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DO POLICE-COMMUNITY MEETINGS WORK? EXPERIMENTAL EVIDENCE FROM MEDELLÍN*



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Do Police–Community Meetings Work? Experimental Evidence from Medellín

Draft chapter for edited volume.

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1 Introduction

In early September, 2020, ten days before the deadline for revisions to this volume, Colombians participated in the biggest anti-police protests in decades. Like the protests that swept the United States just months earlier, they were sparked by the heinous murder of a civilian by a police officer, caught on cell-phone video (Dickinson, 2020). Like the U.S. protests, they were fueled by frustration about underlying social, economic, and political conditions, including a president who fervently defended the police and demonized protesters (ibid). And as in the United States, mobilization in Colombia pushed politicians to consider overdue changes—such as trying officers in civilian court rather than in the military justice system (Doria, Flórez Arias, and Galvis, 2020).

In some ways, the explosion of anti-police sentiment in Colombia was unsurprising. Accusations of police abuse had ticked up in recent months (BLU Radio, 2020). Cell phones rendered abuse newly visible. A year of generalized unrest created a combustible environment. Yet the protests also arrived after decades of improvement in public safety and the quality of police services. Between the early 1990s and 2020, the Colombian homicide rate plummeted, as did police

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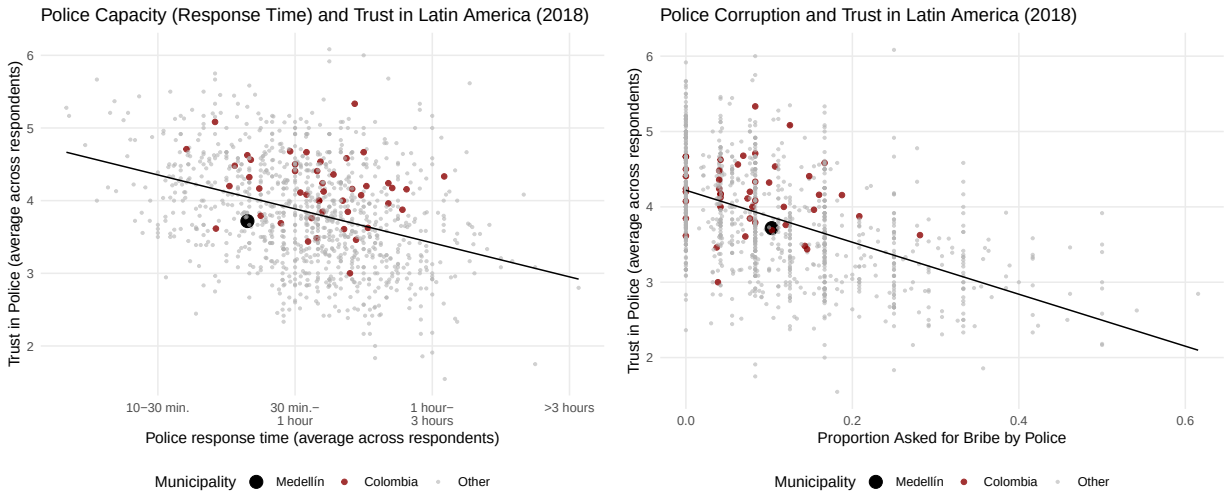
violence (see below). One might have expected these changes to inspire trust in the police, given what we know about the connection between overall government performance and trust in government (Levi and Stoker, 2000). But police–community relations remain contentious at best—not only in the capital, Bogotá, where the protests were largest, but also in our study site, the Colombian city of Medellín.

We investigate the effect of town-hall-style meetings on police–community relations in Medellín. A city of 2.5 million, Medellín exemplifies the national experience of improvements in safety and policing over recent decades. In the early 1990s, Medellín suffered the highest homicide rate of any major city in the world, around 350 per 100,000; today, the homicide rate is nearly 90% lower, about 35 per 100,000 (Durán-Martínez, 2018). To put this change in perspective, consider that during the "Great Crime Decline" in the United States in the early 1990s—the subject of hundreds of academic papers—the homicide rate dropped from approximately 10 to 5 per 100,000.

Policing improved in tandem. As recently as the early 1990s, the Colombian police committed hundreds if not thousands of extra-judicial murders every year (Amnesty International, 1994); today, the Colombian police rank among the least violent in Latin America (Correa, Forné, and Cano, 2019, p. 166). Serious reform efforts began in the mid-1990s (Gonzalez, 2019) and continued through the development of community policing strategies (Bulla et al., 2012) and extensive officer training (Garcia, Mejia, and Ortega, 2013). Moreover, according to survey data, the police in Medellín respond to requests faster than the majority of their counterparts elsewhere in the region (Figure 1a) and they more seldom request bribes (Figure 1b).

This is not to paint a rosy view of the police in Medellín or in Colombia as a whole. Colombian police officers continue to engage in corrupt practices and commit grave human rights abuses, including a disproportionate response to Colombia's recent wave of protests (UNHCHR, 2020, p. 9, 10, 15–16). Rather, the Colombian police are more effective and less violent than they used to be—and they are far less violent than the police elsewhere in Latin America (e.g. Correa, Forné, and Cano, 2019; UNHCHR, 2019; Magaloni, Franco-Vivanco, and Melo, 2020).

Yet public perception of the police in Medellín has changed little. Survey data reveal remark-



(a) Response time and trust in police

(b) Corruption and trust in police

Figure 1: Using data from LAPOP aggregated to 1138 municipalities in 18 Latin American and Carribbean countries, these figures show that (a) the police in Medellín respond to complaints faster than the majority of their counterparts elsewhere in Colombia and in Latin America and that (b) they more seldom request bribes, but that citizens' trust in officers is lower than either of these factors would predict.

able persistence in public perceptions. A 1992 survey of residents found that 41% of residents felt "distrust or fear" upon encountering the police (Restrepo Riaza et al., 1994); by 2017, despite the revolution in public safety and police services in Medellín, 53% of residents in our baseline survey still reported having "no" or "little" trust in police. This is all the more puzzling because crime has been linked to (dis)trust in government not only in general, but in Colombia in particular (Blanco and Ruiz, 2013). And as Figure 1 reveals, residents' trust in officers in Medellín is lower than their relative performance would predict.

Can informal interaction between police officers and city residents help close this gap? The large qualitative and descriptive literature on community policing in general—and on police–community meetings in particular—is inconclusive. Some studies suggest that "providing an opportunity for police and residents to get acquainted" can help build trust (Skogan, 2006). Yet many other scholars find that community policing initiatives can be ineffective or even counterproductive (e.g. Ungar and Arias, 2012). Qualitative literature suggests that community involvement is a key feature of successful reform efforts (Arias and Ungar, 2009), but (to the best of our knowledge)

there is little experimental evidence one way or the other (though see Peyton, Sierra-Arévalo, and Rand, 2019).

We use a large-scale field experiment to evaluate the effect of police–community meetings on residents' beliefs about police, police officers' beliefs about residents, crime reporting behavior, and crime rates. Working with the police in Medellín, we organized a series of three police–community meetings in each of 174 small neighborhoods across the city over a period of nine months. We compare the evolution of residents' beliefs (from baseline to endline) to changes in the beliefs of their counterparts in 173 neighborhoods without police–community meetings. We also surveyed police officers at endline, allowing us to compare the attitudes and beliefs of officers assigned to police–community meetings against those of other officers.

We find that these police–community meetings generally improved citizens' beliefs about the police. In particular, residents of neighborhoods assigned to police–community meetings became more likely to say that the police have the capacity to respond to incidents in a timely manner and to investigate crimes. These differences are substantively small, but statistically significant even after adjusting for multiple comparisons. Residents of treated neighborhoods also became slightly (though not significantly) more likely to to say that police have good intentions: that they take cases seriously and treat people fairly. These results make sense given the focus and content of the meetings. Officers were able to explain how they typically handle problems like noise violations and trash in the streets (two frequently discussed issues), thus providing residents with illustrations of their capacity to handle neighborhood problems.

On the other hand, using the estimators specified in the meta-pre-analysis plan, we find no evidence of effects on crime, crime reporting, police abuse, or officer beliefs. Our results therefore provide little support for the "grand hypothesis that community policing is effective" (Blair, Christia, and Weinstein, 2020), though this may very well be due to analytic limitations of the Colombian context. As noted in our own pre-analysis plan (Arias et al., 2020), our neighborhoods (the units of analysis) were very small, such that local crime incidence was highly variable—so much so that we were powered only to detect implausibly large changes in safety. We therefore

interpret our null results on crime and crime reporting as the natural consequence of our research design, rather than as evidence against a key part of the theory proposed in this volume.

Like the other studies in this Metaketa, ours entailed complex ethical issues. In our context, the principal risks related to the security of officers, residents, and our own study team. We discuss our approach to risk mitigation in detail below.

2 Context

We study the effectiveness of police–community meetings using a large-scale field experiment in the city of Medellín, Colombia. The Colombian police of the year of our intervention (2018–19) were very different from their notoriously violent and corrupt predecessors of the 1980s. Gonzalez (2019, p. 71) summarizes the "dramatic institutional decay" of the Colombian police during that decade: because of Colombia's civil war, the National Police grew by more than half, which entailed lower recruitment and training standards; thousands of officers were killed, and thousands of citizens died at the hands of police. Direct funding from the U.S. government gave the police the clout they needed to resist reform. Even as then-President César Gaviria spearheaded a successful effort to re-found the Colombian state—including but not limited to rewriting the constitution—his administration left the police largely untouched, trotting out the phrase "rotten apples" in response to complaints (ibid, p. 73).

