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Crime victims' children

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Abstract
Crime can disrupt victims' working lives and family circumstances, potentially affecting the environment in which their children develop. We provide causal evidence that non-domestic parental victimization harms children's human capital. Using Danish registers, we implement a matched difference-in-differences design that exploits victimization timing relative to the end of compulsory schooling. Violent victimization reduces mathematics scores by 0.03 standard deviations on average, reaching 0.09 following paternal victimization in early childhood, while property victimization has no detectable effect. Violent victimization of mothers disrupts educational progression, particularly for daughters. Sons experience lower well-being, while daughters report greater perceived safety and have lower rates of police-recorded victimization. Achievement losses are larger in families predicted to experience weaker parental labor-market attachment, a greater likelihood of parental charges for violent crime, and greater relational or residential instability.

Keywords: parental criminal victimization; intergenerational spillovers; children's human capital; early childhood
JEL codes: K42; I24; J13

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1 Introduction

Many individuals become victims of crime. Victimization can disrupt adults' employment and earnings (Bindler and Ketel, 2022), and when a parent is victimized, the costs may extend to their children. Children's skills depend on parental investments and the home environment (Cunha, Heckman and Schennach, 2010), yet remarkably little is known about the potential intergenerational transmission of trauma and the educational consequences of parental victimization originating outside the family. Establishing whether and when such spillovers arise is important for understanding how family shocks shape human capital and for assessing the full social costs of crime.

Parental victimization may affect children through both family resources and the inputs parents provide. Household models emphasize that parental labor supply, time with children, and expenditure are jointly determined (Del Boca, Flinn and Wiswall, 2014). Employment losses can therefore change income and available parental time in opposing directions. Victimization may also disrupt emotional support and family stability. Evidence from preschool children exposed to nearby homicides documents impaired attention and impulse control alongside increased parental distress (Sharkey et al., 2012).

We provide causal evidence that parental victimization originating outside the family harms children's human capital. Using Danish administrative data, we examine children's achievement, educational progression, well-being, and safety together with parental and family responses to characterize these potential intergenerational consequences. Our treatment group consists of children aged 0–21 in the year a parent reports being the victim of a crime. We exclude domestic violence, and our findings complement existing evidence on intra-family violence (Currie, Mueller-Smith and Rossin-Slater, 2022; Bhuller et al., 2024). We further exclude parents younger than 22 at first birth or with prior criminal charges, groups facing exceptionally high victimization risks. These restrictions focus our analysis on victimization events that are less likely to arise from entrenched criminal trajectories.

We use a matched difference-in-differences design that exploits the timing of parental victimization relative to the end of compulsory schooling, adapting the identification

strategy developed in Bingley, Cappellari and Ovidi (2026) for parental job loss. Unlike victims' labor market outcomes, which can be observed repeatedly, children's end-of-school achievement is measured only once, precluding a standard event-study design. We therefore compare children exposed to parental victimization with matched peers whose parents were not victimized. To address residual selection, we exploit the fact that parental victimization occurring after compulsory schooling cannot affect children's exam performance. We subtract the treatment-control achievement gap for post-school exposure from the corresponding gap for exposure in childhood. The resulting estimates identify causal effects if unobserved selection into parental victimization is invariant across ages at exposure. Supporting evidence includes null placebo effects for exposure after age 16, flat achievement profiles across placebo exposure ages among matched controls, and null pre-trends in children's school absences or reported well-being.

Parental victimization has lasting consequences for children, but these differ across outcomes and family members. Violent victimization reduces end-of-school mathematics scores by 0.03 standard deviations (hereafter, σ), reaching 0.05σ for exposure in early childhood. Property crime shows no detectable effects, and we therefore focus on violent offenses. The educational consequences differ by which parent is victimized. Paternal victimization reduces achievement for both sons and daughters exposed in early childhood, with losses extending to later exposure ages for daughters. Maternal victimization instead disrupts educational progression, particularly among daughters, who are less likely to enroll in academic high school and attend schools with lower-achieving peers. Beyond education, effects differ sharply by gender. Sons report lower school well-being and greater psychological distress, whereas daughters report greater perceived safety and less bullying and are less likely to experience police-recorded victimization. That daughters experience educational setbacks alongside improvements in safety is consistent with protective parental responses, although we do not directly observe parenting style.

We document suggestive correspondences between family responses to victimization and its impacts on children. Paternal victimization is followed by larger and more persistent labor-market disruptions and a more marked increase in charges for violent offenses,

mirroring stronger achievement losses for children of victimized fathers. Maternal victimization, particularly in early childhood, is instead more closely associated with residential mobility and family dissolution, echoing the disruptions in daughters' educational progression. We further examine how achievement effects vary with parental responses predicted from pre-victimization characteristics. Achievement reductions are larger in families predicted to undergo weaker labor-market attachment, greater subsequent parental criminal involvement, and greater relational or residential instability, while the evidence is considerably weaker for acute physical-health deterioration or losses in household resources. Taken together, these patterns point to multiple dimensions of family disruption associated with children's educational losses, rather than a single main channel.

Our findings imply that the social costs of crime extend beyond direct victims, potentially reinforcing intergenerational inequality. The concentration of achievement losses in early childhood suggests that interventions targeting victimized families with young children may be particularly valuable. Our evidence also highlights the importance of extending support to victims' children. In Denmark, crime victims can access subsidized psychological care, but their children are not eligible on the basis of parental victimization alone. We find increased psychologist contacts among parents following victimization, but not among children, alongside suggestive evidence consistent with a protective role for psychological support.

We contribute to the growing literature on the costs of criminal victimization. While the existing literature has extensively studied offenders (e.g., Nagin, 2013; Draca and Machin, 2015; Chalfin and McCrary, 2017; Britto, Pinotti and Sampaio, 2022), comparatively little is known about victims and their outcomes (Bindler, Ketel and Hjalmarsson, 2020). Hjalmarsson, Machin and Pinotti (2024) identify victimization and the measurement of its costs as important directions for crime research. Recent studies document persistent effects of victimization on victims' labor market outcomes and health (Adams-Prassl et al., 2024; Bindler and Ketel, 2022; Dustmann and Mertz, 2026), and substantial costs of cohabiting with an abusive partner (Adams et al., 2024).[1]

[1]Evidence from other contexts shows that victimization affects work schedules (Hamermesh, 1999), spatial mobility (Dugan, 1999), avoidance behavior and time allocation (Braakmann, 2012), physical ac-

A related literature examines how the consequences of crime extend beyond direct victims. Facchetti, Rasul and Sijthoff (2026) document worse crime-related perceptions among adults who witness crime or know a victim of burglary or assault. Research on victimization spillovers studies community violence (Koppensteiner and Menezes, 2021), in-utero exposure to maternal victimization (Aizer, 2011; Currie, Mueller-Smith and Rossin-Slater, 2022; Menezes and Koppensteiner, 2026), domestic violence (Bhuller et al., 2024), and effects on victims' parents and peers (Adams et al., forthcoming; Mertz, Mitchell and Skov, 2026).[2] Related work documents educational losses following police killings in students' neighborhoods (Ang, 2021), armed conflict near schools (Monteiro and Rocha, 2017), and school shootings (Cabral et al., 2026). Our contribution is to provide the first causal evidence on how parental victimization originating outside the family affects children's achievement and educational progression.

More broadly, we speak to the literature on the intergenerational impacts of parental adversity. Bütikofer et al. (2024) document associations between parents' and children's mental health. Studies of identifiable adverse events show that maternal exposure to conflict hurts children's socioemotional development (Hidalgo-Aréstegui et al., 2025), while maternal bereavement during pregnancy impairs offspring's mental health (Persson and Rossin-Slater, 2018). We examine parental criminal victimization during children's upbringing and its consequences for their educational outcomes and well-being.

Finally, we contribute to the literature on how the timing of family shocks shapes children's human capital. Models of skill formation emphasize that the productivity of parental inputs varies across childhood and depends on previously accumulated skills (Cunha and Heckman, 2007; Cunha, Heckman and Schennach, 2010), making the timing of family shocks economically consequential. Recent studies also show that the effects of parental job loss depend on children's age at exposure (Bingley, Cappellari and Ovidi, 2026; Carneiro et al., 2026). We extend this evidence to parental criminal victimization, finding the largest achievement losses when exposure occurs in early childhood.

tivity (Janke, Propper and Shields, 2016), mental health (Cornaglia, Feldman and Leigh, 2014; Dustmann and Fasani, 2016), and well-being (Cohen, 2008; Johnston, Shields and Suziedelyte, 2018).

[2]Bhalotra et al. (2026) document strong intergenerational correlations in victimization, with children of victimized parents substantially more likely to experience victimization themselves.

Section 2 describes the data and sample construction. Section 3 illustrates the empirical design and supporting identification evidence. Section 4 presents the impacts of parental victimization on children's outcomes, and Section 5 documents parental and family responses. Section 6 concludes.

2 Data and Sample Construction

2.1 Data Sources and Background

Our analysis is based on population-wide Danish administrative registers linked at the individual level using unique personal identifiers. The core input is the register of criminal victimizations between 2001 and 2022, obtained from national police records (Kruse et al., 2010; Statistics Denmark, 2023). These data include the victim identifier and legal classification of each offense. We restrict attention to offenses involving an identifiable victim and distinguish between violent and property crimes based on the official penal code classifications. Specifically, violent crimes include sexual offenses, assaults, and threats, while property crimes consist of thefts.

We link victims to their families using the Danish Civil Registration System, which provides longitudinal information on family relationships (Pedersen, 2011). This register allows us to identify all children of each victimized adult, follow children and parents over time, and determine each child's age at the time of parental victimization. We construct our study population by selecting children aged 0 to 21 who are either i) exposed to parental victimization in this age window or ii) never exposed. Let $a(i)$ denote the child's age in the year of the reported victimization.[3] We partition childhood into three stages: early childhood ($0 \leq a(i) \leq 5$), middle childhood ($6 \leq a(i) \leq 11$), and adolescence ($12 \leq a(i) \leq 16$). This classification closely follows Carneiro et al. (2021) but defines childhood as ending at age 16 to align with the timing of our outcome measure.

Compulsory schooling in Denmark comprises ten grades (0–9, with grade 0 correspond-

[3]We define treatment based on victimization year rather than its exact date since the latter is only available for crimes in which a suspected perpetrator is formally charged (67% of violent victimizations and 2% of property victimizations).

ing to the preschool class), typically beginning in August of the calendar year in which a child turns six and ending in June of the year in which the student turns 16. School-leaving examinations are taken in English, Danish, mathematics, and science, along with a set of randomly selected additional subjects. To measure educational outcomes, we use national education registers containing end-of-school (9th-grade) examination results for the entire student population (Jensen and Rasmussen, 2011). These data provide teacher-assessed and exam-based grades in mathematics and other subjects, which we convert into standardized scores by cohort. Our main outcomes are standardized mathematics examination scores and an aggregate GPA across subjects. Since ninth-grade test scores are observed between 2002 and 2023, we consider the cohorts of children born between 1986 and 2007. Special grading arrangements applied in 2020–2022; Section 4.1 describes these arrangements and reports robustness checks excluding those examination years.

Beyond academic achievement, we observe monthly data on days of absence for each compulsory school grade from 2010. Moreover, we can link register data to well-being surveys administered to all school children every year. Questions span from general satisfaction with school and teachers to perceived learning experience and school environment. Questionnaires are differentiated by grade, with more detailed surveys administered in grades 4-9. These data are available since 2015.

We additionally follow the school enrollment of children beyond compulsory education, when they can pursue several distinct educational tracks. About half of the students choose to attend an additional tenth grade before progressing to upper secondary or vocational education. General upper secondary programs typically last three years and include academically oriented gymnasiums and business or technical colleges. Vocational programs, in contrast, combine classroom instruction with on-the-job training and usually span two to five years.

We complement our data with information on health, labor market attachment, and local context. Health outcomes are constructed from registers covering both hospital visits and non-hospital contacts with the healthcare system, including general practitioners and mental health providers, for both children and parents (Schmidt et al., 2019; Lynge,

Sandegaard and Rebolj, 2011). Labor market variables are drawn from matched employer–employee registers and include annual employment indicators and earnings for each parent (Elmeskov, Leth-Sørensen and Hummelgaard, 1991). Together, these linked registers provide a rich longitudinal picture of children's family circumstances before and after parental victimization, which we exploit in our research design and analysis of mechanisms.

2.2 Sample Selection and Descriptive Statistics

We focus on victimization events that are external to the family by excluding domestic violence. To identify domestic offenses, we exploit information on the suspected perpetrator charged with the reported crime. Specifically, we classify a crime as domestic if the suspected perpetrator has lived at the same address as the reporting victim at any point during the 10 years preceding the report.[4]

For each victim, we consider the first reported crime observed in the data. Since the victimization register begins in 2001, we reserve the first two years to ensure that the selected events are, at a minimum, the first victimizations observed within the preceding two years. We therefore consider victimization events occurring between 2003 and 2022. This selection is made separately by crime type, i.e., we consider a parent's first reported violent crime and first reported property crime. Nevertheless, we note that most victims report only one crime in our observation period (76%). Throughout the analysis, we treat violent and property crime victims, together with their respective matched control groups, as separate samples.

We first illustrate victimization risk in the general population of parents, following Dustmann and Mertz (2026). Appendix Figure A.1 plots the share of parents reporting property crimes (left-hand panels) or violent crimes (right-hand panels) by age. Panel A compares mothers and fathers. Property victimization averages below 1%, is substantially more common among mothers, and varies little with age. Violent victimization is relatively similar across genders but declines markedly with age. Teenage parents are rare, as

[4]For female victims, this criterion flags 20.4% of assaults and 16.4% of threats as domestic (compared with 6.4% and 3.7%, respectively, for male victims). These statistics are broadly in line with Bindler and Ketel (2022).

the green bars show, but face disproportionately high risks of violent victimization. Panel B compares parents with and without criminal charges recorded by the observation year. The age profiles remain broadly similar, but parents with prior charges are about 1 percentage point (p.p.) more likely to report violent crimes up to age 50, with victimization risks exceeding 5% among the youngest parents.

Motivated by the descriptive evidence, we further restrict the sample based on parents' prior involvement with the justice system and their age at first birth. Specifically, we consider only parents without criminal charges up to the year of victimization. This restriction is intended to exclude victimization events associated with "life of crime" trajectories. Moreover, we exclude parents aged 21 or younger at first birth, who exhibit a distinct risk profile for violent victimization. After applying these restrictions, we consider children exposed to parental victimization between ages 0 and 21 ("treated") or never exposed to parental victimization ("untreated"). This selection yields 54,315 treated children for violent crimes and 124,092 treated children for property crimes.

We observe marked negative selection into parental violent victimization. For treated children, the base year, t_i^*, is the calendar year immediately preceding the parental victimization report. Panel A of Table 1 compares treated children's characteristics in the base year with those of untreated children observed in any year. Treated children's fathers earn DKK 40,000 less on average than untreated children's fathers (about USD 6,000; columns 1–2), corresponding to a standardized mean difference (SMD) of 0.132 (column 3). Treated children are also less likely to be of Danish origin, have mothers who were younger at first birth, and live in areas with a lower share of Danish families and higher crime rates. At age 16, they score 0.22σ lower in mathematics, a gap that may reflect both selection and treatment effects.

Panel A of Appendix Table A.1 reports analogous comparisons for property crime, revealing instead positive selection into this type of victimization.[5] For example, treated children have annual family income that is, on average, DKK 93,000 (USD 14,000) *higher* and mothers with five more months of education than untreated children. Pre-matching

[5]This is consistent with the evidence in Dustmann and Mertz (2026).

gaps in achievement differ sharply from violent crimes: the average examination score in mathematics is nearly indistinguishable between treated and untreated children, and treated children score slightly higher on the GPA. We address selection into treatment through our research design, which we discuss next.

3 Empirical Design

We employ a quasi-experimental research design to estimate the causal impact of parental criminal victimization. Although victimization could be partly random, the likelihood of being subjected to violent or property crime varies across contexts, is spatially concentrated, and may therefore carry a socio-economic component. To address this, we employ a matched Difference-in-Differences (DiD) design. Closely related studies (Bindler and Ketel, 2022 and Dustmann and Mertz, 2026) estimate the costs of criminal victimization for victims by comparing the labor market and socio-economic trajectories of individuals victimized a few years apart over the period separating earlier victims (i.e., treated individuals) from later victims (i.e., control individuals). Unlike these studies, our outcome of interest is measured at a single point in time, namely at age 16, when pupils sit their end-of-school (9th-grade) examinations. Moreover, comparing individuals victimized only a few years apart would not allow us to estimate the effects of parental victimization in early childhood. For these reasons, we adopt a different econometric approach.

