

Family-level stress and children's educational choice: Evidence from parent layoffs^{*}

Julia Tanndal
Brown University
julia_tanndal@brown.edu

Miika Päälysaho
Stockholm University
miika.paallysaho@ne.su.se

This Version: October 15, 2020
Latest Version: [here](#)

Abstract

We analyze the effect of parental layoff on the educational outcomes of their children. We use Swedish administrative data on layoffs caused by shocks to firms' labor demand and variation in child age at the time a parent is laid off to estimate the age-specific impact on high school graduation rates. We find that parental layoffs may have a large impact on high school completion rates, but the effect is restricted to children who are about 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 making optimal education decisions may be costly, even if there are no financial constraints on access to education.

*We thank David Seim, John Friedman, Brian Knight, Matthew Lindquist, José Montalbán Castilla, Emily Oster, Neil Thakral and Jonas Vlachos as well as seminar participants at Brown University and Stockholm University for helpful comments. Funding has been received from the James M. and Cathleen D. Stone Wealth and Income Inequality Project.

1 Introduction

Educational attainment is highly correlated with parental income in a range of different settings.¹ This correlation reflects either differences in optimal return to schooling for families with different earnings potential, or the economic constraints which prohibit poorer households from optimally investing in education. Traditionally, economic constraints are modeled as credit constraints that arise because credit markets are incomplete with respect to future earnings (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. Sapolsky (2018) documents the increasing body of research in neurology on how the increased level of stress associated with lower socio-economic status can lead to reduced function in the pre-frontal 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 (see See and Gorard, 2015, for a review), including causal effects on parent

¹For 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 housing market (Black, 1999). In choice systems - describe- income segregation is still present (Read skolverket's references XXX). Belley and Lochner (2007) describe how the correlation between family income and college enrollment has increased over time.

involvement during adolescence on educational outcomes (Spoth et al., 2008).

In this paper, we attempt to identify the effect of a parental income shock on children's educational outcomes through effects of economic stress on parenting. We consider a setting where education is free, but where the role of parental involvement in the child's education is large and the ability to make a complex choice with delayed rewards is crucial. This setting is the choice of upper secondary school track in Sweden. As in most OECD countries, Swedish families choose whether their children should pursue a vocational or theoretical education at an early age. 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. Our data includes the exact timing of the layoff, which allows us to use variation in the age and school progress of the child at the time the shock hits to estimate the impact of economic insecurity at different points in the child's life. The register data on layoffs include all firms laying off five or more workers due to reductions in demand such as plant closures, reduced production, firm restructuring, etc. This guarantees that all layoffs in our data set are initiated by a firm-level shock, which makes the timing of the layoff plausibly exogenous. Who gets laid off may be driven by unobservables, but exactly when this happens, and how old the child is at the time, is independent of potential outcomes.

For layoffs that coincide with the time when children transition from compulsory to upper secondary school (ages 15–16), we find a significant decrease in the likelihood of ever completing high school. The estimated likelihood of com-

pleting high school on time decreases from 73 to 58 percent for children whose parents are laid off 6–12 months before the school transition. Dropping out of high school is associated with a large decrease in long run earnings in the population, individuals without a high school degree earn on average 25-40 percent less than high school graduates over the first 10 years in the labor market.

A parental layoff at the time of transition to high school has a larger effect on families where the cost of school choice is higher. The primary costs to parental involvement in the school choice are information costs and travel costs. We find that 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. Similarly, we find that the effect is larger for families who live far from the closest high school, increasing cost of attendance and information gathering.

Our findings contribute to the literature on the scarring effects of layoffs as well as to understanding the role of parental income in 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 health and family outcomes.² In contrast, the evidence on the intergenerational effects of parental

²For 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. 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

layoff is mixed (see Mörk et al., 2019, Appendix A, for an overview). Earlier studies on education outcomes show a decrease in graduation year GPA after parental job loss (Rege et al., 2011). Our paper complements this finding as we find medium-run consequences of parental layoffs for the exact same age group. We do, however, find support for a slightly different mechanism. Huttunen et al. (2019) find a shift away from the parent's industry in early employment. Oreopoulos et al. (2008) show that Canadian children whose fathers are displaced by firm closures are less likely to attend college and have 9% lower annual earnings in their late-20s/early-30s, while Hilger (2016) only find small early-career earnings losses and no impact on college enrollment for children in the US. These studies tend to focus on parental layoffs very close to the outcome of interest. We highlight the importance of the exact timing of the layoff event in a child's life, noting that children may react differently to a shock depending on where in their education they are when it happens.

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

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.

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.³. 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 most⁴ 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).

³At 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).

⁴at 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.⁵

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 this paper. We combine individual data on employment, earnings and educational attainment with data on family connections and a detailed registry of all layoffs.

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 unemployment

⁵School 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.

agency office between 6 weeks to 6 months before the termination of employment. The exact timing of the notification depends on the size of the layoff, and we observe both the date of notification for the firm as well as the termination date for the individuals who are laid off.

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, the restriction to firm-induced layoffs allows us to argue that the exact timing of the layoff is exogenous to employee characteristics. Figure 2 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.

These layoffs are due to firm reductions in production, which is arguably less related to the unobserved characteristics of the worker than observed separations where the reason for unemployment is unknown. Furthermore, the exact layoff date gives additional variation in treatment. Table 2 shows characteristics of the sample of laid off employees, notified employees and employees who work for a firm with at least one notification event.

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 13–23.⁶ The panel data follows

⁶See Appendix Figure A2 for the distribution of observations over the year \times cohort \times age distribution.

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). We also observe the field/major of the education.

For parents, we observe a panel of income and employment from 1991–2015. Employer-employee match data allow us to focus on 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 parent layoff on children's educational outcomes, we rely variation in the child's age at the time of layoff, as well as matching families that experience parental layoff to families at risk of parental layoff. In this section, we first describe the restrictions 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 how it is used in our estimation procedure.

In addition to families that 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. 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 common approach in the literature on the effects of layoffs is to define an exogenous layoff based on firm closures (e.g Jacobson et al., 1993; Oreopoulos et al., 2012; Eliason and Storrie, 2006). The control group will then be individuals who work for surviving firms, leading to concern about selection into deferentially successful firms, conditional on industry (see Hilger, 2016; Seim, 2019, for example). By restricting our control group to individuals employed at firms with a non-zero layoff risk, we avoid this concern.

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 our controls to become treated at a later stage.⁷

In addition to this restriction of our control group, we also perform a course

⁷See Borusyak and Jaravel (2017) for a discussion of a pure control group as a solution to the problem highlighted for example by Abraham and Sun (2018) and Goodman-Bacon (2018).

exact matching on child and parent characteristics to ensure the treatment and control group are similar based on observables. We match on the gender and birth year or the child. For a given event year, we match parents who are laid off in this year to parents of the same gender and age who are at risk of layoff by the virtue of being connected to an at-risk firm the same year. For the families in the control, this year will be their potential, or placebo, layoff year.⁸ 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 include families with similar childhood experiences, regardless of when in the child's life the parental layoff occurs.

Table 2 present 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). Column 3 and 4 shows the result of the matching of 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 matching on parental education level, as well as the mechanical effect on matching up income and industry variables.

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

⁸We allow control families to enter with several different potential layoff years, as the exact matching procedure will reduce the weight of the observation accordingly.

the treatment and control group by this matching procedure. This is where we turn to next.

As layoffs are induced by a negative shock to firms⁹ we assume that the exact timing of the layoff is exogenous to the age of the child at the time of layoff. 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 4 does not reject this assumption. Figure 4a shows that conditional on event year and parent age, we find no significant correlation between layoff probability and child age at the time, in the sample of parents at risk of layoff. Given that the parent is laid off, we find no correlation between child age at the time and the size of the income shock, as shown in figure 4b.

If it is true that the child's age is independent of parent layoff, then it is as if random whether the child experience the parental layoff at age 18 or age 20. The difference in age at treatment (parental layoff in our case) should not affect potential outcomes. 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.

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. Any difference between the treatment and control group observed at age 20 is therefore entirely driven by differences in potential outcomes (selection). Also note that if age at treatment is random, (as argued above) the difference in potential outcomes between treatment and control does

⁹see the discussion of the definition of a layoff in or data from Section 2.2

not vary by age. Hence, we can use the estimated difference in potential outcomes at age 20 as an estimate of the average difference in potential outcomes for all age groups. Therefore, normalizing the estimates by the estimated difference between treatment and control at age 20 gives estimates of the treatment effect that are unbiased on average. This is exactly what we do to our empirical estimates.

