# Formal Employment and Organized Crime: Regression Discontinuity Evidence from Colombia

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## Abstract

Canonical models of crime emphasize economic incentives. Yet, causal evidence of sorting into criminal occupations in response to individual-level variation in incentives is limited. We link administrative socioeconomic microdata with the universe of arrests in Medellín over a decade. We exploit exogenous variation in formal-sector employment around a socioeconomic-score cutoff, below which individuals receive benefits if not formally employed, to test whether a higher cost to formal-sector employment induces crime. Regression discontinuity estimates show this policy generated reductions in formal-sector employment and a corresponding spike in organized crime, but no effects on crimes of impulse or opportunity.

**Keywords:** *organized crime, informality, occupational choice, gangs, Medellín*
**JEL Codes:** K42, J46, J24

# 1 Introduction

Many countries, particularly across the developing world and in much of Latin America, are plagued by coincident high degrees of informality in the labor market and criminal activity, often controlled by organized enterprises (Arteaga, 2019; Blattman et al., 2017; Buonanno and Vargas, 2018; Chimeli and Soares, 2017; Dell et al., 2018; DiTella et al., 2010; DiTella and Schargrodsky, 2013; Dix-Carneiro et al., 2018; Sviatschi, 2018). However, the empirical evidence on whether the two phenomena are causally linked by way of occupational sorting decisions of individuals is limited. Classic models of criminal behavior contend that individuals rationally weigh the expected costs and benefits of engaging in criminal activity (Becker, 1968; Ehrlich, 1973). Here, economic incentives play an important role via alternatives to crime: primarily legitimate employment in the labor market. Recent studies have confirmed that crime deterrence can be effective in reducing criminal activity (DiTella and Schargrodsky, 2013, 2014). However, understanding the economic decision to engage in crime is important, as reducing crime through incapacitation can be less effective when the elasticity of individual supply to crime is high (Freeman, 1999).

We use rich administrative data between 2002 and 2013 from Medellín, Colombia to test the relationship between formal employment and participation in crime at the individual level. In this empirical context, where informal employment is common and criminal enterprise activity abounds, financially dissuading individuals from engaging in formal employment could drive some to organized crime as their most lucrative option in the informal sector. Exploiting a discontinuity in the cost of formal work in Colombia, we leverage individual-level variation to empirically illustrate the occupational choice between formal employment and participation in criminal enterprise.

The Colombian government provides health benefits to all residents that reside within a household that has a socio-economic score (known as the *Sisben* score) below a certain threshold. Those formally employed are automatically taxed a fraction of their wages to avail of comparable benefits. Formal employment of any member affects the family's eligibility for this program, raising the relative benefits to other forms of employment.[1] That is, the usual benefits to formal

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[1] Eligibility is determined at the family level, with the employment status and incomes of children under the

employment (e.g., higher wages, job security, legal protections) are at least partially offset by the increased cost of health care coverage for those below the cutoff who would be eligible for full coverage by the government if they were not formally employed. Near complete health care coverage in the population, despite costs representing large proportions of income for many households, reveals the importance of these incentives in this context.

Using a regression discontinuity design, we find that the policy induced a roughly 4 percentage point lower formal employment rate at the margin, consistent with estimates from previous studies.[2] These same individuals are more likely to be arrested for crimes associated with organized criminal activity. At the RD cutoff we find a 0.45 percentage point rise in gang-related violent crimes, a roughly 0.66 percentage point rise in gang-related property crimes, and a less precisely estimated 0.1 percentage point rise in gang-related drug crimes.[3]

Importantly, offenses less likely to be associated with economically motivated organizations, like rape and marijuana consumption, do not show significant increases at the cutoff, allowing us to rule out many alternative theories.[4] At the margin, the program raised the cost of being employed in the formal sector. High-crime environments like Medellín have an informal market that contains many lucrative "employment" opportunities with organized criminal enterprises (i.e., gangs). In fact, related studies have argued that profit-making criminal activity is fully controlled by these organized entities such that most, if not all, economically motivated criminal activity occurs under their oversight (Blattman et al., 2018). Indeed, additional results show that impacts on gang-related criminal arrests are strongest in neighborhoods known to have the highest gang opportunities at baseline. Our results suggest that increases in formal employment can lead to reductions in criminal activities linked to these organized entities. Our magnitudes are similar to the related literature, as we measure an economically meaningful 3.1% increase

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age of 25 living at home also determining eligibility. Accordingly, parents may have reason to discourage their children from joining the formal sector to avoid losing access to benefits. At the beginning of our sample period, the subsidized program covered nearly 60% of health services that the full program covered – this fraction increased consistently to eventually cover 100% of services.

[2] When evaluating the effect on the entire country using a different research design, Camacho et al. (2014) find that the program led to a 4 percentage point decrease in formal employment, consistent with the point estimate we obtain. Despite the reduction in formal employment, reported incomes are not significantly different at the cutoff suggesting a replacement with informal sources of economic activity.

[3] Additional results in which joint outcomes of non-formal employment *and* criminal arrests are studied confirm that those leaving formal work and those arrested are the same. The results for gang-related drug crimes are significant when we simultaneously measure both non-formal employment and arrests as an outcome.

[4] For instance, if social benefits induce risky behavior it should increase non-gang crimes as well.

in arrests for every 1 percentage point fall in formal employment.[5]

Even though models of criminal activity are based on individual behavior, we often test these models using aggregate area-based relationships like unemployment shocks (Agan and Makowsky, 2018; Bennett and Ouazad, 2018; Cornwell and Trumbull, 1994; Entorf, 2000; Foley, 2011; Fougere et al., 2009; Gould et al., 2002; Karin, 2005; Lin, 2008; Machin and Meghir, 2004; Raphael and Winter-Ember, 2001). Area-based relationships are meaningful and policy relevant as they inform how to broadly target crime deterrence strategies. Yet, variation at the individual level is likely to produce different estimates than those that rely on aggregate shocks.[6] In our work, we are interested in the individual-level decision to engage in criminal activity as it allows us to understand (and potentially address) some root causes of why youth choose a life of crime.

Some of the literature's best evidence of individual-level economic decisions related to crime comes from experiments that raise the human capital of individuals (Berk et al., 1980; Blattman and Annan, 2015; Bloom, 2006; Heller, 2014; Kemple et al., 1993; Schochet et al., 2008). We complement this evidence on how changes to human capital affect the returns to both standard employment and criminal activity, by examining the occupational choice between legitimate and illegitimate activity as relative incentives are changed. Many studies that attempt to examine individual-level occupational choices between legitimate and criminal activities rely on associations conditional on extremely rich sets of observables (Freeman, 1999; Grogger, 1998; Gronqvist, 2017; Lochner, 2004), as plausibly exogenous variation is challenging to find.[7]

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[5]In their review of the recent literature, Bennett and Ouazad (2018) show that prior studies usually find that 1 percentage point increase in unemployment rates are associated with a 3-7% increase in crime. In Section 8 we discuss why our elasticities may be on the lower side of estimates in prior work.

[6]For example, unemployment at the regional level reduces the returns to criminal activity (i.e., lowers the resources available to expropriate and is correlated with fewer potential victims in the area (Mustard, 2010)). General equilibrium effects in which a new stock of criminals may crowd-out others, and neighborhood and peer effects both within and across neighborhoods might affect the relationship between area-based employment and choices to engage in crime (Cullen et al., 2006; Dustmann and Damm, 2014; Ihlanfeldt, 2007; Kling et al., 2005, 2007). Fella and Gallipoli (2014) find that general equilibrium effects explain a substantial portion of the relationship between crime and schooling. Additionally, detection rates of crime outcomes may defer as local resources change. Lastly, economic activity and high-income individuals may leave areas with high or increasing crime (Cullen et al., 2005; Cullen and Levitt, 1999; Greenbaum and Tita, 2004), further affecting the association between crime and employment observed at the aggregate level.

[7]It may be difficult to account for unobservables that would determine both employment and crime. For example, factors like high discount rates determine both crime and job-search (DellaVigna and Paserman, 2005; Golsteyn et al., 2014), whereas childhood shocks and decisions may affect both adult employment and crime (Doyle, 2008, 2007; Lochner and Moretti, 2004; Sviatschi, 2018). Reverse causality leads to upward bias as employers are less likely to prefer individuals that may display attributes correlated to criminal behavior

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Lastly, many studies depend on self-reported crime that may under-measure the occurrence of criminal activity, or homicides and victim-based data which capture the likelihood of being a victim rather than a criminal (Freeman, 1999).

We overcome each of these issues raised by previous researchers in examining the relationship between formal employment and criminal activity in Medellín, Colombia. First, we link two sources of administrative data at the individual level: the universe of arrests and the pre-arrest socio-economic characteristics of citizens, overcoming measurement issues in self-reported criminal activity and aggregate area-based measures of crime. Administrative individual-level data allow us to leverage individual-level variation and focus on demographics more likely to be affected. Next, we exploit quasi-experimental variation in the relative cost of formal-sector employment (or relative benefits to informal employment) derived from a social benefits program that requires individuals to be outside the formal sector to be eligible. Rather than associations conditional on observables, we use exogenous variation in financial incentives to isolate the individual-level relationship between employment and crime. Last, our data allow us to distinguish between different types of crime and conduct falsification tests by comparing the impacts on crimes most likely associated with economically motivated criminal organizations to the impacts on other, more idiosyncratic crimes of impulse and opportunity.

Our contributions lie in validating economic models of occupational choice and criminal behavior (Becker, 1968; Ehrlich, 1973), leveraging *individual*-level variation in employment incentives and rich individual-level administrative data to establish a causal relationship between formal employment and crime. Such evidence has proven difficult to find in a literature that has mostly relied on aggregate shocks. Recent studies have highlighted how unemployment shocks, job loss and employment restrictions have lead to increases in criminal activity (Bennett and Ouazad, 2018; Pinotti, 2017; Rose, 2019). Our paper complements this small set of recent studies by testing occupational choice as a result of exogenous variation in exposure to a tax to formal wages.[8] In addition, we stress the importance of distinguishing between different types

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(Grogger, 1995; Kling, 2006; Lott, 1992). Unemployment rates can affect the number of victims even if there are no new criminals: employed persons may have resources that are targeted, or the unemployed may be in the crossfire or suffer substance abuse.

[8]We think our estimated elasticities could differ substantially as job losses and structurally imposed employment restrictions may additionally induce effects on depression, subsequent job search, social stigma etc.

of crime, as some are more likely to be associated with organized criminal enterprises (e.g., homicide) whereas others are more likely to be crimes of impulse, addiction or opportunity (e.g., rape and drug consumption). In doing so, we establish a falsification test to rule out alternative mechanisms that have little to do with occupational choice.

Finally, there are few studies in the developing world, as many look at the US, the UK or Scandinavian countries from which data are more readily available (Bhuller et al., 2018; Dustmann and Damm, 2014; Freeman, 1999). In contrast, we study a high-crime environment similar to most parts of the developing world and, in particular, a city with a significant presence of organized crime, which has been shown to have particularly detrimental effects on growth and development (Alesina et al., 2017; Melnikov et al., 2019), and broader consequences for child development (Arteaga, 2019).[9] We build upon recent evidence from important high-crime environments in Latin America that leverages area-based variation from trade-shocks (Dell et al., 2018; Dix-Carneiro et al., 2018), or district-level unemployment (Buonanno and Vargas, 2018; Cortes et al., 2016).[10] Finally, we highlight an unintended, adverse consequence of welfare policy, contributing to previous work on the interaction between public sector interventions and crime (Chimeli and Soares, 2017; Chioda et al., 2016; Doyle, 2008; Yang, 2008).[11]

# 2 Background

## 2.1 Crime in Medellín

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*).

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[9]More than one in five young men in our sample were arrested. Our high crime context is similar to many parts of the developing world, including other parts of Latin America. In the developed world, the US has high incarceration rates (Kearney et al., 2014) but relatively lower crime rates.

[10]This literature also shows that local trade shocks also affect public goods provisioning, inequality and policing, suggesting that the general equilibrium effects may be substantial (Dix-Carneiro et al., 2018; Feler and Senses, 2017). Indeed, Dix-Carneiro et al. (2018) extensively discuss the various channels through which such aggregate shocks may affect crime.

[11]Related work studies how elected officials may engage in criminal activity (Ferraz and Finan, 2008, 2011; Olken and Pande, 2012), and how multiple prices for public programs lead to distortions (Barnwal, 2018).

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 to care for the entire production chain. 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 motivations behind joining organized 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.[12] Many demobilized militias continue to be involved in crimes like extortion and trafficking, given their experience with using guns and avoiding police (Rozema, 2018).

There are two features of the homicide rate that are pertinent for our analysis. First, it is predominantly male. In 2002, the first year of our data, the male homicide rate was 184 per 100,000 whereas the female homicide rate was about 12, less than one-tenth the rate of males. 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%. Second, youth, between 13 and 26 years, are far more likely to be involved as victims or assailants than other age groups. Approximately, 63% of first arrests are between 13 and 26. Younger individuals are more likely to be engaged in drug trafficking and consumption, whereas slightly older individuals are involved in violent crimes (homicides, extortions, and kidnapping), and the oldest still are involved in property crime. Irrespective of type of crime, however, arrest rates peak within the 13 to 26 age window depicted in Figure A2.

