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

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

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Abstract

We investigate the effect of job loss and unemployment benefits on crime, exploiting unique individual-level data on the universe of workers and criminal cases in Brazil over the 2009-2017 period. We find that the probability of criminal prosecution increases on average by 23% for workers displaced upon mass layoffs, and by slightly less for their cohabiting sons. Using causal forests, we show that the effect is driven entirely by young and low tenure workers, while there is no heterogeneity by education and income. Regression discontinuity estimates indicate that unemployment benefit eligibility completely offsets potential crime increases upon job loss, but this effect completely vanishes immediately after 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

Key words: 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 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.

In this paper, we study the relationship between employment and criminal behavior exploiting detailed registry data on 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 registries of unemployment benefits and other social transfers allowing us, in addition, to recover the household composition for almost half of our sample. We exploit these data to estimate the effect of job loss on the probability of committing (different types of) crime, the spillover effects on other household members, and the mitigating effect of unemployment insurance schemes. The size and richness of our dataset also allows us to characterize the heterogeneity of treatment effects across individuals and their timing around the payment and exhaustion of unemployment benefits. The results of this analysis allow us to gain novel insights into the mechanisms driving the response of criminal behavior to job loss.

In the first part of our analysis, we estimate dynamic treatment effects of job displacement by comparing the criminal behavior of workers displaced upon mass layoffs, before and after displacement, with a matched control group of workers who were not displaced in the same year. Mass layoffs should depend neither on the criminal behavior of each specific worker nor on other individual-level shocks that

simultaneously affect employment and crime; for this reason, they have been widely used as a source of exogenous variation to estimate the effects of job loss on several outcomes, such as subsequent earnings (Couch and Placzek, 2010; Jacobson et al., 1993) and mortality (Sullivan and Von Wachter, 2009). In addition, 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.

Our findings indicate that the probability of committing a crime increases by 23% over the baseline for displaced workers in the year after dismissal compared to the control group, 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 spills over to the cohabiting sons of displaced workers (+18%).

The estimated effect of job loss is unaffected when replicating the analysis at the monthly level and restricting to offenders arrested “in flagrante” (i.e., while committing a crime). Focusing on this group should reduce the possibility of measurement error from differential reporting and delays in judicial prosecution 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.

We then estimate Conditional Average Treatment Effects across individuals using causal forest algorithms (Athey and Imbens, 2016; Athey et al., 2019; Wager and Athey, 2018). Predicted treatment effects do *not* significantly vary with worker earnings and education, nor they vary with local-level variables such as homicide rates and labor informality.¹ This is a remarkable result, given the wide heterogeneity in local socio-economic conditions across the vast Brazilian territory. Instead, the effect is driven mainly by young and low-tenure workers. This last finding suggests that binding liquidity constraints may be an important explanation for the effect of job loss on crime, as young and low-tenure workers typically have

¹Our main data tracks formal employment. Unless when stated otherwise, we use *income* and *employment* to refer to *formal income* and *formal employment* throughout the paper. In Section 2.2 and in Appendix B.2, we quantify the empirical relevance of transitions to informal work after job loss and discuss their implications for interpreting our results.

lower accumulated savings and are entitled to less generous severance pay and unemployment benefits. At the same time, such group differs from other workers in many dimensions – notably, baseline crime rates.

To shed more light on mechanisms and, in addition, understand the effectiveness of alternative policy remedies, we then examine 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.²

We find that the crime rate in the first semester after layoff is 21% lower for marginally eligible workers compared with marginally non-eligible ones. The average effect of unemployment benefits completely offsets the potential increase in crime upon job loss. However, this effect is transitory and vanishes away immediately after benefits expire.

These results suggest that UI policies may attenuate the impact of job loss on crime. They also help us distinguishing between different mechanisms driving the effect of unemployment on crime. The negative effect of UI on labor supply implies that eligible workers take longer to find a new job than ineligible ones – as in, e.g., [Katz and Meyer \(1990\)](#) and [Lalive \(2008\)](#). Therefore, lower crime rates by eligible workers cannot be attributed to substitution between leisure time and time spent on the job (i.e., what we previously called the incapacitation effect of employment): if time substitution were the main driver of the effect, in fact, 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

²[Gerard et al. \(2020\)](#) exploit the same research design with data from earlier years to study the effect UI eligibility effects on unemployment duration. They detect a potential violation of quasi-random assignment, as the density of the assignment variable is mildly discontinuous around the 16-month cutoff, but this issue is not present in our sample period. We also show that crime rates before layoff are continuous around the threshold, which strongly supports the validity of the design.

constraints.³ The existence of such constraints is consistent with the strong but transitory effect of unemployment benefits. To bolster this explanation we show, in addition, that crime increases immediately after the expiration of benefit payments. This finding mirrors previous evidence on liquidity constraints among displaced Brazilian workers, who experience sudden consumption drops upon benefit exhaustion (Gerard and Naritomi, 2021). The spillover effect on cohabiting sons is also consistent with the importance of liquidity constraints and inconsistent with time substitution. In addition, the spillover effect cannot be 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 such as 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, Dell et al., 2019; Dix-Carneiro et al., 2018; Fougère et al., 2009; Gould et al., 2002; Öster and Agell, 2007; Raphael and Winter-Ebmer, 2001). 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 as 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, so 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 much rarer than petty property crimes.

³Foley (2011) provides evidence on the importance of liquidity constraints for criminal behavior using aggregate data on welfare payments.

The present paper addresses these issues by leveraging on individual-level administrative data and recent advancements in econometric methods. It is closest 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., 2020).⁴

Our work advances this literature in several ways. First, this is the first analysis covering the universe of workers in a large developing country, characterized by very high levels of crime. Therefore, our findings are informative about the effect of job loss on crime in a context where the latter is a major social problem. Second, the size and richness of our dataset, coupled with recent advancements in causal forest algorithms for the analysis of heterogeneous treatment effects, allow us to precisely estimate the full distribution of treatment effects conditional on a wide array of individual and local characteristics. These results shed light on the prominent role played by individual characteristics, notably age and tenure, over local socio-economic characteristics – even in a country characterized by extreme variation across geographical areas. Third, the peculiar features of the UI system provide us with a very clean research design for identifying the effect of unemployment benefits. Our results differ from those of Bennett and Ouazad (2019), who find no impact of UI on crime in Denmark – at least when such transfers are unconditional on training and job search, as it is the case in Brazil. These differences confirm the importance of estimating the effects of such policies in a country characterized by much lower income levels and higher crime rates than the countries for which similar data are typically available – notably, European countries and US states. The results we find for treatment effect heterogeneity, spillover effects, and responses to UI benefits allow us to provide cleaner evidence on mechanisms, highlighting the prominent role played by liquidity constraints and psychological stress.

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. Sections 4 and 5 present the main results regarding,

⁴Prior 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.

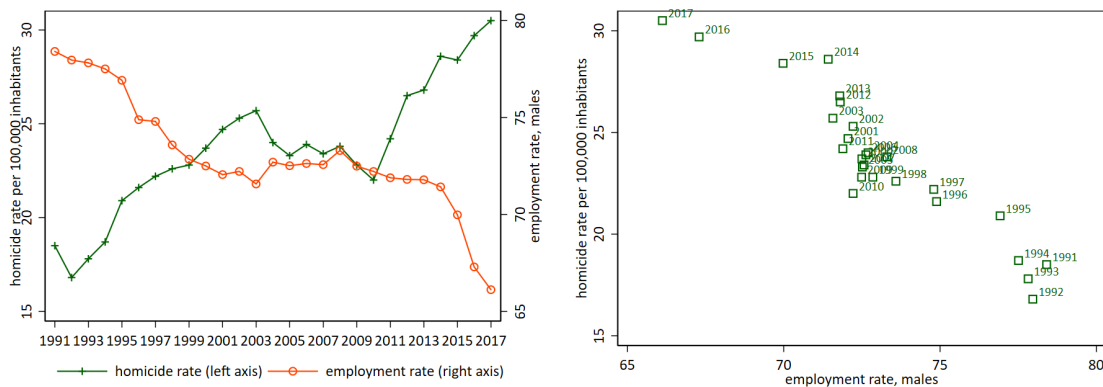
respectively, the impact of job loss and UI on crime; additional robustness checks are contained in Appendices B and C. Section 6 discusses the mechanisms driving the results, while Section 7 concludes.

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, ranking 82nd out of 182 countries in terms of GDP per capita in 2018.

Over time, the homicide rate slowly increased from 18 to 23 per 100,000 inhabitants between 1990 and 2010, before abruptly increasing during the following years. Interestingly, during the later 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.

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.

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 relationship, we thus exploit mass layoff shocks and compare criminal prosecutions over time between displaced and non-displaced workers, and 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 judicial system comprises 27 state courts and 2,697 tribunals, and each tribunal has jurisdiction over one or more of the 5,570 Brazilian municipalities. Criminal investigations are conducted by state judiciary police, either by its own initiative or upon request from the public prosecutor office or crime victims. Once an investigation is concluded, files are sent to the prosecutor office, which decides whether to press or drop the charges. 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.

2.2 Labor Regulation

Brazilian labor legislation is based on at-will employment, whereby firms are free to dismiss workers without a just cause by paying dismissal indemnities, and 93% of all contracts in the private sector are open-ended and full-time. Dismissals without a just cause account for two thirds of job separations, while the remaining third are voluntary quits. Our analysis focuses on the former, which we refer to as “dismissals” or “layoffs” throughout the paper. Dismissed workers are entitled to a mandatory savings account, financed through employer’s monthly contributions amounting to 8% of the worker’s compensation. Workers can only access these funds in case of

a dismissal without just cause, which further entitles them 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.

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. 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 estimates – particularly, the estimated elasticity of crime to (formal) income.

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”, a 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, type of contract, occupation and

sectoral code, and worker’s education and earnings.⁵ 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 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 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.⁶ 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 on a total of 14.5 million, due to imprecisions in the data input process from court diaries or 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

⁵The 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).

⁶Setting 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 insofar as 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.

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, 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 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, prosecution starts immediately for offenders arrested “in flagrante”, because a judge must decide whether to detain 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). In the empirical analysis, we will try to identify instrumental homicides as those reported together with other offenses. 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.⁷

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.⁸ 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* (CadÚnico), a dataset maintained by the Ministry of Development for the administration of all federal social programs.

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. Subsequently, we restrict attention to individuals who have a unique name in the country and merge the court data to the employment records by exactly matching on names. Columns (1)-(3) of Appendix Table A2 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 exhibit 6% more years of education, 12% higher earnings, 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. We will show that our main findings remain robust when including all individuals whose name is unique in the state where they work (rather than in the entire country), in which case the coverage of the country population increases to 70% (columns 4-6 of Table A2).

⁷Table A1 in the Appendix reports the share of each crime category among all offenses and among crimes committed “in flagrante”.

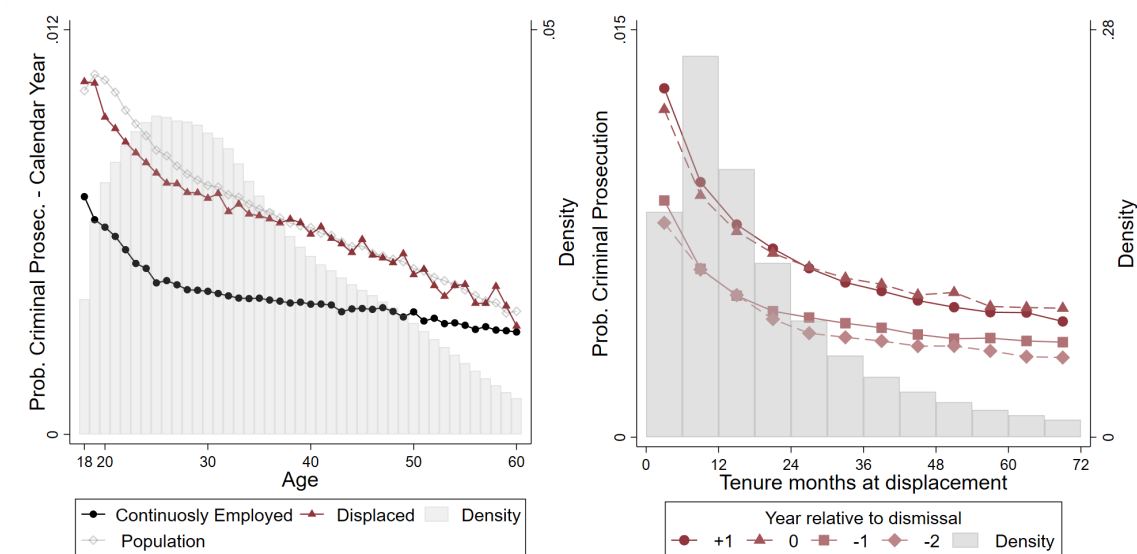
⁸Throughout the paper, we refer to “name” as the person’s full name, i.e. the name-surname(s) combination.

3.3 Descriptive evidence

Figure 2 shows how the average probability of criminal prosecution varies by employment status, age, and tenure. 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 left 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 high prosecution rates for displaced individuals are comparable to that in the broad population – which includes individuals who left formal jobs in the previous calendar years, who work informally, and who are out of the labor force – as shown by the gray line.

The right graph in Figure 2 focuses on crime outcomes of displaced workers two years before and after the job loss, conditional on job tenure. The density function shows that labor turnover is extremely 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.

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



Notes: The left graph compares the average probability of being prosecuted in a given year between workers who are continuously employed, workers losing their job in that year, and individuals in the population irrespective of their employment status, by age. The right graph shows 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. The distributions of age and tenure for employed individuals are also shown in the graphs.

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

4 The effect of job loss 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.⁹ The pool of candidate control workers comprises all individuals employed in firms that did not experience mass layoffs during our period of analysis.¹⁰ 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.¹¹ When treated workers are matched with multiple controls, one control unit is randomly selected.¹²

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.¹³

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

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

⁹Given 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.

¹⁰Our definition of mass layoffs is presented shortly below.

¹¹We aggregate industrial sectors into 9 categories: agriculture, mining, manufacturing, construction, sales, services, education, public services, and tourism.

¹²In 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 either of these two approaches; for instance, [Ichino et al. \(2017\)](#) and [Schmieder et al. \(2018\)](#) define the control group similarly to our baseline setting, while [Jacobson et al. \(1993\)](#) and [Couch and Placzek \(2010\)](#) restrict to workers who are continuously employed through the whole period.

¹³See [Borusyak and Jaravel \(2017\)](#), [Abraham and Sun \(2018\)](#), [Athey and Imbens \(2018\)](#), [Goodman-Bacon \(2018\)](#), [De Chaisemartin and D’Haultfoeuille \(2020\)](#), [Callaway and Sant’Anna \(2019\)](#) and [Imai and Kim \(2019\)](#).

Workers are identified by the subscript i , and $Treat_i$ is a dummy indicating that the worker belongs to the treatment group. $Time_t$'s are dummies identifying 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 $\{\delta_1, \dots, \delta_T\}$ thus identify dynamic treatment effects, whereas $t = 0$ is the baseline omitted period and $\{\delta_{-P}, \dots, \delta_{-1}\}$ estimate anticipation effects.¹⁴ Finally, $Time_t$ fixed-effects absorb time-varying shocks. As a robustness check, we allow for time-varying 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. To summarize the average treatment effect over all periods, we also estimate the equation:

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

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.¹⁵ These layoffs typically depend on negative external shocks at the firm level,

¹⁴Monthly-level estimates are presented as a robustness exercise.

¹⁵We 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 1st of the following year.

rather than the characteristics and behavior of dismissed workers (see e.g. [Gathmann et al., 2020](#)). As we will show, our findings are extremely robust to a wide range of stricter mass layoffs definitions, both in terms of the minimum share of displaced workers and firm size.

Appendix Table [B1](#) 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 right panel of [Figure 2](#)). Although the difference-in-differences design only requires that treatment and control groups follow the same trends in the absence of treatment, which is consistent with our evidence of parallel trends in the pre-treatment period, one could worry that the control group does not provide an adequate counterfactual in light of the level gap. In [Appendix B.3](#), we address this concern by showing that our results are stable under alternative definitions of treated and control groups for which this gap essentially vanishes.

