Childhood Shocks Across Ages and Human Capital Formation

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May 2023

Abstract

We provide estimates of the causal impact of shocks to home environments during childhood on the human capital formation of children and their adult earnings, and document how these impacts differ depending on the age of the child when the shock occurs. We do so by comparing the outcomes of children whose parents experienced an involuntary job loss at different points in time. The rich data we have access to enable us to examine a broad range of short- and long-term educational outcomes related to performance, attainment, and behavior. In addition, for a subsample of our cohorts we can explore earnings effects at age 30. Consistent with other studies, we confirm that early childhood represents a crucial time for acquiring skills and abilities, but also establish that changes in the home environment for children in early adolescence matter as much, and sometimes more. We rationalize these results by noting that sensitive periods for different skills occur at different stages of childhood. Furthermore, it is during early adolescence that children face key junctures in their educational choices.

JEL CODES: I20, J12, J13, J63, D10
KEYWORDS: Early childhood development, intergenerational links, human capital

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1. Introduction

A large literature spanning multiple fields documents that there are very high returns to investments in early childhood (e.g., Carneiro and Heckman 2003, Almond and Currie 2010). A related literature also suggests that the returns to human capital interventions may decline as the child ages (e.g., Heckman 2006). Not only do the early childhood years represent critical learning periods (WHO 2007), but skills beget skills in a complementary and dynamic way such that childhood investments in the first years may generate both the greatest efficiency and effectiveness (Cunha et al. 2010; Heckman 2012; Caucutt and Lochner, 2020).

At the core of this literature is a comprehensive theory of skills that encompasses all forms of human capability (e.g., physical and mental), and this theory aligns well with the basic principles of neuroscience and human capital formation. Specifically, by age five, language and sensory pathways have been almost fully formed, and higher cognitive functions have reached peak development (Shonkoff 2008). These models of early childhood development represent an extremely valuable thought experiment about the optimal portfolio of investments in human capital, and provide a strong theoretical case for investing in early childhood skill formation.

Recent empirical investigations of the idea that returns to human capital interventions fall with age are based on evidence on the relative returns of various human capital interventions across the lifecycle, and its findings are not consensual.[1] In this literature, comparisons are made between children who do not only differ in terms of age, but who also come from different populations and have been subject to fundamentally different types of interventions. With so many factors changing it is difficult to distinguish the role of age at the time of the intervention in determining the returns to human capital investments from the role of all other factors that vary across studies.

More recently, a few papers compare correlations between family income at different stages of childhood and a range of individual outcomes (e.g., Carneiro et al. 2021; Eshaghnia et al. 2022; Eshaghnia et al. 2023). These papers find that family income at different stages of childhood predicts future outcomes, with income in adolescence being sometimes as or more important than income in early childhood. This is consistent with the idea that there are different sensitive periods for different skills (Belsky et al. 2020; Steinberg 2014). However, the ability to

[1] While many of these studies confirm that returns to human capital interventions fall with age (Heckman 1999; Heckman 2006; Elango et al. 2016), others find less support for this hypothesis (e.g., Rea and Burton 2019; Hendren and Sprung-Keyser 2020, Attanasio et al. 2020).

establish causal links between family environments at different ages and child development has been absent so far in this literature because it is difficult to have data linking parents and children, as well as observed measures of child exposure to exogenous shocks to family environments at different ages.

In this paper, we provide causal evidence of the relative effect of changes to the home environment at different stages of children's upbringing based on the same change, comparing similar children, in similar settings and time periods. To do so, we exploit parental job loss induced by mass layoffs and establishment closures as a function of child age, and use this as an exogenous source of variation for changes in family environments over the child's life.

Using mass layoffs and establishment closures to explore this question is ideal, as these events occur often and generate sizable effects on the home environment (Ruhm 1991; Jacobson et al. 1993; Davis and von Wachter 2011; Ichino et al. 2017). We thus have a context in which similar children of all ages face the same large adverse change to the home environment. This allows us to causally trace out if changes to the childhood environment have different impacts on human capital development depending on the age of the child at the time of the event. A particularly novel component of our analysis is our ability to decompose what matters in terms of the home environment – resource quantity (e.g., family income) or other quality aspects of the home environment (e.g., stress). [2]

Our findings challenge the idea that shocks to early childhood environments have larger impacts on human capital development than shocks occurring later in the life of the child. Specifically, for a number of consequential educational outcomes we study – including GPA, high school achievement and college selectivity – parental job loss in the adolescent years has larger impacts than parental job loss occurring at any other point in the child's life cycle. We rationalize these results by noting that early childhood coincides with critical learning periods that are often perceived to matter for human capital development, while adolescence coincides not only with a period of development of the pre-frontal cortex, but also with key junctures in children's human capital development (choice of high school program, national tests, etc.) in which abrupt changes to the home environment may be particularly disruptive. Indeed, among the adolescent sample, we find that the effects are larger the closer the child is to key educational outcome junctures.

[2] Relative to Carneiro et al (2021), in this paper our analysis relies on shocks that can be convincingly argued to be exogenous.

The primary data for this paper comes from matched employer-employee records on all Norwegian residents between 1986 and 2018. These data allow us to link each worker with her employer and identify whether establishments are downsizing or closing down from one year to the next. We combine the linked employer-employee data with information from various population-wide administrative registers, such as the tax register, the family register (linking parents and children), and the education register (from which we can construct several measures of children's human capital). Individual employment characteristics such as work history, plant size, and industry are also available. This allows us to construct sets of households with similar work histories, similar demographics, and with individuals who work in similar plants, industries, locations, and time period, but who experience displacement episodes when their children are of different ages.

To perform our analysis, we first define a set of base years, 1989 through 2006. We then set relative time equal to 0 for all individuals in that base year. Our treatment group are those who were involuntarily separated from their jobs due to establishment closures and mass layoffs between relative time 0 and 1. Our control group are those who were not involuntarily separated from their jobs between relative time 0 and 1.[3] We then use the family register to identify which individuals had a child in relative time 0, and how old that child was in relative time 0. This allows us to identify the age of children at the time of the parental job loss and change to home environment. We follow these children over time and examine the impact of abrupt changes to the home environment on child human capital accumulation and how it translates into differences in earnings (measured at age 30).

In terms of outcomes, we focus on a broad range of educational outcomes that are measured at ages 16 or above, and that are important predictors of success in adulthood: GPA at the end of compulsory school (grade 10), high school graduation, high school quality (as proxied by the minimum GPA required for admission to the specific school-program), high school behavior (absences during high school), college enrollment, and college quality (as proxied by the minimum GPA required for admission to the specific college-program). Taken together, these outcomes provide a comprehensive overview of the impact of a change to childhood environment on

[3] To ensure that our control and treatment groups are similar, we follow prior literature and restrict the sample to individuals who are highly attached to the labor force as defined by having worked at least 20 hours per week during the three years leading up to the base year.

children's short- and long-term educational outcomes in terms of performance, attainment, and behavior – both on the intensive as well as the extensive margin.

In addition, we are able to follow a subsample of children in our analysis (those aged 6 through 16 at the time of the shock) into the labor market and observe their earnings at age 30, allowing us to explore very long-term earnings effects at an age when concerns over lifecycle earnings bias are minimized (Böhlmark and Lindquist 2006; Haider and Solon 2006). This enables us to provide a direct reduced-form estimate of the overall effect of a change in child environment on the children's very long-term labor market outcomes, without having to infer such effects through the impact on educational outcomes.

Our estimation strategy assumes conditional random assignment of involuntary job displacements to families, after controlling for a rich set of controls (e.g., parental work histories) and a detailed set of fixed effects (cohort, age, and municipality). This is a strong assumption, allowing us to identify the level of the impact at each child age, which can then be used to compare the relative magnitudes of the impacts of displacement at different ages. In support of this assumption, we show that treatment and comparison children as well as their parents are identical along several characteristics beyond the ones we condition on (e.g., Apgar score, birth weight, gender, immigrant status, parental income, parental marital status, parental education). Encouragingly, controlling for more variables (or implementing a matching estimator) yield results similar to the ones we present in our main analysis.

We perform several sensitivity tests, and find that our results are robust to accounting for early leavers (removing parents – and their children – from the analysis who leave the establishment in the year preceding a mass layoff / firm closure); focusing only on large firms; restricting to the common support of the propensity score based on parents' characteristics prior to the displacement events; relaxing the employment history restrictions; altering the composition of the control group; and including a battery of additional controls. We also demonstrate that parental outcomes are trending similarly prior to the involuntary displacement event. The robustness of our results across these tests is consistent with the notion that our benchmark estimates are not driven by endogenous selection of households into displacement.

In addition to the robustness analyses discussed above, we show results from an alternative estimation strategy that relies on weaker identifying assumptions than our baseline method, exploiting only the timing of shocks across all children who ever have been exposed to a parental job loss due to mass layoffs or plant closures. The identifying assumption underlying this approach is that the age of the child at the time of the parental displacement is random across families that were ever displaced. The robustness of our results to the use of this alternative estimation approach is consistent with the notion that the effects are not driven by endogenous selection into treatment.

After having identified the effect of the displacement-induced change in home environment on children across childhood, we expand the analysis with a dynamic component and explore the implications of exposure to multiple home environment changes during childhood. Specifically, even though most individuals experience either zero or one job displacement events during their childhood, there is a smaller sample of individuals who experience two or three job displacements. We use this information to investigate the impact of different sequences of shocks on the outcomes of children (e.g., Cunha et al. 2010; Carneiro et al. 2021; Carneiro et al. 2022). We are the first to estimate such sequence effects; something that prior literature has been unable to do due to the challenges in finding one, let alone multiple, exogenous shocks to household environment.

To disentangle the mechanisms through which the home environment impact child human capital – whether it is due to its interaction with critical ages, its relation to key administrative junctures, or the way parents respond to the changes in home environment – we also examine parental outcomes. In particular, we follow the children's parents over time – from relative time *-3* through relative time *+4* – and use a difference-in-differences framework typical in job displacement studies (e.g., Jacobson et al. 1993). In addition to examining differential effects on earnings and employment, we examine the primary channels through which parents may respond to adverse labor shocks: fertility, mobility, education, and permanent exit from the labor force (Salvanes et al. 2022).

Finally, to push our understanding of the underlying mechanisms even further, we merge our register data with information from detailed mental health surveys in Norway. This enables us to explore how the mental wellbeing of parents is affected by the unexpected negative labor market shocks that they experience as a consequence of the involuntary job displacements, and the potential role such effects may have in driving any effects on their children.

We present four core sets of findings. First, we establish that episodes of parental job loss occurring in early adolescence have larger impacts on the human capital outcomes of children than parental job loss occurring at earlier ages. We also show that early childhood shocks have larger impacts than those occurring in the pre-adolescence years. For a subsample of our cohorts, we show that these human capital effects translate into earnings effects at age 30 for these same children.

Second, we demonstrate that these results are not driven by differential parental response to the shocks (earnings, employment, fertility, mobility, schooling, labor market exit). Rather, they reflect differences in how the children are impacted by the same change in home environment as a function of their own age. We rationalize our results by noting that while early childhood coincides with critical learning periods that often are perceived to matter for human capital development, late adolescence coincides with key junctures in children's human capital development (choice of high school program, national tests, etc.) in which abrupt changes to the home environment may be particularly disruptive. Indeed, among the late adolescent sample, we find that the effects are larger the closer the child is to key educational outcome junctures.[4]

Third, since job loss generates a number of changes to the home environment, we are able to expand our contribution to the literature and decompose whether resource levels (e.g., income) or other aspects (e.g., stress) of the home environment matters more. This is a particularly novel feature of our design that allows us to conclude that changes in resource levels (which has been the main focus of prior literature) are not the main drivers of the effects we find. Instead, by linking our data to mental health surveys, we show evidence that other aspects, such as family stress and other changes to the quality of the parent-child interactions, are the main drivers behind our results.

Finally, the more shocks a child is exposed to during childhood, the lower are most (but not all) of her education outcomes. The relationship between outcomes and the number of shocks is close to additive, so we cannot rule out the absence of dynamic complementarity in the production of skills. It is of course possible that there are strong dynamic complementarities in the production of underlying skills (e.g., Cunha et al. 2010), but that the translation of the underlying skill into the education outcomes we study somehow undoes the underlying dynamic complementarity (e.g., Carneiro et al. 2022).

[4] For a subsample of our cohorts, we can also examine performance on low-stakes national tests in grade 5. These tests are low-stakes as they have no impact on children's educational opportunities. Despite the low-stakes nature of these exams, this supplemental analysis is useful as it provides another key administrative juncture through which we can collect additional evidence in favor of the hypothesis that closeness of the shock to such junctures represents an important factor for the impact it may have on children.

In terms of policy implications, our results highlight that the value of insurance against variation in home environment vary substantially depending on the age of the children in the household, and that it is not simply a matter of protecting children of a young age more in order to maximize social and individual gain. This variation is driven not only by the timing of these environment changes relative to critical learning periods, but also relative to key administrative junctures imposed by our social institutions. While most OECD countries have an educational system similar to that in Norway in terms of the timing of high stakes exams and decisions, such that we would expect a similar pattern of effects in neighboring countries, the exact shape of the returns will depend on the timing of the particular administrative junctures imposed by the social institutions in the country of analysis.

The main contribution of our paper is to exploit credible sources of exogenous variation in childhood home environments across children of different ages, and combine this with rich longitudinal register data, to provide causal estimates of the relative impact on human capital of shocks occurring across different child ages. Our research also contributes to the growing literature examining skill formation in childhood as a dynamic process (e.g., Cunha et al. 2010), acknowledging that exposure to multiple adverse shocks in childhood may have disproportionate effects on children's outcomes. We are able to estimate such sequence effects by using multiple exogenous shocks to family environments; something that prior literature has been unable to do due to the challenges in finding one, let alone multiple, exogenous shocks.[5]

Lastly, we also contribute to the literature on the effect of involuntary displacement on individual's labor market and life outcomes (e.g., Rege et al. 2009; Sullivan and von Wachter 2009; Browning and Heinesen 2011; Del Bono et al. 2012; Tanndal et al. 2020; Coelli 2011; Minaya et al. 2020; Salvanes et al. 2022), as well as the impact of parental job loss on children (e.g., Oreopoulos et al. 2008; Rege et al. 2011; Hilger 2016; Huttunen et al. 2020; Mörk et al. 2020; Tanndal and Päällysaho 2020; Willage and Willén 2022). Related to our paper is also the smaller literature on the causal effect of shocks across the life cycle (e.g., Salvanes et al. 2022; Rinz 2021), and how workers' professional and personal lives are impacted by adverse labor shocks (e.g., Davis and von Wachter 2011; Oreopoulos et al. 2012; Adda et al. 2013). These studies

[5] Carneiro et al. (2022) extend the literature on intergenerational mobility by examining the role of parental income at multiple periods of development, as opposed to considering a single measure of parental income. However, they do not have any source of exogenous variation to parental income.

provide novel insights into the effects of shocks on workers' careers, but they do not examine how children of different ages are impacted by such shocks.

2. Background

In this section, we briefly discuss employment relations and labor market protection in Norway. We also provide an overview of the most relevant aspects of the Norwegian welfare state and education system as it relates to the current analysis.

Employment Protection and Social Welfare. Norwegian employment law is governed by the Working Environment Act. Similar to other Nordic countries, Norway has a high degree of employment protection and generous unemployment benefits (Botero et al. 2004; Huttunen et al. 2018). In the event of mass layoffs, there is no rule determining the order in which workers are laid off.[6] Employment contracts typically require three months' notice of termination, though there are some exceptions related to employment tenure.[7] There is no generalized legal requirement for severance pay.

Unemployment benefits are awarded to individuals who have had their work hours reduced by at least 50 percent. The replacement rate is 62 percent of the pre-dismissal income. The standard entitlement period was 186 weeks until 2004, at which point it was reduced to 104 weeks. Unemployment benefits are conditional on filing an employment form with the public employment office every 14 days, and on having a pre-dismissal income above a certain minimum threshold ($16,500 in 2019).

Disability pensions are available to individuals who are unfit for work because of illness or injury. The cause of disability and whether the condition is permanent or temporary does not matter, but the disability must be verified by a doctor. Traditionally, access to disability pensions has been very liberal, and prior literature has identified disability pension as a common channel through which individuals can permanently exit the labor force while still maintaining a modest source of income (Johnsen et al. 2022). The after-tax replacement rate for previously average earners is around 65 percent (Blöndal and Pearson, 1995).[8]

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[6] While seniority is a strong norm, it should not be considered binding (e.g., Salvanes et al. 2022).

[7] For example, workers with less than five years of tenure can legally be dismissed with only one months' notice. However, in practice, the overwhelming majority of young workers receive a three months' notice.

[8] The official retirement age is 67, though an early retirement provision allows all public sector employees, and many private sector employees, to retire at age 62 (applies to all workers covered by the main employees' and employers' organizations). However, very few parents with children under 20 are near retirement age.

Childcare and Family Policies. Maternal job protection, family support and child benefits play a key role in the Nordic welfare state. First, parents are entitled to 12 months of fully paid parental leave provided that they have worked for at least six of the ten months before childbirth and earned a minimum amount (approximately \$12,500 in 2010). While parental leave benefits are subject to a benefit cap, this cap is generous (\$75,000 in 2010), and most employers supplement benefits to ensure 100 percent coverage (Dahl et al. 2016). Second, all children have a fundamental right to childcare from August of the year they turn one. Childcare is heavily subsidized by the state, and the maximum monthly price is currently \$350.[9] Around 80 percent of one-year-olds attend childcare. Third, parents receive non-means tested financial child support from the state until the child turns 18 years old. This is intended to cover some of the expenses associated with raising the child, and amounts to approximately \$130 per month. Finally, the government provides free universal health care and tuition-free education (including higher education) to all residents.

Education System. The Norwegian education system consists of 10 years of mandatory education starting at age 6. Following the successful completion of compulsory school, every child has a statutory right to 3-to-4 years of upper secondary education.

Upper secondary education consists of two different tracks: an academic track which provides students with direct access to higher education, and a vocational track which results in a trade or journeyman's certificate.[10] The vocational track does not directly grant the student access to higher education.[11] Approximately 50 percent of students choose to enroll in the vocational track, and 50 percent choose to enroll in the academic track. Admission to Norwegian high schools is very competitive from an international perspective. Individuals apply to high school with their grades from compulsory school (10th grade GPA), and selection into schools and programs are determined exclusively by the relative GPA ranking of the applicants.

A range of universities and colleges offer higher education in Norway, and the majority are tuition-free public institutions. Admission is conditional on graduating from an academic high

[9] Low-income families are eligible for additional subsidies. This is considerably cheaper than in other OECD countries, such as the US. See for example https://www.cnbc.com/2021/05/19/what-parents-spend-annually-on-child-care-costs-in-2021.html

[10] The two tracks are further subdivided into different programs (5 programs within the academic track and 10 programs within the vocational track). While there is a difference in the type of courses that students take across the different programs within a given track, the structure of the programs within a track is the same. We therefore abstract from this subdivision in the paper.

[11] However, students in vocational programs can pursue supplemental education to secure access to higher education institutions.

school track and satisfying a minimum grade requirement. If the number of applications exceeds the number of seats, students are assigned exclusively based on high school GPA. Education is free at all levels, including post-secondary school.

3. Data

Our primary data comes from matched employer-employee records on all Norwegian residents aged 16 through 74 between 1986 and 2018. These data allow us to link each worker with her employer and identify whether plants are downsizing or closing down from one year to the next. A mass layoff event is defined as a plant losing more than 30 percent of its workforce from one year to the next. In this analysis, we focus on plants with more than 20 employees to prevent misclassification of false closures and mass layoffs. This is consistent with prior work on the topic (e.g., Salvanes et al. 2022).

A unique personal identifier enables us to combine the linked employer-employee data with information from various population-wide administrative registers, such as the education register, the family register, the tax and earnings register, and the social security register. Moreover, we have data on each individual's municipality of residence each year. Plant and regional labor market characteristics such as industry, plant size, and unemployment rate are also available.

Our wage measure is based on pre-tax labor earnings (including income from self-employment) excluding government transfers. An individual is considered employed if she has a plant identifier in the linked employer-employee data in a given year, unemployed if she does not have a plant identifier and receives any unemployment benefits during the year, and not in the labor force if she does not have a plant identification number and does not receive any unemployment benefits during the year.

In terms of demographic information, we have access to data on gender, age, education, marital status, and family composition. We can also observe if individuals are currently enrolled in school or not. Local labor markets are based on commuting distance, and Norway has 160 local labor market regions (Gundersen and Juvkvam 2013).[12]

Crucial to our analysis is the ability to link individuals to their children, something we do through a unique family identifier. By following these children over time, from compulsory school

[12] Local labor markets span more than one municipality (the lowest administrative unit consisting of 435 municipalities during our analysis period), but are typically smaller than counties (the second lowest administrative unit).

into college, we can examine the impact of parental labor market shocks on children's short-and long-run education outcomes as a function of the child's age at the time of the shock. In terms of outcomes, we focus on a broad range of educational outcomes: GPA at the end of compulsory school (grade 10), high school graduation, high school quality (as proxied by the minimum GPA required for admission to the specific school-program), high school behavior (absences during high school), college enrollment, and college quality (as proxied by the minimum GPA required for admission to the specific college-program).[13] Taken together, these outcomes provide a comprehensive overview of the impact of parental labor shocks on children's short- and long-term educational outcomes in terms of performance, attainment, and behavior – both on the intensive as well as the extensive margin.

For a subsample of our cohorts, we can also examine performance on low-stakes national tests in grade 5. These are considered low-stakes as they only serve to provide information on the relative development of the children and the quality of the school without having an impact on their educational opportunities. This is useful as it provides another key (low-stakes) administrative juncture through which we can examine the hypothesis that closeness of the shock to such junctures represents an important factor for the impact it may have on children.

In addition to the human capital outcomes, we can also follow a subsample of our cohorts (children older than 5 at the time of the displacement event) into the labor market and collect their earnings information at age 30. This allows us to estimate an aggregate reduced-form effect of the change in home environment on the children's careers, and provides a method for directly comparing the relative importance of child environment across a large range of child ages on earnings without having to infer such effects through the impact on educational outcomes.

Table 1 provides summary statistics for all of the child outcomes that we use in the analysis (Panel A) as well as the parent outcomes that we use when exploring mechanisms (Panel B). To facilitate the interpretation of our results, we provide these summary statistics separately for each of the three age groups (0-5, 6-10, and 11-16). The samples differ across age groups because not every child has gone through their entire childhood within the period we consider for measuring displacement (1986 through 2009). For example, some children would have been 0-5 before 1986, and therefore will not be in the sample of children potentially experiencing shocks at age 0-5. Note

[13] GPA ranges from 1 through 6 and is calculated by taking the average grade (1-6) of all courses that the student has taken in the given year.

that we do not require these outcomes to be similar across age groups as we compare treated and control individuals within each age group, and we provide extensive balance tests to demonstrate that treated and control individuals within each age group are balanced on observable characteristics in Section 4.1.

