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Crime Victims' Children

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Abstract

We provide the first causal evidence on the intergenerational effects of parental criminal victimization originating outside the family. Using population-wide Danish administrative data, we compare end-of-compulsory-school examination scores of children exposed to parental victimization with those of matched controls, and exploit victimizations occurring after compulsory schooling to account for residual selection. Violent, but not property, victimization lowers children's mathematics scores by 0.03 standard deviations, rising to 0.05 standard deviations for exposure in early childhood, a period when parental inputs are especially consequential for skill formation. Deterioration in family stability, paternal behavior, and labor market attachment following victimization emerge as the primary mechanisms, pointing to an underappreciated channel of intergenerational inequality.

JEL classification

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Keywords

parental criminal victimization, intergenerational spillovers, children's human capital, early childhood

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1 Introduction

Criminal victimization is a traumatic event imposing substantial costs to victims (Bindler and Ketel, 2022; Dustmann and Mertz, 2026). By impairing labor market trajectories and deteriorating physical and mental health, victimization is likely to disrupt the home environment and generate negative spillovers within families. However, in contrast to other major shocks such as parental job loss or death (Jensen and Zhang, 2026; Ruiz-Valenzuela, 2021), the intergenerational consequences of parental victimization on children's human capital are so far unexplored. We provide the first evidence that parental criminal victimization originating outside the family causally reduces children's academic achievement, and show that these intergenerational costs are concentrated when victimization occurs during early childhood.

Theoretical frameworks from psychology, sociology, and economics help illuminate the relationship between parental victimization and children's human capital. Attachment theory emphasizes how victimization affects parents' emotional availability, potentially disrupting the secure attachment crucial for children's development (Spencer, Keilholtz and Stith, 2021). Social learning theory posits that children may adopt maladaptive behaviors modeled by victimized parents (Gagnon, 2018). Family stress models emphasize how crime-induced strain on parental relationships reduces emotional support available to children (Gonggrijp et al., 2023). Most centrally for our analysis, human capital theory posits that parental victimization reduces both financial resources and parental involvement, adversely affecting investments in children's education (Bindler and Ketel, 2022).

We study parental criminal victimization from non-domestic offences. Using population-wide Danish administrative registers from 2001–2022, we link police-recorded victims of crime to their children. We define as treated those children who are aged 0–21 in the year a parent reports being the victim of a crime, and we consider the first observed event separately for violent and property offences. We exclude domestic violence to focus on victimization originating outside the family and to complement existing evidence on intra-family violence in Currie, Mueller-Smith and Rossin-Slater (2022) and Bhuller et al.

(2024). We further exclude parents younger than 22 at first birth and parents with any previous criminal charges, because these groups display distinct, persistently high victimization risks; this restriction directs attention to victimization events that are less likely to arise from entrenched criminal trajectories.

Our empirical strategy designs diffence-in-differences comparisons around the end of compulsory school. Unlike labor market outcomes of victims analysed by Bindler and Ketel (2022) and Dustmann and Mertz (2026), end-of-school achievement is observed once per child, precluding a standard event-study design. Instead, we compare children exposed to parental victimization with carefully matched children whose parents were not victimized. To account for remaining selection, we exploit the fact that parental victimization occurring after children complete compulsory schooling cannot affect their exam performance. This post-school exposure group provides a direct estimate of residual selection into victimization, which we net out from our estimates. We identify the causal effect of parental victimization if unobservable selection into treatment is invariant across ages at exposure. We provide several pieces of evidence in support of this assumption, including null placebo effects for children exposed after age 16 and flat outcome trends in age at exposure among matched control children.

Our findings reveal that parental violent victimization reduces a child's end-of-school mathematics examination score by 0.03 standard deviations (hereafter, σ), with effects concentrated among children exposed during early childhood (0.05σ). Property crime victimization shows no detectable spillovers, and we focus on violent offences in the subsequent analysis. The effects are considerably more pronounced for paternal victimization. In addition, we show that exposed children are more likely to take an extra year in elementary education at age 16, and that after compulsory schooling, they enroll in schools with lower-quality peers.

We provide evidence that the effect of parental victimization on children's achievement is closely linked to a broad deterioration of the home environment, captured by multiple dimensions of behavioral, relational, and residential stability. First, using event-study models, we show that victimization persistently worsens parents' health, labor market

trajectories, and family stability. Beyond existing evidence, we also provide strongly suggestive evidence that victimization increases the likelihood that parents, especially fathers, are subsequently charged with violent crimes. Second, exploiting heterogeneity in predicted parental responses, we show that children's achievement losses are most strongly associated with proxies for longer time spent with the victimized parent, subsequent parental criminal behavior, and family relational and residential instability.

Our findings have important implications for understanding the full social costs of crime and for designing policies to support crime victims and their families. By documenting substantial intergenerational spillovers from parental victimization, we show that crime victimization represents an underappreciated driver of intergenerational inequality in human capital formation. The concentration of effects in early childhood suggests that interventions targeting families with young children may be particularly valuable. Moreover, the finding that victimized parents, especially fathers, are more likely to engage in subsequent criminal behavior highlights potential channels through which crime perpetuates across generations, suggesting that more comprehensive victim support services could have benefits that extend beyond the immediate victims themselves.

We contribute to the nascent literature on the costs of criminal victimization by providing the first evidence of negative intergenerational spillovers on children's human capital. While the economic literature has devoted considerable attention to offenders (Nagin, 2013; Draca and Machin, 2015; Chalfin and McCrary, 2017; Britto, Pinotti and Sampaio, 2022), significantly less is known about crime victims and their outcomes (Bindler, Ketel and Hjalmarsson, 2020). Recent studies find substantial "scarring" effects of victimization on victims' labor market outcomes and health (Bindler and Ketel, 2022; Dustmann and Mertz, 2026).[1] While parental incarceration has been shown to impact children's outcomes (Norris, Pecenco and Weaver, 2021), evidence of intergenerational effects of victimization is limited to in-utero exposure (Menezes and Koppensteiner, 2026; Currie,

[1]Evidence from different contexts shows that crime victimization affects working times (Hamermesh, 1999), spatial mobility (Dugan, 1999), avoidance behavior and time allocation (Braakmann, 2012), physical activity (Janke, Propper and Shields, 2016), mental health (Cornaglia, Feldman and Leigh, 2014; Dustmann and Fasani, 2016), well-being and life satisfaction (Cohen, 2008; Johnston, Shields and Suziedelyte, 2018). Moreover, indirect exposure to violence hurts birth outcomes (Koppensteiner and Menezes, 2021)

Mueller-Smith and Rossin-Slater, 2022), domestic violence (Bhuller et al., 2024), or "reverse" trasmission from youth victimization to parents' outcomes (Mertz, Mitchell and Skov, 2026).[2] We extend this literature by estimating the effects of parental victimization originating outside the family on children's human capital measured in adolescence, and by considering victimization events spanning the entire childhood.

Our paper also speaks to the literature about the timing of parental inputs during childhood. A substantial body of research emphasizes that early life stages are crucial for children's human capital formation (Cunha and Heckman, 2006; Almond and Currie, 2011). The timing of parental inputs matters above and beyond the level of resources invested (Carneiro et al., 2021; Caucutt and Lochner, 2020). However, precisely which "early stages" matter most remains an open question (Almond, Currie and Duque, 2018), with recent evidence suggesting there may be specific "windows of opportunity" for interventions to be most effective (Attanasio et al., 2022). We add to this literature by showing that a potentially traumatic shock such as parental victimization has disproportionately larger negative effects if experienced during early childhood.

The remainder of the paper is structured as follows. Section 2 describes the data and empirical framework underlying our analysis as well as our empirical design. Section 3 presents our results, and Section 4 examines what mechanisms might be at play. Section 5 concludes.

2 Data and Empirical Design

This section describes the setting of our study and the strategy we use to estimate the causal effects of parental victimization on children's educational outcomes.

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 crimi-

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

nal 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 during this age window or ii) never exposed. Let $a(i)$ denote the child's age in the year of the reported victimization. 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, 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. Compulsory school graduates can pursue several distinct educational tracks. About half of all students choose to complete 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 as well as business and technical colleges. In contrast, vocational programs combine classroom instruction with on-the-job training and typically last two to five years.

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 math-

ematics 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. Ninth-grade test scores are observed between 2002 and 2023. We therefore consider the cohorts of children born between 1986 and 2007.

We complement these educational 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. To this end, we exclude domestic violence. To identify domestic offences, 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.[3]

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. Nevertheless, we note that most victims report only one crime (76%). This selection is made separately by crime type. That is, we consider a parent's first reported violent crime

[3]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).

and first reported property crime. Throughout the analysis, we treat violent and property crime victims, together with their respective matched control groups, as separate samples.

We start by illustrating victimization risk in the general population of parents, following Dustmann and Mertz (2026). Appendix Figure A.1 plots the share of parents reporting property (left-hand panels) or violent crimes (right-hand panels) by age. Panel A compares mothers and fathers. The risk of property crime is below 1% on average and substantially higher for mothers, while its age profile is broadly flat. In contrast, the risk of violent victimization is relatively similar across genders but declines markedly with age. As shown by the green bars, we observe very few teenage parents, but they face a disproportionately high risk of violent victimization (reaching up to 7% below age 20 before stabilising at below 1%). Panel B compares parents with and without criminal charges up to the given year. Although the age–risk profiles observed in Panel A are largely preserved, a marked difference emerges: parents with prior criminal charges are about 1 percentage point (p.p.) more likely to report violent crimes up to age 50.

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 58,864 treated children for violent crimes and 133,585 treated children for property crimes.

Our working sample, described in Table 1, shows marked negative selection into parental victimization, especially for violent crimes. Panel A compares children exposed to parental violent victimization, observed in the year before the report (the "base" year), with untreated children, observed in any year. Most strikingly, treated children have an annual family income that is, on average, DKK 34,000 (USD 5,000) lower. Moreover, they are, for example, 3 p.p. less likely to be of Danish origin and have mothers with

1.3 fewer months of education. Potentially reflecting both selection and treatment effects, treated children score 0.2σ lower in mathematics at age 16. Panel B reports analogous comparisons for property crime, revealing instead positive selection into this type of victimization. For example, treated children have annual family income that is, on average, DKK 89,000 (USD 13,000) higher and mothers with five more months of education than untreated children. We address selection into treatment through our research design, which we discuss next.

