



Maternal Dismissals during Pregnancy and the Health of Infants*

Stefano Cellini[†]  Livia Menezes[‡]  Martin Foureaux Koppensteiner[§]

March 16, 2026

Abstract

We examine the consequences of maternal job dismissals during pregnancy on infant health, healthcare utilization, and early-life hospitalization using linked administrative data from Brazil. Job loss during pregnancy leads to lower birthweight and higher low birthweight incidence for single-mother children, with null effects for children in couples. Exploiting a regression discontinuity around unemployment insurance eligibility thresholds, unemployment benefits significantly mitigate these adverse outcomes, particularly for lower-wage mothers. Dismissed mothers also had fewer prenatal visits, reduced private healthcare use, and were less likely to undergo a planned C-section. Maternal dismissals increased infant hospitalization risk in the first year of life.

JEL Codes: I12, I14, J63, J65

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

*We would like to thank Andrew Barr, Thomas Crossley, Janet Currie, Ben Elsner, Eric French, Jason Lindo, Sandra McNally, Matthias Parey, Nirman Saha, Miriam Wüst and seminar participants at the NBER Summer Institute 2024, Birkbeck, Bristol, Nottingham, Reading, Texas A&M, York, RES 2023, the Essen Health Conference 2022, ESPE 2022, EEA 2022, EALE 2022, and AIEL 2022 for their comments. We are grateful to the Labor Secretariat of the Brazilian Ministry of the Economy and the Health Secretariat for help with accessing the data. This research used the Eureka High Performance Computing facilities at the University of Surrey. The usual disclaimer applies.

[†]Netherlands Interdisciplinary Demographic Institute, Lange Houtstraat 19, NL-2511 CV, The Hague, The Netherlands. Email: cellini@nidi.nl

[‡]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 among the most significant economic shocks individuals may face in their lifetimes. The health-related consequences of job loss on the individual are well documented in the literature, ranging from the participation in risky behaviors and unhealthy lifestyles to worsening mental and physical well-being (Black et al., 2015; Schaller and Stevens, 2015; Michaud et al., 2016). These effects result in substantial mortality risks for workers (Sullivan and von Wachter, 2009; Browning and Heinesen, 2012), with negative spillovers affecting cohabiting household members, such as partners and children, who experience financial and mental distress from the event (Marcus, 2013; Bubonya et al., 2017). Despite mounting evidence regarding the adverse effects of involuntary parental job loss during childhood on human capital formation (Oreopoulos et al., 2008; Carneiro et al., 2023), there is limited and ambiguous evidence on 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).

Previous research has investigated the broad theme of unemployment cycles and birth outcomes in various settings. Evidence from the United States suggests a countercyclical relationship between unemployment and newborn health (Dehejia and Lleras-Muney, 2004). In contrast, evidence from Argentina indicates procyclicality (Bozzoli and Quintana-Domeque, 2014), with mixed findings in European countries. For example, van den Berg et al. (2020) document a countercyclical relationship for Sweden, while Olafsson (2016) demonstrates a procyclical relationship for Iceland. However, studies using aggregate shocks or population-level indicators often face challenges in interpreting the findings and disentangling the specific mechanisms through which economic conditions affect infant health.¹

Evidence from individual-level data and variation is scarce and often constrained by data limitations. Focusing on maternal employment status during pregnancy rather than dismissals, Wüst (2015) finds that Danish mothers are more likely to have preterm deliveries when unemployed during pregnancy, attributing the adverse effects to stress from unemployment. Using survey data from the US and the UK, Del Bono et al. (2012) find positive effects of maternal work interruptions on birthweight up to three months before birth, though the definition of job interruptions is very broad, including periods of maternity leave, making it difficult to isolate the effects of unemploy-

¹In some contexts, distressing news or disruptions to maternal services may partially explain the negative association, as in Carlson (2015) and De Cao et al. (2022). On the other hand, increased eligibility for Medicaid for mothers in the US during downturns may mediate the countercyclical relationship Menclova (2013).

ment shocks. Closest to our focus on parental dismissals is the work by Lindo (2011), who studies the consequences of paternal job loss on birthweight in the US. The study reports a reduction in birthweight (-142 g, approximately -4.5%) for babies born in the same year as a paternal layoff. It also finds that these dismissals lead to a significant decrease in pre-birth food expenditures, suggesting a deterioration in prenatal nutrition as the main driver of the reduction of birthweight.

The analysis is primarily focused on the population of working *single* mothers, which allows us to isolate the effect of a maternal dismissal on newborn’s health in households where no partner can compensate for the income loss. Information on partner income is not readily available, as paternal identifiers are often missing from the birth records. For completeness, we also provide estimates for dismissed mothers in stable relationships to complement the estimates for single mothers, despite some uncertainty surrounding partner identity. Nonetheless, our results provide insight into the rapidly growing population of single mothers, which represents a significant share of births in Brazil (40% in 2022) and, more broadly, in Latin America (24%) (Crabtree and Kluch, 2020).²

In this paper, we estimate the effect of maternal dismissals during pregnancy on single mothers who are dismissed *without just cause* very early in pregnancy, before pregnancy is confirmed through a medical examination.³ We contrast the health outcomes of their children with those of children whose mothers remain formally employed throughout pregnancy, controlling for an extensive set of fixed-effects and individual-level characteristics, and applying strict conditions on the definition of treatment and the composition of the sample. We estimate the effect of dismissals during pregnancy on several measures of newborn health at birth, prenatal healthcare utilization of the expectant mother, and children’s health outcomes within their first year of life. The analysis leverages population-level employer-employee data linked with administrative health data from the Brazilian state of Minas Gerais linking individual, formal maternal employment spells with birth records, child hospitalization, and mortality records.⁴

We find that children of dismissed single mothers weigh between 31 and 41 grams less (a reduction of -1.0% to -1.3% of mean birthweight), and are 24% to 28% more likely to be classified as low birthweight ($\leq 2,500$ grams), indicating significant reductions in the birthweights of affected

²This is comparable to the stable share of births to unmarried women of 40% in the US and 51% in the UK (Osterman et al., 2024; ONS, 2024).

³Mean gestational age at pregnancy awareness is about six weeks for American women, with a substantial fraction – one third – of women being aware of their pregnancy much later (Branum and Ahrens, 2017), hence many women in our sample will in all likelihood not even be aware of their pregnancy when they are dismissed.

⁴Minas Gerais, while one of the more prosperous states in the South-East of Brazil, is also very heterogeneous economically, with a vibrant manufacturing and service sector in urban areas and the state capital, but also including rural poor areas in the north of the state, resembling the socioeconomic conditions in relatively poorer states in Brazil and hence makes for an excellent exemplar studying Brazil overall.

children, particularly at the lower end of the birthweight distribution. We also document substantial heterogeneity in the effects on low birthweight along the maternal wage distribution, with much more pronounced effects for mothers earning less than the median wage. In addition, we provide evidence on the short-term mitigating role of formal insurance in the form of unemployment benefits. To do so, we study the tenure-based unemployment insurance (UI) scheme, which provides income support for dismissed workers, exploiting the discontinuity in the eligibility rule of the UI scheme using a regression discontinuity design. We find that UI increases birthweight by approximately 2%, counteracting the negative effect of dismissals for displaced single women, effectively providing insurance against intergenerational risk from dismissal.

Beyond measures of health at birth, we also find effects on prenatal and delivery-related health-care utilization: dismissed mothers tend to have fewer prenatal visits, are 25% less likely to deliver in private clinics, and are 21% less likely to deliver through a planned C-section.

Furthermore, since the effects of in utero exposure to dismissals may not be fully captured by measures of health at birth, we investigate longer-term health effects for infants. Maternal dismissals during pregnancy are found to substantially increase the risk of hospitalization for children: fetal exposure to maternal dismissals increases the probability of hospitalization during the first year of life by 32% and the number of hospitalization episodes by 29%. Beyond hospitalization, we observe that the risk of infant mortality, a rare event, is also substantially increased. Detailed information on the causes, based on ICD-10 codes, reveals that *conditions originating in the perinatal period* drive the increase in hospitalization and infant mortality, indicating that dismissals affect these results from exposure during pregnancy rather than post-birth.

In relation to the existing literature, we make four major contributions. First, using uniquely suited data on the employment histories of single mothers and carefully addressing important identification challenges, we provide plausibly causal estimates of maternal dismissals on early-life human capital. To estimate the causal effect of dismissal during pregnancy, we exploit within-location and time variation, controlling for rich sets of municipality, hospital-of-delivery, firm-location, sector, and conception-time fixed effects, while carefully addressing selection and anticipation concerns. We complement this strategy by exploiting dismissals arising from mass layoffs, which are less likely to reflect individual-level selection.

Second, we explore the underlying mechanisms by which dismissals during pregnancy affect infant health, focusing on three candidates for the mechanism: in-utero selection, gestational length, and intrauterine growth retardation. We find no evidence of selection in utero, with the sex ratio

at birth being unaffected. We also rule out reduced gestational length, for example induced by maternal stress, being responsible for lower birthweight. In contrast, we document a positive effect of dismissals on gestational length, which may partially offset adverse effects on fetal growth and birthweight. The positive effect on gestational length highlights how opposing effects on gestation and fetal growth can generate ambiguous net effects on birthweight.

Third, using the detailed information from the linked employer-employee data, we provide novel insights into the mitigating effect of informal and formal insurance. Using information on wages, we find that the effects on low birthweight are largely driven by expectant mothers with a wage below the median, with no effect for females with income in the top quartile. Expanding on the mitigating effect of income, we study the effect of unemployment insurance. Taking advantage of the discontinuity in the unemployment insurance eligibility rule, we estimate the effect of formal insurance on the health outcomes of children of dismissed mothers. We find a positive effect of unemployment insurance that is larger in absolute value than the negative local dismissal effect, hence effectively overcompensating for the dismissal for this group.

Fourth, we extend the analysis of parental dismissals on infant health across two dimensions, namely by examining prenatal healthcare utilization choices, typically considered important inputs for infant health, and, beyond measures of health at birth, by extending the outcomes to include hospitalization and mortality records during the first year of life. These dimensions allow us to investigate the effects of dismissals on health margins, which previous studies have not explored.