With respect to the child outcomes, the children in our sample appear largely representative of children in Norway (Tungodden and Willen 2022), and differences in these outcomes across the different age groups are small (see Appendix Table A-1). With respect to parent outcomes, we observe slightly different values of the outcomes of interest across the three age groups, with parents of older children having marginally higher income, a higher divorce rate, more children, and being less likely to move (see Appendix Table A-2). This is expected, as parents of older children likely are older themselves as well.

In Appendix Figure A-1, we show the distributions of income for the universe of parents of children aged 10 between 1986 and 2009, and for the set of parents in our sample. The main difference between these two samples is the employment condition we impose on our analytical sample (3 years of continuous employment prior to the potential job loss event). This eliminates the probability of 0 earnings in our sample, and shifts the distribution to the right.

As expected, because of these stringent employment requirements, parents in our sample are richer than those in the universe of parents with children of the same age. Therefore, in this paper we are estimating the impact of the timing of job displacement episodes for parents in the middle and top of the income distribution. With our sample restrictions, we cannot say what would happen to children whose parents are towards the bottom of the income distribution. Furthermore, social insurance programs are relatively less generous for those in the middle than those at the bottom of the earnings distribution, because replacement rates fall with earnings levels. Therefore, we do not expect the state to provide as much insurance to these individuals as a response to their displacement shocks as it would provide to those with lower earnings.

4. Empirical Strategy

Impacts of Job Displacement on Children. To perform our analysis, we utilize involuntary job loss events caused by mass layoffs and establishment closures among high-tenured employees. As discussed in Section 3, we define high-tenured workers as individuals who have worked continuously for three years prior to the potential displacement. We reduce the dimensionality of the problem by dividing childhood in three periods: early (ages 0-5), middle (ages 6-10) and late (ages 11-16). This is consistent with Carneiro et al. (2022). In the appendix, we show several results for disaggregated ages (Appendix Figure A-2 and Appendix Figure A-3).

Our empirical strategy is analogous to what is standard in empirical papers examining impacts of job displacement (e.g., Schmieder et al. 2022). The main difference is that we consider responses in education outcomes fixed in late adolescence (as opposed to studying responses in time-varying outcomes, such as employment or wages).

For our baseline estimates, we first define a set of base years, 1989 through 2006. We set relative time to equal 0 for all parents in that base year. We define our treatment group as children whose parents involuntarily lost their job due to a mass layoff or plant closing between relative time 0 and relative time 1. We define our control group as children with parents who did not lose their job due to a mass layoff or plant closing between relative time 0 and relative time 1. To ensure that our control and treatment groups are similar and comparable, we restrict the sample to children whose parents have worked continuously for the three years leading up to the base year. Thus, the parents in both the control and the treatment group consist of fulltime workers with a stable employment history.[14]

Using this sample of children, we compare the outcomes of children who experienced a parental job displacement between relative time 0 and relative time 1 to the outcomes of children who did not experienced a parental job displacement in that period. We estimate these regressions separately for each of the three child age groups. In all regressions, we include municipality, birth year of the child, and parental age fixed effects (our estimates are robust to including additional controls and fixed effects; see Section 4.3). This empirical framework gives us the impact of parental displacement at a particular age of a child (0 to 5, 6 to 10, and 11 to 16) on education outcomes in late adolescence. We then compare these results across age groups. The benchmark estimating equation is:

$$y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} + \varepsilon_{jbg}, \tag{1}$$

where b denotes the base year and g denotes the age group we are considering. y_{jbgqam} is the outcome for child j in birth year q, parental age a, and municipality m. $Displace_{jg}$ is a binary

[14] It is important to note that we do not impose any restrictions on the post-base year labor market behavior of individuals in our sample, as such restrictions would introduce a selection bias into the analysis. Thus, individuals in the control group (as well as individuals in the treatment group) could be involuntarily displaced in future years.

variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and zero otherwise. Equation (1) also controls for birth year (θ_{gq}), parent age (ρ_{ga}), and municipality ($\emptyset_{gm}$) fixed effects.[15] In the sensitivity analyses we present below, we add additional sets of fixed effects (e.g., industry fixed effects). These fixed effects control for systematic differences across birth years, parent age, and geographic location, that may be correlated with both parental displacement and outcomes.[16]

Our empirical approach assumes conditional random assignment of job displacement, after controlling for parental work histories and a detailed set of fixed effects. It is a strong assumption, under which we can identify the impact of job displacement at each age. We can then use these estimates to compare the relative magnitudes of the impacts of displacement at different ages. This approach is typical in studies of the intergenerational impacts of job displacement (discussed below) because child outcomes are measured at a single point in time, and do not vary before and after displacement. It has also been used in some recent studies of the impacts of job displacement on labor market outcomes of displaced workers (e.g., Schmieder et al. 2022).

To ensure that the conditional random assignment assumption is met, we impose a strong set of sample restrictions and rely on a rich set of controls. Specifically, we take parents in the same municipality, with the same age at displacement (or in the base year), and with similar work histories (continuously employed for the three years leading up to the potential displacement). We then assume that the only reason the outcomes of their children are different is because there was a displacement episode at a particular age of the child in one household, but not in the other. In support of this assumption, we show below that treatment and comparison children and their parents are identical along several characteristics beyond the ones we condition on (e.g., Apgar score, birth weight, gender, immigrant status, parental income, parental marital status, parental education). Consistent with this finding, controlling for more variables (or formally implement a matching estimator) yield similar results as our baseline results.

[15] Parental age and municipality of residence are calculated at the time of displacement for the treatment group, or at the time of potential displacement for the comparison group.

[16] One feature of the stacked job loss estimation approach is that children in the comparison group can appear in the sample multiple times (as long as their parent was continuously employed for three years before each age), because they could have been displaced at different ages. For example, for the 0-5 age group regressions, each comparison child could potentially appear up to 6 times in the sample, one for each age. Therefore, we cluster the standard errors at the child (or parent) level. In our robustness analysis we also estimate models where standard errors are clustered at the family level, explicitly taking into account that some individuals in our sample are siblings. Note that this is not a unique feature of our setting, but is a standard implication in the job loss literature.

It is worth noting that, for the purposes of this paper, we are mainly interested in the relative magnitude of the impacts of shocks occurring at different ages. While we provide strong evidence in favor of the conditional random assignment assumption and are convinced that the assumption holds in our setting, this assumption is stronger than what is required for our setting. Specifically, for the purpose of examining the relative effects across child age, we can relax this assumption and allow bias in the estimates as long as the bias is similar across the different ages.

We subject the estimates from Equation (1) to a rich set of robustness and sensitivity analyses which we discuss in detail below (including additional fixed effects, imposing stricter sample restrictions, and clustering the standard errors at more conservative levels), perform a balance test in which we estimate Equation (1) on a rich set of parent and child characteristics, and explore parallel trends among the children's parents prior to the displacement events. We note that results from these exercises provide further support for the robustness of our benchmark estimates from Equation (1).

The estimates in Equation (1) are interpreted as the impact of displacement on those experiencing the shock in a particular time relative to those not experiencing the shock in that same time. In terms of interpreting these effects, it should be noted that most of the control group (72 percent) is made up of children who never experience any displacement shock. This means that the counterfactual of a parental job displacement at a particular age in our setting is never experiencing a parental job loss instead of a job loss at another time. In addition, in the Appendix we report estimates of the impact of displacement based on the same equation (Equation (1)), but where the control group comprises only children (and parents) never experiencing an involuntary displacement throughout the child's first 17 years of life. Although this could in principle make treatment and control groups more dissimilar, it also makes it less likely that estimates of long-term impacts are contaminated by the fact that some of the control children eventually were treated. As we show below, our estimates using a pure control group are similar to our main estimates.

Impacts of Multiple Displacement Episodes on Children. There are several children who experience more than one job displacement shock from either parent during their childhood. From this sample we can investigate the impact of being subjected to different sequences of shocks on child outcomes. It is important to understand not only if the impacts of the shocks are cumulative, but also if they interact (e.g., if there is dynamic complementarity, as discussed in for example Cunha et al. 2010).

The intuition behind this analysis is to extend Equation (1) to include indicators not only for whether a child was subjected to a shock during a particular age range, but also whether the child experienced more than one shock across age ranges. With our three age ranges, there are seven combinations of job loss timing, conditional on a parental job loss. First, there are three combinations if a child experiences only one parental job loss at each of the three age ranges. Second, there are three combinations if a child experiences two parental job losses (age 0-5 and 6-10, age 0-5 and 11-16, age 6-10 and 11-16). Third, there is one combination if a child experiences a parental job loss in all three age ranges.

The identifying assumption for this analysis is that children are conditionally randomly assigned to each of these categories of shock exposure (conditional on our sample restrictions and the fixed effects included in the model). Under this assumption, we can interpret the estimates of the following equation as the causal impacts of being exposed to a sequence of shocks on child outcomes:

$$
\begin{aligned}
y_{jgqam} = {} & \beta_1 DisplaceAge0to5_{jg} + \beta_2 DisplaceAge6to10_{jg} + \\
& \beta_3 DisplaceAge11to16_{jg} + \beta_4 DisplaceAge0to5and6to10_{jg} + \\
& \beta_5 DisplaceAge0to5and11to16_{jg} + \beta_6 DisplaceAge6to10and11to16_{jg} + \\
& \beta_7 DisplaceAllAges_{jg} + \theta_q + \emptyset_m + \rho_a + \varepsilon_{jg}. \qquad (2)
\end{aligned}
$$

To test dynamic complementarity in this setting one could test, for example, whether the experience of one additional shock depends on the sequence of shocks one was exposed in other periods. Specifically, one could compare $\beta_5 - \beta_2$ (the additional impact of a shock at 0-5 for those experiencing a shock at 11-16) and $\beta_7 - \beta_6$ (the additional impact of a shock at 0-5 for those experiencing shocks both at 6-10 and 11-16). If dynamic complementarity is an important feature of the data, we would expect $\beta_7 - \beta_6 > \beta_5 - \beta_2$. There are, however, several other comparisons one may consider. It is possible that some comparisons provided suggestive evidence for dynamic complementarity while others do not. Below we comment on several of them.

Impacts of Job Displacement on Parents. After examining the effect of job displacement on children, we estimate the impacts of job displacement on parents. One important difference relative to prior estimates of job displacement in the literature is that we allow the effects to be a function of the age of the displaced individuals' children at the time of displacement. The goal of this analysis is to examine if differential effects across ages of children (controlling for the displaced individuals' own age) are driven – at least in part – by parents differently responding to the shocks based on the age of their children.

Exploring the parental adjustment paths is interesting because we know relatively little about how the age of the child at the time of shocks impact the parents' ability to adjust to changing labor market condition. For example, parents of toddlers may be more mobile, parents of young school-aged children may be more restrictive in terms of job search, and parents of teenagers may have accumulated relatively larger amounts of savings. As such, parental responses to adverse shocks – and ultimately how those shocks impact their children – may also differ depending on the age of the child at the time of the shock.

Whereas child outcomes are age dependent, and therefore are measured at a single point in time in our paper, parental outcomes can be observed repeatedly, before and after exposure to job displacement. By adding individual fixed effects to the estimation method, this allows us to rely on event studies and difference-in-differences. The underlying assumption in these models is that trends in these outcomes are common between exposed and non-exposed individuals, and that the outcomes of non-displaced workers (with similar work histories and with children of the same age) provide valid counterfactual trends for displaced workers. Formally, the estimating equation is:

$$\begin{aligned} y_{ibgt} = \alpha + \beta_g\left(Displaced_{ig} * Post_{igbt}\right) \\ +\delta_{1g}Displaced_{ig} + \delta_{2g}Post_{igbt} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}, \end{aligned} \tag{3}$$

where y_{ibgt} is an outcome for individual i at relative time t and base year b with a child in child age group g. Relative time is the difference between calendar year and base year. $Displaced_{ig}$ is a binary variable taking the value of one if the individual was involuntarily displaced in base year b and relative time 0, and zero otherwise. $Post_{igbt}$ is a dummy variable taking the value of one if relative time is greater than 0. The parameter β_g thus identifies the effect of involuntary job displacement on outcome *y*. Equation (3) also controls for year (γ_{gt}) and individual (λ_{ig}) fixed effects. The individual fixed effects control for time-invariant differences in observed and unobserved characteristics across individuals that may be correlated with displacement and the outcomes of interest. We estimate separate models for different *g* groups.

To explore the credibility of the common trends assumption, we use only pre-period data to estimate a set of pre-trend regressions of the following form:

$$y_{ibgt} = \alpha + \left[\pi_g * Displaced_{ig} * RelativeTime_\tau\right] \\ +\psi_g Displaced_{ig} + \delta_{g\tau} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}, \quad (4)$$

where $Displaced_{ig\tau}$ is an indicator variable taking value 1 if the individual is displaced in relative time $\tau = 0$, and zero otherwise. The π_g coefficient identifies relative pre-displacement trends. All other variables are defined as above. If π_g is statistically significant and economically meaningful, that implies that the control and the treatment group were on different paths prior to the potential job displacement episode, and that the control group cannot be used to identify a credible counterfactual of the treatment group and the treated individuals not been treated. Our decision to estimate these pre-trend regressions rather than full non-parametric event studies is based on our desire to parsimoniously summarize the evidence of the identifying assumption.[17] Consistent with our identifying assumption, π_g is a precisely estimated zero for all our outcomes.

4. Results

In this section, we present our main results. We begin by providing evidence to support the identifying assumption. Specifically, we show that pre-determined characteristics are balanced across treatment and control groups. Next, we turn to the main question of interest: whether the impact of parental labor market shocks on children's outcomes depend on the age of the child at the time of the shock. Moreover, we ask if there are differential effects depending on whether the mother or the father is the displaced worker, and whether boys and girls are affected differently. Lastly, given the dynamic nature of human capital accumulation during childhood, we ask what are the implications of exposure to multiple shocks at different times during childhood?

After exploring the impact of parental displacement on children as a function of their age at the time of displacement, we examine how parents themselves are affected by adverse labor shocks depending on the child's age. This analysis enables us to deepen our understanding of the mechanisms through which adverse shocks impact the skill formation of children. In addition, it sheds light on how children may constrain how parents respond following adverse shocks.

4.1 Balance Tests

[17] If we instead estimate full event studies, we would end up with three times as many figures (one figure for each age group and outcome instead of one figure for each outcome), making it more challenging to interpret the results. However, we have also estimated full event studies for all outcomes and age groups, and the results are highly consistent with the lack of any pre-trends that could bias our results. Results for employment and earnings are provided in Appendix Table A-15. Results for the other outcomes look similar and are available upon request.

The key assumption underlying our main analysis is that children of nondisplaced parents who have a similar work history to displaced parents, conditional on municipality, parental age, and child birth cohort, represent an accurate counterfactual of what the outcomes of children to displaced parents would have been had they not been displaced. This assumption is likely to hold as we utilize plausibly exogenous shocks due to involuntary job loss from firm closure and mass layoffs, such that there should be no selective sorting into the treatment and control group.

To examine the credibility of the empirical strategy underlying Equation (1), we begin by presenting a set of balance tests. Concretely, we use a set of pre-determined child and parent characteristics as outcomes of Equation (1). The results are shown in Figure 1. The treatment and control groups very similar at each age group, which provides strong support for the identifying assumption.

In addition to the balance test in Figure 1, we note that the job loss literature has developed a rich set of sensitivity checks and robustness analyses designed to examine the credibility of the job loss design (e.g., Huttunen et al. 2011; Del Bono et al. 2012; Huttunen et al. 2018; Willage and Willén 2022; Salvanes et al. 2022). In Section 4.3, we implement these exercises to ensure that our results are not biased, not driven by spurious correlations, and not caused by endogenous selection into establishments that are closing down or downsizing.

Taken together, these results provide strong support for the assumption of conditional random assignment, allowing us to interpret the effects as causal. However, it is worth noting that for the purpose of examining the relative effects across child age, this is a stronger assumption than we need. Specifically, we could in theory relax this assumption and allow bias in the estimates as long as it is similar across the different age groups.

4.2 The Effect of Parental Job Loss on Child Outcomes

High School Outcomes. Figure 2 shows the impact of parental job displacement at different ages on high school outcomes, obtained from estimating Equation (1). The outcomes we consider are 10^{th} grade (lower secondary) GPA, graduating from high school, high school program quality (as proxied by the minimum GPA of peers attending the same high school program), and high school behavior (absences). High school quality and high school absences are only observed for individuals who enroll in high school, but this is almost the entire population, so we do not expect any selection to bias these estimates.[18] As discussed above, we control for child birth year, parent age, and municipality fixed effects.

Each row corresponds to one of the outcomes listed above. In addition to showing results for all children irrespective of which parent experiences the labor shock (first column of each panel), we also provide figures stratified by whether the mother or the father experiences the job loss (second and third columns of each panel).

With respect to 10th grade GPA, parental job loss has an impact on children who are between 11 and 16 years old at the time of displacement. In terms of magnitude, the job loss event generates a drop in 10th grade GPA of 10 percent of a standard deviation for these children. This is a relatively sizable effect, on part with well-known education interventions such as class size reductions (e.g., Krueger and Whitmore 2001). The effect is larger if it is the mother rather than the father losing her job. In fact, for families where mothers are displaced, we also see a statistically significant, although smaller, impact of experiencing job loss at ages 0-5 on 10th grade GPA.

It is interesting that exposure to maternal labor shocks has a more detrimental effect on children's human capital development than exposure to paternal labor shocks. As fathers tend to hold a larger share of total household labor income, this suggests that the main mechanism through which adverse labor shocks impact children is not income. We explore this in greater detail below.

With respect to high school graduation, the estimated effect is not statistically significant in the overall sample. However, for children whose mothers experienced a job, we find small but significant reductions in the probability of graduating. One potential reason for the much smaller effects on (the extensive margin of) graduating high school relative to the (intensive margin of) lower secondary GPA result, could be that more than 80 percent of Norwegian children complete high school on time. Therefore, there may not be as much room to affect the extensive margin of high school completion.

Turning to the quality of the high school program (measured by the minimum 10th grade GPA of one's high school program), the pattern of results is similar to the results for 10th grade GPA. Specifically, parental job loss at ages 11-16 reduces the minimum GPA of a high school program by about 0.027 GPA points, or about 5% of a standard deviation. This effect is larger if

[18] Specifically, 98 percent of individuals completing compulsory school begins in high school that same year (e.g., https://www.udir.no/tall-og-forskning/publikasjoner/utdanningsspeilet/utdanningsspeilet-2019/videregaende-opplaring---fakta-og-laringsresultater/). High school graduation is considerably lower.

the mother loses her job. Maternal job loss also causes a statistically significant effect on program quality when children were less than 6 years old, although this effect is smaller in magnitude. Children who experience a parental job loss between the ages of 6 and 11 do not appear to be significantly impacted. These results reinforce the notion that maternal job loss appear significantly more detrimental to child development than parental job loss, and that there are two key periods during childhood – from age 0 through age 5 and from age 11 through age 16 – in which parental job loss may have detrimental effects on children's outcomes.

The final outcome we explore at the high school level is the number of school absences the child has during their years in high school. This is an interesting outcome, as it represents a behavior rather than a measure of performance or attainment. The results provide a picture similar to that for the other outcomes, both with respect to the relative effect across child age and with respect to heterogeneous effects across parent gender. Since absences reflect more of a behavior compared to the other outcomes we explore, we have examined this outcome in more detail to see where in the distribution this effect is coming from by estimating the effect on binarized variables taking the value of one if the number of absences exceeds 0, 5, 10, and so on up to 50. In that analysis, we see that the absence effects are driven by people in the middle of the distribution, such that they are not driven by extensive margin effects, and they are not driven by extreme outliers (rather, they are driven by children taking anywhere from 0 through 30 absences in a year).

Taken together, the results presented above demonstrate that the impact of parental labor shocks on children's outcomes is most severe if the child is older and closer to the age at which the key administrative junctures occur and the outcomes are measured. This finding is further reinforced in Appendix Figures A-2 and A-3 – in particular with respect to the intensive margin effects – in which we estimate effects separately for each child age and find that the effects grow stronger the close to the age at which the outcomes are measured.[19] However, shocks occurring during the early period of children's life also have lasting (albeit smaller) impact on their human capital development. We find strong evidence suggesting that most of these negative education

[19] For a subsample of our cohorts, we can also examine performance on low-stakes national tests in grade 5. These tests are low-stakes as they have no impact on children's educational opportunities. Despite the low-stakes nature of these exams, this supplemental analysis is useful as it provides another key administrative juncture through which we can collect additional evidence in favor of the hypothesis that closeness of the shock to such junctures represents an important factor for the impact it may have on children.

effects are driven by maternal job loss rather than paternal job loss. We explore potential mechanisms underlying this heterogeneity below.

There are two (related) reasons why these results are particularly remarkable. First, because the impacts of displacement on earnings are so persistent, early shocks affect household resources for children for many more years than later shocks. Second, since fathers earn more than mothers, the displacement of fathers brings about a greater reduction in household resources. The fact that impacts are larger for later shocks and for displacement episodes experienced by mothers suggests that our results are probably not driven by shocks to income. Again, we discuss this in greater detail this below.

Interestingly, we do not find any meaningful gender differences between boys and girls by age of displacement. These results are provided in Appendix Figure A-4, and it is striking how similar the effects are for boys and girls across the full age distribution.

Higher Education Outcomes. Figure 3 shows results obtained from estimating Equation (1) using college enrollment and college quality (as proxied by the minimum peer high school GPA in the specific college program attended by each individual) as dependent variables.

All results have been estimated using birth year, parent age, and municipality fixed effects. As in the case of high school outcomes, in addition to showing results for all children irrespective of which parent experiences the labor shock, we also provide figures stratified by whether the mother or the father experiences the job loss.

In terms of college enrollment, the impact of job displacement of mothers remains more important than the impact of job displacement of fathers, but there is considerably less variation in effect sizes across the child's age (at the time of the shock) compared with the secondary school outcomes. With respect to college quality, the pattern is similar to the results on 10th grade GPA.[20] Specifically, the figure shows that parental job loss has an impact on children who are at least 11 years old at the time of displacement, and that this effect is larger if the mother loses her job compared to if the father loses his job. There is also a statistically significant effect on children who are less than 6 years old at the time of displacement, though this effect is smaller and only present if the mother loses her job. Interestingly, the lack of extensive margin effects coupled with

[20] Note that college program selectively is only observed for those attending college. However, the impact of parental job loss on college enrollment is quite small, so the role of selection on program selectivity is likely not driving our estimates.

the existence of intensive margin effects with respect to higher education outcomes mirrors the findings from the high school analysis.

