculating Indexes: Item Response Theory, Additive, and Principal Components



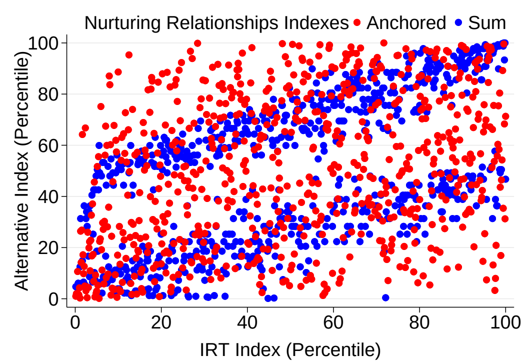
(a) Exposure to Violence



(b) Nurturing Relationships



(c) Exposure to Violence



(d) Nurturing Relationships

Figure 16: Indexes by Estimation Method

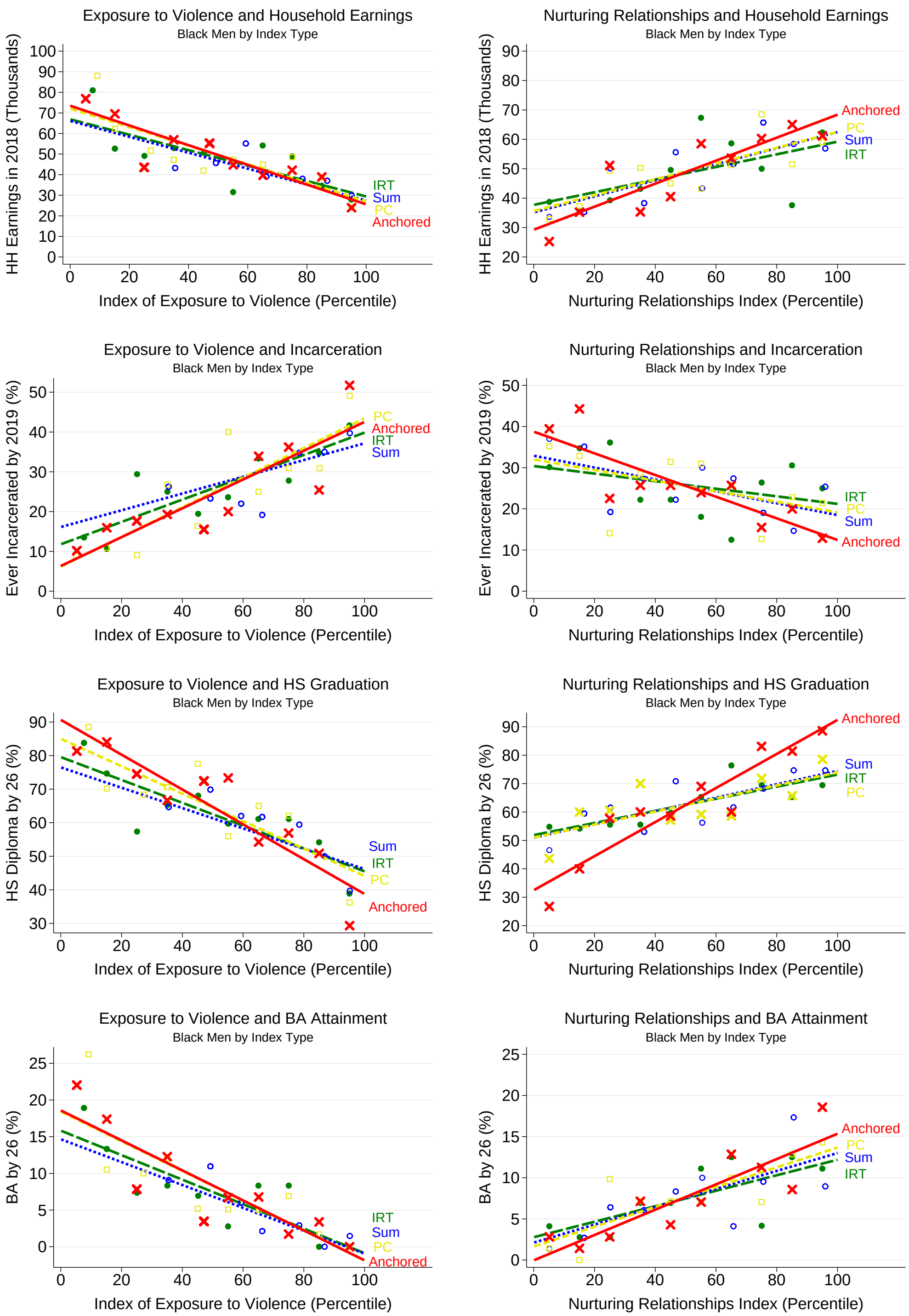


Figure 17: Binned Scatterplots of Black Men's Outcomes and Indexes

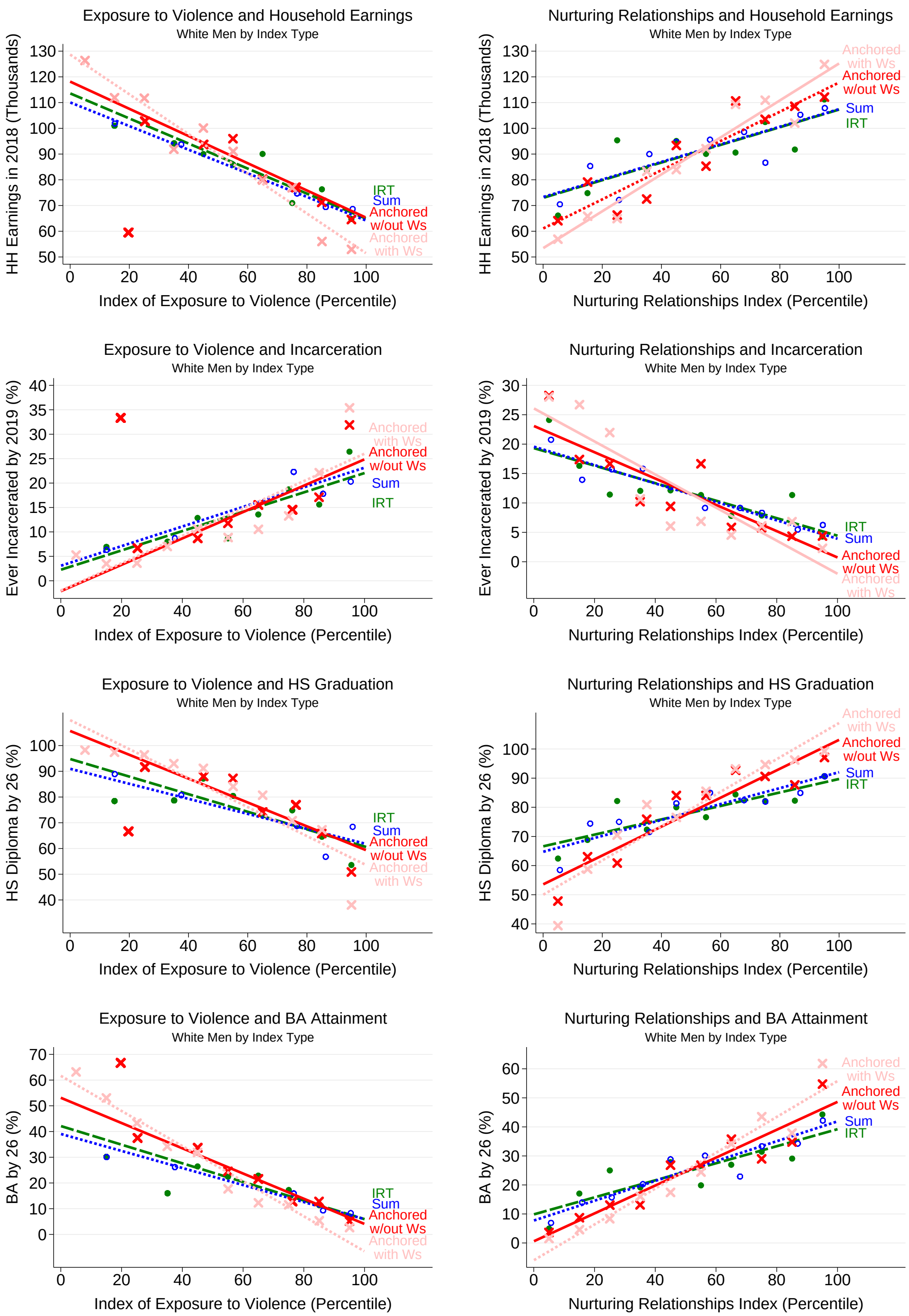


Figure 18: Binned Scatterplots of White Men's Outcomes and Indexes

Figures 19 and 20 show the weights assigned to each item via an estimated linear probability model for attaining a HS diploma by age 26. A few notable features are (1) the range of weights, with some items receiving large weights, both positive and negative, and others receiving weights near 0. (2) the respondent living with their father is highly positive while living with their mother is highly negative. The negative coefficient for the mother is not surprising, as this is net of all of the other questions about the mother. (3) not shown, but consistent with the results in Nielsen (2022), is that anchoring to different outcomes generates different weights.

