Gang rule: Understanding and countering criminal governance*

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June 11, 2024

Abstract

Criminal groups govern millions worldwide. Even in strong states, gangs resolve disputes and provide security. Why do these duopolies of coercion emerge? Often, gangs fill vacuums of official power, suggesting that increasing state presence should crowd out criminal governance. We show, however, that state and gang rule are sometimes complements. In particular, gangs could minimize seizures and arrests by keeping neighborhoods orderly and loyal. If true, increasing state presence could increase incentives for gang rule. In Medellín, Colombia, criminal leaders told us they rule to protect drug rents from police. We test gang responses to state presence using a geographic discontinuity. Internal border changes in 1987 assigned blocks to be closer or further from state security for three decades. Gangs exogenously closer to state presence developed more governance over time. They primarily did so in neighborhoods with the greatest potential drug rents. This suggests new strategies for countering criminal governance.

JEL codes: E26, H11, K42, O17, C21
Keywords: Organized crime, gangs, state building, duopoly, public services, criminal governance, qualitative methods, quasi-experiment, Colombia

*For comments and feedback we thank Ana Arjona, Oriana Bandiera, Abhijit Banerjee, Eli Berman, Jesse Bruhn, Esther Duflo, Leopoldo Fergusson, Claudio Ferraz, Danilo Friere, Donald Green, Soeren Henn, Macartan Humphreys, Stephen Machin, Martin McGuire, Daniel Mejía, Ted Miguel, Mushfiq Mobarak, Eduardo Montero, Daniel Ortega, Gerard Padró i Miquel, Paolo Pinotti, Bob Powell, Gérard Roland, Pieter Serneels, Jake Shapiro, Stergios Skaperdas, Tara Lyn Slough, Carlos Schmidt-Padilla, Rodrigo Soares, Maria Micaela Sviatschi, Lucia Tiscornia, Rocío Titiunik, Francesco Trebbi, Juan Vargas, Maarten Voors, Jeremy Weinstein, Austin Wright, Arthur Zeyang Yu, participants at several seminars and conferences, and Raul Sánchez de la Sierra who was instrumental in conceptualizing the model. Innovations for Poverty Action coordinated all research activities. For research assistance we thank Mexico Vergara-Bahena, Bruno Aravena Maguida, David Cerero, Peter Deffebach, Sebastián Hernández, Sofía Jaramillo, Juan F. Martínez, Juan Pablo Mesa-Mejía, Angie Mondragón, Helena Montoya, José Miguel Pascual, Andrés Preciado, María-Aránzazu Rodríguez-Uribe, Zachary Tausanovitch, Nelson Matta-Colorado, and Martín Vanegas-Arias. We thank the Secretariat of Security of Medellín for their cooperation, especially the former Secretary of Security Andrés Tobón, as well as Lina Calle, Ana María Corpas and Andrés Machuca. For financial support, we thank the Centro de Estudios sobre Seguridad y Drogas (CESED) of Universidad de los Andes; the Peace and Recovery Program (P&R) at Innovations for Poverty Action (IPA); the PROANTIOQUIA foundation; The National Science Foundation (NSF); the UK Foreign, Commonwealth & Development Office trough the Crime and Violence Initiative at J-PAL; and the Economic Development and Institutions Programme (EDI) funded with UK aid from the UK Government, working in partnership with Oxford Policy Management Limited, University of Namur, Paris School of Economics and Aide á la Décision Économique.

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1 Introduction

For tens of millions of people worldwide, social order, property-rights, and dispute resolution are provided by criminal organizations in addition to the state. Such criminal governance is common in Latin American cities, but gangs also rule civilians in Italy, the United Kingdom, India, South Africa, and the American prison system (Arias, 2006; Lessing et al., 2019; Lessing, 2020; Melnikov et al., 2021). In all these places, the government does not have a monopoly on the legitimate use of force. Rather, the state typically dominates wealthy areas, but residents of low- and middle-income neighborhoods often live under a duopoly of coercion (Skaperdas and Syropoulos, 1996; Uribe et al., 2022).

The classic explanation for criminal governance is that criminal groups fill a vacuum left by weak states. By failing to meet people's demand for security, contract enforcement, and dispute resolution, weak states create a market for governance that gangs and warlords fill (Tilly, 1985; Gambetta, 1996; Skaperdas, 2001; Skarbek, 2011; Sánchez De La Sierra, 2020).

This paper identifies a second force that drives criminal groups to rule—one that can lead state and criminal rule to be strategic complements rather than substitutes. Governing is a way to protect other illegal revenues. As a result, in places where illicit rents are high, raising state presence could have the perverse effect of increasing criminal rule.

We illustrate these countervailing forces in Medellín, Colombia's second-largest city and commercial heartland. The city is prosperous, collects extensive tax revenues, and provides considerable public goods and social services to its citizens. Nonetheless, virtually every low- and middle-income neighborhood in the city is also occupied by one of more than 350 small gangs called *combos*, and most combos engage in at least some governance activities. These include prohibiting and punishing property crime, settling disputes between neighbors, enforcing rules, and (in exchange) taxing locals.

This is a clandestine system, and so we began our study with qualitative interviews with criminal organizations. We interviewed leaders and and members in 79 criminal groups, as well as hundreds of community members, leaders, and police.

We then collected systematic data on criminal governance. In 2019 we ran a representative survey of residents and businesses on the services provided by both combos and the state, as well as the legitimacy of each actor. The state is the predominant provider of protection in most low- and middle-income neighborhoods, but in a third of these areas residents reported that the combo is the leading provider of security and dispute resolution, and is often nearly as legitimate as the state.

While almost all combos collect protection fees as revenue, several criminal leaders told us that they govern in part because it protects their other business lines, especially drug

sales. They gave two related reasons. First, they said that providing neighborhood order reduces the need for routine police patrols and special agents to enter. Second, they believed that well-governed residents would be loyal—less likely to inform on gang members and more willing to defend or hide gangs when police did enter.

We illustrate these incentives in a model of imperfect competition that allows for benefits from governance beyond the revenues it generates. Standard duopolistic competition implies that gang and state governance services are substitutes. This captures the classic view that there is a demand for governance and protection, and that criminals enter this market when the state fails to meet that demand. We then introduce the idea that governing simultaneously protects the gangs' other illicit business lines from losses to state repression. This generates strategic complementarities between gang and state rule.

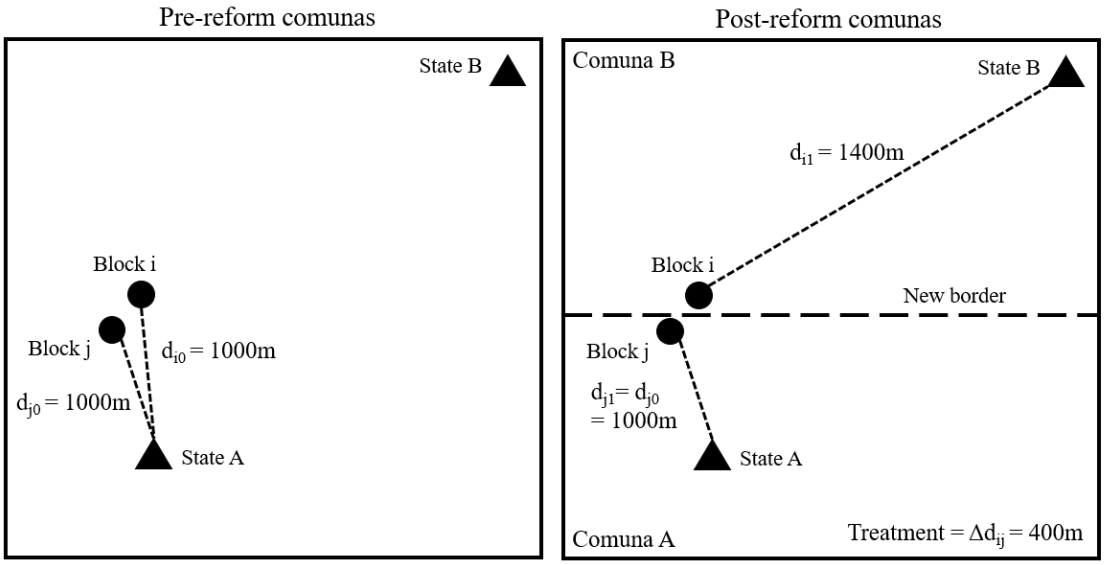
How gangs react to changes in state presence depends on which of these two forces dominates. When illicit profits are high, gangs have incentives to govern on blocks closer to the state. In areas where the state is already dominant (such as rich neighborhoods) or in areas where drug profits are small, combo rule have fewer incentives to establish rule.

To test this, we study an exogenous, decades-long change in the proximity of state protection in Medellín. We take advantage of a natural experiment: a city bill that, in the late 1980s, reorganized Medellín into 16 areas called *comunas*. The bill also mandated that policing, dispute resolution, and family services be organized by comuna. As a result, street blocks on either side of the new borders were no longer served by the same headquarters. Instead, blocks on one side were assigned to headquarters further away. During the following decades, the city largely expanded security spending and personnel—almost tripling police officers per capita and increasing spending by almost twenty-fold. As the city expanded its police and civilian security forces, some blocks were closer to this expansion than others, plausibly introducing exogenous variation in local state presence across block pairs that otherwise look very similar.

This strategy is related to a growing class of geographic regression discontinuity designs (Keele and Titiunik, 2015). In this case, however, the border assigns blocks a continuous distance shock rather than a binary treatment.[1] Figure 1 illustrates. Initially, a pair of nearby blocks i and j received their security and dispute resolution services from the same location 1,000 meters away, as seen on the left. Once the new border was introduced, as seen on the right, block i was assigned to headquarters 400 meters further away. The size and direction of this distance shock varies by block-pairs along each border. We estimate how variation in this distance shock shapes block-pair differences over time. The key identifying

[1] A more conventional example, one that inspired this paper, is that of Henn (2022), who examined how international borders in Africa affect the proximity of the capital to traditional chiefdoms.

Figure 1: Stylized illustration of the natural experiment



Notes: On the left is a stylized representation of a single pre-1987 comuna, with blocks i and j accessing the state at its closest location (State A). On the right is a representation of the split comuna after 1987, with block i being assigned to access the state at a further location (State B) and block j still accessing the state nearby (State A). See section 5 for details.

assumption is that no other characteristic changed discontinuously along the new borders except this state protection distance shock—an assumption borne out by tests of balance.

We use the 2019 survey to assess the long-run impacts of state proximity on the state and combo governance. To standardize the interpretation of the results, we orient the distance shock to imply moving closer to a local state headquarters, and hence experiencing more state growth. We first see that moving closer to the state moderately improved perceptions of state governance. On blocks that moved 400 meters closer to their police and municipal dispute resolution agencies (the median change), residents reported roughly 11% greater state responsiveness to disputes and disorder and 12% more accessible in terms of ease of contact and speed of response. Only the latter impact is statistically significant, however.

Turning to gangs, we see no evidence that state proximity crowded out criminal rule. On the contrary, the average combo responded to state proximity by governing more. Being 400 meters closer to state headquarters increased reports of combo rule by 23% and accessibility by 17%.

Importantly, some of the evidence also suggests that gangs were most likely to compete with the state in the neighborhoods close to profitable drug markets. In less profitable neighborhoods, they do not appear to respond to state proximity at all.

Despite what criminal leaders told us, however, we see no evidence that combo governance fostered loyalty. Rather, in profitable drug neighborhoods, the gang's legitimacy suffered

despite high levels of criminal rule. Residents appear to dislike drug sales and the disorder it brings. As state proximity and combo governance grow, the legitimacy of both actors falls in high-drug areas but rises in low-drug ones. It could be that combo leaders overestimate the degree to which governing fosters loyalty. Alternatively, gang rule could instead mitigate the loyalty penalty that comes from selling drugs. In either case, providing neighborhood order could still reduce the presence of routine police patrols and motives for special agents to enter—the other rationale that combo leaders gave for criminal governance.

These patterns extend beyond Medellín. Uribe et al. (2022) survey 18 Latin American countries and find that gang rule is positively correlated with state presence. Ethnographies in Rio de Janeiro likewise argue that drug-selling gangs respond to state presence by governing more, often for the same reasons claimed by Medellín gangs (Arias and Barnes, 2017; Barnes, 2023). We even see criminal rule in rural enclaves where there are valuable territorial rents to exploit. In eastern Congo, for instance, Sánchez De La Sierra (2020) shows how roving warlords turned themselves into stationary bandits and delivered security and justice when mining rents were large and could be easily taxed.

One interpretation is that a combination of state pressure and local rents discipline armed groups, giving them incentives to maintain local order and foster loyalty. This could help explain why some gangs are so extortionate and repressive. For example, Melnikov et al. (2021) note that Salvadoran gangs have provided little to no order or dispute resolution, and have made no efforts to earn civilian loyalty. The authors also note that El Salvador has virtually no retail drug trade. Without this incentive to foster order, Salvadoran gangs could afford to be repressive and illegitimate.

We also see parallels to a literature arguing that rebel groups offer justice and welfare services to capture civilian "hearts and minds" (Berman et al., 2011, 2013; Crost et al., 2016; Arjona, 2016). One difference is that, in these rural contexts, the evidence suggests that military action and state services raised state legitimacy and crowded out insurgents (Berman and Matanock, 2015). Why would village insurgents be crowded out when city gangs are not? One possibility is that rural insurgents are not as reliant on local rents.

We see similar patterns among urban versus rural criminal groups. In Rio de Janeiro, gangs violently resist state incursions in the neighborhoods they rule and sell drugs (Magaloni et al., 2020). In Mexico, however, drug traffickers can often choose among many potential routes for smuggling their product to the United States. Thus, crackdowns in one city simply displace traffickers and violence to less aggressive municipalities nearby (Dell, 2015).

In terms of policy, these results suggest that governments face some difficult trade-offs. For instance, in cities with established drug markets and gangs, governments may struggle to displace criminal groups from governing, and attacking their illicit rents may reduce their

incentives to keep the peace or treat civilians well. Our conclusion explores what policy approaches might work and suggests directions for future research and policy experimentation.

2 Data

2.1 Qualitative interviews

To better understand the actions, organizations, and motives of Medellín's criminal organizations, we conducted several hundred qualitative interviews between 2016 and 2023. We also consulted numerous secondary sources and experts.

To organize and analyze these source materials, we created a private encrypted wiki we call *WikiCombo*. A collaborative wiki was a good fit for the networked nature of the data, especially when collected by many contributors. We uploaded and encrypted all primary and secondary sources, and assembled information into thematic pages on each group and major topics, linking analysis to uploaded sources when possible.[2] Given the collaborative and ongoing nature of *WikiCombo*, we ensured that interview subjects consented to share their information with selected researchers. Researchers can apply for access through https://wikicombo.com/.

We have four sources of qualitative data. First, we began by interviewing community members about services provided by the gang and state, fees charged, and their opinions about each actor. We conducted semi-structured interviews with 23 community leaders, 144 community members, and dozens of shops and businesses across more than 100 communities.

Second, we consulted active and former public officials. We interviewed 23 city leaders, 10 prosecutors, 19 police officers, and 17 criminal group experts. We also reviewed confidential internal law-enforcement reports on Medellín's criminal organizations (PONAL, 2019).

Third, we hired the city's main organized crime journalist, Nelson Matta, as a consultant. Matta has 18 years of experience covering organized crime in Medellín for the main newspaper, *El Colombiano*. In addition to conducting some interviews with officials, he summarized news articles, private sources, prior interviews, and court transcripts on a range of themes.

Finally, and most importantly, we interviewed members of the criminal groups themselves.[3] In total, we interviewed 140 members across 79 groups, including combos as well as

[2]Primary interviews followed semi-structured interview guides. We recorded and transcribed interviews when possible. When not possible (such as in prison) we took notes and uploaded after each interview.

[3]Most information on criminal markets and organizations comes from secondary sources, such as judicial proceedings or police investigations. Some prominent examples include case studies of the Sicilian mafia (Gambetta, 1996), New York mafia (Reuter, 1983), pirates (Leeson, 2007), and Brazilian and American prison gangs (Skarbek, 2014; Lessing and Denyer Willis, 2019). Medellín does not have such high-quality secondary data, and so we rely on primary interviews. Other examples of studies based on primary sources

13 higher-level, mafia-like organizations called *razones*, discussed below. Our highest-ranking sources include about 24 combo leaders and 16 high-ranking members of the most powerful razones. About half were still active members or leaders.

We conducted roughly half of these interviews in Medellín's three prisons.[4] In the beginning, prison wardens announced the offer to meet with a group of professors in a meeting room. These constituted our earliest sources. Some continued to meet us for multiple interviews. Others referred us to additional sources inside and outside the prison.

Eventually, we developed criminal contacts and referrals outside of prison. Some of these were referrals from imprisoned sources. We also hired a government gang outreach worker (himself a former street-gang member and prison-gang leader), who became a full-time research associate. About one-third of our interviewees are active and former gang members in his personal network.

Altogether this is a convenience sample of criminal actors willing to speak. Some obvious questions are who did so, why, and whether we can trust their responses. It is impossible to know the answers, but our interviews and experiences provide some clues. One observation is that our subjects tended to be self-secure. Lower-ranking members were often hesitant to speak with us, for they lacked the authority to divulge information, and some spoke with us only after receiving a referral or permission from a more senior member. For imprisoned sources, interviews also posed even less risk, since most subjects had already been prosecuted for the criminal activities they discussed.

Our subjects also seemed to be drawn from the members most interested in a chance to exhibit their expertise and insights. Occasionally, we were referred to people who were highly suspicious and unwilling to speak. The ones who did talk to us seemed flattered by academic attention, and several indicated an interest in being the subject of research. In prison, our interviews also offer subjects a respite from routine.

Our sources may also have had their own agendas. Some remarked that the government underestimated their strength, that this interfered with bargaining, and that we could resolve this as we seemed to have a more accurate understanding of the situation. Others openly hoped for some kind of negotiation with the government, and may have seen our project as consistent with those objectives.

In general, we think the information we collected is relatively reliable. One reason is that gang organization and rule are not sensitive subjects or prosecutable offenses. In addition, we sought to validate our observations with multiple sources. For most topics we discuss we

include Levitt and Venkatesh (2000) on a defunct Chicago gang, Sánchez De la Sierra et al. (2022) on corrupt Congolese traffic police, and Rodgers (2006) on a Nicaraguan gang.