Earnings Effects. Figure 4 shows results obtained from estimating Equation (1) using earnings at age 30 as the dependent variable. This outcome measures the overall impact of all observed (and unobserved) effects that a change in childhood environment has on the child's long-term labor market performance. It should be noted that data limitations prevent us from estimating this effect on our entire sample. This is because we can only observe this outcome for children who have turned 30 prior to 2018. In practice, this means that we have no age 30 earnings observations for children whose parents lose their jobs at age 0, we lose about 95 percent of the sample for those whose parents lose their jobs at age 1, and we lose an overall 85 percent of the sample from our youngest cohorts (age 0 through 5). Because of these compositional changes and power challenges, we are unable to estimate the earnings effects on our youngest cohort.

The results for the earnings regressions have been estimated using birth year, parent age, and municipality fixed effects. As in the case of the education outcomes, in addition to showing results for all children irrespective of which parent experiences the labor shock, we also provide figures stratified by whether the mother or the father experiences the job loss.

The results from the earnings analysis demonstrate that the impact of job displacement of mothers remains more important than the impact of job displacement of fathers. Similar to the higher education outcomes there is less variation across the child's age (at the time of the shock). Having said that, the effect remains the largest for children who are exposed to shocks in early adolescence (age 11 through 16). This is a particularly important result, suggesting that even in the very long-run – when all observed and unobserved direct effects of the shocks on children have actualized –the impacts of changes in child environment are larger for children who were in early adolescence when they experienced the shock. When more data become available, we see it as an important extension to examine how the job loss shocks at age 0 through 5 translate into age 30 earnings.

Effects of Multiple Shocks. In this part of the paper, we investigate the impact of different sequences of shocks, including multiple shocks. This is because there are children who are exposed to more than one parental job displacement episode during their childhood.

The identifying assumption underlying this analysis is that conditional on our controls (birth year, parent age, and municipality) and sample restrictions, the timing and frequency of shocks that one is exposed to during childhood is random. Again, the reason why this is a plausible assumption is because the shocks we explore are induced by mass layoffs or plant closures which are outside the control of families, and our sample is restricted to workers with a strong attachment to the labor market. To examine the plausibility of this assumption, we first present results from a balancing exercise which show that the characteristics of children and families exposed to different timing and sequences of shocks are similar in terms of pre-displacement characteristics (see Appendix Figure A-5).

In Figure 5, we explore the implications of exposure to multiple parental labor market shocks during childhood. In particular, each bar in each panel of Figure 5 shows the average outcome for children never exposed to a displacement episode, those exposed to a parental shock in only one of the three age bins, those exposed to parental shocks in two of the three age bins, and those exposed to a parental shock in each of the three age bins.[21] We can then compare the different bars in the figure.

The results provided in Figure 5 demonstrate that for lower secondary GPA, and for the quality of the high school and college programs, more shocks typically lead to worse outcomes. Interestingly, this does not appear to be the case for high school graduation, college enrolment and number of absences in high school, although our benchmark results also show much smaller impacts on these extensive margin outcomes.

The patterns are similar for lower secondary GPA, high school quality, and college quality. For these outcomes, there are almost no meaningful differences between those experiencing no displacement shocks, and those experiencing only one shock at ages 0-5 or 6-10. However, those experiencing a displacement shock at 11-16 have worse outcomes. For these three outcomes, experiencing two shocks is worse than experiencing a single parental job loss at ages 0-5 or 6-10, and similar to experiencing a parental job loss at 11-16. Finally, a job loss in all three age ranges results in the worse outcomes of all. For the fourth outcome, high school graduation, the outcomes are not particularly different across the different combinations of parental job shocks.

Some of the results for GPA and program quality are suggestive of dynamic complementarity, but this pattern is not universal. For example, the impact of a shock at 0-5 (6-

[21] Since we are breaking the data into many more cells, and several of the cells corresponding to multiple shocks are small, lack of statistical power prevents us from reliably examining the effect of multiple shocks separately by mothers and fathers.

10) is larger for those already experiencing a shock at 6-10 (0-5), than for those not experiencing any displacement shock, indicating that impacts of further shocks are larger for those already experiencing prior shocks (complementarity). However, adding a shock at 0-5 or at 6-10 to those experiencing no shocks has a similarly negligible impact on outcomes than adding such a shock to those already experiencing a shock at 11-16.

4.3 Robustness, Sensitivity and Extensions

Robustness and Sensitivity. The main assumption underlying our core findings is that children of nondisplaced parents represent an accurate counterfactual of what the outcomes of children to displaced parents would have been had they not been displaced (conditional on our sample restrictions and fixed effects). This assumption is likely to hold as we utilize as identifying variation plausibly exogenous shocks triggered by involuntary job loss from firm closure and mass layoffs affecting individuals with similar work histories and living in the same municipality, such that there should be no selective sorting into the treatment and control group.

To provide evidence in support of these assumptions, we showed in Figure 1 results from balance tests on a rich set child and parental characteristics. In addition to the balance test in Figure 1, we note that the job loss literature has developed an extensive set of sensitivity checks and robustness analyses designed to examine the credibility of the job loss design (e.g., Huttunen et al. 2011; Del Bono et al. 2012; Huttunen et al. 2018; Willage and Willén 2022; Salvanes et al. 2022). In this section, we implement these exercises, which suggest that our results are not biased, not driven by spurious correlations, and not caused by endogenous selection into establishments that are closing down or downsizing.

In Appendix Figure A-6, we show that the results are unaffected by limiting the analysis to larger firms (sequentially restricting our sample to establishments with more than 30, 40, and 50 employees). This exercise is important for ensuring that the effects we identify are not driven by false mass layoffs and establishment closures.

In Appendix Figure A-7, we show that the results are robust to clustering at the municipality level. Here, we allow the error component to be correlated among individuals within the same municipality. This adjustment has no meaningful impact on the precision of our estimates.

In Appendix Figure A-8, we calculate propensity scores based on the pre-displacement period and show that our results are robust to restricting the sample to those in the common support region of the propensity score. We pursue this exercise in an effort to obtain treatment and control groups that are as comparable as possible, ensuring a meaningful interpretation of the results. By eliminating observations outside the common support region of the propensity score, we ensure that our results are not being driven by treatment and control units that are very different from each other and have little overlap in terms of background characteristics.

In Appendix Figure A-9, we show that accounting for early leavers (individuals who leave the plant one year before the closure/layoff, potentially in anticipation of the event) does not change the results. This exercise is important for ensuring unbiased estimates, as "early leavers" may be positively selected.

In Appendix Figure A-10, we show that the results are unaffected by relaxing the conventional job requirement in the job loss literature – that individuals must have been full-time employed in the three years leading up to the base year. This is an important finding, demonstrating that we are not estimating a very specific local average treatment effect, and that our results extend to children whose parents are less attached to the labor force as well.

In Appendix Figure A-11, we show that the results are unaffected by including a richer set of control variables including child birth month, child sex, parent sex, parent education, parent Norwegian born, and pre-period income as well as robust to the incorporation pre-period industry fixed effects.

In Appendix Figure A-12 we examine what happens to our results if the control group consists only of children never exposed to displacement shocks during their entire childhood. These estimates are consistent with our main results.

In addition to the robustness checks discussed above, we also pursue an alternative estimation strategy that relies on weaker identifying assumptions than our baseline method, exploiting only the timing of shocks across all children who ever have been exposed to a parental job loss due to mass layoffs or plant closures. Specifically, we restrict the sample only to those children who have ever experienced a parental shock, and estimate the following equation:

$$\begin{aligned} y_{jgqam} = \alpha + \beta_1 TreatAge0to5_{gj} + \beta_2 TreatAge11to16_{gj} \\ +\theta_{gq} + \emptyset_{gm} + \rho_{ga} + \varepsilon_{jgqam}, \end{aligned} \tag{5}$$

where θ_{gq} denotes birth year-by-child age group fixed effects, ρ_{ga} denotes parent age-by-child age group fixed effects, and $\emptyset_{gm}$ represents municipality-by-child age group fixed effects. The treatment age group 6 to 10 is omitted from the equation and serves as the baseline treatment effect.

The thought experiment underlying Equation (5) is to imagine two parents of the same age, with the same employment history who live in the same municipality and are born in the same year, who have children of the same age and both parents were exogenously displaced due to a mass layoff or plant closure, but one parent was displaced when their child was young and the other was displaced when their child was older. The identifying assumption underlying Equation (5) is thus that the age of the child at the time of the parental displacement is random across families that were ever displaced.

While Equations (5) relies on weaker identification assumptions than Equation (1), the estimates we obtain abstract away from any level effects associated with parental job loss, instead focusing on patterns between children's ages. Specifically, as all individuals are exposed to a parental job loss in these regressions, the effects we recover are relative effects across child age, absent any overall effect that parental job loss may have on children.

Results obtained through the estimation of Equation (5) are provided in Appendix Figure A-13. The robustness of our results to the use of these alternative estimation approaches is consistent with the notion that the effects are not driven by endogenous selection into treatment.

Taken together, the extensive set of robustness checks, sensitivity analyses, and alternative estimation approaches shows that our key assumptions are likely to hold, and that our main results can be safely interpreted as the causal impact of displacement shocks at different ages on the education of children.

Extension. We note that all child outcomes explored above are measured at age 16 or later. Ideally, we would like to compare and contrast the impact on these outcomes with the impact on outcomes measured at an earlier stage of the children's human capital development. This would allow us to better understand not only the importance of critical learning periods, but also to understand the relationship between effect size and key administrative junctures. To provide some suggestive evidence on this, we note that we have information on student performance on low-stakes national tests in English, Norwegian, and Mathematics, in grade 5 (age 11) for some of the years of our sample period. Even though these tests are low-stakes exams, we believe this provides an interesting early measure of student performance through which we can further examine the impact of key junctures and critical learning periods.

The results from estimating Equation (1) using the children's performance on these national tests as outcomes are provided in Appendix Figure A-14. The results provide two key take-aways. First, both children who are exposed to parental labor shocks in the early years (age 0 through 5), and children who are exposed to parental labor shocks in pre-teen years (age 6 through 10), perform worse on these low-stakes national exams. Second, closeness to key administrative junctures appears to matter, as the effects are marginally larger for the middle age cohort relative to the young age cohort. This is particularly interesting as this is the only outcome that we examine in which the effect is larger for children who experience the change in home environment in the 6-10 age range. That these effects do not translate into large higher education and earnings effects among the middle age cohort is likely because the exams are low-stakes and have no impact on child educational opportunities (which is why the direct effects for age 6 through 10 do not extend into their working lives).

4.4 The Role of Parental Education

We next investigate if there are heterogenous effects by parental education. It is possible that parents with high human capital are better able deal with the consequences of job loss. For example, more educated individuals are more mobile, may have larger work networks, and may possess skills that are more easily transferable to other occupations. Thus, they may find it easier to access new jobs following involuntary job separations.

On the other end, job loss may also involve more stress among high-educated individuals who likely experience more employment protection in general, and who may be less used to dealing with adverse shocks. In addition, they may experience lower replacement rates from unemployment benefits and other welfare programs, and they likely earn above the benefit caps in these programs prior to displacement. To examine this is more detail, we stratify our results based on the parent's level of education. To simplify the analysis, we focus on two levels of education: at most a high school diploma and more than a high school diploma.

The results from this exercise are presented in Figure 6. The results suggest that the effects identified in Figure 1 are disproportionately driven by children of highly educated parents, both in terms of magnitudes and age patterns. This could either be because the home environment in itself makes children more vulnerable to these shocks – because the size of the shocks is different for parents with high and low levels of education – or because more and less educated parents respond differentially to shocks as a function of their child's age.

4.5 Possible Mechanism – Parents' Adjustment Paths

To better understand the channels through which the effects of parental job loss on child outcomes operate, we follow the children's parents over time and use a difference-in-differences approach to compare changes in parental outcomes among those who experienced an involuntary job separation relative to those who did not (Equation (3)). This exercise also helps us to understand how children may constrain parents' adjustment paths following adverse shocks.

Parental Labor Market Effects. In Figure 7, we document the impact of involuntary job separation on the employment and earnings of parents as a function of their children's age at the time of the shock, for the whole sample as well as separately for mothers and fathers.[22] These results have been generated by estimating Equation (3), which includes both time as well as individual fixed effects. The individual fixed effects control for time-invariant differences in observed and unobserved characteristics across individuals that may be correlated with displacement and the outcomes of interest.

With respect to employment, there is a clear negative effect for both mothers and fathers across the age spectrum of their children. The effect amounts to approximately 10 percentage points independent of the age of the child. Notable is the difference between the mother and father for the early ages of the child. Specifically, the reduction in employment is significantly larger for mothers up to the school starting age of 6, after which the effect difference between mothers and fathers converges. This result resembles the finding in Angelov et al. (2016). This differential effect could partly explain why we have stronger effects on child outcomes for maternal than for paternal job loss episodes.

Turning to labor market earnings, there is an economically meaningful and statistically significant negative effect of being displaced both among mothers and fathers across the age distribution of children. The negative earnings effect is approximately 10000 NOK, and is similar for fathers and mother for children up to the age of 10, after which the effect becomes slightly larger for fathers. These parent-specific earnings effects are within the range of earnings effects that have been identified for average workers in the US and in other OECD countries, though

[22] Estimates of pre-trends based on Equation (4) are available in Appendix Figure A-15. These estimated slopes of the pre-trends are precisely estimated zeros.

effects in the US tend to be slightly larger on average (e.g., Jacobsen et al. 1993; Couch and Placzek 2010; Davis and von Wachter 2011; Huttunen et al. 2011; Salvanes et al. 2022).[23]

Interestingly, the earnings and employment effects of displacement are relatively stable across the age of the child at the time of displacement. This is perhaps what one would expect, since our assumption is that these shocks hit families with children at different ages at random. It is, however, conceivable that the reaction of parents to these shocks vary according to the age of their children, which could make the overall impacts of the shocks very different depending on the age of the child at the time of displacement.

Consistent with previous work on the employment effects of job displacement, a formal event study analysis on the employment and earnings effects of displacement for parents show that the employment effects recover relatively quickly, while the earnings effects persist for several years (Appendix Figures A-16). We extend this analysis with additional years in Appendix Figure A-17 and show that part of the earnings loss remains even a decade after the event took place. This is important because it means that although early and late shocks have the same magnitude in the short run, early shocks affect children for a much longer period than late shocks. In Figure 8, we show the impact of experiencing displacement at each age on the total (discounted) household earnings across the entire childhood, which is much larger for early than for late shocks. This supports the idea that income is not the driving mechanism, because shocks occurring in late childhood have much larger impacts on child outcomes than those occurring at earlier ages.

Another way through which we can explore the relative importance of the income component of the shock is to stratify the sample based on whether the parent who experienced the shock was the family primary earner or not. Interestingly, we find no difference in the child human capital effects depending on whether the parent was the breadwinner of the family or not. We show a few of these results in the appendix (Appendix Figure A-18). The result from this supplemental analysis serves to further support the idea that income is not the driving mechanism behind the child effects we identify. This is particularly interesting given the strong link between parental income and child development that has been identified in seminal papers in the past (e.g., Dahl and Lochner 2012). However, these papers have not necessarily distinguished between child ages.

[23] The relatively large and persistent earnings losses may be due to both reduced hours worked as well as a lower hourly wages. The literature mostly finds support for lower hourly wages, although also less hours depending on the age of the worker (Halvorsen et al. 2022). Loss of firm specific and sector specific human capital as well as worse employer-employee matches are most likely explaining the reduced hourly wage (Huttunen et al. 2011).

Parental Labor Market Adjustment Paths. In Figure 9, we study potential parental adjustments to the adverse employment shocks that they experience as a function of their child's age at the time of the shock: mobility, education, fertility, and disability pension. In addition to helping us understand the mechanisms through which adverse shocks impact the skill formation process of children, this exercise allows us to better understand how children of different ages may constrain parents' responses following adverse shocks.

First, parents may respond to adverse employment shocks by moving to a new regional labor market in search for better job opportunities; something that both can mitigate the consequences of job loss and impact the human capital development of children (Huttunen, Møen and Salvanes 2018). In the first row of Figure 9, we examine the impact of involuntary job separation on regional mobility as a function of the child's age. The results demonstrate that both mothers and fathers exhibit a regional mobility response to adverse labor shocks, though the impact on fathers is greater; particularly in the early pre-school years. We speculate that the large drop in the mobility response at the time children start school is due to the potential disruption effect that parents think their children may experience if they have to switch school. However, despite the clear patterns, it is important to emphasize that the magnitude of the effects are relatively modest, with job loss shifting the mobility behavior of parents with at most one percentage point.

Second, it is well established that adults often go back to school to complete a degree following an involuntary job separation (Bennett et al. 2020; Minaya et al. 2020; Salvanes et al. 2022). One likely explanation for this behavior is the desire to reduce the future risk of losing a job by investing in human capital. This adjustment response to an involuntary job separation may depend on the child's age and whether the child is in school, and it may also differ for mothers and fathers. Specifically, existing research has shown that (1) males and females face disparate careers trajectories due to factors such as family formation, educational investment, mobility preferences, and retirement,[24] (2) that men and women differ in career and life choices related to job search, commuting, and childcare,[25] and (3) that there are non-trivial child penalties and "mommy gaps".[26]

In the second row of Figure 9, we see a small effect of job loss on returning to school, though the magnitude of this effect is relatively modest and does not appear to differ substantially

[24] E.g., Kleven et al. (2019); Manning and Swaffield (2008).
[25] For job search, see Cortes et al. (2021). For commuting, see Le Barbanchon et al. (2020). For childcare, see Ellingsæter and Kitterød (2021) as well as Thomas (1994).
[26] E.g., Angelov et al. (2016); Kleven et al. (2019).

between mothers and fathers. However, an interesting result is that the effect on mothers appears to increase as their children enter their early teenage years. While this could be driven by the fact that mothers tend to serve as primary caregivers and that they free up a significant amount of time as their children grow up and become more independent, this is purely speculative.

Third, an involuntary job separation and a decline in earnings could also generate a change in fertility (e.g., Huttunen and Kellokumpu 2016). For instance, the opportunity costs of having children may change as a direct effect of job loss. In the third row Figure 9, we see that fertility is not strongly responsive to job loss. At very young ages, mothers have small increases and fathers have small decreases. Fathers' fertility is unaffected by job loss if it occurs when their current children are above pre-school age. However, fertility for mothers increases following a job loss that takes place when their current children enter school, and the magnitude declines as her children enter adolescence. We speculate that this may be because mothers' who lose their jobs when their children are very young are constrained both in terms of financial resources and time (having to take care of a toddler), such that having an additional child at this point becomes less desirable. However, as the child grows up, the mother has accumulated more resources, and can dedicate less time to children in school, such that having an additional child becomes more attractive. Finally, fertility spacing of ten or more years may be undesirable.

Fourth, an involuntary job loss may lead individuals to permanently exit the labor force through other social security and welfare programs, such as disability pension (see Section 2 for details about this program). In the fourth row of Figure 9, we see that both fathers and mothers experience an increase in exiting the labor force on disability benefits following a job loss when their children are teenagers, and that it is marginally larger for fathers. Parents that lose their jobs when the children are younger do not display any effects. One potential reason for this effect pattern is that parents of young children are in need of greater financial resources and feel a greater financial obligation to their children such that they are less willing to permanently exit the labor force. Parents of teenagers – who are soon-to-be financially independent – may not feel that same pressure and obligation and are therefore more willing to consider permanent exist as an option to adverse labor shocks.

Taken together, the results from the analyses above clearly show that the age of the child at the time of the parental labor market shock does impact the way in which the parent chooses to respond to that shock. However, the results also demonstrate that the differences in effects among parents with differently-aged children are economically modest, and are unlikely to explain the differential impact on the skill formation process of children.

Finally, in addition to the age of the child mattering for the impact of the shock, it is possible that the age of the parent at the time of the shock has an impact on their ability to respond to the shock, and thus how it transmits to the child's human capital formation (Salvanes et al. 2022). To shed light on this, Panels A and B of Appendix Figure A-19 show the income and employment effect on parents for each of the child age groups depending on whether the parent was above or below the mean parental age at the time of child birth. Panels C and D then show effects on children's human capital accumulation using the same identification strategy.

Looking across the figure, it is evident that children who had parents that were relatively older at the time of child birth bore more of the shock impact than children who had parents that were relatively younger (Panels C and D). Interestingly, this does not appear to operate through differential impacts on the employment and earnings dimension of the parents. Specifically, there is no statistically significantly different impact on the employment effect as a function of parent age within each child age, and while there are some differences on the earnings dimension, these are not economically meaningful when examining them as a function of the mean income within each group (as younger parents have lower income on average than older parents). This suggests, again, that the resource impact of the labor shock is not the main driver behind the differential human capital effects on children across age groups. While our focus is on child age, and earlier papers have looked at own age (e.g., Salvanes et al. 2022), we view this as an interesting dimension to explore in future work.

Parental Health Effects. Our two most striking findings are that the impacts of shocks in late adolescence are larger than in other ages, and that the impacts of maternal shocks are larger than the impacts of paternal shocks. These findings are puzzling for different reasons.

With respect to the first finding, this is a puzzling result because even though the short-term impact of shocks on the employment and earnings of parents is similar for children of different ages, the shocks are long-lasing and therefore affect many more years of childhood the earlier they occur (see also Figure 8 discussed above). However, the largest impacts of the shocks are in the later period of childhood, closer to the time when we measure our outcomes, which suggests that income may not be an important driver of these effects.

Regarding the second finding, this result is interesting because the impact of displacement on employment, earnings, and several other family decisions are similar regardless of who the displacement episode is affecting: mothers or fathers. Therefore, it is not easy to explain why impacts are larger when shocks affect mothers rather than fathers.

In this section, we show that one plausible explanation for both these puzzles concerns the potential impact that adverse labor shocks have on the mental well-being of parents. Prior research has demonstrated that such shocks may generate negative health behaviors (e.g., Black et al. 2015), induce psychological stress (e.g., Østhus 2012), and reduce subjective well-being (e.g., Song 2018). If such psychological effects are larger for mothers than fathers, that could potentially shed light on why maternal job loss appears more detrimental to child development than paternal job loss.[27]

To examine this question, we merge our analysis data with mental health data on parents. These data come from the Cohort of Norway data and the National Health Screening Service's Age 40 Program data, two population-based national surveys conducted between 1988 and 2003. The surveys contain information from a survey with questions regarding mental wellbeing. The goal of the surveys was to document the health of all men and women between the ages of 40 and 42 across Norway, with a response rate of between 55 and 80 percent.[28] We use information from both surveys as most of the same information was collected across these two surveys. These data enable us to analyze self-reported mental health as a function of involuntary job displacement for a subset of individuals in our sample. In terms of outcomes, we focus on mental health characteristics that plausibly can be affected by negative labor market shocks: anxiety, nervousness, sleeplessness, and depression. Note that we are unable to examine these outcomes separately by child age due to sample limitations as well as the specific age of individuals that the surveys target.

