Civilian alternatives to policing: Evidence from Medellín's community problem-solving intervention *Operación Convivencia*[*]

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December 12, 2023

Abstract

We experimentally evaluate a community-level intervention designed to improve security by: increasing civilian state presence on the street, empowering community organizations to solve conflicts, and raising trust and cooperation with the state (versus local gangs). In 40 of 80 neighborhoods, Medellín's city government dramatically intensified normal governance services. After 20 months, there was no average impact on its legitimacy or local security. A prespecified analysis shows important heterogeneity, however. In neighborhoods where the state began weak, the state underperformed and opinions worsened. In neighborhoods where the state started strong, the effort raised state legitimacy and reduced crime and emergency calls.

JEL codes: H11, K42, O17, N46, C93
Keywords: Local government, public safety, governance, policing, crime, state building, urban policy, public services, security, gangs, field experiment, Colombia

[*]For comments we thank Thomas Abt, Oriana Bandiera, Eli Berman, Robert Blair, Jennifer Doleac, Sara Heller, Max Kapustin, Zoë Gorman, Macartan Humphreys, Raul Sánchez de la Sierra, Jacob Shapiro, Carlos Schmidt-Padilla, Paolo Pinotti, Ernesto Schargrodsky, Maria Micaela Sviatschi, Juan Vargas, and participants at several seminars and conferences. Innovations for Poverty Action coordinated all research activities. For research assistance we thank Verónica Abril, Bruno Aravena, David Cerero, Peter Deffebach, Felipe Fajardo, Sebastián Hernández, Sofía Jaramillo, Juan F. Martínez, Juan Pablo Mesa-Mejía, Angie Mondragón, Helena Montoya, José Miguel Pascual, Andrés Preciado, Arantxa Rodríguez-Uribe, Zachary Tausanovitch, Nelson Matta-Colorado, Martín Vanegas-Arias and México Vergara. We thank the Secretariat of Security of Medellín for their cooperation, especially the former Secretary of Security Andrés Tobón, as well as Lina Calle and Ana María Corpas. 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 through 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

What can city governments do in neighborhoods with weak social institutions, persistent street crime, and entrenched local gangs and other armed actors? One approach is to intensify policing, which research suggests can sometimes reduce serious crime.[1] At the same time, excessive or violent policing can backfire, undermining police legitimacy and enhancing the reputation of nonstate actors (Acemoglu et al., 2020; Owens, 2019; Owens and Ba, 2021). With police use of force sparking protests worldwide, governments are understandably looking for alternatives and supplements to policing. This often takes the form of crisis-response: social workers and other specialists reacting to emergency calls alone or in concert with police—especially those involving mental health, addiction, homelessness, and domestic disputes.[2] Preventative alternatives are also growing. Some target the highest-risk individuals with intensive services like cognitive behavioral therapy and economic assistance (Heller et al., 2016; Blattman et al., 2017, 2023; Bhatt et al., 2023). Others are more indirect and broad-based, such as jobs or economic support programs for families and youth in high-crime neighborhoods (e.g., Davis and Heller, 2020; Hjalmarsson et al., 2015; Carr and Packham, 2019).[3]

This paper explores another broad-based, civilian-led, preventative approach: increasing the level and quality of everyday state and community governance. The idea is not only to prevent problems and disputes from escalating, but also to generate more trust and cooperation with the state (and less with other armed actors, such as gangs). This approach is common across Latin America; in Colombia it is known colloquially as *convivencia*, meaning coexistence. Typically, it involves putting non-police state representatives on the street. Partly, they are tasked with identifying community grievances and connecting

[1]In U.S. cities, more intensive policing tends to be associated with lower violent crime (Chalfin and McCrary, 2017, 2018). Blattman et al. (2021) find similar results in Bogotá, Colombia. That said, the record of crackdowns and *mano dura* (iron fist) policies is more mixed, especially in the presence of organized crime. In Mexico, crackdowns likely displaced violence to other states or destabilized relations between criminal groups (Dell, 2015). In Chicago, the mass arrest of gang leaders and the demolition of housing projects fragmented gangs and may have contributed to local, inter-group conflicts (Aspholm, 2019; Bruhn, 2018). And militarized policing in Cali, Colombia, had no effect on crime and may have increased human rights abuses (Blair and Weintraub, 2021). In Brazil, success varied according to whether organized criminal groups fostered order and loyalty (Magaloni et al., 2020).

[2]On social workers responding to nonviolent crises, the research is still relatively early, limited, and mixed (Irwin and Pearl, 2020; Seo et al., 2021; Dee and Pyne, 2022). There is more evidence on community-wide violence interruption, in which street outreach workers react to gun violence and threats with mediation and other services. This approach is promising but difficult to rigorously evaluate, and the studies so far show mixed results (Butts et al., 2015).

[3]There are many other preventative approaches. Some communities have turned to grassroots neighborhood monitoring to make walking to school safer (e.g., Gonzalez and Komisarow, 2020). Others seek to mitigate the harmful effects of the criminal justice system (Agan et al., 2023; Aizer and Doyle, 2015). A wide range of social programs also encourages desistance from crime (Doleac, 2023).

residents with state agencies that can solve these problems. Partly, they foster better communication between the community and state institutions and a better understanding of what the state can and cannot do. At the same time, these street-level bureaucrats are also trying to improve the community's ability to solve their own problems—strengthening local organizations, encouraging participation, training them in dispute resolution, and fostering community norms.

Convivencia parallels, in some ways, how U.S. cities manage everyday community problems. A mix of municipal agencies, social work organizations, and constituent-service offices address neighborhood issues, maintain public spaces, and resolve disputes. Civil society organizations and informal leaders also help manage disputes and other disorder. A handful of federal programs have sought to build up these state and community capacities, including the Building Neighborhood Capacity Program (BNCP) and the Neighborhood Revitalization Program (NRI), but they have received limited public and researcher attention.[4]

Convivencia aims to improve security through both direct and indirect channels. Together, state and community actors might reduce disorder and crime directly, by dissuading disorderly people from entering the neighborhood or by intervening in problems before matters escalate. At the same time, better public services in general should enhance trust in and cooperation with state institutions—including the police—and thus contribute to order indirectly as well. There is some precedent for this. For example, an information experiment in rural Pakistan found that people told about improvements in state courts said they were more willing to use them, and were more willing to make cash transfers to the state (Acemoglu et al., 2020).

There is little evidence, however, on the effects of actual improvements in "normal" everyday community governance. One experiment in Liberian towns tested a narrower approach, focused on improving community dispute resolution practices and norms through large-scale training. The program reduced low-scale violence such as threats, fights, and property destruction over at least three years (Blattman et al., 2014; Hartman et al., 2021). These were nonstate interventions, however, and so the effect on state institutions and legitimacy is unknown. It is also unclear whether these approaches can work in large cities, curb more serious violence and disorder, or promote police and state legitimacy.

To test this, we worked with the municipal government of Medellín, the *Alcaldía*, to increase the intensity of their *convivencia* activities in 40 low- and middle-income residential sectors. Medellín is Colombia's second-largest city, with a population of roughly 2.5 million.

[4] For information on the BNCP, see https://www.ojp.gov/ncjrs/virtual-library/abstracts/building-neighborhood-capacity-program-bncp-fact-sheet. For NRI, see https://obamawhitehouse.archives.gov/administration/eop/oua/initiatives/neighborhood-revitalization.

It is one of the nation's industrial and commercial centers, with an annual income of roughly $11,500 per capita in purchasing parity terms. It has a well-organized bureaucracy with high tax revenues and public services.

The intervention had three main components. First, the Alcaldía created a special task force to ensure that problems in the 40 treatment communities would get priority attention from the city's range of specialized agencies—including dispute resolution, family services, and neighborhood cleanup.

Second, the city committed to holding formal community–government meetings twice per year, called *Consejos de Convivencia*. The meetings brought community members into direct conversation with senior officials of the Alcaldía and the police. In principle, meetings concluded in a set of tasks and issues that the Alcaldía and other authorities were obligated to act upon. The city also committed to holding a *Caravana*, a weekend festival that brought representatives of every municipal agency into each neighborhood.

Third, the city hired 40 new full-time "liaisons"—young professionals whose job was to: strengthen the capacity of community organizations; foster norms and skills of community problem-solving; and, when necessary, marshal state services to directly solve problems. To do this, liaisons would advertise and link people to government agencies; identify public service needs for the task force to fix; and connect disputants to the city's professional mediators or family-services officials. Liaisons also organized the Consejos and the Caravana.

Operación Convivencia began in early 2018 and lasted 20 months—until the end of the mayor's term. It represented a massive increase in central and street-level state attention in treated sectors. It did not, however, change policing in the neighborhoods (although liaisons did try to improve community–police communication and understanding).

To evaluate the intervention, we and the city identified 80 eligible sectors and randomized treatment in matched pairs. They were broadly representative of the city. Some sectors already received moderate services from the Alcaldía, while others had seen relatively little state presence on the street. All sectors belonged to neighborhoods that each have an entrenched local gang, with varying degrees of rule and power, a topic we return to below.

We kept the sectors small: up to 10 city blocks with about 1,000–3,000 residents each. We kept the experiment this intensive not because this approach demands it. Rather, the city had a limited budget for new staff. A small sector size maximized treatment intensity and hence statistical power. An experiment of 80 units has obvious limitations in terms of precision and generalizability, but Medellín's willingness to experiment on this scale is still unusual in terms of its scale and rigor.

We prespecified two primary outcomes: *Relative state legitimacy* and *Relative state governance* in the area of security and order. To measure these, we surveyed roughly 2,400 people in the experimental neighborhoods. We also collected administrative data on *Security-related emergency calls* and *Reported crimes.*

The "relative" nature of our primary outcomes is important here because the state in Medellín competes with illegal groups as local governors. Like many Latin American and U.S. cities, Medellín has entrenched street gangs. Virtually all low- and middle-income neighborhoods have a well-organized local group called a *combo*, which engages in a variety of illicit businesses, especially local drug sales. In addition, many combos provide some degree of local security and dispute settlement. The state remains the dominant provider of security, but in practice there is a duopoly of governance and coercion. Such "criminal governance" is common in Latin America, but gangs also rule civilians in Italy, the United Kingdom, India, Central and Southern Africa, and the American prison system (Arias, 2006; Lessing et al., 2019; Lessing, 2020; Melnikov et al., 2020; Sánchez De La Sierra, 2020; Brown et al., 2023).[5] Thus, the Alcadía's motive for governing better was not only to reduce disorder and crime, but also raise its legitimacy and stature relative to a competing actor.

Despite the intensity of the intervention, however, it was largely unsuccessful—at least on average. After 20 months, we see no evidence that dramatically intensifying state attention raised trust and satisfaction with the state, or improved perceptions of state responsiveness to disorder and insecurity. If anything, residents in treated neighborhoods reported a small, nonsignificant *decline* in relative state governance. Furthermore, despite high levels of street presence by the liaisons, we see only weak evidence that the average resident noticed the increased municipal attention, and no evidence that they participated actively.

This null finding, if true, would be an important result. It would imply that dramatically increasing city agency attention and street-level bureaucrats had no effect on trust in the government, and no direct or indirect effect on order or security—even in a city with a reputation for one of the strongest and well-funded municipal governments in Latin America.

That conclusion, however, may be premature. A closer look reveals important heterogeneity. Anticipating that program effects might depend on initial conditions, we prespecified that we would break down impacts by a single baseline measure: initial relative state governance. Doing so reveals stark differences in people's awareness of the municipal staff and their participation.

In treated neighborhoods where the state began relatively strong, residents report sig-

[5] States, even strong ones, often face internal competitors. Traditional leaders, influential persons, and community organizations also regulate everyday life. These groups don't necessarily undermine the state, and are often complementary (Cammett and MacLean, 2014; Van der Windt et al., 2019; Blattman et al., 2014; Henn, 2021). When it comes to public security and justice, however, state legitimacy can suffer when other coercive actors—criminals, paramilitaries, or insurgents—govern the population (Berman and Laitin, 2008; Acemoglu et al., 2020; Cammett and MacLean, 2014). This is one reason why states aim to monopolize the legitimate use of force (Weber, 1946).

nificantly more interactions with city staff. Where the state began initially weak, residents report fewer interactions. The difference is large—27 percent of the average level of engagement with municipal staff and events in the city.

Post-program interviews with community leaders and the liaisons provide some clues why. Where the state had capacity and presence, the task force and liaisons were able to execute the intervention effectively. Elsewhere, the central task force or other officials were sometimes unable to deliver on important promises. The intervention may have raised expectations of what the task force and municipal apparatus could actually deliver, and so failures to follow through were doubly disappointing.

We see divergent impacts on governance and legitimacy as well. In initially well-governed sectors, the program increased state legitimacy by almost 10 percent, decreased reported crimes by 40 percent, and reduced emergency calls about fights and public disorder by 55 percent. Not all indicators improve; the program had no effect on residents' reports of the responsiveness of the police and Alcaldía to disorder. Nonetheless, the improvements in state legitimacy, crime, and security calls are massive. We construct a family index of all four outcomes, and find that the heterogeneous results discussed above are highly statistically significant.

We see little evidence that this heterogeneity is due to gang reactions to the intervention. We see no change in gang legitimacy and governance in any analysis. This suggests that the impacts above are driven mainly by state capacities and actions alone. This is consistent with our qualitative observations. Combos commonly watched liaisons closely at first. In a handful of cases, gangs impeded liaison activities for the first few weeks, until they realized the bureaucrats were benign. For most of the intervention, we and the liaisons observed no gang reaction at all. Generally speaking, the combos appear more concerned with police forces. In a companion paper, we find that long-term increases in policing trigger strategic gang responses, mainly to protect drug rents (Blattman et al., 2023a).

Altogether, these findings speak to a central question facing governments: what are the returns to investments in governance capabilities, and how do they depend on initial capacity and legitimacy? Theoretically, the answer is ambiguous. On the one hand, in areas with little history of state services, we might expect the first investments to have outsized impacts on legitimacy and security. (This was our initial hypothesis in Medellín, where residents of the least-served areas initially expressed relief at finally seeing municipal bureaucrats in their neighborhoods.)[6] On the other hand, establishing robust state governance and legitimacy

[6]This hypothesis finds support in recent literature suggesting that the returns to government investments in fostering political participation might be highest in places where the state is weakest, following tax collection efforts (Weigel, 2020).

might require large and sustained investments, especially from a low starting point. Our results are consistent with the latter hypothesis.

We should be careful not to generalize from a single experiment in 80 small residential sectors, especially when the results rely on subgroup analysis (even if prespecified). Still, Medellín's willingness to experiment is a rare "proof of concept" that it is possible to randomize community-level interventions, including ones improving state capacity. To the best of our knowledge, this is the first randomized evaluation of a community-level civilian security intervention.[7]

At the same time, our results suggest that experimental samples should not necessarily be limited to the most disorderly communities and "hot spots." Crime and disorder are often concentrated in particular places, and most cost-effective security interventions are highly targeted (e.g., Weisburd et al., 2012; Abt, 2019; Blattman et al., 2021; Collazos et al., 2021). But program effectiveness may vary by baseline state and community capacity, and so diverse samples may be important to understanding what interventions work and why.

2 Context

2.1 The state and security in Medellín

Medellín's police force has roughly 2.7 officers per 1,000 people. This level is slightly higher than the U.S. national average, and comparable to U.S. cities like Los Angeles. It is lower, however, than other large U.S. cities, such as New York and Chicago, which have ratios above 4.[8] While low-level corruption and poor responsiveness are common, the Colombian police are fairly professionalized, particularly in comparison with other Latin American countries.

The city is divided into 16 *comunas* and, except for a couple of cases, each comuna is a separate police jurisdiction with its own commander and station. Each police jurisdiction is divided into a large number of *cuadrantes* (quadrants). Each quadrant has 6 assigned officers who patrol on motorbikes, in pairs, in 3 shifts per day.

In Colombia, however, the police are a national institution—a branch of the Defense Ministry. Although the constitution designates mayors as local police authorities, this only gives them influence over tactics and broad policy. The number of officers, their wages, and training decisions are made by the central government, not mayors.

