The Kids Aren't Alright: Parental Job Loss and Children's Outcomes Within and Beyond Schools *

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September 26, 2022

Abstract

We study the effects of parental job loss on children and how access to unemployment benefits can mitigate these impacts. We leverage unique nationwide data from Brazil linking multiple administrative datasets, and take a comprehensive approach studying impacts on education as well as other key dimensions of children's lives. First, leveraging mass layoffs for identification, we show that parental job loss increases school dropouts and age-grade distortion by up to 1.5 percentage points. These effects are pervasive, last for at least six years and significantly reduce high-school completion rates. Second, we document that other important dimensions of children's lives are affected. Following the layoff, children are more likely to work informally, commit crime, and experience early pregnancy. In turn, parents reduce educational investments by moving children from private to lower-quality public schools. Using a clean regression discontinuity design, we show that access to unemployment benefits effectively mitigates some of the intergenerational impacts of job loss, notably on teenage school dropouts and crime, and on parental investments in school quality. 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.

JEL Classification: K42, J63, J65.

*The paper benefited from comments by Koray Aktas, Daniel Araújo, Sonia Bhalotra, Bladimir Carrillo, Tommaso Colussi, François Gerard, Caio de Holanda, Eliana La Ferrara, Clément Imbert, Joana Naritomi, Roberto Hsu, Paolo Pinotti, Carlo Schwarz, Rodrigo Soares, Gabriel Ulyssea, Lucas Warwar, and participants in seminars and conferences at several institutions.

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1 Introduction

A wealth of research has demonstrated that job loss has dire consequences for individuals' lives, causing substantial earning losses and worsening several socioeconomic outcomes such as mental health, mortality and crime.[1] Clearly, such consequences may well extend to the children of displaced workers. On the one hand, children's educational trajectories could be hampered by the financial struggle and stress caused by job loss in the family. On the other hand, some children could even benefit if receiving greater time investments from their parents when the latter are out of work. These mechanisms may also have important implications for other aspects of children's lives, whereby the effects of parental displacement may extend far beyond the realm of children's education.

Despite the relevance of such a shock, there is a lack of comprehensive evidence on how it affects children within and beyond the educational system, as well as the mechanisms through which it operates. Moreover, the literature is silent about whether public policies can successfully mitigate the intergenerational impacts of job loss.

This paper breaks new ground on this topic by investigating the effects of parental job loss and job insurance policies on children, and by tracking a wealth of outcomes with rich administrative registries from Brazil. We link longitudinal individual-level data on parental employment and unemployment insurance to detailed school records covering 37 million children all over the country. These data are complemented with a wide set of additional outcomes from a variety of administrative sources. First, we study the impacts of parental job loss on school outcomes and several other aspects of children's lives, including labor supply, crime, early pregnancy, and school choices made by parents. Second, we provide novel evidence on how access to unemployment benefits can mitigate any impacts of parental job loss using a clean regression discontinuity design. Taken together, our analysis allows us to investigate several potential mechanisms, and provide a complete characterization of heterogeneous treatment effects across individuals and place characteristics over the vast Brazilian territory.

In the first part of the paper, we leverage mass layoffs and plant closures to study the effects of parental displacement on parents' and children's outcomes. We use a difference-in-differences design in which we compare across time children whose parents were displaced in mass layoffs to similar children whose parents were not displaced in the same period.

[1] For the impacts on labor market outcomes, see, e.g., Couch and Placzek (2010); Ichino et al. (2017); Schmieder et al. (2021); Sullivan and Von Wachter (2009). For other outcomes, see Charles and Stephens (2004); Eliason (2012) for divorce, Zimmer (2021); Zimmerman (2006) for mental health, Black et al. (2015) for smoking, Sullivan and Von Wachter (2009) for mortality, (Del Bono et al., 2012, 2015) for fertility, Lindo (2011) for offspring birth weight, and Bhalotra et al. (2021); Britto et al. (2022); Khanna et al. (2021); Rose (2018) for crime and domestic violence.

We start by documenting the impacts of job loss on parental employment. In line with earlier studies (Couch and Placzek, 2010; Schmieder et al., 2021), we show that job loss reduces labor income by 45% in a two-year period, similarly affecting both fathers and mothers.[2] In terms of magnitude, these employment losses lie at the upper end of the available estimates for developed countries (Bertheau et al., 2022).

Turning to the impact on children's education, our analysis shows that parental job loss significantly worsens children's school outcomes. In a two-year period, it reduces enrollment on average by .4 percentage points (p.p.), relative to an 6% dropout rate in the baseline, and increases age-grade distortion – i.e., the probability that the child is overaged for her grade – by .5 p.p, relative to a baseline rate of 18%. 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.[3]

We further show that our estimates turn even larger six years after layoff for children in welfare registries whom we can track for longer in the data, whereby the adverse impacts on school dropouts and age-grade distortion go up to 1.5 p.p. In addition, we develop an additional empirical analysis leveraging variation in the timing of parental job loss to show that it reduces high-school completion rates by 1.5 p.p. Although we cannot track employment outcomes in adulthood because children are too young in our sample, these results on completed education indicate that parental job loss likely has important long-term consequences.

Next, we investigate teenage work outcomes as a potential factor explaining the negative effects on school outcomes. Like in other developing countries, the incidence of child work remains substantial in Brazil and 39% of school dropouts report the necessity to work as the main reason for leaving school.[4] We use longitudinal survey data to show that children aged 14-17 are more likely to take informal jobs following parental displacement. We also provide novel evidence on the consequences of parental job loss for crime by boys aged 14-17, which increases by 33% over the baseline, as measured by the number of children sent to correctional facilities due to criminal offenses.[5]

[2]We use survey data to show that informal employment only mildly compensate the loss of formal labor income. In the main analysis, we focus on formal employment outcomes based on administrative data. Additionally, we show that spousal labor supply responses are small and do not compensate for the loss of income in the family.

[3]We also provide evidence that our results are not driven by large mass layoffs, which could generate substantial spillovers across displaced co-workers, supporting the external validity of our findings. Our estimates are robust to alternative estimators proposed in the recent literature on staggered treatment in difference-in-differences designs (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]4% of children aged 10-13 and 13% of those aged 14-17 work, mainly in the informal labor market. These statistics are based on the PNAD survey and the 2010 Population Census.

[5]Although children below the legal age (18) cannot be arrested or criminally prosecuted in Brazil, they

The evidence on crime and informal labor supply indicates that income losses brought by job loss may be a key mechanism affecting children. These results are consistent with the idea that children turn to work or economic crime to compensate for the financial struggle in the family.[6] Even though these and schooling decisions are taken jointly, these patterns signal that children are affected by the income losses in the family, which in turn could explain adverse impacts on school enrollment and progression.

The income mechanism is also supported by a heterogeneity analysis that reveals a strong gradient over parental income before the job loss. The effects on school enrollment and age-grade distortion are largest at the bottom quartile of the income distribution, where families are more likely to be financially constrained, and null at the top.[7] We complement this evidence by showing that predicted earnings losses – based on a rich set of pre-determined individual characteristics – can strongly predict the adverse impacts on children's education. On the contrary, parental job loss effects vary substantially less across other individual characteristics such as parental age, gender and education, child gender, and across area-level characteristics, despite the large and heterogeneous Brazilian territory.

Subsequently, we investigate the impacts of job loss on parental school choices for children in advantaged families, who experience milder adverse effects on enrollment rates and age-grade distortion. Following the layoff, children who are initially enrolled in private schools are more likely to be reallocated by their parents to public and lower-quality schools. This analysis offers novel evidence associating the worsening of children's outcomes caused by job loss with a reduction in parental investments in their children. Moreover, this evidence provides additional support to the income mechanism.

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 extensive evidence on these aspects, we show that parental job loss increases the likelihood of teenage fertility for girls. The latter is associated with poor outcomes later in life and could hint at worsening psychological conditions in the household (Kearney and Levine, 2012, 2015). Such mechanisms are also in line with the evidence in Bhalotra et al. (2021) and Britto et al. (2022) showing that job loss leads to more domestic violence and non-economic crime in Brazil, which are also indicative of emotional turmoil in the household.

Overall, our findings indicate that any potential benefits resulting from greater parental

can be sent to correctional facilities, which we are able to track in our data.

[6] Economic crimes account for the majority of criminal prosecutions against young defendants, e.g., 75% for defendants aged 18. The legal age (18) is the earliest age for which such statistics is available.

[7] Also consistent with the income mechanism, the effects are stronger in families where the non-displaced parent is not employed in the baseline and in families with more children.

time investments following layoff are unable to overcome the adverse impacts of parental job loss on multiple children's outcomes. Similarly, we do not find much support in the data for additional mechanisms related to family rupture and the relative time inputs by fathers and mothers on children's education.

To understand the effectiveness of alternative policy remedies, and shed more light on mechanisms, we then examine the effect of unemployment insurance (UI), the main policy providing income support for displaced workers in Brazil. For this purpose, we employ a clean regression discontinuity design that compares displaced parents who are barely eligible and ineligible for unemployment benefits due to slight variations in layoff dates.[8]

Our results show that eligibility for UI significantly increases enrollment rates for teenage children in welfare registries, who bear the largest effects of parental job loss on school enrollment. Namely, access to 2.7 months of unemployment benefits with a 85% replacement rate increases children's school enrollment by 1.4 p.p. in the three years following parental job loss.[9] We also find evidence that UI eligibility reduces youth crime and allows parents to keep children in higher-quality schools. A simple back-of-the-envelope welfare analysis reveals that the positive impacts on children's education may significantly reduce the efficient costs of UI provision, highlighting the relevance of these findings for policy. Overall, these results also offer strong support for the idea that income loss following parental job loss is a key mechanism affecting children.[10]

Our paper contributes to several strands of literature. First, it relates to a literature studying the impacts of parental job loss on children's education. Oreopoulos et al. (2008) and Rege et al. (2011) study this question using cross-section variation in (mass) layoffs in Canada and Norway, respectively. In turn, Hilger (2016) uses variation in the timing of parental job loss over children's age to study impacts on college enrollment in the US, while Huttunen and Riukula (2019) and Tanndal and Päällysaho (2021) adopt a similar approach to study effects on children's career choices in Finland and high-school completion rates in Sweden, respectively. We contribute to this literature by providing a comprehensive characterization of the impacts of parental job loss on children, studying several important outcomes within and outside the educational system, investigating both short- and long-term

[8]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.

[9]We find statistically insignificant UI impacts on age-grade distortion, but these estimates are not particularly precise and we may lack the statistical power to detect mild beneficial effects.

[10]In line with the results in our data and with extensive literature, UI reduces labor supply (see, e.g., 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.

effects, and providing rich heterogeneity analysis across individuals and the diverse Brazilian territory. This comprehensive analysis within a single setting allows us to gain insights into the mechanisms driving the impacts on children's education.

Our analysis is also the first to use 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 greater challenge in developing countries, where governments still struggle to keep children at school. To date, the evidence for low- and middle-income countries has been limited to the use of relatively small survey datasets (e.g., see 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 context of conflicts.

Second, the evidence on crime committed by children below the legal age is novel and complements recent work on the impact of job loss on adult crime (Rose, 2018; Bennett and Ouazad, 2020; Khanna et al., 2021; Britto et al., 2022).[11] Our evidence showing that parental displacement increases the incidence of teenage pregnancy is also novel. This finding contributes to a body of literature studying the causes and determinants of early pregnancy (e.g., see Kearney and Levine, 2012, 2015).

Third, we provide new evidence connecting the impacts of parental job loss on children to a reduction in parental investments, as parents move children from private to public and lower-quality schools. This indicates that reductions in parental investments may work as a costly insurance mechanism to absorb economic shocks, contributing to a broad body of literature studying the determinants and consequences of parental investments – e.g., see Cunha and Heckman (2007); Carneiro and Ginja (2016); Francesconi and Heckman (2016).

Finally, we provide the first estimates in the literature on the impacts of unemployment benefits on children, one of the most relevant and widespread social insurance policies around the globe. These findings are a key contribution to the literature given the lack of evidence on policies that may mitigate the intergenerational effects of job loss. In addition to the policy relevance, these findings contribute to understanding the role of income as a mechanism linking parental job loss and children's outcomes. They also contribute to literature studying the impacts of unemployment benefits on non-labor related outcomes – e.g., Britto et al. (2022) on adult crime and Kuka (2020) on health outcomes – and more generally to literature studying the effects of parental income and access to welfare benefits during childhood (Dahl and Lochner, 2012; Hoynes et al., 2016).

[11] To the best of our knowledge, Khanna et al. (2021) – who uses data from one large city in Colombia – is the only paper to provide causal evidence that job loss causes higher arrest rates by young family members in the household aged 14-24. It is unclear whether these results are driven by teenage children below the age of 18.

The remainder of this 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 short- and medium-term impacts of parental job loss, while Section 5 studies long-term impacts on completed education. Section 6 presents the analysis on the effects of unemployment benefits, followed by Section 7, which discusses the paper's results and concludes.

2 Institutional Background

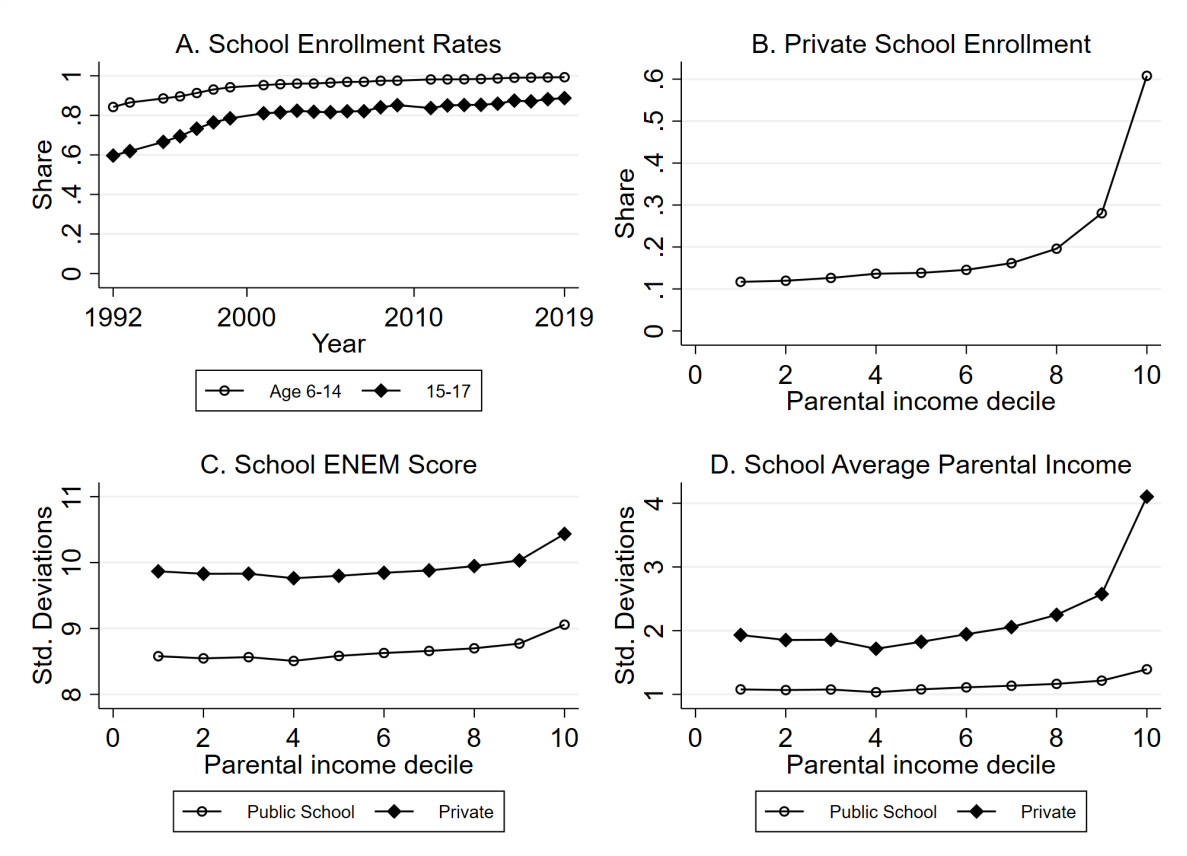
2.1 Education in Brazil

Brazil has experienced a substantial increase in primary and secondary school enrollment rates over the last 30 years, as shown in Panel A of Figure 1. Although primary school has been mandatory since 1971, a significant share of children were still away from school in the 1990s. From there, primary school dropouts were reduced to about 1 percent following several policies, including the opening of new public schools, increasing mandatory schooling age, and the introduction of *Bolsa Família* in 2004, a large cash transfer program conditional on school enrollment. Although enrollment rates in secondary school have improved over time, dropouts remain sizable despite the fact that compulsory schooling goes until the age of 17 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. However, public schools are generally composed of students with relatively lower socioeconomic backgrounds as wealthier families tend to enroll children in private schools, as shown in Panel B of Figure 1. These are generally perceived as higher-quality schools compared to their public counterparts. In line with this, Panel C in the same figure shows that private schools achieve higher scores in ENEM – a national examination following the end of secondary school which is the basis for the admission process of public and private universities. The same panel shows that children in the upper side of the income distribution enroll in better schools. In addition, there is a large variation in quality within private schools, for which prices strongly vary.[12] 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.

