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# The Long Shadow of Bullying: Career Consequences for An American Cohort

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# The long shadow of bullying: career consequences for an American cohort*

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**Abstract**

We document the long-run labor market consequences of youth bully victimization using NLSY97 data. Career outcomes measured at ages 19-40 account for life cycle bias. Victims exhibit lower earnings, lower job satisfaction and hold less-complex occupations. Fewer hours worked and shorter job tenure contribute to cumulative experience gaps that widen with age. Female respondents and adolescent victims are most significantly affected. A decomposition exercise shows that concurrent life-cycle health and education penalties explain half of the observed career penalties. Using household fixed-effects models and rich early-life covariates we show that selection into bullying on these dimensions cannot explain earnings penalties. Our results suggest a role for programs and policies that reduce health and human capital disparities of those bullied during youth.

Keywords: Bullying, Life-cycle outcomes, Earnings, Non-cognitive skills, Human Capital

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# 1 Introduction

A non-trivial share of children grow up in the shadow cast by bullies. More than 30% of teens in wealthy countries, including the US, experience bullying (UNICEF, 2013). Bully victimization is not only common but also costly. Immediate health consequences during youth include increased propensity for anxiety and depression, psychosis and PTSD (Antila et al., 2017; Idsoe et al., 2012; Takizawa et al., 2014; Wolke et al., 2014), decreased life satisfaction and increased health-negative behaviors including smoking (Sarzosa and Urzúa, 2021). Furthermore, these mental health problems can persist into adulthood (Copeland et al., 2013; Lereya et al., 2015; Wolke et al., 2013). Significant health-care costs have been documented in several countries (Jadambaa et al., 2021; Jantzer et al., 2019). Less is known about other private costs. Potential career consequences for individuals may be of particular interest to economists.

There is scope for bully victimization to affect career trajectories by mitigating the acquisition of human capital. The framework proposed by Sarzosa (2021) illustrates that bully victimization decreases cognitive and non-cognitive skill accumulation, opening skill gaps that widen over time. Empirical support for this channel is found across several countries where bully victims exhibit poor school attendance, decreased enrollment and lower grades (Baron, 2014; Eriksen et al., 2014; Ponzo, 2013; Vaillancourt et al., 2013; Kibriya et al., 2017; Ponzo, 2013; Nakamoto and Schwartz, 2010; Sarzosa and Urzúa, 2021) .[1] Victimization is also linked to negative later-life outcomes including crime and other antisocial behaviors generally associated with limited labor market success Farrington and Ttofi (2011); Bender and Lösel (2011); Lösel and Bender (2011); Ttofi et al. (2011). Wolke and Lereya (2015) reviews the earlier literature across disciplines.

Despite obvious importance, studies of the long-run economic consequences are surprisingly limited in number and scope. Brown and Taylor (2008), Brimblecombe et al. (2018) and Takizawa et al. (2014) show that bullying decreased educational attainment, future wages and qualitative measures of wealth among a 1958 British birth cohort.[2] Studies of American bully victims in their mid-twenties find decreased earnings for males (Mukerjee, 2019) and increased incidence of poverty (Wolke et al., 2013).[3] A related literature finds negative impacts from crime victimization on adult's careers (Bindler and Ketel, 2022; Ornstein, 2017; Velamuri and Stillman, 2008).

This paper provides the first long-run examination of the career consequences of youth bully victimization for the US. We use the 1997 National Longitudinal Survey of Youth (NLSY97) data to examine a large suite of labor market outcomes from ages 19−40. Retrospective self-reports allow us to identify and compare respondents who ever report youth bully victimization to those that do not, as well as examining separate victimization periods of childhood (ages <12) and adolescence (ages 12−18). Our findings show that bully victims fare much worse across several career outcomes than their non-bullied counterparts: they earn less hourly and annually (between 10 and 30% less), report an occupation mix that is less complex, supply fewer hours of labor, and experience lower job satisfaction.[4] We also note stark differences across the sexes: almost

[1] Chang and Padilla-Romo (2023) and Grogger (1997) show that exposure to schoolyard violence and neighborhood crime decreases schooling attainment and subsequently labor market performance.

[2] Le et al. (2005) find similar outcomes examining childhood conduct disorders among Australian Twins.

[3] A review of the earlier literature from economics and other disciplines can be found in Diagne (2009).

[4] Interestingly, while other studies find increased unemployment among victims (Takizawa et al., 2014; Varhama and Björkqvist,

all of the observed penalties in income, occupation and job satisfaction accrue among female respondents.

An important exercise after documenting victimization penalties is to explore the role of mediating life-cycle factors within the scope of policymakers. The efficacy of policies aimed at reducing youth may vary by method, duration and age (Ttofi and Farrington, 2011) and such policies cannot help those victimized in the past. Using a simple decomposition methodology introduced by Gelbach (2016) we illustrate that overall health and educational attainment are the most salient mediators. Concurrent life-cycle penalties in these outcomes explain about half of the observed penalties in career outcomes even conditional on a rich set of early-life covariates. Equalizing educational attainment and overall health might close mid-life wage and labor supply gaps by up to one-third and one-quarter, respectively.

One contribution of this study is to providing the first results into mid-career for the US and, as far as we are aware, the first long run analysis of individuals born anywhere since the 1980s. Labour market results for the UK are available at ages 42 and 50 but are limited to a single 1958 birth cohort (Brown and Taylor, 2008; Brimblecombe et al., 2018; Takizawa et al., 2014). Among US studies, Wolke et al. (2013) analyzes individuals from 11 North Carolina counties at ages 19, 21 and 24-26. The only study of national scope is Mukerjee (2019), which examines working respondents during a single calendar year (aged about 24 years). Our estimates over the mid-life cycle are an important extension to the literature to overcome the idiosyncrasies inherent in measuring labor market outcomes in a particular year and also because the effects of bullying may compound with time (Sarzosa, 2021). Our main analysis overcomes cohort bias by examining respondent outcomes at common ages while conditioning on birth year and birth quarter. The importance of our approach is highlighted by differences between our results and the results of Mukerjee (2019), which uses the same data source. The updated evidence we provide is also relevant because social dynamics have changed. Cohorts born since the 1980s experienced the anti-bullying movement that galvanized around the Columbine tragedy, which contributed to a wider change in societal views about bullying and our approaches to bullying behavior.

A second contribution is to provide the first analysis of a wider array of labor market outcomes and mechanisms. We are not aware of any other studies to examine job tenure, occupations and task portfolios or measures of job satisfaction. Though others have examined compensation, to the best of our knowledge we are also the first to document bully victimization penalties on hourly wages, position in the national earnings distribution and non-wage benefits. Brown and Taylor (2008), Brimblecombe et al. (2018), Gorman et al. (2019) and Takizawa et al. (2014) examine weekly earnings, which are the product of the hourly wage and hours worked. In terms of total annual earnings, existing evidence is limited to the year 2006 (Mukerjee, 2019). Excepting Sarzosa (2021) who consider latent measures of non-cognitive skill as a mechanism, the economics literature has focused on educational attainment and has largely overlooked the potential mediating roles of fertility, health, and early life experiences.[5]

Finally, to our knowledge our analysis also represents the first attempt to account for unobserved genetic and

2005; Strøm et al., 2013), we find no relationship with (annual) labor force participation rates.

[5]We cannot completely account for unobserved (latent) non cognitive traits described by Sarzosa and Urzúa (2021), which may include positive parental behaviors documented in Cunha et al. (2023). However, we do present estimates that attempt to capture measurable non-cognitive skills.

time-invariant household factors. We remove this remaining source of selection bias, not already addressed with rich early-life covariates, using household fixed-effects. Earnings penalties remain largely intact when comparing bully victimization status within sibling groups. Health and job satisfaction penalties do not, supporting the household-level interventions proposed by Cunha et al. (2023). From the former finding we conclude that the earnings penalties we document are not the result of sibling-similar factors that pre-dispose individuals to both bully victimization and low earnings, nor are they due to observable childhood environment measures or physical and psychological traits. These results imply important dynamic mechanisms beyond the early-life factors that preoccupy the psychology literature. Within-sibling earnings penalties found here complement causal results that bully victimization decreases pre-labor market outcomes such as school performance and educational attainment: (Sarzosa and Urzúa, 2021) use random class assignment in Korea, and Eriksen et al. (2014) exploit classmates' parental criminal histories in Danish data and Nikolaou (2022) exploits variation in anti-bully policy adoption across US states.

The remainder of the paper proceeds as follows: In Section 2 we provide details about our data including how we measure bully victimization and labor market outcomes. Section 3 documents bully victimization penalties in outcomes. Subsections 3.2 and 3.3 decomposition these penalties to illustrate potential mechanisms and illustrate heterogeneity in our findings, respectively. Section 4 concludes with policy recommendations.

# 2 Data

Our analysis uses rounds $1-20$ of the NLSY97.[6] These data follow 8,984 US residents born from 1980-1984 that were first interviewed in autumn 1997 at ages $12-17$. Interviews occurred yearly until 2011 when they were conducted every two years. The survey design comprises a main cross-sectional cohort and an oversample of black and/or Hispanic or Latino respondents. A total of 96,512 households were sampled following a multi-stage design with eligible cohort respondents selected from those households. Inference in our analysis accounts for survey design and we employ custom survey weights designed to make estimates representative of the US population corresponding to the cohort of analysis across each survey round.

## 2.1 Bully Victimization

The data include two retrospective self-reports about youth bully victimization. All respondents were asked the following yes or no question in round 1 (1997):

> *Before you turned age 12, were you ever the victim of repeated bullying?*

In the survey round following their 18th birthday, respondents were asked a second question:

[6]The NLSY97 survey is sponsored and directed by the U.S. Bureau of Labor Statistics and managed by the Center for Human Resource Research (CHRR) at The Ohio State University. Interviews are conducted by the National Opinion Research Centre (NORC) at the University of Chicago.

*Between the ages of 12 and 18, were you ever the victim of repeated bullying?*

Response rates to both questions are high: 8,833 (or 98%) respondents answered the childhood bullying question, 8,336 (93%) answered the adolescence bullying and = 8,195 respondents (or 91%) answered both questions. We limit our analysis to those who answer both. [7]

Our main measure of bully victimization, $BV_i^Y$ is a binary measure equal to unity if an individual answers yes to either question (reported repeated bullying aged $< 18$) and equal to zero if they answer no to both questions. We also provide results for separate indicators based on the the childhood (aged $<12$) and adolescence (aged $12-18$)questions, which we denote $BV_i^C$ and $BV_i^A$ respectively. Weighed sample means in the top left panel of table 1 show that about 20% of Americans born in the early 1980s were bullied during childhood and about 12% were bullied during adolescence. Bully victimization in at least one period of youth is reported by 26% of respondents.[8]

We consider the potential limitations of our data against others in the literature. Self-reports are also used in Korean data analyzed by Sarzosa and Urzúa (2015) and Nikolaou (2022), though (Brown and Taylor, 2008) and Eriksen et al. (2014) use parental or teacher reports. Our results might be more similar to other self-report analyses if subjective and objective victimization reports differ. We highlight that self-reports are suitable if victimization impacts manifest when they are internalized by victims. Recall bias is also a possibility because bully victimization questions are retrospective. Similar victimization rates in Denmark (Eriksen et al., 2014) and the UK (Brown and Taylor, 2008), bolster confidence in our measure and we suspect that such granular questions, whether a person ever was or never was *repeatedly* bullied, are less prone to recall bias.[9] Finally, attrition is a concern in any longitudinal data. Examining the NLSY97 sample in Appendix Table A.1 we find minimal change in the time-invariant victimization rates over the life-cycle as response rates vary for individuals: rising from 26.5% at age 20 to only about 29.2% by age 40. Our main analysis employs all available observations at each age milestone from a base sample of 6,259 individuals that both $BV_i^C$ and $BV_i^A$ and for whom all the base covariates in Table 1 are available.[10]

## 2.2 Observable early-life characteristics

The richness of the data allows us to account for inherent differences between bully victims and their non-victimized counterparts. Basic demographics in the vector $\boldsymbol{D}$ include (female) sex, birth year and quarter, ethnicity (white or non-white) US citizenship in 1997 and dichotomous variables for urban/rural/uncertain location and Northeast, North Central, South and West census districts. Summary statistics are presented in Table 1.

Childhood health, physical and psychological characteristics all contribute to the propensity to be bullied

[7] 141 respondents answered the first question, 648 answered the second question and only 10 respondents answered neither.

[8] Just over 5% of respondents were bullied during both childhood and adolescence. We do not present results here for this group because N is small, however, they are available upon request. Means for the full NSLY97 sample (not restricted by covariates) are similar: $\bar{BV}_i^C = 0.198$ ; $\bar{BV}_i^A = 0.118$; $\bar{BV}_i^Y = 0.262$.

[9] Bullying is defined as a repeated action that occurs regularly over time (Olweus, 1993).

[10] It will turn out that our findings are robust in a subsample who are observed at all 5-year age milestones from 20-35.

(Lowenstein, 1978; McNamara and McNamara, 1997; O'Moore and Kirkham, 2001; Nansel et al., 2001). The NLSY97 collected several related measures, including reported height, sensory limitation and physical deformity indicators and parental reports of (i) ever having experienced physical/emotional limitations affecting school attendance and (ii) having experienced emotional or learning limitations that affecting school performance. Neighborhood and home environments are also robust predictors of bully victimization (e.g Bowes et al., 2009), which can be captured by indicators for neighborhood gangs, home burglary, shooting witness and theft at school. We also create an initial-period gross household income measure since wealthier families may have more resources to better shelter their children from bullying.[11] School environments are also emphasized by psychologists (Radford et al., 2013; Lereya et al., 2015, for example), and these can be represented through variables for the number of suspensions and school moves, separately for grades $0-6$ (approx. ages $5-12$) and grades $7-12$ (approx. ages $13-18$).[12] Together, these early life health, physical, psychological and environmental covariates comprise the vector $\boldsymbol{Z}$. An analysis in Appendix Section B shows that most early life covariates $\boldsymbol{Z}$ and time-invariant demographics $\boldsymbol{D}$ are robust predictors of bullying during childhood, adolescence or either.

### 2.3 Career Outcomes

Each round of the NLSY97 data includes a comprehensive suite of labor market outcomes. Our main results examine annual employment earnings, hours worked, a labor force participation indicator and cumulative weeks of labor market experience.[13] We supplement these with main job outcomes: the hourly wage, subjective job satisfaction scaled 1(low) to 5(high), and weeks of job tenure.[14] We also convert main-job occupation codes to Autor and Dorn (2013) abstract task scores, a measure of job complexity.

We create age-specific outcomes to address lifecycle bias, a known issue when assessing career outcomes (Bhuller et al., 2011, for example). For each age we determine the correct survey round using reported age at interview. We then smooth across this round and the two adjacent. Advantages of this approach include reducing noise due to a-typical life events in a particular year and providing approximated outcomes when age milestones are reached during non-survey years.[15] NLSY97 surveys were biennial after 2011. Main job characteristics are collected only during survey years while annual summary variables lack these gaps due to retrospective employment history modules. Smoothed continuous outcomes are a weighted average using a triangular kernel centered on the actual year and dichotomous outcomes are smoothed by capturing the

[11] Due to low response rates we use either 1997 or 1998 reports of household gross income deflated to 1997 USD, whichever is available. When both are reported we average the two.

[12] These variables are created from schooling history files. Both Class sizes and private school enrollment were also considered. Sample sizes are small but neither predict victimization. Suspensions may proxy for behavioral concerns (Powell and Ladd, 2010) and school moves or repeated grates may reflect parental interventions to move children away from bullies or forced entry into a new social group as an outsider.

[13] Annual income is reported for the prior year. Labor force participants are identified from weekly employment status arrays.

[14] Respondents may hold more than 10 jobs in some survey rounds. A main job variable, available in each survey round, contains a wave-specific roster number for the primary employer that can be matched to the UID (unique employer identification) variable, which contains an employer identifier for each respondent that is stable across rounds. This UID allows us to pull forward employer and employer-specific characteristics reported in the first wave when an employer is reported or thereafter only if there is a change.

[15] See Bhalla (1980) for a discussion of the early literature and methods for measuring permanent income.

maximum value across the 3 years. [16] Summary statistics for the main outcomes at 5-year age intervals are presented in Appendix Table A.2.

