The Kids Aren't Alright: Parental Job Loss and Children's Education in Brazil*

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Abstract

We study the impacts of parental job loss and unemployment benefits on children's education in Brazil, using rich individual-level data on employment, school enrollment, and unemployment insurance for the entire population. Leveraging mass layoffs for identification, we find that parental job loss has a significant adverse impact on children's educational outcomes. School dropouts and age-grade distortion increase by up to 1 and 2 percentage points. The effect is concentrated on disadvantaged families, persisting for at least six years and leading to lower high school completion rates. We further show that children aged 14-17 are more likely to work informally and to commit crimes following parental displacement. In turn, children in advantaged families are more likely to move to lower-quality schools due to parental displacement. Using a clean regression discontinuity design, we show that access to unemployment benefits mitigates some of the adverse impacts of parental job loss on children. Our findings indicate that the income losses following parental displacement are an important mechanism of the effects on children, highlighting the importance of policies that provide income support for displaced workers. Other explanations related to family rupture, migration to poorer neighborhoods, and changes in household production do not receive much support from the data.
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1 Introduction

Education is key for human capital formation and socioeconomic development (Cunha and Heckman, 2007). However, numerous low and middle-income countries still face huge challenges to prevent school dropouts, a problem that exists but is much less salient in developed economies.[1] Economic factors and the lack of social insurance in poorer countries may help explain this gap and related issues such as children's school performance and grade distortion. This paper studies the impact of parental job loss – one of the most pervasive economic shocks in modern societies – on children's education and the role of unemployment insurance as a mitigating factor. A wealth of research has demonstrated that job loss has dire consequences on individuals' lives.[2] Clearly, such consequences may extend to children and their educational path. Despite the relevance of such shock and the fact that low and middle-income countries still fight to keep children at school, the existing literature on parental job loss and education is vastly concentrated in developed countries. The main reason for that is the lack of rich administrative data linking parental employment to children's school outcomes, which has been primarily available for Scandinavian countries and the US.

This paper overcomes these challenges by exploiting extremely rich and large administrative data from Brazil. We link individual-level data on parental employment and unemployment benefits to detailed school data on 42 million children. We complement this data with information on families from welfare registries and on crime outcomes. The school data allow us to track children's enrollment, grade progression, and a rich set of information on schools throughout several years. We use these data to tackle two main questions. First, we provide novel evidence on the impact of parental job loss on children's education based on rich administrative data in the context of a developing country. The richness of the data allows us to investigate several outcomes and alternative mechanisms, and to provide a complete characterization of heterogeneous treatment effects across individuals and place characteristics across the vast Brazilian territory. Second, we provide novel estimates on the role of unemployment insurance as a mitigating factor, exploiting a clean regression discontinuity design thanks to the Brazilian institutional setting. The latter evidence is not available even in the context of rich countries and sheds light on the spillover effects of traditional social insurance policies on children. Importantly, this analysis allows us to make a step forward understanding the role of income as a mechanism linking parental job loss and children's

[1]57 million children in low and middle-income countries remain out of school, while the share of children out of school in rich countries is close to zero (World Bank 2019).

[2]This includes impacts on labor market outcomes (Couch and Placzek, 2010; Sullivan and Von Wachter, 2009; Schmieder et al., 2021; Ichino et al., 2017), divorce (Charles and Stephens, 2004; Eliason, 2012), mental health (Zimmer, 2021; Zimmerman, 2006), smoking (Black et al., 2015), premature mortality (Sullivan and Von Wachter, 2009), fertility (Del Bono et al., 2012, 2015), offspring birth weight (Lindo, 2011), and crime and domestic violence (Bhalotra et al., 2021; Britto et al., 2021; Rose, 2018).

education.

We leverage mass layoffs and plant closures to identify the effects of parental job loss. Specifically, we use a difference-in-difference design where we compare over time children whose parents were displaced in mass layoffs to similar children whose parents were not displaced in the same period. Treated and control families are exactly matched on several characteristics, including geographical area, education, age, and gender of the displaced parent; and child grade, age, and gender. In addition to showing that treated and control units follow similar trends before the job loss, we provide evidence that our main results are not driven by selection into layoffs – even within mass layoffs. Moreover, our estimates are robust to alternative estimators proposed in the recent literature on staggered treatment in difference-in-difference designs.[3] We also provide evidence that our results are not driven by large mass layoffs, which could generate substantial spillover across displaced co-workers, supporting the external validity of our findings.

We start by showing that job loss causes substantial losses on parental income, employment, and wages both for fathers and mothers, in line with previous literature (Couch and Placzek, 2010; Sullivan and Von Wachter, 2009; Schmieder et al., 2021). Turning to the impact on children's education, our analysis shows that parental job loss significantly worsens children's school outcomes. More specifically, in a two-year period, it reduces school enrollment on average by .4 percentage points (p.p.), relative to an 8% dropout rate in the baseline, and increases age-grade distortion by .6 p.p, relative to a baseline rate of 15%. The impact on age-grade distortion is stronger for younger children, while school dropout effects are stronger for those in secondary schooling age 14-17, both effects going up to 1 p.p. In line with most other countries, school dropouts in Brazil are concentrated at secondary schooling age even though primary and secondary schooling (ages 6-17) are compulsory in Brazil.

We investigate child work outcomes as a potential factor explaining the negative effects on school outcomes. As in other low and middle-income countries, the incidence of child work remains substantial in Brazil – 4 and 13% of children aged 10-13 and 14-17 work, mainly in the informal labor market, and 39% of students dropping out report the necessity of work as the main reason for leaving school.[4] We use longitudinal survey data to show that children 14-17 are more likely to work in the informal labor market following parental job loss.[5] We also provide novel evidence on the consequences of parental job loss on crime by children 14-17, which increases by 63% over the baseline

[3]For example, see Sun and Abraham (2021); Athey and Imbens (2018); De Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Callaway and Sant'Anna (2021); Imai and Kim (2019).

[4]This information is based on the PNAD, a nationally representative survey, and the 2010 Population Census.

[5]These responses are consistent with the several restrictions limiting children below the legal age in Brazil (18) to work in the formal labor market.

– as measured by children sent to correctional facilities due to criminal offenses.[6]

The evidence on crime and informal labor supply indicates that income may be a key mechanism driving our results. More specifically, it supports the idea that children work more to compensate for the income losses brought by job loss in the family, possibly driving them out of school and reducing school performance for those who do not dropout out. The increase in the probability that children commit crimes is also consistent with this hypothesis. Although we cannot distinguish economically motivated from other crimes in our data, the former category accounts for 75% of criminal prosecutions for defendants aged 18.[7] The income mechanism is also supported by heterogeneity analyses which reveal a strong gradient over parental income before the job loss. More specifically, the effect on school enrollment and age-grade distortion is more substantial in poorer families and null for families in the upper quartile of the income distribution. Instead, even though the effects are pervasive, they vary substantially less across several individual characteristics such as parental age, gender and education, child gender, and across area-level characteristics, despite the large and heterogeneous Brazilian territory. In turn, we also do not find much support in the data for several additional, non-exclusive, mechanisms related to family rupture, the allocation of home chores and market production by fathers and mothers, and migration to poorer neighborhoods or municipalities.[8]

We uncover relevant impacts on school choices for children in advantaged families, who experience milder adverse effects of parental job loss on enrollment rates and age-grade distortion. Specifically, following the layoff, children in high-income families who are initially enrolled in private schools are more likely to move to public and lower quality schools. Namely, they move to schools where average parental income and socioeconomic background are lower, and which perform worse in national exams.

Next, we investigate the long-term consequences of parental job loss on children's education. We replicate our main analysis for children present in welfare registries, for whom we can track outcomes for up to six years following displacement, as opposed to only two years in our main analysis. These registries cover about half of the Brazilian population, being concentrated on poorer families. The results show that parental job loss causes persistent adverse effects on children's educational outcomes, lasting at least six years. Six years after the job loss, enrollment rates are over 1 p.p. lower, and grade

[6] Although children below the legal age (18) cannot be arrested or criminally prosecuted in Brazil, they can be sent to correctional facilities, which we are able to track in our data.

[7] The legal age (18) is the earliest age for which such statistics is available.

[8] In line with the evidence that job loss may lead to mental health problems and stress (Charles and DeCicca, 2008; Kuhn et al., 2009; Zimmer, 2021), psychological factors could partially mediate the income mechanism. Although our data do not allow us to provide direct evidence on these aspects, it seems less likely that these factors compose a direct mechanism unrelated to income, linking parental job loss and children's outcomes. A direct psychological mechanism would likely affect individuals across the income distribution more evenly, in contrast to our findings indicating null effects on enrollment and age-grade distortion for high-income families.

distortion probabilities increase by almost 2 p.p.. The long-lasting effects are consistent with the persistent labor market losses following job loss.

These patterns strongly suggest that parental job loss has permanent negative consequences for children's education. To shed further light on this aspect, we provide an additional empirical analysis leveraging variation in the timing of parental job loss to study its impacts on high school completion rates. More specifically, we compare children whose parents were displaced in a mass layoff years before and after the expected high school completion age relative to similar children whose parents were not displaced in the same period. The analysis follows a similar setting to Hilger (2016) who studies the same effects on college enrollment outcomes in the US context. We find that parental job loss, taking place up to four years before the expected graduation age, reduces the probability that children complete high school by 1.5 p.p.. Although we cannot track employment outcomes in adulthood because children are too young in our sample, these results indicate that parental job loss likely has important long-term consequences.

We then analyze whether unemployment benefits succeed at mitigating the impacts of parental job loss on children's enrollment rates and age-grade distortion. In addition to addressing an extremely policy-relevant question, the analysis sheds further light on mechanisms and, specifically, on the role of income. This analysis is based on a clean regression discontinuity design that compares displaced parents who are barely eligible and ineligible to unemployment benefits due to slight variations in layoff dates.[9]

Our results show that the eligibility to unemployment insurance (UI) strongly increases enrollment rates for children in welfare registries, who bear the largest effects of parental job loss. The results indicate that access to 2.8 months of unemployment benefits with a 85% replacement rate increases children's school enrollment by 1.7 p.p. in the two years following parental job loss. These results point again at income as a relevant mechanism.[10]

The paper contributes to the literature studying the impacts of parental job loss on children's education in several aspects. First, it provides the first large-scale evidence relying on rich administrative data sources in the context of low and middle-income countries. While the literature has been concentrated on the US and Scandinavian countries due to data availability, schooling provision is arguably a much larger challenge in developing countries, where governments still struggle to keep children at school.[11]

[9] We provide exhaustive evidence that parents and children near the cutoff are as good as randomly distributed – the running variable density and a rich set of characteristics are shown to be continuous around the cutoff. In addition, we show that children's school outcomes are balanced prior to the layoff, offering compelling evidence supporting the design.

[10] In line with the results in our data and with extensive literature, UI reduces labor supply (Gerard and Gonzaga, 2021; Katz and Meyer, 1990; Lalive, 2008). We provide evidence that increases in unemployment duration are unlikely to explain the impacts of UI on children's educational outcomes.

[11] Specifically, Oreopoulos et al. (2008) and Hilger (2016) study the effects of parental job loss on children

In addition, the prevalence of child work and high crime levels poses additional challenges and distinguishes such contexts from that of developed countries. To date, the evidence for low and middle-income countries has been limited to the use of relatively small survey datasets (e.g., Duryea et al. (2007); Rege et al. (2011)), and the only paper explicitly addressing endogeneity concerns is Di Maio and Nisticò (2019) who study dropouts in Palestine, within the relevant though particular context of conflicts.

Second, the richness of our data allows us to provide a throughout characterization of the impacts of parental job loss on children. We are able to study several outcomes inside and outside the educational system – namely, on enrollment, grade distortion, school quality choices, child work and crime –; to provide rich heterogeneity analysis across individuals and the diverse Brazilian territory; and to study both the short and long-run consequences for children's education. Our empirical setting comparing children's outcomes before and after parental job loss goes a step further relative to important contributions such as Oreopoulos et al. (2008); Rege et al. (2011), addressing the fact that cross-section comparisons across displaced and non-displaced parents (even when based on mass layoffs or plant closures) may not completely eliminate selection bias, as suggested by Hilger (2016). The evidence on crime committed by children below the legal age is novel to this literature and complements recent work on the impact of job loss on adult crime (Rose, 2018; Bennett and Ouazad, 2020; Britto et al., 2021; Khanna et al., 2021).[12] Finally, the effects on school quality indicate that parents reduce parental investment to absorb economic shocks, contributing to a broad literature studying the determinants and consequences of parental investments – e.g., see Cunha and Heckman (2007); Carneiro and Ginja (2016); Francesconi and Heckman (2016).

Third, we provide the first estimates in the literature on the impacts of unemployment benefits, one of the most relevant and widespread social insurance policies around the globe. These findings are a key contribution to the literature. Even though several papers find relevant impacts of parental job loss on children, there is a lack of causal evidence on the role of social insurances transfers as a mitigating factor. In addition to being extremely policy-relevant, these findings contribute to understanding the role of income as a mechanism linking parental job loss and children's education. These findings also contribute to a literature studying the impacts of unemployment benefits on non-labor related outcomes – e.g., Britto et al. (2021) on adult crime and Kuka (2020) on health outcomes –; and, more generally, to a literature studying the effects of parental income and access to welfare benefits during childhood (Dahl and Lochner, 2012; Hoynes et al., 2016).

using US data, whereas Rege et al. (2011), Huttunen and Riukula (2019) and Tanndal and Päällysaho (n.d.) use data from Norway, Sweden and Finland, respectively.

[12] To the best of our knowledge, Khanna et al. (2021) is the only paper providing causal evidence that parental job loss causes higher crime by children below the legal age.

The paper is organized as follows. In Section 2, we present the Brazilian institutional context, followed by our data in Section 3. Section 4 presents the empirical analysis on the impacts of parental job loss, while Section 5 present the analysis on the effects of unemployment benefits. Section 6 discusses the results and underlying mechanisms, followed by Section 7, which concludes.

2 Institutional Background

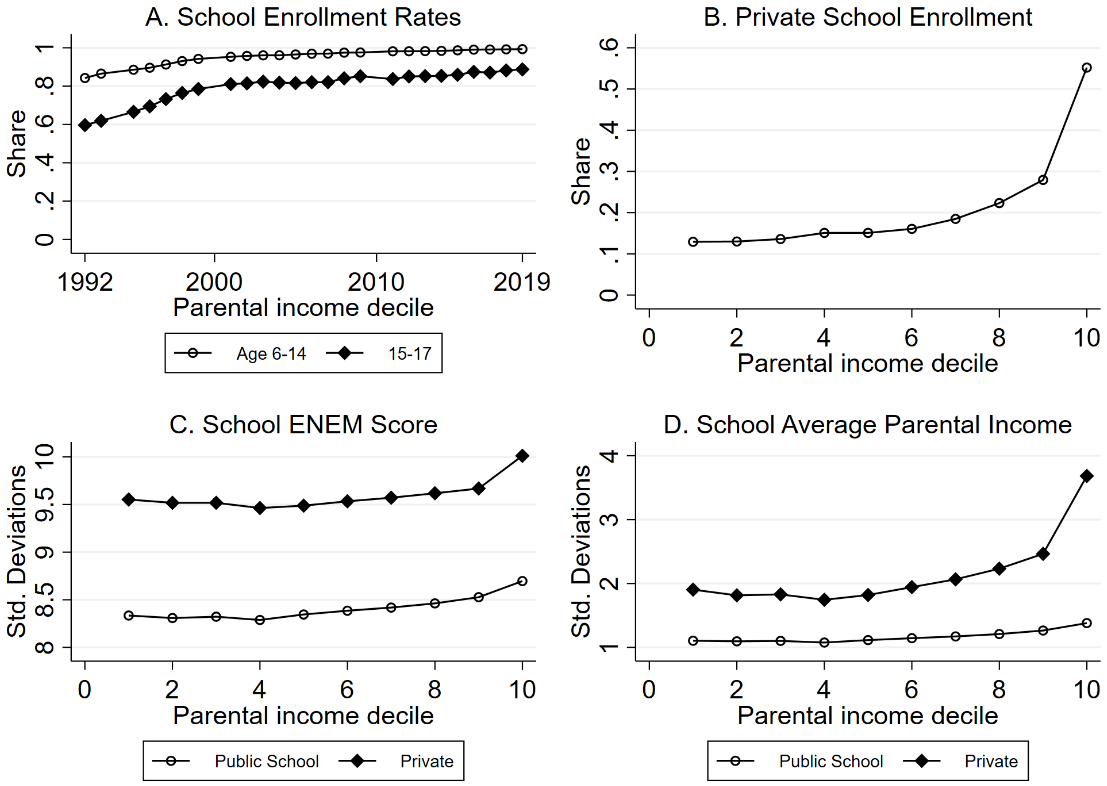
2.1 Education in Brazil

Brazil experienced a substantial reduction in the share of children out of school in primary and secondary schooling age over the last 30 years, as shown in Panel A, Figure 1. Although primary school for children aged 7-14 has been mandatory since 1971, a significant share of children was still not enrolled in the 1990s. From there, primary school dropouts were reduced to about 1 percent following several policies, which include the opening of public schools, increasing mandatory schooling age, and the introduction of *Bolsa Família* in 2004 – a large cash transfer program conditional on school enrollment. For secondary schooling, although enrollment rates have improved over time, dropouts remain sizable even though primary and secondary schools – with expected age ranges 6-14 and 15-17, respectively – are mandatory since 2009.

Public primary education is mainly provided by the 5,570 municipalities, while public secondary education is usually provided by the 27 State governments, both being offered completely free of charge for all citizens. Public schools, however, are generally composed of students with relatively lower socio-economic backgrounds as wealthier families tend to enroll children in private schools, as shown in Panel B in Figure 1. These are generally perceived as higher quality schools compared to their public counterparts. In line with that, Panel C in the same figure shows that private schools achieve higher scores in ENEM – a national examination following the end of secondary school and used in the admission process of public and private universities in Brazil. The same panel shows that children in the upper side of the income distribution enroll in better schools, both within and across the private and public school systems. In addition, there is a large variation in quality within private schools, for which prices vary greatly.[13] In turn, Panel D shows that there is assortative matching in school choices, with high-income parents enrolling children in schools where average parental income is higher.

