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Job Displacement, Unemployment Benefits and Domestic Violence*

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Abstract

We estimate impacts of male job loss, female job loss, and male unemployment benefits on domestic violence (DV) in Brazil. We merge individual-level employment and welfare registers with different measures of domestic violence: judicial cases brought to criminal courts, the use of public shelters by victims, and mandatory DV notifications by health providers. Leveraging mass layoffs for identification, we first show that both male and female job loss, independently, lead to large and pervasive increases in DV. Using a regression discontinuity design, we then show that access to unemployment benefits does not reduce DV while benefits are being paid, and it leads to higher DV risk once benefits expire. Our findings can be explained by the negative income shock brought by job loss and by increased exposure of victims to perpetrators, as partners tend to spend more time together after displacement. Although unemployment benefits partially offset the income drop following job loss, they reinforce the exposure shock as they increase unemployment duration. Since our results cannot be explained by prominent DV theories, we propose a simple model formalizing these mechanisms.

Keywords: domestic violence, unemployment, mass layoffs, unemployment insurance, income shock, exposure, Brazil

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1 Introduction

As many as one in three women report having ever experienced domestic violence (DV) at some stage in their lives (Garcia-Moreno et al., 2006), which makes DV one of the most widespread violations of human rights. It is both a marker and a cause of gender inequality in the economic domain and, yet, it has attracted far less attention from economists than other dimensions of gender discrimination such as the gender pay gap. One reason for relatively limited causal research on DV is that large-scale systematic data on DV are scarce.

This paper studies how economic shocks and policies influence DV using individual-level administrative data from Brazil. In our main analysis, we link DV court cases to population employment data for the 2009-2018 period. We complement the analysis with additional violence measures based on the use of DV public shelters and mandatory DV reports by health providers. We address two main questions. First, we estimate the effects of male job loss on DV perpetration and of female job loss on DV victimization, using a difference-in-differences strategy that leverages mass layoffs for identification. Second, we examine whether unemployment insurance (UI) attenuates any effects of job loss using a clean regression discontinuity (RD) design. Importantly, estimating the impacts of male job loss, female job loss, and unemployment benefits in the same setting allows us to gain insights on the predictive power of domestic violence theories and to investigate different mechanisms.

We find that both male and female job loss, analyzed in isolation, lead to substantial increases in domestic violence against the female partner.[1] DV risk increases by 32% and 56% after men and women lose their jobs, respectively, and these coefficients are not statistically different from each other. These effects are remarkably pervasive over the perpetrators' age, education and income, and across a wide set of area-level characteristics, including factors related to gender norms. In addition, these impacts last for several years and line up with persistent employment and labor income losses following male and female job loss, which we also document.

In contrast with our findings, prominent DV theories would predict *opposite* effects of male and female job loss. In particular, the household bargaining model (Aizer,

[1] To place the effect size in perspective, consider that Angelucci (2008) finds that cash transfers to women amounting to a 35% increase in household income reduce aggressive behavior by 21%; Stevenson and Wolfers (2006) and Brassiolo (2016) find roughly a 30% decline in DV rates after introduction of unilateral divorce in the US and Spain respectively.

2010) predicts that DV risk increases when women lose their jobs (in line with our results) due to higher bargaining power of the man inside the household. By the same argument, however, DV should decrease when men lose their jobs (in contrast with our results). Conversely, the male backlash model (Macmillan and Gartner, 1999) relates DV to deviations from the male breadwinner norm. Therefore, DV risk should increase when men lose their jobs (in line with our results) and it should decrease when women lose their jobs (contrary with our results). The instrumental control and sabotage models, which assume that men commit violence to extract resources from women or sabotage their careers, respectively, deliver similar predictions as the male backlash model (Bloch and Rao, 2002; Anderberg and Rainer, 2013).

We argue that two potential mechanisms can explain the increase in DV following male and female job loss: *income* and *exposure*. First, job loss leads to strong and persistent income losses, causing lower consumption levels in the household. This triggers stress and opens the door for conflict.[2] This *income* mechanism may be present even if partners do not engage in any type of income pooling, as lower consumption by the displaced partner alone can be enough to trigger stress and conflict in the couple. Second, job loss increases women's exposure to DV risk, as displaced workers spend more time at home, potentially interacting with their partners. Exposure may be particularly relevant during the stressful period following job loss. This is an important mechanism discussed in the DV literature (e.g., see Dugan, Nagin and Rosenfeld, 2003), and supported by evidence showing that DV escalates during national holidays, weekends and nights, when families spend more time together (Vazquez, Stohr and Purkiss, 2005). Using survey data we document that partners do spend more time together when one of them is out of a job, supporting the exposure mechanism in our context.

We provide a simple theoretical model that formalizes these explanations. The two-period labor supply model is composed of a male and a female partner. Each of them work and may lose their job, in which case they receive unemployment benefits and search for a new job. DV risk is modeled as a function which depends on consumption and labor supply. When the income mechanism is present, DV risk is decreasing in each partner's consumption level. When the exposure mechanism is

[2] Clark et al. (2008) show that, among a range of negative shocks including bereavement and divorce, job loss stands out as causing persistent unhappiness. The idea that stress may lead to DV is also in line with the evidence in Card and Dahl (2011), who sketch a loss-of-control model in which DV emerges due to emotional queues.

present, DV risk is decreasing in each partner's labor supply. The model predicts higher DV risk after job loss if either or both mechanisms are present.[3]

The model delivers testable predictions. The income mechanism predicts that the effect of job loss on DV will decline with income available upon displacement. In line with this prediction, we find that the impact of job loss is decreasing in severance payments, to the point that no increase in DV is observed for high-tenure workers that receive large severance payments and are less likely to be liquidity constrained.[4]

Turning to the effects of unemployment benefits, the model generates dynamic treatment effects that depend upon the relative play of the two mechanisms. The income mechanism alone suggests that benefits reduce DV by mitigating income losses following displacement; and the exposure mechanism alone suggests that UI transfers increase exposure to DV because they induce workers to remain unemployed for longer periods.[5] When both mechanisms are at play, they will have opposing effects on DV while benefits are being paid and the overall effect will depend on their relative strength. Once benefits expire, UI income effects fade and only the exposure mechanism remains active because UI effects on employment are long-lasting, and this can lead to higher DV.[6] This is what we find in the data. We use a RD design leveraging variation in dismissal dates to study the impacts of access to unemployment benefits on DV for male workers.[7] Unemployment benefits have no impact on DV while they are being paid out (i.e., in the first semester following the layoff), and they *increase* DV during the following period, after benefit payments cease. These results are consistent with the idea that income and exposure effects compensate each other while benefits are paid out, and that higher DV emerges when workers run out of benefits because of increased exposure to DV.[8]

[3]This prediction holds for any arbitrary degree of income pooling between partners which is allowed in the model.

[4]Severance pay in Brazil is mandatory and increases with tenure. The gradient over tenure cannot be explained by differences in exposure as reemployment patterns are similar for high and low-tenure workers. Also, the gradient is unaffected when we account for other differences between high and low tenure workers, such as age, income, and education – in fact, the effect is pervasive across these other dimensions.

[5]A large body of empirical work shows that more generous unemployment benefits lead to lower labor supply – see, e.g., Katz and Meyer (1990); Card, Chetty and Weber (2007); Lalive (2008); Gerard and Gonzaga (2021). We also show that this is the case in our data.

[6]The fact that the UI income effects fade once benefits cease is in line with evidence that unemployed workers exhibit large consumption drops upon benefit expiration and appear to do little consumption smoothing. See Ganong and Noel (2019); Gerard and Naritomi (2021).

[7]The analysis is focused on male workers due to sample size restrictions.

[8]This interpretation is also aided by the fact that unemployment benefits in Brazil are close to a

Summarizing, we show that job loss increases DV risk, and that this risk is not attenuated by unemployment benefits. In fact, benefits increase the risk of DV after benefit expiration on account of lengthening unemployment duration.

We address several challenges to the causal interpretation of these results. A first order concern is endogenous reporting. Women could be less likely to report DV events after losing their jobs, and more likely to report after men lose their jobs. If this were the case, our estimates of the impact of male job loss would be upward biased, while the estimated impact of female job loss would be downward biased. We address this concern by showing that our estimates hold using alternative DV measures: (i) court cases initiated by *in flagrante* arrests (i.e., when the offender is caught "red-handed"); (ii) use of public DV shelters by women; and (iii) mandatory notifications of DV cases by health providers. These measures depend less (if at all) on the victim's discretion in reporting. In the case of public shelter use and notifications by health providers, the police and judicial authorities are not notified, which mitigates concerns of reporting being inhibited by the fear of retaliation by male offenders.[9]

We also investigate additional concerns such as missing information on the identity of suspected offenders and victims due to limitations of our judicial data. Although we study male and female job loss in isolation in our main analysis, we show that our main results hold in a subsample where we can link couples.[10] In addition, we provide extensive robustness analyses addressing different threats to the identification of job loss and unemployment benefits effects.[11]

Related literature. Our paper provides the first individual-level estimates of the impacts of job loss by men and women on DV, and novel evidence on how violence

pure income transfer in the period studied: they are not conditional on job search requirements or participation in training programs.

[9]We also show that the increase in DV is driven by offenses of different degrees of severity. This also supports the idea that our main finding is not purely driven by changes in reporting, as more severe offenses should suffer less from reporting bias.

[10]Namely, we show that women are more likely to suffer DV when their male partner loses his job, and that men are more likely to be prosecuted for DV when their female partner loses her job.

[11]Among others, we address concerns related to endogenous selection into mass layoffs, mass layoff spillover effects, flows of displaced workers into the informal economy, and estimation issues arising in staggered difference-in-differences models (discussed, among others, by de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). We also provide extensive evidence that displaced workers are as-good-as-randomly assigned near the UI eligibility cutoff and that our discontinuity estimates are robust to varying bandwidths, polynomial specifications, permutation tests and a falsification analysis based on pre-displacement DV suits.

in the household is affected by access to unemployment benefits. It relates to several strands of literature. First, it contributes to a literature studying the impacts of economic shocks on DV, which has mainly focused on area-level labor market shocks. For example, Aizer (2010) investigates the impacts of male-female relative wages in the US, and Anderberg et al. (2016) study impacts of male and female unemployment rate in the UK. Both studies find results in line with the household bargaining model, whereby relative improvements in the labor market for men lead to more DV.[12] Our results are not directly comparable because we analyze a different shock, which is actual job loss. Area-level unemployment shocks capture a weighted average of impacts on a relatively small share of workers who actually lose their jobs, and a large share of workers who do not. However, it is only when individuals actually lose their jobs that they experience a loss of earnings and an increase in disposable time – the key mechanisms that explain our findings.[13] The theories highlighted in such research cannot explain the first-order patterns emerging from our analysis.[14] Estimating different effects within one setting and using rich individual-level data allow us to gain insights on mechanisms and contributes to our understanding of the determinants of DV.

More generally, the DV literature tend to focus on interventions designed to empower women through cash transfers, microcredit, skills training, or job assignment (Angelucci, 2008; Bobonis, Gonzalez-Brenes and Castro, 2013; Hidrobo and Fernald, 2013; Luke and Munshi, 2011; Heath, 2014; Kotsadam and Villanger, 2020). We draw attention to the importance of shocks to total household income, and depart from the exclusive focus on women by considering also economic shocks to men.[15]

[12]Other studies reveal an increase in DV following improvements in labor market opportunities for women, contradicting the predictions of the bargaining model (Tur-Prats, 2019; Bhalotra et al., 2019; Erten and Keskin, 2020) and argue that their findings are in line the male backlash model (Macmillan and Gartner, 1999).

[13]Moreover, estimates based on area-level shocks may be contaminated by correlated area-level factors such as public spending on social programs, health care, and law enforcement. Our empirical exercise controls for all such factors by comparing job losers (in mass layoffs) to similar workers, employed in the same industry and area, who face similar area-level conditions.

[14]Though we cannot rule out that these mechanisms may play a second order role in our context, and could contribute to explain the relative magnitudes of the impacts of male and female job loss.

[15]One notable exception is Haushofer et al. (2019) who find that one-off cash transfers to men and women in Kenya reduce DV, consistent with our findings. It fundamentally differs from our analysis which studies different shocks: job loss and unemployment benefits. Job loss affects both income and time availability, in addition to being a more widespread routine phenomenon of general interest relative to one-off income windfalls. Unemployment benefits differs from cash transfers as they more strongly lengthen unemployment duration by directly incentivizing lower job search.

Our work also relates to recent research showing substantial increases in DV during the Covid-19 pandemic.[16] Our findings suggest that the surge in DV could be explained both by the economic losses and higher exposure to DV caused by the pandemic and the related lockdown measures. We see our findings on mechanisms as complementary to this literature. Although some Covid-19 papers have used novel data sources and clever strategies to disentangle between different mechanisms (e.g., see Arenas-Arroyo, Fernandez-Kranz and Nollenberger (2021) and Bhalotra et al. (2023)), a crucial challenge is that the Covid-19 emergency was a major shock affecting everyone in the economy and generating important general equilibrium effects, which makes it more difficult to pin down mechanisms. Our work studies the impacts of a different shock – job loss – during "normal" economic times. The detailed individual-level data allows for a finer empirical analysis aimed at isolating such impacts, and for validating the results with multiple DV measures.

Overall, we demonstrate the pernicious impact of job loss, whether suffered by men or women, on domestic violence. Our novel results show that access to unemployment benefits does not mitigate such impacts and, in fact, may actually backfire. These results are relevant from a policy perspective, especially considering that unemployment benefits are the most widespread policy supporting displaced workers around the world. Our findings suggest that UI may have better chances of mitigating the adverse effects of job loss on DV if accompanied by policies incentivizing the return to work or participation in training programs. Understanding the mechanisms at play and identifying mitigating policies is important given the substantial economic costs that DV imposes on women (Bindler and Ketel, 2020; Peterson et al., 2018) and children (Aizer, 2011; Doyle Jr. and Aizer, 2018; Carrell and Hoekstra, 2010). These findings contribute to a relatively small literature estimating the impacts of welfare policies on DV.[17]

[16]For example, the literature has used helpline calls and/or police reports in the US (Bullinger, Carr and Packham, 2021; Erten, Keskin and Prina, 2022; Hsu and Henke, 2021; Leslie and Wilson, 2020; McCrary and Sanga, 2021; Miller, Segal and Spencer, 2020, 2022; Piquero et al., 2020), India (Ravindran and Shah, 2020), Mexico (Silverio-Murillo, de la Miyar and Hoehn-Velasco, 2023), Peru (Agüero, 2021), the UK (Ivandic et al., 2020), and different Latin American countries (Perez-Vincent and Carreras, 2022); internet search queries (Anderberg, Rainer and Siuda, 2022; Berniell and Facchini, 2021); female homicides (Asik and Ozen, 2021); survey measures in Spain (Arenas-Arroyo, Fernandez-Kranz and Nollenberger, 2021), and Argentina (Gibbons, Murphy and Rossi, 2021); and helpline call, police reports and DV shelter use in Chile (Bhalotra et al., 2023).

[17]Notably, Carr and Packham (2020) shows that the timing of nutritional assistance in-kind benefits significantly affects DV in the US. Our work is distinct to the extent that we evaluate the

Our findings are consistent with and also contribute to a literature documenting the often dramatic impacts of individual job loss on people's lives. The mechanisms we highlight are in line with studies showing that job loss results in mental health problems (Kuhn, Lalive and Zweimüller, 2009; Charles and DeCicca, 2008; Zimmer, 2021; Zimmerman, 2006), substance abuse (Black, Devereux and Salvanes, 2015), premature mortality (Sullivan and Von Wachter, 2009) and divorce (Charles and Stephens, 2004; Eliason, 2012).[18]

Finally, recent studies analyze the impacts of mass layoffs on general crime: Bennett and Ouazad (2019); Khanna et al. (2021); Rose (2018), and, employing the same judicial data and empirical strategy as ours, Britto, Pinotti and Sampaio (2022). A fundamental distinction to these studies is that domestic violence is a different phenomenon. It involves a perpetrator and victim who know each other and whose decisions affect each other. As a consequence, DV strongly depends on household dynamics which demand specific models of behavior and also lead to different empirical results. While the prevalent model in the crime literature is the Becker-Ehrlich model whereby crime depends on expected punishment and the opportunity costs of legal activities, domestic violence theories focus on the household. Empirically, our finding that access to unemployment benefits increases domestic violence is in sharp contrast with the results in Britto, Pinotti and Sampaio (2022); Rose (2018) showing that more generous benefits reduce general crime.[19]

The remainder of this paper is organized as follows. Section 2 introduces a simple theoretical framework that we use to guide the empirical analysis. Section 3 provides background information on the Brazilian context, and Section 4 describes the data. Sections 5 and 6 present the results on the effects of job loss and unemployment

impacts of access to a monetary transfer policy, rather than variations in payment timing of in kind benefits.

[18]In turn, stress (Card and Dahl, 2011) and substance abuse (Lee Luca, Owens and Sharma, 2019) have been linked to DV.

[19]Such difference is explained by the exposure mechanism which is particularly relevant for DV. Our paper also differs in important dimensions from Rose (2018), who also studies impacts of job loss on several crimes including domestic violence using a selected sample of ex-inmates in the state of Washington. First, we use data on the universe of displaced workers from a large and heterogeneous country. Second, we also study the effects of female job loss on DV victimization. As we show in the paper, the latter is crucial for analyzing the underlying mechanisms driving DV and ruling out several alternative theories. We carefully address reporting bias issues which are crucial for the interpretation of the results, given that it is a much more severe concern for DV than for general crime. Finally, we also study impacts of job loss on divorce and on the added worker effect, recognizing that both could influence the strength of the exposure mechanism.

benefits, respectively. Section 7 concludes.

2 Theoretical framework

We propose a simple partial equilibrium labor supply model that relates DV to job loss and unemployment benefits through two mechanisms: income and exposure.[20] Consider a couple composed of two individuals, $i = m, f$, living for two periods $t = \{1, 2\}$. The male partner m is a potential perpetrator of DV violence, while the female partner f is the potential victim. Each partner starts the model either in a state of employment E_{i1}, with probability $1 - l_i$, or in a job loss state J_{i1}, with probability l_i. The probability of each state is independent for each partner. If she/he starts the model employed (E_{i1}), she/he remains employed until the end of the model in $t = 2$ (E_{i2}). In the case of job loss (J_{i1}), the partner searches for a new job with intensity s_i. Without loss of generality, s_i is normalized to the probability that the partner finds a new job. The continuous function ψ_i defines the utility cost of job search, assumed to be increasing and convex $\psi_i' > 0, \psi_i'' > 0$.

If job search is successful, the partner moves into the employment state in the same period 1 (E_{i1}) and remains employed in both periods. When employed, partner i works a fixed number of hours h_i^E at a wage rate equal to w_i. For simplicity, we assume that individuals consume all their income y_i in each period (i.e., there are no savings) and have a zero time discount rate. If job search is unsuccessful, the partner moves into the unemployment state in period 1 (U_{i1}), receiving UI benefits $b_i < w_i h_i^E$ lasting only for that period, and remains unemployed until period 2 (U_{i2}) with subsistence income $y_{i2}^s < b_i$.[21]

Individual consumption $c_{it}(y_{mt}, y_{ft})$ is increasing in own income (y_{it}) and weakly increasing in partner's income (y_{-it}). The budget constraint is given by: $c_{mt}(y_{mt}, y_{ft}) + c_{ft}(y_{mt}, y_{ft}) = y_{mt} + y_{ft}$. This formulation allows for varying degrees of income pooling, including a no income pooling scenario where the each partner's consumption depends only on their own income: $c_{it}(y_{mt}, y_{ft}) = y_{it}$. It also allows for partial spousal insurance, but rules out full spousal insurance: holding constant the other

[20] The structure of our model largely follows job search models used to study unemployment benefits in partial equilibrium; see, e.g., Baily (1978); Chetty (2006, 2008); Landais (2015).

[21] We keep the structure of the model as simple as possible to explain our findings. The key behavioral implications of our model regarding the impacts of job loss and UI on consumption and labor supply are supported by empirical evidence (both from our paper and the literature) – see the remainder of this section and Section 6.4.

partner's income, the consumption level of each partner when unemployed is lower relative to the situation where she/he is employed, i.e. $c_{i1}(w_i h_i^E, y_{-i}) > c_{i1}(b_i, y_{-i})$ and $c_{i2}(w_i h_i^E, y_{-i}) > c_{i2}(y_i^s, y_{-i})$. This is in line with empirical evidence showing that job loss causes substantial reductions in consumption levels (Ganong and Noel, 2019; Gerard and Naritomi, 2021).

The probability of domestic violence in each period is modeled by the continuous function $\phi_t(c_{mt}, c_{ft}, h_{mt}, h_{ft})$, which is (weakly) decreasing in all its arguments because higher consumption reduces stress and conflict – the income mechanism – and more hours worked reduce the time spent together by partners – the exposure mechanism, widely discussed in the DV literature.[22] The model flexibly allows for the presence of each mechanism. When the income mechanism is active, DV risk is strictly decreasing in each partner's consumption ($\frac{\delta \phi_t(.)}{\delta c_{it}} < 0$); when the exposure mechanism is active, DV risk is strictly decreasing in each partner's labor supply ($\frac{\delta \phi_t(.)}{\delta h_{it}} < 0$). In Appendix A.1 we show, using survey data, that partners actually spend more time together when they are unemployed.

Let $v_i(.)$ and $u_i(.)$ denote the utility of consumption during employment and unemployment, respectively (assumed to be continuous, increasing, and concave). The functions E_{mt} and U_{mt} define the value of employment and unemployment for the male partner:

$$E_{m1} = v_m(c_{m1}) + E_{m2} = v_m(c_{m1}) + v_m(c_{m2});$$

$$U_{m1} = u_m(c_{m1}) + U_{m2} = u_m(c_{m1}) + u_m(c_{m2});$$

In turn, the functions E_{ft} and U_{ft} define the value of employment and unemployment for the female partner, which is decreasing on DV risk $\phi_t(.)$:

[22] For simplicity, we do not consider hours dedicated to job search as a relevant driver of exposure. This is based on the fact that unemployed workers dedicate very few weekly hours to job search – on average 41 minutes per week in the US and even less so in Europe (Krueger and Mueller, 2010). In addition, it is unclear whether such hours reduce exposure since a large part of job search can be done from home and does not necessarily decreases interactions among partners.

$$E_{f1} = v_f(c_{f1}) - \phi_1(c_{m1}, c_{f1}, h_{m1}, h_f^E) + E_{f2} =$$
$$= v_f(c_{f1}) - \phi_1(c_{m1}, c_{f1}, h_{m1}, h_f^E) + v_f(c_{f2}) - \phi_2(c_{m2}, c_{f2}, h_{m2}, h_f^E);$$

$$U_{f1} = u_f(c_{f1}) - \phi_1(c_{m1}, c_{f1}, h_{m1}, 0) + U_{f2} =$$
$$= u_f(c_{f1}) - \phi_1(c_{m1}, c_{f1}, h_{m1}, 0) + u_f(c_{f2}) - \phi_2(c_{m2}, c_{f2}, h_{m2}, 0).$$

Finally, the utility of the job losing partner, starting the model in job search J_{i1}, is given by:

$$J_{i1} = s_i E_{i1} + (1 - s_i) U_{i1} - \psi_i(s_i).$$

Within this framework, we can derive the following two propositions.

Proposition 1. If either or both the income ($\frac{\delta \phi_t(.)}{\delta c_{it}} < 0$) and exposure ($\frac{\delta \phi_t(.)}{\delta h_{it}} < 0$) mechanisms are present, expected DV risk ϕ is higher in both periods $t = 1, 2$ when either partner has lost her/his job in period 1, taking as given the other partner's employment status. *Proof*: See Appendix A.2.

Intuitively, job loss reduces income and increases exposures to DV. If at least one of these mechanisms is a relevant driver of violence within the household, DV risk should increase following job loss. The effect persists for more than one period because the effects of job loss on employment are persistent (we will show that this is the case in our data). We derive another testable implication from the model when the income mechanism is present: job loss effects on DV should be decreasing in income available to workers upon displacement because this mitigates the negative income shock of job loss. We will leverage variation in severance payments to test for this, as it adds insight on the role of the income mechanism.

