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Job displacement, unemployment benefits and domestic violence

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Job Displacement, Unemployment Benefits and Domestic Violence*

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Abstract

We estimate impacts of male job loss, female job loss, and male unemployment benefits on domestic violence in Brazil. We merge employer-employee and social welfare registers with administrative data on domestic violence cases brought to criminal courts, use of public shelters by victims and mandatory notifications of domestic violence by health providers. Leveraging mass layoffs for identification, we find that both male and female job loss, independently, lead to large and pervasive increases in domestic violence. Exploiting a discontinuity in unemployment insurance eligibility, we find that eligible men are not less likely to commit domestic violence while benefits are being paid, and more likely to commit it once benefits expire. Our findings are consistent with job loss increasing domestic violence on account of a negative income shock and an increase in exposure of victims to perpetrators, with unemployment benefits partially offsetting the income shock while reinforcing the exposure shock.

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

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1 Introduction

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

In this paper, we study how economic shocks and policies influence DV, exploiting administrative data on the universe of DV cases brought to Brazilian courts in 2009-2018, a total of about 2.4 million cases. We link these data to longitudinal employer-employee registers containing a total of 100 million workers and 60 million yearly employment spells. We complement the analysis with additional DV measures based on the use of DV public shelters and mandatory DV reports by health providers; and information on couples for a subset of our main dataset. We address two main questions. First, we estimate dynamic treatment effects of male job loss on DV perpetration *and* of female job loss on DV victimization, leveraging mass layoffs for identification. Second, we examine whether unemployment insurance (UI) attenuates any effects of job loss, leveraging plausibly exogenous variation in UI eligibility in a clean regression discontinuity (RD) design.

Estimating the impacts of male job loss, female job loss, and unemployment benefits in the same setting places us in a good position to examine potential mechanisms. In particular, we focus on two main mechanisms. First, the negative income shock brought by job loss may trigger stress and re-negotiation of a shrunken household budget, opening the door for conflict (Clark et al., 2008; Buller et al., 2018)[1]. Second, job loss constitutes a positive time shock, increasing women's exposure to DV risk as displaced workers spend more time at home (Dugan, Nagin and Rosenfeld, 2003). This mechanism may be particularly relevant during the stressful period following job loss. An association of exposure with DV is suggested by evidence that DV escalates during national holidays, weekends and nights, when families spend more time together (Vazquez, Stohr and Purkiss, 2005).

[1] Clark et al. (2008) show that, among a range of negative shocks including bereavement and divorce, job loss stands out as causing persistent unhappiness. The idea that stress may lead to DV is also in line with the evidence in Card and Dahl (2011).

Both the income and time shock mechanisms predict an increase in DV following job loss. This is what we find: male and female job loss each, independently, lead to a sharp and persistent increase in DV. In particular, male job loss leads to a 32% increase in the risk of perpetration, and female job loss to a 56% increase in the risk of victimization.[2] These effects are pervasive along the distribution of perpetrator age, education and baseline income, and also across area-level characteristics including baseline DV rates, the gender pay gap, population size, GDP per capita, and the labor informality rate. They are evident among first-time as well as repeat perpetrators (and victims). These patterns line up with persistent employment and labor income losses following male and female job loss, which we also document.

Within this framework, unemployment benefits constitute a positive shock to income and a positive shock to time. In particular, benefits lead to greater DV exposure by reducing labor supply and lengthening unemployment duration (Katz and Meyer, 1990; Card, Chetty and Weber, 2007; Lalive, 2008). While the income effects of benefits will tend to reduce DV by mitigating the income drop brought by job loss, this may be counter-balanced by increased exposure to DV. As a result, the overall impact of unemployment benefits on DV will depend upon the relative size of the income and exposure effects. We find that unemployment benefits have no impact on DV while they are being paid out, and a perverse impact, increasing DV, once benefits expire. These findings are consistent with the income shock offsetting higher exposure as long as benefits are being received, and with the exposure mechanism prevailing after benefits cease.[3] Supporting evidence of the role of cash at displacement is garnered by leveraging the fact that mandatory severance pay is increasing in tenure, and showing that job loss has no effect on DV for high tenure workers.[4] Summarizing,

[2]Our main estimates for male and female job loss are based on somewhat different samples due to data constraints explained in Section 3. We formally test for differences in magnitudes between these estimates, using a homogeneous samples across geographical areas, and cannot reject effects of the same magnitude. 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.

[3]The fact that the UI income effects fade once benefits cease is in line with evidence that unemployed workers appear not to smooth their consumption, and exhibit large consumption drops upon benefit expiration. See Ganong and Noel (2019); Gerard and Naritomi (2021).

[4]This pattern is robust to including flexible controls accounting for differences in the impacts of job loss on DV by age, income and education. The tenure coefficients exhibit limited sensitivity to these controls, making it less likely that unobservables correlated with tenure drive the pattern. We show that it is not the case that employment recovers more readily for high tenure workers, which

our results suggest that job loss increases the risk of DV, and 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 challenges to the causal interpretation of these results. A first order challenge is endogenous reporting. It seems plausible that women are less likely to report DV when they lose their jobs, but more likely to report it when the man loses his job. 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 court cases initiated by *in flagrante* arrests (i.e., when the offender is caught "red-handed"). We also show that the results hold for use of public shelters by women, and mandatory notifications of DV cases by public and private 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.[5] In addition, *in flagrante* arrests and health provider notifications of DV are not subject to time lags between the date of DV and its prosecution, allowing us to measure more precisely the timing of violence and to better inspect pre-trends.

Another key challenge to identification is dynamic selection into job loss. We show that our estimates remain virtually identical when varying the definition of mass layoffs to the limiting case of plant closures, and when adopting an intention-to-treat approach that defines as treated all workers in mass layoff firms (including the non-displaced) so to avoid any potential selection into job loss, even within plausibly exogenous mass layoffs. Our baseline empirical strategy compares the probability of DV between workers displaced in mass layoffs and non-displaced workers that are exactly matched on a wide array of observable worker, firm and area characteristics. We show that the estimates are also robust to including fine-grained fixed effects for the interaction of municipality, 2-digit industry, and time fixed-effects which demonstrates that the control group defined via exact matching flexibly captures local and industry-specific shocks.

makes it unlikely that differences in exposure explain the observed patterns. Thus the evidence points to liquidity differences.

[5] We also exploit information on the severity of DV offenses, as higher willingness to report should result in less severe offenses being reported more often at the margin.

To support our analysis of UI eligibility, we provide extensive evidence that displaced workers are as-good-as-randomly assigned near the RD cutoff, and that the discontinuity estimates are robust to varying bandwidths, polynomial specifications, permutation tests and falsification analysis based on pre-displacement DV suits.

We investigate a number of other concerns, including missing information on the identity of suspected offenders and victims; flows of displaced workers into the informal economy; and estimation issues arising in staggered difference-in-differences models (discussed, among others, by Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). In our main analysis sample, we study male and female job loss in isolation. However we show that our main results hold in a subsample where we can link couples.[6] Using this sample, we also show that the chances of couple separation are not strongly affected by male or female job loss.

Our approach marks a sharp departure from existing research on DV which has primarily focused on *relative*, *potential* income, and where the modal theme has been the empowerment of women. We provide the first estimates of impacts of individual job loss by men and women on DV, and the first estimates of the potential for unemployment benefits to mitigate such effects. In doing this, we shift the focus to *absolute*, *realized* income shocks, and we additionally highlight the relevance of disposable time. In addition, we investigate reporting bias more carefully than many existing studies of DV.

Our finding that job loss of men and women leads to higher DV cannot be explained by existing theoretical constructs in the DV literature, which predict opposite signs on male and female unemployment rates. In the household bargaining model, bargaining power is determined by outside options, and DV is increasing in the relative power of the man and decreasing in the relative power of the woman. Investigating movements in area-level unemployment rates in the UK, (Anderberg et al., 2016) finds evidence consistent with this- increases in male unemployment rates lower DV, while increases in female rates increase it. Similarly, Aizer (2010) finds that improvements in area-level relative wages of women in the United States lower DV. An alternative construct, the male backlash model (Macmillan and Gartner, 1999) generates the reverse predictions on the premise that female employment primes male (breadwinner) identity and that this triggers violence. Consistent with this, a number of

[6] 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.

studies reveal an increase in DV following improvements in labor market opportunities for women (Tur-Prats, 2019; Bhalotra et al., 2019; Erten and Keskin, 2020). Other hypotheses broadly consistent with such patterns are the instrumental control and sabotage models, whereby men commit violence to extract resources from women or sabotage their careers, respectively (Bloch and Rao, 2002; Anderberg and Rainer, 2013).

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. The mechanisms highlighted in existing research, related to outside opportunities modifying relative power within couples, may play a second-order role in our setting, possibly influencing the relative magnitudes of the impact of male and female job loss on DV, but they cannot explain the first-order patterns. 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.

Outside the cited analyses of regional unemployment rates, the DV literature overwhelmingly focuses on interventions designed to empower women, through cash transfers, microcredit, skills training, or job assignment (Angelucci, 2008; Bobonis, Gonzalez-Brenes and Castro, 2013; Hidrobo and Fernald, 2013; Luke and Munshi, 2011; Heath, 2014; Kotsadam and Villanger, 2020). We draw attention to the importance of shocks to total household income, and depart from the exclusive focus on women by considering also economic shocks to men. Another recent study that considers economic shocks to men and women is Haushofer et al. (2019), who analyse one time cash transfers to women and men in Kenya in a controlled experiment.[7]

[7]Consistent with our findings, they find a reduction in DV in both cases. The job loss shock differs from their cash transfer experiment in several aspects. First, job loss causes a negative income shock and this can have different impacts from a positive cash transfer due to loss aversion or liquidity constraints; second, job loss may lower self-esteem which can compound income-related stress; third, job loss generates a direct time shock; and, fourth, it is arguably a more widespread routine phenomenon of general interest than income windfalls through one time cash transfers. Unemployment benefits are also not comparable to cash transfers to the extent that they lengthen

Our findings are consistent with and also contribute to a literature documenting the often dramatic impacts of individual job loss on people's lives. The mechanisms we highlight are in line with studies showing that job loss results in mental health problems (Kuhn, Lalive and Zweimüller, 2009; Charles and DeCicca, 2008; Zimmer, 2021; Zimmerman, 2006), substance abuse (Black, Devereux and Salvanes, 2015), premature mortality (Sullivan and Von Wachter, 2009) and divorce (Charles and Stephens, 2004; Eliason, 2012).[8]

Two studies analyzing crime more generally are related to ours, while differing in important dimensions. Britto, Pinotti and Sampaio (2020) investigate the impact of job loss and unemployment benefits for men on crimes other than DV employing the judicial data used in this paper. Rose (2018) provides a similar analysis, including DV among other crimes but using a sample of ex-inmates in the state of Washington, in contrast to which we use data on the universe of displaced workers from a large and heterogeneous country. Unlike most other crimes, DV emerges within the household where incomes are often shared and negotiated, and cohabitation increases exposure. The interdependence of the couple leads to reporting bias being a much more severe concern for DV than for other types of crime, for which reason we complement the analysis of judicial suits with several alternative DV measures which give us confidence that our key finding that male and female job loss both increase DV levels is not simply an artifact of higher reporting. Other important differences are that the crime literature focuses on perpetration while we additionally study impacts of female job loss on DV victimization, and that we investigate the specific mechanisms driving DV, demonstrating that existing theoretical constructs in the DV domain cannot explain impacts of individual job loss. We uncover an additional pattern which underlines that DV is driven by different mechanisms which is that UI eligibility does not lead to lower DV, in contrast to the case for other crimes (Britto, Pinotti and Sampaio, 2020; Rose, 2018).

Overall, we demonstrate the pernicious impact of job loss, whether suffered by men or women, on domestic violence. Our estimates suggest that unemployment benefits can mitigate if accompanied by policies that mandate or incentivize a return to work. Understanding the mechanisms at play and identifying mitigating policies is

unemployment duration by directly incentivizing lower job search.

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

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).

The remainder of this paper is organized as follows. Section 2 provides background information, Section 3 describes the data, Section 4 presents results for male and female job loss, and Section 5 investigates mitigation with unemployment benefits. Section 6 concludes.