[13]Firpo et al. (2014) shows that school prices are positively correlated with ENEM scores.

Figure 1: Education in Brazil



Notes: The figure provides summary statistics on school enrollment for children in primary and secondary schooling ages (Panel A) based on PNAD household survey (not available for Population Census years 2000 and 2010), and average school characteristics by parental income in the formal labor market, based on School Census data for 2014 (Panels B-D).

2.2 Labor Market

All private firms in Brazil are free to dismiss workers without a just cause.[14] Such dismissals represent roughly 70% of all separations, while job quits virtually cover the remaining part. Workers dismissed without cause are entitled to receive a mandatory severance payment paid by the firm. The amount is equivalent to 40% of the balanced in a forced savings account which receives an 8% monthly contribution over the worker's earnings – also paid by the firm over the entire duration of the labor contract. The account proceedings are only made available to the worker upon dismissal.[15] In total, workers receive about 1.34 monthly earnings per year of tenure upon displacement.

Unemployment insurance (UI) is the main policy supporting these workers, which can last for up to five monthly benefits, with an average replacement rate of 79%. The only other form of income support at the national level is *Bolsa Família*. Although it covers roughly 45 million individuals, about a fifth of the Brazilian population, it targets

[14]Throughout the paper, we refer to separations without cause as dismissals, displacement, or layoffs, interchangeably.

[15]Workers can withdraw from the account during the labor contract in selected exceptional situations such as the acquisition of real estate or severe illness.

very low-income families with per capita income below .1 minimum wages, and the average transfer per family is equivalent to only .16 minimum wages.

The Brazilian labor market is characterized by a very high degree of labor turnover, with roughly 45% and 80% of ended formal job spells lasting less than one and three years, respectively. Labor informality is high, about 45% in the study period, and workers constantly turnover between formal and informal jobs (Ulyssea, 2018). Our main analysis studies parents displaced in mass layoffs from formal jobs, which we can track in administrative employment data. The analysis will overestimate parental employment losses since it misses informal jobs, which account for part of the employment recovery. We will use survey data to show that the income losses from parental formal job loss remain substantial even when considering informal jobs.

3 Data

We mainly rely on three rich administrative data sources that allow us to track parents' careers in the labor market, children's schooling, and family characteristics. The first source is the *Relação Anual de Informações Sociais* (RAIS), which provides detailed information on the population of formal workers and firms in the Brazilian labor market for the period 2002-19. It contains detailed information on each job spell, such as the contracts' starting and ending dates, earnings, the reason for termination, and detailed demographic characteristics such as date of birth, race, and education. Firms and workers can be identified by their unique tax codes – *CNPJ* and *CPF*, respectively – and their (full) names.

Second, we use information from *CadÚnico*, a welfare registry maintained by the Federal Government for the administration of welfare programs – including Bolsa Familia conditional cash transfer. The registry is targeted at the lower part of the income distribution, covering about half of the Brazilian population. We use yearly snapshots of these data for the period 2011-19. The registry identifies the household with a unique id and individuals by their full name and unique tax code, along with addresses and detailed demographic characteristics such as date of birth, municipality of birth, race, and education. We use these data to track couples' separation and migration across neighborhoods. In addition, the position of the person in the household and information on both parents' full name helps us identifying parent-child links.

Finally, we use data from the yearly School Census for the period 2008-17. The census is mandatory and hence filled by all public and private schools in the country. It contains detailed information on students and schools, which can be tracked with unique student and school identifiers over the years. It is possible to track children's enrollment, grade, class, demographic characteristics, and school characteristics.

Our main analysis is based on parent-child links for all students enrolled in 2014, for whom we have information on their student ids in the School Census, full name, birth date, municipality of birth, and both parents' full names. These data allow us to link children's student id to their parent's unique tax code, enabling us to link children and parents throughout several years in the School Census and the employment data. Our main analysis is thus conditional on school enrollment in 2014. Overall, we are able to identify fathers and mothers for 71% and 82% of the 45 million children in the School Census. In Appendix A.1, we provide the details of the data linkage procedure, and we show that children successfully linked to their parents do not significantly differ from the remaining ones in the School Census.

4 Parental Job Loss and Children's Education

4.1 Empirical Strategy

We leverage variation in the timing of mass layoffs to identify the effects of parental job loss with a difference-in-differences design. Such timing is arguably unrelated to the workers' decision or children's educational outcomes and has been widely used in the literature estimating job loss effects on various outcomes. In line with this literature, we define mass layoff firms as those dismissing more than a third of their workforce during a given calendar year and focus on private firms with at least 30 workers.[16]

The data is set at the parent-child level – so that parents with multiple children show multiple times – and we define our treatment group by full-time private sector working parents aged 18-60 who are displaced in mass layoffs in 2015 and 2016.[17] This time frame follows from our data on parent-child links which exclusively cover children enrolled in school in 2014. In addition, we focus on children 9-16 years old in the layoff year so that we can observe school enrollment three years before and two years after layoff – the compulsory schooling age range is 6-17, and the academic year follows the solar year, from January to December. For the same reason, we also restrict the data to children who are expected to be in age-grades 9-16 during the parental job loss year.[18] The expected grade is defined by the grade where the child is observed in the calendar year before parental job loss plus one.

We define the control group via exact matching on a fine set of characteristics, leveraging the large dimension of our data. For each mass layoff year, the set of

[16] We later show that our results are robust to more strict mass layoff definitions and plant closures. We drop from the sample firms reallocating under a new tax id, which are identified when more than 50% of workers move to the same new id following a mass layoff or plant closure.

[17] Our main findings continue to hold when replicating the analysis for mass layoffs taking place in 2011 and 2012, based on a sample of children in *CadÚnico*. This indicates that our main findings are not driven by the business cycle since 2015 and 2016 were recessions years in Brazil.

[18] Throughout the paper, we will refer to grade levels by their age-grade – i.e., age-grade 6 indicates grade 1, age-grade 7 indicates grade 2, and so on, up to age-grade 17, indicating grade 12.

potential control units are parents employed in non-mass layoff firms who have not been displaced in that same year. Each treated parent-child unit is exactly matched to a control unit on job location (27 states), gender, hiring year, education (college and high school dummies), and on child characteristics – gender, birth cohort, and grade in the pre-displacement year. When a treated unit is matched to multiple controls, we randomly select one. Out of 1.6 million parents in the initial mass layoffs pool, we successfully match 97% to a control unit.[19]

Each treatment-control pair defines a single difference-in-differences comparison. Time is defined by calendar years relative to the mass layoff year – control units are assigned a placebo layoff year equal to their treated pair. We then stack each of these single treated-control pairs and build a perfectly balanced panel tracking parent-child outcomes from three years before to two years after the layoff. As a result, our estimator is defined by the simple average over difference-in-differences comparisons for each treatment-control pair, ensuring that no unit receives a negative weight. Importantly, the control group is always composed of never-treated units, ensuring that we do not use already treated units to absorb time effects. This setting addresses the concerns raised by the recent literature on the estimation of dynamic treatment effects in two-way fixed effects settings.[20] In fact, we will show that negative weight issues are not present in our setting, following the diagnostic in De Chaisemartin and D'Haultfœuille (2020), and that our results remain remarkably similar when using an alternative estimator proposed in the same paper.

We estimate the following dynamic difference-in-differences equation:

$$Y_{it} = \sum_{t=-P, t\neq -1}^{T} \delta_t Time_t * Treat_i + \mu_i + \lambda_t + \epsilon_{it} \tag{1}$$

where the subscript i identifies a parent-child link within each treatment-control pair described above, our unit of analysis, and t identifies calendar years since layoff – control units are assigned a placebo layoff date equal to the matched treated unit. $Treat_i$ is an indicator for the treatment group – composed of workers displaced in a mass layoff –, and $Time_t$ indicates each period t. Individual fixed effects μ_i remove any remaining unobserved heterogeneity not captured by our fine matching strategy, whereas time-varying shocks are absorbed by the full set of period fixed effects, λ_t.[21] The coefficients $\{\delta_0, ..., \delta_T\}$ identify dynamic treatment effects, δ_{-1} is the omitted category, and δ_{-P}, ..., δ_{-2} estimate anticipation effects. The latter coefficients test whether treatment and

[19]When there are fewer control units than treated ones, a share of control units is assigned to multiple treated units.

[20]See (Athey and Imbens, 2018; De Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Imai and Kim, 2019; Goodman-Bacon, 2021; Sun and Abraham, 2021).

[21]Our estimates remain exactly the same when adding calendar year fixed-effects, indicating that our main specification perfectly absorbs time shocks.

control units follow similar trends in outcomes prior to the layoff, providing a test for the common-trend assumption. Finally, we estimate the following equation to summarize the average treatment effects:

$$Y_{it} = \beta Post_t * Treat_i + \mu_i + \lambda_t + \epsilon_{it}, \tag{2}$$

where $Post_t$ identifies the post-treatment period following parental job loss and β is the main coefficient of interest.

In Table 1, we show that the treatment and control groups defined via exact matching are similar over a rich array of parents' and children's characteristics, including those not included in the matching process – e.g., parents' labor income, school attributes, and municipality characteristics. In addition, the standardized difference between both groups remains below the threshold of 0.20 suggested by Imbens and Rubin (2015) for all variables. Although the validity of our difference-in-differences design does not require that treatment and control units are similar, such similarity increases the likelihood that they follow similar trends before the treatment, making the common-trend assumption more plausible.

Nevertheless, even in the case of parallel trends in the pre-displacement period, a key challenge for identification is dynamic selection into layoffs, even within mass layoffs where firms have less discretion in choosing whom to fire. For instance, shocks to the household may cause stress in the family, potentially increasing the likelihood that the worker is displaced during a contemporaneous mass layoff and, at the same time, leading to children's poor school performance. We will address this and several other identification concerns in the robustness Section 4.5. In the same section, we will discuss the external validity of our analysis since mass layoffs could, in principle, significantly differ from regular layoffs.

4.2 Effects on parental employment outcomes

We start by analyzing the impact of job loss on the employment outcomes of parents and their spouses, following the dynamic specification in equation (1). Only for this analysis, we set time relative to the exact layoff date and track outcomes up to three years after displacement.[22] In line with the literature, job loss causes substantial income losses for both men and women, as shown in the top two graphs of Figure 2. The absolute impact is roughly 50% larger for fathers, who have higher baseline income before the layoff. Although income recovers over time, the job loss effect is still sizable up to three years after displacement when fathers and mothers earn 54% and 45% less with

[22]This is because the employment data goes up to 2019, and it is possible to track the precise start and end date of each job spell. On the other hand, the School Census runs up to 2017 (individual identifiers changed after that) and only indicates whether the student has been registered in each school during the academic year.

Table 1: Treatment and control groups descriptive statistics

	(1)	(2)	(3)
	Treated	Control	Std. Diff.
PARENT CHARACTERISTICS			
Age	39.4	39.3	0.02
Female	0.25	0.25	0.00
Years of education	10.2	9.8	0.10
Tenure months	28.0	27.5	0.01
Labor income	16013	17571	-0.07
Months worked	9.5	9.6	-0.01
Labor income - other parent	4877	4432	0.03
Months worked - other parent	3.3	2.9	0.08
CHILD CHARACTERISTICS			
Age	12.8	12.8	-0.01
Gender	0.49	0.50	-0.02
Age-grade	11.0	11.0	-0.01
School parental income	1.3	1.3	0.05
School ENEM score	8.8	8.7	0.11
MUNICIPALITY CHARACTERISTICS			
Population	1819191	1975522	-0.05
Pib per capita	25314	27312	-0.10
Gini index	0.64	0.65	-0.10
Labor informality	0.38	0.37	0.10
Homicide rate	31	33	-0.09

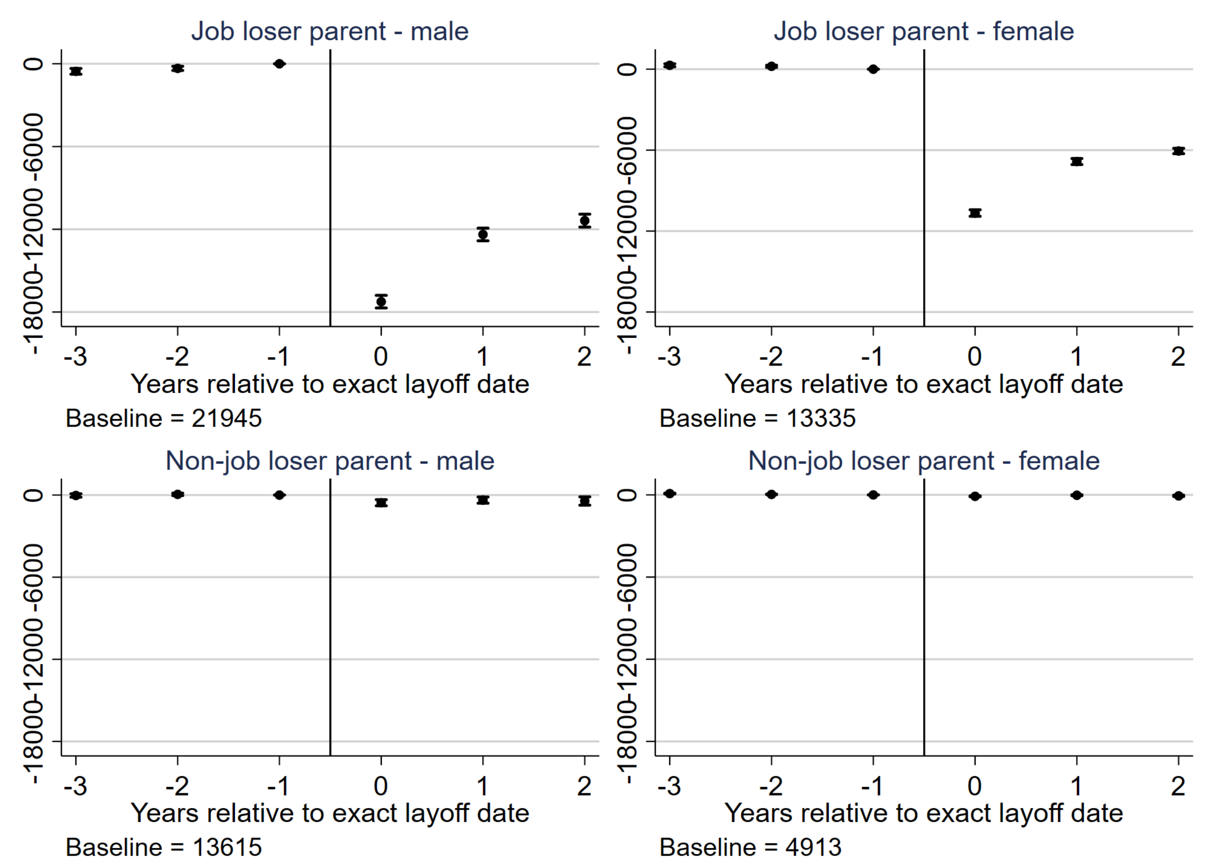
Notes: This table reports the average characteristics for treated workers displaced in mass layoffs (column 1); for matched control workers who are not displaced in the same calendar year (column 2); and the standardized difference between the two groups (column 3).

respect to their baselines. In Appendix B.1, we show that there is a negative effect on additional labor market outcomes such as employment, wages, and job turnover; and that the income drop when taking into account informal jobs is about 20% and 10% smaller for men and women, respectively. In the two bottom graphs of Figure 2, we analyze the impact on the parent not losing his position in the mass layoff– i.e., the child's mother when the job loser is the father, vice-versa. We do not find any economically significant effect, indicating that added worker effects are minor in this context, both for men and women.

4.3 Effects on school enrollment and age-grade distortion

We now turn to the analysis of children's educational outcomes. We start by analyzing school dropouts, by estimating the dynamic specification presented in Equation (1). As shown in Figure 3, left panel, children's school enrollment follows similar trends prior to the parental job loss during mass layoffs, supporting the common-trend identification

Figure 2: Effect of parental job loss on formal labor income

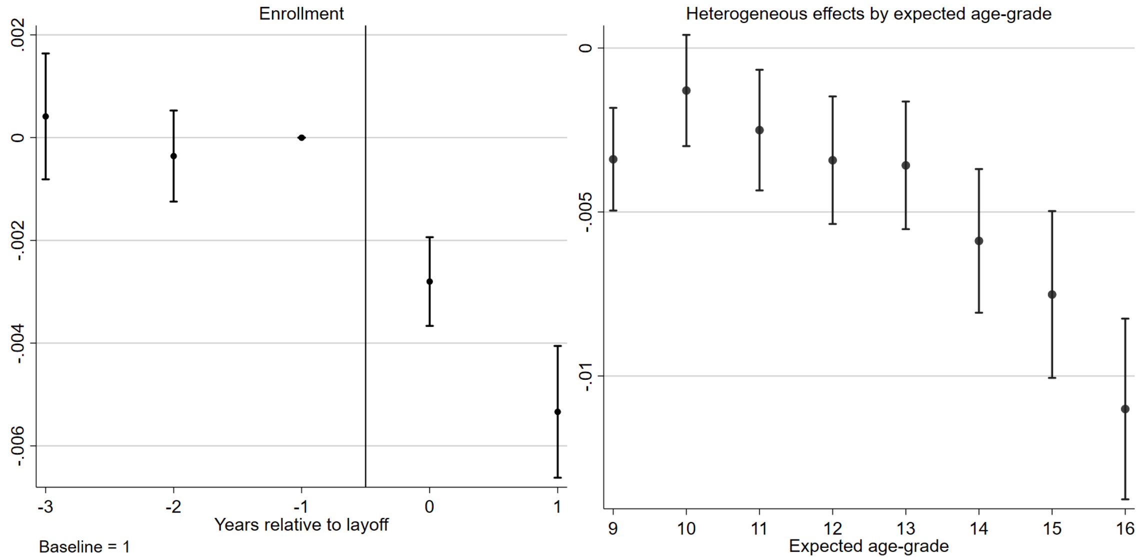


Notes: This figure presents dynamic treatment effects of job loss on employment outcomes, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals (too small to be visible). The top panels show the effect on labor income for the parent losing her/his job, while the bottom panel shows the effect for the non-job loser parent, by gender. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group. Years relative to layoff are defined relative to the exact date of layoff, i.e., t = 1 for the first 12 months after layoff, t = 2 for the following 12 months, and so on. Income variables are measured in Brazilian Reais.

assumption. A clear reduction in children's enrollment emerges during the job loss year, which persists in the subsequent year. The right panel in Figure 3 shows how the results vary by students' expected age-grade in the mass layoff calendar year – defined by one plus the observed age-grade in the year prior to the layoff. We estimate average treatment effects in the post-treatment period as in Equation (2), based on the expected age-grade for each subgroup.[23] There is a negative impact on children in all grades, however, it is largest for children in secondary school who reduce enrollment by up to 1 percentage point. This is consistent with descriptive evidence showing that dropout risk is largest during secondary education (See Figure 1, Panel A, in Section 2.1).