When it comes to the effects of unemployment benefits, the income and exposure mechanisms move DV risk in opposite directions, and the resulting effect may differ between the first and second period. UI transfers mitigate the income loss in the first period, when benefits are paid, but increase exposure to DV in both periods.[23]

[23]Since the model assumes that individuals consume all their income in each period, UI transfers generate short-lived effects on consumption. This construction is motivated and supported by evidence showing that UI beneficiaries do little consumption smoothing. They experience sharp drops in consumption upon benefit expiration – see Gerard and Naritomi (2021) and Ganong and Noel (2019) for evidence using Brazilian and US data, respectively.

Benefits increase exposure to DV because they lower job search effort and reduce employment during and after the benefit period.[24] Hence, the dynamic effects of UI transfers on DV depend on which mechanisms are active, as summarized in the following proposition:

Proposition 2. Consider a partner who is a job loser in period 1. If only the income mechanism is present ($\frac{\delta\phi_t(.)}{\delta c_{it}} < 0$), UI transfers reduce DV risk during the benefit period, and have no effect in subsequent periods. In turn, if only the exposure mechanism is present ($\frac{\delta\phi_t(.)}{\delta h_{it}} < 0$), UI transfers increase DV risk in both periods on account of lower labor supply. If both mechanisms are present, UI transfers have an ambiguous impact on DV risk during the period 1 – the benefit period – and they increase DV risk in period 2, after benefits expire. *Proof*: See Appendix A.2.

We will test for these predictions in Section 6 to gain insight on the relevance of each mechanism.

3 Context and Institutions

Domestic violence and criminal justice in Brazil. The share of women in Brazil who report experiencing DV is 7.5% in the past 12 months, and a third over their lifetime.[25] DV notifications in the health system vary considerably across Brazil, from 15 to 116 per 100,000 thousand inhabitants in municipalities at the 25th and 75th percentiles of the distribution.[26] Rich administrative data and rich variation within Brazil make this an appealing setting for studying DV. Relative to the world, DV rates in Brazil lie just above rates in the OECD, and below rates in most other developing countries – see Appendix Figure B1. Reported rates of DV in Brazil have slowly decreased over time, from 9.6% in 1990 to 7.3% 2017 – see Appendix Figure B2 for a comparison with other large countries.

[24] This is a standard result in job search models which follows immediately from deriving the first-order condition $E_i^1 - U_m^1 = \psi_i'(s_m)$ with respect to b_i: $\frac{\delta s_i}{\delta b_i} = \frac{-1}{\psi_i''(s_i)} U'^1_i < 0$. This is consistent with the evidence we present in Section 6 as well as with extensive evidence from other countries (Katz and Meyer, 1990; Card, Chetty and Weber, 2007; Lalive, 2008; Gerard and Gonzaga, 2021).

[25] See the Gender, Institutions and Development report (OECD, 2019).The "*Central do Atendimento a Mulher - Ligue 180*", a contact line instituted in 2003 by the Ministry of Women, Family and Human Rights, attended 1.4 million requests for help in 2019, leading to 85,000 judicial investigations.

[26] Statistics based on health system notifications based on SINAN for 2013, see Section 5.4 for details on the data.

Domestic violence is a criminal offence that falls under the jurisdiction of 27 state courts, composed of 2,697 tribunals having jurisdiction over Brazil's 5,570 municipalities. The state judiciary police handles DV investigations, which are usually initiated by a victim report though they may also follow from third party reporting without the victim's consent. Following the investigation, the victim decides whether or not to file for DV prosecution, which would then lead to a trial. Importantly, the data we analyze include all reported cases, because the decision to drop the case needs to be overseen by a judge. In addition to reporting DV, women who feel threatened may file a separate request for *protective measures* (PM), introduced in 2006 by the *Maria da Penha* Law. PMs run in courts as a distinct legal instrument independent from the DV prosecution and they must be seen by a judge within 48 hours, in which case perpetrators may immediately receive a restraining order.

Labor markets. Labor law in Brazil allows firms to dismiss workers without a just cause, although it imposes severance payments. We analyze layoffs without a just cause, which account for 65% of all separations (the rest are mainly voluntary quits). Upon layoff, workers receive approximately 1.34 monthly wages for each year of tenure at the time of layoff.[27]

Workers in the formal sector that are dismissed without a just cause may be eligible for unemployment insurance. These benefits last for up to five months with an average replacement rate of 79%. Once unemployment benefits expire, the only income support at the national level is "Bolsa Família", a conditional means-tested cash transfer targeted at very poor families. In 2019, the average transfer per household was 16% of the minimum wage and the maximum per capita family income for eligibility was less than one-fifth of the minimum wage.

Our description so far refers to formal jobs. However, Brazil has a large informal sector, accounting for roughly 45% of all jobs in the analysis period. Job turnover is high in both the formal and informal sector, and workers tend to move frequently between the two. Moreover, it is not uncommon that firms hire both formal and informal workers (Ulyssea, 2018). Since there are no administrative data on informal employment, we restrict our main analysis to layoffs in the formal sector. We use survey data to quantify the degree to which informal work contributes to the recovery

[27]These includes funds from a mandatory savings account financed by the employer through monthly contributions equivalent to 8% of the worker's earnings and a severance payment equivalent to 40% of the account's balance.

of employment and earnings after job loss, and explore heterogeneity in informality rates to study whether labor informality plays a role in explaining our findings.

4 Data

Our main analyses rely on individual data obtained from the link between court and employment registers. We next describe these and other data sources, and how we link different registries.

Judicial registers. We use data on the universe of DV cases filed in all first-degree courts during 2009-18. These include information on the start and end date of the judicial case, court location, subjects being discussed, and full names of the defendant and victims.[28] In total, there are 2.4 million DV cases, comprising 1.23 million DV prosecutions and 1.17 million protective measures. The name of the defendant is available for 1 million of the 2.4 million DV cases. When studying victims, we only use data on protective measures, for which we observe the victim's name in 244,000 out of 1.17 million cases, while their names are missing in virtually all DV prosecutions. Missing data arise for two reasons: mistakes in the process of inputting data from court diaries; and judicial secrecy, which tends to protect the victim's identity.

We address missingness issues in several ways. In our main analyses, we will drop jurisdictions where the share of missing identity is above 90%. On average, offenders and victims' names are missing for 40% and 46% of the judicial cases in the main samples used for our male and female job loss analyses, respectively. In Sections 5.4 and 5.5, we show that missingness status is largely explained by court-level factors, and that our main estimates continue to hold when running the analysis within those jurisdictions where the share of missing names is quantitatively small – e.g., below 20%. Importantly, we also show that our main findings continue to hold when using alternative DV measures that do not suffer from missing data limitations.[29]

Employment registers. We use linked employer-employee data for 2009-2018 cov-

[28] We obtained these data from a private company providing information services to law firms in Brazil. The dataset is compiled from case-level information made publicly available on tribunal websites, complemented with daily diaries of courts.

[29] In any event, missingness challenges identification only to the extent that it might be related to the job status of the defendant or the plaintiff. This is unlikely to be the case, because requests for secrecy are typically made after the case has started, and we are able to capture the identity of the defendant as long as the case is started without secrecy. In addition, the threat of dismissal is not a valid legal motive for invoking secrecy.

ering the universe of formal workers in Brazil (*Relacao Anual de Informacoes Socials*, RAIS). Workers are identified by a unique tax code identifier (CPF) and their full name. The register contains rich information on each job spell such as workers' date of birth, education, earnings, and occupation, job starting and end dates, reason for separation, and firm identifiers. Since employers must provide workers with notice of dismissal at least 30 days in advance, we define the timing of layoff as the official layoff date stated in RAIS minus 30 days.[30]

Linking court and employment records. We merge the judicial and employment data using the (full) name of the individual, which is consistently and accurately reported in both registers. To ensure precision, we restrict our sample to individuals with unique names in the country – about half of the adult population.[31] We identify this sub-population by using the employment records and the register for Federal social programs (CadUnico), which together provide the name and tax identifier for 96% of the adult population, allowing us to measure the commonness of each name in the country.[32]

To assess selection into the estimation sample, we compare characteristics of male and female job losers with and without unique names. The two groups are very similar in all (observable) dimensions, the standardized difference remaining below 0.25 for all variables, indicating that any differences in the underlying distributions are small (Imbens and Rubin, 2015) (see Appendix Table C1). In any event, in Section 5.5, we will assess the sensitivity of our results by retaining all individuals with a unique name within the state (rather than the country), which extends coverage to 70% of the population. We will also show that our main results are robust to reweighting our working sample to perfectly match the characteristics of the entire population of displaced workers.

Household, public shelter and health systems data. For a subsample of our

[30]This period is extended by three days for each completed tenure year, hence considering a 30-day notice period is a conservative choice when testing the parallel trends assumption underlying our identification strategy. In practice, more than a third of workers in our sample were dismissed within a year of employment, thus with a notice period of 30 days, and 90% were dismissed with less than three years in their last job, thus with a notice period of 30-39 days.

[31]Name uniqueness rates are high because Brazilians typically have multiples surnames.

[32]This coverage rate is derived by comparing the total number of individuals in our registry with that of national population statistics, supplied by the Brazilian Institute of Geography and Statistics (IBGE). Restricting attention to adult individuals does not generate measurement error, because we only observe court cases for individuals who are above the legal age of 18.

data, we are able to link couples and families using the registry for Federal social programs (CadUnico).[33] Due to the nature of the registry, it mainly overlaps with the lower and middle part of the income distribution in our main panel. To validate our main results, we will also use data on access to DV public shelters by women and mandatory DV notifications by health providers as alternative measures for domestic violence (see Section 5.4).

5 Job Loss and Domestic Violence

In this section we test the first main prediction of the model, namely that both male and female job loss increase the probability of observing domestic violence, and that such effects are persistent over time.

5.1 Descriptive evidence

The upper panel of Figure 1 shows the probability of DV perpetration (men) and victimization (women) in our sample by employment status and age. DV risk peaks around age 30-35, and declines thereafter. The probabilities of both perpetration and victimization are higher among displaced workers than among employed workers. Of course, the difference between the two groups may reflect both causal effects and selection into job loss; in the remainder of this section, we aim to isolate the former. The graphs in the lower panel of Figure 1 show that the probability of DV perpetration or victimization upon job loss is decreasing in job tenure, an association that we will investigate further. These graphs also illustrate that many jobs are terminated with low tenure, which is in line with the high turnover rate in the Brazilian labor market.

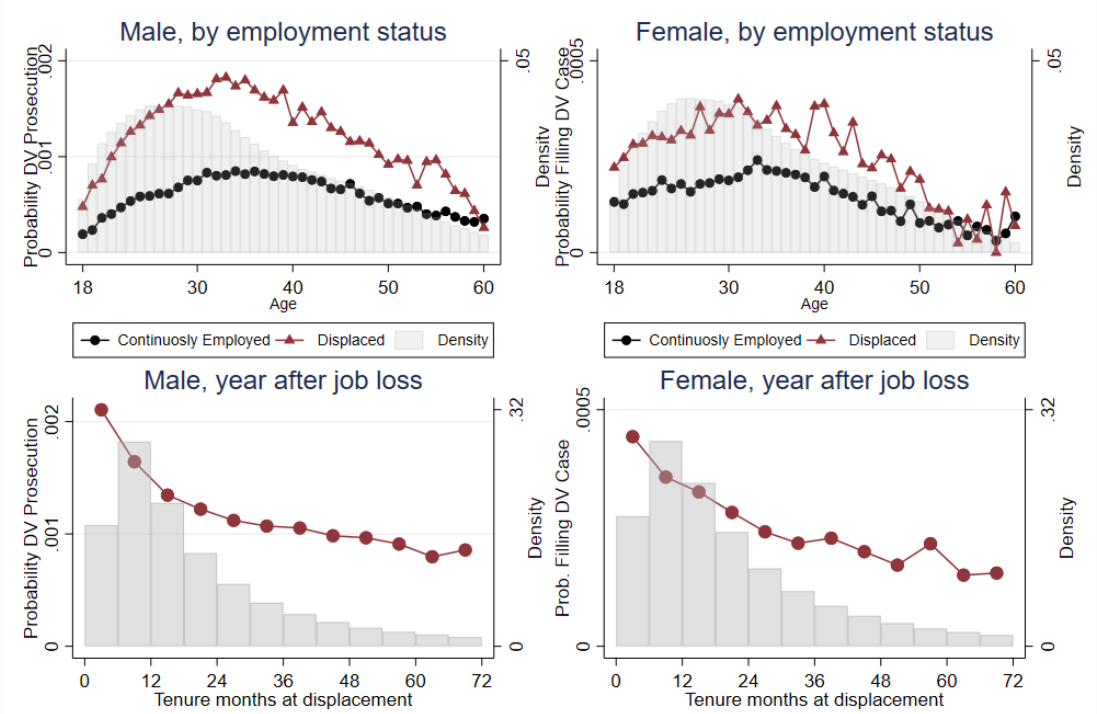
5.2 Identification strategy

We use a difference-in-differences strategy where we define as treated all workers displaced in mass layoffs between 2012 and 2014 – the central years within our sample period, 2009-2018.[34] We use a perfectly balanced panel tracking units from three years

[33]The registry is maintained by the Federal government for administering welfare programs such as Bolsa Familia.

[34]Our baseline definition of mass layoffs includes firms with 30 or more workers dismissing at least 33% of their workforce without just cause in a given a year, similar to Jacobson, LaLonde and Sullivan (1993) and Couch and Placzek (2010). We also exclude firms reallocating under a new

Figure 1: Domestic violence by employment status, age and tenure



Notes: The top graphs compare the yearly probability of DV perpetration in DV suits for men and DV victimization in protective measures for women, comparing workers that are continuously employed to workers losing their job in each year by age. The bottom graphs present the same measures for job losers one year after layoff. The distribution of age and tenure are displayed in gray, right-axes.

before to four years after treatment, and estimate anticipation and dynamic treatment effects throughout the same period.

The pool of potential control workers includes all individuals employed in firms that did not engage in mass layoffs during the analysis period. We leverage the vastness of the data to identify control workers who are not displaced in the same calendar year and are exactly matched on birth cohort, job tenure (by year), earnings category (by R$250/month bins), firm size (quartiles), one-digit firm sector (9), and state (27). In cases where a treated worker is matched with multiple controls, one is randomly selected. The matching process is run separately for men and women, with over 80% of displaced workers being successfully matched to a control, who receives a placebo dismissal date equal to the layoff date of the matched treated worker. We compare changes in outcomes among treated and control workers, before and after

identifier, where reallocation is defined as at least 50% of workers displaced from a firm being found in a new firm by the start of the following year.

dismissal, using the following difference-in-differences equation:

$$Y_{it} = \alpha + \gamma Treat_i + \sum_{t=-P, t\neq 0}^{T} \delta_t (Treat_i * Time_t) + \sum_{t=-P, t\neq 0}^{T} Time_t + \epsilon_{it}. \quad (1)$$

Workers are identified by subscript i, and $Treat_i$ is an indicator for being displaced in a mass layoff. Dummy variables $Time_t$ identify years since layoff, and they are precisely defined using the exact date of layoffs and DV outcomes. Therefore, $t = 0$ for the 12 months before layoff, $t = 1$ for the first 12 months after layoff, $t = -1$ for the 12 months preceding the year before layoff, and so on; the coefficients $\{\delta_1, ..., \delta_T\}$ identify dynamic treatment effects, whereas $\{\delta_{-P}, ..., \delta_{-1}\}$ estimate anticipation effects.

The stacking approach centering the analysis around the treatment timing and using never-treated workers as controls addresses concerns regarding the estimation of two-way fixed-effects models with staggered treatment across units. This follows Britto, Pinotti and Sampaio (2022) and Cengiz et al. (2019), and is line with the recent methodological work by Dube et al. (2023). We also show that our results are robust to using other estimators and diagnostics proposed in the recent methodological literature.

To summarize the magnitude of the effects following job loss, we also estimate the equation:

$$Y_{it} = \alpha + \gamma Treat_i + \beta (Treat_i * Post_t) + \lambda Post_t + \epsilon_{it}, \quad (2)$$

where the dummy $Post_t$ identifies the entire period after layoff, and all other variables are defined as in equation (1).

The difference-in-differences design compares the same workers before and after job loss, ensuring that individual fixed factors, such as age, education and characteristics of the job lost, do not directly affect the estimates.[35] The key purpose of our exact matching strategy is finding a suitable control group that replicates the evolution of outcomes for the treatment group in the pre-displacement period, so that the common-trend assumption is supported. Table 1 shows that treated and (matched) control workers are fairly balanced on a rich set of observable characteristics. The standardized difference between the two groups remain below 0.25 (Imbens and Rubin, 2015) indicating that any differences in the underlying distribution are small for

[35] In fact, coefficient estimates in equations (1) and (2) remain exactly the same when we include individual fixed-effects to the model. Moreover, estimates also remain identical when adding calendar time fixed effects, in addition to relative time fixed effects.

all variables (including several attributes not used for matching such as race, occupation, municipality characteristics and the probability of DV in the pre-displacement period). The only exception is education in the male worker sample – treated workers have 10.0 years of education relative to 10.9 in the control group. In Section 5.5, we show that our main results are robust to adding education to the matching process, and to reweighting the control group to perfectly match all the characteristics of the treatment group. Table 1 also shows that the characteristics of workers displaced in mass layoffs are not largely different from the pool of all displaced workers. This attenuates concerns regarding the external validity of our analysis based on mass layoffs – we address this potential issue in detail in Section 5.5.

The main challenge to identification is dynamic selection into displacement. Parallel trends between treated and control workers in the pre-treatment period attenuate but do not fully address this concern as idiosyncratic, time-varying shocks causing higher DV and layoff risks in a given year may not be revealed in differential pre-trends. Our focus on mass layoffs minimizes this concern, as these events depend on firm-level shocks rather than on the individual behavior of workers (see e.g. Gathmann, Helm and Schönberg, 2020). We provide several robustness tests for potential selection issues and extensively assess the sensitivity of the results to changes in the definition of mass layoffs in Section 5.5.

5.3 Dynamic treatment effects of male and female job loss

We first discuss the effects of job loss on labor market careers. Figure 2 plots the estimated effects of male and female job loss in a mass layoff on labor income using the specification in equation (1). All estimates in the paper are re-scaled by the average outcome level in the treatment group in the year before layoff.[36]

Labor income is 70% lower relative to the baseline after male layoff, followed by a continuous but slow recovery in the subsequent years. Four years after the shock, the negative impact on labor income remains as high as 36%. The estimates are remarkably similar for women, as shown in the right panel of Figure 2. In Appendix D.1, we show that job loss also has an overall adverse and persistent impact on employment, monthly wages, and job turnover. In Appendix D.2, we use survey data

[36]The focus on relative effects is mainly motivated by the strong under-reporting in DV outcomes, so that it is more meaningful to think about relative variations. This is also in line with the general crime literature which faces similar under-reporting issues.

Table 1: Treatment and control groups descriptive statistics, male and female job loss

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main analysis: mass layoffs						All layoffs	
	Male Job Loss			Female Job Loss				
	Treatment	Control	Std Diff	Treatment	Control	Std Diff	Male	Female
Demographic characteristics								
Years of education	10.0	10.9	0.33	11.5	11.7	0.06	10.9	11.7
Age	30.3	30.3	0.00	30.5	30.5	0.00	29.7	29.8
Race - white	41.8%	45.2%	0.07	46.6%	46.5%	0.00	52.1%	55.4%
Race - black	5.7%	5.3%	- 0.02	3.1%	3.8%	0.04	4.7%	2.8%
Race - mixed	43.8%	42.1%	- 0.03	39.0%	40.7%	0.03	34.4%	31.2%
Job characteristics								
Monthly income (R$)	1,438	1,445	0.01	1,063	1,075	0.02	1,411	1,056
Month of worked $t-1$	10.7	11.2	0.17	11.2	11.5	0.09	11.1	11.3
Tenure on Jan 1st (years)	1.1	1.1	0.03	1.4	1.4	0.01	1.6	1.6
Manager	2.5%	4.8%	0.12	6.0%	7.2%	0.05	5.2%	7.5%
Firm size (employees)	724	600	- 0.07	667	560	-0.07	454	419
Local area - municipality								
Large municipality - pop> 1M	42%	44%	0.04	37%	37%	-0.02	36%	30%
Municipality population	2,601,919	2,696,668	0.02	990,340	976,942	-0.01	2,316,118	825,364
Homicide rate (per 100k inhab.)	32.8	31.6	- 0.06	40.8	38.2	-0.12	29.2	34.7
Domestic Violence								
Prob. of DV suit or PM $t-1$	0.0015	0.0011	- 0.01	-	-	-	0.0013	
Prob. of DV suit $t-1$	0.0006	0.0005	- 0.01	-	-	-	0.0006	
Prob. of PM $t-1$	0.0009	0.0006	- 0.01	0.0007	0.0007	0.00	0.0008	0.0007
Observations	810,926	810,926		90,940	90,940		4,219,087	960,396

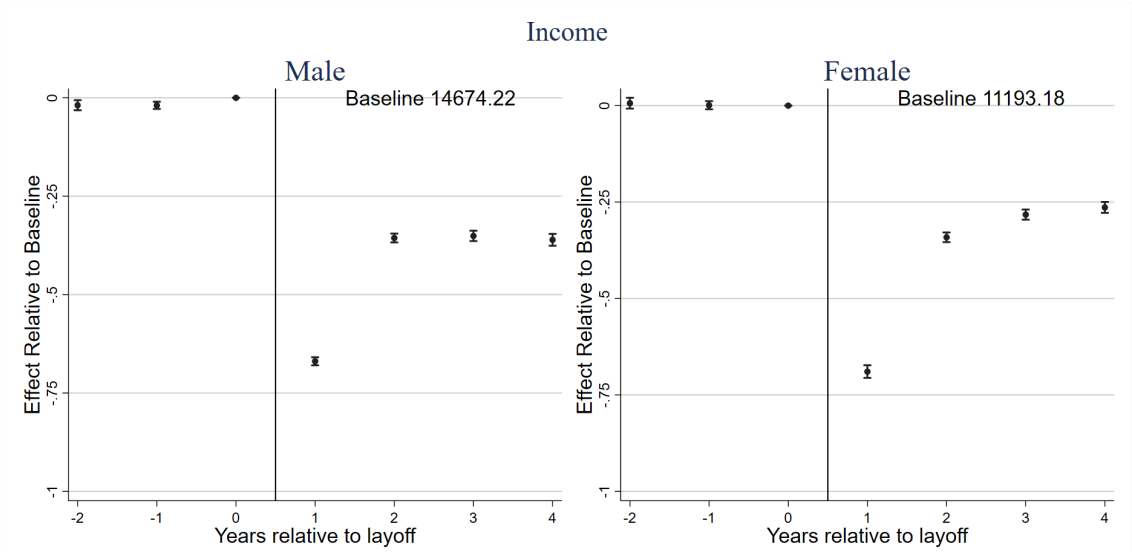
Notes: This table reports by gender the average characteristics for treated workers displaced in mass layoffs, respectively (columns 1 and 4); for matched control workers who are not displaced in the same calendar year (columns 2 and 5); the standardized difference between the two groups (columns 3 and 6); and the average characteristics of workers displaced in any type of layoff(columns 7-8).

to show that the impact of job loss on income is about 10% smaller when we account for informal sector income of displaced workers. Hence, the impact on total income remains substantial even when taking informal work into account.[37]

We next examine how male job loss influences domestic violence, as measured by either DV prosecutions or protective measures. As shown by the left graph in Figure 3, job loss by men causes a sharp increase in the probability of domestic violence in the year following job loss, which persists through the following years. The average

[37]This also implies that our estimates for the elasticities of DV to formal income will (slightly) underestimate elasticities to total income.