[12] 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 of primary and secondary schooling ages (Panel A) based on PNAD household survey (not available in Population Censuses years: 2000 and 2010), and average school characteristics by parental income in the formal labor market, based on school census data and RAIS for 2014 (Panels B-D).

2.2 Labor market

Private firms in Brazil are free to terminate labor contracts without a just cause, although workers are entitled to receive a mandatory severance payment by the firm.[13] Such dismissals represent roughly 70% of all separations, while job quits virtually cover the remaining part. The severance amount is equivalent to 40% of the balance in a forced savings account, which receives an 8% monthly contribution by the firm over the worker's earnings. The account proceedings are only made available to the worker upon dismissal.[14] Overall, workers receive about 1.34 monthly earnings per year of tenure upon displacement.

Unemployment insurance 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

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

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

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 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. We use survey data to show that the income losses from parental formal job loss remain substantial even when considering re-employment in 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 outcomes, and family characteristics. The first source is the *Relação Anual de Informações Sociais* (RAIS), which provides rich information on the population of formal workers and firms in the Brazilian labor market for the 2002-19 period. It contains detailed information on each job spell, such as the contract's starting and ending dates, earnings, the reason for termination, and detailed demographic characteristics such as date of birth, 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. 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 2011-19 period. The registry identifies the household with a unique ID and individuals by their names and unique tax codes, along with addresses and detailed demographic characteristics such as date of birth, and education. We use these data to track teenage fertility, couples' separation and migration across neighborhoods.

Finally, we use data from the yearly school census for the 2008-17 period. 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.

3.1 Linking children to parents across different datasets

Our main analysis is based on children enrolled in 2014 whom we can link to their parents' tax codes. Specifically, for all students in the 2014 school census, we have information on the student's census identifier, name, birth date, and both parents' names. These data allow us to link the students' IDs to their parents' unique tax codes, enabling us to link children and parents throughout several years in the school census and the employment data. Therefore, our main analysis is conditional on school enrollment in 2014. Overall, we are able to identify fathers and mothers for 58% and 72% of the 45 million children in the 2014 school census, respectively.

In Appendix A.1, we provide the details of the data linkage procedure, and we show that children successfully linked to their parents are fairly similar to the population of students enrolled in the 2014 school census. Nevertheless, we will later show that our findings are robust to reweighting our main sample to perfectly match the attributes of the student population in the school census. To conduct additional analysis, we will also use a second sample directly linking children's census ID to their parents' tax codes for all children registered in *CadÚnico* during the 2008-2013 period. Unlike in our main sample, this sample is not conditional on children's school enrollment in 2014.

4 Parental Job Loss and Children's Outcomes

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 worker's decision or children's 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 one-third of their workforce during a given year and focus on private firms with at least 30 workers.[15]

We build a yearly panel at the parent-child level so that parents with multiple children show multiple times. Given that the school census reflects students' enrollment on May 31st in each year, our panel follows yearly cycles from June to May (rather than from January to December).[16] For instance, layoffs taking place between June 2014 to May 2015 are grouped in the time period that we call year 2014 for simplicity. Accordingly, we track all outcomes

[15] 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.

[16] The academic year in Brazil follows the solar year, from January to December.

following June to May cycles.

Our treatment group is composed of full-time private sector working parents aged 18-60 who are displaced in mass layoffs during 2014 and 2015[17] (effectively covering mass layoffs taking place between June 2014 and May 2016).[18] We focus on children aged 9-15 years old immediately before the layoff year so that we can observe school enrollment three years before and two years after layoff, as the compulsory schooling age range is 6-17. For the same reason, we also restrict the data to children in age grades 9-15 just before the layoff. Throughout the paper, we 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.

The control group is defined via exact matching on a fine set of characteristics, leveraging the large dimension of our data. For each mass layoff year, the set of 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, parent's education (college and high-school dummies) and birth cohort (5-year groups), and child characteristics, namely gender, birth cohort, grade in the pre-displacement year and a dummy indicating whether the child is in *CadÚnico* before the layoff. When a treated unit is matched to multiple controls, we randomly select one. Out of 0.6 million parents in the initial mass layoffs pool, we successfully match 89% to a control unit.[19]

Each treatment-control pair defines a single difference-in-differences comparison. Time is defined by years relative to the mass layoff and control units are assigned a placebo layoff date 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

[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 2014-2016 were recession years in Brazil.

[18] This timeframe ensures that we only cover layoffs taking place after May 31st 2014, consistent with our panel, which is composed of children all enrolled in the 2014 school census (Section 3)

[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).

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 years since layoff, whereby 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 $\delta_{-P}, ..., \delta_{-2}$ estimate anticipation effects. The latter coefficients test whether treatment and 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 the two groups remains below the threshold of 0.20, indicating that any differences in the underlying distributions are small (Cohen, 2013). 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

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

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 4.4. In the same section, we will discuss the external validity of our analysis since mass layoffs could, in principle, significantly differ from regular layoffs.

Table 1: Treatment and control group descriptive statistics

	(1)	(2)	(3)
	Treated	Control	Std. Diff.
PARENT CHARACTERISTICS			
Age	38.3	38.2	0.00
Female	0.26	0.26	0.00
Years of education	9.9	9.6	0.11
Tenure months	24.1	23.6	0.02
Labor income	13459	14321	-0.05
Months worked	9.5	9.1	0.10
Labor income - other parent	5454	4806	0.06
Months worked - other parent	4.2	3.7	0.10
CHILD CHARACTERISTICS			
Age	12.2	12.2	0.00
Gender	0.50	0.50	0.00
Age-grade	11.4	11.4	0.00
School parental income	1.2	1.2	0.05
School ENEM score	9.0	8.9	0.11
MUNICIPALITY CHARACTERISTICS			
Population	1715802	1911104	-0.06
Pib per capita	24355	26713	-0.12
Gini index	0.64	0.65	-0.13
Labor informality	0.39	0.37	0.14
Homicide rate	31	33	-0.12
Observations	546,807	546,807	

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 year (column 2); and the standardized difference between across distributions for the two groups (column 3).

4.2 Effects on parental employment outcomes

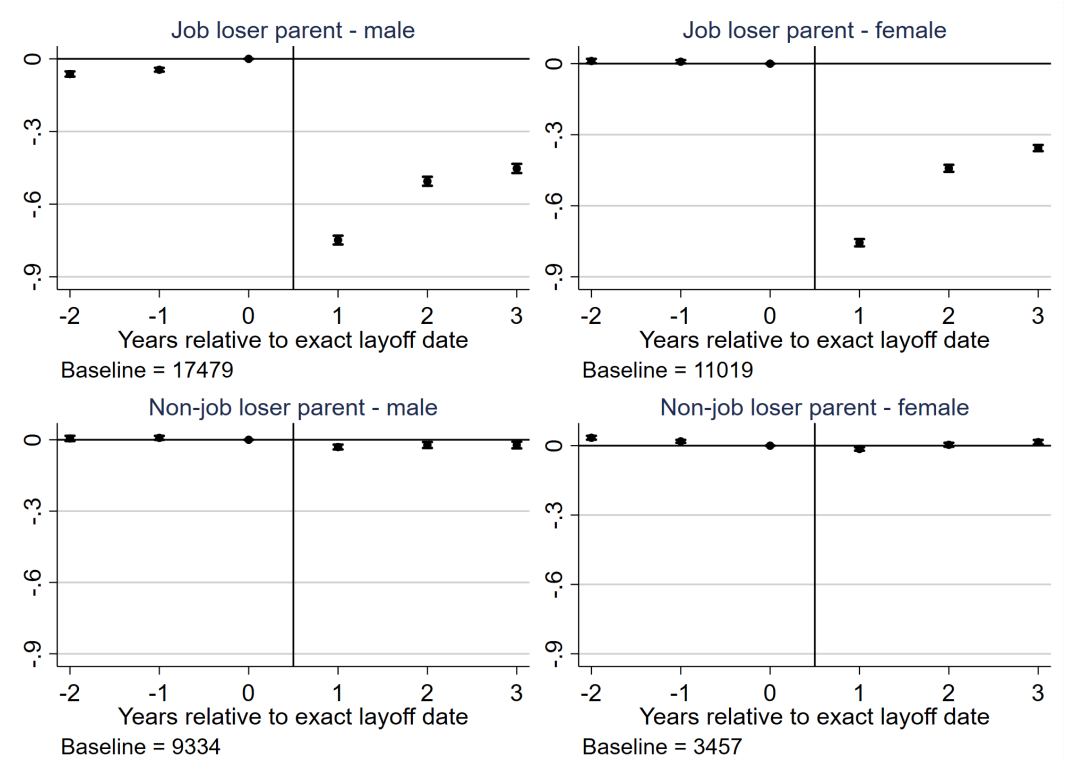
We start by analyzing the impact of job loss on the employment outcomes of parents, following the dynamic specification in equation (1). Only for this analysis, we set time relative

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. Although income recovers over time, the job loss effect remains sizable up to three years after displacement when fathers and mothers earn 48% and 39% less with respect to their baselines.[23] 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 remains sizable, although it decreases by about 20% and 10% for men and women, respectively. Overall, the estimated income losses due to job loss are large relative to existing evidence based on developed countries (e.g., see Bertheau et al. (2022)).

[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 (students' identifiers changed after that) and only indicates whether the student status as of May 31st in each year.

[23]Throughout the paper, we set the baseline as the expected outcome had the treatment not taken place (i.e., $E[Y_i^0|Post == 1, Treat == 1)$, which is given by the mean in the treatment group before treatment plus the mean variation after treatment observed in the control group following the difference-in-difference framework.

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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). 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. Standard errors are clustered at the firm level.

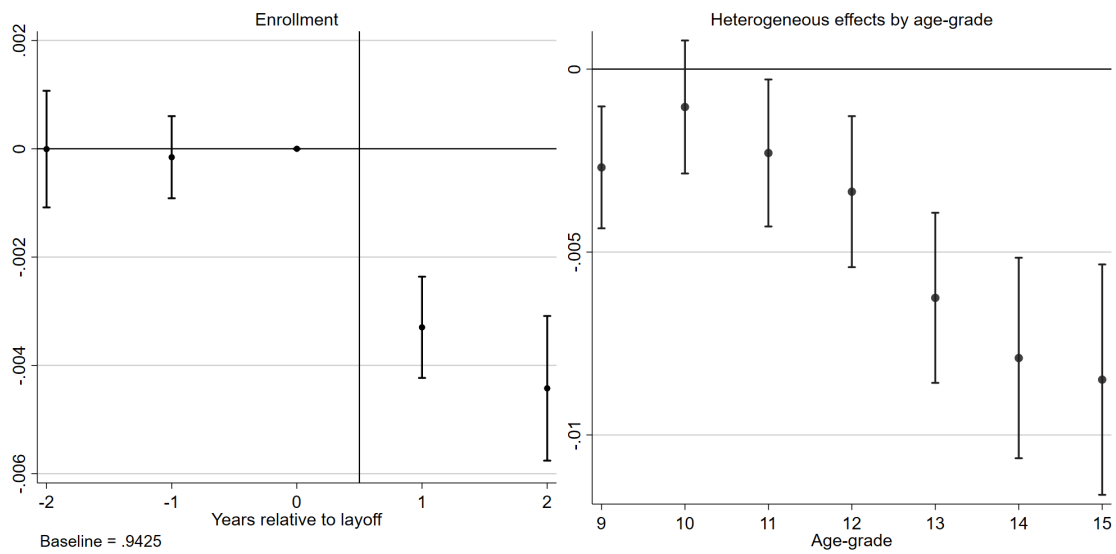
In the two bottom graphs of Figure 2, we analyze the impact on the parent not losing his position in the mass layoff, i.e., the child's mother when the job loser is the father, and vice versa. We do not find any economically significant effect, indicating that added worker effects are minor in this context for both 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 following the dynamic specification in equation (1). As shown in the left panel of Figure 3 children's school enrollment follows similar trends prior to the parental job loss, supporting the common-trend identification assumption. A clear reduction in children's enrollment emerges by the end of the first year following parental job loss and persists in the subsequent year. In the right panel of Figure 3, we show how the average effect in the post-treatment period varies over the students' age-grade, following equation (2) for each

subgroup.[24] There is a negative impact on children in all grades, although it is largest for older children who reduce enrollment by roughly 1 p.p. This is consistent with descriptive evidence showing that dropout risk is largest during secondary education (Figure 1, Panel A).

Figure 3: Effect of parental job loss on school enrollment



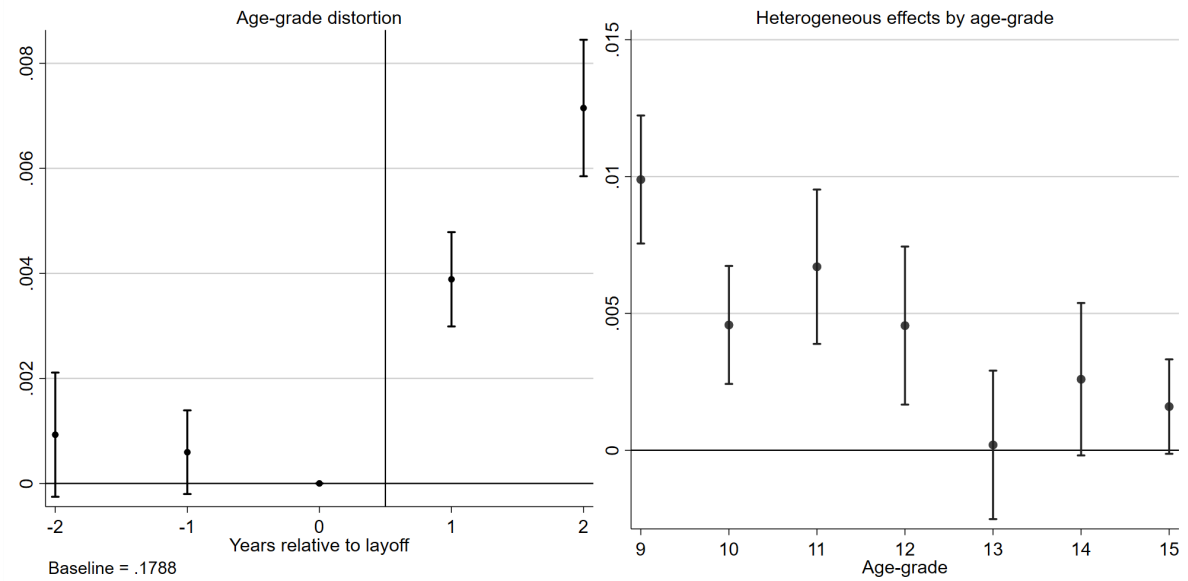
Notes: The figure shows the effect of parental job loss on children's school enrollment. The left graph presents the dynamic treatment effects as estimated from the difference-in-differences equation (1), along with 95% confidence intervals. The right graph shows the average treatment effect in the post-treatment period by the students' age-grade before layoff, as estimated from equation (2) separately for each subgroup. 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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

Next, we study the impacts on age-grade distortion among children who do not dropout after job loss. This outcome indicates that the child is over-age for her current enrollment grade. 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 the left graph of Figure 4 show a significant increase in age-grade distortion amounting to .4 percentage points (p.p.) in the first year following layoff and .7 p.p. in the subsequent year, equivalent to a 2% and 4% increase over the baseline distortion rate. Unlike the impact on enrollment, the adverse effect is concentrated on younger children, as shown in the right graph of Figure 4. 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).

[24] As described in Section 4, we restrict attention to children in age-grade 9-15 before parental job loss so that we can observe them in school for at least three years before and two years after the layoff.

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 age-grade distortion, as opposed to children dropping out and returning to school with grade lags.

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 in the post-treatment period by the students' age-grade before layoff, as estimated from equation (2) separately for each subgroup. 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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

Medium-term effects. In Appendix Figure B.3, we replicate the main analysis for children in *CadÚnico*. Unlike our main sample, these data are not restricted to children enrolled in school in 2014, allowing us to focus on earlier mass layoff years – 2011 and 2012 – and track children's school outcomes for a longer period, up to six years after the layoff (see Section 3.1 for details on these two samples). The results 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 1.5 p.p. in school enrollment and an increase of about 1.6 p.p. in age-grade distortion.

