

Maternal Displacements during Pregnancy and the Health of Newborns

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Abstract

In this paper, we estimate the effect of maternal displacements during pregnancy on birth outcomes by leveraging population-level administrative data from Brazil on formal employment linked to birth records. We find that involuntary job separation of pregnant single mothers leads to a decrease in birth weight (BW) by around 28 grams (-0.89%) and an increase in the incidence of low BW by 10.5%. In contrast, we find a significant positive effect on the mean BW and a decrease in the incidence of low BW for mothers in a marriage or stable union. We find more pronounced negative effects for single mothers with lower earnings and no effect for mothers in the highest income quartile, suggesting a mitigating role of self-insurance from savings. Exploiting variation from unemployment benefits eligibility, we also provide evidence on the mitigating role of formal unemployment insurance using an RD design exploiting the cutoff from an eligibility rule.

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1 Introduction

Losing one's job is one of the most significant shocks individuals might be exposed to over their life-cycle. The consequences of job loss have been widely documented in the literature, ranging from negative effects on consumption from the shock on income (Lepage-Saucier, 2016; Gerard and Naritomi, 2020), effects on future employment (Chan and Stevens, 1999; Arulampalam, 2001), to effects on the health of the worker. The effects of job loss on health have been documented both concerning physical health, including increases in mortality (Sullivan and Von Wachter, 2009; Black et al., 2015; Schaller and Stevens, 2015; Michaud et al., 2016) and mental well-being (Browning et al., 2006; Marcus, 2013). In addition to the direct effects on the worker, the consequences of job loss may also spill over to other family members, including children. Findings in the literature point at negative effects on children's well-being and education (Rege et al., 2011; Mörk et al., 2014; Ruiz-Valenzuela, 2015; Pieters and Rawlings, 2020) as well as at increases in criminal propensity (Khanna et al., 2021; Pinotti et al., 2020).

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, 2011b), potentially including changes in the employment status of the mother. Since the statement of the *fetal origins hypothesis* by Barker (1992), a large body of literature has provided evidence on the negative effect of lower birth weight (BW) and short gestation on socioeconomic outcomes later in life (Almond and Currie, 2011a; Currie and Hyson, 1999; Case et al., 2005; Currie and Moretti, 2007; Black et al., 2007; Oreopoulos et al., 2008; Royer, 2009; Lin and Liu, 2009). Involuntary job loss during pregnancy may also affect the development of the unborn child in line with other shocks in-utero, leading to persistent intergenerational effects. Dismissals differ from other shocks in-utero because the effect of job loss on fetal development is ambiguous. 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 the child’s development, particularly for poorer households, for which the loss of income may impact the nutritional intake of pregnant mothers. A positive effect of relief from the strain of work is likely part of the rationale of maternity leave that can be taken for a period before the due date (Rossin, 2011)¹.

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. Moreover, 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 are interested in understanding the mitigating role of informal and formal insurance to household consumption.

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. 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, providing evidence on the role of a deterioration of prenatal nutrition documenting significant decreases in pre-birth food expenditure. Focusing on maternal employment status during pregnancy, Wüst (2015) finds that Danish working mothers are more likely to deliver pre-term when they are not reported as being employed during gestation. Estimating a dynamic structural model of BW using US and UK longitudinal survey data, 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.

¹Nonetheless, in the cited paper, the author argues that college-educated and married mothers were most able to take advantage of unpaid leave and reap the benefits.

We add to this literature by using individual employment records containing very detailed information on the exact 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 with several different 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 an involuntary 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 wellbeing 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 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, $< 2500\text{g}$) by 10% (marginally significant), and to minor, not significant changes in very (VLBW, $< 1500\text{g}$) and extremely low (ELBW, $< 1000\text{g}$) BW. 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 by at least 4% compared to the mean. These results survive to a battery of robustness checks and alternative specifications and samples, including the adoption of an alternative treatment assignment strategy based on the timing of the first prenatal visit and the use of mass layoffs as a cause for job loss of mothers.

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 - confirming the effect of resting on gestational length (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 (and the presence of spouse/partner), leading to opposing signs in the coefficients in the overall effects across these 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.

Second, we provide evidence on the mechanisms behind the range of effects estimates for the different subsamples of mothers. Focusing on the negative effect of layoffs, we start by investigating the role of income shocks from layoffs for single mothers. We find that the negative effects for single mothers are much more pronounced for individuals on low incomes. We find a negative effect on BW of approximately 40 grams for mothers in the lowest income quartile, while the effect is much smaller and not significantly different from zero for mothers in the top income quartile, indicating the potentially mitigating role of income through self-insurance and consumption smoothing. We also find that layoffs

significantly increase the chance for LBW for the lowest quartile income group. This effect diminishes for higher quartiles. We notice a different effect at play for working mothers in a stable relationship, for which we can link paternal records to their employment status. We observe a positive effect on BW both at the bottom and the top of the paternal earnings distribution. In addition, 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 a brief background on related literature and some context on Brazilian labor market institutions. Section 3 describes the data sources and provides summary statistics. Section 4 presents the main results of maternal layoff on newborns' health, followed by Section 5 discussing the potential mechanisms behind the relationship between layoff and birth outcomes. Finally, Section 6 provides final remarks.

2 Background

2.1 Maternal employment and birth outcomes

Epidemiological and economic research suggests two main channels of transmission of job interruptions effects on newborns' health, which potentially go in opposite directions: a negative effect could be instigated by antenatal stress exposure, while a positive effect may be the result of work interruption. On the one hand, the direct negative consequences of dismissals on pregnant workers' health, e.g., increased stress, can feed through the womb and affect negatively fetal growth (for a systematic review, see [Lima et al., 2018](#)). [Aizer et al. \(2016\)](#) report that in-utero exposure to high cortisol levels (stress hormone) may be

persistent and suggest that the consequences of elevated stress are even more significant for the offspring of women with fewer resources to combat these adverse effects. Furthermore, in the absence of any mechanism to smooth consumption in the aftermath of a dismissal, maternal nutrition and diet can be negatively impacted following a job loss because of the impact on the household budget, including the additional stress from the financial impact (Dave and Kelly, 2010; Smed et al., 2018).

On the other hand, work-related physical and psycho-social strain during pregnancy can be reduced in the aftermath of 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. Jansen et al. (2010) highlight a difference by 45 grams in offspring's mean BW between women with 1–24 h/week and women with longer working hours (40 h/week). Snijder et al. (2012) show that women exposed to longer periods of standing had lower growth rates for fetal head circumference; Niedhammer et al. (2009) and Vrijkotte et al. (2021) suggest that occupational factors (physical work demands, full-time contracts, long working hours) are associated with lower BW through adverse effects on gestational length, but the existing literature suffers from unaddressed endogeneity issues. In this paper, we aim to disentangle these competing channels as much as possible, exploiting the fact that we can estimate the effect separately for mothers where we can vary the income-related shock these mothers are exposed to by focusing on formal and informal insurance available to pregnant women.

2.2 The Brazilian labor market

We here provide some background information on the Brazilian labor market features and the institutional settings of Brazilian employment regulation, paying particular attention to the employment protection regulation of expectant mothers.

In 2019, the female labor participation rate in Brazil was estimated to be at 55%, accounting for almost 44% of the total labor force and with 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 hence is likely a lower bound.

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 forms of separation for open-ended jobs are dismissals without a just cause (70% of all cases) and voluntary quits (29%)². 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. Together with voluntary unemployment, this leads to up to 48% of mothers not being in employment in the first year after childbirth in Brazil (Machado and de Pinho Neto, 2016).

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

²These figures are based on 2011 statistics, but are representative for the entire period of interest in this paper.

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, respectively³. The average wage in the three months prior to layoff (compared to the national minimum wage) determines the replacement rate that the eligible workers will receive, starting from 100% of previous earnings for workers earning the minimum wage. We will use the discontinuity in eligibility for UI, based on the minimum period of continuous employment before dismissal to explore the role UI plays for dismissed pregnant workers later in the paper.

Employers are also mandated to provide a Severance Savings Account (Fundo de Garantia do Tempo de Serviço, FGTS) and a 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 - to catch up with inflation. 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 Section 5.1, we jointly refer to FGTS and the Severance Pay as "lump-sum SP" for consistency with previous research.

