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Paul **BINGLEY**[1]
Lorenzo **CAPPELLARI**[2,3]
Marco **OVIDI**[2]

[1] VIVE Copenhagen, Danemark
[2] Università Cattolica del Sacro Cuore, Italy
[3] Luxembourg Institute of Socio-Economic Research, Luxembourg

When it hurts the most: timing of parental job loss and a child's education *

Paul Bingley†
VIVE Copenhagen

Lorenzo Cappellari‡
Università Cattolica del Sacro Cuore and LISER

Marco Ovidi§
Università Cattolica del Sacro Cuore

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Abstract

We investigate the stages of childhood at which parental job loss is most consequential for their child's education. Using Danish administrative data linking parents experiencing plant closures to their children, we compare end-of-school outcomes to matched peers and to closures hitting after school completion age. Parental job loss disproportionally reduces test taking, scores, and high school enrolment among children exposed during infancy (age 0-1). Effects are largest for low-income families and low-achieving children. The causal chain from job loss to education likely works through reduced family income. Maternal time investment partially offsets the effect of reduced income.

JEL Codes: J13, D10, I24

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†Danish Center for Social Science Research (VIVE).

‡Department of Economics and Finance,Università Cattolica Milano, Via Necchi 5, 20123 Milano (Italy).

§Department of Economics and Finance, Università Cattolica, Via Necchi 5, 20123 Milano (Italy). Contact: marco.ovidi@unicatt.it.

1 Introduction

The effect of parental investments on child human capital may differ across childhood stages. Broad consensus has emerged on the importance 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 need to be better defined (Almond et al., 2018). Evidence on which stages of childhood are most consequential for development may indicate windows of opportunity for policy interventions to mitigate the impact of adverse shocks (Attanasio et al., 2022).

Parental unemployment is a widespread and consequential shock that can affect early childhood. A number of studies investigate intergenerational impacts of job displacement on children's education and earnings in different contexts, with mixed findings (see Ruiz-Valenzuela, 2021 for a review.). However, little evidence exists on children exposed to parental unemployment in early childhood. Carneiro et al. (2021) document that the timing of parental income within childhood is correlated with child outcomes conditional on permanent income, as predicted by models where (at least some) parents face borrowing constraints (Caucutt and Lochner, 2020). While recent studies have focused on the timing of parental job loss with respect to important junctures such as school track choice or labor market entry (Fradkin et al., 2019; Schmidpeter, 2020; Mari et al., 2022), there is little evidence on differential impacts by the age at which a child is exposed.

We investigate the stages of childhood at which parental job loss is most consequential for a child's education. We use Danish administrative data to track children whose parents experienced a plant closure when children were of different ages. We compare end-of-school outcomes of children exposed to parental job loss with those of observationally similar unexposed peers. We use older children at the closure time to control for age-invariant selection effects into parental job loss. We consider different stages of childhood building on Carneiro et al. (2021) and provide new evidence of relatively severe parental job displacement impacts if the child is exposed during infancy (age 0-1).

We identify the impacts of parental job loss by designing difference-in-difference comparisons around the age at which child's outcomes are realised. Our design relies on variation in exposure to parental plant closure and in its timing concerning child outcome realisation. We first select a control group of children with same gender, working parent's gender, and year of birth as the treated children by matching on parental labor market history. For matched control children, we define "placebo" plant closure events as the same age and calendar year in which the treated peer is exposed to parental plant closure. We condition on age at actual or placebo closure throughout our analysis. This design holds constant important confounders correlated with the child's age at parental plant closure (year of closure, child's year of birth, parental age at closure, and at child's birth). We then compare outcome differences between treated and control children with similar differences observed among units for which actual or placebo parental plant closure hits after the outcome of interest is realized. The latter difference controls for selection on unobservables into parental plant closures (Hilger, 2016). The identifying assumption is that absent parental plant closure, the difference between the educational outcomes of treated and control children would have remained constant across the age at closure distribution. We corroborate this assumption by showing null estimates for children exposed after the end of compulsory schooling. Our design improves on studies that only consider variation in the timing of the shock (e.g., Fradkin et al., 2019) as well as those estimating outcome differences between a treatment and a control group (e.g., Carneiro et al., 2022; Uguccioni, 2022).

Plant closure causes moderate but persistent shocks to parental labor market outcomes and family income. Exploiting the panel of job spells, we estimate event studies around plant closure akin to the standard approach in the broad literature on job loss effects (e.g., Bertheau et al., 2022). In the years following plant closure, displaced parents exhibit an eight percentage point (p.p.) increase in the likelihood of receiving unemployment benefits, and a eight percent drop in their labor earnings relative to pre-closure years. Labor market outcomes slowly recover, but remain substantially below pre-displacement levels 10 years after closure. The Danish welfare system and within-family adjustments somewhat mitigate the financial shock of displacement, with an average impact on family post-tax income around two p.p. per year.

Children suffering parental job loss are less likely to complete end-of-school examinations, and impacts are worse the earlier in life they are exposed. Although not mandatory, test-taking in grade nine is nearly universal in Denmark (93% of children obtain teacher assessments or test scores, typically taken at age 16). Parental plant closure decreases test-taking by 0.5 p.p., explaining seven percent of the observed non-completion rate. The estimate increases to one p.p. among children suffering parental job displacement during infancy, monotonically decreasing with age at closure, and becomes undetectable after age five. This "left-tail" outcome likely captures parental job displacement impacts on most vulnerable children, departing from studies only considering achievement conditional on test-taking (Carneiro et al., 2022; Uguccioni, 2022).

Among those completing end-of-school examinations, negative impacts of parental job displacement are concentrated on children exposed in infancy or, to a lesser extent, in adolescence. On average, teacher assessments in mathematics decrease by 0.02 standard deviations (hereafter, σ) due to parental plant closure. The estimate decreases to -0.05σ among children exposed during infancy and to -0.03σ among children exposed in adolescence (age 12-16). Impacts on children exposed in between these stages are smaller and not statistically significant. These results align with correlations between parental income timing and child outcomes documented by Carneiro et al. (2021). If all students were forced to take the tests, we would expect estimated effects to be larger since the sample distribution of potential outcomes is left truncated. We provide several pieces of evidence to argue that potential selection into parenthood anticipating plant closure does not drive our results. Further to our main findings, children suffering parental job loss in infancy are less likely to be enroled in upper-secondary education at age 17.

The negative impacts of parental job displacement on achievement are more substantial for children with relatively weak potential outcomes. A distributional analysis shows the largest impact on the probability of scoring in the bottom half of the achievement distribution. Mirroring average patterns, children exposed during infancy exhibit substantially larger negative estimates. Regardless of the timing of parental job displacement, treatment effects converge to zero in the top quartile of achievement. Consistently, heterogeneous effects show larger negative impacts in low-income families. Both paternal and maternal job loss have especially negative impacts when experienced in infancy, with the effects of maternal job loss relatively more persistent throughout childhood.

Family income losses in early childhood mirror our treatment effects. We show that parental job loss impacts on family income are remarkably heterogeneous and strongly associated with treatment effects among children exposed up to age five. In addition, negative effects of parental job loss in infancy are only detected when the displaced parent is the main earner, regardless of parent's gender. These results align with the positive impacts of cash transfers to disadvantaged parents (Dahl and Lochner, 2012; Aizer et al., 2016; Hoynes et al., 2016).[1] Among children with

[1]Although we cannot offer direct evidence, heterogeneous effects of parental job loss are also consistent with

relatively large family income drops, parental plant closure impacts are milder if the displaced parent spends more time in unemployment, suggesting that additional time to interact with the child may partly compensate for the negative effects of income shocks.[2] The latter result is strongest among children exposed before age five and substantially more pronounced for maternal job losses, in line with the importance of maternal interaction with the child at earliest stages (Del Bono et al., 2016).

Overall, a moderate financial shock to households especially hinders the future achievement of infants. Our findings imply that interventions targeted at displaced workers with young children may substantially benefit children's human capital development. Since the impacts of parental job loss are non-linear in child's age, and their association with family income loss suddenly weakens after age five, our results cannot be explained by the length exposure to the shock and suggest that family income is more productive at earlier stages. This productivity differential may arise because infancy is a "sensitive period" for human capital development (Cunha and Heckman, 2007, 2008) or from structural features of younger families that make income losses harder to compensate for. First, younger parents typically have lower assets, which could allow them to maintain their level of investment into children even after job loss. Second, the mitigation of negative impacts as soon as children reach preschool age may suggest an important role of the education system in absorbing family shocks.

Our study contributes to the nascent literature identifying the childhood stages at which family shocks are most consequential. Carneiro et al. (2021) show that family income spikes during early and late childhood are positively associated with children's long-run education and earnings. Concurrently developed with our study, the work by Carneiro et al. (2022) documents the largest effects of parental job displacement for children exposed in adolescence using mass layoffs in Norway. Uguccioni (2022) finds negative effects on earnings of children aged 2-10 at parental layoff in Canada.[3] We show that infancy represents a crucial stage, whereas no other study has considered ages 0-1 in isolation. Moreover, we provide evidence of differential roles of

a mediating role of psychological distress (e.g., Carneiro et al., 2022; Stans, 2022).

[2] Agostinelli and Sorrenti (2022) show negative impacts of increased maternal labor supply. We provide suggestive evidence of mitigating effects of longer parental unemployment spells after parental job loss.

[3] The importance of timing of crucial events in childhood is also documented in Chetty and Hendren (2018), showing that moving earlier (since age 10) to a high-mobility neighbourhood in the US increases adult income with respect to later moves; and in van den Berg et al. (2014), showing that migration to Sweden around age nine display higher adult height and cognitive ability with respect to migration at other ages.

income shocks and parental time investment in mediating parental job loss effects depending on the childhood stage at job loss.

A broader literature estimates the intergenerational impacts of parental job loss on children's outcomes. Using paternal layoffs in the US, Hilger (2016) finds minor impacts on college enrolment and early career outcomes, mainly explained by the income shock experienced by treated families. Mork et al. (2020) similarly find minor impacts of parental job displacement on child's health, education, and labor market outcomes in Sweden, while several papers show significant adverse effects.[4] By showing substantial differences in parental job displacement impacts depending on the childhood stage at the time of plant closure, our study may help reconcile mixed findings in the previous literature.

We document the consequences of plant closure on displaced parents, adding to the sizeable literature on the effects of job loss on the labor market, family, human capital, and crime outcomes of displaced workers. Job loss is an impactful event, leading to persistent earnings losses and unemployment.[5] We document moderate but persistent effects of plant closure on labor market outcomes in Denmark, found by Bertheau et al. (2022) to be relatively mild with respect to other countries and show substantial heterogeneity by gender, family income, and life cycle stage (Salvanes et al., 2021). Our results on displaced workers' children imply that moderate shocks are sufficient to produce negative intergenerational consequences if they hit in early childhood.

2 Institutional setting

The Danish labor market is characterised by employers having flexible hiring and firing rules and workers having high income security through unemployment benefits, with consequently low levels of income inequality. Kreiner and Svarer (2022) describe this "flexicurity" system

[4]See, e.g., Oreopoulos et al. (2008); Rege et al. (2011); Coelli (2011); Gregg et al. (2012); Pan and Ost (2014); Ruiz-Valenzuela (2020); Andersen et al. (2022). Huttunen and Riukula (2019) find that paternal layoff during a recession in Finland decreases the likelihood of working in father's plant or choosing father's field of study. Britto et al. (2022b) find negative effects of parental job displacement on child's education in the context of a developing country.

[5]See e.g., Jacobson et al. (1993); Davis and Von Wachter (2011); Lachowska et al. (2020); Gulyas and Pytka (2021); Schmieder et al. (2023). Beyond labor market outcomes, job loss has been found to impact workers' health (Browning and Heinesen, 2012; Ubaldi and Picchio, 2023), fertility (Del Bono et al., 2012; Huttunen and Kellokumpu, 2016), regional mobility (Huttunen et al., 2018), and the propensity to commit crime (Britto et al., 2022a).

as causing unemployment because of effectively unlimited unemployment benefit duration until a 1994 reform requiring participation in active labor market programs to retain benefits and limiting maximum benefit duration. This revised flexicurity system reduced income security while retaining employer flexibility, and coinciding with a much shorter unemployment period. Parents are entitled to temporarily leave their jobs around the birth of a child. The parental leave is 14 weeks for mothers and two weeks for fathers, with 32 additional weeks that can be shared between parents, though fathers typically take little of this additional leave (Jorgensen and Sogaard, 2022). Employed parents are entitled to parental leave subsidies equivalent to unemployment benefits, replacing 100% of pre-birth earnings but capped at a level close to the effective minimum wage, resulting in an average replacement rate of 53 percent. Employers commonly match the difference with pre-birth earnings. Before a major reform kicked in in 2002, parental leave was granted a full replacement rate for only 24 weeks, with 52 optional weeks with 60% replacement rate (Houmark et al., 2022). For our analysis, this setting implies that plant closure likely reduces the replacement rate of benefits received by parents on leave, similarly to how unemployment benefits reduce workers' income not on leave.

Compulsory schooling in Denmark spans ten grades, grades one through nine, with a kindergarten grade mandated from 2009. Compulsory schooling typically begins in August of the year a child turns six and usually ends in June of the year a pupil turns 16 when it is required to sit school-leaving exams. English, Danish, mathematics and physics/chemistry are always examined, plus pupils are randomized to exams in one humanities subject and one other science subject. These exams are generally low stakes because teacher recommendations for future lines of study are made in grade eight through the first term of grade nine. However, a 2015 reform introduced minimum requirements for scores in Danish and mathematics for entry into vocational education or training.

Graduates from compulsory school face a range of educational options. Many take an optional tenth grade before continuing to an upper secondary or vocational program. General programs typically last three years, including upper secondary schools (gymnasium, more academically oriented), business colleges, and technical colleges. Vocational programs combine education with on-the-job training, lasting two to five years.

3 Data

We use data from administrative registers of the Danish population. The civil registration system was established in 1968 and everyone resident in Denmark has been registered with a unique personal identification number, which has subsequently been used in all national registers, enabling accurate linkage. Using these identifiers, we can match children to parents and reconstruct families.

Schooling information is reported to the Ministry of Education by educational institutions. Our main outcomes are test scores obtained in grade nine, at the end of compulsory schooling. Records include ninth graders completing their exams in 2002-2018. Starting from a student-subject-grade level dataset, we compute average scores by student-subject standardised to have zero mean and unit variance in the subject-year-specific distribution. Both examination scores and scores from continuous assessments are recorded.

We use matched employer-employee registry data covering the population of workers and job spells. Job spells and plant data are observed in the 1980-2017 period. We match these records to earnings, unemployment, and income registers. Earnings and post-tax income are reflated to 2020 Danish Krone (DKK, with 7.46 DKK per €). Unemployment data records the fraction of time spent in unemployment in the year.

Plant closures

We start by identifying plant closures in the data following Browning and Heinesen (2012). We track a plant's owner (firm), industry, municipality, and employees by linking plant records and job spells. We investigate year-to-year changes in these characteristics between end-November census dates. We consider a plant as closed if we observe several of these characteristics changing from one year to another (see Appendix A for details). Importantly, if despite other changes a large share of employees is re-employed at a single newly-established plant, we do not consider such an event a closure.

Next, we define the closure year based on the magnitude of the employment downsize. We require a minimum downsize of three employees and 30 percent of the workforce, and a minimum plant size of five employees five years before closure. Among the last three years of operation, we define the closure year as the period with the largest employment downsize (see Appendix

A for details). Our procedure identifies 51,002 closures from 1986-2017, around 1,500 per year. As expected, job flows in the closure year suggest that these are disruptive events for workers. Nearly 96% of employees leave the plant the following year, compared to 30% at non-closing plants. Moreover, 83% of workers at closing plants leave their firm, and 30% do not record any job spells in the following year, compared to 30% and 15% in non-closing plants, respectively.

Plant closures are arguably difficult to anticipate. 83% of closures leave the plant with no employees, with an average downsize of 18 workers (median 9) in the closure year. In contrast, the median closing plant has the same headcount the year before closure as in the previous year. Therefore, closures do not generally involve a gradual, predictable downsizing.

Treatment and control groups

Once plant closure is defined, our data construction proceeds in three steps. First, we select a sample of treated and untreated workers. Second, we link treated and untreated workers to their children. Third, we select one suitable control peer for each treated child based on displaced parent's and child's gender, child's birth year, parental labor market history, and family characteristics.

We start by considering employees with a stable working history who have experienced a plant closure. Let t^* indicate the year before plant closure, the "base" year hereafter. We select workers aged between 25 and 60 in t^* with at least three years of tenure at the current firm, regardless of gender. Including mothers represents an extension with respect to studies considering only father's job loss (e.g., Hilger, 2016; Uguccioni, 2022). We focus on private-sector plants because of the difficulty of identifying distinct public-sector workplaces. We do not consider workers experiencing more than one plant closure. This selection yields 315,347 workers treated between 1986 and 2017.

Treated workers are observationally different from those not experiencing plant closure. We select a pool of workers not exposed to plant closure using the same criteria as for the treatment group (aged 25-60, with at least three years of tenure at the current firm).[6] Each worker is considered a potential control every year when these conditions are met. Table C.1 compares characteristics of treated and untreated workers in this sample. Individuals we analyze are

[6] To match requirements imposed on the treated group, we also require potential control workers' plant to employ at least 5 individuals.

more likely male (63 percent of treated workers and 62 percent of untreated), reflecting higher prevalence of stable working histories among men. On average, treated workers have worse labor market outcomes than untreated workers, with lower earnings and post-tax income by 3.4 and 2.2 log points, 0.6 years less tenure, and work in smaller plants. They complete 0.3 years of schooling less than potential controls. Treated workers are also three p.p. more likely than untreated workers to receive some unemployment benefits in t^*, suggesting that unemployment may start diverging before plant closure.

Similarly, children exposed to parental plant closure differ from unexposed peers. We match treated and untreated workers to their children and focus our worker analysis on parents. Since we seek to precisely determine the childhood exposure stage, we consider children exposed to at most one plant closure, i.e., untreated children and children whose either mother or father experienced plant closure. We select children aged between 0 and 22 in $t^* + 1$ (the year of parental plant closure) born between 1986 and 2002; this selection yields 133,531 treated children and 675,321 untreated children. Table C.2 compares characteristics of treated and untreated children in this sample. Sixty-six percent of treated children experience paternal job loss, with the remaining children exposed to maternal plant closure. Parental labor market outcomes differ between treated and untreated children, mirroring the disparities presented in Table C.1. Treated children have lower post-tax family income than untreated children in t^* by 42,000 DKK (about 5,600€), their parents are younger in t^* by more than one year, and younger at childbirth by 0.08 years.

We build a control group of children observationally similar to treated peers but not exposed to parental job displacement. First, we split the sample of children into cells defined by year of birth, gender, and displaced parent's gender. Second, for each year and cell, we select control children through a 1:1 matching algorithm without replacemen,t minimising differences in parental age at childbirth, three-digit industry, municipality of plant, plant size, tenure, and earnings in $t^* - 1$ and $t^* - 2$; child's number of siblings, and birth order. Our algorithm matches 99% of treated children, with a final sample of 131,214 observations in both treatment and control groups. As shown in Table 1, the characteristics of treated and control children are strikingly similar, contrasting with large differences observed with the full sample. In particular, besides own and parents' gender and birth cohort being exactly matched, parent's plant size, tenure, and earnings, and post-tax family income in $t^*$are statistically and substantively indistinguishable in the two groups. Nevertheless, we note that our research design does not rely on the comparability of outcome levels between treated and control children.

4 Empirical strategy

We are interested in the impact of parental job loss on a child's education as a function of the child's age at the time of the shock. Comparing educational outcomes of treated and untreated children may suffer from selection bias if parents exposed to plant closure are self-selected in terms of individual traits that may translate into differential child achievement. Although plant closures may appear exogenously assigned to workers, at least to a greater extent than individual layoffs since no selection occurs within a plant, Hilger (2016) provides evidence of selection into closures based on unobservable characteristics. At the same time, end-of-school outcomes are observed once in a child's life, precluding event study models that follow individuals over time. Using children out of school by the time of parental job loss, we build a research design that does not require the assumption that treated and control children have similar potential outcomes. Our strategy improves on Carneiro et al. (2022) and Uguccioni (2022), who assume conditional random assignment of parental job displacement.

Comparing exposed children with different ages at parental job displacement is also unlikely to deliver causal effects since child's age at closure may be correlated with academic achievement regardless of parental job displacement. First, age at closure is the difference between the year of closure, which may correlate with macroeconomic conditions, and the child's year of birth, which would confound the effect of interest with secular trends in achievement.[7] Second, age at closure is the difference between parental age at closure, which may correlate with parent's potential labor market outcomes, and parental age at child's birth, which is correlated with child's achievement. These concerns are supported in Figure C.1, showing that achievement over an extended age-at-closure span (0 to 22) is correlated with age at closure regardless of parental job displacement.

To overcome the shortcomings of solely exploiting variation either in exposure to parental job loss or in its timing, we design difference-in-difference comparisons around school completion age. The control group described in Section 3 defines a set of children observationally similar

[7] Hilger (2016) shows that selection contaminates the year of closure and the child's birth year.

to treated peers who did not experience parental plant closure. We select control children in the same cohort of treated peers based on parental characteristics in t^* to define "placebo" plant closure events at the same age and calendar year of actual plant closures (Jaravel et al., 2018). In addition, we control for the outcome difference between treated and control children experiencing parental plant closure after end-of-school outcomes are realized. Denote $a(i)$ the age at real or placebo plant closure for child i. We consider children with $0 \leq a(i) \leq 22$ and estimate the following specification:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 T_i \cdot \text{EXP}_i + \phi_{a(i)} + \psi X_i' + u_i, \tag{1}$$

where T_i is a dummy indicating treated children, and the exposure dummy EXP_i indicates exposure to actual or placebo plant closure before the outcome of interest Y_i is observed (for end-of-school outcomes, $\text{EXP}_i \equiv \mathbb{1}[a(i) \leq 16]$). We include dummies for child age at closure ($\phi_{a(i)}$) and varying sets of individual characteristics (X_i') in which we always include dummies indicating the year of actual or placebo parental plant closure and child's birth year. Estimates of β_2 represent the average impact of parental job displacement on children's outcomes throughout childhood (age 0-16). Estimates of β_1, in contrast, represent age-invariant selection into treatment, which is not detected in models directly estimating outcome differences between treated and control children (e.g., Carneiro et al., 2022; Uguccioni, 2022).

Our strategy addresses recent concerns about the validity of staggered designs (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2023). These papers show that, with variation in treatment timing, two-way fixed effects estimation departs from the simple two-by-two difference-in-difference model by pooling comparisons of units initially treated in different periods, including "forbidden" comparisons of later-treated with already-treated units. In our setting, each treatment-control pair defines a 2x2 difference-in-difference comparison, and our control units are never exposed to the treatment in the observation period. Therefore, no forbidden comparisons arise when stacking our pairs by design.