[23] As described in Section 4, we restrict attention to children in expected age-grade 9-16 at parental job loss so that we can observe them in school for at least three years before and two years after the layoff.

Figure 3: Effect of parental job loss on school enrollment

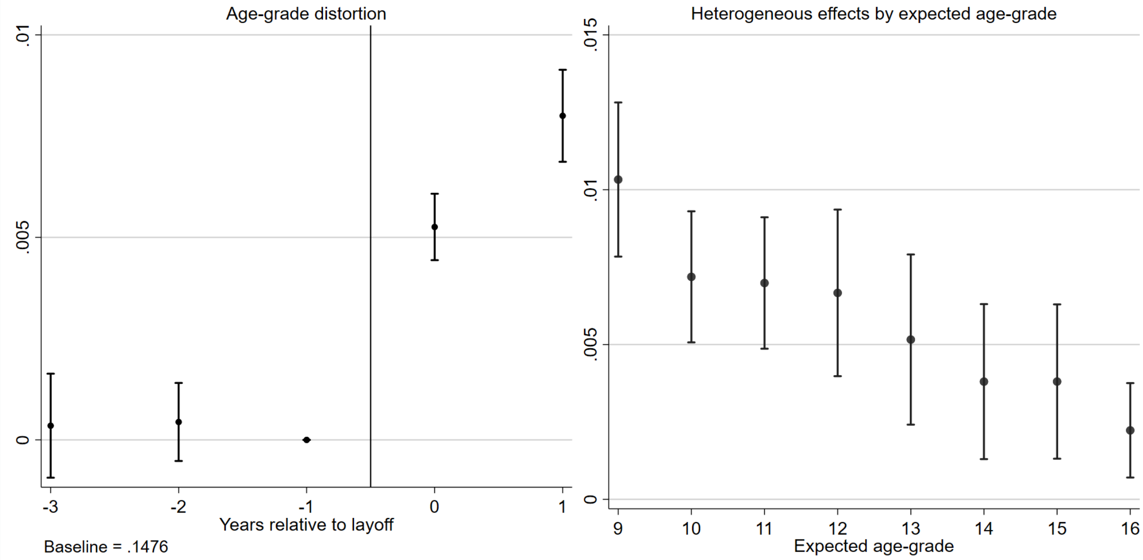


Notes: The figure shows the effect of parental job loss on children's school enrollment. The left graph presents the dynamic treatment effects as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The right graph shows the average treatment effect by expected age-grade in the post-treatment period, as estimated from equation (2). The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group.

Next, we study the impacts on age-grade distortion within children who do not dropout after job loss. We define age-grade distortion by children enrolled in a grade below the expected grade for their age. Selection out of school is not a prime concern since our estimates include individual fixed effects, ensuring that we compare the same children before and after the shock. The results in Figure 4, left graph, show a significant increase in age-grade distortion amounting to up to .5 percentage points (p.p.) in the layoff calendar year and .8 p.p. in the following year, equivalent to a 3% and 5% increase over the baseline distortion rate. Differently from the impact on enrollment, the adverse effect is concentrated on younger children, as shown in the right graph of Figure 4.

Appendix Table B1 summarizes the effects on parental employment, enrollment, and age-grade distortion in the two years following job loss. It also shows that the effect on age-grade distortion remains similar when restricting the sample to children enrolled throughout the entire analysis period (column 5). The latter indicates that grade retention is the main driver of the effect on grade distortion, as opposed to children dropping out and returning to school with grade lags. The larger impact on age-grade distortion for younger children is in line with the fact that mandatory schooling rules are more binding at younger ages (Figure 1, Panel A). For children in secondary schooling age, where dropout risk is higher and compulsory schooling laws are less effective, parental job significantly reduces school enrollment.

Figure 4: Effect of parental job loss on age-grade distortion



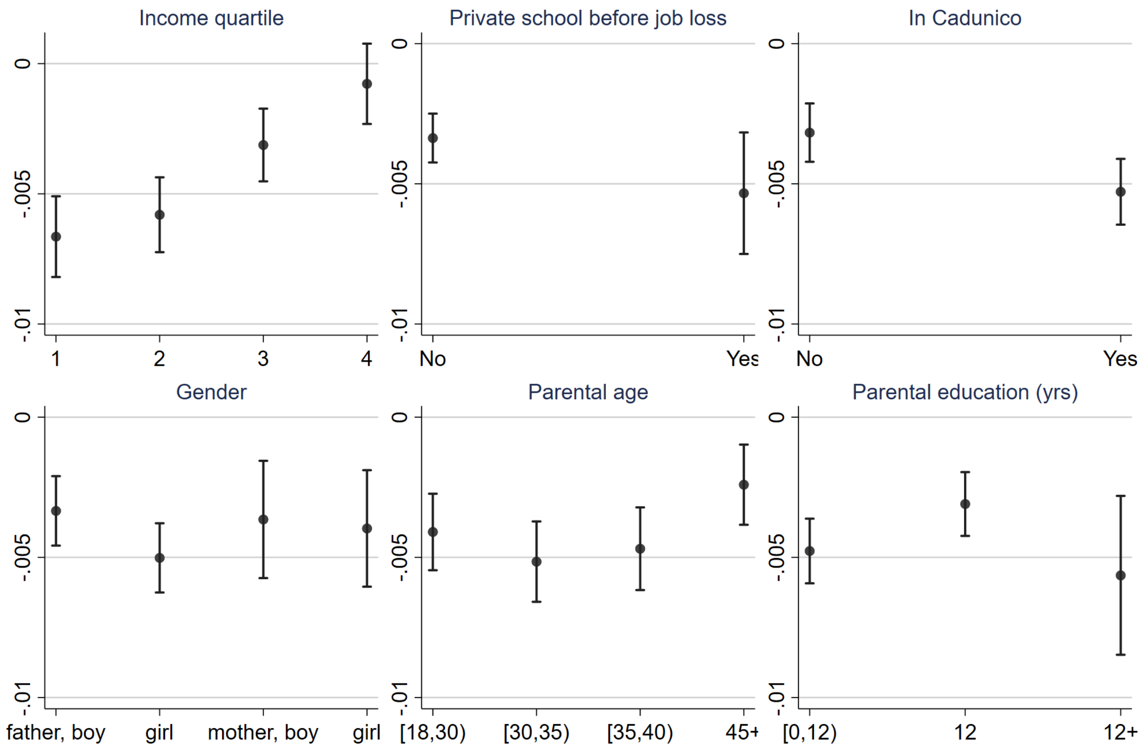
Notes: The figure shows the effect of parental job loss on children's age-grade distortion. The left graph presents the dynamic treatment effects as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The right graph shows the average treatment effect by expected age-grade in the post-treatment period, as estimated from equation (2). The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group.

4.4 Heterogeneity

We now investigate heterogeneous treatment effects of job loss on educational outcomes. Figure 5 shows that the impact on school enrollment is pervasive, affecting most groups in our data. There is a significant gradient over pre-displacement income (top-left graph), which is the strongest among the several characteristics we analyze and suggests that liquidity constraints may be an important explanation for the effect we document. Consistent with this, the effects are stronger in families with low income and in *CadÚnico* welfare registry, whereas families in the upper income quartile are not affected. Although the effects are pervasive, a clear gradient does not emerge over gender, parental age, and education.[24] Appendix B.4 reports similar patterns for the effect on age-grade distortion and shows that parental job loss effects are pervasive over several area-level characteristics, despite the large socioeconomic disparities observed across the Brazilian territory.

[24]Although coefficients are not statistically different, the evidence suggests a U-shaped effect over education, the impact being smaller for parents with completed secondary education with respect to those with primary and college education. The initial negative gradient can be explained because the income is likely smaller for more educated parents. Interestingly, the larger effect for college-educated parents could hint at a mechanism where parental job loss is taken as a signal for returns to education. Specifically, the layoff of college-educated parents could be interpreted as a signal of low returns to education and induce children's dropout.

Figure 5: Effect of parental job loss on school enrollment, heterogeneity analysis



Notes: The figure shows the effect of parental job loss on children's school enrollment, after splitting the sample by several characteristics, as estimated from equation (2) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

Interestingly, the effect for boys and girls is roughly similar when either the father or the mother is displaced (Figure 5, bottom-left graph). *A priori*, one could expect parents to change their allocation of time between home chores and market production, in line with the household production model (Becker, 1993). For instance, non-displaced parents could increase their labor market supply, while displaced parents could allocate more time to home production and invest more in their children. If parental skills on each of these tasks are heterogeneous, the effect on children's educational outcomes would significantly vary with the gender of the displaced parent, in contrast to our evidence showing similar effects across parental and child genders. Moreover, this hypothesis does not square well with the evidence in Figure 2 indicating that added worker effects are small and economically irrelevant for both genders.

4.5 Robustness

In Appendix Section B.3, we discuss in detail several threats to our identification strategy, which we briefly describe here. First, we address selection concerns by showing that our main findings are robust when the scope for selection of workers into job loss is severely reduced. Specifically, our results remain quantitatively similar when focusing on larger mass layoffs compared to our baseline – e.g., events where at least 75% of the workers are displaced – or when using plant closures. Also, our estimates continue to hold when we adopt an intent-to-treat approach in which the treatment group comprises all workers employed (displaced or not) in treated firms at the beginning of each mass layoffs calendar year. This strategy mitigates concerns about workers anticipating mass layoffs. Second, we show that our findings remain robust when using alternative control groups – namely, workers continuously employed throughout the entire analysis period rather than during the mass layoff calendar year – ;[25] or when adding flexible municipality × 2-digit industry × time fixed effects. The latter indicates the ability of our empirical strategy to net out the effect of job loss by comparing parents and children who face similar area level conditions. Third, we discuss in detail the timing patterns of the effects that emerge already during the mass layoff calendar year and provide additional robustness tests. Fourth, we address concerns related to the staggered timing of layoffs by showing that no negative weights emerge in our setting and that our findings are robust to other estimators proposed in this literature. Finally, we also address concerns regarding the external validity of our analysis since mass layoffs could significantly differ from regular layoffs. For instance, they may embody relevant spillovers effects across displaced workers or attract media attention which may magnify its effects on workers. Instead, we show that coefficient estimates remain similar when varying the total number of displaced workers within mass layoffs, indicating that mass layoff size is not a key factor explaining our findings.

4.6 Effects on additional outcomes

The results presented so far indicate that parental job loss has dire consequences to children's educational trajectory. We now leverage the richness of our data to analyze several additional important outcomes. These results will also shed light on the mechanisms driving the effect of parental job loss on children.

4.6.A Child work

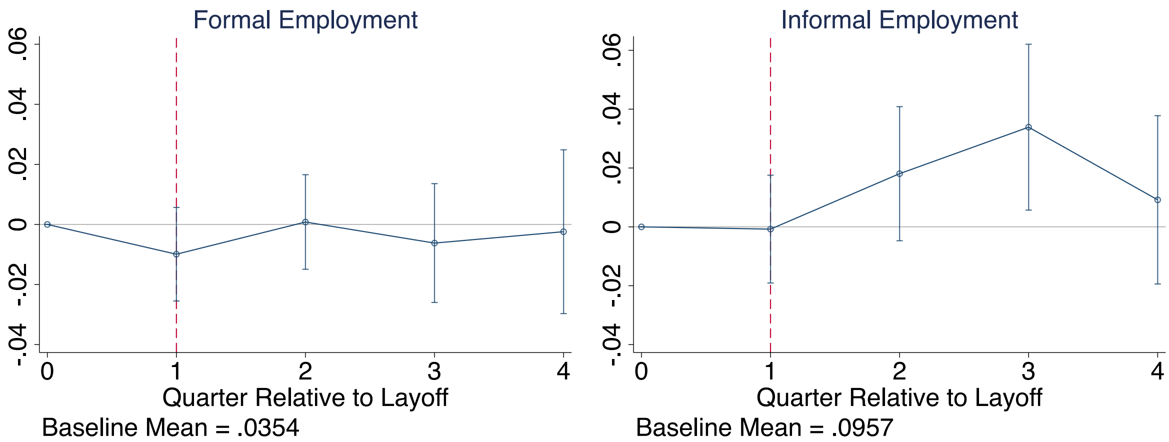
A potential factor driving the effects presented above is that children start working to compensate for the income losses in the family, which in turn could lead them to leave or perform worse at school. Similar to numerous other developing countries, about 13% of children 14-17 work in Brazil, mostly informally, and the necessity of work is the top reason (39%) for leaving school among dropouts in nationally representative surveys.[26]

[25] Previous work have used either of these two approaches; for instance, Ichino et al. (2017) and Schmieder et al. (2018) define the control group similarly to our baseline empirical strategy, while Jacobson et al. (1993) and Couch and Placzek (2010) restrict the control group to workers who are continuously employed throughout the entire sample period.

[26] Child work remains relevant phenomenon in the country even though children below 14 are not allowed to work by Brazilian law, either formally or informally, and several work restrictions apply to those aged 14-17. In addition, the 2010 Population Census indicates that 3.8% of children aged 10-13

To shed light on this aspect, we exploit longitudinal survey data interviewing families for five subsequent quarters tracking formal and informal employment outcomes for individuals from age 14 – the analysis follows the setting described in Appendix B.1. As shown in Figure 7, children aged 14-17 are more likely to work in the informal labor market following parental (formal) job loss when compared to children whose parents are not displaced in the same period. Although we cannot fully replicate our main analysis based on mass layoffs with these data, this evidence is suggestive of an income mechanism linking parental job loss and school outcomes, with dramatic consequences for children 14-17 who take jobs to compensate for income losses in the family. Even though the impacts on child employment are short-lived, they could be enough to trigger school dropouts or set children behind in classes, potentially leading to retention.

Figure 6: Effect of parental job loss on child work



Notes: The figure shows the dynamic treatment effects of parental job loss on child work as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals, based on PNAD household survey. The treatment group comprises workers displaced from a formal job in quarter 1, while the control group comprises formal workers who are not displaced throughout the entire period. The baseline mean indicates the average outcome in the pre-displacement year for the treatment group.

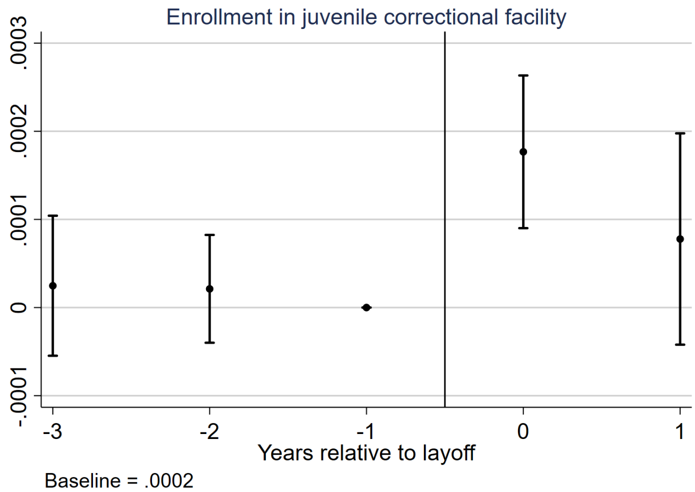
4.6.B Youth criminal behavior

In this section, we analyze if parental job loss affects students' criminal behavior. More specifically, we measure the probability that children aged 14-17 enroll in a school operating in a correctional facility, as observed in the School Census. As shown in Figure 7, parental displacement increases such probability. In line with the fact that there is substantial under-reporting in crime outcomes and that correctional facilities are an extreme measure for children who are repeated offenders or engage in serious crimes, the absolute effect is small in magnitude. However, the relative effect over the baseline probability of entering correctional facilities is substantial, amounting to a 63% increase in the post-treatment period. Although we cannot identify crime types, the

work.

fact that a large portion of crime is economically motivated is consistent with an income mechanism – economically motivated offenses account for 75% of criminal prosecutions for defendants aged 18.[27] Nevertheless, the increase in children's crime probabilities could also hint at a psychological mechanism related to stress, anxiety, and depression in the household, which we discuss later in more detail.

Figure 7: Effect of parental job loss on crime



Notes: The figure shows the dynamic treatment effects of parental job loss on the probability that children enroll in correctional facilities, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline mean indicates the average outcome in the pre-displacement year for the treatment group.

4.6.C School choices

We turn to the analysis on the impacts of parental job loss on school choices. This set of outcomes is arguably more directly related to parental choices, shedding light on parental investments mechanisms. School choices are particularly relevant in the context of low and middle-income countries where school quality is more heterogeneous compared to developed countries, and there is a substantial quality gap between private and public schools. We restrict again the analysis to children who do not dropout and analyze four different measures related to school quality. Namely, we track the school's average parental income and the INSE index based on parental socioeconomic background[28]; school ENEM scores – an important national examination taking place at the last year of secondary education[29]; and whether it is a public school.