2.3 Empirical Design

We employ a quasi-experimental research design to estimate the causal impact of parental criminal victimization. Although victimization is at least partly random, the likelihood of being subjected to violent or property crime varies across contexts, is spatially concentrated, and may therefore have 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 direct costs of criminal victimization by comparing the time-varying 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 those studies, the outcome of interest in our study 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 during early childhood. For these reasons, the econometric approach adopted in this study differs from that in these closely related studies.

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.[4] We successfully match 97% of treated children for violent crimes, yielding an estimation sample of 56,382 matched pairs, and 95% of treated children for property crimes, yielding an estimation sample of 126,914 matched pairs. Table 2 presents key descriptive statistics for the *matched* treated and control groups, showing that the matching procedure successfully balances these characteristics across the two groups. A mathematics achievement gap of 0.12σ in favour of control children nevertheless 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 16 can be interpreted as the causal effect of parental victimization.

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

[4]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, e.g., in the literature on parental job loss (e.g., Bertheau et al., 2023).

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

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 during childhood stage s on Y_i.[5]

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.[6] This assumption may appear to be strong given the wide age range we consider, as the risk behavior of parents with 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:

[5] 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 treatment–control pair forms an independent 2×2 difference-in-differences comparison, and control units are never treated.

[6] 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. This contrasts with the parental outcomes in Section 4, which are panel data and for which event-study variation is available.

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

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 show that the learning outcomes of matched control children are flat across the age at placebo victimization. Since our control group is selected to mirror the observable characteristics of treated children conditional on age, stronger selection into treatment at younger ages would imply lower potential outcomes for children matched to treated peers exposed to parental victimization during early childhood. In contrast, Appendix Figure A.2 shows that control children's achievement is essentially flat across the age at placebo victimization.

Third, stronger selection into treatment at younger ages is not supported by pre-matching observable characteristics. We report key pre-matching descriptive statistics by stage of childhood in Appendix Table A.1. We focus here on violent crimes, since we find no impact of parental victimization on children's learning outcomes for property crimes (see Section 3). Parental education and age at first birth are remarkably similar across children treated at different stages (columns 1–2), as are local-area-level education and criminal victimization. While naturally increasing with age, family income is similarly negatively selected across childhood stages relative to the population of children of the same age, as shown by the treated–control differences in columns (5)–(6).

Fourth, event studies around victimization presented in Section 4 show that victimization events occurring during young adulthood produce similar effects on family stability, parental labor market outcomes, health, and criminal charges as those occurring at younger ages.

Fifth, by adjusting for observable characteristics X_i in equation (1), we show that

accounting for observable differences across age groups leaves our results unchanged. Specifically, we progressively include child and parental characteristics and, in our most demanding specification, additionally control for parish fixed effects, thereby holding granular geographical units constant. We show in Section 3 that our results are remarkably stable across these specifications.

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 3, shows that this is not the case.

Overall, baseline children's characteristics and their learning outcomes exhibit several patterns that are inconsistent with substantially stronger selection into treatment at younger ages in our sample. We therefore conclude that our results are unlikely to be explained by departures from our identifying assumptions. This conclusion may be specific to our context, a country with relatively low crime rates, particularly for violent crimes (Aebi et al., 2024). The evidence we presented suggests that our sample restrictions on parental age at first birth and prior involvement with the justice system direct attention toward parental victimization events that are relatively less predictable, and for which selection is unlikely to differ substantially by age.

3 Results

This section presents the main empirical results on how parental criminal victimization affects children's educational achievement. We begin in Subsection 3.1 by documenting the average effects of violent and property crime victimization on end-of-school examination score in mathematics and GPA, and by examining how these effects vary with the age at which children are exposed. Subsection 3.2 then explores heterogeneity in the estimated effects by the gender of the victimized parent and the child. Together, these results provide the reduced-form evidence motivating the subsequent analysis of mechanisms in Section 4.

3.1 Main Effects on Human Capital

Our baseline results on educational achievement are summarized in Table 3 and Figure 1. Table 3 reports parametric Difference-in-Differences estimates from Equation (1), which groups ages into early childhood (0–5), middle childhood (6–11), and adolescence (12–16), while Figure 1 plots age-specific estimates of the effects of parental violent and property crime victimization. Across specifications, the dependent variable in Panel A of Table 3 is the mathematics examination score at the end of compulsory schooling, while Panel B reports analogous results for overall GPA.

We estimate substantial selection into parental victimization based on unobserved achievement potential. The estimated coefficient on the *Treated* dummy (corresponding to δ_1 in equation (1)) is sizeable and negative. This coefficient measures the achievement gap between children whose parent is victimized only after the completion of compulsory schooling and their matched controls. Since achievement has already been realized at the time of victimization for this group, this gap captures residual selection due to unobserved heterogeneity that is not removed by the matching procedure. It amounts to approximately 0.10σ in mathematics and 0.07σ in overall GPA across specifications. The interaction coefficients discussed below (δ_s in equation (1)) net out this gap and, under the identifying assumption that selection into treatment is invariant across ages at exposure, they identify the causal effects of parental victimization occurring during 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 (columns 1-3 of Table 3), estimated by the "Treated $\times$ Exposed" coefficient, implies a decline in mathematics scores of 0.03σ, depending on the specification. In contrast, the corresponding estimates for parental property crime victimization are small and not statistically different from zero (see Appendix Table A.3). 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 offences, undetectable spillovers of property crimes on children's educational outcomes appear plausible. Henceforth, we focus our analysis

on violent crimes.

Second, the timing of exposure matters greatly. When we decompose the effect by childhood stage (columns 4–6 of Table 3), we uncover a steep gradient in the impact of violent victimization. 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. The estimated effects of exposure during mid-childhood and adolescence are negative but smaller in magnitude and not statistically significant. The age profiles in Figure 1 mirror this pattern: coefficients are close to zero for children whose parents are victimized after age 16 – by construction, a placebo group for whom end-of-school achievement has already been realized – while the largest deviations from zero occur for exposure during the earliest years of life. 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).

Third, the results are robust across a range of specifications that progressively tighten our control for confounding factors. Columns (1) and (4) of Table 3 include only age-at-exposure, cohort, and calendar-year dummies, while columns (2) and (5) add rich parental and child covariates measured in the base year, and columns (3) and (6) further control for parish fixed effects, a very granular spatial unit (there are $2,248$ parishes in our estimation sample). The point estimates for violent victimization are remarkably stable across these specifications, and the precision of the estimates increases as we account for additional sources of residual variation. This stability suggests that our matched Difference-in-Differences design is effective at addressing selection on observables across different stages of childhood, and that the remaining identifying variation is not driven by compositional changes correlated with parental victimization.

To benchmark the magnitude of our estimates, we compare them to the small set of studies in economics that report causal effects of domestic violence or family victimization on child test scores in standard-deviation units. Carrell and Hoekstra (2010) find that

a child's own exposure to domestic violence, reduces the child's composite reading and math score by approximately 0.13σ, whereas the spillover effect on classmates exposed only through peer proximity is an order of magnitude smaller, at roughly 0.02σ, a gap that persists into adolescence (Carrell and Kuka, 2018).

In a Nordic welfare-state setting closer to our own, Bhuller et al. (2024) use a regression discontinuity design around the date of a police-reported domestic violence incident in Norway and find an 0.08σ decline in national exam scores for the victimized child. Our estimated effect of parental victimization on child test scores (0.03σ) is therefore markedly smaller than the direct effects reported in this literature ($0.07 - 0.13\sigma$), and instead falls within the range typically associated with spillover effects (0.02σ) rather than direct victimization effects. This aligns with our treatment measuring indirect exposure to victimization through parents.

We additionally show that parental victimization negatively affects children's educational trajectories. Table 4 reports estimates of equation (1) where the dependent variables measure school enrollment at age 16, after completing compulsory school. Children exposed to parental violent victimisation in early or mid childhood are $2-3$ p.p. more likely to have experienced disrupted educational trajectories, such as grade retention, or failure to complete a grade ("roadblock" index, column 1). We then consider four mutually exclusive indicators in columns (2)-(5), marking the type of institution where students are enrolled. Parental victimisation in early childhood increases the probability of taking an extra year in elementary education (1.8 p.p., column 3), and decreases the probability of being enrolled in general programmes, either academic or technical, though the estimate is imprecise (-1.4 p.p., column 5). Finally, children exposed in early childhood attend institutions with significantly lower peer quality (-0.03σ, column 6), measured by the average end-of-compulsory-school mathematics score among students in the same cohort attending the same institution.

Taken together, the main results show that violent victimization of previously non-criminal parents has a persistent and age-dependent negative impact on their children's human capital, while property crime victimization does not. The concentration of effects

in early childhood suggests that the home environment during the first years of life plays a critical role in mediating the consequences of parental shocks. In the next subsection, we examine how these patterns vary by parent and child gender, and Section 4 then explores the behavioral, health, and economic channels that underlie these reduced-form effects.

3.2 Heterogeneous Effects

The average effects in Table 3 mask substantial heterogeneity across families. Table 5 reports estimates from specifications that allow treatment effects to differ by the gender of the victimized parent and the child. Across all panels, the dependent variable is the standardized examination score in mathematics at the end of compulsory schooling.

Columns (1)-(4) of Table 5 show that the penalties in children's achievement are concentrated among paternal victimisations, reaching 0.09σ in early childhood, while maternal events have undetectable impacts. Interestingly, our model attributes markedly larger selection on unobservables, captured by the "Treated" coefficients (δ_1 in equation (1)) to maternal compared to paternal victimisations. This possibly suggests less frequent interactions with risky environments for mothers, and stronger selection among those who become victims of crime. 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 strong changes in paternal criminal behavior and with the larger labor market losses for fathers that we document in Section 4 below.

We additionally find larger achievement penalties for daughters, especially in middle and late childhood. Columns (5)–(8) of Table 5 examine heterogeneity by child gender. Despite exhibiting similar penalties than boys in early childhood (0.05σ versus 0.04σ), girls additionally experience significant penalties when exposed at later stages (0.04σ in middle childhood and 0.06σ in adolescence).

