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By

Stefano Cellini
(University of Surrey),

Lívia Menezes
(University of Birmingham)

&

Martin Foureaux Koppensteiner
(University of Surrey and IZA).

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School of Economics
University of Surrey
Guildford
Surrey GU2 7XH, UK
Telephone +44 (0)1483 689380
Facsimile +44 (0)1483 689548
Web https://www.surrey.ac.uk/school-economics
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Maternal Displacements during Pregnancy and the Health of Newborns

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

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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 (-1% ca.) 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 document 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 a Regression Discontinuity design exploiting the cutoff from the unemployment insurance eligibility rule.

JEL Classification: D14, I10, J65

Keywords: Dismissals, birth outcomes, informal insurance, unemployment insurance

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

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

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

1 Introduction

Losing one's job is one of the most significant economic shocks individuals might be exposed to over their life cycle. The consequences of job loss have been widely documented in the literature, ranging from negative effects on consumption due to 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).

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, 2011b), potentially including changes in the employment status of the mother. Since Barker's *fetal origins hypothesis* (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 spillovers of dismissals to the next generation. 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. Such 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 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. 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 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. 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 by estimating 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

[1] Nonetheless, 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.

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., 2021).

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 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 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), and to minor, not significant changes in very (VLBW, < 1500g) and extremely low (ELBW, < 1000g) 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 decrease in LBW of around 4% compared to the mean. These results are robust to a battery of robustness checks and alternative specifications and samples, as well as to employing 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 - in line with a positive effect from providing rest from physical and mental strain for pregnant mothers documented in 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 substantially 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. 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 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. For example, the direct negative consequences from the maternal stress associated with dismissals 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 a mechanism to smooth consumption in the aftermath of a dismissal, maternal nutrition and diet can be negatively impacted by its effect on the household budget, and this might lead to additional maternal stress due to the financial impact (Dave and Kelly, 2010; Smed et al., 2018).

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. 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

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 employment protection regulation of expectant mothers.

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 is 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%).[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

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

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. In addition to dismissals, including after the maternity leave period, voluntary unemployment leads to up to 48% of mothers not being in employment one year after giving birth 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 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.[3] 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.

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,

[3]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.

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 lay-off, (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 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.[4] 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 obtained by merging two such 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

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

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 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 (1,835,982 over the period from 2011-2017) with employment records, we start with the sample of singleton births –as is standard in the literature and drop 41,201 multiple births - 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 observations) or her date of birth (131,934 observations),[5] and exclude 378 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[6] and ensure that mothers are economically active in the formal labor market at the time of conception, reducing the number of birth records we link to the RAIS data, leaving us with a final sample of 165,773 births over the seven years period.

[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. We use Stata's reclink command to link the two datasets probabilistically, setting a minimum matching score of 98%. We test the robustness of the findings to varying matching quality in the Appendix.

4 The effect of maternal dismissals on birth outcomes

4.1 Sample selection and empirical strategy

One difficulty in estimating the causal effect 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. 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.[7] 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

[7] We also repeat the analysis by imposing restrictions on the type of contract (full time with more than 30 hours per week), the job sector (non-agricultural sectors) and the number of children alive (at most 2) at conception. The results are robust to these changes in the sample and are available upon request.

displacement with T and the imputed last month of pregnancy with $t+9$.[8] 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). 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 (Figure 1). 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:

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

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 linear and calendar) fixed effects, and ν_m denotes municipality 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

[8] 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.

τ expresses an intention-to-treat (ITT) 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.

In Table 1 we report summary statistics for control and treatment group mothers, separately by marital status. We find that 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 in a stable relationship, who we denote as *couples*, we find that control and treatment groups differ along some demographics and job characteristics: displaced mothers tend to have lower educational attainment, wages and tenure.[9] 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 $|.20|$.

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 identified or 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.

[9] Furthermore, 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, roughly a 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%). 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 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 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), possibly due to differences in the sample compositions and definition of treatment.

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 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 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 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 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.

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 3, 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 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 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 3, we repeat the estimations 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 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.[10] 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.

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

[10] We engage in a similar exercise to varying the minimum matching score for mothers, by providing estimates by paternal match score in Table A3. Our preferred score of 0.98 provides the most conservative estimates.

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, 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 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 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.

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 layoff.[11] This limits the potential impact an *official* pregnancy 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, in Table A4 we find that both displaced single and non-single mothers tend to have fewer prenatal visits and later first prenatal visits, with more pronounced effects for single mothers.[12]

Having established no divergent behavior by marital status, 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).

[11] As 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.

[12] We 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.