The remainder of this paper is organized as follows. The following section provides context on the institutions of the Brazilian labor market and reproductive healthcare settings. Section 3 describes the data sources and describes the construction of the dataset. Section 4 presents the empirical strategy, followed by Sections 5 and 6 which present the empirical results and a discussion of these findings. Section 7 provides some concluding remarks.

2 Institutional context

This section provides background information on labor market features, employment regulation, institutional settings, and the reproductive health system in Brazil.

2.1 Female employment and labor regulation

In 2019, the female participation rate in the formal labor market in Brazil was estimated to be 55%, representing nearly 44% of the total labor force, while the female unemployment rate was approximately 14% (World Bank, 2021). Brazil’s labor market is characterized by a large informal sector, with almost two fifth of workers being informally employed in 2023 (ILO, 2025). While providing employment without formal labor market protection from dismissal or dismissal indemnities, the informal sector can act as a safety net by providing additional employment opportunities in the event of dismissal from formal employment (Ulyssea, 2018). Our estimates are therefore representative of expectant mothers only in the formal sector, as identified data on informal employment that can be matched to birth outcomes are not available.⁵

The vast majority (91%) of formal labor contracts in Brazil are open ended. Under Brazilian labor legislation, firms may dismiss workers without just cause — a form of at-will employment — but are required by law to pay dismissal indemnities to affected workers. Reflecting the prevalence of open-ended contracts, dismissals without just cause are the most common form of job separation (70% of all cases), followed by voluntary quits (29%). Dismissals for cause, defined as terminations resulting from serious misconduct or fundamental violations of the employment contract, account for only a very small fraction of separations.⁶ Employers are required to inform workers of their dismissal, adhering to a mandatory 30-day minimum advance notice period with an additional three days notice for every year of employment. That is, the dismissal must take effect no earlier than 30 days after the worker is informed about the dismissal decision by the employer.

To protect pregnant mothers from discrimination, the law renders dismissal without just cause void if the employer is informed of the pregnancy and the evidence is provided before the end of the notice period. This protection extends up to 5 months after delivery. As a result, dismissals of pregnant mothers are overwhelmingly expected to occur in the first trimester, when expectant mothers may not yet be aware or have medical confirmation of their pregnancy, a pattern confirmed by our data sample.⁷

Brazilian labor regulations provide unemployment insurance (UI) to assist dismissed workers financially. Unemployment benefits can only be claimed by employees dismissed without just cause and are paid for 3 to 5 months, depending on the length of employment in the 36 months prior to

⁵As workers in the informal sector do not benefit from employment protection, namely a notice period, dismissal indemnity pay, eligibility for UI, our results may present a lower bound for the effect on birth outcomes for dismissed mothers in the informal sector.

⁶These figures are based on 2011 statistics, but are representative of the entire period of interest in this paper.

⁷See Appendix Figure A1 for the distribution of dismissal announcements across months of pregnancy.

dismissal. Dismissed workers are entitled to UI payments for 3, 4, or 5 months for prior tenures of 6, 12, or 24 months, respectively.⁸ The average wage in the 3 months prior to layoff determines the replacement rate that eligible workers will receive, starting at 100% of previous earnings for workers earning the minimum wage.⁹

2.2 The Brazilian healthcare system and prenatal care

In Brazil, a nationwide public health system (SUS) provides universal coverage to all citizens, offering services free at the point of use. This universality is upheld through substantial financial investments from all levels of government. Services range from hospitalizations to emergency admissions and community care centers, operating throughout the country (do Carmo et al., 2020). Parallel to the public sector, there is a private supplementary healthcare system, where independent physicians provide medical services within privately owned hospitals and clinics. Approximately a quarter of the population is enrolled in some private health insurance scheme (Menezes-Filho and Politi, 2020)¹⁰

SUS recommends at least six prenatal care visits during pregnancy and provides, for free at the point of access, routine vaccinations, diagnostic laboratory tests, and supplements or medical treatment for complications. Data from Brazilian birth records show the evolution of prenatal care coverage in Brazil. In 1995, more than 10% pregnant women did not have a prenatal care visit; this number dropped to 2.2% in 2015 and has declined further since. Less than half of pregnant women attended seven or more prenatal care visits in 1995, but this increased to 66.5% in 2015, demonstrating the expansion of coverage and the importance of the Brazilian Unified Health System (SUS), created in 1990, in spreading these benefits.

3 Data and sample

Previous work in the empirical literature on the consequences of parental unemployment on newborns' health has used longitudinal survey data (Lindo, 2011; Del Bono et al., 2012) or a combination

⁸Additionally, 16 months must have passed between a worker's job separation date and the layoff date of their last UI claim. These rules were in place up until 2015, before the end of our period of interest.

⁹Employers are also required to provide a Severance Savings Account (Fundo de Garantia do Tempo de Serviço, FGTS) and severance pay to their employees.

¹⁰The majority of these schemes are funded through either individual enrollment in private health insurance plans, frequently employer-sponsored, comprising approximately 72% of all active plans in 2020. Frequently, SUS provision is complemented by directly purchasing services provided by private providers, such as tests, scans, dental treatment, etc. This includes, in some cases, delivery in private clinics. The loss of private health coverage due to dismissal may be one underlying channel, in particular with respect to the choices of delivery (Amorim et al., 2023). Due to the private and decentralized nature of private health insurance, unfortunately we cannot investigate the effect of losing employer-provided health insurance from dismissal.

of survey and administrative data (Wüst, 2015). Although survey data may include rich individual-level information not typically found in administrative data, there is often limited information on the causes of job separation. The source of variation in maternal employment status is frequently unspecified in survey data, making it difficult to distinguish between voluntary and involuntary job separations. Moreover, relying on survey data for birth outcomes means that birthweight information is parent-reported and therefore subject to recall error. Survey sample sizes tend to be small, limiting the analysis of relatively rare events such as dismissal during pregnancy. To overcome these limitations, estimating the causal effect of dismissals on birth outcomes thus requires data on recorded maternal employment spells with reasons for dismissals (to distinguish between voluntary job separation and involuntary dismissals) and on the timing of dismissals, such as from social security records linked to births from vital statistics data. In this paper, we use 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. In the next section, we briefly describe the four datasets.

3.1 Data sources

Employment records. Employment records come from the *Relação Anual de Informações Sociais* (RAIS, namely the Annual Report of Social Information), a linked employer-employee dataset that covers the universe of formal workers and firms in Brazil, made available by the Brazilian Ministry of Labor. RAIS identifies workers by both a unique tax code identifier (Cadastro de Pessoas Físicas (CPF), namely the Natural Persons Register) and their full name, enabling us to link workers to firms over time and to birth records. RAIS data includes detailed characteristics of workers' employment spells, such as start and end dates, job location, contract type, occupation and sector codes, worker education and earnings, as well as work absences, including their timing and causes. The data enable us to identify dismissals, their timing, and the recorded reasons for dismissal.

Birth records. The second dataset comes from birth records drawn from vital statistics data collected through the *Sistema de Informações sobre Nascidos Vivos* (SINASC, namely the Information System on Life Births), available for the years 2011–2014. These records are based on the universe of birth certificates issued in Brazil, whether from hospitals, birth clinics, or midwives after home deliveries, and are close to universally capturing all births during this period, regardless of whether delivery occurs in the public or private hospital care setting. This source provides key variables for

our analysis: conception date¹¹, birthweight, gestational length, babies' gender, and the mother's delivery history (i.e., the 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 Sistema de Informações sobre Mortalidade (SIM, namely Information System on Mortality). This dataset contains information on all natural and non-natural deaths in Brazil, including the precise cause of death and characteristics of the deceased. For deaths occurring up to the age of 1, mortality records are linked to birth records through a unique birth ID.

Hospitalization data. The fourth dataset comprises hospitalization records from the Sistema de Informações Hospitalares (SIH, namely the Information System on Hospitalizations), which contains details on all hospital admissions from referrals and self-referrals in the public healthcare system (SUS),¹² including information on duration, cost, and type of hospitalization, as well as the precise primary cause based on the WHO Classification of Diseases (ICD-10). Unlike linking employment, birth, and mortality records using personal identifiers, hospitalization records are instead linked based on residence address, sex, and date of birth of newborns. This restricts the number of observations available for this part of the analysis due to duplicate identifying variables, namely omitting observations with duplicate postcode, sex and date of birth information from the sample.

Auxiliary data. We supplement the SINASC data with information from the Cadastro Nacional de Estabelecimentos de Saúde (CNES, namely National Registry of Health Establishments). This annual census of hospitals and other health facilities collects information on the legal status of the facilities, such as ownership and the type of health services provided. We categorize hospitals as either *public* or *private*, defining hospitals as public if they are not explicitly categorized as private institutions.¹³

¹¹Conception date is calculated by adding 14 days to the date of the last menstruation, rather than inferred by subtracting gestational weeks from birth date, eliminating problems with endogeneous gestational length and minimizing measurement error, a major advantage of the SINASC data.

¹²In contrast to birth and mortality records, the hospitalization data we have access to only captures cases within the public healthcare system. Therefore, estimates of hospitalization outcomes may underestimate the true impact of dismissals on overall hospitalization risk.

¹³Private hospitals include both, for profit and non-profit facilities.

3.2 Sample definition

We follow the literature and use a sample of singleton births, excluding multiple births per pregnancy, and focus on mothers aged between 16—the legal minimum age for formal employment—and 50 at the time of birth. We also exclude observations where information on the mother’s identity or date of birth is missing, as well as duplicate observations.¹⁴ In the linked dataset of birth and job records, we retain only the first birth observed in the available years for each matched worker and ensure that mothers are economically active in the formal labor market with open-ended contracts in the private sector at the time of conception. The main analysis focuses on mothers who declared themselves as single regarding marital status (in contrast to being married or living in a stable relationship)¹⁵, excluding those with a history of stillbirths and any work absence for maternity reasons in the 6 months prior to conception, as well as any pregnant worker whose job was terminated for reasons other than *dismissal without just cause*. This results in a sample of 45,458 births from single working mothers during the period 2011–2014. For hospitalization outcomes, we have a sample of 29,068 births.

4 Empirical strategy

Our identification strategy exploits plausibly exogenous variation in maternal job dismissals occurring early in pregnancy, before pregnancy confirmation, among women continuously employed at conception. When estimating the effect of job separation on birth outcomes, there are three major obstacles to obtaining causal estimates. First, some characteristics of the local area in which job losses occur may be unobservable to the econometrician. If these characteristics are correlated with job losses and birth outcomes, relying on cross-sectional data will lead to spurious correlations. For example, some areas may be characterized by weaker labor markets, leading to higher dismissal rates and simultaneously affecting the quality of public services, including prenatal healthcare. In this case, one might erroneously conclude that maternal dismissals lead to worse health outcomes, resulting in failed inference.