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 use of survey data means that the source of variation in maternal employment status is

³Additionally, 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.

frequently unspecified, making it difficult to distinguish between voluntary and involuntary job separations. Relying on survey data also means that birth weight information from surveys is reported by parents and, thus, subject to recall error. The sample size tends to be relatively small, limiting the analysis of rare events.⁴ 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 timing of work leave from social security records, linked to the universe of birth records.

Our dataset is obtained by merging two sets of administrative records. The first source is the *Relação Anual de Informações Sociais (RAIS)*, a linked employer-employee dataset covering the universe of formal workers and firms in Brazil, made available by the Brazilian Ministry of Labor from 2002 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. 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 any dismissals and the recorded cause of dismissal of every worker. Moreover, we calculate the statutory job displacement lump-sum SP 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.

To merge births with employment records, we start with the sample of singleton births –as is standard in the literature– of mother's 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 (6

⁴A notable exception is [Wüst \(2015\)](#) with the data being collected during pregnancy and hence limiting recall biases.

observations) and 378 observations with a duplicate birth ID leading to a final sample of 165,773 births [Perhaps we should specify that this is the final sample after merging with the employment data. We also dropped 131,934 because the DoB was missing, almost 93% of this observations were missing in 2011, the year they started collecting DoB of mothers]. We link birth records to the employer-employee matched dataset using personal identifiers available in both datasets.

4 The effect of maternal dismissals on birth outcomes

4.1 Sample selection and empirical strategy

One difficulty in estimating the causal effects of job loss on birth outcomes, when relying on aggregate unemployment shock 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. 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 might be 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.

Our analysis focuses on pregnant female workers holding open-ended contracts in the private sector. We retain only the first birth observed over the available years for each

matched worker⁵. Next, 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 $t + 9$ ⁶ (Figure 1). Crucially, we do not include in our estimation sample the cases in which child i 's month of maternal displacement T is such that $T < t$ (no endogenous pregnancies condition). Lastly, we construct an indicator function for treatment status as follows

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

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 mothers during pregnancy.

We then estimate the following equation:

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

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 linear and calendar) fixed effects, and ν_m denotes municipality

⁵The linking procedure being probabilistic, we set the minimum matching score between worker and birth at 98%. We test the robustness of the findings to varying matching quality in the Appendix.

⁶As 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 10 different months.

of residence fixed effects. The error term is expressed by ϵ_{imt} . We allow for clustering of standard errors at the municipality of residence level. Municipality of residence fixed effects capture the different unobservable characteristics that mothers have in a certain geographical area, including the provision of prenatal health care. The coefficient of interest τ expresses an intention-to-treat (ITT) effect of maternal displacements on birth outcomes such as BW measures and gestational length, identified if $E(\epsilon_{imt}|D_i, \mathbf{X}_i, \boldsymbol{\theta}_t, \nu_m) = 0$ 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 as well as by marital status to test for differential impacts along this margin.

In [Table 1](#) we report summary statistics for control and treatment group mothers, separately by marital status. We find that the demographic and job characteristics are very well-balanced for single mothers, with the pairwise normalized differences being very small and not exceeding $|.20|$ ([Imbens and Rubin, 2015](#)). For mothers as part of a couple, we find that control and treatment groups differ along with some demographics and job characteristics: displaced mothers tend to have lower educational attainment, wages and tenure⁷. Although the normalized differences are below the recommended threshold, both single and couples' control group mothers are more likely to work in larger firms, possibly indicating that workers in smaller firms may be systematically more vulnerable to layoffs.

As the pregnancy status may possibly affect the propensity to be selected into displacement, we provide results using alternative samples and specifications. First, we replicate the analysis by focusing on children whose mothers were displaced *before* the first prenatal visit, suggesting that the pregnancy has not yet been confirmed by a physician. Second, we provide estimates based on (varying definitions of) mass layoffs where a substantial fraction of workers are dismissed in the same period addressing any remaining concerns regarding selection of mothers into treatment.

⁷Furthermore, from [Figure A1](#) we notice how the distributions of single mothers and mothers in stable unions across macro-sectors in Panel (a) are similar; while single mothers and mothers in couples from our sample are equally unlikely to occupy blue-collar positions, we find non-single mothers more likely to work in white-collar, high skilled jobs compared to single mothers.

4.2 Main results

Table 2 reports the results from equation (1) on the sample of pooled births, and separately by marital status. The outcome variables are BW, and indicators for low BW (LBW), very low BW (VLBW) and extremely low BW (ELBW) 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, a roughly 0.78% 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%). We also document an increase in the incidence of low BW of 9 percentage points, a more 10% increase compared to the baseline, significant at the 5% level. Instead, we find an opposite effect for mothers in a stable relationship. Dismissal from employment leads to a positive effect on BW for those mothers, with an increase of 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 just marginally significant. Similarly to the effects for single mothers, we do not find any effect on lower parts of the BW distribution, and we henceforward report results for BW and LBW only. 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).

To ensure that the findings do not depend on the quality of match when linking the two datasets, we repeat the analysis in Table 2 by changing the minimum matching score (originally, 98%). In Panel (a) and (b) of Table A1, 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 at 0.97 and 0.99 respectively (i.e., at slightly worse and slightly better quality of matching between births and employment records). Overall, the uncontrolled and controlled specifications in Table A1 yield extremely similar results compared to the results based on our preferred matching score, with minimal variation in magnitudes and significance of the estimates.

We also run regressions with alternative model specifications in Table A2, 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 specific prenatal care delivered through the hospital of delivery and quality of delivery services, including scheduling of selective c-section, among other hospital specific factors. Firm municipality fixed effects control for the local economic situation of the firm that may vary by region 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 and maternal job sectors fixed effects. 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.

As BW accumulates during gestation, the differential results may arise because of divergent impacts of displacements on gestational length. In Table 3, we propose 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 only small differences across single mothers and couples. More substantially, we find a significant decrease by around 11.5% in preterm birth (< 37 weeks) incidence and a marginally significant reduction of 16% in very preterm birth (< 32 weeks) rate for couples only. Given the pre-existing evidence on the positive association between less strenuous work conditions or maternity leave and gestational length, our results are unsurprising. However, our findings highlight two points: (i) exogenous job interruptions (which are due to involuntary displacements, unlike the take-up of maternity leave) lead to benefits in gestational length through the relief from work and (ii) the differential impacts of BW by marital status do not derive from any differential effect on gestational length.

Next, having established the opposing role of marital status when estimating the effect of maternal displacements during pregnancy on BW, we probe the possible mitigating effect of the presence of the child's father. We 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 and those whose father cannot be linked. In [Table 3](#), we share the estimated effects of maternal displacement 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 diminished 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. The result is once more unaffected by controlling for paternal wage for the sample of mothers, where we can link the father in the employment record. We do not find a similar pattern for the results on LBW for single mothers’ subsamples, while the reduction in LBW for couples’ children is increasing - however, most of these estimates are not significant. 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.

Taken together, these results indicate the existence of two opposing effects at work, where the economic circumstances determine the relative effect size of either of these underlying effects: a negative effect of dismissals on BW possibly caused by stress or the negative economic shock and an opposing positive effect of layoff, possibly caused by increased resting and the reduction in work-related stress. We investigate the underlying mechanisms further in the following section.

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

- (I) The main findings could be driven by dismissals later in pregnancy that may be 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. We expect though the 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). 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 be cases in which mothers, aware of the future layoff, select into pregnancy.

To deal with these two concerns, we also estimate the effect of layoff during pregnancy on BW by trimester of dismissal and report results in [Figure A2](#). 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 equal size groups of 3 months each. 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. The results for dismissals in the third trimester are noisy because of the small number of dismissals we have at our disposal.

4.3 Robustness checks

4.3.1 Timing of prenatal visits

The first exercise to assess the robustness of our main estimates to selection into treatment is based on the exclusion of births whose first prenatal visit has occurred up to the month before layoff⁸. This limits the potential impact an *official* pregnancy condition may have had on selection into treatment (dismissal). For comparability between groups, we test whether displaced single mothers present a different health utilization behavior compared to non-single mothers. Reassuringly, we find in [Table A3](#) that both displaced single and non-single mothers tend to have slightly fewer prenatal visits and late first prenatal visit⁹.

