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Gaurav Khanna
Carlos Medina
Anant Nyshadham
Christian Posso
Jorge Tamayo

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Gaurav Khanna

University of California, San Diego

Carlos Medina

Banco de la Republica de Colombia

Anant Nyshadham

University of Michigan

Christian Posso

Banco de la Republica de Colombia

Jorge Tamayo

Harvard Business School

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Gaurav Khanna^{*}

Carlos Medina[†]

Anant Nyshadham[‡]

Christian Posso[§]

Jorge Tamayo[¶]

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Abstract

We investigate the effects of job displacement, as a result of mass-layoffs, on criminal arrests using a matched employer-employee-crime dataset from Medellín, Colombia. Job displacement leads to immediate and persistent earnings losses, and higher probability of arrest for both the displaced worker and family members. Leveraging a banking policy-reform, we find that greater access to credit attenuates the criminal response to job loss. Impacts on arrests are pronounced for property crimes and among younger men for whom opportunities in criminal enterprises are prevalent. Taken together, our results are consistent with economic incentives contributing to criminal participation decisions after job losses.

Keywords: Job displacements, crime, Medellín

JEL Codes: K42, J63, J65

^{*}University of California – San Diego, gakhanna@ucsd.edu, www.econgaaurav.com

[†]Banco de la Republica de Colombia, cmedindu@banrep.gov.co

[‡]University of Michigan & NBER, nyshadha@umich.edu, anantnyshadham.com

[§]Banco de la Republica de Colombia, cpossosu@banrep.gov.co

[¶]Harvard University, Harvard Business School, jtamayo@hbs.edu, jorge-tamayo.com

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1 Introduction

Job losses have been shown to have substantial impacts on the lives of individuals, from reductions in long-run earnings and employability (Couch and Placzek, 2010; Jacobson et al., 1993) to depression and deterioration of health and well-being (Aghion et al., 2016; Black et al., 2015; Charles and Stephens, 2004; Del Bono et al., 2012; Sullivan and Von Wachter, 2009). These effects could lead to increases in criminality. Indeed, empirical studies of crime document criminal response to both economic incentives (Bignon et al., 2016; Blattman and Annan, 2015; Watson et al., 2019) and a myriad of other behavioral and psychological conditions (Anderson et al., 2015; Blattman et al., 2017; Bondurant et al., 2018; Carpenter, 2005; Lindo et al., 2018). Many low and middle income countries, particularly across Latin America, suffer from a combination of high employment volatility and rampant crime (Dell et al., 2018; Dix-Carneiro et al., 2018), representing ideal contexts in which to investigate the criminal responses to job losses.

We combine rich granular data on the universe of arrests over a decade in Medellín, Colombia, with administrative records on the universe of formally employed workers, the firms for which they work, and household characteristics. We focus on mass-layoff events where large groups of workers lose their jobs. We conduct an event-study analysis to estimate the impact of displacement on the probability of being captured after the event. We assume that these events are unanticipated by, and uncorrelated with the characteristics of affected individuals, such as their proclivity to commit crimes. We show that the displacement event is not associated with the likelihood of being arrested before such events, confirming that dynamic selectivity into displacement is unlikely.

We find that workers suffer significant earnings losses after job displacement that continue to accrue for at least five years. They exhibit a corresponding spike in the likelihood of arrest in the year of job separation and a continued higher likelihood of arrests in subsequent years. This crime response to job displacement is both large and far-reaching in that criminal responses spill over to youth in the household as well.

We then investigate the degree to which financial necessity following job loss is contributing to the criminal response. In particular, by obtaining unique administrative data on the credit histories of individuals, and leveraging a credit-policy reform that unexpectedly expanded access to credit in some neighborhoods more than others, we document the impact of access to an important consumption

smoothing mechanism on the elasticity between job loss and crime. Our instrumental variable strategy relies on the assumption that distance to the newly established bank branches (used to predict credit) is not associated with other household characteristics that may also attenuate the employment-crime relationship coincidentally at the time of new bank branch openings. We find that access to consumption credit weakens the relationship between job loss and criminality, consistent with consumption necessity contributing to criminal responses.

Our study adds to a broader literature that leverages administrative records to study individual entry into criminal activity (Beatton et al., 2018; Bennett, 2018; Carpenter and Dobkin, 2015; Cook and Kang, 2016; Damm and Dustmann, 2014; Doleac, 2017). These studies document demographic patterns of entry into crime, as well as the impacts of policies such as minimum drinking ages and DNA databases on criminal participation. Though some papers focus on poorer areas or those with higher incidence of criminal activity in the United States (Depew and Eren, 2016; Ludwig et al., 2001; Palmer et al., 2019), ours is the first study, to our knowledge, that exercises individual-level matched administrative records from a developing country context with historically high levels of criminality.

We also leverage firm-level mass layoff events, unfortunately common in similar developing country settings, to identify the relationship between employment volatility and participation in crime. In so doing, our results relate to the evidence on “scarring” effects of limited employment opportunities at pivotal moments – such as upon graduation from school, entry into a new country, or release from prison – on subsequent criminal participation decisions of individuals (Bell et al., 2018, 2013; Galbiati et al., 2017; Gould et al., 2002; Schnepel, 2018; Yang, 2017). While these studies are similar to ours in that they link employment opportunities to incentives to participate in criminal activity, our study of job displacements in the broader labor market does not focus on stylized, potentially at-risk populations such as recent immigrants or ex-convicts. Additionally, we study the impacts of employment shocks experienced at the individual level, rather than aggregate economic conditions which also parallel criminal opportunities and police resources.¹

Though a large literature has documented the importance of job destructions on economic liveli-

¹Relatedly, we contribute to the study of the economic motives for criminal employment (Becker, 1968; Ehrlich, 1973). Recent studies present empirical evidence of individual-level sorting into crime based on labor-market policies, and incentives (Fu and Wolpin, 2017; Khanna et al., 2019). We add similar evidence on criminal responses to employment shocks by exploiting individual-level variation in job displacement and access to credit for meeting stop-gap consumption needs, to examine economic incentives as mechanisms underlying criminal responses to employment shocks.

hoods and general well-being, fewer papers, many recent and yet unpublished, have studied the impacts of these events in Latin American settings (Albagli et al., 2020; Amarante et al., 2014; Firpo and Gonzaga, 2010; Hoek, 2006; Kaplan et al., 2010; Menezes-Filho, 2003; Saltiel, 2018). Ours is the first to our knowledge to document impacts on crime. We leverage administrative records including both employment and measures of criminal activity, which are rare in developing-country settings.

In fact, only a few studies have documented impacts of job displacement on criminal activity in any empirical context (Bennett and Ouazad, 2020; Rege et al., 2019; Rose, 2020). Our results indicate, both immediately following the job loss and over the following years, stronger employment-crime elasticities than those found in these other papers, all of which study developed countries.² In general these papers find larger effects on employment in both the short and long-run, but smaller or similar-sized effects on criminality. We contribute evidence on intrahousehold spillovers across gender and age-groups and also provide the first evidence of the mitigative impacts of access to credit.³

Finally, we add to the literature on the intergenerational spillovers of crime (Hjalmarsson and Lindquist, 2013; Meghir et al., 2012) and impacts of job loss (Hilger, 2016; Oreopoulos et al., 2008; Rege et al., 2011) by documenting criminality responses among younger relatives. We provide evidence that shocks to adult employment in the household can have ripple effects on young relatives' criminality. Ignoring such spillovers will lead to gross underestimates of the long-term consequences of job loss on crime.

The rest of the paper is organized as follows. Section 2 describes the data and context. Section 3 presents our empirical strategy, and section 4 reports the main results. Section 5 discusses possible mechanisms, and section 6 concludes.

²See Tables A4 and A5 in the Appendix for comparative elasticity calculations across Bennett and Ouazad (2020); Rege et al. (2019); Rose (2020). We discuss these calculations in greater detail in the Conclusion.

³In fact, to the best of our knowledge, we are among the first to study any relationship between credit access and crime. There is work studying the effects of credit shocks on the financial market, for instance, Angelini and Cetorelli (2003); Gissler et al. (2019); Spiller and Favaro (1984); Tewari (2014); Yildirim and Philippatos (2007).

2 Data and Background

2.1 Data Sources and Summary Statistics

Located in the north-western region of Colombia, Medellín is the second largest city after the capital, Bogotá. It has strong industrial and financial sectors with approximately 2.3 million people or 5.5% of the Colombian population. The urban zone consists of 249 neighborhoods, divided into 21 (*comunas*).

For our analysis, we combine four sources of administrative data using individual identification numbers and dates of birth. The first is the Integrated Information System for Social Protection (SISPRO), containing information from the Integrated Registry of Contributions for all formal workers contributing to health and pension schemes (PILA). The PILA has detailed information on payroll, earnings, days worked, firm and worker identifiers, and demographic information of employees. Using the PILA, we build an employer-employee panel following both individuals and firms over time.

The second data source, from the Judicial Research Unit of the Metropolitan Police of Medellín (SIJIN), is the census of all individuals arrested in Medellín between 2006 and 2015. These data contain type of crime committed, the date and place of arrest, and identifier of the arrested individual. The third source of information is the second wave of the System for the Identification of Potential Beneficiaries of Social Programs (SISBEN II). SISBEN II was introduced in 2005 and classified households into six different socio-economic levels according to the SISBEN score. In particular, these data allow us to identify family members and addresses of households.

Finally, we use data from “Individual Debtor Report and Active Credit Operations” or “Form 341” from the *Superintendencia Financiera de Colombia* (Superfinanciera), the Colombian government agency responsible for overseeing financial regulation. Form 341 provides quarterly records of the census of all interactions with the formal financial sector since 2004, including credit cards, car loans, consumer credits, mortgages, etc. We link these 4 sources of data together at the individual level for all workers in the PILA in 2010. That is, we measure the unexpected firm-level mass-layoff events in 2010 and follow individuals’ earnings from 2008 to 2015 and arrests from 2006 to 2015.