Second, fertility responses to dismissal could generate selection bias if women adjust conception timing in response to job loss. If successful, this self-selection could introduce selection bias, as the

¹⁴Most cases with missing date of birth information (93%) pertain to the year 2011, the first year in which SINASC collected precise birth dates rather than just the age of the mother.

¹⁵To define marital status we make use of the information provided in SINASC defining *single*, as either *single*, *legally separated/divorced* or *widowed*.

underlying characteristics of dismissed mothers may differ from those of non-dismissed expectant mothers. We address this directly by limiting our treatment definition to dismissals occurring very early in pregnancy before medical confirmation of the pregnancy. In addition, by focusing on dismissals rather than job separations, we rule out cases where pregnant women may leave their employment based on private information about their health status or any concern with the pregnancy, a common problem in datasets lacking information on the causes of job separation. Furthermore, focusing on dismissals *without just cause* by the employer, we also exclude cases where the employer had a *just cause* to for dismissal, such as related to the worker’s conduct.

Third, when deciding which employees to make redundant, women may be selected for dismissal based on their characteristics. In line with objectives of the firm, employers are expected to select workers for redundancy by considering observable characteristics while avoiding claims of unfair dismissal. For example, to minimize the impact on productivity, firms may—everything else being equal—dismiss workers with shorter tenure and lower firm-specific human capital.¹⁶ 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 non-dismissed mothers, dismissed mothers, and the difference in means in columns (1) to (3). As expected, dismissed mothers are younger, have lower levels of education, a lower salary, and shorter tenure at conception.¹⁷ If these differences are unobserved and correlated with birth outcomes, they might lead to omitted variable bias. Access to administrative employment records, the same worker information accessible to employers, allows us to include these predetermined characteristics as controls in the regression.

Beyond these sociodemographic characteristics, one might be concerned about potential differences in health endowments of workers. If these are observable to the employer, for example through work absences, employers may select workers for dismissal based on ill-health. Worse health endowments of expectant mothers may also lead to worse health outcomes for their children, hence any correlation between maternal health status and treatment status would be concerning. However, directly addressing this concern, the employment records we have access to provide detailed information on work absences, including different causes and time periods before conception. In Panel B of Table 1, we find that dismissed and non-dismissed workers in our sample of pregnant mothers

¹⁶While there is no official employment protection based on seniority (no “last-in, first-out” rule as is common in the US or UK), the Brazilian labor market experiences structurally high turnover among younger workers (Corseuil et al., 2014). Therefore, tenure and age profiles between dismissed and non-dismissed workers are likely to be systematically different. The severance pay package that high-seniority, high-wage workers are entitled to also makes them less targeted for redundancies.

¹⁷Differences in ethnic composition, as apparent in the table, may be due to correlation with education rather than a direct result of selection based on ethnicity, which may constitute an illegal discriminatory practice.

show virtually identical levels of work absenteeism (for any reason), including unpaid leave and sick days for varying periods before conception. This allows us to rule out the possibility that dismissed workers are selected based on health issues leading to work absences. Conditional on these predetermined characteristics and fixed effects, we assume that remaining variation in dismissal timing is orthogonal to unobserved determinants of infant health.

To address the first identification challenge, we use the time dimension of the data and estimate OLS regression models including location and time fixed-effects, controlling for any location-specific differences that might, for example, simultaneously impact employment and the provision of maternal health services in the same location. We start with the municipality and vary the granularity of the location fixed-effects across different specifications. Including municipality and conception-time fixed effects ensures that identification comes from variation in dismissal timing across otherwise comparable mothers within the same location and conception cohort.

Second, to exclude the possibility of *endogenous* pregnancies (i.e., fertility decisions made in response to being dismissed) we restrict the sample of mothers to include only those already pregnant at the time of dismissal but too early in pregnancy to be aware of it or to have had medically examined confirmation. Because dismissals occur before pregnancy confirmation and before any pregnancy-related work absences, they are unlikely to reflect maternal health shocks or anticipatory fertility behavior. Formally, we denote child i 's announcement month of maternal dismissal with T , the month of occurrence of the first prenatal visit t_v , and the imputed last month of pregnancy as $t + 9$. To rule out selection into pregnancy, we exclude cases in our estimation sample where 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:

$$D = \begin{cases} 1, & \text{if } t \leq T \leq t_v \\ 0, & \text{if } T \in \{\emptyset\} \cup \{t + 10, t + 11, \dots\} \end{cases}, \quad (1)$$

for which child i is treated ($D = 1$) only if the mother experiences dismissal while pregnant but no later than the month of the first prenatal visit. This restriction ensures that our sample includes only pregnant women continuously employed at the point of dismissal.

Third, to address concerns about selection into dismissal by the employer, we include an extensive set of observable maternal and employment characteristics, allowing us to test for selection into treatment based on the observable characteristics of workers available to employers.

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

$$y_{ikt} = \tau D_i + \mathbf{X}'_i \boldsymbol{\beta} + \mathbf{H}'_i \boldsymbol{\Phi} + \nu_k + \theta_t + \epsilon_{ikt} \quad (2)$$

where y_{ikt} represents the outcome of interest for child i , born to a mother living in municipality k , conceived in month t . The indicator D_i is equal to 1 if i is exposed to a maternal dismissal in utero and 0 if otherwise. \mathbf{X}_i is a vector of maternal and employment characteristics, including the number of previous live births, maternal age category, ethnicity, education, occupation category (blue vs white collar), weekly hours and monthly wage, and 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 unobserved differences in local environments, such as in the provision of prenatal healthcare. θ_t denotes the 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 for the effect of maternal dismissals on the number of health outcomes at birth, including birthweight, low birthweight, gestational length, and additional health outcomes.

We further probe our estimates in three additional exercises. First, in addition to location and time fixed-effects, we expand our main specification to include additional fixed-effects, namely hospital, firm municipality, industry sector fixed-effects, and municipality-specific time trends.¹⁸

We also engage in an exercise to rule out the possibility that estimates are affected by anticipation effects, which may result in self-selection into pregnancy to protect against dismissal.¹⁹ This issue differs from selection into pregnancy in response to dismissal, as previously discussed. To investigate whether our results are sensitive to self-selection, we restrict the treatment assignment window so 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 dismissal occurs strictly before a pregnancy can be confirmed during a prenatal visit, and possibly any early pregnancy complications are known to

¹⁸Hospital fixed-effects account for any differences in prenatal care and delivery services for mothers living in the same municipality but giving birth in different hospitals and receiving any antenatal care from the hospital or health center linked to the hospital. These fixed-effects may also capture the fixed characteristics of the neighborhood through any hospital areas within the municipality. In rural municipalities, they account for the fact that delivery may take place in a 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 different municipality from that 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, such as prenatal environmental exposure, not captured by the occupation category dummy included in the individual controls.

¹⁹Brazilian labor laws protect pregnant women from being dismissed. Therefore, women threatened with job displacement could potentially choose pregnancy for protection against dismissal, as recently documented in Hungary (Weber et al., 2023).

the mother, which could lead to a correlation between unobserved pregnancy complications and treatment status.

To further address concerns about selection into dismissal, we exploit dismissals arising from mass layoffs as an alternative identification strategy. While commonly used in the literature to estimate the effects of job loss on earnings (Couch and Placzek, 2010; Flaaen et al., 2019), health (Eliason and Storrie, 2009; Sullivan and von Wachter, 2009) or subsequent fertility (Del Bono et al., 2012; Kristiina and Kellokumpu, 2016), the use of mass layoffs to estimate the effect on birth outcomes is challenging in our setting due to the need to combine mass layoffs with (early) pregnancies, reducing the number of treated individuals compared to other settings in the literature. For this exercise, we focus on mass layoffs where a firm dismisses more than 27 percent of its entire workforce over a period of one year. This threshold is data-driven: 27 percent corresponds to the median displacement rate across firms, weighted by the number of displaced workers, meaning that half of all displaced workers in our sample were employed at firms that dismissed 27 percent or more of their workforce in a given year.²⁰ Workers above this threshold are therefore more plausibly displaced due to firm-level shocks rather than individual-level selection; at the same time, the control group consists only of mothers employed in firms where the dismissal rate was less than 27 percent. To complement the exercise, addressing concerns about the potential for anticipation of mass layoffs recently raised by Weber et al. (2023), we also estimate the effect of *regular* dismissals and exclude workers employed at downsizing firms. If female workers are aware of their firms' economic situation and anticipate downsizing, expectant mothers may be successful in protecting themselves against dismissals and thus end up in the control group, leading to a different composition of expectant mothers selecting for fertility. To test whether anticipatory fertility responses to mass layoffs may affect our estimates, we therefore exclude from the sample any workers in firms undergoing mass layoffs, which we analogously define as firms dismissing more than 27% of the workforce in any given year in a separate exercise.

²⁰Results are robust to alternative cutoffs at the 25th and 75th percentile of the displacement rate distribution - a broad range spanning the bulk of the distribution. Estimates for healthcare utilization and hospitalization outcomes are stable across all cutoffs. Birthweight effects are negative throughout but vary in magnitude and precision, with larger effects at higher cutoffs, consistent with a stricter definition isolating more severe firm-level shocks.

5 Effects on birth outcomes

5.1 Main results

In Table 2, we present the effect of dismissals on birthweight, measured in grams, and on an indicator for low birthweight to investigate the effects on the lower end 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 during pregnancy leads to a reduction in birthweight of more than 30 grams. The magnitude of the estimated effect of dismissal is substantial, approximately twice the size of the (positive) effect of exposure to food stamps in the United States (Almond and Currie, 2011), indicating economically important negative effects on the health endowments of newborns. In column (2), we add an extensive set of controls for maternal, pregnancy, and job characteristics, including the set of observable characteristics that employers have access to and might consider when deciding which employees to dismiss, such as job tenure, wage, hours worked, occupation dummies, education, and information on work absences. The inclusion of these controls increases the reduction in birthweight to approximately 39 grams. This likely reflects some unobserved heterogeneity in the estimates in column (1), but suggests that employer selection into treatment does not drive the negative effect on birth.

