Family-level stress and children's educational choice: Evidence from parental layoffs*

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Abstract

We analyze the effect of parental layoffs on the educational outcomes of their children. Using Swedish administrative data, we exploit shocks to firm labor demand to estimate the age-specific impact of parental layoffs on high school graduation rates. We find that parental layoffs have a significant impact on high school completion rates and that the effect is strongest in the year of application to high school (age 15). We then exploit variation in the fine timing of the layoff to link this effect to a short window before a student chooses where to apply to high school. A parental layoff in the month before the school choice deadline decreases the likelihood that the child will finish high school on time by 9 percentage points relative to a layoff in the same school semester, but after the deadline. The effect is higher for families with less information about high school choice, consistent with the hypothesis that family stress, even if temporary and without financial effects, may disrupt educational choice.

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1 Introduction

Educational attainment is highly correlated with parental income in a range of different settings.[1] This correlation may reflect a causal effect where poorer households are constrained from investing in education. Traditionally, such economic constraints are modeled as credit constraints that arise because credit markets are incomplete with respect to future earnings (e.g. Lochner and Monge-Naranjo, 2012). The evidence of credit constraints as a major impediment for poor families to invest in education is, however, contested (Francesconi and Heckman, 2016; Lovenheim, 2011).

Family income can have a negative effect on the skill formation of children even beyond credit constraints. Heckman and Mosso (2014) emphasize the empirical role of parenting in skill formation. It is possible that economic insecurity affects parents' ability to make optimal long-term decisions or restrict parental time available for involvement in their child's education, both of which are crucial for educational outcomes. For example, Sapolsky (2018) documents the increasing body of research in neurology showing how the increased level of stress associated with lower socio-economic status can lead to reduced function in the prefrontal cortex, associated with executive function and long-term planning. Another factor highlighted in research on education and child psychology points to the causal effect of parental involvement in children's education on educational outcomes.[2]

[1]For US colleges, income has become increasingly predictive of enrollment for men in a way that cannot be explained by ability or college tuition (Lovenheim and Reynolds, 2011). Admission of middle-class students would be substantially higher if admissions were based on SAT scores alone (Chetty et al., 2020). When school access is based on proximity, the quality of school is priced into the housing market (Black, 1999). Even in the case of free school choice, the opportunity to choose tends to be more utilized by families with higher socio-economic status (Ambler, 1994; Skolverket, 2003). Belley and Lochner (2007) describe how the correlation between family income and college enrollment has increased over time.

[2]See See and Gorard, 2015, for a review.

In this paper, we study the effect of a parental income shock on children's educational outcomes beyond credit constraints, in the context of upper secondary school track choice in Sweden. As in most OECD countries, Swedish families choose whether their children should pursue a vocational or theoretical education at age 15.[3] This setting is particularly helpful because education is free, but parents' involvement in this complex choice is crucial. We estimate the effect of parental economic stress during this choice on children's long-term educational outcomes. This allows us to capture any non-financial constraints on low-income parents' ability to provide optimal levels of education for their children.

We estimate the impact of family-level economic insecurity caused by a parent getting laid off. Layoffs are registered in our data when an employer needs to terminate the employment of five or more workers in a single region due to a long term reduction in labor demand. This decrease is driven by planned or unplanned shocks at the firm level such as plant closures, reduced demand for the firm's output, reorganisation of production, etc. Which employee is affected is predetermined by tenure or prior collective bargaining agreements. Therefore, we can rely on the exact timing of the layoff event to be exogenous to the characteristics of an individual's family, primarily the age of his children at the time. This allows us to use variation in the child's age and school progress at the time of the layoff event to estimate the heterogeneous impact of a parental layoff at different points in the child's life.

We find that children in families with a parental layoff are significantly less likely to finish high school than their peers. The effect is concentrated to children for whom the parental layoff coincides with the time when they transition from compulsory to upper secondary school (ages 15–16). The estimated likelihood

[3] The OECD average of the starting age of upper secondary school is 15.2, in Sweden it is 16. (OECD, 2019, Table X1.1b (2014))

of completing high school on time decreases from 73 to 58 percent for children whose parents are laid off 6–12 months before the school transition. For children who are already enrolled in high school at the time of the shock, graduation probability drops around 3 percentage points. We do not find any evidence that children leave high school to take gainful employment.

The effects appear to be driven by parents' time investment in their children's education. The effect of parental layoffs are larger when parental involvement in children's schooling is necessary, and the cost of such involvement is higher. The negative effects are larger when the layoff occurs before the time when children apply for high school, a time when parental support is crucial, than layoffs that happen within the same school semester but after the application deadline. Additionally, families with less information about the high school choice at the time of layoff are more adversely affected than families with more information. We use siblings as a proxy for the information level of the family at the time of the event. If the shock affects the school choice of the oldest child, the estimated effect on graduation is large, but for younger siblings, the estimated impact is not statistically different from 0. Since a younger sibling has the advantage of more family-level information about the choice prior to age 15, she will not be as adversely affected by a reduction of parental investment at the time.

Our findings contribute to the literature on the scarring effects of layoffs as well as to understanding the role of parental income in children's education. A long-standing literature documents that involuntary job loss leads to large and persistent negative effects on the displaced workers' subsequent earnings, labor force participation and employment stability as well as adverse effects on mental health.[4] The evidence on how a layoff affects the next generation, however, is

[4]For notable contributions on the effects of job displacement on earnings and unemployment, see e.g. Jacobson et al. (1993), von Wachter et al. (2009), Couch and Placzek (2010) and Stevens (1997) for the U.S. and Eliason and Storrie (2006) and Seim (2019) for Sweden.

mixed.

There is some evidence of an effect of parental job loss on children in the short run. Rege et al. (2011) find a negative impact on children's GPA at age 16 when fathers loose their job due to plant closures. Children also appear to obtain some information from parents' unemployment, and are less likely to study in the same field as their parent if the layoff occurs during the child's teenage years (Huttunen et al., 2019).

The effect on children's future earnings is less clear. Oreopoulos et al. (2008) find a statistically significant 9 percent negative effect on future earnings for children aged 10-14 at the time of parental job loss, while Mörk et al. (2019) and Hilger (2016) find no indication of negative future labor market outcomes for children who experienced a parental job loss. Mörk et al. (2019) find no effect on high school graduation rates, early unemployment or utilization of social assistance for parental job losses at child ages 6-18. Hilger (2016) finds no significant effect on college enrollment and only small effects on the education quality for parental layoffs at child ages 18-22. The different results may be driven by the different institutional settings or the differences in methodology. Oreopoulos et al. (2008) and Mörk et al. (2019) use firm closures to identify exogenous job losses, while Hilger (2016) uses layoff data. It could also be the case that effects of parental job loss on children are heterogeneous by age. We contribute to this literature by explicitly measuring heterogeneous treatment effects over the child's age and education, and testing the institutional mechanisms behind the differences across age. Similar to Hilger (2016), we use layoffs to identify job

Job loss also increases stress and mental distress, which affects health outcomes. Sullivan and Von Wachter (2009) and Eliason and Storrie (2009) find considerably higher mortality rates among displaced workers, while Black et al. (2015) and Mörk et al. (2019) show that job loss increases show that job loss increases the risk of cardiovascular health problems and hospitalizations due to alcohol use and mental health problems. Brand (2015) provides an overview of the sociological literature on the effects of job loss on the well-being of parents and children.

losses, as this method provides a natural control group, reducing the concern for selection into jobs based on family-level unobservables.

We also contribute to the literature on the role of parent income in children's education. Parent income matters for children's educational attainment (Lovenheim and Reynolds, 2011). Yet, Heckman and Mosso (2014) summarize the research on the role of family income in education by concluding that there is little evidence to support a pure cash transfer to poor families as a successful method to increase children's education. Lovenheim (2011) also finds little direct evidence that credit constraints are driving the increasing gap in educational attainment. Parenting does matter for children's outcomes, however (see See and Gorard, 2015 for a review). Intervention studies have shown that increasing parent's information and involvement can have positive behavioral effects on their children (see for example Rogers and Feller, 2018; Spoth et al., 2008; Barrera-Osorio et al., 2020). We contribute to understanding what affects parental involvement in absence of active interventions. We also link this to economic insecurity, contributing to a fuller understanding of why family income matters, beyond credit constraints.

The remainder of the paper is structured as follows. Section 2 describes the Swedish school system and the administrative datasets we use in the analysis. Section 3 discusses our empirical strategy. In section 4, we first discuss the effect of a layoff on parents, after which we look at the outcomes of their children. In section 5, we test potential mechanisms and narrow the most sensitive time for children down to a few months before the high school choice. Section 6 concludes.

2 Background

2.1 School choice

Upper secondary school in Sweden, like most OECD countries, offers students age 16–18 a choice of specialization through a number of different vocational and theoretical degrees.[5]. This section describes the details around the timeline and the choice market that prospective upper secondary students face.

Figure 1 illustrates the Swedish school system. There are 18 nationally standardized programs which can be either theoretical or vocational. A vocational program teaches occupation-specific skills as well as academic subjects, but may not ensure university-level qualifications. Around 30 percent vocational high school graduates do attend at least one year of higher education, compared to 70 percent of graduates with a theoretical degree (see Table 1).

The choice environment varies by location and time. Since the school reform in 1992, establishment of privately run schools have changed the market. 53 percent of 15 year-olds in 2000 had access to most[6] nationally standardized programs, 71 percent had the same access in 2010. The average number of schools per market has increased from 9 to 12.5. At the same time, the markets are fewer, which means that more students commute across municipalities to their high school. Furthermore, the share of potential students who live in a municipality with no school at all has grown from 33 percent to 42 percent (see Appendix Table A1 for details; see also Fjellman et al., 2019).

[5]At least 40 percent of upper secondary school students attend vocational programs in Australia, Austria, Belgium, Brazil, Chile, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Italy, Japan, Korea, Latvia, Lithuania, Luxembourg, The Netherlands, Norway, Poland, Portugal, Russia, Slovakia, Slovenia, Spain, Sweden, Switzerland and Turkey (see OECD, 2019, Figure B3.3).

[6]at least 15 out of 18

Students who wish to attend high school need to apply to programs in February, their last semester of compulsory school (the exact deadline varies by year and municipality, usually between February 1st and February 15th). Students usually apply to 2–4 programs, ranked by preference (Table 1). Most students who passed the minimum requirement for compulsory school completion are accepted to their first choice, but in the case of limited slots, allocation is based on grades. The preliminary admittance is based on grades in the Fall semester of grade 9 and final admittance is based on the final grade. National exams, which are important for determining the final grades, are administered in late April and early May during the final semester of grade 9, when students are 16 years old.[7]

A failure to pass necessary subjects in primary school will require the student to finish those subjects before entering high school. An individual program is set up for the student, which is designed to on-board the student into a vocational degree of her choice, but may take more than the standard three years (Skolverket, 2020).

2.2 Data

This section describes the Swedish administrative data used in the analysis. We combine individual data on employment, earnings and educational attainment with data on family links and a detailed registry of all layoff events.

The notification registry includes firm layoff notifications of five or more workers between 2005–2013. By law (Lag [1974:13]), all firms at risk of laying off more than five full-time employees in the region must notify the local unem-

[7] School classes are based on year of birth, and the academic year runs from August to June. In the rest of the paper we discuss child age in years since birth, abstracting from school grade, as the two definitions overlap.

ployment agency office between 6 weeks to 6 months before the termination of employment. At the individual level, we observe the termination date, which can be several months after the first notification at the firm-level. We treat the termination date as the relevant time of layoff, as this is the time when income drops and the employment is terminated.

Layoffs are driven by a reduction in labor demand. Stated reasons for notifications include plant closure, bankruptcy, reduction in production or changes in the production process. Separations due to worker misbehaviour are not included in this dataset. See appendix A.1 for a detailed description of the layoff and unemployment process. While employees facing layoffs are still negatively selected based on observables compared to the population at large, the restriction to firm-induced layoffs implies that the exact timing of the layoff is exogenous to employee characteristics. Appendix Figure A2 shows the variation over time in the total number of layoffs and the characteristics of the affected employees. Pre-layoff income increases for layoffs after the great recession, while the share of layoffs in the manufacturing sector decreases.

We restrict our parent sample to individuals who experience a layoff or individuals who are at risk of layoff. We define an employee as having a non-zero risk of layoff if she is currently working at a firm which ever appears in the layoff data. This excludes most public sector jobs as well as a lot of jobs in sectors dominated by individuals with a university degree. For a comparison of the layoff sample to the general population, compare columns (1) and (5) in Table 2.