Hence, we construct a new indicator function for treatment status as follows

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

and repeat the analysis of 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. Our estimates are similar when we adopt this restricted treatment group (losing around 50% and 65% of treated single and non-single mothers, respectively), as reported in Table A5. These results confirm the previous findings, with a reduction in mean BW for single mothers (-40g) and gains for couples' children (+28g). While the effects on LBW are smaller in magnitude and less precise, especially for the single mother group, the effects on gestational length are similar 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 nevertheless 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 and the Role of Insurance against Income Shocks

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 might help to smooth consumption in households with a dismissed pregnant worker. In contrast, laid-off single mothers may lack the financial resources from savings to draw upon as informal insurance. 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 to RAIS: this allows us to use paternal income to learn about its potential as mediating factor for dismissed pregnant mothers.

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 to learn about the role of UI in alleviating the negative impact of dismissals.

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 earnings may use savings to smooth consumption across unemployment spells and benefit from the potential positive effect of layoff on gestational length, 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 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 economic 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 the sample by paternal earnings quartiles and run the usual regression for each group.

5.1.1 Single mothers

The relationship between earnings losses from dismissals and BW 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. 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 display 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. Considering gestational length, we document an interesting pattern: we find that the positive effect of dismissals on gestational length is much more pronounced for the lower two quartiles, possibly indicating that jobs with lower salaries tend to be more physically demanding and the separation from these jobs hence tends to be more beneficial for gestation. Next, we repeat the same analysis by lump-sum SP quartile in Panel (b).[13] The evidence is 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

In this section, 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. Starting

[13] The 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 and Naritomi (2020) (≈ 4.70) as they restrict their attention to longer-tenured workers.

with a reduced sample of mothers in a stable relationship, this exercise reduces the sample further, as we additionally require a positive match of paternal record with RAIS. 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 revealing 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 LBW 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 surprising, and the resulting U-shaped trajectory hints at potentially different mechanisms for different quartiles. To investigate this further, 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 equally benefit from interrupting physically (in the bottom quartile) and mentally (in the top quartile) 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 layoff.[14] 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:

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

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.[15] 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 as follows:

$$\mathrm{E}(y_{it}|\mathrm{UI}_{it} = 1) - \mathrm{E}(y_{it}|\mathrm{UI}_{it} = 0) = \lambda_1 + \lambda_2 Wage_i,$$

[14] 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.

[15] One 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.

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.

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 6, 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.[16] 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

[16] The importance of cash-on-hand for single mothers was already reported by Gruber (1996): cash welfare under the Aid to Families with Dependent Children (AFDC) program provided short-run consumption insurance for women at the point where they become single mothers.

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 (which offsets the original loss in BW from displacement). The net effect would be higher for lower-income mothers.[17]

In Table A6, 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 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.

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 (relieving mothers from strenuous activities at work).

[17]For instance, at the 5^{th} percentile, the net gain is around 45g.

6 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 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 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.

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. In combination with a negative effect from the loss of income and the stress associated with the dismissal, which varies in magnitude because of mediating factors in play, this leads to opposing effects of dismissals on birth outcomes by marital status. We document 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. Next, 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 labour 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. In addition to documenting the mediating role of informal insurance through household pooling of income, 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 not of 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 UI for the health of unborn children, not previously documented in the literature.

