The Effect of Parental Layoff on Children's Academic Performance

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ABSTRACT: While some studies have documented large, long-term effects of parental layoffs on children's future income, there is little evidence on the short-term effects on children's human capital accumulation, particularly for younger children. Using administrative data from British Columbia, Canada, I study the short-term effect of parental layoff on children's academic performance in grades 4, 7, and 10. I find that households lose approximately $10,000 - $12,000 in disposable income in the first two years after the layoff. In spite of such a large loss in financial resources, I find no significant short-term effects on children's test scores due to parental job loss. My estimates for grade 4, where I have the largest sample, rule out short-term negative treatment effects larger than 3.5% of a standard deviation at the 95% confidence level. For children in grade 10, I find a small and non-significant positive treatment effect on test scores, and can rule out negative treatment effects larger than 3.4% of a standard deviation. These results imply that the long-term effects of parental layoffs on Canadian children's future income documented by other studies are not driven by short-run disruptions to children's human capital accumulation. I also find that treatment effect estimates relying solely on cross-sectional comparisons against a control group of children with similar household resources and demographic characteristics are susceptible to selection bias, and that controlling for past measures of ability can reduce this bias significantly.

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1 Introduction

On average, about 7% of Canadian workers experienced a layoff in any given year (Morissette and Qiu, 2020). Layoffs are associated with severe consequenses for individuals: large, persistent fall in earnings (e.g. Jacobson et al., 1993; Stepner, 2019; Illing et al., 2021), increased rates of divorce (Charles and Stephens, 2004), mental health problems (Kuhn et al., 2009), and premature mortality (Sullivan and Von Wachter, 2009). This paper studies the spillover effect of parental layoff on children's academic performance.

Layoffs are a multifaceted shock to the household and there are several potential mechanisms through which a parental job loss could affect a child's academic performance. The two most important factors to consider are the reduced parental income and increased parental time available for child-rearing. Several studies have documented a positive relationship between household financial resources and children's academic performance (e.g Duncan et al., 1998; Blau, 1999; Chetty et al., 2011; Dahl and Lochner, 2012; Løken et al., 2012; Agostinelli and Sorrenti, 2021).[1] Any income losses due to parental layoff could thus have a detrimental effect on children's performance in school. On the other hand, parental job loss could increase parental time investments in child-rearing, which can improve academic performance (e.g Carneiro et al., 2015; Bono et al., 2016; Fort et al., 2020; Agostinelli and Sorrenti, 2021). Though, this additional time investment might be of lower quality due to increased stress from the layoff (Kuhn et al., 2009).

There are two major challenges in estimating reliable treatment effects for parental job loss on children's academic performance. The first is finding appropriate data that contains children's test scores and parent's labour market histories, including a well-defined measure of layoffs. The second is finding an appropriate empirical strategy that accounts for selection into layoffs. I address both these challenges in this paper.

I utilize administrative data from the province of British Columbia (BC), Canada, that tracks all students enrolled in a school in BC between the academic years 2002 and 2017 from kindergarten to high school. The BC K-12 dataset contains students' scores on standardized tests in grades 4 and 7, as well as their scores on compulsory high school exams in grade 10. This dataset is linked with the T1 Family File (tax records), allowing me to observe parental income going back to 1993. Importantly, a new data linkage was conducted by Statistics Canada to bring in Record of Employment (ROE) data for this paper specifically. The ROE dataset provides a well-defined measure of layoffs, as well as the precise timing of the layoff.

Using an eventy study design, I find that layoffs result in a large shock to household resources.

[1] See surveys in Heckman and Mosso (2014) and Kalil (2015) on the role of parental income in children's human capital accumulation.

Compared to their counterfactual, households lose approximately $10,000 - $12,000 in after tax income in the first two years after the layoff. Earnings then recover partially, but even after 10 years post-layoff, households earn about $7,000 less than they would have had they not suffered a job loss.

To study the effect of parental job loss on children's academic performance, I construct a difference-in-difference (DiD) estimator proposed by Hilger (2016), combining two sources of variation: i) a matched control group with similar economic and demographic characteristics to households that experience a layoff found using propensity score matching, and ii) variation in the timing of the layoff relative to the children's test. The second source of variation is important because I find evidence that cross-sectional comparisons of children whose parents are laid off against a matched control group are susceptible to selection bias due to unobserved heterogeneity. Children whose parents were laid off *after* the test scored 6 - 7% of a standard deviation lower than their matched control group. The finding echoes results in Hilger (2016), who finds that children of parents who were laid off when the child is older than 18 years had lower college enrollment rates at age 18 compared to a matched control group of children whose parents were not laid off. In the DiD design, the difference in test scores of children whose parents are laid off after the test and test scores of children in their matched control group provides a measure of selection on unobservable characteristics that remains after the matching procedure.

Using the DiD, I find that parental layoff within the two years prior to the test has no significant impact on children's academic performance in grades 4, 7, and 10. The point estimates for the treatment effects are close to zero for all grades. For grade 4, where I have the largest sample, I can rule out negative treatment effects larger than 3.5% of a standard deviation at the 95% confidence level. For children in grade 10, I find a small and insignificant positive effect (1.5% of standard deviation) on test scores, and can rule out negative treatment effects larger than 3.4% of a standard deviation. I estimate a small and insignificant negative treatment effect (-1.5% of a standard deviation) for grade 7, and can rule out negative treatment effects larger than 5.3% of a standard deviation.

There are two plausible explanations for such negligible effects. First, it's possible that parental time investment offsets any negative effects due to income loss. This is in line with Agostinelli and Sorrenti (2021), who find a strong trade-off between income and parental time investments in the context of the EITC expansion in the US. The second plausible explanation for the negligible effects is that borrowing constraints are not particularly important in the short run. This is consistent with the findings of Carneiro and Heckman (2002) and Cameron and Taber (2004). Cameron and Heckman (2001) also argue that parental background and family environment are much more important than credit constraints in explaining educational achievement gaps. This also aligns with Caucutt and Lochner (2020) who find that while life-cycle borrowing limits play a crucial role in determining parental investments in children, borrowing limits at any point in time are not particularly important.

Findings from the heterogeneity analysis also confirm that borrowing constraints are not playing a significant role. If borrowing constraints were binding, we would expect a large negative treatment effect on children from lower-income households. For all three grades, I find statistically insignificant small negative treatment effects (1.6-2.2% of a standard deviation) for children from households in the lower half of the income distribution.

For grade 10, I find a modest *positive* treatment effect of 3.9% of a standard deviation for children in higher income households, though the estimate is statistically insignificant. This is likely due to higher offsetting time investments in the higher-income households. Guryan et al. (2008) find that higher-earning parents spend more time on child care than their lower-income counterparts.[2] Thus, it is possible that following the layoff, parents in the top half of the income distribution increase time investments much more significantly than parents in the lower half of the distribution. Moreover, parents in higher-income households are likely to be more educated (not observed in the data), making any additional time investments following the layoff much more productive.

The negligible treatment effect for grade 10 masks some important heterogeneity by demographic characteristics. I find that children from households where English is the primary language spoken at home have a large *positive* treatment effect. These children have average test scores that are 5.3% of a standard deviation (p-value = 0.034) higher than the counterfactual case had their parent not been laid off. On the other hand, children from hosueholds where a non-English language (Chinese, Punjabi, French, or other) is the primary language spoken at home have a large *negative* treatment effect of 8.5% of a standard deviation (p-value < 0.01). However, I do not find evidence of such large disparities for children in grades 4 and 7.

I also find that the negative selection observed in the post-test sample can be halved by controlling for past test scores in case of grades 7 and 10. However, the selection bias is not completely eliminated. The past test scores that are added to the extended model were observed three years prior to the current test scores, which could mean that they are not perfectly representative of children's ability just prior to the layoff. This would be consistent with models of human capital accumulation with dynamic complementarities, for instance (e.g Heckman and Navarro, 2007), that lead to a divergence in children's ability in the layoff sample and control group between the time that the past ability is observed and the time of the layoff. It's also possible that in the intervening years parents in the layoff sample invest differently in their children than those in the control group, for e.g., due to differences in education level, which is not observed in my dataset.

[2]The published version of Guryan et al. (2008) only report results for time spent on child care by parent's education level. The authors refer to the working paper version of the same paper available on NBER, which contains results based on income quintiles.

The first contribution of the paper is to study short-term treatment effects of parental layoffs on academic performance for young children.[3] To my knowledge, Ruiz-Valenzuela (2020) is the only other study focusing on children's test scores before high school. The research design is limited by the small sample (54 treated, 124 control), which came from a single school located in Barcelona, Spain, followed over a period of four years. Moreover, Ruiz-Valenzuela (2020) pools students of different ages but the average age of children in her sample is the age of a grade 4 student. The author finds that paternal job loss decreases children's test scores by 15% of a standard deviation on average. The large treatment effect is likely driven by selection bias; for instance, children in the control group in the study are much more likely to be from higer-income families than children in the treated group. My estimates for grade 4 and 7 rule out such large treatment effects for children in British Columbia. These results suggest that the large, long-term effects of parental layoffs on Canadian children's future income documented by Oreopoulos et al. (2008) and Uguccioni (2021) are not driven by short-run disruptions to children's human capital accumulation.[4]

A second contribution of my paper is to study the impact of parental job loss on high school performance using an improved empirical strategy that allows me to relax the selection on unobservables assumption. My treatment effect for grade 10 subsample with *paternal* job loss rules out treatment effects observed by Rege et al. (2011) in the Norweigian context. Rege et al. (2011) use a cross-sectional comparison between children whose parents are laid off and children in a control group using a saturated regression that controlled for household and firm characteristics. As discussed earlier, I show that such cross-sectional comparisons are susceptible to selection bias due to unobserved heterogeneity. Rege et al. (2011) find that paternal job loss due to a mass layoff in the two years prior to grade 10 reduces children's grade 10 GPA by 6% of a standard deviation on average in Norway. In contrast, using my DiD design, I find that paternal job loss in the two years prior to grade 10 in British Columbia has a *positive* treatment effect on children of 4.8% of a standard deviation. While this treatment effect is statistically insignificant at all conventional levels of significance, the 95% confidence interval rules out negative treatment effects larger than 1.1% of a standard deviation. Mörk et al. (2020), on the other hand, again relying on cross-sectional comparisons, find that *maternal* job loss due to firm closure two years prior to grade 10 reduces children's grade 10 GPA by about 7% of a standard deviation for children in Sweden. My point estimate for the treatment effect of maternal job loss is very close to zero (-0.6% of a standard deviation), with a standard error of 0.035. Hence, their estimated effect is outside the 90% confidence interval of

[3]In related work, researchers have also found detrimental effects of parental layoffs on young children's health. Mörk et al. (2014) finds that parental unemployment is associated with increased hospitalization for children in the short and long run. Other papers have linked parental layoffs to worse mental health and socioemotional behaviour for children (Peter, 2016; Bubonya et al., 2017; Schaller and Zerpa, 2019).

[4]Two other studies, Kalil and Ziol-Guest (2008) and Stevens and Schaller (2011), have focused on early educational outcomes using the Survey of Income and Program Participation (SIPP) in the US. However, these studies are limited to studying the effects of parental job loss on grade retention due to the unavailability of test scores in these datasets. Both studies find that paternal job loss increases the likelihood of grade retention. While grade retention is an important measure of educational attainment, it is a low-frequency measure and only relevant for students who were already close to being retained prior to the paternal job loss.

my estimate.

The third contribution of the paper is to show that the large long-term treatment effects based on cross-sectional comparisons against a control group (e.g. Oreopoulos et al., 2008; Uguccioni, 2021) may in part be driven by selection on unobservable characteristics. Hilger (2016) found similar selection bias using data on college enrolment in the US. The assumption of no selection due to unobservable heterogeneity is untestable in studies considering the long run effects of parental job loss on children because children's outcomes are only observed once and the difference-in-difference strategy used in this paper is not feasible to study long-term effects. My findings suggest that controlling household resources and other socioeonomic characteristics, as I do in my baseline sample, might not be enough to eliminate the selection bias. I also show that controlling for past measures of children's ability can substantially reduce this bias.

2 Data

2.1 Student Level Data from British Columbia (BC K-12)

I use K-12 dataset from British Columbia (BC), which contains enrolment information on all students attending a public or independent school in BC from 1991 to 2019. The dataset contains several important demographic variables such as age, gender, and home language.

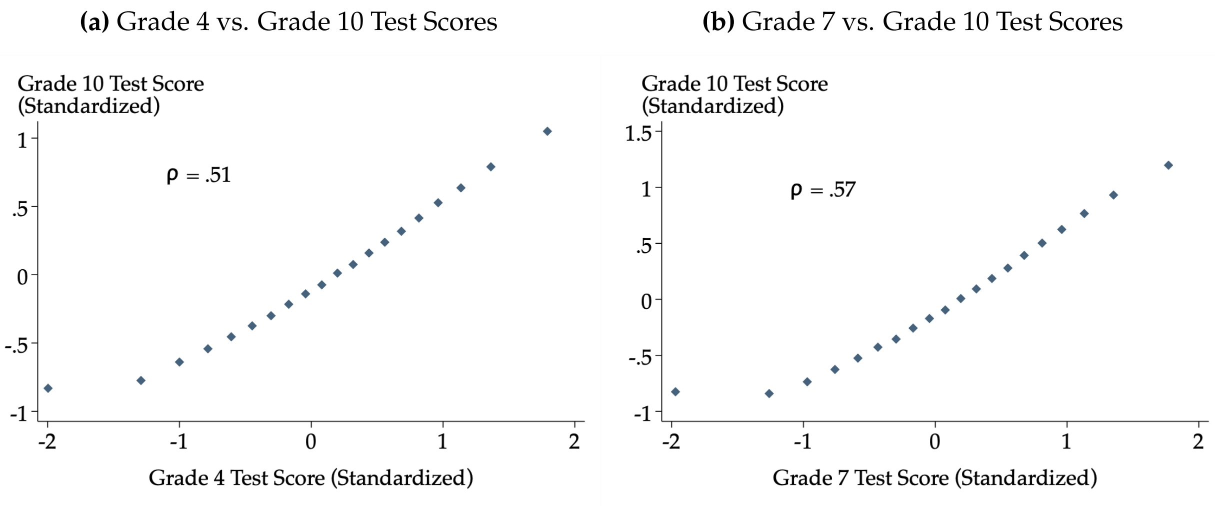
Foundation Skills Assessment (FSA): The province introduced the Foundation Skills Assessment in 2000/2001. The FSA consists of three standardized tests - reading, writing, and numeracy - in grades 4 and 7. The dataset contains test scores on all three subjects.

Until the academic year 2007/2008, the FSA was conducted during the month of May across the province. Since 2008/2009 academic year, the FSA has been administered during January and February. As discussed later, I use the narrow timing of the test administration in my empirical strategy.

There is a high and increasing rate of non-participation on the FSA in both grades 4 and 7. The BC K-12 dataset includes an indicator if the student was offered the test and did not participate. For my main results, I restrict to students that attempted all three assessments, but also report results for non-participation in the appendices. [5]

[5] Students that miss the test are given a score of zero. In some rare cases, students attempt the test, but either leave the assessment blank or write nothing intelligible, which is practically non-participation. These students are also given a zero, and I consider these students as non-participants.

Figure 1: Binscatter Plots for FSA Test Scores Against Grade 10 Test Scores



Notes: ρ is the correlation coefficient using the entire underlying data; not the binned data plotted in the figures.

