Maternal Dismissals during Pregnancy and the Health of Infants*

Stefano Cellini† ⓡ Lívia Menezes‡ ⓡ Martin Foureaux Koppensteiner§

July 22, 2024

Abstract

In this paper, we study the impact of maternal job dismissals during pregnancy on newborn health, prenatal care utilization, and medium-term health outcomes of children using linked Brazilian administrative data and high-dimensional fixed effects. We find that dismissal of single mothers significantly reduces infants weight, increases the incidence of low birthweight, reduces prenatal visits and private health care utilization, and increases hospitalization risks in the year after birth. The estimates are robust to a battery of alternative specifications and definition of treatment. Using a complementary regression-discontinuity design, we show that unemployment insurance mitigates these adverse effects, particularly for lower-earning mothers.

JEL Codes: I12, I14, J63, J65

Keywords: Maternal dismissals, infant health, healthcare choices, unemployment insurance

*We would like to thank Thomas Crossley, Eric French, Sandra McNally, Matthias Parey, Nirman Saha, Miriam Wüst and seminar participants at Birkbeck, Nottingham, Reading, Texas A&M, York, the Essen Health Conference 2022, ESPE 2022, EEA 2022, EALE 2022, AIEL 2022 and the Royal Economic Society annual conference 2023 for their comments. We are grateful to the Labor Secretariat of the Brazilian Ministry of the Economy and the Health Ministry for help with accessing the data. This research used the Eureka High Performance Computing facilities at the University of Surrey. The usual disclaimer applies.

†University of Surrey, School of Economics, Guildford, Surrey, GU2 7XH, UK. Email: s.cellini@surrey.ac.uk.

‡University of Birmingham, Department of Economics, Edgbaston, Birmingham, B15 2TT, UK and IZA, Bonn, Germany. Email: l.menezes@bham.ac.uk.

§Corresponding author, University of Surrey, School of Economics, Guildford, Surrey, GU2 7XH, UK and IZA, Bonn, Germany. Email: m.koppensteiner@surrey.ac.uk.

1 Introduction

Losing one's job is one of the most significant economic shocks individuals might be exposed to over their lifetimes. The health-related consequences of job loss on the individual have been widely documented in the literature, ranging from engagement in risk behaviors and unhealthy lifestyle, to worsened mental and physical well-being (Black et al., 2015; Schaller and Stevens, 2015; Michaud et al., 2016). These result in substantial mortality risks for workers (Sullivan and Von Wachter, 2009; Browning and Heinesen, 2012), and also entail negative spillovers onto cohabiting household members, such as partners and children, who are affected by the financial and mental distress of the event (Marcus, 2013; Bubonya et al., 2017). Despite mounting evidence on the negative effect of parental involuntary job loss during childhood on human capital formation (Oreopoulos et al., 2008; Carneiro et al., 2023), there is only limited and ambiguous evidence regarding the effects of direct exposure to parental job loss during prenatal development, a period sensitive to a variety of shocks to the maternal environment (Almond and Currie, 2011; Aizer and Currie, 2014).

To estimate the effect of maternal dismissals during pregnancy, we leverage population-level administrative data linking individual formal employment spells with birth records, children's hospitalization and mortality registries. This comprehensive dataset presents, for the universe of workers employed in the formal sector, detailed information on all their employment spells and earnings, as well as information on birthweight, gestational length, the prenatal care utilization and delivery choices, hospitalization and death records for their newborns. Primarily, we are interested in estimating the effect dismissals during pregnancy have on a number of measures of health at birth of the newborns, prenatal health care utilization of the expectant mother, and children's health outcomes within the subsequent year of life. We then investigate underlying mechanisms, by focusing on heterogeneity by the financial position of the mother. In particular, we are interested in investigating whether the layoff-induced financial instability may be mitigated by formal unemployment insurance.

We focus most of the analysis on the population of working *single* mothers in Brazil - thus isolating the effects of a potentially negative shock on fetal development and prenatal decisions in households where no partner can compensate the income loss from the maternal dismissal.[1] The social context of single mothers is also becoming more and more relevant with singles accounting

[1]Information on partner's income is also not readily available, with paternal identifiers frequently missing from the birth records. For completeness, despite some uncertainty around partners identity, we also provide estimates for dismissed mothers in a stable relationship to complement the estimates for single mothers.

for a rapidly increasing share of all births, with a share of more than 40% of all births in Brazil (24% of all mothers in Latin America, Crabtree and Kluch, 2020).

In our identification strategy, we compare birth and health outcomes between children from employed workers that remain formally employed throughout the pregnancy (control group) and children from workers that are dismissed *without just cause* while pregnant, but not later than the month of the first prenatal visit (treatment group). This treatment assignment rule identifies pregnant workers that may be too early in (or even unaware of) their pregnancy to be selected into dismissal because of adverse pregnancy status.

We find that children of dismissed single mothers weigh on average 39 grams (-1.2% off mean weight), and are 27% more likely to be classified as low birthweight ($\leq 2,500$ grams). The results are robust to specifications augmented with hospital, firm municipality and sector fixed effects. The mean effects from the unemployment shock conceal much more pronounced effects on birthweight and the incidence of low birthweight incidence for mothers with lower wages. In particular, the propensity of newborns' being classified as low birth weight, is several times larger for women in the bottom of the wage distribution.[2] We also document pronounced effects on prenatal and delivery-related healthcare utilization: dismissed mothers tend to have less prenatal visits, recur less to private clinics for delivery by 35%, and are less likely to deliver with a planned C-section by 21%. It is possible that the loss of access to private health insurance plans could motivate these divergences in healthcare services utilization.

However, the damage that in-utero exposure to a dismissal can do to newborns' health is not fully captured by measures of health at birth, as we find that a maternal dismissal during pregnancy increases the risk of hospitalization of children substantially: fetal exposure to maternal layoff leads to a 32% increase in probability of hospitalization over the first year of life. Hospitalization episodes increase by 29% for children to mothers laid-off during pregnancy. Using information on the individual costs of hospital treatment, we find that subsequent medical expenditures related to hospitalization are substantially higher for children of dismissed mothers. Additionally, infant mortality, a rare outcome, is also associated with exposure to dismissals. Among the most common causes of hospitalization and death, we find that dismissals are driving important increases in the incidence of conditions originating in the perinatal period.

In addition, we provide evidence on the short-term mitigating role of formal insurance in the

[2]We also show that the effects across the distribution of total Severance Pay package (instead of last-earned wage) are much more pronounced for the bottom quartile, with smaller and insignificant effects for higher quartiles.

form of unemployment benefits. We study a tenure-based unemployment insurance (UI) scheme providing income support for displaced workers in Brazil by exploiting the discontinuity in the eligibility rule of UI in a regression discontinuity setting. We find that UI increases BW by around 3%, counteracting the negative effect of dismissals for displaced single women. Incidence of low birthweight among newborns is also reduced for UI-entitled mothers.

Our findings add to a currently unresolved theme in the extensive literature on the consequences of unemployment to the health of the worker and their household members: the effects of in-utero exposure to job losses on newborns' health. On one hand, there is a large consensus on the existence of adverse and severe consequences of dismissals for the individual worker, and that they entail negative health spillovers for spouses and children, who face economic and psychological challenges. On the other hand, an extensive literature on the consequences of unemployment shocks on babies' health do not reach a uniform conclusion, mainly because of demanding data requirements and potentially competing mechanisms for the identification of such effects, especially when using aggregate shocks.

A part of the literature relies on variation over time and across geographical areas in unemployment rates or measures of the business cycle and provides an overall ambiguous picture of the effects on birth outcomes. Evidence for the US shows a counter-cyclical relationship with newborns' health (Dehejia and Lleras-Muney, 2004). In Argentina, in contrast, there is evidence of pro-cyclicality Bozzoli and Quintana-Domeque (2014), and mixed findings are reported across European countries - for example, van den Berg et al. (2020) document a counter-cyclical relationship for Sweden, whereas Olafsson (2016) demonstrates a pro-cyclical relationship for Iceland. However, selection into fertility during recessions and the unobservability of the actual drivers of these relationships represent a limitation when using variation in aggregate indicators.[3]

There is limited previous evidence using individual-level data and variation rather than aggregate shocks and/or aggregate-level outcomes. Focusing on maternal employment status during pregnancy, rather than dismissals, Wüst (2015) finds that Danish mothers are more likely to have preterm deliveries when they are not employed during pregnancy, relating the negative effects to stress from unemployment. Using US and UK survey data, Del Bono et al. (2012) find positive effects of maternal work interruptions on birthweight up to three months before birth, but job interruptions are very broadly defined, including periods of maternity leave, hence making it difficult

[3]Distressing news or disruptions to maternal services may explain in part the negative association in some contexts, as in Carlson (2015) and De Cao et al. (2022). Instead, increased eligibility to Medicaid for mothers in the US during downturns can mediate the counter-cyclical relationship, as suggested by Menclova (2013).

to learn about the effect of unemployment shocks. Closest to our interest in parental dismissals is the work by Lindo (2011), who studies the consequences of a paternal job loss on birthweight in the US. The author reports a reduction in birthweight (-142 g, around -4.5%) of babies born in the same year of a paternal layoff and find that these dismissals lead to a significant decrease in pre-birth food expenditures – suggesting a deterioration of prenatal nutrition as the main driver of the reduction of birthweight.

Thus, we make three major contributions to the literature on the topic. Firstly, we provide the first causal estimates of maternal dismissal on birth outcomes. We focus on the group of single mothers, for which the income shock from dismissal may not be compensated by a partner's labor supply response.

Secondly, we extend the analysis of parental dismissals on infant health by expanding on previously used outcomes in two dimensions: prenatal health care utilization choices, which are typically considered important inputs for infant health and, beyond measures of health at birth, we also have access to hospitalization and mortality records over the first year of all live-born children. This allows us to investigate the effect of dismissals on health margins not routinely available in previous papers.

Thirdly, our setting allows us to contribute to the small but growing evidence on the positive effects of cash transfers to pregnant women on children's birth outcomes (Amarante et al., 2016; Reader, 2023), as we observe improved health at birth for children of mothers eligible for unemployment insurance. We provide further evidence on the importance of the economic shock from dismissal as a major transmission channel, pointing to important policy implications in response to maternal dismissals.

The remainder of this paper is organized as follows. The following section provides the context on Brazilian labor market institutions and on the reproductive healthcare settings in Brazil. Section 3 describes the data sources and provides an outline of the construction of the dataset. Section 4 presents the empirical strategy, followed in Section 5 by the empirical results and a discussion of the results. In Section 6, we provide some concluding remarks.

2 Institutional context

In this section, we provide background information on features of the Brazilian labor market, in particular on the institutional settings of Brazilian employment regulation, as well as on reproductive

healthcare settings in Brazil.

2.1 Female employment and labor regulation

As of 2019, the female labor participation rate in Brazil was estimated to be 55%, accounting for almost 44% of the total labor force, and the female unemployment rate was around 14% (World Bank, 2021). The Brazilian labor market is characterized by a large informal sector, which accounts for approximately 30% of total labor market participation (Ulyssea, 2018). We observe only mothers in the formal sector, hence our estimates are representative only for those mothers in the formal sector. As workers in the informal sector are not protected from unfair dismissals and are ineligible for UI, the results presented in this paper may likely provide a lower bound for dismissals occurring in the informal sector.