Equation shows the regression specification we use estimate the effect of parental layoff 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 individual i , at age a_O . For each child, the variable of interest is the layoff status of her parent at the age of potential layoff, a_E, s . In order to capture the different schooling contexts for children at the time of parental layoff, we define age in (year,semester) intervals. a_E is the age in years since birth from 13–25, semester s is a binary equal to 0 if the layoff occurred in January-June (spring semester) and 1 if the layoff happened July-December (fall semester).

$$y_{i,a_O} = \sum_{a_E=13}^{a_E=25} \sum_{s=0}^{s=1} \delta_{a_E,s} \mathbf{1}_{[\text{Layoff}_{a_E,s,i}=1]} + \tau_c + \gamma_{t_E} + \beta X_i + \varepsilon_i \quad (1)$$

As we are interested in the heterogeneous effect of parental layoff over the child's schooling, we will estimate a set of coefficients $\delta_{a_E,s}$ to describe the average effect of parental layoff in each age-semester (a_E, s). For each treated observation, the dummy matching the child's age and the semester at the time of treatment, Layoff $_{a_E,s,i}$ will be equal to one.

A child in the control group will have no positive dummy Layoff $_{a_E,i}$. The control group is crucial to capture the unequal distribution of ages, time and

cohort (Appendix Figure A2 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 $\delta_{a_E, s}$ for $a_E > a_O$ is a valid estimate of the average difference in potential outcomes between treatment and control group. To follow the convention in event studies, we use the age closest to the first age younger than treatment as the normalization age, but 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. These estimates are in the shaded area in figures 5 to 8.

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, and that the effects are higher for families with higher information costs associated with the high school choice.

4.1 Parental layoff and earnings

The 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. The effects on parents do not vary by the age of the child.

Labor earnings for parents who gets 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 research 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 notification and termination dates 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 the coverage ceiling is fixed in nominal terms over the entire period of interest. See Appendix A.1 for an explanation of UI benefits and severance pay.

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 A7 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).

This increase in economic insecurity does not vary by the age of the child. As discussed in Section 3, this is a key assumption for the identification of the effects of parental layoff on children. Figure 4a shows the probability of parental layoff over child age, controlling for event year and parent age. A linear specification fails to reject the null hypothesis that the age of the child is uncorrelated with the likelihood of parental layoff. Furthermore, the size of the parental income drop after layoff do not vary by the age of the child at layoff, as shown in Figure 4b for the percentage difference between parent income in the year after layoff relative to the year before job loss.

4.2 Parental Layoff 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 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 layoff 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 5 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. The horizontal axis is reversed, showing the age at time of layoff by semester from age 25 to age 13.¹⁰ Children who are 20 or older (grey region) have already completed high school by the time their family is hit by the shock, serving as an additional control group. Children who are younger than 20 (white region) at the time of parental layoff experience the family shock when they are still enrolled in school; therefore their graduation could be affected. In figure 5, the dotted line at age 16 represent the end of compulsory school and start of high school. The dotted line at age 19.5 is the graduation date of high school, under normal school progression.

Parental layoff prior to child high school enrollment have a significant negative effect on children's eventual graduation rates. In Figure 5, 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

¹⁰We denote the time of parental layoff 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.

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 layoff (ages 16.5 to 19) are not significantly less likely to complete high school than their peers. Table 3 shows 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 Table 4. 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 30.¹¹ 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 layoff on child earnings in their early 20s precisely for children of age 15.5 at the time of layoff (see Appendix Figure A6b).

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. Figure 6 shows graduation rates by age 21, when 98 percent of all individuals who will ever complete high school have graduated. Even here,

¹¹See Appendix A.2 for a detailed description of earnings by education level.

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).¹²

There is no effect on the graduation rate from academically more demanding degrees. Figure 7a shows that graduation rates for scientific degree programs are unaffected by parental layoff for all ages. These degree programs are usually attended by highly motivated students.¹³ 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 layoff (see Figure 7b). This is in line with the literature on parental involvement which focuses on at-risk or low-achieving students (See and Gorard, 2015).

The size of the income shock following parental layoff 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, however, find some evidence of larger effects on households with more pre-layoff economic insecurity. Figure 8 shows the difference between single earner and dual earner households, where the father was laid off. Layoffs of fathers in families where the father was the single earner in the household

¹²Calculated from Table 4

¹³Note 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.

has a negative impact on graduation rates of high school enrollees as well as a pronounced dip in graduation rates for events in the most sensitive transition age 15.5. Note that while the point estimate is larger for the group of single earner households, the effect for the two groups are not statistically different (F-tests in Table 5).

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 Appendix Figure A6a 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 will (see Appendix Figure A4). 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

To summarize, we find that the increase in economic insecurity caused by parental layoff can have large negative effects on children’s likelihood to complete high school, if t To summarize, despite the relatively modest impact of layoffs on parental income, we find large effects on high school graduation rates for children who have not yet entered high school at the time of parental layoff. 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. We now turn to examining the drivers of the sizable effect for this particular age group.

¹⁴Note 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.

¹⁵See Appendix A.2 for more details on children’s earnings.

4.3 Transition to high school: Mechanisms

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 standard. Hence, a shock to academic performance prior to high school enrollment may lead to students not getting into their preferred school or require re-take classes or prior to enrollment. Second, consider the high school choice. As described in Section 2, 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 layoff 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.¹⁶ The February and May deadlines correspond to the school choice and academic performance shock channels, respectively.

¹⁶The 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 Feruary 16th. Similarly, we use May 1st as the relevant cutoff for national tests, as these tests are an important factor in the determination of final grades.

Figure 9 illustrates the effect of a parental layoff right before these two events. The binscatter in both panels shows the difference in high school graduation rates between families with and without parental layoff in equally sized bins for the exact termination date associated with the layoff. In each panel, we nonparametrically estimate the change in outcome by child age at the time of layoff, allowing for an interruption in the time series in February (panel a) and May (panel b), respectively. 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 9a. 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.

There is 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 significant effect of parental layoff before February suggests that increased economic insecurity within the family affects children's high school graduation rate by diverting the parent's attention at the time of school choice. The point estimate at the February cutoff is robust to a wide selection of bandwidths (see Appendix Figure A9) and the inclusion of controls (see table 6). We also find that the density and distribution of layoff characteristics and pre-layoff

covariates evolve smoothly around the cutoff, as discussed in Appendix B.1.

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. Figure 10 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).¹⁷

The results from Figure 9 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 affect children's educational outcomes: improved learning and aligned expectations. Involved parents can function as teachers to directly improve learning. They may also 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

¹⁷The interpretation of this result should be done with caution, as we are conditioning on an event that occurs after the treatment.

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 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 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).¹⁸ 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.

¹⁸In 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.

Figure 11 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 clear negative effect of parental layoff 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 11a and 11b. 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 layoff 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.

5 Conclusion

We have explored a particular channel through which economic insecurity can affect children's educational outcome. We show that children in Sweden are relatively insulated against economic shocks to their parents, except for when they are about to transition between they are applying to high schools. The high school choice is a complex decision which require a lot of parental involve-

ment. From the literature of what affects children's educational success (XXX references from inside (See and Gorard, 2015) XXX) that parental involvement and parental competency during adolescence (Spoth et al., 2008) has a causal effect on educational outcomes.

To estimate the effect of an economic shock to parents, we have used layoffs. 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. We also find that families with less information about the high school choice are more sensitive to a parental layoff at the time of high school applications than families who have more information, as they have experienced the process at least once before.

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 are 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 over all (IS IT? XXX SOURCES neuro-guy). Our results highlights 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. If we reduce the income-education channel to credit constraints, we risk to severely underestimate the causal effect of income on education in settings where credit constraints are low.

References

- Abraham, S. and L. Sun (2018, 04). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *SSRN Electronic Journal*.
- Belley, P. and L. Lochner (2007, 02). The changing role of family income and ability in determining educational achievement. *Journal of Human Capital* 1, 37–89.
- Black, S. E. (1999). Do better schools matter? parental valuation of elementary education. *The Quarterly Journal of Economics* 114(2), 577–599.
- Black, S. E., P. J. Devereux, and K. G. Salvanes (2015). Losing heart? the effect of job displacement on health. *ILR Review* 68(4), 833–861.
- Borusyak, K. and X. Jaravel (2017). Revisiting event study designs, with an application to the estimation of the marginal propensity to consume. *Working Paper*.
- Brand, J. E. (2015). The far-reaching impact of job loss and unemployment. *Annual review of sociology* 41, 359–375.
- Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik (2019). Regression discontinuity designs using covariates. *The Review of Economics and Statistics* 101(3), 442–451.
- Calonico, S., M. D. Cattaneo, and R. Titiunik (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica* 82(6), 2295–2326.
- Chetty, R., J. Friedman, E. Saez, N. Turner, and D. Yagan (2020). Income segregation and intergenerational mobility across colleges in the united states. *Quarterly Journal of Economics forthcoming*.