# 3 Results

## 3.1 Baseline victimization penalties

We first document baseline victimization penalties over the life-cycle, across our broad suite of career outcomes. Penalties we measure at each age $19-40$ represent mean differentials between those who do and those who do not report bully victimization status aged $< 18$ years. We derive these from OLS estimates of $\theta_b$ in equation (1)

$$Y_i^a = \eta + \theta_b BV_i^Y + \boldsymbol{D}_i\lambda' + \xi UR_i^a + u_i \tag{1}$$

where each outcome $Y_i^a$ for individual $i$ at age $a$ is also conditioned on the annual unemployment rate $UR_i^a$ that individual experienced at the same age and time-invariant demographics $\boldsymbol{D}_i$. An estimate of less than zero reflects a loss for bully victims relative to their non-bullied counterparts. Our estimates employ custom sample weights and adjust standard errors for survey design with available clustering and stratification variables. We restrict to individuals that are labor force participants, except when estimating cumulative job experience or labor force participation itself. Estimates of $\theta_b$ over the life-cycle are plotted in Figure 1 with corresponding 95% confidence intervals.

Substantial earnings penalties are found at almost all ages. The annual earnings penalty rises from 10% at age 24 to about 15% through age 39 while hourly pay follows a similar pattern, increasing from 5% at age 19. Measures of labour supply tell a more nuanced story. Bully victimization does not appear to affect the propensity to supply labor: point estimates for labor force participation are almost zero for all years up to age 34. Instead, at the intensive margin, there is a penalty of about 75 to 130 hours of work per year during ages $23-35$. These reduced hours coincide with a growing wedge in cumulative weeks of experience that measures half of a calendar year by age 30.

We also examine subjective job satisfaction, weeks of job tenure, and the abstract task content among those employed. Workers with a history of bully victimization during youth report significantly lower average job satisfaction scores and significantly less job duration when observed throughout their early and mid-careers. Taken together we see that bully victims are less satisfied overall in their main jobs and have careers characterized by less job stability. Lower paying jobs among bully victims may be one reason for these patterns. Another reason may be that bully victims hold different types of occupations. From ages $23-37$ bully victims report occupations characterized by significantly lower abstract task scores, suggesting these shorter less-satisfying jobs are also less-complex.

In Appendix Tables A.3$-$A.6 we undertake some robustness exercises on our baseline estimates at age

[16] If all 3 years are reported, kernel weights are 1/4, 1/2 and 1/4, respectively. If only adjacent years are reported, each gets weight 1/2. If the survey year and one adjacent year are reported, the survey year gets 2/3rds weight. If only one of the 3 years is reported, it is given unit weight.

milestones 20, 25, 30, 35 and 40. First we show that results are quite similar when we do not use smoothed outcomes. This is particularly true results based on the main job such as hourly wage, even if these outcomes are reported every other year after 2011. We then assess whether selective nonresponse may affect our results by restricting estimates, separately for each outcome, to a consistent cohort with data available at each 5-year interval aged 20−35. These results are also broadly similar with no observable patterns in the differences between point estimates.

Results for several additional outcomes are available in Appendix table A.2. These include an index for main-job non-wage benefits,[17] main-job weekly hours worked, and cumulative number of jobs since age 14.[18] We also evaluate percentile rank in the national distributions for annual earnings, weekly hours worked and abstract task content using percentile cutoffs that we generate from the annual American Community Survey data for the years 2000−2021. Results suggest penalties in weekly hours worked on the main job, the extent of non-wage benefits and lower positions in the national distribution of earnings, weekly hours worked and abstract tasks, while also contributing to a narrative of higher job turnover by suggesting a greater cumulative number of jobs held but lower cumulative labour market experience. Histograms of broad occupational category differences in Appendix Figure A.3 supplement the task complexity measure showing that bully victims are less likely to hold management or professional occupations beyond age 25 and more likely to be in services or production jobs.

All-together, these results suggest that, when accounting for basic demographics, location, economic conditions, bully victims have markedly less positive career experiences from their early twenties onward. They earn less and work less, at jobs which they like less and which are less complex, and they leave those jobs sooner resulting in lower cumulative experience through mid career. These results are broadly similar to those found in the literature on crime victimization: Bindler and Ketel (2022) find wage penalties of 8−13% lasting about four years for adult victims in their 30's. The duration and onset of penalties presented here differ as might be expected, given that the victimization from bullying during youth, which is both a period of development and also pre-labor market.

In the next section we turn to examining observable mechanisms that might explain baseline victimization penalties and which offer opportunities for policymakers to intervene.

### 3.2 Mechanisms

Dynamic complementarities described by Cunha and Heckman (2007) suggest that intermediate relationships may be important components of life-cycle bully victimization penalties if bullying is also a negative shock to investment in human capital, broadly defined. In this section we outline three intermediate outcomes that may partly explain victimization penalties: educational attainment, health and fertility. Each can

[17] The index in generated using principal component analysis to aggregate up the observed benefits: for all years we observe medical, pension, tuition waivers and stock options and for earlier waves only, which align with age milestones 20−30 in our data, we also observe life insurance, dental, paid maternity leave and childcare. We exclude unpaid maternity leave and flexible hours.

[18] We sum together the cumulative weeks worked as an employee from ages 14−19 and subsequent cumulative weeks worked at all jobs prior to the survey year at age from job history arrays. Our measure excludes informal work aged <19 such as neighborhood lawn cutting for cash.

be observed across the full age range 19−40 in our data and each may contribute to the outcome differences documented above. These mediating factors observed in later life can be considered part-and-parcel of the career outcomes. Documenting the role of intermediate outcomes within the scope of policymaking is an important exercise if society hopes to reduce the harms of this relatively common childhood experience.

Educational attainment has a well understood positive impact on labor market outcomes that we need not summarize here. However, a separate literature has also established that bully victims attain less education during youth (Brown and Taylor, 2008; Sarzosa and Urzúa, 2021; Eriksen et al., 2014; Ponzo, 2013). We capture educational attainment by constructing three age-specific indicators for completion of high school (HS) or GED, an undergraduate degree (associates or bachelors), and a graduate/professional degree.[19] Separate education milestones may be more informative than a continuous measure of years completed due to the "sheepskin effects" of degree completion (Heckman et al., 2006). Figure 2 shows education penalties among bully victims estimated using our baseline specification from equation (1). About 4−5% fewer victim respondents report HS or GED attainment at all ages 19−40. Post-secondary graduation rates drop by age 22 and remain 7−8% lower through age 40, which is considerable since only 45% of respondents obtain this education level by age 35. A reduced propensity to hold a graduate or professional degree appears by age 26.

Poor health has clear implications for labor market performance (Lundborg et al., 2014) and there is ample evidence of increased health problems among bully victims. In terms of mental health, Antila et al. (2017) and Idsoe et al. (2012) find increased anxiety and depression among bully victims. Negative physical health implications include increased illness among school-aged children (Wolke et al., 2001), adolescents (Rigby, 2001), and increased serious illness diagnoses later in life (Wolke et al., 2013). The most consistently reported health measure in the NLSY97 is also the most comprehensive: self-reported overall health.[20] In each survey round, respondents were asked "In general, how is your health?". Responses range from (1) "poor" to (5) "Excellent". We smooth this measure by averaging across adjacent rounds resulting in a continuous measure that proxies for health at a given age. Figure 2 suggests a stable overall health penalty for bully victims at all observed ages.

Life-cycle models including Adda et al. (2017) illustrate considerable career costs of fertility decisions, including lower wages. Labor market performance, then, may be associated with bully victimization if fertility affects both. We construct a dichotomous fertility event variable using separate questions asked to male and female respondents in each survey round: whether a female respondent reports a pregnancy or whether a male respondent reports fathering a child. The final panel of Figure 2 suggests that victimization may affect the timing of fertility decisions: there is some evidence of increased fertility at ages 19−23 and decreased fertility at ages 26−30. While there are strong reasons to suspect this outcome has different effects across the sexes, including differences in source NLSY97 questions, results in Appendix Figure A.4 show that statistically different patterns in fertility are visible only at ages 19 and 20.

[19] Source variables are highest degree ever obtained (rounds 2010 onward) and highest degree obtained in the survey year (earlier rounds). As with other dichotomous outcomes, smoothed education milestones use the maximum value across adjacent years.

[20] Several specific mental health questions were asked irregularly to limited subsamples of NLSY97 respondents. Results available upon request show that the principal component from 5 questions relating to one specific mental health concern (depression) was persistently higher among those who report bullying during youth relative to those that do not.

### 3.2.1 Regression decompositions

In this section we decompose the measured victimization penalties in order to establish the relative importance of these intermediating life-cycle mechanisms: educational attainment ($\boldsymbol{E^a}$), self-reported overall health ($H^a$), and fertility events ($F^a$). Gelbach (2016) provides a simple method that we use to detail the separate conditional effects of these covariate groups.[21] Unlike sequential regressions with additional covariates, covariate ordering does not matter because this methodology is based on a single comparison of a base (equation 1) and a full specification (equation 2). This advantage is particularly important for the task at hand because relationships between various mechanisms are likely. The decomposition employs the bias formula for omitting some variable $k$ in an OLS regression: $\hat{\delta}_k = \hat{\Gamma}_k \hat{\beta}_k$, where $\hat{\beta}_k$ is the full model coefficient and $\hat{\Gamma}_k$ are covariate correlation estimates obtained by regression of variable $k$ on the base covariates.[22] These "bias" terms $\hat{\delta}$ contain the variation to be partialled out of the full model by including each covariate (or covariate group).

$$Y_i^a = \tilde{\eta} + \theta_f BV_i^Y + \boldsymbol{D}_i\tilde{\lambda}' + \boldsymbol{Z}_i\phi' + \boldsymbol{E_i^a}\kappa' + \pi H_i^a + \psi F_i^a + \tilde{\xi} UR_i^a + \tilde{u}_i \tag{2}$$

For a given outcome $Y_i$ at age $a$, we estimate both specifications with heteroskedasticity-robust inference and restrict to a common estimation sample. In Table 2 we present results for our main outcomes in separate panels A−G, one for each outcome. To keep results manageable, we provide estimates only at 5 year intervals (ages 20, 25, 30, 35 and 40). The first two rows of each panel summarize the effect of covariates: coefficients $\theta_b$ and $\theta_f$ represent estimated victimization penalties without (base specification) and with all covariates (full specification), respectively.[23] Thus, $\theta_b$ represents the baseline earnings penalty presented previously after adjusting for any missing values for the intermediate outcomes to be analyzed. The estimated victimization penalty $\theta_f$ represents the unexplained portion of the victimization penalty, which is not due to any observable factors considered here, while each $\hat{\delta}$ represents the corresponding explained portion.

When interpreting the results below it is important to consider that earlier age milestones 20 and 25 ought to be somewhat different from later counterparts because those with more educational attainment are often not in full time work at these ages due to school enrollment. Early-vs-mid career differences are noticeable particularly for earnings measures (panels A and B) as well as annual hours worked (panel E). In light of the evidence for reduced educational attainment, for these years bully victims might be expected to have lower earnings and higher labour supply only because of earlier entry into the labor market.

Education is a dominant mechanism in our data behind earnings penalties and abstract task (occupational make-up) differences particularly after age 25. This factor alone, captured by $\hat{\delta}_E$ tends to explain about 20% of the baseline victimization penalty $\hat{\theta}_b$ in terms of hourly pay by age 30, with rising importance later in life. In terms of occupational composition, education is clearly *the* dominant factor. The relationship that bullying has with lowered educational attainment also plays out in terms of labour supply annually and in the long

[21] The Gelbach (2016) procedure nests the Oaxaca-Blinder decomposition method and does not employ sample weights.
[22] Individual covariate terms can be summed to form groups of covariates because of linearity. See Gelbach (2016) for full details.
[23] Estimates of $\theta_b$ may not exactly match those from Figure 1 because here the sample is restricted to non-missing covariates.

term, explaining a non-trivial amount of the baseline penalty in hours worked per year and cumulative weeks of experience. Instead, education is not an intermediating factor in terms of job satisfaction (at least beyond age 25) nor job tenure. This suggests that any increased job turnover among victims due to dissatisfaction might not be mediated by policies aimed to keep bully victims in school longer.

Estimates of $\hat{\delta}_H$ suggest that health is a less-important but non-trivial moderating factor for earnings penalties, and for annual hours worked, explaining about half as much as education at most ages. In terms of cumulative work experience, health and education explain similar amounts. Instead, health is the only consistently important predictor of job satisfaction and job tenure penalties. This finding implies that a considerable component of any increased propensity to churn through jobs due to bullying is explained by subjective health, which can be expected to capture mental health difficulties that arise partly due to bullying.

Fertility has a negligible effect, conditional on other mechanisms. Though $\hat{\delta}_F$ is significant for some outcome measures in the early to mid-thirties, point estimates are sufficiently small to suggest a very minimal direct impact. Therefore we conclude that fertility decisions are not an important mechanism through which bully victimization leads to career penalties.

Early-life observable covariates have the most robust effects on our estimates of hourly wages (Panel B), job satisfaction (Panel C) and job tenure (Panel F), though there are also statistically significant differences between $\hat{\theta}_b$ and $\hat{\theta}_f$ at ages 25 and 30 for the abstract task index (Panel D) and annual hours worked (panel E) as well as ages 30 and 40 for log annual earnings (Panel A). Together, fertility, health and education can explain at most about half of the bully victimization penalties in our data after conditioning on early life conditions $\boldsymbol{Z}$. The exception, in Panel H, is that the covariates explain most, if not all, of the lifetime experience penalty. In our results, these early life factors are dominant determinants of the magnitude of the bully victimization penalties on early life labour supply (ages 20-30) and both early and mid-career, job tenure and cumulative work experience, explaining almost half of the baseline penalty on their own.

Perhaps the most important take-away from these results is what can and cannot be explained by the sum of these mechanisms. Glancing across panels of Table 2 the estimate $\hat{\theta}_f$ indicates a non-trivial yet-unexplained victimization penalty in all cases except for occupational complexity and cumulative experience. At least half of the earnings, tenure and job satisfaction penalties remain. An important result of this exercise, then, is to illustrate that while bully victimization does affect earnings later in life through its influence on educational attainment, unobserved factors dominate in terms of explaining earnings penalties.

### 3.2.2 Non-cognitive skills

Non-cognitive skill has been recognized as a component of human capital with career implications (Cunha and Heckman, 2007) and a moderating role for non-cognitive skill in bully victimization has been detailed in Sarzosa (2021). Three NLSY97 modules (round 6, 12&14 and 16&17) contain personality questionnaires similar to those typically used to assess non-cognitive skill.[24] Question concepts differ across rounds and

[24]Measurement is the main challenge in the literature (Almlund et al., 2011). Conscientiousness has the strongest link to labor market outcomes. In addition, Bowles and Gintis (1967) and Heckman and Rubinstein (2001) also highlight the importance of

non-response is high. However each aligns closely to ages 20, 25 and 30 for most respondents providing some scope to assess whether non-cognitive skill explains the remainder of victimization penalties at these 3 ages.[25] Following (Heckman et al., 2006) we measure non-cognitive skill $N_i^a$ as the leading principal component from available questions at each age and find that bully victims score lower on these measures (Appendix table C.10).[26] Decomposition estimates $\hat{\delta}_N$ in Appendix Table A.7 suggest that that $N$ explains modestly (about one-tenth) more of $\hat{\theta}_b$, particularly for weeks of tenure, cumulative experience and job satisfaction. An important caveat is that our measures may not fully capture non-cognitive skill. Though unexplained portions of victimization penalties $\hat{\theta}_f$ remain large for several outcomes, this exercise suggests they do hold some importance in the US, as has been shown in Japan (Mori and Onozuka, 2024) and Korea (Sarzosa and Urzúa, 2021). Further details about non-cognitive measures are found in Appendix Section C.

### 3.3 Heterogeneity

The propensity to be bullied and labor market outcomes both differ across characteristics and circumstances. We here document heterogeneity in our baseline victimization penalties for different timings of victimization, across the sexes and across high/low groups in terms of gross household income and exposure to crime and violence. [27] For brevity we present results for outcomes where meaningful victimization penalties are found for at least one sub-group.[28]

Sex differences are the most evident form of heterogeneity. Plots in Figure 3 show that bully victimization affects women more sharply than men. We observe earnings penalties for both sexes at most ages, but penalties are significantly larger for women than men starting from the mid-20s. Women face larger hours worked and job tenure penalties from their mid 20s onward and women alone robustly experience job complexity penalties. Sex differences in cumulative labor market experience are striking. These discrepancies in outcomes are likely explained by considerably larger penalties in terms of educational attainment (below the graduate level) and overall health, but not fertility (see Appendix Figure A.4).

