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STATE-BUILDING ON THE MARGIN:
AN URBAN EXPERIMENT IN MEDELLÍN

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ABSTRACT

Medellín's government wanted to raise its efficacy, legitimacy, and control. The city identified 80 neighborhoods with weak state presence and competing armed actors. In half, they increased non-police street presence tenfold for two years, offering social services and dispute resolution. In places where the state was initially weakest, the intervention did not work, mainly because the government struggled to deliver on its promises. Where the state began stronger, the government raised opinions of its services and legitimacy. If there are indeed low marginal returns to investing in capacity in the least-governed areas, this could produce increasing returns to state-building.

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1 Introduction

State-building is a gradual process of developing organizations that collect taxes, deliver services, and order society. Even in a democratic and middle-income city such as Medellín, Colombia, this process is ongoing and uneven across space. Justice and security often fall short of people's expectations and needs, reducing the legitimacy of the state.

Where the state fails to provide order, traditional leaders and community organizations commonly step in (Cammett and MacLean, 2014; Van der Windt et al., 2019; Blattman et al., 2014; Henn, Henn). When it comes to security and justice, however, non-state actors sometimes undermine the use of and trust in the government (e.g., Berman and Laitin, 2008; Acemoglu et al., 2020; Cammett and MacLean, 2014). Hence, many states try to become the principal arbiter of disputes and providers of public security (Weber, 2013).

Security and dispute resolution can also come from armed groups and criminal gangs. This phenomenon, known as "criminal governance," exists in hundreds of cities worldwide (Arias, 2006; Lessing, 2020). In Medellín, for example, most low- and middle-income neighborhoods have a neighborhood gang called *combo*. In addition to selling drugs, some combos police commercial streets and settle disputes between neighbors for a fee, arguably undermining the state's monopoly on coercion.

Why do non-state actors provide order and security? The conventional wisdom is that both armed and unarmed actors increase their governance in response to a state that is unable or unwilling to project power. Take criminal groups, for instance. Scholars trace the origins of the Sicilian Mafia, Brazilian and California prison gangs, Congolese warlords, and other groups to the state's inability to protect production or regulate illegal transactions (Gambetta, 1996; Skaperdas, 2001; Gray, 2003; Arias, 2006; Skarbek, 2011; Acemoglu et al., 2020; Sánchez De La Sierra, 2020). More broadly, a literature on fixing failed states also emphasizes that weak states must fill sovereignty gaps and empower communities to move away from warlord rule (Ghani and Lockhart, 2009; Del Castillo, 2008; Karim, 2020).

All this raises the question of whether expanding state capacity and presence can earn the trust and engagement of citizens, and solidify the state's position as the preeminent provider of security (and a monopolist on the legitimate use of violence). Put another way: what are the returns, on the margin, to investments in state capability and penetration? The costs of hiring bureaucrats, extending public services, and expanding fiscal capacity are often immediate and clear. But what should a mayor or president expect to receive in terms of efficacy, legitimacy, and public support in the near term?

Equally important is how these returns depend on the initial levels of state strength. When it comes to state penetration and legitimacy, governments face a lot of variation in their own territory. How this affects returns to state building is unclear. On the one hand, in areas with little history of state services, the first investments might have out-sized impacts. This was our initial hypothesis in Medellin, where residents of the least-served areas initially expressed relief at finally seeing municipal bureaucrats in their neighborhoods. On the other hand, establishing thoroughgoing state governance and legitimacy might require large and sustained investments, especially from a low starting point.

This paper describes a city-wide experiment in state-building on the margin—a rare and unusual opportunity to measure the effects of sending full-time non-coercive representatives into under-served communities for an extended period.

In 2018, we worked with Medellín's municipal government, the *Alcaldía*, to identify 80 poor- and middle-income areas in need of more state governance (about half of which received moderate governance services from combos). We randomly selected 40 of these neighborhoods to receive intensive city services for two years. First, the Alcaldía created a special task force to ensure that any needs identified in these communities would get priority attention in the city's many service agencies. Second, in each neighborhood they hired a full-time "liaison"—a street-level bureaucrat whose job was to rejuvenate community government organizations, advertise and link people to government agencies, resolve disputes and dilemmas or introduce professional mediators from the city, and identify public service needs (such as garbage pickup or poor playgrounds). The liaisons would either try to solve the problem themselves, mobilize community organizations to solve it, find a government agent, or contact the task force. There was no change in state criminal justice attention.

The premise of the intervention was simple: by improving public-service delivery, providing non-criminal alternatives for dispute resolution, and strengthening the ability of local groups to identify problems and solutions to everyday community problems, the Alcaldía could increase its relative legitimacy and citizen use of its services.

This was a highly intensive increase in state presence. These were small neighborhoods, about 200–600 households, and we estimate they received a tenfold increase in street-level attention to problems for about 20 months. We monitored liaisons closely, and confirmed a high level of compliance.

Two years later, we see divergent effects depending in initial levels of state presence. On average, across the full sample, the intervention did not significantly change residents' perceptions of state governance and legitimacy—either in absolute terms or relative to the services and legitimacy of the combos. Anticipating that the policy could have heterogeneous effects, however, we pre-specified a subgroup analysis of impacts by initial levels of state governance. We find that the intervention raised people's opinions of state services and legitimacy in neighborhoods with relatively stronger initial state presence. Elsewhere, however, residents' opinions changed little.

Post-treatment qualitative interviews with community leaders, combo members, and municipal liaisons suggest why: the intervention raised residents' expectations, but where initial state presence was weak the Alcaldía failed to deliver on important promises.

Meanwhile, we can reject other explanations for these results. For instance, we see no evidence that combos reacted to the intervention, either by trying to co-opt or sabotage the liaisons. Nor do we see indications that combos tried to compete with the city for citizen loyalty by increasing their own service-provision—a strategic reaction that we do observe as a longer-run response to broad increases in state presence centered on police (Blattman et al., 2021). However, had the city government not stumbled in the neighborhoods where they were weakest (and where gangs generally provide more governance services to residents), then they may have provoked a strategic criminal response.

These results could shed light on a common feature of weak states: high government attention to places where the state is strong, and persistent neglect of the places where the state is weak. While this could be partly driven by state agents taking the path of least resistance—a principal-agent problem within the state—it could also be driven by more strategic considerations. When governments must decide where to invest their marginal effort, their short-term returns might be greatest in the places with existing state capacity. Indeed, it is possible that there are increasing returns to state capacity over some range. If there are low returns to early investments in state capacity, this too would help explain the stark heterogeneity of state presence we see in so many cities and nations. This is an important hypothesis for future research.

2 Context

Medellin is Colombia's second-largest metropolitan area, with a population of almost 4 million. 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.

