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# The lasting consequences of childhood sexual abuse on human capital and economic well-being

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**Abstract**

Childhood sexual abuse is a prevalent problem, yet understanding of later-in-life outcomes is limited due to unobservable determinants. I examine impacts on human capital and economic well-being by estimating likely ranges around causal effects, using a nationally representative U.S. sample. Findings suggest that childhood sexual abuse leads to lower educational attainment and worse labor market outcomes. Results are robust to partial identification methods applying varying assumptions about unobservable confounding, using information on confounding from observables including other types of child abuse. I show that associations between childhood sexual abuse and education outcomes and earnings are at least as large for males as for females. Childhood sexual abuse by someone other than a caregiver is as influential or more so than caregiver sexual abuse in predicting worse outcomes. Considering the societal burden of childhood sexual abuse, findings could inform policy and resource allocation decisions for development and implementation of best practices for prevention and support.

**KEYWORDS**
adverse childhood experiences, child health, child maltreatment, human capital

**JEL CLASSIFICATION**
I14, I24, I30, J24

Childhood sexual abuse is a public health crisis (Maholmes, 2017; Office of the Surgeon General, 2005) and preventable problem (Assini-Meytin et al., 2020; Fix et al., 2021; Letourneau et al., 2017; Mendelson & Letourneau, 2015). In the United States, 16.1% of women and 6.2% of men reported a history of childhood sexual abuse in the 2009–17 surveys of the Behavioral Risk Factor Surveillance System (Giano et al., 2020).[1] The consequences of childhood sexual abuse on immediate trauma are well understood, and associations with worse mental health status persist in adulthood (Fletcher, 2009; Hoertel et al., 2015; Natalie Sachs-Ericsson et al., 2010; Palo & Gilbert, 2015). We know much less about the durable impacts of childhood sexual abuse beyond mental health, which may extend into adulthood. A main barrier to research has been difficulty disentangling childhood environmental factors and other experiences. Yet, measuring long-term impacts of childhood sexual abuse is crucial to understand survivor welfare across domains of well-being, to identify areas for needed support, and to inform policy and resource allocation for prevention, detection, and support.

In this paper, I investigate whether there are durable effects of childhood sexual abuse on adult human capital and economic well-being and estimate likely bounds around the causal effects. I use a U.S. representative sample from the National Longitudinal

Study of Adolescent to Adult Health, which includes self-interview questions on sexual abuse, and take several approaches to address and evaluate confounding. By using large, population-level data, I can study the heterogeneity of response—including among men, who are hugely understudied in the literature on sexual trauma. I examine heterogeneity by sociodemographic characteristics and perpetrator type descriptively without constructing bounds on causal effects, as the degree of confounding from unobservable characteristics may vary across sub-groups.

Childhood sexual abuse is distinct from other types of adverse childhood experiences (ACEs), requiring distinct prevention efforts (Assini-Meytin et al., 2020; Letourneau et al., 2017; Mendelson & Letourneau, 2015) and understanding of the scope of its distinct harms to provide appropriate support. Due to fear, shame, and self-blame, individuals who experience childhood sexual abuse commonly do not disclose to anyone around the time of occurrence (Easton, 2013; Hunter, 2015; O'Leary & Barber, 2008; Xiao & Smith-Prince, 2015), thus are less likely to receive support. Lack of support can foster persistence of mental health symptoms (Fletcher, 2009; Hoertel et al., 2015; Palo & Gilbert, 2015; Sachs-Ericsson et al., 2010), which could result in accumulated impacts on human capital.

There are several possible mechanisms through which childhood sexual abuse might impact adult educational attainment and labor market outcomes, such as direct effects on human capital through reduced mental and physical health (Bucci et al., 2016; Currie, 2020; Goodman et al., 2011). Indeed, research on human capital in adolescence shows worse schooling outcomes (lower grade point averages and higher rates of reporting difficulty paying attention) as a result of forced intercourse among a female sample (Rees & Sabia, 2013). Childhood sexual abuse might also lead to limited labor market opportunities through increased likelihood of criminal legal consequences (Currie & Tekin, 2012) or teen pregnancy (Anda et al., 2001). Further, a history of childhood sexual abuse may lead one to prefer jobs away from high-pressure environments, considering existing mental health burdens, and away from male-dominated fields, given that perpetrators are predominately male (Black et al., 2011), both of which would eliminate some opportunities in higher-wage categories.

The main challenge to identifying causal effects of childhood sexual abuse is confounding from unobservable determinants of sexual abuse which also explain adult outcomes, and this unobservable confounding may occur at the neighborhood, family, or child level. For example, growing up in an economically distressed neighborhood with a high level of crime may cause a child to be both at greater risk of sexual abuse and to attend lower-resourced schools, and the latter alone could explain reduced human capital and economic well-being in adulthood. Having parents who spend little time with their child may leave the child more vulnerable to abuse and suppress cognitive development. In addition, childhood disability may increase risk of sexual abuse (Van Horne et al., 2015) while also presenting challenges in school and on the labor market. Studying childhood sexual abuse is also challenging because there is little opportunity for quasi-experimental design with individual-level data on childhood sexual abuse.

To address potential bias, I control for household socioeconomic status (SES) in childhood, school fixed effects, and other ACEs. While baseline estimates may suffer from bias, I use information from confounding from observables and explicit assumptions about confounding from unobservables to calculate sets of likely ranges where the causal estimates may lie. I adopt partial identification methods developed by Altonji et al. (2002, 2005) and Oster (2019) and show that my approach parses out bias from unobservables correlated with other types of ACEs including physical and emotional abuse.

I find consistent evidence suggesting that childhood sexual abuse leads to lower educational attainment and worse labor market outcomes in adulthood: 36%–41% greater likelihood of high school dropout, 17%–24% lower likelihood of college degree attainment, 6%–8% lower likelihood of full-time employment, and 13%–19% lower earnings. For individuals from lower income households, associations between childhood sexual abuse and reduced educational attainment are attenuated. I show that associations between childhood sexual abuse and education outcomes and earnings are at least as large for males as for females. I find that childhood sexual abuse by someone other than a caregiver is as influential or more so than caregiver sexual abuse in predicting worse outcomes.

The remainder of the paper proceeds as follows. Section 1 provides an overview of the literature. Section 2 describes the data, and Section 3 details the empirical strategy. Section 4 present results, while Section 5 concludes and includes discussion of policy implications.

## 1 | LITERATURE REVIEW

Over the past two decades, there has been increasing attention from researchers, clinicians, public health practitioners, educators, and policymakers on "adverse childhood experiences," or "ACEs." In the 1990s, the ACEs Study, conducted jointly by the U.S. Centers for Disease Control and Prevention and Kaiser Permanente, showed a graded relationship between the number of ACE types reported and risk of mental and physical health outcomes—among beneficiaries of a California Kaiser insurance plan. The 10 ACEs assessed were sexual, physical, and emotional abuse; physical and emotional neglect; having a household member who had been incarcerated, used illicit drugs, or was mentally ill; exposure to domestic violence against the mother; and parental divorce or separation (Dong et al., 2003). Differences in outcomes were significant between participants who reported at least four ACEs compared to those who reported none (Felitti et al., 1998). While much of the subsequent literature on ACEs follows the precedent of studying ACEs as a count exposure, factor analysis reveals three distinct groups: sexual abuse, physical or emotional abuse, and household adversity (Ford et al., 2014).

In a growing body of literature, researchers attempt to examine relationships between ACEs and a variety of outcomes; however, this literature is largely plagued with confounding challenges. There has been no research, to my knowledge, measuring the independent effects of any type of childhood abuse on adult educational attainment or labor market outcomes in U.S. community samples. Beyond the lack of or inadequate approaches to addressing confounding, the small body of work on childhood abuse and adult economic welfare suffers from other important limitations, such as use of abuse measures with low sensitivity and study of narrow populations. Studies in this body of work either use survey data or official child welfare records to identify exposed and non-exposed groups. Child welfare records, however, capture prevalence rates that are less than one-third of the percentage of children who acknowledge experiences of maltreatment in repeated national surveys (Finkelhor et al., 2015; Yi et al., 2020). Thus, research using only reports to authorities to identify maltreatment exposure will suffer, to a large extent, from contamination of the control group (Widom & Morris, 1997) and are not representative of maltreatment experiences (Hanson et al., 1999). While official reports may in general reflect more severe occurrences, the exposure is confounded with intervention—the involvement of child protective services.

