Job Displacement, Unemployment Benefits and Domestic Violence*

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May 8, 2023

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 first show that both male and female job loss, independently, lead to large and pervasive increases in domestic violence. Exploiting a regression discontinuity design, we then show that unemployment benefits do not reduce domestic violence while benefits are being paid, and that they lead to higher domestic violence once benefits expire. These findings can be explained by the negative income shock brought by job loss and by increased exposure of victims to perpetrators, as partners tend to spend more time together after displacement. Although unemployment benefits partially offset the income drop following job loss, they reinforce the exposure shock as they increase unemployment duration.

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

*We thank Dan Anderberg for insightful discussions. The paper benefited from the comments of Koray Aktas, Aimee Chin, Gianmarco Daniele, James Fenske, Manoel Gehrke, Paola Gobbi, Johannes Haushofer, Chinhui Juhn, Andreas Kotsadam, Soren Leth-Petersen, Giovanna Marcolongo, Marta Morando, Eva Mork, Eugenio Proto, Fan Wang and participants in seminars at the Universities of Copenhagen, Delhi, Essex, Gothenburg, Glasgow, Linz, Lahore, Houstan, Chile. We are solely responsible for the contents of this paper. We acknowledge financial support from The Harry Frank Guggenheim Foundation. Bhalotra acknowledges support from ESRC grant ESM010236-1 awarded to the Human Rights, Big Data and Technology project at the Human Rights Centre in Essex.

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1 Introduction

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

In this paper, we study how economic shocks and policies influence DV, exploiting individual-level administrative data on DV cases brought to Brazilian courts in 2009-2018 linked to longitudinal employer-employee registers for the Brazilian population. We complement the analysis with additional violence 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 the effects of male job loss on DV perpetration and of female job loss on DV victimization, using a difference-in-differences strategy that leverages mass layoffs for identification. Second, we examine whether unemployment insurance (UI) attenuates any effects of job loss using a clean regression discontinuity (RD) design. Importantly, estimating the impacts of male job loss, female job loss, and unemployment benefits in the same setting allows us to gain insights on the predictive power of domestic violence theories and to propose new mechanisms.

We find that both male and female job loss, analyzed in isolation, lead to more domestic violence against the female partner, which increase by 32% and 56%, respectively (the effects are not statistically different from each other).[1] These effects are pervasive along the distribution of perpetrator age, education and baseline income, and also across a wide set of area-level characteristics, including factors related to gender norms, such as the gender pay and employment gaps, and DV risk itself. The effects also 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.

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

The large increase in DV following male and female job loss cannot be explained by prominent DV theories. First, in the household bargaining model (Aizer, 2010), DV risk increases with the relative bargaining power of men. In line with our results, it predicts more domestic violence after women lose their jobs, as they lose bargaining power. However, in contrast with our results, it predicts less violence in the household after men lose their jobs, as they have higher bargaining power. Second, in the male backlash model (Macmillan and Gartner, 1999), DV increases when the norm of male dominance and female dependence is threatened. This model predicts higher DV risk after men lose their jobs, in line with our results, but it cannot explain our finding of higher DV risk after female job loss. Two alternative hypotheses lead to the same predictions of the male backlash model and fail to explain our findings. These 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).[2]

We propose two mechanisms that can explain our main empirical findings. First, job loss leads to strong and persistent income losses, causing lower consumption levels in the household. This triggers stress and opens the door for conflict (Clark et al., 2008; Buller et al., 2018).[3] This mechanism may be present even if partners do not engage in any type of income pooling, as lower consumption by the displaced partner alone can be enough to trigger stress and conflict in the couple. Second, job loss increases women's exposure to DV risk, as displaced workers spend more time at home (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).

We formalize these mechanisms into a simple two-period job search model in partial equilibrium. The model is composed of a male and a female partner who may lose their jobs from the onset of the model. When employed, they work a fix

[2]The model in Haushofer et al. (2019) can explain how a negative income shock to both partners may lead to an increase in DV but it cannot explain our empirical finding that UI benefits do not reduce DV. Crucially, it does not incorporate the exposure mechanism which plays a central role in our analysis.

[3]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).

amount of hours. When unemployed, they receive unemployment benefits lasting for one period and search for a job. In this model, DV risk against the female is given by a flexible function which is decreasing both on the consumption of each partner, reflecting the income mechanism, and on the hours worked by each partner, reflecting the exposure mechanism. The model is similar in spirit to Card and Dahl (2011) to the extent that DV emerges due to emotional queues.

The model predicts that job loss by the man or the woman leads to higher DV risk, because of the income and exposure mechanisms. We provide two analyses supporting the model and these two mechanisms. First, the model predicts that DV violence should be decreasing in income available for workers upon displacement because of the income mechanism. To test for that in the data, we exploit the fact that mandatory severance pay is increasing in tenure. In line with our model's prediction, we find that the impact of job loss on domestic violence strongly decreases with tenure at displacement. In fact, we find null effects for high tenure workers who receive large amounts in severance pay and are less likely to be liquidity constrained.[4] The gradient over tenure contrasts with the pervasiveness of the effects across other dimensions of heterogeneity such age, education and income.

Second, we show that our model is able to predict the impacts of access to unemployment benefits on domestic violence. On the one hand, UI transfers should reduce DV by mitigating the income losses following displacement. On the other hand, UI could lead to higher DV risk because it induces workers to take longer to return to the labor market (Katz and Meyer, 1990; Card, Chetty and Weber, 2007; Lalive, 2008; Gerard and Gonzaga, 2021), possibly increasing DV exposure as workers may spend more time at home with their partners. As a result, the overall impact of unemployment benefits on DV during the benefit period depends upon the relative size of the income and exposure mechanisms. Once benefits expire, only the exposure mechanism remains active, given that the negative effects on employment are persistent. Our empirical results are in line with these predictions. They show that unemployment benefits have no impact on DV while they are being paid out, and a

[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 makes it unlikely that differences in exposure explain the observed patterns. Thus the evidence is best explained by differences in liquidity at displacement.

perverse impact, increasing DV, once benefits expire. Hence, they are consistent with the idea that UI transfers offset higher exposure during the benefit period, and with the exposure mechanism prevailing after benefits cease.[5]

Summarizing, our results suggest that job loss increases DV risk, 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 several challenges to the causal interpretation of these results. A first order concern is endogenous reporting. Women could be less likely report DV events after losing their jobs, and more likely to report after men lose their jobs. If this were the case, our estimates of the impact of male job loss would be upward biased, while the estimated impact of female job loss would be downward biased. We address this concern by showing that our estimates hold using alternative DV measures: (i) court cases initiated by *in flagrante* arrests (i.e., when the offender is caught "red-handed"); (ii) use of public DV shelters by women; and (iii) mandatory notifications of DV cases by health providers. These measures depend less (if at all) on the victim's discretion in reporting. In the case of public shelter use and notifications by health providers, the police and judicial authorities are not notified, which mitigates concerns of reporting being inhibited by the fear of retaliation by male offenders.[6]

Our difference-in-differences strategy compares DV outcomes for the same workers before and after the mass layoff, with respect to a suitable control group, purging the influence of any individual fixed characteristics. The control group is composed of non-displaced workers from the same area-sector that are exactly matched on a wide array of observable worker and firm characteristics. A wide set of robustness exercises address concerns related to endogenous selection into mass layoffs, the role of mass spillovers, 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). 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 discontinuity estimates are robust to varying bandwidths, polynomial specifications, permutation tests and

[5]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).

[6]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.

falsification analysis based on pre-displacement DV suits.[7]

We further investigate additional concerns such as missing information on the identity of suspected offenders and victims due to limitations of our data. Although we study male and female job loss in isolation in our main analysis, we show that our main results hold in a subsample where we can link couples.[8] In addition, we use this subsample to show that job loss does not strongly affect couples separation and partner's labor supply.

Our paper provides 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. Other studies investigate the impacts of economic shocks by analyzing area-level shocks in the labor market for men and women. For instance, (Anderberg et al., 2016) finds that increases in male unemployment rates lower DV, while increases in female rates have the opposite effects in UK, and Aizer (2010) finds that improvements in area-level relative wages of women in the United States lower DV, both results being consistent with a household bargaining model. Other studies reveal an increase in DV following improvements in labor market opportunities for women, contradicting the predictions of the bargaining model (Tur-Prats, 2019; Bhalotra et al., 2019; Erten and Keskin, 2020) and argue that their findings are in line the male backlash model (Macmillan and Gartner, 1999).

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

[7] Overall, these robustness largely follow the tests proposed in Britto, Pinotti and Sampaio (2022) who studies the effects of job loss and unemployment benefits on general criminal in the same setting.

[8] 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.

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 off cash transfers to women and men in Kenya in a controlled experiment.[9]

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 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). XXX Relation to Covid papers here XXX

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).[10]

Finally, recent studies analyze study the impacts of mass layoffs on general crime: Bennett and Ouazad (2019); Khanna et al. (2021); Rose (2018), and, employing the

[9]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 unemployment duration by directly incentivizing lower job search.

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

same judicial data as ours, Britto, Pinotti and Sampaio (2022). A fundamental distinction to these studies is that domestic violence is a different phenomenon. Distinctively, it is a type of crime involving a perpetrator and victim who know each other and whose decisions affect each other. As a consequence, DV strongly depends on household dynamics, such as the exposure mechanism and spousal decisions on consumption insurance. These dynamics demand specific models of behavior and also lead to different empirical results. While the prevalent model in the crime literature is the Becker-Ehrlich model whereby crime depends on expected punishment and the opportunity costs of legal activities, domestic violence models such as the household bargaining model and the model here provided focus on the household. Empirically, our findings that access to unemployment benefits increases domestic violence are in sharp contrast with the results in Britto, Pinotti and Sampaio (2022); Rose (2018) showing that more generous benefits reduce general crime.[11]

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 a simple model explaining the main findings. Section 5 investigates mitigation with unemployment benefits, followed by Section 6 which concludes.

2 Context and Institutions

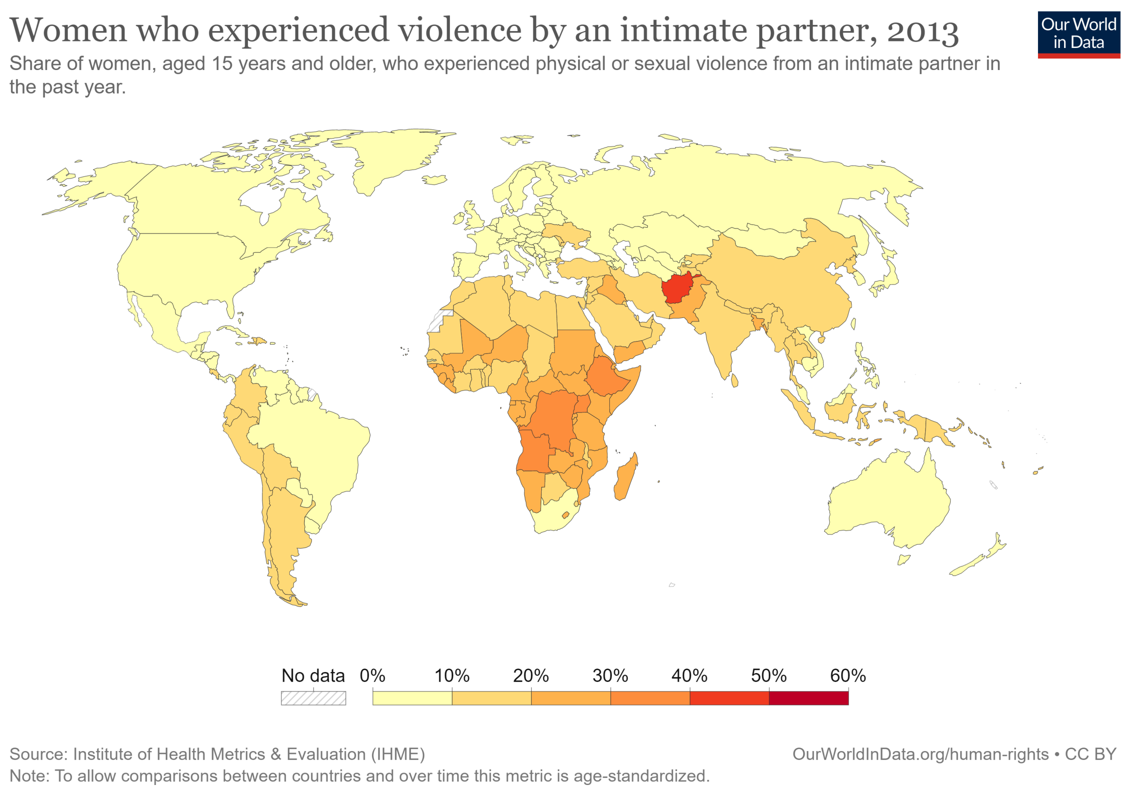
Domestic violence and criminal justice in Brazil. In Figure 1, we compare DV rates in Brazil relative to other countries, using data for 2013, the center of our analysis period.[12] The share of women experiencing DV in the past 12 months in Brazil is 7.5%, just above the 25$^{\text{th}}$ percentile of the distribution across countries. DV rates in Brazil lie between slightly lower rates displayed in high-income countries in North America, Europe and Oceania, and the higher rates registered in other

[11]Our paper also differ in important dimension from Rose (2018) who also studies impacts of job loss on domestic violence, but using a selected sample of ex-inmates in the state of Washington. First, we use data on the universe of displaced workers from a large and heterogeneous country. Second, we also study the effects of female job loss on DV victimization. As we show in the paper, the latter is crucial for analyzing the underlying mechanisms driving DV and ruling out several alternative theories. Finally, we carefully address reporting bias issues which are crucial for the interpretation of the results, given that it is a much more severe concern for DV than for general crime.

[12]We use data by the Institute of Health Metrics & Evaluation (IHME), providing comparable statistics across countries.

developing countries in Latin American, Africa and Asia.[13] DV rates also vary widely within Brazil, in line with the fact that it is a large and heterogeneous country. The rate of DV notifications per 100,000 thousands inhabitants in the health system vary from 15 to 116 in municipalities at the 25th and 75th percentiles of the distribution.[14] The rich data and highly heterogeneous context makes Brazil an appealing setting for studying DV and investigating to which extent the results vary across different area-level and individual characteristics.

Figure 1: Share of Women Experiencing Intimate Partner Violence, Last 12 Months, Across Countries, 2013



Notes The figure shows the share of women experiencing intimate partner violence in the last 12 months across countries for 2013. Source: Institute of Health Metrics & Evaluation (IHME)

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

[13]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.

[14]Statistics based on SINAN notifications for 2013. See Section 4.4 for details on the data.

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

Labor markets. Labor law in Brazil allows firms to dismiss workers without a just cause, although it imposes severance payments.We analyze layoffs without a just cause, which account for 65% of all separations (the rest are mainly voluntary quits). 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.[15] 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. These benefits last for up to five months with an average replacement rate of 79% relative to pre-displacement earnings. Once unemployment benefits expire, the only income support at the national level is "Bolsa Família", a conditional means-tested cash transfer targeted at very poor families. In 2019, the average transfer per household was 16% of the minimum wage and the maximum per capita family income for eligibility was less than one-fifth of the minimum wage.