We rely on the assumption of age-invariant selection into treatment. As long as, absent parental plant closure, the difference between educational outcomes of treated and control children would have remained constant across different ages, our design controls for selection on unobservables. We provide evidence in support of this assumption in Section 5 by estimating a nonparametric specification that replaces EXP_i in Equation (1) with age at closure dummies:

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

where coefficients λ_k are parametrzed with respect to $a(i) = 17$, the earliest age at which plant closure cannot impact end-of-school outcomes. If our identifying assumption holds, estimates of $\lambda_{18}, \ldots, \lambda_{22}$ should not be distinguishable from zero.

One threat to our empirical strategy, especially with our focus starting from the earliest years of life is parental self-selection into fertility around plant closure. Since we cannot date closures within the last year of a plant's operation, a minority of children born in the year of plant closure ($a(i) = 0$) may be conceived after plant closure. Selection into parenthood based on plant closure would violate our identifying assumption. The direction of the bias is unclear. We view negative selection into parenthood (e.g., workers less concerned about potential closure impacts on a child's development) as just as plausible as positive selection (e.g., workers with solid income prospects regardless of plant closure). Nevertheless, we provide evidence in Appendix C that selection into parenthood and anticipation of plant closures, more generally, is unlikely to drive our results.[8]

Our primary interest is in the stages of childhood at which parental job loss is most consequential. We consider groups of ages at parental plant closure building on the analysis in Carneiro et al. (2021). They split childhood into three mutually exclusive stages: early (age 0-5), middle (age 6-11), and late (age 12-17) childhood. We end childhood at age 16 to reflect the timing of our outcome and additionally consider infancy as the earliest stage of childhood (age 0-1, redefining early childhood as age 2-5). Our main specification of interest is:

$$Y_i = \delta_0 + \delta_1 T_i + \sum_{s \in \{I,E,M,L\}} \delta_s T_i \cdot \mathbb{1}[a(i) \in s] + \phi_{a(i)} + \pi X_i' + \eta_i, \tag{3}$$

where s indexes the stage of childhood and I, E, M, and L denote infancy and early, middle, and late childhood, respectively. In this formulation, the coefficient δ_s represents the difference-in-difference comparison of Y_i between treated and control children exposed to real or placebo parental plant closure in childhood stage s versus ages 17-22. Comparing estimates of δ_s across stages, we show differential effects of parental plant closure on educational outcomes by the stage of childhood at which a child is exposed. Other coefficients in equation (3) are by construction

[8]Potential selection into parenthood motivates excluding children born the year after plant closure, either in-utero or not-yet conceived when the establishment closed. We return to this in Appendix C.

equivalent to those in equation (1).

To illustrate the nature and magnitude of the shocks to which treated children are exposed, we perform event studies of parental outcomes around plant closure. We exploit the panel of parental working histories and estimate the following:

$$Y_{it} = \theta_0 + \theta_1 T_i + \sum_{l=-6}^{10} \alpha_l T_i \cdot \mathbb{1}[t = t_i^* + 1 + l] + \sum_{l=-6}^{10} \mu_l \cdot \mathbb{1}[t = t_i^* + 1 + l] + \psi_i + \psi_t + \zeta_{it}, \quad (4)$$

where Y_{it} is a labor market outcome of child i's displaced parent in year t. Time relative to closure is normalised to zero in the closure year ($t_i^* + 1$) and coefficients α_l are estimated relative to $t_i^* - 2$. Since parental outcomes are observed annually, this specification allows the inclusion of child dummies (ψ_i), following the standard approach in the literature on job loss impacts on workers (e.g., Bertheau et al., 2022). Estimates of coefficients α_l depict the dynamic impacts of plant closure on the labor market trajectory of displaced parents. Appendix B also shows how these estimates vary by childhood stage at parental plant closure.

5 Results

We discuss our main results, starting from the labor market and income shocks experienced by parents as a result of plant closure. The subsequent analysis focuses on children, documenting the impact of parental job loss on test taking and scores at the end of compulsory education by childhood stage at parental plant closure. With a distributional analysis, we show which regions of the achievement distribution are most affected by parental plant closure and how this differs by childhood stage. We conclude the section by discussing parental job loss impacts on high school enrolment, showing that effects on end-of-school achievement are consequential for children's educational career.

Parental outcomes

We start by showing the consequences of plant closure on the displaced parent's income and labor market outcomes, constituting the "first stage" of our analysis. We plot estimates of coefficients α_l in Equation (4) parametrised with respect to the period $t^* - 2$, three years prior to the real or placebo plant closure.

Plant closure generates a moderate negative shock to parental earnings. Panel A of Figure 1 plots estimation results using annual earnings as a fraction of the average earnings in pre-treatment periods ("relative" earnings). In the plant closure year, relative earnings drop by six percent with respect to control workers, decreasing by an additional two percent the year after, a loss that persists in the following years and slowly shrinks to three percent ten years after closure. The magnitude of the earning loss is slightly lower than findings by Bertheau et al. (2022) considering mass layoffs, perhaps because parents are either positively selected or more pressed to quickly find a job with respect to the general population of workers. Earnings penalties from job loss in Denmark were the lowest across the seven European countries analyzed by Bertheau et al. (2022).

Parental employment drops after plant closure. The likelihood of positive earnings decreases by three p.p. in the plant closure year and remains one p.p. lower than controls 10 years after (Panel B). While not recording any labor earnings in a year may be an extreme unemployment event, the probability of unemployment jumps by eight p.p. after plant closure, with a slow but close to full recovery after 10 years (Panel C). Similar patterns are observed for the length of unemployment spells (Panel D).

Labor market shocks are mitigated by the welfare system and by adjustments within the family. The post-tax income of the treated worker decreases, on average, by three p.p., a roughly constant loss that is further mitigated in the plant closure year, possibly due to more generous benefits (Panel E). The response of family income is even milder, dropping by around two p.p. per year, suggesting that partner's income can attenuate the shock suffered by treated workers (Panel F).[9]

We find heterogeneous job loss impacts on parents based on age, gender, and baseline income. We highlight here the most relevant results for our subsequent analysis (see Appendix B for more details). Mothers suffer larger employment losses and longer unemployment spells folllowing job loss, while individual and family income drops more after paternal plant closure (Figure B.1), reflecting gender gaps. Moreover, low-income parents suffer worse initial impacts on employment and earnings. At the same time, individual and family income drops more for high-income parents (Figure B.2), likely partly reflecting the Danish safety net. Finally, families where children, and on average also parents, are younger at job loss generally experience higher short-

[9]In each year, we define family income as the sum of post-tax income of the child's parents.

term unemployment but less severe and persistent losses in the medium term (Figure B.3). To interpret our subsequent results, we note that the latter findings imply that the cumulative impacts of parental job loss at age 16, when children's outcomes are measured, are hardly larger among younger families.

Overall, on average, plant closure imposes a persistent and relatively moderate shock on displaced parents. Job loss affects employment conditions more than family income. In what follows, we investigate the consequences of these shocks on children's outcomes.

Taking school-leaving exams

Children of displaced workers are less likely to sit exams at the end of compulsory schooling. We focus here on the probability of obtaining test scores or teacher assessments at grade nine, and the age at which this happens (typically, age 16). Failing to obtain end-of-school grades is a "left-tail" outcome since 93% of children obtain test scores or teacher assessments in at least one subject. Panel A, columns (1)-(3), of Table 2 reports estimates of β_1 and β_2 in Equation (1), where the outcome is a dummy equal to one if the child has not obtained any end-of-school test scores or teacher assessments. Experiencing parental plant closure increases the likelihood of not obtaining scores or assessments by 0.5-0.6 p.p, explaining 6-9% of the observed non-completion. Similarly, columns (4)-(6) show that treated children are 0.6 p.p. less likely to obtain scores or assessments in both Danish and mathematics, the two core subjects. The average impact on the likelihood of sitting grade nine exams by age 16 is negative but not statistically different from zero (-0.4 p.p., columns 7-9). Results are stable across specifications, including demanding models controlling for industry and municipality dummies.[10]

The earlier in childhood parental plant closure hits, the larger the impact on test taking. We start by investigating the nonparametric relationship between treatment effects and age at closure in Figure 2. Plotted are coefficients λ_k in Equation (2), where the outcome is a dummy equal to one if a child does not obtain grade nine scores or assessments in any subject, with control variables specified as in column (2) of Table 2. Results support our identifying assumption by showing statistically zero treatment effects for children treated from age 18 onwards (the joint significance test of the $\lambda_{18}, \dots, \lambda_{22}$ coefficients reports p=0.986). While individual point

[10]Despite being (often marginally) insignificant, estimates of β_1 in Equation (1) are sizeable in magnitude relative to treatment effect estimates of β_2, underlining the importance of control for age-invariant selection into parental plant closure.

estimates are imprecise, treatment effects are persistently around one p.p. in the first years of life and visually decrease with child's age at closure. We next present parametric estimates grouping a child's age at closure by stage of childhood.

A child's test-taking is mainly affected by parental plant closures suffered in infancy. Regressions by childhood stage in Panel B of Table 2, reporting estimates of δ_s in Equation (3), show that children experiencing parental plant closure in infancy (age 0-1), are one p.p. less likely to obtain scores or assessments at grade nine (columns 1-3). This effect contrasts with estimates of 0.7-0.8 p.p. for children exposed at age 2-5 (early childhood), 0.5 p.p. for children exposed at age 6-11 (mid childhood), and 0.3 p.p. for children exposed at age 12-16 (late childhood), with estimates not significant above age five. A similar monotonic pattern, with treatment effects decreasing by age at closure, is found for the probability of taking mathematics and Danish exams (columns 4-6). Impacts on the likelihood of completing grade 9 exams by age 16 are even more skewed towards children exposed during infancy, with an estimated decrease of 1.3-1.4 p.p. and barely detectable impacts at later stages (columns 7-9).[11]

Parental plant closure disproportionally affects children in families with fewer resources. Table 3 presents heterogeneity analyses of average treatment effects in columns (2), (5), and (8) of Table 2. We split the sample based on parental and child characteristics indicated in column headers. The most striking difference in treatment effects is found between children with family income above or below median in $t^* - 2$, three years before plant closure (columns 2-3 of Table 3). Parental job loss substantially decreases test-taking for children in families with below-median income, while we find no effects in families with above-median income. In line with full-sample results, estimates are larger in magnitude among children exposed to parental plant closure during infancy and of smaller magnitude at later childhood stages. Among those hit during infancy, we find qualitatively larger adverse effects on test-taking with a displaced father than a displaced mother (column 8-9) and among sons (columns 10-11).

Overall, parental plant closure decreases the likelihood of taking end-of-school exams, especially if parental job loss is experienced during infancy. The result that negative impacts are concentrated on families with below-median income may suggest that resource constraints are a primary mechanism driving our findings (we return to this conjecture in Section 6). The larger

[11] 78% of children in our estimation sample complete exams by the year they turn 16, 14% postpone them by one or, rarely, by two years.

impacts of parental job loss in the earliest childhood stages are expected in the presence of credit constraints and dynamic complementarities in human capital investments (Caucutt and Lochner, 2020). We turn in the following subsection to analyze academic achievement among children who take grade nine exams.

Child's academic achievement

Conditional on test-taking, children exposed to parental plant closure have lower achievement in mathematics in grade nine. Table 4 presents these results following the same structure as Table 2. On average, parental plant closure decreases teacher assessments in mathematics by 0.02σ (Panel A, columns 1-3 of Table 4), while estimates on test scores are around -0.01σ and are not statistically different from zero (columns 4-6). If all students were forced to take the tests, we would expect estimated effects to be larger since the treatment truncated the left tail from the sample (see Table 2). This truncation is likely the reason why estimates generally decrease when controlling for parental and child characteristics (compare column 1 and 4 with columns 2-3 and 5-6, respectively).

Exposure to parental job loss in infancy, and to a lesser extent in adolescence, particularly hinders mathematics achievement (Panel B of Table 2).[12] In our preferred specifications (columns 2 and 5), parental plant closure in infancy decreases teacher assessments by 0.05σ and test scores by 0.04σ. Impacts are negative but mostly imprecise for children hit in early childhood, and they are close to zero for children exposed in mid-childhood. In contrast to the results of test-taking, negative impacts of parental job loss arise again for children hit in adolescence, with a decrease of 0.03σ in teacher assessments and of 0.02σ in test scores (although the latter are imprecisely estimated). In common with the education literature, we find smaller effects in language than in mathematics (Table C.3).[13] Parental job loss impacts on achievement in Danish are negative only among children exposed in infancy (about -0.02σ), but imprecisely estimated. Hence, negative effects on GPA are likely driven by mathematics.

[12]Figure C.3 presents nonparametric estimates for teacher assessments in mathematics showing that, in line with our identifying assumption, estimated impacts during post-school periods are close to zero. We cannot reject the hypothesis that they are jointly equal to zero (p=0.759).

[13]Performance in mathematics is more responsive to school interventions with respect to language (Cronin et al., 2005; Zheng, 2022).While estimates of β_1 in Equation (1) are relatively small in mathematics, they are sizeable and statistically significant for Danish achievement and GPA, implying substantial age-invariant selection into closure. This may contribute to explain why our effects are generally smaller to the ones found by Carneiro et al. (2022), using GPA as achievement outcome.

We find larger negative impacts among children with fewer resources. Table 5 presents heterogeneity analyses of parental job loss impacts on achievement in mathematics. Larger negative impacts are estimated among children in families with below-median income in $t^* - 2$ (columns 2-3) or whose displaced parent has below-median education (columns 4-5). However, these differences are generally not statistically significant.[14] Negative impacts are more prominent among children whose displaced parent has below-median tenure in t^* (columns 6-7). For these children, we estimate a decrease in mathematics teacher assessments of 0.09σ if exposed during infancy and of 0.04σ if exposed in early childhood or in adolescence.[15]

Males and children experiencing maternal job loss suffer larger decreases in teacher assessments in mathematics. Negative impacts of parental job loss experienced in adolescence are only detected among males (columns 10-11, Panel A of Table 5). This pattern is substantially weaker for test scores (Panel B). Teacher assessments are more consequential than test scores for upper-secondary school choice, adolescence is the period leading to these examinations, and males are found to suffer more than females from test-related psychological distress (Heissel et al., 2021), suggesting that stress may be an explanation of this result. In addition, we find that while impacts of paternal displacement are concentrated in infancy, impacts of maternal displacement are larger for teacher assessments and more equally distributed across ages at closure (columns 8-9). This result is in line with findings by Carneiro et al. (2022) and with their maternal stress hypothesis.

Distributional effects

We combine exam sitting and achievement outcomes in a distributional analysis to investigate which parts of the achievement distribution are most severely impacted by parental plant closure. We consider 80 test score levels in the $(-2\sigma, 2\sigma)$ interval at the equal distance of 0.05σ, and for each level we define a dummy variable equal to one if the child scores at or above that level. We then estimate Equation (3) 80 times using each dummy variable as outcomes.[16] We code our dummy outcomes equal to zero if a child does not obtain teacher assessments or test scores.

[14] In unreported results, we find that parental job loss during infancy also significantly negatively impacts Danish language achievement when displaced parents have below-median education or tenure in t^*.

[15] Evidence of the robustness of these results to potential selection into parenthood around plant closure is presented in Appendix C.

[16] We consider the specification adopted in column (2) of Table 2. Our exercise is similar to Figure D.1 in Campos and Kearns (2023).

The resulting estimates represent the parental job loss impacts on the likelihood of scoring at least at the considered level.

Negative impacts of parental job displacement on mathematics achievement are concentrated in the lower part of the test score distribution. We plot distributional estimates in Figure 3, where dashed red lines indicate the thresholds between quartiles of the achievement distribution. The dependent variable is teacher assessment in mathematics. Across childhood stages, estimated impacts are negative and mostly statistically significant in the bottom quartile. This result is in line with larger negative effects for children with less family resources in Tables 3 and 5.

Throughout the achievement distribution, negative impacts are substantially larger among children exposed to parental job loss in infancy (Panel A of Figure 3). For these children, the likelihood of scoring in the bottom quartile decreases by around two p.p. The corresponding estimates for children exposed in early childhood (Panel B) or in adolescence (Panel D) are minus one p.p. The negative impact for children exposed in mid childhood (Panel B) is less than one p.p. and often of borderline statistical significance. The gap in adverse effects between infancy and later childhood stages grows larger in the second quartile and the lower part of the third quartile. While estimates for children exposed in infancy remain around two p.p. in this distribution region, impacts on children exposed later shift closer to zero. The relative magnitude of estimates across childhood stages reflects our results in Tables 2 and 4.

Regardless of childhood stage, parental job loss's impacts converge to zero towards the top of the achievement distribution. Starting from achievement levels of about $+0.5\sigma$ (in the upper part of the third quartile), the estimated impact of parental job loss becomes zero independently of the stage of childhood when parental plant closure occurs. This result is once again in line with heterogeneous effects showing little or no impacts on children with more family resources.

Overall, the distributional analysis strongly indicates that children with below-median potential outcomes are most negatively affected by parental job loss. This result holds across childhood stages and is particularly severe for children exposed during infancy. While children in the second quartile of potential achievement are mostly unaffected if exposed to parental plant closure at ages later than two, they, too, suffer achievement reductions if exposed in infancy.

School choice outcomes

We show that educational choices after compulsory education are consistent with our main results. At age 16, children in Denmark can either exit formal education or choose from various academic and vocational tracks (see Section 2). Most children (around 60%) opt for an elective tenth grade in school before entering upper-secondary education. We measure enrolment at the beginning of the academic year when a child turns 17 and estimate modified versions of Equation (3) where adolescence is defined as age 12-17 at parental plant closure.

In line with our main results, children suffering parental job loss in infancy are less likely to be enrolled in upper-secondary school at age 17. We estimate a negative effect of 1.6 p.p. (6.5 percent of the control mean; see column 1, Panel C of Table 6). The corresponding decrease in upper-secondary enrolment mainly affects vocational tracks (-0.92 p.p., Panel B) rather than academic tracks (-0.65 p.p., Panel C). However, these track estimates are not statistically different from zero.

Adverse effects on a child's educational career are concentrated in low-income families. Parental plant closure decreases upper-secondary enrolment by 2.7 p.p. among low-income children exposed in infancy, in contrast with a statistically different estimate of 0.4 p.p. among high-income peers, which is not statistically different from zero (columns 2-3, Panel C of Table 6). The larger effect for low-income children mainly reflects decreased enrolment in academic tracks (-2 p.p., statistically significant at the 10% level, see Panel A). In contrast, high-income children exposed in infancy are less likely to enroll in vocational tracks (-2 p.p., Panel B), and have a positive but not statistically significant increase in the likelihood of enrolling in academic tracks (1.5 p.p., Panel A). For older exposure, we find a significant increase in academic track enrolment among high-income children exposed between ages 2 and 11. In contrast, low-income children exposed in mid childhood (age 6-11) are likelier not to enroll in upper-secondary education. Results suggest different educational choice responses to parental job loss based on family income. Although the estimates are not statistically different across groups, we estimate larger negative impacts of exposure in infancy for girls compared to boys (columns 6-7, Panel C), and for maternal rather than paternal plant closures (columns 4-5).

Overall, school enrolment at age 17 reflects our main results, implying detrimental consequences of parental plant closure on children's educational careers, especially those from lessresourced families. The especially negative impact of parental job loss experienced in infancy extends to educational choices at high school age, with likely negative impacts on future outcomes.

6 Mechanisms

We now investigate potential channels through which parental job loss may affect a child's education. The previous section shows how heterogeneous impacts on children exposed in adolescence are consistent with the hypothesis that displacement-related stress affects exam performance, as suggested by several studies (e.g., Carneiro et al. 2022; Mari et al. 2022; Stans 2022). Since we do not observe direct measures of stress or other related health outcomes, we focus here on two additional channels that have been invoked to explain parental shocks' intergenerational impacts. First, parental plant closure may hit a child's education through the subsequent income loss.[17] Tighter budget constraints could affect parents' ability to invest in child development. Second, parental job loss may allow unemployed parents to increase their time investment in their interactions with their children. Provided that the quality of parent-child interactions is not worsened by, e.g., mental health decline following job loss, these interactions may positively affect child development. We present the results of heterogeneity analyses supporting both channels.

Income shock

We begin by generating individual-level estimates of losses from plant closure. Using our treated-control pairs, we compute child-level difference-in-difference comparisons of parental earnings, parental unemployment spells, and family income (see Appendix D for details). Individual parental earning losses are widely dispersed. The average earnings change is -24,500 DKK, remarkably similar to our parental event study estimates (Panel C of Figure C.2), and the interquartile range is (-117,000, 77,000). This variability indicates substantial heterogeneity in parental labor market prospects. Although the positively predicted impacts of displacement on

[17] Within the literature on parental job loss impacts, Hilger (2016) and Britto et al. (2022b) show that larger parental income losses are associated with larger adverse effects on a child's education. More broadly, many studies show positive impacts on children's outcomes of income transfers from public programs (e.g., Dahl and Lochner, 2012; Aizer et al., 2016; Hoynes et al., 2016). Studies considering lottery winners, perhaps perceived as a temporary wealth shock, find instead little effects on children's development (Cesarini et al., 2016; Bulman et al., 2021).

labor earnings may sound surprising, there are several possible reasons why a small proportion of children could expect a parental earnings gain. First, parents could enjoy a better draw on the labor market than their previous job, perhaps getting the chance to increase job match quality. Second, parental earnings gains could be the outcome of mean reversion if the displaced parent previously worked at minimum wage. Finally, a few positive predicted impacts could reflect noise since we are considering individual-level estimates. We find similarly wide dispersion in family income and unemployment spell changes (see Appendix D for details).

Parental job displacement impacts on a child's education are strongly associated with family income losses, more so than with changes in parental earnings or unemployment benefits. We separately estimate equation (3) by quintiles of predicted impacts of parental job loss on family income, earnings, and unemployment spells. As detailed in Appendix D, we regress individual estimates of parental job loss impacts on parental, child, and family characteristics measured at baseline and use fitted values from these regressions to split the sample (Britto et al., 2022b). Since individual gains or losses are treatment outcomes, splitting the sample based on their values would raise endogeneity concerns. Predicted family income loss is highly correlated with treatment effects on teacher grades in mathematics (Panel A of Figure C.4). Children in the bottom quintile (i.e., with largest predicted income losses) exposed to parental job loss in infancy suffer a 0.15σ decrease in achievement. The effect monotonically shrinks in absolute value as predicted income loss reduces, reaching a null estimate in the top quintile (where, on average, we predict a gain in family income). This correlation is somewhat stronger for family income than for labor earnings (Panel B), suggesting that the net impact on total family resources is more consequential than individual parental labor income. The association is considerably weaker for predicted increases in unemployment (Panel C).

The association between treatment effects and income losses is substantially stronger among children exposed by age five. We relate treatment effects to predicted family income loss from parental job loss to estimate the marginal productivity of family resources regarding a child's education. Figure 4 plots estimates in Panel A of Figure C.4 against the average predicted loss in each quintile. For children exposed in infancy (Panel A), 1,000 DKK (about EUR 150) additional family income per year is associated with an increase in mathematics achievement by 0.0008σ ($p<0.01$), and this linear prediction closely fits our point estimates. Similar results hold for children exposed in early childhood (Panel B), with only a slightly poorer linear fit. The association is much weaker for children exposed to parental job loss in mid childhood (Panel C) or adolescence (Panel D).

