WHEN IT HURTS THE MOST: TIMING OF PARENTAL JOB LOSS AND A CHILD'S EDUCATION

Paul Bingley
VIVE Copenhagen, Denmark

Lorenzo Cappellari
Università Cattolica del Sacro Cuore, Italy

Marco Ovidi
Università Cattolica del Sacro Cuore, Italy

Abstract
We estimate how the timing of parental job loss shapes children's human capital by exploiting plant closures in Danish administrative data. We match treated children to observably similar untreated peers and difference out age-invariant selection using children whose parents are displaced after school completion. Job loss during infancy (ages 0–1) reduces ninth-grade mathematics achievement by 0.04 standard deviations and raises non-completion by 1 percentage point. Effects are smaller in later childhood. Treated infants are 1.4 percentage points less likely to enrol in general upper-secondary programmes. We find that liquidity-constrained households drive these results, suggesting that transient income shocks create permanent educational scars. (JEL: J13, D10, I24)

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E-mail: pab@vive.dk (Bingley); lorenzo.cappellari@unicatt.it (Cappellari); marco.ovidi@unicatt.it (Ovidi)

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1. Introduction

The productivity of parental investments and the home environment may vary at different stages of childhood. There is a general agreement on the significance of early years' circumstances for later childhood development and adult outcomes (Almond and Currie 2011). However, the boundaries of a crucial developmental period in early childhood are not well defined (Almond, Currie, and Duque 2018). Understanding which stages of childhood have the largest impact on development can help identify windows of opportunity for targeted policy interventions (Attanasio, Cattan, and Meghir 2022).

The timing of parental inputs may matter over and above the total resources invested. Caucutt and Lochner (2020) show that in the presence of dynamic complementarities or market failures such as borrowing constraints, a child's human capital response to parental investment depends on the stage of childhood at which the investment occurs. Carneiro et al. (2021) document that the timing of parental income during childhood is correlated with a child's long-run outcomes conditional on permanent family income. However, the differential causal impacts of parental inputs across childhood stages are largely unexplored.

We exploit parental job loss to provide novel causal evidence on the impacts of family shocks and their timing on children's education. Using Danish administrative data, we track children aged between 0 and 22 when parents experienced a plant closure, and focus on educational outcomes at the end of compulsory schooling (age 16). We compare the outcomes of treated children with those of a control group of observationally similar untreated peers.[1] Isolating the causal effect is challenging because displaced workers may possess unobserved characteristics that correlate with adverse child outcomes (Hilger 2016). We overcome this challenge by leveraging the observation of children treated *after* completing compulsory schooling (ages 17–22), exploiting the insight that job loss occurring after school completion cannot affect end-of-school performance.

Specifically, we identify stage-specific impacts through a difference-in-differences design centered on the end of compulsory schooling. We define four developmental windows: infancy (ages 0–1), early childhood (ages 2–5), mid-childhood (ages 6–11), and late childhood (adolescence, ages 12–16). Following the job loss literature (e.g., Bertheau et al. 2022), we first match each treated child to an observably similar untreated peer of the same age and gender. In contrast with that literature, end-of-school outcomes are observed once per child, precluding event study designs. We then compare the treatment-control difference for children exposed in each childhood stage against the same difference observed for children beyond school age. This design yields the causal effect of parental job loss under the assumption that the selection bias is

1. Throughout the analysis, 'children' denotes offspring between the ages of 0 and 22. Our control group uses untreated children in different families rather than older siblings. This choice allows us to compare children of the same age and avoids the severe sample reduction required to focus on multi-child families.

constant across children's ages at the time of the shock. Null estimates for children treated after school-leaving age support this assumption.

Our main finding is that parental job loss imposes a significant human capital penalty if experienced during infancy. In our preferred specification, ninth-grade mathematics achievement falls by 0.04 standard deviations (hereafter, σ) for children treated before age 2. We find smaller, statistically insignificant penalties of approximately 0.02σ for exposure during early childhood or adolescence, and null effects for mid-childhood. Treated infants are also 1 percentage point more likely to have missing grades or test scores in the ninth grade, a margin that accounts for 13% of the non-completion rate.

These penalties translate into tangible 'roadblocks' that persist into adolescence. Children exposed in infancy are 1 percentage point more likely to experience grade retention, record multiple spells in the same grade, or fail to complete an academic year. By age 16, they are 0.5 percentage points more likely to have exited the education system entirely and 2 percentage points more likely to remain delayed in elementary education. Most consequentially, they are 1.4 percentage points less likely to enrol in general upper-secondary tracks.

We explore mechanisms by estimating the causal effects of job loss on parental outcomes. Exploiting panel data, we estimate event studies around plant closures, akin to the standard approach in the literature on job loss.[2] Earnings drop more deeply and persistently among parents of adolescents, in line with evidence of larger penalties for older workers (Salvanes, Willage, and Willén 2024). However, milder income shocks are sufficient to impede wealth accumulation among the youngest families, with treated parents of infants holding substantially fewer assets 10 years after job loss. Although younger families may be better positioned to respond to job loss by adjusting their choices, we find little impact on mobility or additional fertility. Beyond these average patterns, we find that job loss impacts on families are remarkably dispersed, in line with recent research (Athey et al. 2024; Gulyas and Pytka 2025). We next correlate these heterogeneous impacts with reductions in children's achievement.

Among younger families, greater income losses lead to larger reductions in children's achievement. Results for children treated in the earliest stages are strongly associated with changes in family income, rather than with impacts on parental unemployment, cohabitation, or sickness benefits. Consistently, achievement declines are detected only when the displaced parent is the primary earner. These results align with research showing positive impacts of increased parental income (Aizer et al. 2016; Dahl and Lochner 2012; Hoynes, Schanzenbach, and Almond 2016).

2. A sizeable literature shows that job loss leads to persistent earnings losses and unemployment (e.g., Lachowska, Mas, and Woodbury 2020; Schmieder, von Wachter, and Heining 2023). Beyond labour market outcomes, job loss reduces workers' health (Browning and Heinesen 2012; Sullivan and von Wachter 2009) and fertility (Del Bono, Winter-Ebmer, and Weber 2012; Huttunen and Kellokumpu 2016), impacts mobility (Huttunen, Moen, and Salvanes 2018), and increases crime (Britto, Pinotti, and Sampaio 2022a).

We suggest that younger families may be more sensitive to income shocks since they typically possess fewer assets. Recent evidence from Denmark shows that asset decumulation is the main compensatory response to income shocks induced by job loss (Andersen et al. 2023). We show that achievement reductions are concentrated among families with fewer baseline assets and larger income declines. We acknowledge that income shocks may interact with unobservable mechanisms, such as declines in parental or child mental health (Carneiro et al. 2024; Fontes et al. 2024), which can act as both causes and consequences of the shock. However, the absence of larger achievement reductions in municipalities and cohorts with lower childcare coverage does not support the view that prolonged time spent with distressed parents drives our results.

In contrast, achievement reductions spurred by parental job loss during adolescence are weakly associated with income shocks. We find significant achievement penalties during adolescence for maternal job losses and among males (both by 0.03σ). Mothers of adolescents are found to suffer increased stress after job loss (Carneiro et al. 2024), and males are more prone than females to exam-related stress (Heissel et al. 2021). Although we cannot offer direct evidence, our results on adolescents are thus consistent with a mediating role of psychological distress.

Our findings have direct implications for the design of the social safety net. Back-of-the-envelope calculations using estimates from the literature imply that the average achievement penalty from parental job loss during infancy corresponds to the impact of a \$1,300 reduction in per-pupil spending sustained over 4 years or a 0.3σ decrease in teacher value-added. These effects are moderate in magnitude, smaller than impacts reported for early-childhood-development programmes, for example, 0.10σ test score gains for Head Start (Deming 2009), yet economically meaningful given the prevalence of job loss events. While unemployment insurance systems typically replace a fixed fraction of earnings regardless of family structure, our results suggest that the social cost of a layoff varies substantially with the age of the worker's children. Policy interventions that target resource stability or enhanced replacement rates to parents of infants could yield high returns by preventing transient liquidity shocks from becoming permanent human capital scars.

Our study contributes to the nascent literature identifying the childhood stages at which family shocks are most consequential. While recent studies focus on critical junctures such as school track choice or labour market entry (Fradkin, Panier, and Tojerow 2019; Mari, Keizer, and Gaalen 2024; Schmidpeter 2020), there is little evidence on differential impacts by the age at which a child is exposed. Concurrently developed with our study, Carneiro et al. (2024) find the largest negative effects of parental job loss on children exposed in adolescence. While no other study has considered ages 0–1 in isolation, we show that infancy represents a crucial stage. Our results suggest that family income is more productive at earlier stages, in line with Cunha and Heckman (2007, 2008).

Several studies investigate the intergenerational impacts of job displacement on children's education and earnings in different contexts, with mixed findings (see Ruiz-Valenzuela 2021, for a review). Using paternal layoffs in the US, Hilger (2016) finds minor impacts on college enrolment and early career outcomes. Mork, Sjoegren, and Svaleryd (2020) similarly find minor impacts on children's health, education, and labour market outcomes in Sweden. On the other hand, several papers show significant adverse effects.[3] By identifying substantial differences in the impact of parental job loss by childhood stage, our study may help reconcile the mixed findings in the literature.

2. Institutional Setting

Denmark's labour market combines high employer flexibility in hiring and firing with generous income protection through unemployment benefits, resulting in relatively low income inequality. Until 1994, this 'flexicurity' model contributed to high unemployment, reflecting the effectively unlimited duration of benefit receipt (Kreiner and Svarer 2022). Subsequent reforms introduced mandatory participation in active labour market programmes to maintain eligibility, thereby limiting maximum benefit duration. These changes reduced income security while preserving employer flexibility and contributed to shorter unemployment spells.