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 supply, and protection by the *razones* at the top of the hierarchy. They confirm that gangs are mainly profit-seeking organizations, earning money from protection, coercive services such as debt collection and drug sales. Anthropological studies and in person interviews show that economic incentives (such as the focus of our study) drive young men in Medellín to join organized crime (Baird, 2011). As many respondents highlight, the reason to

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[12]*Operacion Orion*, followed by the demobilization of paramilitary forces led to a sharp decline in homicides, as the military clamped down on urban militias (Medina and Tamayo, 2011).

join crime is mostly "economic" or for a profitable career.[13] Knowing this, paramilitaries and gangs actively recruit idle youth that are *amurrao* (local slang, literally: 'sitting on the wall') and without a formal sector job.

An 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 formal sector 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, yet those with formal sector jobs pay extortion fees to gangs.[14] Indeed, the options are often presented as an occupational choice: "*are you gonna work [for the gang] or do a normal job?*"[15]

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." These anecdotes are consistent with our hypothesis: higher costs of formal sector jobs (or better benefits for informal work) discourage youth from joining the formal sector, which in turn leads them to be recruited by gangs.[16]

For our sample of young men in the bandwidth of analysis, 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, but are representative of cities in Latin America. 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 high-crime regions in many parts of the developing world, and especially Latin America (Dell et al., 2018).

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[13]See *interview with Gato, p264* and *interview with Armando, p197.*

[14]See *interview with El Peludo, p184.*

[15] See *interview with Notes, p193*

[16]During the demobilization of militias in the mid-2000s, many were encouraged to join the formal sector, given identity cards and medical cards (Rozema, 2018). Yet, this disparity in costs across social benefit regimes, discourages formal sector re-integration.

## 2.2 Access to Health Benefits

In 1993, *Law 100* established two tiers of health insurance: the Contributive Regime (CR) and the Subsidized Regime (SR). The CR covers formal workers with a comprehensive set of health services that includes nearly all of the most common illnesses. The SR covers the families of the poorest informal workers and unemployed with a plan that initially covered fewer illnesses than CR, but was expanded to cover the same benefits.[17] Formal workers and employers fund workers' insurance premiums for coverage by the CR. Between the 1993 reform and 1998, insurance coverage under both grew from 20% to 60%. In 2005, SR was expanded and takeup reached 1.1 million people in Medellín, alone. By 2013, 96% of Colombians were covered, with more than half qualifying under SR (Lamprea and Garcia, 2016).

Colombian employers are required by law to enroll all their employees in a Health Promoting Company, which gives them access to health insurance under the CR. Self-employed workers are allowed to enroll in the CR themselves by paying a monthly fixed amount based on a percentage of the monthly minimum wage. Unemployed or inactive individuals (and informal workers) can either get health insurance as the self-employed do through the CR, or apply for access to the SR. Individuals not covered by the CR or the SR use public hospitals, and are charged fees for both medicines and services.

Formal sector workers make up about 54% of the urban labor force and pay 4% of their monthly wage for enrollment in the CR, while the employer pays the other 8.5%.[18] This implies that effectively employees may bear a burden somewhere between 4 and 12.5% of their monthly wage depending on their bargaining power. Formal workers pay 1.5% of their salary to cover informal workers in SR.[19] Over and above this, formal workers have to pay 4% of their wage for their pensions, and also bear other non-wage labor costs like old-age and disability insurance. These costs rose by between 10.5% and 11.5% after the 1993 reforms, with strong evidence that such costs discourage formal sector employment (Kugler and Kugler, 2009).

To target the SR, roughly 70 percent of the poorest households in the country were in-

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[17]In 2008, the Constitutional Court ordered that the basket of health services covered under SR become equal to that of the CR. However, the reform did not come into effect until July 2012

[18]Employers' contribution was 8% between 1993 (Law 100) and 2007. On that date it was increased to 8.5% (Law 1122). This contribution was eliminated in 2012 (Law 1607) for incomes up to 10 times minimum wages.

[19]Authorities initially expected the formal sector population to rise and cover costs for SR. But the SR grew faster than the CR population, in part due to the lucrative nature of the SR (Lamprea and Garcia, 2016).

terviewed between 1994 and 2003, and a welfare index (*Sisben* score) was calculated using a confidential formula based on respondent characteristics, incomes and assets, disability, education, and housing. Only households with a *Sisben* score below a certain cutoff and not formally employed were eligible to become beneficiaries of the SR.[20] Other public programs use the *Sisben* score, but the SR *Sisben* cutoff did not coincide with other major interventions, at the eligibility cutoff of *Sisben* in the 2000s.[21] The SR health program is by far the largest that has eligibility determined by the *Sisben* score.[22]

## 2.3  Incentives for Informality

Between 2005 and 2013, informal workers made up about 46% of the urban labor force in Medellin. Among informal workers, around 60% were own account workers, 20.5% were private sector employees, 7.8% were domestic workers, 7.7% self-employed workers, and the rest were laborers, family workers, and unpaid workers. Finally, 31.5% of the workers had primary education, 51.8% secondary and 16.7% had some college education.

Effectively, financial incentives embodied in the health coverage options switch from potentially promoting formal employment above the cutoff due to a partial defrayal of the costs of healthcare by the employer, to strongly discouraging formal employment below the cutoff due to a significantly more enticing full defrayal of these large costs by the government for individuals who are not formally employed. Near complete health care coverage in the population despite costs representing large proportions of income, reveals the importance of these incentives. That this policy led to a fall in formal-sector employment has been documented in both the academic literature and public discourse. The Minister of Social Protection, in a news article in *Presidencia de la Republica* (February, 2006), claimed that the people's valuation of SR was so high that it discouraged formal employment. Studying the effects on the entire country, Camacho et al. (2014) use individual-level data and control for both region and time fixed effects to show that informal employment increased by 4 percentage points as SR was

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[20]Households keep their Sisben score until it is updated by the government (expected to take place about every five years). In this case, the government updates the Sisben survey and score for the entire country.

[21]See www.sisben.gov.co/Paginas/Noticias/Puntos-de-corte.aspx for programs by *Sisben 3* cutoff. While the *Sisben* cutoff for SR enrollment may differ across counties, there is only one cutoff for the entirety of Medellín,.

[22]The share of the SR in the total budget accounts to nearly 2% of the GDP, while all other programs sought to reduce poverty represent less than 0.4% of GDP.

rolled out across the country. This is a combination of workers dropping out of the formal sector, but also fewer youth joining the formal sector over time (Lamprea and Garcia, 2016). Recognizing these adverse effects on formal employment, the government drastically lowered the costs of being enrolled in CR right at the end of our study period, when Law 1607 was enacted. This led to a significant increase in formal sector employment (Bernal et al., 2017; Fernández and Villar, 2017; Kugler et al., 2017; Morales and Medina, 2017).

Since the *Sisben* score and targeting is at the family level rather than individual level, older family members may discourage youth within the family from joining the formal labor force for fear of losing access to benefits.[23] Large families stay informal in the hope of retaining benefits (Joumard and Londono, 2013).[24] Indeed, Santamaria et al. (2008) find that half of all SR recipients indicated that they would not switch to formal employment as it would mean losing benefits. These effects are not restricted to men, as women's formal-sector participation also decreased in response to SR (Gaviria et al., 2007). Yet, we find that dis-employment effects on men are about four times larger than on women, consistent with the hypothesis that men have a lucrative alternative outside the formal sector: organized crime.

We leverage the fact that the costs of accessing these benefits change discontinuously at the *Sisben* cutoff. Indeed, as most individuals are covered by one healthcare regime or the other,[25] almost everyone has similar access to benefits on either side of the cutoff. Yet, on one side of the cutoff these benefits are free only if you are not formally employed. The primary driving variation, therefore, is that being outside the formal sector allows you to not pay for benefits on one side of the cutoff. Since by the end of the period almost everyone has healthcare (under either one of the two regimes) and the benefits are similar across the two regimes, there are no discontinuous changes to health benefits at the cutoff.

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[23]By Article 21, Decree 2353 of 205, the *Sisben* score is determined at the family level.

[24]Similarly interviews in Baird (2011) highlight how being involved in crime can sometimes be a 'family decision' (chapter 6).

[25]By 2013 the coverage is 96% (Lamprea and Garcia, 2016).

# 3    Data

Administrative data allow us to identify the relationship between the costs of formal employment and crime. We do not need to rely on self-reported or aggregate victim counts. As our data is at the individual-level we isolate vulnerable demographics (young men), and test both employment outcomes and crime. Additionally, detailed information on the types of crime allow us to isolate mechanisms.

We combine two sources of data at the individual level using national identification numbers and dates of birth. One source is from successive *Sisben* surveys of the Medellín population for three different years: 2002 (baseline *Sisben I*), 2005 (*Sisben II*) and 2009-2010 (*Sisben III*). The *Sisben* dataset consists of cross sections from censuses of the poor, and we match household records across the three waves.[26] The second source is the census of individuals arrested between 2002-13 for each crime, whether or not they were convicted, from the Judicial Police Sectional of the National Police Department.

Our measure of criminal activity is arrests, rather than self-reported crime, and we acknowledge that either measure has its tradeoffs. We follow the literature and 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.[27] Indeed, 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). Accordingly, subsequent criminal behavior is outside the scope of this study.

For similar reasons, we follow recent studies (Gronqvist, 2017; Kling et al., 2005) in focusing on young men in our analysis. Our primary sample is between 21 and 26 years old in the last year of our arrest data, or between 13 and 26 for the entire period (2005-2013) of study, capturing more than 63% of first arrests (as shown in Figure A2). Of the individuals arrested more than once during the observation period, 40% are first arrested before the age of 27. At the same

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[26]Municipalities survey a census of people living in the three poorest socioeconomic strata. In low income municipalities, the survey is a census of the whole population, while in larger cities it amounts to 65-80% of the population.

[27]Our results are robust to including repeat arrests.

time, while incarcerated, individuals would not be able to be arrested for additional crimes and would, therefore, have lower measured propensities to be engaged in new criminal activity. Older individuals may have been arrested in their youth (or currently still be incarcerated) but as our crime data only begins in the early 2000s, we do not have their entire criminal history, and would miss their youth arrest. As such, we exclude older men. Focusing on ages when arrest rates peak reduces these concerns regarding the measurement of criminality, and allows us to emphasize the period when young men first make choices between crime and other jobs in Medellín (Doyle, 2016).

Figure A1 describes the timeline of our data. We use the 2002 *Sisben* as our baseline to create our running variable and predict eligibility for SR.[28] We test for SR enrollment in the 2005 *Sisben*, and for employment status and incomes in the 2009 *Sisben*. We then follow the criminal histories of young men aged 21 to 26 in 2013, between 2005 (after we have a measure of SR enrollment from the second *Sisben*) and 2013. Even though we do not have a panel of formal sector work that follows the census of poor individuals in every year (along with their identification numbers), we do believe that this database, to the best of our knowledge, is one of the most comprehensive data exercises in such contexts.

In Appendix Table 1 presents the 2002 baseline summary statistics of the complete *Sisben* survey and for the subsample of males only.[29] The arrests data include a detailed description of the person arrested (national identification number and date of birth), the type of crime (e.g., homicide, rape, motor vehicle theft, etc.), the precise article associated with the crime in the penal code, the date of arrest, the location of arrest, and a police generated flag for whether the arresting officer knew the perpetrator to be gang affiliated. We classify the crimes into three categories – violent, property, and drug crimes – based on the US Bureau of Justice Statistics' classifications in the Sourcebook of Criminal Justice Statistics (BJS, 1994). If an individual was first arrested for violent crime and later for property crime, they show up once as an arrest for violent crime.

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[28]The formula to compute the Sisben score and the eligibility cutoff varies across the waves (I, II, and III).

[29]The SR status is established based on the previously computed Sisben score, based on the semi-decadal Sisben municipality census of the 70% of the poorest population. After a Sisben survey, it takes around one or two years to get the new Sisben score and whether the household becomes eligible. This is why the correct running variable to use is the lagged Sisben score.

Table 1: Summary Statistics in 2002

| Variable | Complete Sample | | Males | |
| --- | --- | --- | --- | --- |
| | Mean | Std. Dev. | Mean | Std. Dev. |
| *Individual Characteristics* | | | | |
| Male | 0.490 | 0.500 | 1.000 | 0.000 |
| Subsidized Regime | 0.319 | 0.466 | 0.312 | 0.463 |
| Contributive Regime | 0.228 | 0.420 | 0.222 | 0.416 |
| Age 10-15 | 0.105 | 0.306 | 0.109 | 0.311 |
| Age 15-20 | 0.105 | 0.306 | 0.110 | 0.313 |
| Age 20-25 | 0.089 | 0.285 | 0.093 | 0.290 |
| Age 25-30 | 0.068 | 0.251 | 0.068 | 0.251 |
| Ever Arrested | 0.062 | 0.242 | 0.114 | 0.318 |
| | | | | |
| *Household Head (HH) Characteristics* | | | | |
| Female | 0.387 | 0.487 | 0.308 | 0.462 |
| Employed | 0.628 | 0.483 | 0.643 | 0.479 |
| Unemployed | 0.106 | 0.308 | 0.107 | 0.309 |
| Married | 0.345 | 0.475 | 0.377 | 0.485 |
| Attending School | 0.009 | 0.097 | 0.008 | 0.089 |
| Has CR | 0.207 | 0.405 | 0.207 | 0.405 |
| Age | 43.237 | 14.302 | 43.869 | 14.159 |
| Years of Education | 4.542 | 2.451 | 4.480 | 2.454 |
| Owns House | 0.314 | 0.464 | 0.327 | 0.469 |
| Sisben Stratum 1 | 0.271 | 0.444 | 0.273 | 0.446 |
| Sisben Stratrum 2 | 0.620 | 0.485 | 0.620 | 0.485 |
| Sisben score | 45.707 | 9.901 | 45.716 | 9.908 |
| Number of members in household | 4.090 | 1.709 | 4.215 | 1.709 |
| | | | | |
| N | 1,161,446 | | 568,923 | |

Summary tabulations using *Sisben* I survey, conducted in the year 2002, and police arrests data.

