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Job Loss, Credit and Crime in
Colombia

By: Gaurav Khanna
Anant Nyshadham
Carlos Medina
Christian Posso
Jorge Tamayo

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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 novel matched employer-employee-crime dataset in Medellín, Colombia. Job displacement leads to immediate earnings losses, and an increased likelihood of being arrested for both the displaced worker and for other youth in the family. We leverage variation in opportunities for legitimate reemployment and access to consumption credit to investigate the mechanisms underlying this job loss-crime relationship. Workers in booming sectors with more opportunities for legitimate reemployment exhibit lower increases in arrests after job losses. Greater exposure to an expansion in consumption credit also lowers the increase in arrests after employment shocks.

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

‡Boston College & NBER, nyshadha@bc.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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Pérdida del Empleo, Crédito y Crimen en Colombia

Gaurav Khanna Carlos Medina
Anant Nyshadham Christian Posso
Jorge Tamayo

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Resumen

Se estiman los efectos de la pérdida del empleo como resultado de un despido masivo, sobre la probabilidad de ser arrestado, utilizando una novedosa base que paree datos de empleados, empleadores y arrestos en Medellín, Colombia. La pérdida del empleo genera una reducción inmediata en los ingresos, y un incremento en la probabilidad de ser arrestado tanto para el empleado despedido, como para otros familiares jóvenes. Nosotros explotamos la variación en las oportunidades de re-emplearse formalmente y en el acceso a crédito formal de consumo, para estudiar los mecanismos detrás de la relación entre la pérdida del empleo y el crimen. Los trabajadores en sectores en auge, con mayores oportunidades de re-emplearse, ven menos afectada su probabilidad de ser arrestados. Una mayor exposición a una expansión en la oferta de crédito de consumo también debilita la respuesta de los arrestos al choque en el mercado laboral.

Palabras clave: Pérdida del empleo, crimen, Medellín

Códigos JEL: K42, J63, J65

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

¹The only published study to our knowledge estimating the impacts of job loss on crime focuses on a high-income, low-crime environment with strong unemployment safety-nets (Bennett and Ouazad, 2018). The impacts we estimate are larger, especially as we additionally show spillover impacts 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. Our estimates capture 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 on 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; Fu and Wolpin, 2017; Khanna et al., 2019) 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. By using two distinct sources of identifying variation, we are able to make causal inferences about the ability of consumption credit to dampen the criminal response to

employment shocks.

Finally, we add to the literature on the intergenerational spillovers of crime (Hjalmarsson and Lindquist, 2013; Meghir et al., 2012) and of job loss (Hilger, 2016; Oreopoulos et al., 2008; Rege et al., 2011) by being among the first to document 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 7 concludes.

2 Background

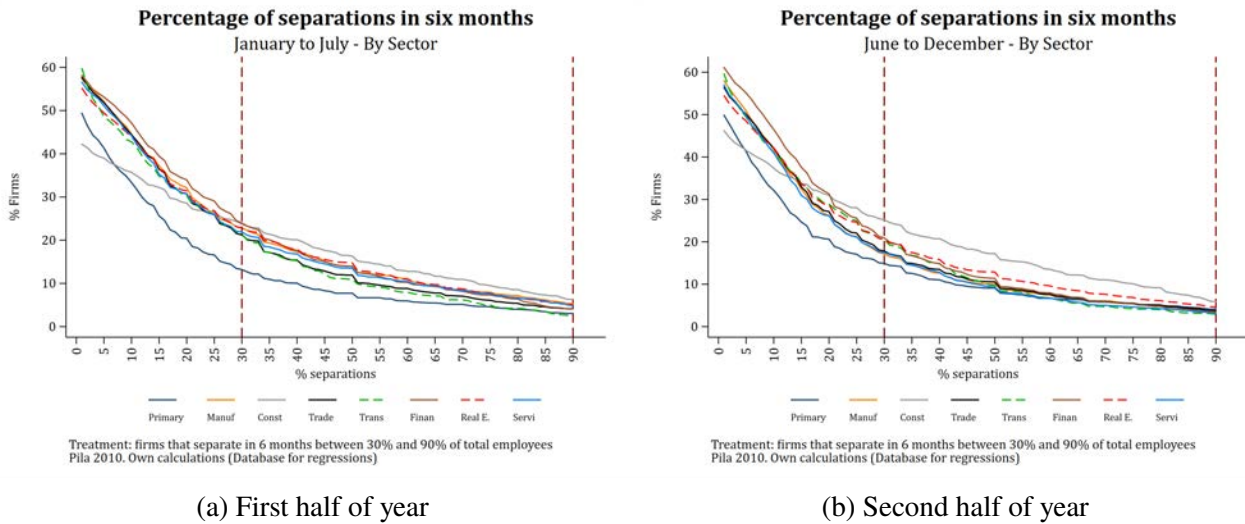
Located in the north-western region of Colombia, Medellín is the second largest city after the capital, Bogota. It has strong industrial and financial sectors with approximately 2.3 million people or 5.5% of the Colombian population. The urban zone consists of 249 neighborhoods, divided into 21 (*comunas*),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