Achievement penalties for children exposed during early childhood are largest in the middle of the potential achievement distribution. Figure 2 plots the estimated treatment effects across the achievement distribution, with dashed vertical lines marking quartile

thresholds.[7] Negative effects are substantially larger for children exposed by age 5 (Panel A). For this group, the probability of scoring at or above the median decreases by approximately 3 p.p. Regardless of the timing of parental victimization, treatment effects converge to zero in the upper achievement quartile, indicating that high-performing students are largely shielded from negative effects.

From a policy perspective, the heterogeneity documented in Table 5 suggests that interventions targeted at families in which fathers have been victimized may have especially large benefits. The concentration of effects among children exposed in early childhood implies that timely support, such as parenting programmes, mental health services, and stable, high-quality childcare, could play an important role in preventing the emergence of persistent gaps in educational achievement.

4 Mechanisms

The results in Section 3 show that parental violent victimization reduces children's end-of-school achievement, with effects concentrated in early childhood and disproportionately large for females and children of victimized fathers. While these reduced-form estimates establish that parental victimization represents a sizable shock to the production of human capital, they are silent on the underlying mechanisms. In this section, we open the black box by tracing how victimization reshapes parental behavior, family resources, and the broader environment in which children grow up.

4.1 Effects on Parents' Outcomes

We analyse the dynamic evolution of parental outcomes following victimization. We characterize these responses by estimating a series of event studies around victimization

[7] 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 assigned a value of zero, effectively treating non-participation as falling below the lowest threshold.

using the following specification:

$$Y_{it} = \theta_0 + \theta_1 T_i + \sum_{l=-6}^{10} \alpha_l T_i \cdot \mathbb{1}[t = t_i^* + 1 + l] + \sum_{l=-6}^{10} \mu_l \cdot \mathbb{1}[t = t_i^* + 1 + l] + \psi_i + \psi_t + \zeta_{it}, \quad (3)$$

where Y_{it} denotes a parental outcome for child i in year t. We normalize event time such that $l = 0$ corresponds to the year of parental victimization ($t_i^* + 1$), and estimate the coefficients α_l relative to the baseline year $l = -1$. By leveraging the panel structure of our data, we include child fixed effects (ψ_i), paralleling the standard worker fixed effects approach in the literature on job loss (e.g., Bertheau et al., 2023). The coefficients α_l therefore capture the dynamic causal effects of victimization on parental trajectories relative to the control group. We estimate this model separately by childhood developmental stage and present the results in Figure 3.

As a first-order effect, violent victimization impairs parental physical health. Panel A of Figure 3 shows very large spikes in the likelihood of visiting a hospital in the year of victimization, which are more pronounced for fathers than for mothers. However, this large initial effect is not persistent, as hospitalizations quickly converge back to levels similar to those observed in the control group. Treated parents also exhibit an increase in access to mental health-related care in the years following the report, with slightly more persistent effects (Panel B). We do not find comparably strong evidence of increased access to mental healthcare for children themselves (Panel A of Figure A.3).

The rise in mental-health contacts could partly reflect the take-up of subsidized psychological help offered to crime victims. Under Denmark's subsidized psychologist scheme, general practitioners refer eligible patients to specialist psychological treatment at reduced out-of-pocket cost, and victims of violent crime are a designated referral group.[8] A victimized parent thus becomes eligible for subsidized specialist care on the basis of the victimization itself, and Panels A-B of Appendix Figure A.4 show that subsidized psy-

[8] Treatment is provided under the practice-sector agreement on psychological care (*praksisoverenskomst om psykologhjælp*), 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, after our 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).

chologist use rises following victimization, tracking the mental-health pattern in Panel B of Figure 3. The absence of a comparable response for children (Panel C of Figure A.4) is consistent with the design of the scheme: it covers direct victims but has no referral category for the relatives or children of a crime victim, who therefore lack a dedicated route into subsidized psychological care. The children our estimates identify as harmed sit outside the support system built around the victim, an institutional gap we return to in Section 5.

Extending recent work on the effects of victimization on victims' own outcomes (Bindler and Ketel, 2022; Dustmann and Mertz, 2026), Panel C of Figure 3 shows that treated parents, and fathers in particular, become more likely to be charged with a violent crime in the years after victimization relative to their matched controls. 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 and cannot corroborate the post-event increase as it does for our other outcomes. We therefore treat this pattern as an association consistent with a behavioral response to victimization.

Victimization seriously impairs parental labor market outcomes, in line with findings by Bindler and Ketel (2022) and Dustmann and Mertz (2026). Panel D of Figure 3 shows immediate and persistent penalties in total labor earnings, which increase in magnitude over time and are, on average, larger for paternal victimizations. The earnings penalties mostly operate through the intensive margin (i.e., lower earnings conditional on working) for fathers, especially for victimizations during early childhood, while they are mostly explained by extensive-margin (i.e., employment) adjustments for mothers (Panels C and D of Figure A.3). Larger labor market penalties for fathers are consistent with the prevalence of intergenerational effects from paternal victimizations singled out by the heterogeneity analysis of Table 5. 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 (Panel B of Figure A.3).

In summary, these results show that the average trajectories of parents after victimiza-

tion mask the key forces driving children's losses in achievement. While parental victimization consistently worsens health, labor market attachment, family stability, and criminal behavior, these average responses do not line up one-for-one with the much larger penalties observed for exposure in early childhood and for paternal victimization. Instead, the evidence points to highly uneven and concentrated impacts, suggesting that only specific, severe patterns of behavioral and relational disruption translate into sizable reductions in children's human capital. In the next subsection, we exploit this heterogeneity across parental responses to pinpoint which dimensions of the victimization shock, particularly those linked to family instability, paternal behavior, and labor market detachment, are most strongly associated with declines in achievement.

4.2 Parental Responses and Children's Achievement Effects

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.

We instead follow the approach in Britto, Melo and Sampaio (2022) and Schmieder, von Wachter and Heining (2023), projecting individual-level difference-in-differences estimates of parental outcomes onto *baseline*, pre-victimization characteristics, and using these predicted values, rather than realized ones, to stratify the sample. Because the projection relies only on information determined before treatment, this procedure is immune

to the endogeneity problem that undermines standard mediation analysis, at the cost of identifying heterogeneity in effects across *predicted* family responses rather than formally partitioning the total effect into mechanism-specific shares.

Achievement reductions following parental victimization are most strongly associated with proxies for time spent with the victimized parent, 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 terciles of predicted changes in parental outcomes following victimization. Figure 4 plots estimated treatment effects on achievement by tercile against the average change in the corresponding parental outcome observed in each group. Across childhood stages, the negative effects of parental victimization are stronger when, after treatment, the reporting parent works fewer weeks (Panel C) or is more likely to be charged with violent crimes (Panel D), when the risk of parental divorce is higher (Panel G), and when moving to a different parish is more likely (Panel H). In contrast, achievement effects are largely uncorrelated with unemployment duration (Panel B) and hospitalization (Panel E).

Among children exposed to parental victimization in early childhood, achievement reductions also correlate with changes in family income and parental mental health. Within this group, negative treatment effects emerge only for children whose families experience the largest post-victimization income losses (Panel A of Figure 4). Yet income alone does not appear to drive our results: the link between income changes and achievement penalties is much weaker at later childhood stages, and heterogeneity by breadwinner status offers no support for income-based explanations. If anything, Appendix Table A.4 shows that, for paternal victimizations, achievement penalties are larger when the victimized father is not the household's main earner. Finally, we find somewhat counterintuitively that larger negative effects arise among children whose victimized parents are predicted to have *fewer* mental health-related visits (Panel F of Figure 4). We interpret this pattern as suggestive of a protective role for mental healthcare, insofar as seeking or accessing treatment may help stabilize the home environment after victimization. Panel B of Figure A.5, focusing on psychologist contacts, shows such pattern in child's healthcare records

as well.

Overall, our evidence points to parental victimization operating less through a single dominant channel and more through a broad erosion of the home environment. Rather than isolating one mechanism, the results show that children's achievement losses are systematically amplified when victimization triggers simultaneous deterioration in behavioral stability, family relationships, residential continuity, and financial resources. This pattern is clearest in Figure 4, where the largest penalties cluster in terciles characterized by severe disruptions along several of these dimensions, especially for children exposed in early childhood or adolescence, the stages at which children's outcomes are most tightly linked to family inputs (Carneiro et al., 2021). Together, these findings underscore that it is the compounding of behavioral, relational, residential, and financial shocks, rather than any single factor, that transforms parental victimization into a powerful driver of long-run losses in children's human capital.

5 Conclusion

This paper provides the first causal evidence on the intergenerational spillovers of criminal victimization outside the domestic sphere. Linking the universe of Danish police records to administrative data on families and educational outcomes, we show that the social costs of crime extend beyond the immediate victim to the next generation, impeding children's human capital accumulation. Our results suggest the existence of "secondary victims", an established concept in victimology but largely absent from the economics of human capital, and document that children of victimized parents suffer measurable and persistent educational penalties as a result.

Our results reveal that parental violent victimization reduces a child's end-of-school achievement by an average of 0.03 standard deviations. These effect sizes are markedly smaller than the direct impacts of domestic violence on children's own test scores documented by Carrell and Hoekstra (2010) and Bhuller et al. (2024), but closer in magnitude to the spillover effects on classmates identified by Carrell and Kuka (2018)

Aggregate effects mask critical heterogeneity regarding the timing of exposure. Con-

sistent with models of skill formation emphasising dynamic complementarity (Cunha and Heckman, 2006; Almond and Currie, 2011), we find that the penalty is doubled (0.05σ) when victimization occurs during early childhood. Our finding challenges the presumption that very young children are shielded from external shocks to their parents. Instead, our evidence suggests that disruptions to the home environment during these formative years, a period when parental inputs are most essential, leave permanent scars on children's cognitive development that schooling alone cannot remediate.

Two sets of mechanisms emerge. First, consistent with the scarring effects on victims documented by Bindler and Ketel (2022), victimization triggers lasting parental labor market detachment and earnings losses, alongside increased family instability, divorce, and residential dislocation, reducing the resources available for investment in the child. Second, victimized parents, and fathers in particular, appear more likely to be charged with violent offences in subsequent years. Because our sample excludes parents with prior charges, this pattern is suggestive rather than cleanly identified, but it is consistent with a behavioral response linking victimization to later aggression.