[4]Two of these prisons have a wing for about 80 to 120 high- and middle-ranking criminals. The third one has roughly a wing for one or two razones and their combos.

have at least 2–3 sources between gang members, ex-members, and experts. Nonetheless, for obvious reasons, we cannot trust these qualitative data entirely.

Finally, we used 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 government. We made every effort to preserve anonymity and confidentiality, while advising subjects in consent scripts of the potential limits to our ability to do so. With prison populations, we also took great efforts to ensure that our interviewees faced no pressure to speak to us. Finally, we consulted extensively with the University of Chicago and Universidad EAFIT human subjects committees, and we obtained written support and noninterference from the Mayor, the head of the National Prison Authority, and the Colombian Minister of Justice.

2.2 Survey data

In order to collect systematic data on governance, in 2019 we surveyed nearly 7,000 residents and businesses in Medellín. We randomly sampled 2,347 of the city's 14,600 blocks, stratified by neighborhood, then tried to interview roughly two households and one business per block (Blattman et al., 2024).[5] The survey collected information on: governance services provided by the state and combo; the perceived legitimacy of both; and incidence of taxation and payments. The survey was representative of all 223 low- and middle-income neighborhoods in Medellín.

Naturally, we are concerned that citizens under-report gang activities. Several pieces of evidence suggest that survey respondents answered questions truthfully, however. First, combos are a routine part of life, and in interviews and surveys, we found that most people spoke freely when interviewed in private. Second, we conducted all surveys anonymously, alone, and indoors. As discussed below, a survey experiment and patterns of non-response do not suggest systematic misreporting.

[5]The survey firm randomly selected households and businesses for interviews. If no one was home, if the business was closed, or if the target refused, the firm selected another household or business from that block. The 2,347 respondents represent about 80% of all homes or businesses selected. The firm did not maintain data on refusals versus not home/open.

3 Context and descriptive analysis

3.1 The state

Medellín has 2.6 million people, with almost 4 million in the metro area. The city is divided into 16 urban *comunas*, which are themselves composed of 269 neighborhoods called *barrios*.

The city has a well-organized, professional bureaucracy with high fiscal capacity and broad public services. Per capita annual income is roughly $11,500, adjusted for purchasing power parity. With a huge commercial sector and tax base, the city has ample revenues.

Two main organizations are responsible for order. The Secretariat of Security is a large civilian organization with roughly 2,500 civilian staff. It reports to the Mayor and is the city's primary organization to address issues related to security and dispute resolution. The Secretariat has several "headquarters" in each comuna: *Inspecciones* host officials who directly resolve community disputes; *Comisarías* host a wide range of family services; and *Casas de Justicia* host both. We refer to all as "municipal headquarters."[6]

The Metropolitan Police are independent from the city government. They are part of the National Police, which is a branch of the Defense Ministry. While low-level corruption and poor responsiveness are common, the police are 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.

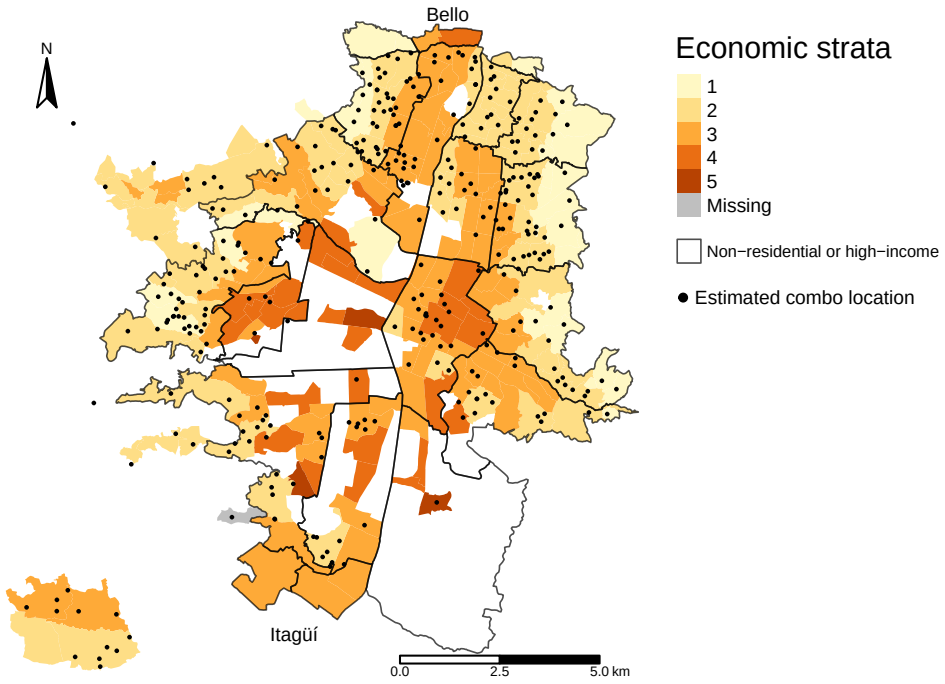
Each comuna has a police station, and the comuna is divided into a large number of *cuadrantes* (quadrants)—a sub-unit relevant only for the police. Each quadrant typically has 6 assigned officers who patrol on motorbike, in pairs, in 3 shifts per day.

In terms of corruption, low levels of police corruption are endemic, if only because the city's drug gangs are extremely profitable and local patrol officers are greatly outnumbered. (We estimate there are roughly a dozen combo members for every on-duty officer.) Even so, our interviews suggest that the middle and upper ranks of the police and the Alcaldía have relatively low levels of corruption. Gang leaders describe an almost entirely adversarial and hostile relationship with the Alcaldía and police (even the patrols). At best, payoffs to patrol officers curb arrests and seizures somewhat, since local officers are held to quotas by higher levels of the force.[7] Each comuna also has a specialized force for organized criminal

[6] In addition to these comuna-based services, the central Secretariat is responsible for: setting security policy and coordinating actions with the police, the prosecutor's office, the prison system, and other components of the criminal justice system; investing in security infrastructure; operating the system for emergency attention, including dispatching all emergency calls; and regulating the use of public space across the city.

[7] As one 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." (Criminal Group Leader 24, interview 5 [12/14/2020]). Finally, the police are a national institution, and the

Figure 2: Combo census: Estimated locations, with barrio income level



investigations and raids that seems to function effectively. Other municipalities, such as Bello to the north, appear to be heavily captured by organized crime. But all our evidence points to Medellín avoiding this fate.

3.2 Combos

Virtually every low- and middle-income residential neighborhood in Medellín has a combo. Combining newspaper, police, and our own qualitative data, we identified about 350 in Medellín and 400 in the metropolitan area (Blattman et al., 2023). We do not have borders for each one, but Figure 2 plots our estimate of each combo's principal location.

Organization and operations

Today, combos are first and foremost retail drug-selling organizations. They typically have a local monopoly on neighborhood drug sales—mainly cocaine and marijuana. Drug corners are known as *plazas de vicio*, and most combos have at least one or two plazas in their territory. Generally speaking, these plazas are the combo's most profitable activity by a wide margin. Profits vary dramatically, however, depending on whether the combo sells to local residents versus middle- and high-income people from elsewhere in the city.

senior ranks are rotated regularly. Thus it is difficult to buy influence above the local level.

The typical combo has 15 to 40 permanent, salaried members, most of whom work in the gang's drug-selling operations: transporting, packing, selling, overseeing finances, and watching for the police or rivals in times of conflict. A majority of combos also charge security fees to some residents and businesses, typically in return for protection services (discussed at length below). When they do so, we estimate that fee collection and services occupy about 10–20 percent of combo staff. On the side, individual combo members also commonly engage in loan-sharking, debt collection, professional theft, contract killings, and local consumer goods markets. But the combo itself is usually not the residual claimant on profits outside of drugs and protection fees.

All combos are headed by a leader called a *coordinador*, who usually comes from the neighborhood and grew up in the gang. A combo's territory is often no more than a few square blocks, and 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, as younger generations take over from older ones.

Combos are the base of a pyramidal criminal structure in Medellín. Above them are roughly 17–19 mafia-like groups sometimes called *razones*. Razones are the wholesale suppliers of drugs to the combos' street retail operations. Most combos have a longstanding relationship with a razón. The razón is also a military ally and helps combos coordinate prices and regulate conflicts with neighboring gangs (Blattman et al., 2023). Despite this long-run relational contract, most combos are autonomous organizations—most choose their own leaders and are the residual claimants on their profits.

Note that this retail drug market and city-wide system of supplier relationships and political alliances emerged in the early 2000s. Prior to this, Medellín's razones and largest combos were focused on the trafficking of drugs to the United States. There was little local drug consumption and few local rents to be earned. Most neighborhoods had combos through the 1980s and 1990s, but these gangs did not have drug plazas or sell protection services. Rather, they served other purposes—identity and companionship, protection from outside combos, and protection from urban militias sponsored by guerrilla groups during the country's civil conflict.

In the 2000s the civil conflict quieted and, more importantly, the razones shifted their efforts away from the international drug trade to local illicit rents—partly in response to competition from other trafficking cartels, and partly due to increasing U.S. interdiction and extradition (Martin, 2012). There are no data on early drug sales, but qualitatively it is around this time—the early 2000s—that combos and razones began to invest in retail plazas and building up local consumer demand for drugs (Salazar and Jaramillo, 1996; Thoumi, 2017). Combo governance also appears to have emerged around the same time.

Governance and legitimacy

Today, many of Medellín's combos provide a degree of order and protection in their neighborhoods. Examples include stopping fights in the street, managing drunk and disorderly people, settling neighbor disputes, collecting debts, and providing security to businesses, vehicles, and homes.[8] At least one combo even installed security cameras for a time.

To assess state and combo governance in 2019, our survey asked residents how frequently each actor responded to 17 common disputes and forms of disorder. Twelve of these questions relate to residents and 5 to business owners. We selected the 17 based on our interviews with residents, community leaders, and combo members. We focused on the most common protection services. We excluded other types of governance (such as infrastructure or trash pickup) in part because our focus is on the provision of order, and in part because combos almost never offer these forms of governance. Our goal was to study the services that combos and the state compete to provide.

Table 1 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 neither the state nor the combo are fully responsive to disputes and disturbances. The state average (0.41) is greater than the combo's (0.33). But for some categories of governance, combos are slightly more responsive than the state, including: unpaid debts, some property crimes, and kids fighting on the street.

These averages conceal a great deal of neighborhood variation, however. Figure 3 maps relative state governance by barrio. In 31%, residents report the combo is more responsive than the state. In others the state is dominant, though to varying degrees. Note, however, that high state governance does not imply combos are absent. Nearly every neighborhood has a strong combo presence. Some combos have chosen not to offer private protection in those neighborhoods. See Appendix Figure A.1 for variation in combo governance.

When they choose to govern, moreover, states and combos vary in their accessibility. Table 1 also report how residents rated the ease of contacting each actor and their response speed (we do not have these data for businesses). Again we rescale a Likert scale to 0–1. On average, 58% said the combo was easy to contact compared to 42% for the state. They also said the combo responded rapidly 55% of the time compared to 38% of the time. We average ease and speed to create a simple index of accessibility.

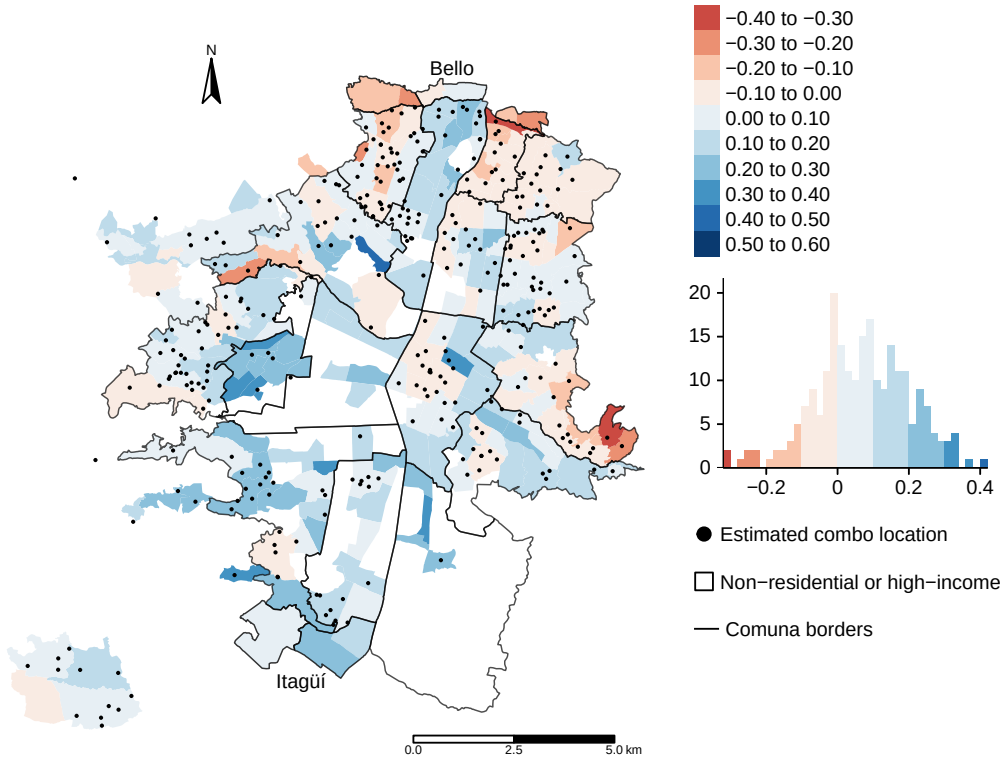
[8]Other forms of governance—such as providing physical infrastructure, or facilitating coordination and decision-making—are solely the province of state institutions. Combos tend to specialize in services that are at least partially excludable, and those that benefit from coercive power.

Table 1: State and combo governance and legitimacy, barrio survey averages, 2019

	Frequency/Rate (0-1 Scale)				Relative state governance
	State		Combo		
	Estimate	SD	Estimate	SD	Difference
	(1)	(2)	(3)	(4)	(5)
Governance Index	0.41	0.26	0.34	0.29	0.07
How often they intervene when:					
HH: Someone is making noise	0.43	0.38	0.19	0.30	0.23
HH: Home improvements affect neighbors	0.41	0.38	0.25	0.34	0.16
HH: There is domestic violence	0.51	0.37	0.35	0.37	0.15
HH: Two drunks fight on the street	0.54	0.36	0.40	0.37	0.13
Biz: Someone disturbs a business	0.50	0.38	0.36	0.38	0.12
Biz: You have to react to a robbery	0.52	0.37	0.40	0.39	0.11
Biz: It is necessary to prevent a theft	0.45	0.37	0.38	0.39	0.07
Biz: Businesses in this sector are robbed	0.42	0.39	0.35	0.38	0.05
HH: People smoking marijuana near children	0.29	0.36	0.25	0.36	0.04
HH: A car or motorbike is stolen	0.46	0.37	0.43	0.38	0.04
HH: Someone is threatening someone else	0.42	0.36	0.41	0.37	0.01
HH: You have to react to a robbery	0.46	0.36	0.45	0.38	0.01
HH: Someone is mugged on the street	0.39	0.36	0.41	0.38	-0.01
HH: It is necessary to prevent a theft	0.40	0.36	0.42	0.38	-0.03
HH: Kids fight on the street	0.29	0.35	0.32	0.37	-0.04
Biz: Someone does not want to pay a debt	0.17	0.31	0.23	0.35	-0.06
HH: Someone refuses to pay a big debt	0.22	0.31	0.39	0.38	-0.16
Accessibility Index	0.42	0.23	0.57	0.28	-0.16
How easy is it to contact...	0.45	0.25	0.58	0.31	-0.13
How fast is the....	0.38	0.29	0.54	0.36	-0.16
Legitimacy Index	0.58	0.21	0.43	0.28	0.13
How much do you trust the...	0.57	0.30	0.36	0.36	0.19
How fair is the...	0.55	0.27	0.41	0.35	0.11
How do you rate the...	0.60	0.22	0.51	0.28	0.09
How would your neighbors trust the...	0.59	0.23	0.50	0.29	0.09
How much do your neighbors trust the...	0.57	0.28	0.47	0.36	0.09

Notes: Different governance questions were asked of household (HH) and business (Biz) respondents. Only households answered legitimacy questions. 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. Modified table from Blattman et al. (2022)

Figure 3: Relative state governance by barrio, 2019



Notes: Our index of relative state governance runs from -1 (full combo governance) to 1 (full state governance). Each barrio's value is the average relative state–combo governance index for all 17 items in Table 1. We did not survey high-income and non-residential barrios.

In some ways, state inaccessibility creates opportunities for the combo to govern. 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 contracting peculiarities, every December to January a large proportion of city staff on contracts are not working. The combo, by contrast, is always present.

Combos have other advantages in governing. They often have more local knowledge and deeper networks than state representatives.[9] Community leaders have good information too, but combos have organized means of coercion to enforce rules and deals. Indeed, a combo's freedom to use force can exceed that of the state. For example, they 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, some combos openly resolve disputes and enforce

[9]As one combo member told us: "Authorities alone are not capable of controlling the problems of the neighborhood. If it weren't for us, this neighborhood would be in disarray. Actually, they look to us to solve problems that they are not capable of doing." —Criminal Group Leader 1, interview 4 [02/18/2020].

contracts in favor of those who hire them or who are most closely connected. Residents have few mechanisms for accountability or voice in shaping and enforcing combo rule.

These differences illustrate the ways in which state and combo governance are differentiated services—substitutes, albeit imperfect ones. We also looked for examples of complementary services. For instance, the state could conceivably work with combos to apprehend other criminals and then prosecute them. We looked extensively for such instances of cooperation and complementarity but found no examples.

These differences between state and combo governance also help explain why residents are conflicted about combo rule. Most people are unhappy about the combo's drug sales and fee collection, but many are also happy to have access to both the combo and the state for protection. For instance, 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.

