

Job Loss, Credit and Crime in Colombia

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Abstract

We investigate the effects of job displacement, as a result of mass-layoffs, on criminal arrests using a novel matched employer-employee-crime dataset from Medellín, Colombia. Job displacement leads to immediate earnings losses, and higher probability of arrest for both the displaced worker and youth in the family. Leveraging variation in opportunities for legitimate reemployment and a policy reform that expanded access to consumption credit, we investigate the underlying economic incentives. Workers in booming sectors with more opportunities for legitimate reemployment exhibit smaller increases in arrests after job losses. An exogenous increase in consumption credit also lowers criminal response to job loss.

Keywords: Job displacements, crime, Medellín

JEL Codes: K42, J63, J65

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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). Each of these effects might lead to increases in criminality, but with vastly different implications for how to combat or insulate against these criminal responses to employment shocks. While canonical models of criminal activity emphasize economic incentives (Becker, 1968; Ehrlich, 1973), empirical studies of criminal activity document the importance of both economic incentives (Bignon et al., 2016; Blattman and Annan, 2015; Watson et al., 2019) and a myriad of other behavioral and psychological drivers (Anderson et al., 2015; Blattman et al., 2017; Bondurant et al., 2018; Carpenter, 2005; Lindo et al., 2018). This begs the question then whether any criminal responses to job losses are driven primarily by the economic consequences of these employment shocks and, accordingly, if muting these economic consequences significantly mutes the criminal response.

Many low and middle income countries, particularly across Latin America, suffer from a combination of high employment volatility, poor unemployment safety-nets and rampant crime (Dell et al., 2018; Dix-Carneiro et al., 2018). These settings, therefore, provide ideal conditions for investigating the criminal responses to job losses when the economic consequences are unmitigated. 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 document a crime response to job-loss that is both large and far-reaching in that criminal responses spill over to youth in the household as well.¹

We study firm-specific shocks to account for worker heterogeneity in unobservable characteristics. We focus on mass-layoff events where large groups of workers lose their jobs. As such, laid-off workers are less likely to have characteristics systematically associated with the propensity to commit crimes. We estimate the impact of displacement on the probability of being captured after the event. In the spirit of an event-study analysis, we show that the displacement event is not associated with the

¹Published work estimating the impacts of job loss on crime focus on high-income, low-crime environments with strong unemployment safety-nets (Bennett and Ouazad, 2018; Rege et al., 2019). The impacts we estimate are larger, especially as we additionally show spillovers on other members of the household.

likelihood of being arrested before such events, confirming that dynamic selectivity into displacement is unlikely. We find that, after job losses, workers suffer a significant earnings loss that lasts at least five years. We show a corresponding spike in arrests in the year of job separation and the year after.

We then leverage varying market conditions for job replacement and consumption credit to investigate the degree to which the economic consequences of job losses are driving the criminal responses. We show that impacts on arrests are weaker for those with better opportunities for legitimate reemployment and that greater access to consumption credit in the year following the job loss dampens the effects on arrests. That is, we first use additional data on industry-level opportunities for legitimate reemployment to document patterns consistent with predictions of occupational sorting models of crime in that workers in booming sectors, with more legitimate employment alternatives, exhibit lower increases in arrests after job destructions. Second, using individual-level geographic variation in exposure to a credit-policy reform, we show that access to consumption credit weakens the relationship between job loss and criminality, consistent with criminal responses being driven by consumption necessity. By obtaining unique administrative data on the credit histories of individuals, and leveraging a credit expansion program, we isolate the causal impact of access to an important consumption smoothing mechanism on the elasticity between job loss and crime.

Our findings contribute primarily to the literature on the impacts of job destructions. Previous work has focused on earnings (Couch and Placzek, 2010; Jacobson et al., 1993) and 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). We build on studies of criminality (Bennett and Ouazad, 2018; Rose, 2019) by providing evidence of the specific roles that alternative employment opportunities and the ability to meet consumption needs play in determining criminality responses.

We also contribute to the study of the economic motives for criminal employment (Becker, 1968; Ehrlich, 1973). Recent studies have presented empirical evidence of individual level sorting into crime based on labor market conditions, policies, and incentives (Fu and Wolpin, 2017; Khanna et al., 2019). This paper adds similar evidence to the related literature on criminal responses to employment shocks by exploiting individual level variation in job displacement, opportunities for legitimate job replacement, and access to credit for meeting stop-gap consumption needs to confirm economic incentives as primary mechanisms underlying criminal responses to employment shocks.

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, and resulting financial strain, can have delayed ripple effects on young relatives' criminality. Ignoring such spillovers will lead to gross underestimates on the long-term consequences of job loss on crime.

The rest of the paper is organized as follows. Section 2 provides some background. Section 3 discusses our data, section 4 our empirical strategy, and section 5 the results. Section 6 discusses the role of credit, and section 7 concludes.