Finally, [Appendix Table B2](#) compares the characteristics of individuals in the treatment group by criminal status, before and after displacement. Interestingly, individuals who select into crime before and after displacement are not strongly different along numerous characteristics such as education, age and income. One exception is that criminally prosecuted workers tend to have lower tenure, the only characteristic for which the standardized difference is above the 0.2 critical value. Later, we will show that the job loss effect strongly correlates with tenure at displacement.

4.2 Main results

Figure 3 shows the impact of job loss on labor market outcomes (panels a-b) and criminal behavior (panels c-f), as estimated from equation (1). Treatment effects are rescaled by the respective baseline outcome in the treatment group at time $t = 0$.

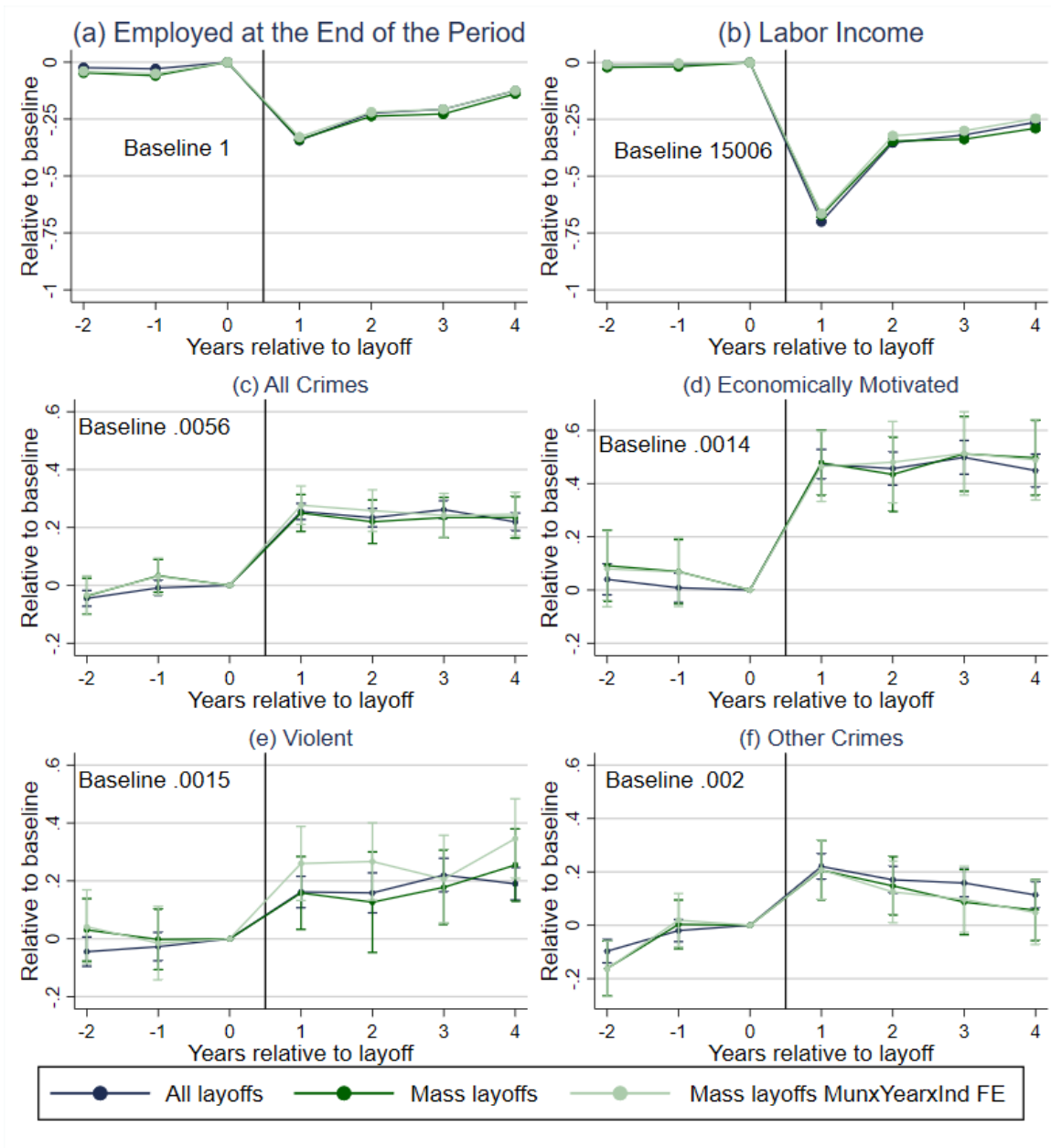
In all graphs, the difference in outcomes between treatment and control groups is stable in the pre-displacement period, supporting the common-trend assumption. After job loss, employment and income decline by 34% and 70%, respectively, for displaced workers relative to the matched control group. These gaps close down very slowly over the following years, as four years after dismissal treated workers still experience 13% lower employment rates and 26% lower labor income.¹⁶

Panels (c)-(f) of Figure 3 show that the drop in employment and earnings drives a sharp increase in the probability of committing (different types of) criminal offenses. These graphs also show that the estimated dynamic treatment effects are unaffected when extending the sample to all layoffs – as opposed to including only workers displaced in mass layoffs – and when controlling for municipality-year-industry fixed effects, thus absorbing time-varying shocks at a very granular level of geographic and sectoral disaggregation. In Appendix Figure B7, we additionally show that both the incidence of first-time offenders and the probability of re-offending increase upon job loss. While the impact on first-time prosecution peaks immediately in year 1, to slowly decrease in the following years, the effect on repeated offenses becomes stronger over time. Overall, these patterns suggest that job loss drives workers into criminal careers, so policies aimed at mitigating the impact of job loss on crime should intervene promptly after job loss.

In Table 1, we quantify the average effect of job loss over the four years after dismissal, as estimated from equation (2). On average, job loss increases the probability of criminal prosecution by 0.12 percentage points, or 23% over the baseline (column 3). Dividing the latter effect by the 40% decrease in earnings reported in column (2), we estimate an implied elasticity of crime to earnings equal to -0.58. Importantly, we do *not* attach a causal interpretation to such elasticity, as this would require that layoffs affect criminal behavior only through (lower)

¹⁶Figure B6 in Appendix provides additional evidence of lasting effects on monthly wages, conditional on being employed, as well as more transitory effects on subsequent job separations.

Figure 3: Effect of job loss on employment and criminal prosecution probabilities



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

earnings. This is clearly not the case, as the effect could go through other mechanisms such as leisure time, psychological stress, and so on – as we will discuss in detail in Section 6. 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 particular, Appendix Table B7 confirms (in line with the visual evidence in Figure 3) that results are virtually unaffected when extending the sample to all layoffs; when restricting the control group to include only workers who are continuously employed during the sample period; and when adding a full set of municipality \times industry \times year fixed effects.¹⁷

The fact that our estimates remain similar when using all layoffs or mass layoffs suggests that spillovers and social multipliers in criminal activity across displaced colleagues may not play a relevant role in this specific context. To gather further evidence on that, we replicate the analysis splitting the sample by quartiles of total number of displaced workers with characteristics that are typically associated with higher involvement in crime – namely males, young, low tenure, UI ineligible, low income, and low education. Appendix Figure B8 shows positive but small and non-statistically significant gradients in all these dimensions.¹⁸

In columns 4-6 of Table 1, we distinguish between different categories of offenses. 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 (17-18% 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; these results are presented in Figure 4. 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

¹⁷To the extent that some of the displaced workers may transit to the informal sector, we over-estimate the magnitude of the drop in employment and earnings upon job loss and under-estimate, in turn, the crime elasticity to earnings. For this reason, in Appendix B.2 we estimate the impact of job loss on total employment – both formal and informal – using the National Longitudinal Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD). This analysis suggests that crime elasticities to total employment are about 12% higher, in terms of magnitude, of the crime elasticities reported in Table 1, so the latter represent a conservative lower bound to the true effect of job loss on crime.

¹⁸The absence of substantial spillovers over the size of mass layoffs is also in line with the evidence that our effects remain similar for stricter definitions of mass layoffs – larger layoff shares and minimum firm size – in Table B3 and Figure B2 in the Appendix.

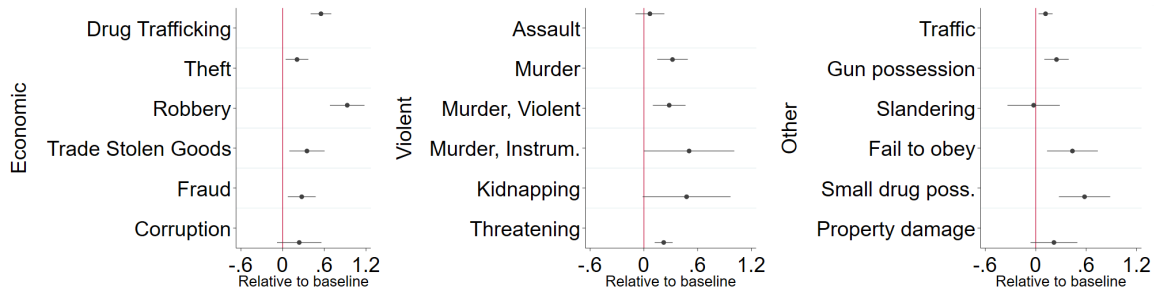
Table 1: Effect of job loss on labor market outcomes and criminal behavior, workers displaced in mass layoffs

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor market effects		Probability of criminal prosecution			
Dependent variable:	Employment	Earnings	Any crime	Economic	Violent	Others
$Treat_i \times Post_t$	-0.20*** (0.002)	-5710.0*** (53.3)	0.0012*** (0.0001)	0.00060*** (0.00006)	0.00025*** (0.00006)	0.00032*** (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

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. The sample includes workers displaced in mass layoffs who are matched to control workers employed in non-mass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $Treated_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses (** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

homicides increasing by 32%. This finding suggests that job loss may affect criminal behavior beyond purely economic motives, although we cannot exclude 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 prosecutions. Although instrumental homicides respond more strongly, both effects are statistically significant and quantitatively relevant.

Figure 4: Effect of job loss on different types of crime



Notes: The graphs show the estimated effect (and confidence interval) of job loss on different types of crime, as estimated from the difference-in-differences equation (2) and rescaled by the average outcome in the treatment group at $t = 0$.

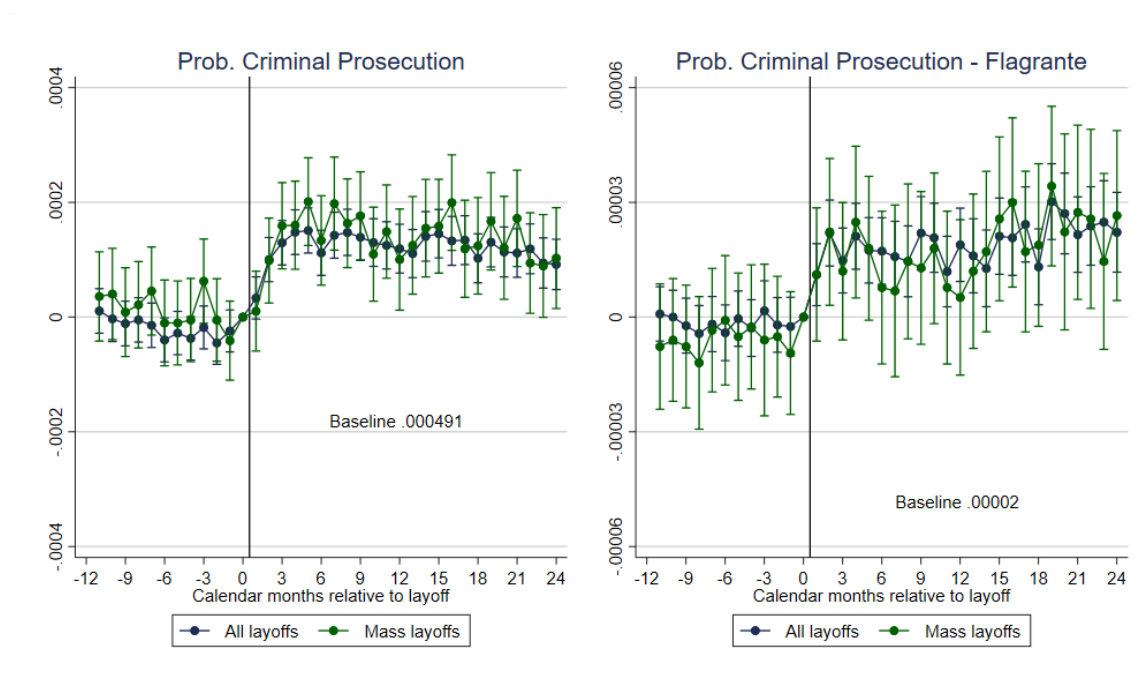
Overall, job loss affects almost all types of crime, including some that clearly have no economic motivation (e.g., traffic violations and failure to obey). These findings suggest that non-economic factors such as psychological stress may also play a role in explaining the increase in crime upon job loss (alongside economic explanations). This is even more evident from Figure B9 in the Appendix, which shows the results for all layoffs (i.e., including non-mass layoffs). In this sample, the effect is statistically significant and sizable for all types of crime, including property damage and slandering – arguably unrelated to economic motives – which increase by 24% and 14%, respectively.

4.3 Measurement of criminal behavior and effect timing

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. In practice, the extent of under-reporting may vary with individual characteristics, the type of offense, and so on. Therefore, 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 the 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

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 (left graph) and being prosecuted “in flagrante” (right graph), 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.

obtained when including all criminal prosecutions (left graph) and only prosecutions “in flagrante” (right 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 in the Appendix), they increase more strongly upon layoff (+134%). This 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 Figure 4).

Appendix B.3 implements additional robustness checks. In particular, we address potential selection into treatment in two ways. First, we replicate the analysis for more stringent definitions of mass layoffs and for plant closures, thus reducing the scope for selection among displaced workers. Second, we implement an alternative intention-to-treat approach by which we compare all workers in mass layoff firms and

in (matched) non-mass layoff firms, respectively. In both cases, the estimated crime elasticity remains unaffected.¹⁹

4.4 Heterogeneous treatment effects using causal forests

The average effects presented in Table 1 mask significant heterogeneity in criminal behavior across individuals. The unusual size and richness of our dataset provides a unique opportunity for characterizing this heterogeneity using causal forest estimators (Athey and Imbens, 2016; Athey et al., 2019; Wager and Athey, 2018). These methods rely on data-driven sample splits, thus limiting researcher discretion when selecting the relevant dimensions of heterogeneity. In addition, they allow us to capture high dimensional non-linearities while avoiding overfitting through the use of both training and estimation samples (“honest approach”).

In essence, we estimate Conditional Average Treatment Effects (CATE) for each individual based on all characteristics included in our registry data (age, tenure, education, and earnings) as well as local level conditions (employment growth by state-occupation and state-sector, municipality-level homicide rates, informality rate, GDP per capita, population, and Gini index of income inequality).²⁰ The estimated effects are rescaled by the expected outcome absent the treatment, estimated with an analogous regression forest approach. In Appendix B.4, we describe in details our implementation.

The predicted CATE is positive and statistically significant (at least) at the 5 percent level for virtually all individuals (98 percent), indicating a pervasive effect of job loss on criminal behavior. The magnitude of the effect ranges between a 7% increase in probability of committing crimes in the first decile of the effect size distribution to a 68% increase in the last decile (see Figure B10 in Appendix).

¹⁹In addition, Appendix B.3 shows that results are unaffected when measuring crime by convictions, thus reducing the scope for type I errors; when restricting to states with a lower share of missing names in criminal records; and when extending the sample to include all offenders with a unique name within their state of residence, rather than in the entire country. If we were to re-scale prosecution rates by the non-missing shares of names in each state, we would find baseline rates and absolute coefficients on general crime 1.96 times larger, while relative effects would not be affected.