Research on childhood abuse and adult educational attainment and labor market outcomes is inconsistent. Below, I discuss studies of various types of childhood abuse in various countries that made some attempt to address confounding by childhood SES (by matching on or controlling for parental income, occupation class, or educational attainment).

Research on the link between childhood abuse and educational attainment is mixed. Currie and Widom (2010) find that women with court-substantiated records of child maltreatment (physical abuse, sexual abuse, or physical neglect) in 1967–1971 had completed half a grade less than controls from the same U.S. Midwestern county matched on age, sex, race/ethnicity, and neighborhood. Using U.S. survey data, Foster et al. (2008) measure a positive, imprecisely estimated association between a combined measure of sexual or physical abuse before grade 6 and the probability of high school graduation in adjusted models (odds ratio: 1.17; 95% confidence interval: 0.91–1.52) and do not include neighborhood controls. Geoffroy et al. (2016), using the 1958 British Birth Cohort, find that sexual abuse is associated with lower educational attainment until controlling for cognition scores at age 16, which itself could be an outcome affected by the abuse. Boden et al. (2007), using New Zealand data from the Christchurch Health and Development Study, find that child sexual abuse was correlated with reduced likelihood of gaining a university degree while no difference in likelihoods of having secondary school qualifications, gaining a higher school certificate, or university attendance in models adjusted for child gender, intelligence quotient (IQ), family socio-economic background, and other ACEs; inclusion of IQ as a regressor may be over-controlling, however.

Next, reviewing literature on childhood abuse and labor market outcomes also yields mixed results. Among studies of childhood abuse and adult labor market outcomes, only Currie and Widom (2010) address potential confounding by childhood SES and neighborhood circumstances. The few studies on childhood abuse and adult employment status are inconsistent but show negative associations with childhood maltreatment when sexual abuse is included (Barrett et al., 2014; Currie & Widom, 2010). Currie and Widom (2010) measure an earnings gap by maltreatment history equaling 23% of mean earnings, while no statistical difference was detected among men once splitting the sample. Studies using national data from Ireland and New Zealand measure negative but imprecise estimates of associations between childhood sexual abuse and adult earnings (Barrett et al., 2014; Fergusson et al., 2013).

## 2 | DATA

I utilize restricted-use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health), which recruited a random sample of children in grades 7–12, first by sampling schools from the national database (Harris, 2013). Add Health has been previously used to study relationships between childhood maltreatment and adult physical health (Shin & Miller, 2012; Suglia et al., 2014), mental health (Dunn et al., 2013; Fletcher, 2009; Foster et al., 2008), and crime (Currie & Tekin, 2012). I construct a sample of participants who responded to the Wave I and Wave IV home interviews. Wave I surveys were collected 1994–95 when participants were 11–18 years old; I use measures of childhood SES, demographics, and ACEs included in this first wave. I measure adult educational attainment and labor market outcomes in Wave IV (2008–2009, 24–32 years old[2]), along with other ACEs surveyed only in adulthood including childhood sexual abuse. I use data from Wave II (1996, 12–19 years old) and Wave III (2001–2002, 18–26 years old) to replace missing data where available and appropriate or for sensitivity analyses. The full sample that completed both Wave I and Wave IV interviews consisted of roughly 15,000 individuals. The response rate for Wave IV was 80%. I use survey weights in all analyses to account for sampling methods and attrition. The results represent the population of U.S. adults in 2008 who were enrolled in grades 7–12 during the 1994–95 academic year (late Generation *X* and early Millenials).

## 2.1 | Key measures

In baseline models I consider the following ACEs observed in either Wave I or Wave IV: sexual abuse, physical abuse, emotional abuse, experience or threat of knife or gun violence, parental incarceration, parental divorce or separation, and availability of illegal drugs in the household. Sexual abuse reflects retrospective reports from self-interviews during the adulthood survey waves of having experienced sexual abuse by anyone before the adult respondent was age 18: (i) report of at least one time to the question "How often did a parent or other adult caregiver touch you in a sexual way, force you to touch him or her in a sexual way, or force you to have sexual relations?" (Waves III and IV), (ii) affirmative response to "Have you ever been forced, in a non-physical way, to have any type of sexual activity against your will? For example, through verbal pressure, threats of harm, or by being given alcohol or drugs? Do not include any experiences with a parent or adult caregiver" (Wave IV), or (iii) affirmative response to "Have you ever been physically forced to have any type of sexual activity against your will? Do not include any experiences with a parent or adult caregiver" (Wave IV).

Questions for each ACE were administered in the audio computer-assisted *self-interview* module except for parental incarceration and parental divorce/separation, which were administered through the computer-assisted personal interview module. Self-interview methods have been found to capture higher rates of sexual and drug-related behaviors than measured from face-to-face interviews (Midanik & Greenfield, 2008; Perlis et al., 2004). Three of the ACEs were assessed during Wave I: knife or gun violence, illegal drugs in the home, and parental divorce/separation. Other measures, including sexual abuse, were constructed from retrospective questions in Waves III and IV, and I address possible recall bias below by allowing correlated measurement error across models of ACEs and outcomes. See survey questions for ACEs in Online Appendix 1.

I use information on family background and where participants went to school from Wave I, and then I categorize race/ethnicity as Hispanic, non-Hispanic Asian/Pacific Islander, non-Hispanic Black, non-Hispanic Native American, non-Hispanic white, or non-Hispanic of another race (includes multi-racial). I use parent-reported household income and highest parental educational attainment from Wave I as measures of childhood SES. I study outcomes reported in Wave IV: high school diploma or higher, Bachelor's degree or higher, full-time employment (35+ hours per week), and earnings level.

## 2.2 | Missing data and imputation

Rates of missing data are generally low, and from the sample participating in Wave I and Wave IV home interviews, rates of missing data are less than 2% for all but two variables in the fully controlled baseline models. I address this issue using multiple imputation for analysis (see Online Appendix 2 for details), which involves imputing multiple values for missing observations, including a random error component; performing analyses on each complete data set; pooling estimates; and adjusting standard errors for the uncertainty of the imputation procedure (Rubin, 1996). I impute missing values for adult earnings, which have slightly less than 5% missing, and for childhood household income, which have 24% missing due to non-participation in the Wave I parent interview. Where parent education level was not available from the parent interview, values were imputed from reports in the Wave I child interview. Online Appendix 3, Table A1 reports child and family characteristics and ACEs across the Wave I child-interview sample, the longitudinal sample with outcomes from complete cases, and the longitudinal sample with imputed cases, which is the sample used for analysis here. Inclusion of childhood sexual abuse only in Waves III and IV surveys precludes ability to study attrition by history of sexual abuse. Among the ACEs measured during Wave I, prevalence rates across the analysis sample and original cohort are statistically different only for knife or gun violence, where the difference is 0.4 percentage points (lower in analysis sample).

### 2.3 | Sample characteristics

History of childhood sexual abuse was prevalent in this nationally representative cohort of late Generation $X$ and early Millennials. In the self-interview module, 14.1% of adults reported a history of contact sexual abuse in childhood. There were large differences in prevalence of childhood sexual abuse by sex and childhood household income. Among females, the prevalence was 20.7 (standard error [s.e.], 0.7) percent, and among males it was 7.9 (0.6) percent. Measuring prevalence by childhood household income quintile, childhood sexual abuse was reported by 20.0% of individuals from the bottom quintile and 9.3% of individuals from the top quintile. Among the group that reported childhood sexual abuse, 54.0% reported abuse by an adult caregiver and 60.3% reported abuse by a non-caregiver.