The vast majority of formal labor contracts in Brazil are open-ended with 91% of contracts in our dataset being open-ended. Brazilian labor legislation is based on at-will employment, whereby firms are free to dismiss workers without a just cause, although they must pay dismissal indemnities. The most common form of separation for open-ended jobs is dismissals without a just cause (70% of all cases) and voluntary quits (29%).[4] Employers must inform workers about their dismissal abiding by a mandatory 30-day minimum advance notice period. That is, the dismissal must come into effect at the earliest 30 days after being informed about the dismissal decision by the employer.

To protect pregnant mothers from discrimination, by law, their dismissal without just cause is void if the employer is made aware of the pregnancy before dismissal, including during the notice period. This protection extends for up to 5 months after delivery. This means that layoffs of pregnant mothers are expected to be overwhelmingly happening in the first trimester, when expectant mothers may not be aware yet that they are expecting or have no confirmation of the pregnancy by a medical practitioner, a pattern confirmed in our data sample.[5]

Brazilian labor regulation provides unemployment insurance (UI) to financially assist dismissed workers. Unemployment benefits can be claimed only by employees dismissed without just cause and are available for three to five months, depending on the length of employment in the 36 months prior to dismissal. Dismissed workers are entitled to UI payments for three, four, or five months for a previous tenure of 6, 12, or 24 months, respectively.[6] The average wage in the three months

[4]These figures are based on 2011 statistics but are representative of the entire period of interest in this paper.

[5]See Appendix Figure A1 for the distribution of layoff announcements across months of pregnancy.

[6]Additionally, 16 months must have passed between a worker's job separation date and the layoff date of their last claim of the UI. These rules were in place up to the year 2015, before the end of our period of interest.

prior to layoff determines the replacement rate that the eligible workers will receive, starting from 100% of previous earnings for workers earning the minimum wage. We will use the discontinuity in eligibility for UI, based on the minimum period of continuous employment before dismissal to explore the role UI plays for dismissed pregnant workers later in the paper.

Employers are also mandated to provide a Severance Savings Account (Fundo de Garantia do Tempo de Serviço, FGTS) and Severance Pay to their employees. The FGTS is an account at the federal bank, Caixa, where employers must deposit 8% of their workers' monthly wage each month in an account under each worker's name. The account pays a low-interest rate, which is aimed at protecting the real value of the deposits. Workers can only withdraw the money from the account once they are involuntarily laid off (other, rarer, conditions grant access to the account) and incur hassle costs if they delay the withdrawal (for further explanations see Gerard and Naritomi, 2020). The Severance Pay is composed of two elements, paid by the employers: (1) a monthly wage as 'advance notice' of layoff, and (2) 40% of the amount deposited in the workers' FGTS account over the employment spell. In the remainder of the paper, we jointly refer to FGTS and the Severance Pay simply as 'Severance Pay' or 'Severance Pay package'.

2.2 The Brazilian healthcare system and prenatal care

In Brazil, a nationwide public healthcare system (SUS) provides universal coverage to all citizens, offering services that are free at the point of use. The achievement of this universality is upheld through substantial financial investments from all levels of government. The services provided range from hospitalizations to emergency admissions, and community care centers, operating throughout the country. The hospital discharge records used in this study are sourced from the SUS. However, running alongside the public sector is a private supplementary healthcare system. In this private sector, independent physicians provide medical services within privately-owned hospitals and clinics.[7]

The Brazilian recommendation for prenatal care in 2012 was of at least six prenatal care visits, including vaccination, routine diagnostic laboratory tests, and the use of supplements or medical treatment for complications. Data from the Brazilian birth records show the evolution of prenatal care coverage in Brazil. In the year of 1995, more than 10% of Brazilian pregnant women did

[7]In this private sector, independent physicians provide medical services within privately-owned hospitals and clinics. The majority of these services are funded indirectly through individual enrollment in private health insurance plans, which are frequently employer-sponsored (comprising approximately 72% of all active plans in 2020). Our results on the effects on delivery choices might be consequence of the loss of private insurance coverage - see Amorim et al. (2023).

not have any prenatal care visit, and in 2015, only 2.2%. Less than half of the pregnant women used to attend seven or more prenatal care visits in 1995, increasing to 66.5% in 2015, showing an expansion of this coverage and the importance of the Brazilian Unified Health System (SUS), created in 1990, to the dissemination of this benefit.

3 Data and sample

Previous work on the health consequences of parental unemployment in the empirical literature used data from longitudinal surveys (Lindo, 2011; Del Bono et al., 2012), or a combination of survey information and administrative data (Wüst, 2015). The use of survey data means that the source of variation in maternal employment status is frequently unspecified, making it difficult to distinguish between voluntary and involuntary job separations. Relying on survey data for birth outcomes also means that birthweight information from surveys is reported by parents and, thus, subject to recall error. Sample sizes of surveys tend to be small, particularly hampering the analysis of relatively rare events. To estimate the causal effect of dismissals on birth outcomes addressing those previous shortcomings hence requires information on recorded maternal employment spells with reasons of dismissals (to distinguish between voluntary job separation and involuntary dismissals) and the time of dismissals, for example, based on social security records, linked to the universe of births from vital statistics data. In this paper, we make use of such linked administrative individual-level data from Brazil. To investigate the effect of dismissals beyond measures at birth, we also link hospitalization and mortality records from administrative sources to birth records. We briefly describe the four datasets below in turn.

3.1 Data sources

Employment records. The first source is the Relação Anual de Informações Sociais (RAIS), a linked employer–employee dataset covering the universe of formal workers and firms in Brazil, made available by the Brazilian Ministry of Labor up to 2014. RAIS identifies workers by both a unique tax code identifier (CPF) and their full name, enabling us to link workers to firms over time and to birth records. The RAIS data includes detailed characteristics of workers' employment spells, such as the start/end date and location of each job, the type of contract, occupation and sector code, and the workers' education and earnings as well information on work absences and their timing and causes. This data enables us to identify dismissals, their timing and every worker's recorded

reason for dismissal.

Birth records. The second dataset comes from birth records from vital statistics data collected through the Sistema de Informações sobre Nascidos Vivos (SINASC), available for the years 2011-2014. These records are based on the universe of birth certificates issued in Brazil, whether they were issued in hospitals, birth clinics or from midwives after home deliveries, accounting for more than 99% of births. This source provides a number of variables of interest for our analysis: information on the date of conception, birthweight, gestational length, babies' gender as well as history of previous deliveries of the mother (i.e., number of live births and stillbirths). We also observe the age and race of the mother. Fundamentally, we use the declared marital status from this dataset to distinguish single mothers.

Infant mortality data. The third dataset comes from vital statistics death records from the Brazilian *Mortality Information System* (Sistema de Informaçoes sobre Mortalidade, SIM, in Portuguese). This dataset contains information on all natural and non-natural deaths in Brazil, including the precise cause of death and characteristics of the deceased. In case of death occurring up to the age of one, mortality records are linked with birth records using the unique birth ID.

Hospitalization data. The fourth dataset comprises hospitalization records from the Sistema de Informaçoes Hospitalares (SIH), which contains details on all admissions from referrals and self-referrals to hospitals in the public health system (SUS),[8] including information on duration, cost and type of hospitalization, and the precise primary causes for hospitalization based on the WHO Classification of Diseases (ICD-10). In contrast to the merge between employment, birth and mortality records using personal identifiers, linking hospitalization records is based, instead, on address of residence, sex and date of birth of the newborns. Because of duplicate identifying variables, this restricts the number of observations we have available for this part of the analysis when omitting observations with duplicate postcode, sex and date of birth information from the sample.

3.2 Sample definition

We follow the literature and focus on the sample of singleton births (dropping any multiple births per pregnancy) and focus on mothers between the ages of 13 and 50 at the time of birth. We also drop observations where there is missing information on the identity of the mother or her date

[8] In contrast to the other health records used in the analysis (birth records and mortality records), the hospitalization data we have access to only captures cases in the public health system. This means that estimates for hospitalization outcomes may be underestimating the true impact of dismissals on the overall hospitalization risk.

of birth, and exclude any duplicate observations.[9] In the linked dataset containing birth and job records, we retain only the first birth observed over the available years for each matched worker and ensure that mothers are economically active in the formal labor market and holding open-ended contracts in the private sector at the time of conception, reducing the number of birth records in the data. We focus mainly on mothers declaring being single as marital status, and exclude mothers with a history of stillbirths and any work absence ended for maternity reasons in the six months prior to conception, as well as any pregnant worker whose job has been terminated for any other reason than *dismissal without just cause*. This leaves us with a sample of 45,458 births from working single mothers in the period 2011-2014. For hospitalization outcomes we have a sample of 29,068 births.

4 Empirical strategy

When estimating the effect of dismissals on birth outcomes, there are three major obstacles to obtaining causal estimates. First, some characteristics of a local area in which job losses occur may be unobservable to the econometrician. If these characteristics are correlated with job losses and with birth outcomes this will lead to spurious correlates when relying on cross-sectional data. For example, some areas may be characterised by a weaker labor market leading to higher rate of dismissals and simultaneously also affect the quality of public services, including prenatal health care. In this case, one might erroneously conclude that maternal dismissals lead to worse health outcomes, a case of failed inference. This is particularly problematic, when using aggregate unemployment shocks.

Second, dismissed workers may strategically respond to dismissal by selection into pregnancy. For example, when being dismissed, women may anticipate pregnancy planned for a later period in response. Such self-selection may lead to underlying characteristics of dismissed mothers being different from expectant mothers in the control group, possibly leading to biased estimates due to selection. While such strategic response is less likely among the single women in our sample, if the information on the timing of dismissals and conception were not available, it would be difficult to rule out strategic fertility responses to dismissals. As we focus on dismissals rather than job separation, we rule out that pregnant women may leave their employment based on private information on their health status or any concern with the pregnancy, a common problem

[9] Most of those cases with missing information on the date of birth (93%) relate to the year 2010, the first year in the SINASC data that collected information on precise birth date, rather than the age of the mother.

in datasets without information on the causes of job separation. By focusing on dismissals *without just cause* by the employer, we also exclude dismissals, where an employer had a *just cause* to dismiss the worker, for example any conduct related reasons for dismissal.

Lastly, pregnant women may be selected into dismissal by their employer based on any concerns relating to the health of the worker when deciding among employees who to make redundant among their work force. Generally, one would expect that employers select workers to be made redundant carefully to meet the firms objectives, while not leading to claims of unfair dismissal. For example, to minimize the impact on productivity, firms may – everything else equal –dismiss workers with shorter tenure and lower firm-specific human capital.[10] Indeed, we find that demographic characteristics vary between dismissed and non-dismissed pregnant mothers. In Panel A of Table 1, we present these characteristics for the non-dismissed mothers, the dismissed mothers and the difference in means in columns (1)-(3). As expected, dismissed mothers are younger, have lower levels of education, a lower salary and shorter tenure at conception.[11] If unobserved and correlated with birth outcomes, these differences might lead to omitted variable bias. Access to administrative employment records gives us access to the same information employers have access to enabling us to include these predetermined characteristics as controls in the regression.

Beyond socio-demographic characteristics, another concern arises from any potential differences in health endowments of mothers. If these are evident to the employer, for example in the form of work absences, they may be used to select workers for dismissal. Worse health endowments of mothers may nevertheless also contribute to worse health outcomes of their children, so any correlation between maternal health status and treatment status would be concerning. The employment records we have access to, also include detailed information on any work absences and we construct a number of measures of work absenteeism, using different causes and periods prior to conception. In Panel B of Table 1 we find that treated and control group have basically an identical level of work absenteeism (for any reason), and for both unpaid leave and sick days, ensuring that dismissed workers are very unlikely selected for dismissal based on health issues leading to work absences.