2 Context and Institutions

Domestic violence and criminal justice in Brazil. The Gender, Institutions and Development report (OECD, 2019) documents that one third of women in Brazil are subject to violence during their lifetime. 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.

Domestic violence is a criminal offence that falls under the jurisdiction of 27 state courts, composed of a total of 2,697 tribunals having jurisdiction over one or more of Brazil's 5,570 municipalities. The state judiciary police handles DV investigations, usually initiated by a victim report, though it 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. As many as 93% of all contracts in the private sector are open-ended, full-time contracts. We analyze layoffs without a just cause, which account for 65% of all separations (the rest are mainly voluntary quits). All workers are entitled to a mandatory savings account financed by the employer through monthly contributions equivalent to 8% of the worker's earnings. Only when dismissed without just cause, workers can access these funds and are further entitled to a severance payment equivalent to 40% of the account's balance.[9] Summing over these two sources, workers receive approximately 1.34 monthly wages for each year of tenure at the time of layoff.

Workers in the formal sector that are dismissed without a just cause may be eligible for unemployment insurance. There are two main conditions for eligibility: first, at least six months of continuous employment; second, at least 16 months between layoff dates for subsequent UI claims. Unemployment benefits last three to five months, with a replacement rate of 100% for workers earning the minimum wage, which decreases smoothly to 67% at the benefit cap (2.65 times the minimum wage).[10] Once unemployment benefits expire, the only income support at the national level is "Bolsa Família", a conditional means-tested cash transfer targeted at very poor families. In 2019, the average transfer per household was 16% of the minimum wage and the maximum per capita family income for eligibility was less than one-fifth of the minimum wage.

Our description so far refers to formal jobs. However, Brazil has a large informal sector, accounting for roughly 45% of all jobs in the analysis period. Job turnover is high in both the formal and informal sector, workers tend to move frequently between the two and 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 sector work contributes to the recovery of employment and earnings after job loss, and show that it is relatively small. In any case, failing to account for transitions of displaced workers to the informal sector implies that we under-estimate the elasticity of DV to labor income upon job loss.

Preliminary evidence. Figure 1 plots the adult employment rate and the female homicide rate, the best available indicator for DV over long periods, for 1990-2018. Feminicide rates increased substantially from 3 homicides per 100k women in 1990 to 4.3 in 2018. Notably, this increase coincides with a worsening in labor market conditions during the period. Of course, these patterns in aggregate data may reflect

[9]A few exceptional cases allow workers to withdraw from the account prior to displacement – e.g., severe illness or when buying their first house.

[10]The duration of unemployment benefits depends on the number of months worked in the three years before dismissal:(6-11, 12-23 and 24+ months of work allow for up to 3, 4 and 5 months of benefits, respectively.

trends in other omitted factors. We next describe the detailed individual-level data that we will use to identify causal effects.

3 Data

The main analysis relies on individual, longitudinal data derived by linking court and employment registers. We describe each data source and, then, the linking procedure.

Judicial registers. We use the universe of DV cases filed in all first-degree courts during 2009-18. These include information on the start and end date of the judicial case, court location, subjects being discussed, and full names of the defendant and plaintiff.[11] In total, there are 2.4 million DV cases (11% of all criminal prosecutions), 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 focusing on victims, we only use data on protective measures, for which we observe the plaintiff name in 244,000 out of 1.17 million cases, while plaintiff names are missing in virtually all DV prosecutions. Missingness arises for two reasons that we cannot distinguish in the data: imprecision in the process of inputting data from court diaries; and judicial secrecy, which tends to protect the victim's identity.

Missing data challenge identification only if they are related to the job status of the defendant or the plaintiff. We argue that this is unlikely to be the case, for at least three reasons. First, 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. Second, the threat of dismissal is not a valid legal motive for invoking secrecy. Finally, the share of missing names varies considerably across jurisdictions, indicating that a large portion of the variation is driven by court-specific factors. In the main analysis, we drop areas where the share of missing names is greater than 90%. In Section 4.4, we provide several robustness tests showing that our findings are robust to the potential bias arising from missing names in several ways. Specifically, we show our main estimates remain similar when restricting the sample to jurisdictions where the share of missing names is quantitatively irrelevant; and we show that using alternative measures of DV which do not suffer from missing

[11] We obtained these data from Kurier, 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.

data limitations lead to similar patterns as in the main analysis – namely, public shelter use and DV notifications by health providers.

Employment registers. We use linked employer-employee data for 2009-2018 covering the universe of formal workers and firms in Brazil (RAIS), made available by the Ministry of Labor. Workers are identified by a unique tax code identifier (CPF) and their full name. Since employers must provide workers with notice of dismissal at least 30 days in advance – each completed year of tenure extending notice by 3 days –, we define the timing of layoff as the official layoff date stated in RAIS minus 30 days. This 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.

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. We restrict our sample to individuals with unique names in the country – about half of the adult population. We identify this subpopulation 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. 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) dimension, the standardized difference remaining below 0.25 for all variables (see Appendix Table A1). In any event, we will assess the sensitivity of our results by retaining all individuals with a unique name within the state (rather than the country), which extends coverage to 70% of the population.

Household, public shelter and health systems data Although we cannot link couples or families in our main panel covering employment and judicial records, we are able to do so for a subsample of our data. Specifically, we use data on household composition from CadUnico – a restricted access social registry maintained by the Federal government for administering welfare programs such as Bolsa Familia. Due to the nature of the social registry, it overlaps with the lower 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 4.4).

4 Job Loss and Domestic Violence

4.1 Descriptive evidence

The upper panel of Figure 2 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 from the latter. The graphs in the lower panel of Figure 2 show that the probability of DV upon job loss is decreasing in job tenure, an association that we will investigate further. These graphs also illustrate the very high turnover rate in the Brazilian labor market.

4.2 Identification strategy

We use a difference-in-differences strategy where we define as treated all workers displaced in mass layoffs between 2012 and 2014 – the central years within our sample period, 2009-2018. Therefore, we estimate dynamic treatment effects for up to four years after displacement, and anticipation effects up to three years before displacement, using a perfectly balanced panel.

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 industrial sector (9), and state (27). In cases where a treated worker is matched with multiple controls, one is randomly selected. We investigate the sensitivity of the results to redefining matched controls as workers who are continuously employed through the entire post-treatment period.[12] Over 80% of displaced workers are successfully matched to a control, who

[12]Previous papers have used both approaches. For instance, Ichino et al. (2017) and Schmieder,

receives a placebo dismissal date equal to the layoff date of the matched treated worker. We compare outcomes between treated and control workers, respectively, using the following difference-in-differences equation:

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

Workers are identified by subscript i, and $Treat_i$ is an indicator for being displaced in a mass layoff. Dummy variables $Time_t$ identify years since layoff. Time units are defined precisely using the exact date of layoffs and DV prosecutions or PM. 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. Time-varying shocks are absorbed by including a full set of period fixed effects, $Time_t$. The inclusion of (a large share of) never-treated workers as controls addresses concerns regarding the estimation of two-way fixed-effects models with staggered treatment across units (we will show the diagnostics recommended by de Chaisemartin and D'Haultfœuille, 2020).

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

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

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

Although the difference-in-differences design does not strictly require the treatment and control groups to be the same in levels, Table 1 shows that treated and (matched) control workers are fairly balanced on observable characteristics. The standardized difference between the two groups is below the threshold of 0.25 for almost 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 being education in the male worker sample.

The main challenge to identification is dynamic selection into displacement. Parallel trends between treated and control workers in the pre-treatment period attenuate

von Wachter and Bender (2018) define the control group similarly to our baseline setting, while Jacobson, LaLonde and Sullivan (1993) and Couch and Placzek (2010) restrict controls to be workers who are continuously employed throughout the period.

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 such concern, as mass-layoffs depend on firm-level shocks rather than on the behavior of displaced workers (see e.g. Gathmann, Helm and Schönberg, 2020). 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.[13] We extensively assess the sensitivity of results to changes in the definition of mass layoffs and to other robustness tests for selection (see Section 4.5).

4.3 Dynamic treatment effects of male and female job loss

We first discuss the effects of job loss on labor market careers. Figure 3 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. In the first year after male layoff, labor income is 70% lower than the baseline level before layoff. Starting in the second year after layoff, there is a continuous but slow recovery, with labor income four years after layoff being still 36% lower for treated workers compared to controls. The estimates are remarkably similar for women, as shown in the right panel of Figure 3. In Appendix B.1, we show that job loss also has an adverse and persistent impact on employment, monthly wages, and job turnover. In Appendix B.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. Therefore, our estimates of elasticities of DV to formal sector income will (slightly) underestimate elasticities to total income.

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 4, layoff leads to 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 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

[13] This definition is similar to Jacobson, LaLonde and Sullivan (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.

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 4 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 (30%), 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 3). In Appendix B.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).

In Appendix B.4, we investigate the fact that the job loss effect on DV remains positive and sizable several years after the layoff. Although recurrent offences partially explain persistence – a fourth of all perpetrators are charged more than once over the ten year period covered by our sample – we also observe a sustained job loss effect on the probability of first offenses following both male and female layoffs. This is important because once initiated DV tends to persist within couples. The observed patterns are consistent with the sustained labor market losses following displacement.

4.4 Reporting bias

There is widespread under-reporting of DV on account of gendered norms, social stigma, concern for children, 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, in which case our estimates of impacts of male job loss on DV prosecutions will be upward biased. However if a woman is less likely to report violence once she loses her job, this will lead to our estimates of the effect of female job loss on DV victimization being downward biased.

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. A higher willingness to report should result in less severe offenses being reported more often at the margin, so male job loss should have a stronger effect on less severe DV cases. On the contrary, Figure 5 shows that male job loss leads to larger increases in DV offenses leading to longer jail times (left graph), and that the effect is pervasive for all types of DV cases (right graph).[14]

Our second strategy is to replicate the analysis using alternative measures of DV that depend less, if at all, on discretion in reporting because they are reported by third parties. First we analyse 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 job loss on this restricted subset of cases is virtually identical to the baseline estimate including all DV cases (see Figure B4 in Appendix B.5.A). Second, we study women's use of public shelters for DV victims, available from the social welfare register for 2011-2013. This is less prone to reporting bias because, unlike judicial prosecutions, it does not directly implicate the male partner. Table B2 in Appendix B.5.A shows that male and female job loss increase the use of DV shelters by the female partner by 24% and 46%, respectively.

We use a third measure of DV drawn from mandatory reports by the health system, available for 2010-2017. All public and private health units in Brazil must file a DV notification in the Sistema de Informação de Agravos de Notificação (SINAN) system when they suspect or know that their patients are victims of domestic violence. 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 which include only more severe cases-a measure previously used in the DV literature, e.g. by Aizer, 2010). Moreover, these notifications remain within the health system (i.e., they 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 B.5.B, we describe the data linkage procedure we use to match DV notifications to data on job losers and matched control workers on (clusters of) exact birth date, municipality, and gender. Since this procedure necessarily entails some degree of measurement error, we extend the mass layoff sample to workers displaced in the period 2012-16, thus tracking SINAN reports for only two years before and after job loss, in order to increase statistical power. Importantly, we validate our data linkage procedure by replicating the estimation of employment

[14] In the left graph of Figure 5, 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.

effects of job loss. In other words, we use the same procedure to match employment outcomes, as if we did not have unique individual identifiers. The results are virtually identical to the baseline employment estimates obtained with linkage based on unique individual identifiers (see Appendix Figure B5).

The results for DV notifications are presented in Figure 6. They confirm our main finding that both male and female job loss lead to an increase in DV. Appendix Table B3 shows that these results are robust to using different maximum cluster sizes (columns 1-3); interacted municipality, time and birth period fixed-effects (columns 4-5); and more stringent mass layoffs definitions (columns 6-7). The relative effect on DV is larger than the baseline estimate, though computing baseline rates is complicated because the matching procedure does not uniquely identify individuals.[15]

Overall, results using three 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 changes in reporting behavior.