Parental leave in Denmark allows parents to temporarily exit the labour force around childbirth. Between 1985 and 1993, mothers were entitled to 14 weeks of leave, fathers to 2 weeks, and an additional 10 weeks could be shared between parents. The compensation rate was tied to the unemployment benefit level, which depends on prior earnings up to a cap. Under collective agreements, many employers topped up benefits to match pre-birth earnings. From 1994 to 2001, both parents were entitled to an additional 52 weeks of leave per child aged 0–8. Initially set at 80% of the unemployment benefit level, the compensation rate was reduced to 70% in 1995, and 60% in 1997. From 2002 to 2022, mothers were entitled to 4 weeks of leave before birth and 14 weeks after, fathers to 2 weeks, and 32 weeks could be shared between parents, with an average replacement rate of 53% (Houmark et al. 2024). For our analysis, this institutional setting implies that job loss likely lowers the effective replacement rate of income for parents on leave, much as unemployment benefits reduce earnings for workers who are not on leave.

Universal access to publicly subsidised daycare is available for all children below school age, irrespective of parental employment status. Fees are means-tested and decline when family income falls, with low-income families eligible for substantial reductions or full exemptions. Municipalities are responsible for organizing and financing daycare provision, typically via a mix of center-based care and regulated family daycare. Beginning in the early 1990s, municipalities must offer a full-time

3. See Oreopoulos, Page, and Stevens (2008); Rege, Telle, and Votruba (2011); Coelli (2011); Pan and Ost (2014); Ruiz-Valenzuela (2020); Andersen et al. (2022); Britto, Melo, and Sampaio (2022b); Fontes et al. (2024).

daycare place within a short waiting period, mostly from six months of age until school entry (Datta Gupta and Simonsen 2010). By the 2000s, the vast majority of 3–5-year-olds, and a large share of 1–2-year-olds, were enroled in publicly subsidised daycare.

Compulsory schooling in Denmark comprises ten grades. One kindergarten grade, made mandatory in 2009, was added to grades 1–9, although kindergarten attendance had already been nearly universal for many years. Compulsory schooling typically begins in August of the calendar year in which a child turns 6 and ends in June of the year in which the student turns 16. School-leaving exams are sat in English, Danish, mathematics, and science, along with a set of randomly selected additional subjects. These exams are generally low-stakes, as teachers' recommendations largely determine students' subsequent educational tracks. For this reason, our main analysis focuses on teacher assessments, which are highly correlated with exam scores (0.86 in our sample).

Compulsory school graduates 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 programmes typically last 3 years and include academically oriented gymnasiums and business or technical colleges. Vocational programmes, in contrast, combine classroom instruction with on-the-job training and usually span 2–5 years.

3. Data

We use longitudinal administrative register data covering the entire Danish population. Since 1968, every resident has been assigned a unique personal identification number, which permits precise linkage across all national registers. Using these identifiers, we link children to their parents and parents to their employers and tax records.

Educational institutions report detailed schooling information to the Ministry of Education. Our primary outcomes are achievement measures in grade 9 (age 16), marking the end of compulsory schooling. We analyse two complementary margins of achievement. First, we examine the completion of the ninth-grade assessment (labeled 'test-taking'), a binary variable indicating at least one recorded mark from either continuous teacher assessment or a formal examination. Absence of any recorded mark typically indicates grade retention, dropout, or transfer to non-standard educational tracks where conventional grading is not applied. Second, conditional on having a recorded mark, we analyse standardised teacher assessments. Because educational records span 2002–2018, the analysis focuses on children born between 1986 and 2002. Grades are standardised to have a mean of 0 and unit variance within the corresponding subject-year distribution.

We use matched employer–employee registry data covering the universe of workers and job spells from 1980 to 2017 to identify job loss events. These records are linked to earnings, unemployment, and income registers. Earnings and post-tax income are expressed in 2020 Danish kroner (DKK, with 7.46 DKK per euro and 6.54 DKK per US dollar). Unemployment data measure the fraction of the calendar year that insured workers spend unemployed; for brevity, we refer to this variable as (annual) unemployment duration. In addition, we observe tax-assessed asset values, sickness benefits, area of residence, and marital and cohabitation status.

3.1. Plant Closures

We begin by identifying plant closures and major downsizings following Browning and Heinesen (2012). We track each plant's owner (firm), industry, municipality, and workforce by linking plant records to individual job spells and examining year-to-year changes in these characteristics between end-of-November census dates. A plant is classified as closed if several of these characteristics change from 1 year to the next (see Online Appendix A for details). Crucially, if, despite other changes, a large share of employees is re-employed at a single newly established plant, we do not treat this as a closure. The analysis is restricted to private-sector plants, as it is difficult to identify distinct workplaces in the public sector.

Next, we define the closure year based on the magnitude of the employment contraction. We require a minimum reduction of three employees and at least 30% of the workforce, and we restrict attention to plants with at least five employees 5 years prior to closure. Among the last 3 years of a plant's operation, the closure year is defined as the year with the largest employment decline (see Online Appendix A for details). This procedure identifies 51,002 closures between 1986 and 2017, or roughly 1,500 per year. Job flows around these events indicate that they are highly disruptive for workers: in the year after closure, 96% of employees leave the plant, compared with 30% at non-closing plants. Moreover, 83% of workers at closing plants separate from their firm, and 30% have no registered job spells in the subsequent year, compared with 30% and 15% among workers at non-closing plants.

Plant closures are arguably difficult to anticipate. In 83% of cases, closures leave the plant with no employees, with an average downsizing of 18 workers (median 9) in the closure year. In contrast, the median closing plant has exactly the same headcount in the year before closure as in the preceding year. Thus, closures generally do not follow a gradual, predictable downsizing trajectory. In Online Appendix Table B.1, we show that our results are robust to restricting the sample to 'pure' plant closures, defined as closures that leave the plant with no employees.

3.2. Sample Selection and Childhood Stages

We consider employees with a stable work history, irrespective of whether they experience a plant closure. We select workers aged 25–60 with at least 3 years of tenure at their firm, regardless of gender. Let t^* denote the 'base' year prior to plant closure for workers exposed to exactly one closure. The selection applies in t^* for exposed workers and in any year for unexposed workers.

Our treatment group consists of the children of workers experiencing plant closure. To define the relevant childhood stage at parental job loss, we consider children treated by at most one parental closure, that is, those whose mother or father, but not both, experiences a plant closure. Let $a(i)$ denote child i's age in the plant closure year $(t^* + 1)$. We partition childhood into four stages: infancy $(0 \leq a(i) \leq 1)$, early childhood $(2 \leq a(i) \leq 5)$, mid-childhood $(6 \leq a(i) \leq 11)$, and late childhood (adolescence, $12 \leq a(i) \leq 16$). This classification closely follows Carneiro et al. (2021).[4] In addition, children treated by parental job loss after school leaving age $(17 \leq a(i) \leq 22)$ serve to further control for selection into treatment. Across ages 0–22, the working sample includes 133,805 treated children. Using the matching procedure described in Section 4, we select a control group from the children of unexposed workers.

Treated children are negatively selected relative to the pool of potential controls, and this selection becomes more pronounced if parental job loss occurs at later childhood stages. Online Appendix Tables D.1 and D.2 compare observed characteristics for the full working sample and by childhood stage, respectively. On average, treated children come from families with annual incomes that are 42,000 DKK lower, and their displaced parent has 0.17 fewer years of schooling. Among adolescents, the corresponding gaps are 49,000 DKK and 0.26 years of schooling, with smaller differences in early childhood. Our research design, described in the next section, is explicitly constructed to address this selection.

4. Empirical Strategy

We examine the effect of parental job loss on end-of-school educational outcomes as a function of the child's age at the time of the shock. A simple comparison between treated and untreated children may yield biased estimates if the former have lower potential achievement. Although plant closures are often treated as quasi-exogenous events because no selection occurs within establishments, Hilger (2016) provides evidence of negative selection into closing plants based on unobservable characteristics, consistent with baseline differences between treated and untreated children (see Online Appendix Table D.1). We address selection bias in two steps. First, following the literature on job loss (e.g., Bertheau et al. 2022), we match each treated child aged between 0 and 22 in $t^* + 1$ to an untreated counterpart of the same age with similar observable characteristics. Second, we use the outcomes of children aged 17–22 at the time of parental job loss and of their matched counterparts to adjust for potential selection on unobservables not captured by the matching procedure.

We begin by matching treated to untreated children based on observable characteristics. This procedure defines placebo plant closure events for the same child

4. Their study distinguishes three stages: early (ages 0–5), middle (ages 6–11), and late (ages 12–17). We end childhood at age 16 to align with the timing of our outcome and split ages 0–5 to separately identify infancy effects.

TABLE 1. Descriptive statistics on treated and control children.