Once we have established no divergent behavior, we proceed with the definition of an alternative assignment window for treatment status. Formally, we denote child i 's month of first prenatal visit with t_v ; we further exclude from our estimation sample the cases in which child i 's month of maternal displacement T is such that $t_v \leq T \leq t + 9$ ([Figure 2](#)). Hence, we construct a new indicator function for treatment status as follows

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

Our estimates are similar when we adopt this restricted treatment group (losing around 50% and 65% for single and non-single mothers, respectively), as reported in [Table 5](#). We repeat the analysis as in [Table 2](#) by running separate regressions for the main outcome variables (BW, LBW, gestational length and preterm birth) on the samples of all births, single and non-single mothers' children. The estimates generally confirm the previous findings, with losses in mean BW for single mothers (+40g) and gains for couples' children

⁸As the data quality regarding prenatal visits is inferior, we also impose some conditions on the number of prenatal visits (≤ 18). Some loss of information also comes from missing values for the month of the first prenatal visit.

⁹We refrain from attaching any causal meaning to these estimates for two reasons: firstly, the relationship between unemployment shocks and health utilization behavior in the short term is unclear; secondly, the identification of the causal link between prenatal care use and birth outcomes is susceptible to adverse self-selection.

(+28g). While the effects on LBW are reduced and inaccurate, especially for the single mother group, we still notice positive effects on gestational length for both subsamples.

4.3.2 Mass layoffs

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, following the previous literature (Pinotti et al., 2020). 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 5 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 very reassuring. They show that layoffs later in pregnancy that can be explained by mass layoffs have a very similar effect on birth outcomes of single and non-single mothers.

5 Possible Mechanisms

As demonstrated in Table 2 and Table 3, there are substantial heterogeneous effects by household type, in the form of paternal economic involvement in the pregnancy. The partner's source of income could serve to smooth consumption for the unemployed pregnant workers in couples. In contrast, unemployed single mothers lack resources to draw upon as informal insurance through savings. We start by testing the hypothesis that an unemployment shock for single mothers may substantially reduce their budget. The most vulnerable

(liquidity constrained) ones will have fewer resources to smooth their consumption, thus experiencing the most adverse consequences. For couples' households, we probe heterogeneous effects by exploiting our subsample of couples with both mother and father linked in RAIS: this allows us to use paternal income to draw a distribution of mothers by possible resources to fall back on once unemployed.

Moreover, we are interested in understanding whether formal insurance can mitigate the negative impact documented for single mothers. For this reason, we set up a sharp RDD leveraging on changes in eligibility for Unemployment Insurance (UI) around the 6-month minimum tenure requirement. Specifically, we compare the birth outcomes of mothers eligible and non-eligible for UI benefits.

5.1 Informal insurance against income shocks

One possible mechanism behind the negative effect of dismissals of single mothers on the health of their offspring may be due to the economic shock from the loss of income affecting the health of the unborn through stress and/or nutritional channel. While mothers with higher foregone earnings may use savings to smooth consumption across unemployment spells and experience work-relief spillovers, the poorest mothers may be excluded from this form of self-insurance from savings. In the absence of non-employment related information on assets or savings, we firstly estimate heterogeneous effects of maternal layoffs on single-mother children by maternal earnings quartiles, testing the potential role of self-insurance. Alternatively, we calculate the statutory lump-sum SP for all mothers displaced and we use it as a measure of a mother's capability to buffer against the income shock from unemployment by constructing quartiles based on the estimated amount of SP at the end of the year (of the spell, for the displaced group).

When present, the partner's earnings can have a cushioning effect for displaced mothers. This means that being laid off during pregnancy for a mother in a stable relationship may not induce the same level of stress and shock to household consumption compared to single

mothers: maternal income loss can be compensated by partner’s resources. At the same time, the exposure of mothers to physical and mental stress from work may be reduced following the dismissal. To explore this further, we aim to disentangle the effects of having a partner (level of) informal insurance from beneficial impacts of relief from work. We use paternal earnings to split children by paternal earnings quartiles and run the usual regression by each group.

5.1.1 Single mothers

The relationship between earnings and BW losses for single mothers is not trivial. While being a displaced lower-earning worker means a lower absolute income loss than displaced higher-earning workers, lower-earning workers may face severe liquidity constraints (and higher losses in terms of future earnings). Hence, we investigate the differential impacts by monthly earnings and the estimated lump-sum SP amount. We construct the quartiles from maternal wages and lump-sum SP distributions and interact these with the treatment indicator.

In [Figure 3](#) we expose the estimates of the effect of displacements during pregnancy by maternal earnings and by lump-sum SP quartiles in Panel (a) and (b), respectively. In Panel (a), we find a gradient of effects by monthly earnings quartile, with children of mothers on the lowest incomes suffering the strongest reduction in mean BW. Children lose around 35g in BW, and layoffs cause a significant increase in the incidence of LBW of an almost 18% rise compared to the baseline incidence. The effects on higher quartiles tend to be smaller for BW and LBW, and are close to zero for the top quartile. These findings suggest the possibility that higher-earning mothers can shield against adverse income shocks, for example, through self-insurance from their savings. Gestational length outcomes by earnings quartiles do not present any relevant pattern. As an ulterior test, we repeat the same analysis by lump-sum SP quartile¹⁰ in Panel (b). The evidence is

¹⁰The average replacement rate of our combined lump-sum SP measure (lump sum SP amount ÷ wage) is 3.34 and 2.57 for control and treatment group workers, respectively. It is not surprising that the rates are lower than in [Gerard](#)

aligned with the previous exercise: the negative effects on BW and LBW are driven by single-mother children in the bottom quartile of the lump-sum SP distribution, suggesting that the availability of resources at dismissal is crucial for shielding against the income shock and adverse pregnancy outcomes.

5.1.2 Couples

Next, we explore the role of paternal income as a source of informal insurance against maternal job loss through the pooling of household income. As partners' income can serve as a buffer against adverse employment shocks, we use the subsample of children whose paternal wage is observed for this part of the analysis.

We begin by splitting the subsample of children whose paternal wage is observed into paternal earnings quartiles. We then run the regression as in (1) for each quartile. The estimated effects of maternal displacement on BW and LBW indicators are reported in [Figure 4](#). The top graph plots the effects of maternal displacement on infant BW by paternal earnings and reveals a positive effect of layoff of non-single mothers for the bottom (+62g) and the top quartile (around +52g) of paternal wage. In contrast, the effects on low BW by paternal earnings are imprecisely estimated. No other major pattern is detected when looking at gestational length measures.

While the positive effect estimated for the top partner's earning quartile is expected, the positive effect for the bottom quartile is somewhat surprising, and the resulting U-shaped trajectory hints at different mechanisms for different quartiles. We document a shift in the distribution of displaced mothers by job occupation and relative skill level as well as by educational attainment across paternal wage quartile in [Figure A3](#). Notably, in the bottom quartile, we find more displaced mothers with primary education only, leaving lower-skilled positions. In contrast, in the top quartile, the dismissed workers tend to have higher education and leave higher-skilled occupations. Hence, these workers may

and Naritomi (2020) (~ 4.70) as they restrict attention to longer-tenured workers.

equally benefit from interrupting physically and mentally straining positions (relative to their peers).

5.2 Formal insurance against income shocks: UI Eligibility

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 layoff¹¹. 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 non-eligible for UI benefits after a displacement by estimating the following equation:

$$y_{imt} = \lambda_1 \text{UI}_{it} + \lambda_2 \text{UI}_{it} \times \text{Wage}_i + f(\text{Tenure}_i) + \mathbf{X}'_i \boldsymbol{\Gamma} + \boldsymbol{\theta}_t + \nu_m + \epsilon_{imt}, \quad (2)$$

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 $\text{Tenure} = 0$ 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. $\text{UI} = 1(\text{Tenure}_i \geq 0)$). 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 main analysis specification.

The coefficient λ_1 in equation (3) estimates the effect of UI eligibility, or equivalently, the intention-to-treat effect of UI claims.¹² We also specify an interaction term between earnings and UI eligibility. This offers the opportunity to allow the effect of UI entitlement

¹¹Secondarily, 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.

¹²One may presume that everyone claims UI at the cutoff, but earlier evidence shows that this is not necessarily the case (Gerard and Naritomi, 2020). Hence the effect would be λ divided by the share of UI claimants.

to differ across the (foregone) wage distribution as follows:

$$E(y_{it}|UI_{it} = 1) - E(y_{it}|UI_{it} = 0) = \lambda + \gamma_1 Wage_i,$$

with the intuition that receiving a cash transfer for displaced pregnant mothers in the left tail of the labor income distribution can be more helpful during economic hardship compared to the ones in the right tail of the distribution, who may have sufficient savings to “weather the storm”.