[27]This statistics is based on data on criminal prosecutions for individuals above the legal age 18.

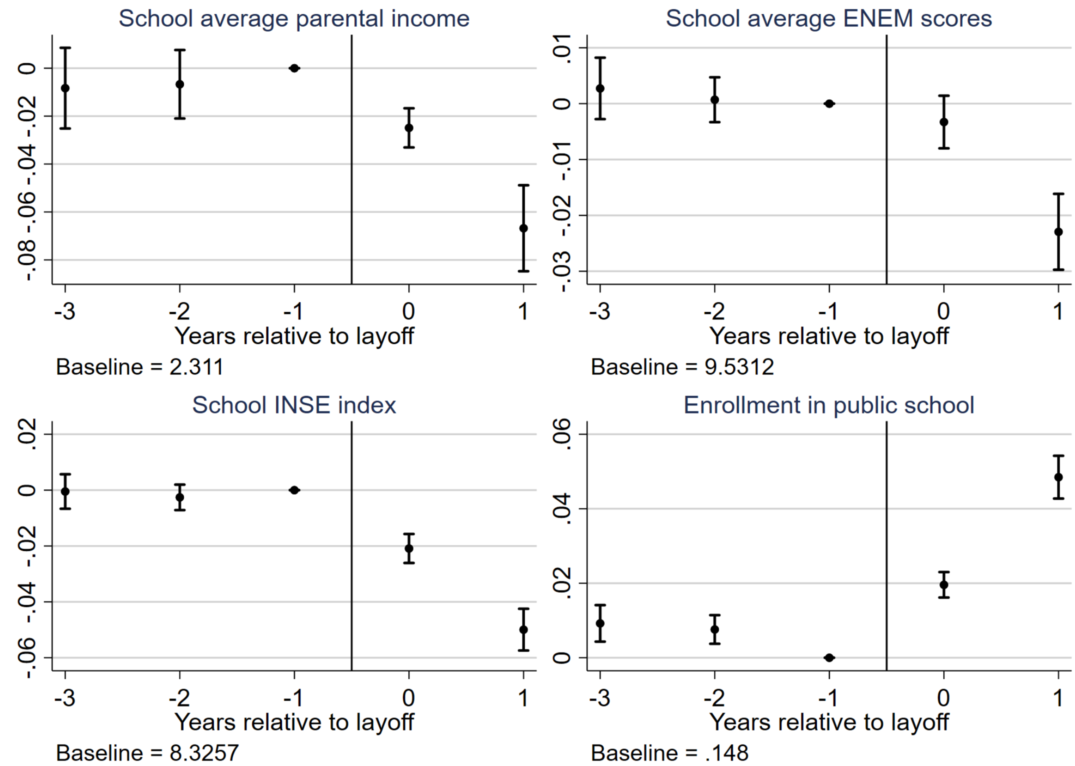
[28]The index is made available by the Ministry of Education and measures the school's socioeconomic background based on goods and services owned by the family, in addition to parental income and education. Additional details about the socioeconomic school index are available here.

[29]The exam determines access to several public and private universities in the country, including the best ones.

We focus on children who are enrolled in private schools prior to parental job loss, whose parents have a larger margin for adjusting school quality. These children come from more advantageous backgrounds in line with the evidence in Figure 2.1. The results, presented in Figure 8, show that parental job loss has significant negative impacts on school quality. In the year following parental job loss, affected children are up to 5 p.p. more likely to enroll in public schools; and move to schools where average parental income, ENEM scores, and INSE index are up to .06, .02, and .05 standard deviations lower. Appendix B.3 provides several robustness exercises on these results. In turn, we report heterogeneous treatment effects on school average parental income in Appendix Section B.6 after estimating the model in the full sample. Although most groups experience a reduction in school quality, the effect is strongly concentrated on children previously enrolled in private schools and high-income and education parents.

Overall, our results indicate that parents resort to costly insurance mechanisms which sacrifice the education quality of the next generation to deal with the income shocks caused by job loss. Different from poorer students whose option is to dropout and work, the effects we document here are, as expected, mainly driven by wealthier students.

Figure 8: Effect of parental job loss on school quality, children enrolled in private school before job loss



Notes: The figure shows the dynamic treatment effects of parental job loss on children's school quality measured by average parental income (top-left), average ENEM scores (top-right), school INSE index (bottom-left), and public school enrollment (bottom-right), as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline mean indicates the average outcome in the pre-displacement year for the treatment group.

4.7 Potential mechanisms

The evidence that children's labor supply and crime, and parental investments in school quality are negatively affected by job loss indicates that income may be a prime mechanism explaining our results. First, increases in labor supply support the idea that children work more to compensate for the income losses brought by job loss in the family. Second, increases in crime probabilities support a similar hypothesis, given that most youth crimes are economically motivated. Third, the negative effects on school quality indicate that parents adopt to costly insurance mechanism to absorb the economic shock brought by job loss. In addition, the income mechanism could also be the driving force explaining the observed gradient over the effects on school enrollment and age-grade distortion. In Appendix B.5, we complement this evidence by following a similar exercise to Hilger (2016). We show that predicted income losses after layoff are a strong predictor of the job loss effects on enrollment and age-grade distortion. In Section 5, we will use variation in access to unemployment benefits to provide further evidence on the role of the income mechanism.

We now discuss several other mechanisms that could potentially explain the effects of parental job loss on school outcomes that we find. We start with family rupture. More specifically, job loss may induce parents to divorce, possibly causing stress in the household, changing each parents' time investment in the child, or creating further financial constraints. Although we do not have data on separations for the entire sample, we study job loss effects on the probability that parents are employed in different municipalities or states in the full sample, as a proxy for parental separation.[30] In addition, for poorer families present in *CadÚnico*, we directly study the probability that both parents live with the child as an outcome. The results presented in Table 2 indicate only small effects on separation probabilities. Finally, the effects on enrollment and age-grade distortion remain similar to the main specification after restricting the sample to children in stable couples – i.e., children for whom we find no evidence of parental separation based on the measures in columns 1-3. Overall, we do not find much support for the hypothesis that family rupture is a key driver of our main findings.

Table 2: The effect of parental job loss on family rupture

	(1)	(2)	(3)	(4)	(5)
Dependent var.:	Parents work in different Municipality	State	Child live with both parents	School Enrollment	Age-grade Distortion
Parental job loss effect	0.0081*	0.005	0.018***	-0.0048***	0.0060***
	(0.003)	(0.003)	(0.001)	(0.0007)	(0.0007)
Sample	Full	Full	Full	Stable couples	Stable couples
Relative Effect	1.5%	2.8%	5.4%	-0.5%	4.1%
Baseline Mean	0.54	0.18	0.33	0.92	0.15
Observations	962,119	962,119	3,114,017	2,674,920	2,554,521

Notes: The table shows the effect of parental job loss on family rupture outcomes (columns 1-3), school enrollment, and age-grade distortion (columns 4-5), as estimated from the difference-in-differences equation (2). Outcomes in columns 1-2 are based on formal employment data, whereas the outcome in column 3 is based on *CadÚnico*. Stable couples is a sample for whom we find no evidence of parental separation based on the outcomes in columns 1-3 for the entire analysis period. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group. Standard errors clustered at the firm level are displayed in parentheses (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

We further study the role of neighborhood choice as a mediating mechanism. More specifically, we track average parental income in the residence location at the postal code level for children present in *CadÚnico*. Although the same measure is not available for the entire sample, we track key characteristics of the municipalities where parents work – GDP p.c., population and inequality, as a proxy for location choices. This evidence is

[30]This analysis is based on 84% of the children in our final sample for whom we can identify both parents' tax id, and conditional on both parents being employed.

presented in Table 3 and does not show substantial changes in any of these measures (columns 1-4). Finally, columns 5-6 shows that the effects on enrollment and age-grade distortion remain similar to the baseline after restricting the sample to individuals for whom we find no evidence of location changes. Overall, the evidence does not support the idea that location changes are a relevant mechanism in our context.

Table 3: The effect of parental job loss on location characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var.:	Parental municipality of work			Postal Code	School	Age-grade
	Gdp pc.	Population	Gini index	Labor income	Enrollment	Distortion
Parental job loss effect	-432	-10613.9	-0.00087*	0.016***	-0.0055***	0.0069***
	(237.6)	(26113)	(0.0004)	(0.002)	(0.0005)	(0.0006)
Sample	Full	Full	Full	*CadÚnico*	Stable location	Stable location
Relative Effect	-1.6%	-0.5%	-0.1%	0.2%	-0.6%	4.0%
Baseline Mean	26963.34	1959846.77	0.65	8.22	0.92	0.17
Observations	5,139,601	5,139,601	5,139,601	2,637,848	5,058,205	4,833,899

Notes: The table shows the effect of parental job loss on the characteristics of the municipality of work by parents (columns 1-3), average formal labor income in the residential postal code (column 4), children's school enrollment, and age-grade distortion (columns 5-6), as estimated from the difference-in-differences equation (2). Outcomes in columns 1-3 are based on formal employment data, whereas the outcome in column 4 is based on *CadÚnico*. Stable location is a sample for whom we find no evidence of location changes based on the outcomes in columns 1-4 for the entire analysis period. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group. Standard errors clustered at the firm level are displayed in parentheses (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

We now discuss the role of psychological mechanisms, since parental job loss could lead to stress, anxiety, and depression in the family. Although our data do not allow us to test psychologically-related outcomes, these responses find support in related literature linking job loss to mental health problems and stress (Kuhn et al., 2009; Charles and DeCicca, 2008; Zimmer, 2021). If such mechanisms were driving the worsening in school outcomes and job loss itself caused psychological distress, one would expect a binding effect for most groups in our data. However, the evidence in Figure 5 and Appendix Figure B7 indicate a smaller shock on advantage families, and in particular a null effect for families in the upper quartile of the income distribution. This evidence suggests that if psychological mechanisms drive the effects of parental job loss, they are likely a mediating factor of the income shock.

4.8 Long-term effects

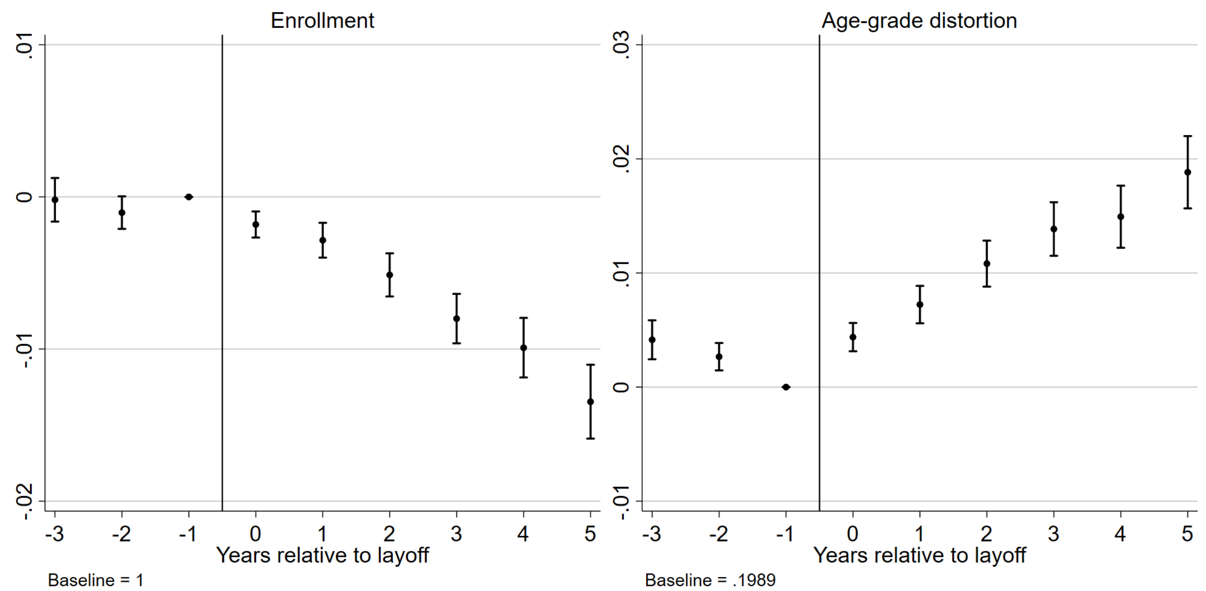
The analysis so far has focused on the impacts of parental job loss up to two years after the parental layoff. We now provide two different analyses shedding light on whether such effects persist over time and affect children's education over the long-run. First, we replicate the previous analysis on a sample of children in *CadÚnico* who can be followed in our data for up to six years after parental displacement. Second, we exploit the timing of parental job loss relative to children's expected high school graduation date to study long-term impacts on completed schooling.

4.8.A Effects on enrollment and age-grade distortion

In this subsection we replicate the same analysis described in Section 4.1 but for children in *CadÚnico*. Differently from our main sample, these data are not restricted to children enrolled in school in 2014, allowing us to focus on earlier mass layoff years and to track children's school outcomes for a longer period. More specifically, we run the analysis on parents displaced in mass layoffs in the period 2011-2012, creating a control group via exact matching as for the previous analysis, but analyzing school outcomes for up to six years after layoff (the School Census data is available for the period 2008-2017).

The results are presented in Figure 9. They show that parental job loss has a persistent impact on children's school enrollment and age-grade distortion. The effects on both outcomes are actually increasing over time up to six years after the layoff, which is consistent with the persistent job loss effects on employment outcomes. Six years after, they indicate a reduction of more than 1 p.p. in enrollment and an increase of about 2 p.p. in age-grade distortion. Overall, these estimates imply that parental job loss effects are not transitory and may have important negative consequences on children's long-run outcomes, such as completed education, which we investigate below.

Figure 9: Effect of parental job loss on enrollment and age-grade distortion



Notes: The figure shows the dynamic treatment effects of parental job loss on children's school enrollment and age-grade distortion, as estimated from equation (2) – along with 95% confidence intervals. The sample is based on children in *CadÚnico*. The treatment group comprises workers displaced in mass layoffs during 2011-2012, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group.

4.8.B Effects on high school graduation

To investigate the impacts of parental job loss on children's completed education, we implement a difference-in-differences design which compares children whose parents lost a job in mass layoffs before and after their (expected) high school completion year. The exercise follows a similar strategy to Hilger (2016) who studies the impact of parental job loss on college enrollment using US data. Since our school data only indicates children's enrollment in a given grade, we will proxy high school completion by children's enrollment in the last high school year – grade 12. We define children's expected high school completion year based on the school grade where they are observed three years before the parental job loss.[31] We select parents losing their jobs in mass layoffs during the period 2011-2013 and focus on children whose parental layoff takes place from four years before to three years after their expected high school completion date. Similar to our previous analysis, we assign a control parent-child to each treated parent-child via exact matching as described in Section 4.1.

We then implement the following difference-in-differences analysis comparing treated and control children's over the timing of the parental layoff relative to their expected high school graduation date:

[31]Our analysis based on expected high school completion date measured three years before job loss rather than over age is motivated by the fact that age-grade distortion and secondary school dropouts rate are large in Brazil.

$$Y_{it} = \sum_{t=-P, t\neq 1}^{T} \delta_t Time_t * Treat_i + \mu_i + \lambda_t + \epsilon_{it} \tag{3}$$

where the subscript i identifies each parent-child, our unit of analysis. The dummy μ_i identifies treated parent-child who lost a job during a mass layoff, whereas t identifies calendar years relative to children's expected high school graduation date. Our exact matching strategy ensures that the control group has the same expected graduation date in comparison to treated units. $Treat_i$ is an indicator for the treatment group and $Time_t$ identifies each period t. Time-varying shocks are absorbed by the full set of period fixed effects λ_t, and the coefficients $\{\delta_{-P}, ..., \delta_0\}$ identify dynamic treatment effects – indicating the effects on children whose parents lost a job before high school conclusion –, δ_1 being the omitted category. Differently from equation (1), we cannot include individual fixed effects since we leverage variation in layoff dates across individuals. Nevertheless, we show as a robustness that our results continue to hold when adding parental characteristics as controls and fixed effects interacting the calendar year of displacement with treatment status and municipality. Finally, we estimate the following equation to summarize the average treatment effects:

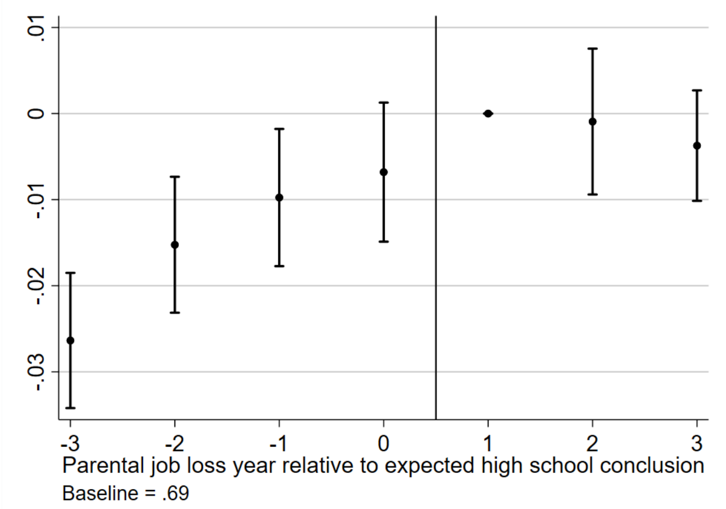
$$Y_{it} = \beta Post_t \times Treat_i + \mu_i + \lambda_t + \epsilon_{it}, \tag{4}$$

where $Post_t$ identifies the treatment period – i.e. layoffs taking place before the expected high school graduation, where β is the coefficient of interest identifying the average impact of parental job loss.