This began to change in 1993. The rape and murder of a nine-year-old girl *inside a police station* united Colombia behind a call for reform (Gonzalez, 2019, p. 76). The resultant legislation, Ley 62 of 1993, constituted "an effort to demilitarize and democratize the police based on . . . community-policing concepts in vogue internationally" (Llorente, 2006, p. 111). This included strengthening civilian (as opposed to military) oversight; opening an Office of Community Participation; distinguishing the urban, civilian, crime-oriented police from the rural, militarized, insurgency-oriented police; and increasing the ratio of supervisors to rank-and-file officers. Parts of the law were implemented successfully; other parts, less so. But overall, the 1993 legislation

kickstarted a decades-long reform process that remade the National Police.

While many of the changes focused on counternarcotics strategies, an important tenet of the reform process was the gradual incorporation of more and more elements of community policing. Already by the 1980s, the National Police had established Immediate Service Centers (*Centros de Atención Inmediata*, or CAIs) in Colombia's big cities; in the 1990s, CAIs spread throughout the country. The idea was to bring officers (physically) closer to the community, though the effort was only partially successful (where police were unable to maintain the CAIs, they became a liability). In 1995, the police began organizing community watch groups (Llorente, 2006, p. 128). In 1998–99, the Bogotá division of the National Police implemented a community policing pilot program, with meaningful training for officers (Vásquez, 2012).

In 2010, the police took a significant step toward problem-oriented and community policing with the National Plan for Beat-Based Community Patrol (*Plan Nacional de Vigilania Comunitaria por Cuadrantes*, PNVCC) (Garcia, Mejia, and Ortega, 2013; Bulla et al., 2012; Policia Nacional de Colombia, 2010). For the first time, the police divided Colombian cities into small geographical areas called *cuadrantes* (literally *quadrants*, but meaning *police beats*). Patrolmen were to work their beats for two full years. Rather than reacting when crime occurred, patrolmen were meant to (a) diagnose the biggest security problems in their beats, (b) design a plan for addressing those problems, and (c) evaluate the result (Garcia, Mejia, and Ortega, 2013, p. 5). The Plan also introduced technology to facilitate this process, such as software visualizing geo-coded criminal activity. A training program helped thousands of officers develop "soft skills" for effective community engagement. In short, *Plan Cuadrantes* encouraged a preventive, problem-oriented, decentralized approach to urban policing in Colombia.

The community-policing initiatives of the Colombian police have had salutary effects. Garcia, Mejia, and Ortega (2013), for example, used randomized timing of a training program to find that "a simple and inexpensive training intervention focused on the quality of the relationship between the police and their community can go a significant way in improving citizen security indicators" (p. 13). Yet Medellín and Colombia continue to face severe security challenges, and the

police continue to commit human rights violations. Despite the dramatic and sustained drop in the homicide rate since the early 1990s, there were approximately 25 murders per 100,000 in 2019—more than twice the World Health Organization's threshold for "endemic violence." Extortion affects a significant fraction of businesses in Medellín. And despite or perhaps because of the ongoing peace process, Colombia has also recently suffered a wave of targeted killings of social leaders and human rights defenders (Prem et al., 2018). The United Nations Office of the High Commissioner for Human Rights documented at least five cases of "arbitrary deprivation of life" by the police in Colombia in 2019, as well as cases of torture, sexual violence, and violence against protesters (UNHCHR, 2020, p. 9, 15–16).

Our policing partners in Medellín—formally, the Policía Metropolitana del Valle de Aburrá (MEVAL)—are a metropolitan division of Colombia's National Police, which subsumed local police forces in the early 1960s (Llorente, 2006, p. 113). Though MEVAL is a division of the national force, the city government has shared jurisdiction over MEVAL policies. This is in part because the Colombian constitution grants mayors authority over local policing; it also stems from the fact that a significant fraction of the MEVAL budget (and the majority of the non-salary budget) comes from the city government. The city's Security Secretariat works closely with the police to define new initiatives and priorities for MEVAL. Relative to several other sites in this Metaketa, the Colombian police have substantial resources: a budget of approximately $18,000 per officer per year, access to motorcycles (and some vehicles), and a sophisticated crime-tracking data center (in the city government, but working closely with the police), among other resources.

Our intervention constituted a natural extension of Colombia's longstanding community policing orientation. Twenty-five years into a fitful but ultimately transformative reform process, and eight years in to the implementation of beat-based patrols and problem-oriented poling, our partners in the Medellín division of the National Police implemented regular, neighborhood-level police–community meetings.

Table 1: Intervention design

Condition	Town hall meetings	Foot patrols	Problem-oriented policing	Citizen feedback	Watch forum
Control	None	Daily	Yes	Hotlines, app	No
Treatment	Bi-monthly	Daily	Yes	Hotlines, app	No

3 Intervention

Because of these previous community policing initiatives, our study site—Medellín—already had three of the five components of the common arm treatment: foot patrols, problem-oriented policing, and mechanisms for citizen feedback (see Table 1). Our intervention implemented a fourth: town-hall meetings.

Even prior to our intervention, police in Medellín had occasionally convened a different type of police–community meeting; high-ranking officers (such as station chiefs) would address residents from across large swaths of the city. But the meetings associated with our intervention involved beat officers and were organized at the level of small neighborhoods. This hyper-local focus was designed to facilitate relationships among citizens and the agents with whom they would otherwise interact: the police officers whom they would be most likely to encounter outside of the intervention.

The police–community meetings entailed town-hall-style discussion among patrolmen and neighborhood residents. While there was considerable variation across the 456 meetings organized as part of the intervention, most meetings took place in community centers in the early evening. Patrol officers were the most frequent representatives of the police, though it was not uncommon for higher-ranking officers to attend as well. Officers generally arrived to find anywhere from three to seventy residents seated in folding chairs; the vast majority were women. Meetings typically began with a short introduction or presentation from the officers, in which they talked about the beat-based approach to policing and provided updates on local security concerns.

Most of the remainder of the meeting time—anywhere from forty minutes to two hours—was devoted to questions and answers. Residents asked officers questions (some friendly, others hostile) and often lodged complaints, about anything from noise violations to armed gangs to slow

responses from the police themselves. Other common topics included homeless people sleeping on the streets or begging, drug consumption in public places, and the influx of Venezuelan migrants. Often, community leaders (members of the *Junta de Acción Comunal*) were in attendance. This format allowed residents to gauge police responsiveness to their concerns and to learn about the efforts and responsibilities of the local (beat-level) police officers.

Most meetings concluded with the signing of a *Cooperation Agreement*, in which both officers and residents agreed to take specific, concrete actions toward addressing problems in the neighborhood; these agreements were revisited at the following meeting. Our team of local research assistants helped officers moderate the meetings and took qualitative notes about the interactions.

To complement the police–community meetings treatment, we also implemented an informational campaign aimed at improving residents' knowledge of police responsibilities and activities, improvements in citizen security, and resources for reporting domestic violence. We describe this second treatment in more detail in Arias et al. (2020). Critically for the analysis of the community policing treatment arm—the focus of this chapter—we do not anticipate complementarities or substitutability between the the two treatment arms.

This intervention posed important ethical concerns. In our context, the principal risk was that traveling to the meetings—generally held in the evenings—would expose officers, residents, or our own study team to crime victimization that they might not otherwise encounter. We mitigated this risk primarily by preemptively cancelling meetings in treated neighborhoods when our counterparts perceived increased risk. (Overall, 66 meetings were cancelled; we describe these exclusions in more detail below.)

A secondary ethical concern was that officers would spend valuable time attending meetings *rather than* providing other services like vehicle or foot patrols. The design of the intervention was informed by preexisting priorities and objectives of the police and the city government in Medellín. Our responsibility was to make the meetings as effective as possible—and, ultimately, to provide evidence with which our counterparts could better evaluate these tradeoffs. The concluding chapter of this volume provides additional discussion of ethics.

Table 2: Treatment conditions

		¬ Leaflets $\neg T_f$	Leaflets T_f
¬ Community Meetings	$\neg T_m$	(Control, $Z_\emptyset$) $N = 87$	Z_f $N = 87$
Community Meetings	T_m	Z_m $N = 87$	Z_{mf} $N = 86$

4 Research Design

4.1 Sampling and treatment assignment

We randomized the intervention across police beats (*cuadrantes*). The total number of police beats in Medellín (at the time of the design) was 413; we excluded from our sample 66 beats that were either (a) located in remote areas of the city or (b) non-residential (e.g., the local airport).