Figure 2: The effect of job loss on labor income

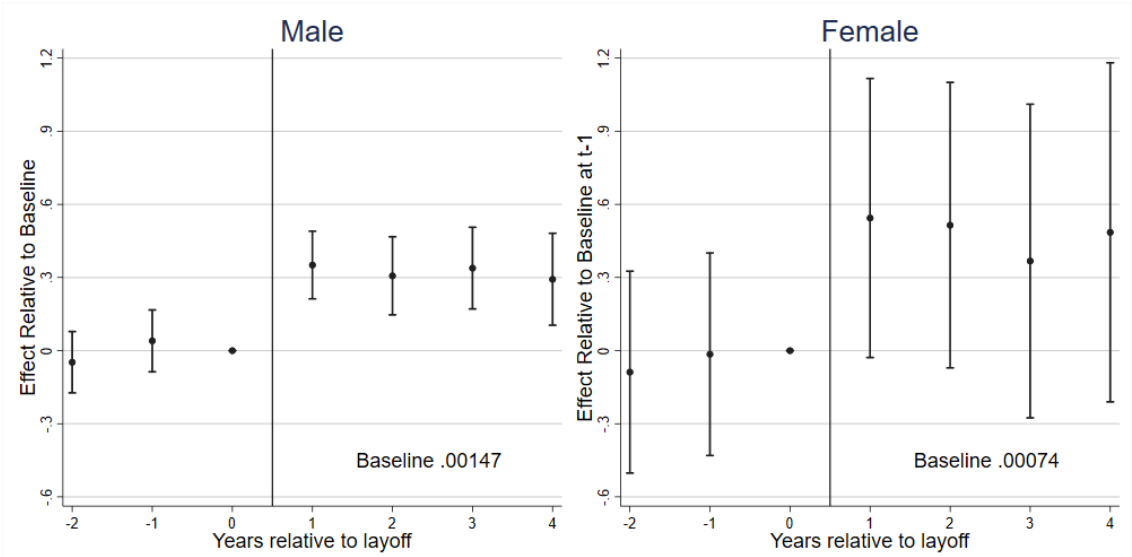


Notes: This figure shows the effect of job loss on formal labor income by gender, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Income variables are measured in Brazilian Reais.

effect over the post-treatment period amounts to a 32% increase in the probability of DV relative to the baseline rate (Panel A of Table 2, column 3). When distinguishing between DV prosecutions and protective measures, the effect is +40% on the former and +30% on the latter (columns 4-5).

Turning to DV victimization, the right graph in Figure 3 shows that female job loss sharply increases victimization in the year following layoff, and that this effect persists for at least four years. The average effect indicates a 56% increase over the baseline (Panel B in Table 2, column 5). The relative effect is larger than the effect of male job loss, although the samples are not based on exactly the same jurisdictions and the female job loss estimates are less precise, being estimated on a smaller sample (see Section 4). In Appendix D.3, we show that the coefficients are similar if we estimate both effects on the same, smaller sample (we cannot reject the null hypothesis that they are equal with a p-value of 0.45).

Figure 3: The effect of male and female job loss on domestic violence, judicial suits



Notes: This figure shows the effect of job loss on the probability of DV perpetration in DV suits for men and DV victimization in protective measures for women, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Table 2: Effect of job loss on labor market outcomes and domestic violence

	(1)	(2)	(3)	(4)	(5)
	Labor market effects		Probability of DV		
Dependent variable:	Employment	Income	Any	DV Prosecution	Protective Measure
PANEL A: MALES DISPLACED IN MASS LAYOFFS, DV PERPETRATION					
Effect of job loss	-0.22***	-6187.2***	0.00048***	0.00025***	0.00028***
	(0.002)	(72.5)	(0.00008)	(0.00005)	(0.00006)
Mean outcome, treated at t=0	1	14,674	0.0015	0.0006	0.0009
Effect relative to the mean	-22%	-42%	32%	40%	30%
Elasticity to earnings			-0.77	-0.95	-0.70
Observations	11,352,964	11,352,964	11,352,964	11,352,964	11,352,964
PANEL B: FEMALES DISPLACED IN MASS LAYOFFS, DV VICTIMIZATION					
Effect of job loss	-0.23***	-4440.5***	-	-	0.00040***
	(0.004)	(68.6)	-	-	(0.0001)
Mean outcome, treated at t=0	1	11,193	-	-	0.0007
Effect relative to the mean	-23%	-40%	-	-	56%
Elasticity to earnings			-	-	-1.41
Observations	1,273,160	1,273,160	-	-	1,273,160

Notes: This table shows the effect of job loss on labor market outcomes (columns 1-2) and DV perpetration/victimization outcomes (columns 3-6), for males in Panel A and females in Panel B, as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Taken together with the sustained labor market losses due to job loss (documented in Figure 2 and Appendix Figure D1), the persistent increases in DV risk following male and female job loss are consistent with the predictions of our theoretical framework (see Proposition 1, Section 2). In Appendix D.4, we further explore the persistence of these effects. In particular, we show that job loss causes a sustained increase in both the probability of the first DV event and in the incidence of recurrent DV. Therefore, once initiated DV tends to persist within couples, in line with the fact that one fourth of perpetrators are charged more than once over the ten year period covered by our sample. These results are also consistent with anecdotal evidence that only a small share of DV cases leads to conviction and prison, which could otherwise interrupt the sequence of DV events.[38]

5.4 Under-reporting of judicial cases and alternative measures of DV

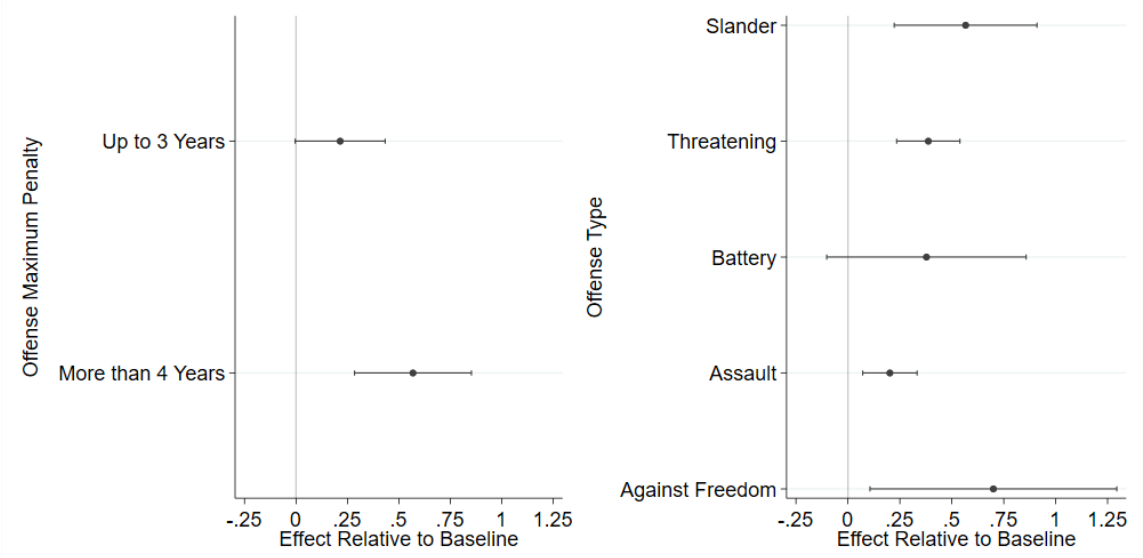
There is widespread under-reporting of DV and the decision to report may depend on several factors, e.g., related to gendered norms and the economic interdependence of the couple. In particular, if a woman is financially dependent on her partner, she might be more likely to report him for DV once he loses his job, which could generate an upward bias to our estimates on the impacts of male job loss on DV prosecution. In turn, if a woman is less likely to report violence once she loses her job, this could generate a downward bias to our estimates on the impacts of female job loss on DV victimization.

We assess whether reporting bias drives our estimates for male job loss in two ways. First, we exploit variation in the intensity of violence, measured by the type of DV reported and jail time sentence. We expect that more severe offenses are less sensitive to reporting issues. Hence, if our findings were purely driven by changes in reporting behavior, one should expect the impacts to be driven by less severe offenses. On the contrary, Figure 4 shows that male job loss has stronger impacts on DV offenses leading to longer jail times (left graph), and that the effect is pervasive for all types of DV cases (right graph).[39] Hence, the increase in DV after job loss is not purely driven by changes in reporting of less serious offenses.

[38] Using sentence data from the State of São Paulo, we observe that only 36% of cases end up with a conviction, among which only 17% include a sentence to prison.

[39] In the left graph of Figure 4, we distinguish between jail time sentences of up to 3 years vs. 4 or more years, respectively, because the Brazilian legislation classifies the former as mild crimes and the latter as ordinary crimes.

Figure 4: The effect of male job loss on domestic violence by offense intensity



Notes: This figure shows the effect of male job loss on the probability of DV perpetration in DV suits by type and maximum penalty in the four years after the layoff, as estimated from the difference-in-differences equation (2) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The post-treatment coefficient is rescaled by the average value of the outcome in the treated group at $t = 0$. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

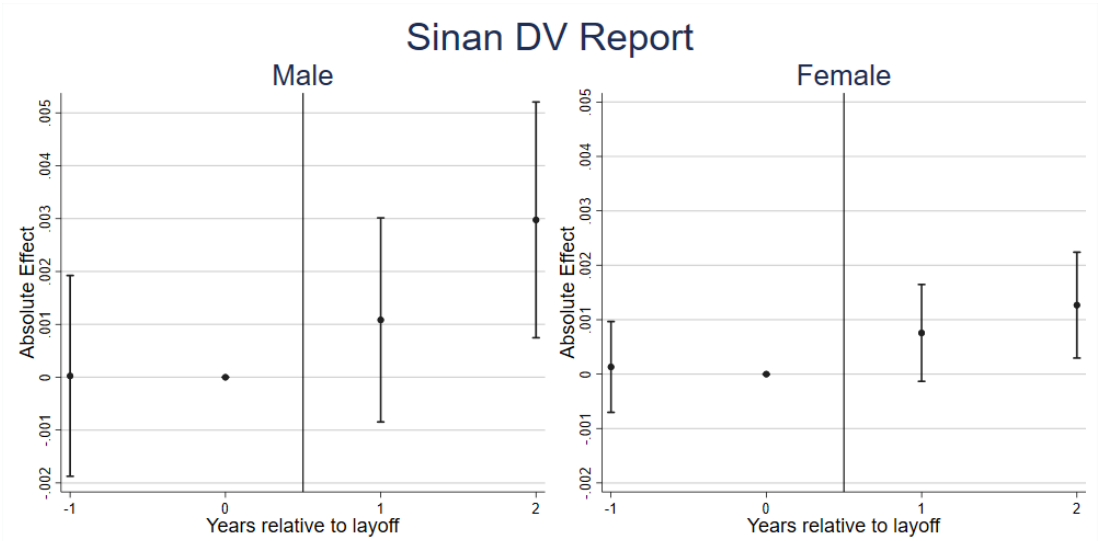
Our second strategy is to replicate the analysis using alternative measures of DV that depend less, if at all, on discretion in reporting because they are reported by third parties. First, we analyze DV cases initiated "in flagrante" by police officers, possibly called by a third party (e.g., a neighbor or a bystander on the street). These circumstances attenuate the risk of reporting bias. The estimated effect of male job loss on this restricted subset of cases is virtually identical to the baseline estimate including all DV cases (see Figure D4 in Appendix D.5.A). Second, we study women's use of public shelters for DV victims, available in Cadunico register for 2011-2013. This is less prone to reporting bias because, unlike judicial prosecutions, it does not directly implicate the male partner. Table D2 in Appendix D.5.A shows that male and female job loss increase the use of DV shelters by the female partner by 24% and 46%, respectively.

Our third and preferred alternative measure is based on mandatory DV reports by the health system, available for 2010-2017. All public and private health units in Brazil must file a DV notification in the Sistema de Informação de Agravos de Notificação (SINAN) system when they suspect or know that their patients are victims of

DV. This generates an ideal measure of DV incidents, as the information is mandatory, reported by a third party, and includes both mild and severe cases (in contrast to DV-related hospitalization mainly covering severe cases – a measure previously used in the DV literature, e.g. by Aizer, 2010).[40] Moreover, these notifications are not sent to the police or judicial authorities, so fear of retaliation from offenders should be a lesser concern.

One complication with the health notifications data is that they do not provide individual identifiers. In Appendix D.5.B, we describe the data linkage procedure based on (clusters of) exact birth date, municipality, and gender; along with validation exercises. The results for DV notifications are presented in Figure 5. They confirm our main finding that both male and female job loss lead to an increase in DV. Appendix D.5.B provides robustness exercises and show that the effects relative to the baseline retain the same order of magnitude of our main analysis.

Figure 5: The effect of male and female job loss on domestic violence, health system DV notifications



Notes: This figure shows the effect of job loss on the incidence of DV in SINAN reports – health system mandatory notifications on DV victims – for displaced men's female partners and displaced women, respectively, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Overall, the results using alternative DV measures that are less subject to report-

[40] Perova, Reynolds and Schmutte (2021) uses the same SINAN data to study the relationship between the gender wage gap and DV.

ing bias confirm that our baseline estimates based on legal prosecutions capture actual increases in DV upon job loss, as opposed to pure changes in reporting behavior.

Our different measures likely track different types of DV cases. Court cases may lean towards more severe cases which exceed women's tolerance level for undertaking judicial measures, although they also cover DV events which do not involve physical violence such as slander and threats. The measure of DV shelter may capture even more extreme cases where women decide to leave the household and seek a safe place for living, protected from the risk of violence by the partners. In turn, SINAN reports likely cover a broader range of DV cases (mainly) involving physical violence. The fact that our results hold for all measures indicates a pervasive increase in different types of DV. In Appendix D.5.C, we show how protective measure in courts vary around the timing when women show up as DV victims in SINAN reports, which are our most accurate measure on the timing of violence. It shows that DV protective measures in court sharply increase in the same year, indicating that they reasonably track the timing of violence events. In addition, we show a negative correlation between PM and DV shelter use, indicating that women may substitute between these two alternatives for seeking protection.

5.5 Robustness

In Appendix D.6, we assess the sensitivity of our baseline estimates to several other robustness checks. Overall, the goal is testing the robustness of our key finding: the fact that both male and female job loss lead to a substantial increase in DV risk. We address several threats to this conclusion by varying the specification, the sample, and econometric estimators. Although point estimates vary to some extent with these tests, they generally support our key findings, showing results that retain the same direction and order of magnitude relative to our main estimates.

First, we show that our main estimates are robust to adding education to the matching process and to reweighting the control group to perfectly match all observable characteristics of the treatment group (Appendix D.6.A). This addresses the fact that there is some residual imbalance in education in our baseline matching strategy (Table 1). Second, estimates using quarterly data provide further support for the hypothesis of common pre-trends (Appendix D.6.B).[41] These results also confirm that

[41]The results for alternative measures of DV discussed in previous Section 5.4 – namely, "in flagrante" cases, DV shelters, and SINAN reports – also allow for a better inspection of pre-trends

impacts on DV emerge quickly after the layoff. Third, the results are robust to the inclusion of fine-grained municipality-industry-year fixed effects, indicating that our results are not driven by specific area-level employment trends and that using individual matched controls finely absorbs area level shocks (Appendix D.6.C).

Fourth, we address selection into treatment by showing that the results remain robust when using stricter mass layoff definitions and plant closures, which severely reduce the room for selection. We also implement an intention-to-treat approach that addresses selection by considering as treated *all* workers in mass layoff firms, i.e. both displaced and non-displaced (Appendix D.6.D). This eliminates the room for firm discretion in choosing which workers to displaced during a mass layoff.

Fifth, we address the fact that our estimates based on mass layoffs could be affected by spillovers across multiple displaced workers (Appendix D.6.E). We show that we reach similar results when focusing on layoffs which should generate little spillover effects, namely mass layoffs with fewer displaced workers or regular layoffs (although the latter is more subject to endogeneity concerns). In addition, our results remain similar when looking at smaller municipalities where mass layoffs represent a larger share of the workforce and where spillover effects should play a larger role.

Sixth, we address the issue of missing victim and perpetrators' names in our judicial data (Appendix D.6.F). We show that missing name status does not strongly vary with case characteristics and that over 50% of the variation in missing status is driven by court-level fixed factors. More importantly, we show that our findings remain robust even when focusing exclusively on jurisdictions where such issues are not quantitatively relevant. In addition, our findings are robust when focusing on "in flagrante" cases which are less subject to name secrecy (Appendix D.5.A), and when using alternative DV measures which do not suffer from missing issues (Section 5.4).

Seventh, we address the fact that our main analysis is restricted to individuals who have unique names in the country. In addition to showing that there is no strong selection over name uniqueness (Table C1), our findings remain similar when increasing the representativeness of our sample in different ways – see Appendix D.6.G. Eighth, we address issues related to staggered treatment in difference-in-differences designs in Appendix D.6.H. Ninth, we address the fact that we study a low probability out-

than the baseline measure based on DV suits, as the former are immediately filed in courts, thus avoiding any lag between the date of violence and judicial prosecutions. The same is true for protective measures, which we use in our baseline estimates of the effect of female job loss on victimization (see Section 4).

come with a difference-in-differences design (Appendix D.6.I). We show the predicted counterfactual probabilities remain in the range zero-one, and that our main findings are robust to different DID estimators well suited to deal with such outcome. Finally, Appendix D.6.J provides additional tests related to pre-trends testing and the validity of the common-trend assumption in our setting.

5.6 Heterogeneity by Worker and Area Characteristics

We now investigate how the effects of job loss on DV vary by worker characteristics, namely age, education, income and tenure at displacement. We focus on male job loss because it is difficult to derive meaningful comparisons in the smaller sample of female layoffs as, once we create sub-groups, the estimates are imprecise. Since workers' characteristics are correlated with one another, we also estimate models in which all coefficients in the equation (2) are interacted with third-order polynomial controls on all other individual-level characteristics.[42]

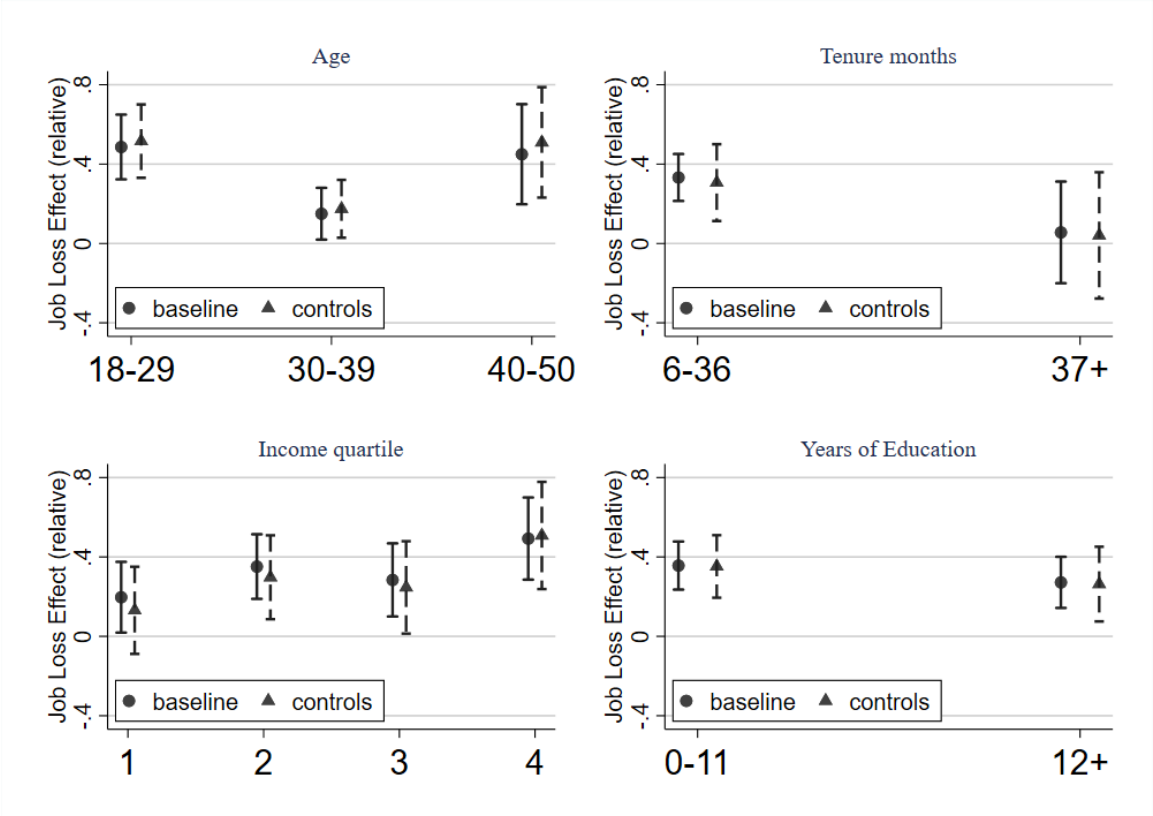
The first striking pattern in Figure 6 is that DV following male job loss is remarkably pervasive, being evident across the entire distributions of age, income and education. In Appendix Figure D11, we also show that the effect is also remarkably pervasive across a range of area-level characteristics – including baseline DV levels, the gender pay gap, informality rates and GDP per capita, despite the vast heterogeneity across Brazilian regions. In turn, Figures D12 and D13 in the Appendix show that impacts on labor income do not vary much over the same set of individual and area-level characteristics.

To provide evidence on the income mechanism, we analyze heterogeneous effects over tenure, exploiting the fact that severance pay is increasing in tenure. In the top-right panel of Figure 6, we compare workers displaced with 3 or more years in the job – who receive on average 7 months' wages in severance pay – to those displaced with 6-36 months tenure – who receive on average less than 2 months' wages.[43] Job loss raises DV for workers with 6-36 months tenure but it has a small

[42]Namely, when running the heterogeneity over characteristic c, we control for third-order polynomials on each other characteristic $-c$ using continuous variables which are interacted with all coefficients in our baseline DID model in equation (2). For example, when analyzing heterogeneity over age groups, we interact all coefficients in eq. (2) with dummies indicating age groups and, as controls, with third-order polynomials on all other (demeaned) characteristics (tenure, income, education, and nine area-level characteristics – see Appendix Figure D11).

[43]We refer to severance pay as the total amount received from the mandatory savings account and the indemnity paid by the employer upon displacement (see Section 3). We estimated such amount

Figure 6: The effect of male job loss on domestic violence, judicial suits, by individual characteristics



Notes: This figure shows the effect of male job loss on the probability of DV perpetration in DV suits in the four years after layoff – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the eq. with third-order polynomials on individual characteristics. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

and statistically insignificant impact on DV among high tenure workers, suggesting that liquidity at displacement may be a mechanism driving DV (Appendix Figure D14 shows dynamic effects for the two groups, revealing the same patterns). These results are not affected by the inclusion of controls, indicating that differential effects by tenure are not capturing differential effects by age, education, and income; or other measurable area-level factors. These results are in sharp contrast to all other dimensions of heterogeneity showing pervasive effects of job loss on DV.

based on tenure and earnings information available in the employment data. We focus on workers with at least 6 tenure months who meet the eligibility requirement for UI. We analyze UI impacts in Section 6.

Next we show how the impacts on DV vary by more granular tenure groups. Figure 7a shows that the effect of job loss on DV is decreasing over tenure, and that the intensity of the gradient mirrors the average amount of severance pay, which is increasing over tenure. There is a considerably smaller and statistically insignificant effect for high tenure workers entitled to receive large sums of severance pay. The most likely explanation is that tenure proxies liquidity at displacement, and that liquidity ameliorates the impact of job loss on DV. This pattern is also consistent with the fact that consumption losses following layoff in Brazil are decreasing in tenure (Gerard and Naritomi, 2021) and that job search is sensitive to cash on hand only among low-tenure workers (Britto, 2022). In turn, Figure 7b shows that job loss effects on months worked do not greatly vary over tenure. This indicates that there is little variation in exposure to DV risk over this dimension, which reinforces the role of the income mechanism as the likely driver of the tenure gradient.