2 Background

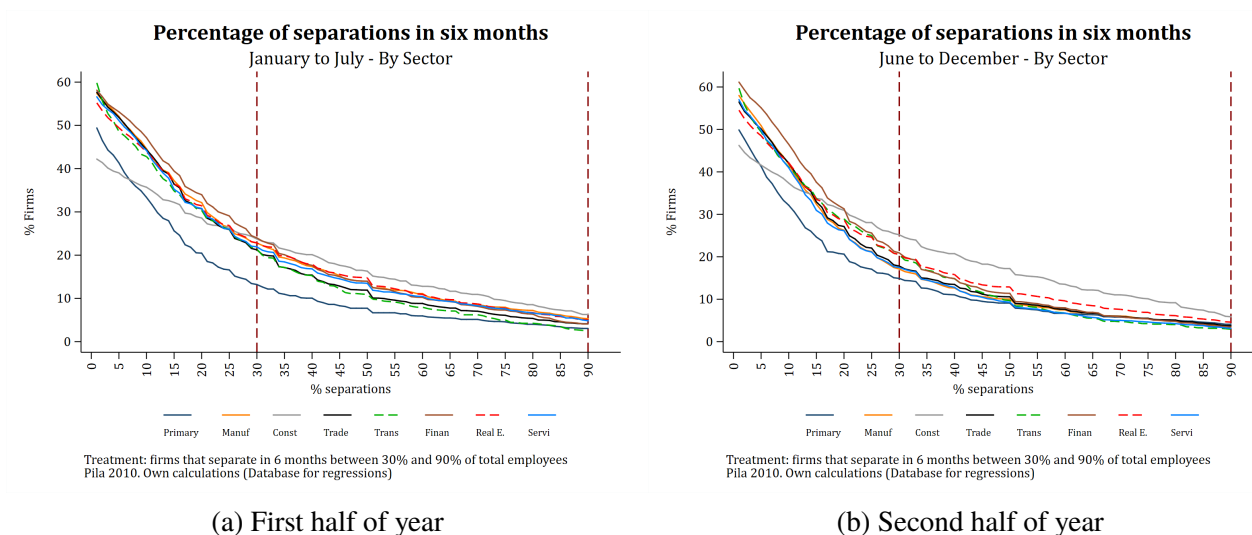
Located in the north-western region of Colombia, Medellín is the second largest city after the capital, Bogotá. It has strong industrial and financial sectors with approximately 2.3 million people or 5.5% of the Colombian population. The urban zone consists of 249 neighborhoods, divided into 21 (*comunas*), 5 of which are semi-rural townships (*corregimientos*).

Mass layoffs in Medellín vary across sectors and seasons. Figures 1a and 1b document patterns in layoffs, where we divide the year into two halves, and sectors into 8 large groups. On the vertical axes we plot the density of firms and on the horizontal axis, the fraction of workers separated. The figures show that separations occur both over the course of the year and across sectors. There is less job churning in the primary (agricultural) sector and a higher rate of job loss in construction.

In keeping with the literature, we define a mass layoff event as between 30% and 90% of workers being separated from the firm within a year.² On average, this represents about 20% of firms each year. While there are differences between the first and second half of the year, such seasonality is relatively minor in magnitude.

²Our results are robust to alternative cutoffs, as we show in Figure A1. We cap it at 90%, as a 100% separation rate may simply indicate a change in ownership without mass layoffs.

Figure 1: Distribution of layoffs



Notes: Figure 1a shows the distribution of layoffs across industries for the first half of the year. Figure 1b shows the distribution of layoffs across industries for the second half of the year. We use the employer-employee matched PILA data and define a separation to be if the worker is no longer employed at the firm in any future date. We then plot the density of firms that have different fractions of separations by time and sector.

2.1 Crime in Medellín

Although Colombian violence has traditionally been high, the emergence of drug cartels in the late 1970s and early 1980s, fueled the emergence of organized crime to support illegal businesses, and guerrilla or paramilitary groups often involved with the drug trade. From the mid 1980s to early 1990s, homicide rates rose rapidly driven by the boom of cartels, paramilitaries, and local gangs. Medellín used to be one of the most violent cities in the world (CCSPJP, 2009), placing our analysis among a handful that study such motivations to join crime in high-crime environments. The high homicide rates are a result of fights among urban militias, local gangs, drug cartels, criminal bands, and paramilitaries based in surrounding areas.³ Demobilized militias continue to be involved in crimes like extortion and trafficking (Rozema, 2018).

The arrest rate is predominantly male. Over the entire sample period (2005-13), 12% of all males (across all age groups) were at some point arrested, while the arrest rate for females was only 1%.⁴ Youth, between 14 and 26 years of age, are far more likely to be involved as victims or assailants than other age groups. Youth are more likely to be engaged in drug trafficking and consumption, whereas

³Operacion Orion in 2002, followed by the demobilization of paramilitary forces between 2003 and 2005, led to a sharp decline in homicides from more than 180 homicides per 100000 inhabitants in 2002, to less than 35 in 2007, as the military clamped down on urban militias (Medina and Tamayo, 2011).

⁴Authors calculations based on data from the Judicial Research Unit of the Metropolitan Police of Medellín (SIJIN).

slightly older individuals are involved in violent crimes (homicides, extortion, and kidnapping), and the oldest still are involved in property crime.

In ongoing research, [Blattman et al. \(2018\)](#) document Medellín's criminal world as hundreds of well-defined street gangs (*combos*) which control local territories and are organized into hierarchical structures, with protection by the *razones* at the top of the hierarchy. Anthropological studies and in-person interviews show that economic incentives (such as the focus of our study) drive younger 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.⁵ Knowing this, paramilitaries and gangs actively recruit men who are "idle" and without a good job.⁶

Often, however, remunerations for gang-members are higher than jobs for those with similar levels of education ([Doyle, 2016](#)). New recruits are employed to run guns (*carritos*), before transitioning to extortion and trafficking. [Blattman et al. \(2018\)](#) estimate that foot soldiers of the *combos* receive well above national minimum wage whereas *combo* leaders earnings put them in the top 10% of income earners in the city.

For young men, 21.5% were arrested over the period of study – 11.1% for drug crimes, 5.6% for property crime, and 4.8% for violent crimes. 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 a gang, 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.

3 Data

We combine four different sources of administrative data based on individual identification numbers and date of birth. The first source is the Integrated Information System for Social Protection (SISPRO),

⁵See [Baird \(2011\)](#) *interview with Gato, p264* and *interview with Armando, p197*.

⁶An 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](#))).

which contains information from the Integrated Registry of Contributions to the social security system of all formal workers in Colombia (PILA). The PILA includes the monthly salaries of all formal workers (i.e., those who pay contributions to health and pension), and the days the individuals have worked per month since July 2008. Using the PILA, we build an employer-employee panel that allows us to follow both individuals and firms over time. The PILA has detailed information on payroll, earnings, days worked, firm and worker identifiers, and some demographic information of employees.