Consistent with a leading mediating role of family income shocks, negative impacts of parental job loss are mainly detected when the displaced parent is also the breadwinner. Table 7 shows estimates of δ_s from equation (3) separately for children whose displaced parent earns more or less than their partner before job loss. We estimate small and statistically null impacts of parental job loss for the secondary earner on teacher assessments in mathematics throughout stages of childhood (column 1, Panel A). In contrast, the primary earner's displacement during infancy decreases achievement by around 0.07σ (column 2). Similar results hold for mathematics test scores (Panel B).

The breadwinner result holds regardless of the displaced parent's gender. Column (3) shows that the relatively few displaced fathers who are not the breadwinner generate no negative impacts on child's achievement. At the same time, we estimate a negative impact of 0.05σ if father is the primary earner and job loss occurs in infancy (column 4). In the few cases where displaced mothers are the breadwinner (column 6), their job loss in infancy decreases achievement dramatically (-0.2σ for grades, Panel A, -0.12σ for scores, Panel B). These parental job losses significantly decrease achievement if experienced in mid or late childhood, albeit to smaller extents. In contrast with results on fathers, some moderate impacts are found when the displaced mother is not the breadwinner (column 5).

Overall, we find robust evidence that our treatment effects are at least partly mediated by family income losses following plant closure. Similar income losses are associated with more harmful impacts when children are exposed by age five. Since income drops after job loss are less persistent for children exposed earlier in life (see Figure B.3), cumulative income shocks by age 16 are hardly larger when parental job loss hits at age 0-5 compared to later stages. Our results are, therefore, unlikely to just reflect more prolonged exposure to a family income shock, suggesting a greater responsiveness of child development to parental income shocks in the first years of life.

The potential role of parental time investment

We find suggestive evidence that the opportunity to spend time with the displaced parent partly compensates adverse income effects of job loss on children. We use impacts on unemployment spells to proxy parental time spent on the job and exploit variation in the relative extent of predicted changes in family income and parental unemployment after plant closure. We start by considering children with the largest family income drops (top tercile) and separately estimate treatment effects by tercile of predicted unemployment change. Figure 5 shows that, for children exposed to parental job loss in infancy or early childhood (Panels A and B), the largest adverse effects are found when the displaced parent spends less time unemployed. Impacts are substantially weaker in the presence of larger predicted impacts on parental unemployment spells. Results suggest that the potential for increased time investment in the interaction with the child counteracts the negative income shock. Treatment effects are weaker and not statistically different from zero regardless of parental unemployment for children exposed in mid childhood or adolescence (Panel C and Panel D).

Maternal job losses drive the compensatory effect of parental unemployment. Figure C.5 plots estimates similar to Figure 5 when considering maternal plant closures only. Compensatory effects of maternal unemployment are apparent at any stage of childhood and especially pronounced when exposure to parental job loss happens below age five. An inverse-U-shaped pattern is observed at all stages, suggesting that, conditional on large predicted income losses, children of mothers with medium-duration unemployment spells following job loss have milder achievement losses than peers whose mothers have longer unemployment spells. In contrast, estimates for paternal plant closures in Figure C.6 show fewer differences across predicted unemployment spell increases. Small but not statistically significant compensatory effects of paternal unemployment are estimated among children exposed below age five. Regardless of paternal unemployment, no treatment effects are detected at later stages. Results align with studies showing the importance of maternal interaction with children in earlier stages (e.g., Del Bono et al., 2016).

Parental unemployment only decreases a child's learning when associated with relatively large income losses. In a second exercise, we consider children with the largest parental unemployment spell increase (top tercile), and separately estimate treatment effects by terciles of family income drop. Results in Figure C.7 show that, for children exposed to parental job loss in infancy or early childhood (Panels A and B), those experiencing the smallest family income shocks exhibit positive treatment effects (although not statistically different from zero). The larger the income shock, the more severe is the estimated impact. We estimated small impacts on children exposed in mid-childhood or adolescence regardless of the extent of income shock (Panel C and Panel D).

Overall, we find suggestive evidence that family income loss is the main driver of the adverse impacts of parental job loss on achievement, as long as children are exposed before age five. These results align with studies of cash transfers to disadvantaged parents (Dahl and Lochner, 2012; Aizer et al., 2016; Hoynes et al., 2016). We acknowledge that we cannot rule out that larger income drops are also associated with larger increases in psychological distress, a potential alternative (perhaps complementary) explanation of our results. Conditional on a relatively large income shock, we find that children whose displaced parent spends more time in unemployment experience more modest achievement drops. This result aligns with recent evidence on the negative impact of maternal labor supply on child development in Agostinelli and Sorrenti (2022). While their study documents that the positive effects of increased labor income on child development are counterbalanced by the negative impacts of increased maternal labor supply, we find evidence that longer parental unemployment spells may compensate the adverse achievement effects of negative labor income shocks.

7 Conclusion

Early years are commonly viewed as a crucial period for child development, yet the boundaries of this critical phase are not clearly understood. Our analysis suggests that family shocks especially harm end-of-school achievement if experienced when a child is less than two years old. We have studied the impacts of persistent labor market shocks hitting one parent. Since adverse effects largely disappear after age five and reappear somewhat during adolescence, our results imply that child development is more responsive to family shocks in the first years of life rather than just reflecting more prolonged exposure to such shocks. This conclusion aligns with the evidence in Carneiro et al. (2021).

We offer insights into the human capital production function across childhood stages by exploring the role of family income and parental time investments in explaining our results. We show that the impacts of parental job loss on family post-tax income are closely related to treatment effects on a child's achievement as long as the child is younger than age five at the time of parental plant closure. At the same time, the association is substantially weaker at later stages. Conditional on large income shocks, we find milder effects among children whose parents spend more time in unemployment, a proxy for time investment in the child's development. This result may suggest that family income and parental interactions are substitutes for producing human capital in early stages.

Children with more vulnerable backgrounds are likely to suffer more serious consequences from parental job loss, raising concerns on the impacts on the equality of opportunities. We find more substantial negative impacts for children with lower family income or potential outcomes in the lower part of the achievement distribution. Our analysis of mechanisms shows great heterogeneity in parental labor market outcomes following plant closure, adn this heterogeneity is associated with differential impacts on child achievement. By forcing workers with heterogeneous earning abilities onto the job market, job loss may increase inequality in their outcomes and their children's prospects.

Our study offers immediate insights for policymakers. Among interventions designed to tackle the consequences of job loss, special attention devoted to workers with younger children could focus educational policies on helping their offspring to compensate for this shock. Our results are especially concerning, given the context we analyze. Since Denmark stands out in international comparisons in terms of welfare generosity and income security, we would expect the effects we find to be lower bounds of potential results in countries where safety nets are less well developed.

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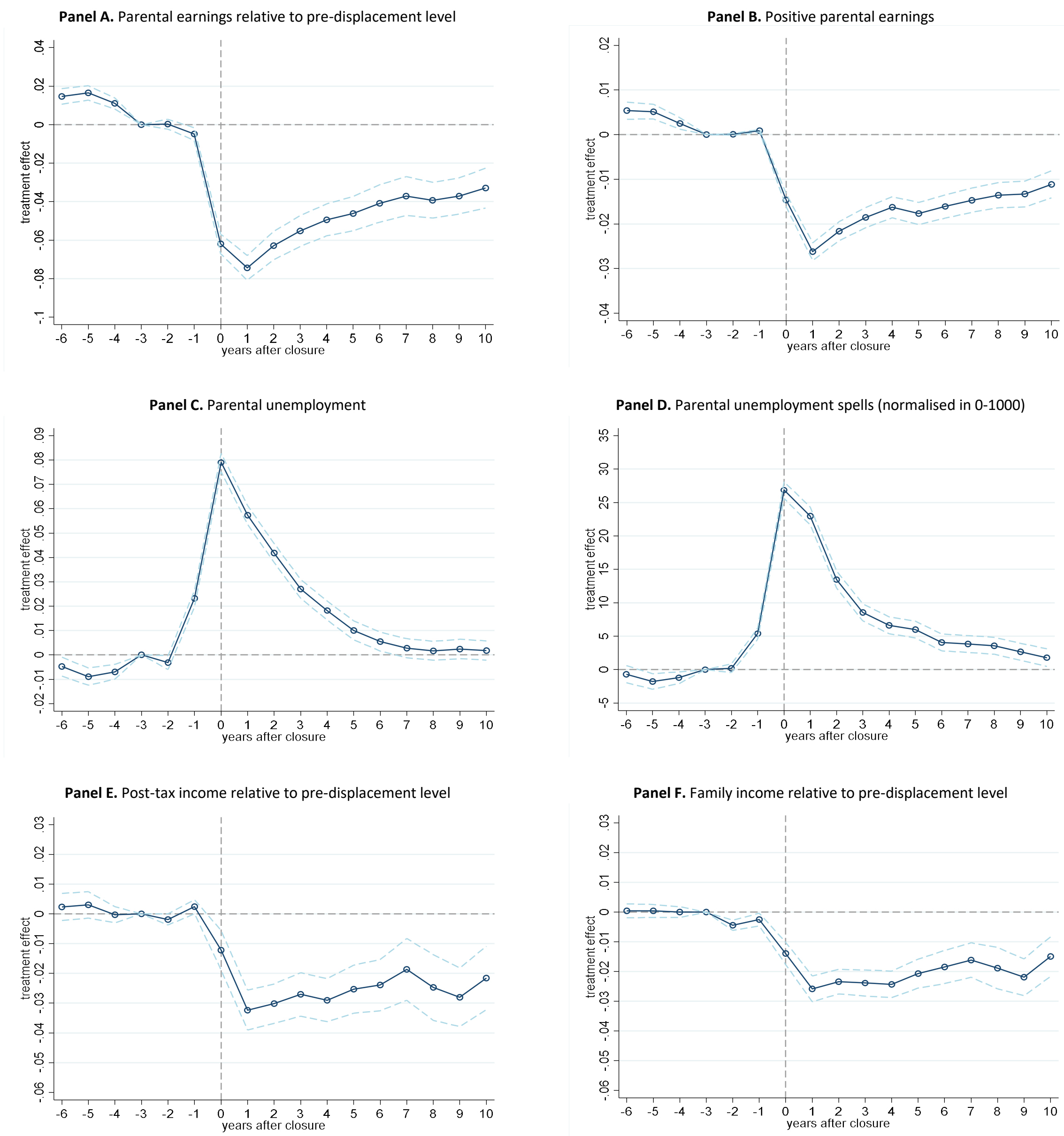
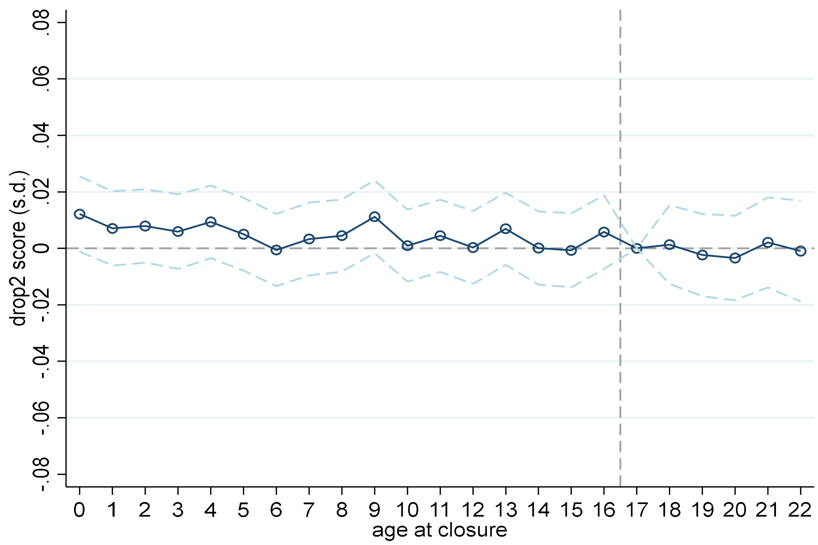
Figure 1: Plant closure impacts on parental outcomes



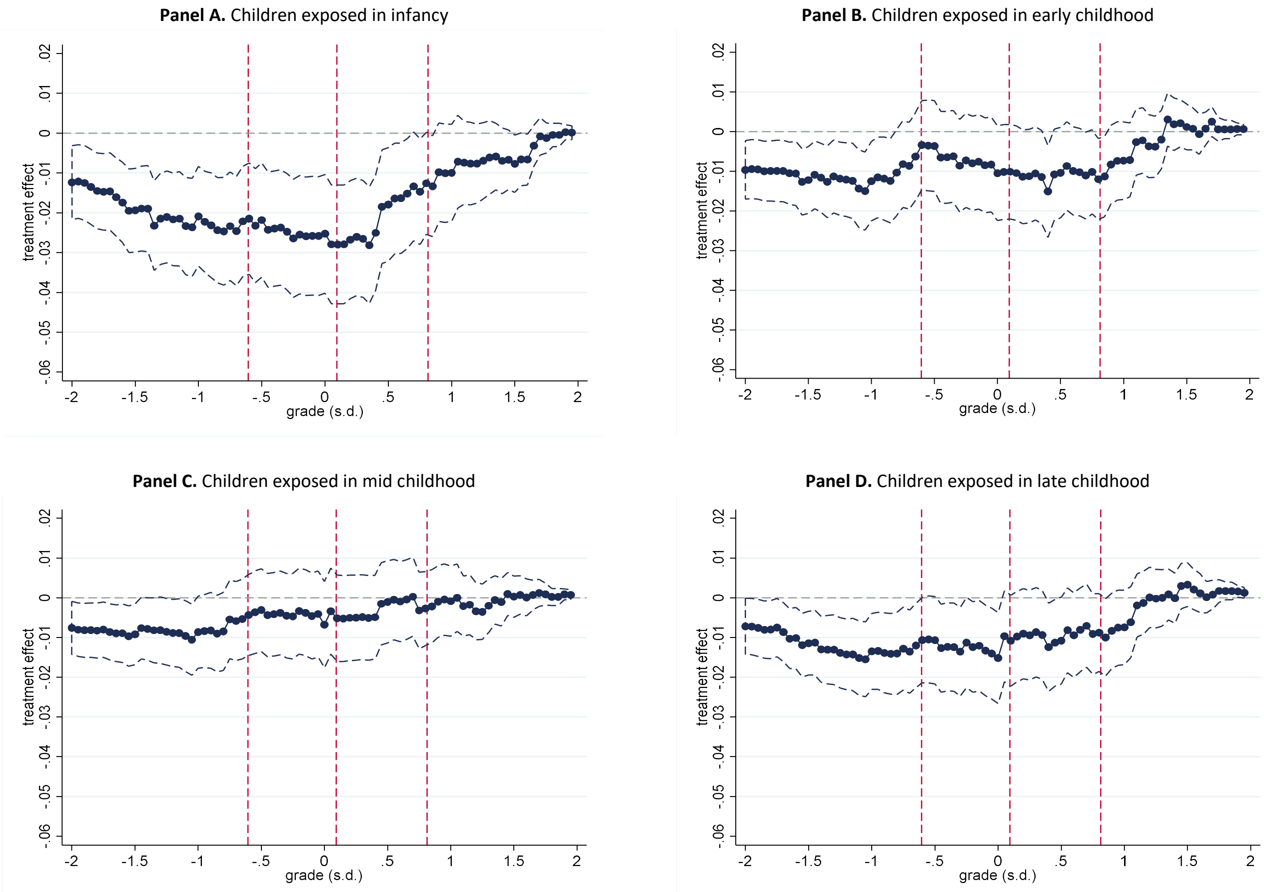
Note. The figure shows event study estimates of the impact of plant closure on parent's outcomes. Plotted are estimates of coefficients α_l in (4). Boundaries of the 95% confidence intervals are plotted as dashed lines. Standard errors are clustered at the individual level. Years to or from plant closure are displayed on the x-axis, and the sample is restricted to observations within 5 years around closure. Panel A considers labor earnings relative to the average before real or placebo plant closure. Panel B, Panel C, and Panel D consider the likelihood of reporting positive earnings in a given year, the likelihood of receiving unemployment insurance, and the length of unemployment spells in the year, respectively. Panel E and Panel F consider individual and family post-tax income, respectively. Family income is the sum of both parents' post-tax income 3 years before closure. See Section 5 for details.

Figure 2: Parental plant closure impacts on test taking by age at closure



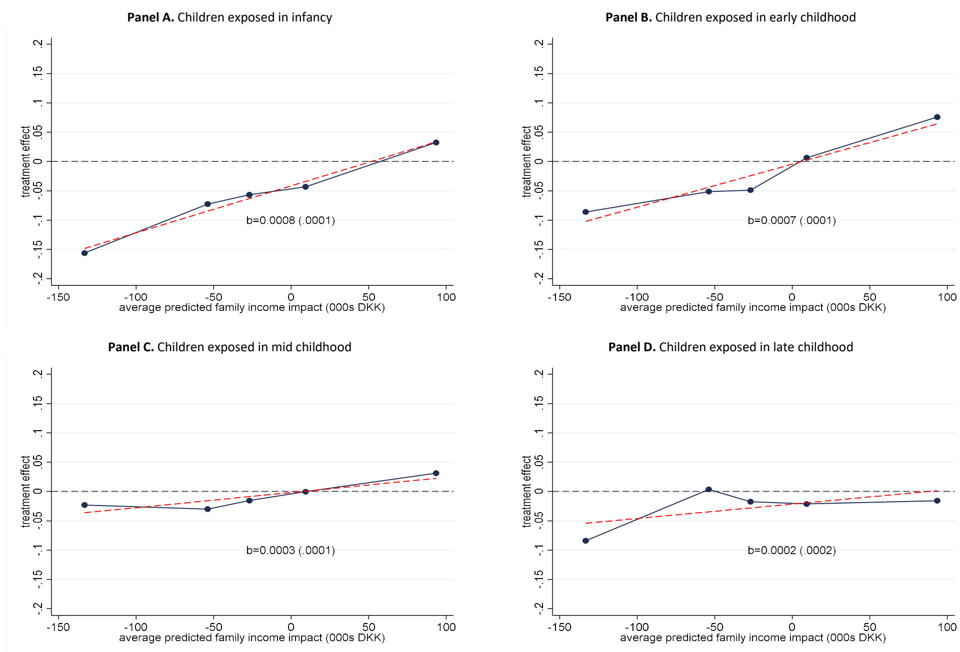
Note. The figure shows estimates of the impact of parental plant closure on child's grade 9 achievement by age at the time of closure. Plotted are estimates of coefficients λ_k from Equation (2). Boundaries of the 95% confidence intervals are plotted as dashed lines. Standard errors are clustered at the family level. Dependent variable is a dummy equal to one if child does not obtain test scores or teacher assessments in grade 9 examinations. Estimated specifications are analogous to column (2) of Table 2. See Section 5 for details.

Figure 3: Distributional impacts of parental plant closure by age at closure



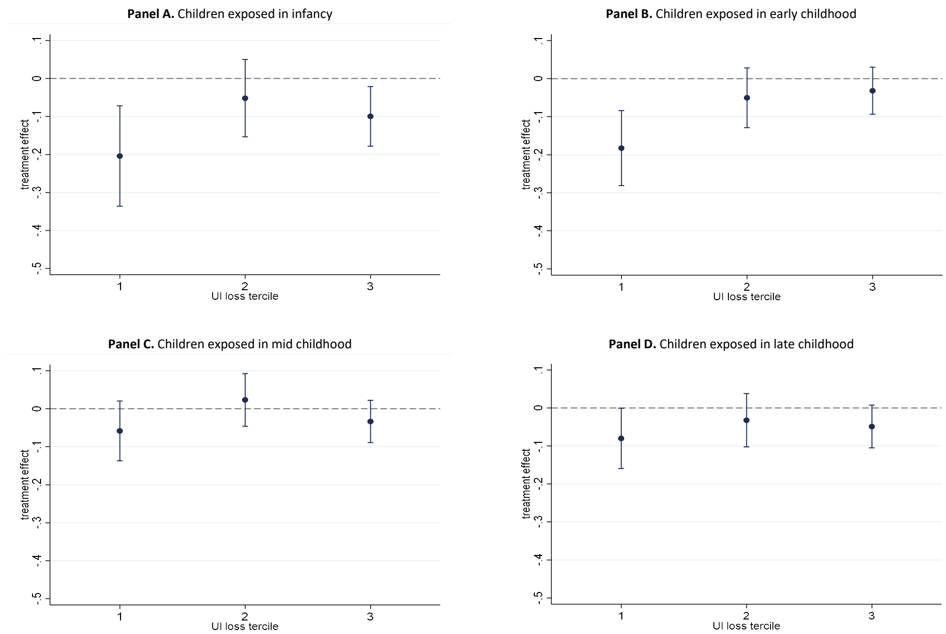
Note. The figure shows estimates of the distributional impacts of parental plant closure on child's grade 9 achievement by age at the time of closure. Plotted are estimates of coefficients δ_s from Equation (3) from 80 different regressions. Dependent variables are dummies equal to one if child scores at or above the considered level in grade 9 teacher assessments in mathematics, and we group scores in the 80 equally spaced 0.05σ-wide intervals between -2σ and 2σ. Outcomes are coded to zero for children not obtaining teacher assessments. Panels A, B, C, and D plot treatment effects on children exposed to parental plant closure in infancy (age 0-1), early (age 2-5), mid (age 6-11) or late (age 12-16) childhood, respectively. Boundaries of the 95% confidence intervals are plotted as shadowed areas. Estimated specification is analogous to column (2) of Table 2. See Section 5 for details.

Figure 4: Treatment effects by predicted family income change



Note. The figure shows estimated treatment effects in Panel A of Figure C.4 against average predicted income loss by childhood stage at parental plant closure. Estimated coefficients for children exposed in infancy, early childhood, mid childhood, and adolescence is plotted in Panel A, Panel B, Panel C, and Panel D, respectively. See Section 6 for details.

Figure 5: Parental plant closure impacts with relatively large family income drop



Note. The figure shows estimated impacts of parental plant closure on child's grade 9 achievement in mathematics by age at the time of closure for children with relatively large predicted family income drop. The sample is formed by children in the top tercile of predicted family income loss. Plotted are separate estimates of δ_s from Equation (3) by tercile of predicted parental unemployment spell increase. Estimated specification is analogous to Panel A of Figure C.4 against average predicted family income loss by childhood stage at parental plant closure. Estimated coefficients for children exposed in infancy, early childhood, mid childhood, and adolescence is plotted in Panel A, Panel B, Panel C, and Panel D, respectively. See Section 6 for details.