While these are rather low-stake tests for students, children's test scores on the FSA in grade 4 and grade 7 show a clear positive relationship with their test scores on compulsory subjects in high school (Figure 1), and hence, are a good indicator of children's academic ability.

High School Exams: The Dogwood diploma was introduced in 2004. Students are required to write tests in several compulsory tests in grades 10, 11, and 12, to graduate high school. The dataset contains test scores on three compulsory subjects in grade 10: science, math, and English language. The final scores on the subjects are comprised of children's scores on test scores on exams conducted by their school, as well as a provincial exam, with the latter weighing 20% in the final score.

Unlike the FSA, the precise timing of the grade 10 exams is not available in the data. Schools offer exams at multiple times through the years and students have some flexibility as to when to write the exam.

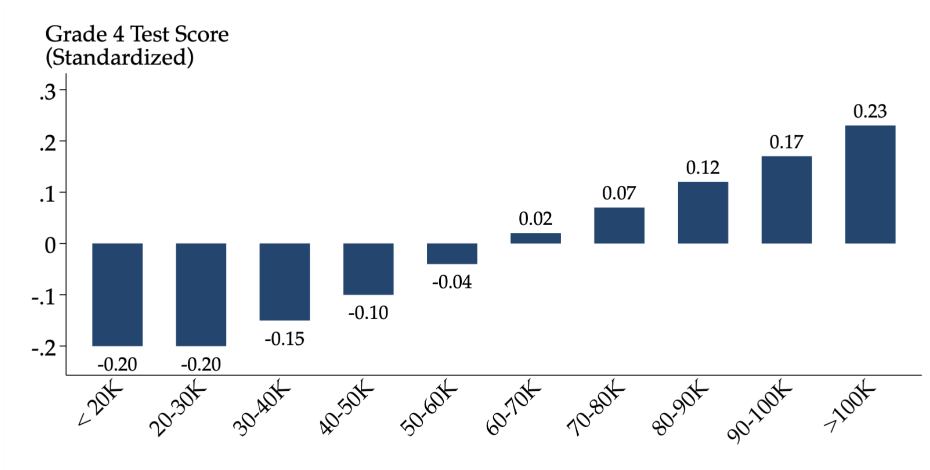
2.2 Linkage with Parental Tax Data (T1FF)

Statistics Canada recently linked the BC K-12 data with the T1 Family File (T1FF) to bring in parental tax records. T1FF is created by Statistics Canada by linking individuals to their spouses and children using information in the individual T1 tax return and child benefit claims, and covers approximately 96% of the population (Statistics Canada, 2022).

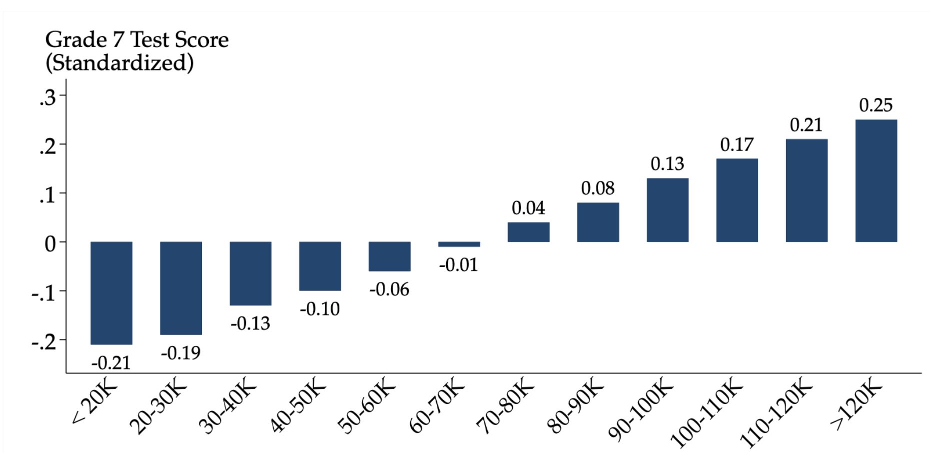
Notably, the way this linkage was conducted imposes some restrictions on years for which test

Figure 2: Test Scores by Household After-Tax Income

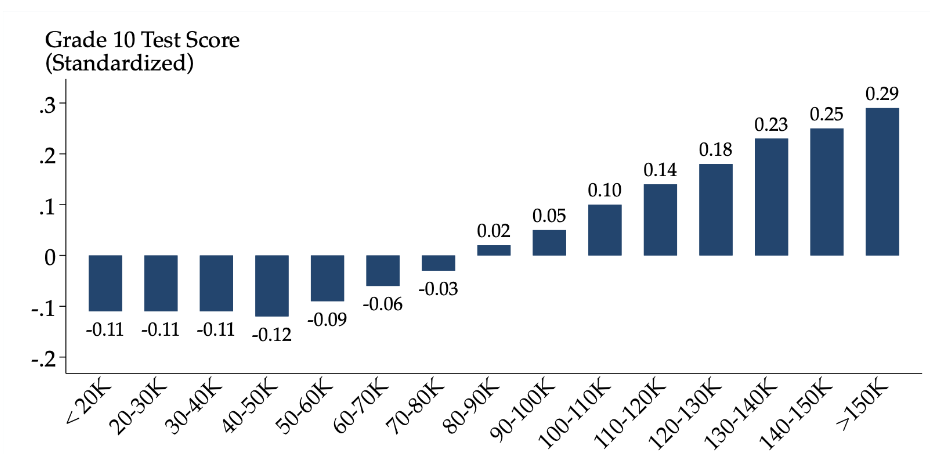
(a) Grade 4 Test Scores by HH After-Tax Income



(b) Grade 7 Test Scores by HH After-Tax Income



(c) Grade 10 Test Scores by HH After-Tax Income



Notes: Household after-tax income is the average after-tax income from two years prior to the test.

scores are observed. Children were linked to their parents using child benefit claims starting in 2011. Since the child benefit can only be claimed for children under the age of 18, any children that are over 18 years old in 2011 cannot be linked to their parents. Children in grade 4 are typically 9 years old. Hence, the earliest academic year when I observe children's FSA scores in grade 4 is 2002/2003. Similarly, I observe FSA scores in grade 7 starting in 2005/2006 academic year, and grade 10 scores starting in academic year 2008/2009.

2.3 Linkage with Record of Employment Data

A new linkage was performed by Statistics Canada for this study. The new linked BC K-12-T1FF dataset was further linked with the Record of Employment (ROE) data, which allows me to identify plausibly exogenous job losses for parents. Employers have to file an ROE slip with the Canadian government after each job separation. This slip also indicates the reason for separation, such as layoff, a quit, a parental leave, etc., and contain information on start and end date for the job These ROE slips are also used to determine eligibility for employment insurance benefits in case of a layoff.

2.4 Cross-sectional Correlations

Figure 2 plots children's test scores against average after-tax household income in the two years prior to the test. To reflect the changing distribution in household income by parent's age, I have expanded the range of incomes for which test scores are shown in grades 7 and grade 10.

Household earnings in the year prior to the layoff for the grade 4 sample are about $\$80,000$ on average. Similarly, the baseline earnings for grade 7 and grade 10 samples are slightly higher at around $\$85,000 - \$90,000$. As Figure 2 shows, in the part of the distribution relevant to the layoff group, an increase of $10,000$ in after tax income is associated with about 5% of a standard deviation improvement in test scores in all grades. These cross-sectional correlations of family income and children's test scores serve as benchmarks for comparing the causal effects of layoffs estimated below.

3 Sample Restrictions

3.1 Setup

In the notation below, I use a to denote academic year in which the child writes the test. There is only one test period in each academic year. Parent's earnings are observed in calendar years. I use l to denote the calendar year in which the parent is laid off (or the calendar year in which a control observation is matched).

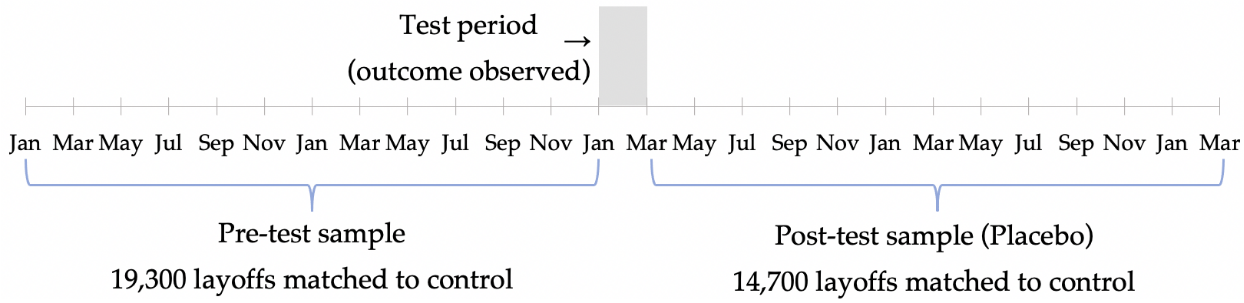
For grades 4 and 7, the test period is the three-month period during which the FSA assessments are conducted. Prior to the school year 2007-2008, assessments were conducted at the end of the school year, in May and June. Later, the timing of the assessment was changed to January and February during the school year. Hence, the test period below refers to the period April to June for academic years up to 2007, and period January to March for $a > 2007$. Students can take grade 10 tests at multiple points in the academic year. I do not observe precisely when they take the test during the year. In part, this depends on when their particular school offers the test. Hence, for grade 10, I define the test period as the entire academic year from September to August. I focus on children that are the typical age for the particular grade: 8-10 years old for grade 4, 11-13 years old for grade 7, and 14-16 years old for grade 10. In cases where a student has written the test multiple times due to grade retention, I only focus on the first test attempt.

I focus on layoffs that occur within a four-year window around the test period (excluding the test period itself). I am interested in the treatment effect of parental job loss on children whose parents are laid off in the two years prior to the test. I first match all such children to a control group of children with similar household characteristics but whose parents did not suffer a layoff.

Hilger (2016) shows that cross-sectional comparisons of children whose parents suffer a layoff and a matched control group are susceptible to bias. Hence, I use children whose parents suffered a layoff in the two-years after the test as a placebo group. I use the same matching strategy to find a suitable control group of children with similar household characteristics but whose parents did not suffer a layoff. For these children, the layoff occurs after the test, hence, any difference in test scores of children in the post-test layoff sample and their matched control group is a difference in pre-treatment outcomes, i.e. selection on unobservable characteristics.

Figure 3 gives a visual illustration of how the sample for grade 4 is created. For purposes of this illustration, I have used January to March as the test period. For grade 4, pre-test sample consists of 19,300 pairs of children whose parents were laid off in the two years prior to the test and a matched control group. The post-test sample consists of 14,700 pairs of children whose parents were laid off in the two years after the test and a matched control group.

Figure 3: Illustration of Sample Timeline - Grade 4



Notes: The figure shows the empirical framework used in constructing the final sample foir grade 4 which includes 19,300 matched pairs based on layoffs in the two years prior to the test, and 14,700 matched pairs using layoffs in the two years after the test.

Below, I discuss the matching procedure and provide detailed summary statistics before and after matching for samples in all three grades.

3.2 Layoff Sample

For each academic year a, I consider all children who have a parent aged 25-54 years old for whom I observe a Record of Employment (ROE) within a four-year window around the time of the test where the ROE lists layoff as the reason for separation. In all cases, I exclude the test period from my estimation window.

I add further restrictions commonly used in the literature (e.g Stepner, 2019; Illing et al., 2021) to ensure that I select workers with high labour market attachment for whom a layoff would most likely be an unexpected, exogenous shock. I restrict to workers with a tenure of at least 1 year (observed in the ROE) and with no job separations in the last two years. This two-year period also serves as "washout" period, so that I can identify the effect of the layoff independent of any prior layoffs that might have increased job instability and led to the current layoff observed. For the same reason, I add a restriction that the worker's spouse also did not suffer a layoff in the same year or the previous year. Further, because I use past earnings for matching, I only consider workers for whom I can observe earnings in the year of the layoff l as well as the last three years, $l-1$, $l-2$, and $l-3$.[6] For all these workers I only consider the first layoff observed during this four-year window around the assessment period.

[6]It's possible that earnings not observed in some years if the worker did not file a tax return.

3.3 Matched control group

The goal is to create a suitable comparison group of children who match the children in the layoff sample on observable characteristics in the data. The pool of potential comparison units include all children with parents aged 25-54 years old who are "never-treated". In this context, never-treated means that the children's parents have no layoffs during the four-year estimation window around the assessment period, ensuring that there is no overlap between my treatment and control groups. I apply the same restrictions on this pool of potential comparison units as in my definition of the layoff sample: I can observe income for the year of the match, l, and for the past three calendar years, $l-3$, $l-2$, and $l-1$. As in the layoff sample, I also impose a restriction that the comparison group did not have any job separations in the two years prior to the year of the match, and that the spouse of the matched parent also did not suffer a layoff in the match year or the prior year.[7]

3.3.1 Summary Statistics

I then perform matching within cells created using the following variables: school year s for the assessment, calendar year l for the parent, parent's gender, and child's gender. This ensures that I compare children of the same sex that write the assessment in the same year, and that the displaced parent is matched with another parent of the same gender. I match each child-parent pair from the layoff sample to an appropriate comparison child-parent pair using propensity scores. I estimate propensity scores for all workers within the cells using a probit regression with the following variables: worker's income in years $l-3$ and $l-2$, household income in period $l-3$, fixed effects for age in years, fixed effects for home language (Chinese, English, French, Punjabi, Other), fixed effects for family size (pooling 5+), fixed effects for family type (together, single, separate), fixed effects for years since last ROE, and fixed effects for years since last layoff.[8] Each child-parent from the layoff sample is then matched with a child-parent from the potential control group with the closest propensity score without replacement. All matches are unique; no child is used as a control unit for multiple children.

Table 1 presents the summary statistics before and after matching in grade 4. Pooling all school years, the final sample for the FSA in grade 4 consists of approximately $34,000$ child-parent pairs who suffer layoffs and are matched to a suitable control unit. Approximately $5,000$ child-parent pairs in the layoff sample were not matched to a suitable comparison unit because their estimated propensity score was outside the common support. Table 1 also illustrates the need for matching.

[7] Note that I cannot apply any tenure restrictions on the comparison group because tenure is observed in the ROE, which is only issued at the time of a job separation.

[8] For fixed effects for years since last ROE and years since last layoff, I use fixed effects for each year 2-10, 11+, and a fixed effect for workers with no ROE/layoff observed in the data as a separate category.