[7]Most evaluations are observational, where causal identification is difficult due to small sample sizes and the difficulty of finding counterfactual comparison communities (Farrell et al., 2016; Roman et al., 2018). There are, however, experimental studies of infrastructure improvement (such as lighting) or urban renewal (Farrington and Welsh, 2008; Cassidy et al., 2014; Blattman et al., 2021).

[8]For U.S. cities and the national average, we use Tables 24 and 26 from the FBI's 2016 Uniform Crime Reporting system, https://ucr.fbi.gov/crime-in-the-u.s/2016/crime-in-the-u.s.-2016.

Police autonomy is one reason why Colombian cities have been experimenting with civilian security measures for decades. Most cities have a large municipal agency, the Secretariat of Security, that directs a diverse array of activities and staff. Medellín's secretariat has roughly 1 staff per 1,000 residents, giving it roughly a third as many personnel as the police. They provide a range of services to residents, including responding to various emergencies and street disorders, directly resolving community disputes and domestic violence, and regulating the use of public space.

The mayor or *Alcalde* oversees the Secretariat and appoints all leadership positions. These leaders, along with other permanent and non-politically appointed senior staff, constitute the top-down task force that was part of the intervention we evaluate in this paper.

These security- and dispute-related units have several "headquarters" in each comuna, including *inspecciones* who directly resolve community disputes through a formal, fast-track justice service, and *comisarías* who provide a wide range of family services aimed at resolving legal problems, mental health problems, domestic violence, child protection, and family law. Each comuna also has a "liaison" that performs community outreach, in order to identify which neighborhoods or people are in need of these services.

In addition to these comuna-based city services, comunas are divided into neighborhoods or *barrios*, and roughly each one has an elected community action board (*Juntas de Acción Comunal*, or JACs) that help local groups regulate and organize their community. They are rarely involved in security, protection, and dispute resolution, however, and so they are not a major focus of the intervention or activities we study in this paper.

2.2 Street gangs

Virtually every low- and middle-income neighborhood in Medellín also has a local gang called a combo. There are roughly 400 in the metropolitan area. Combo territories—often no more than 10 to 25 blocks—tend to be long-standing, well-defined, known to locals, and relatively stable over time.

As part of a larger and ongoing project on organized crime and gangs in Medellín, we conducted a large number of semi-structured qualitative interviews to help us understand how this clandestine system works (Blattman et al., 2023b). This includes interviews with 149 leaders and members across 79 criminal groups. Obviously, this is a convenience sample of criminal actors who agreed to speak with us. Almost half took place in one of Medellín's three major prisons, from which leaders continue to direct street operations.[9]

[9] We believe they spoke to us for several reasons: pride; respite from boredom; interest in speaking with professors; the fact that they were already prosecuted; the fact that we were not asking about indictable information; and a hope that this would further their efforts for a peace process with the government.

We found that, like many cities, Medellín's street gangs are generally small, well-organized illicit firms whose main profits come from local retail drug sales. Combos typically have 15 to 50 permanent members between the ages of 15 and 35, and each member typically has a well-defined position in one of the combo's illicit business lines. Besides holding a monopoly on drug sales in their neighborhoods, combo members frequently participate in and regulate local informal and sometimes legal markets, including microfinance (loan sharking) as well as consumer goods—especially cooking gas, arepas, milk, and eggs.

2.3 State and combo performance

Data To understand the legitimacy of these actors and their security services, we also conducted qualitative interviews with 23 community leaders and 151 residents and shopkeepers. We also interviewed 19 police officers and officials, 17 city officials, 10 prosecutors, and 18 other crime and security experts. These interviews helped us to identify the most common forms of security provision and taxation by the state and combo, and ways of eliciting legitimacy in each actor.

Based on these interviews, we designed a representative survey of nearly 5,000 residents that would measure levels of governance and legitimacy in all of the city's low and middle-income neighborhoods. This representative city sample is distinct from the experimental sample. This section focuses on the city sample alone, mainly for descriptive purposes.[10]

In particular, the survey asked residents several questions related to *State* and *Combo legitimacy*: how much respondents trust each actor; whether they think they are fair; whether residents are satisfied with them; and whether residents thought their neighbors trust the state and combo. We measure police and Alcaldía legitimacy separately.

The survey also asked about security-related governance services. Specifically, it asked

We had several strategies for maintaining the confidentiality of criminal group members. Above all, we were transparent about our research aims and work with 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. Finally, we consulted extensively with the human subjects committees of our institutions, and we obtained written support and assurances of noninterference from several authorities. We discuss these data and ethical considerations in more detail in Blattman et al. (2023a,b).

[10] We conducted both surveys at the end of 2019, after the intervention was completed. For the *city survey*, we collected data in 223 of the city's 250 barrios. This included all low- and middle-income barrios, and excluded high-income and non-residential areas. We stratified the city by these 223 barrios and randomly sampled more than 2,300 blocks—16 percent of all blocks in the city. Enumerators tried to interview two households and one business on each sampled block, for an average of about 21 survey respondents per barrio.

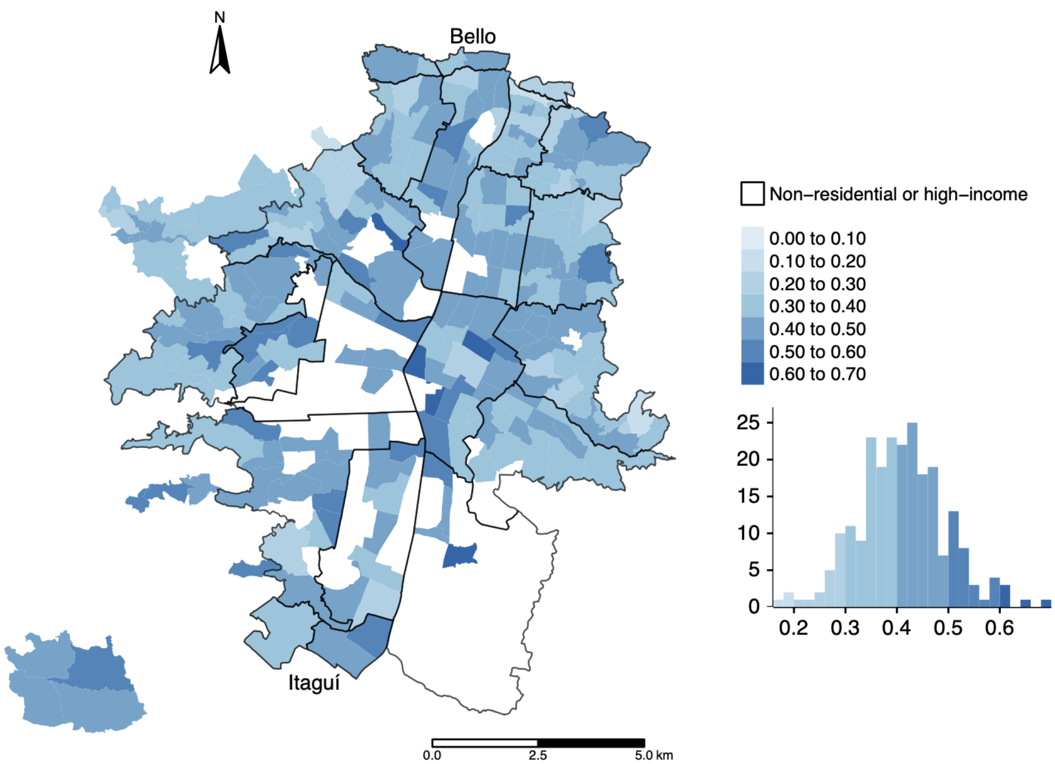
The *experimental sample* is discussed below. It does not overlap with the city sample. There are less than 400 blocks in the experimental sample— less than 2.5 percent of all city blocks. Could the treatment influence outcomes in the city sample? We test for evidence of such spillovers in Appendix Table A.1 and find no evidence of them.

Table 1: State and combo governance and legitimacy, barrio survey averages, 2019 (N=4,598)

	Frequency/Rate (0-1 Scale)				Relative State – Combo	
	State		Combo		City-wide survey	Experimental control group
	Estimate (1)	SD (2)	Estimate (3)	SD (4)	(5)	(6)
Governance Index	0.41	0.26	0.34	0.29	0.07	0.07
How often they intervene when:						
HH: Someone is making noise	0.43	0.38	0.19	0.30	0.23	0.26
HH: Home improvements affect neighbors	0.41	0.38	0.25	0.34	0.16	0.14
HH: There is domestic violence	0.51	0.37	0.35	0.37	0.15	0.15
HH: Two drunks fight on the street	0.54	0.36	0.40	0.37	0.13	0.13
Biz: Someone disturbs a business	0.50	0.38	0.36	0.38	0.12	0.16
Biz: You have to react to a robbery	0.52	0.37	0.40	0.39	0.11	0.12
Biz: It is necessary to prevent a theft	0.45	0.37	0.38	0.39	0.07	0.08
Biz: Businesses in this sector are robbed	0.42	0.39	0.35	0.38	0.05	0.07
HH: People smoking marijuana near children	0.29	0.36	0.25	0.36	0.04	0.03
HH: A car or motorbike is stolen	0.46	0.37	0.43	0.38	0.04	-0.01
HH: Someone is threatening someone else	0.42	0.36	0.41	0.37	0.01	-0.01
HH: You have to react to a robbery	0.46	0.36	0.45	0.38	0.01	-0.02
HH: Someone is mugged on the street	0.39	0.36	0.41	0.38	-0.01	-0.05
HH: It is necessary to prevent a theft	0.40	0.36	0.42	0.38	-0.03	-0.04
HH: Kids fight on the street	0.29	0.35	0.32	0.37	-0.04	-0.03
Biz: Someone does not want to pay a debt	0.17	0.31	0.23	0.35	-0.06	-0.05
HH: Someone refuses to pay a big debt	0.22	0.31	0.39	0.38	-0.16	-0.20
Legitimacy Index	0.58	0.21	0.43	0.28	0.13	0.13
When solving problems in the neighborhood:						
How much do you trust the...	0.57	0.30	0.36	0.36	0.19	0.20
How fair is the...	0.55	0.27	0.41	0.35	0.11	0.12
How do you rate the...	0.60	0.22	0.51	0.28	0.09	0.09
How would your neighbors rate the ...	0.59	0.23	0.50	0.29	0.09	0.08
How much do your neighbors trust the...	0.57	0.28	0.47	0.36	0.09	0.06

Notes: The governance and legitimacy indexes are averages of the component questions listed in this table. Columns 1–5 present averages from the city-wide survey, representative of Medellín's 223 low- and middle-income barrios, with 20–25 respondents per barrio. Column 6 reports averages for the experimental sample of 80 sectors, with roughly 30 respondents per sector. The Relative State measures in Columns 5 and 6 are the differences between columns 1 and 3. All governance scales correspond to: 0 = Never, 0.33 = Occasionally, 0.66 = Frequently, 1 = Always. All legitimacy scales correspond to: 0 = Nothing, 0.33 = A little, 0.66 = Somewhat, 1 = Very. Both households (HH) and businesses (Biz) were surveyed on governance levels (N=4,598), but only households were surveyed on legitimacy (N=2,950). For Column 6, the experimental sample, these sample sizes are 2,362 and 1,906.

Figure 1: State governance levels by barrio (N=4,598)



Notes: The figure displays average levels of state governance reported in each low- and middle-income barrio, using the average of all 17 items from Table 1, averaging across all survey respondents in the barrio. We did not survey high-income residential neighborhoods or non-residential areas, all of which appear in white.

residents how frequently the state and the combo responded to 17 of the most common and important disputes and forms of disorder (pooling the police and Alcaldía for each of the 17 questions to reduce the survey length). The question used a 1–4 Likert scale, but we have rescaled answers to a unit scale, where 0 = Never, 0.33 = Occasionally, 0.66 = Frequent, 1 = Always. We then averaged these 17 items into indexes of *State governance* and *Combo governance*, each one ranging from 0–1. We use these to calculate *Relative state governance*. As the difference between these two measures, it ranges from -1 to 1, where positive values imply that state services exceed that of the combo in that neighborhood and negative values imply otherwise.

Table 1 reports levels of governance and legitimacy for each question plus the overall averages, both for the city sample (the focus of this section) and the experimental sample.

Note that "governance" can also include the provision of public goods as well as collective decision-making and coordination. We focused on protection-related questions because we wanted to assess whether the intervention could affect order and security, the focus of policing activities. Throughout this paper, we use "governance" in this narrower sense of responsiveness to insecurity.

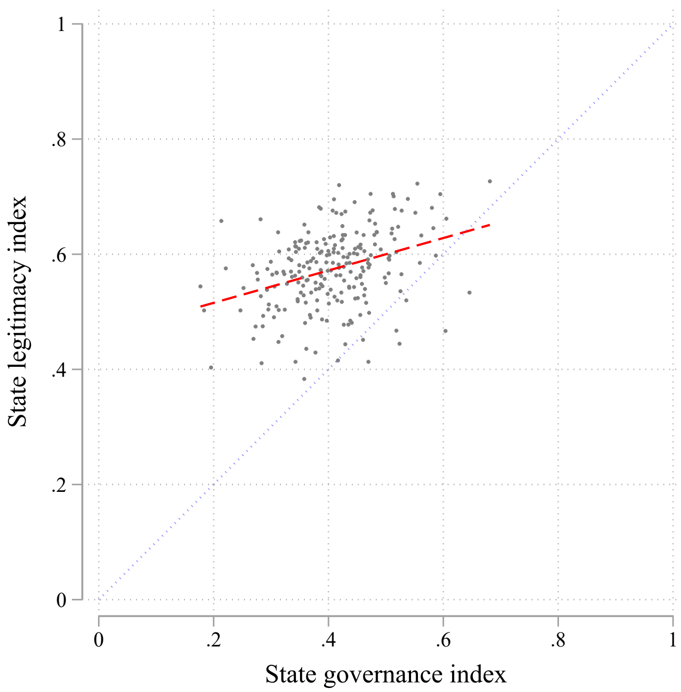
State governance and legitimacy Table 1 shows that overall, low- and middle-income residents score state responsiveness as 0.41 on the 0 to 1 scale—slightly better than "occasionally" responsive to disorder and disputes. State responsiveness is greatest (above 0.5) for robberies, domestic abuse, and adult street fights. It is poorest (below 0.3) for debt collection, teenage disputes, and drugs and smoking near children.

As Figure 1 illustrates, however, state services are unevenly distributed. Some barrios report levels below 0.2, and others above 0.6. The most significant correlate of state responsiveness is elevation. The city lies in the valley of a river running roughly south-north. Barrios along the central south-north axis in the figure are in the lower slopes and valley, while barrios to the left and right are generally built on steep slopes.

Unsurprisingly, trust in and satisfaction with the state is associated with the quality of security. Figure 2 plots state legitimacy against state governance levels by barrio. The relationship is strong and statistically significant.

Combo governance and legitimacy Criminal governance is pervasive across the Americas. Uribe et al. (2022) estimate the number of people living under some form of gang rule in the tens of millions. Medellín is a well-known case (Arias, 2017; Cruz and Durán-Martínez, 2016; Moncada et al., 2018). After the state, combos are the most common organization that residents turn to in order to settle household and business disputes, collect debts, stop fights,

Figure 2: Relationship between state governance and legitimacy, 2019 (N=2,958)



Notes: Each dot is a barrio average of household responses, and the dashed line indicates fitted values. A regression of state legitimacy on governance yields a coefficient of 0.28 (p<0.01). We omit business surveys, which collected governance but not legitimacy measures. We did not survey high-income barrios.

prevent thefts, manage the homeless and drug addicts, and other neighborhood disorder.

In return for these services, combos typically collect weekly fees from local businesses and residents, and may also charge on a fee-for-service basis. Residents and businesses typically call the weekly tax a *pago por la vigilancia* ("security" or "surveillance fee") or, more colloquially, a *vacuna*—literally, a vaccine.[11]

As Table 1 illustrates, the average reported combo governance is 0.34—about 83% the level of the state. Citywide, among the 17 components of this index, the combo is rated as slightly more responsive than the state in five situations: responses to muggings, preventing theft, teenage street fights, and business and household debt collection. Average combo legitimacy is 0.43—about 75% of the state's level.