Next, we divide crimes into crimes that would be more associated with an occupational choice. In Medellín, this implies being associated with a gang. The advantage of this additional classification is that we can test whether the decision to engage in crime is merely about income generation (e.g., engaging in petty theft property crime) or an occupational choice (gang crimes). While both are related, and consistent with the broader Becker (1968) hypothesis, we find that gang-crime arrests increase, even as non-gang income generating property crimes do not (and neither do non gang violent crimes of impulse or passion).

We worked closely with senior police officials in Medellín to divide our crimes into gang-related and non gang-related crimes. Police officials inform us that the best way to classify

arrests as gang related are along two dimensions: (1) the crime, and (2) the location. Fortunately, for about 30% of our data, the police used a system that flagged the arrest with whether the individual was known gang affiliate or not, and information on which gang the individual belonged to. This gang affiliation was based on police intelligence. As the gang-flag system was not available for the entirety of the period, we classify a crime as 'gang-related' if more than 30% of recorded arrests had the gang flag.[30] As a result, for example, we classify homicides as violent gang-related, and rape or domestic violence as violent non gang-related.[31]

In robustness checks, we use a method that relies on the association between these crimes and historically high-gang neighborhoods. In this alternative definition, we classify those crimes as gang-related if they are disproportionately more likely (above the median) to list any of these high-gang neighborhoods as a location of arrest. While these two methods are not perfect, the robustness to alternative definitions gives us solace, and to the best of our knowledge, such a data exercise has not been conducted in such a comprehensive manner before.[32]

In Appendix Table A1, we categorize the 25 (of 103) most prevalent crimes under each classification method. These data-driven methods line up with our priors on types of crime: homicides, motor vehicle theft, extortion, kidnapping, break-ins, and the manufacturing, delivery and trafficking of drugs fall under organized crimes. The remaining crimes are often thought of as crimes of impulse or opportunity (like rape, simple assault, and drug consumption). Indeed, we can distinguish between minute details – such as trafficking cocaine (gang) vs consuming drugs (non gang). The advantage of these classification approaches is that they are purely data driven. Additionally, they may speak to the types of activities that gangs in Medellín engage in: for example, they are more likely to engage in car theft than identity theft.

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[30]Our results are robust to using the median as the cutoff.

[31]Gang-rape gets classified as a gang crime. Our police contacts also describe how burglars are imbibed into gangs based on their work territories and would find it difficult to be a burglar without being a part of the gang.

[32]Additionally, using the crime-level classification (rather than the individual flags) of gang-related crimes protects us against any police biases against specific individuals, or their characteristics (such as insurance status or who the police have more intelligence on).

# 4  Enrollment in the Subsidized Regime (SR)

As only households in the two lowest levels of *Sisben* I (2002), a score below 47, could qualify for the SR, we compare households on either side of the cutoff to identify the effect of SR eligibility. First, we verify if there is a discontinuity in the probability of SR enrollment at the cutoff. Second, we examine how the likelihood of being in the formal sector changes at the cutoff. Last, we examine the effect on different types of criminal activity.

In following RD conventions, we normalize the *Sisben* score so that treated units are individuals with positive values of our new score. Figure 1 presents the first stage: the discontinuity in the probability of SR enrollment using the optimal binning procedure found in Calonico et al. (2014a). The probability of enrollment discontinuously increases by around 26 percentage points.[33] Not all eligible persons enroll in SR, as formal sector jobs may be valuable to some, but enrollment still jumps substantially to 42% at the cutoff.

Figure 1: Discontinuity in the Probability of SR enrollment at Cutoff.
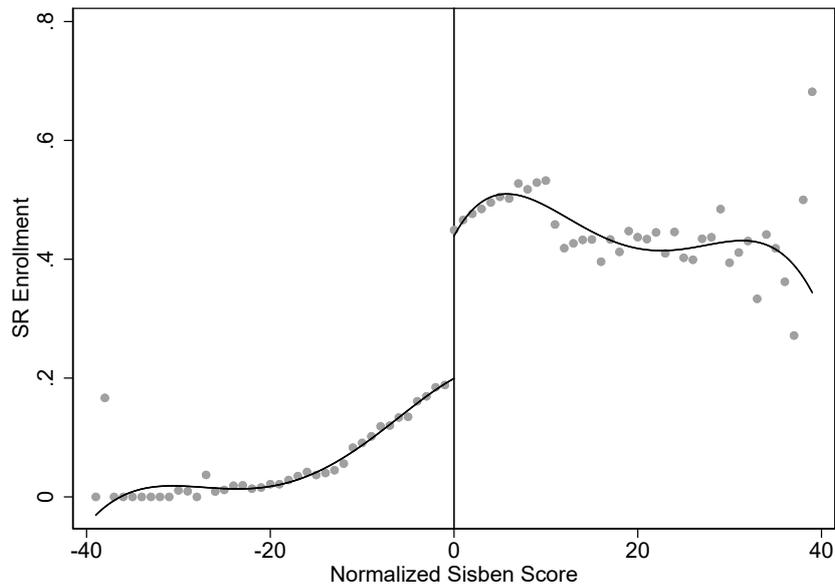


SR enrollment is probability of being enrolled in the subsidized regime in 2005. RD Graph using optimal binning procedure discussed in Calonico et al. (2014a). Normalized Sisben (2002) score on horizontal axis centered around cutoff. Higher values represent low scores (higher poverty).

For two-staged least squares (2SLS) exercises we follow a fuzzy regression discontinuity design, where our running variable is the 2002 *Sisben* score. We use both parametric and non-

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[33]Around 20% of households that have a high 2002 *Sisben* also avail of SR in 2005, as a fraction of households became eligible under a smaller 1998 *Sisben* survey, and the government allows them to keep their benefits for some time after they graduate out of eligibility.

parametric approaches to estimate the effect of SR eligibility at the cutoff. For the parametric approach we follow Hahn et al. (2001), where we instrument enrollment in the SR with the eligibility indicator $1\,[s_i < 47]$, and estimate the following equation as our first stage:

$$SR_{i,n} = \alpha + \alpha_1 1\,[s_{i,n} < 47] + X'_{i,n}\alpha_2 + A_i\,(s_{i,n})\,\alpha_3 + \mu_n + \varepsilon_{i,n}\,, \tag{1}$$

where $A_i$ is a vector of smooth polynomial functions of the *Sisben* score of each individual, $s_{i,n}$. In robustness checks, we also estimate models conditioning on demographics and other baseline characteristics. Here $X_{i,n}$ is a vector of demographic characteristics for individual $i$ living in neighborhood $n$. $\mu_n$ corresponds to neighborhood fixed effects for the 249 neighborhoods.[34]

An important issue in practice is the selection of the smoothing parameter. We use local regressions to estimate the discontinuity in outcomes at the cutoff point. In particular, we estimate local polynomial regressions conducted with a rectangular kernel and employing the optimal data-driven procedure suggested by Calonico et al. (2014b). We use two different optimal bandwidth procedures: the Imbens and Kalyanaraman (2012) method and the Calonico et al. (2014b) bandwidth. The optimal bandwidths from the different procedures lie between 5.5 and 6.2 points, on the 100-point *Sisben* I scale. We present our results for multiple bandwidths to highlight the robust nature of our estimates, varying them from below the optimal bandwidths to larger bandwidths. Specifically, we check for coefficient stability for results spanning these bandwidths ranging between 4 and 10 points around the cutoff. Varying the size of the bandwidth and the polynomial order do not affect the results.

Our first stage results are shown in Table 2, displaying the 26 percentage point increase in SR enrollment shown in Figure 1. As we vary the bandwidths from 4 through 10 the coefficient is stable and both economically and statistically significant. The table also shows that the standard IV F-test suggests a strong instrument, and for our remaining outcomes we conduct two-staged least squares analyses using this is as our first stage.

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[34]We include controls in robustness checks, where we control for various characteristics of the household head in 2002, the baseline year. These controls include an indicator for female-headed households, employment status, years of education, marital status, attendance to any academic institute, year-of-birth fixed effects, socioeconomic strata of the household, and home ownership.

Table 2: SR Enrollment at Sisben Cutoff (First Stage)

| Variables | Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|---|
| Dependent Variable: Enrolled in SR (First Stage) | | | | |
| Below Sisben Cutoff | | 0.260*** | 0.260*** | 0.269*** |
| | | (0.0138) | (0.0132) | (0.0110) |
| F-stat of IV | | 354.97 | 387.97 | 598.02 |
| Number of observations | | 181,132 | 246,974 | 340,581 |
| Sample mean (in bandwidth) | | | | 0.36 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Coefficient of indicator of being below *Sisben* cutoff, with linear controls for 2002 *Sisben* scores that vary flexibly at the cutoff. SR enrollment as measured in the 2005 *Sisben* survey. Standard errors clustered at the *comuna* level.

# 5 Impacts on Formal Employment and Reported Income

We test the simple hypothesis that the SR conditions disincentivized formal-sector employment and led to an increase in organized-crime activities. We first reproduce a well-established result and show that the program has a negative effect on formal employment (Camacho et al., 2014; Gaviria et al., 2007; Joumard and Londono, 2013; Santamaria et al., 2008). We exploit the discontinuity in enrollment rates at the cutoff, by using the eligibility indicator as an instrument for enrollment status to identify the effect of SR on formal employment and income. Here $Emp_{i,n}$ is 1 if the individual $i$ from neighborhood $n$ was formally employed. In robustness checks we include demographic controls in $X_{i,n}$, and neighborhood fixed effects $\mu_n$. We show the reduced form relationship between employment and being above the RD cutoff:

$$Emp_{i,n} = \gamma_0 + \gamma_1 1\left[s_{i,n} < 47\right] + X'_{i,n}\gamma_2 + A_i\left(s_{i,n}\right)\gamma_3 + \mu_n + \varepsilon_{i,n} ,$$

We then instrument for SR enrollment, where $S\hat{R}_{i,n}$ is the predicted SR enrollment probability from the first stage estimated in Equation 1. The second stage is:

$$Emp_{i,n} = \beta_0 + \beta_1 S\hat{R}_{i,n} + X'_{i,n}\beta_2 + A_i\left(s_{i,n}\right)\beta_3 + \mu_n + \epsilon_{i,n} ,$$

Figure 2 captures the fall in formal sector employment at the cutoff, where formal employment is defined as a working individual making wage contributions to benefits as measured in

Figure 2: Discontinuity in Formal Employment (2009).
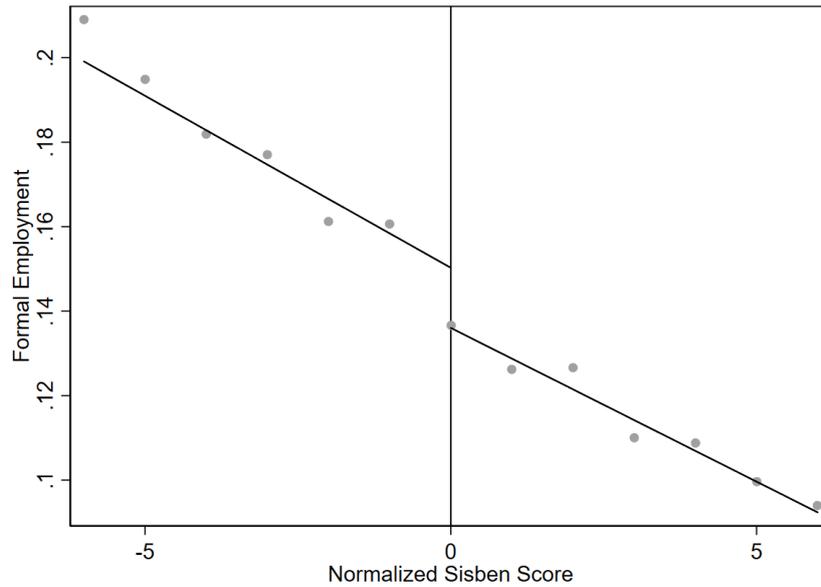


RD Graph using optimal binning procedure discussed in Calonico et al. (2014a). Formal employment based on measures in 2009
*Sisben* survey. Subsample of males. Normalized Sisben (2002) score on horizontal axis centered around cutoff. Higher values
represent low scores (higher poverty).

the 2009 *Sisben* III survey.[35] In our RD figures, we focus on a bandwidth of 6 around the cutoff

as it is the Calonico et al. (2014b) optimal bandwidth.