Mass layoffs in Medellín differ 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 to be if between 30% to 90% of workers were separated from the firm within a year.² On average, this represents about 20% of firms in one 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 other cutoffs, such as 70% of workers being laid off. 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



(a) First half of year

(b) Second half of year

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 no longer shows as being 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 in 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, given their experience (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 Youths, between 14 and 26 years, 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).

⁴According to the Judicial Research Unit of the Metropolitan Police of Medellín (SIJIN).

slightly older individuals are involved in violent crimes (homicides, extortions, 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 relationships of delinquency supply, and 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 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.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 in [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 the information of 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 the employees.

The second administrative data, from the Judicial Research Unit of the Metropolitan Police of Medellin (SIJIN), is the census of all individuals arrested in Medellin 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, this data allow us to split the sample by household socio-economic status.

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 credits 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 year 2004, allowing us to track all credit in the formal financial sector for all individuals in our sample.

PILA allows us to longitudinally follow individuals and firms. We focus on 2010 to measure the unexpected firm-level mass-layoff events that year 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% to 90% of its employees during at least six months 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 males 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 29 days per month and work in firms with approximately 1763 employees. The unconditional probability of arrest is 0.19 percent.

4 Empirical Strategy

We would like to compare the arrest rates among those who lose a job and those who do not. Yet, individual probability of job loss may be correlated with an individual's proclivity to commit crimes. That is, for example, 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:

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

where $Arrested_{it}$ is an indicator variable recording whether individual i was arrested at time t , $Displaced_{it}^k$ is indicator for whether the individual worked in a firm that displaced at least 30 percent of its workers within six consecutive months in year t , and k indexes the set of time indicator variables beginning 4 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 $Arrested_{it} = 1$ 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 sets the coefficient δ_{-1} equal to zero. Consequently, all estimated δ_k parameters are relative to the probability of being captured the year prior to the event. We interpret the statistical 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 at the time of the event, 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 losses in earnings, providing relevance to the underlying source of variation. We hypothesize that such unexpected losses may explain why individuals end up sorting into criminal

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.

activities.

5 Displacement effects

We first present the results of the estimation of equation 1 for earnings and crime outcomes, and some heterogeneous effects by gender and baseline poverty status. We also explore the effect across sectors with positive and negative employment dynamics leading into the event (i.e., booming sectors vs. slumping sectors), to emphasize the role of alternative legitimate employment opportunities in determining the elasticity between job loss and crime.

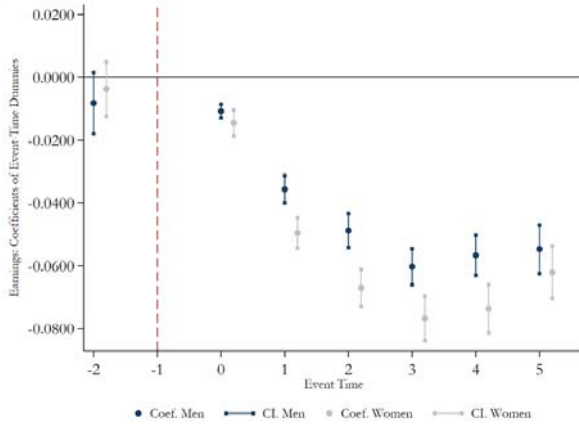
We test whether workers with tighter economic constraints show stronger responses. We explore two types of constraints: income (poverty) constraints and financial constraints. Similarly, we explore whether workers in booming sectors with more legitimate employment alternatives exhibit weaker criminality responses to job destructions. 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

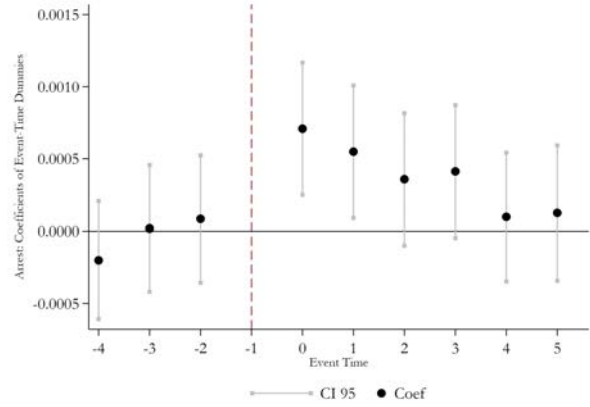
Prior work for developed countries show substantial and long-lasting negative impacts of job displacement on earnings (Couch and Placzek, 2010; Jacobson et al., 1993). Here we document such losses in the context of a low- and middle-income setting. In line with previous work, we define earnings losses 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 events. 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 formal labor earnings. Although we observe effects the year of the event, the largest impact occurs 3 years after the mass-layoff, where earnings are lower by about 7.3% with respect to average earnings in 2009 of COL\$ 910,000. The response in earnings is slightly stronger for females.