The first concern with RD estimates is possible manipulation (Gerard and Gonzaga, 2016) of the running variable. The key assumption for the validity of such 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 5 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). We also provide in tables A4 and A5 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 7, 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, we find that UI provides a strong mitigating role protecting the health of single-mother children, the same does not apply to children from mothers in stable relationship¹³. From the estimates in Columns (1)

¹³The importance of cash-on-hand for single mothers was already reported by Gruber (1996): cash welfare under

and (2), we notice a positive increase in BW due to eligibility to 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 (\sim R\$ 790) results in a net gain of 36.5 grams (which offsets the original loss in BW from displacement). The net effect would be higher for lower-income mothers¹⁴.

In [Table A4](#), we reproduce the analysis in [Table 7](#) 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 could yet be a concern for our RD estimates, we re-estimate the effect of UI on a sample that omits mothers with 6 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.

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

the Aid to Families with Dependent Children (AFDC) program provided short-run consumption insurance for women at the point where they become single mothers.

¹⁴For instance, at the 5th percentile, the net gain is around 45g.

beneficial for the unborn children’s health (relieving mothers from strenuous activities at work).

6 Conclusion

In this paper, we estimate the effect of maternal displacements during pregnancy on children’s health at birth by combining two large and detailed datasets 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 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 even have higher BW (20g). We provide a battery of robustness checks to confirm our estimates, including by restricting displacement exposure assignment with the exclusion of children with the first prenatal visit occurring before maternal displacement and focusing on dismissals that occurred during mass layoffs.

In the paper, we provide evidence on potential underlying channels. Firstly, we find that unemployment shocks have heterogeneous effects by (foregone) income as lower-wage single mothers suffer stronger effects - for them, both psychological stress and liquidity constraint may adversely impact the prenatal environment. The presence of informal insurance granted by partner’s income allows, instead, gains in BW for children born to non-single displaced mothers only at the bottom and top quartile of partner’s earning distribution. We explore this finding descriptively, and we notice that structural differences in labour market positions left by displaced mothers in the bottom and top quartile of partner’s wage distribution could lead to these results. We also provide additional evidence on the

role of liquidity constraints by estimating the effect of unemployment benefits eligibility on birth outcomes (i.e., a formal insurance mechanism) with an RD setting. We find that UI counteracts the negative impact of job loss on BW of children of single mothers but not of non-single mothers.

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Figures

Figure 1: Assignment window for treatment status

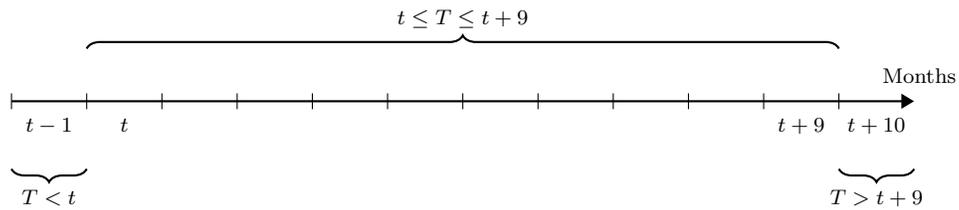


Figure 2: Alternative assignment window for treatment status

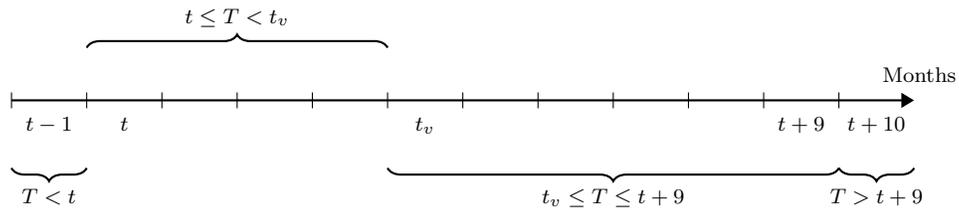
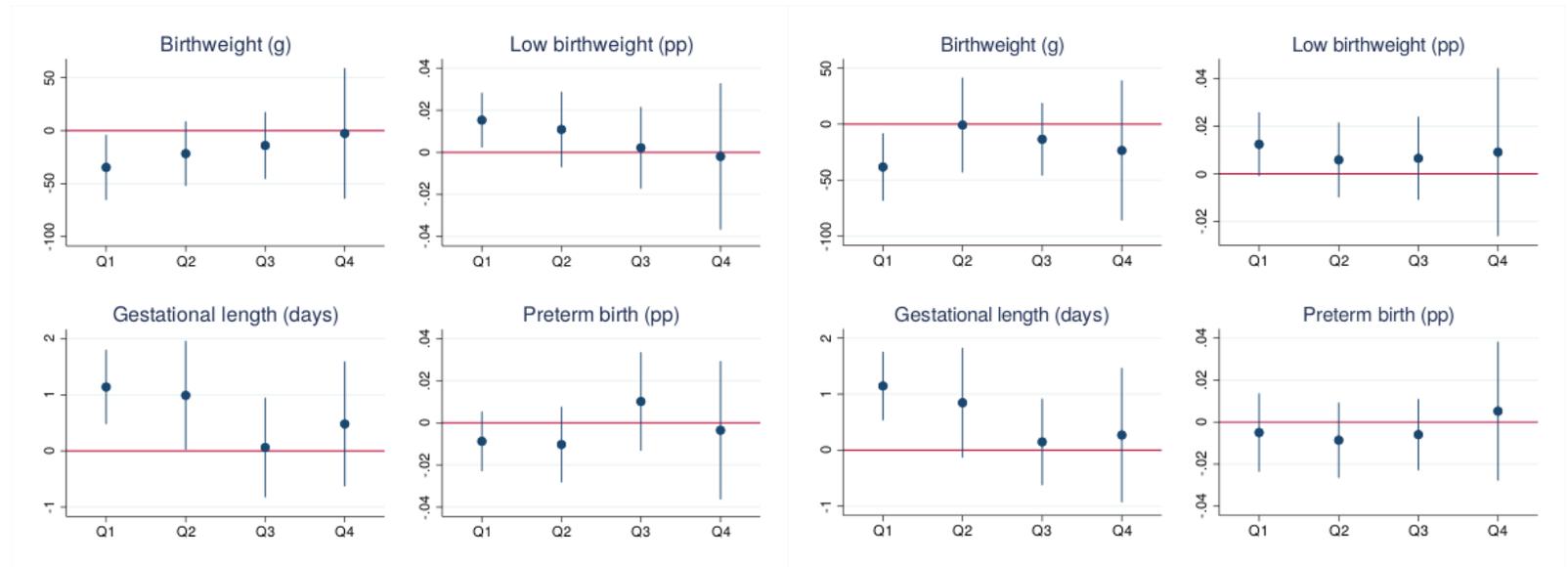


Figure 3: Effect of maternal layoff by maternal earnings and lump-sum SP quartile (single mothers)