Table A2 presents summary statistics. 58% of the workers are male and the average age in 2009 was 35.5 years. The average inflation-adjusted monthly earnings is COL\$909,997 (about US\$462 in 2009). The unconditional probability of arrest is 0.19%.

2.2 Employment, Job Loss and Access to Credit

Outside the formal sector, the opportunities for individuals lie between legitimate employment in the informal sector and illegitimate forms of employment. We use the Large Household Survey (or Gran Encuesta Integrada de Hogares, GEIH 2010) to document the differences in formal and informal opportunities. Men are slightly more likely than women to be informally employed (55.7% vs 48.7%). Youth have similar rates of informality (55.6%) as the general male population. Yet, the formal sector is far more lucrative, with guaranteed social benefits, pensions, health insurance, and much higher pay – average formal sector (post-tax, monetary only) earnings are about 2.3 times informal earnings. On average, informal workers earn only roughly the formal-sector minimum wage, with many earning much less. As such, involuntary separations from formal sector employment can generate meaningful losses to incomes and livelihoods.

We follow the literature on mass layoffs (e.g., [Jacobson et al. \(1993\)](#)) and impose sensible restrictions to our sample. We study private firms with at least 11 workers, and full-time employees aged 20 to 60 with at least 1 year of tenure in the same firm.⁴ Table [A1](#) shows how our sample changes, as we add these restrictions. We again follow the literature and define a mass layoff as an event in which a firm lays off between 30% and 90% of its employees over a six month period in 2010. Our results are robust to alternative cutoffs, as we show in Figure [A1](#). The final sample consists of 457,096 individuals and 11,739 firms, where 28.7% of the firms suffered a mass layoff event affecting 27.9% of the individuals in the estimation sample.⁵

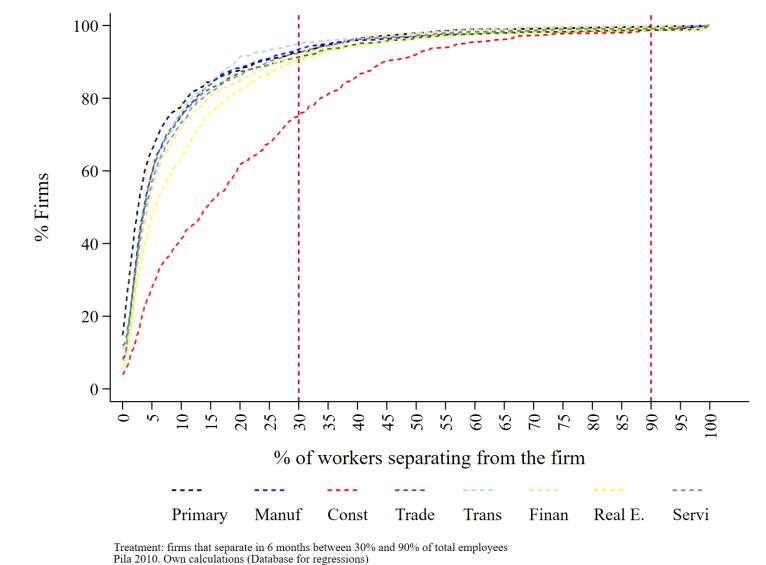
The prevalence of mass layoffs in Medellín varies across industries. Figure [1](#) documents patterns in layoffs, where we divide industries into 8 large groups. On the vertical axis we plot the cumulative distribution of firms that have, at most, a certain fraction of workers separating from the firm (measured on the horizontal axis). The figure shows that separations occur across industries. There is less job churning in the primary (agricultural) sector and a higher rate of job loss in construction.

We examine how the access to credit mitigates the relationship between job loss and crime, using the Superfinanciera Form 341 data. Exposure to credit varies substantially at the individual level.

⁴Most related studies impose restrictions on firm size to protect against young, unstable firms disproportionately driving results and impose minimum tenure restrictions to avoid misinterpreting planned or expected separations of temporary workers.

⁵This rate of displacement is higher than the US in normal times ([Flaaen et al., 2019](#)), but similar to other Latin American contexts ([Firpo and Gonzaga, 2010](#)). We cap the layoffs at 90%, as a 100% separation rate may simply indicate a change in ownership without mass layoffs.

Figure 1: Industry-wise Cumulative Distribution Function of Firms by Fraction of Workers Laid Off



Notes: Figure 1 shows the distribution of layoffs across industries for one year (here, 2010). We use the employer-employee matched PILA data and define a separation to be if the worker is no longer employed at the firm on any future date. We then plot the cumulative distribution function of firms that have different fractions of separations by industry. For instance, in the manufacturing sector (dark blue dashed line), 71% of firms had fewer than 30% of their workers separate from their firm, or 29% of firms had at least 30% of their workers separate.

At baseline, prior to the policy-reform-induced expansion discussed below, youth are less likely to have lines of credit (29% vs 53% for older adults), whereas men (48%) and women (53%) have more balanced utilization rates. 44% of the poor (as measured by eligibility for social programs) have credit access, as compared to 56% of the non-poor. In order to cut past these demographic differences in exposure, we will leverage a credit-expansion program to identify the effect of consumption credit on the employment-crime elasticity.

2.3 Crime in Medellín

Medellín is known historically for being one of the most violent cities in the world. Despite declining homicide rates over the last several decades, Medellín still had the tenth highest homicide rates in the world in 2010, behind cities in Afghanistan and other parts of Latin American (CCSPJP, 2010). Anthropological studies and in-person interviews show that economic incentives drive young men in Medellín to join crime (Baird, 2011). As many respondents highlight, the reason to join crime is mostly “economic” or for a profitable career.⁶ Knowing this, paramilitaries and gangs actively recruit

⁶See Baird (2011) interview with Gato, p264 and interview with Armando, p197.

men who are “idle” and without a good job.⁷ In fact, remunerations for gang-members are often higher than legitimate jobs for those with similar levels of education (Doyle, 2016). Blattman et al. (2018) estimate that foot soldiers of the *combos* (hundreds local street-gangs) receive well above national minimum wage whereas *combo* leaders earnings put them in the top 10% of income earners in the city.

The majority of arrests are in *flagrancia*; that is, the suspect was apprehended around the scene of the crime by the police (or by others, in advance of police arrival). Most property crimes fall under this category, and usually have a ‘victim’ at hand who often witnesses the crime. A smaller fraction (roughly one-tenth) of the arrests are by judicial order, where a judge issued a warrant. Once arrested, the police must release all for whom due process was not followed (including not reading them their rights or other mistreatment). The suspect is then brought in front of a prosecutor who may release the criminal, or formally prosecute and produce them in front of a judge.

Arrests are concentrated among younger males. Over the entire sample period, 12% of males (across all ages) were at some point arrested, while the arrest rate for females was only 1%.⁸ Youth, between 14 and 26 years, are far more likely to be involved in crimes than other age groups. For young men, 21.5% were arrested over the period of study – 11.1% for drug crimes (consumption or trafficking), 5.6% for property crime (like burglary and theft), and 4.8% for violent crimes (homicides, assault, extortion, and kidnapping).

These numbers are high relative to most contexts. Yet, the US has an incarceration rate more than six times the typical OECD nation, where one in ten youths from a low-income family may join crime, 60% of crimes are committed by offenders under the age of 30, and 72% by males (Kearney et al., 2014). Accordingly, in some regards, arrests in our context are similar to not only high-crime regions in many parts of the developing world and Latin America, but also the US.

We restrict our analysis to data on first arrests. Repeat arrests are excluded as time spent under incarceration and the length of sentencing may be endogenous to other characteristics.⁹ First arrests

⁷An interview with El Mono (p191) documents the recruitment process: “those guys would hang out around here and be nice to me and say ‘come over here, have a bit of money’.” Having a reasonable job means that one is not “hanging around the neighborhood” when the gangs come recruiting. A desirable outside option would be a job with benefits and social security (see interview with El Peludo, p184). Indeed, the options are often presented as an occupational choice: “are you gonna work [for the gang] or do a normal job?” (see interview with Notes, p193, (Baird, 2011).

⁸Authors calculations based on data from the Judicial Research Unit of the Metropolitan Police of Medellín (SIJIN).

⁹We show in Appendix Figure A2 that our results are robust to including repeat arrests.

most closely map to the first decision node between legal and illegal activities. Once captured a criminal career begins, with subsequent decisions to repeat, escalate, or exit the criminal sector based on many factors we do not observe (including prison sentences).

3 Empirical Strategy

Our aim is to compare arrest rates between those who lose a job and those who do not. Yet, the individual probability of job loss may be correlated with an individual’s proclivity to commit crimes. That is, for instance, delinquent behavior inside and outside the workplace may go together. To get around this endogeneity issue, we leverage variation from mass layoffs at the firm.

Our baseline specification estimates the impact of job displacements on the probability of being arrested after the event. We use an event study model which allows us to check for differential pre-trends (between workers who were exposed to mass layoffs and workers who were not), and to estimate the dynamic consequences in the post-layoff period. We use the following event study model:

$$Arrested_{i,t} = \alpha_i + \gamma_t + X_{i,t}\beta + \sum_{-4 \leq k \leq 5, k \neq -1} Displaced_{i,t-k} \delta_k + \varepsilon_{i,t}, \quad (1)$$

where $Arrested_{i,t}$ is an indicator recording whether individual i was arrested at time t , $Displaced_{i,t-k}$ is an indicator for whether the individual worked in a firm that displaced at least 30 percent of its workers in year $t - k$, and k indexes the set of time indicator variables beginning four years prior to the displacement up to five years after the event. The parameters δ_k measure the impact of displacement before, during, and after the event. We also estimate similar effects on the individual’s yearly earnings. In additional analysis we document cumulative effects on arrests, by redefining the outcome $Arrested_{i,t} = 1$ if the individual was ever arrested between the time of the mass-layoff event and year t . Following [Cameron et al. \(2011\)](#), and since we use matched employer-employee data, we cluster standard errors at the firm and employee level for inference.

