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GANG RULE: UNDERSTANDING AND COUNTERING CRIMINAL GOVERNANCE

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### **ABSTRACT**

Gangs govern millions worldwide. Why rule? And how do they respond to states? Many argue that criminal rule provides protection when states do not, and that increasing state services could crowd gangs out. We began by interviewing leaders from 30 criminal groups in Medellín. The conventional view overlooks gangs' indirect incentives to rule: governing keeps police out and fosters civilian loyalty, protecting other business lines. We present a model of duopolistic competition with returns to loyalty and show under what conditions exogenous changes to state protection causes gangs to change governance levels. We run the first gang-level field experiment, intensifying city governance in select neighborhoods for two years. We see no decrease in gang rule. We also examine a quasi-experiment. New borders in Medellín created discontinuities in access to government services for 30 years. Gangs responded to greater state rule by governing more. We propose alternatives for countering criminal governance.

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# 1 Introduction

Economic and social interactions require security, enforcement of rules, and dispute resolution. Throughout history, groups specialized in coercion have provided these services, often extracting taxes, fees, and other rents as well. Some liken the origin of states to organized crime and the market for protection (Tilly, 1985; Olson, 1993; Grossman, 1996; Acemoglu and Robinson, 2006; North et al., 2009; Sánchez De La Sierra, 2020).

Despite the rise of nation-states, people continue to turn to criminal organizations for protection today. In cities worldwide, street gangs, mafias, and drug cartels commonly govern civilians and collect taxes. Such criminal governance is prominent in Latin America, especially Brazil, Central America, Jamaica, Mexico, and Colombia. But we can also find it in Italy, the United Kingdom, India, South Africa, 19th-century American cities, and contemporary American prison systems. Hundreds of millions of people currently live under some form of criminal rule (Arias, 2006; Lessing et al., 2019; Lessing, 2020; Melnikov et al., 2020).

What this means is that, in many places, including the most developed countries, states lack a monopoly on coercion and protection within certain areas. Instead, residents of these areas live under a duopoly of violence (Skaperdas and Syropoulos, 1997). This paper asks three questions: How does this duopoly function? What motivates gangs to rule? And what (if anything) can a government do to diminish it?

To answer these questions, we study hundreds of gangs in a city. We developed relationships with dozens of criminal group members and interviewed them about their motives, operation, and groups. We surveyed city residents to measure state and criminal governance systematically. These data reveal some unexpected drivers of criminal rule, as well as possibly unintended consequences of state actions. To test their influence, we identified quasi-experimental variation in exposure to long-run state services. We also conducted the first gang-level randomized trial to assess how criminal groups respond to increased projection of state power.<sup>1</sup>

A common view is that criminal governance flourishes in vacuums of state power (Gambetta, 1996; Skaperdas, 2001; Skarbek, 2011). A possible policy implication is that states can crowd out gang rule by improving the quality and reach of their services, eventually restoring a monopoly on protection and coercion. Our findings challenge this view.

We do find that protection is a good that gangs provide and charge for in competition with the state, and that residents view gang and state services as substitutes. The logic

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<sup>1</sup>A Campbell systematic review of anti-gang interventions outside the OECD found that the entirety of the literature was just four small case studies (Higginson et al., 2015).

of duopolistic competition suggests that gangs thus have market incentives to reduce the quantity of protection they supply in response to a state increase in governance services. But gang leaders described powerful countervailing motives. State presence threatens other illegal activities, like drug selling. Providing order reduces calls for state presence, and fosters civilian loyalty. This protects the criminal group's other rents.

As a consequence, state efforts to expand services, crowd out gangs, and establish a monopoly on protection could have the opposite effect, driving gangs to increase rule. Thus, criminal governance may be as much a product of state strength as of weakness; in the presence of lucrative illegal markets (themselves created by state prohibition of drugs and other activities) criminal groups need to govern more when state presence threatens their broader rents.

Our study takes place in Colombia. Medellín, the second-largest city in the country, is prosperous, collects extensive tax revenues, and provides a considerable range of public goods and social services to its citizens. Nonetheless, virtually every low- and middle-income neighborhood in the city is occupied by one of more than 350 small gangs called *combos*, and practically every combo engages in at least some governance activities. These include: prohibiting and punishing property crime, settling disputes between neighbors, enforcing community rules, and—in exchange—taxing businesses and residents.

Criminal groups are clandestine and poorly understood, and so we began this study with large-scale, systematic interviews with criminal organizations. Whereas previous economist-ethnographer collaborations, such as Levitt and Venkatesh (2000) and Sánchez De la Sierra and Titeca (2020), gained extraordinary access to a single group, we develop sources in many groups at a more moderate depth. Over four years we interviewed dozens of leaders, managers, and foot soldiers in 30 criminal groups on their organization, operations, and rule. We also interviewed dozens of experts, police, prosecutors, and community leaders.

We then set out to develop systematic data on gang rule. We used our qualitative investigations to design a representative, city-wide survey of roughly 7,000 residents and businesses on their experience of gang and state governance, especially the provision of protection and the collection of taxes and extortion.

Together, these data show that, on average, the state is the predominant provider of protection, but the combo is seldom far behind. In dozens of neighborhoods, the combo is the leading provider. Indeed, combos choose whether or not to sell protection, and we find wide variation in the degree to which combos rule within Medellín. Those that do organize service provision and the collection of modest weekly “security fees” as a business line with dedicated staff.

Crucially, we also learned that gang leaders are concerned with maintaining citizen loyalty

and avoiding denunciation to the police. Several explained that they rule less for the direct profits and more because it protects their retail drug profits. Providing order reduces the need for routine patrols and special agents to enter the neighborhood. Also, fostering civilian loyalty reduces the chances citizens inform on them, and increases the chance they help the combo evade the authorities. In line with this, we find that the degree of combo and state rule are positively correlated—combos rule more when the state does so.

Drawing on these stylized facts, we develop a simple model of the relationship between criminal and state governance. In our lead example, we model the market for protection as a classic duopoly—two firms selling differentiated but substitutable services. We explore other approaches, including a model of competing stationary bandits. All these models share a prediction: if one actor’s optimal supply of governance rises, the other’s will fall—what we call “crowding out.” This means that exogenous increases in the state’s provision of protection will lead, in equilibrium, to decreases in gang provision, and vice versa. This logic is consistent with the conventional wisdom—that gang governance flourishes where states are weak—and the idea that state projections of power can reduce criminal rule.

To capture the others benefits that gang leaders described, we introduce to the model a term capturing returns to loyalty, one that depends on relative state–gang rule. The result is that increases in state protection can drive a strategic response by combos, thus “crowding in” gang protection. The relative influence of these countervailing forces determines the size and sign of a crucial comparative static: the optimal response by the gang to increases in state governance. Whether building state capacity crowds gangs in or out is an empirical matter.

To investigate, we assess two efforts to increase state services in Medellín. First, we evaluate the effects of a decades-long effort to increase state services within a set of newly-introduced internal city districts. Second, we study a large-scale experiment where the government heavily intensified state services in a random selection of small neighborhoods. We see the same striking result in both cases: increased protection and other services from the state did not crowd out criminal protection. On the contrary, in the longer run quasi-experiment, we see strong evidence that it crowded in gang rule. The elasticity is the opposite of what the conventional wisdom, the city government, and we initially expected.

The quasi-experiment takes advantage of a city bill that in 1987 reorganized the city into 16 large neighborhoods called *comunas*. Following this, the city expanded policing and some municipal services (such as local justice and family services) and organized their provision within these new *comunas*. Thus, street blocks on either side of the new internal borders were generally similar in demographic and economic terms, but for the next three decades were at different distances from their assigned police and municipal headquarters—almost

half a kilometer different at the median. We use this border discontinuity to estimate a causal effect of state proximity.

We find that being near the state raised services, and that combo rule increased in reaction. On blocks half a kilometer closer to the state, citizens reported about a fifth more state services, plus greater state legitimacy. These citizens also reported about a fifth *greater* combo rule.

For the experiment, we found a city-government outreach unit that had been intensely delivering city services to one neighborhood for several years, with anecdotal success. We worked with the city to scale it up to 40 out of 80 eligible neighborhoods with strong gang presence. These neighborhoods were small, usually just a few blocks. For 20 months, the city intensified attention to these areas roughly five-fold, including: (i) a full-time liaison to organize and communicate state services, refer people to state agencies, and coordinate city services to neighborhood needs; and (ii) regular meetings with city officials and agencies. There was no change in criminal-justice attention or policing strategies. Control neighborhoods received the normal level of city services.

We see no evidence the intervention reduced gang rule. If anything, the program slightly reduced perceptions of relative state-combo governance. Partly, the results could reflect the difficulties of expanding state capacity. Also, it may be hard to change general perceptions of state responsiveness in the span of just 20 months. But the results are also consistent with strategic response by combos, moderated perhaps by the fact that the intervention did not involve police. Either way, the fact remains: even in the neighborhoods where the intervention worked best, we find that a massive increase in street-level governance services failed to reduce criminal supply of protection. Our interviews with combo members and leaders corroborate these two evaluations, further suggesting that state presence provokes a strategic response by gangs to rule more.

Thus our results suggest that profits from governance as a business line are not most gangs' primary motive for rule. Rather, gangs rule in large part to protect themselves from state repression. This reflects a defining characteristic of criminal governance: it is almost always embedded within and subject to state power (Lessing, 2020). The conventional wisdom, that crime flourishes where the state is weak, overlooks this point, predicting crowding out. Instead, we see unintended, paradoxical, and frankly vexing policy effects. For example, while we find that state projection of power did not curtail organized crime, our interviews, model, and results suggest that it does give gangs strong incentives to win citizen loyalty through responsive governance and non-extortionary taxation. We discuss some of these challenges, trade-offs, and policy implications in the paper's conclusion.

These results fit into a few different literatures. First, economists have begun studying

the consequences of criminal rule. Melnikov et al. (2020) show how repressive gangs in San Salvador restrict local development. In eastern Congo, Sánchez De La Sierra (2020) shows how criminal warlords can bring a degree of order and prosperity. In this paper we try to understand why they rule and when they choose to be extractive. Extractive gangs may be a sign that the government is not competing for control or loyalty.

Second, the externalities from gang rule parallel a counter-insurgency literature arguing that rebel groups offer justice and welfare services to capture civilian “hearts and minds.”<sup>2</sup> This literature argues that military action plus state services can raise state legitimacy and crowd out insurgents (Berman and Matanock, 2015). Conceivably, however, these armed groups reacted differently because they have wider areas of operation. Combos have small, well-defined, traditional territories, which abut those of their rivals. Thus, we might expect urban gangs to resist localized state presence more strongly than rebels or drug cartels, since gangs have nowhere else to go. Indeed, Magaloni et al. (2020) find that police crackdowns in Rio’s favelas produced more violence in those favelas where gangs’ baseline rule was strongest. In contrast, organized criminals with a wider base of operations responded differently. Studying Mexican suppression of drug traffickers, for instance, Dell (2015) finds that trafficking and violence get displaced to less aggressive municipalities.

Third, our study complements the broader literature on organized crime in economics. This literature began with studies of the origins, internal organization, and incentives of these illegal firms and primitive states (Schelling, 1971; Fiorentini and Peltzman, 1997; Konrad and Skaperdas, 1998). More recently, there has been a surge of interest in international organized crime, including the personnel economics and career paths of gang members (Khanna et al., 2019; Sviatschi, 2018; Carvalho and Soares, 2016); studies of market structure and the production of violence (Castillo and Kronick, 2020; Brown et al., 2020; Bueno de Mesquita, 2020); the effects of exogenous supply and demand shocks on competition and violence levels (Castillo et al., 2020; Dube et al., 2016; Sobrino, 2019; Limodio, 2018); and the historical origins of drug cartels (Murphy and Rossi, 2020). There are also parallels between gangs strategically increasing rule in response to state presence, and a political economy literature on how organized criminals influence elections (De Feo and De Luca, 2017; Alesina et al., 2019; Dal Bó et al., 2006; Acemoglu et al., 2020).

Finally, methodologically, we demonstrate the usefulness of large-sample qualitative data in economics, especially in informal and clandestine settings. The field’s beliefs about organized crime are based on possibly unrepresentative groups, especially the Sicilian mafia and

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<sup>2</sup>Berman et al. (2011, 2013); Crost et al. (2016); Beath et al. (2012); Albertus and Kaplan (2013). This is part of a more widely-studied phenomenon of insurgent governance, when rebels try to foster civilian support not in a contest for illicit rents but for control of the state itself (Arjona, 2016; Kasfir, 2015).

Table 1: Interviews and focus groups by type of respondent

Respondent type		Total # of participants	# of interviews & focus groups	Total hours
Incarcerated subjects:				
Active or former combo		24	32	45
Active or former razón		12	19	27
Other illegal organization		3	3	2
Criminal actors	Subjects outside prison:			
	Active combo member	12	20	22
	Former combo member	11	16	16
	Active razón member	3	7	9
	Active faction member	1	1	2
	Other illegal organization	4	4	6
Community	Member	133	136	71
	Leader	14	18	14
Experts	Criminal group experts	9	12	14
	Other	1	1	1
Public servants	City officials	14	15	12
	Police (active or former)	13	15	18
	Prosecutor (active or former)	3	7	12
Total		257	306	271

one Chicago gang. Many economists already do informal qualitative work. We illustrate the importance of rigorous qualitative methods and ethnographer-economist collaborations. Many of our most important insights, our theory, the intervention design, and the interpretation of results all come directly from thorough, large-sample, systematically chosen and documented interviews and observation. Multi-method investigations such as this one are essential to understanding, regulating, and reducing complex problems like organized crime.

Section 2 describes the data and our qualitative methodology in more detail, Section 3 the context and our descriptive analysis of gang rule, Section 4 our illustrative model, Section 5 the quasi-experiment, Section 6 the field experiment, and Section 7 concludes.

## 2 Data and methods

### 2.1 Qualitative interviews

We interviewed 70 criminal leaders and members across 30 groups. This includes 17 combos as well as 13 higher-level, mafia-like organizations called *razones*, discussed below. Table 1 lists respondents by type and total interview length. Our highest-ranking sources are deputies to the most powerful crime bosses in the city. Most are lower in the hierarchy. Our interviews collected information on group history; territory; business lines; civilian governance; internal organization and performance incentives; career paths and exit options; inter-group relations; and violent conflicts.

This is a convenience sample of criminal actors who were willing to speak. We conducted



roughly half the interviews in prisons, typically in a wing reserved for high- and middle-ranking criminals. Most continue to run their group’s activities while imprisoned. In the beginning, prison wardens announced that anyone who would like to meet with university professors could meet us at a particular room and time. Following this, our sources might or might not continue to make appointments. Some referred us to additional sources. Because the prison affords little privacy, most interviews took place in public areas, out of earshot from most inmates or prison guards, but in public view, usually for 1–3 hours.

Eventually, we developed criminal contacts outside of prison. In contrast to the self-assured, accessible, and surprisingly candid leaders in prison, we found it difficult to speak to outside members, especially lower-ranking ones. Besides being more vulnerable (some are fugitives), they also often seemed to lack the experience, power, and confidence to feel safe speaking with academics. To improve access, we hired the city’s main organized-crime journalist as a consultant, to provide introductions as well as conduct his own interviews and analysis. We also hired a government gang outreach worker (himself a former *razón* member and former prison gang leader), who became a full-time research associate. He conducted structured interviews with criminal subjects in his personal network.

We believe our subjects spoke to us for several reasons. In prison, our interviews offered subjects a respite from routine and a chance to exhibit their expertise and insights. Interviews also posed little risk, since most subjects had already been prosecuted for the criminal activities they described. They were generally flattered by academic attention, and many harbored hopes of being the subject of books. Finally, some explained that with the end of Colombia’s civil conflict, they expected the government to turn to organized criminal groups with renewed intensity. They hope for a “peace process” that involves *sometimiento*—submission to justice and a surrender of some of their gains in exchange for a path to exit. They may have believed our research could facilitate this. Few policymakers understand what the gangs actually do, how they govern, and how influential they are. Thus, some criminal leaders viewed our study as reducing asymmetric information and facilitating negotiation. We accepted the possibility of playing this role only because we felt it was small, and because on balance it should reduce the possibility of state-gang violence.

Finally, we also interviewed local crime experts, members of the Metropolitan Police and the Attorney General’s office, and also obtained confidential internal law-enforcement reports. Our research assistants conducted more than 153 interviews with community leaders and members in around 108 neighborhoods, mainly on the subject of citizen interactions with organized criminals and use of their services and governing activities. We also returned during the coronavirus pandemic (and city-wide lockdown) to interview criminal group members and community leaders about gang governance during the crisis.

**Ethics and human subjects protections** We had several strategies for maintaining trust, safety, and confidentiality of criminal group members. Above all, we were transparent about our research aims, that we were speaking to other groups and the government, and that we advise the civilian government (but not the criminal justice system). With prison populations, of course, special care to obtain meaningful consent is essential. We took great efforts to ensure that our interviewees faced no pressure to speak to us. In prison, however, it is worth noting that our subjects are shrewd and powerful businessmen who in many respects are in control of their decisions and lives in the prison, if not the prison itself. With all our subjects, we also made every effort to preserve anonymity and confidentiality, while advising subjects in consent scripts of the potential limits to our ability to do so.

We consulted extensively with the University of Chicago and Universidad EAFIT human subjects committees, and we obtained written support and assurances of noninterference from the Mayor, the national head of the prison system, and the Colombian Minister of Justice. We also consulted with multiple journalists who specialize in organized crime, who related that they had never been asked by the criminal justice system to betray sources or materials. For this reason, in practice, we believed that speaking to us carried minimal risks to the leaders. Nonetheless, our consent scripts explicitly highlighted those risks.

**Qualitative methods** Amidst the large and growing amount of fieldwork and primary data collection in economics, informal qualitative research is now widespread. Though they seldom discuss it explicitly, today economists probably do more interviews and observation than ever before. A challenge with unstructured and unsystematic data, however, is that it is often confined to small samples, is not systematically documented or analyzed, and hence is subject to common biases of selection, recall, and salience. Just as there have been high returns to rigorous quantitative methods, there are surely advantages to better qualitative work, especially in informal economies, corrupt politics, armed groups, and crime.

There is, of course, an extensive anthropology and sociology literature that does precisely this, but much of this work rejects conventional economic theory.<sup>3</sup> We show how to combine systematic primary qualitative data with formal methods. Other examples, mainly using secondary sources, include case studies of the Sicilian mafia (Gambetta, 1996), New York mafia (Reuter, 1983), pirates (Leeson, 2007), and prison gangs (Skarbek, 2014; Lessing, 2017). Examples using primary materials include studies of a defunct Chicago gang (Levitt and Venkatesh, 2000), corrupt Congolese traffic police (Sánchez De la Sierra and Titeca, 2020), participant observations of US gangs (Sanchez-Jankowski, 1991), and records

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<sup>3</sup>Notable exceptions include a number of qualitative investigations of informal (not illicit) markets and communities (e.g., Bliss and Stern, 1982; Ensminger, 1996; Venkatesh, 2006).

of Brazil’s Primeiro Comando da Capital (Lessing and Denyer Willis, 2019).

We formed a collaboration between two economists and two ethnographers of crime, also employing a journalist and former razón member. We developed semi-structured interview guides, and adjusted them over time to investigate and informally test hypotheses and claims as we developed them. We recorded and transcribed these interviews when possible. When not possible, especially in prison, we took detailed notes and immediately wrote up the notes after the interview. We also sought to verify our observations with multiple sources. For most topics we discuss we have 2–3 sources between gang members and experts.

To organize the vast number of interviews, we created a private encrypted wiki we call *WikiCombo*. A collaborative wiki was a good fit for the networked, non-linear nature of the data, especially when collected by several contributors. We uploaded and encrypted all primary and secondary sources. We created inter-linked pages on key research themes, individual combos and people, neighborhoods, events such as conflicts, and so forth. Factual claims are linked directly to original transcripts. Every text change and its contributor is tracked and is reversible. Finally, the wiki is an ongoing collaborative tool and sources consented to have their anonymous comments shared with other researchers.