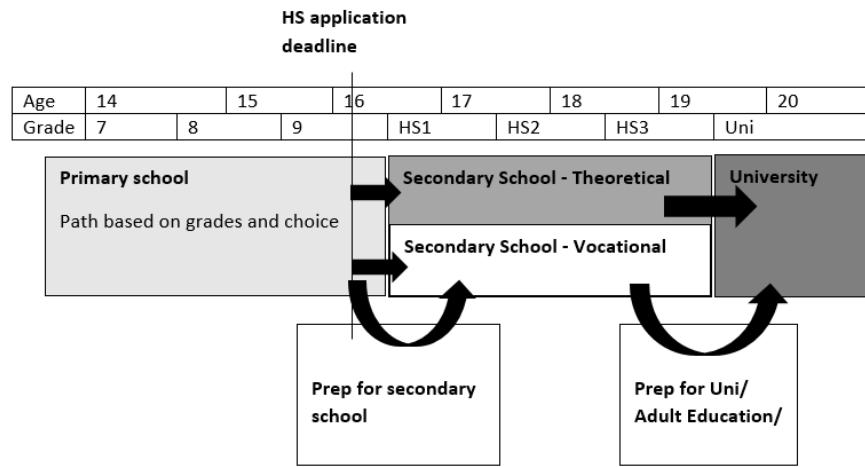
- Chetty, R., J. N. Friedman, and E. Saez (2013, December). Using differences in knowledge across neighborhoods to uncover the impacts of the eitc on earnings. *American Economic Review* 103(7), 2683–2721.
- Couch, K. A. and D. W. Placzek (2010). Earnings losses of displaced workers revisited. *American Economic Review* 100(1), 572–89.
- Eliason, M. and D. Storrie (2006). Lasting or latent scars? swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics* 24(4), 831–856.
- Eliason, M. and D. Storrie (2009). Does job loss shorten life? *Journal of Human Resources* 44(2), 277–302.
- Fjellman, A.-M., K. Y. Hansen, and D. Beach (2019). School choice and implications for equity: the new political geography of the swedish upper secondary school market. *Educational Review* 71(4), 518–539.
- Francesconi, M. and J. J. Heckman (2016). Child development and parental investment: Introduction. *The Economic Journal* 126(596), F1–F27.
- Goodman-Bacon, A. (2018). Difference-in-Differences with Variation in Treatment Timing. (25018).
- Heckman, J. J. and S. Mosso (2014). The economics of human development and social mobility. *Annual Review of Economics* 6(1), 689–733.
- Hilger, N. G. (2016, July). Parental job loss and children’s long-term outcomes: Evidence from 7 million fathers’ layoffs. *American Economic Journal: Applied Economics* 8(3), 247–83.
- Holmes, T. H. and R. H. Rahe (1967). The social readjustment rating scale. *Journal of Psychosomatic Research* 11(2), 213 – 218.

- Huttunen, K., K. Riukula, et al. (2019). Parental Job Loss and Children's Careers. Technical report, Institute of Labor Economics (IZA).
- Imbens, G. and K. Kalyanaraman (2011, 11). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies* 79(3), 933–959.
- Jacobson, L. S., R. J. LaLonde, and D. G. Sullivan (1993). Earnings Losses of Displaced Workers. *The American economic review*, 685–709.
- Lochner, L. and A. Monge-Naranjo (2012). Credit constraints in education. *Annual Review of Economics* 4(1), 225–256.
- Lovenheim, M. F. (2011). The effect of liquid housing wealth on college enrollment. *Journal of Labor Economics* 29(4), 741–771.
- Lovenheim, M. F. and C. L. Reynolds (2011). Changes in postsecondary choices by ability and income: Evidence from the national longitudinal surveys of youth. *Journal of Human Capital* 5(1), 70–109.
- Mörk, E., A. Sjögren, and H. Svaleryd (2019). Consequences of Parental Job Loss on the Family Environment and on Human Capital Formation: Evidence from Plant Closures.
- OECD (2019). *Education at a Glance 2019*.
- Oreopoulos, P., M. Page, and A. H. Stevens (2008). The intergenerational effects of worker displacement. *Journal of Labor Economics* 26(3), 455–483.
- Oreopoulos, P., T. von Wachter, and A. Heisz (2012). The Short- and Long-Term Career Effects of Graduating in a Recession. *American Economic Journal: Applied Economics* 4(1), 1–29.

- Rege, M., K. Telle, and M. Votruba (2011). Parental job loss and children's school performance. *The Review of Economic Studies* 78(4), 1462–1489.
- Sapolsky, R. M. (2018). How economic inequality inflicts real biological harm. *Scientific American* 319(5), 63–67.
- See, B. H. and S. Gorard (2015). The role of parents in young people's education—a critical review of the causal evidence. *Oxford Review of Education* 41(3), 346–366.
- Seim, D. (2019). On the Incidence and Effects of Job Displacement: Evidence from Sweden. *Labour Economics* 57, 131–145.
- Skolverket (2003). Valfrihet och dess effekter inom skolområdet freedom of choice and its effects on schooling. *The Swedish National Agency for Education* 03(805).
- Skolverket (2011). Skolmarknadens geografi: Om gymnasieelevers pendling på lokala och regionala skolmarknader. *Swedish National Agency for Education*.
- Spoth, R., G. K. Randall, and C. Shin (2008). Increasing school success through partnership-based family competency training: Experimental study of long-term outcomes. *School Psychology Quarterly* 23(1), 70–89.
- Stevens, A. H. (1997). Persistent effects of job displacement: The importance of multiple job losses. *Journal of Labor Economics* 15(1, Part 1), 165–188.
- Sullivan, D. and T. Von Wachter (2009). Job displacement and mortality: An analysis using administrative data. *The Quarterly Journal of Economics* 124(3), 1265–1306.
- von Wachter, T., J. Song, and J. Manchester (2009). Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using us administrative data from 1974 to 2004.

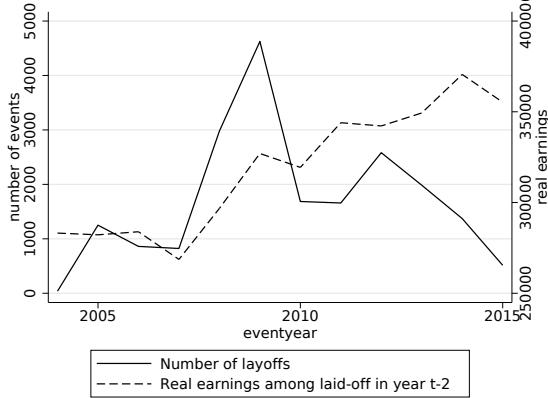
6 Figures

Figure 1: Schooling in Sweden

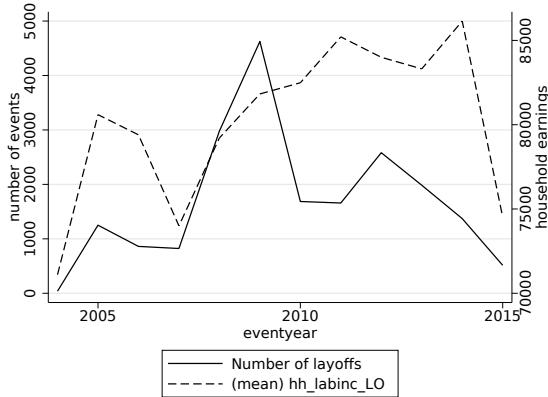


Notes: This figure shows the Swedish education system from grade 7 to university. The academic calendar runs from August to June, and class assignment is based on year of birth. This gives the overlapping of age (age 1 on January 1st, the year after birth year) and grade. In the second semester of grade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

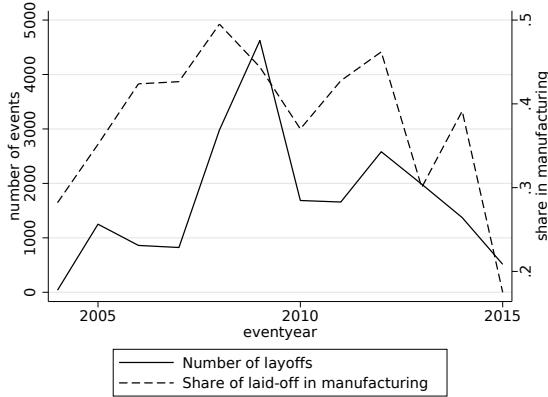
Figure 2: Characteristics of laid off employees by year