The results from this supplemental analysis are provided in Table 2, in which we estimate versions of Equation (1) on the parent-level with the above health outcomes as the dependent variables. First, the results demonstrate that displaced mothers experience significant negative mental health effects because of involuntary job displacements, while fathers do not. In particular,

[27] Due to, for example, the tendency of mothers to invest and interact more with their children such that the added burden of job loss weighs heavier on them.
[28] While the Age 40 Program exclude individuals in Oslo, the Cohort of Norway data includes individuals in Oslo.

mothers are much more likely to experience sleeplessness and nervousness, two mental health traits strongly linked to stress-induced events such as job displacement. In addition to providing strong suggestive evidence on the mechanisms through which the differential effects of maternal and paternal job loss impact children, these results serve to broader our understanding of gender-specific implications of adverse labor market shocks. We see this as an important area for future research in the field.

Second, these negative mental health effects are not long lasting. Specifically, Appendix Table A-3 shows results from estimating the same health regressions for mothers but examining health effects five through seven years after the shock. The results in Appendix Table A-3 illustrate that none of the stress effects are present in the long-run. This provides us with a short-run channel that can explain why later shocks have larger impacts on late adolescence outcomes in spite of having much lower impacts on cumulative home resources during childhood.

6. Discussion and Conclusion

Children's surroundings and home environments matter for their development and later-in-life outcomes. However, different stages of childhood are associated with the formation of different types of skills, and there might be particularly sensitive periods of learning during childhood in which critical human development advances take place. Furthermore, the dynamics of skill accumulation can be such that investments and shocks in different periods can be substitutes or complements.

In this paper, we estimate the causal effect of changes to the home environment at different stages of children's upbringing based on the same change, comparing similar children, in similar settings and time periods. To do so, we exploit parental job loss induced by exposure to mass layoffs and establishment closures as a function of child age, and we use this as an exogenous source of variation for changes in family environments over the child's life. In addition, using data from children experiencing more than one displacement shock in childhood, we extend this analysis by examining the impact of facing different sequences of shocks in childhood on education outcomes in late adolescence.

Our findings challenge the view that shocks to early childhood environments have larger impacts on human capital development than shocks occurring later in the life of the child. Specifically, for a number of consequential educational outcomes we study – including GPA, high school achievement and college selectivity – parental job loss in the adolescent years has larger impacts than parental job loss occurring at any other point in the child's life cycle. In addition, by following a subsample of these children into the labor market and examining their earnings at age 30, we show that children who experience the change in home environment in early adolescence are substantially more affected than children who experience the change in home environment at age 6 through 10. We rationalize these results by noting that adolescence coincides with key junctures in children's human capital development (choice of high school program, national tests, etc.) in which abrupt changes to the home environment may be particularly disruptive. Indeed, among the adolescent sample, we find that the effects are larger the closer the child is to key educational outcome junctures. Our results therefore show that maximization of the return to human capital investments is not simply a matter of investing as much as possible as early as possible.

In terms of policy implications, we view our paper as opening up a new avenue of research on the interaction of adverse labor shocks and child development as well as family structure, and as providing valuable information to policymakers on how to reduce the constraining impact that children may have on their parents' ability to respond to negative shocks. These are central questions for the design of social insurance programs.

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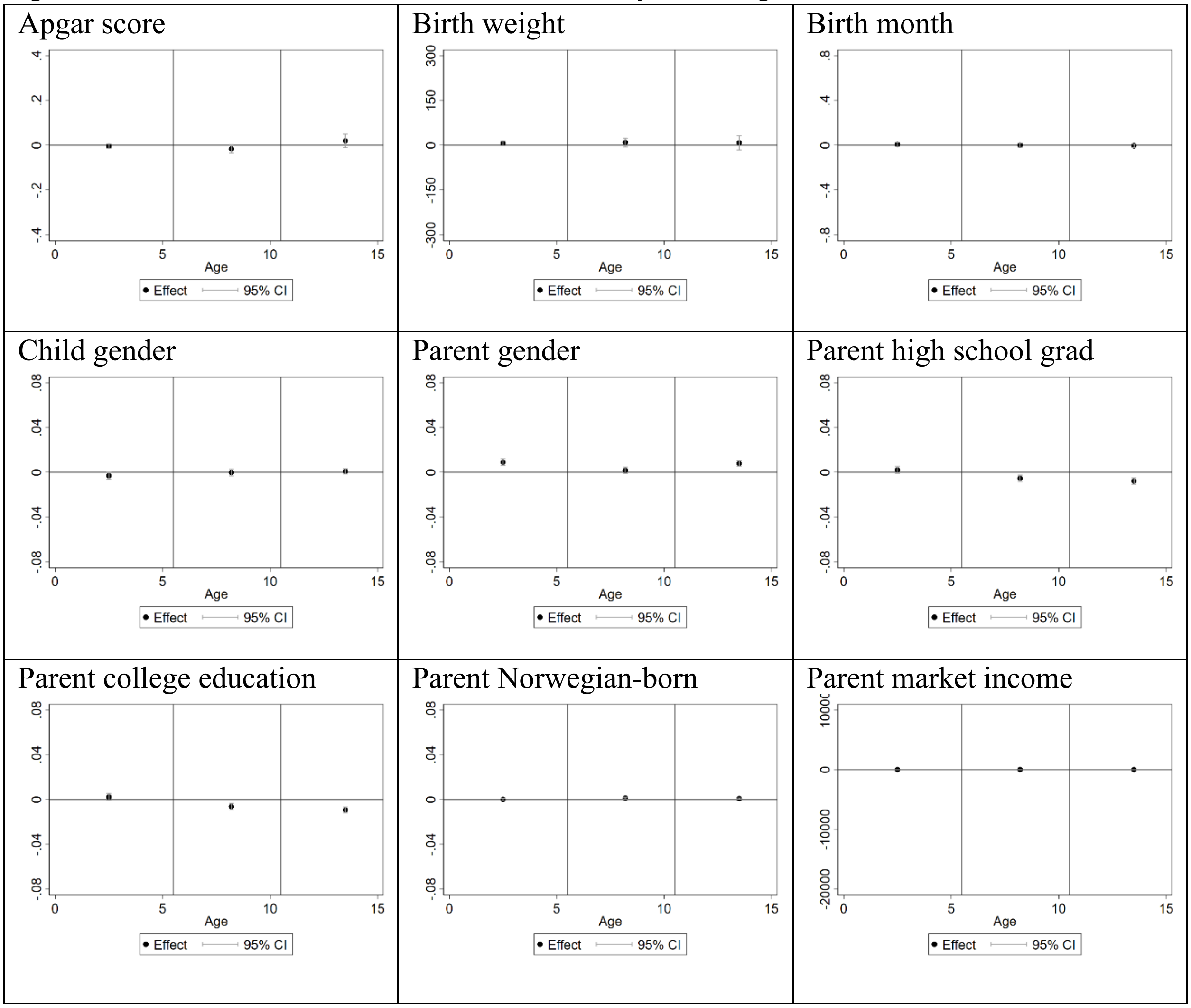
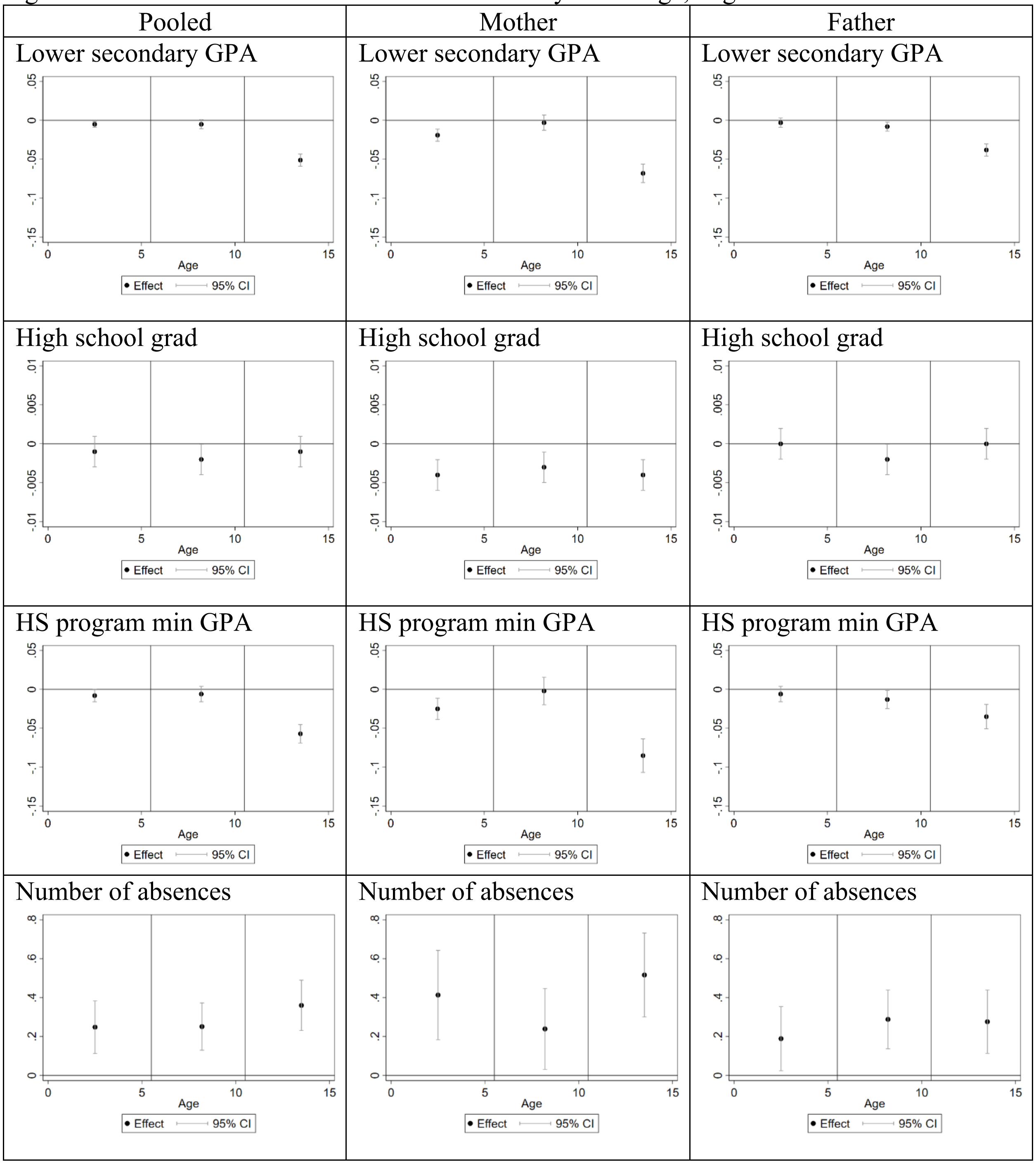
Figure 1: Effects of Parental Job Loss on Children by Child Age, Balance Test



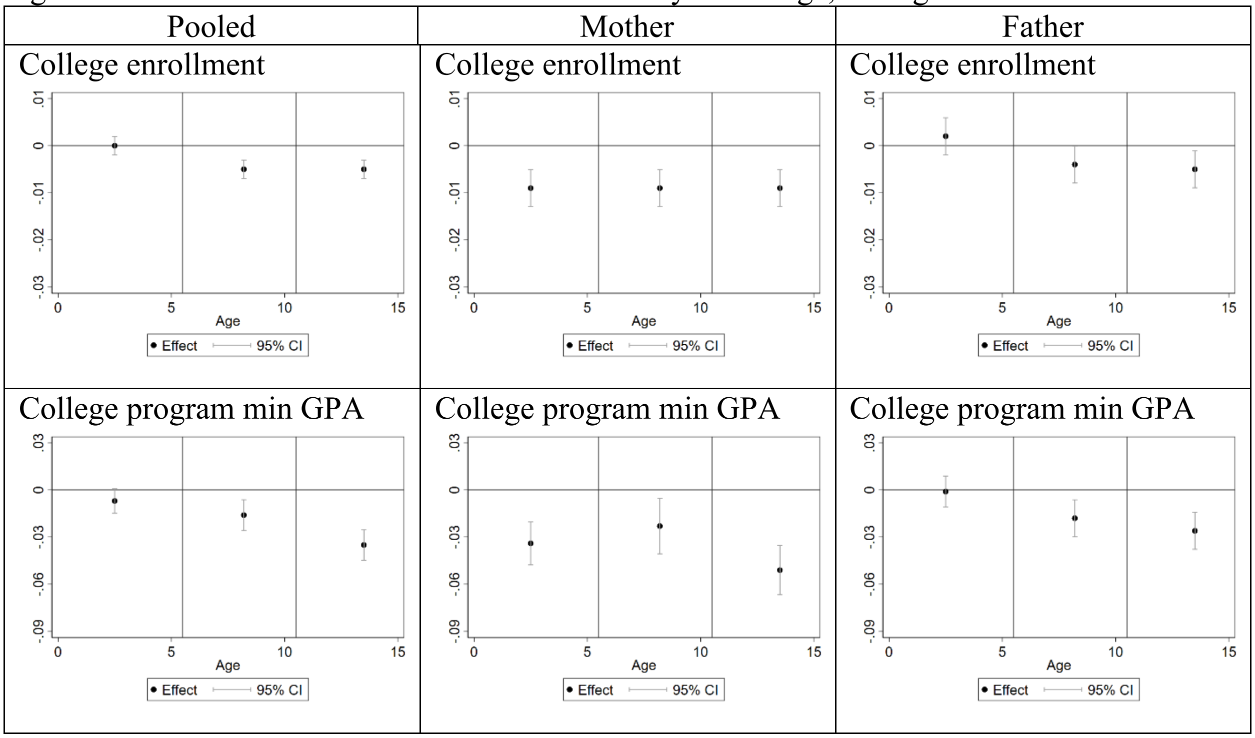
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g}Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Figure 2: Effects of Parental Job Loss on Children by Child Age, High School



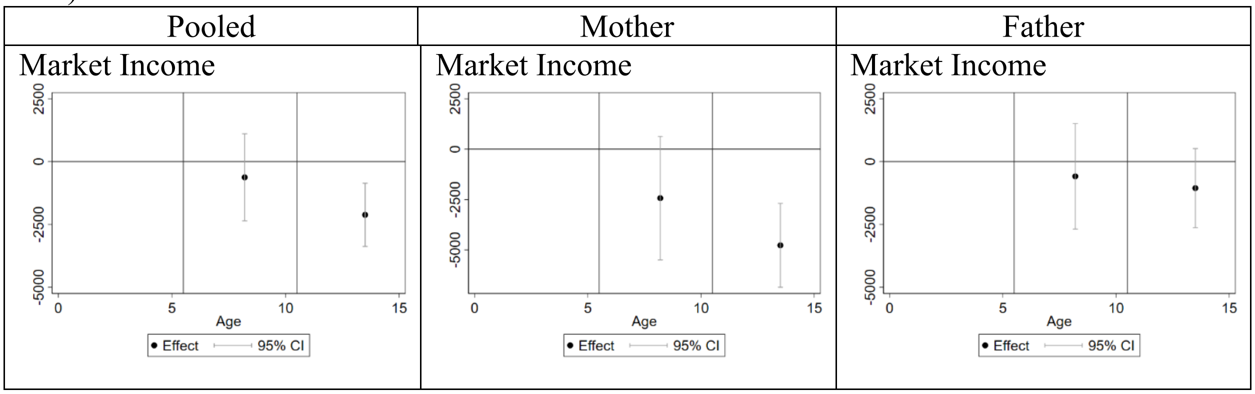
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Figure 3: Effects of Parental Job Loss on Children by Child Age, College



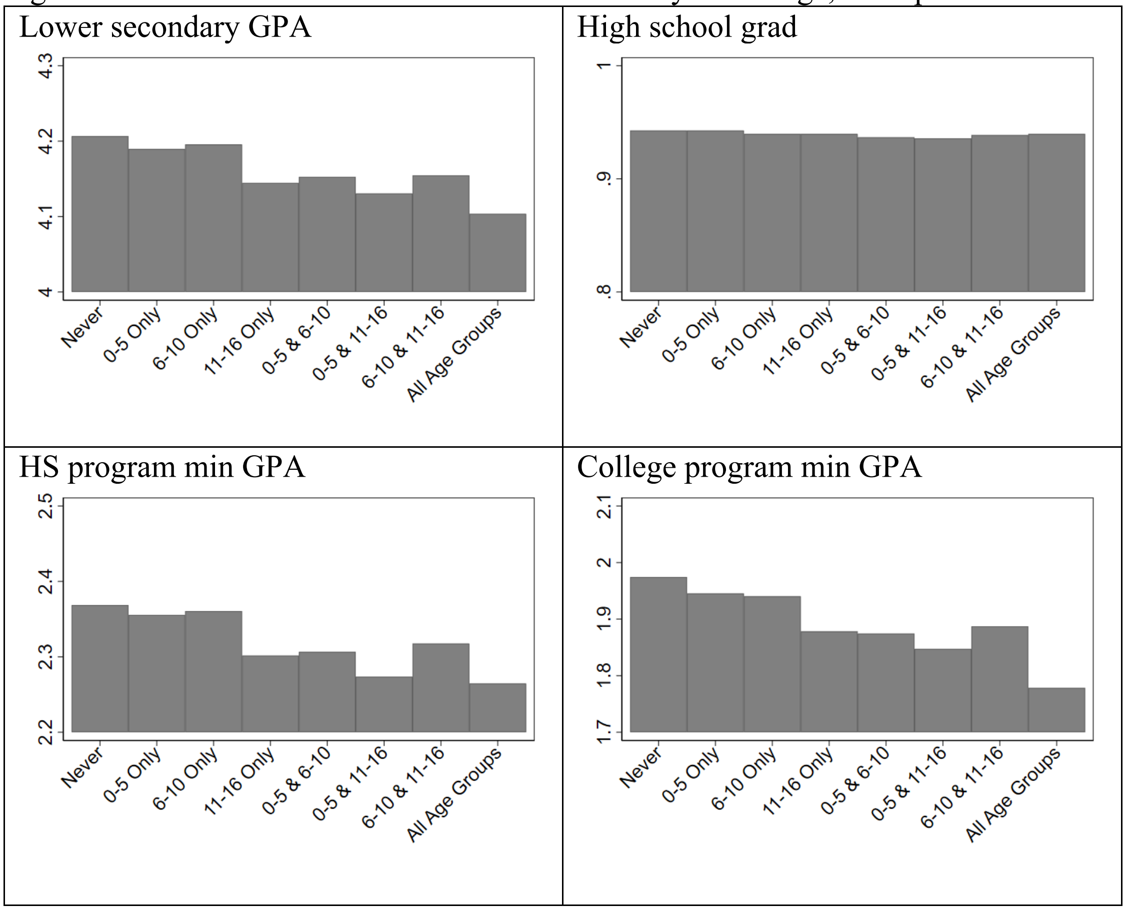
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Figure 4: Effects of Parental Job Loss on Children by Child Age, Market Income Age 30 (NOK 1000)



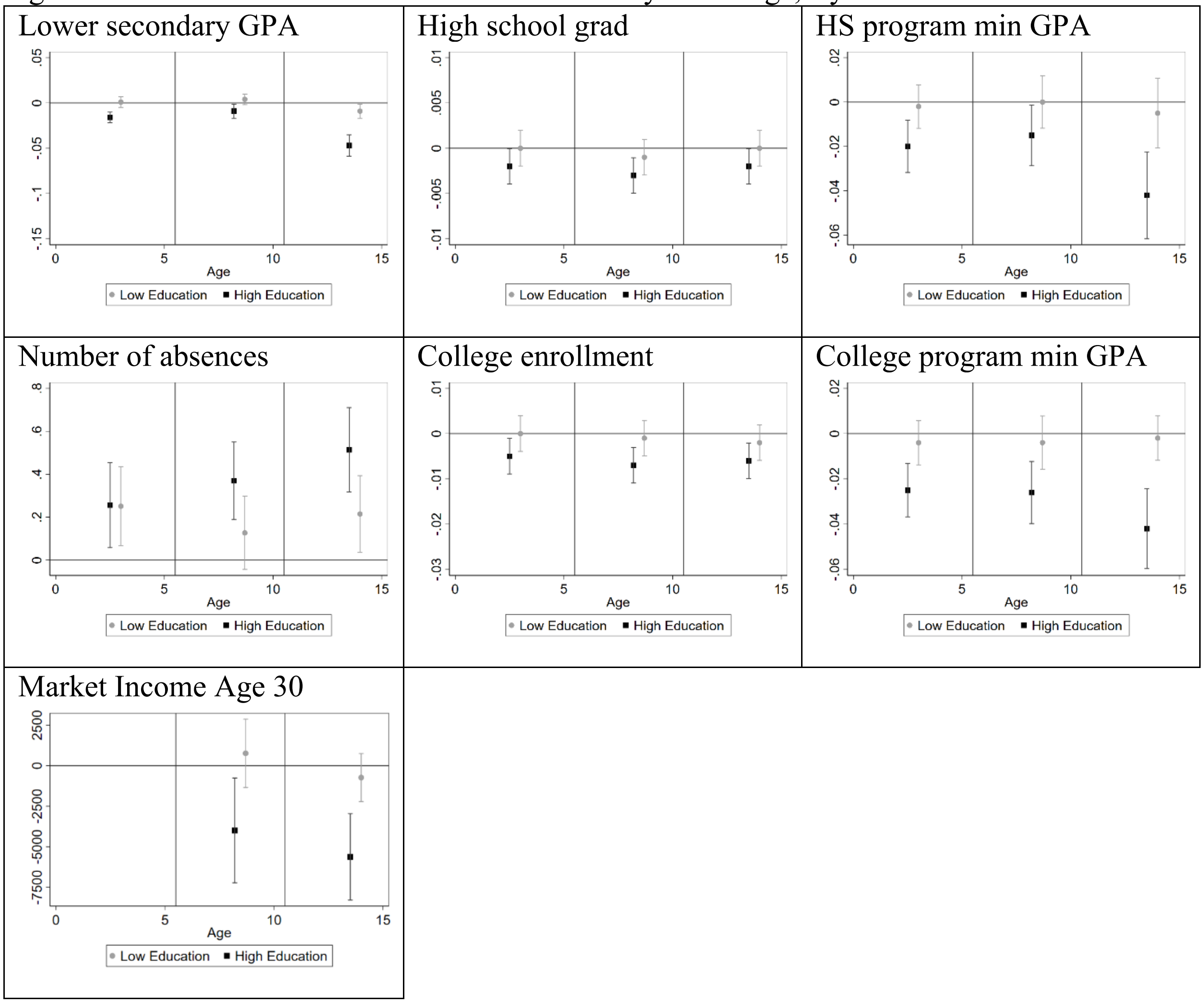
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Figure 5: Effects of Parental Job Loss on Children by Child Age, Multiple Shocks



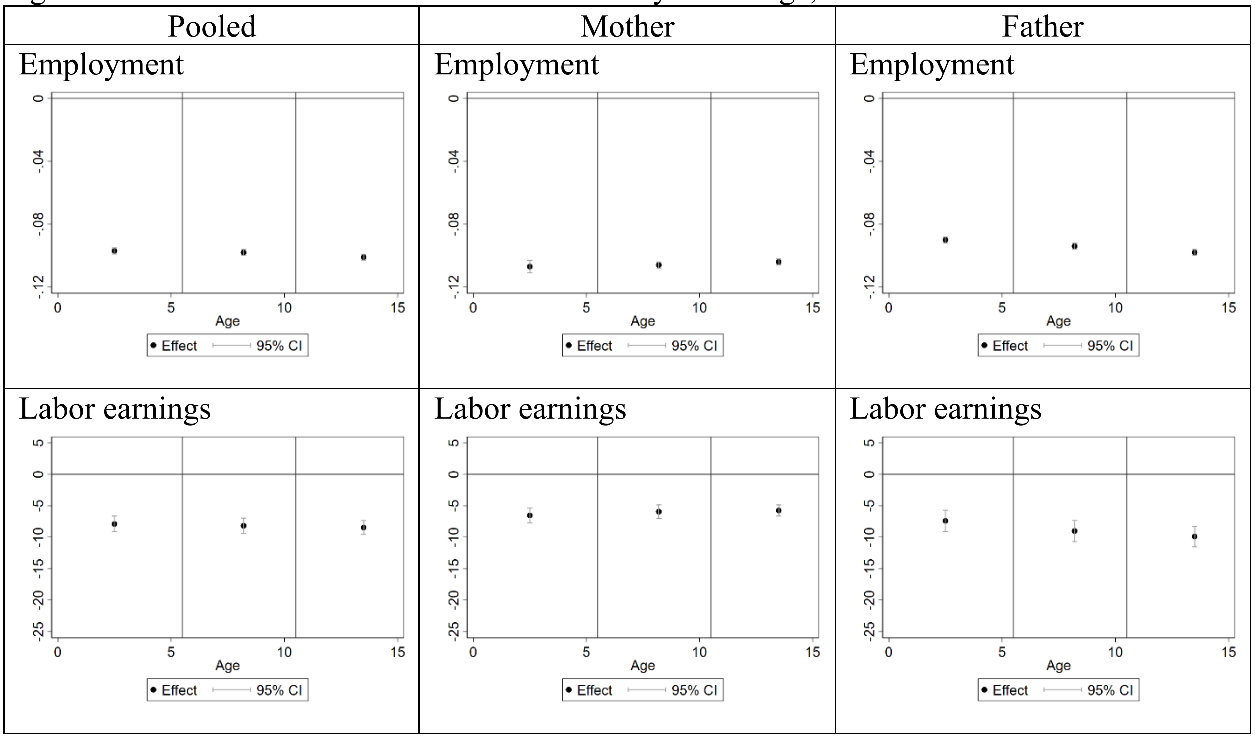
Note: Authors estimation of Equation (2) using population wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jgqam} = \beta_1 DisplaceAge0to5_{jg} + \beta_2 DisplaceAge6to10_{jg} + \beta_3 DisplaceAge11to16_{jg} + \beta_4 DisplaceAge0to5and6to10_{jg} + \beta_5 DisplaceAge0to5and11to16_{jg} + \beta_6 DisplaceAge6to10and11to16_{jg} + \beta_7 DisplaceAllAges_{jg} + \theta_q + \emptyset_m + \rho_a + \varepsilon_{jg}$.

