

FORMAL EMPLOYMENT AND ORGANISED CRIME: REGRESSION DISCONTINUITY EVIDENCE FROM COLOMBIA*

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Safety net programs, common in settings with high informality like Latin America, often use a means test to establish eligibility. We ask: in settings in which organised crime provides lucrative opportunities in the informal market, will discouraging formal employment via benefits eligibility criteria increase criminal enterprise activity? We link administrative socioeconomic microdata with the universe of arrests in Medellín over a decade, and exploit exogenous variation in formal-sector employment around a socioeconomic-score cutoff, below which individuals receive generous benefits if not formally employed. Regression discontinuity estimates confirm this policy reduced formal-sector employment and generated a corresponding increase in arrests associated with organised crime. We do not find increases for crimes unlikely to be associated with organised entities, such as crimes of impulse or opportunity. Effects on arrests are strongest in neighbourhoods where organised crime is most prevalent.

Many countries, particularly across the developing world and in much of Latin America, are plagued by high degrees of informality in the labour market, and offer safety net programs with generous health care, unemployment insurance and cash transfers to address the limited access to benefits that often accompany informal employment (Schneider and Enste, 2000; Ulyseas, 2020; Banerjee *et al.*, 2022). These programs, however, usually use means tests (including formal employment status) as criteria for eligibility, which in turn potentially reinforce informality by discentivising formal employment (de Brauw *et al.*, 2015; Bosch and Schady, 2019; Bergolo and Cruces, 2021). In many of these settings, criminal activity often controlled by organised criminal enterprises also abounds (DiTella *et al.*, 2010; Brown and Velasquez, 2017; Chimeli and Soares, 2017; Buonanno and Vargas, 2019; Dell *et al.*, 2019; Arteaga, 2021; Sviatschi, 2022; Tobón, 2022). In this paper we ask: in settings in which lucrative opportunities in the informal labour market include working in organised crime, if individuals are disincentivised

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from participating in formal employment due to eligibility criteria for generous benefits programs, will they be more likely to engage in criminal enterprise activity?

We use rich administrative data between 2002 and 2012 from Medellín, Colombia, to test the relationship between formal employment and participation in crime at the individual level. In this empirical context, where informality is common and criminal enterprise activity is abundant, financially dissuading individuals from engaging in formal employment could drive some to crime as their most lucrative option outside of the formal sector. Specifically, we exploit a discontinuity in the incentives to remain informal in Colombia.

The Colombian government provides health benefits to all residents of households that have a socioeconomic score (known as the *Sisben* score) below a certain threshold. Formal employment makes the individual and their dependants ineligible for this program, raising the relative benefits to informality as an unintended consequence of the eligibility criteria for this program. Those formally employed are automatically taxed a fraction of their wages to avail of comparable benefits. Accordingly, the usual benefits to formal 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 free coverage by the government if they were not formally employed. The importance of these incentives in our context is emphasised by the near-complete health care coverage in the population, despite costs representing large proportions of income for many households.

Using a regression discontinuity (RD) design, we find that the policy reduced the formal employment rate, consistent with evidence from previous studies (Camacho *et al.*, 2014). These same individuals are arrested at higher rates for crimes ‘likely associated with criminal enterprises’ (LACE) by 1.64 percentage points at the RD cutoff. We find a 0.56 percentage point rise in LACE violent crimes (e.g., firearms trafficking, homicide), a roughly 0.76 percentage point rise in LACE property crimes (e.g., car theft) and a less precisely estimated 0.32 percentage point rise in LACE drug crimes (e.g., cocaine and heroin distribution). The impact on LACE violent crimes is fairly novel, as most related studies only find impacts on property crimes (Bennett and Ouazad, 2020; Watson *et al.*, 2020) and occasionally non-LACE violent crimes like domestic violence (Rose, 2019); while LACE violent crimes are particularly indicative of organised criminal activity in this setting. Importantly, offences not likely associated with economic motivations and organised criminal enterprises (e.g., rape and marijuana consumption) do not show significant increases at the cutoff, allowing us to rule out many alternative theories. For instance, if social benefits induce risky behaviour, it should increase non-LACE crimes as well. Additional results also show that impacts on LACE crimes are strongest in neighbourhoods known to have the highest gang activity at baseline.

Our main contribution lies in highlighting participation in organised criminal activity as a potential unintended, adverse consequence of welfare programs, particularly when eligibility criteria disincentivise formal employment and organised criminal activity abounds in the informal labour market. In some contexts, access to welfare may reduce criminal participation (Deshpande and Mueller-Smith, 2022), but several recent papers have specifically documented that the eligibility criteria for generous benefits programs in Latin American countries can affect formal employment. Bergolo and Cruces (2021) showed that formal employment is impacted in Uruguay, where formal earnings increase the chances of a poverty score rising above an eligibility cutoff for a cash transfer program. Similarly, de Brauw *et al.* (2015) and Bosch and Schady (2019) showed in Brazil and Ecuador, respectively, that cash transfer programs do not reduce overall work, but do lead to a resorting from formal to informal employment. Relatedly, Gerard and

Gonzaga (2021) showed that an unemployment insurance program in Brazil discourages formal-sector employment. Though, in a related study, Gerard *et al.* (2021) showed that a cash transfer program in Brazil actually increased formal employment. In Colombia specifically, prior studies have documented the impact of health insurance payroll costs on formal employment (Kugler and Kugler, 2009; Camacho *et al.*, 2014; Lamprea and Garcia, 2016). In fact, the Colombian government adjusted the costs of the formal-sector health benefits program in response, leading to a significant increase in formal employment (Bernal *et al.*, 2017; Fernández and Villar, 2017; Kugler *et al.*, 2017; Morales and Medina, 2017).

While this literature has not documented changes in participation in organised crime as a result of these eligibility criteria for these social benefit programs, a closely related literature has studied more broadly the link between cash or welfare payment benefit programs and criminality, and in general found negative effects on criminal activity (e.g., Chioda *et al.*, 2016 in Brazil, Crost *et al.*, 2016 in the Philippines and Watson *et al.*, 2020 in the United States (Tuttle, 2019)). We draw inspiration from these studies in asking if a program with eligibility at least partially affected by formal employment, and therefore a likely negative effect on formality, will indeed lead to increases in criminality, especially for crimes that are economically motivated and in settings like Medellín, where the informal market includes lucrative opportunities to engage in organised crime.