The psychology literature has documented sex differences victimization frequency and how bullying is experienced. Ex-ante it is difficult to anticipate which sex would experience a stronger impact. On one hand, our data show more frequent reports of victimization among males (Table B.8). On the other hand, differences in the importance of social relations and how bullying is internalized suggest a more negative experience for females (Ledwell and King, 2015). Our findings support the latter rationale, and are similar to later-life British

measures of trustworthiness, tenacity and perseverance.

[25] Round 6 questions assess agreeableness and conscientiousness, ("Big Five" personality traits (Goldberg, 1993)), rounds 12 and 14 assess a subject's industriousness (from the Chernyshenko Conscientiousness Scale) and propensity to follow rules, and rounds 16 & 17 contain questions from "Duckworth's GRIT scale" that assess an individual's ability to persist through difficulty.

[26] We do not discuss a trajectory in such skill since measures are not comparable over time. For a discussion of the importance of comparability see (Attanasio et al., 2020).

[27] We also estimated results separately during periods of unemployment above/below the median rate but found no clear patterns. Exposure to crime and violence is defined as a respondent who answers yes to any of the following questions about life aged <12: presence of neighborhood gangs, was ever stolen from at school, witnessed a shooting or had their house burglarized. Our estimates are based on marginal effect for $BV^Y$ evaluated across any pair of comparison groups with 95% confidence intervals calculated using the delta method.

[28] Results available upon request suggest slightly larger earnings penalties for whites, larger abstract task penalties for those from low income households and larger labor supply penalties for those exposed to crime and violence as children.

results (Brimblecombe et al., 2018) but *opposite* to the findings of Mukerjee (2019) using the same NLSY97 data. Aside from methodological differences, one reason for disparate results may be that Mukerjee (2019) examines responses only in the year 2006 (where the average age is 24), not by life-cycle milestone. Fertility choices alone cannot explain these sex differences, as shown in Section 3.2. However, we cannot rule out the possibility that negative bullying impacts might be exacerbated by within-household specialization favoring dominant social roles for women in the home.

Cunha and Heckman (2007); Knudsen et al. (2006) highlight the importance of sensitive periods for development. Bully victimization during sensitive periods could be internalized to a greater degree leading to a stronger impact. Ponzo (2013) finds bullying during teen years to be most harmful in Italy and Brown and Taylor (2008) finds somewhat more negative outcomes for victims aged 11 years compared to 7 years in the UK. In US data, Nikolaou (2022) finds that the GPA effects up to grade 10 dissipate over time. A strength of the data at hand is the separate bullying information during childhood and adolescence. Figure 4 illustrates that our data also suggest worse outcomes for adolescent victims, though 95% confidence intervals do overlap in most cases. We find larger annual earnings and annual hours worked penalties at many ages and for job tenure during the later 30s. Hourly wage and job tenure penalties are largely similar regardless of victimization timing. Penalties for cumulative labor market experience, and the propensity to participate beyond age 33 also are greater for those victimized during adolescence. None of the intermediating variables we considered are robustly different by time of victimization. Taken together, these results suggest that the labour supply is the important margin across which victimization timing matters most, at least in the relatively flexible US labor market.

Finally we consider differences in early life circumstances with separate estimates for respondents from high/low income households and for respondents who were/were not exposed to crime and violence. Results in Figure 5 show that penalties for most outcomes do not differ much across income lines. However, these estimates suggest that we can only conclude that job tenure penalties are found among those from low income households while it is only for the high-income household group that we can find a penalty in HS/GED completion propensity up to age 39. These latter two results are mirrored in Figure 6, where we can conclude that there are substantial victimization penalties in tenure only for those who *are* exposed to crime and violence and in HS/GED completion only for those who *are not*. Penalties in the rate of high school graduation are somewhat counterintuitive, perhaps because those exposed to other forms of violence and to poverty are already unlikely to graduate secondary school.

### 3.4 Selection into bullying: within-household estimates

While our data are rich in observable characteristics, we cannot claim that our findings so far identify the average causal impact of bully victimization on career outcomes. Bullying is unlikely to be random and may depend on traits that are difficult to measure. In Section 2.2 we describe observable early-life characteristics identified by Psychologists as predictors of victimization. Counterparts in our data, $\boldsymbol{Z}$, predict the same (see Appendix B) and are also likely to affect labor market performance. In addition, unobserved family or genetic factors may correlate with poor labor market outcomes and bully victimization. Recent findings sup-

port the notion that that early-life household-level factors, specifically, are key determinants of victimization propensity: medical literature suggests that victimization runs in families (Allison et al., 2014) and, using British data, (Chrysanthou and Vasilakis, 2020) find that the family environment, and specifically perceived family support, is the most effective way to reduce victimization. Experimental evidence from Cunha et al. (2023) also shows that bullying perpetration *and* victimization are both reduced by household-level interventions targeting parents. Early-life household characteristics are fixed prior to entry into the labor market and therefore warrant separate analysis from the dynamic mechanisms of health human capital and fertility examined above during ages $19-40$.

In this section we present estimates for select outcomes that account for these sources of selection bias. Household identifiers allow us to identify siblings in the NLSY97 data which facilitates the estimation of models with household fixed-effects.[29] An important caveat is that the usable sibling sub-sample is small: about 40% of the NLSY97 respondents have at least one identified sibling, though victimization status varies within only half of these sibling groups. The sample becomes altogether too small ($N < 150$) for estimation at age 40. Restricting ourselves to the remaining 5-year age milestones spanning $20-35$ we estimate three models for each age-outcome combination: baseline estimates, estimates that include $\boldsymbol{Z}$ and finally estimates that include $\boldsymbol{Z}$ plus household fixed-effects.[30] Here we focus on earnings, job satisfaction and overall health because it is for these outcomes that we observe similar patterns in baseline earnings penalties in our reduced sibling sample.

Hourly wage penalties at age 30 and 35 beyond appear to be very robust to estimation within-household, in Panel B) of Table 3. Raw penalties of about 16% at ages 30 and 35 decrease only to 15% and 11% at ages 30 and 35, respectively, when victimization penalties are estimated by comparing a sibling that was bullied to their other sibling(s) that were not. Point estimates of age 30 and 35 annual income penalties also reman close in magnitude with household fixed-effects, though standard errors are too large to draw clear conclusions. At least in terms of earnings, the bully victimization penalties that we document are not due to observable early life characteristics nor due to selection bias resulting from early life parenting methods or sibling-common genetic factors that pre-dispose individuals both to bully victimization and low earnings in later life.

These same selection mechanisms that we rule out for earnings penalties are also unable to explain occupational content and job duration estimates in our sibling sub-sample. The remaining significant penalty for job tenure (at age 30) is largely unchanged though it decreases somewhat in estimates that only control for $\boldsymbol{Z}$. Instead, baseline abstract task penalties that remain statistically significant at ages 25 and 35 are halved and become statistical zeros by the inclusion of early life covariates even prior to our within-sibling estimates. For both of these outcomes, several point estimates increase in magnitude in the final row which suggests that household fixed-effects remove a positive bias that *reduced* the magnitude of estimated penalties, though statistical significance remains out of reach.

[29] Kinship links produced by Beasley et al. (2023), building on the work of Rodgers et al. (2016), allow us to restrict household identifiers to respondent pairs or groups identified either as a full sibling/dizygotic twin ($R = 0.5$) or a monozygotic twins ($R = 1$).

[30] We absorb high-dimensional fixed effects algorithms of Correia (2023) and cluster standard errors at the household level. This clustering is similar to sample-design inference because households are the sampling unit.

The final results in Panels E and F suggest that selection into bullying at the household-level *does* explain a majority of the penalties present in subjective reports in our data. Job satisfaction penalties, which are barely affected by the addition of early life observables, are reduced considerably from ages 25 onward and overall health status penalties are halved, becoming statistically zero at ages 20 and 35. It is particularly interesting to see the sharp attenuation in early-life (age 20) health penalties but not job satisfaction. The resulting health pattern across the life cycle changes from being a level shift (observed in baseline penalties) to a decline and partial recovery. Instead, where results may be affected by enrollment, it is not surprising that job satisfaction penalties remain given that outside option of schooling may affect selection within household at this age. It is also unsurprising to find similar patterns across these two outcomes not only because they are subjective reports but also because we have shown that overall health is an important moderating mechanism for job satisfaction (Section 3.2). Health was also a dominant mechanism behind estimates of annual hours worked penalties and therefore it is probable that household level selection may explain much of this penalty.[31]

Taken together, these results suggest that selection into bullying on unobservable household factors, including cross-sibling genetic factors, parenting style and so forth, may be the reason why bully victims are less content with their jobs and why they report such a large health gap with non-bully victims. Instead, this selection bias is not the driving force behind our earnings penalties. We can therefore conclude that our results are highly suggestive that the earlier-life causal impacts on schooling documented in Korea (Sarzosa and Urzúa, 2021) and Denmark (Eriksen et al., 2014) and the US Nikolaou (2022) extend to longer-term causal impacts on earnings for Americans born in the 1980s.

# 4 Discussion and Conclusions

This paper is one of few analyses to document long-term career consequences of bully victimization, and to the best of our knowledge, the first to do so for a sample covering the United States. By documenting persistent losses across several measures of career success, this paper complements and extends important work from psychology and related fields that has shown bullying to decrease school attainment and lead to psychological and emotional disorders. The breadth of our analysis sheds important light on which aspects of careers are impacted. We are able to illustrate that labor supply consequences are limited to the intensive margin and, by comparing main job and annual total earnings, that multiple job holding does not appear to be a solution for financial penalties.

Our decomposition results reveal an important intermediating role for both health and educational attainment. Both of these factors are themselves affected by bully victimization and yet, both can explain a considerable portion of the victimization penalties in terms of job satisfaction, earnings, labor supply and the nature of occupations obtained. These results suggest that policymakers might be able to alleviate some of the considerable economic consequences of bully victimization that we document by focusing on programs to remediate health consequences and facilitate continued progress in education.

[31] Estimates of this outcome and for cumulative work experience are noisy and unstable in our sibling sub-sample relative to the full sample. Thus we do not present them but they remain available upon request.

Our results also lend some support to models that suggest non-cognitive skills are an important reason why bullying has long-term effects. Data limitations prevent us from drawing strong conclusions about the relative importance of this form of human capital. Evidence that bullying shapes attitudes toward economic policy (Yamagishi, 2020) suggests that an expanded understanding of the definition and measurement of non-cognitive skill will be an important avenue of further research in the discipline. Impacts on socio-emotional factors such as motivation and the ability to foster working relationships are increasingly recognized as important by economists.

One interesting finding is that bullying appears to impact careers differently depending on the life-cycle timing of victimization. Those bullied during childhood may experience milder wage penalties than those bullied during adolescence. It appears that adolescence may be a particularly sensitive period during which to experience this sort of negative shock, at least as it pertains to one's career trajectory.

Sex differences revealed in our results are considerable and support psychology research suggesting that male and female victims internalize the trauma of bullying differently. An interesting side result is that fertility does not appear to be differentially impacted by victimization across the sexes and also holds a very minor role as an explanation for any career consequences. Cobb-Clark and Tan (2011) have found that non-cognitive skills matter more for women's wages than men's careers, suggesting that the moderation of the non-cognitive skills (which we struggle to measure) may have larger wage implications on bully victimization outcomes for women than for men.

Finally, we would be remiss not to highlight the fact that our results reveal substantive penalties in almost all outcomes *even conditional* on measures of education, fertility, health as well as a swath of personal traits and childhood environmental factors. This finding suggests that programs to help the victims of bullying get back on track at school might be less efficacious than policies and procedures aimed at preventing bullying in the first place. In this light, our results lend support to the findings from Cunha et al. (2023) suggesting that household-level interventions aimed at parenting may provide important in-roads.

As data become available this research can be extended further into late careers and examine the impact on household savings, fertility and retirement. It may be that the consequences of the long shadow cast by bullying reach even beyond a worker's career.

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Table 1: Summary Statistics for estimation sample

| *Bully Victim indicators* | Mean | SD |
|---|---|---|
| $BV_i^Y$ (aged <18) | 0.265 | 0.441 |
| $BV_i^C$ (aged <12) | 0.204 | 0.403 |
| $BV_i^A$ (aged 12−18) | 0.118 | 0.322 |
| | | |
| *Demographics* ($\boldsymbol{D}$) | Mean | SD |
| female | 0.485 | 0.500 |
| born qtr 2 | 0.236 | 0.424 |
| born qtr 3 | 0.261 | 0.439 |
| born qtr 4 | 0.257 | 0.437 |
| white | 0.745 | 0.436 |
| region north-central | 0.284 | 0.451 |
| region south | 0.350 | 0.477 |
| region west | 0.186 | 0.389 |
| born 1981 | 0.199 | 0.399 |
| born 1982 | 0.208 | 0.406 |
| born 1893 | 0.195 | 0.396 |
| born 1984 | 0.196 | 0.397 |
| urban residence | 0.658 | 0.474 |
| unknown residence | 0.050 | 0.217 |
| not US born | 0.975 | 0.157 |

| *Innate traits* ($\boldsymbol{Z}_1$) | Mean | SD |
|---|---|---|
| height '97 | 65.244 | 4.310 |
| phys deformity '97 | 0.017 | 0.128 |
| sensory limit '97 | 0.180 | 0.384 |
| phys/emo limit attend '97 | 0.077 | 0.267 |
| learn/emo limit perf '97 | 0.110 | 0.313 |
| | | |
| *Child environment* ($\boldsymbol{Z}_2$) | Mean | SD |
| neighborhood gangs <12 | 0.411 | 0.492 |
| witness shooting <12 | 0.083 | 0.276 |
| home burglarized<12 | 0.140 | 0.347 |
| school theft victim <12 | 0.234 | 0.423 |
| hh gross inc '97/98 ($1,000) | 49.94 | 48.38 |
| | | |
| *School history* ($\boldsymbol{Z}_3$) | Mean | SD |
| school changes grade 1-6 | 0.033 | 0.223 |
| suspensions grade 1-6 | 0.791 | 4.931 |
| year repeats grade 1-6 | 0.118 | 0.353 |
| school changes grade 7-12 | 1.418 | 1.133 |
| suspensions grade 7-12 | 4.650 | 18.309 |
| year repeats grade 7-12 | 0.223 | 0.560 |

Sample means for estimation sample from the NLSY97 using custom survey weights. $N = 6,259$. Omitted group variables include: northwest census region, 1980 birth year, rural residence, US born citizen, non-whites. To overcome high nonresponse of Initial household income, we use real income in either 1997 or 1998 as available, and average the two where both are reported.