Figure 6: Effects of Parental Job Loss on Children by Child Age, By Parent Education



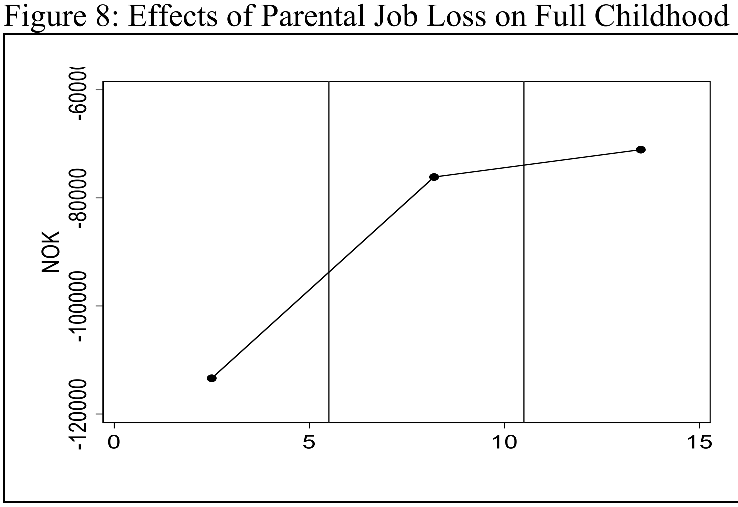
from Statistics Norway. Low education refers to parents with at most a high school diploma. High education refers to parents with more than a high school diploma. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g}Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Figure 7: Effects of Parental Job Loss on Parents by Child Age, Labor Market



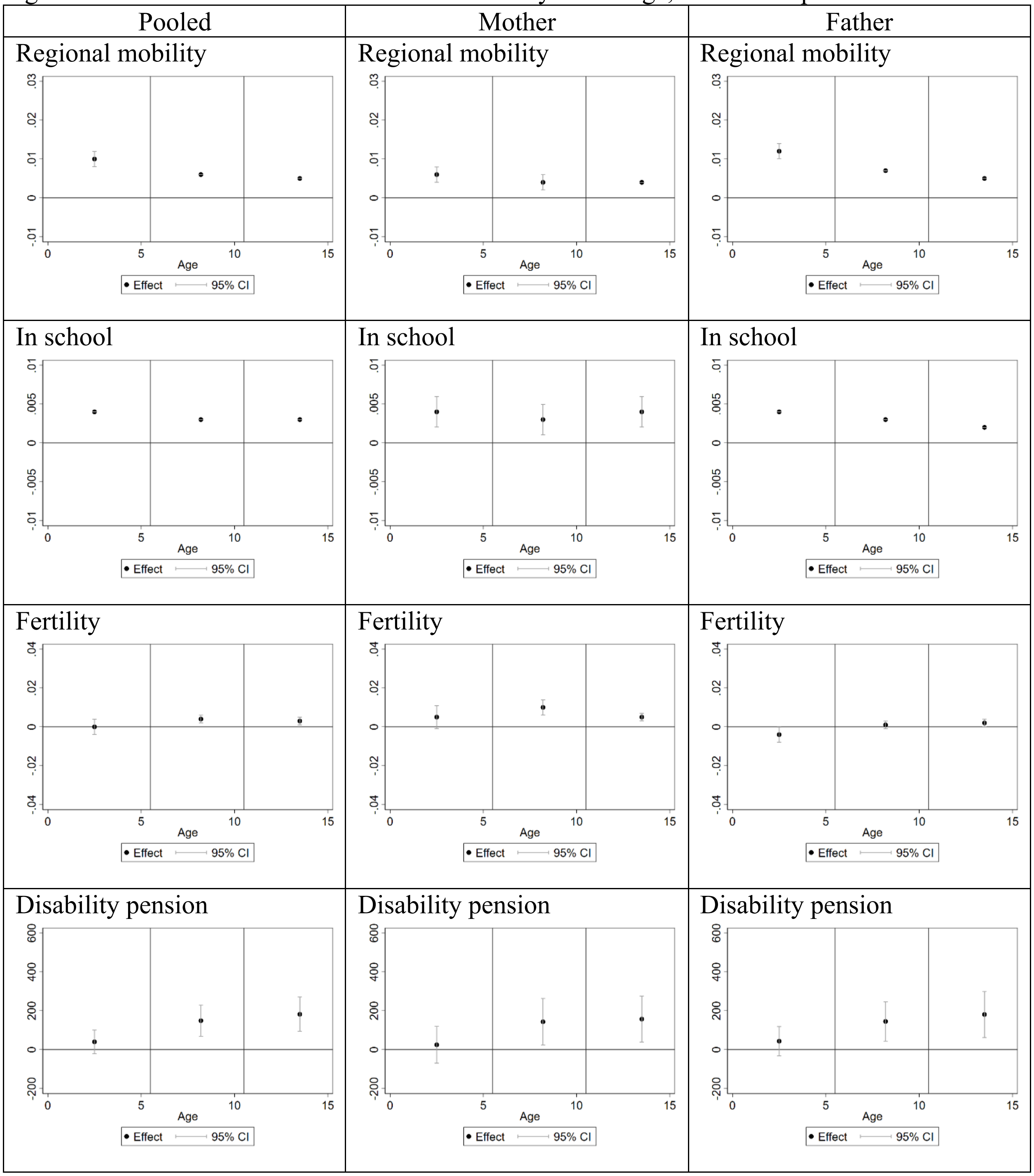
Note: Authors estimation of Equation (3). Dots are point estimates from separate equations, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{ibgt} = \alpha + \beta_g(Displaced_{ig} * Post_{igbt}) + \delta_{1g} Displaced_{ig} + \delta_{2g} Post_{igbt} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}$. where y_{ibgt} is the outcome, $Displaced_{ig}$ is a binary variable taking the value of one if the parent was involuntarily displaced when the child was in that age group, $Post_{igbt}$ is a binary variable taking the value of one if relative time is greater than 0, and the fixed effects for year are γ_{gt}, and for individual parent are λ_{ig}.

Figure 8: Effects of Parental Job Loss on Full Childhood Earnings by Child Age



Note: Authors estimation of Equation (1). Dots are point estimates from separate equations, lines are 95% confidence intervals. Standard errors are clustered at the parent level. Estimating equation: $y_{ibgt} = \alpha + \beta_g(Displaced_{ig} * Post_{igbt}) + \delta_{1g} Displaced_{ig} + \delta_{2g} Post_{igbt} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}$. where y_{ibgt} is the outcome, $Displaced_{ig}$ is a binary variable taking the value of one if the parent was involuntarily displaced when the child was in that age group, $Post_{igbt}$ is a binary variable taking the value of one if relative time is greater than 0, and the fixed effects for year are γ_{gt}, and for individual parent are λ_{ig}.

Figure 9: Effects of Parental Job Loss on Parents by Child Age, Choice Response



Note: Authors estimation of Equation (3) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{ibgt} = \alpha + \beta_g(Displaced_{ig} * Post_{igbt}) + \delta_{1g}Displaced_{ig} + \delta_{2g}Post_{igbt} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}$. where y_{ibgt} is the outcome, $Displaced_{ig}$ is a binary variable taking the value of one if the parent was involuntarily displaced when the child was in that age group, $Post_{igbt}$ is a binary variable taking the value of one if relative time is greater than 0, and the fixed effects for year are γ_{gt}, and for individual parent are λ_{ig}.

TABLES

Table 1: Summary Statistics

Panel A: Child Outcomes			
	Age 0-5	Age 6-10	Age 11-16
Lower secondary GPA	4.19	4.16	4.12
	(0.79)	(0.79)	(0.79)
High school grad	0.83	0.93	0.94
	(0.37)	(0.25)	(0.23)
HS program min GPA	2.04	2.3	2.23
	(1.54)	(1.44)	(1.53)
Number of absences	20.18	20.71	21.58
	(1.00)	(18.23)	(18.24)
College enrollment	0.5	0.6	0.65
	(0.5)	(0.49)	(0.48)
College program min GPA	1.74	2.04	2.18
	(1.67)	(1.64)	(1.64)
Income (1000 NOK)		476	479
		(307)	(325)
Panel B: Parent Outcomes			
	Age 0-5	Age 6-10	Age 11-16
Market Income (100 NOK)	449	476	479
	(298)	(307)	(325)
Disability Pension	120	243	417
	(4370)	(6155)	(7959)
Divorced	0.038	0.067	0.104
	(0.192)	(0.251)	(0.305)
Child Count	1.97	2.42	2.51
	(1.00)	(0.91)	(0.93)
In School	0.016	0.018	0.017
	(0.126)	(0.134)	(0.13)
Move Municipality	0.012	0.006	0.004
	(0.108)	(0.079)	(0.065)

Note: Authors calculations using population-wide administrative data and the sample restrictions discussed in Section 3.

Table 2: Effects of Job Loss on Parent Mental Health First Three Years, by Parent Gender

Panel A: Mothers

	Sleepless	Nervous	Anxious
Effect of Job Loss	0.144**	0.063*	0.007
	(0.064)	(0.036)	(0.027)
N	554	2289	2287

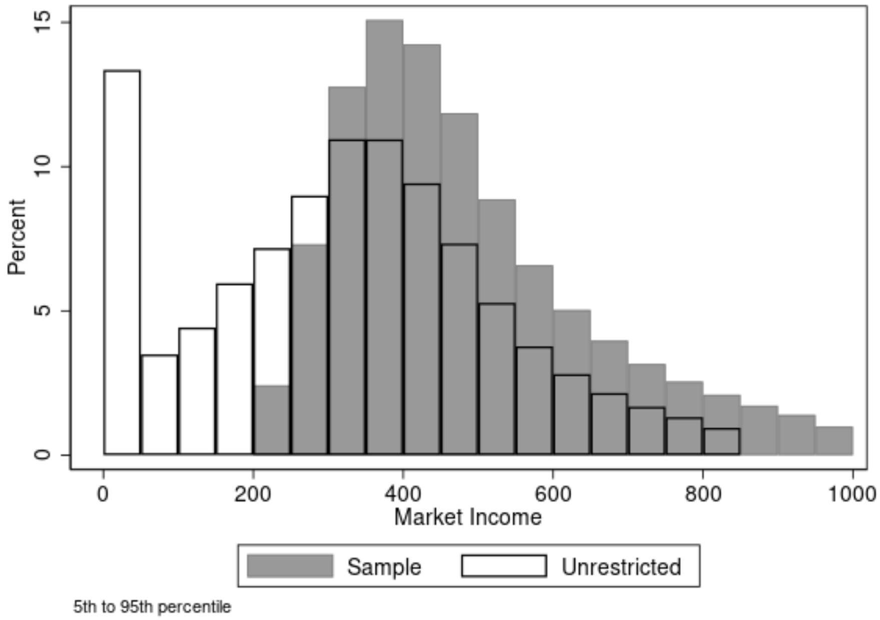
Panel B: Fathers

	Sleepless	Nervous	Anxious
Effect of Job Loss	0.062	-0.016	0.009
	(0.044)	(0.023)	(0.020)
N	913	3939	3920

Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbqam} = \beta_1 Displace_j + \theta_q + \emptyset_m + \rho_a +,$ where y_{jbqam} is the outcome, $Displace_j$ is a binary variable taking the value of one if the child's parent was involuntarily displaced, and the fixed effects for birth year are θ_q, for parent age are ρ_a, and for municipality are $\emptyset_m$.

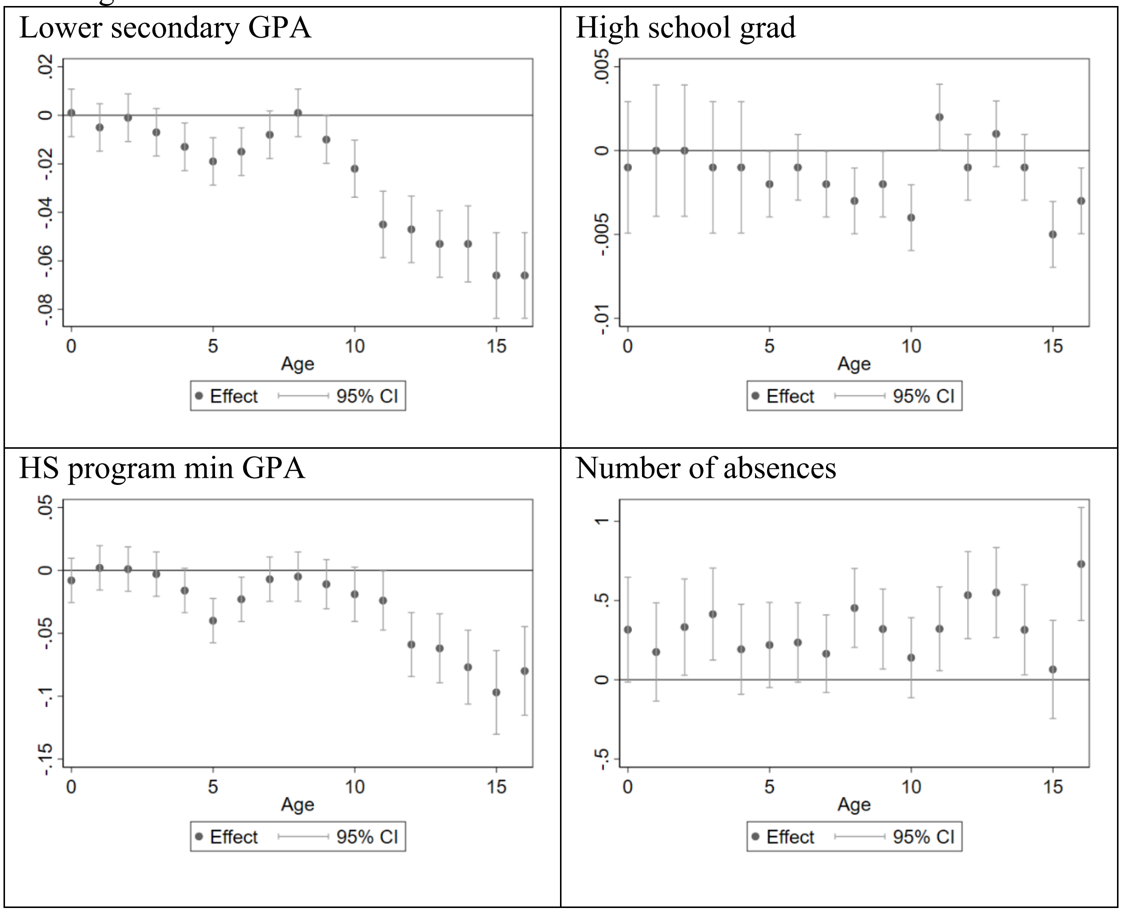
ONLINE APPENDIX

Appendix Figure A-1: Income Distribution, Analysis Sample and Unrestricted



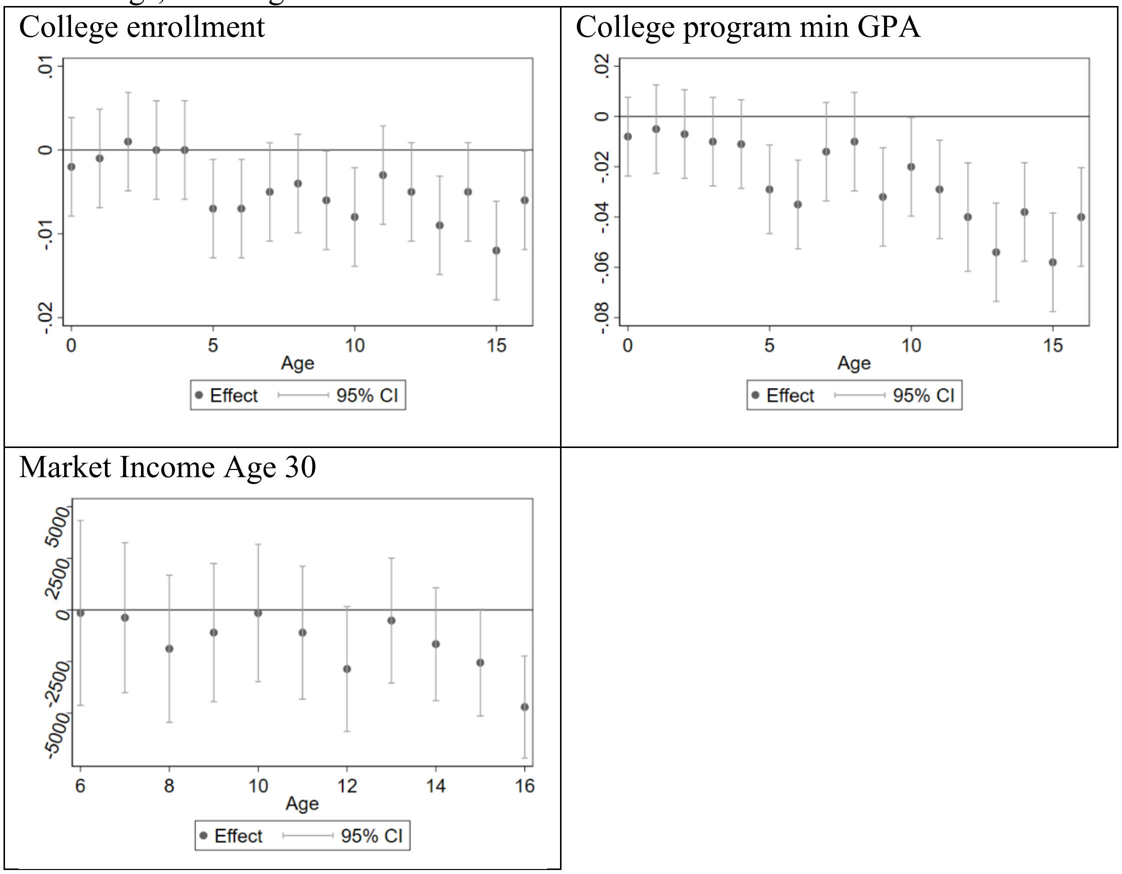
Note: Authors' calculation of the distributions of income for the universe of parents of children aged 10 between 1986 and 2009 (unrestricted), and for the set of parents in our analysis (sample). The main difference between these two samples is the employment condition we impose on our analytical sample (3 years of continuous employment prior to the potential job loss event).