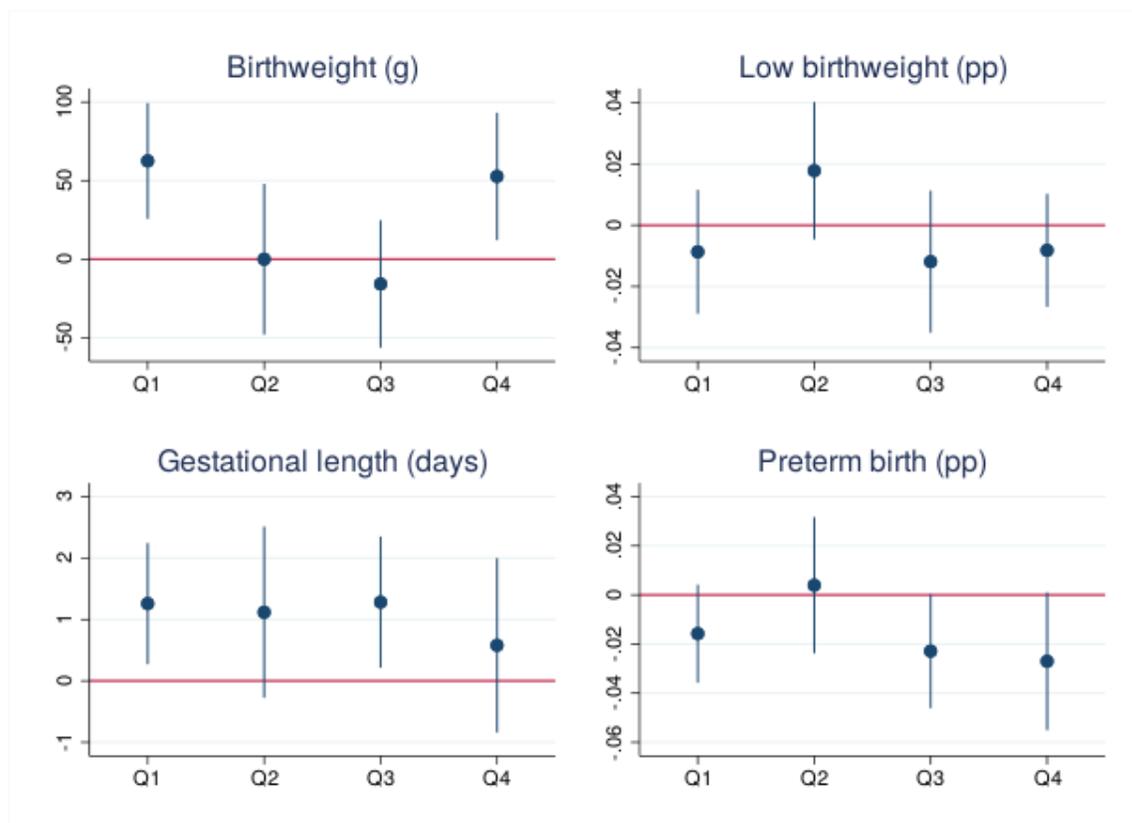
(a) Maternal earnings

(b) Lump-sum SP



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. The effect on *Birthweight* and *Gestational length* is reported in grams and days, respectively. The effect on *Low birthweight* (< 2500g) and *Preterm birth* (< 37 wks) is reported in percentage points. 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 linear 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.

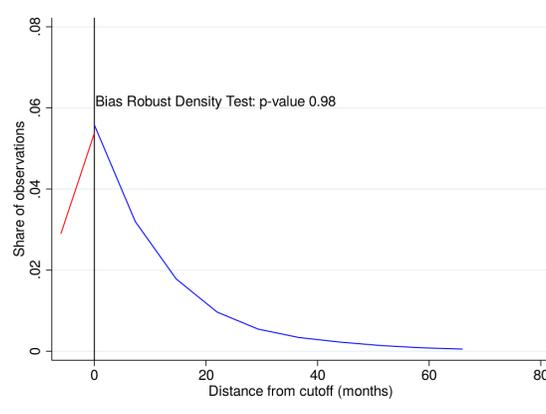
Figure 4: Effect of maternal layoff by paternal earnings quartile (couples)



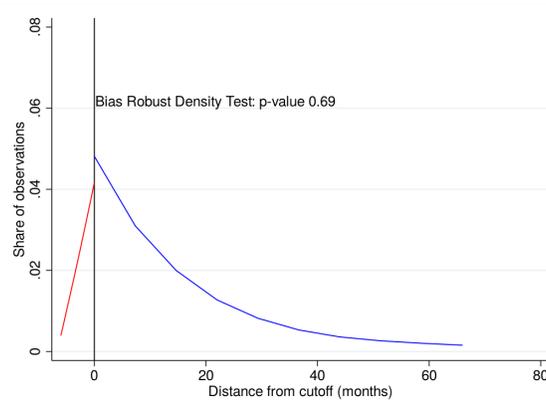
Note: The figure shows the effect of maternal layoff on single-mother births over the period between 2011 and 2014, by maternal monthly wage (R\$ 2014) quartiles. The effect on *Birthweight* and *Gestational length* is reported in grams and days, respectively. The effect on *Low birthweight* (< 2500g) and *Preterm birth* (< 37 wks) is reported in percentage points. 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 couples' 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 linear 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 5: 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 (6th month of continuous employment) for entitlement for UI transfers. The sample consists of displaced pregnant workers. The results of the bias robust test proposed by Cattaneo et al. (2020) are also reported.

Tables

Table 1: Summary statistics on first observed birth by marital status

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers			Couples		
	Control	Treatment	Std Diff	Control	Treatment	Std Diff
<i>Pregnancy information</i>						
Female	0.485 (0.002)	0.497 (0.007)	-0.024	0.488 (0.001)	0.472 (0.007)	0.032
N. of previous live births	0.665 (0.014)	0.849 (0.021)	-0.179	0.567 (0.018)	0.745 (0.019)	-0.181
N. of previous stillbirths	0.162 (0.003)	0.182 (0.006)	-0.044	0.186 (0.002)	0.204 (0.007)	-0.030
<i>Demographics</i>						
Age	26.564 (0.147)	25.697 (0.113)	0.152	29.496 (0.284)	28.111 (0.212)	0.266
Race - white	0.311 (0.026)	0.277 (0.029)	0.073	0.453 (0.021)	0.407 (0.021)	0.093
Race - mixed	0.534 (0.034)	0.559 (0.041)	-0.050	0.414 (0.021)	0.453 (0.023)	-0.078
Race - black	0.095 (0.007)	0.100 (0.007)	-0.018	0.058 (0.003)	0.065 (0.005)	-0.026
Secondary educations	0.714 (0.007)	0.685 (0.012)	0.064	0.632 (0.019)	0.681 (0.008)	-0.102
Higher education	0.082 (0.006)	0.047 (0.004)	0.129	0.255 (0.030)	0.140 (0.013)	0.267
<i>Job characteristics</i>						
Blue collar	0.041 (0.007)	0.043 (0.008)	-0.013	0.033 (0.005)	0.038 (0.005)	-0.031
Weekly hours	42.397 (0.122)	42.616 (0.091)	-0.048	42.022 (0.170)	42.723 (0.089)	-0.129
Monthly wage (R\$ 2014)	1043.208 (32.716)	924.833 (14.483)	0.158	1479.708 (152.753)	1082.447 (57.752)	0.266
Tenure at conception	17.111 (0.236)	13.560 (0.286)	0.153	29.083 (0.656)	19.619 (0.308)	0.285
Firm size	1058.502 (146.687)	662.342 (97.235)	0.113	872.171 (146.123)	489.909 (71.217)	0.118
Observations	51220	4744		98074	6501	

Note: This table reports the pregnancy-specific, demographic and employment information average characteristics at the first observed birth in our single mothers and couples dataset. The value 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 linear 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	
<i>Birthweight</i>	6.588 (5.061) [3155.253]	-1.304 (4.984) [3155.253]	-24.400** (11.806) [3146.038]	-27.984** (11.757) [3146.038]	30.059*** (6.385) [3159.413]	18.497*** (6.459) [3159.413]
<i>Low birthweight</i>	0.001 (0.003) [0.080]	0.002 (0.003) [0.080]	0.008* (0.005) [0.085]	0.009** (0.004) [0.085]	-0.006* (0.003) [0.077]	-0.003 (0.003) [0.077]
<i>Very low birthweight</i>	0.000 (0.001) [0.013]	0.000 (0.001) [0.013]	-0.001 (0.001) [0.014]	-0.001 (0.001) [0.014]	0.000 (0.001) [0.012]	0.001 (0.001) [0.012]
<i>Extremely low birthweight</i>	-0.000 (0.001) [0.006]	-0.000 (0.001) [0.006]	0.001 (0.001) [0.006]	0.001 (0.001) [0.006]	-0.001* (0.001) [0.005]	-0.001* (0.001) [0.005]
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*, *Very low birthweight* and *Extremely low birthweight* are dummies which indicate newborns up to 2,500, 1,500 and 1,000 grams respectively. All regressions include fixed effects for month of conception (both linear 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 gestational length