Table 1: Descriptive statistics on treated and control children

	Treated children		Control children			
	Mean	SD	Mean	SD	Difference	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.5137	0.4998	0.5137	0.4998	0.0000	1.0000
Parent received UI	0.1133	0.3170	0.0814	0.2734	0.0320	0.0000
Parent in manufacturing	0.2191	0.4136	0.2403	0.4273	-0.0212	0.0000
Parent in services	0.1757	0.3806	0.1860	0.3891	-0.0103	0.0000
Parent in other industries	0.6051	0.4888	0.5736	0.4946	0.0315	0.0000
Parent's plant size	145.0278	321.3466	144.7958	384.5900	0.2320	0.8668
Parent's tenure	7.6486	4.8546	7.6238	4.9162	0.0248	0.1927
Parent's earnings (2020 DKK, 000's')	429.4209	251.4452	429.0586	275.8753	0.3623	0.7251
Parent's post-tax income (2020 DKK, 000's)	475.3738	544.7828	483.7045	3584.6295	-8.3307	0.4053
Year of birth	1993.6042	4.7083	1993.6042	4.7083	0.0000	1.0000
Family post-tax income (2020 DKK, 000's)	805.1285	385.3392	804.1336	472.7532	0.9949	0.5546
Age at displacement	10.0166	6.1295	10.0166	6.1295	0.0000	1.0000
N. of siblings	1.2796	0.8788	1.2779	0.8818	0.0017	0.6241
Birth order	1.6387	0.7796	1.6351	0.7825	0.0036	0.2401
Father	0.6583	0.4743	0.6583	0.4743	0.0000	1.0000
Parent's age at birth	30.7077	4.9526	30.6994	5.0120	0.0082	0.6722
Parent's age at base year	39.6426	7.2915	39.6352	7.3272	0.0074	0.7966
Parent's years of education	12.7801	2.2102	12.8802	2.2306	-0.1002	0.0000
N	131,214		131,214			

Note. The table shows descriptive statistics on treated and control children. Sample considered is formed by matched treated-control pairs resulting from the procedure described in Section 3. Parental and child characteristcs are observed in the base year (the year before plant closure). Columns (1) and (3) show average values for treated and control children, respectively, and columns (2) and (4) show corresponding standard deviations. Column (5) computes the difference between column (1) and column (3), and column (6) reports the p-value of the associated t-statistics. See Section 3 for details.

Table 2: Impacts of parental plant closure on test taking

	Do not take test or receive grades			Obtain scores or grades in Maths and Danish			Complete Year 9 examinations by age 16		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A. Average impacts across childhood					
Treated	-0.0034	-0.0032	-0.0027	0.0040	0.0039	0.0036	-0.0001	-0.0004	-0.0017
	(0.0025)	(0.0024)	(0.0025)	(0.0026)	(0.0026)	(0.0026)	(0.0038)	(0.0038)	(0.0038)
Treated X Exposure (age 0-16)	0.0057**	0.0054**	0.0052*	-0.0064**	-0.0062**	-0.0061**	-0.0042	-0.0038	-0.0026
	(0.0027)	(0.0027)	(0.0027)	(0.0028)	(0.0028)	(0.0028)	(0.0042)	(0.0041)	(0.0042)
				Panel B. Impacts by childhood stage					
Treated	-0.0034	-0.0032	-0.0027	0.0040	0.0039	0.0036	-0.0001	-0.0004	-0.0017
	(0.0025)	(0.0024)	(0.0025)	(0.0026)	(0.0026)	(0.0026)	(0.0038)	(0.0038)	(0.0038)
Treated X Infant (0-1 years)	0.0096**	0.0099**	0.0098**	-0.0100**	-0.0104**	-0.0105**	-0.0139**	-0.0144**	-0.0131**
	(0.0041)	(0.0041)	(0.0042)	(0.0044)	(0.0044)	(0.0044)	(0.0065)	(0.0065)	(0.0065)
Treated X Early childhood (2-5 years)	0.0079**	0.0076**	0.0071**	-0.0089**	-0.0087**	-0.0083**	0.0016	0.0019	0.0037
	(0.0034)	(0.0033)	(0.0034)	(0.0036)	(0.0035)	(0.0036)	(0.0053)	(0.0052)	(0.0053)
Treated X Mid childhood (6-11 years)	0.0049	0.0045	0.0046	-0.0056*	-0.0052	-0.0054*	-0.0053	-0.0046	-0.0036
	(0.0031)	(0.0030)	(0.0031)	(0.0032)	(0.0032)	(0.0032)	(0.0048)	(0.0047)	(0.0048)
Treated X Late childhood (12-16 years)	0.0033	0.0030	0.0028	-0.0040	-0.0037	-0.0037	-0.0037	-0.0033	-0.0025
	(0.0032)	(0.0031)	(0.0031)	(0.0034)	(0.0033)	(0.0033)	(0.0050)	(0.0050)	(0.0050)
Constant	0.0697***	0.0785***	0.0789***	0.9204***	0.9108***	0.9103***	0.7814***	0.7692***	0.7679***
	(0.0007)	(0.0025)	(0.0024)	(0.0008)	(0.0027)	(0.0026)	(0.0011)	(0.0041)	(0.0040)
N	262,428	262,428	262,427	262,428	262,428	262,427	262,428	262,428	262,427
Age at closure dummies	X	X	X	X	X	X	X	X	X
Year of birth dummies	X	X	X	X	X	X	X	X	X
Year of shock dummies	X	X	X	X	X	X	X	X	X
Parental characteristics		X	X		X	X		X	X
Children characteristics		X	X		X	X		X	X
Industry and municipality dummies			X			X			X

Note. The table shows estimates of the impact of parental plant closure on child's test-taking at the end of compulsory school. The first row reports estimates of β_2 in equation (1), averaging across exposed children. The following rows report estimates of δ_s in equation (3) from a single regression per column. Constant and sample size are common to both regressions. Columns (1), (4), and (7) include controls for age at closure, year of closure, and child's year of birth dummies. Column (2), (5), and (8) adds parental earnings and post-tax income in the base year, dummies for displaced parent's gender, age, age at birth, tenure and plant size in the base year, n. of siblings, and child's birth order. Column (3), (6), and (9) adds industry (3-digit) and municipality dummies. Standard errors are clustered at the family level. Estimation considers the 131,214 treated children and their corresponding controls indentified in Section 3. Dependent variables are a dummy equal to one if child does not obtain test scores or teacher assessments in grade 9 examinations (columns 1-3), a dummy equal to one if child obtains scores or assessments in both Danish and mathematics (columns 4-6), or a dummy equal to one if child completes grade 9 examinations by age 16 (columns 7-9). See Section 5 for details.

Table 3: Heterogeneous impacts of parental plant closure on test taking

	Full sample	Family income		Parental education		Parental tenure		Parent's gender		Child's gender	
		Below median	Above median	Below median	Above median	Below median	Above median	Mother	Father	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Panel A. Do not take test or receive grades						
Treated	-0.0032	-0.0052	-0.0004	-0.0030	-0.0032	-0.0055	-0.0015	-0.0038	-0.0027	-0.0024	-0.0040
	(0.0024)	(0.0043)	(0.0025)	(0.0028)	(0.0041)	(0.0035)	(0.0031)	(0.0036)	(0.0030)	(0.0030)	(0.0035)
Treated X Infant (0-1 years)	0.0099**	0.0128**	0.0047	0.0101**	0.0092	0.0091*	0.0141**	0.0066	0.0113**	0.0037	0.0153**
	(0.0041)	(0.0061)	(0.0060)	(0.0049)	(0.0070)	(0.0055)	(0.0062)	(0.0065)	(0.0051)	(0.0052)	(0.0061)
Treated X Early childhood (2-5 years)	0.0076**	0.0122**	0.0010	0.0074*	0.0080	0.0103**	0.0053	0.0070	0.0075*	0.0050	0.0099**
	(0.0033)	(0.0053)	(0.0041)	(0.0039)	(0.0056)	(0.0046)	(0.0046)	(0.0053)	(0.0041)	(0.0042)	(0.0049)
Treated X Mid childhood (6-11 years)	0.0045	0.0108**	-0.0011	0.0048	0.0035	0.0069	0.0026	0.0025	0.0053	0.0043	0.0044
	(0.0030)	(0.0052)	(0.0033)	(0.0035)	(0.0051)	(0.0043)	(0.0040)	(0.0047)	(0.0037)	(0.0038)	(0.0044)
Treated X Late childhood (12-16 years)	0.0030	0.0072	-0.0001	0.0026	0.0037	0.0045	0.0021	0.0040	0.0024	0.0035	0.0023
	(0.0031)	(0.0056)	(0.0034)	(0.0037)	(0.0054)	(0.0046)	(0.0041)	(0.0049)	(0.0040)	(0.0040)	(0.0047)
Constant	0.0785***	0.1697***	0.0527***	0.0870***	0.0628***	0.0905***	0.0683***	0.0790***	0.0884***	0.0620***	0.0941***
	(0.0025)	(0.0052)	(0.0018)	(0.0020)	(0.0024)	(0.0022)	(0.0022)	(0.0021)	(0.0016)	(0.0021)	(0.0023)
					Panel B. Obtain scores or grades in Maths and Danish						
Treated	0.0039	0.0048	0.0015	0.0042	0.0023	0.0063*	0.0021	0.0048	0.0031	0.0039	0.0038
	(0.0026)	(0.0045)	(0.0027)	(0.0030)	(0.0044)	(0.0037)	(0.0032)	(0.0039)	(0.0032)	(0.0032)	(0.0037)
Treated X Infant (0-1 years)	-0.0104**	-0.0119*	-0.0068	-0.0102**	-0.0106	-0.0099*	-0.0143**	-0.0074	-0.0118**	-0.0078	-0.0125*
	(0.0044)	(0.0065)	(0.0064)	(0.0052)	(0.0074)	(0.0058)	(0.0066)	(0.0069)	(0.0054)	(0.0055)	(0.0065)
Treated X Early childhood (2-5 years)	-0.0087**	-0.0133**	-0.0008	-0.0097**	-0.0059	-0.0111**	-0.0070	-0.0073	-0.0089**	-0.0079*	-0.0094*
	(0.0035)	(0.0056)	(0.0044)	(0.0042)	(0.0059)	(0.0049)	(0.0048)	(0.0057)	(0.0043)	(0.0044)	(0.0052)
Treated X Mid childhood (6-11 years)	-0.0052	-0.0112**	0.0005	-0.0060	-0.0029	-0.0075	-0.0035	-0.0034	-0.0059	-0.0069*	-0.0033
	(0.0032)	(0.0055)	(0.0035)	(0.0038)	(0.0054)	(0.0046)	(0.0043)	(0.0050)	(0.0040)	(0.0040)	(0.0047)
Treated X Late childhood (12-16 years)	-0.0037	-0.0066	-0.0014	-0.0040	-0.0026	-0.0061	-0.0020	-0.0055	-0.0027	-0.0038	-0.0035
	(0.0033)	(0.0060)	(0.0036)	(0.0040)	(0.0057)	(0.0049)	(0.0044)	(0.0052)	(0.0042)	(0.0042)	(0.0049)
					Panel C. Complete Year 9 examinations by age 16						
Treated	-0.0004	0.0069	-0.0076*	0.0023	-0.0098	0.0062	-0.0057	-0.0006	-0.0005	0.0046	-0.0049
	(0.0038)	(0.0064)	(0.0044)	(0.0044)	(0.0070)	(0.0056)	(0.0049)	(0.0059)	(0.0047)	(0.0047)	(0.0055)
Treated X Infant (0-1 years)	-0.0144**	-0.0223**	-0.0073	-0.0217***	0.0073	-0.0165*	-0.0181*	-0.0154	-0.0138*	-0.0109	-0.0178*
	(0.0065)	(0.0091)	(0.0103)	(0.0076)	(0.0118)	(0.0086)	(0.0100)	(0.0106)	(0.0080)	(0.0081)	(0.0097)
Treated X Early childhood (2-5 years)	0.0019	-0.0096	0.0149**	-0.0028	0.0161*	-0.0003	0.0005	0.0090	-0.0010	0.0024	0.0013
	(0.0052)	(0.0080)	(0.0071)	(0.0061)	(0.0094)	(0.0073)	(0.0073)	(0.0087)	(0.0064)	(0.0065)	(0.0077)
Treated X Mid childhood (6-11 years)	-0.0046	-0.0143*	0.0028	-0.0077	0.0054	-0.0075	-0.0041	-0.0000	-0.0067	-0.0115*	0.0019
	(0.0047)	(0.0077)	(0.0057)	(0.0055)	(0.0086)	(0.0068)	(0.0065)	(0.0076)	(0.0059)	(0.0059)	(0.0070)
Treated X Late childhood (12-16 years)	-0.0033	-0.0151*	0.0050	-0.0050	0.0036	-0.0086	0.0008	0.0042	-0.0076	-0.0102	0.0034
	(0.0050)	(0.0084)	(0.0059)	(0.0058)	(0.0091)	(0.0072)	(0.0067)	(0.0079)	(0.0062)	(0.0062)	(0.0074)
N	262,428	129,219	133,209	191,194	71,234	141,668	120,760	89,682	172,746	127,612	134,816
Year of birth FEs	X	X	X	X	X	X	X	X	X	X	X
Year of shock FEs	X	X	X	X	X	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X	X	X	X	X	X

Note. The table shows estimates of the heterogeneous impacts of parental plant closure on child's test-taking at the end of compulsory school. Estimates and specifications are analogous to columns (2), (5), and (8) of Table 2, replicated here in column (1) to ease comparison (Panel A, B, and C, respectively). Following columns restrict estimation to children subgroups: children with family income below or above-median 3 years before parental plant closure (columns 2-3, respectively), children with parental education below or above median (columns 4-5, respectively), children with parental tenure below or above median in the base year (columns 6-7, respectively), children experiencing maternal or parental job displacement (columns 8-9, respectively), male or female chldren (columns 10-11, respectively). Dependent variables are a dummy equal to one if child does not obtain test scores or teacher assessments in grade 9 examinations (Panel A), a dummy equal to one if child obtains scores or assessments in both Danish and mathematics (Panel B), or a dummy equal to one if child completes grade 9 examinations by age 16 (Panel C). See Section 5 for details.

Table 4: Impacts of parental plant closure on math achievement

	Teacher grade in mathematics			Test score in mathematics		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Average impacts across childhood			
Treated	-0.0094	0.0021	0.0051	-0.0165*	-0.0048	-0.0023
	(0.0098)	(0.0092)	(0.0094)	(0.0099)	(0.0093)	(0.0094)
Treated X Exposure (age 0-16)	-0.0155	-0.0206**	-0.0226**	-0.0064	-0.0117	-0.0149
	(0.0107)	(0.0100)	(0.0101)	(0.0107)	(0.0101)	(0.0101)
			Panel B. Impacts by childhood stage			
Treated	-0.0094	0.0021	0.0051	-0.0165*	-0.0048	-0.0023
	(0.0098)	(0.0092)	(0.0094)	(0.0099)	(0.0093)	(0.0094)
Treated X Infant (0-1 years)	-0.0409**	-0.0521***	-0.0575***	-0.0295*	-0.0413***	-0.0488***
	(0.0163)	(0.0154)	(0.0155)	(0.0164)	(0.0155)	(0.0155)
Treated X Early childhood (2-5 years)	-0.0095	-0.0184	-0.0226*	-0.0027	-0.0118	-0.0181
	(0.0133)	(0.0125)	(0.0126)	(0.0134)	(0.0126)	(0.0126)
Treated X Mid childhood (6-11 years)	-0.0031	-0.0073	-0.0085	0.0045	0.0007	-0.0017
	(0.0122)	(0.0115)	(0.0115)	(0.0123)	(0.0115)	(0.0115)
Treated X Late childhood (12-16 years)	-0.0268**	-0.0274**	-0.0277**	-0.0150	-0.0164	-0.0170
	(0.0126)	(0.0118)	(0.0118)	(0.0126)	(0.0118)	(0.0118)
Constant	0.1027***	0.0209	0.0094	0.1038***	0.0295	0.0201
	(0.0029)	(0.0208)	(0.0196)	(0.0029)	(0.0192)	(0.0180)
N	240,150	240,150	240,148	239,326	239,326	239,324
Year of birth FEs	X	X	X	X	X	X
Year of shock FEs	X	X	X	X	X	X
Parental characteristics		X	X	X	X	X
Children characteristics		X	X	X	X	X
Industry and municipality FEs			X			

Note. The table shows estimates of the impacts of parental plant closure on child's grade 9 achievement in mathematics. Estimates and specifications are analogous to Table 2. Dependent variables are teacher assessments (columns 1-3) or test scores (columns 4-6). The sample considered is formed by children obtaining scores or assessments. See Section 5 for details.

Table 5: Heterogeneous impacts of parental plant closure on math achievement

	Full sample	Family income		Parental education		Parental tenure		Parent's gender		Child's gender	
		Below median	Above median	Below median	Above median	Below median	Above median	Mother	Father	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					Panel A. Teacher grade in mathematics						
Treated	0.0021	-0.0019	-0.0035	0.0005	-0.0016	0.0161	-0.0110	0.0194	-0.0104	-0.0077	0.0115
	(0.0092)	(0.0151)	(0.0105)	(0.0103)	(0.0164)	(0.0131)	(0.0118)	(0.0141)	(0.0111)	(0.0122)	(0.0124)
Treated X Infant (0-1 years)	-0.0521***	-0.0555***	-0.0390	-0.0557***	-0.0346	-0.0892***	0.0013	-0.0709***	-0.0401**	-0.0535**	-0.0501**
	(0.0154)	(0.0213)	(0.0249)	(0.0180)	(0.0278)	(0.0202)	(0.0238)	(0.0252)	(0.0189)	(0.0210)	(0.0217)
Treated X Early childhood (2-5 years)	-0.0184	-0.0110	-0.0234	-0.0280*	0.0126	-0.0350**	-0.0030	-0.0507**	-0.0001	-0.0033	-0.0333*
	(0.0125)	(0.0186)	(0.0170)	(0.0144)	(0.0223)	(0.0171)	(0.0175)	(0.0206)	(0.0150)	(0.0169)	(0.0173)
Treated X Mid childhood (6-11 years)	-0.0073	-0.0216	0.0052	-0.0005	-0.0185	-0.0256	0.0097	-0.0355**	0.0103	-0.0030	-0.0112
	(0.0115)	(0.0181)	(0.0138)	(0.0131)	(0.0202)	(0.0159)	(0.0154)	(0.0181)	(0.0139)	(0.0153)	(0.0157)
Treated X Late childhood (12-16 years)	-0.0274**	-0.0349*	-0.0203	-0.0271**	-0.0214	-0.0346**	-0.0226	-0.0540***	-0.0116	-0.0075	-0.0467***
	(0.0118)	(0.0198)	(0.0141)	(0.0138)	(0.0214)	(0.0170)	(0.0159)	(0.0188)	(0.0147)	(0.0161)	(0.0166)
N	240,150	115,262	124,888	173,542	66,608	128,964	111,186	82,675	157,475	118,875	121,275
					Panel B. Test score in mathematics						
Treated	-0.0048	-0.0135	-0.0074	-0.0028	-0.0182	0.0045	-0.0136	-0.0049	-0.0063	-0.0122	0.0019
	(0.0093)	(0.0152)	(0.0105)	(0.0103)	(0.0165)	(0.0131)	(0.0118)	(0.0141)	(0.0111)	(0.0124)	(0.0123)
Treated X Infant (0-1 years)	-0.0413***	-0.0422**	-0.0263	-0.0467***	-0.0177	-0.0708***	0.0026	-0.0383	-0.0426**	-0.0419**	-0.0399*
	(0.0155)	(0.0213)	(0.0249)	(0.0180)	(0.0279)	(0.0202)	(0.0239)	(0.0253)	(0.0189)	(0.0213)	(0.0215)
Treated X Early childhood (2-5 years)	-0.0118	0.0050	-0.0284*	-0.0245*	0.0291	-0.0309*	0.0098	-0.0119	-0.0108	-0.0065	-0.0161
	(0.0126)	(0.0187)	(0.0170)	(0.0145)	(0.0223)	(0.0171)	(0.0176)	(0.0207)	(0.0151)	(0.0171)	(0.0172)
Treated X Mid childhood (6-11 years)	0.0007	-0.0068	0.0089	0.0061	-0.0050	-0.0108	0.0110	-0.0014	0.0028	0.0030	-0.0015
	(0.0115)	(0.0182)	(0.0138)	(0.0131)	(0.0203)	(0.0159)	(0.0154)	(0.0182)	(0.0139)	(0.0156)	(0.0156)
Treated X Late childhood (12-16 years)	-0.0164	-0.0219	-0.0103	-0.0242*	0.0121	-0.0162	-0.0190	-0.0280	-0.0099	-0.0013	-0.0304*
	(0.0118)	(0.0198)	(0.0141)	(0.0138)	(0.0215)	(0.0170)	(0.0159)	(0.0189)	(0.0147)	(0.0164)	(0.0164)
N	239,326	114,730	124,596	172,801	66,525	128,469	110,857	82,461	156,865	118,334	120,992
Year of birth FEs	X	X	X	X	X	X	X	X	X	X	X
Year of shock FEs	X	X	X	X	X	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X	X	X	X	X	X

Note. The table shows estimates of the heterogeneous impacts of parental plant closure on child's grade 9 achievement in mathematics. Estimates and specifications are anlogous to columns (2) and (5) of Table 4, replicated here in column (1) to ease comparison. Following columns restrict estimation to children subgroups: children with family income below or above-median 3 years before parental plant closure (columns 2-3, respectively), children with parental education below or above median (columns 4-5, respectively), children with parental tenure below or above median in the base year (columns 6-7, respectively), children experiencing maternal or parental job displacement (columns 8-9, respectively), male or female chldren (columns 10-11, respectively). Dependent variables are teacher assessments (Panel A), or test scores (Panel B). See Section 5 for details.