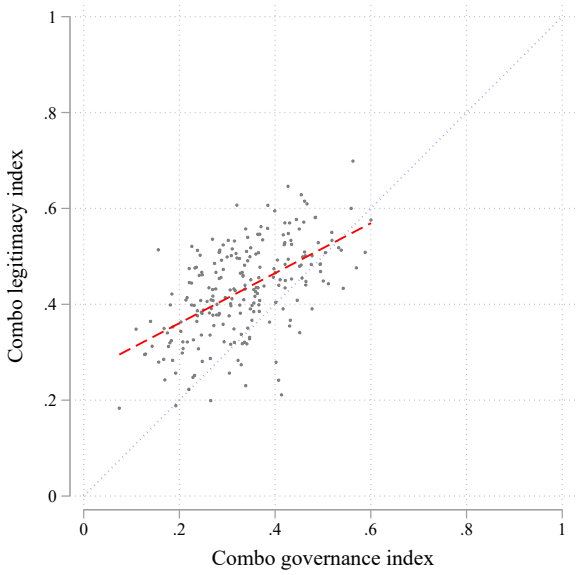
Finally, we asked residents (but not business owners) about several dimensions of legitimacy: whether actors were fair; how much residents trusted each actor; how much residents were satisfied with each actor; and whether residents thought their neighbors trusted and were satisfied with each actor. We averaged these responses into indexes of *State* and *Combo legitimacy* (0 to 1), as well as the difference between them, *Relative state legitimacy*, which can vary from –1 to 1. Table 1 reports barrio averages. Most combos have at least a degree of legitimacy and support in their neighborhood. And while residents rate their trust and satisfaction of the combo lower than the state, the difference is not always large (see Appendix Figure A.2). Not surprisingly, greater combo governance is associated with greater legitimacy, as illustrated in Figure 4.

Fees and taxation

Protection is a business line, and in return for these services, many combos collect one-time fees or weekly security payments. For services such as debt collection or dispute resolution, combos often charge on a fee-for-service basis.[10] Revenues from other services, such as security and protection for homes and shops, are akin to semi-voluntary taxes or a subscrip-

[10]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, interview 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 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, interview 1 [02/22/2020].

Figure 4: Relationship between combo governance and combo legitimacy, 2019



Notes: Each dot is a barrio average, and the dashed line indicates fitted values (correlation of 0.55). We did not survey high-income barrios.

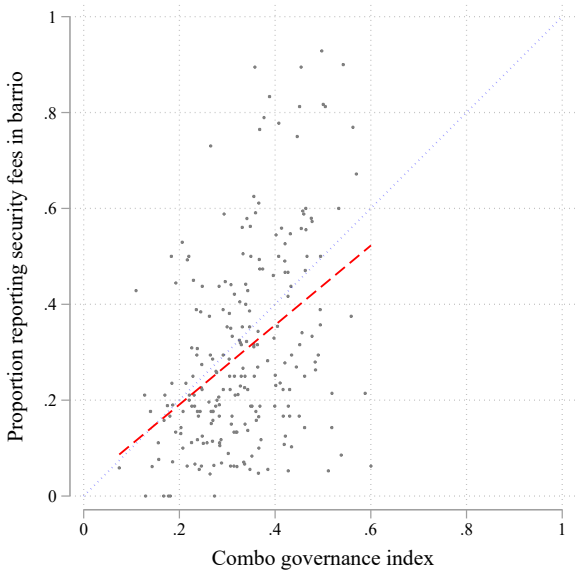
tion. Residents and businesses typically call this tax a *pago por la vigilancia* ("security" or "surveillance fee") or, more colloquially, a *vacuna*—literally, a vaccine.

Vacunas are relatively 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 at the median. While 89% of businesses said they disapprove of vacunas, just 27% of businesses said that vacunas were too high. By comparison, municipal taxes on these enterprises are about 6% of profits, and 54% said they were too high.

Combos also tax local businesses and residents in rough proportion to the services they provide. In our survey, 85% of respondents 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 5 shows that higher combo governance is associated with a higher share of people paying weekly security fees—close to the 45-degree line.

As with any tax, we should not exaggerate the voluntary nature of the vacuna. In some places they are discretionary. In others, payments are obligatory and sometimes generally accepted taxes. 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. In these cases, respondents said that the combo would simply stop providing them security if they failed to pay.

Figure 5: Relationship between gang governance and vacuna payments, 2019



Notes: Each dot is a barrio average, and the dashed line indicates fitted values (a correlation of 0.42). We did not survey high-income barrios.

Other benefits from governing

While protection is a source of revenues, to our surprise, several combo leaders explained that this was not the main reason they governed. Some described intrinsic motives—a sense of duty or a desire for authority and respect.[11] A recurring theme, however, was that governing was an investment in protecting themselves and their drug profits from the police and other competitors. These indirect benefits, as we will see, are important for understanding heterogeneous combo responses to state rule.

Leaders highlighted two ways that governing helps to prevent arrests and drug seizures. First, order on the street means there is less need for state forces to enter the neighborhood. As one combo leader put it, "There is a good relationship with the people," and so "it is easier to bring order in the sector and so the police do not have to come around."[12] When police 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.[13] Combos are especially keen to avoid homicides and

[11]Some combo leaders reported taking pride in ruling, or enjoy 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" (Criminal Group Member 6, interview 2 [02/11/2020]). Others described governance as a moral obligation or social duty to their community. This is often tied to their emergence in the 1980s as local defense forces fighting left-wing militias affiliated with rural guerrilla movements.

[12]Criminal Group Member 6, interview 2 [02/11/2020].

[13]One combo member offered a vivid example: "The police station is across from our headquarters and they

wars because it means that specialized police and municipal units enter their territory—units who are not from the neighborhood, and who are difficult to intimidate or co-opt.

Second, leaders also said that ruling fosters local legitimacy. Thus, when the state does enter, well-governed residents are less likely to collaborate with the police. As one combo member explained, "the neighbors love us, do not rat us out to the cops, watch us doing our stuff and do not interfere, and let us know when the police are coming."[14] Another leader echoed this view: "The community shields you according to your behavior," he told us. "If you do not have the community in your hands and at your back, you have nothing. That is who takes care of you."[15]

This concern for minimizing drug arrests and seizures is consistent with other patterns in the protection market. One is the relatively modest levels of vacunas charged to neighborhood businesses. Some leaders told us that they provided their services at a discount because of the indirect benefits. Others described fees and fines for dispute-resolution services as a way to limit demand and deter disputes, rather than as a money-making strategy.[16]

We also heard evidence that combos also avoid charging businesses whose loyalty is more 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.[17] In contrast, combos tend to be much more extractive of non-resident businesses such as bus lines and delivery vehicles. Most combos charge outside drivers heavy fees, and seldom provide services in return.

4 Conceptual framework

Altogether the evidence suggests two countervailing forces that affect how gangs respond to changes in state presence. The first—interchangeability of state and gang governance—drives their services to be strategic substitutes. This is the dominant explanation for criminal governance: that residents are willing to pay to have contracts enforced, disputes resolved,

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." (Criminal Group Member 5, interview 1 [10/09/2019]).

[14]Criminal Group Member 8, interview 1 [12/30/2020]. One public prosecutor endorsed this view: "They're very interested in winning over the community. That's why it's so hard to get witnesses against them." (Official 12, interview 1 [10/16/2019]).

[15]Criminal Group Leader 13, interview 1 [05/02/2019]. He added: "If you are on good terms with the civilian population, they will tell you themselves if the police are coming, they take care of you."Another said, "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." (Criminal Group Member 6, interview 2 [02/11/2020]).

[16]Criminal Group Leader 24, interview 5 [12/14/2020].

[17]Criminal Group Leader 23, interview 2 [12/28/2020].

and transaction costs reduced, and if the state fails to provide sufficient levels of these services, then people will seek out other providers. By the same logic, however, when the state increases its presence and services, the gang's best response is to reduce its rule.

The second and less recognized force—the positive indirect benefits that criminal governance can have on gangs' other illicit rents—drives state and gang rule towards being strategic complements. This is not a feature of most models of duopolistic competition, but we can introduce it.

We illustrate with a model of Cournot competition, where each player chooses the quantity of protection services and lets prices clear the market. We then introduce a second contest for control of other territorial rents, which depends on the degree of governance each actor provides. The model formalizes the countervailing forces and idenitifies the conditions in which state and gang rule will be strategic substitutes or complements.

4.1 Duopolistic competition and strategic substitutability of rule

Consider a gang g and a state s offering substitutable services to residents of a block in quantities q_g and q_s. For simplicity, we abstract away from the state's objective function and take the state's block-level rule as exogenous, $\bar{q}_s$. We do so because here we are mainly interested in understanding the gang's best response to exogenous changes in state rule.

A profit-seeking gang with fixed marginal cost of production c_g has the utility function:

$$V_g = p_g q_g - c_g q_g \tag{1}$$

Price is determined by a linear inverse demand curve $p = a - \beta q_g - \gamma \bar{q}_s$, where $\gamma \in (0, 1]$ implies the services are substitutes and $\beta > 0$ implies downward-sloping demand.

We are interested in a single comparative static: how gang rule responds to an exogenous increase in state governance on the block. Appendix B shows that:

$$\frac{\partial q_g^*}{\partial \bar{q}_s} = -\frac{\gamma}{2\beta} \tag{2}$$

So long as the services are substitutes, more state rule should "crowd out" gang protection. Any model of imperfect competition produces similar conclusions (see Appendix B).

4.2 Indirect benefits and complementarities in rule

While protection is, for many gangs, an important business line, governance and block level control can have other benefits. We summarize these motives by adding an additional term

to the gang's objective function:

$$V_g = p_g q_g - c_g q_g + \rho(q_g, \bar{q}_s)\pi_g \tag{3}$$

Here, π_g is the gang's return to perfect control of the block. This includes the rents from retail drug sales, loansharking and other business lines, but it could also include non-material benefits such as dominance, status, access to women, and other intrinsic rewards to loyalty and rule (which our interviews suggest are important).

The function $\rho(\cdot)$ is a Contest Success Function—a device commonly used in models of rent-seeking and conflict to capture the idea that competitors can increase their chances of controlling a prize by exerting effort (Tullock, 1980; Hirshleifer, 1989; Skaperdas, 1996). From the gang's perspective, $\rho(\cdot)$ is the share of total possible illicit rents π_g that it can expect to realize and retain in light of its choice of q_g and exogenously given $\bar{q}_s$. $\rho(\cdot)$ is increasing in their efforts to protect themselves (represented here by offering governance services only), and decreases with governance offered by the state.

The elasticity of gang governance to a given level of state governance now becomes:

$$\frac{\partial q_g^*}{\partial \bar{q}_s} = \frac{\lambda\pi_g - \gamma}{2\beta - \delta\pi_g} \tag{4}$$

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

4.3 Conditions in which gang and state rule are complements

Equation 4 helps us identify three circumstances in which the forces pushing towards strategic complements can outweigh those pushing towards substitutes, leading to a positive sign.

Case 1: High drug rents and initial relative combo strength ($\lambda\pi_g > \gamma$) This case corresponds the closest to our gang leader interviews. For the inequality to hold, a necessary condition is that the cross-partial λ is positive.[18] That is, the gang must have more-than-proportional returns to increasing its own rule in response to state proximity or expansion.

Many contest success functions yield this positive cross-partial when state governance is lower than gang governance. Consider the simple classic formulation, $\rho(q_g, \bar{q}_s) = \frac{q_g}{q_g + \bar{q}_s}$. As we show in Appendix B, $\lambda > 0$ when $q_g > \bar{q}_s$ and is negative otherwise. The same holds true

[18] This assumes diminishing returns to governance ($\delta < 0$). We consider the alternative below.

for many other common functional forms for $\rho(\cdot)$.

In other words, on blocks with relatively low state governance, being closer to policing and other forms of state rule can provoke gangs to develop systems of rule. Technically, a huge increase in state governance could crowd out criminal rule on blocks closest to the state. For more modest and marginal changes in state activity, however, gang and state rule can be complements.

What's more, $\lambda\pi_g > \gamma$ implies gangs will be most responsive in neighborhoods with the highest drug rents (high π_g) and when gang and state services are more differentiated (γ is close to 0). In terms of explaining block-to-block variation in our sample, however, drug rents are likely to be more important and measurable. Our heterogeneity analysis, discussed below, tests this variation in early drug potential.[19]

Finally, contest success functions like these also capture the intuition that it seldom makes sense to start an arms race with a rival who is powerful and entrenched. This is likely one reason why we see limited combo rule in wealthy neighborhoods. Even though potential drug rents are high, the state has long had a strong stake and presence in these neighborhoods, and criminal returns to effort are low ($\lambda < 0$). In rich areas and other places with deep state penetration, the gang may not have incentives to develop governance systems over time.

Case 2: Increasing returns to governance ($\delta\pi_g > 2\beta$) Case 1 assumed a positive denominator in Equation 4. This is a reasonable assumption if we think that demand is downward-sloping ($\beta > 0$) and that there are decreasing returns to additional governance in terms of the protection and loyalty it buys gangs ($\delta < 0$).

In principle, however, it is possible for $\frac{\partial q_g^*}{\partial \bar{q}_s} < 0$ if gangs enjoy increasing returns to their own governance, such that $\delta\pi_g > 2\beta > 0$. This could arise, for instance, if residents reward protection with loyalty at increasing rates.

None of our qualitative evidence points to increasing returns to either governance, however, and we are not aware of empirical or theoretical claims such a phenomenon in other cities. Hence we regard this as a technical case that is unlikely to drive gang behavior.

Case 3: Endogenous demand for governance Finally, with further adjustments to the model, there is a third potential driver of complementarities between state and gang rule. An increase in state protection could raise the number and value of transactions in the local economy, thereby increasing demand for governance in general (and gang governance

[19] In reality, gang effort may also have been endogenous to potential drug rents. Our qualitative interviews suggest that $\rho(\cdot)$ and π_g are unlikely to be as separable as our simple model suggests. Early gang investments in building community loyalty and order were in part tied to expectations of drug profitability—closeness to wealthy neighborhoods or other strategic locations, for instance.

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).

In our Cournot example, we can model endogenous growth through the demand curve, writing a as an increasing function of governance: $p = a(q_g, \bar{q}_s) - \beta q_g - \gamma \bar{q}_s$. Appendix B illustrates. The important implication is that endogenous demand, if large enough, can produce crowding in ($\frac{\partial q_g^*}{\partial \bar{q}_s} > 0$) independent of the forces in Cases 1 and 2.

Note, however, that the demand effect would have to be extremely large to overcome the crowding out that arises from normal duopolistic competition. In general equilibrium, moreover, any demand effect might be moderated by increased prices, leading businesses to locate elsewhere. Below we test for this by looking at the effect of state governance on local economic development and migration.

5 Empirical strategy

5.1 A geographic natural experiment

To test the effect of state presence on combo activities, we identified a policy that created discontinuous changes in the distance to police and civilian security headquarters for 30 years. Following this policy change, the city dramatically increased its security spending, meaning some blocks were exogenously closer to or further away from this growth in protection.

Medellín's new jurisdictional borders In 1987, Medellín's elected council divided the city into 16 areas called comunas. Previously, the city was divided into 6 such areas. The new policy subdivided each into 2–3 smaller units, producing 13 new internal borders.

This subdivision changed the jurisdiction of the state's security and justice apparatus. The Secretariat of Security deploys dispute resolution and family services within the comuna from its local headquarters. And the National Police organizes its patrolling services and attention from specialized units (such as drug squads or homicide investigations) from its station within the comuna. And all crime reports must also be filed at the comuna's police station. These organizations make efforts to deploy their services as evenly as possible in the comuna. But in all these cases, their jurisdiction ends at the comuna border.

All other government services are provided at the city or the barrio level. Schools, health services, and infrastructure are organized at the city level, for instance, and residents can access them irrespective of their address (i.e., anyone can cross a comuna border to attend their preferred school or clinic). Meanwhile, local assemblies and their budgets are organized

at the barrio level, and barrio borders were unaffected by the 1987 reform.

Figure 6 displays a map with original and new borders, as well as historical police stations and municipal headquarters. In 1987, the city had 15 full-service police stations and 37 municipal security and justice agencies that provide dispute resolution and family services. They increased this to 39 shortly as a part of the reform. These 54 historical headquarters represent the initial allocation of state services, and we focus on these.

Growth in state security spending and personnel The 1980s and 1990s were also a period of intense public investment in security in Medellín. For instance, there were 1.4 police officers per 1,000 residents in 1983. This doubled to 2.8 by 1997, the next year for which there is archival data (PONAL, 2019). We have annual data thereafter, however, and by 2017 the city had 3.6 police officers per 1,000 people (Appendix Figure A.3).

Similarly, the Secretariat of Security's budget grew almost twenty-fold in the two decades after the new borders were introduced. Our archival work uncovered a budget of $2.23 per capita for 1985, in constant 2019 USD. By 2007 (the next available year we could find data) spending was $39.50 per capita. A decade later the figure was above $50 (Appendix Figure A.3).

What do these two changes imply? The new borders took blocks that were previously served by the nearest state headquarters and assigned one of them to a new headquarters further away—a "shock" to state distance. Subsequently, one block was proximate to the growing staff and spending on security, and one block was more distant. We try to harness this variation in proximity to state growth and examine the effects on gang behavior.