	Treated children		Control children			
	Mean (1)	SD (2)	Mean (3)	SD (4)	Diff. (5)	p (6)
Male	0.514	0.500	0.514	0.500	0.000	1.000
Parent's plant size	144.717	320.602	144.549	382.537	0.168	0.903
Parent's tenure	7.648	4.854	7.643	4.949	0.005	0.774
Parent's earnings (2020 DKK, 000's')	429.440	251.231	428.974	272.183	0.466	0.649
Parent's post-tax income (2020 DKK, 000's)	474.802	533.197	473.850	513.770	0.952	0.642
Family post-tax income (2020 DKK, 000's)	804.468	379.241	801.188	397.529	3.280	0.031
Father	0.658	0.474	0.658	0.474	0.000	1.000
Parent's age at birth	30.707	4.953	30.702	5.009	0.005	0.811
Parent's age	39.630	7.288	39.627	7.328	0.004	0.894
Parent's years of education	12.780	2.210	12.885	2.230	−0.105	0.000
Parent received UI	0.113	0.317	0.082	0.275	0.031	0.000
Unemployment duration (1,000 = full year)	16.161	71.429	10.302	57.339	5.859	0.000
Parent in services	0.357	0.479	0.321	0.467	0.036	0.000
Parent in industry	0.283	0.450	0.308	0.461	−0.025	0.000
Parent in commerce	0.252	0.434	0.273	0.446	−0.021	0.000
	130,927		130,927			

Notes: The table shows descriptive statistics for treated and control children. The sample includes matched treatment-control pairs resulting from the procedure described in Section 4. Parental and child characteristics are observed in the base year (the year before real or placebo job loss). Family post-tax income is defined as the sum of parents' incomes. Statistics in this table average across childhood stages, including children aged beyond school completion at real or placebo parental job loss (ages 0–22). Columns (1) and (3) show average values for treated and control children, respectively. Columns (2) and (4) show the corresponding standard deviations. Column (5) computes the difference between columns (1) and (3). Column (6) reports the p-value of the associated t-statistics. See Section 4 for details.

age and calendar year as the real closures (Jaravel, Petkova, and Bell 2018). Specifically, we partition the sample into cells defined by birth year, child gender, and displaced parent's gender. Within each base year and cell, we perform 1:1 nearest-neighbour matching without replacement, minimising differences in parental age at childbirth, three-digit industry, plant size, tenure, earnings in $t^* - 1$ and $t^* - 2$, child's number of siblings and birth order. We successfully matched 98% of treated children, resulting in a final sample of 130,927 treatment-control pairs.

As shown in Table 1, matched treated and control children exhibit nearly identical characteristics, a striking contrast to the pronounced differences observed in Online Appendix Table D.1. Beyond exact matching on child and parent gender and cohort, parental plant size, tenure, earnings, and family income in t^* are statistically and substantively indistinguishable across groups. Online Appendix Table D.3 further shows that observable characteristics remain balanced across all stages of childhood. In Section 5, we confirm that our findings are robust to controlling for all matching variables, including those with minor residual imbalances (e.g., unemployment benefit receipt and industry).

Although treated and control children are matched on observable characteristics, the treatment group may still differ in unobserved achievement potential. Because end-of-school outcomes are measured once per child, we cannot employ event-study designs to account for such selection. Instead, we approximate selection on unobservables using treatment-control differences among children who had already completed compulsory schooling at the time of real or placebo parental job loss (ages 17–22). Our main estimating equation is:

$$Y_i = \beta_0 + \beta_1 T_i + \sum_{s \in \{I,E,M,L\}} \beta_s T_i \cdot \mathbb{1}[a(i) \in s] + \varphi_{a(i)} + X_i'\pi + u_i, \tag{1}$$

where T_i is an indicator equal to 1 if child i is treated, and s indexes the stage of childhood (with I, E, M, and L denoting infancy and early, middle, and late childhood, respectively). We control for indicators for the child's age at the time of the real or placebo closure ($\varphi_{a(i)}$) and progressively richer sets of individual characteristics (X_i'). The coefficient β_1 captures constant treated-control differences across ages at exposure that are netted out when estimating effects by childhood stage. The coefficients β_s capture the difference-in-differences estimates of the effect of parental plant closure on an end-of-school outcome Y_i during childhood stage s.[5] Comparing β_s estimates across childhood stages allows us to assess how the impact of parental job loss on educational outcomes varies with the timing of exposure.

We rely on the assumption of age-invariant selection into treatment. Our estimates represent the causal effect of parental job loss if, in the absence of treatment, any systematic differences in end-of-school outcomes with the control group would have remained constant across children treated at different ages. We indirectly test this assumption through a nonparametric specification that estimates age-at-closure-specific treatment effects:

$$Y_i = \gamma_0 + \gamma_1 T_i + \sum_{k=0}^{22} \lambda_k T_i \cdot \mathbb{1}[a(i) = k] + \varphi_{a(i)} + X_i'\nu + \varepsilon_i, \tag{2}$$

where coefficients λ_k are normalised relative to $a(i) = 17$, the earliest age at which parental plant closure cannot affect end-of-school outcomes. Under our identifying assumption, the estimates for $\lambda_{18}, \ldots, \lambda_{22}$ should be statistically indistinguishable from zero. We report supportive evidence for this restriction in Section 5. In addition, we find no systematic trends in mathematics achievement among control children by age at placebo parental job loss, conditional on year and birth cohort (see Online Appendix Figure D.1).

5. Abstracting from additional controls, $\beta_s = E[Y_i|T = 1, a(i) \in s] - E[Y_i|T = 0, a(i) \in s] - (E[Y_i|T = 1, a(i) > 16] - E[Y_i|T = 0, a(i) > 16])$. Our identification strategy addresses recent concerns about the validity of staggered designs (e.g., De Chaisemartin and d'Haultfoeuille 2020; Borusyak, Jaravel, and Spiess 2024; Goodman-Bacon 2021). Each treatment–control pair forms an independent 2×2 difference-in-differences comparison, and control units are never treated.

Our research design differs in an important way from that of Carneiro et al. (2024). Their analysis relies on the conditional random assignment of mass layoffs and establishment closures among workers with stable employment histories, holding constant rich information about families, locations, and jobs. In a similar spirit, we match treated to untreated children on an extensive set of observable characteristics.[6] In contrast to their approach, we additionally exploit the observation of children treated after school completion to construct difference-in-differences comparisons. Within our framework, the contribution of this additional step can be evaluated by examining estimates of β_1 in equation (1). This coefficient captures age-invariant selection into treatment that would be missed in simple treatment-control comparisons. As shown in Section 5, treatment-control comparisons yield results broadly similar to those from our matched difference-in-differences design, but small yet meaningful discrepancies support the value of our approach.

Our sample is restricted to children born by the calendar year of parental job loss, which limits potential confounding from selection into parenthood. Identification concerns may nonetheless arise if plant closures are anticipated, allowing for selective fertility responses. In addition, because the exact timing of closure within the plant's final year is not observed, a small share of children born in the job-loss year (i.e., with $a(i) = 0$) may have been conceived after the closure. Robustness checks reported in Online Appendix B alleviate these concerns by showing that our results are not driven by closures that are ex ante more likely to be anticipated (e.g., characterised by gradual downsizes) or by children born in the closure year.

Existing evidence further suggests that fertility responses to job loss are unlikely to account for our findings. Huttunen and Kellokumpu (2016) document no fertility effects of job loss for men and negative effects for women, implying that fertility adjustments cannot explain the estimated impacts of paternal job loss. De Cao, McCormick, and Nicodemo (2022) show that higher unemployment raises fertility among women with high socioeconomic status while reducing it among more disadvantaged women. Such a pattern would lead to a positive selection of treated families, implying that our estimated achievement penalties are, if anything, lower bounds of the true effects.

5. Results

We first consider extensive margin responses. Panel A of Table 2 presents estimates from equation (1) showing the impact on 'test-taking', which we interpret as a proxy for grade completion. This variable takes the value of 1 if the student has any recorded mark (exam or teacher assessment) and 0 otherwise, indicating severe disengagement or dropout. We find that children exposed to parental job loss during

6. Given the strong imbalances in observables between treated units and potential controls in our setting (see Online Appendix Table D.1), constructing the control group through matching mitigates the extrapolation concerns that would arise using only regression adjustment.

TABLE 2. Impacts of parental plant closure.

	(1)	(2)	(3)
Panel A. Test taking			
Treated	0.0033	0.0037	0.0034
	(0.0026)	(0.0025)	(0.0026)
Treated X Infant (0-1 years)	−0.0089**	−0.0094**	−0.0097**
	(0.0043)	(0.0043)	(0.0043)
Treated X Early childhood (2-5 years)	−0.0055	−0.0058*	−0.0057
	(0.0035)	(0.0035)	(0.0035)
Treated X Mid-childhood (6-11 years)	−0.0046	−0.0046	−0.0045
	(0.0032)	(0.0032)	(0.0032)
Treated X Late childhood (12-16 years)	−0.0036	−0.0032	−0.0031
	(0.0033)	(0.0033)	(0.0033)
N	261,854	261,854	261,854
Panel B. Teacher assessment in maths			
Treated	−0.0136	0.0019	0.0031
	(0.0098)	(0.0092)	(0.0093)
Treated X Infant (0-1 years)	−0.0264	−0.0353**	−0.0407***
	(0.0163)	(0.0155)	(0.0155)
Treated X Early childhood (2–5 years)	−0.0061	−0.0190	−0.0216*
	(0.0133)	(0.0125)	(0.0126)
Treated X Mid-childhood (6–11 years)	0.0004	−0.0063	−0.0072
	(0.0122)	(0.0114)	(0.0114)
Treated X Late childhood (12-16 years)	−0.0150	−0.0157	−0.0143
	(0.0126)	(0.0118)	(0.0118)
N	239,598	239,598	239,598
Age at closure dummies	X	X	X
Parental and child characteristics		X	X
Industry and county dummies			X

Notes: The table shows estimates of the impact of parental job loss on children's test-taking (Panel A) and achievement in mathematics (Panel B) at the end of compulsory school (grade 9). The sample includes the 130,927 treatment-control pairs identified as detailed in Section 4. Reported are estimates of β_1 (binary variable 'Treated') and β_s (the interactions with childhood stage at parental job loss) in equation (1). Outcomes are defined as in the note to Figure 1. Column (1) controls for age at closure, year of closure, and child year of birth dummies. Column (2) adds: displaced parents' earnings, unemployment, and post-tax income, dummies for gender, age, and age at birth decile, teen parenthood, tenure quintile, and plant size decile; dummies for child's gender, number of siblings quintile, and birth-order quartile. All characteristics are measured in the base year. Column (3) adds industry (3-digit) and county dummies. The number of observations is computed before the exclusion of singleton observations (three units in column 3). Standard errors are clustered at the family level. See Section 5 for details. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

infancy are 1 percentage point less likely to complete these assessments. Given a baseline participation rate of 92%, this decline explains approximately 13% of the observed non-completion. In contrast, estimates for exposure during early childhood (0.6 percentage points), mid-childhood (0.5 percentage points), and adolescence (0.3 percentage points) are smaller in magnitude and not statistically distinguishable from zero. These results remain robust across specifications with varying controls, including models saturated with industry and county fixed effects (columns 1–3). Notably, the estimated coefficient on the time-invariant treatment group indicator (β_1), although marginally significant, is large relative to the treatment effect, underscoring the importance of our strategy for accounting for underlying selection bias.