## 2.2 Survey data and measurement strategies

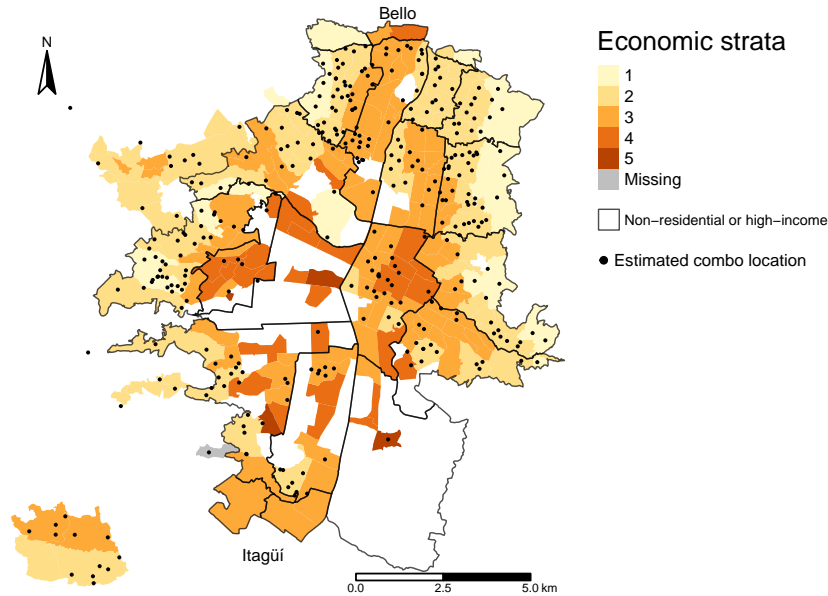
In 2019 we surveyed nearly 7,000 residents and businesses on the degree of state and combo rule, the perceived legitimacy of both, and levels of taxation and payments to combos. The survey was representative of all 223 low- and middle-income neighborhoods in Medellín, plus nine neighborhoods bordering Medellín in other municipalities (see Figure 1). We randomly sampled 2,347 of the city’s 14,600 blocks, stratified by neighborhood, then randomly sampled approximately four households and one business on each block.

**Addressing measurement error** Naturally, we are concerned that citizens may misreport gang activities. They may feel uncomfortable talking to outsiders or embarrassed to admit the role of the combo. If so, then the descriptive statistics below would underestimate the role and legitimacy of the combo.

Combos are a routine part of everyday life, however. We also refined survey questions after dozens of qualitative interviews, fine-tuning language, questions, and approach to elicit truthful answers. We conducted all interviews anonymously and in private, typically indoors. In the context of a secret interview, we believe most respondents answered questions freely and truthfully. Three analyses are consistent with this conclusion.

First, we can compare some of our measures against prior surveys to judge the efficacy of our approach. The city has run surveys in the past on “security fees” paid to the combo.

Figure 1: Combo census: Estimated locations, with barrio income level



City-wide, 19% of our business respondents and 7% of residents report making payments, with negligible non-response. A city survey conducted earlier in the same year found a 13% payment rate for households, with massive incompleteness.

Second, we used a survey experiment to assess under-reporting in security fee payment. We asked some respondents directly whether they paid; others we used a randomized-response technique, where they privately flipped a coin and responded to the question honestly or not depending on the flip. In other contexts, this method has detected under-reporting of sensitive behaviors. We see no differences in payment rates between the techniques, however (see Appendix A), suggesting people did not misreport it.

Third, we found that people who appeared not to want to talk about gang rule or security fees often said “I don’t know” or pass on answering that question. Just 7% of the sample answered in that fashion. If this were driven by worries about the combo, we might expect a correlation between combo governance and the proportion of questions unanswered. We see no such relationship (see Appendix A).

### 3 Descriptive analysis and context

Gangs are generally clandestine organizations with obscure operations and motives. In order to develop theories of gang organization and rule, to understand their relationship to the state, and to evaluate programs, we must first establish some basic facts. While case studies

of gangs and gang rule abound, we know of no prior systematic data collection on a large sample of comparable gangs. This descriptive analysis shapes the theory and the empirical results to follow, and is an equally important part of the analysis.

### 3.1 The state

Medellín has 2.4 million people, with 3.7 million in the metro area. Per capita annual income is roughly \$11,500, adjusted for purchasing power parity. The city is divided into 16 urban *comunas* plus 5 peri-urban *corregimientos*. The comunas are formally divided into 269 neighborhoods or *barrios*. Each barrio has an elected community council to manage various aspects of community affairs.

Medellín has a well-organized, professional bureaucracy with high fiscal capacity and broad-based public services. With a huge commercial sector, the city has ample revenues. Two organizations are responsible for order: the police and the Secretariat of Security.

The Secretariat is a large civilian organization with thousands of staff. It sits directly beneath the Mayor and is the city’s primary organization for setting security policy and investing in security infrastructure. It provides numerous services to residents, including responding to various emergencies and street disorder, directly resolving community disputes and domestic violence, and regulating the use of public space across the city.

The Metropolitan Police are independent from the city government; they are part of the National Police, a branch of the Defense Ministry. While there are common charges of corruption and poor responsiveness, the National Police is fairly professionalized, particularly in comparison with other Latin American countries. There are 280 officers per 100,000 people in Medellín, similar to cities like Los Angeles. That said, street cops are greatly outnumbered: there are roughly a dozen combo members for every poorly-paid officer.

### 3.2 Combo organization and operations

Virtually every low- and middle-income residential neighborhood has a local combo, and nearly every combo governs to some extent the civilians within its territory. Our 2019 combo census identified 380 active combos, 354 in Medellín and the rest in the wider metropolitan area (see Appendix B). Figure 1 displays estimated main locations.

We have detailed organizational data on seven combos. Almost all of these have a core of 15 to 40 permanent, salaried members (one has nearly 100, but this is unusual). The combo territories we observe are sometimes no more than a few square blocks, but borders are usually long-standing, well-defined, and known to most locals. Combos also tend to be long-lived. Many have been present for decades in some form, as younger generations take

over from older ones. While there are of course changes in the size, territory, names, and even existence of some combos, most in our sample have been relatively stable over the last decade at least, and our broader interviews suggest this is true of most combos.

Combo revenues come from four main sources. Practically every combo has a local monopoly on retail drug sales in their neighborhood, which occur at defined locales known as *plazas de vicio*. This is typically their most profitable activity. A large number also charge a security fee known as a *vacuna* (“vaccine”) to at least some residents and businesses, typically in return for protection services (discussed at length below). About a third also engage in a local loan-sharking practice known as *gota a gota* (“drop by drop”), according to the survey. Finally, many combos collect debts for a fee, and also manage, regulate, or participate in local consumer goods markets, such as cooking gas, *arepas*, and eggs.

A companion paper describes the personnel economics and market structure of the combos (Blattman et al., 2020). Members tend to be poor, uneducated young men aged 15 to 35. Almost all were born and grew up in the neighborhood they control, and most still live there. Even low-ranking combo members tend to be well-paid, earning a salary equal to the median salary in the city. They are headed by a leader called a *coordinador*. In general, combos are organized internally by product or service line. Combo members are paid a salary for their role. Other business lines, such as consumer goods sales and loan-sharking, are often given to individual combo members as a personal, local monopoly to operate and exploit. These are key sources of entrepreneurial side income for combo members (Giraldo et al., 2014).

Most combos are small and autonomous; horizontal integration across neighborhoods is rare. In our companion paper, we argue that while such integration increases monopoly rents and mitigates inter-combo conflict, it also exposes combos to prosecution and requires managerial capacities that many combos lack.

Finally, Medellín’s combos form the base of a pyramid of criminal organization. Above them are roughly 17 mafia-like groups sometimes called *razones*. Razones are typically the wholesale suppliers of drugs to the combos’ street retail operations. Most combos have a longstanding business and military alliance with a razón. A small number of combos are vertically integrated into their razón. For the most part, however, combos are small autonomous firms with a long-term relational contract with the razón as their supplier.

### 3.3 Combo governance

A central component of governance is protection: order, security, and property rights enforcement. Many of Medellín’s combos provide these services, often on a private, fee-for-service basis. Examples include dispute resolution, informal contract enforcement, recovering stolen

items, and private security for stores, vehicles, and other property. The gangs also provide less excludable, public forms of protection, often in exchange for taxes, including regulating fights, disorderly conduct, and drug consumption on their blocks. At least one combo even installed security cameras for a time. Even these more public goods, however, are partially excludable. For instance, combos often focus their public protection on blocks where they already have many private customers. Some provide hotlines to those paying security fees.

Of course, governance also includes material public goods such as infrastructure, as well as collective decision-making and coordination. Our interviews and surveys found that combos rarely offer such services. Infrastructure is provided almost solely by the state, while informal leaders and elected neighborhood councils manage most local collective decisions. Instead, combos tend to specialize in services that are at least partially excludable, and those that benefit from coercive power. In the remainder of the paper, we use “governance” as shorthand for this set of protection services in which both gang and state participate.

**Levels and variation of governance** To measure governance, we asked residents how frequently each actor responded to 17 common disputes and forms of disorder (12 from residents and 5 from business-owners). We identified these through our qualitative work. Table 2 reports scaled responses, where 0 = Never, 0.33 = Occasionally, 0.66 = Frequent, 1 = Always. We create average indexes of *State* and *Combo governance* (0 to 1), as well as the difference between them, *Relative state governance*, which can vary from -1 to 1.

The average response for any service by either provider was seldom greater than 0.5, suggesting that for residents, neither the state nor the combo are reliably responsive to disputes and disturbances. In absolute terms, combo response levels were highest for addressing unpaid debts, property crimes (car thefts, home robberies, muggings, etc.), and public disturbances (threats and fights on the street). Combo involvement was somewhat lower for inter-neighbor disputes such as noise complaints and property infringements. In relative terms, combo response was generally lower than the state’s, but higher in five situations: muggings and theft prevention, business and household debt collection, and kids fighting.

These averages conceal much variation across neighborhoods. In some, the combo is the dominant provider of protection. Figure 2 maps relative state governance by barrio. Note, however, that high levels of state governance do not imply combos are absent. Nearly every neighborhood has a strong combo presence, running drug corners and other operations. It may be that they have simply chosen not to sell private protection in those neighborhoods.<sup>4</sup>

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<sup>4</sup>We examine correlates of state and combo governance in Appendix Table D.1. Not surprisingly, larger youth populations are associated with greater relative combo rule. Also, as expected, we see lower combo rule in middle income neighborhoods compared to lower-income ones. We also see less combo rule in areas close to the city center, which leaders told us was too dense and valuable for any one group to control. Also,

Table 2: State and combo governance and legitimacy, barrio survey averages, 2019

	Frequency/Rate (0-1 Scale)				Relative State Governance Difference
	State		Combo		
	Estimate (1)	SD (2)	Estimate (3)	SD (4)	(5)
Governance Index	0.41	0.27	0.33	0.29	0.08
How often they intervene when:					
HH: Someone is making noise	0.42	0.38	0.19	0.30	0.23
HH: Home improvements affect neighbors	0.41	0.37	0.24	0.33	0.16
HH: There is domestic violence	0.50	0.37	0.33	0.37	0.16
Biz: Someone disturbs a business	0.50	0.38	0.36	0.38	0.14
HH: Two drunks fight on the street	0.52	0.36	0.39	0.37	0.13
Biz: You have to react to a robbery	0.53	0.37	0.39	0.39	0.13
Biz: It is necessary to prevent a theft	0.46	0.36	0.37	0.39	0.09
Biz: Businesses in this sector are robbed	0.43	0.39	0.35	0.38	0.07
HH: A car or motorbike is stolen	0.47	0.37	0.41	0.38	0.05
HH: People smoking marijuana near children	0.30	0.36	0.25	0.36	0.05
HH: You have to react to a robbery	0.46	0.36	0.44	0.38	0.02
HH: Someone is threatening someone else	0.41	0.36	0.40	0.37	0.01
HH: Someone is mugged on the street	0.39	0.36	0.40	0.38	-0.01
HH: It is necessary to prevent a theft	0.39	0.36	0.41	0.38	-0.02
HH: Kids fight on the street	0.28	0.35	0.31	0.36	-0.03
Biz: Someone does not want to pay a debt	0.18	0.31	0.24	0.35	-0.06
HH: Someone refuses to pay a big debt	0.21	0.31	0.36	0.37	-0.16
Legitimacy Index	0.57	0.21	0.43	0.28	0.14
When solving problems in the neighborhood:					
How much do you trust the...	0.57	0.30	0.36	0.36	0.21
How fair is the...	0.55	0.27	0.41	0.35	0.14
How much do your neighbors trust the...	0.57	0.28	0.47	0.36	0.10
How would your neighbors trust the...	0.59	0.23	0.50	0.29	0.09
How do you rate the...	0.60	0.22	0.51	0.28	0.09

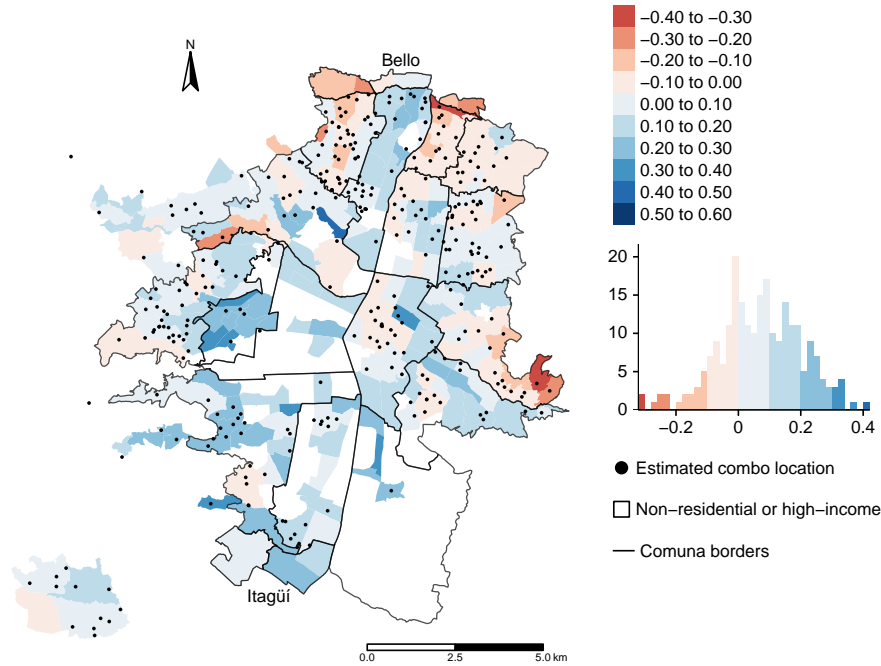
*Notes:* The survey is representative of Medellín's 224 low- and middle-income barrios, with 20–25 respondents per barrio. Governance scales correspond to: 0 = Never, 0.33 = Occasionally, 0.66 = Frequently, 1 = Always. Legitimacy scales correspond to: 0 = Nothing, 0.33 = A little, 0.66 = Somewhat, 1 = Very.

**Legitimacy** We also asked residents (but not business owners) about combo and state legitimacy: how much residents trust each actor; whether actors were fair; whether residents were satisfied with each actor; and whether residents thought their neighbors trust and are satisfied with each actor. We averaged these responses into unit indexes for state and combo legitimacy. Table 2 reports barrio averages. On average, residents rate their trust and satisfaction of the combo lower than the state, although the difference is not always large. Not surprisingly, there is a tight correspondence between combo governance and legitimacy. Figure 3 illustrates this, plotting combo legitimacy against combo governance.

contrary to conventional wisdom, steep settlements high on the slopes of the mountainsides do not have more gang rule, nor do newer neighborhoods or high-immigration places (often places that were informal slums for a long stretch of the late 20th century). We have to take these correlations with caution, however, as they may be biased. For example, we do not see a strong correlation between proximity to state services and relative state governance. Yet, in the quasi-experiment below, we will see evidence of causal effects.



Figure 2: Relative state governance by barrio, 2019



*Notes:* Each barrio’s value is the average relative state governance (state-combo governance index) for all 17 items from Table 2. We did not survey high-income barrios.

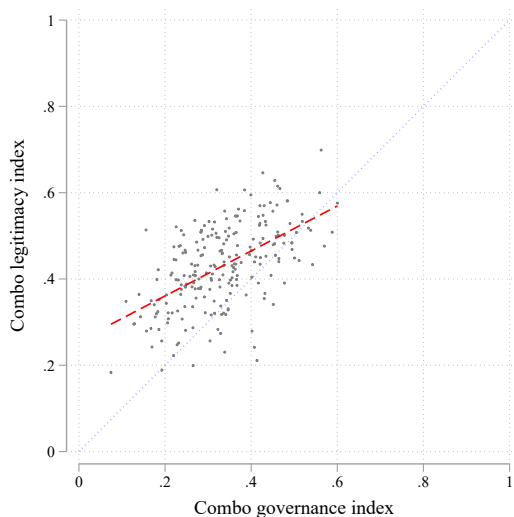
**Efficacy** Combos also outperform state agents in some respects: 67% of survey respondents said the combo was easy to contact compared to 63% for the police and 32% for the Mayor’s office. They also said the combo responded rapidly 58% of the time compared to 41% for the police and 27% for the Mayor’s office.

This is not entirely surprising. With the exception of the police, the city’s street-level bureaucrats are rarely available outside of business hours; offices are closed on Colombia’s frequent holidays; and due to peculiarities in municipal budgeting and labor agreements, every December to January a large proportion of city staff on contracts are not working. The combo, by contrast, is always present.

Combos also have several other advantages. They have more local knowledge and deeper networks than state bureaucrats, and even local police. Community leaders have similar information advantages, but combos have organized means of coercion (acquired, perhaps, to protect other business lines like the drug trade). Finally, like these other actors, combos possess decades of governing experience.

Combos also differentiate their services from competitors in other ways. Combos can carry out swift and sometimes violent sanctions that some residents demand, such as expelling an abusive husband from the neighborhood. Also, whereas the state and community leaders are expected to be impartial and consistent, combos sometimes resolve disputes and

Figure 3: Relationship between combo governance and combo legitimacy, 2019



*Notes:* Each dot is a barrio average, and the dotted line indicates fitted values. We did not survey high-income barrios.

enforce contracts in favor of those who hire them or who are most closely connected.

These comparative advantages and differentiated products help explain why many residents report they are happy to have access to both the combo and the state for protection. Just 46% of survey respondents agreed to the question that the neighborhood would be better off without the combo. Elaborating, some said they feared the vacuum of authority that might open up without this local actor. Others were simply satisfied with the work of the *muchachos* (“local boys”), a common term for combo members.

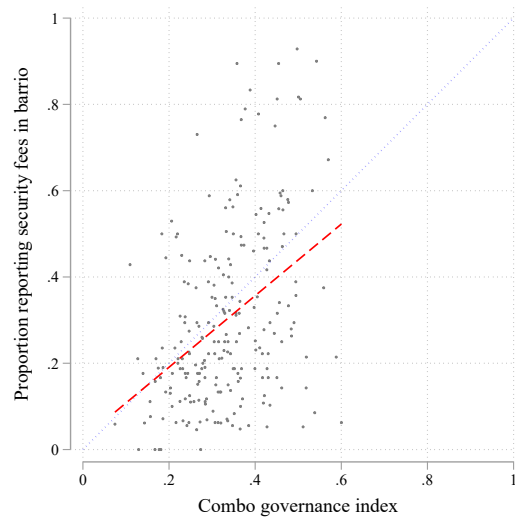
### 3.4 Why do combos govern?

Our interviews point to three important motives: (i) direct revenues from protection as a business line; (ii) indirect benefits of governance on other business lines, especially drug retailing; and (iii) intrinsic rewards from ruling.

**Motive 1: Protection as a business line** First, for some combos, protection services are an important business line that yields significant revenue. For services such as debt collection or dispute resolution, combos commonly charge on a fee-for-service basis.<sup>5</sup>

<sup>5</sup>As one community leader told us: “If a couple starts fighting, they [the gang] come to a kind of trial and fine them. It is the same with the problems between neighbors; they set fines of 100,000 [pesos]” — Community Leader 14/14, interview 1/1 [08/06/2020]. Another leader explained how “if you fight with someone, regardless of whether you provoked it or not, you must pay between 100,000 and 500,000 [pesos], depending on how serious the fight is. They decide what price to impose. There are also fines for theft. For

Figure 4: Relationship between gang governance and vacuna payments, 2019



*Notes:* Each dot is a barrio average, and the dotted line indicates fitted values. We did not survey high-income barrios.

Revenues from other services, such as security and protection for homes and shops, are akin to semi-voluntary taxes or a subscription. Residents and businesses typically call this tax a *pago por la vigilancia* (“security” or “surveillance fee”) or, more colloquially, a *vacuna*—literally, a vaccine. Among the combos where we have internal organization data, most organize vacuna collection and protection services as a specialized unit with dedicated staff. The manager of this business line generally reports directly to the coordinador, and is sometimes referred to as a *relacionista*, or relationship manager.<sup>6</sup>

Most combos tax only a fraction of local businesses and residents. In our survey, 85% reported that the combo charges vacunas in their neighborhood, typically weekly. But within these communities, only a quarter of businesses and a tenth of residents reported being charged this tax themselves. Figure 4 shows that the share of people who report that they or others in the neighborhood pay regular security fees is strongly positively correlated with combo governance levels—close to the 45 degree line. (Our survey experiment, discussed in Section 2.2 above, suggests these reporting rates are roughly accurate.)

Vacunas are also modest. Among those who pay, median weekly amounts were about US\$1 for residents and US\$2 for businesses—roughly 3% of business profits and 1% of sales

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example, something that happens a lot: a neighbor steals some plants from me, so she must buy or return those plants and also pay the fine to them. The price of the fine depends on what was stolen.” —Community Leader 4/14, interview 1/2 [02/22/2020].