First, we match children exposed to parental victimization between ages 0 and 21 to unexposed peers sharing key observable characteristics. This procedure assigns placebo parental victimization events at the same child age and in the same calendar year as the corresponding real victimizations (Jaravel, Petkova and Bell, 2018). Specifically, we first split the working sample into cells defined by gender and year of birth, implying that matched control children have exactly the same gender and age as the corresponding treated child. We then implement 1:1 propensity score matching without replacement separately within each year and cell. For each treated child, we select one untreated child based on both parents' age, education, labor market history, family resources, and

characteristics of the area of residence.[6] We successfully match 99.5% of treated children for violent crimes, yielding an estimation sample of 54,027 matched pairs, and 98.5% of treated children for property crimes, yielding an estimation sample of 122,168 matched pairs. Panel B of Table 1 shows that the matching procedure successfully balances observable characteristics across the two groups, with post-matching SMDs around or below 0.01 for most variables (column 3). Nevertheless, a mathematics achievement gap of 0.12σ in favor of control children persists. This difference could reflect either the causal impact of parental victimization or selection on unobservable determinants of educational outcomes.

Second, to address selection on unobservable characteristics, we exploit the presence of children who had already completed compulsory schooling at the time of parental victimization (i.e., those exposed after age 16). Pupils exposed to parental victimization after age 16 are included in the analysis and matched to untreated pupils in exactly the same way as pupils exposed at younger ages. Because parental victimization cannot affect outcomes that have already been realized, the estimated treatment–control difference in end-of-school achievement for this reference group can be attributed to selection into parental victimization not captured by the matching procedure. Under the assumption that selection into treatment is invariant to the child's age at exposure, any additional treatment–control difference among pupils exposed to parental victimization before age 17 can be interpreted as the causal effect of parental victimization. Columns (4)-(7) of Table 1 and Appendix Table A.1 report stage-specific SMDs between treated and control children, showing that our matching is similarly successful in balancing observable characteristics throughout the age at treatment distribution. Still, a substantial achievement gap remains for children treated after the end of school (an SMD of 0.107 for violent crimes and 0.045 for property crimes), a difference that our design nets out from the treatment effect estimates.

[6]Specifically, we implement propensity score matching based on both parents' age and age at first birth, Danish origin, months of education, employment, earnings, unemployment duration, and weeks worked. In addition to measuring labor market variables in the year before the report, we also match on three lags of employment, unemployment, and earnings. Moreover, we include the child's birth order, number of siblings, single-parent status, and several characteristics of the area of residence (share of treated children, average education level, and average income). Our matching procedure follows the standard approach adopted in the literature on parental job loss (e.g., Bertheau et al., 2023).

We estimate the impact of parental victimization on child i's outcome Y_i using the following specification:

$$Y_i = \delta_0 + \delta_1 T_i + \sum_{s \in \{E,M,A\}} \delta_s T_i \times \mathbb{1}[a(i) \in s] + \phi_{a(i)} + \psi X_i' + \eta_i, \tag{1}$$

where T_i is the treatment indicator, which is interacted with indicators for the stage of childhood: Early childhood (E, ages 0–5), Middle childhood (M, ages 6–11), and Adolescence (A, ages 12–16). We control for indicators of the child's age at the time of the real or placebo parental victimization ($\phi_{a(i)}$) and progressively richer sets of individual characteristics (X_i'). The coefficient δ_1 captures the constant treatment–control difference for the reference group of children exposed to parental victimization between ages 17 and 21. This difference is netted out from the estimated causal effects and approximates selection into treatment based on unobservable characteristics. The coefficients δ_s capture the Difference-in-Differences estimates of the effect of parental victimization occurring in childhood stage s on Y_i.[7] In Section 4, we estimate Equation (1) using cross-sectional outcomes measured at age 16, ranging from end-of-school examinations to well-being, absences, secondary-school enrollment, and criminal involvement.

We rely on the assumption of age-invariant selection into treatment. Our estimates identify the causal effect of parental victimization if, in the absence of treatment, any systematic differences in end-of-school outcomes relative to the control group would have remained constant across children exposed at different ages.[8] This assumption may appear strong given the wide age range we consider, as the risk-taking behavior of parents with

[7] Our identification strategy addresses recent concerns about the validity of staggered designs (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Borusyak, Jaravel and Spiess, 2024). Each coefficient δ_s can be interpreted as a 2×2 difference-in-differences comparing the treated–control outcome gap for children exposed in childhood stage s with the corresponding gap among children exposed after compulsory schooling. Control units are never treated.

[8] One might ask whether a "timing" design, identifying the effects of exposure age from variation in the age at victimization among the treated alone, without matched controls, would be preferable. In our setting it is not. Because end-of-school achievement is observed once per child, such a design has no second difference to exploit: it cannot net out any determinant of achievement that varies systematically with age, regardless of exposure (e.g., year and cohort trends, or family resources), which can be especially worrying when considering a large age span. Beyond this limitation, the timing design would still rest on a comparability-across-ages assumption. As explained below, our design is corroborated by pre-treatment parallel trends of the secondary outcomes such as well-being and absences that we observed repeatedly for the same child.

young children may differ from that of parents with adolescent or young adult children. However, we provide several arguments supporting its validity in our context. First, we assess the plausibility of the identifying assumption through a "nonparametric" specification that estimates age-at-victimization-specific treatment effects:

$$Y_i = \gamma_0 + \gamma_1 T_i + \sum_{k=0}^{21} \lambda_k T_i \cdot \mathbb{1}[a(i) = k] + \phi_{a(i)} + X_i'\nu + \epsilon_i, \tag{2}$$

where the coefficients λ_k are normalized relative to $a(i) = 17$, the earliest age at which parental victimization cannot affect end-of-school outcomes. Under our identifying assumption, the estimates of $\lambda_{18}, \ldots, \lambda_{21}$ should be statistically indistinguishable from zero. We present supportive evidence for this restriction in the next section, while acknowledging that the absence of differential selection into treatment between ages 17 and 21 does not necessarily extrapolate to substantially younger ages.

Second, we examine whether achievement among matched control children varies with age at placebo victimization. At each age, controls are selected to resemble treated children in observable characteristics. Stronger negative selection into treatment at younger ages would therefore predict lower achievement among controls matched to children exposed in early childhood. However, Appendix Figure A.2 shows that control children's achievement varies without a systematic trend across ages at placebo victimization.

Third, event studies of parental and child panel outcomes show no evidence of pre-trends. For children, we use school well-being surveys and monthly school absences. Although these analyses necessarily cover only students already in school at parental victimization (i.e., with $6 \leq a(i) \leq 16$), pre-treatment coefficients in Figure 4 and Appendix Figure A.6 are not statistically distinguishable from zero. Event studies of parental outcomes presented in Section 5 likewise show no evidence of pre-trends. In addition, although effect magnitudes vary, parental responses in family stability, labor market outcomes, health, and criminal charges follow similar dynamic patterns whether children are exposed in young adulthood or earlier, providing further indirect support for comparability across exposure ages.

Fourth, our results are unaffected when controlling for residual observable differences

between children exposed at different ages. We show in Section 4 that estimated impacts of parental victimization are remarkably stable when adjusting for various sets of observable characteristics X_i in equation (1). Specifically, we include all the matching variables and, in our most demanding specification, parish dummies, thereby holding granular geographical units constant. Finally, progressively stronger selection into treatment as the age at parental victimization decreases would generate monotonically increasing patterns in the estimated coefficients λ_k from equation (2). Figure 1, discussed in Section 4, shows that this is not the case.

Overall, patterns in children's baseline characteristics, school achievement, absences, and well-being provide little indication of substantially stronger selection into treatment at younger ages. This conclusion pertains to our restricted sample, which focuses on relatively unexpected parental victimization events.

4 Results

We organize the discussion of results around the different margins through which parental victimization affects children. Section 4.1 first establishes the average effects on educational achievement and how they vary with the timing of exposure. Section 4.2 then examines heterogeneity by the gender of the victimized parent and the child. Section 4.3 turns to subsequent educational progression, including delayed progression, upper-secondary track choice, and peer composition. Finally, Section 4.4 broadens the analysis beyond education to children's subjective well-being, perceived safety, bullying, and subsequent victimization. Together, these results characterize how the consequences of parental victimization differ across educational, psychological, and safety-related margins before we turn, in Section 5, to the responses of parents and the family environment.

4.1 Effects on Educational Achievement and Timing

Our baseline results on educational achievement are summarized in Table 2 and Figure 1. Table 2 reports parametric Difference-in-Differences estimates from Equation (1), which

group ages into early childhood (0–5), middle childhood (6–11), and adolescence (12–16). Figure 1 plots age-specific estimates of the effects of parental victimization from Equation (2). Coefficients in this figure are close to zero for children whose parents are victimized after age 16 – i.e., a placebo group for whom end-of-school achievement has already been realized – lending indirect support to our identifying assumption.[9]

We estimate substantial selection into parental violent victimization based on unobserved achievement potential. The estimated coefficient on the *Treated* dummy in Panel A of Table 2 – corresponding to δ_1 in equation (1) – amounts to approximately -0.10σ in mathematics and -0.07σ in GPA. In contrast, Panel B shows substantially smaller estimates for property crimes (-0.03σ in mathematics, zero in GPA).[10] These coefficients estimate the achievement gap between children whose parent is victimized after the completion of compulsory schooling and their matched controls, capturing residual selection that is not removed by the matching procedure. The interaction coefficients discussed below – δ_s in equation (1) – net out this gap. Under the identifying assumption that selection into treatment is invariant across ages at exposure discussed in Section 3, they identify the causal effects of parental victimization occurring in childhood.

The estimates point to three main findings. First, parental violent victimization leads to a statistically and economically significant reduction in children's achievement. The average effect across childhood (column 1 and 3, Panel A of Table 2), estimated by the "Treated × Exposed" coefficient, implies a decline in mathematics scores of 0.03σ. In contrast, the corresponding estimates for property crimes in Panel B are small and not statistically different from zero. The property crimes studied in this setting primarily comprise non-violent thefts. Since these are typically covered by insurance in Denmark and their direct impact on victims is likely smaller compared with violent offenses, undetectable spillovers of property crimes on children's educational outcomes appear plausible.

[9]For violent victimizations, the F-test for their joint statistical significance yields $p = 0.99$ for mathematics and $p = 0.87$ for the GPA. For property victimizations, it yields $p = 0.66$ for mathematics and $p = 0.63$ for the GPA (see Appendix Figure A.3).

[10]The sample size for GPA in Table 2 is lower than the post-matching sample in Panel B of Table 1 due to a minority of students not taking the test (9%). On top of this, a small number of students do not sit the mathematics paper, slightly reducing the sample in columns (1)-(2) with respect to columns (3)-(4). While treated children are 0.7 p.p. more likely to end up not taking the test than control peers, this difference is fixed throughout ages at treatment. We find a null treatment effect on test-taking.

Beyond statistical significance, the early-childhood property estimate of 0.0014 has a 95% confidence interval of $[-0.023, 0.026]$, which excludes the corresponding violent estimate of -0.0480. Henceforth, we focus our analysis on violent crimes.

Second, the effect is largest for exposure in early childhood. When we decompose the effect by childhood stage (columns 2 and 4 of Table 2), children exposed in early childhood experience the largest penalties, with effect sizes of approximately 0.05σ in mathematics, roughly twice the average effect across all ages.[11] The estimated effects of exposure in mid-childhood and adolescence are negative but smaller in magnitude and not statistically significant, although the three stage-specific coefficients are not statistically distinguishable from one another (the test of equality of the early childhood coefficient reports $p = 0.14$ with respect to mid-childhood and $p = 0.24$ with respect to adolescence). This pattern is consistent with models of skill formation that emphasize dynamic complementarity, whereby shocks in early childhood have persistent consequences that later investments can only partially offset, and supports the hypothesis that early childhood is a critical window for parental inputs (Caucutt and Lochner, 2020). The age pattern of our results could reflect both the specific sensitivity of early childhood and the elapsed time between treatment and outcome. While more years for the shock to accumulate could magnify the impact, there is also more time for offsetting responses or for the shock to dissipate, rendering ex ante ambiguous whether time will work in favor of or against early-treated children.

Third, the results are robust across a range of specifications that progressively tighten our control for confounding factors. Columns (1) and (4) of Appendix Table A.2 include only child's, mother's and father's age-at-exposure dummies, while columns (2) and (5) add year of birth and base year dummies, along with rich parental and child covariates measured in the base year. Finally, columns (3) and (6) replicate the results in Table 2 by further controlling for parish dummies, a very granular spatial unit (there are 2,248 parishes in our estimation sample). The point estimates are remarkably stable, suggesting

[11] This result implies that moderate violations of our identifying assumption would not be sufficient to invalidate our main findings. Compared to the 0.10σ age-invariant selection we estimate, any additional age-specific selection would have to amount to 47% of the reference group's gap to null the early-childhood effect.

that our matched Difference-in-Differences design is effective at addressing observable differences across stages of childhood, and that the remaining identifying variation is not driven by compositional changes correlated with parental victimization.[12]

The magnitude of our results is consistent with related studies on the causal effects of domestic violence or family victimization. Carrell and Hoekstra (2010) find that a child's own exposure to domestic violence reduces composite reading and mathematics score by approximately 0.13σ, whereas Carrell, Hoekstra and Kuka (2018) find that the spillover effect on classmates is roughly 0.02σ. In a Nordic welfare-state setting closer to our own, Bhuller et al. (2024) find a 0.08σ decline in national exam scores for children suffering domestic violence in Norway. Our estimated effect of parental victimization on child test scores (0.03σ) is therefore markedly smaller than the direct effects reported in this literature ($0.08 - 0.13\sigma$), and instead falls within the range typically associated with spillover effects (0.02σ). This comparison is consistent with our treatment capturing a different source of exposure: parental victimization originating outside the family.

We additionally show that the achievement penalties for children exposed in early childhood are largest in the middle of the achievement distribution. Appendix Figure A.5 plots the estimated treatment effects against achievement level, with dashed vertical lines marking quartile thresholds.[13] Negative effects are substantially larger for children exposed by age 5 (Panel A), with the probability of scoring above the median decreasing by 3 p.p. Regardless of the timing of parental victimization, treatment effects converge to zero in the upper achievement quartile.

[12] Our sample period includes special grading arrangements during the COVID-19 pandemic. In 2020, final overall grades replaced examination scores. In 2021 and 2022, only Danish, mathematics, and English were examined, and pupils retained their teacher-assessed grade if it was higher than their examination grade. The register does not distinguish between the two. To assess sensitivity to these arrangements, Appendix Table A.3 excludes examination years 2020–2022. The substantive conclusions remain unchanged: the early-childhood reduction in mathematics is 0.068σ, compared with 0.048σ in Table 2, and the average reduction across childhood is 0.034σ, compared with 0.027σ. Estimates for property crimes remain statistically indistinguishable from zero. Appendix Figure A.4 shows a similar age-specific pattern for violent victimization, with effects concentrated in early childhood and post-school placebo estimates remaining close to zero.

[13] We construct 80 binary indicators corresponding to scoring at or above equally spaced thresholds within the $(-2\sigma, 2\sigma)$ interval. We then estimate equation (1) separately for each indicator. Children who do not participate in the exams are coded as zero on all 80 binary indicators and are therefore counted as not reaching any of the thresholds.

4.2 Parent and Child Gender Heterogeneity

The average effects in Table 2 mask substantial heterogeneity across families. Figure 2 reports separate estimates of the impacts of parental victimization on the end-of-school examination score in mathematics by the child's and victimized parent's gender.

Achievement penalties are concentrated among paternal victimizations. While we find substantial achievement reductions for children exposed to paternal victimization (Panel A of Figure 2), maternal events have undetectable impacts (Panel B). Pooling sons and daughters, the impact of early childhood exposure is -0.09σ for paternal and null for maternal victimization (s.e. $= 0.03$ for both coefficients). Results suggest that shocks to fathers' well-being and behavior have especially detrimental consequences for the production of children's human capital, which is consistent with the larger changes in paternal criminal behavior and labor market outcomes for fathers documented in Section 5 below.

The negative impacts of paternal victimization are concentrated on early childhood exposure for sons, while daughters suffer more similar achievement reductions across childhood stages. Comparing blue (sons) and red markers (daughters) in Panel A, we find similar negative effects on mathematics score across children's genders for those exposed in early childhood (0.09σ for sons, 0.10σ for daughters). The estimate for daughters remains similar for exposure in mid childhood, while it drops to a statistically non-significant -0.03σ for sons. For exposure in adolescence, the estimate for girls only slightly weakens to -0.08σ, and is even closer to zero for sons (-0.01σ). Results suggest that daughters' achievement reacts more negatively than sons' to paternal victimization experienced in mid and late childhood. This finding also weakens the identification concerns on residual negative selection of children exposed at very young ages.[14]

[14]Previous studies document boys' greater sensitivity to disadvantaged family environments (e.g., Bertrand and Pan, 2013; Autor et al., 2019). We document a more nuanced pattern following parental criminal victimization.