Table 1: Summary Statistics Before and After Matching (Foundation Skills Assessment in Grade 4)

	Unmatched		Matched - Pre		Matched - Post	
	Layoff	Control	Layoff	Control	Layoff	Control
Male parent	53%	52%	54%	54%	50%	50%
Parent's age	40.73	41.94	39.76	39.68	41.80	41.84
Male child	52%	51%	52%	52%	52%	52%
Family size *(l-3)*	4.09	4.17	3.88	3.86	3.93	3.92
Earnings *(l-3)*	39,800	52,200	38,700	38,700	42,400	41,700
Household earnings *(l-3)*	67,200	89,900	65,300	65,300	72,700	72,500
No previous ROE observed	8%	18%	7%	8%	9%	9%
Years since last ROE (if observed)	4.89	7.41	4.63	4.66	5.75	5.73
No previous layoff observed	36%	61%	36%	36%	43%	43%
Years since last layoff (if observed)	6.63	10.54	6.28	6.32	8.48	8.56
Family Type, (c-3)						
Together	81%	87%	80%	80%	81%	81%
Single father	2%	1%	2%	2%	1%	1%
Single mother	12%	8%	11%	11%	12%	12%
Separated	6%	4%	7%	7%	5%	6%
Home Language						
English	74%	80%	74%	74%	73%	72%
Chinese	8%	6%	8%	8%	9%	9%
Punjabi	11%	8%	10%	11%	12%	12%
French	1%	1%	1%	<1%	1%	1%
Other	7%	5%	7%	7%	6%	6%
Observations	39,800	2,257,800	19,300	19,300	14,700	14,700

Notes: The table presents means and counts before and after the matching procedure for the grade 4 sample. "Matched - Pre" refers to the sample consisting of layoffs occuring in the two years prior to the test and their matched control group. "Matched - Post" refers to the sample consisting of layoffs occuring in the two years after the test and their matched control group. All counts and earnings are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data.

Most notably, as can be seen in the first two columns, laid-off workers have lower earnings and have experienced a layoff more recently compared to the pool of potential control group. Workers with a non-English home language are over-represented in the laid-off group compared to the rest of the population. Appendix Tables A1 and A2 provide summary statistics before and after matching for children writing the FSA in grade 7 and high school exams in grade 10, respectively.

4 Empirical Strategy

4.1 Impact of Layoffs on Household Earnings

To study the impact of layoffs on household earnings, I estimate an event study following Illing et al. (2021):

$$y_{itl} = \sum_{\substack{k \in (-5,10), \\ k \neq -3}} \gamma_k \mathbb{1}(t = l + k) \; + \sum_{\substack{k \in (-5,10), \\ k \neq -3}} \delta_k \mathbb{1}(t = l + k) \; \times \; Layoff_i \; + \; \alpha_i \; + \; \pi_t \; + \; \epsilon_{itl} \tag{1}$$

where y_{itl} denotes the earnings for worker i, in year t, who is laid off in year l. $Layoff_i$ is a dummy variable equal to 1 for workers that are in the layoff sample and 0 for workers in the matched control group. The specification accounts for worker fixed effects (α_i) and a full set of year fixed effects (π_t). The coefficients of interest are δ_k which measure the change in earnings of displaced workers relative to the path of earnings for non-displaced workers. To avoid perfect collinearity, I omit $k = -3$ - the year 3 years prior to the layoff/match year - from the regression. Standard errors are clustered at the census subdivision level.[9]

4.2 Impact of Layoffs on Children's Test Scores

4.2.1 Cross-sectional Comparisons Are Biased

As a first step, I compare the test scores of children in the layoff sample against their matched control group, in both the pre-test and the post-test samples. To do so, I estimate the following regression:

$$y_{ia} = \theta Layoff_i + \alpha_a + \epsilon_{ia} \tag{2}$$

where y_{ia} denotes average standardized test score for child i in academic year a. Term αa reprsents academic year fixed effects, and ϵ_{ia} is the error term. $Layoff_i$ is dummy variable equal to 1 for children whose parents were laid off. The coefficient of the $Layoff_i$ variable, θ, estimates the difference in test scores between children whose parents are laid off and their matched control

[9] The matching procedure ensures that income for all workers in the layoff sample and the matched control group is observed in the year of the layoff l, and the three previous years. For all other years, I only include matched pairs where I can observe income of both the laid-off worker and the matching comparison worker.

Table 2: Cross-Sectional Comparison of Average Test Scores

	Parent Laid Off Before Test			(Placebo) Parent Laid off After Test		
	Grade 4	Grade 7	Grade 10	Grade 4	Grade 7	Grade 10
Layoff	-0.073***	-0.075***	-0.056***	-0.073***	-0.060***	-0.070***
	(0.013)	(0.014)	(0.014)	(0.011)	(0.017)	(0.021)
Observations	28,900	22,900	18,000	22,400	18,500	11,000
Academic year FE	X	X	X	X	X	X
Mean dep. var. (control)	0.086	0.110	0.009	0.114	0.117	0.030

Notes: The table presents results from estimating equation (2) comparing test scores of children in the layoff sample against their matched control group. Columns 1-3 use the pre-test sample for each grade consisting of children whose parents suffered a layoff in the two years prior to the test. Columns 4-6 use the post-test sample for each grade consisting of children whose parents suffered a layoff in the two years after the test. All counts are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data. Standard errors, reported in parantheses, are clustered at the level of census subdivision three years prior to the layoff.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

group. I cluster standard errors at the level of census subdivision in which the parent lived three years prior to the layoff.

Table 2 shows estimates from a OLS estimation of equation (2)for the pre- and post-test samples for each grade. Columns 1-3 show that children whose parents are laid off before the test score about 6 - 7% of the standard deviation lower than children in the matched control group. However, as columns 4-6 show, children whose parents were laid off after the test similarly scored lower than their matched counterparts. For these children, the test score is observed prior to the layoff, and hence the estimates in columns 4-6 are differences in pre-treatment outcomes, which is evidence of selection on unobservable characteristics that remains after the matching procedure has successfully matched children of displaced parents to children of non-displaced workers using several demographic and socioeconomic variables.

The negative selection observed in the post-assessment periods echoes the findings of Hilger (2016), who uses a very similar research design to study the impact of parental job loss in the years prior to age 18 on college enrollment at age 18. Hilger (2016) finds that children of parents who were laid off when the child is older than 18 had lower college enrollment rates at age 18 than a matched comparison group of children whose parents were not laid off.

4.2.2 Difference-in-differences Estimator

I follow Hilger (2016) in estimating the treatment effect of parental job loss on children's test scores using a difference-in-differences design:

$$y_{ia} = \gamma Layoff_i \ + \ \eta Pre_i \ + \ \delta Layoff_i \ \times \ Pre_i \ + \ \alpha_a \ + \ \epsilon_{ia} \tag{3}$$

where y_{ia} represents the test score for child i in school year a. $Layoff_i$, as before, is dummy variable equal to 1 for children whose parents were displaced. Pre_i is a dummy variable equal to 1 if the child belongs to the layoff sample or the matched comparison group in the pre-test period. The regression also includes fixed effects for school year (α_a) to compare children within that write the test in the same academic year. The parameter γ, the coefficient on the term $Layoff_i$, estimates the selection effect that remains after the matching procedure. This is the difference in average test scores of children in layoff sample and their matched comparison group in the post-assessment period. The parameter δ, the coefficient on the interaction term $Layoff_i \ \times \ Pre_i$, estimates the DiD treatment effect on children's performance on the assessment of a parental job loss in the two-year window prior to the assessment. I cluster standard errors at the census subdivision level.

The difference-in-differences estimator above is valid if selection in the post-test sample is representative of selection in the pre-test sample. Of course, there is no way to test this since selection in the pre-test period is unobserved. The cross-sectional difference between test scores of children in the pre-test layoff sample and their matched contro group is a combination of treatment effect and selection bias. Below, I first verify that observable characteristics are balanced across the pre- and post-test samples, and that selection on unobservable characteristics is constant across groups laid off (and matched) at difference points in time.

4.2.3 DiD Diagnostics 1/2: Observable Characteristics are Balanced

For the difference-in-differences discussed above, I need any cross-sectional differences in observable characteristics between children in the layoff sample and their matched control group to be consttant between the pre- and post-test sample. Table 1 and Appenxix Tables A1 and A2 show that the propensity score matching procedure was successful in matching the observable characteristics of households very closely. Here, I empirically verify that the cross-sectional diffferences in the two periods are constant. To test this, I estimate equation 3 for each observable characteristic and verify that δ, the difference-in-difference estimate is indistinguishable from zero.

Table 3 provides results for these empirical tests for each grade, reporting the coefficient estimate, δ, and the corresponding standard error.

As mentioned earlier, there is a high and increasing rate of non-participation on the FSA in both grades 4 and 7. The BC K-12 dataset includes an indicator if the student was offered the test and did not participate. For my main results in the paper, I restrict to students that attempted all three

Table 3: Observable Characteristics are Balanced (Foundation Skills Assessment in Grade 4)

	Grade 4		Grade 7		Grade 10	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Male parent	0.000	0.004	0.000	0.005	0.000	0.011
Parent's age	0.120	0.097	0.034	0.071	-0.034	0.071
Male child	0.000	0.004	0.000	0.004	0.000	0.007
Family size *(l-3)*	0.014	0.011	-0.002	0.013	0.017	0.019
Earnings *(l-3)*	-700	440	-610	870	-140	690
Household earnings *(l-3)*	-250	670	-530	820	-160	1,100
No previous ROE observed	-0.001	0.004	-0.006	0.005	0.001	0.006
Years since last ROE (if observed)	-0.053	0.042	-0.031	0.065	0.011	0.135
No previous layoff observed	-0.001	0.008	-0.001	0.007	0.002	0.014
Years since last layoff (if observed)	0.029	0.069	0.005	0.117	0.061	0.149
Family Type, (c-3)						
Together	0.005	0.006	0.000	0.007	-0.003	0.010
Single father	-0.001	0.002	-0.001	0.002	0.000	0.003
Single mother	-0.004	0.004	-0.003	0.006	0.004	0.007
Separated	0.000	0.004	0.003	0.004	0.000	0.005
Home Language						
English	-0.005	0.006	-0.010	0.007	-0.008	0.008
Chinese	-0.001	0.002	0.005	0.004	-0.001	0.005
French	0.001	0.001	0.000	0.001	0.000	0.002
Punjabi	0.004	0.005	0.006**	0.003	0.006	0.005
Other	0.000	0.004	0.000	0.004	0.003	0.004

Notes: The table presents results from estimating equation 3 for each characteristic of households in the final samples for each grade. Column "Coeff." reports the point estimates for the coefficient δ, which is the difference in difference of the characteristics between the layoff and matched control group in the pre- and post-test samples. Standard errors, reported in columns labelled "SE", are clustered at the level of census subdivision three years prior to the layoff. Earnings differences and standard errors are rounded to the nearest 10, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

assessments. Appendix Table A3 reports estimates for equation 3 with non-participation indicator variable as the outcome variable. The coefficient of the interaction term shows insignificant treatment effects on non-participation in both grades. Hence, parental layoff in the two years prior to the test does not increase non-participation.

4.2.4 DiD Diagnostics 2/2: Selection on Unobservable Characteristics is Constant (Parallel Trends)

The most important assumption that my research design relies on is that selection on unobservable characteristics is constant across periods. As discussed earlier, the difference in test scores of children with parents in the layoff sample in the post-test period and their matched control group allows me to assess the extent of the selection effect that remains after the matching procedure. This observed selection in the post-test sample is a valid estimate of selection in the pre-test period (unobserved) if the selection that remains after the matching procedure is constant across groups laid off at different points in time.

To assess whether the selection remains constant for groups laid off at different points in time, I divide the two-year pre- and post-test periods into four six-month periods each. For each of the four subperiods in the post-test period, I can then compare the test scores of children in the layoff sample for that subperiod and their matched control group.

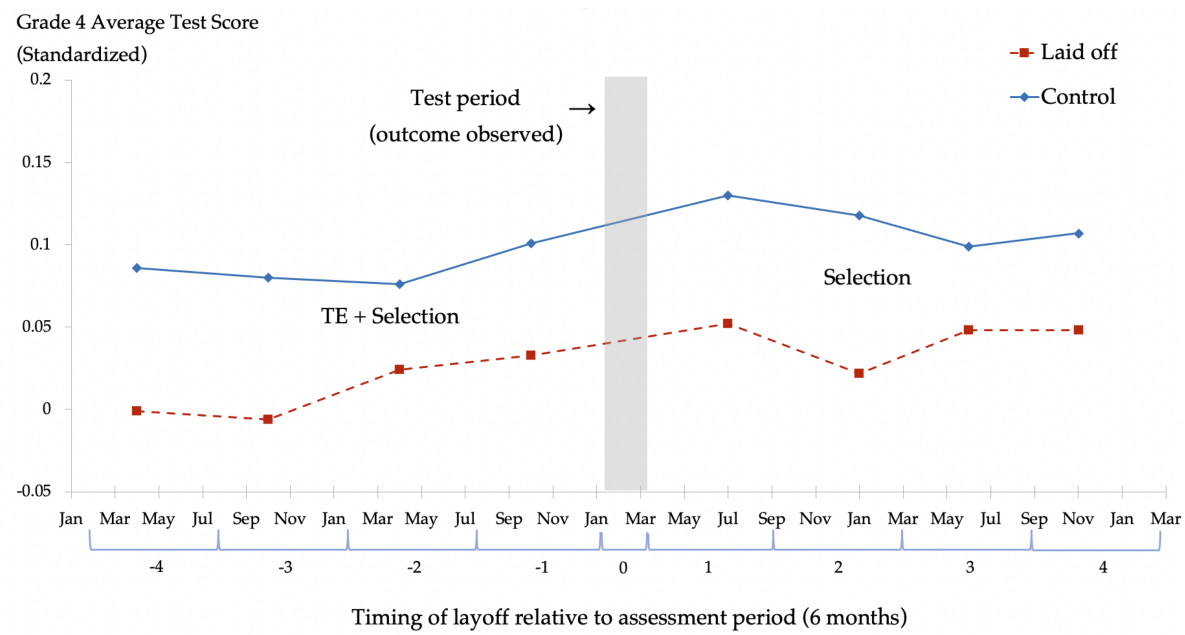
The test is akin to a parallel trends test in a traditional event study design with two important differences. First, in a traditional event study design, the timing of treatment is fixed, and selection is measured by comparing the outcomes of the same treatment and control groups at different points in time. In the difference-in-differnce design below, the timing of the outcome is fixed and the variation in the timing of the treatment is exploited as the second source of variation (in addition to the matched control group). Selection is measured by comparing outcomes of repeated cross-sections of treatment and control groups that are laid off or matched at different points in time. Second, in the traditional event study design, the parallel trends assumption is assessed by considering differences between treated and control units prior to the treatment, i.e. the *pre*-treatment period. In the difference-in-difference design used here, the parallel trends are assessed by observing differences in the *post*-test period.

Figure 4a plots the raw means of standardized test scores for children in grade 4 whose parents are laid off at different points in time and their matched control group. Note that the variation being plotted in Figure 4a is the variation in the timing of the layoff around the test period. All outcomes are observed at time 0, which is the test period. The difference in the test scores of children whose parents are laid off after the test and their matched control group is an estimate of selection, while the cross-sectional difference in the pre-test sample is a combination of treatment effect and selection. Note that these difference in test scores in Figure 4a between the layoff and the matched control group are visual represenations of the difference shown in Table 2 (columns 1 and 4). Figure 4a shows that this selection on unobservable characteristics remains fairly constant across subsamples laid off at different points in time.