²⁰Informality rates and the Gini index are computed based on the 2010 Census. Homicide rates are based on *Sistema de Informações de Mortalidade - SIM*, provided by the Ministry of Health, and municipal population and gdp per capita are estimated by IBGE.

Figure 6 shows how the effect varies with individual and local characteristics. There is a steep gradient in age and tenure, the effect being markedly higher for younger and low tenure workers. These workers likely face more severe liquidity constraints upon job loss, as they tend to have low accumulated savings and, also, receive lower severance payments (a linear function of tenure) and less generous unemployment benefits – workers with less than 6 months in the job are not entitled and maximum duration is reached at 24 tenure months. While age and tenure are clearly correlated across individuals, the left graph in Figure 7 shows that the treatment effect varies significantly over either dimension while keeping the other one constant. In addition, age and tenure remain the key dimensions of heterogeneity also when holding constant education and income, while the latter attributes do not predict variation in the effect of job loss (see Figure B11 in Appendix). In general, the predicted CATE is flat over all other characteristics, including local socio-economic conditions, as shown in Figure 6 and the right graph in Figure 7. This is a remarkable result, given that Brazil is a large and heterogeneous country where homicide rates, informality, and economic conditions vary widely across areas.

Table 2 compares the average characteristics of individuals with above and below median treatment effects, respectively, and formally tests for the difference in means while taking into account multiple hypothesis testing (List et al., 2019). Although all differences are statistically significant due to the large sample size, their magnitude (as measured by the standardized difference) is large and above the critical value (0.2) only for age and tenure. Another simple metric of the importance of each variable for explaining CATE relates to the share of data-driven sample splits over a given characteristic (Athey and Wager, 2019).²¹ Age and tenure rank first and second, driving 29% and 37% of the sample splits, respectively. They are followed by income, which drives only 10% of the sample splits and is, in fact, only weakly associated with the CATE (Table 2). Finally, the predicted CATE is flat over education.

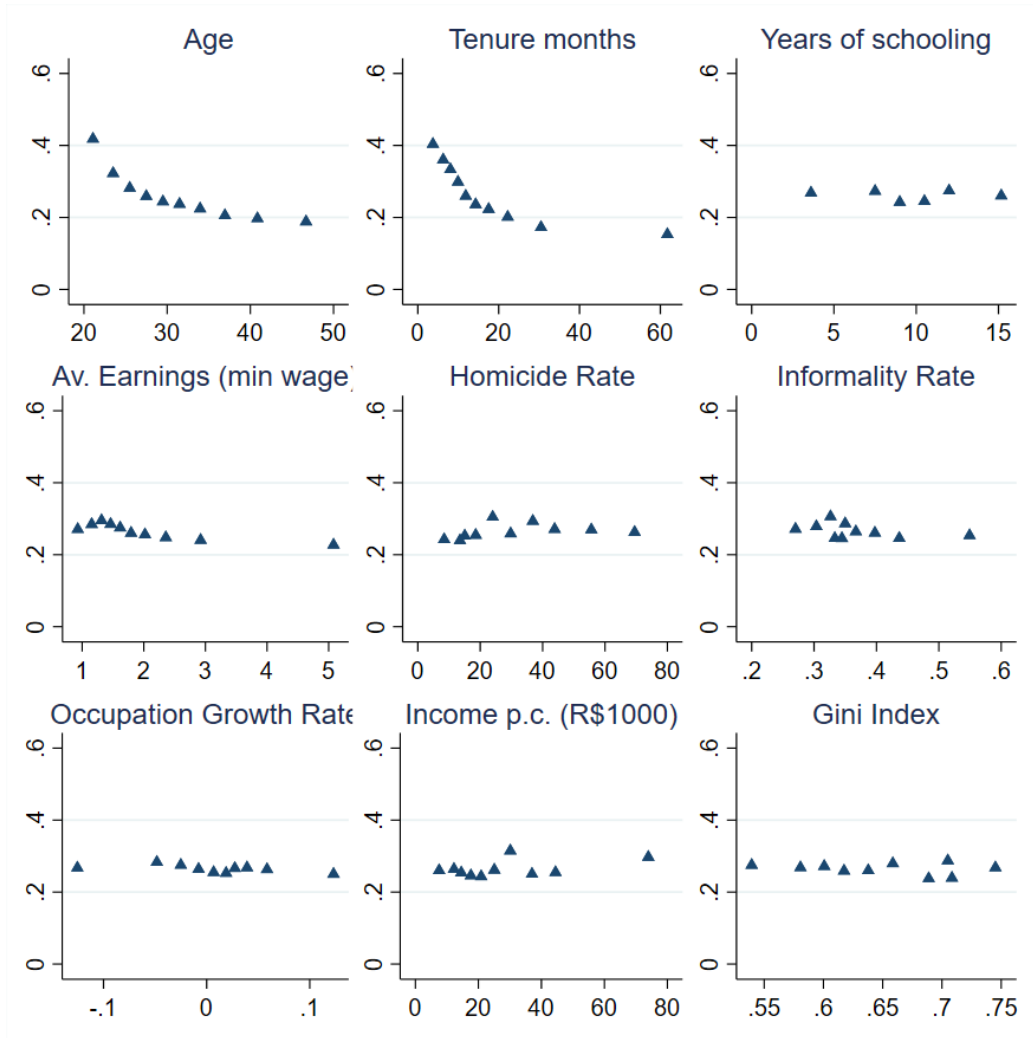
Overall, these patterns are consistent with the idea that liquidity constraints may play a relevant role in driving the effects, as extensive evidence from Brazil confirms that younger and low-tenure workers are more likely to be liquidity-constrained than other groups – including low-wage workers.²² On the

²¹This measure is weighted by the depth of the leaf at which the split takes place.

²²Using large and precise expenditure data from Brazil, Gerard and Naritomi (2021) show that younger and low-tenure workers suffer stronger consumption losses, which are suggesting that

other hand, younger and low-tenure workers differ from other workers in many respects – notably, they exhibit a higher crime rate to begin with (see Figure 2). In Section 5, we provide more direct evidence on the role of liquidity constraints exploiting variation in the eligibility to unemployment benefits across displaced workers and the timing of benefit payments.

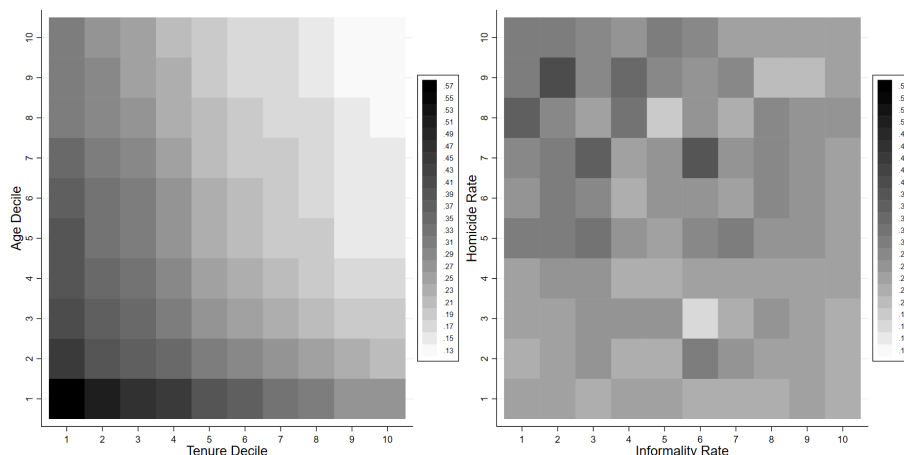
Figure 6: Conditional Average Treatment Effects (CATE) of job loss, by characteristic



Notes: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over individual and municipality level characteristics. CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a random forest.

liquidity constraints may be binding for these groups, while the same is not true for workers at the bottom of the wage distribution, due to the higher UI replacement ratio. Britto (2019) provides additional evidence that job search for low tenure workers is sensitive to cash-on hand, while high tenure workers do not react.

Figure 7: Conditional Average Treatment Effects (CATE) of job loss, by pairs of characteristics



Notes: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over pairs of individual and municipality level characteristics, respectively age and tenure (left graph) and homicide rate and informality (right graph). CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a random forest. Each bin corresponds to a decile over each characteristic.

Table 2: Predicted Conditional Average Treatment Effect (CATE) - job loss effect

	(1)	(2)	(3)	(4)
	Predicted Treatment Effects		Std. diff.	MHT p-value
	Below median	Above median	(1)-(2)	(1)-(2)
Age	33.4	28.9	0.60	0.001
Tenure months	24.1	13.1	0.61	0.001
Education	10.1	10.1	0.00	0.089
Earnings (min wages)	2.2	2.0	0.16	0.001
Homicide rate - mun. level	30.0	32.2	-0.12	0.001
Informality rate - mun. level	0.4	0.4	0.06	0.001
Sector Growth - state level	0.007	0.012	-0.04	0.001
Occupation Growth - state level	0.007	0.006	0.02	0.001
Pib per capita (R\$1000) - mun. level	26.8	28.4	-0.07	0.001
Population - mun. level	2,210,463	1,920,887	0.08	0.001
Gini index - mun. level	0.65	0.64	0.06	0.001

Notes: This table compares individual and local level characteristics for workers with, respectively, above and below median Conditional Average Treatment Effect (CATE) of job loss. CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a causal forest. Column 4 reports p-values testing for differences across groups, while accounting for multiples hypothesis testing, as in [List et al. \(2019\)](#).

4.5 Spillovers to other household members

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 focus on male and female workers aged 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 600,000 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. 26% of the families in this data are composed by at least one children older than 18 years old and such families represent 37% of the individuals in the social registry. 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 3 documents the spillover effects 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.²³

In Appendix Table B10, we examine, in addition, the role of intra-family insurance. Displaced workers are not less likely to live with their partner after job loss – if anything, the opposite is true – and this finding does not depend on selection into

²³Figure B12 in the Appendix plots dynamic treatment effects and confidence intervals. In Appendix Table B9 we show that effect on sons is robust to the addition of interacted municipality-year fixed effects, the exclusion of states where missing data issues are more relevant and to varying definitions of mass layoff.

Table 3: 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 (0.005)	0.0019** (0.0009)	-0.0017 (0.003)	0.00039 (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** (0.007)	0.0017 (0.001)	0.0035 (0.005)	-0.0014 (0.001)
Mean outcome at t=0	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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

the CadUnico registry (i.e., our source on family relationship); see columns (1)-(2). On the other hand, the existence of a stable relationship does not seem to attenuate the impact of job loss on crime (columns 3-4).

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. In this section, we investigate the effect of unemployment benefits, which is the main policy aimed at supporting displaced workers.²⁴ This analysis will also shed light on the potential

²⁴The other main source of income support in Brazil, Bolsa Familia, is a universal program targeted at very poor families and the average transfer per household remains much lower than UI benefits

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 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 1st, 2010, will be able to claim benefits again if dismissed from April 30st, 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:

$$Y_i = \alpha + \beta D_i + f(X_i) + \epsilon_i, \quad (3)$$

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 leading to UI benefits standardized so that $X = 0$ at the cutoff required for eligibility (i.e. 16 months); $f(\cdot)$ is a flexible polynomial regression; and D_i is a dummy equal to 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. In Appendix C.5, 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; we also compare RD estimates of UI effects at the true cutoff with the distribution of estimates obtained 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. To estimate the effect of actual benefit payments, we rescale the intention-to-treat effect by the “first stage”

(see Section 2.2). Appendix Table C1 confirms that the impact of job loss on eligibility to Bolsa Familia as well as on the average amount of transfers received remains negligible.

coefficient obtained when setting benefit payments as the dependent variable and estimating again the same equation.

5.2 Sample and balance tests

We focus on the 2009-14 period, because numerous changes were implemented to the UI system in 2015. We then restrict our initial sample to include only dismissed workers for whom the 16-months eligibility cutoff is binding – namely, workers with at least 6 months of continuous employment at the time of dismissal who received 3 to 5 months of UI benefits following their previous layoff.²⁵ A further sample restriction deals with the cyclical nature of dismissal dates, which naturally creates discontinuities in the density of the running variable following approximately 30-day cycles. As shown in the left graph of Appendix Figure C1, firms concentrate layoffs on the very last and initial days of the month.²⁶ 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 1st 2010 will be able to claim benefits again if dismissed from April 30st 2011. Given the dismissal cycle, when reemployed, she/he will be more likely to be displaced on the last day of the month – April 30st 2011 – rather than during the days immediately before. This creates a (mild) discontinuity in the density function, as shown in the right graph of Appendix Figure C1. However, such discontinuity is not specific to the 16-month period that is relevant for UI eligibility, but it occurs similarly at the turn of any other month.

We address this issue in two ways. In our baseline specification, we restrict the sample to workers who were initially dismissed between the 3rd and 27th of the month, in such a way that the 16-month cutoff date does not overlap with the

²⁵UI data only contain the number of monthly payments, so that we do not directly observe potential benefit duration. Workers initially receiving 3 or 4 payments might not have exhausted UI, which can last for up to 5 months. Since UI rules allow workers to claim residual benefits following subsequent layoffs, we observe some workers not meeting the 16-month eligibility condition drawing residual benefits. Yet, we show below that there is still a sizable gap in UI take-up and duration at the 16-month cutoff.

²⁶There is a missing mass on the 31st, which is explained by the 30-day advance notice period. In months comprising 31 days, a dismissal notified in the 31st actually takes place on the 30th of the following month. In months comprising 30 days, a dismissal notified on the 30th also takes place on the 30th of the following month.

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. Appendix Figure C2 shows no evidence of density discontinuity around the 16-month cutoff in this restricted sample, as also confirmed by the McCrary density test and the bias robust test developed in Cattaneo et al. (2018, 2019). In addition, Appendix Figure C3 provides balance tests for a rich set of (pre-determined) worker characteristics. Finally, the graphs in Appendix Figure C5 show no significant difference in prosecutions within one semester and 3 years before displacement, respectively, while Appendix Table C2 present the respective regression results. Taken together, these figures provide compelling evidence that displaced workers are “as good as randomly assigned” near the cutoff. Nevertheless, in Appendix Table C8 we also show that our main finding is robust to inference that allows for some degree of manipulation in the running variable, using the estimator proposed by Gerard et al. (2020). As an alternative approach to deal with cyclicity, we will include all workers and add cutoff and dismissal date fixed effects in the RD regressions (Appendix Table C7).

5.3 Results

Table 4 shows that workers barely meeting the 16-month condition are 57 percentage points more likely to draw UI, receiving additional 2.58 monthly benefits for a total of R\$2,086 – Panel A, columns (1)-(3). The average replacement rate for UI takers near the cutoff is 78%. Figure C4 in the Appendix provides visual evidence of the discontinuity in benefit payments at the cutoff; it also shows that, in line with the official provisions, virtually all benefits are paid out during the first semester after layoff.

