Job Displacement, Unemployment Benefits and Domestic Violence*

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VERY PRELIMINARY - PLEASE DO NOT CIRCULATE

Abstract

We provide the first causal estimates on a given sample of how individual job loss of men and women influences the risk of domestic violence (DV), and whether unemployment benefits mitigate these impacts. Estimating this confluence of three parameters on a given sample places us in a strong position to illuminate the underlying mechanisms. Using data on about 2 million domestic violence cases brought to criminal courts in Brazil during 2009-2017, we identify the defendant and the plaintiff in longitudinal employment registers. Leveraging mass layoffs for identification, we find that both male and female job loss, independently, lead to a large and persistent increase in domestic violence. These results satisfy a number of checks on selection into treatment, identification of dynamic heterogeneous effects, and the concern that they are driven by endogenous changes in reporting. The same pattern of results emerges using data on couples in the sub-sample of individuals captured in the social welfare register. It also holds when we measure domestic violence using information on women's use of public shelters rather than as prosecution or protective measures against men. Exploiting a discontinuity in unemployment insurance eligibility, we find UI does not have an ameliorating influence while benefits are being paid, but that eligible men (who have longer unemployment durations) are *more* likely to commit DV than ineligible men once benefits expire. Our findings indicate a role for income loss and exposure as mechanisms. The reason that UI does not mitigate is that it increases exposure while relaxing income constraints.

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

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1 Introduction

As many as one in three women report having ever experienced intimate partner violence (DV) at some stage in their lives (Garcia-Moreno et al., 2006). DV is both a marker of and a cause of gender inequality in the economic domain and, yet, it has attracted far less attention from economists than the gender pay gap. In this paper, we study how economic shocks and economic policies influence DV. In particular, we analyse impacts on DV of each of male and female job loss and investigate the potential for unemployment insurance to mitigate any impacts. We shall argue that estimating this confluence of three parameters on a given sample places us in a strong position to illuminate the underlying mechanisms.

Existing models of behaviour available in the literature suggest different mechanisms for the impact of job loss of one or the other partner on domestic violence. First, in a model of household bargaining, job loss modifies options outside marriage and hence the power balance within the couple (Aizer, 2010; Anderberg et al., 2016). Second, it can challenge gender stereotypes by changing the relative earnings of the man (Macmillan and Gartner, 1999; Bhalotra et al., 2019). Third, in situations where men seek to appropriate the earnings of their female partners, job loss can blunt the motive of instrumental control (Bloch and Rao, 2002; Carr and Packham, 2020). Fourth, it leads to the couple spending more time together, increasing opportunities for violence (Dugan, Nagin and Rosenfeld, 2003).

We propose that the income shock of job displacement disturbs an equilibrium, leading couples to have to re-negotiate the allocation of a shrunken pie, creating conditions for conflict. How this conflict is resolved may depend upon which partner lost their job, in line with the bargaining model. We also discuss non-pecuniary sources of stress which might contribute to triggering DV.

We use the universe of DV cases brought to criminal justice courts in Brazil in 2009-2017, a total of about 2 million cases, representing 11 percent of all criminal cases. These data include the names of the defendant and the plaintiff. We identify individuals in the court data in administrative employer-employee registers that track individuals over time and include information on employment spells and earnings. These data contain 100 million workers, over 60 million employment spells and 10 million layoffs per year. To achieve casual identification, we focus on job loss in the event of mass layoffs and plant closures. So as to identify dynamic treatment effects without having to assume treatment effect homogeneity (Goodman-Bacon, 2018; De Chaisemartin and D'Haultfoeuille, 2020), we match displaced workers with workers who retain their jobs but are otherwise similar in location, firm size, sector, birth cohort, tenure, and wages, exploiting the high dimensionality of the data. Our analysis of mitigation through unemployment insurance exploits a discontinuity in eligibility criteria to gain identification.

Our main findings are that both male and female job loss, independently, increase the risk of DV.In both cases, the effects persist through the four years for which we are able to track individuals following job loss. The same pattern of results emerges using the sub-sample of individuals captured in the social welfare register, where we can identify couples. In this sample we are also able to show that the pattern of results is robust to changing the indicator of DV from preventive measures imposed on the male perpetrator to the use of public shelters by victimized women.

To investigate the income shocks hypothesis, we extend the analysis to investigate whether unemployment insurance (or severance pay) mitigate impacts of job loss on DV. Unemployment is synonymous with a drop in income and an increase in exposure (time together at home), and we only ever observe the joint effect of these two changes. However, if we could take a sample of unemployed men (all of whom spend time at home) and assign benefits to some and not to others then we could, in principle, isolate the role of income loss. We adopt two approaches to doing this.[1]

First, we leverage the fact that both unemployment benefits and severance payments in Brazil are increasing in tenure, and examine heterogeneity in the impact of unemployment on DV by the tenure of workers at the time of displacement. We find a stark gradient, such that it is only workers with less than three years tenure that commit DV following job loss. However, low tenure workers differ along other dimensions- for example, they tend to have lower incomes and be younger. To address this, we condition upon interactions of job loss with age, education and income. Once this rich set of characteristics is controlled for, the only observable difference between high and low tenure workers is in benefits. The tenure gradient therefore suggest that the income loss associated with unemployment is a driver of domestic violence.

To investigate this further, we exploit experimental variation generated by a discontinuity in unemployment insurance eligibility. Unemployment benefits in Brazil cover, on average, about 80% of former earnings and last 3 to 5 months. We find that eligibility for unemployment benefits has no concurrent impact on the probability that job loss triggers domestic violence. However, men who were eligible for benefits are 24% *more* likely to commit DV by the second year following displacement. A likely explanation is that eligible men have longer unemployment durations, a stylized fact Katz and Meyer (1990), Lalive (2008), Card, Chetty and Weber (2007). While benefits are being paid, they appear to mitigate the DV-increasing impacts of income loss, but once they expire, the DV-increasing impacts of exposure emerge.

To summarize, we find evidence consistent with two mechanisms are potentially complementary- while income shocks provide the *motivation* for conflict, exposure provides the *opportunity*. Our UI results underline the tension between relaxing income constraints and diminishing exposure. Unemployment benefits need to be combined with policies aimed at getting the unemployed back to work such as training, support with job search or front loading of benefits (Lindner and Reizer, 2020). While severance payments also have behavioural effects (Card, Chetty and Weber, 2007; Basten, Fagereng and Telle, 2014), these tend to be smaller.

An important concern is that our results may be driven by reporting bias. We conjecture that reporting bias is more likely among younger women in less committed relationships and in less severe cases, with femicide for instance always being reported. We then argue that reporting bias is unlikely to drive our results because we find impacts of male job loss on DV across the board for all ages and all degrees of severity — in fact we find larger impacts on DV when the woman is older and when the crime is more severe. The most compelling test we have against reporting bias is that we continue to find a relationship between male job loss and domestic violence when we restrict the sample to *in*

[1]The analysis is now focused on male job loss. Results for female job loss are imprecise and discussed in the paper.

flagrante cases — cases "caught in action," for example, by a neighbour, for which the woman does not need to decide whether or not to report. We also present estimates of male and female job loss on femicide on the premise that femicide is always reported.

Our estimates for job loss are subject to a number of specification checks. We find no evidence of anticipation effects, and we confirm this using monthly as opposed to annual data. The estimates are not sensitive to conditioning upon fixed effects for the three-way interaction of municipality, industry and year, which flexibly controls for local industry-specific shocks. This confirms that the use of matched controls is effective in purging the influence of omitted trends at a fairly fine-grained level. The results are robust to varying the minimum size of firms in the sample and the definition of mass layoff from a minimum of 33% to 90% of all workers, right through to plant closure. They stand up to an alternative intention to treat type approach that analyses all workers in firms with mass layoffs against matched controls in firms without mass layoffs, thereby sidestepping the issue of displaced workers being selected. On account of judicial secrecy, there are cases where names are missing and we cannot be sure that they are missing at random. Since the problem is severe for names of female victims, the analysis for female job loss is carried out restricting the sample to jurisdictions in which at least 50% of cases are identified. However we show that our estimates for both male and female job loss are fairly stable as we vary this threshold.

The rest of this section delineates our contributions to the literature. This is the first study to investigate impacts of job displacement on DV. Previous research investigating impacts of unemployment and earnings on DV has focused on changes in (regional) labour market opportunities of the woman *relative* to the man (Aizer, 2010; Anderberg et al., 2016; **?**; Bhalotra et al., 2019). An important feature of our analysis is that individual job loss of either the man or the woman is a large realized income shock to the household, quite different from a marginal change in local area job opportunities. While there is large literature investigating impacts of job loss on mental health (Zimmer 2020, Browning et al. 2006), smoking (Black et al. 2015), premature mortality (Sullivan and Von Wachter 2009), fertility (Del Bono et al. 2015), birth weight offspring (Lindo 2011) and, most pertinent, divorce (Charles and Stevens 2004, Eliason 2012), we are unaware of analysis of impacts of job displacement on domestic violence.[2]

Although DV has not been systematically analysed through the lens of household income shocks, a number of studies analyse impacts of cash transfers. Most of this literature looks to test the hypothesis that DV will decline with the economic empowerment of women (Angelucci, 2008; Bobonis, Gonzalez-Brenes and Castro, 2013; Luke and Munshi, 2011; Heath, 2014; Tur-Prats, 2019; Estefan, 2019; Kotsadam and Villanger, 2020), although a recent study analyses cash transfers to both men and women (Haushofer et al., 2019). However, cash transfers do not necessarily have impacts symmetrically opposite to the impacts of earnings losses. We argued that when household income falls because one or the other partner loses a job, the couple will likely need to negotiate how to allocate the smaller pie and that this, together with the more general stress of a family suffering job loss can plausibly trigger

[2]Not all of these investigations find the posited association. For instance, the impacts of job loss on indicators of stress is mixed. Our point is simply that impacts of job displacement on a number of cognate health and family related outcomes have been investigated.

violence. However, if cash transfers are made to a family that is already in a (possibly unhappy) equilibrium, it is not clear that they will act to reduce conflict. Indeed, several studies find evidence that men commit violence in order to extract resources from women (Bloch and Rao, 2002; Carr and Packham, 2020).

We present the first investigation of whether unemployment benefits can mitigate impacts of job loss on DV. This is also the first large-scale study to find evidence consistent with the exposure model of DV, emerging from criminology (Dugan, Nagin and Rosenfeld, 2003). While we do not have experimental variation that isolates exposure from income, exposure is consistent with both parts of the analysis- first, with our finding that both male and female unemployment increase DV and second, with our finding that DV increases among UI-eligible men after benefits cease.

We arguably use better strategies for identification than a large part of the literature, and are able to obtain fairly precise estimates by virtue of using administrative data from a populous country. Research analysing impacts on DV of the regional unemployment rates or regional wages of women relative to men (Aizer, 2010; Anderberg et al., 2016; Estefan, 2019; Bhalotra et al., 2019; Tur-Prats, 2019) suffer from the fact that regional changes proxy not only job loss but also changes in welfare payments and possibly public services designed to address domestic violence complaints. We avert this problem by studying individual job loss in mass layoffs. In a notable recent study, Kotsadam and Villanger (2020) examine individual job awards (the converse of job loss), but only for women and we show how much more one can learn about behavioural responses if we have information on job loss for both men and women. Also, for reasons discussed earlier, it is unclear that the mechanisms by which job loss raises DV are reversed under job creation.

In contrast to existing DV studies, we also employ recent advances designed to identify dynamic heterogeneous effects. This allows us to discuss persistence of the impacts of job loss with more confidence. We also provide a relatively thorough analysis of the possibility that our results are driven by changes in reporting behaviour following job loss, an issue that is widely recognized but inherently challenging to address. Some previous work (like us) does use femicide as a measure of violence against women that is not subject to reporting bias. However, femicide constitutes the tip of the iceberg of domestic abuse and it may not directly scale up with all forms of abuse, or similarly for different sub-groups of the population.