Next, we test the robustness of the birthweight estimates by including a set of additional fixed-effects.²¹ In column (3), we include hospital fixed-effects, which control for the quality of delivery services and local prenatal and general health services related to the hospital. The birthweight coefficient remains virtually unchanged. In column (4), we include municipality-of-firm fixed-effects, which account for characteristics specific to the municipality where the firm is located in case the municipality of residence differs from the municipality where the firm is located. Again, the estimate remains very similar. In column (5), we probe the robustness of alternative fixed-effects further by including sector fixed-effects, which account for sector-specific, time-invariant characteristics. In column (6), we include these additional fixed-effects simultaneously. Neither of these specifications changes the estimates significantly, providing additional credibility to the identification strategy. Finally, in column (7), in addition to the full set of fixed-effects, we include municipality-specific time trends, which serve to slightly reduce the size of the negative effect to approximately -35 grams. However, the difference is not statistically significant. In panel (b), we report the effects on

²¹When including additional fixed-effects, a small number of singleton observations reduces the number of observations slightly. The greatest drop occurs when including municipality-specific trends in column (7), where we lose just over 10% of observations, mostly from small and medium municipalities (< 50,000 population).

low birthweight. We find that dismissals during pregnancy substantially increase the probability of low birthweight, by approximately 2.2 percentage points in our preferred specification (which includes the rich set of covariates), representing a 27% increase given the baseline incidence of 8.2%. This indicates that dismissals have a particularly strong negative effect on the lower end of the birthweight distribution. The coefficient remains extremely stable across all the different specifications, including those with various fixed-effects.

As outlined in the empirical strategy section, we next test for the presence of anticipation effects. To do so, we apply an even more stringent treatment assignment strategy to rule out the possibility that the effects are driven by self-selection into pregnancy as a way to protect against dismissal. We achieve this by strictly limiting the occurrence of dismissals to the period before the month of the first prenatal visit, thus reducing the treatment group to children whose mothers were dismissed before the pregnancy could be confirmed and documented by a medical professional. This restriction reduces the number of observations in the treated group by approximately 40%. We report the effects of this exercise in the Appendix Table A1; estimates include fixed-effects of the municipality of residence and months of conception, along with the full set of covariates. We find that the coefficient becomes more pronounced for birthweight (-54 grams, column (1)) and remains very similar in magnitude for low birthweight (0.023, column (2)) compared to the results without this additional restriction. We interpret this as evidence that self-selection into pregnancy in response to dismissal does not drive our effects.

Lastly, we provide estimates based using an alternative estimation strategy by focusing on mass layoffs. We report the coefficients for birthweight and low birthweight in columns (1) and (2) in Table A2. Using only dismissals occurring during mass layoffs we find remarkably similar results, despite the much smaller number of treated individuals in the mass layoff sample. We find a reduction of birthweight of 37 grams compared to 39 grams for our preferred specification in Table 2, with the coefficient being significant at the 10% of significance. The estimate of low birthweight is slightly smaller (0.016) and is not significant. The two sets of coefficients from our main estimates and the mass layoff specifications are not statistically significantly different from each other, lending extra credibility to our main estimates.

Lastly, addressing concerns regarding the potential anticipation of mass layoffs, to complement the previous exercise, we re-estimate the effects of regular dismissals and exclude 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)) remain virtually unchanged compared to

the corresponding coefficients in column (2) of Table 2, further ruling out that anticipation of mass layoff events affects our estimates.

5.2 Heterogeneous effects

To understand possible heterogeneity in the effects, we explore the effect of dismissals across group characteristics. We start the exercise by investigating heterogeneous effects by pregnancy characteristics, as shown in columns (10)-(14) in Appendix Table 3. The findings show slightly more pronounced negative effects for male fetuses, with a reduction of 44 grams compared to 32 grams for female fetuses. Furthermore, the effects on low birthweight for males are more pronounced, aligning with findings in the biomedical literature that indicate that male fetuses are more sensitive to maternal stressors in utero (Hansen et al., 1999). Next, we estimate the effect on birthweight separately by gestational length.²² Consistent with the observed positive effect of dismissals on gestation, we find that dismissals have a less pronounced effect on birthweight among children born prematurely and a more prominent effect on the indicator for low birthweight. However, these coefficients are not statistically significant due to the relatively small sample size.

We then investigate the effects by maternal characteristics in columns (5)-(14) focusing on predetermined worker characteristics such as education, age, race, and occupation. Although we find that the main estimated effects are driven by mothers with at most secondary education, we find a positive, though imprecisely estimated, effect on birthweight for children of mothers with higher education.²³ Additionally, we find that dismissals have a more pronounced effect for mothers younger than 21 years of age at the time of birth (-62 grams versus -34 grams for mothers aged between 21 and 35).²⁴ Next, we estimate the effects separately for *white* versus *non-white* mothers, finding a stark difference in the effects of -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, though insignificant, effect for mothers in blue collar jobs (-114 grams). Taken together, the results presented in Table 3 indicate pronounced heterogeneous effects along several relevant factors, including socioeconomic status.

²²This will allow us to investigate whether the effects on birthweight have a differential effect by gestational length, but the estimates need to be interpreted with caution, as gestational length is in itself an outcome of dismissals.

²³For this group, it is possible that the economic shock of dismissals may not be as severe, while the positive effect from longer gestation may contribute to a net positive effect on birthweight and low birthweight.

²⁴The effect for older mothers is positive (52 grams) though statistically insignificant given the small number of observations in this group. This may reflect that older mothers are better able to protect the shock while benefiting from longer gestation.

5.3 In-utero effects and mechanisms

There are several possible transmission mechanisms behind the effects of dismissals on birthweight and low birthweight through consequences on in-utero development. First, in-utero selection may lead to a culling effect of weaker fetuses, which could affect the sex composition of surviving newborns. Previous research has shown that male fetuses are at higher risk of miscarriage, particularly in response to a detrimental in-utero environment (Kraemer, 2000). Second, low birthweight may be a mechanical result of shorter gestational length. Third, low birthweight may be caused by intrauterine growth retardation, which can originate from maternal, placental, neonatal, genetic, and environmental factors, including maternal malnutrition (Murki and Sharma, 2014).

We start by directly testing the first two channels. In Table 4, we present the coefficients for the effect of dismissals on the sex ratio at birth, which serves as a proxy for sex-specific miscarriage risk,²⁵ gestation, and other birth outcomes (unplanned C-sections and Apgar scores). We find that dismissals have a very small and insignificant effect on a female dummy, indicating that in utero selection does not seem to drive the results on birthweight.

Next, we examine gestation and a dummy for preterm delivery (defined as gestational length fewer than 37 completed weeks). We find a small, marginally significant positive effect on gestational length, of approximately one day. This positive effect is consistent with findings in the literature on the positive association between early maternity leave during pregnancy and birth outcomes, as well as the negative association between physical and mental stress during pregnancy and gestational length (Corchero-Falc3n et al., 2023). The positive effect on gestation rules out shorter gestational length as the primary cause of the effects on birthweight and low birthweight. We also find a small, positive but insignificant effect on preterm delivery. These two findings are not contradictory - the continuous gestational length measure captures a modest upward shift in gestation across the full distribution, while the binary preterm indicator, defined at the 37-week threshold, is insufficiently sensitive to detect a shift of this magnitude at the lower tail. Taken together, the absence of a major role for in utero selection and short gestation thus suggests that the effects on birthweight may be primarily due to impediments to in utero growth. Other indicators of health at birth, as investigated in the table, including emergency C-sections and Apgar scores at 1 and 5 minutes, are small and statistically insignificant.

Given the possibility of a role for intrauterine growth retardation, which can be caused by poor

²⁵Information on miscarriage, particularly early in gestation, are notoriously unreliable. If an environmental shock during gestation favors a female fetus, this might affect mean birthweight since male newborns tend to have higher birthweights. Although we control for the sex of the newborn to account for any culling effect, we still test for its existence.

maternal nutrition during pregnancy, our objective is to explore whether the effects on birthweight vary by the position in the wage distribution of mothers. The heterogeneity of the effects along this distribution may reveal the importance of income shocks from dismissal on birth outcomes, possibly through its effect on maternal nutrition. To assess the role of income loss from dismissal, we estimate the effect of dismissals separately by wage quartiles. We present the coefficients in Figure 1. Except for the third quartile, we find a negative effect of similar magnitude across the income distribution. For the result of low birthweight, income appears to play an important role. We find the strongest positive effects on the indicator of low birthweight for the bottom wage quartiles and much smaller and insignificant effects for the third and top wage quartiles. For women on low income, dismissal increases the chance of low birthweight by more than 40% compared to the mean incidence. In contrast, the effect of dismissal has almost no effect for women in the top wage quartile, possibly indicating that income plays an informal insurance role in cases of job separation.²⁶

5.4 The role of unemployment insurance for health at birth

We expand the analysis on the mitigating role of income by investigating the effect of unemployment insurance in cases of dismissals. In Brazil, workers are eligible for 3 to 5 months of unemployment benefits when dismissed without just cause from a formal job, provided that they have been continuously employed for at least six months prior to dismissal.²⁷ The benefit level is a function of the average wage in the 3 months preceding dismissal, ranging from 100% to 187% of the minimum wage, therefore, high-earner workers are expected to receive relatively less compared to low-earner workers.

5.4.1 Regression Discontinuity Design

For this analysis, we focus on children of working mothers who held open-ended jobs in the private sector who were dismissed during pregnancy, rather than on the more narrow definition of treatment in (1).²⁸ To explore the role of unemployment insurance on birth outcomes of dismissed mothers,

²⁶Since the size of any severance pay available to eligible dismissed mothers is a direct function of wage, any mediating effect of wage on low birthweight may be due to a combination of personal savings and severance pay.

²⁷This condition does not strictly require that the employer must be the same over the previous 6 months, but we assign treatment based on continuous employment with the same firm. This stringent assumption may lead to an underestimation of the effect of eligibility for UI. Furthermore, laid-off workers must wait at least 16 months between the current dismissal 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 point from the primary eligibility criterion.