Our description so far refers to formal jobs. However, Brazil has a large informal sector, accounting for roughly 45% of all jobs in the analysis period. Job turnover is high in both the formal and informal sector, and workers tend to move frequently between the two. Moreover, it is not uncommon that firms hire both formal and informal workers (Ulyssea, 2018). Since there are no administrative data on informal

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

employment, we restrict our main analysis to layoffs in the formal sector. We use survey data to quantify the degree to which informal work contributes to the recovery of employment and earnings after job loss, and explore heterogeneity in informality rates to study whether labor informality plays a role in explaining our findings.

Preliminary evidence. Figure 2 plots the adult employment rate over the period 1990-2018 against the female homicide rate – the best measure of DV over prolongued periods of time. Feminicide rates increased substantially over this period, from 3 homicides per 100k women in 1990 to 4.3 in 2018. Notably, this increase coincides with a worsening in labor market conditions, the employment rate declining from 65 to 55 percent. Of course, these patterns in aggregate data may reflect trends in other omitted factors. We next describe the detailed individual-level data that we will use to identify causal effects.

Figure 2: 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.

3 Data

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

Judicial registers. We use data on the universe of DV cases filed in all first-degree courts during 2009-18. These include information on the start and end date of the judicial case, court location, subjects being discussed, and full names of the defendant and plaintiff.[16] In total, there are 2.4 million DV cases, comprising 1.23 million DV prosecutions and 1.17 million protective measures. The name of the defendant is available for 1 million of the 2.4 million DV cases. When studying victims, we only use data on protective measures, for which we observe the victim's name in 244,000 out of 1.17 million cases, while their names are missing in virtually all DV prosecutions. Missing data arise for two reasons: mistakes in the process of inputting data from court diaries; and judicial secrecy, which tends to protect the victim's identity.

In Sections 4.4 and 4.5, we address missingness issues in several ways. We show that missingness status are largely explained by court-level factors, and that our main estimates continue to hold when running the analysis within those jurisdictions where the share of missing names is quantitatively irrelevant. Moreover, we show that our main findings continue to hold when using alternative DV measures that do not suffer from missing data limitations.[17]

Employment registers. We use linked employer-employee data for 2009-2018 covering the universe of formal workers and firms in Brazil (*Relacao Anual de Informacoes Socials*, RAIS), made available by the Ministry of Labor. Workers are identified by a unique tax code identifier (CPF) and their full name. The register contains rich information on each job spell such as workers' date of birth, education, earnings, and occupation, job starting and end dates, reason for separation, and firm unique identifiers. Since employers must provide workers with notice of dismissal at least 30 days in advance, we define the timing of layoff as the official layoff date stated in RAIS minus 30 days.[18]

Linking court and employment records. We merge the judicial and employment data using the (full) name of the individual, which is consistently and accurately

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

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

[18] This period is extended by three days for each completed tenure year. 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.

reported in both registers. To ensure precision, we restrict our sample to individuals with unique names in the country – about half of the adult population.[19] We identify this sub-population by using the employment records and the register for Federal social programs (*CadUnico*), which together provide the name and tax identifier for 96% of the adult population, allowing us to measure the commonness of each name in the country.[20]

To assess selection into the estimation sample, we compare characteristics of male and female job losers with and without unique names. The two groups are very similar in all (observable) dimensions, the standardized difference remaining below 0.25 for all variables, indicating that any differences in the underlying distributions are small (Imbens and Rubin, 2015) (see Appendix Table A1). In any event, in Section 4.5, we will assess the sensitivity of our results by retaining all individuals with a unique name within the state (rather than the country), which extends coverage to 70% of the population. We will also show that our main results are robust to reweighting our working sample to perfectly match the characteristics of the entire population of displaced workers.

Household, public shelter and health systems data. 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 social registry maintained by the Federal government for administering welfare programs such as Bolsa Familia. Due to the nature of the registry, it mainly overlaps with the lower and middle part of the income distribution in our main panel. To validate our main results, we will also use data on access to DV public shelters by women and mandatory DV notifications by health providers as alternative measures for domestic violence (see Section 4.4).

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

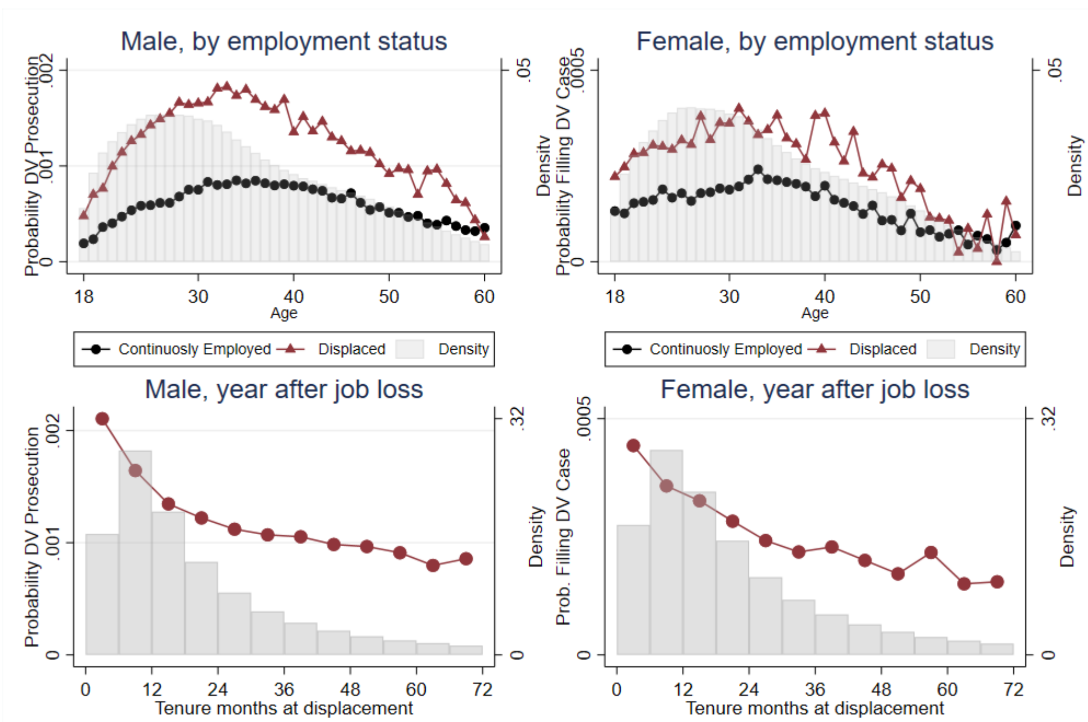
[20]This coverage rate is derived by comparing the total number of individuals in our registry with that of national population statistics, supplied by the Brazilian Institute of Geography and Statistics (IBGE). Restricting attention to adult individuals does not generate measurement error, because we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

4 Job Loss and Domestic Violence

4.1 Descriptive evidence

The upper panel of Figure 3 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 3 show that the probability of DV perpetration or victimization upon job loss is decreasing in job tenure, an association that we will investigate further. These graphs also illustrate that many jobs are terminated with low tenure, which is in line with the high turnover rate in the Brazilian labor market.

Figure 3: 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.

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. We use a perfectly balanced panel tracking units from three before to four years after treatment, and estimate anticipation and dynamic treatment effects throughout the same period.

The pool of potential control workers includes all individuals employed in firms that did not engage in mass layoffs during the analysis period. We leverage the vastness of the data to identify control workers who are not displaced in the same calendar year and are exactly matched on birth cohort, job tenure (by year), earnings category (by R$250/month bins), firm size (quartiles), one-digit 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.[21] The matching process is run separately for men and women, with over 80% of displaced workers being successfully matched to a control, who receives a placebo dismissal date equal to the layoff date of the matched treated worker. We compare changes in outcomes among treated and control workers, before and after dismissal, using the following difference-in-differences equation:

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

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

The stacking approach centering the analysis around the treatment timing and using never-treated workers as controls addresses concerns regarding the estimation of two-way fixed-effects models with staggered treatment across units. This follows

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

Britto, Pinotti and Sampaio (2022) and Cengiz et al. (2019), and is line with the recent methodological work by Dube et al. (2023). We also show that our results are robust to other estimator proposed in the recent methodological literature and 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:

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

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

The difference-in-differences design compares the same workers before and after job loss, ensuring that individual fixed factors, such as age, education and characteristics of the job lost, do not directly affect the estimates.[22] The key purpose of our exact matching strategy is finding a suitable control group that replicates the evolution of outcomes for the treatment group in the pre-displacement period, so that the common-trend assumption is supported. Table 1 shows that treated and (matched) control workers are fairly balanced on a rich set of observable characteristics. The standardized difference between the two groups remain below 0.25 (Imbens and Rubin, 2015) indicating that any differences in the underlying distribution are small for all variables (including several attributes not used for matching such as race, occupation, municipality characteristics and the probability of DV in the pre-displacement period). The only exception is education in the male worker sample – treated workers have 10.0 less years of education relative to 10.9 in the control group. In Section 4.5, we show that our main results remain robust to adding education to the matching process, and to reweighting the control group to perfectly match the characteristics of the treatment group. Table 1 also show that the characteristics of workers displaced in mass layoffs are not largely different from the pool of all displaced workers. This attenuates concerns regarding the external validity of our analysis based on mass layoffs – we address this potential issue in details in Section 4.5.

The main challenge to identification is dynamic selection into displacement. Parallel trends between treated and control workers in the pre-treatment period attenuate but do not fully address this concern, as idiosyncratic, time-varying shocks causing

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

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

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

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

higher DV and layoff risks in a given year may not be revealed in differential pre-trends. Our focus on mass layoffs minimizes this concern, as 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.[23] We provide several robustness tests for potential selection

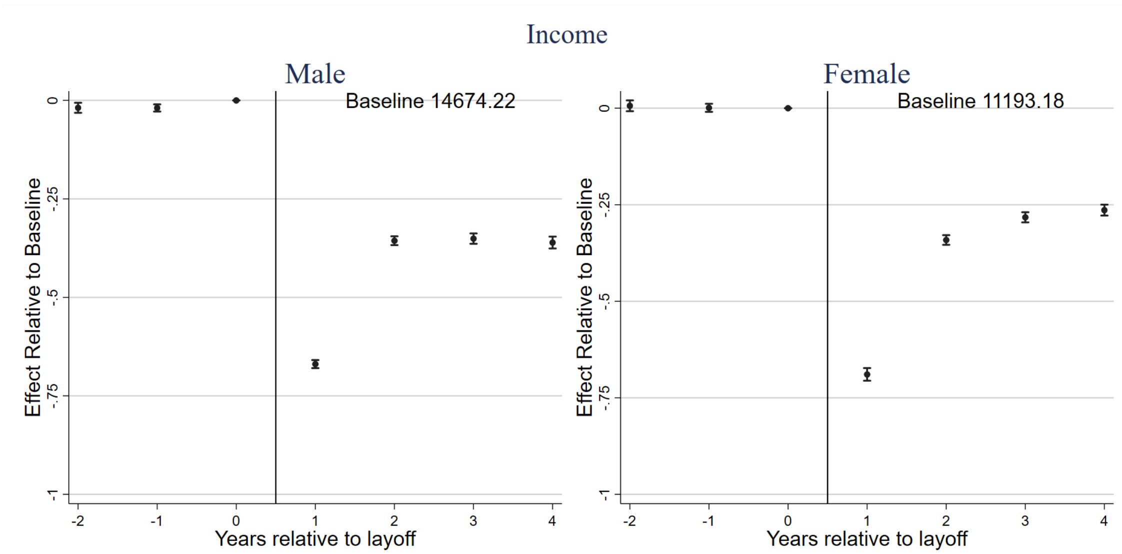
[23]This definition is similar to Jacobson, LaLonde and Sullivan (1993) and Couch and Placzek (2010). We also exclude firms reallocating under a new identifier, where reallocation is defined as at least 50% of workers displaced from a firm being found in a new firm by the start of the following year.

issues and extensively assess the sensitivity of the results to changes in the definition of mass layoffs in 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 4 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. The focus on relative effects is mainly motivated by the strong underreporting in DV outcomes, so that it is more meaningful to think about relative variations.

Figure 4: The effect of job loss on labor income

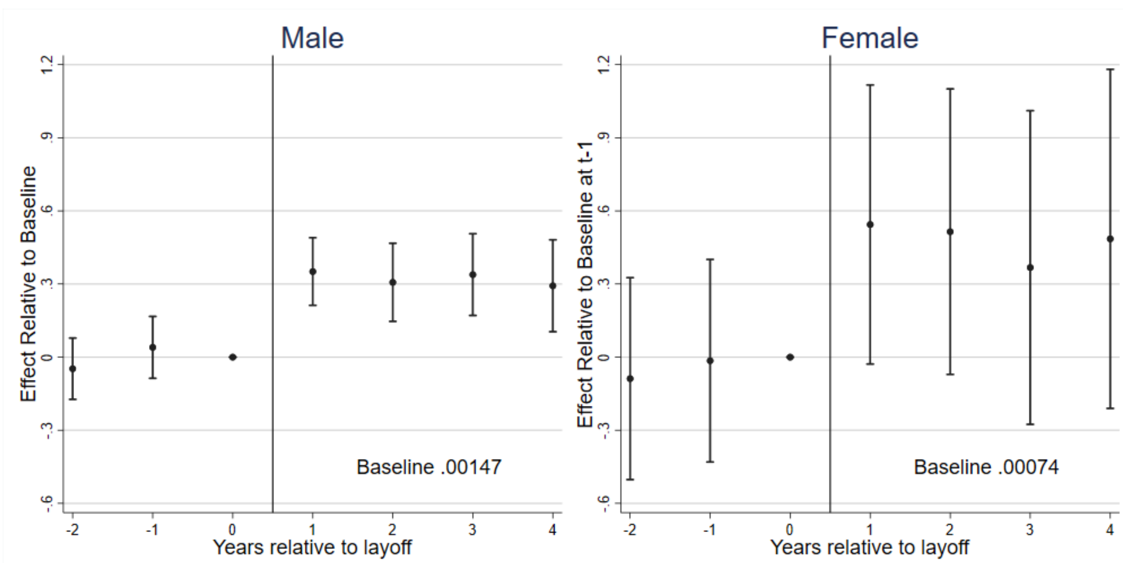


Notes This figure shows the effect of job loss on formal labor income by gender, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among workers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

Labor income is 70% lower relative to the baseline after male layoff, followed by a continuous but slow recovery in the subsequent years. Four years after the shock, the negative impact on labor income remains as high as 36%. The estimates are remarkably similar for women, as shown in the right panel of Figure 4. 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. Hence, the impact on total income remains substantial even when taking informal work into account. This also implies that our estimates for the elasticities of DV to formal 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 5, job loss causes a sharp increase in the probability of domestic violence in the year following job loss, which persists through the following years. The average effect over the post-treatment amounts to a 32% increase in the probability of DV relative to the baseline rate (Panel A of Table 2, column 3). When distinguishing between DV prosecutions and protective measures, the effect is +40% on the former and +30% on the latter (columns 4-5).