(a) Pre-layoff earnings by layoff year



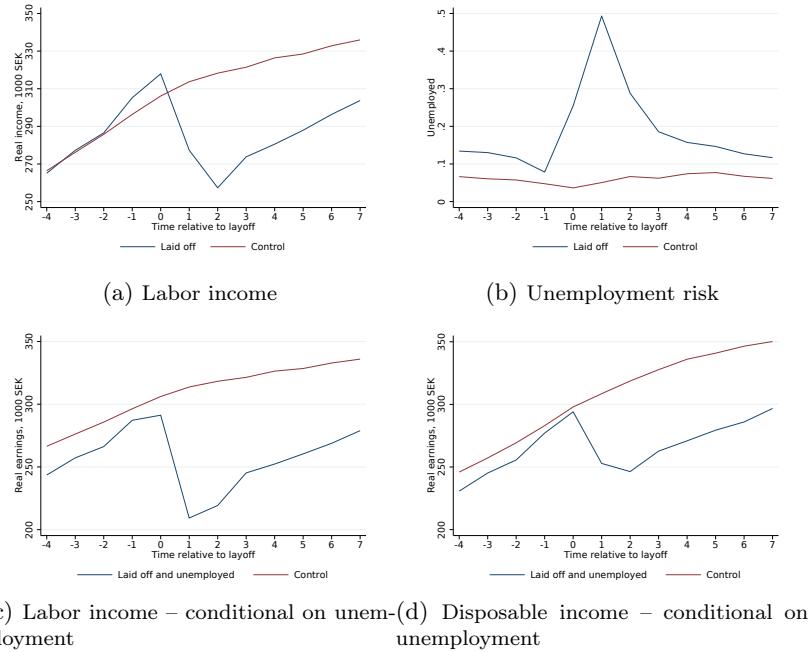
(b) Pre-layoff household earnings by layoff year



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

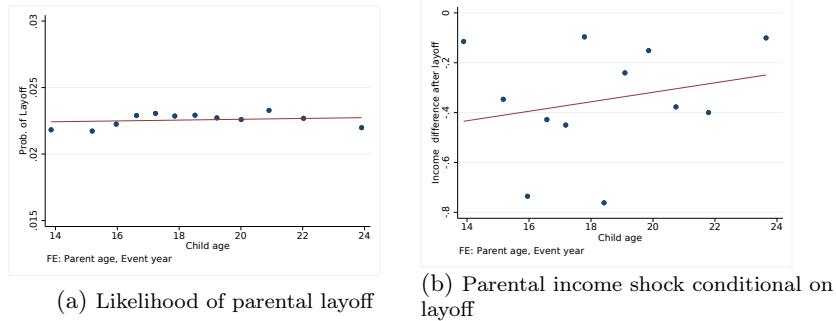
Notes: The solid line represents the total number of layoffs per year in our dataset. This is constant across all panels. The dashed line represents the characteristics of the set of laid off workers in the given year. Earnings are defined as all labor earnings in event year t-2 relative to layoff. Manufacturing 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 3: Effects of layoffs on parent's outcomes



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.

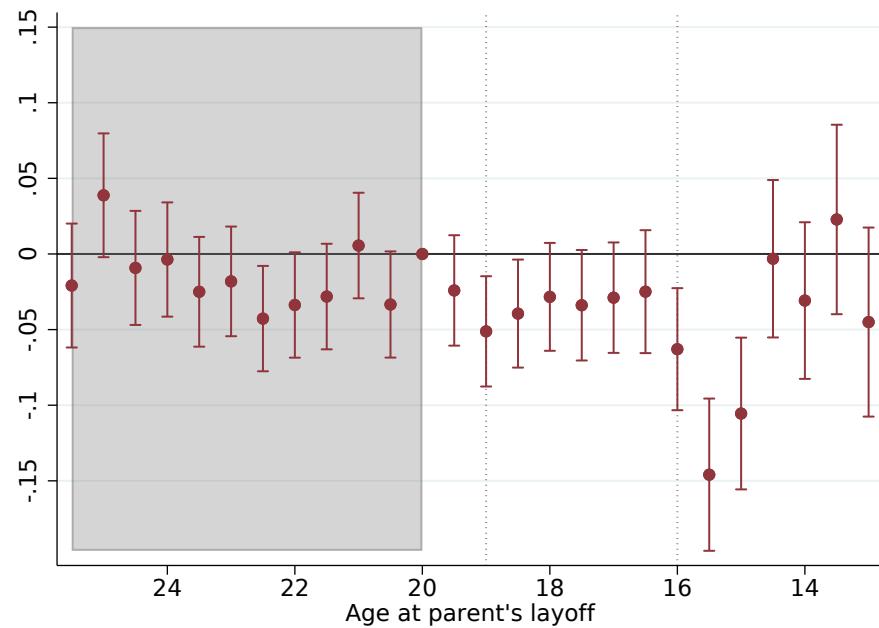
Figure 4: Layoff characteristics by child age



Notes: Panel (a) 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 (-.0000458 , .0001079) with a p-value of 0.428.

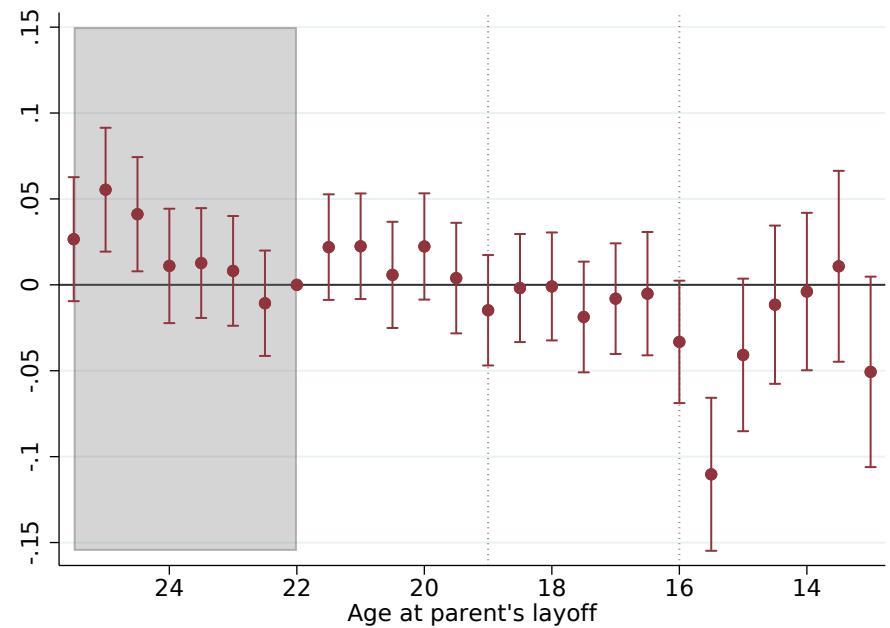
In panel (b), the outcome of interest is parental earnings loss after layoff. Earnings loss is measured as the average share of income earned in the two years after layoff, relative to pre-layoff income. Again, the binscatter shows no significant correlation between the size of the income shock and child age, conditional on parent age and event year. The 95 percent confidence interval on the linear coefficient is (-.0693986 , .031357) with a p-value of 0.459.

Figure 5: High school completion by age 19



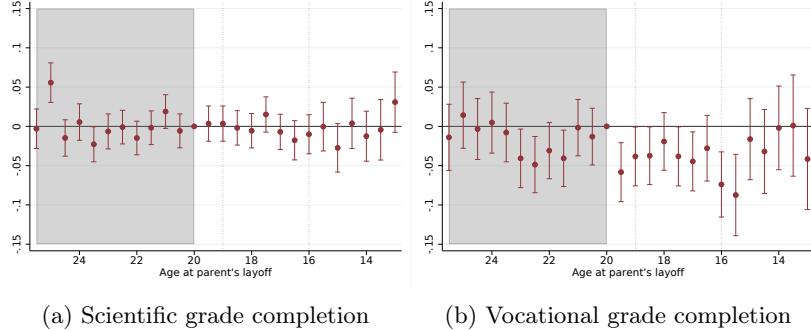
Notes: This figure shows the estimated impact of parental layoffs on children's on-time high school graduation rate separately by child age at the time of parent separation. The dotted line at age 19 represent the time when students are expected to graduate from high school. The dotted line at age 16 represent 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.