4.5 Robustness

In Appendix B.6, we assess the sensitivity of our baseline estimates to additional robustness checks. First, estimates using quarterly data provide further support for the hypothesis of common pre-trends (Appendix B.6.A).[16] Second, results are robust to the inclusion of fine grained municipality-industry-year fixed effects, indicating that using individual matched controls finely absorbs area level shocks; and the results are similar when extending the sample coverage to workers with a unique name in the state of work rather than in the entire country (Appendix B.6.B). Third, we address potential selection within mass layoffs by showing that the results remain robust when using stricter mass layoff definitions and plant closures. 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 B.6.C).

[15]Taking the mean over the outcome in the estimation sample would inflate baseline rates as it would include DV notifications for several individuals. For this reason, we compute the mean for the 13% of workers who are uniquely identified by the characteristics we use to merge the SINAN data.

[16]The results for alternative measures of DV discussed in previous Section 4.4 – 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 protective measures, which we use in our baseline estimates of the effect of female job loss on victimization (see Section 3).

Fourth, our results are essentially unaffected when progressively restricting the data to jurisdictions with a smaller share of missing names in the court data. In fact, our findings remain robust even when focusing exclusively on jurisdictions where such issues are not quantitatively relevant (Appendix B.6.D). Finally, we deal with the identification issues arising in two-way fixed-effects models with staggered treatment (see,e.g., Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). On account of the inclusion of a large share of "pure controls" not experiencing mass layoffs in the entire period over which they are matched to a treated worker, our difference-in-differences coefficient averages heterogeneous treatment effects with positive weights for all cross-sectional units (Appendix B.6.E).

So far, we have studied in isolation the effects of male and female job loss on domestic violence. Not all DV cases refer to domestic violence within couples- they may include cases between non-cohabiting couples, ex-partners, and non-partners. In Appendix B.7, we show that our main findings hold within couples in the subsample of workers present in the welfare register, *CadUnico*, as this allows us to identify cohabiting partners. This is relevant as theoretical models of DV are conceptualized for couples. We also show that male job loss does not influence the probability that the couple stays together, while female job loss has a small impact. We 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.

4.6 Mechanisms and relation to previous work

Before we move on to consider mechanisms and potential policy approaches, we pause here to take stock of how the results so far relate to the existing literature. The estimated effects of male and female job loss in Table 2 cannot be explained by common theoretical constructs in the DV literature. The household bargaining model predicts that male job loss decreases male bargaining power leading to lower DV, but we find higher DV (though our results are in line with predictions of the bargaining model for female job loss). The male backlash, instrumental control and sabotage models referenced earlier both predict that female job loss leads to lower DV, but we find that it leads to higher DV. These models do not incorporate the mechanisms that we propose as the drivers of our findings, namely the stress of a substantial income shock, and increased exposure (or opportunities for crime) at a stressful time. Our results are not directly comparable to results from studies testing these models using area level unemployment or wage shocks on DV, which tend to find opposite effects of male and female labor market conditions on domestic violence (Anderberg et al., 2016; Aizer, 2010; Tur-Prats, 2019; Bhalotra et al., 2019; Erten and Keskin, 2020). The reason is that variations in area-level unemployment rates represent a change in the risk of unemployment for most people, with only a small share of individuals actually being displaced. In contrast, we estimate the impact of realized unemployment shocks, while controlling for local labor market conditions. We compare job losers to similar (matched) workers in the same local labor market, and we control flexibly for shocks at the municipality-industry-year level. Importantly, the income and exposure mechanisms that we suggest drive our results are only triggered upon actual job loss.

4.7 Heterogeneity by Worker and Area Characteristics

We now investigate how impacts of job loss on DV vary by worker characteristics, namely age, education, income and tenure. 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 the worker characteristics are correlated with one another, we estimate models in which all coefficients in the equation (2) are interacted with third-order polynomial controls on other individual-level characteristics.

The first striking pattern in Figure 7 is that DV following male job loss is remarkably pervasive, being evident across the entire distributions of age, income and education. While there is a positive association between poverty and DV, the impact of male job loss on DV is not larger in households that have lower income at baseline. In Appendix Figure B7, we also show that the effect is 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 addition, we show that the impact does not strongly vary with the number of displaced co-workers in the same mass layoff, suggesting that mass layoff effects are not a special case in comparison to regular layoffs. This is in line with the evidence in Appendix Tables B4, Panel D, showing similar impacts when using plant closures.

To provide evidence on the income mechanism, we compare workers displaced with different tenure, exploiting the fact that severance pay is increasing in tenure. We compare workers displaced with 3 or more years in the job – who receive on average of

7 months wages in severance pay – to those displaced with 6-36 months tenure – who receive on average less than 2 months wages.[17] The left panel in Figure 8 shows (on the right vertical axis) large differences in access to liquidity on account of severance pay between the two tenure groups, which have similar access to unemployment benefits.[18] The same figure shows that job loss raises DV for workers with 6-36 months tenure but that it has no impact on DV among high tenure workers, suggesting that liquidity at displacement may be a mechanism driving DV. This pattern is displayed again in the right panel of Figure 8 which shows dynamic effects by tenure.

Differential effects by tenure are not capturing differential effects by age, education, or income, as our specification controls flexibly for gradients in these characteristics. Indeed, the left panel of Figure 8 shows that estimated effects by tenure are very similar when we control for interactions of job loss with these worker characteristics. Robustness of the coefficients to controls for observables makes it less likely that unobservables correlated with tenure drive our results. The null effect on DV among high-tenure workers is also not a result of these workers finding work more readily than low tenure workers, see the centre panel of Figure 8 which shows that impacts of job loss on months worked do not vary significantly by tenure. This also rules out time availability (or opportunities for violence on the part of men) as an explanation of the tenure gradient. The most likely explanation is that tenure proxies liquidity at displacement, and that liquidity ameliorates the impact of job loss on DV. That low-tenure workers in Brazil face tighter liquidity constraints than high-tenure workers is consistent with evidence from Brazil that consumption losses following layoff are decreasing in tenure (Gerard and Naritomi, 2021) and that job search is sensitive to cash on hand only among low-tenure workers (Britto, 2020). In the next section, we bolster this evidence on mechanisms using quasi-experimental variation in access to unemployment benefits.

[17]We exclude workers displaced with less than 6 months in the job who are not eligible for UI, so that our comparison captures differences in severance pay rather than unemployment benefits. We analyse UI impacts in Section 5.

[18]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 2.) Although these sums do not show directly in the data, they are estimated based on tenure and earnings.

5 Do Unemployment Benefits Mitigate Impacts of Job Loss on Domestic Violence?

We now investigate whether unemployment benefits mitigate the impact of male job loss on DV.[19] As already discussed, job loss delivers a negative income shock, but increases potential time with the partner, implying a higher exposure to DV risk. Unemployment benefits mitigate the income shock, but they may increase exposure to DV risk (through increased unemployment duration), so the overall effect is a priori ambiguous.

5.1 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. The maximum benefit duration ranges from 3 to 5 months. For repeat claimants, at least 16 months must have elapsed since their last layoff resulting in a benefit claim. We exploit eligibility rules by retaining workers with at least 6 months tenure and implementing a regression discontinuity (RD) design at the 16-month eligibility cutoff for repeat claimants.[20] 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 the share of workers taking UI benefits, their total amount and duration. The main

[19] 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).

[20] We cannot exploit the 6 month cutoff rule because there is evidence of manipulation around this cutoff. We use data on UI payments to restrict the sample to workers who exhausted all months of UI benefits following the initial displacement. This makes the first-stage around the 16-months cutoff stronger, since workers who did not use the 5 months can claim unused benefits when they do not meet the 16-month requirement.

estimates are based on a local linear model with a narrow bandwidth of 45 days, but we check the sensitivity of our results to a range of bandwidths (including the optimal bandwidth of Calonico, Cattaneo and Titiunik, 2014) and polynomial specifications. We will also perform permutation tests, comparing our estimate at the true cutoff with a distribution of estimates at placebo cutoffs.

5.2 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).[21] We restrict attention to workers displaced during 2009-14 because numerous changes to UI were implemented in 2015. Cyclical peaks in layoffs on the first and last days of the month (see Appendix Figure C1) generate discontinuities in the density of the running variable about every 30 days that are not specific to the 16-month cutoff.[22] 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 C2 shows no evidence of density discontinuity around the 16-month cutoff in this restricted sample, as also confirmed by the McCrary density test (McCrary, 2008) and the bias-robust test developed in Cattaneo, Jansson and Ma (2018, 2020). In addition, Figure C3 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 randomly assigned near the cutoff. In any event, we show in Appendix

[21] Accordingly, we match the employment and judicial registers based on name and the state where the worker and the court are located. Panel B of Table B4 showed robustness of the main results to using the state level restriction.

[22] 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.

C.2 that our main findings remain robust when including workers dismissed on all dates and adding fixed effects for individual-specific cutoff and dismissal dates to control for dismissal cycles. In this specification, the estimates rely upon variation in worker-specific dismissal dates within groups who have the same cutoff date.

5.3 Results and robustness

Liquidity associated with UI eligibility is shown in the upper row of Figure 9. Workers barely meeting the 16-month requirement receive roughly an additional R$2,000 in the first semester after layoff (equivalent to 2.5 UI monthly payments, or 1.5 pre-displacement monthly wages), and close to nothing in the second semester after layoff. Columns (1)-(3) of Table 3, Panel A, quantify these effects; column (4) shows, in addition, that the take up is equal to 57%.

We find that UI does *not* attenuate the increase in DV following job loss, and that it in fact *increases* DV in the second semester after layoff, when benefit payments cease, see the center row in Figure 9. This is confirmed in Table 3, Panel B, which shows a null effect in the first semester and a 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 C1), to permutation tests where we compare our estimates to those at placebo cutoffs (Appendix Figure C4) and to adjusting for cyclicality in hiring and firing (Appendix Table C2). 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 they may, in fact, increase it after benefit expiration. This result contributes to research on the unintended or behavioural impacts of unemployment benefits.

5.4 Discussion and mechanisms

Our finding that UI eligibility increases DV can be explained by the fact that displaced male workers eligible for UI spend on average less time at work in the semesters after layoff, compared to displaced workers that are non-eligible. This implies a potentially longer time with their partners. The bottom row of Figure 9 and Panel D of Table 3 shows that eligible males work 8.6 weeks less in the 3 years after layoff, which is equivalent to a 11.5% reduction over the mean.[23] That UI reduces labor supply is in line with a large literature, see among others, Card, Chetty and Weber (2007); Gerard and Gonzaga (2021); Katz and Meyer (1990); Lalive (2008).[24]

To bolster this argument, Figure 10 compares the dynamics of the effects of UI eligibility on employment and DV suits. Eligibility reduces employment by 2.9 weeks in the first semester after layoff, and the effect remains sizable in the second semester, after which the employment gap between eligible and non-eligible workers narrows considerably. Turning to DV, the positive impact emerges in semester 2, when UI benefits expire, and it vanishes in the third and fourth semester, when the employment gap closes down. The null effect on DV in the first semester after displacement (during which benefits are being paid) suggests that an income effect offsets the effect of increased exposure on DV risk during this period, such that the exposure effect only emerges after benefit expiration. This is consistent with evidence showing that UI beneficiaries fail to smooth consumption, experiencing a sharp consumption drop upon benefit expiration – see Gerard and Naritomi (2021) and Ganong and Noel (2019) for evidence using Brazilian and US data, respectively). That income is a mechanism for DV is line with the evidence in Section 4 showing no job loss effect on DV for high tenure workers, who have great access to liquidity upon displacement.[25]

6 Conclusions

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

[23] We also check that reemployment wages are not affected by UI eligibility, in line with the findings in Gerard and Gonzaga (2021) and Britto (2020).

[24] 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.

[25] Using a range of crimes, Britto, Pinotti and Sampaio (2020) and Rose (2018) find that UI eligibility reduces crime, consistent with the income effect of UI overwhelming the exposure effect of UI in their settings.

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 analyse 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 have a better chance of mitigating impacts of job loss on DV if accompanied by policies including job placement or skills training that facilitate, incentivize or mandate a return to work, differently from our setting, where benefits were unconditional.[26]

Finally, our findings on mechanisms line up well with the remarkable global surge in domestic violence during the Covid-19 pandemic, as the latter is plausibly the result of income losses brought by widespread job loss and lockdown policies which reinforce the exposure effects of job loss.

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[26]Income appears to mitigate, as does reducing exposure. Thus creating work opportunities even if they are in community work or for charitable causes could help.