<sup>6</sup>As a typical example, one combo member told us how in his group, “Three people are in charge of business collection. Two of them collect in one zone and one of them in another. What they collect is delivered to the coordinator who they work for.” —Criminal Group Leader 1/29, interview 2/6 [09/20/2019].

at the median. By comparison, municipal taxes on these enterprises are about 6% of profits. Identifying a tax rate seemed to be relatively straightforward. Typically, a junior member of the *relacionista* staff observes a business for a few days, using the store characteristics and counting the number of customers per day to select an appropriate tax rate.

It's important not to exaggerate the voluntary nature of the *vacuna*. While in some places they are voluntarily paid, and in others they are an obligatory but generally-accepted tax, in some neighborhoods they resemble outright extortion. About half of respondents reported that refusing to pay the *vacuna* would result in threats or assaults. At the same time, that implies half did not expect coercion. 45% said that the *combo* would simply stop providing them security. Also, just 27% of businesses said that *vacunas* were too high, compared to the 54% who said that city taxes were too high. It is commonplace for residents to express support for the boys, and acknowledge the important role they play. As mentioned above, about half think the neighborhood would be worse off without them.

**Motive 2: Indirect impacts on other business lines** A second and potentially even more important motive for governing is that it helps protect the gang's physical security and illicit income from competitors and, perhaps more importantly, police.

First, providing order wins the loyalty of residents. Loyal residents rarely inform on *combos* to police, and may even actively protect them from police repression. Numerous *combo* leaders made this point: "The community shields you according to your behavior... If you do not have the community in your hands and your back, you have nothing. That is who takes care of you."<sup>7</sup> As another explained, "Caring for the neighbors gives a criminal more security. When the community feels comfortable and grateful, they open their houses, so that if you have to hide from the police, the community is going to welcome you. The community goes out to defend you."<sup>8</sup> In another case, asked about the benefits of governing, one member responded, "[T]hat the neighbors love us, do not rat us out to the *tombos* [cops], watch us doing our stuff and do not interfere, and let us know when the police are coming."<sup>9</sup> Finally, as one public prosecutor told us, "They're very interested in winning over the community. That's why it's so hard to get witnesses against them."<sup>10</sup>

Moreover, providing local order may directly reduce police presence. As one *combo* leader put it, "There is a good relationship with the people," and therefore, "it is easier to bring order in the sector and so the police do not have to come around."<sup>11</sup> When police patrol

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<sup>7</sup>Criminal Group Leader 13/29, interview 1/2 [05/02/2019].

<sup>8</sup>Criminal Group Member 6/31, interview 2/3 [02/11/2020].

<sup>9</sup>Criminal Group Member 8/31, interview 1/1 [12/30/2020].

<sup>10</sup>Official 12/13, interview 1/5 [10/16/2019].

<sup>11</sup>Criminal Group Member 6/31, interview 2/3 [02/11/2020].

or respond to service calls, it can scare off drug buyers, require a bribe to the officers, or increase the risk of a seizure. If crime and disorder are high, moreover, local police feel pressure from superiors to crack down. One active combo member offered a vivid example: “The police station is across from our headquarters and they never bother us. They know where our drug corners are and who works there. That’s why it’s important to keep the neighborhood calm: if nothing bad happens, the police don’t squeeze us and let us work.”<sup>12</sup>

Combos are especially worried about new or specialized police units in their territory. As one expert and former prison-gang leader explained, “there is always a police presence, but combos strike non-interference deals with the regular beat cops. When public order gets disrupted, the police must act and officers not part of the deal arrive. The area becomes visible and combos’ activities become more vulnerable.”<sup>13</sup> This suggests that even where bribery is part of how combos avoid police repression, governance can play a key supporting role, by avoiding the attention of less-corrupt police as well.

While our interviews suggest that combos mainly defend against the state, they also face rival criminal-market entrants, and the benefits of establishing loyalty extend to inter-gang conflict as well. Even where combos enjoy firm local monopolies on crime and extortion, they must defend these against local coups, neighboring combos, and their respective *razones* (which could invade or sponsor a coup from combo or non-combo youth from the neighborhood). Residents’ loyalty can mitigate these threats, while dissatisfaction with local combo rule could lead to collaboration with rivals. These events appear rare in equilibrium, but combo leaders’ wariness suggests that their rarity may well be due to vigilant deterrence.

These motives—fostering civilian loyalty and preventing police and rival entry—may explain some of the patterns we observe in the protection market:

- Even though combos have the coercive power to extract higher sums, the tax itself is modest. Only a quarter of our survey respondents said it is “too high.” It may also explain low rates of tax incidence in some neighborhoods, including the fact that combo rule and *vacunas* decrease as distance to the gang grows (Appendix Table D.1).
- Few of the combo leaders we spoke to saw protection as a highly profitable business line, and some said they provided the service at a discount because of the indirect benefits. One former gang member described fees and fines for dispute-resolution services as a way to limit demand and deter disputes, rather than as money-making strategy.<sup>14</sup>
- Combos avoid charging businesses whose ability to pay is low or whose loyalty is more

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<sup>12</sup>Criminal Group Member 5/31, interview 1/2 [10/09/2019].

<sup>13</sup>Criminal Group Leader 24/29, interview 5/5 [12/14/2020].

<sup>14</sup>Criminal Group Leader 24/29, interview 5/5 [12/14/2020].

fragile. For instance, when asked why some grocers were targeted and others were not, one combo member explained that some were more likely to denounce the combo to the police if pressed to pay, and it is better to keep the population loyal.<sup>15</sup> Another explained that “There is no fixed fee; it is voluntary.” He described how, when a new business opens, the combo talks to the owner and agrees on the weekly *vacuna* based on the size and type of business. If the business is doing badly, however, the combo does not demand payment. “We are here to help,” he explained.<sup>16</sup>

- Finally, this indirect motive also help explain the relatively extreme extortion of bus lines. Almost every combo with a passenger bus route in their territory charges drivers steep fees in return for little to no services. Bus lines are small, cash-heavy businesses with many small owners. Most importantly, both drivers and owners come from outside the combo’s territory, and residents do not pay much attention to bus extortion.<sup>17</sup>

In short, all aspects of the protection business, from extortion to semi-voluntary *vacuna* taxation to fee-for-service provision, are disciplined by a need for the loyalty of local residents.

**Motive 3: Intrinsic rewards** Finally, power, authority, and the loyalty of subjects can be their own reward. Some combo leaders reported taking pride in ruling, or simply enjoying the status and moral legitimacy it offers. As one said, “Personally, doing good work feels good. You can be the worst bandit, but you can also have a good heart of your own.”<sup>18</sup>

Some also describe governance as a moral obligation or social duty to their community. Many combos emerged in the 1980s as local defense forces fighting left-wing militias affiliated with rural guerrilla movements. Today, most gangs in Medellín retain a socially conservative and anti-communist ideology. Many of the leaders we met saw themselves as responsible for upholding moral codes, protecting women and girls, and preserving conservative social mores. Others described themselves as critical “*anti-subversivos*”—bulwarks against socialism. They resent the lack of recognition of their contribution to Colombia’s civil war.

Finally, combos may derive utility from the status that their authority confers. In addition to pride and any appreciation of community respect, some of our interviews suggest respected combos may enjoy easier access to women.

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<sup>15</sup>Criminal Group Leader 23/29, interview 2/2 [12/28/2020].

<sup>16</sup>Criminal Group Leader 1/29, interview 1/6 [06/20/2019].

<sup>17</sup>As an aside, buses pass through many territories, and combos have solved the common pool resource problem by developing a collective norm whereby the combo at the bus’ point of origin holds extortion rights.

<sup>18</sup>Criminal Group Member 6/31, interview 2/3 [02/11/2020].

## 4 Conceptual framework

To guide our analysis and interpretation of results, we develop a simple model to understand how gangs will respond to an increase in state capacity and services. We first model them as competing to provide private goods, choosing a fixed  $q_i$  to produce and letting prices clear the market—Cournot competition. We then consider other forms of competition, including models of stationary bandits competing to provide public goods.<sup>19</sup>

### 4.1 Example of Cournot competition

Consider a gang  $g$  and a state  $s$  offering distinct but substitutable protection services to residents in quantities  $q_g$  and  $q_s$  at a constant marginal cost of production  $c_i$ . We can write each organization  $i$ 's utility function as:

$$V_i = p_i q_i - c_i q_i \tag{1}$$

Price is determined by a linear inverse demand curve,  $p_i = a_i - \beta q_i - \gamma q_j$ , where  $\gamma \in (0, 1]$  implies the two services are substitutes, and  $\beta > 0$  implies downward-sloping demand.

We are interested in whether gang rule is crowded in or out when there is an exogenous increase in state governance:  $\frac{\partial q_g^*}{\partial q_s}$ . We derive each organization's best response function, their equilibrium values of  $q_g^*$  and  $q_s^*$ , and this comparative static in Appendix C, showing that:

$$\frac{\partial q_g^*}{\partial q_s} = -\frac{\gamma}{2\beta} \tag{2}$$

So long as the two services are not complements, this comparative static implies that increases in one duopolist's supply of protection will reduce the other's—i.e. “crowding out.”

Initially, we supposed this standard view was correct, and we set out to evaluate two interventions to assess whether the cross-elasticity was small or large. The experimental and quasi-experimental results that follow, however, show an absence of crowding out, and even evidence of crowding in. Thus, here we focus on potential reasons why.

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<sup>19</sup>Note that Cournot fits some of our stylized facts well—especially that governing requires investments and advanced commitments, and that it is hard to adjust output capacity quickly. In modeling duopolistic competition, however, note that we abstract away from competition between combos. We do this in part because gangs are insulated from territorial competition by the rzones, who protect gang property rights.

## 4.2 Additional benefits to governing

Gang leaders described additional benefits to governing—and especially to winning civilians’ loyalty—beyond the money it brings in as a business line. We summarize these diverse motives by adding a single, stylized term,  $\rho(q_i, q_j)\pi_i$ , to the players’ objective functions:

$$V_i = p_i q_i - c_i q_i + \rho(q_i, q_j)\pi_i \quad (3)$$

Set up this way,  $\pi_i$  is the return to full control of the neighborhood. For example,  $\pi_g$  includes the illicit rents from unimpeded retail drug sales, but it also includes non-material benefits, such as status, access to women, and other intrinsic rewards from loyalty and rule. The state has its own distinct  $\pi_s$  in the form of electoral rewards, achievement of policy aims, or preferences for dominance and citizen loyalty.

Meanwhile,  $\rho(\cdot)$  scales each organization’s ability to capture, retain, or enjoy these benefits. We can think of it as the share of  $\pi_i$  each player enjoys, one that is increasing in own governance and decreasing in the other’s, such that:  $\frac{\partial \rho(q_i, q_j)}{\partial q_i} > 0 > \frac{\partial \rho(q_i, q_j)}{\partial q_j}$ . Importantly, however, we remain agnostic here about whether  $\rho(\cdot)$  exhibits increasing or decreasing returns to own and other’s governance provision.

The elasticity of gang governance to state governance now becomes:

$$\frac{\partial q_g^*}{\partial q_s} = \frac{\lambda \pi_g - \gamma}{2\beta - \delta \pi_g} \quad (4)$$

where  $\lambda = \frac{\partial^2 \rho(q_g, q_s)}{\partial q_g \partial q_s}$  represents the cross-partial derivative between gang and state governance, and  $\delta = \frac{\partial^2 \rho(q_g, q_s)}{\partial q_g \partial q_g}$  reflects the rate of increasing or decreasing returns to governing.

This helps distinguish channels by which more state governance could crowd gangs in.

**Case 1: Strategic response to state rule by the combo ( $\lambda \pi_g > \gamma$ )** This case corresponds the closest to gang leader interviews. When the state increases protection, they threaten the gangs’ share of rents  $\rho(\cdot)$  and non-material benefits  $\pi_g$ . The cross-partial derivative,  $\lambda$ , will be positive when the gang has more than proportional returns to increasing its own rule in response to the state’s efforts. This could arise, for example, from a gang leader who values dominance and sole allegiance, or who is averse to losing status. Alternatively, we could think of  $\rho(\cdot)$  as a contest success function for control of illicit markets, with governing akin to arming. For many such functions and ranges of relative “armament” (loyalty-inducing  $q_g$  and  $q_s$  in our setting), the optimal response to an increase in opponent’s arming is to increase one’s own arming, especially when one is strong to begin with (e.g., Hirshleifer, 1989; Skaperdas and Syropoulos, 1996; Konrad and Skaperdas, 1998, 2012).



Any  $\lambda > 0$  will attenuate the state's attempts to crowd the gang out. The larger are  $\lambda$  and  $\pi_g$ , and the more differentiated are gang and state governance services (lowering  $\gamma$ ), the more likely that we observe crowding in, where  $\lambda\pi_g > \gamma$ . Of course, this assumes that the denominator is negative, which is the case when gangs have diminishing returns to their own governance. The alternative brings us to the next case.

**Case 2: Increasing returns to a gang's own level of governance ( $\delta\pi_g > 2\beta$ )** Given downward-sloping demand ( $\beta > 0$ ), the denominator in Equation 4 will be negative only if gangs enjoy large increasing returns to their own governance. This could arise, for instance, if residents reward protection with loyalty at increasing rates. While not impossible, increasing returns should not be assumed, and we saw no evidence of them. Generally, therefore, we expect diminishing returns to governance, making combo strategic response (Case 1) the more plausible of this pair of mechanisms.

Finally, outside of  $\rho(\cdot)\pi_i$ , there is a third way the elasticity between state and gang governance could turn positive: endogenous demand for protection. A state increase in protection could raise the number and value of transactions in the local economy, thereby increasing demand for governance in general and gang governance in particular. This is a common feature of the political economy literature on stationary bandits, where a state monopolist has incentives to provide public goods to grow the very market they will later tax (Olson, 1993; McGuire and Olson, 1996; Grossman, 1996; Bates et al., 2002).

**Case 3: State rule increases general demand for governance.** In our Cournot example, we could model such endogenous growth through the demand curve, writing  $a_i$  as an increasing function of  $q_s$ . This can produce crowding in ( $\frac{\partial q_g^*}{\partial q_s} > 0$ ) even if we assume no additional benefits from dominance or loyalty (see Appendix C). That said, there are two major caveats. First, the demand effect would have to be quite large to overcome the crowding out that arises from normal duopolistic competition. Second, not all models make this prediction about endogenous demand. The prediction reverts to crowding out if we move away from a traditional model of duopoly to a model of stationary bandits competing to provide public goods for taxes (see Appendix C.2). Nonetheless, endogenous demand could contribute to a positive elasticity of gang rule to state rule, and we will look for evidence of this economic development in the empirical analysis below.

### 4.3 General formulation

Finally, as the foregoing suggests, our framework was designed to accommodate a variety of modeling approaches. In its general form, gang responsiveness to state rule is given by:

$$\frac{\partial q_g^*}{\partial q_s} = -\frac{\frac{\partial^2 D(q_g, q_s)}{\partial q_g \partial q_s} + \frac{\partial^2 F(q_g, q_s)}{\partial q_g \partial q_s}}{\frac{\partial^2 D(q_g, q_s)}{\partial q_g \partial q_g} + \frac{\partial^2 F(q_g, q_s)}{\partial q_g \partial q_g}}. \quad (5)$$

Here,  $D(\cdot)$  represents the direct returns to governing, whether we model it through Cournot competition in private goods, Bertrand competition, or stationary bandits providing public goods. Meanwhile,  $F(\cdot)$  represents the additional benefits to establishing rule, dominance, and loyalty—previously  $\rho(\cdot)\pi_i$ . Appendix C presents these alternative models. In general, whether we treat governance services as public or private goods, the cross-partial  $\frac{\partial^2 D(q_i, q_j)}{\partial q_i \partial q_j} < 0$  is negative, making the overall elasticity negative in the absence of additional benefits. Introducing  $F(\cdot)$  generally makes the sign of the elasticity indeterminate, for two reasons: first, the cross-partial  $\frac{\partial^2 F(q_i, q_j)}{\partial q_i \partial q_j}$  itself is often indeterminate and sensitive to specific values of  $q_i$  and  $q_j$ ;<sup>20</sup> and second, even if  $\frac{\partial^2 D(q_i, q_j)}{\partial q_i \partial q_j} > 0$ , it must be large enough to outweigh the crowding-out effect of  $\frac{\partial^2 D(q_i, q_j)}{\partial q_i \partial q_j}$  in order to flip the sign of  $\frac{\partial q_g^*}{\partial q_s}$ .

## 5 Quasi-experimental analysis

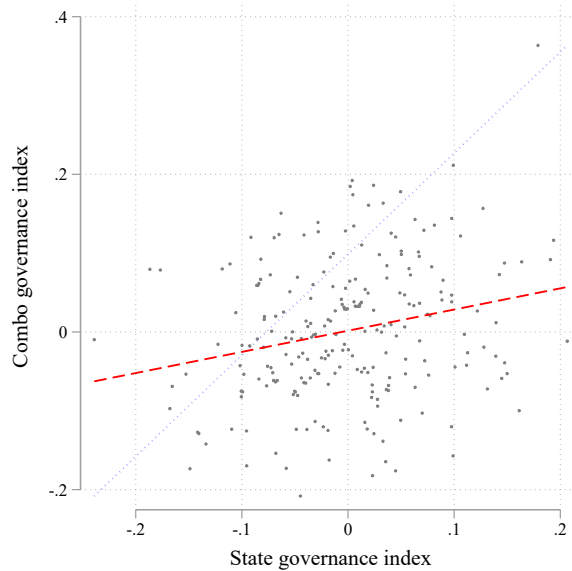
Figure 5 correlates state and gang rule by barrio in 2019, partialing out income, demographic, and geographic traits.<sup>21</sup> We see a strong positive relationship, consistent with both strategic gang responses and state rule increasing the general demand for governance. Naturally, however, this correlation could be confounded by any number of unobserved factors that increase the supply or demand of governance, from social capital to state public goods that drive high levels of economic or social transactions.

As a result, we first turn to a natural experiment that harnesses discontinuous jumps in the distance to local state services, and thus attempts to hold these other confounders constant. These discontinuities arose with the introduction of new internal borders and the subsequent expansion of local state services within them. This comparison helps us understand how long, sustained state presence affects gang rule.

<sup>20</sup>This is the case with virtually all contest success functions, for example.

<sup>21</sup>See Appendix Table D.1 for these correlates. We did not partial out distance to state services.

Figure 5: Correlation between combo and state governance in 2019, adjusted for covariates



*Notes:* Each dot is a barrio average after partialing out block characteristics. The dotted line indicates fitted values. We did not survey high-income barrios.

## 5.1 Medellín’s expansion of state rule within new borders

In 1987, Medellín reorganized the city into 16 neighborhoods called *comunas*.<sup>22</sup> Following this date, the city also began spending and personnel in policing and other protective services. The new *comuna* borders set the jurisdiction for many of these functions. While residents can access schools or clinics irrespective of their *comuna* of residence, the police and several protection agencies (including dispute resolution and family services, for example) were organized along *comuna* lines.<sup>23</sup> Thus, within Medellín, on either side of a *comuna* border, people on one side might be close to their local police station or *comuna* headquarters, while on the other side the block could be distant. These more distant blocks may receive fewer services, or are less aware of the agencies available, despite being otherwise similar, and equally distant from other urban amenities.

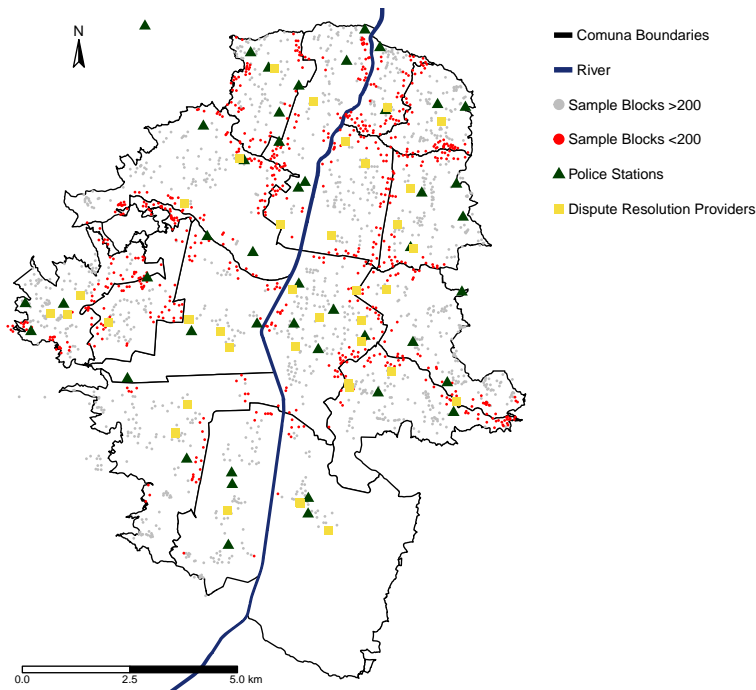
We examine how state and combo services and legitimacy change along with this shift in proximity across borders.<sup>24</sup> Our proxy for distance and access to the state is the average distance of that city block from two key headquarters in their *comuna*: the *comuna* police station, and the *comuna*’s *Casa de Justicia* or “Justice House”—the main *comuna*-level

<sup>22</sup>These *comunas* were created in the Bill 54 by the city council. The previous organization of the city dated back from more than 20 years before (city Bill 52 from 1963).

