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ABSTRACT

The Effect of Job Loss and Unemployment Insurance on Crime in Brazil*

We investigate the effect of job loss and unemployment benefits on criminal behavior, exploiting individual-level data on the universe of workers and criminal cases in Brazil over the 2009-2017 period. We match workers displaced upon plausibly exogenous mass layoffs with observationally-equivalent control groups to identify dynamic treatment effects of job loss while allowing for treatment effect heterogeneity. In our preferred specification, the probability of criminal prosecution increases by 23% upon job loss and remains approximately constant during the following years. Our unusually large dataset allows us to precisely estimate increases in almost all types of crimes - including offenses with no economic motivation - as well as spillover effects on other household members. The estimated effects remain robust when restricting to arrests "in flagrante", which are less subject to differential reporting by employment status. We then evaluate the mitigating effect of unemployment benefits leveraging on discontinuous changes in eligibility. Regression discontinuity estimates suggest that unemployment benefits covering 3 to 5 months after displacement completely offset potential crime increases upon job loss, especially for liquidity-constrained individuals, although this effect completely vanishes upon benefit expiration. Our findings point at liquidity constraints and psychological stress as main drivers of criminal behavior upon job loss, while substitution between time on the job and leisure does not seem to play an important role.

JEL Classification: K42, J63, J65

Keywords: unemployment, crime, unemployment insurance, registry data

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1 Introduction

Crime imposes a heavy burden on societies, especially during economic downturns, as unemployment and low earning opportunities reduce the opportunity cost of committing crimes (Becker, 1968). In a related manner, liquidity-constrained workers may turn to crime immediately upon job displacement to afford subsistence consumption. In addition, unemployment brings an increase in leisure time, which in turn may increase the probability of encountering criminal opportunities: put differently, employment may exert an "incapacitation" effect on potential offenders, which vanishes upon job loss. Finally, criminal behavior may also respond to the emotional distress caused by job loss (the latter being documented, among others, by Black et al., 2015; Schaller and Stevens, 2015). Through this latter mechanism, job loss may also affect the propensity to commit "crimes of passion", defined by Ehrlich (1996) as murders and other violent crimes with little or no economic payoff.

Therefore, there are several reasons to expect that criminal behavior responds to job loss. However, until very recently the evidence on such an effect remained very scant. In an earlier survey on this topic, Freeman (1999) concluded that joblessness is not the overwhelming determinant of crime that many analysts and the public a priori expected it to be". An important reason for this gap between theory and empirics is that confidentiality concerns delayed the distribution of large-scale, individual-level datasets comparable with those available for research on e.g. consumption, health, and taxation (Einav and Levin, 2014). With a few notable exceptions discussed below, previous empirical analyses relied on aggregate unemployment and crime rates across geographical areas, which pose significant challenges for identifying causal effects and distinguishing between alternative mechanisms.

In this paper, we link individual-level data on employment and criminal behavior for the universe of (male) workers in Brazil over the 2009-2017 period. Specifically, our dataset combines employer-employee data on employment spells and earnings; the universe of criminal cases filed in the Brazilian judiciary; and social registries, allowing us – among other things – to recover the household composition for almost half of our sample. This very rich data allow us to precisely estimate the effect of job loss on the probability of committing (different types of) crime and spillover effects on other household members, the mitigating effect of unemployment insurance schemes, and to gain insights into the mechanisms driving such effects.

In the first part of our analysis, we estimate dynamic treatment effects of job displacement by comparing the criminal behavior of displaced workers before and after displacement with a matched control group of workers who were not displaced in the same year. The dimension of our dataset allows us to finely match treated and control individuals on several characteristics (location, firm size and sector, birth cohort, tenure, and wages), controlling for local economic shocks at a very granular level of geographic and sectoral disaggregation. In addition, we leverage variation in employment from mass layoffs – defined as firms dismissing a sizable share of their workforce – and plant closures, as such events should clearly not depend on the criminal behavior of each specific worker nor other individual-level shocks that simultaneously affect employment and crime.[1]

Our findings indicate that the probability of committing a crime increases by 23% over the baseline in the year following dismissal, and it remains stable up to four years after the layoff (the end of our time frame). The average effect reflects an increase in both economically-motivated crimes (+43%) and violent crimes (+17%), and it is considerably stronger for groups that are more likely to be liquidity constrained upon job loss, namely younger workers and those with low job tenure and low educational attainment. However, the probability of committing crimes significantly increases for all groups – including workers with above-median income – albeit to a lesser extent. We also detect important spillovers of parental job loss on children. In particular, the probability of committing a crime increases on average by 18% for the cohabiting sons of displaced workers.

The results are robust to replicating the analysis at the monthly level and restricting to offenders arrested "in flagrante" (i.e. while committing a crime) to reduce measurement error from delays in judicial prosecutions and potentially differential reporting by offenders' characteristics, including employment status. The results are also robust to a variety of empirical exercises aimed at minimizing the scope for selection into job loss (even within mass layoffs). First, we adopt progressively more stringent definitions of mass layoffs. Second, we adopt an "intention-to-treat" approach through which we compare *all* workers in mass layoff firms with a matched control group in non-mass layoff firms, as opposed to potentially-selected displaced and non-displaced workers, as it is usually applied in the literature. After rescaling the increase in crime by the relative drop in earnings under these alternative approaches, the implied elasticity of crime to earnings remains similar to the one estimated in the main specification.[2]

[1] Mass layoffs have been widely used as a source of exogenous variation to estimate the effects of job loss on several outcomes, e.g. subsequent earnings (Couch and Placzek, 2010; Jacobson et al., 1993) and mortality (Sullivan and Von Wachter, 2009).

[2] Such elasticity is useful for comparing estimates across different approaches for robustness purposes. However, we *do not* attach a causal interpretation to such a parameter since job loss can affect crime rates through multiple channels – which we will discuss in detail – in addition to the drop in earnings.

In the second part of the analysis, we turn to the effect of unemployment insurance (UI), the main policy providing income support for displaced workers in Brazil. UI recipients receive on average 80% of the pre-displacement salary and the benefits can last up to five months, quite similar to most US states. Most importantly for identification purposes, UI eligibility varies discontinuously with the timing of previous layoffs used to claim unemployment benefits, as a minimum of 16 months is required between layoff dates for subsequent UI claims. This institutional rule allows us to study the effects of a strong shift in income support – from zero to up to five months of benefits – using a clean regression discontinuity design.[3]

We find that the crime rate in the first semester after layoff is 20% lower for marginally eligible workers compared with marginally non-eligible ones. Rescaling this effect by the take-up rate – approximately equal to 70% – the average effect of unemployment benefits completely offsets the potential increase in crime upon job loss. The effect is entirely driven by groups that are more likely to be liquidity constrained, namely youth with a lower education and below-median wage. However, these effects are transitory and vanish after benefits expire.

These results suggest that UI policies may attenuate the impact of job loss on crime. They also help us to distinguish between the different mechanisms driving the effect of unemployment on crime. In particular, both eligible and non-eligible workers are unemployed upon layoff, whereby lower crime rates by the former cannot be attributed to substitution between time spent on the job (i.e. what we previously called the incapacitation effect of employment) and leisure time. By contrast, we show that – in line with previous studies such as Katz and Meyer (1990) and Lalive (2008) – unemployment benefits reduce labor supply, and during our sample period UI in Brazil was not conditional on participation in training programs so UI increases leisure time in the months after layoff. If time substitution were the main driver of the effect, the eligible should commit more crime than the non-eligible, while the opposite result holds true in our data. Instead, our results support economic explanations, primarily liquidity constraints.[4] The existence of such constraints is indeed consistent with the higher increase in crime observed across

[3] Gerard et al. (2019) exploit the same research design with data from earlier years to study the effect UI eligibility effects on unemployment duration. Accordingly, they detect a potential violation of quasi-random assignment (the density of the assignment variable is mildly discontinuous around the 16-month cutoff). However, this issue is not present in our sample period as both the density of the running variable and other covariates are balanced around the cutoff. Moreover, we also show the crime rates before layoff are continuous around the threshold, thus strongly supporting the validity of the design.

[4] Foley (2011) provides evidence on the importance of liquidity constraints for criminal behavior using aggregate data on welfare payments.

younger, low-tenure and less-educated workers upon job loss, as well as the strong but transitory effect of unemployment benefits on these same groups. The spillover effect on cohabiting sons is also consistent with the importance of liquidity constraints and inconsistent with time substitution. In addition, the latter effect is not explained by changes in the opportunity costs of committing crimes, whereby we show that sons' employment and earnings are not affected by parents' layoff. Finally, the generalized increase in all types of crimes – including purely violent acts and other offenses with no economic motivation (e.g. property damage, traffic violations, and small drug possession) – suggests that psychological stress upon job loss also plays an important role.

This paper adds to a large body of empirical literature on the effect of employment on crime, recently surveyed by Draca and Machin (2015). Several previous papers rely on variation across geographical areas (e.g. regions or provinces within a country) and identify the causal effect of unemployment on crime using Bartik-type instruments that interact national-level shocks with local economic characteristics (see, among others, Raphael and Winter-Ebmer (2001), Gould et al. (2002), Öster and Agell (2007), Fougère et al. (2009), Dix-Carneiro et al. (2018), and Dell et al. (2019)). These studies generally conclude that local crime rates increase with unemployment. However, variation across local areas only provides limited insights into the mechanisms through which unemployment affects criminal behavior, and it does not allow us to explore the mitigating effect of unemployment benefits or other social safety nets because the rules determining such benefits typically do not vary across geographical areas. In addition, such analyses may be ill-powered to detect the determinants of a relatively rare event such as criminal activity with sufficient precision. Even in high-crime countries, offenders remain a very minor fraction of the total population, whereby it may be difficult to precisely identify the determinants of criminal behavior solely based on average data across individuals living in a given area. This is particularly true for severe crimes such as murders, which are rarer compared with petty property crimes.

In this paper, we address these issues leveraging on administrative, individual-level data. In this respect, our work is close to four recent papers using administrative data on employment and crime for high-tenure Danish workers displaced during the 1992-1994 period (Bennett and Ouazad, 2019), 361,000 Norwegian workers in 1992-2008 (Rege et al., 2019), previous offenders released from prison in Washington State in 1992-2016 (Rose, 2018), and workers in the city of Medellín in 2006-2015 (Khanna et al., 2019).[5] The present paper contributes to this literature by providing the first analysis covering the universe of

[5]Prior to these recent contributions, Witte (1980) and Schmidt and Witte (1989) used individual-level data on former prison inmates in North Carolina to study the determinants of recidivism (including employment). However, their approach does not allow identifying causal effects.

workers in a large country – Brazil – characterized by very high levels of crime. Our results are thus particularly informative about the effect of employment on crime in countries where the latter is a major social problem. In addition, the dimension of our dataset also allows us to gain considerable precision when estimating the impact of job loss on criminal behavior. This is especially relevant when trying to distinguish the effect on very disaggregated crime categories, including rare crimes such as murders or other crimes with no clear economic motivation. In a related manner, we have sufficient statistical power to precisely identify the effect on other household members. Both the heterogeneity analysis by type of crime and the evidence on spillover effects on other household members provide important insights into the mechanisms driving the effect of unemployment on crime. Another advantage of focusing on Brazil is that the peculiar features of the UI system provide us with a very clean research design for identifying the effect of unemployment benefits. The results of this analysis are obviously important from a policy perspective, as well as providing further insights into relevant mechanisms.

From a methodological perspective, we take advantage of recent contributions to the econometric literature on estimating dynamic treatment effects with treatment effect heterogeneity. When identification exploits variation in the timing of treatment across treated units (as is the case for layoffs) and the treatment effect is heterogeneous across units (as it seems reasonable to assume in general), the estimated coefficient of interest in a typical two-way fixed effects specification equals a weighted average of heterogeneous treatment effects across units with weights that may be negative for some units (see, among others, Borusyak and Jaravel, 2017; de Chaisemartin and D'Haultfoeuille, 2019; Goodman-Bacon, 2018). Indeed, we show that such an issue is very relevant in the present context. If we were to restrict the analysis to the subsample of displaced workers and exploit only variation in the timing of layoff (as in some previous papers), about 42% of the weights would be negative, and the estimated coefficient would be severely biased downward as a result. Instead, negative weights are not an issue in our analysis because our control group includes a sufficiently large number of "pure" control workers who are never dismissed throughout the sample period.

Finally, information on prosecutions initiated "in flagrante" helps us addressing possible biases from differential under-reporting of crimes by individual characteristics of the offender (including employment status), which are usually a major challenge when studying the determinants of criminal behavior (see e.g. MacDonald, 2002; Soares, 2004).

The remainder of this paper is organized as follows. The next section provides some context for our empirical investigation, before Section 3 describes the data and preliminary evidence. Section 4 presents the effect of job loss on crime, after which Section 5 highlights the impact of unemployment benefits and discusses the mechanisms behind the relationship between unemployment and crime. Finally, Section 6 concludes.

2 Institutional background

Latin America is the most violent region in the world, with Brazil being one of the most violent countries within the region. In 2017, the homicide rate – the only crime statistic that is fully comparable across countries and over time – reached a record of 30.7 homicides per 100,000 inhabitants, the sixth highest in the world (UNODC, 2019). For comparison, homicide rates in Colombia and Mexico – two countries in the same region that have long been plagued by drug-related violence – remain below 25 per 100,000 inhabitants. This level of violence appears particularly high in light of the fact that Brazil is a middle-income country (in 2018, it ranked 82^{nd} out of 182 countries in terms of GDP per capita).

Over time, the homicide rate slowly increased from 20 to 22 per 100,000 inhabitants between 1990 and 2010, before abruptly increasing during the following seven years. Interestingly, during the same period, male employment decreased by 10% (from 73 to 66 percent). More generally, the homicide rate has closely tracked labor market downturns since the 1990s (see Figure 1). On average, a one-standard-deviation increase in the employment rate (3.1 percentage points) in a given year is associated with one-standard-deviation decrease in the homicide rate (3.4 homicides per 100,000 inhabitants), with the correlation between the two variables being close to 90%.

This preliminary evidence at the aggregate level is consistent with the hypothesis that criminal behavior responds to labor market opportunities. On the other hand, raw correlation over time may capture independent long-run trends in both variables or the effect of other external factors (e.g. changes in social policies at the national level). In addition, it is also possible that violence outbreaks affect the level of economic activity. In order to isolate the causal effect of employment downturns on crime and understand the mechanisms driving such a relationship, we will thus exploit mass layoff shocks and compare criminal prosecutions over time between displaced and non-displaced workers, as well as between displaced workers who are eligible and non-eligible for unemployment benefits. For this purpose, we first describe the judicial system and labor market regulations in Brazil.

2.1 Criminal justice

The Brazilian government comprises three administrative divisions: the federal government, 27 sub-national states, and 5,565 municipalities. Local governments have no legislative power concerning criminal law – which is set at the federal level and is uniform across the country – but they are mainly responsible for regular 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.[6] 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 judicial system comprises 27 state courts and four specialized courts: federal, labor, electoral, and military. State courts are responsible for 91.5% of all criminal cases (CNJ, 2019), while the remaining part are mostly international drug trafficking cases and frauds against the federal administration, which are in the competence of federal courts. The 27 state courts comprise 2,697 tribunals, one for each judicial district.

2.2 Labor Regulation

Brazilian labor legislation is based on at-will employment, whereby firms are free to dismiss workers without a just cause, although they must 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%).[7] Our analysis focuses on the former, which we refer to as dismissals or layoffs in the remaining of the paper. Dismissed workers are entitled to a mandatory savings account, financed through monthly contributions by the employer 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, workers receive approximately 1.36 monthly wages per each tenure year upon layoff.

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

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

Although labor informality is high – accounting for roughly 45% of all jobs in 2012 – the formal and informal labor markets strongly interact. Job turnover is high and workers tend to frequently move between formal and informal jobs. In addition, some firms hire workers both formally and informally (Ulyssea, 2018). Due to the lack of administrative data on informal jobs, throughout the paper we mostly focus on workers exiting formal jobs. Since we will classify as unemployed a number of dismissed workers who are instead re-employed in the informal economy, the estimated effect of dismissals provides a lower bound to the effect of being unemployed. In addition, we estimate the share of workers returning to informal jobs based on survey data and take this into consideration when interpreting the magnitude of our effects.

Unemployment insurance is the main policy assisting displaced workers. It 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, at 2.65 minimum wages. Once these benefits expire, the only other form of income support at the national level is "Bolsa Família", the well-known conditional cash transfer targeted at extremely poor families. As of 2019, the average transfer per household is 16% of the minimum wage and the maximum per capita family income for eligibility is less than one-fifth of the minimum wage.

3 Data and descriptive evidence

3.1 Data sources

Our data derive from two main sources. The first source is the *Relação Anual de Informações Sociais* (RAIS), a linked employer-employee dataset covering the universe of formal workers and firms in Brazil, made available by the Ministry of Labor for the 2002-2017 period. The RAIS data contain detailed information such as the start/end date and location of each job, the type of contract, occupation and sectoral code, and the workers education and earnings.[8] The effective date at which dismissed workers leave the job is measured with some degree of 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 from the job during the notice period, although we cannot identify when this happens in the

[8]The RAIS data have been extensively used in previous research on the Brazilian labor market; see e.g. Ferraz et al. (2015), Gerard and Gonzaga (2018), and Dix-Carneiro et al. (2018).

data. Hence, all workers in our sample learn about the job loss at least 30 days before the observed separation date and an unknown share of them are effectively released from the job at the beginning of the notice period. Throughout the analysis, we consider the separation date originally stated in RAIS minus 30 days as the dismissal date.[9] Importantly, RAIS identifies workers by both a unique tax code identifier (CPF) and their full name.

The second data source comprises the universe of criminal cases filed in all first-degree courts during the 2009-17 period, which is supplied by Kurier, a leading company providing information services to law firms all over the country. These data are based on public case-level information available on the tribunals' websites and complemented with information from the courts' daily diaries. For each case, it is possible to observe its start and termination date, court location, and one or more tags on the subjects being discussed. The defendant(s) and plaintiff(s) are identified by their full name.

The defendant(s) name is available for 8 million criminal cases among a total of 14.5 million, due to imprecisions in the data input process from court diaries or due to judicial secrecy. As a rule, judicial acts are public knowledge, yet judges may except the rule in specific instances established by the law. These exceptions typically involve specific types of suits such as sexual offenses and domestic violence, and cases involving individuals under the legal age (18). For this reason, we exclude such offenses from our analysis. As for the other types of crime, it is unlikely that missing data in our records is related to the defendant's job status – our main explanatory variable of interest – for the following reasons. First, the threat of dismissal is not a valid motive for invoking secrecy; in fact, ongoing criminal prosecutions do not constitute a just cause for worker's dismissal by firms, which only applies for definitive criminal convictions. Second, requests for secrecy generally take place after the case has already started, while our data captures the identity of the defendant as long as the case is started without secrecy. Third, for the specific case of offenders arrested "in flagrante" – i.e. caught in the act committing crime – 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, in Section 4.3 we leverage on the large variation in the application of secrecy rules across state jurisdictions and show that our estimates are unaffected when progressively restricting the analysis to states with a lower fraction of

[9] Setting the separation date equal to the minimum notice period is a conservative choice for testing the parallel trends assumption underlying our difference-in-differences design when some workers actually have a longer notice period. In practice, given the high job turnover, 37% and 90% of the workers in our sample are dismissed with less than one and three years in the job, thus having a notice period between the 30 and 39 days, respectively.

missing values in the criminal prosecutions' data.

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) offense date. 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 criminal behavior. In addition, differential reporting by offender characteristics – including employment status – should be less severe for such cases. In Section 4.3, we discuss these measurement issues at length and assess the robustness of our results to including only criminal prosecutions for arrests "in flagrante".

We use the tags on case subjects to drop civil cases, which are covered in the original dataset, and to distinguish – within criminal cases – between economically-motivated and violent offenses. We include in the former category drug trafficking, thefts, robberies, trade of stolen goods, fraud, corruption, tax evasion and extortions, while violent crimes comprise assaults, homicides, kidnappings, and threatening. Some of the latter crimes may be instrumental to other, economically-motivated crimes (e.g. a homicide committed during a robbery). When estimating the impact on homicides, we will try to distinguish between instrumental and non-instrumental homicides by separately coding those reported together or not with other offenses, respectively. Finally, we create a third category of "other" crimes: traffic related, slandering, illegal gun possession, small drug possession, failure to obey, damages to private property, environmental crime, conspiracy, lynching, racial offenses, and prejudice. Appendix Table A1 reports the share of each crime category among all offenses and among crimes committed "in flagrante".

3.2 Merging court and employment records

We merge the judicial and employment data on each individual's full name, which is reported in both datasets.[10] 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, respectively. To identify citizens 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

[10]Throughout the paper, we refer to "name" as the person's full name, i.e. the name-surnames combination.

social programs.[11] 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.[12] Subsequently, we restrict attention to individuals who have a unique name in the country and merge the court data to the employment records by exact matching on names. Columns (1)-(3) of Appendix Table A3 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.[13] We will assess the robustness of our main findings to including all individuals whose name is unique in the state where they work (rather than in the entire country). The coverage of country population increases to 70% in the extended sample, further reducing positive selection (columns 4-6 of Table A3).