Appendix Figure A-2: Effects of Parental Job Loss on Children by Child Age, High School, Each Age



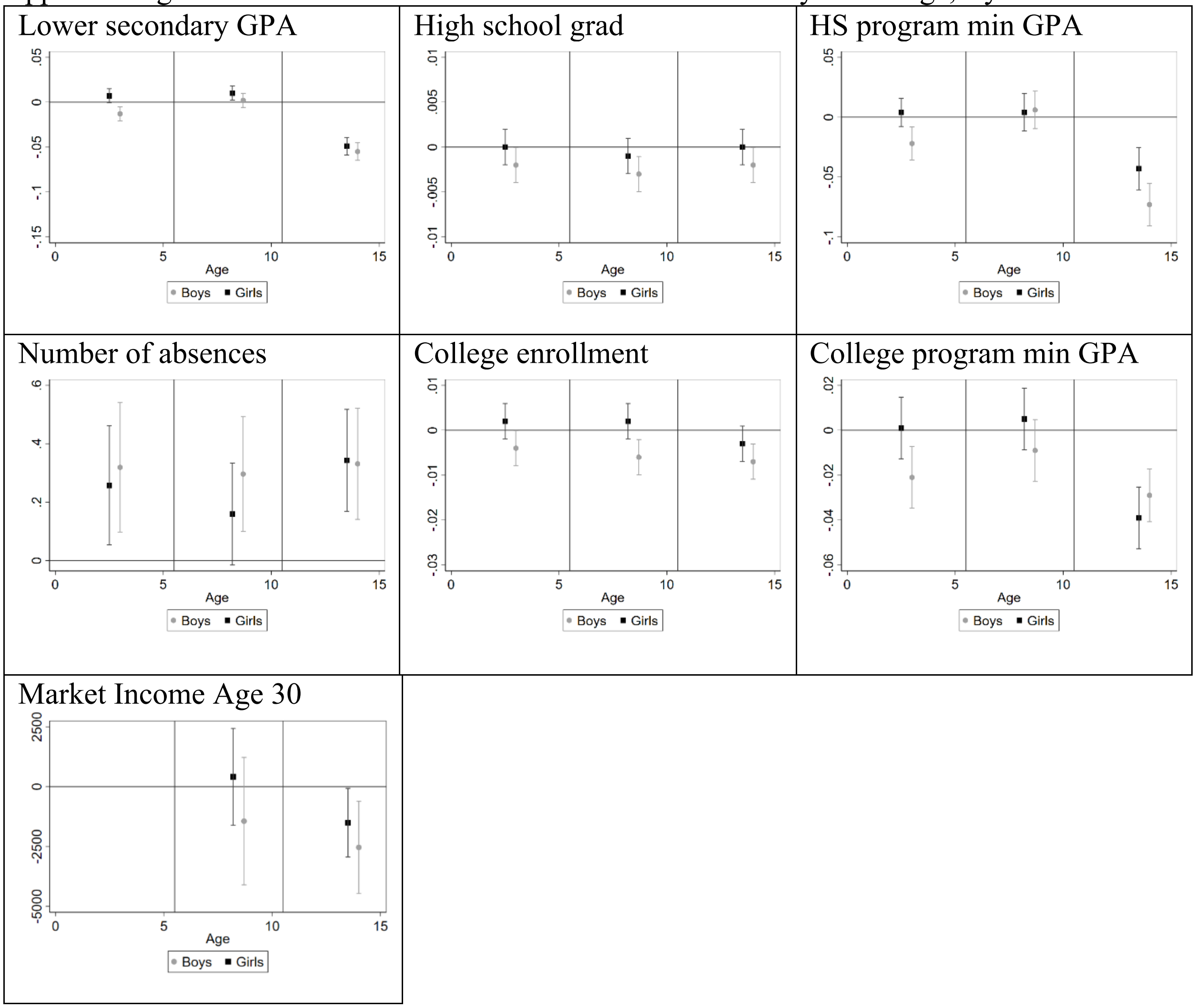
Note: Authors estimation of Equation (1) for each child age (rather than child age group) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was of a specific age, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-3: Effects of Parental Job Loss on Children by Child Age, College and Age 30 Earnings, Each Age



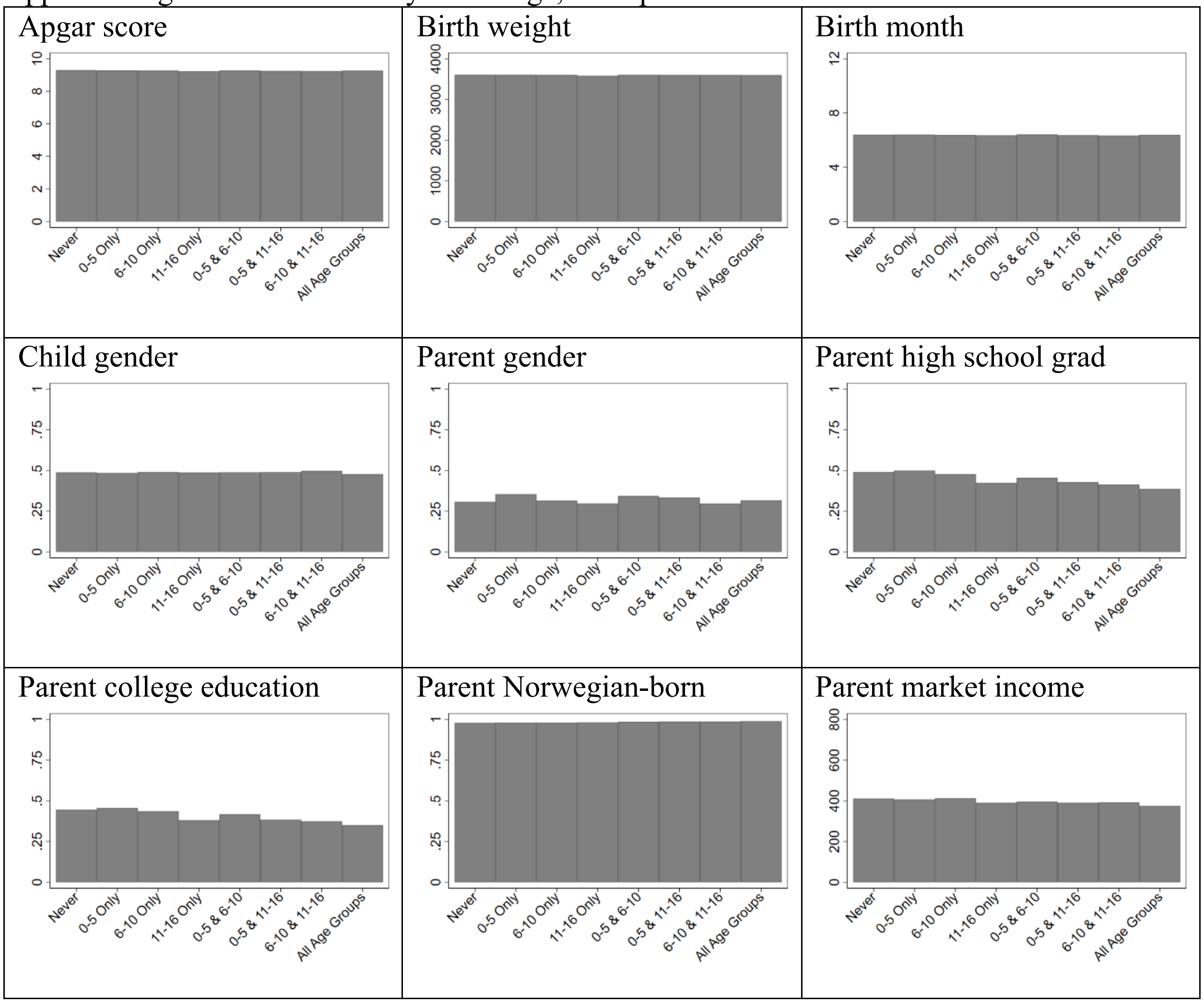
Note: Authors estimation of Equation (1) for each child age using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was of a specific age, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-4: Effects of Parental Job Loss on Children by Child Age, By Child Gender



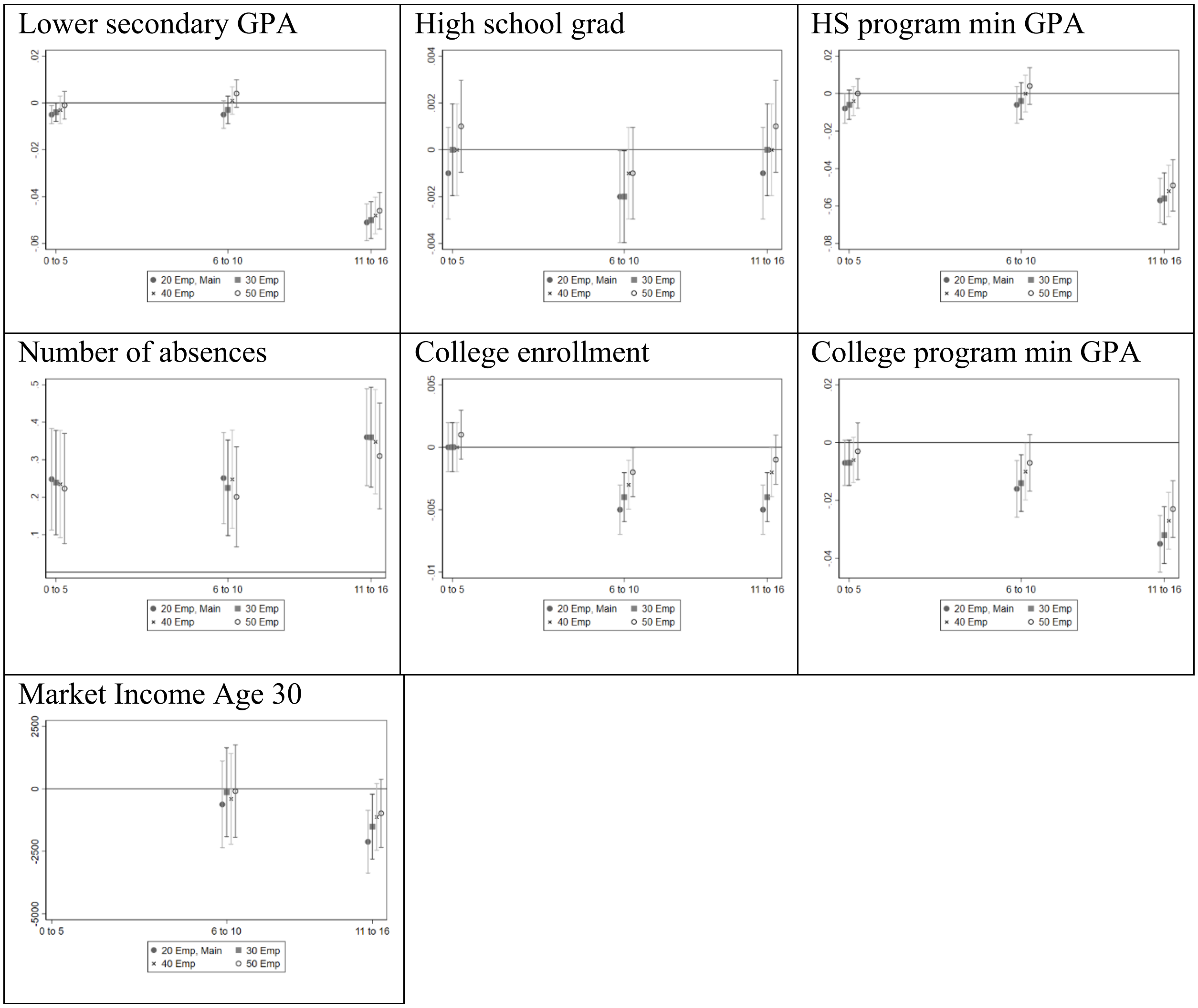
Note: Authors estimation of Equation (1) stratified by child gender using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-5: Balance by Child Age, Multiple Shocks



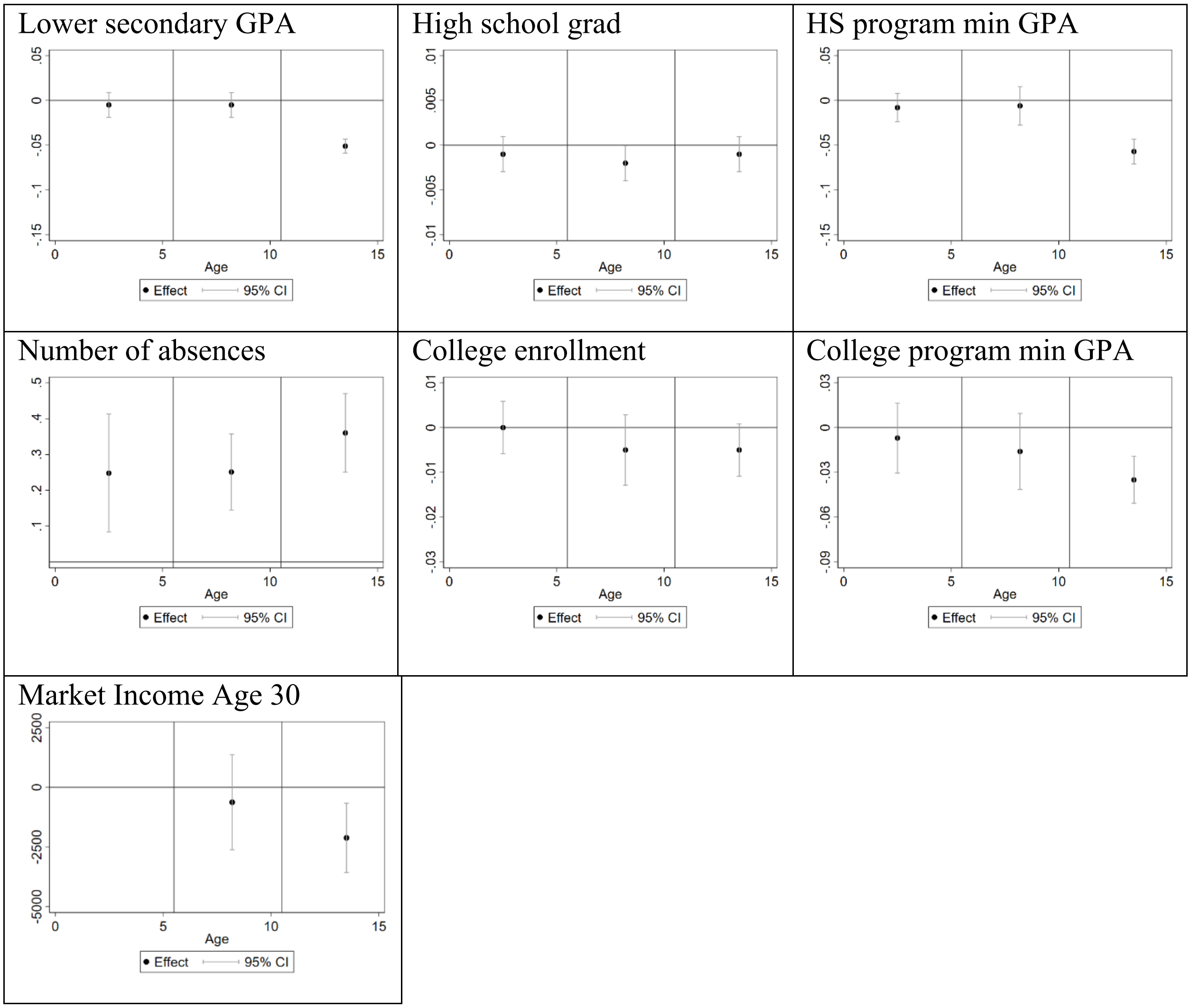
Note: Authors estimation of Equation (2) using population wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jgqam} = \beta_1 DisplaceAge0to5_{jg} + \beta_2 DisplaceAge6to10_{jg} + \beta_3 DisplaceAge11to16_{jg} + \beta_4 DisplaceAge0to5and6to10_{jg} + \beta_5 DisplaceAge0to5and11to16_{jg} + \beta_6 DisplaceAge6to10and11to16_{jg} + \beta_7 DisplaceAllAges_{jg} + \theta_q + \emptyset_m + \rho_a + \varepsilon_{jg}$.

Appendix Figure A-6: Effects of Parental Job Loss on Children by Child Age, Firm Size Restriction



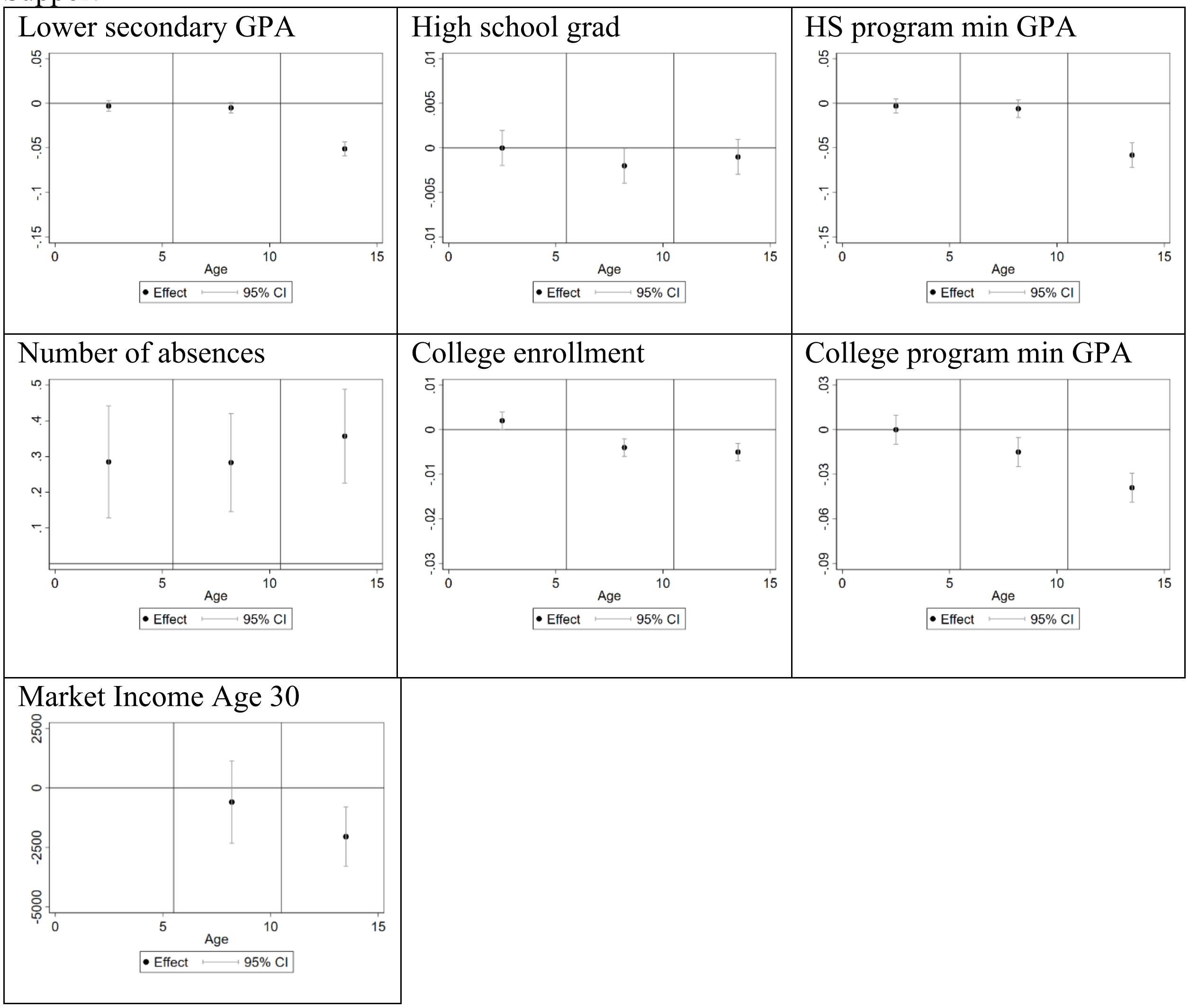
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$. The label at the bottom of each subfigure provides information on the plant size (number of employees at the plant) restriction used to obtain that particular estimate. In our main specification, we focus on plants that have at least 20 employees.

Appendix Figure A-7: Effects of Parental Job Loss on Children by Child Age, Municipality Cluster



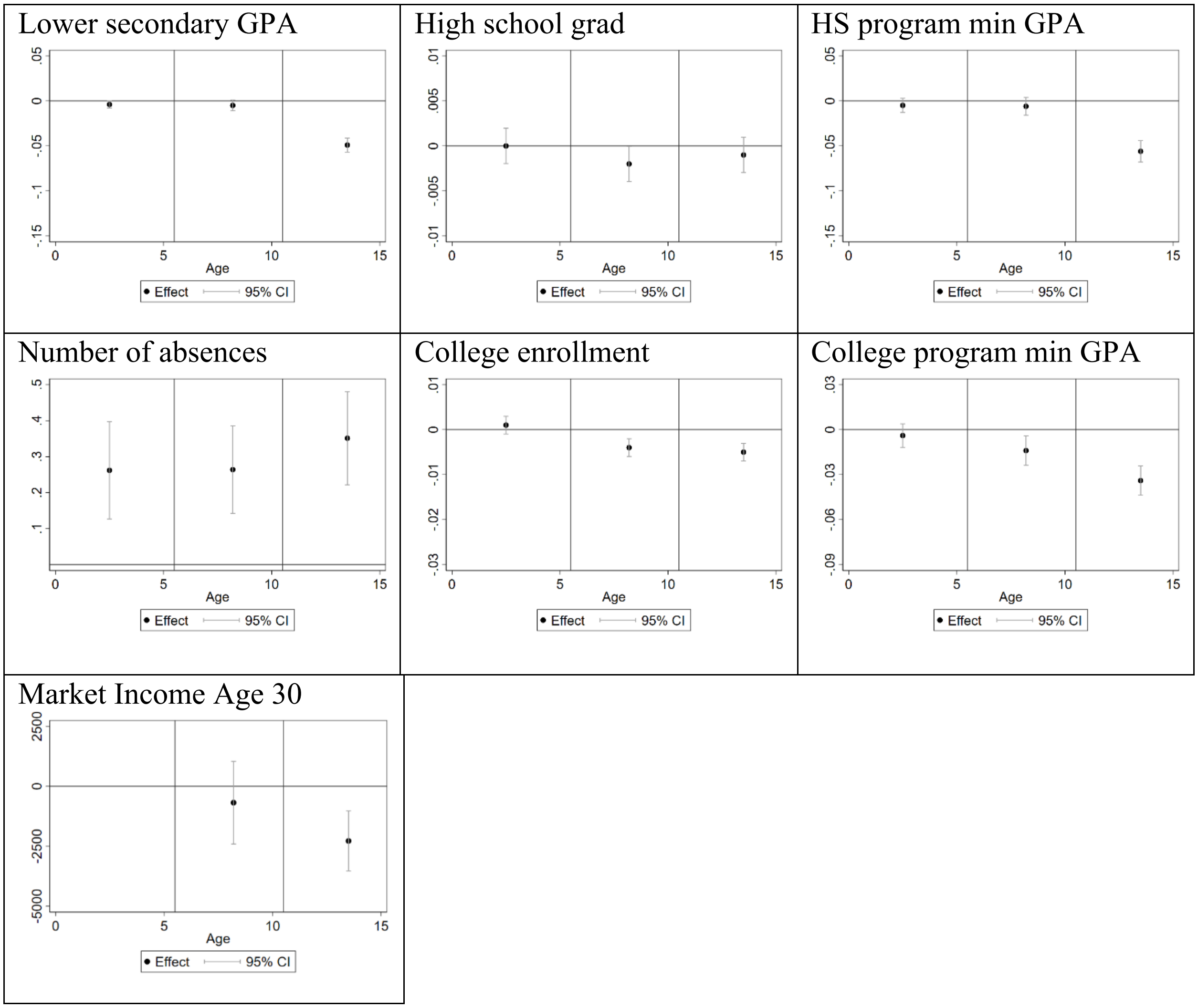
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the municipality level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-8: Effects of Parental Job Loss on Children by Child Age, PSM Common Support