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Figures

Figure 1: Assignment window for treatment status

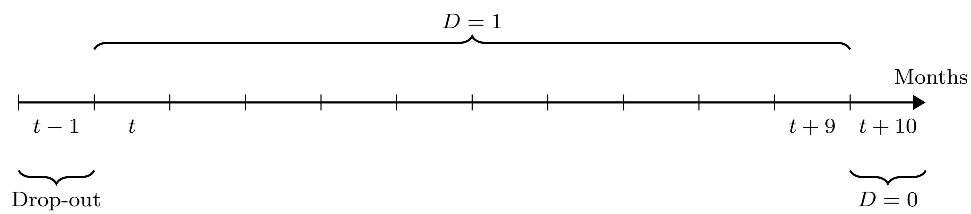


Figure 2: Alternative assignment window for treatment status

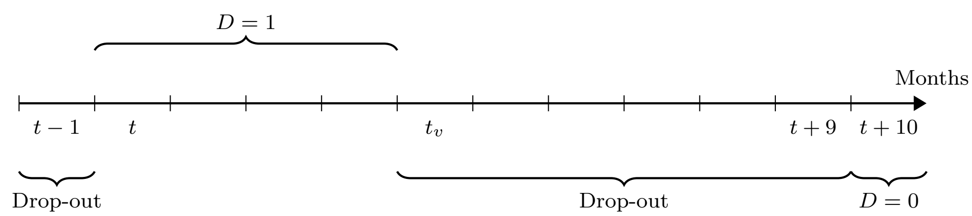
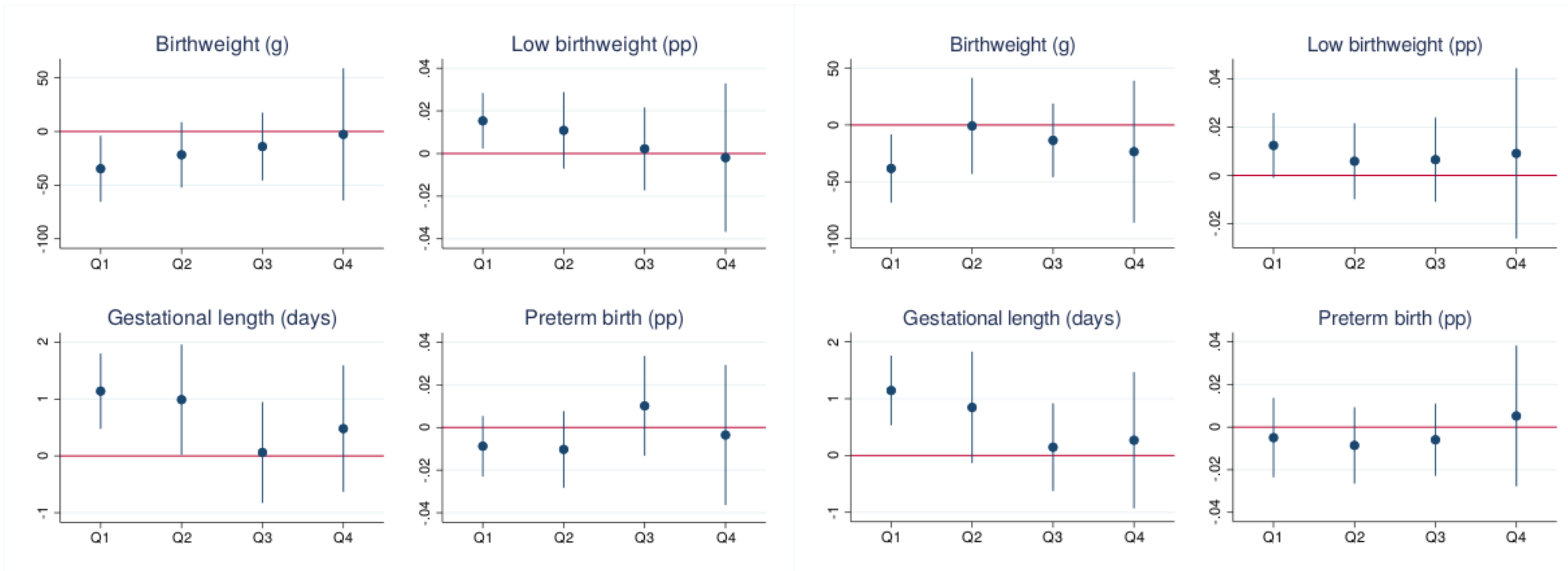