Table 3 presents the results for reported formal employment and incomes in the 2009 *Sisben*

survey. The table presents results for the reduced form change at the cutoff, and the two-staged

least squares (2SLS) effect of enrolling in SR. These results show that the health insurance

program had a negative impact of 4.1 percentage points (when using the optimal bandwidth)

on the probability of being employed in the formal sector in 2009.

Lower formal sector employment at the cutoff may be a combination of fewer youth joining

the formal sector as they enter working-age, lower transition rates out of informal work, and

higher transition probabilities out of formal work at the cutoff. As formal sector employment

affects SR enrollment for the entire family, these are often family decisions, where older family

members may discourage youth from joining the formal sector (Joumard and Londono, 2013).

This effect is larger for men than it is for women (Appendix Table A2), perhaps once again

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[35]While this is a somewhat conservative measure of formal employment, Colombian employees who pay
contributions to health insurance have been widely considered by the literature to be formal employees (See
Attanasio et al., 2017; Morales and Medina, 2017). The Sisben does not explicitly ask households whether
the worker is in the formal sector, which in any case, the response would be misreported underestimating the
formality rate.

Table 3: Reported Formal Employment and Income

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|
| **Panel A: Formal Employment in 2009 (Males)** | | | |
| Above Cutoff | -0.0147*** | -0.0111*** | -0.00845*** |
| Reduced Form | (0.00467) | (0.00280) | (0.00217) |
| Enrolled in SR | -0.0539*** | -0.0411*** | -0.0301*** |
| 2SLS | (0.0166) | (0.0103) | (0.00811) |
| Number of observations | 133,067 | 180,742 | 247,886 |
| Sample mean (only males in bandwidth for 2009) | | | 0.14 |
| **Panel B: Annual Household Income in 2009 (USD)** | | | |
| Above Cutoff | -3.837 | -3.805 | 2.347 |
| Reduced Form | (3.100) | (2.295) | (4.008) |
| Enrolled in SR | -6.481 | -3.042 | 30.49 |
| 2SLS | (9.163) | (8.842) | (27.83) |
| Number of observations | 46,797 | 63,457 | 87,510 |
| Sample mean (households in bandwidth for 2009) | | | 171.24 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. We use the *Sisben* survey of 2009 to construct both outcome variables. Formal employment for males only. The results for women are presented in Table A2. Tables report Two-Staged Least Squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. Regressions control linearly for the Sisben score, flexibly around the cutoff. We cluster standard errors by *comuna*. Household-level income reported in *pesos* and converted to USD using the average 2009 exchange rate. Sample means for males and households only in bandwidth for 2009.

highlighting that males have an outside option in organized crime.[36]

The impact on household-level income is statistically indistinguishable from zero and economically small ($30 per household annually). One caveat is that income is self-reported, and respondents may under-report assets and incomes in order to get a lower *Sisben* score. However, as respondents do not know the score formula, perfect manipulation is impossible (whether or not they are part of a gang), and therefore, as we show below, the density of respondents is smooth around the cutoff.

We may especially expect that incomes from illicit activities are under-reported rather than

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[36]Note, that we should not necessarily think of this result as a 'first-stage' on crime outcomes. Instead, crime and formal employment choices are jointly determined. Indeed, it is possible that, as we predict, the incentives lead individuals to leave the formal sector and join crime. After a few years in crime, an individual may wish to re-join the formal sector, but may be unable to do so given a criminal record.

over-reported, perhaps suggesting that poverty-based desperation is less likely to be driving criminal activity. We should be wary of reading too much into these self-reported income measures, but if anything they suggest that even as workers drop out of the formal sector they find other sources of income. Indeed, by revealed preference, they *choose* to drop out of the formal sector, and as such, should be better off. Yet, we wish to be extremely careful in stressing the crudeness of self-reported income measures.

Canonical crime models (Becker, 1968; Ehrlich, 1973) stress the role of both income and substitution effects when wages in one sector change. In analyses that focus on legitimate sector job-loss and unemployment shocks, the income and substitution effects may work to both increase criminal activity. Interestingly, in contrast, here any gains from the subsidy essentially lower the likelihood of criminal activity.

Together, the results of this section show that higher costs of formal employment discouraged youth from joining the formal sector. As health care coverage via either regime is almost universal, individuals on either side have similar benefits, but on one side are more likely to choose to be outside of the formal sector to avoid high costs of maintaining this coverage. The obvious question that this then raises is how this aversion to formal sector employment affects the likelihood of criminal activity.

# 6   Impacts on Crime

We next turn our attention to outcomes on crime. One important distinction with the formal employment results is that we only measure formal employment in one year, whereas we measure crime cumulatively pooled over a decade. We interpret the impacts on crime as causally related to the incentives to leave the formal sector.[37] We show both the reduced form and two-staged least squares estimates of impacts on crime. In the second stage, we use the eligibility indicator as an instrument for enrollment status to identify the effect of SR enrollment on crime. Here $crime_i$ is 1 if the individual $i$ was arrested between 2005 and 2013.[38]

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[37]Note that by the latter half of this period almost everyone had healthcare (under either one of the two regimes), and the benefits were similar. As such health benefits are not changing at the cutoff, only the incentives behind who pays for it changes.

[38]Even as we have crime data for many years, we have formal employment only recorded at one point of time in 2009. This poses challenges when trying to simultaneously measure changes in employment and crime. Yet

$$Crime_{i,n} = \beta_0 + \beta_1 S\hat{R}_{i,n} + X'_{i,n}\beta_2 + A_i\left(s_{i,n}\right)\beta_3 + \mu_n + \varepsilon_{i,n} ,$$

Our main results do not condition on other factors. In robustness checks, we control for various characteristics of the household head in 2002, the baseline year. These controls include an indicator for female-headed households, employment status, years of education, marital status, attendance to any academic institute, year-of-birth fixed effects, socioeconomic strata of the household,[39] home ownership, and neighborhood fixed effects. A literature on neighborhood effects and crime (Cullen et al., 2006; Dustmann and Damm, 2014) highlight the perils of using area-based relationships (like differences in unemployment rates) to study individual-level occupational choice, and re-iterates the strength of our approach.[40] Our results are unaffected by the inclusion of neighborhood fixed effects that absorb any neighborhood level characteristics (demographics, amenities, property values and police presence) that may affect crime rates. We cluster standard errors at the *comuna* level.

We present results for violent, property, and drug-related crimes, dividing each group between organized-crime related activities and crimes less likely to be associated with organized criminal entities. We choose the most conservative specification, where when looking at the impacts on violent crime, we exclude those whose first arrests were in property or drug crime, and do the same for each type of crime.[41] This is why our number of observations will differ by type of crime. As such, our outcome will be 1 if the person's first arrest was in violent crime, and 0 if they were never arrested in their youth. In robustness checks, we include the other types of crimes as 0s, and our results are more precisely estimated (see Appendix Table A8).

As discussed in the data section, at the point of arrest, the police record a flag if they suspect the arrested individual is involved with a gang or not. We calculate the propensity for being issued this flag for each type of crime, and divide crimes into two groups: high and low-propensity to be organized criminal activity. This data-driven method to group crimes

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our results are robust to doing so (Appendix Table A9).

[39]Urban areas in Colombia are split into six socioeconomic strata, used by authorities to spatially target social spending to neighborhoods.

[40]There may still be general equilibrium effects of the policy that affect the entire country, but since our variation is not driven by differences across neighborhoods, this is all netted out.

[41]Not doing so increases the precision of our estimates (Appendix Table A8).

produces intuitive classifications (Table A1).

We hypothesize that organized criminal activities are directly related to our implicit model of occupational choice across legitimate and illegitimate sectors, whereas non-gang-related crimes should be less affected by the opportunity cost of being in the formal sector and hence serve as a useful falsification test. We expect the effects on the latter group to be zero, as crimes of impulse and passion are less directly related to occupational choice.

As we elaborate in a later discussion, over and above a falsification test, the lack of effects on non-gang crimes also allow us to rule out alternative mechanisms. We do not classify crimes based on whether or not they are pecuniary as that captures crimes of desperation and necessity that arise out of poverty. Instead, we posit that the policy induced an *occupational choice* to work for a gang, and as such use organized crime as a basis for classification. Alternative mechanisms (such as riskier behavior when having insurance) may have weight if non-gang crimes rose as well, but the lack of effects on non-gang crimes allow us to rule them out.

## 6.1   Violent Crime

We first start with the probability of being arrested for violent criminal activities. Based on the police flags for gang-related activity, violent organized crimes include homicides, extortion, and kidnapping. Violent crimes less likely to be associated with an organized entity include domestic violence, rape and injuries. Figure 3 and Table 4 present the results.

Figure 3 shows the jump in violent gang-related crime arrests at the *Sisben* cutoff, concentrating on an optimal bandwidth of 6 points on the 100 point scale. In Table 4 we present the regression discontinuity results varying the bandwidth and specifications. The reduced form results (first row in each panel) show an increase in gang-related violent crime (Panel A), but no corresponding change in less gang-related violent crime (Panel B). Within a bandwidth of 10 points on the *Sisben* scale, and measuring arrests over a decade, these results amount to a 32% increase (or a 0.45 percentage point increase) in violent crime arrests from the mean around the cutoff. These magnitudes are both economically meaningful and similar to those from recent studies in other contexts (Pinotti, 2017).

Our 2SLS results (next two rows of each panel) show an economically and statistically

Table 4: Violent Crimes

| | Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|---|

**Panel A: More Gang-Related Violent Crimes**

| | | 4 | 6 | 10 |
|---|---|---|---|---|
| Above Cutoff | | 0.00722*** | 0.00649** | 0.00456** |
| Reduced Form | | (0.00236) | (0.00249) | (0.00164) |
| | | | | |
| Enrolled in SR | | 0.0257*** | 0.0231*** | 0.0158*** |
| No Covariates | | (0.00873) | (0.00838) | (0.00539) |
| | | | | |
| Enrolled in SR | | 0.0274*** | 0.0232** | 0.0149** |
| Including pre-treatment covariates | | (0.00950) | (0.00937) | (0.00583) |
| | | | | |
| Number of observations | | 18,052 | 24,272 | 33,027 |
| Sample mean (men 13-26 years old in bandwidth) | | | | 0.014 |
| Sample mean for those enrolled in SR and in high-gang comuna | | | | 0.020 |

**Panel B: Less Gang-Related Violent Crimes**

| | | 4 | 6 | 10 |
|---|---|---|---|---|
| Above Cutoff | | 0.00279 | 0.000988 | -0.000581 |
| Reduced Form | | (0.00454) | (0.00304) | (0.00326) |
| | | | | |
| Enrolled in SR | | 0.00994 | 0.00349 | -0.00201 |
| No Covariates | | (0.0158) | (0.0104) | (0.0110) |
| | | | | |
| Enrolled in SR | | 0.00791 | 0.00118 | -0.00322 |
| Including pre-treatment covariates | | (0.0168) | (0.0111) | (0.0125) |
| | | | | |
| Number of observations | | 18,419 | 24,768 | 33,702 |
| Sample mean (men 13-26 years old in bandwidth) | | | | 0.034 |
| Sample mean for those enrolled in SR and in high-gang comuna | | | | 0.039 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report reduced form and two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. The Sisben score is measured in 2002, and SR enrollment in 2005. We measure crime between 2005 and 2013. Regressions control linearly for the Sisben score, flexibly around the cutoff. We cluster standard errors by *comuna*. We consider only males between 21 to 26 years old in 2013. For regressions that have pre-treatment covariates, we include household characteristics, year of birth fixed effects, and neighborhood fixed effects. The sample excludes anybody whose first arrest was a property or drug crime (Appendix Table A8 includes these observations as a robustness).

Figure 3: Gang-Related Violent Crimes
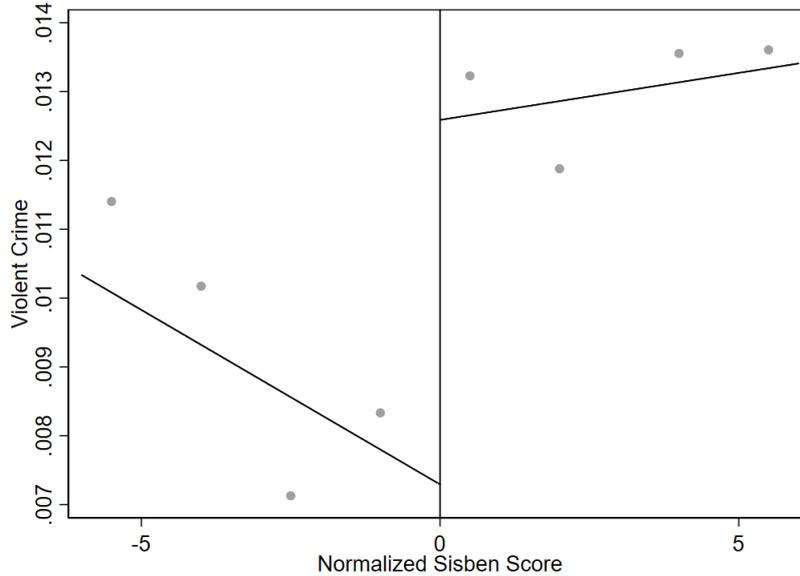
RD Graph using optimal binning procedure discussed in Calonico et al. (2014a). Normalized Sisben (2002) score on horizontal axis centered around cutoff. Higher values represent low scores (higher poverty).

significant increase in the probability of gang-related violent arrests for individuals enrolled in SR. We do not find any meaningful effect on the arrest probability for non organized-crime related violence. A comparison of the various rows in each panel shows that the estimates are robust to including controls, whereas a comparison across columns shows the robustness to bandwidths. While we do not report t-tests for the difference between gang and less-gang related crimes, the coefficients are statistically significantly different for all bandwidths.