⁹Figure A1 shows the effect of firms mass layoff event, five years after the event, on the probability of being formally employed for at least six months.

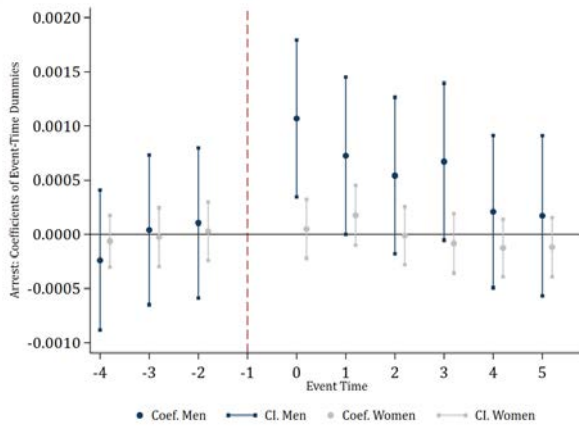
Figure 2: Effects of firm-level mass layoffs on earnings and arrests



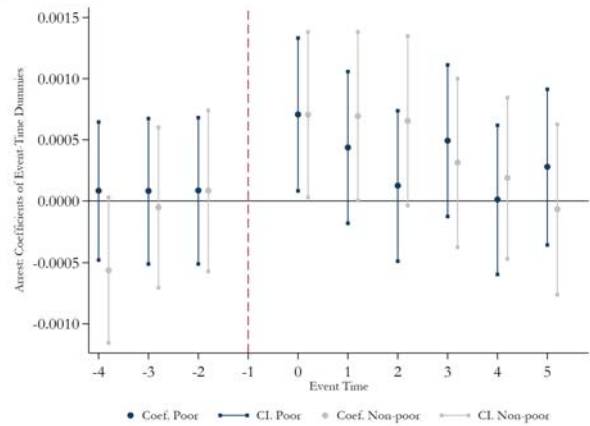
(a) Event study estimates on earnings by gender



(b) Event study estimates on arrests



(c) Event study estimates on arrests by gender



(d) Event study estimates by poverty status

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 (excluding 1% above and below the earnings distribution). We compute annual formal sector earnings by adding inflation-adjusted monthly formal sector earnings during the period covered by our formal employment data using as a base year 2008. 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 firms 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 event on arrest by gender and poverty, respectively. Poor is 1 when employee information is matched with poverty census. 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. 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.

Layoffs generate an immediate significant response in criminal behavior. Figures 2b and 2c, and table A2 show the results for the probability of arrest, for the entire sample and across genders.¹⁰ The probability of arrest is 0.16 percent in 2009, the year before the event, and increases 45 percent in the

¹⁰We show the cumulative effect on arrests in Figure A2.

year of the event and 35 percent 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 only took place among males, and the effects are concentrated on property crimes (robbery, thefts, etc.). The larger effect on men may reflect the opportunity to join criminal enterprises and gangs. Additionally, 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.

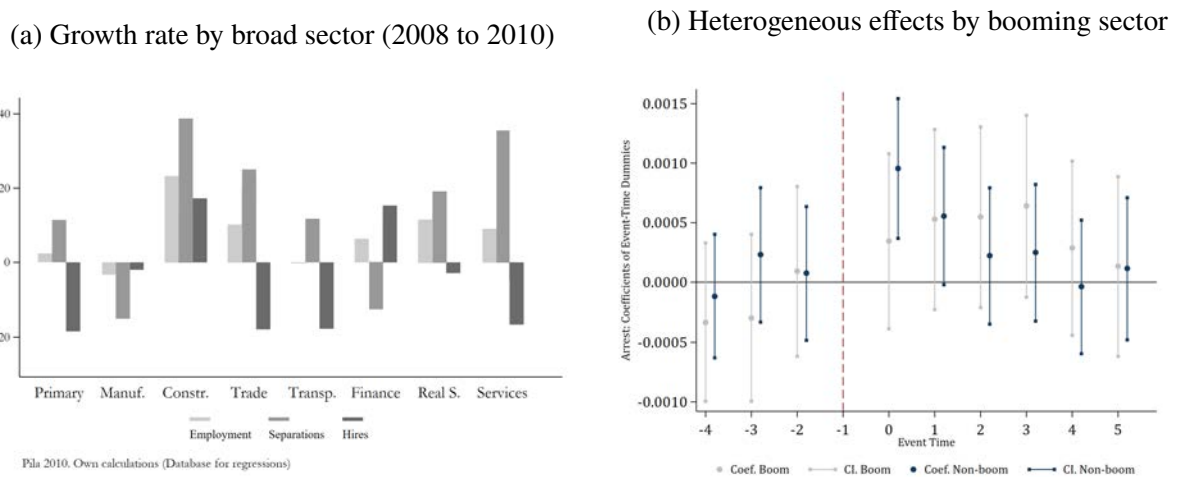
In figure 2d we explore the role played by poverty constraints in determining the relationship between job loss and arrests. Using administrative records from the SISBEN census of the poor, we split the data between poor and non-poor workers. The Colombian government uses the data on assets and wealth from the SISBEN census to create an asset poverty score for each household, based on a formula not available to the public. Poor workers are defined as those whose poverty status places them in the 2005 SISBEN II. We do not find heterogeneous effects in this case; the impacts on the arrest rates are similar in both sub-samples. This suggests that baseline levels of poverty are not strongly associated with the increase in arrests, perhaps as the Colombian government targets safety net programs to those identified as poor by the SISBEN II.