Table 2: Gelbach Decompositions

*Panel A) ln annual income*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | -0.0003 | -0.0898*** | -0.0895*** | -0.0515** | -0.1084*** |
| $\hat{\theta}_b$ | -0.0122 | -0.1154*** | -0.1835*** | -0.0974*** | -0.2082*** |
| $\hat{\delta}_Z$ | -0.0017 | -0.006 | -0.0476*** | -0.0038 | -0.0381*** |
| $\hat{\delta}_E$ | -0.0025 | -0.0007 | -0.0286*** | -0.0289*** | -0.0351*** |
| $\hat{\delta}_H$ | -0.01*** | -0.0183*** | -0.0151*** | -0.0121*** | -0.027*** |
| $\hat{\delta}_F$ | 0.0024 | -0.0005 | -0.0027*** | -0.0011 | 0.0004 |
| N | 4075 | 4401 | 4165 | 2819 | 1445 |

*Panel B) ln hourly pay (main job)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | -0.0423*** | -0.0556*** | -0.0664*** | -0.0565*** | -0.0712*** |
| $\hat{\theta}_b$ | -0.0483*** | -0.0971*** | -0.1278*** | -0.1143*** | -0.1383*** |
| $\hat{\delta}_Z$ | -0.0014 | -0.0151*** | -0.022*** | -0.0186*** | -0.0211*** |
| $\hat{\delta}_E$ | -0.0009* | -0.0139*** | -0.0246*** | -0.0278*** | -0.0307*** |
| $\hat{\delta}_H$ | -0.0037*** | -0.0123*** | -0.0129*** | -0.009*** | -0.016*** |
| $\hat{\delta}_F$ | 0 | -0.0002 | -0.0018*** | -0.0024*** | 0.0006 |
| N | 4887 | 4918 | 4564 | 3257 | 1750 |

*Panel C) subj. job satisfaction (main job)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | -0.0044 | -0.0595*** | -0.0901*** | -0.1467*** | -0.145*** |
| $\hat{\theta}_b$ | -0.0435** | -0.1013*** | -0.1299*** | -0.1706*** | -0.1697*** |
| $\hat{\delta}_Z$ | -0.0061 | -0.0037 | -0.0089* | -0.0013 | 0.0054 |
| $\hat{\delta}_E$ | 0.0001 | -0.0031** | -0.0001 | 0.0006 | -0.0003 |
| $\hat{\delta}_H$ | -0.0332*** | -0.0349*** | -0.0295*** | -0.0223*** | -0.03*** |
| $\hat{\delta}_F$ | 0 | -0.0001 | -0.0013 | -0.0009 | 0.0001 |
| N | 4393 | 4765 | 4323 | 3027 | 1664 |

*Panel D) abstract task index (main job)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | 0.0698*** | -0.0688** | 0.0196 | -0.0738* | 0.0402 |
| $\hat{\theta}_b$ | 0.032* | -0.2177*** | -0.1679*** | -0.2322*** | -0.1508** |
| $\hat{\delta}_Z$ | -0.0173*** | -0.0364*** | -0.0411*** | -0.0348** | -0.0397** |
| $\hat{\delta}_E$ | -0.0036 | -0.0987*** | -0.1235*** | -0.1035*** | -0.1289*** |
| $\hat{\delta}_H$ | -0.0156*** | -0.0143*** | -0.0214*** | -0.0121*** | -0.0222*** |
| $\hat{\delta}_F$ | -0.0012 | 0.0005 | -0.0016 | -0.008*** | -0.0002 |
| N | 4890 | 4923 | 4590 | 3289 | 1777 |

*Panel E) annual hours worked)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | -5.1456 | -37.9414** | -87.1995*** | -55.4016*** | -67.3444** |
| $\hat{\theta}_b$ | 10.8435 | -85.1381*** | -138.7755*** | -80.1333*** | -86.2795*** |
| $\hat{\delta}_Z$ | 14.3066*** | -24.8583*** | -14.8793*** | 0.7201 | 0.1043 |
| $\hat{\delta}_E$ | -2.6773* | -10.176*** | -25.0877*** | -14.2605*** | -8.3288* |
| $\hat{\delta}_H$ | 4.0671** | -12.2897*** | -13.3261*** | -13.6535*** | -10.8441*** |
| $\hat{\delta}_F$ | 0.2927 | 0.1272 | 1.7171** | 2.4622** | 0.1334 |
| N | 4982 | 5043 | 4787 | 3404 | 1809 |

*Panel F) weeks of tenure (main job)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | -1.4114* | -3.5726** | -15.4094*** | -16.4719*** | -40.0841*** |
| $\hat{\theta}_b$ | -3.7524*** | -6.986*** | -22.6825*** | -27.7413*** | -57.5099*** |
| $\hat{\delta}_Z$ | -1.6196*** | -3.4762*** | -6.9212*** | -8.6252*** | -12.7601*** |
| $\hat{\delta}_E$ | -0.1047** | 0.9016*** | 0.4801 | 0.5774 | -1.4048 |
| $\hat{\delta}_H$ | -0.5922*** | -0.8332*** | -0.7082** | -2.7432*** | -3.0165*** |
| $\hat{\delta}_F$ | -0.0245 | -0.0056 | -0.1239 | -0.4784** | -0.2444 |
| N | 4880 | 4918 | 4577 | 3289 | 3707 |

*Panel G) P(labor force participant)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | -0.0037 | 0.0055** | 0.009** | 0.0228*** | -0.009 |
| $\hat{\theta}_b$ | -0.0036 | 0.0035 | 0.0017 | 0.0141*** | -0.0217*** |
| $\hat{\delta}_Z$ | -0.0006 | -0.0014 | -0.0022* | -0.0021 | -0.0033 |
| $\hat{\delta}_E$ | -0.0001 | -0.0015*** | -0.0027*** | -0.0026*** | 0.0004 |
| $\hat{\delta}_H$ | 0.0007* | 0.0009* | -0.0028*** | -0.004*** | -0.0094*** |
| $\hat{\delta}_F$ | 0 | 0 | 0.0004** | 0 | -0.0004 |
| N | 5113 | 5253 | 5152 | 3707 | 2027 |

*Panel H) cumulative weeks of labor market experience)*

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| $\hat{\theta}_f$ | 2.909** | 2.6253 | -5.296* | -4.6367 | 19.5218** |
| $\hat{\theta}_b$ | 0.328 | -5.1841** | -18.2271*** | -19.3541*** | -9.5906 |
| $\hat{\delta}_Z$ | -1.4253*** | -4.8936*** | -7.7934*** | -7.8572*** | -20.5591*** |
| $\hat{\delta}_E$ | -0.5183** | -0.7715* | -2.9471*** | -3.5379*** | -3.1731 |
| $\hat{\delta}_H$ | -0.6122*** | -2.1587*** | -2.2552*** | -3.2059*** | -4.9456*** |
| $\hat{\delta}_F$ | -0.0251 | 0.0145 | 0.0645 | -0.1164 | -0.4346 |
| N | 4696 | 4554 | 4228 | 2971 | 1530 |

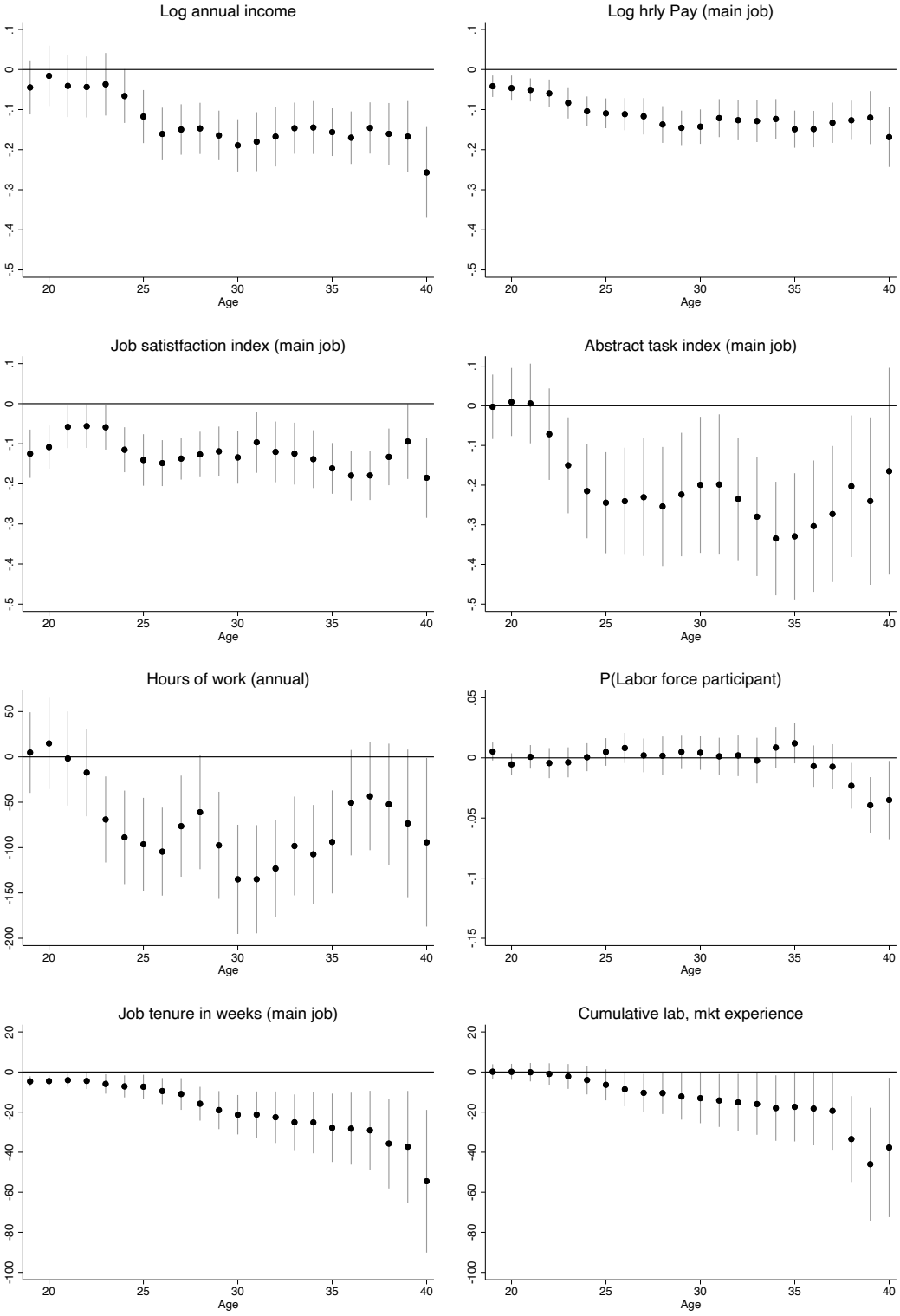
Covariate decompositions using methods proposed by Gelbach (2016) with robust standard errors. $\hat{\theta}_b$ and $\hat{\theta}_f$ are bully victimization penalties in base and full specifications. Estimates $\hat{\delta}_g$ are the decomposed components for early life covariates, education milestones, overall health and fertility.

Table 3: Estimates with and without household (sibling) fixed effects

| | *Panel A) ln annual income* | | | | *Panel B) ln hourly wage* | | | |
|---|---|---|---|---|---|---|---|---|
| | aged 20 | aged 25 | aged 30 | aged 35 | aged 20 | aged 25 | aged 30 | aged 35 |
| base | 0.021 | -0.032 | -0.179** | -0.145** | -0.034 | -0.054 | -0.156*** | -0.155*** |
| | (0.067) | (0.076) | (0.077) | (0.064) | (0.028) | (0.035) | (0.056) | (0.055) |
| $+Z$ | -0.009 | -0.024 | -0.127 | -0.101 | -0.030 | -0.012 | -0.113** | -0.109** |
| | (0.070) | (0.079) | (0.081) | (0.067) | (0.027) | (0.034) | (0.054) | (0.046) |
| + hh FE | -0.032 | -0.070 | -0.153 | -0.123 | -0.006 | 0.011 | -0.147** | -0.107* |
| | (0.077) | (0.080) | (0.096) | (0.099) | (0.030) | (0.042) | (0.066) | (0.055) |
| N | 1,529 | 1,540 | 1,235 | 1,063 | 2,013 | 1,801 | 1,465 | 1,270 |
| | *Panel C) abstract task index* | | | | *Panel D) job tenure (weeks)* | | | |
| | aged 20 | aged 25 | aged 30 | aged 35 | aged 20 | aged 25 | aged 30 | aged 35 |
| base | -0.132 | -0.274** | -0.110 | -0.304* | -4.426 | -3.818 | -33.10*** | -20.17 |
| | (0.084) | (0.110) | (0.162) | (0.177) | (2.865) | (6.549) | (11.62) | (19.47) |
| $+Z$ | -0.068 | -0.118 | -0.010 | -0.171) | -2.652 | -0.904 | -26.97** | -2.743 |
| | (0.085) | (0.106) | (0.153) | (0.172) | (2.892) | (6.999) | (11.66) | (18.95) |
| + hh FE | -0.156 | -0.095 | -0.181 | -0.141 | -2.491 | -9.407 | -34.47** | -13.50 |
| | (0.105) | (0.141) | (0.195) | (0.217) | (4.436) | (7.471) | (14.85) | (23.07) |
| N | 2,020 | 1,792 | 1,492 | 1,306 | 2,006 | 1,797 | 1,480 | 1,297 |
| | *Panel E) subj. job satisfaction* | | | | *Panel F) subj. overall health* | | | |
| | aged 20 | aged 25 | aged 30 | aged 35 | aged 20 | aged 25 | aged 30 | aged 35 |
| base | -0.136** | -0.167*** | -0.055 | -0.169** | -0.209*** | -0.241*** | -0.254*** | -0.169*** |
| | (0.060) | (0.058) | (0.073) | (0.067) | (0.048) | (0.049) | (0.047) | (0.056) |
| $+Z$ | -0.113* | -0.161*** | -0.048 | -0.199*** | -0.157*** | -0.189*** | -0.189*** | -0.137** |
| | (0.061) | (0.057) | (0.074) | (0.064) | (0.046) | (0.050) | (0.048) | (0.056) |
| + hh FE | -0.169** | -0.037 | 0.078 | -0.113 | -0.067 | -0.145** | -0.126* | -0.104 |
| | (0.073) | (0.074) | (0.088) | (0.098) | (0.054) | (0.059) | (0.069) | (0.082) |
| N | 1,681 | 1,702 | 1,341 | 1,092 | 2,219 | 2,036 | 1,866 | 1,673 |

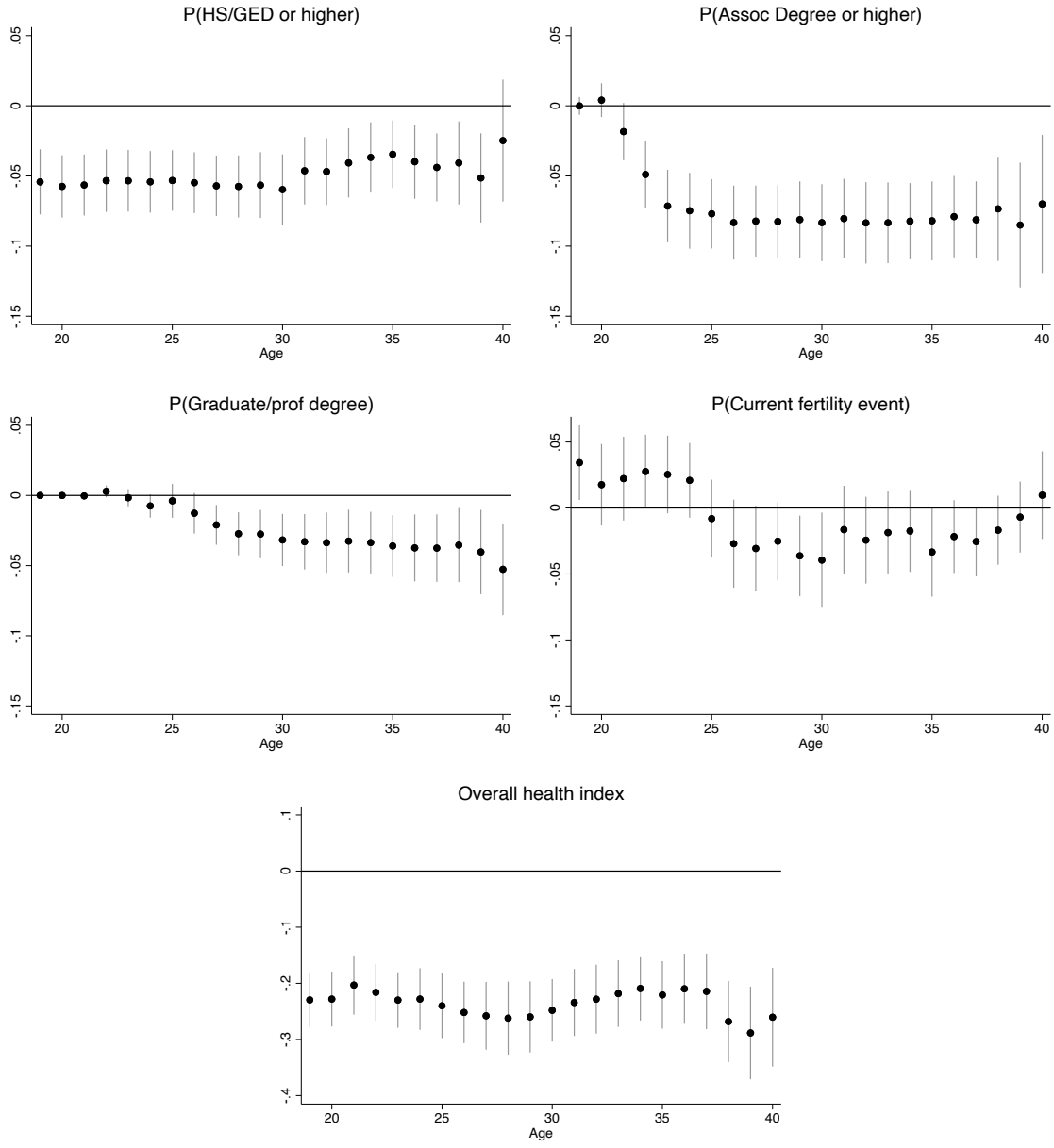
Author estimates of bully victimization penalties using observations from base sample with a respondent sibling. Baseline estimates are OLS estimates with robust standard errors based on survey design and custom survey weights that control for basic demographics $\boldsymbol{D}$ and the unemployment rate. Specifications with "$+Z$" include controls for early-life covariates $\boldsymbol{Z}$ and "+hh FE" estimates include Correia, 2014 estimators that also absorb household fixed effects with standard errors clustered at the household level.