²⁸We do not narrow the sample for the RDD analysis to the treatment group mothers following the assignment rule in (1), namely we take into consideration mothers dismissed independently of the timing with respect to the first prenatal visit.

we compare the birth outcomes of working mothers who are eligible and ineligible for UI benefits after dismissal by estimating the following equation:

$$y_{ikt} = \lambda \text{UI}_{it} + f(\text{Tenure}_i) + \mathbf{X}'_i \boldsymbol{\Gamma} + \mathbf{H}'_i \boldsymbol{\Delta} + \nu_k + \theta_t + \epsilon_{ikt}, \quad (3)$$

where y_{ikt} is the birth outcome of interest for child i (birthweight or low birthweight), to a mother living in municipality k and conceived at time t ; Tenure_i is the running variable of the regression discontinuity (RD) design, representing the months of continuous employment before dismissal, standardized so that $\text{Tenure} = 0$ at the cut-off required for eligibility (i.e. 6 months); and $f(\cdot)$ is a flexible polynomial regression.²⁹ UI_{it} is a dummy taking the value of 1 for workers who are eligible for UI (i.e. $\text{UI} = \mathbb{1}(\text{Tenure}_i \geq 0)$). We include several covariates, \mathbf{X}_i , to enhance the precision of the regression discontinuity estimates, along with the usual set of fixed-effects as in the specification of the main analysis. Any latent health factors are subsumed into \mathbf{H}_i and proxied by preconception absences from work.

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

One concern with RD estimates is the possible manipulation of the running variable. In the present case, this can arise since six months of job tenure may coincide with employee performance evaluations in the form of probation periods (Gerard and Gonzaga, 2021). A key assumption for the validity of the 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. Thus, a jump in the density of the running variable at the cut-off 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, as also confirmed by the bias-robust test with local polynomial density estimators (Cattaneo et al., 2020). In Appendix Figure A3, we provide further balancing tests for

However, we also present results based on the same sample in Table A4 with similar conclusions.

²⁹We estimated specifications with quadratic and cubic polynomial terms. The results do not differ significantly.

the range of covariates among workers' characteristics. Overall, these checks support the assumption of no manipulation of the running variable and no discontinuity in maternal characteristics on either side of the UI eligibility cutoff.

5.4.2 RD Results

The estimates from our RD design are reported in Table 5, showing the results of alternative regression specifications for both birthweight and low birthweight. The first column shows the effect of UI eligibility on the birth outcomes, controlling for newborn sex and maternal covariates, as well as including the usual set of fixed-effects (municipality of residence and month of conception). Column (1) presents an average effect of UI on birthweight of approximately 64 grams (significant at the 5% level) and a decrease in low birthweight by approximately 24%, though imprecisely estimated.³⁰ 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. 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.

To identify any remaining concerns regarding manipulation and intentional misreporting of job tenure, we re-estimate the effect of UI on a sample that omits mothers with 6 months of continuous employment at di in column (3): the results for both birthweight and low birthweight do not differ from those in column (2) in any meaningful way.

Together with the effects by income quartiles, these results suggest that liquidity constraints may be an important driver of the effects of dismissals on the health of single mothers' infants. With diminished financial resources to draw on after dismissal and more pronounced effects for the most financially vulnerable mothers, dismissals may affect the short and long-term health of children by restricting the resources available to single mothers during pregnancy, such as by impacting maternal nutrition, which is a contributing factor to intrauterine growth retardation. Unemployment insurance may be an effective way to protect mothers from the negative income shock of dismissals, thereby reducing the adverse effects on the health of their infants. Providing income support to dismissed pregnant single-mother households after displacement can mitigate the negative consequences and possibly more than counteract the negative effect on birthweight, likely

³⁰The effects are similar in magnitude to the positive effects on birthweight found for conditional cash transfers in Uruguay (Amarante et al., 2016).

benefiting from the positive effect of dismissal on gestation while protecting against the income shock.

5.5 Birth outcomes of non-single women

Although not the focus of this article due to the limited information on spouses and the potential for more complex joint household decisions, we provide the main outcomes for mothers in a relationship who lose their job during pregnancy for completeness.³¹ We present the results in Table 6. In contrast to single mothers, we find that dismissals have a positive but insignificant effect on birthweight, with no effect on low birthweight. In line with the findings for single mothers, we also find a positive effect on gestational length, with the effect being significantly more pronounced than for single mothers (approximately one and a half days longer). The results are consistent with the positive effect of prolonged gestation that overcompensates for any potential negative effect on growth in utero. This may occur because partners' income can help absorb the negative shock on income and/or provide mental support after dismissal. We find a moderate effect on the sex ratio and hence cannot rule out selection in utero, possibly—jointly with the positive effect on gestational length—leading to a positive effect of dismissal on birthweight.

6 Effects on healthcare utilization and longer-term outcomes

6.1 Effects on prenatal visits and mode of delivery

Dismissals may also affect the utilization of health services through their economic impact on household income. Brazil's health system is characterized by a well-functioning public prenatal care system, generally achieving a relatively high level of prenatal care. Most expecting mothers attend eight or more prenatal care visits, well above the minimum of four antenatal visits recommended by the WHO and above the minimum of six visits recommended by Brazilian health authorities. Prenatal visits are important for detecting underlying health issues during pregnancy, such as pre-eclampsia. Furthermore, meeting the minimum recommended number of visits has been shown to positively impact birthweight and decrease the propensity for premature birth (Gajate-Garrido, 2013).³² The quantity and quality of prenatal care may, in turn, affect birth outcomes. In Table 7,

³¹For example, in contrast to single mothers for whom we observe income, employment status, and more, for all mothers, due to limitations with personal identifiers, we can only link a fraction of fathers to employment records. This means we cannot engage in the same exercises as for single women, such as heterogeneous effects by income.

³²However, some evidence suggests that the timing of and meeting the minimum number of prenatal visits is more important than the overall number of visits, particularly beyond the recommended minimum (McDuffie et al., 1996).

we present the estimates for prenatal visits (column (1)) and an indicator for fewer than six visits (column (2)).³³ Dismissals slightly reduce the number of prenatal visits by approximately half a visit on average. Compared to the mean number of visits in the group of women without dismissals, this represents a relatively moderate reduction of 6%. More importantly, we find an increase (0.144) in the propensity to have fewer than the recommended six prenatal visits. Compared to the mean propensity (0.133), this amounts to more than doubling the fraction of women who have fewer than six prenatal visits. Together with the small reduction in the mean prenatal visits, this suggests an unequal impact of dismissals along the distribution of prenatal visits, with women who already have fewer visits than the mean being more affected. The reduction in prenatal visits could be due to the stress caused by the dismissal or the financial impact of the dismissal.³⁴

We further investigate any effect on healthcare utilization by applying information on private healthcare utilization for delivery. In column (3), we provide the coefficient for delivery in private clinics and document a substantial reduction in the fraction of deliveries in private clinics in response to dismissals. Compared to the mean (0.334), dismissals reduce the fraction of deliveries utilizing private clinics by 25%. 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 paid delivery in a private clinic due to the negative income shock. In line with this, we also find a substantial drop in planned C-sections (column (4)) by 21%, corresponding to the drop in delivery in private hospitals. The reduction in elective C-sections may also contribute to the slightly longer gestational length among dismissed mothers. Reassuringly, the overall results on prenatal healthcare utilization and delivery choices outcomes are in the same direction across alternative treatment and control group specifications, as shown in Tables A1, A2 and A3 (columns (5)-(8)).

6.2 Longer-term effects on infant health

Beyond measures of health at birth, we investigate whether in-utero exposure to maternal dismissals has lasting consequences for children’s health in the first year of life. We investigate the effect of maternal dismissals on longer-term health outcomes of children with two measures for 1 year after birth: hospitalization of children and infant mortality. In Table 8, we first present coefficients for

³³In columns (5)–(8) in Tables A1 and A2 in the appendix, we provide the same additional robustness checks as for the main outcomes of birthweight and low birthweight.

³⁴The information on prenatal visits is collected alongside other birth record information and hence reports the total number of prenatal visits irrespective of whether these occurred in the public or private health sector. Therefore, switching from private to public health care does not affect our ability to observe prenatal visits.

several different measures of health using data from hospitalizations, followed by estimates for infant mortality. In contrast to most in utero shocks studied in the literature, which have a transitory nature, the shock we examine in this paper may have an absorbing state that extends beyond the in utero period. This has important implications for the interpretation of the effects on longer-term outcomes. For example, longer-term outcomes, such as hospitalization in the first year after birth, cannot be exclusively attributed to a transitory shock during pregnancy. This means that any effects on outcomes measured after birth could arise from 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. Specifically, we document a 3.2 percentage point increase, which represents a 32% increase compared to the non-dismissed mean and is significant at the 1% level. Newborns exposed to maternal dismissal in utero also show an increase in the number of hospitalization episodes (+29%), although these two outcomes are likely to be strongly correlated. The effect on days hospitalized is positive but imprecisely estimated, and we do not find an effect on the severity of hospitalizations, as proxied by admission to the ICU. Unsurprisingly, given the increase in hospitalizations, we also find a significant increase in the (log) cost from hospitalization. Taken together, these results indicate that dismissals have a substantial negative effect on the health of children in the first year after birth, extending beyond conventional measures of health at birth, such as birthweight.

To rule out the possibility that the results are driven by mothers switching from private to public hospitals rather than an increase in ill health leading to hospitalization, as we do not observe hospitalization in private hospitals in the data, we conduct an additional exercise. In Table A5, we reestimate the effects on hospitalization outcomes, conditional on having delivered in a public hospital. We would expect mothers who deliver in a public hospital to be less likely to switch from a public hospital to a private hospital. We find a relatively moderate reduction in the magnitudes of between 17% and 19% across the hospitalization effects when conditioning on delivery in a public hospital. We interpret this as evidence that dismissals lead to an increase in hospitalizations, although we do not rule out that part of the effect arises from a switch from private to public healthcare settings.

Lastly, we estimate the effect of dismissal on infant mortality. We find that dismissals substantially increase mortality during the first year of life, more than doubling the chance of infant death

compared to the mean of the control group. This finding is consistent with the hospitalization results, although the coefficient is only marginally significant and the estimates of mortality should be interpreted with caution.³⁵

To further investigate the timing of hospitalizations and infant mortality, we estimate separately the effect of dismissals within 7 days from birth (equivalent to the early neonatal period), between 8 and 28 days, and between 4 weeks and 1 year, reporting the coefficient estimates in Figure 2. We find that dismissals have a significant positive effect (in the graph, we display the percent effect compared to the baseline), increasing the risk of hospitalization by more than 30% in the first week after birth, during which hospitalizations of newborns are the most frequent. For the subsequent 3 weeks, the coefficient is even more pronounced, increasing the risk of hospitalization by more than 100%. However, 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 differ slightly, with effects that are largely insignificant for the first month and a pronounced effect for the period after the first 4 weeks after birth, increasing the chances of infant death by more than 400% for infants from dismissed mothers. This pattern is consistent with a lag between ill health, hospitalization, and infant death.