We also complement a related literature that has highlighted how unemployment shocks, job loss and employment restrictions lead to increases in criminal activity (Mastrobuoni and Pinotti, 2015; Pinotti, 2017; Bell *et al.*, 2018; Schnepel, 2018; Rose, 2019; Bennett and Ouazad, 2020; Khanna *et al.*, 2021). Job losses and structurally imposed employment restrictions may induce effects on depression, subsequent job search and social stigma that are less likely in our setting. These sources of variation also generate financial necessity or desperation that can lead to temporary or idiosyncratic criminality, not necessarily reflective of a trade-off between formal employment and criminal enterprise activity. This role of financial necessity has been shown to be a mediating mechanism in the relationship between job loss and criminality (Khanna *et al.*, 2021; Britto *et al.*, 2022), and is emphasised by studies of current criminals and recently released prisoners (Munyo and Rossi, 2015; Blattman *et al.*, 2017). We stress the importance of distinguishing between different types of crime, as some are more likely reflective of the sorting along the margin between formal employment and organised criminal enterprise activity in the labour market (e.g., firearms trafficking and heroin distribution), whereas others are more likely to be crimes of impulse or opportunity (e.g., rape and drug consumption). In doing so, we establish a falsification test to rule out alternative mechanisms.¹

Additionally, there are fewer studies in the developing world, as many look at the United States, the UK or Scandinavian countries from which data are more readily available (Cook *et al.*, 2015; Gelber *et al.*, 2016; Hjalmarsson and Lindquist, 2019; Bhuller *et al.*, 2020; Davis and Heller, 2020). We study a high crime and high informality environment similar to most parts of the developing world and, in particular, a city with a significant presence of organised crime, which has been shown to have particularly detrimental effects on growth and development (Alesina *et al.*, 2019; Blattman *et al.*, 2019; Velásquez, 2020; Melnikov *et al.*, 2022), and broader consequences for child development (Arteaga, 2021). We build upon recent evidence

¹ Related work has documented criminal participation in response to work experience (Schochet *et al.*, 2008; Heller, 2014; Gelber *et al.*, 2016; Davis and Heller, 2020). However, the variation leveraged in these studies could change returns to many, if not all, types of economic activities (formal, informal legitimate and criminal). And so, impacts may not in general be the same as those derived from disincentives for formal employment.

from important high-crime environments in Latin America, by isolating the role of disincentives to formal employment, from other mechanisms arising from job destruction (Khanna *et al.*, 2021; Britto *et al.*, 2022), short-term liquidity constraints (Munyo and Rossi, 2015) and criminal capital deriving from past prison experiences (Tobón, 2022).

1. Background

1.1. *Crime in Medellín*

Located in north-western Colombia, Medellín is the second largest city after Bogota. Over our period of study, Medellín was one of the most violent cities in the world (CCSPJP, 2009), placing our analysis among a handful that study motivations behind criminal participation in environments with high crime, and organised criminal entities.

Economic incentives are strong drivers of criminal participation in Medellín (Khanna *et al.*, 2021), and the high homicide rates are a result of fights among urban militias, local gangs, drug cartels, criminal bands and paramilitaries. In Online Appendix D we describe some features of crime in Medellín, including details on the motivations that drive criminal participation, the criminal justice system, crime data and the role of neighbourhoods.

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. Over the entire sample period, 12% of all males (across all age groups) were at some point arrested, while the arrest rate for females was only 1%. Second, youth are far more likely to be involved than older individuals. Approximately 63% of all 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 in property crime. Arrests peak within the 13 to 26 age window depicted in Online Appendix Figure A1. For our sample of young men in the analysed bandwidth, 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, but are representative of certain parts of the developing world, especially cities in Latin America (Brown and Velasquez, 2017; Sviatschi, 2022).

We describe further details on motivations in criminal participation and organised crime in Online Appendix D1. Like in other parts of Latin America, crime is endemic and gang controlled, and among the potential occupations open to those discouraged from taking up formal work there is wage employment in gangs. Criminal enterprises offer a fixed remuneration, potential benefits to family when in prison and some protection. Criminal activity (even minor theft) in gang territory must usually be sanctioned by gangs, as gangs protect entities they extort. Criminal enterprises might also coerce individuals into being recruited, and this may be particularly effective for young individuals making long-term career choices.

1.2. *Access to Health Benefits*

In 1993, *Law 100* established two tiers of health insurance: the contributive regime (CR) and the subsidised regime (SR). In Online Appendices C1 to C6, we describe the details behind the policy, the costs of access, coverage, eligibility criteria, the corresponding policy variation and changes over time. Here, we summarise the information relevant for the analysis.

CR covers formal workers with a comprehensive set of health services that includes nearly all of the most common illnesses. SR covers the poorest informal workers and their dependants. Formal workers and employers fund workers' insurance premiums for coverage by CR. By 2007, health coverage in Colombia was above 90% (Ministry of Health and Social Protection). In the case of Medellín, insurance was nearly universal, and it stayed at or above 95% by the end of the analysed period (Lamprea and Garcia, 2016).

Colombian employers are required by law to enrol all their employees in a Health Promoting Company, which gives them access to health insurance under CR. Formal employees pay up to 12.5% of their wage, and cross-subsidise informal workers in SR. Independent workers earning above the cutoff (richer) are required to contribute 12.5% of the maximum between one monthly minimum wage and 40% of their earnings to CR. As such, for those above the income cutoff (richer), the cheapest way to get health insurance is to be formal (and pay up to 12.5% of their wages), while for those below the cutoff, the option exists to not be formal and enrol in SR. Unemployed or inactive individuals (and informal workers) can either get health insurance as the self-employed do through CR, or apply for access to SR (if eligible).

To target SR, roughly 70% of the poorest households in the country were interviewed 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. Individuals who live in households with a *Sisben* score below a certain cutoff are eligible for SR if they are not formally employed (and are not dependants of someone formally employed). Households keep their *Sisben* score until it is updated by the government. 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* during the study period.² The SR health program is by far the largest that has eligibility determined by the *Sisben* score: being nearly 2% of GDP, while all other anti-poverty programs represent less than 0.4% of GDP.

1.3. *Incentives for Informality*

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 health care 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.

The attractiveness of SR is compounded by the lower costs of access as well. Over and above their enrolment contributions (up to 12.5% of their wage), workers in the CR pay a co-payment and a *Cuota Moderadora*. The co-payments across health services are a portion of the costs covered by individuals out of pocket that vary with the workers' salary (number of minimum wages). The *Cuota Moderadora* is a fixed out-of-pocket amount paid by an insured for covered services and varies with the workers' salary (number of minimum monthly wages). Informal workers enrolled in SR neither pay for their enrolment nor the *Cuota Moderadora*, and have much lower co-payments. For more details, see Online Appendix C2.

