Econometrica Supplementary Material

SUPPLEMENT TO "THE EFFECT OF JOB LOSS AND UNEMPLOYMENT INSURANCE ON CRIME IN BRAZIL"

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APPENDIX A: APPENDIX TO SECTION 3

TABLE A.I
PROSECUTIONS BY TYPE OF OFFENSE.

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

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

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TABLE A.II

SUMMARY STATISTICS, JOB LOSERS WITH AND WITHOUT UNIQUE NAMES.

	(1)	(2)	(3)	(4)	(5)	(6)
	Country-level			Within State		
	Unique	Others	Std Diff	Unique	Others	Std Diff
DEMOGRAPHIC CHARACTERISTICS						
Years of education	10.8	10.1	−0.21	10.6	10.1	−0.18
Age	29.9	30.8	0.11	30.0	31.0	0.11
Race—white	51.8%	45.5%	−0.13	49.7%	46.3%	−0.07
Race—black	4.9%	6.6%	0.07	5.2%	7.0%	0.08
Race—mixed	34.6%	39.4%	0.10	36.4%	38.5%	0.04
JOB CHARACTERISTICS						
Monthly income (R$)	1736	1548	−0.08	1689	1546	−0.07
Months worked $t-1$	5.1	5.1	−0.01	5.1	5.1	−0.01
Tenure on Jan 1st (years)	1.8	1.7	−0.01	1.8	1.8	0.00
Manager	6.2%	3.6%	−0.12	5.6%	3.4%	−0.11
Firm size (employees)	510	516	0.00	517	506	−0.01
MUNICIPALITY CHARACTERISTICS						
Large municipality—pop > 1 mil.	34%	35%	0.02	34%	35%	0.02
Municipality population	1,919,447	2,068,497	0.04	1,890,405	2,183,803	0.08
Homicide rate (per 100k inhab.)	29.7	30.5	0.04	30.4	29.7	−0.03
Observations	5,868,151	6,652,131		7,901,613	4,618,669	

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

APPENDIX B: APPENDIX TO SECTION 4

B.1. *The Effect of Job Loss on Crime, Treatment, and Control Group Characteristics*

TABLE B.I

SUMMARY STATISTICS, TREATED AND CONTROL WORKERS IN MASS AND NONMASS LAYOFFS.

	(1)	(2)	(3)	(4)	(5)	(6)
	All layoffs			Mass layoffs		
	Treatment	Control	Std Diff	Treatment	Control	Std Diff
DEMOGRAPHIC CHARACTERISTICS						
Years of education	10.8	11.1	0.12	10.1	10.9	0.29
Age	30.3	30.3	0.00	30.7	30.7	0.00
Race—white	54.1%	55.9%	0.04	45.9%	49.3%	0.07
Race—black	4.9%	4.9%	−0.00	5.6%	5.2%	−0.02
Race—mixed	32.2%	31.3%	−0.02	39.7%	37.7%	−0.04
JOB CHARACTERISTICS						
Monthly income (R$)	1413	1420	0.01	1396	1402	0.01
Month of worked $t-1$	11.2	11.5	0.09	10.8	11.3	0.15
Tenure on Jan 1st (years)	1.6	1.6	0.01	1.1	1.2	0.03
Manager	5.2%	6.6%	0.06	3.2%	5.3%	0.10
Firm size (employees)	448	449	0.00	572	505	−0.05
MUNICIPALITY CHARACTERISTICS						
Large municipality—pop > 1M	34%	33%	−0.01	38%	39%	0.01
Municipality population	2,012,523	2,031,573	0.01	2,178,083	2,222,797	0.01
Homicide rate (per 100k inhab.)	28.3	27.1	−0.06	31.1	29.6	−0.07
CRIME OUTCOMES						
Prob. of criminal prosecution $t-1$	0.0057	0.0041	−0.02	0.0052	0.0039	−0.02
Prob. Prosec—economically motivated	0.0015	0.0010	−0.01	0.0014	0.0010	−0.01
Prob. Prosec—drug trafficking	0.0005	0.0003	−0.01	0.0005	0.0003	−0.01
Prob. Prosec—property crime	0.0006	0.0004	−0.01	0.0006	0.0005	−0.01
Prob. Prosec—violent crime	0.0009	0.0007	−0.01	0.0009	0.0007	−0.01
Prob. Prosec—other crimes	0.0026	0.0019	−0.01	0.0024	0.0018	−0.01
Observations	4,870,849	4,870,849		1,167,846	1,167,846	

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

TABLE B.II

SUMMARY STATISTICS, BY CRIMINAL PROSECUTION BEFORE AND AFTER THE JOB LOSS.

	(1)	(2)	(3)	(4)	(5)	(6)
	Criminal prosecutions					
	Before job loss			After job loss		
	No	Yes	Std Diff	No	Yes	Std Diff
DEMOGRAPHIC CHARACTERISTICS						
Years of education	10.1	9.9	0.05	10.1	9.9	0.05
Age	30.7	30.3	0.05	30.7	29.3	0.19
Race—white	45.8%	53.1%	−0.15	45.7%	51.5%	−0.12
Race—black	5.6%	5.5%	0.01	5.6%	5.5%	0.01
Race—mixed	39.8%	31.4%	0.18	39.8%	32.6%	0.15
JOB CHARACTERISTICS						
Monthly income (R$)	1397	1320	0.08	1399	1278	0.14
Month of worked $t-1$	10.8	10.2	0.20	10.8	10.2	0.19
Tenure on Jan 1st (years)	1.1	0.9	0.21	1.1	0.9	0.21
Manager	3.2%	2.1%	0.07	3.2%	2.1%	0.07
Firm size (employees)	573	510	0.04	574	519	0.03
MUNICIPALITY CHARACTERISTICS						
Large municipality—pop > 1M	38%	34%	0.08	38%	34%	0.09
Municipality population	2,183,937	1,770,187	0.12	2,191,327	1,671,132	0.16
Homicide rate (per 100k inhab.)	31.1	28.7	0.13	31.1	30.4	0.04
Observations	1,151,321	16,525		1,138,112	29,734	

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

B.2. *The Effect of Job Loss on Formal and Informal Employment*

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

We thus replicate the analysis of employment effects based on the National Longitudinal Household Survey (*Pesquisa Nacional por Amostra de Domicílios*, PNAD), which contains information on both formal and informal labor income. In fact, the Brazilian Institute of Geography and Statistics (IBGE) computes informality rates based on PNAD. The longitudinal component of PNAD tracks households for five consecutive quarters. Although the microdata does not contain a person ID, it is possible to track individuals over time based on their household ID and characteristics such as gender, their precise birth date and their order in the family. In line with our main analysis of Figure 3 in the main text, we focus on male workers who were initially interviewed during the 2012–2014 period, and compare treated workers who were formally employed in the first but not in the second quarter with a control group who were employed in both the first and second quarter (but possibly displaced in later quarters).

Figure B.1 presents the results for monthly income for both formal and informal jobs. Reassuringly, the average effect on formal earnings over the first four quarters after displacement (−65%) is essentially identical to that estimated in the main analysis. When

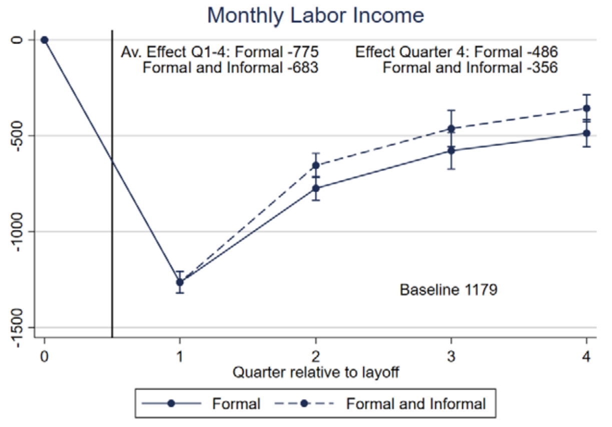

FIGURE B.1.—Effect of job loss on formal and informal labor earnings. *Notes*: This figure shows the effect of job loss on formal and informal monthly labor earnings (along with 95% confidence intervals) as estimated from the difference-in-differences equation (1), based on PNAD longitudinal household survey data following workers for up to five quarterly interviews. The sample covers individuals first interviewed in the period 2012–2014. The treatment group is defined by workers who are formally employed in the first interview and out of employment in the second interview; the control group is composed by workers who are formally employed on the first and second interviews. Earnings are measured in Brazilian Reais. Baseline average values for the treated group at $t = 0$ are also reported.

also including informal employment, the estimated effects on labor earnings are smaller (-58%), as some of the workers displaced from a formal job reallocate into the informal economy within the following year. This suggests that crime elasticity estimates based solely on formal labor income are underestimated, in terms of magnitude, by about 12%.