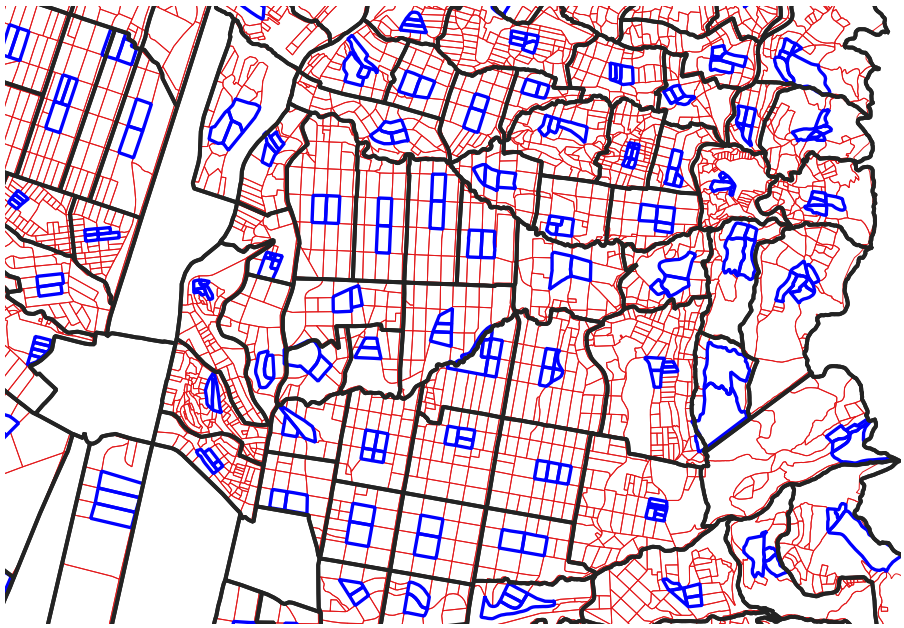
Within each of the remaining 347 police beats, we defined a *prioritized neighborhood* as the set of inhabited, contiguous city blocks closest to the centroid of the police beat. Each prioritized neighborhood comprised about four blocks, depending on the residential density, so as to ensure similar populations. When the centroid of the police beat fell in (for example) a park, we began the prioritized neighborhood at the inhabited block closest to the centroid. Police beats had an average of 5,348 residents (in the 2005 census), our prioritized neighborhoods contained approximately 1,200 residents, or about 400 households. Figure 2 illustrates the location of prioritized neighborhoods located a sample of police beats. It is within these prioritized neighborhoods (henceforth, just *neighborhoods*) that we delivered invitations and informational flyers to 350 households.

To assign police beats (and thereby neighborhoods) to treatment conditions, we block randomized. Each block contained four police beats that (a) belong to the same police station group ($n = 11$)[1] and (b) have the same treatment status (treated or control) in a simultaneous intervention conducted by other researchers. (In other words, we cross-randomized with another trial

[1]There are fourteen police stations, but some of the stations are relatively small, and thus we grouped 6 of these into 3, for a total of 11 police station groups. The fourteen stations are Aranjuez, Belén, Buenos Aires, Candelaria, Castilla, Doce de Octubre, Laureles, Manrique, Poblado, Popular, San Antonio de Prado, San Javier, Santa Cruz, and Villa Hermosa; we grouped Aranjuez with Manrique, Buenos Aires with Vila Hermosa, and Popular with Santa Cruz.

Figure 2: Visualization of Treated Units

A visualization of the *manzanas* that were targeted in beats assigned any non-control arm. Police beats or *cuadrantes* are outlined in black. The prioritized blocks are outlined in blue.



taking place in Medellín). Within each block, we randomly assigned one police beat to each of the four treatment conditions (see Table 2).[2] Figure 3 maps our sample of police beats and their treatment assignment. This simple blocking strategy is sufficient to produce balance on all observable demographic, socioeconomic, and crime characteristics, as shown in Appendix Table A1.

We summarize our research design in Figure 4.

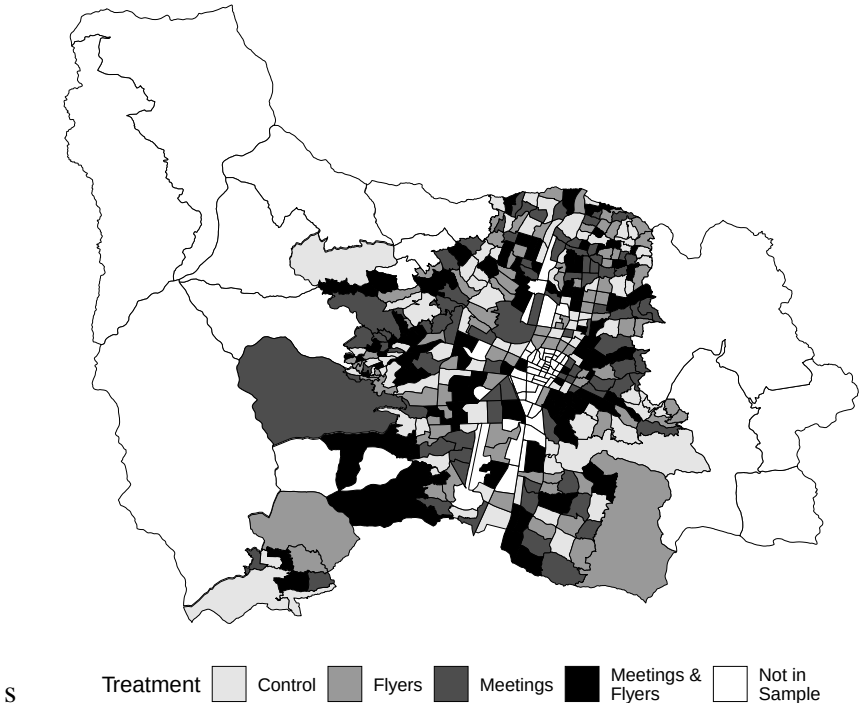
4.2 Data and Estimation

Data. To evaluate the effects of the intervention, we rely on a baseline survey, a panel survey, administrative data on crime reporting, and an endline survey of police. Because the hypotheses we test are the same as those tested in other studies in this volume, we do not reprint them here; however, Table 2 summarizes how Colombia ranked (at baseline) on each outcome.

Several values are important for interpreting our results. First, perceptions of police capacity

[2]There is one block of three police beats, each of which we assign to one of the four conditions with equal probability.

Figure 3: Map of police beats and their treatment assignment in Medellín.



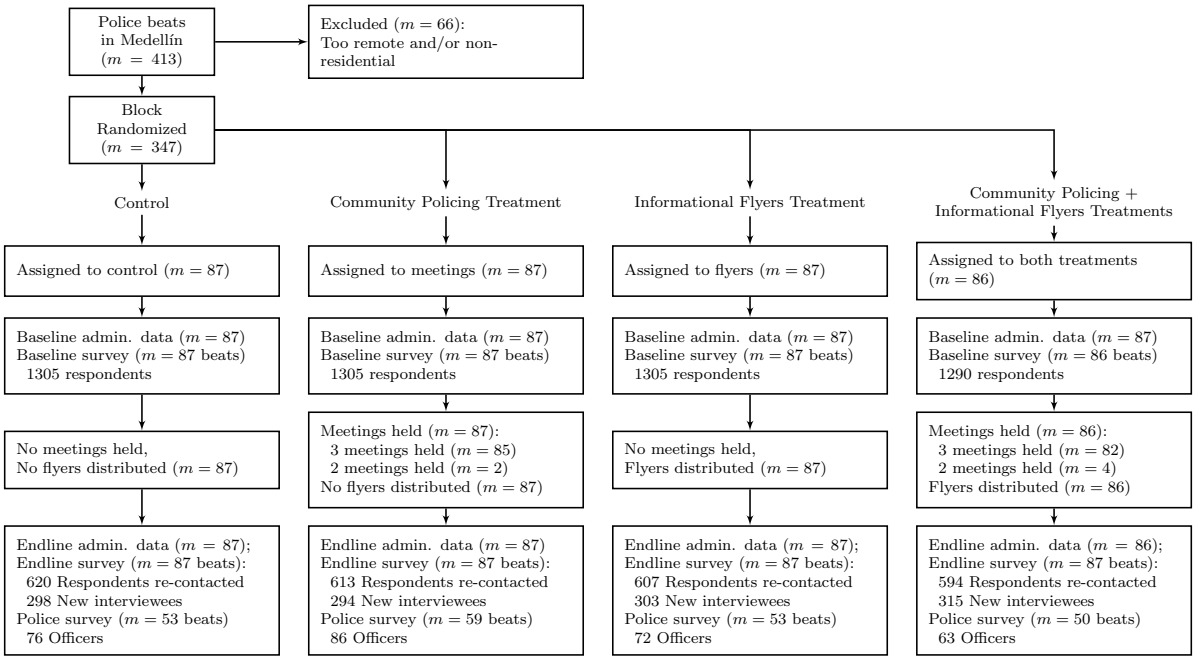
in Colombia were *lower* at baseline than in any other study site. While the meta-analysis does not explicitly look at treatment-effect heterogeneity by prior belief, we might expect larger effects where people initially had more pessimistic views. The same is true of perceived state legitimacy and community trust: they are lower at baseline in Colombia than in other study sites. Second, the treatment effects presented below are standardized; the standard deviations reported in Table 2 permit translation into substantive magnitudes. (We return to this below.)

Citizen Survey. For outcomes measured using our endline survey of citizens, we estimate:

$$Y_{ijb}^{t=1} = \nu_m T_{ij}^m + \nu_f T_{ij}^f + \boldsymbol{\gamma}_b + \delta \widetilde{\mathbf{Y}}_{ijb}^{t=0} + \phi \text{New}_{ij}^{t=1} + u_{ijb}^t \tag{1}$$

where $Y_{ijb}^{t=1}$ is the survey outcome of interest for individual i in neighborhood j in block b at endline ($t = 1$), T_{ij}^m is the community meeting treatment indicator, T_{ij}^f is the alternative treatment arm treatment indicator, and $\boldsymbol{\gamma}_b$ is a vector of block fixed effects. For individuals who were surveyed at

Figure 4: CONSORT diagram of research design



baseline, $\widetilde{\mathrm{Y}}_{ijb}^{t=0}$ is the baseline measurement of the outcome; for individuals who were not surveyed at baseline, we set $\widetilde{\mathrm{Y}}_{ijb}^{t=0}$ to the mean of that outcome in person i's neighborhood at baseline:

$$\widetilde{Y}_{ijb}^{t=0} = \begin{cases} Y_{ijb}^{t=0} & \text{For respondents surveyed at baseline} \\ \overline{Y_{jb}^{t=0}} & \text{For respondents not surveyed at baseline} \end{cases}$$

For some outcomes, baseline measurements are not available, in which case $\widetilde{\mathrm{Y}}_{ijb}^{t=0}$ is omitted. Finally, $\text{New}_{ij}^{t=1}$ is an indicator for respondents who were *not* surveyed at baseline (that is, respondents who appear for the first time at endline). We include these new respondents because attrition was higher than expected (50% vs. the expected 30%).