The state is present in every neighborhood, but varies in its responsiveness and penetration, as we saw above. Likewise, a combo is almost always present, but combos vary widely in the extent to which they offer governance and security services. Many choose to provide no governance at all. Others provide a wide range of services. As a result, while the state is the dominant provider of protection in most neighborhoods, we observe a wide variation. We illustrate this in Figure 3, which reports relative state governance. In 31% of neighborhoods, residents report the combo is more responsive to these 17 forms of disorder than the state.[12]

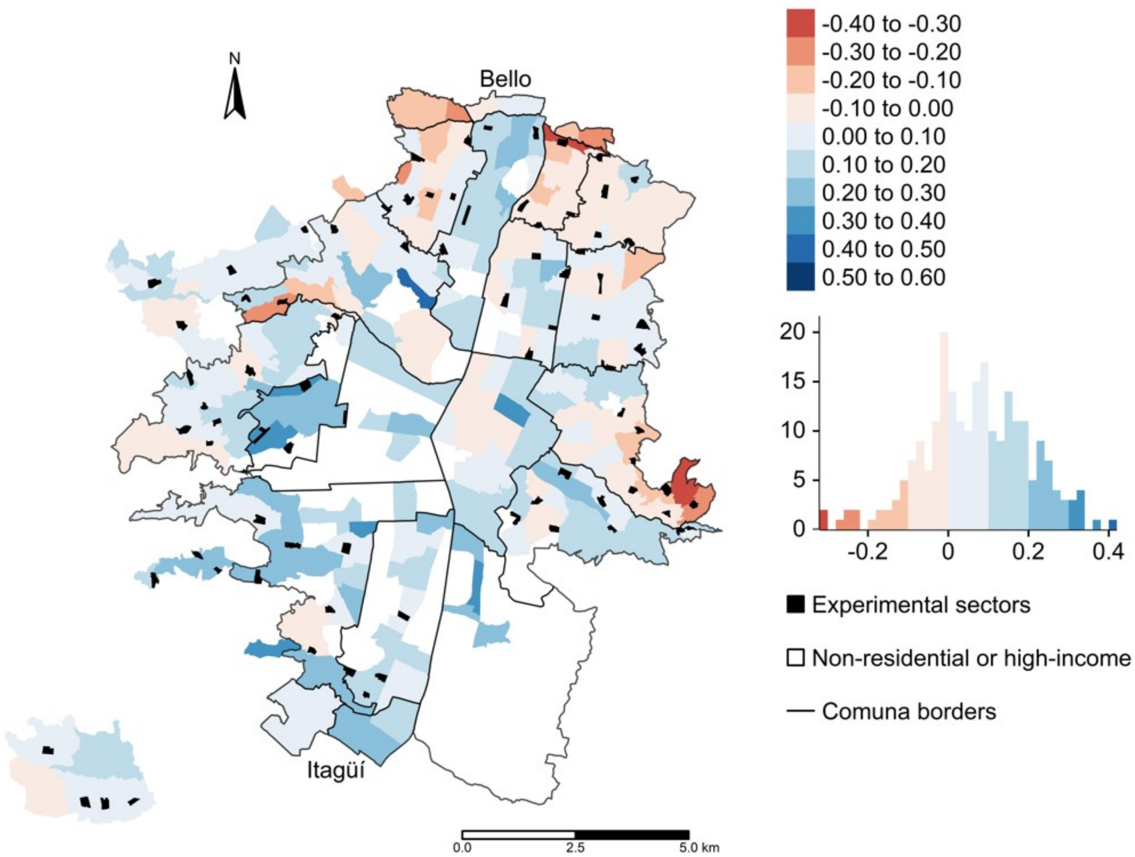
Combos were present in most neighborhoods as early as the 1970s and 80s, but gang rule and taxation is a more recent phenomenon, emerging in the 1990s and early 2000s. A companion paper shows that combos have two main motives for governing. One is a drive for revenues, as gangs could charge taxes and fees in return for settling disputes and providing security. But the combos also rule to protect their other business lines from state predation, especially drugs (Blattman et al., 2023a). The rationale is simple: if gangs maintain order on the streets, the police may be less likely to be called into the neighborhood, and residents may be less likely to share information or collaborate with the state. In that paper, we use exogenous variation in policing and municipal security services to show how, over 30 years, combo governance tended to emerge where the state was strongest and drug profits were greatest.

As a result, combos generally try to foster community goodwill despite the fact that their

[11] In most cases, this is not a purely extortionate protection racket in the sense of demanding money from shops or households in exchange for agreeing not to harm them. At the same time, payment and participation is seldom voluntary. If the local combo decides to provide security services on a block, most shopowners will be compelled to pay the vacuna.

[12] The state and the combo offer different forms of governance, of course. The state's dispute resolution and court systems tend to be impartial and professional, and city leaders are elected in competitive elections. The combo is unelected and relatively unaccountable, and may provide more "justice" to those who hire them or who are closest to them. At the same time, combos may have more local knowledge and deeper networks than most state bureaucrats. Combos are also available all the time, and act swiftly.

Figure 3: Relative state-combo governance levels by barrio, and location of experimental sectors (N=4,598)



Notes: The figure displays average levels of relative state-combo governance in every low- and middle-income barrio, using the average of all 17 items from Table 1, averaging across all survey respondents in the barrio. We did not survey high-income residential neighborhoods or non-residential areas, all of which appear in white. We also display the location of the 80 experimental sectors.

drug-selling and vacuna collection can be unpopular. They do not restrict the movement or activities of residents, and they seldom keep outsiders from entering the neighborhood. They also tend to let city officials and community leaders carry out their activities unimpeded (though new officials might be closely watched or interviewed at first).

3 Intervention

The city government designed the intervention two years before police violence in the United States galvanized policy debates around policing alternatives. Colombia also faced massive protests against police violence shortly after the end of our study, in 2020 and 2021. This triggered a similar debate and a subsequent police reform (Abril et al., 2023).

Even before this, however, the government was interested in increasing public safety and coexistence, and thereby improving its legitimacy and popularity. Ideally, citizens would also seek the government and community out for services at the expense of the gang.

To do so, the city decided to evaluate the impacts of several interrelated, existing services. First, as discussed above, the Secretariat of Security has a number of dispute resolution and family services officers assigned to each of the 16 comunas. Second, the Alcaldía and its various Secretariats also have several mechanisms for receiving and responding to community complaints about public space, such as streetlights, graffiti, or broken playgrounds. One of these mechanisms is called a *Consejo de Convivencia*. This is a meeting, typically held once annually per comuna, where community members and senior city and police officials decide on a plan of action for tackling specific community grievances and issues. Finally, each comuna has one outreach staff member called a "liaison." These liaisons are tasked with community outreach and facilitating the above actions.

The idea for an intensive, localized civilian-led effort came from a small and little-known governance and public safety effort in one of Medellín's poorer and under-served neighborhoods, called *La Loma*. A small unit in the Alcaldía assigned 7 liaisons to the neighborhood—about 1 per 23 blocks, a huge increase over the normal level of 1 per 540 blocks. From 2012–17, these staff set out to improve state legitimacy and governance by: (i) helping existing community organizations better organize themselves to address local problems (such as neighborhood cleanliness, idle youth, or conflict resolution); (ii) connecting residents in need to existing city services (such as dispute resolution and mediation, or family and mental health services); (iii) bringing neighborhood problems to the attention of city agencies (such as garbage collection or broken lights and playgrounds); and (iv) improving lines of communication between the community, local government, and the police.

Our qualitative interviews and observations in La Loma suggested that the intensification of street-level staff increased community organization and access to municipal services, and the legitimacy of the state rose. We brought this initiative to the attention of the Mayor and Secretary of Security. They decided to expand and evaluate the program to assess the viability and returns to the approach.

3.1 Experimental sample

The Alcaldía decided that the appropriate level of intervention would be a "sector." This is an informal but well-defined neighborhood, far smaller than an official barrio, usually with about 200–600 households, covering 5–10 medium-density city blocks. The Alcaldía had funds to intervene in 40 sectors. Therefore, they set out to identify 80 sectors for an experimental sample, drawn from the city's low- and middle-income barrios. Figure 3 displays these sectors.

The experimental sectors were not chosen randomly, but they are broadly representative of the city's neighborhoods in terms of their demographics, geographic features, and variation of state and combo governance.[13] State penetration varies widely across these neighborhoods, however. Some had a long tradition of street-level service delivery from the Alcaldía. In others, residents told us this was one of the first times they had seen a city representative other than the police in their neighborhood. All 80 sectors had some degree of combo presence, but this too varied widely (much like the rest of the city). In this respect, our experimental sample is fairly representative of the variation in Medellín. Appendix Figure A.1 illustrates this, showing that the variation in both state and combo governance in the experimental sectors is broadly representative of the city's low- and middle-income barrios.

3.2 Intervention activities

The intervention began in April 2018, midway through the administration of Mayor Federico Gutiérrez—a center-right politician who, like many former mayors of Medellín, ran for the Presidency at the end of his term.[14] Generally speaking, this aspiration to higher office is a key motive for single-term mayors to produce broad-based policy successes during their term.

The city intensified normal civilian public safety and coexistence services in 40 of the 80 sectors for 20 months, beginning in April 2018 and ending in December 2019. Control sectors received normal services.

[13] Columns 5 and 6 of Table 1 above show that average levels of relative state governance and legitimacy are extremely similar in the city sample and the control sectors in the experimental sample.

[14] He came in third in the 2022 elections, missing a run-off by a relatively small margin.

The intervention had three main components:

Central task force The Alcaldía first created an inter-agency task force to respond to local concerns. This could include normal services—e.g., poor trash pickup or broken playground equipment—but the task force also tried to respond to security concerns, including attention from the city's dispute resolution officers and family services. There was no change in police or other criminal justice attention to the sectors, as both are outside the purview of the Secretariat of Security.

Community-government meetings The city also set out to improve communications and relationships with sector residents. First, officials from the Alcaldía and local police commanders were asked to attend twice-annual *Consejos de Convivencia*. The practice of these consejos, or councils, was established by a legal decree in the 1990s in an attempt to increase the accountability of public officials to community problems. During these council meetings, officials and community members identify specific community problems and agree on mutual responsibilities and commitments. Normally, there is one Consejo per comuna per year. Given that there are no less than 3,000 residents per sector, and as many as 150,000 people per comuna, this is at least 50-fold increase in communications and opportunities to address problems.

In addition to these meetings, the Mayor's office also organized a large one-time event called *Caravana de la Convivencia*—a weekend-long street festival in each sector where, in addition to music, food, and entertainment, representatives from each agency were on hand to explain their services in detail and identify residents in need of assistance.

Street-level liaisons The city also assigned a full-time street-level bureaucrat—a liaison—to each treated sector. Normally, the city has one liaison for each of the 16 comunas—roughly 1 per 540 blocks. For this intervention, the city hired 40 new liaisons as contractors. Thus treated sectors had 1 liaison per 9 blocks—a roughly 60-fold increase in street-level staffing.

Liaisons were expected to spend 3–6 days per week in their assigned sector, and otherwise in the Alcaldía offices. They were given a high level of autonomy to engage and mobilize the sector as they saw fit. Still, liaisons had weekly targets and quotas for neighborhood events and resident referrals, and their major activities and task force responses were formally logged and geolocated.

Liaisons had several roles, including:

- Collect and formally register community concerns to the inter-agency task force in the Alcaldía, and lobby to see that these concerns are addressed

- Organize community events and meetings, including but not limited to the *Consejos de Convivencia* and *Caravana de la Convivencia* mentioned above

- Help community organizations coordinate local collective action (e.g., coordinating garbage spots and dog excrement norms)

- Provide training to community leaders and organizations in dispute resolution and related skills, and encourage them to take an active role in resolving local issues

- Proactively identify individual and neighborhood problems and referred them to the relevant city agency for assistance (e.g. connecting residents with interpersonal conflicts to the comuna's *inspecciones* for dispute resolution or *comisarías* for family disputes)

- Work with police officers to better inform community members of the "police code"—the country's legal guidelines for dealing with and correctly reporting nuisances, misdemeanors, and crimes, what officers were permitted to do, when to call them, and when to approach the Secretariat of Security

Like roughly 70 percent of municipal staff, liaisons were employed on a contract basis through a non-governmental organization with extensive experience providing neighborhood outreach. They had a manager in the Secretariat of Security that trained them, monitored their activities, and controlled quality.

Liaisons were not residents of their assigned community. Rather, they were professional staff hired for this position, and were similar to the city's existing cadre of professional liaisons: university-educated men and women ages 25–35. Nonetheless, all liaisons came from low- and middle-income communities in Medellín, most of which would have had a combo and a degree of criminal governance.

3.3 Implementation issues and non-compliance

Generally speaking, the bottom-up liaison activities and the community–government meetings were implemented with fidelity, while the broader top-down attention and services were only partially successful.

The liaisons had a high level of street presence and visibility for almost two years. We and the city closely monitored liaisons. From the Alcaldía's administrative records and our spot visits we confirmed that they spent 3–6 days or evenings per week in their sector, held regular community events, and generally met their referral quotas, all within the few blocks they were assigned to. Qualitatively, our general impression was one of autonomous, enthusiastic, hardworking efforts by skilled young professionals. Under-performing liaisons were dismissed. For instance, within the first 8 months of the program, the Secretariat had replaced half of the liaisons with more able personnel.

On average, liaisons logged roughly 10 official events per month (Appendix Table A.2). Typically they only logged the major meetings and activities, not the everyday facilitation and interactions. Appendix Figure A.2 displays the formally-logged liaison activities, overlaid atop a map of treatment and control sectors. The figure illustrates the concentration of activities within experimental sectors (which are in most cases obscured by the density of activities reported in or nearby). Most of these major activities were held within a 125 meter radius of the sector, because community centers, meeting spots, and community organization offices were not always located in the 5–10 block sector itself. Unfortunately the city does not maintain similar administrative data on the activities of the its regular 16 liaisons covering all comunas, but given their limited reach (1 per 540 blocks compared to 1 per 5–10 blocks in treated sectors) we presume that control sectors received no more than 1–2 percent as many events or activities.

In terms of top-down compliance, the liaisons reported that the central task force met some of the community's requests, but not all. Unfortunately, there is no formal administrative data on the top-down task force's activities, their attendance of the Consejos, or compliance with the tasks set out in these council meetings. Therefore, to collect proxies, we interviewed all liaisons after the 20-month intervention. On a scale of 0 to 1 (from full compliance to complete failure to deliver) liaisons rated the wider state compliance roughly 0.34, meaning the state "sometimes" failed to deliver on the requested support. We return to these central compliance issues in Section 6.1.

Finally, we monitored gang reactions to and interference in the intervention. Two-thirds of liaisons reported no interference whatsoever. The other third mostly said that the combo was mainly watchful, such as observing public events and meetings from a distance. Liaisons reported that combos rarely interfered with their work or attempted to take credit for services delivered. The exceptions mostly affected the first few weeks of the intervention and afterwards the implementation ran smoothly. For example, in two sectors, the combo initially prevented two liaisons from entering into the community for the first 2–3 weeks, but once the liaisons were able to explain their job and role, they were permitted to enter and perform their jobs without interference. This is consistent with logged liaison events, which if anything were slightly more frequent in sectors with initially low relative state governance (Appendix Table A.2).

3.4 Impact on control and nonexperimental sectors

The city took several steps to minimize any reduction in services to control neighborhoods or neighborhoods outside the experimental sample. Most of all, the 40 new liaisons were newly contracted staff. And while the city did not increase staffing in its other service agencies and other top-down services, the treatment sectors represent just 2.5 percent of Medellín's blocks. Mechanically, a moderate increase in attention to these blocks should have modest effects on the average services received by other blocks. Consistent with this, we see no evidence of spillovers, as seen in Appendix Table A.1. We cannot exclude the possibility that there was a minor reduction of service in control sectors, but this should not affect the validity of estimated treatment effects. From an ethical perspective, elected officials undertook the decision to intensify services in the 40 sectors in order to inform future policy.