² See www.sisben.gov.co/Paginas/Noticias/Puntos-de-corte.aspx for programs by the *Sisben* 3 cutoff. While the *Sisben* cutoff for SR enrolment may differ across counties, there is only one cutoff for the entirety of Medellín, and it did not coincide with other major programs. See also Online Appendix C6.

That this policy reduced 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) controlled for both region and time fixed effects to show that informality increased by 4 percentage points as SR was rolled out across the country. This is a combination of workers dropping out of the formal sector, and fewer youth joining the formal sector over time (Lamprea and Garcia, 2016). Recognising these adverse effects on formal employment, the government lowered CR enrolment costs at the end of our study period, when Law 1607 was enacted. Not surprisingly, 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 is calculated at the family rather than individual level (Article 21, Decree 2353 of 205), other work suggests that older family members may have reason to discourage youth within the family from joining the formal labour force for fear of losing access to benefits, and that large families stay informal in the hope of retaining benefits (Joumard and Londono, 2013; de Brauw *et al.*, 2015; Bergolo and Cruces, 2021). The Ministry of Health maintains a census of all those enrolled in both CR and SR, and if someone in the household becomes formal, and so enrolls in CR, their eligible dependants are made ineligible for SR. These members newly enrolled in CR would have to pay the higher fixed cost of accessing health services, and higher co-payments under CR. Furthermore, the next time the *Sisben* survey is conducted, becoming formal may imply that the household may now receive a lower poverty score and make all household members ineligible for SR. Since the score is not updated instantaneously, if an individual has forfeited their family's eligibility for SR by being formally employed, but then loses their formal employment, their return to eligibility might take years, making the incentive against engaging in formal employment that much stronger.

Similarly, interviews in Baird (2011) highlighted how crime involvement can sometimes be a 'family decision'. This is confirmed by our other work in the same context that shows spillovers in criminal activity across generations within the family (Khanna *et al.*, 2021). Indeed, Santamaria *et al.* (2008) found that half of all SR recipients indicated they would not switch to formal employment as it would mean losing benefits. These effects may also affect women (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: criminal enterprises.

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 health care regime or the other, almost everyone has access to benefits on either side of the cutoff by the end of this period (Lamprea and Garcia, 2016). 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.

2. Data

Administrative data allow us to identify the relationship between incentives for informality and participation in criminal enterprise. We do not need to rely on self-reported or aggregate victim counts. As our data are at the individual level, we isolate vulnerable demographics (young men),

and test SR enrolment, employment outcomes and crime all at the individual level. 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 in 2002 (baseline *Sisben I*), 2005 (*Sisben II*) and 2009–10 (*Sisben III*). These are municipality censuses of people living in the three poorest socioeconomic strata, and we match household records across the three waves (Department of National Planning, 2009). In large cities, like Medellín, it amounts to 65%–80% of the population. The second source is the census of individuals arrested between 2002–12 from the Judicial Research Unit of the Metropolitan Police of the Aburrá Valley (2016). We describe these data in detail in Online Appendix D. We match 78% of arrests to the *Sisben* survey as some arrested may not be residents of Medellín.

We focus on the probability of ever being arrested over the period, which by and large in this young population is their first observed arrest. The literature often restricts data to first arrests, and repeat arrests are excluded as time spent under incarceration and the length of sentencing may be endogenous to other characteristics. 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). Nevertheless, we show that our results are robust to including repeat arrests.

For similar reasons, we follow recent studies (Kling *et al.*, 2005; Gronqvist, 2017) in focusing on young men in our analysis. Our primary sample was between 13 and 18 in 2005, and we follow these cohorts for the entire period of our analysis (i.e., till 2012, the last year of our arrest data). This means that we are only documenting arrests below the age of 26, capturing more than 64% of all first observed arrests (as shown in Online Appendix Figure A1). In robustness checks, we expand the age criteria to include individuals who were as young as 10 in 2005 (or 17 in 2012). Expanding the age criteria produces similar results.

Of the individuals arrested more than once during the observation period for any crime, 40% are first arrested before the age of 27. At the same 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 still be incarcerated), but as we do not have their entire criminal history, we would miss their first arrest. So we exclude older men. Focusing on ages when arrest rates peak reduces these concerns regarding the measurement of criminality, and allows us to emphasise the period when young men first make choices between crime and other jobs.

Figure 1 describes the timeline of our data. We use the 2002 household-level *Sisben* as our baseline to create our running variable and predict eligibility for SR. We test for SR enrolment in 2005, and for employment status and incomes in 2009 *Sisben*. We then follow the criminal histories of young men, from 2002 to 2012.

Table 1 presents the 2002 baseline summary statistics of the complete *Sisben* survey and for the subsample of male youth. SR status is established based on the previously computed *Sisben* score (our running variable), from the semi-decadal *Sisben* municipality census of 70% of the poorest population. The arrests data include a detailed description of the person arrested (national ID number and date of birth), type of crime (e.g., homicide, rape, motor vehicle theft, etc.), the precise penal code article associated with the crime, a description of the act (e.g., trafficking cocaine), date of arrest, location of crime and a police flag for whether the officer knew the

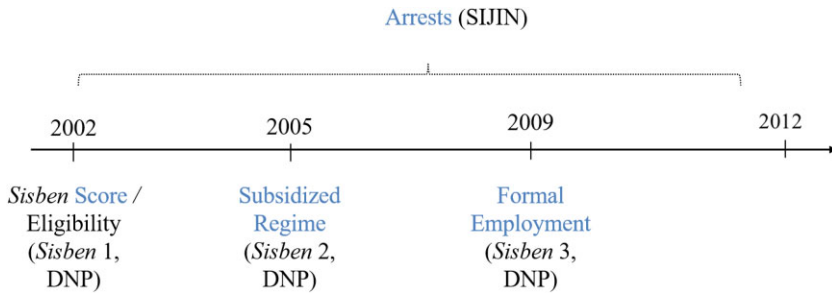


Fig. 1. Timeline and Sources of Data Used for Primary Variables.

Table 1. Summary Statistics in 2002.