To address the first identification challenge, we estimate two-way fixed effects models including location and time fixed effects, controlling for any location-specific differences, which might, for

[10] While there is no official employment protection based on seniority (no 'last-in, first-out' rule, as common in the US or the UK), the Brazilian labor market also presents structurally high turnover among younger workers (Corseuil et al., 2014), thus tenure and age profiles between dismissed and non-dismissed workers are likely to be systematically different. The Severance Pay package the high-seniority high-wage workers are entitled to receive also makes them less targeted for redundancies.

[11] Differences in the ethnic composition apparent in the table may be due to correlation with education, rather than a direct result of selection based on ethnicity, which may constitute discriminatory practice.

example, simultaneously impact employment and the provision of maternal health services in the same location.

Second, we restrict the sample of mothers to exclude the possibility of *endogeneous* pregnancies, i.e. fertility decisions taken in response to being dismissed. We denote child i's month of conception with t, child i's announcement month of maternal dismissal with T, the month of occurrence for the first prenatal visit t_v and the imputed last month of pregnancy with $t+9$. To rule out selection into pregnancy, we do not include cases in our estimation sample in which child i's month of maternal dismissal T is such that $T < t$ or $T > t_v$. An indicator function for treatment status is constructed as follows:

$$D = \begin{cases} 1, \text{if } t \leq T \leq t_v \\ 0, \text{if } T \in \{\emptyset\} \cup \{t+10, t+11, \ldots\} \end{cases}, \tag{1}$$

for which child i is treated ($D = 1$) only if the mother experiences a dismissal while pregnant, but not later than the month of the first prenatal visit. This means, we restrict our sample to include pregnant women that were continuously employed at point of dismissal.

To address any issues with selection into redundancy by the employer, we include the extensive set of observable maternal and employment characteristics, controlling for selection into treatment using the observable characteristics of workers available to the employer.

We then recover the causal effect of dismissals on the health and additional outcomes by estimating the following equation:

$$y_{ikt} = \tau D_i + \mathbf{X}_i'\boldsymbol{\beta} + H_i + \nu_k + \theta_t + \epsilon_{ikt} \tag{2}$$

where y_{ikt} is the outcome of interest for child i, born to a mother living in municipality k, conceived at month t. The indicator D_i is equal to 1 if i is exposed in utero to a maternal layoff, and 0 if otherwise. $\mathbf{X}_i$ is a vector of maternal and employment characteristics including the number of previous livebirths, the mother's age category and ethnicity, education, occupation category (blue vs white collar), weekly hours and monthly wage, as well as months of tenure at conception; H_i is a latent health factor for child i' mother, proxied by pre-conception work absences. ν_k denotes municipality of residence fixed effects capturing any unobserved differences in the local environments, for example differences in the provision of prenatal health care, and θ_t denotes month of conception fixed effects. ϵ_{ikt} denotes the error term. We allow for clustering of standard errors at the municipality-by-month level. τ denotes the coefficient of interest of the effect of maternal dismissals on the

number of health outcomes at birth including birthweight, low birthweight, gestational length and additional health outcomes.

In addition, we probe our estimates further in three exercises. First, in addition to including, location and time fixed effects, we probe our main specification with the inclusion of additional fixed effects, namely hospital, firm municipality, industry sector fixed effects and municipality-specific time trends.[12]

Second, we test whether our results could be affected by anticipation effects and potentially result in self-selection into pregnancy to protect against dismissal.[13] This is an issue separate from selection into pregnancy in response to dismissal, previously introduced. To investigate whether our results are sensitive to self-selection by restricting the timing of layoffs of treated mothers. For this purpose, we restrict the treatment assignment window such that the indicator $D = 1$ only if the month of maternal dismissal T is such that $t \leq T < t_v$. This restriction ensures that the layoff happens strictly before a pregnancy can be confirmed during a pre-natal visit, and possibly any early pregnancy complications are known to the mother and we present estimates from specification for each outcome where the alternative treatment assignment has been applied. Lastly, we re-estimate the effects using regular dismissals and excluding workers dismissed during mass layoffs. This is in response to Weber et al. (2023) who show, that female workers may be aware of their employers' economic problems and anticipate downsizing at her workplace and, if successful, would end up in the control group again leading to a different composition of expectant mothers selecting into fertility. To test whether anticipatory fertility responses to mass layoffs may affect our estimates, we exclude from the sample any workers dismissed in mass layoffs defined by layoffs of more than 75% of the workforce in any given year.

[12]Hospital fixed effects account for any differences in prenatal care and delivery services for mothers living in the same municipality, but giving births in different hospitals; possibly, they also capture fixed neighborhood characteristics through any hospital catchment area within the municipality and in rural municipalities account for the fact that delivery may take place in hospital outside of the municipality of residence. Firm municipality fixed effects capture any differences in the provision of health services in case the worker's firm is situated in a municipality different from the municipality of residence and the worker may attend health services close to the workplace. Industry sector fixed effects capture any sector specific factors that may affect health at birth, for example through prenatal environmental exposure, which are not captured by the occupation category dummy.

[13]As Brazilian labor laws protect pregnant women from being dismissed, women threatened by job displacement could potentially select into pregnancy for dismissal protection - a behavior recently documented for Hungary Weber et al. (2023).

5 Results

5.1 Effects on birth outcomes

In Table 2, we present the effect of dismissals on birthweight as measured in grams and on an indicator for low birthweight to investigate the effects on lower parts of the birthweight distribution. In column (1), we report the coefficient for a specification including municipality and month of conception fixed effects. We find that being dismissed from their job during gestation leads to a reduction in birthweight by over 30 grams. The magnitude of the estimates effect of dismissal is sizeable and comparable with exposure to a variety of other shocks in-utero, such as exposure to economic downturns in Argentina (Bozzoli and Quintana-Domeque, 2014) or maternal dengue infections during pregnancy (Foureaux Koppensteiner and Menezes, 2024). In column (2), we add a large set of controls for mother, pregnancy and job characteristics. These include the set of observable characteristics employers have access to and may take into account when deciding on employees to layoff, for example job tenure, wage, hours worked, occupation dummies, education and information on work absences. The inclusion of these controls leads to an increase in the reduction of birthweight to about 39 grams. Next, we probe the robustness of the estimates by including a set of additional fixed effects.[14] In column (3) we include additionally hospital fixed effects. Hospital fixed effects control for the quality of delivery services and for the quality of local prenatal and general health services linked to the hospital. The coefficient for birthweight is virtually unchanged. In column (4) location of the firm fixed effects, accounting for any constant properties in case municipality of residence is not the same as the municipality of the location of the employer. Again, the coefficient is unchanged. In column (5) we probe the robustness to alternative fixed effects further by including sector fixed effects in addition to the municipality fixed effects accounting for sector specific time invariant characteristics. In column (6), we include those additional fixed effects simultaneously. Both specifications do not change the estimates in a meaningful way. Finally, in column (7), in addition to the full set of fixed effects, we include municipality specific time trends. The inclusion of municipality time trends reduces the size of the negative effect slightly to about -25 grams. In Panel (b), we repeat the exercise for low birthweight. We find that dismissals during pregnancy, increase the chance for low birthweight substantially by about 2.2 percentage points in our preferred specification, including the rich set of covariates, a

[14] When including additional fixed effects, a small number of singleton observations reduces the number of observations slightly. The largest drop occurs when including municipality specific trends in column (7), where we lose just over 10% of observations mostly in small and medium size municipalities ($<$ 50,000 population).

27% increase given the baseline incidence of 8.2%. This indicates that dismissals have a particularly strong negative effect at lower parts of the birthweight distribution. The coefficient is extremely stable across all the different specifications.

As outlined in the empirical strategy section, we probe the robustness of the estimates further with two additional exercises. In the first exercise, we apply the even more rigorous treatment assignment strategy by limiting the occurrence of layoff to be strictly before the month of the first prenatal visit and hence reduce the treatment group to children whose mothers have been dismissed before the pregnancy could be confirmed and documented by a medical professional, reducing the number of observations in the treated group by about 40%. The effects, reported in Appendix Table A2, are more pronounced for birthweight (column (1)), and very similar in magnitude for low birthweight (column (2)). We take this evidence that our estimates are not driven by the effects on the composition of treated and control group from self-selection into pregnancy in anticipation of potential dismissals.

In a second additional robustness exercise, we re-estimate the effects of regular layoffs excluding workers dismissed during mass layoffs. We present the results in Appendix Table A3, and find that the effects on birthweight and low birthweight (columns (1) and (2)) are virtually unchanged when compared to the corresponding coefficients in column (2) of 2, lending further credibility to our estimation strategy.

5.2 Mechanisms and heterogeneous effects

There are a number of possible transmission mechanisms behind the effects on birthweight and low birthweight as a result of dismissal: Firstly, in-utero selection leading to a culling effect, for example affecting the sex composition of surviving newborns. Previous research has shown that male fetuses are at at higher risk of miscarriage, in particular under a detrimental in-utero environment (Kraemer, 2000). Secondly, low birthweight may be mechanical result of shorter gestational length. Thirdly, it may be caused by intra-uterine growth retardation which can originate from maternal, placental, neonatal, genetic and environmental factors, including maternal malnutrition (Murki and Sharma, 2014). We start with directly testing the first two channels. In Table 3, we present the coefficients from the effect of dismissals on sex ratio, serving as a proxy for sex-specific miscarriage risk,[15] gestation as well as other birth outcomes (unplanned C-sections and Apgar scores). We find

[15]Information on miscarriage, in particular early in gestation are notoriously unreliable. If an environmental shock during gestation favors girls, this might affect mean birth weight, because male newborns tend to have higher birthweight. We control for sex of the newborn, which controls for any culling effect, but we still want to test for the existence of such effects.

that dismissals have a very small, insignificant effect on a female dummy, indicating that in-utero selection does not seem to drive the results on birthweight. Next, we look at gestation and a dummy for preterm delivery (a gestational length of less than 37 weeks completed). We find a small and marginally significant, positive effect on gestational length, of about one day. This positive effect is consistent with findings in the literature on the positive association between the early maternity leave during pregnancy and birth outcomes and the negative association between physical and mental strain during pregnancy, and gestational length (Corchero-Falcón et al., 2023). The positive effect on gestation precludes an underlying mechanism based on reduced gestational length. We also find a positive, but insignificant effect on preterm delivery, possibly pointing to a more complex relationship between dismissals and gestational length. Taken together, precluding a major role for in-utero selection and short gestation, this implies in turn that the effects on birthweight may be primarily due to impediments to growth in utero. Other indicators of health at birth, investigated in the table, including emergency c-section, and one and 5 minute Apgar scores are very small and statistically insignificant.

Next, we explore whether dismissals have differential effects on particular subgroups (Appendix Table A1). We start with subgroups by pregnancy characteristics. We find slightly more pronounced negative effects for male fetuses with a reduction of 44 grams compared to 32 grams for female fetuses and more pronounced effects on low birthweight for males, which is in line with the results in the biomedical literature pointing to the relative fragility to maternal stress of male fetuses in-utero (Hansen et al., 1999). In line with the findings on the positive effect of dismissals on gestation, we find that dismissals have less pronounced effect among children born prematurely, but given the relatively small sample size, the coefficient is not statistically significant.

