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

*We thank Dan Anderberg for insightful discussions. The paper benefited from the comments of Koray Aktas, Aimee Chin, Gianmarco Daniele, James Fenske, Manoel Gehrke, Paola Gobbi, Johannes Haushofer, Chinhui Juhn, Andreas Kotsadam, Soren Leth-Petersen, Giovanna Marcolongo, Marta Morando, Eva Mork, Eugenio Proto, Fan Wang and participants in seminars at the Universities of Copenhagen, Delhi, Essex, Gothenburg, Glasgow, Linz, Lahore, Houstan, Chile. We are solely responsible for the contents of this paper. This study was approved by UFPE Review Board (CAAE 72948323.5.0000.5208). We acknowledge financial support from The Harry Frank Guggenheim Foundation. Bhalotra acknowledges support from the European Research Council under the European Union's Horizon 2020 research and innovation programme, grant agreement No. 885698 and from ESRC grant ESM010236-1 awarded to the Human Rights, Big Data and Technology project at the Human Rights Centre in Essex.

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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, 2010) predicts that DV risk increases when women lose their jobs (in line with our

[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.

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 et al., 2003), and supported by evidence showing that DV escalates during national holidays, weekends and nights, when families spend more time together (Vazquez et al., 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 hours worked. When the income mechanism is present, DV risk is decreasing in each partner's consumption level. When the exposure mechanism is present, DV risk is decreasing in each partner's hours worked. The model predicts

[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 loss of control models (e.g., Baumeister and Heatherton, 1996; Bernheim and Rangel, 2004; Card and Dahl, 2011; Loewenstein and O'Donoghue, 2007) and previous work in sociology (e.g., Straus et al., 2017; Johnson, 2017)

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 et al. (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 pure income transfer in the period studied: they are not conditional on job search requirements or

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 in the household is affected by access to unemployment benefits. It relates to several

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.

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 et al., 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]

Our work also relates to recent research showing substantial increases in DV dur-

[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.

ing 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 et al. (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 et al., 2021; Erten et al., 2022; Hsu and Henke, 2021; Leslie and Wilson, 2020; McCrary and Sanga, 2021; Miller et al., 2020, 2022; Piquero et al., 2020), India (Ravindran and Shah, 2020), Mexico (Silverio-Murillo et al., 2023), Peru (Agüero, 2021), the UK (Ivandic et al., 2020), and different Latin American countries (Perez-Vincent and Carreras, 2022); internet search queries (Anderberg et al., 2022; Berniell and Facchini, 2021); female homicides (Asik and Ozen, 2021); survey measures in Spain (Arenas-Arroyo et al., 2021), and Argentina (Gibbons et al., 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 impacts of access to a monetary transfer policy, rather than variations in payment timing of in kind benefits.

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 et al., 2009; Charles and DeCicca, 2008; Zimmer, 2021; Zimmerman, 2006), substance abuse (Black et al., 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 et al. (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 et al. (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. Sections 3 and 4 describe our setting and data, respectively. Sections 5 and 6 present the results on the effects of job loss and unemployment benefits, respectively. Section 7 discusses mechanisms in light of our main empirical findings, and Section 8 concludes.

[18]In turn, stress (Card and Dahl, 2011) and substance abuse (Lee Luca et al., 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.

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.[21] 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 $\psi_i' > 0, \psi_i'' > 0$. Although individuals cannot change states in period 2, such period allow us to study the dynamic effects of UI benefits on DV.

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 $y_{i2}^s < b_i$.[22]

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 each partner's consumption depends only on their own income: $c_{it}(y_{mt}, y_{ft}) = y_{it}$. It also allows for partial spousal

[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 assume that job loss does not affect partner's employment based on the evidence that added worker effects are negligible in our context – see Section 5.7.

[22] 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 employment are supported by empirical evidence (both from our paper and the literature) – see the remainder of this section and Section 7.

insurance, but rules out full spousal insurance: holding constant the other 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. 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 hours worked ($\frac{\delta\phi_t(.)}{\delta h_{it}} < 0$).[23] In Appendix A.1 we show, using survey data, that partners actually spend more time together when they are unemployed.

Let the function $V(.)$ define the value of each state, and let $v_i(.)$ and $u_i(.)$ denote the utility of consumption during employment and unemployment, respectively (assumed to be continuous, increasing, and concave). The value of employment and unemployment for the male partner in the period 1 are given by:

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

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

In turn, the value of employment and unemployment in the period 1 for the female partner are decreasing in DV risk. These values are given by:

[23] 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.