Variable	Complete sample		Young males	
	Mean	SD	Mean	SD
<i>Individual characteristics</i>				
Male	0.490	0.500	1.000	0.000
Subsidised regime	0.319	0.466	0.355	0.478
Contributive regime	0.228	0.420	0.187	0.390
Age 10–15	0.105	0.306	1.000	0.000
Age 15–20	0.105	0.306	0.000	0.000
Age 20–25	0.089	0.285	0.000	0.000
Age 25–30	0.068	0.251	0.000	0.000
Ever arrested (2002–12)	0.062	0.242	0.154	0.361
<i>Household head characteristics</i>				
Female	0.394	0.488	0.405	0.491
Employed	0.628	0.483	0.701	0.458
Unemployed	0.104	0.305	0.099	0.298
Married	0.337	0.473	0.358	0.479
Attending school	0.009	0.095	0.007	0.083
Contributive regime	0.206	0.405	0.189	0.391
Age	43.463	14.358	43.609	9.901
Years of education	4.498	2.454	4.463	2.412
Owens house	0.318	0.466	0.344	0.475
Sisben stratum 1	0.276	0.447	0.295	0.456
Sisben stratum 2	0.616	0.486	0.606	0.489
Sisben score	45.630	9.962	44.983	9.567
Number of members in household	4.058	1.724	4.700	1.708
Observations	1,161,447		61,825	

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

perpetrator to be gang affiliated. In the *Sisben* we observe the location of residence (which we use for neighbourhood fixed effects).

2.1. Classifying Crimes

We classify the crimes into three categories—violent, property and drug crimes—based on the US Bureau of Justice Statistics' classifications (BJS, 1994). We use the detailed description of the act (e.g., distribution/trafficking cocaine) to categorise the crimes (e.g., drug crimes).

We distinguish between LACE crimes and non-LACE crimes. To that end, we worked closely with senior police officials in Medellín to divide our crimes into LACE, and those more likely to reflect impulse, passion or opportunity (non-LACE). For about 30% of our data, the police used a system that flagged the arrest with whether the individual was known to be part of an organised criminal enterprise or not, as well as information on the specific organisation to which the individual belonged. This organisational affiliation was based on extensive police intelligence and follow-up interviews. The data span 284 street gangs, urban militias, *narcotraficante* (drug distributors linked to gangs) and other organised criminal entities.

The police discontinued this system after a period of time. As the gang flags are not available for the entirety of our period, we use the patterns revealed over the subset of arrests for which the flag is available to classify the full sample of arrests. Police officials advised us that the best way to classify arrests is along two dimensions: (1) the crime and (2) the location. Accordingly, in our main analysis, we classify a crime as LACE if more than 30% of recorded arrests for that crime had the gang flag. We use 30% as that is the fraction of the sample under which the police were using the flags, but our results are similar when using the median as the cutoff. As a result of this exercise, for example, we classify homicide arrests as violent LACE, and rape or domestic violence as violent non-LACE.

In robustness checks, we use a method that relies on the association between these crimes and historically high-gang neighbourhoods. In this alternative definition, we classify crimes as LACE if they are more likely (above the median) to list any of these high-gang neighbourhoods as the location of arrest; however, our results are similar when using other cutoffs from 30% to 60% of arrests. Thirdly, we also use methods that predict the likelihood that the crime is LACE given observables, after training it on the sample that reports gang flags. Here we use *k*-means logit to predict LACE crimes based on neighbourhood, crime and individual characteristics.

While neither classification is perfect, the robustness across classification methods helps to validate the exercise. Additionally, using the crime-level classification (rather than the individual flags) of LACE 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).

In Table 2, we categorise the 25 (of 103) most prevalent crimes under each classification method. These data-driven methods line up with our priors: homicides, car theft, extortion, kidnapping, break-ins and the manufacturing, delivery and trafficking of drugs fall under LACE crimes. The remaining crimes are often thought of as crimes of impulse or opportunity (like rape, simple assault and drug consumption). The advantage of these classification approaches is that they are purely data driven. We also show the division between LACE and non-LACE crimes for each category. About 53% of arrests are for LACE crimes.

3. Empirical Strategy

As only individuals with a household *Sisben* I (2002) score below 47 could qualify for SR, we compare individuals on either side of the cutoff to identify the effect of SR eligibility. First, we verify that there is a discontinuity in the probability of SR enrolment at the cutoff, and then we examine how the likelihood of being in the formal sector changes at the cutoff.

We normalise the *Sisben* score so that treated units are individuals with positive values of our new score. We use both parametric and non-parametric approaches to estimate the effect of SR eligibility at the cutoff. For the parametric approach, we follow Hahn *et al.* (2001), and estimate

Table 2. *Top 25 Crimes (of 103) by Data-driven LACE Classifications.*

Crime	Gang flags	Neighbourhood def	Crime incidence (%)
Drug crimes			46
Non-LACE			19
LACE			27
Drug consumption/possession	No	No	26
Trafficking/distribution—marijuana	No	No	15
Trafficking/distribution	Yes	Yes	50
Trafficking/distribution—cocaine	Yes	Yes	8
Trafficking/distribution heroin	Yes	Yes	0.1
Property crime			28
Non-LACE			8
LACE			20
Use of fake Identification/documents	No	No	9
Motor vehicle theft (motorcycles)	No	No	4
Receiving illegal goods	No	No	4
Receiving bribes (as officials)	No	No	0.3
Illegal public monopoly activity	No	No	2
Copyright/fraud	No	No	5
Identity theft	No	No	0.3
Fraud	No	No	3
Theft/assault	Yes	Yes	34
Robbery (to businesses, firms)	Yes	Yes	17
Property vandalism	Yes	Yes	11
Motor vehicle theft—cars	Yes	Yes	5
Burglary	Yes	Yes	3
Violent crime			24
Non-LACE			18
LACE			7
Simple assault/battery	No	No	2
Rape/sexual assault	No	No	4
Personal injuries	No	No	13
Domestic/family violence	No	No	18
Conspiracy to commit murder	Yes	No	3
Homicide	Yes	Yes	10
Extortion	Yes	Yes	5
Assault/battery—against police	Yes	Yes	3
Manufacture, trafficking firearms	Yes	Yes	37
Intimidation and stalking	Yes	Yes	0.3
Terrorism	Yes	Yes	0.4
Kidnapping	Yes	Yes	2

Note: List of top crimes by type and enterprise classification, out of 103 crimes. LACE crimes are those 'likely associated with criminal enterprises' and, as such, most reflective of individual sorting into criminal occupations. Non-LACE crimes are the remaining, more likely representing crimes of impulse or opportunity. The 'gang flags' method lists whether the crime has a high propensity to receive a police reported flag of known gang affiliation at the time of arrest. The 'neighbourhood method' classifies crimes that have a high propensity to be in neighbourhoods known to have high gang activity.

the equation

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

where A_i is a vector of smooth polynomial functions of the *Sisben* score of each individual, $s_{i,n}$, and the $Y_{i,n}$ are our various outcomes (SR enrolment, formal employment, crime, etc.). Here $X_{i,n}$ is a vector of demographic baseline characteristics for individual i living in neighbourhood n ; μ_n corresponds to neighbourhood fixed effects for the 249 neighbourhoods. We control for various

characteristics of the household head in 2002, the baseline year, including an indicator for female-headed households, employment status, years of education, marital status, academic attendance, year-of-birth fixed effects, socioeconomic strata of the household and home ownership. We also show results without controls, and then with additional controls for community characteristics interacted with the *Sisben* cutoff.