Like most large Colombian cities, Medellín has a well-organized bureaucracy with high tax revenues and public services. This includes a large and professional Metropolitan Police force—a branch of the National Police, which in turn is part of the Defense Ministry. Each community also has an elected community action board that helps local groups regulate and organize their community, plus churches and other local organizations. Most public services, however, are provided by the Alcaldía and its various Secretariats. One of the largest municipal agencies is the Secretariat of Security—an organization of roughly 2,500 staff who provide numerous services to residents, including responding to various emergencies and street disorder, directly resolving community disputes and domestic violence, and regulating the use of public space. It sits directly beneath the Mayor and is the city's primary organization for setting security policy and investing in security infrastructure.

In addition to these state and community security organizations, however, many residents of Medellín can also turn to local gangs called combos for many everyday forms of governance. In a recent census of combos, we identified nearly 400 in the metropolitan area (Blattman et al., 2021). Most have 15 to 50 permanent, salaried members between the ages of 15 and 35. Their territories (often no more than a few dozen blocks) tend to be long-standing, well-defined, known to locals, and relatively stable over time. Many combos have been in their neighborhood for generations, and members come from the local area.

Based on several years of qualitative work in these neighborhoods, we identified 17 governance services that both the state and the combos commonly provide to residents and businesses. In 2019 we surveyed nearly 7,000 residents and businesses on the degree to which their neighborhood combo provides these services, as well as the perceived legitimacy of both, and levels of taxation and other payments to combos. The city is divided into roughly 250 areas called *barrios*, and the survey was representative of all 223 low- and middle-income barrios.

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

The average response for any service by either provider was seldom greater than 0.5, suggesting that neither the state nor the combo are regularly responsive. In relative terms, combo response was generally lower than the state's, but higher in five situations: muggings and theft prevention, business and household debt collection, and street fights.

These averages conceal a great deal of variation across combos and blocks, however. Figure 1 maps relative state governance by barrio. The state is present in every neighborhood, but varies in its responsiveness and penetration. 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. As a result, while the state is the dominant provider of protection in most neighborhoods, there is wide variation.

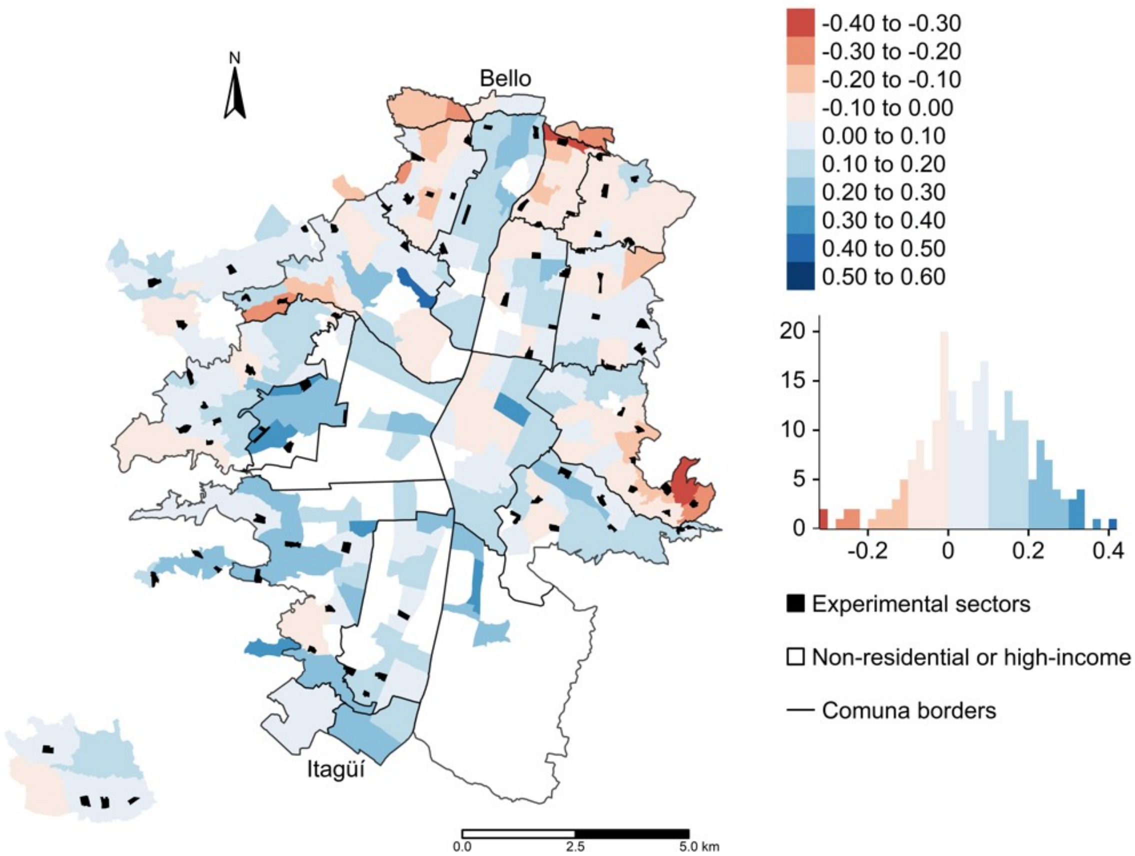
The state offers a different kind of governance, of course. The state's dispute resolution and court systems tend to be impartial and professional, and city leaders are elected in competitive elections every four years. The combo is unelected and relatively unaccountable, and provides "justice" in favor of those who hire them or who are closest to them. At the same

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

	Frequency/Rate (0-1 Scale)				Relative State – Combo	
	State		Combo		City-wide survey	Exp. sample
	Estimate	SD	Estimate	SD		
	(1)	(2)	(3)	(4)	(5)	(6)
Governance Index	0.41	0.26	0.34	0.29	0.07	0.05
How often they intervene when:						
HH: Someone is making noise	0.43	0.38	0.19	0.30	0.23	0.25
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	0.13
Biz: You have to react to a robbery	0.52	0.37	0.40	0.39	0.11	0.08
Biz: It is necessary to prevent a theft	0.45	0.37	0.38	0.39	0.07	0.04
Biz: Businesses in this sector are robbed	0.42	0.39	0.35	0.38	0.05	0.04
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	-0.00
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	-0.03
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	-0.05
HH: Kids fight on the street	0.29	0.35	0.32	0.37	-0.04	-0.04
Biz: Someone does not want to pay a debt	0.17	0.31	0.23	0.35	-0.06	-0.06
HH: Someone refuses to pay a big debt	0.22	0.31	0.39	0.38	-0.16	-0.21
Legitimacy Index	0.58	0.21	0.43	0.28	0.13	0.14
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 trust the...	0.59	0.23	0.50	0.29	0.09	0.10
How much do your neighbors trust the...	0.57	0.28	0.47	0.36	0.09	0.08

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 224 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, but only households were surveyed on legitimacy (hence there are fewer observations).

Figure 1: Experimental sectors and relative state governance by barrio



Notes: The figure displays relative state governance for 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 barrios.) We also depict the shape of the 80 experimental sectors in black. They are widely spread across the city, but note that there are none in the more central areas, which are more commercial or industrial.

time, combos have more local knowledge and deeper networks than most state bureaucrats. Combos are also available all the time, and act swiftly. Thus, 67% of survey respondents said the combo was easy to contact compared to 63% for the police and 32% for the Alcaldía. They also said the combo responded rapidly 58% of the time compared to 41% for the police and 27% for the Alcaldía.