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

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

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

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

Our setting follows from the idea that domestic violence emerges when men lose control of their actions. It considers domestic violence as an unintended outcome arising from partners' interactions, which becomes more likely when partners face higher stress due to low consumption and when partners spend more time together. Given this setting, men do not derive direct utility benefits from violent behavior. This is in line with earlier work in sociology (e.g., Straus et al., 2017; Johnson, 2017) and research based on loss of control models (e.g., Baumeister and Heatherton, 1996; Bernheim and Rangel, 2004; Card and Dahl, 2011; Loewenstein and O'Donoghue, 2007). 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 starts the model in the job loss state, 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.[24] Benefits increase exposure to DV because they lower job search effort and reduce employment during and after the benefit period.[25] 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 starts the model in the job loss state. 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.[26] DV notifications in the health system vary considerably across Brazil, from 15 to 116 per 100,000 thousand inhabitants in municipalities at the 25$^{\text{th}}$ and 75$^{\text{th}}$ percentiles of the distribution.[27] Rich administrative data and rich variation within Brazil make this an appealing setting for studying DV. Relative to the world,

[24]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.

[25]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 et al., 2007; Lalive, 2008; Gerard and Gonzaga, 2021).

[26]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.

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

DV rates in Brazil lie just above rates in the OECD, and below rates in most other developing countries. Reported rates of DV in Brazil have slowly decreased over time, from 9.6% in 1990 to 7.3% 2017.[28]

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.[29]

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

[28] Statistics based on data from the Institute of Health Metrics & Evaluation (IHME) providing comparable DV statistics across countries.

[29] 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.

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 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 (Kurier Tecnologia, 2020). 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.[30] 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

[30] 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.

alternative DV measures that do not suffer from missing data limitations.[31]

Employment registers. We use linked employer-employee data for 2009-2018 covering the universe of formal workers in Brazil (*Relacao Anual de Informacoes Socials*, RAIS) (Ministério do Trabalho e Emprego, 2020). 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.[32]

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.[33] 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.[34]

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 B1). In any event, in Section 5.5, we will assess the sensitivity of our results by retaining all individuals with a unique

[31]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.

[32]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.

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

[34]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.

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 data, we are able to link couples and families using CadUnico, a registry for Federal social programs (Ministério da Cidadania, 2020).[35] 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.[36]

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.