Figure 1: Baseline victimization penalties for various labor market outcomes



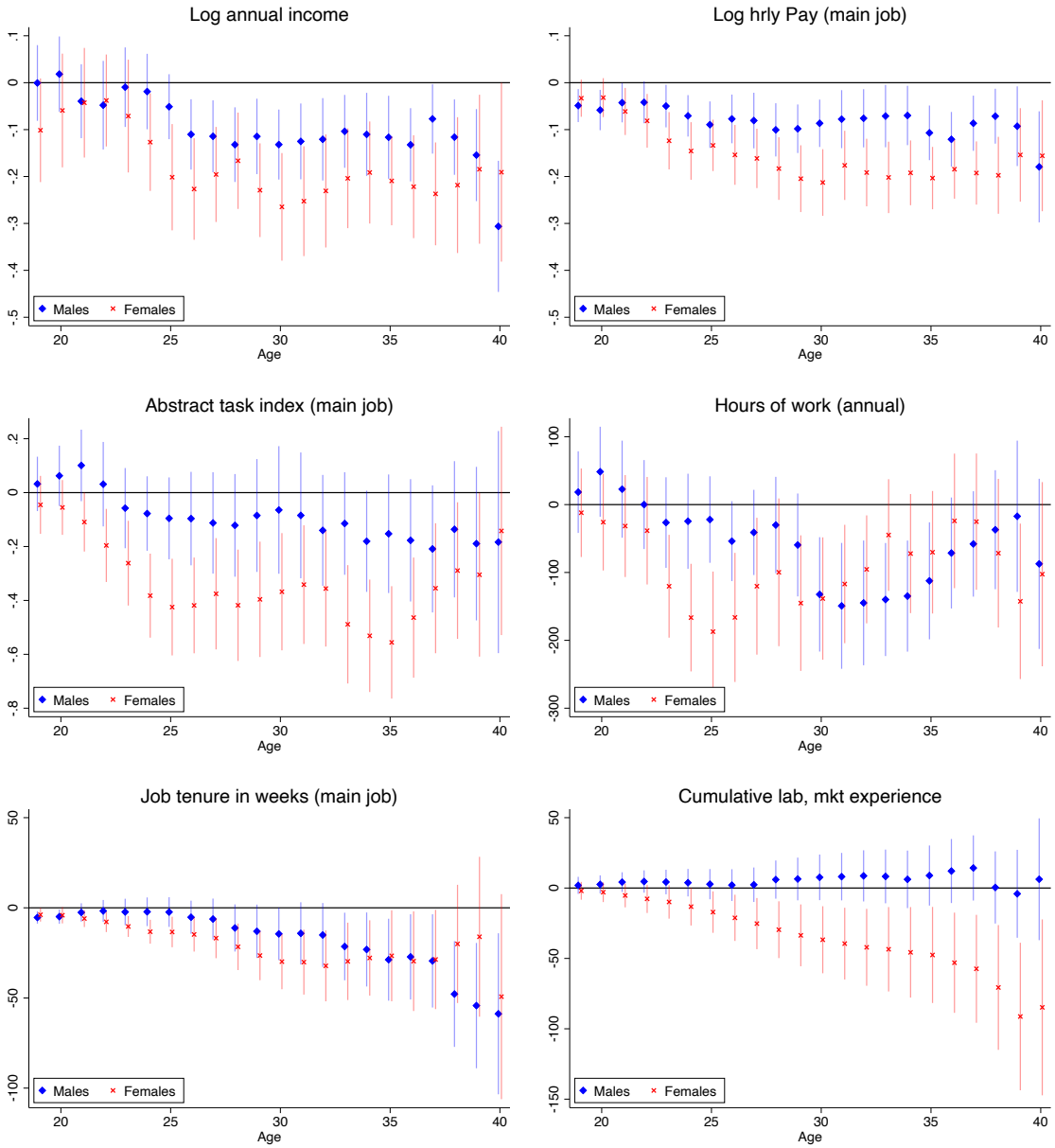

Author calculations using (Bureau of Labor Statistics, 2022) data. Difference in conditional means (victim minus non-victim) estimated as $\hat{\theta}_b$ from OLS estimates of equation (1). Spike plots indicate 95% confidence interval. Except for the propensity to participate in the labour market, these estimates are restricted to labor force participants at each respective age.

Figure 2: Victimization penalties for intermediating outcomes



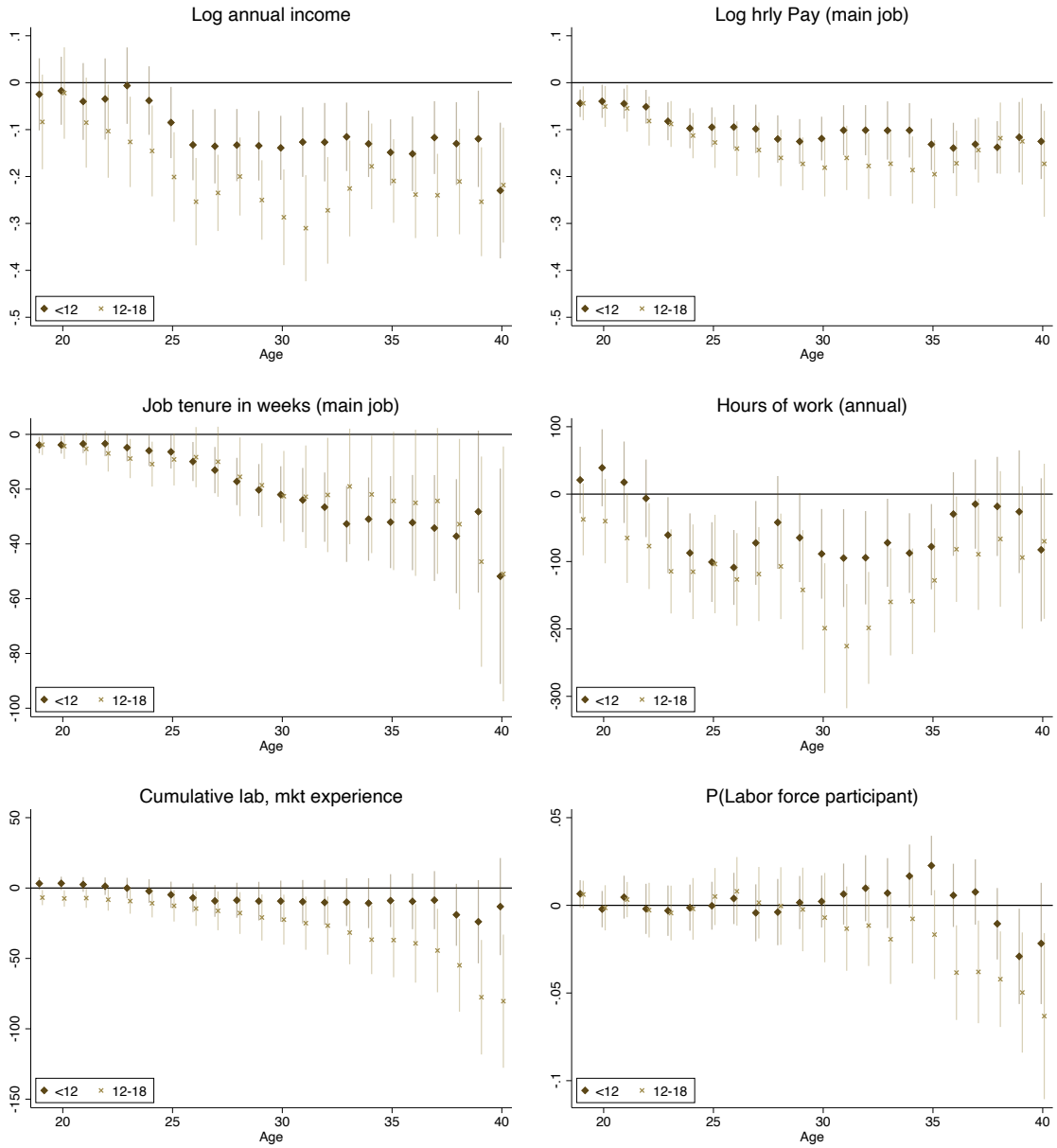

Author's calculation using (Bureau of Labor Statistics, 2022) data. Difference in means between bully victims and non-victimized counterparts (victim minus non-victim) calculated as the coefficient for a bully victim indicator in an OLS regression that also accounts for sex, white ethnicity, citizenship, census regions, urban/rural status birth year, birth month. Spike plots indicate 95% confidence interval.

Figure 3: Sex differences in victimization penalties



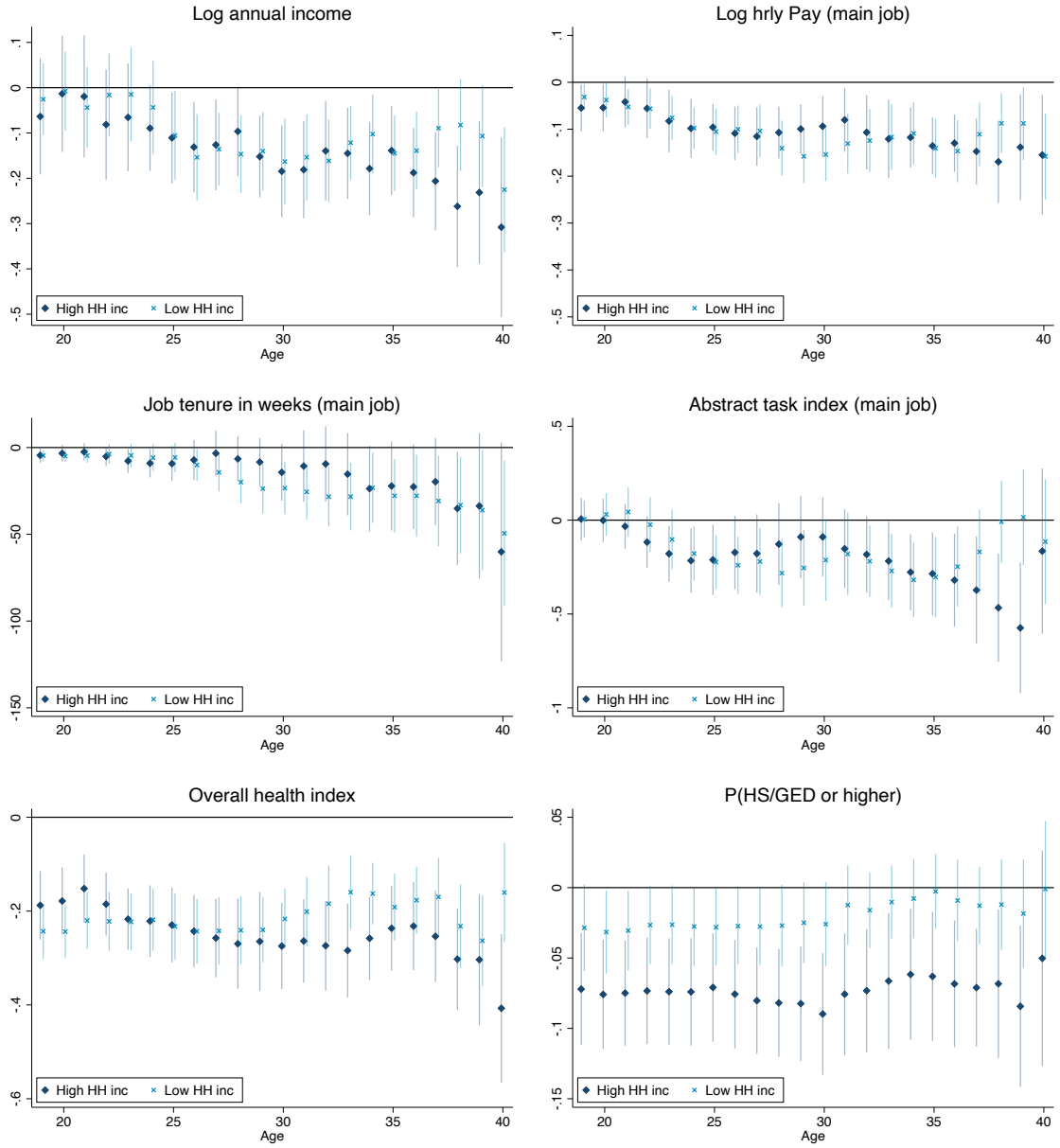

Author's calculation using (Bureau of Labor Statistics, 2022) data. Difference in means between bully victims and non-victimized counterparts (victim minus non-victim) calculated as the coefficient for a bully victim indicator in an OLS regression that also accounts for sex, white ethnicity, citizenship, census regions, urban/rural status birth year, birth month. Spike plots indicate 95% confidence interval.

Figure 4: Differences in victimization penalties by period of victimization



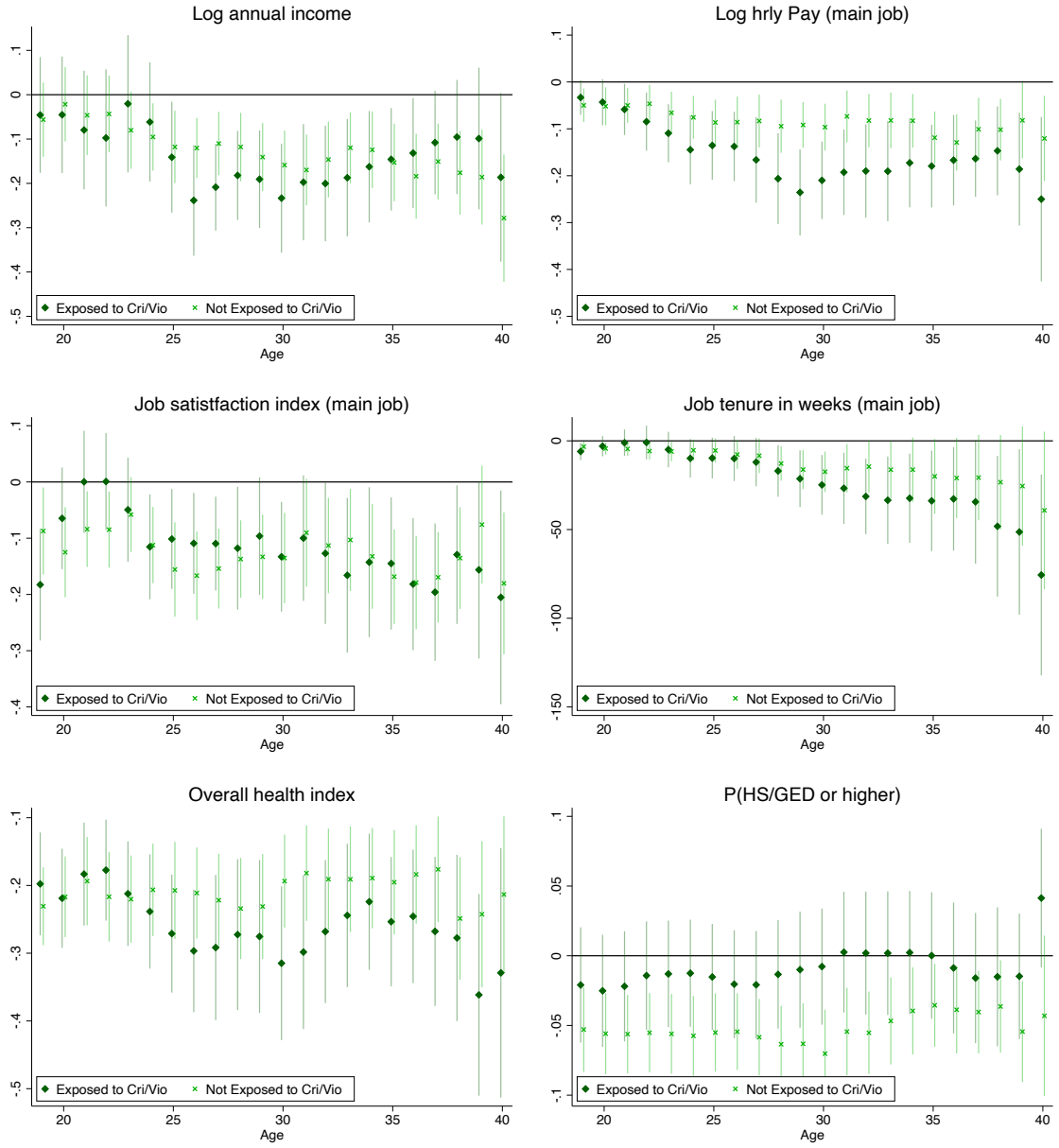

Author's calculation using (Bureau of Labor Statistics, 2022) data. Difference in means between bully victims and non-victimized counterparts (victim minus non-victim) calculated as the coefficient for a bully victim indicator in an OLS regression that also accounts for sex, white ethnicity, citizenship, census regions, urban/rural status birth year, birth month. Spike plots indicate 95% confidence interval.