5.2 Heterogeneous Effects Across Alternative Opportunities

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 find work easily elsewhere. Yet, if the sector were slumping, then it may be difficult to find alternative employment options, inducing more individuals to get involved in risky behavior 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 this split of the results by booming and slumping sectors. Booming sectors are defined as those with employment growth over the average employment growth in Medellin. 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 arrest; while in slumping

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 of the employment growth in Medellin. The figure 3b shows the effect of firms mass layoff event (defined as those firms where 30-90% of their employees were separated during at least six months 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 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 booming sectors: 10 years x 183068 individuals. Number of observations for non-booming sectors: 10 years x 274016 individuals.

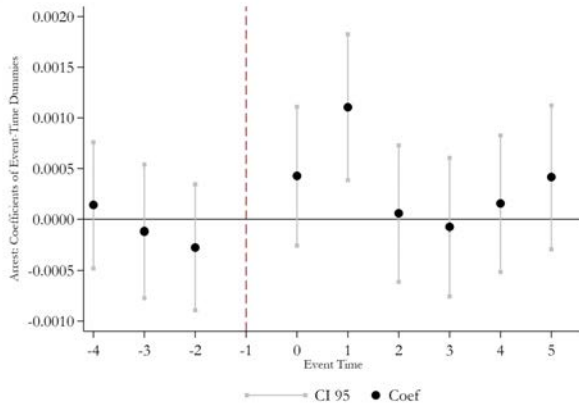
sectors the probability of arrests increase by 70 percent in the year of the event and 51 percent the year after. These patterns suggest strongly that alternative employment options play a crucial role in determining the elasticity between job loss and crime.

5.3 Spillovers to Family Members

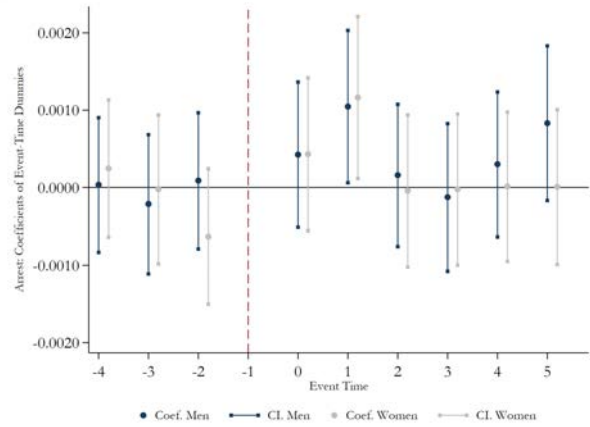
A standard model of consumption and labor supply predicts that after a decrease in a household's income, consumption should reduce while individuals' time allocated to the labor market increases. The job displacement shock may decrease a household's income pushing other members of the family to find additional sources of income. In settings like Medellin where individuals not formally employed have several opportunities to get involved in criminal activities, in particular, young adults, it may occur that young relatives of laid off workers are drawn into organized crime.