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. 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). The optimal bandwidths lie between 5.5 and 7 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.

We devote Online Appendix B to examine balance tests, density tests and differential match rates across *Sisben* rounds. We show balance in baseline covariates, high (and non-discontinuous) match rates across *Sisben* rounds and smooth densities across the cutoff.

4. SR Enrolment, Employment, Education and Income

Online Appendix Figure A2 presents the discontinuity in the probability of SR enrolment using the optimal binning procedure in Calonico *et al.* (2014a). We show results for a broad span of the *Sisben* score, and bandwidth of 10 points. The probability of (individual-level) enrolment discontinuously increases by around 28 percentage points. 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. Not all eligible persons enrol in SR, as formal-sector jobs may be valuable to some, but enrolment still jumps substantially to 42% at the cutoff.

Table 3 shows the impact on SR enrolment, formal employment, education and household income for our focused sample of male youth. Panel A confirms the 28 percentage point increase in SR enrolment shown in Online Appendix Figure A2. As we vary the bandwidths from 4 through 10, the coefficient is stable, and both economically and statistically significant.

Next, we reproduce a well-established result and show that SR criteria had a negative effect on formal employment (Gaviria *et al.*, 2007; Santamaria *et al.*, 2008; Joumard and Londono, 2013; Camacho *et al.*, 2014). Figure 2(a) and panel B of Table 3 capture 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 the 2009 *Sisben* III survey, and earning above the minimum wage.³ Panel B of Table 3 shows a statistically significant reduction in reported formal employment for our sample of interest (young men) in 2009. If we were to take the ratios of the coefficients in panels A and B, we see that enrolling in SR is associated with between a 4 and 6.4 percentage point reduction in the probability of being employed in the formal sector.⁴

³ While this is a somewhat conservative measure of formal employment, paying contributions to health insurance is widely used as a measure of formal employment in Colombia (see Attanasio *et al.*, 2017 and Morales and Medina, 2017). The *Sisben* does not explicitly ask households whether members are in the formal sector.

⁴ Note that we should not necessarily think of this as an IV-2SLS exercise, as crime and formal employment choices are jointly determined.

Table 3. *SR Enrolment, Employment, Education and Income at the Sisben Cutoff for Young Males.*

Bandwidth	4	6	10
<i>Panel A: enrolment in SR</i>			
Above cutoff	0.276*** (0.0266)	0.276*** (0.0216)	0.281*** (0.0174)
Observations	21,720	29,235	39,877
Sample mean			0.394
<i>Panel B: formal employment</i>			
Above cutoff	-0.0172* (0.0092)	-0.0179** (0.0831)	-0.0112* (0.0066)
Observations	21,201	28,502	39,364
Sample mean			0.0896
<i>Panel C: education enrolment</i>			
Above cutoff	-0.0271** (0.0110)	-0.0218** (0.0100)	-0.0207** (0.0073)
Observations	22,125	29,777	40,641
Sample mean			0.338
<i>Panel D: household income (USD)</i>			
Above cutoff	8.195 (7.893)	0.758 (5.112)	36.09 (30.24)
Observations	14,914	20,092	27,712
Sample mean			257.4

Note: Standard errors are given in parentheses. Here ***, ** and * denote significance at 1%, 5% and 10%, respectively. Sample of males, cohort that were 13–18 years in 2005. The table reports reduced-form coefficients of being above the eligibility cutoff. The Sisben score is measured in 2002. We normalise the Sisben score to be centred around the eligibility cutoff where positive values of the score represent eligibility for SR. Regressions control linearly for the Sisben score, flexibly around the cutoff. Individual-level SR enrolment as measured in the 2005 *Sisben* survey. Individual-level education enrolment as measured in the 2007 *Matricula en Linea* data. Individual-level formal employment and household-level income measured in the 2009 *Sisben* survey. Standard errors are clustered at the *comuna* level.

Online Appendix Table A1 performs a number of robustness checks. First, in panel A we replicate the same result for comparison, while panel B includes all age cohorts. Panel C measures formal employment using administrative data from the 2010 *Planilla Integrada de Liquidación de Aportes* (PILA), which contains information on formal-sector firms and workers (Ministry of Health, 2019). Finally, in panel D, we are unable to statistically detect a meaningful effect on women, perhaps suggesting that males have an outside option in organised crime.⁵

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 out of formal work at the cutoff.

We also examine human capital investments to better investigate our mechanisms. One possible implication of SR eligibility would be that individuals are less likely to acquire education if they

⁵ Gender differences may also reflect differences in formal-sector options. Yet, we should note that data from the Gran Encuesta Integrada de Hogares (GEIH) shows that the formality rates (as a proportion of employed individuals, across all incomes groups) for men are about 52%, whereas for women are 46%; and formality rates as a fraction of the total population are 27.9% for men and 18.8% for women (National Administrative Department of Statistics, 2020).

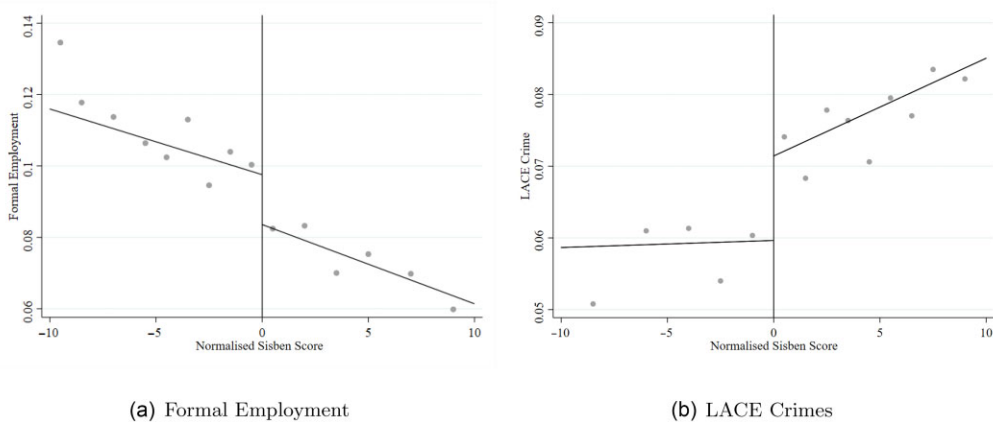


Fig. 2. Discontinuity in Formal Employment and Crime for Young Men.