We then investigate the effects for subgroups based on maternal characteristics. While we find that the estimated main effects are driven by mother's with at most secondary schooling, we find a positive, but imprecisely estimated effect on birthweight for the children of mother's with higher education. Possibly for this subgroup, the economic shock from dismissals may not be as severe while the positive effect from longer gestation may contribute to a positive net effect on birthweight and low birthweight. We also find that dismissals have a more pronounced effect for mothers younger than 21 at birth (-62 grams, versus 33 grams for mothers with an age between 21 and 35). The effect for older mothers is positive (52 grams), but given the small number of observations in this group statistically insignificant - again, this may be due to older mothers being able to shield the shock. Next, we estimate the effects separately for white versus non-white mothers

and find a stark difference in the effects (-12 grams versus -49 grams, respectively). Finally, we estimate the effects separately for mothers in white versus blue collar jobs and find a particularly pronounced, but insignificant, effect for mothers in blue collar jobs (-114 grams). Taken together, the sub-group analysis presented in Table A1 points to pronounced heterogeneous effects along a number of relevant margins, including socioeconomic status.

With an indication of a role for intrauterine growth retardation, we are interested in further exploring whether the effects vary along the wage distribution. This could indicate that the income shock from dismissal contributes to poor nutrition among some single mothers. We investigate the heterogeneous effects by wage quartile and severance pay package. Motivated by the findings on heterogeneous effects, we make use of the detailed information on wage income and severance pay package of single mothers as means of self-insurance to explore the role informal insurance against job loss play in the relationship of dismissals and birth outcomes. We start by examining whether the effect on birthweight and low birthweight differs by income. For this purpose, we estimate the effect of dismissals separately by wage quartiles. We present the coefficients in Figure 1. While we find a negative (positive) effect for birthweight (low birthweight) across almost all wage quartiles, the effects are more pronounced for women with wages in the lowest quartile. The results are aligned with the previous conclusions when looking at the distribution of the effects by quartile of Severance Pay - the bottom quartile shows the strongest and most significant effect for birthweight reduction and low birthweight. The more pronounced effects for women on lower incomes are consistent with the heterogeneous effects by maternal characteristics from the previous section.

5.3 Effects on prenatal visits and mode of delivery

Dismissals may also affect health services utilization through the economic impact on household income. Quality of prenatal care may in itself then play a role a role for explaining effects on birth outcomes. Brazil's health system is characterized by a well functioning public prenatal care system generally achieving high levels of prenatal care with a majority of expecting mothers attending 8 prenatal care visits, well above the minimum 4 antenatal visits recommended by the WHO and above the recommendation by Brazilian health authorities of a minimum of 6 visits. Prenatal visits are important for the detecting underlying health issues, such as pre-eclampsia, and meeting the minimum recommended number of visits has been shown to positively impact birth weight and to

decrease the propensity for premature birth (Gajate-Garrido, 2013).[16] In Table 4 we present the estimates for prenatal visits (column (1)) and an indicator for fewer than 6 visits (column (2)). We find that dismissals reduce the number of prenatal visits slightly by about half a visit; compared to the mean number of visits in the group of women without dismissals this constitutes a relatively moderate 6% reduction. More importantly, we find an increase (0.144) in the propensity to have fewer than the recommended 6 prenatal visits. Compared to the mean propensity (0.133), this amounts to more than double the rate, pointing to a possible role for reduced prenatal investments in response to the dismissals.[17] We investigate this further by making use of the detailed information on the utilization of private health care for delivery. In column (3) we provide the coefficient for delivery in private clinics and document a very substantial reduction of the fraction of deliveries in private clinics in response to dismissals. Compared to the mean (0.22), dismissals reduce the fraction of deliveries utilizing private clinics by 35%. As private health insurance is a frequently offered employment benefit provided by firms, the reduction is likely a combination of loss of access to employer provided private health insurance and privately acquired private health insurance due to the negative shock to the income. These results possibly indicate lower investments of laid-off mothers during pregnancy. We note also a substantial decrease in planned C-sections (column (4)), which may be linked to the less frequent use of private clinics for delivery. Less elective C-sections may also motivate the slightly longer gestational length among dismissed mothers.

5.4 Longer-term effects on infant health

In addition to understanding the effect of dismissals during gestation on immediate measures of health at birth, we are also interested in their longer-term effects on children's health. We investigate the effect of maternal dismissals on longer-term health outcomes of children with two measures: hospitalization of children in their first year and infant mortality, i.e. where the child dies in the first year of life. In Table 5 we first present coefficients for a number of different measures of health using data from hospitalizations, followed by the estimates for infant mortality. In contrast to most shocks in-utero studied in the literature, which have a transitory nature, the shock we study in this paper may have an absorbing state (extending beyond the period in-utero). This has important implications for the interpretation of the effects on longer-term outcomes, because – for example

[16] There is some evidence though that the timing and meeting the minimum number of prenatal visits is more important than the overall number number of visits in particular beyond the recommended minimum (McDuffie and Cross, 1996).

[17] The information on prenatal visits is collected together with the other birth record information and hence reports on the total number of prenatal visits irrespective of whether these occurred in the public or private health sector.

– longer-term outcomes, such as hospitalization in the first year after birth, in this case can no longer be attributed to a transitory shock during pregnancy exclusively. This means any effect on outcomes measured after birth might be due to a combination of the longer-term effects of in-utero exposure to dismissals and the contemporaneous effect of continued joblessness after birth. We find that dismissals during pregnancy lead to a substantial increase in the probability of hospital admission in the first year of birth. We document a 3.2 percentage point increase, a 32% increase compared to the non-dismissed mean, significant at the 1% level of significance. The result points to dismissals having an impact on the health of children beyond conventional measures of health at birth, such as weight. Analogously, newborns exposed to maternal dismissal in-utero are also more likely to have more hospitalization episodes (+29%). The effect on days hospitalized is positive, but insignificant and we find no effect on the severity of hospitalizations, proxied by ICU admission. Unsurprisingly, given the increase in hospitalizations, we also find a significant increase in the (log) cost from hospitalization. Lastly, we estimate the effect of dismissal on infant mortality. We find that dismissals increase mortality during the first year of life substantially, by more than doubling the chance of infant death when compared to the control group mean, although the coefficient is only marginally significant.

To investigate the timing of hospitalizations further, we estimate separately the effect of dismissals on hospitalizations within 7 days from birth (equivalent to the early neonatal period), hospitalization between 8-28 days and hospitalizations between 4 weeks and 1 year and report the coefficient estimates in the first graph of Figure 2. We find that dismissals have a significant positive relative effect (in the graph we display the percent effect compared to the baseline) increasing the hospitalization risk by more than 30% in the first week after birth, during which hospitalizations are most frequent. For the subsequent three weeks, the coefficient is even more pronounced increasing the risk for hospitalization by more than 100% but the estimate is noisier and only marginally significant, while we observe a smaller, positive, but insignificant effect beyond the first 4 weeks after birth. Infant mortality patterns are slightly different, with effects being largely insignificant for the first month and a pronounced effect for the period after the first four weeks after birth increasing the chances for infant death by over 400% for infants from dismissed mothers. This pattern is consistent with infants being hospitalized in the first four weeks after births, and unfortunately some of those infants subsequently passing away after hospital treatment, leading to the lag in the mortality pattern.

To provide insights on the underlying mechanism of the effect on hospitalization, we estimate

the effect of dismissals separately by cause of hospitalization and mortality and display the results in Figure 3.[18] We find that hospitalizations are driven by *Certain conditions originating in the perinatal period*. These include, among others, *Disorders of newborn related to length of gestation and fetal growth (ICD-10 P codes)*, *Respiratory and cardiovascular disorders specific to the perinatal period*, and *Newborn affected by maternal factors and by complications of pregnancy, labor, and delivery*. Coefficients of other causes are relatively small, with the exception of *Diseases of the respiratory system*, but statistically insignificant.[19] The estimates on causes of hospitalization are noisier, but follow a similar pattern, with the most pronounced effect for mortality caused by *Certain conditions originating in the perinatal period. Congenital malformations, deformations and chromosomal abnormalities* display an effect of similar size, but the estimate is again imprecise.

These findings demonstrate that maternal dismissals impact the health of children beyond immediate measures of health. A combination of longer-term in-utero effects and contemporaneous effects of continued joblessness, caused, for example, by reduced financial resources over the first year of life may lead to differential health trajectories over time consistent with the patterns we observe for hospitalization and mortality.

5.5 Birth outcomes of non-single women

Although not focus of this paper, because of the complexities when studying joint household responses to shocks and data limitations, for completeness we provide the main outcomes for mothers in a relationship that lose their job during pregnancy.[20] We present the results in Table 6. In contrast to the single mothers, we find that dismissals have a positive, but insignificant effect on birthweight, with no effect for low birthweight. In line with the findings for single mothers, we also find a positive effect on gestational length with the effect being more pronounced than for our single mothers (about one and a half days longer). The results are consistent with the positive effect of prolonged gestation overcompensating any potential negative effect on growth in-utero. This might be the case as partners income may help to absorb the negative shock on income and/or provide mental support in the aftermath of dismissals, not readily available from a partner in case of single mothers affected by dismissal. We also cannot rule out a role for selection in utero, with a significant effect on the sex ratio at birth.

[18] The different groups of causes are based on letter codes from ICD-10. We focus on the groups with the largest incidence, grouping the remainder of infrequent categories into 'Other'.

[19] ICD-10 N codes include diseases involving the kidneys, breasts, urinary and genital organs.

[20] For example, in contrast to single mothers for which we observe the income perfectly etc., there is much less certainty on fathers, possibly leading to a selected sample.

5.6 The role of unemployment insurance for health at birth

We further investigate whether there is an actual resource constraint problem arising for dismissed pregnant workers. From a policy perspective, it is thus important to understand whether traditional public policies supporting unemployed workers can provide – at least in part – a formal insurance. In this section, we investigate the effect of unemployment benefits, which is the main policy aimed at supporting dismissed workers. Brazilian workers are eligible for 3–5 months of unemployment benefits when dismissed without a just cause from a formal job, conditional on continuous employment in the six months prior to the layoff.[21] The benefit level is a function of the average wage in the three months preceding dismissal and ranges from 100% to 187% of the minimum wage – and hence, high-earning workers are expected to receive relatively less compared to low-earning employees.

5.6.1 Regression Discontinuity Design

For the analysis here, we focus on children of working mothers holding open-ended jobs in the private sector that were dismissed during pregnancy, rather than on the more narrow definition of treatment in (1).[22] In this part of the analysis we explore the role of unemployment insurance using the eligibility criteria of UI for those workers. For that purpose, we compare the birth outcomes of working mothers who are eligible and ineligible for UI benefits after a dismissal by estimating (alternative versions of) the following equation:

$$y_{ikt} = \lambda \text{UI}_{it} + f(Tenure_i) + \mathbf{X}_i'\mathbf{\Gamma} + H_i + \nu_k + \theta_t + \epsilon_{ikt}, \tag{3}$$

where y_{ikt} as the birth outcome of interest for child i (birthweight or low birthweight), to a mother living in municipality k, conceived at time t; $Tenure_i$ is the running variable of the regression discontinuity (RD) design, that is, tenure in months of continuous employment before layoff standardized so that $Tenure = 0$ at the cutoff required for eligibility (i.e., 6 months); $f(\cdot)$ is a flexible polynomial regression. UI_{it} is a dummy taking the value of one for workers who are eligible for UI (i.e., $\text{UI} = 1(Tenure_i \geq 0)$). We include a number of covariates, $\mathbf{X}_i$, to increase the precision of

[21] The condition does not strictly require that the employer must be the same for the previous 6 months, but we assign treatment based on continuous employment in the same firm. This stringent assumption may, if anything, lead to an underestimation of an UI eligibility effect. Secondarily, laid-off workers need a minimum of a 16-month period between the current layoff date and the most recent layoff date used to claim UI in the past. Given the size of our sample and the periods available, we do not exploit additional variation from this condition, but focus on the cutoff from the primary eligibility criterion.