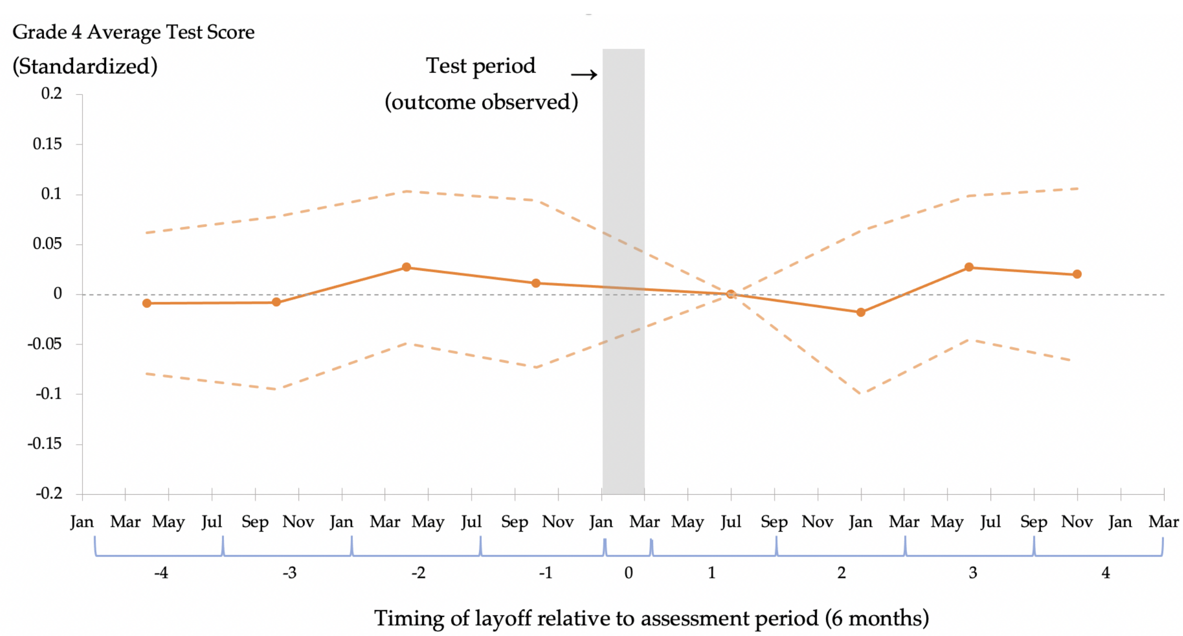
I next retrieve a series of difference-in-difference estimates for each subperiod relative to subperiod

Figure 4: Selection on Unobservable Characteristics is Constant Over Time (Foundation Skills Assessment in Grade 4)

(a) Raw Means



(b) DiD Estimates



Notes: Figure (a) plots the average test scores in grade 4 of children in the layoff and matched control groups by the timing of the layoff (6-month periods) relative to the test period. All test scores are observed in the test period (0 in the figure), and the variation on the x-axis is the timing of the layoff. Figure (b) plots the difference in difference estimates (ϕ_k) for each period relative to period 1, estimated using equation (4). Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

1 using the following equation:

$$y_{it(a)} = \sum_{\substack{k \in (-4,4), \\ k \neq 1}} \omega_k \mathbb{1}[t = t(a) + k] \; + \sum_{\substack{k \in (-4,4), \\ k \neq 1}} \phi_k \mathbb{1}[t = t(a) + k] \; \times \; Layoff_i$$

$$+ \; \eta Layoff_i \; + \; \alpha_a \; + \; \epsilon_{it(a)} \qquad (4)$$

where $y_{it(a)}$ represents the test score child i, in test period $t(a)$ in academic year a. $Layoff_i$ is an indicator for children in the layoff samples. Period $t(a) + 1$, which is the first post-assessment subperiod ($k = 1$), is the omitted from the regression to derive the DiD estimates for each period relative to period $t(a) + 1$. $\epsilon_{it(a)}$ is the error term and α_a are fixed effects for academic year. Standard errors are clustered at census subdivision level.

The coefficient η measures the difference in the test scores for children in the layoff sample in subperiod $t(a) + 1$ compared to the matched control group in period $t(a) + 1$. The coefficients of interest, ϕ_k represent the difference-in-difference estimates for subperiods $t(a) - 4$ to $t(a) + 4$ relative to period $t(a) + 1$. Coefficients $\phi_k, k \in \{2, 3, 4\}$ allow me to assess if selection in the post-test subperiods remains constant, and $\phi_k, k \in \{-1, -2, -3, -4\}$ estimate the treatment effects for children whose parents are laid off at different points in time prior to the test.

Appendix Section B discusses the identifying assumptions for the treatment effects ϕ_k for $k \in \{-1, -2, -3, -4\}$, in a potential outcomes framework.

Figure 4b plots the difference-in-difference estimates, ϕ_k, from the Equation (4). Estaimtes for $\phi_k, k \in \{2, 3, 4\}$ confirm that selection on unobservable characteristics in post-test subperiods $t(a) + 2$, $t(a) + 3$, $t(a) + 4$ is statistically indistinguishable from selection in $t(a) + 1$.

Figures A1 and A2 plot graphs analogous to Figure 4 for the samples of children writing the FSA in grades 7 and high school exams in grade 10, respectively. For both samples, I also find that the selection on unobservable characteristics that remains after the matching procedure is constant across the four post-test subperiods.

4.3 A Note on Using Post-Assessment Layoff Sample Only

Another empirical strategy that can be used to estimate the treatment effect would involve comparing children whose parents were laid off before the test period with children whose parents were laid off after the test. Pan and Ost (2014) use a similar empirical strategy to compare college enrolment at age 18 of children whose parents were laid off when they were 15-17 years old against children whose parents were laid off at ages 21-23. This difference is, of course, part of the differences-in-difference strategy proposed above. This difference by itself could be biased because workers that get laid off at one point in time could be very different from other workers that get laid off at different times. These differences could arise, for instance, due to variation in the macroeconomic conditions or seasonality in layoffs. The difference-in-difference design discussed in the previous section actually accounts for such seasonality as well due to the use of the matched control group.

Another way of thinking about the difference-in-difference strategy used here is to consider the difference between children of parents laid off before the test and children of parents laid off after the test as the primary difference. The goal of the second difference (against a matched control group) then is to remove any selection due to differences in macroeconomic conditions, or seasonality, etc. The matched control groups allow us because the control groups at different points in time also vary in the same way as the treated groups due to the macroeconomic conditions or seasonality.

5 Main Results

5.1 Impact of Layoffs on Household Earnings

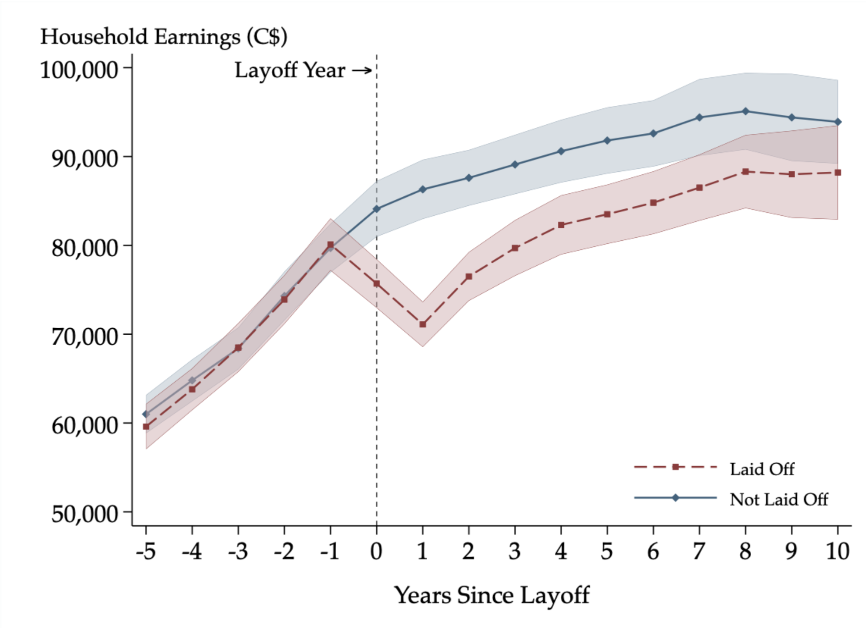
Figure 5a plots the raw means of the household income for workers in the layoff and matched control groups for grade 4. It is clear that households in the two groups have very similar income levels in the years prior to the layoff, and following the layoff, households of laid off workers experience large and persistent drops in earnings. Figure 5b plots the event study coefficients from equation 1.

Figure 5 shows that the baseline household income in the year just prior to the layoff is approximately $80,000. Households, on average, earn about $9,000 less than their counterfactual earnings in the year of the layoff, and about $15,000 less in the first year after layoff. Earnings then recover partially, but even after 10 years post-layoff, households earn about $7,000 less than they would have had they not suffered a job loss. Since the progressive tax system provides some insurance against such income losses, the after-tax income losses are smaller at approimately $3,000 in the year of the layoff and approximately $10,000 in the year after the layoff.

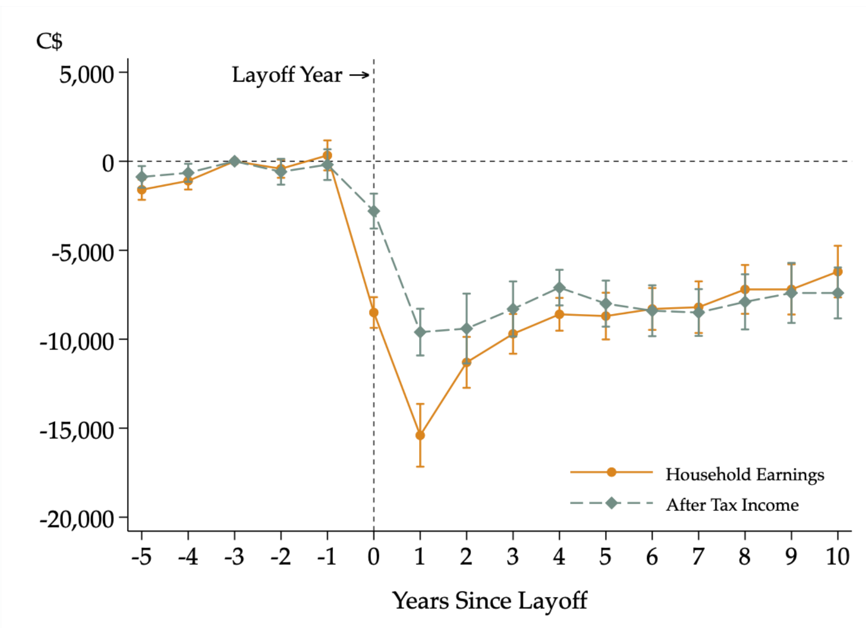
This pattern of significant and sustained income loss after a layoff is in line with other research on worker displacement. The most comparable estimates can be found in Stepner (2019), who also uses the ROE slips to derive a measure of layoffs. Stepner (2019) reports very similar income

Figure 5: Persistent Declines in Earnings after a Layoff (Parents of Children Writing FSA in Grade 4)

(a) Household Earnings



(b) Event Study Estimates



Notes: The figure shows household earnings before and after layoff for the final matched sample for grade 4. Figure (a) plots the average household earnings for the layoff group and the matched control group before and after the layoff (normalized to 0). Figure (b) plots the event study estimates (δ_k) for each period relative to year -3, estimated using equation (1). Event study estimates are reported for household earnings, as well as household after-tax income. These estimates measure the change in earnings of displaced workers relative to the path of earnings for non-displaced workers. Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

losses.[10]

Appendix Figures A3 and A4 report income losses for households in the sample of children writing the FSA in grade 7 and high school exams in grade 10, respectively. Parents in these samples are older and have higher baseline incomes but suffer similar persistent income losses after the layoff.

5.2 Impact of Layoffs on Children's Test Scores

Table 4 reports the 2x2 DiD estimates from equation 3 for standardized average scores on the FSA in grades 4, 7, and 10. Recall that coefficient of the indicator variable *Layoff* measures the selection on unobservable characteristics: difference in test scores of children in post-test layoff sample and their matched control group. The coefficients of the interaction term *Layoff x Pre* represent the treatment effect of parental job loss in the two years prior to the layoff.

For grade 4, the estimated treatment effect is less than 0.001 standard deviations. The precision of the estimate rules out negative treatment effects larger than 3.5% of the standard deviation at the 95% confidence level. I estimate a small negative treatment effect of approximately 1.5% of the standard deviation for children in grade 7, and I can rule out negative treatment effects larger than 5.3% of the standard deviation at the 95% confidence level. To my knowledge, Ruiz-Valenzuela (2020) is the only other study focusing on children's test scores before high school. The author pools students of different ages pools students of different ages but the average age of children in her sample is the age of a grade 4 student. Ruiz-Valenzuela (2020) finds that paternal job loss decreases children's test scores by 15% of a standard deviation on average. The large treatment effect is likely driven by selection bias; for instance, children in the control group in the study are much more likely to be from higer-income families than children in the treated group. My estimates for grades 4 and 7 rule out such large treatment effects for young children in British Columbia.

I find an overall positive but statistically insignificant treatment effect for children in grade 10. In my full sample, I find that children whose parents were laid off in the two years prior to grade 10 scored 1.5% higher than the counterfactual scenario had their parent not suffered a layoff. My overall treatment effect rules out negative treatment larger than 3.4% of a standard deviation in test scores in grade 10.

[10] There is a notable difference between the baseline incomes in this paper and Stepner (2019), in which, the average income of the laid-off workers in the year prior to the layoff was about $45,000. However, note that Stepner's sample includes workers across the country, aged 25-54 years old, with and without children. In my main sample, I only consider workers who experienced a layoff in a four-year window around the time that their child is in grade 4. Hence, the typical worker in my sample has at least one child, and is typically around 40-42 years old.

Table 4: Short-Term Treatment Effect of Parental Job Loss on Standardized Average Test Scores

	Grade 4	Grade 7	Grade 10
Pre	-0.031***	-0.010	-0.019
	(0.012)	(0.013)	(0.017)
Layoff	-0.073***	-0.060***	-0.070***
	(0.011)	(0.017)	(0.021)
Layoff x Pre	0.000	-0.015	0.015
	(0.018)	(0.019)	(0.025)
Observations	51,300	41,400	29,000
Academic year FE	X	X	X
Mean dep. var. (post/control)	0.114	0.117	0.030

Notes: The table presents results from estimating equation 3 for test scores in grades 4, 7, and 10. All counts are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data. Standard errors, reported in parantheses, are clustered at the level of census subdivision three years prior to the layoff.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

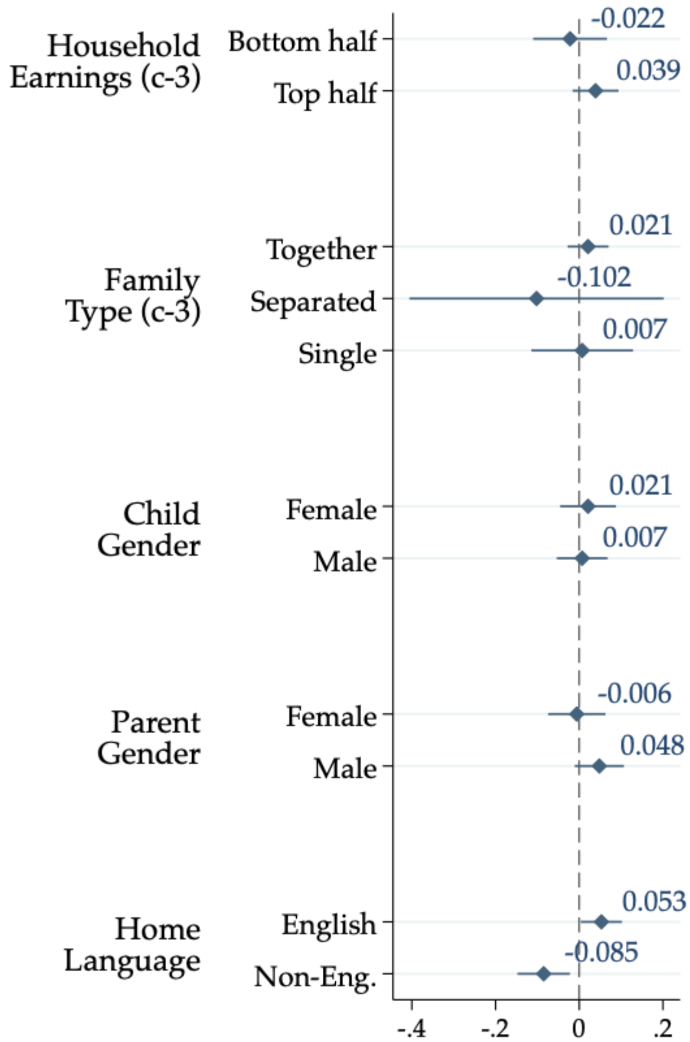
5.3 Heterogeneity

Figure 6 reports treatment effects estimated using equation 3 for different subsamples from grade 10 based on demographic characteristics of households or children that suffer a parental job loss. The negligible treatment effect for grade 10 masks some important heterogeneity by demographic characteristics. I find that children from households where English is the primary language spoken at home have a large *positive* treatment effect. These children have average test scores that are 5.3% of a standard deviation (p-value = 0.034) higher than the counterfactual case had their parent not been laid off. On the other hand, children from hosueholds where a non-English language (Chinese, Punjabi, French, or other) is the primary language spoken at home have a large *negative* treatment effect of 8.5% of a standard deviation (p-value < 0.01).