3.3 Descriptive evidence

Figure 2 shows how the average probability of criminal prosecution varies by employment status, age, tenure, and monthly wage. We focus on workers employed between 2011 and 2015, allowing us to track criminal behavior two years before and after. As in our main analysis, the sample is composed of male, full-time workers in the non-agriculture private sector. The top graph compares the yearly probability of criminal prosecution between workers continuously employed throughout each calendar year and those dismissed in the same year, along the age distribution. Interestingly, the age-crime profile is essentially flat for employed workers, with around 0.4% probability of being prosecuted in a given year. By contrast, the crime rate is more than twice as high for workers displaced at younger ages (up to 1% for 18-20 years old) and declines progressively for workers displaced at older ages.

The bottom graphs in Figure 2 focus on crime outcomes of displaced workers two years before and after the job loss, conditional on job tenure (left graph) and monthly wage (right graph). The density function in the bottom-left graph shows that labor turnover is extremely

[11] CadUnico also contains the full name and tax ID for each individual.

[12] 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 the attention to adult individuals does not generate measurement error, because we only observe criminal cases for individuals who are above the legal age (18).

[13] Appendix Figure A1 also shows that the full distributions of age, income, job tenure, and years of education are similar between the two groups.

high, as a substantial share of workers are displaced within less than a year in the job. The same graph also shows that low-tenure workers are more likely to be criminally prosecuted, both before and after the job loss. Importantly, the prosecution rate is stable in the two years preceding the layoff, before increasing in the two years following the job loss. The bottom-right graph shows similar relationships between prosecution rates, employment status, and monthly wage.

Of course, the differences in criminal behavior by employment status depicted in Figure 2 reflect both causal and selection effects, whereby we isolate the former from the latter in the next section.

4 The effect of unemployment on crime

4.1 Sample selection and empirical strategy

Our individual-level data on employment and crime cover the 2009-2017 period. As is common in previous studies (e.g., Grogger, 1998), we focus on male workers, who are responsible for the large majority of crimes – 81% of all prosecutions in our sample. We further restrict the sample to full-time workers (i.e. those employed for at least 30 hours per week), holding open-ended contracts in the non-agricultural, private sector.

To implement a difference-in-differences strategy, we select as our treatment group all workers displaced between 2012 and 2014 in the 20-50 age range, which allows us to estimate dynamic treatment effects for up to four years after displacement, as well as placebo effects up to three years before displacement.[14] The pool of candidate control workers comprises all individuals employed in firms that did not experience mass layoffs during our period of analysis.[15] We then match each treated worker with a control worker who (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), state (27), and has the same job tenure. When treated workers are matched with multiple controls, one control unit is randomly selected.[16]

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

[15] Our definition of mass layoffs is presented shortly below.

[16] In the baseline specification, control workers are not dismissed in the matching year but may be dismissed in subsequent years. We show that results are robust 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 et al. (2018) define the control group similarly to our baseline setting,

Out of 5.9 million displaced individuals, 4.9 million are successfully matched to a control unit. We then 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 overcome the issues raised by the recent methodological literature when estimating the full path of dynamic treatment effects – particularly, the presence of negative weights attached to some treated units when averaging heterogeneous treatment effects in typical two-way fixed effects regressions.[17] As we will show in Section 4.3, negative weights are not present in our analysis, and our results are robust to the alternative approach proposed by de Chaisemartin and D'Haultfoeuille (2019).

In practice, we estimate the following difference-in-differences equation on the sample of treated and (matched) control workers:

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

Workers are identified by the subscript i, and $Treat_i$ is a dummy indicating that the worker belongs to the treatment group. The set of dummy variables $Time_t$'s identify years since layoff, which we can 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 effects.[18] Finally, we absorb time-varying shocks with $Time_t$. As a robustness check, 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) where the i-th worker is employed at time $t = 0$. These are finer categories with respect to our matching by state (27) and one-digit industrial sector (9). Comparing the results obtained when we include and exclude this additional set of fixed effects thus reveals the ability of our approach to eliminate the effect of confounding shocks at the local level.

while Jacobson et al. (1993) and Couch and Placzek (2010) restrict to workers who are continuously employed through the whole period.

[17] 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).

[18] Monthly-level estimates are presented as a robustness exercise.

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 for 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 may dismiss workers who are more likely to commit crimes before they are actually prosecuted, so selection into treatment on criminal propensity would not be apparent from pre-treatment trends in criminal prosecutions. To overcome this issue, we restrict the analysis to mass layoffs, defined as firms with at least fifteen workers dismissing 33% or more of the workforce within a year without just cause.[19] These layoffs typically depend on negative external shocks at the firm level, rather than the characteristics and behavior of dismissed workers (see e.g. Gathmann et al., 2020).

Table 1 presents summary statistics for treated and controls when including all layoffs (first three columns) or 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 the 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 criminal behavior (see the bottom-left panel of Figure 2). 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 could 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.

[19] We assess the robustness of our findings to alternative definitions of mass layoffs in terms of both the minimum share of displaced workers and firm size. 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.

4.2 Main results

4.2.1 Effect of job loss on employment and earnings

We first show the labor market consequences of job loss, and then move to the effects on criminal behavior. The left graphs in Figure 3 compare the evolution of employment and labor income for treated workers dismissed at date $t = 0$, also distinguishing dismissals upon mass layoffs, and the matched samples of control workers not dismissed in the same calendar year. Since we impose no restriction on the employment status in the pre-period, a substantial share of both treated and control workers are not employed in the years before dismissal due to the high turnover rate in Brazilian labor market.[20] Most importantly for the purposes of our analysis, the control group closely tracks the treated group in terms of both employment and labor income during the pre-treatment period.

In the 12 months after dismissal, employment and earnings dramatically decline for treated workers relative to the controls (the latter may also be dismissed starting in the following calendar year). The right graphs in Figure 3 quantify the difference between the two groups – as estimated by equation (1) – relative to the baseline average for the treated group at $t = 0$. In the first year after dismissal, gaps in employment and income amount to 34% and 70%, respectively, and slowly narrow in the following years. Four years after dismissal, treated workers still experience 13% lower employment rates and 26% lower labor income compared with the control group. The graphs also show that these estimates are unaffected when including the rich set of municipality-year-industry fixed effects, meaning that exactly matching the control group on individual characteristics and exploiting variation over time absorbs time-varying shocks at a very granular level of geographic and sectoral disaggregation.[21]

To the extent that some of the displaced workers may transit to the informal sector – which accounts for 43% percent of economic activity in Brazil during our sample period (IBGE) – the estimates in Figure 3 overstate the relative drop in employment and earnings upon job loss. For this reason, we replicate the analysis of employment effects based on the National Longitudinal Household Survey (*Pesquisa Nacional por Amostra de Domiclios*, PNAD), which contains information on both formal and informal labor income.[22] The

[20]Some previous studies focus only on high-tenured workers who are continuously employed throughout the whole pre-period (e.g. (Jacobson et al., 1993) and Couch and Placzek (2010)). Such an approach would be particularly restrictive in our context of high labor turnover.

[21]Figure A5 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.

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

longitudinal component of PNAD tracks households for five consecutive quarters.[23] In line with our main analysis of Figure 3, we focus on male workers who were initially interviewed during the 2012-2014 period, and compare treated workers who were formally employed in the first but not in the second quarter with a control group who were employed in both the first and second quarter (but possibly displaced in later quarters). Figure A4 in the Appendix presents the results for monthly income for both formal and informal jobs. Reassuringly, the average effect on formal earnings over the first four quarters after displacement (-65%) is essentially identical with that estimated in the main analysis. When also including informal employment, the estimated effects on labor earnings are smaller (-58%), as some of the workers displaced from a formal job reallocate into the informal economy within the following year. This suggests that crime elasticity estimates based solely on formal labor income are overestimated by about 12%.

4.2.2 The effect of job loss on criminal behavior

Figure 4 shows the effect of job loss on the probability of committing a crime during the following years. The main dependent variable is a dummy for being criminally prosecuted in each year relative to the dismissal date, based on the filing date of the judicial case. As in the previous Figure 3, analysis time is set relative to the layoff date, and dynamic treatment effects in the right graphs are rescaled by the baseline crime rate in the treatment group at $t = 0$.

The left graphs in Figure 4 show that the treatment group exhibits higher baseline crime rates than the control group during the pre-treatment period. As discussed in the previous Section 4.1, such a difference reflects the lower job turnover that characterizes the control group by construction. Although our differences-in-differences approach only requires parallel trends between the two groups in the absence of intervention – which is supported by the evidence in Figure 4 – we will show in Section 4.3 that our results are unaffected when adopting more stringent definitions of mass layoffs that also eliminate differences in baseline levels of crime rates during the pre-treatment period.

The probability of criminal prosecution sharply increases in the first year after layoff, and remains constant thereafter. The crime rate also increases for the control group after $t = 1$, albeit much less so than for the treated group. This is consistent with the fact that some

PNAD.

[23] Although the microdata does not contain a person ID, it is possible to track individuals over time based on their household ID and characteristics such as gender, their precise birth date and their order in the family.

workers in the control group are also dismissed between $t = 1$ and $t = 4$, as shown in Figure 3. Most importantly, differential changes estimated from equation (1) – reported in the right of Figure 4 – are large and statistically significant. These estimates remain unaffected when restricting to mass layoffs and including municipality-year-industry fixed effects. The same is true when distinguishing between different categories of offenses, as shown in the bottom panels of Figure 4.

In columns (3) to (6) of Table 2, we quantify the average effect on crime over the four years after dismissal – as estimated from equation 2 – and their ratio over the baseline crime rate, whereby columns (1) and (2) show instead the effect on employment and earnings. The results in Panel A refer to our baseline sample including all displaced workers and control workers not displaced in the same year. On average, job loss increases the probability of criminal prosecution by 0.15 percentage points, or 27% over the baseline. We divide the latter effect by the 40% decrease in earnings reported in column (2) to compute an implied elasticity of crime to earnings equal to -0.66. Importantly, we do *not* attach a causal interpretation to such elasticity, as this would require that layoffs affect criminal behavior only through (decreased) earnings. This is clearly not the case, as the effect could go through other mechanisms such as leisure time, psychological stress, and so on (we discuss the empirical relevance of these mechanisms in Section 5.4). Nevertheless, it is useful to rescale the percent change in crime by the percent change in employment to compare crime effects across different samples and specifications.

In Panel B of Table 2, we restrict the control group to include only workers who are continuously employed during the sample period. Unsurprisingly, the gap with the treated group in employment, earnings, and criminal behavior grows larger under this alternative definition of the control group. In Panel C, our preferred specification, we restrict the sample to include only workers displaced in mass layoffs. Similarly to the results based on all layoffs in Panel A, the latter panel indicates that job loss increases the probability of criminal prosecution by 0.12 percentage points, or 23% over the baseline. Finally, in Panel D we focus again on mass layoffs and also control for the full set of municipality $\times$ industry $\times$ year fixed effects. Again, the estimated effects remain essentially identical to the initial specification in Panel A.

Distinguishing between different categories of offenses (columns 4-6 of Table 2), the effect is mainly driven by economically-motivated crimes (+43% over the baseline), although the effects on violent crimes and other types of crime are also large (+17% over the baseline). Our unusually large sample also allows us to precisely estimate the effects on very detailed categories of crime, including very rare ones. Table 3 presents these results for the mass layoffs specification. Robberies and drug-related crimes (both trafficking and small possession) respond most strongly, increasing by 91 and about 55-58 percent, respectively. Violent crimes also respond strongly, with homicides increasing by 32%. This finding suggests that job loss may affect criminal behavior beyond purely economic motives, although we cannot exclude the notion that a portion of all homicides are instrumental to committing purely economic crimes (e.g. robberies or drug trafficking). While our data do not allow us to perfectly distinguish between instrumental and non-instrumental homicides, we approximate the former by prosecutions for multiple offenses (including at least one homicide) and the latter by standing alone homicide prosecutions. The results in columns (3) and (4) of Panel B show that the non-instrumental homicides are more prevalent, but instrumental homicides respond more strongly. However, both effects are statistically significant and quantitatively relevant. Overall, job loss affects almost all types of crime, including some that clearly have no economic motivation (e.g. traffic violations and failure to obey). These findings suggest that non-economic factors such as psychological stress may also play a role – alongside economic explanations – in increased crime upon job loss. This is even more evident from Table A2 in the Appendix, which shows the results for all layoffs (i.e. including non-mass layoffs). In this extended sample, the effect is statistically significant and sizable for virtually all types of crime, including property damage and slandering – which are arguably unrelated to economic motives – which increase by 24% and 14%, respectively. We further discuss this evidence in Section 5.4 when addressing mechanisms.

4.3 Robustness

Overall, the results in Tables 2 and 3 document strong responses of criminal prosecutions to layoffs. We next explore the sensitivity of these results to using alternative measures of criminal behavior and identification strategies.

4.3.1 Measurement of criminal behavior and timing of the effect

Measuring criminal behavior based on judicial prosecutions is potentially problematic for two main reasons. First, a large number of crimes are not reported or – even if they are – the (suspect) offender is not identified. This is a typical measurement issue in empirical analysis of crime (see e.g. Soares, 2004). If the probability of criminal prosecution conditional on having committed a crime is constant, the estimated effect would be biased towards zero but the relative effect and the implied elasticity to earnings would be unaffected. However, in practice, the extent of under-reporting may vary with individual characteristics, the type of offense, and so on. In particular, we want to be certain that a higher probability of prosecution after layoff reflects an increase in crimes that are actually committed, as opposed to an increased probability of being prosecuted conditional on having committed a crime (e.g. because police or prosecutors may more intensively target unemployed individuals). The second limitation of criminal prosecutions is that they are typically filed with some lag relative to when the crime was actually committed. For this reason, balance tests in the pre-treatment period may fail to capture increases in criminal activity by dismissed workers before dismissal.

We address both issues by replicating our analysis on the subset of criminal prosecutions against offenders apprehended in flagrante. The decision to prosecute these offenders arguably involves much less discretion by police and judicial authorities. Moreover, they are *immediately* prosecuted, so the prosecution date is informative about the timing of crime. Figure 5 compares the results obtained when including all criminal prosecutions (top graph) and only prosecutions following "in flagrante" arrests (bottom graph). We conduct this comparison at monthly frequencies to detect even minor deviations from parallel trends in the pre-treatment period. However, no such deviation emerges, irrespective of whether we include all prosecutions or only prosecutions initiated "in flagrante". Although the latter represent only a minor fraction of all prosecutions (see Table A1), they increase relatively more strongly upon layoff (+134%). The latter may be due to the fact that prosecutions "in flagrante" are more frequent for robberies and drug trafficking, which also respond more to job loss (see Table 3).[24]

4.3.2 Selection into treatment

Our analysis crucially hinges on the assumption that there is no dynamic selection into treatment, implying in turn that the control group approximates the behavior of displaced workers in the absence of displacement. The evidence in Figures 3, 4, and 5 of parallel trends in the pre-treatment period is consistent with such an assumption. Importantly, all results

[24]Figure A6 in Appendix replicates the analysis based on criminal "execution cases" ("processo de execuão penal"), which initiate following a conviction to prison that is no longer passive of appeal. This measure attenuates concerns about type I errors in the prosecution data, i.e. the possibility that cases are filed against individuals who did not commit any crime. As should be expected, the dynamics of the effect is slower than for prosecutions, because the case must have gone through a trial before reaching the conviction stage, and we track the initial date of the "execution case", which cannot be linked to the initial date of the prosecution originating the conviction. However, the probability of conviction increases by 70% over the three years after the layoff, thus being stronger than the effect on prosecutions. In the Appendix Tables A5 and A6 we also show that the results are robust when restricting to states with a lower share of missing observations (listed in Table A4) and when including all individuals with a unique name within their state of residence, respectively.

(for both pre- and post-treatment periods) remain identical when restricting the treated group to workers displaced upon mass layoffs.

However, firms might still have considerable room for choosing whom to dismiss even when firing (at least) one third of employees, as in our baseline definition of mass layoffs. We address this concern in two ways. First, in Table 4 we explore the sensitivity of the results when 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). As we restrict to events in which a larger fraction of workers were dismissed, there should be less scope for selection into treatment. Indeed, the level differences in pre-treatment crime rates between dismissed workers and matched controls – also reported in Table 4 – progressively decline to almost zero when restricting to events in which at least 90% of workers were dismissed, as shown in the last row of each panel. At the same time, the estimated effect on crime is largely unaffected (see also graphical evidence in the Appendix Figure A7). The same is true when focusing on plant closures (column 5) and when varying the minimum firm size (panels B to D).[25]

As a second approach to addressing potential selection effects, in Table 5 we 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). This approach differs from our baseline specification, which follows previous papers in comparing workers who are displaced upon mass layoffs with a matched group of non-displaced workers. Both these groups of workers are potentially selected on individual characteristics among all workers in their respective firms. Drawing an analogy with randomized experiments with imperfect compliance, we may want instead to compare all workers "assigned" to mass and non-mass layoff firms. By retaining all workers employed at the beginning of each year in the mass and non-mass layoff firms, we also avoid potential selection issues driven by *early leavers* who may quit declining firms in advance of mass layoffs. Of course, when we adopt this alternative approach, the change in both labor market outcomes (columns 1-2, 4-5) and the probability of criminal prosecutions (columns 3 and 6) are much weaker compared with our baseline analysis. However, when we rescale crime effects by changes in earnings, the implied elasticity of crime to earnings remains qualitatively similar to our baseline estimate in Table 2.

[25]The sample size increases when moving from column 4 (90% layoffs) to column 5 (plant closures), which may appear counterintuitive upon first consideration. However, 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. Therefore, 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.

4.3.3 Methodological issues in the estimation of dynamic treatment effects

Recent methodological contributions highlight the challenges associated with estimating dynamic treatment effects in difference-in-differences designs when there is (i) variation in the timing of treatment – as in our context – and (ii) treatment effects are heterogeneous across individuals, as is reasonable to assume in most situations. 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).[26]

This problem is most severe when all or a large share of individuals in the sample are treated at some point, as is sometimes the case. Indeed, some previous analyses on the impact of job displacement on crime purposefully restrict the sample to job losers to ensure stronger comparability of treatment and control units in each period. By contrast, our data include a large share of never-treated (control) workers, which should limit the extent of negative weights. Indeed, if we estimate the two-way fixed effect specification in the panel of workers observed over calendar years, no individual treatment effect receives a negative weight. If we were instead to restrict the sample to workers displaced at some point, about 42% of units would receive a negative weight. Consequently, the estimated effects would be about half the strength of those estimated when including never-displaced workers.[27] As a final robustness check, we re-estimate the effect of interest following the approach of de Chaisemartin and D'Haultfoeuille (2019), which compares, in each period, "switchers" – units changing treatment status in a given period – to "non-switchers" – units not changing treatment status in the same period. The results are extremely similar to those of our baseline approach, as shown in the Appendix Figure A9.

[26] 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).

[27] Statistics on negative weights follow the implementation proposed by (de Chaisemartin and D'Haultfoeuille, 2019). Figure A8 in the Appendix compares the estimated effect in the two-way fixed effect model over calendar years when including (top panel) and excluding (bottom panel) pure controls who are never displaced. Compared with our baseline estimate in Figure 4, the impact in the first year is attenuated as treated workers are dismissed throughout the entire calendar year, while the first period in our baseline estimates is set relative to the layoff date.

4.4 Additional results

The results presented thus far document a sizable, robust, and long-lasting increase in the probability of committing crimes. We next investigate how such an effect varies between different groups of workers, and whether it extends to their family members.

4.4.1 Heterogeneity analysis

We explore the heterogeneity of the effect across types of individuals by including additional group indicators and interacting them with all variables in our model. Table 6 reports the interaction coefficients with our treatment variable as well as a measure of the relative effect for each group (in square brackets), i.e. the total effect over the group mean among treated units at $t = 0$.

These results suggest that groups who are more likely to be liquidity constrained – such as youth and low-tenure workers – react more strongly to job loss. In column (1), the probability of committing crimes increases by 18% for workers who are older than 29 and by 28% for younger workers, who may have lower savings and wealth available after displacement. In column (2), we detect even stronger differential increases for low-tenure workers, who receive low severance payment and are less likely to be eligible for unemployment benefits. For instance, the effect for workers who are displaced within five months in the job is about five times stronger in absolute terms and twice as strong in relative terms compared with workers who are displaced after more than five years in the job. Workers in the latter group face much less severe financial constraints given that they receive at least 6.8 monthly wages in dismissal indemnities and are entitled to five months of unemployment benefits. Differential increases are also observed for high-school dropouts and below-median-income workers (columns 3 and 4, respectively), although the interaction coefficient for the latter group is not statistically significant. Interestingly, the effect remains sizable and statistically significant for all groups, including older, more-educated, high-tenure workers, and workers with above-median earnings (see the coefficients in the first row of Table 6).