Our specification controls for time fixed effects γ_t , and individual fixed effects α_i that account for an individual’s time-invariant characteristics. Our parameters of interest are δ_k for $k = 0, 1, \dots, 5$. Our empirical specification includes a constant and as such estimated δ_k parameters are relative to the probability of being captured the year prior to the event δ_{-1} . We interpret the significance of these

coefficients as evidence of the causal relationship between job displacement and crime. Additionally, the coefficients δ_k for years prior to the event test whether the upcoming displacement event is correlated with the probability of being arrested before the event. An absence of meaningful effects in the pre-period would confirm that individual-level dynamic selection into a mass layoff is unlikely.

Identification relies on two features of the mass-layoff. First, the mass-layoff is unanticipated by workers and uncorrelated with worker-specific characteristics. This assertion is partially testable by estimating pre-layoff coefficients. Second, mass-layoffs generate substantial losses to earnings and the likelihood of employment. We hypothesize that such unexpected losses drive some individuals to sort into crime as their most lucrative alternative.

4 Displacements and Arrests

We first present results estimating equation 1 for earnings and crime outcomes by gender and age. We also explore the effects on arrests of relatives.

4.1 Effects of Job-Loss on Earnings and Arrests

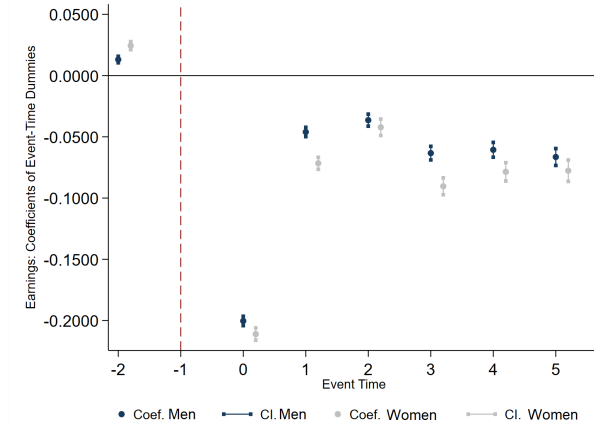
Figures 2a and 2b show earnings effects associated with job displacement events. We observe a sharp loss in earnings in the year of the displacement, with somewhat muted but persistent effects up to five years after the event.¹⁰ We observe large losses in the year of the job displacement, continuing to accrue up to 3 years after the layoff, where earnings are still lower by about 22% relative to average earnings in 2009 of \$COL 909,997. The fall in earnings is similar across genders and age groups.

Layoffs generate an immediate response in criminal behavior. Figures 2c and 2d, show the results for the probability of arrest across genders and age groups, respectively. Table A3 and Figure A4 show the effects on earnings and arrests for the pooled sample.¹¹ The average probability of arrest is 0.16% the year before the event, and for the pooled sample it increases by 47% in the year of the event for displaced workers, and 35% the year after. The incremental yearly effect continues to be positive thereafter, but steadily diminishes.

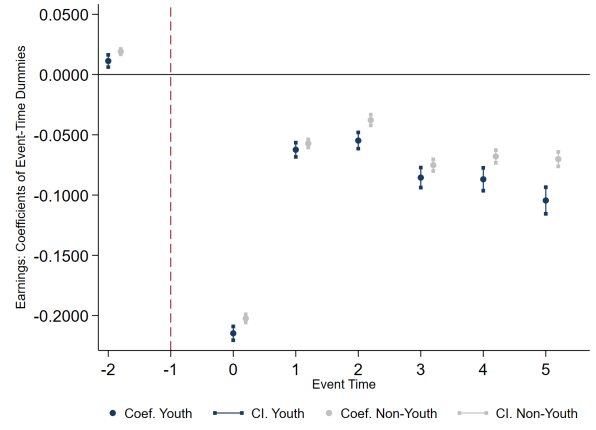
¹⁰Figure A5 shows the effect of mass layoffs, five years after the event, on the probability of being formally employed for at least six months.

¹¹We show the cumulative effect on arrests in Figure A6.

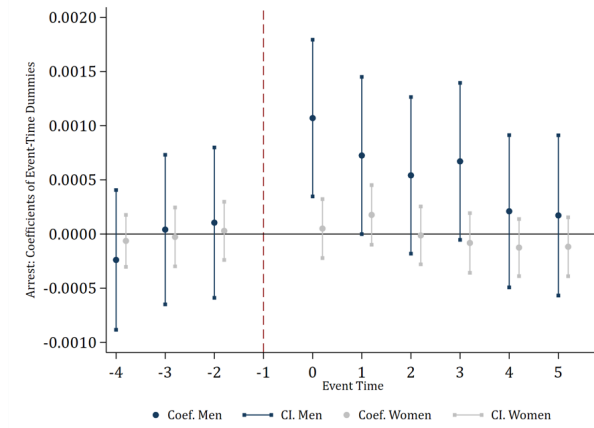
Figure 2: Effects of firm-level mass layoffs on earnings and arrests



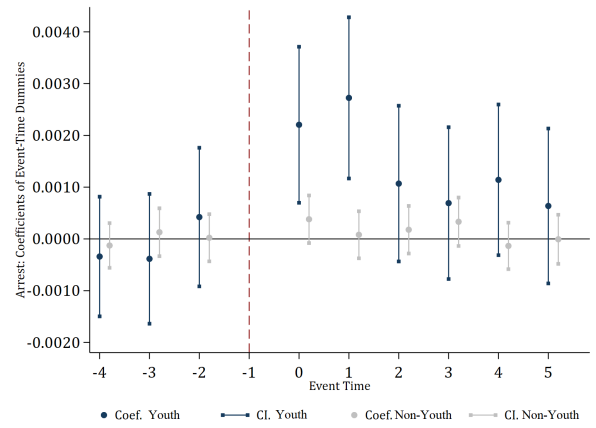
(a) Event study estimates on earnings by gender



(b) Event study estimates on earnings by age group



(c) Event study estimates on arrests by gender



(d) Event study estimates by age group

Notes: Figure 2a and 2b show the effects of a mass layoff event, from two years before the event to five years after the event on average annual earnings (trimming earnings at the 1% and 99%). We show effects separately for men and women (Figure 2a), and for youth (ages 20-25) and non-youth (ages 26-60) (Figure 2b). We compute annual formal sector earnings by summing inflation-adjusted monthly earnings, using 2008 as a base year. Number of observations for men: 10 years x 266521 individuals. Number of observations for women: 10 years x 190575 individuals. Number of observations for youth: 10 years x 69902 individuals. Number of observations for non-youth: 10 years x 387194 individuals. Figures 2c and 2d show heterogeneous effects of mass layoff events on arrest by the same gender and age groups, respectively. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figures. Layoff event is defined as those firms where 30-90% of their employees were separated in 2010. Arrest is 1 if the employee was arrested at least once in a year.

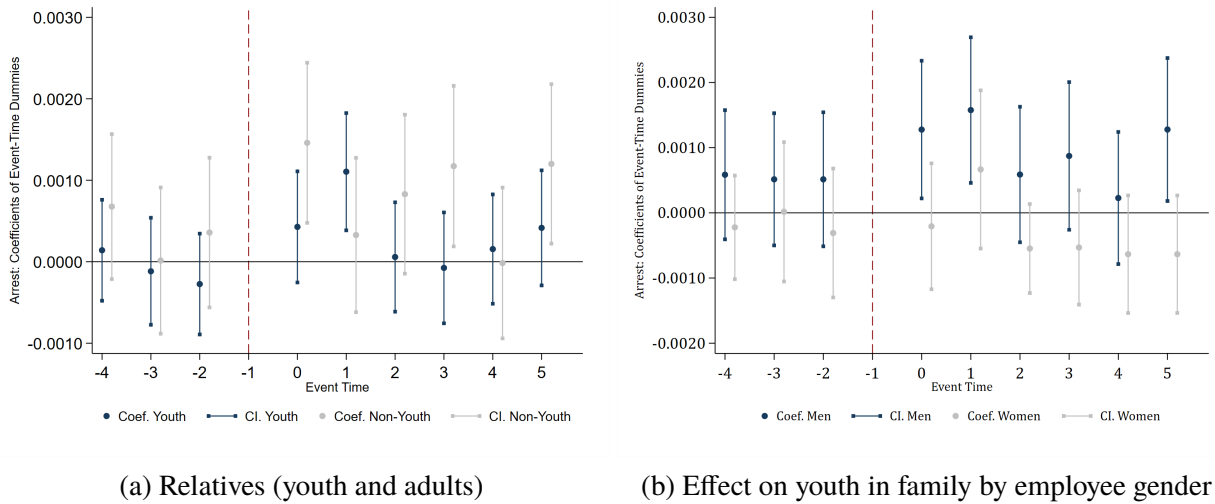
We find that among all workers exposed to mass layoffs, the increase in arrests is most pronounced for males and youth (ages 20-25). This pattern may reflect the opportunity to join criminal enterprises and gangs being greater for young men. Like in other high-crime settings, Medellín shows a strong crime-age pattern where the arrest rate steady declines after the age of 25. In Appendix Figure A3 we document effects by type of crime. Property crime seems to have a sharper response than violent

crime, perhaps reflecting financial necessity following job loss.