The estimates for subsamples based on household income are statistically insignificant, but I find that children from households in the top half of the income distribution score 3.9% of a standard deviation *higher* on average compared to the counterfactual scenario had their parent not been laid off. This is likely due to higher offsetting time investments in the higher-income households. Guryan et al. (2008) find that higher-earning parents spend more time on child care than their lower-income counterparts.[11] Thus, it is possible that following the layoff, parents in the top half of the income distribution increase time investments much more significantly than parents in the lower half of the distribution. Moreover, parents in higher-income households are likely to be more educated (not observed in the data), making any additional time investments following the

[11] The published version of Guryan et al. (2008) only report results for time spend on child care by parent's education level. The working paper version available on NBER, which the authors themselves cite in the published version, contains results on time spent on child care based on income quintiles.

Figure 6: Heterogeneity in Treatment Effects by Demographic Characteristics (Grade 10 Average Test Score)



Notes: The figure reports treatment effect estimates (δ) from equation 3 for different subsamples from grade 10 based on demographic characteristics of households or children that suffer a parental job loss. The estimation accounts for academic year fixed effects. The horizontal bars represent 95% confidence intervals. Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

layoff much more productive.

My treatment effects for grade 10 subsample with *paternal* job loss rules out treatment effects observed by Rege et al. (2011) in the Norweigian context. The authors find that paternal job loss due to a mass layoff in the two years prior to grade 10 reduces children's grade 10 GPA by 6% of a standard deviation on average in Norway. In contrast, I find that paternal job loss in the two years prior to grade 10 in British Columbia has a positive treatment effect on children of 4.8% of a standard deviation. While this treatment effect is statistically insignificant at all conventional levels of significance, the 95% confidence interval rules out negative treatment effects larger than 1.1% of a standard deviation. Rege et al. (2011) use a cross-sectional comparison between children whose parents are laid off and children in a control group using a saturated regression that controlled for household and firm characteristics. As I showed earlier, such cross-sectional comparisons are susceptible to selection bias due to unobserved heterogeneity.

Mörk et al. (2020), on the other hand, again relying on cross-sectional comparisons, find that maternal job loss due to firm closure two years prior to grade 10 reduces children's grade 10 GPA by about 7% of a standard deviation. My point estimate for the treatment effect of maternal job loss is very close to zero (-0.6% of a standard deviation), with a standard error of 0.035. Hence, the 95% confidence interval for my estimate cannot rule out the treatment effect observed by Mörk et al. (2020) in Sweden, but their estimated effect is outside the 90% confidence interval of my estimate in the Candian context.

Figures A5a and A5b report treatment effects by demographic characteristics for grades 4 and 7. For these younger children, treatment effects show very little heterogeneity. I estimate a large positive treatment effect in grade 4 for children with single parents, though this estimate comes from a very small sample and is statistically insignificant.

Appendix Table A4 reports estimates for each grade by individual subjects. There is little heterogeneity in treatment effects for grade 4. For all subjects - numeracy, reading, and writing - point estimates are small and statistically insignificant. For grade 7, I find that the negative treatment effect for average test score (Table 4) is driven by a large negative treatment effect on the writing component of the test, while the treatment effects for the numeracy and reading are close to zero and statistically indistinguishable from zero. For grade 10, once again, treatment effects for all subjects are small and statistically insignificant, though it appears that the positive treatment effect for the average test score (Table 4) is driven by small gains in science and English, while the point estimate for math is very close to zero.

6 Discussion

6.1 Negligible Treatment Effects

As seen in Figure 5, households where a parent is laid off are losing about $\$10,000 - \$12,000$ in after-tax income within the two years after the layoff. Figure 2 shows that an increase of $\$10,000$ in average household after-tax income in the two years prior to the test is associated with about 5% of a standard deviation in gains in test scores. However, the treatment effects estimated in the previous section are much smaller and the confidence interval rules out treatment effects of 5% of a standard deviation.

There are two plausible explanations for such negligible effects. First, it's possible that parental time investment offsets any negative effects due to income loss. This is in line with Agostinelli and Sorrenti (2021), who find a strong trade-off between income and parental time investments in the context of the EITC expansion in the US. Agostinelli and Sorrenti (2021) find that the additional financial resources due to the EITC expansion increased children's test scores. On the other, the increase in maternal labour supply due to the EITC expansion led to a drop in children's test scores. Their reduced-form event study estimates show that children's test scores were unaffected by the EITC expansion in the short run.

The second plausible explanation for the negligible effects is that borrowing constraints are not particularly important in the short run. This is consistent with other work that finds that borrowing constraints are not binding (e.g Carneiro and Heckman, 2002; Cameron and Taber, 2004). This, of course, does not imply that borrowing constraints are irrelavant. As Caucutt and Lochner (2020) find in their calibration of a dynastic model of early and late human captial investments in children, that while increasing borrowing limits at any point in time has only modest effects on parent's investment in children, eliminating life-cycle borrowing constraints can drastically increase investments. Hence, even though I find that children's human capital accumulation is unaffected by parental job loss in the short run, the persistent loss in household financial resources may lead to detrimental effects in the long run. Cameron and Heckman (2001) also argue that parental background and family environment are much more important than credit constraints in explaining educational achievement gaps.

Findings from the heterogeneity analysis also confirm that borrowing constraints are not playing a significant role. If borrowing constraints were binding, we would expect a large negative treatment effect on children from lower-income households. For all three grades, I find statistically insignificant small negative treatment effects (1.6-2.2% of a standard deviation) for children from households in the lower half of the income distribution.

My findings suggest that the large detrimental effects on children's income in adulthood noted in earlier studies (Oreopoulos et al., 2008; Uguccioni, 2021) are not driven by short-term distruptions to children's human capital accumulation. The combination of unemployment insurance, past savings, and increase in parental time investments likely provides enough protection for children in the short run. However, the sustained income losses shown in Figure 5 likely lead to lower educational attainment in the long run. Thus, future policy changes should aim to provide long-term insurance and support to these households to protect against long-term income losses.

6.2 Negative Selection

The negative selection (Table 2) observed in the post-assessment periods echoes the findings of Hilger (2016), who uses a very similar research design to study the impact of parental job loss in the years prior to age 18 on college enrollment at age 18. Hilger finds that children of parents who were laid off when the child is older than 18 had lower college enrollment rates at age 18 than a matched comparison group of children whose parents were not laid off.

Hilger (2016) showed that empirical strategies relying on mass layoffs are in particular susceptible to bias. This is also in line with summary statistics reported by Mörk et al. (2020) (Table 1, page 5): children of coworkers in closing firms had lower test scores than children of coworkers in surviving firms two years before the closure. Seim (2019) comes to a similar conclusion when studying layoffs in Sweden. Seim links the longitudinal employer-employee matched dataset with military enlistment records. Sweden had mandatory conscription until 2010, and all men around age 18 were tested for their cognitive and non-cognitive abilities. Using this linked dataset, Seim finds that workers with low cognitive and non-cognitive skills are significantly more likely to be displaced compared to high-skilled workers. These findings should not be surprising given the evidence of assortative matching between workers and firms (Abowd et al., 1999; Card et al., 2013). If high ability workers sort themselves into highly productive firms, layoffs at unproductive firms are more likely to affect in lower ability workers.

It should be noted that after their matching procedure, Mörk et al. (2020) do not find a statistically significant selection effect for parents at firms that closed after their children's grade 10 exams. This might be due to two important variables that the authors have access to in their datasets and that they use for their matching strategy, which I do not have access to in my datasets: firm identifiers and parent's education level. The firm identifiers allow Mörk et al. (2020) allow the authors to match the laid off parent on the average GPA of their coworkers' children two years prior to the mass layoff, a proxy for children's own academic ability. To the extent that differences in parental education can lead to differences in parental investments in children, controlling for parental educational attainment may significantly reduce the selection observed as well.

In Table 5, I extend my baseline models for grades 7 and 10 by controlling for children's test scores in the past. For grade 7, I control for children's standardized test scores in each of the three subjects - numeracy, reading, writing - on the FSA in grade 4. For grade 10, I control for children's standardized test scores in the three subjects on the FSA in grade 7. As noted earlier, some children miss the test, which explains the drop in the number of observations in the extended model.

First, I find that the treatment effects, coefficients on the interaction term $Layoff x Pre$, change very little when controlling for past test scores. This serves as a robustness check for my methodology for controlling for selection on unobservable characteristics.

Table 5: Grade 7 and 10 Results Controlling for Past Test Scores

	Grade 7		Grade 10	
	Baseline	Extended model	Baseline	Extended model
Pre	-0.010	0.002	-0.019	-0.008
	(0.013)	(0.011)	(0.017)	(0.015)
Layoff	-0.060***	-0.028**	-0.070***	-0.034*
	(0.017)	(0.011)	(0.021)	(0.018)
Layoff x Pre	-0.015	0.002	0.015	0.027
	(0.019)	(0.013)	(0.025)	(0.023)
Observations	41,400	38,100	29,000	25,800
Academic year FE	X	X	X	X
Past test scores		X		X
Mean. dep. var. (post/control)	0.117	0.139	0.030	0.048

Notes: The table presents results from estimating an extended version of equation 3 for test scores in grades 7, and 10. Columns 1 and 3 report the baseline results for ease of comparison. The extended models reported in columns 2 and 4 includes children's past test scores as controls. For grade 7, I control for children's standardized test scores in each of the three subjects - numeracy, reading, writing - on the FSA in grade 4. For grade 10, I control for children's standardized test scores in the three subjects on the FSA in grade 7. All counts are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data. Standard errors, reported in parantheses, are clustered at the level of census subdivision three years prior to the layoff.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

Second, recall that the coefficient on the indicator variable $Layoff$ is the measure of selection on unobservable characteristics. This is the difference in the test scores of children whose parents are laid off *after* their test and their matched control group. The coefficients on the indicator variable $Layoff$ is halved when past test scores are added to the model. However, there is still a statistically significant negative selection on unobservable characteristics even in the extended model. The past test scores that are added to the extended model were observed three years prior to the current test scores, which could mean that they are not perfectly representative of children's ability just prior to the layoff. This would be consistent with models of human capital accumulation with dynamic complementarities, for instance (e.g. Heckman and Navarro, 2007). It is also possible that parents in the layoff sample invest differently in their children than those in their matched control group, for e.g., due to differences in education level, which is not observed in my dataset. In combination with findings from Mörk et al. (2020), my results here suggest that parent's education level might be a key variable in explaining the remaining selection on unobservable characteristics.

These results suggest that the large, long-term treatment effects based on cross-sectional comparisons with a control group (e.g. Oreopoulos et al., 2008; Uguccioni, 2021) may in part be driven by selection on unobservable characteristics. Controlling household resources and other socioeconomic characteristics, as I do in my baseline sample, might not be enough to eliminate the selection bias, and controlling for past measures of ability is crucial in reducing this selection bias.

The assumption of no selection due to unobservable heterogeneity is untestable in studies considering the long run effects parental job loss on children because children's outcomes are only observed once and the difference-in-difference strategy used in this paper cannot be used to study long-term effects. Using my different-in-difference strategy for long-term treatment effects, say for income attainment 15 years after the layoff, would require a "never-treated" control group of children whose parents did not suffer a layoff for at least 15 years. This is infeasible because (a) such a group is likely to be very small, and (b) such a group will likely be very different from the treated children, make it hard to find suitable matches.

7 Robustness

In this section, I present several robustness checks, focusing on children in grade 4 for conciseness. All results are reported in Table 6. I report my baseline results in column 1 for ease of comparison.

In column 2, I add several demographic controls: fixed effects for home language, family size, family type, decile of parent's income three years prior to the layoff, decile of household income three years prior to the layoff, child's gender, and parent's gender. Given that the matching procedure was successful in matching the observable characteristics well (Table 1), adding the demographic controls does not change the results meaningfully.

In column 3, I re-estimate equation 3 including fixed effects for quarter of the year. One might be concerned that one of differences used in my difference-in-differences strategy relies on comparing children of workers laid off at different times. For instance, there might be seasonality in layoffs that changes the profile of workers that get laid off at different times during the year. As discussed in section 4.3 any seasonality in layoffs will not affect the difference-in-difference estimate because any trends in the estimates due to seasonality will be captured by trends in the matched control groups. Results in column 2 confirm this. Including fixed effects for the quarter of the year does not change the results at all.

In column 4, I restrict my sample to matched pairs with propensity score differences lower than the median propensity score difference. The goal is to assess whether the selection observed is due to large differences in propensity scores between the nearest neighbours. While this subsample does have a lower estimate for the selection effect, there is still a statistically significant negative selection into layoff. Once again, the treatment effect estimated is insignificant. Results suggest that the selection effect *can* be reduced by a more strict matching procedure. However, this comes at the expense of a smaller sample, reducing power and possibly the external validity of the results.