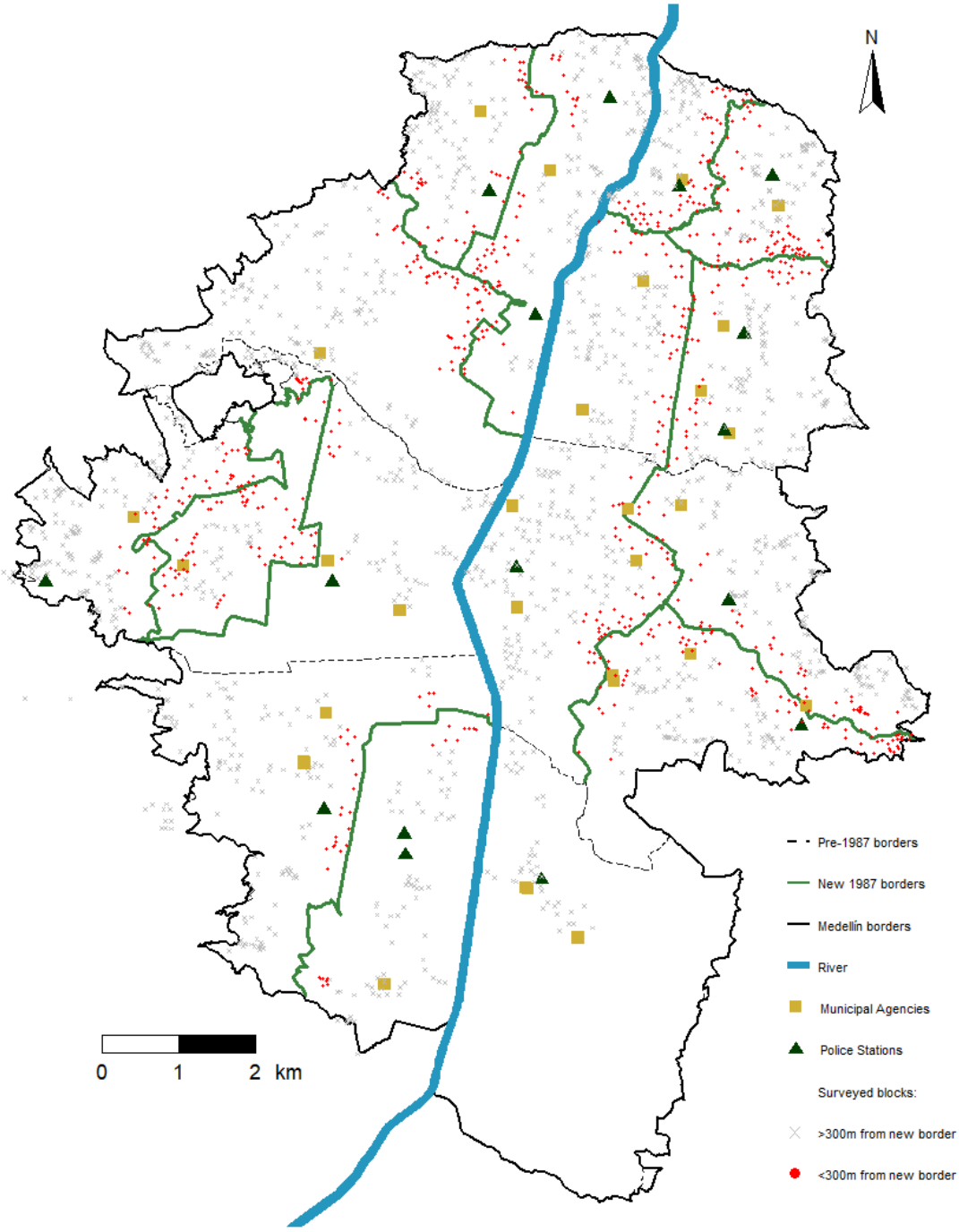
5.2 Unit of analysis and treatment

Our unit of analysis is a nearby block pair—one on each side of the new border. To create these nearest-neighbor block pairs, we use our representative sample of city blocks from 2019. For each surveyed block, we calculate a matrix of distances to every other surveyed block. We then match each block i with the closest block j in the nearest comuna. We limit our sample to pairs where both blocks are within 300 meters of one of the new borders, excluding all borders that run along natural boundaries (such as impassable mountain ridges or rivers). Figure 6 highlights these eligible blocks in red.[20]

To calculate the distance shock, we begin with each block's average distance to its closest historical headquarters. The block assigned to a more distance state headquarters is block i.

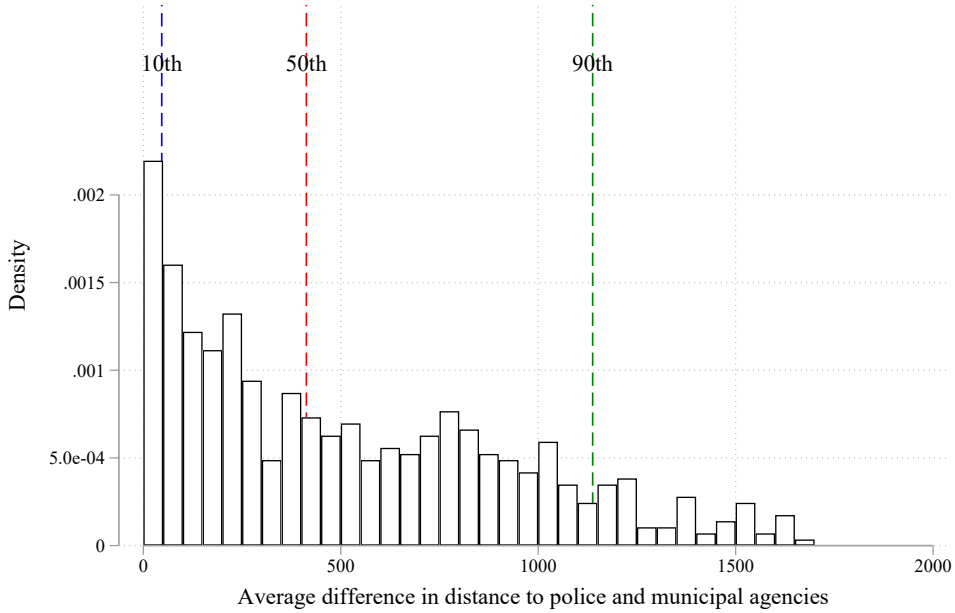
[20]We tested several pairing algorithms, as well as 200- and 100-meter bandwidths, and below show that results are generally robust to different approaches

Figure 6: Surveyed blocks, historical state security headquarters, and pre- and post-reform comuna borders



Notes: Gold squares represent municipal dispute-resolution providers; green triangles are police stations. The dotted lines represent the old comuna borders, the solid green line represents the new borders, and the black line indicates Medellín's municipal boundaries. The blue center line shows Medellín's river. Surveyed blocks are indicated as red dots and grey crosses, where the red dots indicate being in the quasi-experimental sample (within 300 meters of a new border that is not a natural boundary, such as a mountain crest).

Figure 7: Distribution of differences in the distance to municipal agencies between paired blocks within 300 meters of the new comuna borders



Notes: Vertical lines represent the 10th, 50th and 90th percentiles.

To create the distance shock Δd_{ij}, we take i's new distance to state headquarters and subtract the shortest distance to the former state headquarters. Figure 7 displays the distribution of Δd_{ij}. Before the new border, blocks were about 1,000 meters from their headquarters. At the 10th percentile, the distance shock is 40 meters, at the median it is 402 meters, and at the 90th percentile is 1,129 meters closer.

5.3 Identification and balance

We are interested in whether the distance shock Δd_{ij} leads to block-pair differences three decades later. To identify this, there are two key identifying assumptions.

The first is that our outcome is not systematically under-reported when the block is far from the state, and correctly reported when close to the state. This could manufacture a correlation between state proximity and sensitive outcomes, such as combo governance. In Appendix D we use survey experiments and other analysis to show that there is little evidence of systematic under-reporting of sensitive items, such as taxes paid to the combo, and that there is no correlation between misreporting and state distance. Measurement error remains a source of bias, but we show it unlikely to account for the treatment effects we observe.

The second assumption is that the only thing that changes discontinuously at the border

is proximity to comuna-based state services, proxied by d (Keele and Titiunik, 2015). This would be violated if there were other systematic differences between the paired blocks, ΔX_{ij}, correlated with both ΔY_{ij} and Δd_{ij}.

This is the advantage of the border natural experiment. Consider a potential confounder X, such as the distance to local business agglomerations. For example, we might expect businesses to locate themselves close to state protection services. Combos might also choose to govern near these clusters of commerce and the state. This would confound a simple cross-sectional regression of combo governance on distance to the state. Once we look at cross-border differences, however, this confounding should dissipate. Paired blocks should be equally close to business agglomerations, not only because the two blocks are close to one another, but also because they can access and benefit from the economic activity regardless of the border.

If our identification assumptions are correct, then the pair difference in any confounder, ΔX_{ij}, should be uncorrelated with the distance shock Δd_{ij}. This balance test is our setting's analog to a regression discontinuity plot with covariates unaffected by treatment.[21] Table 2 reports test of continuity for observed covariates.[22] Column 1 reports means and standard deviations for all blocks within 300 meters of a new border. Column 2 reports the main identification test—whether differences in paired blocks correlate with Δd_{ij}. Each estimate comes from a separate regression of the covariate on a distance shock controlling for border fixed effects and each block's distance to a common fixed point on the border, as in our main estimating Equation 5 below. We report clustered standard errors as well as wild bootstrap p-values (also described below).

We observe economically small and statistically insignificant differences across 15 of 17 covariates—roughly what we would expect by chance. Blocks that the new border left closer to protection services were slightly newer and slightly more distant from schools. (For instance, the median distance shock of 400 meters would put these blocks about 54 meters further from a school.)

Ideally, we would also have a measure of pre-1987 gang presence, to show that there was no discontinuous change in gang presence at the new borders. Unfortunately, there are no gang data at the block level at any time in Medellin's history other than our 2019 survey, and the city did not begin preserving and geolocating arrest data until the 2000s.

[21] A traditional graphical plot is impossible because treatment is not discrete; our unit of analysis is a matched pair rather than a single street block; the borders do not assign treatment in a single direction; and (most importantly) the new borders assign a distinct and continuous distance shock to each border-pair.

[22] Baseline demographic characteristics come from the 1993 census. Earlier rounds were not available. Although these data were collected slightly after treatment began, we nonetheless expect them to change only negligibly at the border (DANE, 2022).

Table 2: Balance test: How block pair differences vary with Δd_{ij}

Effect on ΔX	Subsample Mean (SD)	Effect of moving 100m closer to state: Estimate (SE) [p-value]	As % of sample mean: Estimate
	(1)	(2)	(3)
Median age (1993)	25.77	0.201	0.8
	(6.17)	(0.185)	
		[0.312]	[0.3]
Share of population no education (1993)	0.04	-0.000	-0.5
	(0.06)	(0.002)	
		[0.881]	[0.9]
Share completed primary ed. (1993)	0.82	-0.003	-0.3
	(0.10)	(0.004)	
		[0.524]	[0.5]
Share completed secondary ed. (1993)	0.46	0.000	0.1
	(0.20)	(0.005)	
		[0.947]	[0.9]
Share completed higher ed. (1993)	0.10	0.003	2.8
	(0.11)	(0.003)	
		[0.374]	[0.4]
Log of total population (1993)	4.75	0.014	0.3
	(1.03)	(0.053)	
		[0.824]	[0.8]
Share of non-mestizo polulation (1993)	0.00	0.000	6.7
	(0.01)	(0.000)	
		[0.212]	[0.2]
Share of women (1993)	0.54	-0.003	-0.6
	(0.06)	(0.004)	
		[0.486]	[0.5]
Block average elevation (meters)	1,606.89	-1.300	-0.1
	(114.60)	(0.913)	
		[0.233]	[0.2]
Block average slope	89.95	-0.021	-0.0
	(0.72)	(0.015)	
		[0.280]	[0.3]
Block present in 1948	0.17	0.004	2.4
	(0.37)	(0.008)	
		[0.625]	[0.6]
Block present in 1970	0.65	-0.006	-1.0
	(0.48)	(0.027)	
		[0.861]	[0.9]
Block present in 1985	0.85	-0.042*	-5.0*
	(0.36)	(0.017)	
		[0.055]	[0.1]
Meters to schools in 1986	225.39	-13.480**	-6.0**
	(132.81)	(5.008)	
		[0.021]	[0.0]
Meters to health centers in 1986	787.17	-9.483	-1.2
	(506.22)	(9.734)	
		[0.409]	[0.4]
Meters to business centers	327.79	-2.219	-0.7
	(55.38)	(3.097)	
		[0.550]	[0.5]
Block area (m^2)	4,992.17	-484.034	-9.6
	(7,242.60)	(326.610)	
		[0.208]	[0.2]

Notes: Column 1 reports summary statistics. Column 2 tests whether differences in paired blocks are correlated with state proximity using Equation 5. Each estimate in Column 2 comes from a separate regression of the covariate on the distance shock controlling for border fixed effects and each block's distance to a common fixed point on the border. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets.

Thus, identification rests on the assumption that the 1987 borders were not drawn by the city in response to gang territories, or along natural boundaries that might also affect gang behavior. This is one reason why we exclude borders that fall along natural boundaries, such as rivers, and mountain ranges, and other obstacles.

5.4 Estimating average treatment effects

To estimate local treatment effects, we run the following ordinary least squares regression:

$$\Delta Y_{ijb} = \alpha_b + \beta \Delta d_{ij} + \theta \Delta X_{ij} + \lambda B_{ij} + \varepsilon_{ijb} \tag{5}$$

As above, ΔY_{ijb} is the difference in outcomes within block pairs. ΔX_{ij} is a vector of pair differences in the baseline control variables from Table 2. B_{ij} is a vector of each block's distance to a common fixed point on the border (akin to a running variable, to account for differences in distance to the border), and α_b is a vector of border fixed effects.[23]

This approach follows Keele and Titiunik (2015) on geographic regression discontinuities, applied to a within-city setting with continuous shocks and variation in the direction of treatment. Within a 300-meter bandwidth of the new border, it treats the distance shock as a random variable conditional on covariates and the distance running variables.

We are primarily interested in the coefficient on Δd—how the intensity of the shock affects long-run differences in the block pairs. The other terms control for potential confounders.

Non-uniform direction of treatment and clustering If treatment was assigned uniformly, the border natural experiment would be akin to a clustered randomized trial with just 13 clusters. That is not the case, as none of the 13 borders assign treatment in a uniform direction. We illustrate in Figure 8. On the far left, the new border assigns block 1 to a more distant headquarters because its pair (block 2) is closer to its comuna headquarters. On the far right, the same border assigns a block on the opposite side (block 6) to a more distant headquarters. Distance shocks $\Delta d_{1,2}$ and $\Delta d_{6,5}$ have similar magnitudes, but the border does not consistently assign blocks on one side to treatment or control.

Moreover, the magnitude of the shock also varies along the border. Any block pair in between those on the far left and right will receive a much smaller shock, as illustrated by blocks 3 and 4. This is partly due to irregular border lines, but it is mainly because of the positioning of state headquarters on either side.

[23] Our main specification includes a fixed effect for each border and treatment direction—26 in all (2 for each of the 13 borders). These fixed effects account for the possibility of idiosyncratic variation arising from 13 new borders and the particular positions of stations. It also accounts for unobserved characteristics of the border, such as high drug value or other territorial characteristics that affect state or combo decision-making.

This switching of treatment directions arguably creates treatment clusters along border segments, as Figure 8 also illustrates. We identify 42 such clusters—roughly 3 per border, with about 14 block pairs per cluster.[24]

We calculate standard errors in two ways. One is the wild bootstrap method, which tries to mimic the original data generating process through repeated random sampling of the data with replacement (Roodman et al., 2019). The wild bootstrap p-value is the proportion of bootstrap statistics that are more extreme than the one from the original sample. The wild bootstrap is especially useful when the number of clusters is small.

The second option is to use calculate clustered standard errors using the 42 block-pairs with the same treatment direction. The intuition is that it generally makes sense to cluster at the level at which treatment was assigned (Abadie et al., 2023; MacKinnon et al., 2023). The closest analogue here are the 42 border-segments, each of which assigned treatment in a uniform direction. Clustering at a higher level would be akin to clustering more coarsely than treatment was assigned, reducing statistical power and over-rejecting hypotheses.

Our tables report both clustered standard errors and wild bootstrap p-values, the latter being slightly larger and more conservative.

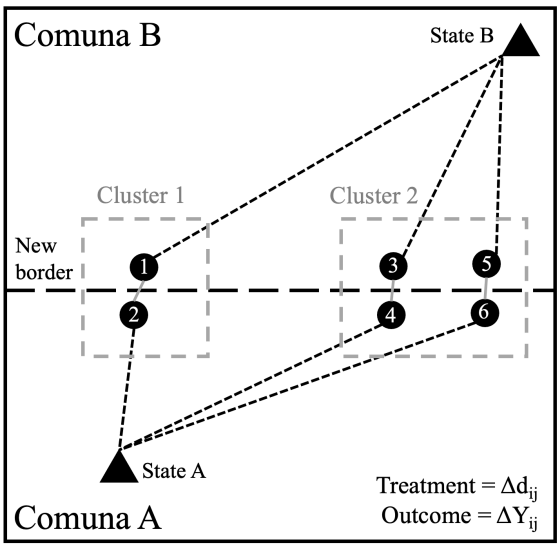
Interpretation and predictions We orient the shock so that a rise in Δd_{ij} implies getting closer to state protection. This simplifies exposition and interpretation of the tables, especially the heterogeneity analysis. It is also consistent with the fact that the border change exposed some blocks to an expansion of services. At the same time, recall that the exogenous shock assigned some blocks further from state services. Naturally, we do not know whether increases and decreases in the proximity to state services (and subsequent growth) have symmetric effects. Nonetheless, our model and our qualitative work suggest that this asymmetry is probably less important than other important forms of heterogeneity (such as proximity to drug corners, described below).

We expect that $\beta_{state} > 0$ when looking at state service provision and legitimacy. As all blocks receive some degree of state services, this estimates the intensive margin of state proximity and expansion.

We are principally interested in the effects on combo governance and accessibility, however. But the sign of β_{combo} is ambiguous. A $\beta_{combo} > 0$ would imply strategic complementarities with state rule. For the most part, there was little to no combo governance until the 2000s, and so equation 5 estimates whether combo governance was more likely to emerge where the state is present or distant.

[24]We use a simple algorithm: we first divide block-pairs by direction of treatment (for a minimum of 2 clusters per border) then run a k-means algorithm (k=4) to find up to 4 points that minimize distance to each subsample. We constrain these 4 clusters to be at least 500 meters apart.

Figure 8: Stylized illustration of variation in both treatment intensity and which side of the border is treated



Notes: On the left, the new border assigns block 1 to a more distant headquarters than its pair (block 2). On the right, the same border assigns a block on the opposite side (block 6) to a more distant headquarters. The shocks $\Delta d_{1,2}$ and $\Delta d_{6,5}$ are similar in magnitude, but the direction of the treatment varies along the border. In light gray we illustrate the two clusters.