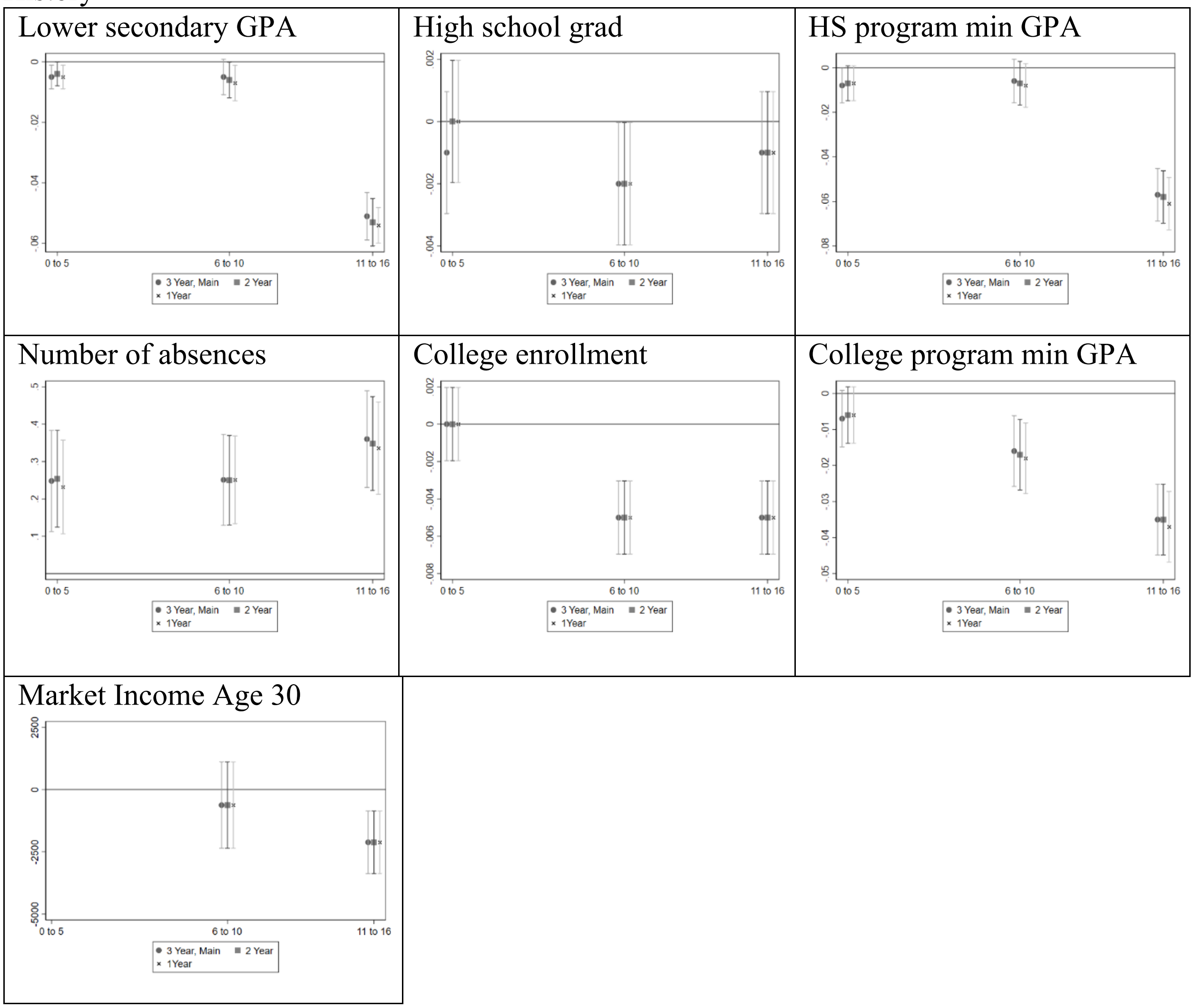
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. To obtain our sample, we calculate propensity scores based on the pre-displacement period (exact match on strata based on birth year, child sex, and parent sex; within each strata, propensity based on parent having at least a high school education, parent having any college education, and parent income). We then restrict our sample to those in our main sample that fall in the common support region of the propensity score. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g}Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-9: Effects of Parental Job Loss on Children by Child Age, Include Early Leavers



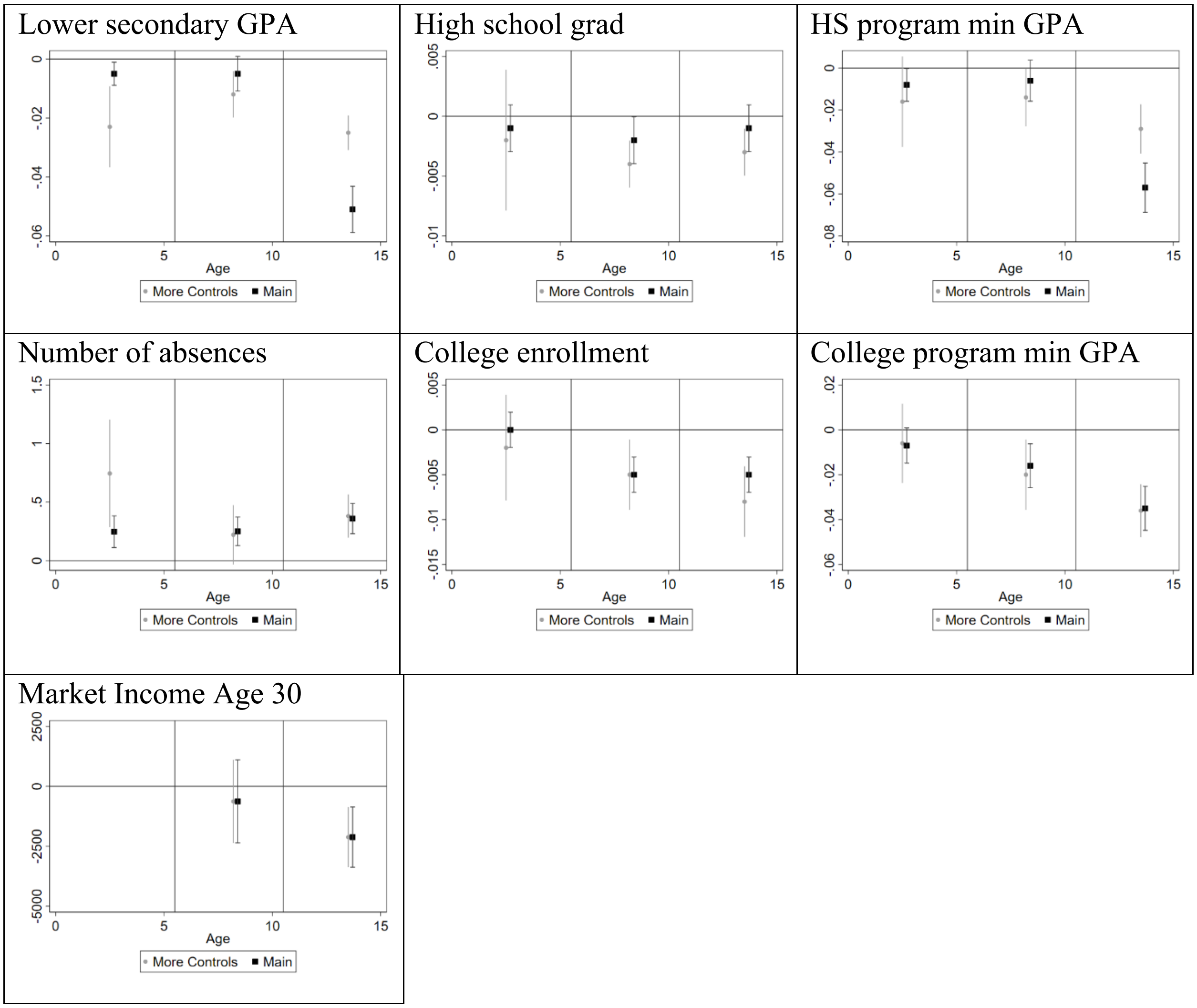
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. The sample underlying these estimates differs from our main sample in that we have eliminated early leavers (individuals who leave the plant one year before the closure/layoff, potentially in anticipation of the event). Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +,$ where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-10: Effects of Parental Job Loss on Children by Child Age, Relax Work History



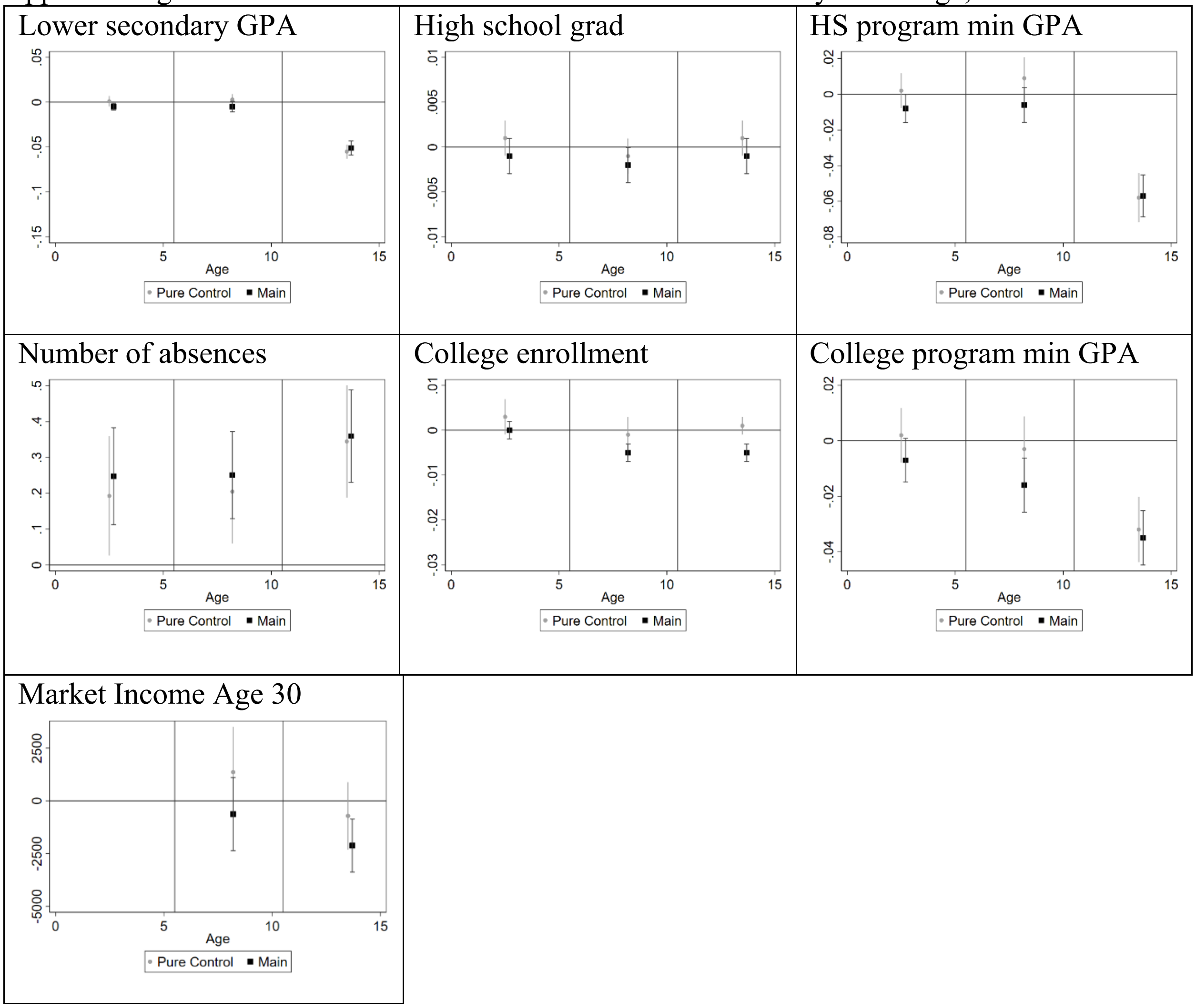
Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$. The label at the bottom of each subfigure provides information on the employment condition (number of continuous work prior to relative time 0) restriction used to obtain that particular estimate. In our main specification, we focus on individuals who have held three years of continuous work prior to the potential displacement event.

Appendix Figure A-11: Effects of Parental Job Loss on Children by Child Age, Additional Controls



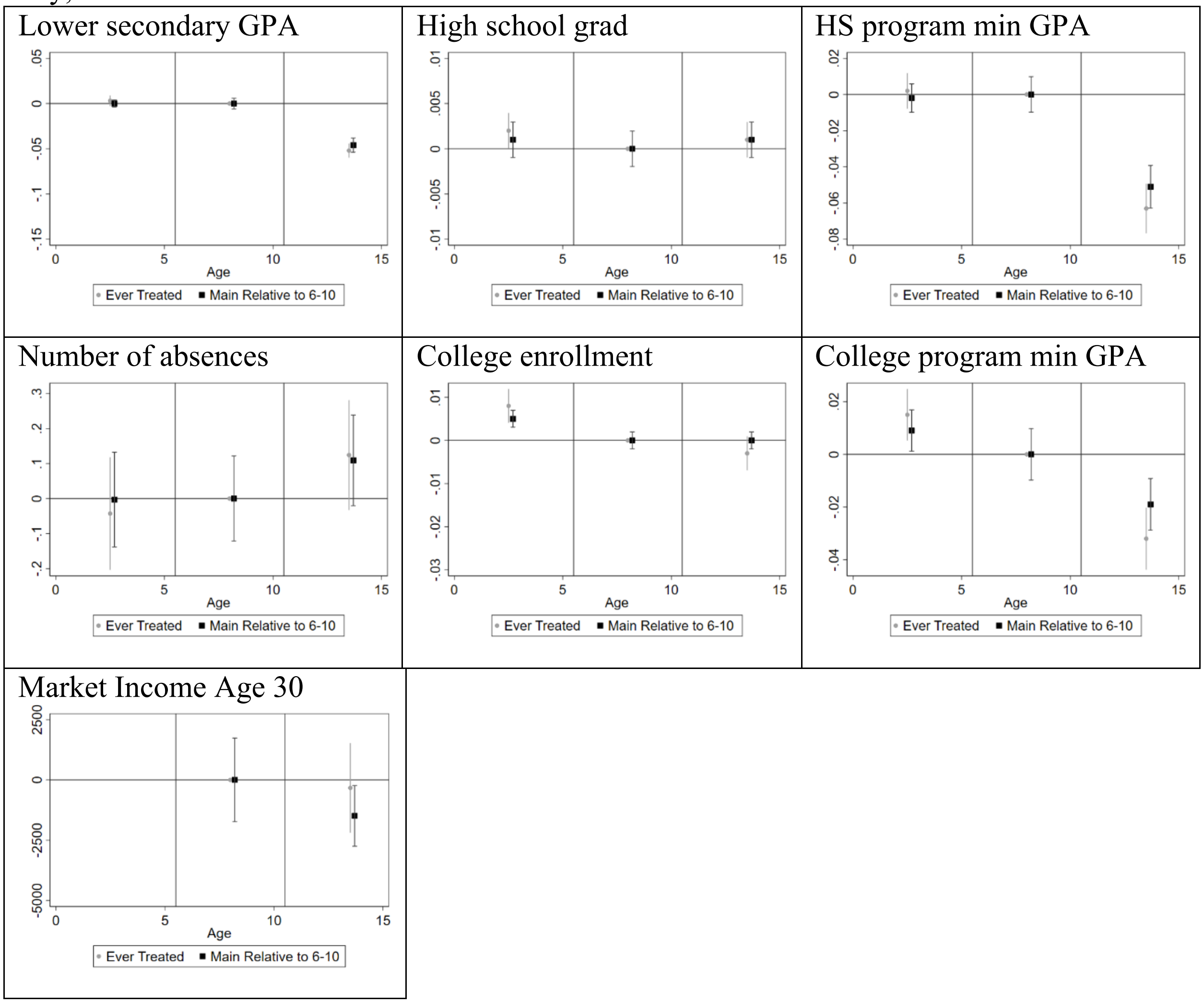
Note: Authors estimation of a modified version of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g}Displace_{jg} + X'\psi + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$. X' is a vector of additional controls, and includes pre-period industry fixed effects as well as child birth month, child sex, parent sex, parent education, parent Norwegian born.

Appendix Figure A-12: Effects of Parental Job Loss on Children by Child Age, Pure Control



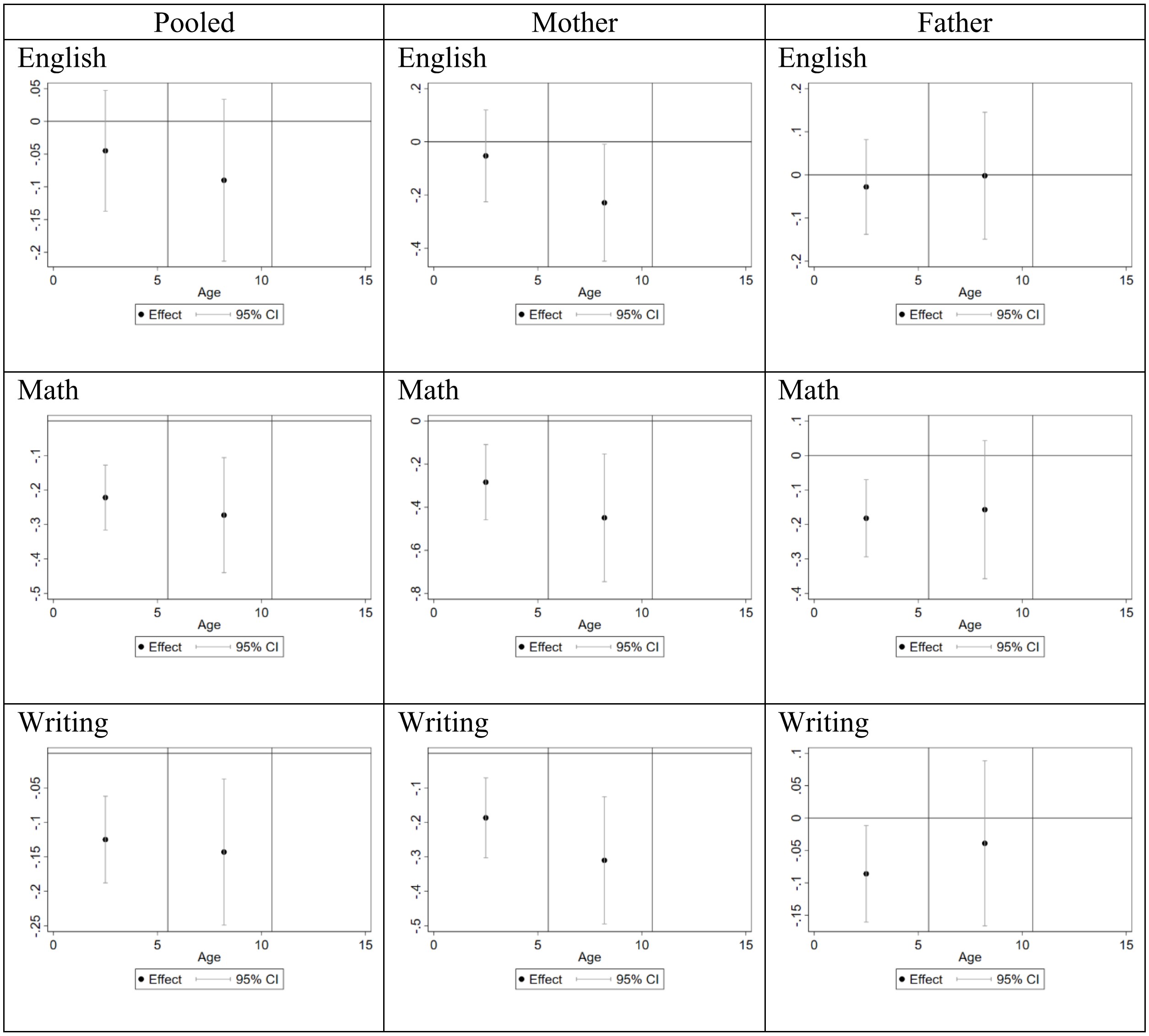
Note: Authors estimation of a modified version of Equation (1) using population-wide register data from Statistics Norway. The control group in the “Pure Control” regressions includes only children who were never exposed to an involuntary parental job displacement during their entire childhood (between birth through age 16). Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child’s parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-13: Effects of Parental Job Loss on Children by Child Age, Ever Treated Only, Stack



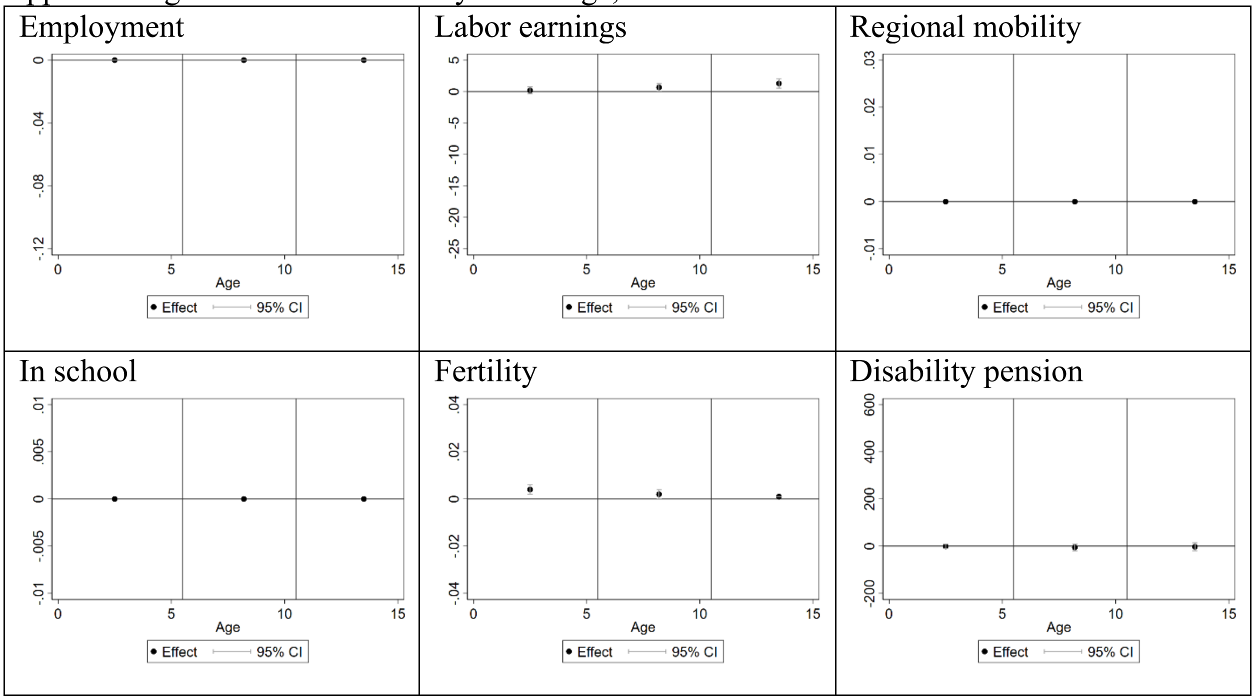
Note: Authors estimation of Equation (1) stratified by parental education level using population-wide register data from Statistics Norway. Low education refers to parents with at most a high school diploma. High education refers to parents with more than a high school diploma. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Main estimating equation: $y_{jbgqam} = \beta_{1g} Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$. Ever treated estimating equation: $y_{jgqam} = \alpha + \beta_1 TreatAge0to5_{gj} + \beta_2 TreatAge11to16_{gj} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} + \varepsilon_{jgqam}$. Main results are relative to age 6-10 for comparison to ever treated results.