Table 6: Robustness Checks for Grade 4

	Baseline	Demographic Controls	Quarter FEs	P-score diff < median	CEM	6-year est. window
Pre	-0.031***	-0.017	-0.016	-0.027*	-0.003	-0.015*
	(0.012)	(0.011)	(0.011)	(0.014)	(0.024)	(0.009)
Layoff	-0.073***	-0.069***	-0.069***	-0.062***	-0.107***	-0.087***
	(0.011)	(0.009)	(0.009)	(0.013)	(0.028)	(0.012)
Layoff x Pre	0.000	0.001	0.001	0.015	0.004	-0.014
	(0.018)	(0.017)	(0.017)	(0.020)	(0.037)	(0.014)
Observations	51,300	51,300	51,300	26,500	9,900	70,300
School year FE	X	X	X	X	X	X
Demographic controls		X				
Quarter FEs			X			
Mean. dep. var. (post/control)	0.114	0.114	0.114	0.172	0.103	0.132

Notes: The table presents results from estimating equation 3 for grade 4. Column 1 reports the baseline results for ease of comparison. Column 2 reports an extended model which includes demographic controls. Column 3 includes quarter of the year fixed effects to account for any seasonality. Column 4 restricts the sample to matches where the p-score differences are less than the median. Column 5 uses an alternative sample created using coarsened exact matching. Column 6 uses an alternative sample created using the baseline propensity score matching but with a 6-year estimation window instead of the baseline 4-year window. All counts are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data. Standard errors, reported in parantheses, are clustered at the level of census subdivision three years prior to the layoff.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

In column 5, I report results using coarsened exact matching (CEM). I did not use CEM, which relies on matching exactly on coarsened variables, as my primary matching strategy because it is highly punitive on the sample size. CEM would have resulted in a much smaller sample being matched, which would have not only reduced the power of my estimates, but also would cast doubts on the external validity of the results. I use CEM here to match exactly on child's gender, parent's gender, assessment year, calendar year of the layoff, decile of earnings three years prior to layoff, decile of household earnings three years prior to layoff, years since last layoff, parent's age in three-year bins (25-27, 28-30, etc.). This matching criteria intentionally omits some variables that were used for propensity score matching to get a reasonably-sized sample. With CEM, I once again get parallel trends in the post-test period (Appendix Figure A6). The estimates in column 4 show that CEM results in a larger negative selection effect: children whose parents were laid off after the assessment scored approximately 10% of a standard deviation lower than their matched comparison group. This is possibly because the layoff and control groups were matched on fewer variables compared to baseline sample. The diff-in-diff estimate of the treatment effect using CEM is once again small and statistically insignificant.

Lastly, in column 6, I report results from an alternative sample using an extended estimation window. I re-matched children and parents using a six-year window instead of the four-year window that I used in my main results. The potential control group here is a subset of the potential control group in the baseline results; those that did not suffer a layoff for the entire six years instead of four years in the baseline. I use the same propensity score matching strategy discussed in section 4. Appendix Figure A7 shows that selection on unobservable characteristics is constant up to three years after the test. Column 6 of Table 6 reports the DiD estimate using this extended sample. The results look very similar to the main results, with a large negative selection into layoff, and a small and statistically insignificant treatment effect.

8 Conclusion

While some studies have documented large long-term effects of layoffs on children, we have little evidence on the short-term effects on children's human capital accumulation, particularly for younger children. Using administrative data from British Columbia, Canada, I study the short-term effect of parental layoff on children's academic performance in grades 4, 7, and 10. To account for selection into layoffs on unobservable characteristics, I use the difference-in-difference strategy developed by Hilger (2016), which exploits the timing of the parental layoff relative to children's test as well as a matched control group of children whose parents do not suffer a layoff.

I find that parental layoff within the two years prior to the test has no significant impact on children's academic performance on the test. I find that this is the case for children in grade 4, 7, and 10. This is in spite of a large shock to household resources. Compared to their counterfactual, households lose approximately $\$10,000 - \$13,000$ in after tax income in the two years after the layoff. These results suggest that the large long-term effects of parental layoffs on Canadian children's future income documented by Oreopoulos et al. (2008) and Uguccioni (2021) are not driven by short-run disruptions to children's human capital accumulation. The combination of unemployment insurance, past savings, and increase in parental time investments likely provides enough protection for children in the short run. However, the sustained income losses shown in Figure 5 likely lead to lower educational attainment in the long run. Thus, future policy changes should aim to provide insurance and support to these households to protect against long-term income losses.

The overall negligible treatment effect for grade 10 masks some important heterogeneity: children from lower-income households and children from non-English speaking households have *lower* test scores due to parental job loss, while children from higher-income households and from English-speaking households have *higher* test scores due to parental layoff. This is likely due to a combination of more binding borrowing constraints for lower-income households and higher offsetting time investments in the higher-income households (Guryan et al., 2008).

In line with Hilger (2016), I also find that cross-sectional comparisons of children whose parents are laid off and a control group found by matching on household resources and demographic characteristics are susceptible to bias. This highlights the need for the difference-in-difference strategy used in this paper. Children whose parents were laid off *after* the test scored 6 - 7% of a standard deviation lower than their matched control group. I also show that controlling for past test scores (in case of grades 7 and 10) can substantially reduce this selection bias. This finding suggests that the large treatment effects based on cross-sectional comparisons of children whose parents are laid off against a matched control group (e.g. Oreopoulos et al., 2008; Rege et al., 2011; Uguccioni, 2021) may in part be driven by selection bias due to unobserved heterogeneity.

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Appendix A Additional Tables and Figures

Table A1: Summary Statistics Before and After Matching (Grade 7)

	Unmatched		Matched - Pre		Matched - Post	
	Layoff	Control	Layoff	Control	Layoff	Control
Male parent	50%	49%	50%	50%	48%	48%
Parent's age	43.05	44.15	42.24	42.20	44.10	44.10
Male child	52%	51%	52%	52%	52%	52%
Family size *(l-3)*	4.13	4.21	3.92	3.91	3.92	3.92
Earnings *(l-3)*	41,000	53,500	39,700	39,800	43,600	43,100
Household earnings *(l-3)*	70,600	93,200	68,900	69,000	75,700	75,300
No previous ROE observed	8%	19%	8%	8%	9%	9%
Years since last ROE (if observed)	5.26	8.22	5.07	5.11	6.14	6.15
No previous layoff observed	35%	61%	35%	35%	42%	42%
Years since last layoff (if observed)	6.92	10.94	6.65	6.75	8.75	8.86
Family Type, (c-3)						
Together	81%	86%	80%	80%	80%	81%
Single father	2%	1%	2%	2%	2%	2%
Single mother	13%	10%	13%	13%	14%	13%
Separated	5%	3%	5%	5%	4%	4%
Home Language						
English	74%	79%	73%	73%	73%	72%
Chinese	8%	6%	9%	9%	9%	10%
Punjabi	11%	9%	11%	11%	12%	13%
French	1%	1%	1%	1%	1%	<1%
Other	6%	5%	7%	7%	6%	6%
Observations	34,100	1,927,800	16,100	16,100	12,600	12,600

Notes: The table presents means and counts before and after the matching procedure for the grade 7 sample. "Matched - Pre" refers to the sample consisting of layoffs occuring in the two years prior to the test and their matched control group. "Matched - Post" refers to the sample consisting of layoffs occuring in the two years after the test and their matched control group. All counts and earnings are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data.

Table A2: Summary Statistics Before and After Matching (Grade 10)

	Unmatched		Matched - Pre		Matched - Post	
	Layoff	Control	Layoff	Control	Layoff	Control
Male parent	48%	47%	49%	49%	43%	43%
Parent's age	45.26	45.91	44.50	44.52	46.75	46.75
Male child	51%	51%	52%	52%	50%	50%
Family size *(l-3)*	4.13	4.20	3.95	3.93	3.92	3.92
Earnings *(l-3)*	43,400	54,900	41,200	41,800	47,100	47,600
Household earnings *(l-3)*	76,800	97,100	73,300	73,700	84,300	84,500
No previous ROE observed	8%	20%	8%	8%	10%	10%
Years since last ROE (if observed)	5.57	8.89	5.42	5.49	6.75	6.83
No previous layoff observed	36%	62%	36%	36%	45%	46%
Years since last layoff (if observed)	7.08	11.25	6.70	6.78	9.91	10.05
Family Type, (c-3)						
Together	83%	87%	82%	82%	82%	82%
Single father	2%	1%	2%	2%	1%	1%
Single mother	12%	10%	12%	13%	13%	14%
Separated	3%	2%	4%	4%	3%	3%
Home Language						
English	74%	80%	73%	74%	72%	71%
Chinese	8%	6%	9%	9%	9%	9%
Punjabi	12%	8%	11%	11%	13%	14%
French	<1%	<1%	<1%	<1%	<1%	<1%
Other	6%	5%	7%	6%	6%	6%
Observations	17,900	1,276,600	9,000	9,000	5,500	5,500

Notes: The table presents means and counts before and after the matching procedure for the grade 10 sample. "Matched - Pre" refers to the sample consisting of layoffs occuring in the two years prior to the test and their matched control group. "Matched - Post" refers to the sample consisting of layoffs occuring in the two years after the test and their matched control group. All counts and earnings are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data.

Table A3: Short-term Treatment Effect on Rates of Non-participation on FSA in Grades 4 and 7

	Grade 4	Grade 7
Pre	-0.002	0.000
	(0.006)	(0.004)
Layoff	0.004	0.007*
	(0.005)	(0.004)
Layoff x Pre	0.004	0.001
	(0.006)	(0.005)
Observations	67,900	57,400
Academic year FE	X	X
Mean dep. var. (post/control)	0.126	0.141

Notes: The table presents results from estimating equation 3 for participation (1/0) on the FSA in grades 4 and 7. All counts are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data. Standard errors, reported in parantheses, are clustered at the level of census subdivision three years prior to the layoff.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

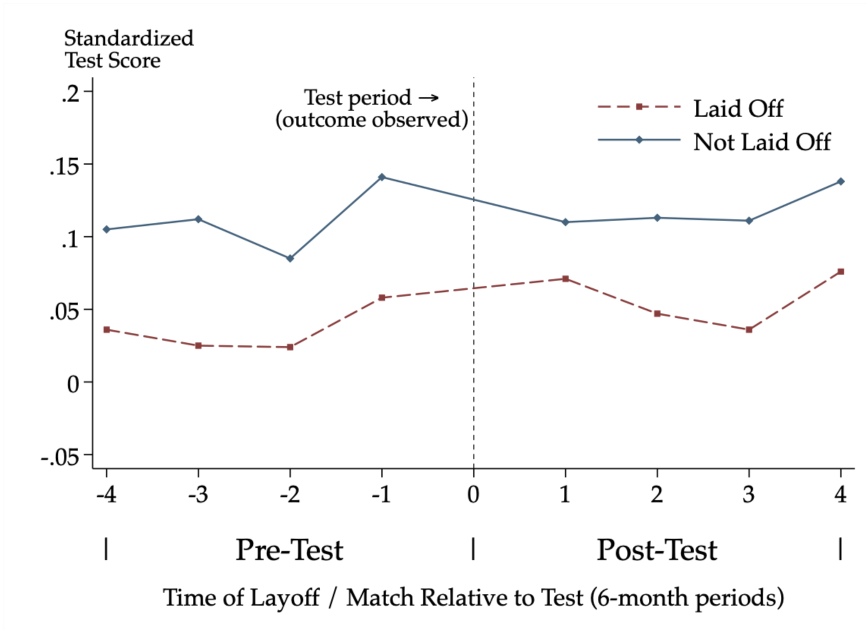
Table A4: Short-Term Treatment Effect of Parental Job Loss on Standardized Test Scores By Subject

	Grade 4			Grade 7			Grade 10		
	Numeracy	Reading	Writing	Numeracy	Reading	Writing	Science	Math	English
Pre	-0.013	-0.031***	-0.034***	-0.023	-0.013	0.013	-0.013	-0.012	-0.025
	(0.013)	(0.009)	(0.011)	(0.015)	(0.013)	(0.013)	(0.015)	(0.016)	(0.016)
Layoff	-0.047***	-0.075***	-0.056***	-0.063***	-0.047***	-0.034***	-0.063***	-0.054***	-0.064***
	(0.013)	(0.009)	(0.011)	(0.019)	(0.015)	(0.012)	(0.023)	(0.019)	(0.018)
Layoff x Pre	-0.016	0.011	0.006	0.001	-0.002	-0.040**	0.010	0.005	0.023
	(0.020)	(0.016)	(0.016)	(0.023)	(0.019)	(0.016)	(0.024)	(0.024)	(0.022)
Observations	51,300	51,300	51,300	41,400	41,400	41,400	29,000	29,000	29,000
Academic year FE	X	X	X	X	X	X	X	X	X
Mean. dep. var. (post/control)	0.072	0.092	0.124	0.092	0.086	0.111	0.065	0.032	0.074

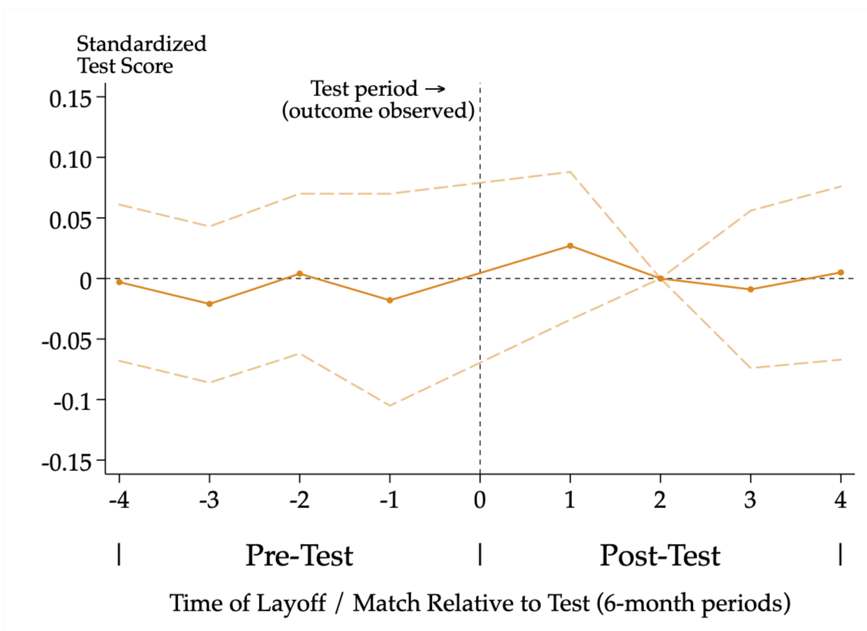
Notes: The table presents results from estimating equation 3 for test scores in individual subjects in grades 4, 7, and 10. All counts are rounded to the nearest 100, consistent with Statistics Canada's guidelines to protect the privacy of individuals in the data. Standard errors, reported in parantheses, are clustered at the level of census subdivision three years prior to the layoff.
***, **, * indicate significance at 1%, 5%, and 10% level, respectively

Figure A1: Selection on Unobservable Characteristics is Constant Over Time (Grade 7)