These results also show that our main findings are not driven by the fact that Brazil

experienced a strong recession during our main analysis period (2014-2015), as we find similar results for the 2011-2012 period when the Brazilian economy was fairly stable. 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 in detail in Section 5.

4.4 Robustness

In Appendix Section B.4, we discuss 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 mass layoffs where a larger share of workers are displaced compared to our baseline, or when using plant closures. Furthermore, our estimates continue to hold when we adopt an intention-to-treat approach in which the treatment group comprises all workers employed (displaced or not) in treated firms at the beginning of each mass layoff year. This strategy mitigates concerns about workers anticipating mass layoffs and leaving the firm before they take place. 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 year – [25], when reweighting the sample to perfectly match population characteristics, and when adding flexible municipality $\times$ time fixed effects. The latter indicates the ability of our empirical strategy to net out the individual effect of job loss by comparing parents and children who face similar area-level conditions. Third, 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 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 children. 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.

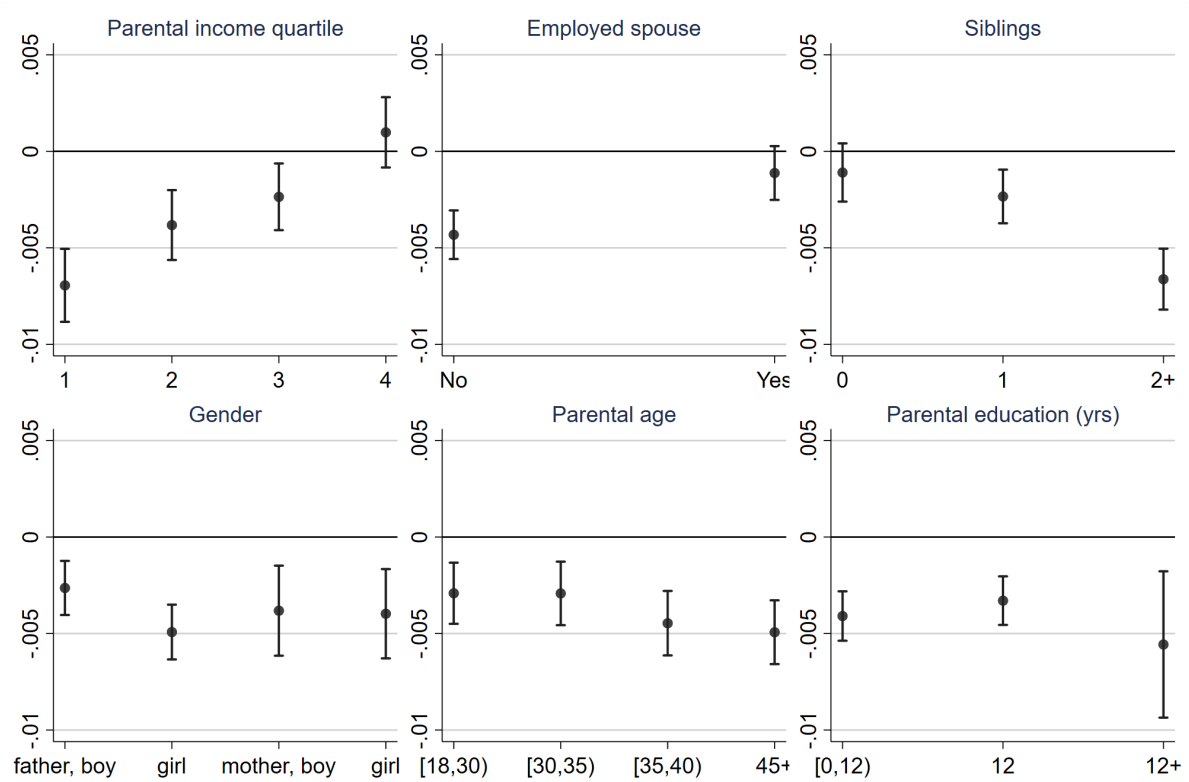
[25] Previous work have used either of these two approaches; for instance, Ichino et al. (2017) and Schmieder et al. (2018) define the control group similarly to our baseline empirical strategy, while Jacobson et al. (1993) and Couch and Placzek (2010) restrict the control group to workers who are continuously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

4.5 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 parental income (top-left graph), which is the strongest among the several characteristics that we analyze.[26] This suggests that the income losses caused by job loss may be an important mechanism since low-income families tend to be more liquidity constrained and face more difficulties in dealing with the large income losses. Consistent with this idea, the effects are stronger in families where the non-displaced parent is not employed in the baseline and in families with more children. In turn, a clear gradient does not emerge over gender, parental age, and education. The similar effects across parental gender do not support mechanisms related to the relative time allocation of fathers and mothers following job loss. Appendix B.5 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.

[26]Parental income is defined by the sum of the both parents' formal labor income in the pre-displacement year.

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 year. Standard errors are clustered at the firm level.

4.6 Effects on children beyond the school system

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

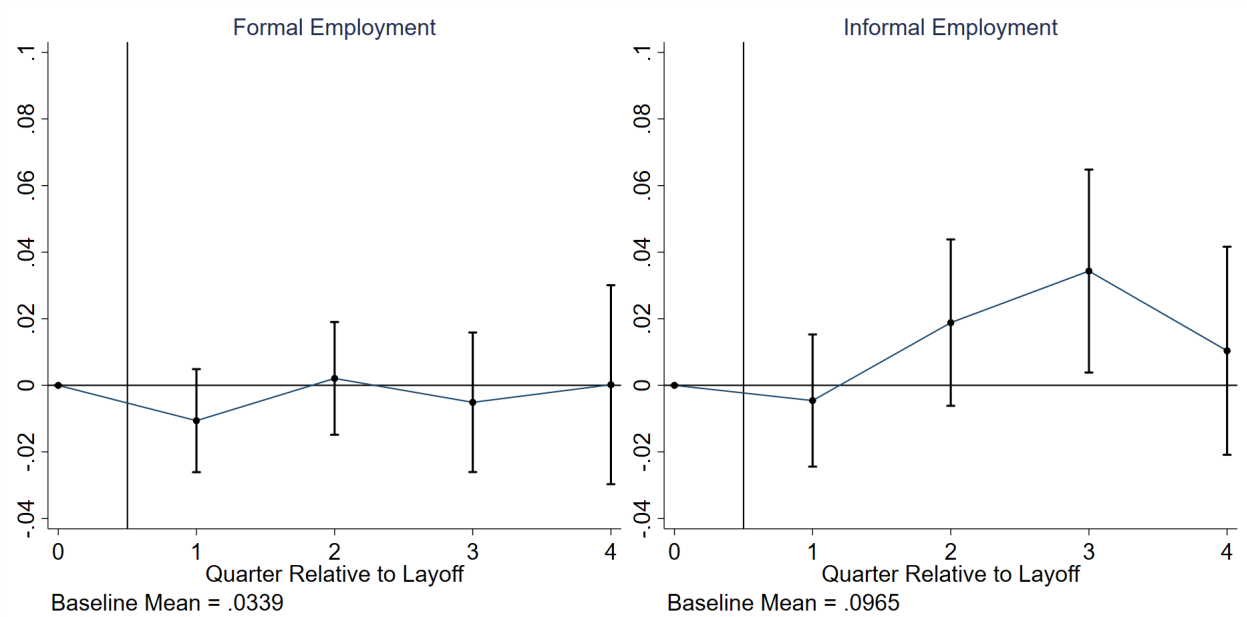
4.6.A Teenage work

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 to them leaving or performing 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 main reason (39%) for leaving school among dropouts in nationally representative surveys.[27] To shed

[27] Child and teenage work remain relevant phenomena in the country although 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. The 2010 Population Census indicates that 3.8% of children aged 10-13 work.

light on this aspect, we exploit longitudinal survey data interviewing families for five subsequent quarters tracking formal and informal employment outcomes for individuals from the age of 14, whereby 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 aged 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 sufficient to trigger school dropouts or set children behind in classes, potentially leading to retention.

Figure 6: Effect of parental job loss on teenage work



Notes: The figure shows the dynamic treatment effects of parental job loss on teenage work as estimated from the difference-in-differences equation (1), along with 95% confidence intervals, based on the 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 indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the individual level.

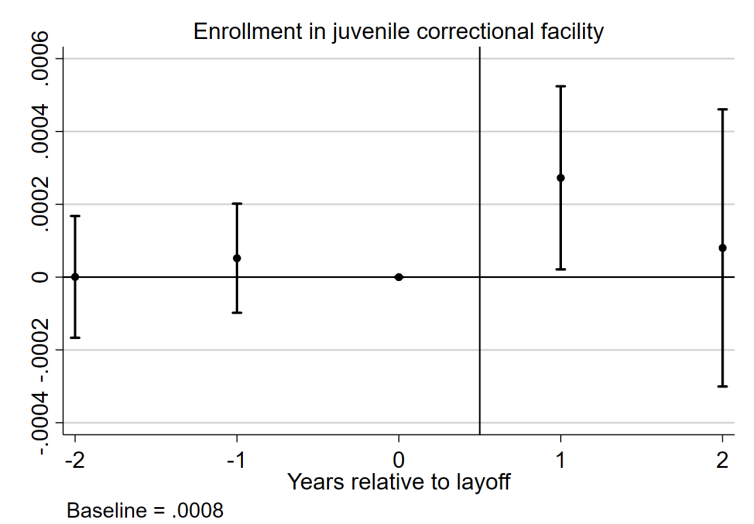
4.6.B Youth criminal behavior

We now analyze whether parental job loss affects children's criminal behavior. More specifically, we measure the probability that teenage boys enroll in a school operating in a correctional facility, as observed in the school census.[28] As shown in Figure 7, parental displacement increases such probability. In line with the fact that there is substantial under-reporting

[28] We focus on boys as they represent 87% of the population in these facilities.

in crime outcomes and 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 33% increase by the end of the layoff year. Although we cannot identify crime types, the fact that a large portion of crime is economically motivated is consistent with an income mechanism.[29] Nevertheless, the increase in children's crime probabilities could also hint at emotional turmoil in the household, which we discuss later in detail.

Figure 7: Effect of parental job loss on teenage crime, boys



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 year. The sample covers male children aged 14-16 in the baseline period. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

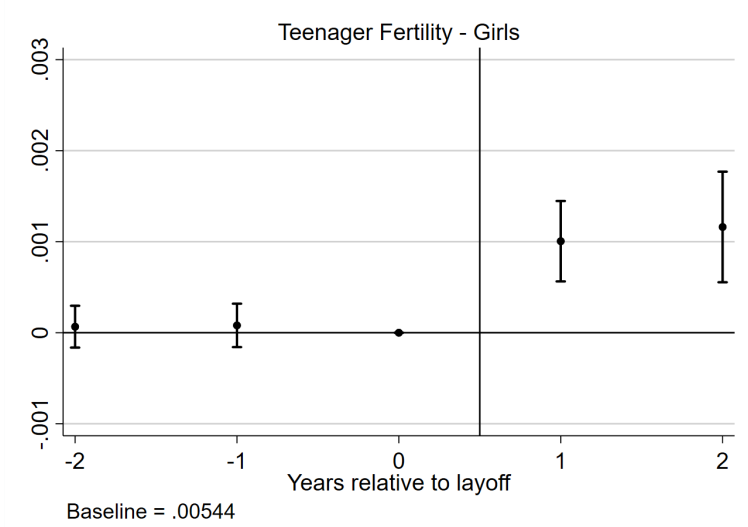
4.6.C Teenage fertility

We now study whether parental job loss affects early pregnancy by teenage girls using *CadÚnico* data to track fertility. Early motherhood is associated with several negative outcomes for both the mother and their offspring (e.g., see Kearney and Levine, 2012, 2015) and might be indicative of psychological distress in the household. The results are presented in Figure 8, indicating that parental job loss increases the risk of teenage fertility for girls. The effect is quantitatively large, indicating that such probability increases by about 18% with respect to the baseline. This suggests that the income losses brought about by parental job

[29] Economically motivated offenses account for 75% of criminal prosecutions for defendants aged 18.

loss may also bring emotional turmoil to the household, possibly contributing to children's worse school outcomes.[30]

Figure 8: Effect of parental job loss on teenage fertility



Notes: The figure shows the dynamic treatment effects of parental job loss on the probability of teenage fertility by children, as estimated from the difference-in-differences equation (1), along with 95% confidence intervals. The sample covers female children aged 11-15 in the baseline 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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

4.6.D School choices

We turn to the analysis of the impacts of parental job loss on school choices. This set of outcomes is 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 again restrict the analysis to children who do not drop out and analyze two different measures related to school quality. First, we track the INSE index, which measures the school's socioeconomic background based on goods and services owned by the pupils' families, in addition to parental income and education.[31] Second, we track the probability that the child enrolls in a public school.

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

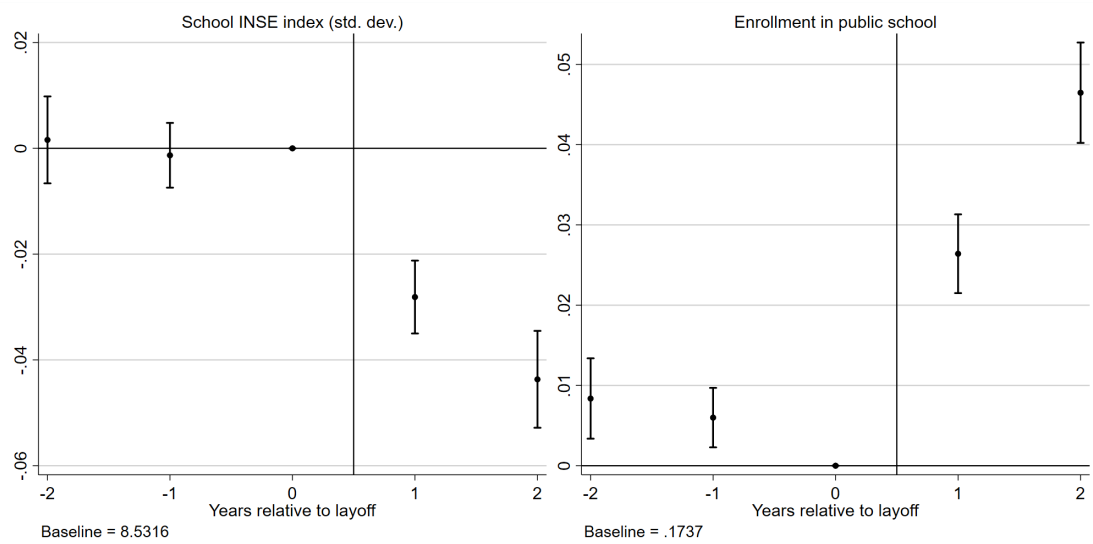
[30] We find null effects on teenage fertility for boys, which is a less common phenomenon for which baseline rates are about 65% lower relative to girls.

[31] The index is provided by the Ministry of Education.

advantageous backgrounds in line with the evidence in Figure 1. The results – presented in Figure 9 – show that parental job loss has significant negative impacts on school quality. In the years following parental job loss, affected children move to schools where socioeconomic background is lower – up to .04 standard deviations lower INSE index – and are up to 4.5 p.p. more likely to enroll in public schools.[32] In Appendix Figure B.9, we show that similar results hold when using alternative measures for school quality: (i) school ENEM scores, an important national examination taking place at the last year of secondary education, which determines access to several public and private universities in the country; and (ii) the average parental labor income in the school.

Overall, these results indicate that parents resort to costly insurance mechanisms that sacrifice the education quality of the next generation to deal with the income losses caused by job loss. Unlike poorer students who experience higher dropout risk, the effects along the school quality margin are – as expected – mainly driven by wealthier students. As for our main outcomes, we show in Appendix Section B4 that the impacts on crime, fertility and school quality survive a battery of robustness tests addressing several identification concerns.

Figure 9: 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 school INSE index (left), and public school enrollment (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 year. The sample covers children enrolled in private schools in the baseline period. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard a clustered at the firm level.

[32] In Appendix B.6, we address the fact that there is a mild pre-trend deviation for the results on public school enrollment. Specifically, we show that these effects are robust to inference allowing for some degree of violation of the common-trend assumption, following the method proposed by Rambachan and Roth (2022).