Appendix Figure A-14: Effects of Parental Job Loss on Children by Child Age, Grade Low-Stakes Exams in Grade 5



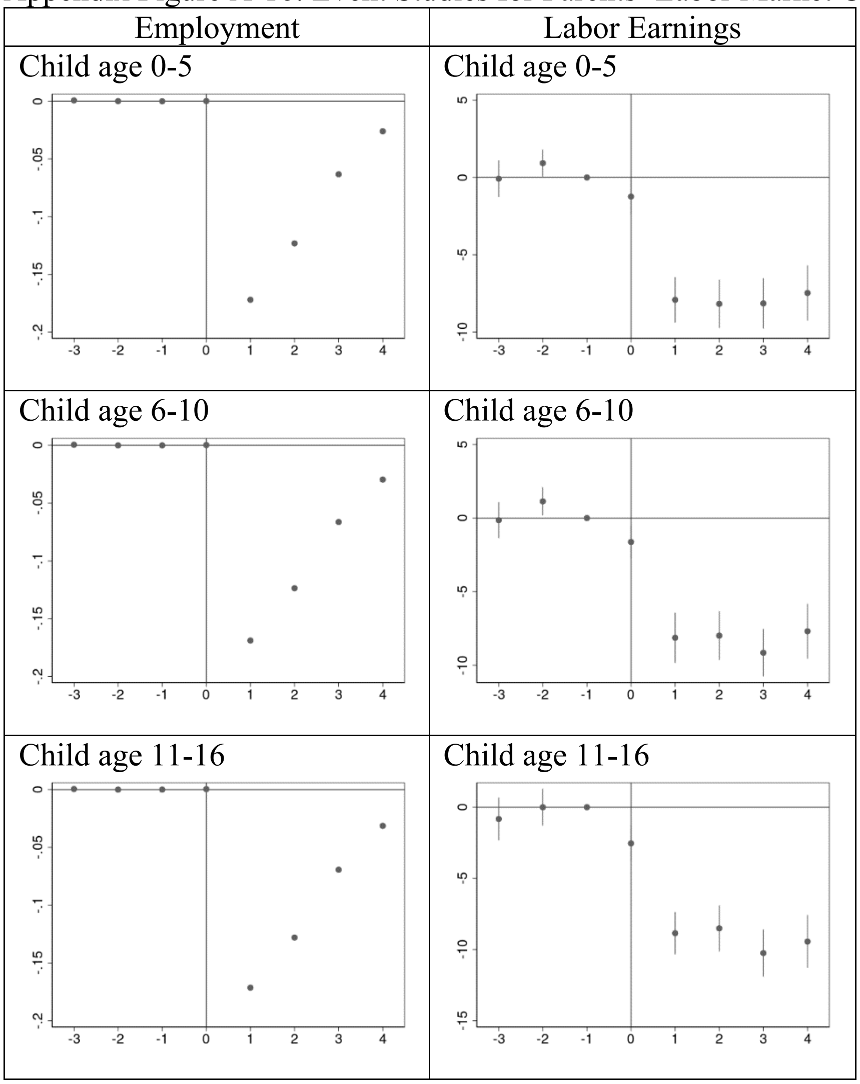
Note: Authors estimation of Equation (4) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Using **only** pre-period data, the estimating equation is: $y_{ibgt} = \alpha + \left[\pi_g * Displace_{ig} * RelativeTime_{\tau}\right] + \psi_g Displaced_{ig} + \delta_{g\tau} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}$, where $Displace_{ig\tau}$ is an indicator variable taking value 1 if the individual is displaced in relative time $\tau = 0$, and zero otherwise. The π_g coefficient identifies relative pre-displacement trends. The regression also includes fixed effects for birth year θ_{gq}, parent age ρ_{ga}, and municipality $\emptyset_{gm}$.

Appendix Figure A-15: Pre-trend by Child Age, Parent Outcomes



Note: Authors estimation of Equation (4) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Using **only** pre-period data, the estimating equation is: $y_{ibgt} = \alpha + \left[\pi_g * Displace_{ig} * RelativeTime_\tau\right] + \psi_g Displaced_{ig} + \delta_{g\tau} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}$, where $Displace_{ig\tau}$ is an indicator variable taking value 1 if the individual is displaced in relative time $\tau = 0$, and zero otherwise. The π_g coefficient identifies relative pre-displacement trends. The regression also includes fixed effects for birth year θ_{gq}, parent age ρ_{ga}, and municipality $\emptyset_{gm}$.

Appendix Figure A-16: Event Studies for Parents’ Labor Market Outcomes

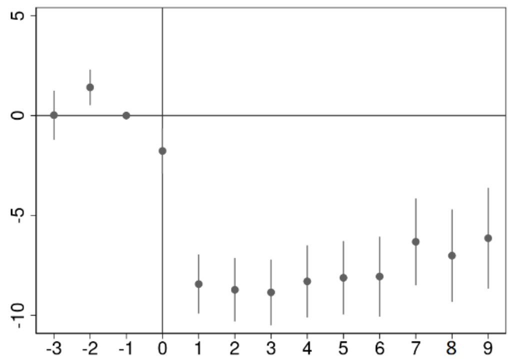


Note: Authors estimation using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{ibgt} = \alpha + \sum_{t=-3}^{4}\left[\pi_t\left(Displaced_{ig}\right)\right] + \gamma_t + \lambda_{ig} + \varepsilon_{ibgt}$, where the π_t coefficients trace out relative pre treatment trends as well as time varying treatment effects. $Displaced_{ig}$ is an indicator variable taking value 1 if the individual is displaced is a binary variable taking the value of one if the parent was involuntarily displaced when the child was in that age group, and zero otherwise. The regression also includes fixed effects for birth year θ_{gq}, parent age ρ_{ga}, and municipality $\emptyset_{gm}$.

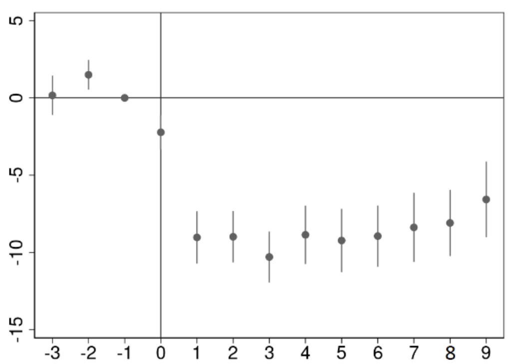
Appendix Figure A-17: Event Studies for Parents' Earnings, Extended Post Period

Labor Earnings

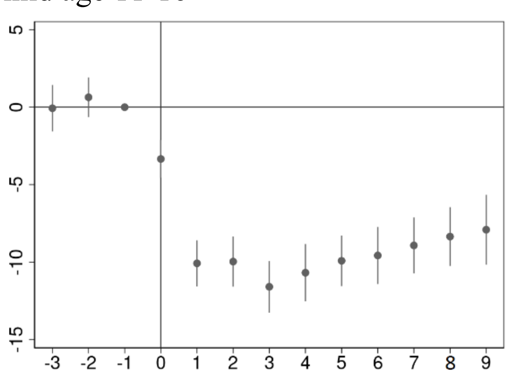
Child age 0-5



Child age 6-10

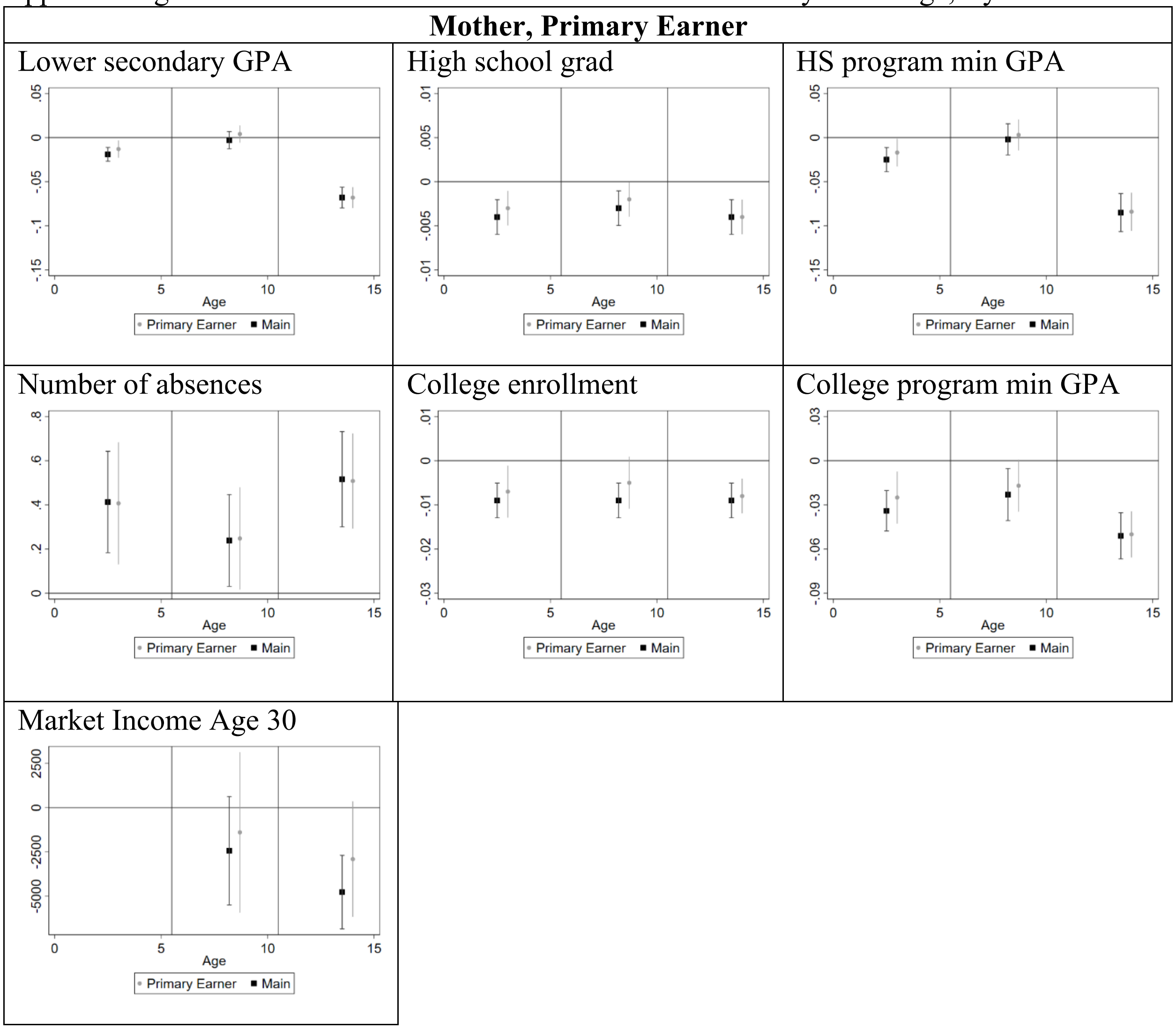


Child age 11-16



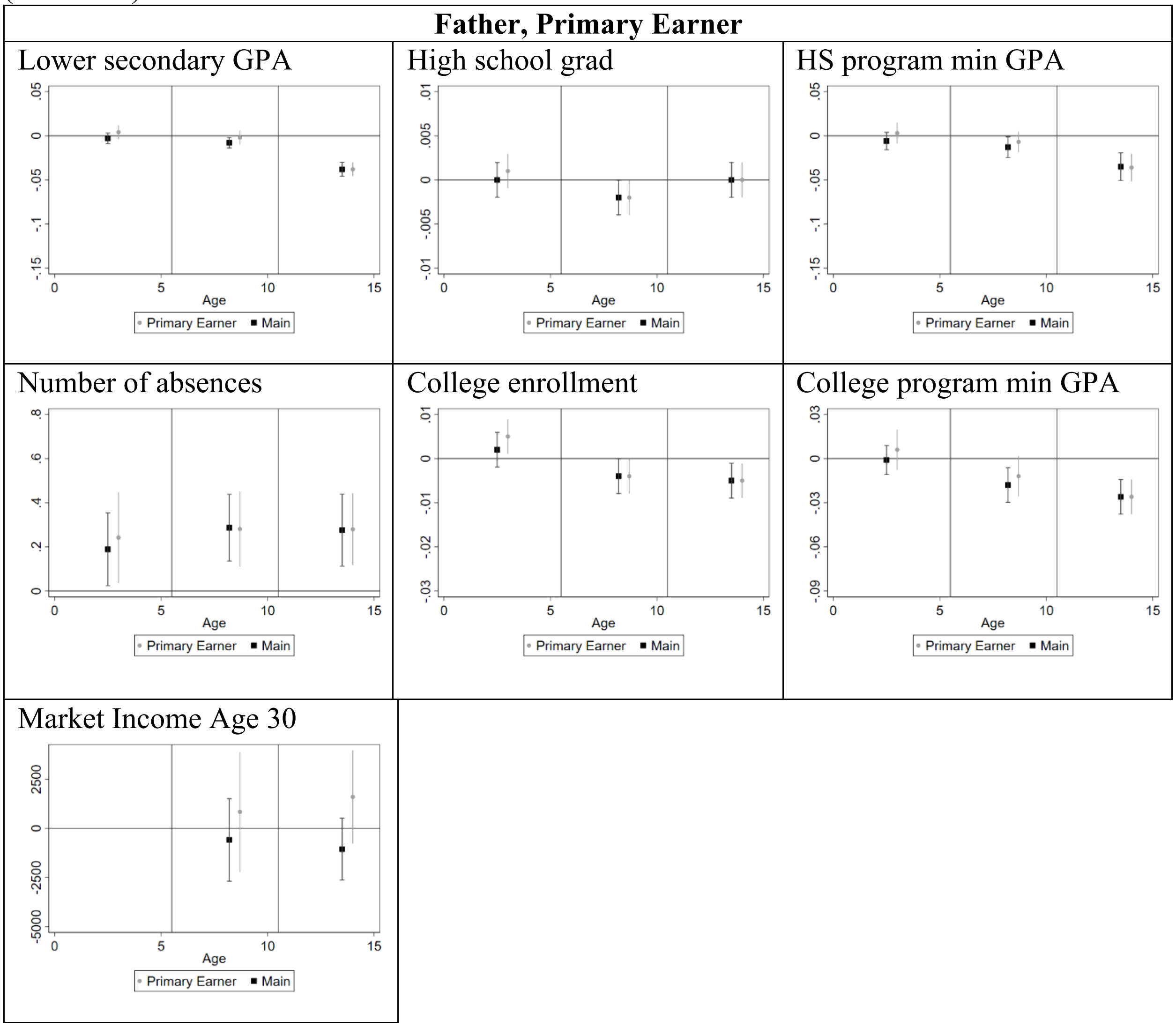
Note: Authors estimation using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{ibgt} = \alpha + \sum_{t=-3}^{10}\left[\pi_t\left(Displaced_{ig}\right)\right] + \gamma_t + \lambda_{ig} + \varepsilon_{ibgt}$, where the π_t coefficients trace out relative pre treatment trends as well as time varying treatment effects. $Displaced_{ig}$ is an indicator variable taking value 1 if the individual is displaced is a binary variable taking the value of one if the parent was involuntarily displaced when the child was in that age group, and zero otherwise. The regression also includes fixed effects for birth year θ_{gq}, parent age ρ_{ga}, and municipality $\emptyset_{gm}$.

Appendix Figure A-18: Effects of Parental Job Loss on Children by Child Age, By Main Earner



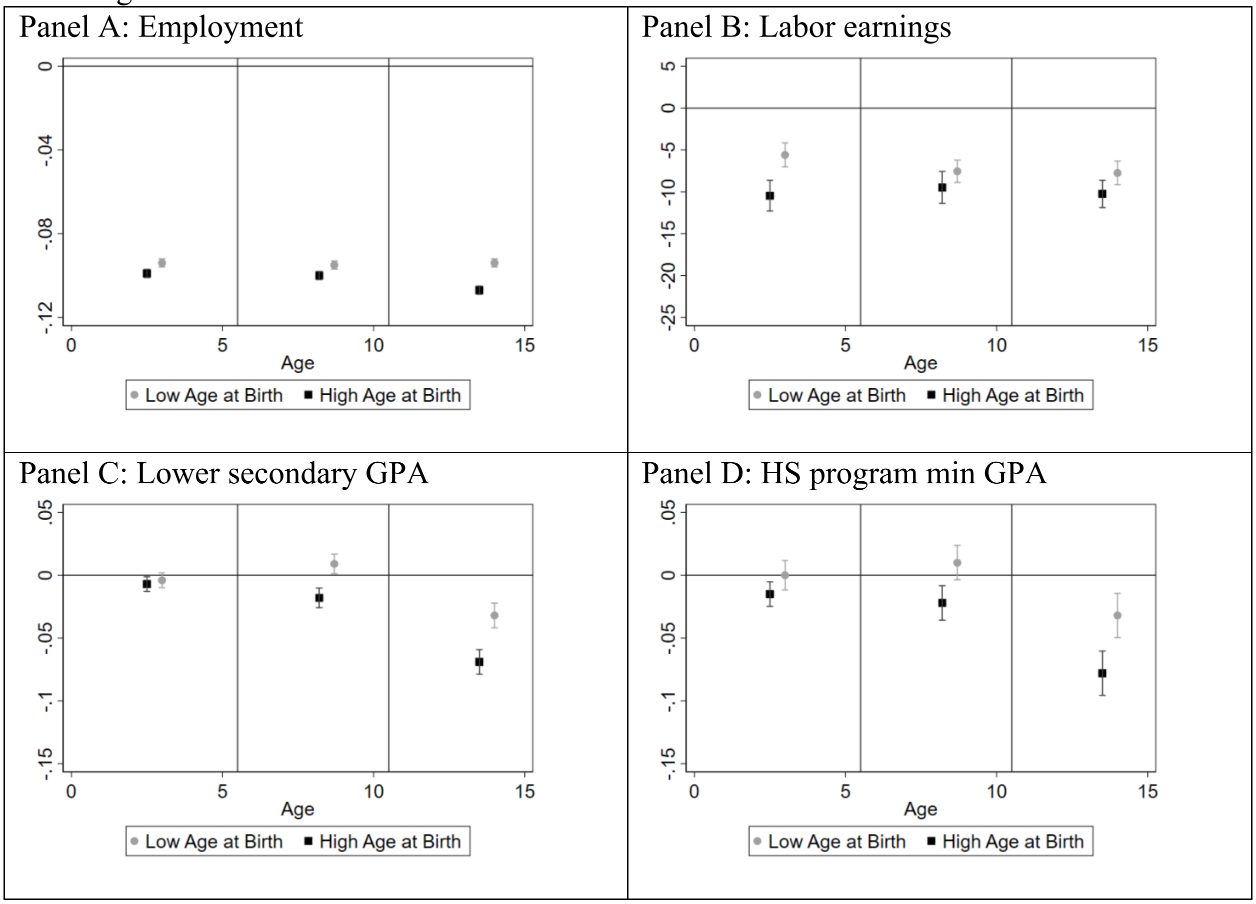
(continued on next page)

Appendix Figure A-19: Effects of Parental Job Loss on Children by Child Age, By Main Earner (continued)



Note: Authors estimation of a modified version of Equation (1) using population-wide register data from Statistics Norway. The control group in the "Pure Control" regressions includes only children who were never exposed to an involuntary parental job displacement during their entire childhood (between birth through age 16). Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g}Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Figure A-20: Effects of Parental Job Loss on Parents and Child by Child Age, By Parent Age



Note: Panels A and B: Authors estimation of Equation (3). Dots are point estimates from separate equations, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{ibgt} = \alpha + \beta_g(Displaced_{ig} * Post_{igbt}) + \delta_{1g}Displaced_{ig} + \delta_{2g}Post_{igbt} + \gamma_{gt} + \lambda_{ig} + \varepsilon_{ibgt}$. where y_{ibgt} is the outcome, $Displaced_{ig}$ is a binary variable taking the value of one if the parent was involuntarily displaced when the child was in that age group, $Post_{igbt}$ is a binary variable taking the value of one if relative time is greater than 0, and the fixed effects for year are γ_{gt}, and for individual parent are λ_{ig}. Panels C and D: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbgqam} = \beta_{1g}Displace_{jg} + \theta_{gq} + \emptyset_{gm} + \rho_{ga} +$, where y_{jbgqam} is the outcome, $Displace_{jg}$ is a binary variable taking the value of one if the child's parent was involuntarily displaced when the child was in that age group, and the fixed effects for birth year are θ_{gq}, for parent age are ρ_{ga}, and for municipality are $\emptyset_{gm}$.

Appendix Table A-1: Summary Statistics, Children, Analysis Sample and Unrestricted

	Sample	Unrestricted
Lower secondary GPA	4.19	4.06
High school grad	0.88	0.87
HS program min GPA	2.17	2.01
Number of absences	20.1	21.2
College enrollment	0.52	0.48
College program min GPA	1.84	1.68
Income age 30 (1000 NOK)	419.44	400.65

Note: Authors calculations using population-wide administrative data. The sample column based on restrictions discussed in Section 3. Limited to children in the analysis at age 10.

Appendix Table A-2: Summary Statistics, Parents, Analysis Sample and Unrestricted

	Sample	Unrestricted
Employed	1.00	0.73
Market Income (100 NOK)	513.89	367.56
Disability Pension	248.13	5853.19
Divorced	0.08	0.10
Child Count	2.48	2.59
In School	0.02	0.05
Move Municipality	0.01	0.04
Age	40.25	39.05
College Ed	0.39	0.32

Note: Authors calculations using population-wide administrative data. The sample column based on restrictions discussed in Section 3. Limited to children in the analysis at age 10.

Appendix Table A-3: Effects of Job Loss on Parent Mental Health, Years 5-7, Mothers

	Sleepless	Nervous	Anxious
Effect of Job Loss	0.008	0.007	0.010
	(0.061)	(0.041)	(0.034)
N	420	1929	1926

Note: Authors estimation of Equation (1) using population-wide register data from Statistics Norway. Dots are point estimates from a separate equation, lines are 95% confidence intervals. Standard errors are clustered at the individual parent level. Estimating equation: $y_{jbqam} = \beta_1 Displace_j + \theta_q + \emptyset_m + \rho_a +,$ where y_{jbqam} is the outcome, $Displace_j$ is a binary variable taking the value of one if the child's parent was involuntarily displaced, and the fixed effects for birth year are θ_q, for parent age are ρ_a, and for municipality are $\emptyset_m$.