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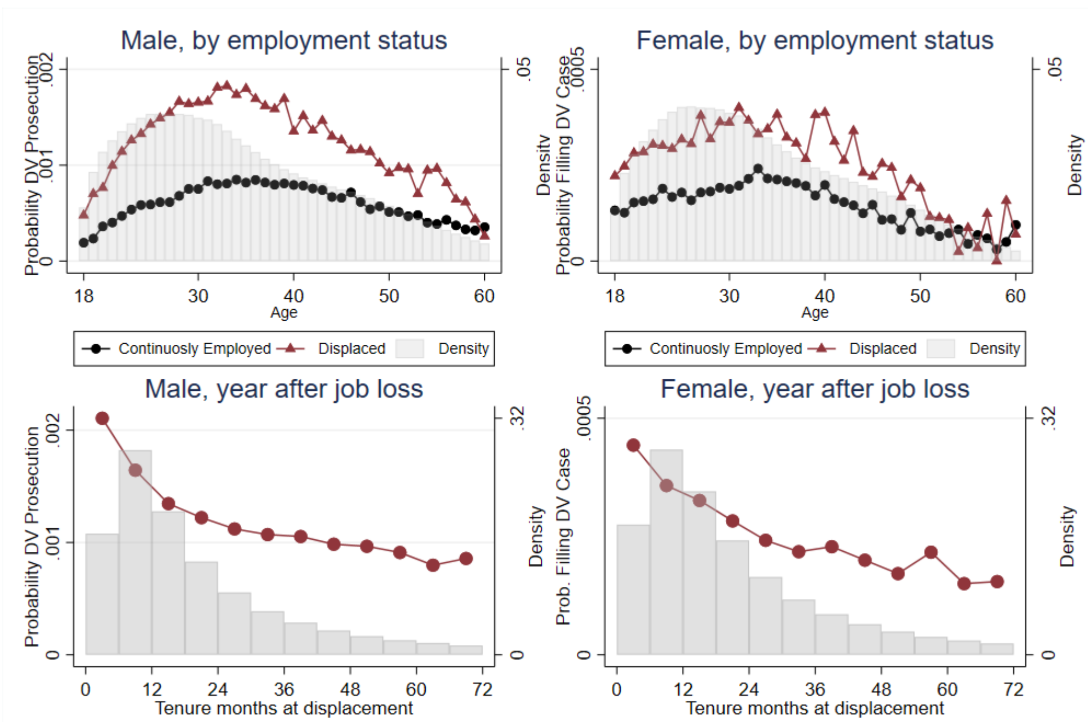
Figures

Figure 1: Trends in Femicide and The Employment Rate in Brazil

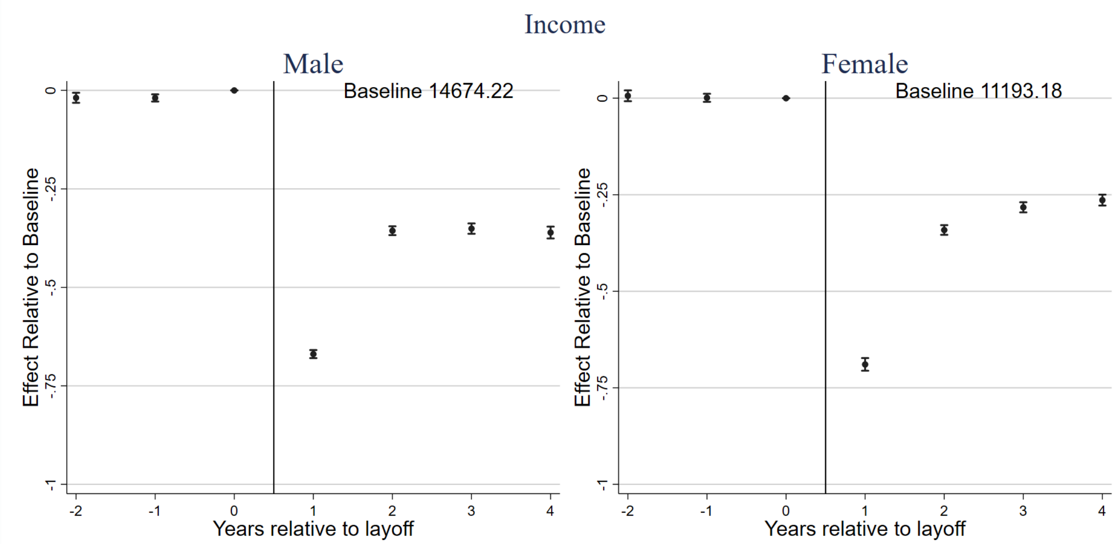
Notes The graph shows the evolution of the feminicide rate per 100,000 inhabitants (left vertical axis) and the employment rate (right vertical axis) in Brazil over the 1990-2018 period.

Figure 2: Domestic violence by employment status, age and tenure



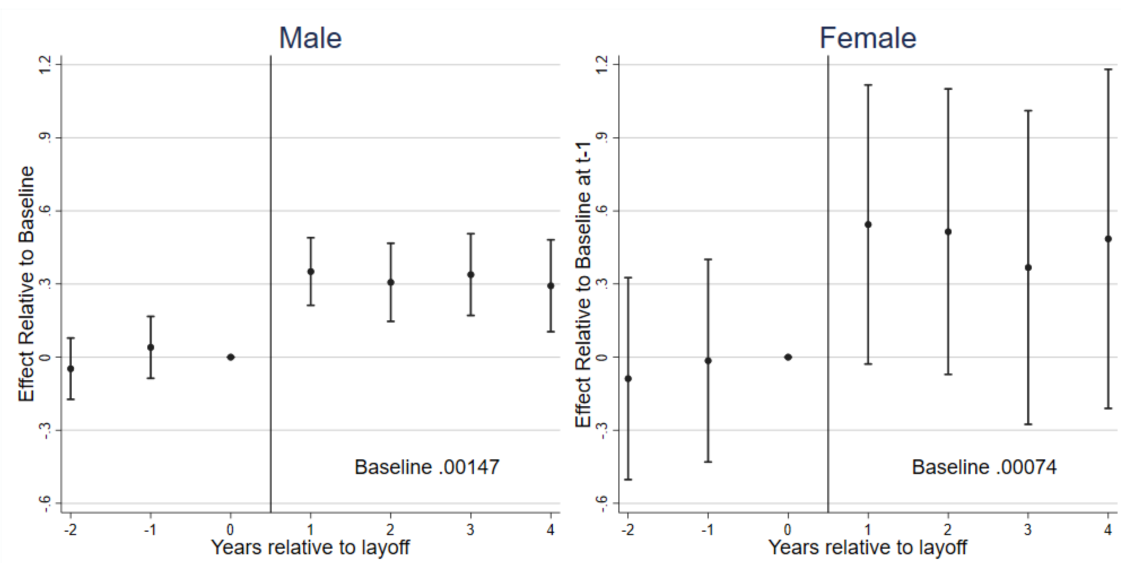
Notes The top graphs compare the yearly probability of DV perpetration in DV suits for men and DV victimization in protective measures for women, comparing workers that are continuously employed to workers losing their job in each year by age. The bottom graphs present the same measures for job losers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

Figure 3: The effect of job loss on labor income



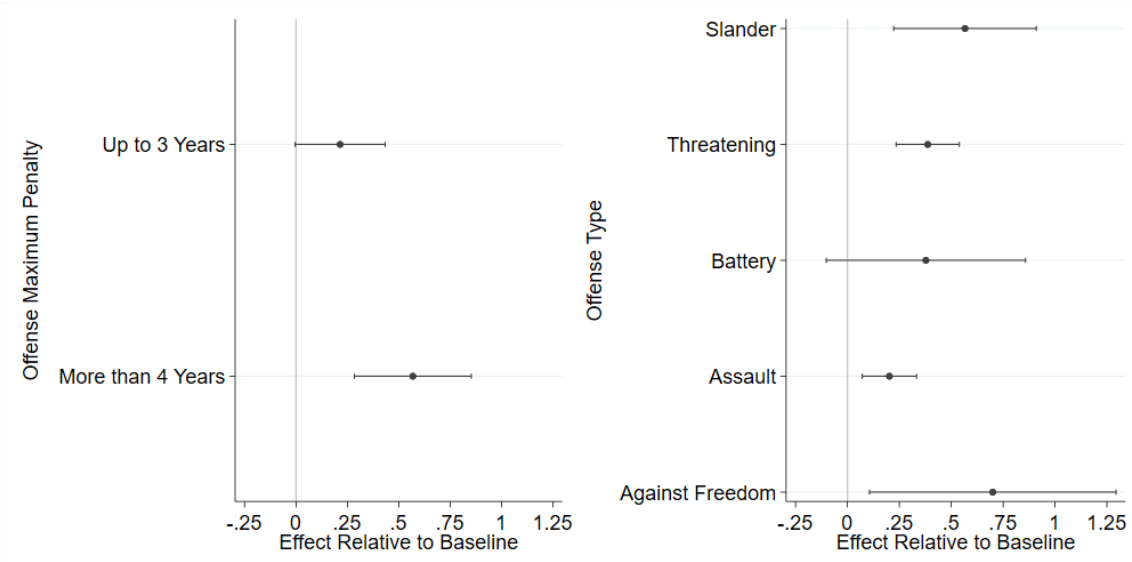
Notes This figure shows the effect of job loss on formal labor income by gender, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers 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.

Figure 4: The effect of male and female job loss on domestic violence, judicial suits



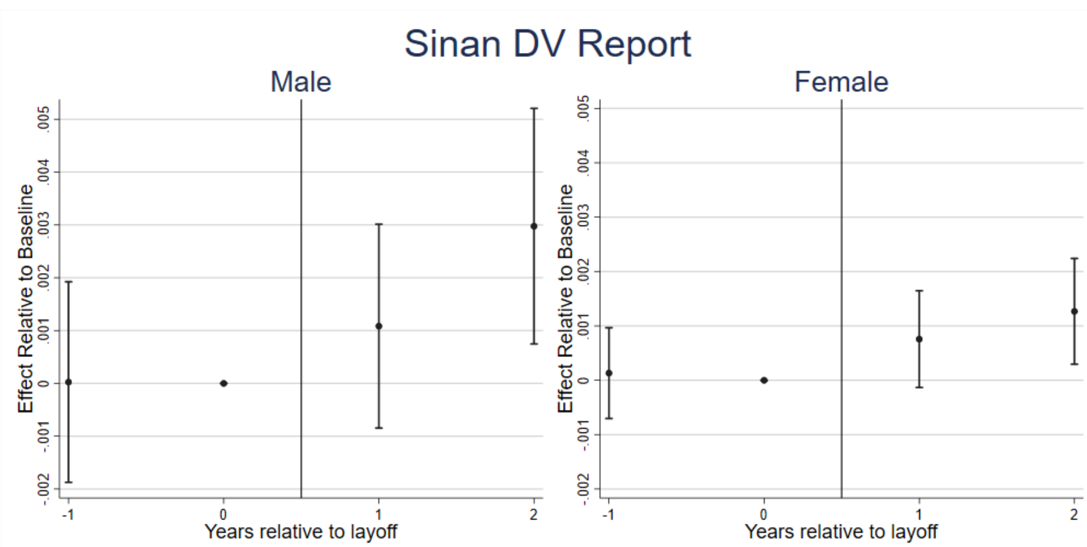
Notes. This figure shows the effect of job loss on the probability of DV perpetration in DV suits for men and DV victimization in protective measures for women, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced 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.