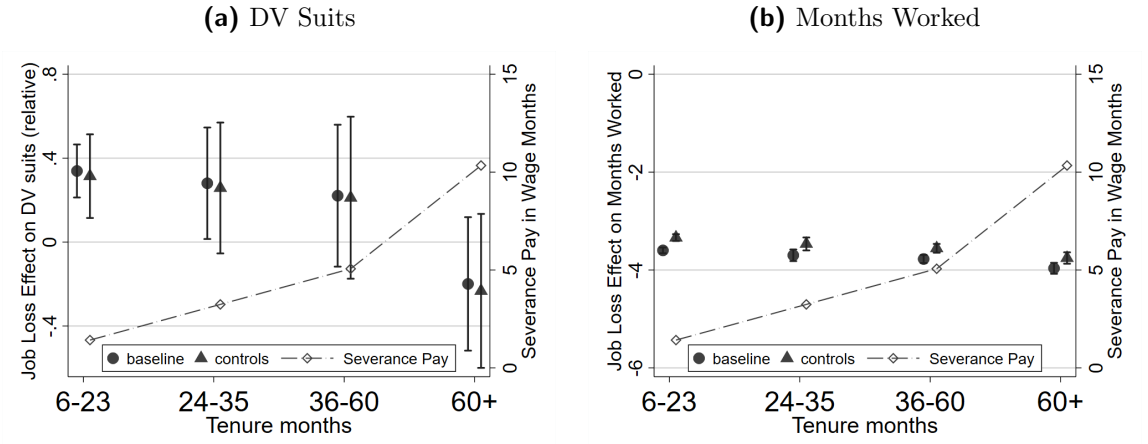
Overall, these results support the relevance of the income mechanism described in our theoretical framework. In the next section, we provide further evidence on mechanisms leveraging quasi-experimental variation in access to unemployment benefits.[44]

5.7 Couples Data

So far, we have studied the effects of male and female job loss separately. Not all DV cases occur within couples; they may involve, in fact, non-cohabiting couples, ex-partners, and non-partners. Since theoretical models of DV (including the one that we introduced in Section 2) are conceptualized for couples, we show in Appendix D.7 that our main findings hold within couples identified in CadUnico. Using these data, we also report heterogeneous effects by baseline household characteristics. While the sub-group coefficients are not significantly different from one another, the results are broadly in line with the mechanisms we propose. Finally, we show that male and female job loss have no significant impact on partner's employment. The fact that partners do not work more after one is laid off supports the underlying assumption of the exposure mechanism – the idea that partners spend more time together after

[44]In Appendix D.9, we investigate whether the take-up of informal jobs after (formal) job loss could play a role explaining our findings, as an additional mechanism. In particular, since these jobs could be more risk and stressful, they could be a driver of higher DV. However, our results remain similar when focusing on workers who are less exposed to labor informality due to their location and sector of work. Hence, we do not find much support for the hypothesis that labor informality plays a major role in explaining our findings.

Figure 7: The effect of male job loss on domestic violence and months worked, and access to severance pay by tenure groups



Notes: This figure shows the effect of male job loss on the probability of DV perpetration in DV suits (left) and month worked (right) in the four years after layoff – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the eq. with third-order polynomials on individual characteristics. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients in the left panel are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

layoff.

We also show that male job loss does not influence the probability of separation, while female job loss has a small impact on this outcome.[45] One possible explanation for this findings is that marital dissolution rates are relatively low in Brazil, in line with Latin American countries (Molina and Abel, 2010).[46] Hence, separations could be constrained by social norms despite the increased risk of domestic violence. At the same time, these results do not necessarily imply that higher DV after job loss does not lead to couples' separations. We may lack statistical power to detect such effects because only a relatively small share of women suffers DV after job loss. Consistent with this idea, in Appendix D.8 we show that couples are 11 p.p. less likely to live together in the three years after the first DV event is filed in courts. Although we do

[45]Previous literature which finds mixed results for the effects of job loss on divorce. While Eliason (2012) finds a 13% increase in divorce rates after job loss using Swedish data, Huttunen and Riukula (2019) find no statistically significant effect using Finnish data, similar to us. In turn, Charles and Stephens (2004) find positive effects when analyzing regular layoffs, but no effects for plant closures, using PSID survey data for the US.

[46]Crude divorce rates in Brazil are .9 as of 2006, ranking 81st out of 94 countries for which data is available (Ortiz-Ospina and Roser, 2020).

not attach a causal interpretation to these estimates because treatment is endogenous, this evidence suggests that separation is, at least to some extent, a feasible option despite the low dissolution rates in Brazil.

6 Unemployment Benefits and Domestic Violence

We now investigate whether unemployment benefits mitigate the impact of male job loss on DV.[47] Our goal is twofold. The first is evaluating the impacts of the most common policy supporting displaced workers around the world. The second is gathering further evidence on the income and exposure mechanisms described in our theoretical framework.

6.1 Research Design

Brazilian formal sector workers dismissed without a just cause are eligible for UI benefits as long as they have been in continuous employment for at least 6 months before layoff.[48] The maximum benefit duration ranges from 3 to 5 months. For repeated claimants, at least 16 months must have elapsed since their last layoff resulting in a benefit claim. We proceed by retaining workers with at least 6 tenure months and implementing a regression discontinuity (RD) design at the 16-month eligibility cutoff for repeated claimants.[49,50] We compare the behavior of workers who are barely eligible and ineligible as follows:

$$Y_i = \alpha + \beta D_i + f(X_i) + \epsilon_i, \tag{3}$$

where Y_i is an outcome for i-th worker; X_i is time elapsed since the previous layoff resulting in a UI claim (the running variable), standardized so that $X = 0$ at 16 months, the eligibility threshold; $f(.)$ is a flexible polynomial with varying coefficients

[47] We focus on males because the number of females workers is too small in the RD analysis, leading to imprecise estimates (i.e., statistically indistinguishable from zero without being precisely estimated zeros).

[48] UI benefits in Brazil come with no conditionalities such as minimum job search requirements or participation in training programs.

[49] We use data on UI payments to restrict the sample to workers who exhausted all months of UI benefits following the initial displacement. This makes the first-stage around the 16-months cutoff stronger, since workers who did not use the 5 months can claim unused benefits when they do not meet the 16-month requirement.

[50] We cannot exploit the 6-month cutoff rule because there is evidence of manipulation around this cutoff Gerard and Gonzaga (2021).

on each side of the cutoff; and D_i is an indicator for eligibility (i.e. $D = 1(X_i \geq 0)$). The coefficient β in equation (3) estimates the effect of UI eligibility, or equivalently, the intention-to-treat effect of UI claims. We use data on UI payments to quantify the share of workers taking UI benefits, their total amount and duration. The main estimates are based on a local linear model with a narrow bandwidth of 45 days, but we check the sensitivity of our results to different polynomial specifications and bandwidths (including the optimal bandwidth of Calonico, Cattaneo and Titiunik, 2014). We will also perform permutation tests, comparing our estimate at the true cutoff with a distribution of estimates at placebo cutoffs.

6.2 Data and Balance Tests

In order to increase statistical power of the RD analysis, the sample includes all workers who have unique names in the state (about 70% of the universe of workers), rather than only workers with a unique name in the entire country as in the analysis of job loss (about 50% of the universe).[51] We restrict attention to workers displaced during 2009-14 because numerous changes to UI were implemented in 2015.

Cyclical peaks in layoffs on the first and last days of the month (see Appendix Figure E1) generate discontinuities in the density of the running variable about every 30 days that are not specific to the 16-month cutoff.[52] In our baseline specification, we address this issue by restricting the sample to workers initially dismissed between the 3rd and 27th of the month, so that the 16-month cutoff date does not overlap with the monthly dismissal cycles. Importantly, this restriction is based on the initial layoff date which determines the RD cutoff, and not the current layoff date determining the running variable. Figure E2 shows no evidence of density discontinuity around the 16-month cutoff in this restricted sample, as also confirmed by the McCrary density test (McCrary, 2008) and the bias-robust test developed in Cattaneo, Jansson and Ma (2018, 2020). In addition, Figure E3 in the Appendix shows that a rich set of

[51] Accordingly, we match the employment and judicial registers based on name and the state where the worker and the court are located. Panel B of Table D6 showed robustness of the job analysis to using the state level restriction.

[52] Workers who are initially displaced close to the last day of the month are more likely to be dismissed again on the last day of any month (including the 16-month eligibility cutoff). For instance, a worker dismissed on January 1st 2010 will be able to claim benefits again if dismissed from April 30st 2011. Given the dismissal cycle, when re-employed, s/he will be more likely to be displaced on the last day of the month – April 30st 2011 – rather than during the days immediately before, which creates a mild discontinuity in the density function. However, this discontinuity is not specific to the 16-month period that is relevant for UI eligibility.

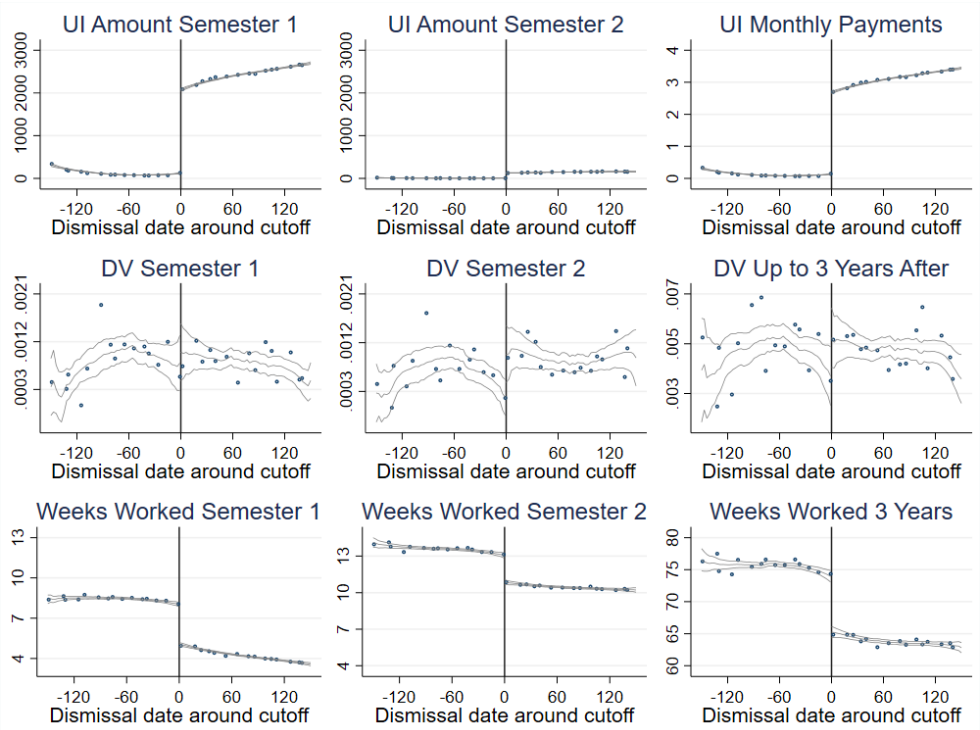
pre-determined worker characteristics are balanced at the cutoff; most importantly, there are no significant differences in DV prosecution rates before displacement (Table 3, Panel C). Overall, these results provide compelling evidence that displaced workers are as good as randomly assigned near the cutoff. In any event, we show in Appendix E.2 that our main findings remain robust when including workers dismissed on all dates and adding fixed effects for individual-specific cutoff and dismissal dates to control for dismissal cycles. In this specification, the estimates rely upon variation in worker-specific dismissal dates within groups who have the same cutoff date.

6.3 Results and robustness

The top panels in Figure 8 shows that workers barely meeting the 16-month requirement have higher access to UI transfers. The additional transfers ares paid out during the first semester after layoff and are worth about R$2,000 (equivalent to 2.5 UI monthly payments, or 1.5 pre-displacement monthly wages). In the second semester after layoff, the gap in UI transfers around the cutoff is virtually eliminated. These effects are quantified in columns 1-3 of Table 3, Panel A, which also shows a 57 p.p. impact on UI take-up rates (column 4).

Figure 8, center row, graphically shows our main results on DV. Access to unemployment does not affect DV in the first semester after layoff, and it *increases* DV risk in the second semester following layoff, after benefit payments cease. This is confirmed in Table 3, Panel B, which shows a null effect in the first semester and a statistically significant positive effect in the following semester. In a three-year period, UI eligibility increases the probability of facing a DV lawsuit by almost a third. The adverse impact on DV in the second semester is robust to alternative bandwidths and polynomials in the running variable (Appendix Table E1), to permutation tests where we compare our estimates to those at placebo cutoffs (Appendix Figure E4) and to adjusting for cyclicality in hiring and firing (Appendix Table E2). The impact on the overall DV probability up to 3 years after displacement is less robust. We conclude that UI benefits fail to reduce DV and that they may, in fact, increase it after benefit expiration.

Figure 8: The effect of UI eligibility, male workers



Notes: The graphs plots UI outcomes (top), the probability of DV perpetration in DV suits (center) and employment outcomes (bottom) around the cutoff date for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 10-day bins. The lines are based on a local linear polynomial smoothing with a 45-day bandwidth with 95% confidence intervals. UI amounts in Brazilian reais.

Finally, the bottom row of Figure 8 and Panel D of Table 3 show that eligible men work 8.6 weeks less in the 3 years after layoff, which is equivalent to a 11.5% reduction over the mean. These findings are in line with a large literature showing negative effects of UI on labor supply (see, among others, Katz and Meyer, 1990; Card, Chetty and Weber, 2007; Lalive, 2008; Gerard and Gonzaga, 2021).

In Appendix Table E3, we compare the characteristics of workers in the RD sample with workers in the job loss analysis in Section 5. Although workers in the RD sample have by construction lower tenure, absolute and standardized differences indicate reasonably small differences across several other characteristics. Nevertheless, to gather evidence on how differences in underlying characteristics may affect the comparability of the two results, we re-estimate the RD analysis on DV after reweighting the sample so that it perfectly matches the characteristics of the job loss sample.[53] Appendix

[53]We use the entropy algorithm by Hainmueller (2012); Hainmueller and Xu (2013) to generate weights.

Table E4 shows that the results remain extremely similar to our baseline results in Table 3, Panel B. Next, we discuss the underlying mechanisms driving DV in light of our theoretical model and the results obtained in the job loss and UI analyses.

Table 3: Effect of UI eligibility, male workers

	(1)	(2)	(3)	(4)
PANEL A: UI PAYMENTS				
	Semester 1	Semester 2	Payments	Take up
Eligibility for UI benefits	1950.5***	121.0***	2.55***	0.57***
	(18)	(4)	(0.02)	(0.005)
Mean outcome at the cutoff	83.7	3.8	0.1	0.0
Observations	98,167	98,167	98,167	98,167
PANEL B: DV - AFTER LAYOFF				
	Semester 1	Semester 2	Semester 3	Up to Year 3
Eligibility for UI benefits	0.0002	0.0008**	0.0002	0.0015*
	(0.0004)	(0.0003)	(0.0004)	(0.0009)
Mean outcome at the cutoff	0.0008	0.0006	0.0009	0.0047
Effect relative to the mean	23.7%	124.4%	21.5%	31.6%
Observations	98,167	98,167	98,167	98,167
PANEL C: DV - BEFORE LAYOFF - PLACEBO				
	Semester 1	Semester 2	Semester 3	Up to Year 3
Eligibility for UI benefits	0.0001	0.000	-0.0002	-0.0006
	(0.0003)	(0.0003)	(0.0003)	(0.0006)
Mean outcome at the cutoff	0.0	0.0	0.0	0.0
Effect relative to the mean	16.1%	0.0%	-39.2%	-23.3%
Observations	98,167	98,167	98,167	98,167
PANEL D: EMPLOYMENT	Weeks worked			
	Semester 1	Semester 2	Semester 3	Up to Year 3
Eligibility for UI benefits	-2.97***	-2.16***	-1.03***	-8.63***
	(0.1)	(0.1)	(0.2)	(0.7)
Mean outcome at the cutoff	8.3	13.4	13.5	75.2
Effect relative to the mean	-35.8%	-16.1%	-7.6%	-11.5%
Observations	98,167	98,167	98,167	98,167

Notes: This table shows the effect of unemployment insurance (UI) eligibility on UI outcomes (Panel A), the probability of DV perpetration after and before layoff (Panel B and C) and employment outcomes (Panel D), as estimated from equation (3) using a Regression Discontinuity Design. Semesters are set relative to the layoff date. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 45 days around the cutoff required for eligibility for unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. The table also reports the baseline mean outcome at the cutoff and the percentage effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

6.4 Discussion on mechanisms

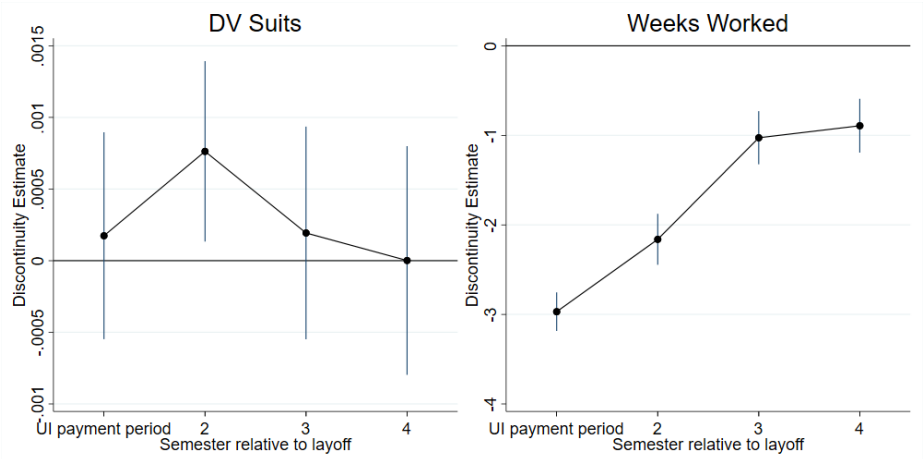
The findings that UI eligibility has (i) a null impact on DV during the benefit period, and (ii) a positive impact on DV risk after benefits expire are in line with both the income and exposure mechanisms – see Proposition 2, Section 2. The former result can be explained by the income and exposure mechanisms offsetting each other while benefits are paid out.[54] After UI transfers cease, DV risk increases because of the persistent impacts of UI on labor supply, which in turn increases the potential time spent together by partners. This is consistent with the dynamics of the effects of UI eligibility on DV suits and employment, displayed in Figure 9, left and right panel, respectively. In semester 1 following layoff, the exposure effect is offset by the income effect of UI transfers paid out in the same semester. During this period, eligible individuals work 2.97 weeks less, equivalent to a 35.8% decrease relative to the baseline (Table 3, Panel C, column 1).[55] The positive impacts on DV emerge in semester 2, when UI transfers cease, but higher exposure to DV is still present because the negative impacts on employment are still sizable. From semester 3, UI impacts on DV are again null because the employment gap closes up considerably, so the exposure effect becomes weaker.

The fact that UI income effects are short-lived is consistent with evidence showing that UI beneficiaries do little consumption smoothing. They experience sharp drops in consumption upon benefit expiration – see Gerard and Naritomi (2021) and Ganong and Noel (2019) for evidence using Brazilian and US data, respectively. That income is a mechanism for DV is line with the evidence in Section 5.6, showing no job loss effect on DV for high tenure workers, who have great access to liquidity upon displacement.

[54] Unemployment benefits were not conditional on attendance of training programs or minimum job search requirements in our analysis period. In 2012-14, there were attempts to condition benefits on attendance of training programs (PRONATEC). However, data from the Ministry of Labor show that only 1.2% of UI beneficiaries participated.

[55] We also check that reemployment wages are not affected by UI eligibility, in line with the findings in Gerard and Gonzaga (2021) and Britto (2022)

Figure 9: The effect of UI eligibility on DV and Employment, male workers, RD discontinuity estimates by period after the layoff



Notes: The graphs plots RD discontinuity estimates around the cutoff date for eligibility for unemployment benefits on the probability of DV perpetration in DV suits and employment in semesters after layoff. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. The RD estimates are based on a local linear polynomial with a 45-day bandwidth and vertical bars show 95% confidence intervals.

Overall, the income and exposure mechanisms are able to explain our evidence on the effects of job loss and unemployment benefits, which instead cannot be immediately reconciled with other theoretical constructs in the DV literature.[56] First, the similar effects in direction and magnitude of male and female job loss, which we extensively documented in Section 5, are hard to reconcile with several previous models – notably the household bargaining, male backlash, instrumental control, and sabotage models – which predict opposite responses of DV after male and female job loss. Second, it is also difficult to reconcile the impacts of UI transfers on DV based on the same models. For instance, the household bargaining model would predict higher bargaining power by men and higher DV during the benefit period. In turn, male backlash and instrumental control model would likely suggest lower DV during the benefit period, differently from what we find in the data.

[56]In addition, the income and exposures mechanisms in isolation cannot explain the key patterns emerging from our analysis. Specifically, exposure does not explain why job loss effects on DV are decreasing in tenure and access to liquidity (see Section 5.6); and why the adverse effects of unemployment benefits on DV emerge only after benefit payments cease. In turn, the income mechanism is unable to explain the adverse impacts of UI transfers on DV.

7 Conclusions

Domestic violence imposes substantial costs on women, society and the next generation. It creates physical and mental health problems, reduces productivity among women, and has further adverse consequences for their children (Aizer, 2010, 2011; Currie, Mueller-Smith and Rossin-Slater, 2020; Carrell and Hoekstra, 2010). Recent global estimates reveal that DV occurs on a very large scale, and that it does not dissipate with economic development. It is therefore important to understand its causes, and we contribute in this paper to illuminating how DV evolves with a key economic shock experienced every year by millions of workers worldwide: the loss of a job.

Our main finding is that male and female job loss lead to an escalation of domestic violence. These results are consistent with DV increasing under income scarcity and when families spend more time together during the stressful period of unemployment. This paper complements and extends a large literature studying the effects of local economic shocks on domestic violence. These studies analyze relative variation in labor market conditions for men and women as influencing DV by affecting their *potential* income and the balance of power within the household. In contrast, our findings reveal the dramatic effects caused by actual job loss. Although only a relatively small share of the total population suffers job loss in economic downturns, this represents millions of individuals. For instance, the International Labour Organization estimates that 212 million workers worldwide were displaced during the 2008 financial crisis (ILO, 2010). Our results emphasize the need for interventions supporting potential victims in households where either of the partners has lost a job.

A new and important insight of this paper is that the provision of unemployment benefits, a natural policy response, can misfire if it generates behavioural responses that lead men to remain unemployed for longer. This suggests that unemployment benefits might have a better chance of mitigating impacts of job loss on DV if accompanied by policies that attenuate the exposure mechanism. These include job placement or skills training that facilitate the return to work, differently from our setting where UI benefits were unconditional. Finally, our findings on mechanisms line up well with the remarkable global surge in domestic violence during the Covid-19 pandemic, as the latter is plausibly the result of income losses brought by widespread job loss and lockdown policies which reinforce the exposure effects of job loss.

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Online Appendix

A Appendix to Section 2

A.1 Employment status and time partners spend together

We gather evidence on whether partners spend more time together following job loss. Ideally, we would use time use information. Since such data is not available for Brazil, we rely on two household surveys. First, we use the cross-section version of PNAD for the period 2012-2014, the large nationally representative household survey carried out by IBGE. The survey indicates who is the survey respondent for each individual interview in the household. When the person is present at home, interviewers are instructed to collect the information directly from that person.[57] We focus on individuals in the age range 20-50, in line with our main analysis, and study whether non-employment status correlates with being at home and responding to the survey in person, which we use as a proxy for time spent together by partners. Columns 1-2 in Table A1 show that men and women are 13 and 14 p.p. more likely to be at home and answer the survey when they are not employed. Second, we use the Pesquisa de Orçamentos Familiares (POF) for 2017, a nationally representative household survey on expenditures conducted by IBGE. In addition to information on expenditures, the survey asks detailed questions on the timing of food consumption. We study whether partners who are not employed in the reference period (12 months) are more likely to consume food at home and to have the main daily meals (lunch and dinner) at home at the same time. More specifically, we use a dummy for whether the meal was consumed or prepared at home as proxy for whether a person is at home. Ideally, we would use information on whether the meal was consumed at home rather than prepared but such information is not available. We restrict attention to couples in ages 20 to 50 years old and analyze two main outcomes: (i) for each consumption hour, we track whether meal was consumed (or prepared) at home; (ii), and we track the probability that both partners consumed (or prepared) the meal at home in the same hour of the day. Columns 3-6 in Table A1 shows that male and female partners who are not employed are 6.1-7.0 p.p. more likely to consume food at home, and 3.7-6.4 p.p. more likely to consume the main daily meals together. When running

[57] The same information is not available for the longitudinal version of PNAD, for which reason we cannot replicate the analysis in Section D.2.

all regressions, we control for age (quadratic polynomial), and several fixed-effects: fine sampling regions, dividing Brazil in 300 thousands subareas, education, and race. Overall, these results support the idea that partners spend more time at home and more time together upon unemployment, and are in line with the proposed exposure mechanism.

