Maternal Displacements during Pregnancy and the Health of Newborns

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Abstract

We estimate the effect of maternal displacements during pregnancy on birth outcomes using Brazilian administrative data on employment and birth records. Displacements of pregnant single mothers lead to lower mean birthweight (BW), a 10% increase in low BW incidence and a substantial increase in infant mortality; but to BW gains for displaced mothers in a marriage or stable union. Dismissals cause increased gestational length, as well as lower attendance to prenatal visits and lower take-up of planned c-sections for both groups. We document powerful mitigating roles of partners, own resources and formal insurance.

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1 Introduction

Losing one's job is one of the most significant economic shocks individuals might be exposed to over their life cycle. The consequences of job loss on the individual have been widely documented in the literature. Layoffs have been associated with worse cardiovascular health (and increased smoking behavior), higher count of high-risk biomarkers (Black et al., 2015; Michaud et al., 2016), worse self-reported health, activity limitations, and worse mental health (Schaller and Stevens, 2015). Ultimately, the health consequences of layoffs are persistent and increase mortality of displaced workers, through either worse health conditions, suicide and suicide attempts, traffic accidents, alcohol-related diseases, and mental illnesses (Sullivan and Von Wachter, 2009; Browning and Heinesen, 2012). In addition to the direct effects on the worker, the consequences of job loss may also spill over to other family members, including spouses (Marcus, 2013) and children: findings in the literature point also at negative effects on children's well-being and education (Rege et al., 2011; Pieters and Rawlings, 2020).

The spillover effects of parental job loss on children in the family may be felt even earlier during the time in-utero. Fetal development is sensitive to a variety of shocks to the environment of the mother and child (Almond and Currie, 2011a), potentially including changes in the employment status of the mother. Since Barker's *fetal origins hypothesis* (Barker, 1992), long-term outcomes of health at birth has spurred a large body of literature, providing evidence on the negative effect of lower birthweight (BW) and short gestation on socioeconomic outcomes later in life (see Almond and Currie, 2011b, for a comprehensive review of the related literature). An involuntary job loss during pregnancy may then also affect the development of the unborn child in line with other shocks in-utero, leading to spillovers of dismissals to the next generation. However, dismissals differ from other shocks in-utero because a maternal job interruption on fetal development could have ambiguous consequences. Relief from physical strain and stress linked to the workplace may positively affect the unborn child. In contrast, the stress associated with dismissals and the shock to household income may negatively affect the child's development, particularly for poorer households, for which the loss of income may, for example, harm the nutritional intake of pregnant mothers.

In this paper, we leverage population-level administrative data linking individual formal employment spells with birth records from a populous state in Brazil over 2011-2014. These data allow us to use plausibly exogenous maternal job loss events (i.e., layoffs defined as involuntary displacements) during pregnancy to estimate the effects of maternal displacements on birth outcomes of the affected children. We assess the impact of maternal displacement on health at birth, measured by birth weight (BW) and low BW classifications and additional birth outcomes. Furthermore, we explore whether layoffs entail consequences in prenatal health utilization behavior and delivery choices for the displaced mothers. We also follow these newborns up to 1 year from birth, assessing their patterns of infant mortality. The rich administrative data allow us to investigate the different potential underlying mechanisms through which a maternal layoff can affect infant health. In particular, we focus on understanding the mitigating role of informal and formal insurance to household income shocks with respect to birth outcomes.

The literature on job loss and its effects on birth outcomes is limited, possibly due to stringent demands on the data to establish causal effects - recovered mainly from intra-family variation. Lindo (2011) uses panel survey data focusing on paternal job loss documenting a reduction in BW (-142 grams, around -4.5%) of babies born in the year of a paternal layoff in the US, documenting that these dismissals lead to significant decreases in pre-birth food expenditure - suggesting a deterioration of prenatal nutrition as main driver of BW losses. Focusing on maternal employment status during pregnancy, Wüst (2015) finds that Danish working mothers are more likely to have preterm deliveries when they are not reported as being employed during gestation, claiming that exclusion from employment may stress mothers in countries with high-female employment rates. Contrarily, Del Bono et al. (2012) find positive effects of maternal work interruptions on birth weight up to three months before birth, but no information on the reasons for those job interruptions is available. A variety of studies uses variation in different measures of aggregate unemployment across areas to identify the effects of unemployment shocks on birth outcomes at the individual level. Despite selection into pregnancy issues in most of this literature, as highlighted by Dehejia and Lleras-Muney (2004), these suggest that birth outcomes tend to be pro-cyclical and local negative economic conditions can impact infant health through several channels (e.g. distressing news, as in Carlson, 2015, or maternal services availability, as in De Cao et al., 2022).

We add to this literature by using individual employment records containing very detailed information on the causes and the precise timing of dismissals from current employment linked to birth records from administrative birth records at the population level. This allows us to address the shortcomings in previous papers arising from the use of survey data or the limitation of studying only paternal dismissals (and hence focusing on mothers in stable relationships). We investigate in detail heterogeneous effects along several margins, in particular regarding household composition, i.e. marital status or the presence of the father. Marital status and household composition have been shown to matter for various outcomes, including children's well-being. For example, individuals growing up with a single mother during childhood (or experiencing parental separation) are less successful in terms of education and socioeconomic outcomes both in the short and in the long term (Musick and Meier, 2010; Richter and Lemola, 2017). Economic hardship among single-mother families is also substantial following a job loss (Eamon and Wu, 2011), and there is evidence of negative effects of job displacement among single mothers on children's educational attainment and social-psychological well-being in young adulthood (Brand and Simon Thomas, 2014).

In this paper, we make two main contributions to the literature. First, we provide novel insights from administrative individual-level data on the effect of involuntary job loss during pregnancy on children's birth outcomes, effectively comparing children born to working mothers laid-off during pregnancy with a control group of children whose mothers were not laid-off during pregnancy while controlling for a rich set of mother and pregnancy characteristics and time and location fixed effects. While we find small, statistically insignificant effects on BW and several additional outcomes for the full sample, we document important heterogeneous effects by marital status. We find that job loss of a single mother leads to a decrease in BW by 28 grams ($\approx 1\%$), significant at the 5% level, and an increase in the incidence of low BW (LBW, < 2500g) by 10% (marginally significant). In contrast, for married mothers or mothers in a stable relationship, we find a significant positive effect on BW of around 20 grams and a slight decrease in LBW. These results are robust to a battery of alternative specifications and robustness checks, including enriching our baseline specification with additional fixed effects and restricting our analysis to mothers fired in a mass layoff. Contrarily, we do not find differential impacts on gestational length nor on prenatal care and delivery choices, as displaced mothers generally tend to have a slightly longer gestation, delay their first prenatal visit and have a lower attendance, recur less to private clinics and c-sections. Hence, pregnancy complications for single mothers are likely to be due to economic distress and poorer nutritional intake, despite the increased gestational length. We also report that maternal layoffs during pregnancy not only harm health at birth for single mothers' babies, but also significantly increase their mortality within their first year of life.

Second, we establish the presence of two competing underlying effects of job loss during pregnancy on birth outcomes, a positive effect on BW due to the positive effect of leaving employment on gestational length present across all mothers - in line with biomedical evidence on the association between stressful working conditions and worse pregnancy outcomes (Cai et al., 2020) - and a negative effect due to a likely combination of loss of income and stress associated with dismissal. We provide further evidence on the relative strength of these two channels using different subsamples of mothers to study the role of informal and formal insurance mechanisms. The availability of any type of buffer against unemployment (either through presence of a partner or own resources) is revealed to determine which underlying effect prevails. We start by looking at the presence of spouse/partner in birth records, splitting single and non-single mothers in subsamples. We find that the effect of layoffs varies from between -33 grams for single mothers without a partner declared on the birth certificate of the child to +33 grams for mothers with a partner in formal employment declared in the birth certificate and effect sizes in between those for different subsamples of mothers in a variety of circumstances. These results by subsample shed light on the inconclusive effects estimated previously in the literature due to differences in the samples of mothers used and differences in the definition of job loss. Moreover, these findings strongly hint at the role of partners as providers of informal insurance in adverse economic conditions for these pregnant women. Focusing on the negative effect of layoffs, we proceed by investigating the role of income shocks from layoffs for single mothers. We find that the negative effects for single mothers are substantially more pronounced for individuals on low incomes or entitled to a low Severance Pay. We find a negative effect on BW of approximately 40 grams for mothers in the lowest quartile, while the effect is much smaller and not significantly different from zero for mothers in the top quartile, indicating the potentially mitigating role of income through self-insurance. We also find that layoffs significantly increase the chance for LBW for the lowest quartile. This effect diminishes for higher quartiles. The positive gradient in wage/Severance Pay is confirmed for working mothers in a stable relationship: the highest and most significant gains in BW are found only in top quartiles. The effects of dismissal also present a gradient in wage/Severance Pay for other outcomes of interest, such as timing of first prenatal visit and attendance, type of clinic for delivery and take-up of c-sections. Lastly, we provide evidence on the mitigating role of formal insurance in the form of unemployment benefits. We study a tenure-based unemployment insurance (UI) scheme providing income support for displaced workers in Brazil by exploiting the discontinuity in the eligibility rule of UI in a regression discontinuity setting. We find that UI increases BW for single-mother children, counteracting the negative effect of dismissals for displaced single women. No significant effect of UI is found for couples.

The remainder of this paper is organized as follows. The following section provides some context on Brazilian labor market institutions and a brief background on related literature. Section 3 describes the data sources and provides summary statistics. Section 4 presents the empirical strategy, followed in Section 5 by the main results of maternal layoff on newborns' health. In Section 6, we discuss the potential insurance mechanisms behind the relationship between layoff and birth outcomes. Section 7 provides final remarks.

2 Institutional context and background

In this section, we provide some background information on the Brazilian labor market features and the institutional settings of Brazilian employment regulation, paying particular attention to the job displacement insurance. Moreover, we describe the possible channels of transmission of job interruptions on fetal and infant health as highlighted by the biomedical literature.

2.1 Female employment, regulation of maternity and unemployment in Brazil

In 2019, the female labor participation rate in Brazil was estimated to be 55%, accounting for almost 44% of the total labor force, and an unemployment rate of around 14% (World Bank, 2021). The vast majority of formal labor contracts in Brazil are open-ended (91.4%). The Brazilian labor market is characterized by a large informal sector, which accounts for approximately 30% of total labor market participation. Our estimates are representative only for mothers in the formal sector. As workers in the informal sector are not protected from unfair dismissals and are not eligible for unemployment insurance, any negative effect estimated for formal jobs likely would need to be considered to be a lower bound for dismissals from informal employment.

Brazilian labor legislation is based on at-will employment, whereby firms are free to dismiss workers without a just cause, although they must pay dismissal indemnities. The most common form of separation for open-ended jobs are dismissals without a just cause (70% of all cases) and voluntary quits (29%).[1] Employers have to inform workers about their dismissal abiding by a mandatory 30-day minimum advance notice period, i.e. the dismissal coming into effect at the earliest 30 days after being informed about the dismissal decision by the employer. To protect pregnant mothers from discrimination, by law, their dismissal without just cause is void if the employer is being made aware of the pregnancy before dismissal, including during the notice period. This protection extends for up to 5 months after delivery. Layoffs of pregnant mothers are expected to be overwhelmingly happening in the first trimester, when expectant mothers may not be aware yet that they are expecting or have no confirmation of the pregnancy by a medical practitioner. Layoffs of pregnant mothers are still possible and in accordance with the law in case of mass layoffs and plant closures, during which firms part with a very large share of their workers or close down completely, for example, in case of the shutdown of a plant.