## 6.2 Property Crime

In Figure 4 and Table 5 we analyze the effects on property crimes. Based on police flags, we establish that gang-related property crimes include crimes like motor vehicle theft and break-ins to businesses and residences. Crimes like fraud and identify theft are classified as less gang-related. Once again, in the reduced form we see that gang-related property crimes increase, with little change to less gang-related property crimes. This estimate, over the entire decade, constitutes a 21% increase (or a 0.66 percentage point increase) from the mean around the cutoff within a bandwidth of 10 points.

In the 2SLS results, we also find an economically and statistically significant increase for gang related property crime arrests, and no strong effect for property crimes less associated

## Table 5: Property Crimes

| | Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|---|
| **Panel A: More Gang-Related Property Crimes** | | | | |
| Above Cutoff | | 0.0106** | 0.00930** | 0.00666* |
| Reduced Form | | (0.00387) | (0.00389) | (0.00350) |
| | | | | |
| Enrolled in SR | | 0.0380*** | 0.0331*** | 0.0232** |
| No Covariates | | (0.0123) | (0.0126) | (0.0113) |
| | | | | |
| Enrolled in SR | | 0.0408*** | 0.0341*** | 0.0240** |
| Including pre-treatment covariates | | (0.0139) | (0.0131) | (0.0108) |
| | | | | |
| Number of observations | | 18,426 | 24,740 | 33,625 |
| Sample mean (men 13-26 years old in bandwidth) | | | | 0.032 |
| Sample mean for those enrolled in SR and in high-gang comuna | | | | 0.040 |

**Panel B: Less Gang-Related Property Crimes**

| | Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|---|
| Above Cutoff | | -0.00263 | -0.00217 | -0.00205 |
| Reduced Form | | (0.00554) | (0.00425) | (0.00336) |
| | | | | |
| Enrolled in SR | | -0.00941 | -0.00772 | -0.00712 |
| No Covariates | | (0.0194) | (0.0149) | (0.0112) |
| | | | | |
| Enrolled in SR | | -0.0116 | -0.00872 | -0.00854 |
| Including pre-treatment covariates | | (0.0212) | (0.0156) | (0.0119) |
| | | | | |
| Number of observations | | 18,240 | 24,523 | 33,358 |
| Sample mean (men 13-26 years old in bandwidth) | | | | 0.024 |
| Sample mean for those enrolled in SR and in high-gang comuna | | | | 0.028 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report reduced form and two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. The Sisben score is measured in 2002, and SR enrollment in 2005. We measure crime between 2005 and 2013. Regressions control linearly for the Sisben score, flexibly around the cutoff. We cluster standard errors by *comuna*. We consider only males between 21 to 26 years old in 2013. For regressions that have pre-treatment covariates, we include household characteristics, year of birth fixed effects, and neighborhood fixed effects.The sample excludes anybody whose first arrest was a violent or drug crime (Appendix Table A8 includes these observations as a robustness).

Figure 4: Gang-Related Property Crimes
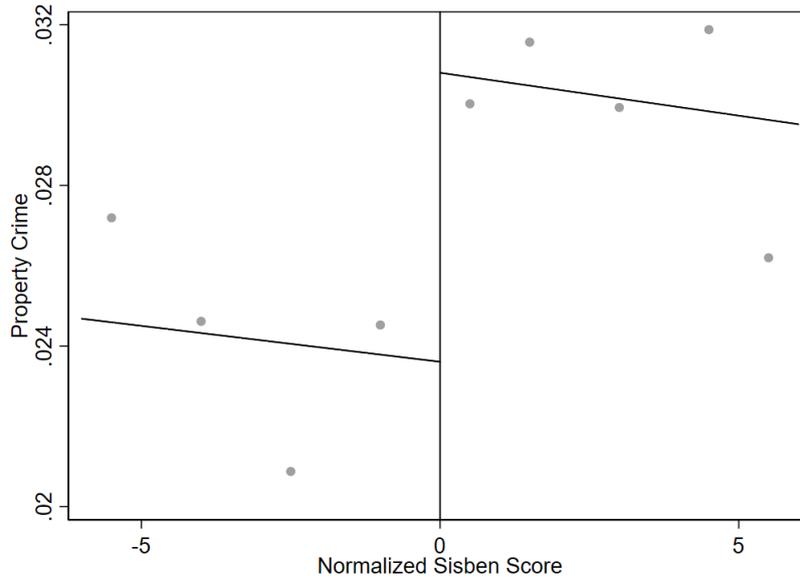


RD Graph using optimal binning procedure discussed in Calonico et al. (2014a). Normalized Sisben (2002) score on horizontal axis centered around cutoff. Higher values represent low scores (higher poverty).

with organized entities. Once again, our estimates are quite robust to the inclusion of control variables and the choice of the bandwidth, and our magnitudes are economically meaningful.

It is interesting to note that many of the less gang-related property crimes may also be income generating (even if they are not occupational choices), and as such it may be consistent with Becker (1968) if we found effects on them as well. Instead, we find that in this context, it is the decision to join a gang that seems to be the driving force. This is consistent with information the anthropological interviews, where gangs recruit idle youth, and joining a gang is very lucrative. The difference in coefficients between gang and less-gang related crimes are statistically significantly different for all bandwidths.

## 6.3 Drug Crime

The last type of crime involves the drug trade in Medellín. We analyze the impact on the probability to engage in drug-related crimes in Figure 5 and Table 6. Organized-crime related drug arrests include the manufacturing, distribution, and trafficking of hard drugs like cocaine and heroin. Drug crimes less likely to be related to organized entities include possession and consumption of drugs, as these are mostly indicative of personal recreational use, along with marijuana-related crimes.

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Table 6: Drug Crimes

| | Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|---|

**Panel A: More Gang-Related Drug Crimes**

| | | 4 | 6 | 10 |
|---|---|---|---|---|
| Above Cutoff | | 0.00799 | 0.00348 | 0.00133 |
| Reduced Form | | (0.00721) | (0.00492) | (0.00458) |
| | | | | |
| Enrolled in SR | | 0.0285 | 0.0124 | 0.00461 |
| No Covariates | | (0.0240) | (0.0169) | (0.0155) |
| | | | | |
| Enrolled in SR | | 0.0303 | 0.0135 | 0.00524 |
| Including pre-treatment covariates | | (0.0270) | (0.0180) | (0.0159) |
| | | | | |
| Number of observations | | 18,463 | 24,857 | 33,851 |
| Sample mean (men 13-26 years old in bandwidth) | | | | 0.038 |
| Sample mean for those enrolled in SR and in high-gang comuna | | | | 0.045 |

**Panel B: Less Gang-Related Drug Crimes**

| | | 4 | 6 | 10 |
|---|---|---|---|---|
| Above Cutoff | | -0.00976 | -0.0129 | -0.00788 |
| Reduced Form | | (0.00774) | (0.00798) | (0.00629) |
| | | | | |
| Enrolled in SR | | -0.0348 | -0.0458 | -0.0274 |
| No Covariates | | (0.0280) | (0.0293) | (0.0218) |
| | | | | |
| Enrolled in SR | | -0.0385 | -0.0501 | -0.0277 |
| Including pre-treatment covariates | | (0.0299) | (0.0329) | (0.0230) |
| | | | | |
| Number of observations | | 19,150 | 25,740 | 35,104 |
| Sample mean (men 13-26 years old in bandwidth) | | | | 0.073 |
| Sample mean for those enrolled in SR and in high-gang comuna | | | | 0.088 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report reduced form and two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. The Sisben score is measured in 2002, and SR enrollment in 2005. We measure crime between 2005 and 2013. Regressions control linearly for the Sisben score, flexibly around the cutoff. We cluster standard errors by *comuna*. We consider only males between 21 to 26 years old in 2013. For regressions that have pre-treatment covariates, we include household characteristics, year of birth fixed effects, and neighborhood fixed effects. The sample excludes anybody whose first arrest was a property or violent crime (Appendix Table A8 includes these observations as a robustness).

Figure 5: Gang-Related Drug Crimes
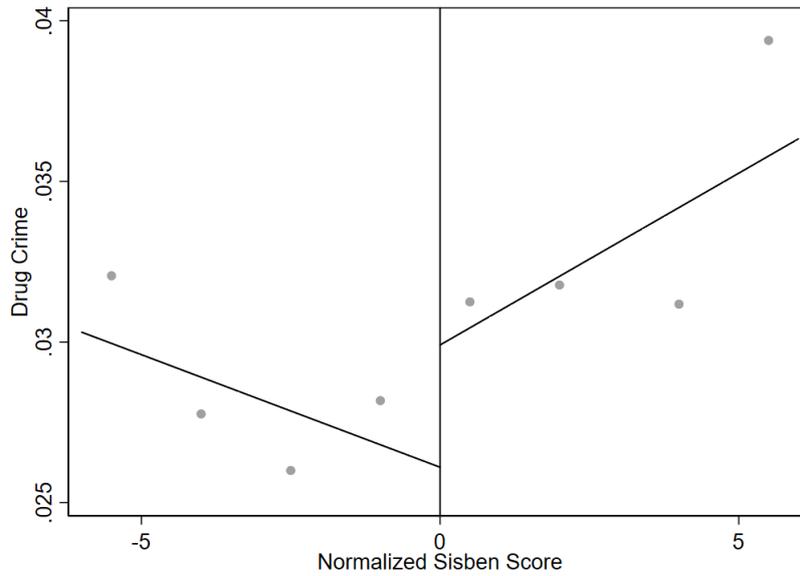


RD Graph using optimal binning procedure discussed in Calonico et al. (2014a). Normalized Sisben (2002) score on horizontal axis centered around cutoff. Higher values represent low scores (higher poverty).

In Figure 5, even though the discontinuity in drug crime arrests is visible, and there is a change in the slope of the relationship, the binned averages suggest a somewhat imprecise effect at the cutoff. In Table 6 the direction of effects are what we may expect, but our results are not precisely estimated.[42] One possibility for the lack of precision is in the measurement error associated with the classification of such crimes: the difficulty in classifying possession of drugs as consumption or trafficking likely introduces noise. Indeed, offenses related to the trafficking of marijuana are problematic as small amounts of personal possession were made legal during this period. While homicides, assaults and theft produce clear evidence of crimes, encouraging an arrest, drug crimes are often difficult to detect and record. Not having any evidence of a crime actually being committed (e.g., a victim) may also allow authorities to under-report, especially if cartels pressure authorities to do so.

In sum, our results indicate that the drop in formal employment as a result of the subsidized benefits for informal workers raised the likelihood of being arrested for gang-related violent and property crimes.[43] Even as health coverage is similar across the cutoff, the costs of being in

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[42]In Appendix Table A9 we explore an alternative specification where we look at arrests conditional on not being in the 2009 formal sector. Here we have enough precision to measure a significant increase in gang-related drug crimes at the cutoff.

[43]Note that we measure post-treatment formal employment once in the twelve year period, but crime every year. Nevertheless, our formal employment result is also confirmed by a long literature showing similar results.

the formal sector while maintaining coverage change discontinuously at the cutoff, prompting individuals to avoid the formal sector. As gang-crime is a lucrative option in the Medellín informal sector, we see a higher probability of arrests for gang-related crimes on one side of the cutoff.

The magnitudes of the estimated impacts are also economically meaningful. The pattern of results is similar but imprecise for drug crimes. Importantly, the results also show that non gang-related crimes of each type are not impacted by SR enrollment, ruling out many alternative mechanisms. In the following section, we investigate whether impacts are strongest in *comunas* that were historically associated with high organized crime activity as further evidence in support of our occupational choice interpretation.

# 7 Heterogeneity, Specification Tests and Robustness

## 7.1 Heterogeneity by Comuna: the Importance of Neighborhoods

Previous studies have emphasized that the opportunities in a neighborhood affect how easy it is to induce youth into crime (Kling et al., 2005). Understanding the heterogeneity by neighborhood helps us speak to much of the literature which relies on area-based variation. High crime neighborhoods may have more policing and higher detection rates that may lower the employment-crime elasticity, but may also have more opportunities to join a gang and thereby raise the elasticity.

We investigate if *comunas* with a high incidence of gangs demonstrate stronger impacts on gang-related arrests, at the RD cutoff. If the policy induces men to join organized crime, then we may expect that neighborhoods that have more such opportunities would have a larger impact. Figure A3 shows the spatial distribution of the locations where criminals were arrested in the act between 2005 and 2013, by type of crime.[44]

We select the five *comunas* with the highest number of gang members captured by the police, and create an indicator variable for whether individuals lived in these *comunas* in 2002,

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[44]The red circle specifies the downtown of the city. In our main results we already show specifications that include neighborhood fixed effects, and we cluster errors at spatial levels larger than neighborhoods. Our results are robust to clustering at smaller spatial levels, like the neighborhood.