4.7 Discussion on mechanisms

The results presented so far are consistent with the idea that the negative impacts on children's education are driven by the income losses brought about by parental job loss. As shown in Section 4.2, these losses are sizable and persistent, imposing large financial costs on the family. In line with the income mechanism, the effects on children are stronger in groups who are expected to be the most financially constrained following job loss, namely families with lower income, more children and where the non-displaced parent is not employed (see Section 4.5). The income mechanism is further supported by the evidence in Section 4.6 showing that children's labor supply and crime and parental investments in school quality are negatively affected by job loss. Although these are joint decisions, they are broadly consistent with the idea children are affected by the income losses in the family, which in turn may drive the adverse effects on education. In Appendix B.8, we further study migration patterns leveraging *CadÚnico* information on families' residential location and re-employment location for parents. We find suggestive evidence that children are more likely to move to poorer areas. Although these effects are not particularly large quantitatively, they are indicative of the financial struggle faced by families following job loss and in line with the idea that income losses are an important mechanism driving the impacts of parental job loss on chidlren.

Beyond the economic dimension, the income losses brought by job loss may create emotional turmoil in the household, which may work as a mediating factor for the negative effects on children. Although we cannot directly test these mechanisms in our data, this idea finds support in related literature linking job loss to mental health problems and stress (Kuhn et al., 2009; Charles and DeCicca, 2008; Zimmer, 2021). The positive impacts that we find on teenage fertility rates are indicative of psychological distress in the household and such a channel is also in line with existing evidence studying job loss impacts in Brazil during a similar period. First, Bhalotra et al. (2021) shows that job loss by men and women leads to more domestic violence in the household.[33] Second, Britto et al. (2022) shows that workers are more likely to commit non-economically motivated crime following job loss, which may be indicative of psychological distress in the household.[34]

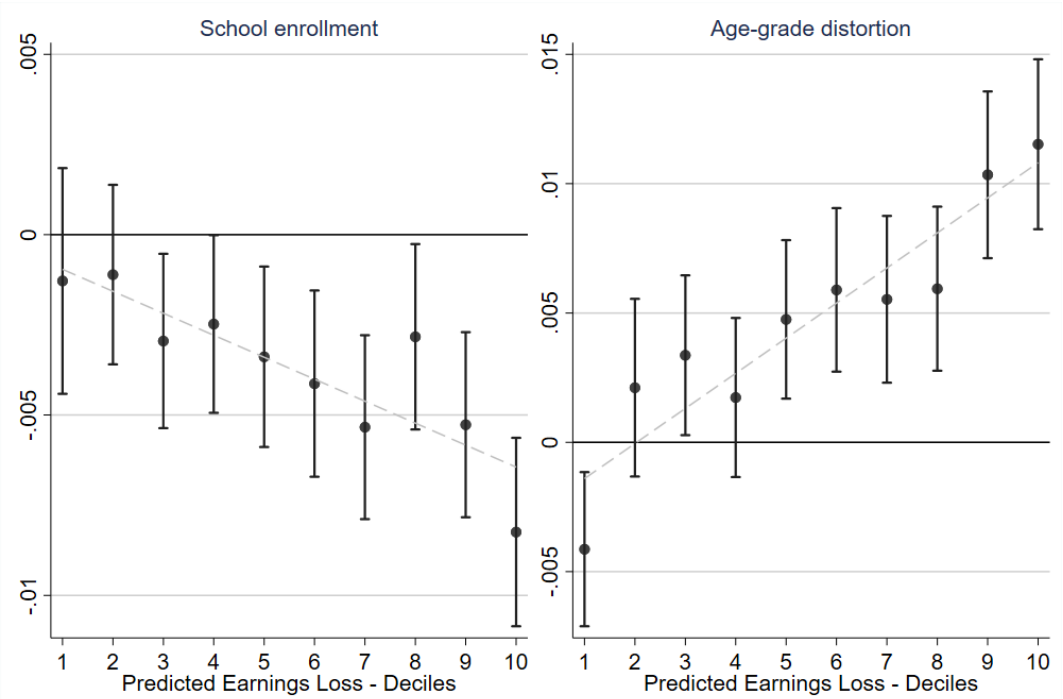
We complement these results by testing whether predicted income losses following parental job loss can explain the impacts on children's educational outcomes. Accordingly, we use

[33]Despite the increase in domestic violence, we do not find higher separation rates for poorer families in *CadÚnico*, it is possible to track such an outcome. This may be explained by the fact that families are financially constrained and separation is costly. These results are presented in Appendix B.8.

[34]Although job loss could create psychological damage independently of the financial losses, this idea is not fully consistent with the fact that we find null effects for children in high-income families (see Figures 5 and B6) and families whose predicted earnings losses by job loss are small (see Figure 10).

several pre-displacement characteristics to predict labor income losses for each parent, and study how the impacts on children's education vary across the predicted losses, in an exercise similar to Hilger (2016).[35] Subsequently, we re-estimate the effect of job loss on school enrollment and age-grade distortion by deciles of the predicted income losses. The results presented in Figure 10 show that the predicted loss in labor income can predict the negative impacts on children's education remarkably well. While the impacts are close to zero for families with low predicted income losses, they are substantially larger for families with large predicted losses.[36]

Figure 10: 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 labor 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 year. Standard errors are clustered at the firm level.

[35]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.

[36]For distortion, parental job loss may even be beneficial for families where income losses are expected to be low due to fast employment recovery. This may be explained by an increase of parental time investments, also being in line with the evidence in Figure B6 showing beneficial effects (albeit not statistically significant) on distortion for families in the upper quartile of the income distribution.

Overall, these several pieces of evidence suggest that income losses are likely to be a key driver of the impacts on children's education. In Section 6, we will use variation in access to unemployment benefits to provide more direct and compelling evidence on the role of the income mechanism.

5 Parental Job Loss Effects on Children's Completed Education

So far, our analysis has shown negative effects on children's education two years after job loss, and that these impacts remain strong up to six years after the shock for children in *CadÚnico* welfare registry. We now exploit the timing of parental job loss relative to children's expected high-school graduation date to quantify long-term impacts on completed schooling.

5.1 Empirical strategy

We implement a difference-in-differences design that compares children whose parents lost a job in mass layoffs before and after their expected high-school completion year.[37] 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, namely age-grade 17. 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.[38] We select parents losing their jobs in mass layoffs during the 2011-2013 period and focus on children whose parental layoff takes place from three 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.[39] We run this analysis for children in *CadÚnico* who we can link to their parents without the restriction on school enrollment in 2014 (see Section 3.1 for the details on these two samples).

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

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

[37] We follow a similar strategy to Hilger (2016) who studies the impact of parental job loss on college enrollment using US data.

[38] Our analysis based on expected high-school completion date measured three years before job loss is motivated by the fact that age-grade distortion rates are high in Brazil. Hence, implementing this analysis over age would imply a large degree of noise given that the statutory high-school completion age is a poor predictor of completion timing.

[39] We additionally impose that control units have the same expected graduation date as treated units.

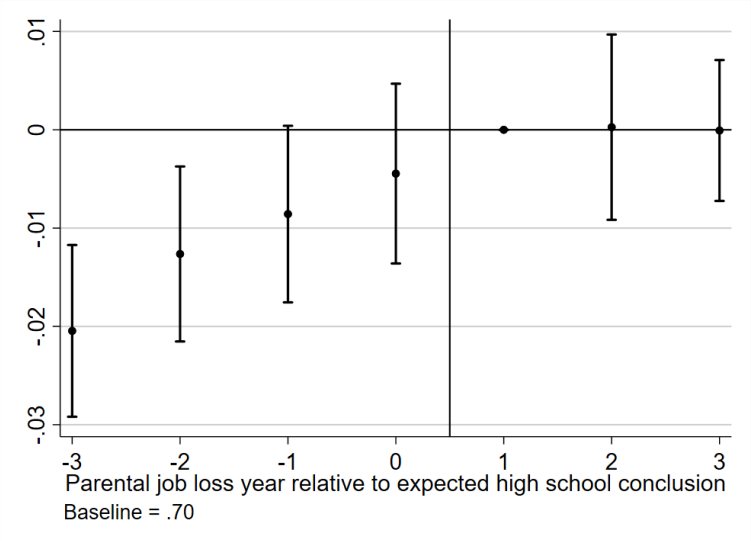
where the subscript i identifies each parent-child – our unit of analysis – and t identifies calendar years relative to children's expected high-school graduation date. $Treat_i$ is an indicator for the treatment group and $Time_t$ identifies each period t. Time-varying shocks are absorbed by the full 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 completion – δ_1 being the omitted category. Unlike equation (1), we cannot include individual fixed effects since we leverage variation in layoff dates across individuals.

5.2 Results

The results of the analysis are presented in Figure 11. It shows that children whose parents are displaced before their expected graduation year are less likely to complete high school. Children whose parents lose their job earlier are more strongly affected, being 2 p.p. less likely to complete high school when the shock takes place three years before the expected completion date. 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. In Appendix C.1, we show that these estimates are not driven by compositional changes in the characteristics of displaced parents over time. Specifically, they are robust to controlling for parental characteristics – education, gender, income, and tenure – and the inclusion of year of displacement X treatment status X municipality fixed effects. Our preferred specification including all controls shows that treated children are on average 1.5 p.p. less likely to complete high school (Appendix Table C1, column 5).

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 is indicative of long-lasting human capital losses.

Figure 11: 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. Standard errors are clustered at the firm level.

6 Unemployment Insurance and Children's Outcomes

The results presented above establish a strong link between job loss and children's outcomes, within and beyond schools. In this section, we investigate the possible mitigating effects of UI. The objective here is twofold: first, to evaluate the policy; and second, this analysis will provide more direct evidence on the role of income losses as mechanism driving the impacts of job loss on children.

6.1 Research design

UI 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, while the replacement rate is generally higher than in both places. All workers displaced with at least six tenure months are eligible, aside from the fact that repeated UI claims require a minimum sixteen-month waiting period between the layoff dates 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.[40] Specifically, we compare workers displaced

[40] Given that job turnover rates are extremely high in Brazil, 37% and 90% of formal jobs lasting less than one and three years, respectively, it is not uncommon that workers are displaced twice within relatively short time spams.

a few days before and after the sixteen-month waiting period. We estimate the following local linear equation:

$$Y_i = \alpha + \beta D_i + \gamma_1 X_i + \gamma_2 D_i * X_i + \epsilon_i \tag{4}$$

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 sixteen-month cutoff, whereas D_i is a 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 that 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) – permutation tests, and 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 2009-14 period, using data on UI payments for the same period to study UI take-up.[41] 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 sixteen-month cutoff date is within three 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 sixteen-month cutoff.[42]

Our main UI analysis is based on a sample of children in *CadÚnico*, whose linkage to parents in the employment data is not conditional on school enrollment (see Section 3.1 for the details).[43] In addition, we focus on displaced parents in the first three quartiles of the income distribution, for whom UI replacement rate is higher at 85% compared to only 50% in the upper quartile.[44]

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

[41] 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.

[42] It is worth noting that the sixteen-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.

[43] Using our main parent-child linking sample would not allow us to study impacts on children's enrollment for parents displaced in the 2009-14 period, since it is conditional on children's school enrollment in 2014.

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

6.2 Effects on school enrollment and age-grade distortion

Table 2 presents our main estimates based on equation (4). Panel A shows that eligible parents are 64 p.p. more likely to take up UI benefits, which last on average for 2.7 months.[45] 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), whereas the effects on age-grade distortion are concentrated on younger pupils (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 average enrollment for older children by 1.4 p.p. in the three-year period after the layoff, 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 12 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, permutation tests where the main estimate is compared to the distribution of RD estimates at placebo cutoff points, dropping from the sample observations near the cutoff; and manipulation robust inference (see Appendix Tables D1, D2, and D3, and Figure C4).

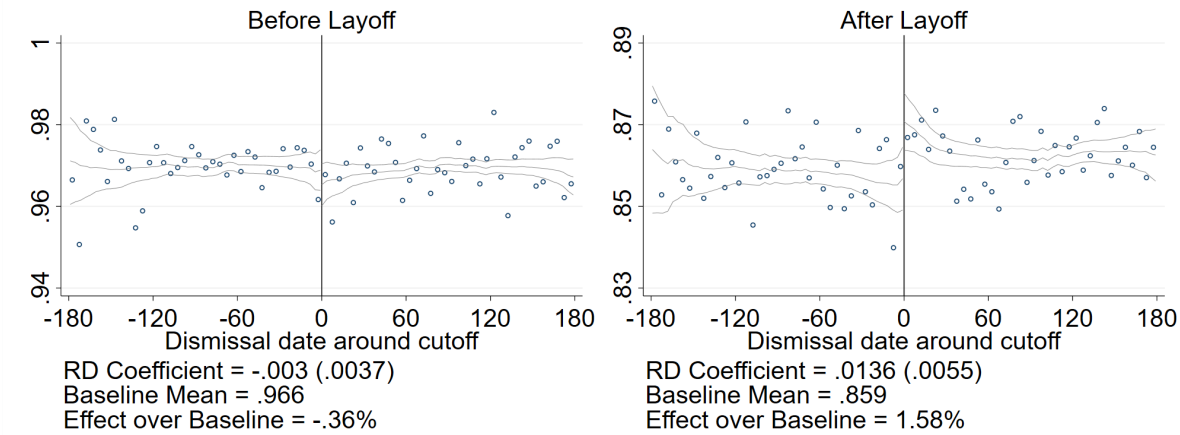
The positive impact of UI eligibility on enrollment supports the idea that income losses are a relevant mechanism driving the impacts of job loss on children's education. However, it is well known that UI eligibility induces workers to take longer to find jobs – as is the case in our data, see column 4 in Table 2 – and may increase the time that parents spend with children. Thus, an alternative hypothesis is that UI reduces dropouts due to parental time. We test for whether unemployment duration is a mediating factor by controlling for it 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 1.4 p.p. (0.006 s.e.) 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. However, these estimates are not particularly precise, and we may lack the power to identify meaningful effects comparable to the estimates presented in Section 4.[46]

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

[46] Standard errors are 0.7 p.p. and 0.9 p.p. in the sample for younger and older children, which are somewhat large compared to the impacts on grade distortion found in the previous section going up to 1 p.p. for younger pupils (Figure 4).

Figure 12: Effect of UI eligibility on enrollment, children in age-grade 13-17, before (placebo) and after job loss



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

Table 2: 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	0.643***	2.709***	1832.053***	11.493***
	(0.006)	(0.023)	(16.798)	(0.919)
Baseline Mean	0.09	0.30	213.11	46.93
Observations	112,912	112,912	112,912	112,912
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	-0.009	-0.004	-0.013	0.014**
	(0.009)	(0.004)	(0.009)	(0.006)
Baseline Mean	0.27	0.97	0.32	0.86
Observations	43,014	37,399	40,637	43,014
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	0.003	0.004	0.001	-0.002
	(0.006)	(0.003)	(0.007)	(0.003)
Baseline Mean	0.17	0.97	0.26	0.96
Observations	69,898	59,438	69,066	69,898

Notes: This table shows the effect of UI eligibility – as estimated from the local linear RD equation (4) – on UI outcomes (Panel A, columns 1-3), unemployment duration (Panel A, column 4) and children's average school outcomes two years before and three years after parental layoff (Panels B and C). Enrollment rates before layoff are measured two years before since all children in the sample are enrolled in the year before layoff. The sample includes displaced parents with at least six months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility for unemployment benefits, namely sixteen 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).

6.3 Effects on crime and teenage fertility

We now evaluate whether access to UI can improve other children's outcomes beyond school, testing for its impacts on crime and fertility by teenage boys and girls, respectively. In Section 4.6, we have shown that both outcomes are adversely affected by job loss. The estimates shown in Appendix Table D4, columns 1-2 show statistically insignificant results on teenage fertility. In turn, they show that UI eligibility has a strong negative effect on the probability that children are sent to juvenile correctional facilities, which decreases by .004 p.p. in the three years following the layoff, a 49% decline relative to the baseline (see Appendix Figure D.5a for the graphical evidence). This is in line with the idea that the increase in children's crime may be driven by a mechanism where children try to compensate for the income losses in the family with economic crime. Similarly to the results on enrollment, the crime reduction effects does not seem to be mediated by higher parental time investments as the estimates barely change when unemployment duration is added to the RD regression as a control.[47] These estimates are fairly robust to varying RD specifications (Table D5), and permutation tests (Figure D.6a).

6.4 Effects on school choices

In Appendix Table D4, columns 3-4 we replicate the RD analysis using our main parent-child links dataset (used in Section 4) conditional on school enrollment in 2014 to study UI impacts on school quality.[48] 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.D). Although the sample is largely reduced by this restriction and the statistical power is not particularly high, the results in Appendix Table D4 offer some indication that UI eligibility could partially mitigate job loss effects on school quality. They show that UI eligible parents enroll children in schools with .094 standard deviations higher INSE index and that are 1.3 p.p. more likely to be private, although only the former effect is statistically significant. In Appendix Table D5, we show that the results on school quality – as measured by the INSE index – are robust to alternative RD specifications (Table D5), and permutation tests (Figure D.6b) (see Appendix Figure D.5b for the graphical evidence). Overall, this analysis offers some suggestive evidence that UI may also mitigate some of the impacts of job loss on school quality.