Finally, we compare the effect in municipalities with higher and lower homicide rates, which is an important dimension of heterogeneity in light of the extreme variability in violence across local areas in Brazil. The homicide rate ranges between 10 per 100,000 inhabitants in São Paulo – the richest state in the country – and 62 per 100,000 in Rio Grande do Norte. The results in column (5) suggest that crime increases upon job loss are about one-third larger in municipalities with above-median homicide rates – where crime opportunities tend to be higher – compared with other municipalities (+26% and +20%, respectively).

4.4.2 Household-level analysis

The effect of job loss can propagate to other household members. We estimate these spillover effects by leveraging on CadUnico data, which maintains information on household composition that is used for the administration of social programs. Due to the nature of this dataset, household composition is only available for 47% of the population, mainly coming from the lower part of the income distribution. Merging this data with RAIS, we obtain a total 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. Once the treatment and control group are defined, we identify all household members for each individual in the sample. In line with our main analysis, we focus on the criminal behavior of male individuals in this sample with an age between 20-50 who have a unique name in the country. Due to the selection of households present in CadUnico, baseline crime rates in this analysis are above average compared with the general population. We identify the effect of (household members') job loss exploiting variation from mass layoffs. Household members working in the same firm as the job losers are dropped from the sample so that we can clearly isolate spillover effects from common employment shocks.

Table 7 and Figure A10 in the Appendix show the effect on both employment and criminal behavior of three categories of household members: sons (22 years old on average); brothers, by age group; and male partners of displaced female workers. The probability of criminal prosecution increases by 0.2 percentage points for sons (+18% over the baseline crime rate), while there are no significant effects on siblings' and male partners' crime rates. These findings are line with a liquidity constraints mechanism, given that economic dependence should be highest for sons. A mechanism related to the psychological dimension of the job loss would likely affect household members more homogeneously. However, we cannot completely rule out that the children are more strongly affected by the emotional shock.

5 Effect of unemployment insurance on crime

The results in the previous section establish that job displacement has dire consequences for criminal behavior. From a policy perspective, it is thus important to understand whether traditional public policies supporting unemployed workers can mitigate – at least in part – these adverse impacts. As we will show, this analysis also sheds light on the potential mechanisms driving the effect of unemployment on crime.

5.1 Research design

Brazilian workers are eligible for 3-5 months of unemployment benefits when dismissed without a just cause from a formal job, conditional on satisfying two conditions: (i) continuous employment in the 6 months prior to the layoff, and (ii) a minimum 16-month period between the current layoff date and the most recent 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. Within the group of workers satisfying condition (i) above, we leverage changes in eligibility around the 16-month cutoff implied by condition (ii) as an ideal regression discontinuity (RD) design. Specifically, we compare the criminal behavior of workers who are eligible and non-eligible for UI benefits by estimating the following equation:

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

where Y_i is an indicator variable for the i-th worker committing a crime after job loss; X_i is the running variable of the RD design, i.e. time elapsed since the previous layoff standardized so that $X = 0$ at the cutoff required for eligibility (i.e. 16 months); $f(.)$ 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 bandwidth (including the optimal bandwidth according to Calonico et al., 2014) and polynomial choices, as well as permutation tests where our discontinuity estimates are compared with the 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 et al. (2019) show that – for an earlier period (2004-2008) – 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 $\beta/0.7$.

5.2 Sample and balance tests

We focus on the 2009-14 period and restrict our initial sample – 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.[28] A further sample restriction is imposed to deal with cyclical patterns in dismissal dates, which naturally creates discontinuities in the density of the running variable following approximately 30-day cycles. As documented in the Appendix Figure A11, firms concentrate layoffs on the very last and initial days of the month.[29] 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 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.[30]

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 6 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 Cattaneo et al. (2018, 2019). In addition, the bottom graphs in Figure 7 show no significant difference in prosecutions within one semester and 3 years before displacement, respectively. Finally, Figure A13 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.

As an alternative approach to deal with cyclicality, we include all workers and add cutoff and dismissal date fixed effects in the RD regressions.

[28]We limit the sample period to the years prior to 2015 because numerous changes were implemented to the UI system in this year.

[29]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.

[30]See Figure A12 in Appendix.

5.3 Results

The top-left graph in Figure 7 plots the probability that dismissed workers around the 16-month cutoff are prosecuted within the first semester after dismissal (i.e. the period during which they are possibly receiving unemployment benefits). Displaced workers who are marginally eligible for UI commit less crime than non-eligible workers. The estimated effect – reported in column (1) of Table 8, Panel A – amounts to -0.07 percentage points (-20.1% over the baseline). Rescaling this reduced form coefficient by the first-stage increase in take-up estimated by Gerard et al. (2019), the average effect on compliers would be -28%. Therefore, the beneficial effect of UI completely offsets the increase in crime caused by job loss. On the other hand, this estimate is only marginally significant (p-value 8.2%). However, the effect is larger in magnitude and more precisely estimated when we restrict the sample to groups that are more likely to be financially constrained, namely youth with low education and below-median income, while there is no effect on the other groups; see Figure 8 and columns (2)-(7) of Table 8. Figure A9 confirms that such effects are statistically different from placebo distributions obtained by running the same estimates on placebo cutoff dates.

These results are consistent with the evidence in Table 6 in suggesting that liquidity constraints may be an important driver of the increase in crime observed upon job loss.[31] Also in line with this interpretation, the beneficial effect of UI disappears immediately after the expiration of benefits. The top-right graph in Figure 7 shows no evidence of discontinuity in prosecutions over the following period (see also Panel B of Table 8).

Table A7 in the Appendix shows that these results are confirmed when considering different bandwidths (including the optimal bandwidth according to Calonico et al., 2014) and controlling for a polynomial regression in the running variable. The average effect on the total sample is marginally non-significant in some specifications, while the effect on liquidity constrained groups remains larger and more precisely estimated. In Table A8 we control for the ciclycality in hiring/firing discussed in the previous Section 5.2. In the first four columns, we progressively include fixed effects for the individual-specific cutoff date and for each dismissal date – defining the running variables – thus relying on variation in the worker-specific dismissal date within groups who have the same cutoff date; in the last two columns, we also 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. Both the average effect on the total sample and the effects on liquidity constrained groups

[31] In the RDD analysis, we cannot address heterogeneity by tenure (as we did instead in Table 6) because workers included in the RDD sample have similar (low) tenure by construction.

are larger and more precisely estimated than in the baseline specification of Table 8. The same is true when extending the sample to include all individuals with a unique name within each state (Table A9). These additional results suggest that the somewhat weak significance of the average coefficient in the baseline specification of Table 8 likely reflects the relatively small number of observations near the cutoff. When extending the sample to include workers hired and dismissed at any date or workers with a unique name within each state (rather than within the entire country), such coefficient is largely unaffected in magnitude and more precisely estimated.

5.4 Discussion on Mechanisms

Our findings on the effects of unemployment benefits suggest that social insurance policies may attenuate the adverse consequences of negative labor market shocks on criminal activity. They also shed light on the empirical relevance of alternative mechanisms through which job loss affects criminal behavior. Our main results on the effect of job loss on crime (Section 4) are consistent with both economic mechanisms – namely the reduced opportunity cost of crime and binding liquidity constraints – and non-economic explanations. In particular, displaced workers have more leisure time and thus a higher probability of encountering crime opportunities, which we previously called the "incapacitation" effect of employment.

However, the latter explanation does not square well with the fact that displaced workers who are eligible for unemployment benefits exhibit lower crime rates than the non-eligible. Both groups are unemployed immediately after layoff, although unemployment duration is on average longer for UI eligible workers due to the negative impact of unemployment benefits on labor supply. Figure A14 and Table A10 in the Appendix show that the probability of finding employment in the first semester after layoff decreases by 13 percentage points (-20.7% relative to the mean) and unemployment duration increases by 6.5 weeks (+18.8%) for eligible workers compared with non-eligible ones near the cutoff. If employment had an incapacitation effect on potential offenders, eligible workers should commit more crime than the non-eligible (instead, they commit less).[32] Increased opportunity cost of crime is also unlikely to explain the strong effect of unemployment benefits in the first semester after layoff, for two reasons. First, unemployment benefits reduce the payoff of formal work – benefits are ceased if the beneficiary finds a new formal job – and thus, if anything, it should

[32]It is worth noting that 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, information provided by the Ministry of Labor shows 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.

incentivize other activities such as informal work or crime. Second, in Brazil, unemployment benefits are not ceased when the recipient is arrested.

Instead, the negative effect of UI eligibility on the probability of committing crimes suggests that a significant portion of displaced workers are subject to binding liquidity constraints. This explanation is also consistent with the stronger effect detected for those who are most likely to be liquidity constrained, namely youth, low-income, and less-educated workers. For these groups, the effect of UI entirely offsets the increase in crime caused by job loss. Finally, the fact that the effect vanishes upon the termination of benefits also supports this explanation. In particular, this finding is consistent with recent evidence showing that UI beneficiaries experience sharp drops in consumption upon benefit expiration, as documented by Gerard and Naritomi (2019) using Brazilian data for workers displaced in a similar period (2011-13).[33]

In addition, two pieces of evidence discussed in previous Section 4 further support the importance of liquidity constraints vis-a-vis the opportunity cost of crime and time substitution. First, our heterogeneity analysis shows that the effect on crime is substantially stronger on those groups of individuals who are most likely to be liquidity constrained, namely those who are younger, with lower tenure and less educated. In particular, the impact on crime is about five times stronger in absolute value terms and twice as strong in relative terms for low-tenure workers – who have lower access to liquidity from severance payments and unemployment benefits – compared with high-tenure individuals. Second, although parental job loss does not represent a shock for the sons' earnings opportunities or time availability – as confirmed by the absence of effects on sons' employment – there is a sizable spillover on the probability that sons engage in crime (top-left panel of Table 7). Indeed, the effect on cohabiting sons is almost as strong as the direct effect on workers displaced upon mass layoffs (+18% and +23%, respectively).

Overall, these pieces of evidence strongly suggest that liquidity constraints are an important mechanism mediating the impact of job loss on crime. Even though we cannot completely rule out other mechanisms, our findings do not support the relevance of time substitution and the opportunity costs of earnings in the legal economy as mediators of the impact. In particular, "incapacitation" effects seem the least relevant in light of the evidence on the impact of eligibility for unemployment benefits.

Finally, we stress the potential relevance of psychological factors related to the job loss. These mechanisms are supported by the detailed evidence showing that job loss has a

[33]Based on US data, Ganong and Noel (2019) presents similar evidence documenting the lack of consumption smoothing by unemployed workers.

substantial impact on a wide range of offenses, not exclusively related to economically-motivated crimes or violent crimes committed for economic motives. For example, traffic-related offenses and failure to obey increase by 12% and 44% after mass layoffs (Table 3), while slandering and property damage increase by 14% and 24% in an extended sample covering all layoffs (Table A2 in the Appendix). In addition, there is a sizable impact on single-offense homicide cases – committed in isolation from other charges and less likely to be instrumental – which increase by 28% after mass layoffs and by 36% in the extended sample covering all layoffs.

6 Conclusion

Taking advantage of detailed data on the universe of workers and criminal prosecution in Brazil – a large country with very high levels of crime – we are able to precisely estimate the impact of unemployment on crime. It is shown that the probability of criminal prosecution increases by 23% from the first year following the job loss, before remaining stable over a four-year period. This substantial effect is not solely explained by economically-motivated crimes (+43%), but also extends to violent crimes (+17%) and other crimes such as traffic offenses and failure to obey. The fact that non-economically motivated crimes increase suggests that psychological stress may be a relevant mechanism. Importantly, we find that access to unemployment benefits offsets the impact of job loss on crime during the benefit period, roughly lasting one semester. Based on these findings as well as extensive evidence on heterogeneity and spillovers on other household members, we conclude that in the present context liquidity constraints are the main mechanism through which job loss affects criminal behavior.

In terms of policy recommendations, our findings highlight that unemployment benefits can offset the potential increase in crime immediately after layoff, particularly for those workers who are more likely to be financially constrained. However, these effects are temporary and vanish upon the termination of unemployment benefits. Therefore, income support should be accompanied by active labor market policies aimed at speeding up the return of workers to jobs and guaranteeing stable income rather than temporary income assistance. They also suggest that both passive and active policies should be targeted at vulnerable groups – e.g. through means-tested schemes – because such groups are at greater risk of poverty upon layoff and consequently are more likely to commit crimes.

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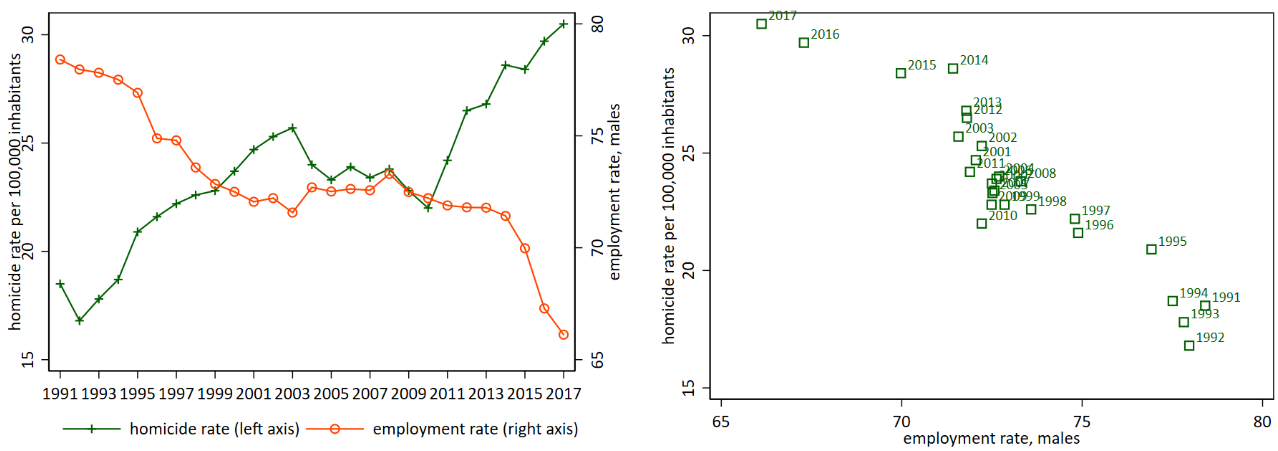
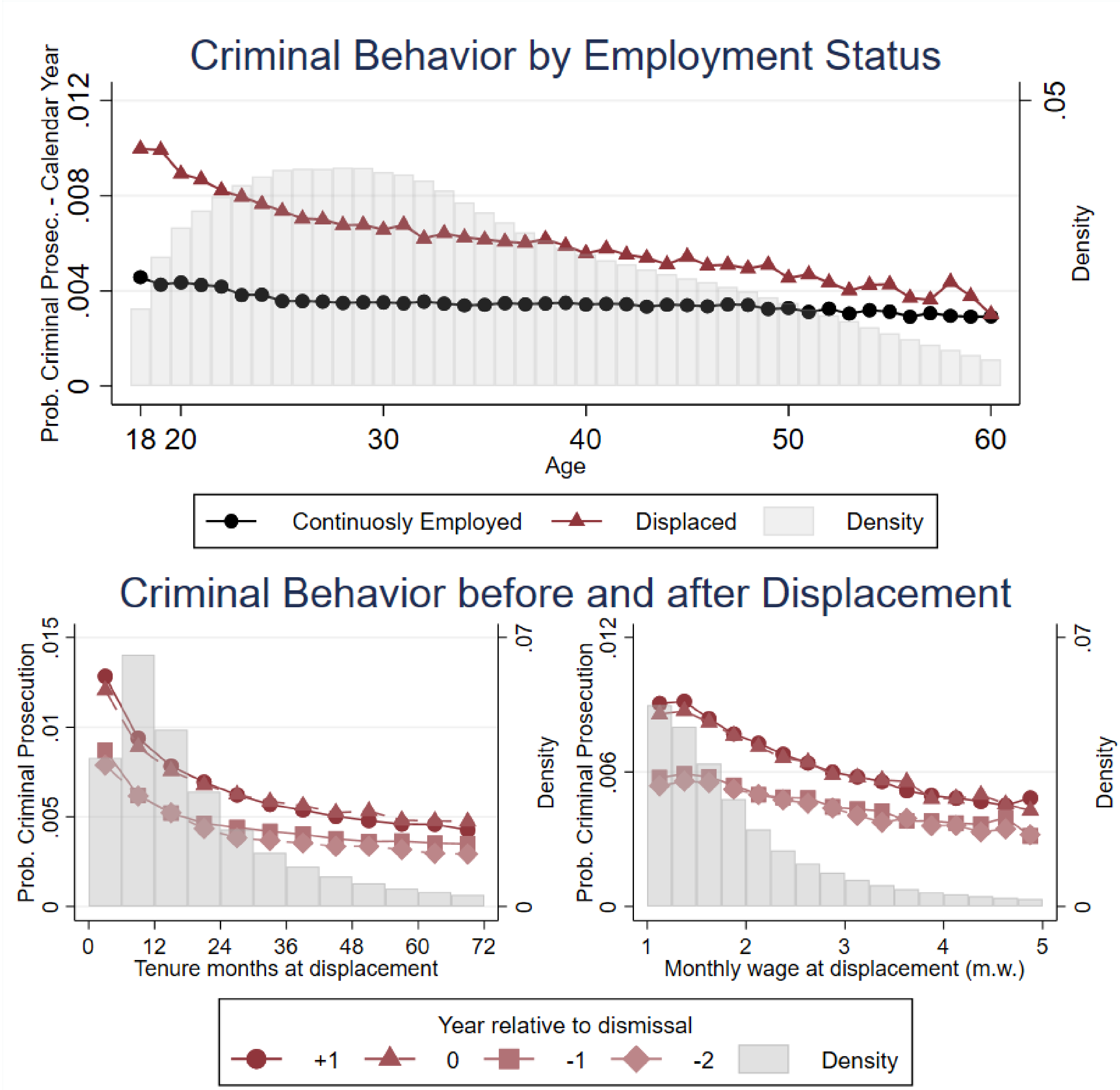
Figures

Figure 1: Homicides and employment in Brazil, 1991-2017



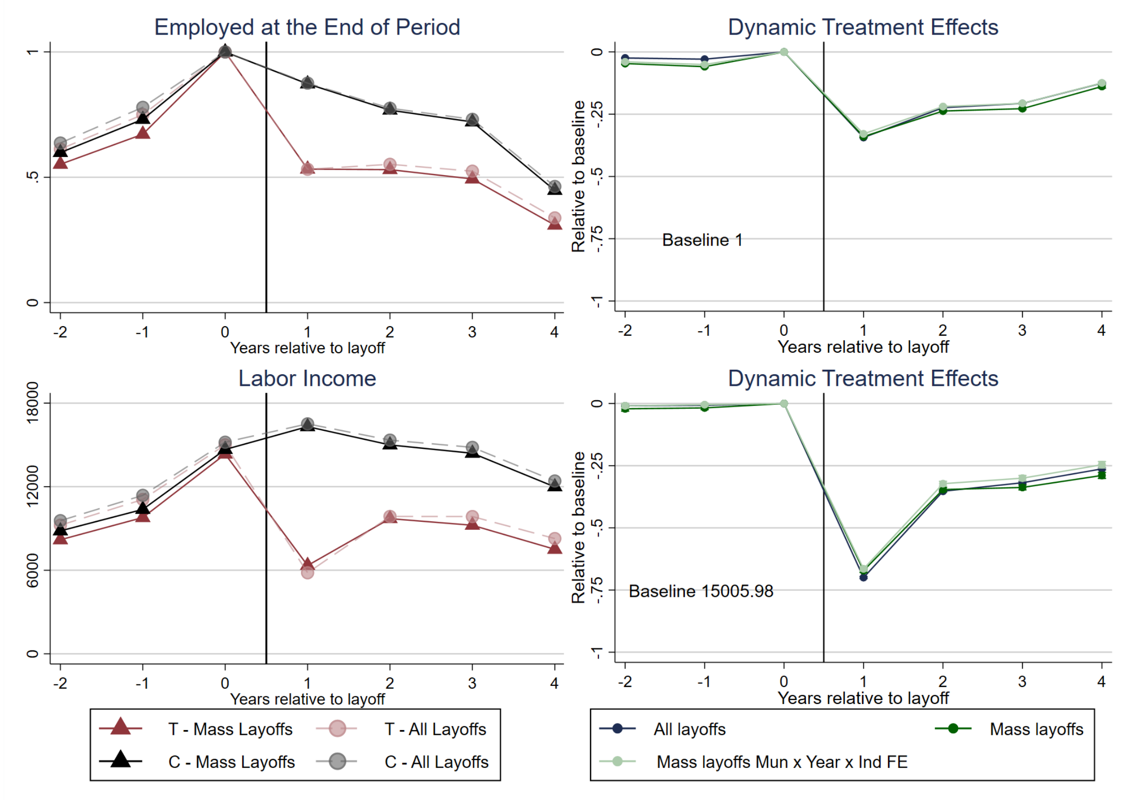
Notes: The left graph shows the evolution of the homicide rate per 100,000 inhabitants (left vertical axis) and the male employment rate (right vertical axis) in Brazil over the 1991-2017 period. The right graph plots the relationship between the two variables over time.