Brazilian labor regulation provides unemployment insurance (UI) to assist displaced workers. These benefits can be claimed only by employees dismissed without just cause and are available for three to five months, depending on the length of employment in the 36 months prior to dismissal. Dismissed workers are entitled to UI payments for three, four, or five months for previous tenure of 6, 12, or 24 months, respectively.[2] The average wage in the three months prior to layoff determines the replacement rate that the eligible workers will receive, starting from 100% of previous earnings for workers earning the minimum wage. We will use the discontinuity in eligibility for UI, based on the minimum period of continuous employment before dismissal to explore the role UI plays for dismissed pregnant workers later in the paper.

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

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

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

2.2 Possible channels of transmission: previous evidence

Economic and epidemiological research suggests two main channels of transmission of job interruptions effects on newborns' health, which potentially work in opposite directions: a negative effect could be instigated by the income shock or by exposure to stress in-utero, while a positive effect may be the result of the relief from a physically or mentally demanding work environment.

The negative impacts on fetal and birth health can be explained by: reduced food expenditure and/or unhealthy consumption due to budget restrictions and the absence of a consumption smoothing mechanism (Lindo, 2011; Dave and Kelly, 2012); maternal stress from the job separation that can feed through the womb and affect negatively fetal growth. There is an overall consensus on the positive association between antenatal stress exposure and likelihood of lower birth weight and shorter gestation, with evidence from both biomedical studies (see Lima et al., 2018, for an assessment of cohort analyses) and social sciences - where stress-generating shocks during pregnancy have been identified in several ways, e.g. homicides (Koppensteiner and Manacorda, 2016); terrorist attacks (Camacho, 2008; Cozzani et al., 2021); or bereavement (Black et al., 2016).[3]

On the other hand, work-related physical and psycho-social strain during pregnancy might be lower following a displacement, possibly ameliorating the prenatal environment. Work-related risk factors, such as long work hours and physically demanding work, have been suggested to influence pregnancy outcomes adversely. Among the studies in Cai et al. (2020) on occupational activities during pregnancy and pregnancy outcomes, there is evidence of positive association between heavy physical workload, as well as long working hours, and indicators of poor health at birth, such as preterm delivery, low BW or small for gestational age. The possible relief from the work strain is likely part of the rationale of prenatal maternity leave - both unpaid and paid maternity leave have been found as beneficial for birth weight and gestation (Rossin, 2011; Stearns, 2015).[4]

The empirical literature on the relationship between parental, and specifically maternal employment, and birth outcomes has provided evidence in favor of either channel (Lindo, 2011; Del Bono et al., 2012; Wüst, 2015). In this paper, we aim at disentangling these competing channels as much as possible, exploiting the fact that we can estimate the effect separately for mothers where we can vary their income-related shock and by focusing on available formal and informal insurance during pregnancy.

3 Data sources

Previous work on the health consequences of parental unemployment in the empirical literature mostly relies on data from longitudinal surveys (Lindo, 2011; Del Bono et al., 2012), or a combination of survey information and administrative data (Wüst, 2015). The

[3]While the effects on infant health of in-utero exposure to maternal stress are found to be generally small, they may be more relevant and producing longer-term repercussions for the offspring of women with fewer resources to combat these adverse effects (Aizer et al., 2016).

[4]Importantly, Rossin (2011) notices that the effects of unpaid leave were driven by children of college-educated and married mothers, who were most able to take advantage of unpaid leave, while Stearns (2015) highlights particularly large impacts of paid leave on the children of unmarried and black mothers.

use of survey data means that the source of variation in maternal employment status is frequently unspecified, making it difficult to distinguish between voluntary and involuntary job separations. Relying on survey data for birth outcomes also means that birth weight information from surveys is reported by parents and, thus, subject to recall error. Sample sizes of surveys tend to be small, particularly hampering the analysis of rare events. The ideal data to estimate the causal effect of dismissals on birth outcomes addressing those previous shortcomings, hence includes information on recorded maternal employment spells with reasons of dismissals (to distinguish between voluntary and involuntary dismissals) and the time of dismissals, for example, based on social security records, linked to the universe of births from vital statistics data.

Our dataset is mainly obtained by merging two such administrative records using individual identifiers. The first source is the Relação Anual de Informações Sociais (RAIS), a linked employer-employee dataset covering the universe of formal workers and firms in Brazil, made available by the Brazilian Ministry of Labor up to 2014. RAIS identifies workers by both a unique tax code identifier (CPF) and their full name, enabling us to link workers to firms over time and to birth records. The RAIS data includes detailed characteristics of workers' employment spells such as the start/end date and location of each job, the type of contract, occupation and sector code, and the workers' education and earnings. This data enables us to identify dismissals and every worker's recorded cause of dismissal. Moreover, we calculate the statutory job displacement Severance Pay for all working mothers in our dataset. The second dataset comes from birth records from vital statistics data collected through the Sistema de Informações sobre Nascidos Vivos (SINASC), available for the years 2011-2014. These records are based on the universe of birth certificates issued in Brazil, whether they were issued in hospitals, birth clinics or from midwives after home deliveries, accounting for more than 99% of births. This source provides a number of variables of interest for our analysis: information on the date of conception, birth weight, gestational length, babies' gender as well as history of previous deliveries of the mother (i.e., number of live births and stillbirths). We also observe the age and race of the mother, the declared marital status (single, married, in a stable relationship, divorced or widowed) and whether a father is identified.

Furthermore, we use mortality data from vital statistics death records from the Brazilian Mortality Information System (Sistema de Informações sobre Mortalidade (SIM), in Portuguese). This data set contains information on all natural and non-natural deaths in Brazil, including precise cause of death and characteristics of the deceased. In case of death occurring up to the age of one, these data also register the characteristics of mothers and birth outcomes, hence allowing us to link birth records with information on child mortality using the birth ID from SINASC.

To merge births (1,051,449 over the period from 2011-2014) with employment records, we start with the sample of singleton births (dropping twins) of mothers between the ages of 13 and 50 at the time of birth, and drop cases where there is missing information on the identity of the mother or her date of birth,[5] and exclude duplicate observations. We link birth records to the employer-employee matched dataset using personal identifiers available in both datasets. We retain only the first birth observed over the available years for each matched worker and ensure that mothers are economically active in the formal labor market and holding open-ended contracts in the private sector at the time of conception, reducing the number of birth records, leaving us with a final sample of 165,773 births over the four years period.[6]

[5]Most of those cases with missing information on the date of birth (93%) relate to the year 2011, the first year in the SINASC data that collected information on precise birth date, rather than the age of the mother.

[6]This ensures a consistent set of births over the entire period, both for the 'treated' and 'control' group of mothers. For the vast majority of observations (61%) we can merge the datasets perfectly based on individual identifiers. For the remainder, not to lose those observations, we use probabilistic matching procedure and check for false positives and negatives. Our preferred sample includes only mother-birth pairs whose matching score is at least 98%. We test the robustness of the findings to varying matching quality in the Appendix.

4 Empirical strategy

One difficulty in estimating the causal effect of job loss on birth outcomes, when relying on aggregate unemployment shocks linked to birth outcomes in the same area, is that some characteristics of a local area in which job losses occur are unobserved to the econometrician. Some of these characteristics may be correlated with job losses and with birth outcomes. For example, an economic downturn in one area may lead to higher unemployment and may affect public services, including prenatal health care. In this case, one might erroneously conclude that dismissals leading to higher unemployment may lead to worse health outcomes, but the relationship may be more complex. Moreover, when using individual-level information on job separation from survey data rather than aggregate information, the reason for job separation is often unobserved. This is problematic for estimating the effect of leaving employment on birth outcomes because some mothers may decide to leave a job voluntarily during pregnancy. If the decision to quit is correlated with the health status of the mother or unborn child, one might erroneously conclude that the correlation of poor maternal or child health in-utero and poor health at birth is caused by the job separation. To overcome these identification problems, we leverage conception dates derived from birth records and detailed information on the start/end months of job spells and timing of involuntary job separations to estimate the causal effect these dismissals have on the health of newborns.

Our analysis focuses on pregnant female workers holding open-ended contracts in the private sector. We only consider job separations based on dismissal without just cause, and hence eliminating any voluntary job separations subjecting the *treatment* to self-selection. We denote child i's month of conception with t, child i's month of maternal displacement with T and the imputed last month of pregnancy with $t+9$.[7] Crucially, we do not include

[7] As gestational length may mechanically affect the propensity to have a displacement towards the end of pregnancy, i.e. that mothers with shorter gestational length have a smaller risk to experience such an event, we assign treatment based on a full-term gestation. We use discrete months for data limitations: hence, for a 280-days long imputed duration of a pregnancy, the gestation spans ten different months.

in our estimation sample the cases in which child i's month of maternal displacement T is such that $T < t$ (no endogenous pregnancies). We construct an indicator function for treatment status as follows

$$D = \begin{cases} 1, \text{if } t \leq T \leq t+9 \\ 0, \text{if } T \in \{\emptyset\} \cup \{t+10, t+11, \ldots\} \end{cases},$$

for which child i is treated ($D = 1$) only if the mother experiences a layoff while being pregnant. In this way, we effectively contrast outcomes at the first birth observed between displaced and continuously employed pregnant women, without further concerns on their underlying probability of being selected into treatment (since both groups were in employment at conception).

We then estimate the following equation:

$$y_{imt} = \tau D_i + \mathbf{X}_i' \boldsymbol{\beta} + \boldsymbol{\theta}_t + \nu_m + \epsilon_{imt} \tag{1}$$

where y_{imt} is the outcome of interest for child i, born to a mother living in municipality m, conceived at time t. The indicator D_i is equal to 1 if i is exposed in-utero to a maternal layoff, and to 0 if otherwise. $\mathbf{X}_i$ is a vector of maternal and employment characteristics which includes mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, the number of previous live births and stillbirths, race, education and occupation type (blue vs white collar). $\boldsymbol{\theta}_t$ denotes month of conception (both running and calendar) fixed effects, and ν_m denotes municipality of residence fixed effects capturing the different unobservable characteristics that mothers have in a certain geographical area, including the provision of prenatal health care. The error term is expressed by ϵ_{imt}. We allow for clustering of standard errors at the municipality of residence level. The coefficient of interest τ expresses an intention-to-treat effect of maternal displacements on birth outcomes such as BW measures and gestational length, identified if $\mathrm{E}(\epsilon_{imt}|D_i, \mathbf{X}_i, \boldsymbol{\theta}_t, \nu_m) = 0$ holds, i.e. if displacements are exogenous conditional on controls and various fixed effects. We estimate the effect of displacement for the full sample of mothers and by marital status to test for differential impacts along this margin.

As there is a clear interdependence between the determinants of life-cycle marital status and labor force participation decisions of women (Van Der Klaauw, 1996), we expect maternal characteristics to significantly vary between single mothers and mothers in a stable relationship, who we denote as *couples*. While the difference is modest, single mothers are more likely to be displaced during pregnancy (see Table A1). Single and non-single mothers, unsurprisingly, also differ along a number of characteristics like age at delivery, racial composition, educational attainment, wage and tenure at conception. When splitting the sample by marital status, as in Table 1, we find that pregnancy history characteristics, demographic and job variables are very well-balanced across groups for single mothers, with the standardized differences being very small and not exceeding $|.20|$ (Imbens and Rubin, 2015). For mothers in couples, we find that control and treatment groups slightly differ along some demographics and job characteristics: displaced mothers tend to have lower educational attainment, wages and tenure. Both single and couples' control group mothers are more likely to work in larger firms, possibly indicating that workers in smaller firms may be more vulnerable to layoffs, although the normalized differences are still below the reference threshold of $|.20|$.

Our main estimates are extensively probed with alternative specifications, enriched with a full set of fixed effects, and robustness checks that test how sensitive our results are to sample selection and exogeneity assumptions. Importantly, we exploit (varying definitions of) mass layoffs, where a substantial fraction of co-workers are dismissed in the same year, addressing any remaining concerns regarding selection into treatment. We also show trimester-specific estimates in an additional model specification to show whether exposure effects may differ across different periods of gestation. Furthermore, we estimate whether consequences on BW are accompanied by effects on gestational length and prenatal health utilization behavior and delivery choices. Lastly, we present our results on the consequences of maternal dismissals in utero on longer-term outcomes, such as neonatal and infant mortality.