The second administrative data, 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. The data contains type of crime committed, the date and place of arrest, and demographic information of the arrested individual such as age and marital status. These data allow us to build crime outcomes such as whether individuals were arrested for having committed any crime over a specific time period, and to identify the type of crime (e.g., homicide, robbery, burglary or drug trafficking).

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.⁷ We contend that first 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).

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, with level one representing the most economically disadvantaged households and level six the least disadvantaged ones (Bottia et al., 2012). In particular, these data allow us to identify family members and other youth in the household.

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 data that contains the census of all credit with the formal financial sector for more than 250 million credit transactions, including credit cards, car loans, consumer credits, mortgage credit, and other credit. This information

⁷Our results are robust to including repeat arrests.

is available quarterly since 2004, allowing us to track all credit in the formal financial sector for all individuals in our sample.

We link these 4 sourced of data together at the individual level in 2010. We can then longitudinally follow individuals and firms using the PILA. That is, we measure the unexpected firm-level mass-layoff events in 2010 and follow individuals' earnings and arrests until 2015, the most recent date for which we have crime data available. We restrict our estimation sample to firms with at least 11 workers, and to full-time employees aged 20 to 60, with at least 1 year of tenure in the same firm just before 2010. Finally, we 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. 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.

Table [A1](#) provides descriptive information for our sample. 58 percent of the workers are male and the average age in 2009 was 35.5 years old. We compute annual formal sector earnings by adding inflation-adjusted monthly formal sector earnings during the period covered by our formal employment data, using 2008 as a base year. The average monthly earnings is COL\$910,000, which was about US\$462 in 2009. On average, individuals in the sample work in firms with approximately 1763 employees. The unconditional probability of arrest is 0.19 percent.

4 Empirical Strategy

Our aim is to compare the 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 layoff events at the firm, as has been done by well-regarded work over the last few decades ([Aghion et al., 2016](#); [Black et al., 2015](#); [Charles and Stephens, 2004](#); [Couch and Placzek, 2010](#); [Del Bono et al., 2012](#); [Jacobson et al., 1993](#); [Sullivan and Von Wachter, 2009](#)). Mass layoffs capture time-specific characteristics of the firm rather than the worker, allowing us to navigate around individual-specific unobservable differences across workers.

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 variable 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. Our specifications control for time fixed effects γ_t , and individual fixed effects α_i that account for an individual's time-invariant characteristics.

In additional analysis we document the cumulative effect 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.⁸

Our parameters of interest are δ_k for $k = 0, 1, \dots, 5$. Our empirical specification includes a constant and as such our 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, i.e. $k = -2, -3, -4$, test whether the displacement event is correlated with the probability of being arrested before the event. Economic significance of such coefficients is evidence of dynamic selectivity into displacement, whereas a lack of meaningful effects in the pre-period suggest that individual-level dynamic selection into a mass layoff is unlikely.

Identification relies on two features of the mass-layoff. First, the mass-layoff was unanticipated by workers, and uncorrelated with worker-specific characteristics. This assumption is supported by the lack of pre-trends in our analysis. Second, a substantial group of workers lose their jobs leading to earnings losses, providing relevance to the underlying source of variation. We hypothesize that such unexpected losses explain why individuals end up sorting into criminal activities.

⁸[Cameron et al. \(2011\)](#) suggest that in matched employer-employee studies, practitioners should allow for clustering at both employer and employee levels, in particular when there are repeated observations at the employee level.

5 Displacement effects

We first present results estimating equation 1 for earnings and crime outcomes, and heterogeneous effects by gender and age. We also explore the effect across sectors with differing employment dynamics (i.e., comparing booming sectors to slumping sectors), to emphasize the role of alternative legitimate employment opportunities in determining the elasticity between job loss and crime.

We test whether workers with tighter economic constraints show stronger responses. We explore two types of constraints: occupational constraints and financial constraints. For the former, we explore whether workers in booming sectors with more legitimate employment alternatives exhibit weaker criminality responses to job loss. For the latter, we leverage a credit expansion reform, to analyze whether exogenously improved access to credit mitigates this elasticity between job loss and crime. Finally, we explore the effects on young relatives of workers that suffer significant earning losses. These within-family spillovers allow us to document the aggregate consequences on the family.

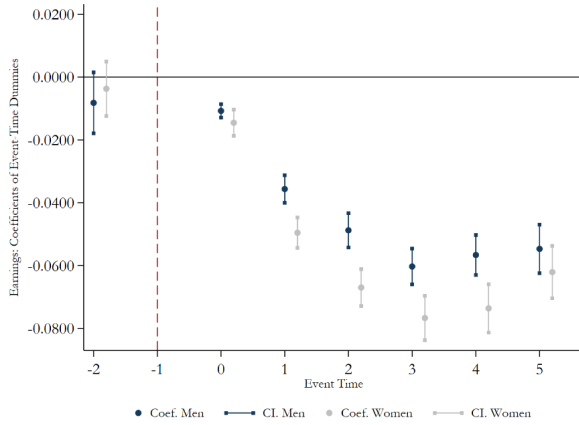
5.1 The Effect of Job-Loss on Earnings and Arrests

Prior work for developed countries shows substantial and long-lasting negative impacts of job displacements on earnings (Couch and Placzek, 2010; Jacobson et al., 1993). Here we document such losses in the context of a low- and middle-income setting. In line with previous work, we define earnings losses as the difference between their actual and expected earnings had the events that led to their job losses not occurred. Figure 2a shows the effects associated with the job displacement event. Since formal earnings are only available after 2007, we show that the displacement event is not correlated with earnings just before the mass-layoff. We observe a persistent loss in earnings that lasts up to five years after the event.⁹ The magnitudes are meaningful enough to suggest there might be permanent negative effects on labor earnings. We observe strong effects in the year of the job loss, increasing up to 3 years after the layoff, where earnings are lower by about 7.3% with respect to average earnings in 2009 of COL\$ 910,000. The fall in earnings is slightly stronger for females.