Conditional on taking the test, parental job loss significantly reduces mathematics achievement, with the most severe penalties observed for shocks occurring during infancy. Panel B of Table 2 presents estimates for teacher assessments in mathematics. In our preferred specification with full controls (column 3), job loss during infancy reduces mathematics achievement by 0.04σ. We estimate a smaller reduction of 0.02σ for exposure in early childhood and a comparable coefficient for adolescence, though the latter is statistically imprecise ($p = 0.22$). Effects for mid-childhood exposure are negligible. If the attrition induced by the treatment (see Panel A) comes from the lower end of the ability distribution, these estimates represent a lower bound of the true negative effects. This interpretation is consistent with the decreasing coefficient magnitudes when we add controls for parental and child characteristics (columns 1–3), as these covariates partially account for the negative selection into non-participation.

Nonparametric estimates reveal no significant effects of exposure after age 16, supporting our key identifying assumption. Figure 1 displays the coefficients λ_k from equation (2), estimated with the full set of controls (corresponding to column 3 of Table 2). For both test-taking (Panel A) and mathematics assessments (Panel B), treatment effects are statistically indistinguishable from zero for job losses occurring after compulsory school age. Consistent with this visual evidence, F-tests for the joint significance of the post-compulsory coefficients ($\lambda_{18}, \ldots, \lambda_{22}$) fail to reject the null hypothesis ($p = 0.49$ for test-taking and $p = 0.92$ for mathematics).

Our research design nests a conditional-independence strategy, which compares treated and untreated children solely on observables. In Online Appendix Table D.4, we leverage this feature to demonstrate the substantive importance of accounting for selection on unobservables, as our preferred difference-in-differences model does. Specifically, we contrast our baseline estimates (reproduced in columns 1–2) with results from a specification that compares matched treated and control children within each childhood stage, but excludes the older reference group (ages 17–22). While the findings are broadly consistent across designs, two notable differences emerge. First, estimates for adolescent exposure (columns 9–10) in the alternative design are more sensitive to the inclusion of control variables, likely reflecting stronger selection into parental job loss at older ages (as discussed in Section 3). Second, the magnitude of the effect on test-taking is notably smaller in the alternative specification, suggesting that our preferred model successfully corrects for substantial age-invariant selection bias that would otherwise attenuate the results. The alternative specification adapts the identification strategy employed by Carneiro et al. (2024) to our setting, that is, relying on propensity score matching without the additional adjustment for age-invariant unobservables. Carneiro et al. (2024) report particularly strong adverse effects for exposure during adolescence, especially for maternal displacement, and emphasise maternal and child mental-health mechanisms. While our robustness checks show that age patterns are broadly similar across designs, some differences in headline results may still reflect sample composition, differences in outcomes measured or

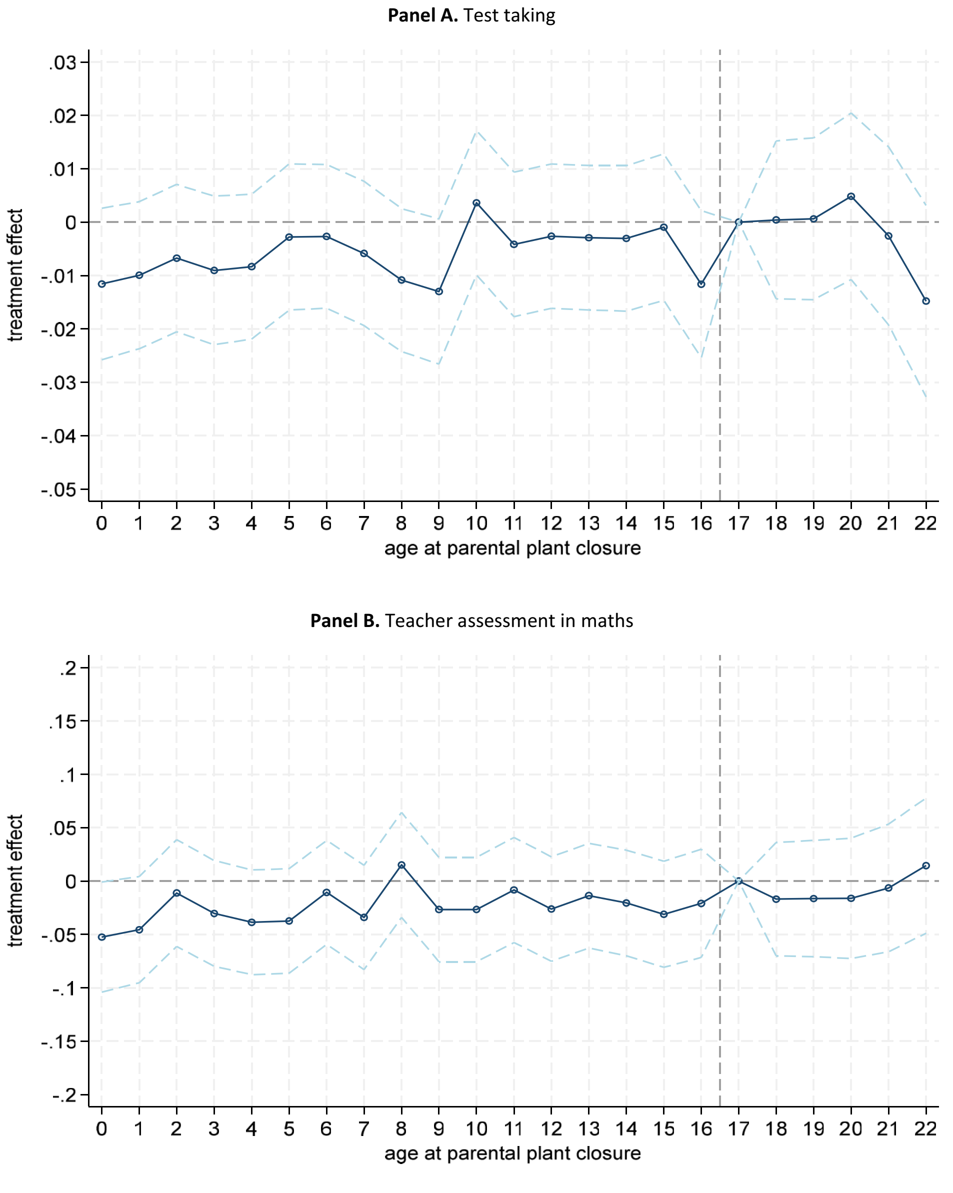

FIGURE 1. Parental plant closure impacts by age at closure. The figure shows estimates of the impact of parental plant closure on test-taking (Panel A) or a child's grade 9 achievement in mathematics (Panel B) by age in the year of closure. Plotted are estimates of coefficients λ_k from equation (2). Boundaries of the 95% confidence intervals are plotted as dashed lines. Standard errors are clustered at the family level. In Panel A, the outcome is a binary variable equal to 1 if the child does not obtain teacher assessments or test scores in grade 9 examinations. In Panel B, the dependent variable is teacher assessment in mathematics, standardised to have a mean of zero and unit variance within each year's student population. The estimated specifications are analogous to column (3) of Table 2. See Section 5 for details.

their timing (end of grade 9 vs. tenth-grade GPA and absenteeism), or the relative importance of psychosocial channels during adolescence.

Analysis of additional educational outcomes confirms that parental job loss is most detrimental when experienced during infancy (Table 3). Panel A reports estimates for further achievement measures. Consistent with the literature, we find smaller effects on achievement in language scores than in mathematics.[7] Although estimated effects are smaller for standardised tests, the results in Panels B and C suggest that the achievement shocks measured by teacher assessments translate into tangible disruptions in students' educational trajectories.

Panel B shows that by age 16, children treated during infancy are more likely to experience a fragmented educational history. For this group, a composite 'roadblock' index, capturing non-standard annual progression, grade retention, or failure to complete an academic year, increases by 1 percentage point (column 1). Although statistically imprecise, point estimates also suggest that parental job loss during infancy increases the probability of changing schools and of opting for the voluntary tenth grade, thereby delaying high school entry (columns 2–3). A similar increase in school mobility is also observed for children treated during adolescence.

Children experiencing parental job loss during infancy are significantly less likely to enrol in general upper secondary programmes by age 16. Panel C of Table 3 examines enrolment outcomes at the start of the academic year. We find that children treated during infancy are 0.5 percentage points more likely to have exited the education system entirely and 2 percentage points more likely to remain in elementary education (delayed progression) compared to the control group (columns 1–2).[8] This disruption primarily displaces students from higher tracks: enrolment in vocational programmes falls by 0.95 percentage points (column 3), and enrolment in technical or academic high schools declines even more sharply by 1.4 percentage points (column 4). Notably, we also find a smaller but statistically significant increase in the likelihood of dropout for parental job losses during adolescence (0.3 percentage points, column 1).