5 The effect of maternal dismissals on birth outcomes

5.1 Effects on birthweight

Table 2 reports the results from equation (1) on the sample of pooled births, and separately by marital status. The outcome variables are BW (in grams) and an indicator for low BW (LBW), with entries from separate regressions. In Columns (1) and (2), we report the results for the full sample of all births, without and with individual level controls, respectively. For this sample, we find no effect on BW; the estimates are very close to zero and not statistically significant. The estimates for the low BW classifications are very close to zero. These estimates on the full sample may nevertheless conceal important heterogeneous effects, which become evident in Columns (3)-(6). In these columns, we report the estimates separately by marital status, first for 'single mothers' (mothers who report in the birth certificate to not be in a relationship), and for 'couples' (mothers who report being in a relationship, either married or another form of stable union).

In contrast to the pooled sample, for single mothers, we find strong negative effects of around 24g, roughly a 0.80% decrease in BW. The inclusion of individual-level controls makes little difference to the estimates, with a minimal absolute increase of 3g in the coefficient, confirming the results on the balancing properties across treated and control (-28g, i.e., -0.89%). Estimating effects along the distribution of BW, we also document an increase in the incidence of low BW of 9 percentage points, a more than 10% increase compared to the baseline, significant at the 5% level. In contrast to the negative effects estimated for single mothers, we find the opposite sign when estimating the effect for mothers in a stable relationship. Dismissal from employment leads to a positive effect on BW for those mothers, with an increase by 30g. For this subsample, the inclusion of the large set of mother characteristics as controls does affect the coefficient, with a reduction by around one third - nonetheless, the accuracy of the estimate is not affected. In line with the effects on BW, we also find (a smaller) decrease in the fraction of children classified as low BW, but the effects are marginally significant. The reversal of the sign of the coefficients in the estimates are striking and may help to shed light on the ambiguous effects found in the literature reporting opposite sign effects (Del Bono et al., 2012; Wüst, 2015), possibly due to differences in the sample compositions and definition of treatment.

We also run regressions with alternative model specifications in Table 3, by enriching equation (1) with additional fixed effects. In Column (1) of Panel (a) and (b), we report the basic specification results for single mothers and couples. We firstly augment the model with municipality of residence-specific trends in Column (2), then we separately include hospital and maternal workplace municipality fixed effects in Column (3) and (4), respectively. Hospital fixed effects control for quality of any prenatal care delivered through the hospital of delivery and quality of delivery services, including scheduling of elective c-section, among other hospital-specific factors. Firm municipality fixed effects control for the local economic situation that may vary by municipality and prenatal care received locally. We also include both hospital and firm municipality fixed effects in Column (5). Finally, we re-run the regression with all the previous fixed effects plus industry fixed effects of maternal employment. Overall, this exercise demonstrates the stability of the effect of maternal displacement on BW and LBW, with only small differences in the estimates and precision.

5.1.1 Robustness checks

We repeat the analysis in Table 2 by changing the matching score (originally, 98%) to check whether the findings are sensitive to the quality of the match when linking the two datasets.

In Panel (a) and (b) of Table B1, we report the results for BW and LBW measures on the same samples (all births, single mothers and couples) when setting the minimum matching score to 0.97 and 0.99 respectively (i.e., at slightly less and slightly more stringent minimum matching score). Overall, the uncontrolled and controlled specifications in Table B1 yield extremely similar results compared to the results based on our preferred matching score, with minimal variation in magnitudes and significance of the estimates. Hence, we can conclude that our estimates are not produced by our linkage strategy.

In a second robustness check, we use dismissals exclusively from mass layoffs. We focus on firms with at least 10 employees and vary the fraction of dismissed in the mass layoff between more than 33% and more than 50% of workers in one calendar year. This heavily reduces the overall sample of births but treated and control groups are affected disproportionately: when using the 33% threshold, we lose 80% in treated births and 30% in control ones, for both single and non-single mother groups. In Table B2 we report the effects from this exercise. We find a very similar pattern to the one documented in Table 2, a negative effect of between 22 and 36 grams reduction in BW for single mothers, and a positive effect for non-single mothers of between 32 and 46 grams. However, the coefficients for single mothers are not statistically significant - expectedly, given the reduction in the number of treated observations compared to the main specification in Table 2. These alternative estimates based on mass layoffs confirming the results for regular dismissals are nevertheless very reassuring.

There are two final concerns that we address in this section:

(I) The main findings could be driven by dismissals later in pregnancy, with those dismissals being subject to reverse causality, i.e. pregnant mothers with complicated pregnancies being selected into a layoff. This might happen if mothers with generally worse health conditions are selected for dismissals, for example, due to relatively lower productivity. Nevertheless, we expect the vast majority of dismissals to happen early in pregnancy when mothers may not be aware yet of the pregnancy, or the pregnancy is yet to be confirmed by a physician (as pregnant women are protected from dismissals once the employer is informed about the pregnancy, apart from special firm circumstances, i.e. plant closure). The data confirms this: dismissals of pregnant employees observed in the data happen early in pregnancy, with around 90% of layoffs happening in the first trimester and of those almost 40% in the first month of gestation.

(II) Since layoffs are typically preceded by an advance notice required to be released at least 30 days before the actual separation date, there may theoretically be cases in which mothers, aware of the future layoff, select into pregnancy to challenge the dismissal.

To address these two concerns, we also estimate the effect of layoff during pregnancy on BW by trimester of dismissal and report results in Figure B1. We construct the trimesters by dropping the cases in which the displacement month is the same as the conception month ($T = t$) to exclude any possible selection into pregnancy and divide the remaining months into trimesters. Consistent with the previous estimates and the two diverging effects, we find a positive effect on BW for couples for exposure in the first trimester and a negative effect for single mothers. For later exposure, the negative effect for single mothers is much more pronounced, and the positive effect for couples is reduced. This is consistent with the positive channel having a stronger effect for earlier exposure rather than for later exposure - reducing the time the positive effect can impact BW positively through longer gestation. The results for dismissals in the third trimester are less precise because of the small number of dismissals we have at our disposal.

5.2 Additional outcomes

We also look at other potentially interesting outcomes at birth such as the incidence of emergency c-sections (among mothers who did not choose to have a planned c-section), and

Apgar scores taken after 1 and 5 minutes from delivery in Table B3.[8] Noticeably, emergency c-sections are significantly reduced among displaced mothers in stable relationships by 4%. Although the patterns of effects on Apgar scores confirm the tendency of treated single-mother (couples') babies to have worse (better) health at birth, the estimates on Apgar scores are not statistically significant.

As BW accumulates over the gestational period, gestational length is a key determinant of BW, and we are hence interested in the effect of displacements on gestational length. In Table 4, we report the estimated effect of maternal layoff on gestational length. For the full sample of births in Column (1), we observe an increase of 1 day in gestational length, with comparatively small differences across single mothers and couples, which is in stark contrast to the finding of the effects on BW by marital status. More substantially, we find a significant decrease in the incidence of preterm birth (< 37 weeks) by around 11.5% and a marginally significant reduction by 16% in very preterm birth (< 32 weeks) rate for couples. Given results in the literature on the positive association between less strenuous work conditions (or maternity leave) and gestational length, our results are unsurprising. Our findings highlight two important points: (i) exogenous job interruptions (which are due to involuntary displacements, unlike the take-up of maternity leave) may equally lead to benefits in gestational length (ii) the differential impacts of BW by marital status do not derive from any differential effect on gestational length.

We further test whether displaced single mothers present a different prenatal health utilization behavior or make different delivery-related choices compared to their non-single counterparts. In Table 5, we find that both displaced single and non-single mothers tend to delay the first prenatal visit, which on average happens between the second and third month of pregnancy, with the latter delaying it to a lesser extent. Analogously, maternal dismissals are associated with lower attendance to prenatal visits (which are offered for free from the public health sector), although the attendance is on average very high - at least 8

[8] The Apgar score is based on a doctor's observation of the baby's skin color, heart rate, reflexes, muscle tone, and breathing shortly after birth, and is reported on a 0–10 scale. Scores below 7 are considered low.

visits. Again, displaced mothers in couples reduce less their attendance compared to their single counterparts. We also estimate the effect of a dismissal during pregnancy on other important delivery-related choices: delivery in a private clinic and take-up of a planned c-section. We document a reduction by 33% and 15% in deliveries occurring in private clinics and with planned c-sections, respectively, for displaced single mothers. Similarly, dismissed non-single mothers are less likely to go to a private clinic and opt for a planned c-section by 15% and 5%, respectively. While we find a similar size in coefficients for use of private clinics, given the higher uptake of private clinics for delivery and planned c-section of non-single mothers, the effect size is about half for private clinic use and only a third for planned c-section. We will discuss the potential reasons for the differential effects in detail in the following section. The number of prenatal visits and their timing, as well as the use of private health services and planned c-section may contribute to the poorer health at birth in a number of ways. There is evidence of a positive relationship between early and regular prenatal care attendance and health at birth, as in Abrevaya and Dahl (2008) and Qian (2022). Whether the effect of c-section on children's health is not well established; Card et al. (2019) causally estimate an increased incidence of acute respiratory conditions for infants born in hospitals with high c-section rates, but a reduction in infant mortality.

Lastly, we estimate the effect of layoffs on infant mortality. To do so, we use the link between mortality records and birth records and estimate separately the effect on deaths occurring within seven days, 28 days, 22 weeks during the early neonatal, neonatal, perinatal period, respectively, and within one year, defined as infant mortality. We report the coefficients in Table 6. We find that maternal layoffs increase the risk for early neonatal mortality substantially for children born to single mothers (column 2), while we find no effect for non-single mothers (column 3). The coefficients for early neonatal mortality (0.2 percentage points) point to a very substantial increase of about two thirds compared to the mean incidence of the risk for very early neonatal death. The coefficient for neonatal death is identical, suggesting that there is no further increase in death in the subsequent three weeks following the first week of life. This is consistent with maternal layoffs leading to a negative health effect during gestation or during delivery, in line with the findings on the negative effect on BW for infants born to single mothers affected by layoffs and the change in delivery provider and delivery type discussed above. Subsequently, there is small increase in the coefficients for death occurring in the perinatal period and over the entire first year of life, possibly indicating a small additional effect on mortality over these later periods in early life. We cannot distinguish though whether these effects on mortality are the result of the longer-term negative consequences of maternal layoffs in-utero or the result of contemporaneous effects due to the longer-term consequences of the layoff, for example due to a prolonged state of maternal unemployment and scarcity of resources. Overall, these effects are striking and point to very sizable increase in mortality of children born to single mothers who lose their job during pregnancy pointing to a severe penalty and reduced survival chances of those children, while we find no effect for non-single mothers.

6 The mitigating role of partners and financial resources

In this section we investigate the role of partners, and informal and formal insurance mitigating the negative shocks of dismissal, starting with the role of informal insurance provided by the presence of partners.