Across specifications, the coefficients δ_k for years prior to the event ($k = -2, -3, -4$) allow us to test for differential pre-trends, i.e., whether the onset of the displacement event is correlated with the probability of being arrested before the event. In general, we do not find individual or joint statistical significance in such coefficients (coefficients and test statistics reported in Table A3). We interpret this evidence as the absence of dynamic selectivity into job displacement on the basis of arrest likelihood, supporting the validity of the design.

4.2 Spillovers to Family Members

Figure 3: Event study estimates of arrests on family members



Notes: Figures show the effect of a firm's mass layoff event (where 30-90% of employees were separated) on arrests (equals 1 if the employee was arrested at least once in a year) from four years before the event to five years after the event. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. Here we only use the subsample of relatives of workers in SISBEN II households, where family members do not work in the same firm. Figure 3a compares the effects on other family members. We split it up by youth in the household (ages 14-24) and non-youth (ages 25-35). In Figure 3b we look at the change in arrest rates for other youth (ages 14-24) in the household that were not already working for a firm with mass-layoffs. We examine effects by gender of the laid-off adult. In Figure 3a number of observations youth: 10 years x 169881 individuals. Number of observations for non-youth: 10 years x 104250 individuals. Number of observations in Figure 3b for male workers heads of household: 10 years x 63496 and for female working heads of household: 10 years x 27395.

The job displacement of an adult may decrease a household's income enough to push other members of the family to find additional sources of income. Additionally, the shock might discourage younger relatives from seeking legitimate employment in the formal sector. In settings like Medellín where individuals, particularly youth, not formally employed have lucrative criminal opportunities,

young relatives of laid-off workers may be susceptible to be drawn into crime. Finally, role-model effects may induce other members of the household to follow them into criminal enterprises. We observe all members of the household in the SISBEN surveys, and restrict our sample to households where family members do not work at the same firm, so as to isolate only indirect spillover effects of layoffs.

Figures 3a and 3b show event-study estimates for relatives. On average, we find meaningful impacts on arrest probabilities of relatives after the layoff (around half the size of the effect on displaced workers themselves). Prime aged relatives (25-35) show immediate impacts in the year of the displacement; while younger relatives (below age 25) show stronger impacts the year after.¹² The delayed arrest response of younger relatives is perhaps consistent with role-model effects possibly contributing to household spillovers.¹³

Results indicate that the gender of the laid-off earner matters, with male displaced workers having a larger effect on the probability of arrests for youth in their household (Figure 3b), despite impacts of losses on earnings being balanced across gender in Figure 2a. In Figure A8 in the Appendix, we split the effects on young relatives by gender to study whether sons and daughters respond more strongly to job displacements of mothers or fathers. Results show that arrest probabilities of all children respond to father job displacements; while only that of daughters responds (but with smaller magnitudes) to mother displacements. The household survey (GEIH 2010) documents that fathers contribute more (64%) to the household income of households with young children than mothers do, which might explain the larger response to father job displacements. The daughter-specific response to mother displacements may be consistent with both role-model effects and a gender-specific discouragement of formal sector employment among young relatives following the parent's job displacement.

5 Possible Mechanism

We next investigate whether workers with tighter economic constraints show stronger responses. We leverage a credit expansion reform to analyze whether increased access to credit mitigates the elasticity

¹²We ignore relatives above the age of 35 as arrest probabilities, especially for property crimes, are negligible.

¹³While we focus here on spillover effects on young relatives between the ages of 14 to 24, in Appendix Figure A7, we show that as we vary the definition of youth for different age-groups, our results remain similar. Since we only observe relatives in the SISBEN surveys, the estimations are conducted for this sub-sample. Our main effects of job-loss on crime are similar in the SISBEN and non-SISBEN samples.

between job loss and crime. Focusing on retail consumption credit as the most proximate mitigator of financial necessity, we match individual-level credit records to the employer-employee-crime data used above.

Pre-layoff credit information allows us to study heterogeneous effects between those who did and did not have access to credit the year before the event; while the longitudinal nature of the data allows us to measure changes in access due to a policy reform in the aftermath of the employment shock, and any pass-through effects on criminality. In Figure 4a we present arrest responses to job loss across baseline access to consumption credit.¹⁴ Increases in arrest rates are most pronounced among those who did not have consumption credit before the event; their probability of arrests increases by 63% in the year of the event and 51% the year after.

5.1 Instrument for Change in Access to Credit

However, given the potential endogeneity between the socioeconomic status of the individual and their likelihood of being arrested, a simple comparison of workers with and without access to credit may provide biased estimates of mitigative effects. Accordingly, we leverage a supply shock derived from the 2009 financial reform in Colombia (Act 1328 of 2009) that, among other changes, newly empowered some retail and finance outlets to operate as commercial credit institutions. In practice, the reform created a supply shock to the number of commercial bank branches. In total, four financing companies and one retail chain became commercial banks between May 2010 and May 2011, with 19 new branches in Medellín by the end of 2011.¹⁵ That is, as a result of the reform, households that happened to live near retail and financial outlets that did not historically have the ability to extend personal lines of consumption credit, now were suddenly closer to commercial bank branches.

Using Google My Maps and information from the chambers of commerce of Medellín (RUES), we locate the coordinates for new branches. SISBEN data record the block where the individual lived before the reform.¹⁶ We compute the Euclidean distance from each new branch in the city to each individual in our SISBEN sample. Our main instrument is the distance to the nearest new branch

¹⁴We show the cumulative effect on arrests in Figure A6.

¹⁵Falabella Bank introduced 5 branches in September 2011, Pichincha Bank introduced 5 branches in July 2011, W Bank introduced 3 in October 2011, Bancoomeva introduced 5 branches in January 2011 and Finandina Bank introduced 1 branch in January 2011.

¹⁶The SISBEN census of the poor represents 54 percent of all individuals in the job-displacement sample, where 94 percent of the sample have a valid address in Medellín.

in Medellín for each individual. We predict access to new consumption credit using this instrument, controlling for *comuna* (neighborhood) fixed effects and a set of covariates.¹⁷ The first-stage estimates for this regression are presented in Table 1.

Table 1: First Stage and Correlations with Distance

	(1) First Stage IV regression	Correlation with Distance	
		(2) Separate	(3) Joint
Minimum Distance	-0.025 (0.007)		
Percentage of Youth		0.0013 (0.0028)	0.0027 (0.0027)
Percentage of Men		-0.0024 (0.0015)	-0.0014 (0.0013)
Formal Income at Home		0.0057 (0.0050)	-0.0026 (0.0036)
Income per Capita at Home		0.0090 (0.0073)	-0.0036 (0.0065)
Sisben Score		0.0008 (0.0005)	0.0009 (0.0006)
Low Education Level		-0.0073 (0.0044)	-0.0020 (0.0023)
Observations	148,386	351,978	351,978
First-stage F stat	10.92	Joint F-stat:	0.88

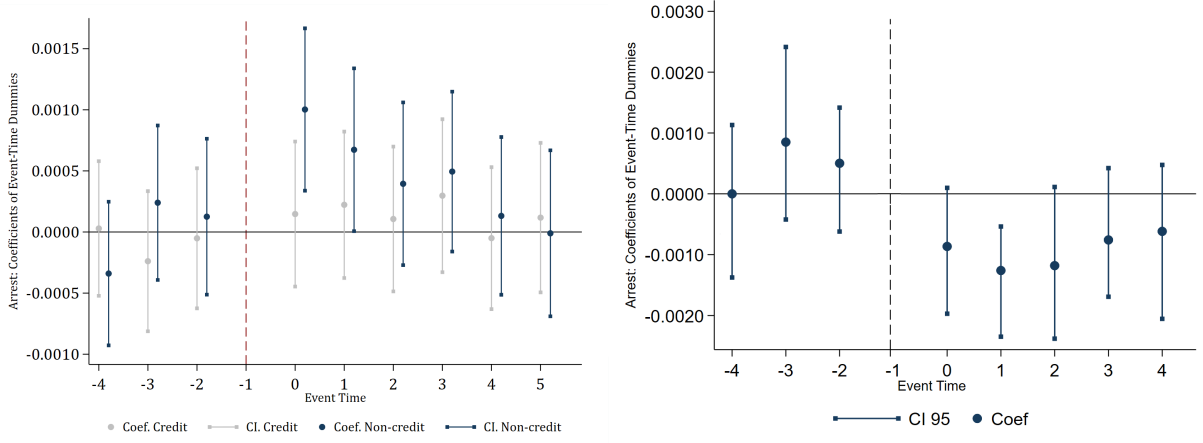
Notes: Regression (1) at individual level for sample of employed workers. Standard errors are clustered at the neighborhood level. The regression includes *comuna* fixed effects and controls for the SISBEN score (poverty index), education, socioeconomic strata, gender and age of individuals. The independent variable is the minimum distance between ones residence and the nearest new branch opened under the credit-expansion program. The average minimum distance to new banks is 2.8 kilometers. The dependent variable is the amount of credit in millions \$COL. Regressions (2) and (3) are at the household level (for all households) using SISBEN II. Regression column (2) shows the results from separate uni-variate regressions (i.e. each row is a coefficient estimated not conditioning on the other variables). Regression column (3) shows the results from a single multi-variate regression, where all the coefficients are estimated jointly. The dependent variable is a dummy of being near a bank, below 2.8km (the mean distance). Regressions include neighborhood fixed effects and standard errors are clustered at neighborhood level.