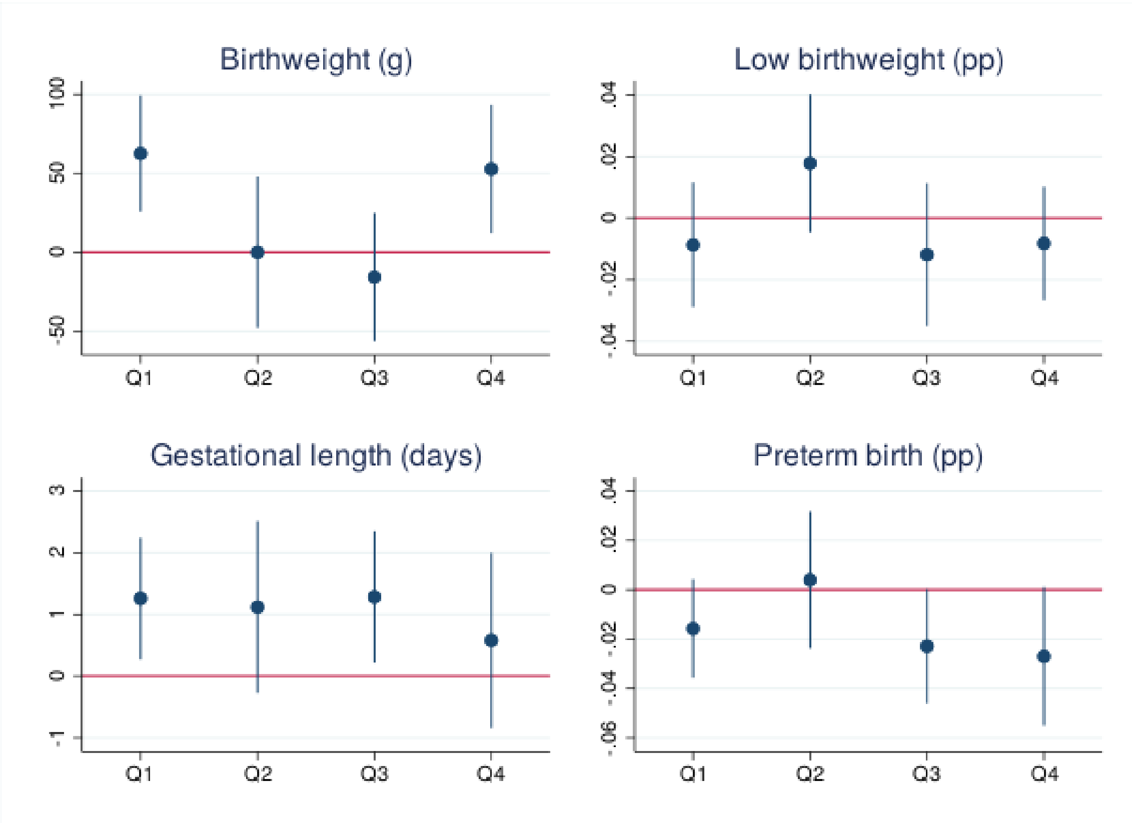
Figure 3: Effect of maternal layoff by maternal earnings and lump-sum SP quartile (single mothers)

(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.

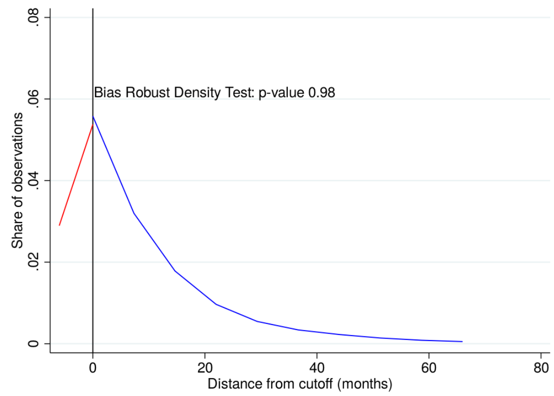
Figure 4: Effect of maternal layoff by paternal earnings quartile (couples)



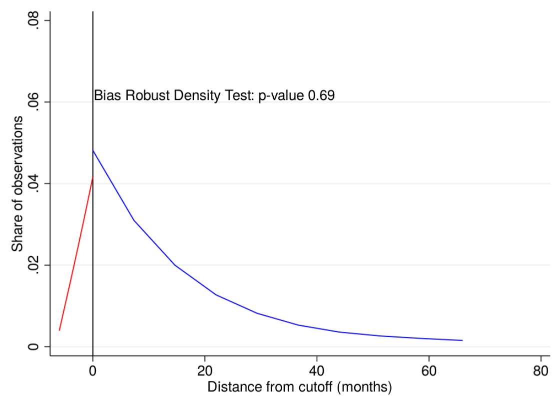
Note: The figure shows the effect of maternal layoff on single-mother births over the period between 2011 and 2014, by maternal monthly wage (R$ 2014) quartiles. The effect on *Birthweight* and *Gestational length* is reported in grams and days, respectively. The effect on *Low birthweight* (< 2500g) 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 (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.

Tables

Table 1: Summary statistics 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
Pregnancy information						
Female	0.485	0.497	-0.024	0.488	0.472	0.032
	(0.002)	(0.007)		(0.001)	(0.007)	
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 educations	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 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	
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]
Very low birthweight	0.000	0.000	-0.001	-0.001	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.013]	[0.013]	[0.014]	[0.014]	[0.012]	[0.012]
Extremely low birthweight	-0.000	-0.000	0.001	0.001	-0.001*	-0.001*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.006]	[0.006]	[0.006]	[0.006]	[0.005]	[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) 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]
Extremely preterm birth	0.000	0.000	-0.000
	(0.000)	(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. *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			
Birthweight	-33.710**	-26.033*	-9.799	-10.071
	(13.449)	(15.004)	(20.067)	(20.001)
	[3149.914]	[3144.082]	[3144.947]	[3144.947]
Low birthweight	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			
Birthweight	10.898	21.242***	33.044***	32.712***
	(10.042)	(7.811)	(10.431)	(10.405)
	[3179.504]	[3153.757]	[3147.632]	[3147.632]
Low birthweight	-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 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 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 6: 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)
	[3124.915]		[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)
	[0.091]		[0.072]	
Controls	Y	Y	Y	Y
Observations	4566	4566	6306	6306