Our population of interest are children who may be affected by parental layoff. As the notification data covers layoffs in 2005–2013, we focus on families with children born between 1980 and 1992. This ensures that we can have a balanced panel of child outcomes for children aged 13–23.[8] The panel data follows individuals from age 18 (or 16 if they are employed) and onward. We know the biological parents of the children, and can also observe the household they live in when they first appear in the data. We can also observe whether both biological parents are in the same household in any given year. We observe the highest level of education by year: high school is registered after graduation, while college is registered as years of enrollment. High school graduation usually occurs at age 19, but we see students who finish at age 20 and 21 as well. Students enroll in university at ages 20–23. See Appendix Figure A1 for details on education levels by age. We also observe the field/major of education.

For parents, we observe a panel of income and employment from 1991–2015. Employer-employee match data indicates which individuals are currently working at firms in our layoff data, which makes it possible to identify individuals who are employed at a firm with layoff risk. We focus on parent income and industry of employment as matching characteristics, as well as demographic variables. Table 2 shows the characteristics of the parents in our sample, as well as in the population at large. These tables also illustrate the matching procedure, which we turn to next.

3 Empirical strategy

To identify the effect of a parental layoff on children's educational outcomes, we rely on variation in the child's age at the time of layoff, as well as matching families who experience parental layoffs to families who do not, but where a parent is at risk of getting laid off. In this section, we first describe the restric-

[8] See Appendix Figure A3 for the distribution of observations over the year $\times$ cohort $\times$ age distribution.

tions on the control group and the matching procedure. Second, we discuss the assumption that the exact timing of parental layoff is exogenous of the child's year of birth. Third, we describe how this assumption is used to estimate the expected difference in potential outcomes between the treatment and control group, and we explain our estimation procedure.

In addition to families who experience a parental layoff, we use families in which one parent is at risk of layoff as a control group. We define all firms in the notification data as firms with a non-zero layoff risk, as they all have had at least one negative shock causing layoff over the 8 year period in which we observe layoffs. Any individual who is working for a firm that ever appear in the layoff data is considered to have a non-zero risk of layoff.

A parent in our control group will not be laid off in year t and will still be employed at the firm in t+1. Her exposure to layoff risk can either come from a layoff at the firm in year t where she wasn't notified or from a layoff in a different year, before or after t. We do not require the control to be laid off in a future event, nor that she still is employed by the firm when it happens. So while we ensure all parents are exposed to risk, we will have a pure control group and not have to adjust or estimates for staggered event times.[9]

In addition to this restriction of our control group, we also perform a coarse exact matching on child and parent characteristics to ensure the treatment and control group are similar based on observables. Initially, we do an exact matching of the gender and birth year of the child, as well as the gender of the parent. For a given event year, we match parents who are laid off in this year to parents of the same age who are at risk of layoff by the virtue of being connected to an at-risk firm

[9]See Borusyak and Jaravel (2017) for a discussion of a pure control group as a solution to the problem of staggered event studies, highlighted for example by Abraham and Sun (2018) and Goodman-Bacon (2018).

the same year. For the families in the control, this year will be their potential, or placebo, layoff year.[10] We also match the families based on their income and industry of employment (2-digit level). We match all families on income and industry characteristics from when the child is 12. This ensures that the control group includes families with similar childhood experiences, regardless of when in the child's life the parental layoff occurs.

Table 2 presents the result of the matching. Column 1 and 2 show the parents with a layoff and the parents at risk of layoff (Layoff and control group) without additional matching on child and parent characteristics. Column 3 and 4 show the result of matching the control group to the layoff group. Column 5 shows the overall population over 30 over the same time range. Restricting the control group to parents with a non-zero risk of layoff is the most important step to ensure a good match on observables. Adding the match on child and parent characteristics increases the precision of the similarity between the groups on the set of matched characteristics and on parental education level and marriage rates. Even after matching, there are some observable differences between the treatment and control group, for example in the tenure at the current job. Tenure is a good example of a variable that we know drives the likelihood of layoff, due to the mandated last-in first-out principle. There are likely to be other factors, related to layoff probability but unobservable to us, that are not balanced over the treatment and control group by this matching procedure. Therefore, to adjust for selection on observables we also rely on variation in the exact timing of parental layoffs. This is where we turn next.

As layoffs are induced by negative shocks to firms, we assume that the exact timing of the layoff is exogenous to the age of the child at the time of layoff.[11]

[10] We allow control families to enter with several different potential layoff years, as the exact matching procedure will reduce the weight of the observation accordingly.

[11] see the discussion of the definition of a layoff in or data from Section 2.2

Even if unobserved family characteristics that predict both the future educational attainment of the children and the likelihood of layoff for the parent, the exact arrival time of the layoff shock has to be unrelated to the birth year of the child. Figure 2 does not reject this assumption, as it shows that conditional on event year and parent age, we find no significant correlation between layoff probability and child age in the sample of parents at risk of layoff.

If potential outcomes differ systematically between the treatment and control group (selection into layoff) then we can use the random variation in the age of the child to estimate this difference. Let $Y_i(Z)$ be the outcome for an individual i after receiving the treatment Z, where $Z \in \{0, z\}$ can be either no parental layoff, $Z = 0$, or a parental layoff in the year the individual is z years old, $z \in [14, 25]$. Let S_1 be the set of treated individuals and S_0 be the set of non-treated individuals. Under the assumption that selection into layoff is independent of the age of the child at the time of layoff, then the expected outcome for any $Z = z$ are the same. For example, will be as-if random whether the child experiences the parental layoff at age 18 or age 20, $E(Y_i(Z)|z = 18, i \in S_1) = E(Y_i(Z)|z = 20, i \in S_1)$.

Now, consider the main outcome of interest in our analysis, on-time high school graduation. This is a one-time event, which either realizes or not at age 19. If a child is treated at age 20, this can not have any effect on his outcome, as it was already realized last year. Therefore, the potential outcome for treatments at age 20 or above will be equal to the potential outcome of not being treated at all. $Y_{i,19}(z \geq 20) = Y_{i,19}(Z = 0)$. This allows us to obtain an estimate of the counterfactual effect of not being treated for the control group.

$$E(Y_{i,19}(z \geq 20)|i \in S_1) = E(Y_{i,19}(Z = 0)|i \in S_1)$$

$$E(Y_{i,19}(Z = 20)|i \in S_1) - E(Y_{i,19}(Z = 0)|i \in S_0, \text{age} = 20) =$$

$$E(Y_{i,19}(Z = 0)|i \in S_1) - E(Y_{i,19}(Z = 0)|i \in S_0)$$

Any difference between the treatment and control group observed at age 20 is therefore entirely driven by differences in potential outcomes. Also note that if we assume that the timing of layoff would be random also for individuals in our control group, the difference in potential outcomes between treatment and control does not vary by age. Hence, we can use the estimated difference in outcomes at age 20 as an estimate of the average difference in potential outcomes for all age groups. $(Y_{i,19}(Z = 20)|i \in S_1) - E(Y_{i,19}(Z = 0)|i \in S_0, \text{age} = 20)$ is observable, and we use the estimated value of this difference to normalize the empirical estimation of treatment effects.

Equation 1 shows the regression specification we use to estimate the effect of parental layoffs on children's educational outcomes. Note that, even though we have panel data on families, the regression is cross-sectional on the child level. The outcome of interest, y_{i,a_O}, is observed once for each child i, at age a_O. The variable of interest is the layoff status of the parent at the child's age and semester in school (a_E). In order to capture the different schooling contexts for children at the time of parental layoff, we define age as the age in semesters, from January – June when the child is 13 ($a_E = 1$) to July – December when the child is 25 ($a_E = 24$). As we are interested in the heterogeneous effect of parental layoffs over the child's schooling, we will estimate a set of coefficients

δ_{a_E} to describe the average effect of parental layoffs in each age-semester a_E.

$$y_{i,a_O} = \sum_{a_E=1}^{24} \delta_{a_E} \mathbf{1}_{[\text{Layoff}_{a_E,i}=1]} + \tau_c + \gamma_{t_E} + \beta X_i + \varepsilon_i \tag{1}$$

A child in the control group will have no positive dummy $\text{Layoff}_{a_E,i}$. The control group is crucial to capture the unequal distribution of time, age and cohort (Appendix Figure A3 illustrates this issue) by cohort and event year fixed effects, τ_c, γ_{t_E}. Control variables X_i are captured by the matching procedure described above and are either time invariant, measured at age 12, or at the year of (potential) layoff. The results are also robust to including separate event year fixed effects for the treated, to alleviate any concern about differential selection into treatment over time.

Any of the estimates δ_{a_E} for $age_E > a_O$ is a valid estimate of the average difference in potential outcomes between treatment and control group. To mimic the presentation of a conventional event study, we normalize the effect using the youngest of the post-outcome group. However, we include all estimates for children above the outcome age up to age 25 in our figures, to allow the reader to assess the validity of the assumption that any of the older children can be used to estimate the bias due to selection in unobservables. These estimates are in the shaded area in Figures 4 to 7.

4 Results

This section describes how children's educational outcomes are affected by parental layoff. We begin by establishing that layoffs lead to increased economic insecurity within the family through an increase in unemployment risk and earnings variability, along with a decrease in earnings in the short-to-medium run. We then move on to the impact on child high school graduation rates. We find that children who are transitioning from compulsory school to high school are adversely affected by parental layoff, with lower graduation rates from high school and no increased uptick in employment. Finally, we study mechanisms and find that the layoffs that coincide with the high school choice have the most negative impact, and that the effects are higher for families with higher information costs associated with the high school choice.

4.1 Parental Layoffs and Earnings

The initial step, or first stage, in the intergenerational transmission of a layoff shock is to understand how parents are affected. In this section, we document the short and medium term effects of layoff on parents' earnings and unemployment. We show that insecurity about the future earnings increase, and we interpret a higher level of uncertainty as an important effect of the layoff.

Labor earnings for parents who get laid off initially drop by 20 percent relative to the control group, and the gap remains economically significant even 7 years after the layoff, as shown in Figure 3a. This is in line with previous findings on displacements in Sweden (e.g. Eliason and Storrie, 2006; Mörk et al., 2019; Seim, 2019). As explained in Section 2.2, we define a layoff as a termination of employment induced by the employer due to a reduction in labor demand at the firm. Layoffs do not necessarily lead to unemployment, as there may be sufficient time between the time of notification and termination for the employee to find a new job. Figure 3b shows that only 50 percent of laid off parents register as unemployed in the following year.

Conditional on unemployment, annual labor income falls by almost one third in the year after layoff, see Figure 3c. Unemployment insurance (UI) covers up to 80 percent of pre-unemployment income, which is reflected in the effects on disposable income shown in Figure 3d. Unemployment insurance coverage is not automatic, and any severance pay affects the start of UI benefits.[12]

In addition to a decrease in expected disposable income due to unemployment or a worse match with the next employer, the variability of earnings increases at the time of layoff. Two years after the employment shock, disposable income is lower than pre-layoff for 75 percent of the sample, and the spread between the 25th and 75th percentiles of labor income in the layoff sample increases by 50 percent 7 years after layoff relative to the pre-layoff range. (Appendix Figure A5 shows the increased range of income after layoff).

We will refer to this combination of a spike in probability of unemployment, a moderate drop in expected future earnings in both the short and the long run and an increase in variability of earnings as *economic insecurity*. Getting fired is a major factor of increased stress (Holmes and Rahe, 1967), and even in the Swedish context where the unemployment insurance is relatively generous, there is evidence that job loss causes an increase in mental distress and harmful behaviors, in the worst case leading to premature death (Eliason and Storrie, 2009).

4.2 Parental Layoffs and Children's Educational Outcomes

We now turn to our main results - the impact of the increase in economic insecurity on children's educational attainment. We find large effects for children

[12]See Appendix A.1 for a detailed discussion of the layoff procedure, UI benefits and severance pay.

who are about to transition to high school, and we find some evidence that the impact is larger for families where the economic insecurity from a layoff is more severe.

The impact of parental layoffs on children's high school graduation rates are concentrated to layoffs that occur in the last three semesters of compulsory school, right before the transition to high school. Figure 4 shows the estimated age-semester effect on the graduation rate of children with a laid-off parent relative to children in the control group in the same age-semester. Recall that all estimates shaded in gray are estimates for children who are older at the time of treatment than at the time of outcome. These coefficients serve as an additional control group, showing that there is no significant difference between the estimate at age 20 (used for normalization) and any of the other ages where we expect the treatment effect to be zero. We denote the time of parental layoffs in the spring semester (Jan-June when the child is 13 as 13.0 and a parental layoff in the fall semester (July-Dec) as 13.5 for each age 13.0-25.5. In Figure 4, the dotted line at age 16 represents the end of compulsory school and start of high school. The dotted line at age 19 represents the high school graduation age for students who graduate on time.