There were several differences in observable characteristics across those who experienced childhood sexual abuse and those who did not (Table 1). Compared to individuals without a history of childhood sexual abuse, those with a history were more often from families of lower SES and had higher rates of other ACEs, disability, and depressive symptoms in childhood. Adult survivors of childhood sexual abuse had much higher rates of depressive symptoms, and as the unadjusted means in the table show, they also had lower levels of education, lower rates of full-time employment, and lower earnings. While some children experienced multiples types of ACEs, there were low correlations between childhood sexual abuse and other ACEs (see Online Appendix 3, Table A2). The largest correlation was 0.186, with physical abuse.

## 3 | EMPIRICAL APPROACH

I employ several strategies to evaluate impacts of childhood sexual abuse on education and labor market outcomes. In the baseline model, I implement school fixed effects regression controlling for childhood SES, childhood disability, and other ACEs. To address the possibility of remaining confounding, I construct bounds under varying assumptions about the importance of unobservable confounders. I examine robustness to alternative methodology and evaluate whether there were pre-existing differences in birth weight or cognitive ability. I then examine heterogeneity by family sociodemographic characteristics, perpetrator type, and availability of school services.

### 3.1 | Baseline model

In the baseline model, I control for demographics, childhood SES, physical disability, school fixed effects, and other ACEs. In the baseline specification below, let $Y_i$ denote the outcome for individual $i$. $Y_i$ is either a binary variable for having a high school diploma or higher education, having a Bachelor's degree or higher, currently employed full-time; or earnings level. The parameter of interest is $\alpha$ in the following equation:

$$f(Y_i) = \alpha \cdot ChildhoodSexualAbuse_i + X_i'\beta + \varepsilon_i, \qquad (1)$$

where $ChildhoodSexualAbuse_i$ is a binary variable equal to one for persons who reported childhood sexual abuse in the self-interview and $X_i$ is a vector of controls and includes a constant. To examine the importance of confounding from particular observable characteristics on changes in $\alpha$, I include components of $X_i$ in successive regressions. Following unadjusted regression of outcomes on childhood sexual abuse, the first adjustment adds demographics (age and sex reported when outcomes were measured in Wave IV, race/ethnicity reported in Wave I). The second adjustment adds controls for childhood household SES (highest parental education level and log household income reported in Wave I), the third adjustment adds school fixed effects, the fourth adjustment adds control for childhood physical disability reported in Wave I, and the final adjustment resulting in the preferred specification adds controls for the other ACEs described above (physical abuse, emotional abuse, experience or threat of knife or gun violence, parental incarceration, parental divorce or separation, and availability of illegal drugs in the household). The model includes an individual idiosyncratic error term $\varepsilon_i$.

By including school fixed effects, I parse out unobserved characteristics of the school and school-neighborhood environment that affect both the likelihood of experiencing childhood sexual abuse and the likelihood of success in school and on the labor market, such as local crime and childcare resources. In the following sections, I address the possibility of remaining unobserved confounding at the family or individual level.

I use a linear probability model for binary outcomes then examine annual earnings with a two-part model (Dow & Norton, 2003) to include the 7.1% of individuals with zero earnings.[3]

TABLE 1 Sample means across childhood sexual abuse history

| | (1) | | (2) | | |
|---|---|---|---|---|---|
| | **No** | | **Yes** | | **(3)** |
| Female | 0.459 | (0.008) | 0.729 | (0.017) | *** |
| Race/ethnicity | | | | | *** |
| Hispanic | 0.108 | (0.016) | 0.124 | (0.015) | |
| Non–Hispanic Asian/Pacific Islander | 0.027 | (0.007) | 0.018 | (0.007) | |
| Non–Hispanic black | 0.143 | (0.02) | 0.171 | (0.026) | |
| Non–Hispanic Native American | 0.008 | (0.003) | 0.015 | (0.007) | |
| Non–Hispanic white | 0.782 | (0.024) | 0.748 | (0.028) | |
| Non–Hispanic, race not listed | 0.033 | (0.003) | 0.049 | (0.007) | |
| Parent education | | | | | |
| High school diploma | 0.871 | (0.013) | 0.830 | (0.014) | * |
| College degree | 0.328 | (0.019) | 0.233 | (0.017) | *** |
| Childhood household income, median (2010$) | $58,782 | | $44,087 | | — |
| Adverse childhood experiences | | | | | |
| Sexual abuse | 0.000 | (0.000) | 1.000 | (0.000) | — |
| Physical abuse | 0.145 | (0.005) | 0.360 | (0.018) | *** |
| Emotional abuse | 0.091 | (0.004) | 0.260 | (0.012) | *** |
| Knife or gun violence | 0.135 | (0.007) | 0.169 | (0.013) | ** |
| Parental incarceration | 0.128 | (0.008) | 0.249 | (0.014) | *** |
| Parental divorce or separation | 0.296 | (0.008) | 0.426 | (0.018) | *** |
| Illegal drugs in household | 0.027 | (0.002) | 0.048 | (0.007) | *** |
| Child health | | | | | |
| Physical disability | 0.022 | (0.002) | 0.038 | (0.007) | *** |
| Learning disability | 0.124 | (0.007) | 0.152 | (0.013) | |
| Depressive symptoms | 0.285 | (0.008) | 0.428 | (0.019) | *** |
| Adult outcomes | | | | | |
| Depressive symptoms | 0.244 | (0.009) | 0.403 | (0.017) | *** |
| High school diploma or higher | 0.863 | (0.01) | 0.748 | (0.017) | *** |
| Bachelor's degree or higher | 0.340 | (0.018) | 0.201 | (0.016) | *** |
| Full–time employment | 0.741 | (0.009) | 0.623 | (0.017) | *** |
| Earnings, median (2010$) | $30,414 | | $22,318 | | — |
| Observations | 11,124 | | 1772 | | |
| Column mean of full sample | 0.859 | | 0.141 | | |

*Note*: Column (1) displays weighted variable means and standard errors for the sample who reported no childhood sexual abuse, with the median value displayed where noted. Column (2) displays this information for the sample who reported childhood sexual abuse.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

*Source*: Author's calculation from Add Health data.

## 3.2 | Bounds in consideration of unobservable confounding

Because childhood sexual abuse is non-random, the baseline estimates may be contaminated by unobserved confounders which explain childhood sexual abuse and adult outcomes. For example, level of parental investment in children may explain sexual abuse—whether by a caregiver or when unsupervised—as well as adult educational attainment and labor market outcomes. To evaluate the likelihood that unobservable confounding explains the full magnitude of the baseline estimates, in this section I calculate likely bounds on the effects of childhood sexual abuse under varying assumptions about the degree of confounding from unobservables relative to confounding from observables, following partial identification methods developed by Altonji et al. (2002, 2005) and Oster (2019). Partial identification methods allow the researcher to recover bounds on estimated effects in contexts where unobservable variables may cause confounding (Altonji et al., 2002, 2005; Krauth, 2016; Oster, 2019).[4] Methods developed by Altonji et al. (2002, 2005), Krauth (2016), and Oster (2019) rely on the premise that confounding on observables provides insight into the influence of confounding on unobservables; they present effect bounds under an assumption on the upper bound of the amount of confounding from unobservables relative to the amount of confounding from observables.

I adopt the Oster (2019) approach, which extends work by Altonji et al. (2002, 2005) and Krauth (2016) by showing that treatment effects are bounded not only by the relative confounding parameter but also by the amount of variance explained by unobserved confounders. In general, it is unlikely that the exposure, controls, and the unobservable confounders fully explain the outcome, because not all unobserved elements that influence outcomes are unobserved *confounders* (Oster, 2019). Here, idiosyncratic error might include elements of personality that influence educational attainment and labor market outcomes but are uncorrelated with ACEs and child, family, and school controls. The key parameters are (i) the amount of confounding from unobservables relative to confounding from observables (ratio $\delta$) and (ii) the amount of outcome variance that would be explained in a hypothetical regression including any unobservable confounders on the right-hand side ($R_{\max}$, the $R^2$ from the hypothetical regression).