Figure 5: 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.

Turning to DV victimization, the right graph in Figure 5 shows that female job loss sharply increases victimization in the year following layoff, and that this effect persists for at least four years. The average effect indicates a 56% increase over the baseline

(Panel B in Table 2, column 5). The relative effect is larger than the effect of male job loss, although the samples are not based on exactly the same jurisdictions and the female job loss estimates are less precise, being estimated on a smaller sample (see Section 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).

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).

In Appendix B.4, we investigate the fact that the job loss effect on DV remains positive and sizable several years after the layoff. First, it shows that job loss causes a sustained increase in the probability of the first DV event, both for displaced men and women. This is consistent with the sustained labor market losses due to job loss (documented in Figure 4 and Appendix Figure B1). Second, it shows that male and female job loss also cause a sustained increase in the probability of recurrent DV. This indicates that once initiated DV tends to persist within couples, being in line with the fact that one fourth of perpetrators are charged more than once over the ten year period covered by our sample. In addition, these results also suggest that incapacitation due to reporting is low, being consistent overall with anecdotal evidence showing that only a small share of DV cases leads to conviction and prison.[24]

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

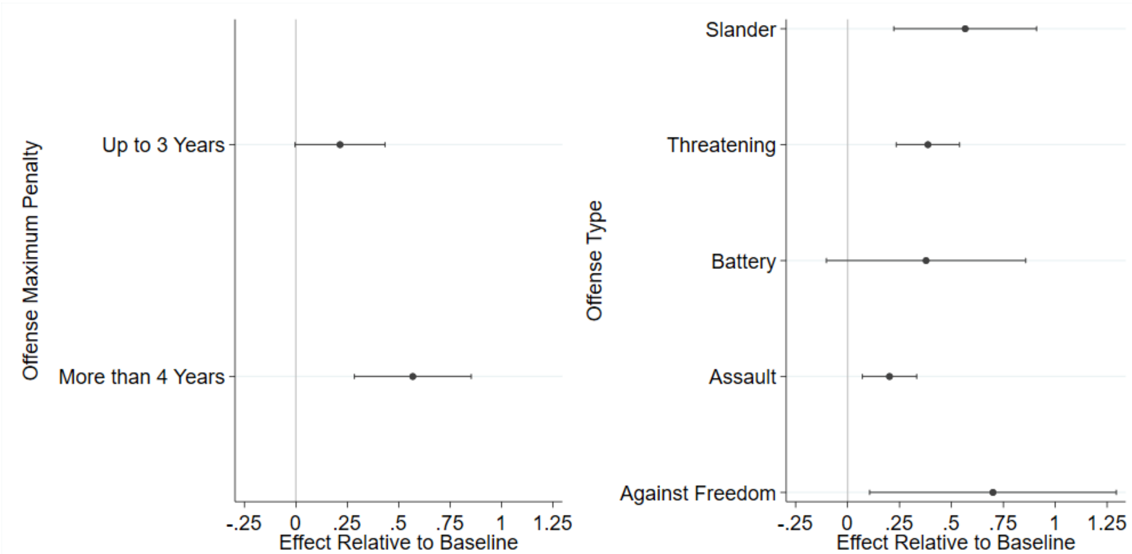
There is widespread under-reporting of DV on account of gendered norms, social stigma, concern for children, and the economic interdependence of the couple. In particular, if a woman is financially dependent on her partner, she might be more likely to report him for DV once he loses his job, which could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

We assess whether reporting bias drives our estimates for male job loss in two ways. First, we exploit variation in the intensity of violence, measured by the type of DV reported and jail time sentence. We expect that more severe offenses are less sensitive to reporting issues. Hence, if our findings were purely driven by changes in reporting behavior, one should expect the impacts to be driven by less severe offenses. On the contrary, Figure 6 shows that male job loss has stronger impacts on DV offenses leading to longer jail times (left graph), and that the effect is pervasive for all types of DV cases (right graph).[25]

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

[25]In the left graph of Figure 6, we distinguish between jail time sentences of up to 3 years vs. 4 or more years, respectively, because the Brazilian legislation classifies the former as mild crimes and the latter as ordinary crimes.

Figure 6: The effect of male job loss on domestic violence by offense intensity



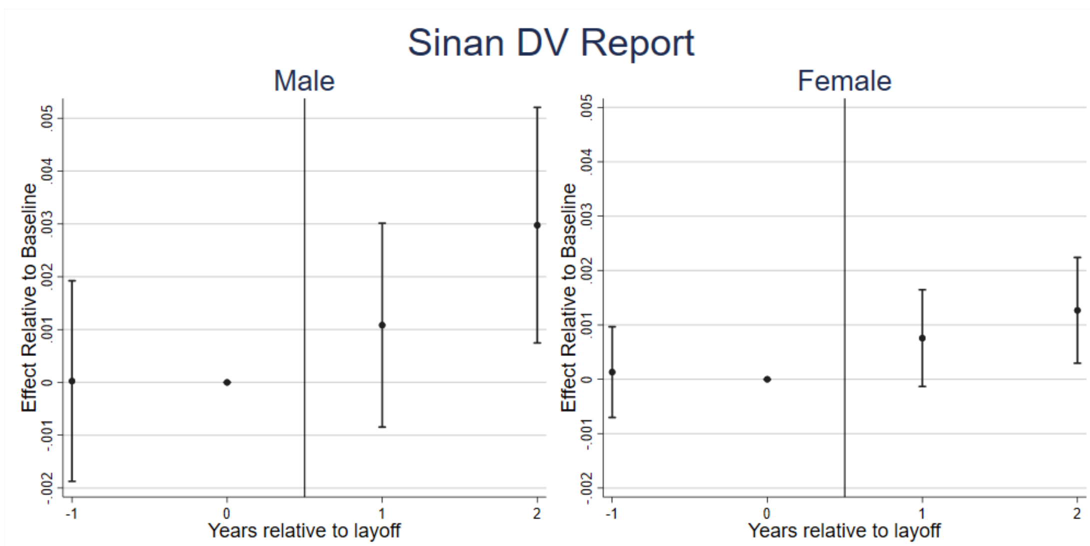
Notes. This figure shows the effect of male job loss on the probability of DV perpetration in DV suits by type and maximum penalty in the four years after the layoff, as estimated from the difference-in-differences equation (2) – along with 95% confidence intervals. The treatment group comprises workers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

Our second strategy is to replicate the analysis using alternative measures of DV that depend less, if at all, on discretion in reporting because they are reported by third parties. First, we 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).[26] 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 based on (clusters of) exact birth date, municipality, and gender; along with validation exercises. The results for DV notifications are presented in Figure 7. They confirm our main finding that both male and female job loss lead to an increase in DV. Appendix B.5.B provides robustness exercises and show that the effects relative to the baseline retain the same order of magnitude of our main analysis.

Figure 7: The effect of male and female job loss on domestic violence, health system DV notifications



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

Overall, results using three alternative DV measures that are less subject to re-

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

porting 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.

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

4.5 Robustness

XXX Maybe shorten up XXX

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

First, we show that our main estimates are robust to adding education to the matching process and to reweighting the control group to perfectly match all observable characteristics of the treatment group (Appendix B.6.A). This addresses the fact that there is some residual imbalance in education in our baseline matching strategy (Table 1). Second, estimates using quarterly data provide further support for the hypothesis of common pre-trends (Appendix B.6.B).[27] These results also confirm that impacts on DV emerge quickly after the layoff. Third, the results are robust to the inclusion of fine-grained municipality-industry-year fixed effects, indicating that our results are not driven by specific area-level employment trends and that using individual matched controls finely absorbs area level shocks (Appendix B.6.C).

Fourth, we address potential selection within mass layoffs by showing that the results remain robust when using stricter mass layoff definitions and plant closures, which severely reduce the room for selection into layoff. 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.D).

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

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

Seventh, we address the fact that our main analysis is restricted to individuals who have unique names in the country. In addition to showing that there is no strong selection over name uniqueness (Table A1), our findings remain similar when increasing the representativeness of our sample in different ways. Eighth, we address related to

[27]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).

staggered treatment in difference-in-differences designs(see, e.g. , Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). Finally, we address the fact that we study a low probability outcome with a difference-in-differences design. We show the predicted counterfactual probabilities remain in the range zero-one, and that our main findings are robust to different DID estimators well suited to deal such outcome.

4.6 Couples Data

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*, which 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 of separation, while female job loss has a small impact on this outcome. In addition, 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. Finally, we show that male and female job loss have no significant impact on partner's employment. The latter results are in line with the idea that partners spend more time together after displacement, supporting the exposure mechanism.

XXX Additional results on DV events and separations XXX XXX Additional stats on separations in Brazil and the world XXX

4.7 Mechanisms and relation to previous work

The positive effects of male and female job loss on DV in Table 2 cannot be explained by common theoretical constructs in the DV literature. They generally focus on the relative position of men and women in the couple and, in contrast to our findings, predict that male and female job loss have opposite effects on DV. For instance, the household bargaining model predicts that male job loss decreases men's bargaining power leading to lower DV, and that female job loss has the opposite effect. The male backlash, instrumental control and sabotage models referenced earlier predict just the opposite: male job loss should lead to higher DV, while female job loss should reduce

DV.[28]

We propose a simple model that explains the increase in DV after male and female job loss based on two mechanisms: income and exposure. The two-period model describes a representative couple composed of a male and a female partner who are a potential perpetrator and victim, respectively. Each partner starts the model either employed or in a state of job loss with some probability (independent for each partner). We model DV risk as a function of consumption and hours worked by each partner: $\phi_t(c_{mt}, c_{ft}, h_{mt}, h_{ft})$ where c_{it} is consumption and h_{it} is labor supply by partner i in period t. We assume that DV risk is decreasing in consumption and in labor supply. The idea is that lower consumption increases DV risk because it leads to higher stress – *the income mechanism*. Such mechanism is present even if partners do not pool income, since higher stress by one partner may be enough for DV to emerge. Increasing stress may arise as a direct consequence of lower consumption or because lower consumption increases the competition for resources in the couple. This mechanisms follows the spirit of Card and Dahl (2011) who models DV as a consequence of emotional cues. In Section 4.8, we will provide some evidence on the role of the income mechanisms leveraging variation in access to severance pay upon job loss. In turn, DV risk increases as partners work less, since they have more time for potential interactions among each other – *the exposure mechanism*. In Appendix B.8, we use survey data to provide suggestive evidence that partners spend more time together when out of a job. These results offer some support for the underlying behind the exposure mechanisms.

XXX Relation to DV literature: covid papers exposure XXX

In Appendix B.9, we provide a formal description of the model and two key predictions. First, we show that the model predicts higher DV risk upon job loss. This is because of the income mechanism – as job loss leads to lower income and consumption – and the exposure mechanism – as job loss leads to lower labor supply, increasing exposure to DV. Second, we use the model to generate predictions about the impacts of unemployment benefits on DV risk. The model has different predictions for such

[28] 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, for most people, variations in area-level unemployment rates represent an *expected* change in the risk of unemployment, 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.

effects during the benefit period, while workers are receiving the transfer, and for the subsequent period after benefits cease. During the benefit period, the transfers increase consumption, reducing DV risk because of the income mechanism. However, unemployment benefits lead to lower labor supply which increases DV risk because of the exposure mechanism. The overall impact on DV then depends on the relative strengh of each mechanism. After benefits cease, only the later mechanisms remains as the effects of unemployment benefits on labor supply are persistent, and the model predicts an increase in DV risk. We will empirically investigate some of our assumptions and test for these predictions in Section 5.

In Appendix B.10, we investigate whether the take-up of informal job after formal layoff could play a role explaining our findings, as an additional mechanism. In particular, since these jobs could be more risk and stressful, they could be a driver of higher DV. However, we show that our results remain similar when focusing on workers who are less exposed to labor informality due to their location and sector of work.

4.8 Heterogeneity by Worker and Area Characteristics

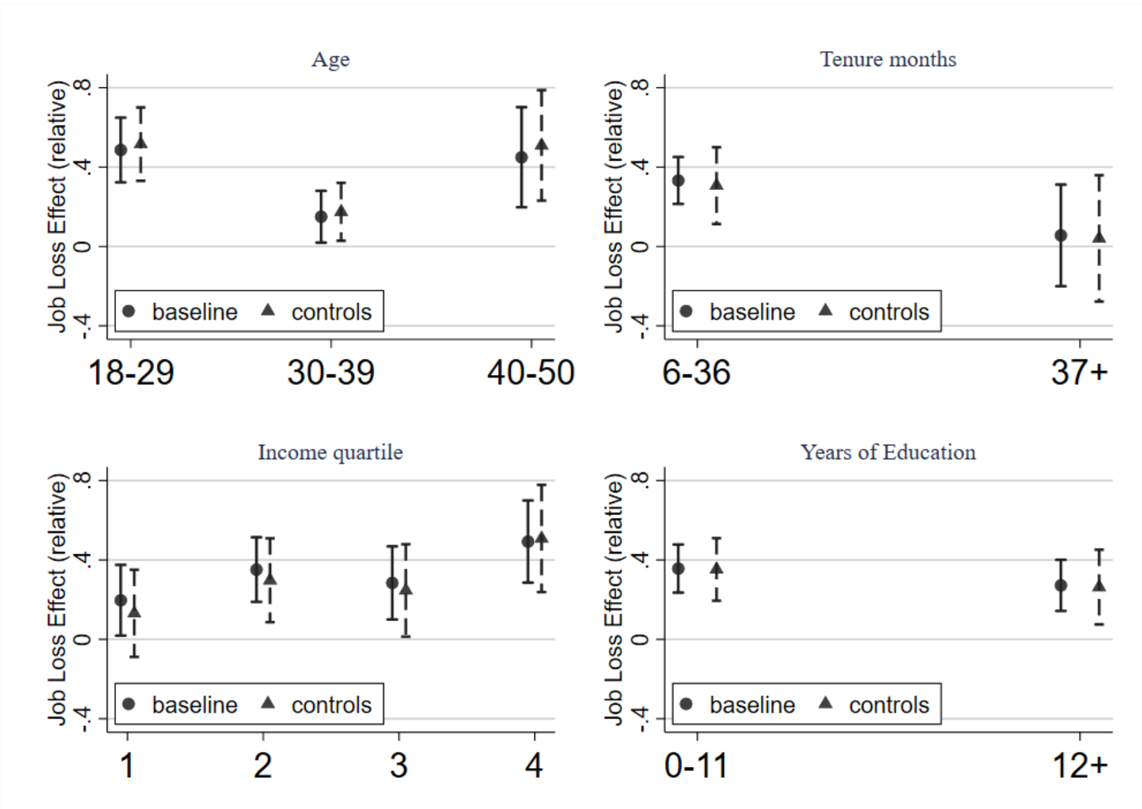
We now investigate how impacts of job loss on DV vary by worker characteristics, namely age, education, income and tenure at displacement. We focus on male job loss because it is difficult to derive meaningful comparisons in the smaller sample of female layoffs as, once we create sub-groups, the estimates are imprecise. Since the worker characteristics are correlated with one another, we also estimate models in which all coefficients in the equation (2) are interacted with third-order polynomial controls on other individual-level characteristics.[29]

The first striking pattern in Figure 8 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 B9, we also show that the effect is pervasive across a range of

[29] Namely, when running the heterogeneity over characteristic c, we control for third-order polynomials on each other characteristic $-c$ using continuous variables which are interacted with all coefficients in our baseline DID model in equation (2). For example, when analyzing heterogeneity over age groups, we interact all coefficients in eq. (2) with dummies indicating age groups and, as controls, with third-order polynomials on all other (demeaned) characteristics (tenure, income, education, and nine area-level characteristics – see Appendix Figure B9).

area-level characteristics – including baseline DV levels, the gender pay gap, informality rates and GDP per capita, despite the vast heterogeneity across Brazilian regions. In turn, Figures B10 and B11 in the Appendix show that impacts on labor income are largely similar over the same set of individual and area-level characteristics.