In summary, our analysis uncovers moderate yet economically meaningful achievement penalties for children exposed to parental job loss before age two; estimates that remain robust across specifications within our identification framework. We also detect smaller but economically meaningful adverse effects for exposure during the remainder of early childhood and adolescence, a pattern consistent with the timing of income shocks most strongly associated with long-run outcomes in the literature (Carneiro et al. 2021). The concentration of these penalties in the earliest stages of childhood aligns with theoretical models featuring credit constraints and dynamic complementarities in human capital investment (Caucutt and Lochner 2020). Using estimates from other studies in the literature

7. Performance in mathematics is more responsive to school interventions than language performance (Zheng 2022).

8. Dropout is defined as being not in education at age 16 or failing to complete the school grade started at that age. The estimated dropout increase equals 36% of the control group average (see Table D.5).

TABLE 3. Impacts of parental job loss–additional outcomes.

	(1)	(2)	(3)	(4)
	Panel A. Achievement			
	Teacher assessment in Danish	Std. Test Mathematics	Std. Test in Danish	
Treated	−0.0083	−0.0016	−0.0127	
	(0.0088)	(0.0094)	(0.0089)	
Treated X Infant (0–1 years)	−0.0231	−0.0259*	−0.0175	
	(0.0147)	(0.0156)	(0.0149)	
Treated X Early childhood (2-5 years)	−0.0028	−0.0171	−0.0049	
	(0.0119)	(0.0126)	(0.0121)	
Treated X Mid-childhood (6-11 years)	−0.0016	−0.0003	−0.0012	
	(0.0109)	(0.0115)	(0.0110)	
Treated X Late childhood (12-16 years)	0.0021	−0.0082	0.0115	
	(0.0111)	(0.0118)	(0.0113)	
N	240,006	238,787	240,142	
	Panel B. Education disruption			
	Roadblock	School move	Take grade 10	
Treated	−0.0046	−0.0070*	−0.0046	
	(0.0030)	(0.0041)	(0.0048)	
Treated X Infant (0-1 years)	0.0112**	0.0091	0.0121	
	(0.0053)	(0.0070)	(0.0081)	
Treated X Early childhood (2-5 years)	0.0029	0.0102*	−0.0000	
	(0.0043)	(0.0057)	(0.0065)	
Treated X Mid-childhood (6-11 years)	0.0041	0.0047	0.0003	
	(0.0039)	(0.0052)	(0.0059)	
Treated X Late childhood (12-16 years)	0.0056	0.0104**	0.0017	
	(0.0040)	(0.0053)	(0.0061)	
N	260,500	260,500	260,500	
	Panel C. Enrolment at age 16			
	Dropout	Compulsory school	Vocational programme	General programme
Treated	−0.0029**	−0.0066	0.0044	0.0047
	(0.0012)	(0.0047)	(0.0028)	(0.0042)
Treated X Infant (0-1 years)	0.0050**	0.0228***	−0.0095**	−0.0144**
	(0.0020)	(0.0079)	(0.0043)	(0.0072)
Treated X Early childhood (2–5 years)	0.0025	0.0049	−0.0046	−0.0014
	(0.0016)	(0.0064)	(0.0036)	(0.0058)
Treated X Mid-childhood (6–11 years)	0.0025*	0.0084	−0.0043	−0.0058
	(0.0015)	(0.0058)	(0.0033)	(0.0052)

Table 3. Continued

	(1)	(2)	(3)	(4)
Treated X Late childhood (12–16 years)	0.0034**	0.0042	−0.0068**	−0.0010
	(0.0015)	(0.0060)	(0.0035)	(0.0054)
N	250,348	261,854	261,854	261,854
Age at closure dummies	X	X	X	X
Parental and child characteristics	X	X	X	X
Industry and county dummies	X	X	X	X

Notes: The table shows estimates of the impact of parental job loss on additional educational outcomes. Estimated specifications are analogous to column (3) of Table 2. Panel A considers additional achievement measures. Standardised tests are scores from formal examinations. Teacher assessments are grades from continuous assessments. Panel B considers indicators of fragmented educational spells. Dependent variables are binary, equal to 1 if the outcome is realised at least once by the end of compulsory school (age 16). The 'roadblock' index equals 1 if a student either recorded multiple educational spells within the same academic year, grade retention, or failure to complete an academic year. Panel C considers children's enrolment status at age 16. Dropout is defined as being not in education at age 16 or failing to complete the school grade started at that age. The latter completion is not observed for the older cohort (year of birth 2002). See Section 5 for details. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

helps contextualize the economic significance of our results. Jackson and Mackevicius (2024) find that increasing spending by \$1,000 per pupil for four years improves test scores by 0.03σ. Applied to our framework, this implies that the estimated achievement penalty of 0.04σ for parental job loss during infancy is equivalent to a \$1,300 reduction in per-pupil spending for four years. Chetty, Friedman, and Rockoff (2014) estimate that a 1σ increase in teacher value-added raises achievement by 0.14σ, implying that our estimate corresponds to the impact of a 0.3σ reduction in teacher quality.

5.1. Heterogeneous Effects

The adverse impacts of parental job loss during infancy are disproportionately concentrated among low-income families. In Table 4, we investigate treatment effect heterogeneity by stratifying the sample according to the baseline characteristics listed in the column headers. Achievement reductions for parental job loss during infancy are similar across the gender of the displaced parent (Panel B, columns 1–2) and child gender (columns 3–4). However, the estimated penalty is 0.07σ for families with pre-treatment income below the median (column 5), double the average estimate in Table 2. We also find that the reduction in test-taking is more pronounced for daughters (Panel A of Table 4, column 3).

In contrast, the adverse impacts of parental job loss during adolescence are more pronounced for maternal plant closures and among sons (approximately 0.03σ; Panel B, columns 1 and 4). Given that teacher assessments critically determine upper-secondary school tracks and are performed during adolescence, psychological stress is a plausible mechanism. This interpretation is supported by evidence that males experience greater declines in performance than females due to exam-related stress (Heissel et al. 2021). Furthermore, Carneiro et al. (2024) find that mothers of

TABLE 4. Heterogeneous impacts of parental plant closure.

	(1) Mother	(2) Father	(3) Female	(4) Male	(5) Family income	(6)
					Low	High
Panel A. Test taking						
Treated	0.0064	0.0021	0.0035	0.0031	0.0031	0.0017
	(0.0041)	(0.0033)	(0.0033)	(0.0038)	(0.0050)	(0.0028)
Treated X Infant (0–1 years)	−0.0101	−0.0098*	−0.0147***	−0.0050	−0.0102	−0.0068
	(0.0070)	(0.0055)	(0.0055)	(0.0065)	(0.0066)	(0.0066)
Treated X Early childhood (2–5 years)	−0.0091	−0.0043	−0.0046	−0.0065	−0.0076	−0.0006
	(0.0058)	(0.0044)	(0.0046)	(0.0052)	(0.0060)	(0.0045)
Treated X Mid-childhood (6–11 years)	−0.0043	−0.0046	−0.0062	−0.0026	−0.0052	−0.0029
	(0.0051)	(0.0041)	(0.0041)	(0.0047)	(0.0058)	(0.0036)
Treated X Late childhood (12–16 years)	−0.0079	−0.0005	−0.0037	−0.0022	−0.0049	−0.0014
	(0.0052)	(0.0042)	(0.0042)	(0.0049)	(0.0063)	(0.0036)
N	89,510	172,344	127,336	134,518	129,009	132,845
Panel B. Achievement (mathematics)						
Treated	0.0122	−0.0002	−0.0032	0.0095	0.0193	−0.0124
	(0.0151)	(0.0119)	(0.0127)	(0.0132)	(0.0162)	(0.0114)
Treated X Infant (0–1 years)	−0.0384	−0.0431**	−0.0386*	−0.0401*	−0.0663***	−0.0054
	(0.0259)	(0.0194)	(0.0213)	(0.0222)	(0.0220)	(0.0252)
Treated X Early childhood (2–5 years)	−0.0187	−0.0214	0.0033	−0.0459***	−0.0314	−0.0192
	(0.0214)	(0.0156)	(0.0173)	(0.0178)	(0.0195)	(0.0175)
Treated X Mid-childhood (6–11 years)	−0.0265	0.0033	−0.0004	−0.0137	−0.0268	0.0077
	(0.0188)	(0.0144)	(0.0157)	(0.0162)	(0.0190)	(0.0144)
Treated X Late childhood (12–16 years)	−0.0357*	−0.0019	0.0053	−0.0337**	−0.0249	−0.0038
	(0.0191)	(0.0149)	(0.0163)	(0.0169)	(0.0202)	(0.0143)
N	82,528	157,070	118,633	120,965	114,986	124,612
Age at closure dummies	X	X	X	X	X	X
Parental and child characteristics	X	X	X	X	X	X
Industry and county dummies	X	X	X	X	X	X

Notes: The table shows estimates of the heterogeneous impacts of parental job loss on children's test-taking (Panel A) and achievement in mathematics (Panel B). Specifications and outcomes are analogous to column (3) of Table 2, with sample splits defined in column headers. Estimation is restricted to: children exposed to maternal or paternal job loss (columns 1–2); daughters or sons (columns 3–4); children with family income below or above the median (columns 5–6). Family income sums post-tax income across parents and is measured 2 years prior to the real or placebo job loss. See Section 5 for details. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