Parental layoffs prior to child high school enrollment have a significant negative effect on children's eventual graduation rates. In Figure 4, we see a clear dip in the graduation rates of students who experience parent layoff in age 15-16, right around the completion of compulsory school and transition to high school. The impact on high school graduation rates for students in the semester prior to compulsory school graduation is large, decreasing the expected on-time graduation rate from 73 percent for students with no family shock to 58 percent for students who experienced a parental layoff at this time in their school life (age 15.5). Students already enrolled in high school at the time of parental layoffs (ages 16.5 to 19) are not significantly less likely to complete high school than their peers. Tables 3 and 4 show the joint significance test for students of high-school age.

To drop out of school without a completed high school degree is associated with a substantially lower lifetime earnings than a high school graduate. In our sample, earnings are up to 48 percent lower for dropouts compared to high school graduates in the first years out of high school, see Appendix Table A2. The earnings of high school dropouts in the population follow a parallel income path to high school graduates at 60 to 75 percent of earnings up until at least age 30.[13] As children's earnings may be impacted by local labor market shocks that are correlated with parental layoffs, we are hesitant to argue that our empirical framework would be valid to study the impact on intergenerational income correlation directly. Nevertheless, we do observe a negative – albeit imprecisely estimated – effect of parental layoffs on child earnings in their early 20s precisely for children of age 15.5 at the time of layoff (see Figure 6b).

We do not find any evidence that children drop out of school to find employment to support their family financially. In addition to lower expected earnings later in life, children who do not graduate high school are not earning more than their peers at ages 16–19.[14] Figure 6a shows the estimated effect on children's cumulative earnings in their late teens by parental layoff. We find no significant impact, but the point estimates are noisy due to the low share of teenagers with observable income in our sample. In the population, those who will not get a high school degree by age 19 earn less from age 16 onward than those who

[13]See Appendix A.2 for a detailed description of earnings by education level.

[14]Note that we define high school dropouts by the absence of high school graduation. Therefore, students who are still attempting to complete their degree but fail to ever pass the minimum number of classes required will be considered dropouts. This will not allow for time to seek outside employment.

will (see Appendix Figure A6). Employment during high school can be facilitated through vocational program internships, increasing the teenage earnings of students in vocational relative to theoretical high school tracks.[15]

Children who do not graduate from high school on time at age 19 may still be able to graduate later, in which case the economic impact would not be as severe. Our estimates, however, are robust to looking at graduations by age 21, thus allowing for two years of grade repetition or complementary education to finish primary school. Appendix Figure A8 shows graduation rates by age 21, when 98 percent of all individuals who will ever complete high school have graduated. Even here, we find a drop in graduation rates of 9 percentage points for students in the last two semesters of compulsory school. Even a delay in graduation age (i.e. someone who has finished by age 21 but not by age 19) signals lower educational achievement than on-schedule graduation. Students who delay graduation tend to have lower earnings during their high school years, earning 20 percent less than on-time graduates during high school, and 14 percent less at age 22 (the first year after graduation).[16]

Parental layoffs do not affect the graduation rate from academically more demanding degrees. Figure 5a shows that graduation rates for scientific degree programs are unaffected by parental layoffs for all ages. These degree programs are usually attended by highly motivated students.[17] Graduation rates from vocational degree programs are depressed for students who are about to start their high school, and to a smaller extent for students already enrolled at the time of parental layoffs (see Figure 5b). This is in line with the literature on parental involvement which focuses on at-risk or low-achieving students (See

[15] See A.2 for more details on children's earnings.
[16] Calculated from Table A2.
[17] Note that we do not observe grade enrollment in real time, but only after graduation. Hence, we do not know which program dropouts enrolled in, if any.

and Gorard, 2015).

To summarize, we find that the increase in economic insecurity caused by parental layoffs can have large negative effects on children's likelihood to complete high school, if the shock occurs when children are transitioning from compulsory school to high school. We find that the impact doesn't vary by the size of the income shock, but that academically strong students are insulated from the shock as they do not appear to change their choice of track to less prestigious degrees or fail to graduate. We now turn to examining the drivers of the sizable effect for this particular age group.

5 Mechanisms

5.1 Parent income

The size of the income shock following parental layoffs does not significantly alter the impact on children's high school dropout rates. The realization of the economic uncertainty caused by the layoff will occur after the layoff date, and the duration of the shock varies. Hence, our current setup allows us to capture the effect of the uncertainty, not the effect of a material decrease in living standards.

We do not find significant evidence of larger effects on households with more pre-layoff economic insecurity. Figure 7 shows the split between households with better or worse economic circumstances at the time of layoff. In panels 7a and 7b, we see the estimated effects for children with a single earning father getting laid off relative to a father layoff in a two-earner household. The effects appear to be larger for the single earner family, but are not statistically different from each other. Similarly, panel 7c and 7d show families with above or below median pre-income earnings. Here, the trends are more similar and not statistically different. We also show the split based on the realized outcome of the number of days in UI in panels 7e and 7e. This outcome occurs after the intervention of interest, the layoff date, and we are therefore careful in interpreting the results. We believe the actual days in UI can be informative about the perceived severity of the layoff shock at the termination date, but we acknowledge that this is a bad control. F-tests of the difference between coefficients to the right and left in figure 7 can be found in Table 5).

We conclude that better economic conditions prior to the layoff shock do not insulate children from experiencing the economic uncertainty caused by a parental layoff.

5.2 Transition to high school

There are two potential reasons why the transition between compulsory school and high school can be a vulnerable time in the student's education path. First, consider the effect of enrollment on high school completion rates. Schools are incentivized to help struggling students in their programs to completion, but not to take on new students who do not appear to meet their academic standards. A shock to academic performance prior to high school enrollment may lead to students not getting into their preferred school, or require students to retake classes prior to enrollment. If a student was already enrolled in the high school, when the shock happened, she might get additional support. Second, consider the high school choice. As described in Section 2.1, the choice of high school requires students and their parents to know the expected labor market return to at least 15 different education tracks as well as to individual schools. This is a family-level decision with a high degree of parental involvement (Skolverket, 2003, Chapter 5). Not having sufficient information and parental support can lead to a suboptimal school choice which decreases the child's likelihood of graduation. As high school is not mandated by law, teenagers with a higher discount factor may even take this opportunity to not apply to any schools at all.

We first differentiate between these two channels by considering the exact timing of the parental layoffs relative to the child's school calendar. We focus on two important dates in the last semester of compulsory school: the high school application deadline in early February and national exams in the second week of May.[18] February and May deadlines correspond to the school choice and academic performance shock channels, respectively. We detect no change in graduation rates for layoffs around the time of national exams, see appendix figure A9 for the raw data on families with parental layoffs only. We do, however, find a need to investigate the drop around the high school application deadline further.

Figure 8 illustrates the effect of a parental layoff right before the february deadline. We show a nonparametric estimate of the change in outcome by child age at the time of layoff, allowing for an interruption in the time series in February. Layoffs right before the school choice deadline in February lead to significantly lower high school graduation probabilities for children than layoffs with termination deadlines after February as shown in Figure 8. The nonparametric specification and the selection of the optimal bandwidth follow the approach in Calonico et al. (2014), treating the time series interruption as an RD cutoff.

[18] The exact test dates and application dates vary by school year. We use February 1st as the application deadline, as the historical application dates have varied between February 1st and February 16th.

We find no significant effect of parental layoffs around the time of the national exams. This contrasts with Rege et al. (2011) who find a negative effect on final grades at age 16 in a similar setting in Norway. As we do not observe grades directly, we cannot reject any effects on grades following layoffs around May in the last semester of compulsory school, but we do not find any medium run effects of shocks close to the final exams. To the extent that compulsory school grades are affected, we do not observe any direct effect on high school graduation from grades, separate from the effect on high school application behavior.

The point estimate at the February cutoff is robust to a wide selection of bandwidths (see Appendix Figure A10) and the inclusion of controls (see Table 6). Figure A10 shows the robustness of the point estimates with respect to the bandwidth of the nonparametric estimate. The standard errors are chosen based on the discussion of optimal bandwidths for regression discontinuity designs (Imbens and Kalyanaraman, 2011; Calonico et al., 2014). The point estimate at the cutoff is robust to including or excluding controls and event year fixed effects, see Table 6.

Figure 9 shows the smoothness around the application deadline cutoff in density, pre-determined variables and parent characteristics. There is no change in density around the February cutoff, but it should be noted that higher ages in calendar years can include more cohorts, and we therefore see an increase in observations for older age group (panel 9e). Pre-determined variables such as parental pre-layoff income (panel 9a) and the birth year of the child (panel 9c) are not different on each side of the cutoff. The characteristics around parental layoffs also appear to be similar around the deadline, with an equal number of days in unemployment (panel 9d) and similar earnings in the two years after the layoff (panel 9b).

We find that the estimated difference at the cutoff is larger for parents with a binding termination date. As the time between the first individual notification and actual termination can be several months, the termination date is not necessarily binding for all parents. If the parent has already managed to secure a new job prior to her termination date, she might actually have more time to help her child with their education than in the absence of a layoff. Appendix Figure A11 shows the result for parents who transition from the employer who laid them off to registered unemployment. For this sample, we know the termination date is binding, as they have not found new employment before termination. For families with their first unemployment date the day before the high school application deadline, the estimated effect implies a drop of 23 percentage points in the probability of completing high school on time (see Table 6).[19]

The results from Figure 8 are consistent with a model of limited parental time with differential returns to parental involvement depending on the school activity. After the layoff is announced, the parent will need to devote more of her leisure time to job search, which decreases the time available to engage with her children's schooling. Increased stress due to economic insecurity may also have a negative impact on the quality of parental involvement, holding time constraints fixed. See and Gorard (2015) identify two mechanisms through which parental involvement affects children's educational outcomes: improved learning and aligned expectations. Involved parents improve learning by functioning as an additional teacher, or they can reinforce the school's message about the expectations about student behavior and the importance of going to school. Spoth et al. (2008) find that increasing parental competency and communication skills has a positive effect on educational outcomes of children in the same age group as in this case (the intervention takes place at age 14 and outcomes

[19] The interpretation of this result should be done with caution, as we are conditioning on an event that occurs after the treatment.

are observed at age 18). In our case, parental involvement in the high school choice process may be important to communicate the importance of going on to high school (which is voluntary), as well as expectations about what kind of track choice would be acceptable. Parents may be better suited than teachers to help students understand their individual returns to different schooling options. In order to credibly be involved, parents need to be informed about the current state of the high school market.

Informational costs related to the high school choice are high (Skolverket, 2003). In the average high school market, there were 10 different schools to choose from in 2005, with the number rising to 16 different schools by 2013 (see Appendix Table A1). Schools advertise themselves to prospective students and their families primarily through visiting days (evenings) outside of regular office hours, which means that unemployed parents have no time advantage.

We proxy for the family's knowledge about the high school choice market using the sibling order of the child whose high school choice is impacted by parental layoff. Given the nature of knowledge, the cost of gathering information is going to be highest the first time a choice is made. Repeat choices may require some updating, but we assume that families don't unlearn over time (similar to Chetty et al., 2013).[20] Therefore, the cost of parental involvement in the high school choice of the oldest sibling is going to be higher than the cost for younger siblings.

Figure 10 shows the estimated effects for the oldest (or only) child in the family (panel a) and younger siblings (panel b). For older siblings, we estimate a

[20]In Chetty et al. (2013), the authors argue that if knowledge about the specifics of the tax code is present in a location in one year, people will still remember the following year, so information can only grow. In this paper, we argue that once the family has gathered information about the high school choice for their first child, the family will still remember once it is time to make the same choice for the next child.

clear negative effect of parental layoffs prior to high school choice. For younger siblings, the estimates are more noisy, but the point estimate at age 15 is only a third of that for older siblings, and not statistically different from 0. Table 7 tests the equality of the point estimates shown in Figures 10a and 10b. For each semester-age 15, 15.5 and 16, we test if the effect of parent layoff on the oldest sibling is equal to the effect on a younger sibling in the same situation. The effects are not statistically different for on-time completion (by age 19), but if we allow for delayed students to also complete their degrees (by age 21), we do find a significantly lower completion rate for oldest siblings who experience parental layoff, compared to younger siblings.