The results of the analysis are presented in Figure 10. It shows that children whose parents are displaced before expected graduation are less likely to conclude high school. Supporting the common-trend assumption, graduation rates do not significantly diverge for children whose parents lost a job one to three years after the expected graduation date. Table 4 shows that treated children are on average 1.5 p.p. less likely to complete high school. The same table shows that these estimates are not driven by compositional changes in the characteristics of displaced workers over time. Specifically, they are robust to controlling for parental characteristics – education, gender, income, and tenure – and to the inclusion of year of displacement X treatment status X municipality fixed effects.

Overall, this evidence indicates that parental job loss leads to relevant long-term losses for children. Although we cannot track long-term employment outcomes because children in our sample are too young, the impact on graduation rates are indicative of long-lasting persistent losses.

Figure 10: Long-term effect of parental job loss on high school completion



Notes: The figure shows the effects of parental job loss on the probability that children enroll in the last high school year (grade 12), as estimated from the difference-in-differences equation (3) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline mean indicates the average outcome for $t = 1$ in the treatment group.

Table 4: Long-term effect of parental job loss on high school completion

	(1)	(2)	(3)	(4)	(5)
PANEL A. Dependent var.: High School Completion					
Parental job loss effect	-0.015***	-0.014***	-0.014***	-0.014***	-0.015***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Layoff year FE		Y			
Layoff year X Municipality FE			Y		
Layoff year X Treat X Mun. FE				Y	
Layoff year X Treat X Mun. X Industry FE					Y
Relative Effect	-2.5%	-2.3%	-2.3%	-2.3%	-2.5%
Baseline Mean	0.61	0.61	0.61	0.61	0.61
Observations	963,814	963,814	963,814	961,280	929,166

Notes: The table shows the effect of parental job loss on on the probability that children enroll in the last high school year (grade 12), as estimated from the difference-in-differences equation (4). The baseline indicates the average outcome for $t = 1$ in the treatment group. Standard errors clustered at the firm level are displayed in parentheses (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

5 Unemployment Insurance and Children's Education

The results presented above establish a strong link between job loss and children's educational outcomes. In this section, we investigate the possible mitigating effects of unemployment insurance. The objective here is two-fold. First, to evaluate the policy. Second, this analysis will further illuminate on the role of income as mechanism.

5.1 Research Design

Unemployment insurance in Brazil covers formal workers displaced without a just cause, lasting from three to five months with a replacement rate of about 80%. The duration is comparable to that in most US states and shorter than in most European countries, whereas the replacement rate is generally higher than in both places. All workers displaced with at least six tenure months are eligible, except that repeated UI claims require a minimum 16-month waiting period between the layoff date used in each request. We exploit the later rule to identify the effects of UI eligibility using a regression discontinuity design that compares barely eligible and ineligible repeated claimants. Specifically, we compare workers displaced a few days before and after the 16-month waiting period. We estimate the following local linear equation:

$$Y_i = \alpha + \beta D_i + \gamma_1 X_i + \gamma_2 D_i * X_i + \epsilon_i \tag{5}$$

in which Y_i is an outcome for parent-child i. X_i indicates the time elapsed since the previous layoff leading to a successful UI claim, centered around the 16-month cutoff, whereas D_i is dummy indicating that the worker is eligible for UI ($X_i \geq 0$) and ϵ_i is the error term. β is the main coefficient of interest identifying the impact of UI eligibility. Our main local linear estimates are based on a narrow bandwidth which equals 60 days. We will show that our main findings are robust to varying polynomial orders and bandwidth choices – including the optimal ones proposed in Calonico et al. (2014); to permutation tests; and to manipulation robust inference proposed by Gerard et al. (2020) (even though no evidence of manipulation is detected in our data).

The working sample comprises full-time private working parents who are displaced for a second time 10 to 22 months after an initial layoff giving access to 3-5 months of UI benefits. We focus on layoffs in the period 2009-14, using data on UI payments for the same period to study UI take-up.[32] Because layoffs typically take place at the very beginning and the very end of each month – See Figure C1 in Appendix –, we drop from the sample workers whose 16-month cutoff date is within 3 days from the start and end of the month, so that our RD cutoff does not coincide with the dismissal cycle – which is not specific to the 16-month cutoff.[33]

Our main parent-child link used in the previous analysis (Section 4.1) is not well suited to study the impacts of UI on enrollment. This is because this sample is restricted to children enrolled in 2014, whereas the RD analysis is based on workers displaced in the period 2009-14. As a result, all children of displaced parents in our main

[32] Our research design is well suited for the period before 2015, as numerous changes were introduced to the UI system after that year. In addition, our data on UI payments end in 2014.

[33] It is worth noting that the 16-month cutoff date is determined by the initial layoff date giving access to unemployment benefits, which is pre-determined and thus not endogenous to the variation used in the RD analysis, based on the date of the subsequent layoff.

sample are enrolled in 2014, after parental displacement. For this reason, our main UI analysis is based on a sample of children in *CadÚnico* – the same sample used in Section 4.8.A, whose linkage to parents in the employment data is not conditional on school enrollment. In addition, we focus on displaced parents in the first three quartiles of the income distribution, for whom UI replacement rate is higher – 85% compared to 50% in the upper quartile.[34]

Appendix Figures C3 and C2 show that a rich set of predetermined characteristics of parents and children, and the running variable density function are continuous around the cutoff.

5.2 Effects on school enrollment and age-grade distortion

Table 5 presents our main estimates based on equation (5). Panel A shows that eligible parents are 66% more likely to take-up UI benefits which last on average for 2.8 months.[35] To study the impacts on children, we split the sample by grade, since parental job loss effects on enrollment are stronger for older children (Figure 3, Section 4.3), while the effects on age-grade distortion are concentrated on younger ones (Figure 4, Section 4.3). Panels B and C, columns 1-2, show that both outcomes were balanced prior to job loss, offering compelling evidence supporting the RD design. Panel B, column 4, shows that UI eligibility increases enrollment for older children by 1.7 p.p., while effects on younger children are small and not statistically significant. These results are in line with the fact that parental job loss effects on enrollment are small for the latter group. Figure 11 presents the graphical evidence on the impacts for older children, showing a clear discontinuity that emerges after the layoff. This finding is robust to varying bandwidths – including the optimal one by Calonico et al. (2014) – and local polynomial choices; to permutation tests where the main estimate is compared to distribution of RD estimates at placebo cutoff points; and to manipulation robust inference (see Appendix Tables C1 and C2, and Figure C4).

The positive impact of UI eligibility on enrollment supports the idea that income losses are a relevant mechanism driving the impacts of job loss on children's education. However, it is well known that UI eligibility induces workers to take longer to find jobs – as is the case in our data, see column 4 in Table 5 – and may increase the time which parents spend with children. Thus, an alternative hypothesis is that UI reduces dropouts because of parental time. We test for whether unemployment duration is a mediating factor by controlling for that in our main RD regression either linearly or with flexible fixed-effects for duration measured in weeks. Coefficient estimates barely change with the addition of these controls – both indicating a 1.8 p.p. (0.006 s.e.)

[34]The UI institutional rules set replacement rates at 100% for workers earning the minimum wage which continuously decreases over income.

[35]The take-up rate we find is similar to that presented in Gerard and Naritomi (2021).

statistically significant increase in enrollment –, suggesting that the increase in duration is not a key mediating factor and offering further support for the income mechanism.

Estimates on the UI impacts on age-grade distortion are reported in Panels B and C, column 3, showing non-statistically significant results. These estimates, however, are not particularly precise, and we may lack the power to identify meaningful effects comparable to the estimates presented in Section 4.[36]

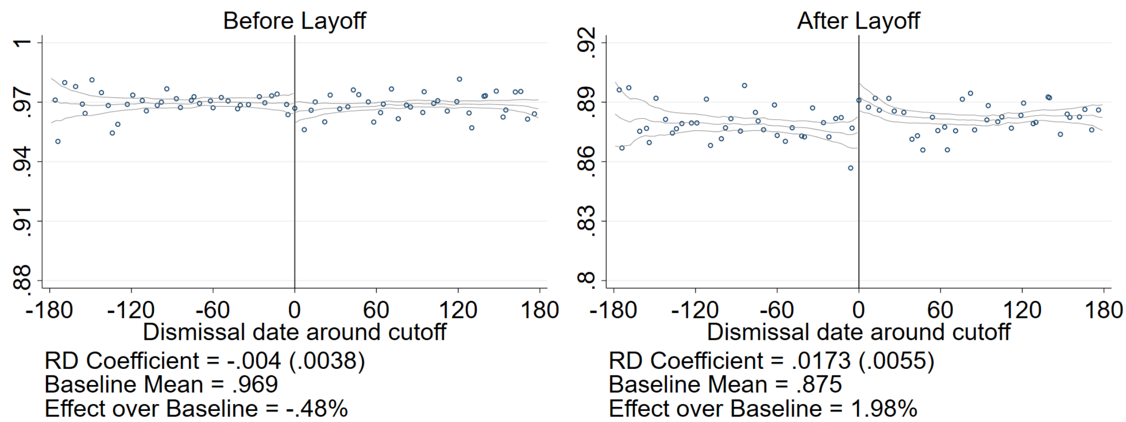
Table 5: The effects of UI eligibility

	(1)	(2)	(3)	(4)
PANEL A: FULL SAMPLE				
Dependent var.:	Unemployment Benefits			Unemployment
	Take-up	Months	Amount	Duration (wks)
UI eligibility effect	0.646***	2.721***	1838.023***	11.988***
	(0.006)	(0.024)	(16.918)	(0.924)
Baseline Mean	0.05	0.09	64.58	41.36
Observations	107,137	107,137	107,137	107,137
PANEL B: OLDER CHILDREN, AGE-GRADE 13-17				
Dependent var.:	Before layoff (placebo)		After layoff	
	Age-grade distortion	Enrollment	Age-grade distortion	Enrollment
UI eligibility effect	-0.005	-0.009	-0.012	0.017***
	(0.004)	(0.009)	(0.009)	(0.006)
Baseline Mean	0.97	0.22	0.27	0.88
Observations	35,817	34,686	38,712	41,396
PANEL C: YOUNGER CHILDREN, AGE-GRADE 6-12				
Dependent var.:	Before layoff (placebo)		After layoff	
	Age-grade distortion	Enrollment	Age-grade distortion	Enrollment
UI eligibility effect	0.004	-0.004	-0.003	0.002
	(0.003)	(0.006)	(0.006)	(0.003)
Baseline Mean	0.96	0.14	0.21	0.96
Observations	55,365	53,329	64,718	65,741

Notes: This table shows the effect of eligibility for UI benefits, as estimated from equation (5), on UI outcomes (Panel A, columns 1-3), unemployment duration (Panel A, column 4) and children's average school enrollment rates two years before and after parental layoff (columns 5-7). The sample includes displaced parents with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. The table also reports the baseline mean outcome at the cutoff. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

[36]Standard errors are 0.7 p.p. and 0.5 p.p. in the sample for younger and older children, which are somewhat large when compared to the impacts on grade distortion found in the previous section.

Figure 11: Effect of UI eligibility on enrollment, children in age-grade 13-17, before (placebo) and after job loss



Notes: The graphs plot children's average school enrollment rates two years before and after layoff around the cutoff date for parental eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

5.3 Effects on school choices

In Appendix Section C.4, we replicate the RD analysis on our main sample, used in Section 4 and conditional on school enrollment in 2014, to study UI impacts on school quality. Since parental job loss reduces school quality mainly for children in advantaged families, we restrict the sample to children initially enrolled in private schools (in line with Section 4.6.C). Although the sample is severely reduced by this restriction and the statistical power is not particularly high, the results in Appendix Table C3 offer some indication that UI eligibility could partially mitigate job loss effects on school quality. In Appendix Table C4, we assess the robustness of these findings. Although coefficient estimates are generally positive in sign and the impact on INSE index is relatively robust, they indicate that these findings should be interpreted with caution and that we may lack statistical power to draw stronger conclusions about the mitigating impacts of UI on school quality.

6 Discussion

Our analysis on parental job loss has shown that it imposes significant losses to children's educational outcomes. It generates persistent losses to parental employment and affects several key aspects of children's educational trajectories. More specifically, it reduces enrollment rates, increases age-grade distortion, and causes parents to reduce educational investments by moving children to lower-quality schools. Although these impacts are heterogeneous, they are fairly pervasive and affect, to a greater or lesser extent, most groups in our data – also being relevant to all combinations of parental and child genders.

The impact on enrollment is significantly larger for families with lower socioeconomic backgrounds – namely for parents present in welfare registries, with lower earnings, and in secondary schooling age. Based on survey data, we find that children 14-17 are more likely to work, taking informal jobs, once their parents lose their jobs. These findings are consistent with an income mechanism where children dropout from school to work. Additional evidence suggests that the income losses may also be compensated by an increase in crime by children, as parental job loss increases the likelihood that children are sent to correctional facilities. Although we cannot distinguish economic from other types of crimes, the former category responds to a substantial share of crimes, especially those committed by very young adults. Finally, our findings that eligibility to unemployment benefits mitigate such responses further support the income mechanism.

The analysis on age-grade distortion also indicates that effects are concentrated on families with lower socioeconomic backgrounds. These results reveal that even when children are kept in school, there are adverse consequences in terms of performance. The reduction in performance could be possibly driven by psychological factors such as stress, anxiety, and depression brought by the job loss. The stronger impacts on families with low socioeconomic backgrounds who suffer more from these shocks suggest that the psychological shock could mediate the income mechanism. This is in line with evidence linking job loss to worse mental health and higher stress, although we are not able to provide direct evidence on these aspects. The loss in performance could also be explained by the fact that children work more after a parental job loss – possibly reducing studying time and increasing fatigue.

Although children in our sample are young, and we cannot estimate long-term impacts on outcomes in adulthood, our analyses in Section 4.8 provide evidence that parental job loss has important and persistent consequences for children. In particular, job loss reduces children's enrollment up to over 1 p.p. and increases age-grade distortion up to 2 p.p., persisting for at least six years after displacement. Additional results indicate that parental job loss taking place during upper-middle school and high school reduces high school completion by 1.5 p.p..

Finally, the results on transitions to lower quality schools reveal that even children in advantaged families may experience relevant educational losses due to job loss. In fact, the effect is concentrated on families with wealthier backgrounds that are previously enrolled in higher-quality schools. They indicate that even such families resort to costly insurance mechanisms, which may have significant consequences for children's education.

7 Conclusion

Overall, our findings are especially relevant for the context of poor and middle-income countries where child work remains a pervasive phenomenon, and crime levels are typically high; and point at the lack of adequate social insurance policies. Nevertheless, the results on school choices made by parents reveal a mechanism where parents reduce investment in children's education which may be more general and extended to the context of richer countries.

In terms of policy relevance, we provide novel estimates on the effectiveness of social insurance policies in mitigating the adverse consequences of parental job loss. Although we do not find statistically significant effects for all educational outcomes, they reveal that unemployment benefits may be a successful policy to prevent some of the impacts of parent job loss on children. More generally, it suggests that other job insurance policies may be effective tools to alleviate liquidity constraints upon job displacement, such as mandatory severance pay. These results may be taken into account when implementing a broader cost-benefit analysis on social insurance policies, especially if parents do not fully internalize children's well-being.

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A Appendix to Section 3

A.1 Identifying parents and children in the School Census

To identify parents and children, we start by creating a person registry. We do so by combining all individuals ever observed in RAIS 2002-19 or *CadÚnico* 2011-2019 into a single registry containing their full name, unique tax code, and date of birth. This covers almost the totality of the Brazilian population (96% when compared to the Brazilian Population Census) and allows us to identify individuals who are uniquely identified either by their full name or by their full name and birth date.

Our goal is to identify parents and children in the School Census 2014, in which we observe children's full name, birth date and the municipality of birth, and parents' full names. We start by identifying children in the School Census 2014 with their tax id, proceeding in rounds. First, we identify students present in *CadÚnico* 2008-13, for whom we directly observe a link between their student and tax id – this is based on an additional dataset provided by the Ministry of Citizenship. Second, we identify those children who can be uniquely identified by their full names and birth dates in our person registry. Third, we identify those who can be uniquely identified only by their full names in our person registry. The later procedure is aided by the fact that Brazilian have multiple surnames and, as a result, above 50% of the population have a unique full name. This allows us to identify 42,891,100 million students in the School Census 2014 with their tax code, 95% of the total. This allows us to track these children in *CadÚnico* to study household characteristics over time.

Next, we identify their parents with their unique tax ids. First, we identify parents of children in *CadÚnico* for whom we can directly observe their parent in the household or uniquely identify the parent by her/his full name available on *CadÚnico*. Second, we identify those parents who can be uniquely identified by their names in our person registry. This allows us to identify 31,842,680 fathers and 42,891,100 mothers with their tax codes, 71% and 82% of the total in the School Census 2014, respectively. In Table A1, we show that the characteristics of children in compulsory schooling age who we can and cannot link to their parents are similar, indicating that our analysis sample is fairly representative of the population.