Figure 2: Criminal prosecutions by employment status, age, job tenure, and monthly wage



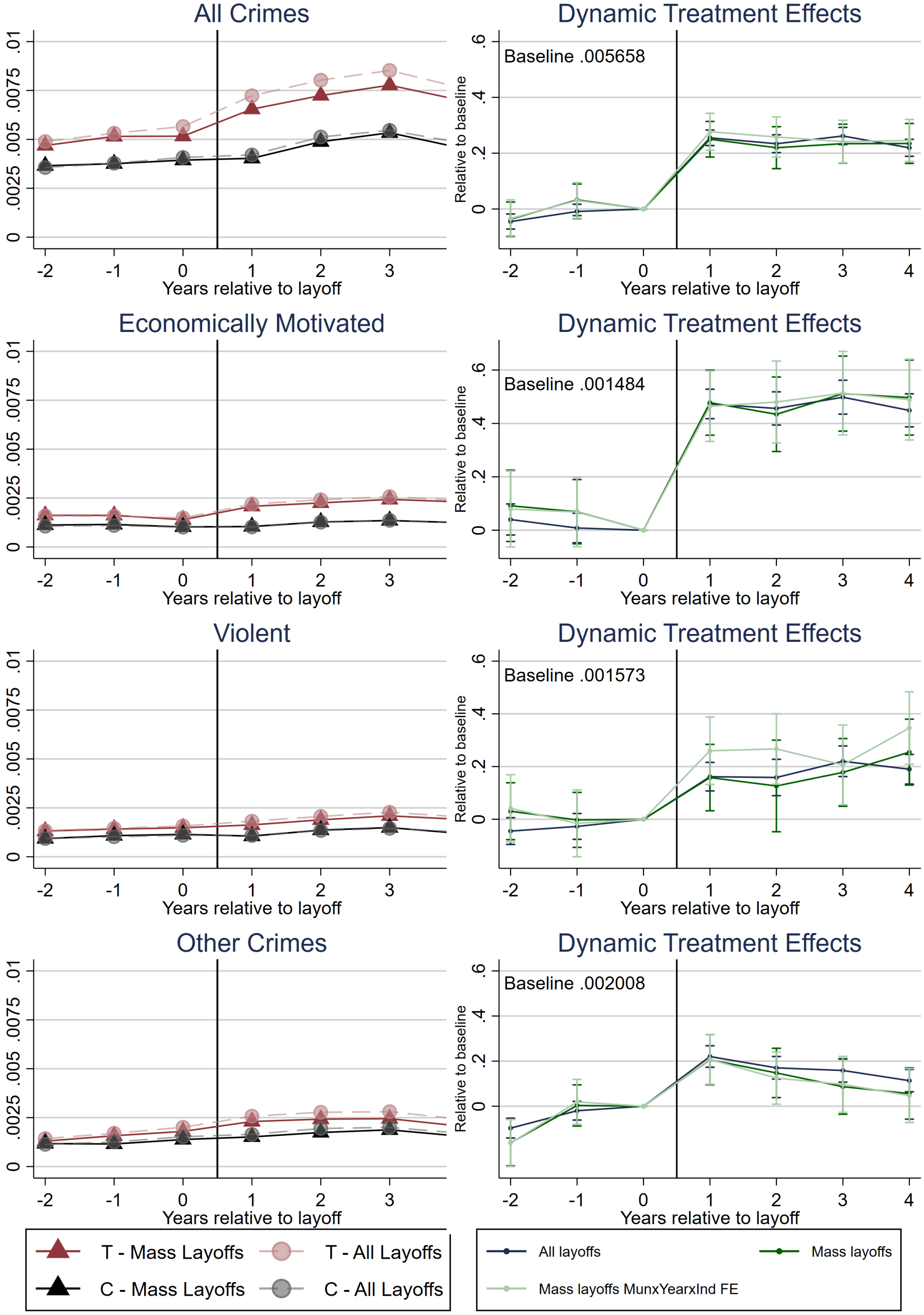
Notes: The top graph compares the average probability of being prosecuted in a given year between, respectively, workers who are continuously employed and workers losing their job in that year, respectively, by age. The bottom graphs show the probability of being prosecuted among displaced workers in the first two years after dismissal and in the last two years before dismissal, by tenure (left graph) and monthly wage – as multiples of the minimum wage – (right graph). The distributions of age, tenure, and monthly wage are also shown in the graphs.

Figure 3: Effect of job loss on subsequent employment and earnings



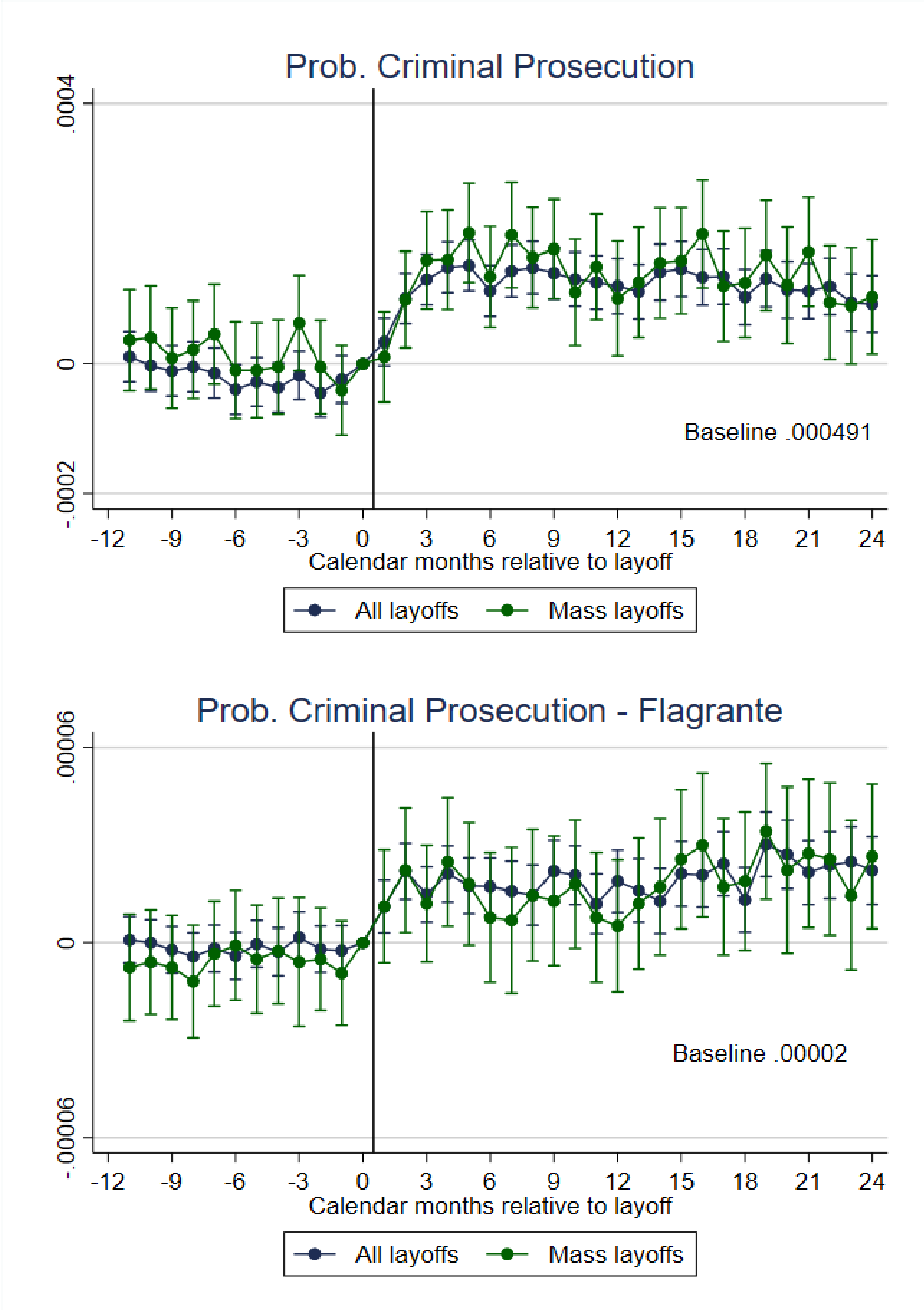
Notes: This figure shows the effect of job loss on individual employment (top graphs) and labor earnings (bottom graphs). Graphs on the left show the dynamics of such variables for treated workers (T) displaced at time 0 in mass and non-mass layoffs, respectively, and for matched control workers (C) in non-mass layoff firms who are not displaced in the same calendar year. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Graphs on the right report the dynamic treatment effects of layoff – estimated according to equation (1) – along with 95% confidence intervals (too small to be visible). All coefficients are rescaled by the baseline average value of the outcome variable in the treated group at $t = 0$, which is also reported. Income variables are measured in Brazilian Reais.

Figure 4: Effect of job loss on crime - prosecution probabilities



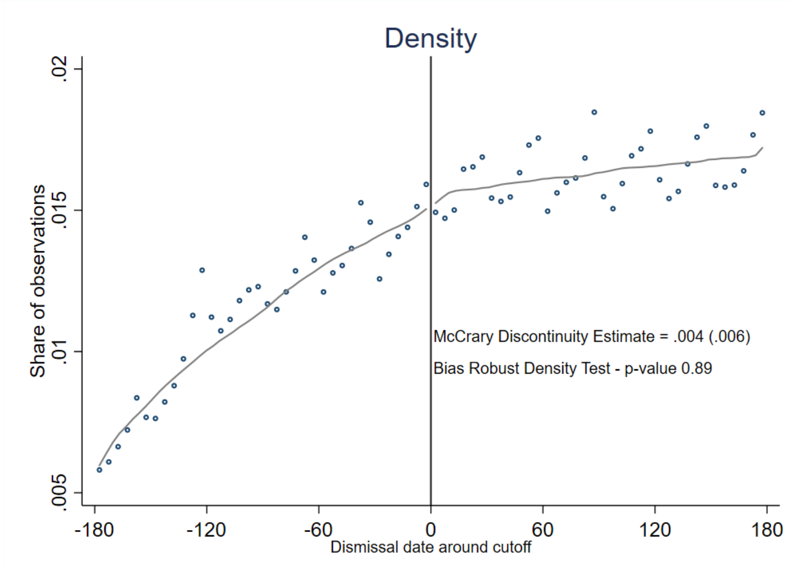
Notes: This figure shows the effect of job loss on the probability of being prosecuted for different types of crime. Graphs on the left show the dynamics of such probability for treated workers (T) displaced at time 0 in mass and non-mass layoffs, respectively, and for matched control workers (C) in non-mass layoff firms who are not displaced in the same calendar year. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Graphs on the right report the dynamic treatment effects of layoff – estimated by equation (1) – along with 95% confidence intervals. All coefficients are rescaled by the baseline average value of the outcome variable in the treated group at $t = 0$, which is also reported.

Figure 5: Effect of job loss on all prosecutions and prosecutions *in flagrante*, monthly-level analysis



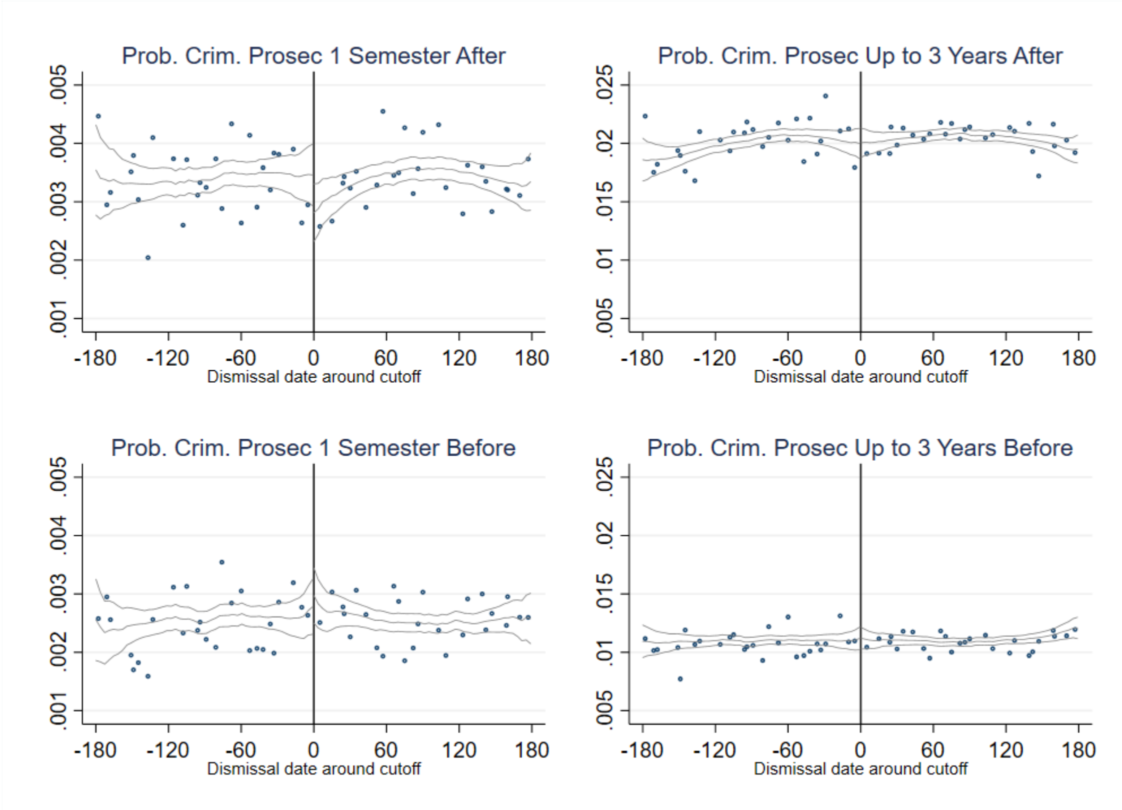
Notes: This figure shows the effect of job loss on the probability of being prosecuted (top graph) and being prosecuted "in flagrante" (bottom graph). Graphs report the dynamic treatment effects of layoff, estimated by equation (1) at a monthly frequency, along with 95% confidence intervals. The treatment group comprises workers displaced at time 0 in mass and non-mass layoffs, while the control group is defined via matching among workers in non-mass layoff firms who are not displaced in the same calendar year.

Figure 6: Distribution of observations around the UI eligibility threshold



Notes: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past). The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. The results of McCrary density test and the bias robust test proposed by Cattaneo et al. (2018, 2019) are also reported.

Figure 7: Effect of UI eligibility on crime

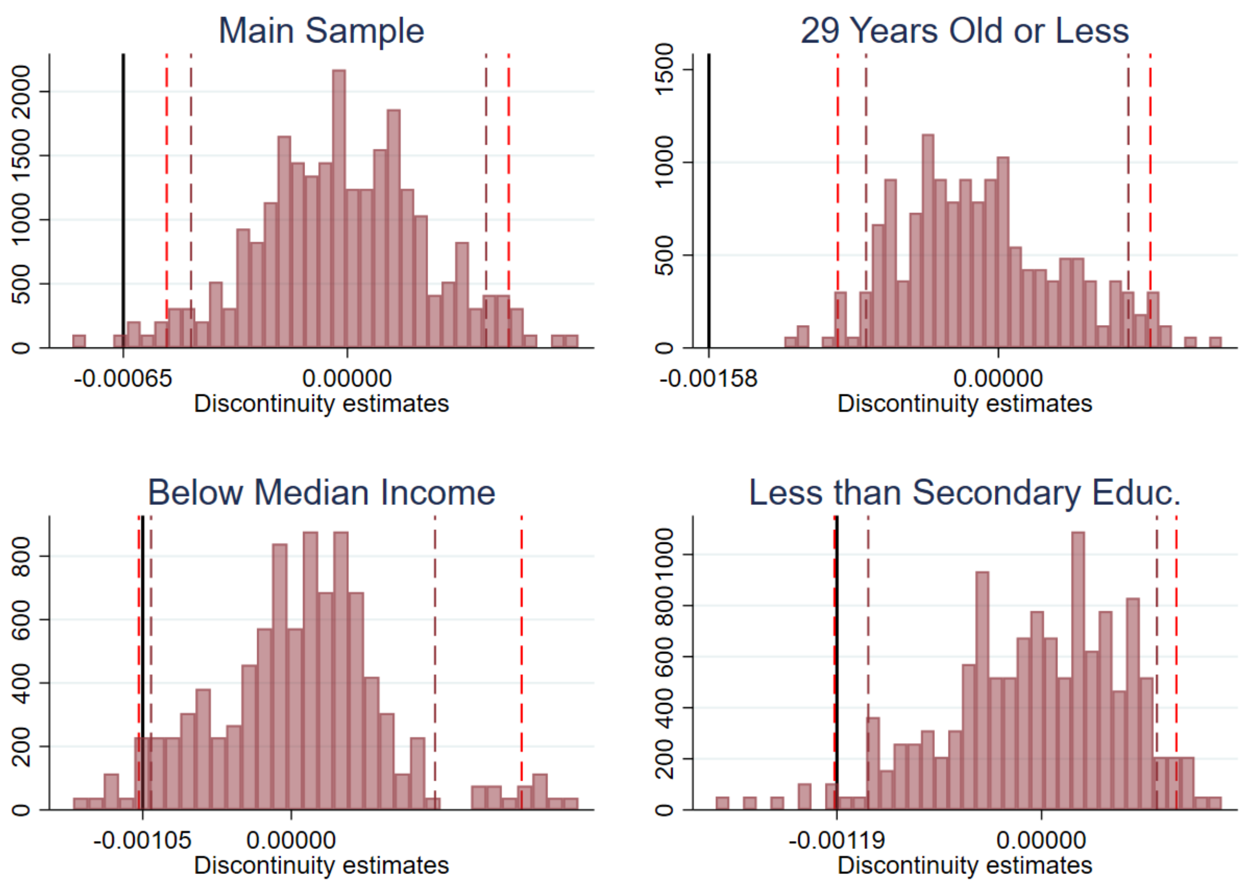


Notes: The graphs plot the probability of criminal prosecution after and before layoff (top and bottom graphs, respectively) around the cutoff date for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

Figure 8: Effect of UI eligibility on crime in the first semester after layoff, by group

Notes: The graphs plot the probability of criminal prosecution within one semester since layoff around the cutoff date for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of tenure. Separate graphs by age, income, and education groups are presented. 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.

Figure A9: Effect of UI eligibility on the probability of committing crimes in the first semester after layoff, permutation tests



Notes: The graphs compare discontinuity estimates of the effect of UI eligibility on the probability of committing crimes in the first semester after layoff obtained at the true cutoff for UI eligibility (vertical black line) with the distribution of estimates obtained at all possible placebo cutoffs within 180 days away from the actual threshold, for different groups of individuals (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Each estimate is based on a local linear polynomial smoothing with a 60-day bandwidth, as in equation (3).