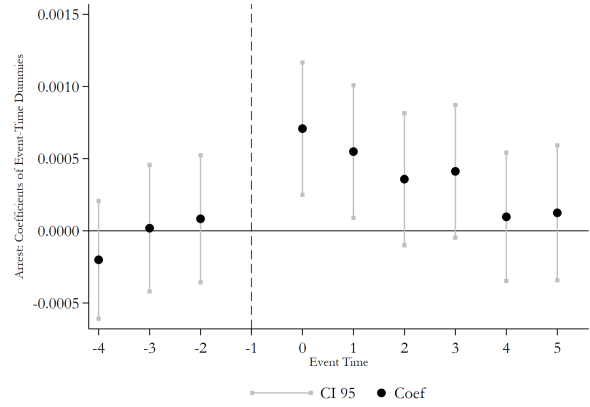
Layoffs generate an immediate response in criminal behavior. Figures 2b to 2d, and table A2 show the results for the probability of arrest, for the entire sample and across genders and age groups,

⁹Figure A2 shows the effect of a mass layoff event, five years after the event, on the probability of being formally employed for at least six months.

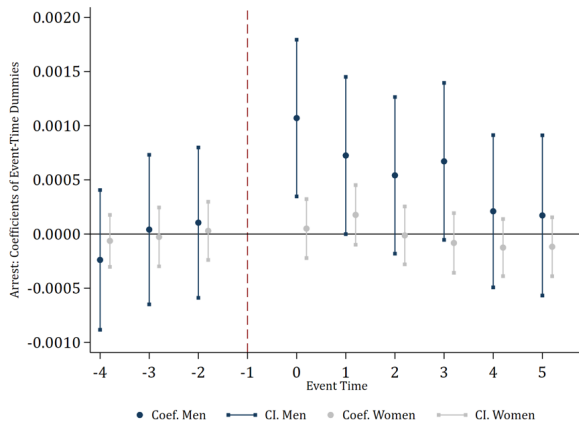
Figure 2: Effects of firm-level mass layoffs on earnings and arrests



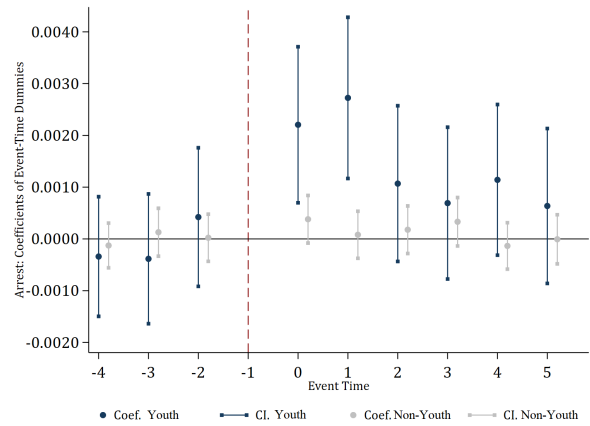
(a) Event study estimates on earnings by gender



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



(c) Event study estimates on arrests by gender



(d) Event study estimates by age group

Notes: Figure 2a shows the effect of firms mass layoff event, from two years before event year to five years after the event on average annual earnings, for men and women (trimming the earnings distribution at the 1% and 99%). We compute annual formal sector earnings by summing the inflation-adjusted monthly formal sector earnings during the period covered by our formal employment data 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. Figure 2b shows the effect of a firm's mass layoff event from four years before event year to five years after the event on arrests. Number of observations: 10 years x 457096 individuals. Figures 2c and 2d show heterogeneous effects of mass layoff events on arrest by gender and age, respectively. 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. 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.

respectively.¹⁰ The average probability of arrest is 0.16% the year before the event, and for the full sample it increases 45% in the year of the event, and 35% the year after. The effect steadily reduces to zero after that.

We find that among all workers exposed to mass layoffs, the increase in arrests was particularly

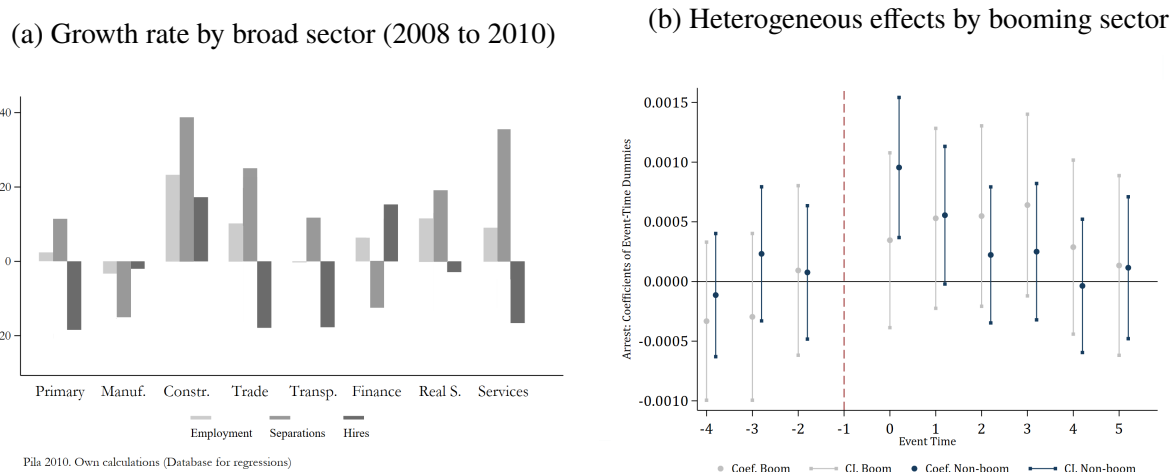
¹⁰We show the cumulative effect on arrests in Figure A3.

driven by young men (Figures 2c and 2d). The larger effect on male youth may reflect the opportunity to join criminal enterprises and gangs. Like in other high-crime settings (Sampson and Laub, 2005), Medellín shows a strong crime-age pattern where the arrest rate steady declines after the age of 25.