[22] We narrow the sample for the RDD analysis to The sample for this empirical analysis is not defined, initially, as per the treatment assignment rule in (1). However, we also present results based on the same sample.

the regression discontinuity estimator, and the usual set of FE as in the specification of the main analysis. Any latent health factors is subsumed into H_i.

The coefficient λ estimates the effect of UI eligibility, or equivalently, the intention-to-treat effect of UI claims. In alternative specifications we allow the effect of UI entitlement to differ across the (forgone) wage distribution by specifying an interaction term between earnings and UI eligibility. This is based on the intuition that receiving a cash transfer for dismissed pregnant mothers in the left tail of the labor income distribution provides a more tangible benefit during economic hardship compared to the ones in the right tail of the distribution, who may have sufficient savings and a lower replacement rate from the cash transfer.

The first concern with RD estimates is the possible manipulation of the running variable, which in the present case may, for example, arise because six months of job tenure may be a salience point for evaluating employees' performance (Gerard and Gonzaga, 2021). A key assumption for the validity of RD research design is that the distribution of individuals' potential outcomes varies continuously with the running variable around the cutoff. This ensures that the only systematic difference between units close to but on different sides of the cutoff is their treatment assignment. Hence, a jump in the density of the running variable at the cutoff is argued to be a strong indication of manipulation (McCrary, 2008). Appendix Figure A2 shows no evidence of density discontinuity around the 6-month cutoff for neither single mothers nor couples, as also confirmed by the bias robust test with local polynomial density estimators developed in Cattaneo et al. (2020). In Appendix Figure A3 we also provide further balancing tests for the range of covariates among worker's characteristics. Overall, these checks support the assumption of no manipulation of the running variable and no discontinuity in characteristics of mothers on either side of the UI eligibility cutoff.

5.6.2 Results

The estimates from our RD design are reported in Table 7, where we show the results of alternative regression specifications on both birthweight and low birthweight. The first column shows the effect of UI eligibility on the birth outcomes of interest, when controlling for newborn's sex and mother-specific covariate and including the usual set of FE (municipality of residence and month of conception). Presented in column (1), the average effect of UI on birthweight is about 64 grams (statistically significant at 10%) and on low birthweight is around 24%, being insignificant

nonetheless.[23] In column (2) we enrich the specification with a slope effect from the interaction of UI eligibility with the last wage earned by the mother. Interestingly, we find that the effect of UI eligibility decreases in the foregone wage, thus doubling the average effect of UI eligibility. This implies that births to low-earning mothers have a greater benefit from UI claims compared to high-earning mothers'.

As manipulation and intentional misreporting of job tenure could yet be a concern for our RD estimates, we re-estimate the effect of UI on a sample that omits mothers with six months of continuous employment at layoff in column (3): the results for both birthweight and low birthweight do not differ from the ones in column (2) in any meaningful way.

In an additional exercise, we re-estimate the effects in Table 7 with our main estimation sample. With just less than half the available observations in the same sample used in the analysis of dismissal effects, we still retrieve qualitatively similar effects of UI eligibility, but the estimates are less precise. We present the results of this exercise in Appendix Table A4.

Taken together with the heterogeneous effects by income quartiles, these results suggest that liquidity constraints may be an important driver of the effects of dismissals on the health of infants for single mothers. With diminished financial resources to draw upon after dismissal, and more pronounced effects for the most financially vulnerable mothers, dismissals may affect the short and long-term health of children by constraining the resources available to single mothers during pregnancy, for example through impacting maternal nutrition contributing to intra-uterine growth retardation. Unemployment insurance may be an effective way of protecting mothers from the negative income shock from dismissals and hence reduce the negative effect on the health of their infants. Providing these single-mother households with a cash transfer after displacement can, instead, mitigate the negative consequences and possibly be even transiently beneficial for the unborn children's health at birth, relieving mothers from strenuous activities at work.

6 Concluding remarks

In this paper, we estimate the effect of dismissals during pregnancy on birth and infant outcomes using administrative Brazilian vital statistics records linked to employer-employee data. We find that dismissals of pregnant mothers from employment reduce birthweight by almost 40 grams and increases the chance for low birthweight by more than 25% compared to the baseline incidence. The

[23] The effects are also similar in magnitude to the positive effects on birthweight found for conditional cash transfers in Uruguay (Amarante et al., 2016).

effects are more pronounced for workers in the lowest wage quartile possibly indicating a nutritional channel leading to growth retardation. We document considerable mitigating effects of formal unemployment insurance, exploiting an eligibility rule of unemployment insurance in a regression discontinuity setting. We also find that dismissals have lasting effects on the health of children, documenting a stark increase in hospitalizations of children in the first year after birth and a very substantial increase in infant death as consequence of dismissals during pregnancy. The results are important for policy as they provide new insights on the negative consequences of dismissals on single mothers not previously known, both on measures of health at birth and medium-term measures such as hospitalizations. The results also inform about important mitigating factors, including self-insurance and unemployment benefits.

References

Aizer, Anna and Janet Currie, "The intergenerational transmission of inequality: Maternal disadvantage and health at birth," *Science*, 2014, *344* (6186), 856–861.

Almond, Douglas and Janet Currie, "Human capital development before age five," in "Handbook of Labor Economics, Volume 4B," Vol. 4, Elsevier B.V., 2011, pp. 1315–1486.

Amarante, Verónica, Marco Manacorda, Edward Miguel, and Andrea Vigorito, "Do cash transfers improve birth outcomes? Evidence from matched vital statistics, program, and social security data," *American Economic Journal: Economic Policy*, 2016, *8* (2), 1–43.

Amorim, Guilherme, Diogo G.C. Britto, Alexandre de Fonseca, and Breno Sampaio, "Job loss, Unemployment Insurance and Health: Evidence from Brazil," *SSRN Electronic Journal*, 2023.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes, "Losing heart? The effect of job displacement on health," *Industrial and Labor Relations Review*, 2015, *68* (4), 833–861.

Bozzoli, Carlos and Climent Quintana-Domeque, "The weight of the crisis: Evidence from newborns in Argentina," *The Review of Economics and Statistics*, 07 2014, *96* (3), 550–562.

Browning, Martin and Eskil Heinesen, "Effect of job loss due to plant closure on mortality and hospitalization," *Journal of Health Economics*, 2012, *31* (4), 599–616.

Bubonya, Melisa, Deborah Cobb-Clark, and Mark Wooden, "Job loss and the mental health of spouses and adolescent children," *IZA Journal of Labor Economics*, 2017, *6* (1), 1–27.

Carlson, Kyle, "Fear itself: The effects of distressing economic news on birth outcomes," *Journal of Health Economics*, 2015.

Carneiro, Pedro, Kjell Salvanes, Barton Willage, and Alexander Willen, "Childhood Shocks Across Ages and Human Capital Formation," *HCEO Working Paper*, 2023.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma, "Simple local polynomial density estimators," *Journal of the American Statistical Association*, 2020, *115* (531), 1449–1455.

Corchero-Falcón, MDR, Gómez-Salgado J., García-Iglesias JJ., Camacho-Vega JC, Fagundo-Rivera J., and Carrasco-González AM., "Risk Factors for Working Pregnant Women and Potential Adverse Consequences of Exposure: A Systematic Review.," *International Journal of Public Health*, 2023, *68*, 1–14.

Corseuil, Carlos Henrique, Miguel Foguel, Gustavo Gonzaga, and Eduardo Pontual Ribeiro, "Youth Turnover in Brazil: Job and Worker Flows and an Evaluation of a Youth-Targeted Training Program," CEDLAS, Working Papers 0155, CEDLAS, Universidad Nacional de La Plata February 2014.

Crabtree, Steve and Sofia Kluch, "How many women worldwide are single moms," *GALLUP, Inc*, 2020.

De Cao, Elisabetta, Barry McCormick, and Catia Nicodemo, "Does unemployment worsen babies' health? A tale of siblings, maternal behaviour, and selection," *Journal of Health Economics*, 2022, *83*, 102601.

Dehejia, Rajeev and Adriana Lleras-Muney, "Booms, busts, and babies' health," *Quarterly Journal of Economics*, 2004, *119* (3), 1091–1130.

Del Bono, Emilia, John Ermisch, and Marco Francesconi, "Intrafamily resource allocations: A dynamic structural model of birth weight," *Journal of Labor Economics*, 2012, *30* (3), 657–706.

Foureaux Koppensteiner, Martin and Livia Menezes, "Maternal dengue and birth outcomes," *American Economic Journal: Applied Economics*, 2024, *12* (2), 1–25.

Gajate-Garrido, Gissele, "The Impact of Adequate Prenatal Care on Urban Birth Outcomes: An Analysis in a Developing Country Context," 2013.

Gerard, François and Gustavo Gonzaga, "Informal labor and the efficiency cost of social programs: Evidence from unemployment insurance in Brazil," *American Economic Journal: Economic Policy*, 2021, *13* (3), 167–206.

_ **and Joana Naritomi**, "Job displacement insurance and (the lack of) consumption-smoothing," *American Economic Review*, 2020.

Hansen, Dorthe, Henrik Møller, and Jørn Olsen, "Severe periconceptional life events and the sex ratio in offspring: follow up study based on five national registers," *BMJ*, 1999, *319* (7209), 548–549.

Kraemer, Sebastian, "The Fragile Male," *British Medical Journal*, 2000, *321* (7276), 1609–1612.

Lindo, Jason M., "Parental job loss and infant health," *Journal of Health Economics*, 2011, *30* (5), 869–879.

Marcus, Jan, "The effect of unemployment on the mental health of spouses – Evidence from plant closures in Germany," *Journal of Health Economics*, 2013, *32* (3), 546–558.

McCrary, Justin, "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics*, 2008, *142* (2), 698–714.

Menclova, Andrea, "The Effects of Unemployment on Prenatal Care Use and Infant Health," *Journal of Family and Economic Issues*, December 2013, *34* (4), 400–420.

Michaud, Pierre Carl, Eileen M. Crimmins, and Michael D. Hurd, "The effect of job loss on health: Evidence from biomarkers," *Labour Economics*, 2016, *41*, 194–203.

Murki, Srinivas and Deepak Sharma, "Intrauterine Growth Retardation - A Review Article," *Journal of Neonatal Biology*, 2014, *3* (3).

Olafsson, Arna, "Household financial distress and initial endowments: Evidence from the 2008 financial crisis," *Health Economics*, 2016, *25* (S2), 43–56.

Oreopoulos, Philip, Mark Stabile, Randy Walld, and Leslie L. Roos, "Short-, Medium-, and Long-Term Consequences of Poor Infant Health," *Journal of Human Resources*, 2008, *43* (1), 88–138.

Reader, Mary, "The infant health effects of starting universal child benefits in pregnancy: Evidence from England and Wales," *Journal of Health Economics*, 2023, *89*, 102751.

Robert, Beck Arne Bischoff Kimberley McDuffie and Orleans Miriam Cross, "Effect of Frequency of Prenatal Care Visits on Perinatal Outcome Among Low-Risk Women: A Randomized Controlled Trial," *Journal of the American Medical Association*, 1996, *275.*

Schaller, Jessamyn and Ann Huff Stevens, "Short-run effects of job loss on health conditions, health insurance, and health care utilization," *Journal of Health Economics*, 2015, *43*, 190–203.

Sullivan, Daniel and Till Von Wachter, "Job displacement and mortality: An analysis using administrative data," *Quarterly Journal of Economics*, 2009, *124* (3), 1265–1306.

Ulyssea, Gabriel, "Firms, Informality, and Development: Theory and Evidence from Brazil," *American Economic Review*, 2018, *8* (108), 2015–47.