Table 6: Impacts of parental plant closure on school enrolment at age 17

	All	Famliy income		Parent's gender		Own gender	
		Below median	Above median	Mother	Father	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A. Enolred in high school (academic or commercial)				
Treated	-0.0058	0.0001	-0.0146**	-0.0001	-0.0100	-0.0068	-0.0049
	(0.0050)	(0.0083)	(0.0057)	(0.0078)	(0.0061)	(0.0066)	(0.0067)
Treated X Infant (0-1 years)	-0.0065	-0.0196*	0.0154	-0.0058	-0.0059	-0.0091	-0.0034
	(0.0080)	(0.0112)	(0.0127)	(0.0132)	(0.0098)	(0.0107)	(0.0111)
Treated X Early childhood (2-5 years)	0.0060	-0.0061	0.0230***	-0.0056	0.0123	0.0061	0.0059
	(0.0066)	(0.0099)	(0.0089)	(0.0110)	(0.0079)	(0.0088)	(0.0090)
Treated X Mid childhood (6-11 years)	0.0045	-0.0089	0.0162**	-0.0057	0.0109	0.0035	0.0058
	(0.0060)	(0.0097)	(0.0073)	(0.0097)	(0.0074)	(0.0080)	(0.0082)
Treated X Late childhood (12-16 years)	0.0024	-0.0046	0.0089	-0.0060	0.0075	0.0055	-0.0006
	(0.0060)	(0.0101)	(0.0071)	(0.0097)	(0.0075)	(0.0081)	(0.0084)
Constant	0.5702***	0.4412***	0.6854***	0.5498***	0.5331***	0.6597***	0.4836***
	(0.0090)	(0.0091)	(0.0037)	(0.0041)	(0.0029)	(0.0041)	(0.0040)
			Panel B. Enroled in vocational track				
Treated	0.0063	0.0096	0.0057	0.0136**	0.0024	0.0045	0.0081
	(0.0042)	(0.0069)	(0.0042)	(0.0061)	(0.0048)	(0.0046)	(0.0058)
Treated X Infant (0-1 years)	-0.0092	-0.0069	-0.0196**	-0.0180*	-0.0044	-0.0097	-0.0095
	(0.0064)	(0.0094)	(0.0093)	(0.0104)	(0.0077)	(0.0075)	(0.0095)
Treated X Early childhood (2-5 years)	-0.0025	-0.0035	-0.0055	-0.0069	0.0005	0.0036	-0.0083
	(0.0053)	(0.0083)	(0.0065)	(0.0087)	(0.0062)	(0.0061)	(0.0077)
Treated X Mid childhood (6-11 years)	-0.0085*	-0.0095	-0.0092*	-0.0069	-0.0092	-0.0038	-0.0134*
	(0.0049)	(0.0081)	(0.0053)	(0.0076)	(0.0058)	(0.0056)	(0.0071)
Treated X Late childhood (12-16 years)	-0.0041	-0.0092	-0.0011	-0.0052	-0.0034	-0.0003	-0.0077
	(0.0050)	(0.0085)	(0.0052)	(0.0076)	(0.0059)	(0.0056)	(0.0072)
Constant	0.1868***	0.1854***	0.1355***	0.1932***	0.1986***	0.1233***	0.2464***
	(0.0039)	(0.0077)	(0.0027)	(0.0033)	(0.0023)	(0.0029)	(0.0034)
			Panel C. Not in high school or vocational track				
Treated	-0.0005	-0.0097	0.0090*	-0.0133*	0.0076	0.0023	-0.0031
	(0.0043)	(0.0075)	(0.0049)	(0.0068)	(0.0054)	(0.0058)	(0.0061)
Treated X Infant (0-1 years)	0.0157**	0.0265***	0.0041	0.0237**	0.0103	0.0187**	0.0127
	(0.0070)	(0.0102)	(0.0108)	(0.0115)	(0.0087)	(0.0094)	(0.0101)
Treated X Early childhood (2-5 years)	-0.0035	0.0095	-0.0176**	0.0124	-0.0128*	-0.0097	0.0023
	(0.0057)	(0.0091)	(0.0076)	(0.0096)	(0.0071)	(0.0077)	(0.0082)
Treated X Mid childhood (6-11 years)	0.0041	0.0183**	-0.0070	0.0125	-0.0017	0.0003	0.0075
	(0.0053)	(0.0088)	(0.0062)	(0.0085)	(0.0065)	(0.0071)	(0.0075)
Treated X Late childhood (12-16 years)	0.0017	0.0139	-0.0079	0.0112	-0.0041	-0.0052	0.0083
	(0.0053)	(0.0092)	(0.0061)	(0.0084)	(0.0067)	(0.0071)	(0.0076)
Constant	0.2430***	0.3734***	0.1790***	0.2570***	0.2683***	0.2170***	0.2700***
	(0.0057)	(0.0083)	(0.0031)	(0.0036)	(0.0026)	(0.0037)	(0.0036)
N	250,896	124,180	126,716	85,604	165,292	121,892	129,004
Age at closure dummies	X	X	X	X	X	X	X
Year of birth dummies	X	X	X	X	X	X	X
Year of shock dummies	X	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X	X

Note. The table shows estimates of the impacts of parental plant closure on child's educational status in the academic year beginning when child turns 17. Estimates and specifications in column (1) are analogous to column (2) of Table 2. Reported are estimates of δ_1 and δ_s coefficients from Equation (3). Following columns restrict estimation to children subgroups: children with family income below or above-median 3 years before parental plant closure (columns 2-3, respectively), children experiencing maternal or parental job displacement (columns 4-5, respectively), male or female chldren (columns 6-7, respectively). Dependent variables are indicators equal to one if child is enroled in academic or commercial high school (Panel A), basic or practical commercial school (Panel B), or child is either still in compulsory school, out of education, or in prep school (Panel C). See Section 5 for details.

Table 7: Main earner analysis

	All displaced parents		Father displaced		Mother displaced	
	Not main earner	Main earner	Not main earner	Main earner	Not main earner	Main earner
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Teacher grade in mathematics			
Treated	-0.0180	0.0144	-0.0375	-0.0024	-0.0059	0.0677***
	(0.0144)	(0.0112)	(0.0234)	(0.0127)	(0.0182)	(0.0231)
Treated X Infant (0-1 years)	-0.0018	-0.0760***	0.0141	-0.0485**	-0.0133	-0.1957***
	(0.0259)	(0.0188)	(0.0502)	(0.0208)	(0.0306)	(0.0458)
Treated X Early childhood (2-5 years)	-0.0194	-0.0230	0.0327	-0.0098	-0.0444*	-0.0508
	(0.0209)	(0.0150)	(0.0376)	(0.0167)	(0.0254)	(0.0368)
Treated X Mid childhood (6-11 years)	0.0116	-0.0190	0.0590*	-0.0017	-0.0159	-0.0742**
	(0.0184)	(0.0139)	(0.0310)	(0.0156)	(0.0229)	(0.0305)
Treated X Late childhood (12-16 years)	-0.0188	-0.0342**	0.0189	-0.0198	-0.0427*	-0.0801**
	(0.0192)	(0.0147)	(0.0316)	(0.0167)	(0.0242)	(0.0311)
Constant	0.0624***	-0.0008	-0.0701***	-0.0392***	0.0742***	-0.1310***
	(0.0095)	(0.0079)	(0.0185)	(0.0065)	(0.0103)	(0.0226)
N	80,422	157,406	26,883	129,586	53,539	27,820
			Panel B. Test score in mathematics			
Treated	-0.0272*	0.0069	-0.0339	0.0011	-0.0229	0.0241
	(0.0144)	(0.0112)	(0.0234)	(0.0127)	(0.0183)	(0.0231)
Treated X Infant (0-1 years)	0.0103	-0.0640***	0.0238	-0.0524**	0.0032	-0.1173**
	(0.0260)	(0.0189)	(0.0503)	(0.0208)	(0.0307)	(0.0460)
Treated X Early childhood (2-5 years)	-0.0086	-0.0167	0.0224	-0.0199	-0.0230	0.0298
	(0.0210)	(0.0151)	(0.0376)	(0.0167)	(0.0255)	(0.0369)
Treated X Mid childhood (6-11 years)	0.0237	-0.0119	0.0468	-0.0074	0.0100	-0.0231
	(0.0184)	(0.0139)	(0.0309)	(0.0156)	(0.0230)	(0.0305)
Treated X Late childhood (12-16 years)	-0.0097	-0.0190	0.0065	-0.0129	-0.0202	-0.0399
	(0.0192)	(0.0147)	(0.0316)	(0.0167)	(0.0243)	(0.0312)
Constant	0.0777***	0.0056	-0.0738***	-0.0330***	0.0925***	-0.1342***
	(0.0096)	(0.0079)	(0.0185)	(0.0065)	(0.0103)	(0.0226)
N	80,208	156,806	26,821	129,046	53,387	27,760
Age at closure dummies	X	X	X	X	X	X
Year of birth dummies	X	X	X	X	X	X
Year of shock dummies	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X

Note. The table shows estimates of the heterogeneous impacts of parental plant closure on child's grade 9 achievement in mathematics. Estimates and specifications are anlogous to columns (2) and (5) of Table 4, replicated here in column (1) to ease comparison. Following columns restrict estimation to children whose displaced parent is the secondary or the main earner among parents (columns 2 and 3, respectively). The same sample restriction is considered among paternal (columns 4-5) or maternal (columns 6-7) displacements only. Dependent variables are teacher assessments (Panel A), or test scores (Panel B). See Section 6 for details.

Appendix (for on-line publication only)

A Definition of plant closures

We use plant closures as treatment variable in our analysis to identify parental job displacement. We detail here the approach we follow to define plant closures, which is borrowed from Browning and Heinesen (2012).

First, we consider a couple of consecutive years (t and $t+1$), and flag plants which firm does not change in this time interval ("same owner" plants). We similarly define plants which 2-digit industry code does not change ("same industry" plants) and plants which municipality does not change ("same municipality" plants) between t and $t+1$. Second, we link plants to job spells observed in t and $t+1$ and define the share of employees that are retained in the same plant, as well as those who are employed at a newly-established plant. We repeat this procedure for each couple of consecutive years between 1980 and 2016, the time span of plant data we observe.

We consider plant-level data in year t, which we call the base year. A plant is not considered closed between t and $t+1$ if one of the following events is recorded:

- The plant has the same owner and the same industry in $t+1$.
- The plant has the same owner and the same employees. The latter condition is defined here as retaining at least the 30% of employees *either* with respect to the base year or to the subsequent year.
- The plant has the same industry and the same employees, or the same municipality and the same employees. The latter condition is defined as retaining at least the 30% of employees *both* with respect to the base year or to the subsequent year.
- At least 40% of workers are re-employed at a newly established plant.

In any case, if an active plant in year t records no employees in year $t+1$ it is considered closed.

When a closure is recorded, we define the year of closure based on the employment downsize observed. First, we employ sample restrictions to capture meaningful events. We only consider closures involving a downsize of at least 30% of employees and at least 3 workers. Moreover, we consider plants with at least 5 employees 5 years before closing down. The latter restriction excludes nearly 68% of closures. Second, when multiple years meet the closure definition for the same plant, we consider the year of maximum employment downsize. Third, once a unique year of closure is identified for all closing plants, we define the event year as the one in which the highest downsize is recorded among the three periods preceding a closure. This step addresses the possibility that employment downsize starts earlier than the actual closure. The event year is the exact year of closure in the 92% of plants.

B Additional results on parental outcomes

We present here detailed heterogeneity analysis of the impacts of plant closure on parental outcomes, sketched in Section 5.

Maternal labor market outcomes especially worsen after plant closure. Unemployment shows the most striking gender gap, increasing by nine p.p. in the closure year for mothers and seven p.p. for fathers, and then declining in parallel for mothers and fathers (Figure B.1, Panel C). Unemployment spells for treated mothers spike in the closure year by 50 percent more than for fathers, implying that mothers also remain unemployed longer (Panel D). The likelihood of positive earnings, however, does not differ by gender, suggesting that mothers' labor market attachment becomes less stable (Panel B). Mothers also suffer greater earnings losses in the first years after plant closure (nine percent compared with six percent for fathers, Panel A). Despite these gaps, both individual and family post-tax income exhibit larger drops after paternal compared to maternal displacement (four and three p.p. compared with two and one p.p. for mothers, respectively, Panels E and F), likely reflecting gender wage gaps.

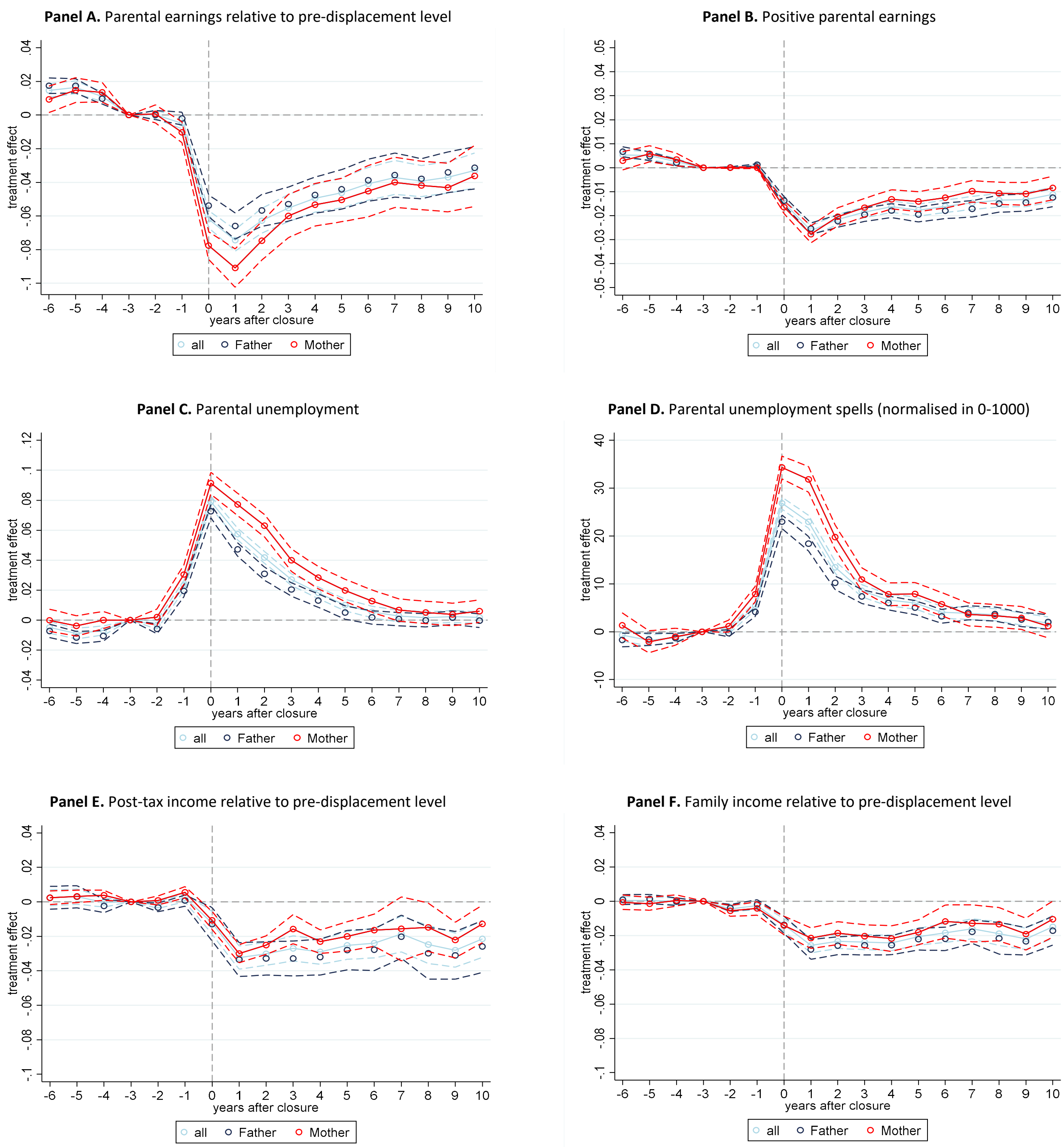
Labor market impacts of plant closure also differ by family income. Figure B.2 plots event study estimates around plant closure separately for children with above or below-median family income in $t^* - 2$. Displaced parent's unemployment jumps by 10 p.p. in the plant closure year in low-income families, compared with six p.p. for high-income families (Panel C). The initial drop in earnings is slightly larger in low-income families but this gap reverses two years after plant closure (Panel A). Faster recovery of parental earnings for low-income children may reflect a greater pressure for financially constrained parents to get back to work. Individual and family income decreases more markedly in high-income families, perhaps reflecting the fact that these parents have more income to lose (Panels E and F).

Parents of younger children suffer smaller income losses from plant closure. Figure B.3 plots event study estimates from Equation (4) separately by childhood stage. While earnings losses peak at five percent of the pre-displacement level among parents displaced during child's infancy, the peak increases to levels close to 10 percent for parents with adolescent or adult children (Panel A of Figure B.3). The most impressive difference is the pace of earnings recovery after the initial loss, with parents of infants returning to pre-displacement earnings in the fifth year after plant closure while parents of older children suffer a permanent penalty of around five percent up to 10 years after plant closure. Differences in earning losses are reflected in similarly larger post-tax income losses among parents of older children (Panels E and F). These results are in line with findings in Salvanes et al. (2021).

Parents of younger children, however, suffer greater employment shocks in the short run. Displaced parents of infants are four p.p. more likely to record no labor earnings in the year after plant closure, twice the effect estimated among parents of adolescents (Panel B of Figure B.3). While parents of younger children exhibit slightly lower short-term increases in the probability of receiving unemployment insurance benefits, an outcome possibly constrained by eligibility among younger workers, the pace of recovery is considerably slower than that we estimate for parents with older children (Panel C). Consistently, parents of infants suffer the largest increase in the length of unemployment spells (Panel D).

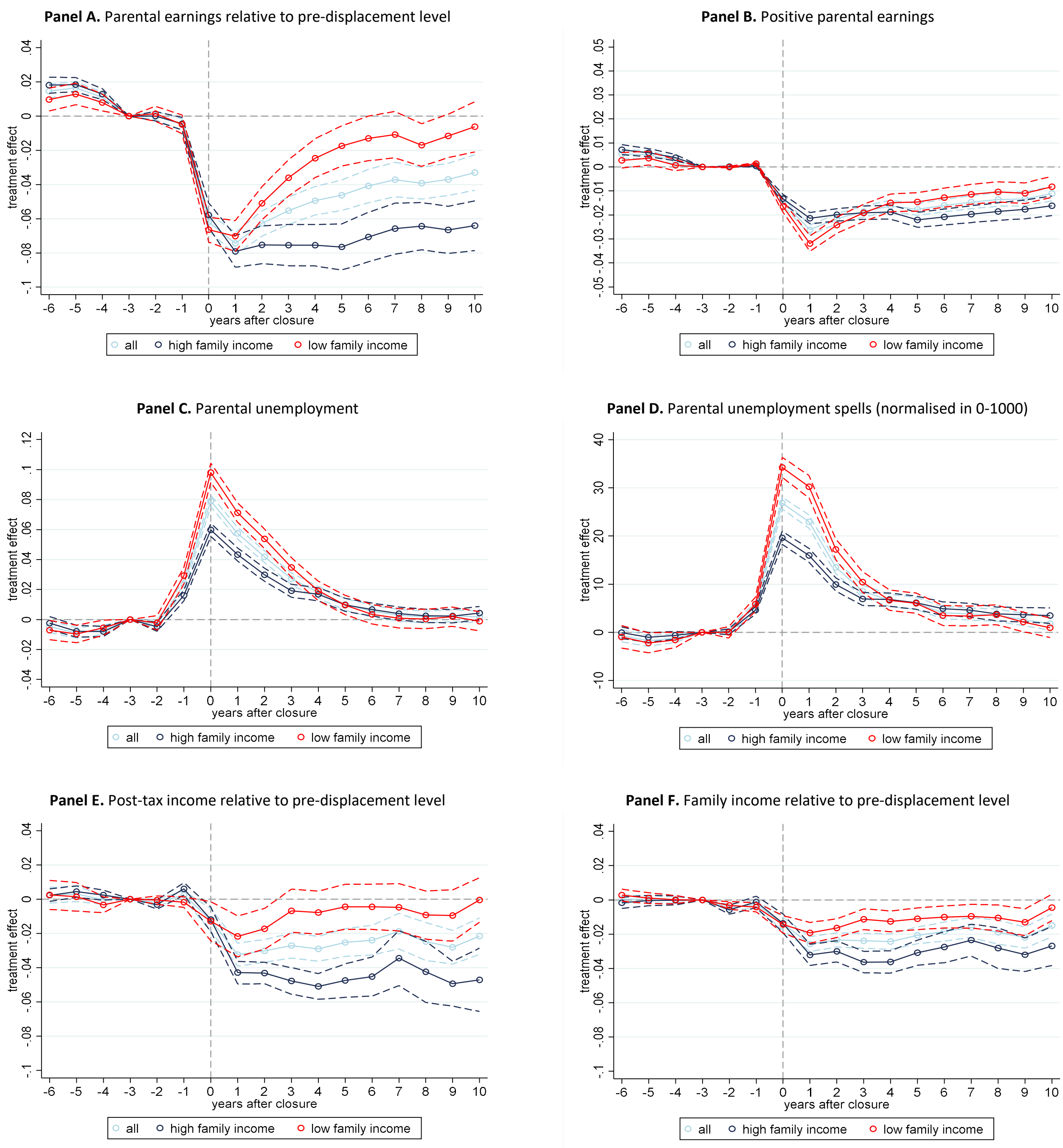
Paternal job losses cause larger income losses than maternal displacements only among parents of relatively older children. Separate estimates of parental event studies by parent's gender and childhood stage are plotted in Figure B.4. Displaced mothers of infants have dramatically longer unemployment spells after job loss, almost fivefold in the year after plant closure (Panel A). This gender gap declines with child's age, closing among parents of adolescents. Consistently, only among parents of infants, earnings loss after plant closure is more severe for mothers (Panel B). As a result, family income drops by similar magnitudes for displaced mothers and fathers with younger children (Panel C).

Figure B.1: Plant closure impacts on parental outcomes by gender



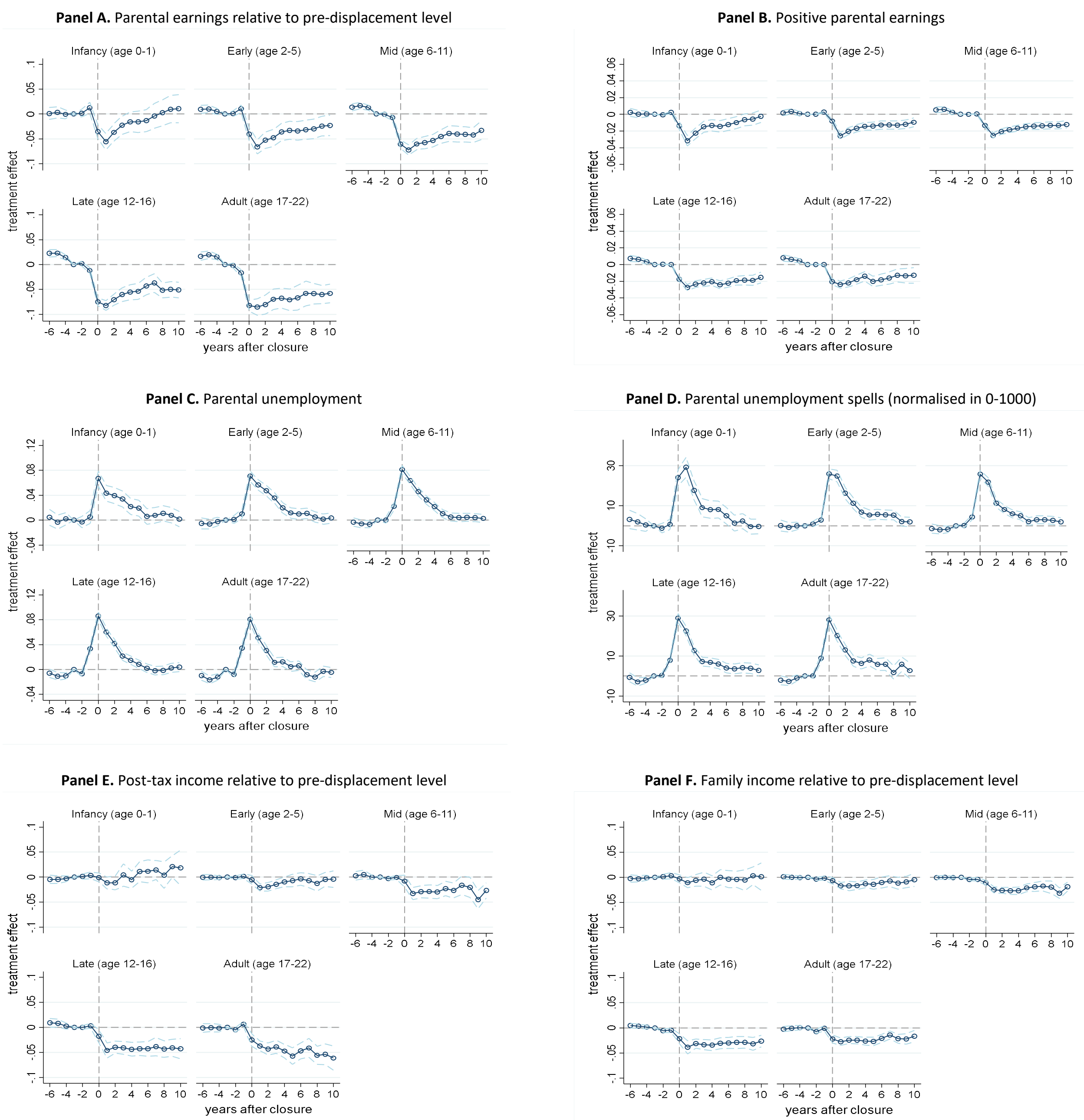
Note. The figure shows event study estimates of the impact of plant closure on parent's outcomes by parent's gender. Plotted are estimates of from coefficients α_l in equation (4) seprately by gender, with specifications similar to Figure 1. Panel F additionally plots absolute family income in thousands of 2020 DKK. See Section 5 for details.