B.3. *The Effect of Job Loss on Crime, Additional Robustness Checks*

B.3.1. *Selection Into Treatment*

Our analysis of the effects of job loss, presented in Section 4, crucially hinges on the assumption that there is no dynamic selection into treatment, implying in turn that the control group approximates the behavior of displaced workers in the absence of displacement. The evidence of parallel trends in the pretreatment period (Figures 3 and 5) is consistent with such an assumption. Importantly, the same figures show that all results are virtually identical when including all displaced workers or, alternatively, restricting the treated group to workers displaced upon plausibly exogenous mass layoffs; also, results are unaffected when controlling for municipality $\times$ industry $\times$ year fixed effects.

However, firms might still have considerable room for choosing whom to dismiss even when firing (at least) one-third of employees, as in our baseline definition of mass layoffs. We address this concern in two ways. First, in Table B.III we explore the sensitivity of the results when varying the definition of mass layoffs, in terms of both the fraction of dismissed employees (columns 1 to 4) and firm size (panels A to D). As we restrict to events in which a larger fraction of workers were dismissed, there should be less scope for selection into treatment. Indeed, differences in the level of crime rates between dismissed workers and matched controls during the pretreatment period—reported in the last row of each panel of Table B.III—progressively decline to almost zero when restricting to events in which at least 90% of workers were dismissed. At the same time, the estimated

TABLE B.III

EFFECT OF JOB LOSS ON CRIME, VARYING THE DEFINITION OF MASS LAYOFFS.

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

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

effect on crime is largely unaffected; see also Figure B.2, which shows the dynamic treatment effects under these alternative specifications. The same is true when focusing on plant closures (column 5) and when varying the minimum firm size (panels B to D).

As a second approach to addressing potential selection effects, in Table B.IV we expand the treated group to include *all* workers—both displaced and nondisplaced—employed at the beginning of each year in mass layoff firms (columns 1–6), and in nonmass layoff firms (columns 4–6). This approach differs from our baseline specification, which follows previous papers in comparing workers who are displaced upon mass layoffs with a matched

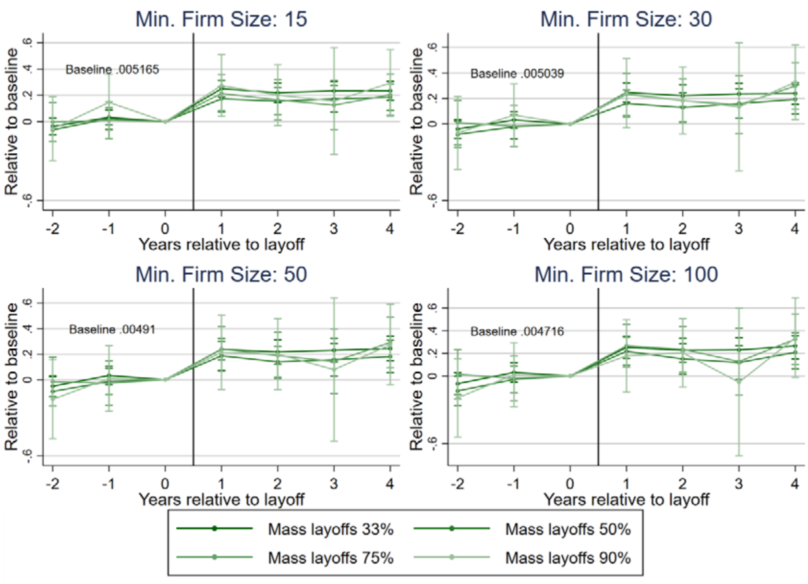

FIGURE B.2.—Effect of job loss on crime, robustness to alternative definitions of mass layoffs. *Notes*: The graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime using different mass layoff definitions and estimated according to equation (1), along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

group of nondisplaced workers. Both of these groups of workers are potentially selected on individual characteristic. Drawing an analogy with randomized experiments with imperfect compliance, we may want instead to compare all workers "assigned" to mass and nonmass layoff firms. By retaining all workers employed at the beginning of each year in the mass and nonmass layoff firms, we also avoid potential selection issues driven by early leavers who may quit declining firms in advance of mass layoffs. Not surprisingly, when we adopt this "intention-to-treat" approach, the change in both labor market outcomes (columns 1–2, 4–5) and the probability of criminal prosecutions (columns 3 and 6) are much weaker compared with our baseline analysis. However, when we rescale crime effects by changes in earnings, the implied elasticity remains very similar to our baseline estimate in Table I.

B.3.2. *Methodological Issues in the Estimation of Dynamic Treatment Effects*

Recent methodological contributions highlight the challenges associated with estimating dynamic treatment effects in difference-in-differences designs when there is (i) variation in the timing of treatment—as in our context—and (ii) treatment effects are heterogeneous across individuals, as is reasonable to assume in most situations. Under these conditions, the treatment effects for individuals who are treated at some point might enter the double differences estimating the dynamic treatment effects with opposite signs in different time periods. As a result, the estimated difference-in-differences coefficients in

TABLE B.IV

EFFECT OF JOB LOSS ON LABOR MARKET OUTCOMES AND CRIME, INCLUDING ALL WORKERS IN MASS AND NONMASS LAYOFF FIRMS.

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

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

a two-way fixed effect specification equal a weighted average of the individual treatment effects with possibly negative weights (Chaisemartin and D'Haultfœuille (2020)).[1]

This problem is most severe when all or a large share of individuals in the sample are treated at some point, as is sometimes the case. Indeed, some previous analyses on the impact of job displacement on crime purposefully restrict the sample to job losers to ensure stronger comparability of treatment and control units in each period. By contrast, our data include a large share of never-treated workers (i.e., "pure controls"), which should limit the extent of negative weights. Indeed, if we estimate the two-way fixed effect specification in the panel of workers observed over calendar years, no individual treatment effect receives a negative weight. If we were instead to restrict the sample to workers displaced at some point, about 42% of units would receive a negative weight. Consequently, the estimated effects would be about half the strength of those estimated when including never-displaced workers; see Figure B.3, comparing the estimated effect in two-way fixed effect regressions when including (left panel) and excluding (right panel) pure controls who are never displaced.[2] As a final robustness check, we reestimate the effect of interest following the approach of Chaisemartin and D'Haultfœuille (2020), which compares, in each period, "switchers"—units changing treatment status in a given period—to "nonswitchers"—units not changing treatment status in the same period. The results are extremely similar to those of our baseline approach, and are reported in Figure B.4.

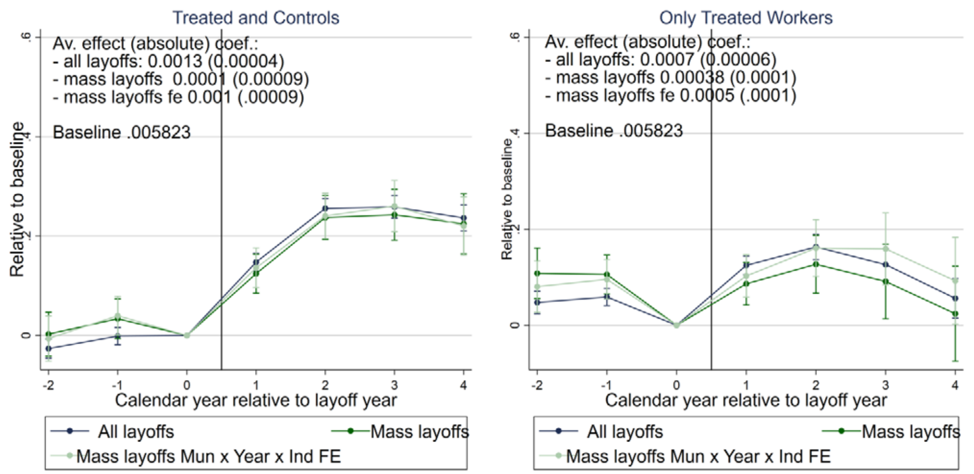

FIGURE B.3.—Effect of job loss on crime, two-way fixed effects panel estimates. *Notes*: The left graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime in an yearly panel, two-way fixed effects specification, along with 95% confidence intervals. The right graph reports estimates based on the same model but restricted to displaced workers, that is, without the control group constructed via matching. All coefficients are rescaled by the baseline average values of each variable for the treated group at $t = 0$, which are also reported.

[1]Goodman-Bacon (2021) provide a similar decomposition; see also Borusyak and Jaravel (2017), Sun and Abraham (2021), Athey and Imbens (2018), Callaway and Sant'Anna (2021), and Imai and Kim.