Note that we expect to estimate *within-combo* effects on gang rule. Combo borders generally do not coincide with comuna borders, and oftentimes the paired blocks will be under the same combo. Thus, β_{combo} estimates how combos are allocate investment and effort within their territory.[25]

5.5 Discerning mechanisms

Combo strategic response Our criminal leader interviews and model implied that strategic complementarities with state rule should be strongest where gangs' rents from criminal activities other than extortion, such as drug retailing, are greatest. One way to test this is by interacting the treatment variable with a measure of the potential for illicit rents, π_{ij}:

$$\Delta Y_{ijb} = \alpha_b + \beta \Delta d_{ij} + \gamma \pi_{ij} + \delta \Delta d_{ij} \times \pi_{ij} + \theta \Delta X_{ij} + \lambda B_{ij} + \varepsilon_{ijb} \tag{6}$$

If π_{ij} is a standardized, normally-distributed measure, then the coefficient on Δd_{ij} estimates the effect of state proximity in neighborhoods with average levels of illicit rents (and so its

[25]This could imply interference between "treatment" and "control" blocks on either side of the new borders. In principle, this interference would be reduced by excluding from our estimation the block pairs that are closest to the border. We examine this as a robustness check, below.

magnitude should be similar to the average treatment effect estimated in equation 5). The coefficient on the interaction term estimates the responsiveness to illicit rents. If our criminal leader accounts are correct, we expect this to be positive.

One option is to use proximity to present-day drug markets as a measure of π_{ij}. The city's principal organized crime journalist and a former razón leader and mediator between gangs mapped the 139 major drug markets for us. Since most of these markets emerged in the 2000s, equation 6 would assess the degree to which combo governance and the retail drug trade co-moved post-treatment. This would be consistent with the strategic response mechanism, but would not be a heterogeneity analysis based on exogenous characteristics.

To develop an exogenous proxy for future drug profitability, we use baseline data to predict proximity to lucrative present-day drug markets. For each block pair, we calculated the average distance to their 10 closest drug markets. We then trained a lasso model to predict a block pair's drug market proximity. Appendix C summarizes our approach and results.

6 Results

6.1 Governance

Table 3 reports the effect of a block being 100 meters closer to the state on two measures of governance. We look at the frequency with which survey respondents observed the state and combo responding to 17 forms of disorder or disputes in their neighborhoods, as well as accessibility in terms of the ease of contacting both actors and their speed of response.

For every 100 meters closer to state headquarters, a block's reported level of state services rises by 0.011 and its accessibility rises by 0.012. Recall that the median difference in distance is roughly 400 meters. Compared to the control means reported in Table 1, this implies the median change is associated with an 11% improvement in both measures (Column 3). The effect on governance services is not significant ($p = 0.11$ with clustered standard errors, and 0.18 with the wild bootstrap) but the accessibility measure is significant at the 5 percent level. Appendix Tables A.1 and A.2 look at impacts on the components of each index.

Combo governance moves in the same direction as state governance, consistent with combo and state governance being strategic complements. For every 100 meters closer to state headquarters, combo governance rises by 0.019 (p=0.03) and combo accessibility rises by 0.23 (p=0.05). At the median change in distance, this represents a 24% rise in gang governance services and an 18% rise in accessibility.[26]

[26]Table A.3 looks at impacts on taxes and security fees. As state distance grows closer, we see no changes

Table 3: Impacts on governance and legitimacy of being 100 meters closer to the state

Effect on ΔY	Subsample Mean (SD)	Effect of moving 100m closer to state: Estimate (SE) [p-value]	Median change as % of sample mean: Estimate
	(1)	(2)	(3)
Governance (N = 570, 563)			
Δ State Governance Index (0-1)	0.41	0.011	10.8
	(0.19)	(0.007)	
		[0.186]	
Δ Combo Governance Index (0-1)	0.32	0.019**	24.1
	(0.22)	(0.007)	
		[0.027]	
Accessibility (N = 429, 384)			
Δ State Accessibility Index (0-1)	0.42	0.012*	11.6
	(0.16)	(0.006)	
		[0.050]	
Δ Combo Accessibility Index (0-1)	0.54	0.023**	17.7
	(0.23)	(0.008)	
		[0.024]	
Legitimacy (N = 431, 425)			
Δ State Legitimacy Index (0-1)	0.58	0.001	0.9
	(0.14)	(0.008)	
		[0.883]	
Δ Combo Legitimacy Index (0-1)	0.42	0.005	4.8
	(0.21)	(0.014)	
		[0.749]	

Notes: The unit of analysis is a pair of matched blocks on either side of a new border. The dependent variable is the difference in governance or legitimacy between the block pair. Each estimate in Column 2 comes from a regression of the dependent variable on the effect of being 100 meters closer to the state (the distance shock), controlling for other baseline bock-pair differences, a running variable to the border, and border fixed effects. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets. Only residents (not business respondents) were asked about accessibility and legitimacy, and some blocks have only residents, hence the lower sample size. Non-response to combo accessibility is also higher than for the state.

Table 4: Heterogeneity: Impacts on governance and legitimacy of being 100 meters closer to the historical state, by predicted proximity to major drug plazas

		Governance (Δ)		Accessibility (Δ)		Legitimacy (Δ)	
		State	Combo	State	Combo	State	Combo
	Sample Mean (SD)	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect of moving 100m closer to state	-4.966	0.011	0.017*	0.009	0.023**	0.002	0.004
	((4.378)	(0.007)	(0.007)	(0.006)	(0.010)	(0.007)	(0.015)
		[0.114]	[0.074]	[0.108]	[0.023]	[0.787]	[0.850]
100m closer to state X Predicted drug profitability (std.)	-2.016	0.006	0.010**	-0.002	-0.004	-0.010**	-0.011**
	(5.478)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
		[0.158]	[0.039]	[0.46]	[0.90]	[0.044]	[0.024]
Controls and fixed point running var		✓	✓	✓	✓	✓	✓
Bi-directional Border FE		✓	✓	✓	✓	✓	✓
N		563	555	429	384	429	423

Notes: This table estimates Equation 6, regressing governance and legitimacy outcomes on the distance shock, a standardized measure of predicted drug profitability, and an interaction between the two. Predicted profitability comes from the predicted values of a lasso regression of post-treatment drug market proximity on baseline variables and distance to wealthy neighborhoods (Appendix C). The coefficient on the distance shock estimates the treatment effect in neighborhoods at the average pair-average distance from drug plazas, and the coefficient on the interaction tells us how the impacts of the distance shock are different in neighborhoods closer to early drug corners. The measure of distance to drug markets is included in the regression, but we do not report it in the table as the coefficient is not relevant. (The dependent variable is the difference in governance and legitimacy between the block pairs, and the distance from drug markets is by construction identical for the pair.) We report clustered standard errors in parentheses and wild bootstrap p-values in brackets.

Evidence on combo strategic response to drug rents Recall that combo governance was relatively rare in 1987. It emerged mainly in the 2000s as retail drug markets developed. Therefore, in practical terms, the average treatment effects are telling us that combos were more likely to develop governance services after 1987 in the areas closer to the state. Also, since most combo territories cross the new comuna borders, this implies that combos allocated attention to the portions of the territory with greater potential state presence.

What's more, combo governance appears to have been most responsive to state proximity when potential drug profits were high. Table 4 estimates the heterogeneity analysis in Equation 6 using our baseline measure of predicted drug profitability. A range of baseline geographic, economic, and demographic variables are reliable predictors of the density of drug markets and drug seizures 30 years later (especially the block-pair's proximity to middle- and high-income neighborhoods, as seen in Appendix C).

Looking at state and combo governance, the coefficients on being 100 meters closer to the state are similar to the estimates in Table 3 (as we expect given that the drug profitability measure is standardized to have zero mean and unit standard deviation.) We are more

in the proportion of people paying state taxes or combo security fees. This is consistent with the market for protection not being the main reason gangs decided to rule. We do observe, however, that as state distance grows closer, respondents were 41% more likely to report that the combo's security fees are "too high." One interpretation is that combos target higher fees to a smaller number of locales where they also provide private goods, as some of our interviews suggest.

interested in the coefficient on the interaction, which for combo governance is positive and statistically significant at the 1 percent level. The magnitude is about half that of the average effect of being closer to the state. This implies that there is almost no combo governance response on blocks two standard deviations further away from future drug markets, and a dramatic response on those two standard deviations closer to drug markets. In contrast, we do not see any evidence that state responsiveness is sensitive to drug rents.

We do not see the same relationship with combo accessibility, however. Combos appear to respond to state proximity by being easier to contact and faster to respond in neighborhoods with and without the potential for large drug rents. IT could be that combos expand the breadth of their services rather than their responsiveness in the most profitable neighborhoods.

State versus combo governance An obvious question is why the distance shock has a larger and more precise effect on combo versus state governance. We should draw this conclusion with caution, for the two governance point estimates in Table 3 are not statistically distinguishable from one another. Nonetheless, there are several reasons to expect state and combo responses to differ.

One is that the state is not equally responsive to all forms of disturbance and disorder. Appendix Table A.1 looks at treatment effects on the 17 components of the governance index. In most cases, being closer to the state increases reports of state responsiveness. Some of the exceptions are telling, however, such as children or drunks fighting on the street or domestic abuse. More minor street and household disorders have traditionally been the purview of informal authorities such as the combo. To the extent that the state does not tackle such incidents, it would attenuate the effect of any distance shock.

Another potentially more important factor is that the state has deliberately taken measures to mitigate the connection between distance and service provision. City agencies commonly require some degree of active outreach to peripheral communities. Also, police patrols are assigned to quadrants so that all areas of the comuna receive coverage.

The combo, meanwhile, is making a much more local allocation decision about where to allocate services and taxation, typically within their territory. The results suggest that combos respond more elastically to state proximity.

6.2 Legitimacy

Table 3 also reports average treatment effects on state and combo legitimacy—a composite index of trust, perceived fairness, and satisfaction. Both estimates are close to zero. State and combo legitimacy do not rise as blocks get closer to the state. Rather, respondents

simply report observing greater service provision from both the state and combo, and this does not affect their reported levels of trust in either actor or perceived fairness.

Table 4, however, illustrates a striking variation in legitimacy according to distance from drug markets. When a block is relatively close to drug markets, being close to the state *lowers* state legitimacy. Combo legitimacy also suffers, despite higher levels of combo governance. In contrast, on blocks far away from drug markets, state presence produces greater state legitimacy, and the combo governance response produces combo legitimacy as well.

These effects can be quite large. Again, the average distance shock places one of the block pairs about 400 meters closer to the state. On blocks one standard deviation closer to drug markets, state legitimacy falls by 0.032 (8% relative to the mean), and combo legitimacy falls by 0.04 (10% relative to the mean).

These results are consistent with our qualitative interviews in communities, where residents tended to emphasize their disdain for the presence of drug plazas. Retail narcotics sales bring undesirable people into the neighborhood, create local addicts, and can be a source of disorder, among other ills.

The results also suggest that combo governance may in part be a strategy to minimize the loyalty and legitimacy penalty that comes from selling drugs. Recall that combo leaders gave two reasons for responding strategically to the state: providing order as a preventative measure, reducing the likelihood that state representatives enter; and bolstering civilian loyalty. Conceivably the adverse legitimacy effect of state presence and drug proximity would be even worse if combos did not provide order and services

6.3 Economic development and sorting

Finally, we consider how state proximity shaped economic development. In principle, over three decades, proximity to police, dispute resolution, and other local services could increase the volume of social and economic transactions. In addition, households and businesses who demand governance may move to better-governed neighborhoods. Both could increase demand for governance from both actors, as demonstrated in the model. We can test the influence of this mechanism by estimating average treatment effects on block pair differences in economic development on either side of the new borders. (Note that this is not an identification concern for β when we look at treatment effects on gang governance, but rather a question of mechanism and interpretation—to what degree is β attributable to the state's effects on growth versus the combo's strategic response.)

The evidence is not consistent with in-migration and economic growth being major drivers of the impacts on gang rule we saw in previous tables. Table 5 estimates the effects of state

Table 5: Impacts on neighborhood prosperity and demographics of being 100 meters closer to the local state

Effect on ΔY	Subsample Mean (SD)	Effect of moving 100m closer to state: Estimate (SE) [p-value]	Median change as % of sample mean: Estimate
	(1)	(2)	(3)
Administrative outcomes (N = 473), index	0.00	0.074*	
	(1.47)	(0.043)	
		[0.090]	
Multidimentional Poverty Index (2018)	13.64	-0.301	-9.1
	(14.23)	(0.603)	
		[0.620]	
Unemployment rate (2018)	0.11	-0.003	-12.3
	(0.07)	(0.004)	
		[0.450]	
Log of total population (2018)	5.56	0.003	0.2
	(1.36)	(0.082)	
		[0.968]	
Log of economic value of land (2014)	13.34	0.017	0.5
	(0.63)	(0.015)	
		[0.238]	
Log of average housing value	11.85	-0.005	-0.2
	(0.62)	(0.031)	
		[0.864]	
School enrollment rate (2018)	0.90	0.011**	5.0
	(0.12)	(0.005)	
		[0.026]	
Adult educational attainment (2018)	9.40	0.130*	5.7
	(1.73)	(0.071)	
		[0.075]	
Percent of population who recently migrated (2018)	4.40	-0.003	-0.3
	(3.73)	(0.172)	
		[0.985]	
Count of business in a 100m radius	109.59	6.563**	24.5
	(102.53)	(3.025)	
		[0.036]	
Survey outcomes (N = 231), index	0.00	-0.033	
	(1.41)	(0.045)	
		[0.472]	
Absence of firms	8.10	0.010	16.9
	(14.42)	(0.017)	
		[0.557]	
Log of total number of firms	1.50	0.021	5.7
	(1.17)	(0.041)	
		[0.615]	
Log of mean profits (2019)	13.36	-0.023	-0.7
	(1.16)	(0.074)	
		[0.753]	
Log of mean sales (2019)	14.78	-0.032	-0.9
	(1.17)	(0.087)	
		[0.716]	
Number of employees (2019)	2.38	-0.126	-21.6
	(2.71)	(0.251)	
		[0.619]	
Number of observations		570	
Minimum N		228	

Notes: Each estimate comes from a separate regression. The indexes are standardized to have zero mean and unit standard deviation. They are weighted averages of the components measures below, where weights come from a principal components analysis. Business survey data are not available for all blocks (since some blocks do not have businesses) and so we compute a separate index for these measures. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets.

proximity to the state on a range of economic and demographic measures from administrative data, the 2018 census, and our 2019 survey. To reduce the number of hypotheses tested, we construct standardized indexes of all measures, using principal components analysis for weights. We create separate indexes for administrative data (available for all blocks) and for our business questionnaire outcomes (since not all blocks had businesses to survey).

Only one of the two indexes indicates a modest increase in development. An index of administrative measures suggests that development is 0.08 standard deviations greater for every 100 meters closer to the state, significant at the 10 percent level. We should interpret the components of the index with caution, given the number of hypothesis tests, but the largest increases are in block-pair differences adult school attainment, child school enrolment, and the density of businesses with 100 meters of the block. Meanwhile, impacts on other development measures such as a poverty measure, the unemployment rate, and housing values run in the opposite direction. The same is true of an index of survey outcomes from businesses, which also have a negative sign.

Altogether, it seems unlikely that the impacts on combo governance are driven by increased economic development. First, the median state distance shock changed combo governance by 23 percent. Even a much larger and robust increase in development would be unlikely to account for such a large change. Finally, the treatment effects are significant in only one of two indexes, and are not necessarily concentrated in the measures that would suggest increased transactions and demand for governance.

Our intention is not to discount these development impacts. It would be surprising if there was no effect of state proximity on development over three decades, and indeed we anticipated larger effects. We principally observe an effect on human capital, which could suggest selective migration or investments. (Block-level data on migration are also quite limited, and so we cannot test the effects on selection more directly.) Overall, our results are consistent with state proximity leading to slightly more demand for combo governance. But we simply do not see the magnitudes or the patterns to suggest this is the main channel by which state presence affects gang rule.

6.4 Robustness and threats to identification

Generally, all results are robust to alternative estimation approaches and models (see Appendix D.1). We also show that there is no effect of state proximity on outcomes when we pair blocks over placebo borders (Appendix D.2). This is akin to a quasi-experimental permutation inference approach, and suggests that our results are unlikely to arise by chance.

Finally, we address measurement error concerns in detail in Appendix D.3. A survey ex-

periment shows no evidence of under-reporting potentially sensitive questions. What misreporting we do observe is uncorrelated with treatment. What's more, we model measurement error and show that most forms of under-reporting would actually lead us to understate treatment effects.

7 Conclusions

In many cities, gangs, drug trafficking, and criminal governance are major concerns for mayors and police chiefs. Yet many officials have a limited understanding of their city's organized criminal structures, virtually no data whatsoever on the extent of gang rule or taxation, and little sense of these groups' relationship to residents and the consequences for state service delivery and legitimacy. Absent this kind of data and diagnosis, governments can neither craft effective strategies that anticipate what gangs and communities will do in return, nor evaluate their impact once implemented.

Contract enforcement and protection are valuable sources of criminal revenue. But the experience of Medellín also suggests that large-scale and systematic criminal governance is most likely to emerge when these groups have other illicit rents to defend. This is consistent with patterns of criminal rule in Brazil or the Democratic Republic of the Congo where there are drug corners and mines to protect, and it could help explain the absence of "good" gang governance in places like El Salvador, where drug retail markets are insignificant (Barnes, 2023; Lessing, 2020; Sánchez De La Sierra, 2020; Melnikov et al., 2021).

Collectively, these findings suggest that gang rule might most acutely affect "semi-strong states" rather than either well-established or truly weak ones. By "semi-strong" we mean governments that are powerful enough to create illicit markets and threaten criminal gangs, yet too weak to eliminate them entirely.

These conclusions are speculative, however, mainly because there have been so few efforts to measure the phenomena, and even less rigorous evidence on what policies work. We believe that the first priority is more information in more places. Cities routinely collect fine-grained data on homicides and arrests and use these to measure performance and evaluate policies. But they seldom have equivalent data on other aspects of organized crime, including extortion, criminal governance, drug markets and prices, or police and state legitimacy. Uribe et al. (2022) have already shown that it is possible to analyze basic survey data on gang presence and criminal governance cross-nationally. Cost and safety concerns make the intensive qualitative and quantitative data collection we did in Medellín dependent on local political conditions. But it is clear most cities need better information systems and diagnostics of organized crime than they currently possess.