We then interact new consumption credit predicted by this distance (controlling for neighborhood fixed effects) with the mass layoff event and track differential effects on arrests in the years following the reform-induced creation of new bank branches. For differences in arrest impacts of layoffs across predicted credit levels to capture mitigative impacts of access to credit, it must be that the distance to the new branches (used to predict credit) is not associated with other household characteristics that

¹⁷The set of covariates includes SISBEN score (poverty index), education, socioeconomic strata, gender and age.

may also attenuate the employment-crime relationship coincidentally at the time of new bank branch openings. While this is not a fully testable assumption, we check in Table 1 that distance to the new bank is not correlated with household observables, including income and SISBEN (poverty) Score.

Figure 4: Heterogeneous effects by access to credit



(a) OLS: Event study estimates by consumption credit

(b) IV: Total consumption credit at new branches

Notes: First stage instrumental variables estimation where we instrument the access to consumption credit with distance to expanded bank branches is shown in col (1) of Table 1. Figure 4a shows the effect of a firm's mass layoff on arrests, from four years before the displacement year to five years after. The regression include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. Number of observations for credit : 10 years x 228368 individuals. Number of observations for non-credit: 10 years x 228728 individuals. Figure 4b shows the heterogeneous effect of mass layoff event by access to credit on arrest. The blue vertical dashed line documents the year of the credit expansion. The regression includes *comuna* and year fixed effects. Confidence Intervals at 95% are calculated with bootstrap 1000 repetitions following Ashraf and Galor (2013) procedure. Here we only use the subsample of workers in SISBEN II, so as to use the geolocations of residences. Number of observations: 10 years x 148386.

We estimate a model in which we fully interact the predicted amount of consumption credit with the job displacement variable as follows:

$$\begin{aligned} Arrested_{i,t} = & \alpha + \gamma_t + X_{i,t}\beta + \sum_{-4 \leq k \leq 5, k \neq -1} \beta_k Displaced_{i,t-k} \times \widehat{Credit}_i \\ & \sum_{-4 \leq k \leq 5, k \neq -1} Displaced_{i,t-k} \delta_k + \eta \widehat{Credit}_i + \theta_c + \varepsilon_{i,t}, \end{aligned} \quad (2)$$

where \widehat{Credit}_i is the predicted consumption credit from the first stage on distance to new bank branches. In this specification, we also control for the dynamic effect of job displacement on arrests, along with *comuna* fixed effects θ_c , year fixed effects γ_t , and covariates \mathbf{X} . For standard errors, to account for the presence of a generated regressor, we employ a two-step bootstrap.

Figure 4b plots the estimates for the β_k coefficients of equation 2. We find that those who benefited

from the credit supply-shock exhibit a reduction in the job loss-crime elasticity in years that follow the banking-expansion.¹⁸ The magnitudes in Figure 4b confirm the pattern suggested in Figure 4a, that access to consumption credit might entirely mitigate the crime response to job displacement. As in previous results, no detectable differences appear in the years prior. Note that coefficients plotted in Figure 4b measure the differences between the impacts of job-loss on arrests for high and low predicted credit individuals each year. As such, the significant impacts in 2011 and 2012 indicate changes from any immediate differences in arrests in the year of displacement (2010) and the insignificant prior coefficients confirm that, in fact, no differences in arrests existed prior to displacement nor immediately after displacement, prior to new bank branch openings.

6 Conclusion

We document that mass-layoffs produce an immediate and persistent earnings loss and a sharp increase in the likelihood of being arrested. Some recent studies document similar patterns in high-income countries with stronger judicial and police institutions. A comparison of the implied elasticities between employment or earnings and entry into crime emphasizes the larger magnitude of the effects we find, both immediately after the job displacement and over the following five years.

Our estimates imply an employment-crime elasticity of -2.12 in the year of job loss and -1.76 over the 5 years following the job loss. As calculated in Table A4 in the Appendix, [Bennett and Ouazad \(2020\)](#) estimate a smaller and less persistent impact on criminality in response to a larger change in employment in Denmark. In the US, [Rose \(2020\)](#) estimates a similarly sized and persistent effect on criminality to ours, but in response to a larger effect on employment. Similarly, our estimates imply an earnings-crime elasticity of -2.12 in the short-run and -2.02 in the longer-run. The analogous calculated elasticities in [Bennett and Ouazad \(2020\)](#), presented in Table A5 in the Appendix, once again indicate smaller and less persistent effects; while those in [Rose \(2020\)](#) indicate smaller but nearly as persistent effects as ours. On the other hand, the estimates in [Rege et al. \(2019\)](#) from Norway imply smaller but more persistent earnings-crime elasticities, with a seemingly delayed onset of both earnings and crime effects.

We also build on these previous studies by estimating spillover impacts on other members of the

¹⁸The corresponding reduced form coefficients are plotted in Figure A9.

household. We find that when fathers are laid off, criminality rises for children in the family. Together, these estimates imply that the aggregate household-level crime elasticity is even larger.

The literature on the impacts of job losses has documented physical, emotional, and mental health consequences in addition to the proximate economic losses. Accordingly, it is unclear whether the demonstrated criminal response to employment shocks is realized, at least in part, through economic channels such as acute financial necessity. We note that, although the effects on earnings are observed in both male and female samples, the increases in arrest rates are most pronounced among males (most likely to have criminal employment opportunities). Also, additional results presented in the Appendix show the effects are concentrated in property crimes (most likely to reflect economic need). Finally, evidence that greater access to consumption credit can attenuate arrest responses to job displacements further points to economic incentives contributing to the employment-crime relationship we document.

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Online Appendix

Table A1: Sample restrictions and representativeness

		Days of work restriction		Firm restrictions		Employee restrictions	
	Full database	greater than 1	greater than 20	1st Restriction	2nd Restriction	3rd Restriction	4th Restriction
Individuals	982,676	964,523	916,253	831,014	634,387	601,764	457,096
% of Individuals in database	100%	98%	93%	85%	65%	61%	47%
<i>Worker characteristics</i>							
Percentage of Men	58%	58%	58%	58%	60%	60%	58%
Percentage of Youth	15%	15%	14%	15%	17%	16%	15%
Average wage	869567.9	860538	875782.9	829058.7	891044.1	905345.1	909997.0

Notes: Individuals in the “Full database” are formal workers, 20 to 60 years old. Moving left through right we add additional restrictions one-by-one. In the second row, we document what fraction of the sample remains as we add each additional restriction. In the bottom three rows we measure how sample characteristics change as we add each additional restriction. The “Days of work restriction” restricts the sample to workers who have worked for the firm for at least a certain number of days – we use more than 20 days as our cutoff. The “Firm restrictions” are sample restrictions based on firm characteristics – the ‘1st restriction’ is working in a private firm. The ‘2nd restriction’ is working in a firm with more than 10 employees. The “Employee restrictions” are sample restrictions based on employee characteristics. The ‘3rd restriction’ is that the employee has at least 12 months of uninterrupted tenure. The ‘4th restriction’ is that the employee was working at only one firm for all those 12 months. The ‘Average wage’ is yearly earnings in 2009 \$COL.

Table A2: Summary statistics for estimation sample

	Mean	Std	Number of workers
Male	0.5831	0.4931	457096
Age in 2009	36.5	10	457096
Average earnings in 2009	0.91	0.94	457096
Average monthly days of work in 2009	29.28	1.69	457096
Firm size	1763	3794	457096
Probability of arrest 2006-2015	0.0019	0.0433	457096
Access to Consumer Credit 2009	0.4996	0.5	457096
Probability in Sisben II (high poverty)	0.5359	0.4987	457096
Probability of arrest 2006-2015 by:			
Age:			
20-30	0.0030	0.0547	138,166
30-40	0.0019	0.0438	144,148
40-50	0.0010	0.0323	117,698
50-60	0.0008	0.0277	55,156
Sex:			
Male	0.0030	0.0168	266,521
Female	0.0003	0.0548	190,575
Poverty Status:			
Poor	0.0020	0.0452	244,954
Non-Poor	0.0017	0.0410	212,142
Consumer Credit 2009:			
Have Credit	0.0012	0.0351	228,368
Non have Credit	0.0025	0.0501	228,728
Amount of New Credit 2006-2015 (million \$COL)			
Total Credit	7.8164	19.4014	236,853
Consumer Credit	6.1740	12.1569	224,432
Credit at Banks	8.4523	21.3845	160,779

Notes: Sample of workers with at least one formal sector job spell. Employees in sample are people that work in a private firm with at least 11 employees, with a tenure of 12 months in the same firm (in 2009) and are full-time workers (20 or more days worked in the month), with only one job in 2009. Average earnings and credit in millions of nominal \$COL.

Table A3: Event study estimates on arrests

	Arrests		
	(1) All	(2) Men	(3) Women
$t = -4$	-0.000201 (0.000208)	-0.000239 (0.000329)	-0.000063 (0.000122)
$t = -3$	0.000018 (0.000224)	0.000041 (0.000352)	-0.000026 (0.000139)
$t = -2$	0.000083 (0.000225)	0.000105 (0.000354)	0.000030 (0.000137)
$t = 0$	0.000708 (0.000234)	0.00107 (0.000369)	0.000051 (0.000139)
$t = 1$	0.000549 (0.000235)	0.000725 (0.00037)	0.000177 (0.00014)
$t = 2$	0.000358 (0.000233)	0.000542 (0.000369)	-0.000013 (0.000136)
$t = 3$	0.000412 (0.000234)	0.000671 (0.00037)	-0.000082 (0.000141)
$t = 4$	0.000097 (0.000227)	0.000210 (0.000359)	-0.000125 (0.000134)
$t = 5$	0.000125 (0.000238)	0.000172 (0.000377)	-0.000117 (0.000139)
Observations	4570960	2665210	1905750
Dep. Var. Mean	0.001880	0.003010	0.000280
Joint significance (2006-2008) <i>p-value</i>	$F(3, 457094) = 0.68$ 0.5613	$F(3, 266519) = 0.41$ 0.7428	$F(3, 190574) = 0.17$ 0.9141
Joint significance (2010-2015) <i>p-value</i>	$F(6, 457095) = 2.30$ 0.0317	$F(6, 266520) = 1.95$ 0.0696	$F(6, 190574) = 1.04$ 0.3986

Notes: Table A3 lists δ_k from equation (1). Standard errors are clustered at the individual and firm level. The sample includes drug, property, violent, and other crimes. Event time is measured in years. Arrest outcome is binary indicator: 1 if the person was arrested at any point in the year, 0 otherwise.