(a) Raw Means



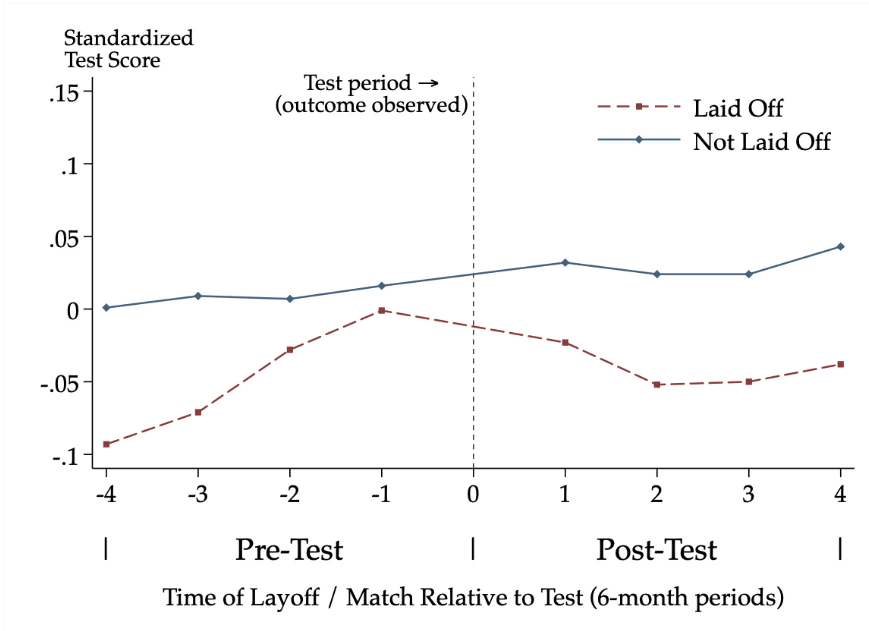
(b) DiD Estimates



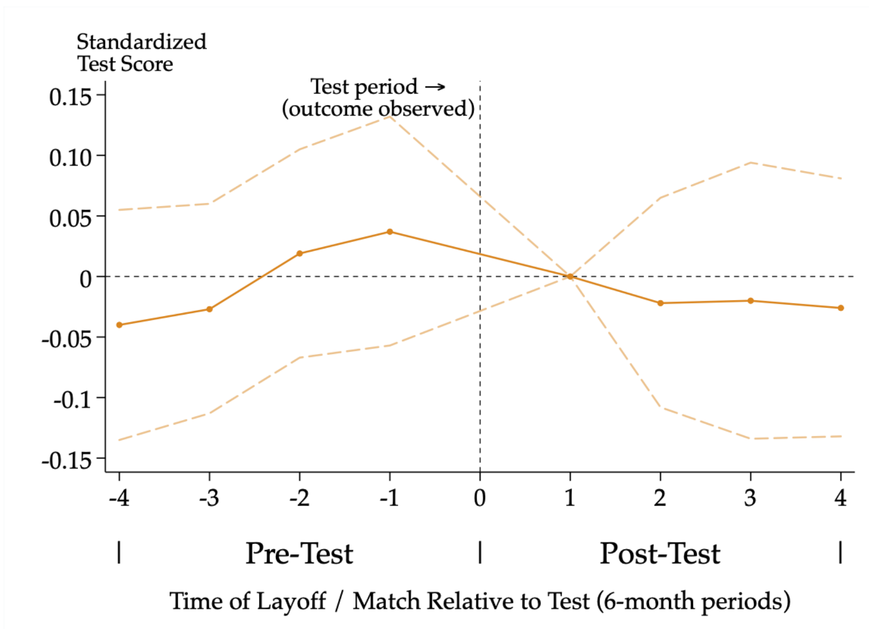
Notes: Figure (a) plots the average test scores in grade 7 of children in the layoff and matched control groups by the timing of the layoff (6-month periods) relative to the test period. All test scores are observed in the test period (0 in the figure), and the variation on the x-axis is the timing of the layoff. Figure (b) plots the difference in difference estimates (ϕ_k) for each period relative to period 1, estimated using equation (4). Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Figure A2: Selection on Unobservable Characteristics is Constant Over Time (Grade 10)

(a) Raw Means



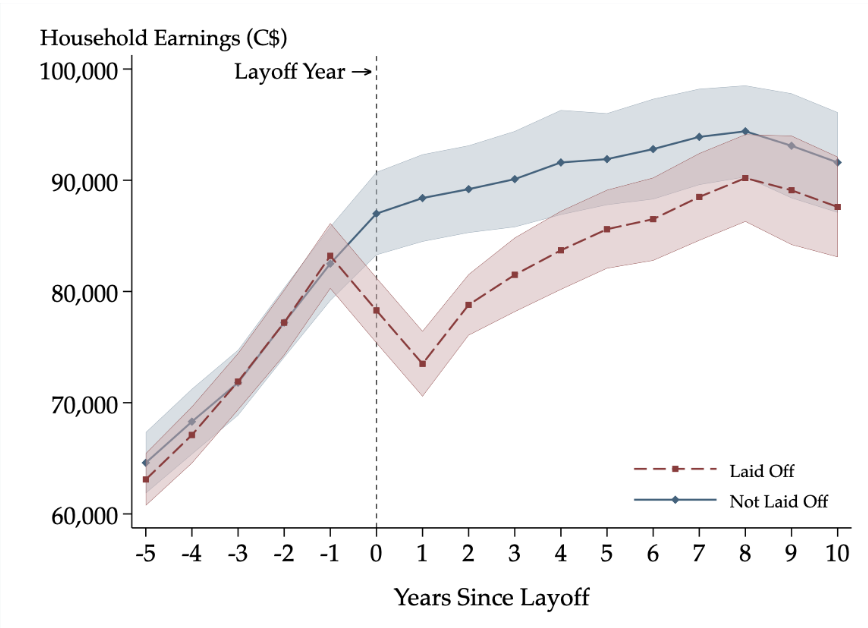
(b) DiD Estimates



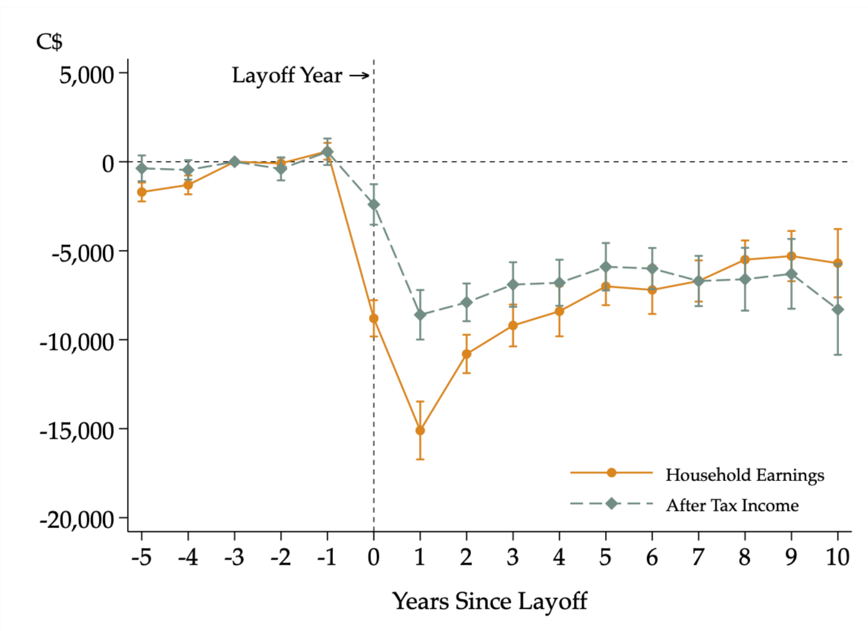
Notes: Figure (a) plots the average test scores in grade 10 of children in the layoff and matched control groups by the timing of the layoff (6-month periods) relative to the test period. All test scores are observed in the test period (0 in the figure), and the variation on the x-axis is the timing of the layoff. Figure (b) plots the difference in difference estimates (ϕ_k) for each period relative to period 1, estimated using equation (4). Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Figure A3: Persistent Declines in Earnings after a Layoff (Parents of Children in Grade 7)

(a) Household Earnings



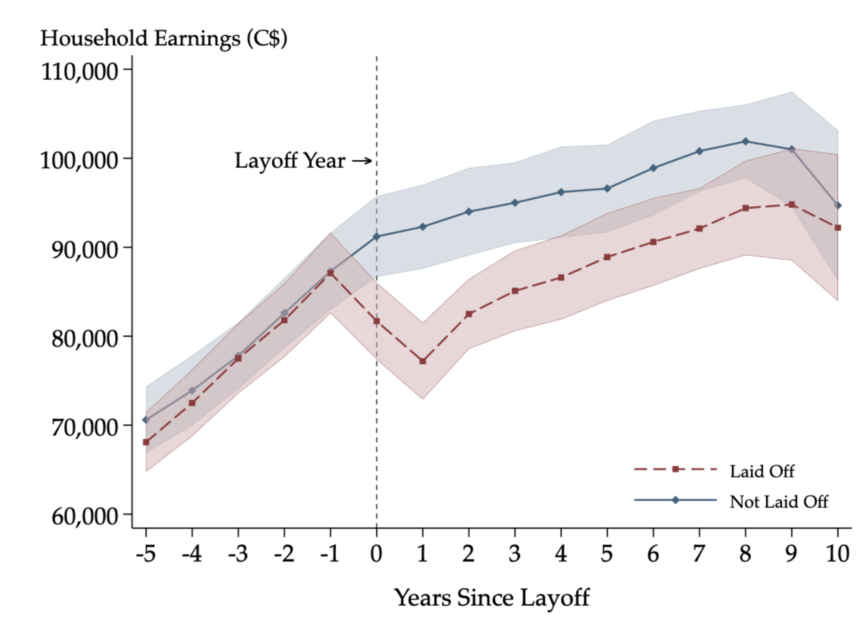
(b) Event Study Estimates



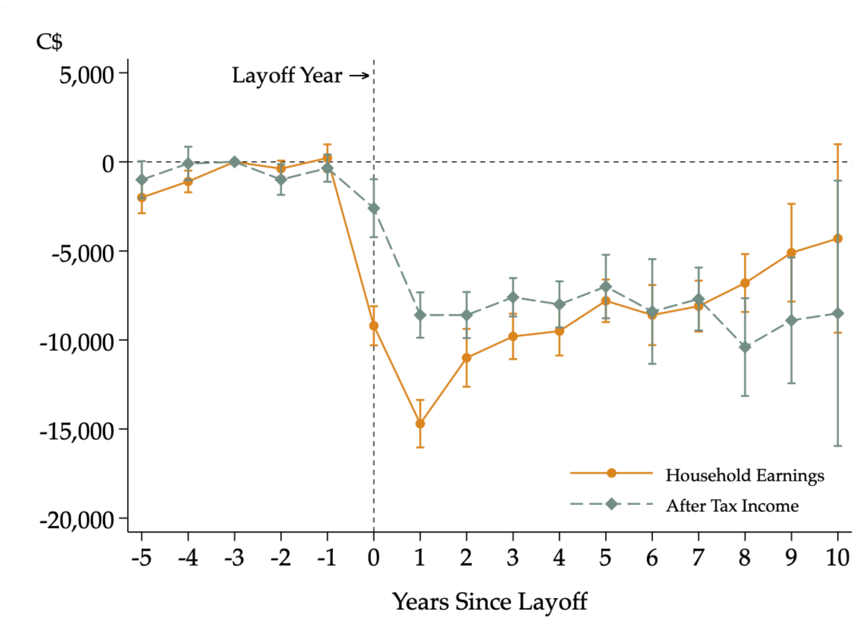
Notes: The figure shows household earnings before and after layoff for the final matched sample for grade 7. Figure (a) plots the average household earnings for the layoff group and the matched control group before and after the layoff (normalized to 0). Figure (b) plots the event study estimates (δ_k) for each period relative to year -3, estimated using equation (1). Event study estimates are reported for household earnings, as well as household after-tax income. These estimates measure the change in earnings of displaced workers relative to the path of earnings for non-displaced workers. Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Figure A4: Persistent Declines in Earnings after a Layoff (Parents of Children in Grade 10)

(a) Household Earnings



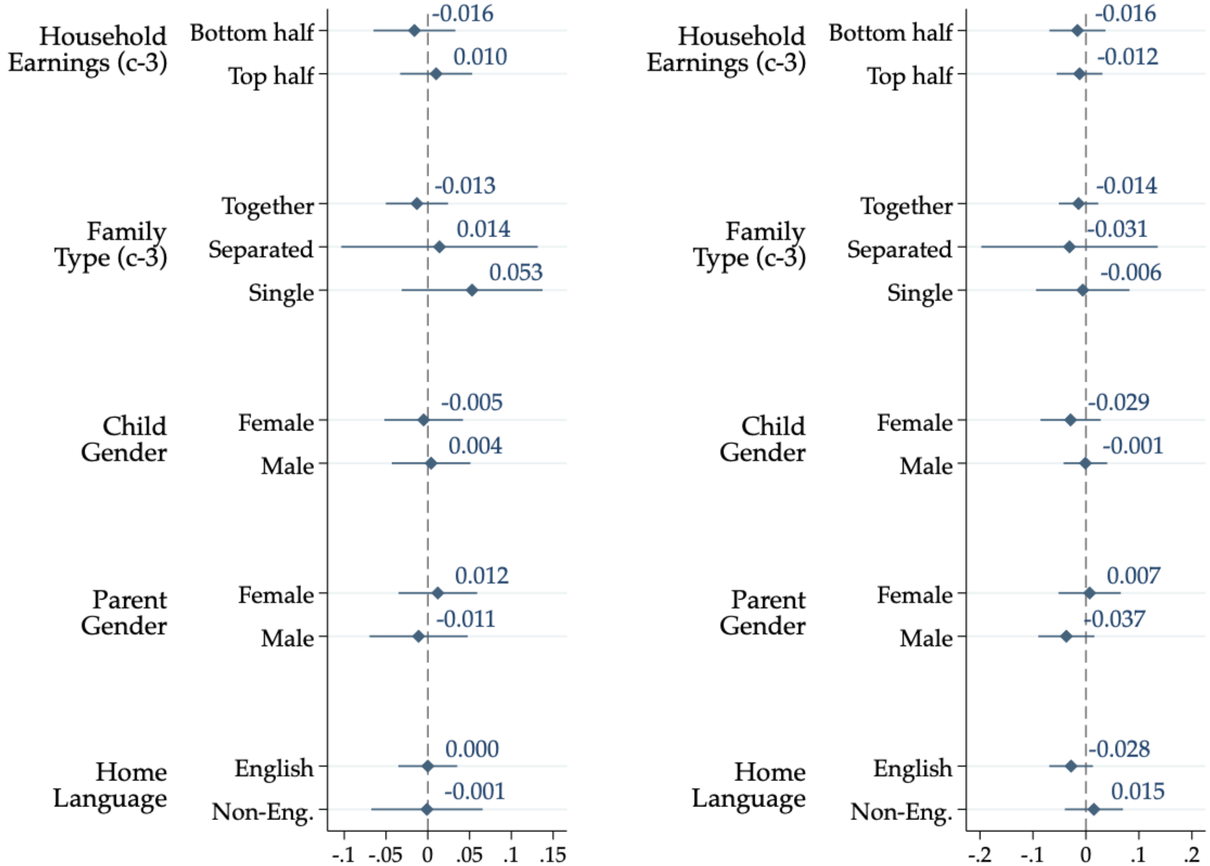
(b) Event Study Estimates



Notes: The figure shows household earnings before and after layoff for the final matched sample for grade 10. Figure (a) plots the average household earnings for the layoff group and the matched control group before and after the layoff (normalized to 0). Figure (b) plots the event study estimates (δ_k) for each period relative to year -3, estimated using equation (1). Event study estimates are reported for household earnings, as well as household after-tax income. These estimates measure the change in earnings of displaced workers relative to the path of earnings for non-displaced workers. Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Figure A5: Heterogeneity in Treatment Effects by Demographic Characteristics