By virtue of using longitudinal individual data for a large populous country from administrative sources we are able to estimate specifications that are demanding of statistical power and to obtain mostly precise estimates. Research on DV has been severely hampered by a scarcity of systematic large-scale longitudinal data. There are two separate issues here. First, neither national governments nor UN agencies have sufficiently prioritized monitoring of violence against women, possibly because the decision making bodies are dominated by men. Second, women are reluctant to report on account of internalized social norms, social stigma, financial reliance on their partners, fear of male backlash, fear of adverse impacts on their children, and a lack of trust in the criminal justice system. Previous studies have used self-reported survey data, data on hospitalization of women for injuries or data on female homicides (Anderberg et al., 2016; Bhalotra et al., 2019; Aizer, 2010; Estefan, 2019). We

primarily use court registers to identify all DV cases resulting in either prosecution or preventive orders. We supplement these data with data on women's use of public shelters for domestic violence and data on homicides.

In addition to contributing to a literature on DV, this paper adds new evidence to a wider literature on the role of economic activity in determining women's empowerment (Duflo, 2012), a literature on the ramifications of male job loss including impacts on mental health and substance use (Sullivan and von Wachter, 2009; Kuhn, Lalive and Zweimüller, 2009), research on unintended or behavioural impacts of unemployment benefits (Card, Chetty and Weber, 2007), and the recent surge of research analysing impacts of Covid-19 on each of job loss (Adams-Prassl et al., 2020*a*; Hupkau and Petrangolo, 2020), mental health (Adams-Prassl et al., 2020*b*; Banks and Xu, 2020; Etheridge and Spantig, 2020) and domestic violence (Leslie and Wilson, 2020), though no previous research has analysed the relationship between these outcomes.

2 Context and Institutions

Brazil ranks fifth in the world on a domestic violence index, preceded by El Salvador, Colombia, Guatemala and Russia. Brazil is a large country with a population of 210 million, so the population at risk is large. In 2015, the Brazilian government released studies showing that every seven minutes a woman is a victim of domestic violence in Brazil and that more than 70% of the Brazilian female population is expected to suffer violence during their lifetime. In 2017, there were an estimated 606 cases of violence and 164 cases of rape per day. Led by a strong women's rights movement, Brazil has implemented a bold set of policy initiatives to address domestic violence, with potential to provide lessons for other countries. As only a small fraction of cases were reported to the police, the government reacted by pioneering from 1985 onwards the idea of creating Women Police Stations (Delegacia da Mulher) to encourage reporting, and thereby redress (XX). In 2006, it took a great leap forward by promulgating a Domestic Violence law, the Maria da Penha Law, which criminalized violence against women, intensified punishment and expanded judicial and civil systems to deal with it. Once a woman reports a case to the police, legal processes begin even if the woman does not follow through. Article 5 of this law defines domestic violence as "any action or omission of action motivated by gender that results in death, lesion, physical, sexual or psychological suffering, moral or patrimonial hazard." There are no systematic data on domestic violence for Brazil or indeed for other countries, as a result of which there has been limited monitoring of trends, but recent data suggest an escalation of femicide. Both the scale of the problem and the scale of the government's response to domestic violence in Brazil have drawn the attention of scholars in multiple disciplines including law, human rights and women's studies (XX). However, little is known about economic causes of domestic violence in Brazil. This is topical in view of recent recessionary conditions in the country. After declining for fifteen years, the unemployment rate in Brazil has risen secularly since 2015, to 12.2% in 2019. Our analysis of unemployment benefits is also moot. Benefit levels were reduced in 2015, labour law was reformed in 2017 and since Covid-19 there is a policy debate on universal income.

Figure 1 suggests a clear contemporaneous relationship between job loss and domestic violence.

Using the data that we link for the analysis in this paper, it shows that the average probability of prosecution (of men) for domestic violence is almost twice as large in years in which they lose their jobs. This is the case through the age distribution for men, although the difference does begin to narrow after the age of 40. The direction of causation in this association is ambiguous, but it motivates a more formal analysis. The rest of this section describes features of the judiciary and the labour market that are relevant to the formal analysis.

2.1 Criminal justice

Brazil has three layers of government- federal, 27 states, and 5,565 municipalities. The judicial system comprises 27 state courts and four specialized courts: federal, labor, electoral, and military. The 27 state courts comprise 2,697 tribunals, one for each judicial district. Each has jurisdiction over one or more municipalities. Domestic violence cases are classified as criminal cases and they fall under the jurisdiction of state courts (CNJ, 2019) Criminal law is formulated at the federal level and is uniform across the country but local governments are responsible for policing and criminal investigations. The latter are conducted by state judiciary police (*"Polícia Judiciária"* or *"Polícia Civil"*), either by its own initiative or upon request from the public prosecutor office or crime victims. As an exception, the public prosecutor office may also directly carry out investigations. Once an investigation is concluded by the police, the files are sent to the prosecutor office, which decides whether to press or drop the charges.[3] Even if the prosecutor decides not to press charges following the investigation, a new court case is filed since the decision to drop must be approved by a judge. Consequently, all concluded investigations are registered as judicial cases.

The average length of proceedings to first trial in 2017 was on average XX months. Although investigations are generally registered as a judicial case only upon conclusion, there are two relevant exceptions. First, when the investigation demands a court order, for instance a prohibition order that prevents the perpetrator from making contact with the victim, it is immediately registered as a judicial case in such way that a judge can review the request. Second, *"in flagrante"* arrests immediately lead to an investigation procedure which is filed as a judicial case that is mandated for review within 24 hours. In that case, the judge decides whether to maintain the arrest as a preventive measure while the offender waits for trial.

2.2 Labor Regulation

Labor law in Brazil allows firms to dismiss workers without a just cause, although it does require that they pay dismissal indemnities. 93% of all contracts in the private sector are open-ended full-time contracts. The most common forms of separation for open-ended jobs are dismissals without a just cause (65% of all cases) and voluntary quits (33%).[4] Our analysis will isolate dismissals. All workers are entitled to a mandatory savings account financed through monthly contributions by the employer

[3] Alternatively, the prosecutor may postpone the decision by sending the case back to the police with a request for further investigation.

[4] These statistics refer to 2012, but they are fairly stable over time.

amounting to 8% of the worker's compensation. In case of dismissals without just cause, workers can access these funds and are further entitled to a severance payment equivalent to 40% of the account's balance. Summing over these two sources, workers receive approximately 1.36 monthly wages for each year of tenure at the time of layoff.

The informal labour market in Brazil is large, accounting for roughly 45% of all jobs in 2012. Job turnover in both the formal and informal sectors is high and workers tend to move frequently between them. Some firms operate in both markets, hiring formal and informal workers (Ulyssea, 2018). As there are no administrative data on informal employment, we analyse layoffs in the formal sector. We do not observe if these workers gain employment in the informal economy, rather, as long as they do not re-enter the formal sector, we classify them as unemployed. The consequence of this is that our estimates of the impact of job loss on DV will provide a lower bound on the true effect. However, we will use survey data to estimate the share of workers returning to informal jobs and use this to interpret the magnitude of the reported effects.

In addition to receiving severance payments, displaced workers may be eligible for unemployment insurance. This is restricted to workers dismissed without a just cause and ranges from three to five months, depending on the length of employment in the 36 months prior to dismissal. The generous replacement rate starts at 100% for workers earning the minimum wage and decreases smoothly to 67% at the benefit cap which is 2.65 times the minimum wage. Once unemployment benefits expire, the only income support at the national level is "Bolsa Família", the well-known conditional cash transfer targeted at extremely 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.

3 Data Sources and Linkage

3.1 Data sources

The analysis will use individual longitudinal data derived by linking administrative court and employment records. We first describe each data source and then the linking procedure.

Employment registers. The *Relação Anual de Informações Sociais* (RAIS) is a linked employer-employee dataset covering the universe of formal workers and firms in Brazil, made available by the Ministry of Labor for the period 2002-2017. Our main analysis comprises the period 2009-2017, covering about 100 million workers and more than 10 million yearly layoffs.

The data contain the start and end dates and the location of each job, the type of contract, occupation and sectoral code, and the worker's race, education and earnings. Workers are identified by both a unique tax code identifier (CPF) and their full name. We retain the sample of full-time non-agricultural private sector workers age 18-50, that is, those employed for at least 30 hours per week. XX% of formal sector workers are in the public sector but public sector workers cannot be dismissed. In the retained sample, XX% are men and the remainder are women.

The date of dismissal is likely to be measured with error due to a mandatory 30-day advance notice

period, which is extended by 3 days for each complete year of tenure and capped at 90 days. It is fairly common that firms release workers during the notice period, but we cannot identify when this happens in the data. In the analysis, we define the date of layoff as the date stated in RAIS minus 30 days. Since some workers will actually have a longer notice period, this is a conservative choice for testing the parallel trends assumption underlying our difference-in-differences design. In practice, given high rates of job turnover, 37% and 90% of the workers in our sample are dismissed with less than one and three years in the job respectively, thus having a notice period between 30 and 39 days.

Judicial registers. We have the universe of domestic violence cases filed in all first-degree courts during 2009-17. In this period we observe about 2 million domestic violence cases which account for more than 11% of all criminal cases. Among these, 55% of cases are for assault and battery, 22% for inhibition of freedom and 15% on account of threatening behaviour. The data are supplied by Kurier, a private company that provides information services to law firms in Brazil. They draw the data from case-level information made publicly available on tribunal websites and complement it with information from the daily diaries of courts. For each case, the data contain the start and end date, court location, tags for the subjects being discussed and the full name of the defendant and plaintiff in the case. There are very few (XX%) cases in which the plaintiff is a man. We drop these and analyse cases in which the plaintiff is a woman and the defendant is a man.

The name of the defendant is available for XX million of XX million cases, the missing names arising either from imprecisions in the data input process from court diaries or on account of judicial secrecy. As a rule, judicial acts are public knowledge, but judges may make exceptions to the rule in specific instances established by the law. Exceptions often apply in cases of domestic violence, as for cases involving individuals under the legal age of 18.

What is important is that it is unlikely that missing data in our records are related to the job status of the defendant or the plaintiff, which is the independent variable of interest. First, requests for secrecy generally take place after the case has started, while our data capture the identity of the defendant as long as the case is started without secrecy. Second, the threat of dismissal is not a valid motive for invoking secrecy. Ongoing criminal prosecutions do not constitute a just cause for worker's dismissal by firms which only applies for definitive criminal convictions and, in any case, we focus on layoffs that occur for no just cause. Third, for the case of offenders arrested "in flagrante" – i.e. caught in the act of committing domestic violence – judges generally take the initial decision on case secrecy exclusively based on the police form describing the arrest (*"auto de prisão em flagrante"*), thus lacking specific information on the defendant's characteristics such as employment status. Nevertheless, we will leverage the large variation in the application of secrecy rules across state jurisdictions to investigate whether our estimates are sensitive to progressively restricting the analysis to states with a lower fraction of missing values in the criminal prosecutions data (see Section 8).

Another measurement issue concerns the timing of criminal behavior, as the dataset reports only the initial date of the prosecution case rather than the (alleged) date of the DV offence. However, for offenders arrested "in flagrante", the prosecution is initiated immediately because a judge must decide whether to maintain the defendant while awaiting for trial. For this subset of cases, we can

thus precisely measure the timing of the act of violence. In Section 8, we discuss these measurement issues at length and assess the robustness of our results to including only criminal prosecutions for arrests "in flagrante". We will in any case want to restrict to these cases to assess reporting bias since these are cases in which the woman does not report the case, rather the man is caught in action.