Figure 6: High school completion by age 21



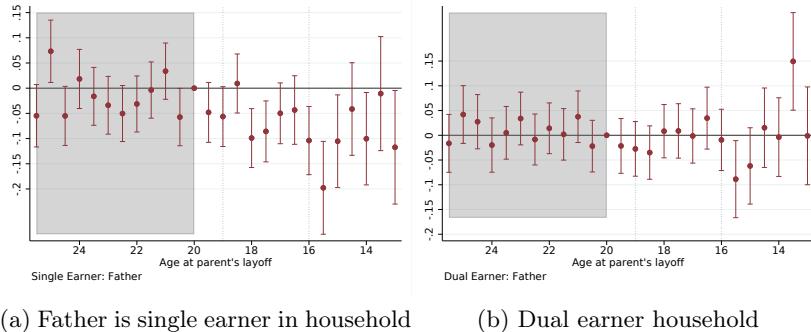
Notes: This figure shows the estimated impact of parental layoffs on children's on-time high school graduation rate separately by child age at the time of parent separation. The dotted line at age 19 represent the time when students are expected to graduate from high school if they are graduating on time. The dotted line at age 16 represent 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 7: Graduation rates from specific degree programs



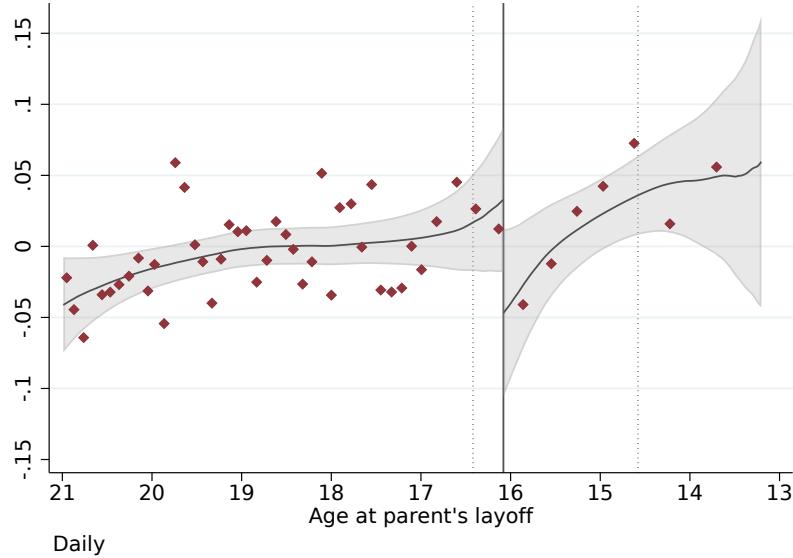
Notes: Panel (a) shows the likelihood of graduation from a scientific degree program (*Naturvetenskapliga programmet*) by age 19. Panel (b) shows the likelihood of graduation from any vocational degree program (*Yrkesprogram*) by age 19. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ 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 represent the time when students are expected to graduate from high school if they are graduating on time. The dotted line at age 16 represent the time when students transition from compulsory school to high school.

Figure 8: Heterogeneity by number of household earners

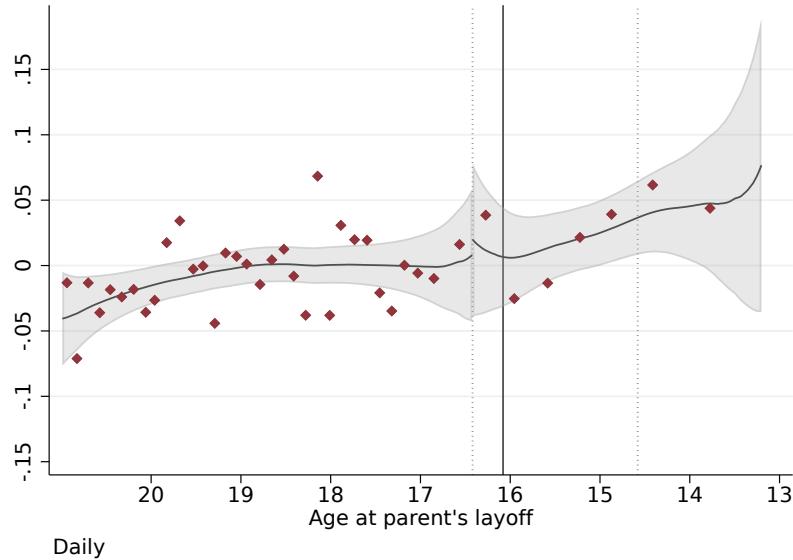


Notes: Panel (a) shows the likelihood of on-time high school graduation for parental layoffs when the parent was the single earner in the household. Panel (b) shows the effect of a parental layoff when a second working parent is present. The control groups are also restricted to be in single/dual earner 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. Tests of equality of coefficients are presented in Table 5.

Figure 9: High school graduation rate by separation date



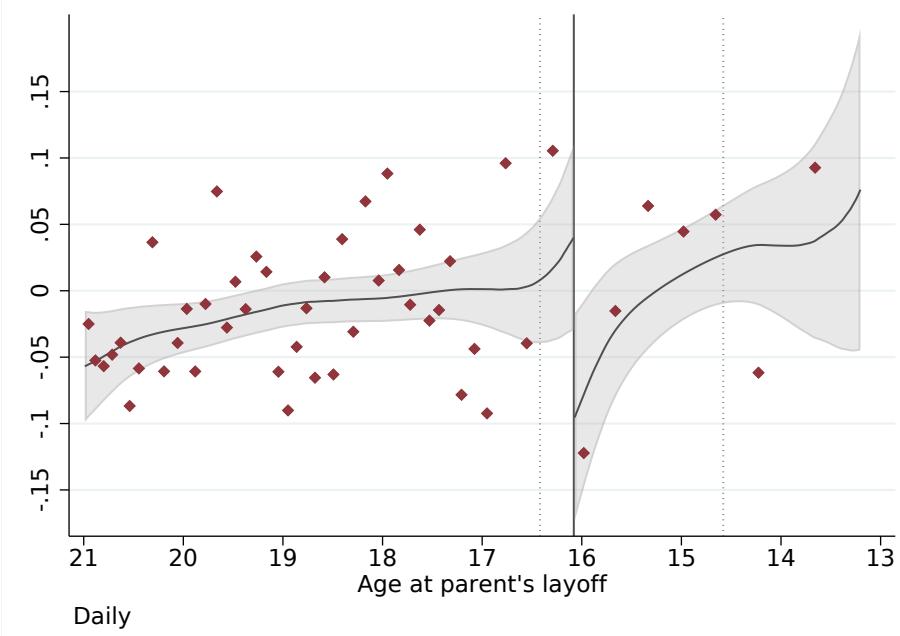
(a) Time series break at high school application deadline



(b) Time series break at Primary school final exams

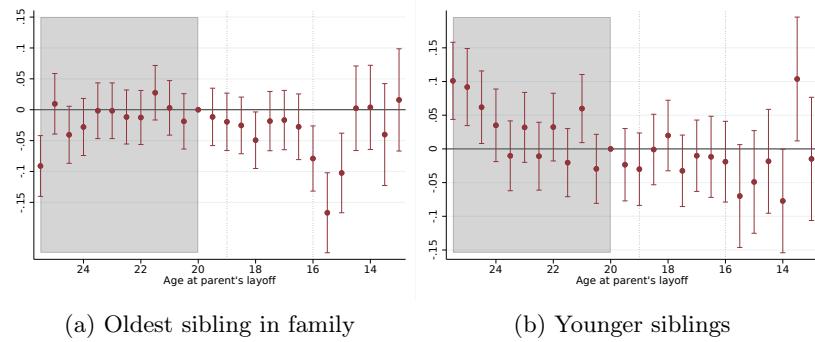
Notes: This figure shows the differential impact of parent layoffs on on-time high school graduation around the time of high school applications (panel a) and compulsory school final exams (panel b). The outcome is defined as the high school completion rate by age 19 relative to the matched control group. The estimated effect at the cutoff is robust to a wide selection of bandwidths, see Appendix Figure A9. See Appendix B.1 for placebo tests. The solid line represent the high school graduation deadline in 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 10: 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 represent the high school graduation deadline in 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 11: Information: Heterogeneity by sibling order



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.