Table A1: Descriptive statistics, children with and without linked parents, School Census 2014

	(1)	(2)	(3)
	Linked parent	Non-linked parents	Std. Diff.
Child characteristics			
Age	12.2	11.6	0.16
Gender	0.49	0.49	0.00
Age-grade	11.1	10.7	0.12
School parental income	1,987.3	2,226.0	-0.17
School ENEM score	5,072.3	5,233.7	-0.30
Municipality characteristics			
Population	831,995	759,725	0.04
Pib per capita	16,490	16,814	-0.02
Gini index	0.62	0.62	0.03
Labor informality	0.46	0.46	0.00
Homicide rate	34	33	0.06

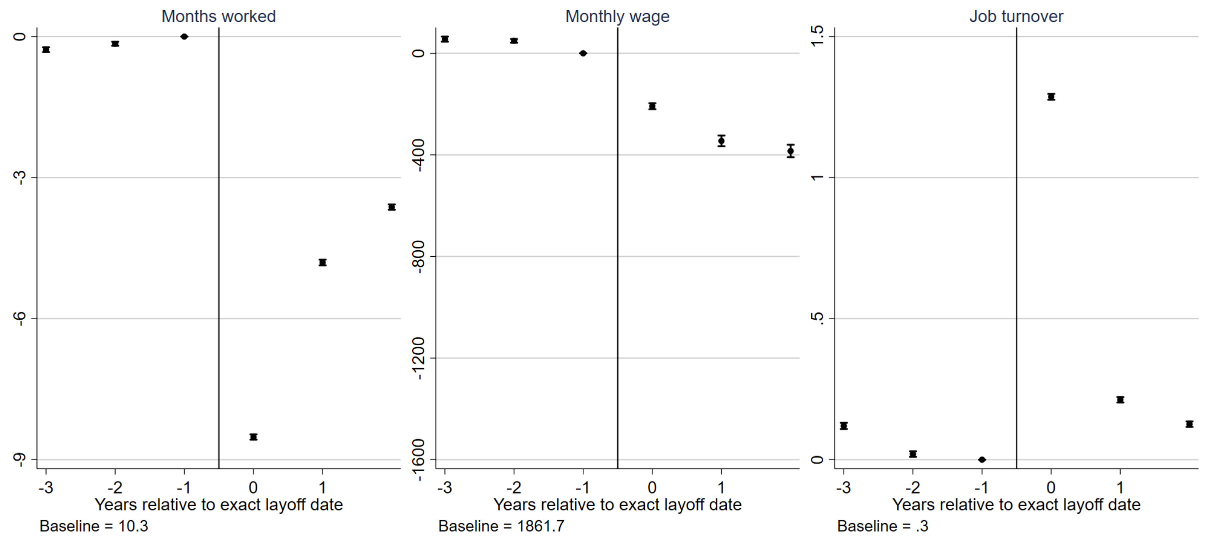
Notes: This table reports the average characteristics for children whom we successfully link the tax code for at least one parent in the School Census 2014 (column 1); the remaining children (column 2); and the standardized difference between the two groups (column 3).

B Appendix to Section 4

B.1 The effect of job loss on labor market outcomes

Our main analysis shows substantial and persistent losses in formal labor income following job loss. Figure B1 shows that job loss worsens several (formal) employment outcomes such as employment, wages, and turnover.

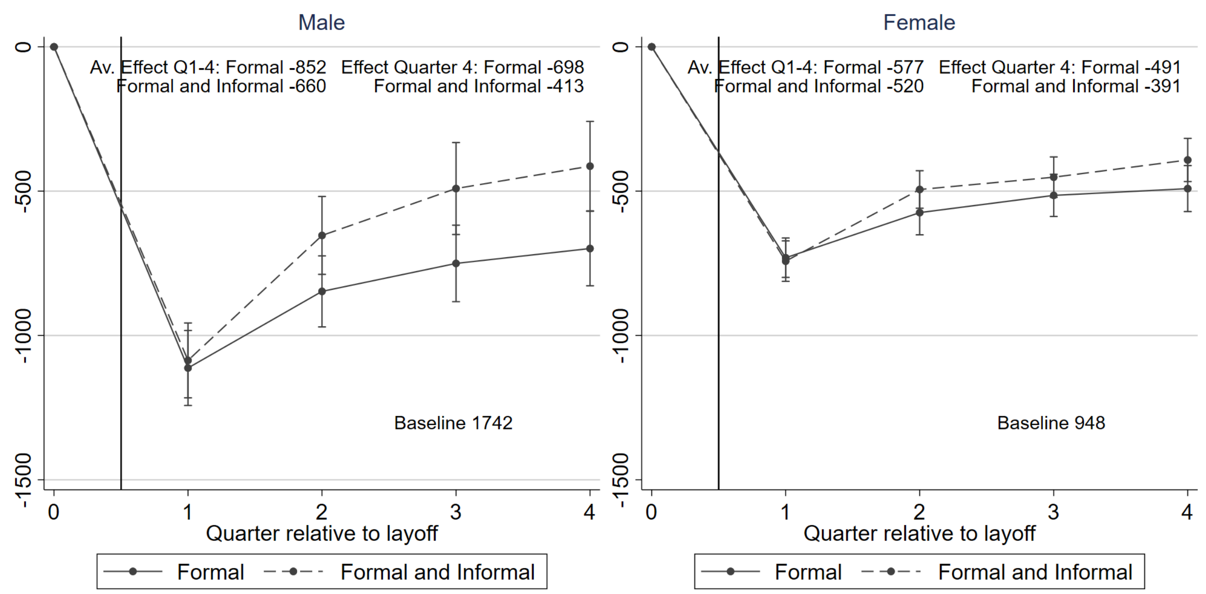
Figure B1: Effect of parental job loss on formal employment outcomes



Notes: This figure shows the dynamic treatment effects of parental job loss on other labor market outcomes, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises displaced workers, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group.

Because labor informality is high in Brazil, the effective drop in total employment could be substantially smaller than in the previous estimates because many displaced workers in our data may take informal jobs – which are not observed in the main analysis. We use data from PNAD – a large-scale representative household survey covering about 400,000 individuals each year – to study the impact of job loss on formal and informal employment outcomes in the period 2012-19. Families are interviewed for five subsequent quarters in the longitudinal survey. Similarly to our main analysis, we implement a different-in-differences design where the treatment group is composed of individuals who are initially formally employed in interview quarter 1 and who were displaced in quarter 2; whereas the control group is composed of workers employed in both quarters. In addition, we implement the same sample restrictions as in our main analysis – parents in the age range 18-60 years old initially formally employed in private firms. The results in Figure B2 indicate that employment losses become only about 20% and 10% smaller for men and women when taking into account the take-up of informal jobs.

Figure B2: Effect of parental job loss on formal and informal labor market outcomes



Notes: The figure shows the effect of job loss on formal and informal labor income (along with 95% confidence intervals) by gender as estimated from the difference-in-differences equation (1), based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. The sample covers individuals first interviewed in the period 2012-19. The treatment group is defined by workers who are formally employed in the first interview and out of employment in the second interview; the control group is composed of workers who are formally employed on the first and second interviews. Earnings are measured in Brazilian Reais. Baseline average values for the treated group at $t = 0$ are also reported.

B.2 The effect of parental job loss on parental income and children's educational outcomes

Table B1: The effect of parental job loss on parental income and children's educational outcomes

	(1)	(2)	(3)	(4)	(5)
Dependent var.:	Parental labor income		School	Age-grade	Age-grade
	Job loser	Other parent	Enrollment	Distortion	Distortion
Parental job loss effect	-10900.7***	-186.8***	-0.0041***	0.0060***	0.0060***
	(121)	(15.3)	(0.0004)	(0.0005)	(0.0006)
Sample	Full	Full	Full	Full	Always enrolled
Relative Effect	-54%	-5%	0.44%	3.4%	3.5%
Baseline Mean	20081.77	4087.58	0.92	0.18	0.17
Observations	7,887,500	6,648,350	7,887,500	7,531,907	6,554,820

Notes: This table shows the effect of parental job loss on parental income (columns 1-2), and children's educational outcomes (columns 3-5), as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes workers displaced in mass layoffs who are matched to control workers employed in the control group. Standard errors clustered at the firm level are displayed in parentheses (*** p ≤ 0.01, ** p≤0.05, * p≤0.1).

B.3 Robustness

B.3.A Selection issues

We address several potential concerns regarding our main results. We start by addressing potential selection into layoffs, even within mass layoffs. For instance, workers experiencing family issues may be more likely to have children with declining school performance and, at the same time, be more likely to be dismissed than other co-workers during mass layoffs. This would drive a spurious correlation between children's poor school performance and mass layoff status, biasing our analysis. We address such concerns in several ways. First, in Table B2, we show that our estimates remain similar when focusing on firms displacing a higher share of workers, compared to the 33% in our baseline specification, and on firms completely closing. The latter essentially eliminates the scope for selection.

Table B2: The effect of parental job loss on school outcomes, varying mass layoff intensity

	(1)	(2)	(3)	(4)	(5)
PANEL A: DEP. VAR. ENROLLMENT					
Parental job loss effect	-0.0041***	-0.0031***	-0.0036***	-0.0028***	-0.0024**
	(0.0004)	(0.0007)	(0.0008)	(0.0007)	(0.0009)
Mass layoff sample	≥ 33%	≥ 50%	≥ 75%	≥ 100 workers	≥ 250 workers
Observations	7,887,500	4,005,630	2,086,380	4,136,590	2,789,190
PANEL B: DEP. VAR. AGE-GRADE DISTORTION					
Parental job loss effect	0.0060***	0.0043***	0.0059***	0.0050***	0.0043***
	(0.0005)	(0.0007)	(0.0009)	(0.0008)	(0.001)
Mass layoff sample	≥ 33%	≥ 50%	≥ 75%	≥ 100 workers	≥ 250 workers
Observations	7,531,907	3,818,004	1,994,796	3,943,471	2,656,368
PANEL C: DEP. VAR. SCHOOL AV. PARENTAL INCOME					
Parental job loss effect	-0.040***	-0.046***	-0.042***	-0.051***	-0.053***
	(0.003)	(0.005)	(0.005)	(0.004)	(0.006)
Mass layoff sample	≥ 33%	≥ 50%	≥ 75%	≥ 100 workers	≥ 250 workers
Observations	1,060,190	525,340	300,448	567,071	390,971
PANEL D: DEP. VAR. INSE INDEX					
Parental job loss effect	-0.032***	-0.038***	-0.033***	-0.043***	-0.046***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
Mass layoff sample	≥ 33%	≥ 50%	≥ 75%	≥ 100 workers	≥ 250 workers
Observations	643,314	309,928	185,760	335,326	227,417
PANEL E: DEP. VAR. ENEM SCORES					
Parental job loss effect	-0.013***	-0.020***	-0.013**	-0.017***	-0.017***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
Mass layoff sample	≥ 33%	≥ 50%	≥ 75%	≥ 100 workers	≥ 250 workers
Observations	497,683	235,898	145,458	255,855	172,216
PANEL F: DEP. VAR. ENROLLED IN PUBLIC SCHOOL					
Parental job loss effect	0.027***	0.028***	0.024***	0.032***	0.033***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Mass layoff sample	≥ 33%	≥ 50%	≥ 75%	≥ 100 workers	≥ 250 workers
Observations	1,151,320	573,295	324,785	618,535	427,515

Notes: This table shows the average treatment effect of job loss on school outcomes, as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. The sample is restricted to (1) mass layoffs of at least 33% of the workforce, (2) ≥ 50%, (3) ≥ 75%, (4) at least 100 workers, and (5) at least 250 workers. Panels A-B are based on the full sample, whereas Panels C-F are based on children enrolled in private schools before parental job loss. The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes workers displaced in mass layoffs who are matched to control workers employed in the control group. Standard errors clustered at the firm level are displayed in parentheses (*** p ≤ 0.01, ** p≤0.05, * p≤0.1).

We provide yet another test addressing selection issues following an intention-to-treat approach. Instead of defining the treatment group by workers displaced in mass layoffs, we consider as treated all workers employed in mass layoff firms at the beginning of the calendar year and replicate our empirical strategy. This strategy also addresses the fact that some workers may anticipate the mass layoffs by quitting in advance, further reducing the scope for selection. Table B3 presents the results. The estimates are in line with our main results, supporting the robustness of our main findings.

Table B3: The effect of parental job loss on school outcomes, intention-to-treat approach

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent var.:	Labor Income	Enrollment	Age-grade distortion	School av. parental income	INSE index	ENEM score	Enrollment Public school
PANEL A: ALL WORKERS IN MASS LAYOFF FIRMS VS. NON-DISPLACED CONTROL WORKERS							
Parental job loss effect	-6678.1***	-0.0031***	0.0039***	-0.022***	-0.016***	-0.0047**	0.013***
	(85.2)	(0.0004)	(0.0004)	(0.002)	(0.002)	(0.002)	(0.001)
Relative Effect	-31%	-0.34%	2.43%	-0.87%	-0.19%	-0.05%	9.76%
Baseline Mean	21818	0.92	0.16	2.52	8.43	9.59	0.13
Observations	16,055,530	16,055,530	15,358,078	2,482,047	1,559,947	1,241,312	2,678,290

Notes: This table shows the average treatment effect of job loss on school outcomes, as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. Columns 1-3 are based on the full sample, whereas Columns 4-7 are based on children enrolled in private schools before parental job loss. The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes all workers in mass layoff firms – displaced and not – who are matched to control workers employed in the control group. Standard errors clustered at the firm level are displayed in parentheses (*** p ≤ 0.01, ** p≤0.05, * p≤0.1).

B.3.B External validity: mass vs. regular layoffs

We also address the concern that our findings are specific to the context of mass layoffs and may not be informative about general layoffs. For instance, mass layoffs may attract more media attention and generate more discontent and stress in the household, or spillovers across displaced co-workers could play a role. We address this issue by exploiting the fact that our sample covers both low and high-intensity mass layoffs, as measured by the number and share of displaced workers. Arguably, the external validity concerns described above should be more relevant in high-intensity mass layoffs. However, this is not consistent with the findings in Table B2 showing that high-intensity mass layoffs produce similar effects. The same table also shows that estimates remain similar when firms displace many workers in the same event, suggesting again that mass layoff intensity is not a key driver of the impacts.

B.3.C Area-level fixed effects and alternative control group

We show that our main estimates are robust to the addition of fine municipality (5,570) X 2-digit industry X time fixed effects, as shown in Table B4. They indicate that our matching strategy finely compares parents and children facing a similar environment, so that our estimates remain robust once we include flexible fixed-effects capturing changes in are area-level conditions over time, such as labor market and school environment conditions.

In addition, we show that our findings are also robust to the choice of the control group. While some papers in the literature define the control group by workers not displaced in the same calendar year (Britto et al., 2021; Ichino et al., 2017; Schmieder et al., 2018), similar to our case, others define the control group by workers who are not displaced throughout the entire panel (Couch and Placzek, 2010; Sullivan and Von Wachter, 2009). We show that such choice has no impact on our estimates – see Table B4.

Table B4: The effect of parental job loss on school outcomes, additional fixed effects, and alternative control group

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var.:	Enrollment	Age-grade distortion	School av. parental income	INSE index	ENEM score	Enrollment Public school
PANEL A: ADDITIONAL MUNICIPALITY X 2-DIGIT INDUSTRY X TIME FIXED EFFECTS						
Parental job loss effect	-0.0026***	0.0031***	-0.034***	-0.025***	-0.010**	0.021***
	(0.0005)	(0.0006)	(0.004)	(0.003)	(0.004)	(0.002)
Mass layoff sample	≥ 33					
Municipality X Industry X Time FE		Y				
Observations	7,821,110	7,465,487	1,018,272	605,812	463,760	1,108,945
PANEL B: ALTERNATIVE CONTROL GROUP, CONTINUOSLY EMPLOYED WORKERS						
Parental job loss effect	-0.0049***	0.0069***	-0.044***	-0.031***	-0.016***	0.024***
	(0.0005)	(0.0006)	(0.003)	(0.003)	(0.003)	(0.002)
Mass layoff sample	≥ 33					
Municipality X Industry X Time FE		Y				
Observations	4,956,250	4,739,315	714,966	436,918	340,591	774,945

Notes: This table shows the average treatment effect of job loss on school outcomes, as estimated from the difference-in-differences equation (2x), with additional fixed effects (Panel A) and restricting the control group for continuously employed workers (Panel B). The dependent variable is indicated on top of each column. The dependent variable is indicated on top of each column. Columns 1-2 are based on the full sample, whereas Columns 3-6 are based on children enrolled in private schools before parental job loss.The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. All regressions include individual and year fixed effects. The sample includes workers displaced in mass layoffs who are matched to control workers employed in the control group. Standard errors clustered at the firm level are displayed in parentheses (*** p ≤ 0.01, ** p≤0.05, * p≤0.1).

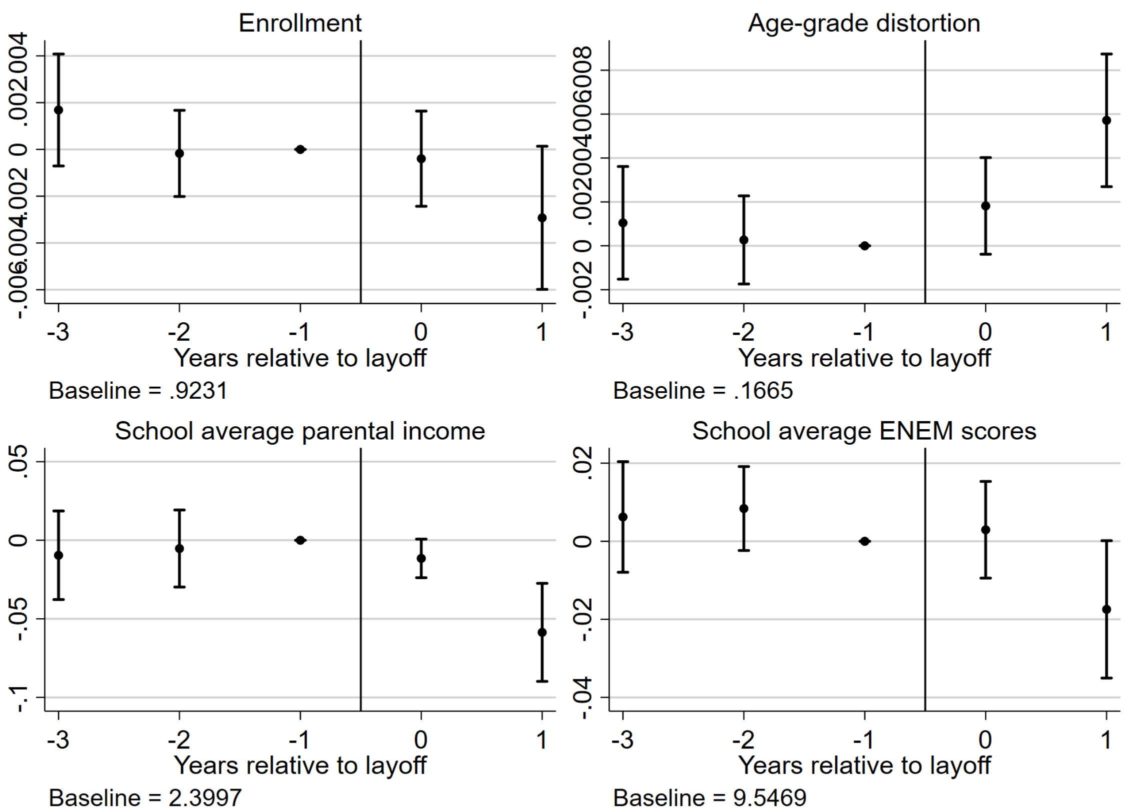
B.3.D Timing of the effects

Our estimates follow calendar years because we do not observe the exact timing when students dropout from school, rather we only observe whether they are enrolled in some given year. Although schools should file information about students enrolled on May 31^{st}, there is evidence that they do not precisely follow these instructions. Specifically, it is not uncommon to observe students who change schools during the year showing up twice in the data, enrolled in two different schools. This indicates that several schools file information for students enrolled after the cutoff date and may also file information for students who left the school before such date.