Figure 8: 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.

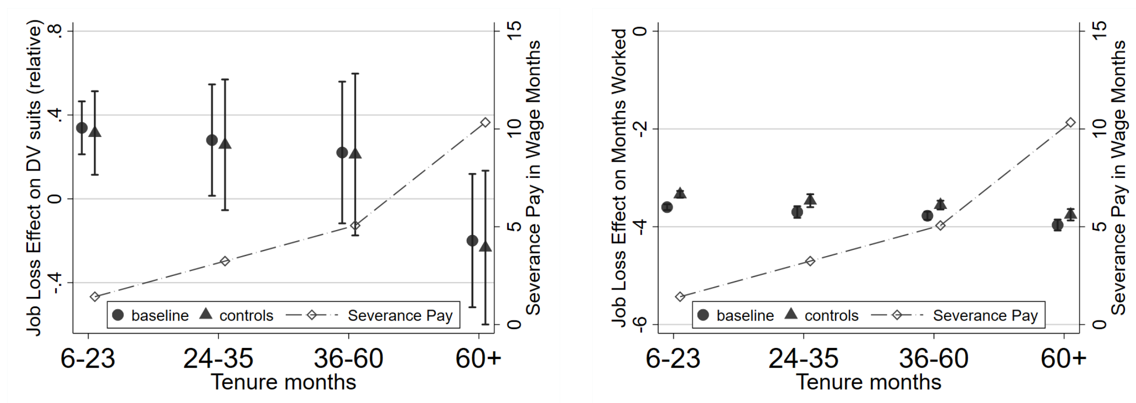
To provide evidence on the income mechanism, we analyze heterogeneous effects over tenure, exploiting the fact that severance pay is increasing in tenure. In the top-right panel of Figure 8, we compare workers displaced with 3 or more years in the job – who receive on average 7 months' wages in severance pay – to those displaced with 6-36 months tenure – who receive on average less than 2 months' wages.[30] Job loss raises DV for workers with 6-36 months tenure but it has a small and statistically insignificant impact on DV among high tenure workers, suggesting that liquidity at displacement may be a mechanism driving DV (Appendix Figure B12 shows dynamic effects for the two groups, revealing the same patterns). The specification with controls shows the same pattern, indicating that differential effects by tenure are not capturing differential effects by age, education, and income; or other measurable area-level factors. These results are in sharp contrast to all other dimensions of heterogeneity showing pervasive effects of job loss on DV.

Next we show how the impacts on DV vary by more granular tenure groups. Figure 9a shows that the effect of job loss on DV is decreasing over tenure, and that the intensity of the gradient mirror the average amount of severance pay, which is increasing over tenure. There is a considerably smaller and statistically insignificant effect for high tenure workers entitled to receive large sums of severance pay. In turn, Figure 9b shows that impacts on months worked do not greatly vary over the same dimension. This indicates that the null effect on DV among high-tenure workers is not a result of these workers finding work more readily than low tenure workers. Hence, this evidence does not support time availability (or exposure) as an explanation for 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.

[30] 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. We focus on workers with at least 6 tenure months who meet the eligibility requirement for UI. We analyse UI impacts in Section 5.

Figure 9: The effect of male job loss on domestic violence and months worked, and access to severance pay by tenure groups

(a) DV Suits **(b)** Months Worked



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

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.[31] Our goal is two fold. The first is evaluating the impacts of the policy which is the most common instrument supporting displaced workers around the world. The second is gathering further evidence on the predictive power of our DV model and the mechanisms we propose.

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

[31]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).

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

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).[33] 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

[32]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.

[33]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 B5 showed robustness of the main results to using the state level restriction.

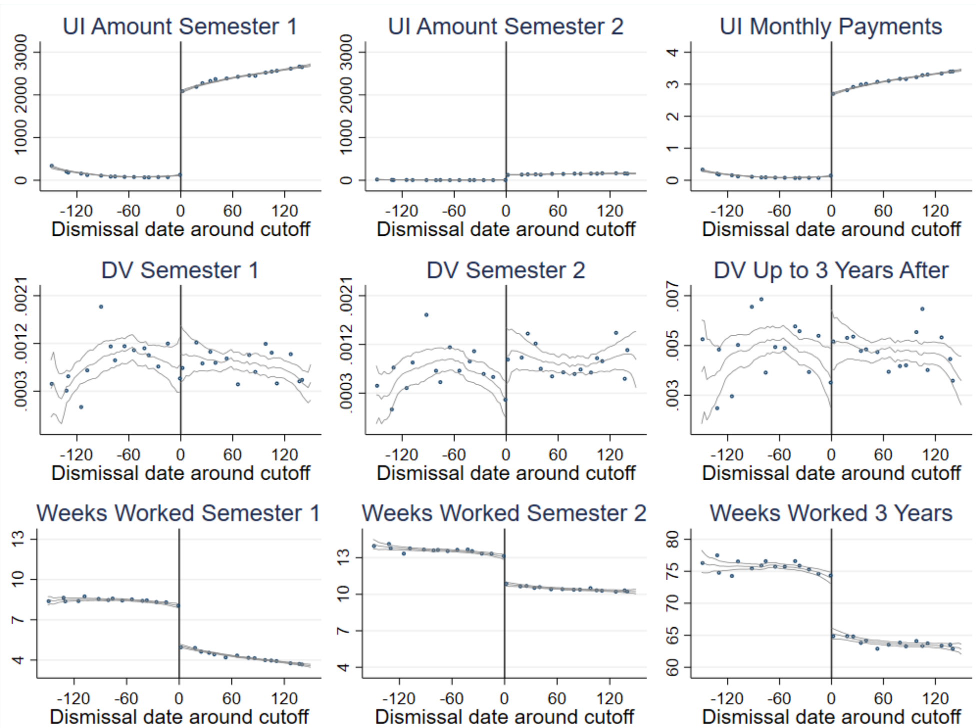
Figure C1) generate discontinuities in the density of the running variable about every 30 days that are not specific to the 16-month cutoff.[34] In our baseline specification, we address this issue by restricting the sample to workers initially dismissed between the 3^{rd} and 27^{th} of the month, so 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 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

Figure 10 shows that workers barely meeting the 16-month requirement have higher access to UI transfers. The additional transfers ares paid out during the first semester after layoff and are worth about R$2,000 (equivalent to 2.5 UI monthly payments, or 1.5 pre-displacement monthly wages). In the second semester after layoff, the gap in UI transfers around the cutoff is virtually eliminated. These effects are quantified in columns 1-3 of Table 3, Panel A, which also shows a 57% impact on UI take-up rates (column 4).

[34] 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.

Figure 10: 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, center row, graphically shows our main results on DV. Access to unemployment does not affect DV in the first semester after layoff, and it *increases* DV risk in the second semester following layoff, after benefit payments cease. This is confirmed in Table 3, Panel B, which shows a null effect in the first semester and a statistically significant positive effect in the following semester. In a three-year period, UI eligibility increases the probability of facing a DV lawsuit by almost a third. The adverse impact on DV in the second semester is robust to alternative bandwidths and polynomials in the running variable (Appendix Table 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 that they may, in fact, increase it after benefit expiration.

Finally, the bottom row of Figure 10 and Panel D of Table 3 show that eligible men work 8.6 weeks less in the 3 years after layoff, which is equivalent to a 11.5% reduction over the mean. These findings are in line with a large literature showing negative effects of UI on labor supply (see, among others, Katz and Meyer, 1990; Card, Chetty and Weber, 2007; Lalive, 2008; Gerard and Gonzaga, 2021).

Table 3: Effect of UI eligibility, male workers

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

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

In Appendix Table C3, we compare the characteristics of workers in the RD sample with workers in the job analysis in Section 4. Although workers in the RD sample have by construction lower tenure, absolute and standardized differences indicate reasonably small differences across several other characteristics. Nevertheless, to gather evidence on how differences in underlying characteristics may affect our results, we re-estimate the RD analysis on DV after reweighting the sample so that it perfectly matches the characteristics of the job loss sample.[35] Appendix Table 4 shows that the results remain extremely similar to our baseline results in Table 3, Panel B. Next, we discuss the mechanisms in light of the overall results obtained in the job loss and UI analyses.

5.4 Discussion on mechanisms

The findings that UI eligibility has a null impact on DV during the benefit period and a positive impact on DV risk after benefits expire are in line with our DV model. They can be explained by the income and exposure mechanisms which compensate each other while benefits are paid out.[36] After UI transfers cease, DV risk increases because of the persistent impacts of UI on labor supply, which in turn increases the potential time spent together by partners. This is consistent with the dynamics of the effects of UI eligibility on employment and DV suits, displayed in Figure 11. In semester 1 following layoff, the exposure effect driven by lower employment is compensated by the UI transfers paid out in the same semester. During this period, eligible individuals work 2.97 weeks less, equivalent to a 35.8% decrease relative to the baseline (Table 3, Panel C, column 1).[37] The positive impacts on DV emerge in semester 2 when UI transfers cease, but higher exposure to DV is still present because the negative impacts on employment are still sizable. From semester 3, UI impacts on DV are again null because the employment gap closes up considerably, offsetting the exposure mechanism.

The fact that UI income effects are short-lived is consistent with evidence showing that UI beneficiaries do little consumption smoothing. They experience sharp drops in

[35] We use the entropy algorithm by Hainmueller (2012) to generate weights.

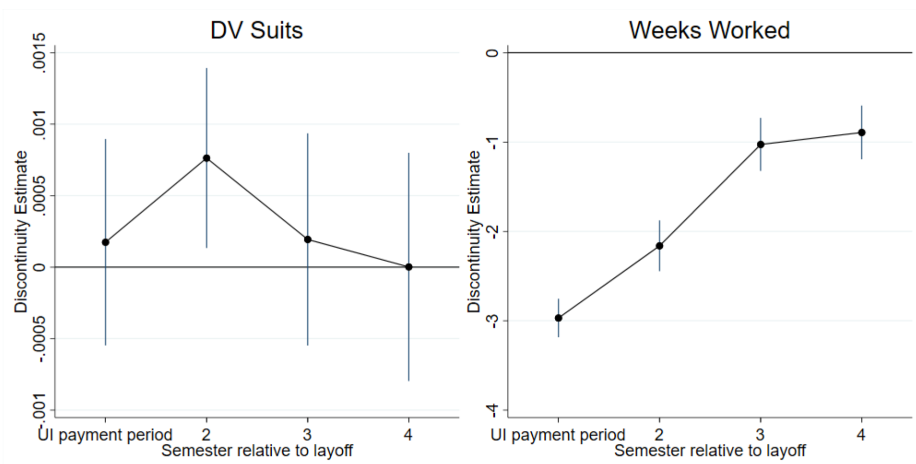
[36] Unemployment benefits were not conditional on attendance of training programs or minimum job search requirements in our analysis period. In 2012-14, there were attempts to condition benefits on attendance of training programs (PRONATEC). However, data from the Ministry of Labor show that only 1.2%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

[37] 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)

consumption upon benefit expiration – see Gerard and Naritomi (2021) and Ganong and Noel (2019) for evidence using Brazilian and US data, respectively. That income is a mechanism for DV is line with the evidence in Section 4 showing no job loss effect on DV for high tenure workers, who have great access to liquidity upon displacement.[38]

Overall, the income and exposures mechanisms are able to explain our key empirical findings, which cannot be explained by existing DV theories. Specifically, they can explain the increase in DV after either men or women lose their jobs, and why unemployment benefits backfire after transfer expire.

Figure 11: 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.