Figure 5: Differences in victimization penalties by 1997/98 average gross household income




Author's calculation using (Bureau of Labor Statistics, 2022) data. Difference in means between bully victims and non-victimized counterparts (victim minus non-victim), separately by ethnicity, calculated as the marginal effect for a bully victim indicator in an OLS regression that interacts a victimization indicator with an indicator for above median income and also accounts for sex, white ethnicity, citizenship, census regions, urban/rural status birth year, birth month. Spike plots indicate 95% confidence interval.

Figure 6: Differences in victimization penalties by exposure to crime and violence aged <12




Author's calculation using (Bureau of Labor Statistics, 2022) data. Difference in means between bully victims and non-victimized counterparts (victim minus non-victim), separately by ethnicity, calculated as the marginal effect for a bully victim indicator in an OLS regression that interacts a victimization indicator with an indicator for having been exposed to crime and violence aged <12 and also accounts for sex, white ethnicity, citizenship, census regions, urban/rural status birth year, birth month. Spike plots indicate 95% confidence interval.

# Appendix

## A Supplemental tables and figures

Table A.1: Valid responses to bullying, by age milestones...

| | Childhood (age <12) | | Adolescence (age 12−18) | | Either Period (age <18) | | Both Periods (<12 & 12−18) | |
|---|---|---|---|---|---|---|---|---|
| Age | Share | N | Share | N | Share | N | Share | N |
| 20 | 0.202 | 7275 | 0.119 | 7324 | 0.265 | 7209 | 0.055 | 7209 |
| 25 | 0.205 | 6947 | 0.120 | 6864 | 0.267 | 6757 | 0.057 | 6757 |
| 30 | 0.203 | 6749 | 0.124 | 6661 | 0.270 | 6548 | 0.056 | 6548 |
| 35 | 0.206 | 6470 | 0.124 | 6340 | 0.271 | 6242 | 0.058 | 6242 |
| 40 | 0.208 | 2427 | 0.167 | 2408 | 0.292 | 2347 | 0.081 | 2347 |

Data source: (Bureau of Labor Statistics, 2022). Author estimates of mean responses to dichotomous variables about bullying by corresponding age milestone. Shares are weighted with custom survey weights for all survey rounds. N illustrates the number of valid bully question responses that align with non-missing interviews at later-life milestones.

Table A.2: Smoothed outcome variable summary statistics at select age intervals

| | aged 20 | aged 25 | aged 30 | aged 35 | aged 40 |
|---|---|---|---|---|---|
| log annual income | 8.110 | 9.597 | 10.118 | 10.465 | 10.736 |
| | 4998 | 5043 | 4670 | 4161 | 1560 |
| log hourly wage | 6.621 | 7.063 | 7.337 | 7.606 | 7.855 |
| (main job) | 5955 | 5455 | 4922 | 4529 | 2613 |
| job satisfaction index | 3.962 | 3.965 | 3.977 | 4.160 | 4.172 |
| (main job) | 5344 | 5271 | 4655 | 4182 | 2453 |
| abstract task index | 1.958 | 2.823 | 3.249 | 3.444 | 3.621 |
| (main job) | 5958 | 5459 | 4966 | 4594 | 2656 |
| annual hours worked | 1183 | 1648 | 1691 | 1737 | 1679 |
| | 6225 | 6043 | 5820 | 5523 | 3005 |
| P(lab force participant) | 0.979 | 0.959 | 0.932 | 0.920 | 0.904 |
| | 6258 | 6077 | 5869 | 5573 | 3059 |
| job tenure in weeks | 52.48 | 97.96 | 178.4 | 246.3 | 328.7 |
| (main job) | 5942 | 5454 | 4947 | 4587 | 2652 |
| Cum. lab mkt experience | 162.9 | 362.0 | 560.2 | 770.5 | 967.0 |
| | 5869 | 5438 | 5113 | 4712 | 4205 |
| P(HS/GED or greater) | 0.809 | 0.821 | 0.822 | 0.833 | 0.841 |
| | 6000 | 6017 | 5653 | 5184 | 3013 |
| P(Assoc/Bach or greater) | 0.028 | 0.334 | 0.405 | 0.451 | 0.470 |
| | 6000 | 6017 | 5653 | 5184 | 3013 |
| P(Prof/grad degree) | | 0.032 | 0.090 | 0.124 | 0.145 |
| | 6000 | 6017 | 5653 | 5184 | 3013 |
| Overall Health index | 3.963 | 3.835 | 3.749 | 3.677 | 3.578 |
| | 6234 | 5829 | 5565 | 5193 | 3017 |
| P(Fertility event) | 0.257 | 0.289 | 0.278 | 0.203 | 0.100 |
| | 5287 | 5432 | 5176 | 3727 | 2976 |

Author estimates of mean values from base sample reporting bully victimization using custom sample weights from NLSY97 data. Sample size below estimate.

Table A.3: Earnings penalties by age: robustness to consistent sample and soothing

| | main result | without smoothing | present 20-35 | main result | without smoothing | present 20-35 |
|---|---|---|---|---|---|---|
| aged 20 | ln income | ln income | ln income | ln hrly pay | ln hrly pay | ln hrly pay |
| $\hat{\beta}$ | -0.029 | 0.028 | 0.023 | -0.049*** | -0.051*** | -0.041* |
| $se$ | (0.037) | (0.047) | (0.043) | (0.015) | (0.019) | (0.021) |
| $N$ | 5,135 | 3,270 | 2,976 | 6,211 | 5,246 | 4,005 |
| aged 25 | ln income | ln income | ln income | ln hrly pay | ln hrly pay | ln hrly pay |
| $\hat{\beta}$ | -0.121*** | -0.071** | -0.078** | -0.113*** | -0.111*** | -0.102*** |
| $se$ | (0.032) | (0.031) | (0.035) | (0.018) | (0.021) | (0.020) |
| $N$ | 5,122 | 3,707 | 2,976 | 5,685 | 4,920 | 4,005 |
| aged 30 | ln income | ln income | ln income | ln hrly pay | ln hrly pay | ln hrly pay |
| $\hat{\beta}$ | -0.190*** | -0.201*** | -0.171*** | -0.145*** | -0.137*** | -0.140*** |
| $se$ | (0.032) | (0.042) | (0.037) | (0.021) | (0.030) | (0.022) |
| $N$ | 4,726 | 3,149 | 2,976 | 5,128 | 2,736 | 4,005 |
| aged 35 | ln income | ln income | ln income | ln hrly pay | ln hrly pay | ln hrly pay |
| $\hat{\beta}$ | -0.158*** | -0.190*** | -0.120*** | -0.156*** | -0.127*** | -0.145*** |
| $se$ | (0.030) | (0.059) | (0.034) | (0.023) | (0.033) | (0.023) |
| $N$ | 4,228 | 1,505 | 2,976 | 4,700 | 2,630 | 4,005 |
| aged 40 | ln income | ln income | ln income | ln hrly pay | ln hrly pay | ln hrly pay |
| $\hat{\beta}$ | -0.279*** | -0.258*** | -0.219*** | -0.162*** | -0.140** | -0.189*** |
| $se$ | (0.050) | (0.065) | (0.064) | (0.040) | (0.058) | (0.043) |
| $N$ | 1,583 | 738 | 986 | 1,852 | 874 | 1,427 |

Author estimates of baseline victimization penalties using specification in equation (1). OLS estimates with robust standard errors based on survey design and custom survey weights that control for basic demographics $\boldsymbol{D}$ and the unemployment rate.

Table A.4: Job satisfaction and tasks by age: robustness to consistent sample and soothing

| | main result | without smoothing | present 20-35 | main result | without smoothing | present 20-35 |
|---|---|---|---|---|---|---|
| aged 20 | job sat | job sat | job sat | abs task | abs task | abs task |
| $\hat{\beta}$ | -0.115*** | -0.072* | -0.129*** | 0.008 | 0.047 | -0.000 |
| $se$ | (0.026) | (0.038) | (0.040) | (0.042) | (0.058) | (0.048) |
| $N$ | 5,573 | 3,918 | 3,162 | 6,215 | 5,279 | 4,077 |
| aged 25 | job sat | job sat | job sat | abs task | abs task | abs task |
| $\hat{\beta}$ | -0.136*** | -0.159*** | -0.157*** | -0.239*** | -0.253*** | -0.200*** |
| $se$ | (0.032) | (0.040) | (0.036) | (0.061) | (0.066) | (0.070) |
| $N$ | 5,493 | 4,300 | 3,162 | 5,688 | 4,969 | 4,077 |
| aged 30 | job sat | job sat | job sat | abs task | abs task | abs task |
| $\hat{\beta}$ | -0.122*** | -0.126** | -0.155*** | -0.189** | -0.132 | -0.216** |
| $se$ | (0.034) | (0.055) | (0.040) | (0.084) | (0.120) | (0.085) |
| $N$ | 4,851 | 2,514 | 3,162 | 5,173 | 2,783 | 4,077 |
| aged 35 | job sat | job sat | job sat | abs task | abs task | abs task |
| $\hat{\beta}$ | -0.154*** | -0.138*** | -0.134*** | -0.332*** | -0.358*** | -0.287*** |
| $se$ | (0.031) | (0.051) | (0.033) | (0.077) | (0.116) | (0.083) |
| $N$ | 4,338 | 2,363 | 3,162 | 4,768 | 2,685 | 4,077 |
| aged 40 | job sat | job sat | job sat | abs task | abs task | abs task |
| $\hat{\beta}$ | -0.185*** | -0.148 | -0.168** | -0.119 | -0.082 | -0.189 |
| $se$ | (0.051) | (0.094) | (0.071) | (0.130) | (0.226) | (0.160) |
| $N$ | 1,760 | 807 | 1,108 | 1,878 | 884 | 1,465 |

Author estimates of baseline victimization penalties using specification in equation (1). OLS estimates with robust standard errors based on survey design and custom survey weights that control for basic demographics $\boldsymbol{D}$ and the unemployment rate.

Table A.5: Labor supply measures by age: robustness to consistent sample and soothing

| | main result | without smoothing | present 20-35 | main result | without smoothing | present 20-35 |
|---|---|---|---|---|---|---|
| aged 20 | yrly hours | yrly hours | yrly hours | P(LF part) | P(LF part) | P(LF part) |
| $\hat{\beta}$ | 11.415 | 17.604 | 3.495 | -0.008 | 0.006 | -0.006 |
| $se$ | (24.805) | (26.167) | (23.168) | (0.005) | (0.008) | (0.005) |
| $N$ | 6,367 | 5,771 | 5,681 | 6,563 | 6,542 | 5,828 |
| aged 25 | yrly hours | yrly hours | yrly hours | P(LF part) | P(LF part) | P(LF part) |
| $\hat{\beta}$ | -98.782*** | -101.072*** | -92.947*** | 0.003 | 0.001 | 0.002 |
| $se$ | (25.422) | (29.647) | (26.878) | (0.006) | (0.010) | (0.006) |
| $N$ | 6,071 | 5,552 | 5,681 | 6,379 | 6,334 | 5,828 |
| aged 30 | yrly hours | yrly hours | yrly hours | P(LF part) | P(LF part) | P(LF part) |
| $\hat{\beta}$ | -128.539*** | -139.633*** | -110.577*** | 0.002 | -0.002 | 0.001 |
| $se$ | (29.406) | (34.738) | (32.431) | (0.007) | (0.008) | (0.007) |
| $N$ | 5,670 | 5,208 | 5,681 | 6,158 | 6,070 | 5,828 |
| aged 35 | yrly hours | yrly hours | yrly hours | P(LF part) | P(LF part) | P(LF part) |
| $\hat{\beta}$ | -87.605*** | -88.797*** | -51.301* | 0.012 | -0.003 | 0.012 |
| $se$ | (27.883) | (27.873) | (29.919) | (0.008) | (0.010) | (0.008) |
| $N$ | 5,302 | 4,906 | 5,681 | 5,840 | 5,738 | 5,828 |
| aged 40 | yrly hours | yrly hours | yrly hours | P(LF part) | P(LF part) | P(LF part) |
| $\hat{\beta}$ | -98.162** | -102.521** | -141.238** | -0.032** | -0.038** | -0.032** |
| $se$ | (48.019) | (50.409) | (55.460) | (0.016) | (0.018) | (0.016) |
| $N$ | 1,962 | 1,742 | 2,115 | 2,202 | 2,032 | 2,195 |

Author estimates of baseline victimization penalties using specification in equation (1). OLS estimates with robust standard errors based on survey design and custom survey weights that control for basic demographics $\boldsymbol{D}$ and the unemployment rate.

Table A.6: Job tenure and total experience by age: robustness to consistent sample and soothing

| | main result | without smoothing | present 20-35 | main result | without smoothing | present 20-35 |
|---|---|---|---|---|---|---|
| aged 20 | tenure wks | tenure wks | tenure wks | cum exp | cum exp | cum exp |
| $\hat{\beta}$ | -4.548*** | -4.107** | -4.098** | -0.318 | -0.379 | -1.351 |
| $se$ | (1.405) | (1.743) | (1.883) | (1.961) | (1.992) | (2.310) |
| $N$ | 6,201 | 5,267 | 4,054 | 6,002 | 5,486 | 4,932 |
| aged 25 | tenure wks | tenure wks | tenure wks | cum exp | cum exp | cum exp |
| $\hat{\beta}$ | -8.501*** | -7.733** | -6.662** | -8.093** | -7.285** | -8.375** |
| $se$ | (2.831) | (3.168) | (3.198) | (3.610) | (3.651) | (4.152) |
| $N$ | 5,687 | 4,964 | 4,054 | 5,462 | 5,149 | 4,932 |
| aged 30 | tenure wks | tenure wks | tenure wks | cum exp | cum exp | cum exp |
| $\hat{\beta}$ | -20.873*** | -17.345** | -17.536*** | -14.409*** | -12.957** | -16.611*** |
| $se$ | (4.733) | (6.921) | (5.286) | (5.497) | (5.746) | (6.212) |
| $N$ | 5,153 | 2,774 | 4,054 | 4,987 | 4,696 | 4,932 |
| aged 35 | tenure wks | tenure wks | tenure wks | cum exp | cum exp | cum exp |
| $\hat{\beta}$ | -26.847*** | -21.962** | -23.659*** | -21.900*** | -18.673*** | -20.993** |
| $se$ | (8.238) | (10.466) | (8.343) | (6.790) | (6.664) | (8.101) |
| $N$ | 4,765 | 2,673 | 4,054 | 4,539 | 4,246 | 4,932 |
| aged 40 | tenure wks | tenure wks | tenure wks | cum exp | cum exp | cum exp |
| $\hat{\beta}$ | -49.052*** | -52.849* | -50.754** | -18.633 | -25.451* | -39.213** |
| $se$ | (17.308) | (31.466) | (20.199) | (14.203) | (14.546) | (17.176) |
| $N$ | 1,886 | 886 | 1,464 | 1,617 | 1,580 | 1,769 |

Author estimates of baseline victimization penalties using specification in equation (1). OLS estimates with robust standard errors based on survey design and custom survey weights that control for basic demographics $\boldsymbol{D}$ and the unemployment rate.