Notes: RD graph using optimal binning procedure discussed in Calonico *et al.* (2014a). Normalised Sisben (2002) score on the horizontal axis centred around the cutoff. Higher values represent lower scores (higher poverty). LACE crimes are those 'likely associated with criminal enterprises', as determined by the data-driven classifications summarised in Table 2, and reflect individual sorting into criminal occupations. Formal employment is based on measures in the 2009 Sisben survey. Subsample of young males.

are unlikely to join the formal sector. We link individual-level school data (*Matricula en Linea*) to the *Sisben* and examine school enrolment for our cohorts (Secretary of Education, 2019).⁶ Panel C of Table 3 shows that school enrolment is indeed discontinuously lower for youth eligible for SR.⁷

Finally, in panel D of Table 3 we study incomes (from all sources, not just salaries) in the *Sisben* survey. The impact on household-level income is statistically indistinguishable from zero and economically small. These results suggest that, even as workers drop out of the formal sector, they find replacement sources of income. Note, if anything, we may particularly expect that incomes from illicit activities would be under-reported rather than over-reported. Accordingly, the absence of a negative impact on income might suggest that desperation is less likely to be driving any increase in criminal activity we document below.

The results of this section confirm that the program encouraged informality among young men. The obvious question that this raises is how such discouragement of formal-sector employment affects the likelihood of criminal activity.

⁶ *Matricula en Linea* (ML) provides us with detailed information on the students who were enrolled in schools in 2007, for elementary (primary, grades 1 to 5) and secondary high (grades 6 to 11) schools. The ML information system was a monitoring mechanism for schools in Medellín, implemented by the local government. We have information about the ID number, names, age and other socioeconomic variables.

⁷ We may expect that the family structure determines the sensitivity to SR eligibility. For instance, we may expect that the results are muted for individuals who have adult family members already in the formal sector, as they are unlikely to be eligible for SR (if they are dependants), and so less likely to change employment decisions. In additional analysis, we fail to detect any impact on youth who have an adult family member working in the formal sector. Though encouraging, we do not present these results here for the sake of brevity and because we believe one should interpret these results with caution, as formal status is an endogenous choice.

5. Impacts on Crime

We turn our attention to outcomes on crime, where we measure crime cumulatively over a decade. We interpret the impacts on crime as causally related to the incentives to leave the formal sector. Note that, by the latter half of this period, almost everyone had health care (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.

A literature on neighbourhood effects and crime (Cullen *et al.*, 2006; Dustmann and Damm, 2014) highlight the perils of using area-based relationships (like local recessions) to study individual-level occupational choice, and re-iterates the strength of our individual-level variation. Indeed, policing, returns to crime and non-monetary benefits vary across neighbourhoods. We cluster standard errors at the *comuna* level, but everywhere our results are robust to clustering at other geographic levels, like smaller neighbourhoods (*barrios*) or households.

We present results for total, violent, property and drug-related crimes, dividing each group between crimes ‘likely associated with criminal enterprises’ (LACE) and those more likely to reflect impulse or opportunity (non-LACE). We hypothesise that enterprise-related crimes are reflective of choice between legitimate and illegitimate sectors, especially in such a setting with high levels of organised crime. In contrast, crimes of impulse/passion should be less affected by incentives for remaining informal.

As we elaborate below, over and above a falsification test, the lack of effects on non-LACE crimes also allows us to rule out alternative mechanisms. We do not classify crimes based on whether or not they are pecuniary, as that would capture crimes of desperation and necessity that arise out of poverty. Instead, we posit that the policy induced work decisions, and as such, use activities associated with criminal enterprises as a basis for classification. Alternative mechanisms (such as riskier behaviour when having insurance) may have weight if non-LACE crimes rose as well, but the lack of effects on non-LACE crimes allows us to rule them out.

We first start with the probability of being arrested for any LACE crime. Figure 2(b) shows a sharp and discontinuous increase in all LACE crime at the *Sisben* cutoff. The upward slope reflects the possibility that poorer individuals engage in more organised crime, perhaps out of necessity and the lack of meaningful resources that allow them to participate in legitimate activities. Yet, the effect we focus on is the discontinuous increase at the SR eligibility cutoff.

The discontinuity is corroborated by the left-hand side of panel A of Table 4, where we detect an increase in the arrest rate for all LACE crimes. On the right-hand side of panel A, we fail to detect any meaningful increase in non-LACE crimes. If anything, there may be a small decrease in non-LACE crimes, perhaps as, if individuals are more likely to engage with organised criminal entities, their first arrest is less likely to be in non-LACE crimes. These patterns are similar across bandwidths, and the difference between coefficients for LACE and non-LACE crimes are statistically different for all bandwidths.

Next we separately examine violent, property or drug crime. In panel B of Table 4 and Online Appendix Figure A3a we present results for crimes related to violent criminal activities. Based on the police flags for gang-related activity, violent LACE crimes include homicides, extortion, kidnapping and firearms trafficking. Non-LACE violent crimes include domestic violence and rape. Online Appendix Figure A3a shows the jump in arrests for violent LACE crimes at the *Sisben* cutoff. 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 0.385 percentage point increase) in violent crime arrests from the mean around the cutoff. These magnitudes are both

Table 4. *Effects on Crimes.*

Bandwidths	LACE crimes			Non-LACE crimes		
	4	6	10	4	6	10
<i>Panel A: all crimes</i>						
Above cutoff	0.0219** (0.00813)	0.0164** (0.00655)	0.0110 (0.00660)	-0.0104 (0.00874)	-0.0133 (0.00784)	-0.00940* (0.00505)
Sample mean			0.0709			0.1126
<i>Panel B: violent crimes</i>						
Above cutoff	0.00624*** (0.00199)	0.00563** (0.00210)	0.00385*** (0.00129)	0.00176 (0.00372)	0.000532 (0.00262)	-0.000609 (0.00300)
Sample mean			0.0118			0.0287
<i>Panel C: property crimes</i>						
Above cutoff	0.00899** (0.00340)	0.00758** (0.00313)	0.00571** (0.00267)	-0.00277 (0.00449)	-0.00188 (0.00354)	-0.00182 (0.00302)
Sample mean			0.0268			0.0201
<i>Panel D: drug crimes</i>						
Above cutoff	0.00666 (0.00616)	0.00322 (0.00412)	0.00148 (0.00375)	-0.00940 (0.00706)	-0.0120 (0.00720)	-0.00696 (0.00543)
Sample mean			0.0324			0.0638
Observations	21,720	29,235	39,877	21,720	29,235	39,877

