# THE EFFECT OF JOB LOSS AND UNEMPLOYMENT INSURANCE ON CRIME IN BRAZIL

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We investigate the impact of job loss on crime and the mitigating role of unemployment benefits, exploiting detailed individual-level data linking employment careers, criminal records, and welfare registries for the universe of male workers in Brazil. The probability of committing crimes increases on average by 23% for workers displaced by mass layoffs, and by slightly less for their cohabiting sons. Using causal forests, we show that the effect is entirely driven 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 vanishes completely immediately after benefit expiration. Our findings point to liquidity constraints and psychological stress as the 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.

KEYWORDS: Unemployment, crime, unemployment insurance, registry data.

## 1. INTRODUCTION

CRIME IMPOSES A HEAVY BURDEN ON SOCIETIES, especially during economic downturns, as unemployment and limited earning opportunities reduce the opportunity cost of committing crimes (Becker (1968)). In a related effect, 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, Devereux, and Salvanes (2015), Schaller and Huff 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.

We study the relationship between employment and criminal behavior by exploiting detailed registry data on the universe of (male) workers in Brazil over the 2009–2017 period. Specifically, our data set 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. We exploit these data to estimate the effect of job loss on the probability of committing crime and the mitigating effect of unemployment benefits. The size and richness of our data set allow us to characterize the heterogeneity of treatment effects across individuals, their timing around the payment and exhaustion of unemployment benefits, and the spillover effects on other household members. Our results provide novel insights into the mechanisms driving the response of criminal behavior to job loss.[1]

In the first part of the paper, we estimate dynamic treatment effects of job displacement by comparing the criminal behavior of workers displaced by mass layoffs, before and after displacement, with a matched control group of workers who were not displaced in the same year. Mass layoffs 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 (Jacobson, LaLonde, and Sullivan (1993), Couch and Placzek (2010)) and mortality (Sullivan and Von Wachter (2009)). In addition, the scope of our data set 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.

We find 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 4 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 does not change 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 intended to minimize 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), Wager and Athey (2018), Athey, Tibshirani, and Wager (2019)). 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, which is remarkable given the wide heterogeneity in local socioeconomic 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 lower accumulated savings and are entitled to less generous severance pay and unemployment benefits. At the same time, as a group they differ from other workers in many dimensions, including baseline crime rates.

To shed more light on the mechanisms and, in addition, understand the effectiveness of alternative policy remedies, we then examine the effect of unemployment insurance (UI),

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[1]Our main data tracks formal employment. Unless when stated otherwise, we use *employment* and *income* to refer to *formal employment* and *formal income* throughout the paper. In Section 2.2 and in Appendix B.2 of the Online Supplementary Material (Britto, Pinotti, and Sampaio (2022)), we quantify the empirical relevance of transitions to informal work after job loss and discuss their implications for interpreting our results.

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 5 months—quite similar to most US states. Importantly, 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 5 months of benefits—using a clean regression discontinuity design.[2]

We find that the crime rate in the first 6 months after layoff is 21% lower for marginally eligible workers compared with marginally noneligible ones. The average effect of unemployment benefits completely offsets the potential increase in crime upon job loss. However, this effect is transitory and vanishes immediately when benefits expire.

These results suggest that UI policies may attenuate the impact of job loss on crime. They also help us to distinguish 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, for example, 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, those eligible for UI should commit more crime than the noneligible, while the opposite result holds true in our data.

Instead, our results support economic explanations, primarily liquidity constraints, which are consistent with the strong but transitory effect of unemployment benefits.[3] 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, whose consumption drops suddenly when benefits are exhausted (Gerard and Naritomi (2021)). The spillover effect on cohabiting sons is also consistent with the importance of liquidity constraints and inconsistent with time substitution. Instead, the spillover effect *cannot* be explained by changes in the opportunity costs of committing crimes, because 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 earlier papers rely on variation across geographical areas (e.g., regions or provinces within a country) and identify the causal effect of unemployment on crime using Bartik-type instruments that interact national-level shocks with local economic characteristics (see, among others, Raphael and Winter-Ebmer (2001), Gould, Weinberg, and Mustard (2002), Öster and Agell (2007), Fougère, Kramarz, and Pouget (2009), Dix-Carneiro, Soares, and Ulyssea (2018), Dell, Feigenberg, and Teshima (2019)). These studies generally conclude that local crime rates increase with unemployment. However, variation across local areas only provides limited

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[2]Gerard, Rokkanen, and Rothe (2020) exploit the same research design with data from earlier years to study the effect of UI eligibility 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.

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

insights into the mechanisms through which unemployment affects criminal behavior and the mitigating effect of unemployment benefits or other social safety nets, because the rules determining eligibility for such benefits are typically constant across geographical areas. In addition, average data across local areas may not be sufficiently powered to detect precisely the determinants of a relatively rare event such as criminal activity: even in high-crime countries, offenders constitute a very minor fraction of the total population. This is particularly true for severe crimes such as murders, which are much more rare than petty property crimes.

We address these issues by leveraging individual-level administrative data and recent advancements in econometric methods. Our paper is closest to four recent papers that use 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, Skardhamar, Telle, and Votruba (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, Medina, Nyshadham, Posso, and Tamayo (2021)).[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 data set, coupled with recent advancements in the use of causal forest algorithms for the analysis of heterogeneous treatment effects, allow us to precisely estimate the distribution of treatment effects conditional on a wide array of individual and local characteristics. These results shed light on the prominent influence of individual characteristics, notably age and tenure, over local socioeconomic 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 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 regions for which similar data are typically available—notably, European countries and US states. Overall, our results 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.

Our paper is organized as follows. In the next section, we provide some context for our empirical investigation, and in Section 3 we describe the data and preliminary evidence. In Sections 4 and 5, we present the main results on the impact of job loss and UI on crime; additional robustness checks are in Appendices B and C in the Online Supplementary Material. In Section 6, we discuss the mechanisms driving the results, and the conclusion is in Section 7.

## 2. INSTITUTIONAL BACKGROUND

Latin America is the most violent continent in the world, and Brazil is one of the most violent countries within the continent. In 2017, the homicide rate—the only crime statis-

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[4]Prior to these recent contributions, Witte and Dryden (1980) and Schmidt and Dryden Witte (1989) used individual-level data on former prison inmates in North Carolina to study the determinants of recidivism (including employment). However, it is not possible with their approach to identify causal effects.
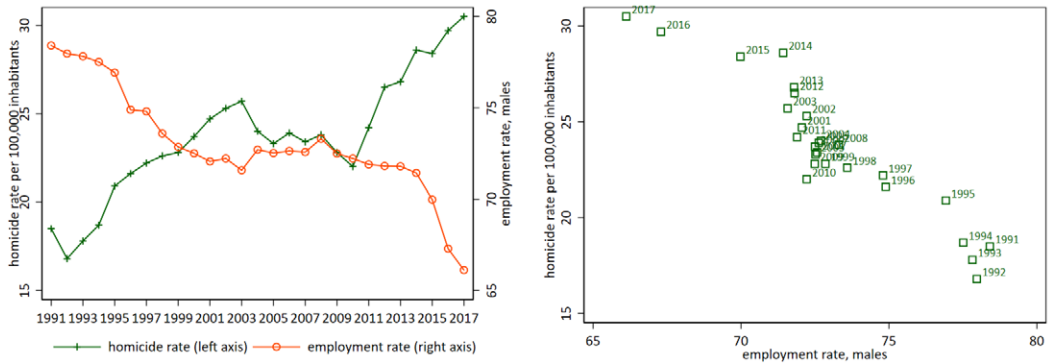
FIGURE 1.—Homicides and employment in Brazil, 1991–2017. *Notes*: The left graph shows the evolution of the homicide rate per 100,000 inhabitants (left vertical axis) and the male employment rate (right vertical axis) in Brazil over the 1991–2017 period. The right graph plots the relationship between the two variables over time.

tic 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.