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

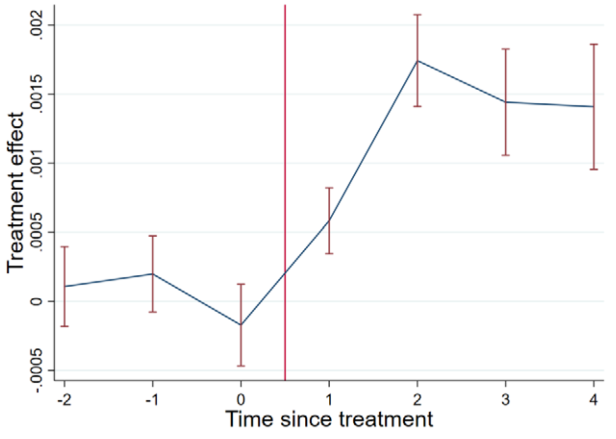

FIGURE B.4.—Effect of job loss on crime, two-way fixed effects panel estimates with correction from Chaisemartin and D'Haultfœuille (2020). *Notes*: The graph reports the dynamic treatment effects of job loss on the probability of being prosecuted for a crime in an yearly panel, two-way fixed effects specification using the estimator proposed by Chaisemartin and D'Haultfœuille (2020), along with 95% confidence intervals.

B.3.3. *Additional Measurement Issues*

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

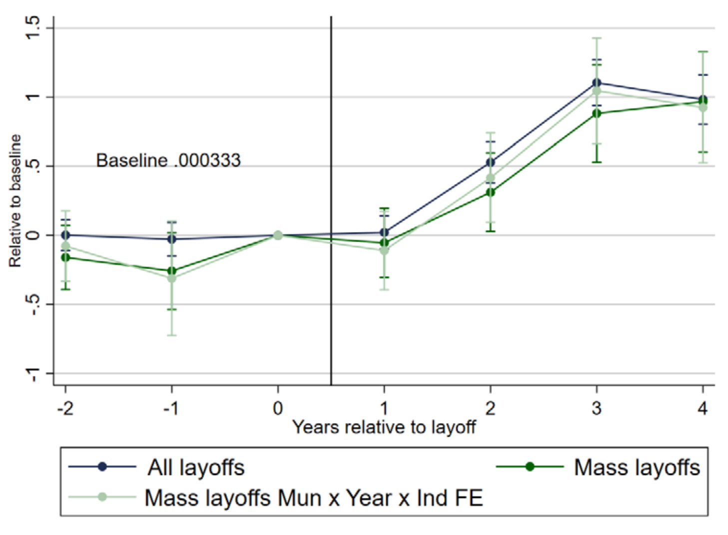

FIGURE B.5.—Effect of job loss on final criminal conviction. *Notes*: The graph reports the dynamic treatment effects of job loss on the probability of a final criminal conviction, estimated according to equation (1), along with 95% confidence intervals. Years relative to layoff are defined relative to the exact date of layoff, that is, $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. All coefficients are rescaled by the baseline average value of the outcome variable in the treated group at $t = 0$, which is also reported.

TABLE B.V

SHARE OF PROSECUTIONS REPORTING THE NAME OF THE OFFENDER, BY STATE.

State	Nonmissing share	Obs
Tocantins	92.7%	166,604
Goiás	90.4%	8405
Paraná	89.3%	476,160
Rondônia	88.2%	15,938
Sergipe	81.3%	166,806
Piauí	86.8%	121,567
Bahia	78.4%	510,540
Alagoas	79.2%	118,152
Maranhão	81.0%	183,117
Espírito Santo	80.0%	302,554
Pará	78.3%	100,487
Acre	76.0%	143,704
Roraima	72.1%	15,930
Rio de Janeiro	66.3%	1,521,375
Paraíba	62.7%	186,081
Rio Grande do Norte	65.9%	208,702
Amazonas	65.4%	189,620
Mato Grosso do Sul	59.1%	531,998
Santa Catarina	57.4%	906,246
Rio Grande do Sul	63.0%	3,781,713
Amapá	53.8%	63,723
Pernambuco	51.6%	423,933
Ceará	49.6%	239,112
Distrito Federal	43.2%	525,550
São Paulo	29.1%	2,008,080
Minas Gerais	12.9%	1,843,531
Total	53.9%	14,759,628

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

Table B.VI shows that results are unaffected when restricting to states in which the share of nonmissing names is as high as 80% or more.

B.4. *The Causal Forest Approach for Heterogeneous Treatment Effects*

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

We follow the implementation in Athey, Tibshirani, and Wager (2019). Since we have a difference-in-differences setting (Davis and Heller (2017), Bertrand, Crépon, Marguerie, and Premand (2017), differently from most application based on randomized control tri-

TABLE B.VI

EFFECT OF JOB LOSS ON CRIME, INCLUDING ONLY STATES WITH A MINIMUM SHARE OF NONMISSING NAMES IN THE PROSECUTION RECORDS—MASS LAYOFFS SAMPLE.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Only States With a Share of Nonmissing Names in Prosecution Records Above:						
Prob. of Criminal Prosecution	0%	20%	30%	50%	60%	70%	80%
$\text{Treat}_i \times \text{Post}_t$	0.0012	0.0014	0.0017	0.0017	0.0018	0.0013	0.0016
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Mean outcome at $t = 0$ (treated)	0.0052	0.0057	0.0072	0.0074	0.0076	0.0037	0.0038
Effect relative to the mean	23%	24%	24%	23%	24%	35%	42%
Observations	16,349,844	13,945,064	9,429,070	8,449,672	7,048,958	3,929,716	1,913,380

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

als such as), we run the causal forest over first-differences. In this way, the treatment group indicator is orthogonal to the covariates, so the unconfoundness assumption in Wager and Athey (2018) holds.

The main outcome is the probability of criminal prosecution. The algorithm starts by building trees. Each of them is defined by data driven sample splits characterizing leafs, which are followed by a prediction of the causal effect over the characteristics X. Given our large sample and the fact that the goal is estimating a small quantity, we require that each leaf contains at least 5000 observations to improve precision.[3] To avoid over fitting, the sample is randomly split in two equal parts: one is used to define the sample splits (leafs), the other is used for estimating the predicted CATE ("honest approach"). The procedure is repeated multiple times, leading to 10,000 trees. The final causal forest prediction is a weighted average over the predictions in each tree, which is shown to be consistent and asymptotically normal (and is also clustered at the individual level). In addition, valid confidence intervals are estimated.

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

[3] Avoiding leafs, which are too small, also speeds up computational time.

B.5. *Additional Figures and Tables*

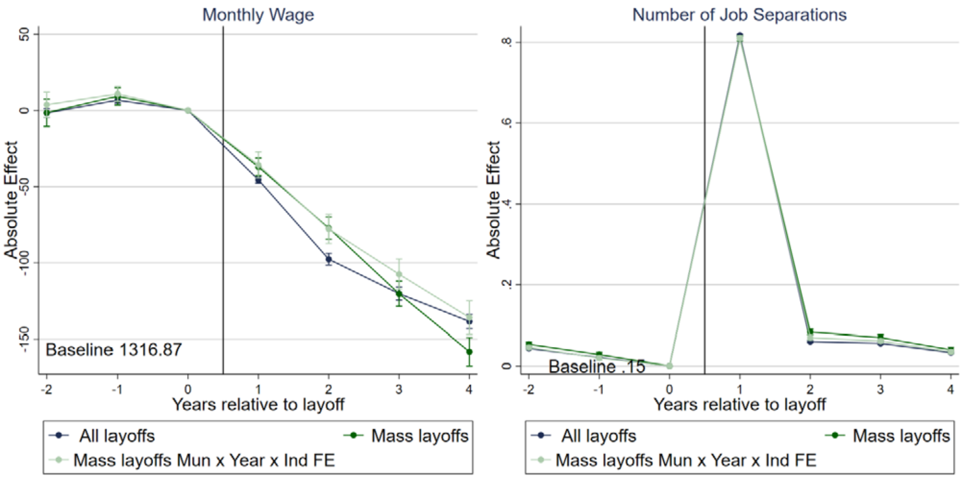

FIGURE B.6.—Effect of job loss on subsequent monthly wages and job turnover. *Notes*: This figure shows the effect of job loss on individual monthly wages conditional on being employed (left graph) and the number of job separations per year (right graph), as estimated from the difference-in-differences equation (1)—along with 95% confidence intervals (too small to be visible). The treatment group comprises displaced workers, while the control group is defined via matching among workers in 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.