3.2 Linking court and employment records

We merge the judicial and employment data using the full name of the individual, which is reported in both datasets. To minimize errors, we restrict the analysis to individuals who have unique names in the country. This is the case for about half of the adult population, because Brazilians typically have multiple surnames, with at least one surname from the father and mother. To identify individuals with a unique name, we create a registry of individuals by merging the RAIS data with the *Cadastro Único* (CadUnico), a dataset maintained by the Ministry of Development for the administration of all federal social programs, since CadUnico contains the full name and tax ID for each individual. The resulting registry contains the name and tax ID for 96% of the Brazilian adult population, allowing us to almost perfectly identify the commonness of each name in the country.[5]

Columns (1)-(3) of Appendix Table **??** compare the characteristics of job losers with and without unique names, respectively. There is some mild positive selection into the former group, as workers with unique names achieve 6% more years of education, earn 12% more, and are 2.6 percentage points more likely to be managers. However, the standardized difference remains below 0.2 for all variables but education. In addition, the two groups live in municipalities with similar characteristics and are similar in terms of job tenure, firm size, and age.[6] We will assess robustness of our main findings to including all individuals whose name is unique in the state where they work rather than unique in the entire country. This raises coverage of the country's population to 70% in the extended sample and it reduces positive selection into the sample (columns 4-6 of Table **??**). The merged sample has XX workers, who face a risk of layoff of XX.

4 Empirical Strategy: Job Loss

We want to estimate the impact of job loss on the risk of intimate partner violence. We do this twice, first to estimate the probability that male job loss leads to perpetration and then to estimate the probability that female job loss leads to victimization.

An association between unemployment and domestic violence could arise if men who perpetrate violence against their partners are also men with behaviours that make them prone to job loss. This is not what we want to capture. What we want to understand is how unexpected job loss, that has nothing to do with the behaviours of men, influences their propensity to commit violence. And

[5] 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.

[6] Appendix Figure **??** also shows that the full distributions of age, income, job tenure, and years of education are similar between the two groups.

similarly, we want to understand whether women who lose their jobs through no fault of their own are more likely to be victimized. This is what is most relevant to public policy, as it could help us understand how events outside the control of individuals such as recessions or social distancing mandates might impact domestic violence through generating unemployment.

We define as treated all workers displaced in mass layoffs between 2012 and 2014 in the 20-50 age range. Since the available sample covers 2009-2017, this choice allows us to estimate dynamic treatment effects for up to four years after displacement, as well as anticipation or placebo effects up to three years before displacement.[7]

We match each treated individual with a control. Control group workers are drawn from the set of individuals employed in firms that did not experience layoffs during the period of analysis. The individual matches are defined such that the control worker (i) is not displaced in the same calendar year, and (ii) belongs to the same birth cohort, earnings category (by R$250/month bins), firm size (quartiles), one-digit industrial sector (9 sectors), state (27 states), and has the same job tenure. When the algorithm results in treated workers being matched with multiple controls, then one control unit is randomly selected. In the baseline specification, control workers are not dismissed in the matching year but may be dismissed in subsequent years. However, we will investigate sensitivity of the results to including only control workers who are continuously employed throughout the entire sample period. 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 through the period. Out of 5.9 million displaced individuals, 4.9 million are successfully matched to a control individual. We assign to controls a placebo dismissal date equal to the layoff date of the matched treated worker, and compare outcomes for the two groups at different time intervals relative to the layoff date. The presence of never-treated workers in the analysis allows us to estimate the full path of dynamic treatment effects, overcoming the potential problem of negative weights attached to some treated units when averaging heterogeneous treatment effects in typical two-way fixed effects regressions. See Borusyak and Jaravel (2017), Abraham and Sun (2018), Athey and Imbens (2018), Goodman-Bacon (2018), de Chaisemartin and D'Haultfoeuille (2019), Callaway and Sant'Anna (2019) and Imai and Kim (2019). We will show in Section 8 that negative weights are not present in our analysis, and that our results are robust to the alternative approach proposed by de Chaisemartin and D'Haultfoeuille (2019).

Using the treated workers and their matched controls, we estimate the following difference-in-differences equation :

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

Workers are identified by the subscript i, and $Treat_i$ is a dummy indicating that the worker suffered layoff in a mass layoff. The set of dummy variables $Time_t$'s identify years since layoff, which we can

[7] Given that our data on prosecutions cover offenders above the legal age of 18, we focus on the 20-50 age range so that we observe criminal behavior for at least two years before the layoff.

define very precisely because the exact dates of layoffs (and criminal prosecutions) are reported in our data. Therefore, $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on; analogously, $t = 0$ for the 12 months before layoff, $t = -1$ for the previous 12 months, and so on. The coefficients $\{\delta_1, ..., \delta_T\}$ thus identify dynamic treatment effects, whereas $\{\delta_{-P}, ..., \delta_0\}$ estimate anticipation or placebo effects. We will present estimates using monthly rather than yearly data in a specification check- this is quite demanding of the data but it does afford a cleaner description of pre-trends and a richer description of the path of treatment effects. We absorb time-varying shocks by including fixed effects for the year or month, as the case may be, $Time_t$. In an extended specification we allow for time shocks specific to municipality-industry cells by including the triple interaction $Time_t * Mun_{j(i)} * Ind_{k(i)}$, where $Mun_{j(i)}$ and $Ind_{k(i)}$ are fixed effects for the municipality (5,565) and two-digit industry (87) in which the i-th worker is employed at time $t = 0$. These are finer categories than used to implement the matching, which used state (27) and one-digit industrial sector (9). Comparing estimates obtained before and after conditioning on these richer fixed effects will indicate the degree to which the use of matched controls is successful in purging the influence of potentially confounding shocks at the local level. To summarize the average treatment effect over all periods, 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 (1).

The main challenge to identification is potential selection into displacement. Parallel trends between treated and controls in the pre-treatment period attenuate but do not entirely address such concerns. For instance, we cannot exclude a priori that firms are more likely to dismiss workers who experience marriage instability at some point, which in turn is likely to be directly linked to IPV. To address this, we restrict the analysis to job loss that occurs in a mass layoff. Mass layoff typically follows negative external shocks at the firm level, rather than the characteristics or behaviors of dismissed workers (see e.g. Gathmann, Helm and Schönberg, 2020). We define mass layoff as occurring when firms with at least fifteen workers dismiss 30% or more of the workforce within a year without just cause. We assess robustness of our findings to alternative definitions of mass layoffs in terms of both the minimum share of displaced workers and firm size. We also investigate the limiting case of plant closures. We exclude from mass layoffs firms reallocating under a new ID. In line with the literature, we assume that firms reallocate when at least 50% of the workers displaced from a firm are found to be employed in a new establishment by January 1^{st} of the following year.

Table **??** presents summary statistics for treated and control workers when including all layoffs (first three columns) and when restricting to mass layoffs (last three columns). The two groups are balanced in terms of demographics, job characteristics, and local area characteristics. This holds true even for variables that are not part of the matching process, such as education, race, occupation and municipality characteristics. The standardized difference between the two groups is below the threshold of 0.20 suggested by Imbens and Rubin (2015) for all variables except education in the mass layoff sample. However, there is a noticeable gap in the probability of a criminal prosecution prior

to displacement, which is 28% lower in the control group when considering all layoffs and 25% when focusing on mass layoffs. This gap can be explained by the fact that turnover is higher by construction in the treatment group (each control worker has to remain employed at least for the calendar year in which the matched is treated) and, in turn, job turnover is positively related to the probability of committing domestic abuse (see the bottom-left panel of Figure **??**). Although the difference-in-differences design only requires trends to be parallel (which is the case in our data, as we will show below), one may worry that the control group does not provide an adequate counterfactual in light of the level gap. We will address this concern by showing that our results are stable under alternative definitions of treated and control groups for which this gap essentially vanishes.

5 Results: First Stage

In this section we provide estimates of the impacts of layoff on subsequent labour market outcomes for men and women which illuminate time to recovery. In subsequent sections we will provide reduced form estimates of the impacts of male and female job loss on DV.

Figures **??** and **??** plot the estimated effects of layoff on unemployment and labor income over time for treated workers dismissed at date $t = 0$ relative to matched controls (not dismissed in the same calendar year) using the specification in equation (1). The corresponding single coefficient models are displayed in Tables **??** and **??** The estimates are more or less identical for men and women. The figures shows that the control group closely tracks the treated group in terms of both employment and labor income during the pre-treatment period, the estimated difference in trends being very close to zero.

Following dismissal, there is a substantial decline in employment and earnings of displaced (treated) workers relative to the controls, and this difference persists through the four year window available in the data despite the fact that by construction control group workers may suffer dismissal starting in the following calendar year. Tables 1 and 5 show that the average decline across the four years after dismissal is roughly 20% for employment and 40% for earnings. The event study plots show that, in the first year after dismissal, employment and income are 34% and 70% lower respectively, and there is gradual recovery after that. Four years after dismissal, treated workers experience 13% lower employment rates and 26% lower labor income compared with the control group. As may be expected, the estimates are larger if we restrict the control group to include only workers who are continuously employed during the sample period. The estimates are similar irrespective of whether we analyse all layoffs or mass layoffs and they are not sensitive to conditioning on the rich set of municipality-year-industry fixed effects. This demonstrates that our baseline strategy involving a fine-grained matching on individual characteristics to identify the control group together with being able to exploit variation over time effectively absorbs time-varying shocks at a very granular level of geographic and sectoral disaggregation.[8]

Adjusting for informal sector earnings. As discussed earlier, some of the workers dismissed

[8] Figure **??** in the Appendix provides additional evidence of lasting effects on monthly wages, conditional on being employed, as well as more transitory effects on subsequent job separations.

from formal sector jobs may find employment in the informal sector and, in this case, the estimates in Figures ?? and ?? will overstate the drop in employment and earnings upon job loss. To investigate how much, we repeated analysis of employment effects of layoff using the National Longitudinal Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD), which contains information on both formal and informal labor income.[9] The longitudinal component of PNAD tracks households for five consecutive quarters. Although the microdata do not contain a person ID, we can track individuals over time based on their household ID and characteristics such as gender, their precise birth date and their order in the family. We focus on male workers who were initially interviewed during 2012-2014, and compare workers who were formally employed in the first but not in the second quarter (treated) with a control group who were employed in both the first and second quarter (but possibly displaced in later quarters). Figure ?? in the Appendix presents estimates of monthly income for both formal and informal jobs. The average effect on formal earnings over the first four quarters after displacement, at about 65%, is essentially identical to that estimated using the administrative data. Once we include informal sector employment, the estimated effect on labor earnings is, as expected, smaller at 58%. This suggests that the DV elasticity estimates based on formal labor income are overestimated by about 12%.

6 Results: Male Job Loss

Figure ?? shows the effect of male job loss on the probability of committing domestic violence in the following years. The dependent variable is a dummy for criminal prosecution or being subject to a protective measure in each year relative to the dismissal date, based on the filing date of the judicial case. Analysis time is set relative to the layoff date, and dynamic treatment effects are re-scaled by the baseline DV rate in the treatment group at $t = 0$.

The probability that men receive either a prosecution or a protective order sharply increases in the first year after layoff and persists thereafter. As for employment and earnings, so for IPV, the estimates are again similar for all layoffs and mass layoffs and not sensitive to including municipality-year-industry fixed effects. Table ?? reports estimates from the single coefficient model, equation 2, of the average effects on DV over the four years after dismissal, and their ratio to the baseline rate. Panel A uses the baseline sample including all displaced workers and matched control workers who were not displaced in the same year. Male job loss increases the probability of DV by 30% (or about 0.04 percentage points).

Dividing the impact of job loss on DV by the 40% decrease in earnings following job loss gives an elasticity of DV to earnings of -0.75. We do not attach a causal interpretation to this as that would require that layoffs affect DV only through decreased earnings. In fact layoffs can directly affect earnings through exposure or psychological stress, which we will discuss in Section 12). In Panel B, we restrict the control group to include only workers who are continuously employed during the sample period. Unsurprisingly, the treatment effects are larger. In Panel C, our preferred specification, we restrict the sample to include only workers displaced in mass layoffs. In Panel D, retaining only men

[9] In fact, the Brazilian Institute of Geography and Statistics (IBGE) computes informality rates based on PNAD.

displaced in mass layoffs, we add controls for the full set of municipality $\times$ industry $\times$ year fixed effects. The estimated effects in Panels C and D are essentially identical to those in Panel A.