Figure 5: The effect of male job loss on domestic violence by offense intensity



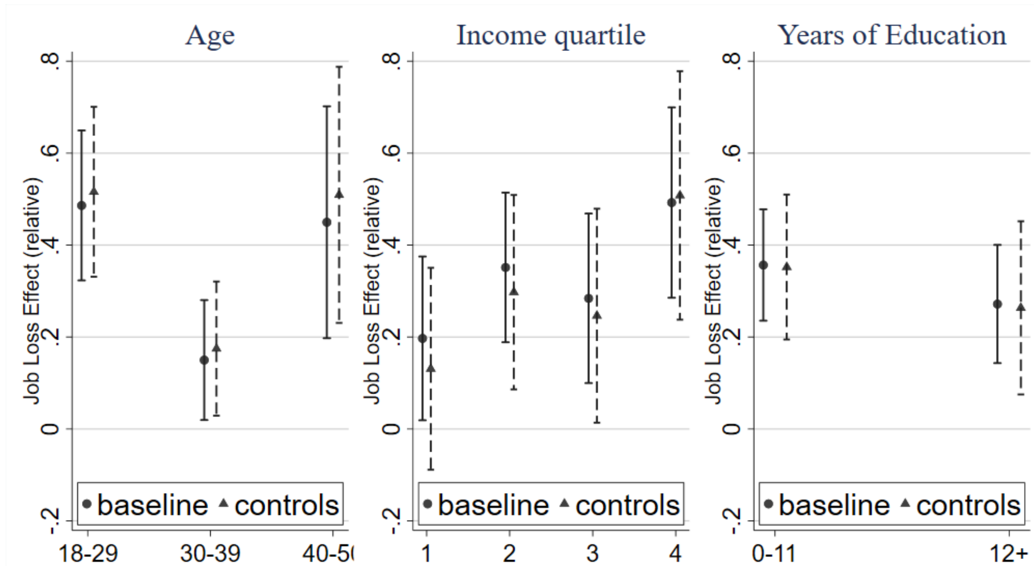
Notes. This figure shows the effect of male job loss on the probability of DV perpetration in DV suits by type and maximum penalty in the four years after the layoff, as estimated from the difference-in-differences equation (2) – along with 95% confidence intervals. The treatment group comprises workers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

Figure 6: The effect of male and female job loss on domestic violence, health system DV notifications



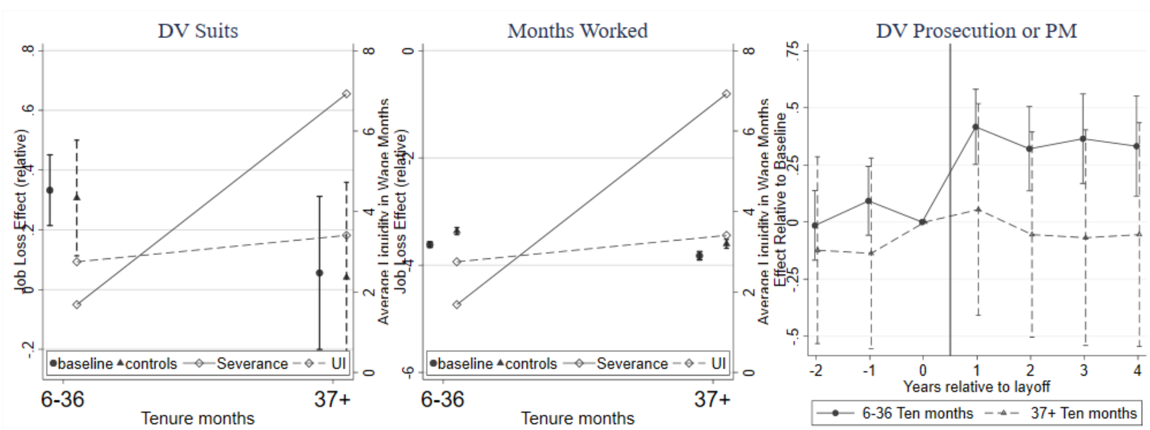
Notes. This figure shows the effect of job loss on the incidence of DV in SINAN reports – health system mandatory notifications on DV victims – for displaced men's female partners and displaced women, respectively, as estimated from the difference-in-differences equation (1) – along with 95% confidence 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.

Figure 7: The effect of male job loss on domestic violence, judicial suits, by individual characteristics



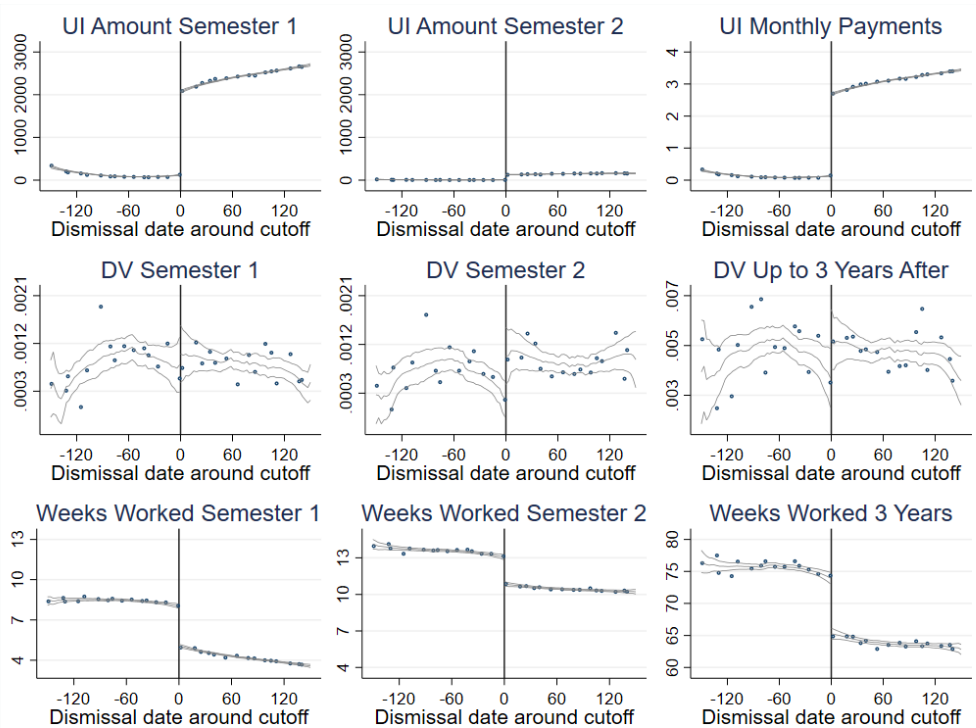
Notes. This figure shows the effect of male job loss on the probability of DV perpetration in DV suits in the four years after layoff – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the 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.

Figure 8: The effect of male job loss on domestic violence by tenure



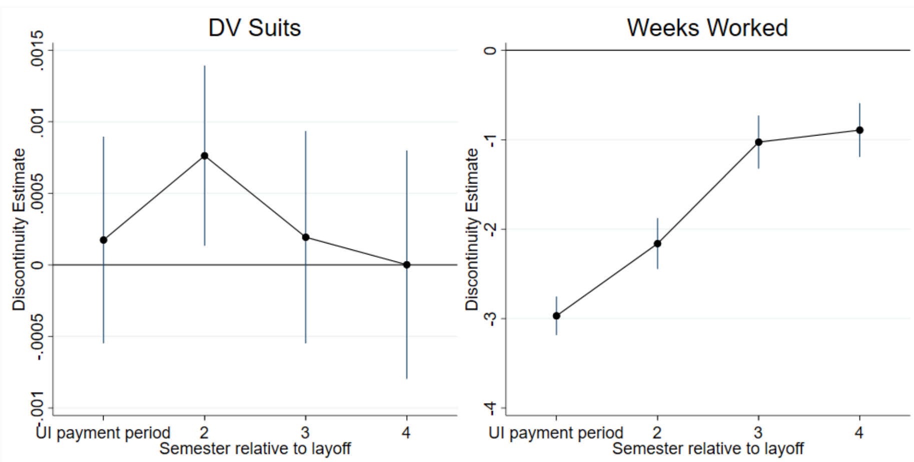
Notes. This figure shows: (i) the average effect of male job loss on probability of DV perpetration in DV suits by tenure at displacement (left panel) and on months worked (center panel), as in eq. (2), and (ii) the dynamic effects of male job loss on probability of DV perpetration in DV suits by tenure at displacement (right panel), as in eq. (1) – along with 95% confidence intervals. The left and center panel report the total predicted lump-sum amount workers receive upon displacement in severance payment and unemployment benefits in month wages- the liquidity scale is on the right vertical axis . The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year. All coefficients are rescaled by the average value of the outcome in the treated group at $t = 0$, which is also reported. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

Figure 9: 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.

Figure 10: 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.

Tables

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

	Male Job Loss			Female Job Loss		
	Treatment	Control	Std Diff	Treatment	Control	Std Diff
Demographic characteristics						
Years of education	10.0	10.9	0.33	11.5	11.7	0.06
Age	30.3	30.3	0.00	30.5	30.5	0.00
Race - white	41.8%	45.2%	0.07	46.6%	46.5%	0.00
Race - black	5.7%	5.3%	- 0.02	3.1%	3.8%	0.04
Race - brown	43.8%	42.1%	- 0.03	39.0%	40.7%	0.03
Job characteristics						
Monthly income (R$)	1,438	1,445	0.01	1,063	1,075	0.02
Month of worked $t-1$	10.7	11.2	0.17	11.2	11.5	0.09
Tenure on Jan 1^{st} (years)	1.1	1.1	0.03	1.4	1.4	0.01
Manager	2.5%	4.8%	0.12	6.0%	7.2%	0.05
Firm size (employees)	724	600	- 0.07	667	560	-0.07
Local area - municipality						
Large municipality - pop> 1M	42%	44%	0.04	37%	37%	-0.02
Municipality population	2,601,919	2,696,668	0.02	990,340	976,942	-0.01
Homicide rate (per 100k inhab.)	32.8	31.6	- 0.06	40.8	38.2	-0.12
Domestic Violence						
Prob. of DV prosecution or protective measure $t-1$	0.0015	0.0011	- 0.01	-	-	-
Prob. of DV prosecution $t-1$	0.0006	0.0005	- 0.01	-	-	-
Prob. of protective measure $t-1$	0.0009	0.0006	- 0.01	0.0007	0.0007	0.00
Observations	810,926	810,926		90,940	90,940	

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); and the standardized difference between the two groups (columns 3 and 6).

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 main 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 period 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).

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
Effect relative to the mean	-	-	-	-
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).

A Appendix to Section 3

Table A1: Descriptive statistics by name uniqueness

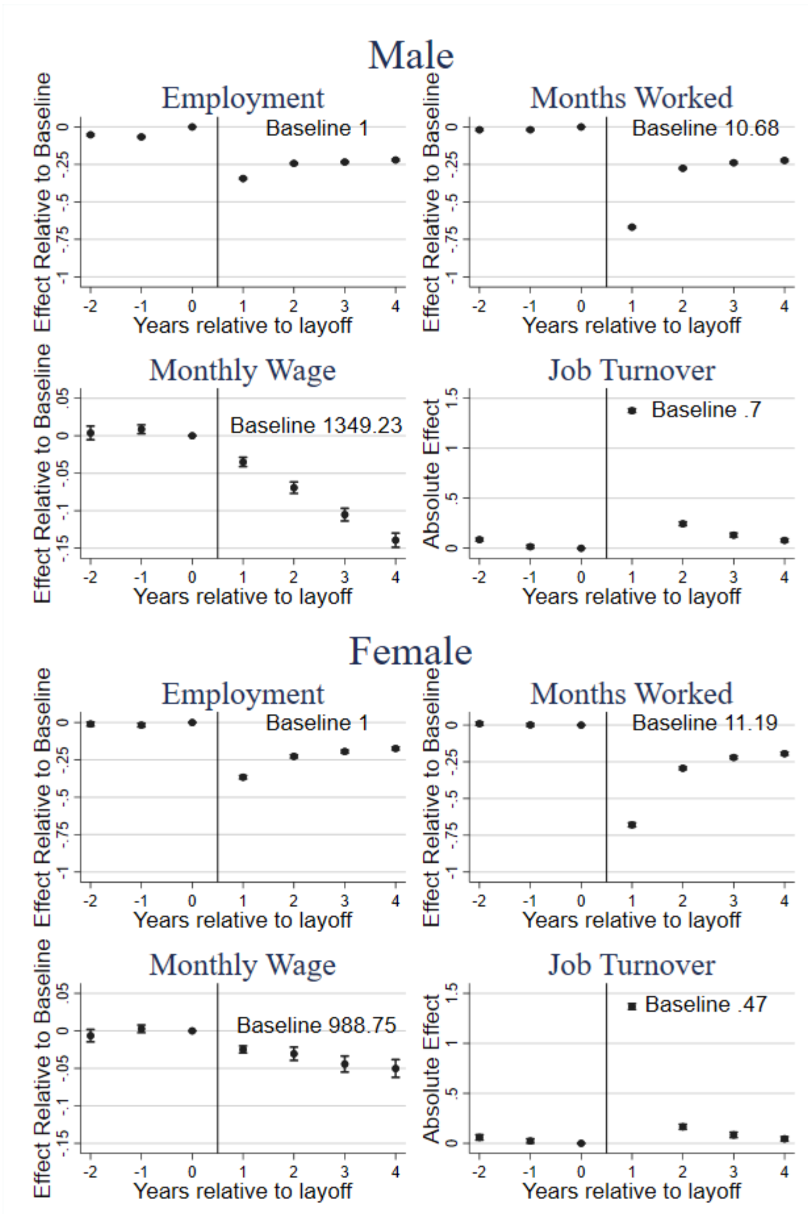
	Male			Female		
	Unique	Others	Std Diff	Unique	Others	Std Diff
Demographic characteristics						
Years of education	10.7	10.2	- 0.21	11.7	11.3	-0.19
Age	30.8	32.0	0.15	30.7	31.0	0.03
Race - white	51.7%	45.7%	- 0.12	60.0%	53.4%	-0.13
Race - black	4.9%	6.6%	0.07	3.6%	5.2%	0.08
Race - brown	34.7%	39.2%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