Tables

Table 1: Summary statistics, treated and control workers in mass and non-mass layoffs

	(1)	(2)	(3)	(4)	(5)	(6)
	All layoffs			Mass layoffs		
	Treatment	Control	Std Diff	Treatment	Control	Std Diff
Demographic characteristics						
Years of education	10.8	11.1	0.12	10.1	10.9	0.29
Age	30.3	30.3	0.00	30.7	30.7	0.00
Race - white	54.1%	55.9%	0.04	45.9%	49.3%	0.07
Race - black	4.9%	4.9%	-0.00	5.6%	5.2%	-0.02
Race - brown	32.2%	31.3%	-0.02	39.7%	37.7%	-0.04
Job characteristics						
Monthly income (R$)	1,413	1,420	0.01	1,396	1,402	0.01
Month of worked $t-1$	11.2	11.5	0.09	10.8	11.3	0.15
Tenure on Jan 1^{st} (years)	1.6	1.6	0.01	1.1	1.2	0.03
Manager	5.2%	6.6%	0.06	3.2%	5.3%	0.10
Firm size (employees)	448	449	0.00	572	505	-0.05
Local area - municipality						
Large municipality - pop> 1M	34%	33%	-0.01	38%	39%	0.01
Municipality population	2,012,523	2,031,573	0.01	2,178,083	2,222,797	0.01
Homicide rate (per 100k inhab.)	28.3	27.1	-0.06	31.1	29.6	-0.07
Crime						
Prob. of criminal prosecution $t-1$	0.0057	0.0041	-0.02	0.0052	0.0039	-0.02
Prob. Prosec - economically motivated	0.0015	0.0010	-0.01	0.0014	0.0010	-0.01
Prob. Prosec - drug trafficking	0.0005	0.0003	-0.01	0.0005	0.0003	-0.01
Prob. Prosec - property crime	0.0006	0.0004	-0.01	0.0006	0.0005	-0.01
Prob. Prosec - violent crime	0.0009	0.0007	-0.01	0.0009	0.0007	-0.01
Prob. Prosec - other crimes	0.0026	0.0019	-0.01	0.0024	0.0018	-0.01
Observations	4,870,849	4,870,849		1,167,846	1,167,846	

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

Table 2: Effect of job loss on labor market outcomes and criminal behavior

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor market effects		Probability of criminal prosecution			
Dependent variable:	Employment	Earnings	Any crime	Economic	Violent	Others
PANEL A: ALL DISPLACED WORKERS						
$Treat_i \times Post_t$	-0.21***	-6048.1***	0.0015***	0.00067***	0.00033***	0.00041***
	(0.0006)	(28.1)	(0.00005)	(0.00003)	(0.00003)	(0.00003)
Mean outcome, treated at t=0	1	15,006	0.0057	0.0015	0.0016	0.0020
Effect relative to the mean	-21%	-40%	27%	45%	21%	20%
Implied elasticity to earnings			-0.66	-1.12	-0.52	-0.51
Observations	68,191,886	68,191,886	68,191,886	68,191,886	68,191,886	68,191,886
PANEL B: ALL DISPLACED WORKERS VS. ALTERNATIVE CONTROL GROUP (CONTINUOUSLY EMPLOYED WORKERS)						
$Treat_i \times Post_t$	-0.40***	-8600.1***	0.0029***	0.0010***	0.00070***	0.00097***
	(0.0006)	(32.7)	(0.00006)	(0.00003)	(0.00003)	(0.00004)
Mean outcome, treated at t=0	1	14,115	0.0051	0.0013	0.0015	0.0018
Effect relative to the mean	-40%	-61%	57%	74%	48%	55%
Implied elasticity to earnings			-0.94	-1.85	-1.19	-1.36
Observations	59,737,776	59,737,776	59,737,776	59,737,776	59,737,776	59,737,776
PANEL C: DISPLACED IN MASS LAYOFFS						
$Treat_i \times Post_t$	-0.20***	-5710.0***	0.0012***	0.00060***	0.00025***	0.00032***
	(0.002)	(53.3)	(0.0001)	(0.00006)	(0.00006)	(0.00006)
Mean outcome, treated at t=0	1	14,340	0.0052	0.0014	0.0015	0.0018
Effect relative to the mean	-20%	-40%	23%	43%	17%	18%
Implied elasticity to earnings			-0.58	-1.08	-0.42	-0.45
Observations	16,349,844	16,349,844	16,349,844	16,349,844	16,349,844	16,349,844
PANEL D: DISPLACED IN MASS LAYOFFS, CONTROLLING FOR MUNICIPALITY × INDUSTRY × YEAR FIXED-EFFECTS						
$Treat_i \times Post_t$	-0.19***	-5433.7***	0.0013***	0.00061***	0.00039***	0.00030***
	(0.001)	(59.9)	(0.0001)	(0.00006)	(0.00006)	(0.00006)
Mean outcome, treated at t=0	1	14,340	0.0052	0.0014	0.0015	0.0018
Effect relative to the mean	-19%	-38%	25%	44%	26%	17%
Implied elasticity to earnings			-0.66	-1.15	-0.69	-0.44
Observations	16,250,836	16,250,836	16,250,836	16,250,836	16,250,836	16,250,836

Notes: This table shows the effect of job loss on labor market outcomes (columns 1-2) and the probability of criminal prosecution for different types of crime (columns 3-6), as estimated from the difference-in-differences equation (2). The dependent variable is indicated on top of each column. The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. Panel A includes in the sample all displaced workers and matched control workers employed in non-mass layoff firms who are not displaced in the same calendar year; Panel B restricts the control group to workers who remain continuously employed after the matched treated worker has been displaced; Panel C restricts the treated group to workers who are displaced in mass layoffs; and finally, Panel D adds municipality × industry × 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 and the percent change in earnings. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 3: Effect of job loss on different types of crime

	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A. PROB. ECONOMIC CRIMES	Drug trafficking	Theft	Robbery	Trade of stolen goods	Fraud	Corruption
$Treat_i \times Post_t$	0.00025***	0.000067**	0.00019***	0.000052**	0.000046**	0.000032
	(0.00003)	(0.00003)	(0.00003)	(0.00002)	(0.00002)	(0.00002)
Mean outcome at $t = 0$ (treated)	0.0005	0.0003	0.0002	0.0001	0.0002	0.0001
Effect relative to the mean	55%	21%	91%	35%	28%	24%
PANEL B. PROB.	Assault	Homicide	Homicides		Kidnapping	Threatening
VIOLENT CRIMES			Single offense	Mult. offenses		
$Treat_i \times Post_t$	0.000033	0.000080***	0.000063**	0.000015*	0.000041*	0.00016***
	(0.00004)	(0.00002)	(0.00002)	(0.000008)	(0.00002)	(0.00004)
Mean outcome at $t = 0$ (treated)	0.0005	0.0003	0.0002	0.00003	0.0001	0.0007
Effect relative to the mean	7%	32%	28%	52%	48%	22%
PANEL C. PROB. OTHER CRIMES	Traffic related	Illegal gun possession	Slandering	Fail to obey	Small drug possession	Property damage
$Treat_i \times Post_t$	0.00013**	0.000068***	-0.0000056	0.000065**	0.000063***	0.000023
	(0.00005)	(0.00002)	(0.00004)	(0.00002)	(0.00002)	(0.00001)
Mean outcome at $t = 0$ (treated)	0.0011	0.0003	0.0002	0.0001	0.0001	0.0001
Effect relative to the mean	12%	25%	-3%	44%	58%	22%

Notes: This table shows the effect of job loss on different types of crime (indicated on top of each column), as estimated from the difference-in-differences equation (2). The explanatory variable of main interest is a dummy $Treat_i$ equal to 1 for workers displaced upon mass layoffs, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 4: Effect of job loss on crime, varying the definition of mass layoffs

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Minimum layoff share				Plant
Prob. of criminal prosecution	33%	50%	75%	90%	closure
Panel A. Minimum firm size 15					
$Treat_i \times Post_t$	0.0012***	0.00091***	0.00078***	0.00082**	0.00074***
	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0002)
Mean outcome at $t = 0$ (treated)	0.0052	0.0049	0.0045	0.0041	0.0047
Relative Effect	23%	19%	17%	20%	16%
Observations	16,349,844	7,404,544	2,721,712	1,069,446	1,877,890
Baseline gap in crime, T-C	31%	27%	16%	4%	14%
Panel B. Minimum firm size 30					
$Treat_i \times Post_t$	0.0012***	0.00094***	0.00094***	0.00089**	0.00066**
	(0.0001)	(0.0002)	(0.0002)	(0.0004)	(0.0003)
Mean outcome at $t = 0$ (treated)	0.0050	0.0048	0.0043	0.0040	0.0045
Effect relative to the mean	24%	20%	22%	22%	15%
Observations	12,975,228	6,013,280	2,191,266	850,430	1,364,188
Baseline gap in crime, T-C	31%	29%	12%	6%	18%
Panel C. Minimum firm size 50					
$Treat_i \times Post_t$	0.0012***	0.00095***	0.0010***	0.00100**	0.00096***
	(0.0001)	(0.0002)	(0.0002)	(0.0004)	(0.0003)
Mean outcome at $t = 0$ (treated)	0.0049	0.0047	0.0044	0.0041	0.0041
Effect relative to the mean	24%	20%	23%	24%	23%
Observations	10,888,920	5,157,236	1,862,154	723,380	1,065,946
Baseline gap in crime, T-C	31%	29%	11%	10%	10%
Panel D. Minimum firm size 100					
$Treat_i \times Post_t$	0.0012***	0.0011***	0.0011***	0.00095**	0.00087**
	(0.0002)	(0.0002)	(0.0003)	(0.0004)	(0.0003)
Mean outcome at $t = 0$ (treated)	0.0047	0.0046	0.0045	0.0042	0.0039
Effect relative to the mean	25%	24%	25%	23%	22%
Observations	8,516,872	4,143,622	1,501,150	603,792	754,054
Baseline gap in crime, T-C	30%	30%	8%	10%	13%

Notes: This table shows the effect of job loss on the probability of being prosecuted for a crime, as estimated from the difference-in-differences equation (2) using different definitions of mass layoffs. The explanatory variable of main interest is a dummy $Treat_i$ equal to 1 for workers displaced upon mass layoffs, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. Columns (1) to (4) progressively increase the minimum share of dismissed workers used to define mass layoffs – indicated on top of each column – while column (5) restricts the treated group to workers who are either dismissed or quit in plant closures. Panels A to D progressively increase the minimum size of firms used to define mass layoffs. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 5: Effect of job loss on labor market outcomes and crime, including all workers in mass and non-mass layoff firms

	(1)	(2)	(3)	(4)	(5)	(6)
Sample definition:	Treated: all workers in mass layoff firms Controls: displaced in non-mass layoff firms			Treated: all workers in mass layoff firms Controls: all workers in non-mass layoff firms		
Dependent variable:	Employment	Earnings	Prob. Any crime	Employment	Earnings	Prob. Any crime
$Treat_i \times Post_t$	-0.17***	-6146.1***	0.00090***	-0.10***	-1983.2***	0.00018**
	(0.002)	(159)	(0.00009)	(0.002)	(144.2)	(0.00008)
Mean outcome at t=-1	1	23363	0.0050	1	23141	0.0052
Relative effect	-17%	-26%	18%	-10%	-9%	3%
Implied elasticity			-0.68			-0.41
Observations	27,322,876	27,322,876	27,322,876	29,602,748	29,602,748	29,602,748

Notes: This table shows the effect of job loss on labor market outcomes and probability of being prosecuted for a crime, as estimated from the difference-in-differences equation (2) using different definitions of treated and control groups. The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for *all* workers employed at the beginning of a calendar year in firms undergoing mass layoffs during that year, interacted with a dummy $Post_t$ that is equal to 1 for the period after displacement. In columns (1)-(3), the control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year; in columns (4)-(6), the control group is extended to *all* workers employed in non-mass layoff firms that are matched to treated workers on individual characteristics. The table also reports the baseline mean outcome for the treated group at the date of displacement; the percent effect relative to the baseline mean; and the implied elasticity of crime to earnings, computed as the ratio between the percent change in crime and the percent change in earnings. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

Table 6: Effect of job loss on crime, heterogeneity of the effect across different groups

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Differential effect by:				
Prob. of criminal prosecution	Age	Tenure	Education	Earnings	Homicide rate
$Treat_i \times Post_t$	0.00086***	0.00047*	0.0010***	0.0011***	0.00097***
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0001)
	[18%]	[16%]	[21%]	[23%]	[20%]
$Treat_i \times Post_t \times$ 20-29 years old	0.00073***				
	(0.0002)				
	[28%]				
$Treat_i \times Post_t \times$ 0-5 tenure months		0.0019***			
		(0.0004)			
		[29%]			
$Treat_i \times Post_t \times$ 6-24 tenure months		0.00076**			
		(0.0003)			
		[25%]			
$Treat_i \times Post_t \times$ 25-60 tenure months		-0.000078			
		(0.0003)			
		[10%]			
$Treat_i \times Post_t \times$ Less than sec. school			0.00041**		
			(0.0002)		
			[25%]		
$Treat_i \times Post_t \times$ below median income				0.00024	
				(0.0002)	
				[24%]	
$Treat_i \times Post_t \times$ above median homicide rate					0.00044**
					(0.0002)
					[26%]
Observations	16,349,844	16,349,844	16,349,844	16,349,844	16,334,276

Notes: This table shows the effect of job loss on the probability of being prosecuted for a crime across different groups of individuals, as estimated from the difference-in-differences equation (2). The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for workers displaced upon mass layoffs, interacted with a dummy $Post_t$ that is equal to 1 for the period after displacement. Such variable is further interacted with a set of dummies for individual age, tenure, education, earnings, and municipality homicide rate. The same dummies are also interacted with all other variables in the regression (i.e., $Treat_i$ and a set of year fixed effects). The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The total relative effect for each group is reported in square brackets, i.e. the ratio between the total effect estimated for each group relative to the group's average outcome among treated units at $t = 0$. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 7: Effect of job loss on household members' employment and crime

Household members:	cohabiting sons		brothers, 20-29 y.o.	
Dependent variable:	employment	crime	employment	crime
$Treat_i \times Post_t$	0.0055	0.0019**	-0.0017	0.00039
	(0.005)	(0.0009)	(0.003)	(0.0005)
Mean outcome at t=-1	0.4172	0.0106	0.3855	0.0077
Effect relative to the mean	1%	18%	-0.4%	5%
Observations	334,061	334,061	863,940	863,940

Household members:	brothers, 30-50 y.o.		male partner	
Dependent variable:	employment	crime	employment	crime
$Treat_i \times Post_t$	0.014**	0.0017	0.0035	-0.0014
	(0.007)	(0.001)	(0.005)	(0.001)
Mean outcome at t=-1	0.3316	0.0047	0.4446	0.0086
Effect relative to the mean	4%	36%	0.8%	-16%
Observations	145,684	145,684	212,513	212,513

Notes: This table shows the effect of worker's displacement on the employment and the probability of criminal prosecution for different categories of household members (indicated on top of each column), as estimated from the difference-in-differences equation (2). The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for the household members of workers displaced upon mass layoffs, interacted with a dummy $Post_t$ that is equal to 1 for the period after displacement. The control group includes household members of workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table 8: Effect of UI Eligibility on crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Total	Age		Education		Income	
Prob. of criminal prosecution	sample	$\leq$ 29	>30	< 12 years	$\geq$ 12 years	$\leq$ R$1035	> R$1035
PANEL A. 1 SEMESTER AFTER LAYOFF							
Eligibility for UI benefits	-0.0007*	-0.0016***	0.0003	-0.0012*	-0.0003	-0.0011*	-0.0003
	(0.0004)	(0.0006)	(0.0005)	(0.0006)	(0.0005)	(0.0006)	(0.0005)
Mean outcome at the cutoff	0.0035	0.0042	0.0028	0.0040	0.0031	0.0042	0.0028
Effect relative to the mean	-20.1%	-38.3%	10.7%	-29.7%	-9.8%	-26.4%	-10.6%
PANEL B. 3 YEARS AFTER LAYOFF							
Eligibility for UI benefits	0.000	-0.0018	0.0017	-0.0006	0.0004	-0.0007	0.0006
	(0.0009)	(0.0014)	(0.0012)	(0.0015)	(0.0012)	(0.0014)	(0.0012)
Mean outcome at the cutoff	0.0205	0.0237	0.0173	0.0225	0.0189	0.0225	0.0185
Effect relative to the mean	0.0%	-7.6%	9.8%	-2.7%	2.1%	-3.1%	3.2%
PANEL C. 1 SEMESTER BEFORE LAYOFF (PLACEBO)							
Eligibility for UI benefits	0.0002	0.000	0.0003	-0.0005	0.0006	0.0003	0.000
	(0.0003)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Mean outcome at the cutoff	0.0025	0.0027	0.0023	0.0029	0.0022	0.0029	0.0021
Effect relative to the mean	7.9%	0.0%	12.9%	-17.3%	26.8%	10.3%	0.0%
PANEL D. 3 YEARS BEFORE LAYOFF (PLACEBO)							
Eligibility for UI benefits	0.000	-0.0005	0.0006	-0.0007	0.0006	-0.0001	0.0002
	(0.0007)	(0.001)	(0.001)	(0.0011)	(0.0009)	(0.001)	(0.0009)
Mean outcome at the cutoff	0.0109	0.0110	0.0109	0.0123	0.0099	0.0121	0.0098
Effect relative to the mean	0.0%	-4.6%	5.5%	-5.7%	6.0%	-0.8%	2.0%
Bandwidth	60	60	60	60	60	60	60
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	362,631	179,875	182,756	158,158	204,473	180,161	182,470

Notes: This table shows the effect of eligibility for UI benefits on the probability of being prosecuted for a crime, as estimated from equation (3). The dependent variable is the probability of being prosecuted for a crime within one semester and 3 years after layoff (Panels A and B, respectively); as a placebo exercise, we estimate the same model for the probability of being reported within one semester and 3 years before layoff (Panels C and D, respectively). The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Columns (2) to (7) estimate separate regressions for the different groups indicated on top of each column. The table also reports the baseline mean outcome at the cutoff and the percent effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses (*** p$\leq$0.01, ** p$\leq$0.05, * p$\leq$0.1).

Appendix

Figure A1: Distribution of individual characteristics across displaced workers with a unique name and other displaces workers

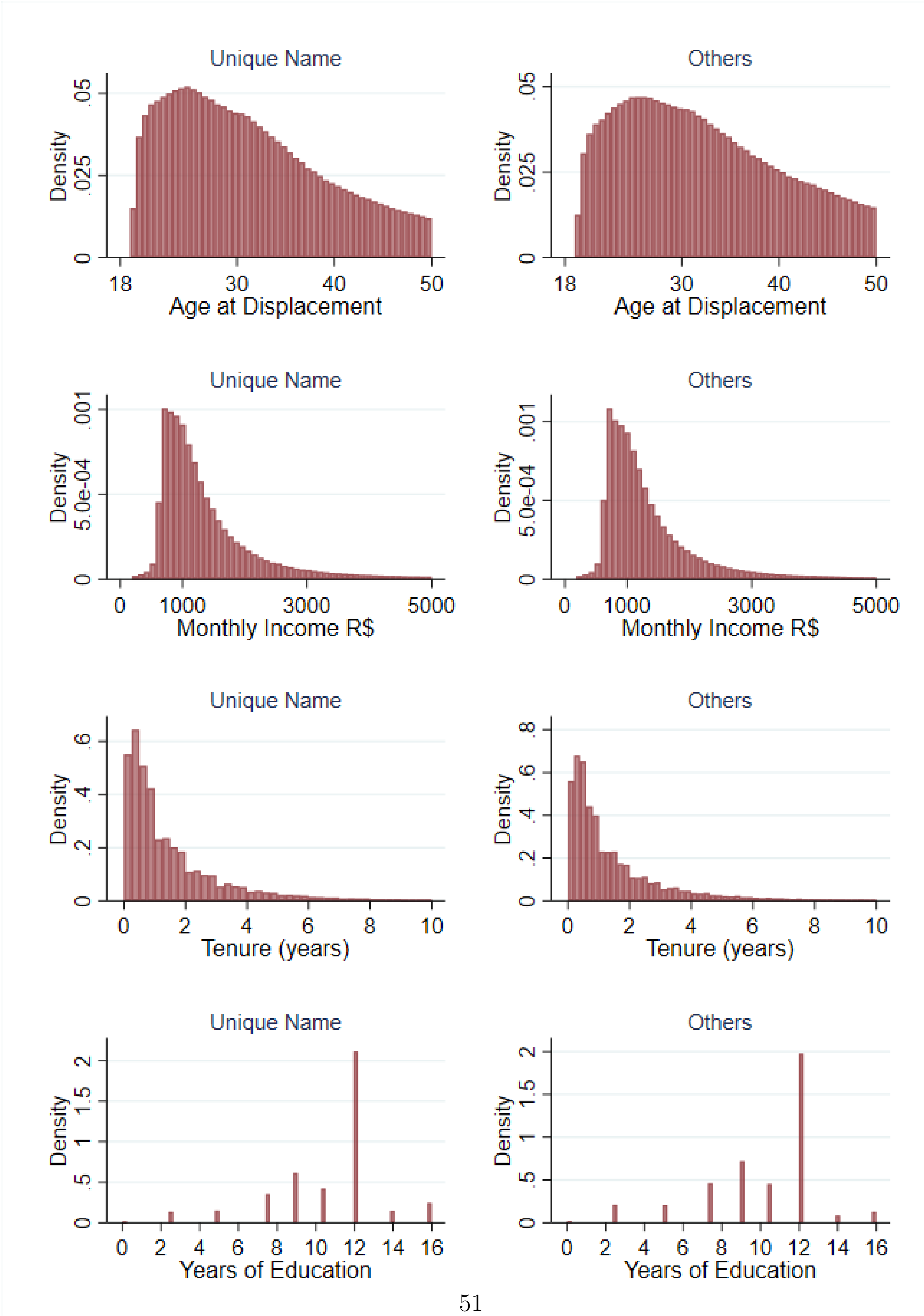


Figure A2: Distribution of individual characteristics in the treatment and control groups, all layoffs