Figure B.2: Plant closure impacts on parental outcomes by family income



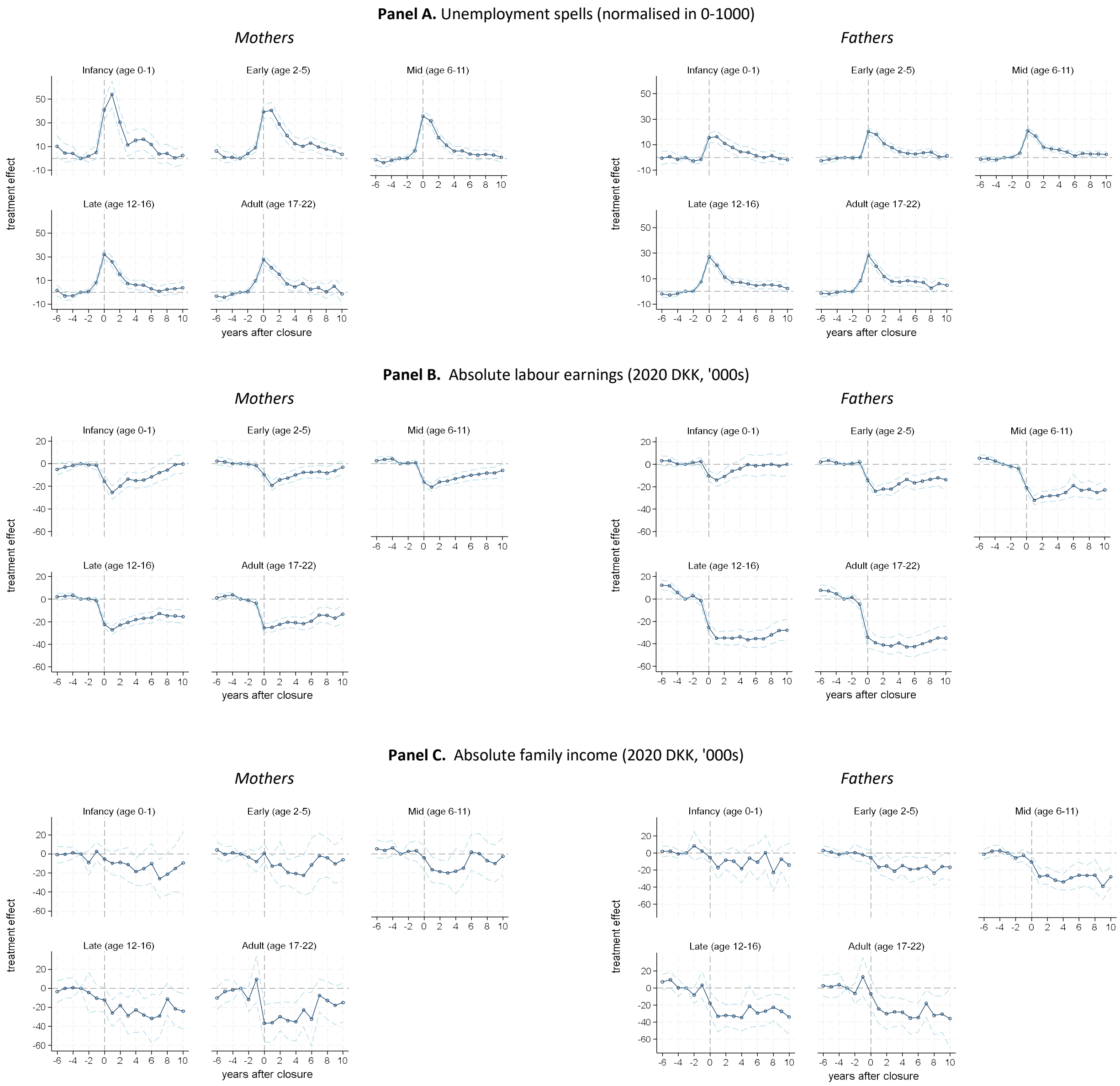
Note. The figure shows event study estimates of the impact of plant closure on parent's outcomes by pre-displacement family income. Plotted are estimates of coefficients α_l in equation (4) seprately for parents with baseline family income above or below median, with specifications similar to Figure 1. See Section 5 for details.

Figure B.3: Plant closure impacts on parental outcomes by childhood stage



Note. The figure shows event study estimates of the impact of plant closure on parent's outcomes. Plotted are estimates of coefficients α_l in (4). Estimates and specifications are analogous to Figure 1, seprately estimated for children in infancy (age 0-1), early childhood (age 2-5), mid childhood (age 6-11), adolescence (age 12-16) or adulthood (age 17-22) at parental plant closure. See Section 6 for details.

Figure B.4: Plant closure impacts on parental outcomes by childhood stage and parent's gender



Note. The figure shows event study estimates of the impact of plant closure on parent's outcomes. Plotted are estimates of coefficients α_l in equation (4), following specification and structure of Figure 1. Panel A considers yearly unemployment spells normalised in the (0,1000) interval, Panel B considers absolute labor earnings in thousands of 2020 DKK, while corresponding amounts of pfamily income are considered in Panel C. Within each panel, left-hand graphs consider maternal job displacements only, while right-hand graphs consider paternal job displacements only. See Section 6 for details.

C Robustness checks

We provide evidence that our results presented in Section 5 are not driven by selection into parenthood around plant closure. Results discussed so far have shown that we cannot reject null estimates when looking at children older than school leaving age at parental plant closure, and that our findings are robust to the inclusion of detailed controls such as industry and municipality dummies. We turn here to the concern that our stronger negative results among children exposed in infancy are driven by negative selection into parenthood around plant closure. Our treatment group of infants includes, indeed, children born in the closure year ($a(i) = 0$), who were potentially not yet conceived at job loss.[1]

First, for fertility decisions to be strategic, plant closures would need to be anticipated by the potential parent. We show in columns (1)-(7) of Table C.1 that treatment effects on teacher assessments in mathematics are not driven by plant closures which are likely more predictable as suggested by plant data. Columns (1)-(2) replicate the specification in Panel B, column (2) of Table 2, separately by treatment-control pairs where the treated child's plant experienced one single versus multiple plant closures (only eight percent of plants meet the closure definition more than once). Despite negative results being stronger for multiple-closure plants, they are similar to the main results for single-closure plants, where eventual closure is less predictable. Columns (3)-(4) consider plants recording large versus small downsizes between the base year and the closure year (all employees, and at least 10, compared to smaller downsizes). Sudden closure such as the former are likely harder to anticipate. The coefficient on children exposed in infancy, those potentially subject to selection into parenthood, is very similar across the two groups.

Columns (5)-(6) separate between plants recording a substantial downsize also in the year before closure (between $t^* - 1$ and t^*, at least 10 percent of the workforce and three employees) versus plants that don't, with the latter closures likely harder to anticipate. The coefficient on children exposed in infancy is lower in the latter case, suggesting once again that results are not stronger when considering more predictable closures. In column (7), we exclude from the sample the small number of treated-control pairs in which control child's parent has left a closing plant within the previous 5 years (3% of the estimation sample), and confirm that results remain

[1] This scenario would materialise if children were born late in the year and closure happened in the first months of the year.

similar.

Second, we vary our sample selection by age at closure to show that selection into parenthood does not drive our results. Column (7) shows that the result for infancy are robust to the exclusion of children born in the year of closure (now the coefficient is solely identified by one-year olds at closure). Column (8) adds children born in the year after closure (age "-1" at closure), and estimates a separate treatment effect for these individuals. Estimates are close to zero and not significant for both teacher grade (Panel A) and test score (Panel B). The result that children born after closure exhibit weaker impacts on achievement suggest that, if anything, there is positive selection into parenthood, e.g., only workers with better income prospects decide to have children despite plant closure.

Third, plant closure may be anticipated if other plants in the same local labor market are closing. We compute for each closed plant the leave-one-out share of plants closed in the same industry and municipality over the previous years. Columns (9)-(10) separate between plants with or without closures in the same local labor market, while columns (11)-(12) separate between plants where closure share in the local labor market is above or below the median among plants with some closures. In both cases, estimated effects are worse among children exposed to plant closures in declining local labor markets. However, these differences by local labor markets are similar regardless child's age at plant closure, suggesting that selection into parenthood is not driving this result.

Overall, considering more versus less predictable closures, varying age groups definition around infancy, and considering patterns of closures in the local labor market, we find little evidence for larger parental job loss impacts on child's test scores when closure hits during infancy can be explained by selection into parenthood, and by anticipation of closures more generally.[2]

[2]We show the same robustness checks on test-taking in Table (C.4). Results have similar patterns, with the exception of a significant negative impacts on test-taking among children born in the year after closure (in-utero at closure, column 7). However, the estimate is similar in magnitude to children exposed to closure during infancy, suggesting little selection into parenthood.

Table C.1: Robustness checks: impacts of parental job displacement on achievement

	Same plant's closures		Downsize in t*		Downsize in t*-1		Parent has not left a closing plant	Age at closure > 0	Include children in-utero at closure	Closures in local market		% of closures in local market	
	Single	Multiple	Small	Large	Small	Large				Yes	No	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. Teacher grade in mathematics													
Treated	0.0011	0.0088	-0.0156	0.0138	0.0158	-0.0286*	0.0004	0.0018	0.0023	0.0299**	-0.0301**	0.0456***	-0.0162
	(0.0097)	(0.0291)	(0.0147)	(0.0119)	(0.0112)	(0.0164)	(0.0093)	(0.0092)	(0.0092)	(0.0126)	(0.0136)	(0.0173)	(0.0109)
Treated X In-utero (age "-1")									0.0088				
									(0.0213)				
Treated X Infant (0-1 years)	-0.0456***	-0.1213**	-0.0519**	-0.0520***	-0.0636***	-0.0272	-0.0464***	-0.0641***	-0.0413***	-0.0664***	-0.0425*	-0.0533**	-0.0600***
	(0.0162)	(0.0522)	(0.0240)	(0.0202)	(0.0183)	(0.0288)	(0.0157)	(0.0193)	(0.0155)	(0.0201)	(0.0242)	(0.0266)	(0.0191)
Treated X Early childhood (2-5 years)	-0.0094	-0.1198***	-0.0289	-0.0104	-0.0190	-0.0221	-0.0182	-0.0182	-0.0119	-0.0376**	-0.0002	-0.0424*	-0.0126
	(0.0131)	(0.0422)	(0.0196)	(0.0163)	(0.0150)	(0.0229)	(0.0127)	(0.0125)	(0.0126)	(0.0166)	(0.0193)	(0.0221)	(0.0153)
Treated X Mid childhood (6-11 years)	-0.0086	0.0104	0.0012	-0.0138	-0.0185	0.0157	-0.0061	-0.0072	0.0006	-0.0083	-0.0095	-0.0242	-0.0028
	(0.0121)	(0.0371)	(0.0180)	(0.0149)	(0.0138)	(0.0208)	(0.0116)	(0.0115)	(0.0115)	(0.0155)	(0.0170)	(0.0211)	(0.0136)
Treated X Late childhood (12-16 years)	-0.0269**	-0.0295	-0.0405**	-0.0173	-0.0414***	0.0021	-0.0269**	-0.0274**	-0.0164	-0.0436***	-0.0089	-0.0422*	-0.0219
	(0.0124)	(0.0382)	(0.0186)	(0.0153)	(0.0143)	(0.0210)	(0.0119)	(0.0118)	(0.0118)	(0.0161)	(0.0174)	(0.0221)	(0.0140)
N	219,257	20,893	100,348	139,802	168,988	71,162	233,144	230,247	249,139	136,063	104,087	76,984	163,166
Panel B. Test score in mathematics													
Treated	-0.0053	-0.0005	-0.0231	0.0075	0.0082	-0.0340**	-0.0067	-0.0051	-0.0047	0.0197	-0.0329**	0.0434**	-0.0246**
	(0.0097)	(0.0294)	(0.0146)	(0.0120)	(0.0112)	(0.0164)	(0.0093)	(0.0093)	(0.0093)	(0.0126)	(0.0136)	(0.0174)	(0.0109)
Treated X In-utero (age "-1")									-0.0210				
									(0.0213)				
Treated X Infant (0-1 years)	-0.0359**	-0.1019**	-0.0347	-0.0467**	-0.0512***	-0.0201	-0.0352**	-0.0433**	-0.0521***	-0.0503**	-0.0388	-0.0336	-0.0562***
	(0.0163)	(0.0519)	(0.0241)	(0.0203)	(0.0184)	(0.0289)	(0.0158)	(0.0194)	(0.0154)	(0.0202)	(0.0245)	(0.0268)	(0.0192)
Treated X Early childhood (2-5 years)	-0.0039	-0.1025**	-0.0133	-0.0105	-0.0127	-0.0139	-0.0120	-0.0116	-0.0185	-0.0324*	0.0105	-0.0434*	-0.0028
	(0.0131)	(0.0426)	(0.0195)	(0.0164)	(0.0150)	(0.0230)	(0.0127)	(0.0126)	(0.0125)	(0.0166)	(0.0194)	(0.0222)	(0.0153)
Treated X Mid childhood (6-11 years)	-0.0019	0.0313	0.0069	-0.0039	-0.0062	0.0140	0.0024	0.0009	-0.0074	0.0016	-0.0031	-0.0225	0.0075
	(0.0121)	(0.0375)	(0.0180)	(0.0149)	(0.0138)	(0.0208)	(0.0116)	(0.0115)	(0.0115)	(0.0155)	(0.0171)	(0.0212)	(0.0137)
Treated X Late childhood (12-16 years)	-0.0168	-0.0111	-0.0326*	-0.0042	-0.0310**	0.0143	-0.0157	-0.0164	-0.0275**	-0.0319**	0.0017	-0.0358	-0.0093
	(0.0124)	(0.0384)	(0.0185)	(0.0153)	(0.0143)	(0.0211)	(0.0119)	(0.0118)	(0.0118)	(0.0161)	(0.0174)	(0.0221)	(0.0140)
N	218,499	20,827	100,052	139,274	168,405	70,921	232,383	229,463	248,278	135,634	103,692	76,770	162,556
Year of birth FEs	X	X	X	X	X	X	X	X	X	X	X	X	X
Year of shock FEs	X	X	X	X	X	X	X	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X	X	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X	X	X	X	X	X	X	X

Note. The table shows estimates of the impacts of parental plant closure on child's achievement at the end of compulsory school. Estimates and specifications are analogous to column (2) of Table 2. In columns (1) and (2), the sample is restricted to treated children whose parent's closed plant suffered a single closure or multiple closure, respectively, and their matched control peers. Similarly, we consider plants with relatively small or large (all employees leave, at least 10) downsize in the plant closure year in columns (3) and (4), respectively; plants with small or large (10% employees leave, at least 3) downsize in the year preceding plant closure year in columns (5) and (6), respectively. In column (7), we consider only treated-control pairs where control child's parents has not left a closing plant within 5 years (97% of the full sample). In column (8), we exclude children born in the plant closure year, while in column(9) we add to the full sample children born in the year after closure. In columns (10) and (11), we consider plants with and without other closures in own local market (municipality and 3-digit industry) in the previous 5 years, respectively. In columns (12) and (13), we consider plants with above or below-median share of closures in own local market in the previous 5 years, respectively (with the median computed among plants with at least one closure). Dependent variable is average standardised end-of-school teacher grade in mathematics. See Section 5 for details.

D Predicted impacts on parental labor market outcomes

We investigate potential mechanisms behind our treatment effects in Section 6 by presenting heterogeneity analyses based on predicted parental labor market impacts of parental job displacement. We detail here how these predicted impacts are estimated. We follow the procedure in Britto et al. (2022b) (see also Hilger, 2016 for a similar exercise).

First, we construct child-level DiD comparisons of parental and family outcomes using our treated-control pairs. For each treated child, we compute the difference between parental outcomes change after plant closure and the same change observed for their matched control peer. Specifically, we consider the 5-year average following displacement and subtract the value of parental outcome observed in $t^* - 2$, 3 years before plant closure. The difference in this change with respect to matched control peers represent an individual-level estimate of the impact of plant closure. We construct such comparisons for three different outcomes: parental labor earnings, family income, and parental unemployment spells. Individual-level estimates are substantially dispersed, as noted in Section 6.

Second, we predict child-level impacts of parental job displacement on parental labor market outcomes only using baseline child, parent, and family characteristics. We regress the individual-level estimated impacts described above on the number of siblings, birth order, parental age at displacement and at child's birth, years of schooling, tenure and plant size in t^*, earnings at displacement and in the two preceding years, industry and municipality of work dummies in t^*, and family income in in $t^* - 2$. The rationale of this procedure is that using estimated changes directly would raise endogeneity concerns since we would split the sample based on outcome variables.

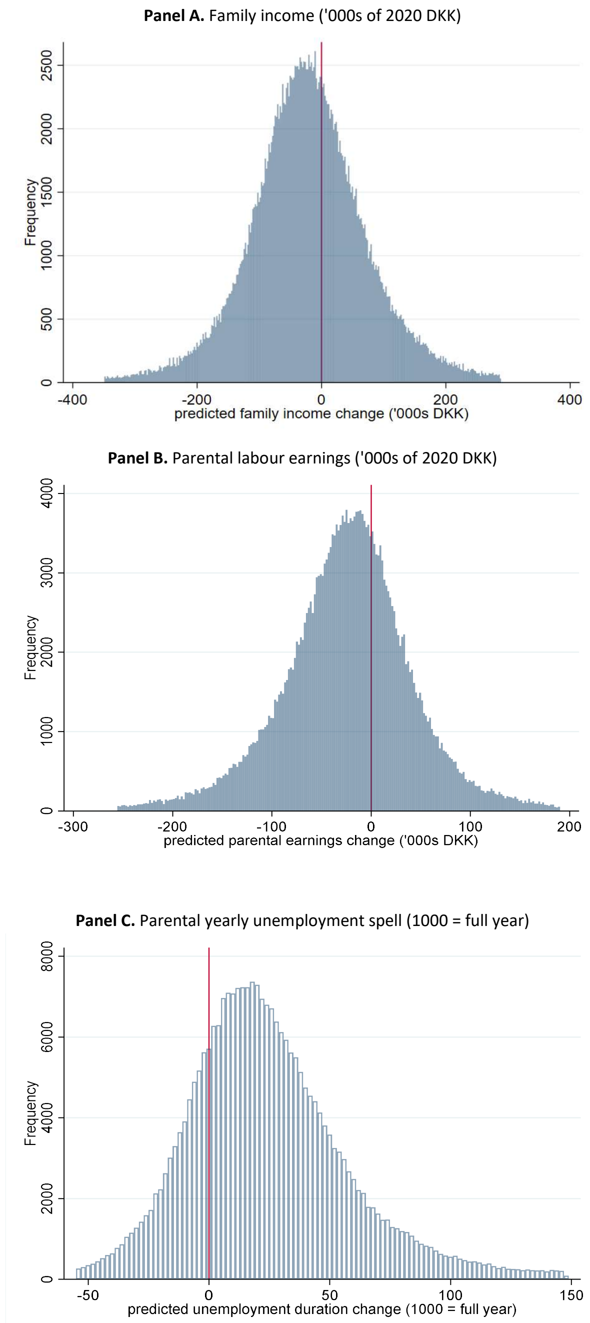
Predicted parental labor market impacts of plant closure are remarkably heterogeneous. The distributions of the predicted impacts we obtain are plotted in Figure D.1 and summarised in Table D.1. Panel A of both exhibits describe predicted impacts on family income. The average predicted impact is a loss of about 22,000 DKK, remarkably similar to parental event study estimates (see Figure 1). Although most probability density is attached to negative values, a non-negligible share of children are predicted to experience a family income *gain* as effect of parental plant closure. Similar considerations apply to parental labor earnings (Panel B of Figure D.1 and Table D.1). We offer some insights in Section 6 to rationalise these apparently surprising predictions. The predicted increase in parental unemployment spells is, on average, of 2.5% of working time each year (unemployment spell duration is normalised in a 0-1000 scale). This statistics reflects again marked heterogeneity, with a few children experiencing a lower change in parental unemployment around parental plant closure with respect to their control peers (Panel C of Figure D.1 and Table D.1). We note that predicted impacts have by construction the same mean values of individual-level estimates but are substantially less dispersed.

Third, we present heterogeneous effects of parental job displacement on child's education by subgroups defined based on predicted impacts on parental labor market outcomes. Results presented in Section 6 derive from separate estimation of equation (3) by quintiles of predicted changes in parental labor earnings, family income, and parental unemployment spells. Table D.1 reports statistics on predicted changes by quintile and parental outcome. For example, Panel A shows that we predict an average family income loss of 174,702 DKK in the bottom quintile, a milder loss in the second quintile (67,525 DKK), small negative or positive losses in the third and fourth quartile, respectively, while we predict family income gains in the top quintile (on average, 130,836 DKK). Note that, in the case of length of unemployment spell (Panel C), top quintiles are associated with *worse* parental labor market outcomes.

Heterogeneity analyses by predicted parental outcome changes capture a combination of parental and family characteristics associated with the consequences of parental job displacement. We report in Table D.2 key parental characteristics by quintiles of predicted changes in parental labor earnings, family income, and parental unemployment spells. Larger drops in family income (Panel A) and parental earnings (Panel B) are predicted for parents losing a relatively solid job due to plant closure. Displaced parents of children in the bottom quintile are more likely fathers, are older at the time of displacement, have larger earnings and family income, and work in larger plants with longer tenure. Interestingly, these job characteristics monotonically worsen in upper quintiles apart for the top quintile. In the latter cell, where we predict average *gains* from plant closure, we find on average the most educated parents, with larger family income and earnings with respect to peers in second to fourth quintiles. Overall, statistics suggest that larger financial losses from job displacement are predicted for relatively less educated parents who were anyway enjoying a relatively high-quality job, while more educated parents with similarly good jobs and much to lose from plant closure manage to find an even better alternative on the market.