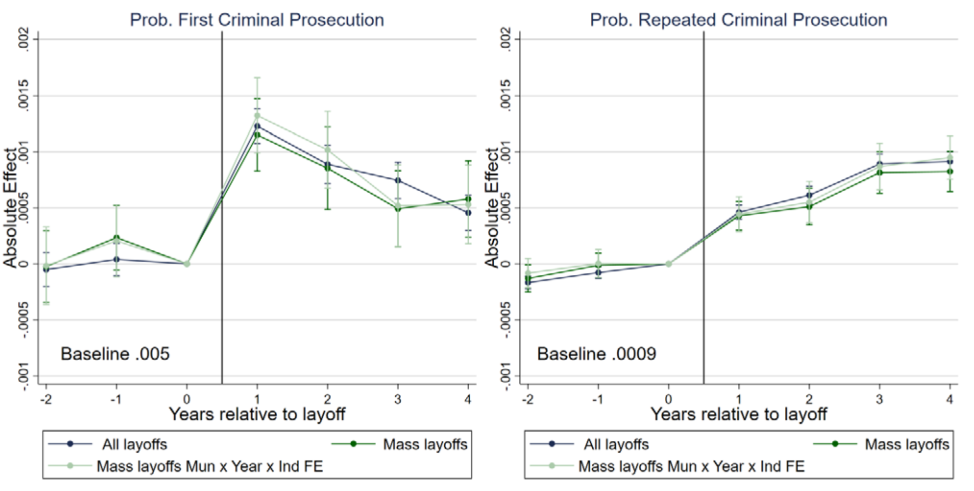

FIGURE B.7.—Effect of job loss on crime, first versus repeated prosecution. *Notes*: The graph reports the dynamic treatment effects of job loss on the probability of being criminally prosecuted for the first time, within our panel, and on the prob. of a repeated prosecution, estimated according to equation (1), along with 95% confidence intervals. Years relative to layoff are defined relative to the exact date of layoff, that is, $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on.

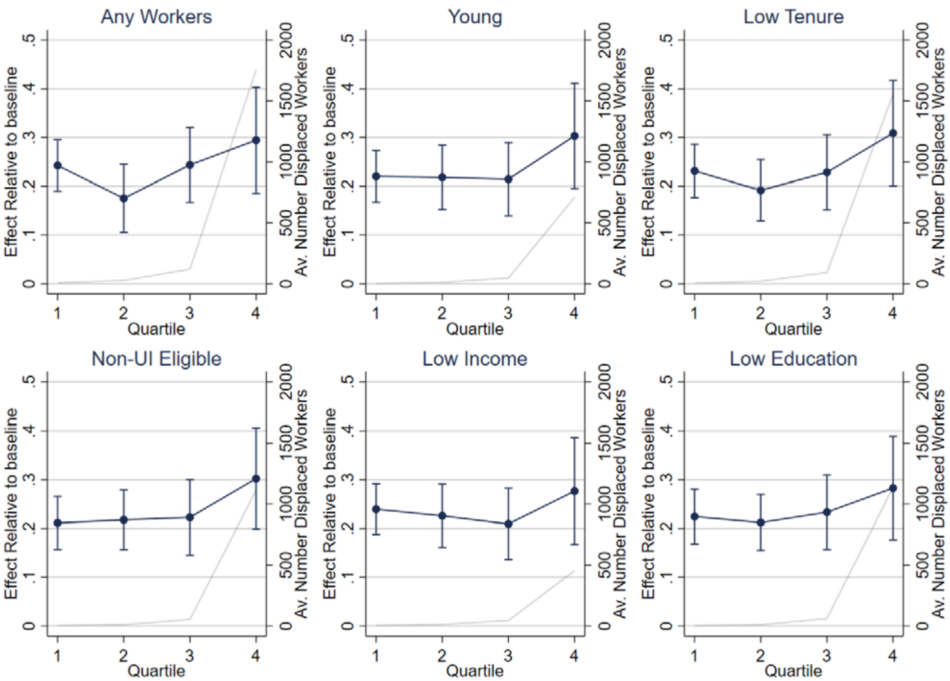

FIGURE B.8.—Effect of job loss on criminal behavior, by number of displaced individuals in the same firm, mass layoffs. *Notes*: This table shows the effect of job loss in a mass layoff on the probability of criminal prosecution up to 4 years after, as estimated from the difference-in-differences equation (2), after splitting the sample on the number of workers displaced in the same mass layoff event with given characteristics (by quartiles). Years relative to layoff are defined relative to the exact date of layoff, that is, $t = 1$ for the first 12 months after layoff, $t = 2$ for the following 12 months, and so on. All coefficients are rescaled by the baseline average value of the outcome variable in the treated group at $t = 0$, which is also reported.

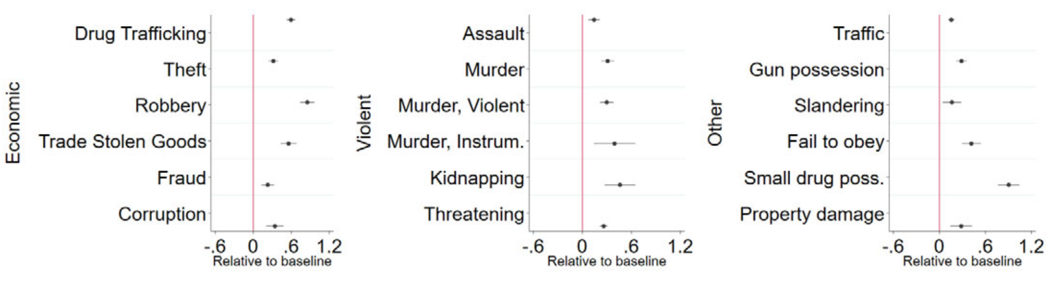

FIGURE B.9.—Effect of job loss on different types of crime—all layoffs. *Notes*: The graphs show the effect of job loss on different types of crime (and associated confidence interval) as estimated from the difference-in-differences equation (2) and rescaled by the average outcome in the treatment group at $t = 0$.

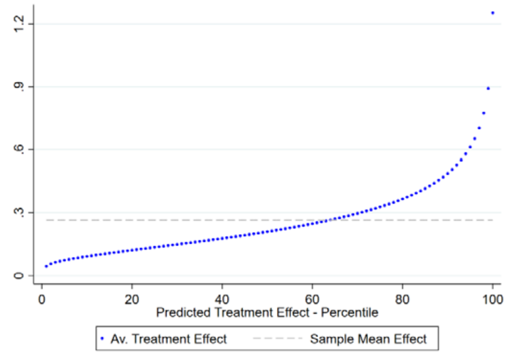

FIGURE B.10.—Job loss—predicted conditional average treatment effects. *Notes*: This figure shows how the predicted Conditional Average Treatment Effect (CATE) varies over its rank, aggregated over percentiles. A causal forest is implemented to estimate the CATE and estimates are rescaled by the predicted crime outcome in the post period absent the job loss—also based on a random forest—reflecting a proportional effect. Ninety-eight percent of the predicted CATE at the individual level are statistically different from zero in the sample.

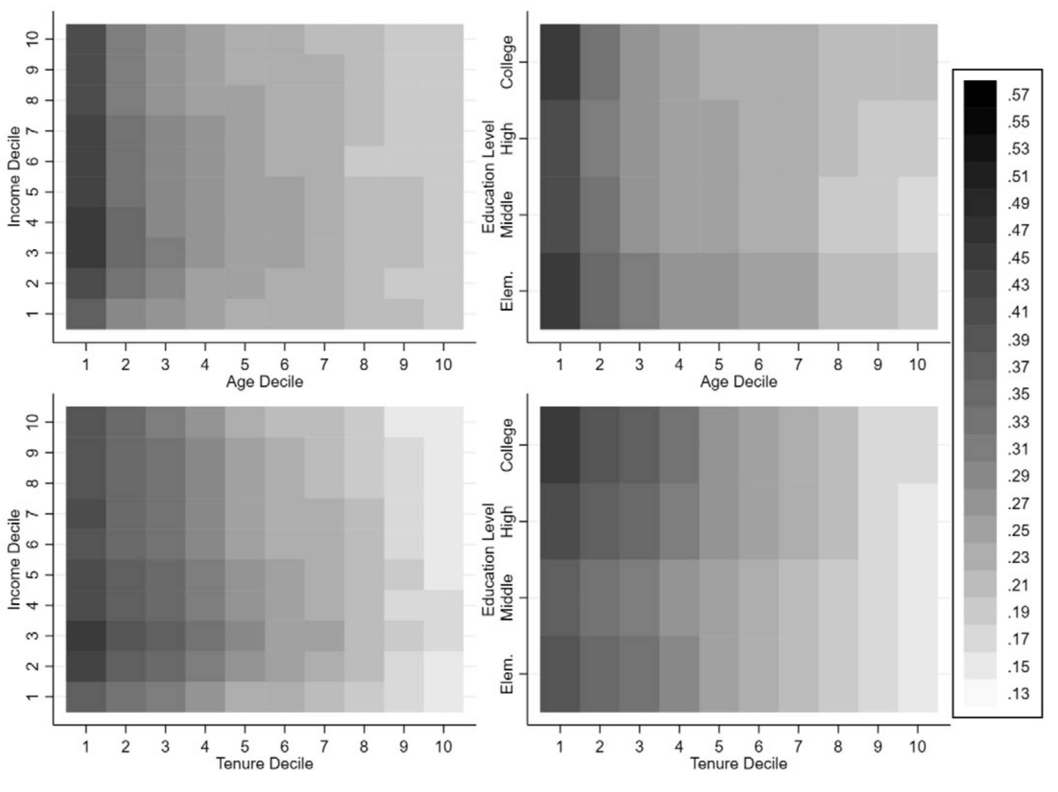

FIGURE B.11.—Job loss—predicted conditional average treatment effects, by pairs of characteristics. *Notes*: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over pairs of individual characteristics. A causal forest is implemented to estimate the CATE and estimates are rescaled by the predicted crime outcome in the post period absent the job loss—also based on a random forest—reflecting a proportional effect. Each map bin corresponds to a decile over each characteristic (years of education is an exception due to the discrete nature of the variable).