6.1 Informal insurance through partners

Having established the opposing role of marital status when estimating the effect of maternal displacements during pregnancy on BW, we probe the potential mitigating role of the presence of the child's father. For this purpose, we make use of the rich information we have available in the birth records and employment records from RAIS and further explore the heterogeneity by marital status. Beyond marital status, in the vital statistics data, we observe whether mothers have declared the identity of the newborn's father; if they do, we match the birth record (and mother) to the father's record in RAIS. We then re-estimate model (1) for the following subsamples within single mothers' and couples' infants, respectively: children with no declared father at birth and children with a declared father at birth. Among the latter group, we further distinguish between those whose father has a successful match in RAIS for the pregnancy period, suggesting that these fathers are in formal employment, and those whose father cannot be linked. In Table 7, we repeat the estimations of maternal displacement effects by the subsamples as defined above. Columns (1) to (4) report regression results on single-mother and couples' children subsamples in Panel (a) and (b), respectively. Starting with single mothers that do not declare a father in the birth record, we find a slightly more pronounced effect on BW of about 34g in Column (1). The effect is slightly smaller and noisier for mothers who declare a father in the birth records, but where we cannot link a father to the employment records, in Column (2). In Columns (3) and (4), we estimate the effect on the sample of births, for which we can link the declared father to the employment records of RAIS, and where we also control for paternal wage, respectively. We find a much reduced negative effect of around 10 grams for both specifications, but the effects are not significant. In Panel (b), we repeat the exercise for women in a stable relationship, finding a very similar pattern. We find a smaller positive and insignificant effect for mothers whose marital status is non-single, but where there is no declaration of the child's father in the birth certificate. This pattern continues with a strengthening of the positive effect for births with a declared father (21g), which is slightly larger than for the whole sample of non-single mothers in Table 2. For births, for which we can link the father to the employment record, the effect is much more pronounced, with an increase of about 33 grams, around a 50% increase compared to the benchmark result.

This constitutes a striking pattern; birth outcomes for mothers are less negative/more positive the stronger the link and capacity of a partner to make an economic contribution in the partnership. The result is largely unaffected when controlling for paternal wage for the sample of mothers, where we can link the father in the employment record. The pattern is much less evident for the outcome of LBW. We do not find an equivalent pattern for the results on LBW for the single mother subsamples, while the reduction in LBW for couples' children is increasing - however, most of these estimates are not significant.

In Tables C1-C3, we assess how maternal layoff effects on other outcomes vary across these subsamples. Generally, gestational length increases more for mothers with a declared father, especially if linked in RAIS. The effect sizes on timing of first visit and attendance tend to be stable across subsamples, with the exception of the decrease in attendance, which is significantly smaller for single mothers declaring the child's father, and even more if the latter is found in RAIS. Similarly, effect sizes on private clinic and planned c-section choices do not vary much across subsamples of single mothers. Interestingly, the effects on planned c-section take-up are (non-significantly) positive for mothers in couples where the identified father is found in RAIS - possibly, pointing at a greater availability of resources coming from partners for these women.

6.2 Informal insurance through own resources

We are also interested to learn about whether financial resources of the expecting mothers moderate the negative impact of layoffs. In the absence of direct measures of wealth, we proxy it with maternal earnings and the size of Severance Pay mothers are eligible for. These two measures potentially look at different ways of self-insurance. Maternal earnings allow individuals to smooth consumption after job losses using voluntary savings, while Severance Pay is provided by the employer, which are more akin to forced savings. While we don't know whether maternal earnings are translated into savings, as we have no information on assets of the household, we can calculate the precise amount of the Severance Pay from the full employment history of the worker. To investigate the effect of maternal earnings and severance pay, we then separately estimate the effect of layoffs on the number of outcomes separately by earnings and severance pay quartiles and plot the coefficients in Figure 1 for single mothers and Figure 2 for non-single mothers.[9] Starting with maternal earnings in panel (a) for single mothers, we document a clear gradient by earnings for our birth outcomes of BW, low BW and gestational length. The negative effect of maternal layoffs are much more pronounced among the lowest earnings quartile, whereas there is no effect for the highest earnings quartiles, indicative of a mitigating effect of financial resources in the aftermath of dismissals that possibly works through savings. We find a similar pattern for preterm birth, timing and total number of prenatal visits, with the lowest quartile being most affected. The most striking pattern by earnings quartiles we find for the use of private health services for the delivery and planned c-sections. We find that the use of private health services is substantially lower for quartiles 1-3 in the aftermath of a layoff, whereas we find that for the top quartile there is a substantial and statistically significant increase in the use of private health services for delivery. We find a similar, but slightly less pronounced pattern for planned c-section. This pattern is largely repeated for single mothers when focusing on Severance Pay. Lower earnings quartiles are affected more by dismissals compared to the highest earnings quartile with a very pronounced gradient for private health services use and planned c-section, indicating a reversal in the sign when comparing the lowest with the highest quartile. These results are striking and demonstrate that the overall negative coefficient on private health care utilization and planned c-section in chart (b) of Table 5 is not universal across earnings quartiles, in line with a differential use of savings or Severance Pay by quartile. Indeed, as Severance Pay provides some mothers with a lump-sum payment leading to a positive income shock in the short-term, which may be used to pay for the use of private health delivery services and planned c-section deliveries. This is in line with the findings in Gerard and Naritomi (2020) who show that workers in Brazil increase their spending after layoffs.

We repeat the exercise for non-single mothers in Figure 2. Overall, we find a similar

[9]There are some limitations to this exercise beyond the preference for a more direct measure of wealth. For example, beyond the capacity for savings, wage income is correlated with different types of occupations and dismissals from different occupations may have a differential impact on birth outcomes, even in the absence of moderating effect of wages.

picture, with some gradients more pronounced and cleaner. For example, preterm birth, timing of first prenatal visit, number of prenatal visits and private health service and c-section all reveal a very clear gradient – both for earnings and Severance Pay – including the previously observed sign reversals.

6.3 Formal insurance through unemployment benefits

The results in the previous sections establish that resource availability either through the presence of declared father, or possibility to use own resources (like wage or the Severance Pay), mitigates or even reverts the adverse impacts of a job displacement during pregnancy. However, so far we have not been to disentangle socioeconomic status from resource availability to conclude whether there is an actual resource constraint problem arising for displaced pregnant workers. From a policy perspective, it is thus important to understand whether traditional public policies supporting unemployed workers can provide – at least in part – a formal insurance. In this section, we investigate the effect of unemployment benefits, which is the main policy aimed at supporting displaced workers.

Brazilian workers are eligible for 3-5 months of unemployment benefits when dismissed without a just cause from a formal job, conditional on continuous employment in the six months prior to the layoff.[10] The benefit level is a function of the average wage in the three months preceding dismissal and ranges from 100 percent to 187 percent of the minimum wage - and hence, high-earning workers are expected to receive relatively much less compared to low-earning ones.

We focus on the 2011-14 period and restrict our initial sample – children of working mothers holding open-ended jobs in the private sector – to include only children of dismissed workers. Then, we compare the birth outcomes of working mothers who are eligible and

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

non-eligible for UI benefits after a displacement by estimating the following equation:

$$y_{imt} = \lambda_1 \mathrm{UI}_{it} + \lambda_2 \mathrm{UI}_{it} \times Wage_i + f(Tenure_i) + \mathbf{X}_i' \mathbf{\Gamma} + \boldsymbol{\theta}_t + \nu_m + \epsilon_{imt}, \tag{2}$$

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

The coefficient λ_1 in equation (3) estimates the effect of UI eligibility, or equivalently, the intention-to-treat effect of UI claims. We also specify an interaction term between earnings and UI eligibility. This offers the opportunity to allow the effect of UI entitlement to differ across the (foregone) wage distribution, based on the intuition that receiving a cash transfer for displaced pregnant mothers in the left tail of the labor income distribution provides a more tangible benefit during economic hardship compared to the ones in the right tail of the distribution, who may have sufficient savings and have a lower replacement rate from the cash transfer.

The first concern with RD estimates is possible manipulation of the running variable, which in the present case may, for example, arise because six months of job tenure may be a salience point for evaluating employees' performance (Gerard and Gonzaga, 2021). A key assumption for the validity of RD research design is that the distribution of individuals' potential outcomes varies continuously with the running variable around the cutoff. This ensures that the only systematic difference between units close to but on different sides of the cutoff is their treatment assignment. Hence, a jump in the density of the running variable at the cutoff is argued to be a strong indication of manipulation (McCrary, 2008).

Figure D1 shows no evidence of density discontinuity around the 6-month cutoff for neither single mothers nor couples, as also confirmed by the bias robust test with local polynomial density estimators developed in Cattaneo et al. (2020). In figures D2 and D3, we also provide further balancing tests for the range of covariates among worker's characteristics. Overall, these checks support the assumption of no manipulation of the running variable and balancedness in characteristics of mothers on either side of the UI eligibility cutoff.

The estimates from our RD design are reported in Table 8, where we show the results of separate regressions run by marital status. The first two columns contain the results on BW for single-mother children with varying polynomial order, while the last two are based on couples' children. Regarding the effect of UI eligibility, while we find that UI provides a strong mitigating effect protecting the health of unborn children to single-mothers, the same does not apply to children from mothers in stable relationship.[11] From the estimates in Columns (1) and (2), we notice a positive effect on BW for children of mothers eligible for unemployment benefits (+70g). The effect of maternal eligibility to UI is also negative for LBW incidence among single mothers, but it is not statistically significant. In contrast, mothers in couples with entitlement to withdraw UI are largely not affected by eligibility for UI. For a deeper insight into the mechanisms of UI reception, we also notice that the effects of UI eligibility for single-mother children are decreasing in foregone earnings. The total effect on BW for an eligible median-earning displaced worker (≈R$ 790) results in a net gain of 36.5 grams (i.e., the benefits of being financially supported and resting more than offset the original loss in BW from displacement).

In Table D1, we reproduce the analysis in Table 6 by assessing the robustness of the estimates. The estimated effect of UI eligibility on BW for single-mother children appears stable when $f(Tenure_i)$ is specified to be a cubic polynomial in Column (1); the point estimate is even higher (+85g) when we include hospital, firm municipality and sector fixed effects in Column (2). As manipulation and intentional misreporting of job tenure

[11] The greater importance of cash benefits reception for birth outcomes to more vulnerable mothers has been recently highlighted by Hoynes et al. (2015) and Amarante et al. (2016), among others.

could yet be a concern for our RD estimates, we re-estimate the effect of UI on a sample that omits mothers with six months of continuous employment at layoff: the result is a slightly lower and noisier estimate of the effect of UI eligibility (+62g), but still significant at 10%. No relevant effects are found on LBW for single-mother children nor birth outcomes among couples.

In tables D2 and D3, we estimate whether UI eligibility impacts other margins. Gestational length appears to be significantly reduced by around 2 days for eligible mothers in couples, contrarily to the 1 day increase reported for single mothers (although insignificantly). We do not observe any other significant effect on prenatal care-related behaviors nor type of clinic chosen for delivery. Notably, we find that UI increases the planned c-section take up for both eligible single and non-single mother, but only the former show a significant increment (by around 20%).

Overall, these results are consistent with the evidence suggesting that liquidity constraints may be an important driver of BW decrease following a job loss for single mothers. With lesser resources to draw upon, the most vulnerable mothers face the economic shock from the loss of income, with the potential detrimental effect on the prenatal environment and newborns' health. Providing these single-mother households with a cash transfer after displacement can, instead, mitigate the negative consequences and possibly be even beneficial for the unborn children's health (possibly relieving mothers from strenuous activities at work).

7 Conclusion

In this paper, we estimate the effect of maternal displacements during pregnancy on children's health at birth by combining two large sets of administrative microdata from Brazil. We contribute to the literature on the externalities of displacements and identify spillovers onto unborn children: we reconcile the empirical literature findings on this topic by leveraging uniquely suitable data on individual employment spells from social security data linked to administrative birth records. Our results demonstrate that plausibly exogenous job losses can imply opposing effects for different household types. We are the first to provide evidence of these opposing effects of layoffs on pregnancy outcomes by marital status. We estimate that children from single mothers exposed to maternal displacement during pregnancy have a BW reduced by about 30 grams on average. In comparison, children born to couples where the mother is dismissed tend to have higher BW (20g). We provide a battery of robustness checks to probe our estimates, including the restriction to dismissals occurring during mass layoffs.

We also provide evidence on potential channels underlying the opposing effects by marital status and mediating factors. We document a positive effect of dismissals on gestational length, generally expected to positively affect BW, both for single mothers and for mothers in a couple. Displaced mothers also tend to delay their first prenatal care visit and attend less visits. Mothers dismissed during pregnancy are also less likely to deliver in private clinics and choose a c-section.