In the last part of Section 6, we combine predicted changes in family income and parental unemployment spell. We observe substantial variation in predicted family income loss conditional on similar predicted changes in unemployment spells, and vice versa. Table D.3 reports the joint distribution of children across terciles of predicted family income change and predicted unemployment spell change after parental job loss. While it is less likely to predict large unemployment increase among parents with relatively low predicted income loss, and vice versa, the joint distribution is not sparse. Heterogeneity analyses in Figure 5 and C.7 are based on subsets of this joint distribution. Finally, we report in Table D.4 parental characteristics associated with the subgroups considered in the analysis. Patterns of parental characteristics by joint predicted impacts on parental income and unemployment are similar to what described above (see Table D.2).

Figure D.1: Distribution of predicted impacts of plant closure on parental outcomes



Note. The figure shows the distribution of predicted parental job displacement impacts on family income (Panel A), parental labor market earnings (Panel B), and parental unemployment spells (Panel C). Impacts are first estimated at the child level as DiD comparisons between each treated unit and the matched control peer. Impacts are then regressed on a detailed set of parental, child, and family characteristics and fitted values from the regressions are used to compute predicted impacts. In each panel, outlier observations below the 1st or above the 99th percentile are not considered. The red vertical lines indicate a null predicted impact. See Section 6 and Appendix D for details.

Table D.1: Quintiles of predicted impacts of plant closure on parental outcomes

Quintile of predicted impact (1)	Mean (2)	Median (3)
Panel A. Family income ('000s of 2020 DKK)		
First	-174.702	-139.138
Second	-67.525	-66.805
Third	-22.981	-23.046
Fourth	23.112	22.118
Fifth	130.836	100.822
TOTAL	-22.253	-23.046
Panel B. Parental labour earnings ('000s of 2020 DKK)		
First	-132.582	-109.095
Second	-53.512	-52.823
Third	-21.989	-21.963
Fourth	7.728	7.226
Fifth	78.124	56.154
TOTAL	-24.447	-21.963
Panel C. Parental yearly unemployment spell (1000 = full year)		
First	-20.955	-14.959
Second	5.676	5.888
Third	20.572	20.447
Fourth	37.705	37.241
Fifth	79.885	69.414
TOTAL	24.576	20.446

Note. The table shows predicted impacts of parental job displacement by quintile. Panel A, Panel B, and Panel C report predicted impacts on family income, parental earnings, and parental unemployment recipience, respectively. Unemployment is measured as the proportion of time in a year on unemployment benefits, nomalised in the (0,1000) interval. Impacts are predicted by regressing individual gains or losses in the considered outcomes on parental and child baseline characteristics. Individual gains or losses are computed as individual DiD around potential plant closure between each treated child and her matched control peer, using treated-control pairs described in Section 3. Post-closure outcomes are averaged across the 5 years following displacement, pre-closure outcomes are measured in $t^* - 2$. See Section 6 and Appendix D for details.

Table D.2: Parental characteristics by quintile of predicted plant closure impacts

Quintile of predicted impact	Parent's age	Parents' years of schooling	Father	Family income	Parental labour earnings	Parental plant size	Parental tenure
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Family income ('000s of 2020 DKK)							
First	41.278	12.467	0.676	914927.000	423799.600	190.128	7.893
Second	39.496	12.507	0.697	780472.300	394874.500	139.140	7.600
Third	39.005	12.691	0.671	764516.500	391437.700	135.021	7.603
Fourth	38.898	12.953	0.621	763058.200	394467.400	127.883	7.622
Fifth	39.517	13.532	0.627	800180.900	437110.300	132.388	7.464
TOTAL	39.639	12.830	0.658	804631.000	408337.700	144.912	7.636
Panel B. Parental labour earnings ('000s of 2020 DKK)							
First	41.410	12.572	0.714	875335.600	484258.200	190.637	8.071
Second	39.767	12.597	0.658	786316.800	404786.400	125.722	7.700
Third	39.259	12.723	0.651	775780.800	389805.000	126.828	7.710
Fourth	39.101	12.952	0.626	782980.200	384682.000	129.832	7.625
Fifth	38.658	13.307	0.642	802741.600	378155.600	151.541	7.075
TOTAL	39.639	12.830	0.658	804631.000	408337.700	144.912	7.636
Panel C. Parental yearly unemployment spell (1000 = full year)							
First	39.609	13.182	0.808	843580.000	431924.600	148.532	6.912
Second	39.469	12.996	0.704	812335.000	414381.300	147.944	7.500
Third	39.478	12.868	0.628	799222.300	403724.800	122.861	7.739
Fourth	39.719	12.686	0.585	789998.700	395779.200	114.850	7.892
Fifth	39.919	12.419	0.566	778018.100	395878.000	190.375	8.138
TOTAL	39.639	12.830	0.658	804631.000	408337.700	144.912	7.636

Note. The table shows parental characteristics by quintile of predicted impacts of parental job displacement on family income (Panel A), parental earnings (Panel B), and parental unemployment spells (Panel C). Unemployment spells are measured as the proportion of time in a year on unemployment benefits, nomalised in the $(0, 1000)$ interval. Impacts are predicted by regressing individual gains or losses in the considered outcomes on parental and child baseline characteristics. Individual gains or losses are computed as individual DiD around potential plant closure between each treated child and her matched control peer, using treated-control pairs described in Section 3. Post-closure outcomes are averaged across the 5 years following displacement, pre-closure outcomes are measured in $t^* - 2$. For each quintile of predicted impact, the average parental age at displacement is reported in column (2), while parental years of schooling, male indicator, family income, labor earnings, plant size, and tenure are reported in columns (3) to (8). See Section 6 and Appendix D for details.

Table D.3: Joint distribution of predicted impacts of plant closure on parental outcomes

	Terciles of predicted parental unemployment change			
Terciles of predicted family income loss	1	2	3	TOT
1	41,296	28,802	17,378	87,476
2	27,916	32,348	27,212	87,476
3	18,264	26,326	42,886	87,476
TOT	87,476	87,476	87,476	262,428

Note. The table shows frequency counts by terciles of predicted impacts of parental job displacement on unemployment and on family income. Top tercile of predicted family income loss is associated with larger negative predicted changes, while top tercile of predicted impact on parental unemployment is associated with larger positive predicted changes. See Section 6 and Appendix D for details.

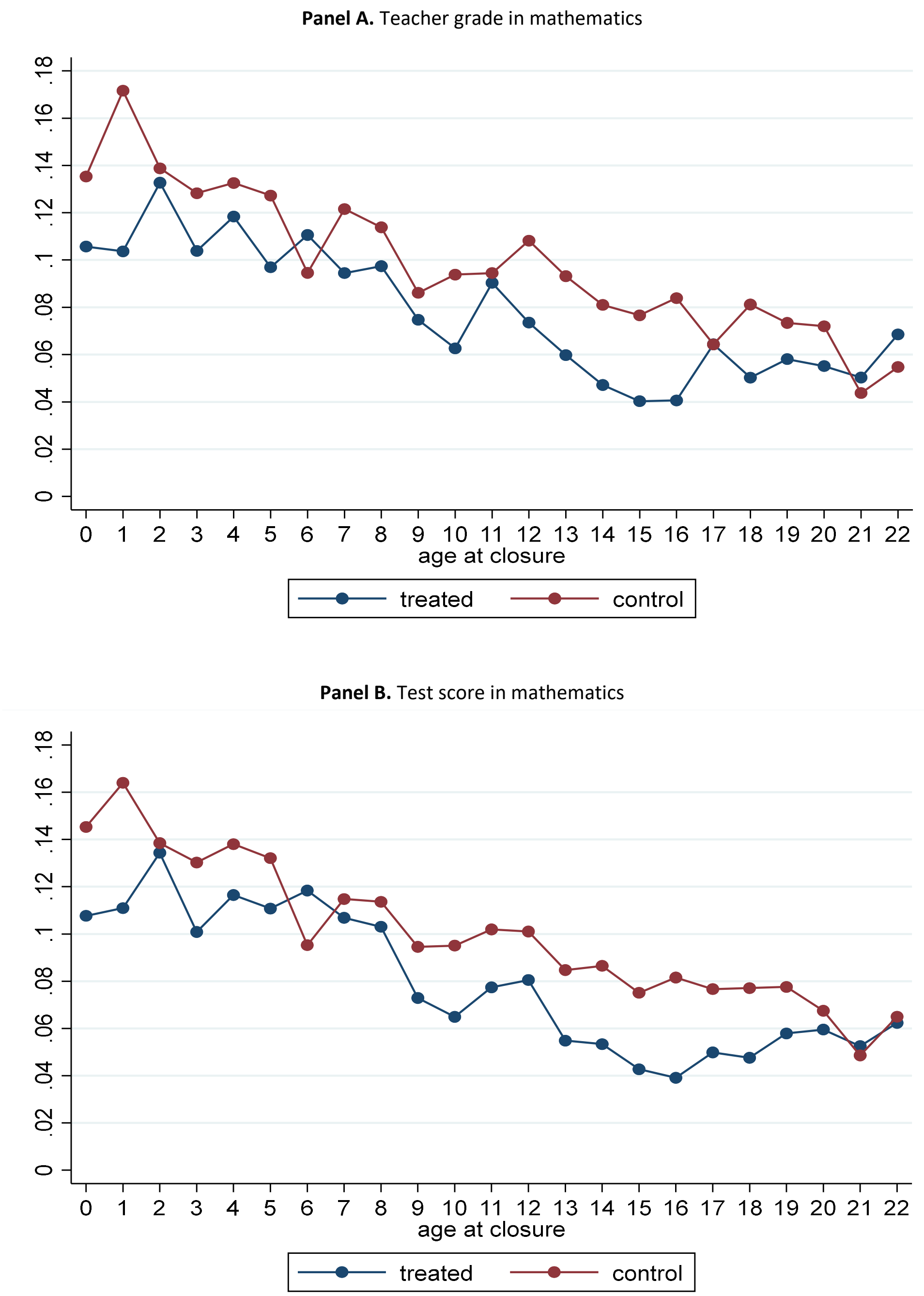
Table D.4: Parental characteristics by predicted impacts on family income and parental unemployment

	Parent's age	Parents' years of schooling	Father	Family income	Parental labour earnings	Parental plant size	Parental tenure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Children with relatively large predicted family income loss							
Terciles of predicted parental unemployment change							
1	40.710	12.735	0.748	951200.300	411189.400	149.464	7.082
2	40.535	12.568	0.692	864555.500	409754.600	122.305	7.644
3	40.628	12.308	0.654	826258.000	415565.300	207.150	8.149
TOT	40.617	12.475	0.685	863870.200	412902.900	169.572	7.774
Panel B. Children with relatively large predicted parental unemployment increase							
Terciles of predicted family income loss							
1	39.624	13.019	0.434	740525.100	383876.700	101.707	8.072
2	38.718	12.508	0.526	738837.500	371590.200	123.791	7.880
3	40.628	12.308	0.654	826258.000	415565.300	207.150	8.149
TOT	39.835	12.512	0.570	782031.600	395590.300	160.271	8.050

Note. The table shows parental characteristics by tercile of predicted impacts of parental job displacement on unemployment conditional on relatively large predicted family income losses (Panel A), or parental characteristics by tercile of predicted impacts of parental job displacement on faimily income conditional on relatively large predicted increase in unemployment (Panel B). Sample considered in Panel A includes children in the top tercile of predicted family income drop from parental job displacement. Sample considered in Panel B includes children in the top tercile of predicted parental unemployment increase from parental job displacement. Variables definition and reporting follows Table D.2. See Section 6 and Appendix D for details.

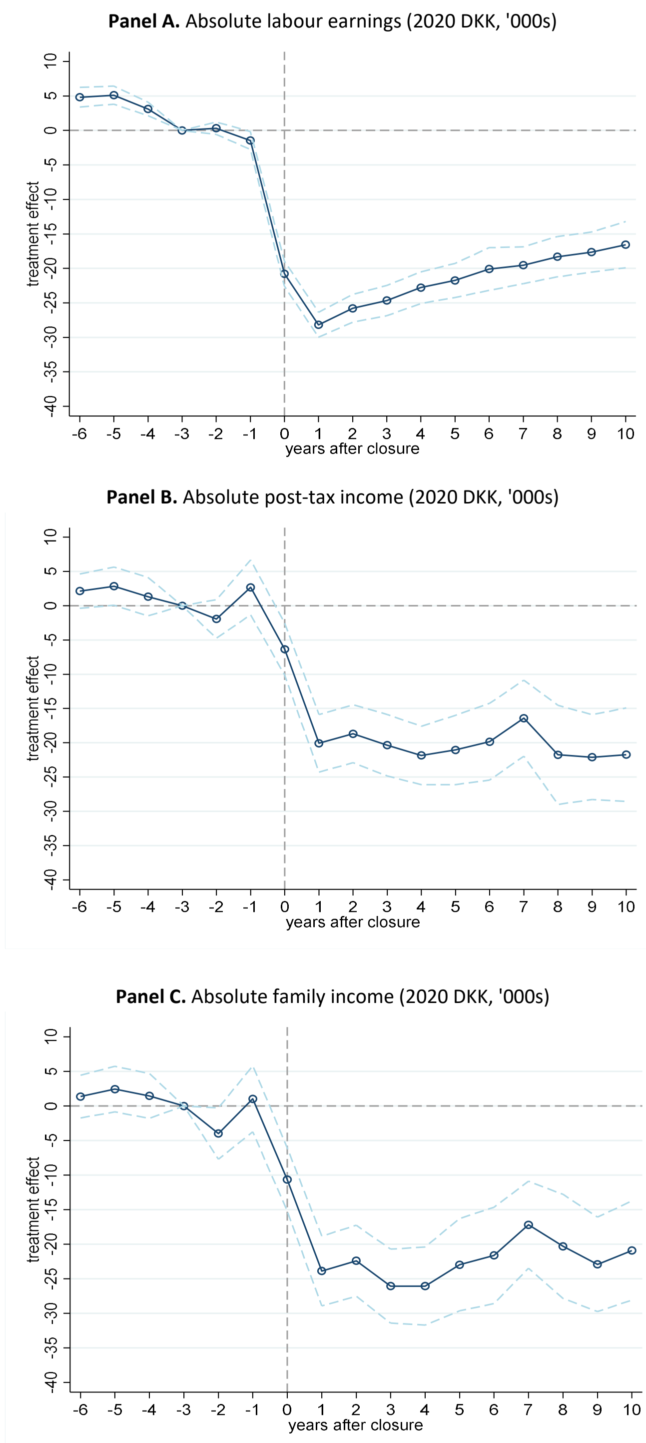
E Additional Tables and Figures

Figure C.1: Achievement by age at parental plant closure



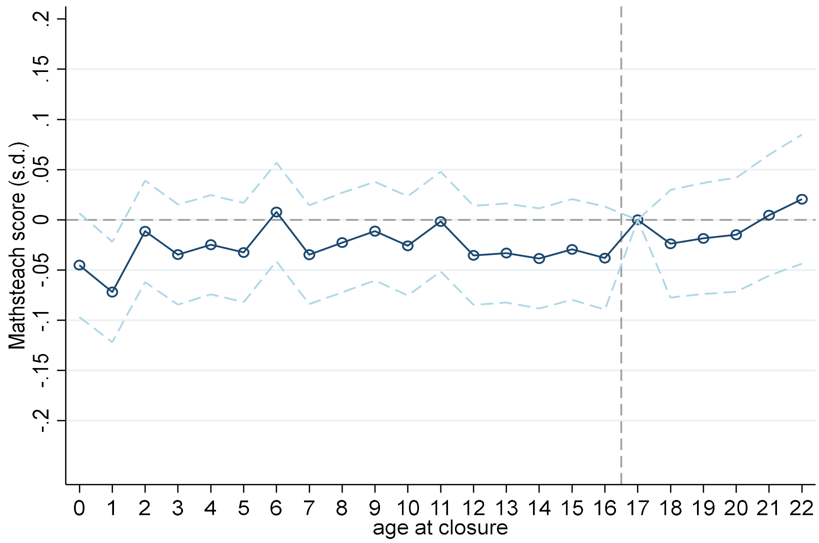
Note. The figure shows raw trends in grade 9 achievement outcomes by age at real or placebo plant closure. Blue lines consider treated children and red lines consider control children, selected through a 1:1 matching algorithm without replacement. Panels A and B plot examinations score and teacher assessment in mathematics, respectively, among students taking grade 9 examinations. See Section 3 and Section 4 for details.

Figure C.2: Plant closure impacts on additional parental outcomes



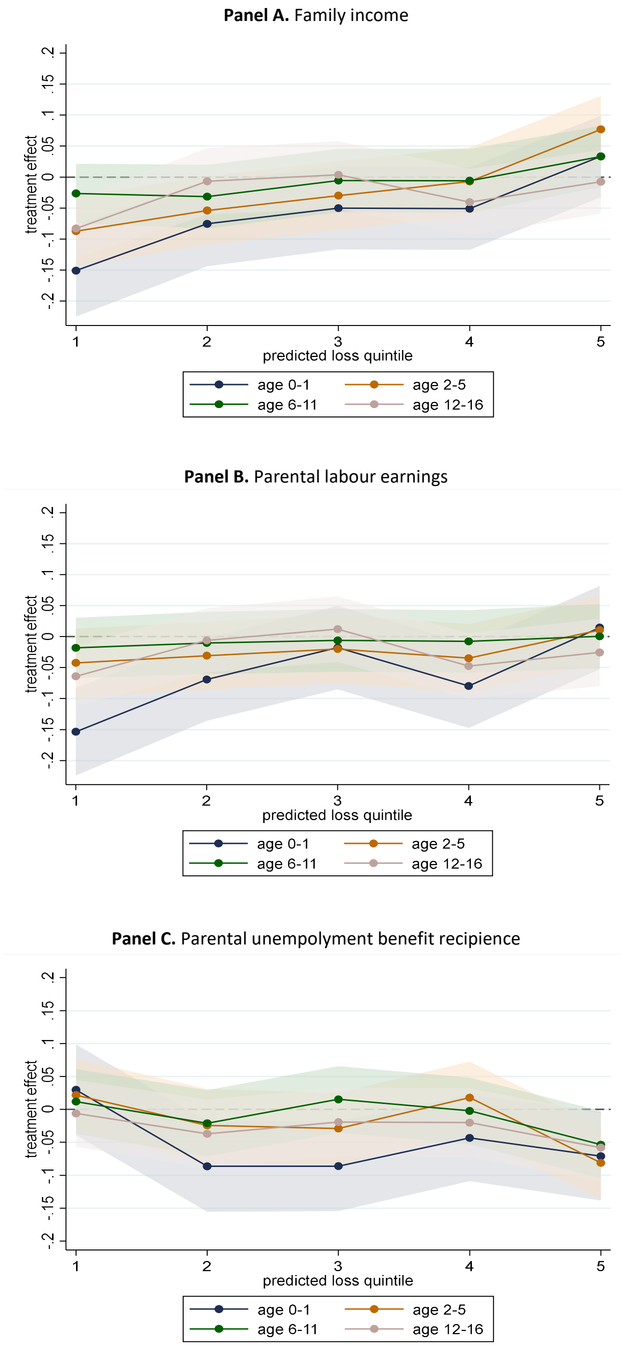
Note. The figure shows event study estimates of the impact of plant closure on parent's outcomes. Plotted are estimates of coefficients α_l in equation (4), following specification and structure of Figure 1. Panel A considers absolute labor earnings in thousands of 2020 DKK, while corresponding amounts of post-tax income and family income are considered in Panel B and Panel C, respectively. Panel D and considers the fraction of time in a year spent on unemployment benefits (normalised from 0-1000). Family income is the sum of both parents' post-tax income 3 years before closure. See Section 5 for details.

Figure C.3: Parental plant closure impacts on math achievement by age at closure



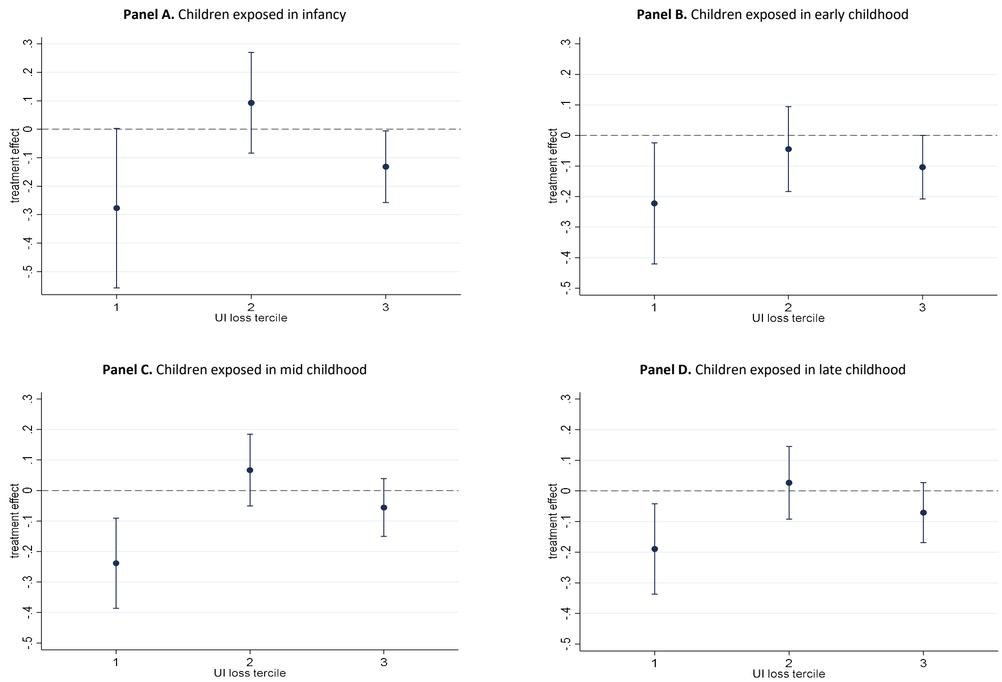
Note. The figure shows estimates of the impact of parental plant closure on child's grade 9 achievement by age at the time of closure. Plotted are estimates of coefficients λ_k from Equation (2). Boundaries of the 95% confidence intervals are plotted as dashed lines. Standard errors are clustered at the family level. Dependent variable is grade 9 teacher assessments in mathematics. Estimated specifications are analogous to column (2) of Table 2. See Section 5 for details.