4 Motivation and conceptual framework

By setting out to solve neighborhood problems, increase state-community communications, and raise its street presence, it is intuitive why Operacion Convivencia might improve citizen trust in and satisfaction with the state. As one liaison remarked, "Some of these sectors were forgotten places, with no institutional presence. There were situations or issues that could be addressed, and the community realized that things could be done differently, because not everything can be handled by the combo." Another liaison told us how, "Community members expressed things like: 'We have never been this close to anyone in authority before'," and went on to say that "They were very grateful for it. They welcomed us warmly into the community. It was an opportunity to show them different ways of doing things that they were completely unaware of."

The connection between these activities and security and disorder is not as obvious, however. The 17 forms of order and security we measured in Table 1 include several incidents where the liaisons, the Consejos, and the central task force seldom intervened directly. Generally speaking, they did not disrupt street fights, deter or respond to thefts, or collect debts. The city's dispute resolution officers worked to resolve conflicts, but they were not a rapid response force that reacted to street disorder or violence.

Therefore, to the extent that the intervention can affect order and security, the mechanisms are probably indirect. We see several channels through which this could happen. The first is through the actions of community organizations. The liaisons worked with local leaders and organizations to build their conflict resolution and problem-solving skills. They also tried to shape collective beliefs about appropriate behaviors, as well as forums or rules for resolving disputes. These skill and norm changes are the foundation of most alternative dispute resolution (ADR) programs (Mnookin, 1998; Lieberman and Henry, 1986). Fostering these informal institutions appear to have played a significant role in the success of an ADR program in Liberia (Blattman et al., 2014; Hartman et al., 2021).

A second possibility is that disorderly people avoid or change their behavior in neighborhoods with more visible state presence or more active community organizations. Tackling minor problems and disorder could also avert escalation into larger and more violent disputes. These ideas underlies many urban upgrading and renewal programs, and there is some evidence these programs reduce youth violence and some crime in the United States (e.g. Farrington and Welsh, 2008; Cassidy et al., 2014). This is sometimes referred to as the "broken windows" theory (Kelling and Wilson, 1982), although this term is also used to describe policing strategies that aggressively police minor infractions. Meta-analyses suggest that programs have been most effective at reducing crime when they originate from community and problem-solving aimed at concentrated social and physical disorder rather than aggressive policing of disorderly individuals (Braga et al., 2015).

Third, the liaisons were charged with improving communication between the community, the police, and municipal officials through town halls and by educating the neighborhood on what services are available. "Some people didn't know what the 'Casa de Justicia' is," one liaison explained, "or what the 'Comisaria de Familia' does, or that there's the possibility of free conciliation in a Conciliation Center. So, when they use that strategy, it generates more trust."

The liaisons also set out to educate people on what to expect form the police versus other municipal agencies. This included information on the police code—the responsibilities of officers and the limits on what they are allowed to do. Improved communications and town halls is a central component of "proactive policing" and "community policing"—strategies that also commonly include increasing the frequency of patrols, decentralizing police decision-making, and involving civilians in diagnosing and solving problems (Greene and Mastrofski, 1988; Skogan, 2003). Meta-analyses offer mixed evidence on the success of these broader, more intensive strategies (Weisburd et al., 2019; Blair et al., 2021). Nonetheless, in principle better communication and realistic expectations could increase perceived effectiveness of the police, their legitimacy, and civilian collaboration.[15]

[15] Some of the lessons could be very basic, such as not knowing which jurisdiction they belong to. As one liaison explained to us: "When they have a problem that requires calling the police, since they live close Laureles, they call the Laureles Police Station. However, the officers at the Laureles Police Station tell them, 'That's not our jurisdiction.' ...Or the community inquires about the [dispute resolution office] in Santa Monica, but they're told, 'That has nothing to do with us...' So, the community ends up not calling anywhere."

Finally, the intervention could also reduce citizen dependence on combo governance. This was one of our initial hypotheses. At the outset, we viewed the intervention through the lens of duopolistic competition, whereby the state and the combo were offering residents distinct but substitutable governance services. Should one side exogenously increase production, it was possible that its relative share of services should rise, crowding out combo rule. We illustrate this theoretical possibility with a simple model of Cournot competition in Appendix B.

Subsequently, observing the intervention in progress, we moderated this view. One reason is because neither the state nor the combo appears to fulfill the community's governance needs. Citizens have a huge range of everyday disputes, minor forms of neighborhood are commonplace, and neither the state, the combo, nor community organizations respond to all. Both the state and combo governance measures are well below 0.5 in our indexes in Table 1 above. Thus more of one service will not necessarily crowd out the other, given unmet demand. Nonetheless, we continued to expect that state legitimacy would rise by increasing the quality and quantity of services.

We also moderated our views of crowding out combo rule because, as noted above, combos generally regarded the liaisons and regular city services as benign. Their main concern were the police. Indeed, in a longer-run companion study, we discovered that police presence could have the opposite effect on gang rule. In order to protect drug revenues, gangs may decide to provide governance services to reduce police presence and collaboration with civilians. This could reduce the extent to which state and combo governance are substitutes, and raises the possibility that they are strategic complements. A quasi-experimental analysis of a 30-year increase in both policing and municipal survey suggests both mechanisms are at work, and that over several decades the strategic complementarity may dominate (Blattman et al., 2023a). Whether this is also true over a 20-month horizon and this purely civilian intervention is unclear.

5 Experimental procedures and data

We preregistered our design, outcomes, estimation, and heterogeneity analysis in the American Economic Association registry in April 2018 as Operacion Convivencia was launched. We refined and re-registered the design in Octiber 2019, prior to final data collection, as a Journal of Development Economics registered report.[16] There are no major deviations from these plans. We clarify a few minor deviations below.

[16]https://afosterri.org/jdepreresults/sample-page/ and https://www.socialscienceregistry.org/trials/2622

5.1 Outcomes

We prespecified two primary outcomes: *Relative state legitimacy* and *Relative state governance*. Table 1 above lists each index's components, with experimental sample means in Column 6. By design, the governance measure focuses on security-related matters, and does not capture broader aspects of governance. That is because we were mainly interested in evaluating the impacts of intensified service provision on disorder. We supplement these survey-based outcomes with administrative data on reported crimes and calls to the city's emergency line.

Our primary outcomes come from a December 2019 endline survey, conducted 20 months after the intervention began. We conducted the survey in conjunction with the representative city survey, and they share the same questions. As discussed above, the experimental and representative samples are distinct. The city survey interviewed roughly 21 residents on 7 to 10 randomly-selected blocks per barrio. None of these blocks are located in the experimental sectors. For the experimental sample, we interviewed 3 to 5 residents on an average of 9 blocks in each sector, for a total of roughly 30 respondents per experimental sector. The survey was roughly 30 minutes long, and was delivered in person by enumerators on handheld tablets. Enumerators were employed by one of the country's largest survey firms, and had no affiliation or identification with the intervention.

Naturally, we are concerned that citizens may under-report gang activities, attenuating estimated treatment effects somewhat. Section 6.4 discusses measurement error, and why it is unlikely to influence our results. Briefly, combos are a part of everyday life and not systematically stigmatized. We also designed a survey experiment and find no evidence of response bias.

5.2 Randomization and balance

We grouped the 80 sectors into 40 matched pairs using four baseline measures of security and governance. We then randomized one in each pair to treatment. Such matched pair designs can maximize statistical precision, especially in a relatively small sample (Bai, 2022).

This approach works best when the baseline measures are prognostic of potential outcomes. For one of the four measures, we constructed an *Index of reported crime* from the previous decade, weighted by severity using criminal sentences. Otherwise, however, the sectors had relatively little baseline data. They are small informal neighborhoods, much smaller than the barrio, and there is no administrative reporting at the sector level. Only when there is block-level data can we create sector-level aggregates.

Thus, in order to assess baseline security governance in the sectors, in February 2018 we surveyed roughly three officials per sector—80 local representatives of the Secretariat of Security and 149 resident leaders. We used the survey questions to develop three indexes for matching: *Relative state-combo governance*, an *Relative state-combo visibility* on the street, and *Perceptions of local security and drug use*.[17]

We blocked the 80 sectors into pairs based on a measure of multivariate "distance" between one another using the four baseline variables. This produced the expected degree of balance along baseline covariates, as seen in Table 2. Give the small sample size, we report p-values from randomization inference. We also report a selection of additional baseline variables that we did not use in the matching algorithm. Only one variable—the baseline crime index—shows a chance imbalance.

5.3 Accounting for spillovers

To reduce the chance of interference between units, we selected sectors at least 250 meters distant from one another. A total of 40 intervention sectors also ensured that increased service delivery would minimize any decline in city attention to control sectors, since these represent less than 2.5 percent of all city blocks. In addition, we can use our representative city-wide survey to test for spillover effects into non-treated areas, by comparing blocks close to treatment sectors to those close to control sectors. We see no evidence of such spillovers, as seen in Appendix Table A.1.

5.4 Estimation

We estimate intent-to-treat effects via the simple OLS regression:

$$Y_{isb} = \beta T_s + \gamma X_{sb} + \alpha_b + \epsilon_{isb}$$

where Y is the outcome from survey respondent i in sector s and matched pair b; T is an indicator for random assignment to treatment; X is a vector of the four main baseline indexes; and δ_b is a vector of matched pair fixed effects (Bai, 2022). We calculate p-values using randomization inference (10,000 iterations).

[17]One potential drawback of these data is that the perspective of residents and community members could be different. One of the primary uses of these variables is our heterogeneity analysis, particularly the measure of relative state-combo governance. To the extent our baseline measure is noisy, this will increase noise and decrease statistical power of the test. To the extent that some community leaders over-report state governance, our heterogeneity measure (an indicator for below-median relative state governance) will capture treatment effects on communities where leaders are more candid about low state capacity or high levels of gang rule.

Table 2: Baseline summary statistics and test of balance

Covariate	Means		Regression Difference		N
	Control	Treated	Coeff	RI p-value	
Baseline indices used for matching (standardized)					
Standardized index of frequency of combo visibility	0.02	-0.03	-0.05	0.34	2379
Standardized values of relative state-combo governance	0.04	-0.04	-0.09	0.10*	2314
Standardized index of perceived insecurity and drugs	0.06	-0.07	-0.14	0.14	2379
Index of crime	0.09	-0.12	-0.19	0.01**	2379
Other baseline variables					
Index of distance from public goods and services	-0.14	0.14	0.28	0.10	2379
Distance to nearest public transit (meters)	176.55	237.37	60.11	0.17	2379
Distance to nearest cultural center (meters)	92.43	107.20	14.93	0.32	2379
Distance to nearest educational facility (meters)	44.04	77.11	33.24	0.05*	2379
Distance to nearest justice or police center (meters)	556.16	547.61	-5.49	0.46	2379
Distance to nearest religious center (meters)	163.97	168.67	5.21	0.45	2379
Distance to nearest social services (meters)	273.33	328.97	55.26	0.22	2379
Ease to work in sectror for community leaders	1.05	1.30	0.25	0.04**	2379
Area of sector (square meters)	30411.18	29166.02	-1278.39	0.26	2379
Block present in 1970	0.50	0.44	-0.06	0.26	2379
Multidimentional Poverty Index (2018)	14.36	17.30	2.77	0.11	2237
Total population (2018)	2737.45	1583.38	-1171.34	0.25	2379
Percent of women (2018)	52.34	52.31	-0.05	0.47	2379
Percent of population aged 0 to 14 (2018)	18.90	19.47	0.59	0.29	2379
Percent of population aged 15 to 34 (2018)	36.01	37.44	1.46	0.08*	2379
Percent of population who was born on another municipality (2018)	36.24	39.12	2.83	0.07*	2379
Percent of population who recently migrated (2018)	4.24	5.13	0.89	0.06*	2379
Schooling rate (2018)	0.89	0.89	-0.00	0.41	2379
Unemployment rate (2018)	0.11	0.11	0.00	0.32	2379
Median age (2018)	33.77	32.69	-1.11	0.15	2379
Percent of houses with water services (2018)	0.86	0.87	0.01	0.31	2379
Percent of houses with internet services (2018)	0.49	0.48	-0.01	0.36	2379
Percent of houses with electricity (2018)	0.87	0.87	0.00	0.49	2379
Percent of houses with trash collection (2018)	0.87	0.87	-0.00	0.47	2379
Percent of houses with gas services (2018)	0.56	0.59	0.03	0.29	2379
Percent of houses with sewage (2018)	0.86	0.86	-0.00	0.47	2379
Distance to the respective razon headquarters (100 meters)	17.38	19.62	3.50	0.28	1871
Endline survey respondent demographics					
Female	0.66	0.68	0.02	0.18	2379
Respondent age between 18 and 25	0.19	0.19	-0.00	0.50	2379
Respondent age between 26 and 40	0.29	0.31	0.01	0.26	2379
Respondent age between 41 and 64	0.39	0.37	-0.01	0.27	2379
Respondent is business owner	0.20	0.20	0.00	0.47	2379

Notes: This table reports treatment and control group means and a test of balance for the covariates used to match treatment and control sectors (the first five variables) and for some of the covariates selected by the lasso method as prognostic of endline absolute state governance. Regression differences come from an OLS regression of each covariate on an indicator for treatment, calculated at the individual survey level, clustering standard errors at the sector level.

With this design, we estimated we were powered to detect improvements in state governance and legitimacy of about 12% with a two-tailed test.

Heterogeneity analysis We prespecified heterogeneity analysis by initial levels of relative state-combo governance. We estimate subgroup impacts via the OLS regression:

$$Y_{isb} = \beta T_s + \delta(T_s \times Low_s) + \lambda Low_s + \gamma X_{sb} + +\alpha_b + \epsilon_{isb}$$

Variables are the same as in Equation 1, and *Low* is an indicator for neighborhoods with below-median relative state governance. In that case, β estimates the program impact on relatively high-state neighborhoods, δ estimates the difference between high and low neighborhoods, and $\beta + \delta$ is the impact on low-state neighborhoods. Appendix Table A.3 shows that treatment-control balance within the subgroups.

Note that *Low* comes from the baseline measure of *Relative state-combo governance* as reported by the three community and city leaders per sector. Unfortunately we did not collect separate absolute measures of governance for the two actors.[18]

Deviations from the preanalysis plan The only substantive change to our plan is that we initially committed to report treatment effects for the four major quartiles of baseline relative governance. With just 10 treated and 10 control sectors per subgroup, however, that analysis is under-powered. Thus we concentrate on one of these quartiles—the median—reporting treatment effects in sectors above and below this median level of initial relative governance. Appendix Table A.4 reports the other quartiles for transparency.

Otherwise, the only other change from the preanalysis plan is in broadening the framing of the intervention. As noted above, we initially believed that state governance could not only raise state performance and legitimacy, but also that this could crowd out combo governance. We still explore this possibility. But as the intervention unfolded, and we had a chance to interact with the liaisons and task force over two years, our understanding of their work evolved. Liaisons generally did not try to crowd out the gang, and gangs generally did not react to a non-police intervention. Instead, we recognized that *convivencia* was more focused on fostering local order, and that our analysis should try to assess this by considering crime and calls to the police as secondary analyses. What's more, following the launch of the intervention, policing debates in Colombia and the United States highlighted the importance of this intervention as a civilian-led approach to neighborhood order. Our current framing—a

[18] The preanalysis plan says that we will perform heterogeneity by initial level of criminal governance, but this was a misnomer, as we only ever had a relative measure available.

focus on what cities can do in neighborhoods with weak social institutions, persistent street disorder, and entrenched crime and gangs—tries to capture all of these questions at once.

6 Results

6.1 Quality and consistency of implementation

We first analyze treatment compliance using several proxies for program execution. As we will see, heterogeneity in implementation quality shapes program impacts.

Liaisons appear to have been highly motivated and completed their quotas of measured activities, as discussed in Section 3.3 and seen in Figure A.2 and Appendix Table A.2. Of course, these come from liaison administrative records alone, and are limited to the larger-scale and longer-run tasks, not the day-to-day activities, referrals, and street presence. To assess whether residents noticed the increase in general municipal government activity, our 2019 endline survey included questions on Alcaldía personnel and activities in their neighborhood. Table 3 reports average treatment effects on six survey questions as well as a family index averaging all six responses (to reduce the number of hypotheses tested). The survey did not ask about police activity.