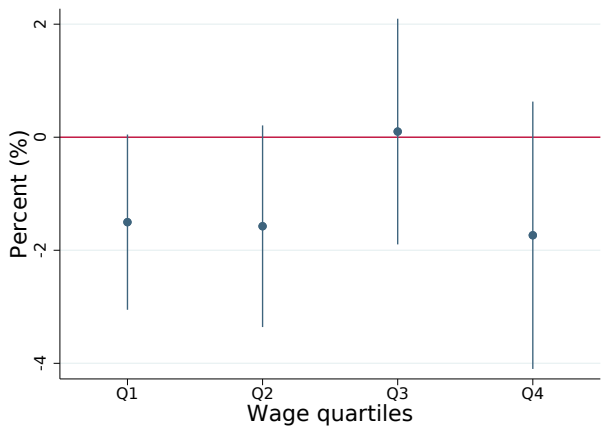
van den Berg, Gerard J., Alexander Paul, and Steffen Reinhold, "Economic conditions and the health of newborns: Evidence from comprehensive register data," *Labour Economics*, 2020, *63*, 101795.

Weber, Andrea, Anna Bardits, Anna Adamecz, MÃ¡rta Bisztray, and Agnes Szabo-Morvai, "Precautionary Fertility: Conceptions, Births, and Abortions around Employment Shocks," CEPR Discussion Papers 17988, C.E.P.R. Discussion Papers March 2023.

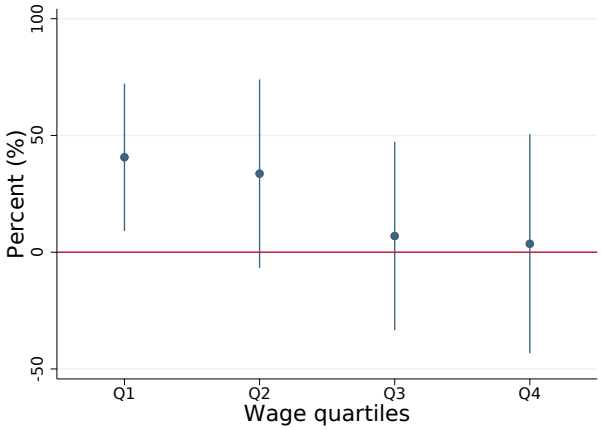
World Bank, "Labor Force, labor force participation and unemployment," 2021. Data retrieved from World Development Indicators.

Wüst, Miriam, "Maternal employment during pregnancy and birth outcomes: evidence from Danish siblings.," *Health Economics*, 2015, *24* (6), 711–725.

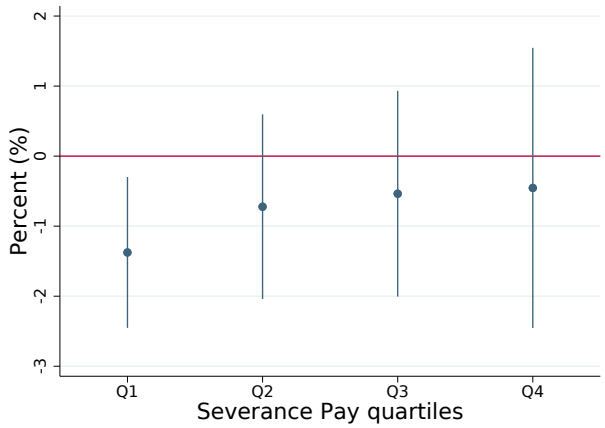
Figures and Tables



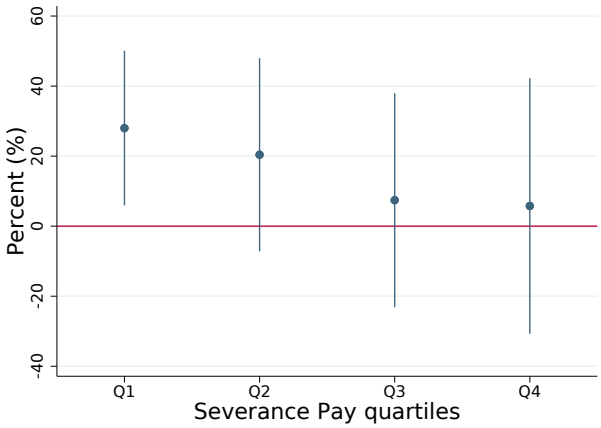
(a) Birthweight effects by wage quartile



(b) Low birthweight effects by wage quartile



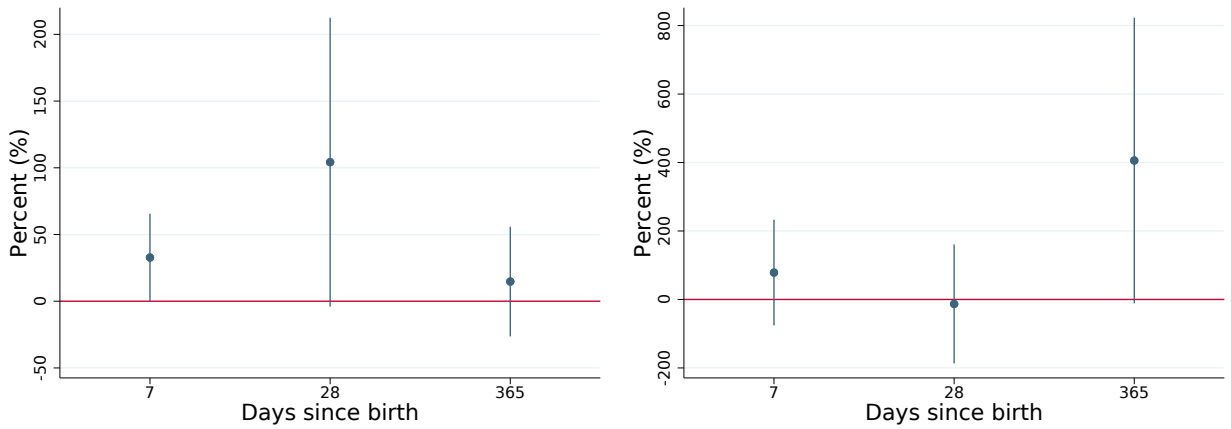
(c) Birthweight effects by SP quartile



(d) Low birthweight effects by SP quartile

Figure 1: Effect of maternal dismissals on birthweight by wage and Severance Pay quartile.

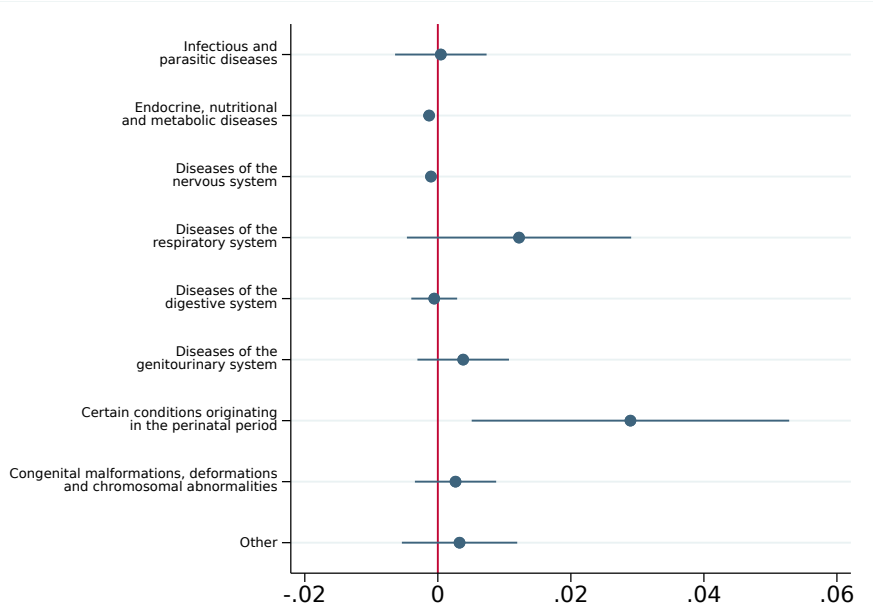
Note: This figure shows the effect of maternal dismissals on birthweight measures by maternal monthly wage quartiles, based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. On the vertical axis, we display the coefficients, in percentage terms over the baseline mean, (and 95% CI) from regressions of outcome variables on a maternal dismissal indicator for exposure during pregnancy. On the horizontal axis, we display the quartile for which the regressions are run. All regressions include fixed effects for municipality of residence and month of conception. We control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception.



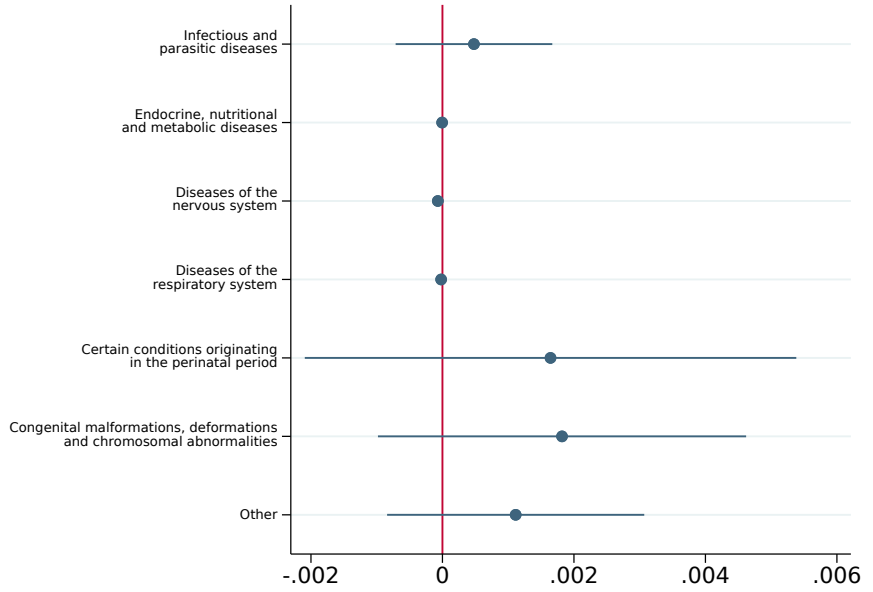
(a) Hospitalization **(b)** Mortality

Figure 2: Effect of maternal dismissals on period-specific hospitalization and mortality.

Note: This figure shows the effect of maternal dismissals on period-specific hospitalization and mortality, based on births over the period between 2011 and 2014. All outcomes are measured within the first year from birth. *Hospitalization* indicates whether the infant has ever been hospitalized. *Mortality* indicates whether the infant has died. On the vertical axis, we display the coefficients, in percentage terms over the baseline mean, (and 95% CI) from regressions of outcome variables on a maternal dismissal indicator for exposure during pregnancy. On the horizontal axis, we display the period for which the regressions are run, i.e., 0-7 days, 8-28 days, 29-365 days. All regressions include fixed effects for municipality of residence and month of conception. We control for infant's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception.



(a) Causes of hospitalization



(b) Causes of death

Figure 3: Effect of maternal dismissals by cause of hospitalization and mortality.

Note: This figure shows the effect of maternal dismissals by cause of hospitalization and mortality, based on births over the period between 2011 and 2014. Causes of hospitalization follow the International Statistical Classification of Diseases and Related Health Problems (ICD-10). We report the coefficients for the most common reasons for hospitalization and mortality. The remaining causes are grouped in the category "Other". All outcomes are measured within the first year from birth. On the vertical axis, we display the causes for which the regressions are run. On the horizontal axis, we display the coefficients (and 95% CI) from regressions of outcome variables on a maternal dismissal indicator for exposure during pregnancy. All regressions include fixed effects for municipality of residence and month of conception. We control for infant's sex and mother-specific covariates - including mother's prior live birth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception.