Table A1: Employment status and time spent together by intimate partners

	(1)	(2)	(3)	(4)	(5)	(6)
Source	PNAD		POF			
Gender	Male	Female	Male		Female	
Dependent variable:	Reply survey at home		Meal at home	Main meal with partner	Meal at home	Main meal with partner
Not employed	0.13***	0.14***	0.061***	0.064*	0.070***	0.037**
	(0.006)	(0.003)	(0.02)	(0.03)	(0.007)	(0.01)
Mean outcome	0.44	0.73	0.84	0.40	0.89	0.37
Effect relative to the mean	29%	19%	7%	16%	8%	10%
Observations	110,750	112,381	4,033	2,279	5,662	3,059

Notes: This table shows the coefficient of an OLS regression on employment status using Pesquisa Nacional por Amostra de Domicílios (PNAD) for 2012-2014, columns 1-2, and Pesquisa de Orçamentos Familiares (POF) for 2017, columns 1-2, by men and women in living with an intimate partner in the household. Reply survey at home indicates that the individual directly answer to PNAD survey in person (columns 1-2). Meal at home indicates the share of meals consumed or made at home (columns 3 and 5), and main meal with partner indicates the share of meals in lunch or dinner time consumed with the partner (columns 4 and 6). The regression controls for quadratic age and fixed-effects on sampling geographical unit, education and race. (*** p≤0.01, ** p≤0.05, * p≤0.1).

A.2 Proof of model's propositions

Proposition 1. If either or both the income ($\frac{\delta\phi_t(.)}{\delta c_{it}} < 0$) and exposure mechanisms ($\frac{\delta\phi_t(.)}{\delta h_{it}} < 0$) are present, expected DV risk ϕ is higher in both periods $t = 1, 2$ when either partner has lost her/his job in period 1, taking as given the other partner's employment status.

Proof: Consider first the situation where the male partner is the potential job loser. Let $\phi_t|E_{m1}$ and $\phi_t|U_{m1}$ be the DV risk in period t given that the male partner is respectively employed and unemployed in period t. In any period, DV risk is higher when he is unemployed than when he is employed:

$$\phi_t|U_{mt} = \phi_t(c_{mt}|U_{mt}, c_{ft}|U_{mt}, 0, \bar{h}_f)$$

$$> \phi_t(c_{mt}|E_{mt}, c_{ft}|E_{mt}, h_m^E, \bar{h}_f) = \phi_t|E_{mt} \quad (1)$$

This directly follows from the fact that DV risk is strictly decreasing in the consumption of each partner and in the working hours of the male partner. If the income mechanism is present, DV risk is higher when the male partner is unemployed because his consumption is strictly lower ($c_{mt}|U_{mt} < c_{mt}|E_{mt}$), and the consumption of the female partner is weakly lower ($c_{mt}|U_{mt} \leq c_{mt}|E_{mt}$). If the exposure mechanism is present, DV risk is higher when the male partner is unemployed because he works less hours ($h_f^E > 0$).

Now let $\phi_t|J_{m1}$ be DV risk given that the male partner has lost his job in period 1. We aim to show that $E[\phi_t|J_{mt}] > \phi_t|E_{mt}]$. Expected DV risk under job loss is a weighted average of DV risk when the partner finds a new job and does not find a new job. It is strictly higher relative to the situation where the male partner does not lose his job in period 1:

$$E[\phi_t|J_{mt}] = s_1.\phi_t|U_{mt} + (1 - s_1)\phi_t|E_{mt} > \phi_t|E_{mt}$$

The inequality directly follows from equation (1). An analogously argument follows when we vary the employment status of the female partner, and take the employment status of the male partner as given.

Proposition 2. Consider a partner who is a job loser in period 1. If only the income mechanism is present ($\frac{\delta\phi_t(.)}{\delta c_{it}} < 0$), UI transfers reduce DV risk during the benefit period, and have no effect in subsequent periods. In turn, if only the exposure mechanism is present ($\frac{\delta\phi_t(.)}{\delta h_{it}} < 0$), UI transfers increase DV risk in both periods on account of lower labor supply. If both mechanisms are present, UI transfers have an ambiguous impact on DV risk during the period 1 – the benefit period – and they increase DV risk in period 2, after benefits expire.

Proof. Expected DV risk in period 1 depends both on the probability that the partner finds a new job (s_1) and on DV risk in case she/he does not find a new job:

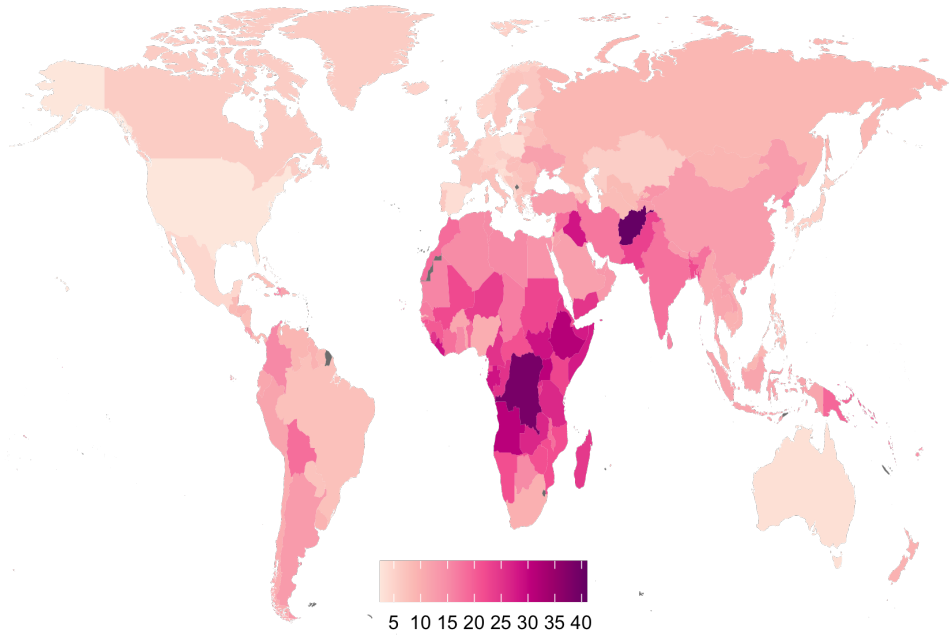
$$E[\phi_t|J_{mt}] = s_1.\phi_t|U_{mt} + (1 - s_1)\phi_t|E_{mt}$$

On the one hand, higher benefit level in period 1 reduce expected DV risk because it lowers DV risk when job search is unsuccessful ($\phi_t|U_{mt}$). This is because

higher benefits increases the partner's own consumption and has a non-negative impact on the other partner's consumption – the income mechanism. On the other hand, higher benefit level reduces the probability that the partner finds a new job, increasing the likelihood that she/he stays in unemployment where DV risk is higher ($\phi_t|U_{mt} > \phi_t|E_{mt}$) – the exposure mechanism. Therefore, depending on which mechanism dominates, expected DV risk could be higher, lower or the same during the benefit period 1. Instead, once UI benefits expire in period 2, the income mechanism is no longer present. In such case, expected DV risk is necessarily higher because of the exposure mechanism. If only the income mechanism is present, UI transfers reduce DV risk during the benefit period because of higher consumption. Instead, if only the exposure mechanism is present, UI transfers increase DV risk in both periods because of higher exposure on account of lower labor supply.

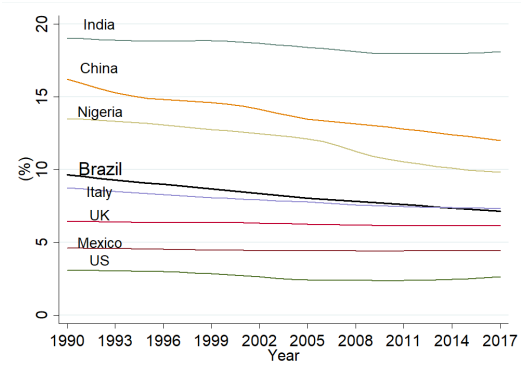
B Appendix to Section 3

Figure B1: Share of Women Experiencing Intimate Partner Violence, Last 12 Months, Across Countries, 2013



Notes The figure shows the share of women experiencing intimate partner violence in the last 12 months by country. We use data for 2013, the middle of our analysis period, from the Institute of Health Metrics & Evaluation (IHME), which provides comparable statistics across countries

Figure B2: Share of Women Experiencing Intimate Partner Violence, Last 12 Months, Across Countries, 1990-2017



Notes The figure shows the share of women experiencing intimate partner violence in the last 12 months across countries for the period 1990-2017. Source: Institute of Health Metrics & Evaluation (IHME)

C Appendix to Section 4

Table C1: Descriptive statistics by name uniqueness

	Male			Female		
	Unique	Others	Std Diff	Unique	Others	Std Diff
Demographic characteristics						
Years of education	10.7	10.2	- 0.21	11.7	11.3	-0.19
Age	30.8	32.0	0.15	30.7	31.0	0.03
Race - white	51.7%	45.7%	- 0.12	60.0%	53.4%	-0.13
Race - black	4.9%	6.6%	0.07	3.6%	5.2%	0.08
Race - mixed	34.7%	39.2%	0.09	28.4%	33.5%	0.11
Job characteristics						
Monthly income (R$)	1,697	1,538	- 0.07	1,362	1,182	-0.11
Month of worked $t-1$	5.1	5.1	- 0.01	5.3	5.3	0.00
Tenure on Jan 1st (years)	1.7	1.7	- 0.00	1.9	1.8	-0.02
Manager	5.9%	3.5%	- 0.11	9.7%	6.4%	-0.12
Firm size (employees)	501	509	0.01	447	472	0.02
Local area - municipality						
Large municipality - pop> 1M	34%	35%	0.02	35%	37%	0.04
Municipality population	1,898,158	2,067,751	0.05	2,116,420	2,350,872	0.06
Homicide rate (per 100k inhab.)	29.7	30.4	0.03	27.4	28.2	0.04
Observations	6,283,650	6,615,024		4,426,710	2,889,899	

Notes: The table reports the average characteristics of displaced workers during the period 2012-2014, with and without a unique name in the country, and the standardized difference between the two groups, by gender.

D Appendix to Section 5

D.1 The effect of job loss on employment outcomes

Figure D1: The effect of job loss on employment outcomes

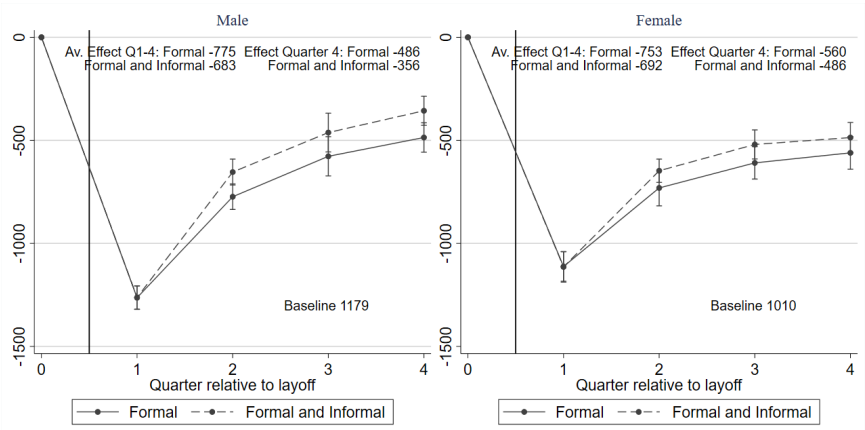
Notes: This figure shows the effect of job loss on formal employment outcomes by gender, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. Except for job turnover, all coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Employment is measured at the end of each period, while job turnover indicates the activation or termination of a job spell. Income variables are measured in Brazilian Reais.

D.2 The effect of job loss on employment outcomes: informal work

If displaced workers return to jobs in the informal sector then our main estimates using data on formal jobs will overstate the drop in employment and earnings following layoff. To investigate this, we replicate our main analysis using the National Household Sample Survey (PNAD), which contains information on both formal and informal sector employment and income. PNAD is the largest Brazilian household data and is a reliable source of data on informal employment, being conducted by the Brazilian Institute of Geography and Statistics (IBGE) which has considerable experience on generating statistics on the informal sector. The microdata do not contain a person ID but we can track individuals over time through five consecutive quarters based on their household ID and characteristics, including gender, precise birth date and their order in the family. We focus on workers who were initially interviewed during 2012-2014, and compare those who were formally employed in the first but not in the second quarter (treated) with a control group who were employed in both the first and second quarter (but possibly displaced in later quarters).

Figure D2 presents the results. Accounting for informal sector income reduces our estimate of earnings losses in the first year after job loss by about 12% for male and 8% for female workers.

Figure D2: The effect of job loss on formal and informal labor income



Notes: This figure shows the effect of job loss on formal and informal monthly labor earnings (along with 95% confidence intervals) by gender as estimated from the difference-in-differences equation (1), based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. The sample covers individuals first interviewed in the period 2012-14. The treatment group is defined by workers who are formally employed in the first interview and out of employment in the second interview; the control group is composed by workers who are formally employed on the first and second interviews. Earnings are measured in Brazilian Reais. Baseline average values for the treated group at $t = 0$ are also reported.

D.3 The effect of job loss on domestic violence: comparable sample

Table D1: Effect of job loss on domestic violence, comparable sample, same jurisdictions

	(1)	(2)	(3)
	Labor market effects		Probability of DV
Dependent variable:	Employment	Income	Protective Measure
PANEL A: MALES DISPLACED IN MASS LAYOFFS, DV PERPETRATION			
Effect of job loss	-0.23***	-5423.3***	0.00044***
	(0.004)	(88.3)	(0.0001)
Mean outcome, treated at t=0	1	12,996	0.0006
Effect relative to the mean	-23%	-42%	74%
Elasticity to earnings			-1.77
Observations	3,431,680	3,431,680	3,431,680
PANEL B: FEMALES DISPLACED IN MASS LAYOFFS, DV VICTIMIZATION			
Effect of job loss	-0.23***	-4445.3***	0.00040***
	(0.004)	(69)	(0.0001)
Mean outcome, treated at t=0	1	11,204	0.0007
Effect relative to the mean	-23%	-40%	56%
Elasticity to earnings			-1.40
Observations	1,266,034	1,266,034	1,266,034

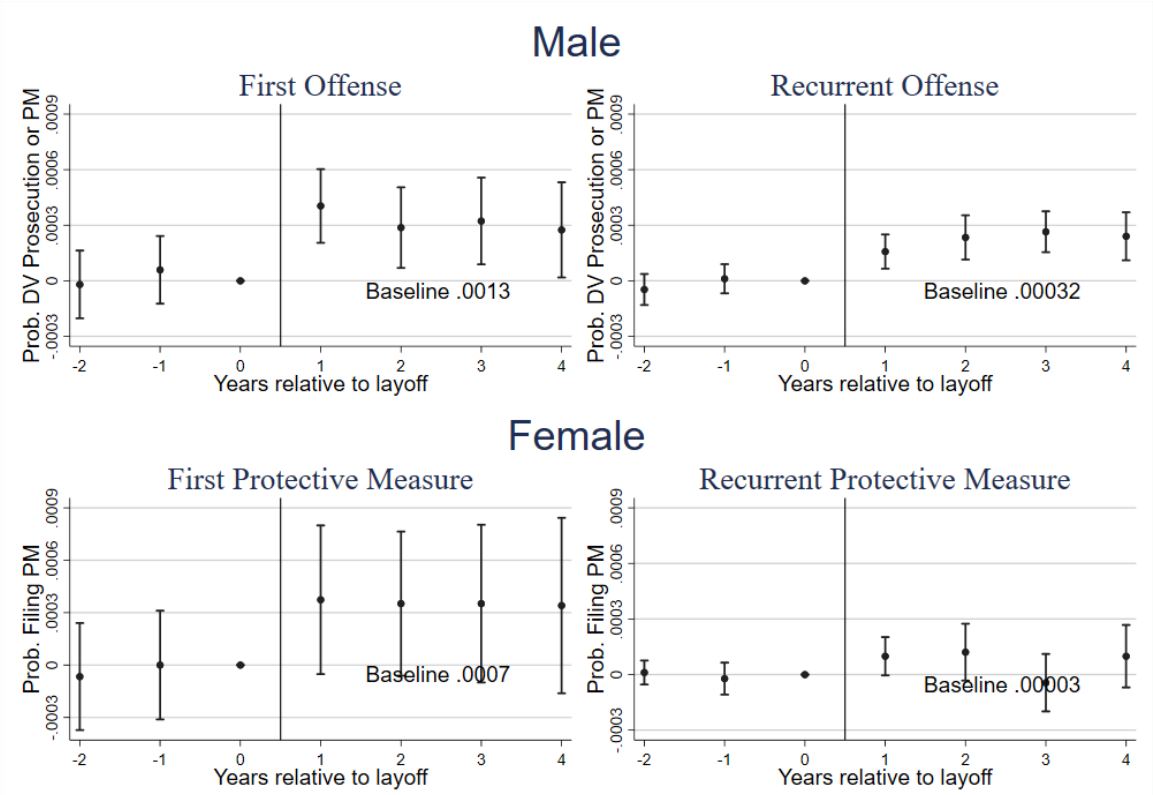
Notes: This table shows the effect of job loss on labor market outcomes (columns 1-2) and DV perpetration/victimization outcomes (columns 3) using a comparable sample covering the same jurisdictions for male and female displacement, as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.4 The effect of job loss on domestic violence: persistence

To investigate the observed persistence, we re-estimate the equation distinguishing the first registered DV case for an individual from repeated cases, see Figure D3. The results show that about half of the (absolute) male job loss effect is driven by first offenses, while the other half is related to repeated offenses. The sustained effect on first offenses is consistent with the sustained labor market losses following displacement. Similarly, female job loss increases DV victimization in non-repeated

cases several years after job loss. Unlike in the case of male layoff, impacts on repeated victimization are muted, but this pattern may derive in part from the fact that missing names in the court data are more frequent for victims, so that we may fail to identify repeated reporting.

Figure D3: The effect of male and female job loss on domestic violence - persistence

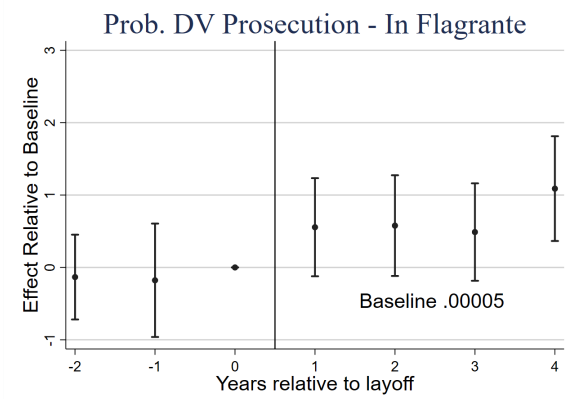


Notes: This figure shows the effect of job loss on the probability of DV prosecution for men and on the probability of filing a DV Protective measure for women, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

D.5 Reporting bias: Alternative measures of DV

D.5.A Arrests "in flagrante" and DV shelters

Figure D4: The effect of job loss on domestic violence - in flagrante arrests



Notes: This figure shows the effect of male job loss on probability of being prosecuted for DV following from an in flagrante arrest, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Table D2: Effect of male and female job loss on domestic violence, use of DV shelters

	(1)	(2)
Job Loser	Male	Female
Dep. var.:	DV Shelter Use By Female Partner	DV Shelter Use
Effect of Job Loss	0.00064**	0.00020***
	(0.0003)	(0.00006)
Mean outcome, treated at t=0	0.0027	0.0004
Relative variation	24%	46%
Observations	460,152	1,476,852

Notes: This table shows the effect of job loss on the probability that women access DV public shelters, as estimated from the difference-in-differences equation (2). In column 1, the sample is restricted to workers present in the social registry for whom the female partner, who may use the DV shelter, is identified. No such restriction is necessary in column 2 as shelter use data directly identify the woman. The dependent variable is measured at the end of each calendar year and the sample is restricted to 2011-13, the period for which the outcome is available. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, not displaced in the same calendar year. Regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are shown in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.5.B DV notifications by the health system (SINAN data)

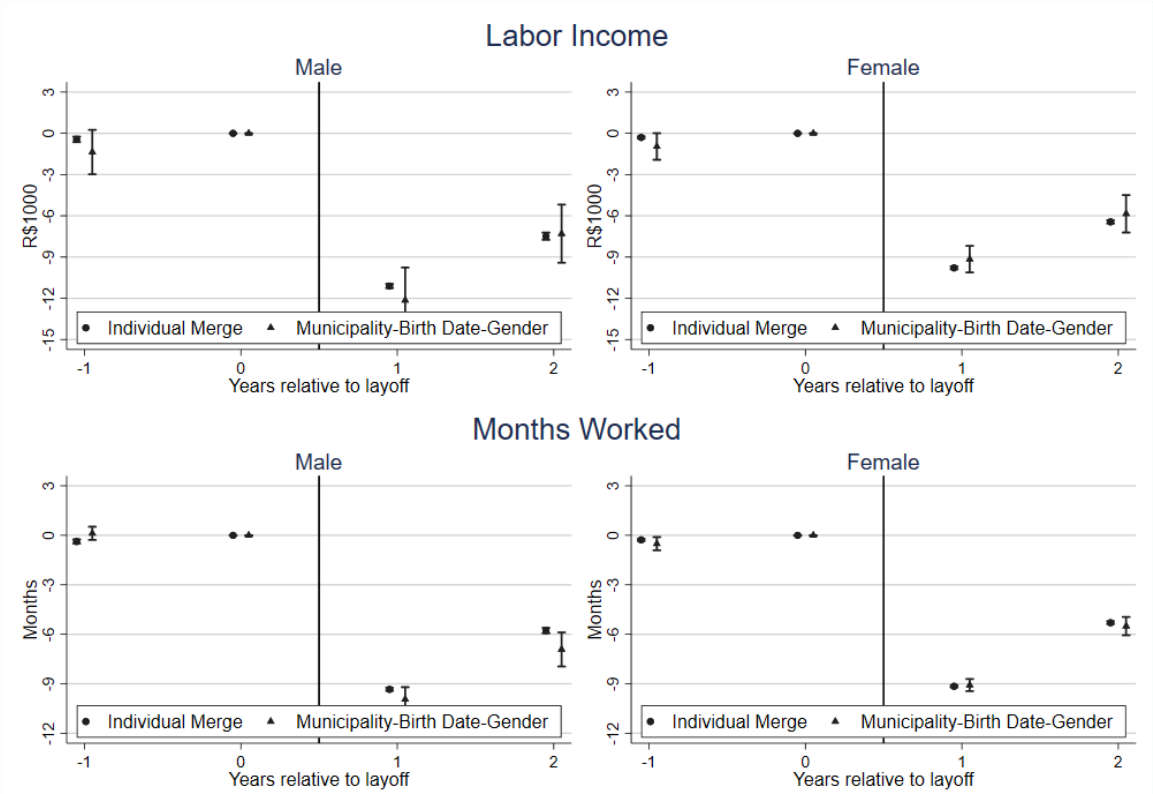
The key challenge in using the SINAN data is that unique individual identifiers are not available. To address this challenge, we link job losers (and their matched control workers) to health system DV notifications based on clusters defined by the individual's exact birth date, gender and municipality. To minimize measurement error, we restrict the sample to individuals in clusters with no more than 80 people, which is equivalent to dropping the upper quintile of the cluster size distribution. Cluster size is measured by the number of individuals with the same birth date, municipality and gender observed in the labor and social registry data.