Comparing Employment-crime and Earnings-crime Elasticities:

We compare the size and timing of our effects to other established work from developed economics: notably from Denmark (Bennett and Ouazad, 2020), the US (Rose, 2020) and Norway (Rege et al., 2019). We define the employment-crime elasticity to be the percentage change in arrests for a 1% increase in employment. To estimate the elasticities, we use the following formula:

$$\epsilon = -\frac{\gamma_c}{\theta_c} \left(\frac{\gamma_f}{\theta_f} \right)^{-1},$$

where γ_c is the estimated coefficient of mass-layoff on crime, θ_c is the average crime rate before mass-layoff event, γ_f is the coefficient of mass-layoff on formal employment (or earnings), and θ_f is the average earnings or employment rate before the mass-layoff event.

We report the elasticities of crime and employment in Table A4. For Medellín, we take the coefficients estimated in Table A3 and the formal-employment coefficients from Figure A5. We estimate the elasticities for Bennett and Ouazad (2020) using the effect on crime and the effect on firm size reported in their Table 5 and Table 3, respectively. From their main text, we infer the average size of the firm before the shock. We estimate the elasticities for Rose (2020) using the effect on crime and employment reported in their Table 4, which reports the average crime rate and employment rate before the mass-layoff.

Table A4: Employment-Crime Elasticities

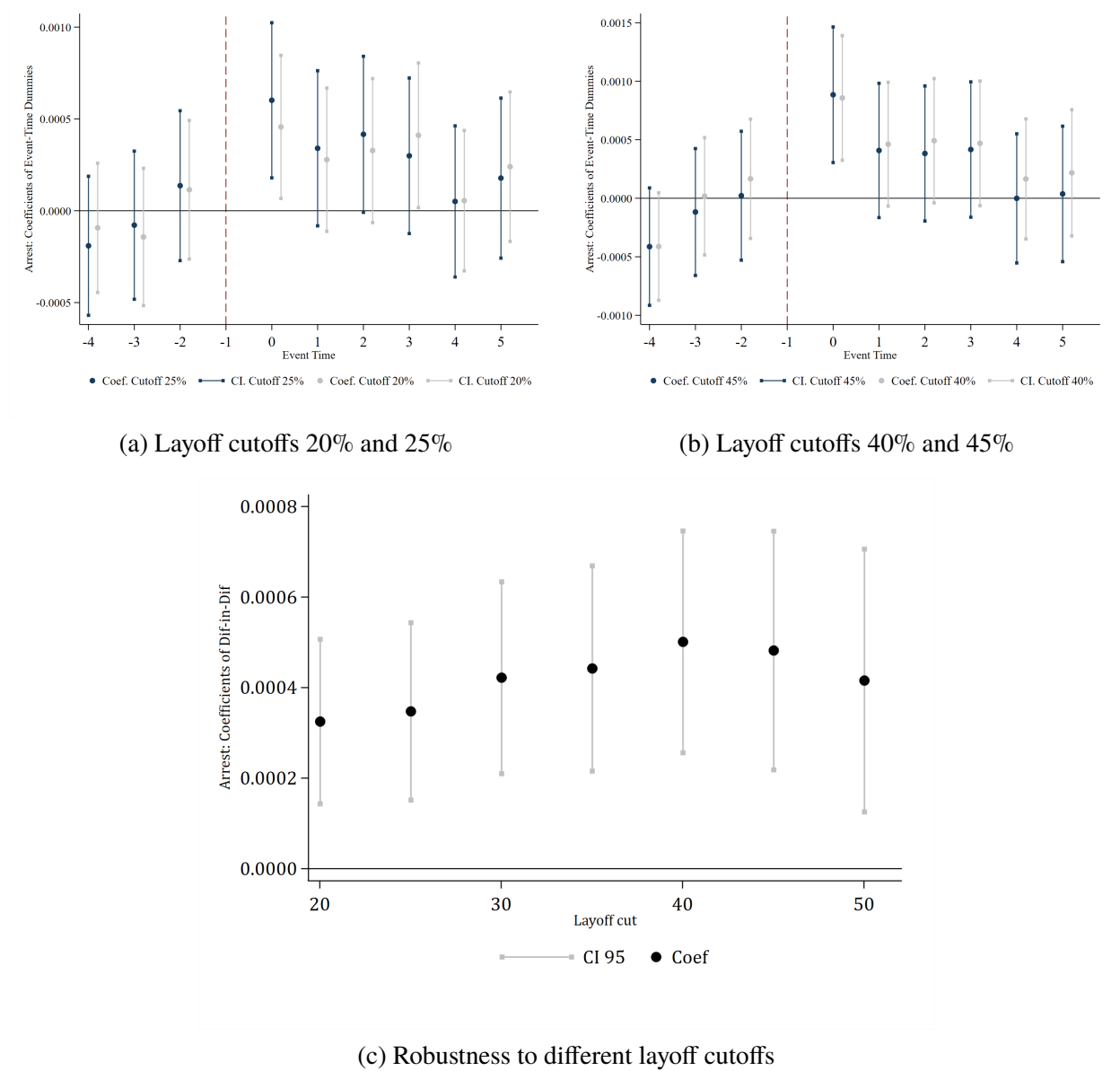
	Medellín	Denmark (Bennett & Ouazad, 2020)	USA (Rose, 2020)
First period post job-loss		Short-term	
Coefficient for Arrests	0.07	0.57	2.31
Coefficient for Formal Employment	22.00	67.8	52.6
Mean Arrests	0.15	1.80	4.6
Mean Formal Employment	100	171.4	100
% Change crime	47%	32%	50%
% Change Formal Employment	22%	40%	53%
Elasticity = % Change in arrests/ % change in emp	2.12	0.80	0.95
Five- periods cumulative effect		Medium-term	
Coefficient for Arrests	0.17	0.78	5
Coefficient for Formal Employment	64.30	299.5	134.8
Mean Arrests	0.15	1.8	4.6
Mean Formal Employment	100	171.4	100
% Change crime	113%	43%	109%
% Change Formal Employment	64%	175%	135%
Elasticity = % Change in arrests/ % change in emp	1.76	0.25	0.81

We report earnings-crime elasticities in Table A5. For Medellín, we take the coefficients estimated in Table A3 and the wage coefficients of Figure A4b. We estimate the elasticities for Bennett and Ouazad (2020) using the effect on crime and the effect on earnings reported in their Table 5 and appendix Table F, respectively. From their main text, we infer average earnings prior to the job loss. We estimate elasticities for Rose (2020), using the effect on crime and earnings reported in their Table 4, which reports the average crime rate and earnings before the mass-layoff. From their main text, we infer average earnings prior to the displacement. We estimate the elasticities for Rege et al. (2019) using the effect on crime and earnings reported in their Table 4. From their main text, we infer average earnings prior to the job loss.

Table A5: Earnings-Crime Elasticities

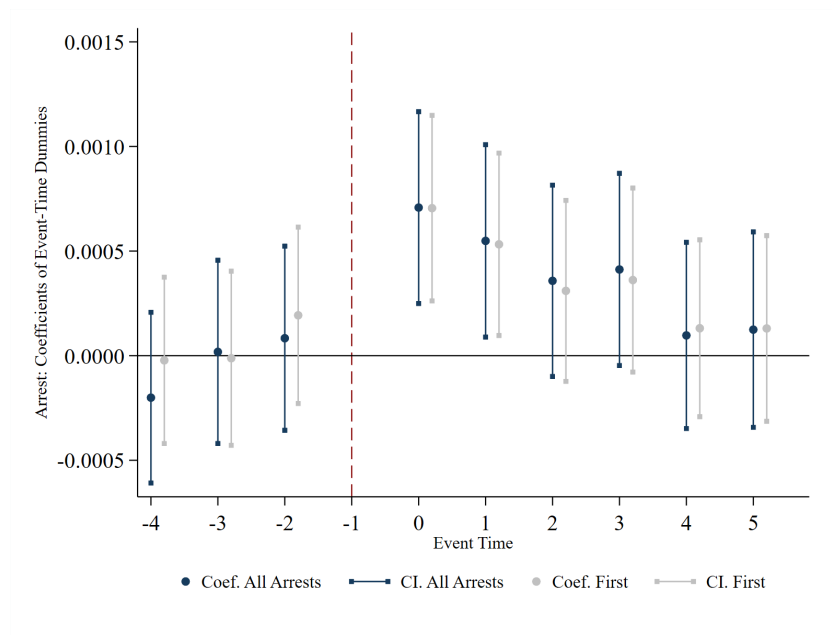
	Medellín	Denmark (Bennett & Ouazad, 2019)	USA (Rose, 2020)	Norway (Rege et al, 2019)
First period post job-loss	Short-term			
Coefficient for Arrests	0.07	0.57	2.31	0.35
Coefficient for Earnings	0.20	50.2	5.00	190.51
Mean Arrests	0.15	1.80	4.60	1.75
Mean Earnings	0.91	100.0	9.26	1270.1
% Change crime	47%	32%	50%	20%
% Change Earnings	22%	50%	54%	15%
Elasticity = % Change arrests / % change earnings	2.12	0.63	0.93	1.33
Five- periods cumulative effect	Medium-term			
Coefficient for Arrests	0.17	0.78	4.80	0.89
Coefficient for Earnings	0.51	256.0	13.20	357.33
Mean Arrests	0.15	1.8	4.60	1.75
Mean Earnings	0.91	100	9.26	1270.1
% Change crime	113%	43%	104%	51%
% Change earnings	56%	256%	143%	28%
Elasticity = % Change arrests / % change earnings	2.02	0.17	0.73	1.81