These findings suggest that cost-of-crime estimates omitting intergenerational spillovers understate the social returns to crime reduction. They also suggest that victim support services, which typically focus on the legal and medical needs of the adult victim, could usefully extend to the needs of children in the household. The concentration of negative effects in early childhood suggests that timely interventions, such as access to high-quality childcare or family counselling, could mitigate the damage before it becomes a permanent educational deficit. Treating the family unit rather than the individual victim may allow policymakers to interrupt this transmission of disadvantage across generations.

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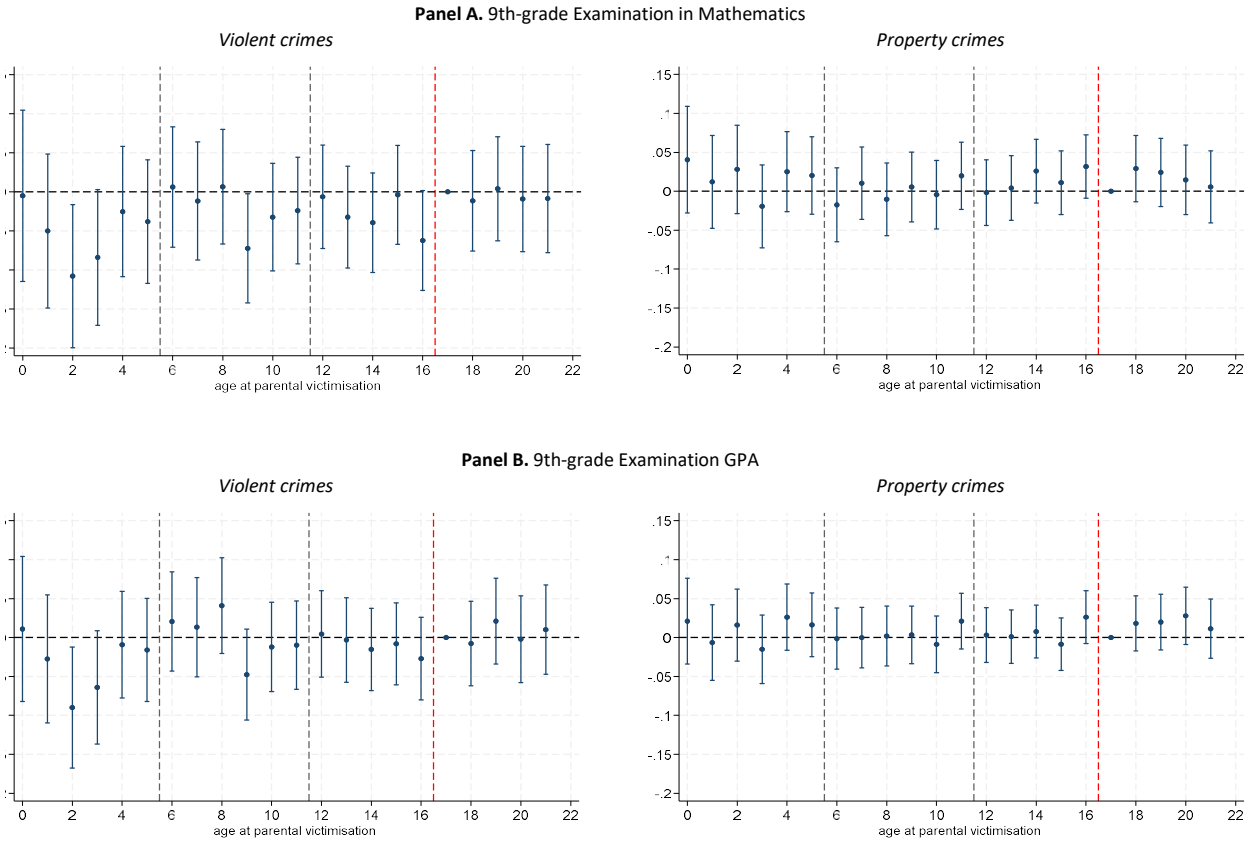


Figure 1: Impacts of Parental Victimization by age at treatment

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. Within each panel, left-hand graphs consider children exposed to violent parental victimization and right-hand graphs consider children exposed to property parental victimization. The estimation sample include 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.

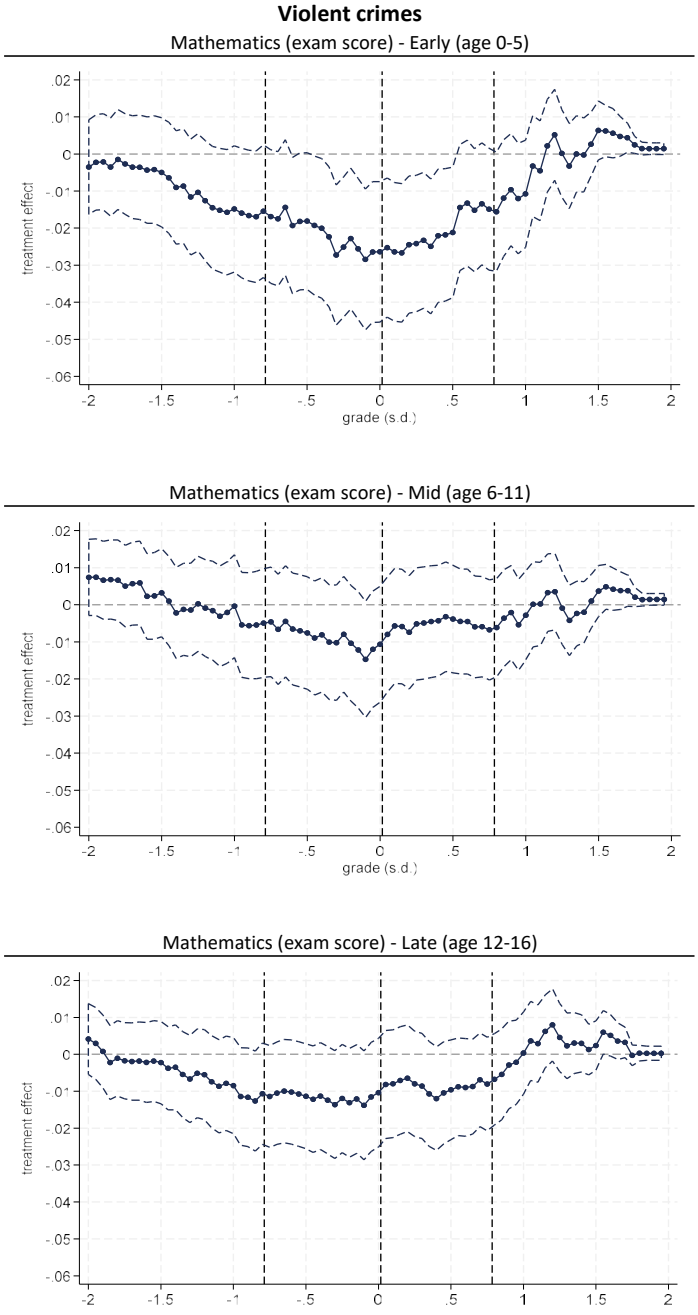


Figure 2: Distributional analysis

Note. The figure plots estimates of the distributional impacts of parental victimisation 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. Outcomes are coded as zero for children who do not receive an examination score. Panels A, B, and C plot treatment effects on children exposed during 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 (6) of Table 3.

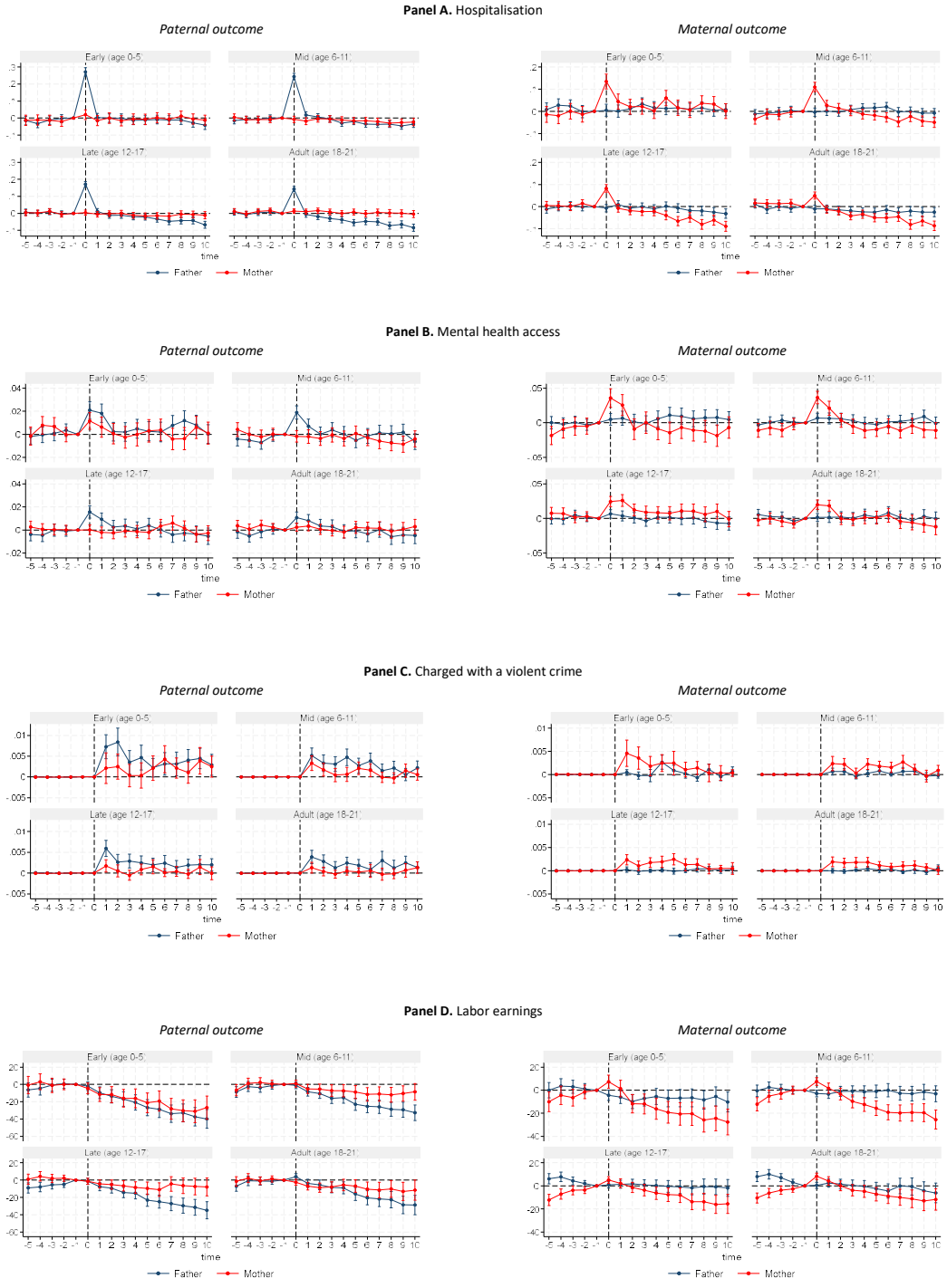


Figure 3: Parental Event Studies

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: an indicator for visiting an hospital in the year (Panel A); an indicator for healthcare access for mental-health related reasons in the year (Panel B); an indicator for being charged with a violent crime in the year (Panel C); and total annual labor earnings (Panel D). Separate estimates are plotted by childhood stage in subpanels. Within each panel, left-hand graphs consider paternal outcomes and right-hand graphs consider maternal outcomes. Red connected markers refer to maternal victimizations, while blue connected markers refer to paternal victimizations. Spikes around point estimates represent the 95% confidence interval.