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. We interpret this evidence as absence of dynamic selectivity into job displacement on the basis of arrests.

5.2 Heterogeneous Effects Across Alternative Opportunities

Figure 3: Event study estimates by alternative work opportunities: booming and non-booming sectors



Notes: Figure 3a shows employment growth by broad sector categorization over the period 2008-10. Booming sectors are defined as those economic sectors with employment growth over the average employment growth in Medellín. The figure 3b shows the effect of firms mass layoff event (defined as those firms where 30-90% of their employees were separated in 2010) from four years before the event year to five years after the event on arrests (arrest is 1 if the employee was arrested at least once in a year). 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. Number of observations for booming sectors: 10 years x 183068 individuals. Number of observations for non-booming sectors: 10 years x 274016 individuals.

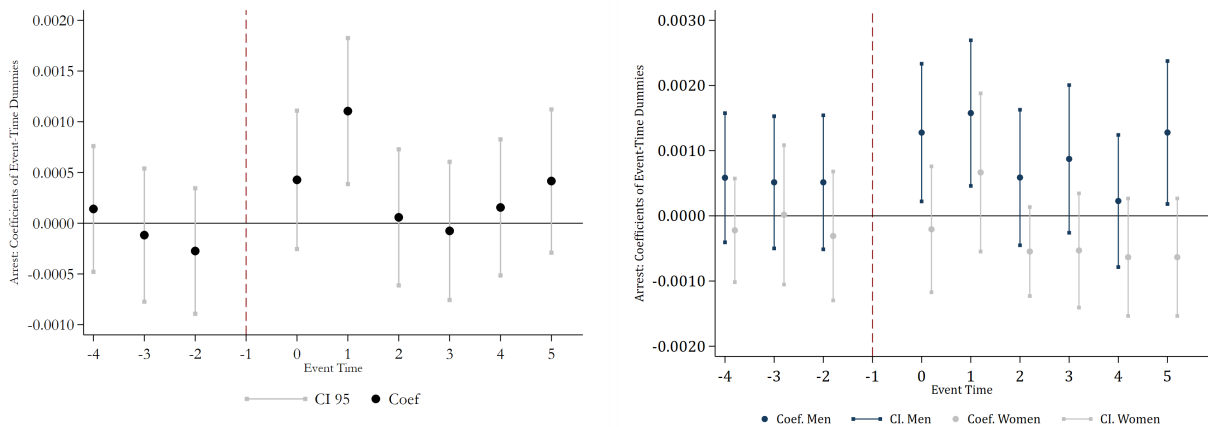
A worker's outside options may play an important role in determining their decision to join a criminal enterprise. For instance, if the construction sector is booming, then a construction worker who is laid off may easily find work elsewhere. Yet, if the sector were slumping, then it may be difficult to find alternative legitimate employment options, inducing more individuals to get involved

in illegitimate activities after the layoff. We follow workers according to their baseline sector of employment, and compare those working in booming sectors to those in slumping sectors.

Figure 3b presents the effects on arrests by booming and slumping sectors. Booming sectors are defined as those with employment growth greater than the average employment growth in Medellín. Consistent with predictions of occupational sorting models, in booming sectors with more legitimate employment alternatives, we cannot rule out the possibility of no effect on arrests; while in slumping sectors the probability of arrest increases by 70% in the year of the event and 51% the year after. These patterns strongly suggest that alternative employment options play a crucial role in determining the elasticity between job loss and crime.

5.3 Spillovers to Family Members

Figure 4: Event study estimates of arrests on other youth in the family



(a) Employees relatives (youth)

(b) By employee gender (household heads)

Notes: Figures show the effect of firms mass layoff event (defined as those firms where 30-90% of their employees were separated in 2010) from four years before event year to five years after the event on arrests (arrest is 1 if the employee was arrested at least once in a year). 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 that show up in SISBEN II. Number of observations in Figure 4b for male workers heads of household: 10 years x 63496 and for female working heads of household: 10 years x 27395.

The job displacement shock may decrease a household's income sufficiently to push other members of the family to find additional sources of income. Additionally, as some displaced workers are pushed into crime, role-model effects may induce other youth in the household to follow them into criminal enterprises. In settings like Medellín where individuals, particularly youth, not formally employed have

ready opportunities to get involved in crime, young relatives of laid-off workers may be particularly susceptible to be drawn into crime.

Figures 4a and 4b show event-study estimates for young relatives. On average, we find more meaningful impacts in the year after the layoff. Importantly, we find 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 4b). This may suggest that role model effects are a contributing mechanism, since if income-based necessity was the only channel via which youth were induced into crime, then the gender of the laid off worker should matter less.

While we focus on spillovers effects on young relatives between the ages of 14 to 24, our results are similar for others in adjacent age ranges. Since we only observe relatives in the SISBEN surveys, the estimations are conducted for this sub-sample.¹¹

6 Job-Loss and Credit

Finally, we investigate the role that access to consumption credit might play in the criminal response to job loss. To the best of our knowledge, we are the first to study the relationship between access to credit and crime.¹² We focus on retail consumption credit, as the most proximate determinant of financial necessity in the aftermath of a job loss. We match individual-level administrative records from *Superfinanciera*, the Colombian government agency responsible for overseeing financial regulation, 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 the policy reform in the aftermath of the employment shock, as well as any pass-through effects on criminality.