7 Results: Female Job Loss

Figure **??** and Table **??** show that the probability that women are victims of domestic violence jumps sharply following the event of their losing their jobs, and persists. The dependent variable is a dummy for a woman having filed for a protective measure. As explained, the name of the female victim is much more likely to be missing in court records than the name of the male defendant and this is even more so when the case involves prosecution, so now we only model protective measures and, even then, we are working with a much smaller data set. Still, the specifications shown and the order of specifications checks is the same as for male job loss.

As any missing data on names and under-reporting are reflected in the baseline means, the estimated coefficients expressed relative to the mean. These figures indicate an 85% (or about 0.053 percentage points) increase in the probability that women file for protective measures. This is robust to varying the definition of matched controls and to conditioning upon municipality-year-industry fixed effects.

The estimates discussed so far condition upon retaining in the sample only jurisdictions in which the woman's name is available for at least 50% of court cases. In Table XX we assess sensitivity of the estimates to this restriction, varying the minimum share of non-missing cases from zero to 80% in increments of 10 pp. With the exception of the small sample created with the 80% threshold, there is a statistically significant positive relationship in every sample, the relative effect varying between 44 and 83 percent.

8 Robustness Checks and Extensions

Selection into treatment. Identification rests on the assumption that there is no dynamic selection into treatment. If this is right then control group workers should approximate the behavior of displaced workers in the absence of displacement. The evidence in Figures **??**, **??** and **??** for male and female job loss respectively that the treated and control groups follow parallel trends in the pre-treatment period is consistent with this. Importantly, all results, for the pre- and post-treatment periods, are more or less identical whether or not we restrict the treated group to workers displaced in mass layoffs.

We have already demonstrated robustness of the estimates to varying the definition of the matched controls and to conditioning upon municipality-industry-year fixed effects, which limits potential concerns about omitted shocks.

Although displacement in mass layoffs is often assumed exogenous, we acknowledge that there remains room for selection even when a firm dismisses a third of its employees (which is our baseline definition of mass layoff). We address this concern as follows. First, we explore the sensitivity of the results to varying the definition of mass layoffs, in terms of both the fraction of dismissed employees (columns 1 to 4) and firm size (panels A to D) see Table **??** for male and female job loss respectively.

We expect that as we restrict to events in which a larger fraction of workers is dismissed, there should be less scope for selection into dismissal. Indeed, level differences in pre-treatment DV rates between dismissed workers and matched controls progressively decline to almost zero when restricting to events in which at least 90% of workers were dismissed, see the last row of each panel in Table **??**. However, the estimated effect on DV is largely unaffected (see also graphical evidence in Appendix Figure **??**). The same is true for the limiting case of plant closures (column 5) and when varying minimum firm size (panels B to D).[10] For the case of female job loss the smaller cell sizes for the larger firm samples begin to challenge significance.

A second approach we take to addressing potential selection effects is to expand the treated group to include *all* workers, both displaced and non-displaced, employed at the beginning of each year in mass layoff firms (columns 1-6), and in non-mass layoff firms (columns 4-6), see Table **??**. This does away with the problem that displaced workers are selected within their firm and it also avoids concerns over selection driven by *early leavers* who quit declining firms in advance of mass layoffs. This approach delivers an intent to treat estimate, analogous to estimates from randomized experiments with imperfect compliance. Treatment effects on employment and earnings (columns 1-2, 4-5) as well as on DV (columns 3 and 6) are now weaker. However, if we rescale DV effects by the change in earnings, the implied elasticity is similar to that implied by the baseline estimate in Table **??**.

Estimation of dynamic treatment effects. Several recent studies highlight the challenges associated with estimating dynamic treatment effects in difference-in-differences designs when there is variation in the timing of treatment and treatment effects are heterogeneous across individuals, as is typically plausible. Under these conditions, the treatment effects for individuals who are treated at some point might enter the double differences estimating the dynamic treatment effects with opposite signs in different time periods. As a result, the estimated difference-in-differences coefficients in a two-way fixed effect specification equal a weighted average of the individual treatment effects with possibly negative weights (de Chaisemartin and D'Haultfoeuille, 2019).[11]

This problem is most severe when all or a large share of individuals in the sample are treated at some point. However, our data include a large share of never-treated workers. To investigate their empirical relevance, we estimate the share of units with negative weights following (de Chaisemartin and D'Haultfoeuille, 2019) with and without. When we estimate the two-way fixed effect specification on the panel of workers observed over calendar years, no individual treatment effect receives a negative weight. If we instead restrict the sample to workers displaced at some point, about 42% of units receive a negative weight, see Figure **??** in the Appendix. This restriction would lead the estimated effects to be about half the size of the effects we estimate using a sample that includes never-displaced workers. This is relevant to bear in mind when comparing estimates across studies, some of which may include only

[10] The sample size increases when moving from column 4 (90% layoffs) to column 5 (plant closures), which may appear counterintuitive. This is because our definition of mass layoffs is based on the share of workers dismissed without a just cause, thus excluding workers who voluntarily quit a declining firm. Plant closures may involve firms in which e.g. 75% of the workers are dismissed and 25% quit. For this reason, plant closures are not necessarily a subset of firms with 90% layoffs.

[11] Goodman-Bacon (2018) provide a similar decomposition; see also Borusyak and Jaravel (2017), Abraham and Sun (2018), Athey and Imbens (2018), Callaway and Sant'Anna (2019) and Imai and Kim (2019).

individuals treated at some time.[12] We nevertheless re-estimate the relationship of interest following de Chaisemartin and D'Haultfoeuille (2019) and comparing in each period, "switchers" (units changing treatment status in a given period) to "non-switchers". The results are very similar to the baseline estimates, see Appendix Figure **??**.

Lags in filing and sentencing. If the case is filed with a lag relative to when the alleged incident occurred then balance tests in the pre-treatment period may fail to capture increases in domestic violence that occur prior to job loss. We address this concern by restricting the sample to cases where there is no lag. About XX% of DV cases are "in flagrante" or caught in action. These cases are filed immediately and also judged very soon after. Restricting to "in flagrante" cases also allays somewhat the concern that the police and/or prosecutors may be quicker to move on DV cases where the man in unemployed. This concern is vitiated when the DV incident is identified by a neighbour or possibly the police happening perchance to witness it. The decision to prosecute is less dependent on the victim statements or on finding witnesses, and will tend to involves less discretion on the part of the criminal justice system. [13] The estimates are in Figure XX and Table XX. There is no evidence of differential pre-trends, and the estimated coefficients are, if at all, larger. For female job loss, there are too few in flagrante cases to conduct this test.

Estimates for couples. Initially, in order to use the entirety of the available data, we do not seek to identify couples. Instead, analysis of male job loss simply associates defendants in the court data with men in the employment register. Similarly, analysis of female job loss relies upon identifying plaintiffs in the court data as women in the employment register. However, not all domestic violence cases are for intimate partner violence. Data from hospital reports in Brazil (add citation/source XX) indicate that 50% of domestic violence is committed by partners, 14% by ex-partners, 10%, by friends, another 10% by unknown persons, 3% by parents, 3% by siblings and 5% by children. In this section we re-estimate the relationships of interest using the social register to identify couples. It is useful to check if the patterns we find hold for couples because some theoretical models and policies focus on intimate partner violence. It also allows us to investigate heterogeneity in the treatment effect by the presence of young children, and by baseline family income.

About 47% of the Brazilian population is registered for a social programme and thus appears in the social register, and they are selectively poor. We identify couples as XX(define partners- married and cohabiting?) partners living in the same household. We then identify the man and woman in the employment register using XX their unique tax identification number. A limitation XX is that we only retain individuals that, at some point, have held a formal sector job, which reduces our sample to XX percent of all couples in the social register. Merging the social welfare and employment registers we have a sample of 7.4 million male and females workers in the age range of 18-60 who were dismissed without a just cause between 2012 and 2014. Replicating the matching procedure described above, we are able to match just over 6 million of the workers to a control unit. In line with our main analysis,

[12] Compared with our baseline estimate in Figure **??**, the impact in the first year is attenuated as treated workers are dismissed throughout the calendar year, while the first period in our baseline estimates is set relative to the layoff date.

[13] In fact our analysis of cases resulting in protective measures rather than prosecution is also robust to the timing problem because protective measures are required by law to be issued within 48 hours of reporting to the police.

we focus on the behavior of men aged 20-50 years who have a unique name in the country. Due to the selection of households present in CadUnico, baseline DV rates in this analysis are above average compared with the general population. Also the DV outcome is restricted to preventive orders because the strategy now involves identifying the name of the man and woman and, as discussed earlier, we only have XX(number or percent) cases of prosecution orders in which the name of the woman is identified.

Results are in Table XX and XX. Male job loss is associated with a 75% (0.14 pp) increase in the chances of the man being subject to a preventive measure following a case filed by his partner. This is larger than the increase in DV that we estimated using all available cases against male perpetrators. It is not sensitive to whether or not the couple has a child under the age of ten, or to household income at the date of layoff being above or below XX XX(explain choice 95 as the cut). Female job loss is estimated to increase the chances that a woman files for a protective measure by 83% (0.05 pp), which is almost identical to the estimate obtained using all cases. For female job loss there is evidence of heterogeneity by family characteristics. Women are more likely to be victimized (or more likely to report victimization) when the family has a child under the age of ten and when the family is relatively poor at the time of layoff. XX These differences border on significance.

Use of public shelters for women: Alternative measure of DV. The welfare register contains information on whether and when women used public shelters intended to shield women from domestic abuse. This allows us to test whether job loss influences domestic abuse using an alternative measure of abuse to that provided by the court data. XX describe data set up and limitations here. Following male job loss we find about a 32% (0.09 to 0.12 pp) increase and following female job loss an approximately 47% (0.02 to 0.033 pp) increase in the probability that a woman uses a shelter. Thus our finding that both male and female job loss lead to an increase in DV is robust to using this alternative measure of DV. In the next section we discuss whether these results might be driven by endogenous reporting bias.

9 Investigating Reporting Bias

It is a stylised fact that women the world over are reluctant to report DV to the police for reasons that include trust in the criminal justice system, social stigma, social norms, concern over the consequences for their children, or their own financial reliance on their partner. So the cases in the court registers will only be a fraction of all DV incidents. If the probability of being prosecuted or receiving a preventive order conditional on having committed domestic violence is constant, our estimates will be biased towards zero but the estimate relative to the mean will be unaffected. For this reason, all tables show the coefficient scaled by the baseline mean. In this section we focus on the separate concern that the propensity to report depends on job loss.

A common problem with analysing data on reported acts of violence is that it can be very difficult to disentangle changes in the actual incidence of violence from changes in reporting behaviour. In particular, women may be more likely to report violence when the man has lost his job, and less likely to report it when they themselves lose their jobs, for instance because they are financially reliant on

their husbands. This raises the concern that our estimates of the relationship between job loss and DV are driven by endogenous reporting. We tackle this in multiple ways.

First, as discussed, our results hold when we replace court measures of domestic violence with women's use of public shelters. However, women may be dissuaded from accessing shelters for the same reasons that they are reluctant to report cases to the police. So while it is compelling to find exactly the same pattern of results for male vs female job loss, it does not fully address reporting bias.

Second, we note that if male and female job loss influence reporting in the hypothesised direction, then we would expect to see an increase in reported violence following male job loss, but a decrease in reported violence following female job loss. Since in fact we find an increase in reported violence (and an increase in use of public shelters) following male *and* female job loss, it seems unlikely that our results are driven by reporting. However, it does remain possible that the results for male job loss are driven by changes in reporting. We therefore now show further checks for male job loss.[14]

We conjecture that women only report domestic violence when it is above a given tolerance level. So less severe cases get reported when that threshold shifts to the left and it seems plausible that this happens when the man loses his job. We leverage the richness of the court data to classify cases by severity of the charge in two ways- first by type of violence (e.g. physical battery and assault may be deemed more severe than threats of violence) and, second, by maximum jail sentence associated with the charge. If changes in reporting rather than changes in actual violence were driving our results, we would see a stronger relationship between job loss and domestic violence for less severe cases. But we find the opposite, see Table XX.