To our initial surprise, we see no evidence that residents noticed the increase in the Alcaldía's activity, or that they attended more events. The average change in the overall index is 0.01—less than a 3 of the control mean. One measure is actually negative—knowing about community events, which fell roughly 12 percent. Only one measure is positive and statistically significant—seeing municipal staff in the sector, which rose roughly 8 percent relative to the control mean. This is promising, because this street presence (rather than events) is probably what citizens should have noticed most and may be the best proxy for implementation. But still, the magnitude is modest. This is a striking finding given the dramatic increase in community-state meetings and street-level staff time.

We anticipated that program impacts could depend on initial state governance and legitimacy. We knew this heterogeneity could go in either direction, but our hypothesis was that there are diminishing marginal returns to state personnel and attention—meaning the intervention would be most noticed and effective in the least-served neighborhoods. Table 3 reports program impacts according to this prespecified heterogeneity, along with randomization inference p-values. Columns (3) and (4) report ITT estimates in sectors above and below the median levels of initial governance, and Column 5 reports the difference between the two.[19]

[19]In all tables, above-median treatment effects come from the estimated coefficient on treatment, the

Table 3: Did citizens notice increases in state activity? Survey-based measures, average treatment effects, and heterogeneity by initial relative state governance

Dependent variable	Control Mean	ATE Estimate [p-value]	Het. by baseline rel. gov. Above median Estimate [p-value]	Het. by baseline rel. gov. Below median Estimate [p-value]	Het. by baseline rel. gov. Diff. Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	(6)
Index of first-stage variables (0-1)	0.33	0.010	0.052**	-0.038*	0.090***	1,908
		[0.286]	[0.013]	[0.055]	[0.002]	
Attended public events carried by State, binary	0.21	-0.002	0.033	-0.041	0.074*	1,876
		[0.459]	[0.121]	[0.121]	[0.052]	
Knew about public events carried by State, binary	0.52	0.031	0.106**	-0.054	0.159**	1,876
		[0.230]	[0.032]	[0.180]	[0.028]	
Attended community events, binary	0.10	-0.012	0.007	-0.034*	0.041	1,856
		[0.227]	[0.382]	[0.072]	[0.116]	
Knew about community events, binary	0.30	-0.037*	0.018	-0.097**	0.115**	1,856
		[0.084]	[0.273]	[0.014]	[0.017]	
Saw mayoral employees in sector, binary	0.61	0.049**	0.090**	0.003	0.086*	1,892
		[0.042]	[0.035]	[0.460]	[0.070]	
Interacted with mayoral employees in sector, binary	0.24	0.027	0.059*	-0.008	0.067*	1,892
		[0.150]	[0.091]	[0.373]	[0.090]	

Notes: This table reports answers to six Yes/No questions in the survey regarding whether residents and businesses noticed municipal employees and events or attended them. Each row is a different dependent variable. Column 1 reports control sector means. Column 2 intent-to-treat (ITT) estimates of program impacts using Equation 1. Columns (3) to (5) report treatment heterogeneity using Equation 1—treatment effects in sectors above and below the median level of baseline relative state governance, and the difference between the two groups. The unit of observation is the individual survey respondent, and we cluster standard errors at the sector level (the unit of randomization).

We see divergent effects depending on the initial levels of state presence, but not in the direction we expected. In initially high state-governed sectors, residents report a 16 percent increase in municipal activities and participation. In sectors where the state governed relatively less, they reported a roughly 12 percent decline. The total divergence in the index between the two kinds of sectors is dramatic—0.09, equivalent to 27 percent of the control mean.

We see this divergence in every component of the index (Column 5). In sectors with relatively greater initial state rule, residents and businesses were dramatically more likely to notice and interact with municipal staff and be aware of and attend community events.

Post-treatment qualitative and quantitative investigations To better understand this divergence, we collected post-treatment qualitative and quantitative data in early 2020. Altogether, these data suggest that the street-level liaisons logged activities consistently in most sectors. Unfortunately, in the neighborhoods where the central state had the least relative presence, there are indications that the city's central task force and other senior actors had trouble delivering on the promises. Top-down compliance appears to be lowest in the sectors with low initial state presence.

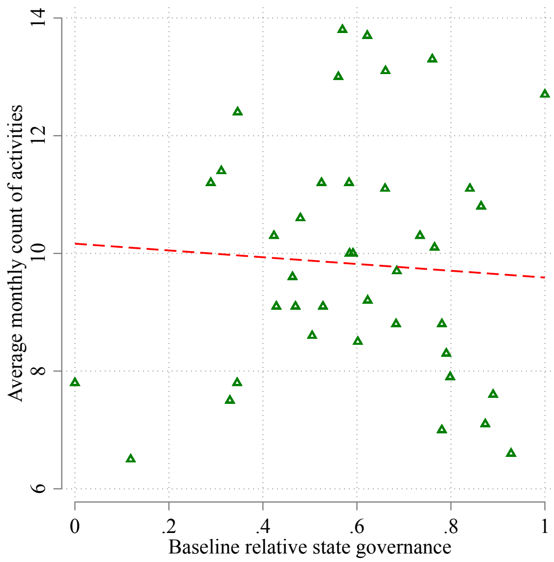
First, the administrative data suggest that liaison compliance and effort were high in all neighborhoods. Panel (a) of Figure 4 uses program data on all events and activities logged by the liaison in the 40 treated sectors. It plots the number of documented activities by baseline relative state governance. Activities are numerous and unrelated to initial government presence. Indeed, if anything, liaisons logged slightly more official events in and around below-median governance sectors (see Appendix Table A.2). We need to regard liaison self-reports with some caution, but these reports are consistent with our qualitative observations that liaisons were highly active in their sectors, physically present 3–6 days a week for 20 months.

The same is not true of the Alcaldía's wider activities, however, including attention from higher-level politicians, bureaucrats, and the task force. Panel (b) of Figure 4 reports the degree to which the central administration failed to deliver on promises in the 40 treated sectors. These come from a post-program survey of liaisons, which asked the liaisons to rate state compliance on a scale of 0 to 1, from full compliance to complete failure to deliver. On average, liaisons rated the Alcaldía's compliance at roughly 0.34, meaning the state "sometimes" failed to deliver on the requested support. But they reported these failures twice as often in the sectors with relatively low initial state presence.

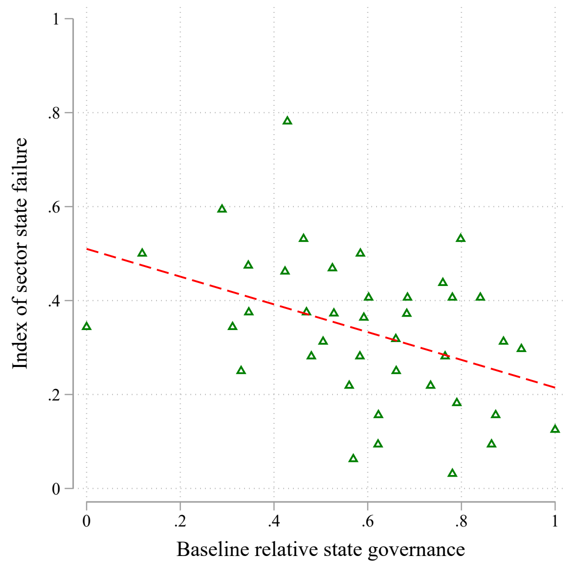
difference between sectors comes from the estimated coefficient on the interaction term, and the below-median treatment effects are calculated as the sum of these two estimated coefficients.

Figure 4: How treatment experiences varied by initial levels of relative state governance (treated sectors only)

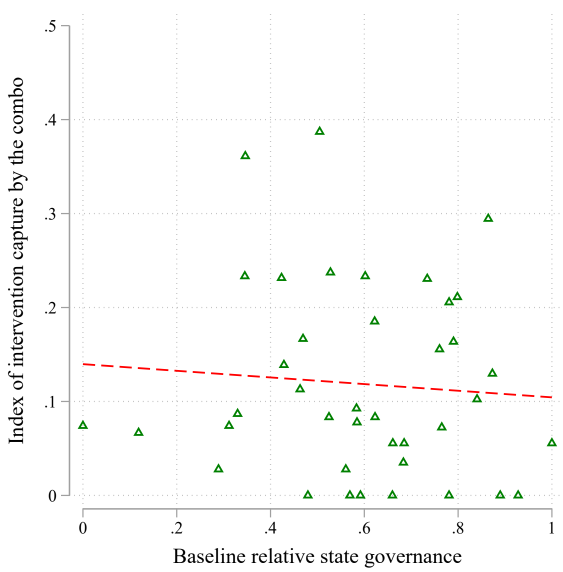
(a) *Count of treatment activities*



(b) *Failed promises of the wider state apparatus*



(c) *Instances of combo interference and capture*



Notes: The city required liaisons to log their activities, and Panel (a) reports the number of activities they logged, by levels of baseline relative state governance. Panels (b) and (c) contain data from a post-program survey of all liaisons. Based on their responses, we created two indexes. Panel (b) reports the frequency of various failures of the liaison or the wider state apparatus to deliver on promises. This includes a scale of the perceived frequency of failures and binary variables for whether specific local state agency failed. Values closer to 1 mean higher state failure. The data in Panel (c) capture the degree with which the combo interfered with liaison activities. This aggregates several measures: a scale for the frequency and difficulties of interaction with local gangs; a set of binary variables on whether local actors (including the gang) took credit for the intervention; and a set of binary variables for activities by which the gang helped the liaison. Values closer to 1 represent higher involvement from locals gang on intervention activities.

Based on our qualitative interviews and focus groups with all 40 liaisons, the most common were failures of the city to respond to community needs and meetings. Equipment might go unrepaired, for example. Or, as one liaison explained, "I managed to gather more than 60 people for the Consejo de Convivencia, but no one from the city showed up." Another liaison reported that the dispute resolution officer "never came up to [this sector] all the time I was there. And he never gave us an answer to why he did not."

Several liaisons also related how they faced difficulties in neighborhoods where initial police quality or responsiveness was poor. "The police have very little credibility," said one, "I had a police station near my territory and, honestly, I rarely saw patrols come in here." Another said how they had publicized the new police code—which includes official guidelines for when citizens should call the police versus one of the civilian security and services agencies—but the residents were frustrated because the police did not follow it reliably. "It's very difficult to talk to people about the rules when they are witnessing a different behavior from police in practice," the liaison explained.

These data suggest that expectations may have been raised by the program beyond the capacity or willingness of officials to deliver, at least in initially under-served areas. But even if expectations were not raised too high, the state apparatus did not follow through on basic promises in the neighborhoods where historically they did not have a strong presence.

Why do we observe adverse effects in poorly-governed neighborhoods? These patterns can help explain why, in initially well-governed sectors, residents were more likely to notice staff and events and attend activities. It is less clear why residents in relatively poorly-governed sectors should report knowing about and attending fewer events compared to control sectors.

One possibility is that, over 20-months, residents in poorly-governed sectors became aware that events were held, but too late to attend. The presence of liaisons may have raised the expectations (and reference point) for what a state or community event is compared to control sectors. Moreover, the answers to these questions are inherently subjective, and may simply be capturing residents' sentiments towards the liaisons and activities—where expectations went unmet, there may be negative attitudes towards events. Program impacts on reports of state governance (below) are similar and consistent with all these explanations. Ultimately, however, we do not know why we observe this pattern.

Combo response Finally, we see no evidence that combo responses shaped the heterogeneous results. Granted, combos noticed an increased presence of the Alcaldía. In qualitative interviews, for instance, almost all liaisons described having to explain their presence to the combo, which is commonplace. Thus, we are confident that combos were generally aware of increased state activity from the beginning.

Most of the evidence, however, suggests that the combos did not react to the presence of these liaisons and the attention of the task force. Most liaisons reported that the combos were indifferent to their organizing. None reported extended harassment and none reported being asked to pay vacunas or bribes. The liaison surveys support this, Panel (c) of Figure 4, captures the degree with which liaisons reported that the combo interfered with their activities.[20] We do not see much evidence that the combo tried to capture the liaison's activities or take credit for their work. The levels are low and there is little relationship with initial state presence. Nor do we see any evidence that combos escalated their governance services or legitimacy in response to the state. The coefficients on the combo indexes in Table 4 are generally close to zero.

This is consistent with what we know of combos in general. They try to minimize community hostility, and even foster loyalty, and so they seldom impede the activities of civilian officials, community leader, and regular residents. Nor is it common to seek vacunas or bribes from community officials to operate. To do so might risk a call to the police, which brings the risk of arrest or drug seizure (or being forced to pay a bribe to the police to avoid arrest and seizure).

We do not want to understate the role of combos, since low initial state presence could be endogenous to gang strength. But there is nothing in our results to suggest direct interference. The results on combo governance and legitimacy below reinforce this conclusion.

6.2 Impacts on governance and legitimacy

We see no evidence the intervention improved state performance on average, consistent with our first-stage results. Table 4 reports program impacts and heterogeneity on the primary outcomes as well as absolute levels of legitimacy and both for the state and combo. Column 2 reports average treatment effects. We see no signs of significant improvement on any measure, and we actually observe a 0.025 *decrease* in relative state governance (p=0.125).

Nonetheless, we continue to see see signs that program impact varied by initial levels of relative state governance. Columns (3) to (5) report program impacts on sectors with initially high and low relative state governance.[21]

[20] See the foot of the figure for details on how we built the intervention capture measure.

[21] In addition to looking at above- and below-median comparisons, we also committed to report treatment effects in the four major quartiles, as seen in Appendix Table A.4. With just 20 sectors per subgroup, however, that analysis is under-powered. In general, however, the legitimacy results persist, with some evidence that the backlash is concentrated in the lowest quartile. The patterns we observe in crime and calls to police are generally consistent with the above- and below-median analysis described below. The

Table 4: Program impacts on governance and legitimacy: Average treatment effects and heterogeneity by baseline governance quality

Dependent variable	Control Mean	ATE Estimate [p-value]	Het. by baseline rel. gov. Above median Estimate [p-value]	Below median Estimate [p-value]	Diff. Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	(6)
Relative state legitimacy index	0.07	0.016	0.050*	-0.021	0.071	1,845
		[0.285]	[0.099]	[0.325]	[0.112]	
State legitimacy index	0.41	0.013	0.033**	-0.010	0.043**	1,906
		[0.136]	[0.026]	[0.275]	[0.032]	
Combo legitimacy index	0.35	-0.002	-0.015	0.012	-0.027	1,845
		[0.455]	[0.295]	[0.380]	[0.277]	
Relative state governance index	0.13	-0.025	-0.018	-0.033	0.015	2,314
		[0.125]	[0.289]	[0.138]	[0.368]	
State governance index	0.57	-0.012	-0.006	-0.018	0.013	2,362
		[0.191]	[0.375]	[0.176]	[0.316]	
Combo governance index	0.44	0.011	0.010	0.012	-0.002	2,316
		[0.281]	[0.371]	[0.308]	[0.478]	

Notes: The table reports intent-to-treat (ITT) estimates of program impacts and treatment heterogeneity using Equations 1 and 1. Each row is a different dependent variable. For the ITT estimates (Column 1) we regress each dependent variable on an indicator for treatment and our prespecified control vector: 5 baseline variables and sector-pair fixed effects. The unit of observation is the individual survey respondent, and we cluster standard errors at the sector level (the unit of randomization). Columns (2) to (4) report treatment effects in sectors above and below the median level of baseline relative state governance, and the difference between the two groups. Both households and businesses were surveyed on governance levels (N=2,379), but only households were surveyed on legitimacy and hence there are fewer observations (N=1,910).

First, relative state legitimacy rises by 0.05 in initially well-governed sectors. This is equal to 40 percent of the state-combo difference in legitimacy (Column 1) and 9 percent of the average level of absolute state legitimacy of 0.57. There is also a small, statistically non-significant decrease in legitimacy below-median sectors. As a result, the difference between the two types of sectors is even larger—0.071, equivalent to 13 percent of the city-wide average. We see the same pattern if we look at absolute state legitimacy.