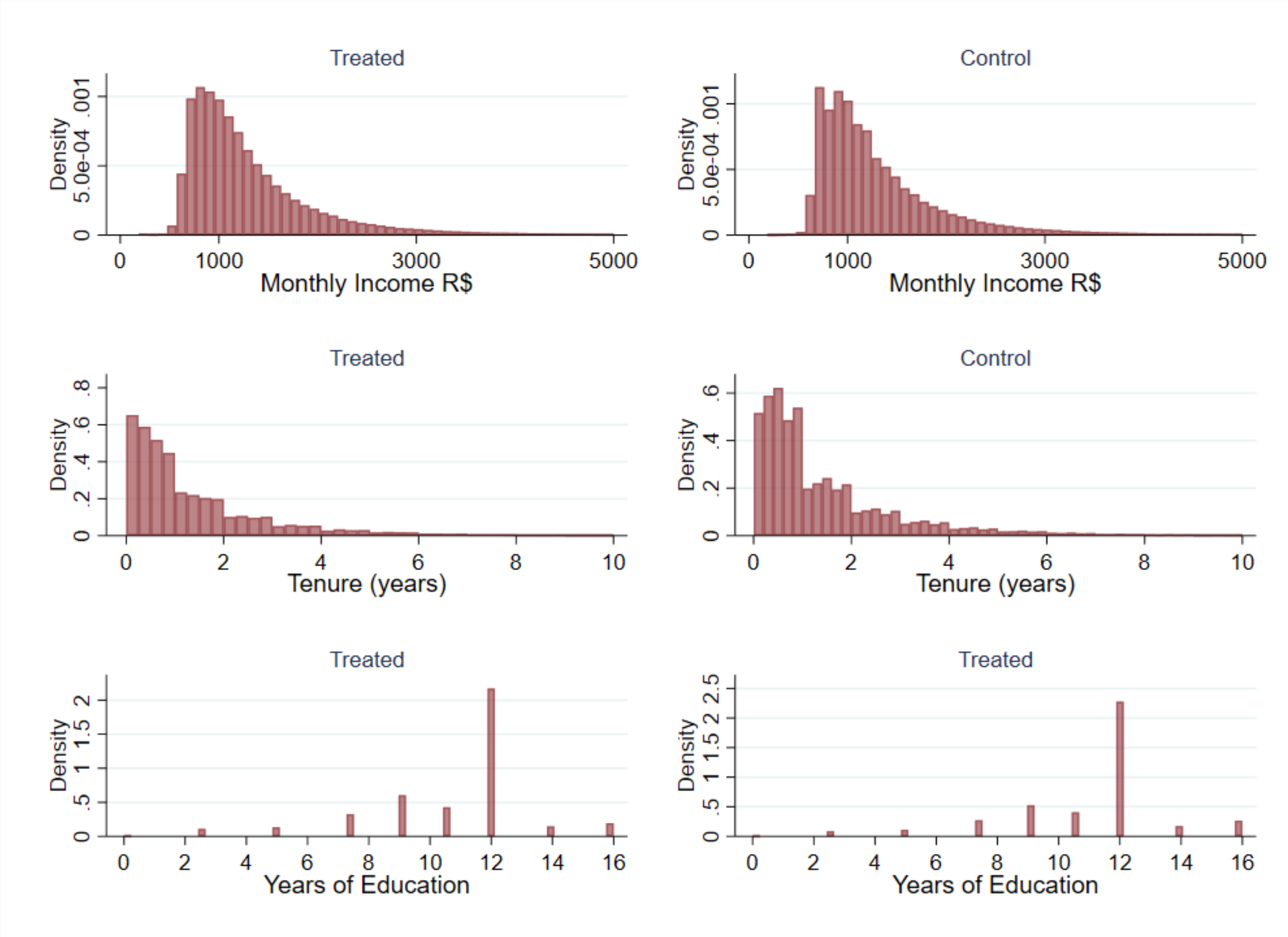


Figure A3: Distribution of individual characteristics in the treatment and control groups, mass layoffs

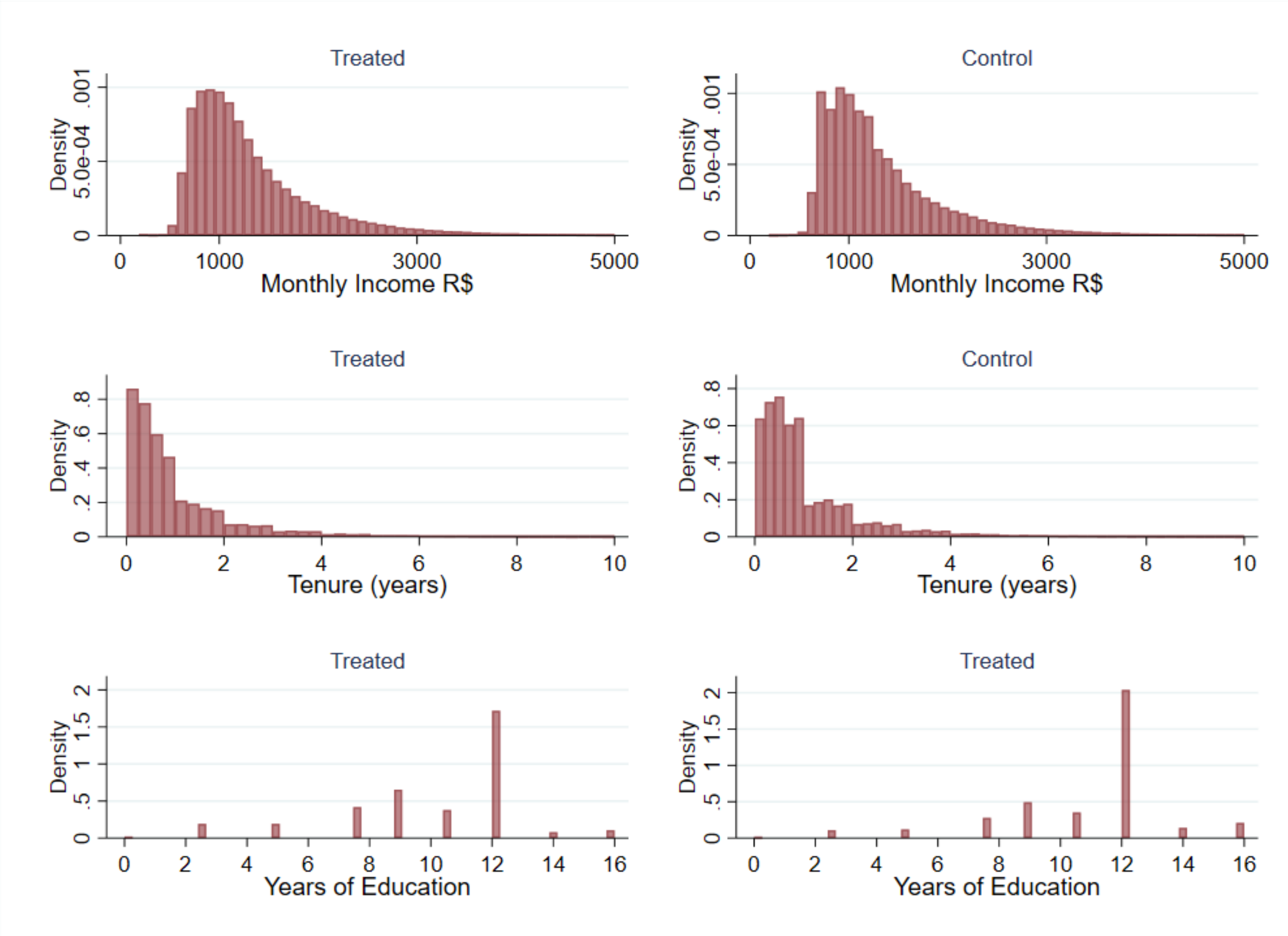
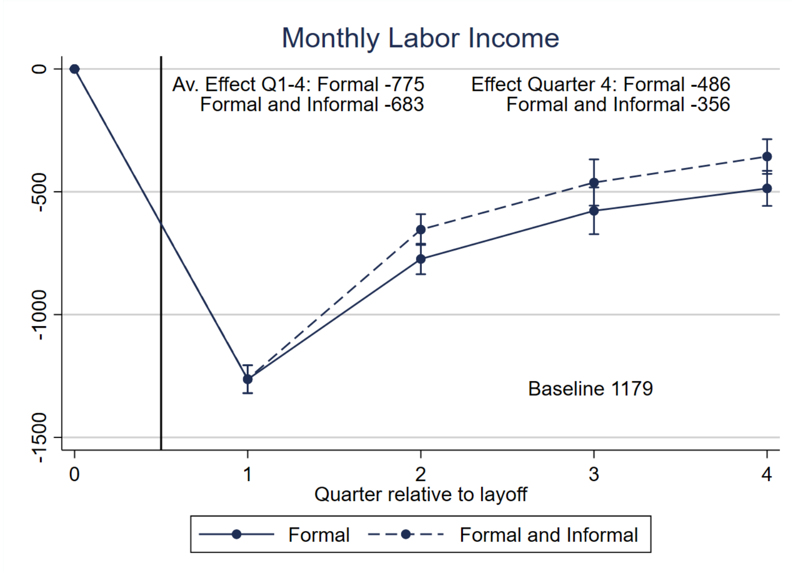
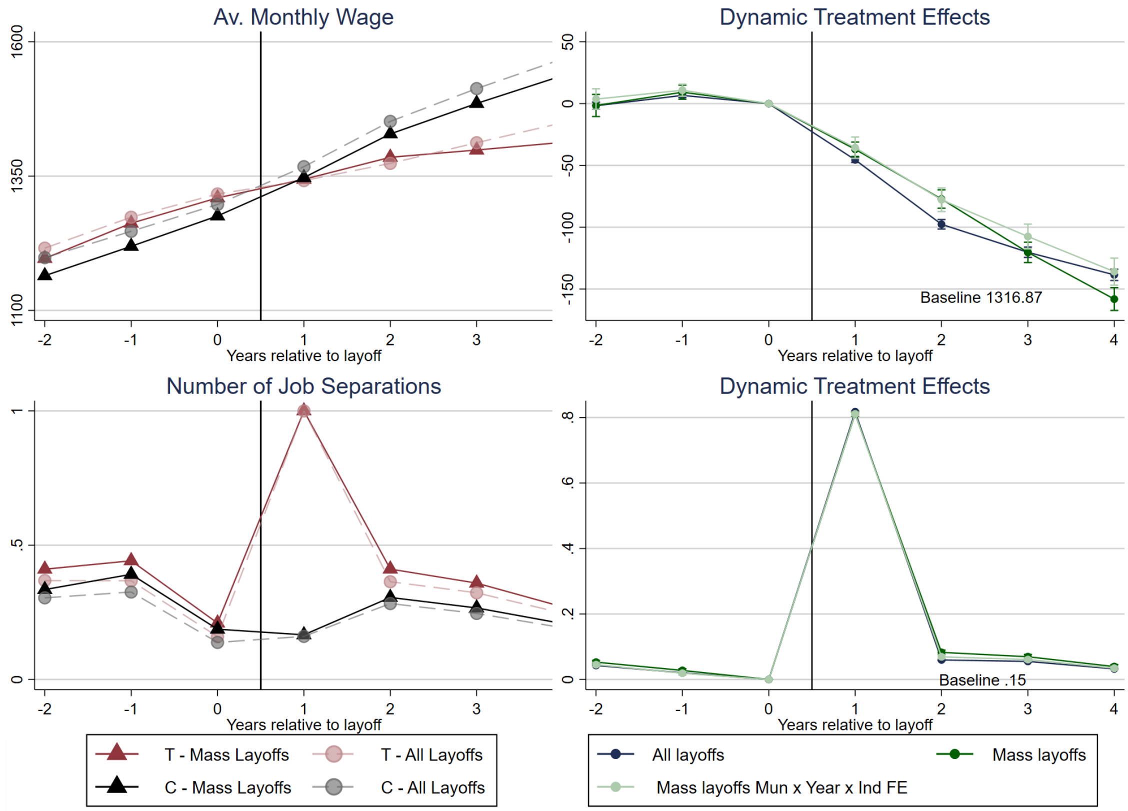


Figure A4: Effect of job loss on formal and informal labor earnings



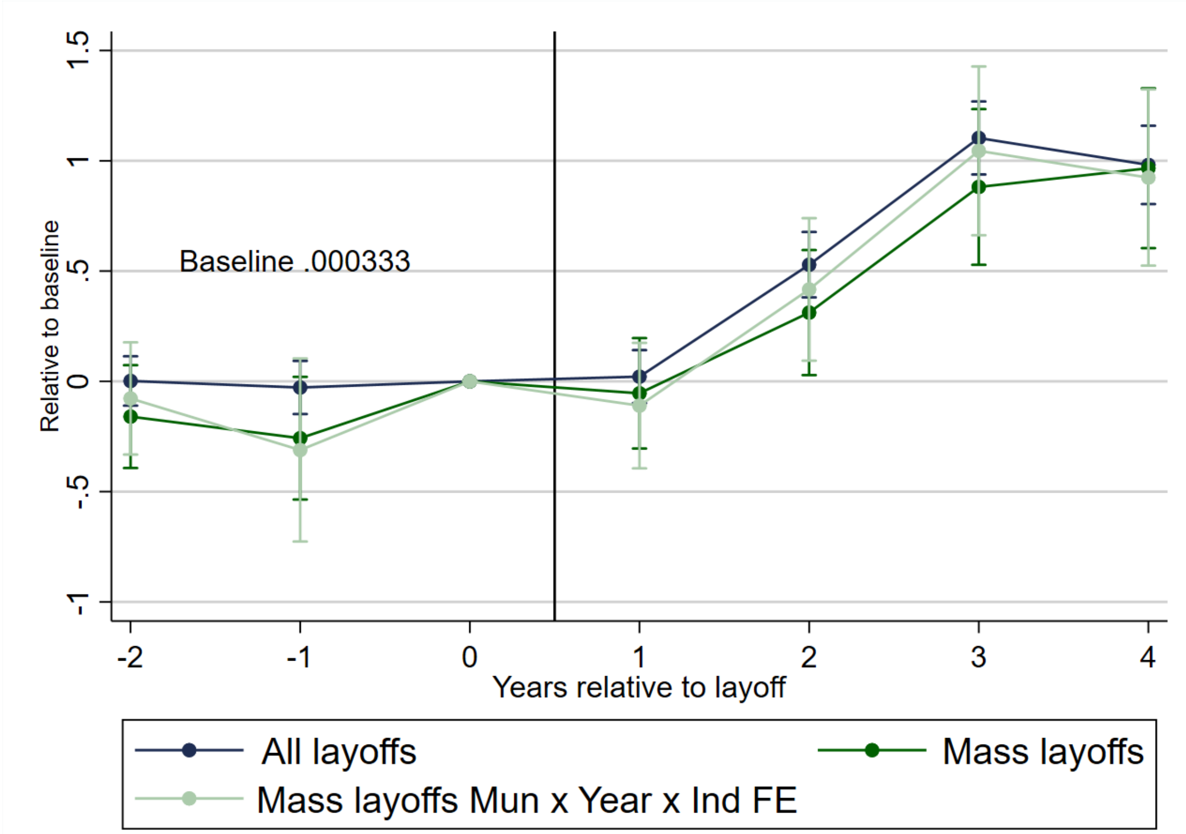
Notes: This figure shows the effect of job loss on formal and informal monthly labor earnings, based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. The sample covers individuals first interviewed in the period 2012-14. The treatment group is defined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

Figure A5: Effect of job loss on subsequent monthly wages and job turnover



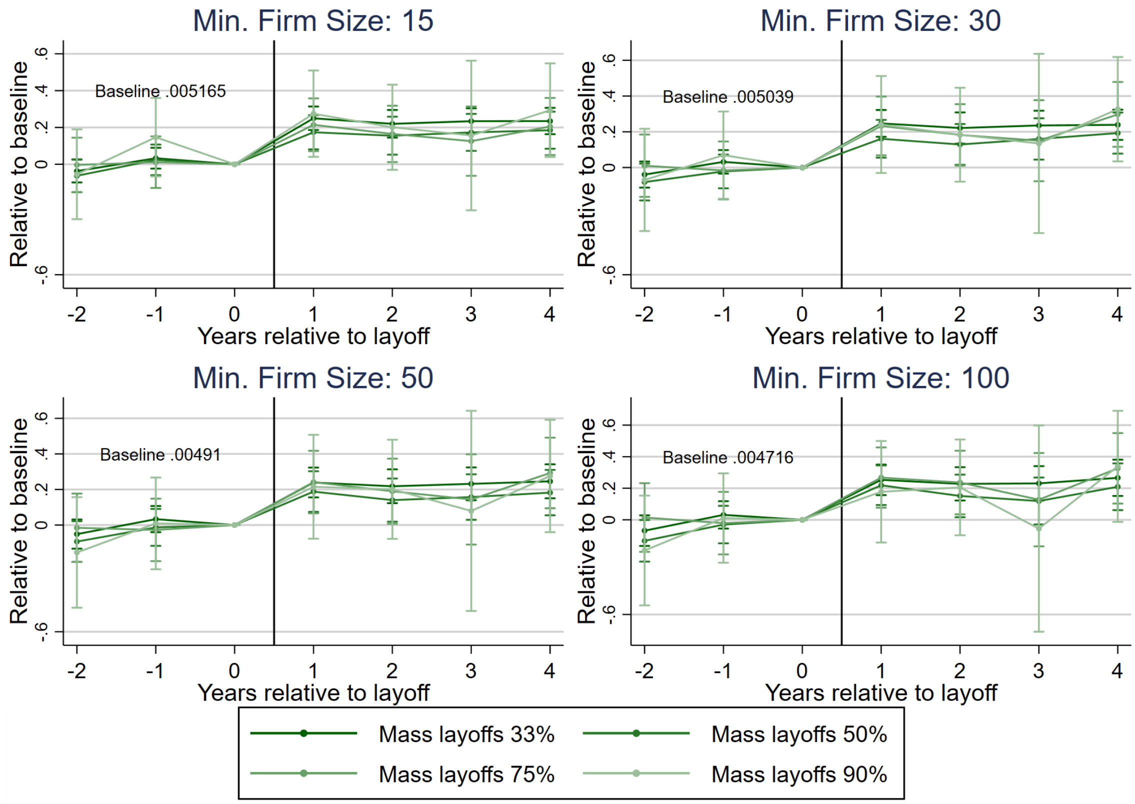
Notes: This figure shows the effect of job loss on individual monthly wages conditional on being employed (top graphs) and the number of job separations per year (bottom graphs). Graphs on the left show the dynamics of such variables for treated workers (T) displaced at time 0 in mass and non-mass layoffs, respectively, and for matched control workers (C) in non-mass layoff firms that are not displaced in the same calendar year. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Graphs on the right report the dynamic treatment effects of layoff, estimated according to equation (1), along with 95% confidence intervals (too small to be visible). All coefficients are rescaled by the baseline average value of the outcome variable in the treated group at $t = 0$, which is also reported. Monthly wages are measured in Brazilian Reais.

Figure A6: Effect of job loss on final criminal conviction



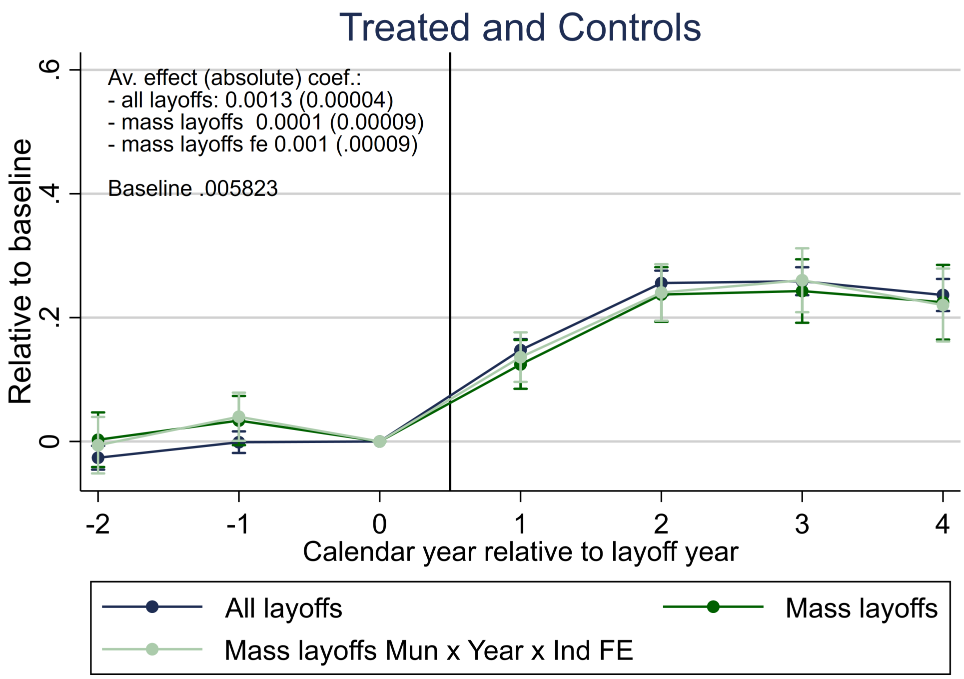
Notes: The graph reports the dynamic treatment effects of job loss on the probability of a final criminal conviction, estimated according to equation (1), along with 95% confidence intervals. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. All coefficients are rescaled by the baseline average value of the outcome variable in the treated group at $t = 0$, which is also reported.