The top-left graph in Figure 8 plots the probability that dismissed workers around the 16-month cutoff are prosecuted within the first semester after dismissal. Displaced workers who are marginally eligible for UI commit less crime than non-eligible workers. The estimated effect amounts to -0.077 percentage points, or -21% over the baseline – Panel A of Table 4, column (4). The effect is robust to the addition of individual controls and municipality fixed-effects, emphasizing the fact that our quasi-experiment compares similar workers within the same area. Rescaling

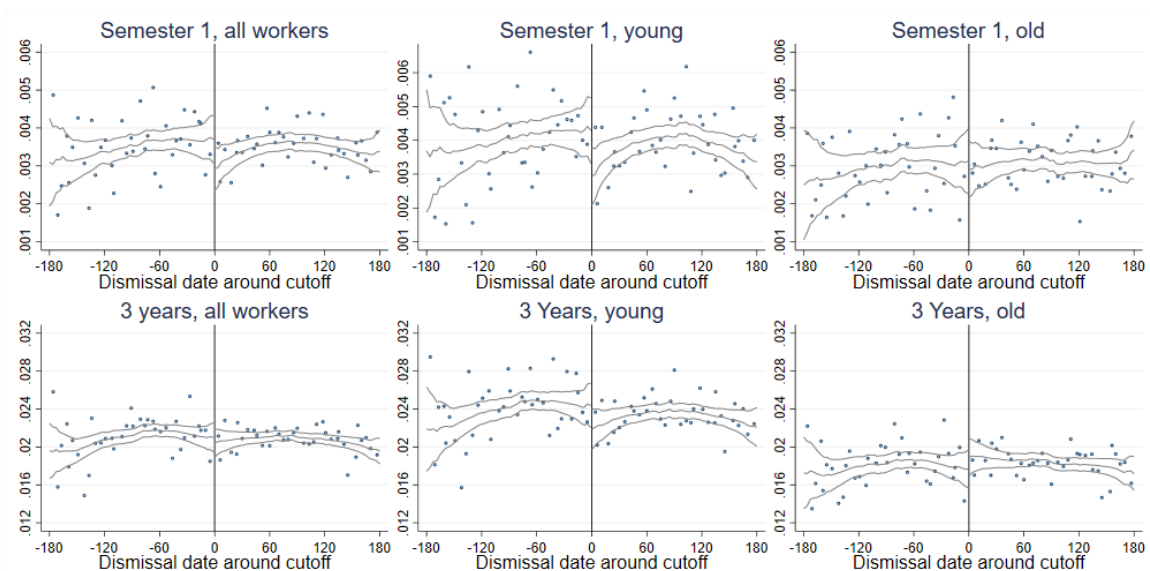
Table 4: Effect of UI Eligibility on crime

Dep. Var.:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	UI			Prob. criminal prosecution after:			
	Take-up	Payments	Amount	6 months	6 months	3 years	3 years
PANEL A. FULL SAMPLE							
Eligibility to UI benefits	0.57*** (0.0029)	2.58*** (0.012)	2086.0*** (10.5)	-0.00077* (0.00044)	-0.00085* (0.00045)	-0.00062 (0.0011)	-0.0013 (0.0011)
Mean outcome at the cutoff	0.07	0.1	130	0.0037	0.0037	0.0213	0.0213
Effect relative to the mean				-21.0%	-23.1%	-2.9%	-6.1%
Observations	270,880	270,880	270,880	270,880	268,458	270,880	268,458
PANEL B. YOUNGER WORKERS, AGE \leq 29							
Eligibility to UI benefits	0.58*** (0.0041)	2.60*** (0.017)	2018.9*** (14.1)	-0.0013* (0.00067)	-0.0015** (0.00068)	-0.0025 (0.0017)	-0.0037** (0.0017)
Mean outcome at the cutoff	0.07	0.1	112	0.0043	0.0043	0.0246	0.0246
Effect relative to the mean				-30.2%	-34.9%	-10.2%	-15.1%
Observations	134,558	134,558	134,558	134,558	132,920	134,558	132,920
PANEL C. OLDER WORKERS, AGE \geq 30							
Eligibility to UI benefits	0.56*** (0.0041)	2.56*** (0.017)	2153.2*** (15.5)	-0.00023 (0.00058)	-0.00033 (0.00059)	0.0012 (0.0014)	0.001 (0.0014)
Mean outcome at the cutoff	0.08	0.2	149	0.0031	0.0031	0.0181	0.0181
Effect relative to the mean				-7.5%	-10.8%	6.6%	5.5%
Observations	136,322	136,322	136,322	136,322	134,694	136,322	134,694
Controls	N	N	N	N	Y	N	Y

Notes: This table shows the effect of eligibility for UI benefits, as estimated from equation (3), on UI outcomes (Columns 1-3) and the probability of being prosecuted for a crime within one semester and 3 years after layoff (Columns 4-7). 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. Each panel estimates separate regressions for the different groups, as indicated in their title. The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. 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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

this reduced form coefficient by the first-stage increase in take-up (57 p.p.) leads to an average effect of -37% on compliers – larger than the average estimated effect of job loss on crime in Section 4.2 (+26%), but compatible with the stronger impact found for low tenure workers, which compose the RD sample. 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 8.3%).

Figure 8: Effect of UI eligibility on crime



Notes: The graphs plot the probability of criminal prosecution 1 semester and 3 years after layoff around the cutoff date for eligibility for unemployment benefits, for different groups of workers. Young and old groups comprise workers with below and above the median age (30 years old), respectively. 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.

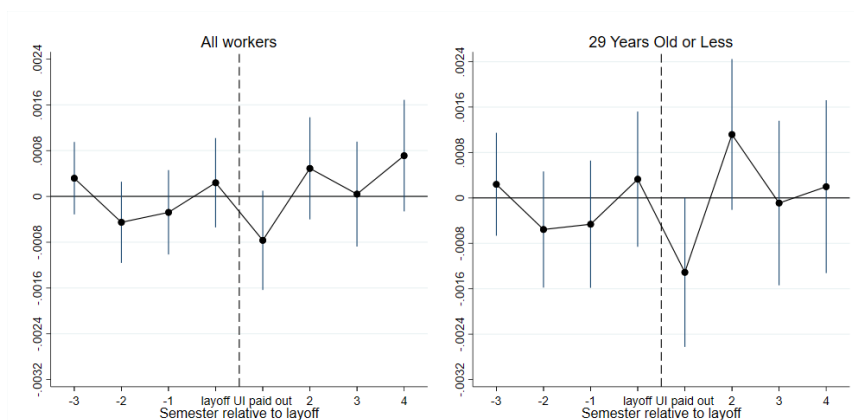
In line with the previous findings on the effect of job loss, the effect of UI is larger and more precisely estimated for younger workers while there is no significant effect on older workers; see the second and third graphs on top of Figure 8, and Panels B and C in Table 4. Additional heterogeneity and robustness analyses are provided in Appendices C.4 and C.5, respectively. The overall reduction in crime 3 years after layoff for young workers, although not very precisely estimated, is compatible in magnitude with the reduction in crime estimated in the first semester after the layoff; see columns (6)-(7) of Table 4, Panel B, and the second graph on bottom of Figure 8.²⁷ This indicates that UI effects do not go beyond benefits' expiration and are indicative of a role for liquidity constraints in explaining the reduction in crime.

²⁷In Appendix C.5, we show that this effect is robust across RD specification choices once we add controls to the regression, which increase the precision of the estimates.

5.4 Timing of the effect and the importance of liquidity constraints

We now examine more closely the timing of UI effects to shed additional evidence on the role of liquidity constraints as a key mechanism. In particular, Figure 9 confirms that UI eligibility decreases the probability of committing crime for displaced workers in the first semester after job loss, but the effect vanishes away immediately after.

Figure 9: Effect of UI eligibility on crime before and after layoff, by semester



Notes: The graphs plot RD estimates on the effect of eligibility for unemployment benefits on the probability of criminal prosecution in semesters before and after the layoff. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Each estimate is based on a local linear regression with a 60-day bandwidth. Vertical lines show 95% confidence intervals.

To further shed light on liquidity effects, we estimate changes in crime around benefit exhaustion. Using high-frequency data on consumption expenditure across displaced Brazilian workers, [Gerard and Naritomi \(2021\)](#) document sudden drops in consumption immediately after benefit exhaustion. To investigate whether criminal behavior exhibit the same abrupt changes, we replicate the difference-in-differences analysis described in Section 4.1 at monthly frequency and focus on “in flagrante” arrests, which do not suffer from prosecution lags. We include in the sample all UI beneficiaries in the 2009-14 period and consider as treated workers that already exhausted the maximum duration of UI benefits (i.e., 5 months). We successfully match 3.2 million UI beneficiaries to a control worker who is employed during the same period and displays the same set of individual characteristics described in Section 4.1, and compare the probability that they are arrested “in flagrante” four months before

and after UI expiration.

Figure 10 clearly shows that the probability of committing crimes increases immediately after benefit expiration. The effect is also sizable (+36%) and, in line with previous results, it is even larger for younger workers (+55%). Overall, these estimates suggest that liquidity constraints play an important role as mechanism linking job loss and crime. These findings mirror the evidence on consumption drops in Gerard and Naritomi (2021).

Overall, Figures 9 and 10 provide very convincing evidence about the importance of liquidity constraints for explaining both the increase in the probability of committing crimes upon job loss and the (temporary) mitigating effect of unemployment benefits.

Figure 10: In flagrante prosecution around UI benefit exhaustion



Notes: This figure shows estimated changes in the probability of “in flagrante” criminal prosecution around UI benefits exhaustion for UI beneficiaries relative to matched control workers employed throughout the entire period, estimated by equation (1) at a monthly frequency, along with 95% confidence intervals. Standard errors are clustered at the worker level.

6 Discussion

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.

6.1 Mechanisms

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.

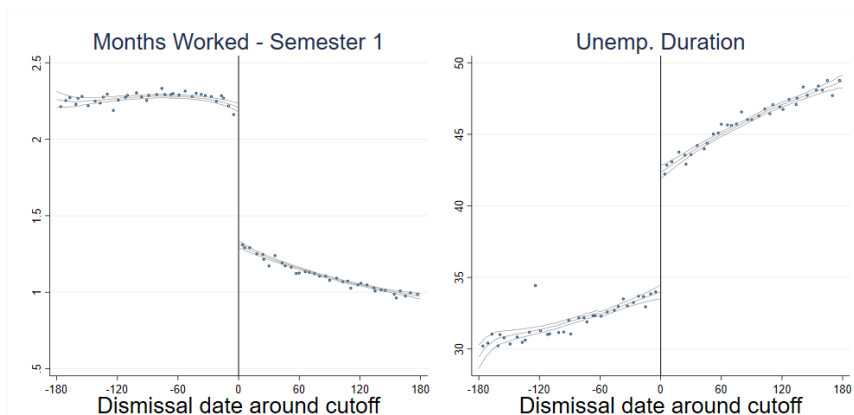
However, the latter explanation does not square well with the fact that displaced workers who are eligible for unemployment benefits exhibit lower crime rates than the non-eligible. Both groups are unemployed immediately after layoff, but displaced workers eligible for UI work fewer months in the first semester after displacement (-39%) and remain unemployed for longer periods of time (+25%), due to the negative impact of unemployment benefits on labor supply; see Figure 11 and Appendix Table C10. If employment had an incapacitation effect on potential offenders, eligible workers should commit more crime than the non-eligible (instead, they commit less).²⁸

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, UI reduces the payoff of formal work, because benefits are ceased if the beneficiary finds a new formal job; if anything, UI should then 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 and the timing of such effects, discussed in the previous section, suggest that a significant portion of displaced workers are subject to binding liquidity constraints. This explanation is also consistent with the stronger effect detected for younger and low-tenure workers, who are most likely to be liquidity constrained,

²⁸It 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.

Figure 11: Effect of UI eligibility on employment



Notes: The graphs plots the number of months worked (left graph) and unemployment duration (right 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. Unemployment duration is measured in weeks and censored at 36 months, the end of our sample period.

and with the spillover effects on cohabiting sons, who are likely subject to the same liquidity constraints. 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 addition, despite the large heterogeneity across the Brazilian territory, area-level conditions do not seem to play a primary role in driving the results.

Finally, we note the potential relevance of psychological stress associated to job loss. This explanation is supported by the fact that job loss has a substantial impact on a wide range of offenses that have no economic motivation. For example, traffic-related offenses and failure to obey increase by 12% and 44% after mass layoffs (Figure 4), while slandering and property damage increase by 14% and 24% in an extended sample covering all layoffs (Figure B9 in the Appendix). In addition, there is a sizable impact on non-instrumental homicides (i.e., homicide cases that are not associated to other criminal charges) which increase by 28% after mass layoffs and by 36% in the extended sample covering all layoffs. Although our data do not allow for a direct assessment of job loss effects on psychological factors, such mechanism is consistent with evidence linking job displacement to mental health problems and stress (Charles and DeCicca, 2008; Kuhn et al., 2009; Zimmer, 2020).

6.2 Implications for theoretical models of crime

Based on these findings, we discuss a few features that might be desirable in economic models studying the relationship between job loss and crime. First, crime is triggered by binding liquidity constraints. In such context, lack of adequate social insurance causes individuals to engage in other forms of self-insurance ([Chetty and Looney, 2009](#)), which may be extremely costly for the society such as crime. In addition, the sharp increase in crime following predictable income drops, such as UI exhaustion, indicates that workers fail to smooth consumption, such pattern being consistent with behavioral models with myopic or present-biased individuals [Ganong and Noel \(2019\)](#); [Gerard and Naritomi \(2021\)](#).

Second, it is often argued that the workhorse economic model of crime applies mainly, or exclusively, to economically-motivated crimes, which is also consistent with evidence from previous papers (e.g., [Machin and Meghir, 2004](#); [Mastrobuoni and Pinotti, 2015](#)). Differently from these papers, we estimate significant and large effects for a wide range of crimes with no economic motivation – possibly because our extremely large dataset allows us to detect with sufficient precision the effect on violent crimes and other non-economic crimes, which are typically less frequent. These results confirm earlier theoretical insights of [Ehrlich \(1973\)](#): “[s]ince those who hate need not respond to incentives any differently from those who love or are indifferent to the well-being of others, the analysis [...] would apply, with some modifications, to crimes against the person as well as to crime involving material gains”; in particular, “independent changes in legitimate market opportunities may also have a systematic effect on participation in crimes against the person”. In addition, one could augment the model to allow for a direct effect of the psychological stress (from economic insecurity or other reasons) through changes in risk aversion or intertemporal discount factors.

Finally, our results may inform previous theoretical work about the magnitude of crucial parameters such as the sensitivity of crime to income transfers following displacement. UI eligible workers who have an average replacement rate of .79 and who access 2.58 monthly benefits are 21% less likely to be criminally prosecuted in the semester following layoff. The impact is entirely driven by young workers who reduce crime by 30.2% while drawing the same number of monthly benefits.

6.3 Social assistance policies and crime

Our results are in line with other studies in the literature showing crime reduction effects of social assistance policies. Several US based analyses focus on ex-offenders, a narrower context compared to ours. [Yang \(2017\)](#) and [Tuttle \(2019\)](#) find that cuts in food stamps and other welfare benefits increases recidivism. [Rose \(2018\)](#) finds that a 10% increase in UI benefit amount reduces ex-offender’s recidivism by 5.4% in the three quarters following job loss. Outside the US context, [Munyo and Rossi \(2015\)](#) estimate that increasing prison release gratuity from 30 to 100 pesos eliminates first day recidivism. Other papers studying the impact of social assistance policies in the broader population also find crime reducing effects. [Watson et al. \(2020\)](#) show that universal basic income in Alaska reduces property crime by 10%, although substance-abuse incidents increase by 8%, while [Fishback et al. \(2010\)](#) estimate that 10% increase in welfare relief spending decreases crime by 1.5% using US data. Finally, [Chioda et al. \(2016\)](#) find that an expansion in Bolsa Família coverage to children with 16-17 years old significantly reduces crime by 6.5% in school neighborhoods.

7 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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A Appendix to Section 3

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: Summary statistics, workers with and without unique names

	(1)	(2)	(3)	(4)	(5)	(6)
	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 - mixed	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
MUNICIPALITY CHARACTERISTICS						
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.