Between 1991 and 2017, the homicide rate increased from 18.5 to 30 homicides per 100,000 inhabitants (+62% over the baseline); interestingly, male employment decreased from 78 to 66% (−16% over the baseline) during the same period. More generally, the homicide rate has closely tracked labor market downturns since the 1990s (see Figure 1).

This preliminary evidence at the aggregate level is consistent with a high elasticity of crime to employment. 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 outbreaks of violence affect the level of economic activity. To isolate the causal effect of employment downturns on crime and to understand the mechanisms driving such a relationship, we exploit mass layoff shocks and compare criminal prosecutions over time between displaced and nondisplaced workers, and between displaced workers who are eligible and noneligible for unemployment benefits. To this end, we first describe the judicial system and labor market regulations in Brazil.

### 2.1. *Criminal Justice*

The judicial system comprises 27 state courts and 2697 tribunals, and each tribunal has jurisdiction over one or more of the 5570 Brazilian municipalities. Criminal investigations are conducted by state judiciary police, either on their own initiative or upon request from the Public Prosecutor's Office or crime victims. Once an investigation is concluded, files are sent to the Prosecutor, who decides whether to press charges. Even if the prosecutor decides not to press charges following the investigation, a new court case is filed since the decision not to pursue a case 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 just cause by paying dismissal indemnities, and 93% of all contracts in the private sector are open-ended and full-time. Dismissals without just cause account for two-thirds of job separations, while the remaining one-third are voluntary resignations; our analysis focuses on the former. Dismissed workers are entitled to a mandatory savings account, into which employers make monthly contributions amounting to 8% of the worker's compensation. Workers can only access these funds in case of a dismissal without just cause, and they are also entitled to a severance payment equal to 40% of the account balance. The sum of these payments amounts to approximately 1.36 times their monthly wages per tenure year upon layoff.

Although labor informality is high—accounting for roughly 45% of all jobs in 2012—the formal and informal labor markets are tightly connected, as workers tend to move frequently between formal and informal jobs. Due to the lack of administrative data on informal jobs, we mostly focus on workers exiting formal jobs. At the same time, we estimate the share of workers returning to informal jobs using survey data, and we take it into consideration when interpreting the magnitude of our estimates—particularly the estimated elasticity of crime to (formal) income.

Unemployment insurance is the main policy for assisting displaced workers. It is restricted to workers dismissed without just cause and lasts from 3 to 5 months, depending on the period 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 the "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 data set 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

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[5]The RAIS data have been extensively used in previous research on the Brazilian labor market; see, for example, Ferraz, Finan, and Szerman (2015), Gerard and Gonzaga (2021), and Dix-Carneiro, Soares, and Ulyssea (2018).

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 2009–2017 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, we can observe the start and termination date, court location, and one or more tags on the subjects being litigated. The defendant(s) and plaintiff(s) are identified by their full name(s).

The name of the defendant or defendants is available for 8 million criminal cases out of 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 make an exception in specific instances established by the law. These exceptions typically involve specific types of suits such as sexual offenses and domestic violence, and cases involving individuals under the legal age (18). For this reason, we exclude such offenses from our analysis. As for the other types of crime, it is unlikely that missing data in our records is related to the defendant's job status—our main explanatory variable of interest—for the following reasons. First, the threat of dismissal is not a valid motive for invoking secrecy; in fact, ongoing criminal prosecutions do not constitute a just cause for worker's dismissal by firms—this only applies to definitive criminal convictions. Second, requests for secrecy generally take place after the case has already started, while our data capture the identity of the defendant as long as the case is started without secrecy. Third, for the specific case of offenders arrested *in flagrante*, that is, caught in the act of committing a 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 data set reports only the initial date of the prosecution case rather than the date of the alleged offense. However, prosecution starts immediately for offenders arrested *in flagrante*, because a judge must decide whether to detain the defendant while awaiting trial. For this subset of cases, we can precisely measure the timing of the 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 data set, 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 threats. Some of the latter crimes may be

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[6]Insofar as some workers actually have a longer notice period, setting the separation date equal to the minimum notice period is a conservative choice for the purpose of testing the parallel trends assumption underlying our difference-in-differences design. In practice, given the high job turnover, 37% and 90% of the workers in our sample are dismissed with less than 1 and 3 years in the job, thus having a notice period between the 30 and 39 days, respectively.

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, damage to private property, environmental crime, conspiracy, lynching, and racial offenses.[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 data sets.[8] To minimize errors, we restrict the analysis to individuals who have unique names. This applies to about half of the adult population, because Brazilians typically have multiple surnames, with at least one surname from each parent. To identify citizens with a unique name, we create a registry of individuals by merging the RAIS data with the *Cadastro Único* (CadUnico), a data set maintained by the Ministry of Development for the administration of all federal social programs.

This 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. We restrict attention to individuals who have a unique name and merge the court data to the employment records by exactly matching on names. Columns (1)–(3) of Appendix Table A.II of the Online Supplementary Material compare the characteristics of dismissed workers with and without unique names, respectively. There are slight differences between the two groups, 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 0.25 for all variables. 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 national population increases to 70% and differences in observable characteristics relative to the general population are further reduced (columns 4–6 of Table A.II).

### 3.3. *Descriptive Evidence*

Figure 2 shows how the average probability of criminal prosecution varies between displaced and nondisplaced workers, by age and tenure. We focus on workers employed between 2011 and 2015, allowing us to track criminal behavior in the 2 years before and after being displaced. As in our main analysis, the sample comprises male, full-time workers in the nonagriculture 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

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[7]Table A.I in the Appendix of the Online Supplementary Material reports the share of each crime category among all offenses as well as the share of offenders arrested *in flagrante* by crime category.

[8]Throughout the paper, we refer to "name" as the person's full name, that is, the name-surname(s) combination.
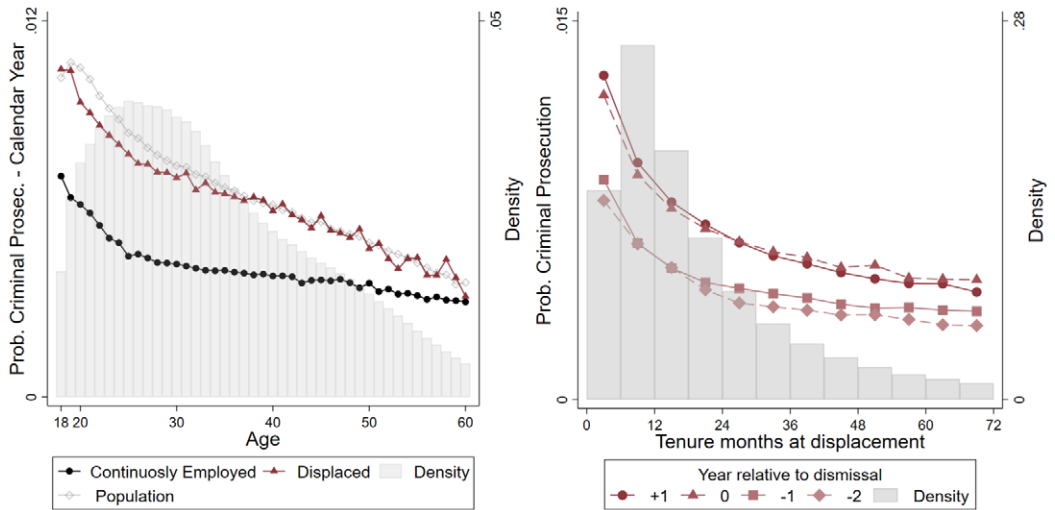
FIGURE 2.—Criminal prosecutions by employment status, age, and job tenure. *Notes*: The left graph compares the average probability of being prosecuted in a given year between workers who are continuously employed, workers losing their job in that year, and individuals in the population irrespective of their employment status, by age. The right graph shows the probability of being prosecuted among displaced workers in the first 2 years after dismissal and in the last 2 years before dismissal, by tenure. The distributions of age and tenure for employed individuals are also shown in the graphs.