In combination with a negative effect from the loss of income and the potential stress associated with the dismissal, this leads to opposing effects of dismissals on birth outcomes by marital status. We find that the effects vary by the quality of the paternal involvement, both for single mothers and mothers in a stable relationship. The negative effects are more pronounced for single mothers without information on the father in the birth certificate, while labor market attachment of the father reduces the negative effect. We find a similar pattern for mothers in a relationship, but starting with an insignificant positive effect. Thus, a declared father in birth records might provide informal insurance and buffer against the income shock for example through the sharing of resources. We also document more pronounced negative effects for single mothers with lower earnings and no effect for mothers in the highest income quartile. A similar gradient in available resources (proxyed by last wage level or estimated Severance Pay) arises for mothers in couples. This overall suggests a mitigating role of self-insurance from savings.

In addition to documenting the mitigating role of informal insurance through the presence of a declared child's father or own maternal resources, we also provide additional evidence on the role of formal insurance by estimating the effect of unemployment benefits eligibility on birth outcomes in an RD setting, exploiting a sharp UI eligibility cutoff. We find that UI counteracts the negative impact of job loss on BW of children of single mothers but does not have any effect on non-single mothers.

The effects we document in this paper inform about the significant intergenerational externalities of maternal job loss and the important role of informal and formal insurance mechanisms. As informal insurance depends on maternal wages or on the presence of a partner in the household, this raises the importance of formal UI for single mothers and for mothers (and partners) on low incomes, documenting an important role of Unemployment Insurance for the health of unborn children.

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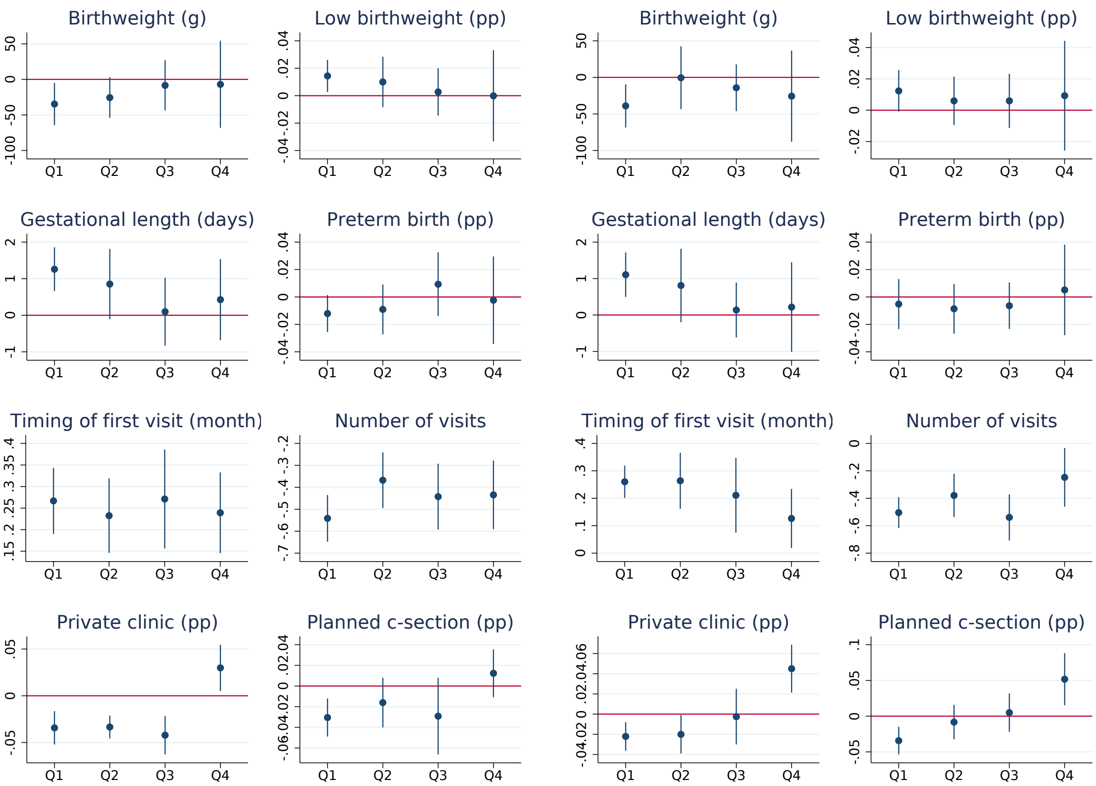
Figures

Figure 1: Effect of maternal layoff by maternal earnings and Severance Pay quartile (single mothers)

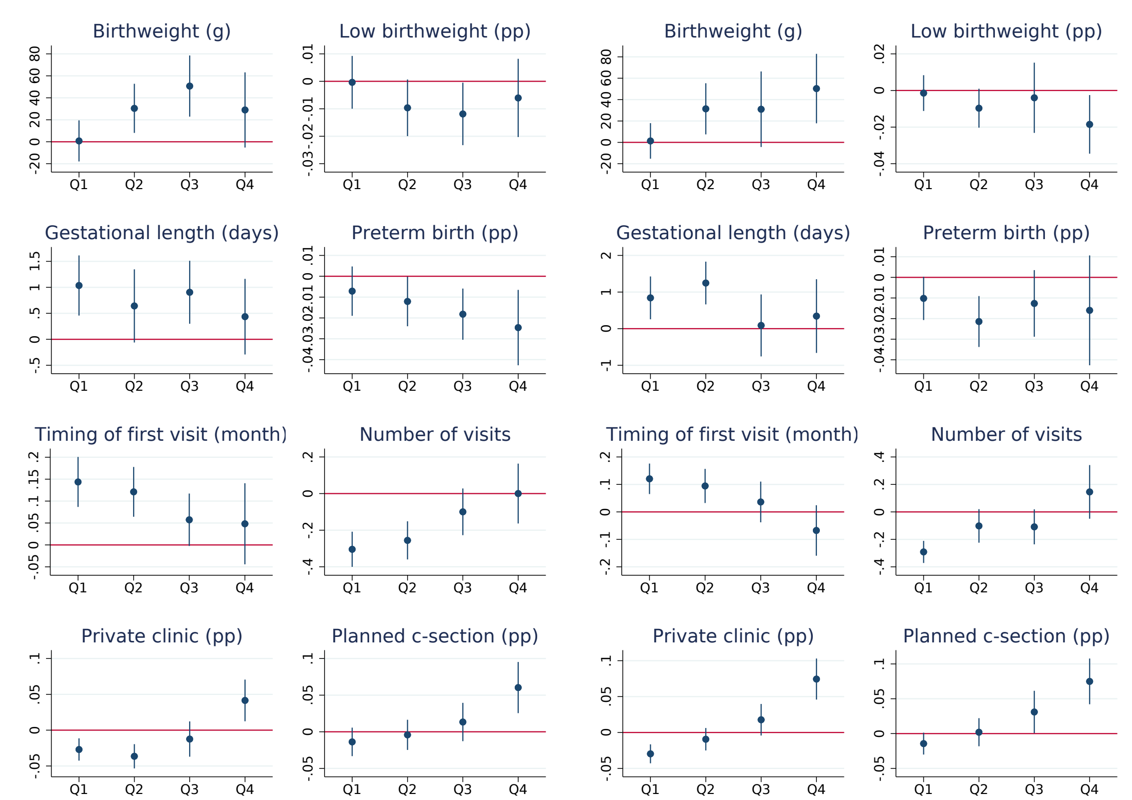
(a) Maternal earnings (b) Severance Pay



Note: The figure shows the effect of maternal layoff on single-mother births over the period between 2011 and 2014, by maternal monthly wage (R$ 2014) and lump-sum SP quartile in Panel (a) and (b) respectively. On the vertical axis, we display the coefficients (and 95% CI) from regressions of outcome variables on a maternal layoff indicator for exposure during pregnancy for single-mother births. On the horizontal axis, we display the quartile for which the regressions are run. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar). 95% CI are constructed with robust standard errors clustered at the municipality of residence level.

Figure 2: Effect of maternal layoff by maternal earnings and Severance Pay quartile (couples)

(a) Maternal earnings (b) Severance Pay



Note: The figure shows the effect of maternal layoff on births to couples over the period between 2011 and 2014, by maternal monthly wage (R$ 2014) and lump-sum SP quartile in Panel (a) and (b) respectively. On the vertical axis, we display the coefficients (and 95% CI) from regressions of outcome variables on a maternal layoff indicator for exposure during pregnancy for single-mother births. On the horizontal axis, we display the quartile for which the regressions are run. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar). 95% CI are constructed with robust standard errors clustered at the municipality of residence level.

Tables

Table 1: Balance check on first observed birth by marital status

	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers			Couples		
Variable	Control	Treatment	Std Diff	Control	Treatment	Std Diff
Previous pregnancies						
N. of previous live births	0.665	0.849	-0.179	0.567	0.745	-0.181
	(0.014)	(0.021)		(0.018)	(0.019)	
N. of previous stillbirths	0.162	0.182	-0.044	0.186	0.204	-0.030
	(0.003)	(0.006)		(0.002)	(0.007)	
Demographics						
Age	26.564	25.697	0.152	29.496	28.111	0.266
	(0.147)	(0.113)		(0.284)	(0.212)	
Race - white	0.311	0.277	0.073	0.453	0.407	0.093
	(0.026)	(0.029)		(0.021)	(0.021)	
Race - mixed	0.534	0.559	-0.050	0.414	0.453	-0.078
	(0.034)	(0.041)		(0.021)	(0.023)	
Race - black	0.095	0.100	-0.018	0.058	0.065	-0.026
	(0.007)	(0.007)		(0.003)	(0.005)	
Secondary education	0.714	0.685	0.064	0.632	0.681	-0.102
	(0.007)	(0.012)		(0.019)	(0.008)	
Higher education	0.082	0.047	0.129	0.255	0.140	0.267
	(0.006)	(0.004)		(0.030)	(0.013)	
Job characteristics						
Blue collar	0.041	0.043	-0.013	0.033	0.038	-0.031
	(0.007)	(0.008)		(0.005)	(0.005)	
Weekly hours	42.397	42.616	-0.048	42.022	42.723	-0.129
	(0.122)	(0.091)		(0.170)	(0.089)	
Monthly wage (R$ 2014)	1043.208	924.833	0.158	1479.708	1082.447	0.266
	(32.716)	(14.483)		(152.753)	(57.752)	
Tenure at conception	17.111	13.560	0.153	29.083	19.619	0.285
	(0.236)	(0.286)		(0.656)	(0.308)	
Firm size	1058.502	662.342	0.113	872.171	489.909	0.118
	(146.687)	(97.235)		(146.123)	(71.217)	
Observations	51220	4744		98074	6501	

Note: This table reports the average characteristics on previous pregnancies, demographic and employment information at the first observed birth in our single mothers and couples dataset. The values displayed for Std Diff in Columns (3) and (6) are the pairwise normalized differences in the means across the groups, (1)-(2) and (4)-(5), respectively. Robust standard errors clustered at the municipality of residence level in parentheses. Municipality of residence and month of conception (both running and calendar month) fixed effects are included.