4.3 Educational Progression

Beyond end-of-school achievement, parental victimization negatively affects children's educational trajectories. However, treatment effects on this margin exhibit different heterogeneity profiles from those observed for achievement in Figure 2. Figure 3 reports estimates of Equation (1) where the dependent variables measure school enrollment at age 16, the usual transition out of compulsory schooling, estimated separately by victimized parent's and child's gender. Panel A considers a "roadblock" index, i.e., a dummy variable indicating disrupted educational trajectories such as grade retention, school move, multiple spells within a year, or failure to complete a grade. Similarly to our findings on achievement, exposure in early childhood impacts this outcome more negatively across all groups.[15] However, here the largest impacts are found for maternal victimization, which significantly increases the likelihood of disrupted trajectories both for children exposed in early and mid childhood, and especially for the mother-daughter cell. In the latter group, disrupted trajectories are 5 p.p. more likely for exposure in early childhood, and 4 p.p. for exposure in mid childhood.

We find similar patterns when considering children's school enrollment at age 16. Daughters of victimized mothers exposed in early childhood are 5 p.p. more likely to spend an extra year in elementary education (Panel B), and less likely by a similar magnitude to be enrolled in the academic high school track (Panel C). We do not detect treatment effects of exposure at later stages. This negative effect on educational progression is accompanied by 0.06σ lower peer quality in the mother-daughter cell (Panel D), measured by the average ninth-grade GPA of children enrolled in the same school and year.

4.4 Well-being, Safety, and Other Child Responses

We additionally explore the impact of parental victimization on child outcomes beyond academic achievement and educational progression. First, we exploit the availability of yearly school well-being surveys and monthly school absence data for a sub-sample of

[15] Since the roadblock index considers the full compulsory school career, this pattern may be explained by the longer time elapsed since treatment. Parental victimization has more scope to impact educational trajectories if experienced before starting school or soon after.

cohorts. Leveraging their panel structure, we analyze the dynamic evolution around parental victimization by estimating the following event-study specification:

$$Y_{it} = \theta_0 + \sum_{l \in L} \alpha_l T_i \cdot \mathbb{1}[t = t_i^* + 1 + l] + \sum_{l \in L} \mu_l \cdot \mathbb{1}[t = t_i^* + 1 + l] + \psi_i + \psi_t + \zeta_{it}, \quad (3)$$

where Y_{it} denotes the well-being or absence outcome for child i in year or month t. We normalize event time such that $l = 0$ corresponds to the time of parental victimization $(t_i^* + 1)$, and estimate the coefficients α_l relative to the baseline $(l = -1)$. This specification allows the inclusion of child fixed effects (ψ_i). The coefficients α_l therefore capture the dynamic causal effects of parental victimization on children's well-being and absence relative to the control group. The event window L follows the span of the underlying data: -3 to 5 years for the well-being surveys, -10 to 24 months for absences.

Reported school well-being, especially school attachment, steadily falls following parental victimization. Panel A of Figure 4 shows that treated children are about 1 p.p. less likely to report being happy about their school in the years immediately following parental victimization, dropping to $3 - 4$ p.p. five years after the event. In Panel B, we construct a standardized index of school attachment combining the scores in several related questions such as reported happiness with class, teachers, and classroom environment. This index falls by 0.02σ in the treatment year, steadily worsening to 0.07σ five years after. In contrast, we find weaker evidence of treatment effects when combining survey questions related to psychological distress such as experiencing loneliness, headaches, or stomach aches in Panel C, with more negative values of the index indicating higher distress. We only find some statistically non-significant increase in distress in the first three years after victimization, with effects returning toward zero thereafter.

Declining school attachment is unlikely to be explained by children being kept out of school. Using the sub-sample of victimizations for which the exact date of crime is known, we estimate monthly event studies of school absences. Appendix Figure A.6 shows no impacts on total days of absence, nor on sickness-related or unauthorized absences.

Second, we measure reported well-being and own criminal outcomes at age 16 and use these variables as outcomes of our main model in Equation (1). The results reported in

Table 3 again show differential responses by child's and victimized parent's gender.

Sons experience the largest deterioration of reported well-being. While children treated in early childhood are 4 p.p. less likely to report being happy with the school on average (column 1, Panel A), the estimate increases to 7 p.p. among sons (column 2). For sons, we also find smaller negative impacts of exposure at later stages. Consistent with our evidence on achievement, reported happiness declines especially for paternal victimizations (6 p.p., column 4). Similar patterns are found when considering the psychological distress index (Panel B), with sons exposed in early childhood reporting increased distress by 0.11σ. Consistent but noisier evidence using the standardized school attachment index, and an overall well-being index that combines all survey questions, is presented in Panels A and B of Appendix Table A.4.

In contrast, exposed daughters report higher perceived safety and are less likely to suffer own victimization. Daughters experiencing parental victimization in early childhood are 5 p.p. more likely to report feeling safe (Panel C of Table 3) and 4 p.p. less likely to report being bullied (Panel D). For bullying, a similar reduction is also detected following exposure in adolescence. These results are consistent with the estimated *reductions* in psychological distress among daughters in Panel B. Survey evidence is matched by administrative records showing a 0.4 p.p. *lower* likelihood of police-recorded victimization for daughters (Panel E), regardless of the childhood stage at exposure.[16] One possible explanation is that parental victimization induces more protective behavior toward daughters, reducing their subsequent exposure to risky environments, although we do not directly observe parenting responses.

Taken together, the results show that parental violent victimization has lasting consequences for children, especially if experienced in early childhood, but that these consequences differ sharply across outcome margins and family members. Achievement losses are driven primarily by paternal victimization, with effects extending to later exposure ages particularly for daughters, whereas maternal victimization is more strongly associated with disruptions in daughters' subsequent educational progression. Children also

[16] In contrast, we find no statistically detectable effect on the probability of being charged with a violent offense by age 16 (Panel D of Appendix Table A.4).

respond differently outside education: boys experience clearer deteriorations in subjective well-being, while girls become less exposed to bullying and subsequent police-recorded victimization and report feeling safer. These patterns therefore point away from a single dimension of child vulnerability or a common mechanism.[17] In the next section, we examine how victimization affects parental health, labor market outcomes, behavior, and family stability, and whether the dimensions of family disruption predicted to be most severe are also associated with larger educational losses.

5 Parental and Family Responses

5.1 Effects on Parental and Family Outcomes

We analyze the dynamic evolution of parental outcomes following victimization. We estimate event study models as in Equation (3) separately by childhood developmental stage and victimized parent's gender. The event window L spans -5 to 10 years. We present the results in Figure 5.

As a first-order effect, violent victimization impairs parental physical health. Panel A of Figure 5 shows very large spikes in the likelihood of visiting a hospital in the year of victimization. These spikes are more pronounced for fathers than for mothers, and especially large for exposure in early childhood (about 30 p.p. for fathers against 15 p.p. for mothers). The initial effects are not persistent, as hospitalizations quickly converge to levels similar to those observed in the control group. Contacts with a psychologist increase sharply but moderately for the victimized parent, with relatively larger impacts for mothers (4 p.p. against 2 p.p. for fathers, Panels A and B of Appendix Figure A.8). Although these results may suggest weaker responses for mental health, they could also reflect willingness or access to mental healthcare.

An important institutional feature helps interpret the mental-health care responses. Under Denmark's subsidized psychologist scheme, general practitioners refer eligible pa-

[17] We additionally investigate children's contacts with a psychologist as an outcome in Equation (3). We find no evidence of parental victimization impacts on this margin (see Panel C of Appendix Figure A.8).

tients to specialist psychological treatment at reduced out-of-pocket cost, and victims of violent crime are a designated referral group. [18] A victimized parent thus becomes eligible for subsidized specialist care on the basis of the victimization itself, while their relatives lack a dedicated route. Consistent with this asymmetry, we do not detect any effect of parental victimization on *children*'s contacts with a psychologist (Panel C of Appendix Figure A.8). These children do not automatically qualify for subsidized psychological care on the basis of parental victimization alone.

Victimization hurts parental labor market outcomes, in line with findings by Bindler and Ketel (2022) and Dustmann and Mertz (2026). Panel B of Figure 5 shows immediate and persistent penalties in total yearly labor earnings, which increase in magnitude over time. On average, earnings losses are larger for paternal victimizations – close to 40k DKK ten years after treatment – but the gap is smallest for early childhood exposure, where the divergence with maternal outcomes is relatively small and only visible 5 to 10 years after victimization.[19] The earnings penalties mostly operate through the intensive margin for fathers, especially for victimizations in early childhood (i.e., lower earnings conditional on working, Panel B of Appendix Figure A.7). In contrast, earnings penalties are mostly explained by extensive-margin adjustments for mothers (i.e., labor market attachment, Panel A). Larger labor market penalties for fathers, especially for exposure in mid or late childhood, are consistent with the larger achievement reductions for paternal victimizations presented in Figure 2.

Extending recent work on the effects of victimization on victims' own outcomes (Bindler and Ketel, 2022; Dustmann and Mertz, 2026), Panel C of Figure 5 shows that treated parents, and fathers in particular, become more likely to be charged with a violent crime in

[18] Treatment is provided under the practice-sector agreement on psychological care (*praksisoverenskomst om psykologhjælp*) (Regionernes Lønnings- og Takstnævn and Dansk Psykolog Forening, 2019), which specifies the groups eligible for a general-practitioner referral. Victims of robbery, violence, or rape are one such group; over our sample period, they face a reduced co-payment, with treatment becoming free of charge only in 2025 (Region Sjælland, 2025), after our victimization observation window. Eligibility extends to indirect victims only where they were themselves exposed to real danger and explicitly excludes witnesses and bystanders; there is no referral category for the relatives or children of a crime victim, who can qualify only through the scheme's general routes (e.g., bereavement or a diagnosed disorder).

[19] Our matching design conditions on earnings in the base year and its three preceding lags, so the treated and control series converge mechanically at $l = -1$. The pre-event coefficients in Panel B should be read as that convergence rather than as a parallel-trends test, and the post-event path is identified relative to the $l = -1$ normalization.

the years after victimization relative to their matched controls, especially for exposure in early childhood (+1 p.p., against 0.5 p.p. for mothers). We read this as suggestive rather than causal: because we restrict the sample to parents with no charges up to the year of victimization, pre-victimization charges are zero by construction, so the flat pre-event path is mechanical. We therefore treat this pattern as an association consistent with a behavioral response to victimization.

Finally, we find moderate increases in the likelihood of parental divorce or moving to a different parish shortly after the report, but only for maternal victimizations and for exposure in early childhood (Panel D). Maternal victimizations in this stage lead to a 5 p.p. higher likelihood of moving, and a $1-2$ p.p. higher likelihood of parental divorce. These patterns match the heterogeneity analysis in Figure 3, suggesting that negative impacts on daughters' educational progression may be associated with disruptions to family stability.

Taken together, these results show that parental victimization generates disruptions across several dimensions, but that the nature of these responses differs markedly by the gender of the victimized parent. Paternal victimization is followed by larger and more persistent labor-market and behavioral responses, which parallels the stronger achievement losses documented for children of victimized fathers. Maternal victimization, particularly in early childhood, is instead more closely associated with residential mobility and family dissolution, echoing the disruptions in daughters' subsequent educational progression. These correspondences are suggestive, however, and do not establish that any single parental response mediates the effects on children. We therefore next exploit heterogeneity in predicted parental responses to ask whether children in families expected to experience larger disruptions along these dimensions also suffer larger educational losses.

5.2 Predicted Responses and Children's Achievement Effects

Following the approach in Britto, Melo and Sampaio (2022) and Schmieder, von Wachter and Heining (2023), we project individual-level difference-in-differences estimates of parental outcomes onto *baseline*, pre-victimization characteristics, and use these predicted values

to stratify the sample. Because the projection relies only on information determined before treatment, this procedure avoids conditioning on a realized post-treatment mediator and thus mitigates the endogeneity problem of conditioning on outcomes, at the cost of identifying heterogeneity in effects across *predicted* family responses rather than formally partitioning the total effect into mechanism-specific shares.[20]

Achievement reductions following parental victimization are most strongly associated with labor market attachment, the risk of subsequent parental criminal behavior, and the deterioration of family relational and residential stability. Using mathematics examination scores as the outcome, we estimate equation (1) separately by tercile of predicted change in parental outcomes following victimization. Figure 6 plots estimated treatment effects on achievement by tercile against the average predicted change in the corresponding parental outcome. For exposure in early childhood, reductions of 0.05 to 0.10σ are found where the victimized parent's worked weeks fall most (Panel A), where the risk of a charge for violent crime rises (Panel B), where the risk of parental divorce rises most (Panel C), and where the risk of moving to a different parish rises (Panel D). Similar patterns hold for exposure at later stages, especially adolescence. The relationship is not monotone in Panels B and D, where the middle tercile is estimated to be at least as negative as the tercile with the largest predicted increase, so the figure separates families predicted to experience little disruption from the rest rather than tracing a dose-response. The tercile-specific estimates are imprecise, and we do not test them against one another.

The association of treatment effects with health deterioration is, instead, less clear. Achievement reductions are uncorrelated with predicted impacts on parental hospitalization (Panel A of Appendix Figure A.9). For mental health, children exposed in early childhood suffer larger reductions when predicted psychologist contacts *decrease* most,

[20] We deliberately depart from a standard mediation analysis, which would decompose the total effect of parental victimization into direct and indirect components operating through each realized post-treatment outcome (e.g., Imai, Keele and Tingley, 2010). Such an approach requires assuming that mediators are themselves unconfounded conditional on observables, a sequential ignorability assumption that is particularly hard to justify here, since the very channels we study (e.g., labor market detachment, divorce, criminal behavior) are outcomes of victimization and therefore correlated with the same unobserved shocks that may independently affect children's achievement. Controlling for realized post-treatment mediators would introduce bad-control bias into our estimates (Heckman and Pinto, 2015), and the fact that our candidate mechanisms are numerous, mutually correlated, and jointly determined makes a clean additive decomposition ill-defined in our setting.

both when considering parental (Panel B) and child contacts (Panel C). This result may suggest a protective role of accessing mental healthcare. Exposure in adolescence, instead, generates more severe reductions when predicted contacts are *higher*, possibly marking more severe mental health issues.

We find little support for a purely income-based explanation. Although negative treatment effects are larger when predicted family income drops most for children exposed in early childhood (Panel A of Appendix Figure A.10), we find several contrasting pieces of evidence. First, this pattern is not present at later childhood stages. Second, predicted unemployment benefit receipt is uncorrelated with treatment effects (Panel B). Third, heterogeneity by breadwinner status offers no support for income-based explanations. If anything, Appendix Table A.5 shows that, for paternal victimizations, achievement penalties are larger when the victimized father is not the household's main earner.

Taken together, the predicted-response analysis points to a broad pattern of family disruption rather than to a single dominant mechanism. Children experience larger educational losses in families predicted to respond to victimization through weaker labor-market attachment, greater subsequent criminal behavior, and greater relational or residential instability, while the evidence is considerably weaker for acute physical-health deterioration or losses in household resources. These patterns mirror the parental responses documented in Section 5.1 and the heterogeneous effects on child outcomes presented in Section 4, but they should not be interpreted as a causal decomposition of the total effect. Rather, they show that the intergenerational consequences of parental victimization are concentrated in families whose pre-treatment characteristics predict more disruptive post-victimization trajectories, suggesting that the persistence of the shock within the family environment is more consequential for children than the immediate severity of the event alone.

6 Conclusion

Criminal victimization can have persistent effects on victims' employment and earnings (Bindler and Ketel, 2022). Because childhood circumstances shape later human capital (Almond, Currie and Duque, 2018), a crime against a parent may also affect the next

generation. This paper provides causal evidence that parental criminal victimization originating outside the family harms children's human capital. Using Danish administrative data and a matched difference-in-differences design, we show that parental violent victimization reduces end-of-school mathematics scores by 0.03 standard deviations on average, reaching approximately 0.09 following paternal victimization in early childhood. We find no detectable effect of property victimization.

The consequences differ across parents and children. Early paternal victimization reduces achievement for both sons and daughters, with losses extending to later exposure ages for daughters. Maternal victimization disrupts educational progression, particularly among daughters. Sons experience lower well-being, whereas daughters report greater perceived safety and have lower subsequent police-recorded victimization. Educational losses are larger in families predicted, from pre-victimization characteristics, to experience weaker parental labor-market attachment, more violent-crime charges, and greater relational or residential instability. These patterns suggest several possible channels but do not identify their causal contributions.

Our findings identify an intergenerational dimension of crime's social costs, complementing efforts to extend conventional accounting frameworks (Heeks et al., 2018) to witnesses (Facchetti, Rasul and Sijthoff, 2026). The concentration of achievement losses in early childhood highlights the potential value of timely support for victimized families. We observe increased psychologist contacts among parents but not children following victimization.