We break down legitimacy into its five component questions in Appendix Table A.6. The survey asked about the legitimacy of the police and municipal government separately, and we show program impacts on both. In above-median sectors, both mayoral and police legitimacy increase. Interestingly the impact on police is slightly larger and more robust, though the differences between mayoral and police impacts are statistically indistinguishable.

Next we turn to governance, where we see weaker evidence of heterogeneous effects. There is no indication the program increased perceived state responsiveness in above-median sectors. Perceptions of relative and absolute state governance decline slightly in all sectors, with no statistically significant difference between initially well- and poorly-governed sectors.

These results are generally robust to alternative estimation approaches, including the omission of control variables, the addition of demographic traits of survey respondents, and the use of a lasso control vector (see Appendix Table A.5). The p-values from randomization inference are generally slightly larger than standard p-values from OLS (not shown), and we stick with these more conservative estimates.

These patterns hold if we break the governance index into its 17 components and into more and less police-related actions, as reported in Appendix Table A.7. We classify the 17 forms of disorder into 8 that are more likely to elicit a call to police or a police officer response, and 9 that are commonly solved by a variety of city and community actors. This is the only way to assess potential differences between police and mayoral staff because, unlike legitimacy, we did not ask all 17 questions for the police and Alcaldía separately (to keep the survey brief). The fall in perceived police responsiveness is slightly greater than the fall in non-police governance, especially in below-median governed sectors, but none of these differences are statistically significantly different from one another.[22]

Finally, we see little evidence of a combo response. Table 4 shows no evidence of program impacts on combo governance or legitimacy. This is notable given the results of a companion

governance results are less consistent, and adverse effects are not necessarily concentrated in the lowest quartiles.

[22]The survey also included a number of supplementary measures of efficacy, including the speed of response, ease of accessing services, and the value placed on the actor. We report these in Appendix Table A.8. We see no evidence of that residents perceived an improvement in the speed or ease of contacting the police or mayoral staff, on average or in above-median sectors. There is some evidence that perceived value of the Alcaldía declined in below-median sectors.

study of gang reactions to a sustained 30-year increase in police and Alcaldía attention. In Blattman et al. (2023a), we found that combos responded strategically to long-term state presence by increasing their governance over civilians and fostering legitimacy. The results, plus interviews with gang leaders, suggested that the combos were seeking to protect their retail drug businesses. They reduced street disorder and increase civilian loyalty to minimize police presence in the neighborhood. We see no evidence of a strategic combo response in above-median sectors, however. This could be because the combo does not feel threatened by non-police state presence. Alternatively, 20 months of mayoral attention may have been insufficient to provoke a combo response. Qualitatively, our interviews with liaisons are consistent with the combo not feeling threatened by their activities. Indeed, to the extent that the community organization and crime reduction they engendered actually reduced the need for security-related emergency calls, the treatment may have reduced the threat to combo drug rents. We turn to this next.

6.3 Impacts on crime and emergency calls

Given the rise in legitimacy (in well-governed sectors) but no corresponding change in state responsiveness, the intervention may have raised trust and satisfaction with the state in other ways. We turn to two available sources of administrative data: reported crimes and security-related calls to the city emergency line. As above, w see no impact on average, but in initially well-governed sectors the intervention appears to have reduced property crimes, fights, and calls related to fights and other street disorder.

Both measures are for the 20-month intervention period, and both collect all crimes and calls geolocated within a 125 meter radius of the sector.[23] Noe that these are not calls to the police, but rather represent all calls to the city's emergency number that relate to Unfortunately, there are no data on the frequency of police patrols by sector, so it is impossible to know the effects of the intervention on normal police presence outside of these demand-driven calls.

To reduce the number of outcomes and hypotheses tested, we focus on two summary outcomes: (1) an index of all crimes reported, ranging from 0 to 1, with crimes are weighted by their severity (proxied by sentence length guidelines for each crime); and (2) a count of all security-related calls. The tables also list the major components of these summary measures. Note that of this analysis was prespecified, and should be treated as exploratory (especially the many subcomponents).

[23] We chose 125 meters for both measures because of our requirement that every sector be at least 250 meters from one another. 125 meters is half this distance, ensuring no overlap. Patterns are qualitatively similar for other radii.

Before getting to results, it is important to note what each measure captures. In Colombia, crimes can only be reported at a comuna's central police station. Speaking to a police officer or calling the police will not result in a formal crime report (a fact that is widely known). Thus reporting requires traveling up to a kilometer and can take several hours to complete forms. Evidence from other Colombian cities like Bogotá suggests that thefts of vehicles and other high-value items are frequently reported (for insurance purposes), as are crimes that result in serious injury or death. Because of the hassle, however, most petty crime goes unreported (Blattman et al., 2021).

Emergency calls to the police come from local residents and businesses. The vast majority report of callers are reporting a street fight, a case of domestic abuse, or a drug-related complaint—either a concern about a drug seller or (more commonly) drug users causing a public disturbance or loitering. Thus they are a measure of disorder and a perceived need for emergency intervention. All calls are logged and geolocated to an address when police respond.[24] The coding of type of incident is relatively crude, however, and we can primarily distinguish between physical altercations (mixing domestic and street disputes), narcotics-related nuisances, and armed fights.

Starting with crime, the weighted index falls by 0.137 in above-median sectors—a 40% decline, significant at the 5 percent level. The divergence between above- and below-median sectors is even greater, a 0.16 decline. Proportionally speaking, these declines are large for most crime types—vehicle thefts, other thefts and robber, and assault. Curiously, however, we see a rise in homicides overall in treated areas. We must treat all index component analyses as suggestive, however, and we have not adjusted standard errors for multiple hypothesis tests.

Note that these reductions in crime are unlikely to arise from differential reporting of crime in treated and control communities. Residents in initially well-governed treated sectors view the state as more legitimate, and so if anything should be more willing to report crimes to the state. Moreover, the intervention explicitly educated communities on the police code and facilitated semi-annual meeting between the community and local police commander, thus making them more familiar with reporting requirements. In principle, these factors could increase crime reporting rates in treated sectors, leading us to understate treatment effects.

The evidence from security-related calls further suggests that, in initially well-governed sectors, municipal staff or the community itself is either dealing with everyday street disorder

[24]Administrative logs say that more than 97 percent of calls receive a police response and are geolocated. We do not know the location of the 3 percent of unresponded calls, and so cannot assess program impacts on police response. Since nonresponse is low, it seems unlikely to qualitatively affect the results.

Table 5: Program impacts on crime index components: Average treatment effects and heterogeneity by baseline governance quality

			Het. by baseline rel. gov.			
		ATE	Above median	Below median	Diff.	
Dependent variable	Control Mean	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	(6)
Sentence-weighted crime index	0.35	-0.061*	-0.137**	0.023	-0.160**	80
		[0.066]	[0.024]	[0.691]	[0.026]	
Homicides	0.04	0.029**	0.025	0.032	-0.007	80
		[0.025]	[0.114]	[0.206]	[0.413]	
Vehicle thefts	0.33	-0.040	-0.123*	0.053	-0.176*	80
		[0.231]	[0.090]	[0.534]	[0.056]	
Thefts and robbery	1.44	-0.463**	-0.886**	0.009	-0.895**	80
		[0.030]	[0.020]	[0.969]	[0.034]	
Assaults	0.64	-0.104**	-0.146**	-0.057	-0.089	80
		[0.037]	[0.046]	[0.546]	[0.230]	

Notes: The table reports summary statistics and treatment effects for the sentence-weighted crime index in Table 4 and its four main components. Each row is a different dependent variable. The index is standardized to have zero mean and unit standard deviation. Average treatment effects and treatment heterogeneity are calculated using the same approach as in Table 4.

Table 6: Impacts of treatment on security-related emergency calls

			Het. by baseline rel. gov.			
		ATE	Above median	Below median	Diff.	
Dependent variable	Control Mean	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	(6)
Security-related emergency calls	135.75	-34.969**	-63.250**	-3.519	-59.731**	80
		[0.028]	[0.011]	[0.902]	[0.044]	
Physical altercations	93.40	-18.414**	-35.583***	0.678	-36.261**	80
		[0.038]	[0.006]	[0.961]	[0.039]	
Narcotics related incidents	30.90	-15.870*	-24.909*	-5.819	-19.090	80
		[0.068]	[0.091]	[0.727]	[0.255]	
Armed incidents	11.45	-0.684	-2.758	1.622	-4.381*	80
		[0.327]	[0.105]	[0.582]	[0.093]	
Knife related incidents	9.25	-0.968	-2.772**	1.038	-3.810*	80
		[0.201]	[0.037]	[0.628]	[0.051]	
Firearm related incidents	2.20	0.284	0.014	0.584	-0.570	80
		[0.358]	[0.480]	[0.700]	[0.362]	

Notes: This table reports the total number of resident calls to the police emergency line over 20 months, including all calls made within each sector plus a 125 meter buffer zone around the sector. Calls are only geolocated within the city if the police actually respond to the call, meaning we cannot track impacts on unmet calls. But administrative records suggest that more than 97% of calls receive a response, and so are unlikely to affect out results. Average treatment effects and treatment heterogeneity are calculated using the same approach as in Table 4.

without the police, or successfully prevented forms of disorder. There is no change in calls to the police overall, but in above-median treated sectors calls fall by 63 relative to a control mean of 136—a 55 percent decline, significant at the 5 percent level. The divergence between above- and below-median sectors is even larger. We see this decline across every category of call, except for the very small number of firearm-related altercations. The largest decline (and the only statistically significant component) is in calls regarding unarmed street fights and domestic abuse.

To avoid concerns of multiple hypothesis testing and non-prespecified outcomes, we construct a family index of all four measures and test for average and heterogeneous treatment effects, in Appendix Table A.5. The results are largely consistent with the patterns discussed above: no evidence of an average treatment effect, but robust evidence of improvements in the initially well-governed areas.

6.4 Measurement error correlated with treatment

Our governance and legitimacy measures are self-reported survey data, and hence subject to potential response bias. The fact that we see similar results in administrative police crime and call data reduces this concern somewhat. But there are several additional reasons to believe that measurement error is low, or at least not correlated with treatment status.

First, we do not believe that the presence of combos—a familiar and historical part of everyday life in our sectors—significantly distorted responses. We refined survey questions after dozens of qualitative interviews, fine-tuning language, questions, and approach to elicit truthful answers. For data collection, we used an independent survey firm that already conducted annual security surveys to avoid any connection with the intervention, and to minimize experimenter demand effects. They conducted all interviews anonymously and in private, typically indoors. In the context of a secret interview, we believe most respondents answered questions freely and truthfully. Three analyses are consistent with this conclusion.

Second, we can compare our approach against prior efforts. The city has run surveys in the past on "security fees" paid to the combo. City-wide, 19% of our business respondents and 7% of residents report making payments, with negligible non-response. A city survey conducted earlier in the same year reported a 3% payment rate, with 80% non-response. This suggests our approach was actually more successful in eliciting honest responses.

Third, for our results to be spurious would require a very specific pattern of misreporting. Residents would need to systematically under-report state governance or overstate combo governance only in the treatment sectors that had low initial government presence—in essence, the reverse of normal experimenter demand.

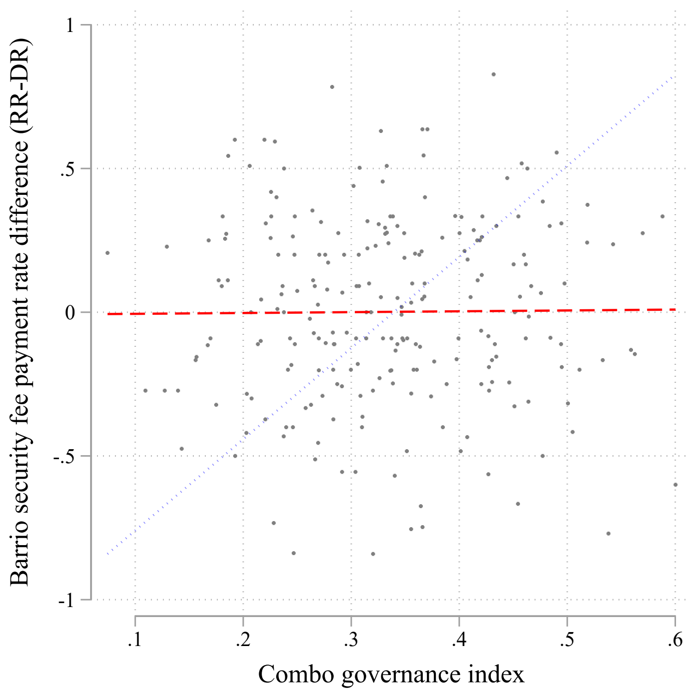
Fourth, we used a survey experiment to assess under-reporting in security fee payment—a measure our qualitative work deemed as one of the most sensitive questions on gang-related activities. We asked some respondents directly whether they paid (Direct response, or DR); others we used a randomized-response (RR) technique, where they privately flipped a coin and responded to the question honestly or not depending on the flip. In other contexts, this method has detected under-reporting of sensitive behaviors.

We see little differences in payment rates between the approaches, suggesting people did not misreport this topic. Randomized response elicited an extortion rate of 22.6% from businesses and 6% from households, compared to 19.4% and 7.8% when directly asked. The differences run in opposite directions and are not statistically significant.

We also see no correlation between assignment to treatment and a RR–DR difference. On average, across all respondents, randomized response results in 4 percentage points higher vacuna payments (not statistically significant). The treatment effect on this RR–DR difference is -0.05, with a standard error of 0.063 (p=0.430).

Figure 5 also calculates the difference between the RR and DR methods at the barrio level, and plot this difference against combo governance levels. A simple regression line is relatively flat at zero, indicating that misreporting is no more or less common in areas where the combos are more involved in daily life, and hence where legitimacy or fear could potentially have influenced under-reporting.

Figure 5: Difference between randomized response (RR) and direct response (DR) to survey questions on combo "security fee" payment



Notes: This figure plots the difference between the RR and DR responses to the survey question on extortion against combo governance. Each point represents a barrio average from the 2019 representative city-wide survey. The figure also plots the 45-degree line and a fitted regression line.

7 Discussion and conclusions

Cities in Latin America and the United States are being pushed to tackle street disorder and insecurity using civilian forces rather than police. In Medellín, we experimentally evaluate an attempt to intensify existing "normal" city services and within-community capacity for managing everyday disorder and disputes. Many American cities have parallel systems that combine local elected officials, city service agencies, and non-profit organizations contracted to organize the community and provide outreach.

Even though this was a long and intense intervention, after 20 months there was little sign of change in people's opinions of the state (at least on average). There was no improvement in perceived state legitimacy or state responsiveness to disorder. Nor did the effort change perceptions of street-level state presence on average (despite confirming the almost daily presence of the liaisons). There were signs the intervention decreased crime and security-related emergency calls on average, but neither outcome was prespecified, and a family index of all outcomes shows no significant change.

On its own terms, this is an important result—a dramatic and multifaceted increase in attention from the municipal government had little average effect on citizen opinions of the state, let alone security. Not all of the evidence points to a null effect, however. The quality of implementation varied, and this could account for the inconclusive average program effects.

Interviews and our prespecified heterogeneity analysis suggest that the return to public investment was high in some neighborhoods, and negative in others. Legitimacy, crime, and security-related emergency calls all fell substantially in the neighborhoods where the state had the highest relative presence and ability to deliver on its promises. We must be cautious when we divide an experimental sample of 80 into subgroups of 40, but still, our results imply that returns depended on initial state capacity or factors correlated with this trait.

One implication is that the marginal returns to state governance may follow an S-curve. At low levels of state capacity, the returns to investments could be low or even negative. At higher and more sustained levels of initial capacity, however, the returns to state efforts seem greater, and may even exhibit increasing returns over some range. This might help explain a common feature of cities worldwide: high government attention to places where the state and community are already strong, and a persistent "neglect trap" in regions where the state is weak or contested, making investment unattractive to politicians.