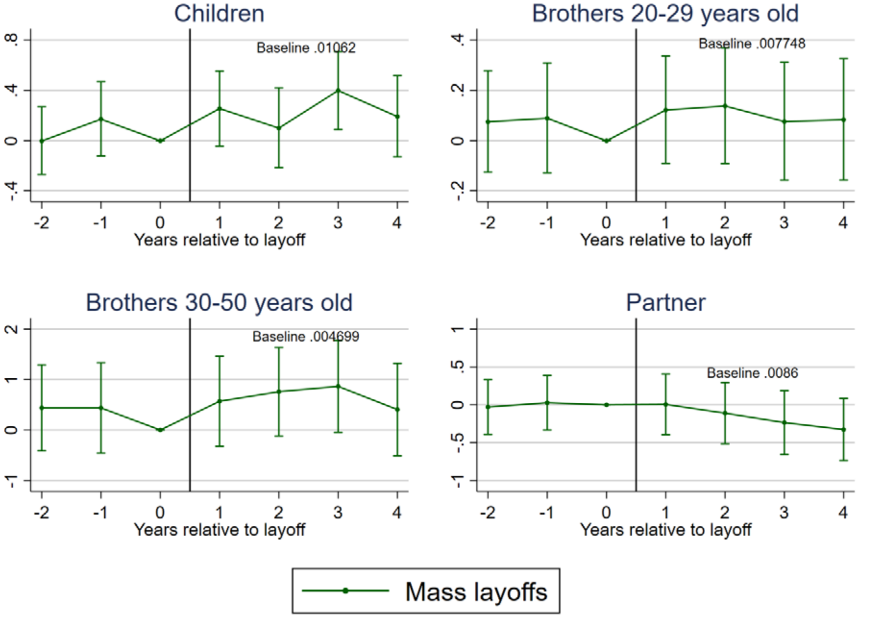

FIGURE B.12.—Effect of job loss on household members' criminal behavior. *Notes*: This table shows the effect of worker's displacement upon mass layoff on the probability of criminal prosecution for different categories of household members (indicated on top of each graph), as estimated from equation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

TABLE B.VII

EFFECT OF JOB LOSS ON LABOR MARKET OUTCOMES AND CRIMINAL BEHAVIOR—ALTERNATIVE SPECIFICATIONS.

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor Market Effects		Probability of Criminal Prosecution			
Dependent Variable:	Employment	Earnings	Any Crime	Economic	Violent	Others
PANEL A: ALL DISPLACED WORKERS						
$\text{Treat}_i \times \text{Post}_t$	−0.21	−6048.1	0.0015	0.00067	0.00033	0.00041
	(0.0006)	(28.1)	(0.00005)	(0.00003)	(0.00003)	(0.00003)
Mean outcome, treated at $t = 0$	1	15,006	0.0057	0.0015	0.0016	0.0020
Effect relative to the mean	−21%	−40%	27%	45%	21%	20%
Implied elasticity to earnings			−0.66	−1.12	−0.52	−0.51
Observations	68,191,886	68,191,886	68,191,886	68,191,886	68,191,886	68,191,886
PANEL B: ALL DISPLACED WORKERS VS. ALTERNATIVE CONTROL GROUP (CONTINUOUSLY EMPLOYED WORKERS)						
$\text{Treat}_i \times \text{Post}_t$	−0.40	−8600.1	0.0029	0.0010	0.00070	0.00097
	(0.0006)	(32.7)	(0.00006)	(0.00003)	(0.00003)	(0.00004)
Mean outcome, treated at $t = 0$	1	14,115	0.0051	0.0013	0.0015	0.0018
Effect relative to the mean	−40%	−61%	57%	74%	48%	55%
Implied elasticity to earnings			−0.94	−1.85	−1.19	−1.36
Observations	59,737,776	59,737,776	59,737,776	59,737,776	59,737,776	59,737,776
PANEL C: DISPLACED IN MASS LAYOFFS, CONTROLLING FOR MUNICIPALITY × INDUSTRY × YEAR FIXED EFFECTS						
$\text{Treat}_i \times \text{Post}_t$	−0.19	−5433.7	0.0013	0.00061	0.00039	0.00030
	(0.001)	(59.9)	(0.0001)	(0.00006)	(0.00006)	(0.00006)
Mean outcome, treated at $t = 0$	1	14,340	0.0052	0.0014	0.0015	0.0018
Effect relative to the mean	−19%	−38%	25%	44%	26%	17%
Implied elasticity to earnings			−0.66	−1.15	−0.69	−0.44
Observations	16,250,836	16,250,836	16,250,836	16,250,836	16,250,836	16,250,836

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 Treat_i that is equal to 1 for displaced workers, interacted with a dummy Post_t equal to 1 for the period after displacement. Panel A includes in the sample all displaced workers and matched control workers employed in non-mass layoff firms who are not displaced in the same calendar year; Panel B restricts the control group to workers who remain continuously employed after the matched treated worker has been displaced; Panel C restricts the treated group to workers who are displaced in mass layoffs and adds municipality × industry × year fixed effects (5565 municipalities and 27 industries). The table also reports the baseline mean outcome for the treated group at the date of displacement; the percent effect relative to the baseline mean; and the implied elasticity of crime to earnings, computed as the ratio between the percent change in crime and the percent change in earnings. All regressions are included on the right-hand side Treated_i and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

TABLE B.VIII

EFFECT OF JOB LOSS ON CRIME, ROBUSTNESS TO INCLUDING ALL WORKERS WITH A UNIQUE NAME WITHIN THEIR STATE OF RESIDENCE.

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

Note: This table shows the effect of job loss on the probability of criminal prosecution for different types of crime, as estimated from the difference-in-differences equation (2). The sample includes all workers with a unique name within their state of residence—rather than in the whole country, as in the sample used for the main analysis. The dependent variable is indicated on top of each column. The explanatory variable of main interest is a dummy Treat_i that is equal to 1 for displaced workers, interacted with a dummy Post_t equal to 1 for the period after displacement. Panel A includes in the sample all displaced workers and matched control workers employed in nonmass layoff firms who are not displaced in the same calendar year; panel B restricts the treated group to workers who are displaced in mass layoffs; and finally, panel C adds municipality × industry × year fixed effects (5565 municipalities and 27 industries). The table also reports the baseline mean outcome for the treated group at the date of displacement; the percent effect relative to the baseline mean; and the implied elasticity of crime to earnings, computed as the ratio between the percent change in crime and the percent change in earnings. All regressions are included on the right-hand side Treated_i and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

TABLE B.IX

EFFECT OF JOB LOSS ON COHABITING SONS.

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

Note: This table shows the effect of worker's displacement on the probability of criminal prosecution for sons living in the same household, as estimated from the difference-in-differences equation (2). The explanatory variable of main interest is a dummy Treat_i that is equal to 1 for the sons of workers displaced upon mass layoffs, interacted with a dummy Post_t that is equal to 1 for the period after displacement. The control group includes sons of workers employed in nonmass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. Each column reports results based on a different specification. Columns (2) presents results when excluding data from the states of Minas Gerais and São Paulo, where the share of missing data on criminal outcomes is high, above 70%. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions are included on the right-hand side Treated_i and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

TABLE B.X

EFFECT OF JOB LOSS, FAMILY INSURANCE.

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

Note: This table shows the effect of job loss on the probability that workers are found to live with a partner in CadUnico, and enter CadUnico (columns 1–2); are criminally prosecuted for workers in CadUnico living and not living with a partner in the predisplacement period ($t = 0$) (columns 3–4), as estimated from the difference-in-differences equation (2). The panel covers the period 2011–2017 when yearly snapshots of CadUnico data are available. The explanatory variable of main interest is a dummy Treat_i that is equal to 1 for workers displaced upon mass layoffs, interacted with a dummy Post_t that is equal to 1 for the period after displacement. The control group includes workers employed in nonmass 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 are included on the right-hand side Treated_i and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

APPENDIX C: APPENDIX TO SECTION 5

C.1. *Social Insurance Other Than Unemployment Benefits*

TABLE C.I

EFFECT OF JOB LOSS, SOCIAL INSURANCE.