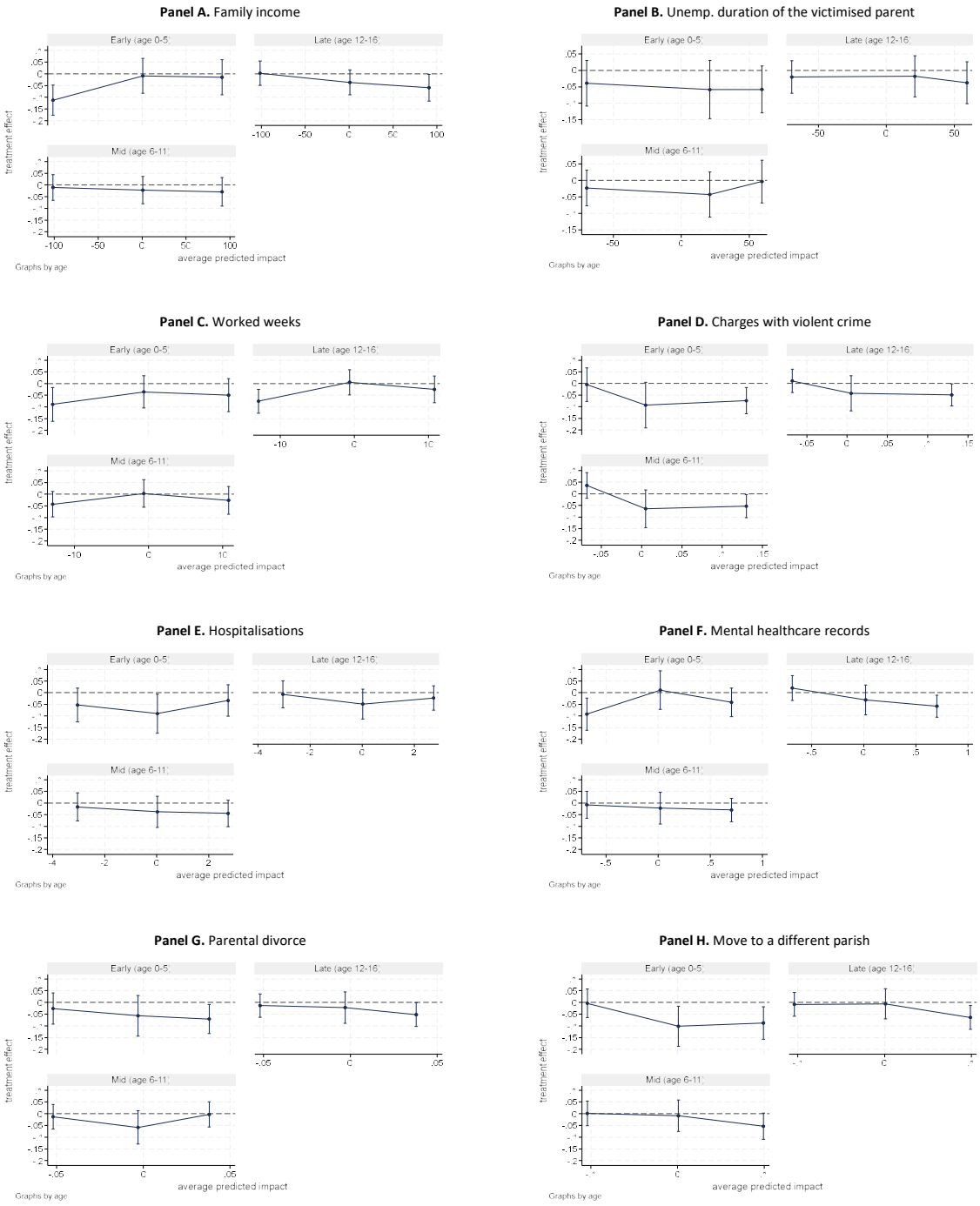


Figure 4: Heterogeneity by predicted victimization impact on parental outcomes

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 are analogous to column (6) of Table 3. Changes in parental outcome are computed as predicted values of individual-level difference-in-differences in parental outcomes around victimization from a regression on baseline parental and children characteristics. The considered parental outcome is indicated in a panel's header. The x-axis measures the average predicted change in the considered tercile.

Table 1: Descriptive Statistics on Treated and Untreated Children

	Treated		Untreated		T-U	
	Mean	SD	Mean	SD	Diff.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Violent crime			
Age	11.983	5.742	10.582	6.174	1.401	0.000
Female	0.489	0.500	0.487	0.500	0.002	0.318
Danish	0.899	0.301	0.926	0.262	-0.026	0.000
Father's age	42.091	12.645	40.838	13.056	1.253	0.000
Mother's age	41.706	7.454	40.817	7.737	0.889	0.000
Father's age at first birth	28.700	8.568	29.053	8.955	-0.353	0.000
Mother's age at first birth	27.546	4.386	28.268	4.411	-0.722	0.000
Father's education (months)	150.365	46.411	151.860	48.482	-1.496	0.000
Mother's education (months)	160.141	31.770	161.485	31.242	-1.345	0.000
Father employed	0.792	0.406	0.806	0.395	-0.014	0.000
Mother employed	0.821	0.383	0.850	0.357	-0.029	0.000
Father's earnings ('000s DKK)	385.527	299.280	424.674	315.887	-39.146	0.000
Mother's earnings ('000s DKK)	312.500	228.105	314.358	218.872	-1.859	0.040
Family income ('000s DKK)	913.802	842.542	947.580	1589.202	-33.778	0.000
Danish families in local area	0.877	0.142	0.904	0.118	-0.027	0.000
Education in local area	170.327	8.095	170.586	8.297	-0.259	0.000
Treated in local area	0.010	0.010	0.004	0.005	0.006	0.000
Mathematics score	-0.076	0.984	0.138	0.964	-0.214	0.000
GPA	-0.047	0.847	0.104	0.831	-0.151	0.000
N	58,864		21,700,110			
			Panel B. Property crime			
Age	11.656	5.775	10.582	6.174	1.074	0.000
Female	0.502	0.500	0.485	0.500	0.017	0.000
Danish	0.917	0.277	0.925	0.263	-0.008	0.000
Father's age	42.222	13.069	40.754	13.037	1.469	0.000
Mother's age	42.318	7.274	40.708	7.752	1.611	0.000
Father's age at first birth	29.291	8.965	28.968	8.931	0.324	0.000
Mother's age at first birth	28.572	4.396	28.157	4.410	0.415	0.000
Father's education (months)	155.683	49.051	151.098	48.259	4.585	0.000
Mother's education (months)	165.751	31.760	160.697	31.149	5.053	0.000
Father employed	0.803	0.398	0.805	0.396	-0.002	0.063
Mother employed	0.850	0.357	0.848	0.359	0.003	0.008
Father's earnings ('000s DKK)	447.559	340.019	418.427	311.525	29.132	0.000
Mother's earnings ('000s DKK)	342.347	238.280	310.363	216.292	31.984	0.000
Family income ('000s DKK)	1023.580	1167.332	934.042	1629.934	89.538	0.000
Danish families in local area	0.868	0.143	0.903	0.121	-0.035	0.000
Education in local area	172.228	8.851	169.581	8.057	2.647	0.000
Treated in local area	0.016	0.011	0.007	0.008	0.009	0.000
Mathematics score	0.126	0.963	0.123	0.967	0.003	0.339
GPA	0.149	0.818	0.084	0.834	0.065	0.000
N	133,585		20,026,102			

Note. The table reports descriptive statistics comparing children exposed to parental victimization (columns 1-2) with untreated children (columns 3-4). Columns (5) and (6) report the difference between columns (1) and (3) and the p-value of the associated t-statistics, respectively. Panel A and Panel B consider, respectively, parental violent victimizations and parental property victimizations.