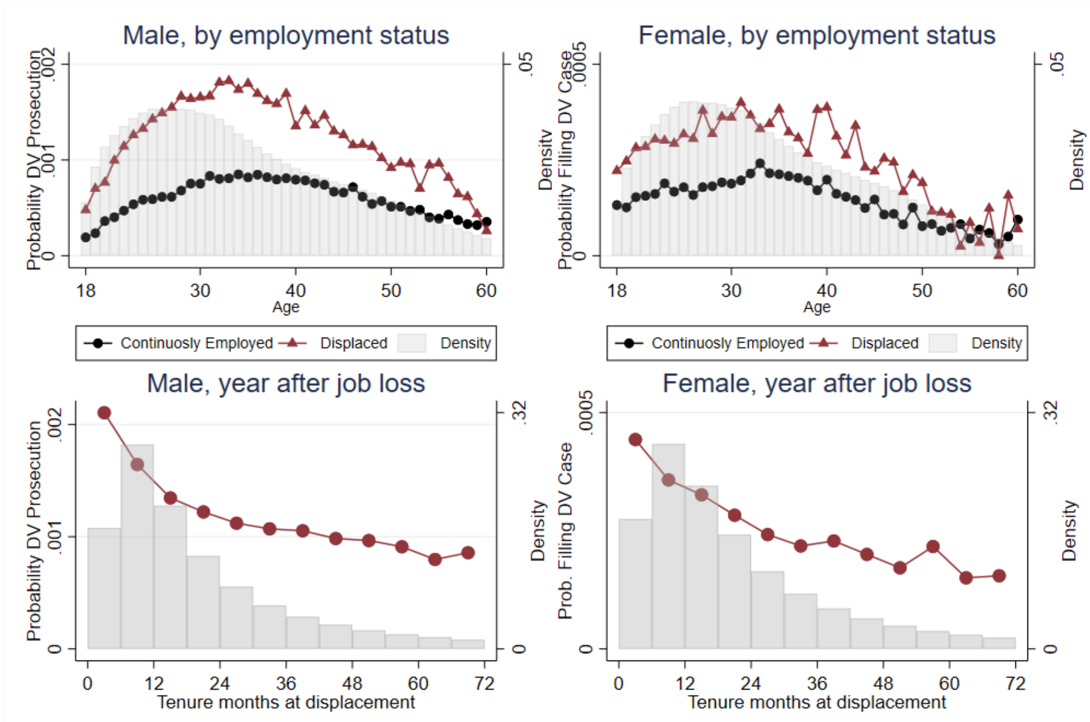
5.2 Identification strategy

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

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

[36]The implementation of our analyses throughout the paper has relied on the following State packages: Bravo (2022); Correia (2017); Calonico et al. (2017); Correia (2016); de Chaisemartin et al. (2019); Hainmueller and Xu (2013); Jann (2004).

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.

period, 2009-2018. 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.[37] We use a perfectly balanced panel tracking units from three years 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

[37]This definition is similar to Jacobson et al. (1993) and Couch and Placzek (2010). We also exclude firms reallocating under a new 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.

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

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

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 et al. (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:

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

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.[38] 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 Ru-

[38]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.

bin, 2015) indicating that any differences in the underlying distribution are small for 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.

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 1^{st} (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

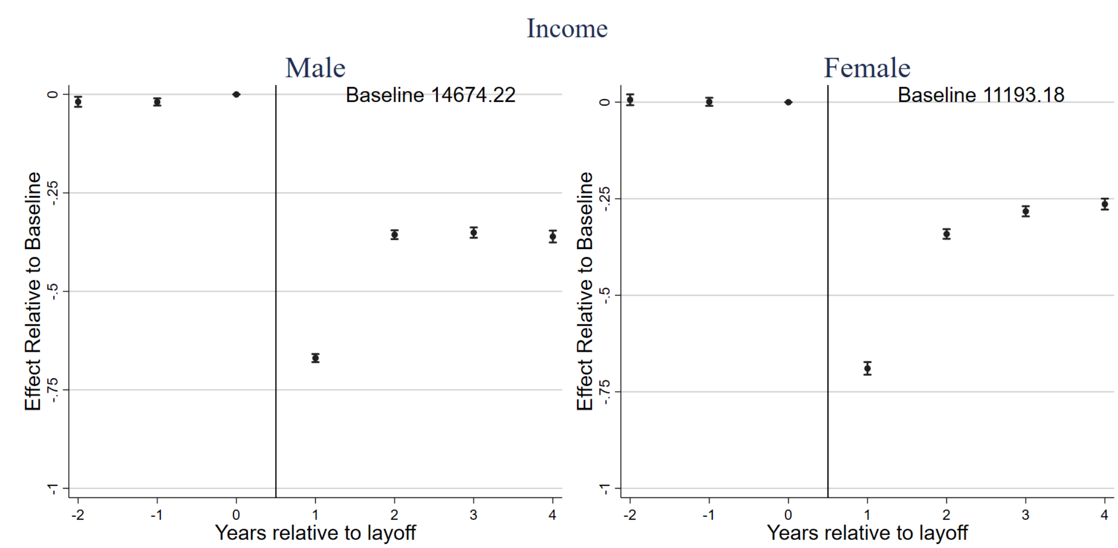
Notes: This table reports by gender the average characteristics for treated workers displaced in mass layoffs, respectively (columns 1 and 4); for matched control workers who are not displaced in the same calendar year (columns 2 and 5); the standardized difference between the two groups (columns 3 and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

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 et al., 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.[39]

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.

Labor income is 70% lower relative to the baseline after male layoff, followed by

[39] 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.