We find that the large drop in graduation rates caused by parental layoffs is most severe around the time of high school choice, a complex and individual choice where parental involvement is crucial. Evidence from the exact timing of the layoff favors the school choice over grades as the driver of the result, and we also find evidence that families with more prior information about the school choice are not as adversely impacted by the layoff.

6 Conclusion

We have explored a particular channel through which economic insecurity can affect children's educational outcomes. We show that children in Sweden are relatively insulated against economic shocks that affect their parents, except for when they are about to transition between compulsory school and high school. The high school choice is a complex decision which requires a lot of parental involvement. From the education literature, we know that that parental involvement (See and Gorard, 2015; Barrera-Osorio et al., 2020; Rogers and

Feller, 2018) and parental competency during adolescence (Spoth et al., 2008) has a causal effect on educational outcomes.

We use parental layoffs as a source of variation in the level of economic insecurity faced by parents. Data on individual layoffs initiated by the firm have several advantages relative to identifying job loss from plant closure. First, we ensure that the timing of the parental shock is independent of the child's year of birth, leading to exogenous variation in the age of the child at time of layoff. Second, we can identify parents at risk of layoff as a basis for the control group. Third, we know the exact layoff date, and can use variation in time relative to specific deadlines within the school year to identify the effect of a shock close to exams and application deadlines.

Parental layoffs which happen right before the child's high school application deadline have the largest impact on high school graduation rates. The impact is precise enough to exclude the effect of parental layoffs on grades as the driver of the effect on graduation rates. We also find that families who are likely to have less information about the high school choice are more sensitive to a parental layoff at the time of high school applications than families with more experience of the application process. This highlights the parent's role in communicating and aligning home and school expectations over the parent's role as an additional teacher.

Using layoffs as a natural experiment restricts our understanding to a scenario when the economic insecurity of the family sharply increases over night. We do not find evidence that the actual drop in material standards is what is affecting children's educational outcomes. At the time of layoff, the future economic outcome of the family is unknown, and we find that the economic insecurity affects children about to transition to high school negatively, regardless of what the future outcome of earnings is.

It is well established, however, that lower income families experience a higher level of economic stress and insecurity than more affluent families.[21] Our results highlight the link between economic insecurity, parental involvement and educational outcomes. This link can be an important explanation for the correlation between parental income and educational outcomes. As discussed by Heckman and Mosso (2014) and others, the causal link between parent income and child education is not very well understood. A model which reduces the income-education channel to credit constraints risks severely underestimating the causal effect of income on education in settings where credit constraints are low.

[21] See for example Sapolsky (2018) for neurological evidence.

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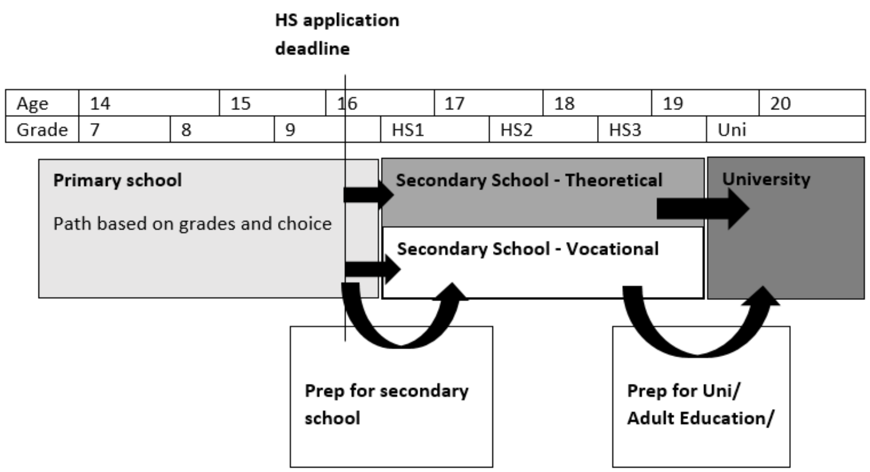
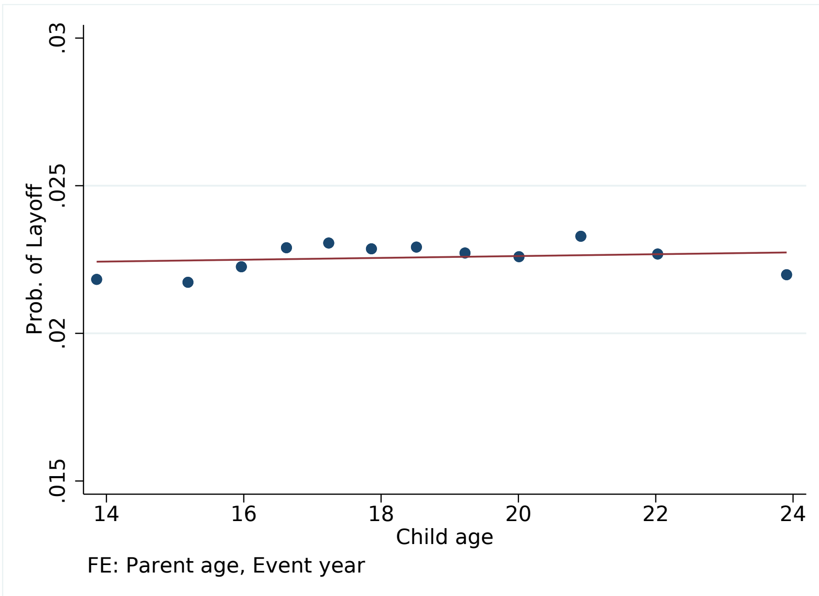
7 Figures

Figure 1: Schooling in Sweden



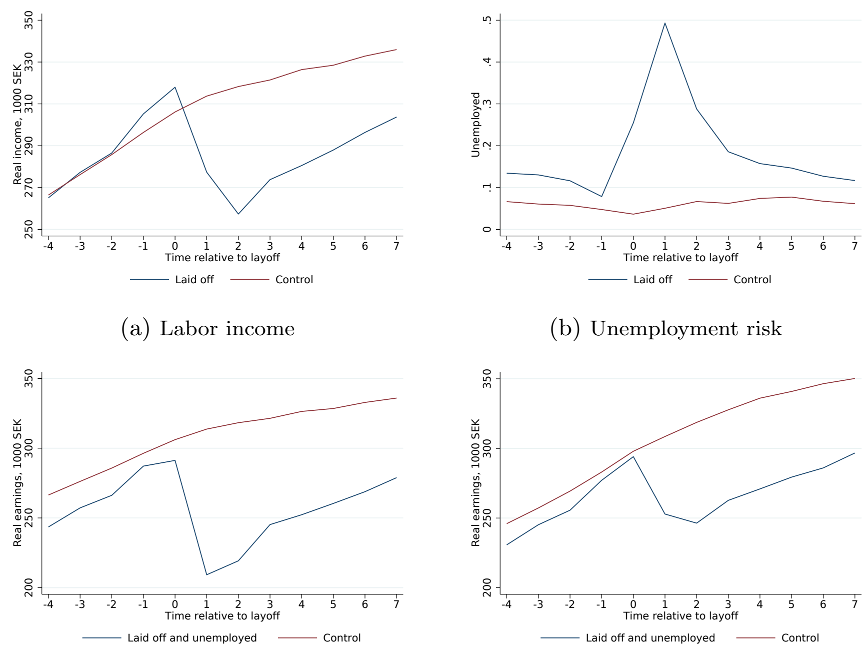
Notes: This figure shows the Swedish education system from grade 7 to university. The academic calendar runs from August to June, and class assignment is based on year of birth. This gives the overlapping of age (age 1 on January 1st, the year after birth year) and grade. In the second semester of grade 9, at age 16, students apply to the high school program of their choice.

Figure 2: Likelihood of parental layoff



Notes: The figure shows a binscatter of the likelihood of layoff in the sample of parents connected to firms in the Notification registry. Conditional on parent age and year of layoff, we cannot reject the null hypothesis that there is no linear relation between child age and parent layoff probability. The 95 percent confidence interval on the linear coefficient is (-.00005, .00011) with a p-value of 0.428.

Figure 3: Effects of layoffs on parent's outcomes



(a) Labor income

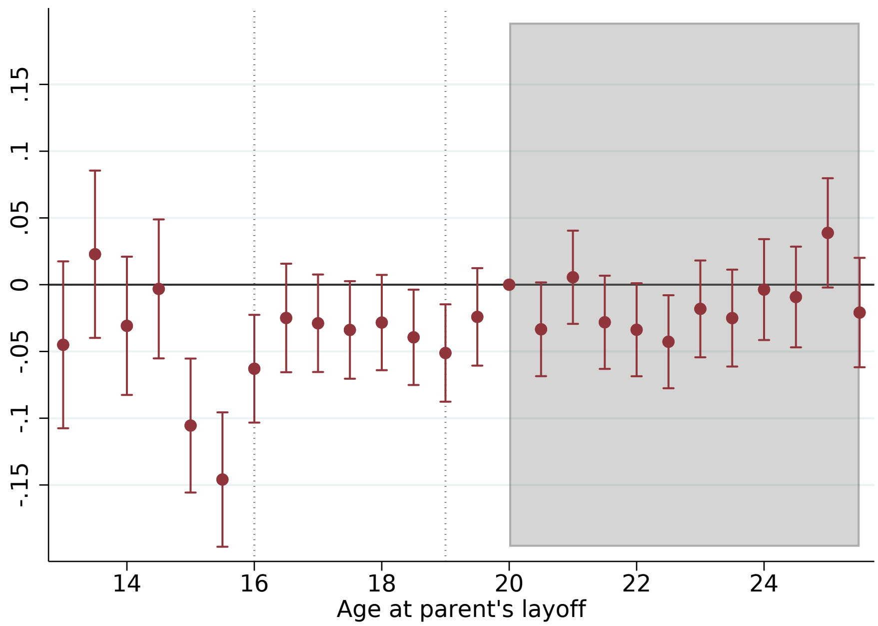
(b) Unemployment risk

(c) Labor income – conditional on unemployment

(d) Disposable income – conditional on unemployment

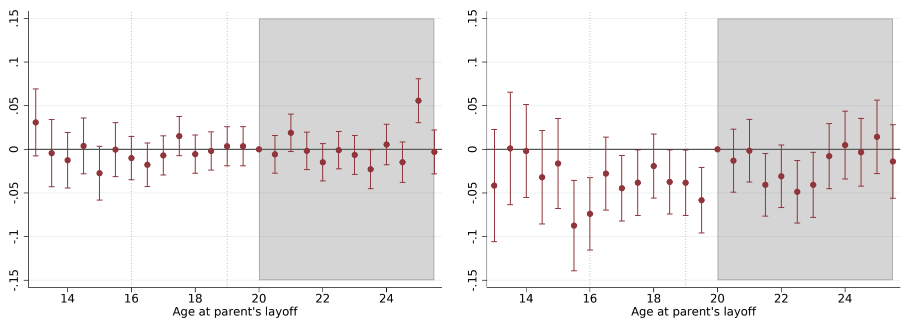
Notes: In panels (a) and (b), the layoff group is defined as individuals who appear in the layoff data with a termination date in event year 0. In panels (c) and (d), the layoff group only includes individuals who experience unemployment following layoff. The control group is weighted according to the procedure described in Section 3. Disposable income includes unemployment benefits and labor income. Both labor and disposable income are measured annually in 1000s of 2010 SEK. For panel (b) – note that the unemployment probability is higher for the layoff group, pre-layoff. Mechanically, this will happen as layoffs are determined by a law (Lagen om anställningsskydd, LAS) with a strong last-in first-out component. See Appendix Section A.1 for further details.

Figure 4: High school completion by age 19



Notes: This figure shows the estimated impact of parental layoffs on children's on-time high school graduation rate separately by child age at the time of parent separation. The dotted line at age 19 represents the time when students are expected to graduate from high school. The dotted line at age 16 represents the time when students transition from compulsory school to high school. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 1 for child ages at parent layoff of $a_E \in [13, 25]$, normalized to 0 at $a_E = 20, s = 0$. The regressions include cohort and event year fixed effects and are run on the matched sample. The outcome, high school graduation by age 19, is defined as a dummy equal to 1 if the child's highest education 19 years after birth is a completed 2- or 3-year high school degree or higher, and 0 otherwise. 84 percent of all students who will graduate high school have graduated by age 19. Estimates and F-tests are reported in table 4.