[47] Specifically, the estimates remain -.0039 (.0018) and -.0040 (0.0018) when adding unemployment duration as a linear or weekly fixed effects control, respectively.

[48] This sample is better suited to studying the impacts on school quality choices – as it is more relevant for richer children initially enrolled in private schools – than the *CadÚnico* sample, which is focused on poorer families mainly enrolled in public, lower-quality schools.

6.5 Implications for welfare analysis

Access to UI is effective at mitigating the impacts of job loss on school enrollment for teenagers. It nearly entirely offsets the negative effect of parental job loss on enrollment rates in the three-year period following the layoff,[49] particularly if we consider that UI benefits only cover a small portion of the income losses brought by job loss: while the average additional transfers at the UI cutoff amount to R$1,838, the average yearly labor income losses are as large as R$5,161 in the first three years following the layoff.[50] The effectiveness of these transfers is in line with the idea that they take place when workers are most in need, namely the first semester following layoff.[51]

In light of this sizable effect – which compensates most of the negative effects of parental job loss on teenage enrollment – we discuss some implications for welfare analysis. We follow the traditional social insurance literature where optimal UI provision balances the benefits of consumption smoothing against the efficiency costs due to longer unemployment duration caused by unemployment benefits (e.g., see Baily (1978); Chetty (2006)). Specifically, we focus on the fact that the additional education accrued by children will likely generate higher wages and additional government revenue through taxation. This may compensate part of the efficient costs due to lower labor supply by workers receiving unemployment benefits. Even more so if one considers that returns to schooling are typically high in developing countries.

We provide a simple back-of-the-envelope calculation for the net present value of the additional government revenue in Appendix Table D6. We consider a working life of 35 years, returns to schooling of 12% for each additional year of education in Brazil as in Ferreira and Paes de Barros (1999)[52], a yearly real interest rate of 5%, a baseline yearly formal income of R$9,447 and a taxation equal to 32.5% following the tax wedge for Brazil in (OECD, 2016).[53] The additional government income amounts to 14 cents for each R$1 reaching mechanical beneficiaries.[54] This is sizable compared to recent estimates of UI efficiency costs in Brazil,

[49]We re-estimate the impact of parental job loss on the sample used in the RD analysis, finding a 1.8 p.p. statistically significant reduction in enrollment rates in the three-year period following the layoff.

[50]Labor income losses follow the specification in equation 2 based on a sample similar to the one used in the UI analysis.

[51]This is in line with the strong decline in consumption after displacement in Brazil reported in Gerard and Naritomi (2021).

[52]This is a conservative choice as other estimates for developing countries find higher returns to schooling, e.g., see Patrinos and Psacharopoulos (2020).

[53]Yearly labor income is based on the average formal labor income for displaced workers in the *CadÚnico* sample. The 5% interest rate is below the 3% rate used in other studies such as Hendren and Sprung-Keyser (2020). We follow Hendren and Sprung-Keyser (2020) in using the labor tax wedge to calculate forgone government revenues on labor income. This is a conservative choice as it ignores forgone government revenues on consumption taxes, which are sizable in Brazil.

[54]We estimate a net present value of R$891 in forgone tax revenues. The average transfer reaching me-

which amount to 21.7 cents per R$1 reaching mechanical beneficiaries while taking into account traditional job search behavioral responses (Gerard and Gonzaga, 2021).

While a more comprehensive welfare evaluation is beyond the scope of this paper, this simple back-of-the-envelope estimate indicates that our findings may be quantitatively relevant for analysis on optimal UI provision in similar contexts when studying transfers to displaced workers with teenage children. These estimates could be even higher when taking into account the impacts of UI eligibility on crime and school quality (Sections 6.3 and 6.4).

7 Discussion and Concluding Remarks

Overall, our analysis reveals that parental job loss imposes significant losses for children in the household. It causes large and persistent income losses to the family and generates adverse effects on children that extend well beyond the educational domain to several life dimensions. In addition to increasing dropouts and age-grade distortion, it increases teenage work, crime and pregnancy rates, and causes parents to reduce educational investments by moving children to lower-quality schools. The impacts on children's education are persistent and lead to a sizable 1.5 p.p. reduction in the probability that they complete high school. Such impacts likely extend in the long term with probable consequences for children's lifetime earnings in the labor market. The impacts on education are remarkably pervasive, affecting most groups and geographical areas in our data, despite the fact that Brazil is a large and heterogeneous country.

In terms of mechanisms, several pieces of evidence provided in Section 4 support the idea that income losses caused by job loss are an important mechanism driving the effects on children's education. In turn, the analysis on UI eligibility offers more direct and compelling evidence supporting the idea that income losses are a key mechanism. Notably, it shows that income provision mitigates some of the impacts of parental job loss on children's outcomes – in particular on teenage enrollment and crime – while leveraging exogeneous variation in income support.

In terms of policy relevance, our analysis shows that widespread and traditional job insurance policies such as UI may mitigate the adverse impacts on children's outcomes. Our back-of-the-envelope calculation suggest that these findings may also be quantitatively relevant for welfare analyses. In particular, they indicate that the positive UI impacts on children's education may significantly attenuate UI efficiency costs due to distortions on job search behavior. More generally, it suggests that other job insurance policies alleviating

chanical beneficiaries is given by the average transfer divided by one plus the 21.7%, given by the behavioral response estimated in Gerard and Gonzaga (2021) for Brazil in a similar time period.

liquidity constraints upon job displacement – such as mandatory severance pay – could also be effective. These results may be taken into account when implementing a broader cost-benefit analysis on social insurance policies.

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Online Appendix to "The Kids Aren't Alright: Parental Job Loss and Children's Outcomes Within and Beyond Schools "

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September 26, 2022

A Appendix to Section 3

A.1 Linking parents to children across administrative datasets

To link children and parents across different administrative datasets, we develop a procedure to link the students' ID in the school census to their and their parents' tax codes. We start by creating a person registry for the Brazilian population by combining all individuals ever observed in the employment data (RAIS) 2002-19 or *CadÚnico* welfare registry 2011-19 into a single registry containing their (full) name, unique tax code, and date of birth. This covers almost the totality of the Brazilian population (90% when compared to the Brazilian population in 2019).

We use this registry to identify parents and children in the 2014 school census with their tax code. For this purpose, we use information on the child's name, birth date, and parents' names available for all children enrolled in the 2014 school census. We start by identifying children with their tax code, proceeding in rounds. First, we identify students present in *CadÚnico* 2008-13, for whom we directly observe a link between their student census ID and tax code 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 latter procedure is aided by the fact that Brazilians have multiple surnames, and as a result about 50% of the population have a unique full name. This procedure by rounds allows us to identify 33,325,985 million students in the 2014 school census with their tax code, accounting for 74% 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 tax codes. 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 procedure allows us to identify 25,911,310 fathers and 32,200,395 mothers with their tax codes – 58% and 72% of the total in the 2014 school census, respectively – while 37,040,161 children can be linked to at least one parent. We will use the parents' tax codes to track their employment outcomes in the employment data. In Table A1, we show that the characteristics of children successfully linked to their parents are similar to the population of students in the 2014 school census, indicating that our analysis sample is fairly representative of the population.

Table A1: Descriptive statistics, children with and without linked parents, 2014 school census

	(1)	(2)	(3)
	Linked parent	Population	Std. Diff.
Child characteristics			
Age	11.6	11.5	0.02
Gender	0.49	0.49	0.00
Age-grade	10.7	10.6	0.03
Age-grade distortion	0.13	0.14	-0.01
Private school	0.15	0.12	0.07
Municipality characteristics			
Population	753076	720021	0.02
Pib per capita	16522	16141	0.02
Gini index	0.62	0.62	0.01
Labor informality	0.47	0.47	-0.03
Homicide rate	33	33	0.00

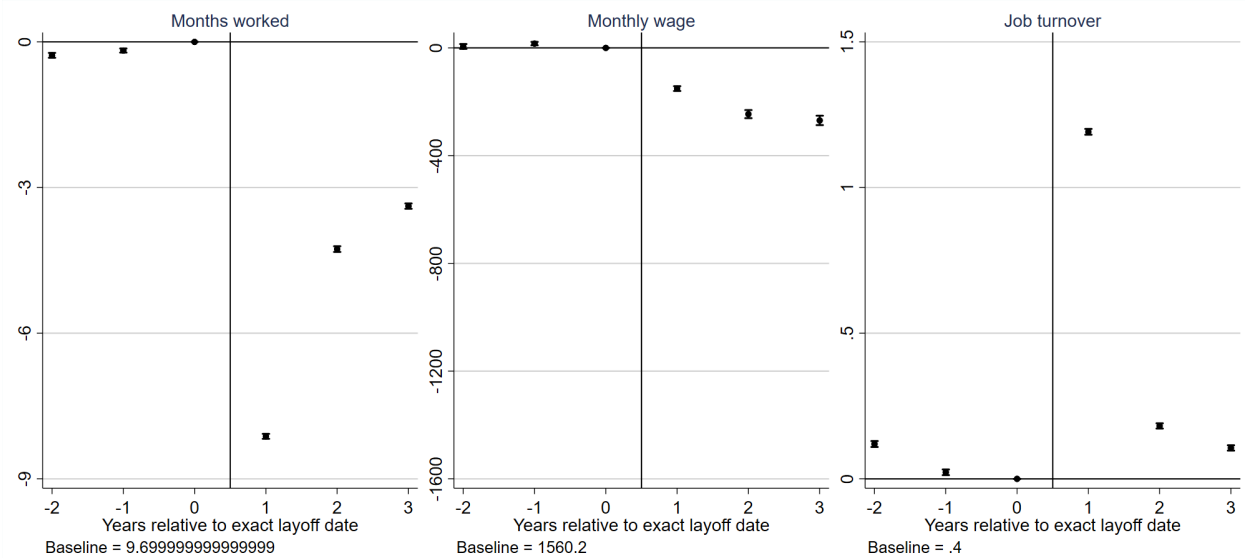
Notes: This table reports the average characteristics for children whom we successfully link the tax code for at least one parent in the 2014 school census (column 1), the remaining children (column 2), and the standardized difference between the two groups (column 3).

B Appendix to Section 4

B.1 Effects 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 other (formal) employment outcomes such as employment, wages, and turnover.

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

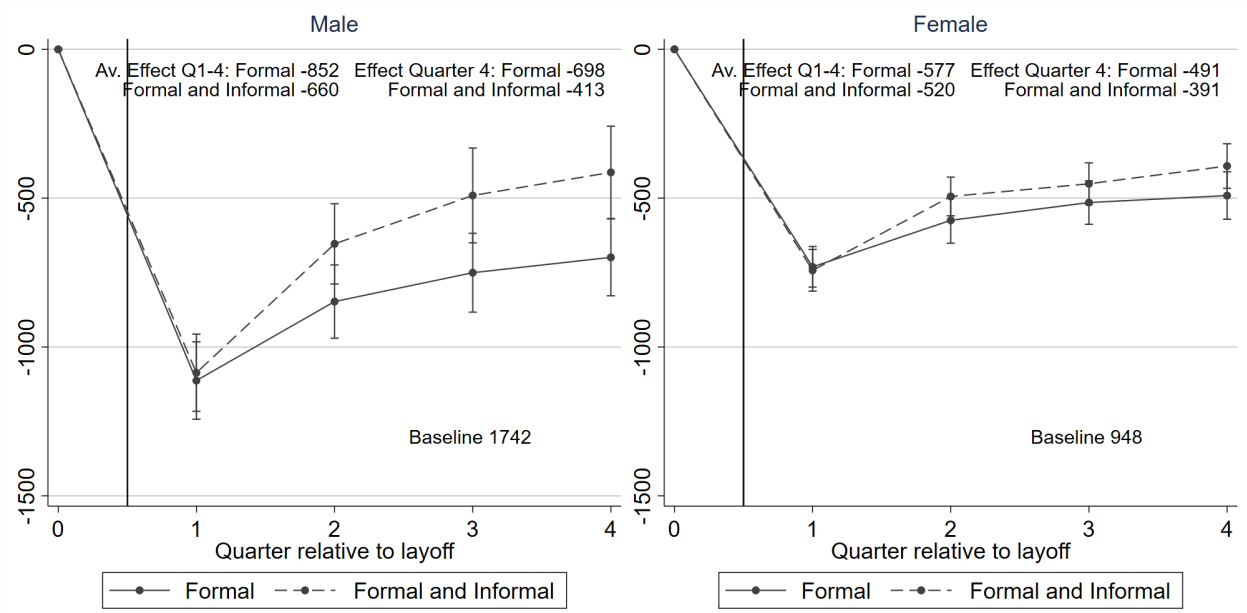


Notes: This figure shows the dynamic treatment effects of parental job loss on other labor market outcomes, as estimated from the difference-in-differences equation (1), along with 95% confidence intervals. The treatment group comprises displaced workers, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

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 2012-19 period. 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 comprises individuals who are initially formally employed in interview quarter 1 and who were displaced in quarter 2, whereas the control group comprises workers employed in both quarters. In addition, we implement the same sample restrictions as in our main analysis, namely parents in the age range of 18-60 years old initially formally employed

in private firms. The results in Figure B2 indicate that income losses are only about 20% and 10% smaller for men and women, respectively, when taking into account the take-up of informal jobs.

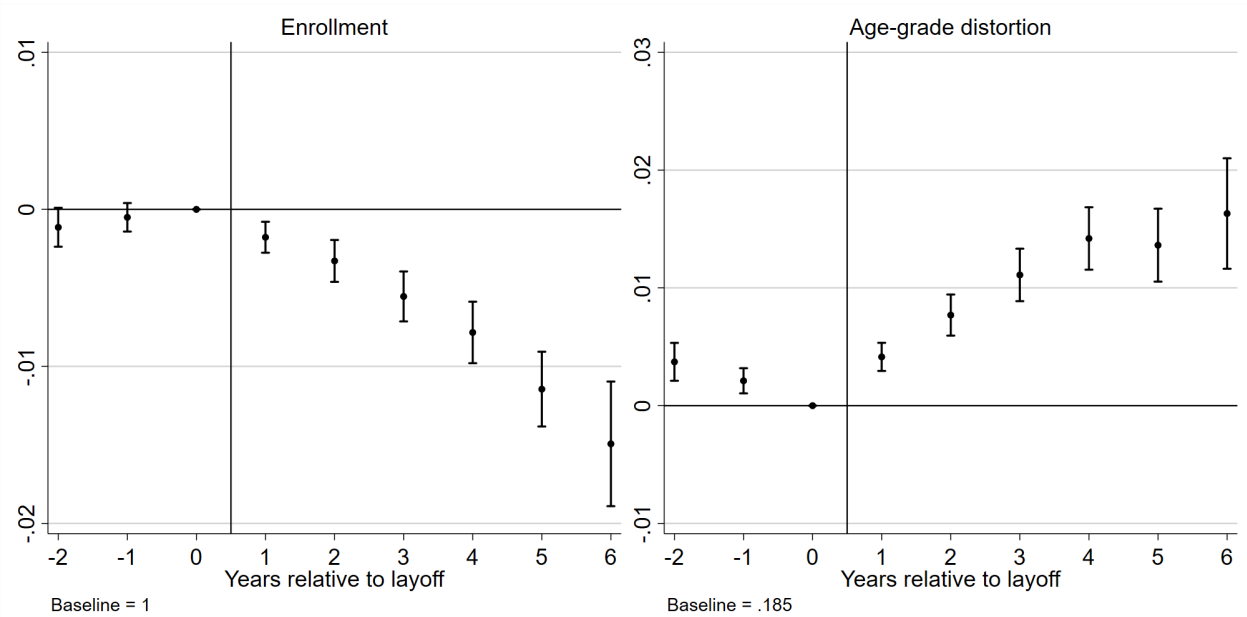
Figure B2: Effect of parental job loss on formal and informal labor market outcomes



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 2012-19 period. The treatment group is defined by workers who are formally employed in the first interview and out of employment in the second interview, while the control group comprises workers who are formally employed in the first and second interviews. Earnings are measured in Brazilian Reais. Baseline average values for the treated group at $t = 0$ are also reported. Standard errors are clustered at the individual level.