For its services, the state collects fees and taxes from businesses and residents. Likewise, when combos provide services, they typically do so at a price. They fine people who fight or cause disturbances on the street. Companion papers describe the personnel economics and market structure of the combos (Blattman et al., 2021), and the political economy of criminal governance and taxation (Blattman et al., 2021).

Finally, both the state and the combo enjoy a reasonably high level of legitimacy in the eyes of residents. The survey asked residents how much they trust each actor; whether each actor was fair; whether residents were satisfied with each actor; and whether residents thought their neighbors trust and are satisfied with each actor. We averaged these responses into unit indexes for state and combo legitimacy, reported in Table 1. On average, residents rate their trust and satisfaction of the combo lower than the state, although the difference is not large.

3 Experimental procedures

We worked with the city to expand and experimentally evaluate an existing intervention. In 2017, we found a little-known effort in one of Medellín's under-served barrios, with a population of roughly 20,000. A small unit in the government sent 7 outreach staff to the barrio. From 2012–17, these "liaisons" set out to build and improve civil society organization and connect residents to existing city services. Based on our community interviews and observation, it appeared that citizen use of state services increased and that access to and legitimacy of the state rose. Our interviews with combo members in other parts of the city suggested that they would respond to city presence with relief, as they saw governance as a tedious and unprofitable service.

3.1 Sample

The Alcaldía identified 80 "sectors" from its low- and middle-income barrios, choosing ones with varying levels of both state and combo governance. A sector is an informal neighborhood, far smaller than a barrio, usually with about 1,000–3,000 residents and comprising 5–10 medium-density city blocks. The 80 sectors in our experimental sample are fairly representative of the variation across Medellín's low- and middle-income barrios in terms of their relative state and combo governance, as seen in column 6 of Table 1 and Appendix Figure A.1. Figure 1 displays the sectors.

We intentionally kept the sample of sectors small in order to achieve the desired level of intensity, as the city's short-term budget would limit them to hiring 40 new staff. We also wanted to minimize the possibility of spillovers, and growing the number of treated sectors would have raised the risk of contamination.

3.2 Intervention

The city intensified normal municipal services in 40 of the 80 sectors for 20 months, beginning April 2018. Control sectors received normal services.

First, at the city level, the Mayor's office created an inter-agency task force to respond to local concerns—including poor trash pickup, broken playground equipment, or a lack of attention from the city's dispute resolution officers. Relatedly, city officials also attended semi-annual formal government-community meetings in the treated sectors, known as *Consejos de Convivencia*, where they and community members would agree on a formal list of commitments. They also organized a large one-time event called *Caravana de la Convivencia*—a weekend-long street festival in each sector where, in addition to music, food, and entertainment, representatives from each agency were on hand to explain their services in detail and provide some.

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

Liaisons were similar to the city's normal professional staff: university educated men and women ages 25–35. They had weekly or monthly quotas for the above activities and were held accountable by their supervisors. The liaisons were not so much directly involved in dispute resolution and service delivery as they were advocates, a source of information, and a source of organizational capacity.

One way to characterize the intensity of this intervention is to note that normally the city has one liaison per comuna—about 1 per 540 blocks. For the intervention, the Secretariat of Security assigned one liaison to each treatment sector—about 1 per 9 blocks. In some sectors, this was the first time the sector had any direct street presence by the city government other than police. While the liaison represents a 60-fold increase in street-level staff, the broader range of city agencies and services did not increase their efforts to the same degree. Speculatively, we estimate this to be a tenfold increase in normal municipal government presence.

The aim of the intervention was to increase the visibility, accessibility, and speed of state services and improve trust in and satisfaction with the Alcaldía as a result. This approach was rooted in the conventional wisdom that unmet demand for contract enforcement and lower transaction costs opens up business opportunities for strongmen and gangs. Importantly, the aim was not to directly challenge gang rule or crowd out their services, but simply to better deliver existing city services.

Also importantly, the intervention did not affect police and criminal justice activities. There are several reasons for this, including: an interest in testing a theoretically more focused intervention; a desire to test non-coercive approaches; the basis in an existing, small-scale approach; and the fact that police and prosecutors are outside the Mayor's chain of command. The combos are far more aware of and concerned with police presence than with municipal bureaucrats.

3.3 Compliance

There was a high level of street presence and visibility of the liaisons for almost two years. The program closely monitored liaisons. They had weekly targets and quotas for neighborhood events and resident referrals, and their activities and task force responses were formally logged and geolocated. From these records we know that they spent 3–6 days or evenings per week in their sector, held frequent community events, and generally met their referral quotas, all within the few blocks they were assigned. The research team monitored and interviewed the liaisons and task force as well, and our general impression was one of autonomous, enthusiastic, hardworking efforts.

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

As for the task force and broader city government, the liaisons reported that the majority of their municipal requests were met. But we also saw some evidence that municipal agencies struggled to deliver some aspects of the 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 performance failures below.

3.4 Data and outcomes

For baseline data on the sectors, in February 2018 we surveyed three officials per sector for their assessment of: relative governance service provided by the combo and the state; relative street presence of the combo and the state; and their perceptions of local security and drug use. We also have rich, geolocated administrative data including distance to various state and criminal headquarters, crimes committed, and demographics.

Outcome data come from our December 2019 city-wide survey are summarized in Table 1 above. In addition to the representative sample of barrios, we surveyed approximately 30 residents and businesses per experimental sector. Our primary outcome is relative state governance. Relative legitimacy is our secondary outcome. We also consider the sub-components of each index (i.e. absolute state and combo levels). We pre-registered the design and outcomes in April 2018, then again prior to final data collection.[1] In addition, we pre-specified heterogeneity analysis by our baseline measure of relative state governance.

3.5 Empirical strategy

We grouped the 80 sectors into 40 matched pairs using baseline relative state-combo governance, street presence, and crime levels. We then randomized one in the pair to treatment. This produced the expected degree of balance along baseline covariates, both overall (Appendix Table A.1) and within the two major subgroups (by high/low initial relative state governance, in Appendix Table A.2).

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

$$Y_{isb} = \alpha + \beta T_s + \Theta X_{isb} + \epsilon_{isb} \tag{1}$$

[1]See the Journal of Development Economics pre-results registered report for the the final analysis plan (`https://drive.google.com/file/d/1QiEegA-GDdO34-QONMTcxe5bD6MO7nFI/view`), and the social science registry for previous rounds (AEARCTR-0002622).

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; and X is a vector of 41 baseline controls selected via double-lasso regression from a large number of potential covariates, including sector-pair fixed effects (the randomization strata) and a range of sector-, block-, and individual-level variables.[2] The majority of selected control variables are measured at the city block or survey respondent level, leaving sufficient degrees of freedom at the sector level. We cluster 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.