Evaluating whether support for victims' children can mitigate educational losses is an important next step. Violence against a parent can carry an educational cost for the next generation. These spillovers connect two central economic questions: how family shocks shape human capital, and how the social costs of crime should be measured. The evidence presented in this paper suggests that evaluations of crime prevention and support for victimized families should therefore follow children's educational trajectories alongside the recovery of the victimized parent.

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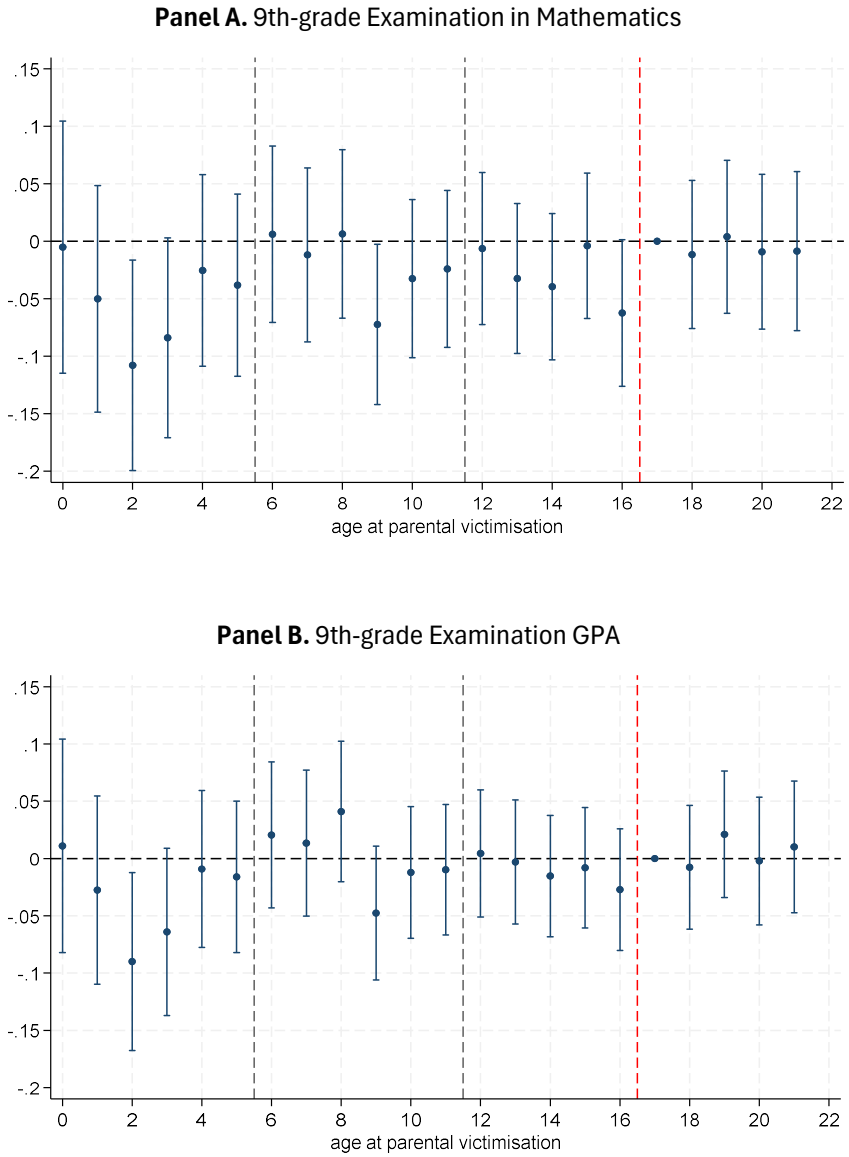


Figure 1: Impacts of Parental Victimization by age at treatment – Violent Crimes

Note. The figure plots estimates of λ_k coefficients in Equation (2) on end-of-school (age 16) examination score. Panel A and Panel B consider mathematics score only or the grade point average (GPA) across all subjects, respectively. The sample is restricted to parental violent victimizations. The estimation sample includes exposed children and their matched control peers. The x-axis represents age at real or placebo parental victimization. Childhood stages are visually separated by dotted vertical lines, with red lines separating causal effects of parental victimization from placebo effects on children exposed after end-of-school achievement is realized. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

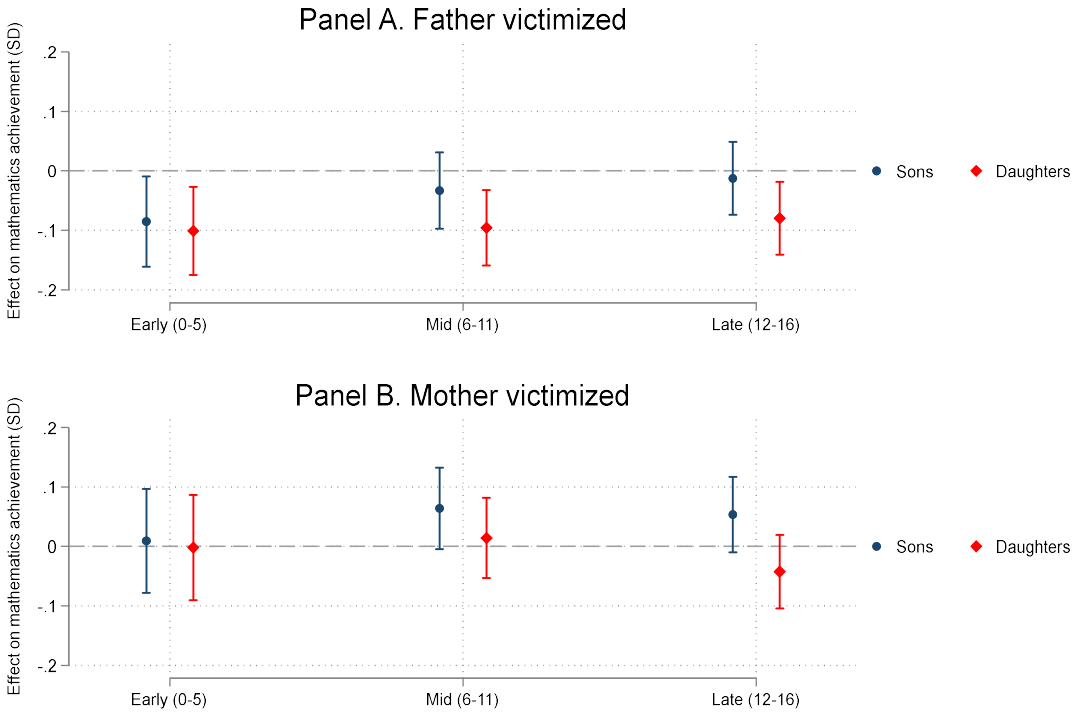


Figure 2: Heterogeneous Impacts of Parental Victimization – Violent Crimes

Note. The figure plots estimates of δ_s coefficients in Equation (1) on end-of-school examination score in mathematics. Estimated specifications are similar to column (2) of Table 2. The x-axis represents the childhood stage at parental victimization. Sample is restricted to paternal victimizations in Panel A and maternal victimizations in Panel B. Within each panel, estimates for sons are reported in blue and estimates for daughters are reported in red. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

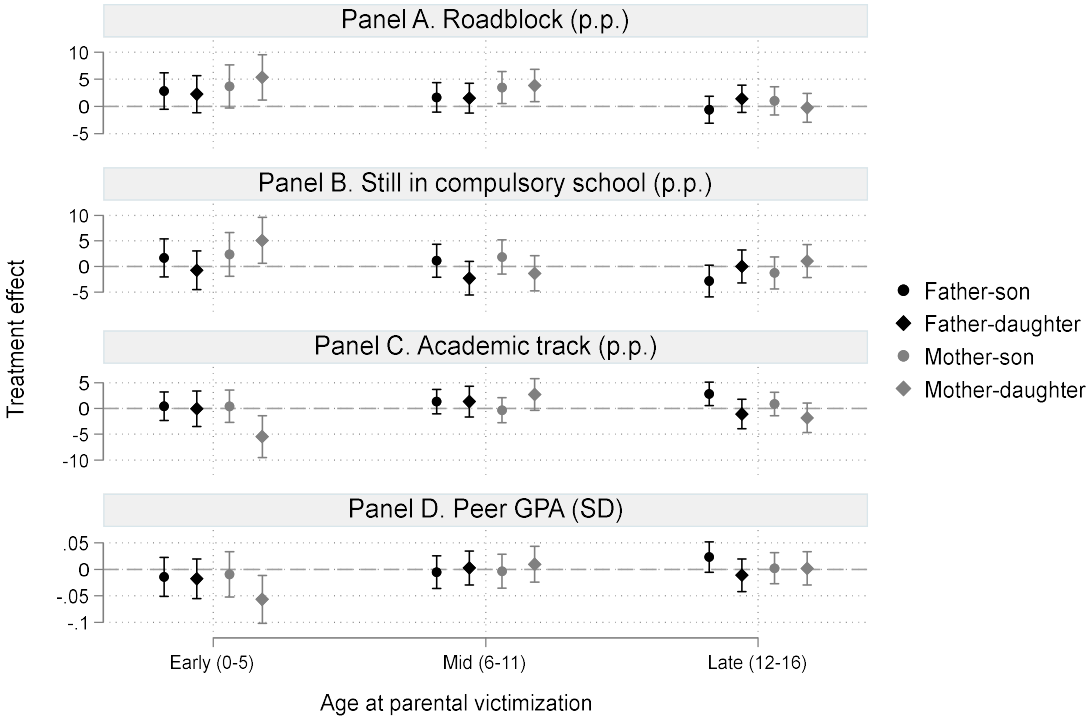


Figure 3: Parental Victimization and Educational Progress – Violent Crimes

Note. The figure plots estimates of δ_s coefficients in Equation (1) on end-of-school enrollment outcomes. Estimated specifications are similar to column (2) of Table 2. The x-axis represents the childhood stage at parental victimization. The dependent variables, measured at age 16, are: a dummy variable indicating grade retention, school move, multiple spells within a year, or failure to complete a grade (Panel A); a dummy variable indicating that the child is still enrolled in compulsory school (Panel B); a dummy variable indicating enrollment in the academic high school track (Panel C); the average ninth-grade GPA at the school where enrolled (Panel D). The y-axis is scaled in percentage points in Panels A-C and in standard deviation units in Panel D. Each panel reports separate estimates by victimized parent's and child's gender. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

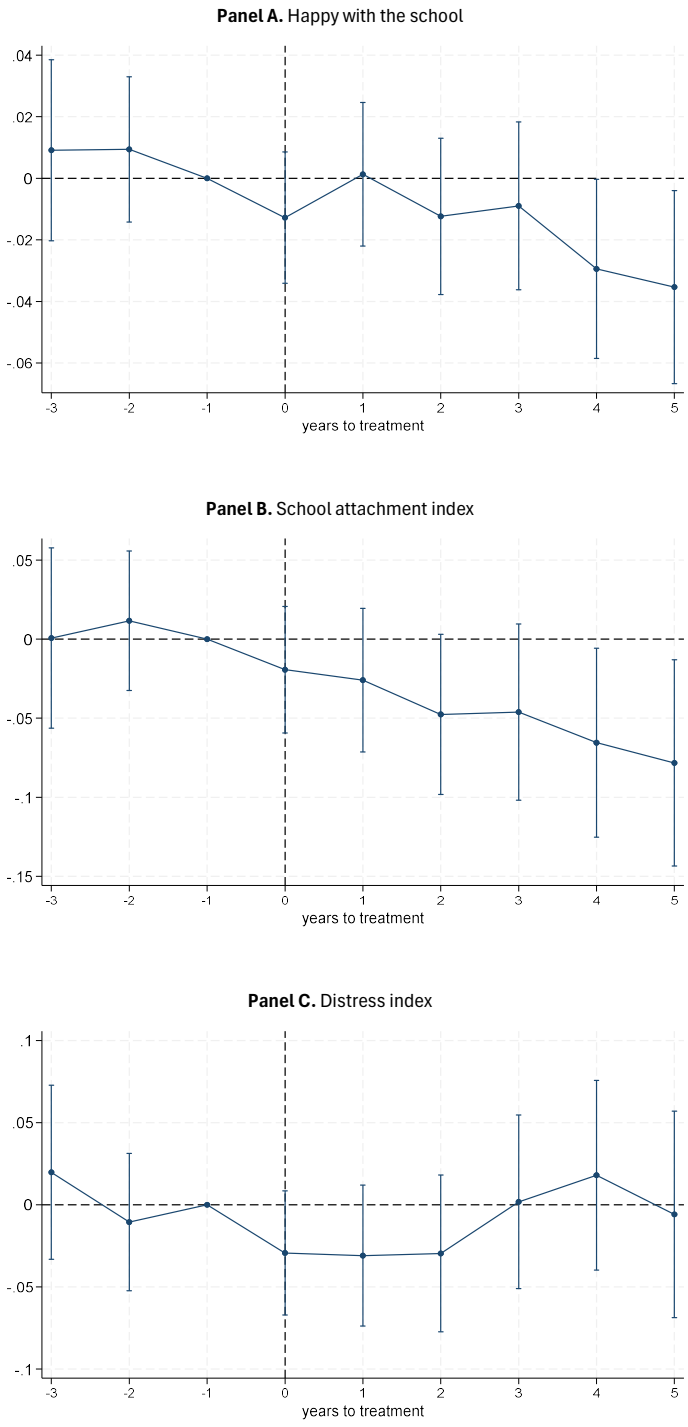


Figure 4: Event Studies of child's school well-being

Note. The figure plots event studies of child school well-being. We plot estimates of α_l coefficients in Equation (3) against year relative to parental victimization, measured in the x-axis. The sample includes children exposed to parental victimization for violent crimes and their matched control peers – limited to birth cohorts 1999-2007 to reflect the time span of the available data (2015-2025). Dependent variables are: an indicator for reporting to be happy with the school (Panel A); a standardized index of survey questions related to school attachment, including the one considered in Panel A (Panel B); a standardized index of survey questions related to psychological distress (Panel C, lower values indicate higher distress). Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

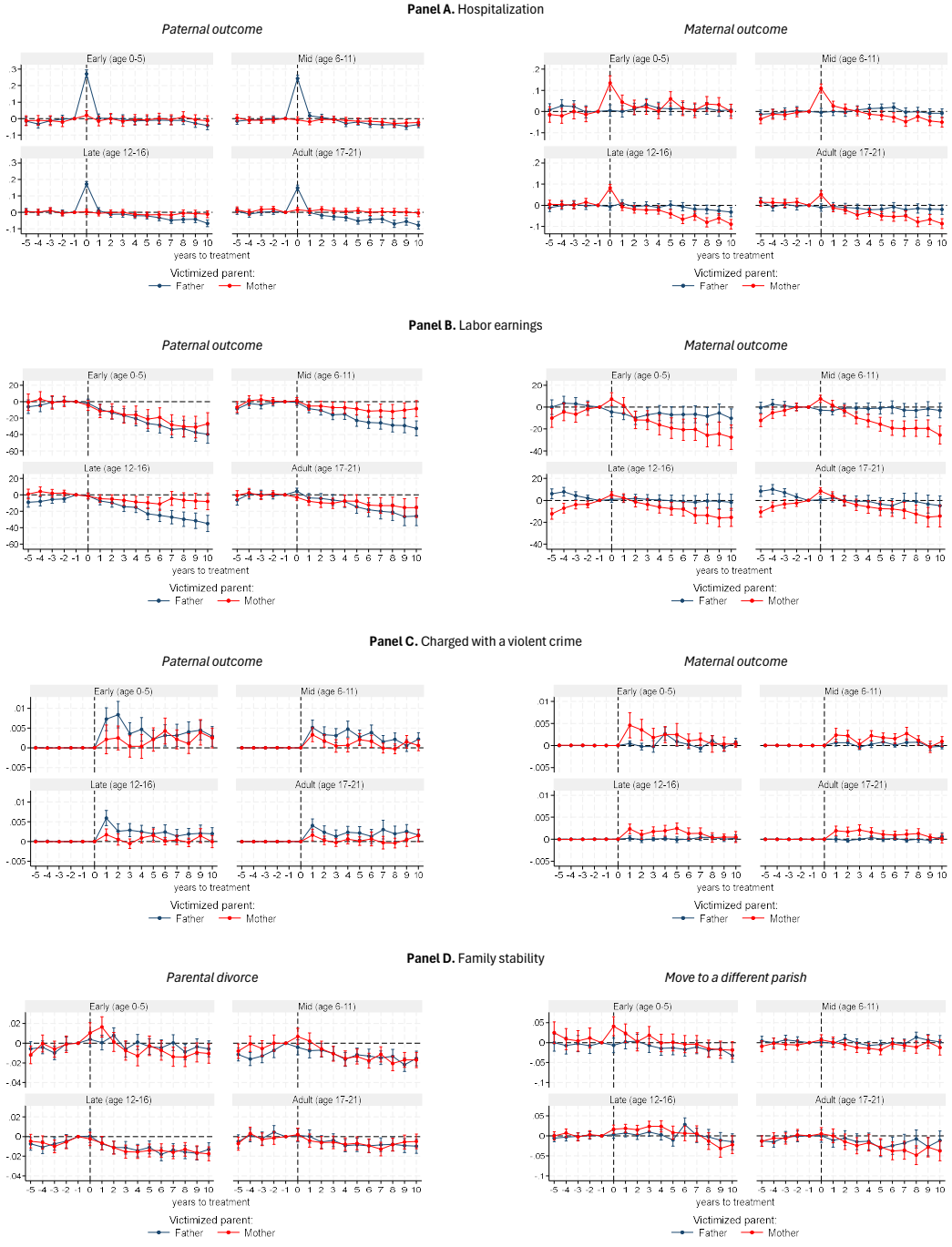


Figure 5: Parental Event Studies

Note. The figure plots estimates of α_l coefficients in Equation (3) against year relative to parental victimization, measured in the x-axis. The sample includes children exposed to parental victimization for violent crimes and their matched control peers. Dependent variables are: an indicator for visiting a hospital in the year (Panel A, coded to zero if the person is not found in the hospitalization register); total annual labor earnings (thousands of DKK, Panel B); an indicator for being charged with a violent crime in the year (Panel C); indicators for parental divorce or child moving to a different parish in the year (Panel D). Separate estimates are plotted by childhood stage at treatment in subpanels. In Panels A-C, left-hand graphs consider paternal outcomes and right-hand graphs consider maternal outcomes. Red connected markers refer to maternal victimizations, blue connected markers refer to paternal victimizations. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