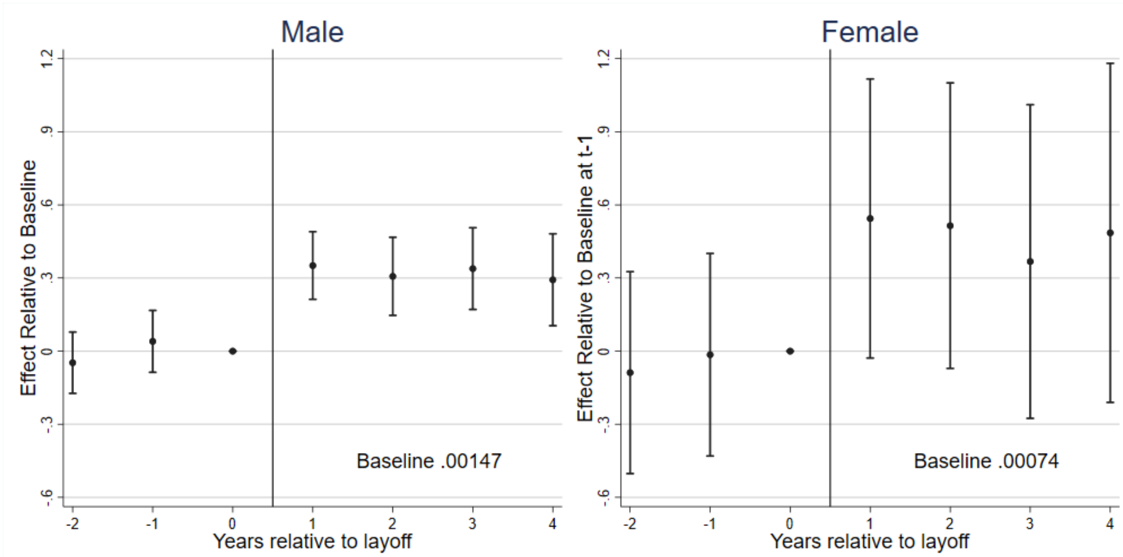
a continuous but slow recovery in the subsequent years. Four years after the shock, the negative impact on labor income remains as high as 36%. The estimates are remarkably similar for women, as shown in the right panel of Figure 2. In Appendix C.1, we show that job loss also has an overall adverse and persistent impact on employment, monthly wages, and job turnover. In Appendix C.2, we use survey data 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.[40]

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 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 C.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).

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

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 C1), 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 C.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.[41]

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.

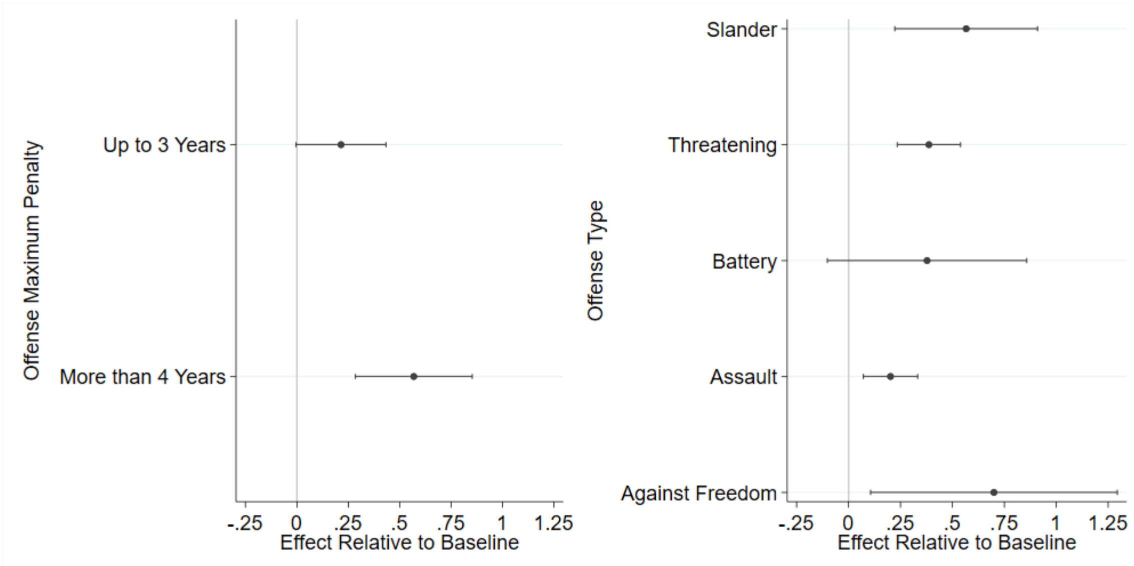
We assess whether reporting bias drives our estimates for male job loss in two ways. First, we exploit variation in the intensity of violence, measured by the type of DV reported and jail time sentence. We expect that more severe offenses are less sensitive to reporting issues. Hence, if our findings 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).[42] Hence, the increase in DV after job loss is not purely driven by changes in reporting of less serious offenses.