7 Tables

Table 1: High school choice

Panel A: High school applications

	Mean	(sd)
Nr Choices	3.343	(1.439)
Share of students who attend 1st choice	0.652	(0.476)
Share of students who were admitted to first choice	0.897	(0.304)

Panel B: Track choice

	Vocational	Theoretical
Share of HS students	0.54	0.42
Share of students in track who proceed to university	0.72	0.29

Note: Summary statistics on high school applications are based on data from Statistics Sweden.

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 (0.442)	0.265 (0.441)	0.207 (0.405)	0.208 (0.406)	0.464 (0.499)
Age	40.69 (5.225)	41.49 (5.267)	40.62 (4.645)	40.66 (4.645)	39.29 (7.325)
Labor income (2010 SEK)	274849.3 (161831.4)	301971.3 (210699.1)	297828.8 (170528.3)	299090.6 (209228.8)	239647.7 (201762.2)
Income vintile	13.77 (4.262)	14.62 (4.356)	14.51 (3.965)	14.41 (4.074)	12.76 (5.130)
Highest ed: University	0.119 (0.324)	0.166 (0.372)	0.123 (0.329)	0.128 (0.334)	0.296 (0.457)
Highest ed: High school	0.538 (0.499)	0.502 (0.500)	0.542 (0.498)	0.542 (0.498)	0.408 (0.492)
Married	0.557 (0.497)	0.546 (0.498)	0.569 (0.495)	0.584 (0.493)	0.347 (0.476)
Unemployed	0.0779 (0.268)	0.0379 (0.191)	0.0607 (0.239)	0.0408 (0.198)	0.0845 (0.278)
Tenure at job (months)	57.81 (41.32)	71.90 (42.82)	60.63 (41.64)	72.68 (44.20)	49.95 (35.66)
Manufacturing	0.455 (0.498)	0.468 (0.499)	0.460 (0.498)	0.461 (0.498)	0.236 (0.425)
Year	2001.5 (2.352)	2001.0 (2.694)	2001.6 (2.235)	2001.7 (2.226)	2001.5 (2.332)
Any children?	1	1	1	1	0.893 (0.309)
<i>N</i>	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 (0.00542)	-0.00387 (0.00724)	0.0205*** (0.00784)	0.00247 (0.00615)	0.0226* (0.0116)
Comp. school (Age 14.5-16)	-0.0602*** (0.0152)	-0.0400* (0.0207)	-0.0857*** (0.0215)	-0.0605*** (0.018)	-0.0585** (0.0286)
High school (Age 16.5-19)	-0.0162* (0.00905)	-0.018 (0.0122)	-0.0148 (0.0129)	-0.0200* (0.0104)	-0.00649 (0.0186)
Constant	0.637*** (0.00427)	0.599*** (0.00574)	0.686*** (0.00611)	0.645*** (0.00483)	0.609*** (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 5 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: 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 (6664.9)	3644.1 (6562.1)	2561.7 (6494.0)	3879.6 (6657.0)
Age 17	5626.9 (14417.4)	7741.7 (11566.8)	5578.8 (13613.7)	8281.1 (11552.0)
Age 18	12578.8 (27422.0)	16022.7 (20662.1)	11996.2 (25119.1)	17319.2 (21031.8)
Age 19	29117.4 (50167.1)	50860.2 (49222.0)	26559.7 (45475.6)	58116.4 (50105.7)
Age 20	52173.2 (76473.8)	109137.4 (91116.9)	51019.1 (71388.0)	123550.5 (91765.8)
Age 21	70909.5 (92493.2)	140402.5 (104072.7)	78690.0 (93259.3)	150370.0 (104196.2)
Age 22	87670.8 (103576.9)	162874.6 (112757.7)	99762.1 (106138.8)	170618.2 (112820.2)
Age 23	103704.6 (113974.9)	178837.4 (119975.4)	116881.7 (117006.6)	185664.2 (119425.7)
<i>N</i>	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.

Table 5: Heterogeneity by number of earners in household

VARIABLES	(1)		(2)	
	Fathers only		Both parents	
	Single fathers	2 earner HH	Single parent	2 earner HH
Layoff age 15.0	-0.0941*	-0.0694	-0.0449	-0.0969**
	(0.0547)	(0.0530)	(0.0485)	(0.0427)
Layoff age 15.5	-0.196***	-0.0916	-0.155***	-0.101**
	(0.0574)	(0.0587)	(0.0533)	(0.0442)
Layoff age 16.0	-0.0968**	-0.0123	-0.0876**	-0.0303
	(0.0440)	(0.0384)	(0.0401)	(0.0327)
Layoff age 16.5	-0.0446	0.0198	-0.0437	-0.0231
	(0.0486)	(0.0437)	(0.0443)	(0.0364)
Layoff age 17.0	-0.0494	-0.00426	-0.0287	-0.00675
	(0.0396)	(0.0357)	(0.0363)	(0.0311)
Layoff age 17.5	-0.0837**	0.00448	-0.0637*	0.000335
	(0.0411)	(0.0367)	(0.0373)	(0.0311)
Layoff age 18.0	-0.0944**	0.00979	-0.0512	0.0137
	(0.0384)	(0.0353)	(0.0349)	(0.0300)
Layoff age 18.5	0.0114	-0.0462	0.0302	-0.0536*
	(0.0398)	(0.0358)	(0.0362)	(0.0303)
Layoff age 19.0	-0.0554	-0.0327	-0.0307	-0.0260
	(0.0400)	(0.0356)	(0.0361)	(0.0304)
Single father	-0.107***		-0.116***	
	(0.00688)		(0.00572)	
Layoff event	-0.0247		-0.0110	
	(0.0240)		(0.0210)	
Constant	0.680***		0.681***	
	(0.00154)		(0.00120)	
Observations	373,171		569,075	
R ²	0.018		0.024	

Panel B: F tests	Standard errors in parentheses		
(Single = Dual), age 15.0	0.100		0.650
Prob>F, 15	0.746		0.420
(Single = Dual), age 15.5	1.600		0.610
Prob>F, 15.5	0.206		0.433
(Single = Dual), age 16.0	2.090		1.230
Prob>F, 16	0.148		0.268
Age 16-19 = 0 for single	1.910		1.430
Prob>F, single	0.0758*		0.199

Standard errors in parentheses

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

N46s:

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** (0.0431)	0.09568* (0.05172)	0.09566* (0.05171)	0.235*** (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 the 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: Heterogeneity: Sibling order comparison

VARIABLES	(1) HS graduate, age 19	(2) HS graduate, age 21
Older sibling	0.0478*** (0.00615)	0.0402*** (0.00554)
Layoff	-0.0243 (0.0259)	-0.0199 (0.0233)
Oldest: Treated age 15.0	-0.0924** (0.0439)	-0.0852** (0.0396)
Younger: Treated age 15.0	-0.0282 (0.0503)	0.0479 (0.0453)
Oldest: Treated age 15.5	-0.160*** (0.0469)	-0.153*** (0.0422)
Younger: Treated age 15.5	-0.0522 (0.0529)	-0.0139 (0.0475)
Oldest: Treated age 16.0	-0.0609* (0.0362)	-0.0268 (0.0325)
Younger: Treated age 16.0	0.000923 (0.0391)	-0.0172 (0.0352)
Constant	0.606*** (0.00138)	0.723*** (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 11a and 11b. 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 3 shows the evolution of mean earnings over time relative to layoff for the treatment and control group, including ?? and excluding 3a 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 2 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 A4 (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 A5 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 unemployment¹⁹ 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 no appear in this administrative dataset.

B Appendix: Additional results

B.1 Interrupted time series

We use the exact termination date specified in the layoff to estimate the time series - explain method.

Age in days are calculated as the difference in days between the termination date in the parental layoff and January first of the birth year of the child. As education cohorts and birth year cohorts are identical, Age at parent's layoff tells us where in the school system the child is if she followed the normal path of schooling.

¹⁹Individuals aged 15 to 24

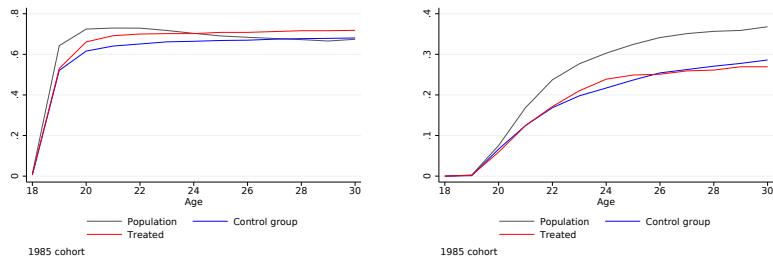
Figure A8 shows the raw data on graduation rates by time of parental layoff. Panel A groups the data into equally-sized bins over the age-date distribution, panel B shows the monthly discrete bins. The pattern is qualitatively similar to that in figure (9a). In order to ensure that figure 9a is comparable with figure ??, we calculate the layoff probability relative to the control group within each weighting cell, and include only the treated families with valid matches.