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

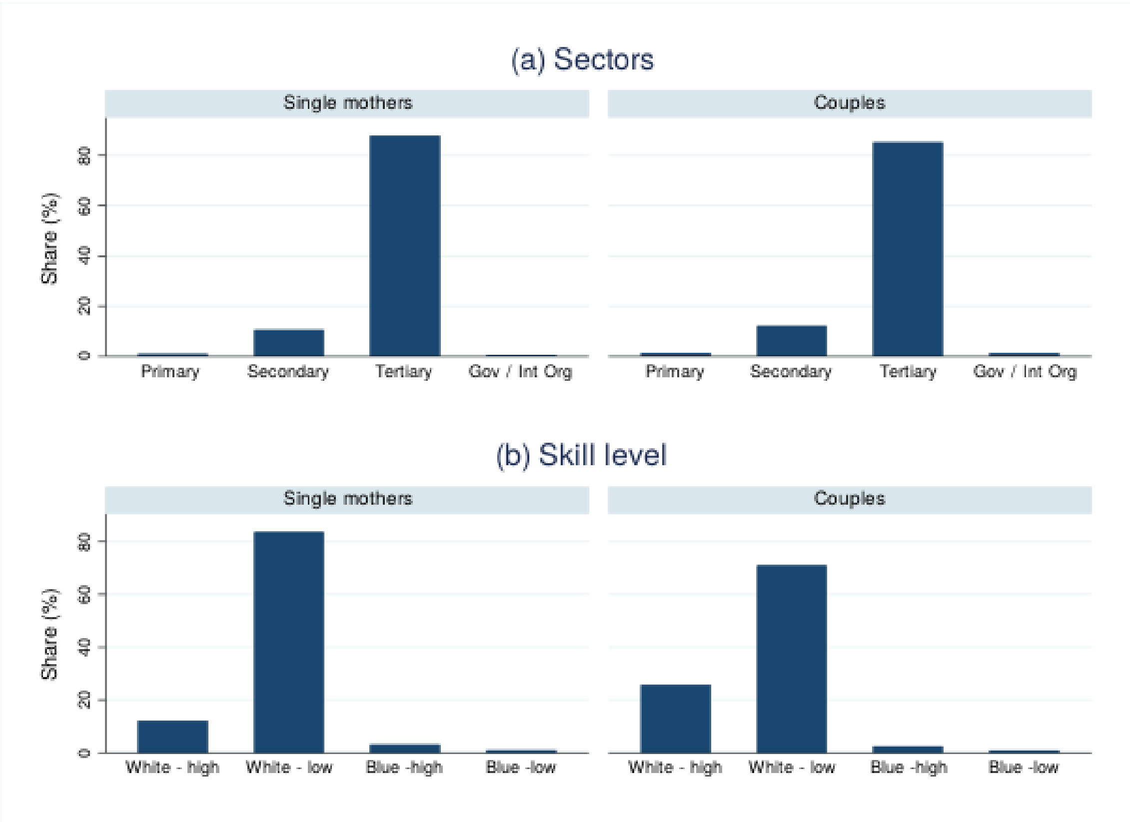
Robust standard errors clustered at the municipality of residence level in parentheses.

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).