Since this procedure necessarily entails some degree of measurement error, we extend the mass layoff sample to workers displaced in the period 2012-16, thus tracking SINAN reports for only two years before and after job loss, in order to increase statistical power. If we assume that measurement error is classical and exogenous to the post-treatment variable of interest, this will cause our estimates to be more imprecise but not biased (this situation is different from classical measurement error in a regressor, which leads to attenuation bias). Importantly, we validate this assumption and our data linkage procedure by replicating the estimation of employment effects of job loss. In other words, we use the same procedure to match employment outcomes, as if we did not have unique individual identifiers. Figure D5 shows estimates of the impact of layoff on employment outcomes estimated with the cluster-level match, compared with our baseline estimates obtained with identified matches. Although the standard errors are larger using the cluster-level match, the estimates are statistically significant and similar in magnitude to the baseline estimates. This evidence supports our merging procedure.

SINAN data are effectively available from 2010 to 2017. We drop observations for 2009, the implementation year of these reports, because coverage around the country was limited. The analysis of male job loss is restricted to the subsample of individuals observed in the social registry in 2011, for whom we can identify their cohabiting female partner and track them in SINAN – which covers information on the victims but does not identify the perpetrator. We drop cases for which the aggressor is identified as a relative (e.g., the victim's father or mother), while we keep cases for which the relationship with the aggressor is not reported (10% of all cases) so that our measure is not endogenous to the victim's willingness to identify the perpetrator. The results are presented in Figure 5 in the main text.

Appendix Table D3 shows that these results are robust to using different maximum cluster sizes (columns 1-3); interacted municipality, time and birth period fixed-effects (columns 4-5); and more stringent mass layoffs definitions (columns 6-7). The relative effect on DV is larger than the baseline estimate, though computing baseline rates is complicated because the matching procedure does not uniquely identify individuals. Taking the mean over the outcome in the estimation sample would inflate baseline rates as it would include DV notifications for several individuals. For this reason, we compute the mean for the 13% of workers who are uniquely identified by the characteristics we use to merge the SINAN data.

Figure D5: Robustness of merging procedure based on municipality-gender-birth date



Notes: This figure shows the effect of male and female job loss on the incidence on employment outcomes, respectively, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals, which are too small to be visible in some specifications. It compares the results when we merge outcomes at the individual level and when do so based on clusters at the municipality-gender-birth date level. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

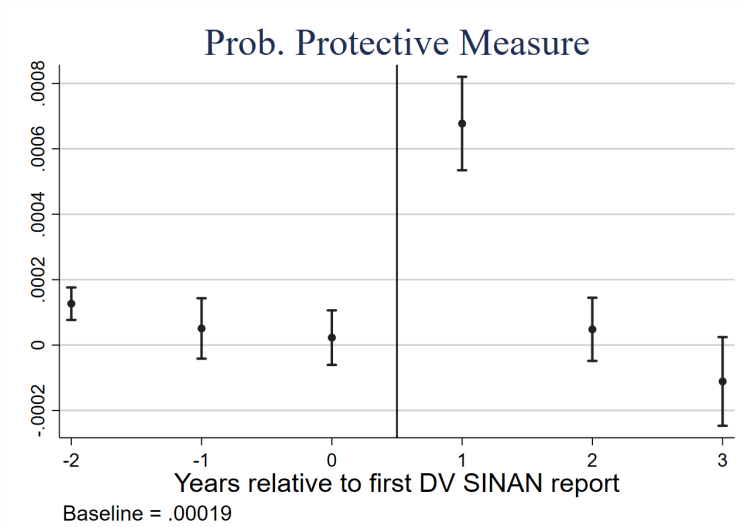
Table D3: The effect of job loss on DV notifications in the SINAN data: robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome				SINAN DV Report			
PANEL A: MALE JOB LOSS, PROB. FEMALE PARTNER IN SINAN DV REPORT							
Effect of job loss	0.0018**	0.0020**	0.0018**	0.0013	0.0016*	0.0026**	0.0019
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0009)	(0.001)	(0.001)
Mean outcome, treated at t=0, cluster size=1	0.0018	0.0018	0.0018	0.0018	0.0018	0.0018	0.0018
Relative variation	102%	113%	102%	74%	91%	147%	107%
Observations	1,560,324	1,475,404	1,246,048	1,475,156	1,398,264	758,164	269,984
PANEL B: FEMALE JOB LOSS, PROB. FEMALE WORKER IN SINAN DV REPORT							
Effect of job loss	0.00093**	0.00095**	0.00076**	0.00059*	0.00064*	0.00082	0.0013*
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0005)	(0.0008)
Mean outcome, treated at t=0, cluster size=1	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026	0.0026
Relative variation	36%	37%	30%	23%	25%	32%	51%
Observations	5,330,060	5,028,324	4,208,812	5,028,164	4,947,156	2,050,360	848,420
Max cluster size	120	80	50	80	80	80	80
Mass layoff definition	33%	33%	33%	33%	33%	50%	75%
Mun X Time FE				Y			
Mun X Birth Quadrimester X Time FE					Y		

Notes: This table shows the effect of female job loss on the incidence of DV in SINAN reports for displaced men's partners and women, respectively, as estimated from the difference-in-differences equation (2), for varying specifications. In Panel A, the sample is restricted to displaced workers present in the social registry, for whom it is possible to identify the female partner, while no such restriction is necessary in Panel B. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for workers displaced upon mass layoffs, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome in the pre-displacement year for the treatment group – for individuals uniquely identified by the municipality-birth date – and the percent effect relative to the baseline mean. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.5.C Interactions between alternative DV measures

Figure D6: DV protective measures in criminal courts around DV events in SINAN reports



Notes: This figure shows how the probability that women file DV protective measures in courts vary around the first time women show up in DV sinan reports – along with 95% confidence intervals. The analysis is based on women who show for the first time in SINAN reports during 2012, tracking protective measures probabilities from three years before to three years after (2009-2015 period). To remove year specific trends, we report the differences relative to women who will show up in DV SINAN reports for the first time 3 years later (2016).

Table D4: Correlation between DV protective measures and alternative DV measures

	(1)	(2)	(3)
Dependent variable:	Prob. of filling DV protective measure		
SINAN DV report	0.00083***		0.00083***
	(0.0002)		(0.0002)
DV shelter use		-0.00045***	-0.00045***
		(0.00002)	(0.00002)
Observations	506072	506072	506072

Notes: This table shows the correlation between alternative DV measures for women in our main sample, using a linear regression. It shows how the probability that women file DV protect measures in courts (dependent variable) is related to the probability that women show up in DV SINAN reports and that they use DV shelters. The sample is restricted to women who are more easily identified in SINAN by their date of birth and municipality, so that we can identify SINAN outcomes – see Section D.5.B for the details on the data linkage procedure procedure. Standard errors clustered at the individual level and are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.6 The effect of job loss on DV: Robustness

D.6.A Baseline differences between the treatment and control groups

Table 1 shows that our exact matching approach creates a control group which is strongly similar to the treatment group in terms of several baseline characteristics, many of which are not included in the matching process. However, years of education is .9 years lower in the control group for the male job loss analysis. We now investigate whether such imbalance can affect our main results. First, we re-weight the control group to perfectly match the treatment groups over all characteristics displayed in Table 1. We use the entropy algorithm developed by Hainmueller (2012); Hainmueller and Xu (2013) setting the weights to perfectly match the first two moments of each these characteristics, so that any remaining imbalances in these observables are eliminated. Next, we re-estimate our difference-in-differences model using such weights. The results in Table D5, Panel A, show 28% and 55% higher DV risk after male and female job loss. These estimates remain extremely similar to our baseline estimates in Table 2. Second, instead of using weights, we add education (4 categories) to our original exact matching process and re-estimate our model – Panel B, Table D5. Also in this case, the results remain similar to our baseline estimates – 23% and 71% higher DV risk after male and female job loss, respectively. Overall, these tests indicate that remaining imbalances between the treatment and control groups are unlikely to threaten our main findings.

Table D5: Effect of job loss on labor market outcomes and domestic violence, eliminating differences in observables between the treatment and control groups

	(1)	(2)	(3)	(4)	(5)	(6)
Job loser	Male			Female		
	Labor market effects		Probability of DV	Labor market effects		Prob. of Filing
Dependent variable:	Employment	Income	Any	Employment	Income	Protective Measure
PANEL A: REWEIGHTING CONTROL GROUP TO PERFECTLY MATCH OBSERVABLES OF TREATED GROUP						
Effect of job loss	-0.21***	-5943.8***	0.00041***	-0.21***	-4147.5***	0.00039**
	(0.002)	(92.5)	(0.00008)	(0.003)	(61.6)	(0.0002)
Mean outcome, treated at t=0	1	14,674	0.0015	1	11,193	0.0007
Effect relative to the mean	-21%	-41%	28%	-21%	-37%	55%
Elasticity to earnings			-0.68			-1.47
Observations	11,313,141	11,313,141	11,313,141	1,252,986	1,252,986	1,252,986
PANEL B: ADDITIONALLY MATCHING ON EDUCATION						
Effect of job loss	-0.21***	-5435.8***	0.00035***	-0.23***	-4174.9***	0.00052***
	(0.002)	(65.7)	(0.00011)	(0.0046)	(69.8)	(0.00016)
Mean outcome, treated at t=0	1	13,868	0.0015	1	10,807	0.0007
Effect relative to the mean	-21%	-39%	23%	-23%	-39%	71%
Elasticity to earnings			-0.60			-1.85
Observations	7,304,192	7,304,192	7,304,192	864,122	864,122	864,122

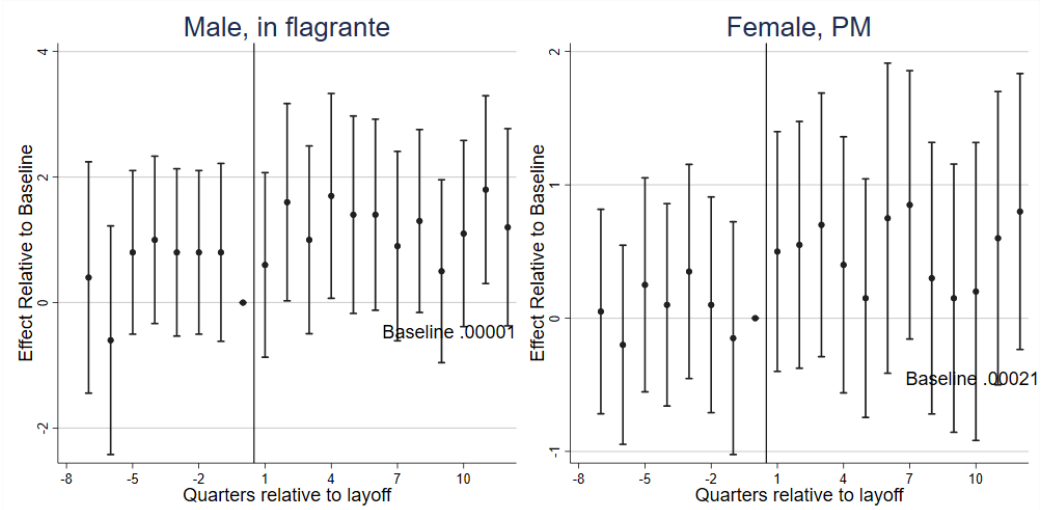
Notes: This table shows the effect of job loss on DV perpetration/victimization outcomes, as estimated from the difference-in-differences equation (2). Panel A uses regression weights set to eliminate any differences in observable characteristics between the treatment and control groups. We use the entropy algorithm developed by Hainmueller (2012); Hainmueller and Xu (2013) to perfectly match the first two moments of observable characteristics up to the second moment. Panel B is based on a sample for which the control group is built as in our baseline, but adding education to the set of matching variables. The dependent variable is indicated on top of each column. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.6.B Timing of violence: quarterly data

One concern regarding our main specification is that we measure violence timing based on when DV suits are filed rather than when the violence occurs. For instance, we could fail to detect diverging pre-trends in our analysis because of this lag. To address this concern in the male job loss analysis, we focus on "in flagrante" cases, which offer a timely measurement of DV since they are immediately filed in courts (see section 4). Our main results are confirmed using this measure, see Figure D4, Section D.5.A. The female job loss analysis already relies on data on protective measures which are

immediately filed in courts. Our analysis using DV notifications by the health system also overcomes these concerns since these data include the exact date when victims seek medical assistance (see Section 5.4). Finally, we replicate the analysis based on "in flagrante" cases for men and protective measures for women at the quarterly level which allows for a finer inspection of potentially diverging pre-trends. Although the results are less precise, they reveal similar patterns to our baseline estimates and show no evidence of diverging pre-trends – Figure D7.

Figure D7: The effect of job loss on domestic violence, judicial suits, quarterly data



Notes: This figure shows the effect of job loss on the probability of DV perpetration in DV "in flagrante" suits for men and DV victimization in protective measures for women, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Time refer to calendar quarters relative to the layoff quarter.

D.6.C Municipality-industry-year fixed effects

In the main analysis, matched controls are defined on state, 1-digit industry, and individual characteristics and this should difference out local shocks. Nevertheless, we show that our results are not sensitive to controlling for a more granular set of fixed effects, namely year × municipality×2-digit industry. We also restrict the definition of potential (matched) controls to include only workers remaining employed through the entire post-displacement period, rather than only in the year that their treated match is dismissed. In all cases, our results remain similar, see panels A-B of Table D6 showing higher DV in the ranges 28%-46% and 55%-80% after male and female job loss, respectively.

D.6.D Dynamic selection into layoffs

We demonstrate robustness to a series of tests for dynamic selection, that allay the concern that displaced workers are selectively pre-disposed towards DV at the time of displacement, even within mass layoffs. We vary the definition of mass layoffs from 33% up to 75%, while jointly varying the minimum size of firms in the sample from 30 to 70. The larger the fraction of workers dismissed, the more limited is the scope for selection into dismissal. Table D7 shows that the estimated effects of male and female job loss are broadly similar to the baseline estimates. Similarly, all results hold when restricting the treated group to workers in closing plants (Panel C, Table D6). The results are also robust to defining as treated all workers in mass layoff firms at the beginning of the calendar year when the mass layoff occurs, rather than just workers who are actually displaced. This approach avoids concerns regarding the selection of workers dismissed from downsizing firms. As it delivers an intention-to-treat estimate (analogous to estimates from randomized experiments with imperfect compliance), the relative effect is smaller (21% rather than 32%) but it is still statistically significant (Panel D, Table D6). In both Panels C and D the estimated elasticity of DV to earnings retains the same order of magnitude. We do not attach a causal interpretation to these elasticities as that would require that layoffs affect DV only through decreased earnings. In fact, layoffs can directly affect DV through other mechanisms such as exposure.

Table D6: Effect of job loss on domestic violence, robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Job loser	Male			Female		
	Labor market effects		Probability of DV	Labor market effects		Prob. of Filing
Dependent variable:	Employment	Income	Any	Employment	Income	Protective Measure
PANEL A: ADD MUN X IND X YEAR FE						
Effect of job loss	-0.21***	-5943.8***	0.00041***	-0.21***	-4147.5***	0.00039**
	(0.002)	(92.5)	(0.00008)	(0.003)	(61.6)	(0.0002)
Mean outcome, treated at t=0	1	14,674	0.0015	1	11,193	0.0007
Effect relative to the mean	-21%	-41%	28%	-21%	-37%	55%
Elasticity to earnings			-0.68			-1.47
Observations	11,313,141	11,313,141	11,313,141	1,252,986	1,252,986	1,252,986
PANEL B: ONLY CONTINUOSLY EMPLOYED IN THE CONTROL GROUP						
Effect of job loss	-0.42***	-10516.1***	0.00071***	-0.45***	-7606.6***	0.00057***
	(0.002)	(97.7)	(0.00009)	(0.005)	(104.5)	(0.0002)
Mean outcome, treated at t=0	1	16,108	0.0016	1	11,193	0.0007
Effect relative to the mean	-42%	-65%	46%	-45%	-68%	80%
Elasticity to earnings			-0.70			-1.17
Observations	4,911,578	4,911,578	4,911,578	572,922	572,922	572,922
PANEL C: ALL WORKERS IN CLOSING PLANTS						
Effect of job loss	-0.21***	-3535.5***	0.00025**	-0.29***	-3263.9***	0.00023*
	(0.004)	(120.1)	(0.0001)	(0.004)	(68.3)	(0.0001)
Mean outcome, treated at t=0	1	14,808	0.0010	1	9,338	0.0004
Effect relative to the mean	-21%	-24%	25%	-29%	-35%	66%
Elasticity to earnings			-1.05			-1.87
Observations	1,381,136	1,381,136	1,381,136	598,374	598,374	598,374
PANEL D: ALL WORKERS IN MASS LAYOFF FIRMS						
Effect of job loss	-0.23***	-4131.2***	0.00027***	-0.23***	-2893.7***	0.00016*
	(0.002)	(73.3)	(0.00006)	(0.004)	(69.4)	(0.00009)
Mean outcome, treated at t=0	1	13,068	0.0013	1	9,766	0.0005
Effect relative to the mean	-23%	-32%	21%	-23%	-30%	35%
Elasticity to earnings			-0.65			-1.19
Observations	18,869,718	18,869,718	18,869,718	1,623,685	1,623,685	1,623,685

Notes: This table shows the effect of male (columns 1-3) and female (columns 4-6) job loss on labor market outcomes and the probability of DV perpetration/victimization for different samples, as estimated from the difference-in-differences equation (2). The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. Panel A includes workers displaced in mass layoffs and adds interacted municipality-industry-year fixed effects, while Panel B restricts the control group only to workers who are continuously employed throughout the post-treatment period. In Panel C, the treatment group is restricted to closing plants. In Panel D, the treatment group is composed by displaced and non-displaced workers employed in mass layoff firms at the beginning of the calendar year of the event. The table also reports the baseline mean outcome for the treated group at the date of displacement; the percent effect relative to the baseline mean; and the implied elasticity of crime to earnings, computed as the ratio between the percent change in crime and the percent change in earnings. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table D7: Effect of job loss on domestic violence, robustness to mass layoffs definition

	(1)	(2)	(3)	(4)	(5)	(6)
Job loser	Male			Female		
Dependent variable:	Minimum layoff share			Minimum layoff share		
Prob. of DV	33%	50%	75%	33%	50%	75%
PANEL A: MINIMUM FIRM SIZE 30						
Effect of job loss	0.00048***	0.00048***	0.00045**	0.00040***	0.00044**	0.00050*
	(0.00008)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0003)
Relative Effect	32%	34%	29%	56%	120%	93%
Mean - Treatment Group	0.0015	0.0014	0.0015	0.0007	0.0004	0.0005
Observations	11,352,964	5,226,816	1,936,536	1,273,160	532,266	233,366
PANEL B: MINIMUM FIRM SIZE 50						
Effect of job loss	0.00050***	0.00046***	0.00044**	0.00040**	0.00047**	0.00064**
	(0.00009)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Relative Effect	33%	32%	27%	64%	177%	142%
Mean - Treatment Group	0.0015	0.0014	0.0016	0.0006	0.0003	0.0004
Observations	9,555,448	4,493,944	1,651,160	946,708	421,428	186,760
PANEL C: MINIMUM FIRM SIZE 70						
Effect of job loss	0.00053***	0.00051***	0.00051**	0.00037**	0.00041*	0.00057*
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Relative Effect	34%	35%	31%	64%	154%	135%
Mean - Treatment Group	0.0015	0.0015	0.0017	0.0006	0.0003	0.0004
Observations	8,502,466	4,053,056	1,492,386	793,282	366,870	166,390

Notes: This table shows the effect of male (columns 1-3) and female (columns 4-6) job loss on the probability of DV perpetration/victimization for varying mass layoff definitions, as estimated from the difference-in-differences equation (2). The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.6.E Mass layoff spillovers

We study to which extent our results might be driven by the fact that they are based on mass layoffs, which could generate significant spillover effects. Namely, the fact that many workers are displacement at the same time could play a role and make our estimates significantly different from the impacts of regular layoffs, where workers are displaced in isolation. We investigate this matter by studying how our estimates change when varying the absolute number of displaced workers in the mass layoffs – see Table D8. If spillovers were the primary driver of our main results, one should expect smaller effects when using layoffs where a smaller number of co-workers are displaced. The table shows that our main estimates retain the same direction and order of magnitude when restricting the sample to mass layoffs where fewer workers from the same firm are displaced together. It also reports results obtained based on a different sample covering all layoffs in our data, but excluding mass layoffs (we repeat the same exact matching procedure to find control workers in this larger sample). Although results should be interpret with some caution because job loss is more likely to be endogenous in this sample, they still point at positive effects which retain the same order of magnitude relative to our baseline estimates based on mass layoffs.

Other three pieces of evidence do not support the idea that mass layoff spillovers strongly drive our main findings. First, as shown in previous results, our estimates retain the same direction and order of magnitude when we focus on events where a higher share of the workforce is displaced and when studying larger firms where multiple workers are displaced at the same time (Table D7). In addition, the same holds when entirely focusing on plant closures (Table D6, Panel D). Second, Figure D11 shows that our estimates are not driven by small municipalities where mass layoffs represent a larger share of the workforce and where, in principle, they should generate stronger spillovers. Third, we show in Table D9 that workers displaced in mass layoffs are not strongly different from the overall population of displaced workers. Overall, these several pieces of evidence do not support the idea that our results are primarily driven by spillover effects steaming from the fact that our analysis focuses on mass layoffs.

Table D8: Effect of job loss on domestic violence, robustness to mass layoffs spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PANEL A: MALE JOB LOSS							
Dependent var.: Prob. of DV	Mass layoffs with less than X workers displaced						All layoffs
	Baseline	1000	500	100	50	30	excluding mass layoffs
Effect of job loss	0.00048***	0.00039***	0.00039***	0.00036***	0.00040***	0.00038***	0.00038***
	(0.00008)	(0.00007)	(0.00007)	(0.00008)	(0.00009)	(0.0001)	(0.00002)
Mean outcome at t=-1 (treated)	0.0015	0.0014	0.0014	0.0013	0.0013	0.0013	0.0010
Effect relative to the mean	32%	28%	28%	28%	31%	29%	38%
Observations	11,352,964	9,576,350	8,609,482	5,564,314	3,856,160	2,405,396	47,714,254
PANEL B: FEMALE JOB LOSS							
Dependent var.: Prob. of filing	Mass layoffs with less than X workers displaced						All layoffs
Protective Measure	Baseline	1000	500	100	50	30	excluding mass layoffs
Effect of job loss	0.00040***	0.00042***	0.00047***	0.00051***	0.00052***	0.00059***	0.00014***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.00004)
Mean outcome at t=-1 (treated)	0.0007	0.0008	0.0008	0.0009	0.0008	0.0009	0.0005
Effect relative to the mean	57%	53%	59%	57%	65%	66%	27%
Observations	1,273,160	1,136,380	1,049,440	774,872	602,938	416,738	12,172,384

Notes: This table shows the effect of male (Panel A) and female (Panel B) job loss on the probability of DV perpetration/victimization using different samples, as estimated from the difference-in-differences equation (2). Columns 1-6 show the results when dropping from the sample mass layoffs in which more than a given number of workers from the same firm are displaced at the same time. Column 7 shows the results obtained when using a different sample covering all layoffs in the data, but excluding mass layoffs. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table D9: Descriptive statistics, mass layoffs vs. all layoffs

	(1)	(2)	(3)	(4)
	Male		Female	
	Mass layoffs	All layoffs	Mass layoffs	All layoffs
Demographic characteristics				
Years of education	10.0	10.9	11.5	11.7
Age	30.3	29.7	30.5	29.8
Race - white	41.8%	52.1%	46.6%	55.4%
Race - black	5.7%	4.7%	3.1%	2.8%
Race - mixed	43.8%	34.4%	39.0%	31.2%
Job characteristics				
Monthly income (R$)	1,438	1,411	1,063	1,056
Month of worked $t-1$	10.7	11.1	11.2	11.3
Tenure on Jan 1st (years)	1.1	1.6	1.4	1.6
Manager	2.5%	5.2%	6.0%	7.5%
Firm size (employees)	724	454	667	419
Local area - municipality				
Large municipality - pop> 1M	42%	36%	37%	30%
Municipality population	2,601,919	2,316,118	990,340	825,364
Homicide rate (per 100k inhab.)	32.8	29.2	40.8	34.7
Domestic Violence				
Prob. of DV suit or PM $t-1$	0.0015	0.0013	-	
Prob. of DV suit $t-1$	0.0006	0.0006	-	
Prob. of PM $t-1$	0.0009	0.0008	0.0007	0.0007
Observations	810,926	4,219,087	90,940	960,396

Notes: This table reports by gender the average characteristics for workers displaced in mass layoffs in our sample (columns 1 and 3), vs. workers displaced in all layoffs (columns 2 and 4).