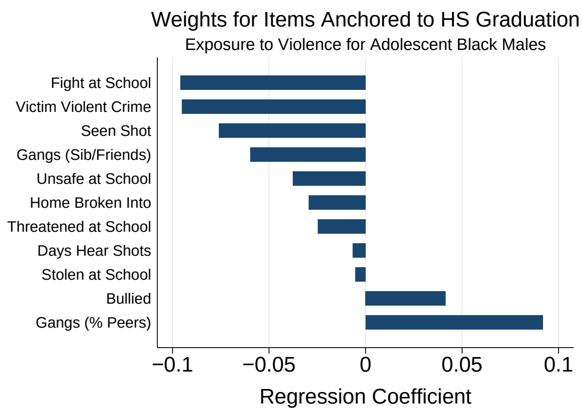
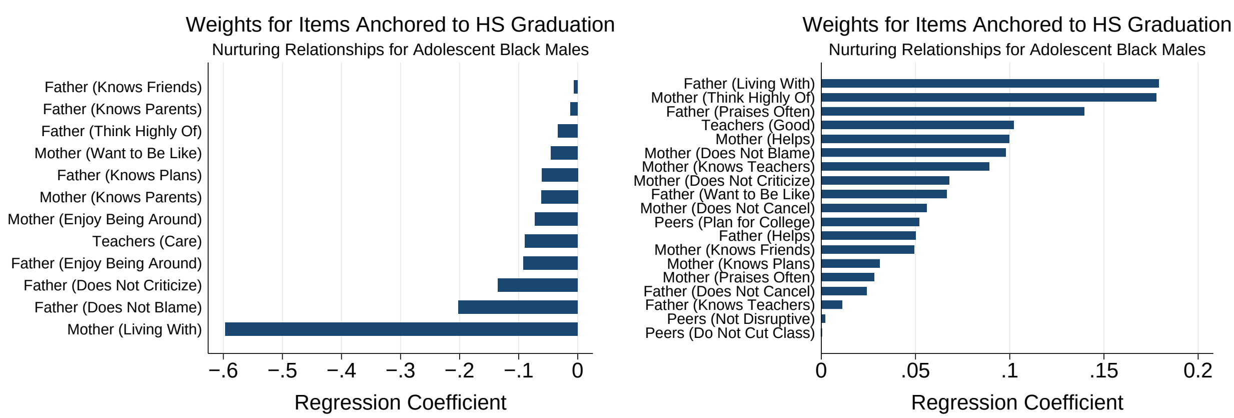


Figure 19: Item-Anchored Weights



(a) HS Graduation by Nurturing Relationships Indexes (b) HS Graduation by Nurturing Relationships Indexes

Figure 20: Item-Anchored Weights

Appendix References

de Ayala, Rafael Jaime (2022). *The Theory and Practice of Item Response Theory*. Guilford Press, second edition.

Felitti, Vincent J., Robert F. Anda, Dale Nordenberg, David F. Williamson, Alison M. Spitz, Valerie Edwards, and James S. Marks (1998). "Relationship of childhood abuse and household dysfunction to many of the leading causes of death in adults: The Adverse Childhood Experiences (ACE) Study." *American Journal of Preventive Medicine*, 14(4), pp. 245–258. doi:10.1016/S0749-3797(98)00017-8.

Finkelhor, David, Anne Shattuck, Heather Turner, and Sherry Hamby (2015). "A revised inventory of Adverse Childhood Experiences." *Child Abuse & Neglect*, 48, pp. 13–21. doi:10.1016/j.chiabu.2015.07.011.

Harwell, Michael R., Frank B. Baker, and Michael Zwarts (1988). "Item parameter estimation via Marginal Maximum Likelihood and an EM Algorithm: A didactic." *Journal of Educational Statistics*, 13(3), pp. 243–271. doi:10.3102/10769986013003243.

Johnson, Eric and C. Lockwood Reynolds (2013). "The effect of household hospitalizations on the educational attainment of youth." *Economics of Education Review*, 37, pp. 165–182. doi:10.1016/j.econedurev.2013.09.002.

Logan, John R., Brian Stults, and Zengwang Xu (2016). "Validating population estimates for harmonized Census tract data, 2000-2010." *Annals of the American Association of Geographers*, 106(5), pp. 1013–1029. doi:10.1080/24694452.2016.1187060.

Logan, John R., Zengwang Xu, and Brian Stults (2014). "Interpolating US decennial Census tract data from as early as 1970 to 2010: a longitudinal tract database." *Professional Geographer*, 66(3), pp. 412–420. doi:10.1080/00330124.2014.905156.

Logan, John R., Charles Zhang, Brian Stults, and Todd Gardner (2021). "Improving estimates of neighborhood change with constant tract boundaries." *Applied Geography*, (132). doi:10.1016/j.apgeog.2021.102476.

Manson, Steven, Jonathan Schroeder, David Van Riper, and Steven Ruggles (2017). *IPUMS National Historical Geographic Information System*. University of Minnesota, Minneapolis, 12.0 edition. doi:10.18128/D050.V12.0.

Masten, Matthew A., Alexandre Poirier, and Linqi Zhang (Forthcoming). "Assessing sensitivity to unconfoundedness: estimation and inference." *Journal of Business & Economic Statistics*. doi:10.1080/07350015.2023.2183212.

NCHS (2021). *Health, United States*. National Center for Health Statistics, Washington, DC. URL https://www.cdc.gov/nchs/hus/data-finder.htm.

Nielsen, Eric Reed (2022). “Test questions, economic outcomes, and inequality.” *Mimeo, Federal Reserve Board of Governors*.

Raykov, Tenko and George A. Marcoulides (2018). *A Course in Item Response Theory and Modeling with Stata*. Stata Press.

Stata (2021). *Item Response Theory Reference Manual*. StataCorp LLC, College Station, Texas, release 17 edition.