B Appendix to Section 4

B.1 The effect of job loss on crime, treatment and control group characteristics

Table B1: 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 - mixed	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
MUNICIPALITY CHARACTERISTICS						
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 OUTCOMES						
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 B2: Summary statistics, by criminal prosecution before and after the job loss

	(1)	(2)	(3)	(4)	(5)	(6)
	Criminal prosecutions					
	Before job loss			After job loss		
	No	Yes	Std Diff	No	Yes	Std Diff
DEMOGRAPHIC CHARACTERISTICS						
Years of education	10.1	9.9	0.05	10.1	9.9	0.05
Age	30.7	30.3	0.05	30.7	29.3	0.19
Race - white	45.8%	53.1%	- 0.15	45.7%	51.5%	-0.12
Race - black	5.6%	5.5%	0.01	5.6%	5.5%	0.01
Race - mixed	39.8%	31.4%	0.18	39.8%	32.6%	0.15
JOB CHARACTERISTICS						
Monthly income (R\$)	1,397	1,320	0.08	1,399	1,278	0.14
Month of worked $t - 1$	10.8	10.2	0.20	10.8	10.2	0.19
Tenure on Jan 1 st (years)	1.1	0.9	0.21	1.1	0.9	0.21
Manager	3.2%	2.1%	0.07	3.2%	2.1%	0.07
Firm size (employees)	573	510	0.04	574	519	0.03
MUNICIPALITY CHARACTERISTICS						
Large municipality - pop > 1M	38%	34%	0.08	38%	34%	0.09
Municipality population	2,183,937	1,770,187	0.12	2,191,327	1,671,132	0.16
Homicide rate (per 100k inhab.)	31.1	28.7	0.13	31.1	30.4	0.04
Observations	1,151,321	16,525		1,138,112	29,734	

Notes: This table reports the average characteristics of treated workers by criminal prosecution status in periods before (columns 1-2) and after displacement (columns 5-6); and the standardized difference between the two groups (columns 3 and 6).

B.2 The effect of job loss on formal and informal employment

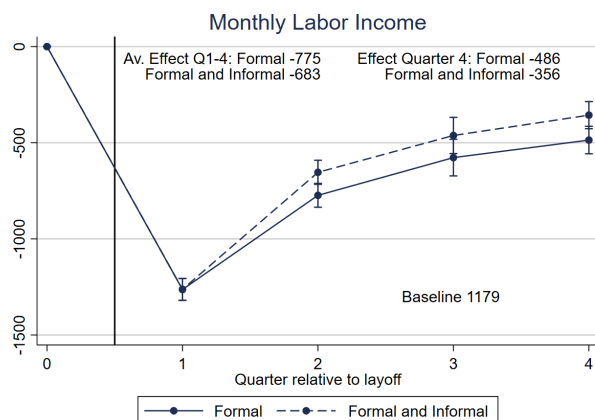
To the extent that some of the displaced workers may transit to the informal sector – which accounts for 43% percent of economic activity in Brazil during our sample period (IBGE) – the estimates in panels (a)-(b) of Figure 3 in Section 4.2 overstate the drop in employment and earnings for displaced workers relative to the control group. In turn, crime elasticities to formal labor earnings in Table 1 would underestimate the magnitude of crime elasticity to total labor earnings (i.e., including both formal and informal earnings).

We thus replicate the analysis of employment effects based on the National Longitudinal Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD), which contains information on both formal and informal labor income. In

fact, the Brazilian Institute of Geography and Statistics (IBGE) computes informality rates based on PNAD. The longitudinal component of PNAD tracks households for five consecutive quarters. 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. In line with our main analysis of Figure 3 in the main text, 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 B1 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 to 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 under-estimated, in terms of magnitude, by about 12%.

Figure B1: 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 (along with 95% confidence intervals) as estimated from the difference-in-differences equation (1), based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. The sample covers individuals first interviewed in the period 2012-14. The treatment group is defined by workers who are formally employed in the first interview and out of employment in the second interview; the control group is composed by workers who are formally employed on the first and second interviews. Earnings are measured in Brazilian Reals. Baseline average values for the treated group at $t = 0$ are also reported.

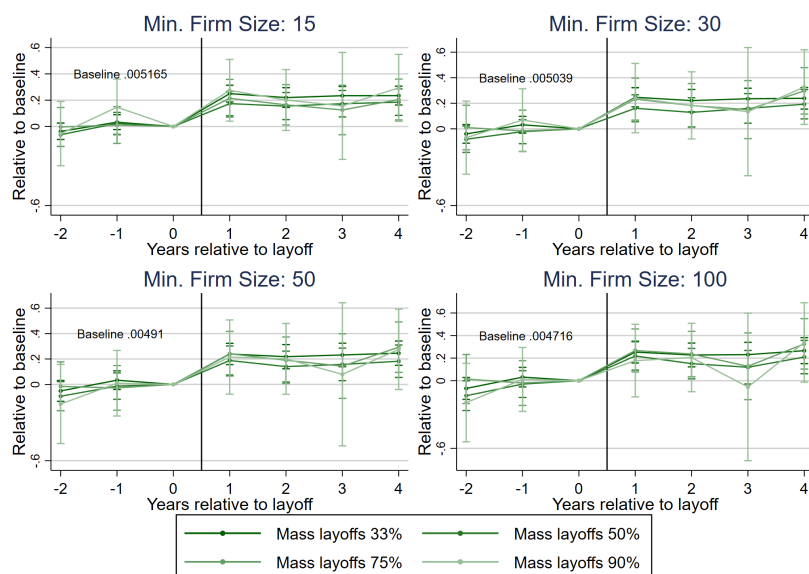
B.3 The effect of job loss on crime, additional robustness checks

B.3.1 Selection into treatment

Our analysis of the effects of job loss, presented in Section 4, 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 of parallel trends in the pre-treatment period (Figures 3 and 5) is consistent with such an assumption. Importantly, the same figures show that all results are virtually identical when including all displaced workers or, alternatively, restricting the treated group to workers displaced upon plausibly exogenous mass layoffs; also, results are unaffected when controlling for municipality \times industry \times year fixed effects.

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 B3 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, differences in the level of crime rates between dismissed workers and matched controls during the pre-treatment period – reported in the last row of each panel of Table B3 – progressively decline to almost zero when restricting to events in which at least 90% of workers were dismissed. At the same time, the estimated effect on crime is largely unaffected; see also Figure B2, which shows the dynamic treatment effects under these alternative specifications. The same is true when focusing on plant closures (column 5) and when varying the minimum firm size (panels B to D).

Figure B2: 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.

As a second approach to addressing potential selection effects, in Table B4 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 characteristic. 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. Not surprisingly, when we adopt this “intention-to-treat” 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

Table B3: 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*** (0.0001)	0.00091*** (0.0001)	0.00078*** (0.0002)	0.00082** (0.0003)	0.00074*** (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*** (0.0001)	0.00094*** (0.0002)	0.00094*** (0.0002)	0.00089** (0.0004)	0.00066** (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*** (0.0001)	0.00095*** (0.0002)	0.0010*** (0.0002)	0.00100** (0.0004)	0.00096*** (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*** (0.0002)	0.0011*** (0.0002)	0.0011*** (0.0003)	0.00095** (0.0004)	0.00087** (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 (***) $p \leq 0.01$, (**) $p \leq 0.05$, (*) $p \leq 0.1$.

crime effects by changes in earnings, the implied elasticity remains very similar to our baseline estimate in Table 1.

Table B4: 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*** (0.002)	-6146.1*** (159)	0.00090*** (0.00009)	-0.10*** (0.002)	-1983.2*** (144.2)	0.00018** (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 (***) $p \leq 0.01$, (**) $p \leq 0.05$, (*) $p \leq 0.1$).

B.3.2 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

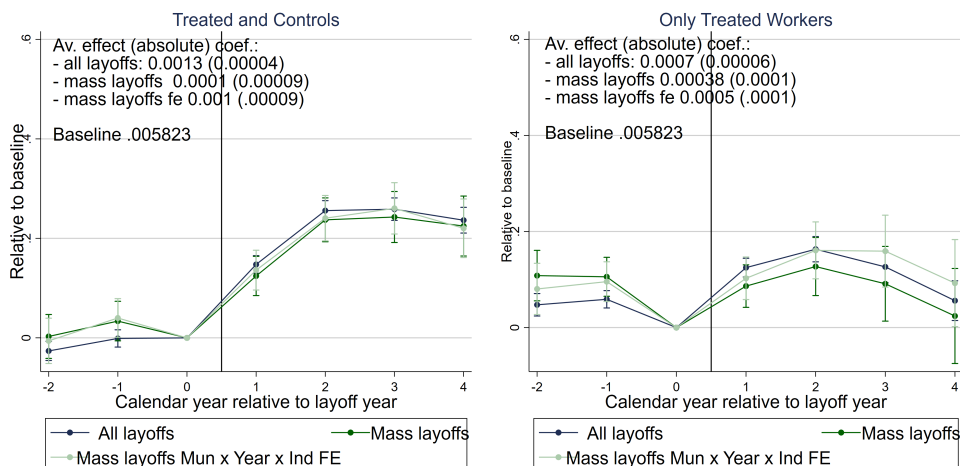
weights (De Chaisemartin and D’Haultfoeuille, 2020).²⁹

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 workers (i.e., “pure controls”), 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; see Figure B3, comparing the estimated effect in two-way fixed effect regressions when including (left panel) and excluding (right panel) pure controls who are never displaced.³⁰ As a final robustness check, we re-estimate the effect of interest following the approach of De Chaisemartin and D’Haultfoeuille (2020), 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, and are reported in Figure B4.

²⁹Goodman-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).

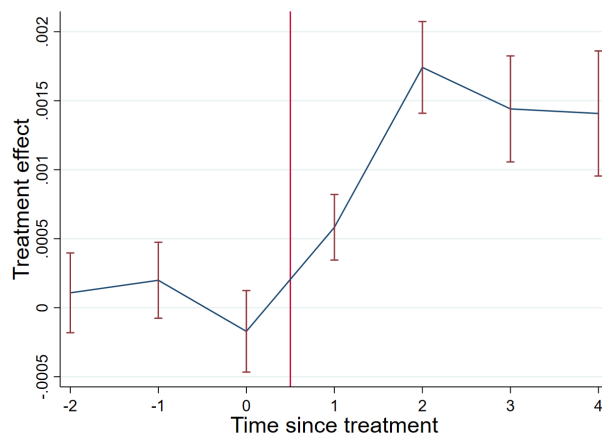
³⁰In both graphs of Figure B3, the estimated effect in the first year after treatment is attenuated compared to our baseline estimates in Figure 3. The reason is that periods are defined by calendar years in Figure B3 and by the exact number of months since the layoff date in Figure 3, respectively. Therefore, most displaced workers are treated for only part of the first post-treatment period in the former figure, while they are treated for the entire period in the latter graph.

Figure B3: Effect of job loss on crime, two-way fixed effects panel estimates



Notes: The left graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime in an yearly panel, two-way fixed effects specification, along with 95% confidence intervals. The right 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 B4: Effect of job loss on crime, two-way fixed effects panel estimates with correction from De Chaisemartin and D'Haultfoeuille (2020)

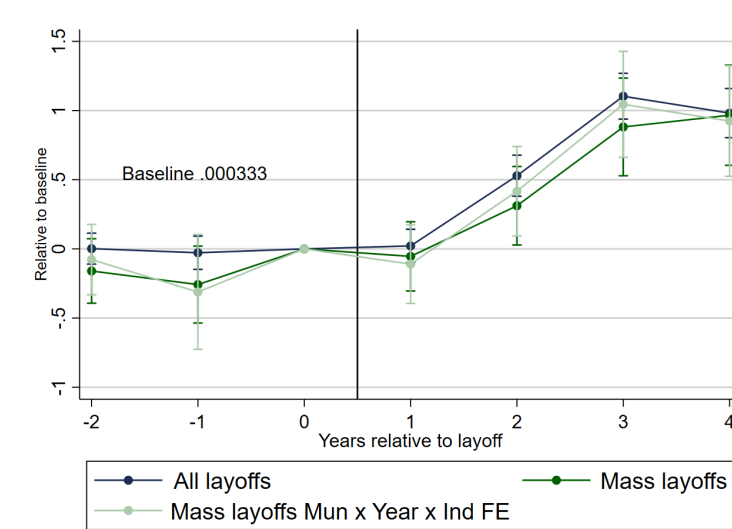


Notes: The graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime in an yearly panel, two-way fixed effects specification using the estimator proposed by De Chaisemartin and D'Haultfoeuille (2020), along with 95% confidence intervals.

B.3.3 Additional measurement issues

The results in Figure 5 show that our main estimates are unaffected when including all prosecutions or only prosecutions started “in flagrante”, respectively. Figure B5 shows that results are also robust to measuring crime by convictions as opposed to prosecutions, thus reducing the scope for type I errors in the measurement of crime.

Figure B5: 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.

Another source of measurement error is that the defendant name is missing for 6.5 million cases on a total of 14 million. As discussed in Section 3.1, there are several reasons to believe that incidence of missing names is uncorrelated with employment status. Nevertheless, we assess the validity of our results to progressively restricting the sample to Brazilian states with a lower share of missing names in criminal data, as listed in Table B5.

Table B6 shows that results are unaffected when restricting to states in which the share of non-missing names is as high as 80% or more.

Table B5: 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
Piauí	86.8%	121,567
Bahia	78.4%	510,540
Alagoas	79.2%	118,152
Maranhão	81.0%	183,117
Espírito 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
Paraíba	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 B6: 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*** (0.0001)	0.0014*** (0.0001)	0.0017*** (0.0002)	0.0017*** (0.0002)	0.0018*** (0.0002)	0.0013*** (0.0002)	0.0016*** (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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

B.4 The Causal Forest approach for heterogeneous treatment effects

The causal forest method is a development of supervised machine learning techniques used that can be used for predicting heterogeneity in causal treatment effects (Athey and Imbens, 2016; Athey et al., 2019; Wager and Athey, 2018). The goal is estimating Conditional Average Treatment Effects (CATE), $E[Y_{1i} - Y_{0i} | X_i = x]$, where Y_1 and Y_0 denote the potential outcomes of interest for the i -th individual when treated and untreated, respectively, and X is a vector of observable characteristics.

We follow the implementation in Athey et al. (2019). Since we have a difference-in-differences setting (Bertrand et al., 2017; Davis and Heller, 2017, differently from most application based on randomized control trials such as), we run the causal forest over first-differences. In this way, the treatment group indicator is orthogonal to the covariates, so the unconfoundness assumption in Wager and Athey (2018) holds.

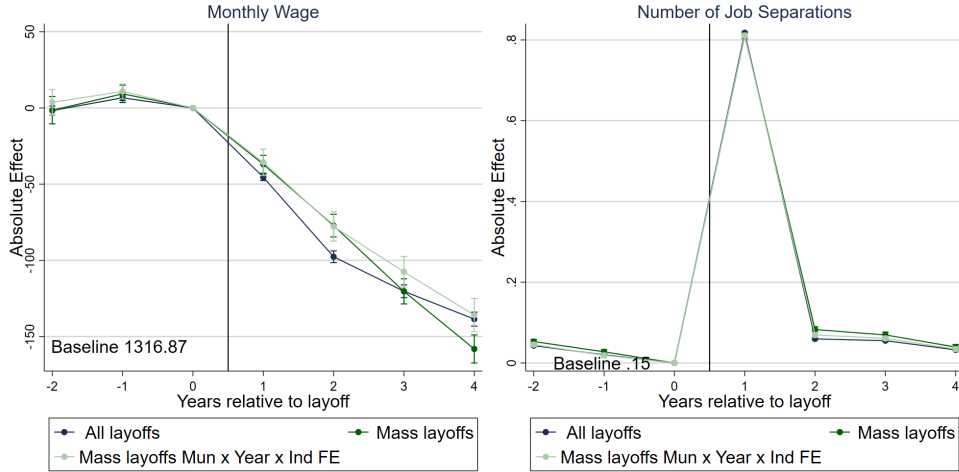
The main outcome is the probability of criminal prosecution. The algorithm starts by building trees. Each of them is defined by data driven sample splits characterizing leafs, which are followed by a prediction of the causal effect over the characteristics X . Given our large sample and the fact that the goal is estimating a small quantity, we require that each leaf contains at least 5,000 observations to improve precision.³¹ To avoid over fitting, the sample is randomly split in two equal parts: one is used to define the sample splits (leafs), the other is used for estimating the predicted CATE (“honest approach”). The procedure is repeated multiple times, leading to 10,000 trees. The final causal forest prediction is a weighted average over the predictions in each tree, which is shown to be consistent and asymptotically normal (and is also clustered at the individual level). In addition, valid confidence intervals are estimated.