Our second strategy is to replicate the analysis using alternative measures of DV

[41]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.

[42]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



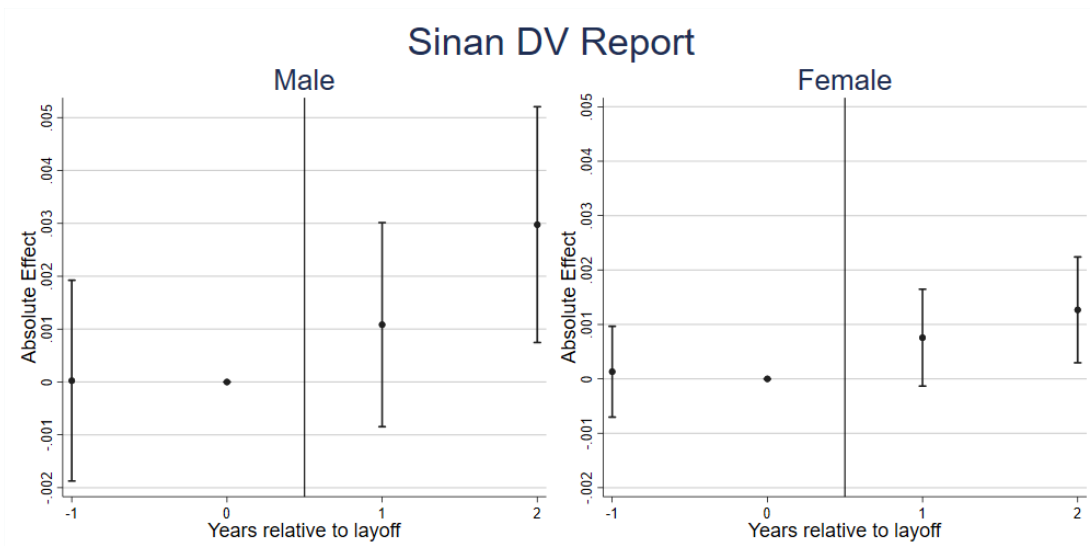
Notes: This figure shows the effect of male job loss on the probability of DV perpetration in DV suits by type and maximum penalty in the four years after the layoff, as estimated from the difference-in-differences equation (2) – along with 95% confidence intervals. The treatment group comprises workers 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.

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 similar to the baseline estimate including all DV cases (on average, 77% vs. 32% in our main specification, see Figure C4 in Appendix C.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 C2 in Appendix C.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 (Ministério da Saúde, 2021a). 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).[43] 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 C.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 C.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 reporting 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.

[43] Perova et al. (2021) uses the same SINAN data to study the relationship between the gender wage gap and DV.

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 C.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 C.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 C.6.A). This addresses the fact that there is some residual imbalance in education in our baseline matching strategy (Table 1). Second, 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 C.6.B).

Third, 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 C.6.C). This eliminates the room for firm discretion in choosing which workers to displaced during a mass layoff. Fourth, we address the fact that our estimates based on mass layoffs could be affected by spillovers across multiple displaced workers (Appendix C.6.D). 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.

Fifth, we address the issue of missing victim and perpetrators' names in our judicial data (Appendix C.6.E). 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 C.6.A), and when using alternative DV measures which do not suffer from missing issues (Section 5.4).

Sixth, 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 B1), our findings remain similar when increasing the representativeness of our sample in different ways – see Appendix C.6.F. Seventh, we address issues related to staggered treatment in difference-in-differences designs in Appendix C.6.H. Eighth, we address the fact that we study a low probability outcome with a difference-in-differences design (Appendix C.6.G). 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. Ninth, Appendix C.6.I provides additional tests related to pre-trends testing and the validity of the common-trend assumption in our setting. Finally, estimates using quarterly data provide further support for the hypothesis of common pre-trends (Appendix C.6.J).[44] These results also confirm that impacts on DV emerge quickly after the

[44]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 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

layoff.