D.6.F Missing data on names

We now address selection concerns due to the fact that we cannot observe the alleged offender's identity in 40% of DV cases (prosecutions plus PM) and the victim's identity in 46% of cases in our main analyses samples. Although the judicial records do not include any demographic information on victims or perpetrators, we start by providing some information on the extent of selection across cases with and without missing names. In Table D10, we show the distribution of offense types across the

two groups. Even though some differences emerge, offense types do not sharply differ between the two groups. We also investigate to which extent missing name status is driven by court-level factors. A OLS regression with court-fixed effects shows that 49% and 58% of the variation in perpetrator and victims' missing name status are driven by court-level factors. Since our analysis leverage variation in layoffs within individuals working in the same area, this attenuates to some extent selection issues in our analysis.

We proceed with three robustness tests. First, we take advantage of the fact that the share of court cases with missing identity varies widely across states and jurisdictions, and show in Table D11 that our estimates are robust to progressively dropping from the sample areas where the share of missing data is above a certain level. In particular, our key findings hold when exclusively looking at areas where missing data is not a substantial concern. Second, our findings on male job loss hold when we restrict the sample to DV prosecutions initiated "*in flagrante*" (Figure D4). In such cases, judges take the initial decision on case secrecy based on the police form describing the arrest while lacking additional information on the defendant characteristics' such as employment status. Hence, differential under-reporting should be a lesser concern. Third, in Section 5.4 we show that the same key findings emerge when we analyze alternative DV measures – SINAN notifications and DV shelter use – which are not subject to these missing data issues.

Table D10: Distribution of offense type, by missing name status in judicial cases

	(1)	(2)	(3)	(4)
	Perpetrator		Victim	
	Missing status		Missing status	
Offense type:	Yes	No	Yes	No
Slander	0.08	0.12	0.13	0.11
Threatening	0.24	0.39	0.42	0.51
Battery	0.01	0.03	0.02	0.02
Assault	0.41	0.42	0.32	0.27
Against freedom	0.26	0.04	0.10	0.09

Notes: This table shows the distribution of offense types in DV court cases filed in the 2009-2017 period. It compares DV cases for which the perpetrators' names are missing (columns 1-2) and for which the victims' names are missing (columns 3-4).

Table D11: Effect of job loss on domestic violence, robustness to missing values in the judicial data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: MALE JOB LOSS								
Dependent var.: Prob. of DV	Only states with a share of non-missing names in prosecution records above:							
	10%	20%	30%	50%	60%	70%	80%	85%
Effect of job loss	0.000480***	0.000607***	0.000598***	0.000608***	0.000609***	0.000543***	0.000463*	0.000497*
	(0.0000788)	(0.000118)	(0.000129)	(0.000133)	(0.000157)	(0.000145)	(0.000255)	(0.000291)
Mean outcome at t=-1 (treated)	0.0015	0.0020	0.0021	0.0021	0.0025	0.0011	0.0009	0.0005
Effect relative to the mean	32%	30%	29%	28%	24%	49%	52%	106%
Observations	11,352,964	7,452,536	6,740,468	6,534,304	5,213,936	2,859,430	1,035,216	744,282
PANEL B: FEMALE JOB LOSS								
Dependent var.: Prob. of filing	Only jurisdictions with a share of non-missing names in prosecution records above:							
Protective Measure	10%	20%	30%	40%	50%	60%	70%	80%
Effect of job loss	0.000397***	0.000415***	0.000526***	0.000478***	0.000504***	0.000450***	0.000558***	0.0000713
	(0.000137)	(0.000144)	(0.000174)	(0.000139)	(0.000145)	(0.000149)	(0.000193)	(0.000268)
Mean outcome at t=-1 (treated)	0.0007	0.0007	0.0009	0.0007	0.0007	0.0007	0.0009	0.0001
Effect relative to the mean	56%	55%	59%	73%	74%	63%	62%	61%
Observations	1,273,160	1,201,018	967,477	805,840	771,253	706,888	479,080	118,440

Notes: This table shows the effect of male (columns 1-3) and female (columns 4-6) job loss on the probability of DV perpetration/victimization, as estimated from the difference-in-differences equation (2), while progressively restricting the sample to states/jurisdictions in which the share of non-missing names in prosecution records is above a certain threshold (indicated on top of each column). The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for workers displaced upon mass layoffs, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.6.G Sample selection due to unique name restriction

We rely on individuals' full names to merge the employment data with the judicial records. To ensure precision in the merge procedure, we restrict our main sample to workers who have unique names in the country (about 50% of the Brazilian population). We now address the fact that this restriction could have implications for the representativeness of our sample. First, in Table C1, we have shown that individuals with unique names are reasonably similar to individuals without unique names, which mitigates selection concerns. Second, we run our analysis based on a matching procedure which significantly increases population coverage. Specifically, we restrict the data to workers who have unique names within their state of work (rather than in the entire country), and merge the employment data with the judicial records based on individuals' names and state. This increases sample coverage from 50% to about

70% of the population. The results in Table D12, Panel A, show that we find similar results using this extended sample. Third, we re-estimate our analysis after reweighting our sample to match the characteristics of all workers displaced in mass layoffs (with and without unique names), using the entropy algorithm by Hainmueller (2012); Hainmueller and Xu (2013). The results in Table D12, Panel B, remain similar to our main analysis. Overall, these exercises support the idea that selecting on unique names does not generate strong representativeness concerns for our main analysis.

Table D12: Effect of job loss on domestic violence, robustness to sample representativeness due to unique name restriction

	(1)	(2)	(3)	(4)	(5)	(6)
Job loser	Male			Female		
	Labor market effects		Probability of DV	Labor market effects		Prob. of Filing
Dependent variable:	Employment	Income	Any	Employment	Income	Protective Measure
PANEL A: EXTENDED SAMPLE, WORKERS DISPLACED IN MASS LAYOFFS WITH UNIQUE NAMES IN THE STATE OF WORK						
Effect of job loss	-0.22***	-6104.8***	0.00025***	-0.23***	-4402.3***	0.00034**
	(0.002)	(70.5)	(0.00007)	(0.004)	(64.4)	(0.0001)
Mean outcome, treated at t=0	1	14,452	0.0014	1	11,059	0.0008
Effect relative to the mean	-22%	-42%	17%	-23%	-40%	42%
Elasticity to earnings			-0.41			-1.07
Observations	16,716,854	16,716,854	16,716,854	1,694,364	1,694,364	1,694,364
PANEL B: REWEIGHTING SAMPLE TO PERFECTLY MATCH OBSERVABLES OF ALL DISPLACED IN MASS LAYOFFS						
Effect of job loss	-0.23***	-7881.3***	0.00052***	-0.24***	-5499.0***	0.00043***
	(0.0024)	(151.1)	(0.000091)	(0.0055)	(252.3)	(0.00013)
Mean outcome, treated at t=0	1	17,176	0.0015	1	12,705	0.0007
Effect relative to the mean	-23%	-46%	34%	-24%	-43%	63%
Elasticity to earnings			-0.74			-1.46
Observations	11,352,964	11,352,964	11,352,964	1,273,160	1,273,160	1,273,160

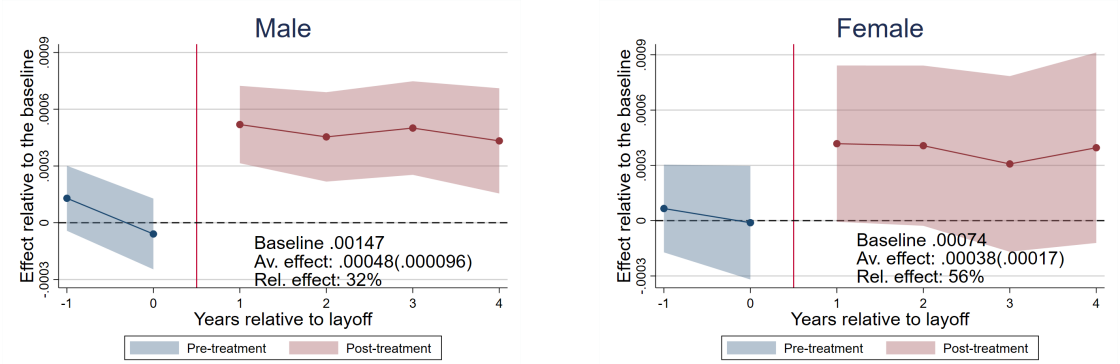
Notes: This table shows the effect of male (columns 1-3) and female (columns 4-6) job loss on the probability of DV perpetration/victimization, as estimated from the difference-in-differences equation (2). Panel A follows our main specification but restricting the data to workers with unique names in their state of work rather than in the entire country, increasing sample coverage. Panel B follows our main specification but reweighting the sample to match the characteristics of all workers displaced in mass layoffs, with and without unique names. Weights are obtained with the entropy algorithm by Hainmueller (2012); Hainmueller and Xu (2013). The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for workers displaced upon mass layoffs, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.6.H Estimation of dynamic treatment effects

Several recent studies highlight the challenges associated with estimating dynamic treatment effects in two-way fixed effects settings when there is variation in the treatment timing and treatment effects are heterogeneous across individuals. Under these

conditions, some treated individuals might enter the double differences estimating the dynamic treatment effects with weights of opposite signs in different time periods. As a result, the estimated treatment effect differs from the average treatment effect, nor it is representative of any relevant population of interest – see Sun and Abraham (2020); Athey and Imbens (2018); Borusyak and Jaravel (2017); Callaway and Sant'Anna (2020); de Chaisemartin and D'Haultfœuille (2020); Goodman-Bacon (2021); Imai and Kim (2021). This problem is most severe when all or a large share of individuals in the sample are treated at some point. We overcome these issues by including a large share of never-treated workers. We estimate the share of units with negative weights following de Chaisemartin and D'Haultfœuille (2020), and find that no individual treatment effect receives a negative weight both for male and female job loss. It is worth noting that our difference-in-differences estimator is similar in spirit to the strategies proposed in the recent literature, which mainly vary in the way of selecting "pure", not yet treated, control units. In fact, we show in Figure D9 that our estimates remain similar when using the estimator proposed by Callaway and Sant'Anna (2020), which further supports the robustness of our main empirical strategy.

Figure D8: The effect of male and female job loss on domestic violence, judicial suits, Callaway and Sant'Anna (2020) estimator



Notes: This figure shows the effect of job loss on the probability of DV perpetration in DV suits for men and DV victimization in protective measures for women, following the estimator proposed by Callaway and Sant'Anna (2020) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. The reported baseline is the average value of the outcome in the treated group at $t = 0$. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

D.6.I Low probability outcome with difference-in-differences

As pointed by Athey and Imbens (2006), studying probability outcomes with standard difference-in-difference designs (DID) may result in predicted counterfactual probabilities which lie outside the range zero-one. The fact that we define the control group via matching makes DV rates more comparable in the pre-treatment period and reduces the likelihood of this issue. Similar baseline probabilities also mitigate the potential issue that proportional effects could strongly differ in the treatment and control groups for given absolute variations in the outcome. In fact, predicted counterfactual yearly probabilities in the post-treatment period are .18 and .11 p.p. in our male and female job loss analyses, respectively. Hence, they remain within the range zero-one.

In any event, we implement the changes-in-changes estimator proposed by Athey and Imbens (2006), where the common-trend assumption refers to proportional changes in the probability of DV rather than level changes, as in our baseline approach. Under some additional assumptions (including conditional independence), this estimator is well suited to deal with probability outcomes. Table 13 shows that the results remain fairly similar to our main estimates.

Finally, in Table 14, we follow yet another approach by re-estimating our main model for groups of workers with low and high ex-ante DV risk. We use a wide set of interacted fixed-effects, grouping observations by municipality of work, age, race, education and income level dummies, to generate these predictions. We use a leave-one-out procedure to avoid overfitting issues. The table shows that DV risk increases significantly both for low and high DV risk groups, and the effects retain the same order of magnitude relative to our main estimates. Baseline DV risk is about three and ten times larger in the high risk groups for the analyses on male and female job loss, respectively (although they remain low in absolute terms). This test is yet another way to address the potential issues of studying a low probability outcome in our difference in differences setting. Overall, all these alternative approaches support our main findings: they show positive effects of job loss on DV risk and coefficients indicate the same order of magnitude in terms of effect sizes.

Table 13: Effect of job loss on labor market outcomes and domestic violence, Athey and Imbens (2006) changes-in-changes estimator

	(1)
Dependent var.: Prob. of DV	changes-in-changes
PANEL A: MALE DISPLACED IN MASS LAYOFFS, DV PERPETRATION	
Effect of job loss	0.00055***
	(0.00012)
Mean outcome at t=-1 (treated)	0.0013
Effect relative to the mean	41%
Observations	3,243,704
PANEL B: FEMALE DISPLACED IN MASS LAYOFFS, DV VICTIMIZATION	
Effect of job loss	0.0004**
	(0.00016)
Mean outcome at t=-1 (treated)	0.0005
Effect relative to the mean	88%
Observations	363,760

Notes: This table shows the effect of job loss on DV for male (Panel A) and female (Panel B) workers, using the changes-in-changes estimator by Athey and Imbens (2006). The dependent variable is indicated on top of each column. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. The data is collapsed at at the average into a single pre and post-treatment period: 3 years before and 4 years after job loss. Bootstrapped standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 14: Effect of job loss on labor market outcomes and domestic violence, by predicted DV risk

	(1)	(2)	(3)	(4)
PANEL A: JOB LOSS	Male, perpetration		Female, victimization	
Dependent var.: Prob. of DV	Predicted DV risk		Predicted DV risk	
	low	high	low	high
Effect of job loss	0.00046***	0.00072**	0.00036***	0.0040**
	(0.00007)	(0.0004)	(0.0001)	(0.002)
Mean outcome at t=-1 (treated)	0.0013	0.0039	0.0006	0.0067
Effect relative to the mean	37%	19%	56%	59%
Observations	10,396,974	955,990	1,258,600	14,560

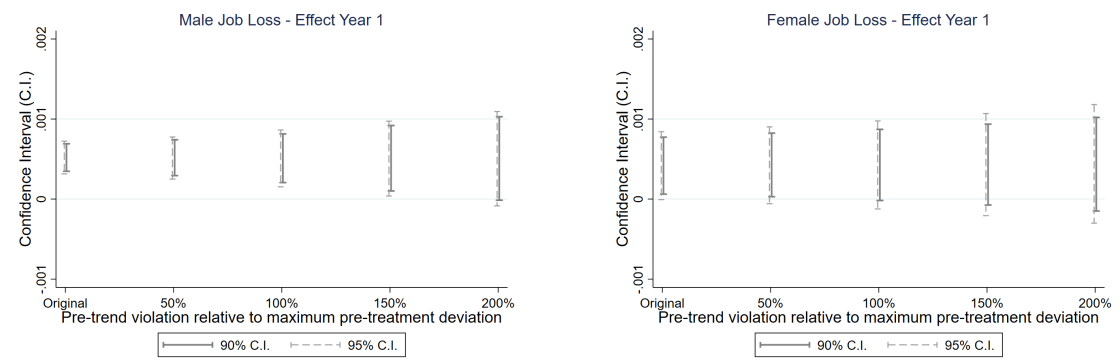
Notes: This table shows the effect of job loss on DV perpetration/victimization outcomes, for males in Panel A and females in Panel B, as estimated from the difference-in-differences equation (2). We split the samples according to predict DV risk in the pre-treatment period. Predictions are based on a set of interacted fixed-effects, by municipality of work, age, race, education and income level dummies. We use leave-one-out predictions to prevent overfitting issues. The dependent variable is indicated on top of each column. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.6.J Additional pre-trend testing

We engage with recent literature on pre-trends testing which should support the common-trend assumption in difference-in-differences designs. Recent papers discuss that standard tests on pre-trends coefficients may have low power to detect meaningful deviations which could generate sizable biases to the estimated treatment effects – e.g., see Bilinski and Hatfield (2018); Roth (2022). In our setting, in addition to show that there is no evidence on pre-trends deviation for the effects of job loss, both at the yearly and quarterly levels (Figures 3 and D7), we provide numerous tests addressing issues related to selection into layoffs, even within mass layoffs, which we consider the main threat to the common-trend assumption in our setting – see Section D.6.D. Nevertheless, two additional pieces offer additional transparency and evidence supporting our main results. In light of the low power issues related to pre-trends testing, we follow the suggestion in Bilinski and Hatfield (2018) and report non-inferiority tests for our pre-trends. The idea is taking some level of pre-trend deviation as the null hypothesis and testing whether it is possible to reject such deviation. We

find that we can reject a pre-trend deviations with 50% of the size of the effect we detect in the first year after layoff both for the analysis on male and female job loss (.003 and .081 p-values, respectively). Second, we report results based on inference which is robust to some degree of pre-trend deviation, as developed by Rambachan and Roth (2023). Figure D9 shows confidence intervals which are robust to varying degrees of pre-trend deviations relative to the maximum deviation detected in the period, studying the impacts on the first year after the layoff. The left-panel shows that male job loss effects retain statistical significance even for extreme scenario where trends deviation are assumed to be twice as large relative to the maximum deviation observed in the pre-treatment period. In turn, despite the fact that the analysis of female job loss is based on a smaller sample and statistical power is considerably lower, it also displays confidence intervals largely above zero for varying degrees of pre-trends deviation. Overall, we interpret these additional results as supportive of our main findings.

Figure D9: The effect of male and female job loss on domestic violence, judicial suits, inference robust to pre-trends deviation, Rambachan and Roth (2023) estimator



Notes: This figure shows 90% and 95% confidence intervals for the (absolute) effect of job loss on the probability of DV perpetration in DV suits for men and DV victimization in protective measures for women, one year after the layoff, following the estimator proposed by Rambachan and Roth (2023). It assumes different levels of pre-trends deviation based on the maximum deviation observed in pre-treatment coefficients before the treatment (see x-axis). The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

D.7 Estimates for Couples

We now show that our main findings hold within a sample of cohabiting couples. We use the registry for Federal social programs (CadUnico) to identify the cohabiting partners of job losers in our main sample. In just 3% of this sample, both spouses lose their jobs in a mass layoff, and we drop these cases. Estimates on the remaining sample for whom it is possible to identify a cohabiting spouse, using the same empirical strategy (Section 5) are in Table D15. We find that male job loss results in a 77% increase in female partners filing protective measure requests (Panel A, column 1), and that female job loss increases the probability that male partners are judicially charged for DV by 38% (Panel B, column 1).

The household sample allows us to investigate heterogeneity in the impacts of job loss by baseline household characteristics (Table D15, Columns 2-5). We find that both male and female job loss have larger impacts on DV when there is a child under the age of ten in the home, consistent with the income shock being more stressful in young families with dependents. Male job loss has a larger impact on DV when the female partner is not employed at baseline. This is consistent with both an income mechanism – losses are stronger when the partner is not employed – and with an exposure mechanism – partners spend more time together during the unemployment period. Yet, these comparisons should be interpreted with caution as estimates are not particularly precise in this reduced sample and sub-group estimates are often not statistically significantly different from one another.

Next, we investigate couple's stability for job losers who continue to show in the social registry in the post displacement period, see Table D15. Interpreting these estimates required some caution as the probability of continuing in the social registry increases after job loss, albeit this is arguably a small effect, ranging from 3% to 5% relative to the baseline (column 6). We find that job loss does not strongly affect the probability that job losers stay with the same partner after the layoff. Male job loss does not affect the probability that men retain the same partner, while female job loss reduces partner's stability by 3% relative to the baseline (column 7). Finally, in column 8, we show that partners' employment is not significantly affected by job loss, indicating that "added worker effects" are not sizable in our context. The latter supports the premise of our exposure mechanism whereby partners spend more time together following displacement.

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Table D15: Effect of male and female job loss on partners using household data

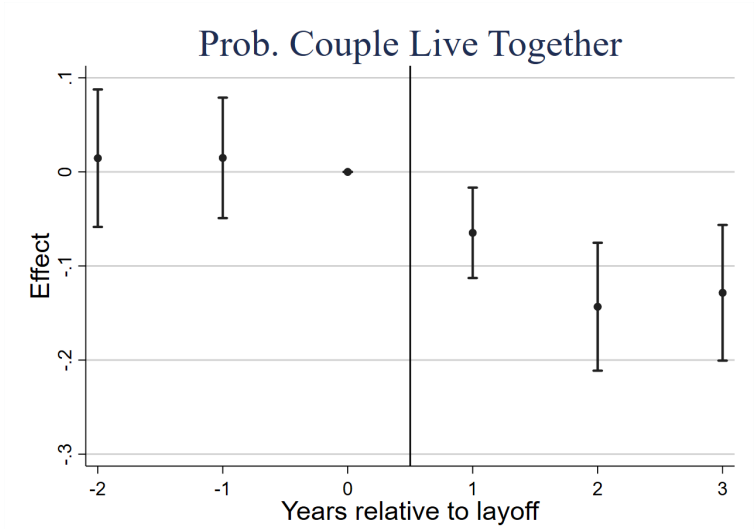
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Domestic Violence					In CadUnico	Same Couple	Employment
Sample	All	Youngest Child Age ≤ 10	Youngest Child Age > 10	Partner Employed $t = 0$	Partner Not Employed $t = 0$	All	All	All
PANEL A: MALE JOB LOSS, PROB. FEMALE PARTNER FILES PROTECTIVE MEASURE								
Effect of job loss	0.00050*	0.00068***	0.00027	0.00029	0.00053*	0.043***	0.0072	0.0012
	(0.0003)	(0.0003)	(0.0005)	(0.0005)	(0.0003)	(0.005)	(0.005)	(0.002)
Mean outcome, treated at t=0	0.0006	0.0007	0.0006	0.0014	0.0005	0.8630	0.9048	0.1474
Relative variation	77%	96%	47%	21%	103%	5%	1%	1%
Observations	433,990	238,655	195,335	63,970	370,020	311,512	232,109	433,990
PANEL B: FEMALE JOB LOSS, PROB. MALE PARTNER PROSECUTED FOR DOMESTIC VIOLENCE								
Effect of job loss	0.00092*	0.0016**	0.00029	0.0011	0.00082	0.030***	-0.026***	0.000035
	(0.0005)	(0.0007)	(0.0006)	(0.0007)	(0.0006)	(0.005)	(0.005)	(0.003)
Mean outcome, treated at t=-1	0.0024	0.0026	0.0022	0.0023	0.0025	0.9657	0.7825	0.3754
Relative variation	38%	61%	13%	49%	33%	3%	-3%	0%
Observations	236,280	116,435	119,845	88,700	147,580	183,540	158,851	236,280

Notes: Columns 1-5 in this table show the effect of male job loss on DV victimization by the partner (Panel A) and the effect of female job loss on DV perpetration by the partner (Panel B), as estimated from the difference-in-differences equation (2). In both panels, the sample is restricted to displaced workers present in the social registry in 2011, for whom it is possible to identify the respective partner. Columns 6-7 presents the same results on the prob. that the worker is still present in the registry after the job loss and, if registered, that she/he has the same partner, while column 8 shows the results for the partners' employment probabilities. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

D.8 Couples' separation around the timing when women file DV cases

In Figure D10, we provide an analysis investigating separation patterns around the time when DV cases are filed against the male partner. The outcome is the probability that couples in the social registry (CadUnico) continue to live together. Specifically, we study couples for which the male partner is prosecuted for the first time in 2013, and take couples prosecuted in the future (2016) as controls. The results show that an average 11 p.p. reduction in the probability that the couple continue to live together in the three-year period after the initial prosecution (2013).