B.2 Effects on enrollment and age-grade distortion, medium-term effects

Figure B.3: 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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

B.3 Effects of parental job loss on parental income and children's educational outcomes

Table B1: 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	-7360.2***	-185.2***	-0.0038***	0.0050***	0.0053***
	(64.7)	(13.6)	(0.0005)	(0.0006)	(0.0006)
Sample	Full	Full	Full	Full	Always enrolled
Relative Effect	-45%	-4%	0%	3%	3%
Baseline	16503.25	4750.52	0.94	0.18	0.17
Observations	5,468,070	4,334,780	5,468,070	5,272,604	4,758,720

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 at the 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 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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level and 95% confidence intervals are displayed in parentheses (*** p $\leq$ 0.01, ** p$\leq$0.05, * p$\leq$0.1).

B.4 Robustness

B.4.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, while also being 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, thus 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 firms completely closing (columns 1-3). The latter essentially eliminates the scope for selection.

Table B2: 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	-0.0038***	-0.0029***	-0.0047***	-0.0028***	-0.0022**
	(0.0005)	(0.0007)	(0.0008)	(0.0007)	(0.001)
Observations	5,468,070	2,681,700	1,440,260	2,807,050	1,886,190
PANEL B: DEP. VAR. AGE-GRADE DISTORTION					
Parental job loss effect	0.0050***	0.0038***	0.0040***	0.0046***	0.0045***
	(0.0006)	(0.0009)	(0.001)	(0.0009)	(0.001)
Observations	5,272,604	2,581,396	1,390,812	2,701,283	1,813,293
PANEL C: DEP. VAR. INSE INDEX					
Parental job loss effect	-0.036***	-0.038***	-0.040***	-0.051***	-0.055***
	(0.004)	(0.006)	(0.007)	(0.006)	(0.007)
Observations	350,947	164,233	101,201	176,970	120,533
PANEL D: DEP. VAR. ENROLLED IN PUBLIC SCHOOL					
Parental job loss effect	0.032***	0.035***	0.032***	0.043***	0.046***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Observations	614,980	296,115	174,435	320,775	222,535
PANEL E: DEP. VAR. ENROLLMENT IN JUVENILE CORRECTIONAL FACILITY - BOYS					
Parental job loss effect	0.00022*	0.00021	0.00044*	0.00032**	0.00022
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0002)
Observations	637,719	314,217	169,060	326,469	218,834
PANEL F: DEP. VAR. TEENAGE PREGNANCY - GIRLS					
Parental job loss effect	0.0010***	0.0011***	0.0013***	0.00089***	0.00089***
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Observations	2,108,580	1,038,840	554,380	1,083,470	728,200
Mass layoff sample	$\geq$ 33%	$\geq$ 50%	closure	$\geq$ 100 workers	$\geq$ 250 workers

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 at the top of each panel. The sample is restricted to (1) mass layoffs of at least 33% of the workforce, (2) $\geq$ 50%, (3) plant closures, (4) at least 100 displaced workers, and (5) at least 250 displaced workers. Panels A, B, E, and F are based on children enrolled in any school before job loss, whereas Panels C-D is based on children enrolled in private schools before 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 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 year. Standard errors are clustered at the firm level and displayed in parentheses (*** p $\leq$ 0.01, ** p$\leq$0.05, * p$\leq$0.1).

We provide yet another test addressing selection issues following an intention-to-treat (ITT) 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 mass layoff 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. These estimates are consistent with the main analysis, supporting the robustness of our main findings. The coefficients are somewhat smaller relative to our baseline estimates in Table B2, column 1, which is consistent with the fact that average income losses are smaller in this ITT approach as it considers both displaced and non-displaced workers as treated.

Tables B2 and B3 also show the robustness of our findings on additional outcomes. In line with the main analysis on these outcomes in Section 4.6, regressions on school quality indicators focus on children previously enrolled in private school, while crime outcomes and teenage fertility are based on teenage boys and girls, respectively.

Table B3: 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	INSE index	Enrollment Public school	Juvenile Correctional Fac.	Teenage Pregnancy
PANEL A: ALL WORKERS IN MASS LAYOFF FIRMS							
Parental job loss effect	-5502.9***	-0.0030***	0.0032***	-0.022***	0.019***	0.00020**	0.00029***
	(61.3)	(0.0005)	(0.0006)	(0.003)	(0.002)	(0.0001)	(0.00008)
Baseline Mean	19287	0.95	0.15	8.68	0.15	0.00048	0.00
Observations	7,269,910	7,269,910	7,033,328	603,284	1,005,265	861,800	5,691,040

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 at the top of each column. Columns 1-3 and 6-7 are based on children enrolled in any school before job loss, whereas columns 4-5 are based on children enrolled in private schools before job loss. The main explanatory variable of 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 are clustered at the firm level and displayed in parentheses (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

B.4.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, while spillovers across displaced co-workers could also 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 (columns 1-3). 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 (columns 4-5). In addition, spillovers should be stronger when mass layoffs take place in smaller municipalities, as each firm represents a larger share of the workforce. To the contrary, the fact that our estimates are somewhat smaller in smaller municipalities (Figures B5 and B7) suggests again that our main findings are not driven by mass layoffs spillovers in the local area.

B.4.C Area-level fixed effects, alternative control group and reweighting

We show that our main estimates are robust to the addition of fine municipality (5,570) X time fixed effects, as shown in Table B4, Panel A. 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 area-level conditions over time, such as labor market and school environment conditions.

We also 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 year (Britto et al., 2022; 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 a choice has no impact on our estimates (see Table B4, Panel B).

Finally, Panel C in Table B4 shows that our results are robust to reweighting the sample to match the characteristics of the population of students' in the 2014 school census. This supports the idea that the subsample of students who can be linked to parents used in our main analysis is fairly representative of the student population.

Table B4 also shows the robustness of our findings for additional outcomes. In line with the main analysis on these outcomes in Section 4.6, regressions on school quality indicators focus on children previously enrolled in private school, while crime outcomes and teenage fertility are based on teenage boys and girls, respectively.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var.:	Enrollment	Age-grade distortion	INSE index	Enrollment Public school	Juvenile Correctional Fac.	Teenage Pregnancy
PANEL A: ADDING MUNICIPALITY X YEAR FIXED EFFECTS						
Parental job loss effect	-0.0038***	0.0045***	-0.034***	0.032***	0.00023*	0.0011***
	(0.0004)	(0.0005)	(0.004)	(0.002)	(0.0001)	(0.0002)
Baseline Mean	0.94	0.18	2.10	8.39	9.68	0.18
Observations	5,466,270	5,270,770	347,586	611,755	634,838	2,105,935
PANEL B: ALTERNATIVE CONTROL GROUP, CONTINUOSLY EMPLOYED WORKERS						
Parental job loss effect	-0.0041***	0.0058***	-0.034***	0.029***	0.00021	0.00093***
	(0.0005)	(0.0007)	(0.004)	(0.003)	(0.0001)	(0.0002)
Baseline Mean	0.94	0.18	8.48	0.17	0.00042	0.0054
Observations	3,969,360	3,832,787	275,530	477,815	464,067	1,528,080
PANEL C: REWEIGHTING TO MATCH ATTRIBUTES OF ALL STUDENTS IN SCHOOL CENSUS 2014						
Parental job loss effect	-0.0047***	0.0046***	-0.034***	0.027***	0.00020*	0.0013***
	(0.0008)	(0.001)	(0.006)	(0.004)	(0.0001)	(0.0003)
Baseline Mean	0.94	0.22	8.27	0.20	0.00032	0.0059
Observations	5,459,655	5,264,458	350,769	614,680	636,793	2,105,180

Notes: This table shows the average treatment effect of job loss on school outcomes – as estimated from the difference-in-differences equation (2) – when including additional fixed effects (Panel A), restricting the control group for continuously employed workers (Panel B), and reweighting the sample to match the characteristics of the population of students in the 2014 school census (Panel C). The dependent variable is indicated at the top of each column. Columns 1-2 and 5-6 are based on children enrolled in any school before job loss, whereas columns 3-4 are based on children enrolled in private schools before 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 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 year. Standard errors are clustered at the firm level and displayed in parentheses (*** p $\leq$ 0.01, ** p$\leq$0.05, * p$\leq$0.1).

B.4.D Staggered diff-in-diff

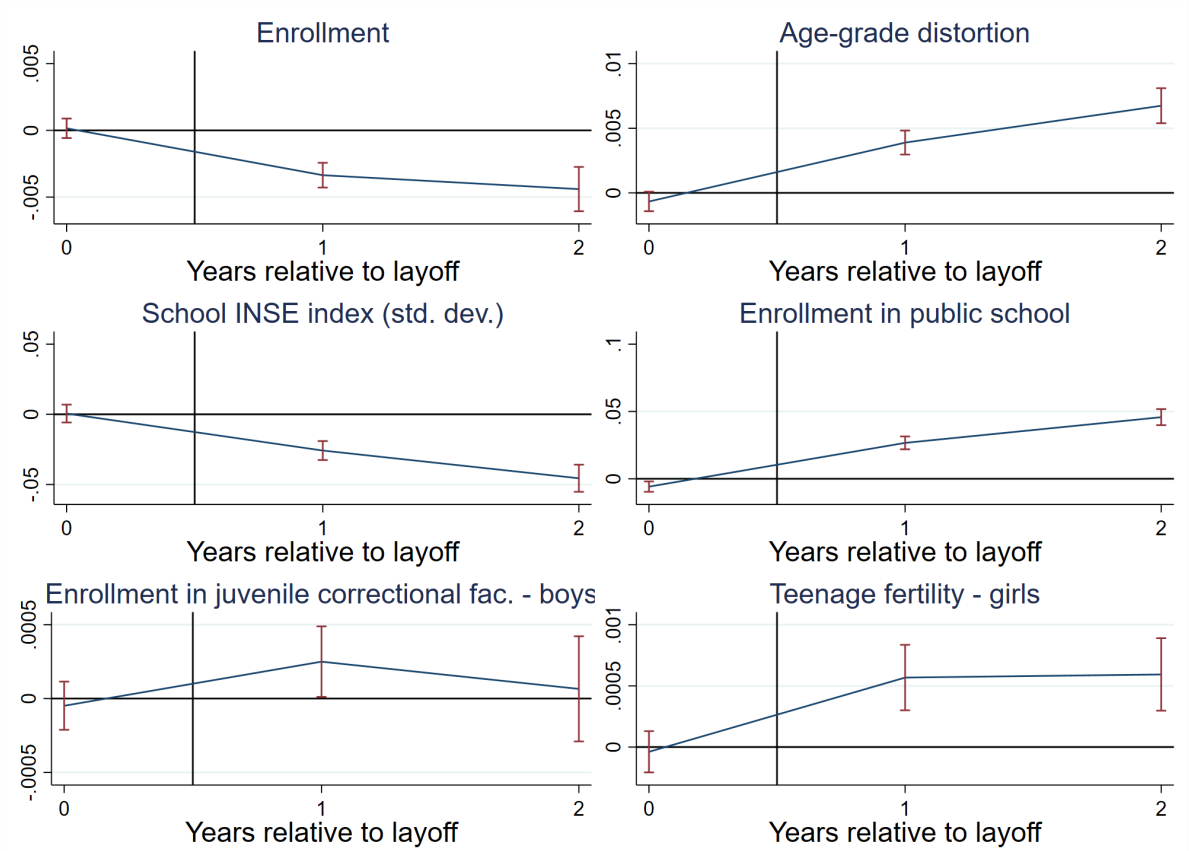
We now address concerns related to the recent literature on difference-in-differences designs with staggered treatment (see Sun and Abraham (2021); Athey and Imbens (2018); De Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Callaway and Sant'Anna (2021); Imai and Kim (2019)). These papers 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 effects, as the control group may be composed

of units that 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, namely 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 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 an 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 also shows the robustness of our findings to additional outcomes. In line with the main analysis on these outcomes in Section 4.6, regressions on school quality indicators focus on children previously enrolled in private school, while crime outcomes and teenage fertility are based on teenage boys and girls, respectively.

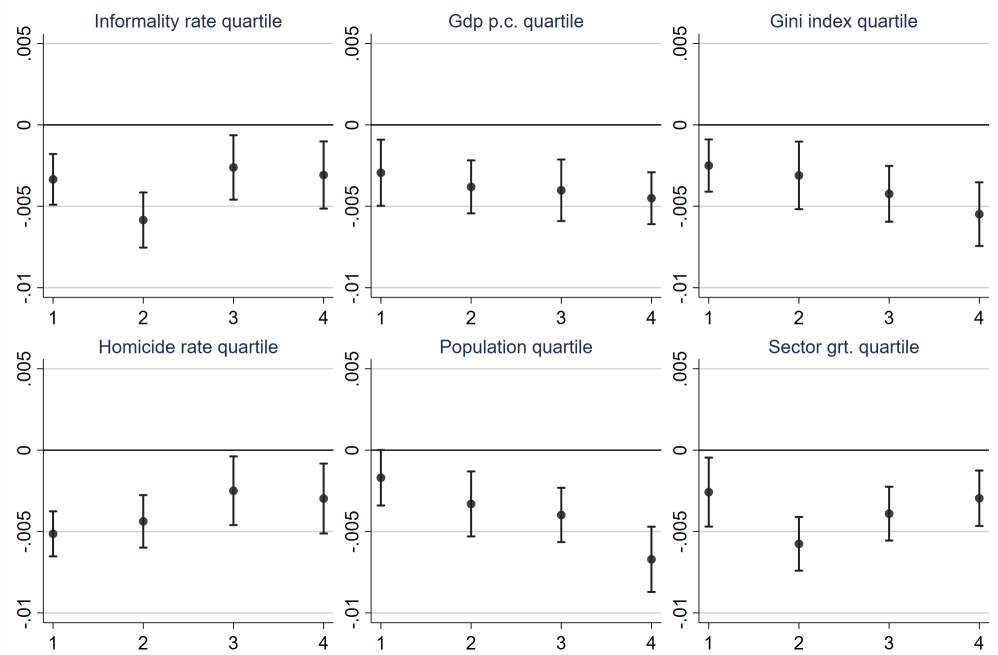
Figure B4: Effect of parental job loss on school outcomes, alternative estimators



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. The dependent variable is indicated at the top of each column. Estimates for enrollment, age-grade distortion, enrollment in juvenile correctional facilities and teenage fertility are based on children enrolled in any school before job loss, whereas the remaining ones are based on children enrolled in private schools before job loss. 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 year. Standard errors are clustered at the firm level.

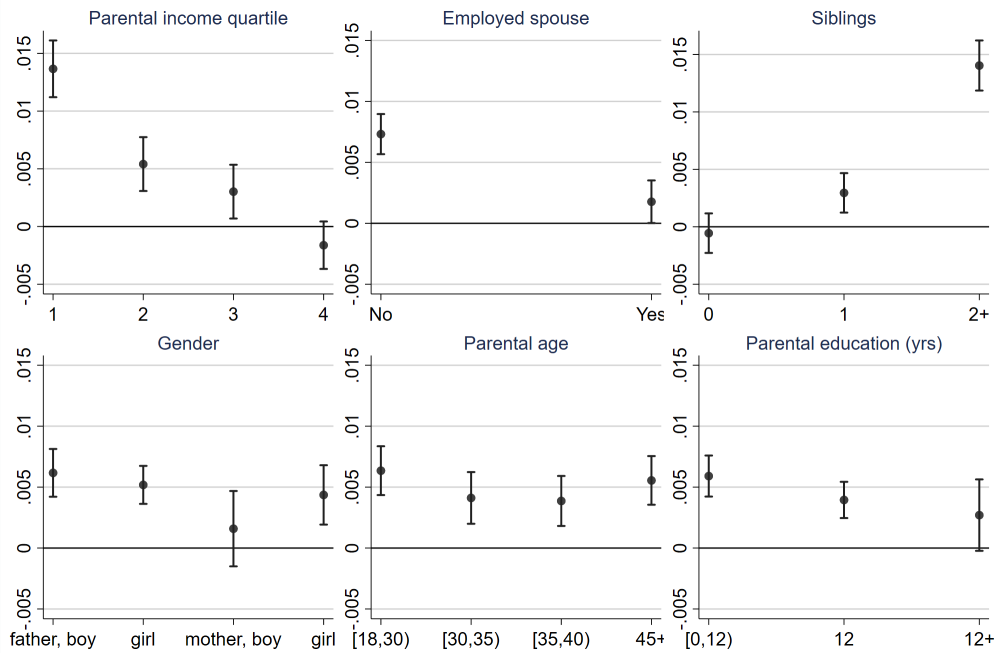
B.5 Effects of parental job loss, heterogeneity analysis

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



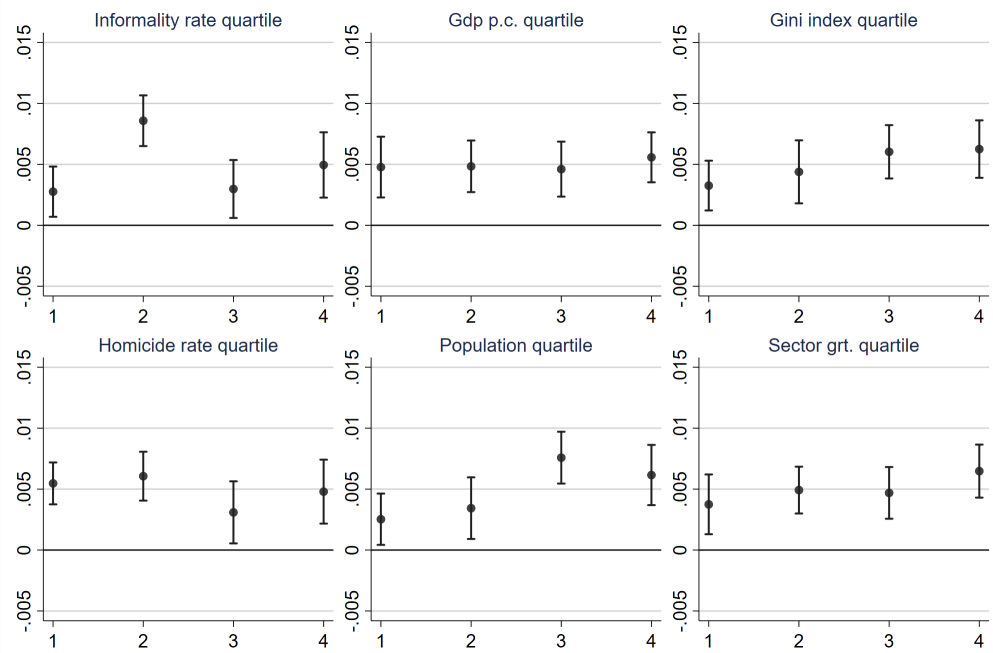
Notes: The figure shows the effect of parental job loss on children's school enrollment, after splitting the sample by several area-level characteristics, as estimated from equation (2), along with 95% confidence intervals. Informality rate, GDP per capita, Gini index, and population are based on the 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 two-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 year. Standard errors are clustered at the firm level.