Figure 5: Graduation rates from specific degree programs

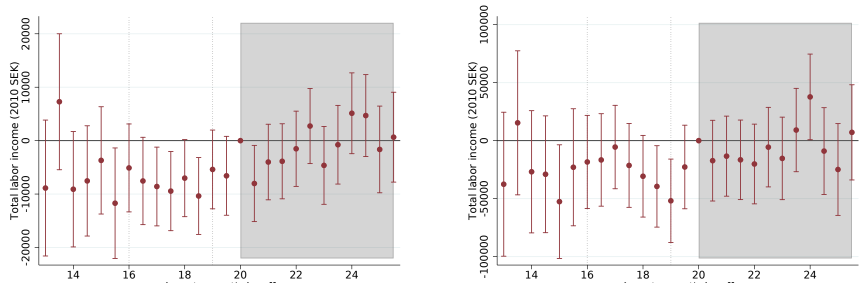


(a) Scientific grade completion

(b) Vocational grade completion

Notes: Panel (a) shows the likelihood of graduation from a scientific degree program (*Naturvetenskapliga programmet*) by age 19. Panel (b) shows the likelihood of graduation from any vocational degree program (*Yrkesprogram*) by age 19. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 1 where the outcome is graduation by age 19 from a scientific or vocational program, respectively. The regressions include cohort and event year fixed effects and are run on the matched sample. The dotted line at age 19 represents the time when students are expected to graduate from high school if they are graduating on time. The dotted line at age 16 represenst the time when students transition from compulsory school to high school.

Figure 6: Effect of parent layoff on child earnings

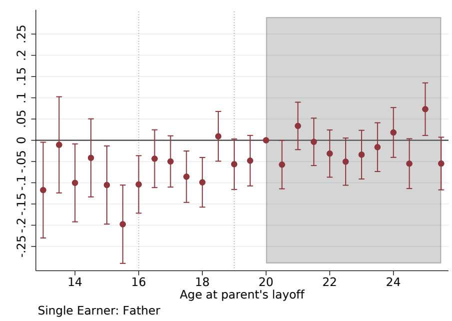


(a) Cumulative earnings, age 16-19

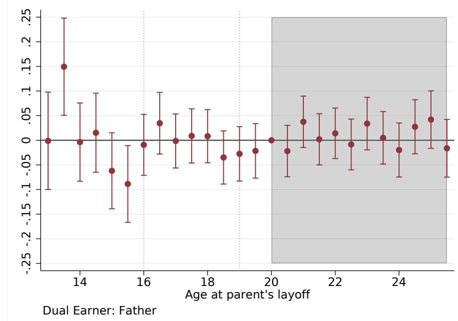
(b) Cumulative earnings, age 20-23

Notes: Cumulative earnings are the sum of real earnings (2010 SEK) during high school (panel a) and 4 years after graduation (panel b). The dotted line at age 19 represents the time when students are expected to graduate from high school. The dotted line at age 16 represents the time when students transition from compulsory school to high school. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 1 for child ages at parent layoff of $a_E \in [13, 25]$, normalized to 0 at $a_E = 20, s = 0$. The regressions include cohort and event year fixed effects and are run on the matched sample.

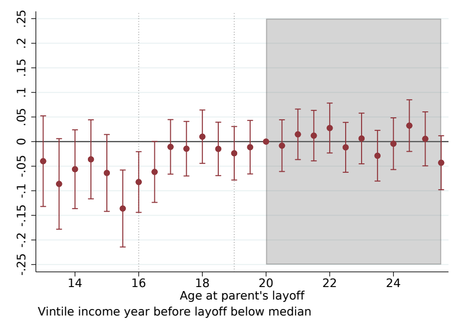
Figure 7: Heterogeneity by Parent characteristics



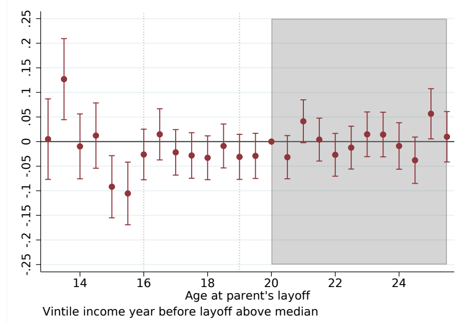
(a) Father is single earner in household



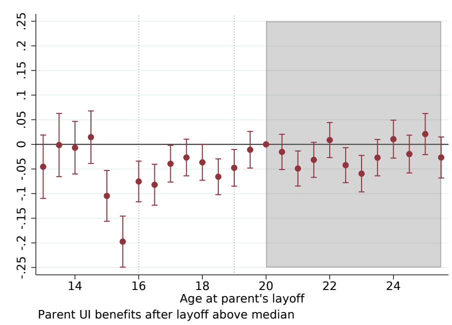
(b) Dual earner household



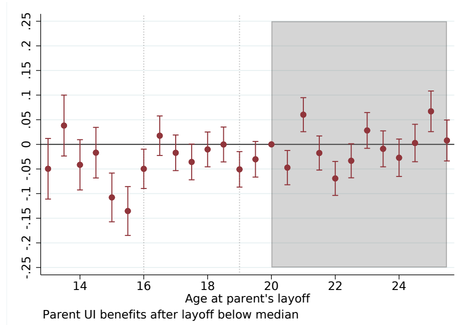
(c) Below median pre-layoff earnings



(d) Above median pre-layoff earnings



(e) Above median days in unemployment



(f) Below median days in unemployment

Notes:

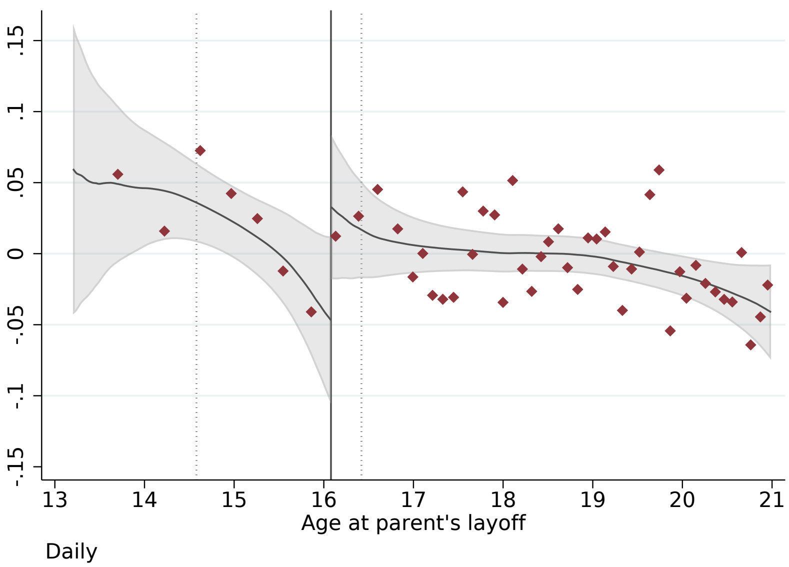
Panel (a) shows the likelihood of on-time high school graduation for parental layoffs when the parent was the single earner in the household. Panel (b) shows the effect of a parental layoff when a second working parent is present. The control groups are also restricted to be in single/dual earner households, in addition to standard matching. Note that we restrict the households to fathers in this case. Our baseline results are robust to this specification, see Table 3.

Panel (c) and (d) shows the estimated coefficients in restricted samples where the pre-layoff parental income is below or above the median income in the layoff sample. The control group is also split at the same income rate. Here, both fathers and mothers are included.

Panel (e) and (f) show the estimated coefficients for families with below or above median days in UI following a layoff. The entire control group is included for both estimates, as the split is relevant only for the families where a layoff actually occurs.

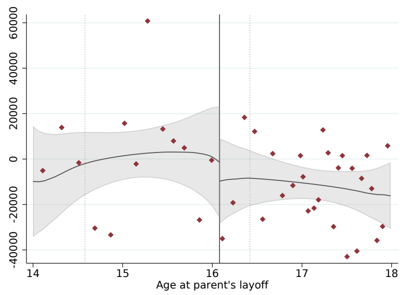
Tests of equality of coefficients are presented in Table 5.

Figure 8: High school graduation rate by separation date

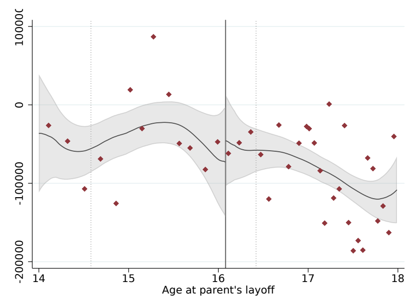


Notes: This figure shows the differential impact of parent layoffs on on-time high school graduation around the time of high school applications. The outcome is defined as the high school completion rate by age 19 relative to the matched control group. The estimated effect at the cutoff is robust to a wide selection of bandwidths, see Appendix Figure A10. See Figure 9 for placebo tests. The solid line represents the high school graduation deadline on February 1st, the spring semester when children are 15. The dotted line furthest to the left represents the last final exam date, in the second week of May. For completeness, we also show the line at the start of grade 8, in the fall semester at age 14, when children first start to receive grades.

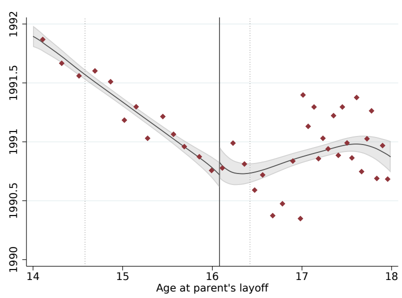
Figure 9: Robustness check of time series interruption around application deadline



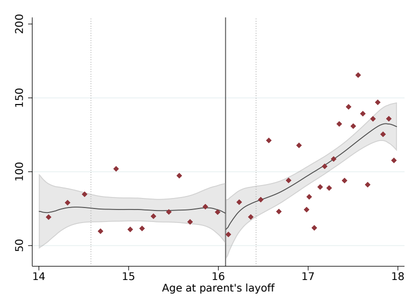
(a) Parent pre-layoff earnings



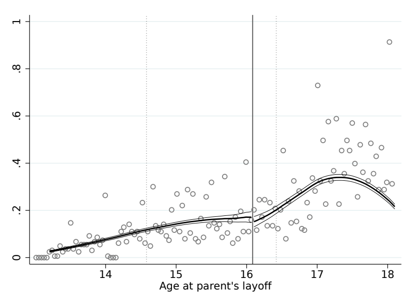
(b) Parent post-layoff earnings



(c) Child cohort



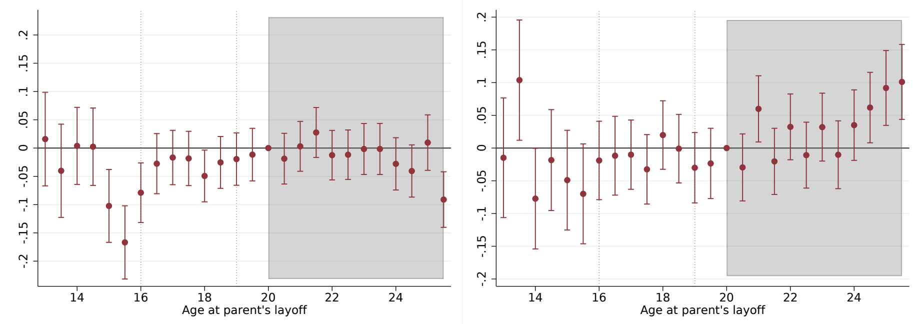
(d) Parent days of unemployment



(e) Density

Figure notes: The non-parametric fit is estimated by a local linear estimator (polynomial of degree 1) with an MSE-optimal bandwidth following Calonico et al. (2014). The cutoff is set to be February 1st, age 16, which represents the high school application deadline studied in figure 8.

Figure 10: Information: Heterogeneity by sibling order



(a) Oldest sibling in family

(b) Younger siblings

Notes: Panel (a) shows the likelihood of on-time high school graduation for the sample of children who are the oldest sibling or only child in their family. Panel (b) shows the effect on graduation for younger siblings only. The control group is also restricted to only include children of the right sibling order. Sibling order is used as a proxy for information about the high school choice, as younger siblings have the benefit of a family-level recent experience with applying for high school. Tests of equality of coefficients are presented in Table 7.