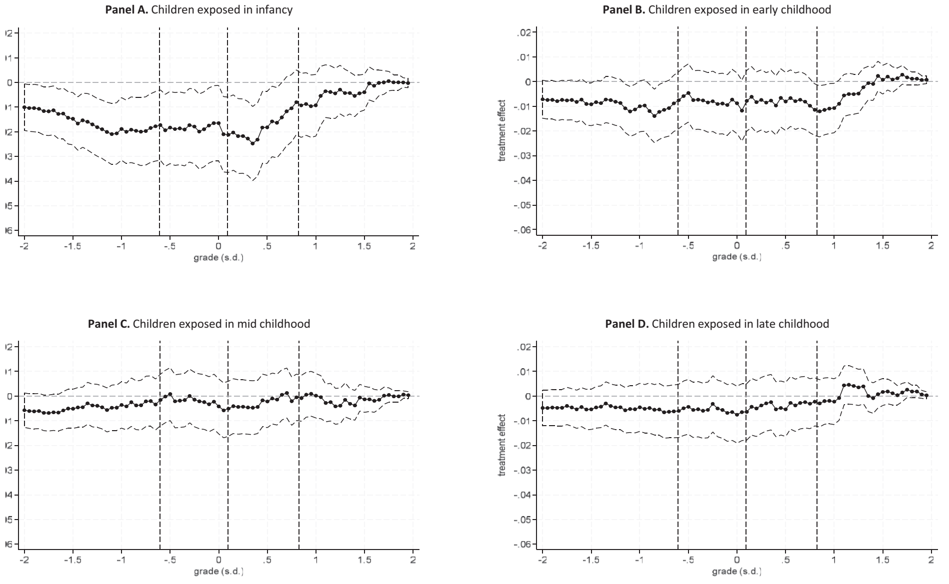

FIGURE 2. Distributional impacts of parental plant closure by age at closure. The figure shows estimates of the distributional impacts of parental plant closure on a child's grade 9 achievement by age in the year of closure. Plotted are estimates of coefficients δ_s from equation (1) from 80 different regressions. Outcomes are binary variables equal to 1 if the child's teacher-assessed mathematics 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 0 for children who do not receive teacher assessments. Panels A, B, C, and D plot treatment effects on children exposed to parental plant closure in infancy (age 0–1), and early (age 2–5), 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 (3) of Table 2. See Section 5 for details.

adolescents suffer particularly severe mental health shocks following job loss, which may compound the stress transmitted to the child.

By integrating test-taking and achievement scores, we next demonstrate that parental job loss during infancy disproportionately harms children at the lower end of the potential outcome distribution. Figure 2 plots the estimated treatment effects across the achievement distribution, with dashed vertical lines marking the quartile thresholds.[9] Negative impacts are substantially larger for children exposed during infancy (Panel A). For this group, the probability of scoring above the median falls by approximately 2 percentage points. Notably, the gap in adverse effects between infancy and later childhood widens in the second and third quartiles. In contrast, regardless of the timing of the shock, treatment effects converge to zero in the upper

9. 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.

achievement quartile, indicating that high-performing students are largely shielded from these negative impacts.

In summary, the adverse effects of parental job loss during infancy are persistent across demographic subgroups but are particularly acute for children from low-income families and those with below-median potential achievement. While characterising this heterogeneity can help policymakers target the most vulnerable populations, it leaves the underlying channels largely unexplained and offers limited insight into why certain children suffer more severe penalties. We address this gap in the following Section.

6. Mechanisms

We now turn to investigating the mechanisms driving the adverse effects of parental job loss on children's achievement. Overall, the evidence points most strongly to a household-resources channel: achievement losses are larger when predicted family income shocks are larger and when baseline buffers are weaker (e.g., lower assets). In contrast, we find limited support for mechanisms operating primarily through residential mobility, subsequent fertility, or childcare availability; this evidence is less informative about stress- or mental-health channels that affect the quality of parent–child interactions. We begin by documenting the dynamic evolution of parental labour market outcomes, family income, household stability, and sickness benefits following job loss. We then leverage heterogeneity in these parental responses to examine how they correlate with children's educational outcomes. Finally, we discuss the role of financial constraints in explaining why younger children appear especially vulnerable to income shocks.

6.1. Effects on Parents' Outcomes

We analyse the dynamic evolution of parental outcomes following job loss. Because parental responses to economic shocks vary systematically over the life cycle (Salvanes, Willage, and Willén 2024), these dynamics are a primary candidate for explaining the age-dependent effects we document. We characterise these responses by estimating a series of event studies around plant closures using the following specification:

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

where Y_{it} denotes a parental outcome for child i in year t. We normalise event time such that $l = 0$ corresponds to the year of plant closure ($t_i^* + 1$), and we estimate the coefficients α_l relative to the baseline year $l = -3$ ($t_i^* - 2$). 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. 2022). The coefficients α_l thus capture the dynamic causal impact of plant closure on parental trajectories relative to the control group. We estimate this model separately by childhood developmental stage and present the results in Figure 3.

Job displacement triggers an immediate and substantial decline in labour market outcomes. In the year following plant closure, the probability of the displaced parent recording positive earnings falls by approximately 3 percentage points on average, with slightly larger declines observed for parents of infants (Figure 3, Panel A). Concurrently, parental earnings drop by approximately DKK 25,000, with losses being notably more severe and persistent for parents of adolescents (Panel B). This gradient is consistent with Salvanes, Willage, and Willén (2024). Finally, displaced parents experience a sharp increase in unemployment duration, the magnitude of which is similar across all childhood developmental stages (Panel C).

Job displacement translates into sizable reductions in family income. On average, family income falls by about DKK 20,000 in the year after job loss (Panel D). Reflecting larger earnings penalties at older ages, short-term income losses are roughly twice as large for families with adolescent children as for those with children in infancy. However, cumulative income shocks appear damaging for younger households: 10 years after job loss, families of children exposed during infancy hold substantially fewer assets and are about 1 percentage point less likely to own their home (Panels A and B of Online Appendix Figure D.2).

Job loss also leads to a moderate decline in family stability and an increase in sickness benefit receipt. The probability of parental cohabitation falls by approximately 0.5 percentage points on average, an effect that is most pronounced for families with the youngest children (Figure 3, Panel E). This dissolution emerges several years post-displacement, manifesting as a slow but persistent deterioration of the family environment. Concurrently, sickness benefits spike in the year of job loss, suggesting an immediate adverse shock to parental health. Because observed sickness subsidies in our data include maternity leave benefits, the increase is substantially larger for displacements occurring during the child's infancy. Online Appendix Figure D.3 confirms that this spike is driven by maternal job losses. In contrast, we detect no significant effects on residential mobility (moving to another parish, Online Appendix Figure D.2, Panel C) or subsequent fertility (birth of a younger sibling, Panel D).

In summary, these results indicate that average responses to parental job loss do not always align with the sharper achievement penalties observed among infants. However, average trajectories may conceal substantial variation. In the following subsection, we exploit this heterogeneity to identify which dimensions of parental shock are most strongly associated with reductions in achievement.

6.2. Parental Outcomes and Achievement Effects

We estimate individual-level impacts of parental job loss following the approaches in Britto, Melo, and Sampaio (2022b) and Schmieder, von Wachter, and Heining (2023).