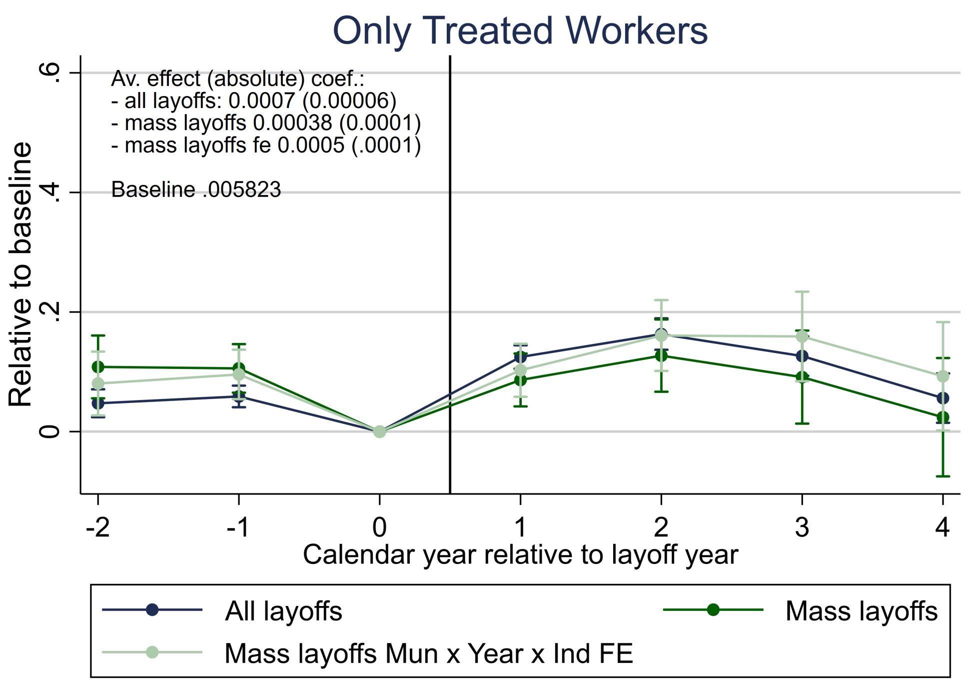
Figure A7: Effect of job loss on crime, robustness to alternative definitions of mass layoffs



Notes: The graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime using different mass layoff definitions and estimated according to equation (1), along with 95% confidence intervals. Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. All coefficients are rescaled by the baseline average values of each variable for the treated group at $t = 0$ which are also reported.

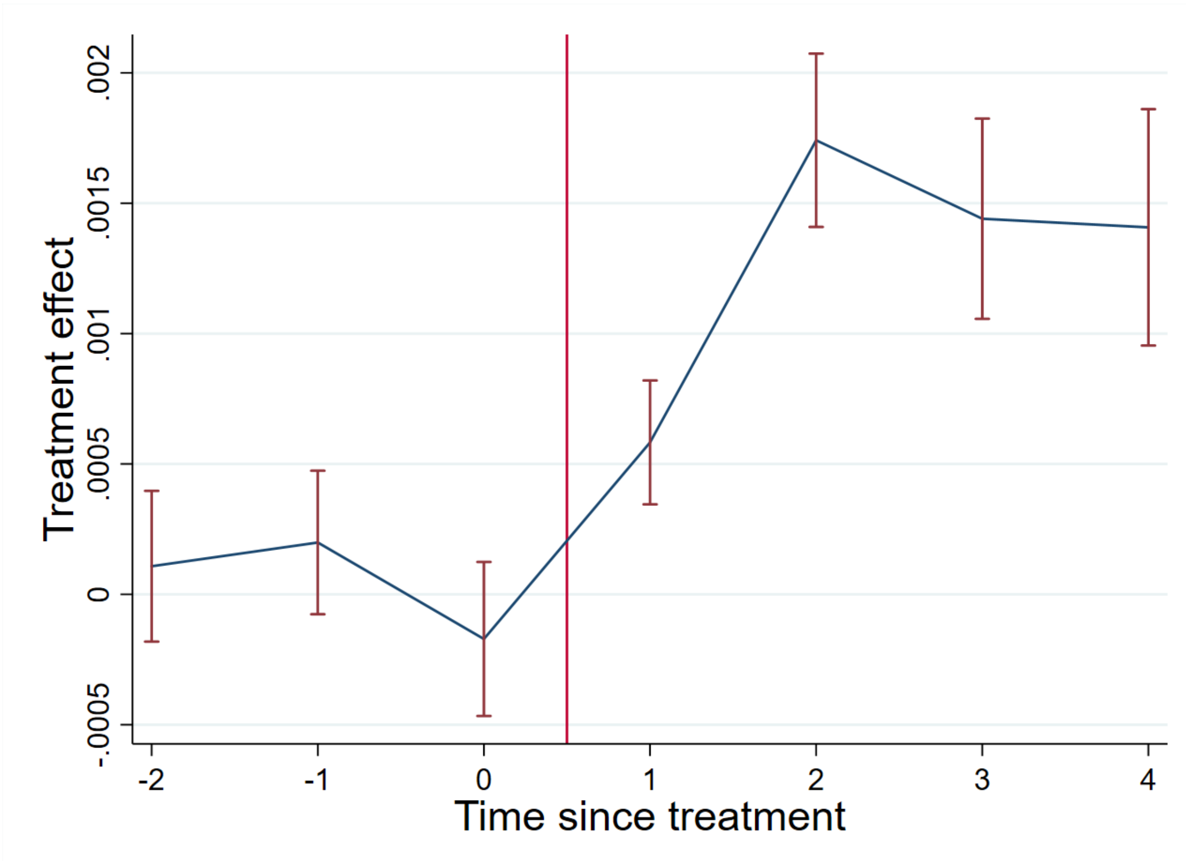
Figure A8: Effect of job loss on crime, two-way fixed effects panel estimates





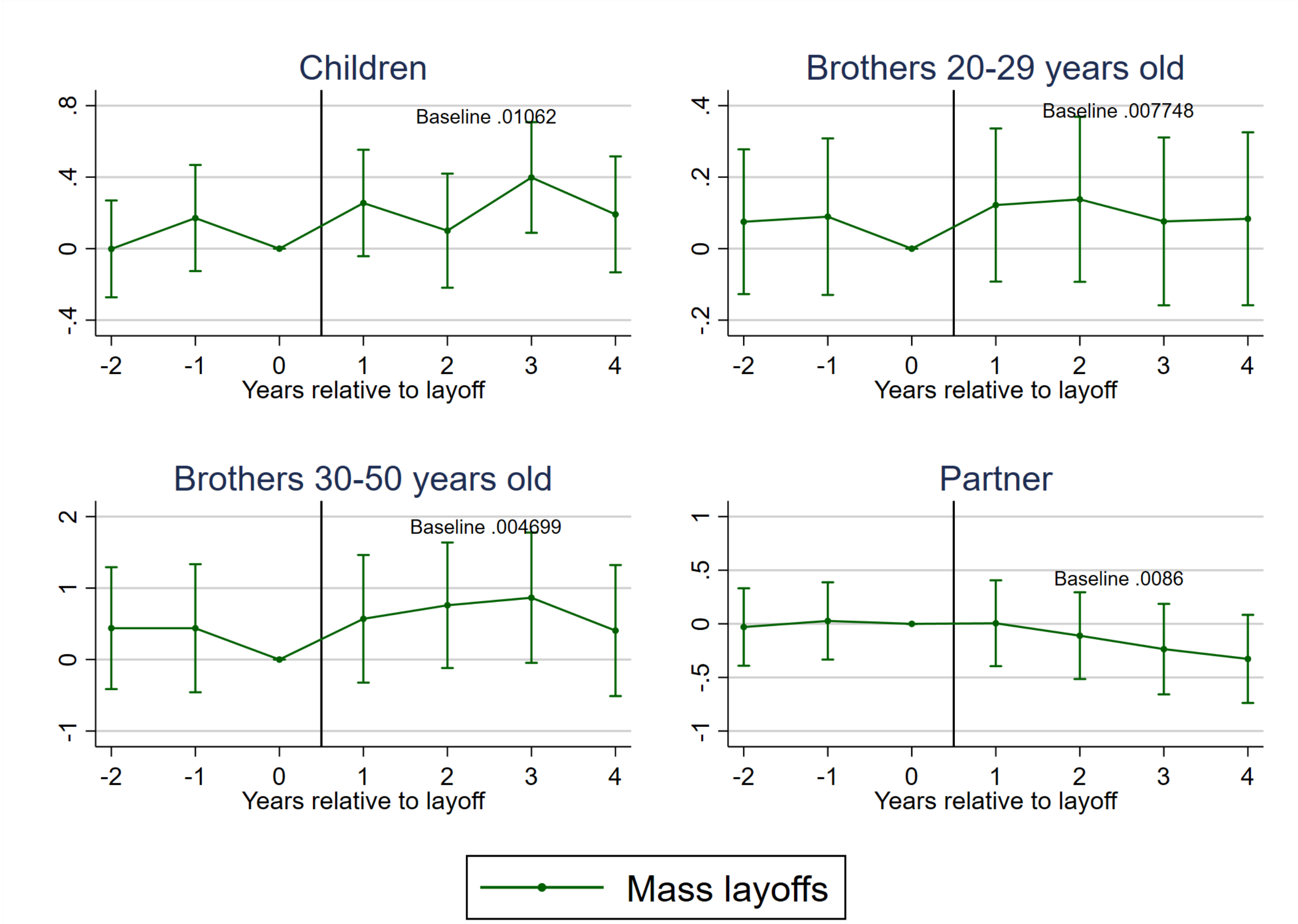
Notes: The top graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime based on equation (1) in an yearly panel covering mass layoffs, along with 95% confidence intervals. The bottom graph reports estimates based on the same model but restricted to displaced workers, i.e. without the control group constructed via matching. All coefficients are rescaled by the baseline average values of each variable for the treated group at $t = 0$ which are also reported.

Figure A9: Effect of job loss on crime, two-way fixed effects panel estimates with correction from de Chaisemartin and D'Haultfoeuille (2019)



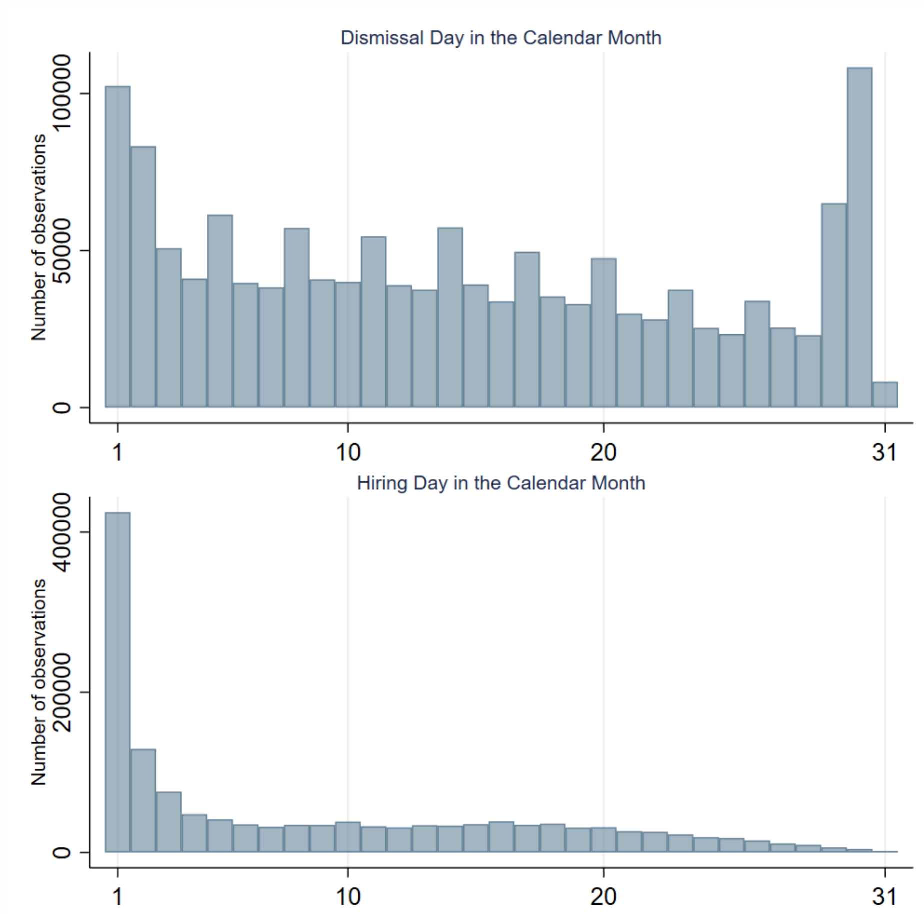
Notes: The top graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime according to the estimator proposed by de Chaisemartin and D'Haultfoeuille (2019), based on an yearly panel covering mass layoffs, along with 95% confidence intervals.

Figure A10: Effect of household members' job loss on crime



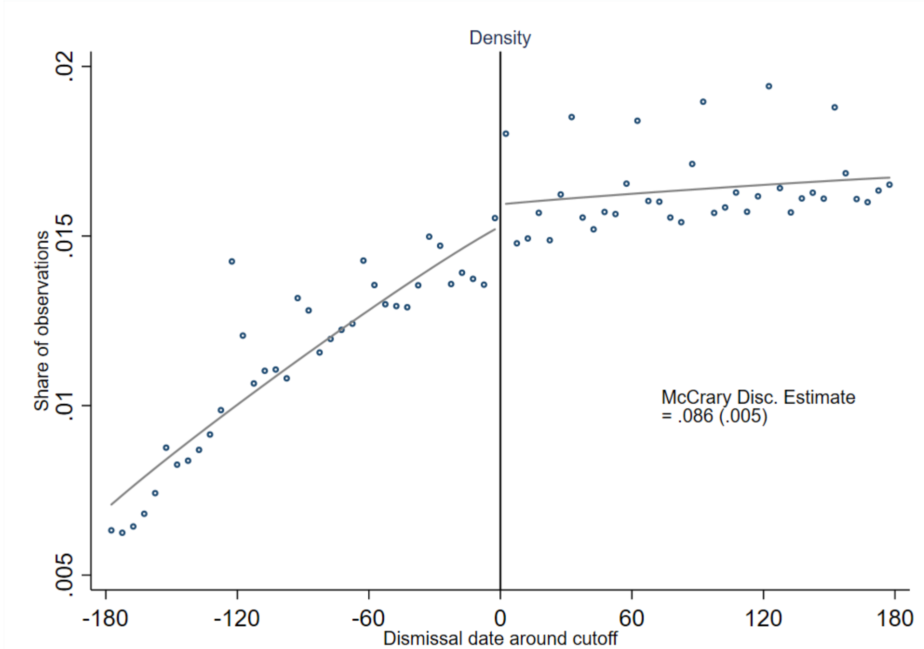
Notes: This table the effect of worker's displacement upon mass layoff on the probability of criminal prosecution for different categories of household members (indicated on top of each graph), as estimated from equation (1). Years relative to layoff are defined relative to the exact date of layoff, i.e., $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Baseline refers to the average value in treatment group including all layoffs at $t = 0$.

Figure A11: Cyclicality in hiring and firing of workers



Notes: The graphs presents the distribution of dismissal and hiring dates by calendar day within each month, based on an initial sample that includes all dismissal dates.

Figure A12: Density of running variable, including all dismissal dates



Notes: The graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

Figure A13: Balance of pre-determined covariates across workers near the cutoff for UI eligibility

Notes: The graphs show the balance of pre-determined covariates around the cutoff for UI eligibility. 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.

Figure A14: Effect of UI eligibility on employment

Notes: The graphs plot different labor market outcomes, indicated on top of each graph, around the cutoff date for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals. Labor income is measured in Brazilian Reais; and unemployment duration is measured in weeks and censored at 36 months, the end of our sample period.

Table A1: Prosecutions by type of offense

	Share of all crimes	"In flagrante" (within crime)
Economically motivated Crimes		
Drug trafficking	10.0%	21.3%
Theft	9.4%	13.7%
Robbery	6.5%	13.3%
Trade of stolen goods	2.8%	6.5%
Fraud	3.7%	1.7%
Corruption	2.5%	0.5%
Others	0.7%	0.2%
Violent crimes		
Assault	7.4%	1.3%
Homicide	3.9%	2.7%
Kidnapping	2.9%	0.8%
Threatening	10.9%	2.3%
Other crimes		
Traffic related	9.7%	11.3%
Slandering	5.6%	0.4%
Illegal gun possession	3.3%	9.1%
Small drug possession	2.4%	0.3%
Fail to obey	2.2%	0.9%
Property damage	1.8%	1.2%
Environmental crime	1.4%	0.2%
Others	2.6%	1.8%

Notes: This table shows the distribution of criminal prosecutions, by type of offense. The first column shows the share of prosecutions for each type of offense across all criminal prosecutions. These shares do not add up to 100% because it is not possible to observe the specific charge for 17% of all cases and because some cases cover multiples charges. The second column shows the share of prosecutions initiated "in flagrante" within all prosecutions for each type of charge.

Table A2: Effect of job loss on different types of crime, all layoffs

Prob. Economically motivated crimes	Drug Trafficking	Theft	Robbery	Trade	Fraud	Corruption
$Treat_i \times Post_t$	0.00027***	0.00010***	0.00018***	0.000083***	0.000038***	0.000046***
	(0.00002)	(0.00001)	(0.00001)	(0.00001)	(0.000009)	(0.000009)
Relative Effect	59%	31%	92%	50%	19%	26%
Mean - Treatment Group	0.0005	0.0003	0.0002	0.0002	0.0002	0.0002

Prob. Violent crimes	Assault	Homicide	Homicides		Kidnapping	Threatening
			Single Offense	Mult. Offenses		
$Treat_i \times Post_t$	0.000071***	0.000078***	0.000067***	0.000011**	0.000039***	0.00019***
	(0.00002)	(0.00001)	(0.000009)	(0.000004)	(0.000008)	(0.00002)
Relative Effect	12%	36%	36%	36%	44%	25%
Mean - Treatment Group	0.0006	0.0002	0.0002	0.00003	0.0001	0.0008

Prob. Other crimes	Traffic Related	Illegal Gun Possession	Slandering	Fail to Obey	Small Drug Possession	Property Damage
$Treat_i \times Post_t$	0.00017***	0.000079***	0.000036**	0.000062***	0.000098***	0.000030***
	(0.00002)	(0.00001)	(0.00001)	(0.000009)	(0.000008)	(0.000008)
Relative Effect	13%	33%	14%	37%	99%	24%
Mean - Treatment Group	0.0013	0.0002	0.0003	0.0002	0.0001	0.0001

Notes: This table shows the effect of job loss on different types of crime (indicated on top of each column), as estimated from the difference-in-differences equation (2). The explanatory variable of main interest is a dummy $Treat_i$ equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. The control group includes workers employed in non-mass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table A3: Summary statistics, workers with and without unique names

	Country-level			Within State		
	Unique	Others	Std Diff	Unique	Others	Std Diff
Demographic characteristics						
Years of education	10.8	10.1	-0.21	10.6	10.1	-0.18
Age	29.9	30.8	0.11	30.0	31.0	0.11
Race - white	51.8%	45.5%	-0.13	49.7%	46.3%	-0.07
Race - black	4.9%	6.6%	0.07	5.2%	7.0%	0.08
Race - brown	34.6%	39.4%	0.10	36.4%	38.5%	0.04
Job characteristics						
Monthly income (R\$)	1,736	1,548	-0.08	1,689	1,546	-0.07
Months worked $t-1$	5.1	5.1	-0.01	5.1	5.1	-0.01
Tenure on Jan 1^{st} (years)	1.8	1.7	-0.01	1.8	1.8	0.00
Manager	6.2%	3.6%	-0.12	5.6%	3.4%	-0.11
Firm size (employees)	510	516	0.00	517	506	-0.01
Local area - municipality						
Large municipality - pop > 1 mil.	34%	35%	0.02	34%	35%	0.02
Municipality population	1,919,447	2,068,497	0.04	1,890,405	2,183,803	0.08
Homicide rate (per 100k inhab.)	29.7	30.5	0.04	30.4	29.7	-0.03
Observations	5,868,151	6,652,131		7,901,613	4,618,669	

Notes: The first three columns report the average characteristics of displaced workers with or without the same name within the country, and the standardized difference between the two groups. The last three columns report the average characteristics of workers with or without the same name within the state, and the standardized difference between the two groups.