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

[38]Using a range of crimes, Britto, Pinotti and Sampaio (2022) 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.[39] 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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[39]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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A Appendix to Section 3

Table A1: Descriptive statistics by name uniqueness

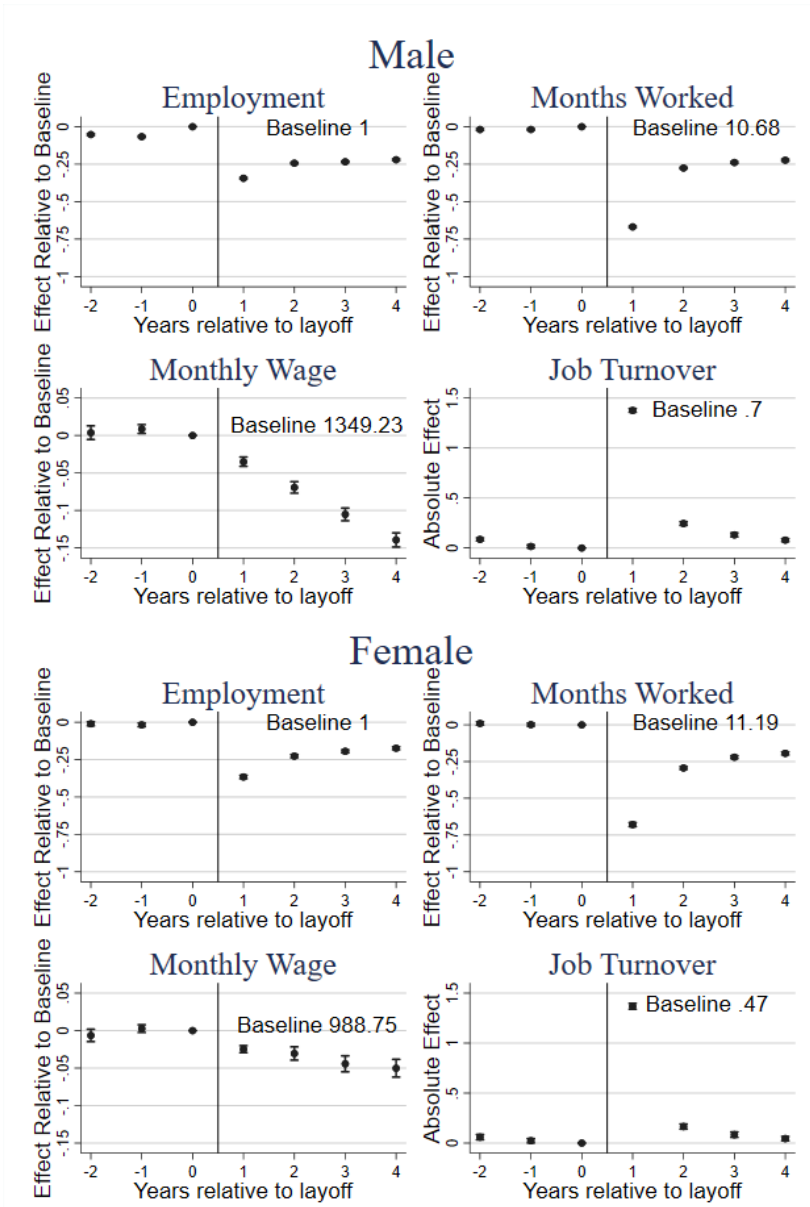
	Male			Female		
	Unique	Others	Std Diff	Unique	Others	Std Diff
Demographic characteristics						
Years of education	10.7	10.2	- 0.21	11.7	11.3	-0.19
Age	30.8	32.0	0.15	30.7	31.0	0.03
Race - white	51.7%	45.7%	- 0.12	60.0%	53.4%	-0.13
Race - black	4.9%	6.6%	0.07	3.6%	5.2%	0.08
Race - mixed	34.7%	39.2%	0.09	28.4%	33.5%	0.11
Job characteristics						
Monthly income (R$)	1,697	1,538	- 0.07	1,362	1,182	-0.11
Month of worked $t-1$	5.1	5.1	- 0.01	5.3	5.3	0.00
Tenure on Jan 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



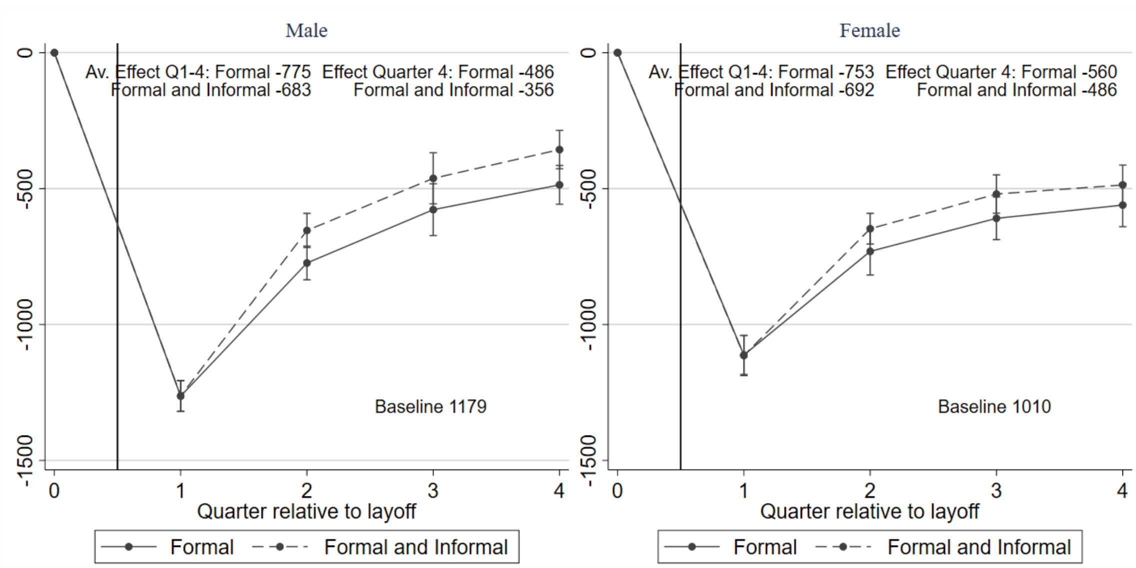
Notes. This figure shows the effect of job loss on formal employment outcomes by gender, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching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

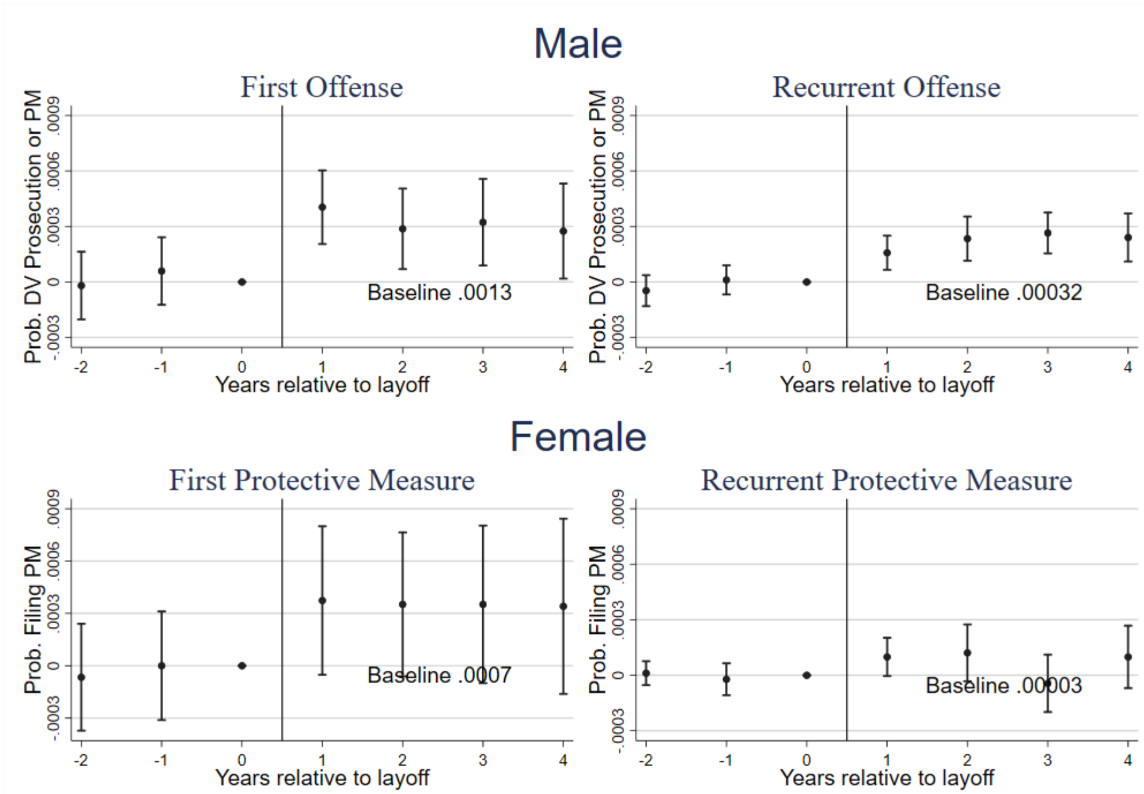
	(1)	(2)	(3)
	Labor market effects		Probability of DV
Dependent variable:	Employment	Income	Protective Measure
PANEL A: MALES DISPLACED IN MASS LAYOFFS, DV PERPETRATION			
Effect of job loss	-0.23***	-5423.3***	0.00044***
	(0.004)	(88.3)	(0.0001)
Mean outcome, treated at t=0	1	12,996	0.0006
Effect relative 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

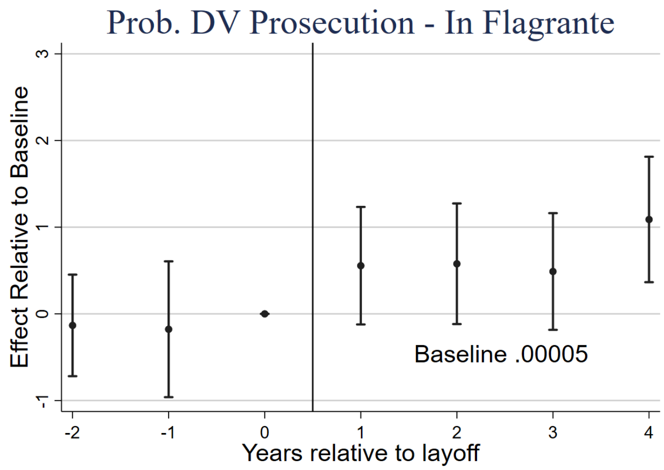


Notes. This figure shows the effect of job loss on the probability of DV prosecution for men and on the probability of filing a DV Protective measure for women, as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals. The treatment group comprises workers displaced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

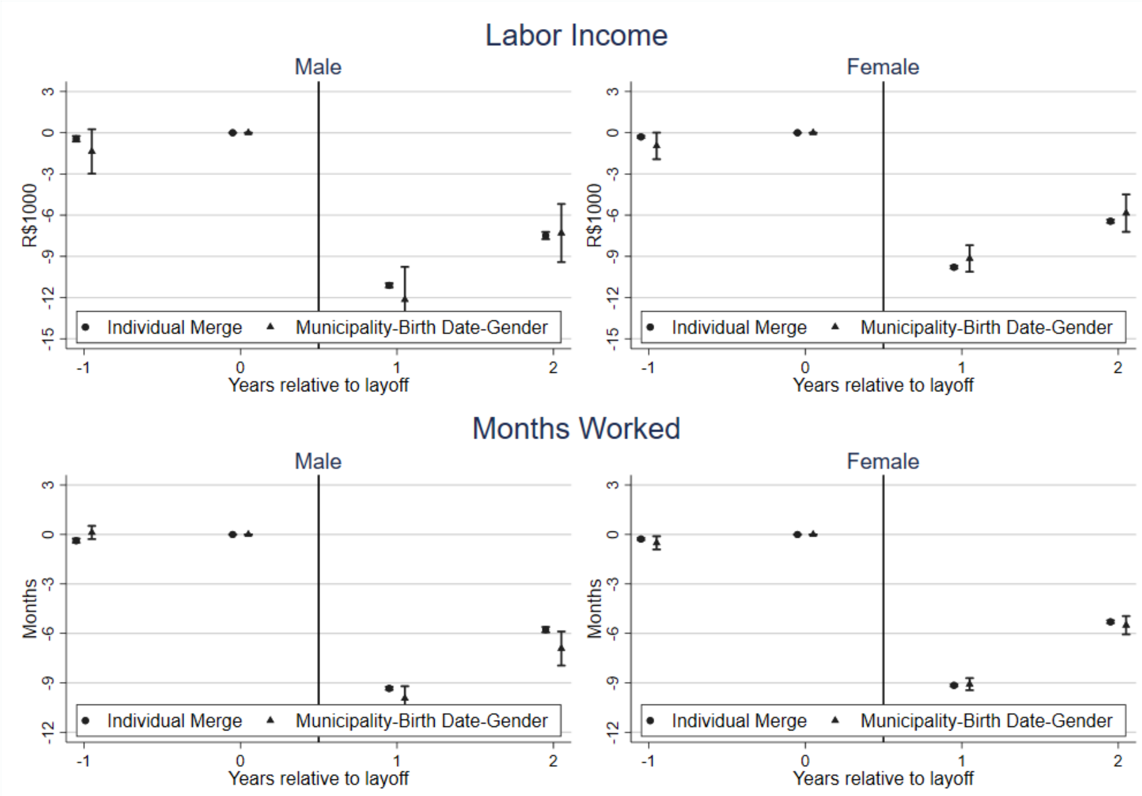
The key challenge in using the SINAN data is that unique individual identifiers are not available. To address this challenge, we link job losers (and their matched control workers) to health system DV notifications based on clusters defined by the individual's exact birth date, gender and municipality.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

Since this procedure necessarily entails some degree of measurement error, we extend the mass layoff sample to workers displaced in the period 2012-16, thus tracking SINAN reports for only two years before and after job loss, in order to increase statistical power. If we assume that measurement error is classical and exogenous to the post-treatment variable of interest, this will cause our estimates to be more imprecise but not biased (this situation is different from classical measurement error in a regressor, which leads to attenuation bias). Importantly, we validate this assumption and our data linkage procedure by replicating the estimation of employment effects of job loss. In other words, we use the same procedure to match employment outcomes, as if we did not have unique individual identifiers. Figure 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.

SINAN data are effectively available from 2010 to 2017. We drop observations for 2009, the implementation year of these reports, because coverage around the country was limited. The analysis of male job loss is restricted to the subsample of individuals observed in the social registry in 2011, for whom we can identify their cohabiting female partner and track them in SINAN – which covers information on the victims but does not identify the perpetrator. We drop cases for which the aggressor is identified as a relative (e.g., the victim's father or mother), while we keep cases for which the relationship with the aggressor is not reported (10% of all cases) so that our measure is not endogenous to the victim's willingness to identify the perpetrator. The results are presented in Figure 7 in the main text.

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

Figure 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.5.C Interactions between alternative DV measures

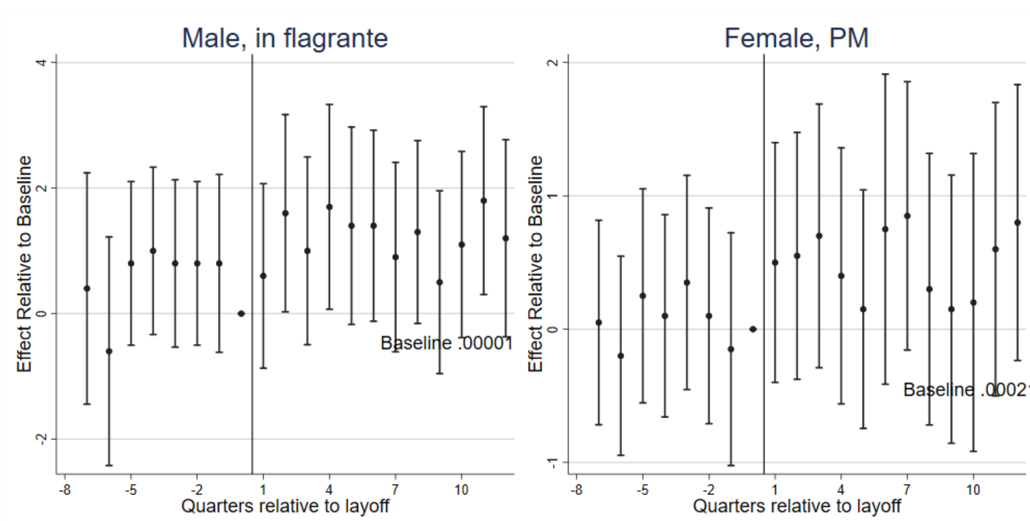
XXX Add Figure on PM and shelthr use around SINAN reports XXX

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).