progressively for workers displaced at older ages. The high prosecution rate for displaced workers is comparable to that for the overall population—which includes individuals who left formal jobs in the previous calendar years, who work informally, and who are out of the labor force, which is shown by the gray line.

The graph on the right in Figure 2 focuses on crime outcomes of displaced workers 2 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 from the job within less than a year. 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 2 years preceding the layoff, and increases in the 2 years following the job loss.

Of course, these differences in criminal behavior by employment status 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 nonagricultural, 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 4 years after displacement, and placebo effects

up to 3 years before displacement.[9] The pool of candidate control workers comprises all individuals employed in firms not experiencing mass layoffs during our period of analysis. 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), and state (27), and has the same job tenure. When treated workers match with multiple controls, one control unit is randomly selected.[10]

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 on staggered difference-in-differences designs—particularly, the presence of negative weights attached to some treated units when averaging heterogeneous treatment effects in typical two-way fixed effects regressions.[11]

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

$$Y_{it} = \alpha + \gamma \text{Treat}_i + \sum_{t=-P}^{T} \delta_t (\text{Treat}_i * \text{Time}_t) + \sum_{t=-P}^{T} \lambda_t \text{Time}_t + \epsilon_{it}. \tag{1}$$

Workers are identified by the subscript $i$, and $\text{Treat}_i$ is a dummy indicating that the worker belongs to the treatment group. $\text{Time}_t$'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 $\{\delta_1, \ldots, \delta_T\}$ thus identify dynamic treatment effects, whereas $t = 0$ is the baseline omitted period and $\{\delta_{-P}, \ldots, \delta_{-1}\}$ estimate anticipation effects.[12] Finally, $\text{Time}_t$ 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 $\text{Time}_t * \text{Mun}_{j(i)} * \text{Ind}_{k(i)}$, where $\text{Mun}_{j(i)}$ and $\text{Ind}_{k(i)}$ are fixed effects for the municipality (5565) and two-digit industry (87) where the $i$th worker is employed at time $t = 0$. These are finer categories with respect to states and one-digit industrial sectors (27 and 9 categories, resp.) that we employ to match treated subjects and controls. Comparing the results obtained when we include and exclude this additional set of fixed effects thus reveals the capacity of our approach to eliminate the effect of confounding shocks at the local level. To summarize the average treatment effect over all

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[9]Given that our data on prosecutions cover offenders above the legal age (18), we focus on the 20–50 age range so that we observe criminal behavior for at least 2 years before the layoff.

[10]In the baseline specification, control workers are not dismissed in the matching year but may be dismissed in subsequent years. We show that results are robust to including only control workers who are continuously employed throughout the entire sample period. Previous papers have used either of these two approaches; for instance, Ichino, Schwerdt, Winter-Ebmer, and Zweimüller (2017) and Schmieder, von Wachter, and Bender (2018) define the control group similar to our baseline setting, while Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010) restrict the control group to workers who are continuously employed through the whole period.

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

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

periods, we also estimate the equation:

$$Y_{it} = \alpha + \gamma \text{Treat}_i + \beta(\text{Treat}_i * \text{Post}_t) + \lambda \text{Post}_t + \epsilon_{it}, \tag{2}$$

where the dummy $\text{Post}_t$ 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 pretreatment 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 pretreatment trends in criminal prosecutions. To overcome this issue, we restrict the analysis to mass layoffs, defined as firms with at least 15 workers dismissing 33% or more of the workforce within a year.[13] These layoffs typically depend on negative external shocks at the firm level, rather than the characteristics and behavior of dismissed workers (see, e.g., Gathmann, Helm, and Schönberg (2020)). As we will show, our findings are extremely robust to a wide range of stricter mass layoff definitions, both in terms of the minimum share of displaced workers and firm size.

Appendix Table B.I in the Online Supplementary Material 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 remains below 0.25 for all variables except education in the mass layoff sample. The noticeable gap in the probability of a criminal prosecution prior to displacement—28% (25%) lower in the control group when considering all (mass) layoffs—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 worker 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 exhibit the same trends (not necessarily the same levels) in the absence of treatment, one could worry that the control group does not provide an adequate counterfactual in light of the level gap. In Appendix B.3 of the Online Supplementary Material, we address this concern by showing that our results are stable under alternative definitions of treated and control groups, for which the gap in levels also essentially vanishes.

Finally, Appendix Table B.II in the Online Supplementary Material 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.

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[13]We exclude from mass layoffs firms re-forming under a new ID. In line with the literature, we assume that firms reform under a new ID 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.