To provide insights into the underlying mechanism of the effect on hospitalization, we estimate the effect of dismissals separately by cause of hospitalization and mortality, showing the results in Figure 3.³⁶ We find that hospitalizations are driven by *Certain conditions originating in the perinatal period*. These include, among others, *Disorders of newborn related to gestational length and fetal growth*, *Respiratory and cardiovascular disorders in the perinatal period*, and *Newborn affected by maternal factors and complications of pregnancy, labor and delivery*. The coefficients of other causes are relatively small, except for *diseases of the respiratory system*, though statistically insignificant. Estimates on causes of mortality are noisier, but follow a similar pattern, with the most pronounced effects of mortality caused by *Certain conditions originating in the perinatal period* and *Congenital malformations, deformations and chromosomal abnormalities*, although estimates are imprecise.

Taken together, our findings demonstrate that maternal dismissals impact the health of children beyond immediate measures of health. A combination of longer-term in utero effects and contempo-

³⁵In columns (9)–(13) of Tables A1, A2 and A3 in the appendix, we provide the same additional robustness checks as for the main outcomes of birthweight and low birthweight.

³⁶The different groups of causes are based on letter codes from ICD-10. We focus on the groups with the highest incidence, grouping the remainder of infrequent categories into 'Other'

aneous effects of continued joblessness—caused, for example, by reduced financial resources in the first year of life—may lead to differential health trajectories over time, consistent with the patterns we observe for hospitalization and mortality.

7 Concluding remarks

In this paper, we estimate the effect of dismissals during pregnancy on birth and infant outcomes using Brazilian administrative vital statistics records linked to employer–employee data. We find that the dismissal of pregnant mothers from employment reduces the birthweight by more than 30 grams and increases the likelihood of low birthweight by more than 20% compared to the baseline incidence. The effects are more pronounced for workers in the lowest wage quartile, possibly indicating a nutritional channel that leads to growth retardation, while we observe a small positive effect on gestational length.

We document the considerable mitigating effects of formal unemployment insurance, exploiting an eligibility rule of unemployment insurance within a regression discontinuity setting. We also find that dismissals affect healthcare choices, with dismissed women less likely to reach a minimum number of prenatal care visits, less likely to give birth in private clinics, and less likely to deliver through a planned C-section. In addition to the short-term effects on health at birth, we find that dismissals have lasting effects on the health of children, documenting a pronounced increase in hospitalizations of children in the first year after birth and a substantial increase in infant death as a consequence of dismissals during pregnancy.

We provide a battery of robustness and sensitivity checks, by providing estimates for a variety of combinations of location, time fixed-effects and maternal characteristics, and different definitions of treatment and the estimation sample. In particular, when employing an alternative identification strategy based on mass layoffs, we find estimates that are very close to our main estimates, providing further credibility to our main estimates.

The results have policy implications by providing novel insights into the negative consequences of dismissals on single mothers, both in terms of measures of health at birth and long-term complications. The results also highlight important mitigating factors, emphasizing the important role of unemployment benefits for the health of children and the potential role of self-insurance. Due to the limited information on fathers, this article focuses on single mothers, who are becoming a significant demographic in Brazil and elsewhere. More detailed information, particularly regarding

the employment, income, and labor market responses of parents, would allow for an investigation into the mitigating role of joint household income in the aftermath of the dismissals of pregnant women.

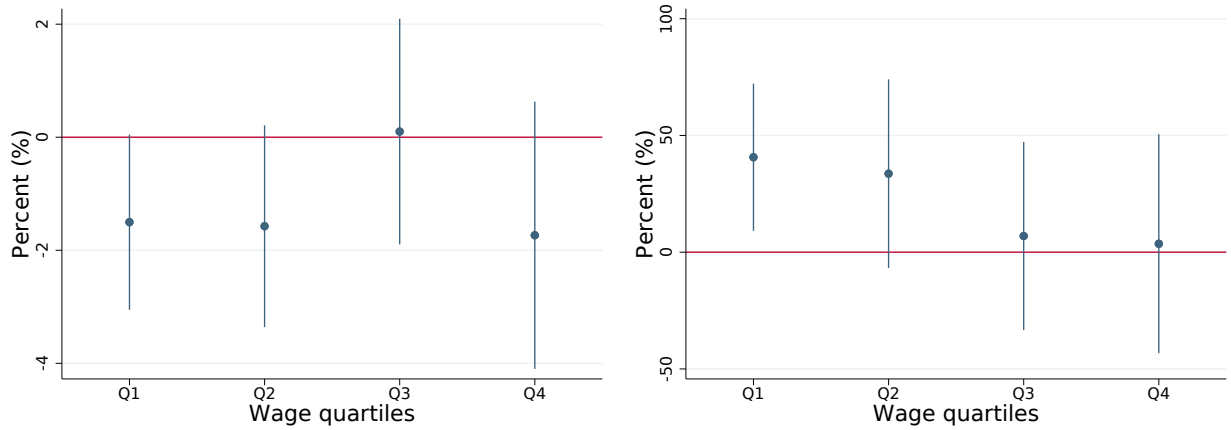
References

- 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 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.
- Branum, Amy and Katherine Ahrens**, “Trends in Timing of Pregnancy Awareness among US Women,” *Maternal and Child Health Journal*, 2017, *21*, 715–726.
- 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.
- Couch, Kenneth A. and Dana W. Placzek**, “Earnings Losses of Displaced Workers Revisited,” *American Economic Review*, 2010, *100* (1), 572–89.

- 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.
- do Carmo, Maria, Ana Paula Esteves-Pereira, Elaine Fernandes Viellas, Rosa Maria Soares Madeira Domingues, and Silvana Granado Nogueira da Gama**, “Prenatal Care in the Brazilian Public Health Services,” *Resvista de Saude Publica*, September 2020, *54* (8).
- Eliason, Marcus and Donald Storrie**, “Does Job Loss Shorten Life?,” *Journal of Human Resources*, March 2009, *44* (2), 277–302.
- Flaen, Aaron, Matthew D. Shapiro, and Isaac Sorkin**, “Reconsidering the Consequences of Worker Displacements: Firm versus Worker Perspective,” *American Economic Journal: Macroeconomics*, September 2019, *11* (2), 193–227.
- 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.
- Hansen, Dorte, 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.
- ILO**, “Labour Force Statistics (LFS),” *Last updated: 05/06/2025 07:04:54*, 2025.
- Kraemer, Sebastian**, “The Fragile Male,” *British Medical Journal*, 2000, *321* (7276), 1609–1612.
- Kristiina, Huttunen and Jenni Kellokumpu**, “The Effect of Job Displacement on Couples’ Fertility Decisions,” *Journal of Labor Economics*, April 2016, *34* (2), 403–442.
- 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.

- McDuffie, Robert, Arne Beck, Kimberley Bischoff, 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 (11), 847–851.
- 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.
- Menezes-Filho, Naercio and Ricardo Politi**, “Estimating the Causal Effects of Private Health Insurance in Brazil: Evidence from a Regression Kink Design,” *Social Science Medicine*, 2020, 264.
- 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.
- ONS**, “Births in England and Wales: 2022,” *statistical bulletin*, 2024.
- 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.
- Osterman, MJK, BE Hamilton, JA Martin, AK Driscoll, and CP. Valenzuela**, “Births: Final Data for 2022,” *National Vital Statistics Reports*, 2024, 73 (2).
- 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), 1265–130.
- 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 Bárdits, Anna Adamecz, Márta Bisztray, and Ágnes Szabó-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

Figure 1: Effect of maternal dismissals on birthweight by wage 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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours and months of tenure at conception.