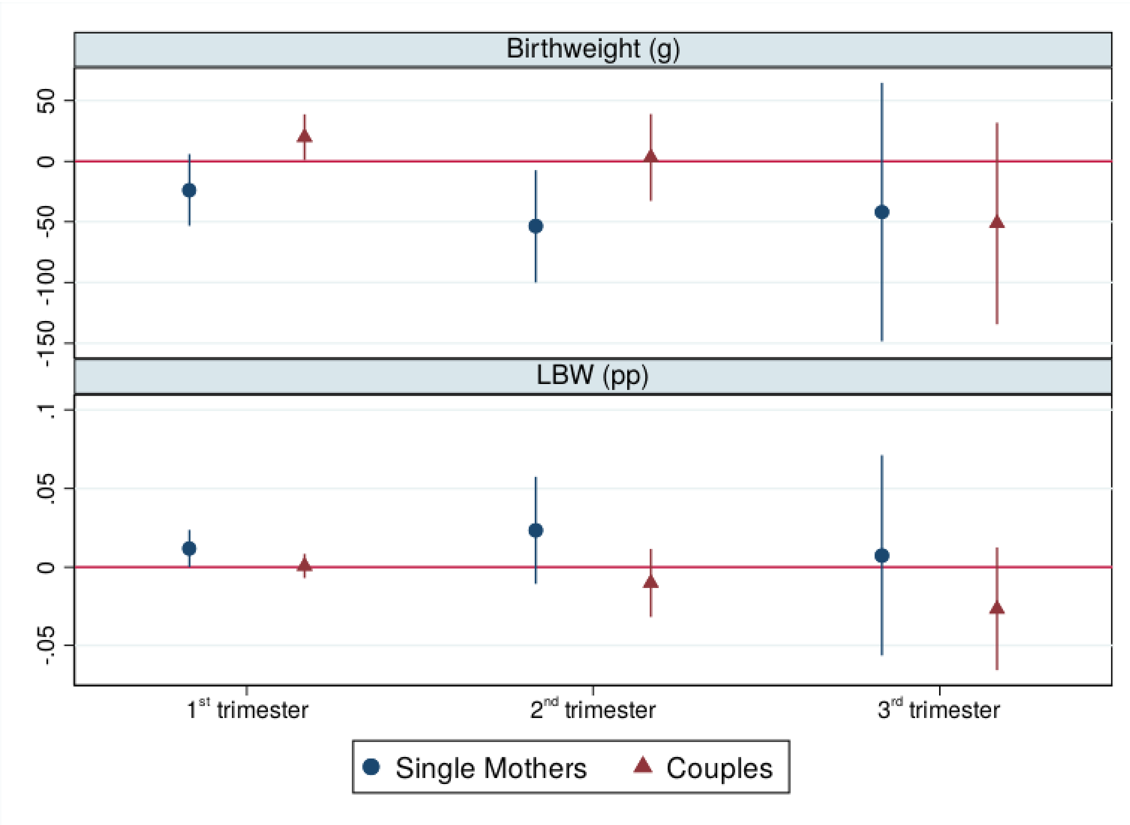
Appendix

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).

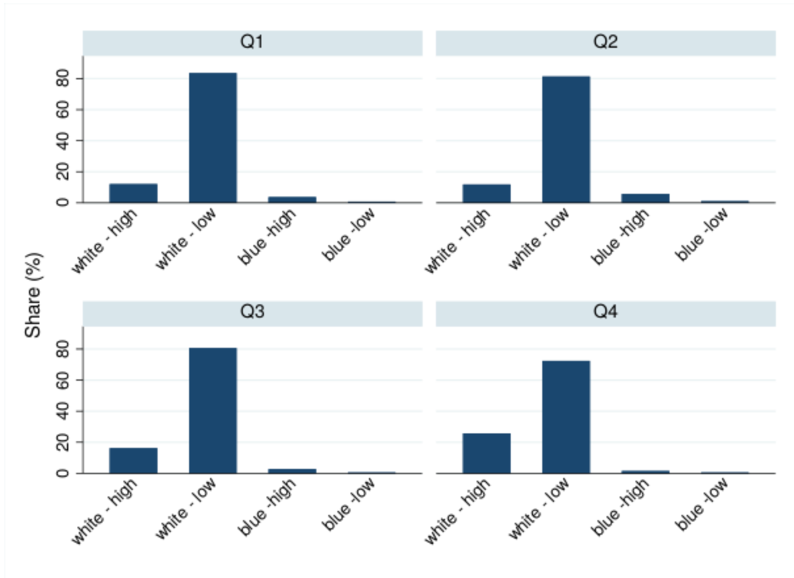
Figure A2: Effect of maternal layoff for different trimesters of exposure



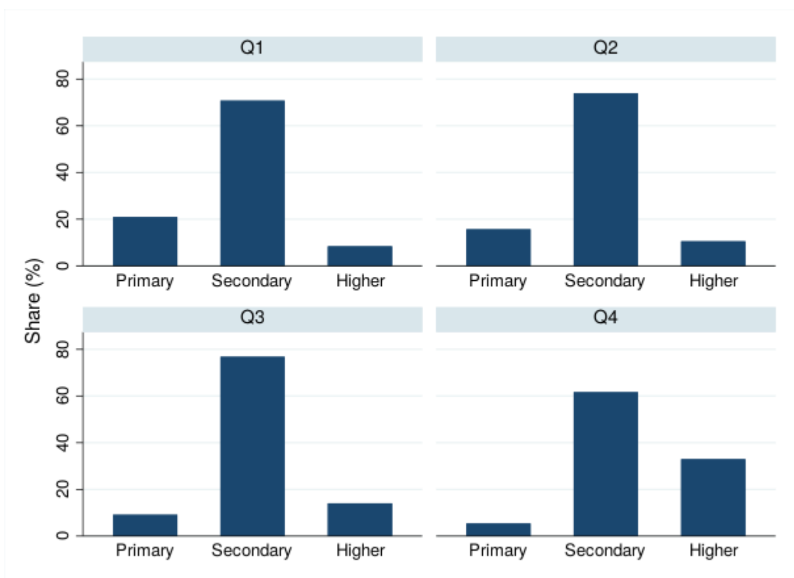
Note: The figure shows the effect of maternal layoff on births over the period between 2011 and 2014, for different trimesters of exposure. The effect on *Birthweight* and *Low birthweight* (< 2500g) is reported in grams and percentage points, respectively. For consistency, we exclude births conceived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