Next, we conjecture that older women who are more likely to be in a long-term relationship and have children with the perpetrator are more likely to under-report DV to protect their families. If this is right and if reporting behaviour drives our results, we would expect to find larger impacts of male job loss on reported violence among older women. In fact, a statistically significant relationship holds in every age group and is in fact largest for the youngest women, age 20-24, see Table XX.

While all of these tests pulled together undermine the concern that changes in reporting drive our results, the most conclusive way to avoid conflation by reporting choices is to study cases where that choice is stripped out (of the equation). This is so for in flagrante cases which, as discussed earlier, are cases in which the perpetrator is "caught in action". As shown in Figure XX and Table XX (referenced in the previous section), male job loss has an even stronger impact on DV when we restrict the sample to these cases. As a final check on reporting but also because it is of substantive interest, we investigated impacts of male (and female? XX) job loss on femicide. We find XX, see Table XX.

[14] We acknowledge that if the direction of the reporting bias were the opposite of that conjectured- such that women are *more* likely to report violence after they lose a job- then our findings for women's job loss are also potentially driven by reporting. As discussed, the sample for female job loss is substantially smaller, making it harder to estimate heterogeneity by victim or crime characteristics, and there simply are not enough in flagrante cases in the protective measures category. However, if we can show that the results for male job loss are not driven by reporting then it is unlikely that the results for female job loss are.

10 Heterogeneity by Worker Characteristics

In the preceding section we examined heterogeneity in impacts of male job loss by age of the woman and by severity of the alleged offence. In this section we investigate heterogeneity in impacts of male job loss by age, education, income and tenure of the man. We similarly estimated heterogeneity in impacts of female job loss by characteristics of the woman. Since we have a much smaller sample for the case of female job loss, these estimates are imprecise and so we only briefly mention these results.

Since the worker characteristics we investigate are correlated with one another, we estimate models in which we include interactions of job loss with each characteristic. In an alternative specification, following XXHainmueller (2012) we re-weight the sample to match first and second moments of each characteristic as well as race, state and industry of a baseline group for the characteristic of interest. Tables **??** report the interaction coefficients and the relative effects, defined as the estimated coefficient divided by the group mean among treated units at $t = 0$. We observe a statistically significant relationship for every age, education and income group, but a sharp gradient in tenure, indicating that it is only low tenure workers that commit domestic violence following job loss. We now elaborate these results.

Men with less than high school are more likely than men with higher education to commit DV following job loss. However, this relationship is not stronger among men with lower earnings at baseline than it is among higher earning men- in fact the absolute and relative increase is largest for men with baseline earnings in the top quartile. The most stark result is that the relationship is entirely driven by men with tenure less than three years at the time of layoff. The coefficients (unscaled and scaled by the mean) are close to zero for men with more than 3 years tenure.

This sharp shift is clear irrespective of whether we condition upon job loss gradients in age, income and education of the man, or if we re-weight to match all available characteristics of the lowest tenure group. Once this is done, the relevant observable difference between low and high tenure workers is in eligibility for severance pay and unemployment benefits. For every tenure year at displacement, workers receive about 1.3 monthly wages as dismissal indemnities. Also workers dismissed with less than 5 tenure months are not eligible for unemployment insurance, workers with 6-11 tenure months receive 3 months of benefits, workers with tenure 12-23 months receive 4 months, and those with more than 24 months tenure receive 5 months. Thus, on average, workers with more than 3 years tenure receive XX monthly wages over the 5 months after layoff in contrast to XX monthly wages among workers displaced with less than 3 years tenure.

11 Eligibility for Unemployment Insurance

The results shown so far establish that unemployment of men and women tends to escalate DV. In this section we investigate whether unemployment benefits act to mitigate this tendency. We focus on male job loss and benefits for men because the RD sample for the analysis is smaller than for the main analysis, and estimates for the particularly small sample of women are imprecise. We nevertheless briefly report the results.

11.1 Research design

Workers in the formal sector in Brazil workers are eligible for between 3 and 5 months of unemployment benefits when dismissed without a just cause as long as they satisfy two conditions, namely, that they have been in continuous employment in the 6 months prior to layoff, and that a minimum of 16 months has elapsed between the current layoff date and the last layoff date used to claim UI in the past. For instance, a worker who claims UI benefits following a dismissal in January 1^{st} 2010 will be able to claim benefits again if dismissed from April 30^{st} 2011. We select the group of workers satisfying the first condition and leverage the sharp change in eligibility at the 16-month cutoff implied by the second condition in a regression discontinuity (RD) design. Thus, we compare the behavior of workers who are just eligible for benefits with that of behaviours who are just ineligible as follows:

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

where Y_i is an indicator variable for the i-th worker being charged with DV after job loss; X_i is the running variable, which is time elapsed since the previous layoff standardized so that $X = 0$ at 16 months, the cutoff required for eligibility); $f(.)$ is a flexible polynomial regression; and D_i is a dummy taking the value of one for workers who are eligible for UI (i.e. $D = 1(X_i \geq 0)$). To ensure comparability between eligible and non-eligible workers and avoid extrapolation bias in the regression, our main estimates are based on a local linear model with a narrow bandwidth of 60 days. We show that our findings are robust to a range of bandwidths, including the optimal bandwidth of (Calonico, Cattaneo and Titiunik, 2014), and to alternative polynomial choices. We will also present permutation tests, comparing our discontinuity estimates with a distribution of estimates at placebo cutoffs.

The coefficient β in equation (3) estimates the effect of UI eligibility, or equivalently, the intention-to-treat effect of UI claims. We do not have data for actual UI claims in our sample, but Gerard, Rokkanen and Rothe (2019) show for an earlier period (2004-2008) that the take-up rate at the cutoff was 70%. Under the assumption of a similar take-up rate in our sample, the treatment effect of actually receiving UI benefits would be $\beta/0.7$.

11.2 Sample construction, cyclicality in dismissal dates, balance tests

We restrict the initial sample of full-time workers holding open-ended jobs in the non-agricultural private sector to include only dismissed workers with at least 6 months of continuous employment at the time of dismissal. We focus on dismissals occurring during 2009-14 because numerous changes were implemented to the UI system in 2015. We need to address the fact that there are cyclical patterns in dismissal dates, with firms concentrating layoffs on the first and last days of the month, see Appendix Figure **??**.[15] This creates discontinuities in the density of the running variable in roughly 30-day cycles. Consequently, 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

[15] There is a missing mass on the 31^{st}, which is explained by the 30-day advance notice period. In months comprising 31 days, a dismissal notified in the 31^{st} actually takes place on the 30^{th} of the following month. In months comprising 30 days, a dismissal notified on the 30^{th} also takes place on the 30^{th} of the following month.

from April 30^{st} 2011. Given the dismissal cycle, when reemployed, she/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. This creates a (mild) discontinuity in the density function, albeit not specific to the 16-month period that is relevant for UI eligibility, see Figure **??** in the Appendix.

We address this issue in two ways. In our baseline specification, we restrict the sample to workers who were initially dismissed between the 3^{rd} and 27^{th} of the month, in such a way that the 16-month cutoff date does not overlap with the dismissal monthly cycles. Importantly, this restriction is based on the initial layoff date – determining the RD cutoff – and not the current layoff date determining the running variable. Figure **??** shows no evidence of density discontinuity around the 16-month cutoff in this restricted sample, as also confirmed by the McCrary density test and the bias robust test developed in **??**. In addition, the bottom graphs in Figure **??** show no significant difference in prosecutions within one semester and 3 years before displacement, respectively. Finally, Figure **??** in the Appendix provides balance tests for a rich set of (pre-determined) worker characteristics. Taken together, these figures provide compelling evidence that displaced workers are "as good as randomly assigned" near the cutoff. The second approach we take to deal with cyclicality in dismissal dates is to include all workers in the sample and add cutoff and dismissal date fixed effects in the RD regressions.

11.3 Results

The results in Table XX and Figure **??** show that, in the sample of displaced male workers near the discontinuity, there is no significant difference in the probability of DV between workers eligible vs ineligible for UI in the first year following dismissal. However, eligible workers exhibit a 24% higher propensity to commit DV from the second year. Rescaling this reduced form coefficient by the first-stage increase in take-up estimated by Gerard, Rokkanen and Rothe (2019), the average effect on compliers is 34.3% which happens to be very similar to the main effect of male job loss on DV.

These results are robust to alternative bandwidths and to replacing the linear with a polynomial expression for the running variable, see Appendix Table **??**. They also stand up to adjusting for cyclicality in hiring and firing, see Table **??**. In the first four columns, we progressively include fixed effects for the individual-specific cutoff date and for each dismissal date, the parameters that define the running variable, so that now the estimates rely upon variation in worker-specific dismissal dates within groups who have the same cutoff date. In the last two columns, we enlarge the sample to include all workers who were initially dismissed near the beginning and end of the calendar month, thus dropping our initial restriction. The estimates are now larger and more precisely estimated than in the baseline specification of Table **??**. The same is true when extending the sample to include all individuals with a unique name within each state (Table **??**). These additional results suggest that the somewhat weak significance of the average coefficient in the baseline specification of Table **??** likely reflects the relatively small number of observations near the cutoff.

To supplement the RD analysis, we implement the difference in difference design used in analysis of the main effect of job loss on DV but now distinguishing samples of eligible and ineligible men. Although statistical power is now challenged by the smaller sample using unemployed men around

the threshold, we are able to replicate the broad pattern. We see, first, that there is a jump in DV for both groups of displaced men. Second, we see that, in the first year after dismissal, eligible men commit less crime than the ineligible but that the curves cross in the second year. In neither case are these differences significant but the patterns replicate the patterns revealed in the RD analysis.

To recapitulate, the evidence is that, while benefits are potentially being received, they fail to have any influence on DV and, worse, they that eligibility has a perverse DV-increasing effect once benefits expire. We propose that both of these results can be explained by the fact that workers who are just eligible for UI have longer unemployment durations than just ineligible workers, see Table XX which shows that the average difference during the three years post-dismissal is 6.55 weeks or 19%. In the first semester, when benefits are potentially flowing, the difference is 0.78 months (or 3.38 weeks), attenuating to 0.68 months in the second year from dismissal. That UI eligibility leads to lower labour supply and longer unemployment durations is a stylized fact in the literature (Katz and Meyer (1990), Lalive (2008), Card, Chetty and Weber (2007)). That longer unemployment durations lead to higher DV is consistent with the perpetrator being at home for longer and hence facing increased opportunities for crime. Since the longer unemployment duration of eligible workers is apparent rightaway- in the first trimester (6 months)- after dismissal but DV only starts to rise after benefits expire, it seems plausible that the receipt of benefits ameliorates tendencies towards DV and that, in our sample, this acts to offset the role of exposure. We may have expected that eligible workers would smooth their consumption to avoid a dip in liquidity at the point of expiry of benefits. However recent evidence from Brazil shows that UI beneficiaries experience sharp drops in consumption upon benefit expiration (Gerard and Naritomi, 2020). Using US data, Ganong and Noel (2019) presents similar evidence documenting the lack of consumption smoothing among the unemployed.

Although we have experimental variation in cash benefits, we are not able to isolate the role of income constraints with this experiment, and the reason is that benefits have behavioural effects that re-introduce the relevance of time spent at home. It is important to note that we do not dismiss a potential for UI to mitigate impacts of job loss on DV. Rather, our findings highlight that UI has unintended effects on unemployment duration that countervail the beneficial effects of relaxing cash constraints. The implication for policy is that UI is more likely to protect women from the risk of DV after male job loss when it is accompanied by policies such as skills training or job search support that encourage men back into work.[16]

12 Mechanisms

As discussed in the Introduction, we gain insight into mechanisms by virtue of analysing the coefficients for male and female unemployment and investigating whether eligibility for unemployment benefits mitigates impacts of unemployment on DV. In this section, we first discuss why our results appear

[16] During our sample period of 2009-2014, UI was not conditional on meeting job search requirements or attending training. In the 2012-14 period, there were attempts to make benefits conditional on attendance of training programs (PRONATEC). However, data from the Ministry of Labor show that only 1.2% of UI beneficiaries participated in the program in this period. Therefore, there was no incapacitation effect from alternative labor training programs while unemployed.

inconsistent with commonly used constructs for DV and then argue that they are consistent with the mechanisms of exposure and liquidity constraints.