Minimizing the risk of 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 not reduce services in control sectors. Ex-post, we can use our representative city-wide survey to estimate and control for spillover effects, by comparing blocks close to treatment sectors to those close to control sectors. We see no evidence of systematic spillovers to neighboring areas (Appendix Table A.3).

Addressing measurement error Naturally, we are concerned that citizens may misreport gang activities. They may feel uncomfortable talking to outsiders or embarrassed to admit the role of the combo. Such measurement error could attenuate estimated treatment effects somewhat.

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

First, we can compare 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.

[2] We have a relatively modest number of experimental units. We use the double-lasso method of Urminsky, Hansen, and Chernozhukov (Urminsky et al.) to select covariates in a rules-based way. No variables were predictive of treatment (as is expected in a randomized trial) and so the algorithm selects control variables that explain variation in the dependent variable, reducing standard errors. This represents a slight departure from the pre-specified approach, which included sector-pair fixed effects and four sector-level variables in the control vector—an equally unbiased but less efficient set of estimates. We report this specification in Appendix Table A.4, discussed below.

Table 2: Program impacts on primary and secondary outcomes

	Control Mean (1)	ATE (2)	SE (3)	P-value (4)	N (5)
Relative State Governance Index	0.066	0.003	0.011	0.802	2,314
Δ State Governance Index (0-1)	0.413	0.010	0.010	0.291	2,362
Δ Combo Governance Index (0-1)	0.345	0.006	0.012	0.630	2,316
Relative State Legitimacy Index	0.131	0.025	0.020	0.227	1,845
Δ State Legitimacy Index (0-1)	0.572	0.010	0.010	0.311	1,906
Δ Combo Legitimacy Index (0-1)	0.437	-0.013	0.015	0.380	1,845

Notes: The 80 sectors in the experimental sample were blocked into pairs, and one of each pair was randomly assigned to treatment. Each row is a separate intent-to-treat estimate of program impacts. We regress each dependent variable on an indicator for treatment and a vector of 41 control variables selected through double-lasso regression. The unit of observation is the individual survey respondent, and we cluster standard errors at the sector level (the unit of randomization). Both households and businesses were surveyed on governance levels, but only households were surveyed on legitimacy (and hence there are fewer observations).

This suggests our approach was more successful in eliciting honest responses.

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

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

4 Results

4.1 Average treatment effects

Despite the length and intensity of additional state presence, we see no evidence the intervention improved opinions of the state in treated areas. Table 2 reports program impacts in the 80 sectors. We estimate a 0.003 increase in relative state governance, where the confidence interval rules out improvements in relative state governance greater than 0.025. These results are robust to alternative control vectors (Appendix Table A.4). In some specifications, the sign on treatment runs in the opposite direction (reducing perceptions of state governance) and is statistically significant at the 10% level.

Looking at the components of the relative governance index—state and combo governance—we draw similar conclusions. State governance, for example, rises by just 0.01—a 2.5% increase relative to the control group mean, not statistically significant. The confidence interval rules out increases in state governance larger than 7% of the control mean.

Turning to legitimacy, the impact on relative state legitimacy is in the expected direction but small (0.025 standard deviations) and not statistically significant. The confidence interval rules out increases in absolute state legitimacy greater than 5% of the control mean.

We see similar patterns across most of the measures that comprise these indexes (not shown). We also see no evidence of an average treatment effect on other survey measures of state efficacy, such as the speed of response and ease of accessing services (Appendix Table A.5).

4.2 Heterogeneity by baseline state governance

As we've noted, in some of the experimental sectors the Alcaldía was already active and visible on the street. In others, residents remarked that this was the first time they had seen a representative of the government in their sector who was not a policeman. Certainly there had never been meetings with officials or a municipal services fair held there before, let alone meetings focused on sector-specific problems.

We see divergent effects depending on these initial levels of state presence—the sole subgroup analysis we pre-specified. Note that we do not have representative city-wide survey data from residents at baseline, and so our pre-specified measure of initial state strength comes from baseline interviews with three community and city leaders per sector, and their assessment of the relative role of the state and the combo in providing everyday governance. Table 3 reports treatment effects in two subgroups: Panel A reports sectors with above median relative state governance; and Panel B sectors with below median relative state governance. We report the difference in treatment effects between the two subgroups in Panel C.

In the sectors where the state was already relatively strong and active, treatment is associated with statistically significant increases in relative and absolute state governance and legitimacy (Panel A). Relative state governance rises by 0.03, significant at the 5% level, and relative state legitimacy rises by 0.08, significant at the 1% level. In those places where state penetration was relatively low, however, estimated treatment effects are closer to zero and not significant (Panel B). The differences between these two subgroups are generally

Table 3: Program impacts in prespecified subgroups: High vs. low relative state governance

	Control Mean (1)	ATE (2)	SE (3)	P-value (4)	N (5)
Panel A: Above median baseline relative state governance					
Relative State Governance Index	0.117	0.031**	0.014	0.038	1,168
State Governance Index (0-1)	0.433	0.052***	0.015	0.001	1,195
Combo Governance Index (0-1)	0.313	0.020	0.017	0.258	1,169
Relative State Legitimacy Index	0.152	0.082***	0.023	0.001	926
State Legitimacy Index (0-1)	0.576	0.041***	0.013	0.003	965
Combo Legitimacy Index (0-1)	0.419	-0.039**	0.016	0.024	926
Panel B: Below median baseline relative state governance					
Relative State Governance Index	0.015	-0.013	0.018	0.470	1,146
State Governance Index (0-1)	0.393	-0.010	0.016	0.554	1,167
Combo Governance Index (0-1)	0.377	0.004	0.018	0.832	1,147
Relative State Legitimacy Index	0.110	0.011	0.017	0.511	919
State Legitimacy Index (0-1)	0.568	-0.011	0.014	0.459	941
Combo Legitimacy Index (0-1)	0.456	-0.019	0.013	0.163	919
Panel C: Subgroup difference					
Relative State Governance Index	0.066	0.045**	0.022	0.042	1,168
State Governance Index (0-1)	0.413	0.063***	0.022	0.003	1,195
Combo Governance Index (0-1)	0.345	0.015	0.024	0.532	1,169
Relative State Legitimacy Index	0.131	0.070**	0.027	0.010	926
State Legitimacy Index (0-1)	0.572	0.051***	0.019	0.006	965
Combo Legitimacy Index (0-1)	0.437	-0.020	0.020	0.339	926

Notes: The 80 sectors in the experimental sample were blocked into pairs, and each pair was classified as above- or below-median relative state governance, based on baseline surveys of community leaders. Here we report program effects in these two subgroups (Panel A and B) and the difference between the subgroups (Panel C), in a prespecified heterogeneity analysis. Each row in Panels A and B is a separate intent-to-treat estimate. We regress each dependent variable on an indicator for treatment and a vector of 41 control variables selected through double-lasso regression. The unit of observation is the individual survey respondent, and most control variables vary at the individual or city block level. We cluster standard errors at the sector level (the unit of randomization). Both households and businesses were surveyed on governance levels, but only households were surveyed on legitimacy (and hence there are fewer observations).

statistically significant (Panel C).