I present lower bounds under the assumptions that confounding from unobservables is as large as confounding from observables ($\delta = 1$) and, following Oster's proposed condition, $R_{\max} = 1.3\tilde{R}$, where $\tilde{R}$ is the $R^2$ from Equation (1) with the full set of observed controls.[5] I review robustness of bounds to varying values of $\delta$ from 0 to 1 along with $R_{\max} = 1.3\tilde{R}$ or $R_{\max} = 2\tilde{R}$, following applications in the literature.[6] Allowing $R_{\max} = 2\tilde{R}$ assumes that unobserved confounders explain as much variance in the outcomes as do demographics, SES, ACEs, unobservables correlated with the ACEs other than sexual abuse, childhood disability, and school fixed effects. In addition to bounds on effect sizes, I calculate how large $\delta$ must be to imply that unobserved confounding fully explains the estimated effects of childhood sexual abuse (under assumptions $R_{\max} = 1.3\tilde{R}$ or $R_{\max} = 2\tilde{R}$).

Formally, to construct bounds on the effects of childhood sexual abuse or the ratio $\delta$ for which unobserved confounders would fully explain results, consider a modified version of Equation (1), omitting the individual subscripts $i$:

$$Y = \ddot{\alpha} \cdot ChildhoodSexualAbuse + X'\ddot{\beta} + W_2 + \eta, \tag{2}$$

where $W_1 \equiv X'\ddot{\beta}$, and $W_2$ represents the unobservable confounders, which impact the outcome $Y$ and are correlated with the likelihood of childhood sexual abuse but are not correlated with any of the observed controls. For example, level of parental investment in children will be contained in $W_2$ only if the partial correlation with childhood sexual abuse, conditional on $X$, is non-zero. By definition, as a confounder, $\text{cov}(W_2, Y) \neq 0$ and $\text{cov}(W_2, ChildhoodSexualAbuse) \neq 0$. The orthogonality requirement on $W_2$ described above ($\text{cov}(W_2, W_1) = 0$) implies that $W_1$ captures observables in addition to any confounding from unobservables correlated with the observed controls. Thus, the confounding of concern (from $W_2$) is limited to elements correlated with childhood sexual abuse and outcomes which are uncorrelated with physical abuse, emotional abuse, or any other controls. Here, the parameter $\ddot{\alpha}$ is the true effect of childhood sexual abuse on outcome $Y$, and the goal is to estimate bounds for $\ddot{\alpha}$. Last, $\eta$ is the individual idiosyncratic error term containing unobservables that impact the outcome $Y$ but are uncorrelated with childhood sexual abuse, controls, or $W_2$, such as personality as suggested above. Formally, the ratio of confounding from unobservables to confounding from observables is defined as:

$$\delta = \frac{\sigma_{2,csa}}{\sigma_2^2} \Big/ \frac{\sigma_{1,csa}}{\sigma_1^2},$$

where $\sigma_{j,csa} = \text{cov}(W_j, ChildhoodSexualAbuse)$ and $\sigma_j^2 = \text{var}(W_j)$ for $j \in \{1, 2\}$.

## 4 | RESULTS

### 4.1 | Baseline results

First, when grouping all ACEs together, the measure "any ACE" was associated with 8.2 (s.e., 0.9) percentage points lower likelihood of high school diploma receipt, 10.5 (1.0) percentage points lower likelihood of college degree attainment, 2.7 (0.9) percentage points lower likelihood of full-time employment, and \$2174 (702) lower earnings (7.1% of median earnings) when controlling for demographic characteristics, race/ethnicity, parent education, childhood household income, childhood physical disability, and school fixed effects (results not shown). Next, when splitting ACEs into the individual components, sexual abuse was associated with 7.1 percentage points lower likelihood of high school diploma receipt, 7.2 percentage points lower likelihood of Bachelor's degree receipt, 5.9 percentage points lower likelihood of full-time work, and $4815 lower earnings (16% of median earnings) (Table 2).[7] Knife or gun violence, parental incarceration, and parental divorce or separation also were associated with reduced educational attainment, while only parental incarceration, in addition to childhood sexual abuse, was associated with reduced likelihood of full-time employment and reduced earnings when considering all ACEs simultaneously (Online Appendix 3, Tables A3–A6). Meanwhile, among types of childhood abuse, sexual abuse was uniquely predictive of worse education and labor market outcomes in adulthood. In sensitivity analyses excluding individuals who experienced sexual abuse prior to Wave I (so that family controls are measured before the abuse), results are larger in magnitude. (Notably, in the sample for this exercise, individuals were older when experiencing childhood sexual abuse compared to the full sample.)

In these baseline models, observables explain 38% of the raw difference in rates of high school diploma receipt between those who did and did not report childhood sexual abuse and 48% of the raw difference in college completion rates (Table 2). Observed characteristics explain about 50% of the raw difference in full-time employment and 52% of the raw difference in earnings.

**TABLE 2** Estimates of average marginal effects of childhood sexual abuse on education and labor market outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Controls** | **None** | **Demo–graphics[a]** | **Col. 2 + SES[b]** | **Col. 3 + school FE[c]** | **Col. 4 + disability[d]** | **Col. 5 + other ACEs[e]** |
| Panel A. High school diploma or higher; mean (s.d.): 0.827 (0.378) | | | | | | |
| OLS | −0.115*** | −0.125*** | −0.101*** | −0.090*** | −0.088*** | −0.071*** |
| | (0.015) | (0.016) | (0.015) | (0.013) | (0.013) | (0.013) |
| $R^2$ | 0.012 | 0.026 | 0.094 | 0.147 | 0.148 | 0.170 |
| Observations | 13,672 | 13,672 | 13,672 | 13,672 | 13,672 | 13,672 |
| Panel B. Bachelor's degree or higher; mean (s.d.): 0.301 (0.459) | | | | | | |
| OLS | −0.139*** | −0.156*** | −0.105*** | −0.096*** | −0.094*** | −0.072*** |
| | (0.015) | (0.014) | (0.013) | (0.012) | (0.012) | (0.012) |
| $R^2$ | 0.011 | 0.040 | 0.208 | 0.257 | 0.258 | 0.270 |
| Observations | 13,672 | 13,672 | 13,672 | 13,672 | 13,672 | 13,672 |
| Panel C. Full–time employment; mean (s.d.): 0.711 (0.453) | | | | | | |
| OLS | −0.118*** | −0.075*** | −0.066*** | −0.070*** | −0.069*** | −0.059*** |
| | (0.017) | (0.017) | (0.017) | (0.017) | (0.017) | (0.017) |
| $R^2$ | 0.008 | 0.041 | 0.048 | 0.094 | 0.095 | 0.097 |
| Observations | 13,697 | 13,697 | 13,697 | 13,697 | 13,697 | 13,697 |
| Panel D. Earnings; mean (s.d.): $35,006 (44,144); median: $30,414 | | | | | | |
| 2p.m. | −$10,099*** | −$6499*** | −$4450** | −$4854*** | −$4798*** | −$4815*** |
| | (1,144) | (1,270) | (1,365) | (1,032) | (1,036) | (1,133) |
| Observations | 12,478 | 12,478 | 12,478 | 12,478 | 12,471 | 12,305 |

*Note*: This table displays estimated average marginal effects of childhood sexual abuse and standard errors. For a given outcome, each of the six models were implemented in a constant sample requiring non–missing data for the full control set.

[a]Demographic controls include age when outcome was measured, sex, and race/ethnicity.

[b]Household SES controls include log of childhood household income and highest parental educational attainment categorized as (i) less than high school, (ii) general equivalency degree, (iii) high school diploma, (iv) vocational school after high school, (v) some college, (vi) college graduate, or (vii) beyond 4–year college.

[c]School fixed effects: schools were the primary sampling unit.

[d]Disability represents childhood physical disability.

[e]Other ACEs include physical abuse, emotional abuse, knife or gun violence or threat, parental divorce or separation, parental incarceration, and illegal drugs in home.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

*Source*: Author's calculation from Add Health data.