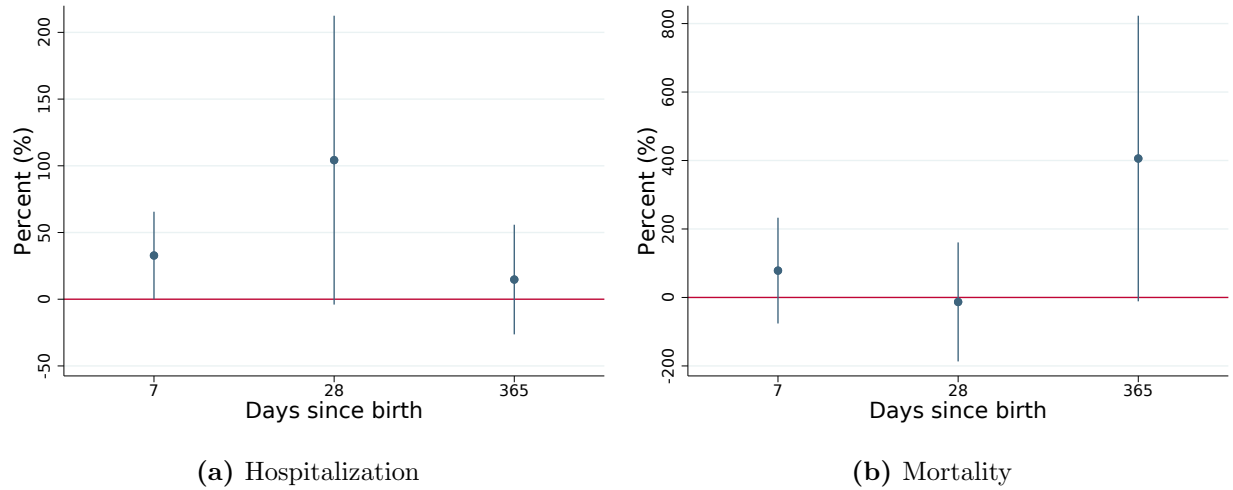
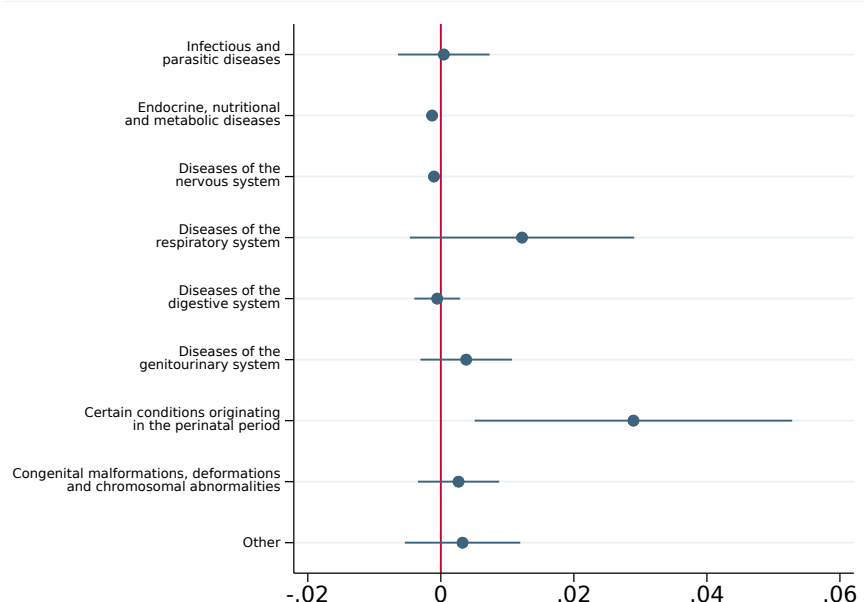
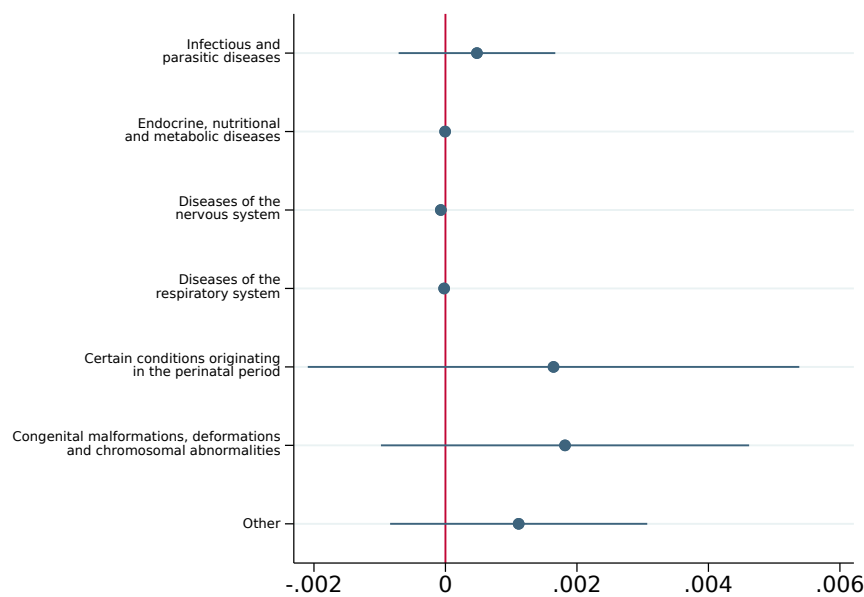


Figure 2: Effect of maternal dismissals on period-specific hospitalization and mortality.

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 - i.e., dummies for mother’s prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 - i.e., dummies for mother’s prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 live birth (≥ 1)	0.389 (0.541)	0.489 (0.500)	0.100***
20 y.o. or less	0.165 (0.423)	0.225 (0.424)	0.059***
21-35 y.o.	0.765 (0.464)	0.731 (0.447)	-0.034***
36 y.o. or more	0.070 (0.286)	0.044 (0.206)	-0.026***
White	0.317 (0.898)	0.256 (0.453)	-0.061***
Mixed	0.530 (1.131)	0.574 (0.522)	0.045***
Black	0.092 (0.367)	0.110 (0.302)	0.017**
Primary educ. or less	0.171 (0.418)	0.252 (0.454)	0.081***
Secondary educ.	0.726 (0.480)	0.684 (0.478)	-0.042***
Higher Education	0.084 (0.328)	0.035 (0.187)	-0.049***
Blue collar	0.039 (0.290)	0.040 (0.199)	0.001
Weekly hours	42.382 (5.948)	42.468 (4.932)	0.086
Monthly wage (R\$ 2014)	1,043.719 (1,260.725)	918.678 (689.082)	-125.041***
Tenure at conception (months)	16.872 (25.165)	13.260 (16.295)	-3.612***
Panel B: Work absences			
<i>Any reason:</i>			
6-3 months to conception	0.034 (0.280)	0.030 (0.186)	-0.003
within 3 months to conception	0.025 (0.226)	0.021 (0.148)	-0.003
<i>Unpaid leave:</i>			
6-3 months to conception	0.002 (0.051)	0.002 (0.045)	0.000
within 3 months to conception	0.001 (0.038)	0.001 (0.026)	-0.001
<i>Sick leave (work-unrelated):</i>			
6-3 months to conception	0.030 (0.260)	0.029 (0.179)	-0.001
within 3 months to conception	0.022 (0.211)	0.021 (0.146)	-0.002
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 based on a t-test on the difference between non-dismissed (control) and dismissed (treatment) group means. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Effect of maternal dismissals on birthweight.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) Dep.Var.: <i>Birthweight</i> (g)							
Dismissed	-31.346** (14.808)	-39.079*** (14.667)	-40.882*** (14.683)	-38.001** (14.814)	-39.010*** (14.657)	-39.827*** (14.822)	-34.787** (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.: <i>Low birthweight</i>							
Dismissed	0.020** (0.008)	0.022*** (0.008)	0.023*** (0.008)	0.023*** (0.008)	0.022*** (0.008)	0.023*** (0.008)	0.021** (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., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 dismissal 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.: <i>Birthweight</i> (g)														
Dismissed	-44.491** (21.396)	-32.020 (20.927)	-32.578** (13.592)	-2.638 (60.375)	-6.428 (30.727)	-54.544*** (17.014)	50.444 (65.906)	-61.888** (27.754)	-33.885* (18.360)	52.116 (72.394)	-12.486 (26.871)	-48.756*** (17.175)	-36.832** (14.809)	-133.481 (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.: <i>Low birthweight</i>														
Dismissed	0.024** (0.011)	0.022* (0.012)	0.014** (0.006)	0.045 (0.043)	0.019 (0.018)	0.026*** (0.009)	-0.034 (0.039)	0.002 (0.016)	0.027*** (0.010)	-0.033 (0.035)	0.008 (0.015)	0.027*** (0.009)	0.023*** (0.008)	0.012 (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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 other birth-related outcomes.

	Gestation			Additional birth outcomes		
	(1) <i>Female</i>	(2) <i>Length (days)</i>	(3) <i>Preterm</i>	(4) <i>Emergency C-section</i>	(5) <i>Low 1-min Apgar</i>	(6) <i>Low 5-min Apgar</i>
Dismissed	0.020 (0.013)	0.828* (0.441)	0.011 (0.008)	0.001 (0.011)	0.005 (0.007)	0.003 (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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 UI eligibility on birthweight.

	(1)	(2)	(3)
(a) Dep.Var.: <i>Birthweight</i>			
UI Eligibility	63.670** (31.690)	114.921*** (43.745)	110.999** (46.774)
UI Eligibility \times Wage		-0.060* (0.032)	-0.059* (0.033)
Baseline mean	3,102.114	3,102.114	3,102.250
(b) Dep.Var.: <i>Low birthweight</i>			
UI Eligibility	-0.023 (0.018)	-0.040* (0.024)	-0.044* (0.026)
UI Eligibility \times 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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours and monthly wage. 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 dismissals on couples' newborns

	(1) <i>Birthweight</i>	(2) <i>Low birthweight</i>	(3) <i>Female</i>	(4) <i>Gestation length (days)</i>	(5) <i>Preterm</i>	(6) <i>Emergency C-section</i>	(7) <i>Low 1-min Apgar</i>	(8) <i>Low 5-min Apgar</i>
Dismissed	16.270 (13.136)	0.000 (0.007)	-0.040*** (0.013)	1.399*** (0.359)	-0.011 (0.007)	0.008 (0.011)	0.002 (0.006)	0.003 (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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 maternal dismissals on prenatal visits and delivery choices.

	(1) <i>Prenatal visits</i>	(2) <i>Fewer than 6 visits</i>	(3) <i>Private clinic</i>	(4) <i>Planned C-section</i>
Dismissed	-0.530** (0.259)	0.144*** (0.012)	-0.083*** (0.011)	-0.071*** (0.011)
Non-dismissed mean	8.405	0.133	0.334	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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effect of maternal dismissals on hospitalization and mortality.

	Hospitalization					
	(1) <i>Probability</i>	(2) <i>Episodes</i>	(3) <i>Days</i>	(4) <i>ICU</i>	(5) <i>Cost (log)</i>	(6) <i>Infant death</i>
Dismissed	0.032*** (0.012)	0.036** (0.016)	0.469 (0.299)	0.002 (0.005)	0.356*** (0.135)	0.005* (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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

to accompany *Maternal Dismissals during Pregnancy and the Health of Infants*

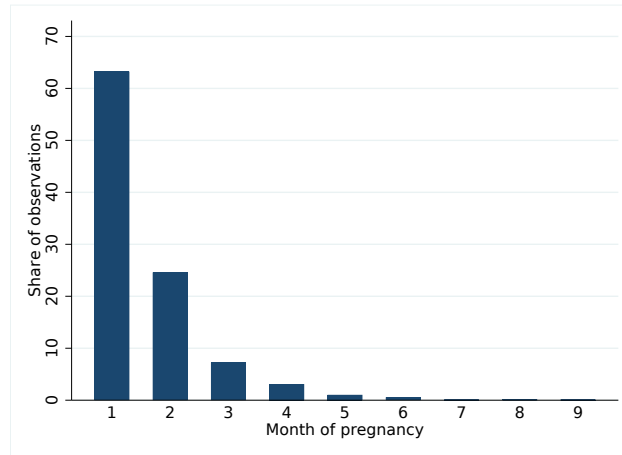


Figure A1: Distribution of observations by month of pregnancy of occurrence of dismissal announcement.