To get a sense of absolute magnitudes, we can look at the changes in perceptions of absolute rather than relative state governance. In sectors with higher initial state presence (Panel A) the estimated treatment effects imply a 12% increase in perceptions of state governance, compared to the control mean, and a 7% increase in legitimacy. There is virtually no change in either index in the low initial state presence sectors, and the subgroup differences are significant at the 1% level.

These results are robust to changes in our measure of initial state strength and model specification. Appendix Table A.6 shows how the treatment effects in relative state governance are similar if we use a measure of initial state presence based on a weighted average of administrative measures, including distances to state headquarters (Column 2).[3] The results are also robust to alternative control vectors (Columns 3 and 4).

4.3 Mechanisms

Our qualitative observations and program implementation data suggest that the liaisons and (especially) the task force had difficulty delivering effective governance in the low-state presence sectors, and that this could account for divergent responses. We see no evidence of combos attempting to capture the intervention or responding strategically.

Failed promises of the central state Initial relative state governance had little effect on the efforts and activities of liaisons. Panel (a) of Figure 2 uses program administrative data on all events and activities logged by the liaison. It plots the number of logged activities by baseline relative state governance. Activities are numerous and unrelated to initial government presence.[4]

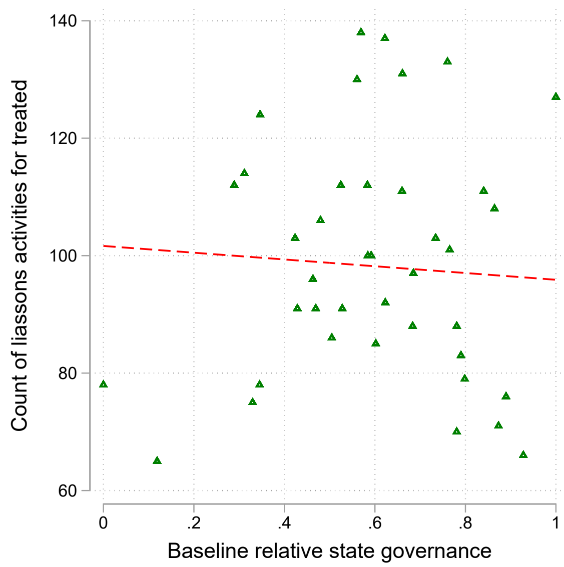
The same is not true of the Alcaldía's wider activities. Panel (b) of Figure 2 reports the frequency that the central administration failed to deliver on promises. These come from a post-program survey of liaisons. They reported task force and other central municipal government failures twice as often in the sectors with relatively low initial state presence. Most common were failures of the city government to respond to community needs. Equipment might go unrepaired, for example. Or, as one liaison explained, they organized a meeting between the community and city officials, and the officials never arrived. Another said how

[3] To create this summary measure, we run a lasso regression of endline state governance on a range of baseline measures of distance to the state, policing, and so forth, and use the estimated coefficients to predict a measure of state governance independent of treatment and then code this into an indicator for above/below median state governance.

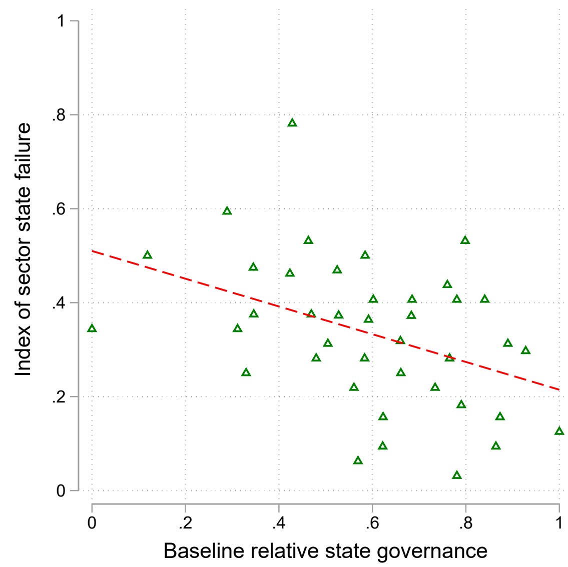
[4] In a small number of cases, the combo barred the liaison from entering the neighborhood for a period of time. These issues were typically resolved in a handful of weeks and did not affect the larger intervention.

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

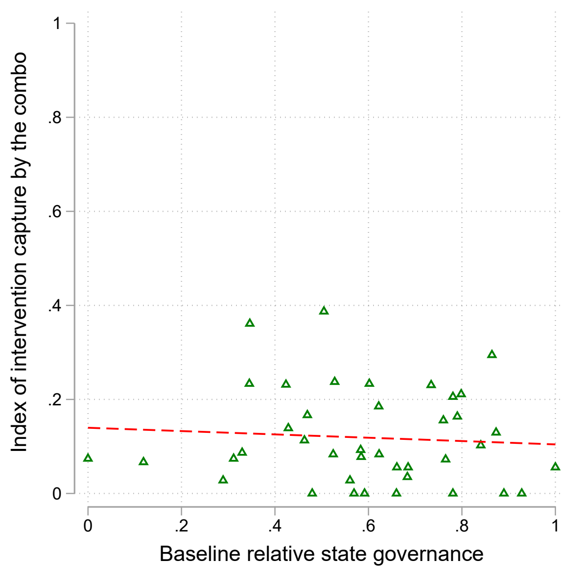
(a) *Count of treatment activities*



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



(c) *Instances of combo interference and capture*



Notes: The city required liaisons to log their activities in an online form using their mobile phone, and Panel (a) reports the number of activities they logged over the 20 months, by levels of baseline relative state governance. Panels (b) and (c) contain data from a post-program survey of all liaisons about their experiences in their sector. Based on their responses, we created two indexes of program experiences. Panel (b) reports the frequency of various failures of the liaison or the wider state apparatus to deliver on promises. This include a scale of the perceived frequency of failures from the liaison, police, and mayor's office bureaucrats and binary variables for whether specific local state agency failed. Values closer to 1 mean higher state failure. The data in Panel (c) capture the degree with which the combo interfered with liaison activities. This aggregates several measures from the liaison survey: a scale for the frequency and difficulties of interaction with local gangs; a set of binary variables on whether local actors (including the gang) took credit for the intervention activities; and a set of binary variables for activities by which the gang curtailed or helped the liaison on the interventions. Values closer to 1 represent higher involvement from locals gang on intervention activities.

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.