### 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 expressed
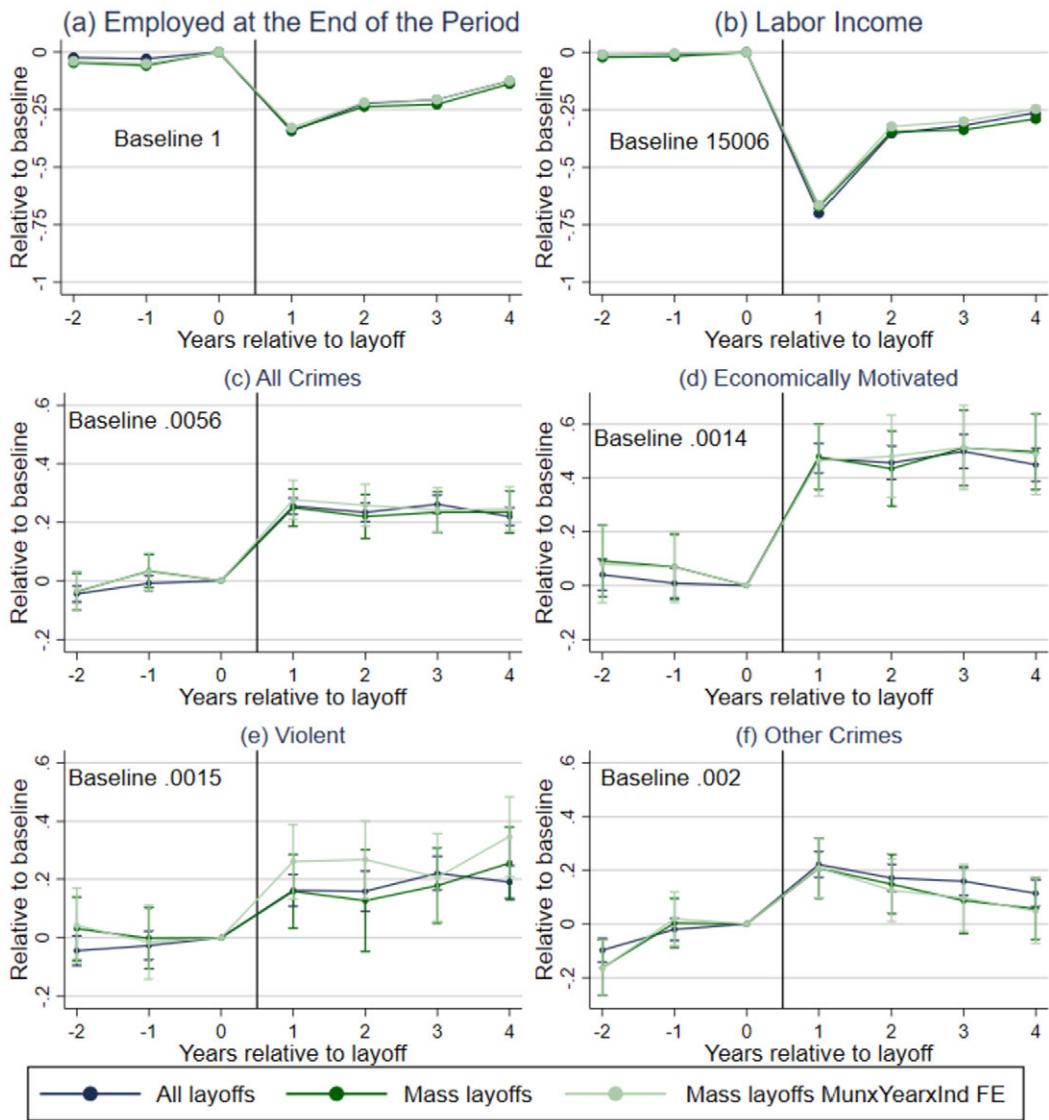


FIGURE 3.—Effect of job loss on employment and criminal prosecution probabilities. *Notes*: This figure shows the effect of job loss on employment outcomes and the probability of being prosecuted for different types of crime, as estimated from the difference-in-differences equation (1)—along with 95% 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 nonmass 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, that is, $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. Income variables are measured in Brazilian Reais.

in terms of relative differences from the baseline outcome of the treatment group at time $t = 0$.

In all graphs, the difference in outcomes between treatment and control groups is stable in the predisplacement 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 very slowly; four years after dismissal, treated workers still experience 13% lower employment rates and 26% less income.[14] Panels (c)–(f) of Figure 3 show that job loss drives a sharp increase in the probability of committing (different types of) criminal offenses. Appendix Figure B7 further shows that both the incidence of first-time offenders and the probability of reoffending increase upon job loss. While the impact on first-time prosecution peaks immediately in year 1, and slowly decreases 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 policy interventions to mitigate the impact of job loss on crime should act promptly after job loss. In all of these graphs, 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 Table I, we quantify the average effect of job loss over the 4 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) earnings. This is clearly not the case, as the effect could

TABLE I

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 |
| $\text{Treat}_i \times \text{Post}_t$ | −0.20 | −5710.0 | 0.0012 | 0.00060 | 0.00025 | 0.00032 |
| | (0.002) | (53.3) | (0.0001) | (0.00006) | (0.00006) | (0.00006) |
| Mean outcome, treated at $t = 0$ | 1 | 14,340 | 0.0052 | 0.0014 | 0.0015 | 0.0018 |
| Effect relative to the mean | −20% | −40% | 23% | 43% | 17% | 18% |
| Implied elasticity to earnings | | | −0.58 | −1.08 | −0.42 | −0.45 |
| Observations | 16,349,844 | 16,349,844 | 16,349,844 | 16,349,844 | 16,349,844 | 16,349,844 |

*Note*: 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 $\text{Treat}_i$ that is equal to 1 for displaced workers, interacted with a dummy $\text{Post}_t$ equal to 1 for the period after displacement. The sample includes workers displaced in mass layoffs who are matched to control workers employed in nonmass layoff firms, who are not displaced in the same calendar year. All regressions include on the right-hand side $\text{Treated}_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

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

go through other mechanisms such as leisure time, psychological stress, and so on (we discuss such mechanisms 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 B.VII in the Online Supplementary Material confirms (in line with the visual evidence in Figure 3) that results are virtually unaffected when extending the sample to all layoffs and when adding a full set of municipality × industry × year fixed effects, and the estimated magnitude increases when restricting the control group to include only workers who are continuously employed during the sample period.[15]

The fact that our estimates remain similar when including all layoffs or restricting to mass layoffs suggests that spillovers and social multipliers in criminal activity across coworkers do not have an effect in this specific context. To gather more evidence, we replicate the analysis splitting the sample by quartiles of the 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 nonstatistically significant gradients in all these dimensions.[16]

In columns 4–6 of Table I, we distinguish between different categories of offenses. The effect is mainly driven by economically-motivated crimes (+43% over the baseline), but 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 estimate precisely 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%, respectively. Violent crimes also respond strongly, with homicides increasing by 32%. This finding suggests that job loss may affect criminal behavior through more than just an economic channel, although we
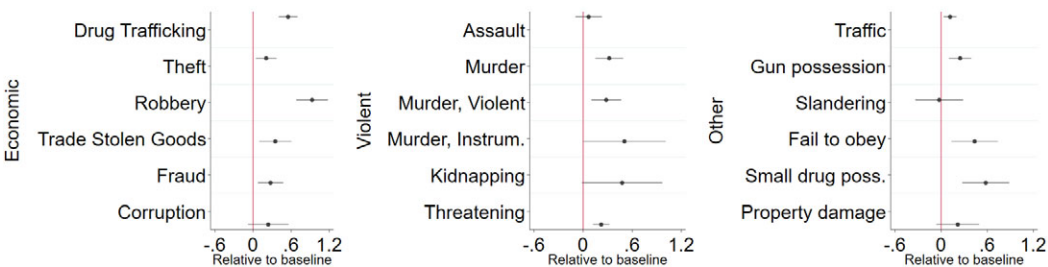


FIGURE 4.—Effect of job loss on different types of crime. *Notes*: The graphs show the estimated effect (and confidence interval) of job loss on different types of crime, as estimated from the difference-in-differences equation (2) and rescaled by the average outcome in the treatment group at $t = 0$.

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[15]To the extent that some of the displaced workers may transit to the informal sector, we overestimate the magnitude of the drop in employment and earnings upon job loss and underestimate, 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 when allowing for transitions into the informal labor market, so the estimates in Table I represent a conservative lower bound to the elasticity of crime to earnings.

[16]There is only some weak evidence that very large mass layoffs in the upper quartile yield slightly larger effects.

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 noninstrumental homicides, we approximate the former by prosecutions for multiple offenses (including at least one homicide) and the latter by stand-alone prosecutions. Although instrumental homicides respond more strongly, both effects are statistically significant and quantitatively relevant.

Overall, job loss affects almost all types of crime, including some that clearly have no economic motivation (e.g., traffic violations and failure to obey). These findings suggest that noneconomic factors such as psychological stress may also contribute to 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 nonmass layoffs). In this sample, the effect is statistically significant and sizable for all types of crime, including property damage and slandering (+24% and +14%, resp.), which are arguably unrelated to economic motives.