In addition, two factors are important for explaining the fact that our effects emerge during the same calendar year as the parental layoff. First, displaced parents learn about the job loss from 30 to 90 days in advance due to a mandatory advanced notice period, and it is not uncommon that they are released from their jobs upon notice. Second, public school strikes – where over 85% of children in our data are enrolled – are fairly common. This implies that the academic year – planned to go from January to December each year –, may exceed the calendar year to recover class days lost during strikes, and often go through one or more initial months of the subsequent calendar year. We collect data on school strikes in the state-government and municipal school system for capital cities during 2014-2015. Only in this period, state government schools went on strike in 21 different states out of 27; while municipal schools went on strike in 17 different state capitals out of 26; 50% of the strikes lasting for longer than 21 days and 25% lasting longer than 46 days. The fact that the school year may go beyond the calendar year due to strikes explains our findings on age-grade distortion during the same calendar year as the layoff. In particular, children whose parents are displaced in the first months of the calendar year may be affected during their final exams, and be retained, explaining the immediate impacts on age-grade distortion, and school enrollment.

Nevertheless, we provide evidence that the timing of our effects is consistent with the timing of job loss by focusing on layoffs taking place at the last two months of the year. As shown in Figure B3, the effects on enrollment, age-grade distortion, and school quality only emerge in the subsequent calendar year when parents are displaced in the last bimester of the year. This provides further evidence that pre-trends are indeed parallel, and that school outcomes do not drop before layoffs effectively take place. Due to the reasons described above, we cannot provide such test for parents displaced before, e.g., in the third quarter of each calendar year, because (i) schools may not strictly follow the enrollment cutoff date when filing the school census; (ii) and because several parents displaced in the third quarter may have learned about displacement or have left the job in the second quarter – so that it is possible that we observe effects already in the same calendar year.

Figure B3: Effect of parental job loss on education outcomes, parents displaced during November and December



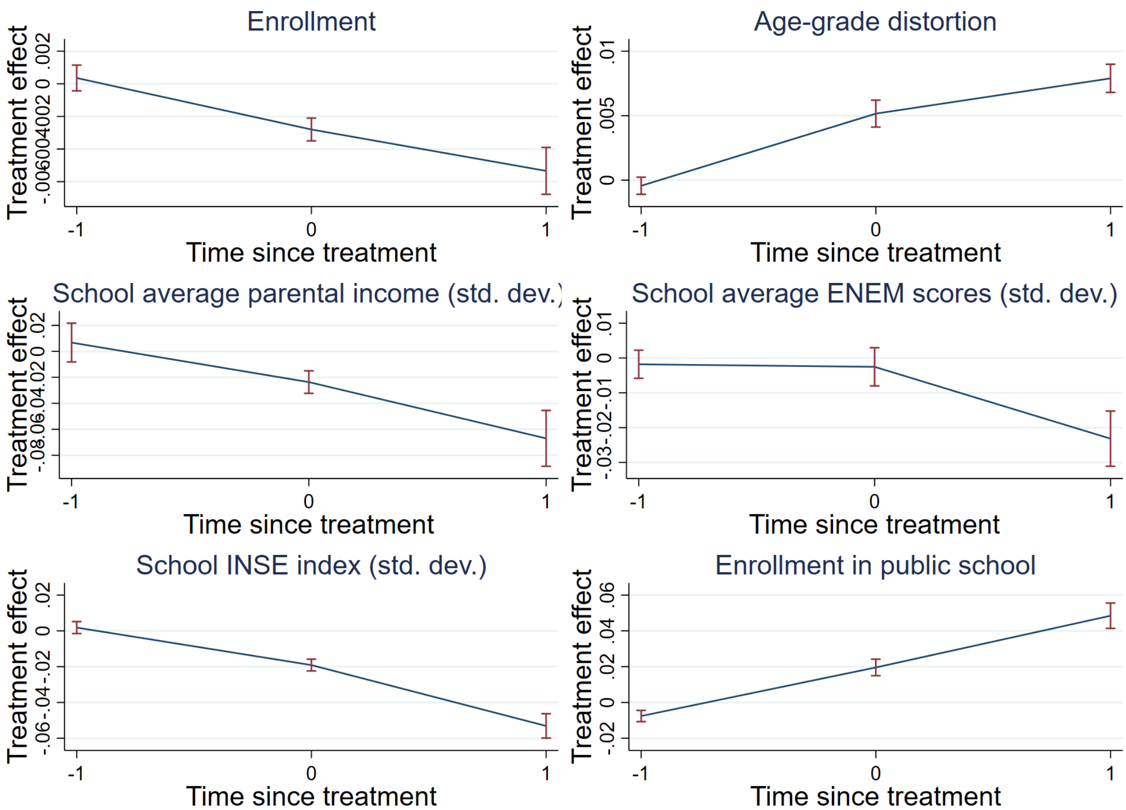
Notes: The figure shows the effect of parental job loss on education outcomes, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs during the months of November and December in each year, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the average outcome in the pre-displacement year for the treatment group.

B.3.E Staggered diff-in-diff

We now address concerns related to the recent literature on difference-in-differences designs with staggered treatment (See Sun and Abraham (2021); Athey and Imbens (2018); De Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Callaway and Sant'Anna (2021); Imai and Kim (2019)). These papers show that two-way fixed estimators can be expressed as the weighted average of several difference-in-differences comparisons between cross-sectional units. First, they show that some of these comparisons may be inadequate under the presence of dynamic treatment effect, as the control group may be composed of units which are already treated. Second, they show that some units may receive negative weights so that the final estimator does not recover any meaningful treatment effect quantity. As described in Section 4.1, our setting addresses both issues as our estimator derives from the simple average of each difference-in-difference comparison between each treated parent-child with respect to their control parent-child unit. By construction, our control group is entirely composed of never-treated workers – non dismissed workers in non-mass layoff firms.

We confirm this point by running the diagnosis proposed by De Chaisemartin and D'Haultfœuille (2020) which inspects the presence of negative weights. In line with the argument from above, we detect no negative weights when running their diagnostic. In addition, we run the estimator proposed in the same paper finding similar results as shown by Figure B4 below. It is worth noting that such estimator can only generate $n-2$ placebo, pre-treatment, coefficients when n pre-treatment periods are available, explaining why only one placebo coefficient is available in each estimate presented below.

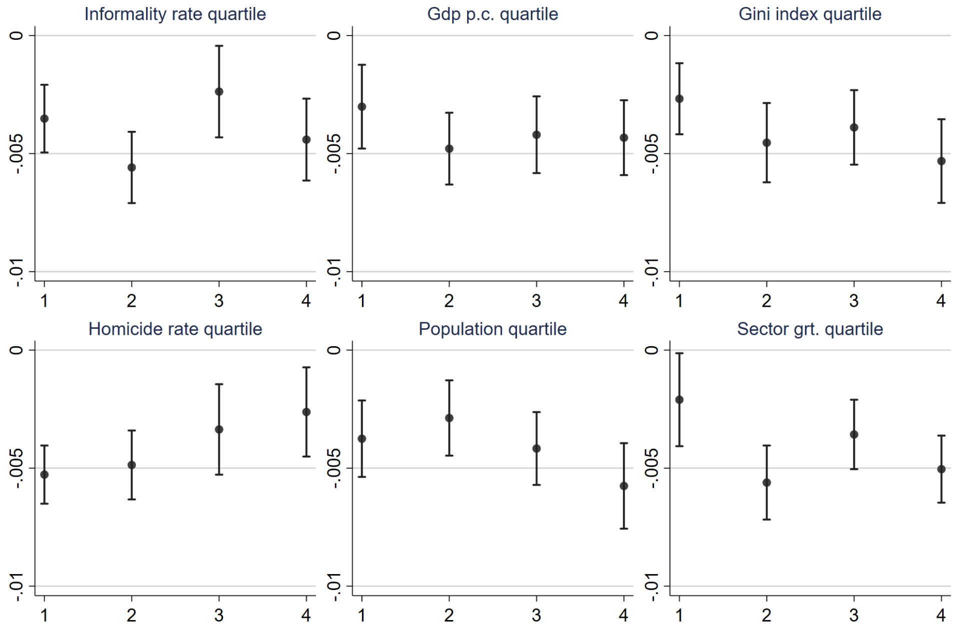
Figure B4: Effect of parental job loss on school outcomes, alternative estimators



Notes: The graph reports the dynamic treatment effects of job loss on school outcomes, based on the estimator proposed by De Chaisemartin and D'Haultfœuille (2020), along with 95% confidence intervals.

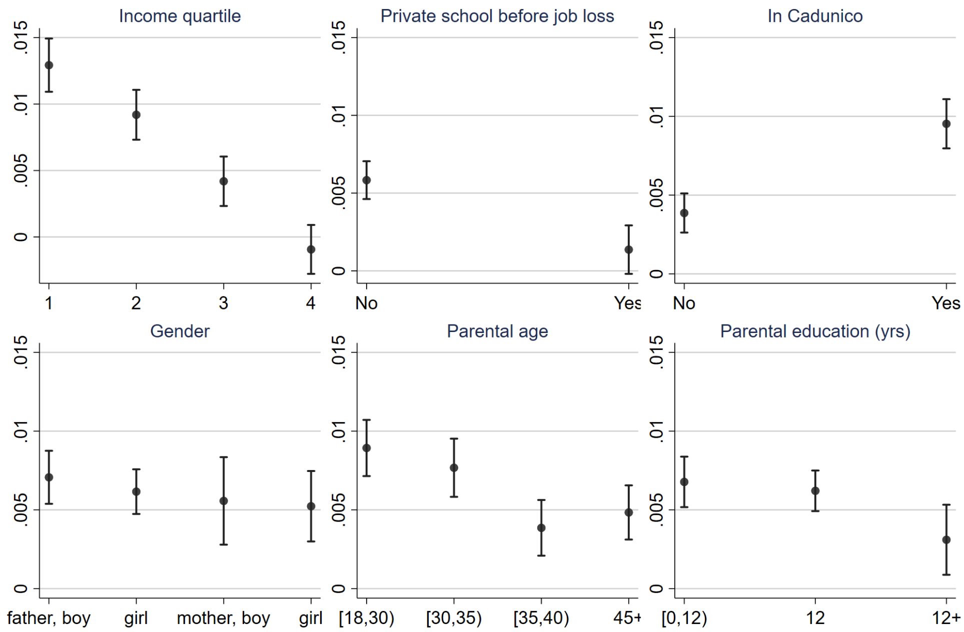
B.4 The effect of parental job loss, heterogeneity analysis

Figure B5: Effect of parental job loss on school enrollment, heterogeneity analysis by area-level characteristics



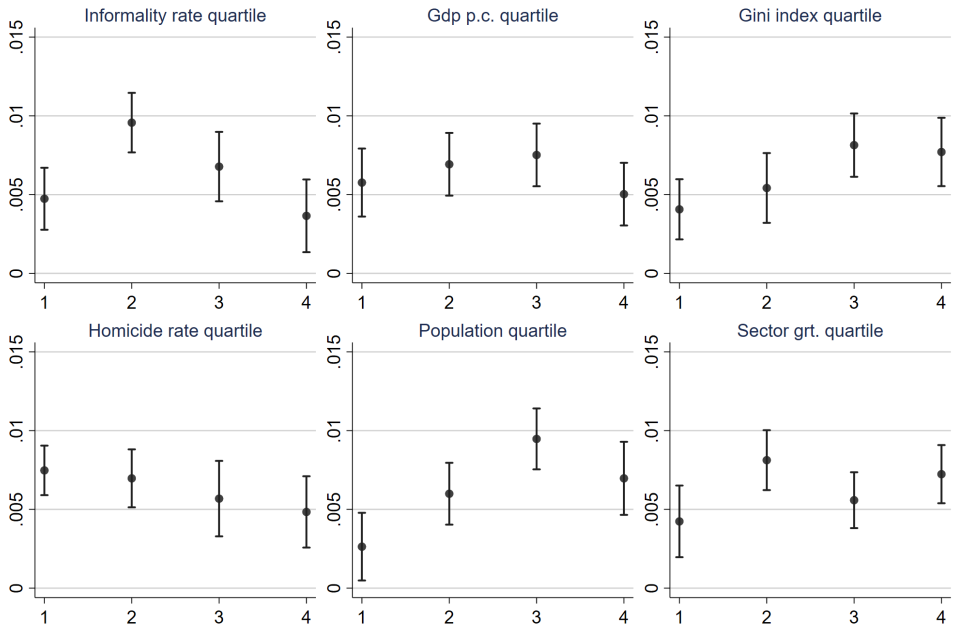
Notes: The figure shows the effect of parental job loss on children's school enrollment, after splitting the sample by several area-level characteristics, as estimated from equation (2) – along with 95% confidence intervals. Informality rate, GDP per capita, Gini index, and population are based on the 2010 Population Census at the municipality level. The homicide rate is based on death records (SIM) at the municipality level and employment sector growth rate is computed at the state by 2-digit-sector level. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

Figure B6: Effect of parental job loss on age-grade distortion, heterogeneity analysis



Notes: The figure shows the effect of parental job loss on children's age-grade distortion, after splitting the sample by several characteristics, as estimated from equation (2) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

Figure B7: Effect of parental job loss on age-grade distortion, heterogeneity analysis, area-level characteristics

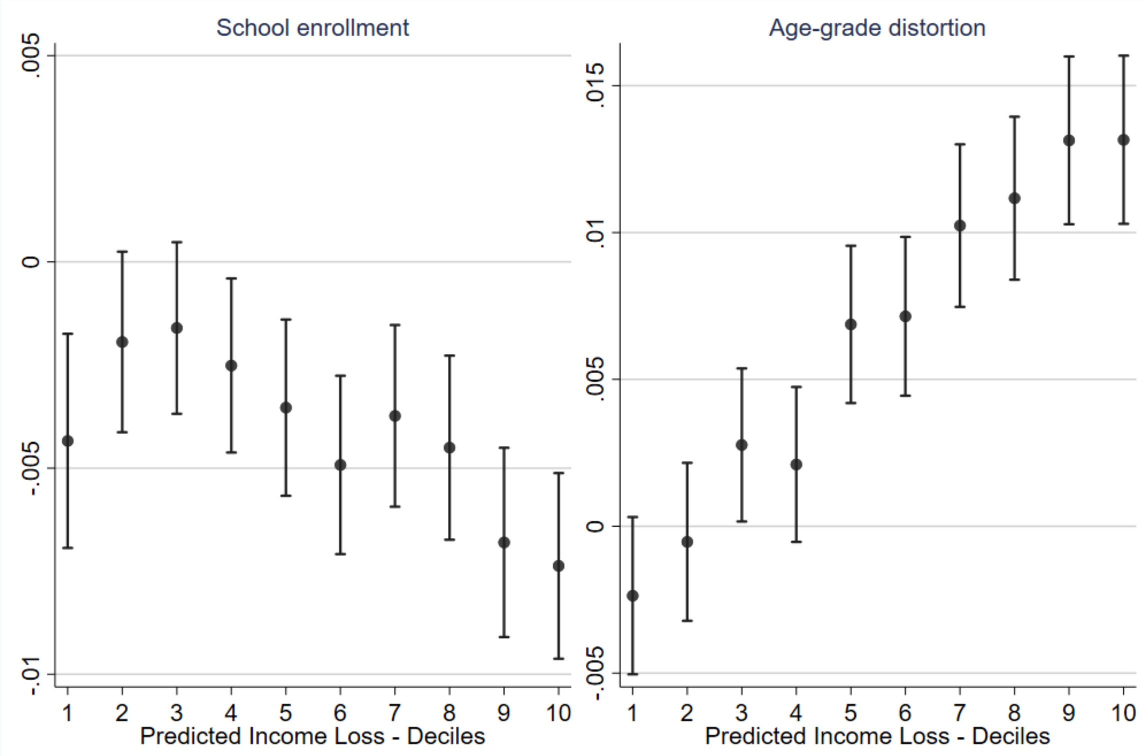


Notes: The figure shows the effect of parental job loss on children's age-grade distortion, after splitting the sample by several area-level characteristics, as estimated from equation (2) – along with 95% confidence intervals. Informality rate, GDP per capita, Gini index, and population are based on the 2010 Population Census at the municipality level. The homicide rate is based on death records (SIM) at the municipality level and employment sector growth rate is computed at the state by 2-digit-sector level. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

B.5 Testing whether predicted income losses explain the effect of parental job loss on enrollment and age-grade distortion,

We provide evidence that the predicted income drop based on parental and child characteristics explains the effects of parental job loss on enrollment rates and age-grade distortion – following a similar exercise to Hilger (2016). More specifically, we compute the job effect on labor income for each displaced parent – in comparison to the respective control unit, following equation (2) –, regress it on a rich set of pre-displacement characteristics, and compute the predicted income losses. This regression includes income, tenure, dummies for parental and child age, parental schooling, and municipality-industry fixed effects as explanatory variables. Then, we re-estimate the effect of job loss on school enrollment and age-grade distortion, by deciles of the predicted income losses. The results in Figure B8 show that predicted income losses are strongly correlated with the estimated impact on enrollment rates and age-grade distortion, and indicate null effects for workers with low predicted income losses, compatible with the evidence in Figure 5. These results offer further support to the idea that income losses driven by parental job loss are an important mechanism.