Future work could also improve our understanding of mechanisms. Our analysis suggests that the loyalty channel is not as effective as we (and some gang leaders) expected, but future work could focus on more fine-grained and better-validated measures of slippery concepts like loyalty. Meanwhile, the alternative channel—that gang rule protects rents by reducing the frequency that the state enters—remains the most plausible explanation for what we observe in Medellín. Future research could try to test this directly (as police location and response data do not exist in Medellin). Finally, we saw modest impacts of state presence on economic development. These impacts are probably too small to explain the massive combo governance response, but admittedly we have no data on the demand for governance, and we know of no research on how development affects that demand. All of these are important areas for future study.

With better diagnosis, governments will be in a better position to tackle organized crime and criminal governance. Currently, mayors and police chiefs have almost no model policies, let alone evidence, to follow. Nonetheless, the results in Medellín provide some guidance for policy design and impact evaluation.

First, both our results and our theoretical discussion suggest that common policy interventions could backfire in the presence of these indirect motives to rule. For instance, popular responses to organized crime and extortion include police crackdowns on gangs, or attempts to prosecute extortion. But crackdowns and denunciations could actually increase incentives for the gang to govern and foster legitimacy in the most valuable neighborhoods. Efforts to reduce extortion also overlook the fact that many gangs would have an incentive to rule even if they were unable to collect fees at all.

Public investments in better governance and security can also have mixed effects. These services should make residents better off, not only because of the state services themselves, but also because drug-selling gangs will be disciplined by the threat posed to their main source of revenues. At the same time, these investments could have the perverse effect of strengthening and legitimizing profitable drug gangs.

What's more, investing in public security is no panacea, as some of our other research shows. In Blattman et al. (2023), a 2–year experiment that increased non-police state presence in dozens of small neighborhoods did not automatically translate into effective service delivery. In neighborhoods where the state began with higher capacity, civilian security attention generated better outcomes and higher state legitimacy. In areas with low baseline state capacity, however, the government failed to meet expectations and legitimacy declined. There was no average effect on governance and legitimacy. As a result, we see no signs of a combo reaction. This implies that state-building efforts, and attempts to discipline gangs, may need to be long, sustained, and effectively delivered (like the one in this paper).

Our evidence and theoretical framework suggest an alternative approach: gang rule could be best weakened by reducing gangs' illicit revenues. Marijuana legalization, addiction treatment, and other policies could lower the demand for illicit drugs. This could reduce optimal gang size as well as their incentives to govern. For example, 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—loansharking, numbers games, and labor racketeering (Kroger, 2008). Demand-reduction programs are a crucial area for rigorous research.

Yet our own results point to a terrible policy trade-off for city governments: weakening gangs could make their neighborhoods more violent and coercive. If cities cracked down on extortion, legalized drug sales, treated addicts, or otherwise reduced retail demand for drugs, gangs would have fewer incentives to treat residents well and maintain peace in their neighborhoods. Similarly, suppose a successful marketing campaign delegitimzed gangs and deprived them of local loyalty. In any of these cases, eliminating the gang's incentives to win residents' loyalty could inadvertently lead to more abusive and extortionate patterns of rule as we observe in El Salvador's recent history.

In short, criminal governance brings order to a significant share of the population, which makes policies that curb it prone to major unintended consequences. This only heightens the importance of careful policy experimentation and evaluation.

Data and Code Availability Statement

The data sources and code underlying this article are available in the replication repository located at the following URL: https://doi.org/10.5281/zenodo.10909964

All raw data that can be disclosed was collected by the authors and are available under a Creative Commons Non-commercial license. There are some original administrative databases that cannot be shared due to the sensitivity of their data or the potential identifiability of subjects. In all cases, however, we share the relevant data aggregated at the city block level. Researchers interested in access to finer data may contact the authors. It can take some months to negotiate data use agreements and gain access to the data. The authors will assist with any reasonable replication attempts for one year following publication.

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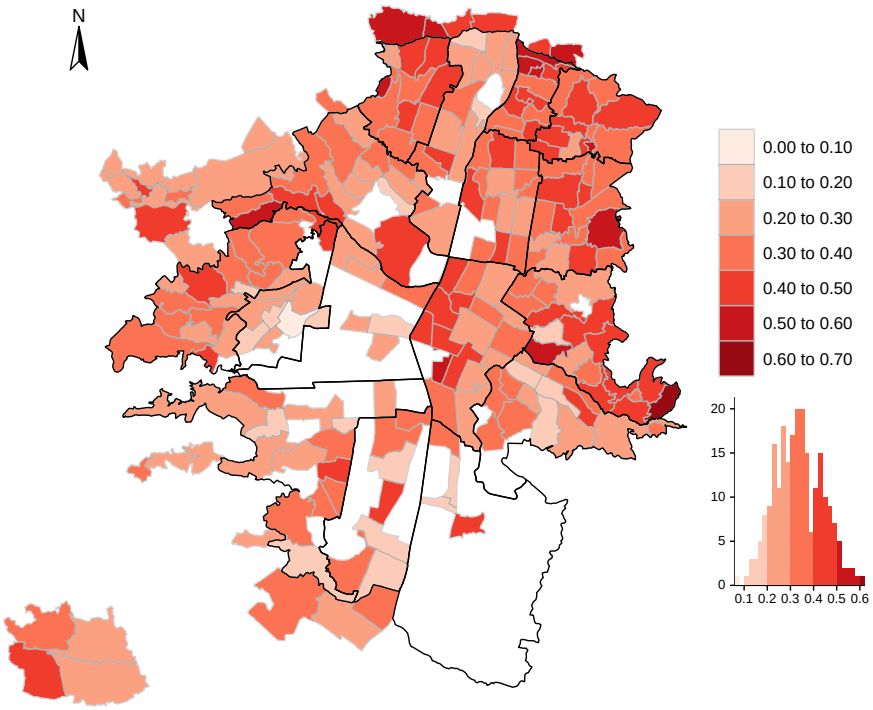
Appendix

A Supplemental tables and figures

Figure A.1: Combo governance by barrio, 2019



Notes: Each barrio's value represents the average combo governance reported for all 17 items from Table 1. Combo governance is an index running from 0 (never responds) to 1 (always responds). We did not survey high-income barrios.

Table A.1: Coefficient of closeness to state services for blocks along the inner comuna borders of Medellín on governance index components

Effect on ΔY	Subsample Mean (SD)	State Estimate (SE) [p-value]	Combo Estimate (SE) [p-value]
	(1)	(2)	(3)
Governance (N = 570, 563)	0.08	0.011	0.018**
		[0.218]	[0.039]
HH: Someone refuses to pay a big debt	-0.15	0.014	0.028*
		[0.333]	[0.063]
HH: There is domestic violence	0.16	-0.006	0.011
		[0.678]	[0.552]
HH: Two drunks fight on the street	0.14	-0.011	0.018
		[0.616]	[0.263]
HH: Kids fight on the street	-0.01	0.021	0.008
		[0.142]	[0.469]
HH: Home improvements affect neighbors	0.17	0.010	0.015
		[0.566]	[0.322]
HH: Someone is making noise	0.25	-0.005	0.006
		[0.775]	[0.391]
HH: People smoking marijuana near children	0.05	0.030*	-0.008
		[0.087]	[0.632]
HH: Someone is mugged on the street	-0.02	0.003	0.018
		[0.826]	[0.317]
HH: A car or motorbike is stolen	0.06	-0.015	0.012
		[0.318]	[0.459]
HH: Someone is threatening someone else	0.01	0.003	0.016
		[0.912]	[0.190]
HH: It is necessary to prevent a theft	-0.00	0.024*	0.021*
		[0.079]	[0.069]
HH: You have to react to a robbery	0.02	0.005	0.019
		[0.754]	[0.183]
Biz: Someone does not want to pay a debt	-0.04	0.021	0.041
		[0.538]	[0.215]
Biz: Businesses in this sector are robbed	0.07	0.017	0.032
		[0.407]	[0.433]
Biz: Someone disturbs a business	0.13	0.017	0.028
		[0.496]	[0.387]
Biz: It is necessary to prevent a theft	0.09	0.044	0.030
		[0.132]	[0.418]
Biz: You have to react to a robbery	0.12	0.032	0.012
		[0.295]	[0.746]
Number of observations		562	555

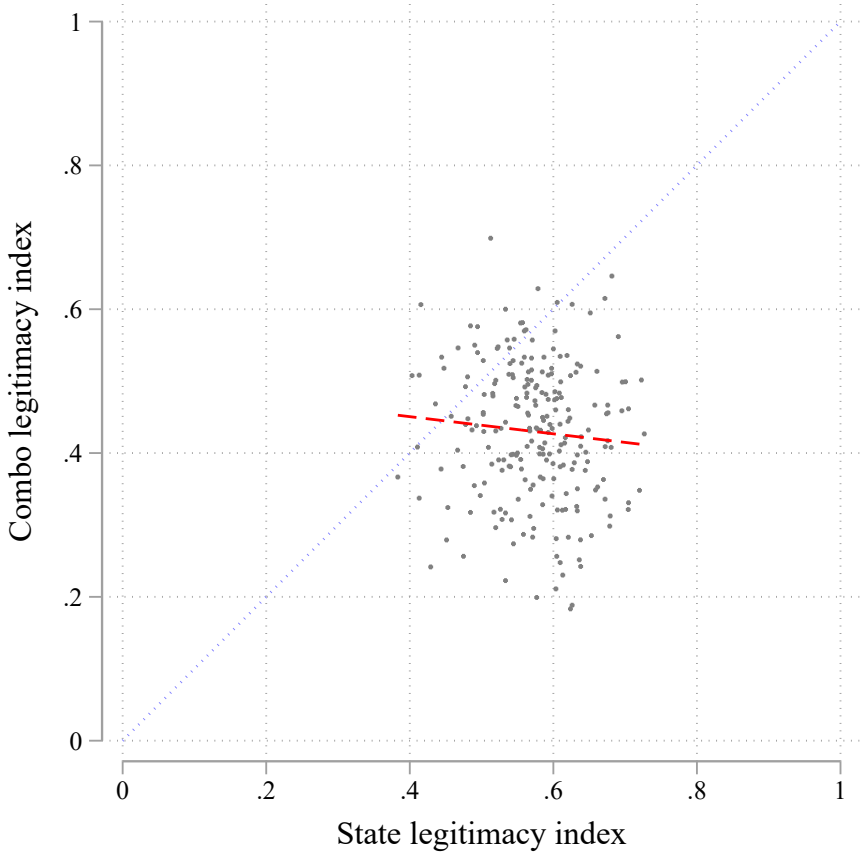
Notes: This table calculates the effect of being 100 meters further from the state on the 17 components of our governance indexes. HH indicates questions asked to households, and Biz represents questions asked to businesses. We report wild bootstrap p-values in brackets.

Table A.2: Impacts on accessibility of being assigned to be 100 meters closer to the state

Effect on ΔY	Subsample Mean (SD)	Effect of moving 100m closer to state Estimate (SE) [p-value]	Median change as % of sample mean Estimate
	(1)	(2)	(3)
Panel A: Effects on accessibility to state	0.01	0.051*	
	(1.00)	(0.027)	
		[0.050]	
How easy is it to contact the state.	0.46	0.012*	10.9
	(0.18)	(0.007)	
		[0.099]	
How fast is the state	0.39	0.011	12.0
	(0.21)	(0.009)	
		[0.171]	
Panel A: Effects on accessibility to combo	-0.07	0.075**	
	(1.03)	(0.027)	
		[0.024]	
How easy is it to contact the combo.	0.56	0.022*	16.0
	(0.26)	(0.009)	
		[0.059]	
How fast is the combo	0.52	0.027	21.4
	(0.30)	(0.015)	
		[0.180]	

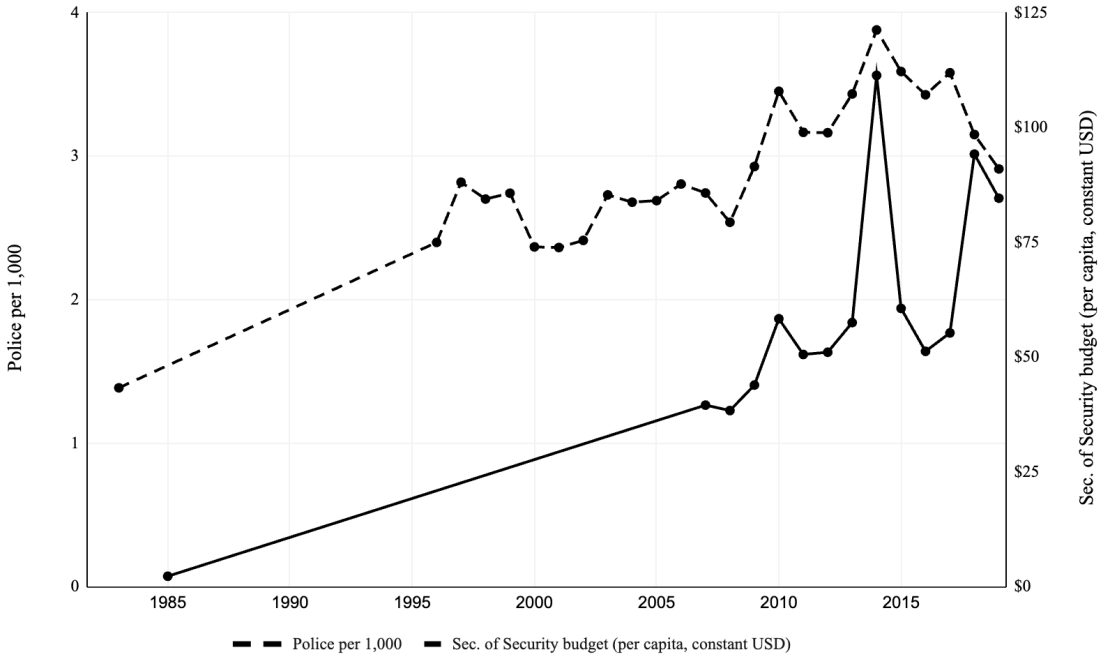
Notes: The unit of analysis is a pair of matched blocks on either side of a new border. The dependent variable is the difference within the block pair in the speed and ease of accessing each actor. Each estimate in Column 2 comes from a regression of the dependent variable on the effect of being 100 meters closer to the state (the distance shock), controlling for other baseline bock-pair differences, a running variable to the border, and border fixed effects. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets.

Figure A.2: Relationship between combo legitimacy and state legitimacy, 2019



Notes: Each dot is a barrio average, and the dashed line indicates fitted values (correlation is -0.08). We did not survey high-income barrios.

Figure A.3: Growth in state security and spending personnel



Notes: The figure depicts the evolution of police per 1,000 residents (reported on the left vertical axis) and the evolution of per capita city expenditures on security and protection (reported on the right vertical axis). Data is from the City of Medellín.

Table A.3: Impacts on taxes and security fees of being assigned to be 100m closer to the state, using historical headquarters

Effect on ΔY	Subsample Mean (SD)	Effect of moving 100m closer to state: Estimate (SE) [p-value]	Median change as % of sample mean: Estimate
	(1)	(2)	(3)
Panel A: Effects on payments to state			
Percentage of bussines paying taxes	0.52	0.010	8.1
	(0.49)	(0.021)	
		[0.685]	
Approves of city's local taxes	0.60	-0.013	-9.2
	(0.33)	(0.021)	
		[0.627]	
Says local taxes are too high	0.61	-0.008	-5.4
	(0.34)	(0.015)	
		[0.633]	
Panel B: Effects on payments to combo			
Payment rate of security fee	0.12	0.005	18.1
	(0.27)	(0.010)	
		[0.630]	
Says neighbors pay security fees	0.33	0.001	0.7
	(0.36)	(0.014)	
		[0.968]	
Approves of combo security fee	0.06	-0.003	-24.5
	(0.16)	(0.006)	
		[0.560]	
Says security fees are too high	0.28	0.041**	60.1
	(0.37)	(0.015)	
		[0.047]	

Notes: The unit of analysis is a pair of matched blocks on either side of a new border. The dependent variable is the difference in survey outcomes between the block pair. Each estimate in Column 2 comes from a regression of the dependent variable on the distance shock, controlling for other baseline bock-pair differences, a running variable to the border, and border fixed effects. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets. Only residents (not business respondents) were asked about legitimacy, and some blocks have only residents, hence the lower sample size..

B Formal presentation and extensions of model

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

B.1 Simple Cournot competition

In our baseline setup, a state and a gang engage in Cournot competition with differentiated products. 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.

Setup In each neighborhood, a gang g and a state s compete to sell protection in quantities q_g and q_s. The gang chooses q_g to maximize its pay-off with a constant marginal cost c. We assume that the state chooses $q_s = \bar{q_s}$ following a maximization process that we don't formalize, and we treat it as an exogenous parameter. Products are differentiated, and the price of the gang protection is given by the linear inverse demand function $p = a - \beta q_g - \gamma \bar{q_s}$. 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 the gang is $V = pq_g - cq_g$. For simplicity, we assume an interior solution.

Best response We begin by deriving the best response function for the gang:

$$\begin{aligned} \max_{q_g} V &= (a - \beta q_g - \gamma \bar{q_s}) q_g - c q_g \\ \frac{\partial V}{\partial q_g} &= a - 2\beta q_g - \gamma \bar{q_s} - c = 0 \\ q_g^* &= \frac{a - c}{2\beta} - \frac{\gamma}{2\beta} \bar{q_s} \end{aligned}$$

Comparative statics We are mainly interested in whether gang rule is crowded in or out when there is an exogenous increase in state governance: $\frac{\partial q_g^*}{\partial \bar{q_s}}$. To obtain this comparative static, we obtain the cross-partial:

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

Since the two services are not complements, increases in the state's supply of protection will reduce the gang's.

B.2 Cournot competition with benefits to governing

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

B.2.1 General formulation

Setup As above, but now the payoff for the gang is $V = (a - \beta q_g - \gamma \bar{q_s})q_g - cq_g + \rho(q_g, \bar{q_s})\pi$, where $\rho(q_g, \bar{q_s})\pi$ captures the externalities described in Section 4. For simplicity, we assume an interior solution.