Another implication is that governments should take care of managing expectations—their own and the public's.[25] Our measures of governance and legitimacy are subjective, and it is possible that the reports of less street presence and lower state governance in below-median sectors are a function of raised expectations that went unmet. In any case, it certainly seems true that both high-level city leaders and street-level staff overestimated their ability to shape community outcomes in the places where they historically had less presence. A final possibility is that the intervention would have been more effective if policing increased in concert with civilian services. This would be consistent with the literature on counter-insurgency, which argues that a combination of military action followed by state

[25] We see this in other scenarios. For instance, Gottlieb (2016) argues that a civics education program in Mali raised citizen expectations of politicians and led to greater survey-based willingness to sanction leaders. Blair et al. (2019) hypothesize that a policing intervention in Liberia failed to improve state and police legitimacy because of raised expectations as well.

service provision increases state legitimacy and civilian collaboration against the insurgents (Albertus and Kaplan, 2013; Berman et al., 2011, 2013; Berman and Matanock, 2015).

All of these explanations are consistent with the results of another study of state presence in Medellín—one that showed how resident opinions and combo activity responded significantly to a 30-year change in police and mayoral presence (Blattman et al., 2023a). A 1987 reorganization of internal city borders created quasi-experimental variation in the presence of both civilian security agencies and police patrols on neighboring city blocks. Greater state presence raised residents' opinions of state security and lowered crime. Unexpectedly, however, combos also responded by governing more. The evidence suggests that, to protect their drug profits, gangs responded to state presence by reducing street disorder and thus lowering the chances that police are called to the street (Blattman et al., 2023a). There are several potential reasons for this difference, including the sustained change and the fact that it included the police. Potentially, initial state capacity might matter less when the reallocation of state attention is sufficiently long and large.

Of course, intensifying services and community organization in a few dozen 10-block sectors of 1000–3000 people each is very different from a generalized increase in city staff and state and community capacity across a major metropolis. We must be careful in what we generalize from this localized experiment, as the general equilibrium effects are unknown—especially combo responses. These limitations are balanced, we believe, by the value of carrying out the first community-level randomized evaluation of a civilian security intervention.

Altogether, these results imply that bringing about order and building state capacity and legitimacy is complicated, and there may be no simple policy solutions—especially in the presence of organized crime. If nothing else, this experiment illustrates the importance of increased experimentation with new strategies alongside rigorous evaluation.

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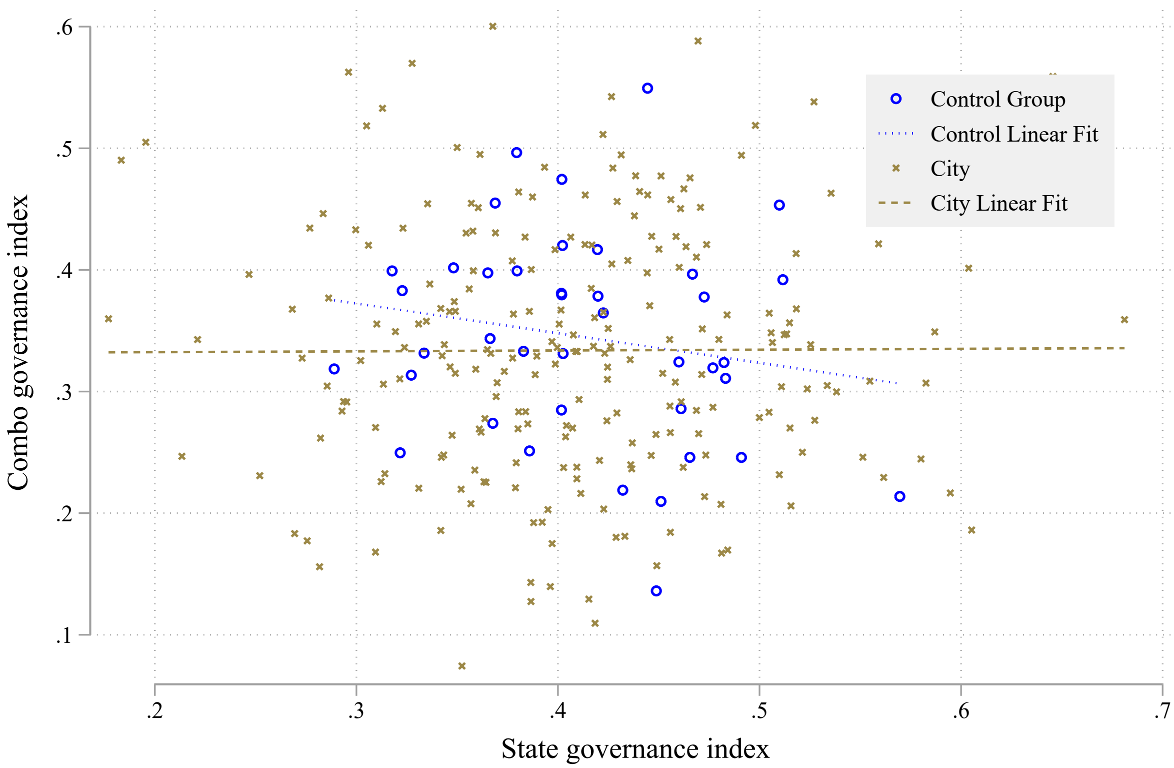
Appendix

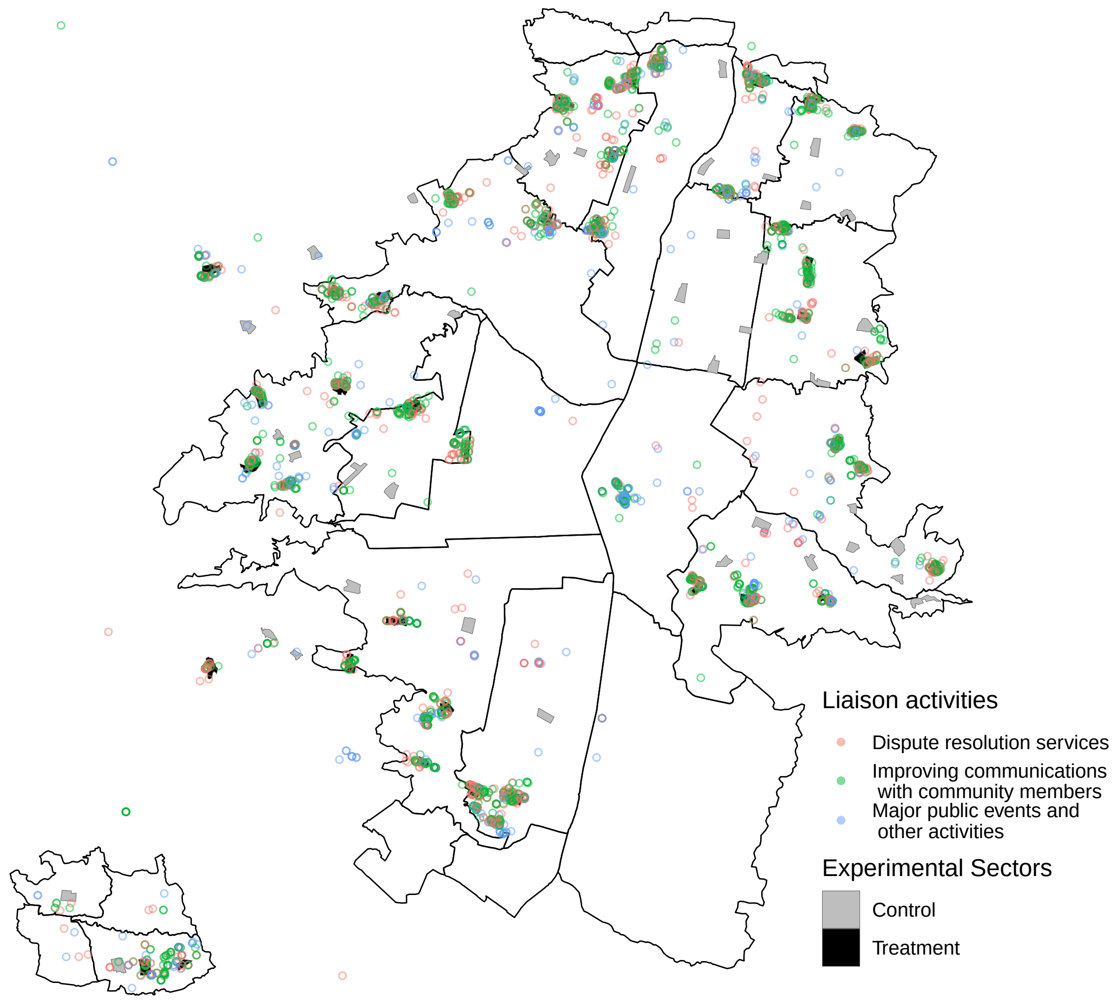
A Supplemental tables and figures

Figure A.1: Comparison of the experimental and city (representative) sample of blocks in 2019: State and combo governance levels



Notes: The figure plots average 2019 state and combo governance levels in each city barrio as well as the 40 experimental control sectors. We omit treated sectors because the 2019 survey is post-treatment. The dashed lines are lines of best fit for the two samples. The experimental sectors are widely distributed, much like the city barrios, though there are slightly more high combo/low state governance areas in the experimental sample.

Figure A.2: Alcaldía and liaison activities by experimental sector



Notes: Liaisons were instructed to report their major events and referrals, including the location. This figure depicts all major liaison activities conducted, overlaid atop a map of treatment and control sectors. We group the interventions into three subgroups based on their goals. Treatment sectors are mostly obscured by the density of activities, while control communities receive relatively few.

Table A.1: Estimating treatment spillovers onto blocks within a 250 meter radius

	Treatment Estimate (1)	P-value (2)	0m-250m Spillover Estimate (3)	P-value (4)
Relative State Governance Index	-0.031	0.121	-0.067	0.919
State Governance Index (0-1)	-0.015	0.232	-0.030	0.946
Combo Governance Index (0-1)	0.014	0.378	0.031	0.869
Relative State Legitimacy Index	0.006	0.889	-0.051	0.706
State Legitimacy Index (0-1)	0.011	0.341	-0.011	0.583
Combo Legitimacy Index (0-1)	0.006	0.776	0.036	0.847

Notes: Our sample includes 6977 survey respondents, including 2,379 in the experimental sectors and 4,598 on blocks from the representative city survey. The tale reports treatment estimates along with an indicator for blocks in the experimental sectors and an indicator for blocks within 250 meters of a treated sector. As Blattman et al. (2021) note, spillovers in a dense network of blocks can lead to fuzzy clustering, where clusters do not conform to defined areas. Hence we use randomization inference to estimate exact p-values under the sharp null of no treatment effect for any unit, correcting estimates for fuzzy clustering. To address systematic exposure to spillovers due to the geographic distribution, we weight each observation by the inverse probability of each treatment category: treated, <250 meters, and >250 meters.

Table A.2: Count of officially-logged liaison activities per sector

			Het. by baseline rel. gov.			
		ATE	Above median	Below median	Diff.	
	Control Mean (SD)	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	(6)
N. activities in 125m sector buffer area per month	0.188	6.927***	7.091***	6.780***	0.311	80
	(0.475)	[0.000]	[0.000]	[0.000]	[0.901]	80
N. activities per month	0.000	9.853***	9.896***	9.814***	0.311	80
	(0.000)	[0.000]	[0.000]	[0.000]	[0.974]	80

Notes: To illustrate treatment compliance, and spillovers of liaison activities into control sectors, this table reports summary statistics and treatment effects for all activities officially logged by liaisons. We examine the count of all activities within the experimental sector itself, as well as within a 125 meter buffer. Note that the activities of other municipal employees are not logged, and so this is an incomplete measure of city staff activities. Average treatment effects and treatment heterogeneity are calculated using the same approach as in Table 4. Note that only households and not businesses were surveyed on legitimacy.

Table A.3: Randomization balance within prespecified subgroups

	High relative state gov.				Low relative state gov.			
	Means		Regression Difference		Means		Regression Difference	
Covariate	Control	Treated	Coeff	RI p-value	Control	Treated	Coeff	RI p-value
Baseline indices used for matching (standardized)								
Standardized index of frequency of combo visibility	0.02	-0.03	0.10	0.35	0.02	-0.03	-0.20	0.11
Standardized values of relative state-combo governance	0.04	-0.04	-0.14	0.08*	0.04	-0.04	-0.05	0.33
Standardized index of perceived insecurity and drugs	0.06	-0.07	-0.11	0.34	0.06	-0.07	-0.16	0.11
Index of crime	0.09	-0.12	-0.21	0.05**	0.09	-0.12	-0.17	0.05*
Other baseline variables								
Index of distance from public goods and services	-0.14	0.14	0.17	0.26	-0.14	0.14	0.39	0.15
Distance to nearest public transit (meters)	176.55	237.37	1.97	0.49	176.55	237.37	116.85	0.12
Distance to nearest cultural center (meters)	92.43	107.20	22.48	0.32	92.43	107.20	7.57	0.44
Distance to nearest educational facility (meters)	44.04	77.11	25.04	0.13	44.04	77.11	41.24	0.12
Distance to nearest justice or police center (meters)	556.16	547.61	-20.16	0.45	556.16	547.61	8.82	0.50
Distance to nearest religious center (meters)	163.97	168.67	-23.45	0.33	163.97	168.67	33.17	0.33
Distance to nearest social services (meters)	273.33	328.97	130.76	0.07*	273.33	328.97	-18.42	0.39
Ease to work in sectror for community leaders	1.05	1.30	0.14	0.21	1.05	1.30	0.34	0.10*
Area of sector (square meters)	30411.18	29166.02	3245.19	0.10	30411.18	29166.02	-5692.25	0.03**
Block present in 1970	0.50	0.44	-0.03	0.42	0.50	0.44	-0.10	0.24
Multidimentional Poverty Index (2018)	14.36	17.30	3.85	0.13	14.36	17.30	1.74	0.28
Total population (2018)	2737.45	1583.38	-2149.97	0.28	2737.45	1583.38	-216.44	0.38
Percent of women (2018)	52.34	52.31	0.75	0.19	52.34	52.31	-0.83	0.15
Percent of population aged 0 to 14 (2018)	18.90	19.47	1.13	0.13	18.90	19.47	0.07	0.48
Percent of population aged 15 to 34 (2018)	36.01	37.44	1.38	0.20	36.01	37.44	1.55	0.12
Percent of population who was born on another municipality (2018)	36.24	39.12	1.11	0.34	36.24	39.12	4.50	0.05**
Percent of population who recently migrated (2018)	4.24	5.13	0.50	0.27	4.24	5.13	1.26	0.07*
Schooling rate (2018)	0.89	0.89	0.03	0.16	0.89	0.89	-0.04	0.10
Unemployment rate (2018)	0.11	0.11	0.00	0.42	0.11	0.11	0.01	0.33
Median age (2018)	33.77	32.69	-1.18	0.15	33.77	32.69	-1.03	0.28
Percent of houses with water services (2018)	0.86	0.87	0.02	0.23	0.86	0.87	0.00	0.46
Percent of houses with internet services (2018)	0.49	0.48	-0.01	0.40	0.49	0.48	-0.02	0.40
Percent of houses with electricity (2018)	0.87	0.87	0.01	0.38	0.87	0.87	-0.01	0.43
Percent of houses with trash collection (2018)	0.87	0.87	0.01	0.33	0.87	0.87	-0.01	0.37
Percent of houses with gas services (2018)	0.56	0.59	0.05	0.27	0.56	0.59	0.01	0.42
Percent of houses with sewage (2018)	0.86	0.86	0.01	0.30	0.86	0.86	-0.01	0.34
Distance to the respective razon headquarters (100 meters)	17.38	19.62	-6.56	0.08*	17.38	19.62	13.12	0.15
Endline survey respondent demographics								
Female	0.66	0.68	0.02	0.27	0.66	0.68	0.03	0.24
Respondent age between 18 and 25	0.19	0.19	0.00	0.41	0.19	0.19	-0.00	0.42
Respondent age between 26 and 40	0.29	0.31	-0.02	0.21	0.29	0.31	0.05	0.08*
Respondent age between 41 and 64	0.39	0.37	0.03	0.19	0.39	0.37	-0.06	0.05*
Respondent is business owner	0.20	0.20	-0.00	0.12	0.20	0.20	0.01	0.10

Notes: This table reports treatment and control group means and a test of balance for all covariates in Table 2, but does so within the two prespecified subgroups: above and below median baseline relative state governance.