Table 1: Summary statistics and differences in means

	(1) Non-dismissed group	(2) Dismissed group	Difference in means (2)-(1)
Panel A: Demographics and other variables			
Prior livebirth (≥ 1)	0.389	0.489	0.100***
	(0.541)	(0.500)	
20 y.o. or less	0.165	0.225	0.059***
	(0.423)	(0.424)	
21-35 y.o.	0.765	0.731	-0.034***
	(0.464)	(0.447)	
36 y.o. or more	0.070	0.044	-0.026***
	(0.286)	(0.206)	
White	0.317	0.256	-0.061***
	(0.898)	(0.453)	
Mixed	0.530	0.574	0.045***
	(1.131)	(0.522)	
Black	0.092	0.110	0.017**
	(0.367)	(0.302)	
Primary educ. or less	0.171	0.252	0.081***
	(0.418)	(0.454)	
Secondary educ.	0.726	0.684	-0.042***
	(0.480)	(0.478)	
Higher Education	0.084	0.035	-0.049***
	(0.328)	(0.187)	
Blue collar	0.039	0.040	0.001
	(0.290)	(0.199)	
Weekly hours	42.382	42.468	0.086
	(5.948)	(4.932)	
Monthly wage (R$ 2014)	1,043.719	918.678	-125.041***
	(1,260.725)	(689.082)	
Tenure at conception (months)	16.872	13.260	-3.612***
	(25.165)	(16.295)	
Panel B: Work absences			
Any reason:			
6-3 months to conception	0.034	0.030	-0.003
	(0.280)	(0.186)	
within 3 months to conception	0.025	0.021	-0.003
	(0.226)	(0.148)	
Unpaid leave:			
6-3 months to conception	0.002	0.002	0.000
	(0.051)	(0.045)	
within 3 months to conception	0.001	0.001	-0.001
	(0.038)	(0.026)	
Sick leave (work-unrelated):			
6-3 months to conception	0.030	0.029	-0.001
	(0.260)	(0.179)	
within 3 months to conception	0.022	0.021	-0.002
	(0.211)	(0.146)	
Observations	44,006	1,452	

Note: This table reports the pregnancy-specific, demographic and employment information average characteristics (and standard deviations) at the first observed birth in our sample of single mothers, as well as the differences in means, with associated statistical significance, between the non-dismissed (control) and dismissed (treatment) group. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Effect of maternal dismissals on birthweight.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) Dep.Var.: *Birthweight* (g)							
Dismissed	-31.346**	-39.079***	-40.882***	-38.001**	-39.010***	-39.827***	-34.787**
	(14.808)	(14.667)	(14.683)	(14.814)	(14.657)	(14.822)	(16.458)
Non-dismissed mean	3,150.485	3,150.485	3,150.920	3,150.399	3,150.493	3,150.741	3,150.273
(b) Dep.Var.: *Low birthweight*							
Dismissed	0.020**	0.022***	0.023***	0.023***	0.022***	0.023***	0.021**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Non-dismissed mean	0.082	0.082	0.082	0.082	0.082	0.082	0.082
Municipality FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
Covariates		✓	✓	✓	✓	✓	✓
Hospital FE			✓			✓	✓
Firm municipality FE				✓		✓	✓
Sector FE					✓	✓	✓
Municipality × Month FE							✓
Observations	45,458	45,458	45,375	45,316	45,457	45,221	40,423

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. All regressions include fixed effects for municipality of residence and month of conception. Covariates include newborn's sex and mother-specific covariates - i.e., mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of maternal dismissals on other birth-related outcomes.

		Gestation		Additional birth outcomes		
	(1)	(2)	(3)	(4)	(5)	(6)
	Female	*Length* (days)	*Preterm*	*Emergency C-section*	*Low 1-min Apgar*	*Low 5-min Apgar*
Dismissed	0.020	0.828*	0.011	0.001	0.005	0.003
	(0.013)	(0.441)	(0.008)	(0.011)	(0.007)	(0.003)
Non-dismissed mean	0.485	269.665	0.101	0.217	0.064	0.009
Observations	45,458	45,458	45,458	45,458	45,458	45,458

Note: The analysis is based on births over the period between 2011 and 2014. *Female* indicates female newborns. *Length* is reported in days. *Preterm* indicates gestational length shorter than 37 weeks. *Emergency C-sections* indicates C-sections happening after labor began. *Low 1-min Apgar* and *Low 5-min Apgar* indicate newborns with 1-minute and 5-minute Apgar scores below 7, respectively. All regressions include fixed effects for municipality of residence and month of conception. We control for newborn's sex (Columns (2)-(6)) and mother-specific covariates - including mother's prior live birth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of maternal dismissals on prenatal visits and delivery choices.

	(1) *Prenatal visits*	(2) *Fewer than 6 visits*	(3) *Private clinic*	(4) *Planned C-section*
Dismissed	-0.530**	0.144***	-0.077***	-0.071***
	(0.259)	(0.012)	(0.008)	(0.011)
Non-dismissed mean	8.405	0.133	0.219	0.334
Observations	45,458	45,458	45,458	45,458

Note: The analysis is based on births over the period between 2011 and 2014. *Prenatal visits* measures the number of prenatal visits attended by the mother. *Fewer than 6 visits* indicates whether there have been less than 6 prenatal visits. *Private clinic* indicates deliveries in private clinics. *Planned C-section* indicates deliveries via C-section before labor. All regressions include fixed effects for municipality of residence and month of conception. We control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of maternal dismissals on hospitalization and mortality.

	Hospitalization					
	(1) *Probability*	(2) *Episodes*	(3) *Days*	(4) *ICU*	(5) *Cost* (log)	(6) *Infant death*
Dismissed	0.032***	0.036**	0.469	0.002	0.356***	0.005*
	(0.012)	(0.016)	(0.299)	(0.005)	(0.135)	(0.003)
Non-dismissed mean	0.100	0.125	1.035	0.021	R$ 309.80	0.004
Observations	29,068	29,068	29,068	29,068	29,068	45,458

Note: The analysis is based on births over the period between 2011 and 2014. All outcomes are measured within the first year from birth. *Probability* indicates whether the infant has ever been hospitalized. *Episodes* are the total number of hospitalizations. *Days* are the total number of days in hospital. *ICU* indicates whether the infant used ICU. *Cost* is the logarithm of the cost of the hospitalization. *Infant death* indicates whether the infant has died. All regressions include fixed effects for municipality of residence and month of conception. We control for infant's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of maternal layoff on couples' newborns

	(1) *Birthweight*	(2) *Low* birthweight	(3) *Female*	(4) *Gestation length* (days)	(5) *Preterm*	(6) *Emergency C-section*	(7) *Low 1-min Apgar*	(8) *Low 5-min Apgar*
Dismissed	16.270	0.000	-0.040***	1.399***	-0.011	0.008	0.002	0.003
	(13.136)	(0.007)	(0.013)	(0.359)	(0.007)	(0.011)	(0.006)	(0.003)
Non-dismissed mean	3159.513	0.075	0.487	268.594	0.094	0.253	0.052	0.007
Observations	84,155	84,155	84,155	84,155	84,155	84,155	84,155	84,155

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. *Female* indicates female newborns. *Gestation length* is reported in days. *Preterm* indicates gestational length shorter than 37 weeks. *Emergency C-sections* indicates C-sections happening after labor began. *Low 1-min Apgar* and *Low 5-min Apgar* indicate newborns with 1-minute and 5-minute Apgar scores below 7, respectively. All regressions include fixed effects for municipality of residence and month of conception. We control for newborn's sex, where possible, and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of UI eligibility on birthweight.

	(1)	(2)	(3)
(a) Dep.Var.: *Birthweight*			
UI Eligibility	63.670**	114.921***	110.999**
	(31.690)	(43.745)	(46.774)
UI Eligibility×Wage		-0.060*	-0.059*
		(0.032)	(0.033)
Baseline mean	3,102.114	3,102.114	3,102.250
(b) Dep.Var.: *Low birthweight*			
UI Eligibility	-0.023	-0.040*	-0.044*
	(0.018)	(0.024)	(0.026)
UI Eligibility×Wage		0.000	0.000
		(0.000)	(0.000)
Baseline mean	0.097	0.097	0.098
No threshold individuals			✓
Observations	2,821	2,821	2,612

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. The sample includes only dismissed-mother children. Column (1) reports the coefficients from regressions with the sole dummy for UI eligibility (i.e., $Tenure_{it} \geq 6$ months) as variable of interest; Column (2) reports the coefficients from regressions with both a dummy for UI eligibility and its interaction with monthly wage; Column (3) is the same as (2) but the sample excludes all mothers at the cutoff. All regressions include a cubic polynomial in distance from 6-month UI eligibility cutoff (in months), fixed effects for municipality of residence and month of conception. We control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

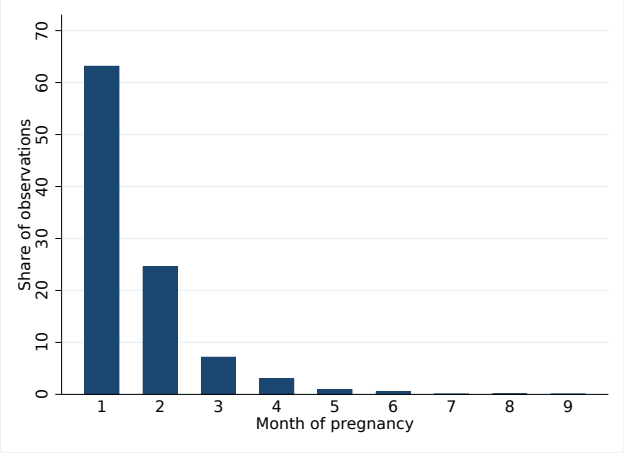


Figure A1: Distribution of observations by month of pregnancy of occurrence of layoff announcement.

Note: This figure shows the distribution of dismissed mothers by the timing of layoff announcement across months of pregnancy. The sample consists of dismissed pregnant workers.