Table 7: Heterogeneity by Comuna

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|
| **Panel A: Gang-Related Violent Crimes** | | | |
| Enrolled in SR | 0.0267*** | 0.0211** | 0.0150*** |
| | (0.00892) | (0.00914) | (0.00538) |
| Enrolled* Gang Comuna | -0.00152 | 0.0141*** | 0.00563 |
| | (0.00464) | (0.00376) | (0.00537) |
| F stat | 90.2 | 154.6 | 232.9 |
| Number of observations | 18,052 | 24,272 | 33,027 |
| **Panel B: Gang-Related Property Crimes** | | | |
| Enrolled in SR | 0.0344** | 0.0273** | 0.0190* |
| | (0.0134) | (0.0137) | (0.0115) |
| Enrolled* Gang Comuna | 0.0282 | 0.0364** | 0.0258** |
| | (0.0209) | (0.0167) | (0.0116) |
| F stat | 86.7 | 145.5 | 249.6 |
| Number of observations | 18,426 | 24,740 | 33,625 |
| **Panel C: Gang-Related Drug Crimes** | | | |
| Enrolled in SR | 0.0282 | 0.0131 | 0.00296 |
| | (0.0248) | (0.0177) | (0.0166) |
| Enrolled* Gang Comuna | 0.000310 | -0.00590 | 0.00690 |
| | (0.0136) | (0.0135) | (0.0129) |
| F stat | 96 | 149.1 | 201.7 |
| Number of observations | 18,463 | 24,857 | 33,851 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff and an interaction between high-gang comunas and being below the cutoff. The Sisben score is measure in 2002, SR enrollment in 2005, and crime outcomes are measured between 2005 and 2013. Regressions include comuna fixed effects and an interaction between high-gang comunas and indicators for SR enrollment. Regressions control linearly for the Sisben score, flexibly around the cutoff. We consider only males between 21 to 26 years old in 2013. We cluster errors by comuna. The mean arrest rate across all five gang comunas are 18%, which is also the mean arrest rate in non-gang comunas. See Appendix Table A5 for the less gang related crimes.

our baseline year.[45] These are not necessarily high crime areas, as the mean arrest rates for young men is 18% in both gang and non-gang *comunas* . Yet, gang crimes make up 43% of arrests in gang *comunas* , and 37% of arrests in non-gang *comunas* .

We interact this variable with the cutoff to analyze the heterogeneity in effects by area-level gang activity. Table 7 presents the results. Since we have an interaction term, we report the IV first stage F-statistics as well. The effects on crime are present in both high and low gang-activity areas, but for property crime are larger in areas that have more gang activity. For violent crime the interaction term is strongly positive for only one of the bandwidths. This suggests that opportunities present in the neighborhood affect the likelihood of inducement into organized property crime at the cutoff.

In Appendix Table A5 we show the results for less gang related crimes. Once again, there is no evidence of SR enrollment being associated with less gang-related crimes in either the gang *comunas* or the non-gang *comunas* . Notice, it is not that our identification strategy protects against any increases in policing activity in gang comunas, as we are comparing one side of the *Sisben* cutoff to the other. Additionally, our main tables all show a row of results that also include *comunas* fixed effects.

## 7.2   Density Tests and Balance Tests

In our study, identification relies on the assumption that all other determinants of the outcome vary smoothly at the cutoff. We show that an extensive set of observables display no systematic patterns in discontinuities. In Tables A3 and A4, we show that baseline characteristics from 2002 (three years before our crime data begins) are balanced for the entire sample and for the sample used in the regressions (young males), respectively. We consider two sets of baseline characteristics: one for household-level socioeconomic variables, and the other for individuals. We report estimates over the range of different bandwidths used in our main analysis: between 4 and 10 points around the cutoff. We find no evidence of systematic discontinuities in covariates at the threshold. In the first row of Tables A3 and A4, we report a summary measure in which we collapse these variables by taking their first principal component and repeat the same

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[45]The top five are chosen on the criteria of having the most gang-flags as a ratio of total crimes.

RD analysis that we do for our main results. Again, there is no detectable difference in this composite measure of baseline characteristics, even for the largest bandwidth of 10.

Additionally, for the empirical strategy to be valid, households must not be able to manipulate their score to cross the cutoff. Work by Camacho and Conover (2011) highlights politically motivated manipulation in certain municipalities in other parts of Colombia where elections were being held. This includes both under-reporting of wealth (not necessarily a threat to our design), but also manipulation of the final score. In other parts of the country the 1998 mayoral elections (years before our sample begins) show evidence of manipulation. We use the raw survey data and the 2002 *Sisben* score only for Medellín, and so are less concerned about any manipulation of the final score.[46] Indeed, our tests of balance in the large set of baseline characteristics of the household are indicative of a lack of systematic manipulation in this context. Importantly, the cutoff for SR eligibility was determined well after the 2002 *Sisben* scores were released, and as such the cutoffs were not known to anybody during the survey. As such, we are confident that nobody could manipulate the 2002 score, even if they joined a life of crime in later years.

We test whether there was a discontinuity in the density of scores at the cutoff for the particular context of Medellín after 2002. We do this by following two methods used in the literature: the McCrary (2008) test and a test recently developed by Cattaneo et al. (2017). The Cattaneo et al. (2017) test yields a conventional t-statistic of 0.0489 or a p-value of 0.961, and a robust bias corrected p-value of 0.940, confirming that there is no statistically detectable evidence of manipulation. Figure A4 shows the distribution of the *Sisben* score for males (non-criminals and criminals) and conducts a McCrary (2008) test. Note that the distribution appears to be smooth with no evidence of bunching before the cutoff between *Sisben* levels 2 and 3 (red line).

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[46]The timing of the *Sisben* surveys do not coincide with the Medellín mayoral elections, which is when some evidence of manipulation was found in other parts of the country.

## 7.3 Alternative Crime Classifications, Different Bandwidths, Specifications and Subsamples, and the Nonparametric RD

We conduct a number of robustness checks. First, we re-classify crimes into gang-related and non gang-related groups based on the *location* where these types of crimes are more likely to occur. We calculate the relative propensity of each crime in each neighborhood. The crimes that have a higher propensity to take place in neighborhoods that were traditionally associated with organized crime are classified as gang-related crimes. These are neighborhoods that also have the highest proportion of gang-related flags associated with them. To be specific, we sort the crimes by the fraction of first arrests that happen in a gang-neighborhood. The top half of this list is classified as gang-related crimes.

This 'Neighborhood Classification Method' of crimes produces a list similar to the one where we use the police generated flags, with minor differences.[47] The lists of the top prevalent crimes by classification method can be found in Appendix Table A1. In Table A6, we re-examine our main results using the alternative classification for gang-related crimes. These results are similar to before, with the added statistical significance of drug crimes under some specifications.

Next, we re-examine our main results using the bias-correction methods suggested by Calonico et al. (2014a). In Table A7, we show results that conduct a polynomial bias correction at a larger bias-correction bandwidth (reported in the table). Once again, our results show an economically and statistically significant increase in gang-related violent and property crimes, but the effects on drug crimes are small and imprecise.

In our main specifications, when looking at a specific type of crime, we exclude arrests from other crimes.[48] In the specification shown in Table A8 we include the other categories along with the non-criminals, and show that results are similarly robust.

As our story is about both non-formal employment and violent crime, we present a specification in Table A9 that simultaneously captures both. The advantage of our individual-level data is that we can measure both employment and criminal behavior for the same individual.

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[47]The big differences among the top 25 crimes is with 'conspiracy to commit murder' that is classified as gang-related under the original classification but not gang-related under the neighborhood definition. Outside the top 25 there are other differences in the definition.

[48]For instance, when studying violent crimes, we exclude property and drug crimes from the sample altogether.

However, since we do not have annual data on formal employment, we use the 2009 *Sisben* to measure formal employment. Here the dependent variable is an indicator that equals one when the individual was not formally employed and arrested for a crime. Our results again show an increase in gang-related criminal activity, with even the drug crimes now being economically and statistically significant. This result allows us to address any concerns that the increase in informality and increase in arrests were independent of each other.

Finally, in Figure A5 we vary the bandwidth through a much wider range – every integer between 2 and 10. Gang-relates violent and property crimes consistently display a positive RD coefficient, whereas drug crimes are not statistically indistinguishable from zero, even as the coefficients are positive and fairly large for smaller bandwidths.

# 8    Interpretation and Alternative Mechanisms

The simultaneous decrease in formal sector employment and rise in arrests related to organized crime supports a model in which a higher cost of being formally employed induces an occupational choice into a life of crime (Becker, 1968).[49] Indeed, using the 2009 *Sisben* measures, we show in Table A9 that there was an increase in individuals that were simultaneously not in the formal sector and arrested. This result is statistically and economically significant for all gang crimes, including drug crimes, and validates the interpretation of our results as being driven by individual-level occupational choice across formal employment and potentially criminal activity in the informal sector. Given the lack of effects for non gang-related crimes, it is difficult to find alternative explanations to reconcile these results.

We consider three alternative theories. First, better health benefits at the cutoff may induce one to engage in riskier behaviors, yet it is difficult to support why these riskier behaviors would not also include non gang-related crimes (e.g., drug consumption). Furthermore, as health coverage is near universal by the end of this period, individuals on both sides of the cutoff have the same coverage, with the only difference being who pays (which depends on formal sector employment). Indeed, the benefits themselves are quite comparable for most of this period, and

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[49]While we do not discuss in detail specific pathways, anthropological evidence lends credence to active recruitment by gang members of young men that 'hang around' in neighborhoods with idle time, and are not in the formal sector (Baird, 2011).

as such there are no discontinuities in healthcare at the cutoff. This suggests that the driving force is the cost of coverage under formal sector employment.

Second, formal workers vesting more into the health system may fear losing their jobs if arrested and reduce criminal activities as a result. However, again this should be just as true for non gang-related crimes. Third, the police may falsely target informal workers even if they are not criminals, but it is unlikely that they would be booked disproportionately under gang-related crimes. Indeed, it may be easier to falsely target potential criminals for petty crime rather than more serious offenses like homicide or auto theft. The distinction between gang and non-gang crimes powerfully helps exclude alternative mechanisms and lends credence to the occupational choice story we posit.

We find that a 1 percentage point fall in formal employment is related to 3.1% increase in arrests, which lies in the lower range of recently reviewed estimates.[50] Our estimates reflect slight differences with other estimates in the literature. Becker (1968) discusses both income and substitution effects when wages rise in one sector. For much of the other literature, job loss or unemployment in the formal sector may produce income and substitution effects that both accentuate criminal activity. In contrast, we may think that the gains from the subsidy in our case may actually reduce criminal activity. Additionally, while we are directly testing the choice of occupation, most of the related work studies reduced access to legitimate jobs as a consequence of job losses or work restrictions, and thereby recover different elasticities.[51]

As our estimates are similar to those obtained from other recent causal analysis leveraging individual-level variation (Pinotti, 2017), we interpret our results to be not only economically meaningful, but also plausible. Yet, these magnitudes should be understood to be context-specific. We study a high-crime environment, similar only to other developing countries and especially Latin America.[52] Furthermore, we estimate a Local Average Treatment Effect (LATE) on marginal workers in the neighborhood of an income cutoff. It is plausible that at higher income levels, healthcare is a less important fraction of expenditures, and is less likely to in-

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[50]A recent review by Bennett and Ouazad (2018) discusses how a 1 percentage point increase in unemployment usually corresponds to a 3-7% increase in crime.

[51]Job losses and structurally imposed employment restrictions may additionally induce effects on depression, subsequent job search, social stigma etc. whereas our variation in relative costs of employment should not.

[52]Recent evidence from Latin America suggests much larger employment-crime elasticities (Dell et al., 2018; Dix-Carneiro et al., 2018).

duce such behavior. Finally, it should be noted that the newly induced marginal criminals may be unlike the average criminal along many dimensions, including ease of avoiding arrests, and as such our results may not be widely generalizable for other sub-populations. Nevertheless, since the exogenous probability of getting caught conditional on committing a crime has no reason to be discontinuous at the cutoff, our estimates are unbiased even in the presence of such heterogeneity in criminal "skill."

# 9    Conclusion

In this paper, we highlight an important fact: disincentivizing formal employment can lead to substantial increases in criminal activity when informal opportunities include employment by organized criminal entities. We evaluate this claim in the context of the high-crime environment of Medellín, Colombia. We first provide strong evidence showing that the criteria behind the health benefits policy led to a sharp decrease in formal sector employment. At the margin, the policy raised the opportunity cost of formal-sector employment and induced workers to join the informal labor market.

In Medellín, this informal market contains significant opportunities related to organized crime. We follow these same individuals over a decade and show that this decrease in formal sector employment led to an increase in the probability of being arrested for organized-crime related activities. On the other hand, crimes less likely to be associated with criminal economic enterprises, like crimes of impulse or opportunity, show no such impacts at the eligibility threshold, lending credence to the occupational choice mechanism we advance. Together, our simple calculations suggest that as the policy pushed workers out of the formal sector, a meaningful fraction of these workers were drawn into organized crime. These effects were largest in neighborhoods that had, at baseline, greater opportunities to join organized crime.

Crime deterrence may have limited benefits if the supply elasticity to criminal activity is high (Freeman, 1999). Investigating the decisions behind choosing a life of crime, as we do here, is essential in the fight against crime. Importantly, our work speaks to the determinants of engaging in criminal activity at the individual level. The strength of our approach is that we do not need to use area-based variation or disequilibrium shocks like job-losses to identify the

occupation choice between individual employment opportunities and crime. We do this using a unique data set that matches the census of arrests with socio-economic outcomes over a decade, in the context of one of the most violent cities in the world. We find a source of exogenous variation generated by policy rules, and use a regression discontinuity design to estimate our effects.