### 4.3. *Measurement of Criminal Behavior and Effect Timing*

Using judicial prosecutions as a measure of criminal behavior has two main limitations. First, a large number of crimes are not reported or, if they are, the (suspect) offender is not identified. This is a typical measurement issue in the 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 toward zero but the relative effect and the implied elasticity to earnings would be unaffected. In practice, the extent of underreporting 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 pretreatment 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 prosecuted *immediately*, so the prosecution date is informative about the timing of the crime. Figure 5 compares the results obtained when including prosecutions of all crimes (left graph) and only crimes apprehended *in flagrante* (right graph). We conduct this comparison at monthly frequencies to detect even minor deviations from parallel trends in the pretreatment period. However, no such deviation emerges, irrespective of whether we include all prosecutions or only prosecutions initiated for crimes apprehended *in flagrante*. Although the latter represent only a minor fraction of all prosecutions (see Table A.I in the Appendix), they increase more strongly upon layoff (+134%). This may be partly 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).

In Appendix B.3, we implement 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
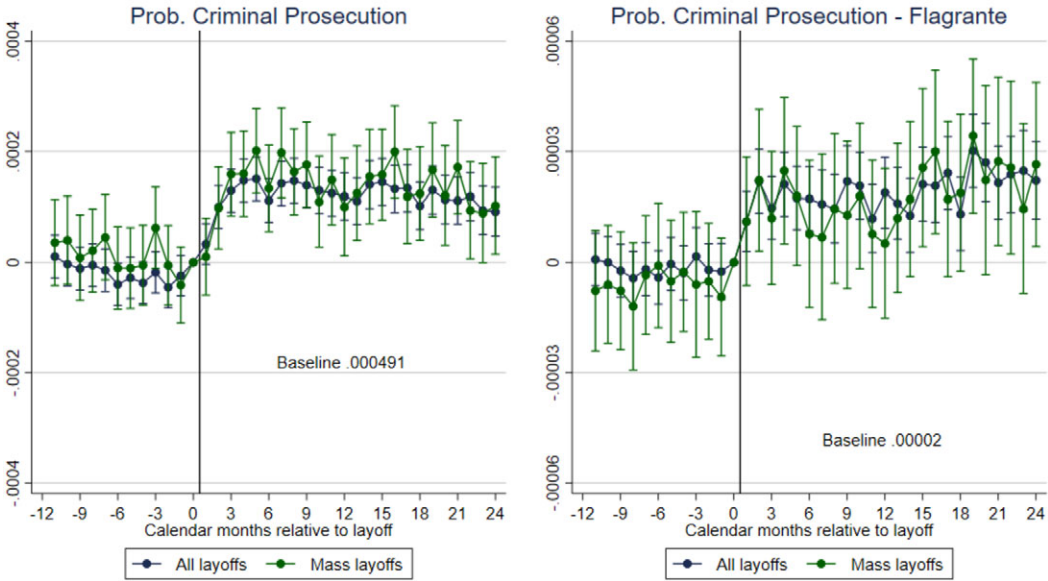
FIGURE 5.—Effect of job loss on all prosecutions and prosecutions *in flagrante*, monthly-level analysis. *Notes*: This figure shows the effect of job loss on the probability of being prosecuted (left graph) and being prosecuted for a crime apprehended *in flagrante* (right graph), estimated by equation (1) at a monthly frequency, along with 95% confidence intervals. The treatment group comprises workers displaced at time 0 in mass and nonmass layoffs, while the control group is defined via matching among workers in nonmass layoff firms who are not displaced in the same calendar year.

selection among displaced workers. Second, we implement an alternative intention-to-treat approach in which we compare all workers in mass layoff firms and in (matched) nonmass layoff firms. In both cases, the estimated crime elasticity remains unaffected.[17]

### 4.4. *Heterogeneous Treatment Effects Using Causal Forests*

The average effects presented in Table I mask significant heterogeneity in criminal behavior across individuals. The unusual size and richness of our data set provides a unique opportunity for characterizing this heterogeneity using causal forest estimators (Athey and Imbens (2016), Wager and Athey (2018), Athey, Tibshirani, and Wager (2019)). 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 nonlinearities 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

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[17]In 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 nonmissing shares of names in each state, baseline rates and absolute coefficients on general crime would be 1.96 times larger, while relative effects would not change.

and state sector, municipality-level homicide rates, informality rate, GDP per capita, population, and Gini index of income inequality).[18] 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 the details of the procedure.

The predicted CATE is positive and statistically significant for virtually all individuals (98%), 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 the Appendix).

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 receive lower severance payments
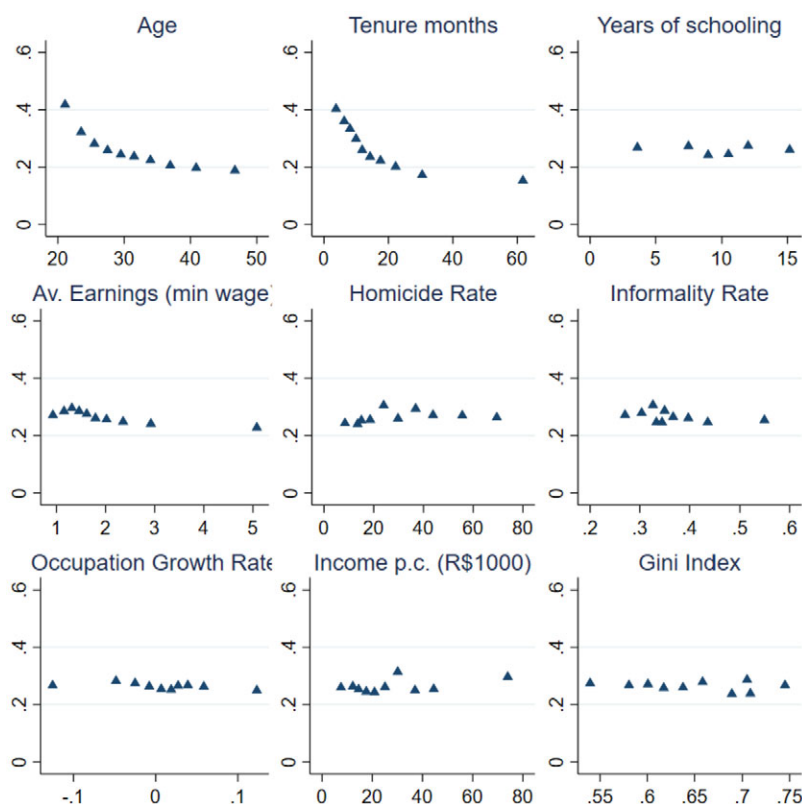


FIGURE 6.—Conditional average treatment effects of job loss, by characteristic. *Notes*: This figure shows the mean predicted Conditional Average Treatment Effects (CATE) the over individual and municipality level characteristics. CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a random forest.