8 Tables

Table 1: High school choice

Panel A: High school applications		
	Mean	(sd)
Nr Choices	3.343	(1.439)
Attend 1st choice	0.652	(0.476)
Admitted to first choice	0.897	(0.304)
Panel B: Track choice		
	Vocational	Theoretical
Share of HS students	0.54	0.42
Share in track who proceed to university	0.72	0.29

Note: Summary statistics on high school applications are based on data from Statistics Sweden.
Panel A uses data on high school applications, and defines an admission as the students having passing grades at least as high as the minimum grade among students admitted to the program. In panel B: Schooling individualized programs are not classified as either theoretical or vocational.
Panel B shows the share of High school graduates who complete a Vocational or theoretical degree. Individualized programs are most commonly set up for students who did not pass the minimum requirement to finish compulsory school on time, but can also be applied to other cases, for example sports programs. The last row shows the share of students in each type of program who go on to get some university credits by age 30.

Table 2: Summary Statistics: At time of matching (when child is 12 years old)

	(1) Layoff	(2) Control group	(3) Layoff matched	(4) Control matched	(5) Pop. >30yo
Female	0.265	0.265	0.207	0.208	0.464
	(0.442)	(0.441)	(0.405)	(0.406)	(0.499)
Age	40.69	41.49	40.62	40.66	39.29
	(5.225)	(5.267)	(4.645)	(4.645)	(7.325)
Labor income	274849.3	301971.3	297828.8	299090.6	239647.7
(2010 SEK)	(161831.4)	(210699.1)	(170528.3)	(209228.8)	(201762.2)
Income vintile	13.77	14.62	14.51	14.41	12.76
	(4.262)	(4.356)	(3.965)	(4.074)	(5.130)
Highest ed:	0.119	0.166	0.123	0.128	0.296
University	(0.324)	(0.372)	(0.329)	(0.334)	(0.457)
Highest ed:	0.538	0.502	0.542	0.542	0.408
High school	(0.499)	(0.500)	(0.498)	(0.498)	(0.492)
Married	0.557	0.546	0.569	0.584	0.347
	(0.497)	(0.498)	(0.495)	(0.493)	(0.476)
Unemployed	0.0779	0.0379	0.0607	0.0408	0.0845
	(0.268)	(0.191)	(0.239)	(0.198)	(0.278)
Tenure at job	57.81	71.90	60.63	72.68	49.95
(months)	(41.32)	(42.82)	(41.64)	(44.20)	(35.66)
Manufacturing	0.455	0.468	0.460	0.461	0.236
	(0.498)	(0.499)	(0.498)	(0.498)	(0.425)
Year	2001.5	2001.0	2001.6	2001.7	2001.5
	(2.352)	(2.694)	(2.235)	(2.226)	(2.332)
Any children?	1	1	1	1	0.893
					(0.309)
N	17172	680786	11323	48277	6469957

mean coefficients; sd in parentheses

Notes: Matching is based on child year of birth and gender (not shown) as well as parent age, gender, individual and household income and industry of last employer. Time-varying characteristics are all matched based on characteristics measured when the child is 12 years old. Note that layoff probability decreases by tenure on the job.

Table 3: Results for population subsets - gender

	(1) Baseline	(2) Men	(3) Women	(4) Fathers	(5) Mothers
Layoff	0.00668	-0.00387	0.0205***	0.00247	0.0226*
	(0.00542)	(0.00724)	(0.00784)	(0.00615)	(0.0116)
Comp. school	-0.0602***	-0.0400*	-0.0857***	-0.0605***	-0.0585**
(Age 14.5-16)	(0.0152)	(0.0207)	(0.0215)	(0.018)	(0.0286)
High school	-0.0162*	-0.018	-0.0148	-0.0200*	-0.00649
(Age 16.5-19)	(0.00905)	(0.0122)	(0.0129)	(0.0104)	(0.0186)
Constant	0.637***	0.599***	0.686***	0.645***	0.609***
	(0.00427)	(0.00574)	(0.00611)	(0.00483)	(0.00903)
Ob	223,119	126,664	96,455	135,247	87,872
R-squared	0.011	0.016	0.018	0.012	0.03
FE:	cohort year	cohort year	cohort year	cohort year	cohort year

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Children younger than 14.5 dropped

Notes: Column 1 runs the baseline regression (corresponding to Equation 1 and Figure 4 but with age brackets based on high school. Ages 14 and younger are dropped from the estimation. Columns 2 to 5 run the same specification for different subsets of the population based on the gender of the child (columns 2 and 3) and parent (columns 4 and 5).

Table 4: Test of the joint significance of coefficients in Figure 4

VARIABLES	(1) HSever19
Treatment age 15.0	-0.112***
	(0.0333)
Treatment age 15.5	-0.152***
	(0.0349)
Treatment age 16.0	-0.0627**
	(0.0263)
Treatment age 16.5	-0.0313
	(0.0291)
Treatment age 17.0	-0.0270
	(0.0247)
Treatment age 17.5	-0.0319
	(0.0249)
Treatment age 18.0	-0.0207
	(0.0240)
Treatment age 18.5	-0.0415*
	(0.0244)
Treatment age 19.0	-0.0513**
	(0.0246)
1.layoff event	0.0221
	(0.0169)
Constant	0.638***
	(0.00103)
Observations	445,642
R^2	0.015
F HS = 0	0.880
Prob>F	0.507
F 19 = 0	4.360
Prob>F, 19	0.0368
F: 19-18.5 =0	2.500
Prob>F, 19-18.5	0.0819
F: 19-18 =0	1.740
Prob>F, 19-18	0.156
F: 19-17.5 =0	1.320
Prob>F, 19-17.5	0.261
F: 19-17 =0	1.740
Prob>F, 19-17	0.383
F: 19-16.5 =0	0.880
Prob>F, 19-16.5	0.507

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Parent Heterogeneity - F-tests

	(1) Single vs dual earner HHs	(2) Rel. income pre-layoff	(3) UI days post-layoff	(4) Real income post-layoff	(5) Rel. income post-layoff
F, 15.0	0.65	0.18	2.97	0.08	0.05
	(0.42)	(0.674)	(0.085)	(0.776)	(0.823)
F, 15.5	0.61	0.16	5.98	1.05	0.00
	(0.433)	(0.686)	(0.014)	(0.305)	(0.945)
F, 16.0	1.23	1.52	3.18	1.15	0.12
	(0.268)	(0.217)	(0.074)	(0.283)	(0.732)

P-values in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: Each estimation represents the F test of equality at each semester-age for children with different family characteristics. Column (1) tests the equality of coefficients between single and dual earner households. Column (2) tests the equality of coefficients between households with above and below median pre-layoff relative earnings. Column (3) tests the equality of coefficients between households where the parent spent above or below median number of days in UI, following layoff. Column (4) tests the equality of coefficients between households with above or below median real income in the two years following the layoff. Column (5) performs the same test, but using the same relative income specification as in column (2). In columns 1 and 2, both the treatment and the control group are split by the characteristics. In columns 3-5, we split only the families affected by layoff, as these are post-layoff outcomes. The median income is therefore relative to other individuals who were laid off.

Table 6: Interrupted time series estimates of effect of layoff at application deadline

VARIABLES	(1) Graduation	(2) Graduation	(3) Graduation	(4) Graduation | Parental unemp
RD Estimate	0.10504**	0.09568*	0.09566*	0.235***
	(0.0431)	(0.05172)	(0.05171)	(0.0829)
Effective Obs Pre	703	473	473	289
Effective Obs Post	1218	663	663	375
Bandwith (MSR)	1.20	0.747	0.747	0.815
Cohort-year FE	No	Yes	Yes	No
Control weights	No	No	Yes	No

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: We follow Calonico et al. (2019) to estimate the optimal bandwidth and robust confidence interval for the point estimate at the time series interruption as if it was an RD with controls. We use cohort-year dummies as controls. In column 3, we include the control group to be able to allow for re-weighting, but this only has a marginal impact on the estimate. Column 4 is specified as in column 1, but only for parents who experience unemployment after layoffs.

Table 7: Heterogenity: Sibling order comparison

VARIABLES	(1) HS graduate, age 19	(2) HS graduate, age 21
Older sibling	0.0478***	0.0402***
	(0.00615)	(0.00554)
Layoff	-0.0243	-0.0199
	(0.0259)	(0.0233)
Oldest: Treated age 15.0	-0.0924**	-0.0852**
	(0.0439)	(0.0396)
Younger: Treated age 15.0	-0.0282	0.0479
	(0.0503)	(0.0453)
Oldest: Treated age 15.5	-0.160***	-0.153***
	(0.0469)	(0.0422)
Younger: Treated age 15.5	-0.0522	-0.0139
	(0.0529)	(0.0475)
Oldest: Treated age 16.0	-0.0609*	-0.0268
	(0.0362)	(0.0325)
Younger: Treated age 16.0	0.000923	-0.0172
	(0.0391)	(0.0352)
Constant	0.606***	0.723***
	(0.00138)	(0.00124)
Observations	569,075	574,331
R-squared	0.015	0.017
F Test: (Old = Young) age 15.0	0.930	4.900
Prob>F, age 15.0	0.336	0.0269
F Test: (Old = Young) age 15.5	2.320	4.760
Prob>F, 15.5	0.128	0.0291
F Test: (Old = Young) age 16.0	1.350	0.0400
Prob>F, 16.0	0.246	0.842

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: This table tests the equality of the point estimates shown in figure 10a and 10b. For each semester-age 15, 15.5 and 16, we test if the effect of parent layoff on the oldest sibling is equal to the effect on a younger sibling in the same situation. The hypothesis is that when the oldest sibling is about to apply for high school (age 15.5) the family has less information about the process and is more vulnerable to a shock.

The regression specification adds a control for sibling order (binary: Oldest or younger) as well as interactions between this control and age of layoff and age of layoff for treated families. Note that we do not include family fixed effects. If there are two siblings in the same family, we will observe the family as treated twice, once at the age of the oldest children in the layoff year, once at the age of the younger child in the same year.

A Appendix: Background

A.1 Layoffs

The register data on layoffs in this paper comes from firms reporting notifications to the public unemployment authority (Arbetsförmedlingen). According to Swedish law (1974:13), firms that plan to lay off five or more full-time employees are required to report this to the local UI office. The minimum notification period varies by the size of the layoff. The reason for layoff needs to be specified in the report, ensuring that layoffs are initiated because of a reduction in labor demand at the firm. Initially, the firm has to report the number of individuals getting laid off. After negotiations with the unions are finished, they also need to report which employees are affected.

Who gets laid off is determined by last-in first-out laws, but can in practice be rounded by negotiations over severance pay. An employee is categorized as laid off if she is registered as subject to layoff and we observe her termination date, but we do not require her to actually register as unemployed. Hence, there may be selection into layoff, as mediated by the laws and union negotiations, but we do not condition treatment status on being unable to find a new job after the initial shock.

To receive UI (Unemployment insurance) benefits, the laid off worker needs to have been a member of a UI insurance fund (A-kassa) for at least a year prior to their termination date, and have run out of severance pay. (Any severance payment sum is calculated to wage equivalents based on the last monthly wage prior to layoff.) Hence, we do not expect all layoffs to appear at the UI office. In the data, around 50 percent of everyone experiencing layoff is registered at the UI office the year after layoff, see Figure 3b. If eligible, UI benefits are 80 percent of earnings up to a ceiling. The ceiling is determined by law, holding it fixed in nominal terms 2002-2014. In 2005, the ceiling was at roughly median earnings.

The average drop in real earnings following the layoff is not very large, but has a permanent effect on earnings. Figure A4 shows the evolution of mean earnings over time relative to layoff for the treatment and control group, excluding (Figure A4a) and including (Figure A4b) UI benefits. Unfortunately, we cannot separately observe severance pay from labor earnings, and there appears to be severance payments in the year after layoff as well as in the layoff year. Earnings drop by 15-20 percent 2 years after layoffs relative to pre-layoff earnings. 7 years after the shock (in a balanced panel), earnings are still 10 percent lower than in the control group.

Characteristics of the laid off population varies by year of layoff. This is to be expected as recession layoffs are going to be less adversely selected than layoffs in years when the business cycle is more favorable. Figure A2 shows the pre-layoff earnings, household income and share of layoffs in manufacturing over the time period.

A.2 Earnings by education level

Individuals with less than a high school degree in our data earn significantly less than graduates. Figures A6 (a) to (h) show the income paths for selected cohorts by highest degree in 2015 (the last year of observation). The sample is restricted to native-born individuals with a known level of education. The earnings path for high school dropouts is parallel to the path for high school graduates for all cohorts, both in mean and median earnings, until at least age

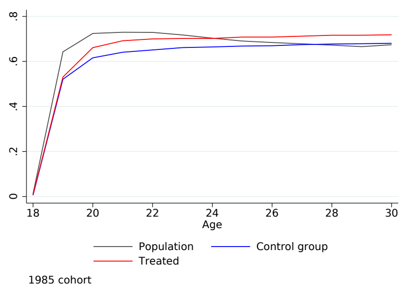
30. Dropouts earn on average less than 70 percent of a high school graduate's earnings up to the age of 30.