	(1)	(2)	(3)
	All births	Single mothers	Couples
<i>Gestational length (days)</i>	0.986*** (0.118) [268.866]	0.907*** (0.211) [269.633]	1.128*** (0.176) [268.476]
<i>Preterm birth</i>	-0.007** (0.003) [0.098]	-0.003 (0.006) [0.103]	-0.011*** (0.003) [0.096]
<i>Very preterm birth</i>	-0.001 (0.001) [0.013]	-0.001 (0.002) [0.016]	-0.002* (0.001) [0.012]
<i>Extremely preterm birth</i>	0.000 (0.000) [0.004]	0.000 (0.001) [0.005]	-0.000 (0.001) [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. *Gestational length* is reported in days. *Preterm birth*, *Very preterm birth* and *Extremely preterm birth* are dummies that indicate gestational length less than 37, 32 and 28 weeks, respectively. All regressions include fixed effects for month of conception (both linear 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 birthweight by availability of paternal information

	(1)	(2)	(3)	(4)
	(a) Single mothers			
<i>Birthweight</i>	-33.710**	-26.033*	-9.799	-10.071
	(13.449)	(15.004)	(20.067)	(20.001)
	[3149.914]	[3144.082]	[3144.947]	[3144.947]
<i>Low birthweight</i>	0.010*	0.009	-0.012	-0.011
	(0.005)	(0.007)	(0.010)	(0.010)
	[0.091]	[0.081]	[0.079]	[0.079]
Observations	23007	32756	11814	11814
	(b) Couples			
<i>Birthweight</i>	10.898	21.242***	33.044***	32.712***
	(10.042)	(7.811)	(10.431)	(10.405)
	[3179.504]	[3153.757]	[3147.632]	[3147.632]
<i>Low birthweight</i>	-0.002	-0.003	-0.007	-0.007
	(0.006)	(0.004)	(0.005)	(0.005)
	[0.077]	[0.076]	[0.079]	[0.079]
Observations	23623	80775	35596	35596
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 that indicates 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 linear 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 5: Effect of maternal layoff on birthweight when $T < t_v$

	(1)	(2)	(3)
	All births	Single mothers	Couples
<i>Birthweight</i>	-6.092 (6.759) [3157.250]	-40.416*** (10.164) [3150.026]	28.406*** (9.940) [3160.426]
<i>Low birthweight</i>	0.003 (0.004) [0.078]	0.004 (0.006) [0.083]	-0.001 (0.005) [0.076]
<i>Gestational length (days)</i>	1.156*** (0.202) [268.877]	0.883*** (0.338) [269.695]	1.599*** (0.280) [268.462]
<i>Preterm birth</i>	-0.003 (0.004) [0.098]	0.005 (0.006) [0.102]	-0.014** (0.007) [0.095]
Controls	Y	Y	Y
Observations	151910	50867	96312

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

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 that indicates newborns up to 2,500 grams. *Gestational length* is reported in days. *Preterm birth* is a dummy that indicates gestational length less than 37 weeks. The sample includes births with a reasonable number of prenatal visits (≤ 18) and available information on month of the first prenatal visit. The treatment assignment is conditional on the exposure timing regarding the first prenatal visit: a birth is considered treated if maternal displacement month (T) precedes the first prenatal visit month (t_v); it drops out of the sample if otherwise. All regressions include fixed effects for month of conception (both linear 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 birthweight using mass layoffs

	(1)	(2)	(3)	(4)
	Single mothers		Couples	
	$\geq 33\%$	$\geq 50\%$	$\geq 33\%$	$\geq 50\%$
<i>Birthweight</i>	-22.213	-35.805	46.258***	31.904*
	(21.416)	(30.945)	(11.993)	(17.935)
	[3147.657]	[3147.794]	[3156.326]	[3155.506]
<i>Low birthweight</i>	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 that indicates 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 linear 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 7: Effect of UI eligibility on birthweight

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

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

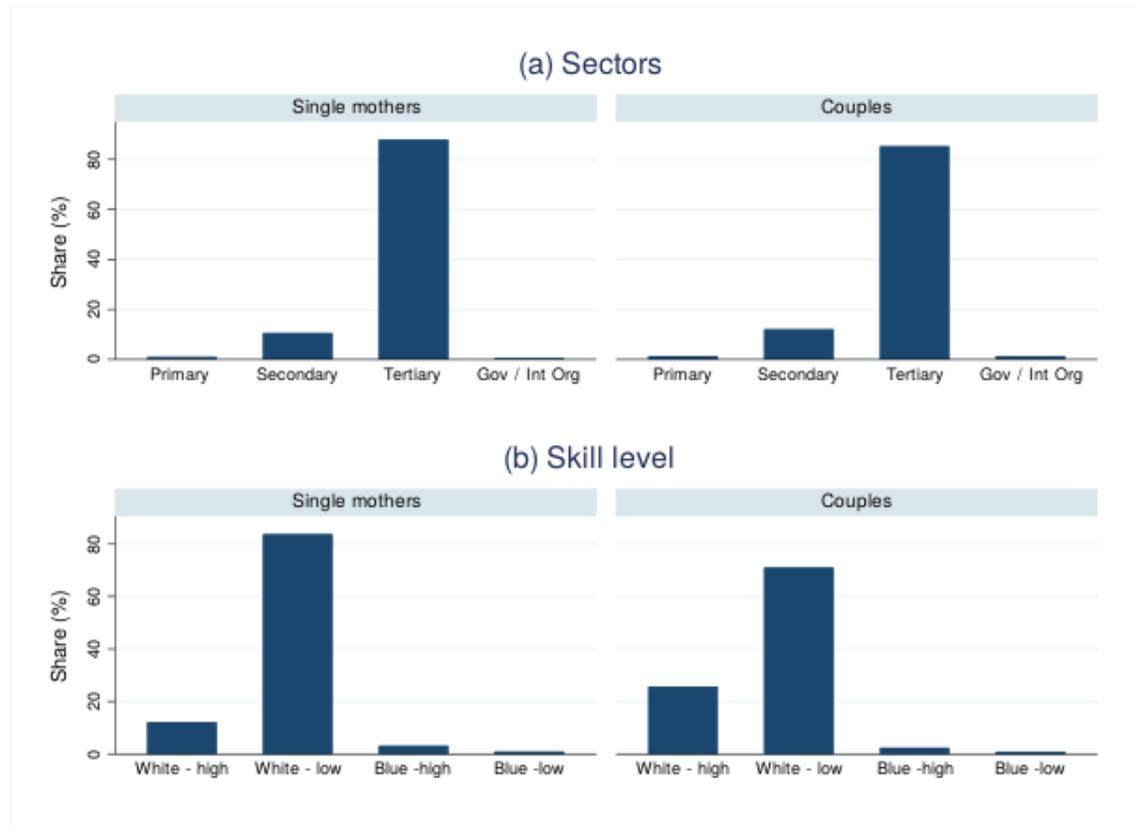
Robust standard errors clustered at the municipality of residence level in parentheses.

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 that indicates 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 linear and a quadratic polynomial in tenure at separation, respectively. All regressions include fixed effects for month of conception (both linear 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).