Figure B6: Protective measures and DV Shelter use around DV events in SINAN reports



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 The effect of job loss on DV: Robustness

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

Table 1 shows that our exact matching approach creates a control group which is strongly similar to the treatment group in terms of several baseline characteristics, many of which are not included in the matching process. However, years of education is .9 years lower in the control group for the male job loss analysis. We now investigate whether such imbalance can affect our main results. First, we re-weight the control group to perfectly match the treatment groups over all characteristics displayed in Table 1. We use the entropy algorithm developed by Hainmueller (2012) setting the weights to perfectly match the first two moments of each these characteristics, so that any remaining imbalances in these observables are eliminated. Next, we re-estimate our difference-in-differences model using such weights. The results in Table B4, Panel A, show that estimates remain extremely similar to our baseline estimates (Table 2). Second, instead of using weights, we add education (XXX categories) to our original exact matching process and re-estimate our model. Also in this case, the results remain similar to our baseline estimates. Overall, these tests indicate remaining imbalances between the treatment and control groups are unlikely to threaten our main findings.

Table B4: Effect of job loss on labor market outcomes and domestic violence, eliminating differences in observables between the treatment and control groups

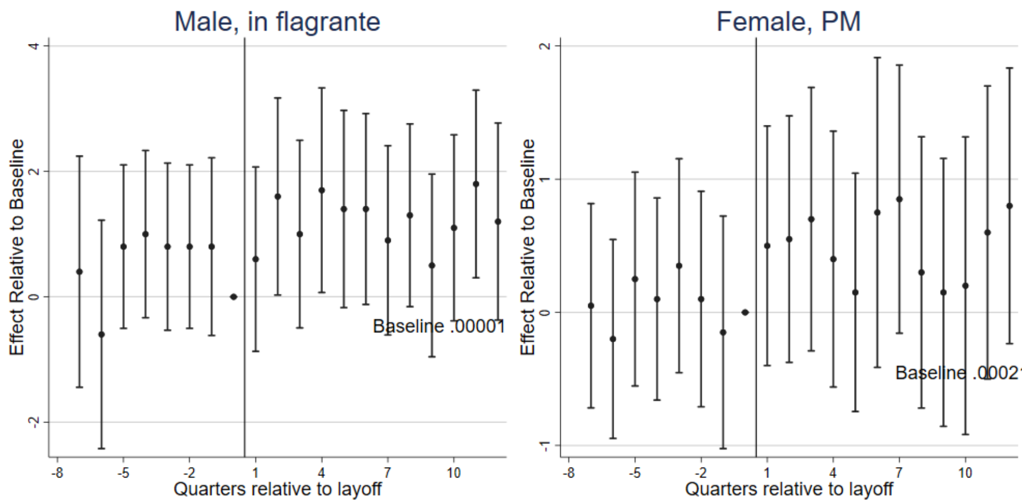
	(1)	(2)	(3)	(4)	(5)	(6)
Job loser	Male			Female		
	Labor market effects		Probability of DV	Labor market effects		Prob. of Filing
Dependent variable:	Employment	Income	Any	Employment	Income	Protective Measure
PANEL A: REWEIGHTING CONTROL GROUP TO PERFECTLY MATCH OBSERVABLES OF TREATED GROUP						
Effect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

Notes: This table shows the effect of job loss on DV perpetration/victimization outcomes, as estimated from the difference-in-differences equation (2). Panel A uses regression weights set to eliminate any differences in observable characteristics between the treatment and control groups. We use the entropy algorithm developed by Hainmueller (2012) to perfectly match the first two moments of observable characteristics up to the second moment. Panel B is based on a sample for which the control group is built as in our baseline, but adding education to the set of matching variables. The dependent variable is indicated on top of each column. The explanatory variable of 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.6.B Timing of violence: quarterly data

One concern regarding our main specification is that we measure violence timing based on when DV suits are filed rather than when the violence occurs. For instance, we could fail to detect diverging pre-trends in our analysis because of this lag. To address this concern in the male job loss analysis, we focus on 'in flagrante" cases, which offer a timely measurement of DV since they are immediately filed in courts (see section 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 "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 B7 show are similar pattern to our baseline estimates and present no evidence of diverging pre-trends.

Figure B7: 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.C Municipality-industry-year fixed effects

In the main analysis, matched controls are defined on state, 1-digit industry, and individual characteristics and this should difference out local shocks. Nevertheless, we show that our results are not sensitive to controlling for a more granular set of fixed effects, namely year $\times$ municipality$\times$2-digit industry. We also restrict the definition of potential (matched) controls to include only workers remaining employed through the entire post-displacement period, rather than only in the year that their treated match is dismissed. In all cases, our results remain similar, see panels A-B of Table B5.

B.6.D Dynamic selection into layoffs

We demonstrate robustness to a series of tests for dynamic selection, that allay the concern that displaced workers are selectively pre-disposed towards DV at the time of displacement even within mass layoffs. We vary the definition of mass layoffs from 33% up to 75%, while jointly varying the minimum size of firms in the sample from 30 to 70. The larger the fraction of workers dismissed, the more limited is the scope for selection into dismissal. Table B6 shows that the estimated effects of male and female job loss are broadly similar to the baseline estimates. Similarly, all results hold when restricting the treated group to workers in closing plants (Panel C, Table B5). The results are also robust to defining as treated all workers in mass layoff firms at the beginning of the calendar year when the mass layoff occurs, rather than just workers who are actually displaced. This approach avoids concerns regarding the selection of workers dismissed from downsizing firms. As it delivers an intention-to-treat estimate (analogous to estimates from randomized experiments with imperfect compliance), the relative effect is smaller (21% rather than 32%) but it is still statistically significant (Panel D, Table B5). In both Panels C and D the estimated elasticity of DV to earnings retains the same order of magnitude. We do not attach a causal interpretation to these elasticities as that would require that layoffs affect DV only through decreased earnings. In fact, layoffs can directly affect DV through other mechanisms such as exposure.

B.6.E Mass layoff spillovers

We study to which extent our results might be driven by the fact that they are based on mass layoffs, which could generate significant spillover effects. Namely, the fact that many workers are displacement at the same time could play a role and make our estimates significantly different from the impacts of regular layoffs, where workers are displaced in isolation. We investigate this matter by studying how our estimates change when varying the absolute number of displaced workers in the mass layoffs – see Table B7. If spillovers were the primary driver of our main results, one should expect smaller effects when using layoffs where a smaller number of co-workers are displaced. The table shows that our main estimates retain the same direction and order of magnitude when restricting the sample to mass layoffs where fewer workers from the same firm are displaced together. It also reports results obtained based on a different sample covering all layoffs in our data, but excluding mass layoffs (we repeat the same exact matching procedure to find control workers in this larger sample). Although results should be interpret with some caution because job loss is more likely to be endogenous in this sample, they still point at positive effects which retain the same order of magnitude relative to our baseline estimates based on mass layoffs. Other three pieces of evidence do not support the idea that mass layoff spillovers drive our main findings. First, as shown in previous results, our estimates retain the same direction and order of magnitude when focus on events where a higher share of the workforce is displaced and when studying larger firms where multiple workers are displaced at the same time (Table B6). In addition, the same holds when entirely focusing on plant closures (Table B5, Panel D). Second, Figure B.11 shows that our estimates are not driven by small municipalities where mass layoffs represent a larger share of the workforce and where, in principle, they should generate stronger spillovers. Third, we show in Table B8 that workers displaced in mass layoffs are not strongly different from the overall population of displaced workers. Overall, these several pieces of evidence do not support the idea that our results are primarily driven by spillover effects steaming from the fact that our analysis focuses on mass layoffs.

Table B5: Effect of job loss on domestic violence, robustness

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

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

Table B8: Descriptive statistics, mass layoffs vs. all layoffs

	(1)	(2)	(3)	(4)
	Male		Female	
	Mass layoffs	All layoffs	Mass layoffs	All layoffs
Demographic characteristics				
Years of education	10.0	10.9	11.5	11.7
Age	30.3	29.7	30.5	29.8
Race - white	41.8%	52.1%	46.6%	55.4%
Race - black	5.7%	4.7%	3.1%	2.8%
Race - mixed	43.8%	34.4%	39.0%	31.2%

Table B6: 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).

B.6.F Missing data on names

We now address selection concerns due to the fact that we cannot observe the alleged offender's identity in 40% of DV cases (prosecutions plus PM) and the victim's identity in 46% cases in our main analysis sample. Although the judicial records do not include any demographic information on victims or perpetrators, we start by providing some information on the extent of selection across cases with and without missing names. In Table B9, we show the distribution of offense types across the two groups. Even though some differences emerge, offense types do not sharply differ between the two

Table B7: Effect of job loss on domestic violence, robustness to mass layoffs spillovers

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

Notes: This table shows the effect of male (Panel A) and female (Panel B) job loss on the probability of DV perpetration/victimization using different samples, as estimated from the difference-in-differences equation (2). Columns 1-6 show the results when dropping from the sample mass layoffs in which more than a given number of workers from the same firm are displaced at the same time. Column 7 shows the results obtained when using a different sample covering all layoffs in the data, but excluding mass layoffs. The explanatory variable of 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).

groups. We also investigate to which extent missing name status is driven by court-level factors. A OLS regression with court-fixed effects shows that 49% and 58% of the variation in perpetrator and victims' missing name status are driven by court-level factors. Since our analysis leverage variation in layoffs within individuals working in the same area, this attenuates to some extent selection issues in our analysis.

We proceed with three robustness tests. First, we take advantage of the fact that the share of court cases with missing identity varies widely across states and jurisdictions, and show in Table B10 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.

Table B9: Distribution of offense type, by missing name status in judicial cases

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

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

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

We rely on individuals' full names to merge the employment data with the judicial records. To ensure precision in the merge procedure, we restrict our main sample to workers who have unique names in the country (about 50% of the Brazilian population). We now address the fact that this restriction could have implications for the representativeness of our sample. First, in Table A1, we have shown that individuals with unique names are reasonably similar to individuals without unique names, which mitigates selection concerns. Second, we run our analysis based on a matching procedure which significantly increases population coverage. Specifically, we restrict the data to workers who have unique names within their state of work (rather than in the entire country), and merge the employment data with the judicial records based on individuals' names and state. This increases sample coverage from 50% to about 70% of the population. The results in Table B11, Panel A, show that we find similar results using this extended sample. Third, we re-estimate our analysis after reweighting our sample to match the characteristics of all workers displaced in mass layoffs (with and without unique names), using the entropy algorithm by Hainmueller (2012). The results in Table B11, Panel B, remain similar to our main analysis. Overall, these exercises support the idea that selecting on unique names does not generate strong representativeness concerns for our main analysis.

Table B10: 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.6.H Estimation of dynamic treatment effects

Several recent studies highlight the challenges associated with estimating dynamic treatment effects in two-way fixed effects settings when there is variation in the treatment timing and treatment effects are heterogeneous across individuals. Under these conditions, some treated individuals might enter the double differences estimating the dynamic treatment effects with weights of opposite signs in different time periods. As a result, the estimated treatment effect differs from the average treatment effect, nor it is representative of any relevant population of interest (Sun and Abraham, 2020; Athey and Imbens, 2018; Borusyak and Jaravel, 2017; Callaway and Sant'Anna, 2020; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Imai and Kim, 2021) This problem is most severe when all or a large share of individuals in the sample are treated at some point. We overcome these issues by including a large share of never-treated workers. We estimate the share of units with negative weights following de Chaisemartin and D'Haultfœuille (2020), and find that no individual treatment effect receives a negative weight both for male and female job loss. It is worth noting

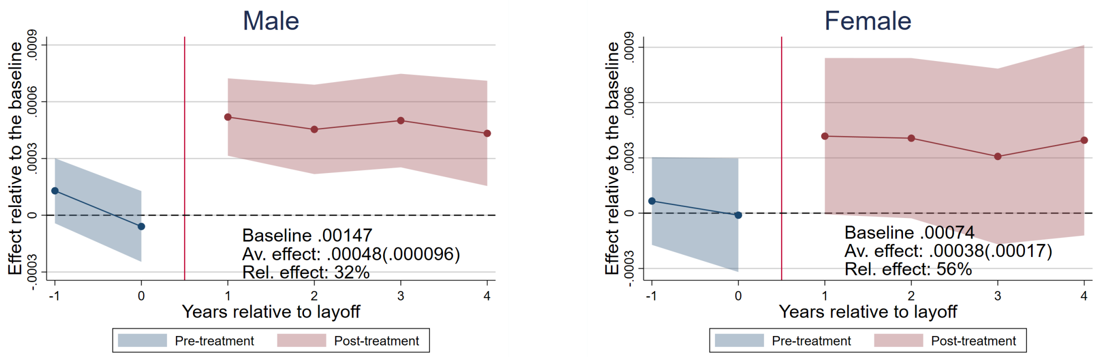
Table B11: Effect of job loss on domestic violence, robustness to sample representativeness due to unique name restriction

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

Notes: This table shows the effect of male (columns 1-3) and female (columns 4-6) job loss on the probability of DV perpetration/victimization, as estimated from the difference-in-differences equation (2). Panel A follows our main specification but restricting the data to workers with unique names in their state of work rather than in the entire country, increasing sample coverage. Panel B follows our main specification but reweighting the sample to match the characteristics of all workers displaced in mass layoffs, with and without unique names. Weights are obtained with the entropy algorithm by Hainmueller (2012). 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).

that our difference-in-differences estimator is similar in spirit to the strategies proposed in the recent literature, which mainly vary in the way of selecting the control group. In fact, we show in Figure B8 that our estimates remain similar when using the estimator proposed by Callaway and Sant'Anna (2020), which further supports the robustness of our main empirical strategy.

Figure B8: The effect of male and female job loss on domestic violence, judicial suits, Callaway and Sant'Anna (2020) estimator



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

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

XXX Decide whether to keep logit/probit regressions XXX

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

In any event, we implement the changes-in-changes estimator proposed by Athey and Imbens (2006), where the common-trend assumption refers to proportional changes in the probability of DV rather than level changes, as in our baseline approach. Under some additional assumptions (including conditional independence), this estimator is well suited to deal with probability outcomes. Table 12 shows that the results remain fairly similar to our main estimates. The same table also shows that using logistic or probit regressions yield results in the same direction (the table reports marginal effects at the mean) – following Blundell et al. (2004). Finally, we rescale DV risk in the treatment and control groups by their respective group-level baseline risk in the pre-treatment period, and re-estimate equation (2). In this approach, the common-trend assumption is no longer based on levels changes across groups, but on proportional changes. Accordingly, the results in column 4 indicate the proportional, relative effect of job loss on DV risk, which are comparable to our main estimates. Finally, in Table 13, we follow year another approach by re-estimating our main model for groups of workers with low and high ex-ante DV risk. We use a wide set of interacted fixed-effects, grouping observations by municipality of work, age, race, education and income level dummies, to generate these predictions. We also use a leave-one-out to avoid overfitting issues. The table shows that DV risk increases significantly both for low and high DV risk groups, and the effects retain the same order of magnitude relative to our main estimates. Baseline DV risk is about three and ten times larger in the high risk groups for the analyses on male and female job loss, respectively (although they remain low in absolute terms). This test is yet another way to address the potential issues of studying a low probability outcome in our difference in differences setting. Overall, all these alternative approaches support our main findings: they show positive effects of job loss on DV risk and coefficients indicate the same order of magnitude in terms of effect sizes.