Following the guidelines for the meta-analysis, we weight our estimates of Equation 1 by the inverse of the sampling probability of citizens in each neighborhood. We cluster standard errors at the level of the neighborhood j.

The coefficient of interest for the meta-analysis is ν_m: the effect of assignment to police–

Table 2: Baseline Summary

Outcome Family	Outcome	Range	Mean	SD	Study Ranking				rank_5
Compliance	Foot patrol frequency	0-5	1.84	2.07	Lbr.	Uga.	**Col.**	Phl.	Pak.
	Vehicle patrol frequency	0-5	4.21	1.34	Lbr.	Uga.	Pak.	Phl.	**Col.**
	Community meeting awareness	0-1	0.05	0.23	Pak.	**Col.**	Lbr.	Phl.	Uga.
Perceived future insecurity	Feared violent crime	1-4	2.69	1.10	Lbr.	Uga.	Phl.	Pak.	**Col.**
	Feared walking	0-1	0.42	0.49	**Col.**	Phl.	Uga.	Lbr.	Pak.
Overall perceptions of police	Trust in police	0-3	1.49	1.00	Pak.	**Col.**	Lbr.	Phl.	Uga.
Police abuse	Police abuse	0-1	0.18	0.38	Phl.	Uga.	**Col.**	Pak.	Lbr.
Crime reporting	Violent crimes reported (personal)	0-2	0.02	0.15	Phl.	**Col.**	Pak.	Lbr.	
	Non-violent crimes reported (personal)	0-1	0.03	0.16	Phl.	**Col.**	Pak.	Lbr.	
	Burglary resolution	0-1	0.43	0.50	Lbr.	Phl.	**Col.**	Uga.	Pak.
	Domestic abuse resolution	0-1	0.56	0.50	Lbr.	Phl.	Uga.	Pak.	**Col.**
Crime tips	Contacted police for suspicious activity	0-1	0.15	0.36	Pak.	Lbr.	Phl.	**Col.**	Uga.
	Gave information to police	0-1	0.06	0.23	**Col.**	Phl.	Lbr.	Uga.	Pak.
Police abuse reporting	Reported police beating	1-4	3.00	0.98	Pak.	Uga.	Lbr.	Phl.	**Col.**
	Reported police abuse	0-1	0.04	0.20	Phl.	Uga.	**Col.**	Pak.	Lbr.
Perceived police intentions	Police will investigate	0-4	1.93	1.12	**Col.**	Lbr.	Pak.	Phl.	Uga.
	Police are corrupt	1-5	3.24	1.15	Phl.	Uga.	Lbr.	**Col.**	Pak.
	Police serve equally	0-4	1.80	1.15	Lbr.	**Col.**	Pak.	Phl.	Uga.
Cooperation norms	Reporting norm (theft)	1-3	2.39	0.66	Lbr.	Uga.	Pak.	**Col.**	Phl.
	Reporting norm (domestic abuse)	1-4	2.19	1.38	Lbr.	Pak.	**Col.**	Uga.	Phl.
Perceived police capacity	Police timeliness	0-4	2.03	1.16	**Col.**	Lbr.	Pak.	Phl.	Uga.
	Police investigation capacity	0-4	2.24	1.15	**Col.**	Lbr.	Phl.	Pak.	Uga.
Perceived police responsiveness	Perceived police responsiveness	0-4	2.07	1.13	**Col.**	Pak.	Lbr.	Phl.	Uga.
Perceived state legitimacy	Perceived state legitimacy	0-3	0.88	0.87	**Col.**	Lbr.	Pak.	Phl.	
Community trust	Community trust	0-3	1.79	0.79	Uga.	**Col.**	Lbr.	Pak.	Phl.

community meetings on citizen beliefs and self-reported behaviors.

Administrative Data. For outcomes measured using the administrative data, we estimate:

$$\mathbf{Y}_{jb}^{t=1} = \alpha + \xi^m T_{jb}^m + \xi^f T_{jb}^f + \boldsymbol{\gamma}_b + \delta \mathbf{Y}_{jb}^{t=0} + e_{jb} \tag{2}$$

where $Y_{jb}^{t=1}$ is outcome Y in neighborhood j in block b using data from the six months following the first meeting in block b (beginning in the month in which the meeting was held). $\mathbf{Y}_{jb}^{t=0}$ is outcome Y in neighborhood j in block b measured in the six months *preceding* the first meeting in block b. The coefficient of interest for the meta-analysis is ξ_m: the effect of police–community meetings on outcome Y.

5 Results

5.1 Effects on Citizen Beliefs and Behavior

Compliance. The community policing intervention facilitated police–community meetings, but it did not mandate citizen or police attendance or participation. For this reason, we first assess uptake: were residents of treated neighborhoods more likely to hear about, see, or attend a police–community meeting in their neighborhoods?

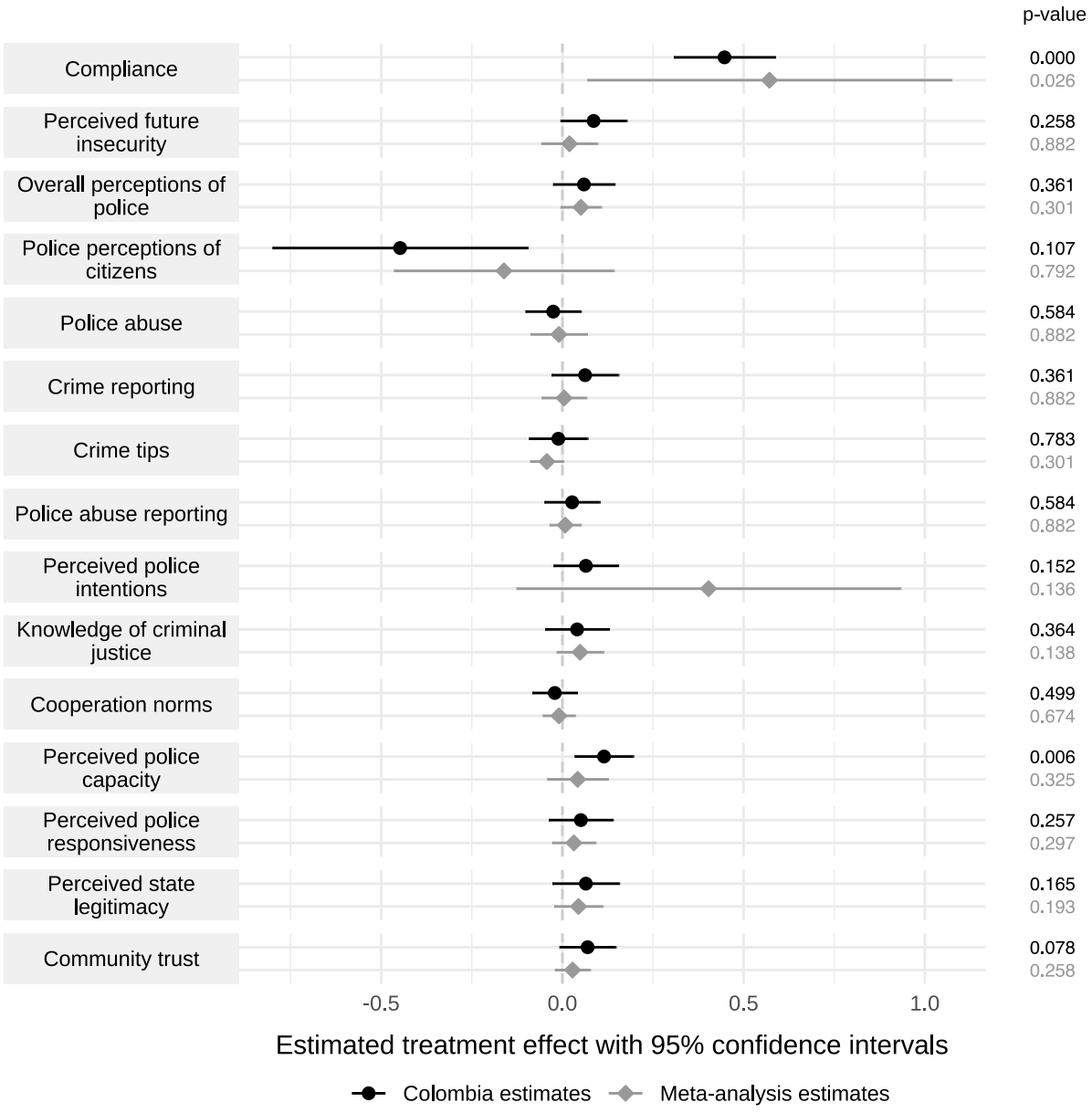
We find strong evidence that they were. The first panel of Figure 5 reports the results of the pre-specified analysis of compliance with treatment assignment. We observe a large effect of the intervention on compliance: the point estimate is 0.27 (baseline) standard deviations. Substantively, at endline, residents of neighborhoods with meetings were 19 percentage points more likely to say that they had heard of, seen, or attended a community meeting with the police than residents of neighborhoods without meetings (as noted previously, there was no difference at baseline).