In figure 5a we explore the arrest response to job loss across baseline access to consumption credit.¹³ Consistent with desperation-driven criminality and the liquidity constraints derived from not being formally employed, we find that among all workers exposed to mass layoff events, the increases in arrest rates only took place among those who did not have consumption credit before the event.

¹¹Our main effects of job-loss on crime are similar in the SISBEN and non-SISBEN samples.

¹²There 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 \(2007\)](#).

¹³We show the cumulative effect on arrests in Figure A3.

Within the sub-sample of individuals who did not have access to consumer credit before the event, the probability of arrests increases by 63% in the year of the event and 51% the year after. This suggests that consumer credit acts as a safety net and prevents individuals from resorting to crime out of financial necessity.

6.1 Instrumental Variables for Access to Credit

Our primary objective is to measure the causal effect of having access to consumption credit on the elasticity between job losses and arrests. Given the potential endogeneity between 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 the causal effect. We leverage a supply shock associated with the 2009 financial reform in Colombia (Act 1328 of 2009) that regulated the entrance and formalization of branch offices of foreign credit institutions and national financing companies. In addition, the reform gave the power to both foreign and national institutions to operate as a credit institution (i.e., as banks). In practice, the reform created a supply shock to the number of bank branches in Colombia. In total, five financing companies became banks between May 2010 and May 2011. In Medellín, five new banks entered with 19 new branches in 2011.¹⁴

We first document that the probability of access to credit strongly depends on the distance between an individual's residence and the new branches. We geocode each individual's address in our administrative data, and the geo-location of all bank branches to estimate the effect of distance to new branches on changes in access to credit. Doing so allows us to exploit a supply-shock to credit, not associated with an individual's characteristics, alleviating concerns of endogeneity.

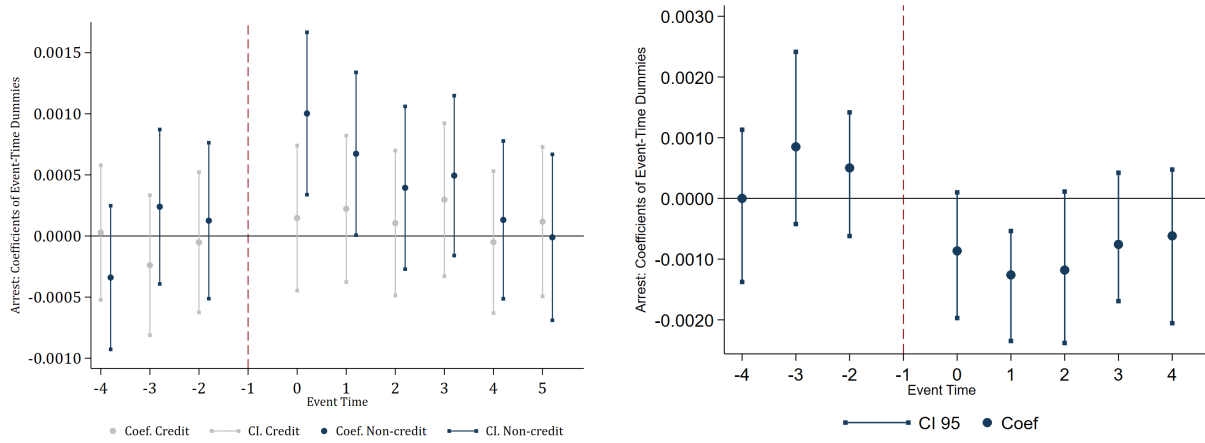
Using Google MY MAPS and information from the business and social registry of the chambers of commerce of Medellín (RUES), we locate the coordinates for new branches. In addition, SISBEN data allow us to locate the block where the individual was located before the reform.¹⁵ The SISBEN census of the poor represents 54 percent of all individuals in the job-displacement sample, where 94 percent of the sample have a valid address in Medellín. Then, we compute the Euclidean distance from each new branch in the city to each individual in our SISBEN sample. Our main instrument

¹⁴Falabella 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. At the end of 2011, we identify 19 new bank branches in Medellín.

¹⁵As [Alcaldía de Medellín \(2011\)](#) point out intra-urban migration in Medellín is very low.

is the distance to the nearest new branch in Medellín for each individual. We find that the nearest distance is a strong predictor of the amount of credit associated with new consumption credit. We predict access to new consumption credit using our instrument (the nearest distance) and controlling for *comuna* (neighborhood) fixed effects and a set of covariates.¹⁶ The first-stage estimates for this regression are presented in table A3, and shows a strong relationship between distance to the new bank branches and the amount of credit.

Figure 5: 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 table A3, with an F-statistic of 11. Figure 5a shows the effect of a firm's mass layoff event from four years before event year to five years after the event on arrests. The regression includes 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 5b shows the heterogeneous effect of mass layoff event by access to credit on arrest. 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 that in SISBEN II, so as to use the geolocation of residence. Number of observations: 10 years x 148386.

Using the prediction on the amount of credit associated with new consumption credit, we re-estimate our event study model. In this new model, we fully interact the predicted amount of credit with the job displacement variable as follows:

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

where \widehat{Credit}_i is the predicted credit from the first stage on distance to new bank branches. In this

¹⁶The set of covariates includes SISBEN score (poverty index), education, socioeconomic strata, gender and age.

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 follow the two-step bootstrapping algorithm procedure applied in [Ashraf and Galor \(2013\)](#).

Figure 5b shows the estimates for the β_k coefficients of the equation 2. We find that among all workers exposed to mass layoffs, those who benefit from the credit supply shock exhibit a reduction in the job loss-crime elasticity in 2011 and 2012, although the 2011 effect (when new branches were first opened) is only statistically significant at 10% level. Like before, the interaction between the displacement event and the credit variable is not correlated with the probability of being arrested before the job displacement event.