Table 12: Effect of job loss on labor market outcomes and domestic violence, Athey and Imbens (2006) changes-in-changes estimator

	(1)	(2)	(3)	(4)
Dependent var.: Prob. of DV	changes-in-changes	logit	probit	rescaled outcome
PANEL A: MALE DISPLACED IN MASS LAYOFFS, DV PERPETRATION				
Effect of job loss	0.00055***	0.00044	0.00059*	0.19102**
	(0.00012)	(0.00035)	(0.00034)	(0.08134)
Mean outcome at t=-1 (treated)	0.0013	0.0013	0.0013	1
Effect relative to the mean	41%	33%	44%	19%
Observations	3,243,704	3,243,704	3,243,704	3,243,704
PANEL B: FEMALE DISPLACED IN MASS LAYOFFS, DV VICTIMIZATION				
Effect of job loss	0.0004**	0.00086**	0.00092**	0.91935***
	(0.00016)	(0.00042)	(0.00042)	(0.29706)
Mean outcome at t=-1 (treated)	0.0005	0.0005	0.0005	1
Effect relative to the mean	88%	189%	202%	92%
Observations	363,760	363,760	363,760	363,760

Notes: This table shows the effect of job loss on DV for male (Panel A) and female (Panel B) workers, using different methods. Column 1 is based on the changes-in-changes estimator by Athey and Imbens (2006); columns 2-3 on a logistic and probit regression (reporting marginal effects at the mean); and column 4 is based on eq. (2) after rescaling the outcome for treated and controls groups by their respective levels in the pre-treatment period. 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. The data is collapsed at at the average into a single pre and post-treatment period: 3 years before and 4 years after job loss. Bootstrap standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 13: Effect of job loss on labor market outcomes and domestic violence, by predicted DV risk

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

Notes: This table shows the effect of job loss on DV perpetration/victimization outcomes, for males in Panel A and females in Panel B, as estimated from the difference-in-differences equation (2). We split the samples according to predict DV risk in the pre-treatment period. Predictions are based on a set of interacted fixed-effects, by municipality of work, age, race, education and income level dummies. We use leave-one-out predictions to prevent overfitting issues. The dependent variable is indicated on top of each column. The explanatory variable of 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.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 B14. 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 B14, 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.

Next, we investigate couple stability for job losers who continue to show in the social registry in the post displacement period, see Table B14. Interpreting these estimates required some caution as the probability of continuing in the social registry increases after job loss, albeit this is arguably a small effect, ranging from 3% to 5% relative to the baseline (column 6). We find that job loss does not strongly affect the probability that job losers stay with the same partner after the layoff. Male job loss does not affect the probability that men retain the same partner, while female job loss reduces partner's stability by 3% relative to the baseline (column 7). Finally, in column 8, we show that partners' employment is not significantly affected by job loss, indicating that "added worker effects" are not sizable in our context. The latter supports the premise of our exposure mechanism that partners spend more time together following displacement.

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Table B14: Effect of male and female job loss on partners using household data

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

Notes: Columns 1-5 in this table show the effect of male job loss on DV victimization by the partner (Panel A) and the effect of female job loss on DV perpetration by the partner (Panel B), as estimated from the difference-in-differences equation (2). In both panels, the sample is restricted to displaced workers present in the social registry in 2011, for whom it is possible to identify the respective partner. Columns 6-7 presents the same results on the prob. that the worker is still present in the registry after the job loss and, if registered, that she/he has the same partner, while column 8 shows the results for the partners' employment probabilities. The explanatory variable of 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.8 Employment status and time partners spend together

We gather evidence on whether partners are more likely to spend more time together following job loss. Ideally, we would use time use survey along with information on job loss. Since such data is not available for Brazil, we rely on two household survey to provide such evidence. First, we use Pesquisa Nacional por Amostra de Domicílios (PNAD), a large nationally representative household survey. The survey indicates who is the survey respondent for each individual interview in the household. When the person is present at home, interviewers are instructed to collect the information directly from that person. We focus on individuals in the age range 20-50, in line with our main analysis, and study whether non-employment status correlates with responding to the survey in person, which we use as proxy for staying at home. Columns 1-2 in Table B15 show that men and women are 13 and 14 p.p. more likely to be the survey respondent when they are not employed. Second, we use the Pesquisa de Orçamentos Familiares (POF) for 2017, a nationally representative household survey conducted by IBGE. In addition to information on expenditures, the survey asks detailed questions on the timing of food consumption. We study whether partners who are not employed in the reference period (12 months) are more likely to consume food at home and to have the main daily meals (lunch and dinner) at home at the same time. More specifically, we use a dummy for whether food was consumed or prepared at home as proxy for whether a person is at home. Ideally, we would use information on whether food was consumed at home rather than prepared but such information is not available. We restrict attention to couples in ages 20 to 50 years old and analyze two main outcomes: (i) for each consumption hour, we track whether food was consumed or prepared at home; (ii), and we track the probability that both partners consumed or prepared food at home in the same hour of the day. Columns 3-6 in Table B15 shows that male and female partners who are not employed are 6.1-7.0 p.p. more likely to consume food at home, and 3.7-6.4 p.p. more likely to consume the main daily meals together. When running all regressions, we control for age (quadratic polynomial), and several fixed-effects: fine sampling regions, dividing Brazil in 300 thousands subareas, education, and race. Overall, these results support the idea that partners spend more time at home and more time together upon unemployment, and are in line with the exposure mechanism.

Table B15: Employment status and time spent together by intimate partners

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

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

B.9 Model

The model is based on a representative couple composed of two individuals $i = m, f$ and runs in two discrete periods $t = \{1, 2\}$. The male partner m is a potential perpetrator of DV violence, while the female partner f is the potential victim.

Each partner starts the model either in a state of employment E_{i1} with probability $1 - l_i$ or in a job loss state J_{i1} with probability l_i. The probability of each state is independent for each partner. If she/he starts the model employed ($E_i 1$), she/he remains employed until the end of the model in $t = 2$ ($E_i 2$). In the case job loss (J_{i1}), the partner searches for a new job with intensity s_i. Without loss of generality, s_i is normalized to the probability that she/he finds a new job. The continuous function ψ_i defines the utility cost of job search, assumed to be increasing and convex

$\psi_i' > 0, \psi_i'' > 0$. If job search is successful, she/he moves into the employment state in the same period 1 (E_{i1}) and remains employed in both periods. If job search is unsuccessful, the partner moves into the unemployment state in period 1 (U_{i1}), receiving UI benefits b_i lasting only for that period, and remains unemployed until period 2 (U_{i1}). In period 2, the unemployed partner earns subsistence income $y_i^s < b_i$ and no longer search for a job ($s_i = \phi(0) = 0$).

When employed in any period, partner i works a fix amount of hours h_i^E with wage rate w_i. For simplicity, we assume that individuals have a zero time discounting rate and consume all their income y_i in each period, i.e., there are no savings.In line with the fact that unemployment benefits replacement rate is generally lower than one, we assume that income is lower during unemployment ($b_i < w_i h_i^E$).

The probability of domestic violence in each period is given by the continuous function $\phi_t(c_{mt}, c_{ft}, h_{mt}, h_{ft})$ where c_{it} is consumption and h_{it} is working hours by partner i in period t. DV risk is decreasing in consumption and in working hours ($\delta\phi_t(.)/\delta x > 0$ for $x = \{c_{mt}, c_{ft}, h_{mt}, h_{ft}\}$). The idea is that lower consumption increases DV risk because it leads to higher stress – *the income mechanism.* It is worth noting that such mechanism may be present even if there is no income pooling among partners, since higher stress levels for one of the partners may be enough to increase DV risk. Increasing stress may arise as a direct consequence of lower consumption – in line with XX and XX – or because the latter increases the likelihood of conflict for resources in the couple. In turn, DV risk increases as partners work less, since they have more time for potential interactions among each other – *the exposure mechanism.* [40]

Let E_{it} and U_{it} define the value functions of partner i when employed and unemployed in period 1:

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

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

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

[40]For simplicity, we do not consider hours dedicated to job search as a relevant driver of exposure. This is based on the fact that unemployed workers dedicate few weekly hours to job search (e.g., see XX and XX) and because it is unclear whether such hours reduce exposure since a large part of job search can be done from home and does not necessarily decreases interactions among partners. Nevertheless, our findings would continue to hold if allowing job search to reduce exposure – there would be higher exposure if a partner is unemployed since hours of job search are low relative to working hours in a regular job, and unemployment benefits lead both to lower job search and employment probabilities.

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

The functions $v_i(.)$ and $u_i(.)$ define the utility of consumption during employment and unemployment, respectively, assumed to be continuous, increasing and concave ($u_i' > 0, u_i'' < 0$). The utility of the female partner f is decreasing in the DV risk ϕ_t. In turn, consumption for each partner i in period t is defined by a function $c_{it}(y_{mt}, y_{ft})$ defining how total income is split in the couple. The functions are increasing in the partner's own income, so that higher income by each partner increases her/his own consumption levels ($\delta c_{it}/\delta y_{it} > 0$). They also satisfy the budget constraint so that total consumption does not exceed income ($c_{mt}(y_{mt}, y_{ft}) + c_{ft}(y_{mt}, y_{ft}) = y_{mt} + y_{ft}$). These functions allow for varying degrees of income pooling, including a no income pooling scenario where $c_{it}(y_{mt}, y_{ft}) = y_{it}$. This setting allows for partial spousal insurance, but rules out full spousal insurance: holding constant the other partner's income, consumption levels by each partner when unemployed is lower relative to the situation where she/he is employed [$c_{i1}(\bar{w}_i\bar{h}_i, y_{-i}) > c_{i1}(b_i, y_{-i})$ and $c_{i2}(\bar{w}_i\bar{h}_i, y_{-i}) > c_{i2}(y_i^s, y_{-i})$]. This is in line with empirical evidence showing that job loss causes substantial reductions in consumption levels [Gerard and Naritomi (2021), Noel XX].

The utility of the partners when starting the model in job search J_{i1} is given by:

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

Proposition 1. Expected DV risk ϕ is greater in both periods $t = 1, 2$ when either partner has lost her/his job in period 1, taking as given the other partner's employment status.

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

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

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

This directly follows from the fact that DV risk is strictly decreasing in the consumption of both partners and in the working hours of the male partner. First, when the male partner is unemployed rather than employed, his consumption is strictly lower ($c_{mt}|U_{mt} < c_{mt}|E_{mt}$), and consumption of the female partner weakly lower ($c_{mt}|U_{mt} < c_{mt}|E_{mt}$). This follows from the fact that his income is lower during unemployment relative to employment. Second, exposure is higher during unemployment as the male partner works less hours ($h_f^E > 0$).

Now let $\phi_t|J_m1$ be DV risk given that the male partner has lost his job in period

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

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

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

We now analyze how access to unemployment benefits affects DV risk in periods 1 and 2, given that one of the partners has lost her/his job in period 1 (in line with our empirical analysis exploiting variation in UI benefits among displaced workers). One key result for this analysis is the fact the job search effort is decreasing on UI benefits generosity – this immediately follows in our standard job search model (see Appendix X below).

Proposition 2. Given that the partner is a job loser in period 1, a higher level of unemployment benefits has an ambiguous impact on DV risk during the period 1 – the benefit period – and it increases DV risk in period 2, after benefits expire.

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

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

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

Appendix X *Showing that job search is lower when unemployment benefits are higher* After job loss in period 1, the partner chooses job search effort s_i following the first-order condition (FOC):

$$E_i^1 - U_m^1 = \psi_i'(s_m)$$

Optimal search effort balances the benefits of being employed rather than unemployed (left-hand side) against the cost of job search (right-hand side). In line with the job search literature, a higher level of unemployment benefits b leads to lower job search, which follows from the implicitly deriving the FOC from above with respective to b_i:

$$\frac{\delta s_i}{\delta b_i} = \frac{-1}{\psi_i''(s_i)} U'^{1}_{i} < 0$$

B.10 The role of labor informality

In Section B.2, we show that labor income losses become about 10% smaller once we take into account reemployment in informal jobs. Hence, one possible explanation for our main finding is that workers take informal jobs, which might be more stressful due to the low job security or safety standards, which in turn could drive the observed increase in domestic violence. We gather evidence on this by studying how our results change across workers exposed to different degrees of labor informality. Previous results in Figure 8 shows some initial evidence on that for male job loss. In particular, they show that our main estimates are similar in areas with more and less labor informality. It is worth noting that informality rates greatly vary across Brazil, from XX in the State of XX to XX in the State of XX. In Table B16, we use a more fine grained informality measure, at the 2-digit sector by state level. The results show that our results retain the same direction and order of magnitude as we progressively exclude from the sample workers more exposed to labor informality, both for male and female job loss. Only one coefficient loses statistical significant because estimates become more imprecise due to the smaller sample size. Overall, these results do not support the idea that our main findings are primarily driven by exposure to informal jobs after formal displacement.