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.[45]

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 C12, 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 C.13 and C.14 in the Appendix show that impacts on labor income do not strongly over the same set of individual and area-level characteristics.[46]

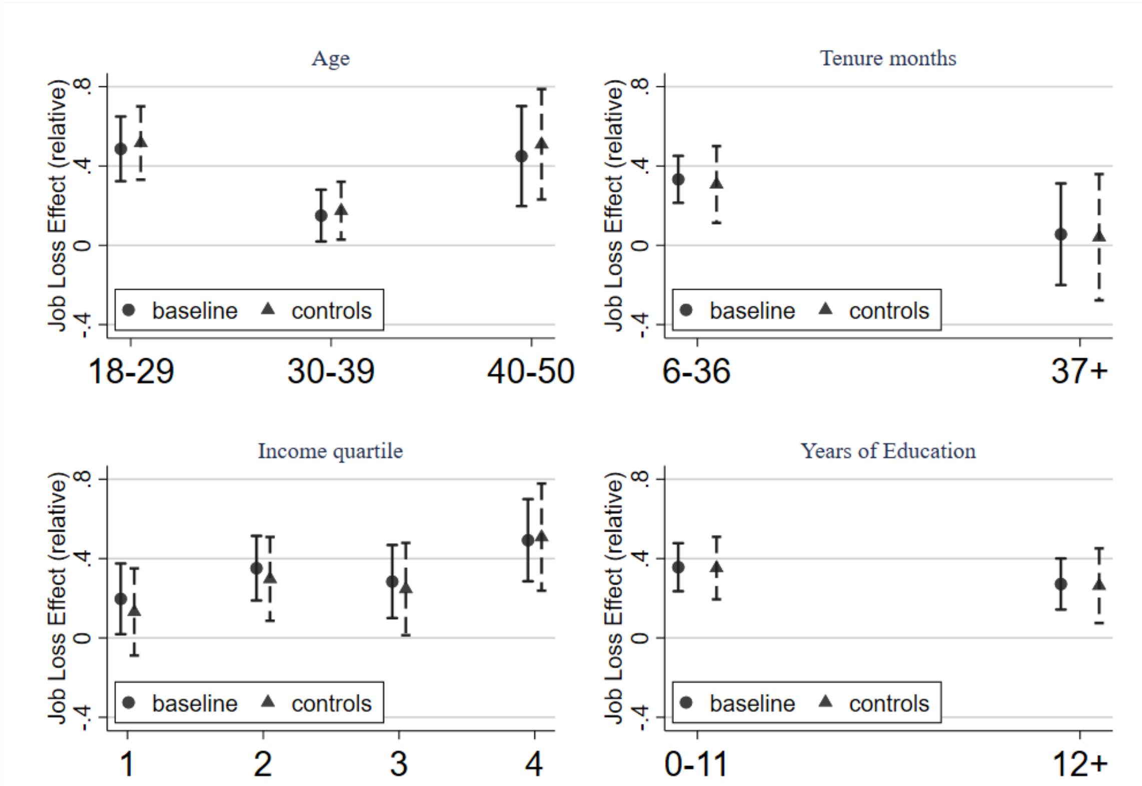
To provide evidence on the income mechanism, we analyze heterogeneous effects over tenure, exploiting the fact that severance pay is increasing in tenure. In the top-right panel of Figure 6, we compare workers displaced with 3 or more years in the job – who receive on average 7 months' wages in severance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

protective measures, which we use in our baseline estimates of the effect of female job loss on victimization (see Section 4).

[45]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 C12).

[46]If anything, groups experiencing somewhat larger labor income losses (Figure C14) also experience larger effects on DV (Figure 6), being in line with the income mechanism discussed in Section 2. Namely, the effects on DV and labor income are stronger for workers with lower tenure, higher baseline income, and lower education. The effects on age do not follow the same patterns, which might be explained by the fact that family structure changes widely over this dimension. To measure area-level characteristics at the municipal level, we use additional data from IBGE (2020); IPEA (2021); Ministério da Saúde (2021b).

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.

wages.[47] Job loss raises DV for workers with 6-36 months tenure but it has a small and statistically insignificant impact on DV among high tenure workers, suggesting that liquidity at displacement may be a mechanism driving DV (Appendix Figure C15 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

[47] 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 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.

dimensions of heterogeneity showing pervasive effects of job loss on DV.

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.