Table 2: Effect of maternal layoff on birthweight

	(1)	(2)	(3)	(4)	(5)	(6)
	All births		Single mothers		Couples	
Birthweight	6.588	-1.304	-24.400**	-27.984**	30.059***	18.497***
	(5.061)	(4.984)	(11.806)	(11.757)	(6.385)	(6.459)
	[3155.253]	[3155.253]	[3146.038]	[3146.038]	[3159.413]	[3159.413]
Low birthweight	0.001	0.002	0.008*	0.009**	-0.006*	-0.003
	(0.003)	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)
	[0.080]	[0.080]	[0.085]	[0.085]	[0.077]	[0.077]
Controls		Y		Y		Y
Observations	165773	165773	55964	55964	104575	104575

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* is a dummy for newborns up to 2,500 grams. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table 3: Effect of maternal layoff on birthweight (alternative specifications)

	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Single mothers					
Birthweight	-27.984**	-29.660**	-30.274**	-29.663**	-30.584***	-30.416**
	(11.757)	(12.209)	(11.724)	(12.096)	(11.756)	(11.820)
	[3146.038]	[3146.038]	[3146.350]	[3145.935]	[3146.203]	[3146.209]
Low birthweight	0.009**	0.010**	0.009*	0.010**	0.009**	0.009**
	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
	[0.085]	[0.085]	[0.085]	[0.085]	[0.085]	[0.085]
Observations	55964	55964	55883	55810	55726	55725
	(b) Couples					
Birthweight	18.497***	17.689***	17.882***	16.616**	16.660**	16.122**
	(6.459)	(6.461)	(6.474)	(6.496)	(6.513)	(6.481)
	[3159.413]	[3159.413]	[3159.605]	[3159.415]	[3159.607]	[3159.601]
Low birthweight	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
	[0.077]	[0.077]	[0.077]	[0.077]	[0.077]	[0.077]
Observations	104575	104575	104480	104512	104416	104414
Controls	Y	Y	Y	Y	Y	Y
Running month FE	Y	Y	Y	Y	Y	Y
Calendar month FE	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y
Municipality-specific trends		Y	Y	Y	Y	Y
Hospital FE			Y		Y	Y
Firm municipality FE				Y	Y	Y
Firm sector FE						Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* is a dummy for newborns up to 2,500 grams. This table repeats the analysis in Table 2 by enriching the original specification with different sets of fixed effects. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table 4: Effect of maternal layoff on gestational length

	(1) All births	(2) Single mothers	(3) Couples
Gestational length (days)	0.986***	0.907***	1.128***
	(0.118)	(0.211)	(0.176)
	[268.866]	[269.633]	[268.476]
Preterm birth	-0.007**	-0.003	-0.011***
	(0.003)	(0.006)	(0.003)
	[0.098]	[0.103]	[0.096]
Very preterm birth	-0.001	-0.001	-0.002*
	(0.001)	(0.002)	(0.001)
	[0.013]	[0.016]	[0.012]
Controls	Y	Y	Y
Observations	165773	55964	104575

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Gestational length* is reported in days. *Preterm birth* and *Very preterm birth* are dummies for gestational length less than 37 and 32 weeks, respectively. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table 5: Effect of maternal layoff on prenatal care and delivery choices

	(1) All births	(2) Single mothers	(3) Couples
	(a) Prenatal care		
Timing of first visit	0.191***	0.282***	0.129***
	(0.016)	(0.020)	(0.017)
	[2.196]	[2.454]	[2.055]
Number of visits	-0.345***	-0.503***	-0.233***
	(0.028)	(0.040)	(0.027)
	[8.510]	[8.077]	[8.743]
Controls	Y	Y	Y
Observations	158217	53090	100219
	(b) Delivery provider and type		
Private clinic	-0.061***	-0.069***	-0.056***
	(0.012)	(0.014)	(0.010)
	[0.308]	[0.206]	[0.365]
Planned c-section	-0.035***	-0.048***	-0.025***
	(0.004)	(0.006)	(0.005)
	[0.422]	[0.327]	[0.472]
Controls	Y	Y	Y
Observations	165773	55964	104575

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. The sample in Panel (a) is restricted to births with a reasonable number of prenatal visits (≤ 18) and available information on month of the first prenatal visit. *Timing of first prenatal visit* is the month of pregnancy in which the first prenatal visit occurs. *Private clinic* is a dummy for deliveries in private clinics. *Planned c-section* is a dummy for c-sections before labor. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table 6: Effect of maternal layoff on infant mortality

	(1) All births	(2) Single mothers	(3) Couples
Early neonatal mortality	0.001	0.002**	-0.000
	(0.000)	(0.001)	(0.001)
	[0.003]	[0.003]	[0.003]
Neonatal mortality	0.001*	0.002**	0.000
	(0.001)	(0.001)	(0.001)
	[0.003]	[0.004]	[0.003]
Perinatal mortality	0.001*	0.003**	-0.000
	(0.001)	(0.001)	(0.001)
	[0.004]	[0.004]	[0.004]
Infant mortality	0.001*	0.003**	-0.000
	(0.001)	(0.001)	(0.001)
	[0.004]	[0.005]	[0.004]
Controls	Y	Y	Y
Observations	165773	55964	104575

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Early neonatal mortality*, *Neonatal mortality*, *Perinatal mortality* and *Infant mortality* are dummies for mortality within 7 days of birth, 28 days, 22 weeks and 1 year, respectively. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table 7: Effect of maternal layoff on birthweight by availability of paternal information

	(1)	(2)	(3)	(4)
	(a) Single mothers			
Birthweight	-33.710**	-26.033*	-14.124	-14.353
	(13.449)	(15.004)	(19.669)	(19.604)
	[3149.914]	[3144.082]	[3144.244]	[3144.244]
Low birthweight	0.010*	0.009	-0.011	-0.011
	(0.005)	(0.007)	(0.010)	(0.010)
	[0.091]	[0.081]	[0.079]	[0.079]
Observations	23007	32756	11933	11933
	(b) Couples			
Birthweight	10.898	21.242***	33.753***	33.415***
	(10.042)	(7.811)	(10.400)	(10.379)
	[3179.504]	[3153.757]	[3147.543]	[3147.543]
Low birthweight	-0.002	-0.003	-0.008	-0.007
	(0.006)	(0.004)	(0.005)	(0.005)
	[0.077]	[0.076]	[0.079]	[0.079]
Observations	23623	80775	35847	35847
Conditions:				
Father declared	N	Y	Y	Y
Father linked in RAIS			Y	Y
Controls	Y	Y	Y	Y
+ Paternal wage				Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* is a dummy for newborns up to 2,500 grams. Column (1) reports the coefficients from regressions of outcome variables on a maternal layoff indicator for exposure during pregnancy for the sample of births with no father declared. Column (2) is as in (1), but for the sample of births with a declared father. Column (3) is as in (2), but with a successfully matched father in RAIS, and Column (4) is as in (3), but it also includes paternal wage (R$ 2014) in the set of controls. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table 8: Effect of UI eligibility on birthweight

	(1)	(2)	(3)	(4)
	Single mothers		Couples	
	Linear	Quadratic	Linear	Quadratic
DEP. VAR.: *Birthweight*				
UI Eligibility	68.048***	69.449**	-9.322	-6.541
	(25.912)	(29.523)	(49.626)	(50.681)
UI Eligibility ×	-0.040**	-0.040**	0.020	0.020
Monthly wage (R$ 2014)	(0.018)	(0.018)	(0.049)	(0.049)
Mean	3124.915	3124.915	3188.659	3188.659
DEP. VAR.: *Low birthweight*				
UI Eligibility	-0.014	-0.017	-0.005	-0.005
	(0.014)	(0.016)	(0.024)	(0.025)
UI Eligibility ×	0.000	0.000	-0.000	-0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	0.091	0.091	0.072	0.072
Controls	Y	Y	Y	Y
Observations	4566	4566	6306	6306

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* is a dummy for newborns up to 2,500 grams. The sample includes workers displaced during pregnancy. Columns (1)-(3) and (2)-(4) report the coefficients from regressions of outcome variables on a dummy for UI eligibility (i.e., $Tenure_i \geq 6$ months) and an interaction term between UI eligibility and monthly wage (R$ 2014) for specifications with a running and a quadratic polynomial in tenure at separation, respectively. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014) and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

A Appendix to Section 4

Table A1: Balance check on first observed birth: single mothers vs couples

	(1)	(2)	(3)
Variable	Couples	Single mothers	Std Diff
Laid-off	0.062	0.085	-0.089
	(0.003)	(0.002)	
Previous pregnancies			
N. of previous live births	0.578	0.681	-0.103
	(0.018)	(0.014)	
N. of previous stillbirths	0.188	0.164	0.044
	(0.002)	(0.003)	
Demographics			
Age	29.410	26.491	0.525
	(0.282)	(0.140)	
Race - white	0.450	0.308	0.290
	(0.021)	(0.026)	
Race - mixed	0.417	0.536	-0.239
	(0.021)	(0.035)	
Race - black	0.059	0.095	-0.140
	(0.003)	(0.007)	
Secondary education	0.635	0.711	-0.161
	(0.018)	(0.007)	
Higher education	0.248	0.079	0.433
	(0.029)	(0.006)	
Job characteristics			
Blue collar	0.033	0.041	-0.043
	(0.005)	(0.007)	
Weekly hours	42.066	42.416	-0.068
	(0.165)	(0.118)	
Monthly wage (R$ 2014)	1455.012	1033.174	0.325
	(147.858)	(31.055)	
Tenure at conception	28.495	16.810	0.382
	(0.648)	(0.219)	
Firm size	848.407	1024.920	-0.053
	(142.226)	(142.345)	
Observations	104575	55964	

Note: This table reports the average characteristics on displacement propensity, previous pregnancies, demographic and employment information at the first observed birth in our dataset. The values displayed for Std Diff in Column (3) are the pairwise normalized differences in the means across the groups (1)-(2). Robust standard errors clustered at the municipality of residence level in parentheses. Municipality of residence and month of conception (both running and calendar month) fixed effects are included.

B Appendix to Section 5

B.1 Robustness checks on birthweight results

Table B1: Effect of maternal layoff on birthweight by matching score

	(1)	(2)	(3)	(4)	(5)	(6)
	All births		Single mothers		Couples	
	(a) Match score $\geq$ 0.97					
Birthweight	5.623	-1.961	-25.717**	-28.823***	27.705***	16.324***
	(4.642)	(4.553)	(11.241)	(11.019)	(6.030)	(6.127)
	[3155.707]	[3155.707]	[3145.558]	[3145.558]	[3160.423]	[3160.423]
Low birthweight	0.001	0.003	0.009**	0.010**	-0.005	-0.002
	(0.003)	(0.002)	(0.004)	(0.004)	(0.003)	(0.003)
	[0.080]	[0.080]	[0.086]	[0.086]	[0.077]	[0.077]
Controls		Y		Y		Y
Observations	175335	175335	59078	59078	110723	110723
	(b) Match score $\geq$ 0.99					
Birthweight	4.842	-3.903	-26.764**	-31.723**	28.605***	16.572**
	(5.530)	(5.402)	(13.015)	(13.056)	(6.880)	(6.981)
	[3154.756]	[3154.756]	[3146.975]	[3146.975]	[3158.312]	[3158.312]
Low birthweight	0.001	0.003	0.008*	0.010**	-0.005	-0.002
	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)
	[0.080]	[0.080]	[0.085]	[0.085]	[0.077]	[0.077]
Controls		Y		Y		Y
Observations	156717	156717	53721	53721	98092	98092

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* is a dummy for newborns up to 2,500 grams. Panel (a) and (b) repeat the analysis in Table 2 by setting the minimum matching score at 0.97 and 0.99, respectively (i.e., at a slightly worse and slightly better quality of matching between births and employment records). All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table B2: Effect of maternal layoff on birthweight using mass layoffs

	(1)	(2)	(3)	(4)
	Single mothers		Couples	
	$\geq 33\%$	$\geq 50\%$	$\geq 33\%$	$\geq 50\%$
Birthweight	-22.213	-35.805	46.258***	31.904*
	(21.416)	(30.945)	(11.993)	(17.935)
	[3147.657]	[3147.794]	[3156.326]	[3155.506]
Low birthweight	0.004	0.006	-0.014**	-0.017*
	(0.010)	(0.013)	(0.006)	(0.009)
	[0.085]	[0.085]	[0.077]	[0.077]
Controls	Y	Y	Y	Y
Observations	37630	37038	69820	69056