<sup>23</sup>It is permitted to report a crime or seek assistance from any state office, police patrols are organized by *comuna*, and case loads in city agencies would be dominated by people within the *comuna*.

<sup>24</sup>This emulates a strategy by Henn (2020), who looks at the proximity of chiefdoms to the state.

Figure 6: Surveyed blocks and comuna-based state services relative to comuna borders



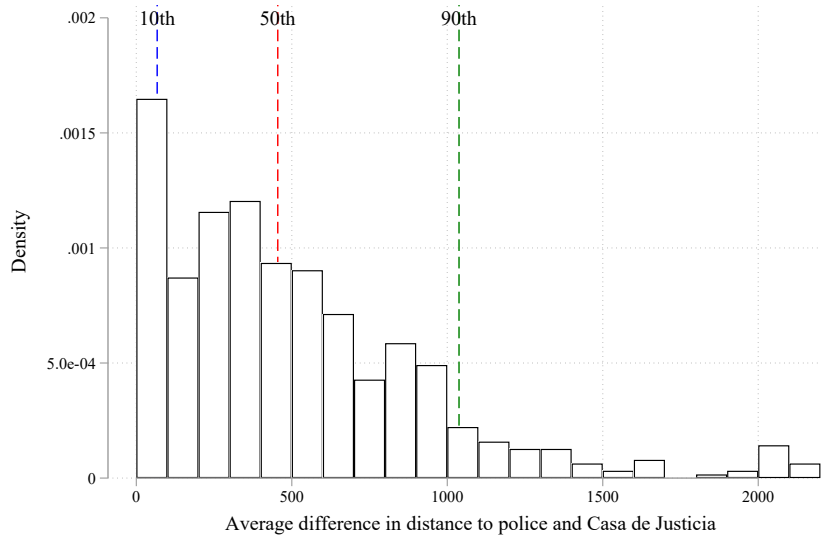
building, which houses dispute resolution services among other agencies, most commonly family services (but others as well).

This is a lengthy and multifaceted “treatment,” representing a significant intensification of several kinds of state presence over three decades. Note that the discontinuity does not simply estimate exposure to the two agencies whose headquarters we observe. Other city agencies (past and present) operate at the comuna level and their penetration into distant blocks will be correlated with police and Casa de Justicia locations. Thus we should think of the average distance measure we use as a proxy for general exposure to the local state. This is mainly an increase in the intensive margin of state services, since the city was present in most border blocks at the time the comunas were created (although to varying degrees). This treatment, however, could have affected gang rule on both the intensive and extensive margins depending on the neighborhood, because combo presence and rule extend back to the 1980s and 1990s in many barrios, but not all.

## 5.2 Empirical strategy

Figure 6 displays comuna boundaries, local headquarters, and city blocks from our December 2019 survey. We focus on blocks within 200 meters of an internal border (in red). We omit borders that fall along natural river and mountainous boundaries. Later we will see that

Figure 7: Distribution of differences in the distance to comuna headquarters between paired blocks within 200 meters of the new comuna borders



other bandwidths and border criteria make little difference to the results.

Our “treatment” is the difference in the paired blocks’ distance to the local state headquarters in hundreds of meters (that we denote  $\Delta StateLocDist_{ij}$ ).<sup>25</sup> Figure 7 displays the distribution of the absolute value of this difference. The 10th percentile is 67 meters, the median is 454 meters, and the 90th percentile is 1,037 meters. We hypothesized that relative state governance and legitimacy fall as  $\Delta StateLocDist_{ij}$  rises.

To estimate treatment effects from the discontinuity, we use the following OLS regression:

$$\Delta Y_{ij} = \beta_0 + \beta_1 \Delta DistLocState_{ij} + \beta_2 \Delta DistOther_{ij} + \Theta \Delta X_{ij} + \eta_b + f(lat_i, long_i) + f(lat_j, long_j) + \epsilon_i \quad (6)$$

The dependent variable,  $\Delta Y_{ij}$ , is the within-pair outcome difference from the block farthest from state services to the closest.

We control for possible confounders:  $\Delta DistOther_{ij}$  is a vector of differences in the distance to other amenities—schools, hospitals, and business agglomerations;  $\Delta X_{ij}$  is a vector of differences in demographic and geographic covariates; and  $\eta_b$  are comuna border fixed effects. As we will see below, paired blocks are fairly well-balanced on these potential confounders, and including or omitting them has little effect on our estimates. Finally, following Keele and Titiunik (2015),  $f(lat_i, long_i)$  and  $f(lat_j, long_j)$  are first-degree polynomials of the coordinates

<sup>25</sup>We created pairs in the following way: For a given comuna border, we take the block closest to the border, then identify the nearest block on the other side of the border. For each block, we calculate a simple average of the distance to the police station and Justice House. We order  $i$  and  $j$  so that we can interpret the independent variable as moving 100 meters further from the state.

of each block in our sample and its match (akin to a distance running variable). Our balance test uses the same equation, omitting  $\Delta X_{ij}$ , as these are the dependent variables.

For the most part, this strategy will estimate *within-combo* effects on gang rule. Combo borders do not coincide with comuna borders, and oftentimes the paired blocks will be under the same combo. Since we do not have precise borders for most combos, we cannot say how often this is the case. Nonetheless, the estimates should reflect how combos respond within their territories to different levels of state penetration.

Ideally, we would also have some measure of potential rents,  $\Pi$ , or of initial levels of relative state governance, in order to assess how gangs respond to state rule differently based on these initial conditions. Unfortunately, no credible historical data exists, and current measures of rents would be endogenous and misleading. Exploring such heterogeneity is an important area for future research.

**Identification assumptions and balance** The key identification assumption is that, after the new border was introduced, the only thing that changes discontinuously at the border is the distance from the local state service providers. Broadly this seems to be the case. We have demographic micro-data from a 1993 census (earlier rounds were not available), plus distance from centers of economic activity and other state organs (which could affect volume of transactions and the demand for governance services). Although these data were collected slightly after treatment began, we nonetheless expect them to change negligibly at the border.

Table 3 examines balance in these covariates. Column 1 reports means and standard deviations for all blocks in our sample. Column 2 compares the covariates in the block farthest from the state to the block closest, controlling for the border fixed effects and coordinate polynomials in Equation (6). This tells us whether blocks farther from the state are systematically different from one another (but does not account for the degree of distance). Thus, this is not our principal balance test, but it is useful. Broadly speaking, blocks do not have statistically significant differences in most characteristics. Blocks further from the state have slightly higher elevations, slightly fewer women, and are less likely to have been present before 1948. These first two differences are substantively insignificant, however, being less than 1–2% of the sample mean.

Column 3 reports the main identification test—whether baseline differences in paired blocks correlate with relative local state distance,  $\Delta Y_{ij}$ . The column reports the results of estimating equation (6), except the dependent variables are the covariates  $X$ . Differences in distance to the state are expressed in units of 100 meters. We see a handful of statistically significant correlations, but all are substantively small compared to the sample mean. Thus,

Table 3: Balance test: How baseline traits vary with  $\Delta DistState_{ij}$

	Subsample Mean (SD)	$\Delta$ Difference across border Estimate (SE)	As % of sample mean Estimate (SE)	Correlation with $\Delta StateDist$ Estimate (SE)	As % of sample mean Estimate (SE)
	(1)	(2)	(3)	(4)	(5)
Block average elevation	1,599.58 (115.02)	13.283*** (1.387)	0.008	0.926** (0.460)	0.001
Block present in 1948	0.16 (0.37)	-0.027** (0.012)	-0.166	-0.006 (0.005)	-0.036
Block present in 1970	0.68 (0.47)	0.015 (0.022)	0.022	-0.001 (0.010)	-0.002
Block present in 1985	0.86 (0.34)	0.006 (0.017)	0.007	0.001 (0.007)	0.001
Block present in 1996	0.95 (0.22)	-0.008 (0.012)	-0.008	0.003 (0.006)	0.003
Block average slope	89.95 (0.70)	-0.025 (0.057)	-0.000	-0.013 (0.026)	-0.000
Log of total population (1993)	4.70 (1.09)	0.089 (0.055)	0.019	-0.040* (0.024)	-0.009
Share of women (1993)	0.54 (0.07)	-0.007* (0.004)	-0.013	-0.000 (0.002)	-0.001
Share of population no education (1993)	0.04 (0.07)	0.001 (0.003)	0.028	-0.002 (0.001)	-0.040
Meters to schools	181.65 (118.95)	-3.668 (4.630)	-0.020	0.410 (1.829)	0.002
Meters to health centers	483.11 (315.19)	11.418 (7.039)	0.024	5.733** (2.746)	0.012
Meters to business centers	328.95 (54.89)	2.766 (2.524)	0.008	6.796*** (1.076)	0.021
Number of observations		537	537	536	536
Minimum N		521	521	520	520

*Notes:* Column (1) reports summary statistics. Column (2) computes the difference in covariates in the block farthest from the state to the block closest to the state, adjusting for border fixed effects and coordinates. Column (3) tests whether differences in paired blocks is correlated with state proximity using Equation 6. Standard errors after bootstrapping are nearly identical to our main specification.

a neighborhood that is 100 meters more remote from the local state is about 1 meter higher, 0.04 log points lower population, and be about 6–7 meters farther from business and health centers. A small, systematic correlation between remoteness and such factors is not surprising, but they should not materially affect our estimates. We control for these covariates, however, and later show that their omission has no effect on estimates.

### 5.3 Results

Table 4 reports estimates of the coefficient on  $\Delta DistState_{ij}$  from Equation (6). These reflect the effects of being 100 meters more distant from the state. As expected, state governance and legitimacy fall on blocks farther from state services. For every 100 meters more distant, state governance falls by 0.019, significant at the 1% level. Recall that the median change in distance is 454 meters. Compared to the control mean of 0.41, this implies the median change is associated with a 21% decline in state governance services.

Table 4: Impacts on governance and legitimacy of being 100 meters more distant from the local state, comuna border discontinuity

	$\Delta Y$ sample mean	Correlation with $\Delta StateDist$	Correlation with $\Delta StateDist$ (as sample sd)	
	Subsample Mean (SD)	Estimate (SE)	Estimate (SE)	
	(1)	(2)	(3)	
	(1)	(2)	(4)	
Relative State Governance Index	0.08 (0.24)	0.013 (0.304)	-0.002 (0.006)	-0.006
State Governance Index	0.42 (0.19)	-0.033 (0.272)	-0.019*** (0.005)	-0.101
Combo Governance Index	0.33 (0.22)	-0.033 (0.278)	-0.014** (0.005)	-0.065
Relative State Legitimacy Index	0.14 (0.24)	0.050 (0.328)	-0.009 (0.007)	-0.036
State Legitimacy Index	0.57 (0.15)	-0.021 (0.224)	-0.014*** (0.005)	-0.094
Combo Legitimacy Index	0.43 (0.21)	-0.051 (0.287)	-0.007 (0.006)	-0.036
N for Governance outcomes		532	531	531
N for Legitimacy outcomes		406	404	404

*Notes:* Each estimate comes from a separate regression. Only residents (not business respondents) were asked about legitimacy, and some blocks have only residents, hence the lower sample size.

As blocks get more distant from the state, however, they do not receive more combo rule. On the contrary, combo rule falls along with state responsiveness, by 0.014 per 100 meters. At the median change in distance, this represents a 19.3% decline in gang governance services. Hence we estimate virtually no change in relative state governance in neighborhoods further from the state. In other words, more remote neighborhoods have less protection of all kinds.

We see similar results for police stations and Casas de Justicia when we estimate these terms separately in the same regression (Appendix Table D.2). The point estimates for the Casas are slightly less precise but similar in magnitude to the point estimates for police. This suggests the combo is responding strategically to the less coercive arm of the state as well. We have to take these estimates with caution, however, since these forms of state presence may be correlated with other unobserved municipal services.

**Robustness** These results are robust to using 100- and 300-meter bandwidths, other block matching strategies, different distance measurement and specifications (Appendix Table D.3). All suggest qualitatively similar results. These impacts are also fairly consistent across all 17 individual varieties of governance (Table D.4). Finally, these results are consis-



tent with a range of related outcomes, reported in Appendix Table D.6.<sup>26</sup> For instance, as blocks get more distant from the state, residents are more likely to say that both the state and combo are more difficult to contact and slower to respond to problems. They also report lower perceptions of vacunas; 33% of people in the sample believes others pay security fees, and this is 1.7 percentage points lower for every 100 meters more distant from the state.

## 5.4 Alternative channels of impact

We see three channels of impact through which state proximity could increase gang rule.

**Strategic response by the combo** Sections 3 and 4 discussed the qualitative and theoretical case at length, and we will not repeat it here, despite this being in our view a major, if not the main, driver of our treatment effects.

**Economic development and sorting** Section 4 laid out the theoretical case for economic change contributing to the crowding in of gang rule by state rule. Over three decades, proximity to police, dispute resolution, and other local services could increase prosperity and transactions, and with it demand for combo governance. In addition, households and businesses who demand governance may have moved to better-governed neighborhoods.

We see mixed supporting evidence, though the effects on sorting and growth are not so consistent or large that they seem to be able to plausibly account for the crowding in we observe. Table 5 estimates the effects of state proximity to the state on a range of economic and demographic measures from administrative data, the 2018 census, and our 2019 survey. We see no significant change in the city’s administrative classification of a block’s income stratum, in population, or in business sales and size. For every 100 meters more distant from the state, however, we see significant changes in other proxies for development: a 0.06 standard deviations increase in a finer-grained poverty index; a 0.08 standard deviations increase in the unemployment rate; and a 0.075 standard deviations decrease in the profits reported by firms in our survey. We do not see any observable evidence of residential sorting, as there is little difference in demographic composition. It is notable that we see no difference in business size. It seems unlikely, however, that these changes in economic activity alone can explain such large increases in combo rule—19.6% at the median.

**Misattribution and misreporting** Finally, we consider two kinds of measurement error correlated with treatment, and judge that these too are unlikely to account for the large

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<sup>26</sup>Since the experiment pre-specified the governance index as the primary outcome and legitimacy as the secondary outcome, we focus on these for our main results.

Table 5: Impacts on neighborhood prosperity and demographics of being 100 meters more distant from the local state, comuna border discontinuity

	Subsample Mean (SD)	$\Delta Y$ sample mean Estimate (SE)	Correlation with $\Delta StateDist$ Estimate (SE)	Correlation with $\Delta StateDist$ (as sample sd) Estimate (SE)
	(1)	(2)	(3)	(4)
<i>Panel A: Economic transactions</i>				
Income strata (2018)	2.54 (0.94)	-0.148 (0.833)	-0.018 (0.015)	-0.019
Multidimensional Poverty Index (2018)	13.73 (14.02)	-0.093 (16.438)	0.863*** (0.316)	0.062
Log of mean profits (2019)	13.42 (1.12)	0.049 (1.447)	-0.084* (0.049)	-0.075
Log of mean sales (2019)	14.84 (1.17)	0.053 (1.739)	-0.050 (0.050)	-0.042
Number of employees (2019)	2.49 (2.98)	0.343 (9.480)	-0.090 (0.234)	-0.010
Unemployment rate (2018)	0.11 (0.07)	0.006 (0.093)	0.006*** (0.002)	0.080
Schooling rate (2018)	0.91 (0.12)	0.034 (0.189)	-0.009*** (0.003)	-0.073
Log of total population (2018)	5.18 (0.94)	0.078 (1.261)	-0.015 (0.022)	-0.015
<i>Panel B: Demographics</i>				
Percent of women (2018)	47.49 (15.84)	-0.478 (21.783)	0.119 (0.390)	0.007
Percent of population no education (2018)	1.82 (1.77)	-0.009 (2.181)	0.084** (0.042)	0.048
Percent of population aged 0 to 14 (2018)	14.80 (7.51)	1.935 (9.256)	0.210 (0.165)	0.028
Percent of population who recently migrated (2018)	4.08 (5.12)	-0.501 (6.868)	-0.138 (0.134)	-0.026
Percent of houses with electricity (2018)	0.91 (0.13)	0.028 (0.190)	-0.001 (0.004)	-0.008
Percent of houses with water services (2018)	0.90 (0.13)	0.029 (0.192)	-0.002 (0.004)	-0.018
Percent of houses with sewage (2018)	0.89 (0.14)	0.026 (0.200)	-0.001 (0.004)	-0.006
Percent of houses with gas services (2018)	0.64 (0.29)	0.001 (0.359)	-0.013** (0.006)	-0.046
Percent of houses with trash collection (2018)	0.90 (0.13)	0.025 (0.191)	-0.002 (0.004)	-0.013
Percent of houses with internet services (2018)	0.58 (0.21)	0.015 (0.250)	0.003 (0.004)	0.014
Number of observations		537	536	536
Minimum N		233	230	230

Notes: Each estimate comes from a separate regression.

crowding in effects we observe. A first possibility is *misattribution*—citizens honestly report their perceptions, but they systemically misjudge who provides what services in a way correlated with distance to the state. For instance, suppose the state improves service delivery and residents erroneously attribute this to the combo. Combos could claim credit, or residents could see a more ordered society and credit them. We cannot rule misattribution out, but we see little evidence for it, especially errors so large they explain our sizeable treatment effects. First, we see no instances of credit claiming by the combo. In the field experiment, below, we collect systematic data and find it to be quite rare. Second, respondents reported increased ease of contact and speed of delivery of both the state and combo (Appendix Table D.6)—questions which seem less vulnerable to misattribution. Finally, we designed the questions specifically to minimize this concern: when problem  $x$  happens, how often does  $y$  respond? If we were to point to the absence of problem  $x$  and which actor  $y$  was responsible, this would be a much more speculative answer, prone to attribution problems. In contrast, our impression is that most residents know who actually responds to governance.

A second possibility is *misreporting*—residents know who delivered the services, but misrepresent their responses in a way that is correlated with state proximity. In this case, they would need to be less willing to report combo governance as the state became more remote, which is an unusual direction of measurement error. Moreover, as discussed in Section 2, we conducted the survey to minimize discomfort, and we obtained relatively unbiased answers according to our survey experiment. More concretely, however, we see no significant correlation between state distance and the results of our survey experiment testing for systematic measurement error: the difference between direct and randomized response techniques on security fee payments (Table D.5).

## 6 Field experiment

Finally, we collaborated with the Mayor’s office to scale up and evaluate an intervention designed to increase local, non-coercive state presence and improve access to government services, with the aim of crowding out gang governance. Compared to the border discontinuity, the experimental treatment is of shorter duration, more narrowly focused, and did not change police presence.

In 2017, we learned of a little-known effort in one of Medellín’s under-served barrios, La Loma, with a population of roughly 20,000. A small unit sent 7 outreach staff to the barrio. From 2012–17, these “liaisons” set out to build and improve the organization of social groups and civil society organizations, liaise between city services and the community, and link citizens to existing services. Qualitatively, it appeared that citizen use of state services

increased, that access and legitimacy of the state rose, and (more speculatively) that combos provided less protection services. We worked with the city to test this approach at scale.

## 6.1 Intervention

The city identified 80 “sectors” with a combo but varying levels of relative state–combo governance. A sector is an informal neighborhood, significantly smaller than a barrio like La Loma, usually with about 1,000–3,000 residents. The city intensified normal municipal services in 40 of these sectors for 20 months, beginning April 2018. Control sectors received normal services. See Appendix Figure D.1 for a distribution of these sectors across the city.

At the city level, the Mayor’s office created an inter-agency team to respond to local concerns, such as poor trash pickup, broken playground equipment, or a lack of attention from the city’s dispute resolution officers. Importantly, the aim of the intervention was not to directly challenge gang rule or crowd out their services. Rather, the aim was to increase the visibility, accessibility, and speed of state services. City officials also attended semi-annual formal government-community meetings in the treated sectors, known as *Consejos de Convivencia*, where they and community members would agree on a formal list of commitments. They also organized a large one-time event called *Caravana de la Convivencia*—a weekend-long street festival in the sector where, in addition to music, food and entertainment, representatives from each agency are present to explain their services in detail. The intervention did not affect police and criminal justice activities.

The city also assigned a full-time street-level bureaucrat, a liaison, to each treated sector. Their responsibilities included: coordinating the communication of local concerns, community-state meetings, and the other events above; helping community organizations coordinate local collective action (e.g., coordinating garbage spots and dog excrement norms); providing training to community leaders in dispute resolution and related skills; proactively identifying individual and neighborhood problems and referring them to the relevant city agency for assistance; communicating the city and police’s recommended guidelines for dealing with and correctly reporting nuisances, misdemeanors, and crimes; and referring residents with interpersonal conflicts to the comuna’s dispute resolution office.