Table A.7: Gelbach Decompositions with Non-cognitive skills indexes

| | *Panel A) ln annual income* | | | *Panel B) ln hourly pay (main job)* | | |
|---|---|---|---|---|---|---|
| | aged 20 | aged 25 | aged 30 | aged 20 | aged 25 | aged 30 |
| $\hat{\theta}_f$ | -0.053** | -0.1028*** | -0.1069*** | -0.0672*** | -0.0577*** | -0.0743*** |
| $\hat{\theta}_b$ | -0.0655** | -0.1356*** | -0.1939*** | -0.0682*** | -0.1032*** | -0.1363*** |
| $\hat{\delta}_Z$ | 0.0105 | -0.0004 | -0.0357*** | 0.0043 | -0.0109*** | -0.0226*** |
| $\hat{\delta}_E$ | -0.0035 | 0.0002 | -0.0298*** | -0.0027** | -0.0151*** | -0.0236*** |
| $\hat{\delta}_H$ | -0.0149*** | -0.0158*** | -0.0102*** | -0.0023 | -0.0135*** | -0.0104*** |
| $\hat{\delta}_F$ | -0.0005 | -0.0016* | -0.0035** | 0.0000 | -0.0004 | -0.0028*** |
| $\hat{\delta}_N$ | -0.0042* | -0.0152*** | -0.0079*** | -0.0002 | -0.0056*** | -0.0026** |
| N | 2288 | 3546 | 2991 | 2790 | 3927 | 3279 |

| | *Panel C) subj. Job satisfaction (main job)* | | | *Panel D) abstract ask index (main job)* | | |
|---|---|---|---|---|---|---|
| | aged 20 | aged 25 | aged 30 | aged 20 | aged 25 | aged 30 |
| $\hat{\theta}_f$ | 0.0364* | -0.0260 | -0.0743*** | 0.0393* | -0.0759** | -0.0153 |
| $\hat{\theta}_b$ | -0.0104 | -0.0901*** | -0.1215*** | 0.0040 | -0.2539*** | -0.1995*** |
| $\hat{\delta}_Z$ | -0.0049 | -0.0022 | -0.0045 | -0.0152* | -0.0362*** | -0.0395*** |
| $\hat{\delta}_E$ | -0.0003 | -0.0031* | 0.0006 | -0.0051 | -0.1147*** | -0.1174*** |
| $\hat{\delta}_H$ | -0.0342*** | -0.0343*** | -0.0253*** | -0.0122*** | -0.0092* | -0.0188*** |
| $\hat{\delta}_F$ | 0.0002 | -0.0001 | -0.0017 | 0.0002 | 0.0007 | -0.0036 |
| $\hat{\delta}_N$ | -0.0075*** | -0.0244*** | -0.0163*** | -0.0028 | -0.0187*** | -0.0049* |
| N | 2530 | 3815 | 3075 | 2791 | 3935 | 3296 |

| | *Panel E) annual hours worked* | | | *Panel F) P(labor force participant)* | | |
|---|---|---|---|---|---|---|
| | aged 20 | aged 25 | aged 30 | aged 20 | aged 25 | aged 30 |
| $\hat{\theta}_f$ | 28.250* | -26.024* | -124.55*** | -0.0075* | 0.0027 | 0.0115** |
| $\hat{\theta}_b$ | 43.216** | -86.304*** | -166.05*** | -0.0082** | 0.0004 | 0.0057 |
| $\hat{\delta}_Z$ | 20.985*** | -18.674*** | -2.8429 | -0.0010 | -0.0018* | -0.0007 |
| $\hat{\delta}_E$ | -3.3313** | -11.644*** | -23.821*** | -0.0001 | -0.0013*** | -0.0022*** |
| $\hat{\delta}_H$ | 1.8684 | -7.8792*** | -7.1805*** | 0.0006* | 0.0014** | -0.0017** |
| $\hat{\delta}_F$ | -0.0410 | -0.0793 | 2.0625** | 0.0000 | 0.0002* | 0.0001 |
| $\hat{\delta}_N$ | -4.5153*** | -22.003*** | -9.7180*** | -0.0003 | -0.0008** | -0.0015*** |
| N | 2860 | 4029 | 3422 | 2927 | 4191 | 3678 |

| | *Panel G) weeks of tenure (main job)* | | | *Panel H) cum. weeks of experience* | | |
|---|---|---|---|---|---|---|
| | aged 20 | aged 25 | aged 30 | aged 20 | aged 25 | aged 30 |
| $\hat{\theta}_f$ | -2.5088** | -1.6820 | -21.407*** | 3.2323* | 2.7088 | -8.0103** |
| $\hat{\theta}_b$ | -4.8523*** | -4.8776*** | -27.734*** | 0.5024 | -6.955*** | -21.1904*** |
| $\hat{\delta}_Z$ | -1.2139*** | -2.8426*** | -6.5916*** | -0.9032 | -4.043*** | -5.7606*** |
| $\hat{\delta}_E$ | -0.1288 | 0.9590*** | 0.6138* | -0.6721** | -1.4026*** | -3.2903*** |
| $\hat{\delta}_H$ | -0.6935*** | -0.5670** | -0.1902 | -0.5668*** | -1.2945*** | -2.5264*** |
| $\hat{\delta}_F$ | 0.0059 | -0.0399 | -0.2810 | -0.0420 | 0.0194 | -0.1500 |
| $\hat{\delta}_N$ | -0.3132*** | -0.7051*** | 0.1220 | -0.5458*** | -2.9432*** | -1.4527*** |
| N | 2786 | 3927 | 3287 | 2758 | 3796 | 3289 |

Covariate decompositions comparing specifications (1) and (2) using OLS based methods proposed by Gelbach (2016) with robust standard errors. $\hat{\theta}_b$ and $\hat{\theta}_f$ are bully victimization penalties in base and full specifications, respectively, while $\Delta$ is their difference. Estimates $\hat{\delta}_Z$, $\hat{\delta}_E$, $\hat{\delta}_H$ and $\hat{\delta}_F$ are the decomposed components of the difference between $\hat{\theta}_b$ and $\hat{\theta}_f$, educational attainment, health and fertility, respectively.

Figure A.1: Unemployment rates and respondent years at age

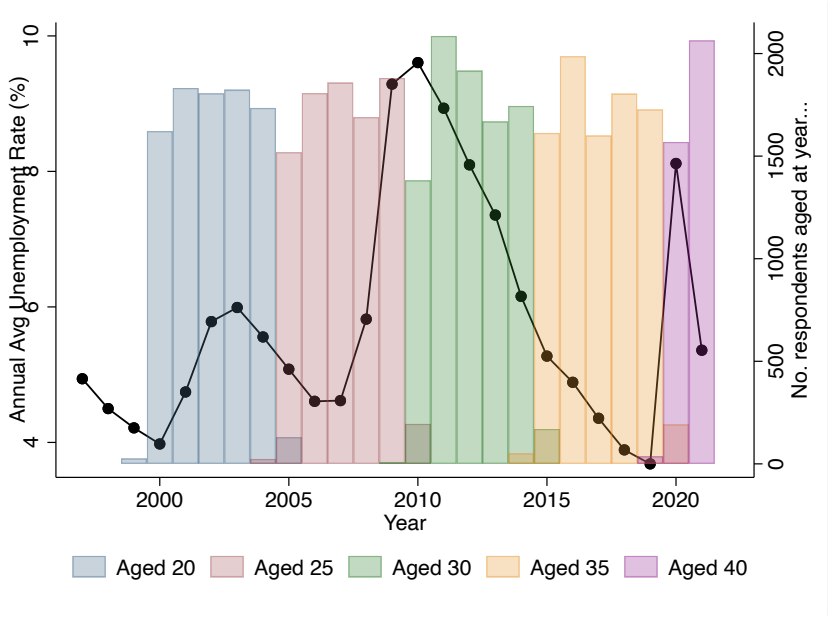
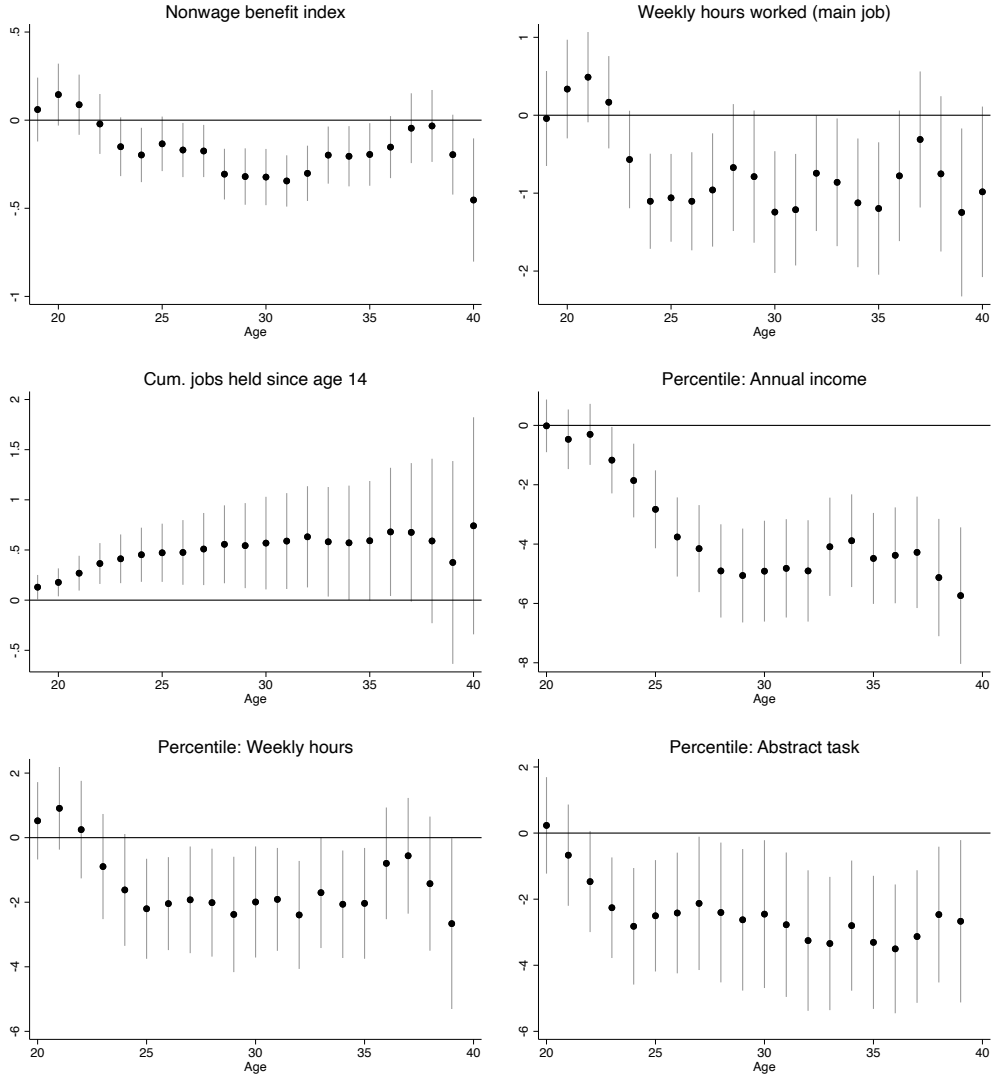
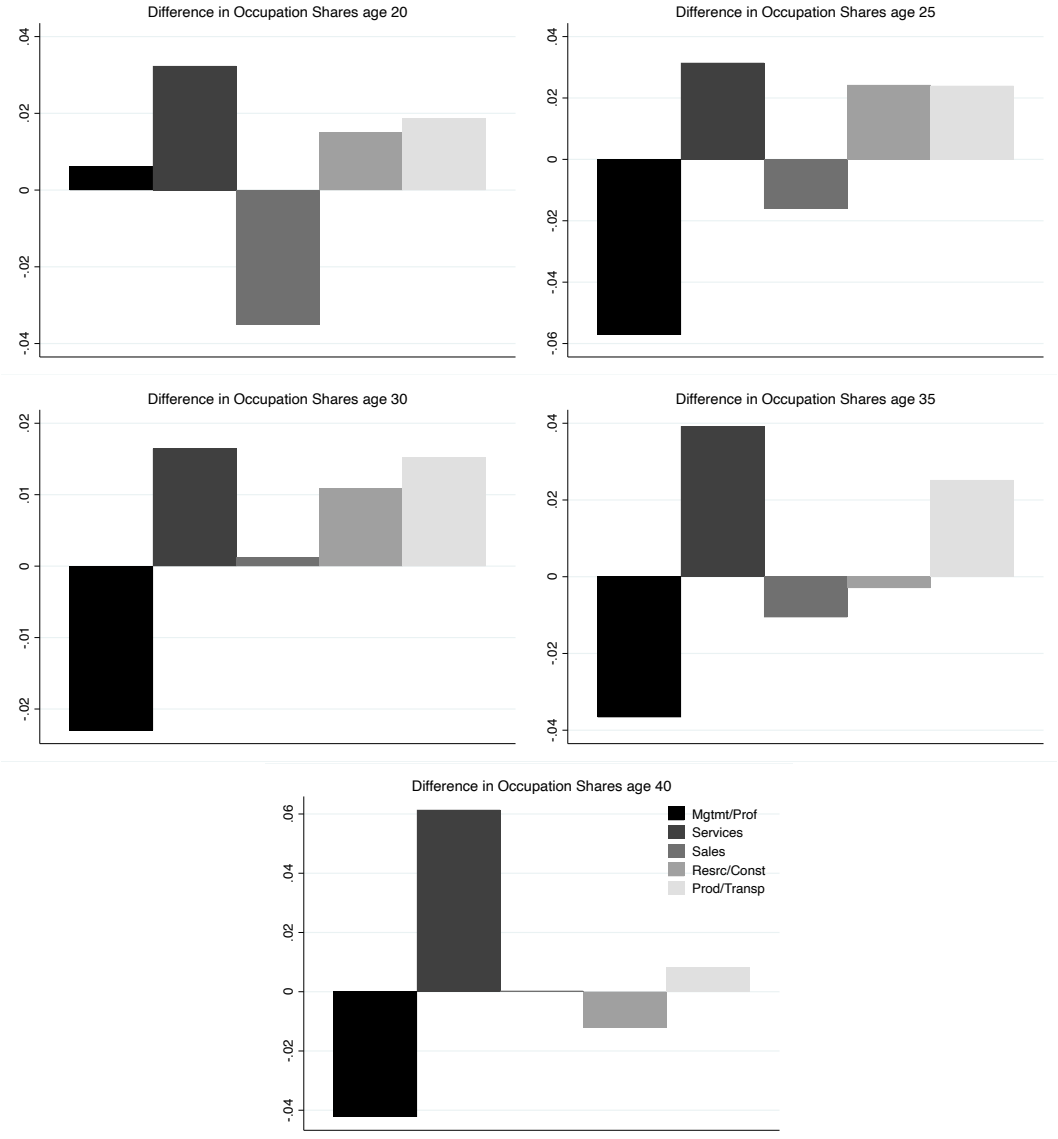