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

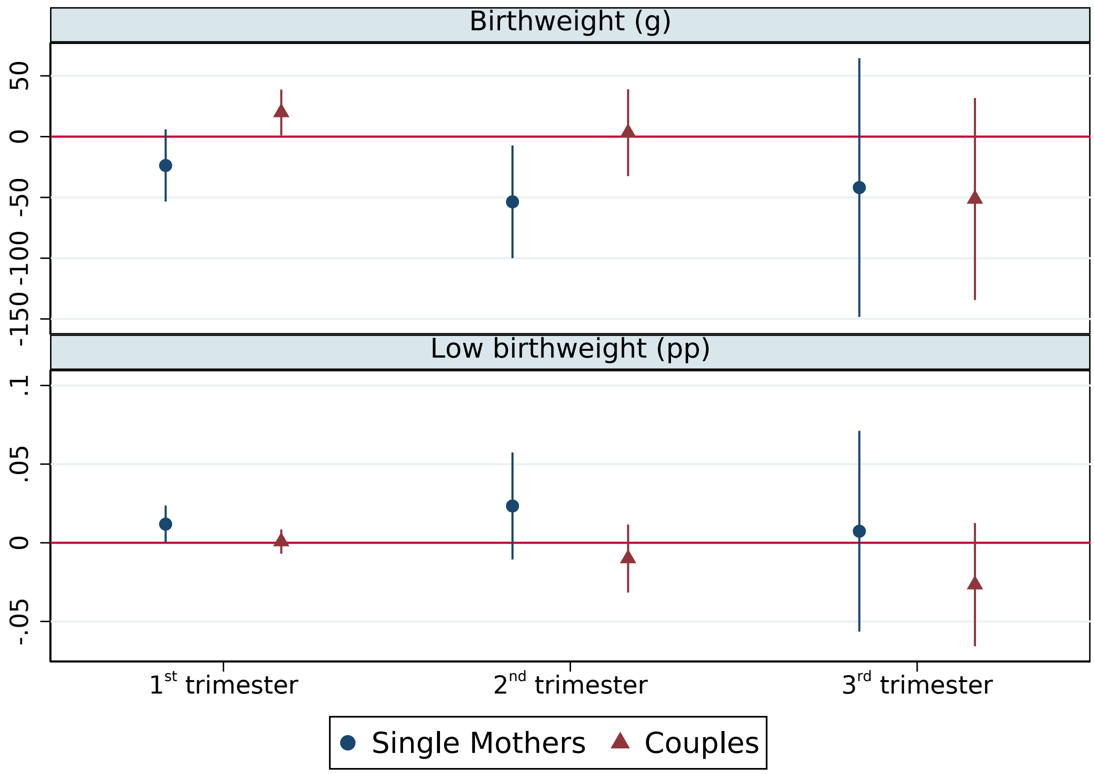
Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams. *Low birthweight* is a dummy for newborns up to 2,500 grams. The treatment assignment is conditional on the share of maternal co-workers displaced during the year: $\geq 33\%$ for Column (1) and (3), $\geq 50\%$ for Column (2) and (4). The control group does not include births whose mothers survived a mass layoff. Both treatment and control group do not include births whose maternal firm size is less than ten employees. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

B.2 Trimester exposure effects

Figure B1: Effect of maternal layoff by trimester of exposure



Note: The figure shows the effect of maternal layoff on births over the period between 2011 and 2014, for different trimesters of exposure. The effect on *Birthweight* is reported in grams. The effect on *Low birthweight* (< 2500g) is reported in percentage points. For consistency, we exclude births conceived in the same month as displacement from the treatment group. On the vertical axis, we display the coefficients (and 95% CI) from regressions of outcome variables on a maternal layoff indicator for trimester exposure during pregnancy for the single-mother births and couples' births, respectively. On the horizontal axis, we display the timing of maternal layoff grouped by trimester. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar). 95% CI are constructed with robust standard errors clustered at the municipality of residence level.

B.3 Other health at birth outcomes

Table B3: Effect of maternal layoff on other birth outcomes

	(1) All births	(2) Single mothers	(3) Couples
Emergency c-section	-0.015**	-0.007	-0.021***
	(0.006)	(0.008)	(0.007)
	[0.417]	[0.322]	[0.481]
Controls	Y	Y	Y
Observations	95802	37568	55137
Apgar score (1 minute)	-0.008	-0.030	0.005
	(0.013)	(0.021)	(0.015)
	[8.425]	[8.352]	[8.463]
Controls	Y	Y	Y
Observations	163271	55267	102862
Apgar score (5 minute)	-0.004	-0.020	0.003
	(0.009)	(0.015)	(0.012)
	[9.380]	[9.346]	[9.398]
Controls	Y	Y	Y
Observations	163317	55287	102888

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses. Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Emergency c-section* is a dummy for c-sections happening after labor began. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

C Appendix to Section 6.1

Table C1: Effect of maternal layoff on gestational length by availability of paternal information

	(1)	(2)	(3)	(4)
	(a) Single mothers			
Gestational length (days)	0.731**	0.858***	1.231**	1.228**
	(0.314)	(0.308)	(0.518)	(0.517)
	[270.408]	[269.108]	[269.075]	[269.075]
Preterm birth	-0.002	-0.002	-0.008	-0.007
	(0.007)	(0.008)	(0.010)	(0.010)
	[0.107]	[0.100]	[0.097]	[0.097]
Observations	23007	32756	11933	11933
	(b) Couples			
Gestational length (days)	0.556**	1.291***	1.465***	1.467***
	(0.279)	(0.225)	(0.288)	(0.288)
	[269.650]	[268.136]	[268.006]	[268.006]
Preterm birth	-0.000	-0.015***	-0.016***	-0.016***
	(0.007)	(0.004)	(0.005)	(0.005)
	[0.093]	[0.096]	[0.097]	[0.097]
Observations	23623	80775	35847	35847
Conditions:				
Father declared	N	Y	Y	Y
Father linked in RAIS			Y	Y
Controls	Y	Y	Y	Y
+ Paternal wage				Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Gestational length* is reported in days. *Preterm birth* is a dummy for gestational length less than 37 weeks. Column (1) reports the coefficients from regressions of outcome variables on a maternal layoff indicator for exposure during pregnancy for the sample of births with no father declared. Column (2) is as in (1), but for the sample of births with a declared father. Column (3) is as in (2), but with a successfully matched father in RAIS, and Column (4) is as in (3), but it also includes paternal wage (R$ 2014) in the set of controls. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table C2: Effect of maternal layoff on prenatal care by availability of paternal information

	(1)	(2)	(3)	(4)
	(a) Single mothers			
Timing of first visit	0.259***	0.264***	0.214***	0.216***
	(0.034)	(0.031)	(0.046)	(0.046)
	[2.651]	[2.315]	[2.261]	[2.261]
Number of visits	-0.556***	-0.412***	-0.270**	-0.277**
	(0.047)	(0.066)	(0.135)	(0.136)
	[7.708]	[8.336]	[8.436]	[8.436]
Observations	21642	31245	11420	11420
	(b) Couples			
Timing of first visit	0.074**	0.139***	0.116***	0.118***
	(0.038)	(0.019)	(0.030)	(0.030)
	[2.238]	[2.002]	[1.978]	[1.978]
Number of visits	-0.236***	-0.211***	-0.189***	-0.198***
	(0.062)	(0.031)	(0.057)	(0.058)
	[8.272]	[8.880]	[8.937]	[8.937]
Observations	22193	77846	34677	34677
Conditions:				
Father declared	N	Y	Y	Y
Father linked in RAIS			Y	Y
Controls	Y	Y	Y	Y
+ Paternal wage				Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014.The sample is restricted to births with a reasonable number of prenatal visits (≤ 18) and available information on month of the first prenatal visit. *Timing of first prenatal visit* is the month of pregnancy in which the first prenatal visit occurs. Column (1) reports the coefficients from regressions of outcome variables on a maternal layoff indicator for exposure during pregnancy for the sample of births with no father declared. Column (2) is as in (1), but for the sample of births with a declared father. Column (3) is as in (2), but with a successfully matched father in RAIS, and Column (4) is as in (3), but it also includes paternal wage (R$ 2014) in the set of controls. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table C3: Effect of maternal layoff on delivery choices by availability of paternal information

	(1)	(2)	(3)	(4)
	(a) Single mothers			
Private clinic	-0.012**	-0.085***	-0.096***	-0.098***
	(0.006)	(0.018)	(0.024)	(0.024)
	[0.045]	[0.319]	[0.352]	[0.352]
Planned c-section	-0.021**	-0.052***	-0.038***	-0.039***
	(0.008)	(0.008)	(0.014)	(0.014)
	[0.214]	[0.407]	[0.418]	[0.418]
Observations	23007	32756	11933	11933
	(b) Couples			
Private clinic	-0.011	-0.055***	-0.045***	-0.047***
	(0.007)	(0.010)	(0.014)	(0.013)
	[0.091]	[0.445]	[0.486]	[0.486]
Planned c-section	-0.014	-0.022***	0.006	0.005
	(0.010)	(0.007)	(0.013)	(0.013)
	[0.343]	[0.510]	[0.525]	[0.525]
Observations	23623	80775	35847	35847
Conditions:				
Father declared	N	Y	Y	Y
Father linked in RAIS			Y	Y
Controls	Y	Y	Y	Y
+ Paternal wage				Y

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

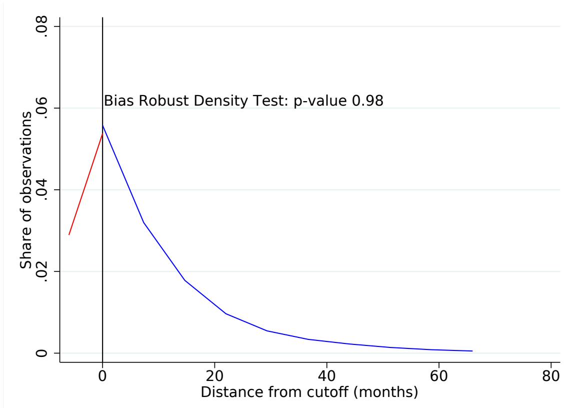
Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Private clinic* is a dummy for deliveries in private clinics. *Planned c-section* is a dummy for c-sections before labor. Column (1) reports the coefficients from regressions of outcome variables on a maternal layoff indicator for exposure during pregnancy for the sample of births with no father declared. Column (2) is as in (1), but for the sample of births with a declared father. Column (3) is as in (2), but with a successfully matched father in RAIS, and Column (4) is as in (3), but it also includes paternal wage (R$ 2014) in the set of controls. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