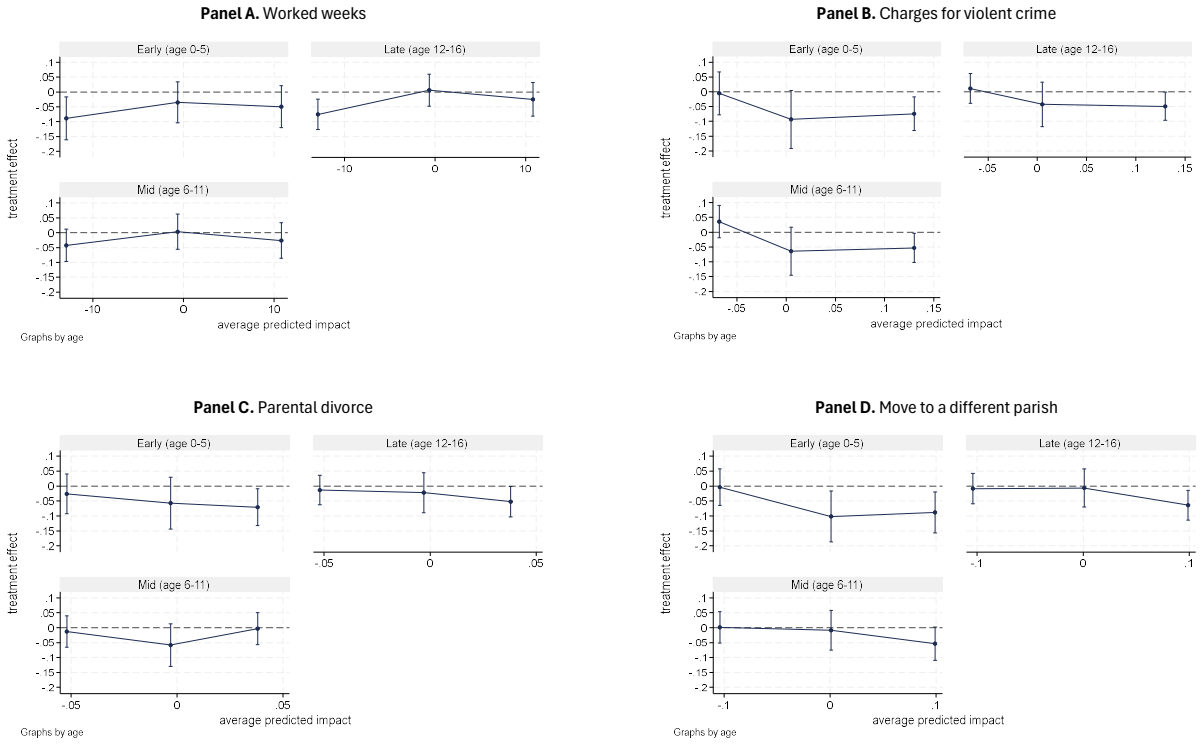


Figure 6: Heterogeneity by predicted victimization impact on parental outcomes

Note. The figure plots heterogeneous effects of parental victimization on children's learning by predicted changes in parental outcomes after victimization. Graphed are coefficients from Equation (1) estimated separately by predicted change tercile, with specifications analogous to column (2) of Table 2. The dependent variable is end-of-school examination score in mathematics. Changes in parental outcome are computed as predicted values of individual-level difference-in-differences around victimization from a regression on baseline parental and children characteristics. We compare the average outcome over the ten years following victimization with its value in the base year (the year before victimization). The considered predicted response is: parental worked weeks in the year (Panel A); parental charges for violent crime (Panel B); an indicator for parental divorce in the year (Panel C); an indicator for child moving to a different parish in the year (Panel D). The x-axis measures the average predicted change in the considered tercile. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

Table 1: Descriptive Statistics – Violent Crimes

	All			Age 0-5	Age 6-11	Age 12-16	Age 17-21
	C mean (1)	T mean (2)	SMD (3)	SMD (4)	SMD (5)	SMD (6)	SMD (7)
				Panel A. Pre-matching			
Age	10.126	11.535	-0.246	-0.148	-0.089	-0.061	0.035
Female	0.487	0.489	-0.004	0.001	-0.018	0.007	-0.006
Danish	0.929	0.902	0.098	0.150	0.141	0.078	0.044
Father's age at first birth	28.973	28.618	0.041	0.074	0.042	0.016	0.033
Mother's age at first birth	28.260	27.546	0.163	0.176	0.181	0.147	0.149
Father's education (months)	151.974	150.533	0.030	0.081	0.010	0.000	0.024
Mother's education (months)	161.837	160.496	0.043	0.088	0.025	0.013	0.032
Father employed	0.826	0.813	0.035	0.050	0.018	0.022	0.022
Mother employed	0.861	0.831	0.083	0.115	0.107	0.088	0.064
Father's earnings ('000s DKK)	434.775	394.851	0.132	0.198	0.151	0.142	0.135
Mother's earnings ('000s DKK)	316.920	314.565	0.011	0.105	0.053	0.052	0.048
Mother's weeks worked	19.636	23.441	-0.159	-0.170	-0.206	-0.075	0.065
Father's weeks worked	18.572	23.028	-0.184	-0.224	-0.249	-0.092	0.048
Family income ('000s DKK)	960.435	926.800	0.026	0.052	0.043	0.039	0.044
Danish families in local area	0.906	0.879	0.207	0.276	0.258	0.179	0.125
Education in local area	170.542	170.307	0.029	0.022	0.023	0.046	0.085
Treated in local area	0.004	0.010	-0.800	-1.076	-0.905	-0.726	-0.649
Outcome variables							
No exam	0.078	0.093	-0.053	-0.068	-0.035	-0.047	-0.074
Mathematics score	0.144	-0.071	0.221	0.270	0.234	0.212	0.194
GPA	0.108	-0.043	0.180	0.218	0.193	0.171	0.156
N	20,082,646	54,315	20,136,961	4,246,368	5,978,105	5,347,126	4,565,362
				Panel B. Post-matching			
Age	11.527	11.527	0.000	0.000	0.000	0.000	0.000
Female	0.488	0.488	0.000	0.000	0.000	0.000	0.000
Danish	0.898	0.902	-0.012	-0.015	-0.009	-0.016	-0.008
Father's age at first birth	28.501	28.620	-0.014	-0.014	-0.020	-0.015	-0.008
Mother's age at first birth	27.474	27.548	-0.017	-0.007	-0.022	-0.021	-0.012
Father's education (months)	150.140	150.519	-0.008	-0.014	-0.005	-0.018	0.003
Mother's education (months)	160.291	160.484	-0.006	-0.013	-0.006	-0.007	-0.001
Father employed	0.811	0.813	-0.006	-0.007	-0.001	-0.008	-0.008
Mother employed	0.827	0.830	-0.008	-0.015	-0.009	-0.012	0.002
Father's earnings ('000s DKK)	390.461	394.920	-0.015	-0.015	-0.010	-0.030	-0.003
Mother's earnings ('000s DKK)	312.659	314.400	-0.008	-0.020	-0.016	-0.005	0.000
Mother's weeks worked	23.137	23.377	-0.010	-0.012	-0.008	-0.012	-0.010
Father's weeks worked	22.813	22.985	-0.007	0.006	-0.010	-0.013	-0.003
Family income ('000s DKK)	916.916	924.437	-0.007	0.003	-0.013	-0.011	-0.004
Danish families in local area	0.873	0.879	-0.042	-0.039	-0.040	-0.046	-0.041
Education in local area	170.265	170.309	-0.005	-0.005	-0.010	0.002	-0.010
Treated in local area	0.009	0.010	-0.084	-0.058	-0.073	-0.089	-0.096
Outcome variables							
No exam	0.086	0.093	-0.026	-0.031	-0.023	-0.026	-0.025
Mathematics score	0.050	-0.072	0.124	0.149	0.126	0.126	0.107
GPA	0.030	-0.043	0.087	0.116	0.084	0.087	0.076
N	54,027	54,027	108,054	14,766	28,308	34,118	30,862

Note. The table reports descriptive statistics comparing the average characteristics of children exposed to parental victimization (column 2) with untreated or matched-control children (column 1, Panel A and Panel B, respectively). Columns (3)-(7) display standardized mean differences, computed over the full sample (column 3) or separately by stage of childhood (columns 4-7). The sample is restricted to parental violent victimizations.

Table 2: Impacts of Parental Victimization on End-of-School Achievement

	Mathematics		GPA	
	(1)	(2)	(3)	(4)
	Panel A. Violent Crimes			
Treated	-0.1026***	-0.1025***	-0.0666***	-0.0666***
	(0.0113)	(0.0113)	(0.0095)	(0.0095)
Treated X Exposed	-0.0270**		-0.0154	
	(0.0133)		(0.0111)	
Treated X Early (age 0-5)		-0.0480**		-0.0373**
		(0.0199)		(0.0167)
Treated X Mid (age 6-11)		-0.0188		-0.0055
		(0.0164)		(0.0137)
Treated X Late (age 12-16)		-0.0248		-0.0142
		(0.0153)		(0.0128)
N	96,808	96,808	97,909	97,909
	Panel B. Property Crimes			
Treated	-0.0339***	-0.0339***	-0.0006	-0.0006
	(0.0076)	(0.0076)	(0.0063)	(0.0063)
Treated X Exposed	-0.0046		-0.0089	
	(0.0088)		(0.0073)	
Treated X Early (age 0-5)		0.0014		-0.0047
		(0.0126)		(0.0103)
Treated X Mid (age 6-11)		-0.0132		-0.0115
		(0.0106)		(0.0088)
Treated X Late (age 12-16)		0.0001		-0.0086
		(0.0101)		(0.0083)
N	222,504	222,504	224,653	224,653
Parent and child age dummies	X	X	X	X
Additional covariates	X	X	X	X
Parish dummies	X	X	X	X

Note. The table reports estimates of δ_1 ("Treated") and δ_s (interactions) coefficients in Equation (1) on end-of-school (age 16) examination scores. The sample is formed by children exposed to parental victimization and their matched control peers. Panel A and Panel B restrict the sample to violent and property parental victimizations, respectively. Average estimates across childhood are reported in columns (1) and (3), while columns (2) and (4) report estimates by childhood stage at real or placebo parental victimization. All specifications include dummies for year, cohort, and age at real or placebo parental victimization, parental and child characteristics in the base year, and dummies for child's parish in the base year. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Impacts of Parental Victimization Beyond Education – Violent Crimes

	All	Son	Daughter	Father	Mother
	(1)	(2)	(3)	(4)	(5)
			Panel A. Happy with the school		
Treated X Early (age 0-5)	-0.0397**	-0.0740***	-0.0092	-0.0567**	-0.0146
	(0.0159)	(0.0229)	(0.0237)	(0.0229)	(0.0245)
Treated X Mid (age 6-11)	-0.0259*	-0.0542**	0.0054	-0.0193	-0.0341
	(0.0151)	(0.0218)	(0.0223)	(0.0223)	(0.0219)
Treated X Late (age 12-16)	-0.0197	-0.0430*	0.0111	-0.0118	-0.0288
	(0.0154)	(0.0221)	(0.0229)	(0.0229)	(0.0221)
N	30,504	15,717	14,282	16,187	13,836
			Panel B. Distress index		
Treated X Early (age 0-5)	-0.0099	-0.1145**	0.1452**	-0.0615	0.0986*
	(0.0366)	(0.0505)	(0.0567)	(0.0518)	(0.0568)
Treated X Mid (age 6-11)	-0.0266	-0.0867*	0.0710	-0.0602	0.0171
	(0.0345)	(0.0486)	(0.0525)	(0.0503)	(0.0510)
Treated X Late (age 12-16)	-0.0005	-0.0765	0.1096**	-0.0583	0.0561
	(0.0350)	(0.0490)	(0.0532)	(0.0517)	(0.0506)
N	30,201	15,481	14,218	16,031	13,682
			Panel C. Report feeling safe		
Treated X Early (age 0-5)	0.0170	-0.0141	0.0530**	0.0120	0.0286
	(0.0152)	(0.0203)	(0.0244)	(0.0219)	(0.0233)
Treated X Mid (age 6-11)	-0.0012	-0.0200	0.0274	-0.0154	0.0214
	(0.0144)	(0.0192)	(0.0230)	(0.0214)	(0.0214)
Treated X Late (age 12-16)	-0.0032	-0.0110	0.0135	-0.0260	0.0161
	(0.0145)	(0.0195)	(0.0232)	(0.0219)	(0.0208)
N	29,915	15,320	14,103	15,882	13,549
			Panel D. Report being bullied		
Treated X Early (age 0-5)	-0.0062	0.0216	-0.0371*	0.0189	-0.0312
	(0.0147)	(0.0209)	(0.0222)	(0.0210)	(0.0228)
Treated X Mid (age 6-11)	0.0124	0.0238	-0.0010	0.0262	-0.0085
	(0.0140)	(0.0202)	(0.0209)	(0.0205)	(0.0210)
Treated X Late (age 12-16)	-0.0099	0.0100	-0.0350*	0.0202	-0.0369*
	(0.0141)	(0.0203)	(0.0212)	(0.0210)	(0.0207)
N	29,806	15,286	14,018	15,828	13,494
			Panel E. Victimized by age 16		
Treated X Early (age 0-5)	-0.0009	0.0015	-0.0040**	-0.0012	-0.0013
	(0.0013)	(0.0020)	(0.0017)	(0.0019)	(0.0018)
Treated X Mid (age 6-11)	-0.0012	0.0018	-0.0042**	-0.0017	-0.0010
	(0.0013)	(0.0020)	(0.0017)	(0.0019)	(0.0019)
Treated X Late (age 12-16)	-0.0029**	-0.0009	-0.0046**	-0.0045**	-0.0017
	(0.0015)	(0.0022)	(0.0020)	(0.0021)	(0.0021)
N	94,134	48,299	45,630	49,452	44,459
Parent and child age dummies	X	X	X	X	X
Additional covariates	X	X	X	X	X
Parish dummies	X	X	X	X	X

Note. The table reports estimates of δ_s coefficients in Equation (1) on child reported well-being and own victimization. Estimated specifications are similar to column (2) of Table 2. For well-being, we consider binary outcomes indicating that the most positive answer was reported for happiness with the school (Panel A) or perceived safety (Panel C), or that the most negative answer was reported for perceived bullying (Panel D). Scores in survey items about loneliness, being afraid of being made fun of, stomachaches and headaches are combined in a standardized distress index in Panel B, with lower values indicating greater distress. Panel E considers a binary outcome indicating own police-recorded victimization. The full sample is used in column (1). The sample is restricted to sons in column (2), daughters in column (3), paternal victimizations in column (4) and maternal victimizations in column (5). Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix (For On-line Publication Only)