We also observe evidence of possible spillovers. At baseline, five percent of respondents had heard about, seen, or attended police–community meetings; at endline, this proportion increased to approximately 19 percent (population-weighted) in neighborhoods without meetings and 38 percent (population-weighted) in neighborhoods assigsned to meetings. We visualize this increase in Figure 6.[3] The distribution of awareness shifts rightward from baseline (top) to endline (bottom) across both groups, though the shift is much more pronounced in neighborhoods assigned to treatment. We are not aware of any other new police–community meeting initiatives in Medellín during this period; thus, the substantial increase in awareness in neighborhoods without meetings suggests the presence of spillovers.

Figure 6 also indicates uneven uptake of the intervention: the variance of awareness (across neighborhoods) increases dramatically from baseline to endline. This resonates with descriptive data collected on citizen attendance in community meetings. Figure 7 reveals heterogeneous patterns of citizen participation. Average attendance was 17.9 citizens per meeting, or 53.2 citizens

[3]Note that the analysis implied by this plot is not pre-specified.

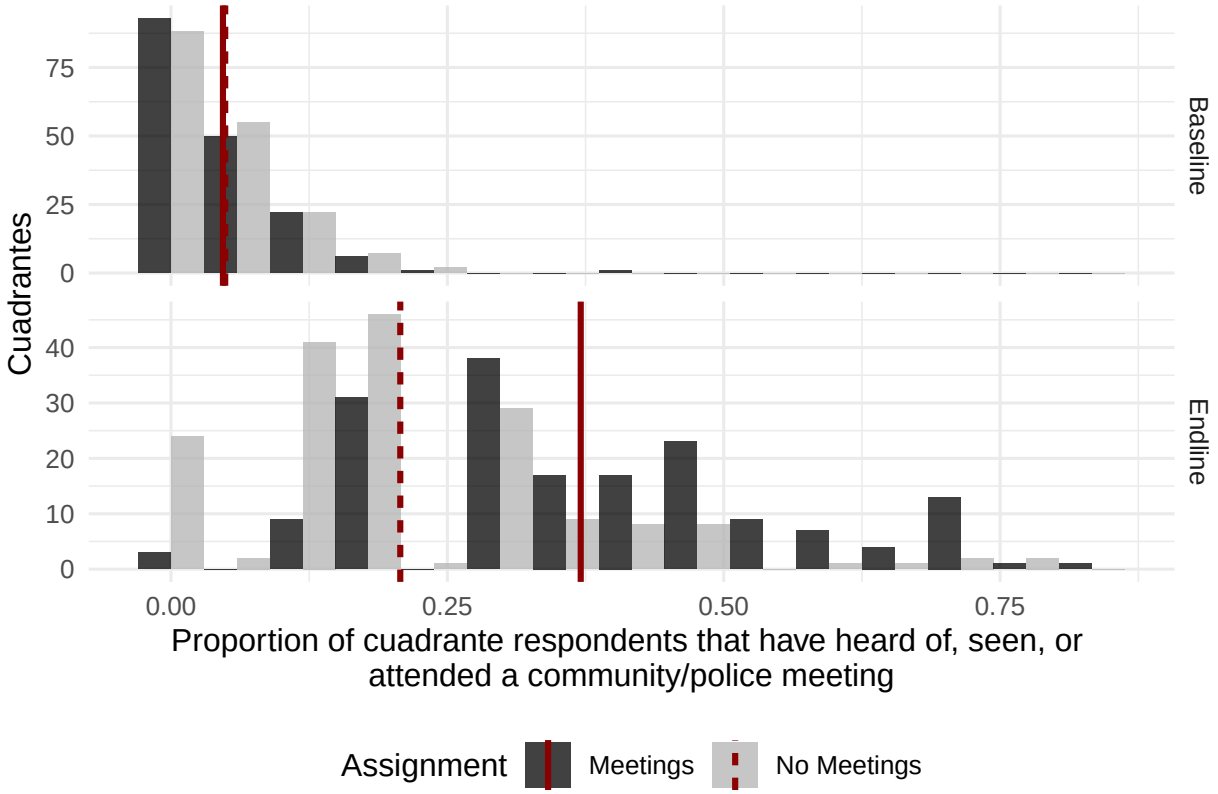
Figure 5: Effects of Community Policing in Colombia and Meta-Analytic Estimates



per neighborhood over two or three meetings. The minimum cumulative (over meetings) attendance was four, and the maximum was 118.

This analysis yields two key takeaways for the interpretation of outcomes of interest. First, there is strong evidence that the community policing treatment increased citizen awareness of and attendance at police–community meetings. At baseline, in the absence of the intervention, awareness of such meetings was quite limited. Second, there is suggestive evidence of spillovers

Figure 6: *Cuadrante*-level averages of resident familiarity with police–community meetings at baseline and endline. The vertical lines represent the mean across neighborhoods.

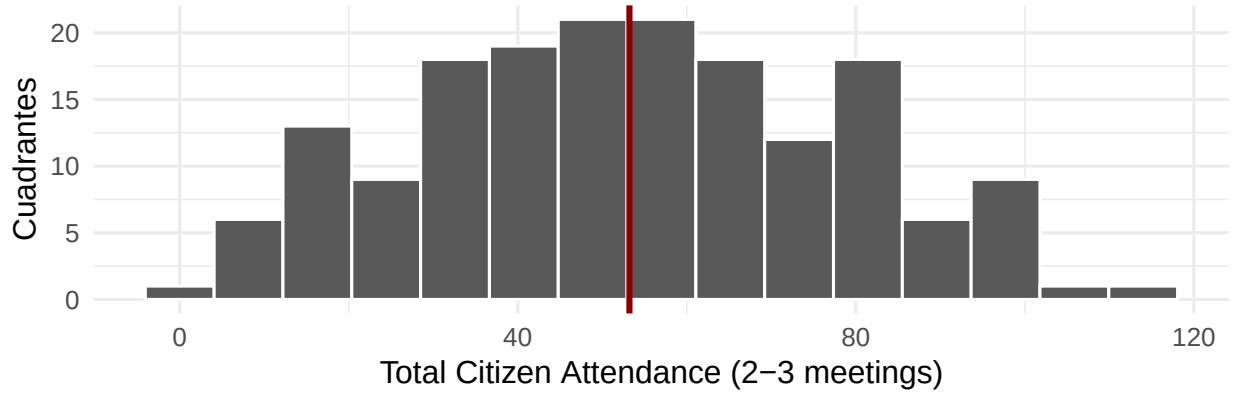


in awareness of police–community meetings to neighborhoods not assigned to receive meetings. This raises questions about what quantity is being estimated in the analysis presented below. Nevertheless, we adhere to the pre-specified estimators in order to present the information that our study contributes to the meta-analysis.

Crime victimization. We now turn to crime victimization, considering data from two sources. First, we examine data from self-reported crime victimization on the citizen surveys. Second, we examine a select set of crimes that appear in geo-coded police case databases maintained by the Information System for Security and Community Relations (SISC), a unit within the government of the City of Medellín.

Our survey-based crime victimization outcomes represent a subset of those analyzed in the meta-analysis in this volume, for two reasons. First, our survey-based crime reports are binary;

Figure 7: Cumulative neighborhood-level citizen attendees over three police–community meetings (167 neighborhoods) or two police–community meetings (6 neighborhoods).



the survey did not elicit how many times an individual was victimized. Second, based on their knowledge of norms and culture in Medellín, enumerators were uncomfortable asking some questions about respondents' own experiences (e.g., experiences of sexual violence); in these cases, we measure victimization of acquaintances. For that reason, the estimates in Figure 8 are considered secondary for the purposes of the meta-analysis.

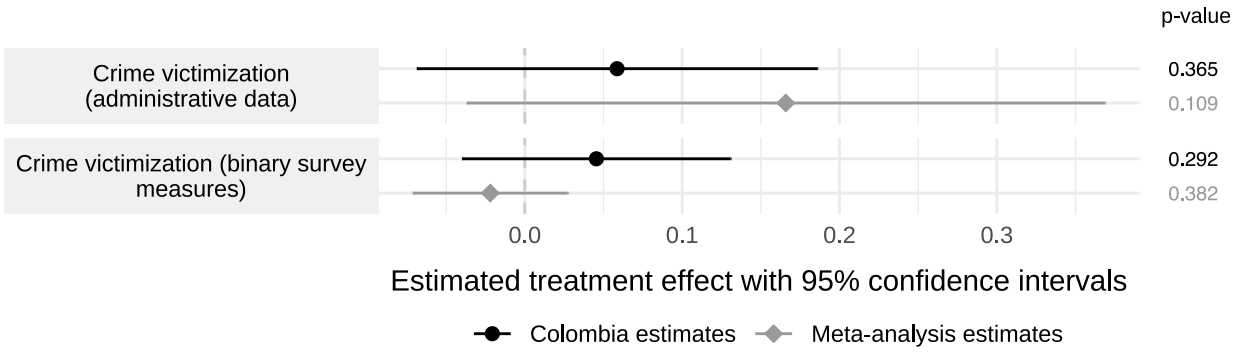
We find no evidence of a reduction in crime incidence. The second panel of Figure 8 visualizes the ITT estimates on self-reported crime victimization; all point estimates are substantively small and statistically indistinguishable from zero. This null result does not arise because base rates were too low or too high to detect changes: overall, 22.5% of respondents reported that someone in their households suffered burglary, armed robbery, or simple assault in the past year.[4]

Nor does the administrative data reveal any evidence that the intervention affected crime rates. The first panel of Figure 8 plots the ITT estimates on outcomes available in the administrative data; the point estimate is small and statistically indistinguishable from zero. The slight positive point estimate is driven by a few outlying burglary counts; the effects on violent crime and domestic violence are very small.