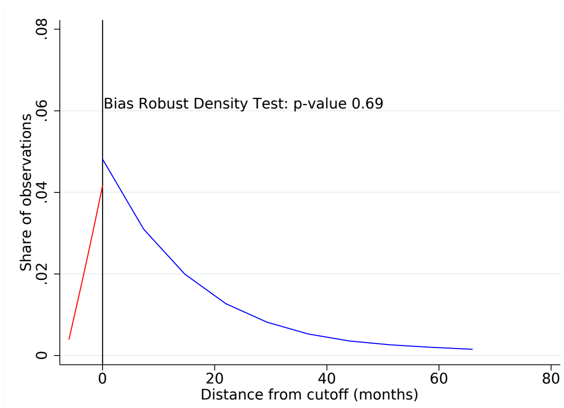
D Appendix to Section 6.2

Figure D1: Distribution of observations around the UI eligibility cutoff

(a) Density for single mothers

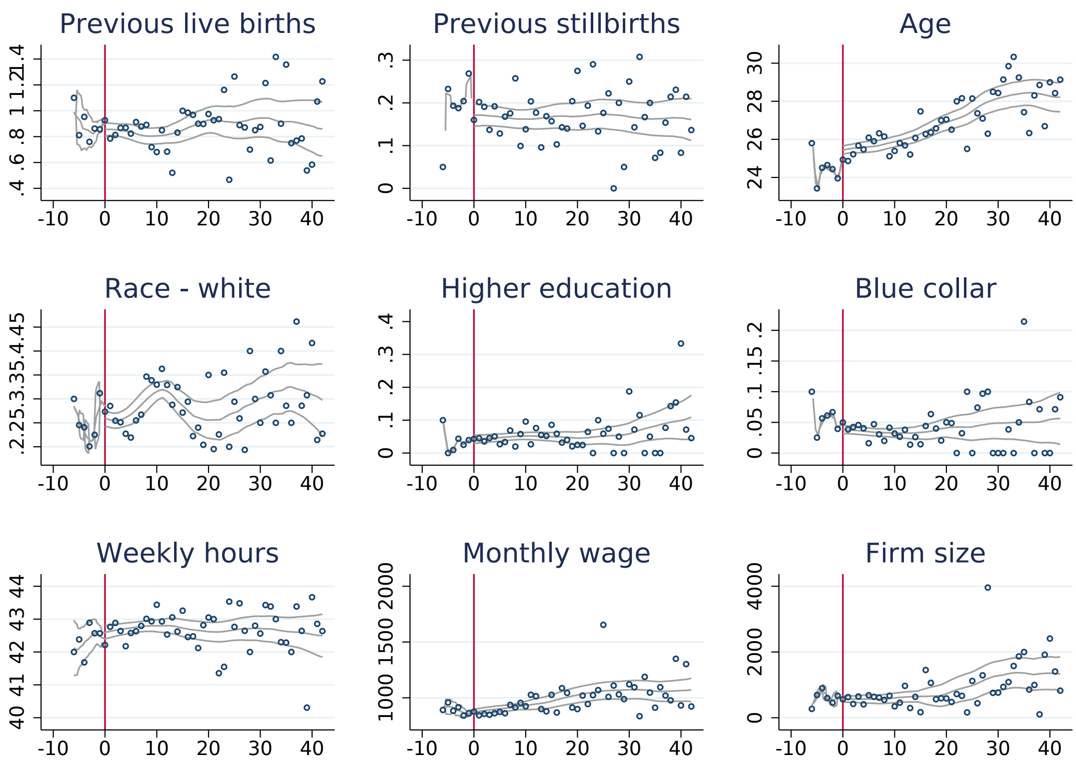


(b) Density for mothers in couples



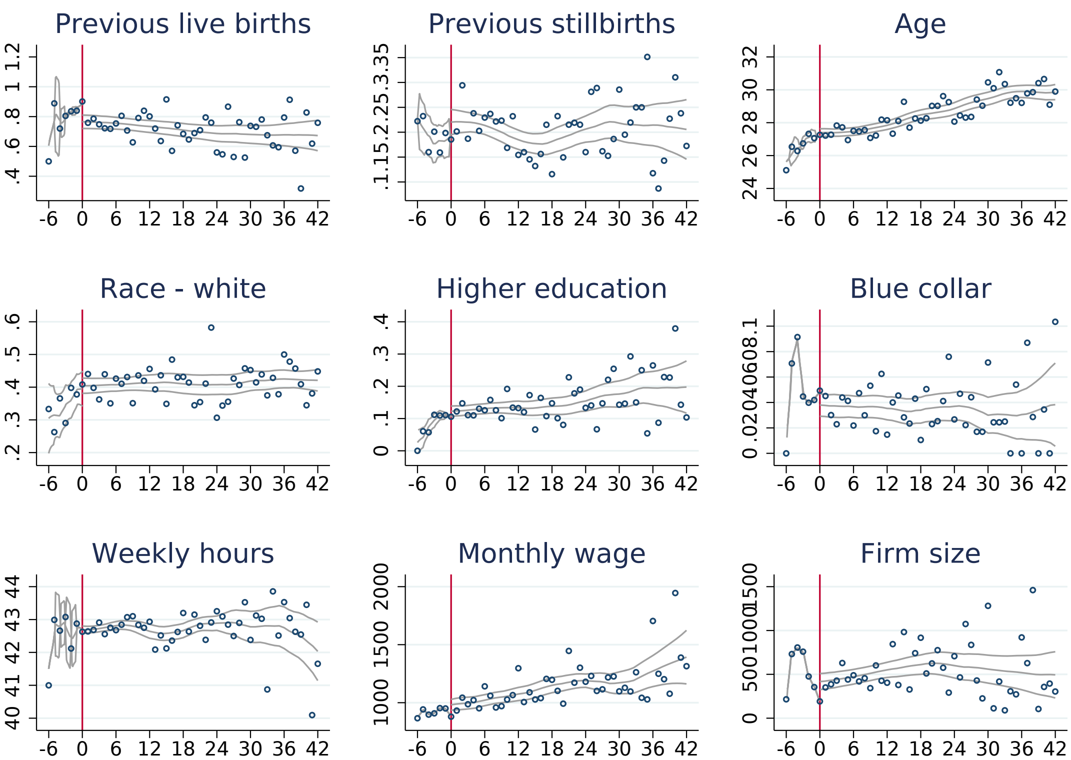
Note: This figure shows the density of dismissal months around the cutoff month (6^{th} month of continuous employment) for entitlement for UI transfers. The sample consists of displaced pregnant workers. The results of the bias robust test proposed by Cattaneo et al. (2020) are also reported.

Figure D2: Balance of pre-determined covariates across pregnant workers around cutoff for UI eligibility (single mothers)



Note: The graphs show the balance of pre-determined covariates around the cutoff for UI eligibility for the sample of displaced single mothers. Dots represent averages based on monthly bins (we show up to 48 months of continuous employment). The lines are based on a local linear polynomial smoothing with 95% confidence intervals.

Figure D3: Balance of pre-determined covariates across pregnant workers around cutoff for UI eligibility (mothers in couples)



Note: The graphs show the balance of pre-determined covariates around the cutoff for UI eligibility for the sample of displaced partnered mothers. Dots represent averages based on monthly bins (we show up to 48 months of continuous employment). The lines are based on a local linear polynomial smoothing with 95% confidence intervals.

Table D1: Effect of UI eligibility on birthweight (robustness checks)

	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers			Couples		
DEP. VAR.: *Birthweight*						
UI Eligibility	76.027**	85.107**	62.212*	-19.645	-12.130	-4.398
	(30.088)	(34.913)	(32.658)	(49.892)	(53.050)	(49.819)
UI Eligibility ×	-0.039**	-0.052**	-0.037**	0.020	0.021	0.022
Monthly wage (R$ 2014)	(0.018)	(0.023)	(0.019)	(0.049)	(0.050)	(0.048)
Mean	3124.915	3123.376	3122.096	3188.659	3189.492	3188.330
DEP. VAR.: *Low birthweight*						
UI Eligibility	-0.020	-0.027	-0.019	-0.002	-0.001	-0.005
	(0.019)	(0.019)	(0.016)	(0.025)	(0.026)	(0.026)
UI Eligibility ×	0.000	0.000	0.000	-0.000	-0.000	-0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	0.091	0.092	0.092	0.072	0.072	0.073
Controls	Y	Y	Y	Y	Y	Y
Robustness check	Cubic	Additional FE	$Tenure_i \neq 6$	Cubic	Additional FE	$Tenure_i \neq 6$
Observations	4566	4373	4259	6306	6075	5888

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Note: The analysis is based on births over the period between 2011 and 2014. *Birthweight* is reported in grams.*Low birthweight* is a dummy that indicates newborns up to 2,500 grams. The sample includes workers displaced during pregnancy. This table repeats the analysis in Table 8 by altering the original specification: Column (1) and (4) use a cubic polynomial in tenure at separation; Column (2) and (5) use additional fixed effects; Column (3) and (6) drop the observations at the cutoff ($Tenure_i \neq 6$). All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Additional fixed effects include hospital, firm municipality and sector fixed effects. Controls include mother's age and age squared, monthly wage (R$ 2014), months of tenure at conception and contract hours, dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table D2: Effect of UI eligibility on gestational length

	(1)	(2)	(3)	(4)
	Single mothers		Couples	
	Linear	Quadratic	Linear	Quadratic
DEP. VAR.: *Gestational length (days)*				
UI Eligibility	1.160	1.093	-2.069**	-1.979**
	(0.786)	(0.830)	(0.917)	(0.945)
UI Eligibility ×	-0.001	-0.001	0.001	0.001
Monthly wage (R$ 2014)	(0.001)	(0.001)	(0.001)	(0.001)
Mean	270.673	270.673	269.951	269.951
DEP. VAR.: *Preterm birth*				
UI Eligibility	-0.018	-0.014	0.002	-0.000
	(0.018)	(0.016)	(0.022)	(0.022)
UI Eligibility ×	0.000	0.000	-0.000	-0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	0.102	0.102	0.085	0.085
Controls	Y	Y	Y	Y
Observations	4566	4566	6306	6306

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. *Gestational length* is reported in days. *Preterm birth* is a dummy for gestational length less than 37 weeks. The sample includes workers displaced during pregnancy. Columns (1)-(3) and (2)-(4) report the coefficients from regressions of outcome variables on a dummy for UI eligibility (i.e., $Tenure_i \geq 6$ months) and an interaction term between UI eligibility and monthly wage (R$ 2014) for specifications with a running and a quadratic polynomial in tenure at separation, respectively. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014) and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).

Table D3: Effect of UI eligibility on prenatal care and delivery choices

	(1)	(2)	(3)	(4)
	Single mothers		Couples	
	Linear	Quadratic	Linear	Quadratic
	(a) Prenatal care			
DEP. VAR.: *Timing of first visit*				
UI Eligibility	0.055	0.052	-0.022	-0.020
	(0.096)	(0.096)	(0.094)	(0.096)
UI Eligibility ×	-0.000	-0.000	0.000	0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	2.798	2.798	2.290	2.290
DEP. VAR.: *Number of visits*				
UI Eligibility	-0.053	-0.145	0.169	0.162
	(0.175)	(0.185)	(0.150)	(0.157)
UI Eligibility ×	-0.000	-0.000	-0.000	-0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	7.471	7.471	8.327	8.327
Controls	Y	Y	Y	Y
Observations	4295	4295	6007	6007
	(b) Delivery provider and type			
DEP. VAR.: *Private clinic*				
UI Eligibility	-0.007	-0.011	0.012	0.002
	(0.021)	(0.019)	(0.026)	(0.027)
UI Eligibility ×	0.000	0.000	-0.000	-0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	0.112	0.112	0.249	0.249
DEP. VAR.: *Planned c-section*				
UI Eligibility	0.057**	0.047*	0.027	0.029
	(0.025)	(0.025)	(0.035)	(0.035)
UI Eligibility ×	-0.000	-0.000	-0.000	-0.000
Monthly wage (R$ 2014)	(0.000)	(0.000)	(0.000)	(0.000)
Mean	0.253	0.253	0.400	0.400
Controls	Y	Y	Y	Y
Observations	4566	4566	6306	6306

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robust standard errors clustered at the municipality of residence level in parentheses.

Mean values in brackets.

Note: The analysis is based on births over the period between 2011 and 2014. The sample in Panel (a) is restricted to births with a reasonable number of prenatal visits (≤ 18) and available information on month of the first prenatal visit. *Timing of first prenatal visit* is the month of pregnancy in which the first prenatal visit occurs. *Private clinic* is a dummy for deliveries in private clinics. *Planned c-section* is a dummy for c-sections before labor. The samples include workers displaced during pregnancy. Columns (1)-(3) and (2)-(4) report the coefficients from regressions of outcome variables on a dummy for UI eligibility (i.e., $Tenure_i \geq 6$ months) and an interaction term between UI eligibility and monthly wage (R$ 2014) for specifications with a running and a quadratic polynomial in tenure at separation, respectively. All regressions include fixed effects for month of conception (both running and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 2014) and contract hours as well as dummies for child's sex, number of previous live births and stillbirths, race, education and occupation type (blue vs white collar).