Like in other high-crime settings (Sampson and Laub, 2005), Medellin shows a strong crime-age pattern where the arrest rate peaks in the late teens and declines rapidly thereafter. The peak ages of arrest lie between 14 and 24 years old. While we focus on the spillovers effects on young relatives

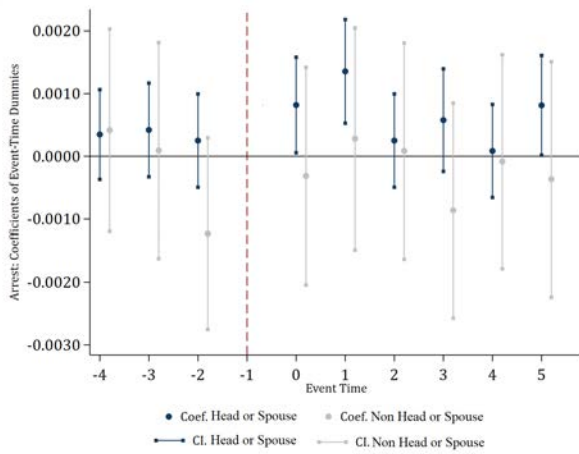
Figure 4: Event study estimates of arrests on other youth in the family



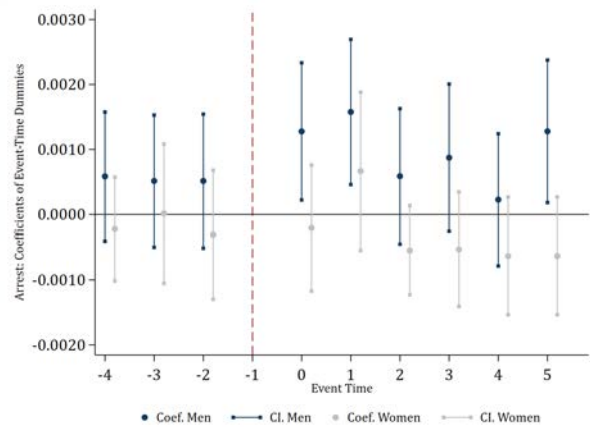
(a) Employees relatives (youth)



(b) Effects on youth family members by gender



(c) By whether employee was head of household



(d) 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 during at least six months 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 regression 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 4a: 10 years x 169881 individuals. Number of observations in Figure 4b for men: 10 years x 84430 individuals. Number of observations in Figure 4b for women: 10 years x 85451 individuals. Number of observations in Figure 4c for workers heads of household or spouses: 10 years x 90891. Number of observations in Figure 4c for workers non heads of household or spouses: 10 years x 78990. Number of observations in Figure 4d for man workers heads of household or spouses: 10 years x 63496. Number of observations in Figure 4d for woman workers heads of household or spouses: 10 years x 27395.

between 14 to 24 years old, 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.

Figures 4a-4d show event-study estimates for young relatives. Contrary to the direct effects on workers, for younger relatives we find a large significant increase in the probability of arrests only a year after, and not the year of the displacement event. Once we focus on layoffs for the relatives of the

head of households, we observe an effect on both the year of the event and one year after the event (figure 4c). This suggests that the spillovers to youths are more immediate when the shock affects the main economic provider. Finally, the gender of the earner laid off matters, with male displaced workers having a larger effect on the probability of arrests of youths in their household (figure 4d).

6 Job-Loss and Credit

Finally we turn to the role played by access to credit. To the best of our knowledge, we are not aware of any work that studies the relationship between access to credit and the probability of arrests. We focus on a specific kind of credit that helps displaced workers smooth their consumption over time, when necessary. We obtain unique data documenting the credit histories of individuals with their identification numbers, allowing us to match these data with our administrative database. We first show heterogeneous effects by baseline access to credit. Next, we leverage variation in exposure to a credit expansion policy in an instrumental variables approach to document the causal impact of access to credit on the elasticity between job loss and arrests.

Our attempt is to document one of the first causal relationships between the access to credit and the probability of arrests.¹¹ Credit here works as a mitigating mechanism of the initial shock of job loss. We obtain administrative records from *Superfinanciera*, the Colombian government agency responsible for overseeing financial regulation, allowing us to identify workers that before and after the event did have access to consumer credit. The baseline information allows us to study heterogeneous effects between those who did and did not have access the year before the event.

In figure 5a we explore the arrests response to job loss across baseline access to consumption credit. 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. Within the sub-sample of individuals who did not have access to consumer credit before the event, the probability of arrest increases by 63 percent in the year of the event and 51 percent the year after. This suggests that consumer credit acts as a safety net and allows individuals to not resort to crime.

¹¹There 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 \(2007\)](#).