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

Table A1: Effects of maternal dismissals for single mothers' newborns - additional dismissal timing restriction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	<i>Birthweight</i>	<i>Low birthweight</i>	<i>Female</i>	<i>Gestation length (days)</i>	<i>Prenatal visits</i>	<i>Fewer than 6 visits</i>	<i>Private clinic</i>	<i>Planned C-section</i>	<i>Hospitalization (probability)</i>	<i>Hospitalization (days)</i>	<i>ICU</i>	<i>Hospitalization cost (log)</i>	<i>Infant death</i>
Dismissed	-54.285*** (18.558)	0.023** (0.010)	0.023 (0.017)	0.559 (0.592)	-0.502 (0.420)	0.227*** (0.016)	-0.087*** (0.014)	-0.065*** (0.014)	0.043*** (0.015)	0.604 (0.377)	0.007 (0.008)	0.498*** (0.175)	0.006* (0.003)
Non-dismissed mean	3,150.485	0.082	0.485	269.665	8.405	0.133	0.334	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	44,899	28,705	28,705	28,705	28,705	44,899

Note: For the analysis in this table, we restrict the definition of dismissals 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* measures the number of prenatal visits attended by the mother. *Fewer than 6 visits* 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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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 - mass layoffs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	<i>Birthweight</i>	<i>Low birthweight</i>	<i>Female</i>	<i>Gestation length (days)</i>	<i>Prenatal visits</i>	<i>Fewer than 6 visits</i>	<i>Private clinic</i>	<i>Planned C-section</i>	<i>Hospitalization (probability)</i>	<i>Hospitalization (days)</i>	<i>ICU</i>	<i>Hospitalization cost (log)</i>	<i>Infant death</i>
Dismissed	-37.187*	0.016	0.033*	1.509**	-0.824***	0.136***	-0.095***	-0.080***	0.041**	0.766	0.009	0.476**	0.006
	(20.475)	(0.011)	(0.018)	(0.613)	(0.311)	(0.016)	(0.015)	(0.015)	(0.017)	(0.497)	(0.009)	(0.198)	(0.004)
Non-dismissed mean	3150.516	0.082	0.485	269.595	8.416	0.131	0.348	0.343	0.098	1.045	0.021	R\$ 316.96	0.004
Observations	36,337	36,337	36,337	36,337	36,337	36,337	36,337	36,337	23,318	23,318	23,318	28,860	36,337

Note: For the analysis in this table, we restrict the definition of treatment to dismissals from mass layoffs (where a firm dismisses at least 27 percent of its total workforce in a given year) and control to workers employed in non-mass layoff firms (number of displaced workers in a given year being less than 27 percent of the 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* measures the number of prenatal visits attended by the mother. *Fewer than 6 visits* 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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at 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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	<i>Birthweight</i>	<i>Low birthweight</i>	<i>Female</i>	<i>Gestation length (days)</i>	<i>Prenatal visits</i>	<i>Fewer than 6 visits</i>	<i>Private clinic</i>	<i>Planned C-section</i>	<i>Hospitalization (probability)</i>	<i>Hospitalization (days)</i>	<i>ICU</i>	<i>Hospitalization cost (log)</i>	<i>Infant death</i>
Dismissed	-43.646** (21.092)	0.029** (0.011)	0.003 (0.019)	0.226 (0.652)	-0.147 (0.419)	0.154*** (0.017)	-0.088*** (0.015)	-0.075*** (0.016)	0.021 (0.016)	0.104 (0.330)	-0.006 (0.006)	0.228 (0.177)	0.004 (0.004)
Non-dismissed mean	3150.487	0.082	0.485	269.596	8.416	0.131	0.348	0.343	0.098	1.046	0.021	R\$ 317.397	0.004
Observations	36,332	36,332	36,332	36,332	36,332	36,332	36,332	36,332	23,337	23,337	23,337	23,337	36,332

Note: For the analysis in this table, we restrict the definition of treatment to dismissals outside of mass-layoffs firms (number of displaced workers in a given year being less than 27 percent of the 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* measures the number of prenatal visits attended by the mother. *Fewer than 6 visits* 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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

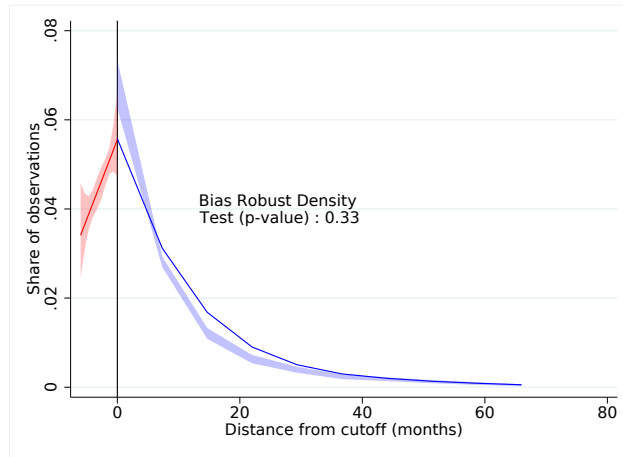


Figure A2: Distribution of observations around the UI eligibility cutoff.

Note: This figure shows the density of dismissal months around the cutoff month (6th 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.

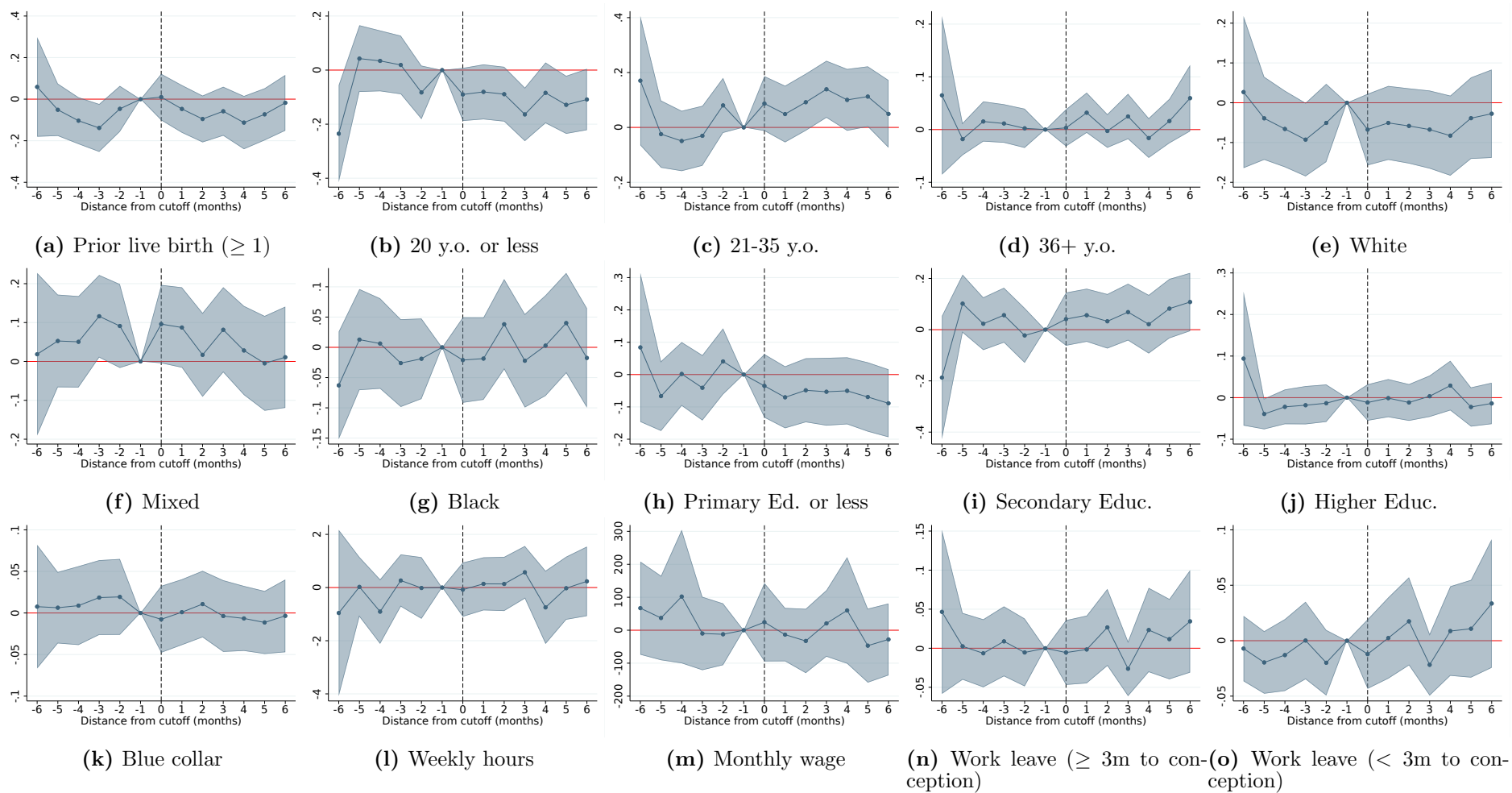


Figure A3: Balance of pre-determined covariates.

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

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

	(1)	(2)	(3)
(a) Dep.Var.: <i>Birthweight</i>			
UI Eligibility	45.193 (53.001)	103.347 (64.637)	98.126 (67.805)
UI Eligibility × Wage		-0.068* (0.040)	-0.076* (0.041)
Baseline mean	3,111.490	3,111.490	3,112.274
(b) Dep.Var.: <i>Low birthweight</i>			
UI Eligibility	-0.016 (0.031)	-0.032 (0.037)	-0.044 (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 - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours and monthly wage. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of maternal dismissals on hospitalization conditional on public clinic delivery.

	Hospitalization				
	(1) <i>Probability</i>	(2) <i>Episodes</i>	(3) <i>Days</i>	(4) <i>ICU</i>	(5) <i>Cost (log)</i>
Dismissed	0.026* (0.014)	0.030 (0.019)	0.387 (0.345)	-0.001 (0.006)	0.289* (0.156)
Non-dismissed mean	0.121	0.153	1.331	0.026	399.459
Observations	19106	19106	19106	19106	19106

Note: The analysis is based on births, occurred in public clinics, 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. All regressions include fixed-effects for municipality of residence and month of conception. We control for infant's sex and mother-specific covariates - i.e., dummies for mother's prior live birth, age, ethnicity, education level, type of occupation, and work absence terminating 6 to 3 months and within 3 months prior to conception, as well as variables for weekly hours, monthly wage, months of tenure at conception. Robust standard errors clustered at the municipality-by-month level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.