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

B Appendix to Section 4

B.1 The effect of job loss on employment outcomes

Figure B1: The effect of job loss on employment outcomes



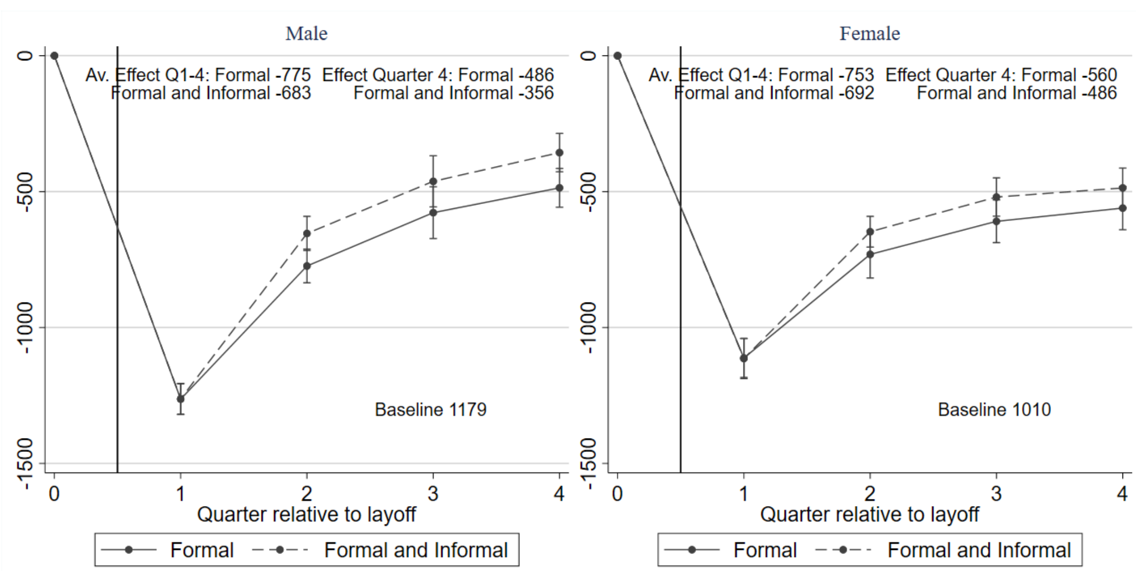
Notes. This figure shows the effect of job loss on formal employment outcomes by gender, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching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

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

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

The results are in Figure B2. Accounting for informal sector income reduces our estimate of earnings losses in the first year after job loss by about 12% for male and 8% for female workers.

Figure B2: The effect of job loss on formal and informal labor income



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

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

Table B1: Effect of job loss on domestic violence, comparable sample, same jurisdictions

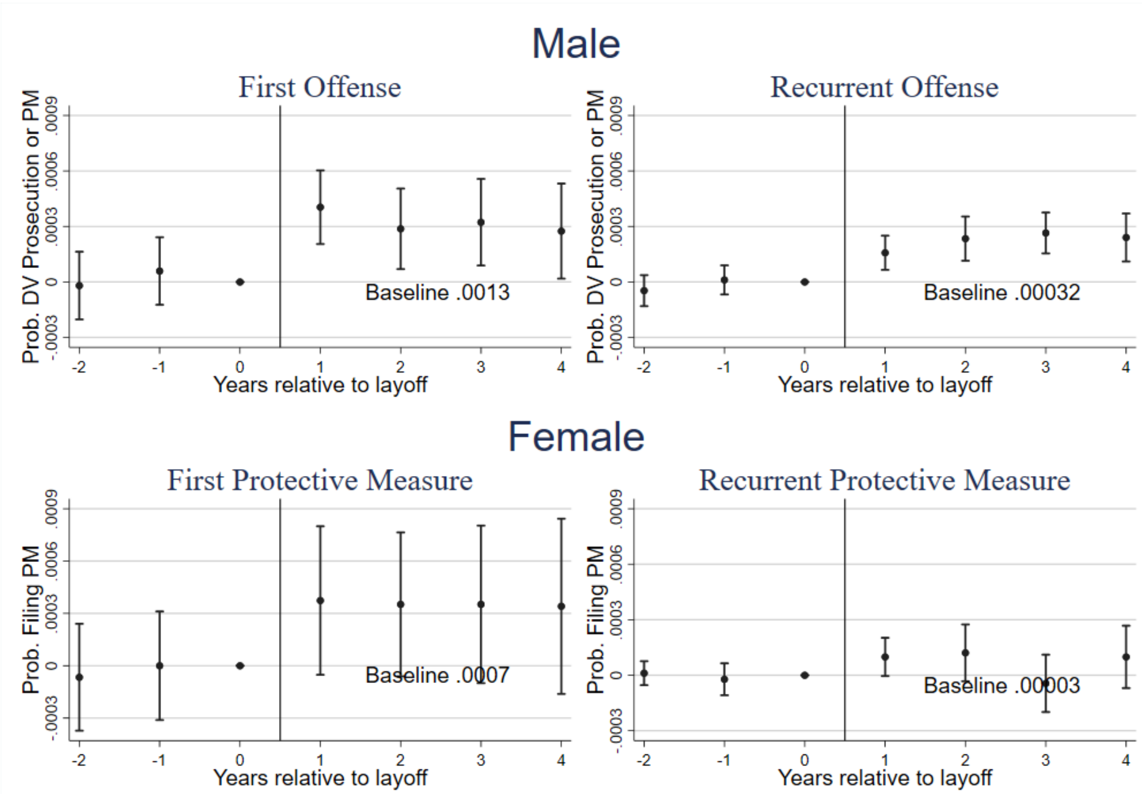
	(1)	(2)	(3)
	Labor market effects		Probability of DV
Dependent variable:	Employment	Income	Protective Measure
PANEL A: MALES DISPLACED IN MASS LAYOFFS, DV PERPETRATION			
Effect of job loss	-0.23***	-5423.3***	0.00044***
	(0.004)	(88.3)	(0.0001)
Mean outcome, treated at t=0	1	12,996	0.0006
Effect relative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

Notes: This table shows the effect of job loss on labor market outcomes (columns 1-2) and DV perpetration/victimization outcomes (columns 3) using a comparable sample covering the same jurisdictions for male and female displacement, as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. The explanatory variable of main 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 period 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).

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

To investigate the observed persistence, we re-estimate the equation distinguishing the first registered DV case for an individual from repeated cases, see Figure B3. The results show that about half of the (absolute) male job loss effect is driven by first offenses, while the other half is related to repeated offenses. The sustained effect on first offenses is consistent with the sustained labor market losses following displacement. Similarly, female job loss increases DV victimization in non-repeated cases several years after job loss. Unlike in the case of male layoff, impacts on repeated victimization are muted, but this pattern may in part derive from the fact that missing names in the court data are more frequent for victims, so that we may fail to identify repeated reporting.

Figure B3: The effect of male and female job loss on domestic violence - persistence

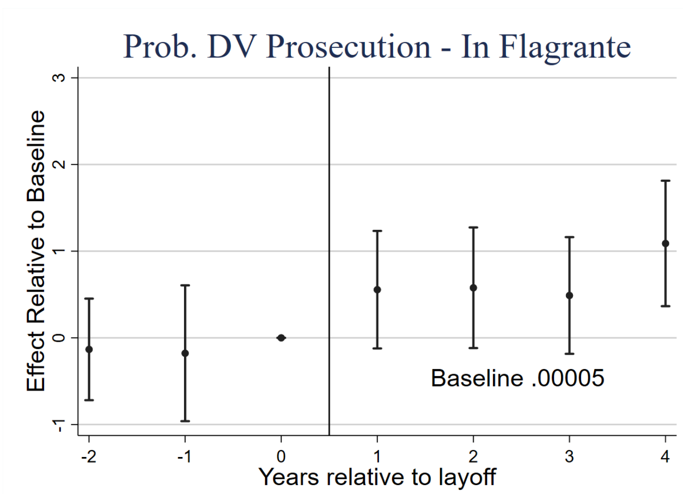


Notes. This figure shows the effect of job loss on the probability of DV prosecution for men and on the probability of filing a DV Protective measure for women, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced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

B.5 Reporting bias: Alternative measures of DV

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

Figure B4: The effect of job loss on domestic violence - in flagrante arrests



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

Table B2: Effect of male and female job loss on domestic violence, use of DV shelters

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

Notes: This table shows the effect of job loss on the probability that women access DV public shelters, as estimated from the difference-in-differences equation (2). In column 1, the sample is restricted to displaced workers present in the social registry, for whom it is possible to identify the female partner. No such restriction is necessary in column 2 as shelter use is reported by women. The dependent variable is measured at the end of each calendar year and the sample is restricted to 2011-13, the period for which the outcome is available. The explanatory variable of interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the period 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).

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

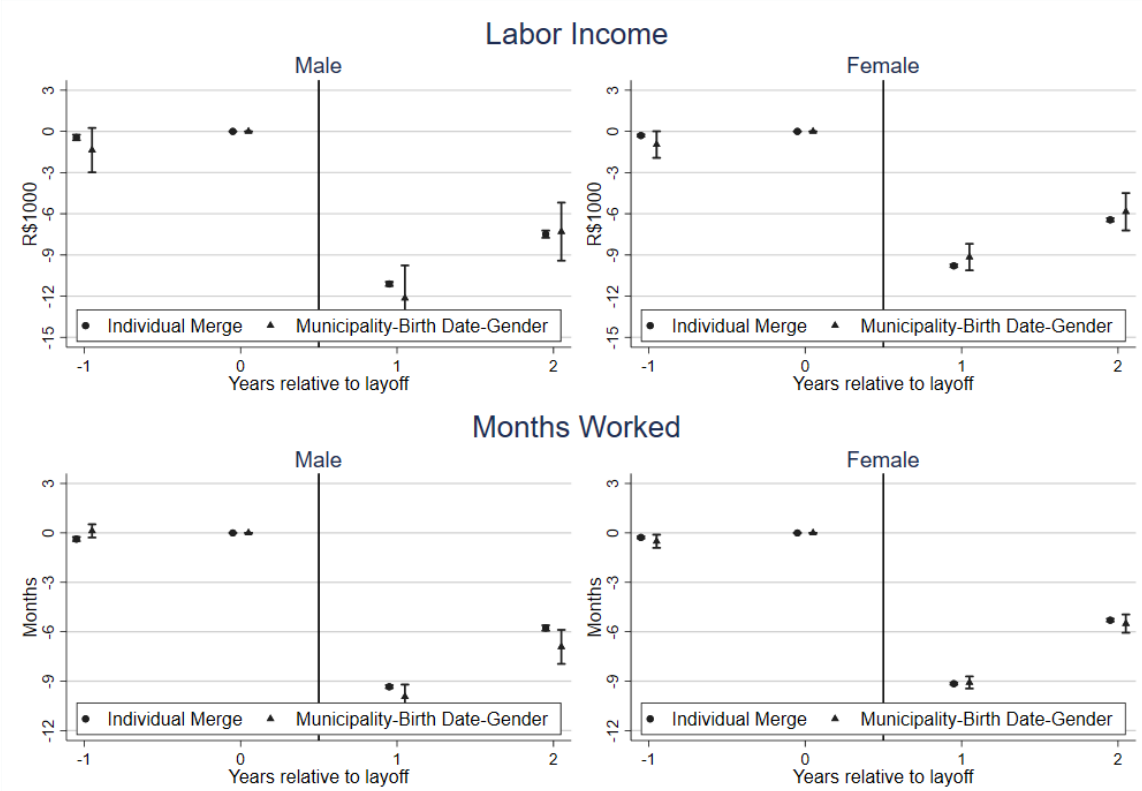
The key challenge in using the SINAN data is that unique individual identifiers are not available. To address this challenge, we link job losers (and their matched control workers) to health system DV notifications by clusters defined by the individual's exact birth date, gender and municipality. As these characteristics do not perfectly identify individuals, this procedure generates some degree of measurement error in the outcome variable. If we assume that measurement error is classical and exogenous to the post-treatment variable of interest, this will cause our estimates to be more imprecise but not biased.[27] We assess this assumption by implementing the matching procedure on employment outcomes, for which we do have individual identifiers. To minimize measurement error, we restrict the sample to individuals in clusters with no more than 80 people, which is equivalent to dropping the upper quintile of the cluster size distribution.[28] Appendix Figure B5 shows estimates of the impact of layoff on employment outcomes estimated with the cluster-level match, compared with our baseline estimates obtained with identified matches. Although the standard errors are larger using the cluster-level match, the estimates are statistically significant and similar in magnitude to the baseline estimates. This evidence supports our merging procedure.