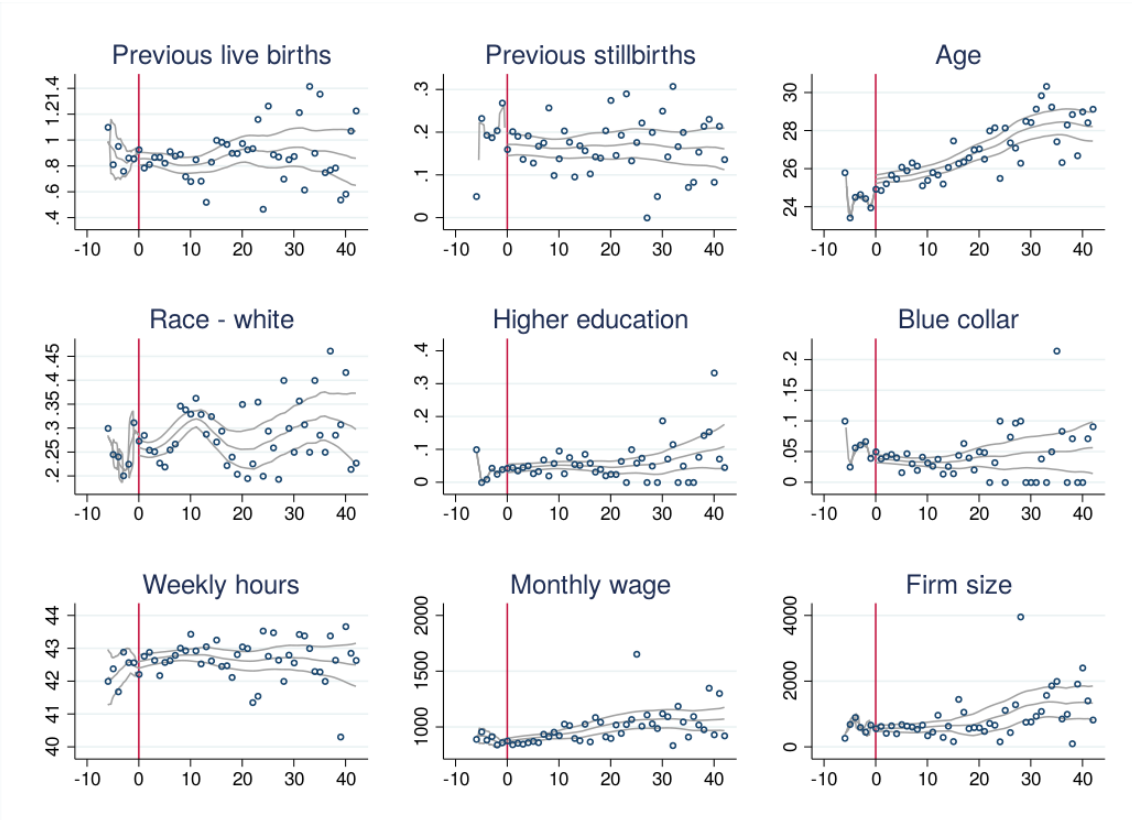


(b) Educational attainment



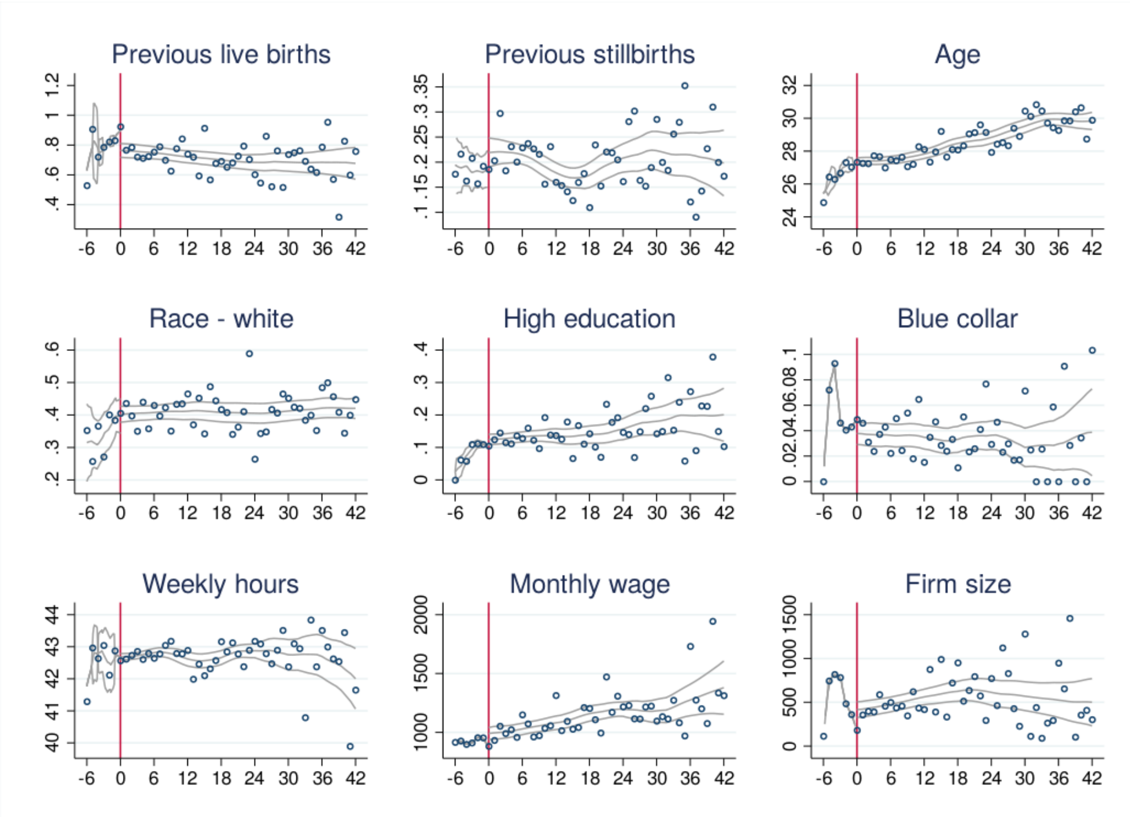
Note: The figure shows the distribution of displaced mothers in couples across paternal earnings quartiles by skill level of their job occupation, in Panel (a), and educational attainment, in Panel (b).

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					
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 ≥ 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 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					
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
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 birthweight (couples), by paternal matching score

	(1)	(2)	(3)
Birthweight	35.849***	42.346***	44.497***
	(11.639)	(11.300)	(13.489)
	[3146.973]	[3146.831]	[3145.458]
Low birthweight	-0.008	-0.011*	-0.015**
	(0.006)	(0.006)	(0.007)
	[0.078]	[0.079]	[0.079]
Controls	Y	Y	Y
Minimum matching score	0.98	0.99	0.999
Observations	33018	31928	27658

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

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. Columns (1) to (3) repeat the analysis in Panel (b) in Table 4 for the sample of births with a declared father, successfully matched in RAIS, by setting the minimum matching score with paternal employment records at 0.98, 0.99 and 0.999, 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 A4: Effect of maternal layoff on number of prenatal visits and timing of first visit