Figure A7 shows the earnings by education for the same cohorts, focused on earnings age 16-20. Dropouts earn less than in-school peers, even during the school years. Official unemployment surveys show that youth unemployment[22] in Sweden has been 15-25 percent since 2001. Among 15-19 year olds in the labor force, the unemployment rate is 22-37 percent for the same time period. This relation is driven by selection or negative opportunity costs of going to high school. A vocational school may facilitate connections to employers in their vocation, offering higher-paid job opportunities than to those who've left school. We do not observe the time of dropout, therefore it is possible that some share in the dropout group are students who attempt to graduate but fail to meet minimum requirements. Note that informal jobs will not appear in this administrative dataset.

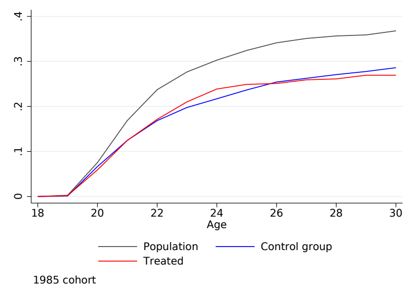
[22] Individuals aged 15 to 24

Appendix Figures and Tables

Figure A1: Highest education by age



(a) Share of population with at least a high school degree by age



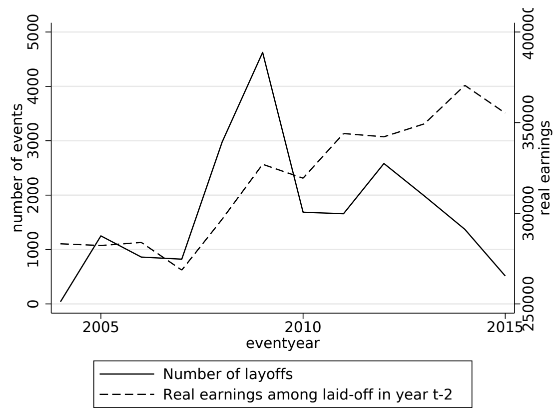
(b) Share of population with at least one year of University by age

Table A1: High School Choice Environment

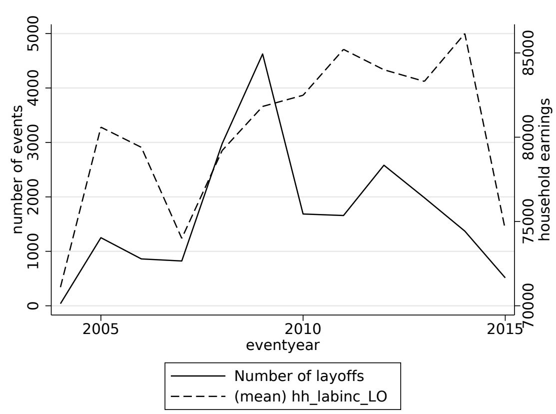
Year	School Market level			Municipality level	
	N Schools	**N Programs**	**Programs ≥15**	**N Schools**	**No school**
2000 mean	8.96	14.51	0.53	1.04	0.33
sd	(14.84)	(2.26)	(0.50)	(1.08)	(0.47)
2005 mean	10.41	15.28	0.64	0.89	0.36
sd	(14.73)	(2.83)	(0.48)	(0.86)	(0.48)
2010 mean	12.46	15.65	0.71	0.84	0.42
sd	(16.32)	(2.59)	(0.46)	(0.90)	(0.49)
2013 mean	15.61	15.75	0.69	1.55	0.38
sd	(19.02)	(2.76)	(0.46)	(1.80)	(0.48)

The average number of schools and program a student making a high school choice will face in her school market* or municipality.
* School markets are defined based on 2010 commuting patterns defined by Skolverket (2011).
Source: Skolverket

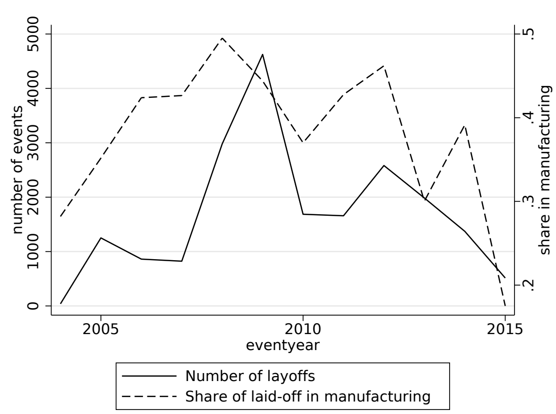
Figure A2: Characteristics of laid off employees by year



(a) Pre-layoff earnings by layoff year



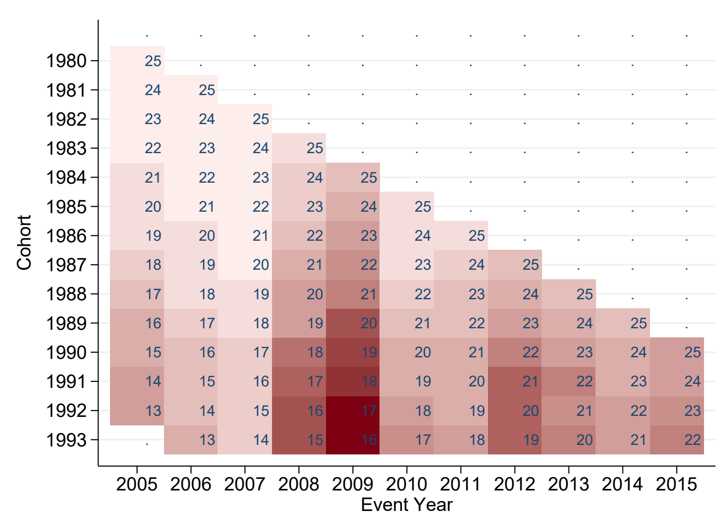
(b) Pre-layoff household earnings by layoff year



(c) Pre-layoff employment in manufacturing sector by layoff year

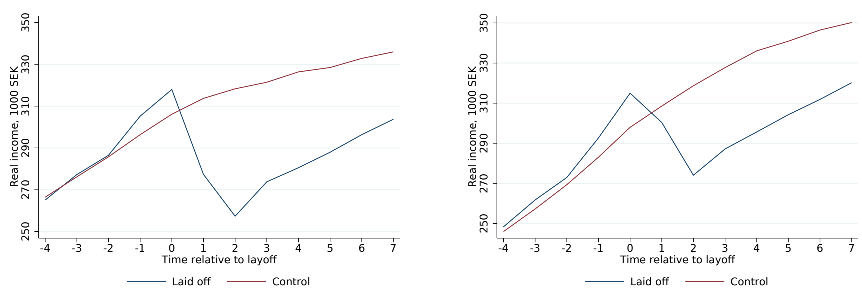
Notes: The solid line represents the total number of layoffs per year in our dataset. This is constant across all panels. The dashed line represents the characteristics of the set of laid off workers in the given year. Earnings are defined as all labor earnings in event year t-2 relative to layoff. Manufacturing is defined as a dummy equal to one if the parent's the main employer in event year t-2 is defined as a manufacturing firm according to the 2-digit industry (SNI) code.

Figure A3: Age of treated children in sample by event year and cohort



Note: Mapping of the distribution of treated children by year of parent layoff, child cohort and age. The number of observations in the lightest area is 35 and the darkest red area is 618. Each shade represents an interval of 40 observations.

Figure A4: Effects of layoffs on parent's outcomes



(a) Labor income

(b) Disposable income

Notes: In panels (a) and (b), the layoff group is defined as individuals who appear in the layoff data with a termination date in event year 0. The control group is weighted according to the procedure described in Section 3. Disposable income includes unemployment benefits and labor income. Both labor and disposable income are measured annually in 1000s of 2010 SEK. See Appendix Section A.1 for further details.

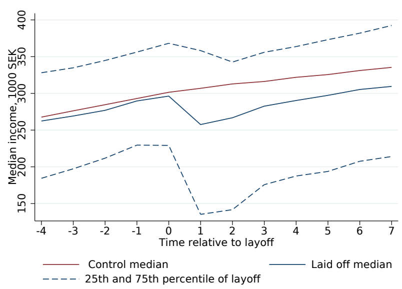
Table A2: Earnings path for dropouts and high school graduates in sample

	Dropout at 21 (1)	HS graduate at 21 (2)	Dropout at 19 (3)	HS graduate at 19 (4)
Age 16	2527.7	3644.1	2561.7	3879.6
	(6664.9)	(6562.1)	(6494.0)	(6657.0)
Age 17	5626.9	7741.7	5578.8	8281.1
	(14417.4)	(11566.8)	(13613.7)	(11552.0)
Age 18	12578.8	16022.7	11996.2	17319.2
	(27422.0)	(20662.1)	(25119.1)	(21031.8)
Age 19	29117.4	50860.2	26559.7	58116.4
	(50167.1)	(49222.0)	(45475.6)	(50105.7)
Age 20	52173.2	109137.4	51019.1	123550.5
	(76473.8)	(91116.9)	(71388.0)	(91765.8)
Age 21	70909.5	140402.5	78690.0	150370.0
	(92493.2)	(104072.7)	(93259.3)	(104196.2)
Age 22	87670.8	162874.6	99762.1	170618.2
	(103576.9)	(112757.7)	(106138.8)	(112820.2)
Age 23	103704.6	178837.4	116881.7	185664.2
	(113974.9)	(119975.4)	(117006.6)	(119425.7)
N	75461	83393	102975	55879

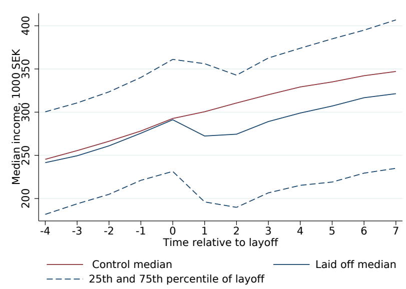
mean coefficients; sd in parentheses

Note: Real income by age in baseline sample with weights. Individuals with any university enrollment at age 23 or younger are excluded from the sample. Column 1 and 2 define individuals as high school graduates or dropouts based on their highest level of education at age 21. Column 3 and 4 use the highest level of education at age 19.

Figure A5: Range of parent outcome around layoff



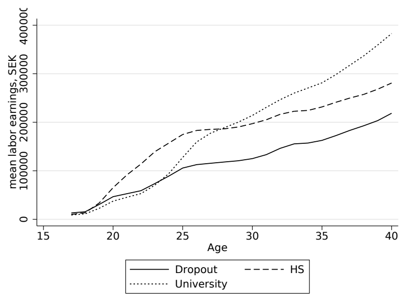
(a) Labor income, range



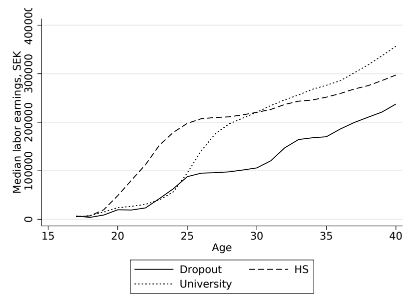
(b) Disposable income, range

The treatment group is defined as individuals who appear in the layoff data with a termination date normalized to year 0. The control group is weighted according to the procedure described in section 3. Disposable income includes unemployment benefits and labor income. Income is measured annually in 1000s of 2010 SEK. Percentiles are estimated from the annual distribution.