The data are effectively available from 2010 to 2017. We drop observations for 2009, the implementation year of these reports, because coverage around the country was limited. The analysis of male job loss is restricted to the subsample of individuals observed in the social registry in 2011, for whom we can identify their cohabiting female partner and track them in SINAN – which covers information on the victims but does not identify the perpetrator. We drop cases for which the aggressor is identified as a relative (e.g., the victim's father or mother), while we keep cases for which the relationship with the aggressor is not reported (10% of all cases) so that our measure is not endogenous to the victim's willingness to identify the perpetrator. The results are presented in Figure 6 in the main text.

[27]This situation is different from classical measurement error in a regressor, which leads to attenuation bias.

[28]Cluster size is measured by the number of individuals with the same birth date, municipality and gender observed in the labor and social registry data.

Figure B5: Robustness of merging procedure based on municipality-gender-birth date



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

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

B.6.A Timing of violence: quarterly data

One concern regarding our main specification is that we measure violence timing based on when DV suits are filed rather than when the violence occurs. For instance, we could fail to detect diverging pre-trends in our analysis because of this lag. To address this concern in the male job loss analysis, we focus on 'in flagrante" cases, which offer a timely measurement of DV since they are immediately filed in courts (see section 3). Our main results are confirmed using this measure, see Figure B4, Section B.5.A. The female job loss analysis already relies on data on protective measures which are immediately filed in courts. Our analysis using DV notifications by the health system also overcomes these concerns since these data include the exact date when victims seek medical assistance (see Section 4.4). Finally, we replicate the analysis based on

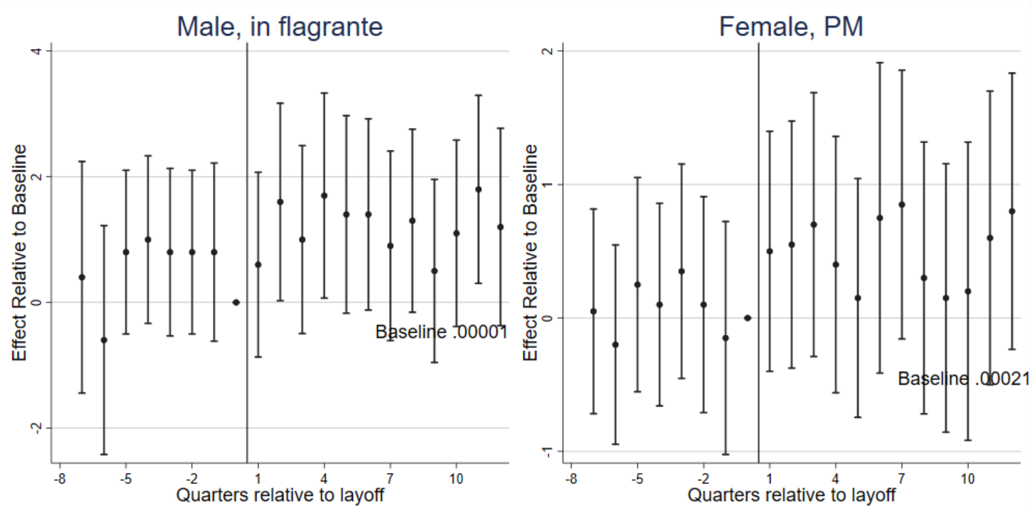
Table B3: The effect of job loss on DV notifications in the SINAN data: Robustness

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

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

"in flagrante" cases for men and protective measures for women at the quarterly level which allows for a finer inspection of potentially diverging pre-trends. The results in Figure B6 show are similar pattern to our baseline estimates and present no evidence of diverging pre-trends.

Figure B6: The effect of job loss on domestic violence, judicial suits, quarterly data



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

B.6.B Municipality-industry-year fixed effects and enlarging the sample to all workers with unique names in the state of work

In the main analysis, matched controls are defined on state, 1-digit industry, and individual characteristics and this should difference out local shocks. Nevertheless, we show that our results are not sensitive to controlling for a more granular set of fixed effects, namely year $\times$ municipality$\times$2-digit industry. We then show that our results remain similar when enlarging the sample to cover all workers with unique names in the state where they work – matching the employment data to judicial records based on name and location, and improving the coverage of our data. Finally, we also restrict the definition of potential (matched) controls to include only workers remaining employed through the entire post-displacement period, rather than only in the year that their treated match is dismissed. In all cases, our results remain similar, see panels A-C of Table B4.

B.6.C Dynamic selection into layoffs

We demonstrate robustness to a series of tests for dynamic selection, that allay the concern that displaced workers are selectively pre-disposed towards DV at the time of displacement even within mass layoffs. We vary the definition of mass layoffs from 33% up to 75%, while jointly varying the minimum size of firms in the sample from 30 to 70. The larger the fraction of workers dismissed, the more limited is the scope for selection into dismissal. Table B5 shows that the estimated effects of male and female job loss are broadly similar to the baseline estimates. Similarly, all results hold when restricting the treated group to workers in closing plants (Panel D, Table B4). The results are also robust to defining as treated all workers in mass layoff firms at the beginning of the calendar year when the mass layoff occurs, rather than just workers who are actually displaced. This approach avoids concerns regarding the selection of workers dismissed from downsizing firms. As it delivers an intention-to-treat estimate (analogous to estimates from randomized experiments with imperfect compliance), the relative effect is smaller (21% rather than 32%) but it is still statistically significant (Panel E, Table B4). In both Panels D and E the estimated elasticity of DV to earnings retains the same order of magnitude. We do not attach a causal interpretation to these elasticities as that would require that layoffs affect DV only through decreased earnings. In fact, layoffs can directly affect DV through other mechanisms such as exposure.

B.6.D Missing data on names

We provide three robustness tests to address the fact that we cannot observe the alleged offender's identity in 40% of DV cases (prosecutions plus PM) and the victim's identity in 46% cases in our main analysis sample. First, we take advantage of the fact that the share of court cases with missing identity varies widely across states and jurisdictions, and show in Table B6 that our estimates are robust to progressively dropping from the sample areas where the share of missing data is above a certain level. In particular, our key findings hold when exclusively looking at areas where missing data is not a substantial concern. Second, our findings on male job loss hold when we restrict the sample to DV prosecutions initiated "*in flagrante*" (Figure B4). In such cases, judges take the initial decision on case secrecy based on the police form describing the arrest rather than by reference to defendant characteristics such as employment status, so differential under-reporting should be a lesser concern. Third, in Section 4.4 we show that the same key findings emerge when we analyze DV SINAN-notifications on DV victims filed in the health system, an alternative measure of domestic violence which is not subject to these missing data issues. Finally, we show in Section 4.4 that our findings are robust to using another DV measure that is independent of the court process.

B.6.E Estimation of dynamic treatment effects

Several recent studies highlight the challenges associated with estimating dynamic treatment effects in two-way fixed effects settings when there is variation in the treatment timing and treatment effects are heterogeneous across individuals. Under these conditions, some treated individuals might enter the double differences estimating the dynamic treatment effects with weights of opposite signs in different time periods. As a result, the estimated treatment effect differs from the average treatment effect, nor it is representative of any relevant population of interest (de Chaisemartin and D'Haultfœuille, 2020).[29] This problem is most severe when all or a large share of individuals in the sample are treated at some point. We overcome these issues by including a large share of never-treated workers. We estimate the share of units with negative weights following de Chaisemartin and D'Haultfœuille (2020), and find that no individual treatment effect receives a negative weight both for male and female job loss. It is worth noting that our difference-in-differences estimator is similar to de Chaisemartin and D'Haultfœuille (2020) with the difference that they select the control group on a period by period basis to include all non-switchers while we fix the control group over all periods.

[29] See also Goodman-Bacon (2021), Borusyak and Jaravel (2017), Sun and Abraham (2020), Athey and Imbens (2018), Callaway and Sant'Anna (2020) and Imai and Kim (2021).

Table B4: Effect of job loss on domestic violence, robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Job loser	Male			Female		
	Labor market effects		Probability of DV	Labor market effects		Prob. of Filing
Dependent variable:	Employment	Income	Any	Employment	Income	Protective Measure
PANEL A: ADD MUN X IND X YEAR FE						
Effect of job loss	-0.21***	-5943.8***	0.00041***	-0.21***	-4147.5***	0.00039**
	(0.002)	(92.5)	(0.00008)	(0.003)	(61.6)	(0.0002)
Mean outcome, treated at t=0	1	14,674	0.0015	1	11,193	0.0007
Effect relative to the mean	-21%	-41%	28%	-21%	-37%	55%
Elasticity to earnings			-0.68			-1.47
Observations	11,313,141	11,313,141	11,313,141	1,252,986	1,252,986	1,252,986
PANEL B: ADD MUN X IND X YEAR FE, EXTENDED SAMPLE: UNIQUE NAMES WITHIN STATE						
Effect of job loss	-0.15***	-2114.7***	0.00029***	-0.21***	-4076.4***	0.00024*
	(0.0008)	(51.8)	(0.00006)	(0.003)	(55.6)	(0.0001)
Mean outcome, treated at t=0	1	9,960	0.0014	1	11,050	0.0008
Effect relative to the mean	-15%	-21%	20%	-21%	-37%	30%
Elasticity to earnings			-0.95			-0.82
Observations	16,679,411	16,679,411	16,679,411	1,668,366	1,668,366	1,668,366
PANEL C: ONLY CONTINUOSLY EMPLOYED IN THE CONTROL GROUP						
Effect of job loss	-0.42***	-10516.1***	0.00071***	-0.45***	-7606.6***	0.00057***
	(0.002)	(97.7)	(0.00009)	(0.005)	(104.5)	(0.0002)
Mean outcome, treated at t=0	1	16,108	0.0016	1	11,193	0.0007
Effect relative to the mean	-42%	-65%	46%	-45%	-68%	80%
Elasticity to earnings			-0.70			-1.17
Observations	4,911,578	4,911,578	4,911,578	572,922	572,922	572,922
PANEL D: ALL WORKERS IN CLOSING PLANTS						
Effect of job loss	-0.21***	-3535.5***	0.00025**	-0.29***	-3263.9***	0.00023*
	(0.004)	(120.1)	(0.0001)	(0.004)	(68.3)	(0.0001)
Mean outcome, treated at t=0	1	14,808	0.0010	1	9,338	0.0004
Effect relative to the mean	-21%	-24%	25%	-29%	-35%	66%
Elasticity to earnings			-1.05			-1.87
Observations	1,381,136	1,381,136	1,381,136	598,374	598,374	598,374
PANEL E: ALL WORKERS IN MASS LAYOFF FIRMS						
Effect of job loss	-0.23***	-4131.2***	0.00027***	-0.23***	-2893.7***	0.00016*
	(0.002)	(73.3)	(0.00006)	(0.004)	(69.4)	(0.00009)
Mean outcome, treated at t=0	1	13,068	0.0013	1	9,766	0.0005
Effect relative to the mean	-23%	-32%	21%	-23%	-30%	35%
Elasticity to earnings			-0.65			-1.19
Observations	18,869,718	18,869,718	18,869,718	1,623,685	1,623,685	1,623,685

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

Table B5: Effect of job loss on domestic violence, robustness to mass layoffs definition

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

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

Table B6: Effect of job loss on domestic violence, robustness to missing values in the judicial data

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

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

B.7 Estimates for Couples

We now show that our main findings hold within a sample of cohabiting couples. We use the registry for Federal social programs (CadUnico) to identify the cohabiting partners of job losers in our main sample. In just 3% of this sample, both spouses lose their jobs in a mass layoff, and we drop these cases. Estimates on the remaining sample for whom it is possible to identify a cohabiting spouse, using the same empirical strategy (Section 4) are in Table B7. We find that male job loss results in a 77% increase in female partners filing protective measure requests (Panel A, column 1), and that female job loss increases the probability that male partners are judicially charged for DV by 38% (Panel B, column 1). Baseline rates of DV of job loss are larger in this poorer segment of the population, and differences in relative magnitudes may reflect that it is a lower income population.