Optimality condition We begin by deriving the optimality condition for the gang:

$$\begin{aligned} \max_{q_g} V &= (a - \beta q_g - \gamma \bar{q_s})q_g - cq_g + \rho(q_g, \bar{q_s})\pi \\ \frac{\partial V}{\partial q_g} &= a - 2\beta q_g - \gamma \bar{q_s} - c + \frac{\partial \rho(q_g, \bar{q_s})}{\partial q_g}\pi = 0 \\ q_g^* &= \frac{a - c + \frac{\partial \rho(q_g^*, \bar{q_s})}{\partial q_g}\pi}{2\beta} - \frac{\gamma}{2\beta}\bar{q_s} \end{aligned}$$

Note that without extra assumptions about the functional form of $\rho(q_g^*, \bar{q_s})$, the last result is not a best response function, as we have q_g^* as a function of itself. Still, it is an optimality condition.

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

$$G(q_g, \bar{q_s}) \equiv \frac{\partial V}{\partial q_g} = a - 2\beta q_g - \gamma \bar{q_s} - c + \frac{\partial \rho(q_g, \bar{q_s})}{\partial q_g}\pi$$

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

$$G(q_g^*, \bar{q_s}) = a - 2\beta q_g^* - \gamma \bar{q_s} - c + \frac{\partial \rho(q_g^*, \bar{q_s})}{\partial q_g}\pi = 0$$

Additionally, we assume that $2\beta \neq \frac{\partial^2 \rho(q_g^*, \bar{q_s})}{\partial q_g \partial q_g}\pi$, thus:

$$\frac{\partial G(q_g^*, \bar{q_s})}{\partial q_g} = -2\beta + \frac{\partial^2 \rho(q_g^*, \bar{q_s})}{\partial q_g \partial q_g}\pi \neq 0$$

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

$$\frac{\partial q_g^*}{\partial \bar{q_s}} = -\frac{\partial G(q_g, \bar{q_s})/\partial \bar{q_s}}{\partial G(q_g, \bar{q_s})/\partial q_g} = \frac{\lambda\pi - \gamma}{2\beta - \delta\pi}$$

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

To the extent that there are decreasing returns to governing, the denominator $2\beta - \delta\pi > 0$. In that case, the sign of $\frac{\partial q_g^*}{\partial \bar{q_s}}$ hinges on whether $\lambda\pi$ is greater than or less than γ. This in turn hinges on the sign of λ. In the next section, we show that for many common formulations of $\rho(\cdot)$, $\lambda > 0$ when the gang is the dominant provider of security in a neighborhood.

B.2.2 Illustration using a classic conflict success function

As noted in the main text, $\rho(\cdot)$ is naturally interpreted as a Contest Success Function (CSF)—a device commonly used in models of tournaments, rent-seeking, and conflict to capture the idea that the expected probability of victory is a function of relative effort.

One of the earliest and simplest CSFs, from Tullock (1980), takes the form $\rho(e_i, e_j) = \frac{e_i}{e_i + e_j}$, where e is the level of effort expended by parties i and j. While there are many dimensions to state and gang investment and effort in a neighborhood, our model simplifies this to the quantity of governance services offered, $\rho(q_g, \bar{q_s}) = \frac{q_g}{q_g + \bar{q_s}}$.

Like many CSFs, this classic one implies that $\frac{\partial q_g^*}{\partial \bar{q_s}}$ has the intuitive properties described above: the returns to governance are decreasing ($\delta < 0$), and the cross-partial derivative between gang and state governance (λ) hinges on relative effort by the gang versus the state.

Specifically,

$$\delta = \frac{\partial^2 \rho(q_g, \bar{q_s})}{\partial q_g \partial q_g} = -2\frac{\bar{q_s}}{(q_g + \bar{q_s})^3} < 0$$

and,

$$\lambda = \frac{\partial^2 \rho(q_g, \bar{q_s})}{\partial q_g \partial \bar{q_s}} = \frac{q_g - \bar{q_s}}{(q_g + \bar{q_s})^3}.$$

Thus, λ is positive when gang effort (proxied by quantity of governance) exceeds that of the state. Likewise, gang and state rule are more likely to be complements when $\lambda\pi > \gamma$ —

that is, when state effort is exogenously low and potential drug rents are large (relative to the substitutability of the services between both organizations).

B.2.3 More general conflict success functions

The same holds true for a range of common CSFs. Consider the function $\rho(e_i, e_j) = \frac{f(e_i)}{f(e_i)+f(e_j)}$, where $f(\cdot)$ is increasing and non-negative. We know from the previous section that only the cross-partial of the contest function matters. The second order condition is:

$$\frac{\partial^2 \rho(e_i, e_j)}{\partial e_i \partial e_j} = \frac{f'(e_1)f'(e_2)(f(e_1) - f(e_2)))}{(f(e_1) + f(e_2))^3}$$

The necessary and sufficient condition for having a positive cross-partial is $f(e_1) > f(e_2)$. As the function is increasing, this is equivalent to the condition $e_1 > e_2$. This would include the particular case $f(x) = e^x$.

B.3 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."

Setup As before, except we now generalize the functional form of demand such that products are differentiated so the price of the gang's product each is determined by $p_g = a(q_g, \bar{q_s}) - \beta q_g - \gamma \bar{q_s}$, where $a(q_g, \bar{q_s})$ is twice continuously differentiable. The payoff function of the gang is $V = (a(q_g, \bar{q_s}) - \beta q_g - \gamma \bar{q_s})q_g - cq_g + \rho(q_g, \bar{q_s})\pi$, where $\rho(q_g, \bar{q_s})\pi$ captures the externalities described above in section B.2. Again, we assume an interior solution.

Optimality condition As above, we begin by deriving the optimality condition for the gang:

$$\begin{aligned}
\max_{q_g} V &= (a(q_g, \bar{q_s}) - \beta q_g - \gamma \bar{q_s})q_g - cq_g + \rho(q_g, \bar{q_s})\pi \\
\frac{\partial V}{\partial q_g} &= a(q_g, \bar{q_s}) + \frac{\partial a(q_g, \bar{q_s})}{\partial q_g} q_g - 2\beta q_g - \gamma \bar{q_s} - c + \frac{\partial \rho(q_g, \bar{q_s})}{\partial q_g}\pi = 0 \\
q_g^* &= \frac{a(q_g^*, \bar{q_s}) - c + \frac{\partial \rho(q_g^*, \bar{q_s})}{\partial q_g^*}\pi}{2\beta - \frac{\partial a(q_g^*, \bar{q_s})}{\partial q_g^*}} - \frac{\gamma}{2\beta - \frac{\partial a(q_g^*, \bar{q_s})}{\partial q_g^*}} \bar{q_s}
\end{aligned}$$

Comparative Statics As before, to obtain the key comparative static, we define:

$$G(q_g, \bar{q_s}) \equiv \frac{\partial V}{\partial q_g} = a(q_g, \bar{q_s}) + \frac{\partial a(q_g, \bar{q_s})}{\partial q_g} q_g - 2\beta q_g - \gamma \bar{q_s} - c + \frac{\partial \rho(q_g, \bar{q_s})}{\partial q_g} \pi$$

which is a continuously differentiable function from $\mathbb{R}^2 \to \mathbb{R}$. At the optimum, we know that $G(q_g^*, \bar{q_s}^*) = 0$. We also assume that $\frac{\partial G(q_g^*, \bar{q_s}^*)}{\partial q_g} \neq 0$. Then, we can use the implicit function theorem to obtain our main comparative static in the formulation with endogenous demand:

$$\frac{\partial q_g}{\partial \bar{q_s}} = -\frac{\partial G(q_g, \bar{q_s})/\partial \bar{q_s}}{\partial G(q_g, \bar{q_s})/\partial q_g} = -\frac{\frac{\partial a(q_g, \bar{q_s})}{\partial \bar{q_s}} + \frac{\partial^2 a(q_g, \bar{q_s})}{\partial q_g \partial \bar{q_s}} q_g - \gamma + \lambda \pi}{2 * \frac{\partial a(q_g, \bar{q_s})}{\partial q_g} + \frac{\partial^2 a(q_g, \bar{q_s})}{\partial q_g \partial q_g} q_g - 2\beta + \delta \pi}$$

Now, in addition to our previous conditions ($\delta < 0$ and $\lambda > 0$) a necessary condition for gang and state governance to be complements is that $a(\cdot)$ is downward sloping on the product $\left(\frac{\partial a(q_g, \bar{q_s})}{\partial q_g} < 0\right)$ and that the decrease is at decreasing rates $\left(\frac{\partial^2 a(q_g, \bar{q_s})}{\partial q_g \partial q_g} < 0\right)$.

B.4 General formulation and alternative models

Here we abstract away from the example of Cournot (or Bertrand) competition. Instead of modeling competition with one model or the other, we could use a general form $D(q_g, \bar{q_s})$ that encompasses all of these models (including Bertrand). Likewise, instead of modeling the externality as $\rho(q_g, \bar{q_s})\pi$ we use a general form $F(q_g, \bar{q_s})$.

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

$$V = D(q_g, \bar{q_s}) + F(q_g, \bar{q_s})$$

Then we can define the first partial in q_g as:

$$G(q_g, \bar{q_s}) \equiv \frac{\partial V}{\partial q_g} \quad = \quad \frac{\partial D(q_g, \bar{q_s})}{\partial q_g} + \frac{\partial F(q_g, \bar{q_s})}{\partial q_g}$$

which is a continuously differentiable function from $\mathbb{R}^2 \to \mathbb{R}$. As a technical note, we assume that there exists a point such that $\frac{\partial V}{\partial q_g} = 0$, 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 $G(q_g^*, \bar{q_s}) = 0$. We also assume that $\frac{\partial G(q_g^*, \bar{q_s})}{\partial q_g} \neq 0$.

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 $(q_g^*, \bar{q_s})$ such that when q_g is close enough to q_g^*, we have a unique $\bar{q_s}$ such that $G(q_g, \bar{q_s}) = 0$. This makes $\bar{q_s}$ a continuous function of q_g. The comparative static is:

$$\frac{\partial q_g}{\partial \bar{q_s}} = -\frac{\partial G(q_g, \bar{q_s})/\partial \bar{q_s}}{\partial G(q_g, \bar{q_s})/\partial q_g} = -\frac{\frac{\partial^2 D(q_g, \bar{q_s})}{\partial q_g \partial \bar{q_s}} + \frac{\partial^2 F(q_g, \bar{q_s})}{\partial q_g \partial \bar{q_s}}}{\frac{\partial^2 D(q_g, \bar{q_s})}{\partial q_g \partial q_g} + \frac{\partial^2 F(q_g, \bar{q_s})}{\partial q_g \partial q_g}}$$

Again, in the case of a positive denominator (decreasing returns to production in loyalty and profit) a positive numerator is sufficient for a positive cross partial.

C Predicted drug profitability and heterogeneity

To develop an exogenous proxy for future drug profitability, we use baseline data to predict lucrative present-day drug markets. For each block pair, we calculated the average distance to their 10 closest drug markets. We then trained a lasso model to predict a block pair's drug market proximity.

We use the full sample of all blocks in the 2019 city survey (not just those within a 300 meter bandwidth of the border). We focus on predicting major drug, permanent drug markets rather than smaller more mobile and largely unmapped minor drug corners.

The pool of predictors include all baseline variables in Table 2 as well as the distance to neighborhoods in each income strata. We use a lasso model to facilitate interpretation of the predictors, but alternative methods produce similar results.

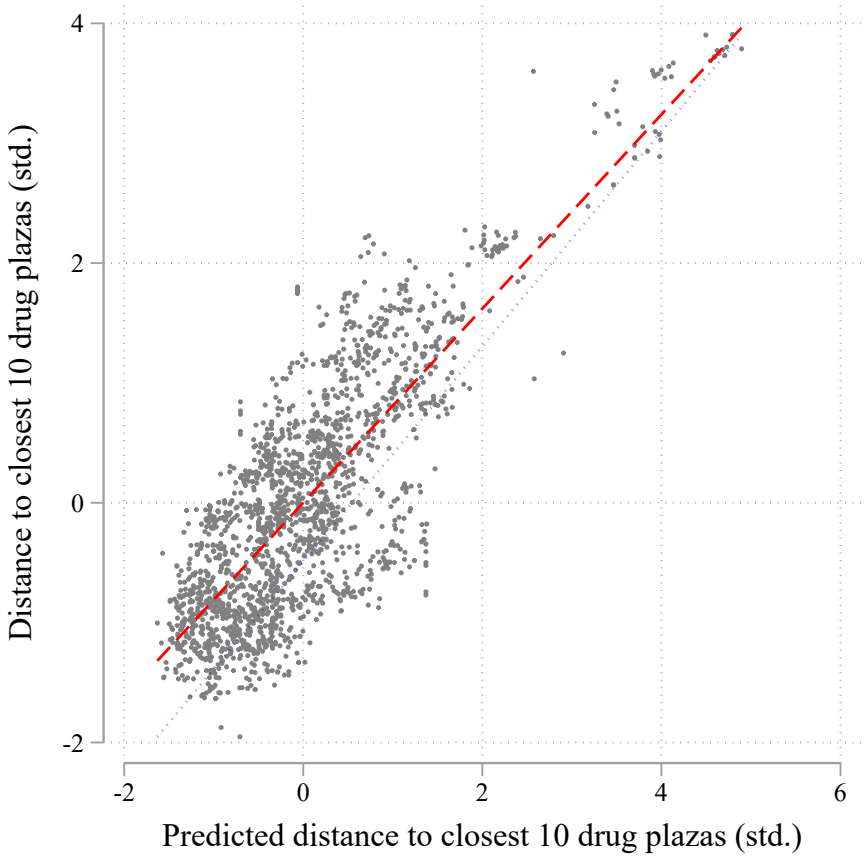
We chose our specification by minimizing the prediction error via cross-validation, but the model selects most of the variables in each case. The predictions and the heterogeneity results are highly robust to including or excluding other categories of variables, including distances to the 1–10 closest police stations and municipal protection services.

Appendix Table C.1 reports lasso coefficients. Most baseline variables are selected by the model, with proximity to middle and high-income neighborhoods being among the strongest predictors.

Figure C.1 shows that predicted proximity is strongly associated with actual post-treatment proximity. Appendix Figure C.2 shows that predicted values are correlated with other indicators of drug activity, such as the value of drug seizures 2014–19. To calculate π_{ij}, we average the predicted value for the block pair.

Note that results are virtually identical if we predict any number of nearest drug markets, from 1 to 20, or if we expand the pool of baseline predictors to include closest proximity to state protection services (not shown.

Figure C.1: Relationship between distance to drug corners and its predicted value



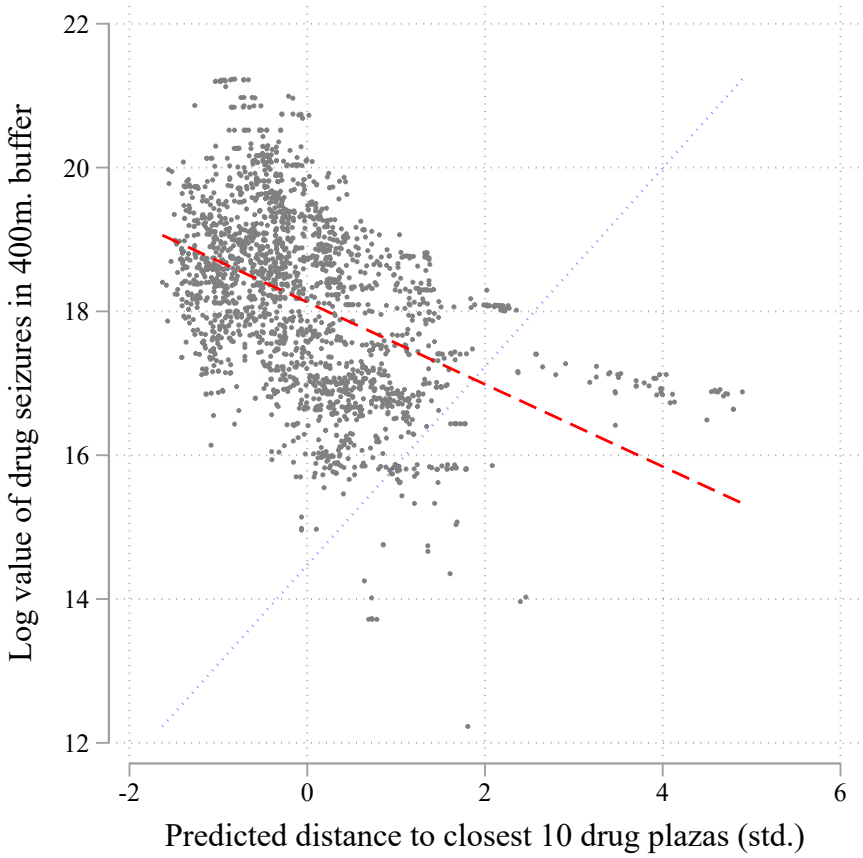
Notes: The sample includes all blocks in the 2019 city survey, including those more than 300 meters from a new border. We plot average distance to the 10 nearest major drug corners (the vertical axis) against predicted values from the lasso regression reported in Appendix Table C.1. Each dot is a block, and the dashed line indicates fitted values (correlation = 0.81).

Table C.1: Baseline predictors of proximity to drug markets

Standarized index	Lasso Coefficients
Total women (1993)	-0.090
Median age (1993)	-0.046
Total population no education (1993)	0.030
Share of population with primary education (1993)	-0.108
Share of population with secondary education (1993)	-0.021
Share of population with higher educ. (1993)	0.007
Log of total population (1993)	-0.033
Total non-mestizo polulation (1993)	-0.003
Share of women (1993)	0.069
Share of non-mestizo polulation (1993)	-0.021
Share of population no education (1993)	-0.066
Median number of room (1993)	0.019
Distance to neighborhood of income level 1	-0.019
Distance to neighborhood of income level 2	-0.175
Distance to neighborhood of income level 3	0.435
Distance to neighborhood of income level 4	1.106
Distance to neighborhood of income level 5	2.846
Distance to neighborhood of income level 6	-0.003
Distance to neighborhood of income level 4, 5 or 6	-1.082
Distance to neighborhood of income level 5 or 6	-3.100
Distance to education centers (mean in comuna)	-0.124
Distance to local business centers (mean in comuna)	-0.019
Distance to transport (subway stations)	0.032
Meters to health centers	0.248
Meters to schools	0.096
Constant	0.004

Notes: The sample includes all blocks in the 2019 city survey, including those more than 300 meters from a new border. The dependent variable is a block's average distance to the 10 nearest drug corners in the mid-2000s. We estimate a lasso regression using all baseline variables in Table 2 as well as the distance to neighborhoods in each income strata. For cross validation, we employed a 10-fold approach. This means that a random tenth of the sample was left aside to test the model trained on the rest of the data. This process allows us to compute a cross-validation error per each fold, which is then averaged to obtain a cross-validation error measure for the model.