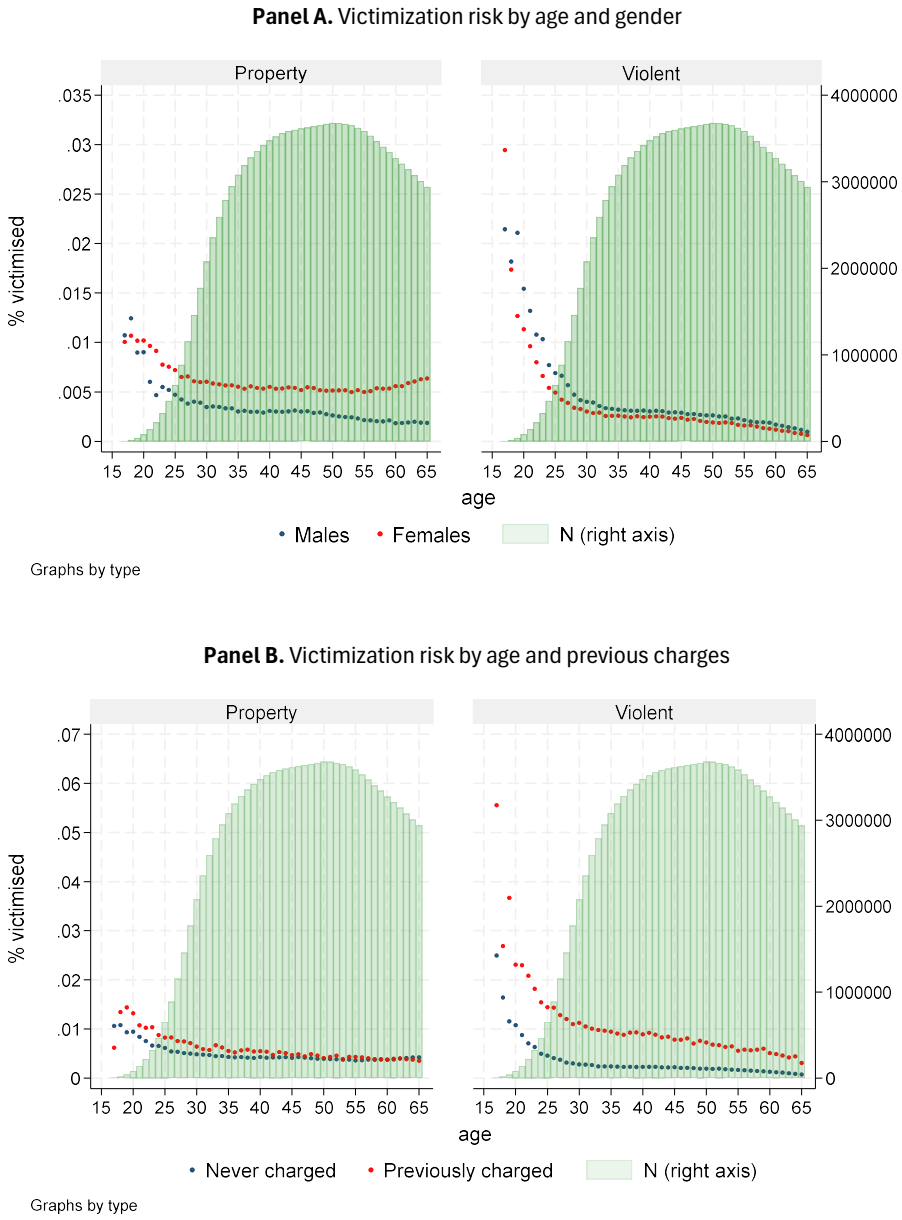


Figure A.1: Victimization risk of parents

Note. The figure plots parents' victimization risk by age, separately by gender (Panel A) or involvement with justice (Panel B). The working sample is the full population of parents aged 17-65 in each year from 1996 to 2022, and markers report the share of parents reporting a crime in the year when their age turns to the x-axis value. In Panel A, red and blue markers refer to mothers and fathers, respectively. In Panel B, red and blue markers refer to parents who are previously charged or never charged as suspect perpetrator of a reported crime, respectively. Green bars plot the frequency of observations, measured in the right-hand y-axis.

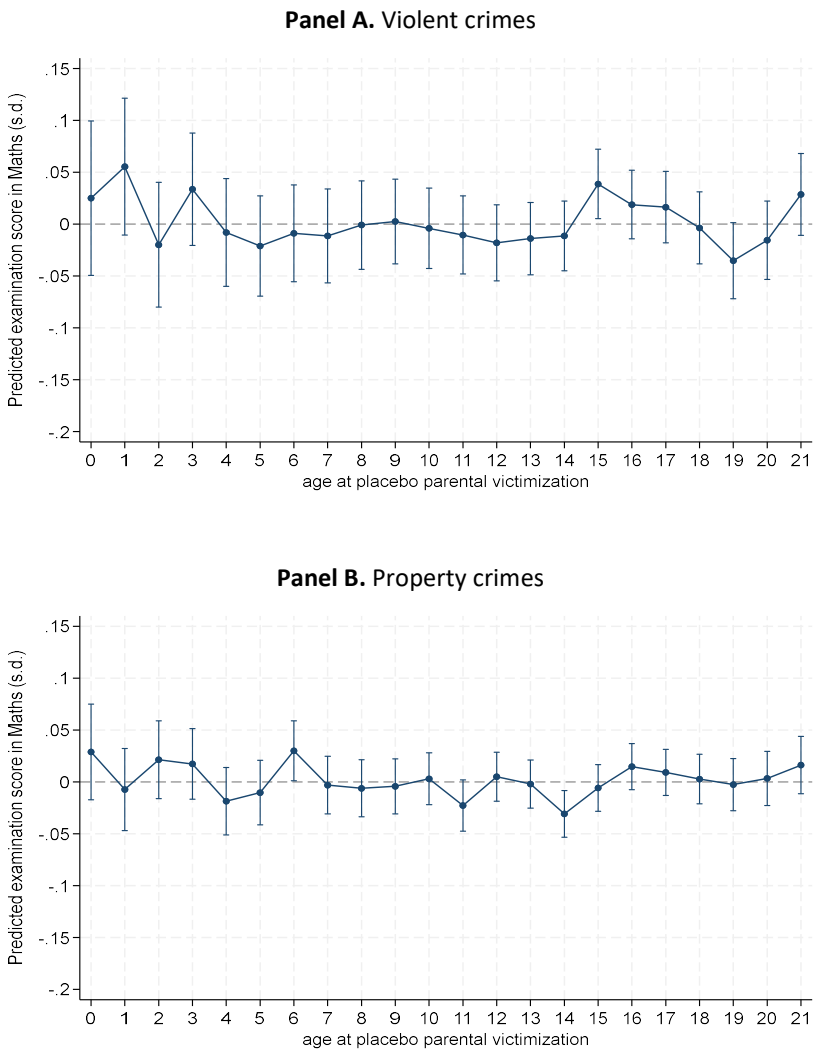


Figure A.2: Achievement of matched control children

Note. The figure plots control children's 9th-grade examination score in mathematics against age at placebo parental victimization, separately for the violent crime (Panel A) and the property crime (Panel B) samples. Examination score is standardized by year and computed as residual from a regression on year and cohort dummies. Spikes indicate 95% pointwise confidence intervals based on a regression of residualized scores on age-at-placebo-victimization indicators, with standard errors clustered at the family level. The intervals do not account for the uncertainty from estimating the residuals.

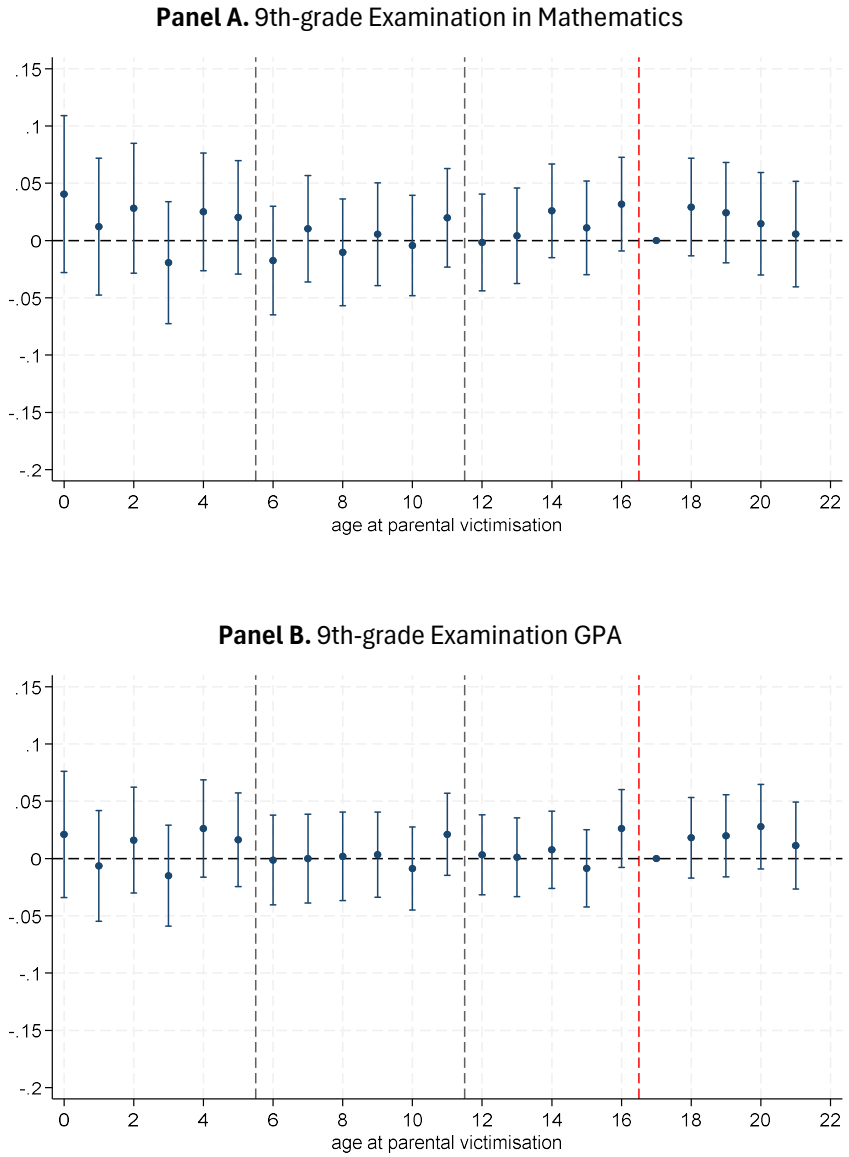


Figure A.3: Impacts of Parental Victimization by age at treatment – Property Crimes

Note. The figure plots estimates of λ_k coefficients in Equation (2) on end-of-school (age 16) examination score. Panel A and Panel B consider mathematics score only or the grade point average (GPA) across all subjects, respectively. The sample is restricted to parental property victimizations. The estimation sample includes exposed children and their matched control peers. The x-axis represents age at real or placebo parental victimization. Childhood stages are visually separated by dotted vertical lines, with red lines separating causal effects of parental victimization from placebo effects on children exposed after end-of-school achievement is realized. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

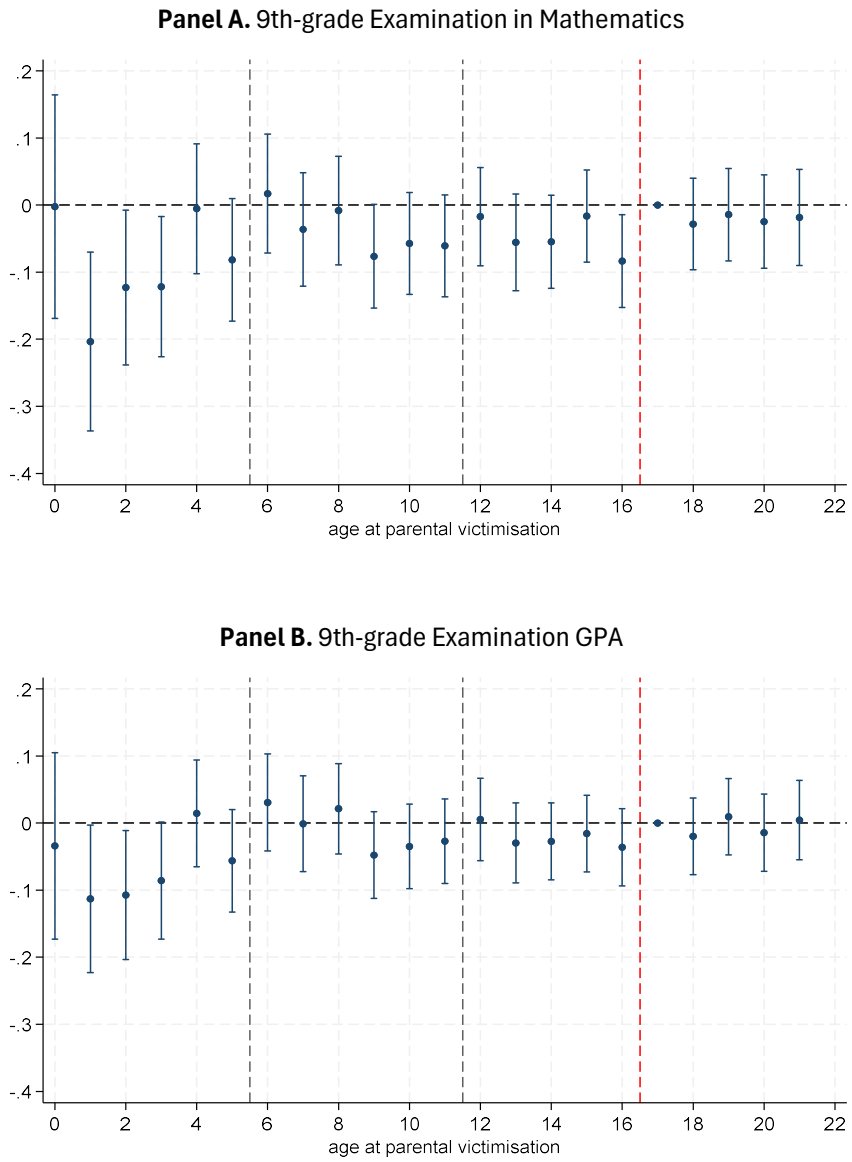


Figure A.4: Age-Specific Effects of Parental Violent Victimization – Excluding Examination Years 2020–2022

Note. The figure plots estimates of λ_k coefficients in Equation (2) on end-of-school (age 16) examination score. Panel A and Panel B consider mathematics score only or the grade point average (GPA) across all subjects, respectively. The sample is restricted to parental violent victimizations and excludes examination years from 2020 to 2022. The estimation sample includes exposed children and their matched control peers. The x-axis represents age at real or placebo parental victimization. Childhood stages are visually separated by dotted vertical lines, with red lines separating causal effects of parental victimization from placebo effects on children exposed after end-of-school achievement is realized. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

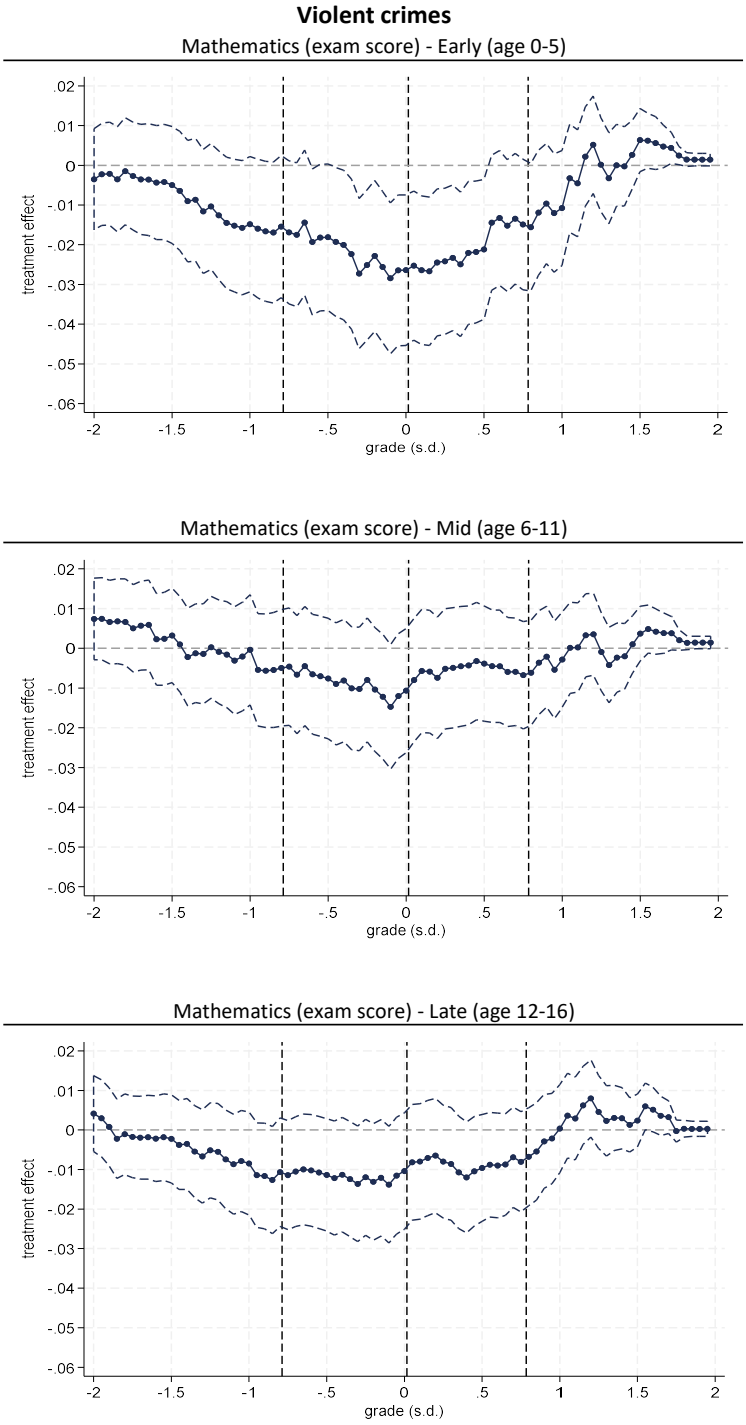


Figure A.5: Distributional analysis

Note. The figure plots estimates of the distributional impacts of parental victimization on a child's 9th-grade examination score in mathematics. Plotted are estimates of coefficients δ_s from Equation (1) from 80 different regressions. Outcomes are binary variables equal to 1 if the child's score meets at least the level in question. We consider 80 levels using equally spaced 0.05σ-wide intervals from -2σ to 2σ. Children who do not participate in the exams are coded as zero on all 80 binary indicators and are therefore counted as not reaching any of the thresholds. Panels A, B, and C plot treatment effects on children exposed in early (age 0-5), mid (age 6-11) or late (age 12-16) childhood, respectively. Boundaries of the 95% confidence intervals are plotted as dashed lines. The estimated specification is analogous to column (2) of Table 2. Standard errors are clustered on families.