	(1) All births	(2) Single mothers	(3) Couples
Number of visits	-0.345***	-0.503***	-0.233***
	(0.028)	(0.040)	(0.027)
	[8.510]	[8.077]	[8.743]
Timing of first prenatal	0.191***	0.282***	0.129***
visit (t_v)	(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 A5: Effect of maternal layoff on birthweight when $T < t_v$

	(1) All births	(2) Single mothers	(3) Couples
Birthweight	-6.092	-40.416***	28.406***
	(6.759)	(10.164)	(9.940)
	[3157.250]	[3150.026]	[3160.426]
Low birthweight	0.003	0.004	-0.001
	(0.004)	(0.006)	(0.005)
	[0.078]	[0.083]	[0.076]
Gestational length (days)	1.156***	0.883***	1.599***
	(0.202)	(0.338)	(0.280)
	[268.877]	[269.695]	[268.462]
Preterm birth	-0.003	0.005	-0.014**
	(0.004)	(0.006)	(0.007)
	[0.098]	[0.102]	[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 ($\leq$18) and available information on month of the first prenatal visit. The treatment assignment is conditional on the exposure timing regarding the first prenatal visit: a birth is considered treated if maternal displacement month (T) precedes the first prenatal visit month (t_v); it drops out of the sample if otherwise. All regressions include fixed effects for month of conception (both linear and calendar month) and municipality of residence. Controls include mother's age and age squared, monthly wage (R$ 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 A6: 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 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).