Figure A1: Robustness to different layoff cutoffs



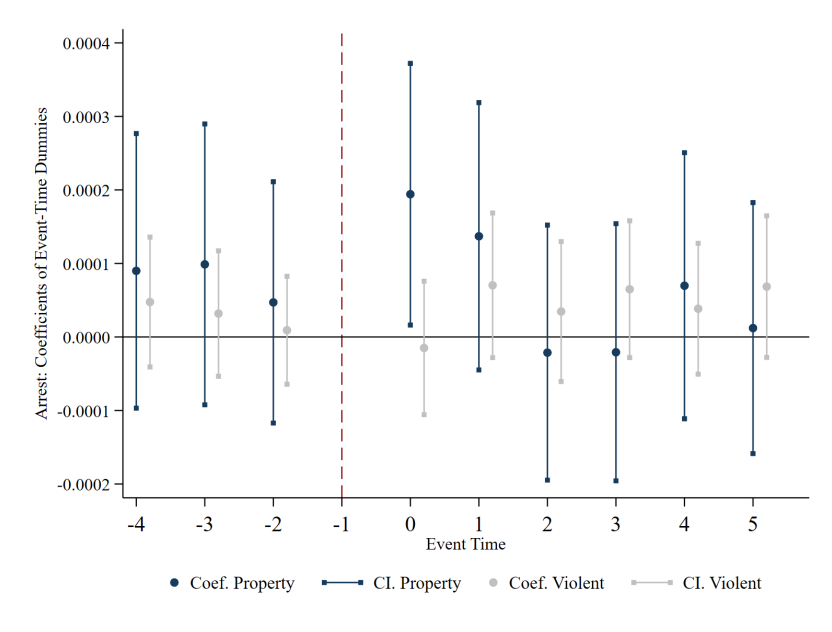
Notes: Figure A1 shows evidence of how our main effects change when we use alternative cutoffs to define mass-layoff events. In the top panel, we estimate event-study coefficients, comparing workers before and after the mass layoff, and workers in firms with and without layoffs. In the bottom panel, we estimate the difference-in-differences coefficient, comparing before-after the mass layoff, and workers in firms with and without layoffs. The horizontal axis varies the layoff cutoff value from 20% of job separations at a firm to 50% of job separations at a firm. For our main analysis we use the 30% layoff cutoff.

Figure A2: Event study estimates: All arrests vs First arrests



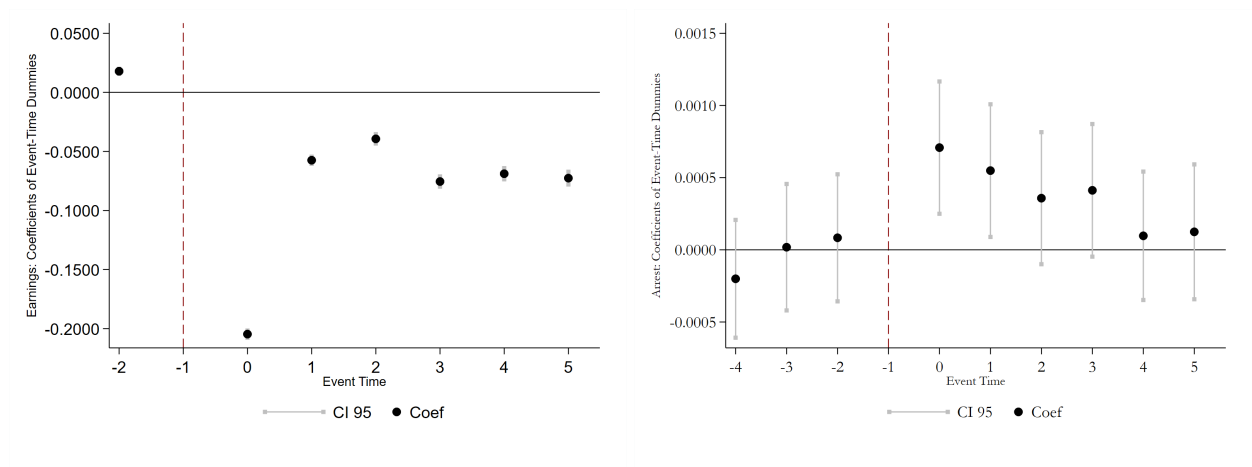
Notes: Figure A2 shows the effect of mass-layoff events on the probability of being arrest. Number of observations: 10 years x 457096 individuals. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. Layoff event is defined as those firms where 30-90% of their employees lost their jobs.

Figure A3: Event study estimates by type of crime



Notes: Figure A3 shows the effect of mass layoff events on the likelihood of being arrested for property and violent crimes. Number of observations: 10 years x 457096 individuals. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. A layoff event is defined as those firms where 30-90% of their employees lost their jobs.

Figure A4: Effect on Earnings and Arrest (Full Sample)

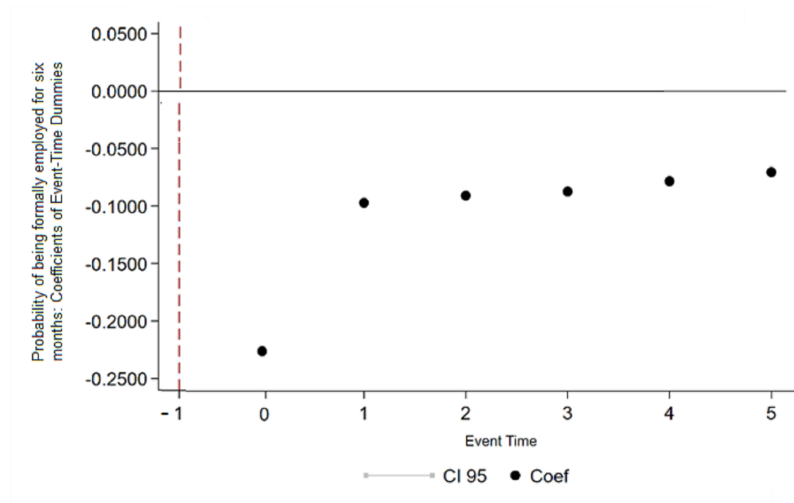


(a) Event study estimates on earnings

(b) Event study estimates on arrest (full sample)

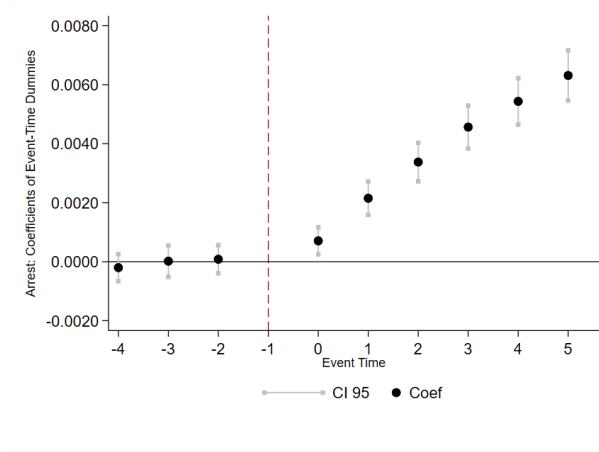
Notes: Figure A4a shows the effect of mass-layoffs, from two years before event year to five years after the event, on average annual earnings. We compute annual formal sector earnings by summing the inflation-adjusted monthly formal sector earnings using 2008 as a base year. Figure A4b shows the effect of a firm's mass layoff event, from four years before the event to five years after the event, on arrests. Number of observations: 10 years x 457096 individuals.

Figure A5: Event study estimates on formal employment

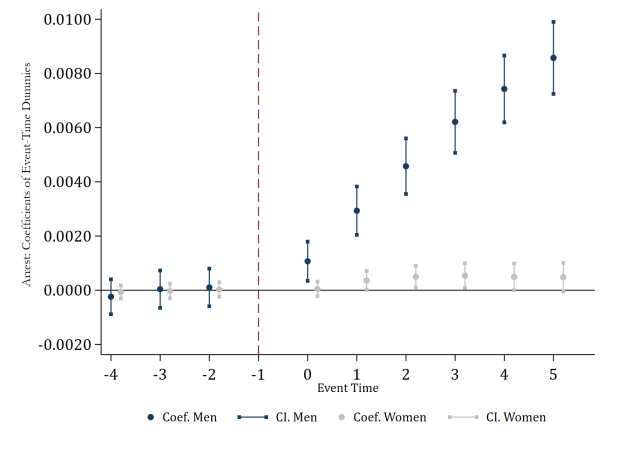


Notes: Figure A5 shows the effect of mass-layoffs on the probability of being formally employed for at least six months within a year. The sample is restricted (by construction) to individuals who were employed before the shock. Number of observations: 6 years x 457096 individuals. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. Layoff event is defined as those firms where 30-90% of their employees lost their jobs.