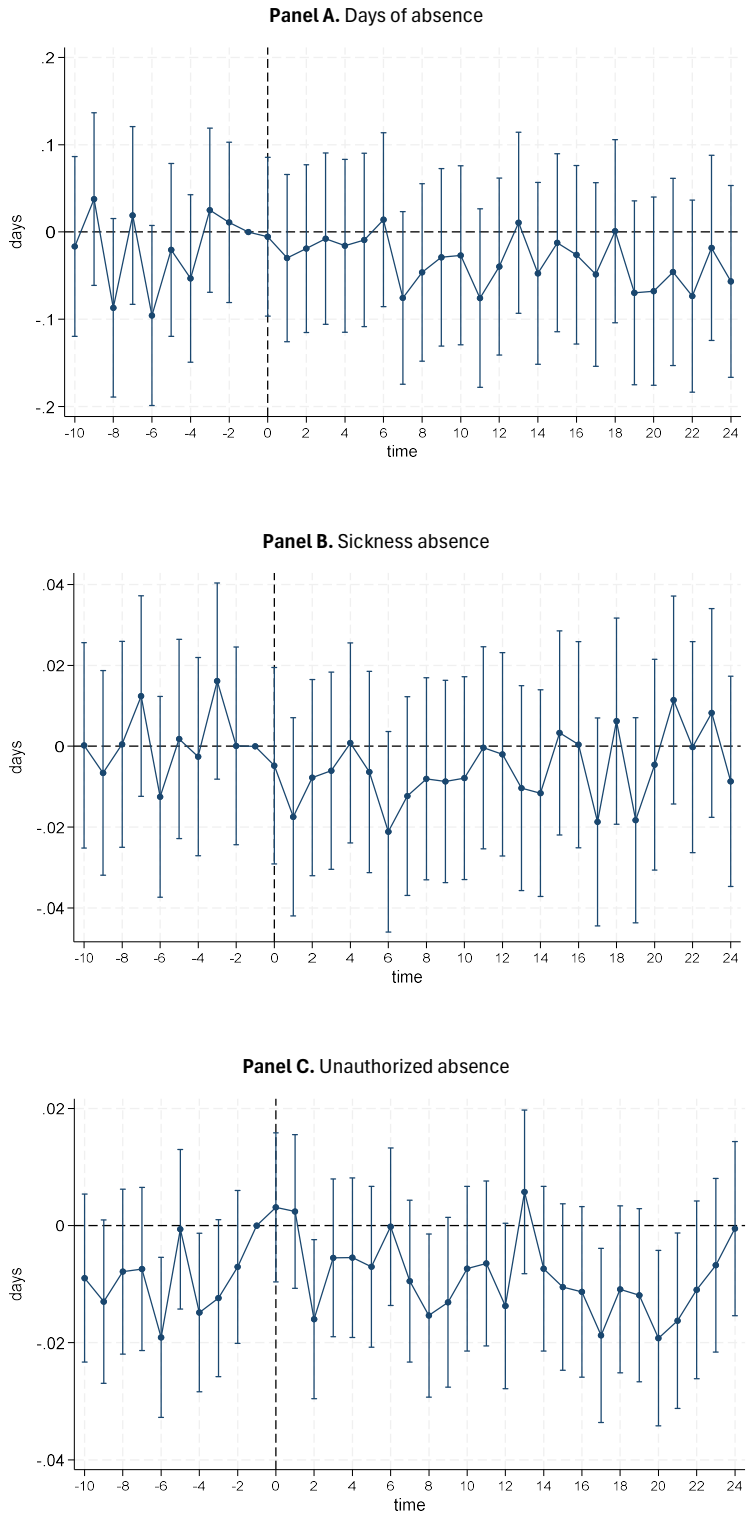


Figure A.6: Event Studies of child's school absences

Note. The figure plots event studies of child school absences. We plot estimates of α_l coefficients in Equation (3) against month relative to parental victimization, measured in the x-axis. The sample includes children exposed to parental victimization for violent crimes and their matched control peers – limited to birth cohorts 1994-2007 to reflect the time span of the available data (2010-2025), and to victimizations for which the exact event date is available. Dependent variables are the number of days of absence counting all occurrences (Panel A), sickness-related absences only (Panel B); or unauthorized absences only (Panel C). Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

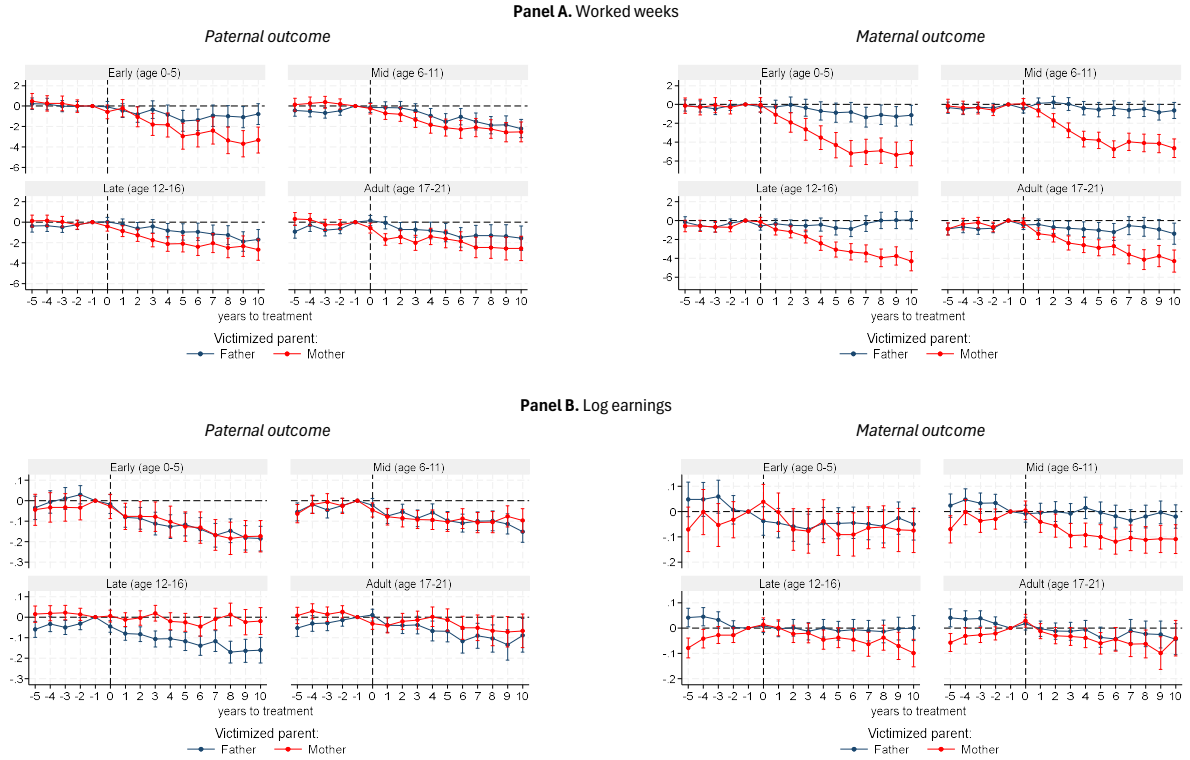


Figure A.7: Additional Parental Event Studies

Note. The figure plots estimates of α_l coefficients in Equation (3) against time relative to parental victimization, measured in the x-axis. The sample includes children exposed to parental victimization for violent crimes and their matched control peers. Dependent variables are parental weeks worked in the year (Panel A) or parental log earnings in the year (Panel B). Separate estimates are plotted by childhood stage at treatment in subpanels. Left-hand graphs consider paternal outcomes and right-hand graphs consider maternal outcomes. Red connected markers refer to maternal victimizations, blue connected markers refer to paternal victimizations. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

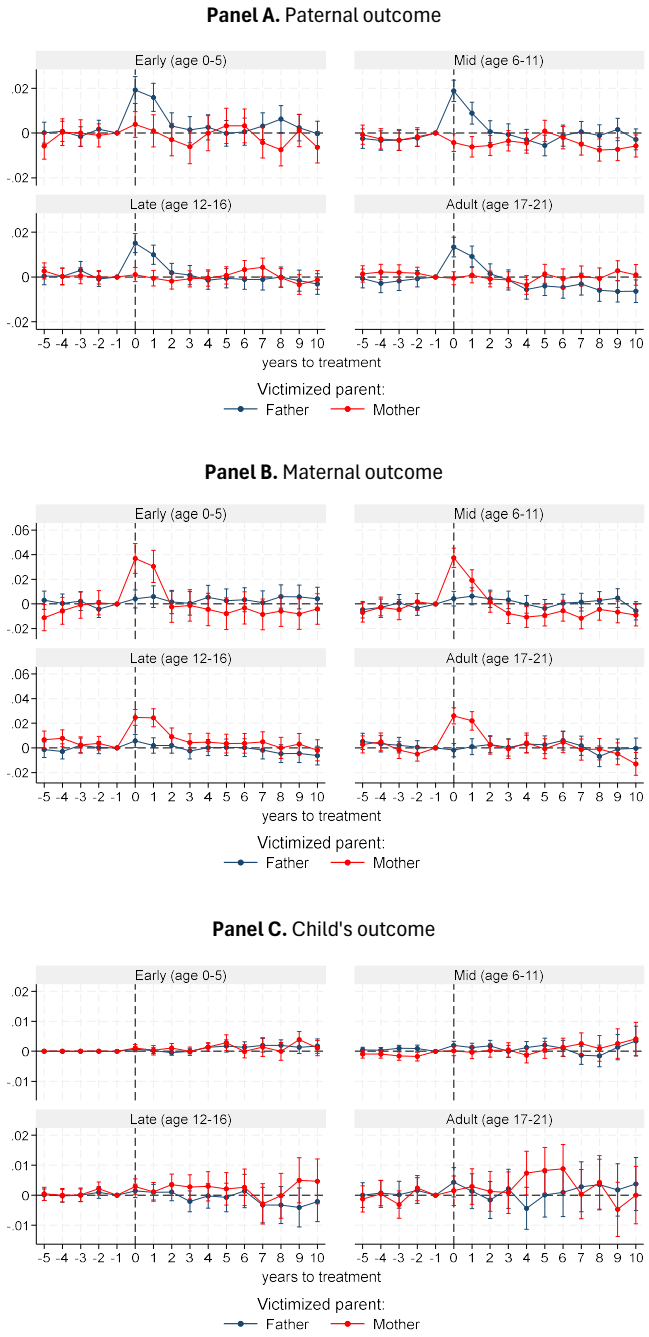


Figure A.8: Psychologist contact

Note. The figure plots estimates of α_l coefficients in Equation (3) against time relative to parental victimization, measured in the x-axis. The sample includes children exposed to parental victimization for violent crimes and their matched control peers. Dependent variables are: an indicator for paternal (Panel A), maternal (Panel B), or child's (Panel C) contact with a psychologist in the year. All indicators are coded to zero if the person is not found in the healthcare register. Separate estimates are plotted by childhood stage at treatment in subpanels. Red connected markers refer to maternal victimizations, blue connected markers refer to paternal victimizations. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

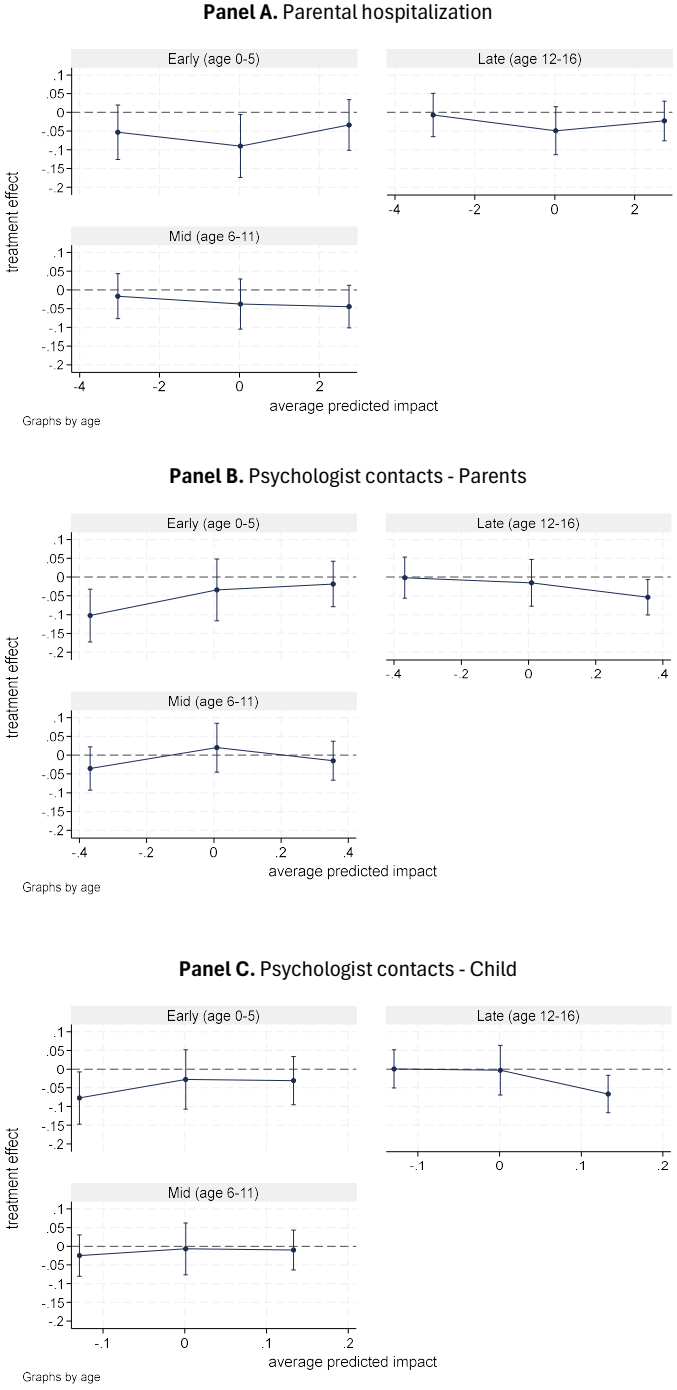


Figure A.9: Heterogeneity by predicted health responses

Note. The figure plots heterogeneous effects of parental victimization on children's learning by predicted changes in parental outcomes after victimization. Graphed are coefficients from Equation (1) estimated separately by predicted change tercile, with specifications analogous to column (2) of Table 2. The dependent variable is end-of-school examination score in mathematics. Changes in parental outcome are computed as predicted values of individual-level difference-in-differences around victimization from a regression on baseline parental and children characteristics. We compare the average outcome over the ten years following victimization with its value in the base year (the year before victimization). The considered predicted response is parental hospitalization (Panel A), or parental (Panel B) or child (Panel C) contact with psychologist. The x-axis measures the average predicted change in the considered tercile. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