Figure A9 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.

There is no change in density around the February cutoff relative to the May 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 groups. Figure A10 shows pre-determined variables such as the birth year of the child (panel A10c) and parental pre-layoff income (panel A10a) is not different on each side of the cutoff. The unemployment spells also appear to be similar around the deadline, with an equal number of days in unemployment (panel A10d) and similar earnings in the two years after the layoff (panel A10b).

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

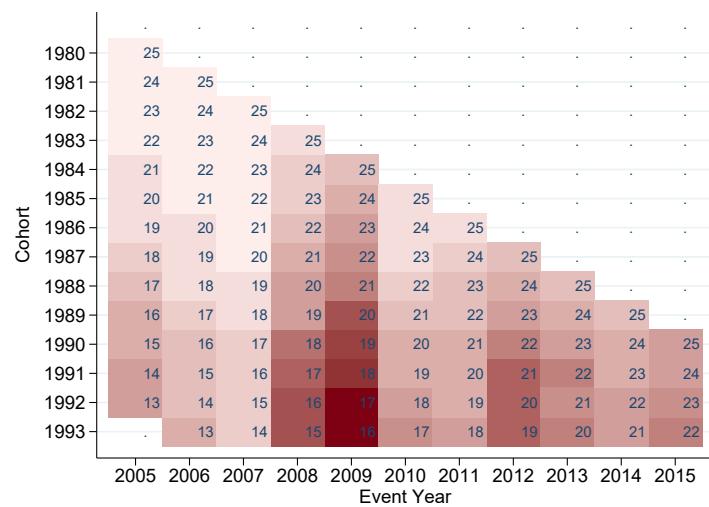
Table A1: High School Choice Environment

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.

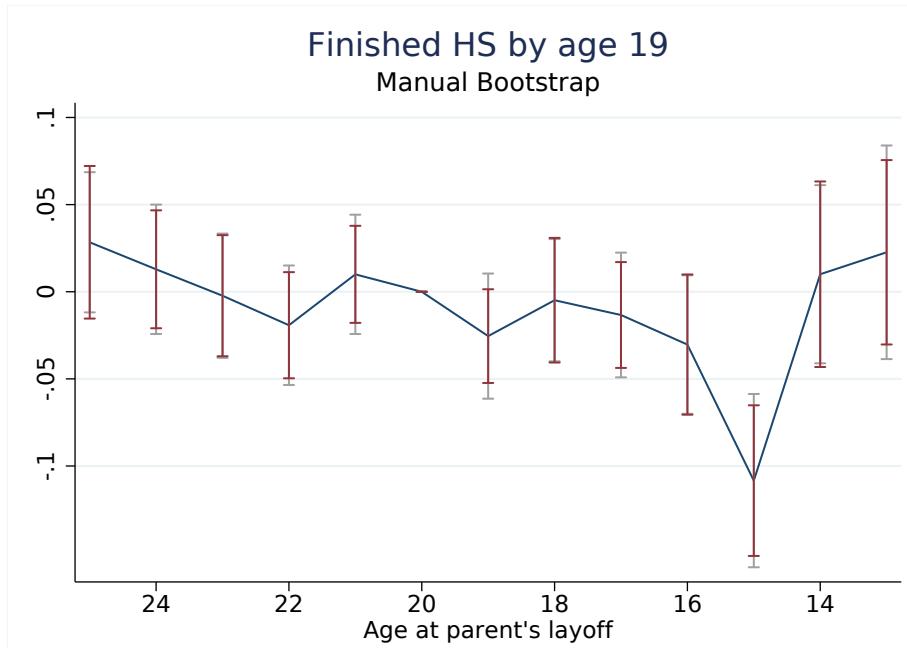
* School markets are defined based on 2010 commuting patterns defined by Skolverket (2011).
Source: Skolverket

Figure A2: 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 A3: High school completion by age 19, SE



A comparison between bootstrapped and analytical standard errors. Bootstrapped confidence intervals are in red and analytical confidence intervals in gray.