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

Note: This table shows the effect of job loss on the probability that workers receive Bolsa Familia cash transfer and the respective amount (columns 1–2), as estimated from the difference-in-differences equation (2). The panel covers the period 2011–2017 when yearly snapshots of CadUnico data are available. The explanatory variable of main interest is a dummy Treat_i that is equal to 1 for workers displaced upon mass layoffs, interacted with a dummy Post_t that is equal to 1 for the period after displacement. The control group includes workers employed in nonmass layoff firms who are matched to treated workers on individual characteristics and are not displaced in the same calendar year. The table also reports the baseline mean outcome for the treated group at the date of displacement and the percent effect relative to the baseline mean. All regressions include on the right-hand side Treated_i and a full set of year fixed effects. Standard errors clustered at the firm level are displayed in parentheses.

C.2. *Cyclicality in Hiring and Firing of Workers*

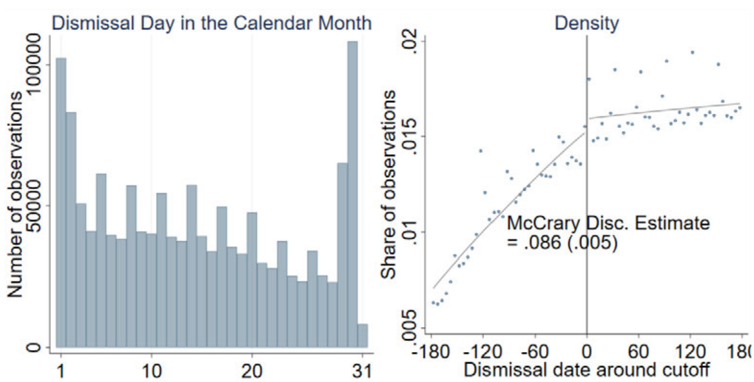

FIGURE C.1.—Cyclicality in hiring and firing of workers. *Notes*: The left graph presents the distribution of dismissal dates by calendar day within each month. The right graph presents the running variable density function around the cutoff, based on an initial sample that includes all dismissal dates.

C.3. *The Effect of UI on Crime, Evidence on Design Validity*

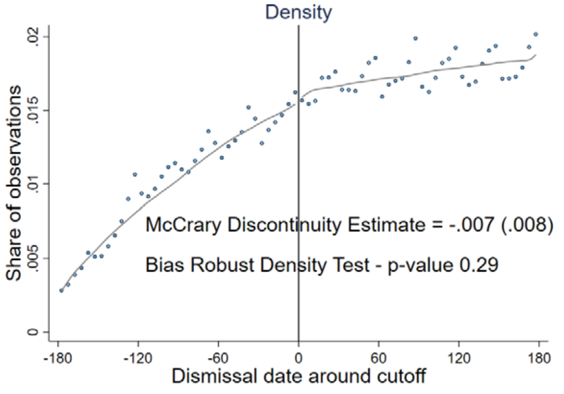

FIGURE C.2.—Distribution of observations around the UI eligibility cutoff, main sample. *Notes*: This figure shows the density of dismissal dates around the cutoff date for eligibility for unemployment benefits (i.e., 16 months since the previous layoff date in the past) in our main working sample. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. The results of McCrary density test and the bias robust test proposed by Cattaneo, Jansson, and Ma (2018, 2020) are also reported.

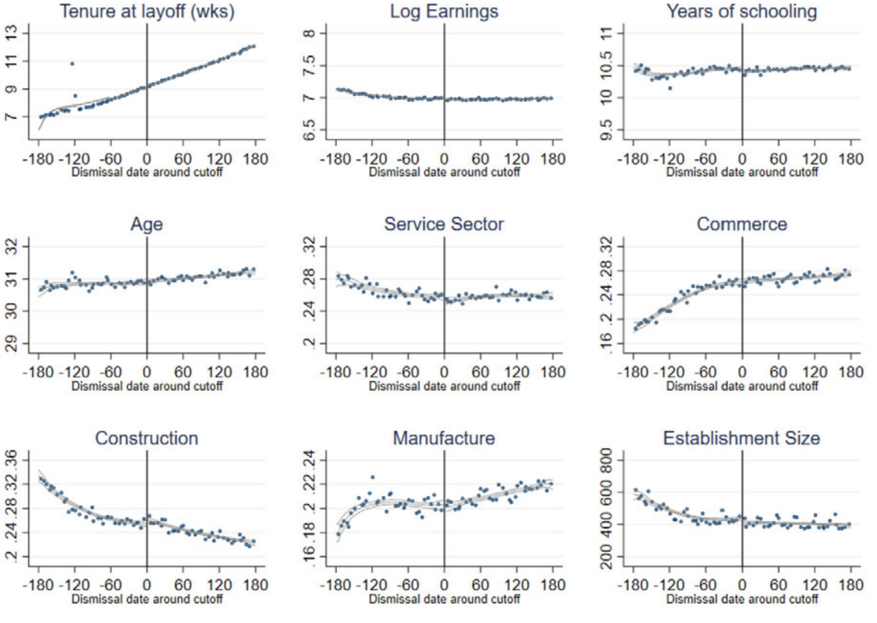

FIGURE C.3.—Balance of predetermined covariates across workers near the cutoff for UI eligibility. *Notes*: The graphs show the balance of pre-determined covariates around the cutoff for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

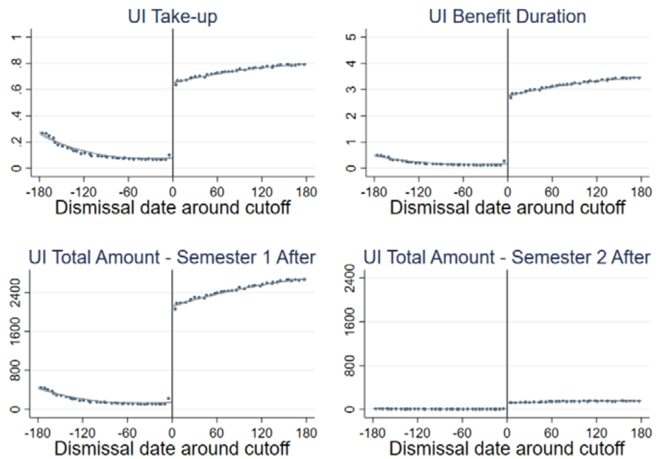

FIGURE C.4.—Effect of UI Eligibility on UI Outcomes. *Notes*: The graphs plot UI outcomes around the cutoff date for eligibility for unemployment benefits. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

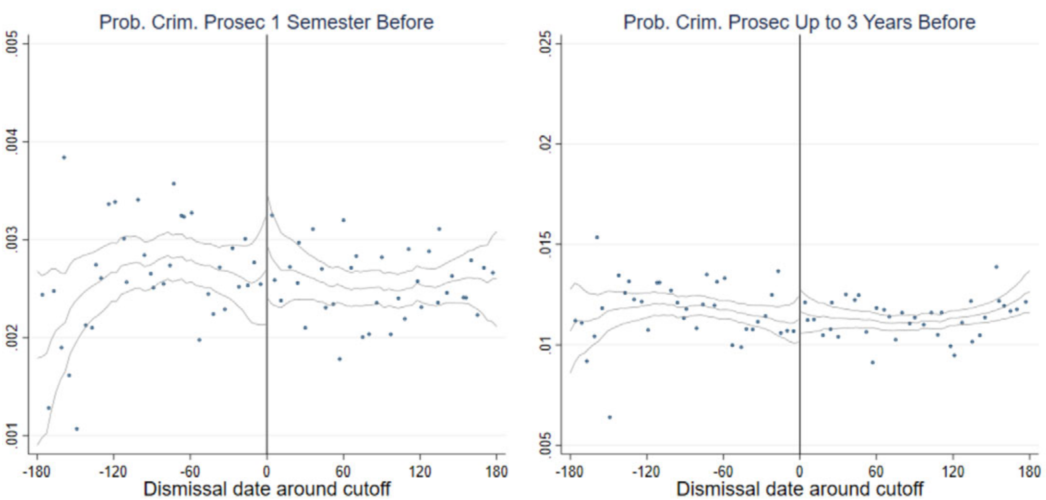

FIGURE C.5.—Effect of UI Eligibility on Crime Before Layoff (Placebo). *Notes*: The graphs plot the probability of criminal prosecution 6 months and 3 years before layoff (top and bottom graphs, resp.) around the cutoff date for eligibility for unemployment benefits, as a placebo exercise. The sample includes displaced workers with at least 6 months of continuous employment prior to layoff. Dots represent averages based on 5-day bins. The lines are based on a local linear polynomial smoothing with a 60-day bandwidth with 95% confidence intervals.

TABLE C.II

EFFECT OF UI ELIGIBILITY ON CRIME—PLACEBO.