Liaisons were similar to the city’s normal professional staff: men and women under the age of 35, with a university education. They had weekly or monthly quotas for the above activities and were held accountable by their supervisors. The liaisons were not so much directly involved in dispute resolution and service delivery as they were an advocate, a source of information, and a source of organizational capacity.

In terms of intensity, normally the city has one liaison per comuna—about 1 per 540

blocks. For the intervention, the Secretariat of Security assigned one liaison to each treatment sector (about 1 per 9 blocks). In some neighborhoods, this was the first time the sector had any direct street presence by the city government other than police. While the liaison represents a 60-fold increase in street-level staff, the broader range of city agencies and services did not increase their efforts to the same degree. The intervention likely resulted in at most a 5-fold increase in state governance.

**Compliance** Compliance was good. The program closely monitored liaisons, and we surveyed and interviewed them all after program completion. From administrative records we know that they spent 3–6 days or evenings per week in their sector, held frequent community events, and generally met their referral quotas, all within the few blocks they were assigned. Thus there was a high level of street visibility of the liaisons for almost two years.

The liaisons reported that combos rarely interfered with their work or attempted to take credit for services delivered. Two-thirds reported no interference whatsoever. The other third mainly said that the combo was mainly watchful, such as observing public events and meetings from a distance.<sup>27</sup> Another liaison described the combo helping her set up for a major event on one occasion. There are few incidents of preventing liaisons from doing their job. The combo prevented two liaisons from entering into the community for the first few weeks, but once they were able to explain their job and role, the liaisons were permitted to enter and perform their jobs without interference.

We see some evidence that municipal agencies struggled to deliver some aspects of the intervention, especially in the sectors where it’s initial presence was weakest. On a scale of 0 to 1 (from full compliance to complete failure to deliver) liaisons rated the wider state compliance roughly 0.34, meaning the state “sometimes” failed to deliver on the requested support. For example, one explained how they organized a meeting between the community and city officials, and the officials never arrived. Another said how they had publicized the new police code—which includes official guidelines for when citizens should call the police versus one of the civilian security and services agencies—but the residents were frustrated because the police did not follow it reliably. These failures were more common in the neighborhoods where relative state rule was weakest at baseline (Appendix Figure D.2). Many of the liaison concerns, however, refer to police presence, which was not experimentally varied and so is not a compliance issue (and is a problem likely shared by control communities). Overall, liaisons reported that the vast majority of their municipal requests were met.

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<sup>27</sup>Appendix Figure D.2 plots incidence of liaison reports of interference on baseline levels of relative combo rule. These incidents were relatively rare and unrelated to gang rule.

## 6.2 Experimental Design

Our primary outcome is relative state governance. Relative legitimacy is our secondary outcome. We also consider the sub-components, but we have not adjusted for multiple comparisons so these must be taken with some caution. We pre-registered the design and outcomes in April 2018, then again prior to final data collection.<sup>28</sup> With 80 sectors, the experiment was powered to detect at least a 12% change in relative state governance.

**Data** Outcome data come from our December 2019 city-wide survey, where in addition to the representative sample of barrios we surveyed approximately 30 residents and businesses per experimental sector. For baseline sector data, we surveyed three officials per sector for their assessment of: (i) relative governance service provided by the combo and the state; (ii) relative street presence of the combo and the state; and (iii) their perceptions of local security and drug use. We also have (iv) geolocated crime statistics for the sector.

**Randomization and balance** We grouped the 80 sectors into 40 matched pairs using the four main baseline measures, then randomized one in the pair to treatment. This produced the expected degree of balance along baseline covariates (Appendix Table D.7).

**Estimation** We estimate intent to treat (ITT) effects via the OLS regression:

$$Y_{isb} = \beta_0 + \beta_1 T_{sb} + \gamma_b + \Theta X_{sb} + \Delta Z_{isb} + \epsilon_{sb} \quad (7)$$

where  $Y$  is the outcome from person  $i$  in sector  $s$  and matched pair  $b$ ;  $T$  is an indicator for random assignment to treatment;  $\gamma$  is a vector of pair fixed effects (the randomization strata);  $X$  is a vector of sector baseline controls; and  $Z$  is a vector of survey respondent demographics. We cluster standard errors at the sector level.

**Addressing spillovers** To minimize the chances of interference between units, we selected neighborhoods at least 250 meters distant from one another. A total of 40 intervention sectors also ensured that increased service delivery would not reduce services in control neighborhoods. Ex-post, we can use our representative city-wide survey to estimate and control for spillover effects, by comparing blocks close to treatment sectors to those close to control sectors. We do not see evidence of such spillovers.<sup>29</sup>

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<sup>28</sup>See social science registry AEARCTR-0002622 and the Journal of Development Economics protocol.

<sup>29</sup>See Appendix Table D.8. As Blattman et al. (2021) note, spillovers in a dense network of blocks can lead to fuzzy clustering, where clusters do not conform to defined areas. Hence we use randomization inference to estimate exact p-values under the sharp null of no treatment effect for any unit.

Table 6: Program impacts on primary and secondary outcomes

	Control Mean	ATE	SE	P-value	ATE as % of SD	N
	(1)	(2)	(3)	(4)	(5)	(6)
Relative State Governance Index	0.066	-0.028*	0.015	0.064	-0.088	2,314
State Governance Index	0.413	-0.017*	0.010	0.089	-0.065	2,362
Combo Governance Index	0.345	0.009	0.011	0.414	0.032	2,316
Relative State Legitimacy Index	0.131	0.012	0.019	0.550	0.037	1,845
State Legitimacy Index	0.572	0.012*	0.007	0.094	0.060	1,906
Combo Legitimacy Index	0.437	0.002	0.015	0.874	0.009	1,845

### 6.3 Results

We see no evidence the intervention crowded out combo governance in treated sectors. Table 6 reports program impacts. On the contrary, we estimate a 0.09 standard deviation *decrease* in relative state governance, significant at the 10% level. This is striking given the intensity and street visibility of the intervention. The sign and general magnitude is consistent across different specifications and controls, though the decrease in relative state governance is not always even marginally significant (Appendix Table D.9). Nonetheless, it is clear we see no increase as originally anticipated.

Looking at the two components of this relative measure, residents in control sectors reported state responsiveness to neighborhood problems of 0.413 on the 0–1 scale, compared to 0.345 for combos (where 0.33 represents “occasional” and 0.66 “frequent” responsiveness). The decrease in the relative measure seems to be driven by a small decrease in perceived state responsiveness and a small increase in combo responsiveness. Both estimates are imprecise, however, and were we to adjust in any manner for the two additional comparisons, they would not be statistically significant at conventional levels. We see similar patterns across most of the 17 individual relative and absolute governance measures (Appendix Table D.10).<sup>30</sup>

Turning to legitimacy, the effect is near zero but in the expected direction (0.037 standard deviations) though not statistically significant. This appears to be associated mainly with an increase in state legitimacy but the sub-component estimates are too imprecise to say.

<sup>30</sup>Business respondents in particular seem to report larger and more statistically significant increase in relative combo governance than residents, though both point in the similar direction, and with so many outcomes we should be cautious about such comparisons.

## 6.4 Candidate explanations

Why do we not see evidence of gang rule getting crowded out? Arguably, these results are consistent with the fact that changing perceptions of state capacity is difficult in the short run, and potentially the beginnings of a modest strategic response by combos. Incidents of non-compliance do not seem to explain the overall experimental results, nor does systematic measurement error. Whatever the reason, however, we see no evidence that people turn to the combo for protection less as a result of intense focus by the state on problem-solving, dispute resolution, and community-led public order.

**Combo strategic response** Combos were often the first to notice an increased state presence, and almost all liaisons described having to explain their presence to the combo. Thus, even if the liaisons and broader state took time to respond to the problems of the average citizen, the combo would have been aware of increased state activity from the beginning. Residents report an almost 0.1 standard deviation decline in perceived relative state governance. The coefficients on both combo governance and legitimacy are positive, albeit not significant. Both effects, however imprecise, are still consistent with the beginnings of a modest strategic response by combos.

Note, however, the intervention does not directly threaten drug corners and other illicit rents in the same manner as police. Liaisons could even reduce the need for police calls. Therefore, perhaps we should not expect the same degree of combo strategic response compared to an intervention that increases the presence of patrols. In other words, non-policing interventions might not generate externalities large enough to generate a large and positive cross-elasticity between state and gang governance, even in the long run. That said, recall that the results of the quasi-experiment suggested that combos were equally responsible to the coercive and non-coercive arms of the state (Appendix Table D.2).

**Difficulty of increasing awareness of state capacity in the short run** On the one hand, 20 months of highly visible street presence in a relatively small area is a dramatic increase in governance. On the other hand, these results could indicate that it may take more time for a randomly selected citizen to experience an issue and personally experience the state's responsiveness. Likewise, they may update their beliefs about the state only after repeated interaction over longer periods of time. If so, it suggests that increasing state legitimacy is not only limited by the normal challenges of growing state capacity, but that these effects may take time to turn into civilian loyalty and collaboration.

What should we make of the negative sign on absolute state governance? There are a few reasons to treat the sign with caution, and focus on the relative state governance



measure. First, recall that this decline is not especially robust, and that we did not adjust standard errors on these sub-components for multiple comparisons. Moreover, the absolute responsiveness is a subjective measure, and survey respondent answers may be influenced by the relative responsiveness of the state versus the combo. Finally, a decrease in state responsiveness stands in contrast to the marginally significant increase in state legitimacy. For all these reasons, we rely more on the prespecified relative state governance measure.

Nonetheless, there are signs that the negative sign is driven by the neighborhoods that experienced partial non-compliance issues. We turn to this next.

**Partial noncompliance** At the outset, all 80 sectors had a strong combo presence and a degree of gang rule, but levels of relative state governance varied widely. We anticipated that the intervention could have different impacts where gangs had chosen to provide more rule. Thus, we prespecified heterogeneity analysis by our baseline measure of relative state governance. Table 7 reports treatment effects by these sub-groups.

As discussed above, liaisons reported modest compliance issues in the sectors with the lowest initial levels of relative state governance (see Appendix Figure D.2). Perhaps not surprisingly, the negative treatment effect on absolute and relative state governance appears to be concentrated in those neighborhoods with the highest initial levels of relative combo governance (Table 7, Panel A). Granted, the difference in governance treatment effects between the two subgroups is not statistically significant (Panel C). Nonetheless, it's consistent with partial noncompliance undermining the state's efforts to bolster its capacity and legitimacy.

If we focus on the subgroup with high levels of compliance (Panel B), it is striking that we still see no evidence of crowding out of gang rule. The impact on relative state governance still has a negative sign, and the impact on combo governance still has a positive sign. Both are imprecise, but both point in the opposite direction of the conventional wisdom.

What's more, we see large and statistically significant increases in relative and absolute state legitimacy in these high-compliance neighborhoods. This is arguably consistent with our other candidate explanations—a strategic response by the combo (elevating relative state legitimacy but not relative service provision) and also the difficulty of affecting beliefs on state responsiveness in the short term (but legitimacy credit for visible presence).

**Misattribution and misreporting** Finally, measurement error seems unlikely to account for our results. In terms of misattribution, we saw almost no instances of combo credit-claiming. Also, liaisons were highly visible and quite clearly not part of the combo.

In terms of misreporting, residents in treated sectors would have needed to systematically under-report state governance and over-report combo governance to an independent survey

Table 7: Program impacts in pre-specified subgroups: High versus low relative gang rule

	Control Mean	ATE	SE	P-value	ATE as % of SD	N
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Above median baseline relative state governance</i>						
Relative State Governance Index	0.015	-0.043**	0.020	0.038	-0.135	1,146
State Governance Index	0.393	-0.029**	0.014	0.045	-0.117	1,167
Combo Governance Index	0.377	0.012	0.014	0.385	0.042	1,147
Relative State Legitimacy Index	0.110	-0.031	0.027	0.265	-0.096	919
State Legitimacy Index	0.568	-0.010	0.009	0.277	-0.048	941
Combo Legitimacy Index	0.456	0.022	0.022	0.329	0.080	919
<i>Panel B: Below median baseline relative state governance</i>						
Relative State Governance Index	0.117	-0.014	0.022	0.519	-0.046	1,168
State Governance Index	0.433	-0.005	0.013	0.719	-0.018	1,195
Combo Governance Index	0.313	0.007	0.017	0.700	0.024	1,169
Relative State Legitimacy Index	0.152	0.056**	0.026	0.039	0.185	926
State Legitimacy Index	0.576	0.035***	0.011	0.002	0.174	965
Combo Legitimacy Index	0.419	-0.019	0.021	0.369	-0.068	926
<i>Panel C: Subgroup difference</i>						
Relative State Governance Index	0.066	-0.030	0.029	0.306	-0.093	1,146
State Governance Index	0.413	-0.026	0.019	0.170	-0.101	1,167
Combo Governance Index	0.345	0.005	0.022	0.800	0.019	1,147
Relative State Legitimacy Index	0.131	-0.085**	0.037	0.022	-0.270	919
State Legitimacy Index	0.572	-0.045***	0.014	0.001	-0.216	941
Combo Legitimacy Index	0.437	0.039	0.030	0.188	0.142	919

firm that they had no reason to associate with the intervention. And they would need to do so especially in treated areas with high initial combo governance. Their motive for doing so is not apparent. Appendix Table D.11 tests for correlations between treatment status and our proxies for measurement error—non-response to combo-related questions relative to responses about the state, and the randomized response survey experiment. We do not see significant evidence of misreporting. It does appear that there is a slight tendency to under-report extortion in control sectors, but this estimate is imprecise.

## 7 Conclusions

Organized crime and gang rule is a problem faced by many emerging strong states. In hundreds of major cities, states operate in an uneasy duopoly of coercion, rule, and taxation with urban gangs. Like Medellín, these governments are often relatively strong in the sense of fiscal capacity and public goods provision. But before states can regulate or reduce criminal systems and rule, we first have to understand them.

Our work suggests a few important insights. First, the market for protection is not the main reason gangs decide to rule. This direct profit motive matters, and selling protection

can be a profitable business line. But this direct profit motive is overshadowed by the externalities for other business lines, especially retail drug sales. It may even be dominated by less pecuniary payoffs, such as gang leaders' desire for status and respect in the community.

Second, as a consequence, it may be very difficult for governments to crowd out gangs simply by striving to govern more and better. This is especially true once gangs have already made investments in speed and access of service, and in their capacity to collect fees and "taxes." Our quasi-experimental results suggest that the state may have inadvertently crowded in gang rule, directly by spurring a strategic response by the combos, and indirectly by increasing demand for their existing services. (Our model and results also suggest a prediction that could be tested in future work—a combo's strategic response ought to increase with the size and profitability of drug markets. At present, neither historical nor contemporary data on drugs are available. Research on these markets and their interaction with criminal governance is an important area for future research.)

Third, in the short run, gangs and the state both face costs and capacity constraints in ruling, making it slow and costly to scale up. Our experimental results point to the difficulty of making headway. Even in the neighborhoods where the city did intensify actual governance, the average citizen was not immediately aware, suggesting it takes time to build relationships, build awareness of and coordinate on state actors, and foster legitimacy—even before worrying about a combo's strategic response.

Fourth, both our results and our theoretical discussion suggest that other common policy interventions may not reduce gang rule. For instance, popular responses to extortion include police crackdowns, ease of anonymous denunciation, or facilitating collective action among merchants. But our model and findings suggest these measures could fail in the presence of gang rule. They ignore the indirect payoffs to governing, and the fact that gangs would have an incentive to rule even if they were unable to collect extortion at all. Crackdowns and denunciations, moreover, may even increase incentives for the gang to foster legitimacy. And since extortion is a modest percentage of business sales (about 1%), merchants may have weak incentives to undertake costly, risky collective action.

Fifth, gang abuses, including purely extractive extortion, are disciplined primarily by their need for community loyalty, collaboration, and respect. Medellín's gangs are most extractive of outsiders, like bus lines. This suggests that civilian perceptions and attitudes can be a powerful tool on gang regulation. Speculatively, it may be possible to undermine norms of loyalty to the gang through social norms marketing campaigns or collective action. This seems like a crucial area for policy experimentation.

Finally, it suggests that besides prosecuting criminal leaders and governing better, states need to tackle gang revenues. For instance, lower profitability of drug markets (e.g., because

of marijuana legalization) could reduce optimal gang size as well as incentives to govern. In defeating the American mafia, prosecutors attribute their success not simply to more aggressive investigation and sentencing, but to the slow erosion of the mob's main sources of revenues. In the late 20th century, loansharking, numbers games, and labor racketeering with widespread access to consumer credit, lotteries, and the decline of unionization (Kroger, 2008). So long as there is high consumer demand for illicit drugs and loans, crackdowns and crowding out will not undermine the gang's main motives for existence and ruling.

The world needs more descriptive and theoretical work on gangs, and more evaluations. Organized crime is arguably the largest threat to national security and development in the century ahead, especially in the Western hemisphere. When gangs fight one another or the government, they provoke armed violence exceeding most civil wars (Lessing, 2017). Organized criminal groups are the main source of illegal guns, migrants and refugees into the US. Unfortunately, the policy cupboard is nearly bare. Mayors and police chiefs do not know how to combat organized crime or associated phenomena, such as gang rule.

What's more, the problem is set to worsen and widen in coming decades. Many organized criminal groups in Latin America emerged during and after wars, from demobilizing paramilitaries and rebels. Likewise, in Sicily, the first mafias emerged from the ranks of unemployed private security forces from the former feudal estates. Fighters in Iraq, Syria, Afghanistan, Myanmar, and other states could follow the same path in the coming decades.

We view our qualitative interviews, the descriptive analysis, and the field experiment in particular as proof-of-concept exercises. We show how it is possible to develop systematic qualitative and quantitative data on these organizations, and that it is possible to evaluate gang-level interventions rigorously, ethically, and logistically.

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# Appendix

## A Assessing measurement error in outcomes

**Vacuna survey experiment** Paying vacunas seemed to be one of the more sensitive questions on the survey, according to our qualitative experiences. To test this, we randomized how we asked the question. Some respondents were asked directly whether they paid a regular vacuna (Direct Response, or DR); others were asked to use a Randomized Response (RR) technique, where they privately flipped a coin and reported honestly only if it is heads.<sup>31</sup>

We see little statistically significant differences across the two methods. Randomized response elicited an extortion rate of 22.6% from businesses and 6% from households, compared to 19.4% and 7.8% when directly asked. The differences run in opposite directions and are not statistically significant.

In Figure A.1 we calculate the difference between the RR and DR methods at the barrio level, and plot this difference against combo governance levels. A simple regression line is relatively flat at zero, indicating that misreporting is no more or less common in areas where the combos are more involved in daily life, and hence where legitimacy or fear could potentially have influenced under-reporting.

**Patterns of non-response** We also examine patterns of non-response. One concern we had in piloting the survey was that respondents who do not want to talk about the combo may say “I don’t know” or pass on answering that question, and enumerators are permitted to skip questions. Just 7% of the sample answered don’t know or skipped at least 25% of the combo services questions. If this were primarily driven by worries about combo, we might expect a correlation between combo governance and the proportion of questions unanswered. Figure A.2 plots each respondent’s percentage of answered questions against barrio-level measures of combo and mayoral governance levels. We see no substantively or statistically significant correlations. Control group members answer 85 to 97% of sensitive questions regarding the combo. This is 0.2 to 1.1 percentage points lower in the treatment group, though neither coefficient is statistically significant.

## B Combo census

The authorities have never developed a systematic list of combos. To create our census, we began by integrating five existing lists that are known to be incomplete:

1. A public list of 246 combos from the leading local newspaper El Colombiano from 2014, updated in 2017;

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<sup>31</sup>Others were asked the same question using the List Experiment technique (where half are asked to give the number of four nonsensitive actions they engaged in, and half see a list of 5 actions including paying extortion). In general, these list experiment results are extremely noisy, as is commonly the nature of list experiments. The results are consistent with what we see in the RR and DR differences. There is no evidence of systematic measurement error.