Figure A4: Income paths by education level - selected cohorts

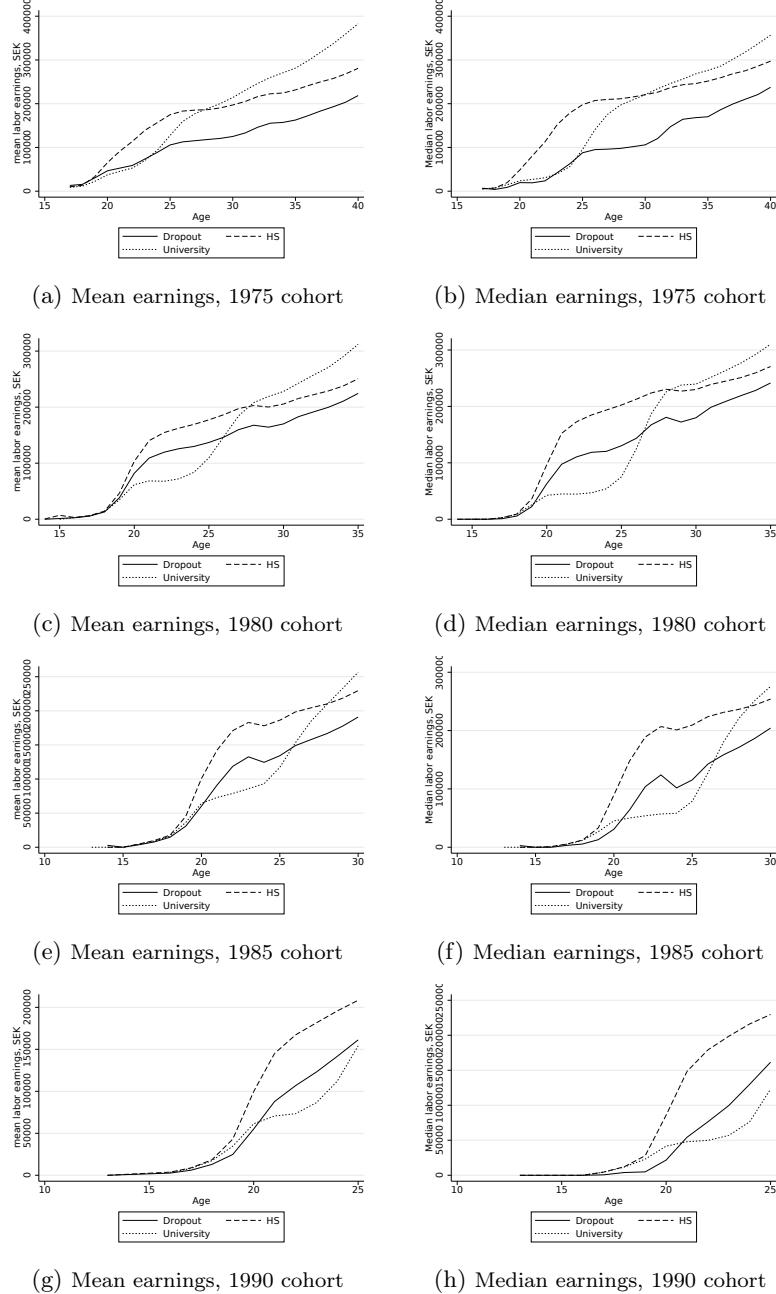


Figure A5: Income paths by education level age 16-20 - selected cohorts

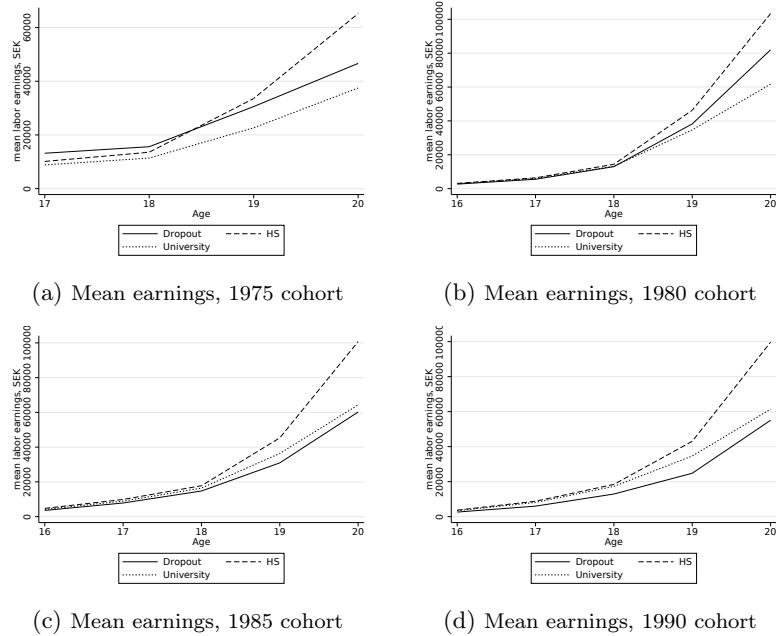


Figure A6: Effect of parent layoff on child earnings

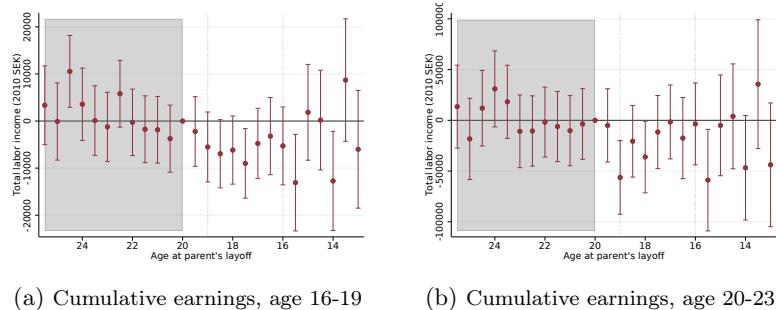
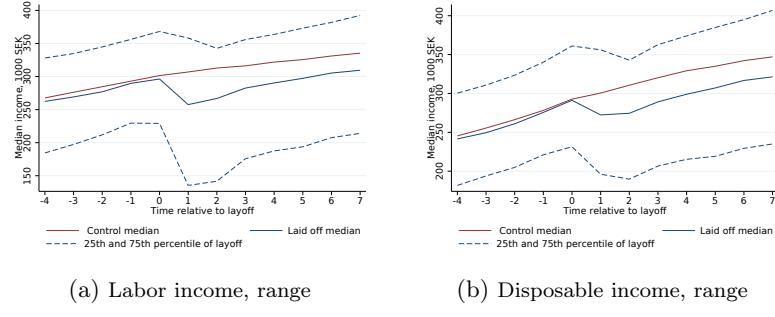
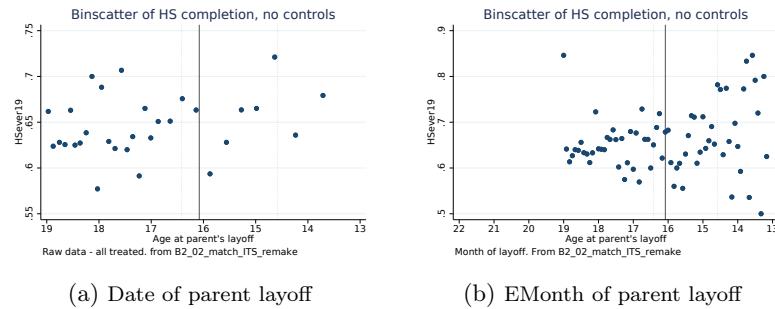


Figure A7: Range of parent outcome around layoff



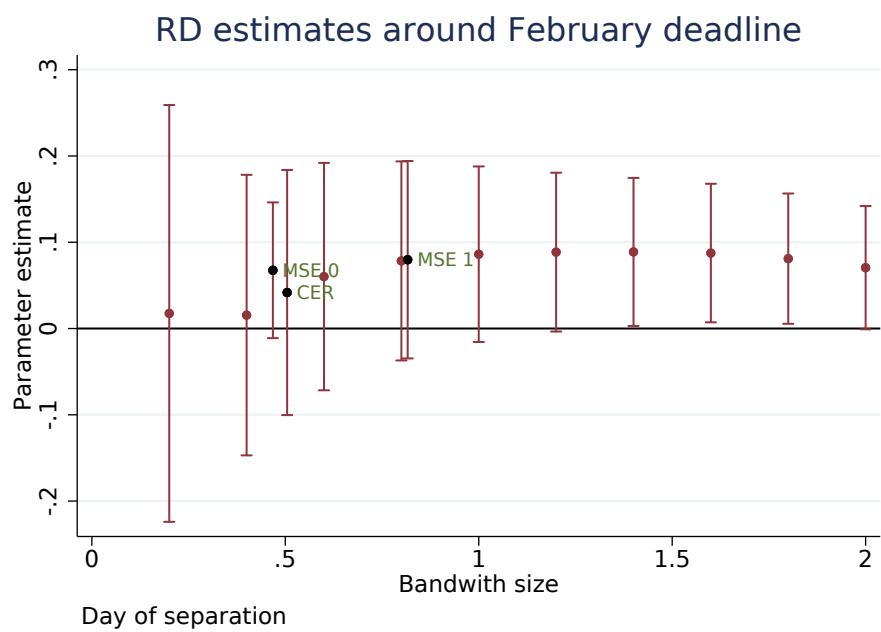
The treatment group is defined as individuals who appear in the layoff data with a termination date normalized to year 0. The control group is weighted according to the procedure described in section 3. Disposable income includes unemployment benefits and labor income. Income is measured annually in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

Figure A8: Graduation rates for children with parental layoff only



Raw data: Binscatter of the high school completion by day (panel a) and month (panel b) of separation. No controls, only data from families with a parental layoff. Panel a is approximated by equally sized bins across the distribution of layoffs. In panel B, each scatter is a calendar month-age bin.

Figure A9: 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 A10: Placebo test of time series interruption

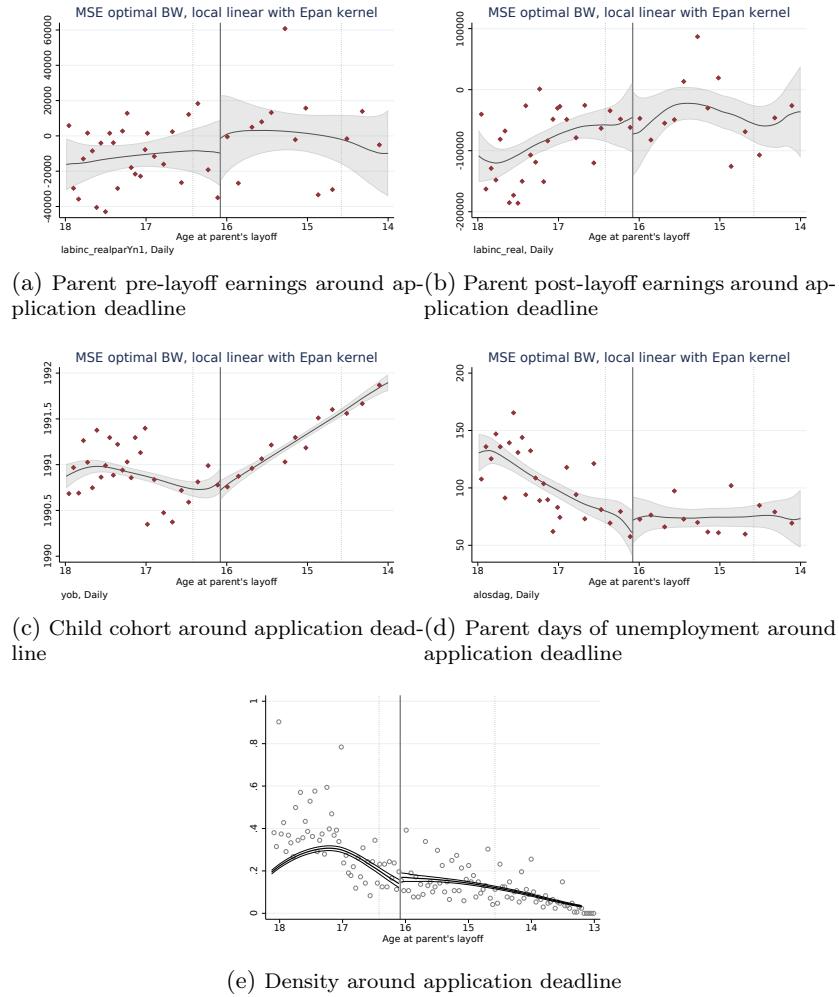


Figure notes: The non-parametric fit is estimated by a local linear estimator (polynomial of degree 1) with an MSE-optimal bandwidth following Calonico et al. (2014)