Table A.4: Heterogeneity analysis by quartiles of relative baseline state governance

	Dependent Variable: Relative governance	Dependent Variable: Relative legitimacy	Dependent Variable: Sentence-weighted crime index	Dependent Variable: Security-related emergency calls
	(1)	(2)	(3)	(4)
Program impacts:				
Q1 (0^{th} - 25^{th} quartile baseline rel. gov)	-0.025	-0.071	-0.019	-14.369
	(0.332)	(0.146)	(0.386)	(0.352)
Q2 (25^{th} - 50^{th} quartile baseline rel. gov)	-0.038	0.018	0.058	5.289
	(0.113)	(0.411)	(0.155)	(0.435)
Q3 (50^{th} - 75^{th} quartile baseline rel. gov)	0.022	0.056	-0.086	-57.214*
	(0.291)	(0.113)	(0.182)	(0.071)
Q4 (75^{th} - 100^{th} quartile baseline rel. gov)	-0.061	0.038	-0.194**	-70.493*
	(0.123)	(0.281)	(0.050)	(0.062)
Differences relative to Q1:				
Q2	-0.013	0.089	0.076	19.658
	(0.446)	(0.172)	(0.196)	(0.320)
Q3	0.048	0.127*	-0.067	-42.845
	(0.227)	(0.055)	(0.250)	(0.210)
Q4	-0.035	0.109	-0.175*	-56.124
	(0.331)	(0.110)	(0.092)	(0.182)

Notes: This table replicates the results of Table 4 but partitioning the sample in 4 subgroups (quartiles) as opposed to 2. Here we report program effects on each each subgroup in the first 4 rows, while the last 3 report differences with respect to the lowest governance group. Unfortunately this leaves just 20 sectors per quartile subgroup, making this analysis somewhat underpowered. Both households and businesses were surveyed on governance levels, but only households were surveyed on legitimacy (and hence there are fewer observations).

Table A.5: Robustness table

	Control Mean	N	Main spec.	No controls	With respondent demog.	Lasso controls
Average treatment effect						
Family index of all indices (z-score)	-0.11	80	0.246	0.222	0.317*	0.135
			(0.133)	(0.134)	(0.092)	(0.253)
Relative state legitimacy index	0.13	1845	0.016	0.011	0.021	0.041*
			(0.278)	(0.332)	(0.219)	(0.059)
Relative state governance index	0.07	2314	-0.025	-0.028	-0.022	-0.017
			(0.124)	(0.109)	(0.143)	(0.214)
Sentence-weighted crime index	0.35	80	-0.061*	-0.063*	-0.034	-0.032
			(0.066)	(0.072)	(0.220)	(0.174)
Security-related emergency calls	135.75	80	-34.969**	-37.375**	-34.274**	-28.387**
			(0.028)	(0.010)	(0.040)	(0.038)
ATE in above median baseline governance						
Family index of all indices (z-score)	-0.11	80	0.665**	0.672***	0.832**	0.520**
			(0.016)	(0.007)	(0.010)	(0.033)
Relative state legitimacy index	0.13	1845	0.050*	0.072	0.057*	0.051*
			(0.084)	(0.137)	(0.058)	(0.068)
Relative state governance index	0.07	2314	-0.018	0.037	-0.016	0.009
			(0.294)	(0.213)	(0.311)	(0.408)
Sentence-weighted crime index	0.35	80	-0.137**	-0.135**	-0.112*	-0.058
			(0.024)	(0.040)	(0.070)	(0.151)
Security-related emergency calls	135.75	80	-63.250**	-64.400***	-66.878***	-41.691*
			(0.011)	(0.006)	(0.009)	(0.056)

Notes: This tables replicates our main results (Column 1) and re-estimates average treatment effects with three alternative control vectors: no added covariates (Column 2); standard covariates plus survey respondent demographics (Column 2), including gender, age, and a resident versus business indicator; and a control vector determined via a lasso regression of the dependent variables on all available pre-treatment covariates (Column 3). We also construct a standardized family index of all four outcomes as a simple unweighted average of the four indexes, themselves standardized before averaging. We calculate p-values through randomization inference.

Table A.6: Program impacts on police and mayor's office legitimacy components: Average treatment effects and heterogeneity by baseline legitimacy

			Het. by baseline rel. gov.			
		ATE	Above median	Below median	Diff.	
Dependent variable	Control Mean	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	
Police legitimacy index	0.57	0.006	0.032**	-0.022*	0.054***	1,906
		[0.223]	[0.026]	[0.094]	[0.006]	
How much do you trust the police	0.56	0.002	0.034**	-0.032*	0.066**	1,900
		[0.370]	[0.050]	[0.086]	[0.012]	
How fair is the police	0.57	-0.006	0.007	-0.019	0.026	1,838
		[0.431]	[0.285]	[0.218]	[0.145]	
How do you rate the police	0.59	0.007	0.032**	-0.019	0.051**	1,871
		[0.203]	[0.041]	[0.121]	[0.020]	
How would your neighbors rate the police	0.59	0.016*	0.037**	-0.006	0.043**	1,771
		[0.083]	[0.011]	[0.369]	[0.040]	
How much do your neighbors trust the police	0.57	0.013	0.057**	-0.034*	0.091***	1,780
		[0.173]	[0.013]	[0.062]	[0.004]	
Mayor legitimacy index	0.57	0.012	0.026*	-0.003	0.028	1,906
		[0.123]	[0.064]	[0.465]	[0.150]	
How much do you trust the mayoral staff	0.57	0.004	0.018	-0.011	0.029	1,881
		[0.354]	[0.184]	[0.369]	[0.208]	
How fair is the mayoral staff	0.53	0.006	0.022	-0.010	0.032	1,776
		[0.297]	[0.141]	[0.352]	[0.156]	
How do you rate the mayoral staff	0.61	0.003	0.017	-0.012	0.030	1,857
		[0.341]	[0.162]	[0.320]	[0.163]	
How would your neighbors rate the mayoral staff	0.59	0.019*	0.019	0.018	0.001	1,708
		[0.070]	[0.132]	[0.212]	[0.432]	
How much do your neighbors trust the mayoral staff	0.55	0.033**	0.047*	0.018	0.029	1,761
		[0.036]	[0.069]	[0.220]	[0.228]	
Combo legitimacy index	0.44	-0.006	-0.022	0.011	-0.033	1,845
		[0.460]	[0.285]	[0.387]	[0.284]	
How much do you trust the combo	0.36	0.003	-0.012	0.018	-0.030	1,822
		[0.422]	[0.405]	[0.355]	[0.325]	
How fair is the combo	0.41	-0.001	-0.033	0.034	-0.067	1,689
		[0.433]	[0.265]	[0.184]	[0.142]	
How do you rate the combo	0.50	0.001	-0.015	0.018	-0.033	1,642
		[0.445]	[0.356]	[0.314]	[0.275]	
How much do your neighbors trust the combo	0.51	-0.010	-0.031	0.011	-0.042	1,618
		[0.351]	[0.176]	[0.388]	[0.203]	
How would your neighbors rate the combo	0.48	-0.011	-0.027	0.007	-0.034	1,671
		[0.378]	[0.284]	[0.451]	[0.310]	

Notes: The table reports summary statistics and treatment effects for 5 survey-based measures of legitimacy per actor, plus a summary index for the 5 questions. Each row is a different dependent variable. Each row is a different dependent variable. Average treatment effects and treatment heterogeneity are calculated using the same approach as in Table 4. Note that only households and not businesses were surveyed on legitimacy.

Table A.7: Program impacts on relative state governance components: Average treatment effects and heterogeneity by baseline governance quality

Dependent variable	Control Mean	ATE Estimate [p-value]	Het. by baseline rel. gov.: Above median Estimate [p-value]	Het. by baseline rel. gov.: Below median Estimate [p-value]	Het. by baseline rel. gov.: Diff. Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	
Relative state governance index (less police related)	0.09	-0.021	-0.015	-0.026	0.011	2,279
		[0.128]	[0.277]	[0.163]	[0.368]	
HH: Someone is making noise	0.26	-0.019	-0.021	-0.016	-0.005	1,747
		[0.240]	[0.228]	[0.360]	[0.467]	
HH: Home improvements affect neighbors	0.14	0.004	0.011	-0.004	0.016	1,567
		[0.443]	[0.378]	[0.452]	[0.387]	
HH: There is domestic violence	0.15	-0.004	0.028	-0.037	0.065	1,559
		[0.452]	[0.220]	[0.210]	[0.122]	
HH: Two drunks fight on the street	0.13	-0.004	0.004	-0.014	0.018	1,645
		[0.444]	[0.470]	[0.403]	[0.387]	
Biz: Someone disturbs a business	0.16	-0.087*	-0.050	-0.128**	0.078	382
		[0.095]	[0.332]	[0.037]	[0.277]	
HH: People smoking marijuana near children	0.03	0.003	0.022	-0.017	0.039	1,682
		[0.458]	[0.256]	[0.359]	[0.250]	
HH: Kids fight on the street	-0.03	-0.017	-0.005	-0.028	0.023	1,552
		[0.257]	[0.440]	[0.234]	[0.330]	
Biz: Someone does not want to pay a debt	-0.05	-0.006	0.011	-0.024	0.036	370
		[0.429]	[0.398]	[0.340]	[0.296]	
HH: Someone refuses to pay a big debt	-0.20	-0.032*	-0.005	-0.058**	0.053	1,434
		[0.096]	[0.454]	[0.028]	[0.137]	
Relative state governance index (more police related)	0.02	-0.030	-0.013	-0.049	0.036	2,252
		[0.142]	[0.382]	[0.106]	[0.258]	
Biz: You have to react to a robbery	0.12	-0.097**	-0.133**	-0.057	-0.075	372
		[0.048]	[0.046]	[0.253]	[0.272]	
Biz: It is necessary to prevent a theft	0.08	-0.078*	-0.069	-0.088	0.019	396
		[0.100]	[0.243]	[0.128]	[0.434]	
Biz: Businesses in this sector are robbed	0.07	-0.072	-0.075	-0.069	-0.006	362
		[0.100]	[0.153]	[0.222]	[0.496]	
HH: A car or motorbike is stolen	-0.01	0.020	0.053	-0.017	0.070	1,557
		[0.270]	[0.124]	[0.363]	[0.146]	
HH: Someone is threatening someone else	-0.01	-0.026	0.013	-0.065	0.078	1,589
		[0.219]	[0.359]	[0.101]	[0.115]	
HH: You have to react to a robbery	-0.02	-0.017	0.012	-0.048	0.060	1,635
		[0.302]	[0.403]	[0.150]	[0.175]	
HH: Someone is mugged on the street	-0.05	0.020	0.044	-0.005	0.049	1,569
		[0.260]	[0.200]	[0.466]	[0.230]	
HH: It is necessary to prevent a theft	-0.04	-0.007	0.025	-0.042	0.067	1,692
		[0.420]	[0.304]	[0.192]	[0.160]	

*Notes:*The table reports summary statistics and treatment effects for the 17 components of the governance index in Table 4. We create sub-indexes for what our qualitative work suggests are more and less police-related forms of governance. Each row is a different dependent variable. Average treatment effects and treatment heterogeneity are calculated using the same approach as in Table 4. Note that both households and businesses were surveyed on governance.

Table A.8: Impacts of treatment on survey measures of police, mayoral, and combo efficacy

Dependent variable	Control Mean	ATE Estimate [p-value]	Het. by baseline rel. gov. Above median Estimate [p-value]	Het. by baseline rel. gov. Below median Estimate [p-value]	Het. by baseline rel. gov. Diff. Estimate [p-value]	N
	(1)	(2)	(3)	(4)	(5)	
Police efficacy index	0.55	0.005	-0.011	0.021	-0.033	1,790
		[0.400]	[0.400]	[0.440]	[0.480]	
How easy is it to contact the police	0.54	0.015	0.004	0.025	-0.021	1,649
		[0.240]	[0.160]	[0.440]	[0.200]	
Perceived value of the police	0.71	-0.010	-0.032	0.011	-0.043	1,706
		[0.240]	[0.200]	[0.440]	[0.280]	
How fast is the police	0.42	0.016	-0.010	0.041	-0.051	1,589
		[0.320]	[0.440]	[0.320]	[0.400]	
Mayoral staff efficacy index	0.45	-0.009	-0.002	-0.016	0.014	1,906
		[0.280]	[0.440]	[0.120]	[0.280]	
How easy is it to contact mayoral staff	0.35	-0.014	-0.003	-0.024*	0.021	1,869
		[0.240]	[0.440]	[0.080]	[0.240]	
Perceived value of the mayoral staff	0.66	-0.003	-0.010	0.004	-0.014	1,864
		[0.480]	[0.400]	[0.440]	[0.400]	
How fast is the mayoral staff	0.34	-0.009	0.011	-0.029	0.040	1,868
		[0.360]	[0.440]	[0.200]	[0.200]	
Combo efficacy index	0.55	-0.009	-0.001	-0.017	0.016	1,907
		[0.280]	[0.360]	[0.160]	[0.200]	
How easy is it to contact the combo	0.59	-0.010	-0.004	-0.017	0.013	1,881
		[0.360]	[0.480]	[0.280]	[0.440]	
Perceived value of the combo	0.52	-0.009	-0.011	-0.007	-0.003	1,880
		[0.240]	[0.320]	[0.160]	[0.240]	
How fast is the combo	0.56	-0.007	0.012	-0.025	0.036	1,879
		[0.400]	[0.440]	[0.240]	[0.200]	

Notes: This table reports summary statistics and treatment effects on 3 survey-based measures of efficacy per actor, plus a summary index for the three questions. Each row is a different dependent variable. Average treatment effects and treatment heterogeneity are calculated using the same approach as in Table 4. Note that only households and not businesses were surveyed on efficacy.

B Conceptual framework

B.1 Cournot competition in local governance

To understand why the intervention could crowd combos out of local governance, we can look at the intervention through the lens of imperfect competition for governance services. Any model of imperfect competition should produce similar comparative statics, but we illustrate with Cournot competition, where each side chooses a fixed quantity of protection services to provide and let prices clear the market.[26]

Of course, states are not necessarily profit-maximizing and have broader objectives. We model this in simple form below. **?** consider a fuller range of models and additional assumptions, focusing on the strategic response of gangs. But that paper also shows how the results here would be similar in other forms of imperfect competition, including a model of stationary bandits competing to provide public goods.

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

Nash Equilibria The best response function for each organization are derived as follows:

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

Replacing values we obtain (for each organization):

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

[26] Note that Cournot fits some of our stylized facts well—especially that governing requires investments and advanced commitments, and that it is hard to adjust output capacity quickly.

Comparative statics We are interested in how the quantity of services supplied by the gang behave in response to any increase in state governance: $\frac{\partial q_i^*}{\partial q_j}$. To obtain this comparative static, we begin by defining:

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

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

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

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

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

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

B.1.1 Cournot competition with benefits to governing

In the simple model above, increases in the quantity supplied by the state would mainly come from reductions in the state's marginal cost of providing these goods. One way to conceive the experimental intervention is an exogenous investment by the state in lowering the marginal cost of providing governance services. Another way to view the intervention, however, is the result of an exogenous increase in the value the state places on being the market leader in that neighborhood, or even a monopolist. To illustrate this, we introduce a new term to the utility function.

Setup As above, but now the payoff for each organization is $V_i = p_i q_i - c_i q_i + \rho(q_i, q_j)\pi_i$, where $\rho(q_i, q_j)\pi_i$ represents each player's returns to loyalty, legitimacy, and control of the neighborhood.
Set up this way, π_i is the return to full control of the block. For example, π_s includes electoral rewards, achievement of policy aims, or preferences for dominance and citizen loyalty.

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

Nash Equilibria For simplicity, we assume an interior solution. We can derive the best response function for each organization:

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

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

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

with an identical function for q_j^*.

The state's equilibrium level of governance services supplied is increasing in the value they place on neighborhood control, π_i, and their expected returns to investment in citizen loyalty and neighborhood control, $\frac{\partial \rho(q_i,q_j)}{\partial q_i}$.