Note: Standard errors are given in parentheses. Here ***, ** and * denote significance at 1%, 5% and 10%, respectively. LACE crimes are those 'likely associated with criminal enterprises', as determined by the data-driven classifications summarised in Table 2, and as such, most reflective of individual sorting into criminal occupations. Non-LACE crimes are the remaining, more likely representing crimes of impulse or opportunity. The tables reports reduced-form coefficients of being above the eligibility cutoff. The Sisben score is measured in 2002. We normalise the Sisben score to be centred around the eligibility cutoff where positive values of the score represent eligibility for SR. Crime data are from 2002 to 2012. Regressions control linearly for the Sisben score, flexibly around the cutoff. We consider only males who were between 13 to 18 years old in 2005. We cluster standard errors by *comuna*.

economically meaningful and similar to those from recent studies in this context (Khanna *et al.*, 2021). We do not find any meaningful effect on the probability of arrest for non-LACE violent crimes.

In Online Appendix Figure A3b and panel C of Table 4 we analyse the effects on property crimes. LACE property crimes include motor vehicle theft, and burglary of businesses and residences. Crimes like fraud and identify theft are classified as non-LACE. Once again, we see that LACE property crimes increase, with little change to non-LACE property crimes. The reduced-form estimate, over the entire decade, constitutes a 38.5% increase (or a 0.57 percentage point increase) from the mean around the cutoff within a bandwidth of 10 points. We find no strong effect for property crimes less associated with organised entities.

It is interesting to note that many non-LACE property crimes may also be income generating (even if they do not reflect occupational choices), and as such, impacts on these non-LACE crimes may be consistent with early economic models of crime (Becker, 1968). Yet, we find that it is the decision to join a criminal enterprise that seems to be the driving force. This is consistent with

the anthropological interviews discussed in Online Appendix D1, which illustrate how gangs and organised criminal entities recruit idle youth, stressing that working for a gang is lucrative. The difference in coefficients between LACE and non-LACE crimes are statistically different for all bandwidths.

In Online Appendix Figure A3c, even though the discontinuity in LACE drug crimes is minor, there is a change in the slope of the relationship. In panel D of Table 4 the direction of effects is what we may expect, but our results are imprecise. LACE drug crimes include the manufacturing, distribution and trafficking of hard drugs like cocaine and heroin. Non-LACE drug crimes include possession and consumption of marijuana, usually for personal recreational use.

One possibility for the reduced precision in arrests for drug crimes is error associated with the classification of such crimes. That is, the difficulty in classifying possession of drugs as consumption versus trafficking or distribution likely introduces noise for administrative data reasons. Indeed, offences related to the trafficking of marijuana are problematic, as small amounts of personal possession were not prosecuted during this period. While homicides, assaults and theft involve victims as clear evidence of crimes, 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 subsidised benefits for informal workers raised the likelihood of being arrested for LACE violent and property crimes. 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-LACE crimes of each type did not rise, ruling out many alternative mechanisms that we discuss in greater detail in Section 6. These include changes in riskier behaviour by individuals, or police targeting arrests to informal workers.

5.1. Robustness

We conduct a number of robustness checks. First, we re-classify crimes into LACE and non-LACE groups based on the *location* where these types of crimes are more likely to occur. We sort the crimes by the fraction of first observed arrests that happen in a high-gang neighbourhood. The top half of this list is classified as LACE crimes. Online Appendix Table A2 shows similar results, but with the added statistical significance of drug crimes. Additionally, we use machine-learning methods to train our data on the sample that has the police classification, and use *k*-means logit to predict the likelihood that the crime committed was a LACE crime based on individual, crime and neighbourhood characteristics. We then weight by the propensity to be a LACE crime. Online Appendix Table A3 shows qualitatively similar patterns to our main results.

Next, we re-examine our main results using the bias-correction methods suggested by Calonico *et al.* (2014a). In Online Appendix Table A4, we show robust results that conduct a polynomial bias correction at a larger bias-correction bandwidth (reported in the table). In Online Appendix Table A5, we cluster standard errors at the household level, and our results remain similar to before.

In our main specifications, we include the entire sample of young men. If SR induces one to be more likely to be first arrested for a LACE crime, this may mean a slightly lower likelihood that one is arrested for a non-LACE crime. We first exclude samples where the first arrest was a non-LACE (for LACE outcomes) or LACE (for non-LACE outcomes) crime (Online Appendix

Table A6). Finally, we exclude individuals whose first arrests are any other crime at all (Online Appendix Table A7). All of these changes in the sample produce similar results.

In Online Appendix Figure A5, we vary the bandwidth through a much wider range—every integer between 2 and 10. We also examine the robustness to varying the cohorts under analysis. In our main specifications, we follow the cohort of 13–18 year-olds in 2005. In Online Appendix Figure A6, we expand the range of ages under study, examining impacts on those as young as 10 in 2005 (or 18 in 2013). We are reluctant to study older ages, as we would not have previous arrests for them if they were ever arrested before 2002.

In other checks, we include repeat arrests (Online Appendix Table A8) and find similar results. Finally, as our hypotheses involve the intersection of both informality and violent crime, we present a specification in Online Appendix Table A9 that simultaneously captures both. Our individual-level data can measure both employment and criminal behaviour for the same individual. 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 LACE criminal activity.

In Online Appendix Table A10 we look at impacts on formal employment and crime for older cohorts (up to 50 years old in 2005). There is a similar fall in formal employment at the cutoff; but, with the exception of property crime, there is no detectable corresponding increase in crimes.

While we already show results with neighbourhood fixed effects, the right panel of Online Appendix Table A11 controls for pre-treatment covariates and community characteristics interacted with the treatment indicator. The left panel shows that excluding all controls produces similar results.

5.2. *The Importance of Neighbourhoods*

Previous studies emphasise that opportunities in a neighbourhood affect how easy it is to induce youth into crime (Kling *et al.*, 2005). High-crime neighbourhoods 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 LACE arrests at the RD cutoff. If the policy induces men to join organised crime, then we may expect that neighbourhoods that have more such opportunities would have a larger impact. Online Appendix Figure A4 shows the spatial distribution of arrests, by type of crime.