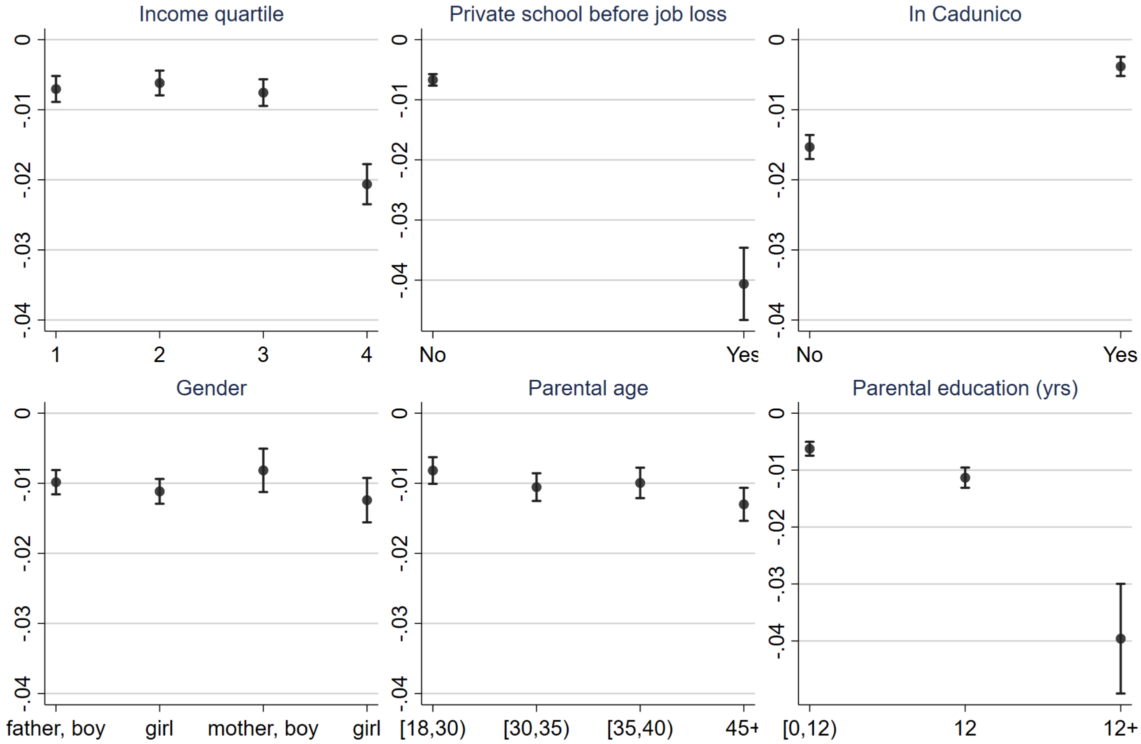
Figure B8: Predicted income losses and the effect of parental job loss on school enrollment and age-grade distortion



Notes: The figure shows the effect of parental job loss on children's enrollment (left graph) and age-grade distortion (right graph), after splitting the sample by predicted income losses, as estimated from equation (2) – along with 95% confidence intervals. Predicted income losses are computed after regressing individual parental job loss effects on income on several parent and children's characteristics: income, tenure, dummies for parental and child age, parental schooling, and municipality-industry fixed effects. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

B.6 The effect of parental job loss on school choices

Figure B9: Effect of parental job loss on school quality, heterogeneity analysis

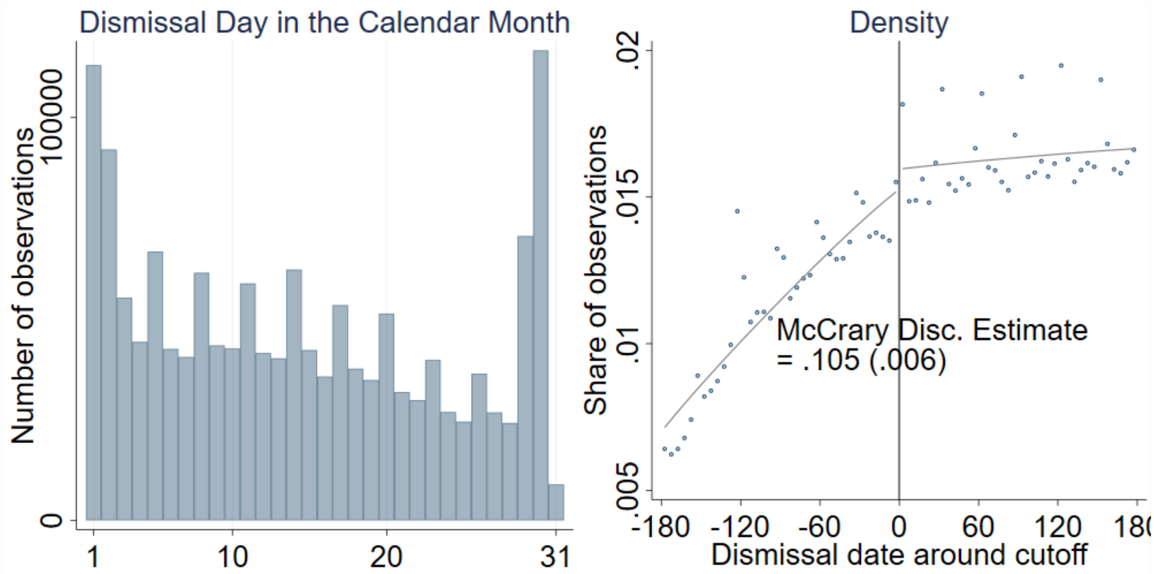


Notes: The figure shows the effect of parental job loss on children's school quality, after splitting the sample by several characteristics, as estimated from equation (2) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

C Appendix to Section 5

C.1 Monthly Cycles in Dismissal Dates

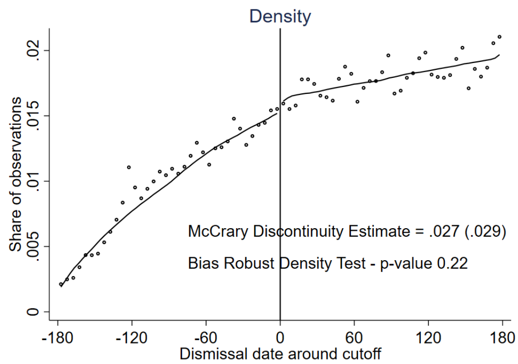
Figure C1: Dismissal dates monthly cycles



Notes: The left graph presents the distribution of dismissal dates by calendar day within each month. The right graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

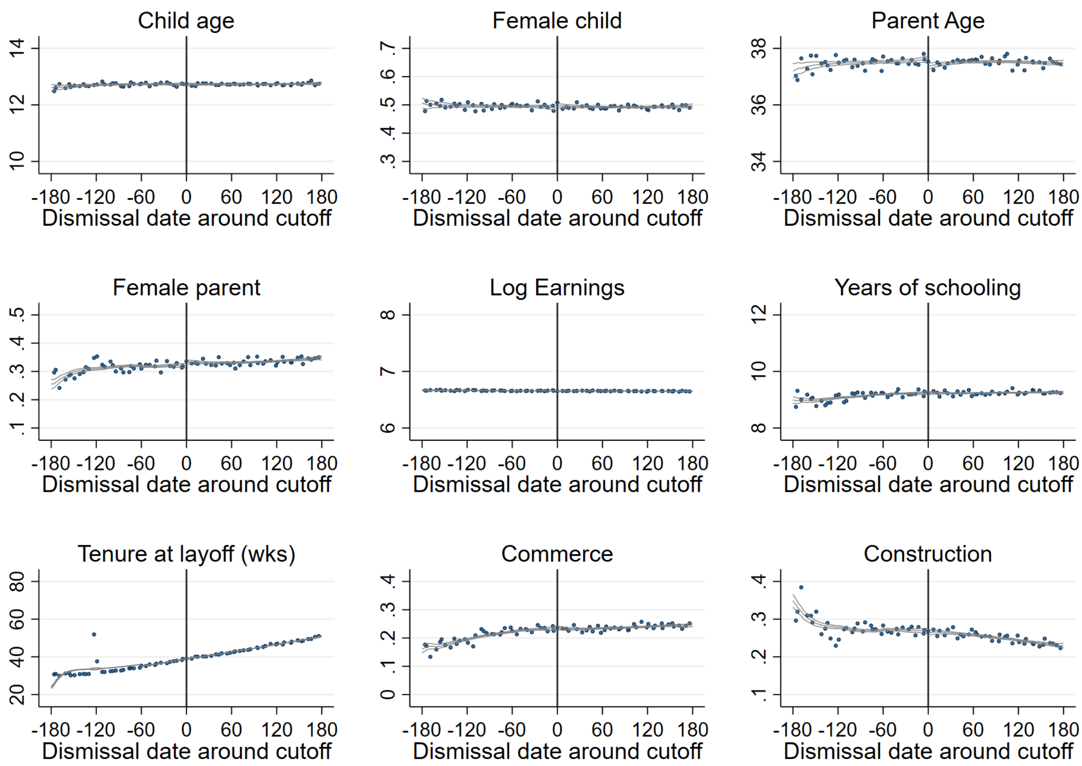
C.2 Effect of UI eligibility, evidence on the validity of the RD design

Figure C2: Effects of UI eligibility, density function



Notes: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past) in our main working sample. The sample includes displaced parents with at least 6 months of continuous employment prior to layoff. The results of McCrary density test and the bias robust test proposed by Cattaneo et al. (2018, 2019) are also reported.

Figure C3: Effects of UI eligibility, balance of covariates



Notes: The graphs show the balance of pre-determined covariates around the cutoff for eligibility for unemployment benefits. The sample includes displaced parents with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

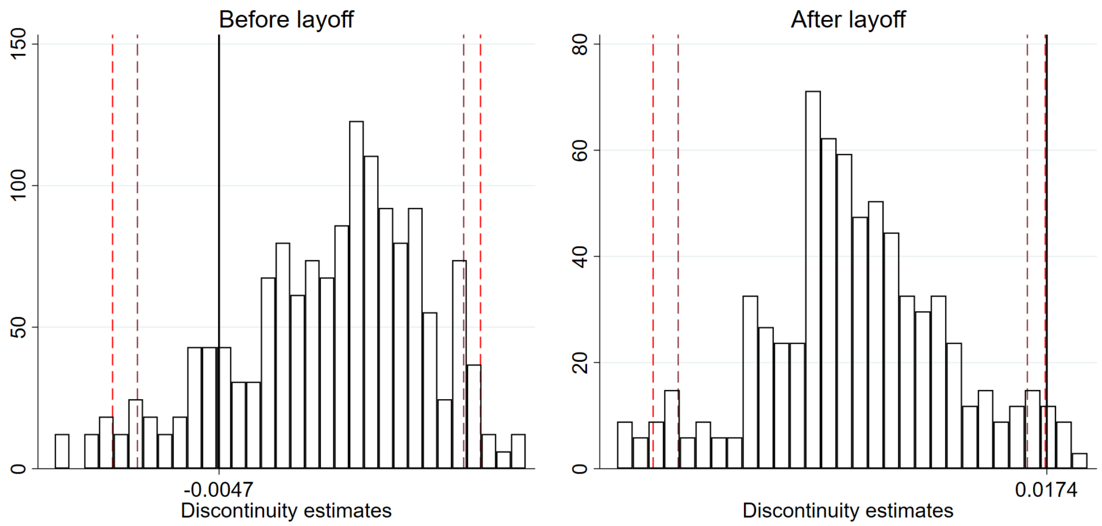
C.3 Effect of UI eligibility on enrollment, robustness

Table C1: Effect of UI eligibility on school enrollment, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Dep. Var. School Enrollment								
Eligibility to UI benefits	0.0168***	0.0195**	0.0174***	0.0145***	0.0211***	0.0174***	0.0136***	0.0241***
	(0.0054)	(0.0078)	(0.0056)	(0.0046)	(0.0065)	(0.0054)	(0.005)	(0.0087)
Observations	11,006	21,138	41,396	60,859	29,683	96,798	111,824	37,823
Bandwidth (days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2

Notes: This table replicates the regression discontinuity analysis in Table 5 for different specifications of the polynomial regression and different bandwidths (indicated on bottom of the table). CCT denotes the optimal bandwidth according to Calonico et al. (2014).

Figure C4: Effects of UI eligibility on school enrollment, permutation tests



Notes: The graphs compare discontinuity estimates of the effect of UI eligibility on school enrollment rates at the true cutoff for UI eligibility (vertical black line) with the distribution of estimates obtained at all possible placebo cutoffs within 180 days away from the actual threshold, for different groups (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in eq. (5).

Table C2: Effect of UI eligibility on school enrollment after layoff, manipulation inference

	(1)	(2)
Dep. Var:		
School Enrollment		
	Estimate	C.I.
PANEL A. MAIN ESTIMATES		
Share always assigned	0.082	
ITT: Ignoring manipulation	0.01737	[0.007,0.02775]
ITT: Bounds inference	[0.00767,0.08409]	[-0.00187,0.10274]
PANEL B. HYPOTHETICAL SHARE OF MANIPULATION		
Share always assigned		
0.025	[0.01461,0.04025]	[0.00578,0.04907]
0.05	[0.01169,0.06432]	[0.00276,0.07325]
0.1	[0.00538,0.09304]	[-0.00379,0.10098]
0.15	[-0.00168,0.12056]	[-0.01111,0.12854]
0.2	[-0.00961,0.12533]	[-0.01936,0.13184]

Notes: This table shows discontinuity estimates in school enrollment rates after layoff, while allowing for manipulation in treatment assignment around the 16-month cutoff for UI eligibility, using the estimator proposed by Gerard et al. (2020). Panel A presents estimates ignoring manipulation and bounds based on the estimated manipulation share in the running variable density. Panel B presents bounds estimates for hypothetical shares of manipulation.

C.4 Effect of UI eligibility on school quality, children in private schools before parental job loss

Table C3: The effects of UI eligibility on school quality, children in private schools before parental job loss

	(1)	(2)	(3)	(4)
PANEL A: UI AND EMPLOYMENT OUTCOMES				
Dependent var.:	Unemployment Benefits			Unemployment
	Take-up	Months	Amount	Duration (wks)
UI eligibility effect	0.615***	2.648***	1804.027***	13.336***
	(0.012)	(0.05)	(36.408)	(1.979)
Baseline Mean	0.05	0.10	73.96	41.91
Observations	16,746	16,746	16,746	16,746
PANEL B: SCHOOL QUALITY OUTCOMES				
Dependent var.:	Av. parental income	ENEM score	INSE index	Private school
UI eligibility effect	0.049	0.094*	0.089**	0.014
	(0.04)	(0.051)	(0.045)	(0.013)
Baseline Mean	2.20	10.82	9.88	0.69
Observations	15,996	6,029	11,017	16,724

Notes: This table shows the effect of eligibility for UI benefits, as estimated from equation (5), on UI outcomes (Panel A, columns 1-3), unemployment duration (Panel A, column 4) and school quality outcomes where children enroll up to two years after parental layoff (Panel B). The sample includes displaced parents with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. The table also reports the baseline mean outcome at the cutoff. Standard errors are clustered at the individual level and displayed in parentheses (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

Table C4: Effect of UI eligibility on school quality, children in private schools before parental job loss, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: DEP VAR AV. PARENTAL INCOME								
Eligibility to UI benefits	0.0402	0.1108**	0.0488	-0.0119	0.0509	0.0178	0.0022	0.1131*
	(0.0357)	(0.0564)	(0.0399)	(0.0328)	(0.049)	(0.0387)	(0.036)	(0.0657)
Observations	5,044	8,149	15,996	23,609	11,259	37,358	43,205	15,026
PANEL B: DEP VAR ENEM SCORE								
Eligibility to UI benefits	0.0467	0.1201	0.0941*	0.0243	0.0731	0.0516	0.0564	0.1702**
	(0.0419)	(0.073)	(0.0509)	(0.0419)	(0.0656)	(0.0497)	(0.0459)	(0.084)
Observations	2,288	3,113	6,029	8,885	4,107	14,110	16,380	5,676
PANEL C: DEP VAR INSE INDEX								
Eligibility to UI benefits	0.0383	0.1670***	0.0887**	0.0552	0.1376**	0.0854**	0.0793**	0.3013***
	(0.0372)	(0.063)	(0.0446)	(0.0365)	(0.0578)	(0.0431)	(0.0398)	(0.0773)
Observations	3,998	5,625	11,017	16,267	7,356	25,830	29,856	11,249
PANEL D: DEP VAR PUBLIC SCHOOL								
Eligibility to UI benefits	0.0125	0.023	0.0142	0.007	0.0179	0.0131	0.0122	0.0176
	(0.0101)	(0.0191)	(0.0135)	(0.011)	(0.0157)	(0.013)	(0.0121)	(0.0195)
Observations	7,915	8,509	16,724	24,669	12,955	39,060	45,156	19,075
Bandwidth (days)	CCT	30	60	90	CCT	150	180	CCT
Polynomial Order	0	1	1	1	1	2	2	2

Notes: This table replicates the regression discontinuity analysis in Table C3, Panel B, for different specifications of the polynomial regression and different bandwidths (indicated on bottom of the table). CCT denotes the optimal bandwidth according to Calonico et al. (2014).