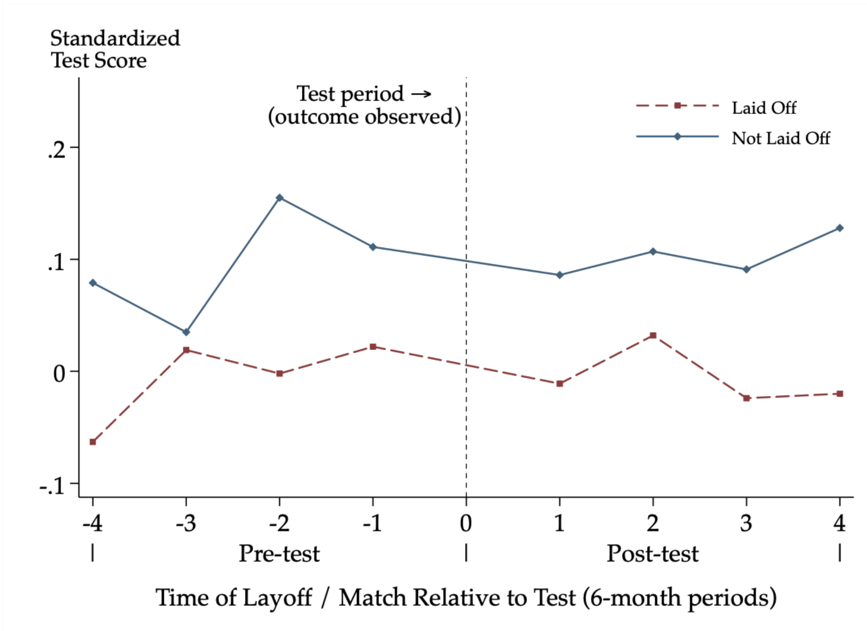
(a) Grade 4 Average Test Score **(b)** Grade 7 Average Test Score



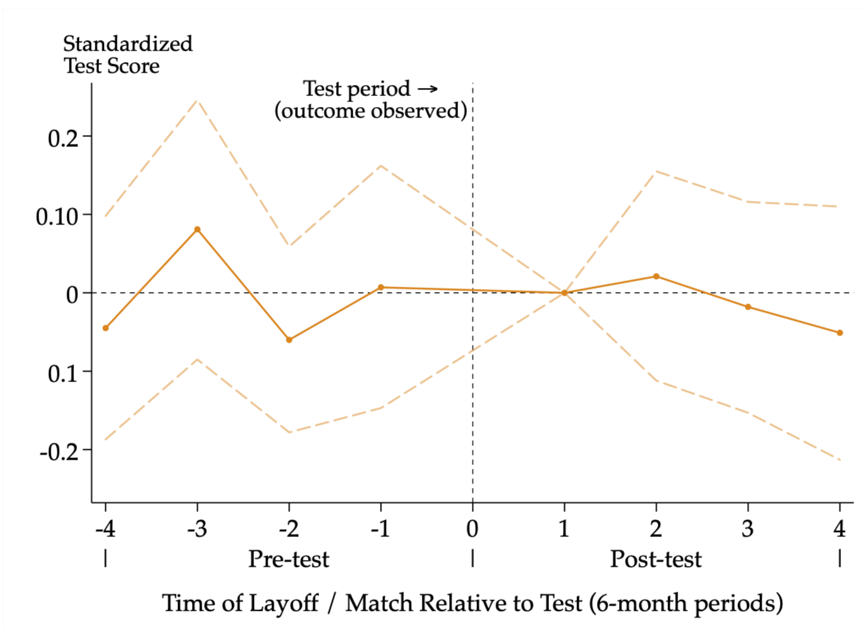
Notes: The figure reports treatment effect estimates (δ) from equation 3 for different subsamples based on demographic characteristics of households or children that suffer a parental job loss. Figure (a) reports treatment effects for grade 4 subsamples and Figure (b) reports treatment effects for grade 7 subsamples. The estimation accounts for academic year fixed effects. The horizontal bars represent 95% confidence intervals. Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Figure A6: Selection on Unobservable Characteristics is Constant Over Time Using CEM (Grade 4)

(a) Raw Means



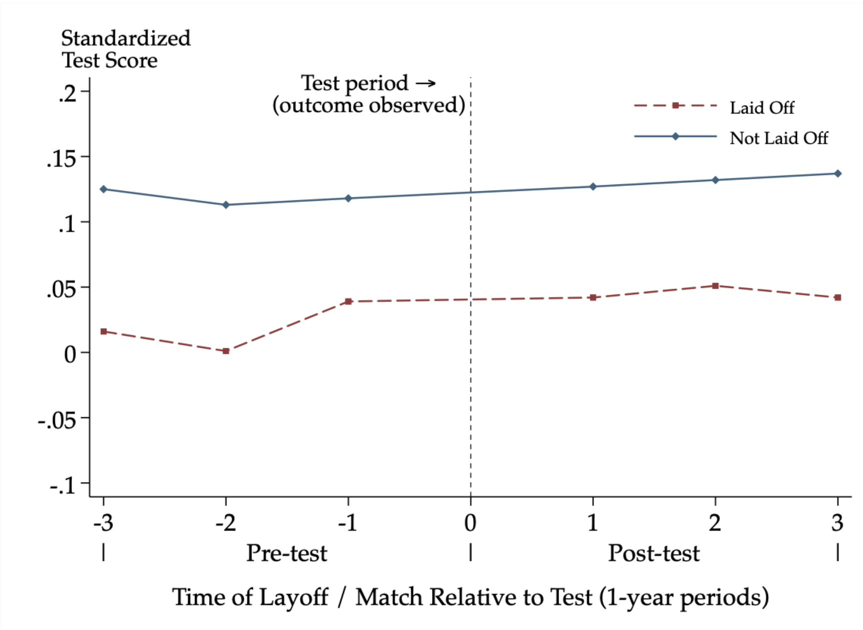
(b) DiD Estimates



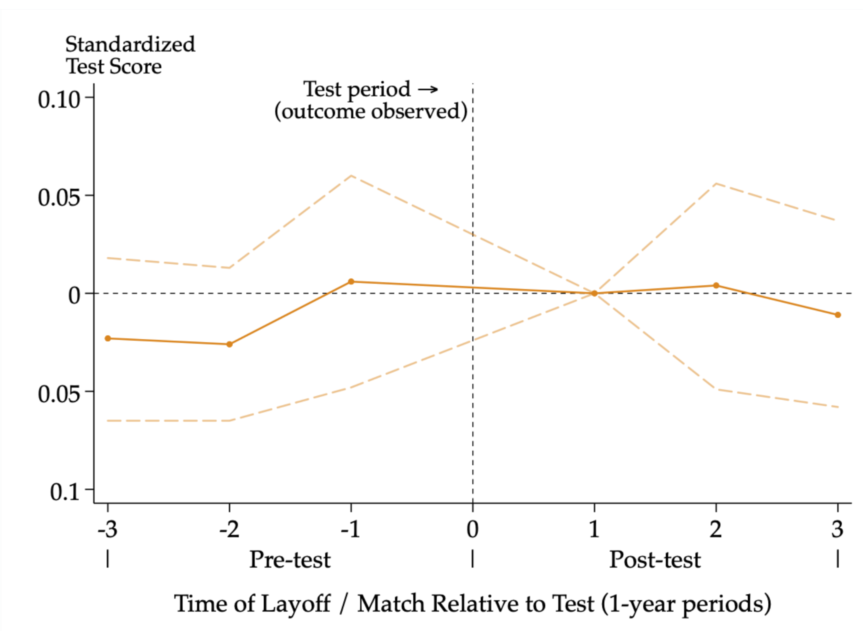
Notes: The figure is analagous to Figure 4, but uses an alternative sample for grade 4 created using coarsened exact matching. Figure (a) plots the average test scores in grade 4 of children in the layoff and matched control groups by the timing of the layoff (6-month periods) relative to the test period. All test scores are observed in the test period (0 in the figure), and the variation on the x-axis is the timing of the layoff. Figure (b) plots the difference in difference estimates (ϕ_k) for each period relative to period 1, estimated using equation (4). Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Figure A7: Selection on Unobservable Characteristics is Constant Over Time Using Extended Sample (Grade 4)

(a) Raw Means



(b) DiD Estimates



Notes: The figure is analagous to Figure 4, but uses an alternative sample for grade 4 created using an extened 6-year estiamation window. Figure (a) plots the average test scores in grade 4 of children in the layoff and matched control groups by the timing of the layoff (1-year periods) relative to the test period. All test scores are observed in the test period (0 in the figure), and the variation on the x-axis is the timing of the layoff. Figure (b) plots the difference in difference estimates for each period relative to period 1, estimated using equation (4). Standard errors, are clustered at the level of census subdivision three years prior to the layoff.

Appendix B Identifying Assumptions: Potential Outcomes Framework

This appendix explains the difference-in-difference estimates from section 4.2.4 in a potential outcomes framework.

Subsection B.1 Parameter of interest

For all children that are being compared the academic performance is measured in the same test period $t(a)$ within academic year a. Since, there is only one test period in each academic year, I shorten the notation $t(a)$ to t here. Let $D_{t-1} = 1$ for children whose parents experienced a layoff in a six-month period $t-1$, and let $D_{t-1} = 0$ for all children whose parents do not experience a layoff in period $t-1$. Let Y^1 denote the potential test score of a child (observed in period t) whose parent experienced a layoff in $t-1$, and Y^0 denote the potential test score of a child whose parents do not experience a layoff in $t-1$.

I am interested in the average treatment effect on the treated (ATET) for children whose parents were laid of in period $t-1$:

$$\delta_{t-1} = \mathbb{E}[Y^1 - Y^0 | D_{t-1} = 1]$$

The identification challenge is that I do not observe $\mathbb{E}[Y^0 | D_{tz-1} = 1]$, which is the average test score of children whose parents were laid off in period $t-1$ had they, counterfactually, not been laid off.

Subsection B.2 Matching estimator

Simply comparing children of displaced workers against children of all other workers will give us a biased estimator due to selection into layoffs.

To eliminate the selection effect, in the past, researchers have relied on either saturated regressions with demographic and socioeconomic controls or matching methods to find a subset of non-displaced workers who are similar to the displaced workers. The matching estimator relies on two assumptions:

1. Common support: $0 < Pr(D_{t-1} = 1 | X) < 1$

2. Conditional independence: $Y^0, Y^1 \perp\!\!\!\perp D_{t-1} | X$

where X is a vector of observable characteristics that are used to match laid-off workers (and their children) to workers (and their children) who do not suffer a layoff. The first assumption simply requires there to be enough overlap between displaced and non-displaced units for the researcher to be able to find appropriate matches. The second assumption says that after we have matched the displaced units to a set of appropriate comparison units using variables X in our dataset, potential outcomes (Y^0, Y^1) are independent of treatment assignment.

The researcher can then retrieve the ATET for children whose parents suffered a job loss in period $t-1$ by comparing their test scores to those from a matched group of children whose parents were not displaced:

$$\begin{aligned}
&\mathbb{E}\{\mathbb{E}[Y^1|D_{t-1}=1,X] - \mathbb{E}[Y^0|D_{t-1}=0,X]|D_{t-1}=1\} \\
&\qquad = \underbrace{\mathbb{E}\{\mathbb{E}[Y^1|D_{t-1}=1,X] - \mathbb{E}[Y^0|D_{t-1}=1,X]|D_{t-1}=1\}}_{\delta_{t-1} = \text{ATET for layoff at } t-1} \\
&\qquad\quad + \underbrace{\mathbb{E}\{\mathbb{E}[Y^0|D_{t-1}=1,X] - \mathbb{E}[Y^0|D_{t-1}=0,X]|D_{t-1}=1\}}_{\gamma_{t-1} = \text{Selection into layoffs at } t-1 \text{ after matching on } X}
\end{aligned}$$

Under these assumptions listed above, $\gamma_{t-1} = 0$ because:

$$\begin{aligned}
\mathbb{E}[Y^1|D_{t-1}=1,X] &= \mathbb{E}[Y^1|D_{t-1}=0|X] \\
\mathbb{E}[Y^0|D_{t-1}=1,X] &= \mathbb{E}[Y^0|D_{t-1}=0|X]
\end{aligned}$$

As shown in Table 2 and Figure 4, this matching estimator is susceptible to bias. I find a negative "treatment effect" when comparing children of laid off workers against their matched control group even when the children had written the test prior to the layoff. The finding echoes the results in Hilger (2016).

Subsection B.3 Proposed difference-in-differences estimator

Let $D_{t+1} = 1$ for children whose parents experienced a layoff in period $t+1$. Let $D_{t+1} = 0$ for all children whose parents do not experience a layoff in period $t+1$.

Now, consider the following difference-in-difference estimator that compares the test scores of children whose parents are laid off and their matched comparison group (first difference / matching estimator) before and after the assessment (second difference):

$$\underbrace{\mathbb{E}\{\mathbb{E}[Y^1|D_{t-1}=1,X]-\mathbb{E}[Y^0|D_{t-1}=0,X]|D^{t-1}=1\}}_{\text{Matching estimator at } t-1}$$

$$-\underbrace{\mathbb{E}\{\mathbb{E}[Y^1|D_{t+1}=1,X]-\mathbb{E}[Y^0|D_{t+1}=0,X]|D_{t+1}=1\}}_{\text{Matching estimator at } t+1}$$

$$=\delta_{t-1}-\delta_{t+1}+(\gamma_{t-1}-\gamma_{t+1})$$

Assumption 1: *There is no treatment effect on children whose parents are laid off after the children's assessment period, i.e.* $\delta_{t+1}=0$.

Layoffs in period $t+1$ occur after the assessment period. Since, children have already written the assessment before the layoff occurs, it is safe to assume that their test scores are unaffected by the parent's layoff. Under this assumption, the difference in the average test scores of the two groups is equal to γ_{t+1}, which measures the selection on unobservable characteristics in period $a+1$ that remains after my matching procedure.

Assumption 2: *Any unobservable selection that remains after the matching procedure is constant across the pre-assessment period ($t-1$) and the post-assessment period ($t+1$), i.e.* $\gamma_{t-1}-\gamma_{t+1}$

It is impossible to test if $\gamma_{t-1}=\gamma_{t+1}$ because γ_{t-1} is unobserved. However, similar to the test of parallel trends in a traditional event study design, I can test whether selection remains constant *after* the assessment, i.e. whether $\gamma_{t+1}=\gamma_{t+2}=\gamma_{t+3}=\gamma_{t+4}$.

Under assumptions 1 and 2, the DiD estimator above provides an unbiased estimate of δ_{t-1}.

The coefficients, $\phi_k, k\in\{2,3,4\}$, in section 4.2.4 represent the differences in selection observed at periods $t+2, t+3$, and $t+4$ relative to period $t+1$, i.e. $\gamma_{t+2}-\gamma_{t+1}$, $\gamma_{t+3}-\gamma_{t+1}$, and $\gamma_{t+4}-\gamma_{t+1}$. Coefficients $\phi_k, k\in\{-1,-2,-3,-4\}$ estimate the treatment effects for children whose parents are laid off at different points in time prior to the test: δ_{t-1}, δ_{t-2}, δ_{t-3}, δ_{t-4} under the assumption that selection on unobservable characteristics in these periods is equal to selection on unobservable characteristics measured in period $t+1$, i.e. $\gamma t+1$.

The estimator resembles a differences-in-discontinuities design used by Grembi et al. (2016), but with calendar time as the running variable and the assessment date as the threshold. Note that I cannot implement a differences-in-discontinuities design because I do not observe the exact day of the assessment, only the month-long period when the assessments were held across the province. Furthermore, such a design would produce a *local* / *instantaneous* treatment effect of layoff, which would be uninteresting.

Hilger (2016) implements the same research design to study the impact of parental layoffs on children's college enrollment at age 18-22. It is unclear whether Hilger (2016) uses a "never-treated" control group. In absence of a never-treated control group, it is possible that treated units from one point in time could spill over into the control group at another point in time, and vice versa. This would bias the treatment effect towards zero. To avoid this, I use "never-treated" control group: children whose parents did not suffer a layoff during the entire 4-year estimation window.