Table A4: Share of prosecutions reporting the name of the offender, by state

State	Non-missing share	Obs
Tocantins	92.7%	166,604
Goiás	90.4%	8,405
Paraná	89.3%	476,160
Rondônia	88.2%	15,938
Sergipe	81.3%	166,806
Piau	86.8%	121,567
Bahia	78.4%	510,540
Alagoas	79.2%	118,152
Maranhão	81.0%	183,117
Esprito Santo	80.0%	302,554
Pará	78.3%	100,487
Acre	76.0%	143,704
Roraima	72.1%	15,930
Rio de Janeiro	66.3%	1,521,375
Paraba	62.7%	186,081
Rio Grande do Norte	65.9%	208,702
Amazonas	65.4%	189,620
Mato Grosso do Sul	59.1%	531,998
Santa Catarina	57.4%	906,246
Rio Grande do Sul	63.0%	3,781,713
Amapá	53.8%	63,723
Pernambuco	51.6%	423,933
Ceará	49.6%	239,112
Distrito Federal	43.2%	525,550
São Paulo	29.1%	2,008,080
Minas Gerais	12.9%	1,843,531
Total	53.9%	14,759,628

Table A5: Effect of job loss on crime, including only states with a minimum share of non-missing names in the prosecution records - mass layoffs sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prob. of criminal prosecution	Only states with a share of non-missing names in prosecution records above:						
	0%	20%	30%	50%	60%	70%	80%
$Treat_i \times Post_t$	0.0012***	0.0014***	0.0017***	0.0017***	0.0018***	0.0013***	0.0016***
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Mean outcome at t=0 (treated)	0.0052	0.0057	0.0072	0.0074	0.0076	0.0037	0.0038
Effect relative to the mean	23%	24%	24%	23%	24%	35%	42%
Observations	16,349,844	13,945,064	9,429,070	8,449,672	7,048,958	3,929,716	1,913,380

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

Table A6: Effect of job loss on crime, robustness to including all workers with a unique name within their state of residence

	(1)	(2)	(3)	(4)
Prob. prosecution for:	Any crime	Economic	Violent	Other
PANEL A: ALL DISPLACED WORKERS				
$Treat_i \times Post_t$	0.00116***	0.000548***	0.000168***	0.000376***
	(0.0000448)	(0.0000251)	(0.0000177)	(0.0000286)
Mean outcome, treated at t=0	0.0054	0.0015	0.0009	0.0025
Effect relative to the mean	22%	38%	19%	15%
Observations	93,673,818	93,673,818	93,673,818	93,673,818
PANEL B: DISPLACED IN MASS LAYOFFS				
$Treat_i \times Post_t$	0.000740***	0.000368***	0.000147***	0.000166**
	(0.0000967)	(0.0000545)	(0.0000385)	(0.0000594)
Mean outcome, treated at t=0	0.0047	0.0014	0.0008	0.0021
Effect relative to the mean	16%	27%	19%	8%
Observations	23,719,920	23,719,920	23,719,920	23,719,920
PANEL C: DISPLACED IN MASS LAYOFFS - MUN X IND X YEAR FIXED-EFFECTS				
$Treat_i \times Post_t$	0.000852***	0.000427***	0.000151***	0.000243***
	(0.0000942)	(0.0000556)	(0.0000381)	(0.0000582)
Mean outcome, treated at t=0	0.0047	0.0014	0.0008	0.0021
Effect relative to the mean	18%	31%	19%	12%
Observations	23,618,581	23,618,581	23,618,581	23,618,581

Notes: This table shows the effect of job loss on the probability of criminal prosecution for different types of crime, as estimated from the difference-in-differences equation (2). The sample includes all workers with a unique name within their state of residence – rather than in the whole country, as in the sample used for the main analysis. The dependent variable is indicated on top of each column. The explanatory variable of main interest is a dummy $Treat_i$ that is equal to 1 for displaced workers, interacted with a dummy $Post_t$ equal to 1 for the period after displacement. Panel A includes in the sample all displaced workers and matched control workers employed in non-mass layoff firms who are not displaced in the same calendar year; Panel B restricts the treated group to workers who are displaced in mass layoffs; and finally, Panel C adds municipality × industry × 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 and the percent change in earnings. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1).

Table A7: Effect of UI eligibility on crime in the first semester after layoff, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prob. crim. prosecution	Total	Age		Education		Income	
1 semester after layoff	sample	≤ 29	>30	< 12 years	≥ 12 years	≤ R$1035	> R$1035
PANEL A. POLYNOMIAL ORDER 0, BANDWIDTH 30 DAYS							
Eligibility for UI benefits	-0.00043	-0.00106***	0.00019	-0.00080*	-0.00015	-0.00075*	-0.00012
	(0.00027)	(0.00041)	(0.00035)	(0.00045)	(0.00033)	(0.00041)	(0.00035)
Effect relative to the mean	-12.4%	-25.4%	6.8%	-19.8%	-4.9%	-18.0%	-4.3%
Observations	183,625	91,143	92,482	80,230	103,395	91,259	92,366
PANEL B. POLYNOMIAL ORDER 0, BANDWIDTH 45 DAYS							
Eligibility for UI benefits	-0.00041*	-0.00111***	0.00027	-0.00057	-0.0003	-0.00072**	-0.00013
	(0.00022)	(0.00034)	(0.00029)	(0.00037)	(0.00027)	(0.00034)	(0.00029)
Effect relative to the mean	-11.8%	-26.6%	9.7%	-14.1%	-9.8%	-17.3%	-4.6%
Observations	273,575	135,765	137,810	119,443	154,132	135,926	137,649
PANEL C. POLYNOMIAL ORDER 0, BANDWIDTH CCT							
Eligibility for UI benefits	-0.00058*	-0.00099**	0.00016	-0.00065	-0.00017	-0.00073*	-0.00007
	(0.0003)	(0.00048)	(0.00034)	(0.00047)	(0.00032)	(0.00044)	(0.00036)
Effect relative to the mean	-16.7%	-23.7%	5.7%	-16.1%	-5.6%	-17.5%	-2.5%
Observations	155,467	66,867	119,826	72,615	113,129	78,168	86,119
PANEL D. POLYNOMIAL ORDER 1, BANDWIDTH 60 DAYS							
Eligibility for UI benefits	-0.00065*	-0.00158***	0.00025	-0.00119*	-0.00026	-0.00105*	-0.00028
	(0.00038)	(0.00057)	(0.00049)	(0.00063)	(0.00046)	(0.00058)	(0.00048)
Effect relative to the mean	-18.7%	-37.8%	8.9%	-29.5%	-8.5%	-25.2%	-9.9%
Observations	362,631	179,875	182,756	158,158	204,473	180,161	182,470
PANEL E. POLYNOMIAL ORDER 1, BANDWIDTH 90 DAYS							
Eligibility for UI benefits	-0.0005	-0.00121**	0.00019	-0.00097*	-0.00015	-0.00118**	0.00015
	(0.00031)	(0.00047)	(0.00041)	(0.00052)	(0.00038)	(0.00048)	(0.0004)
Effect relative to the mean	-14.4%	-29.0%	6.8%	-24.0%	-4.9%	-28.4%	5.3%
Observations	536,301	265,977	270,324	234,399	301,902	265,605	270,696
PANEL F. POLYNOMIAL ORDER 1, BANDWIDTH 120 DAYS							
Eligibility for UI benefits	-0.00044	-0.00105**	0.00016	-0.00089**	-0.00011	-0.00093**	0.00003
	(0.00027)	(0.00041)	(0.00036)	(0.00045)	(0.00034)	(0.00042)	(0.00036)
Effect relative to the mean	-12.6%	-25.1%	5.7%	-22.0%	-3.6%	-22.3%	1.1%
Observations	704,571	349,108	355,463	308,471	396,100	347,111	357,460
PANEL G. POLYNOMIAL ORDER 1, CCT							
Eligibility for UI benefits	-0.00044	-0.00140**	0.00033	-0.00112*	0.00011	-0.00119**	0.00038
	(0.00041)	(0.00058)	(0.00044)	(0.00058)	(0.00046)	(0.00057)	(0.00045)
Effect relative to the mean	-12.6%	-33.5%	11.8%	-27.7%	3.6%	-28.6%	13.5%
Observations	313,029	178,176	273,378	184,851	228,491	187,102	229,411
PANEL H. POLYNOMIAL ORDER 2, BANDWIDTH 180 DAYS							
Eligibility for UI benefits	-0.00057*	-0.00151***	0.00036	-0.00112**	-0.00017	-0.00134***	0.00013
	(0.00033)	(0.00051)	(0.00043)	(0.00055)	(0.00041)	(0.00051)	(0.00043)
Effect relative to the mean	-16.4%	-36.2%	12.9%	-27.7%	-5.6%	-32.2%	4.6%
Observations	1,004,910	495,474	509,436	440,044	564,866	488,718	516,192
Mean outcome at the cutoff	0.0035	0.0042	0.0028	0.0040	0.0031	0.0042	0.0028

Notes: This table replicates the regression discontinuity analysis in Table 8 for different specifications of the polynomial regression and different bandwidths (indicated on top of each panel).

Table A8: Effect of UI eligibility on crime in the first semester after layoff, robustness to cyclicality in hiring and firing of workers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prob. crim. prosecution	Total	Age		Education		Income	
1 semester after layoff	sample	$\leq$ 29	>30	< 12 years	$\geq$ 12 years	$\leq$ R$1035	> R$1035
PANEL A. MAIN SAMPLE, BASELINE							
Eligibility for UI benefits	-0.00065*	-0.00158***	0.00025	-0.00119*	-0.00026	-0.00105*	-0.00028
	(0.00038)	(0.00057)	(0.00049)	(0.00063)	(0.00046)	(0.00058)	(0.00048)
Effect relative to the mean	-19.3%	-39.8%	9.0%	-30.6%	-9.1%	-26.5%	-9.7%
Observations	362,631	179,875	182,756	158,158	204,473	180,161	182,470
PANEL B. MAIN SAMPLE, DISMISSAL DATE FE							
Eligibility for UI benefits	-0.00063*	-0.00154***	0.00031	-0.00123*	-0.0002	-0.00097*	-0.00031
	(0.00038)	(0.00058)	(0.0005)	(0.00064)	(0.00046)	(0.00058)	(0.00049)
Effect relative to the mean	-18.7%	-38.8%	11.1%	-31.6%	-7.0%	-24.4%	-10.8%
Observations	362,631	179,874	182,756	158,158	204,473	180,161	182,470
PANEL C. MAIN SAMPLE, CUTOFF DATE FE							
Eligibility for UI benefits	-0.00064*	-0.00154***	0.00025	-0.00122*	-0.00033	-0.00099*	-0.00029
	(0.00038)	(0.00058)	(0.0005)	(0.00063)	(0.00047)	(0.00058)	(0.00049)
Effect relative to the mean	-19.0%	-38.8%	9.0%	-31.4%	-11.5%	-24.9%	-10.1%
Observations	362,629	179,874	182,753	158,153	204,468	180,157	182,465
PANEL D. MAIN SAMPLE, DISMISSAL AND CUTOFF DATE FIXED-EFFECTS							
Eligibility for UI benefits	-0.00063*	-0.00155***	0.00031	-0.00124*	-0.00029	-0.00093	-0.00034
	(0.00038)	(0.00059)	(0.00051)	(0.00064)	(0.00047)	(0.00059)	(0.0005)
Effect relative to the mean	-18.7%	-39.1%	11.1%	-31.9%	-10.1%	-23.4%	-11.8%
Observations	362,629	179,873	182,753	158,153	204,468	180,157	182,465
PANEL E. UNRESTRICTED SAMPLE							
Eligibility for UI benefits	-0.00053*	-0.00117**	0.00009	-0.00066	-0.00043	-0.00063	-0.00042
	(0.00031)	(0.00048)	(0.0004)	(0.00051)	(0.00038)	(0.00046)	(0.00041)
Effect relative to the mean	-15.7%	-29.5%	3.2%	-17.0%	-15.0%	-15.9%	-14.6%
Observations	502,560	247,922	254,638	217,464	285,096	252,421	250,139
PANEL F. UNRESTRICTED SAMPLE, DISMISSAL AND CUTOFF DATE FIXED-EFFECTS							
Eligibility for UI benefits	-0.00056*	-0.00124**	0.00015	-0.00076	-0.00053	-0.0006	-0.00057
	(0.00031)	(0.00048)	(0.00041)	(0.00052)	(0.00039)	(0.00047)	(0.00042)
Effect relative to the mean	-16.6%	-31.3%	5.4%	-19.5%	-18.5%	-15.1%	-19.8%
Observations	502,558	247,921	254,635	217,459	285,091	252,417	250,134
Mean outcome at the cutoff:							
Main sample	0.0035	0.0042	0.0027	0.0040	0.0029	0.0042	0.0029
Unrestricted sample	0.0034	0.0040	0.0028	0.0039	0.0029	0.0040	0.0029

Notes: This table replicates the regression discontinuity analysis in Table 8 when including fixed effects for dismissal and cutoff dates, and when including all dismissal and cutoff dates within each month (Panels E and F). The sample and specification are indicated on top of each panel.

Table A9: Effect of UI eligibility on crime in the first semester after layoff, extended sample including all workers with a unique name within their state of residence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Prob. crim. prosecution	Total	Age		Education		Income	
1 semester after layoff	sample	≤ 29	>30	< 12 years	≥ 12 years	≤ R$1035	> R$1035
PANEL A. POLYNOMIAL ORDER 0, BANDWIDTH 30 DAYS							
Eligibility for UI benefits	-0.00041*	-0.00115***	0.0003	-0.00064*	-0.00022	-0.00098***	0.00016
	(0.00022)	(0.00034)	(0.00028)	(0.00035)	(0.00028)	(0.00034)	(0.00028)
Effect relative to the mean	-12.7%	-29.8%	11.4%	-18.0%	-7.4%	-24.2%	6.7%
Observations	250,166	121,831	128,335	114,107	136,059	125,566	124,600
PANEL B. POLYNOMIAL ORDER 0, BANDWIDTH 45 DAYS							
Eligibility for UI benefits	-0.00033*	-0.00088***	0.00021	-0.00027	-0.00038*	-0.00078***	0.00011
	(0.00018)	(0.00028)	(0.00023)	(0.00029)	(0.00023)	(0.00028)	(0.00023)
Effect relative to the mean	-10.2%	-22.8%	8.0%	-7.6%	-12.9%	-19.2%	4.6%
Observations	372,789	181,514	191,275	170,119	202,670	186,999	185,790
PANEL C. POLYNOMIAL ORDER 0, BANDWIDTH CCT							
Eligibility for UI benefits	-0.00048**	-0.00107**	0.00051*	-0.00058	-0.00023	-0.00088**	0.00039
	(0.00023)	(0.00042)	(0.00031)	(0.00037)	(0.00028)	(0.00036)	(0.00033)
Effect relative to the mean	-14.9%	-27.8%	19.4%	-16.3%	-7.8%	-21.7%	16.2%
Observations	263,124	78,613	156,902	100,826	135,408	113,904	82,568
PANEL D. POLYNOMIAL ORDER 1, BANDWIDTH 60 DAYS							
Eligibility for UI benefits	-0.00060*	-0.00173***	0.00047	-0.00107**	-0.00021	-0.00134***	0.00013
	(0.00031)	(0.00048)	(0.0004)	(0.00049)	(0.0004)	(0.00049)	(0.00039)
Effect relative to the mean	-18.6%	-44.9%	17.9%	-30.1%	-7.1%	-33.0%	5.4%
Observations	494,277	240,483	253,794	225,376	268,901	247,908	246,369
PANEL E. POLYNOMIAL ORDER 1, BANDWIDTH 90 DAYS							
Eligibility for UI benefits	-0.00043*	-0.00112***	0.00024	-0.00078*	-0.00014	-0.00125***	0.00038
	(0.00026)	(0.0004)	(0.00033)	(0.00041)	(0.00033)	(0.0004)	(0.00033)
Effect relative to the mean	-13.3%	-29.0%	9.1%	-21.9%	-4.7%	-30.8%	15.8%
Observations	730,666	355,236	375,430	333,531	397,135	365,433	365,233
PANEL F. POLYNOMIAL ORDER 1, BANDWIDTH 120 DAYS							
Eligibility for UI benefits	-0.00038*	-0.00093***	0.00015	-0.00065*	-0.00015	-0.00106***	0.00028
	(0.00022)	(0.00035)	(0.00029)	(0.00035)	(0.00029)	(0.00035)	(0.00028)
Effect relative to the mean	-11.8%	-24.1%	5.7%	-18.3%	-5.1%	-26.1%	11.6%
Observations	960,226	466,342	493,884	438,965	521,261	477,537	482,689
PANEL G. POLYNOMIAL ORDER 1, CCT							
Eligibility for UI benefits	-0.00066**	-0.00154***	0.00032	-0.00083*	-0.00002	-0.00129***	0.00035
	(0.00031)	(0.00046)	(0.0004)	(0.00047)	(0.00039)	(0.00046)	(0.00037)
Effect relative to the mean	-20.4%	-39.9%	12.2%	-23.3%	-0.7%	-31.8%	14.6%
Observations	501,335	255,737	268,814	238,966	327,044	266,723	286,300
PANEL H. POLYNOMIAL ORDER 2, BANDWIDTH 180 DAYS							
Eligibility for UI benefits	-0.00056**	-0.00155***	0.00039	-0.00108**	-0.00013	-0.00146***	0.0003
	(0.00028)	(0.00043)	(0.00035)	(0.00043)	(0.00035)	(0.00043)	(0.00035)
Effect relative to the mean	-17.3%	-40.2%	14.8%	-30.4%	-4.4%	-36.0%	12.5%
Observations	1,369,296	661,933	707,363	626,336	742,960	672,789	696,507
Mean outcome at the cutoff	0.0032	0.0039	0.0026	0.0036	0.0030	0.0041	0.0024

Notes: This table replicates the regression discontinuity analysis in Table 8 for different specifications of the polynomial regression and different bandwidths (indicated on top of each panel). The sample includes all individuals with a unique name within the state – rather than in the whole country, as in the sample used for the main analysis.

Table A10: Effect of UI eligibility on employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total	Age		Education		Income	
	sample	$\leq$ 29	>30	< 12 years	$\geq$ 12 years	$\leq$ R$1035	> R$1035
PANEL A. DEPENDENT VARIABLE: MONTHS WORKED SEMESTER 1							
Eligibility for UI benefits	-0.77***	-0.79***	-0.75***	-0.88***	-0.68***	-0.85***	-0.68***
	(0.016)	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)
Mean outcome at the cutoff	2.1460	2.1463	2.1458	2.0799	2.1967	2.1009	2.1899
Effect relative to the mean	-35.9%	-36.8%	-35.0%	-42.3%	-31.0%	-40.5%	-31.1%
PANEL B. DEPENDENT VARIABLE: INCOME SEMESTER 1							
Eligibility for UI benefits	-990.4***	-897.4***	-1077.8***	-1064.4***	-920.7***	-825.3***	-1122.4***
	(38.6)	(39.2)	(66.1)	(38.4)	(61.5)	(26.9)	(70.8)
Mean outcome at the cutoff	3205.3410	2754.0010	3651.1390	2684.1330	3604.4910	2142.5950	4238.3450
Effect relative to the mean	-30.9%	-32.6%	-29.5%	-39.7%	-25.5%	-38.5%	-26.5%
PANEL C. DEPENDENT VARIABLE: EMPLOYMENT SEMESTER 1							
Eligibility for UI benefits	-0.13***	-0.14***	-0.13***	-0.15***	-0.12***	-0.15***	-0.11***
	(0.0033)	(0.0046)	(0.0046)	(0.0049)	(0.0043)	(0.0046)	(0.0046)
Mean outcome at the cutoff	0.6286	0.6297	0.6275	0.6301	0.6275	0.6175	0.6394
Effect relative to the mean	-20.7%	-22.2%	-20.7%	-23.8%	-19.1%	-24.3%	-17.2%
PANEL D. DEPENDENT VARIABLE: UNEMPLOYMENT DURATION (WEEKS)							
Eligibility for UI benefits	6.54***	6.89***	6.20***	7.65***	5.68***	7.64***	5.39***
	(0.3)	(0.4)	(0.44)	(0.44)	(0.4)	(0.43)	(0.41)
Mean outcome at the cutoff	34.7046	33.4301	35.9634	34.0539	35.2029	35.6697	33.7665
Effect relative to the mean	18.8%	20.6%	17.2%	22.5%	16.1%	21.4%	16.0%
Bandwidth	60	60	60	60	60	60	60
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Observations	362,631	179,875	182,756	158,158	204,473	180,161	182,470

Notes: This table shows the effect of eligibility for UI benefits on different labor market outcomes, as estimated from equation (3). The dependent variable is indicated on top of each panel. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits – namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Columns (2) to (7) estimate separate regressions for the different groups indicated on top of each column. The table also reports the baseline mean outcome at the cutoff and the percent effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses (*** p≤0.01, ** p≤0.05, * p≤0.1). Labor income is measured in Brazilian Reais; and unemployment duration is censored at 36 months, the end of our sample period.