We follow a similar procedure to estimate CATE for our RD design studying the impact of UI eligibility. We focus on the narrow bandwidth of 45 days around the cutoff, so that the unconfoundness assumption in Wager and Athey (2018) is satisfied. Then, we estimate the causal tree as described above. The only difference is that we grow a larger number of trees – 20,000 – to reduce the excess error and reduce the minimum leaf size to 300 observations in light of the smaller sample. In both analyses, the excess error is below 2×10^{-10} .

³¹Avoiding leafs which are too small also speeds up computational time.

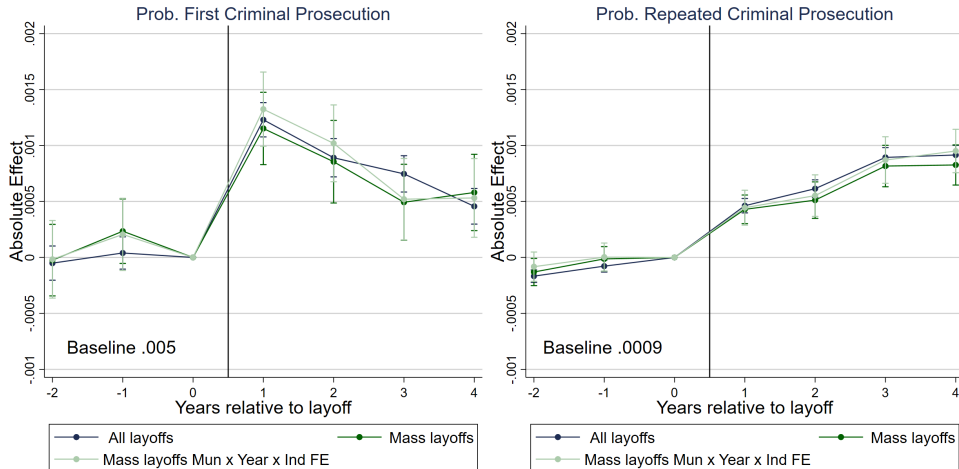
B.5 Additional figures and tables

Figure B6: Effect of job loss on subsequent monthly wages and job turnover



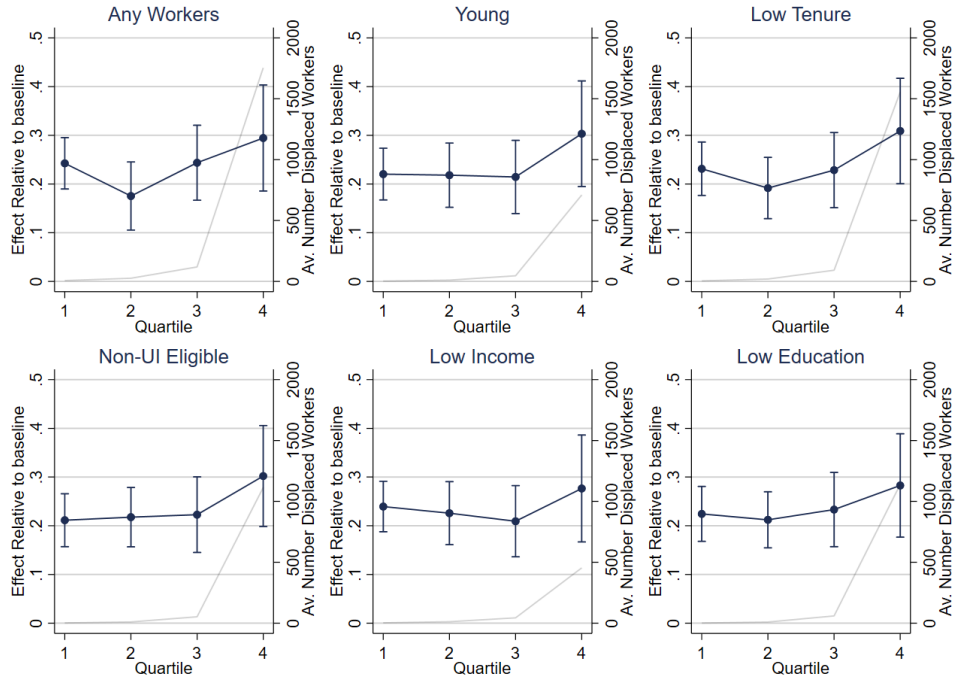
Notes: This figure shows the effect of job loss on individual monthly wages conditional on being employed (left graph) and the number of job separations per year (right graph), as estimated from the difference-in-differences equation (1) – along with 95% confidence intervals (too small to be visible).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

Figure B7: Effect of job loss on crime, first versus repeated prosecution



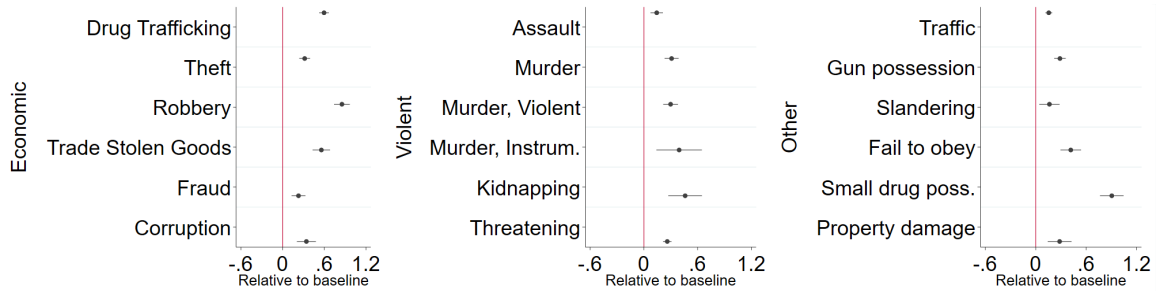
Notes: The graph reports the dynamic treatment effects of job loss on the probability of being criminally prosecuted for the first time, within our panel, and on the prob. of a repeated prosecution, estimated according to equation (1), along with 95% confidence intervals. Years relative to layoff are 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.

Figure B8: Effect of job loss on criminal behavior, by number of displaced individuals in the same firm, mass layoffs



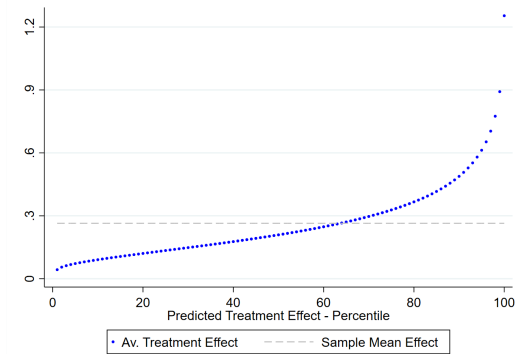
Notes: This table shows the effect of job loss in a mass layoff on the probability of criminal prosecution up to 4 years after, as estimated from the difference-in-differences equation (2), after splitting the sample on the number of workers displaced in the same mass layoff event with given characteristics (by quartiles). 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 B9: Effect of job loss on different types of crime - all layoffs



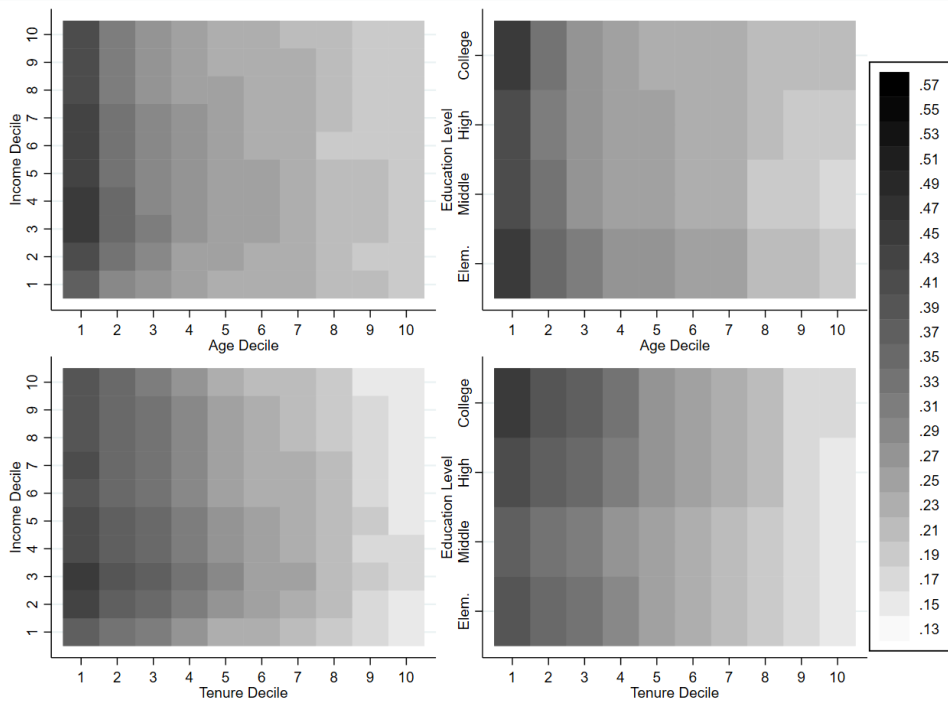
Notes: The graphs show the effect of job loss on different types of crime (and associated confidence interval) as estimated from the difference-in-differences equation (2) and rescaled by the average outcome in the treatment group at $t = 0$.

Figure B10: Job loss - Predicted Conditional Average Treatment Effects (CATE)



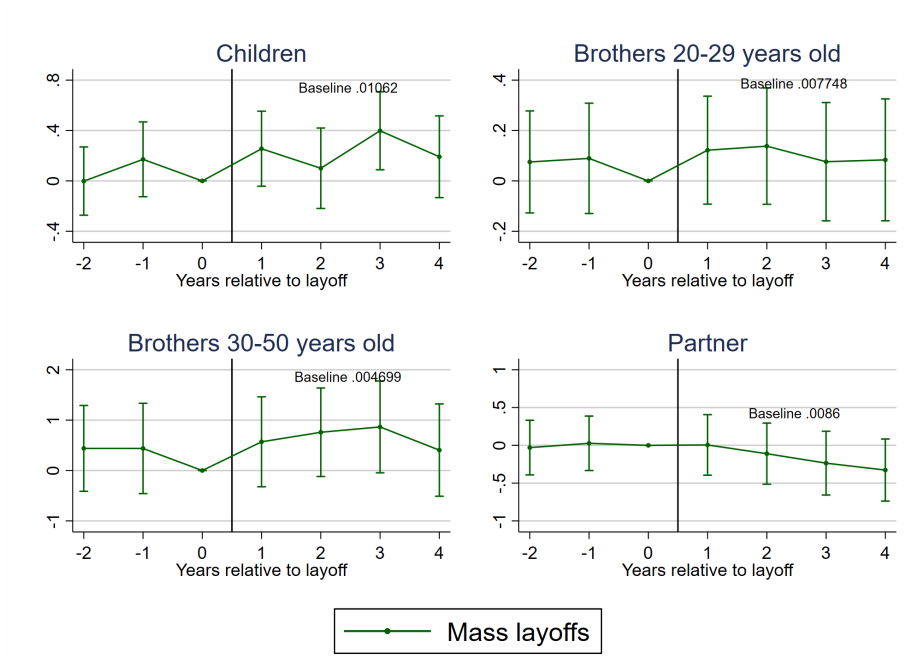
Notes: This figure shows how the predicted Conditional Average Treatment Effect (CATE) vary over its rank, aggregated over percentiles. A causal forest is implemented to estimate the CATE and estimates are rescaled by the predicted crime outcome in the post period absent the job loss – also based on a random forest – reflecting a proportional effect. 98% of the predicted CATE at the individual level are statistically different from zero in the sample.

Figure B11: Job loss - Predicted Conditional Average Treatment Effects (CATE), by pairs of characteristics



Notes: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over pairs of individual characteristics. A causal forest is implemented to estimate the CATE and estimates are rescaled by the predicted crime outcome in the post period absent the job loss – also based on a random forest – reflecting a proportional effect. Each map bin corresponds to a decile over each characteristic (years of education is an exception due to the discrete nature of the variable).

Figure B12: Effect of job loss on household members' criminal behavior



Notes: This table shows 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 $t = 0$.

Table B7: Effect of job loss on labor market outcomes and criminal behavior - alternative specifications

	(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*** (0.0006)	-6048.1*** (28.1)	0.0015*** (0.00005)	0.00067*** (0.00003)	0.00033*** (0.00003)	0.00041*** (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*** (0.0006)	-8600.1*** (32.7)	0.0029*** (0.00006)	0.0010*** (0.00003)	0.00070*** (0.00003)	0.00097*** (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, CONTROLLING FOR MUNICIPALITY \times INDUSTRY \times YEAR FIXED-EFFECTS						
$Treat_i \times Post_t$	-0.19*** (0.001)	-5433.7*** (59.9)	0.0013*** (0.0001)	0.00061*** (0.00006)	0.00039*** (0.00006)	0.00030*** (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 adds municipality \times industry \times 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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$.

Table B8: 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*** (0.0000448)	0.000548*** (0.0000251)	0.000168*** (0.0000177)	0.000376*** (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*** (0.0000967)	0.000368*** (0.0000545)	0.000147*** (0.0000385)	0.000166** (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*** (0.0000942)	0.000427*** (0.0000556)	0.000151*** (0.0000381)	0.000243*** (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 \times industry \times 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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

Table B9: Effect of job loss on cohabiting sons

	(1)	(2)	(3)	(4)	(5)
Dep.: Probability of Criminal Prosecution					
PANEL A: JOB LOSERS' SONS					
Effect of job loss	0.0019** (0.0009)	0.0033** (0.001)	0.0026** (0.001)	0.0035** (0.002)	0.0047** (0.002)
Relative Effect	18%	23%	24%	36%	57%
Mean - Treatment Group	0.0106	0.0141	0.0106	0.0098	0.0082
Observations	334,061	194,537	329,455	116,676	52,759
Drop MG and SP		Y			
Mun X Year FE			Y		
Min. Mass layoff share				60%	80%

Notes: This table shows the effect of worker's displacement on the probability of criminal prosecution for sons living in the same household, 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 sons 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 sons 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. Each column reports results based on a different specification. Columns (2) presents results when excluding data from the states of Minas Gerais and São Paulo, where the share of missing data on criminal outcomes is high, above 70%. 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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

Table B10: Effect of job loss, family insurance

	(1)	(2)	(3)	(4)
Dep. Var.	Live with partner	In CadUnico	Prob. Criminal Prosecution	Prob. Criminal Prosecution
Effect of job loss	0.017*** (0.0007)	-0.00047 (0.001)	0.0011*** (0.0002)	0.0014*** (0.0003)
Relative Effect	14%	0%	27%	27%
Mean outcome, treated at t=0	0.1244	0.3185	0.0041	0.0053
Observations	14,088,020	14,088,020	1,760,866	2,646,212
Sample	Full	Full	Living with partner at t=0	Not Living with partner at t=0

Notes: This table shows the effect of job loss on the probability that workers are found to live with a partner in CadUnico, and enter CadUnico (columns 1-2); are criminally prosecuted for workers in CadUnico living and not living with a partner in the pre-displacement period (t=0) (columns 3-4), as estimated from the difference-in-differences equation (2). The panel covers the period 2011-17 when yearly snapshots of CadUnico data are available. 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. 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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

C Appendix to Section 5

C.1 Social insurance other than unemployment benefits

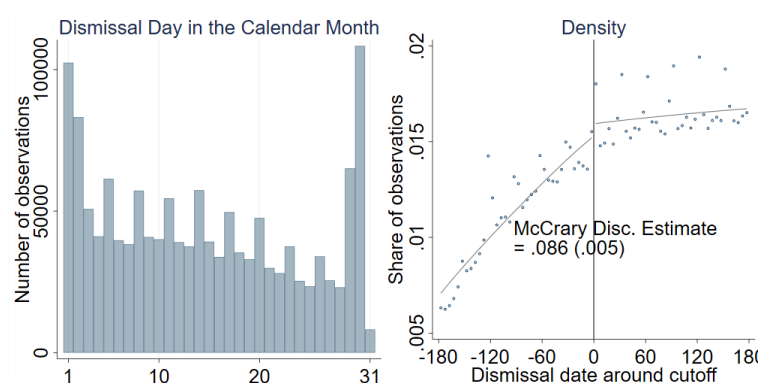
Table C1: Effect of job loss, social insurance