Criminal capture and strategic responses Qualitatively, it was clear that combos were often the first to notice an increased presence of the Alcaldía. Almost all liaisons described having to explain their presence to the combo, for example. 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. For example, Panel (c) of Figure 2, captures the degree with which liaisons reported that the combo interfered with their activities.[5] 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 Tables 2 and 3 are generally close to zero. This is notable partly because, in a companion study begun after the completion of this experiment, Blattman et al. (2021) interviewed gang leaders and learned that some see themselves as directly competing with the state for civilian loyalty. Gangs value this loyalty as it protects their drug rents. Using a simple model of imperfect competition in the market for security and protection, that paper shows that a combo solely concerned with selling protection will reduce its governance services in response to an increase in competition from the state. Once you introduce the drug market, however, and the returns to loyalty and legitimacy, the optimal response of the combo may switch, and it will elevate its governance services in response to state intervention. That paper studies a large, sustained increase in policing and municipal services over 30 years, and sees evidence that combo's respond strategically to state presence by governing more.

We see no such strategic response in the context of this short-term experiment, however. Besides the explanation that the intervention did not shift people's perceptions of state services and legitimacy, another possibility is that, given police presence did not change, the intervention did not directly threaten drug corners and other illicit rents in the same manner. Liaisons could even reduce the need for police calls. Therefore, perhaps we should not expect

[5] Our intervention capture measure aggregates the following sub components: a scale for the frequency and difficulties of interaction with local gangs, a set of binary variables on whether local actors, including the gang, took credit for the intervention activities; and, a set of binary variables for activities by which the gang curtailed or helped the liaison on the interventions. Values closer to 1 represent higher involvement from locals gang on intervention activities.

the same degree of combo strategic response compared to more coercive interventions.

4.4 Addressing measurement error concerns

Finally, could measurement error account for these results? Several factors suggest this is unlikely. First, residents in treated sectors would have needed to systematically underreport state governance or overstate combo governance, especially in the treatment sectors that had low initial government presence. Their motive for doing so is not apparent. Second, they would also have to do so with an independent survey firm that had been conducting citizen security surveys for half a decade, and that residents had no reason to associate with the intervention. Appendix Table B.1 tests for correlations between treatment status and our proxies for measurement error—non-response to combo-related questions relative to responses about the state, and the randomized response survey experiment. We do not see significant evidence of misreporting correlated with treatment.

5 Discussion and conclusions

Nearly two thirds of the world lives in cities. Municipal governments typically provide the most basic and everyday public services—lights, water, property rights, and security and public order. Many neighborhoods receive few government services at all. A natural question is why. In principle, elected officials ought to have incentives to offer voters these services—especially when these services already exist, and under-served communities merely need to learn to access them.

This experience of Medellín suggests one possible explanation for the variation in state penetration: there may be nonlinear returns to investments in state building. In particular, returns to some of the initial investments may be low, to the extent that local experience, presence, structures and accountability are important for effective service delivery.

A related possibility is that the coercive arm of the state needs to increase as well—meaning that municipal services alone are not enough, or are not the main source of state legitimacy. This would be consistent with a literature on counter-insurgency, which has argued that a combination of military action followed by state service provision increases state legitimacy and civilian collaboration against the insurgents (Albertus and Kaplan, 2013; Berman et al., 2011, 2013; Berman and Matanock, 2015). On the other hand, increased policing could provoke a strategic response from competing armed actors, as with combos in Blattman et al. (2021), attenuating the effects on state relative governance and legitimacy.

Altogether these hypotheses suggest that, in the places a state is weakest, building capacity and legitimacy may require longer, larger, and multifaceted interventions than previous expected. In other words, there may be increasing returns to investments in weak state capacity. If so, short-term electoral incentives may actually run against investing in state capacity, most of all in the neighborhoods with the weakest state presence. Governments and international organizations may need to think more about how to design institutions, term limits, federal aid, and international assistance to incentivize long run investments in state capacity and reducing the influence and legitimacy of other armed actors in society.

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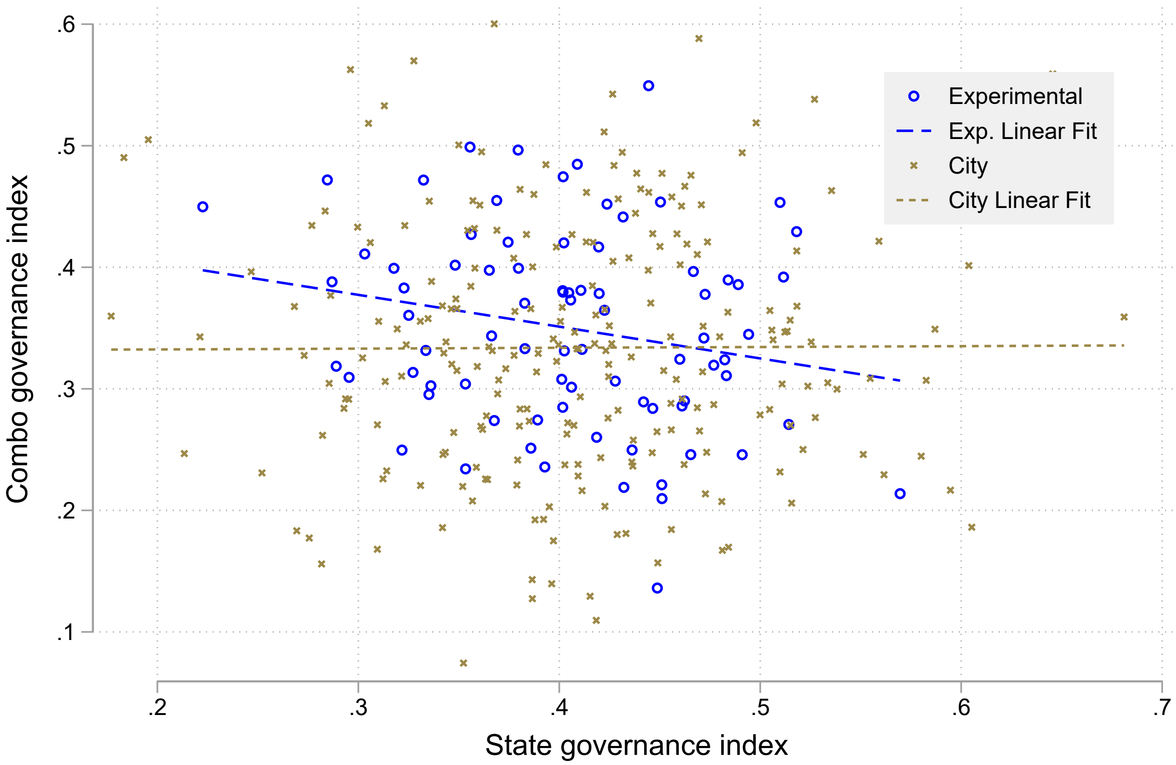
Appendix

A Supplemental analysis

Figure A.1: Endline governance levels in the representative city sample and the experimental sample



Notes: The figure plots average 2019 state and combo governance levels in each city barrio as well as the 80 experimental sectors. 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.