We divide *comunas* of residence at the median value of the share of gang crimes recorded by the police (i.e., arrests with gang flags as a ratio of total crimes), and create an indicator for whether individuals lived in these *comunas* in 2002. These are not necessarily high-crime areas, but rather have a higher share of crimes classified as LACE rather than non-LACE crimes.

We interact this variable with the cutoff to analyse the heterogeneity in effects by area-level gang activity. Table 5 presents the results. The effects on crime are present in both high and low gang activity areas, but for violent crime are larger in areas that have more gang activity.

Online Appendix Table A12 shows the results for non-LACE crimes. Again, there is no evidence of SR enrolment being associated with non-LACE crimes in either gang *comunas* nor non-gang *comunas*. Note that our identification strategy protects against increases in policing activity in gang *comunas*, as we are comparing one side of the *Sisben* cutoff to the other.

Table 5. *Heterogeneity by Comuna.*

Bandwidth	4	6	10
<i>Panel A: LACE crimes</i>			
Cutoff × gang comuna	0.0260** (0.0107)	0.0229** (0.00919)	0.0179* (0.00872)
Cutoff × non-gang comuna	0.0179** (0.00651)	0.00967 (0.00606)	0.00466 (0.00549)
<i>p</i> -value of the difference	0.351	0.130	0.057
Sample mean			0.0709
<i>Panel B: violent LACE crimes</i>			
Cutoff × gang comuna	0.00795*** (0.00235)	0.00739*** (0.00223)	0.00615*** (0.00122)
Cutoff × non-gang comuna	0.00460* (0.00236)	0.00362 (0.00249)	0.00127 (0.00179)
<i>p</i> -value of the difference	0.057	0.015	0.000
Sample mean			0.0118
<i>Panel C: property LACE crimes</i>			
Cutoff × gang comuna	0.00927* (0.00525)	0.0111* (0.00534)	0.00809* (0.00416)
Cutoff × non-gang comuna	0.00900*** (0.00250)	0.00431 (0.00264)	0.00387* (0.00211)
<i>p</i> -value of the difference	0.956	0.177	0.318
Sample mean			0.0268
<i>Panel D: drug LACE crimes</i>			
Cutoff × gang comuna	0.00874 (0.00723)	0.00445 (0.00548)	0.00365 (0.00518)
Cutoff × non-gang comuna	0.00426 (0.00539)	0.00174 (0.00347)	−0.000487 (0.00321)
<i>p</i> -value of the difference	0.344	0.579	0.365
Sample mean			0.0324
Observations	21,720	29,235	39,877

Note: Standard errors are given in parentheses. Here ***, ** and * denote significance at 1%, 5% and 10%, respectively. LACE crimes are those 'likely associated with criminal enterprises', as determined by the data-driven classifications summarised in Table 2, and as such, most reflective of individual sorting into criminal occupations. The table reports reduced-form coefficients of being above the eligibility cutoff interacted with whether or not the individual lives in a gang comuna. The Sisben score is measured in 2002. We normalise the Sisben score to be centred around the eligibility cutoff where positive values of the score represent eligibility for SR. Crime data are from 2002 to 2012. Regressions control linearly for the Sisben score, flexibly around the cutoff. We also control for the interaction between the formal employment rate in the comuna and the cutoff. We consider only males who were between 13 to 18 years old in 2005. We cluster standard errors by *comuna*. See Online Appendix Table A12 for the non-LACE crimes.

6. Alternative Mechanisms

The coincident decrease in formal-sector employment and rise in LACE-related arrests suggests disincentives to formal employment from the health benefits program leads to greater probability of employment for the organised criminal entities prevalent in this context. 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 neighbourhoods with idle time, and are not in the formal sector (Baird, 2011).

We discuss three of the most likely alternative theories in light of the full pattern of empirical results. Overall, we argue that the distinction between effects on LACE and non-LACE crimes powerfully helps to address these alternative interpretations. First, better health benefits at the cutoff may induce one to engage in riskier behaviours, which in turn leads to higher probability of arrest. However, it is difficult to support why these riskier behaviours would not also (or even primarily) include non-LACE 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 nearly identical coverage, with the only difference being who pays. If anything, any early differences in coverage favoured better care for formal employees, which would lead to effects of the opposite sign of what we find.

Second, formal workers vesting more into the health system may fear losing their jobs if arrested. As a result, they may throttle any criminal activity once formally employed. However, again, this should be just as true for all criminal behaviour, not only for LACE crimes.

Finally, the police may be more likely to falsely target informal workers, leading to a sharp rise in the probability of arrest for workers who become informal at the cutoff. But police are unlikely to know any particular individual’s Sisben score or formal status. Furthermore, it may be easier to fabricate petty (non-LACE) crime rather than more serious offences like homicide or auto theft. Finally, 91% of the arrests occur *in flagrante* (in the act) and so are unlikely to be fabricated or the result of targeting an individual. In Online Appendix Table A13 we demonstrate robustness of all our main results when restricting the sample to the subset of *in flagrante* crimes.

7. Conclusion

In this paper, we hypothesise that disincentivising formal employment by way of eligibility criteria for generous social benefit programs can lead to substantial increases in criminal activity when informal opportunities include employment by criminal enterprises. We evaluate this claim in Medellín, Colombia, a setting with a high prevalence of organised criminal entities. We first provide strong evidence showing that the criteria behind the health benefits policy unintentionally led to a sharp decrease in formal-sector employment.

In Medellín, this informal market contains significant opportunities related to organised crime. We follow these individuals over a decade and show that this decrease in formal employment led to an increase in the probability of being arrested for criminal-enterprise activities. On the other hand, crimes less likely to be associated with criminal enterprises, like crimes of impulse or opportunity, show no such impacts at the eligibility threshold. These effects were largest in neighbourhoods that had, at baseline, greater opportunities for organised crime.

We conclude that Colombia’s well-intentioned and broad-based subsidies for health care had the unintended consequence of incentivising participation in criminal enterprise by way of its distortionary rules. The program being important for providing subsidised 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 is distortionary, and as such, warrants examination. Recognising these adverse effects, policy-makers lowered the costs of CR enrolment at the end of our study period (Law 1607), leading to significant increases in formal-sector employment (Bernal *et al.*, 2017; Kugler *et al.*, 2017; Morales and Medina, 2017).

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 are far reaching: less crime, less policing and incarceration, and fewer negative externalities on families and children. This has welfare implications for the design of many programs across the developing world that often have far-reaching and under-studied consequences on seemingly unrelated outcomes and behaviours. 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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Additional Supporting Information may be found in the online version of this article:

Online Appendix Replication Package

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