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 year. Standard errors are clustered at the firm level.

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



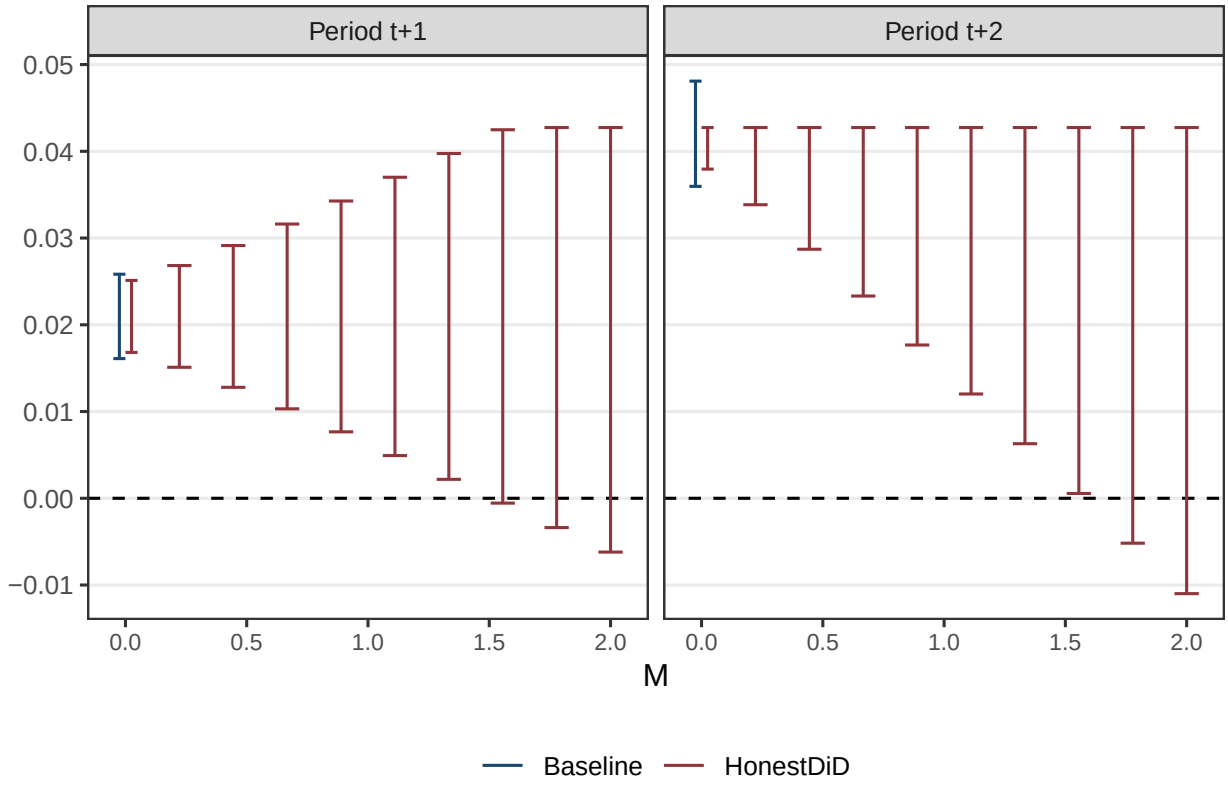
Notes: The figure shows the effect of parental job loss on children's age-grade distortion after splitting the sample by several area-level characteristics, as estimated from equation (2), along with 95% confidence intervals. Informality rate, GDP per capita, Gini index, and population are based on the 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 two-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 year. Standard errors are clustered at the firm level.

B.6 Effects of parental job loss on public school enrollment, robustness to pre-trends deviation

In Figure 9, we show that children in private school before layoff are more likely to enroll in public schools following parental job loss. However, there is a small pre-trend deviation deviation for this outcome. Even though the deviation goes on the opposite direction of our effect so that a linear extrapolation would imply that the estimated effect is attenuated, we implement the methods proposed by Rambachan and Roth (2022) to address this potential issue. These methods of partial identification provide bounds for the causal parameter in DID settings while allowing for some level of violation in the common-trend assumption. We focus on the RM method which allows for some degree of deviation in the linear trend. Hence, we retain the assumption of linear trends, which is in line with the fact that pre-trends in our setting follow linear patterns. As suggested by the authors, we allow for varying degrees of such deviation based on the maximum deviation observed in the pre-treatment period (M). Figure B.8 shows that our effects one and two years after the shock (left and

right panels, respectively) remain statistically significant even when allowing for deviations which are about 1.5 larger than the one observed in the pre-treatment period, supporting our finding that parental job loss leads to higher public school enrollment.

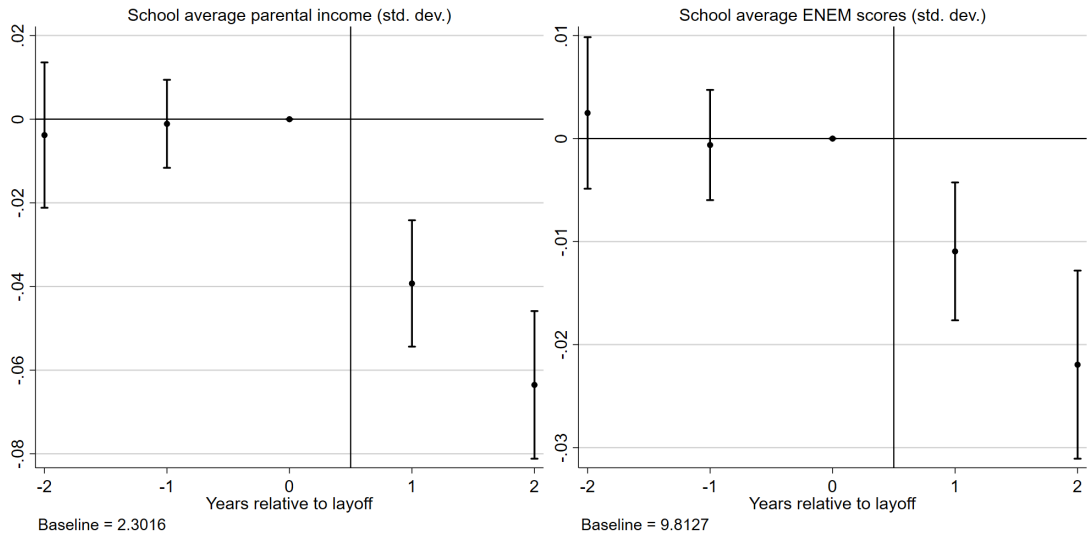
Figure B.8: Effect of parental job loss on public school enrollment, children enrolled in private school before job loss, robustness to pre-trends deviation



Notes: The figure shows 95% confidence intervals (CI) for the effects of parental job loss on children's school enrollment one (left) and two (right) years after the shock. The blue lines report our baseline CIs, as estimated from the difference-in-differences equation (1) – for $M = 0$ –, while the red lines report the CIs estimated as in Rambachan and Roth (2022), using the Delta RM method allowing for linear trend deviations – for $M > 0$. The size of the linear trend deviation (M) in the x-axis is expressed in multiples of the maximum deviation observed in the pre-treatment period.

B.7 Effects of parental job loss on school quality, additional outcomes

Figure B.9: 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 (left) and average ENEM scores (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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level.

B.8 Parental separation, migration and neighborhood quality

Although we do not have data on separations for the entire sample, we study job loss effects on this outcome for poorer families present in the *CadÚnico* welfare registry where we observe such information. Specifically, we directly study the probability that both parents live with the child as an outcome. Although the interpretation of these results requires some caution to the extent that they are conditional on children being observed over time in *CadÚnico* – which could be endogenous to the job loss – they may offer some insights into the role of parental separation. The results in Table B.5, column 1 indicate that such a probability is not affected by parental job loss. Although the layoff could lead to emotional distress in the household and favor separations, financial constraints may induce couples to remain together as an insurance mechanism to absorb the income losses (e.g., avoiding paying a second rent), especially in low-income families.

We next study the role of migration as a potential mediating factor. First, we study that the probability that children move to a different neighborhood within the *CadÚnico* sample for which such information is available. In addition, we track the probability that

parents take jobs in a different Brazilian state as a proxy for migration. Similar to the analysis on parental separation, the interpretation of these results requires some caution given that these two outcomes are conditional on children being in *CadÚnico* and parental re-employment after displacement, respectively. The results in Table B.5, columns 2-3 indicate relatively small but statistically significant impacts on these probabilities. Second, we study the characteristics of the areas where children are moving, namely the average income in the neighborhood observed in *CadÚnico*, and the GDP per capita, income inequality and population size of the municipality where parents find a new job. Table B.5, columns 4-7 shows that children tend to move to lower-quality areas, namely places with lower per capita income and higher inequality. Although the magnitudes of these effects do not seem particularly sizable, they are consistent with the idea that families move to worse areas to accommodate the income losses brought by job loss.

Table B.5: Effect of parental job loss on location characteristics and parental separation, children in *CadÚnico*

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var.:	Child live with both parents	Neighborhood Migration	Parent work in different state	Neighborhood Income (std. dev.)	Parental municipality of work GDP pc.	Gini index
Parental job loss effect	0.0006	0.0085***	0.015***	-0.0012	-518.1***	0.00069***
	(0.001)	(0.001)	(0.002)	(0.001)	(93)	(0.0002)
Sample	*CadÚnico*	*CadÚnico*	Full	*CadÚnico*	Full	Full
Relative Effect	0.1%	8.0%	33.3%	0.0%	-1.9%	0.1%
Baseline Mean	0.63	0.11	0.05	9.47	26601.21	0.64
	2,102,280	2,717,873	4,829,549	2,144,018	4,829,452	4,829,452

Notes: The table shows the effect of parental job loss on the probability that the children live with both parents, migrate to a different postal code, and that the displaced parental live in a different state (1-3), the average formal labor income in the residential postal code (4), the GDP per capita and Gini index in the municipality where the displaced parents work (5-6), as estimated from the difference-in-differences equation (2). Outcomes in columns 3-6 are based on formal employment data, whereas the outcomes in columns 1-2 are based on *CadÚnico*. 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 year. The baseline indicates the counterfactual mean outcome in the treatment group in the post-treatment period ($E[Y_i^0|Post = 1, Treat = 1]$). Standard errors are clustered at the firm level and 95% confidence intervals are displayed in parentheses (*** p $\leq$ 0.01, ** p$\leq$0.05, * p$\leq$0.1).

C Appendix to Section 5

C.1 Effects of parental job loss on high-school completion rates, robustness

As a robustness check, we show 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. We add these controls to the following equation to summarize the average treatment effects:

$$Y_{it} = \beta Post_t \times Treat_i + Treat_i + \lambda_t + \epsilon_{it}, \tag{C.5}$$

where $Post_t$ identifies the treatment period – i.e. layoffs taking place before the expected high-school graduation – and β is the coefficient of interest identifying the average impact of parental job loss. Table presents the results, showing that our main estimates barely change as we progressively add controls on parental characteristics and finer fixed effects.

Table C1: Long-term effect of parental job loss on high-school completion

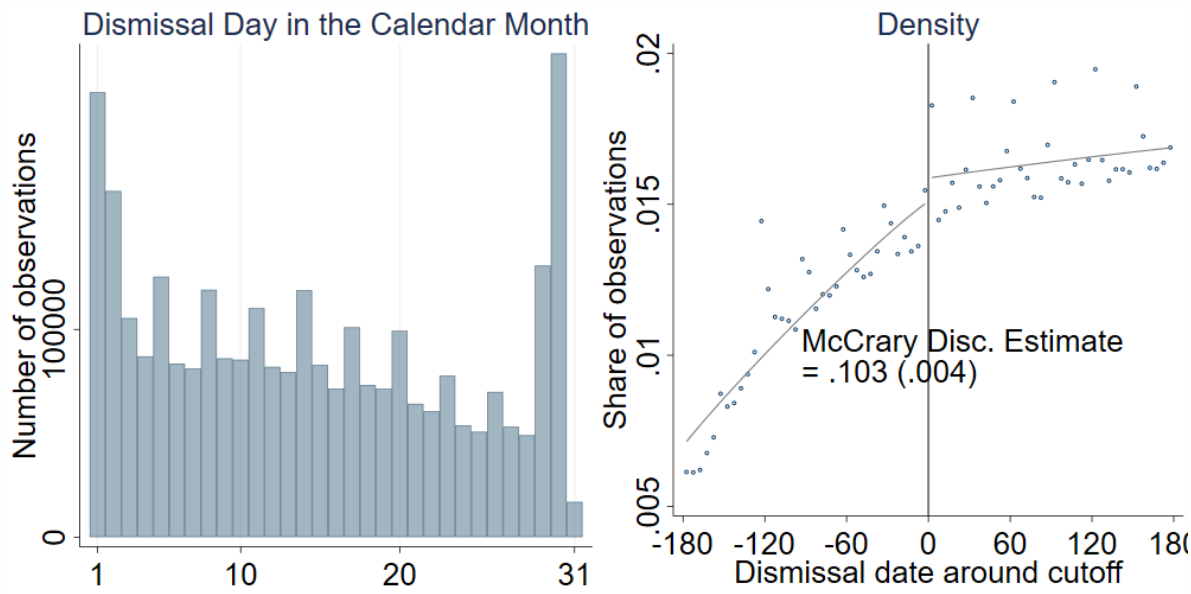
	(1)	(2)	(3)	(4)	(5)
PANEL A. Dependent var.: High-School Completion					
Parental job loss effect	-0.013***	-0.012***	-0.012***	-0.013***	-0.015***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Layoff year FE		Y			
Layoff year X Municipality FE			Y		
Layoff year X Treat X Mun. FE				Y	
Layoff year X Treat X Mun. X Industry FE					Y
Relative Effect	-2.1%	-1.9%	-1.9%	-2.1%	-2.4%
Baseline Mean	0.62	0.62	0.62	0.62	0.62
Observations	771,337	771,337	771,337	768,625	738,249

Notes: The table shows the effect 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 (C.5). The baseline indicates the average outcome for $t = 1$ in the treatment group. 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 year. Standard errors are clustered at the firm level and 95% confidence intervals are displayed in parentheses (*** p $\leq$ 0.01, ** p$\leq$0.05, * p$\leq$0.1).