Figure A.1: Survey experiment results: Difference between randomized response (RR) and direct response (DR) to vacuna payment rate

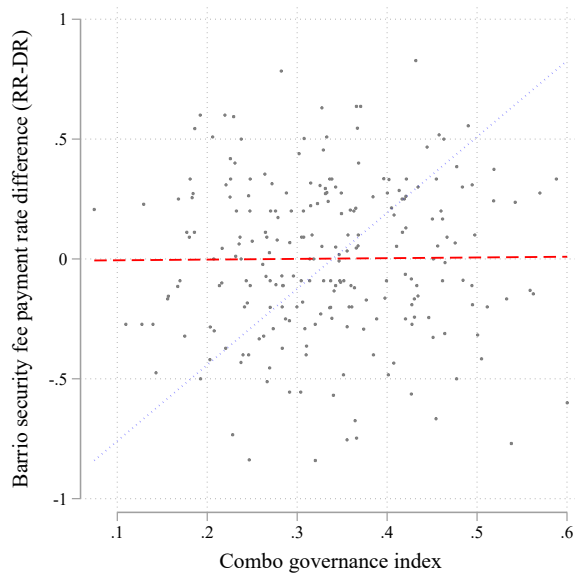
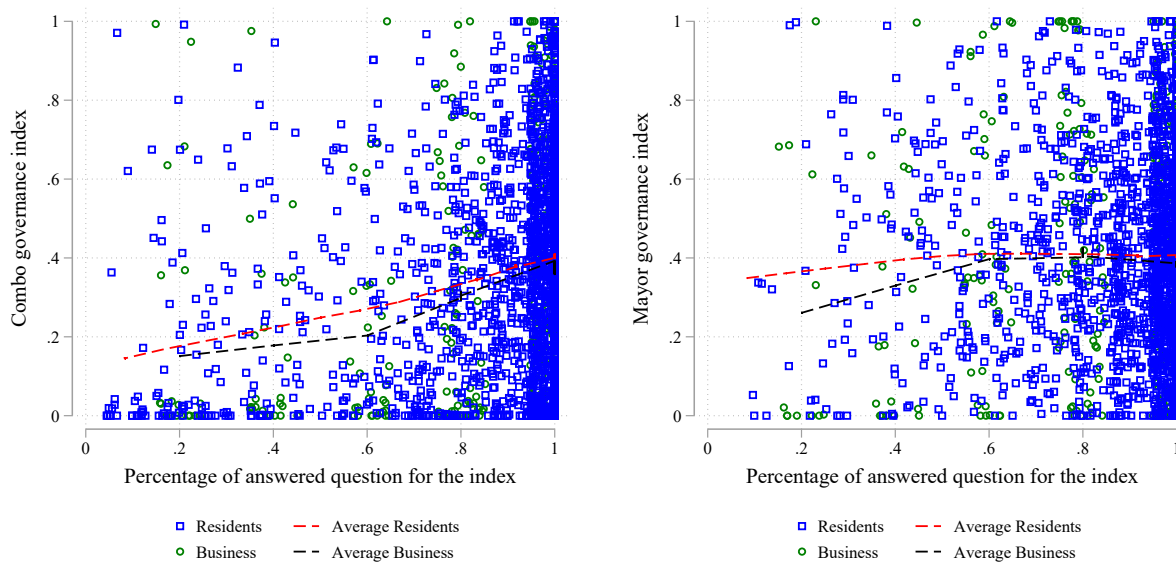


Figure A.2: Correlation of respondent's answer percentage and governance levels



*Notes:* Y- axis plots barrio-level measures of combo and mayoral governance levels, X-axis plots each respondent's %age of answered questions.

2. A confidential list of 96 combos from the Medellín's Mayor's Office from 2010;
3. A confidential list of 190 combos from the Office of the Attorney General from 2015;
4. A confidential list of 87 combos from the Medellín's Mayor's Office from 2017; and
5. A confidential list of 89 combos from the Medellín's Mayor's Office, the Office of the General Attorney, and the National Police from 2018.

We then tried to validate the existence of the combos as well as identify a prime location, usually a drug selling spot or their headquarters. We did so using primary contacts with current and former combo and razón members, local experts (especially journalists and community leaders), as well as with law enforcement officials. The principal challenges here included:

- distinguishing between razones and combos;
- determining what groups were actually "sub-combos"—departments or subdivisions within a single combo;
- identifying cases of multiple names for the same combo (these may be historical names, nicknames, or police/journalist error in one of the lists; and
- assessing defunct combos that have disbanded or absorbed into another combo.

Altogether, this exercise has some limitations, including:

- We did not physically search the streets for combos, or speak directly to residents and leaders in a systematic way, and confirm combos in that matter. Our many qualitative interviews with residents, leaders, combo members and experts did inform our classification, however.
- We also did not systematically count and verify combos in neighboring municipalities, and so 50 combos in the rest of the metropolitan area is a rough estimate.
- Our merger and validation of the existing lists relies heavily on the expertise and judgment of 3 professional experts, journalists, and former criminal group members.

## C Formal presentation and extensions of model

This section elaborates details of the model and several claims in Section 4.

### C.1 Cournot competition

**Setup** In each neighborhood, a gang  $g$  and a state  $s$  compete to sell protection in quantities  $q_g$  and  $q_s$ . Each organization chooses  $q_i$  to maximize their respective pay-off, and each has constant marginal cost  $c_i$ . Products are differentiated, and the price of each one is given by the linear inverse demand function  $p_i = a_i - \beta q_i - \gamma q_j$ . Here,  $\gamma \in (0, 1]$  since the services offered by both organizations are substitutes, and  $\beta > 0$  for downward-sloping demand. The pay-off for each organization is  $V_i = p_i q_i - c_i q_i$ . For simplicity, we assume an interior solution.

**Nash Equilibria** We begin by deriving the best response function for each organization:

$$\begin{aligned} \max_{q_i} V_i &= (a_i - \beta q_i - \gamma q_j) q_i - c_i q_i \\ \frac{\partial V_i}{\partial q_i} &= a_i - 2\beta q_i - \gamma q_j - c_i = 0 \\ q_i^* &= \frac{a_i - c_i}{2\beta} - \frac{\gamma}{2\beta} q_j \end{aligned}$$

We obtain an identical best response function for the other organization analogously, and replacing values we obtain:

$$q_i^* = \frac{2\beta(a_i - c_i) - \gamma(a_j - c_j)}{(4\beta^2 - \gamma^2)}$$

and,

$$q_j^* = \frac{2\beta(a_j - c_j) - \gamma(a_i - c_i)}{(4\beta^2 - \gamma^2)}.$$

**Comparative statics** We are mainly interested in whether gang rule is crowded in or out when there is an exogenous increase in state governance:  $\frac{\partial q_i^*}{\partial q_j}$ . To obtain this comparative static, we begin by defining:

$$G(q_i, q_j) \equiv \frac{\partial V_i}{\partial q_i} = a_i - 2\beta q_i - \gamma q_j - c_i$$

which is a continuously differentiable function from  $\mathbb{R}^2 \rightarrow \mathbb{R}$ . At the optimum, we know:

$$G(q_i^*, q_j^*) = a_i - 2\beta q_i^* - \gamma q_j^* - c_i = 0.$$

Since  $-2\beta \neq 0$ , we can use the implicit function theorem to obtain our main comparative static:

$$\frac{\partial q_i^*}{\partial q_j} = -\frac{\partial G(q_i, q_j)/\partial q_j}{\partial G(q_i, q_j)/\partial q_i} = -\frac{\gamma}{2\beta}$$

Since the two services are not complements, this comparative static implies that increases in one duopolist's supply of protection will reduce the other's.

### Cournot competition with benefits to governing

We now introduce a non-standard feature: externalities stemming from gang rule.

**Setup** As above, but now the payoff for each organization is  $V_i = (a_i - \beta_i q_i - \gamma q_j)q_i - c_i q_i + \rho(q_i, q_j)\pi_i$ , where  $\rho(q_i, q_j)\pi_i$  captures the externalities described in Section 4. For simplicity, we assume an interior solution.

**Nash Equilibria** We begin by deriving the best response function for each organization:

$$\begin{aligned} \max_{q_i} V_i &= (a_i - \beta q_i - \gamma q_j)q_i - c_i q_i + \rho(q_i, q_j)\pi_i \\ \frac{\partial V_i}{\partial q_i} &= a_i - 2\beta q_i - \gamma q_j - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i}\pi_i = 0 \\ q_i^* &= \frac{a_i - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i}\pi_i}{2\beta} - \frac{\gamma}{2\beta}q_j \end{aligned}$$

We obtain an identical best response function for the other organization analogously, and replacing values we obtain:

$$q_i^* = \frac{2\beta(a_i - c_i) - \gamma(a_j - c_j) + \left(2\beta \frac{\partial \rho(q_i, q_j)}{\partial q_i}\pi_i - \gamma \frac{\partial \rho(q_i, q_j)}{\partial q_j}\pi_j\right)}{(4\beta^2 - \gamma^2)}$$

with an identical function for  $q_j^*$ .

**Comparative statics** Again we are interested in whether gang rule is crowded in or out when there is an exogenous increase in state governance:  $\frac{\partial q_i^*}{\partial q_j}$ . To obtain this comparative static, we begin by defining:

$$G(q_i, q_j) \equiv \frac{\partial V_i}{\partial q_i} = a_i - 2\beta q_i - \gamma q_j - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i}\pi_i$$

which is a continuously differentiable function from  $\mathbb{R}^2 \rightarrow \mathbb{R}$ . At the optimum, we know:

$$G(q_i^*, q_j^*) = a_i - 2\beta q_i^* - \gamma q_j^* - c_i + \frac{\partial \rho(q_i^*, q_j^*)}{\partial q_i} \pi_i = 0$$

Additionally, we assume that  $2\beta \neq \frac{\partial^2 \rho(q_i^*, q_j^*)}{\partial q_i \partial q_i} \pi_i$ , thus:

$$\frac{\partial G(q_i^*, q_j^*)}{\partial q_i} = -2\beta + \frac{\partial^2 \rho(q_i^*, q_j^*)}{\partial q_i \partial q_i} \pi_i \neq 0$$

We can use the implicit function theorem to obtain our main comparative static:

$$\frac{\partial q_i^*}{\partial q_j} = -\frac{\partial G(q_i, q_j)/\partial q_j}{\partial G(q_i, q_j)/\partial q_i} = \frac{\lambda \pi_i - \gamma}{2\beta - \delta \pi_i}$$

where  $\lambda = \frac{\partial^2 \rho(q_i, q_j)}{\partial q_i \partial q_j}$  represents the cross-partial derivative between gang and state governance, and  $\delta = \frac{\partial^2 \rho(q_i, q_j)}{\partial q_i \partial q_i}$  reflects the rate of increasing or decreasing returns to governing. We discuss conditions for this comparative static to be positive in section 4.

## C.2 Cournot competition with endogenous demand

We now consider the possibility that providing governance can produce economic growth, which in turn may produce greater demand for governance. This section incorporates this idea into the Cournot framework by “endogenizing demand”. The next section considers a political economy model in which “stationary bandits” provide public goods in order to grow the economic pie that they will tax.

**Setup** As before, except we now generalize the functional form of demand such that products are differentiated so the price of each one is determined by  $p_i = a_i(q_i, q_j) - \beta q_i - \gamma q_j$ , where  $\gamma \in (0, 1]$  as services provided by both organizations are substitutes, and  $a_i(q_i, q_j)$  is twice continuously differentiable. The payoff function is  $V_i = (a_i(q_i, q_j) - \beta q_i - \gamma q_j)q_i - c_i q_i + \rho(q_i, q_j)\pi_i$ , where  $\rho(q_i, q_j)\pi_i$  captures the externalities described in Section 4. Again, we assume an interior solution.

**Nash Equilibria** As above, we begin by deriving the best response function for each organization:

$$\begin{aligned} \max_{q_i} V_i &= (a_i(q_i, q_j) - \beta q_i - \gamma q_j)q_i - c_i q_i + \rho(q_i, q_j)\pi_i \\ \frac{\partial V_i}{\partial q_i} &= a_i(q_i, q_j) + \frac{\partial a_i(q_i, q_j)}{\partial q_i} q_i - 2\beta q_i - \gamma q_j - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i} \pi_i = 0 \\ q_i^* &= \frac{a_i(q_i, q_j) - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i} \pi_i}{2\beta - \frac{\partial a_i(q_i, q_j)}{\partial q_i}} - \frac{\gamma}{2\beta - \frac{\partial a_i(q_i, q_j)}{\partial q_i}} q_j \end{aligned}$$

We obtain an identical best response function for the other organization analogously, and replacing values we obtain:

$$q_i^* = \frac{\left(a_i(q_i, q_j) - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i} \pi_i\right) \left(2\beta - \frac{\partial a_j(q_i, q_j)}{\partial q_j}\right) - \gamma \left(a_j(q_i, q_j) - c_j + \frac{\partial \rho(q_i, q_j)}{\partial q_j} \pi_j\right)}{\left(2\beta - \frac{\partial a_i(q_i, q_j)}{\partial q_i}\right) \left(2\beta - \frac{\partial a_j(q_i, q_j)}{\partial q_j}\right) + \gamma^2}$$

and similarly for  $q_j^*$ .

**Comparative Statics** To obtain the key comparative static, we define

$$G(q_i, q_j) \equiv \frac{\partial V_i}{\partial q_i} = a_i(q_i, q_j) + \frac{\partial a_i(q_i, q_j)}{\partial q_i} q_i - 2\beta q_i - \gamma q_j - c_i + \frac{\partial \rho(q_i, q_j)}{\partial q_i} \pi_i$$

which is a continuously differentiable function from  $\mathbb{R}^2 \rightarrow \mathbb{R}$ . At the optimum, we know that  $G(q_i^*, q_j^*) = 0$ . We also assume that  $\frac{\partial G(q_i^*, q_j^*)}{\partial q_i} \neq 0$ .

Then, we can use the implicit function theorem to obtain our main comparative static in the formulation with endogenous demand. Note this implies that there is a neighborhood of  $(q_i^*, q_j^*)$  such that when  $q_i$  is close enough to  $q_i^*$ , we have a unique  $q_j$  such as  $G(q_i, q_j) = 0$ , which makes  $q_j$  a continuous function of  $q_i$ . The comparative static is:

$$\frac{\partial q_i}{\partial q_j} = - \frac{\partial G(q_i, q_j) / \partial q_j}{\partial G(q_i, q_j) / \partial q_i} = - \frac{\frac{\partial a_i(q_i, q_j)}{\partial q_j} + \frac{\partial^2 a_i(q_i, q_j)}{\partial q_i \partial q_j} q_i - \gamma + \frac{\partial^2 \rho(q_i, q_j)}{\partial q_i \partial q_j} \pi_j}{2 * \frac{\partial a_i(q_i, q_j)}{\partial q_i} + \frac{\partial^2 a_i(q_i, q_j)}{\partial q_i \partial q_i} q_i - 2\beta + \frac{\partial^2 \rho(q_i, q_j)}{\partial q_i \partial q_i} \pi_i}$$

Now, a sufficient condition for having a positive cross partial is that  $a_i(\cdot)$  is downward sloping on the product  $\left(\frac{\partial a_i(q_i, q_j)}{\partial q_i} < 0\right)$ , that the decrease is at decreasing rates  $\left(\frac{\partial^2 a_i(q_i, q_j)}{\partial q_i \partial q_i} < 0\right)$ , that the loyalty function is concave  $\left(\frac{\partial^2 \rho(q_i, q_j)}{\partial q_i \partial q_i} < 0\right)$ , and that  $\frac{\partial a_i(q_i, q_j)}{\partial q_j} + \frac{\partial^2 a_i(q_i, q_j)}{\partial q_i \partial q_j} q_i - \gamma + \frac{\partial^2 \rho(q_i, q_j)}{\partial q_i \partial q_j} > 0$ . The interpretation of the last condition would depend on what captures  $a_i(\cdot)$ .

### C.3 Bertrand competition with differentiated products

Now we model a state and a gang engaging in Bertrand competition with differentiated products and externalities from gang governance. Each organization has a certain valuation of the loyalty of the people.

**Setup** A gang  $g$  and a state  $s$  compete over prices  $p_g$  and  $p_s$ . Each organization chooses to maximize their respective pay-off based on parameters. Both organizations have a constant marginal cost  $c$ . Products are differentiated so the quantity demanded of each one is given by  $q_i = a_i - b_i p_i - \gamma p_j$ , where  $\gamma < 0$  as goods produced by both organizations are assumed to be substitutes. The pay-off for each organization is  $\nu_i = (a_i - b_i p_i - \gamma p_j)(p_i - c) + F(p_i, p_j)$ , where  $F(p_i, p_j)$  captures externalities. For simplicity, we assume an interior solution.

**Nash Equilibria** We begin by deriving the best response function for each organization:

$$\begin{aligned}\max_{p_i} \nu_i &= (a_i - b_i p_i - \gamma p_j)(p_i - c) + F(p_i, p_j) \\ \frac{\partial \nu_i}{\partial p_i} &= (a_i - 2b_i p_i - \gamma p_j - b_i c) + \frac{\partial F(p_i, p_j)}{\partial p_i} = 0 \\ p_i^* &= \frac{\left(a_i - b_i c + \frac{\partial F(p_i, p_j)}{\partial p_i}\right)}{2b_i} - \frac{\gamma p_j}{2b_i}\end{aligned}$$

Analogously, we obtain an identical best response function for the other organization. Replacing values we obtain:

$$p_i^* = \frac{2b_j a_i - \gamma a_j - (2b_j b_i - \gamma b_j)c + 2b_j \frac{\partial F(p_i^*, p_j^*)}{\partial p_i} - \gamma \frac{\partial F(p_i^*, p_j^*)}{\partial p_j}}{(4b_i b_j - \gamma^2)}$$

**Comparative statics** To obtain our key comparative static, we define:

$$M(p_i, p_j) \equiv \frac{\partial \nu_i}{\partial p_i} = (a_i - 2b_i p_i - \gamma p_j - b_i c) + \frac{\partial F(p_i, p_j)}{\partial p_i}$$

which is a continuously differentiable function. In the optimum we know that:

$$M(p_i^*, p_j^*) = \frac{\partial \nu_i}{\partial p_i} = (a_i - 2b_i p_i^* - \gamma p_j^* - b_i c) + \frac{\partial F(p_i^*, p_j^*)}{\partial p_i} = 0$$

Additionally, we assume that  $2b_i \neq \frac{\partial^2 F(p_i^*, p_j^*)}{\partial p_i \partial p_i}$ , so:

$$\frac{\partial M(p_i^*, p_j^*)}{\partial p_i} = -2b_i + \frac{\partial^2 F(p_i^*, p_j^*)}{\partial p_i \partial p_i} \neq 0$$

Then we can use the implicit function theorem, obtaining the following result:

$$\frac{\partial p_i}{\partial p_j} = -\frac{\partial M / \partial p_j}{\partial M / \partial p_i} = -\frac{\gamma - \frac{\partial^2 F(p_i^*, p_j^*)}{\partial p_i \partial p_j}}{2b_i - \frac{\partial^2 F(p_i^*, p_j^*)}{\partial p_i \partial p_i}} \quad (8)$$

Thus, to have a crowding in effect ( $\frac{\partial p_i}{\partial p_j} > 0$ ) we require that  $\gamma < \frac{\partial^2 F(p_i^*, p_j^*)}{\partial p_i \partial p_j}$ . Since  $\gamma < 0$ , a sufficient condition for the state to crowd in the gang is that both services are complements in loyalty. Generally, it is enough that the complementarity in loyalty is higher than the degree of substitution of these services.

## C.4 Public goods and encompassing interest

Section C.2 showed how state-provided protection and governance may “grow the pie”, and how this can be incorporated into standard models of duopolistic competition. This idea lies at the very heart of standard political economy of governance and public-goods provision. This section adapts the classic Olson & McGuire (1996) (henceforth OM) model, in which stationary bandits face incentives to curtail their own coercive taxation and provide public goods at their own expense, precisely because doing so grows the pie that the bandit later taxes. We abstract from OM’s comparison of autocracy and democracy, instead comparing the baseline OM model of a monopolistic, autocratic stationary bandit to a modified version in which two stationary bandits tax and provide public goods to the same subject population. A simple two-bandit model predicts crowding out. We first illustrate the single-bandit baseline model and then add the second stationary bandit.

### C.4.1 Baseline: One Autocratic Stationary Bandit

- One player: The state ( $S$ ) makes two independent choices, setting a level of public goods provision ( $G_s$ ) and a uniform tax rate  $t_s \in [0, 1]$ .
- The output of the economy  $Y$  is increasing convexly in the total amount of public goods provided  $G$  (which here equals  $G_s$  since there is only one stationary bandit), and no production is possible without *some* amount of public goods. That is, for  $Y(G)$  we assume  $Y(0) = 0$ ,  $\frac{\partial Y(G)}{\partial G} > 0$ , and  $\frac{\partial^2 Y(G)}{\partial G^2} < 0$ .
- We assume that taxation distorts economic activity. Write  $\tau(t_s) \in [0, 1]$  represent the loss factor due to taxation, so that final GDP is equal to  $\tau(t_s) * Y(G_s)$ . We assume  $\frac{\partial \tau(t_s)}{\partial t_s} < 0$ .