[18]Informality 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.
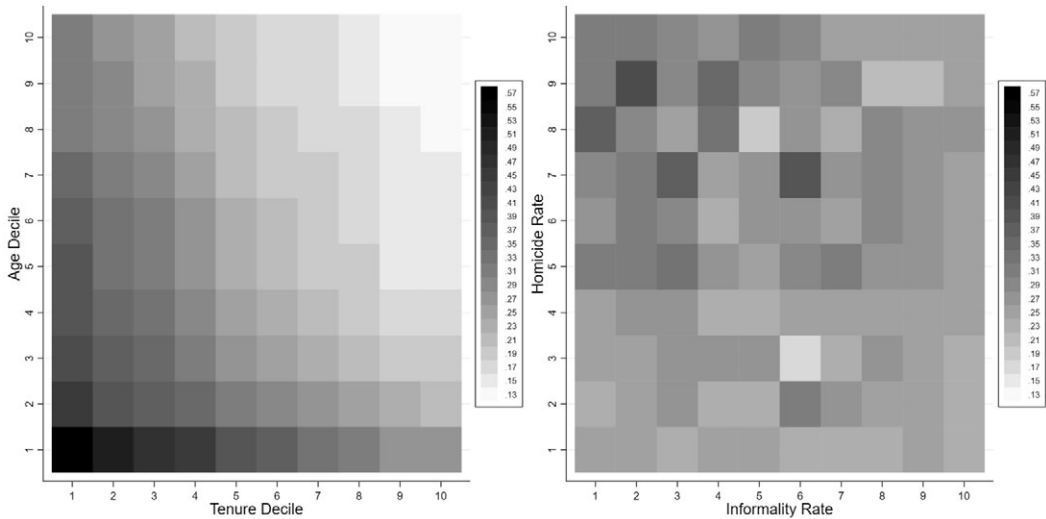
FIGURE 7.—Conditional average treatment effects of job loss, by pairs of characteristics. *Notes*: This figure shows the mean predicted Conditional Average Treatment Effects (CATE) over pairs of individual and municipality level characteristics, respectively, age and tenure (left graph) and homicide 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.

and less generous unemployment benefits.[19] While age and tenure are clearly correlated across individuals, the left graph in Figure 7 shows that the treatment effect varies significantly along either dimension while keeping the other variable constant. In addition, age and tenure remain the key dimensions of heterogeneity when holding constant education and income, while the latter attributes do not predict variation in the effect of job loss (see Figure B11 in the Appendix). In general, the predicted CATE is flat over all other characteristics, including local socioeconomic 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 II 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, Shaikh, and Xu (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)).[20] 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 II). Finally, the predicted CATE is flat over education.

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[19]Severance payments are a linear function of tenure. As for unemployment benefits, workers with less than 6 months in the job are not entitled to any benefit, and maximum duration is reached at 24 tenure months.

[20]This measure is weighted by the depth of the leaf at which the split takes place.

TABLE II

PREDICTED CONDITIONAL AVERAGE TREATMENT EFFECT—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 |

*Note*: This table compares individual and local level characteristics for workers with, respectively, above and below median Conditional Average Treatment Effects (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, Shaikh, and Xu (2019).

Overall, these patterns are consistent with the idea that liquidity constraints may play a relevant role in driving the effects, as younger and low-tenure workers are more likely to be liquidity-constrained than other groups, including low-wage workers.[21] On the other hand, younger and low-tenure workers differ from other workers in many respects—notably, they exhibit higher baseline crime rates; see Figure 2. In Section 5, we provide more direct evidence on the role of liquidity constraints by exploiting variation in the eligibility for unemployment benefits across displaced workers and the timing of benefit payments.

### 4.5. *Spillovers to Other Household Members*

The effect of job loss can spill over to other household members. We estimate these 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 data set, 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 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 aged 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

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[21]Using large and precise expenditure data from Brazil, Gerard and Naritomi (2021) show that younger and low-tenure workers suffer stronger consumption losses, suggesting that liquidity constraints may be binding for these groups; the same is not true for workers at the bottom of the wage distribution, due to a higher UI replacement ratio. Britto (2020) provides additional evidence that job search for low tenure workers is sensitive to cash on hand, while high tenure workers do not react.

TABLE III

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 |
| $\text{Treat}_i \times \text{Post}_t$ | 0.0055 | 0.0019 | −0.0017 | 0.00039 |
| | (0.005) | (0.0009) | (0.003) | (0.0005) |
| Mean outcome at t=-1 | 0.4172 | 0.0106 | 0.3855 | 0.0077 |
| Effect relative to the mean | 1% | 18% | −0.4% | 5% |
| Observations | 334,061 | 334,061 | 863,940 | 863,940 |

| Household Members: | Brothers, 30–50 y.o. | | Male Partner | |
|---|---|---|---|---|
| Dependent Variable: | Employment | Crime | Employment | Crime |
| $\text{Treat}_i \times \text{Post}_t$ | 0.014 | 0.0017 | 0.0035 | −0.0014 |
| | (0.007) | (0.001) | (0.005) | (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 |

*Note*: This table shows the effect of a 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 $\text{Treat}_i$ that is equal to 1 for the household members of workers displaced upon mass layoffs, interacted with a dummy $\text{Post}_t$ 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 $\text{Treated}_i$ and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

compared to the general population. Twenty-six percent of the families in this data set include at least one child more than 18 years old, and such families represent 37% of the individuals in the social registry. As in the main analysis, we exploit variation in (household members') job loss from mass layoffs. Household members working in the same firm as those who lose their job are dropped from the sample, so that we can clearly isolate spillover effects from common employment shocks.

Table III 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.[22]

In Appendix Table B.X, we examine, in addition, the role of intrafamily 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 the CadUnico registry (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).

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[22]Figure B12 in the Appendix plots dynamic treatment effects and confidence intervals. In Appendix Table B.IX, we show that effect on sons is robust to including interacted municipality-year fixed effects, to excluding states with a larger share of missing data, and to varying the definition of mass layoff.

## 5. THE EFFECT OF UNEMPLOYMENT INSURANCE ON CRIME

The results in the previous section establish that job displacement has dire consequences for criminal behavior. This highlights the importance of determining whether public policy can mitigate the adverse impacts on recently unemployed workers. In this section, we investigate the effect of unemployment benefits, which is the main policy for supporting displaced workers.[23] This analysis will also shed light on the potential mechanisms driving the effect of unemployment on crime.

### 5.1. *Research Design*

Brazilian workers are eligible for 3–5 months of unemployment benefits when dismissed without just cause from a formal job, if they satisfy 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 previous layoff resulting in a UI claim. For instance, a worker who claims UI benefits following a dismissal on January 1, 2010, will be able to claim benefits again if dismissed after April 30, 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 noneligible for UI benefits by estimating the following equation:

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

where $Y_i$ is an indicator variable for the $i$th worker committing a crime after job loss; $X_i$ is 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); $f(.)$ 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 when using different polynomial choices and bandwidths (including the optimal bandwidth according to the criterion of Calonico, Cattaneo, and Titiunik (2014)); we also compare RD estimates of UI effects at the true cutoff with the distribution of estimates obtained at placebo cutoffs.

The coefficient $\beta$ 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" effect of UI eligibility on the take-up of unemployment benefits, estimated using the same specification as in (3).

### 5.2. *Sample and Balance Tests*

We focus this part of the analysis on the period 2009–2014, because numerous changes were made to the UI system in 2015. We then restrict our initial sample to include only

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[23]The 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 (see Section 2.2). Appendix Table C.I confirms that the impact of job loss on eligibility for Bolsa Familia (or on the amount of transfers received) remains negligible.

dismissed workers for whom the 16-month 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.[24]

A further sample restriction deals with the cyclicality 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 first days of the month.[25] Consequently, workers who are initially displaced close to the last day of the month are more likely to be dismissed again on the last day of any month (including the 16-month eligibility cutoff). For instance, a worker dismissed on January 1, 2010, will be able to claim benefits again if dismissed from April 30, 2011. Given the dismissal cycle, when reemployed, they will be more likely to be displaced on the last day of the month—April 30, 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. The discontinuity is not specific to the 16-month period that is relevant for UI eligibility, as 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 monthly dismissal 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, Jansson, and Ma (2018, 2020). In addition, Appendix Figure C3 shows balance tests for a rich set of (predetermined) worker characteristics. Finally, the graphs in Appendix Figure C5 show no significant difference in prosecutions within 6 months and 3 years before displacement, while Appendix Table C.II contains the 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 C.VIII 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, Rokkanen, and Rothe (2020). As an alternative approach to deal with cyclicality, we include all workers and add cutoff and dismissal date fixed effects in the RD regressions (Appendix Table C.VII).