Figure D10: The effect of male job loss on domestic violence, judicial suits, by area-level characteristics



Notes: This figure shows how the probability that couple live together vary around the timing when the male partner is prosecuted for DV in criminal courts. The estimation follows the difference-in-differences equation (1) – along with 95% confidence intervals, with the event being defined by the first DV prosecution. The treatment group comprises couples where the male partner was first prosecuted for DV in 2013, while the control group is composed by couples with first DV prosecution in 2016. The analysis follows calendar years from 2010 to 2015.

D.9 The role of labor informality

In Section D.2, we show that labor income losses become about 10% smaller once we take into account reemployment in informal jobs. Hence, one possible explanation for our main finding is that workers take informal jobs, which might be more stressful due to the low job security or safety standards, which in turn could drive the observed increase in domestic violence. We gather evidence on this by studying how our results change across workers exposed to different degrees of labor informality. Previous results in Figure D11 shows some initial evidence on that for male job loss. In particular, they show that our main estimates are similar in areas with more and less labor informality. It is worth noting that informality greatly vary across Brazil, varying from 31% to 70% when moving from the first to tenth decile of the distribution of informality rates across municipalities. In Table D16, we use a more fine grained informality measure, at the 2-digit sector by state level. The results show that our results retain the same direction and order of magnitude as we progressively exclude from the sample workers more exposed to labor informality, both for male and female job loss. Only one coefficient loses statistical significant because estimates become more imprecise due to the smaller sample size. Overall, these results do not support

the idea that our main findings are primarily driven by exposure to informal jobs after formal displacement.

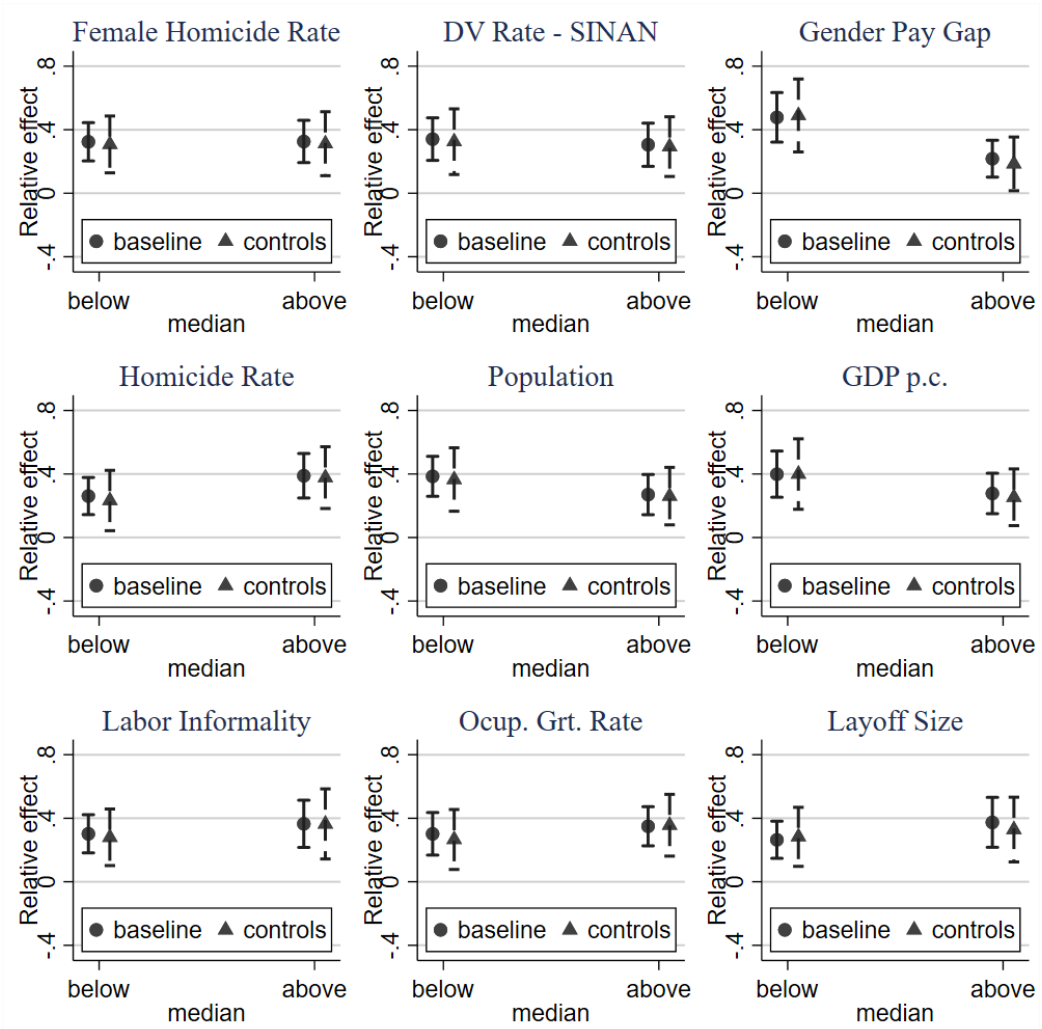
Table D16: Effect of job loss on domestic violence, the role of labor informality

	(1)	(2)	(3)	(4)
PANEL A: MALE JOB LOSS				
Dependent var.: Prob. of DV	Informality rate at sector-state level in the first X quartiles			
	4	3	2	1
Effect of job loss	0.00048***	0.00050***	0.00052***	0.00048***
	(0.00008)	(0.00009)	(0.0001)	(0.0001)
Mean outcome at t=-1 (treated)	0.0015	0.0017	0.0019	0.0015
Effect relative to the mean	33%	30%	27%	33%
Observations	11,352,936	8,524,026	6,031,270	2,868,642
PANEL B: FEMALE JOB LOSS				
Dependent var.: Prob. of filing	Informality rate at sector-state level in the first X quartiles			
Protective Measure	4	3	2	1
Effect of job loss	0.00040***	0.00038**	0.00045**	0.0004
	(0.0001)	(0.0002)	(0.0002)	(0.0003)
Mean outcome at t=-1 (treated)	0.0007	0.0007	0.0006	0.0005
Effect relative to the mean	56%	54%	71%	77%
Observations	1,273,160	955,738	641,004	320,348

Notes: This table shows the effect of male (Panel A) and female (Panel B) job loss on the probability of DV perpetration/victimization using different samples, as estimated from the difference-in-differences equation (2). Columns 1-4 show the results when dropping from the sample workers in 2-digit sectors-state cluster with informality rates above the X_{th} quartile. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for treated workers, interacted with a dummy $Post_t$ equal to 1 for the periods after displacement. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treat_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

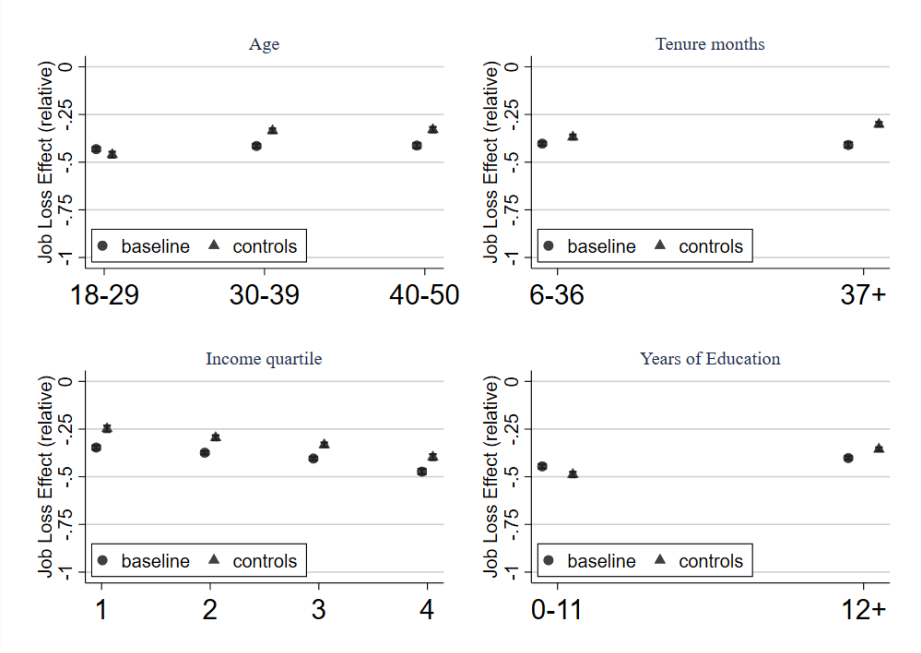
D.10 Additional heterogeneity analyses

Figure D11: The effect of male job loss on domestic violence, judicial suits, by area-level characteristics



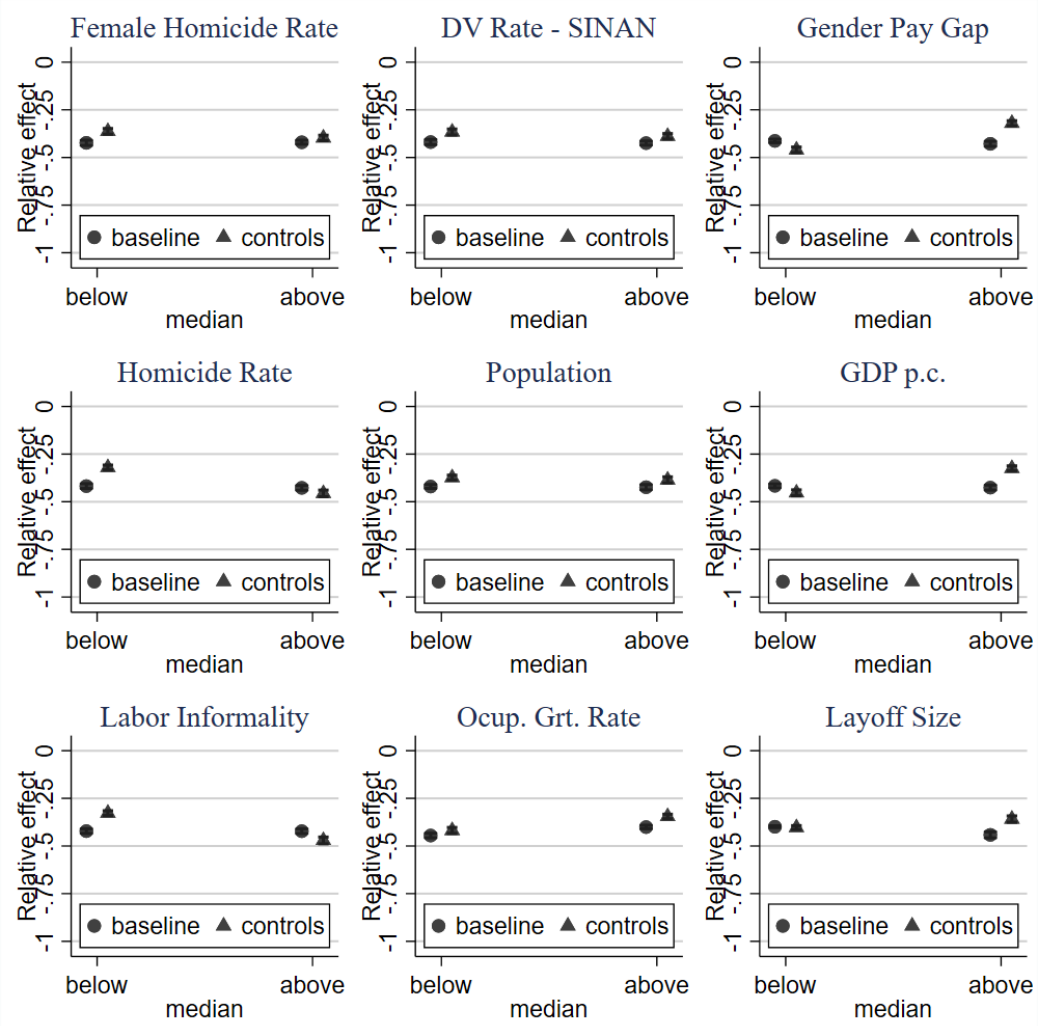
Notes: This figure shows the effect of male job loss on the probability of DV perpetration in DV suits in the four years after layoff, by area-level characteristics – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the eq. with third-order polynomials on individual characteristics. The gender pay gap is computed at the microregion level in a regression with interacted dummies controlling for hours, occupation, tenure and education. Layoff size indicates the number of displaced individuals in the same mass layoff event. GDP, population and labor informality are based on 2010 pop. Census. Employment growth rate in the worker occupation is computed at the yearly level based on RAIS. SINAN DV rate is based on mandatory DV notifications by health providers. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Figure D12: The effect of male job loss on labor income by individual characteristics



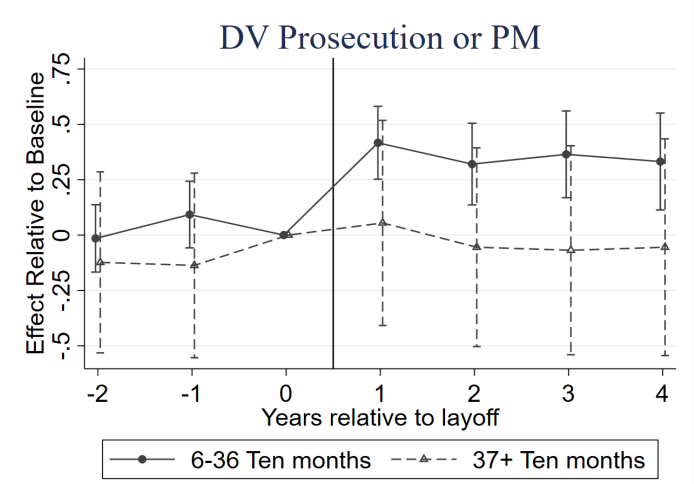
Notes: This figure shows the effect of male job loss on labor income in the four years after layoff – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the eq. with third-order polynomials on individual characteristics. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Figure D13: The effect of male job loss on labor income by area-level characteristics



Notes: This figure shows the effect of male job loss on labor income in the four years after layoff, by area-level characteristics – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the eq. with third-order polynomials on individual characteristics. The gender pay gap is computed at the microregion level in a regression with interacted dummies controlling for hours, occupation, tenure and education. Layoff size indicates the number of displaced individuals in the same mass layoff event. GDP, population and labor informality are based on 2010 pop. Census. Employment growth rate in the worker occupation is computed at the yearly level based on RAIS. SINAN DV rate is based on mandatory DV notifications by health providers. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Figure D14: The effect of male job loss on domestic violence by tenure

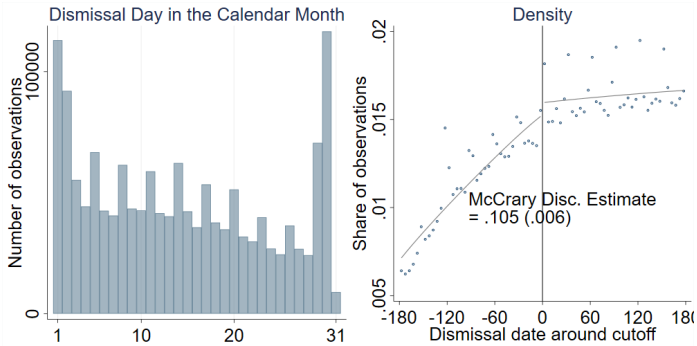


Notes: This figure shows the dynamic effects of male job loss on probability of DV perpetration in DV suits by tenure at displacement as in eq. (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

E Appendix to Section 6

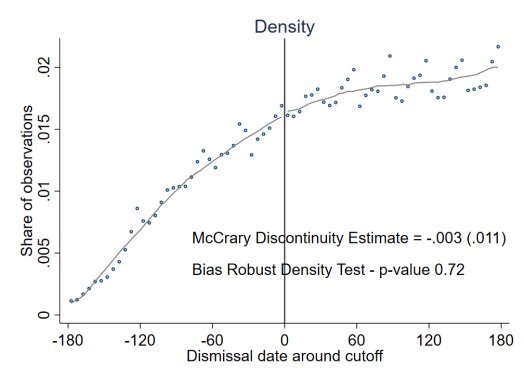
E.1 Dismissal cycles

Figure E1: The effect of UI eligibility, dismissal patterns, extended sample



Notes: The left graph presents the distribution of dismissal dates by calendar day within each month. The right graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

Figure E2: The effect of UI eligibility, density around the cutoff, main sample, male workers



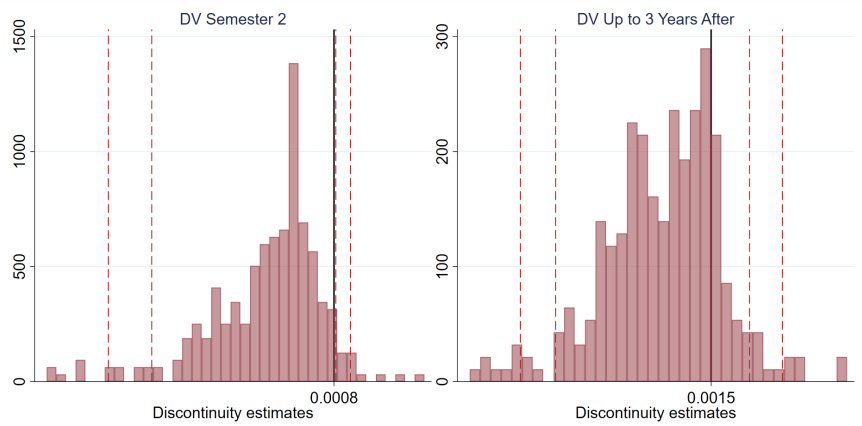
Notes: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past) in our main working sample. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. The results of McCrary density test and the bias robust test proposed by Cattaneo, Jansson and Ma (2018, 2020) are also reported.

E.2 Robustness

Figure E3: The effect of UI eligibility, balance of covariates, male workers

Notes: The graphs show the balance of pre-determined covariates around the cutoff for UI eligibility. Dots represent averages based on 10-day bins. The lines are based on a local linear polynomial smoothing with a 45-day bandwidth with 95% confidence intervals.

Figure E4: The effect of UI eligibility on domestic violence, permutations tests, male workers



Notes: The graphs compare discontinuity estimates of the effect of UI eligibility on the probability of DV perpetration (vertical black line) with the distribution of estimates obtained at all possible placebo cutoffs within 180 days away from the actual threshold, for different periods after layoff (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 45-day bandwidth, as in eq. (3).

Table E1: Effect of UI eligibility on domestic violence, male workers, robustness to specification choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PANEL A: Dep. Var.: Probability of DV - semester 2 after layoff									
eligibility for UI benefits	0.0006***	0.0008***	0.0005	0.0008***	0.0006**	0.0006*	0.0010***	0.0010***	0.0006*
	(0.0002)	(0.0002)	(0.0004)	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Mean outcome at the cutoff	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006
Effect relative to the mean	93.3%	124.4%	77.8%	124.4%	93.3%	93.3%	155.5%	155.5%	93.3%
Observations	65,962	60,714	65,962	130,186	191,195	84,495	246,835	295,723	112,429
PANEL B: Probability of DV - up to 3 years after layoff									
eligibility for UI benefits	0.0009*	0.0012**	0.0009	0.0012	0.0011*	0.0015*	0.0017**	0.0011	0.0017
	(0.0005)	(0.0006)	(0.001)	(0.0007)	(0.0006)	(0.0008)	(0.0008)	(0.0007)	(0.0012)
Mean outcome at the cutoff	0.0047	0.0047	0.0047	0.0047	0.0047	0.0047	0.0047	0.0047	0.0047
Effect relative to the mean	19.0%	25.3%	19.0%	25.3%	23.2%	31.6%	35.8%	23.2%	35.8%
Observations	65,962	60,457	65,962	130,186	191,195	117,016	246,835	295,723	117,618
Bandwidth (days)	30	CCT	30	60	90	CCT	120	150	CCT
Polynomial Order	0	0	1	1	1	1	2	2	2

Notes: This table shows the effect of eligibility for UI benefits, as estimated from equation (3), on the probability of DV perpetration for varying specification choices. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 45 days around the cutoff required for eligibility for unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Each panel estimates separate regressions for the different groups, as indicated in their title. The table also reports the baseline mean outcome at the cutoff and the percent effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table E2: Effect of UI eligibility on domestic violence, male workers, robustness to dismissal and cutoff fixed-effects and extended sample design

	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: Dep. Var.: Probability of DV - semester 2 after layoff						
eligibility for UI benefits	0.00076**	0.00074**	0.00082**	0.00082**	0.00061**	0.00069**
	(0.00032)	(0.00034)	(0.00032)	(0.00034)	(0.00028)	(0.0003)
Mean outcome at the cutoff	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006
Effect relative to the mean	118.2%	115.1%	127.5%	127.5%	100.8%	114.0%
Observations	98,167	98,165	98,157	98,155	136,364	136,353
PANEL B: Dep. Var.: Probability of DV - up to 3 years after layoff						
eligibility for UI benefits	0.00153*	0.00142	0.00177**	0.00164*	0.00154**	0.00184**
	(0.00085)	(0.00087)	(0.00086)	(0.00088)	(0.00071)	(0.00074)
Mean outcome at the cutoff	0.0047	0.0047	0.0047	0.0047	0.0046	0.0046
Effect relative to the mean	32.3%	29.9%	37.3%	34.6%	33.2%	39.6%
Observations	98,167	98,165	98,157	98,155	136,364	136,353
Dismissal date FE		X		X		X
Cutoff date FE			X	X		X
Sample	Main	Main	Main	Main	Extended	Extended

Notes: This table shows the effect of eligibility for UI benefits, as estimated from equation (3), on the probability of DV perpetration in DV suits for varying specifications and samples indicated in the bottom of the table. The first four columns progressively include fixed effects for the individual-specific cutoff date and for each dismissal date – defining the running variables – thus relying on variation in the worker-specific dismissal date within groups who have the same cutoff date. In the last two columns, the sample is enlarged to include all workers who were initially dismissed near the beginning and the end of calendar months, thus dropping the initial restriction in the main sample. All regressions include displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 45 days around the cutoff required for eligibility for unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Each panel estimates separate regressions for different reference periods, as indicated in their title. The table also reports the baseline mean outcome at the cutoff and the percent effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table E3: Descriptive statistics of estimation sample for job loss and UI analyses

	(1)	(2)	(3)
	Sample		
	Job loss analysis	UI analysis	Std Diff
Demographic characteristics			
Years of education	10.0	10.6	0.23
Age	30.3	32.4	0.28
Race - white	41.8%	50.5%	0.18
Race - black	5.7%	4.8%	- 0.04
Race - mixed	43.8%	36.2%	- 0.16
Job characteristics			
Monthly income (R$)	1,438	1,384	- 0.04
Tenure on Jan 1st (years)	1.07	0.25	- 0.75
Manager	0.03	0.04	0.10
Firm size (employees)	724	400	- 0.20
Local area - municipality			
Large municipality - pop> 1M	0.42	0.35	- 0.14
Municipality population	2,601,919	2,102,801	- 0.13
Homicide rate (per 100k inhab.)	33	30	- 0.11
Observations	810,926	98,167	

Notes: This table reports the average characteristics of workers in the job loss analysis in Section 5 (column 1), and UI eligibility analysis in Section 6 (column 2); and the standardized difference between the two samples (column 3).

Table E4: Effect of UI eligibility, male workers, reweighting to match characteristics of workers in job loss analysis

PANEL A: DV - AFTER LAYOFF				
	Semester 1	Semester 2	Semester 3	Up to Year 3
Eligibility to UI benefits	0.0003	0.0010***	0.0007	0.0019*
	(0.0004)	(0.0004)	(0.0008)	(0.0011)
Mean outcome at the cutoff	0.0008	0.0006	0.0032	0.0047
Effect relative to the mean	35.6%	155.5%	21.8%	40.1%
Observations	98,167	98,167	98,167	98,167

Notes: This table shows the effect of unemployment insurance (UI) eligibility on the probability of DV perpetration after layoff, as estimated from equation (3) using a Regression Discontinuity Design. Regressions use weights that balance the first two moments for a set of characteristics with the sample used for the job loss analysis in Section 5. Weights are obtained using the entropy algorithm in Hainmueller (2012); Hainmueller and Xu (2013). Semesters are set relative to the layoff date. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 45 days around the cutoff required for eligibility for unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. The table also reports the baseline mean outcome at the cutoff and the percentage effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

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