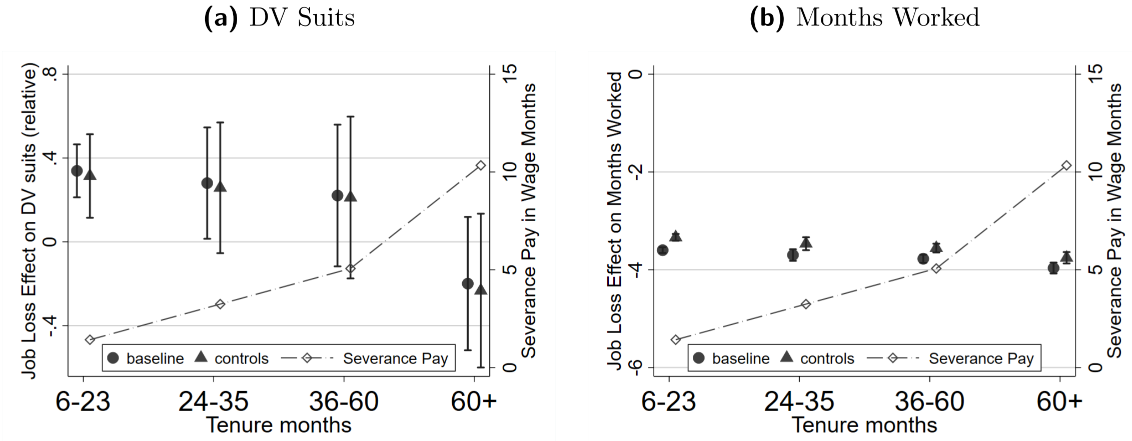
Overall, these results support the relevance of the income mechanism described in our theoretical framework. In Section 6, we will provide further evidence on mechanisms leveraging quasi-experimental variation in access to unemployment benefits.[48]

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 C.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

[48] In Appendix C.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.[49] 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).[50] 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 C.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 not attach a causal interpretation to these estimates because treatment is endogenous,

[49] 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.

[50] 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).

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.[51] 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.[52] 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.[53,54] 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 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

[51] 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).

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

[53] We use data on UI payments to restrict the sample to workers who exhausted all months of UI benefits following the initial displacement (Ministério do Trabalho e Emprego, 2021). 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.

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

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 et al., 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).[55] 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 generate discontinuities in the density of the running variable about every 30 days that are not specific to the 16-month cutoff.[56] In our baseline specification, we address this issue by restricting the sample to workers initially dismissed between the 3^{rd} and 27^{th} 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 D1 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 et al. (2018, 2020). In addition, Figure D2 in the Appendix shows that a rich set of 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 ran-

[55] Accordingly, we match the employment and judicial registers based on name and the state where the worker and the court are located. Appendix Section C.6.F showed robustness of the job loss analysis to using the state level restriction (Figure C7, row 29).

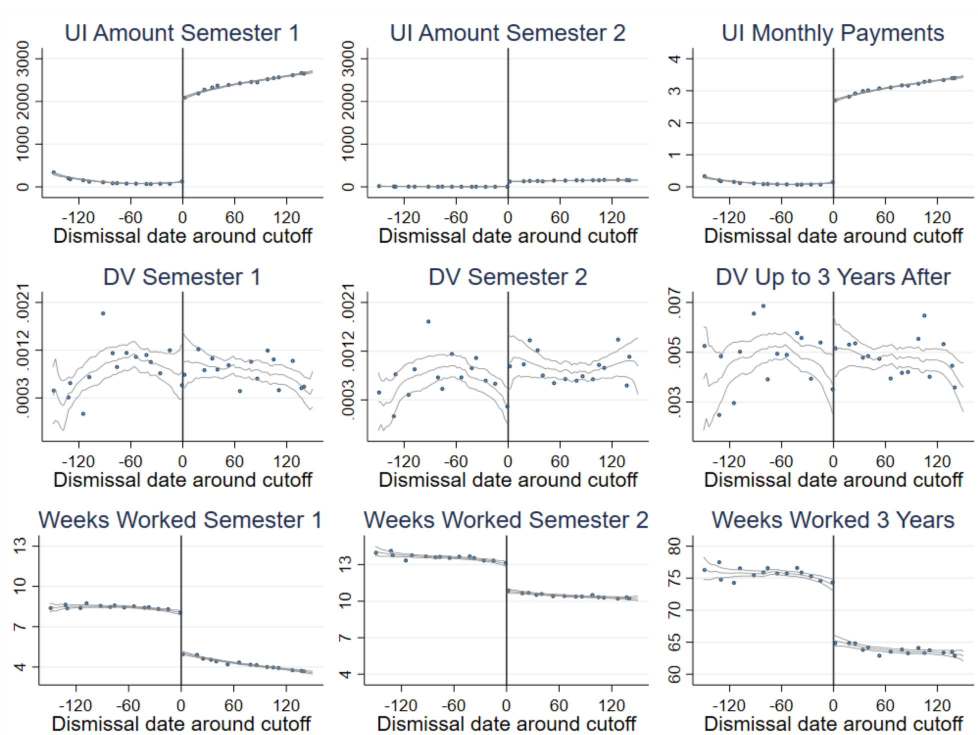
[56] Workers who are initially displaced close to the last day of the month are more likely to be dismissed again on the last day of any month (including the 16-month eligibility cutoff). For instance, a worker dismissed on January 1^{st} 2010 will be able to claim benefits again if dismissed from April 30^{st} 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 30^{st} 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. Evidence showing such patterns is available upon request.