Figure C.4: Potential mechanisms



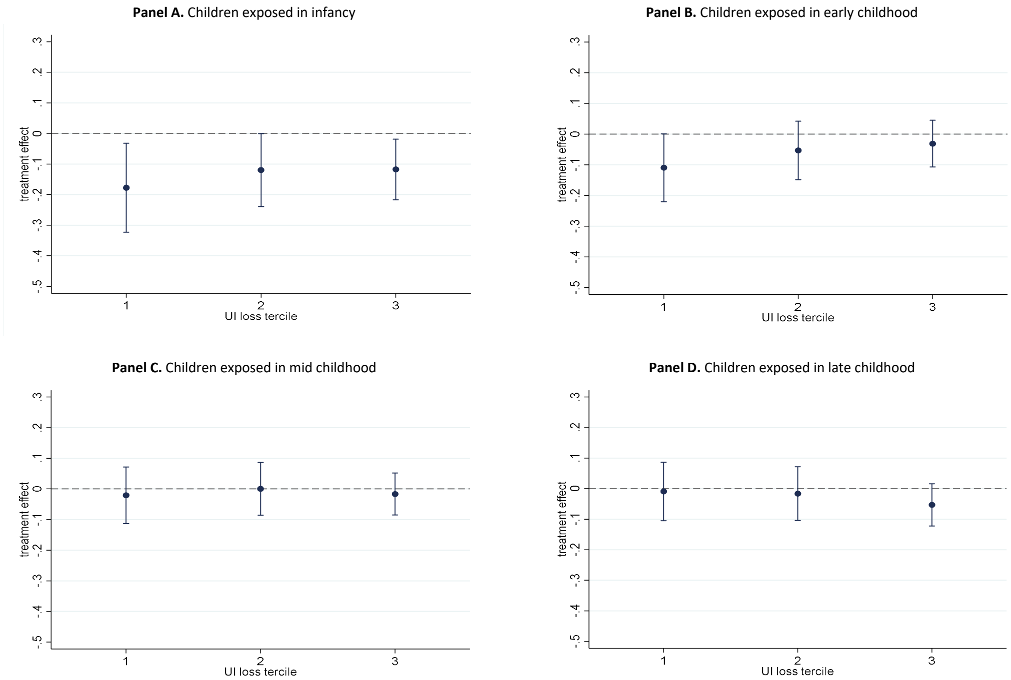
Note. The figure shows estimates of the impacts of parental plant closure on child's grade 9 achievement in mathematics by age at the time of closure and by predicted impacts on family income (Panel A), parental labor earnings (Panel B) or unemployment recipience (Panel C). Plotted in each panel are estimates of coefficients δ_s from Equation (3) from 5 different regressions splitting the sample by quintile of predicted impacts. Predicted impacts are estimated in two steps. First, we compute the individual difference-in-difference change in parental outcomes using treated-control pairs selected in Section 3. Second, predicted changes are computed as predicted values from regressions of individual changes on baseline parental and child characteristics. The dependent variable is teacher grade in mathematics. Estimated specification is analogous to column (2) of Table 2. See Section 6 for details.

Figure C.5: Maternal plant closure impacts with relatively large family income drop



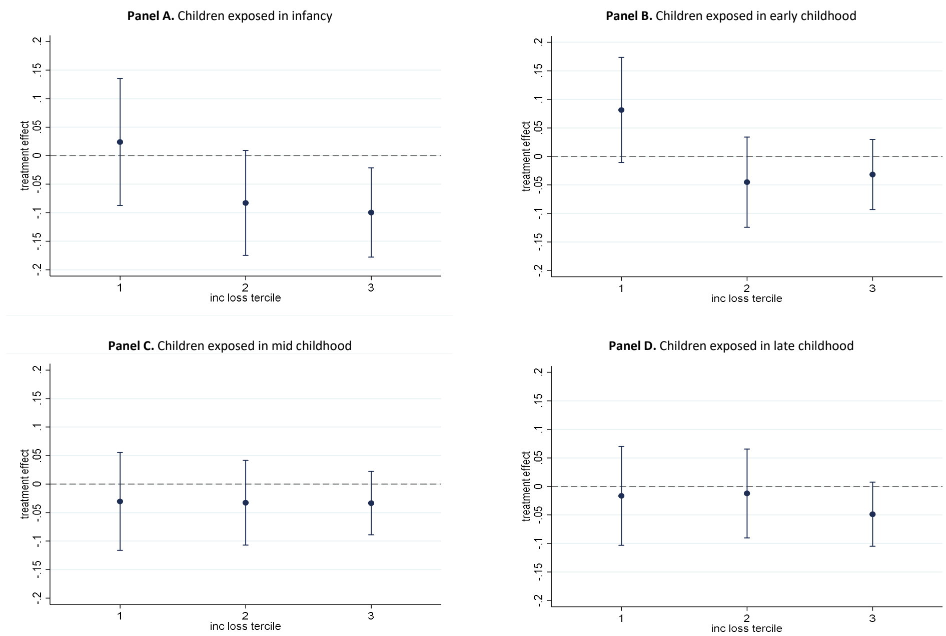
Note. The figure shows estimated impacts of parental plant closure on child's grade 9 achievement in mathematics by age at the time of closure for children with relatively large predicted family income drop. The sample is formed by children in the top tercile of predicted family income loss exposed to real or placebo maternal plant closure. Estimates and specifications follow Figure 5. See Section 6 for details.

Figure C.6: Paternal plant closure impacts with relatively large family income drop



Note. The figure shows estimated impacts of parental plant closure on child's grade 9 achievement in mathematics by age at the time of closure for children with relatively large predicted family income drop. The sample is formed by children in the top tercile of predicted family income loss exposed to real or placebo paternal plant closure. Estimates and specifications follow Figure 5. See Section 6 for details.

Figure C.7: Parental plant closure impacts with relatively large parental unemployment increase



Note. The figure shows estimated impacts of parental plant closure on child's grade 9 achievement in mathematics by age at the time of closure for children with relatively large predicted parental unemployment increase. The sample is formed by children in the top tercile of predicted parental unemployment increase. Plotted are separate estimates of δ_s from Equation (3) by tercile of predicted parental unemployment increase. Estimated specification is analogous to Panel A of Figure C.4 against average predicted unemployment increase by childhood stage at parental plant closure. Estimated coefficients for children exposed in infancy, early childhood, mid childhood, and adolescence is plotted in Panel A, Panel B, Panel C, and Panel D, respectively. See Section 6 for details.

Table C.1: Descriptive statistics on treated and untreated workers

	Treated workers		Treated workers			
	Mean	SD	Mean	SD	Difference	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.6247	0.4842	0.6180	0.4859	0.0067	0.0000
Plant size	148.4557	314.3236	295.2763	779.6073	-146.8206	0.0000
Tenure	7.5900	4.6797	8.1831	5.3067	-0.5931	0.0000
Earnings (2020 DKK, 000's')	388.5701	224.7084	398.4687	239.6105	-9.8986	0.0000
Post-tax income (2020 DKK, 000's)	431.3630	650.8522	439.3168	847.5840	-7.9538	0.0000
Log earnings	12.7305	0.5894	12.7645	0.5673	-0.0340	0.0000
Log post-tax income	12.8498	0.4631	12.8718	0.4524	-0.0220	0.0000
Year of birth	1956.0851	13.2736	1958.4137	13.4103	-2.3287	0.0000
Age	42.3378	9.7789	42.4488	9.7174	-0.1110	0.0000
Years of schooling	12.1493	2.6789	12.4504	2.6383	-0.3011	0.0000
Received UI	0.1142	0.3181	0.0772	0.2669	0.0370	0.0000
Manufacturing	0.2220	0.4156	0.2525	0.4345	-0.0305	0.0000
Services	0.1541	0.3611	0.1729	0.3781	-0.0187	0.0000
Other industry	0.6239	0.4844	0.5746	0.4944	0.0493	0.0000
N	315,347		14,374,000			

Note. The table shows descriptive statistics on treated and untreated workers. Sample considered is formed by workers experiencing plant closure and those whose plant is not closed meeting the requirements described in Section 3. Characteristcs are observed in the base year for treated workers, in any year for untreated workers. Columns (1) and (3) show average values for treated and control children, respectively, and columns (2) and (4) show corresponding standard deviations. Column (5) computes the difference between column (1) and column (3), and column (6) reports the p-value of the associated t-statistics. See Section 3 for details.

Table C.2: Descriptive statistics on treated and untreated children

	Treated children		Untreated children			
	Mean	SD	Mean	SD	Difference	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.5139	0.4998	0.5130	0.4998	0.0009	0.5134
Parent received UI	0.1163	0.3205	0.0752	0.2637	0.0411	0.0000
Parent in manufacturing	0.2187	0.4134	0.2560	0.4364	-0.0373	0.0000
Parent in services	0.1757	0.3806	0.1894	0.3919	-0.0138	0.0000
Parent in other industries	0.6056	0.4887	0.5546	0.4970	0.0511	0.0000
Parent's municipality of work	463.4619	258.9403	473.8048	254.4448	-10.3429	0.0000
Parent's plant size	144.4480	320.5018	273.6802	717.6548	-130.0000	0.0000
Parent's tenure	7.6393	4.8529	8.4335	5.4711	-0.7942	0.0000
Parent's earnings (2020 DKK, 000's)	429.8378	260.8660	440.1975	295.9130	-10.3597	0.0000
Parent's post-tax income (2020 DKK, 000's)	476.7409	590.1633	489.4819	682.1497	-12.7409	0.0000
Year of birth	1993.5990	4.7106	1994.1364	4.6529	-0.5374	0.0000
Family post-tax income (2020 DKK, 000's)	806.4286	442.3418	848.3456	713.5593	-41.9170	0.0000
Age at displacement	9.9965	6.1288	11.0691	6.1940	-1.0727	0.0000
N. of siblings	1.2812	0.8819	1.2957	0.8670	-0.0145	0.0000
Birth order	1.6390	0.7808	1.6485	0.7794	-0.0095	0.0000
Father	0.6575	0.4745	0.6436	0.4789	0.0139	0.0000
Parent's age at birth	30.7076	4.9615	30.7876	4.8773	-0.0800	0.0000
Parent's age	39.6225	7.2976	40.7769	7.3690	-1.1544	0.0000
Parent's years of education	12.7777	2.2133	12.9488	2.2293	-0.1712	0.0000
N	133,531		6,945,910			

Note. The table shows descriptive statistics on treated and untreated children. Sample considered is formed by children experiencing parental plant closure and those unexposed meeting the requirements described in Section 3. Parental and child characteristcs are observed in the base year (the year before closure) for treated children, while they are observed in any year for untreated children. Columns(1)-(2), therefore, report statistics from child-level observations while columns (3)-(4) report statistics from child-year observations. The number of untreated children is 675,321. Column (5) computes the difference between column (1) and column (3), and column (6) reports the p-value of the associated t-statistics. See Section 3 for details.

Table C.3: Impacts of parental job displacement on achievement (other subjects)

	Teacher grade in Danish			Test score in Danish			GPA (test + teacher)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A. Average impacts across childhood					
Treated	-0.0310***	-0.0174*	-0.0140	-0.0330***	-0.0191**	-0.0169*	-0.0274***	-0.0153**	-0.0123
	(0.0098)	(0.0092)	(0.0093)	(0.0098)	(0.0092)	(0.0093)	(0.0083)	(0.0077)	(0.0078)
Treated X Exposure (age 0-16)	0.0055	-0.0009	-0.0041	0.0080	0.0017	-0.0020	0.0013	-0.0039	-0.0070
	(0.0106)	(0.0100)	(0.0100)	(0.0107)	(0.0100)	(0.0101)	(0.0090)	(0.0083)	(0.0083)
				Panel B. Impacts by childhood stage					
Treated	-0.0310***	-0.0174*	-0.0140	-0.0330***	-0.0191**	-0.0169*	-0.0274***	-0.0153**	-0.0123
	(0.0098)	(0.0092)	(0.0093)	(0.0098)	(0.0092)	(0.0093)	(0.0083)	(0.0077)	(0.0078)
Treated X Infant (0-1 years)	-0.0092	-0.0213	-0.0275*	-0.0017	-0.0150	-0.0216	-0.0092	-0.0213*	-0.0271**
	(0.0163)	(0.0154)	(0.0155)	(0.0163)	(0.0155)	(0.0155)	(0.0138)	(0.0128)	(0.0128)
Treated X Early childhood (2-5 years)	0.0104	0.0001	-0.0055	0.0147	0.0045	-0.0015	0.0032	-0.0055	-0.0108
	(0.0133)	(0.0125)	(0.0126)	(0.0134)	(0.0126)	(0.0127)	(0.0113)	(0.0104)	(0.0105)
Treated X Mid childhood (6-11 years)	0.0090	0.0033	0.0009	0.0063	0.0011	-0.0022	0.0082	0.0038	0.0010
	(0.0122)	(0.0114)	(0.0114)	(0.0122)	(0.0115)	(0.0115)	(0.0104)	(0.0095)	(0.0095)
Treated X Late childhood (12-16 years)	0.0025	0.0005	-0.0011	0.0085	0.0064	0.0047	-0.0052	-0.0060	-0.0073
	(0.0125)	(0.0118)	(0.0117)	(0.0125)	(0.0118)	(0.0118)	(0.0106)	(0.0098)	(0.0097)
Constant	0.0827***	0.0019	-0.0169	0.0772***	-0.0034	-0.0231	0.0318***	-0.0424**	-0.0589***
	(0.0029)	(0.0187)	(0.0172)	(0.0029)	(0.0189)	(0.0175)	(0.0025)	(0.0181)	(0.0169)
N	240,516	240,516	240,514	240,723	240,723	240,721	243,980	243,980	243,978
Year of birth FEs	X	X	X	X	X	X	X	X	X
Year of shock FEs	X	X	X	X	X	X	X	X	X
Parental characteristics		X	X		X	X		X	X
Children characteristics		X	X		X	X		X	X
Industry and municipality FEs			X			X			X

Note. The table shows estimates of the impacts of parental plant closure on child's grade 9 achievement. Estimates and specifications are analogous to Table 4. Dependent variables are teacher assessments (columns 1-3) or test scores (columns 4-6) in Danish or child's GPA across all scores and assessments (columns 7-9). The sample considered is formed by children obtaining scores or assessments. See Section 5 for details.

Table C.4: Robustness checks: impacts of parental job displacement on test-taking

	Same plant's closures		Downsize in t*		Downsize in t*-1		Parent has not left a closing plant	Age at closure > 0	Include children in-utero at closure	Closures in local market		% of closures in local market	
	Single	Multiple	Small	Large	Small	Large				Yes	No	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
							Panel A. Do not take test or receive grades						
Treated	-0.0033	-0.0012	-0.0030	-0.0029	-0.0054*	0.0015	-0.0033	-0.0033	-0.0033	-0.0047	-0.0015	-0.0069	-0.0016
	(0.0026)	(0.0077)	(0.0038)	(0.0032)	(0.0029)	(0.0044)	(0.0025)	(0.0024)	(0.0024)	(0.0033)	(0.0036)	(0.0045)	(0.0029)
Treated X In-utero (age "-1")									0.0111**				
									(0.0055)				
Treated X Infant (0-1 years)	0.0094**	0.0131	0.0058	0.0130**	0.0109**	0.0085	0.0101**	0.0076	0.0099**	0.0158***	0.0008	0.0205***	0.0043
	(0.0043)	(0.0137)	(0.0063)	(0.0054)	(0.0049)	(0.0078)	(0.0042)	(0.0052)	(0.0041)	(0.0053)	(0.0065)	(0.0070)	(0.0051)
Treated X Early childhood (2-5 years)	0.0069**	0.0153	0.0069	0.0080*	0.0077*	0.0081	0.0086**	0.0076**	0.0076**	0.0128***	-0.0000	0.0128**	0.0051
	(0.0035)	(0.0112)	(0.0052)	(0.0043)	(0.0039)	(0.0062)	(0.0034)	(0.0033)	(0.0033)	(0.0044)	(0.0051)	(0.0058)	(0.0041)
Treated X Mid childhood (6-11 years)	0.0046	0.0019	0.0086*	0.0015	0.0075**	-0.0024	0.0045	0.0045	0.0045	0.0087**	-0.0009	0.0099*	0.0021
	(0.0032)	(0.0100)	(0.0047)	(0.0040)	(0.0036)	(0.0056)	(0.0031)	(0.0030)	(0.0030)	(0.0040)	(0.0046)	(0.0055)	(0.0036)
Treated X Late childhood (12-16 years)	0.0029	0.0041	0.0071	0.0000	0.0032	0.0024	0.0030	0.0030	0.0030	0.0008	0.0053	0.0015	0.0035
	(0.0033)	(0.0102)	(0.0049)	(0.0041)	(0.0038)	(0.0056)	(0.0032)	(0.0031)	(0.0031)	(0.0042)	(0.0047)	(0.0057)	(0.0037)
N	239,524	22,904	109,688	152,740	184,608	77,820	254,736	251,662	272,178	148,420	114,008	83,822	178,606
							Panel B. Obtain scores or grades in Maths and Danish						
Treated	0.0044	-0.0011	0.0056	0.0024	0.0054*	0.0010	0.0039	0.0039	0.0040	0.0067*	0.0007	0.0084*	0.0020
	(0.0027)	(0.0082)	(0.0041)	(0.0034)	(0.0031)	(0.0046)	(0.0026)	(0.0026)	(0.0026)	(0.0035)	(0.0038)	(0.0048)	(0.0031)
Treated X In-utero (age "-1")									-0.0084				
									(0.0059)				
Treated X Infant (0-1 years)	-0.0105**	-0.0089	-0.0092	-0.0115**	-0.0104**	-0.0113	-0.0103**	-0.0078	-0.0104**	-0.0173***	-0.0004	-0.0206***	-0.0053
	(0.0046)	(0.0146)	(0.0067)	(0.0058)	(0.0051)	(0.0082)	(0.0044)	(0.0055)	(0.0044)	(0.0057)	(0.0068)	(0.0074)	(0.0054)
Treated X Early childhood (2-5 years)	-0.0083**	-0.0132	-0.0087	-0.0087*	-0.0083**	-0.0104	-0.0093***	-0.0087**	-0.0087**	-0.0145***	-0.0008	-0.0147**	-0.0060
	(0.0037)	(0.0119)	(0.0055)	(0.0046)	(0.0042)	(0.0065)	(0.0036)	(0.0035)	(0.0035)	(0.0046)	(0.0055)	(0.0062)	(0.0043)
Treated X Mid childhood (6-11 years)	-0.0060*	0.0033	-0.0112**	-0.0009	-0.0066*	-0.0024	-0.0053	-0.0052	-0.0052	-0.0102**	0.0011	-0.0102*	-0.0031
	(0.0034)	(0.0106)	(0.0050)	(0.0042)	(0.0038)	(0.0059)	(0.0033)	(0.0032)	(0.0032)	(0.0043)	(0.0048)	(0.0058)	(0.0039)
Treated X Late childhood (12-16 years)	-0.0041	-0.0008	-0.0094*	0.0003	-0.0028	-0.0059	-0.0036	-0.0037	-0.0037	-0.0025	-0.0049	-0.0012	-0.0048
	(0.0035)	(0.0108)	(0.0052)	(0.0043)	(0.0040)	(0.0059)	(0.0034)	(0.0033)	(0.0033)	(0.0045)	(0.0049)	(0.0061)	(0.0040)
N	239,524	22,904	109,688	152,740	184,608	77,820	254,736	251,662	272,178	148,420	114,008	83,822	178,606
Year of birth FEs	X	X	X	X	X	X	X	X	X	X	X	X	X
Year of shock FEs	X	X	X	X	X	X	X	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X	X	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X	X	X	X	X	X	X	X

Note. The table shows estimates of the impacts of parental plant closure on child's test-taking at the end of compulsory school. Estimates and specifications are analogous to Table C.4. Dependent variable is a dummy equal to one if child does not obtain test scores or teacher assessments in grade 9 examinations. See Section 5 for details.

Table C.5: Main earner analysis (test-taking)

	All displaced parents		Father displaced		Mother displaced	
	Not main earner	Main earner	Not main earner	Main earner	Not main earner	Main earner
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Do not take test or receive grades					
Treated	-0.0010	-0.0029	-0.0022	-0.0019	-0.0000	-0.0061
	(0.0036)	(0.0030)	(0.0061)	(0.0034)	(0.0046)	(0.0063)
Treated X Infant (0-1 years)	0.0037	0.0113**	0.0152	0.0105*	0.0002	0.0121
	(0.0066)	(0.0051)	(0.0131)	(0.0056)	(0.0076)	(0.0125)
Treated X Early childhood (2-5 years)	0.0014	0.0084**	0.0053	0.0066	-0.0004	0.0172*
	(0.0053)	(0.0041)	(0.0098)	(0.0045)	(0.0064)	(0.0100)
Treated X Mid childhood (6-11 years)	0.0038	0.0033	0.0068	0.0041	0.0021	-0.0030
	(0.0047)	(0.0038)	(0.0080)	(0.0042)	(0.0057)	(0.0083)
Treated X Late childhood (12-16 years)	0.0051	0.0005	0.0089	0.0001	0.0025	0.0012
	(0.0049)	(0.0040)	(0.0082)	(0.0045)	(0.0060)	(0.0085)
Constant	0.0648***	0.0849***	0.0863***	0.0872***	0.0631***	0.1064***
	(0.0024)	(0.0021)	(0.0048)	(0.0017)	(0.0026)	(0.0062)
N	86,857	172,837	29,252	142,281	57,605	30,556
	Panel B. Obtain scores or grades in Maths and Danish					
Treated	0.0014	0.0033	0.0026	0.0021	0.0005	0.0071
	(0.0039)	(0.0032)	(0.0065)	(0.0037)	(0.0049)	(0.0066)
Treated X Infant (0-1 years)	-0.0025	-0.0121**	-0.0127	-0.0109*	0.0006	-0.0147
	(0.0070)	(0.0054)	(0.0139)	(0.0060)	(0.0082)	(0.0132)
Treated X Early childhood (2-5 years)	-0.0017	-0.0094**	-0.0073	-0.0077	0.0007	-0.0166
	(0.0057)	(0.0043)	(0.0104)	(0.0048)	(0.0068)	(0.0107)
Treated X Mid childhood (6-11 years)	-0.0058	-0.0030	-0.0106	-0.0039	-0.0032	0.0038
	(0.0050)	(0.0040)	(0.0086)	(0.0045)	(0.0061)	(0.0088)
Treated X Late childhood (12-16 years)	-0.0066	-0.0002	-0.0094	-0.0001	-0.0048	0.0000
	(0.0052)	(0.0042)	(0.0088)	(0.0048)	(0.0065)	(0.0090)
Constant	0.9273***	0.9032***	0.9031***	0.9001***	0.9291***	0.8809***
	(0.0026)	(0.0022)	(0.0051)	(0.0018)	(0.0028)	(0.0066)
N	86,857	172,837	29,252	142,281	57,605	30,556
Age at closure dummies	X	X	X	X	X	X
Year of birth dummies	X	X	X	X	X	X
Year of shock dummies	X	X	X	X	X	X
Parental characteristics	X	X	X	X	X	X
Children characteristics	X	X	X	X	X	X

Note. The table shows estimates of the heterogeneous impacts of parental plant closure on child's grade 9 achievement in mathematics. Estimates and specifications are anlogous to columns (2) and (5) of Table 4, replicated here in column (1) to ease comparison. Following columns restrict estimation to children whose displaced parent is the secondary or the main earner among parents (columns 2-3, respectively). The same sample restriction is considered among paternal (columns 4-5) or maternal (columns 6-7) displacements only. Dependent variables are teacher assessments (Panel A), or test scores (Panel B). See Section 6 for details.

LUXEMBOURG INSTITUTE OF
SOCIO-ECONOMIC RESEARCH

11, Porte des Sciences
Campus Belval
L-4366 Esch-sur-Alzette

T +352 58 58 55-1
F +352 58 58 55-700

www.liser.lu