Table 2: Descriptive Statistics on Treated and Control Children

	Treated		Control		T-C	
	(1) Mean	(2) SD	(3) Mean	(4) SD	(5) Diff.	(6) p-value
			Panel A. Violent crime			
Age	11.923	5.745	11.923	5.745	0.000	1.000
Female	0.488	0.500	0.488	0.500	0.000	1.000
Danish	0.903	0.296	0.899	0.301	0.003	0.051
Father's age	41.927	12.695	41.767	12.798	0.160	0.036
Mother's age	41.655	7.448	41.567	7.460	0.088	0.048
Father's age at first birth	28.611	8.590	28.487	8.646	0.124	0.015
Mother's age at first birth	27.548	4.378	27.472	4.337	0.076	0.004
Father's education (months)	150.351	46.780	149.971	47.561	0.379	0.177
Mother's education (months)	160.430	31.648	160.265	32.378	0.165	0.387
Father employed	0.811	0.392	0.809	0.393	0.002	0.313
Mother employed	0.831	0.375	0.828	0.377	0.003	0.214
Father's earnings ('000s DKK)	394.975	296.782	390.498	299.330	4.477	0.012
Mother's earnings ('000s DKK)	316.247	226.613	314.525	232.439	1.723	0.208
Family income ('000s DKK)	927.191	756.554	921.910	1440.957	5.281	0.441
Danish families in local area	0.878	0.141	0.872	0.149	0.006	0.000
Education in local area	170.343	8.087	170.297	8.278	0.047	0.337
Treated in local area	0.010	0.009	0.009	0.010	0.001	0.000
Mathematics score	-0.072	0.984	0.050	0.978	-0.122	0.000
GPA	-0.043	0.846	0.031	0.847	-0.075	0.000
N	56,382		56,382			
			Panel B. Property crime			
Age	11.544	5.779	11.544	5.779	0.000	1.000
Female	0.500	0.500	0.500	0.500	0.000	1.000
Danish	0.920	0.272	0.919	0.273	0.001	0.419
Father's age	42.003	13.088	42.020	12.996	-0.017	0.740
Mother's age	42.192	7.256	42.191	7.254	0.001	0.983
Father's age at first birth	29.196	8.978	29.214	8.888	-0.019	0.598
Mother's age at first birth	28.552	4.383	28.564	4.366	-0.012	0.475
Father's education (months)	155.679	49.450	156.222	49.139	-0.543	0.005
Mother's education (months)	165.940	31.654	166.050	31.224	-0.110	0.378
Father employed	0.824	0.381	0.826	0.379	-0.003	0.052
Mother employed	0.857	0.350	0.858	0.349	-0.001	0.394
Father's earnings ('000s DKK)	458.454	336.159	463.645	338.571	-5.191	0.000
Mother's earnings ('000s DKK)	344.897	236.816	347.410	241.552	-2.513	0.008
Family income ('000s DKK)	1038.103	1002.325	1036.901	1159.408	1.202	0.780
Danish families in local area	0.870	0.142	0.867	0.147	0.003	0.000
Education in local area	172.206	8.823	172.409	9.005	-0.203	0.000
Treated in local area	0.016	0.010	0.016	0.011	0.000	0.000
Mathematics score	0.131	0.962	0.179	0.966	-0.047	0.000
GPA	0.152	0.817	0.166	0.824	-0.014	0.000
N	126,914		126,914			

Note. The table reports descriptive statistics comparing treated (columns 1-2) and control children (columns 3-4). The sample is obtained through a 1:1 matching algorithm without replacement of each treated child to an untreated child of the same age and with the same own and parent's gender. Columns (5) and (6) report the difference between columns (1) and (3) and the p-value of the associated t-statistics, respectively. Panel A and Panel B consider, respectively, parental violent victimizations and parental property victimizations.

Table 3: Impacts of Parental Violent Victimization on Child's Achievement

	(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
Parental age and cohort dummies	X	X	X	X	X	X
Parent and child controls		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 year, cohort, and age at real or placebo parental victimization. Columns (2) and (5) add 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 4: Impacts of Parental Violent Victimization on Child's School Enrollment

	Roadblock	Dropout	Compulsory school	Vocational Programme	General Programme	Peer Quality
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.0281***	-0.0001	0.0083	0.0005	-0.0087*	-0.0240***
	(0.0046)	(0.0034)	(0.0057)	(0.0019)	(0.0052)	(0.0054)
Treated X Early (age 0-5)	0.0291***	0.0010	0.0177*	-0.0043	-0.0144	-0.0268***
	(0.0092)	(0.0056)	(0.0100)	(0.0038)	(0.0092)	(0.0100)
Treated X Mid (age 6-11)	0.0242***	-0.0034	-0.0028	-0.0026	0.0088	-0.0059
	(0.0071)	(0.0048)	(0.0082)	(0.0028)	(0.0075)	(0.0080)
Treated X Late (age 12-16)	0.0032	0.0057	-0.0087	-0.0012	0.0042	-0.0004
	(0.0064)	(0.0046)	(0.0078)	(0.0027)	(0.0070)	(0.0074)
N	106,267	107,187	107,187	107,187	107,187	99,490
Parental age and cohort dummies	X	X	X	X	X	X
Parent and child controls	X	X	X	X	X	X
Parish dummies	X	X	X	X	X	X

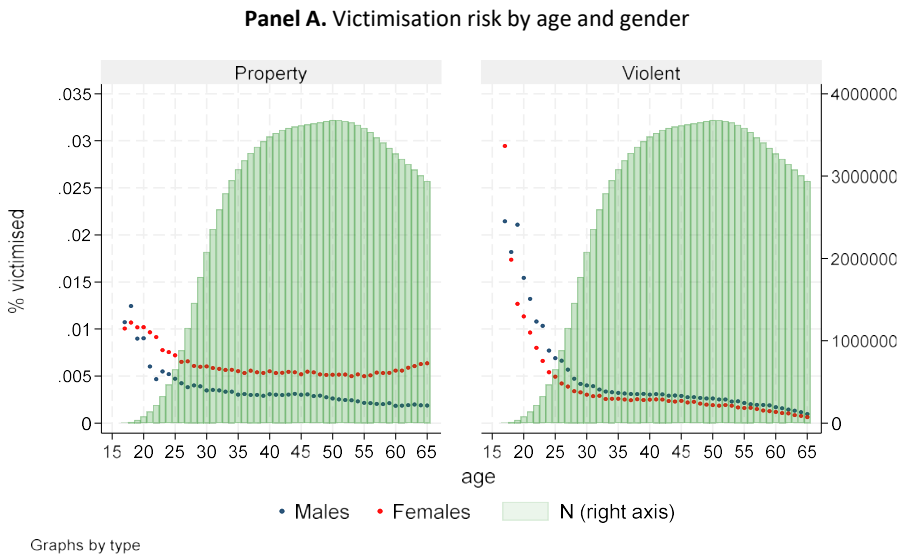
Note. The table reports estimates of δ_1 ("Treated") and δ_s (interactions) coefficients in Equation (1) on post-compulsory school enrollment (age 16). Estimated specifications are similar to column (6) of Table 3. The dependent variable in column (1) is an index of disrupted educational trajectories, taking value one if the student have experienced grade repetition, uncompleted educational spells, or more than one spell in a given academic year. Columns (2)-(5) indicate the type of institution where child enrolls, with "general programme" in column (5) defined as either academic or technical high school track. Peer quality in column (6) is the average mathematics score at 9-th grade examination of students enrolled at the same institution in the year when child is observed. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Violent Crime: Heterogeneous Impacts by Victimized Parent and Child Gender

	Father		Mother		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.0567***	-0.0567***	-0.1516***	-0.1526***	-0.1190***	-0.1191***	-0.0890***	-0.0893***
	(0.0163)	(0.0164)	(0.0162)	(0.0162)	(0.0161)	(0.0162)	(0.0159)	(0.0159)
Treated X Exposed	-0.0652***		0.0123		0.0033		-0.0532***	
	(0.0188)		(0.0193)		(0.0190)		(0.0187)	
Treated X Early (age 0-5)		-0.0913***		-0.0024		-0.0383		-0.0532*
		(0.0267)		(0.0312)		(0.0284)		(0.0281)
Treated X Mid (age 6-11)		-0.0733***		0.0408*		0.0110		-0.0422*
		(0.0228)		(0.0242)		(0.0233)		(0.0230)
Treated X Late (age 12-16)		-0.0455**		-0.0040		0.0128		-0.0623***
		(0.0217)		(0.0221)		(0.0219)		(0.0217)
N	51,485	51,486	45,080	45,082	48,747	48,749	47,854	47,857
Parental age and cohort dummies	X	X	X	X	X	X	X	X
Parent and child controls	X	X	X	X	X	X	X	X
Parish dummies	X	X	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. Estimation and specification are analogous to columns (3) and (6) of Table 3. The sample is restricted to subgroups defined by the child's and the victimized parent's gender as indicated in column headers. Standard errors are clustered at the family level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix (For On-line Publication Only)



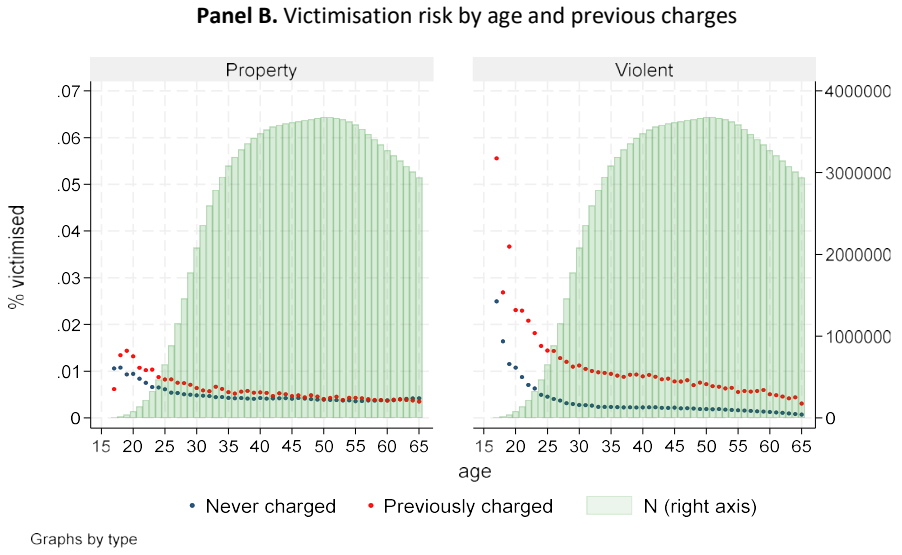


Figure A.1: Victimization risk of parents

Note. The figure plots parents' victimisation 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 2003 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.