Overall, the ITT analysis provides no evidence that assignment to police–community meetings decreased crime. This is true both according to self-reports in the citizen victimization survey and

[4]In general, it is harder to assess rates of victimization from crimes perpetrated against acquaintances given differing salience/publicness by type or crime.

Figure 8: Secondary Hypotheses: Effects in Colombia and Meta-Analytic Estimates



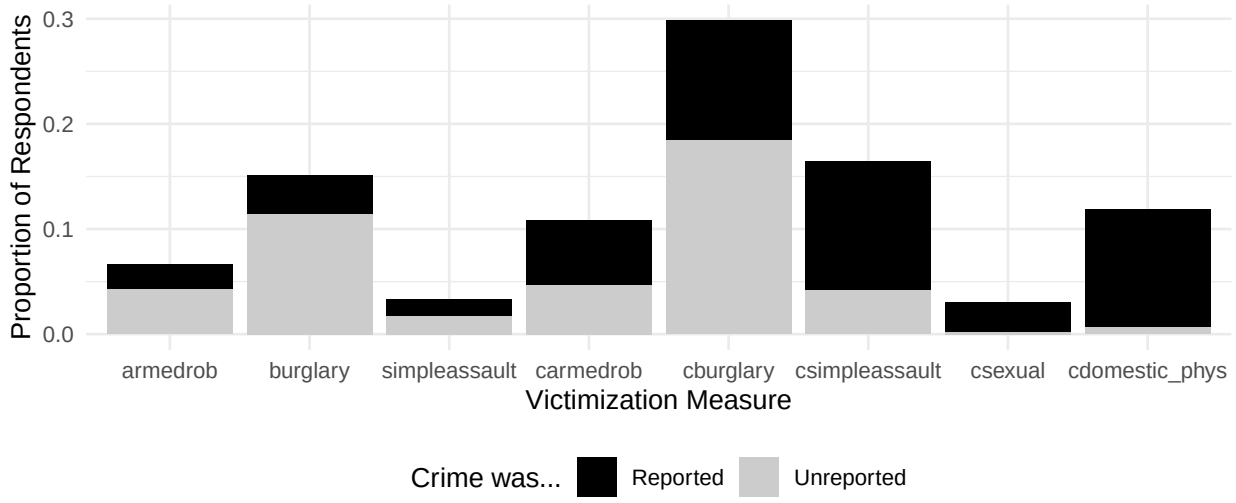
according to administrative data. This is perhaps unsurprising given the character and design of our specific intervention. Unlike several other studies in this volume, our intervention did not involve increased police patrols. Moreover, as noted above and in Arias et al. (2020), our intervention was not powered to detect changes in crime incidence.

Crime Reporting. Assessing whether the police–community meetings affected crime reporting entails two empirical challenges. First, only crime victims can choose to report or not report the incident to the police; those who were not victimized do not make this choice. This means that the outcome—crime reporting—is undefined for those who were not victimized (see Slough, 2019). However, following the pre-specified meta-analysis plan, we impute a zero for each non-victim's crime reporting outcome. In other words, non-victims and victims who chose not to report enter identically into the analysis. In order to interpret the results as ITTs, therefore, we require an additional assumption: not only that the treatment did not affect the crime rate, but that there were *exactly zero* crimes that occurred only because of the treatment ("if-treated crimes") and *exactly zero* crimes that occurred only because of the absence of treatment ("if-untreated crimes").

The second challenge is statistical power. Given that self-reported victimization rates were below 15% for all crimes except burglary and assault (Figure 9), the maximum possible increase in crime reporting was small in absolute terms.

The fifth panel of Figure 5 reveals that (under the assumption just articulated) the treatment had a generally positive—but statistically insignificant—effect on crime reporting. Even in the

Figure 9: Crime and reporting rates at endline (pure control only).



(theoretically) best-powered test on the crime reporting index, using the pre-specified one-tailed hypothesis test and without adjusting for multiple comparisons, we can not reject the null hypothesis of no effect; the point estimate is 0.035 standard deviations, $p = 0.18$ (see Appendix Table A2). The adjusted p-value is 0.42. Results using analysis of administrative data are consistent with this substantive finding. The sixth panel of Figure 5 ("Crime tips") reports the (standardized) effect of the police–community meetings treatment on the number of calls to the city's 123 line (analogous to 911); the effect is small and imprecisely estimated. We could gain additional analytical leverage by using temporal variation in meeting timing to examine how calls vary over shorter time horizons, but these analyses are beyond the scope of this chapter.

Descriptively, respondents are much more likely to say that a friend or acquaintance reported a crime to the police (after being victimized) than to say that they themselves reported a crime to the police (after being victimized). One possible explanation is that respondents do not hold one single friend or acquaintance in mind when answering these questions, but rather think of *at least one* friend or acquaintance who reported to the police (despite the fact that the survey instrument was designed to avoid this). Another possible explanation is social desirability bias: perhaps respondents want to show evidence of crime reporting, and it may be easier to exaggerate someone else's behavior than their own behavior. Regardless, the mismatch between respondents'

own reporting rates and their friends' apparent reporting rates merits further investigation.

Citizen Perceptions of Insecurity. Even if, as the above analysis suggests, the police–community meetings treatment did not affect actual crime rates, it might have affected citizens' *perceptions* of insecurity. In principle, the effect could go in either direction. On the one hand, meeting participants might learn that crime rates are lower than they thought—or, at least, that crime rates declined quickly in recent years. Likewise, developing a relationship with the local patrol officer might make people feel safer. On the other hand, hearing the concerns of other meeting participants might make crime and insecurity more salient for some attendees. Moreover, the mere fact of being invited to a police–community meeting might create the impression of heightened police activities in the neighborhood, which could be interpreted as a response to growing insecurity.

The second panel of Figure 5 ("perceived future insecurity") suggests that, if anything, these latter effects dominated; assignment to police–community meetings made citizens feel slightly *less* secure. The effect is imprecisely estimated (adjusted p-value$= .42$; see Appendix Table A2).

Citizen Perceptions of the Police. Thus far we have reported that the police–community meetings treatment did not substantially affect crime incidence, crime reporting, or citizens' perceptions of security.

But the treatment does appear to have affected citizens' views of and opinions about the police. Residents of treated neighborhoods were more likely to adopt favorable views of police capacity (see "Perceived police capacity" panel of Figure 5). Residents of treated neighborhoods became much more likely to report that the police have the capacity to respond to incidents in a timely manner and that they have the capacity to investigate crimes effectively. The treatment effect on the police capacity index is 0.1 standard deviations and significantly different from zero ($p < 0.01$; see Table A2), even under a multiple comparisons correction. Respondents in treated neighborhoods are also slightly (but not significantly) more likely to report that the police "act upon citizen comments and complaints in my community" (see "Perceived police responsiveness" panel of Figure 5).

The treatment also had a slightly positive—but not significant—effect on residents' propensity to say that police provide the same quality of service to all citizens, that the police are not corrupt, that the police take cases seriously, and that the police treat the victim and the suspect fairly (see "Perceived police intentions" panel of Figure 5).

We offer two conjectures about possible explanations for these effects. One is that the meetings only (or primarily) reshaped the opinions of citizens who selected into attending. Hearing the officers' perspectives may have helped citizens realize how often and how quickly the police do, in fact, respond to incidents. Given that the cumulative attendance rate (over three meetings) was on average one per 12 adults (53 attendees, $\approx$ 600 adult residents), it is entirely possible that attendees' views changed dramatically. A second possible explanation is that merely being invited to the meetings changed residents' views of police capacity. Residents might have reasoned that a police force capable of organizing police–community meetings was likely capable of fulfilling more basic functions like responding to calls and investigating crimes.

These changes in citizen beliefs did not obviously translate into changes in citizen *satisfaction* with the police (see "Overall perceptions of police" panel of Figure 5); the effect is positive but not significant. Nor do we find any evidence that the police–community meetings treatment consistently affected citizens' knowledge of the criminal justice system ("Knowledge" panel of Figure 5). This is not especially surprising given that discussion at the meetings did not typically center around education about the criminal justice system.

Finally, we estimate positive—but small and statistically insignificant—effects of the police–community meetings on citizen trust in their communities and in the Colombian (national) government at large (see the last two panels of Figure 5, "Perceived state legitimacy" and "Community trust"). The former result is consistent with, if not strong evidence of, the hypothesis that the meetings helped build mutual trust among citizens. The latter result makes sense given that the Colombian police are a national organization with considerable local jurisdiction over operations: perceptions of the police might reasonably affect beliefs about the national government, but perhaps only weakly.

Police perceptions of citizens. In addition to studying effects on citizen beliefs and behaviors, we study effects on officer beliefs. Because we were not able to survey police officers at baseline, we evaluate hypotheses about police attitudes and behaviors using the prespecified equation:

$$Y_{ijb}^{t=1} = \eta_m T_{ij}^m + \eta_f T_{ij}^f + \boldsymbol{\gamma}_b + \epsilon_{ijb}^t \tag{3}$$

where $Y_{ijb}^{t=1}$ is the survey outcome of interest for police officer i in neighborhood j in block b at endline ($t = 1$), and all other variables are as defined above. We only include those officers who reported their police beat ID number on the survey ($N = 472$ of the 694 officers surveyed). The coefficient of interest for the discussion in this volume is η_m, the ITT for police–community meetings.

Because only 472 of 694 surveyed officers reported their police beat ID numbers, and perhaps also because we did not survey the universe of officers, there are many police beats for which we lack officer surveys. Fortunately, we find that treatment assignment appears uncorrelated with this missingness (Appendix Table A3). Moreover, the proportion of beats with at least one officer survey is similar for every police station except Belén, in the southwest of the city, where only 14.2% of beats have corresponding officer surveys (see map in Appendix Figure A2).