### 5.3. *Results*

Table IV shows that workers barely meeting the 16-month condition are 57 percentage points more likely to draw UI, receiving an additional 2.58 monthly benefits for a total of R\$2086—panel A, columns (1)–(3). The average replacement rate for UI takers near the cutoff is 78%. Figure C4 in the Appendix is a visual representation 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 6 months after layoff.

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[24]UI 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.

[25]There 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 on 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.

TABLE IV

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 for UI benefits | 0.57 | 2.58 | 2086.0 | −0.00077 | −0.00085 | −0.00062 | −0.0013 |
| | (0.0029) | (0.012) | (10.5) | (0.00044) | (0.00045) | (0.0011) | (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 ≤ 29 | | | | | | | |
| Eligibility for UI benefits | 0.58 | 2.60 | 2018.9 | −0.0013 | −0.0015 | −0.0025 | −0.0037 |
| | (0.0041) | (0.017) | (14.1) | (0.00067) | (0.00068) | (0.0017) | (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 ≥ 30 | | | | | | | |
| Eligibility for UI benefits | 0.56 | 2.56 | 2153.2 | −0.00023 | −0.00033 | 0.0012 | 0.001 |
| | (0.0041) | (0.017) | (15.5) | (0.00058) | (0.00059) | (0.0014) | (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 |

*Note*: 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 6 months 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.

The top-left graph in Figure 8 plots the probability that dismissed workers around the 16-month cutoff are prosecuted within the first 6 months after dismissal. Displaced workers who are marginally eligible for UI commit less crime than noneligible workers. The estimated effect amounts to −0.077 percentage points, or −21% over the baseline—panel A of Table IV, 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 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, who make up 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%).

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 at the top of Figure 8, and panels B and C in Table IV. Additional heterogeneity and robustness analyses are provided in Appendices C.4
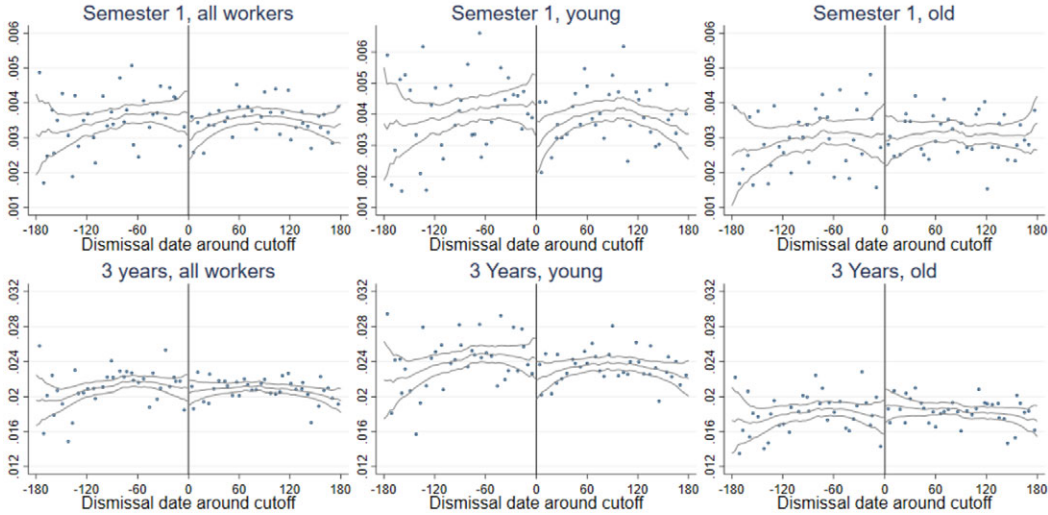
FIGURE 8.—Effect of UI eligibility on crime. *Notes*: The graphs plot the probability of criminal prosecution within 6 months and 3 years after layoff around the cutoff date for eligibility for unemployment benefits, for different groups of workers. Young and old groups comprise workers who are below 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.

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 6 months after the layoff; see columns (6)–(7) of Table IV, panel B, and the second graph at the bottom of Figure 8.[26] This indicates that UI effects do not extend beyond the expiration of benefits and suggest a role for liquidity constraints in explaining the reduction in crime.

## 5.4. *Timing of the Effect and the Importance of Liquidity Constraints*

We now examine more closely the timing of UI effects to interrogate the role of liquidity constraints as a key mechanism. Figure 9 confirms that UI eligibility decreases the probability of committing crime for displaced workers in the first 6 months after job loss, but the effect vanishes immediately when UI benefits cease.

To learn more about liquidity effects, we estimate changes in criminal behavior around benefit exhaustion. Using high-frequency data on consumption expenditure by displaced Brazilian workers, Gerard and Naritomi (2021) document sudden drops in consumption immediately after benefit exhaustion. To investigate whether criminal behavior exhibits 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 2009–2014 period and consider as treated workers that had 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

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[26]In 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.
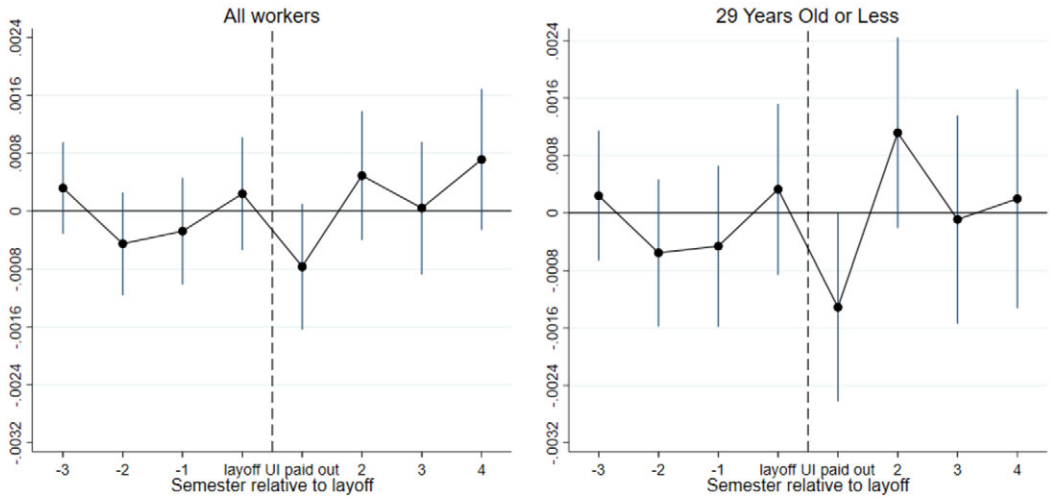
FIGURE 9.—Effect of UI eligibility on crime before and after layoff, by 6-month period. *Notes*: The graphs plot RD estimates on the effect of eligibility for unemployment benefits on the probability of criminal prosecution in 6-month periods before and after the layoff. The sample includes displaced 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.

individual characteristics described in Section 4.1, and compare the probability that they are arrested "in flagrante" 4 months before and after UI expiration.