Figure A.2: Victimization penalties for additional labor market outcomes



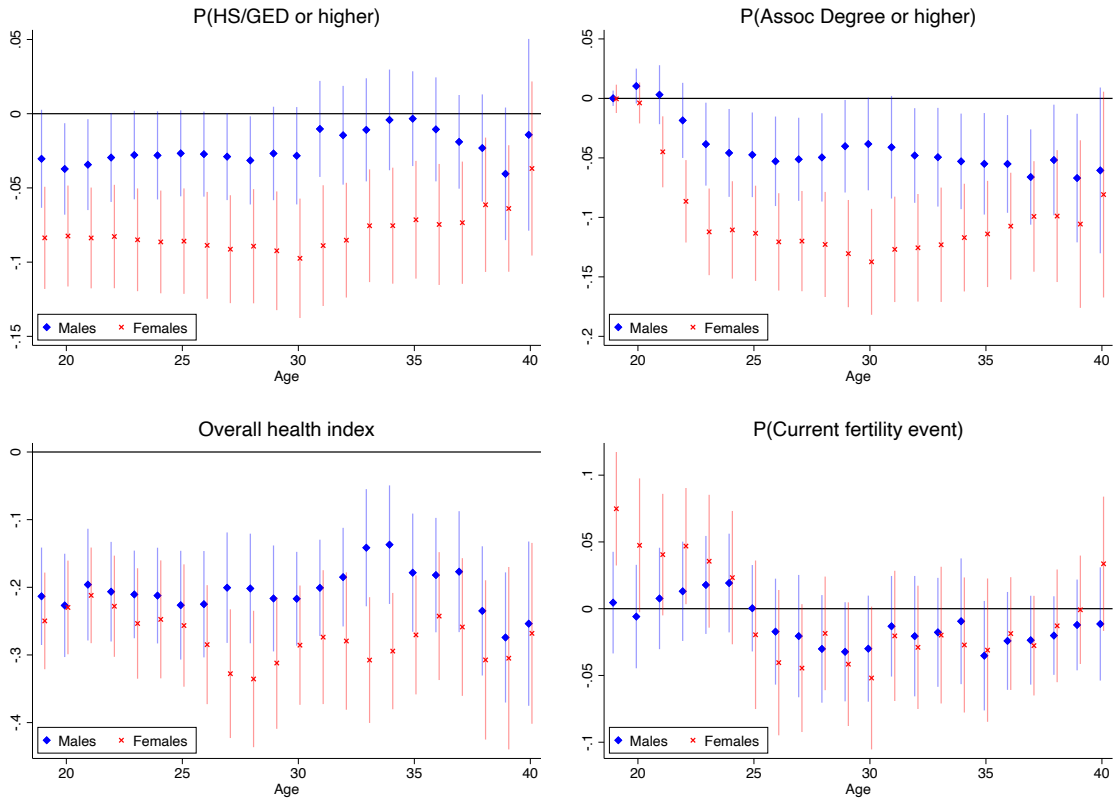

Author calculations using (Bureau of Labor Statistics, 2022) data. Difference in conditional means (victim minus non-victim) estimated as $\hat{\theta}_b$ from OL S estimates of equation (1). Spike plots indicate 95% confidence interval. Except for the propensity to participate in the labour market, these estimates are restricted to labor force participants at each respective age.

Figure A.3: Differences in main job occupation shares by bully victimization under age 18




Data source: Bureau of Labor Statistics, 2022. Author calculations of occupation share differentials (victim minus non-victim) based on respondent's main job at respective ages. Custom survey weights used. Results include full sample of labor force participants reporting occupation and bully victimization status during childhood and adolescence.

Figure A.4: Sex-specific differences in victimization penalties for intermediating outcomes




Author's calculation using (Bureau of Labor Statistics, 2022) data. Difference in means between bully victims and non-victimized counterparts (victim minus non-victim), separately by sex, calculated as the marginal effect for a bully victim indicator in an OLS regression that interacts a victimization indicator with sex and also accounts for white ethnicity, citizenship, census regions, urban/rural status birth year, birth month. Spike plots indicate 95% confidence interval.

## B Victimization propensity

To understand how early life characteristics contribute to who is bullied during youth we estimate the following probit model for all three of our victimization indicators $BV_i$ in our base sample:

$$P(BV_i = 1|\boldsymbol{D}, \boldsymbol{Z}_1, \boldsymbol{Z}_2, \boldsymbol{Z}_3) = \Phi(\alpha + \boldsymbol{D}_i\beta' + \boldsymbol{Z}_{1i}\gamma' + \boldsymbol{Z}_{2i}\pi' + \boldsymbol{Z}_{3i}\zeta') \tag{3}$$

Marginal effects for covariates significant in at least one specification are presented in Table B.8.[32]

Victimization during childhood (aged<12), column (1), is 4% less common among females and in the US Northeast census region. Instead, adolescent victimization (aged 12−18 in column (2)), is more common among earlier birth cohorts and white respondents. A school changes during grades 1-6 (7-12) decreases adolescent victimization propensity by about 5% (2%). Both childhood and adolescent victimization are predicted by the AFQT score, various measures of childhood exposure to crime and violence, parental reports that a child ever experienced learning or emotional performance limitations and the presence of sensory conditions. Lower household net worth has a weakly negative association with childhood victimization, with each $10,000 reducing victimization propensity by just under 1%.[33] In column (3) we add estimates for $BV_i^A$ are conditional on $BV_i^C$ because adolescent victimization could depend on childhood victimization. Results remain similar, though gang exposure and household net worth are no longer as important. Finally, in column (4) we present results for our main measure, $BV_i^Y$, which captures whether or not an individual was ever bullied during youth. Almost all covariates are found to be important in this blended result.

We also supplement these results with linear probability model estimates in column (5), which absorbs household (sibling) fixed effects in a reduced sample of 2,227 individuals from households with multiple respondents that both appear in our base sample. We employ survey weights and cluster by household. The results are broadly similar, which is interesting and suggests that the likelihood of bullying, at least with respect to a battery of observable traits, is similar with and across households. Schooling changes are less predictive within-siblings, perhaps because siblings may often switch schools together, while exposure to neighborhood shootings and height are more predictive. Though statistically insignificant, the point estimates for sex and white ethnicity are similar (not more different than column (4) re-estimated with a linear probability model).

[32]Marginal effects evaluated at the mean. We retain all covariates as controls during the main analysis. Insignificant covariates include birth quarter dummies; urban/rural dwelling indicators; a citizenship indicator; the number of repeated grades and school suspensions, separately for grades 1-6 and 7-12; parental reports of ever experiencing physical or emotional limitations that affected attendance at school or work; height in 1997 and an indicator for the presence of a physical deformity.

[33]Each10-point AFQT score increase raises victimization propensity by 1%, which is a sizeable amount. School history effects are somewhat collinear with indicators for limitations in school performance and attendance. Estimates available upon request show that incarceration aged 12−18 was statistically insignificant. Incarceration under age 12 was statistically significant, however only 4 respondents report incarceration at these ages and incarceration covariates limit sample sizes considerably.

Table B.8: Individual characteristics and the propensity to be bullied

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Bullied age<12 | Bullied age 12−18 | Bullied age 12−18 | Bullied age <18 | Bullied age <18 |
| Bullied < 12 | | | 0.142*** | | |
| | | | (0.011) | | |
| Female | -0.047*** | -0.002 | 0.005 | -0.043*** | -0.042 |
| | (0.012) | (0.010) | (0.010) | (0.013) | (0.029) |
| White | 0.035** | 0.048*** | 0.039*** | 0.054*** | 0.032 |
| | (0.014) | (0.011) | (0.010) | (0.015) | (0.093) |
| Born 1981 | 0.002 | -0.069*** | -0.069*** | -0.048*** | -0.025 |
| | (0.018) | (0.013) | (0.014) | (0.015) | (0.040) |
| Born 1982 | -0.004 | -0.090*** | -0.089*** | -0.062*** | -0.056 |
| | (0.019) | (0.013) | (0.012) | (0.020) | (0.037) |
| Born 1983 | -0.001 | -0.081*** | -0.077*** | -0.054** | -0.082** |
| | (0.020) | (0.015) | (0.015) | (0.022) | (0.040) |
| Born 1984 | 0.029 | -0.114*** | -0.117*** | -0.050** | -0.050 |
| | (0.020) | (0.017) | (0.016) | (0.025) | (0.047) |
| Birth Q2 | -0.011 | -0.002 | 0.001 | -0.011 | -0.016 |
| | (0.015) | (0.012) | (0.012) | (0.016) | (0.039) |
| Birth Q3 | -0.021 | -0.029** | -0.024* | -0.027 | -0.064* |
| | (0.016) | (0.014) | (0.013) | (0.020) | (0.035) |
| Birth Q4 | -0.015 | -0.016 | -0.012 | -0.021 | -0.045 |
| | (0.015) | (0.013) | (0.012) | (0.019) | (0.038) |
| CR 2 '97 | 0.027 | -0.013 | -0.017 | 0.016 | |
| | (0.019) | (0.018) | (0.016) | (0.020) | |
| CR 3 '97 | 0.046** | -0.022 | -0.030* | 0.025 | |
| | (0.021) | (0.017) | (0.016) | (0.021) | |
| CR 4 '97 | 0.050** | -0.017 | -0.025 | 0.027 | |
| | (0.023) | (0.018) | (0.016) | (0.026) | |
| Gang Exposure | 0.080*** | 0.026** | 0.011 | 0.072*** | 0.061* |
| | (0.012) | (0.010) | (0.010) | (0.012) | (0.032) |
| Witness Shooting | 0.073*** | 0.007 | -0.008 | 0.066*** | 0.142*** |
| | (0.015) | (0.016) | (0.016) | (0.019) | (0.052) |
| Home Burglarized | 0.083*** | 0.026** | 0.011 | 0.088*** | 0.050 |
| | (0.014) | (0.012) | (0.011) | (0.016) | (0.053) |
| Theft vict. at school | 0.073*** | 0.028*** | 0.014* | 0.079*** | 0.025 |
| | (0.013) | (0.009) | (0.008) | (0.014) | (0.031) |
| N School chg Gr 1-6 | 0.028 | -0.039* | -0.044** | 0.005 | -0.024 |
| | (0.023) | (0.021) | (0.021) | (0.025) | (0.065) |
| N School susp Gr 1-6 | 0.002** | -0.001 | -0.001 | 0.001 | -0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.003) |
| N School chg Gr 7-12 | | 0.014*** | 0.012*** | 0.020*** | 0.004 |
| | | (0.004) | (0.004) | (0.006) | (0.016) |
| hh grss inc '97/'98 (1000s) | -0.002 | -0.003** | -0.003** | -0.004** | |
| | (0.001) | (0.001) | (0.001) | (0.001) | |
| Phs/Emp Limit att. '97 | 0.046** | 0.027* | 0.018 | 0.045** | -0.048 |
| | (0.020) | (0.016) | (0.016) | (0.020) | (0.061) |
| Lrn/Emo Limit Perf. '97 | 0.046** | 0.036** | 0.027* | 0.056*** | -0.019 |
| | (0.018) | (0.014) | (0.015) | (0.019) | (0.056) |
| Height (ft) 1997 | 0.001 | 0.000 | 0.000 | 0.002 | 0.010** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.004) |
| Sensory cond. 1997 | 0.054*** | 0.045*** | 0.034*** | 0.068*** | 0.074* |
| | (0.014) | (0.011) | (0.011) | (0.014) | (0.039) |
| Observations | 6,259 | 6,259 | 6,259 | 6,259 | 2,227 |
| hh FE | No | No | No | No | Yes |

Columns 1-4 are marginal effects calculated at the mean from probit regressions that account for sampling design. Column 5 is a linear probability model with sibling (houshold) identifier fixed-effects and standard errors clustered by sibling. All specifications estimated with sample weights. Covariates always insignificant are suppressed but include urban/rural dwelling and citizenship indicators, school suspension grades 1-6 and 7-12, school repeats grades 1-6, parental reports the presence of a physical deformity and the constant term in column 5.

## C Non Cognitive Skills

Here we describe how we capture and measure non-cognitive skills at ages 20,25 and 30.

Round 6 provides questions from the Goldberg (1993) Big Five personality traits: Agreeableness and conscientiousness. Other big five traits include Extraversion, Neuroticism and Openness to Experience, but these are not measured in round 6. Respondents rate themselves on a scale of 1-5 against the following descriptions: organized vs. disorganized, conscientious vs. unconscientious, undependable vs. dependable, thorough vs. careless, agreeable vs. quarrelsome, stubborn vs. flexible, trustful vs. distrustful, and uncooperative vs. cooperative. Individuals aged 18-21 (birth years 1982-1984) answered the round 6 questions, which limits our sample size considerably.

In Rounds 12 and 14, eight statements are provided and respondents rank whether these statements describe themselves on a scale of 1 ("disagree strongly") to 7 ("agree strongly"). Four assess a subject's propensity to follow rules or traditions and another four, which form a subset of questions from the Chernyshenko Conscientiousness Scale, are used to measure industriousness. Statements on rule following include: "I do not intend to follow every little rule that others make up", "When I was in school, I used to break rules quite regularly", "I support long-established rules and traditions", "Even if I knew how to get around the rules without breaking them". Statements on industriousness include: "I do not work as hard as the majority of people around me", "I have high standards and work toward them", "I do what is required, but rarely anything more", and "I make every effort to do more than what is expected of me". We capture responses for individuals from whichever round corresponds more closely to age 25, and then restrict to ensure responses are within the age range 23-27.

Round 12 also includes questions from the 10-item personality inventory (TIPI), which is a variation on the Big Five. We prefer the round 12/14 questions because they enable larger sample sizes, but preliminary results using the TIPI questions were found to be similar.

Round 16/17/18 questions assess personality using "Duckworth's GRIT scale", and were asked at different survey years to different respondents. The Grit scale self assessment scores 8 questions from 1 ("very much like me") to 5 ("not like me at all"). The 8 questions include,"New ideas sometimes distract me from previous ones"; "Setbacks don't discourage me."; "I have been obsessed with a certain idea or project for a short time but later lost interest."; "I am a hard worker."; "I often set a goal but later choose to pursue a different one"; "I have difficulty maintaining my focus on projects that take more than a few months to complete."; "I finish whatever I begin" and "I am diligent." Individuals were asked these questions only once. We restrict ourselves to responses from rounds 16 & 17 by ensuring an age range of 28-32 among respondents.

For each round of questions we implement principal component analysis, capturing only the leading principal component as our measure of non-cognitive skill. In many cases, questions are posed in the negative (for example, higher values are associated with un-cooperative vs cooperative) while other questions are posed in the positive - an intentional structure in subjective self-report surveys. We reverse the response ordering of any negative variables prior to performing the dimension reduction so that all contributors are indicative of favorable non-cognitive traits. Table C.9 summarized the loading of each of the original NLSY97 personality

measures onto our resulting non-cognitive skill indexes.

Table C.9: Summary: Leading PCA measure of Non-Cognitive Skill

| *Round 6 (2002) questions* | age 20 | age 25 | age 30 |
|---|---|---|---|
| Organized vs. disorganized | 0.305 | | |
| Dependable vs. undependable | 0.348 | | |
| Cooperative vs. uncooperative | 0.439 | | |
| Flexible vs. Stubborn | 0.344 | | |
| Thorough vs. Careless | 0.378 | | |
| Conscientious vs. unconscientious | 0.185 | | |
| Agreeable vs. quarrelsome | 0.413 | | |
| Trustful vs. distrustful | 0.355 | | |
| *Round 12/14 (2008/2010) questions* | | | |
| Work as hard as majority | | -0.375 | |
| Often do more than required | | -0.332 | |
| Work toward high standards | | 0.408 | |
| Try to do more than expected | | 0.453 | |
| Intend to follow every rule | | 0.280 | |
| Rarely broke school rules | | 0.272 | |
| Support established rules/traditions | | 0.332 | |
| Would not break rules, even if could | | 0.339 | |
| *Round 16/17(2013/15) questions* | | | |
| New ideas distract | | | 0.364 |
| Setbacks don't discourage | | | -0.053 |
| Short term obsessions | | | 0.400 |
| Hard worker | | | -0.276 |
| Change goals frequently | | | 0.406 |
| Able to maintain focus | | | 0.458 |
| Able to Finish projects | | | -0.413 |
| Diligent | | | -0.289 |
| *Summary for leading principal component* | | | |
| Eigenvalue | 2.317 | 2.468 | 2.759 |
| Proportion | 0.290 | 0.309 | 0.345 |
| N | 4847 | 6350 | 5424 |

Table C.10: Non-Cognitive Skill differences by Bully Victim status

| | age 20 | age 25 | age 30 |
|---|---|---|---|
| Mean $BV_i^Y = 1$ | -0.140 | -0.295 | -0.299 |
| Mean $BV_i^Y = 0$ | 0.088 | 0.072 | -0.033 |
| Difference | -0.228*** | -0.367*** | -0.266*** |
| N | 2556 | 3325 | 2904 |