The state maximization problem is given by:

$$V_s = \tau(t_s) \cdot t_s \cdot Y(G) - cG_s \quad (9)$$

For simplicity, we normalize  $c$  to 1. By construction,  $S$  sets taxes independently of the desired level of public good. At the optimal  $t_s^*$ ,  $S$ ’s gains from taxation and the increases of potential output losses due to further distortion into the economy are equal:  $\tau(t_s) * t_s = \frac{\partial \tau(t_s)}{\partial t_s}$ . This can be seen in the FOC for Equation 9:

$$\begin{aligned} \frac{\partial V_s}{\partial t_s} &= \left( \tau(t_s) + t_s \frac{\partial \tau(t_s)}{\partial t_s} \right) \cdot Y(G_s) = 0 \\ \tau(t_s) + t_s \frac{\partial \tau(t_s)}{\partial t_s} &= 0 \\ t_s^* &= - \frac{\tau(t_s)}{\frac{\partial \tau(t_s)}{\partial t_s}} \end{aligned} \quad (10)$$



Finally, the state selects the level of public good in the point where the marginal revenue is equal to the marginal cost of the public good  $c$  multiplied by the reciprocal of state's share of the national potential income.

$$\frac{\partial V_s}{\partial G_s} = \tau(t_s) \cdot t_s \cdot Y'(G_s) - 1 = 0$$

$$Y'(G_s^*) = \frac{1}{t_s^* \tau(t_s^*)} \quad (11)$$

#### C.4.2 Dual stationary bandits

The setup is similar but with two players, a state ( $s$ ) and gang ( $g$ ).

- Players  $i \in \{s, g\}$  simultaneously choose levels of public-goods provision ( $G_i$ ) and a uniform tax rate ( $t_i$ ) which, as before, is independent of public-goods provision.
- Economic output depends on the total of the two actors' public good provision:  $Y(G)$  where  $G \equiv G_s + G_g$ . As before,  $Y(0) = 0$ ,  $\frac{\partial Y(G)}{\partial G} > 0$  and  $\frac{\partial^2 Y(G)}{\partial G^2} < 0$
- Distortion  $\tau(t)$  depends on the total amount of taxes levied:  $t \equiv t_s + t_g$ . As before:  $\tau(0) = 1$  and  $\frac{\partial \tau}{\partial t} < 0$ .
- To ensure that neither player sets  $t_i > .5$ , we assume that  $\tau(.5) = 0$ .

Players' utility functions and maximization problems are symmetrical:

$$V_i = t_i \cdot \tau(t_i + t_j) \cdot Y(G_i + G_j) - c_i G_i \text{ for } i, j \in \{s, g\} \quad (12)$$

In this simplest, symmetric-players iteration, we will assume that  $c_i = c_j = 1$ . Asymmetric costs raise important questions of sequencing, and will be considered in future iterations. As before, we solve the two maximization problems separately, starting with taxation.

#### Optimal taxation with dual stationary bandits

**Lemma C.1.** *In equilibrium, players' optimal tax rates are identical:  $t_i^* = t_j^*$ .*

*Proof.* From 12, the FOC for player  $i$  is

$$\frac{\partial V_i}{\partial t_i} = \tau(t_i + t_j) Y(G) + t_i \frac{\partial \tau(t_i + t_j)}{\partial t_i} Y(G) = 0 \quad (13)$$

$$= \tau(t_i^* + t_j^*) + t_i \frac{\partial \tau(t_i + t_j)}{\partial t_i} = 0 \quad (14)$$

We can rewrite  $\frac{\partial \tau(t_i + t_j)}{\partial t_i}$  as  $\frac{\partial \tau(t_i + t_j)}{\partial t} \frac{\partial t}{\partial t_i} = \frac{\partial \tau(t_i + t_j)}{\partial t} = \tau'(t)$ . This yields:

$$t_i^*(t_j) = -\frac{\tau(t_i^* + t_j)}{\tau'(t_i^* + t_j)}$$

By a similar derivation,  $t_j^*(t_i) = -\frac{\tau(t_i+t_j^*)}{\tau'(t_i+t_j^*)}$

So in Nash Equilibrium:

$$t_i^* = -\frac{\tau(t_i^* + t_j^*)}{\tau'(t_i^* + t_j^*)} = t_j^*$$

□

In words, when  $i$  increases  $t_i$  he gets a larger share of a smaller pie. These two effects must be of equal size at the optimum  $t_i^*$ . But the negative effect on the size of the pie is the same whether  $i$  or  $j$  is raising their rate. Therefore, the increase in  $i$ 's share at  $t_i^*$  must be the same as the change in  $j$ 's share if she were to raise *her* rate. But these “shares” are just each player’s tax rate. So these must be equal.

**Optimal public-goods provision with dual stationary bandits** In this simultaneous setup, we identify Nash equilibria in which player  $i$ 's choice of  $G_i$  is a best response to player  $j$ 's choice of  $G_j$  and vice versa. Solving FOC for Equation 12 for  $G_i$  and  $G_j$  yields

$$\begin{aligned} \max_{0 \leq G_i} V_i &= t_i \cdot \tau(t_i + t_j) \cdot Y(G_i + G_j) - c_i G_i \\ \frac{\partial V_i}{\partial G_i} &= t_i \tau(t_i + t_j) Y'(G_i + G_j) - c_i \leq 0 \end{aligned}$$

where the last condition hold with equality if  $G_i > 0$ . We can write player  $i$ 's best response function implicitly (i.e.  $i$  wants to set  $G_i^*$  such that):

$$G_i^*(G_j) : Y'(G_i^* + G_j) \leq \frac{c_i}{t_i^* \tau(t_i^* + t_j^*)} \quad (15)$$

and player  $j$  wants to set  $G_j^*$  such that

$$G_j^*(G_i) : Y'(G_i + G_j^*) \leq \frac{c_j}{t_j^* \tau(t_j^* + t_i^*)} \quad (16)$$

If we assume  $c_i = c_j = c$  and with no loss of generality that  $c = 1$  then (because  $t_i^* = t_j^*$ ) there is a unique total  $G$  that is optimal for both players, call it  $G_{2B}^*$ :

$$Y'(G_{2B}^*) = \frac{1}{t_i^* \tau(t_j^* + t_j^*)} \quad (17)$$

And there is a continuum of Nash equilibria characterized by  $G_i^* = G_{2B}^* - G_j^*$ . Obviously, the cross-partial of this relationship,  $\frac{\partial G_i^*}{\partial G_j^*}$  is negative.

In words, there is an optimal total amount of public-goods to be provided, and either player is happy to provide goods until total goods supplied reached that optimum. Obviously, each prefers that the other do it. But for any division of the optimal total amount between the two players, neither has an incentive to deviate. Whats more, if some force outside the

model pushed the result from one equilibrium to another, in which one player's public goods provision increased, it is obvious that the other player's optimal response would decrease.

In this way, the basic stationary bandit model, which explicitly accounts for economic growth produced by governance provision, can be said to predict crowding out.

## C.5 General formulation and alternative models

Here we abstract away from the examples of Cournot and Bertrand competition, or stationary bandits above. Instead of modeling competition with one model or the other, we could use a general form  $D(q_i, q_j)$  that encompasses all of these models (including Bertrand). Likewise, instead of modeling the externality as  $\rho(q_i, q_j)\pi_i$  we use a general form  $F(q_i, q_j)$ .

**Comparative statics** We now define a value function where we are agnostic about how duopolistic competition takes place:

$$V_i = D(q_i, q_j) + F(q_i, q_j)$$

Then we can define the first partial in  $q_i$  as:

$$G(q_i, q_j) \equiv \frac{\partial V_i}{\partial q_i} = \frac{\partial D(q_i, q_j)}{\partial q_i} + \frac{\partial F(q_i, q_j)}{\partial q_i}$$

which is a continuously differentiable function from  $\mathbb{R}^2 \rightarrow \mathbb{R}$ . As a technical note, we assume that there exists a point such that  $\frac{\partial V_i}{\partial q_i} = 0$ , and the functions  $D(\cdot)$  and  $F(\cdot)$  are concave so that the sum of both functions is also concave. This implies there is a unique solution. At the optimum, we know that  $G(q_i^*, q_j^*) = 0$ . We also assume that  $\frac{\partial G(q_i^*, q_j^*)}{\partial q_i} \neq 0$ .

Finally, we can use the implicit function theorem to obtain our main comparative static in the general formulation. Note this implies that there is a neighborhood of  $(q_i^*, q_j^*)$  such that when  $q_i$  is close enough to  $q_i^*$ , we have a unique  $q_j$  such that  $G(q_i, q_j) = 0$ . This makes  $q_j$  a continuous function of  $q_i$ . The comparative static is:

$$\frac{\partial q_j}{\partial q_i} = -\frac{\partial G(q_i, q_j)/\partial q_j}{\partial G(q_i, q_j)/\partial q_i} = -\frac{\frac{\partial^2 D(q_i, q_j)}{\partial q_i \partial q_j} + \frac{\partial^2 F(q_i, q_j)}{\partial q_i \partial q_j}}{\frac{\partial^2 D(q_i, q_j)}{\partial q_i \partial q_i} + \frac{\partial^2 F(q_i, q_j)}{\partial q_i \partial q_i}}$$

where  $\frac{\partial^2 D(q_i, q_j)}{\partial q_i \partial q_i} \leq 0$ ,  $\frac{\partial^2 F(q_i, q_j)}{\partial q_i \partial q_i} \leq 0$  would mean decreasing returns of production in loyalty and profit. With this assumption, a positive numerator is sufficient for a positive cross partial.

## D Supplemental analysis

Table D.1: Correlates of relative state governance

	Governance				
		Relative State	State	Combo	Vacuna incidence
	Sample Mean (SD)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)
	(1)	(2)	(3)	(4)	(5)
Income strata (2018)	2.430 (0.940)	0.026*** (0.010)	0.001 (0.008)	-0.026*** (0.008)	-0.010 (0.019)
Avg distance to drug corner and gang main location (100 meters)	3.452 (2.158)	0.011*** (0.003)	0.006*** (0.002)	-0.005** (0.002)	-0.011** (0.005)
Average distance to the casas de justica and police (100 meters)	11.467 (12.837)	-0.001 (0.001)	-0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)
Block average elevation (100 meters)	16.262 (1.426)	0.007 (0.006)	-0.007 (0.004)	-0.014*** (0.005)	-0.064*** (0.011)
Average slope of the neighborhood	89.933 (0.185)	-0.014 (0.027)	0.019 (0.023)	0.024 (0.025)	-0.073 (0.076)
Years of antiquity of the neighborhood	38.920 (15.186)	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)
Log of total population (2018)	8.836 (0.992)	-0.009 (0.006)	0.000 (0.005)	0.009 (0.005)	0.027** (0.013)
Percent of population no education (2018)	2.206 (1.098)	-0.017* (0.009)	-0.002 (0.007)	0.014* (0.008)	0.063*** (0.017)
Percent of population aged 0 to 14 (2018)	16.892 (4.951)	-0.010*** (0.003)	-0.004* (0.002)	0.007*** (0.002)	0.012** (0.006)
Percent of population who recently migrated (2018)	5.543 (3.441)	-0.001 (0.002)	0.001 (0.002)	0.002 (0.002)	0.008* (0.004)
Area of city center inside neighborhood (per sqr meter)	0.149 (0.280)	-0.068*** (0.026)	0.016 (0.022)	0.072*** (0.024)	0.007 (0.053)
Individual Demographics		✓	✓	✓	✓
Block Clustered SE		✓	✓	✓	✓
Dep. var. mean		0.069	0.408	0.333	0.331
<i>N</i>		4,399	4,544	4,403	1,830
# of Clusters		1,877	1,898	1,878	1,297

*Notes:* We run ordinary least squares (OLS) regressions of each measure on a range of available block- and neighborhood characteristics. Regressions are estimated at the individual level including individual demographics such as age, gender, time in the neighborhood and a dichotomous variables for whether the respondent belongs to business or household. Standard errors are clustered by block. Income strata is a city-block level administrative measure from the 2018 census running from 1 to 6. Years of antiquity of neighborhood is the mean of the approximate number of years each block has been historically present, based on historical cartography for 1948, 1970, 1985 and 1996.

Table D.2: Coefficient of closeness to state services for police stations and Casas de Justicia, separately

	Subsample Mean (SD) (1)	Police & Casa de Justicia Estimate (SE) [p-value] (2)	Casas de Justicia Estimate (SE) [p-value] (3)	Police station Estimate (SE) [p-value] (4)
Relative State Governance Index	0.01 (0.30)	-0.002 (0.006) [0.792]	0.010** (0.005) [0.047]	0.005 (0.005) [0.287]
State Governance Index	-0.03 (0.27)	-0.019*** (0.005) [0.000]	-0.006 (0.004) [0.170]	-0.009** (0.004) [0.031]
Combo Governance Index	-0.03 (0.28)	-0.014** (0.005) [0.010]	-0.015*** (0.005) [0.003]	-0.010** (0.004) [0.024]
Relative State Legitimacy Index	0.05 (0.33)	-0.009 (0.007) [0.227]	0.004 (0.007) [0.522]	-0.019*** (0.006) [0.001]
State Legitimacy Index	-0.02 (0.22)	-0.014*** (0.005) [0.003]	-0.006 (0.004) [0.152]	-0.004 (0.004) [0.307]
Combo Legitimacy Index	-0.05 (0.29)	-0.007 (0.006) [0.223]	-0.009* (0.005) [0.096]	0.010** (0.005) [0.041]
N for Governance outcomes		531	531	531
N for Legitimacy outcomes		404	404	404

*Notes:* This table calculates the effect of being 100 meters further from either the nearest comuna police station, or the nearest Casa de Justicia in the comuna. Note that other state agencies and service may be correlated with these two agencies, and so we cannot interpret these are causal effects of the individual headquarters with any certainty.

Table D.3: Robustness: Coefficient on closeness to state services for blocks along the inner comma borders of Medellín, by different specifications

	Subsample Mean (SD)	Main														
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)		
		Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)	Estimate (SE)
Relative State Governance Index	0.01 (0.30)	-0.002 (0.006)	-0.002 (0.005)	0.006 (0.009)	-0.001 (0.006)	-0.006 (0.006)	-0.005 (0.006)	0.003 (0.006)	0.001 (0.006)	0.001 (0.006)	-0.002 (0.008)	-0.001 (0.006)	-0.000 (0.006)	-0.000 (0.006)	-0.000 (0.005)	-0.000 (0.005)
State Governance Index	-0.03 (0.27)	-0.019*** (0.005)	-0.017*** (0.004)	-0.023*** (0.007)	-0.019*** (0.005)	-0.021*** (0.005)	-0.022*** (0.005)	-0.024*** (0.005)	-0.021*** (0.005)	-0.021*** (0.005)	-0.019*** (0.007)	-0.019*** (0.005)	-0.017*** (0.005)	-0.017*** (0.005)	-0.013*** (0.004)	-0.013*** (0.004)
Combo Governance Index	-0.03 (0.28)	-0.014** (0.005)	-0.012*** (0.005)	-0.022*** (0.007)	-0.015*** (0.005)	-0.012** (0.006)	-0.013** (0.005)	-0.023*** (0.006)	-0.012*** (0.005)	-0.019*** (0.005)	-0.014 (0.009)	-0.014*** (0.005)	-0.013** (0.005)	-0.013** (0.005)	-0.010** (0.004)	-0.010** (0.004)
Relative State Legitimacy Index	0.05 (0.33)	-0.009 (0.007)	-0.007 (0.006)	-0.019* (0.011)	-0.009 (0.007)	-0.012 (0.008)	-0.011 (0.007)	-0.018** (0.008)	-0.012* (0.007)	-0.012* (0.007)	-0.009 (0.010)	-0.007 (0.007)	-0.007 (0.007)	-0.007 (0.007)	-0.003 (0.006)	-0.003 (0.006)
State Legitimacy Index	-0.02 (0.22)	-0.014*** (0.005)	-0.013*** (0.004)	-0.001 (0.007)	-0.013*** (0.005)	-0.011** (0.005)	-0.010** (0.005)	-0.013*** (0.005)	-0.010** (0.005)	-0.010** (0.005)	-0.014* (0.008)	-0.012** (0.005)	-0.012** (0.005)	-0.012** (0.005)	-0.006 (0.004)	-0.006 (0.004)
Combo Legitimacy Index	-0.05 (0.29)	-0.007 (0.006)	-0.008 (0.005)	0.013 (0.009)	-0.007 (0.006)	-0.005 (0.006)	-0.004 (0.006)	0.004 (0.007)	0.001 (0.006)	0.001 (0.006)	-0.007 (0.007)	-0.007 (0.006)	-0.006 (0.006)	-0.006 (0.006)	-0.005 (0.005)	-0.005 (0.005)
N for Governance outcomes		531	736	303	531	528	531	479	531	531	531	533	534	534	544	544
N for Legitimacy outcomes		404	535	227	404	402	404	368	404	404	404	407	407	407	415	415

Notes: This table compares alternative specifications, changing one feature of the model at a time (not cumulatively). Column (2) reports our main results, as expressed in Table 4. Columns (3) and (4) use 300-meter and 100-meter bandwidths around the comma borders rather than the 200-meter default. Columns (5) and (7) drop covariates corresponding to demographics and distance to other state agencies and business centers (these covariates are displayed in Table 3). Column (6) uses a measure of distance to other state agencies and business center in the block's comma, rather than the nearest. Column (8) uses an alternate distance to the border running variable, calculated to a fixed point in the nearest border for each pair (rather than coordinates). Column (9) adjusts the treatment variable, distance to police and Casa de Justicia, by elevation. Column (10) computes standard error clustered by border. Columns (11), (12) and (13) use alternate matching algorithms for pairing blocks, adjusting matching distances by elevation and relaxing various matching criteria.

Table D.4: Coefficient of closeness to state services for blocks along the inner comuna borders of Medellín on governance index components

	Subsample Mean (SD)	Relative state Estimate (SE)	State Estimate (SE)	Combo Estimate (SE)
	(1)	(2)	(3)	(4)
Relative State Governance Index	0.08 (0.24)	-0.002 (0.006)	-0.019*** (0.005)	-0.014** (0.005)
HH: Someone refuses to pay a big debt	-0.15 (0.33)	0.004 (0.012)	-0.007 (0.009)	-0.007 (0.009)
HH: There is domestic violence	0.16 (0.32)	0.003 (0.011)	-0.017* (0.009)	-0.015* (0.009)
HH: Two drunks fight on the street	0.15 (0.34)	-0.027** (0.012)	-0.028*** (0.009)	-0.007 (0.009)
HH: Kids fight on the street	-0.03 (0.30)	0.017* (0.010)	-0.014 (0.009)	-0.022*** (0.008)
HH: Home improvements affect neighbors	0.17 (0.32)	-0.014 (0.010)	-0.030*** (0.010)	-0.012 (0.008)
HH: Someone is making noise	0.23 (0.31)	-0.048*** (0.010)	-0.048*** (0.008)	-0.002 (0.006)
HH: People smoking marijuana near children	0.07 (0.31)	-0.008 (0.010)	-0.033*** (0.008)	-0.017** (0.008)
HH: Someone is mugged on the street	-0.03 (0.32)	-0.007 (0.010)	-0.029*** (0.008)	-0.014* (0.008)
HH: A car or motorbike is stolen	0.06 (0.36)	0.002 (0.012)	-0.025*** (0.009)	-0.024*** (0.008)
HH: Someone is threatening someone else	0.02 (0.35)	-0.011 (0.011)	-0.017* (0.009)	-0.011 (0.008)
HH: It is necessary to prevent a theft	-0.00 (0.34)	-0.010 (0.010)	-0.015* (0.008)	-0.009 (0.009)
HH: You have to react to a robbery	0.01 (0.36)	-0.002 (0.011)	-0.017* (0.009)	-0.012 (0.008)
Biz: Someone does not want to pay a debt	-0.06 (0.38)	-0.037* (0.020)	-0.030** (0.015)	-0.030** (0.014)
Biz: Businesses in this sector are robbed	0.07 (0.47)	0.016 (0.023)	-0.013 (0.016)	-0.027 (0.018)
Biz: Someone disturbs a business	0.12 (0.45)	-0.017 (0.020)	-0.024* (0.013)	-0.010 (0.015)
Biz: It is necessary to prevent a theft	0.07 (0.47)	-0.009 (0.020)	-0.026** (0.013)	-0.022 (0.015)
Biz: You have to react to a robbery	0.11 (0.48)	-0.014 (0.024)	-0.009 (0.014)	-0.014 (0.017)
Number of observations		525	531	526

*Notes:* This table calculates the effect of being 100 meters further from the state on the 17 components of our governance indexes. HH indicates questions asked to households, and Biz represents questions asked to businesses.

Table D.5: Test of systematic measurement error: Coefficient of closeness to state services for blocks along the inner comuna borders of Medellín on measurement error proxies

	$\Delta Y$ sample mean	Correlation with $\Delta StateDist$	Correlation with $\Delta StateDist$ (as sample sd)
	Subsample Mean (SD)	Estimate (SE)	Estimate (SE)
	(1)	(2)	(3)
Vacuna payment rate difference (RR-DR)	-0.05 (0.82)	0.042 (1.085)	-0.021 (0.032)
Difference in proportion of questions answered for governance index (State - Com)	0.03 (0.13)	-0.037 (0.188)	-0.005 (0.003)
Proportion of questions answered for mayor governance index	0.89 (0.14)	0.008 (0.197)	0.009** (0.004)
Proportion of questions answered for combo governance index	0.86 (0.18)	0.045 (0.265)	0.015*** (0.005)
Difference in proportion of questions answered for legitimacy index (State - Com)	0.09 (0.18)	-0.012 (0.215)	-0.002 (0.005)
Proportion of questions answered for state legitimacy index	0.97 (0.08)	0.012 (0.101)	0.001 (0.002)
Proportion of questions answered for combo legitimacy index	0.87 (0.20)	0.023 (0.243)	0.003 (0.006)
Number of observations		537	536
Minimum N		235	234

*Notes:* This table takes the proxies for measurement error discussed in Appendix A and calculates the effect of being 100 meters more distant from the state on these proxies, using the same estimation for our main treatment effects. The vacuna rate difference computes the difference between randomized response and direct response to the question of whether the household pays vacunas. The other measures capture non-response to sensitive items (the proportion of questions answered). We look at the proportion of questions answered for each index, and whether this is different for the state versus the combo. More questions answered for the state could indicate a reluctance to talk about or disclose combo activities.