This limitation of the officer survey leads us to present ITTs for two specifications. First, we use the pre-specified estimator, which includes indicators (fixed effects) for the blocks used in treatment assignment (denoted γ_b in Equation 3). But because of the pattern of missing police officer surveys, the use of block fixed effects reduces our effective sample from 82 (of 87) blocks to 64 blocks: only 64 blocks have within-block variation in treatment assignment. We therefore also report estimates from an equation analogous to Equation 3 that excludes block fixed effects. While not pre-specified, the latter estimates are important for transparency, given the large number of beats without officer surveys.

Several characteristics of the assignment of officers to police beats, and of the administration of our survey of police, unfortunately limit the scope of our analysis of the resulting data. While in

theory the vast majority of officers should have been assigned to one and only one police beat during the course of the intervention, in practice officers are frequently reassigned. Moreover, officers assigned to one beat were sometimes sent to attend police–community meetings in another beat. This reassignment and cross-beat meeting attendance muted the relationship between treatment assignment and officers' meeting attendance for officers: in control police beats (those without police–community meetings), a majority (53%) reported attending police–community meetings over the past year; in police beats assigned to treatment, the proportion attending was just 10.5 percentage points higher (64%).

The weakness of this first-stage relationship between treatment assignment and meeting attendance informs our discussion of the findings. That said, we do not conceive of the treatment as exclusively a function of meeting attendance. Indeed, the ITT estimates permit multiple causal channels between treatment assignment and outcomes.

We find a negative and significant effect on police perceptions (Figure 5). "Police perceptions" is an index of four components of officer attitudes; the component related to officers' perceptions of police corruption drives the overall negative coefficient. Asked about (hypothetical) corrupt behaviors such as accepting bribes in exchange for not issuing a parking ticket, officers assigned to treatment were apparently *less* likely to report that these behaviors constituted "serious misconduct." Treatment did not affect the other three components of the index (whether officers feel empathy for citizens, whether police leadership punishes officer misconduct, and whether police abuse of force constitutes "serious misconduct"). It is possible that assignment to police–community meetings actually softened officers' opposition to petty corruption; perhaps, for instance, attending meetings convinced officers that petty corruption did not rank among citizens' primary concerns. However, given our caveats about the police survey (in particular, the weak relationship between treatment assignment and meeting attendance), and given the inconsistent results across components of the "officer perceptions" index, we are hesitant to over-interpret this result. (In contrast, coefficients on *citizens'* perceptions of police are consistently positive, as discussed above). Moreover, police strongly oppose corrupt behaviors in both treatment and control groups, meaning that the standard

deviation of these attitudes is small. The large standardized coefficient (-0.4sd) is thus small in absolute terms.

6 Discussion

We began this chapter by noting a striking divergence between the trajectory of citizen security outcomes and policing quality, on the one hand, and citizen perceptions of police on the other. Since the 1990s, and also since the early 2000s, the security environment in Medellín has improved dramatically, and the Colombian police have become much less violent and more professional. Yet available survey data show little improvement in citizens' views of the police. This disjuncture recalls similar problems in the United States and elsewhere (Esberg and Mummolo, 2018).

Together with the authors of the other chapters in this volume, and following a long line of literature about community policing in other contexts (e.g. Skogan, 2011, 2015; Skogan and Hartnett, 1999), we hypothesized that informal contact between officers and citizens might help close the gap. Our counterparts in the police and the city government in Medellín agreed: they had held occasional town-hall-style meetings between high-ranking officers and residents in the past, and they were optimistic that expanding the number of these meetings—and making them more local—would improve police–community relations, perhaps increasing citizen awareness of police officers' effort.

We find that, to some extent, these consequences materialized: relative to residents in neighborhoods not assigned to police–community meetings, residents in police–community meetings neighborhoods developed somewhat more positive evaluations of police capacity and police intentions. For example, residents assigned to police–community meetings were more likely to say that the police have the capacity to respond to incidents in a timely manner. That these and related differences are statistically distinguishable from zero is all the more notable given attendance rates: only one in every twenty-three residents (perhaps one in every twelve adults) attended at least one police–community meeting during the intervention. This means that the changes in attendees' at-

titudes were very large, that their experiences affected the beliefs of those around them, or that publicizing repeated meetings is sufficient for citizens to update on the police. In any case, these findings provide evidence that, consistent with quasi-experimental studies such as Skogan (2011), "beat meetings" can reshape citizens' perceptions of the police.

These effects on citizen perceptions of the police are larger than the meta-analysis estimates and more consistent (across questions) than those of other study sites. We propose two possible explanations.

The first rests on changing citizens' beliefs about police officers' jurisdiction and power—in short, lowering expectations. Officers frequently told meeting attendees that the police face severe constraints: gangs, laws regarding police conduct, and a dysfunctional criminal justice system. The police were understaffed, officers told people, because of changes in retirement rules. At some meetings, officers shared this information at the very beginning—before attendees could voice any complaints. Staffing problems damaged officers' ability to respond to calls and to conduct patrols, they said. Meeting attendees often responded to this information by expressing frustration with the municipal government and sympathy for the officers themselves. Officers also explained—correctly—that other state authorities were responsible for addressing certain concerns (such as dealing with stray animals). Clarifying the scope of their responsibilities might replace people's perception that the police "don't do anything" with the knowledge that, in many contexts, there is little they can or should do.

The second possible explanation relates to the hyper-local targeting of our police–community meetings: while other sites targeted meeting invitations across (for example) multiple villages, our context allowed us to target invitations to a few contiguous city blocks. Every resident of these treated blocks received the flyer inviting them to the meeting. And because the Colombian police had previously implemented a beat-based patrol system, the invitation flyers emphasized the presence of "officers from your police beat"—in other words, residents' own local officers, rather than an anonymous high-level chief whom residents would never see again. The intimacy of (geographically) small communities meeting with dedicated local officers strikes us as a plausible

explanation for our differential findings on perceptions of police.

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A Appendix

Table A1: Balance tests

	Meetings		Leaflet		Community & Leaflet		Control Mean	Control Std. dev.	p-value for dif.
Population	69.701	-(582.57)	-352.08	-(586.49)	-221.56	-(569.71)	6180.57	4716.71	0.87
Households	13.529	-(155.04)	-82.425	-(156.01)	-9.167	-(156.63)	1671.44	1214.78	0.92
People per household	0.06	-(0.055)	-0.034	-(0.050)	-0.032	-(0.050)	3.63	0.48	0.27
Share of rented homes	0.002	-(0.004)	-0.002	-(0.003)	0.002	-(0.004)	0.02	0.02	0.43
Avg. bedrooms per household	-0.027	-(0.052)	0.071	-(0.054)	-0.009	-(0.052)	2.25	0.4	0.28
Share of overcrowding households	0.009	-(0.009)	0	-(0.009)	0.008	-(0.010)	0.05	0.07	0.67
Household shares cooking with electricity	0.001	-(0.004)	-0.003	-(0.003)	0.001	-(0.005)	0.02	0.02	0.49
Household shares cooking with piped gas	0.004	-(0.012)	-0.01	-(0.010)	0.004	-(0.014)	0.07	0.07	0.49
Household shares with fridge or TV	0.004	-(0.005)	-0.001	-(0.004)	0.004	-(0.006)	0.03	0.03	0.67
Household shares with computer	0.004	-(0.008)	-0.006	-(0.006)	0.003	-(0.010)	0.04	0.04	0.51
Household shares with motorcycle	0.005	-(0.008)	-0.006	-(0.007)	0.005	-(0.010)	0.05	0.04	0.39
Household shares with landline	0.004	-(0.014)	-0.002	-(0.013)	0.016	-(0.012)	0.9	0.1	0.43
Household shares with pipeline gas	-0.007	-(0.017)	-0.003	-(0.019)	-0.018	-(0.017)	0.23	0.23	0.72
Employed per household	0.004	-(0.020)	0.005	-(0.018)	-0.013	-(0.020)	1.31	0.13	0.83
Unemployed per household	0.001	-(0.006)	0	-(0.005)	-0.005	-(0.005)	0.11	0.05	0.69
Retirees per household	0.001	-(0.001)	-0.001	-(0.001)	0.002	-(0.001)	0	0.01	0.17
Household shares with family living abroad	0	-(0.004)	0.003	-(0.004)	0.005	-(0.004)	0.04	0.03	0.43
Share of males per household	-0.009*	-(0.005)	-0.001	-(0.004)	-0.008	-(0.006)	0.47	0.03	0.13
Share of females per household	0.002	-(0.006)	0.006	-(0.005)	0	-(0.007)	0.52	0.03	0.55
Share of under-aged per household	0.002	-(0.007)	0.004	-(0.007)	-0.006	-(0.007)	0.24	0.07	0.53
Share of seniors per household	0.008	-(0.009)	-0.005	-(0.007)	0.014	-(0.010)	0.09	0.05	0.15
Household shares with male head	-0.007	-(0.007)	0.005	-(0.006)	-0.008	-(0.008)	0.63	0.04	0.16
Household shares with single-male parent	-0.008	-(0.005)	-0.002	-(0.005)	-0.010**	-(0.005)	0.13	0.05	0.13
Household shares with single-female parent	-0.003	-(0.006)	-0.002	-(0.006)	-0.007	-(0.006)	0.32	0.04	0.69
Household shares w/o children at home	-0.01	-(0.009)	0.001	-(0.008)	-0.007	-(0.009)	0.3	0.1	0.53
Household shares with university students	0.033	-(0.041)	-0.056	-(0.039)	-0.004	-(0.040)	2.8	0.32	0.12
Household head born in Colombia	-0.01	-(0.009)	0.003	-(0.007)	-0.017	-(0.010)	0.96	0.05	0.15
Household head born in Medellín	0.004	-(0.012)	-0.004	-(0.011)	-0.002	-(0.011)	0.39	0.07	0.94
Log of monthly rent	0.1	-(0.075)	0.049	-(0.072)	0.027	-(0.073)	12.41	0.66	0.58
Homicide rate	-5.493	-(12.530)	-16.499	-(11.523)	15.001	-(19.392)	34.81	78.04	0.25
Theft rate	-4.739	-(5.615)	-6.461	-(5.515)	-4.83	-(5.089)	18.38	59.13	0.71
Lagged homicide rate	3.053	-(6.980)	-1.413	-(6.095)	9.098	-(7.425)	29.16	41.24	0.49
Lagged theft rate	-43.301	-(40.045)	-46.651	-(40.743)	-36.574	-(39.085)	57.55	426.86	0.70
Second study treatment condition	0.034	-(0.024)	0.023	-(0.022)	0.039*	-(0.022)	0.1	0.31	0.34
Second study control condition	-0.023	-(0.023)	-0.034	-(0.023)	-0.019	-(0.020)	0.14	0.35	0.52