7 Conclusion

We document that mass-layoffs produce a permanent earnings loss and a sharp increase in the likelihood of being arrested for both the worker who directly suffers the unexpected shock and their younger relatives. While prior work has focused on high-income countries with strong judicial and police institutions ([Bennett and Ouazad, 2018](#)), we evaluate these effects in the context of low- and middle-income settings with weak institutions and high-crime. To do so, we build an employer-employee-crime matched database that follows individuals and firms over a decade in Medellín, Colombia. We combine these data with unique administrative information on crimes of family members, credit histories, and geolocations to evaluate the degree to which economic incentives mediate the criminal response to job losses.

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 then whether the demonstrated criminal response to employment shocks is realized primarily through economic channels such as restricted access to legitimate reemployment or acute financial necessity. We note that, although the effects on earnings are observed in both male and female samples, the increases in arrest rates only took place among males (most likely to have criminal employment opportunities).¹⁷ We

¹⁷Also, additional analysis not included, but available upon request, shows the effects are concentrated in property crimes (most likely to reflect economic need).

also leverage an additional source of identifying variation, an instrument for access to consumption credit derived from a policy reform that generated a geographically targeted credit expansion, to investigate the causal impacts of access to consumption credit on the individual-level job loss-crime elasticity. We find a strong mitigative effect of consumption credit, as well as complementary evidence of heterogeneity in the criminal response to job loss by opportunities for employment replacement by industry. Taken together, we argue this evidence confirms a primary role for economic incentives in determining the criminal response to formal employment shocks.

It is important to note, however, that access to credit markets usually depends on whether the individual has a formal job, and access to formal employment often depends on credit histories. In this way, an unexpected employment shock may actually limit access to new lines of credit, and any defaults on existing lines of credit after such a shock may limit opportunities for reemployment, trapping individuals in a vicious cycle. Impacts on mental, emotional, and physical health can also contribute, limiting an individual's productivity. Similarly, criminal arrests will limit access to gainful reemployment. Future research should investigate these holistic impacts of employment shocks, focusing on which conditions make an affected individual most susceptible to descending into poverty trap type conditions and/or what policies might insulate against these compounding impacts on economic opportunity.

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Online Appendix

Table A1: Summary statistics

| | 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 in Booming Sector | 0.4025 | 0.4904 | 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 |
| Booming-Sector: | | | |
| Booming | 0.0019 | 0.0440 | 184,000 |
| Non-Booming | 0.0018 | 0.0427 | 273,096 |
| 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. Credit in millions of nominal \$COL.

Table A2: 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 |

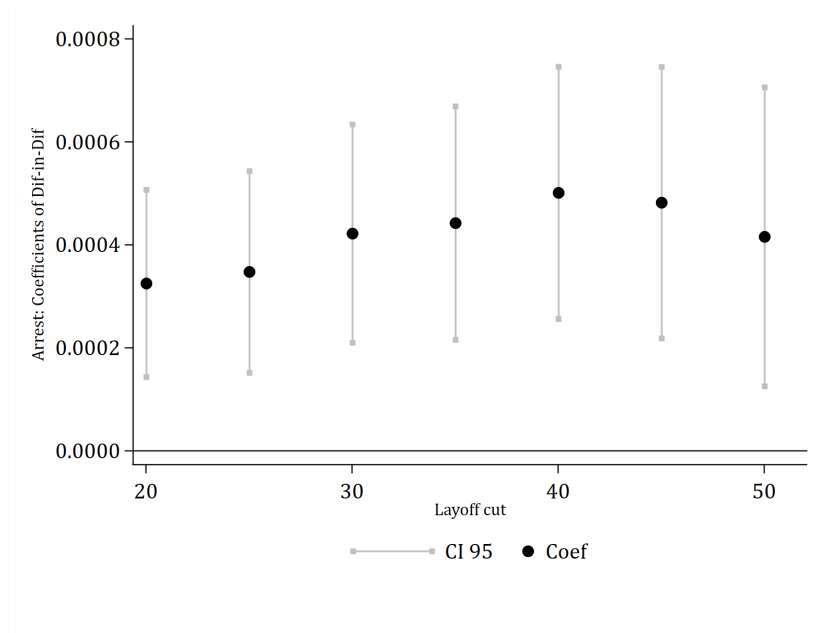
Notes: Standard errors are clustered at the individual and firm-level. The sample includes drug, property, violence, and other crimes. Table A2 lists δ_k from equation (1). Event time is measured in years. Arrested outcome is binary indicator: 1 if the event occurred at any point in the year, 0 otherwise.

Table A3: First stage for credit access and minimum distance to branch

| | Amount of Credit |
|--------------------|-------------------|
| Minimum Distance | -0.025 (0.007) |
| Observations | 148,386 |
| R-squared | 0.012 |
| First stage F-stat | 10.922 |