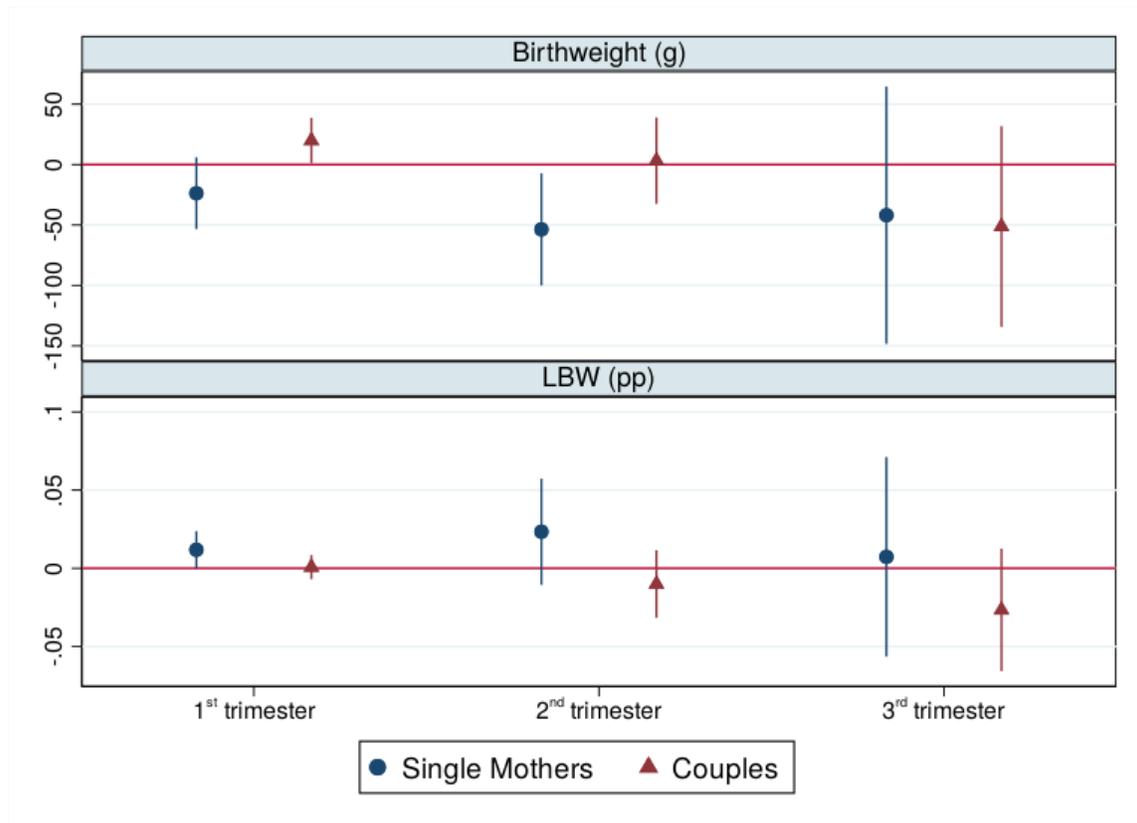
Appendix

Figure A1: Distribution of mothers across sectors and skill levels



Note: The figure shows the distribution of mothers across macro-sectors (primary, secondary, tertiary and governmental) in Panel (a) and job occupations (blue vs white collar) by skill level (high vs low skill) in Panel (b).

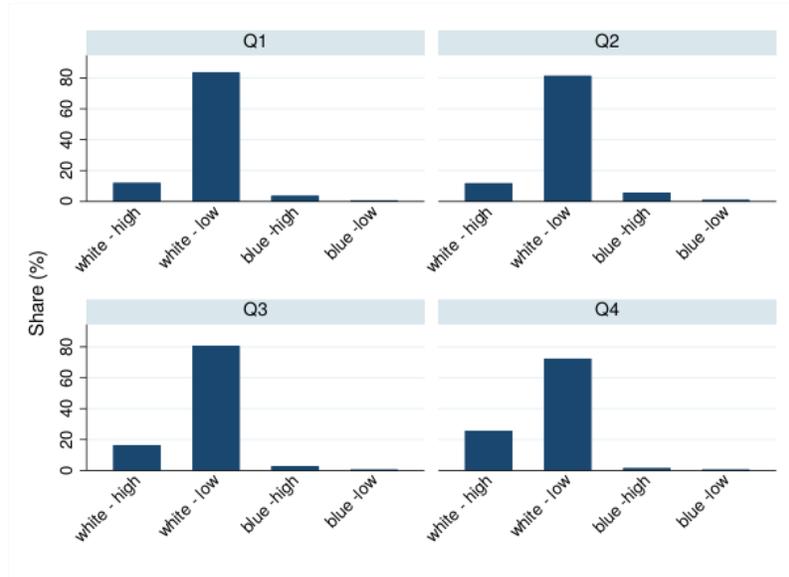
Figure A2: Effect of maternal layoff for different trimesters of exposure



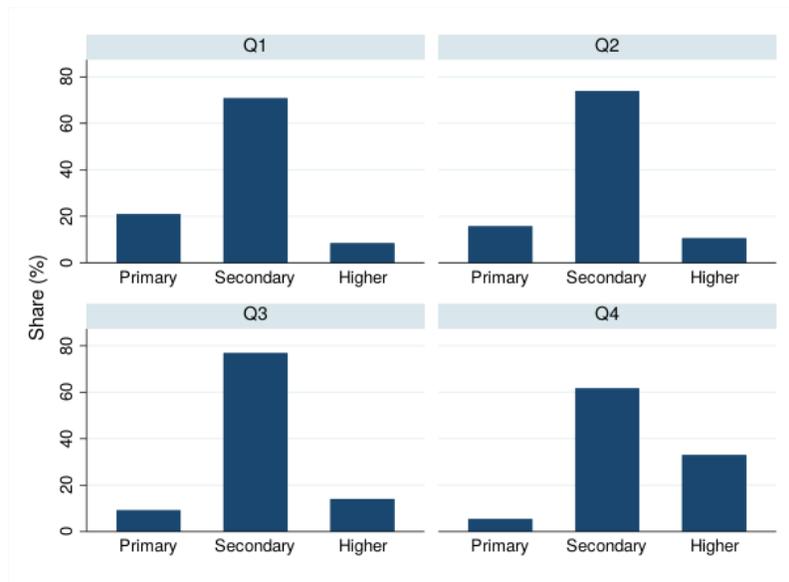
Note: The figure shows the effect of maternal layoff on births over the period between 2011 and 2014, for different trimesters of exposure. The effect on *Birthweight* and *Low birthweight* (< 2500g) is reported in grams and percentage points, respectively. For consistency, we exclude births conceived 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 linear 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.

Figure A3: Distribution of displaced mothers in couples across paternal earnings quartiles by skill level and educational attainment