### 4.2 | Robustness to additional controls

To inform the bounding exercise, I perform sensitivity analyses with additional controls to examine changes in coefficients on childhood sexual abuse and the amount of variance explained across regressions. Estimates of the effects of childhood sexual abuse are robust to the inclusion of additional regressors that may be confounders: parent disability, childhood learning disability, teen dating violence, physical or supervisory neglect, and foster care placement (Table 3). Additional controls were excluded from baseline models due to high rates of missing data (surveyed separately from participant in-home Wave I and Wave IV interviews) or the possibility of being outcomes of sexual abuse and resulting in over-controlling. Comparing the coefficients from the baseline model (applied to the samples with non-missing observations of the added control) show very small reductions in the magnitude of estimates for childhood sexual abuse (Online Appendix 3, Tables A3–A6). While the coefficients on childhood learning disability are larger in magnitude than the estimates for childhood sexual abuse, including learning disability as a control reduces OLS estimates of effects of childhood sexual abuse by less than one percentage point.

### 4.3 | Bounds in consideration of unobservable confounding

Based on the robustness to varying assumptions about confounding from unobservables, the results in Figure 1 suggest that childhood sexual abuse causes lower educational attainment and worse labor market outcomes. Allowing as much confounding from unobservables as from observables and applying the Oster parameter $R_{max} = 1.3\tilde{R}$, along with baseline results reported above, suggest that childhood sexual abuse leads to a 6.1–7.1 percentage points lower likelihood of high school diploma receipt, 5.1–7.2 percentage points lower likelihood of Bachelor's degree attainment, and 4.3–5.9 percentage points lower likelihood of full-time employment. In relative terms, these magnitudes translate to 35.6%–41.2% greater likelihood of high school dropout, 16.9%–23.9% lower likelihood of Bachelor's degree attainment, and 6.0%–8.3% lower likelihood of full-time employment.[8] Bounds under these assumptions suggest that childhood sexual abuse leads to 12.9%–18.7% lower earnings.[9]

The figure also shows alternative bounds under varied assumptions about the ratio of confounding from unobservables to confounding from observables and the influence of unobserved confounders on outcomes. Lower bounds still suggest negative effects on high school diploma receipt under the strictest assumptions and, for other outcomes, only cross zero under the combination of strictest assumptions modeled ($\delta = 1$, while $R_{max} = 2\tilde{R}$).

Online Appendix 3, Table A7 presents the relative confounding parameter $\delta$ which would imply no effect of childhood sexual abuse; that is, that unobserved factors fully explain the coefficients on childhood sexual abuse estimated from baseline models. Under condition $R_{max} = 1.3\tilde{R}$, the amount of remaining unobservable confounding would have to be more than twice the amount of observable confounding in order for unobservables to explain away estimated effects of childhood sexual abuse on any outcome. If $R_{max} = 2\tilde{R}$, the minimum level of unobservable confounding required to explain away an estimated effect is for Bachelor's degree receipt: 82% of the amount of observed confounding.

### 4.4 | Robustness to alternative methodology

While Oster (2019) methods address any type of unobservable confounding, I next take alternative approaches focused on specific sources of confounding. To address the concern of recall or other measurement bias being present and correlated with the other ACEs or reports of education or labor market outcomes, I model each ACE as an outcome observed with error—as a function of the other controls (demographics, household SES, childhood physical disability, and school fixed effects)—simultaneously with Equation (1) modeling the education or labor market outcome. Estimates of the effects of childhood sexual abuse from seemingly unrelated regressions, including models of ACEs as outcomes observed with error, show that once allowing correlated error across models of each ACE and the education or labor market outcome, estimated effect sizes are similar to baseline results (Online Appendix 3, Table A8).

Next, I address the possibility of confounding by unobserved family characteristics by controlling for sibling experience of childhood sexual abuse. Among the analysis sample with siblings in Add Health (2106 families), once controlling for sibling experience of sexual abuse, own experience of sexual abuse remains negatively correlated with all outcomes and significant in all but the model of full-time employment (Table 4).[10] Meanwhile there is no association between sibling experience of sexual abuse and own adult outcomes. Results suggest that own experience of sexual abuse—separate from the experience of growing up in a family in which childhood sexual abuse occurred—is related to negative outcomes in adulthood.

TABLE 3 Estimates of average marginal effects of childhood sexual abuse on education and labor market outcomes: robustness to additional controls

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Panel A. High school diploma or higher; mean (s.d.): 0.827 (0.378) | | | | | |
| Sexual abuse | −0.070*** (0.013) | −0.067*** (0.014) | −0.068*** (0.016) | −0.074*** (0.014) | −0.075*** (0.013) |
| Parent disability | −0.047** (0.016) | | | | |
| Child learning disability | | −0.099*** (0.017) | | | |
| Teen dating violence | | | −0.078 (0.041) | | |
| Neglect | | | | −0.022 (0.013) | |
| Foster care | | | | | −0.142* (0.060) |
| $R^2$ | 0.172 | 0.179 | 0.179 | 0.180 | 0.176 |
| Observations | 13,672 | 12,371 | 9766 | 11,059 | 11,479 |
| Panel B. Bachelor's degree or higher; mean (s.d.): 0.301 (0.459) | | | | | |
| Sexual abuse | −0.072*** (0.012) | −0.065*** (0.012) | −0.065*** (0.015) | −0.069*** (0.014) | −0.071*** (0.014) |
| Parent disability | −0.019 (0.018) | | | | |
| Child learning disability | | −0.159*** (0.017) | | | |
| Teen dating violence | | | −0.034 (0.036) | | |
| Neglect | | | | −0.020 (0.016) | |
| Foster care | | | | | −0.064* (0.029) |
| $R^2$ | 0.270 | 0.284 | 0.283 | 0.274 | 0.275 |
| Observations | 13,672 | 12,371 | 9766 | 11,059 | 11,479 |
| Panel C. Full–time employment; mean (s.d.): 0.711 (0.453) | | | | | |
| Sexual abuse | −0.059*** (0.017) | −0.054** (0.017) | −0.064** (0.020) | −0.077*** (0.019) | −0.075*** (0.018) |
| Parent disability | −0.018 (0.021) | | | | |
| Child learning disability | | −0.092*** (0.017) | | | |
| Teen dating violence | | | −0.015 (0.045) | | |
| Neglect | | | | 0.020 (0.016) | |
| Foster care | | | | | −0.024 (0.052) |
| $R^2$ | 0.096 | 0.107 | 0.095 | 0.092 | 0.093 |
| Observations | 13,697 | 12,394 | 9785 | 11,078 | 11,501 |
| Panel D. Earnings; mean (s.d.): $35,006 (44,144); median: $30,414 | | | | | |
| Sexual abuse | −$4836*** (1123) | −$4911*** (1208) | −$3890*** (1077) | −$4376*** (1175) | −$4504*** (1192) |
| Sexual abuse (AME % of median earnings) | −16% | −16% | −13% | −14% | −15% |
| Parent disability | $612 (1666) | | | | |
| Child learning disability | | –$9701*** (1040) | | | |
| Teen dating violence | | | $3218 (2424) | | |
| Neglect | | | | $781 (1291) | |
| Foster care | | | | | –$7329** (2547) |
| Observations | 12,305 | 11,063 | 8544 | 9856 | 10,209 |

*Note*: Each regression includes the full control set described in notes to Table 2: demographics, childhood SES, school fixed effects, physical disability, and other ACEs. Binary outcomes are modeled with OLS, and average marginal effects on earnings are estimated from a two–part model. See full regression results in Online Appendix 3, Tables A3–A6.

$*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

*Source*: Author's calculation from Add Health data.