The household bargaining model (Manser 1980, McElroy 1981) predicts that male unemployment lowers DV, while female unemployment increases it (Anderberg et al 2016; also see Aizer 2010 who analyses relative wages instead of relative unemployment rates). The premise is that labour market opportunities of the partners influence their outside options and hence the proclivity of men to commit DV and the willingess of women to tolerate it. The male backlash model, emerging from sociology (Macmillan and Gartner 1999), reverses the predictions of the bargaining model. In particular, it argues that an improvement in the relative earnings of women primes male identity and, in regions where the male breadwinner norm is prevalent, triggers DV (Tur Prats 2020, Bhalotra et al. 2019). A common feature of these two constructs is that they predict that male and female job loss will move DV in opposite directions. But we find that male and female job loss move DV in the same direction. Our result that female job loss triggers DV is also at odds with the prediction of models of instrumental control (Bloch et al 2002, Anderberg et al 2011) that improved earnings for women trigger may DV by inciting men to appropriate a share of their income (Angelucci et al 2008, Bobonis et al. 2013, Carr and Packham 2020, Estefan 2019, Kotsadam et al 2020).

Our results are consistent with the exposure model originating in criminology (Dugan et al 2003) which predicts that DV is increasing in time the couple spends together. First it rationalizes the finding that male and female job loss both increase DV. Second, it rationalizes the result that UI-eligible workers (with longer unemployment durations are more likely to commit DV. The fact that DV among UI-eligible workers only increases when benefits expire indicates that relaxing liquidity constraints ameliorates impacts of job loss on DV. That liquidity constraints are a mechanism is also indicated by our finding that the identified impacts of job loss on DV are entirely driven by low tenure workers. Importantly this is not because low tenure proxies age, education or income. The tenure gradient is preserved once we condition upon gradients of impact by these worker characteristics. It continues to hold when we also effectively condition on race, state and industry. The salient observable difference between low and high tenure workers is in liquidity - severance payments are linearly increasing in tenure and UI payments step up from zero at less than 5 months tenure to 3, 4 and 5 months of benefits at 6, 12 and 24 months of tenure. Finally, the mechanism of liquidity constraints is also consistent with the main result of DV rising with male and female job loss.[17]

While we are unaware of causal evidence of the exposure mechanism in the economics literature on DV, it is consistent with the stylized fact that DV tends to escalate on holidays, weekends and nights (Vazquez et al 2005) and in periods of bad weather (RAINN). It is the reverse of incapacitation effects of employment on crime (XX) and of schooling on risky behaviours in adolescence (XX). Recent media coverage of the Covid-19 surge in DV (UN Women 2020) implicitly refers to exposure by making reference to couples being stuck together during lockdown, with women unable to ”escape”. However, liquidity constraints associated with male and female job loss during the pandemic may

[17] In principle, we might look to test heterogeneity by tenure in the RDD analysis of UI eligibility but in practice workers included in the RDD sample have low tenure by construction, and there is not enough variation in tenure to do this.

have contributed along with increased exposure. It is plausible that liquidity constraints cause stress which triggers conflict. This is broadly consistent with research highlighting expressive (as opposed to instrumental) motives for DV (Card et al. 2011). Recent work shows that liquidity constraints can generate cognitive stress (Cassidy 2019). While they do not identify if liquidity constraints are a pathway, a number of studies show that male job loss results in psychological stress (Kuhn et al. 2009, Sullivan et al. 2009).[18] Although the literature on DV in economics has not, to our knowledge, identified the role of liquidity constraints, Haushofer et al. (2019) show that unconditional cash transfers to men reduce DV.[19]

13 Conclusions

Our main finding is that male and female job loss lead to an increase in domestic violence. These effects persist through the four years in the data. There is no evidence of anticipation effects and the results are robust to the definition of layoff, the definition of matched controls, controls for unobserved local shocks and missing data. The same pattern emerges using data on couples and the pattern is robust to changing the measure of DV from preventive measures imposed upon men to the use of public shelters by victimized women. For male job loss, we find that the effects are fairly pervasive, being evident across the distribution of age, education and (baseline) income of the perpetrator. A number of checks suggest that our findings are unlikely to be driven by endogenous shifts in reporting. Most clearly, we find a large positive impact of job loss on DV when we restrict to incidents that are not dependent upon the victim reporting but, rather, are caught in action.

Comparing workers with low vs high tenure at displacement after adjusting for all observable differences indicates that access to severance pay and benefits can mitigate impacts of job loss on DV. This points to liquidity constraints as a mechanism. A separate exercise that leverages experimental variation in eligibility for unemployment benefits also suggests that relaxing liquidity constraints attenuates impacts of job loss during the months in which benefits are being paid. However, this appears to be offset by greater exposure given that eligible men exhibit longer unemployment durations than ineligible men. Analysis of the dynamics of the relationship reveals that, once benefits expire, men who were eligible for benefits are more likely to commit violence. The upshot for policy is that unemployment benefits do have the potential to keep DV in check following male job loss but they need to be accompanied by strategies that get the men out of the house and into productive activities, for example skills training, job search support or incentives to return to work. The same applies to severance pay.

Overall, our analysis of mechanisms suggests that job loss leads to an escalation of domestic violence both because it leads to a tightening of liquidity constraints and because it leads to families

[18] There is also some evidence that male UI duration is associated with depression and cardiac events, see Ahammer et al 2020).

[19] Other studies of DV have investigated cash transfers to women (Bobonis et al. XX, Angelucci XX) as part of a broader agenda seeking to identify whether interventions that improve the economic empowerment of women are effective in reducing DV (Watt et al XX, Kotsadam and Villanger 2020). While some studies find beneficial effects, others find an increase in DV and attribute this either to male backlash or to instrumental control. Overall, liquidity constraints as a mechanism is under-explored.

spending more time together. A new and important insight of this paper is that what may appear to be a natural policy response- provision of unemployment benefits- can badly misfire if it generates behavioural responses that lead to men spending even longer at home.

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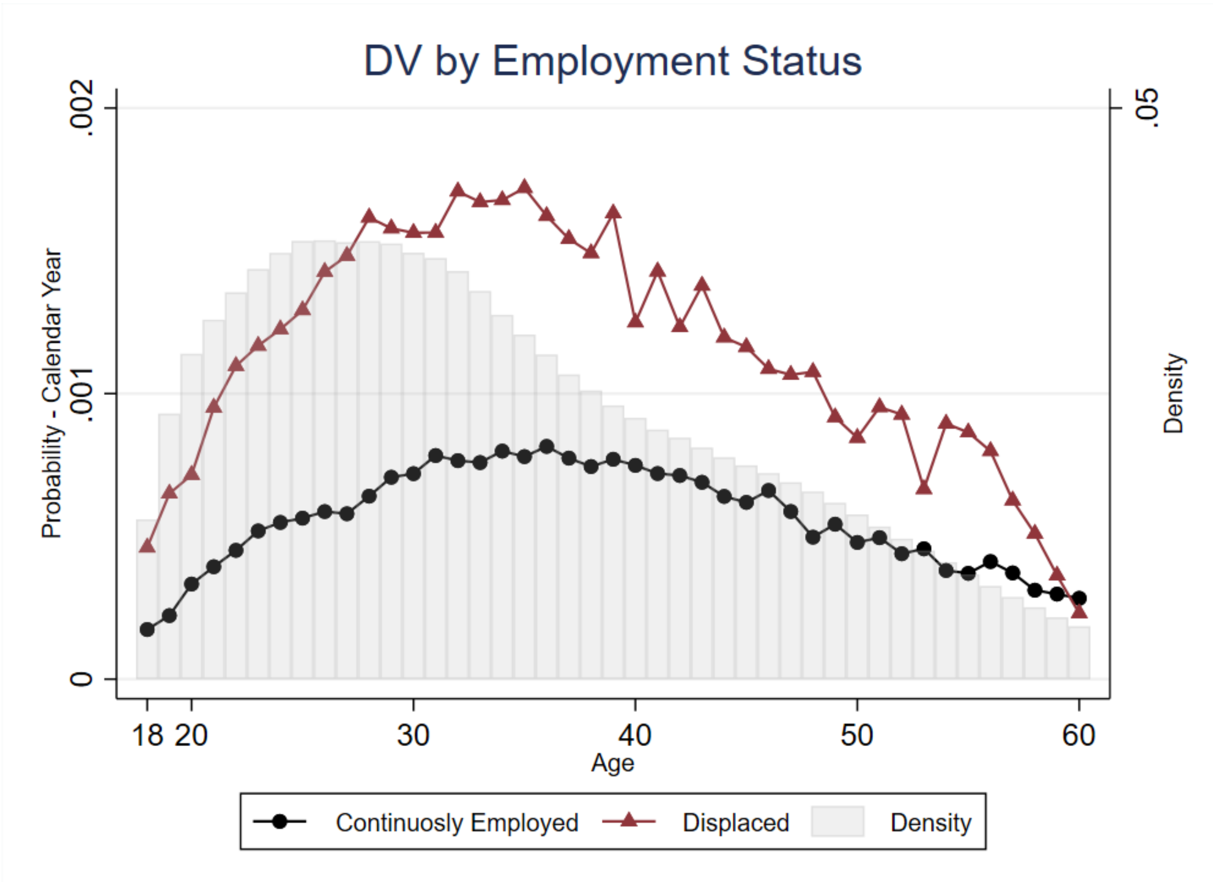
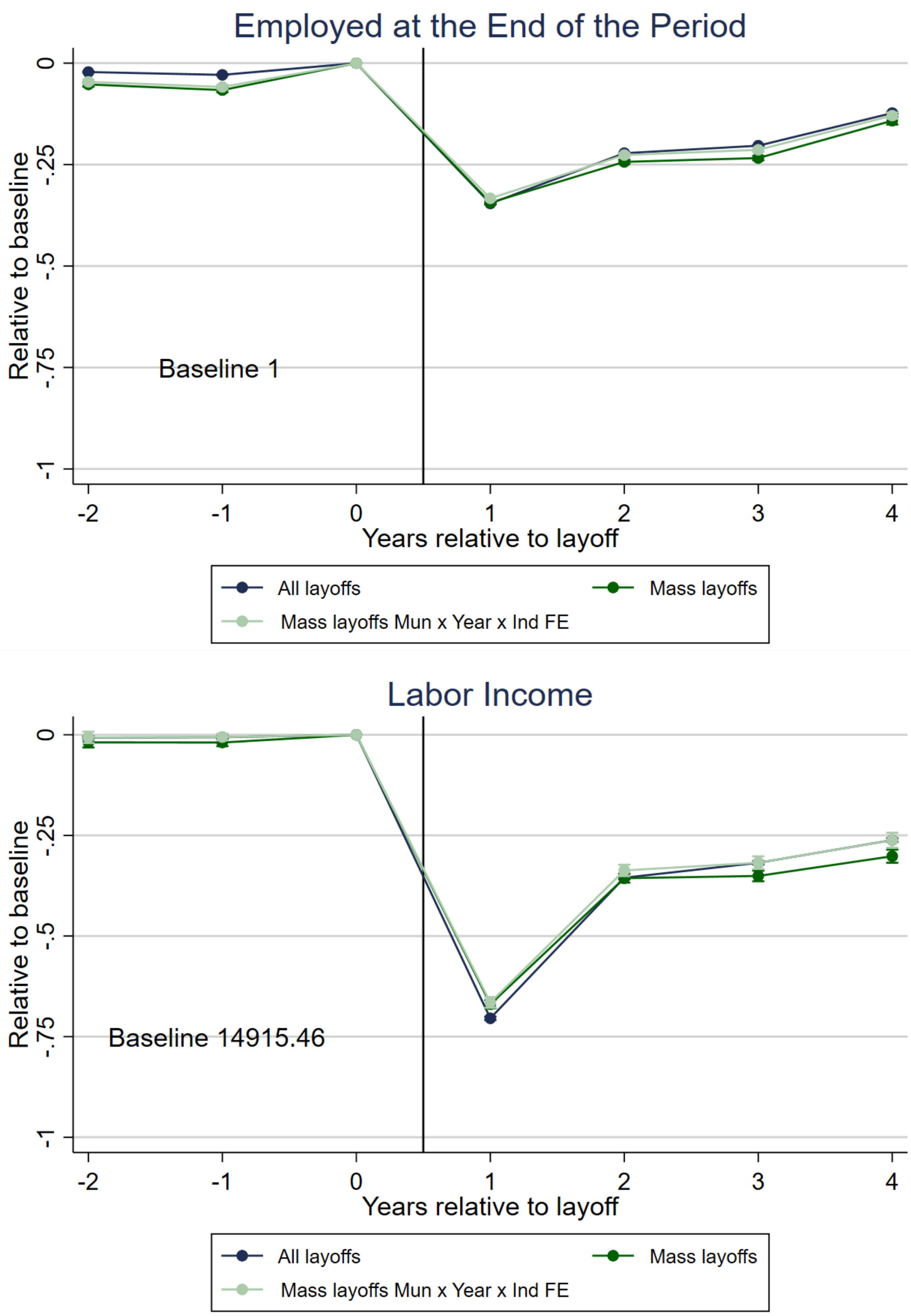
Figure 1: Domestic violence by employment status and age