(a) Skill level

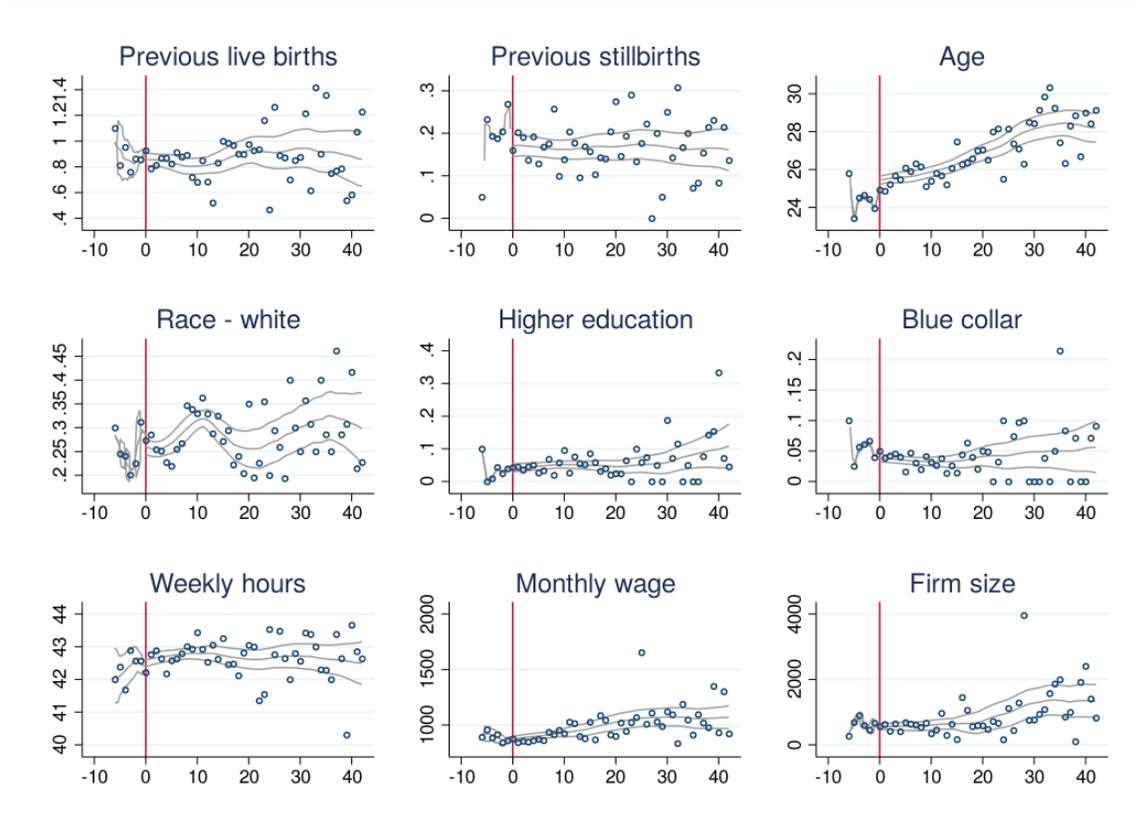


(b) Educational attainment



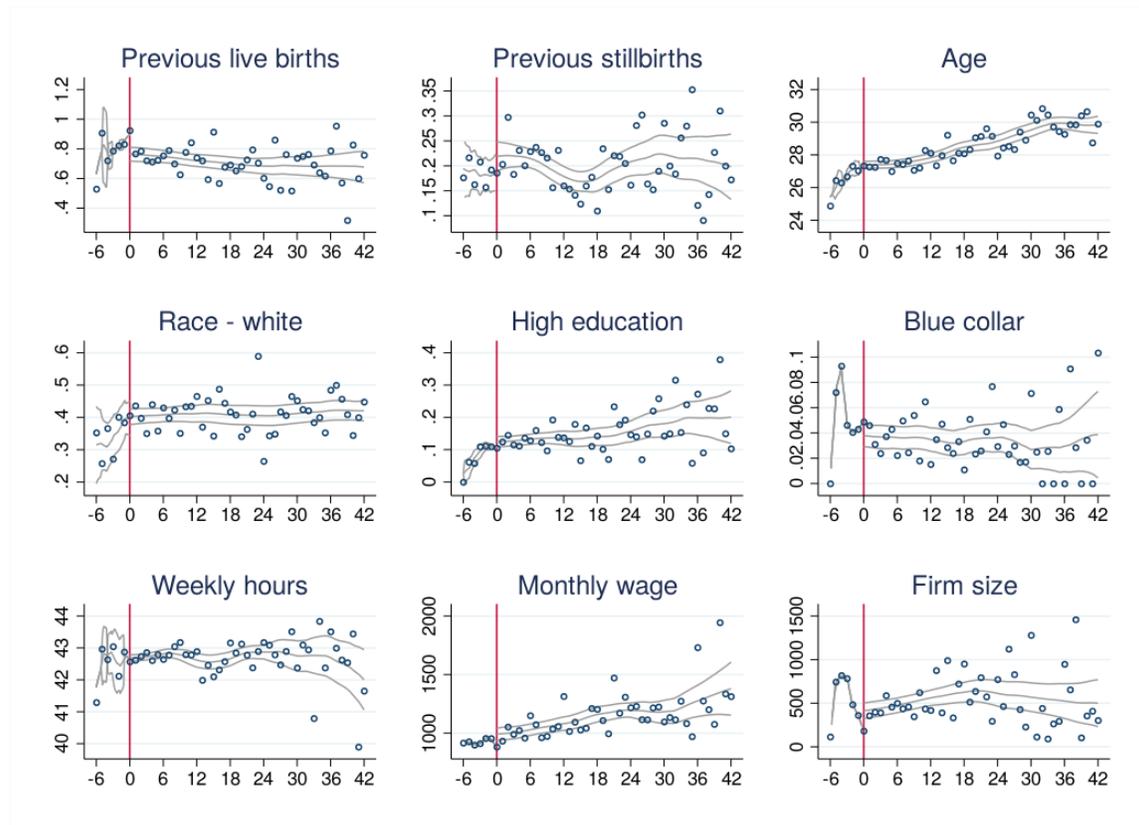
Note: The figure shows the distribution of displaced mothers in couples across paternal earnings quartiles by skill level of their job occupation, in Panel (a), and educational attainment, in Panel (b).

Figure A4: 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 A5: 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 A1: Effect of maternal layoff on birthweight by matching score

	(1)	(2)	(3)	(4)	(5)	(6)
	All births		Single mothers		Couples	
	(a) Match score ≥ 0.97					
<i>Birthweight</i>	5.623 (4.642) [3155.707]	-1.961 (4.553) [3155.707]	-25.717** (11.241) [3145.558]	-28.823*** (11.019) [3145.558]	27.705*** (6.030) [3160.423]	16.324*** (6.127) [3160.423]
<i>Low birthweight</i>	0.001 (0.003) [0.080]	0.003 (0.002) [0.080]	0.009** (0.004) [0.086]	0.010** (0.004) [0.086]	-0.005 (0.003) [0.077]	-0.002 (0.003) [0.077]
Observations	175335	175335	59078	59078	110723	110723
	(b) Match score ≥ 0.99					
<i>Birthweight</i>	4.842 (5.530) [3154.756]	-3.903 (5.402) [3154.756]	-26.764** (13.015) [3146.975]	-31.723** (13.056) [3146.975]	28.605*** (6.880) [3158.312]	16.572** (6.981) [3158.312]
<i>Low birthweight</i>	0.001 (0.003) [0.080]	0.003 (0.003) [0.080]	0.008* (0.005) [0.085]	0.010** (0.005) [0.085]	-0.005 (0.003) [0.077]	-0.002 (0.003) [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 that indicates 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 linear 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 A2: Effect of maternal layoff on birthweight (alternative specifications)

	(1)	(2)	(3)	(4)	(5)	(6)
(a) Single mothers						
<i>Birthweight</i>	-27.984** (11.757) [3146.038]	-29.660** (12.209) [3146.038]	-30.274** (11.724) [3146.350]	-29.663** (12.096) [3145.935]	-30.584*** (11.756) [3146.203]	-30.416** (11.820) [3146.209]
<i>Low birthweight</i>	0.009** (0.004) [0.085]	0.010** (0.005) [0.085]	0.009* (0.004) [0.085]	0.010** (0.005) [0.085]	0.009** (0.005) [0.085]	0.009** (0.005) [0.085]
Observations	55964	55964	55883	55810	55726	55725
(b) Couples						
<i>Birthweight</i>	18.497*** (6.459) [3159.413]	17.689*** (6.461) [3159.413]	17.882*** (6.474) [3159.605]	16.616** (6.496) [3159.415]	16.660** (6.513) [3159.607]	16.122** (6.481) [3159.601]
<i>Low birthweight</i>	-0.003 (0.003) [0.077]	-0.003 (0.003) [0.077]	-0.003 (0.003) [0.077]	-0.003 (0.003) [0.077]	-0.003 (0.003) [0.077]	-0.003 (0.003) [0.077]
Observations	104575	104575	104480	104512	104416	104414
Controls	Y	Y	Y	Y	Y	Y
Calendar FE	Y	Y	Y	Y	Y	Y
Residence Municipality FE	Y	Y	Y	Y	Y	Y
Calendar \times Residence Municipality FE		Y	Y	Y	Y	Y
Hospital FE			Y		Y	Y
Firm Municipality FE				Y	Y	Y
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 that indicates 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 linear 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 A3: Effect of maternal layoff on number of prenatal visits and timing of first visit

	(1)	(2)	(3)
	All births	Single mothers	Couples
<i>Number of visits</i>	-0.345***	-0.503***	-0.233***
	(0.028)	(0.040)	(0.027)
	[8.510]	[8.077]	[8.743]
<i>Timing of first prenatal visit (t_v)</i>	0.191***	0.282***	0.129***
	(0.016)	(0.020)	(0.017)
	[1.196]	[1.454]	[1.055]
Controls	Y	Y	Y
Observations	158218	53090	100220

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

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 includes births with a reasonable number of prenatal visits (≤ 18) and available information on month of the first prenatal visit. *Timing of first prenatal visit (t_v)* is the month of pregnancy in which the first prenatal visit occurs. All regressions include fixed effects for month of conception (both linear 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 A4: Effect of UI eligibility on birthweight (robustness checks)

	(1)	(2)	(3)	(4)	(5)	(6)
	Single mothers			Couples		
DEPENDENT VARIABLE: <i>Birthweight</i>						
UI Eligibility	76.027** (30.088)	85.107** (34.913)	62.212* (32.658)	-19.645 (49.892)	-12.130 (53.050)	-4.398 (49.819)
UI Eligibility × Monthly wage (R\$ 2014)	-0.039** (0.018)	-0.052** (0.023)	-0.037** (0.019)	0.020 (0.049)	0.021 (0.050)	0.022 (0.048)
Mean	3124.915	3123.376	3122.096	3188.659	3189.492	3188.330
DEPENDENT VARIABLE: <i>Low birthweight</i>						
UI Eligibility	-0.020 (0.019)	-0.027 (0.019)	-0.019 (0.016)	-0.002 (0.025)	-0.001 (0.026)	-0.005 (0.026)
UI Eligibility × Monthly wage (R\$ 2014)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-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 7 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 linear 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).