As final robustness checks, I evaluate the potential threat that childhood sexual abuse co-occurred alongside other limits in human capital development or developmental health at birth, such as low parental investment in time with children or adverse prenatal environment (due to parent behavior or other circumstances during gestation). I do not detect any pre-existing

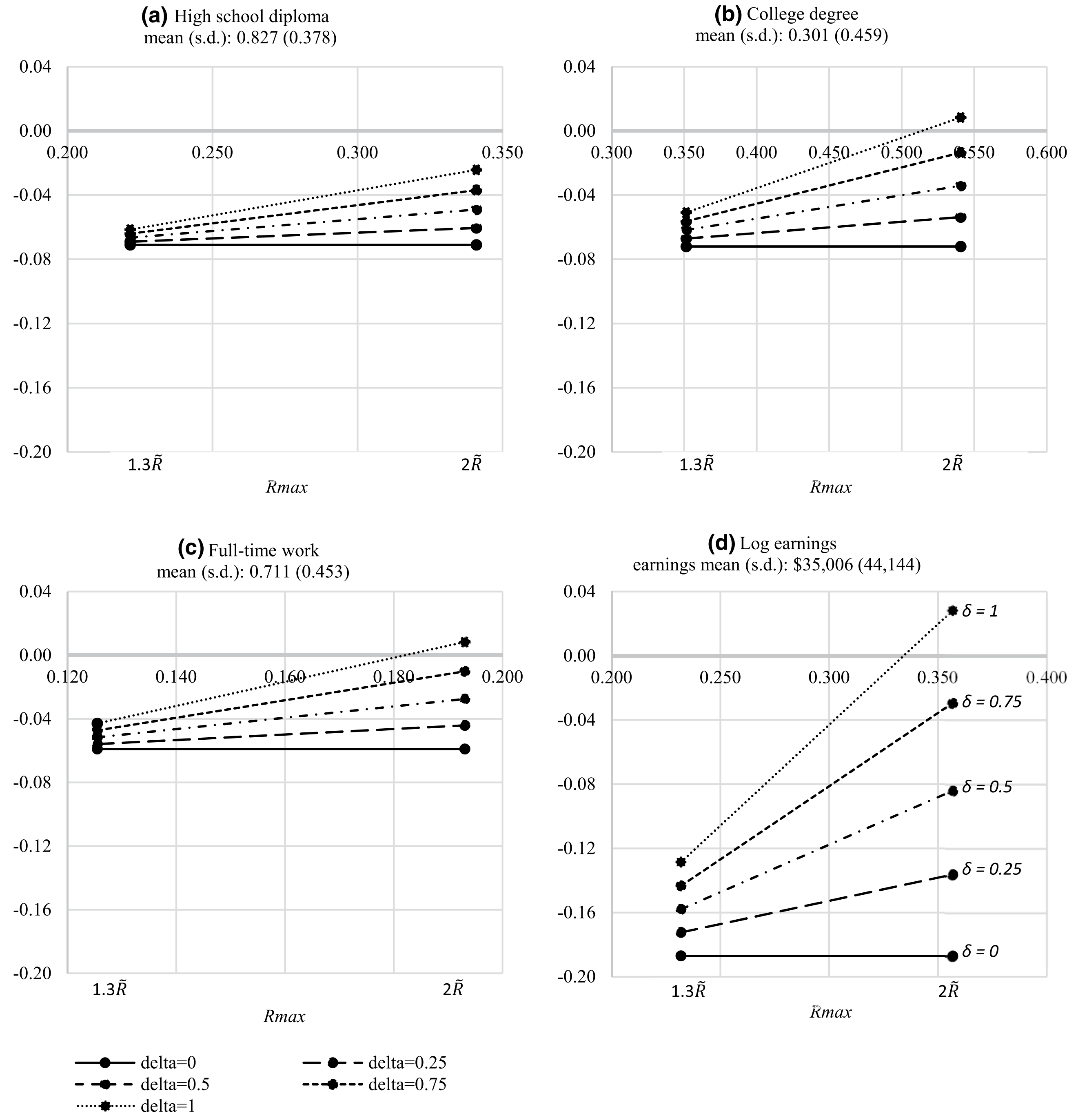


FIGURE 1 Bounds on the Effects of Childhood Sexual Abuse Under Varying Assumptions About Confounding from Unobservables. The figure above depicts the average marginal effect of childhood sexual abuse under varying assumptions about the importance of unobservables. Models controlled for age when outcome was measured, sex, race, highest parental educational attainment, childhood household income, school fixed effects, childhood disability, and other adverse childhood experiences (ACEs) (childhood physical abuse, emotional abuse, knife or gun violence or threat, parental divorce or separation, parental incarceration, and illegal drugs in home). The points marked represent, from left to right: $R_{max} = 1.3\tilde{R}$ and $R_{max} = 2\tilde{R}$. When $\delta = 0$, $R_{max}$ must equal $\tilde{R}$, but the average marginal effect for $\delta = 0$ is plotted over the same values of $R_{max}$ for ease of comparison with the estimates under assumptions of $\delta > 0$. [a] Panel (d) models log earnings, among those with any earnings. OLS models of the binary outcome "having any earnings" showed no relationship between childhood sexual abuse and having positive earnings. Descriptive statistics for earnings noted here are provided for the sample with positive earnings. *Source*: Author's calculation from Add Health data

**TABLE 4** Sibling experience of sexual abuse

| | (1) High school diploma or higher | (2) Bachelor's degree or higher | (3) Full–time employment | (4) Earnings |
|---|---|---|---|---|
| *Own* experience of sexual abuse | −0.078* (0.034) | −0.059* (0.028) | −0.034 (0.032) | –$3854* (1707) |
| *Sibling* experience of sexual abuse | −0.022 (0.028) | −0.024 (0.032) | −0.047 (0.038) | –$664 (1917) |
| Observations | 4837 | 4837 | 4822 | 3040 |

*Note*: This table displays estimates of average marginal effects and standard errors for own experience of childhood sexual abuse and sibling experience of childhood sexual abuse among the sample with siblings, where columns (1)–(3) display OLS estimates, and column (4) displays estimates from a two–part model. Each regression controls for demographics, childhood socioeconomic status, physical disability, and other ACEs and includes school fixed effects as in the baseline model (see Table 2 notes).

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

*Source*: Author's calculation from Add Health data.

**TABLE 5** Estimates of average marginal effects of childhood sexual abuse on education and labor market outcomes: results in subsamples by sex

| | (1) High school diploma or higher | (2) Bachelor's degree or higher | (3) Full–time employment | (4) Earnings |
|---|---|---|---|---|
| Sample: Females | −0.070*** (0.017) | −0.076*** (0.017) | −0.046* (0.020) | –$2749 (1468) |
| Observations | 7390 | 7390 | 7389 | 6700 |
| Sample: Males | −0.103*** (0.030) | −0.074*** (0.018) | −0.057 (0.034) | –$7960*** (1763) |
| Observations | 6310 | 6310 | 6308 | 5761 |

*Note*: This table displays estimates of the average marginal effects of childhood sexual abuse separately for females and for males. Columns (1)–(4) display OLS estimates, and column (5) displays average marginal effects from a two–part model, with standard errors in parentheses. The baseline regression described above, but excluding school fixed effects and sex, is implemented separately in the subsample of females and subsample of males.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

*Source*: Author's calculation from Add Health data.

differences in birth weight (Online Appendix 3, Table A9) or cognitive performance before experience of sexual abuse (Online Appendix 3, Table A10).

## 4.5 | Heterogeneity by child, perpetrator, and school characteristics

In separate regressions, I modify the baseline model by interacting childhood sexual abuse with sex, race/ethnicity, log childhood household income, and school services to consider how supports in childhood, adult personal networks, and labor market discrimination experienced may vary across groups and modify relationships between childhood sexual abuse and adult outcomes. In addition, I replicate baseline models in the two subsamples of males or females, as the former is largely understudied in literature on sexual abuse.