domly assigned near the cutoff. In any event, we show in Appendix D.1 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: 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.

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).

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 D1), to permutation tests where we compare our estimates to those at placebo cutoffs (Appendix Figure D3), and to adjusting for cyclicality in hiring and firing (Appendix Table D2). 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.

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 employment (see, among others, Katz and Meyer, 1990; Card et al., 2007; Lalive, 2008; Gerard and Gonzaga, 2021).

In Appendix Table D3, 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.[57] Appendix Table D4 shows that the results remain extremely similar to our baseline results in Table 3, Panel B.

7 Discussion on mechanisms

We now discuss the mechanisms driving DV in light of our theoretical model and the results obtained in the job loss and UI analyses. Our main result from Section 5 shows that both male and female job loss leads to higher domestic violence. Our theoretical model shows that such increase can be explained by either the income or exposure mechanisms, or both, see Proposition 1, Section 2. In Section 5.3, we provided evidence supporting the idea that the income mechanism plays a role in explaining higher DV after layoff. Namely, we show that job loss effects on DV are substantially smaller for workers receiving larger severance payments at displacement.

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

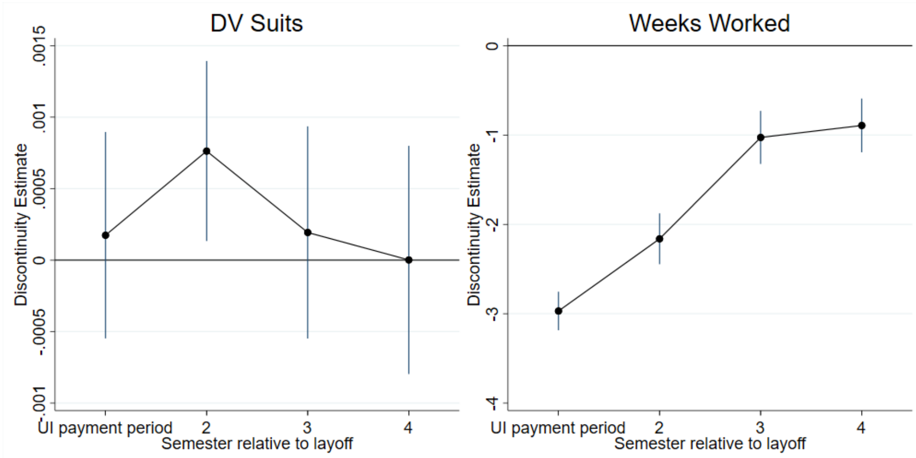
Finally, our findings in Section 6 that UI eligibility has (i) a null impact on DV during the benefit period, and (ii) a positive impact on DV risk after benefits expire, are consistent with both the income and exposure mechanisms – see Proposition 2, Section 2. The null impact during the benefit period can be explained by the income and exposure mechanisms offsetting each other while benefits are paid out.[58] After UI transfers cease, DV risk increases because of the persistent impacts of UI on employment, 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).[59] 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.

[58] 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.

[59] 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 proposed in our simple model 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.[60] 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 these models. For instance, the household bargaining model would predict higher bargaining power among 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.

8 Conclusions

Domestic violence imposes substantial costs on women, society and the next generation. It creates physical and mental health problems, reduces productivity among

[60]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.

women, and has further adverse consequences for their children (Aizer, 2010, 2011; Currie et al., 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.

Data Availability Statement

Most parts of the data used in this article consists of restricted access records from Brazil, which cannot be shared publicly. A replication package including codes, instructions on how to access the restricted access data, and the part of the data that can be shared publicly, are available on Zenodo at https://doi.org/10.5281/zenodo.14200854.

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