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 loss and arrests. Given the potential endogeneity between socioeconomic status of the individual and his probability 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). Act 1328 (2009) 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., 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 Medellin, five new banks entered with 19 new branches in 2011.¹²

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 Medellin (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.¹³ 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 Medellin. Then, we compute the Euclidian 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 in Medellin 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* fixed effects and a set of covariates.¹⁴ The first-stage estimates for this regression are presented in table A3,

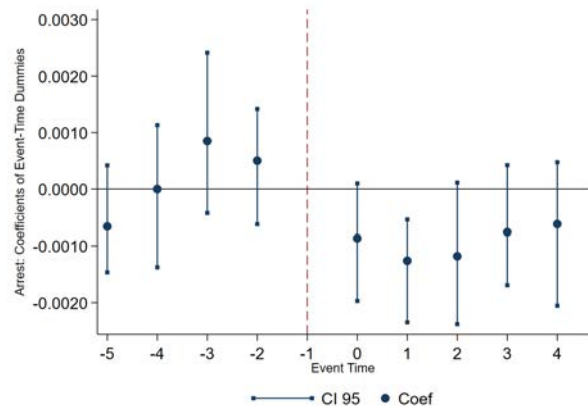
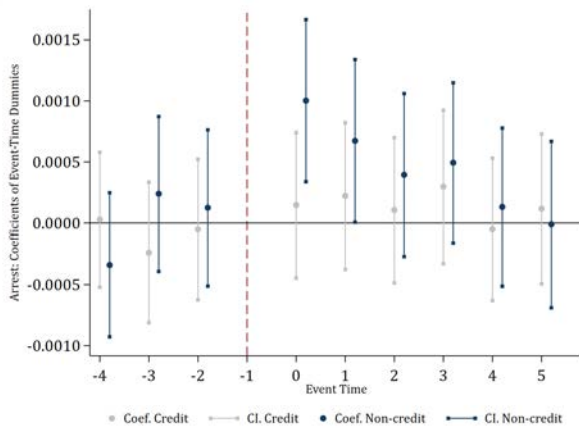
¹²Falabella Bank introduced 5 branches in September 2011, Pichincha Bank introduced 5 branches in July 2011, W Bank introduced 3 in October 2011, Bancoomeva introduced 5 branches in January 2011 and Finandina Bank introduced 1 branch in January 2011. At the end of 2011, we identify 19 new bank branches in Medellin.

¹³As [Alcaldía de Medellín \(2011\)](#) point out intra-urban migration in Medellin is very low.

¹⁴The set of covariates includes Sisben score (poverty index), education, socioeconomic strata, gender and age.

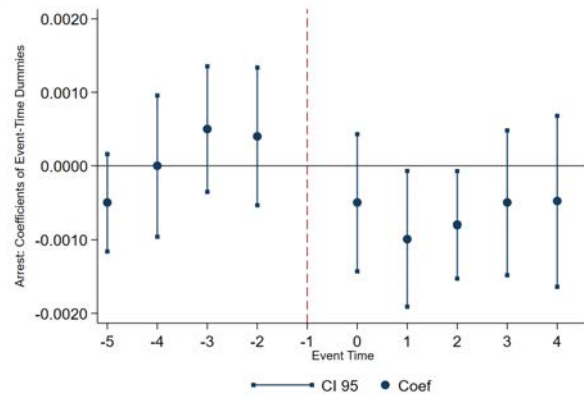
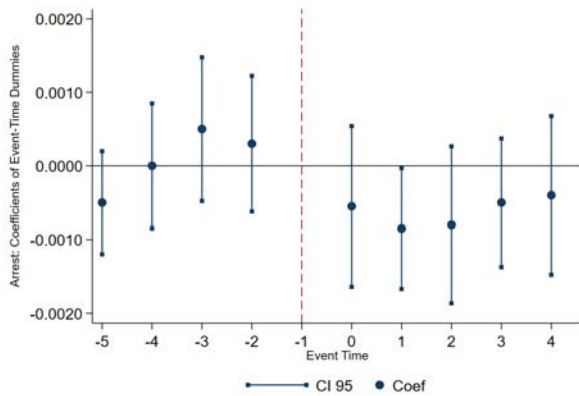
and shows a strong relationship between distance to the new bank branches and the amount of credit.

Figure 5: Heterogeneous effects by access to credit



(a) OLS: Event study estimates by consumption credit

(b) IV: Total consumption credit at new branches



(c) IV: Total amount of any type of credit

(d) IV: Total amount of credit at any bank

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 firms 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. Figures 5b to 5d show the heterogeneous effect of mass layoff event by access to credit on arrest from five years before and four years after the entry of banks. 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. 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:

$$\begin{aligned}
Captures_{it} = & \alpha + \sum_{k \geq -5} \beta^k \widehat{Credit}_{it}^k \times Displaced_i + \sum_{k \geq -5} \gamma^k \times Displaced^k \\
& \delta_1 Displaced_i + \delta_1 \widehat{Credit}_i + \delta \mathbf{X} + \theta_c + \epsilon_{it} ,
\end{aligned} \tag{2}$$

where \widehat{Credit}_{it}^k is the predicted 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 captures, along with *comuna* and year fixed effects, and covariates. 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.

While our main result is to do with consumption credit, we also present a specification where our independent variable of interest is the access to any amount of credit from any bank. Figures 5c and 5d show the β^k of equation 2, which captures the causal ameliorative effect of credit on the job loss-arrests relationship.