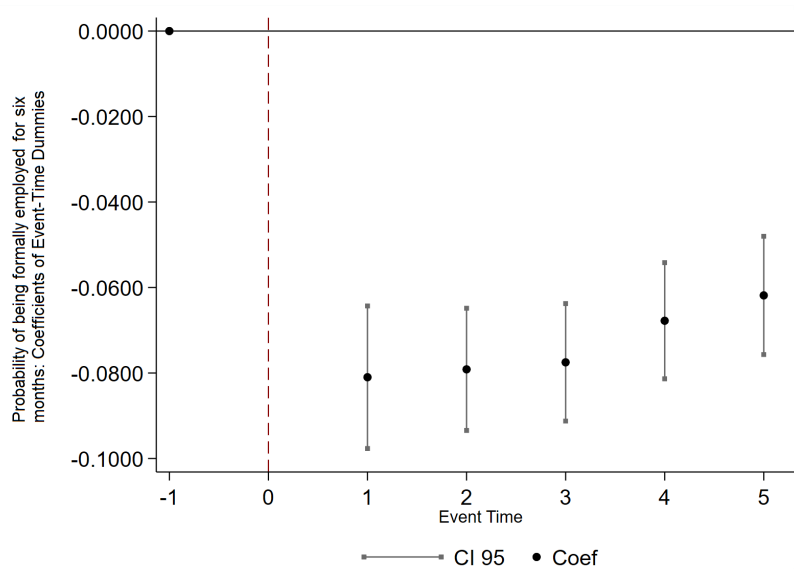
Notes: Standard errors are clustered at the neighborhood level. The regression includes *comuna* fixed effects and controls for the Sisben score (poverty index), education, socioeconomic strata, gender and age. The independent variable is the minimum distance between ones residence and the nearest new branch opened under the credit-expansion program. The average minimum distance to a new banks is 3 kilometers. The dependent variable is the amount of credit in millions \$COL.

Figure A1: Robustness to different layoff cutoffs



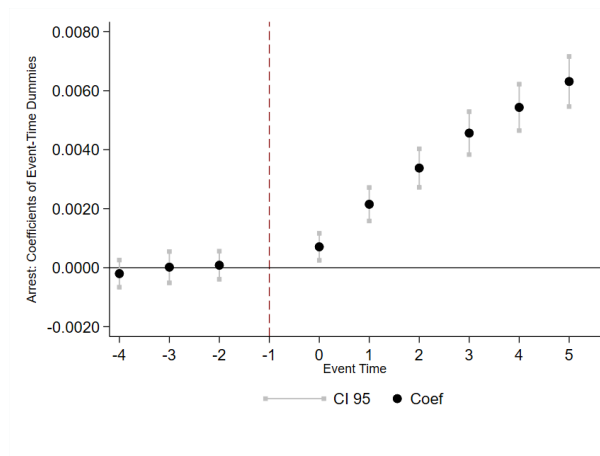
Notes: Figure A1 shows robust evidence of using alternative cutoffs to define layoff events. We estimate the difference-in-differences coefficient, comparing before-after the mass layoff, and workers in firms with and without layoffs. The horizontal axis varies the layoff cutoff value from 20% of job 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 on Formal Employment

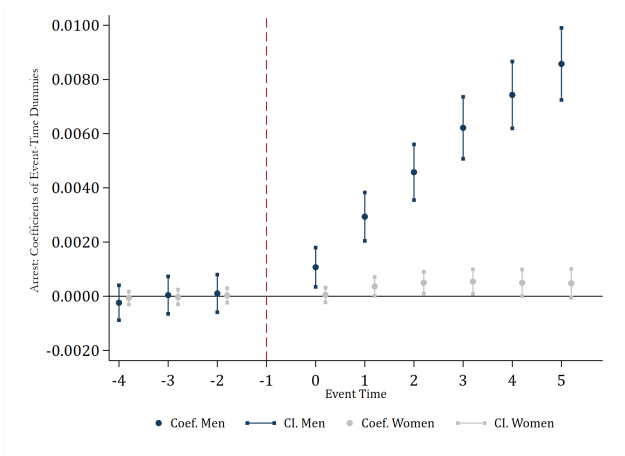


Notes: Figure A2 shows the effect of firm mass layoff events, five years after the event, on the probability of being formally employed for at least six months within a year. Number of observations: 5 years x 457096 individuals. The regressions include individual and year fixed effects. Standard errors 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.

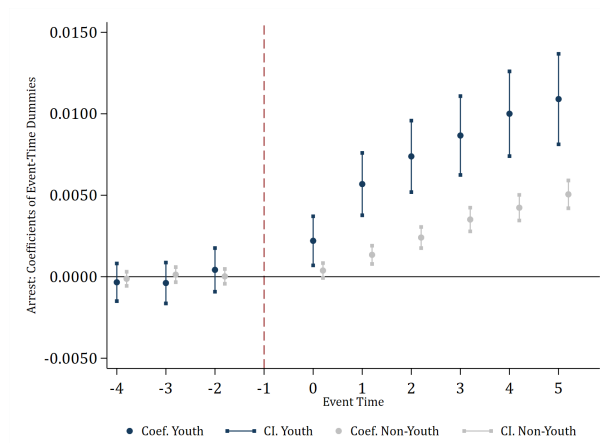
Figure A3: Effects of firm-level mass layoffs on cumulative arrest



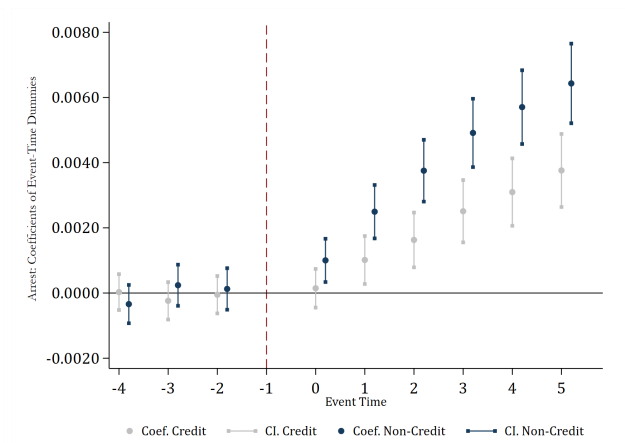
(a) Event study estimates on cumulative arrest



(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 firms 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 A3a shows the effect of firms mass layoff event, from four years before event year to five years after the event on cumulative probability of being arrest. Number of observations: 10 years x 457096 individuals. Figures A3b to A3d show heterogeneous effects of mass layoff event 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. Layoff event is defined as those firms where 30-90% of their employees were separated during at least six months in 2010. Arrest is 1 if the employee was arrested at least once in a year.