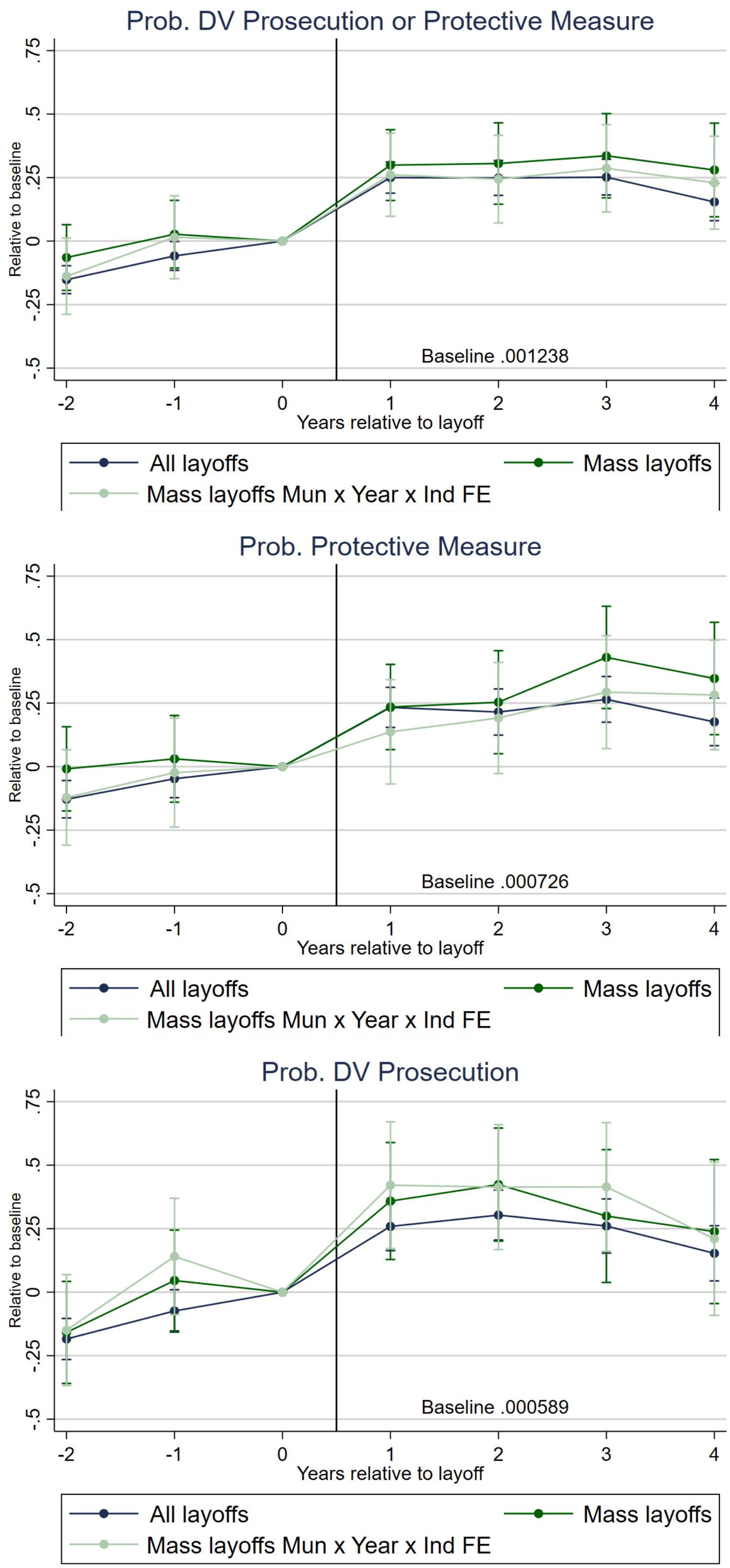
Notes. The top graph compares the average probability of being prosecuted in a given year between, respectively, workers that are continuously employed and workers losing their job in that year, respectively, by age. The distribution of age is displayed in gray.

Figure 2: The effect of *male* job loss on employment and earnings



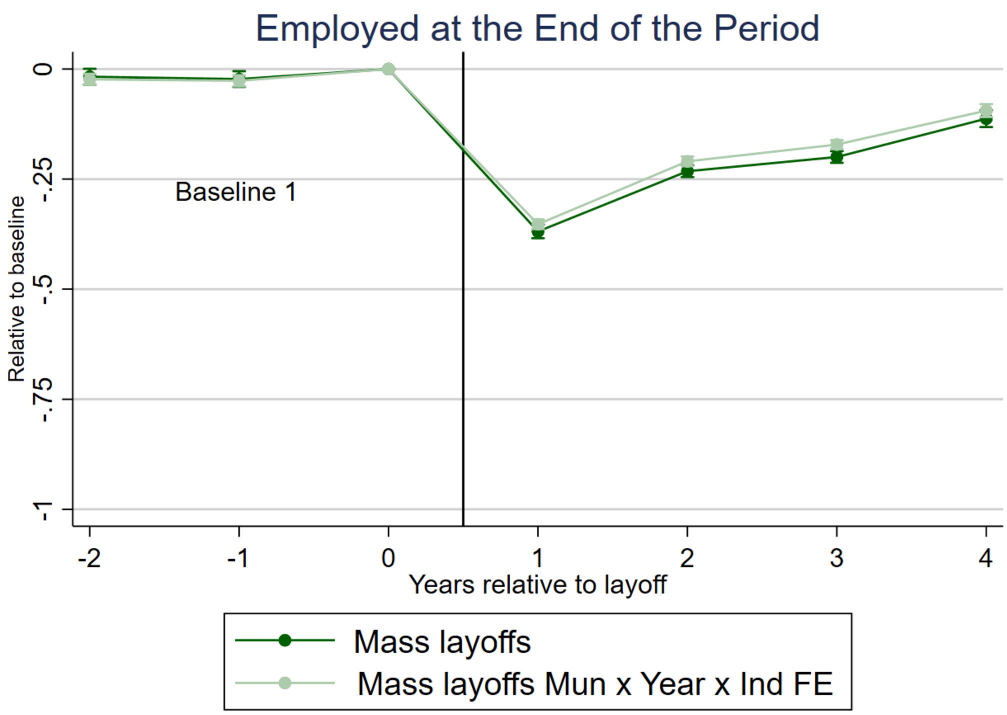
Notes. This figure shows the effect of job loss on individual employment (top) and labor earnings (bottom).

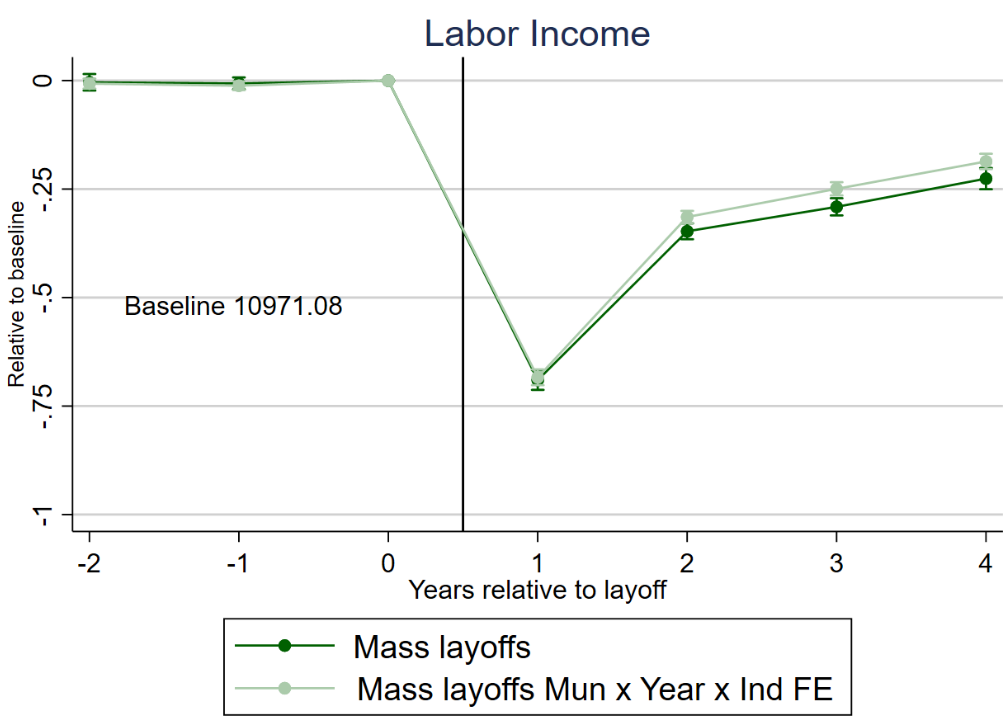
Figure 3: The effect of *male* job loss on the probability of DV prosecution or protective measure



Notes. This figure shows the effect of job loss on the probability of being prosecuted for DV or protective measure.

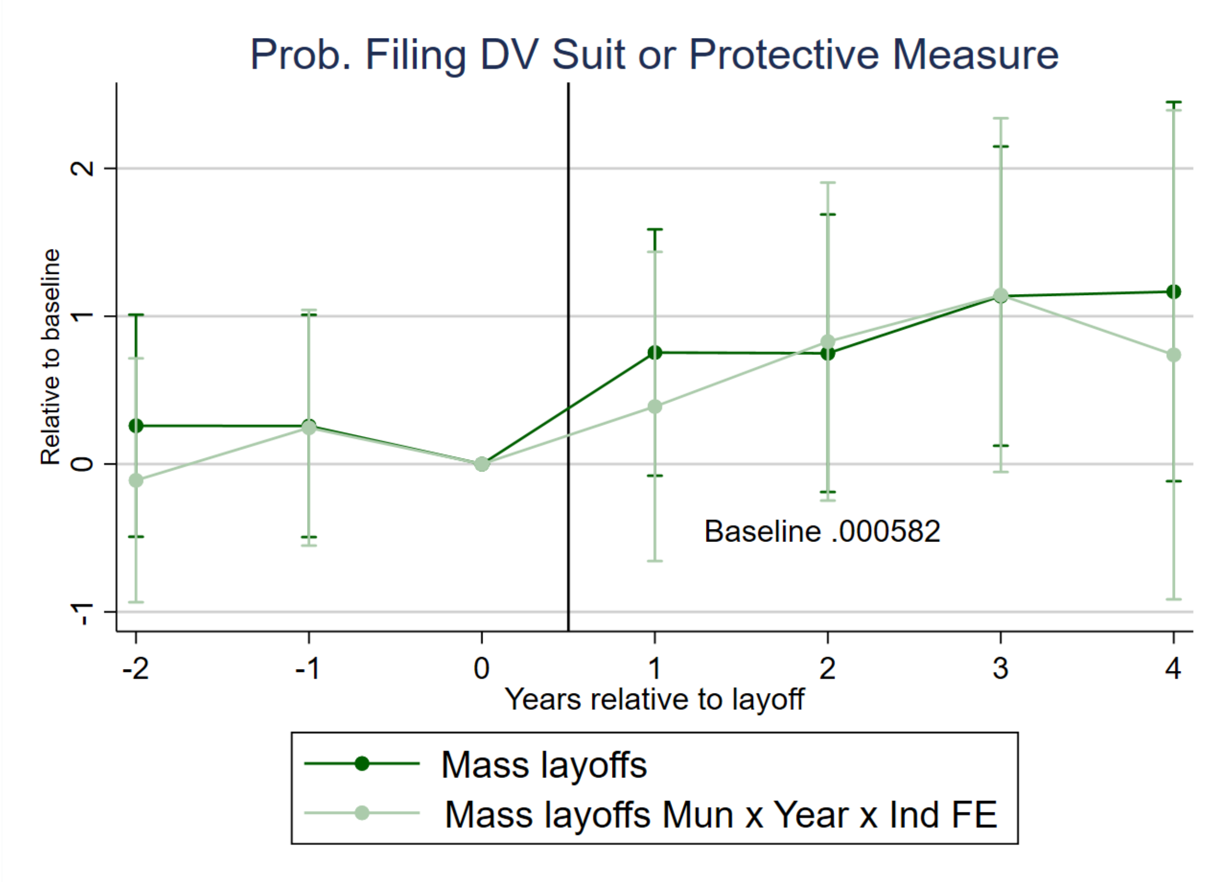
Figure 4: The effect of *female* job loss on employment and earnings