Figure C.2: Relationship between predicted distance to drug corners and drug seizures



Notes: The sample includes all blocks in the 2019 city survey, including those more than 300 meters from a new border. We plot the logged value of all drug seizures from 2014–19 in a 400 meter radius of each block (the vertical axis) against predicted values of drug market proximity from the lasso regression reported in Appendix Table C.1. Each dot is a block, and the dashed line indicates fitted values (correlation = -0.53).

Table C.2: Heterogeneity: Impacts on being 100m closer to the state on neighborhood development, by predicted drug profitability

	Economic Development Index	
	Admin. outcomes	Survey outcomes
	Estimate (SE) [p-value]	Estimate (SE) [p-value]
	(2)	(3)
Effect of moving 100m closer to state	0.063	-0.023
	(0.040)	(0.047)
	[0.125]	[0.633]
100m closer to state X Predicted drug profitability (std.)	-0.025	0.067***
	(0.025)	(0.021)
	[0.330]	[0.003]
Predicted drug profitability (std.)	-0.037	0.219*
	(0.103)	(0.115)
	[0.719]	[0.066]
Controls and fixed point running var	✓	✓
Bi-directional Border FE	✓	✓
N	471	228

Notes: This table estimates Equation 6, regressing differences in our two main economic development indexes on the distance shock, a standardized measure of distance from the earliest drug plazas in Medellín (which emerged in the mid-2000s), and an interaction between the two. As a result, the coefficient on the distance shock estimates the treatment effect in neighborhoods at the average difference from drug plazas, and the coefficient on the interaction tells us how the impacts of the distance shock are different in neighborhoods far from early drug corners. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets.

D Robustness and sensitivity analysis

D.1 Robustness to alternative estimation strategies

Generally, all results are robust to alternative estimation approaches. Appendix Table D.1 illustrates. Dropping controls have little effect on point estimates (consistent with the results of our balance tests), and the same is true of using fewer border fixed effects (one per border). Removing clustering of standard errors increases precision, as expected. We see similar results if we match blocks to the closest block across any border (rather than the closest block across the closest border), and if we use a 200-meter bandwidth instead of 300-meter along borders. Finally, one might worry that, on blocks closest to the border, survey data are contaminated by circumstances on the other side of the border—a potential violation of the independence assumption. Dropping the 25% of blocks closest to the order does not change our results.

D.2 Placebo borders

Are there other unobserved block characteristics that are associated both with differences in proximity to historical state presence and to motives for combo governance? Our border discontinuity should reduce the likelihood of these confounders. In addition, they would need to have a stronger relationship with both combo and state governance than our observed confounders (such as the availability of other state services, or the distance to business agglomerations). This is possible. For example, some borders might not have been arbitrarily drawn—although our anecdotal evidence on the process suggests otherwise. We address this by conducting a placebo exercise. We randomly matched 1,500 times our baseline sample of blocks located within 300 meters of comuna borders with other blocks inside the same comuna, ensuring that the matched blocks are at most 600 meters away from each other—so that we resemble our baseline specification. To build our treatment variable, we assign one of the block pairs to state services within the comuna and the other to services in the neighboring comuna (depending on the distance to average services for each). The distribution of treatment effects for combo governance and legitimacy is reported in Figure D.1. Our observed treatment effects lie at the edge of the distribution, suggesting they are unlikely to be explained by other confounders.

D.3 Measurement error correlated with treatment

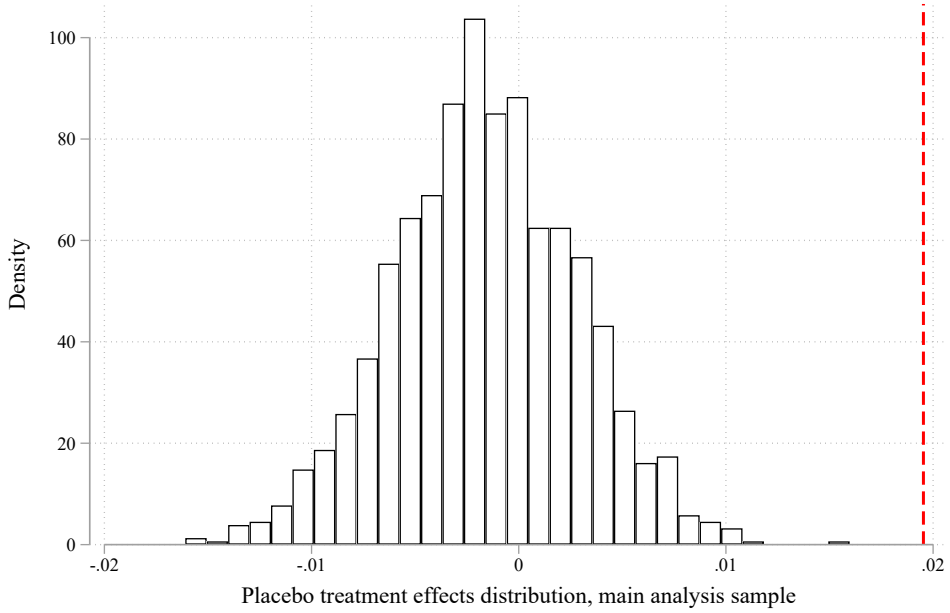
Finally, we consider different kinds of measurement error correlated with treatment and judge that these are unlikely to account for the large crowding in effects we observe. We first

Table D.1: Robustness of impacts on state and combo rule of being 100 meters closer to the local state

	Median Δd_{ij}	Δ State Governance	Δ Combo Governance	Δ State Legitimacy	Δ Combo Legitimacy
	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)
Main specification	4.128	0.011	0.019**	0.001	0.005
		(0.007)	(0.007)	(0.008)	(0.014)
		[0.186]	[0.027]	[0.883]	[0.749]
Drop all control variables	4.128	0.013	0.020**	-0.001	0.008
		(0.008)	(0.007)	(0.006)	(0.013)
		[0.152]	[0.017]	[0.887]	[0.594]
Remove clustering	4.128	0.011*	0.019***	0.001	0.005
		(0.006)	(0.007)	(0.005)	(0.008)
		[0.064]	[0.003]	[0.805]	[0.607]
13 Border fixed effects	4.128	0.011	0.014**	0.007	0.009
		(0.006)	(0.006)	(0.006)	(0.010)
		[0.229]	[0.026]	[0.276]	[0.396]
Matching to nearest block over any new border	4.069	0.015	0.013	0.001	0.004
		(0.007)	(0.007)	(0.006)	(0.012)
		[0.115]	[0.133]	[0.869]	[0.754]
200m bandwith to border	4.019	0.011	0.021**	0.006	0.010
		(0.008)	(0.009)	(0.008)	(0.016)
		[0.276]	[0.037]	[0.480]	[0.631]
Drop the 25% of blocks closest to new border	4.069	0.011	0.024**	0.013**	0.008
		(0.007)	(0.008)	(0.005)	(0.011)
		[0.168]	[0.023]	[0.040]	[0.558]

Notes: This table compares alternative specifications, changing one feature of the model at a time. Each row is a different estimation of treatment effects. The unit of analysis is a pair of matched blocks on either side of a new border. The dependent variable is the difference in governance or legitimacy between the block pair. Each estimate in Column 2 comes from a regression of the dependent variable on the distance shock, controlling for other baseline bock-pair differences, a running variable to the border, and border fixed effects. We report clustered standard errors in parentheses and wild bootstrap p-values in brackets. Only residents (not business respondents) were asked about legitimacy, and some blocks have only residents, hence the lower sample size.

Figure D.1: Placebo treatment effects, inner border, main analysis sample, 1,500 simulations.



Notes: The figure depicts the distribution of average treatment effects of the difference in distance to the state on the difference in combo governance. Rather than matching pairs of blocks across the border, as in our main specification, we randomly matched our baseline sample of blocks with other blocks within the same comuna, using 1,500 simulations. We take blocks within 300 meters of the border and match them with blocks within a distance of at most 600 meters, resembling our baseline specification. The vertical line indicates our observed treatment effect.

illustrate this through survey experiments, then consider various models of measurement error.

Survey experiments

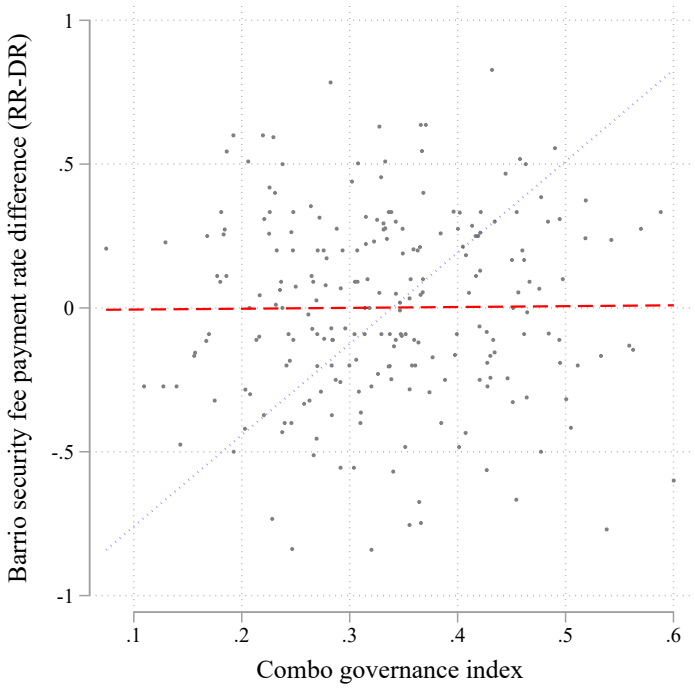
We tested for measurement error (and correlation with treatment) using a survey experiment. We took one of the variables we thought could be the most sensitive—whether people paid "taxes" to the combo in the form of security fees and extortion payments. In our city-wide survey of thousands of residents, we randomly assigned respondents to either a direct question on whether they paid the combo, versus 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.[27] With the whole sample, randomized response (RR) elicited an extortion rate of 22.6% from businesses and 6% from households, compared to 19.4% and 7.8% with direct responses (DR). The differences RR–DR run in opposite directions for households and businesses, and are not statistically significant.

Appendix Figure D.2 plots block averages for this RR–DR difference on block combo governance in the full city sample, and finds little systematic correlation. Appendix Table D.2 reports summary statistics within our sample of blocks close to new borders, and examines correlates between differential reporting and our treatment variable. On average, the direct responses on vacunas are 6 percentage points lower than the randomized response (RR), as seen in Column 1 (first row). So there is evidence of slight under-reporting in this subsample, even if it is not statistically significant. There is no statistically significant correlation, however, between the RR–DR pair-block difference and the difference in distance to state headquarters. As the state grows closer, direct reporting decreases somewhat relative to randomized response. But this is small and imprecise. Furthermore, if under-reporting is larger as the state grows closer, it is likely that our main estimates are a lower bound of the actual treatment effect.

We also investigate whether patterns of non-response are correlated with treatment. For instance, people might decide to skip combo governance questions if they are uncomfortable. As we show in Table D.2 (rows 2 and 3), respondents answered about 85% of combo governance and legitimacy questions, compared to 90% for the state, again consistent with slight under-reporting of combo governance (which, as we noted, would act to understate crowding in). People are somewhat less likely to answer these questions the closer they are

[27] Others were asked the same question using a List Experiment, where half are asked to give the number of four nonsensitive actions they engaged in, and half see a list of five actions, including paying extortion. In general, these list experiments are extremely noisy. Yet, the results are consistent with what we see in these randomized response and direct response questions. There is no evidence of systematic measurement error.

Figure D.2: Survey experiment results: Difference between randomized response (RR) and direct response (DR) to security fee payment rate



Notes: Each dot represents a barrio average. The dotted line indicates fitted values. We did not survey high-income barrios.

to the state, however—the opposite of the direction we are worried about. If the proportion of questions indeed proxy for under-reporting, this pattern again implies our findings are more likely a lower bound of the actual treatment effect.

Modeling measurement error

What's more, few forms of measurement error will bias our estimate of combo governance upwards. For instance, if people tend to understate gang rule in general, we will tend to underestimate crowding in. In only one scenario will we overestimate crowding in: if citizens under-report gang governance when gang governance is high and the state is far away, but do not misreport when the government is close.

To demonstrate this, we focus on three types of measurement error: reporting endogenous to gang rule, reporting endogenous to relative state governance, and reporting endogenous to both gang and state governance separately. In each case, we study how the reporting error changes the coefficient we estimate in the main results of the paper, and discuss the direction of the bias it induces.

Table D.2: Test of systematic measurement error: Coefficient of closeness to state services for blocks along the inner comuna borders of Medellín on measurement error proxies

Effect on ΔY	Subsample Mean (SD)	Effect of moving 100m closer to state: Estimate (SE)
	(1)	(2)
Extortion payment rate difference (RR-DR)	-0.06	-0.099**
	(0.82)	(0.042)
Proportion of questions answered for state	0.90	-0.003
	(0.13)	(0.003)
Proportion of questions answered for combo	0.85	-0.013
	(0.18)	(0.008)
Number of observations		566
Minimum N		248

Notes: This table examines the correlation between proxies for measurement error and being 100 meters more distant from the state, using the same estimation for our main treatment effects. The extortion rate difference computes the difference between randomized response and direct response to the question of whether the household pays extortion. 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.

Reporting endogenous to gang rule Suppose the true relationship between combo and state governance is given by:

$$g_c^* = \alpha + \beta g_s^* + \epsilon \tag{7}$$

However, suppose g_c^* is systematically under-reported in the survey at a rate proportional to combo governance $0 < \delta < 1$:

$$g_c = \delta g_c^* + \mu \tag{8}$$

Then:

$$\frac{g_c - \mu}{\delta} = \alpha + \beta g_s^* + \epsilon \tag{9}$$

and hence we would estimate:

$$g_c = \delta\alpha + \delta\beta g_s^* + \nu \tag{10}$$

where $\nu = \delta\epsilon + \mu$. Using observed data we will estimate $\hat{\delta}\hat{\beta} < \beta$, which means that we underestimate the impact of state governance on gang governance.

Reporting endogenous to relative state governance Now let's continue to the same true relationship between g_c^* and g_c^*, but now under-reporting depends on relative state/combo governance:

$$g_c = \lambda(g_c^* - g_s^*) + \mu \tag{11}$$

Where $0 < \lambda < 1$ is the reporting rate of relative state governance. Then:

$$g_c^* = \frac{1}{\lambda} g_c + g_s^* - \frac{\mu}{\lambda} \tag{12}$$

and hence we would estimate:

$$g_c = \lambda\alpha + \lambda(\beta - 1) g_s^* + \eta \tag{13}$$

Where $\eta = \lambda\epsilon + \mu$. Using observed data we will estimate $\hat{\lambda}(\hat{\beta} - 1) < \beta$, which means that

we, again, underestimate the impact of state governance on gang governance.

Reporting endogenous to gang and state governance, separately Now suppose reporting rates can vary by absolute levels of gang and state goevrnance:

$$g_c = \delta g_c^* + \lambda g_s^* + \mu \tag{14}$$

Where $0 < \delta < 1$ and λ could be greater than 1. Then:

$$g_c^* = \frac{1}{\delta}(g_c + \lambda g_s^* - \mu) \tag{15}$$

and we will estimate:

$$g_c = \delta\alpha + (\delta\beta + \lambda)g_s^* + \mu + \delta\epsilon \tag{16}$$

If there is a bias toward under-reporting combo governance overall, and also a willingness to report increases with state proximity, this could bias the results towards complementarity between state and gangs.

Other correlates of combo governance

The modeling above suggests that the cleanest test of systematic measurement error is our survey experiment and the missingness analysis. Nonetheless, we can also illustrate that combo governance has many of the expected correlations with other gang-related variables. Table D.3 reports bivariate correlations. We see evidence of higher levels of gang governance as blocks grow closer to combo headquarters, closer to razón headquarters, and where there are denser collections of combos (thus potentially more rivalry). We also see more combo governance in areas with greater levels of drug seizures and poorer neighborhoods. Arguably, if we were worried that gang presence leads people to underreport gang rule, we'd expect the opposite correlation with closeness to headquarters and combo density.

What about administrative police or judicial data? Unfortunately these do not indicate gang-related crimes. It might be possible to obtain data on leader arrests. But such arrest data would be problematic because more state presence (the treatment) might result in more arrests mechanically. Moreover, if anything, we might expect fewer senior arrests in the well-ordered neighborhoods where gangs are disciplined and governed, resulting in a biased test (maybe even the opposite sign). We would have simiklar problems with reported crime and police call data. This is a common problem with administrative data when it comes to organized crime: it is potentially less complete and more biased than survey data not only

because of its clandestine nature, but also because arrests and other police data are the product of strategic decisions by the gangs and the state.

Table D.3: Correlates of combo governance

		Combo Governance						
	Sample Mean (SD)	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]	Estimate (SE) [p-value]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log of drug seizure value	18.135	0.059***						
	(1.433)	(0.022)						
		[0.008]						
Distance from gang HQ	2.543		-0.153***					
	(2.059)		(0.022)					
			[0.000]					
Count of combo groups	0.351			0.093***				
	(0.563)			(0.022)				
				[0.000]				
Distance to razón HQ	14.928				-0.118***			
	(9.648)				(0.025)			
					[0.000]			
Block average elevation	1,613.698					0.119***		
	(132.844)					(0.022)		
						[0.000]		
Poverty Index (2018)	14.413						0.151***	
	(15.641)						(0.022)	
							[0.000]	
Distance to state	1,319.409							0.009
	(937.994)							(0.022)
								[0.697]
N		2,014	1,996	2,014	1,499	2,014	2,014	2,004

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 block level and using standardized measures. 143 missing values were imputed for the poverty index.