Online Appendix 3, Tables A11–A14 show there are some differences in the relationship between childhood sexual abuse and adult outcomes by sociodemographic characteristics. Table 5 confirms that childhood sexual abuse predicts lower educational attainment and earnings for men. I do not detect differences by sex in the relationships between child sexual abuse and any of the education or labor market outcomes. Examination of results from models with interactions between childhood sexual abuse and race/ethnicity shows that the association between sexual abuse and high school diploma receipt is attenuated for non-Hispanic Native Americans, while the association between sexual abuse and earnings level is attenuated for non-Hispanic Asian or Pacific Islanders. Considering differences by childhood household income, for every 10% increase in household income, childhood sexual abuses was associated with an additional 0.5 percentage point reduction in the likelihood of college degree attainment. None of the disparities by sociodemographic characteristics described here are explained by differential rates of current depressive symptoms. Neither sex, nor race, nor childhood household income modify the relationship between childhood sexual abuse and adult depressive symptoms.

Next, examining results by perpetrator identity show that the associations between non-caregiver sexual abuse and outcomes are larger in magnitude than the associations between caregiver sexual abuse and outcomes—by more than double for full-time employment—though these differences are not statistically significant (Table 6).

TABLE 6 Perpetration by caregivers versus non–caregivers

| | (1) High school diploma or higher | (2) Bachelor's degree or higher | (3) Full–time employment | (4) Earnings |
|---|---|---|---|---|
| Sexual abuse *by caregiver* | −0.053** (0.018) | −0.049** (0.016) | −0.033 (0.022) | –$3750** (1289) |
| Sexual abuse *by non–caregiver* | −0.061*** (0.017) | −0.079*** (0.017) | −0.072*** (0.019) | –$5452*** (1495) |
| Observations | 13,642 | 13,642 | 13,666 | 12,829 |

*Note*: This table displays estimates of average marginal effects of childhood sexual abuse and standard errors, where columns (1)–(3) display OLS estimates, and column (4) displays estimates from a two–part model. Each regression controls for demographics, childhood socioeconomic status, physical disability, and other ACEs and includes school fixed effects as in the baseline model (see Table 2 notes).

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

*Source*: Author's calculation from Add Health data.

Lastly, I replaced school fixed effects with specific school services reported by school administrators and interacted childhood sexual abuse with availability of these services at the child's school: emotional counseling, rape counseling, physical violence program (*e.g.*, family violence, partner abuse), drug or alcohol use disorder program, and drug awareness and resistance education.[11] Having a physical violence program at the school attenuated the negative association of childhood sexual abuse with high school diploma receipt by more than half and was associated with a direct increase in the likelihood of full-time employment by about three-quarters of the magnitude of the coefficient on childhood sexual abuse (Online Appendix, Tables A11–A15). These results must be considered with caution since services are not randomly distributed across schools and may be correlated with other aspects of the school and local environment that are no longer swept out through school fixed effects. Indeed, presence of a drug awareness and resistance education program was associated with lower likelihood of full-time employment.

### 4.6 | Ancillary outcomes: Exploration of mechanisms

In further analyses, I replicated baselines models with ancillary outcomes which could be mechanisms between childhood sexual abuse and adult education and labor market outcomes: childhood depressive symptoms, grades, childhood report of missing school or work due to a health or emotional problem in the past month, adult report of diagnosis of anxiety ever (including panic disorder and post-traumatc stress disorder), adult depressive symptoms, and adult report of health- or emotional-related absence in the past month.[12] Childhood sexual abuse was associated with worse mental health in adolescence and adulthood, lower grades in adolescence, and greater likelihood of a recent health- or emotional-related absence in adulthood. Specifically, childhood sexual abuse before Wave I was associated with 5.8 percentage points greater likelihood of depressive symptoms in childhood and 0.76 times the odds of a higher grade compared to children not exposed to sexual abuse before Wave I, driven by differences in grades in English and science (Online Appendix 3, Tables A16 and A17). Notably, childhood sexual abuse after Wave I was not associated with any differences in Wave I outcomes. Sexual abuse at any time before age 18 was associated with 9.3 percentage points greater likelihood of having been diagnosed with anxiety ever, 10.5 percentage points greater likelihood of depressive symptoms in adulthood, and 4.4 percentage points greater likelihood of recent health- or emotional-related absence in adulthood (Online Appendix 3, Table A16).

Exploring mechanisms behind the relationship between childhood sexual abuse and adult earnings shows that results are not fully explained by depressive symptoms at one point in time, adding depressive symptoms from childhood and adulthood as regressors in separate models (Online Appendix 3, Table A18). In addition, the negative relationship persists when accounting for survivors' reduced educational attainment: the average marginal effect of childhood sexual abuse on earnings is –$3491, or 11% of median earnings, when education level is included as a regressor.

## 5 | DISCUSSION AND CONCLUSIONS

This paper provides a collection of evidence suggesting that childhood sexual abuse has durable impacts on human capital and economic well-being. Using longitudinal survey data from a U.S. nationally representative sample, I measure negative effects of childhood sexual abuse on education and labor market outcomes when controlling for childhood SES and school fixed effects. I show that results persist once parsing out confounding from other ACEs including physical and emotional abuse and childhood SES along with unobservables correlated with these and other controls, and results are fairly robust to varying assumptions about the remaining amount of confounding from unobservables, robust to alternative models, and robust to tests for pre-existing differences in birth weight and cognitive performance. Evidence of impacts of childhood sexual abuse is strongest for high school dropout; these estimates survive even the strictest assumptions about the importance of unobservable confounding. Exploration of mechanisms shows that childhood sexual abuse was associated with worse mental health in adolescence and adulthood, worse school performance in adolescence, and greater likelihood of missed work due to health or emotional barriers in adulthood. Findings are consistent with Goodman et al. (2011) measuring large impacts of childhood psychological problems on reduced likelihood of working and reduced family income in adulthood, using prospective longitudinal data from a British birth cohort.

Results suggest that childhood sexual abuse leads to a 36%–41% greater likelihood of high school dropout, 17%–24% lower likelihood of Bachelor's degree attainment, 6%–8% lower likelihood of full-time employment, and 13%–19% lower earnings in young adulthood. To put in perspective, these bounds suggest that childhood sexual abuse leads to greater likelihood of high school dropout than being born low birth weight (which has been estimated to increase likelihood of dropout by 32%) and similar penalties on employment and earnings as being born low birth weight (Johnson & Schoeni, 2011). Disparities in earnings might widen with age. Considering these bounds for the effect of childhood sexual abuse on the likelihood of high school dropout along with data from the Current Population Survey on the earnings penalty of dropping out of high school, a back-of-the-envelope calculation suggests that the aggregate productivity loss due to childhood sexual abuse totals roughly $38 billion to $44 billion each year.[13] Beyond productivity losses, childhood sexual abuse (and other maltreatment) is associated with large externalities through costs to child protective services, disability pensions and other welfare programs, the criminal legal system, and the carceral system (Conti et al., 2021; Doyle Jr. & Aizer, 2018). Yet, previous estimates of the U.S. economic cost of childhood sexual abuse have excluded productivity losses for men due to lack of evidence of even an association with men's labor market outcomes (Letourneau et al., 2018).

While these estimated effect sizes are large, they may be conservative. I present lower bounds for effects of childhood sexual abuse under the assumption that confounding from unobservables is as large as confounding from observables, which may be a strict assumption. Any remaining unobservable confounders must be factors not correlated with other types of childhood abuse, violence victimization, or household adversity measured meanwhile exceed the amount of observable confounding—which is fairly large in this setting, in particular by sex, childhood SES, and other ACEs. Separately, underreporting of childhood sexual abuse could bias estimates toward zero due to a contaminated control group,[14] and sampling recruitment and retention might also lead to conservative estimates. The data used are from a national study using school-based recruitment and home interviews, which may miss some children with great disadvantages and lower school attendance rates, such as homeless children, who also face higher rates of sexual abuse (Cutuli et al., 2013; Tyler & Schmitz, 2018).[15] Thus, true population figures of the effects of childhood sexual abuse may be even larger than estimates presented here.