	(1)	(2)
Dep. Var.	Receive	Yearly Amount
	Bolsa Familia	Bolsa Familia
Effect of job loss	0.0035** (0.001)	37.0*** (2.2)
Relative Effect	2%	17%
Mean outcome, treated at t=0	0.1743	223.6023
Observations	14,088,020	14,088,020

Notes: This table shows the effect of job loss on the probability that workers receive Bolsa Familia cash transfer and the respective amount (columns 1-2), as estimated from the difference-in-differences equation (2). The panel covers the period 2011-17 when yearly snapshots of CadUnico data are available. 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. 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 (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

C.2 Cyclicity in hiring and firing of workers

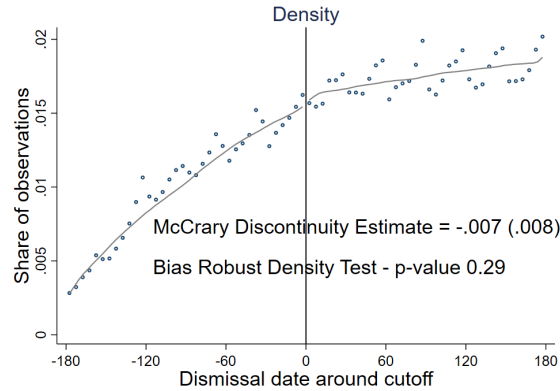
Figure C1: Cyclicity in hiring and firing of workers



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

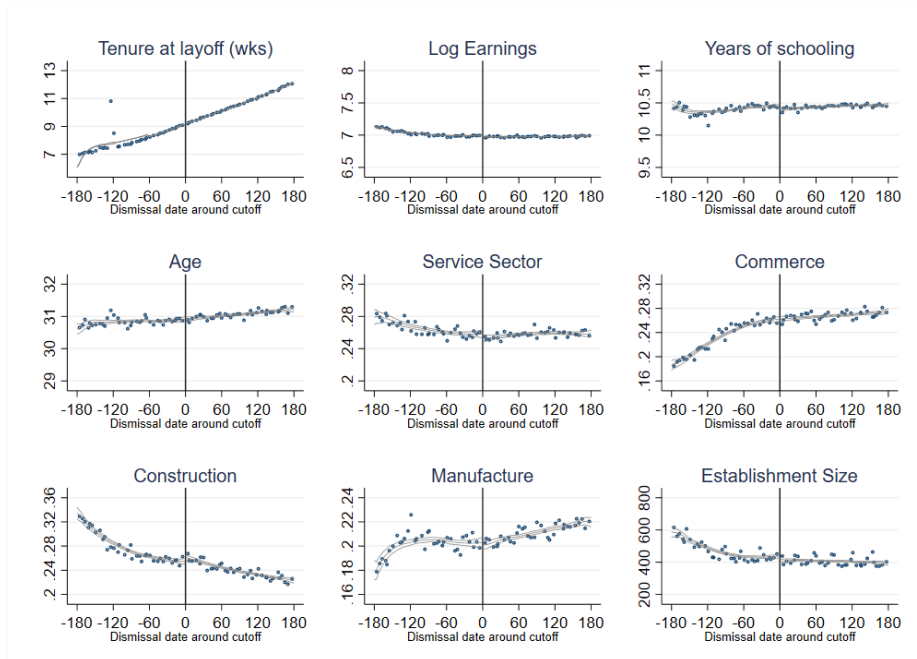
C.3 The effect of UI on crime, evidence on design validity

Figure C2: Distribution of observations around the UI eligibility cutoff, main sample



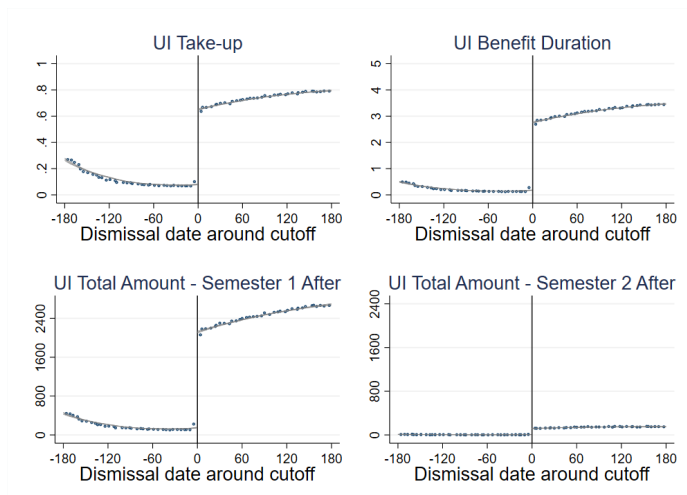
Notes: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past) in our main working sample. The sample includes displaced workers with at least 6 months of continuous employment prior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

Figure C3: Balance of pre-determined covariates across workers near the cutoff for UI eligibility



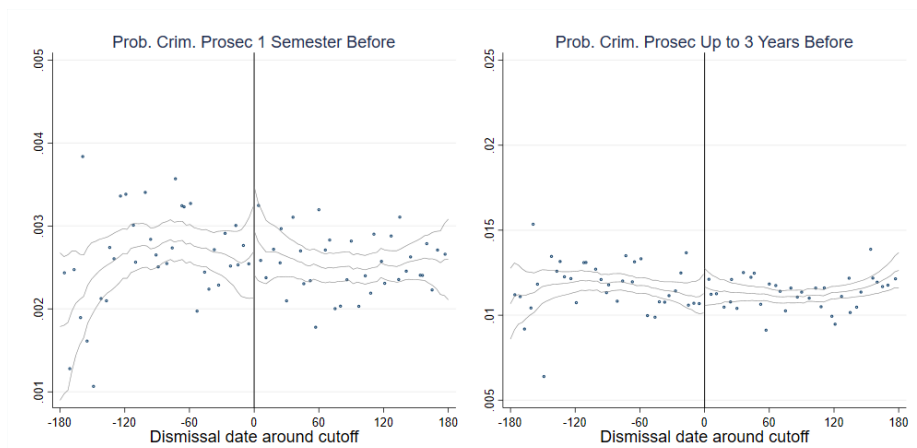
Notes: The graphs show the balance of pre-determined covariates around the cutoff for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

Figure C4: Effect of UI eligibility on UI Outcomes



Notes: The graphs plot UI outcomes 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 C5: Effect of UI eligibility on crime before layoff (placebo)



Notes: The graphs plot the probability of criminal prosecution 1 semester and 3 years before layoff (top and bottom graphs, respectively) around the cutoff date for eligibility for unemployment benefits, as a placebo exercise. 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.

Table C2: Effect of UI Eligibility on crime - placebo

Dep. Var.:	(1)	(2)	(3)	(4)
	Prob. criminal prosecution before layoff:			
	6 months	6 months	3 years	3 years
PANEL A. FULL SAMPLE				
Eligibility to UI benefits	0.00024 (0.0004)	0.00016 (0.0004)	0.00038 (0.00081)	0.00007 (0.00081)
Mean outcome at the cutoff	0.0037	0.0213	0.0026	0.0113
Effect relative to the mean	6.5%	0.8%	14.6%	0.6%
Observations	270,880	268,458	270,880	268,458
PANEL B. YOUNGER WORKERS, AGE ≤ 29				
Eligibility to UI benefits	0.00033 (0.00061)	0.000078 (0.00062)	-0.000082 (0.0012)	-0.0008 (0.0012)
Mean outcome at the cutoff	0.0043	0.0246	0.0028	0.0113
Effect relative to the mean	7.7%	0.3%	-2.9%	-7.1%
Observations	134,558	132,920	134,558	132,920
PANEL C. OLDER WORKERS, AGE ≥ 30				
Eligibility to UI benefits	0.00015 (0.00052)	0.00016 (0.00052)	0.00083 (0.0011)	0.0011 (0.0011)
Mean outcome at the cutoff	0.0031	0.0181	0.0024	0.0112
Effect relative to the mean	4.9%	0.9%	35.1%	9.8%
Observations	136,322	134,694	136,322	134,694
Controls	N	Y	N	Y

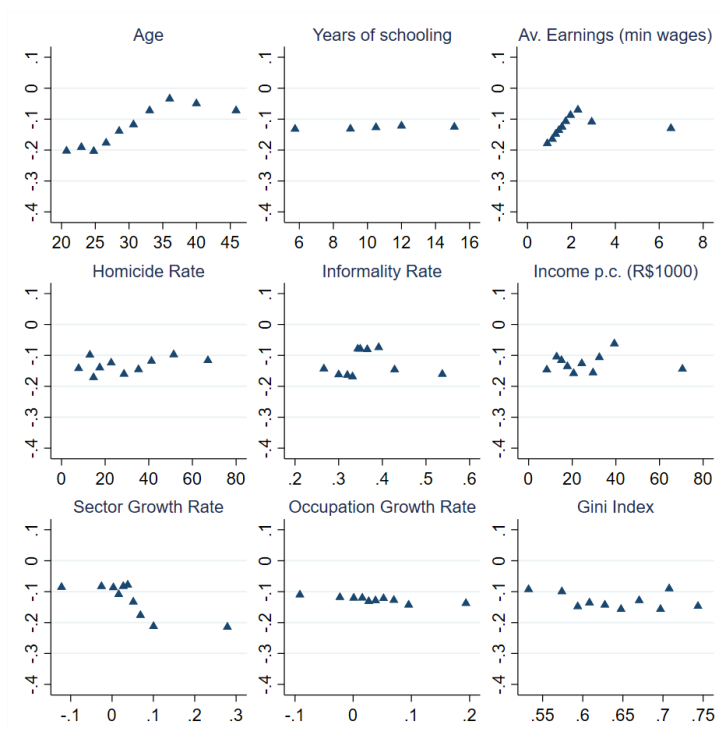
Notes: This table shows the effect of eligibility for UI benefits, as a placebo exercise, on the probability of being prosecuted for a crime within one semester and 3 years before layoff, as estimated from equation (3). The sample includes displaced workers with at least 6 months of continuous employment 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. Each panel estimates separate regressions for the different groups, as indicated in their title. The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. 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 (** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

C.4 The effect of UI on crime, heterogeneity

Figures C6 and C7 and Table C3 investigate the heterogeneity of RD estimates using causal forests, like we did in Section 4.4 for the effect of job loss. In this case, we cannot address heterogeneity by tenure, because workers included in the RD sample have by construction a similar (low) tenure. We focus on the narrow bandwidth of 45 tenure days around the cutoff, so that the unconfoundedness assumption in Wager and Athey (2018) is likely satisfied. The causal forest algorithm is then implemented without local polynomial controls.³²

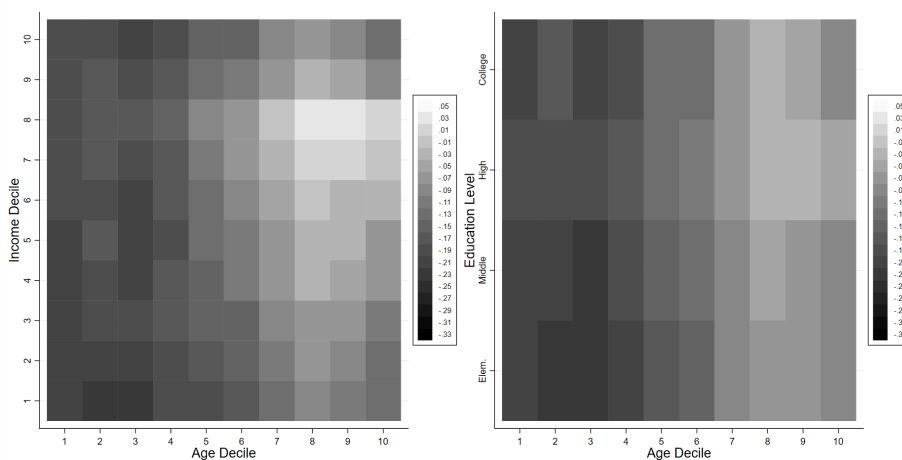
³²This approach is similar in spirit to the local randomization of Cattaneo et al. (2015, 2017).

Figure C6: Cond. Average Treatment Effects of UI eligibility, by characteristic



Notes: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over individual and municipality level characteristics. CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a causal forest. The causal forest is constructed within a narrow bandwidth of 45 days around the cutoff.

Figure C7: Cond. Average Treatment Effects of UI eligibility, by pair of characteristics



Notes: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over pairs of individual characteristics, namely income and age (left graph) and education and age (right graph). CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a causal forest. The causal forest is constructed within a narrow bandwidth of 45 days around the cutoff.

Table C3: Predicted Conditional Average Treatment Effect (CATE) - UI eligibility effect

	(1)	(2)	(3)	(4)
	Predicted Treatment Effects		Standardized difference	MHT p-value
	Below median	Above median	Diff (1)-(2)	Diff (1)-(2)
Age	28.3	33.5	-0.74	0.001
Education	10.4	10.5	-0.03	0.001
Earnings (min wages)	2.1	2.3	-0.08	0.001
Homicide rate	28.4	30.7	-0.13	0.001
Informality rate	0.36	0.36	0.01	0.001
Sector Growth - state level	0.066	0.021	0.05	0.001
Occupation Growth - state level	0.040	0.035	0.05	0.001
Pib per capita (R\$1000) - mun. level	27.1	27.0	0.01	0.112
Population - mun. level	1,768,035	1,730,149	0.01	0.025
Gini index - mun. level	0.64	0.63	0.19	0.001

Notes: This table compares individual and local level characteristics for workers with, respectively, above and below median Conditional Average Treatment Effect (CATE) of UI. CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a causal forest. Column 4 reports p-values testing for differences across groups, while accounting for multiples hypothesis testing, as in [List et al. \(2019\)](#). Below median CATE reflect a stronger reduction in crime rates caused by UI eligibility.

Age is the key gradient, as younger workers respond more to benefit eligibility than older workers. Such variable displays the largest standardized difference by far, and it ranks first in terms of importance, driving 20% of the endogenous sample splits. In addition, heat plots in Figure C7 show that the age gradient remains relevant when keeping constant income and education.

C.5 The effect of UI on crime, additional robustness checks

Our RD results showing that UI eligibility reduces crime rates in the semester after layoff, while UI benefits are being paid out, (Table 4 of Section 5.3) are confirmed when considering different bandwidths (including the optimal bandwidth according to [Calonico et al., 2014](#)) and controlling for different polynomial regressions in the running variable; see Table C4. The average effect on the total sample is marginally non-significant in some specifications, while the effect on younger individuals remains large and more precisely estimated, especially when controls are added, improving the precision of the estimates. We reach similar results when extending our main sample to workers with a unique name within each state (rather than within the entire country), as shown in Table C5. Finally, we show that our main findings remain

robust when dropping data from Minas Gerais and São Paulo, for which missing data in criminal records is high, above 70%. The results are presented in Table C6 and are based on the larger sample including all individuals with a unique name within each state so that statistical power is maximized. In turn, Figure C8 confirms that both the estimated effect on the main sample and on the subsample of younger workers are statistically different from placebo distributions obtained by running the same estimates on placebo cutoff dates.