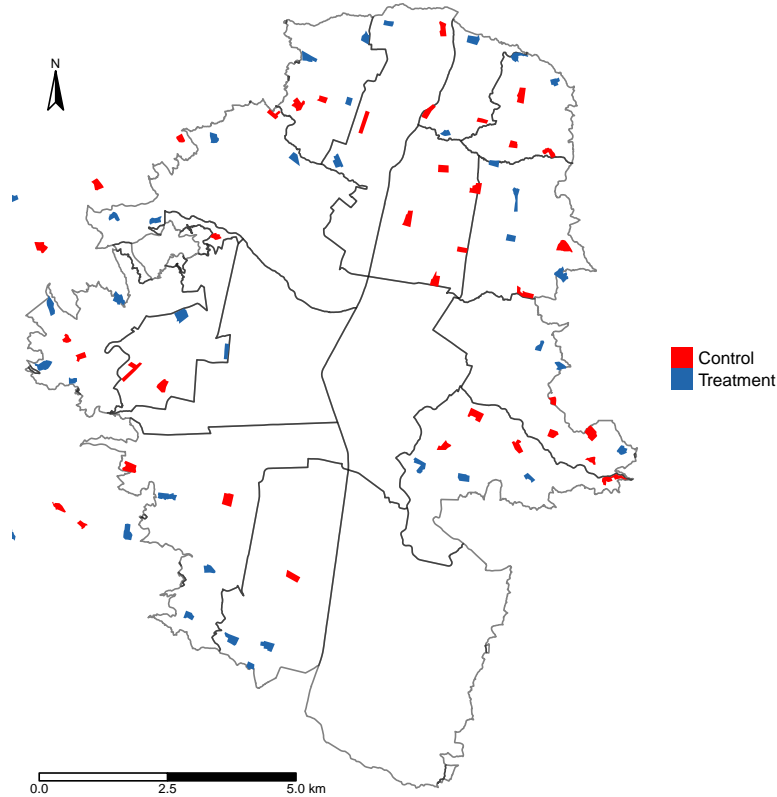


Table D.6: Coefficient on closeness to state services for blocks along the inner comuna borders of Medellín, other outcomes

		$\Delta Y$ sample mean	Correlation with $\Delta StateDist$	Correlation with $\Delta StateDist$ (as sample sd)
	Subsample Mean (SD)	Estimate (SE)	Estimate (SE)	Estimate (SE)
	(1)	(2)	(3)	(4)
<i>Panel A: Efficacy outcomes</i>				
How easy is it to contact the combo	0.57 (0.25)	0.004 (0.268)	-0.013** (0.005)	-0.072
How fast is the...	0.52 (0.29)	-0.013 (0.302)	-0.024*** (0.007)	-0.114
How easy is it to contact the state	0.45 (0.18)	-0.009 (0.354)	-0.023*** (0.008)	-0.093
How fast is the...	0.38 (0.21)	-0.073 (0.422)	-0.025** (0.010)	-0.083
<i>Panel B: Payments to combos and state</i>				
Payment rate of security fee (vacuna)	0.13 (0.28)	-0.011 (0.352)	-0.007 (0.008)	-0.025
Perception about neighbors of this sector pay security fee (vacuna)	0.32 (0.35)	0.030 (0.469)	-0.017* (0.010)	-0.045
Approves that the combo charges a security fee (vacuna)	0.06 (0.15)	-0.003 (0.221)	-0.004 (0.004)	-0.024
Considers security fees (vacuna) by the combo very high	0.28 (0.37)	0.019 (0.536)	0.011 (0.013)	0.030
Percentage of bussines paying taxes	0.50 (0.49)	-0.022 (0.700)	-0.032* (0.019)	-0.066
Approves that the Mayor's office taxes for services	0.59 (0.30)	0.013 (0.446)	-0.022*** (0.008)	-0.068
Considers that the Mayor's office charges very high	0.61 (0.31)	-0.042 (0.491)	0.009 (0.010)	0.028
Number of observations		537	536	536
Minimum N		307	306	306

*Notes:* This table calculates the effect of being 100 meters further from the state on a number of additional outcomes. We preregistered relative state governance and legitimacy as primary and secondary outcomes for the experiment. We collected a number of other measures of state and combo performance, however, and report them here.

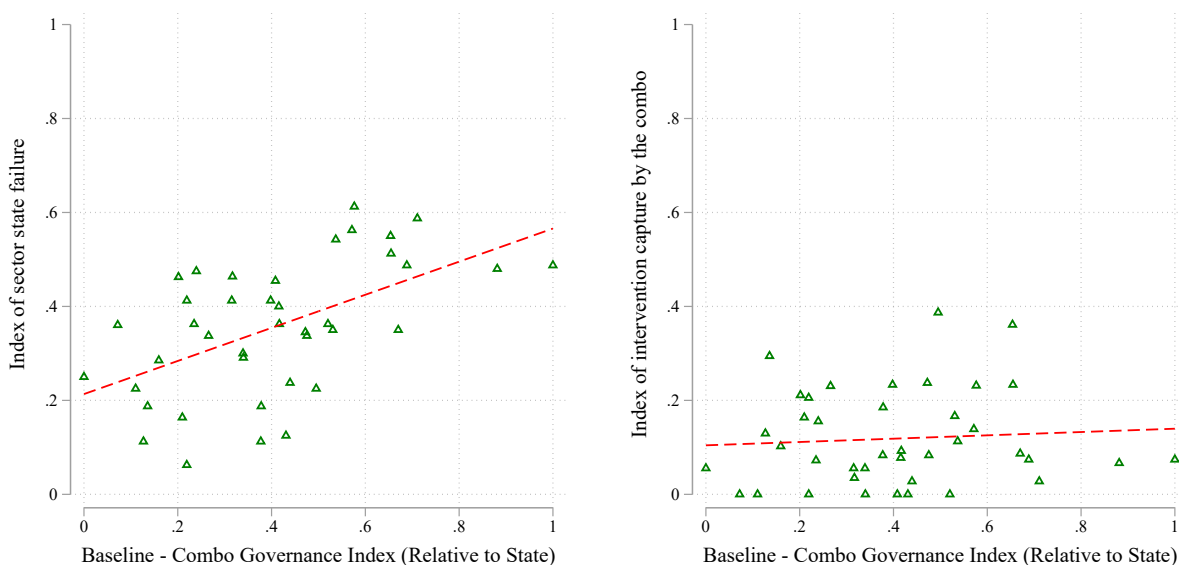
Figure D.1: Treatment and control sectors



*Notes:* The main constraint on the number of treated sectors was the city's immediate implementation capacity. To select the 80, first the city omitted non-residential downtown areas, where crime is organized differently, there are few territorial combos, and criminal governance is limited. Second, city staff from each comuna were asked to identify small, informal neighborhoods where a combo: (i) provided security and taxed residents for security; (ii) was a major resource for the community to resolve disputes; (iii) regulated illegal and legal markets; and (iv) provided other government services (e.g., garbage collection). Our research team validated these sectors with city social workers from other branches of the government (e.g., the Victims Unit). Many hundreds of sectors have a serious combo presence, and the city narrowed these to the 80 where they believed criminal governance was greatest. From a list of 100 such sectors provided by the city, we discarded those less than 250 meters away from one another to minimize the risk of spillovers, for a final set of 80 sectors.

Figure D.2: How treatment experiences varied by initial levels of gang rule  
(treated sectors only)

(a) *Failed promises of the wider state apparatus* (b) *Instances of combo interference and capture*



*Notes:* In February 2020, after completion of the experiment, we surveyed every liaison from the experiment about their experiences in their sector. Based on their responses, we created two indexes of program experiences. The first, in Panel (a), is the frequency of various failures of the liaison or the wider state apparatus to deliver on promises. This include a scale of the perceived frequency of failures from the liaison, police, and mayor’s office bureaucrats and binary variables for whether specific local state agency failed. Values closer to 1 mean higher state failure. The second, in Panel (b), captures the degree with which the combo interfered with liaison activities. Our intervention capture measure aggregates the following sub components: a scale for the frequency and difficulties of interaction with local gangs, a set of binary variables on whether local actors, including the gang, took credit for the intervention activities; and, a set of binary variables for activities by which the gang curtailed or helped the liaison on the interventions. Values closer to 1 represent higher involvement from locals gang on intervention activities.

Table D.7: Field experiment baseline summary statistics and balance

Covariate	Means		Regression Difference		
	Control	Treated	Coeff	p-value	SE
Neighborhood size (floorspace area in sq meters)	28,252.83	27,159.17	-1,093.65	0.69	2,729.92
Additive index of combo presence and governance	0.01	-0.01	-0.02	0.88	0.13
Index of relative visibility of the combo and the state	0.01	-0.01	-0.02	0.88	0.13
How frequently do you see combo members when you visit the sector? (0-3)	2.38	2.35	-0.02	0.86	0.14
How infrequently do you see mayor's workers when you visit the sector? (0-3)	0.87	0.90	0.03	0.77	0.10
How infrequently do you see police when you visit the sector? (0-3)	0.97	0.90	-0.06	0.49	0.09
How many of the youth support the combo? (0-3)	1.74	1.47	-0.27	0.03**	0.12
Ease to work in sector for liasons	1.50	1.57	0.08	0.63	0.15
Ease to work in sector for community leaders	1.05	1.30	0.25	0.06*	0.13
Index of relative governance of the combo and the state	0.01	-0.01	-0.02	0.90	0.13
Who solves disputes between neighbors in the sectors? (0-2)	0.95	1.00	0.05	0.55	0.08
Who solves intra-family violence in the sector? (0-2)	0.73	0.78	0.05	0.55	0.09
Who solves thefts in the sector? (0-2)	1.04	1.00	-0.04	0.64	0.09
Who gives permission to use sports fields in the sector? (0-2)	0.32	0.37	0.05	0.46	0.07
Who solves disputes over permits in the sector? (0-2)	0.88	0.91	0.04	0.68	0.09
Who solves infrastructure problems in the sector? (0-2)	0.12	0.18	0.05	0.29	0.05
Who solves problems of hunger and poverty in the sector? (0-2)	0.25	0.26	0.01	0.89	0.08
Who solves problems of people doing drugs in inappropriate places in the sector? (0-2)	1.17	1.07	-0.10	0.31	0.10
Who solves problems of sexual abuse in the sector? (0-2)	0.86	0.73	-0.13	0.11	0.08
Who solves problems of disappearing people in the sector? (0-2)	0.42	0.45	0.03	0.75	0.09
Who solves murders in the sectors? (0-2)	0.45	0.42	-0.03	0.78	0.09
Who gives permission for political events in the sector (0-2)?	0.45	0.48	0.03	0.73	0.09
Who gives permission for who becomes a JAC in the sector? (0-2)	0.48	0.40	-0.08	0.28	0.07
Who gives permission for loud parties in the sector? (0-2)	0.94	0.88	-0.05	0.53	0.08
Standardized index of perceived insecurity and drugs	0.07	-0.07	-0.14	0.23	0.11
How insecure is the sector in general? (0-3)	0.61	0.61	-0.00	1.00	0.08
How insecure is walking at night in the sector? (0-3)	1.33	1.42	0.09	0.35	0.09
How insecure is talking on your cell at night in the sector? (0-3)	1.12	1.00	-0.12	0.32	0.12
How insecure is it to walk at night as a man? (0-3)	1.53	1.36	-0.17	0.22	0.14
How insecure is it to walk at night as a woman? (0-3)	1.62	1.70	0.08	0.42	0.10
How many of the youth do drugs? (0-3)	2.09	1.88	-0.21	0.01**	0.08
How would you characterize drugs in the 'vice' areas of the sector?	2.40	2.37	-0.02	0.84	0.11
Index of crime	0.10	-0.10	-0.19	0.02**	0.08
Count of homicides per median sector area in 2014 to 2017	1.51	1.26	-0.25	0.42	0.30
Count of gang-related homicides per median sector area 2015 to 2017	0.83	0.57	-0.25	0.22	0.20
Count of thefts per median sector area in 2014 to 2017	18.19	14.82	-3.36	0.25	2.89
Count of calls related to violence per median sector area 2016 to 2017	38.82	47.26	8.44	0.08*	4.62
Count of calls related to drugs per median sector area 2016 to 2017	8.28	6.24	-2.04	0.19	1.53
Index of distance from public goods and services	-0.14	0.14	0.29	0.19	0.21
Distance to nearest downtown (meters)	307.87	339.74	31.87	0.64	67.99
Distance to nearest social services (meters)	273.53	330.70	57.17	0.38	63.82
Distance to nearest public transit (meters)	175.53	234.99	59.46	0.32	58.69
Distance to nearest cultural center (meters)	91.99	107.95	15.96	0.59	29.09
Distance to nearest educational facility (meters)	43.60	77.05	33.45	0.09*	19.36
Distance to nearest justice or police center (meters)	553.49	549.62	-3.88	0.97	100.84
Distance to nearest religious center (meters)	162.90	169.58	6.69	0.88	42.43

Notes: This table reports treatment and control group means and uses an OLS regression to calculate treatment-control differences for neighborhood size and four main indexes, along with all index components. The four indexes are used as control variables in estimation of treatment effects. All estimates included fixed effects for the 40 treatment-control pairs.

Table D.8: Estimating treatment spillovers onto non-experimental units within a 250 meter radius

	Spillover Rings			
	Treatment Estimate (1)	P-value (2)	0m-250m Spillover Estimate (3)	P-value (4)
Relative State Governance Index	-0.0432	0.0170	-0.0677	0.1580
State Governance Index	-0.0227	0.0850	-0.0516	0.0650
Combo Governance Index	0.0168	0.2980	0.0113	0.7130
Relative State Legitimacy Index	-0.0026	0.8820	-0.0290	0.1780
State Legitimacy Index	0.0072	0.5150	-0.0021	0.5040
Combo Legitimacy Index	0.0156	0.3820	0.0176	0.7040

*Notes:* Our sample includes 6977 survey respondents, including 2,379 in the experimental sectors and 4,598 on blocks from the representative city survey. The table reports treatment estimates along with an indicator for blocks in the experimental sectors and an indicator for blocks within 250 meters of a treated sector. We calculate p-values using randomization inferences, correcting estimates for fuzzy clustering. To address systematic exposure to spillovers due to the geographic distribution, we weight each observation by the inverse probability of each treatment category: treated, <250 meters, and >250 meters. See Blattman et al. (2021) for an explanation of this estimation strategy.

Table D.9: Robustness of experimental results to changes in the specification

	Subsample Mean (SD)	Main specification Estimate (SE) [p-value]	Dropping individual controls Estimate (SE) [p-value]	Add sector and block controls Estimate (SE) [p-value]	Main collapsed Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)
Relative State Governance Index	0.07	-0.028* (0.015) [0.064]	-0.029* (0.016) [0.062]	-0.014 (0.013) [0.296]	-0.030 (0.022) [0.181]
State Governance Index	0.41	-0.017* (0.010) [0.089]	-0.017* (0.010) [0.096]	-0.013 (0.008) [0.112]	-0.017 (0.014) [0.243]
Combo Governance Index	0.35	0.009 (0.011) [0.414]	0.010 (0.012) [0.375]	-0.002 (0.012) [0.842]	0.010 (0.016) [0.535]
Relative State Legitimacy Index	0.13	0.012 (0.019) [0.550]	0.010 (0.020) [0.629]	0.027 (0.026) [0.319]	0.009 (0.028) [0.762]
Combo Legitimacy Index	0.44	0.002 (0.015) [0.874]	0.005 (0.015) [0.768]	-0.009 (0.019) [0.642]	0.005 (0.022) [0.803]
State Legitimacy Index	0.57	0.012* (0.007) [0.094]	0.013* (0.008) [0.097]	0.016* (0.010) [0.096]	0.013 (0.011) [0.241]
N for Governance outcomes		2,362	2,362	2,362	80
N for Legitimacy outcomes		1,845	1,845	1,845	80

*Notes:* Column (2) shows our main specification as reported in Table 6. Each column makes one change relative to this main specification, non-cumulatively. Column (3) drops respondents demographics which include age, gender, time in the neighborhood, and whether respondent represents a business or a household. Column (4) adds block and sector level covariates. Column (5) collapses our individual observations into an 80-sector dataset.

Table D.10: Intervention effects on 17 sub index component of the governance index

	Subsample Mean (SD)	Relative state Estimate (SE)	State Estimate (SE)	Combo Estimate (SE)
	(1)	(2)	(3)	(4)
Relative State Governance Index	0.07	-0.028* (0.015)	-0.017* (0.010)	0.009 (0.011)
HH: Someone refuses to pay a big debt	-0.20	-0.025 (0.016)	-0.005 (0.014)	0.014 (0.017)
HH: There is domestic violence	0.15	-0.015 (0.021)	-0.022 (0.017)	-0.007 (0.019)
HH: Two drunks fight on the street	0.13	-0.017 (0.026)	-0.001 (0.015)	0.010 (0.018)
HH: Kids fight on the street	-0.03	-0.020 (0.018)	-0.010 (0.012)	0.002 (0.016)
HH: Home improvements affect neighbors	0.14	-0.007 (0.018)	-0.016 (0.014)	-0.010 (0.014)
HH: Someone is making noise	0.26	-0.031* (0.018)	-0.009 (0.016)	0.022** (0.010)
HH: People smoking marijuana near children	0.03	0.001 (0.021)	0.004 (0.014)	0.012 (0.016)
HH: Someone is mugged on the street	-0.05	0.013 (0.023)	0.000 (0.016)	-0.005 (0.019)
HH: A car or motorbike is stolen	-0.01	0.011 (0.022)	0.011 (0.016)	0.002 (0.017)
HH: Someone is threatening someone else	-0.01	-0.020 (0.020)	-0.003 (0.015)	0.012 (0.017)
HH: It is necessary to prevent a theft	-0.04	-0.014 (0.023)	-0.028** (0.013)	-0.010 (0.017)
HH: You have to react to a robbery	-0.02	-0.019 (0.023)	-0.018 (0.017)	-0.003 (0.017)
Biz: Someone does not want to pay a debt	-0.05	-0.009 (0.026)	0.048** (0.023)	0.032 (0.029)
Biz: Businesses in this sector are robbed	0.07	-0.070* (0.041)	-0.033 (0.034)	0.008 (0.032)
Biz: Someone disturbs a business	0.16	-0.073* (0.044)	-0.024 (0.034)	0.035 (0.032)
Biz: It is necessary to prevent a theft	0.08	-0.085* (0.044)	-0.086*** (0.032)	-0.004 (0.035)
Biz: You have to react to a robbery	0.12	-0.088** (0.037)	-0.061** (0.030)	0.026 (0.034)
Number of observations		2,314	2,362	2,316

*Notes:* This table calculates the treatment effects on the 17 components of our governance indexes. HH indicates questions asked to households, and Biz represents questions asked to businesses.

Table D.11: Treatment-control differences in indicators of Measurement error

	Percentage					
	Control Mean	ATE	SE	P-value	ATE as % of SD	N
	(1)	(2)	(3)	(4)	(5)	(6)
<hr/>						
Extortion rates						
Sector vacuna payment rate difference (RR-DR)	0.041	-0.051	0.053	0.339	-0.217	80
<hr/>						
Proportion of questions answered						
Proportion of questions answered for relative state governance index	0.033	-0.003	0.008	0.690	-0.096	80
Proportion of questions answered for mayor governance index	0.891	-0.005	0.011	0.632	-0.113	80
Proportion of questions answered for combo governance index	0.858	-0.002	0.013	0.876	-0.036	80
Proportion of questions answered for relative state legitimacy index	0.082	-0.001	0.011	0.895	-0.027	80
Proportion of questions answered for state legitimacy index	0.971	-0.013	0.006	0.056	-0.471	80
Proportion of questions answered for combo legitimacy index	0.889	-0.011	0.014	0.420	-0.168	80

*Notes:* This table takes the proxies for measurement error discussed in Appendix A and calculates the correlation with our randomized treatment on these proxies, using the same estimation for our main treatment effects. The vacuna rate difference computes the difference between randomized response and direct response to the question of whether the household pays vacunas. The other measures capture non-response to sensitive items (the proportion of questions answered). We look at the proportion of questions answered for each index, and whether this is different for the state versus the combo. More questions answered for the state could indicate a reluctance to talk about or disclose combo activities.