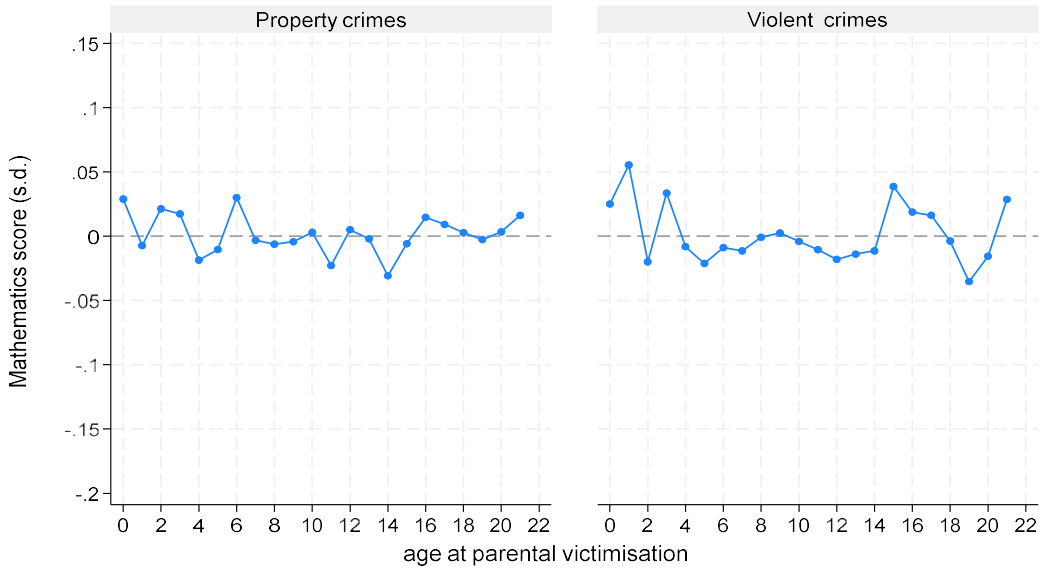


Figure A.2: Achievement of matched control children

Note. The figure plots control children's 9th-grade examination score in mathematics against age at placebo parental victimization, separately for the property crime and the violent crime samples. Examination score is standardized by year and computed as residual from a regression on year and cohort dummies.

Figure A.3: Mechanisms: 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: an indicator for child's mental-healthcare contact in the year (Panel A); indicators for parental divorce or child moving to a different parish in the year (Panel B) parental weeks worked in the year (Panel C); parental log earnings in the year (Panel D). Separate estimates are plotted by childhood stage in subpanels. Within Panels C and D, left-hand graphs consider paternal outcomes and right-hand graphs consider maternal outcomes. Red connected markers refer to maternal victimizations, while blue connected markers refer to paternal victimizations. Spikes around point estimates represent the 95% confidence interval.

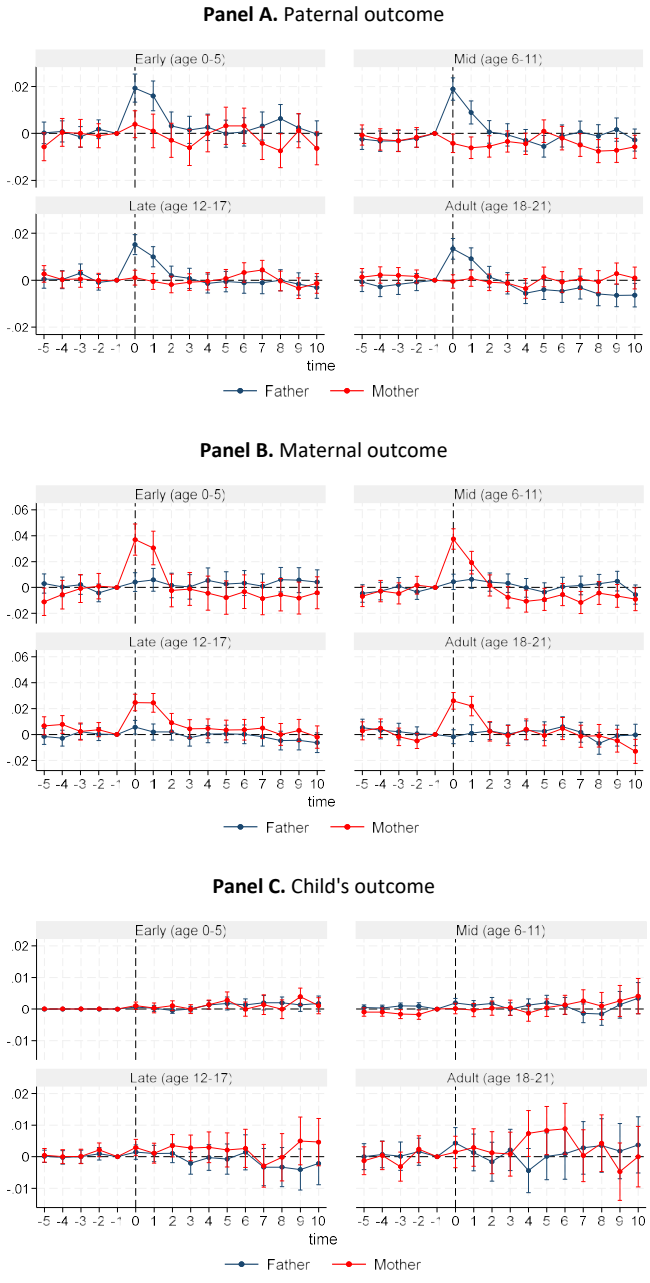


Figure A.4: Psychologist contact

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 child's (Panel A), maternal (Panel B), or paternal (Panel C) contact with a psychologist in the year. Separate estimates are plotted by childhood stage in subpanels. Red connected markers refer to maternal victimizations, while blue connected markers refer to paternal victimizations. Spikes around point estimates represent the 95% confidence interval.

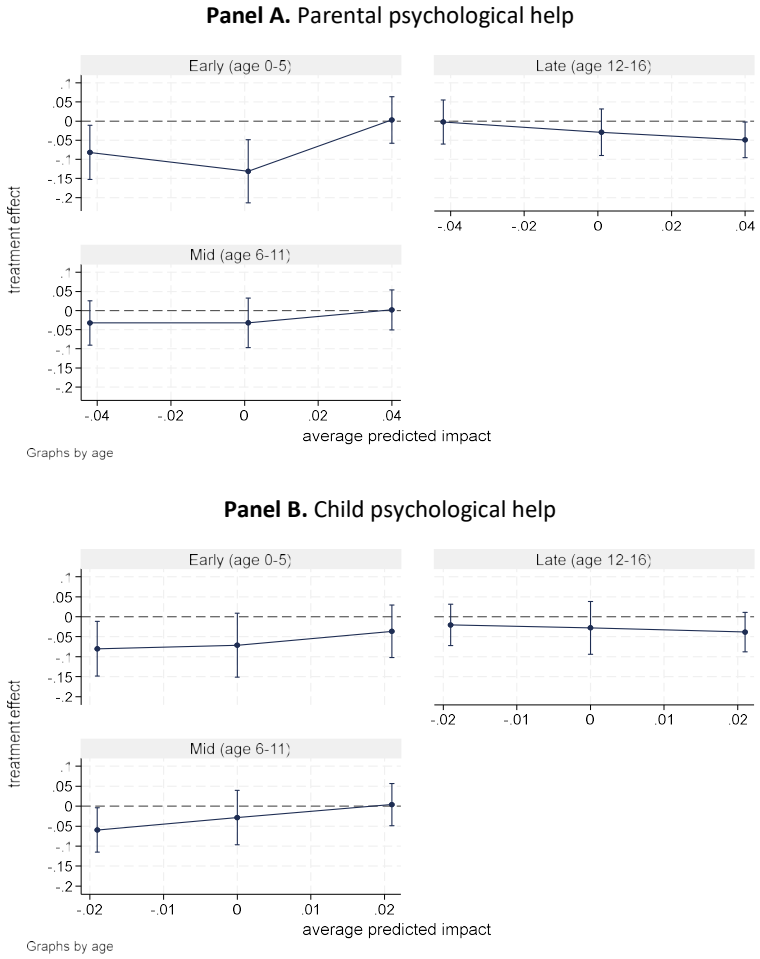


Figure A.5: Heterogeneity by predicted contact with a psychologist

Note. The figure plots heterogeneous effects of parental victimization on children's learning by predicted changes in contact with psychologist . Graphed are coefficients from Equation (1) estimated separately by predicted change tercile, with specifications are analogous to column (6) of Table 3. Predicted changes in contact with psychologist are computed as predicted values of individual-level difference-in-differences in parental outcomes around victimization from a regression on baseline parental and children characteristics. Panel A predicts the likelihood of at least one contact for either parent, Panel B for the child. The x-axis measures the average predicted change in the considered tercile.

Table A.1: Descriptive Statistics on Treated and Untreated Children by Childhood Stage