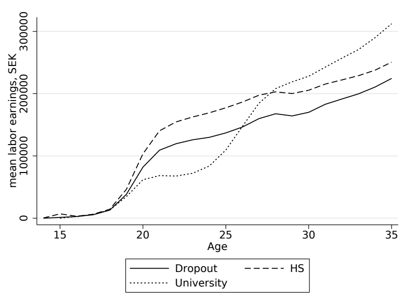
Figure A6: Income paths by education level - selected cohorts



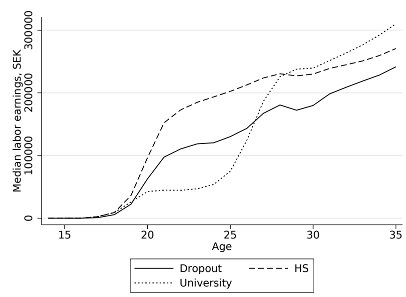
(a) Mean earnings, 1975 cohort



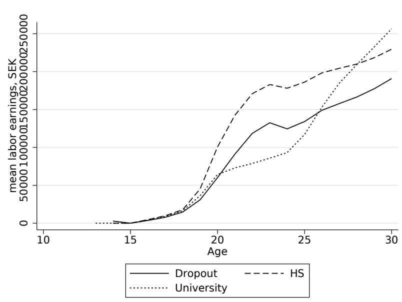
(b) Median earnings, 1975 cohort



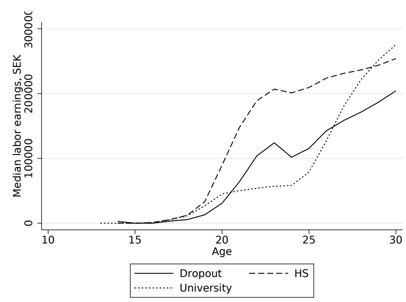
(c) Mean earnings, 1980 cohort



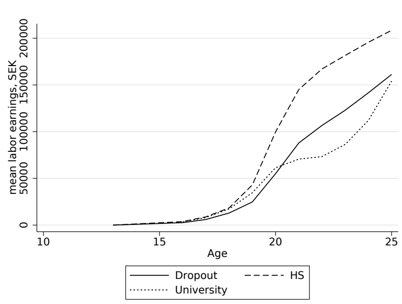
(d) Median earnings, 1980 cohort



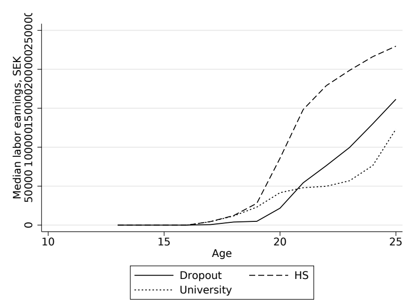
(e) Mean earnings, 1985 cohort



(f) Median earnings, 1985 cohort

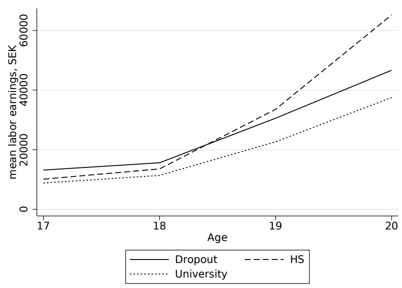


(g) Mean earnings, 1990 cohort

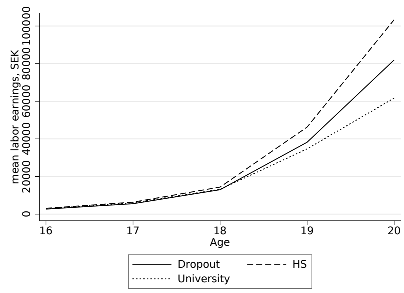


(h) Median earnings, 1990 cohort

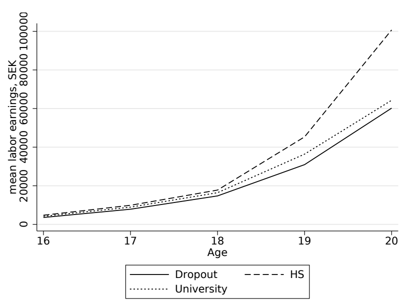
Figure A7: Income paths by education level age 16-20 - selected cohorts



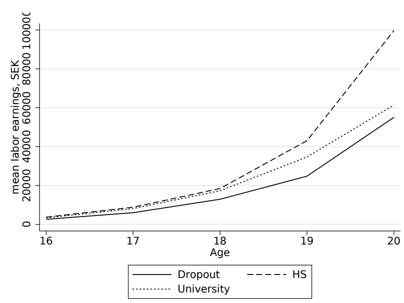
(a) Mean earnings, 1975 cohort



(b) Mean earnings, 1980 cohort

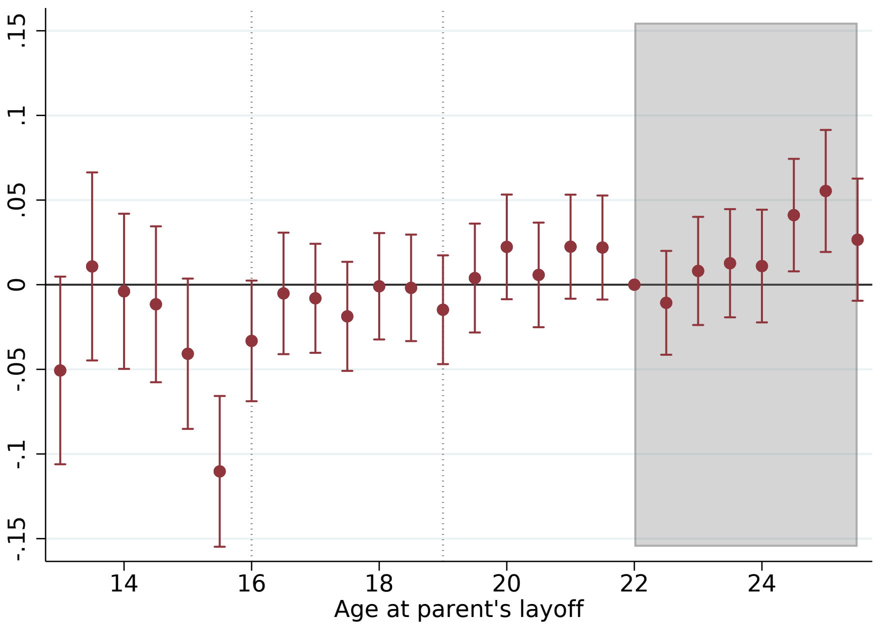


(c) Mean earnings, 1985 cohort



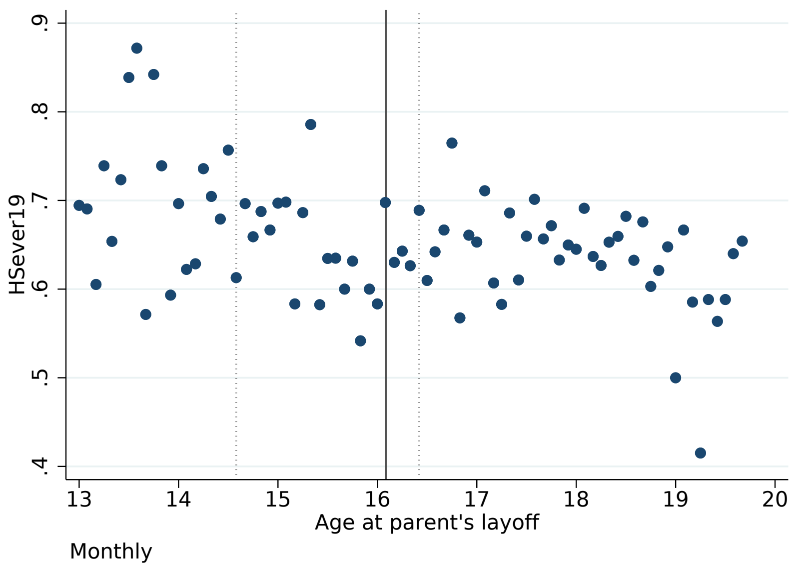
(d) Mean earnings, 1990 cohort

Figure A8: High school completion by age 21



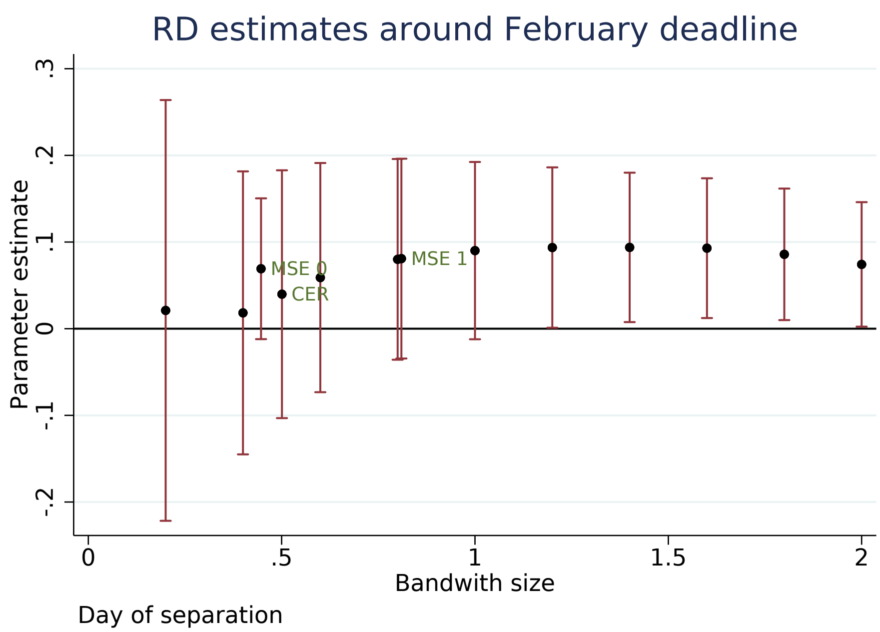
Notes: This figure shows the estimated impact of parental layoffs on children's on-time high school graduation rate separately by child age at the time of parent separation. The dotted line at age 19 represents the time when students are expected to graduate from high school if they are graduating on time. The dotted line at age 16 represents the time when students transition from compulsory school to high school. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 1 for child ages at parent layoff of $a_E \in [13, 25]$, normalized to 0 at $a_E = 22, s = 0$. The regressions include cohort and event year fixed effects and are run on the matched sample. The outcome, high school graduation by age 21, is defined as a dummy equal to 1 if the child's highest education 21 years after birth is a completed 2- or 3-year high school degree or higher, and 0 otherwise. 98 percent of all high school graduates have graduated by age 21.

Figure A9: Graduation rates for children with parental layoffs only



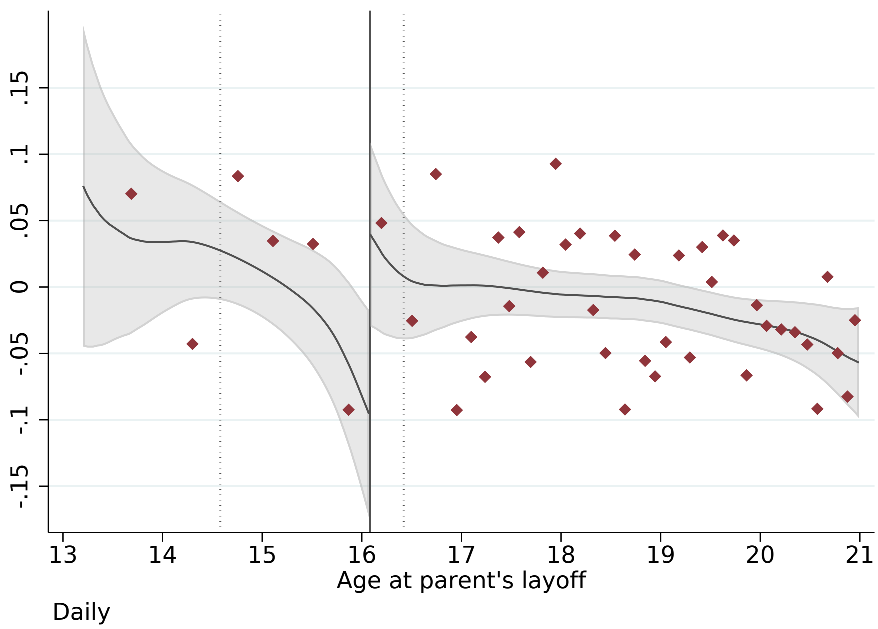
Raw data: Binscatter of the high school completion and month of separation. No controls, only data from families with a parental layoff. Each scatter is a calendar month-age bin.

Figure A10: Robustness of ITS - different bandwidths



Note: The MSE optimal bandwith follows Imbens and Kalyanaraman (2011) and the CER optimal bandiwth follows Calonico et al. (2014), in line with the literature on regression discontinuity designs.

Figure A11: High school graduation rate by first day of parent unemployment



Notes: This figure shows the differential impact of parent layoffs around the time of high school applications for parents who experience unemployment subsequent to their termination date. The outcome is defined as the high school completion rate relative to the matched control group. The bandwidth is chosen to be MSE optimal at the interruption time using the method of Calonico et al. (2014), following the standard approach for regression discontinuity designs. The solid line represents the high school graduation deadline on February 1st, the spring semester when children are 15. The dotted line furthest to the left represents the last final exam date, in the second week of May. For completeness, we also show the line at the start of grade 8, in the fall semester at age 14, when children first start to receive grades.