D Appendix to Section 6

D.1 Monthly cycles in dismissal dates

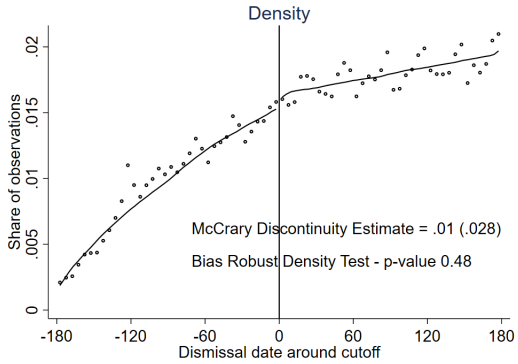
Figure C1: Dismissal dates monthly cycles



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.

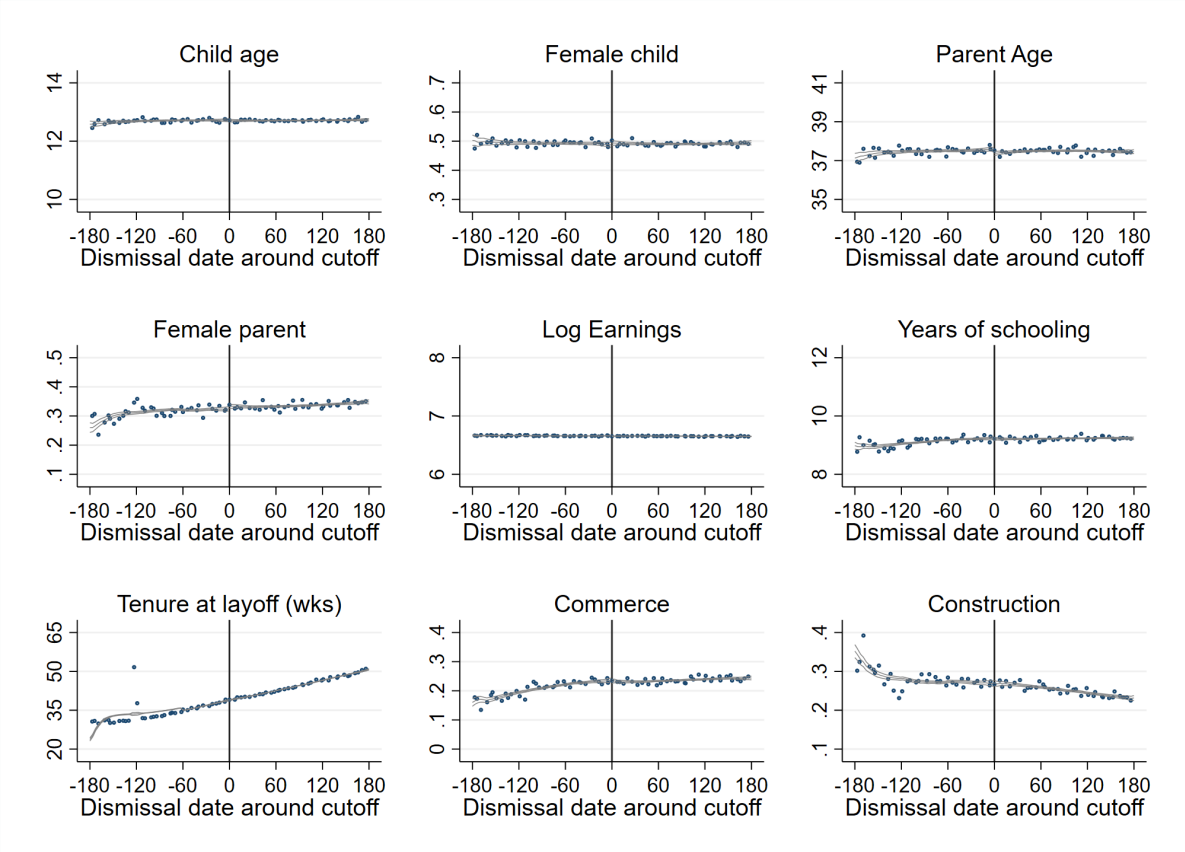
D.2 Effect of UI eligibility, evidence on the validity of the RD design

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., sixteen months since the previous layoff date in the past) in our main working sample. The sample includes displaced parents with at least six months of continuous employment prior to layoff. The results of the 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 six months of continuous employment prior to layoff. Dots represent averages based on five-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

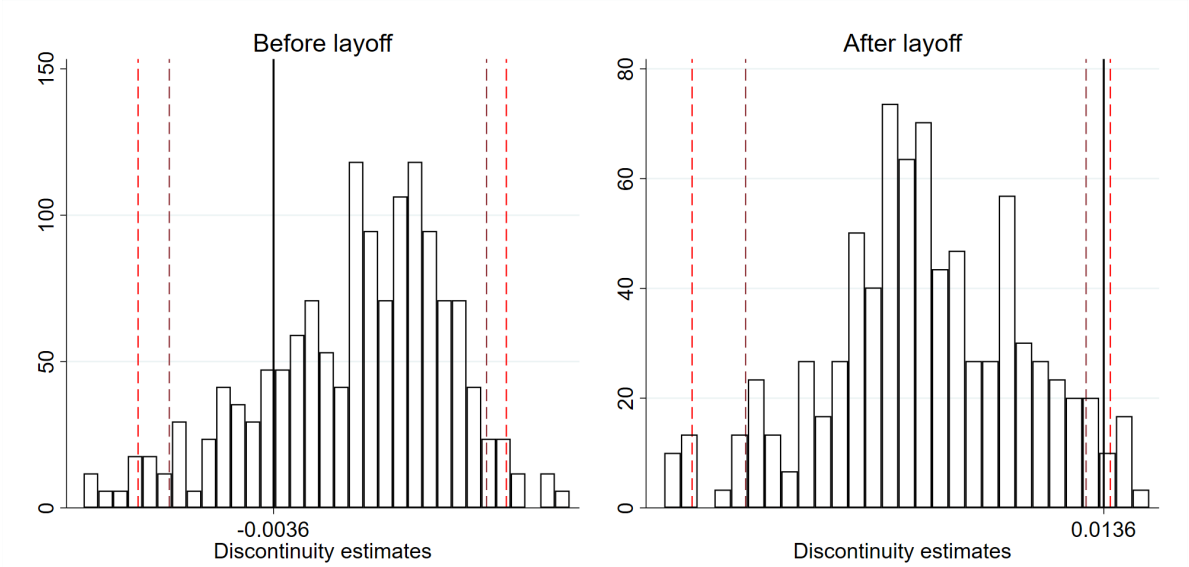
D.3 Effect of UI eligibility on enrollment, robustness

Table D1: Effect of UI eligibility on school enrollment, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.: School Enrollment, 3 years after layoff								
Panel A. FULL SAMPLE								
Eligibility for UI benefits	0.0117**	0.0164***	0.0136**	0.0152***	0.0179***	0.0165**	0.0172***	0.0139*
	(0.005)	(0.0064)	(0.0055)	(0.005)	(0.0068)	(0.0068)	(0.0059)	(0.0083)
Observations	13,205	32,483	43,014	53,131	28,364	63,161	82,638	40,995
Bandwidth (days)	CCT	45	60	75	CCT	90	120	CCT
Polynomial Order	0	1	1	1	1	2	2	2

Notes: This table replicates the regression discontinuity analysis in Table 2 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). Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

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. (4).

Table D2: Effect of UI eligibility on school enrollment after layoff, manipulation robust inference

	(1)	(2)
Dep. Var:		
School Enrollment		
	Estimate	C.I.
PANEL A. MAIN ESTIMATES		
Share always assigned	0.073	
ITT: Ignoring manipulation	0.01363	[0.00328,0.02398]
ITT: Bounds inference	[0.00336,0.07433]	[-0.00651,0.09542]
PANEL B. HYPOTHETICAL SHARE OF MANIPULATION		
Share always assigned		
0.025	[0.01031,0.03595]	[0.00151,0.04475]
0.05	[0.00681,0.05944]	[-0.00209,0.06803]
0.1	[-0.00076,0.09155]	[-0.00989,0.09958]
0.15	[-0.00923,0.11317]	[-0.01861,0.12064]
0.2	[-0.01876,0.13213]	[-0.02844,0.13971]

Notes: This table shows discontinuity estimates in school enrollment rates after layoff, while allowing for manipulation in treatment assignment around the sixteen-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.

Table D3: Effect of UI eligibility on school enrollment after layoff, dropping observations near the cutoff

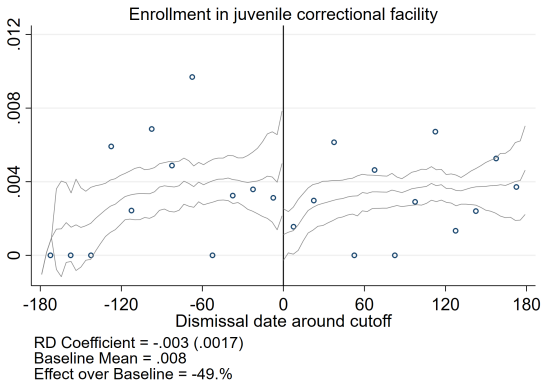
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.: School Enrollment, 3 years after layoff										
Panel A. FULL SAMPLE, LOCAL LINEAR POLYNOMIAL, BANDWIDTH 60 DAYS										
Eligibility for UI benefits	0.0136**	0.0133**	0.0143**	0.0131**	0.0137**	0.0141**	0.0170**	0.0132*	0.0165**	0.0137*
	(0.0055)	(0.0056)	(0.0058)	(0.006)	(0.0062)	(0.0065)	(0.0067)	(0.007)	(0.0072)	(0.0075)
Observations	43,014	42,697	42,037	41,381	40,526	39,728	38,998	38,260	37,537	36,877
Panel B. FULL SAMPLE, LOCAL QUADRATIC POLYNOMIAL, BANDWIDTH 120 DAYS										
Eligibility for UI benefits	0.0172***	0.0169***	0.0182***	0.0171***	0.0182***	0.0190***	0.0226***	0.0187**	0.0228***	0.0201**
	(0.0059)	(0.006)	(0.0062)	(0.0064)	(0.0067)	(0.007)	(0.0073)	(0.0076)	(0.0079)	(0.0082)
Observations	82,638	82,321	81,661	81,005	80,150	79,352	78,622	77,884	77,161	76,501
Drop obs within X days distance to the cutoff										
	0	1	2	3	4	5	6	7	8	9

Notes: This table shows discontinuity estimates in school enrollment rates after layoff, as estimated from equation (4), after dropping observations near the sixteen-month cutoff for UI eligibility. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

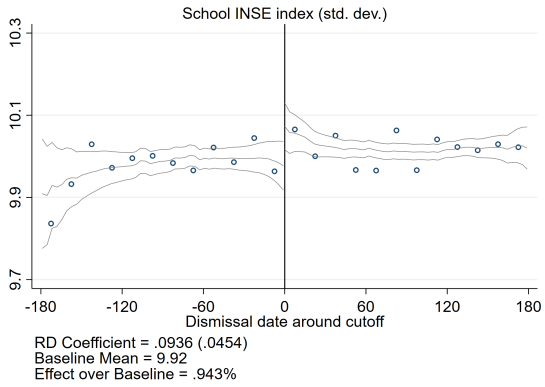
D.4 Effect of UI eligibility on additional outcomes

Figure D.5: Effects of UI eligibility on additional outcomes

(a) Crime



(b) School quality



Notes: The graphs plot the probability that children enroll in juvenile correctional facilities and the average school quality – as measure by the INSE index – in the three-year period after layoff, around the cutoff date for parental eligibility for unemployment benefits. Panel (a) is based on children in *CadÚnico* enrolled in any school before job loss, whereas Panel (2) is based on children enrolled in private schools before job loss. The sample includes displaced workers with at least six months of continuous employment prior to layoff. Dots represent averages based on fifteen-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

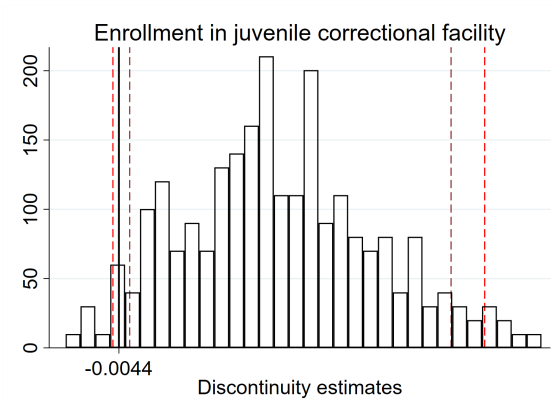
Table D4: Effects of UI eligibility on additional outcomes

	(1)	(2)	(3)	(4)
Dependent var.:	Enrollment Correctional Facility Boys	Teenage Fertility Girls	School INSE Index	Enrollment Private School
UI eligibility effect	-0.0040**	0.0021	0.094**	0.013
	(0.0018)	(0.0029)	(0.045)	(0.014)
Baseline Mean	0.008	0.018	9.92	0.64
Observations	15,247	39,844	11,212	15,195

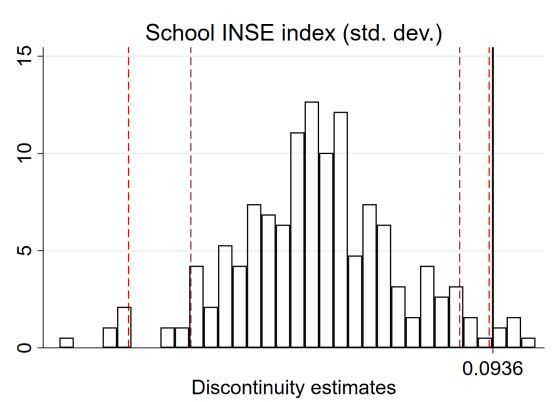
Notes: This table shows the effect of eligibility for UI benefits – as estimated from equation (4) – on several outcomes indicated on top of each column. Columns 1-2 are based on children in *CadÚnico* enrolled in any school before job loss, whereas columns 3-4 are based on children enrolled in private schools before job loss. The sample includes displaced parents with at least six months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility for unemployment benefits, namely sixteen 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), the 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).

Figure D.6: Effects of UI eligibility on additional outcomes after the layoff, permutation tests

(a) Crime



(b) School quality



Notes: The graphs compare discontinuity estimates of the effect of UI eligibility on crime and school quality 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. (4).

Table D5: Effect of UI eligibility on additional outcomes, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: DEP VAR ENROLLMENT IN JUVENILE CORRECTIONAL FACILITY								
Eligibility for UI benefits	-0.0032**	-0.0039**	-0.0040**	-0.0031*	-0.0043**	-0.0051**	-0.0022	-0.0023
	(0.0015)	(0.0019)	(0.0018)	(0.0016)	(0.002)	(0.0022)	(0.0019)	(0.0027)
Observations	4,954	11,536	15,247	18,901	9,900	22,557	29,348	12,773
PANEL B: DEP VAR SCHOOL INSE INDEX								
Eligibility for UI benefits	0.0308	0.0631	0.0936**	0.0734*	0.1221**	0.1213**	0.0806*	0.2118***
	(0.0373)	(0.0523)	(0.0455)	(0.0408)	(0.0554)	(0.0561)	(0.0487)	(0.0704)
Observations	4,283	8,543	11,212	13,841	8,384	16,532	21,642	12,270
Bandwidth (days)	CCT	45	60	75	CCT	90	120	CCT
Polynomial Order	0	1	1	1	1	2	2	2

Notes: This table replicates the regression discontinuity analysis in Table D4, 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).

D.5 Implication for welfare analysis

Table D6: Estimated reduction in UI efficiency costs due to children's additional education

		(1)
a.	Estimated yearly increased in enrollment rate (3-year period)	0.014
b.	Additional years of schooling	0.042
c.	Returns to schooling	12%
d.	Baseline yearly income (R$)	9447
e.	Working years	35
f.	Tax rate	32.5%
g.	Yearly interest rate	5.0%
h.	Net present value of additional government revenues (R$)	219
i.	Average UI transfer at the cutoff (R$)	1832
j.	Share accrued by mechanical beneficiaries (Gerard and Gonzaga, 2021)	85%
k.	Amount reaching mechanical beneficiaries (R$)	1,564
m.	Additional government revenues per R$ reaching mechanical beneficiaries	0.140
n.	Efficiency cost due to longer unemployment duration per R$ reaching mechanical beneficiaries (Gerard and Gonzaga, 2021)	0.217

Notes: This table provides a back-of-the-envelope calculations for the additional government revenue generated by the positive impacts of eligibility for unemployment benefits on children's school enrollment. It computes the net present value of the additional revenue (h) relative to the amount reaching mechanical beneficiaries of UI benefits (k). (h) is given by the product of (b)*(c)*(d)*(f) accumulated over (e) years and discounted with the yearly interest rate (g). It also consider that the additional revenue only starts flowing after three years. The share accrued by mechanical beneficiaries is based on Gerard and Gonzaga (2021), who study UI in the Brazilian context for a similar period.