Table A.1: Baseline summary statistics and test of balance

Covariate	Means		Regression Difference		
	Control	Treated	Coeff	p-value	SE
Additive index of combo presence and governance	0.00	-0.02	-0.02	0.92	0.22
Baseline - Combo Governance Index (Relative to State)	0.00	-0.02	-0.02	0.91	0.22
Standardized index of perceived insecurity and drugs	0.06	-0.07	-0.13	0.58	0.22
Index of crime	0.09	-0.12	-0.21	0.35	0.22
Index of distance from public goods and services	-0.14	0.14	0.28	0.22	0.22
Respondent age between 18 and 25	0.19	0.19	-0.00	0.98	0.02
Respondent age between 26 and 40	0.29	0.31	0.01	0.52	0.02
Respondent age between 41 and 64	0.39	0.37	-0.01	0.58	0.02
Respondent is business owner	0.20	0.20	0.00	0.90	0.00
Multidimentional Poverty Index (2018)	14.34	17.26	2.93	0.20	2.26
Block Longitude	-75.58	-75.58	-0.01	0.37	0.01
Block present in 1970	0.50	0.44	-0.06	0.54	0.10
Median age (2005)	27.20	26.31	-0.90	0.41	1.08
Total women (2005)	133.86	142.04	8.18	0.53	12.90
Total non-mestizo polulation (1993)	0.53	0.61	0.08	0.66	0.18
Median age (1993)	24.16	24.71	0.56	0.60	1.06
Share of women (1993)	0.53	0.52	-0.01	0.68	0.01
Distance to the respective razon headquarters (100 meters)	17.28	19.12	1.84	0.66	4.15

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 four variables) and for covariates selected by the double-lasso method as prognostic of endline relative 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.

Table A.2: Balance by pre-specified subgroups

Covariate	High relative state gov.				Low relative state gov.			
	Control mean	Treatment mean	Coeff	p-value	Control mean	Treatment mean	Coeff	p-value
Additive index of combo presence and governance	-0.57	-0.63	-0.06	0.764	0.59	0.61	0.03	0.932
Baseline - Combo Governance Index (Relative to State)	-0.69	-0.75	-0.06	0.718	0.70	0.72	0.02	0.950
Standardized index of perceived insecurity and drugs	0.07	-0.07	-0.14	0.654	0.05	-0.06	-0.11	0.738
Index of crime	0.01	-0.17	-0.18	0.535	0.17	-0.06	-0.23	0.493
Index of distance from public goods and services	-0.21	0.17	0.39	0.268	-0.06	0.10	0.16	0.563
Respondent age between 18 and 25	0.18	0.18	-0.00	0.852	0.20	0.21	0.00	0.891
Respondent age between 26 and 40	0.26	0.31	0.05	0.103	0.33	0.30	-0.02	0.457
Respondent age between 41 and 64	0.42	0.36	-0.05*	0.083	0.35	0.38	0.03	0.452
Respondent is business owner	0.20	0.20	0.00	0.122	0.20	0.19	-0.00	0.225
Multidimentional Poverty Index (2018)	11.75	12.93	1.19	0.645	16.98	21.73	4.75	0.171
Block Longitude	-75.59	-75.59	-0.00	0.934	-75.57	-75.58	-0.01	0.225
Block present in 1970	0.60	0.51	-0.09	0.479	0.40	0.38	-0.03	0.846
Median age (2005)	29.19	27.61	-1.58	0.315	25.18	24.96	-0.22	0.869
Total women (2005)	135.53	142.93	7.40	0.696	132.15	141.12	8.96	0.619
Total non-mestizo polulation (1993)	0.58	0.20	-0.38*	0.095	0.49	1.04	0.55**	0.032
Median age (1993)	25.80	26.42	0.62	0.702	22.49	22.96	0.47	0.692
Share of women (1993)	0.52	0.51	-0.00	0.855	0.54	0.53	-0.01	0.603
Distance to the respective razon headquarters (100 meters)	17.86	23.49	5.63	0.456	16.69	14.62	-2.07	0.528

Notes: This table reports treatment and control group means and a test of balance for all covariates in Table A.1, but does so within the two pre-specified subgroups: above and below median baseline relative state governance.

Table A.3: 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	0.0003	0.9940	-0.0341	0.8100
Δ State Governance Index (0-1)	0.0103	0.4900	0.0218	0.3240
Δ Combo Governance Index (0-1)	0.0083	0.5710	0.0521	0.1940
Relative State Legitimacy Index	0.0041	0.8900	-0.0317	0.1520
Δ State Legitimacy Index (0-1)	0.0089	0.4590	0.0119	0.8710
Δ Combo Legitimacy Index (0-1)	-0.0042	0.8230	0.0261	0.3670

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.4: Robustness of experimental results to changes in the control vector

	Specification of control vector		
	Main ("Long" lasso control vector incl. sector-pair dummies)	Control vector omits sector-pair FE	Controls incl. sector-pair FE & pair matching vector only
	Estimate (SE) (1)	Estimate (SE) (2)	Estimate (SE) (3)
Governance variables			
Relative State Governance Index	0.002	-0.001	-0.024*
	(0.011)	(0.016)	(0.014)
State Governance Index	0.009	0.005	-0.013
	(0.010)	(0.012)	(0.009)
Combo Governance Index	0.005	0.005	0.009
	(0.012)	(0.015)	(0.012)
Legitimacy variables			
Relative State Legitimacy Index	0.016	0.022	0.017
	(0.022)	(0.025)	(0.019)
State Legitimacy Index	0.010	0.016	0.010
	(0.010)	(0.012)	(0.007)
Combo Legitimacy Index	-0.013	-0.011	-0.006
	(0.015)	(0.018)	(0.014)

Notes: Column (1) replicates our main specification, as reported in Table 2, which includes more than 40 covariates selected by lasso from a wide set of block and survey respondent characteristics, as well as 40 sector-pair fixed effects. Column (2) reports treatment effects where the covariate selection method does not include sector-pair fixed effects. Column (3) does not use the double-lasso method of covariate selection, but rather includes as covariates only the 40 sector-pair fixed effects and the four covariates used to pair sectors.

Table A.5: Impacts of treatment on survey measures of state efficacy

	Control Mean (1)	ATE (2)	SE (3)	P-value (4)	N (5)
State efficacy index	0.505	-0.007	0.010	0.525	1,907
How easy is to contact the combo	0.453	-0.013	0.014	0.362	1,881
How would this sector be without the combo	0.683	0.001	0.011	0.951	1,880
How fast is the State	0.381	-0.009	0.017	0.612	1,879
Combo efficacy index	0.547	-0.017*	0.010	0.088	1,790
How easy is it to contact the combo	0.591	-0.003	0.016	0.865	1,649
How would this sector be without the combo	0.524	-0.021	0.016	0.192	1,706
How fast is the combo	0.560	-0.022	0.019	0.252	1,589

Notes: This table calculates the treatment effects on 6 measures of efficacy, and indexes constructed for these measures, using the same approach as in Table 2.