	Treated		Untreated		T-U	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	SD	Mean	SD	Diff.	p-value
	Panel A. Early childhood (age 0-5)					
Age	1.955	1.632	1.713	1.694	0.243	0.000
Female	0.486	0.500	0.488	0.500	-0.001	0.806
Danish	0.888	0.315	0.931	0.253	-0.043	0.000
Father's age	33.263	9.685	33.278	9.361	-0.016	0.884
Mother's age	32.261	5.329	32.464	4.871	-0.203	0.000
Father's age at first birth	28.696	8.308	29.306	8.288	-0.610	0.000
Mother's age at first birth	27.550	4.350	28.302	4.222	-0.752	0.000
Father's education (months)	151.881	44.790	155.511	45.329	-3.630	0.000
Mother's education (months)	161.559	31.536	164.367	29.889	-2.808	0.000
Father employed	0.829	0.376	0.847	0.360	-0.017	0.000
Mother employed	0.786	0.410	0.832	0.374	-0.045	0.000
Family income ('000s DKK)	761.758	845.711	807.735	939.850	-45.977	0.000
Education in local area	169.564	8.014	169.754	8.234	-0.190	0.046
Treated in local area	0.009	0.007	0.003	0.004	0.006	0.000
N	7,603		4,375,969			
	Panel B. Mid childhood (age 6-11)					
Age	7.769	1.693	7.618	1.696	0.151	0.000
Female	0.496	0.500	0.487	0.500	0.009	0.027
Danish	0.889	0.314	0.928	0.258	-0.039	0.000
Father's age	38.645	10.513	38.179	11.090	0.467	0.000
Mother's age	37.883	5.274	37.913	5.104	-0.029	0.485
Father's age at first birth	28.736	8.269	29.091	8.879	-0.355	0.000
Mother's age at first birth	27.498	4.313	28.284	4.369	-0.786	0.000
Father's education (months)	151.712	44.792	152.271	48.056	-0.559	0.160
Mother's education (months)	161.065	31.424	161.857	30.801	-0.792	0.002
Father employed	0.811	0.392	0.819	0.385	-0.008	0.009
Mother employed	0.819	0.385	0.857	0.350	-0.037	0.000
Family income ('000s DKK)	870.617	605.312	911.536	1202.425	-40.919	0.000
Education in local area	169.813	7.919	170.008	8.120	-0.195	0.004
Treated in local area	0.009	0.009	0.003	0.004	0.006	0.000
N	14,538		6,088,614			
	Panel C. Adolescence (age12-16)					
Age	13.090	1.395	13.001	1.414	0.088	0.000
Female	0.484	0.500	0.487	0.500	-0.003	0.379
Danish	0.900	0.300	0.922	0.268	-0.022	0.000
Father's age	43.305	11.556	42.966	12.512	0.339	0.000
Mother's age	42.800	5.380	43.117	5.395	-0.317	0.000
Father's age at first birth	28.848	8.445	29.009	9.110	-0.162	0.018
Mother's age at first birth	27.601	4.424	28.280	4.494	-0.679	0.000
Father's education (months)	150.998	46.081	151.056	49.267	-0.059	0.874
Mother's education (months)	160.357	32.036	160.871	31.844	-0.514	0.032
Father employed	0.792	0.406	0.799	0.400	-0.007	0.014
Mother employed	0.830	0.376	0.860	0.347	-0.030	0.000
Family income ('000s DKK)	955.544	1020.307	1004.775	1544.146	-49.231	0.000
Education in local area	170.531	8.117	170.917	8.302	-0.386	0.000
Treated in local area	0.010	0.010	0.004	0.005	0.006	0.000
N	17,783		5,546,990			
	Panel D. Young adulthood (age 17-22)					
Age	18.265	1.699	18.344	1.701	-0.079	0.000
Female	0.490	0.500	0.487	0.500	0.003	0.377
Danish	0.912	0.284	0.922	0.268	-0.011	0.000
Father's age	47.204	13.483	47.557	14.105	-0.353	0.001
Mother's age	47.469	5.616	48.239	5.613	-0.769	0.000
Father's age at first birth	28.534	9.004	28.857	9.373	-0.324	0.000
Mother's age at first birth	27.530	4.419	28.211	4.515	-0.681	0.000
Father's education (months)	148.105	48.476	149.360	50.326	-1.255	0.001
Mother's education (months)	158.644	31.825	159.440	31.965	-0.796	0.001
Father employed	0.763	0.426	0.768	0.422	-0.005	0.101
Mother employed	0.828	0.377	0.847	0.360	-0.019	0.000
Family income ('000s DKK)	969.377	804.235	1039.490	2257.826	-70.113	0.000
Danish families in local area	0.878	0.136	0.894	0.123	-0.016	0.000
Education in local area	170.835	8.197	171.524	8.416	-0.688	0.000
Treated in local area	0.010	0.011	0.005	0.005	0.005	0.000
N	18,828		5,620,709			

Note. The table reports descriptive statistics comparing children exposed to parental victimization (columns 1-2) with untreated children (columns 3-4). Columns (5) and (6) report the difference between columns (1) and (3) and the p-value of the associated t-statistics, respectively. Panel A and Panel B consider, respectively, parental victimization for a violent or a property crime.

Table A.2: Descriptive Statistics on Treated and Control Children by Childhood Stage

	Treated		Control		T-C	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	SD	Mean	SD	Diff.	p-value
Panel A. Early childhood (age 0-5)						
Age	1.962	1.632	1.962	1.632	0.000	1.000
Female	0.487	0.500	0.487	0.500	0.000	1.000
Danish	0.891	0.311	0.887	0.317	0.005	0.373
Father's age	33.241	9.717	33.099	9.882	0.142	0.378
Mother's age	32.284	5.299	32.208	5.127	0.076	0.373
Father's age at first birth	28.666	8.339	28.552	8.489	0.114	0.409
Mother's age at first birth	27.552	4.326	27.520	4.201	0.031	0.654
Father's education (months)	151.817	44.924	151.153	46.790	0.664	0.379
Mother's education (months)	161.753	31.204	161.328	32.473	0.424	0.418
Father employed	0.835	0.371	0.832	0.373	0.003	0.675
Mother employed	0.791	0.406	0.785	0.411	0.006	0.354
Family income ('000s DKK)	757.427	384.871	759.518	943.902	-2.091	0.860
Education in local area	169.570	8.012	169.530	8.166	0.040	0.764
Treated in local area	0.009	0.007	0.009	0.008	0.000	0.000
N	7,383		7,383			
Panel B. Mid childhood (age 6-11)						
Age	7.755	1.692	7.755	1.692	0.000	1.000
Female	0.498	0.500	0.498	0.500	0.000	1.000
Danish	0.892	0.310	0.889	0.314	0.003	0.446
Father's age	38.575	10.528	38.359	10.540	0.216	0.084
Mother's age	37.877	5.266	37.713	5.241	0.164	0.009
Father's age at first birth	28.692	8.271	28.526	8.234	0.166	0.090
Mother's age at first birth	27.503	4.307	27.408	4.251	0.095	0.062
Father's education (months)	151.845	44.920	151.625	45.567	0.220	0.683
Mother's education (months)	161.277	31.372	161.099	32.030	0.178	0.637
Father employed	0.826	0.379	0.825	0.380	0.000	0.950
Mother employed	0.826	0.379	0.823	0.382	0.004	0.435
Family income ('000s DKK)	881.403	607.887	873.138	703.475	8.265	0.290
Education in local area	169.828	7.910	169.751	8.035	0.077	0.414
Treated in local area	0.009	0.008	0.009	0.009	0.001	0.000
N	14,154		14,154			
Panel C. Adolescence (age12-16)						
Age	13.079	1.397	13.079	1.397	0.000	1.000
Female	0.481	0.500	0.481	0.500	0.000	1.000
Danish	0.905	0.294	0.900	0.300	0.005	0.140
Father's age	43.188	11.639	43.033	11.837	0.155	0.223
Mother's age	42.797	5.354	42.727	5.466	0.070	0.233
Father's age at first birth	28.770	8.476	28.646	8.604	0.124	0.180
Mother's age at first birth	27.613	4.408	27.519	4.410	0.094	0.050
Father's education (months)	150.997	46.470	150.146	47.353	0.851	0.094
Mother's education (months)	160.706	31.952	160.468	32.556	0.238	0.496
Father employed	0.811	0.391	0.808	0.394	0.003	0.440
Mother employed	0.840	0.367	0.836	0.371	0.004	0.277
Family income ('000s DKK)	969.076	904.382	955.992	1342.595	13.083	0.291
Education in local area	170.557	8.120	170.577	8.400	-0.020	0.827
Treated in local area	0.010	0.009	0.009	0.010	0.001	0.000
N	17,059		17,059			
Panel D. Young adulthood (age 17-22)						
Age	18.265	1.691	18.265	1.691	0.000	1.000
Female	0.486	0.500	0.486	0.500	0.000	1.000
Danish	0.914	0.280	0.912	0.284	0.002	0.441
Father's age	46.991	13.643	46.864	13.708	0.126	0.384
Mother's age	47.457	5.650	47.408	5.605	0.049	0.408
Father's age at first birth	28.372	9.034	28.277	9.058	0.096	0.319
Mother's age at first birth	27.520	4.426	27.459	4.390	0.062	0.188
Father's education (months)	147.933	49.132	147.997	49.525	-0.064	0.902
Mother's education (months)	158.942	31.703	158.965	32.403	-0.023	0.946
Father employed	0.789	0.408	0.786	0.410	0.003	0.484
Mother employed	0.842	0.365	0.842	0.364	-0.001	0.850
Family income ('000s DKK)	993.926	809.148	995.442	2017.546	-1.517	0.926
Education in local area	170.869	8.178	170.781	8.348	0.089	0.311
Treated in local area	0.010	0.010	0.009	0.011	0.001	0.000
N	17,786		17,786			

Note. The table reports descriptive statistics comparing children exposed to parental victimization (columns 1-2) with untreated children (columns 3-4), separately by stage of childhood. The sample is restricted to violent crimes. Columns (5) and (6) report the difference between columns (1) and (3) and the p-value of the associated t-statistics, respectively. Panels A, B, C, and D consider, respectively, parental victimizations occured during early and mid childhood, adolescence, and young adulthood. Average statistics across stages are reported in Table 1.

Table A.3: Impacts of Parental Property Crime Victimization on Child's Achievement

	Mathematics (test scores)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. 9th-grade Examination in Mathematics					
Treated	-0.0416***	-0.0342***	-0.0339***	-0.0416***	-0.0342***	-0.0339***
	(0.0082)	(0.0076)	(0.0076)	(0.0082)	(0.0076)	(0.0076)
Treated X Exposed	-0.0085	-0.0075	-0.0046			
	(0.0094)	(0.0088)	(0.0088)			
Treated X Early (age 0-5)				0.0026	-0.0013	0.0014
				(0.0135)	(0.0126)	(0.0126)
Treated X Mid (age 6-11)				-0.0171	-0.0157	-0.0132
				(0.0114)	(0.0106)	(0.0106)
Treated X Late (age 12-16)				-0.0062	-0.0033	0.0001
				(0.0107)	(0.0101)	(0.0101)
N	223,148	223,148	222,504	223,148	223,148	222,504
	Panel B. 9th-grade Examination GPA					
Treated	-0.0070	-0.0005	-0.0006	-0.0070	-0.0005	-0.0006
	(0.0069)	(0.0063)	(0.0063)	(0.0069)	(0.0063)	(0.0063)
Treated X Exposed	-0.0100	-0.0101	-0.0089			
	(0.0079)	(0.0073)	(0.0073)			
Treated X Early (age 0-5)				0.0002	-0.0044	-0.0047
				(0.0113)	(0.0104)	(0.0103)
Treated X Mid (age 6-11)				-0.0130	-0.0128	-0.0115
				(0.0096)	(0.0088)	(0.0088)
Treated X Late (age 12-16)				-0.0123	-0.0104	-0.0086
				(0.0090)	(0.0083)	(0.0083)
N	225,305	225,305	224,653	225,305	225,305	224,653
Parental age and cohort dummies	X	X	X	X	X	X
Parent and child controls		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 for parental property victimization. Estimated specifications are similar to Table 3. Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: 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
Parental age and cohort dummies	X	X	X	X	X	X
Parent and child controls	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 scores. Estimated specifications are similar to column (6) of Table 3. 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). Standard errors are clustered on families and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1