Table A1: Effect of maternal layoff on birthweight: heterogeneous effects

	Pregnancy characteristics				Maternal characteristics									
	(1) Male-born	(2) Female-born	(3) ≥37 wks	(4) <37 wks	(5) Prim. Educ.	(6) Second. Educ.	(7) Higher Educ.	(8) ≤20y.o.	(9) 21-35y.o.	(10) 36+y.o.	(11) White	(12) Non-white	(13) White collar	(14) Blue collar
(a) Dep.Var.: *Birthweight* (g)														
Dismissed	-44.491**	-32.020	-32.578**	-2.638	-6.428	-54.544***	50.444	-61.888**	-33.885*	52.116	-12.486	-48.756***	-36.832**	-133.481
	(21.396)	(20.927)	(13.592)	(60.375)	(30.727)	(17.014)	(65.906)	(27.754)	(18.360)	(72.394)	(26.871)	(17.175)	(14.809)	(85.994)
Non-dismissed mean	3,205.601	3,091.251	3,224.413	2,493.323	3,103.869	3,162.357	3,124.908	3,166.331	3,148.566	3,127.458	3,147.836	3,152.120	3,150.921	3,144.652
(b) Dep.Var.: *Low birthweight*														
Dismissed	0.024**	0.022*	0.014**	0.045	0.019	0.026***	-0.034	0.002	0.027***	-0.033	0.008	0.027***	0.023***	0.012
	(0.011)	(0.012)	(0.006)	(0.043)	(0.018)	(0.009)	(0.039)	(0.016)	(0.010)	(0.035)	(0.015)	(0.009)	(0.008)	(0.039)
Non-dismissed mean	0.073	0.092	0.041	0.451	0.090	0.078	0.112	0.086	0.081	0.088	0.078	0.084	0.082	0.087
Observations	23,289	21,948	40,809	4,470	7,433	34,679	3,007	7,734	32,874	3,620	14,206	31,060	43,659	1,681

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. All regressions include fixed effects for municipality of residence and month of conception. Where possible, we control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Effects of maternal dismissals for single mothers' newborns - additional layoff timing restriction

	(1) *Birthweight*	(2) *Low* birthweight	(3) *Female*	(4) *Gestation length* (days)	(5) *Fewer than 6 visits*	(6) *Private clinic*	(7) *Planned C-section*	(8) *Hospitalization (probability)*	(9) *Hospitalization* (days)	(10) *ICU*	(11) *Hospitalization cost* (log)	(12) *Infant death*
Dismissed	-54.285***	0.023**	0.023	0.559	0.227***	-0.076***	-0.065***	0.043***	0.604	0.007	0.498***	0.006*
	(18.558)	(0.010)	(0.017)	(0.592)	(0.016)	(0.010)	(0.014)	(0.015)	(0.377)	(0.008)	(0.175)	(0.003)
Non-dismissed mean	3,150.485	0.082	0.485	269.665	0.133	0.219	0.334	0.100	1.035	0.021	R$ 390.80	0.004
Observations	44,899	44,899	44,899	44,899	44,899	44,899	44,899	28,705	28,705	28,705	28,705	44,899

Note: For the analysis in this table, we restrict the definition of layoffs such that dismissals happen strictly before a pregnancy can be confirmed during a pre-natal visit. The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. *Female* indicates female newborns. *Gestation length* is reported in days. *Prenatal visits* < 8 indicates whether there have been less than 6 prenatal visits (population average). *Private clinic* indicates deliveries in private clinics. *Planned C-section* indicates deliveries via C-section before labor. *Hospitalization (probability)* indicates whether the infant has ever been hospitalized. *Hospitalization (days)* are the total number of days in hospital. *ICU* indicates whether the infant used ICU. *Hospitalization cost (log)* is the logarithm of the cost of the hospitalization. *Infant death* indicates whether the infant has died. All regressions include fixed effects for municipality of residence and month of conception. Where possible, we control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Effects of maternal dismissals for single mothers' newborns - no mass-layoff firms

	(1) *Birthweight*	(2) *Low* birthweight	(3) *Female*	(4) *Gestation length* (days)	(5) *Fewer than 6 visits*	(6) *Private clinic*	(7) *Planned C-section*	(8) *Hospitalization (probability)*	(9) *Hospitalization* (days)	(10) *ICU*	(11) *Hospitalization cost* (log)	(12) *Infant death*
Dismissed	-36.433**	0.022**	0.015	1.039**	0.152***	-0.080***	-0.074***	0.032***	0.451	0.001	0.357**	0.004
	(15.646)	(0.009)	(0.014)	(0.470)	(0.012)	(0.009)	(0.012)	(0.012)	(0.308)	(0.006)	(0.139)	(0.003)
Non-dismissed mean	3150.539	0.082	0.485	269.666	0.133	0.219	0.334	0.100	1.036	0.021	R$ 309.94	0.004
Observations	45,127	45,127	45,127	45,127	45,127	45,127	45,127	28,860	28,860	28,860	28,860	45,127

Note: For the analysis in this table, we restrict the definition of treatment to dismissals outside of mass layoffs (number of displaced workers in a given year $< 75\%$ of total work force). The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. *Female* indicates female newborns. *Gestation length* is reported in days. *Prenatal visits* < 8 indicates whether there have been less than 6 prenatal visits (population average). *Private clinic* indicates deliveries in private clinics. *Planned C-section* indicates deliveries via C-section before labor. *Hospitalization (probability)* indicates whether the infant has ever been hospitalized. *Hospitalization (days)* are the total number of days in hospital. *ICU* indicates whether the infant used ICU. *Hospitalization cost (log)* is the logarithm of the cost of the hospitalization. *Infant death* indicates whether the infant has died. All regressions include fixed effects for municipality of residence and month of conception. Where possible, we control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, months of tenure at conception and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

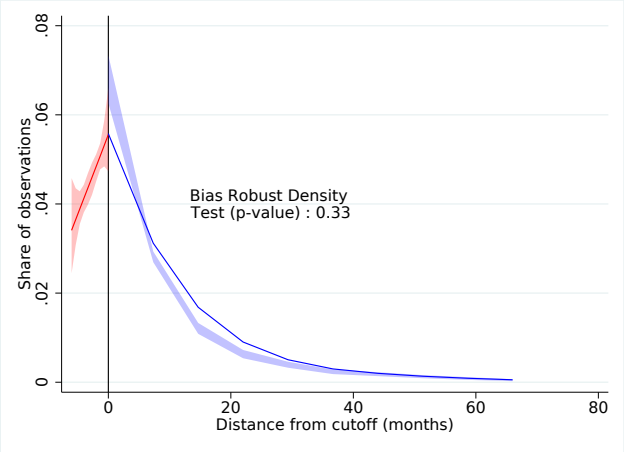
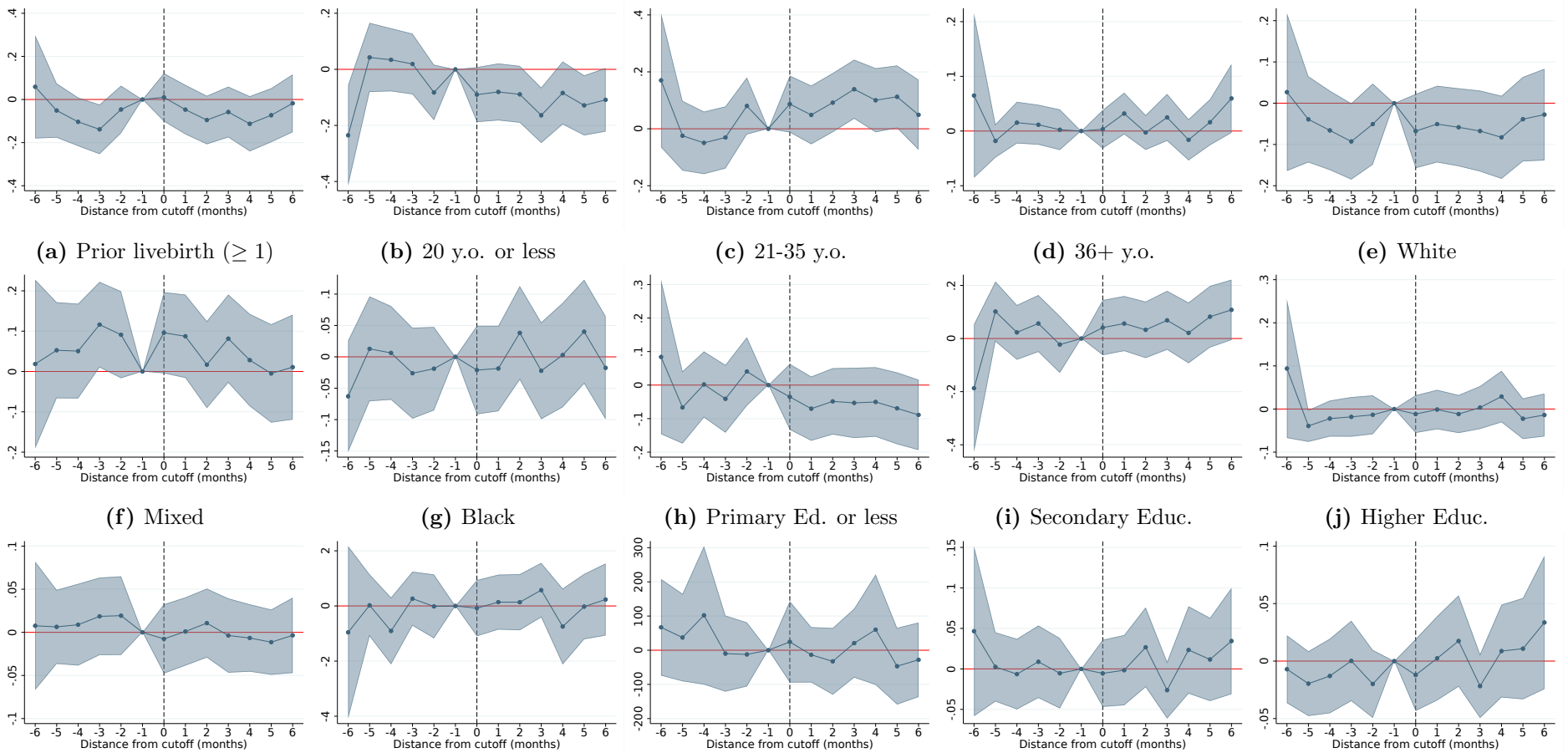


Figure A2: Distribution of observations around the UI eligibility cutoff.

Note: This figure shows the density of dismissal months around the cutoff month (6^{th} month of continuous employment) for entitlement for UI transfers. The sample consists of displaced pregnant workers. The results of the bias robust test proposed by Cattaneo et al. (2020) are also reported.



(a) Prior livebirth (≥ 1) **(b)** 20 y.o. or less **(c)** 21-35 y.o. **(d)** 36+ y.o. **(e)** White

(f) Mixed **(g)** Black **(h)** Primary Ed. or less **(i)** Secondary Educ. **(j)** Higher Educ.

(k) Blue collar **(l)** Weekly hours **(m)** Monthly wage **(n)** Work leave ($\geq$ 3m to conception) **(o)** Work leave ($<$ 3m to conception)

Figure A3: Balance of pre-determined covariates.

Note: The graphs show the balance of pre-determined covariates around the cutoff for UI eligibility for the sample of dismissed single mothers. Dots represent the point estimate of the difference between each bin (up to the sixth month from cutoff) and the cutoff. The shaded area indicates the corresponding 95% confidence interval.

Table A4: Effect of UI eligibility on birthweight (layoff announcement sample).

	(1)	(2)	(3)
(a) Dep.Var.: *Birthweight*			
UI Eligibility	45.193	103.347	98.126
	(53.001)	(64.637)	(67.805)
UI Eligibility×Wage		-0.068*	-0.076*
		(0.040)	(0.041)
Baseline mean	3,111.490	3,111.490	3,112.274
(b) Dep.Var.: *Low birthweight*			
UI Eligibility	-0.016	-0.032	-0.044
	(0.031)	(0.037)	(0.040)
UI Eligibility×Wage		0.000	0.000
		(0.000)	(0.000)
Baseline mean	0.093	0.093	0.094
No threshold individuals			✓
Observations	1,309	1,309	1,209

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* indicates newborns up to 2,500 grams. The sample includes only dismissed-mother children. Column (1) reports the coefficients from regressions with the sole dummy for UI eligibility (i.e., $Tenure_{it} \geq 6$ months) as variable of interest; Column (2) reports the coefficients from regressions with both a dummy for UI eligibility and its interaction with monthly wage; Column (3) is the same as (2) but the sample excludes all mothers at the cutoff. All regressions include a cubic polynomial in distance from 6-month UI eligibility cutoff (in months), fixed effects for municipality of residence and month of conception. We control for newborn's sex and mother-specific covariates - including mother's prior livebirth dummy, age dummies, ethnicity, education level, type of occupation, weekly hours, monthly wage and dummies for a work absence terminating 6 to 3 months and within 3 months prior to conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.