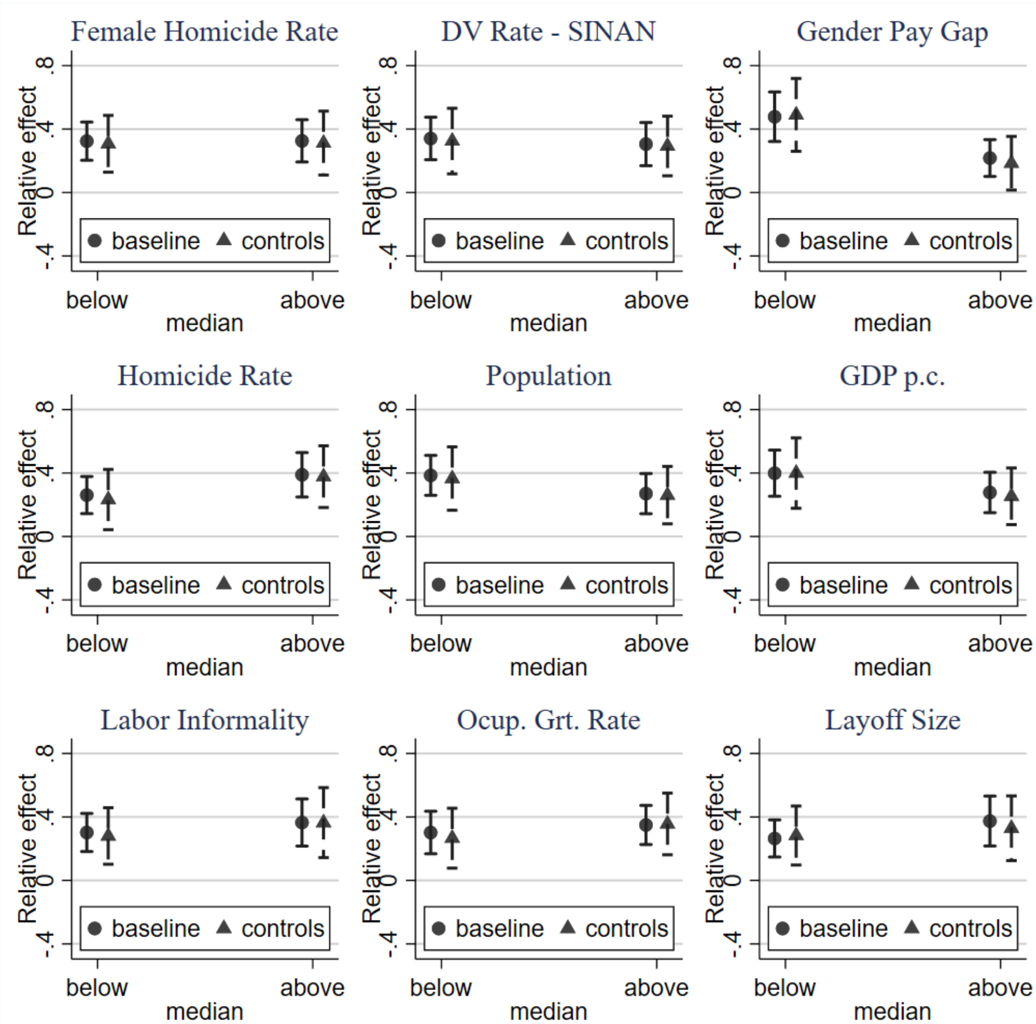
Table B16: Effect of job loss on domestic violence, the role of labor informality

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

Notes: This table shows the effect of male (Panel A) and female (Panel B) job loss on the probability of DV perpetration/victimization using different samples, as estimated from the difference-in-differences equation (2). Columns 1-4 show the results when dropping from the sample workers in 2-digit sectors-state cluster with informality rates above the X_{th} quartile. The explanatory variable of 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).

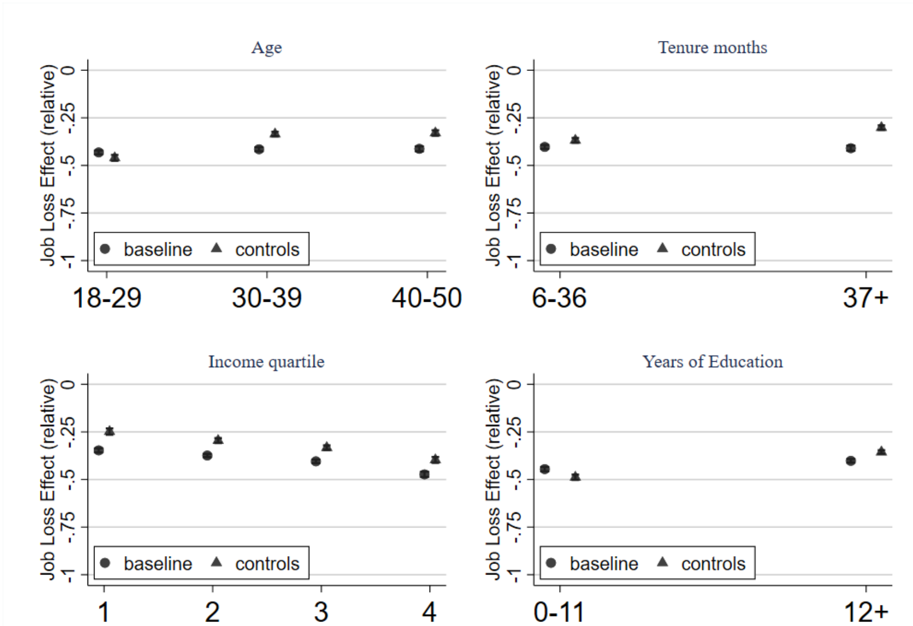
B.11 Area-level heterogeneity

Figure B9: The effect of male job loss on domestic violence, judicial suits, by area-level characteristics



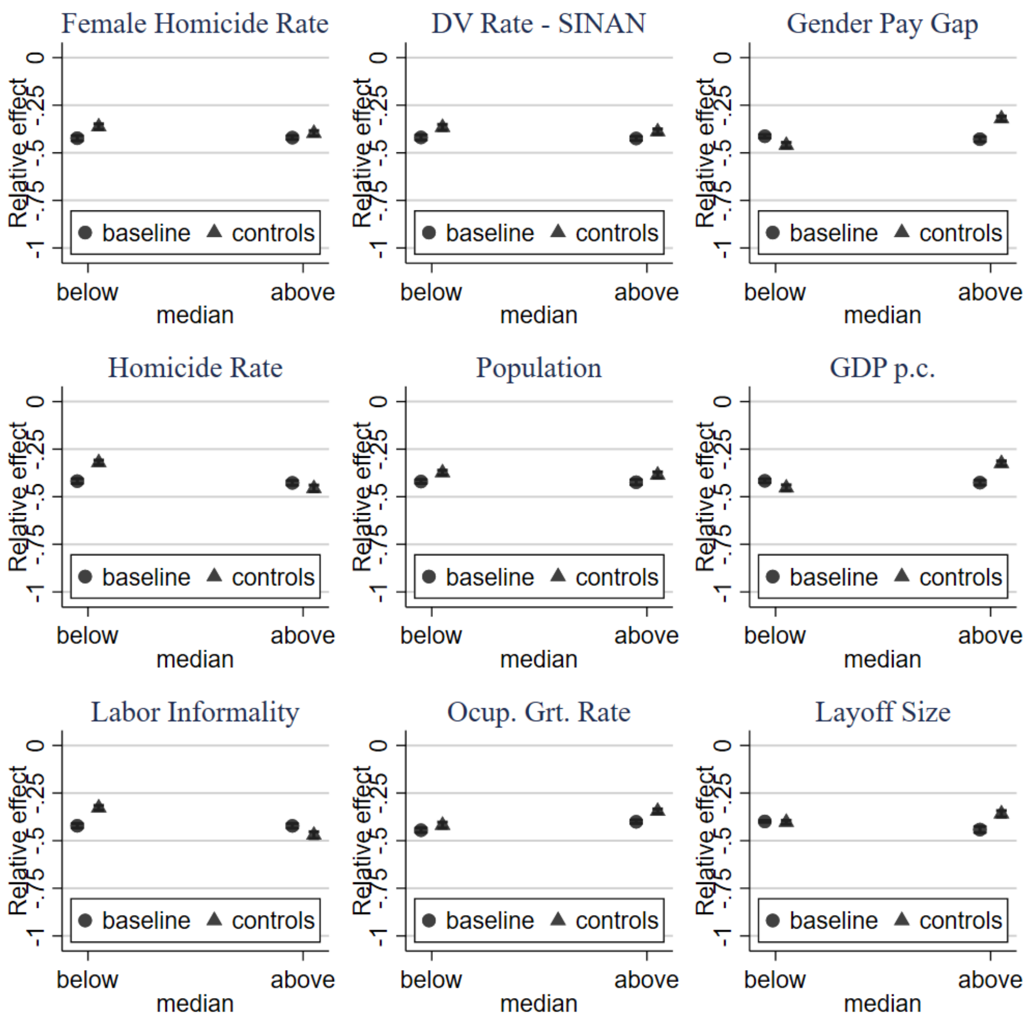
Notes. This figure shows the effect of male job loss on the probability of DV perpetration in DV suits in the four years after layoff, by area-level characteristics – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts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

Figure B10: The effect of male job loss on labor income by individual characteristics



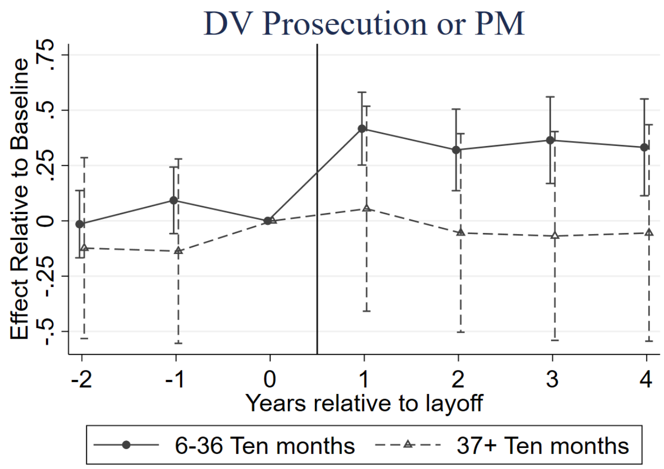
Notes. This figure shows the effect of male job loss on labor income in the four years after layoff – along with 95% confidence intervals. The baseline follows the difference-in-differences equation (2), while a second specification interacts all coefficients in the eq. with third-order polynomials on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

Figure B11: The effect of male job loss on labor income by area-level characteristics



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

Figure B12: The effect of male job loss on domestic violence by tenure

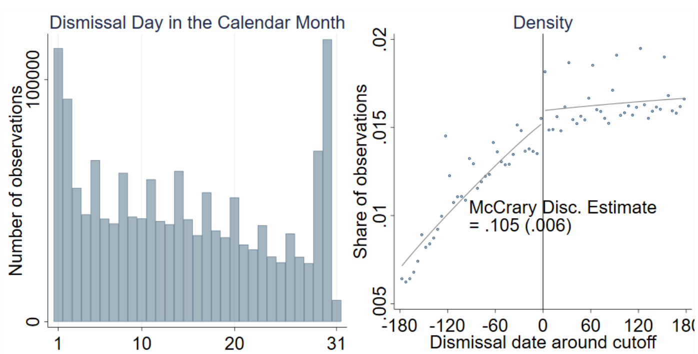


Notes. This figure shows the dynamic effects of male job loss on probability of DV perpetration in DV suits by tenure at displacement as in eq. (1) – along with 95% confidence intervals. The treatment group comprises workers displaced in mass layoffs, while the control group is defined via matching among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

C Appendix to Section 5

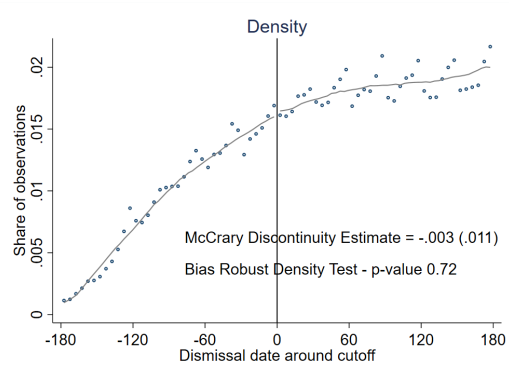
C.1 *Dismissal cycles*

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.

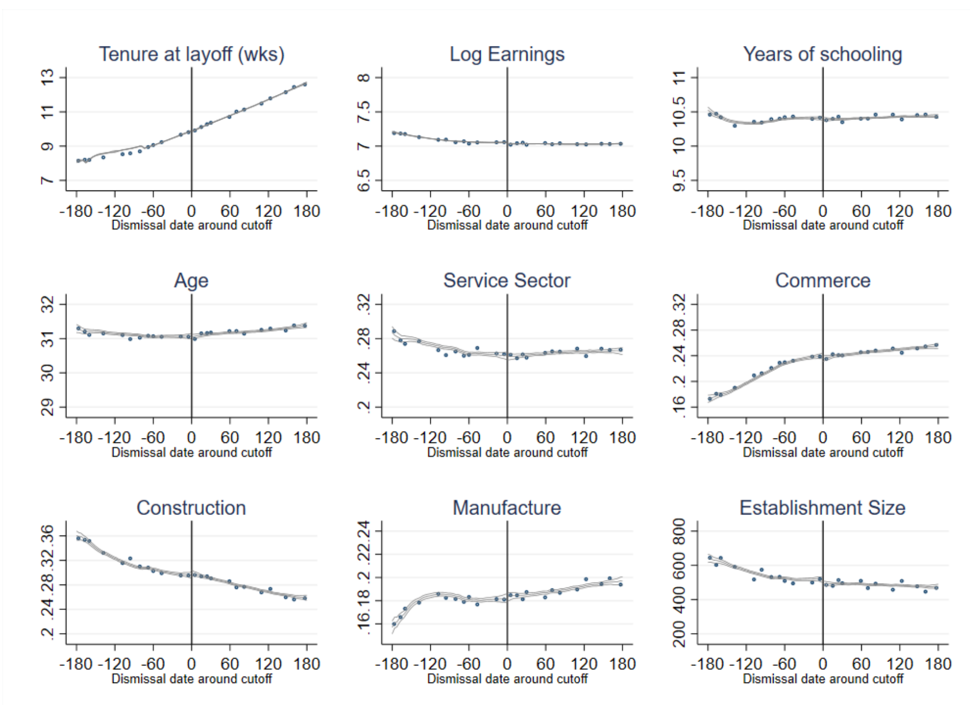
Figure C2: The effect of UI eligibility, density around the cutoff, main sample, male workers



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.

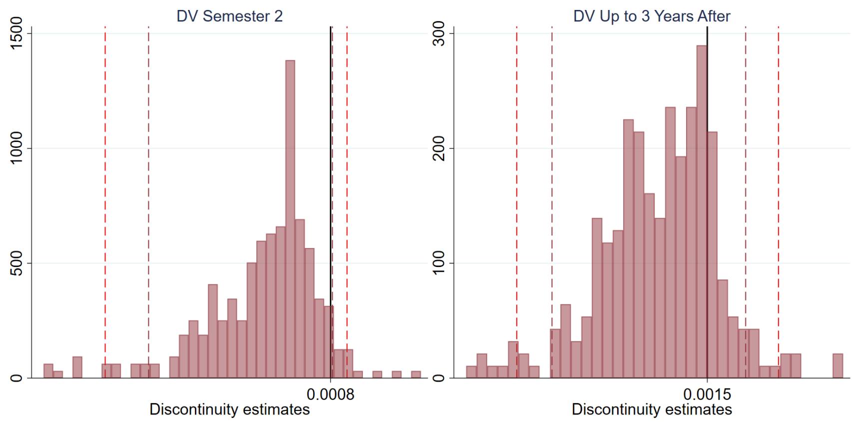
C.2 Robustness

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≤0.01, ** p≤0.05, * p≤0.1).

Table C3: Descriptive statistics of estimation sample for job loss and UI analyses

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

Notes: This table reports the average characteristics of workers in the job loss analysis in Section 4 (column 1), and UI eligibility analysis in Section 5 (column 2); and the standardized difference between the two samples (column 3).

Table 4: Effect of UI eligibility, male workers, reweighting to match characteristics of workers in job loss analysis

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

Notes: This table shows the effect of unemployment insurance (UI) eligibility on the probability of DV perpetration after layoff, as estimated from equation (3) using a Regression Discontinuity Design. Regressions use weights that balance the first two moments for a set of characteristics with the sample used for the job loss analysis in Section 4. Weights are obtained using the entropy algorithm in Hainmueller (2012). 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).