Figure 10 clearly shows that the probability of criminal activity 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
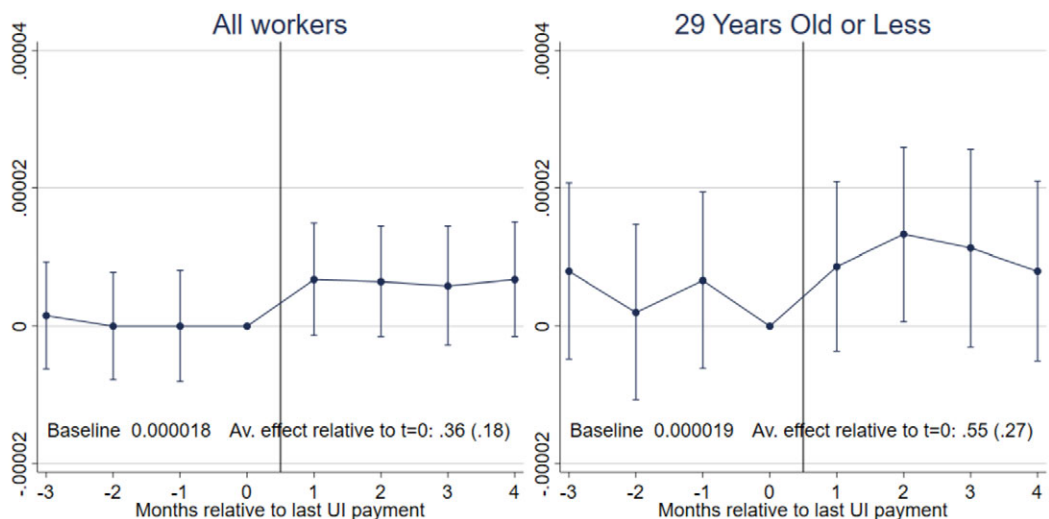


FIGURE 10.—*In flagrante* prosecution around UI benefit exhaustion. *Notes*: This figure shows estimated changes in the probability of *in flagrante* criminal prosecution around UI benefits exhaustion for UI beneficiaries relative to matched control workers employed throughout the entire period, estimated by equation (1) at a monthly frequency, along with 95% confidence intervals. Standard errors are clustered at the worker level.

liquidity constraints play an important role as a mechanism linking job loss and crime. These findings mirror the evidence on consumption drops in Gerard and Naritomi (2021).

Taken together, Figures 9 and 10 provide compelling evidence about the role 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.

## 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 noneconomic 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 noneligible. Both groups are unemployed immediately after layoff, but displaced workers eligible for UI work fewer months in the first 6 months 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 C.X. If employment
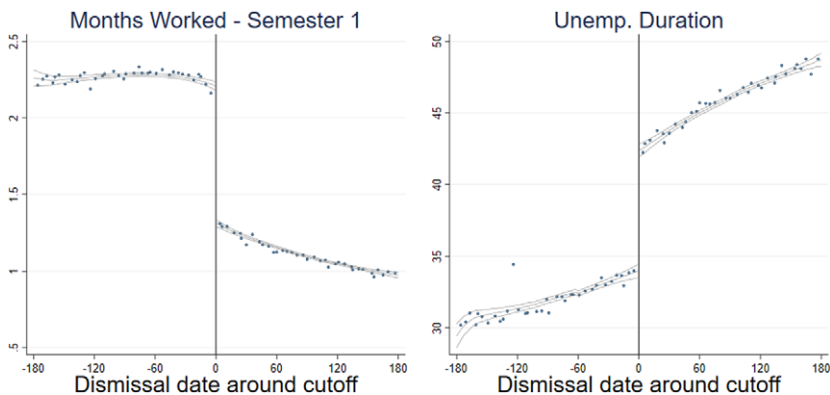


FIGURE 11.—Effect of UI eligibility on employment. *Notes*: The graphs plots the number of months worked (left graph) and unemployment duration (right graph) around the cutoff date for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment 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.

had an incapacitation effect on potential offenders, eligible workers should commit more crime than the noneligible (instead, they commit less).[27]

The increased opportunity cost of crime is also unlikely to explain the strong effect of unemployment benefits in the first 6 months after layoff, for two reasons. First, UI reduces the payoff of formal work, because benefits cease 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 canceled 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, and with the spillover effects on cohabiting sons, who are likely subject to the same liquidity constraints.

Finally, we note the potential relevance of psychological stress associated with 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 B10 in the Appendix). In addition, there is a sizable impact on non-instrumental homicides (i.e., homicide cases that are not associated with 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 a mechanism is consistent with evidence linking job displacement to mental health problems and stress (Kuhn, Lalive, and Zweimüller (2009), Charles and DeCicca (2008), Zimmer (2021)).

## 6.2. *Implications for Theoretical Models of Crime*

Based on these findings, we discuss a few features to consider in economic models studying the relationship between job loss and crime. First, crime is triggered by binding liquidity constraints. In such context, a 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 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, with this pattern being consistent with behavioral models with myopic or present-biased individuals (Ganong and Noel (2019), Gerard and Naritomi (2021)).

Second, previous papers detect stronger effects of economic shocks on economically-motivated crimes (e.g., Machin and Meghir (2004), Mastrobuoni and Pinotti (2015)). Instead, we estimate significant and large effects for a wide range of crimes with no economic motivation, possibly because our extremely large data set allows us to detect with sufficient precision the effect on violent crimes and other noneconomic crimes, which are typically less frequent. These results confirm earlier theoretical insights of Ehrlich

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[27]It is worth noting that during our sample period of 2009–2014, UI was not conditional on meeting job search requirements or attending training. In the 2012–2014 period, there were attempts to make benefits conditional on attendance at 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.

(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 0.79 and who access 2.58 monthly benefits are 21% less likely to be criminally prosecuted in the 6-month period following layoff.[28] The impact is entirely driven by young workers who reduce crime by 30.2% while drawing the same number of monthly payments.

### 6.3. *Social Assistance Policies and Crime*

Our results are in line with other studies in the literature showing the 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 the UI benefit amount reduces ex-offenders' recidivism by 5.4% in the three quarters following job loss. Outside the US context, Munyo and Rossi (2015) estimate that increasing the 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, Guettabi, and Reimer (2020) show that universal basic income in Alaska reduces property crime by 10%, although substance-abuse incidents increase by 8%, while Fishback, Johnson, and Kantor (2010) estimate that a 10% increase in welfare relief spending decreases crime by 1.5% using US data. Finally, Chioda, De Mello, and Soares (2016) find that an expansion in Bolsa Família coverage to children 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. The probability of criminal prosecution increases by 23% from the first year following the job loss, and is then stable over a 4-year period. This substantial effect is not solely driven by economically-motivated crimes (+43%), but it also extends to violent crimes (+17%) and other crimes such as traffic offenses and failure to obey. The fact that noneconomically 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, lasting roughly 6 months. Based on these findings, as well as extensive evidence on heterogeneity and spillovers on other household members, we conclude that in the present

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[28]Our results can also be expressed as semielasticities. Prosecution rates decrease by 8.1% for each additional UI monthly payment, or by 10.3% for each additional monthly wage in UI transfers.

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 cessation of unemployment benefits. Therefore, income support should be accompanied by active labor market policies to speed up the return of workers to jobs and to guarantee a stable income rather than temporary income assistance. Our findings also suggest that both passive and active policies should be targeted at vulnerable groups, for example, through means-tested schemes—because such groups are at greater risk of poverty upon layoff and, consequently, they are more likely to commit crimes.

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