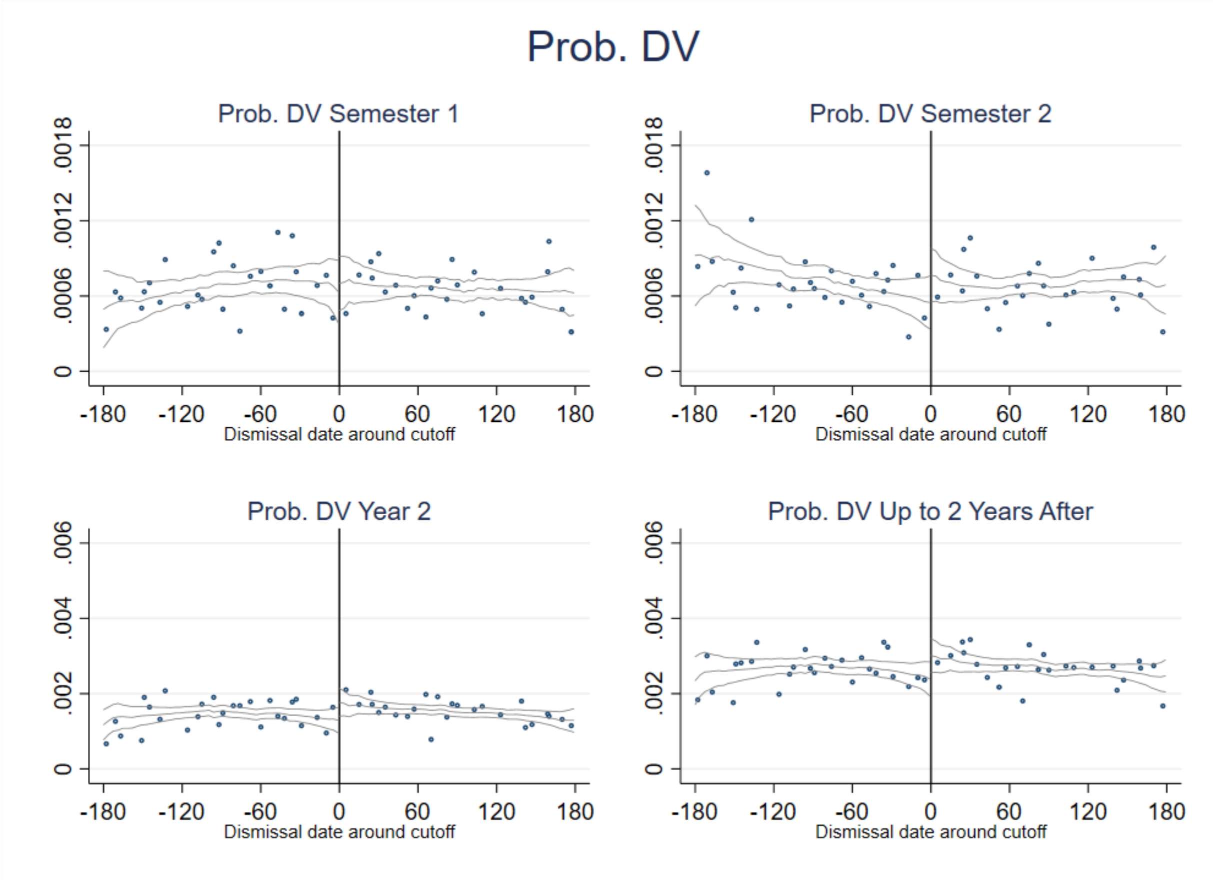
Notes. This figure shows the effect of job loss on individual employment (top) and labor earnings (bottom).

Figure 5: The effect of *female* job loss on the probability of filing a DV prosecution or protective measure



Notes. This figure shows the effect of job loss on the probability filing a DV prosecution or protective measure.

Figure 6: The effect of UI eligibility on DV prosecution and protective measure



Notes. The graphs show how DV prosecution evolve around the threshold. Dots represent averages based on 5- day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

Table 1: The effect of *male* job loss on labor market outcomes and DV

Dependent variable:	Labor market effects		Probability of DV
	Employment	Income	Any
	(1)	(2)	(3)
PANEL A: ALL DISPLACED WORKERS			
Effect of job loss	-0.21***	-6047.9***	0.00037***
	(0.0007)	(31.4)	(0.00003)
Mean outcome, treated at t=0	1	14,915	0.0012
Effect relative to the mean	-21%	-41%	30%
Elasticity to earnings			-0.74
Observations	59,193,078	59,193,078	59,193,078
PANEL B: DISPLACED IN MASS LAYOFFS			
Effect of job loss	-0.20***	-5967.1***	0.00045***
	(0.002)	(72.4)	(0.00007)
Mean outcome, treated at t=0	1	14,664	0.0014
Effect relative to the mean	-20%	-41%	32%
Elasticity to earnings			-0.79
Observations	11,381,006	11,381,006	11,381,006
PANEL C: DISPLACED IN MASS LAYOFFS - MUN X IND X YEAR FE			
Effect of job loss	-0.19***	-5741.3***	0.00042***
	(0.002)	(90.2)	(0.00008)
Mean outcome, treated at t=0	1	14,664	0.0014
Effect relative to the mean	-19%	-39%	30%
Elasticity to earnings			-0.76
Observations	11,341,085	11,341,085	11,341,085

Notes. This table shows the effect of *male* job loss on labor market outcomes (columns 1-2) and the probability of DV (column 3), as estimated from the difference-in-differences equation (**??**). The dependent variable is indicated on top of each column. Panel A includes in the sample all displaced workers and matched control workers employed in non-mass layoff firms that are not displaced in the same calendar year; Panel B restricts the treated group to workers displaced in mass layoffs; Panel C adds municipality X industry X year fixed effects (5,565 municipalities and 27 industries). 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 over the percent change in earnings. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 2: The effect of *male* job loss on DV, by age

Dependent var.: Prob. of DV	Age			
	[20,24]	[25,29]	[30,39]	[40,50]
	(1)	(2)	(3)	(4)
Effect of job loss	0.00050***	0.00052***	0.00028**	0.00061***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Mean outcome, treated at t=0	0.0008	0.0015	0.0019	0.0013
Relative variation	65%	36%	15%	48%
Observations	2,839,116	2,842,154	3,889,018	1,810,718

Notes. This table shows the effect of *male* job loss on DV, as estimated from the difference-in-differences equation (**??**). Columns (1) to (4) varies the age interval, indicated on top of each column. 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. Standard errors clustered at the firm level are displayed in parentheses (*** p$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.1).

Table 3: The effect of *male* job loss on DV, by crime type

Dependent var.: Prov. of DV				Assault		Against
	Slander	Threatening	Battery	Ordinary	Severe	Freedom
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: ALL DISPLACED WORKERS						
Effect of job loss	0.0000329***	0.0000736***	0.00000788**	0.0000870***	0.00000270**	0.0000103***
	(0.00000736)	(0.0000124)	(0.00000374)	(0.0000144)	(0.00000131)	(0.00000262)
Mean outcome, treated at t=0	0.000122	0.000359	0.000035	0.000466	0.000002	0.000017
Relative variation	27%	21%	23%	19%	163%	61%
Observations	59,193,078	59,193,078	59,193,078	59,193,078	59,193,078	59,193,078
PANEL A: DISPLACED IN MASS LAYOFFS						
Effect of job loss	0.0000546**	0.000157***	0.0000139	0.000126**	0.00000277	0.0000134**
	(0.0000188)	(0.0000328)	(0.00000886)	(0.0000392)	(0.00000333)	(0.00000572)
Mean outcome, treated at t=0	0.0001223	0.0003588	0.0000345	0.0004662	0.00000166	0.0000168
Relative variation	45%	44%	40%	27%	167%	80%
Observations	11,381,006	11,381,006	11,381,006	11,381,006	11,381,006	11,381,006

Notes. This table shows the effect of *male* job loss on different types of DV crimes (indicated on top of each column), as estimated from the difference-in-differences equation (**??**). 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. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 4: The effect of *male* job loss on DV, by crime severity

Dependent var.: Prob. of DV	Crime Severity by Max Jail Time		
	0-4y	4+y	Murder/Attempt
	(1)	(2)	(3)
PANEL A: ALL DISPLACED WORKERS			
Effect of job loss	0.0000481***	0.0000436***	0.00000071
	(0.00000929)	(0.00000772)	(0.000000542)
Mean outcome, treated at t=0	0.0002091	0.0001523	0.0000005
Relative variation	23%	29%	150%
Observations	59,193,078	59,193,078	59,193,078
PANEL B: DISPLACED IN MASS LAYOFFS			
Effect of job loss	0.0000483*	0.0000590**	0.00000195
	(0.0000258)	(0.0000205)	(0.00000139)
Mean outcome, treated at t=0	0.0002091	0.0001523	0.0000005
Relative variation	23%	39%	412%
Observations	11,381,006	11,381,006	11,381,006

Notes. This table shows the effect of *male* job loss on DV crimes by severity of punishment (indicated on top of each column by max jail time), as estimated from the difference-in-differences equation (**??**). 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. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 5: The effect of *female* job loss on labor market outcomes and DV

Dependent variable:	Labor market effects		Probability of Filing
	Employment	Income	Protective Measure
	(1)	(2)	(3)
PANEL A: DISPLACED IN MASS LAYOFFS			
Effect of job loss	-0.21***	-4229.4***	0.00039**
	(0.004)	(64)	(0.0001)
Mean outcome, treated at t=0	1	10,966	0.0006
Effect relative to the mean	-21%	-39%	67%
Elasticity to earnings			-1.74
Observations	1,026,683	1,026,683	1,026,683
PANEL B: DISPLACED IN MASS LAYOFFS - MUN X IND X YEAR FE			
Effect of job loss	-0.19***	-4040.3***	0.00035**
	(0.003)	(55.5)	(0.0001)
Mean outcome, treated at t=0	1	10,966	0.0006
Effect relative to the mean	-19%	-37%	60%
Elasticity to earnings			-1.64
Observations	1,019,088	1,019,088	1,019,088

Notes. This table shows the effect of *female* job loss on labor market outcomes (columns 1-2) and the probability of DV (column 3), as estimated from the difference-in-differences equation (**??**). The dependent variable is indicated on top of each column. Panel A restricts the treated group to workers displaced in mass layoffs; Panel B adds municipality X industry X year fixed effects (5,565 municipalities and 27 industries). 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 over the percent change in earnings. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).