Table A2: Results Table for Main Hypotheses

Hyp.	Outcome	Estimate	S.E.	Conf. Int.	p-value	Adj. p-value
C	Compliance idx.	0.268	0.035	(0.199, 0.337)	0.000	
1b	Perceived future insecurity idx.	0.066	0.035	(-0.004, 0.137)	0.064	0.422
2	Overall perceptions of police idx.	0.054	0.037	(-0.020, 0.127)	0.150	0.422
3b	Police abuse idx.	-0.025	0.038	(-0.102, 0.051)	0.511	0.597
4a	Crime reporting idx.	0.035	0.026	(-0.016, 0.086)	0.181	0.422
4b	Crime tips idx.	0.022	0.056	(-0.090, 0.133)	0.703	0.703
4c	Police abuse reporting idx.	0.018	0.025	(-0.032, 0.067)	0.485	0.597
M1a	Perceived police intentions idx.	0.043	0.030	(-0.016, 0.102)	0.152	
M1c	Cooperation norms idx.	-0.057	0.030	(-0.118, 0.004)	0.065	
M2a	Perceived police capacity idx.	0.101	0.036	(0.030, 0.172)	0.006	

M2b	Perceived police responsiveness	0.051	0.045	(-0.038, 0.140)	0.257
S1	Perceived state legitimacy	0.065	0.046	(-0.027, 0.157)	0.165
S2	Community trust	0.070	0.039	(-0.008, 0.147)	0.078
M1b	Knowledge of criminal justice idx.	0.000	0.021	(-0.042, 0.042)	0.996

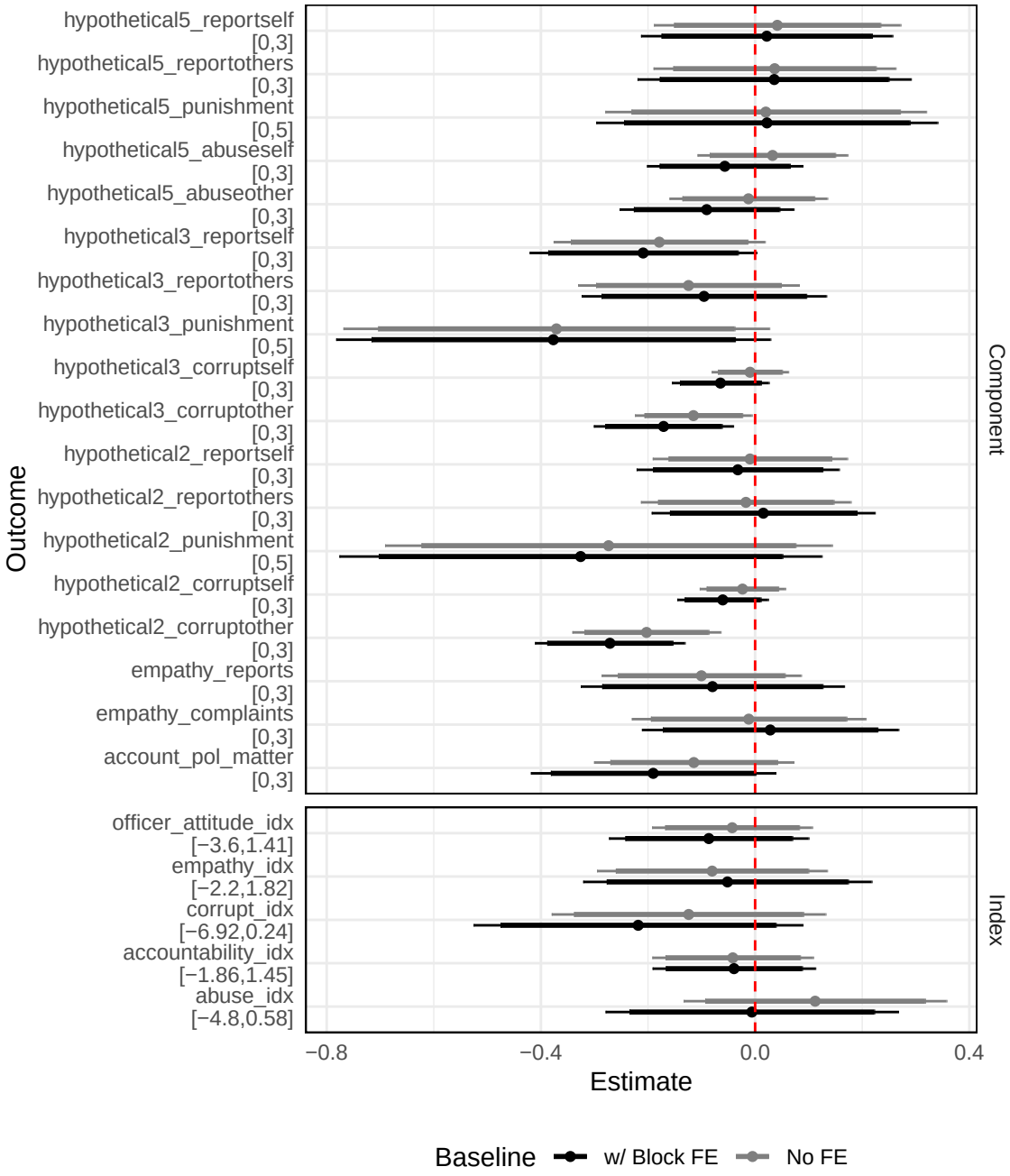


Figure A1: ITT estimates on police behavior outcomes from the officer survey. The values below each outcome name represent the range and scale of the variable. The estimators with and without block fixed effects are indicated by color. The thick lines represent 90% CIs and the thin lines represent 95% CIs constructed on standard errors clustered at the *cuadrante* level.

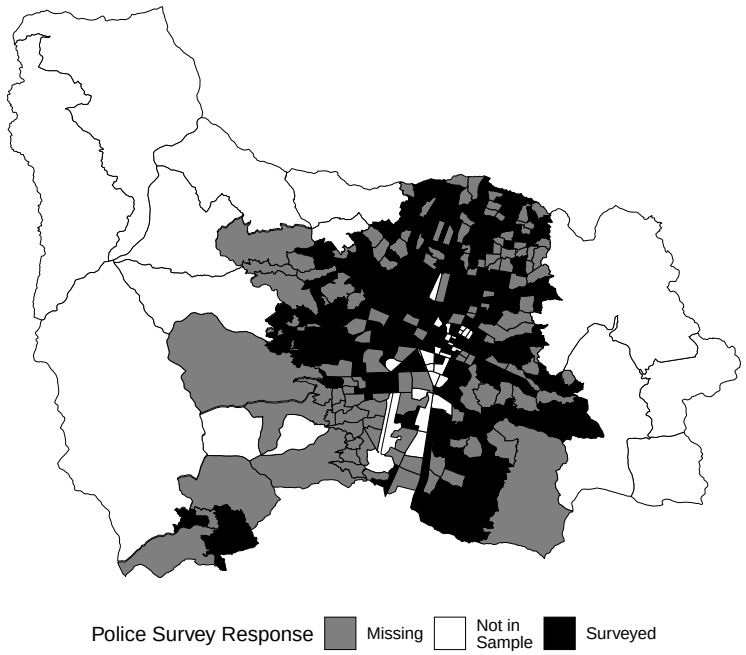


Figure A2: Police beats by missingness/non-missingness of police survey outcome data.

Police Survey Responses Unrelated to Treatment Assignment

Estimate of the ITT of each treatment arm on police survey non-missingness in terms of number of surveys completed (Columns [1]-[2]) and the presence of at least one survey (Columns[3]-[4]). Heteroskedasticity robust standard errors in parentheses.

	Dependent variable:			
	# of Respondents		Any Response	
	(1)	(2)	(3)	(4)
Common arm: Police-community meetings	0.010	0.017	0.007	0.009
	(0.091)	(0.091)	(0.041)	(0.042)
Alternative arm: Informational flyers	−0.151	−0.144	−0.039	−0.037
	(0.091)	(0.090)	(0.041)	(0.041)
Blocks		✓		✓
Mean, Pure Control	0.874	0.874	0.609	0.609
St. Dev., Pure Control	0.938	0.938	0.491	0.491
Observations	347	347	347	347