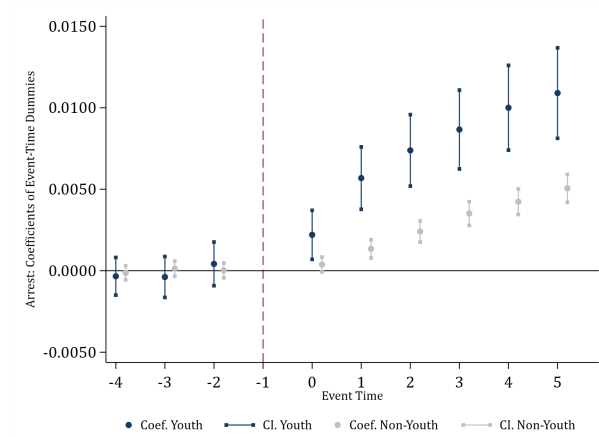
Figure A6: Effects of firm-level mass layoffs on cumulative arrest



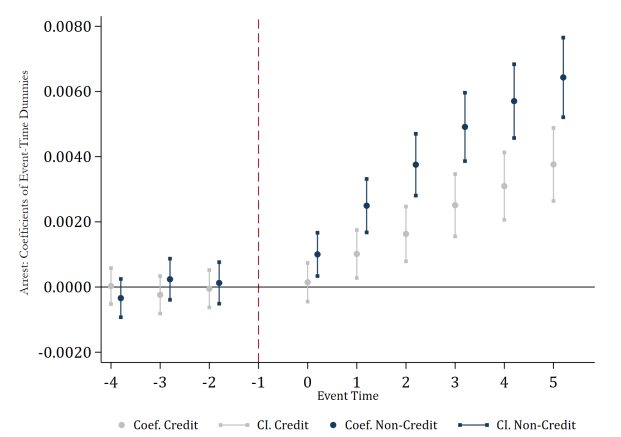
(a) Event study estimates on cumulative arrest



(b) Event study estimates on arrests by gender



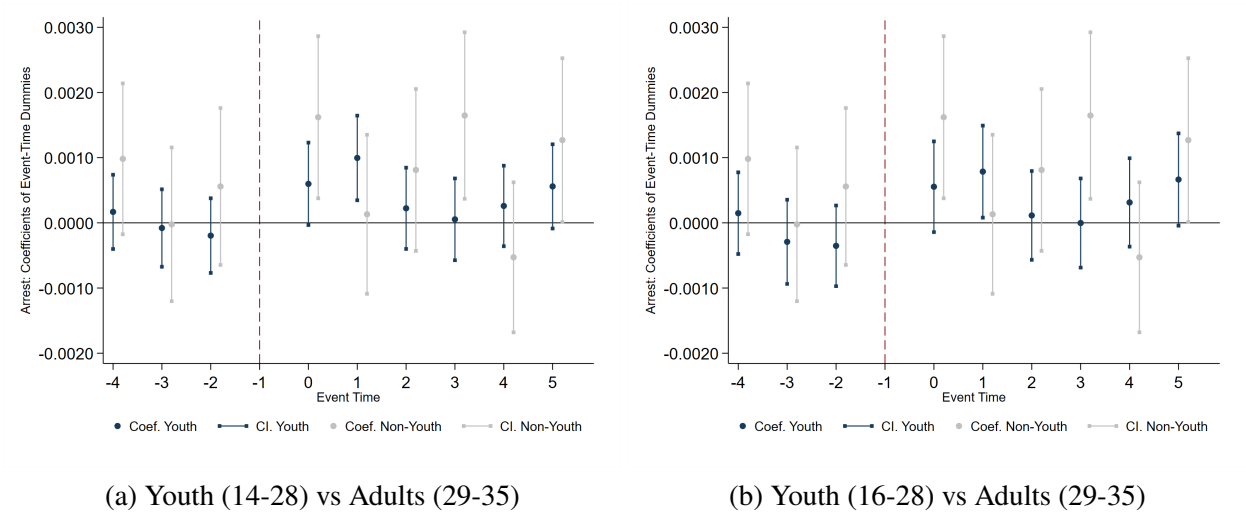
(c) Event study estimates on arrests by age



(d) Event study estimates on arrests by credit

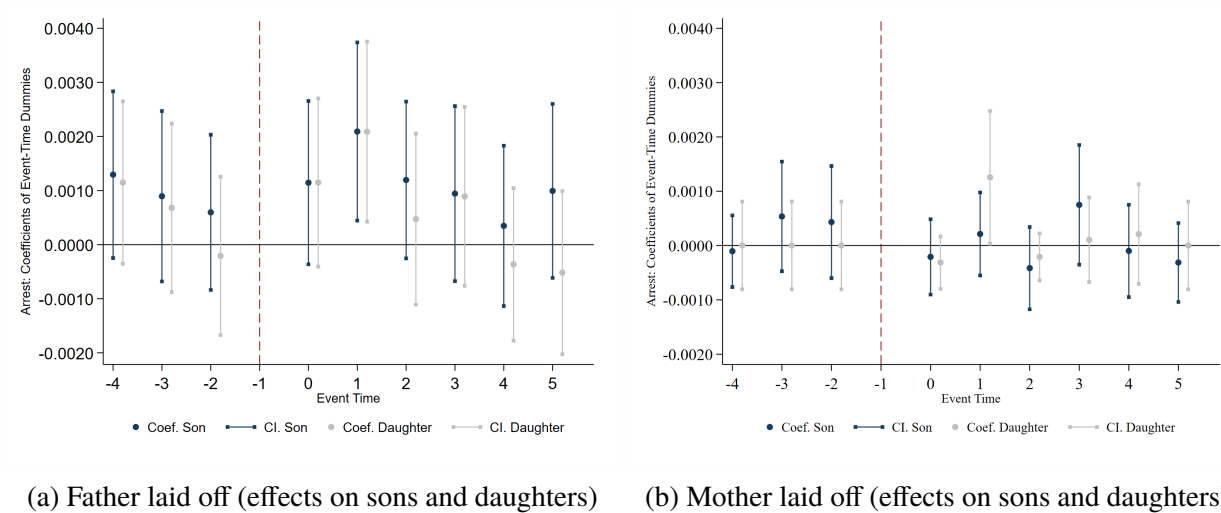
Notes: Figures show the effect of mass layoff events on cumulative arrests after the layoff event. We re-define the post-period of arrests in the post-layoff period to be an indicator= 1 if the individual was ever arrested between the time of the layoff and the year. Figure A6a shows the effect of firms mass layoff event on the cumulative probability of being arrest. Number of observations: 10 years x 457096 individuals. Figures A6b to A6d show the heterogeneous effects of mass layoff events on arrest by gender, poverty status and consumption credit. Number of observations for women: 10 years x 190575 individuals. Number of observations for poor: 10 years x 244954 individuals. Number of observations for non-poor: 10 years x 212142 individuals. Number of observations for youth: 10 years x 69902 individuals. Number of observations for non-youth: 10 years x 387194 individuals. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. A layoff event is defined as those firms where 30-90% of their employees separated for at least six months.

Figure A7: Robustness to age-groups: Event study estimates on other family members



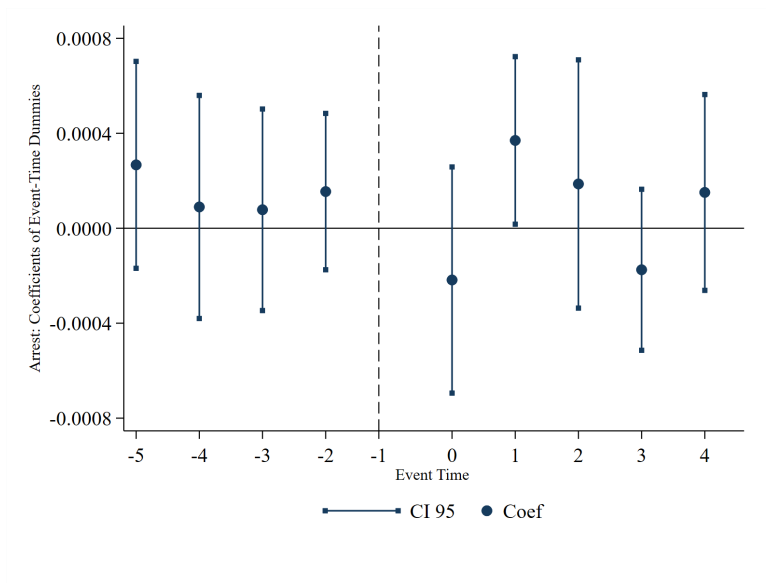
Notes: Figures show the effect of mass layoff events on arrests. Figures A7a and A7b shows the effects on youth and non-youth arrest of relatives. In Figure A7a, number of observations for youth (14-28): 10 years x 214481 individuals, number of observations for non-youth (29-35): 10 years x 59650 individuals. In Figure A7b, number of observations for youth (16-28): 10 years x 181901 individuals, Number of observations for non-youth (29-35): 10 years x 59650 individuals. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. A layoff event is defined as those firms where 30-90% of their employees separated for at least six months. Arrest is 1 if the employee was arrested at least once in a year.

Figure A8: Event study estimates on children by gender of laid-off adult



Notes: Figure A4a shows the effect of mass layoff events on sons and daughters in the household. Figure A8a show the effect on sons and daughters for male laid-off employees. Number of observations for sons: 10 years x 30049 individuals. Number of observations for daughters: 10 years x 28828 individuals. Figure A8b show the effect on sons and daughters for female laid-off employees. Number of observations for sons: 10 years x 14308 individuals. Number of observations for daughters: 10 years x 14456 individuals. The regressions include individual and year fixed effects. Standard errors are clustered at the employee-firm level. 95% confidence intervals are presented in the figure. A layoff event is defined as those firms where 30-90% of their employees lost their jobs.

Figure A9: Reduced Form Effect of distance to bank following a layoff



Notes: Figure A9 shows the effect of minimum distance between ones residence and the nearest new branch opened under the credit-expansion program for employees displaced, on the probability of being arrest. Number of observations: 10 years x 148,386 individuals. Standard errors are clustered at the neighborhood level. The regression includes *comuna* fixed effects and controls for the SISBEN score (poverty index), education, socioeconomic strata, gender and age. The average minimum distance to a new banks is 2.8 kilometers. 95% confidence intervals are presented in the figure.