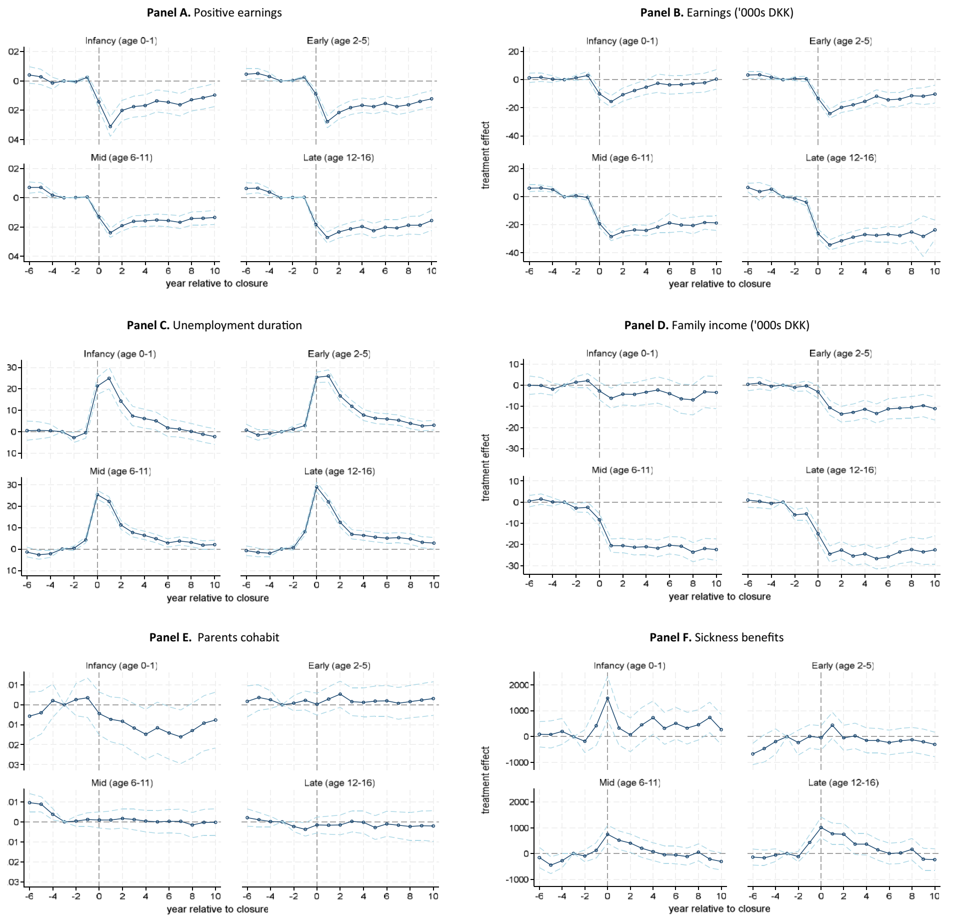

FIGURE 3. Plant closure impacts on parental outcomes by childhood stage. The figure shows event study estimates of the impacts of plant closure on parents' outcomes. Plotted are estimates of coefficients α_l from equation (3), separately for children in infancy (age 0–1), early childhood (age 2–5), mid-childhood (age 6–11), or adolescence (age 12–16) at parental job loss. Boundaries of the 95% confidence intervals are plotted as dashed lines. Years to or from plant closure are displayed on the x-axis, with $x = 0$ marking the year of job loss. The sample is restricted to observations in the $(-6{,}10)$ relative time window, and coefficients are parameterised with respect to $x = -2$. Reported outcomes are: employment (a binary variable equal to 1 if positive earnings are recorded in the year Panel A); labour earnings in DKK thousands (Panel B); the length of unemployment spells in the year, normalised to 1,000 (Panel C); the sum of parents' post-tax incomes (Panel D); parental cohabitation (a binary variable equal to 1 if parents live together, Panel E); sickness or maternity leave benefits received from the municipality in DKK thousands (Panel F). Standard errors are clustered at the individual level. See Section 6 for details.

Using our matched treatment-control pairs, we compute difference-in-differences estimates at the child level for four outcomes: family income, parental unemployment status, cohabitation arrangements, and receipt of sickness benefit. To examine treatment effect heterogeneity, we project individual treatment effects onto baseline characteristics and use these predictions to stratify our sample (see Online Appendix C for details). We rely on predicted rather than realised impacts because the latter are influenced by parental job loss, which would introduce endogeneity bias into our heterogeneity analysis.

Predicted family income changes are remarkably dispersed, as illustrated in Panel A of Online Appendix Figure C.1. The average predicted reduction is DKK 24,000 per year, a magnitude consistent with our event study estimates (Figure 3, Panel D). The interquartile range spans from a loss of DKK 78,000 to a gain of DKK 24,000 per year.[10] Heterogeneity in predicted family income loss is relatively more pronounced among children experiencing parental job loss in the earliest stages; this subgroup exhibits only half the average loss observed in older families, yet records a comparable standard deviation. Predicted changes in unemployment, parental cohabitation, and sickness benefits are also heterogeneous but substantially less dispersed (Panels B–D of Online Appendix Figure C.1). Heterogeneous labour market prospects following job loss are consistent with evidence in Athey et al. (2024) and Gulyas and Pytka (2025).

Achievement reductions spurred by parental job loss during early stages are strongly associated with the severity of the family income shock. Using teacher assessments in mathematics, we estimate equation (1) separately for terciles of predicted changes in family income. Terciles are computed across the *entire* distribution of income changes in our sample; that is, they are not specific to a given childhood stage. In this way, we compare children experiencing income shocks of similar magnitude, with the only difference being the timing of the shock. In Panel A of Figure 4, we estimate a 0.07σ reduction in achievement among families with infant children in the first and second terciles, those corresponding to the largest income losses (see Online Appendix Table C.1). In contrast, the effect for the third tercile, in which family income remains similar to or increases after job loss, is statistically indistinguishable from zero. This gradient exists but is attenuated for children treated at later stages.

Two additional exercises corroborate the importance of family income shocks. First, achievement penalties from parental job loss during infancy scale linearly with the magnitude of the income drop. Online Appendix Figure D.4 plots the estimated impacts on mathematics assessments against the average income shock within each *quintile* of predicted change. For children exposed during infancy (Panel A), each additional DKK 10,000 of annual family income loss is associated with a 0.006σ reduction in mathematics achievement, and this linear relationship closely tracks

10. While positive income effects following job loss may appear counterintuitive, they likely reflect improved job match quality for displaced parents, mean reversion among prior minimum-wage earners, or estimation noise inherent in individual-level predictions.

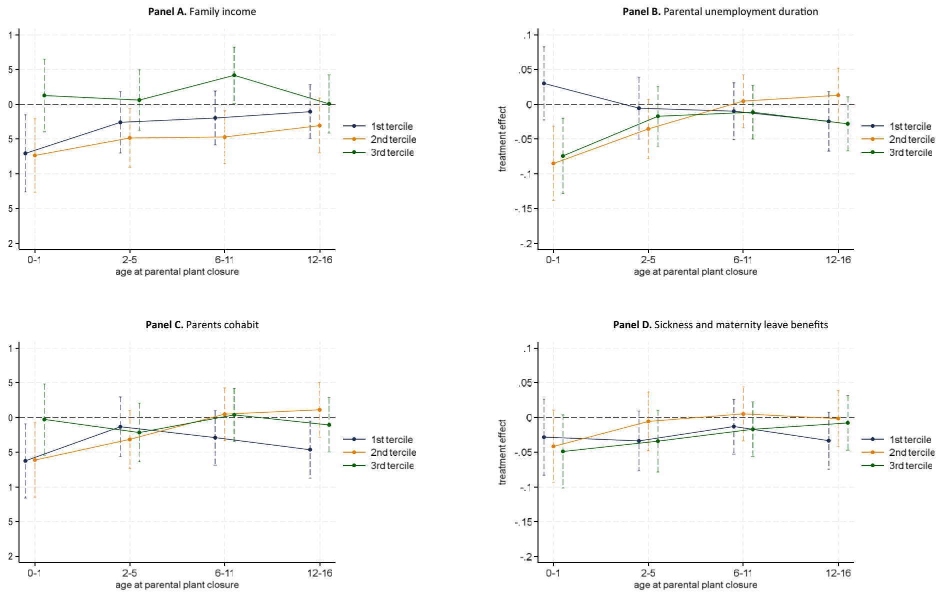

FIGURE 4. Predicted job loss impacts and math achievement. The figure shows estimates of the impacts of parental plant closure on children's grade 9 teacher assessment in mathematics by childhood stage at parental job loss and by predicted impacts on family income (Panel A), parental unemployment (Panel B), parental cohabitation (Panel C), or sickness benefits (Panel D). See the note to Figure 3 for definitions of parental outcomes. Plotted are estimates of the coefficients δ_s from equation (1) from separate regressions that split the sample by tercile of predicted impacts. Impacts are first estimated at the child level as difference-in-differences comparisons between each treated unit and its matched control, and are then regressed on a detailed set of parental, child, and family characteristics; the fitted values from the regressions are used to compute predicted impacts. The estimated specification is analogous to column (3) of Table 2. See Section 6 and Online Appendix C for details.

our point estimates. This gradient attenuates for children exposed in early and mid-childhood (Panels B and C) and vanishes entirely for those treated in adolescence (Panel D).

Second, we observe adverse effects on achievement only when the job loss affects the household's primary earner. Online Appendix Figure D.5 presents estimates of β_s from equation (1), stratified by whether the displaced parent's baseline earnings exceeded ('primary earner') or fell short of ('secondary earner') their partner's. Job loss of a primary earner during a child's infancy reduces test-taking by nearly 2 percentage points and mathematics scores by 0.06σ; in contrast, we detect no significant effects for secondary earners. While the magnitude is smaller, the displacement of a primary earner also generates adverse impacts when exposure occurs at later childhood stages. Similarly, increased educational fragmentation and missed or delayed enrolment into higher tracks are concentrated among job losses involving a primary earner (see Online Appendix Table D.6).

Panel B of Figure 4 shows how the achievement effect of parental job loss varies by terciles of parental unemployment duration. In addition to its negative effects through income losses, unemployment may also positively impact children's outcomes through increased availability of parental time. However, to the extent that unemployed parents experience low mood or stress, this latter channel may negatively affect children. The figure shows that for most childhood stages, the estimated effects are statistically indistinguishable across terciles, suggesting that the duration of the parent's unemployment spell is not the primary driver of our results. An exception appears in infancy, where children exposed to the longest parental unemployment spells suffer significantly larger penalties. While this exception might suggest a distinct role for parental distress in accounting for our results, we interpret this pattern with caution.

First, as already noted, predicted changes in unemployment duration are substantially more compressed than predicted changes in family income. Online Appendix Table C.1 illustrates this contrast. Moving from the bottom to the top tercile of predicted family income change, the within-tercile average loss increases by about DKK 190,000, close to one quarter of income for the average family (Table 1). In contrast, moving from the bottom to the top tercile of predicted unemployment change increases the within-tercile average by only about 4.3% of a year, that is, roughly 16 days on unemployment benefits. Given this limited dispersion, differences in predicted unemployment duration changes imply relatively modest variation in the availability of parental time, so one would not expect large differences in child outcomes to arise from this channel alone.

Second, to address the interplay between our findings and parental time availability, we use data on childcare coverage at the municipal level over time. Online Appendix Table D.7 presents heterogeneous effects by degree of cohort-specific childcare quality and availability. We find that achievement reductions are not larger among children in municipalities with lower child-care availability, who are more likely to spend time at home with their parents. This pattern therefore provides little support for mechanisms operating primarily through differences in parental time availability. In light of this evidence, we interpret the gradient observed in Panel B for infants as reflecting underlying income effects rather than as an indication of additional mechanisms operating through unemployment, although we cannot rule out stress- or mental-health-related channels that affect the quality of interactions and may be only weakly related to childcare availability.