Table C4: Effect of UI eligibility on crime one semester after layoff, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.: Criminal prosecution - 1 semester after layoff								
PANEL A. FULL SAMPLE								
Eligibility to UI benefits	-0.00048 (0.00032)	-0.00051* (0.00026)	-0.00065* (0.00036)	-0.00077* (0.00044)	-0.0005 (0.00037)	-0.00056* (0.00032)	-0.00046 (0.00045)	-0.00054 (0.0004)
Observations	137,526	204,616	102,676	270,880	399,542	521,255	275,313	729,187
PANEL A. FULL SAMPLE, WITH CONTROLS								
Eligibility to UI benefits	-0.00058* (0.00032)	-0.00055** (0.00027)	-0.00076** (0.00035)	-0.00080* (0.00045)	-0.00061* (0.00037)	-0.00061* (0.00033)	-0.00066 (0.00045)	-0.00064 (0.0004)
Observations	135,896	203,884	108,952	270,180	398,893	520,644	269,776	728,643
PANEL C. YOUNGER WORKERS, AGE ≤ 29								
Eligibility to UI benefits	-0.00079* (0.00048)	-0.0010*** (0.0004)	-0.00072 (0.00055)	-0.0013* (0.00067)	-0.00088 (0.00056)	-0.00097** (0.00049)	-0.0013* (0.00065)	-0.0012* (0.0006)
Observations	68,229	101,707	50,094	134,558	198,192	258,324	145,026	359,838
PANEL D. YOUNGER WORKERS, AGE ≤ 29 , WITH CONTROLS								
Eligibility to UI benefits	-0.00092* (0.00049)	-0.0011*** (0.00041)	-0.0010* (0.00053)	-0.0014** (0.00068)	-0.0011** (0.00056)	-0.0011** (0.00049)	-0.0012* (0.00069)	-0.0014** (0.0006)
Observations	67,000	100,924	56,080	133,784	197,430	257,598	134,955	359,143
Bandwidth (days)	30	45	CCT	60	90	120	CCT	180
Polynomial Order	0	0	0	1	1	1	1	2

Notes: This table replicates the regression discontinuity analysis in Table 4 for different specifications of the polynomial regression and different bandwidths (indicated on bottom of the table). The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. CCT denotes the optimal bandwidth according to Calonico et al. (2014).

Table C5: 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)	(8)
Dep. Var.: Criminal prosecution - 1 semester after layoff								
PANEL A. YOUNGER WORKERS, AGE \leq 29								
Eligibility to UI benefits	-0.00088** (0.00039)	-0.00078** (0.00033)	-0.00086** (0.00043)	-0.0014** (0.00057)	-0.00080* (0.00047)	-0.00082** (0.00041)	-0.0013** (0.00053)	-0.0013** (0.00051)
Observations	91,432	136,144	72,555	180,129	264,975	345,411	208,259	481,454
PANEL B. YOUNGER WORKERS, AGE \leq 29, WITH CONTROLS								
Eligibility to UI benefits	-0.00093** (0.0004)	-0.00082** (0.00034)	-0.0010** (0.00042)	-0.0016*** (0.00057)	-0.00089* (0.00047)	-0.00081** (0.00041)	-0.0012** (0.00054)	-0.0014*** (0.00051)
Observations	90,025	135,355	79,844	179,365	264,258	344,731	208,893	480,820
Bandwidth (days)	30	45	CCT	60	90	120	CCT	180
Polynomial Order	0	0	0	1	1	1	1	2

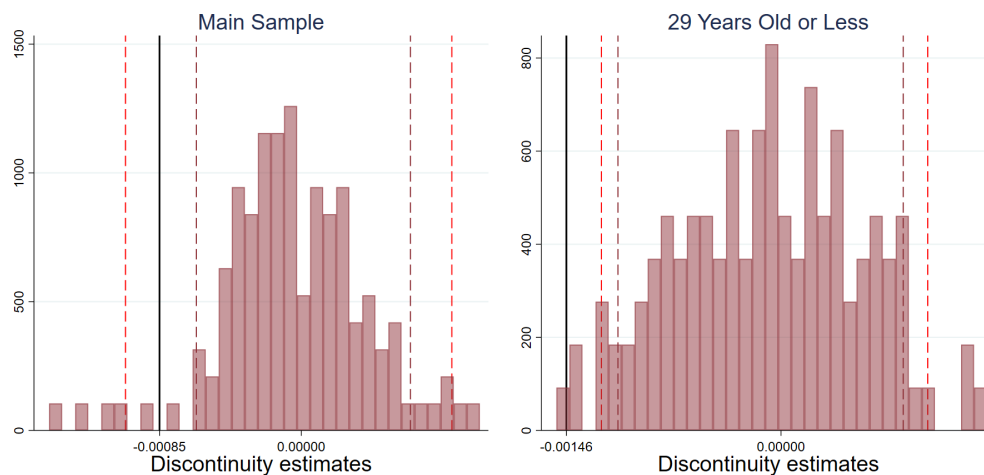
Notes: This table replicates the regression discontinuity analysis in Table 4 for different specifications of the polynomial regression and different bandwidths (indicated on bottom of the table). The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. The sample includes all individuals with a unique name within their state of work – rather than in the whole country, as in the sample used for the main analysis.

Table C6: Effect of UI Eligibility on crime, dropping states with high missing share, extended sample including all workers with a unique name within their state of residence

Dep. Var.:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	UI			Prob. criminal prosecution after:			
	Take-up	Payments	Amount	6 months	6 months	3 years	3 years
PANEL A. FULL SAMPLE							
Eligibility to UI benefits	0.57*** (0.0031)	2.55*** (0.013)	2006.6*** (11.1)	-0.00082 (0.00054)	-0.00094* (0.00055)	-0.00042 (0.0013)	-0.0014 (0.0013)
Mean outcome at the cutoff	0.07	0.1	116	0.0046	0.0046	0.0265	0.0265
Effect relative to the mean				-18.0%	-20.6%	-1.6%	-5.3%
Observations	231,235	231,235	231,235	231,235	229,237	231,235	229,237
PANEL B. YOUNGER WORKERS, AGE \leq 29							
Eligibility to UI benefits	0.58*** (0.0045)	2.55*** (0.019)	1925.0*** (15.4)	-0.0019** (0.00086)	-0.0021** (0.00087)	-0.0018 (0.0021)	-0.0032 (0.0021)
Mean outcome at the cutoff	0.06	0.1	98	0.0054	0.0054	0.0309	0.0309
Effect relative to the mean				-35.3%	-39.0%	-5.8%	-10.4%
Observations	109,258	109,258	109,258	109,258	107,960	109,258	107,960
Drop MG and SP	Y	Y	Y	Y	Y	Y	Y
Controls	N	N	N	N	Y	N	Y

Notes: This table shows the effect of eligibility for UI benefits, as estimated from equation (3), on UI outcomes (Columns 1-3) and the probability of being prosecuted for a crime within one semester and 3 years after layoff (Columns 4-7). The sample includes all individuals with a unique name within their state of work – rather than in the whole country, as in the sample used for the main analysis; and excludes data from the states of Minas Gerais and São Paulo, where the share of missing data on criminal outcomes is high, above 70%. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Each panel estimates separate regressions for the different groups, as indicated in their title. The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. 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 (*** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$).

Figure C8: Effect of UI eligibility on crime one semester after layoff, permutation tests



Notes: The graphs compare discontinuity estimates of the effect of UI eligibility on the probability of criminal prosecution one 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 (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in eq. (3).

In Table C7 we control for the cyclicalities in hiring/firing discussed in Section 5.2. We focus on the larger sample including all individuals with a unique name within each state which provides higher statistical power. 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 the end of calendar months, thus dropping our initial restriction. All regressions include individual controls and municipality fixed-effects which increase the precision of estimates. Both the average effect on the total sample and on the younger group are statistically significant in all specifications. Finally, Table C8 shows that our main results remain robust when using the inference method proposed by Gerard et al. (2020), which estimates effect bounds while allowing for some degree of manipulation in the RD running variable.

Table C7: Effect of UI eligibility on crime one semester after layoff, robustness to cyclical in hiring and firing of workers, extended sample including all workers with a unique name within their state of residence

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.: Criminal prosecution - 1 semester after layoff						
PANEL A. FULL SAMPLE						
Eligibility to UI benefits	-0.00070*	-0.00071*	-0.00069*	-0.00068*	-0.00055*	-0.00057*
	(0.00037)	(0.00037)	(0.00037)	(0.00038)	(0.00031)	(0.00031)
Mean outcome at the cutoff	0.0034	0.0034	0.0034	0.0034	0.0033	0.0033
Effect relative to the mean	-20.6%	-20.9%	-20.3%	-20.0%	-16.6%	-17.2%
Observations	367,064	367,064	367,061	367,061	505,448	505,443
PANEL B. YOUNGER WORKERS, AGE ≤ 29						
Eligibility to UI benefits	-0.00155***	-0.00143**	-0.00148**	-0.00138**	-0.00094*	-0.00089*
	(0.00057)	(0.00058)	(0.00058)	(0.00058)	(0.00049)	(0.00049)
Mean outcome at the cutoff	0.0039	0.0039	0.0039	0.0039	0.0037	0.0037
Effect relative to the mean	-39.3%	-36.2%	-37.5%	-35.0%	-25.3%	-23.9%
Observations	178,183	178,183	178,181	178,181	244,511	244,508
Dismissal date FE		X		X		X
Cutoff date FE			X	X		X
Controls	X	X	X	X	X	X
Sample	Main	Main	Main	Main	Extended	Extended

Notes: This table replicates the regression discontinuity analysis in Table 4 when including fixed effects for dismissal and cutoff dates, and when including all dismissal and cutoff dates within each month. The sample and specification are indicated on bottom of the table. The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. The sample includes all individuals with a unique name within their state of work – rather than in the whole country, as in the sample used for the main analysis.

Table C8: Effect of UI eligibility on crime one semester after layoff, manipulation inference

	(1)	(2)	(3)	(4)
Dep. Var.: Prob. Prosecution	FULL SAMPLE		YOUNG WORKERS	
6 months after layoff			AGE ≤ 29	
	Estimate	C.I.	Estimate	C.I.
PANEL A. MAIN ESTIMATES				
Share always assigned	0.035		0.052	
ITT: Ignoring manipulation	-0.00077	[-0.00164,0.00011]	-0.00131	[-0.00261,-0.00001]
ITT: Bounds inference	[-0.0037,-0.00066]	[-0.00427,0.0001]	[-0.00429,-0.00115]	[-0.00512,-0.00002]
PANEL B. HYPOTHETICAL SHARE OF MANIPULATION				
Share always assigned	0.025	[-0.0037,-0.00069]	[-0.00425,0.00005]	[-0.00429,-0.00123]
	0.05	[-0.0037,-0.00061]	[-0.00425,0.00014]	[-0.00429,-0.00115]
	0.1	[-0.0037,-0.00044]	[-0.00425,0.00033]	[-0.00429,-0.00098]
	0.15	[-0.0037,-0.00025]	[-0.00425,0.00054]	[-0.00429,-0.00078]
	0.2	[-0.0037,-0.00003]	[-0.00425,0.00078]	[-0.00511,0.00065]

Notes: This table shows discontinuity estimates in prosecution rates after layoff, while allowing for manipulation in treatment assignment around the 16-month cutoff for UI eligibility, using the estimator proposed by Gerard et al. (2020). Panel A presents estimates ignoring manipulation and bounds based on the estimated manipulation share in the running variable density. Panel B presents bounds estimates for hypothetical shares of manipulation.

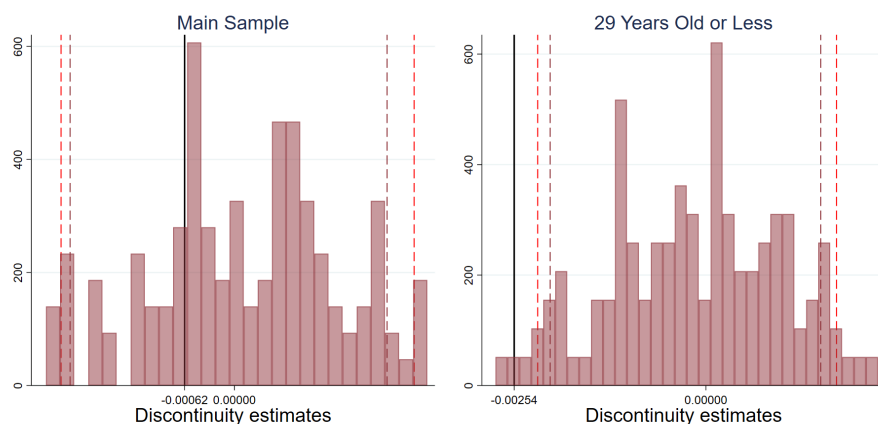
Table C9 shows the robustness for the effect of UI eligibility on crime up to 3 years after the layoff in the sample of younger workers. While results are not always statistically significant in the main specification, they are robust when including individual controls and municipality fixed effects. Figure C9, right panel, shows that the impact on younger workers is also robust to randomization inference. Overall, although the evidence is not particularly strong, it indicates that the crime reducing effects of UI eligibility in the semester following displacement do not bounce back over time.

Table C9: Effect of UI eligibility on crime three years after layoff, robustness to different specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.: Criminal prosecution - 3 years after layoff								
PANEL A. YOUNGER WORKERS, AGE \leq 29								
Eligibility to UI benefits	-0.0023**	-0.0021**	-0.0021	-0.0025	-0.002	-0.0020*	-0.0021	-0.0021
	(0.0012)	(0.00096)	(0.0013)	(0.0017)	(0.0014)	(0.0012)	(0.0016)	(0.0015)
Observations	68,229	101,707	59,105	134,558	198,192	258,324	161,671	359,838
PANEL B. YOUNGER WORKERS, AGE \leq 29, WITH CONTROLS								
Eligibility to UI benefits	-0.0030**	-0.0027***	-0.0028**	-0.0036**	-0.0027**	-0.0024**	-0.0028*	-0.0029**
	(0.0012)	(0.00096)	(0.0013)	(0.0017)	(0.0014)	(0.0012)	(0.0015)	(0.0015)
Observations	67,000	100,924	49,746	133,784	197,430	257,598	159,658	359,143
Bandwidth (days)	30	45	CCT	60	90	120	CCT	180
Polynomial Order	0	0	0	1	1	1	1	2

Notes: This table replicates the regression discontinuity analysis in Table 4 for different specifications of the polynomial regression and different bandwidths (indicated on bottom of the table). The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. CCT denotes the optimal bandwidth according to [Calonico et al. \(2014\)](#).

Figure C9: Effect of UI eligibility on crime three years after layoff, permutation tests



Notes: The graphs compare discontinuity estimates of the effect of UI eligibility on the probability of criminal prosecution three years 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 (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95 and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in eq. (3).

C.6 The effect of UI on crime, additional results

Table C10: Effect of UI eligibility on employment

Dep. Var.: Employment Outcomes	(1)	(2)	(3)	(4)
	Semester 1			Unemployment
	Months	Income	Employed	Duration
PANEL A. FULL SAMPLE				
Eligibility to UI benefits	-0.89*** (0.018)	-1086.4*** (36.9)	-0.17*** (0.0037)	8.37*** (0.34)
Mean outcome at the cutoff	2.27	3048	0.66	33.2
Effect relative to the mean	-39.3%	-35.6%	-25.7%	25.2%
Observations	270,880	270,880	270,880	270,880
PANEL B. YOUNGER WORKERS, AGE ≤ 29				
Eligibility to UI benefits	-0.92*** (0.026)	-989.8*** (40.4)	-0.17*** (0.0053)	8.58*** (0.46)
Mean outcome at the cutoff	2.29	2753	0.67	31.7
Effect relative to the mean	-40.1%	-36.0%	-25.5%	27.1%
Observations	134,558	134,558	134,558	134,558
PANEL C. OLDER WORKERS, AGE ≥ 30				
Eligibility to UI benefits	-0.86*** (0.026)	-1178.6*** (61.5)	-0.16*** (0.0053)	8.18*** (0.5)
Mean outcome at the cutoff	2.24	3341	0.66	34.8
Effect relative to the mean	-38.4%	-35.3%	-24.4%	23.5%
Observations	136,322	136,322	136,322	136,322

Notes: This table shows UI eligibility effects on employment outcomes, as estimated from eq. (3). 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. The local linear regression includes a dummy for eligibility for UI benefits, time since the cutoff date for eligibility, and the interaction between the two. Standard errors are clustered at the individual level and displayed in parentheses (***) $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$). Labor income is measured in Brazilian Reais; and unemp. dur. is censored at 36 months.