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

Note: This table shows the effect of eligibility for UI benefits, as a placebo exercise, on the probability of being prosecuted for a crime within 6 months and 3 years before layoff, as estimated from equation (3). The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits—namely, 16 months since the previous layoff resulting in UI claims. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Each panel estimates separate regressions for the different groups, as indicated in their title. The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. The table also reports the baseline mean outcome at the cutoff and the percent effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses.

C.4. *The Effect of UI on Crime, Heterogeneity*

Figures C.6 and C.7 and Table C.III investigate the heterogeneity of RD estimates using causal forests, like we did in Section 4.4 for the effect of job loss. In this case, we cannot address heterogeneity by tenure, because workers included in the RD sample have by construction a similar (low) tenure. We focus on the narrow bandwidth of 45 tenure days around the cutoff, so that the unconfoundness assumption in Wager and Athey (2018) is likely satisfied. The causal forest algorithm is then implemented without local polynomial controls.[4]

Age is the key gradient, as younger workers respond more to benefit eligibility than older workers. Such variable displays the largest standardized difference by far, and it ranks first in terms of importance, driving 20% of the endogenous sample splits. In addition, heat plots in Figure C.7 show that the age gradient remains relevant when keeping constant income and education.

[4]This approach is similar in spirit to the local randomization of Cattaneo, Frandsen, and Titiunik (2015), Cattaneo, Titiunik, and Vazquez-Bare (2017).

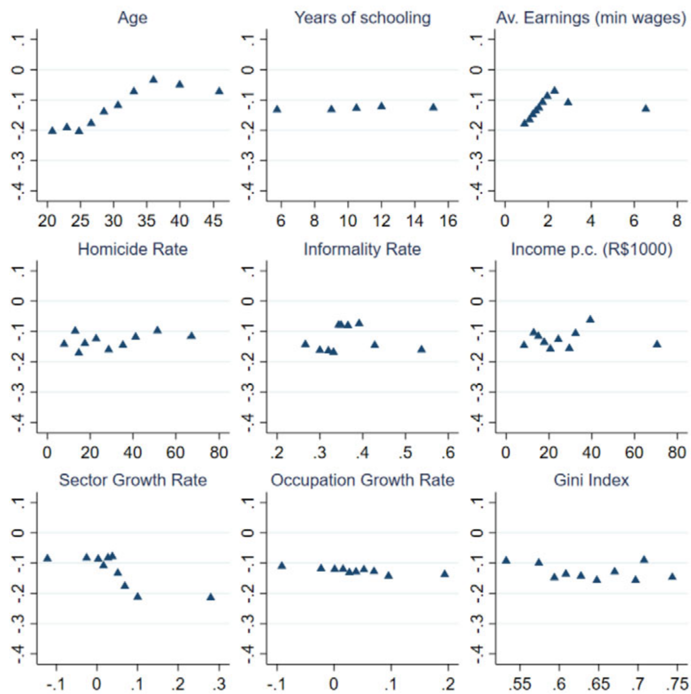

FIGURE C.6.—Conditional average treatment effects of UI eligibility, by characteristic. *Notes*: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over individual and municipality level characteristics. CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a causal forest. The causal forest is constructed within a narrow bandwidth of 45 days around the cutoff.

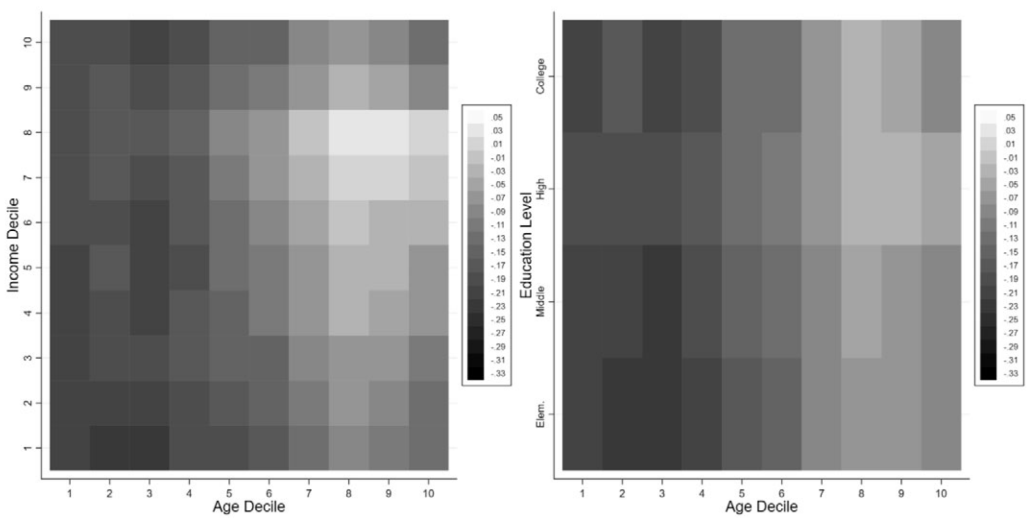

FIGURE C.7.—Conditional average treatment effects of UI eligibility, by pair of characteristics. *Notes*: This figure shows the mean predicted Conditional Average Treatment Effect (CATE) over pairs of individual characteristics, namely income and age (left graph) and education and age (right graph). CATE are estimated using causal forest algorithms and rescaled by the predicted crime outcome in the post period absent the job loss, also based on a causal forest. The causal forest is constructed within a narrow bandwidth of 45 days around the cutoff.

TABLE C.III

PREDICTED CONDITIONAL AVERAGE TREATMENT EFFECT—UI ELIGIBILITY EFFECT.

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

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

C.5. *The Effect of UI on Crime, Additional Robustness Checks*

Our RD results showing that UI eligibility reduces crime rates in the semester after layoff, while UI benefits are being paid out, (Table IV of Section 5.3) are confirmed when considering different bandwidths (including the optimal bandwidth according to Calonico, Cattaneo, and Titiunik (2014)) and controlling for different polynomial regressions in the running variable; see Table C.IV. The average effect on the total sample is marginally nonsignificant in some specifications, while the effect on younger individuals remains large and more precisely estimated, especially when controls are added, improving the precision of the estimates. We reach similar results when extending our main sample to workers with a unique name within each state (rather than within the entire country), as shown in Table C.V. Finally, we show that our main findings remain robust when dropping data from Minas Gerais and São Paulo, for which missing data in criminal records is high, above 70%. The results are presented in Table C.VI and are based on the larger sample including all individuals with a unique name within each state so that statistical power is maximized. In turn, Figure C.8 confirms that both the estimated effect on the main sample and on the subsample of younger workers are statistically different from placebo distributions obtained by running the same estimates on placebo cutoff dates.

In Table C.VII, we control for the cyclicality in hiring/firing discussed in Section 5.2. We focus on the larger sample including all individuals with a unique name within each state, which provides higher statistical power. In the first four columns, we progressively include fixed effects for the individual-specific cutoff date and for each dismissal date—defining the running variables—thus relying on variation in the worker-specific dismissal date within groups who have the same cutoff date. In the last two columns, we also enlarge the sample to include all workers who were initially dismissed near the beginning and the end of calendar months, thus dropping our initial restriction. All regressions include individual controls and municipality fixed effects, which increase the precision of estimates. Both the average effect on the total sample and on the younger group are statistically sig-

TABLE C.IV

EFFECT OF UI ELIGIBILITY ON CRIME ONE SEMESTER AFTER LAYOFF, ROBUSTNESS TO DIFFERENT SPECIFICATIONS.

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

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

TABLE C.V

EFFECT OF UI ELIGIBILITY ON CRIME IN THE FIRST SEMESTER AFTER LAYOFF, EXTENDED SAMPLE INCLUDING ALL WORKERS WITH A UNIQUE NAME WITHIN THEIR STATE OF RESIDENCE.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Var.: Criminal Prosecution—1 Semester After Layoff								
PANEL A. YOUNGER WORKERS, AGE $\leq$ 29								
Eligibility to UI benefits	−0.00088	−0.00078	−0.00086	−0.0014	−0.00080	−0.00082	−0.0013	−0.0013
	(0.00039)	(0.00033)	(0.00043)	(0.00057)	(0.00047)	(0.00041)	(0.00053)	(0.00051)
Observations	91,432	136,144	72,555	180,129	264,975	345,411	208,259	481,454
PANEL B. YOUNGER WORKERS, AGE $\leq$ 29, WITH CONTROLS								
Eligibility to UI benefits	−0.00093	−0.00082	−0.0010	−0.0016	−0.00089	−0.00081	−0.0012	−0.0014
	(0.0004)	(0.00034)	(0.00042)	(0.00057)	(0.00047)	(0.00041)	(0.00054)	(0.00051)
Observations	90,025	135,355	79,844	179,365	264,258	344,731	208,893	480,820
Bandwidth (days)	30	45	CCT	60	90	120	CCT	180
Polynomial Order	0	0	0	1	1	1	1	2

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

TABLE C.VI

EFFECT OF UI ELIGIBILITY ON CRIME, DROPPING STATES WITH HIGH MISSING SHARE, EXTENDED SAMPLE INCLUDING ALL WORKERS WITH A UNIQUE NAME WITHIN THEIR STATE OF RESIDENCE.