Finally, we find largely overlapping effects across terciles of predicted changes in parental cohabitation (Panel C of Figure 4) and sickness benefit receipt (Panel D), indicating that family stability and parental health do not account for the observed achievement penalties. However, in the absence of direct measures of mental health, we cannot rule out a role for parental depression, which may be correlated with income shocks.[11]

6.3. Discussion

Our findings indicate that family income loss is the primary mechanism through which parental job displacement adversely affects children's educational outcomes. This interpretation aligns with evidence of positive effects on child development of cash transfer programmes targeting disadvantaged families (Aizer et al. 2016; Dahl and Lochner 2012; Hoynes, Schanzenbach, and Almond 2016). Studies that specifically examine job loss also find that larger parental income losses generate more substantial reductions in children's educational attainment (Britto, Melo, and Sampaio 2022b; Hilger 2016). One plausible channel is reduced household consumption: evidence shows that spending on necessities such as groceries declines sharply during unemployment spells, as households struggle to smooth consumption (Ganong and Noeng 2019; Gerard and Naritomi 2021).

The association between family income loss and reductions in achievement is concentrated among children exposed to parental job loss before age 5. Notably, because annual income drops are smaller and less persistent for younger children (Panel D of Figure 3), cumulative income losses by age 16 are not substantially larger for this group compared to children exposed at later stages. This pattern indicates that the effects are not driven by prolonged exposure to reduced income, but rather by specific vulnerability of early childhood development to income shocks.

We provide evidence that younger families' lower asset holdings may drive their specific vulnerability to income shocks. In the Danish context, Andersen et al. (2023) demonstrate that households' primary mechanism for smoothing consumption after job loss is asset decumulation; consequently, families with fewer assets are likely to experience sharper declines in consumption. Consistent with this mechanism, Figure 5 shows that achievement losses are concentrated among families with low baseline assets, particularly those facing the largest predicted income shocks. In the latter group, low-asset families experience a 0.1σ reduction in achievement (Panel A), an effect that doubles to 0.2σ among non-homeowners (Panel B).[12]

The latter findings support the hypothesis that assets serve as a critical buffer against resource loss. This interpretation aligns with the differential dynamics of family assets following job loss across childhood stages (Online Appendix Figure D.2, Panel A). Older families exhibit a large, immediate reduction in assets, consistent with active decumulation to smooth consumption. In contrast, younger families experience significant asset penalties only in the long run, suggesting that job loss primarily

11. Bhalotra et al. (2025) find that larger income drops after job loss lead to increased domestic violence, plausibly through heightened stress.

12. Before job loss, the median families with infants owns about half the assets compared to families with adolescents (DKK 974,000 vs. DKK 1.8 million). Moreover, only 73% of infants' families own a home in our sample, against 80% at later stages.

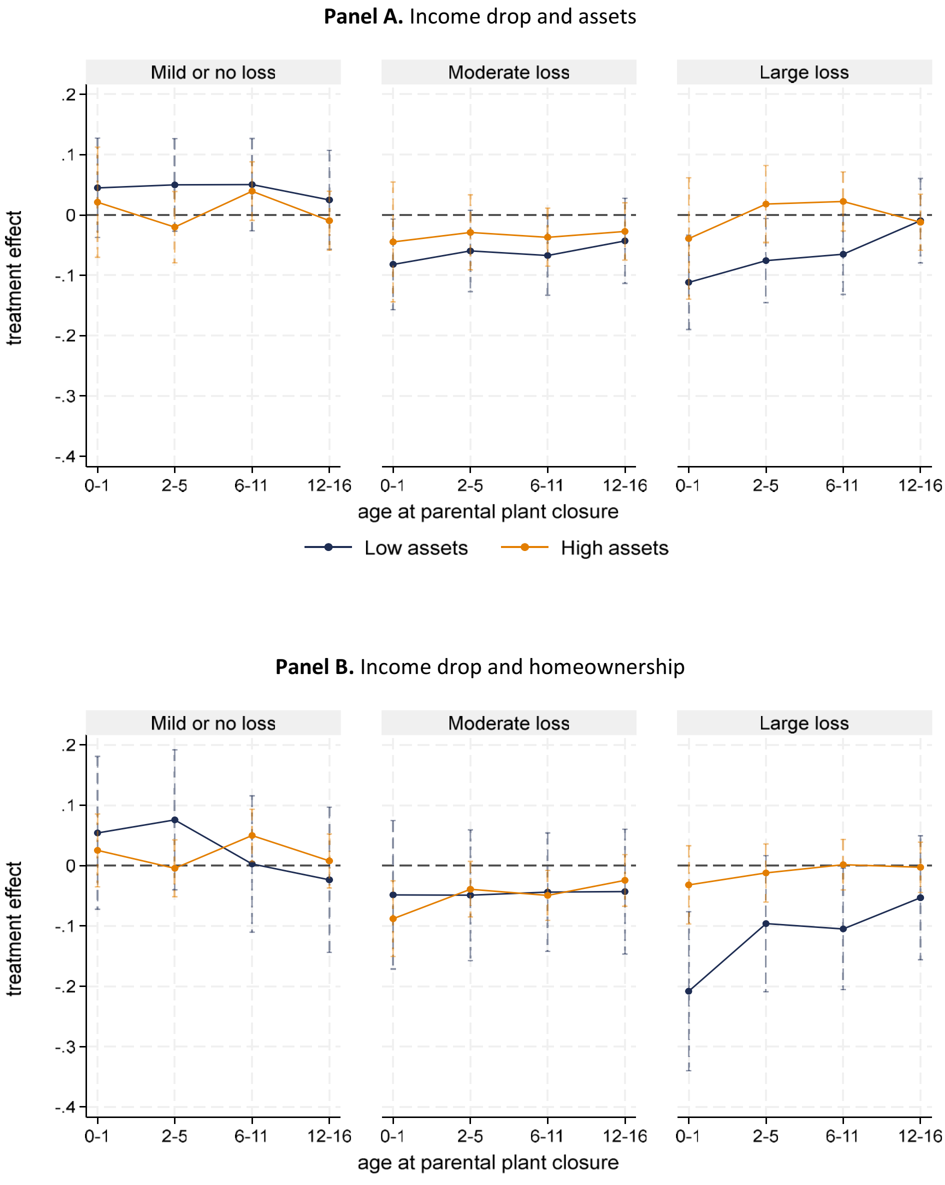

FIGURE 5. Parental plant closure impacts, family income, and assets drop. The figure shows estimates of the impacts of parental job loss on children's grade 9 teacher assessment in mathematics by childhood stage at parental job loss, by predicted impacts on family income, and by baseline family asset level. Estimates are analogous to Panel A of Figure 4, with additional sample splits by asset level. 'Mild or no loss', 'moderate loss', and 'large loss' indicate predicted family income changes following job loss in the third, second, or first tercile, respectively. Panel A splits the sample based on median family assets two years before job loss. Asset values are summed between parents. Outlier asset values are winsorised at levels exceeding the 75th percentile by three times the interquartile range. Panel B splits the sample by home ownership status 2 years before job loss. See Section 6 and Online Appendix C for details.

disrupts life-cycle asset accumulation, while their limited initial holdings constrain their ability to draw down assets for immediate consumption smoothing.

7. Conclusion

This paper provides new causal evidence that the timing of economic shocks determines the extent of their intergenerational transmission. By exploiting plant closures in Denmark and accounting for selection bias with a novel design, we document that parental job loss generates more severe human capital scars when the shock hits children during infancy. For children exposed before age 2, the shock significantly reduces ninth-grade achievement, increases the probability of grade retention, and lowers enrolment in general upper-secondary programmes.

These results highlight the cumulative nature of human capital formation. Test-taking and ninth-grade performance act as gatekeepers that determine future opportunities. A shock in infancy does not merely lower a test score; it alters the probability that a child clears the specific administrative and academic hurdles necessary to enter general upper-secondary education. This path dependence suggests that a transient liquidity shock in the first years of life can dictate adult socioeconomic status. Our findings thus provide empirical support for theories of dynamic complementarity, in which early-life inputs lay the foundation for the productivity of subsequent investments.

Our analysis sheds light on the ‘black box’ of the early-stage sensitivity by examining the interaction between household resources and child age. Our findings suggest that financial constraints, rather than other mechanisms, mainly drive the infancy penalty. We show that achievement reductions are concentrated among families with below-median income and among those with limited assets, who experience substantial income declines after job loss. In these households, the inability to smooth consumption likely represents the primary channel of damage.

The context of our study, a generous welfare state with universal healthcare and subsidised childcare, implies important lessons for external validity. That we detect significant scarring effects in Denmark suggests that the actual cost of early-life shocks may be even higher in settings with weaker social protection. Even with high-quality public daycare, the financial distress caused by a parental layoff permeates the home environment enough to derail long-run outcomes.

Finally, our findings suggest that policymakers could optimise social insurance design by accounting for children’s developmental needs. Current unemployment benefit schedules typically replace a fixed share of income regardless of family composition, implicitly assuming that the social cost of a layoff is constant across workers. Our evidence indicates that this ‘one-size-fits-all’ approach may be inefficient. Since the marginal damage of a dollar lost is highest during a child’s infancy, social safety nets could achieve higher long-run returns by targeting enhanced stability resources to parents of very young children. Such policies could take the form of higher replacement rates, lump-sum transfers upon birth, or extended benefit durations for new parents. By preventing transient liquidity shocks from becoming permanent human capital scars, these targeted interventions may offer a cost-effective strategy to disrupt the intergenerational transmission of inequality.

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Supplementary Data

Supplementary data are available at *JEEA* online.

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