Table A.6: Subgroup analysis: Robustness to changes in subgroup indicator & controls

	Specification of het variable / control vector			
	Main ("Long" lasso control vector incl. sector-pair dummies)	Alternate subgroup indicator using predicted state gov.	Control vector omits sector-pair FE	Controls incl. sector-pair FE pair matching vector only
	Estimate (SE) (1)	Estimate (SE) (2)	Estimate (SE) (3)	Estimate (SE) (4)
Relative State Governance Index				
High initial state subgroup	0.031**	0.040**	0.042**	-0.023
	(0.014)	(0.016)	(0.020)	(0.020)
Low initial state subgroup	-0.013	-0.029**	-0.039*	-0.028
	(0.018)	(0.014)	(0.022)	(0.017)
Subgroup difference	0.045**	0.072***	0.080***	0.006
	(0.022)	(0.021)	(0.029)	(0.025)
Relative State Legitimacy Index				
High initial state subgroup	0.082***	0.016	0.089***	0.040**
	(0.023)	(0.030)	(0.030)	(0.020)
Low initial state subgroup	0.011	0.018	-0.022	-0.018
	(0.017)	(0.020)	(0.025)	(0.031)
Subgroup difference	0.070**	-0.003	0.109***	0.057
	(0.027)	(0.035)	(0.038)	(0.036)

Notes: Column 1 replicates our main subgroup analysis, as reported in Table 3. Column 2 reproduces the same analysis, using a different method for identifying subgroups. Instead of using a *relative* state measure, we attempt to generate a measure of absolute levels of state governance by creating a weighted average of distance to state headquarters and other baseline administrative and survey data. Specifically, we use machine learning methods to identify the baseline covariates more prognostic of endline absolute state governance. We use the coefficients to predict and index, then coarsen this measure into an above/below median measure of state governance. Finally, Columns 3 and 4 reproduce the estimates in Column 1 with the same alternative control vectors as in Table A.4 above.

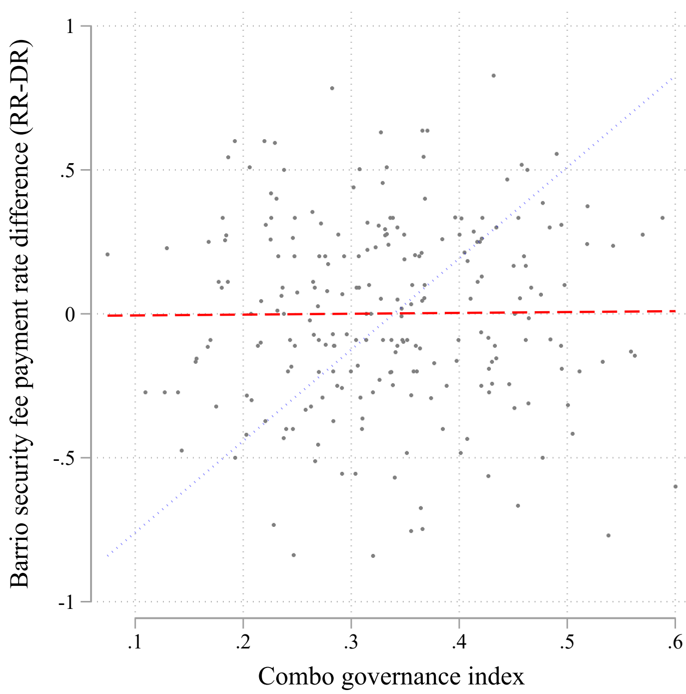
B Assessing measurement error in outcomes

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

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

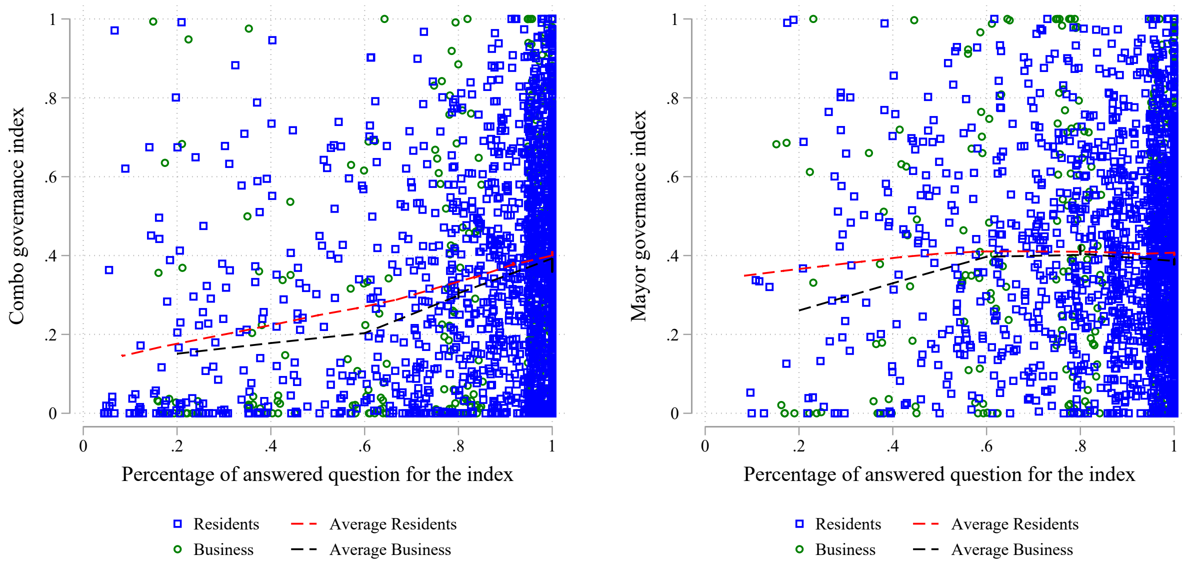
In Figure B.1 we calculate the difference between the RR and DR methods at the barrio level, and plot this difference against combo governance levels. A simple regression line is relatively flat at zero, indicating that misreporting is no more or less common in areas where the combos are more involved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

Figure B.1: 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.

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



Notes: The survey asked about 17 forms of combo and state governance, and the figure plots the percentage of questions each survey respondent answered of these questions by the barrio-level average of combo and state governance.

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

Table B.1: Treatment-control differences in potential indicators of measurement error

	Control Mean (1)	ATE (2)	SE (3)	P-value (4)	N (5)
Extortion rates					
Sector vacuna payment rate difference (RR-DR)	0.041	-0.051	0.053	0.339	80
Proportion of questions answered					
Proportion of questions answered for relative state governance index	0.033	-0.003	0.008	0.690	80
Proportion of questions answered for mayor governance index	0.891	-0.005	0.011	0.632	80
Proportion of questions answered for combo governance index	0.858	-0.002	0.013	0.876	80
Proportion of questions answered for relative state legitimacy index	0.082	-0.001	0.011	0.895	80
Proportion of questions answered for state legitimacy index	0.971	-0.013	0.006	0.056	80
Proportion of questions answered for combo legitimacy index	0.889	-0.011	0.014	0.420	80

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