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

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 one semester and 3 years after layoff (columns 4–7). The sample includes all individuals with a unique name within their state of work—rather than in the whole country, as in the sample used for the main analysis; and excludes data from the states of Minas Gerais and São Paulo, where the share of missing data on criminal outcomes is high, above 70%. The local linear regression includes a dummy for eligibility for UI benefits (i.e., the variable of main interest), time since the cutoff date for eligibility, and the interaction between the two. Each panel estimates separate regressions for the different groups, as indicated in their title. The control set includes tenure, earnings, education, firm size, dummies for white workers and sectors (services, retail, construction, manufacturing), and municipality fixed effects. The table also reports the baseline mean outcome at the cutoff and the percent effect relative to the baseline mean. Standard errors are clustered at the individual level and displayed in parentheses.

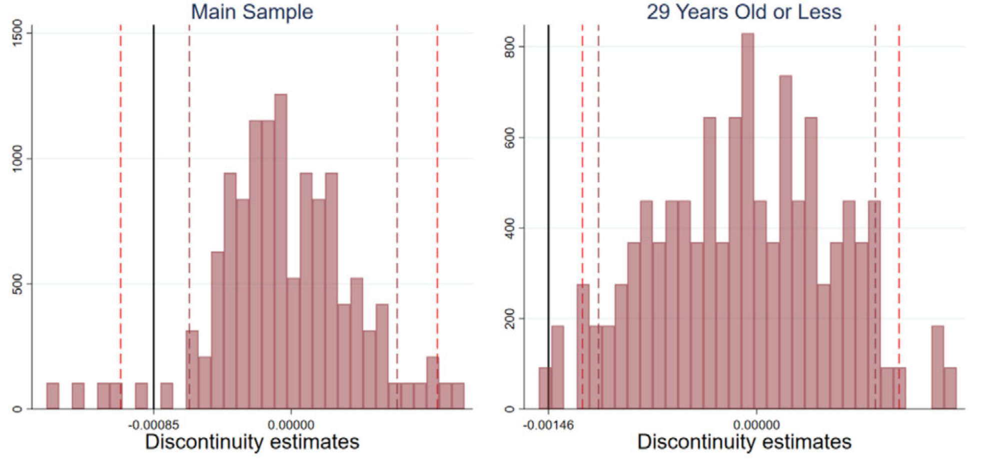

FIGURE C.8.—Effect of UI eligibility on crime one semester after layoff, permutation tests. *Notes*: The graphs compare discontinuity estimates of the effect of UI eligibility on the probability of criminal prosecution one semester after layoff obtained at the true cutoff for UI eligibility (vertical line) with the distribution of estimates obtained at all possible placebo cutoffs within 180 days away from the actual threshold, for different groups (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95, and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in equation (3).

TABLE C.VII

EFFECT OF UI ELIGIBILITY ON CRIME ONE SEMESTER AFTER LAYOFF, ROBUSTNESS TO CYCLICALITY IN HIRING AND FIRING OF WORKERS, EXTENDED SAMPLE INCLUDING ALL WORKERS WITH A UNIQUE NAME WITHIN THEIR STATE OF RESIDENCE.

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

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

nificant in all specifications. Finally, Table C.VIII shows that our main results remain robust when using the inference method proposed by Gerard, Rokkanen, and Rothe (2020), which estimates effect bounds while allowing for some degree of manipulation in the RD running variable.

Table C.IX shows the robustness for the effect of UI eligibility on crime up to 3 years after the layoff in the sample of younger workers. While results are not always statistically significant in the main specification, they are robust when including individual controls and municipality fixed effects. Figure C.9, right panel, shows that the impact on younger workers is also robust to randomization inference. Overall, although the evidence is not particularly strong, it indicates that the crime reducing effects of UI eligibility in the semester following displacement do not bounce back over time.

TABLE C.VIII

EFFECT OF UI ELIGIBILITY ON CRIME ONE SEMESTER AFTER LAYOFF, MANIPULATION INFERENCE.

	(1)	(2)	(3)	(4)
Dep. Var: Prob. Prosecution 6 Months After Layoff	Full Sample		Young Workers Age $\leq$ 29	
	Estimate	C.I.	Estimate	C.I.
PANEL A. MAIN ESTIMATES				
Share always assigned	0.035		0.052	
ITT: Ignoring manipulation	−0.00077	[−0.00164, 0.00011]	−0.00131	[−0.00261, −0.00001]
ITT: Bounds inference	[−0.0037, −0.00066]	[−0.00427, 0.0001]	[−0.00429, −0.00115]	[−0.00512, −0.00002]
PANEL B. HYPOTHETICAL SHARE OF MANIPULATION				
Share always assigned				
0.025	[−0.0037, −0.00069]	[−0.00425, 0.00005]	[−0.00429, −0.00123]	[−0.00511, −0.00013]
0.05	[−0.0037, −0.00061]	[−0.00425, 0.00014]	[−0.00429, −0.00115]	[−0.00511, −0.00003]
0.1	[−0.0037, −0.00044]	[−0.00425, 0.00033]	[−0.00429, −0.00098]	[−0.00511, 0.00017]
0.15	[−0.0037, −0.00025]	[−0.00425, 0.00054]	[−0.00429, −0.00078]	[−0.00511, 0.0004]
0.2	[−0.0037, −0.00003]	[−0.00425, 0.00078]	[−0.00429, −0.00057]	[−0.00511, 0.00065]

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

TABLE C.IX

EFFECT OF UI ELIGIBILITY ON CRIME 3 YEARS AFTER LAYOFF, ROBUSTNESS TO DIFFERENT SPECIFICATIONS.

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

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

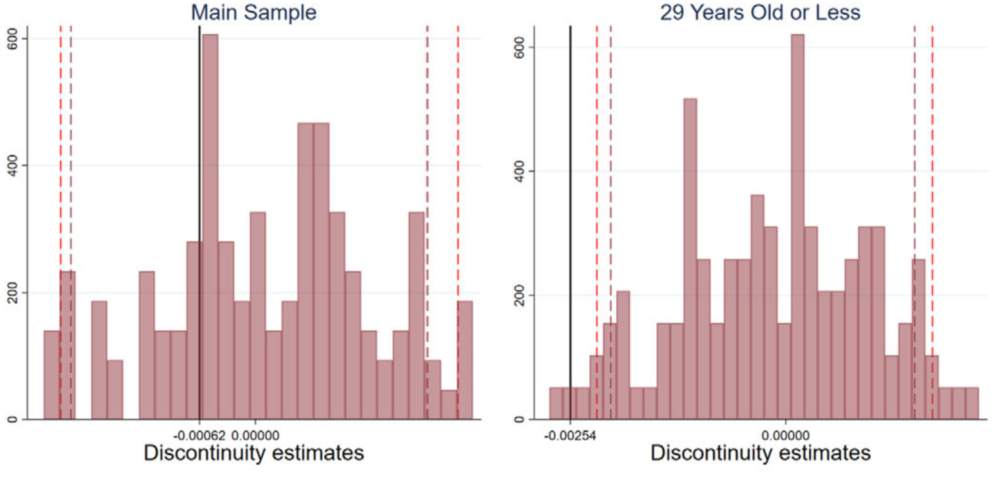

FIGURE C.9.—Effect of UI eligibility on crime 3 years after layoff, permutation tests. *Notes*: The graphs compare discontinuity estimates of the effect of UI eligibility on the probability of criminal prosecution three years after layoff obtained at the true cutoff for UI eligibility (vertical line) with the distribution of estimates obtained at all possible placebo cutoffs within 180 days away from the actual threshold, for different groups (indicated on top of each graph). The dashed lines represent the 2.5, 5, 95, and 97.5 percentiles in the distribution of placebo cutoffs. Estimates are based on a local linear polynomial smoothing with a 60-day bandwidth, as in equation (3).

C.6. *The Effect of UI on Crime, Additional Results*

TABLE C.X

EFFECT OF UI ELIGIBILITY ON EMPLOYMENT.

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

Note: This table shows UI eligibility effects on employment outcomes, as estimated from equation (3). The sample includes displaced workers with at least 6 months of continuous employment prior to layoff who are displaced within a symmetric bandwidth of 60 days around the cutoff required for eligibility to unemployment benefits. The local linear regression includes a dummy for eligibility for UI benefits, time since the cutoff date for eligibility, and the interaction between the two. Standard errors are clustered at the individual level and displayed in parentheses. Labor income is measured in Brazilian Reais; and unemp. dur. is censored at 36 months.

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