We conclude that Colombia's well-intentioned and broad-based subsidies for healthcare had the unintended consequence of incentivizing gang participation by way of its distortionary provision rules. The program being important for providing subsidized health access to low income families implies that there is little reason to do away with it. Yet, the formality-clause governing the selection into the program may be distortionary, and as such warrant further examination.[53]

Removing the emphasis on informality (but still targeting the poor) may negate the increase in criminal activity around the cutoff. The costs underlying such a change would be a larger fiscal burden as even low-income formal sector workers would be eligible for SR. The benefits, on the other hand, are far reaching: less crime, less policing and incarceration, and less negative externalities on families and children. This has important welfare implications for the design of many such programs across the developing world which often have far-reaching and understudied consequences on seemingly unrelated outcomes and behaviors. Our results provide guidance for how impactful improving access to and incentives for formal sector employment can be for deterring criminal activity.

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[53]Recognizing these adverse effects, policy-makers lowered the costs of CR enrollment at the end of our study period, when Law 1607 was enacted, leading to a significant increase in formal sector employment (Bernal et al., 2017; Kugler et al., 2017; Morales and Medina, 2017).

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# Online Appendix: Additional Tables and Figures

Figure A1: Timeline of Data Used

Arrests (SIJIN)

| 2002 | 2005 | 2009 | 2013 |

*Sisben* Score / Eligibility (*Sisben* 1, DNP)

Subsidized Regime (*Sisben* 2, DNP)

Formal Employment (*Sisben* 3, DNP)
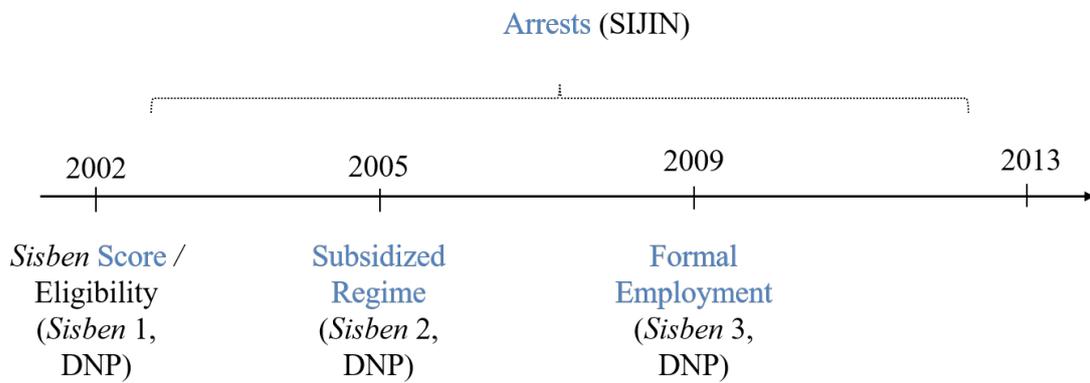
Figure A2: Distribution of Age at Arrest (Males)
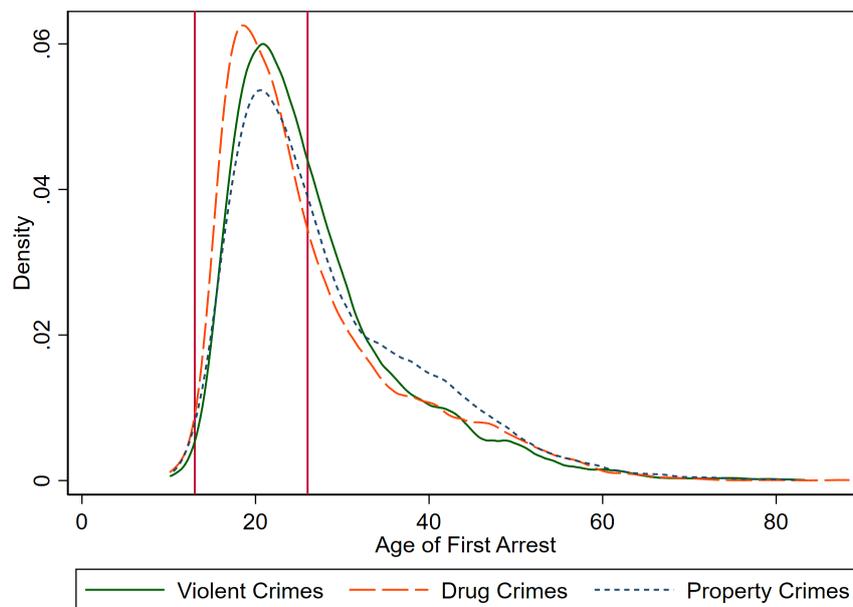


Source: Policía Nacional de Colombia. Vertical lines represent ages 13 and 26.

Figure A3: Location of 'in-the-act' arrests by type of crime, 2005-2013.



Homicides

Property

Intrafamily Violence

Drug Trafficking
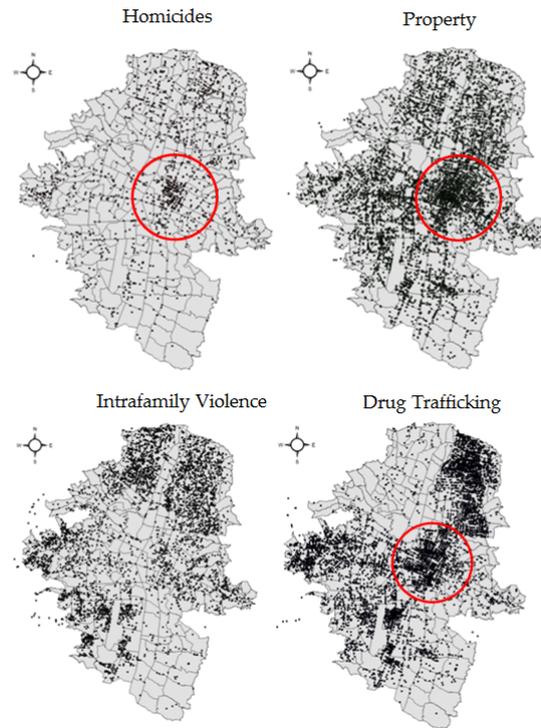
Source: Medina and Tamayo (2011) using Policía Nacional de Colombia. Dots indicate arrests. Bold lines are neighborhood boundaries. Red circle is downtown.

Figure A4: Sisben score Distribution (All Males).
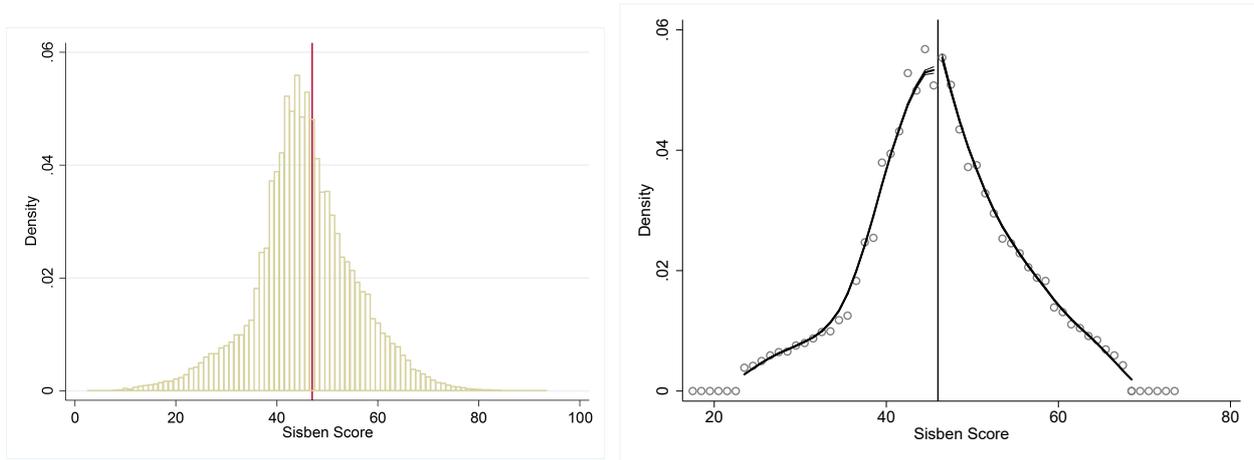


Source: *Sisben* survey of 2002. The figure includes all males (i.e. both non-criminals and arrested individuals). The left panel shows the histogram and the right panel conducts a McCrary (2008) test.

Table A1: List of Top Crimes by Data-driven Classifications

| Crime | Type | Gang Flags | Neighborhood Method |
|---|---|---|---|
| Drug Consumption / Possession | Drug | No | No |
| Drug trafficking / Distribution - Marijuana | Drug | No | No |
| Drug trafficking / Distribution | Drug | Yes | Yes |
| Drug trafficking / Distribution - Cocaine paste | Drug | Yes | Yes |
| Drug trafficking / Distribution Heroin | Drug | Yes | Yes |
| Use of Fake Identification, false document | Property | No | No |
| Motor vehicle theft (Motorcycles) | Property | No | No |
| Receiving Bribes (as officials) | Property | No | No |
| Copyright/Fraud | Property | No | No |
| Identity Theft | Property | No | No |
| Fraud | Property | No | No |
| Theft / Assault | Property | Yes | Yes |
| Robbery (To Businesses, firms) | Property | Yes | Yes |
| Property Vandalism | Property | Yes | Yes |
| Motor Vehicle Theft - Cars | Property | Yes | Yes |
| Burglary | Property | Yes | Yes |
| Simple Assault/Battery | Violent | No | No |
| Rape/Sexual Assault | Violent | No | No |
| Conspiracy to commit murder | Violent | Yes | No |
| Homicide | Violent | Yes | Yes |
| Extortion | Violent | Yes | Yes |
| Assault / Battery - Against Police | Violent | Yes | Yes |
| Manufacture, Trafficking Firearms / Weapons | Violent | Yes | Yes |
| Intimidation and Stalking | Violent | Yes | Yes |
| Terrorism | Violent | Yes | Yes |
| Kidnapping | Violent | Yes | Yes |

List of top crimes by type and gang classification, out of 103 crimes. The 'Gang Flags' lists whether or not the crime has a high propensity to receive a police reported flag of gang-related at the time of arrest. The 'Neighborhood Method' classifies crimes that have a high propensity to be in neighborhoods that also receive a higher fraction of gang-related flags at the time of arrest.

## Table A2: Formal Employment By Gender

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|
| **Panel A: Men Formal Employment in 2009** | | | |
| Enrolled in SR | -0.0539*** | -0.0411*** | -0.0301*** |
| | (0.0166) | (0.0103) | (0.00811) |
| Number of observations | 133,067 | 180,742 | 247,886 |
| **Panel B: Women Formal Employment in 2009** | | | |
| Enrolled in SR | 0.00560 | -0.0130* | -0.0169* |
| | (0.00757) | (0.00786) | (0.00889) |
| Number of observations | 156,942 | 213,755 | 292,980 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. We use the *Sisben* survey of 2009 to construct formal employment. Tables report two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. Regressions control linearly for the 2002 Sisben score, flexibly around the cutoff. We cluster standard errors by *comuna*.

Table A3: Baseline (2002 *Sisben* Survey) balance tests

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|
| First Principal Component | 0.0184 | -0.0023 | 0.0282 |
| | (0.0158) | (0.0191) | (0.0234) |
| | | | |
| Years of Education | 0.0183 | 0.00248 | -0.0408 |
| | (0.0685) | (0.0606) | (0.0453) |
| Age | 0.0339 | 0.0255 | 0.0177 |
| | (0.0469) | (0.0300) | (0.0239) |
| Age Specific Education Gap | 0.00566 | 0.0159 | 0.0580 |
| | (0.0613) | (0.0602) | (0.0435) |
| HH Head Years of Education | -0.0463 | -0.0832 | -0.105** |
| | (0.0652) | (0.0592) | (0.0451) |
| Unemployed | 0.0166* | 0.0113 | 0.00971 |
| | (0.00878) | (0.00738) | (0.00726) |
| Married | 0.0161 | 0.0275 | 0.0307*** |
| | (0.0228) | (0.0175) | (0.0106) |
| Employed | -0.00846 | -0.0115 | -0.0182** |
| | (0.0129) | (0.0102) | (0.00760) |
| Attending School | -0.000226 | 0.000756 | 0.000217 |
| | (0.00247) | (0.00270) | (0.00221) |
| Neighborhood Stratum 2 | -0.00551 | -0.0129 | 0.00394 |
| | (0.0215) | (0.0144) | (0.0110) |
| Neighborhood Stratum 1 | 0.0199 | 0.0236* | 0.00311 |
| | (0.0201) | (0.0126) | (0.00955) |
| Own House | 0.0249 | 0.0218 | 0.0192** |
| | (0.0159) | (0.0127) | (0.00789) |
| Less than 6 years Olds | 0.00783 | 0.0131 | 0.0201* |
| | (0.0104) | (0.0129) | (0.0113) |
| HH Head Age | -0.000226 | 0.000756 | 0.000217 |
| | (0.00247) | (0.00270) | (0.00221) |
| | | | |
| Observations | 181,132 | 246,974 | 340,581 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report reduced form coefficients on being below the Sisben cutoff, where Sisben score is measured in 2002. Regressions control linearly for the Sisben score, flexibly around the cutoff. All variables are measured in 2002. We cluster standard errors by *comuna*. Neighborhood strata indicate the official socioeconomic strata of the neighborhood. First Principal Component takes the first principal component of all other variables.