7 Conclusion

Studying the relationship between employment shocks and criminal activity comes with certain challenges. The first is related to identification. We need to isolate the effects of an individual's employment opportunities unrelated to their unobservable characteristics, such as motivation and ability. As such, we use mass layoffs which helps us cut past individual-specific unobservables that may contaminate our estimates. Additionally, the lack of pre-trends suggest that it is unlikely that workers are differentially selecting into firms that will have mass layoffs in the future.

We document that mass-layoffs cause a permanent loss on earnings, and a temporary 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. In particular, we build an employer-employee-crime matched database that follows individuals and firms over a decade in Medellin, Colombia. We combine these data with unique administrative information on crimes of family members, credit histories, and geolocations to identify channels underlying the elasticity between job loss and crime.

We find a loss in individuals' earnings that lasts up to five years after their firm massively lays workers off, the longest horizon our data allow us to assess, suggesting there might be permanent negative effects on formal labor earnings. Additionally, individuals experience an increase in the probability of being arrested the year of the event, and the year after. Although the effects on earnings are observed in both male and female samples, the increases in arrest rates only took place among males, and the effects are concentrated in property crimes (robbery, thefts, etc.). In figure A2 we document the cumulative effects of such layoff events on arrest rates, showing stark patterns across both gender and access to credit.

Our evidence indicates that financial constraints and economic sectors with less employment alternatives have played a key role on changes in arrest rates, enabling us to shed light on mechanisms which may or may not have been at play. For instance, our causal estimates suggest that effects are likely to be stronger among workers that did not have access to consumer credit before the shock. In fact, we did not find evidence that there was an effect on those individuals that already had formal credit. In the particular context of Colombia, access to credit markets depends on whether the individual has a formal job. After an unexpected job displacement, individuals who lost their job and did not previously have access to formal credit, may have a hard time looking for access to consumer credit in order to smooth the financial burden, leading to a viscous cycle between the lack of formal financial safety nets and arrest rates. In addition, consistent with predictions of occupational sorting models, workers in booming sectors with more employment alternatives exhibit weaker responses on arrest to mass layoffs.

Finally, our evidence suggests that the job displacement may have spillovers on arrest rates of young relatives. The income shock may decrease the household's income pushing other members of the family to find additional sources of income. In settings like Medellin where individuals not formally

employed have several opportunities to get involved in criminal activities, in particular, the adolescent and young adults, a fraction of relatives, especially those in young ages, might potentially have been drawn into organized crime. As such, ignoring spillovers on youth in the family underestimates the overall impacts of job losses on crime, as measured by arrest rates, for this generation and the next.

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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 (in 2009) and 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

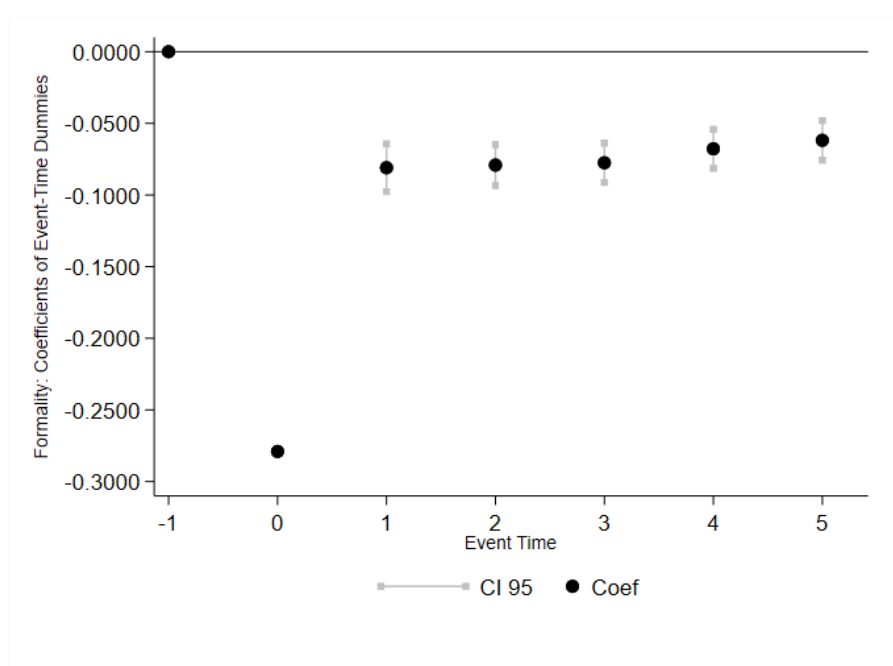
Notes: Standard errors are clustered at the individual and firm-level. The sample includes drug, property, violence, and other crimes. Table A2 lists δ_k from equation (1). Event time is measured in years. Arrested outcome is binary indicator: 1 if the event occurred at any point in the year, 0 otherwise.