To be clear, I estimate bounds around average impacts of childhood sexual abuse and contribute evidence on contextual modifiers, while individual experiences which comprise these average results may vary greatly. In the psychology field, the term "post-traumatic growth" describes the phenomenon that, in some people, terrible adversity has stimulated remarkable strengthening (Tedeschi, Park, & Calhoun, 1998; Woodward & Joseph, 2003). Examining the heterogeneity of outcomes, here I show that associations between childhood sexual abuse and Bachelor's degree attainment are attenuated for those raised in lower income households, which might reflect greater resilience among those burdened by multiple types of adversity (Brunst et al., 2020) or diminishing marginal effects of disadvantage for other reasons. Association between childhood sexual abuse and high school diploma receipt was attenuated for non-Hispanic Native Americans, and the association between childhood sexual abuse and earnings was attenuated for non-Hispanic Asian or Pacific Islanders. This paper also establishes that associations between childhood sexual abuse and educational attainment and earnings are at least as large for men as for women. In addition, childhood sexual abuse by someone other than a caregiver is as influential or more so as caregiver sexual abuse in predicting worse outcomes.

There remains a critical need for identifying and implementing effective approaches to prevent, detect, and alleviate the lasting burdens of childhood sexual abuse. There is insufficient development of best practices for prevention through the health care setting (U.S. Preventive Services Task Force et al., 2018), schools, or communities. Researchers, however, have identified school-based prevention interventions and targeted prevention for survivors of sexual abuse and people with sexual interest in children as promising strategies showing some reductions in behaviors related to risk of perpetrating sexual abuse of children (Assini-Meytin et al., 2020; Fix et al., 2021; Letourneau et al., 2017). Other literature suggests how economic and other public policies can reduce rates of childhood sexual abuse. Prior work has attributed reductions in the rates of childhood sexual abuse to increased rates of male employment (Lindo et al., 2018) and increased minimum wage (Raissian & Bullinger, 2017). Bitler and Zavodny (2004) showed that legal access to abortion reduced rates of child maltreatment (while not shown for sexual abuse in particular). The protocol for addressing childhood sexual abuse, and maltreatment more generally, when reported to authorities is to refer to child protective services and consider investigation and placement in foster care. Empirical studies, however, have shown that when children who were maltreated and at the margin of being removed from their homes were placed in foster care, they earned less as adults and were much more likely to enter the criminal legal system (Doyle, 2007, 2008).[16] Another set of literature demonstrates the large, lasting value of early childhood schooling programs for children burdened by socioeconomic disadvantage—through improved child cognition status, reduction in externalizing behaviors, and increased academic motivation (García et al., 2020; Heckman et al., 2013). Providing similar supportive interventions to children who experience, or are at greater risk of experiencing, childhood sexual abuse may prevent the negative impacts on education and economic well-being, as measured here, and impacts on involvement in the criminal legal system demonstrated previously (Currie & Tekin, 2012). Considering the societal burden of childhood sexual abuse measured in this nationally representative sample, this study could inform policy and program decisions for resource allocation to prevention, detection, and support.

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## CONFLICT OF INTEREST

Dr. Henkhaus reports grants from Agency for Healthcare Research and Quality, during the conduct of the study. Dr. Henkhaus has no conflicts to disclose.

## DATA AVAILABILITY STATEMENT

Because this paper uses restricted-use data, the author is unable to make the data set publicly available. Information on how to obtain the Add Health data files is available on the Add Health website (http://www.cpc.unc.edu/addhealth).

## ETHICS STATEMENT

This work was approved by the institutional review boards at the University of Southern California and Vanderbilt University.

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## ENDNOTES

[1] Thirty-eight states included the questions about childhood sexual abuse in their BRFSS survey at some point during 2009–17.

[2] Fifty-two respondents were aged 33–34 years old at the time of the Wave IV interview.

[3] The first part of the two-part model is a probit model estimating the probability of having positive earnings, and the second part is a generalized linear model (log link function and gamma distribution) estimating earnings levels, using observations with positive earnings.

[4] Krauth (2016) presents a review of partial identification methods. Other methods of partial identification, such as Manski bounds, may apply when the outcome is bounded or may require the assumption that treatment effects are constant for a given set of observed covariates, which are not appropriate here (Manski, 1990).

[5] From a set of results from randomized studies published in top economics journals that reported uncontrolled and controlled estimates, Oster (2019) calculates that when holding fixed $\delta = 1$, 90 percent of the results would survive if $R_{\max} = 1.3\tilde{R}$. To "survive" means both that the identified set does not include zero and the identified set is within 2.8 standard errors of the fully controlled estimate.

[6] These choices for $R_{max}$ were informed by a review of literature citing Oster (2019). For example, see Ebenstein et al. (2017); Lavy and Sand (2019); Beach and Hanlon (2017); Lin et al. (2018); Anger et al. (2017); Álvarez and Palencia (2018); El-Mallakh et al. (2018); Gambaro et al. (2018), among several others.

[7] Allowing that some children might have attended schools outside of their home neighborhoods, in ancillary analyses I additionally control for information from the census block group of children's home residence: percentage of adults without a high school degree. The results were nearly identical to estimates reported here. In replications of OLS models for education and employment with probit and logit regression, average marginal effects and standard error estimates were identical or nearly identical to the OLS results.

[8] I translate average marginal effects to relative terms by dividing the average marginal effect by outcome means.

[9] Because the Oster method requires OLS to measure changes in $R^2$, I use log earnings for the bounding exercise. Childhood sexual abuse is not significantly associated with probability of positive earnings in the fully controlled baseline model, as shown in the two-part model of earnings in Online Appendix 3, Table A6.

[10] There are 2107 families with siblings in the analysis sample for the baseline model (one with missing information for sibling experience of childhood sexual abuse). Considering individuals in the base model analysis sample without siblings included in or with available data in Add Health, participants in the base model analysis sample come from 12,126 families.

[11] Models including specific school services included state fixed effects.

[12] Models of ancillary outcomes measured in adulthood are identical to the baseline model, while models of outcomes measured in childhood during the Wave I survey replace the key regressor in the main model (childhood sexual abuse at any time before age 18) with the two measures: (1) childhood sexual abuse before Wave I and (2) childhood sexual abuse after Wave I but before age 18. The distinction in timing of abuse before or after Wave I was identified by a subsequent question in the child maltreatment section in which respondents were asked, “How old were you the first time this happened?”, along with child age during the Wave I survey.

[13] Upper bound of aggregate productivity loss: 206 million (U.S. working age population per Federal Reserve Bank of St. Louis, 2022) x 14.1% (prevalence of childhood sexual abuse measured by author from Add Health) x 7.1 percentage points (lower likelihood of high school diploma receipt as measured in baseline model by author with Add Health) x $21,320 [earnings penalty of having less than a high school diploma calculated by author from a U.S. Bureau of Labor Statistics (2021) report using the Current Population Survey ($1029 median weekly earnings for all workers–$619 median weekly earnings for population with less than high school diploma) x 52 weeks in a year] = $44 billion. Lower bound of aggregate productivity loss: substitution with the lower bound of 6.1 percentage points lower likelihood of high school diploma receipt yields $38 billion.

[14] Underreporting of childhood sexual abuse leads to a contaminated control group including false negatives. As long as average outcomes of false negatives (people who experienced childhood sexual abuse but report no history of childhood sexual abuse) are no better than the average outcomes of true negatives (people who did not experience childhood sexual abuse and report no history of childhood sexual abuse), estimated effects of childhood sexual abuse, without accounting for this measurement error, will be biased toward zero.

[15] Comparison of child and family characteristics in Add Health Wave I with characteristics of the study sample with Wave IV data on educational attainment and labor market outcomes show that the study sample here had higher parental educational attainment and greater percentages of participants who were non-Hispanic white, experienced parental divorce or separation in childhood, and had a physical disability in childhood. Survey weights are used in all analyses to represent adults in 2008 enrolled in grades 7–12 during the 1994–1995 academic year (when Wave I was fielded).

[16] Whether findings from Doyle (2007) and Doyle (2008) hold for children who experienced sexual abuse is unknown, as they were excluded from main analyses due to inability to exploit random assignment of case managers–these cases were directed toward staff trained to handle sexual abuse reports.

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## SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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