Table A4: Baseline (2002 *Sisben* Survey) balance tests, Male Youth.

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|
| First Principal Component | 0.0174 | -0.00246 | -0.0158 |
| | (0.0603) | (0.0446) | (0.0300) |
| | | | |
| Years of Education | 0.0411 | 0.0275 | -0.0247 |
| | (0.0629) | (0.0552) | (0.0401) |
| Age | 0.0132 | 0.00976 | 0.00376 |
| | (0.0129) | (0.0113) | (0.00913) |
| Age Specic Education Gap | -0.0321 | -0.0208 | 0.0260 |
| | (0.0590) | (0.0600) | (0.0415) |
| HH Head Years of Education | -0.0167 | -0.0560 | -0.0840* |
| | (0.0647) | (0.0583) | (0.0436) |
| Unemployed | 0.0179** | 0.0111 | 0.00924 |
| | (0.00846) | (0.00697) | (0.00737) |
| Married | 0.0171 | 0.0254 | 0.0314*** |
| | (0.0227) | (0.0175) | (0.0109) |
| Employed | -0.0204** | -0.0139 | -0.0123 |
| | (0.00968) | (0.0106) | (0.00998) |
| Attending School | -0.000356 | 0.000788 | 0.000270 |
| | (0.00241) | (0.00271) | (0.00224) |
| Neighborhood Stratum 2 | -0.00588 | -0.00299 | 0.00571 |
| | (0.00932) | (0.00713) | (0.00626) |
| Neighborhood Stratum 1 | -0.00118 | 0.00362 | -0.00524 |
| | (0.0163) | (0.00916) | (0.00796) |
| Own House | 0.0116 | 0.00752 | 0.0114* |
| | (0.0147) | (0.0110) | (0.00646) |
| Less than 6 years Olds | 0.00745 | 0.0122 | 0.0187 |
| | (0.0103) | (0.0126) | (0.0112) |
| HH Head Age | 0.341* | 0.338** | 0.220** |
| | (0.167) | (0.136) | (0.100) |
| | | | |
| Observations | 21,366 | 28,675 | 38,888 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report reduced form coefficients on being below the Sisben cutoff, where Sisben score is measured in 2002. Regressions control linearly for the Sisben score, flexibly around the cutoff. All variables are measured in 2002. We cluster standard errors by *comuna*. Neighborhood strata indicate the official socioeconomic strata of the neighborhood. First Principal Component takes the first principal component of all other variables.

Table A5: Less Gang-related Crimes: Heterogeneity by Comuna

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|
| **Panel A: Less Gang-Related Violent Crimes** | | | |
| Enrolled in SR | 0.00886 | 0.00333 | -0.00421 |
| | (0.0167) | (0.0114) | (0.0123) |
| Enrolled* Gang Comuna | 0.00773 | 0.00236 | 0.0126 |
| | (0.0240) | (0.0150) | (0.0144) |
| F stat | 92.6 | 134.6 | 204.3 |
| Number of observations | 18,419 | 24,768 | 33,702 |
| **Panel B: Less Gang-Related Property Crimes** | | | |
| Enrolled in SR | -0.00571 | -0.00526 | -0.00503 |
| | (0.0168) | (0.0120) | (0.00969) |
| Enrolled* Gang Comuna | -0.0176 | -0.0122 | -0.0100 |
| | (0.0180) | (0.0153) | (0.0122) |
| F stat | 100.2 | 161.8 | 241.4 |
| Number of observations | 18,240 | 24,523 | 33,358 |
| **Panel C: Less Gang-Related Drug Crimes** | | | |
| Enrolled in SR | -0.0379 | -0.0494 | -0.0292 |
| | (0.0299) | (0.0307) | (0.0222) |
| Enrolled* Gang Comuna | 0.0132 | 0.0155 | 0.00281 |
| | (0.0292) | (0.0211) | (0.0195) |
| F stat | 94.8 | 135.1 | 197.3 |
| Number of observations | 19,150 | 25,740 | 35,104 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff and an interaction between high-gang comunas and being below the cutoff. The Sisben score is measure in 2002, SR enrollment in 2005, and crime outcomes are measured between 2005 and 2013. Regressions include comuna fixed effects and an interaction between high-gang comunas and indicators for SR enrollment. Regressions control linearly for the Sisben score, flexibly around the cutoff. We consider only males between 21 to 26 years old in 2013. We cluster errors by comuna. The mean arrest rate across all five gang comunas are 18%, which is also the mean arrest rate in non-gang comunas.

## Table A6: Neighborhood Classification Method

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|

### Panel A: Gang-Related Violent Crimes

| | 4 | 6 | 10 |
|---|---|---|---|
| Enrolled in SR | 0.0171** | 0.0121* | 0.00891** |
| | (0.00751) | (0.00684) | (0.00387) |
| Number of observations | 17,995 | 24,198 | 32,931 |

### Panel B: Gang-Related Property Crimes

| | 4 | 6 | 10 |
|---|---|---|---|
| Enrolled in SR | 0.0335** | 0.0271** | 0.0192* |
| | (0.0131) | (0.0122) | (0.0107) |
| Number of observations | 18,426 | 24,740 | 33,625 |

### Panel C: Gang-Related Drug Crimes

| | 4 | 6 | 10 |
|---|---|---|---|
| Enrolled in SR | 0.0284* | 0.0108 | -0.00197 |
| | (0.0163) | (0.0126) | (0.0115) |
| Number of observations | 18,909 | 25,447 | 34,661 |

Note: Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. The Sisben score is measured in 2002 and SR enrollment in 2005. Crime data are from 2005 to 2013. Results use the neighborhood classification method described in the text to classify crimes. Regressions control linearly for the Sisben score, flexibly around the cutoff. We consider only males between 21 to 26 years old in 2013.

Table A7: Semi-parametric RD with Bias Correction

| Type of Crime | Violent | Property | Drug |
|---|---|---|---|
| Enrolled in SR | 0.0164 | 0.02794 | 0.00768 |
| Standard error | (0.00972) | (0.01636) | (0.02069) |
| Bias corrected p-value | 0.077 | 0.052 | 0.57 |
| Bandwidth | 5.2 | 5.8 | 6.6 |
| Bias correction bandwidth | 9.9 | 8.9 | 9.6 |
| Number of observations | 24,206 | 26,511 | 29,102 |

Note: Results using the Calonico et al. (2014a) CCT method for estimation, where the primary estimation uses a linear functional form and the bias correction uses a quadratic form. Tables report fuzzy RD two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff, where Sisben score is measured in 2002. Crime data is from 2005 to 2013. We consider only males between 21 to 26 years old in 2013.

# Figure A5: Robustness to Bandwidths (Gang-Related Crime)
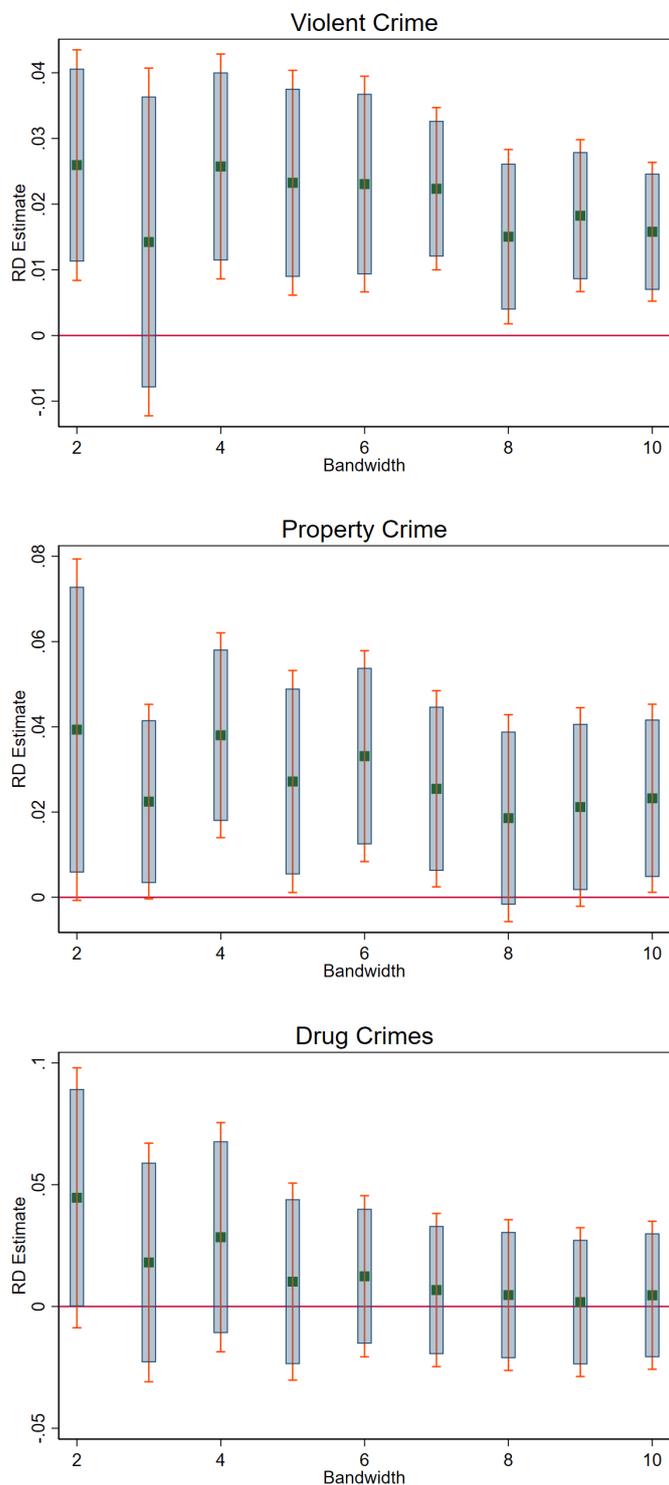


Note: Coefficients of RD 2SLS regressions where the first stage is SR Enrollment on being below the Sisben cutoff.
Sample of gang-related crimes only. Grey bars indicate 90% confidence intervals. Red lines indicate 95% confidence intervals.

Table A8: Robustness Check: Including Other Crimes in the Sample

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|

**Panel A: Gang-Related Violent Crimes**

| | | | |
|---|---|---|---|
| Enrolled in SR | 0.0213*** | 0.0195*** | 0.0136*** |
| | (0.00718) | (0.00676) | (0.00416) |
| Number of observations | 21,720 | 29,235 | 39,877 |

**Panel B: Gang-Related Property Crimes**

| | | | |
|---|---|---|---|
| Enrolled in SR | 0.0320*** | 0.0289*** | 0.0204** |
| | (0.0100) | (0.00998) | (0.00894) |
| Number of observations | 21,720 | 29,235 | 39,877 |

**Panel C: Gang-Related Drug Crimes**

| | | | |
|---|---|---|---|
| Enrolled in SR | 0.0238 | 0.0107 | 0.00405 |
| | (0.0195) | (0.0138) | (0.0129) |
| Number of observations | 21,720 | 29,235 | 39,877 |

Note: The sample includes other crimes. For instance, when looking at violent gang-crime arrests as the outcome of interest, property crime, drug crime and violent non-gang crime arrests are also in the sample grouped with the people never arrested in this period. Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. Regressions control linearly for the Sisben score, flexibly around the cutoff. We consider only males between 21 to 26 years old in 2013.

Table A9: Simultaneously both Non-formally Employed (in 2009) and Arrested

| Bandwidths: | 4 | 6 | 10 |
|---|---|---|---|

Panel A: Gang-Related Violent Crimes

| | | | |
|---|---|---|---|
| Enrolled in SR | 0.0104 | 0.00997** | 0.00990** |
| | (0.00771) | (0.00405) | (0.00436) |
| Number of observations | 12,015 | 16,023 | 21,733 |

Panel B: Gang-Related Property Crimes

| | | | |
|---|---|---|---|
| Enrolled in SR | 0.0393** | 0.0331*** | 0.0183 |
| | (0.0184) | (0.0122) | (0.0128) |
| Number of observations | 12,244 | 16,319 | 22,074 |

Panel C: Gang-Related Drug Crimes

| | | | |
|---|---|---|---|
| Enrolled in SR | 0.0207 | 0.0299*** | 0.0291** |
| | (0.0177) | (0.00983) | (0.0126) |
| Number of observations | 12,253 | 16,368 | 22,199 |

Note: The outcome is arrests only for those not formally employed as measured in 2009. We exclude all arrests post 2009. Standard errors in parentheses. *** significant at 1%; ** significant at 5%; * significant at 10%. Tables report two-staged least squares (2SLS) coefficients where the first stage is SR enrollment on being below the Sisben cutoff. The Sisben score is measured in 2002, and SR enrollment in 2005. Crime is measured between 2005 and 2009. Regressions control linearly for the Sisben score, flexibly around the cutoff. We consider only males between 21 to 26 years old in 2013.