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

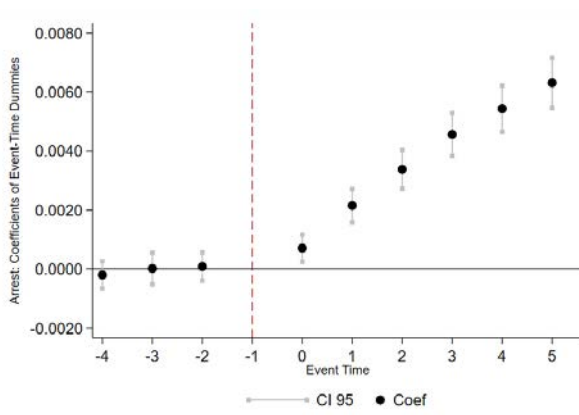
Notes: Standard errors are clustered at the neighborhood level. The regression includes *comuna* fixed effects and controls for the Sisben score (poverty index), education, socioeconomic strata, gender and age. The average minimum distance to a new banks is 3 kilometers. The dependent variable is the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

Figure A1: Event study estimates on Formal Employment

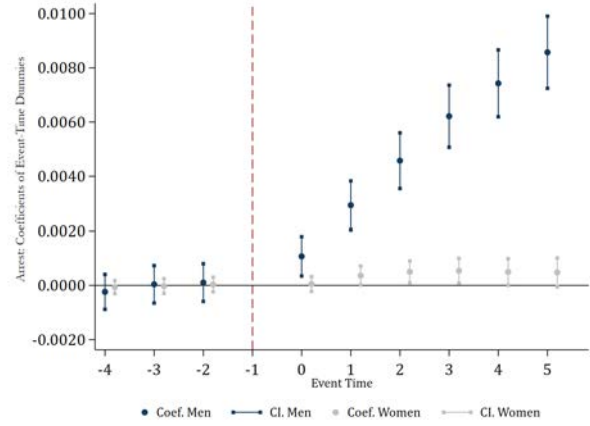


Notes: Figure A1 shows the effect of firms mass layoff event, five years after the event, on the probability of being formally employed for at least six months. We standardized the effect of the event to the share of individuals that experienced a layoff the year of the event, and to 0 the year before the event. 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 were separated during at least six months in 2010.

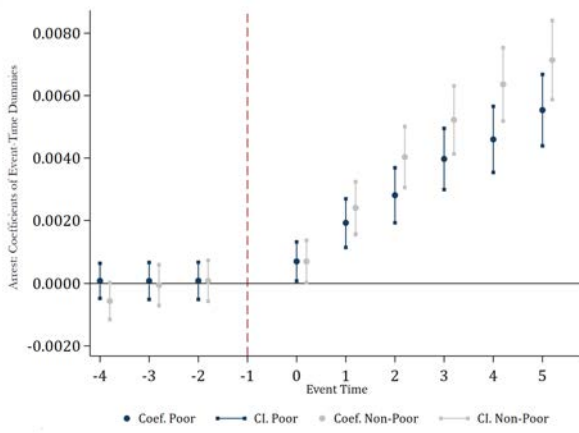
Figure A2: Effects of firm-level mass layoffs on cumulative arrest



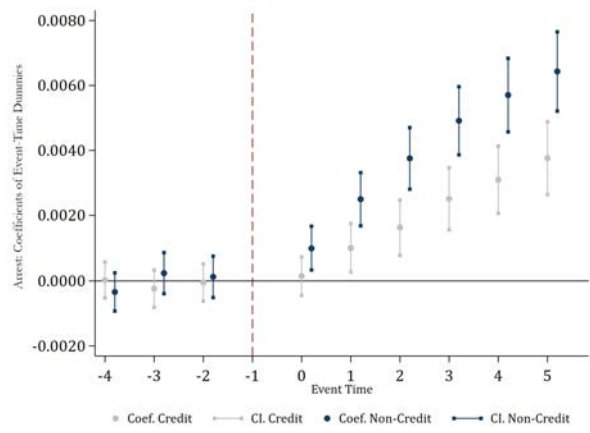
(a) Event study estimates on cumulative arrest



(b) Event study estimates on cumulative arrest by gender



(c) Event study estimates on cumulative arrest by poverty status



(d) Event study estimates on cumulative arrest by consumption 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 A2a 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 A2b to A2d show heterogeneous effects of mass layoff event on arrest by gender, poverty status and consumption credit. Poor is 1 when employee information is matched with poverty census. 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 credit : 10 years x 228368 individuals. Number of observations for non-credit: 10 years x 228728 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.