The household sample allows us to investigate heterogeneity in the impacts of job loss by baseline household characteristics (Table B7, Columns 2-5). We find that both male and female job loss have larger impacts on DV when there is a child under the age of ten in the home, consistent with the income shock being more stressful in young families with dependents. Male job loss has a larger impact on DV when the female partner is not employed at baseline. This is consistent with both an income mechanism – losses are stronger when the partner is not employed – and with an exposure mechanism – partners spend more time together during the unemployment period. Yet, these comparisons should be interpreted with caution as the sub-group estimates are often not statistically significantly different from one another.

Finally, we investigate couple stability for job losers who continue to show in the social registry in the post displacement period, see Table B7. Interpreting these estimates required some caution as the probability of continuing in the social registry increases after job loss, albeit this is arguably a small effect, ranging from 3% to 5% relative to the baseline (column 6). We find that job loss does not strongly affect the probability that job losers stay with the same partner after the layoff. Male job loss does not affect the probability that men retain the same partner, while female job loss reduces partner's stability by 3% relative to the baseline (column 7).

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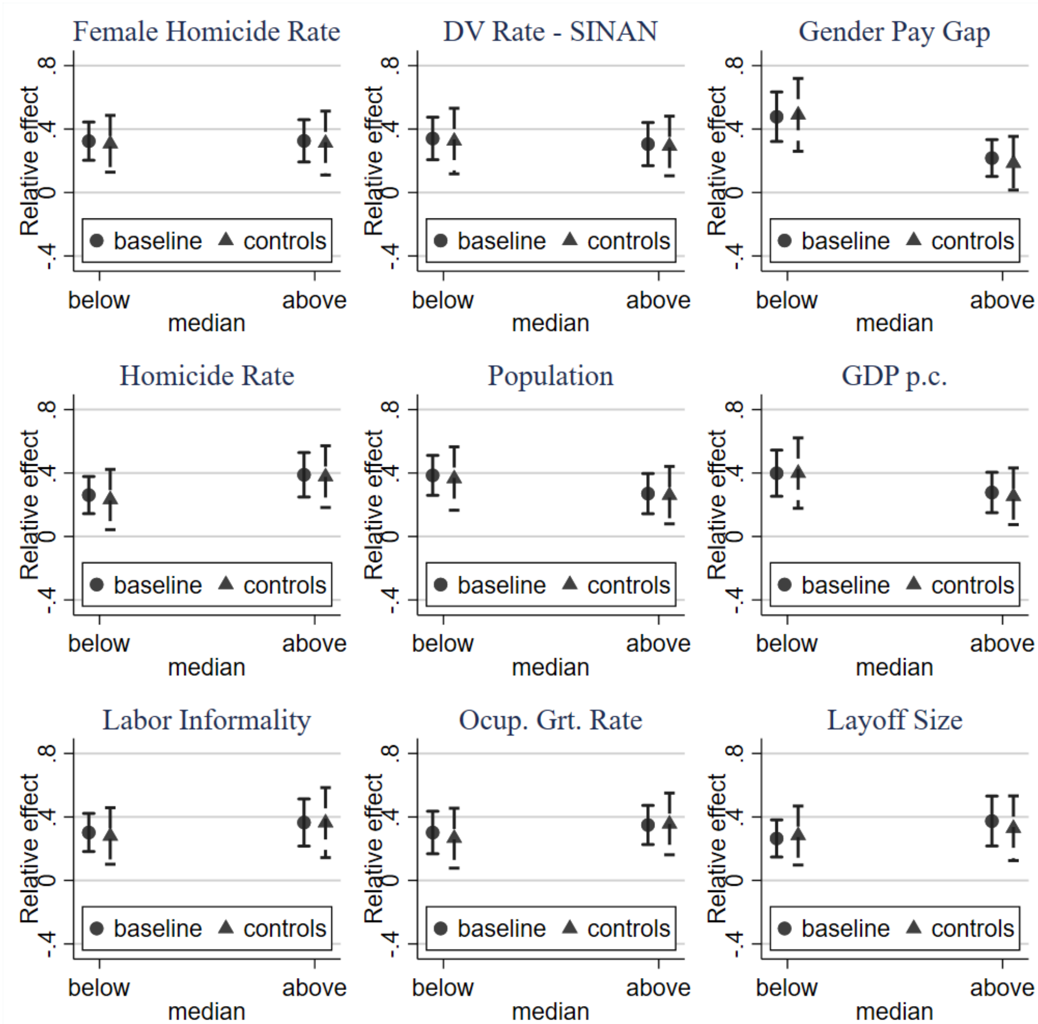
Table B7: Effect of male and female job loss on partners using household data

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

Notes: Columns 1-5 in this table show the effect of male job loss on DV victimization by the partner (Panel A) and the effect of female job loss on DV perpetration by the partner (Panel B), as estimated from the difference-in-differences equation (2). In both panels, the sample is restricted to displaced workers present in the social registry in 2011, for whom it is possible to identify the respective partner. Columns 6-7 presents the same results on the prob. that the worker is still present in the registry after the job loss and, if registered, that she/he has the same partner. The explanatory variable of main 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 period 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$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.1).

B.8 Area-level heterogeneity

Figure B7: The effect of male job loss on domestic violence, judicial suits, by area-level characteristics

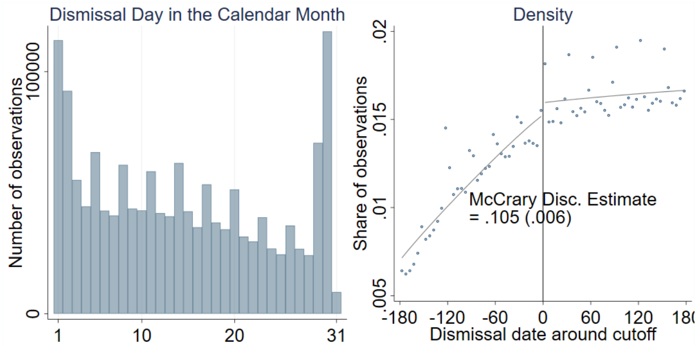


Notes. This figure shows the effect of male job loss on the probability of DV perpetration in DV suits in the four years after layoff, by area-level characteristics – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts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

C Appendix to Section 5

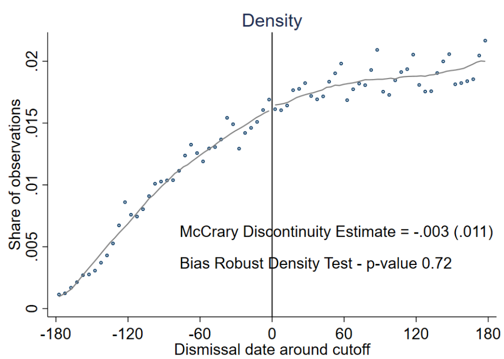
C.1 Dismissal cycles

Figure C1: The effect of UI eligibility, dismissal patterns, extended sample



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

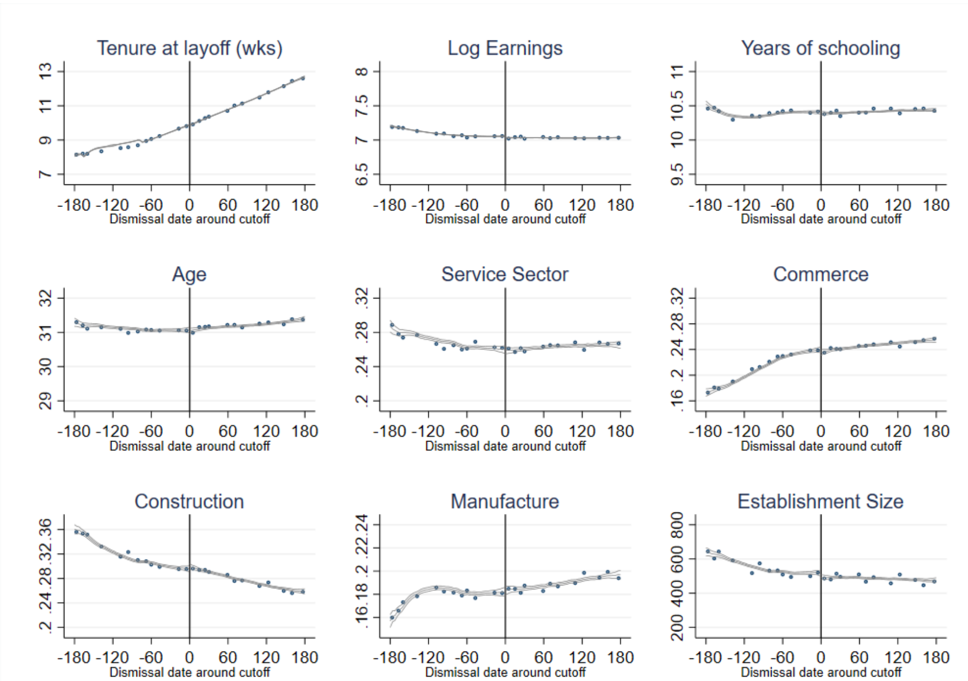
Figure C2: The effect of UI eligibility, density around the cutoff, main sample, male workers



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

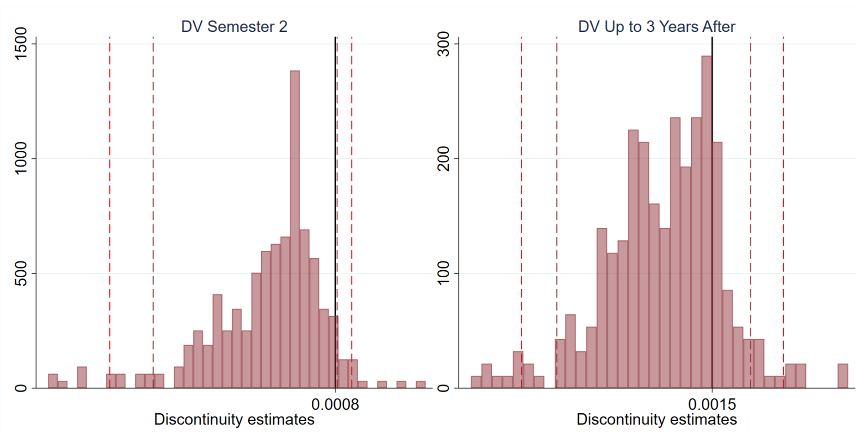
C.2 Robustness

Figure C3: The effect of UI eligibility, balance of covariates, male workers



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

Figure C4: The effect of UI eligibility on domestic violence, permutations tests, male workers



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

Table C1: Effect of UI eligibility on domestic violence, male workers, robustness to specification choice

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

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

Table C2: Effect of UI eligibility on domestic violence, male workers, robustness to dismissal and cutoff fixed-effects and extended sample design

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

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