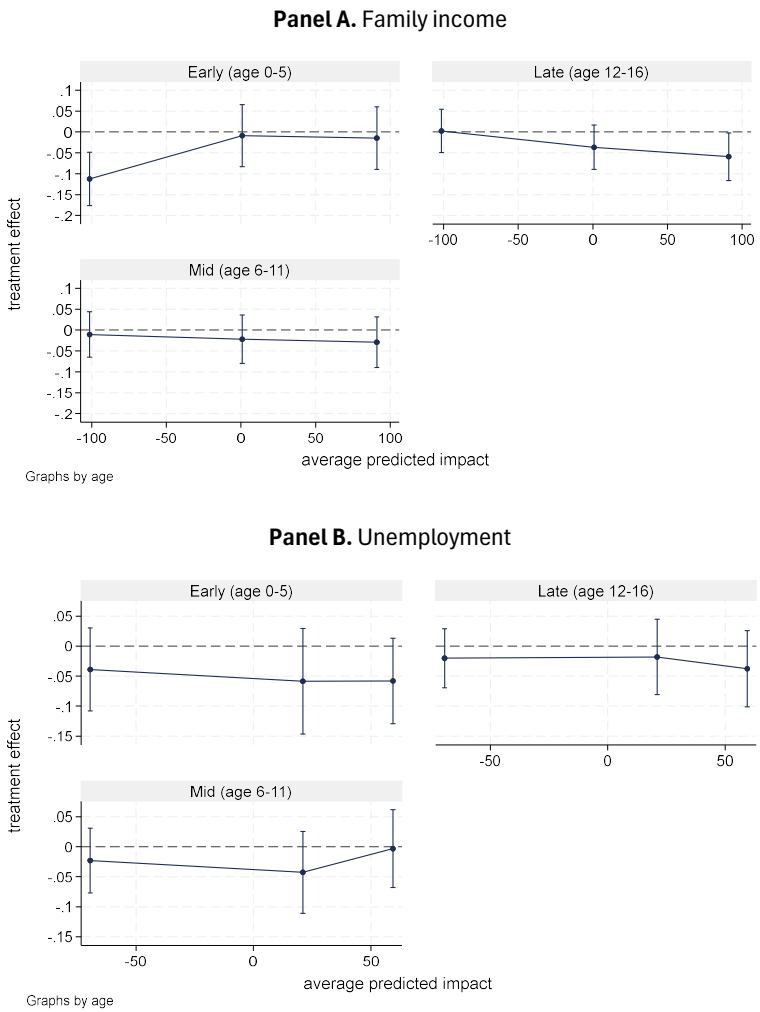


Figure A.10: Heterogeneity by predicted income responses

Note. The figure plots heterogeneous effects of parental victimization on children's learning by predicted changes in parental outcomes after victimization. Graphed are coefficients from Equation (1) estimated separately by predicted change tercile, with specifications analogous to column (2) of Table 2. The dependent variable is end-of-school examination score in mathematics. Changes in parental outcome are computed as predicted values of individual-level difference-in-differences around victimization from a regression on baseline parental and children characteristics.We compare the average outcome over the ten years following victimization with its value in the base year (the year before victimization). The considered predicted response is family income (thousands of DKK, Panel A) or the fraction of year spent on unemployment benefits by the victimized parent (Panel B). The x-axis measures the average predicted change in the considered tercile. Spikes around point estimates represent the 95% confidence interval. Standard errors are clustered on families.

Table A.1: Descriptive Statistics – Property Crimes

	All			Age 0-5	Age 6-11	Age 12-16	Age 17-21
	C mean (1)	T mean (2)	SMD (3)	SMD (4)	SMD (5)	SMD (6)	SMD (7)
				Panel A. Pre-matching			
Age	10.125	11.226	-0.192	-0.131	-0.054	-0.054	0.065
Female	0.485	0.502	-0.035	-0.008	-0.019	-0.048	-0.050
Danish	0.929	0.920	0.034	0.043	0.063	0.015	0.011
Father's age at first birth	28.888	29.191	-0.034	-0.020	-0.039	-0.034	-0.051
Mother's age at first birth	28.149	28.557	-0.093	-0.107	-0.079	-0.080	-0.120
Father's education (months)	151.207	155.803	-0.094	-0.109	-0.129	-0.098	-0.075
Mother's education (months)	161.053	166.117	-0.162	-0.205	-0.200	-0.161	-0.134
Father employed	0.825	0.826	-0.001	-0.033	-0.026	-0.009	0.017
Mother employed	0.859	0.858	0.003	-0.017	0.012	0.014	0.016
Father's earnings ('000s DKK)	428.392	459.979	-0.098	-0.097	-0.118	-0.092	-0.042
Mother's earnings ('000s DKK)	313.049	344.633	-0.140	-0.115	-0.146	-0.107	-0.063
Mother's weeks worked	19.567	24.136	-0.190	-0.238	-0.303	-0.113	0.066
Father's weeks worked	18.491	23.232	-0.195	-0.288	-0.329	-0.106	0.082
Family income ('000s DKK)	946.938	1039.526	-0.064	-0.068	-0.083	-0.057	-0.024
Danish families in local area	0.905	0.870	0.268	0.335	0.333	0.236	0.176
Education in local area	169.542	172.231	-0.318	-0.378	-0.359	-0.284	-0.235
Treated in local area	0.007	0.016	-0.984	-1.296	-1.076	-0.888	-0.785
Outcomes							
No exam	0.080	0.076	0.016	-0.015	0.020	0.044	-0.007
Mathematics score	0.129	0.131	-0.002	-0.006	0.011	-0.001	-0.016
GPA	0.088	0.153	-0.078	-0.087	-0.064	-0.075	-0.096
N	18,522,952	124,092	18,647,044	3,928,822	5,535,714	4,953,468	4,229,040
				Panel B. Post-matching			
Age	11.177	11.177	0.000	0.000	0.000	0.000	0.000
Female	0.500	0.500	0.000	0.000	0.000	0.000	0.000
Danish	0.919	0.920	-0.003	-0.005	-0.010	0.001	0.001
Father's age at first birth	29.201	29.191	0.001	-0.005	-0.001	0.001	0.007
Mother's age at first birth	28.557	28.549	0.002	-0.019	0.002	0.004	0.010
Father's education (months)	156.327	155.789	0.011	0.001	0.012	0.014	0.012
Mother's education (months)	166.191	166.067	0.004	-0.009	0.001	0.005	0.014
Father employed	0.829	0.826	0.008	-0.003	0.006	0.012	0.011
Mother employed	0.859	0.858	0.003	-0.003	0.005	0.005	0.004
Father's earnings ('000s DKK)	464.554	459.122	0.016	0.000	0.018	0.018	0.020
Mother's earnings ('000s DKK)	346.388	343.784	0.011	0.001	0.010	0.017	0.011
Mother's weeks worked	23.924	23.958	-0.001	0.002	-0.001	0.000	-0.004
Father's weeks worked	23.174	23.056	0.005	0.006	0.002	0.007	0.005
Family income ('000s DKK)	1035.534	1036.404	-0.001	-0.014	-0.004	0.002	0.003
Danish families in local area	0.867	0.870	-0.018	-0.015	-0.013	-0.021	-0.021
Education in local area	172.404	172.194	0.024	0.003	0.028	0.027	0.028
Treated in local area	0.016	0.016	-0.028	-0.020	-0.019	-0.035	-0.035
Outcomes							
No exam	0.078	0.076	0.007	0.024	0.010	0.012	-0.011
Mathematics score	0.179	0.130	0.051	0.041	0.062	0.051	0.045
GPA	0.166	0.151	0.019	0.009	0.025	0.024	0.011
N	122,168	122,168	244,336	36,594	66,790	76,650	64,302

Note. The table reports descriptive statistics comparing the average characteristics of children exposed to parental victimization (column 2) with untreated or matched-control children (column 1, Panel A and Panel B, respectively). Columns (3)-(7) display standardized mean differences, computed over the full sample (column 3) or separately by stage of childhood (columns 4-7). The sample is restricted to parental property victimizations.

Table A.2: Additional Specifications - Violent Crimes

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. 9th-grade Examination in Mathematics					
Treated	-0.1056***	-0.1089***	-0.1026***	-0.1056***	-0.1089***	-0.1025***
	(0.0121)	(0.0112)	(0.0113)	(0.0121)	(0.0112)	(0.0113)
Treated X Exposed	-0.0251*	-0.0236*	-0.0270**			
	(0.0141)	(0.0132)	(0.0133)			
Treated X Early (age 0-5)				-0.0428**	-0.0398**	-0.0480**
				(0.0212)	(0.0198)	(0.0199)
Treated X Mid (age 6-11)				-0.0216	-0.0164	-0.0188
				(0.0174)	(0.0163)	(0.0164)
Treated X Late (age 12-16)				-0.0203	-0.0227	-0.0248
				(0.0162)	(0.0152)	(0.0153)
N	97,218	97,218	96,808	97,218	97,218	96,808
	Panel B. 9th-grade Examination GPA					
Treated	-0.0663***	-0.0690***	-0.0666***	-0.0663***	-0.0690***	-0.0666***
	(0.0103)	(0.0094)	(0.0095)	(0.0103)	(0.0094)	(0.0095)
Treated X Exposed	-0.0144	-0.0147	-0.0154			
	(0.0121)	(0.0111)	(0.0111)			
Treated X Early (age 0-5)				-0.0346*	-0.0347**	-0.0373**
				(0.0181)	(0.0166)	(0.0167)
Treated X Mid (age 6-11)				-0.0091	-0.0054	-0.0055
				(0.0149)	(0.0137)	(0.0137)
Treated X Late (age 12-16)				-0.0102	-0.0140	-0.0142
				(0.0138)	(0.0127)	(0.0128)
N	98,327	98,327	97,909	98,327	98,327	97,909
Parent and child age dummies	X	X	X	X	X	X
Additional covariates		X	X		X	X
Parish dummies			X			X

Note. The table reports estimates of δ_1 ("Treated") and δ_s (interactions) coefficients in Equation (1) on end-of-school (age 16) examination scores. The sample is formed by children exposed to parental violent victimization and their matched control peers. Panel A and Panel B consider mathematics and the GPA across subjects, respectively. Average estimates across childhood are reported in columns (1)-(3), while columns (4)-(6) report estimates by childhood stage at real or placebo parental victimization. Columns (1) and (4) include dummies for mother's, father's, and child's age at real or placebo parental victimization. Columns (2) and (5) add cohort dummies, base year dummies, and parental and child characteristics in the base year. Columns (3) and (6) add dummies for child's parish in the base year. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Effects of Parental Victimization on Achievement – Excluding Examination Years 2020–2022

	Mathematics		GPA	
	(1)	(2)	(3)	(4)
	Panel A. Violent victimization			
Treated	-0.1014***	-0.1014***	-0.0652***	-0.0652***
	(0.0116)	(0.0116)	(0.0097)	(0.0097)
Treated X Exposed	-0.0335**		-0.0198*	
	(0.0141)		(0.0118)	
Treated X Early (age 0-5)		-0.0677***		-0.0525***
		(0.0237)		(0.0197)
Treated X Mid (age 6-11)		-0.0247		-0.0097
		(0.0178)		(0.0149)
Treated X Late (age 12-16)		-0.0295*		-0.0174
		(0.0164)		(0.0136)
N	78,353	78,353	79,324	79,324
	Panel B. Property victimization			
Treated	-0.0324***	-0.0324***	0.0007	0.0007
	(0.0077)	(0.0077)	(0.0064)	(0.0064)
Treated X Exposed	-0.0070		-0.0121	
	(0.0092)		(0.0076)	
Treated X Early (age 0-5)		-0.0134		-0.0141
		(0.0151)		(0.0123)
Treated X Mid (age 6-11)		-0.0145		-0.0152
		(0.0114)		(0.0095)
Treated X Late (age 12-16)		0.0006		-0.0092
		(0.0104)		(0.0086)
N	184,024	184,024	185,918	185,918
Parent and child age dummies	X	X	X	X
Additional covariates	X	X	X	X
Parish dummies	X	X	X	X

Note. The table reports estimates of δ_1 ("Treated") and δ_s (interactions) coefficients in Equation (1) on end-of-school (age 16) examination scores. Estimated specifications and table structure are analogous to Table 2. The sample excludes examination years from 2020 to 2022, affected by pandemic-related grade inflation. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Child Outcomes Beyond Education – Additional Results

	All	Son	Daughter	Father	Mother
	(1)	(2)	(3)	(4)	(5)
	Panel A. School attachment index				
Treated X Early (age 0-5)	-0.0101	-0.0718	0.0553	-0.0936*	0.1081*
	(0.0378)	(0.0555)	(0.0560)	(0.0538)	(0.0583)
Treated X Mid (age 6-11)	0.0299	0.0025	0.0801	-0.0172	0.0796
	(0.0357)	(0.0528)	(0.0518)	(0.0525)	(0.0527)
Treated X Late (age 12-16)	0.0122	-0.0150	0.0557	-0.0517	0.0789
	(0.0363)	(0.0536)	(0.0533)	(0.0540)	(0.0522)
N	30,321	15,574	14,246	16,095	13,743
	Panel B. Overall well-being index				
Treated X Early (age 0-5)	-0.0094	-0.0629	0.0458	-0.1139**	0.0990*
	(0.0378)	(0.0563)	(0.0547)	(0.0542)	(0.0579)
Treated X Mid (age 6-11)	0.0077	-0.0016	0.0320	-0.0709	0.0772
	(0.0357)	(0.0541)	(0.0503)	(0.0529)	(0.0522)
Treated X Late (age 12-16)	-0.0075	-0.0284	0.0216	-0.1283**	0.0930*
	(0.0359)	(0.0545)	(0.0510)	(0.0536)	(0.0518)
N	30,357	15,597	14,254	16,113	13,762
	Panel C. Days absent				
Treated X Early (age 0-5)	0.5479	0.6126	0.0930	0.9001	0.0881
	(0.4194)	(0.5647)	(0.6395)	(0.5587)	(0.6770)
Treated X Mid (age 6-11)	-0.1258	-0.0363	-0.3708	-0.2487	-0.0427
	(0.3545)	(0.4955)	(0.5224)	(0.5051)	(0.5312)
Treated X Late (age 12-16)	-0.0065	0.5776	-0.8307	-0.3759	0.3989
	(0.3572)	(0.5082)	(0.5299)	(0.5154)	(0.5267)
N	53,629	28,054	25,185	28,469	24,805
	Panel D. Charged with violent crime by age 16				
Treated X Early (age 0-5)	0.0022	0.0054	-0.0000	0.0044	-0.0002
	(0.0026)	(0.0048)	(0.0021)	(0.0036)	(0.0042)
Treated X Mid (age 6-11)	-0.0008	-0.0016	0.0002	-0.0029	0.0027
	(0.0022)	(0.0040)	(0.0016)	(0.0030)	(0.0032)
Treated X Late (age 12-16)	0.0008	-0.0001	0.0020	0.0011	0.0004
	(0.0021)	(0.0038)	(0.0015)	(0.0030)	(0.0029)
N	94,134	48,299	45,630	49,452	44,459
Parent and child age dummies	X	X	X	X	X
Additional covariates	X	X	X	X	X
Parish dummies	X	X	X	X	X

Note. The table reports estimates of δ_s coefficients in Equation (1) on child well-being, absence and own criminal perpetration. Estimated specifications and table structure are analogous to Table 3. For well-being, we consider standardized indices combining scores in survey items about happiness with school, class, or teachers (Panel A) or all the survey questions (Panel B). Panel C reports the average yearly number of days when a student is absent from school. Panel D considers an indicator for the child being charged with violent crimes by age 16. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Heterogeneity by parental breadwinner status

	Breadwinner	Non-Breadwinner	Father Breadwinner	Father Non-Breadwinner	Mother Breadwinner	Mother Non-Breadwinner
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.0955***	-0.1049***	-0.0710***	-0.0418	-0.1502***	-0.1447***
	(0.0166)	(0.0161)	(0.0214)	(0.0268)	(0.0257)	(0.0220)
Treated X Early (age 0-5)	-0.0465	-0.0494*	-0.0729**	-0.1210***	-0.0422	0.0306
	(0.0298)	(0.0276)	(0.0344)	(0.0458)	(0.0513)	(0.0414)
Treated X Mid (age 6-11)	-0.0263	-0.0162	-0.0495*	-0.0861**	0.0235	0.0403
	(0.0241)	(0.0229)	(0.0297)	(0.0375)	(0.0384)	(0.0326)
Treated X Late (age 12-16)	-0.0353	-0.0151	-0.0162	-0.0722**	-0.0096	-0.0013
	(0.0224)	(0.0216)	(0.0284)	(0.0355)	(0.0346)	(0.0300)
N	45,813	50,760	31,061	20,019	19,340	25,359
Parent and child age dummies	X	X	X	X	X	X
Additional covariates	X	X	X	X	X	X
Parish dummies	X	X	X	X	X	X

Note. The table reports estimates of δ_1 ("Treated") and δ_s (interactions) coefficients in Equation (1) on end-of-school (age 16) examination score in mathematics. Estimated specifications are similar to column (2) of Table 2. The sample is restricted to treatment-control pairs where the victimized parent is the main (column 1) or the secondary (column 2) earner; further restrictions by main earner status and gender are considered in columns (3)-(6). Main earner status is measured at baseline. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1