How Criminal Governance Undermines Elections

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September 20, 2024

Abstract

How does criminal governance affect elections? Existing accounts explore the consequences of criminal involvement in politics, but have not thoroughly examined how such groups exert their influence. I argue that criminal groups undermine elections through two mechanisms: (1) *gatekeeping* prevents rival candidates from accessing voters and (2) *corralling* influences voter choice. I use a natural experiment that leverages exogenous variation in voter assignment to voting booths and a novel dataset on criminal governance to test my theory in Rio de Janeiro, Brazil. I show that gatekeeping restricts the candidate pool while corralling yields more votes for the local leading candidate. Together, these mechanisms decrease electoral competition. I illustrate the logic underpinning the mechanisms using qualitative data based on interviews and voter complaints. These findings bring together the literatures on clientelism and criminal governance by demonstrating that criminal groups leverage the power they derive from governing to sway elections.

1 Introduction

> "The majority of the residents of our neighborhood are being threatened by the local drug trafficking leader, who received R$60,000 to force the residents from here to vote for *CANDIDATE A*. It appears that last week they prohibited *CANDIDATE B* from entering any homes... Also, it's not allowed to have any other form of campaign materials inside your house that's not for *CANDIDATE A*. According to stories, the trafficker made it clear that if he discovered who didn't vote for *CANDIDATE A*, you'd be killed."[1]

For nearly a quarter of Rio de Janeiro's residents, campaign season has undertones of such electoral promises, threats, or violence. Criminal groups play an active role in local politics, electioneering through their interactions with both candidates and voters.

This paper illustrates *how* criminal groups deliver votes by working as political brokers. A fast-growing literature establishes that criminal groups are influential political actors (Arias 2017; Duran-Martinez 2017; Lessing 2017; Yashar 2018). This literature has focused on why *criminal groups* might want to get involved in electoral politics (Dell 2015; Trejo and Ley 2020), but has overlooked the reasons why criminal groups might be appealing partners for candidates.

I argue that criminal groups derive certain capacities from governing that give them unprecedented access to and influence over voters, making them particularly effective at getting votes. There are two mechanisms through which they deliver votes to their candidate partners. *Gatekeeping* moderates territorial access to candidates. *Corralling* leverages criminal groups' influence over residents to mobilize blocs of voters. Both mechanisms stem from criminal groups' territorial control, either on the border of or within areas they dominate. By elucidating the strengths drawn from criminal governance that power these two mechanisms, this paper brings the literatures on criminal governance and clientelism closer together.

To estimate the electoral returns of gatekeeping and corralling, I examine municipal elections in Rio de Janeiro, where many voters live under criminal governance. I create an original database of criminal governance by scraping thousands of blog posts about crime in the city's 1,018 *favelas*, informal settlements. My database reveals which criminal groups governed *favelas* from 2015–2020. I combine this with fine-grained data on exogenous voter assignment to voting booths to compare voting between residents who live under criminal

1. Voter complaint 1, September 2016. All anonymous complaints are from the TRE-RJ (Regional Electoral Tribunal of Rio de Janeiro) .

governance and their neighbors who do not. I then leverage temporal variation in criminal governance to show that voting covaries with criminal control – even when holding individual voters constant.

I show that elections in criminally controlled *favelas* are less competitive. Leading candidates win by a higher margin and fewer unique candidates receive votes in criminally governed *favelas*. Back-of-the-envelope calculations suggest that 27% of elected city councilors eked out a victory because of votes mobilized from criminal brokers. To substantiate these findings, I draw on 18 months of field research in Rio de Janeiro. I pair insights from more than 50 in-depth interviews with voter complaints to show how gatekeeping and corralling operate.

This paper makes three main contributions. First, by detailing how territorial control matters for criminal governance, it differentiates criminal groups from other classes of brokers, setting the stage for future research on this crucial yet underappreciated category of political broker. Second, by testing two mechanisms through which criminal brokers influence voting – gatekeeping and corralling – I show empirically that criminal governance restricts candidates' access to voters and influences vote choice, and that these tactics reduce the competitiveness of elections. Third, this paper makes a methodological contribution. My measure of criminal governance represents an advance in the precision of the study of organized crime at the sub-municipal level.[2] My empirical design precisely identifies which voters reside under criminal governance. Overall, the findings from this paper speak to open questions about criminal influence over politics, and how certain politicians leverage criminal governance for political gain.

2 Criminal Groups as Brokers

Scholars generally agree that criminal groups can be influential political actors, but have only begun to elaborate the different roles they play (Barnes 2017). One underexplored category includes criminal groups working as political brokers. Despite being undertheorized, contractual, brokerage-style relationships have been documented worldwide between candidates and a range of criminal groups, from Indonesia to Jamaica (Arias 2017; Tajima 2018). Meanwhile, theories of clientelism introduce rich typologies of brokers, but often omit

2. The "treatment" of criminal governance is generally assigned at the municipality level, masking neighborhood-level variation. Blattman et al.'s (2023) census on gang *combos* in Medellín is a notable exception.

criminal organizations as a type of broker (Holland and Palmer-Rubin 2015; Novaes 2018; Stokes et al. 2013).

I bring together these disconnected literatures to claim that criminal groups can be highly effective political brokers, due to their governance over various dimensions of civic life. They leverage their control over a territory and its voters to undermine elections for a particular candidate, sometimes as a repeated interaction in service of their long-term goals and sometimes as a profitable opportunity in the present time period. In its simplest form, criminal groups sell their brokerage services to candidates who need votes.

Past studies cover a limited range of roles for criminal groups in politics. Existing accounts center on cases of criminal groups violently targeting candidates (Alesina et al. 2019; Daniele and Dipoppa 2017; Trejo and Ley 2020), voters (Acemoglu et al. 2013; Córdova 2019; Dell 2015; LeBas 2013), or striking deals with politicians to earn economic and political benefits (Blume 2022; De Feo and De Luca 2017). The most relevant wave of this scholarship acknowledges criminal groups' potential to mobilize voters and has generated useful concepts, such as "armed clientelism" (Eaton 2006), "divided governance" (Arias 2017), and "criminalized electoral politics" (Albarracín 2018). These studies have yet to elucidate the potential mechanisms that explain *why* criminal groups may be effective brokers or to empirically examine the relationship between criminal governance and voting. My theory builds on this body of work to enumerate two mechanisms through which criminal groups deliver votes.

2.1 The Role of Territorial Control

Criminal groups can be effective brokers because they govern territory and voters. Criminal governance extends beyond mere physical presence; it implies that the criminal group is a rule-making authority and provider of certain goods or services for residents in their area. I adopt Lessing's (2020) expansive definition of criminal governance, "the imposition of rules or restriction on behavior by a criminal organization." While there is variation in what criminal governance looks like, criminal groups that govern have some degree of control over territory and the residents who live there.[3] I highlight two aspects of criminal governance over territory that set criminal groups apart from competing brokers.

My argument differentiates between two subtypes of territorial control, which I refer to as border control and internal control. *Border control* refers to control over the perimeter of the governed area. Criminal groups commonly employ low-ranking "lookouts" to monitor

3. Criminal groups that are not territory-based are beyond the scope of this paper.

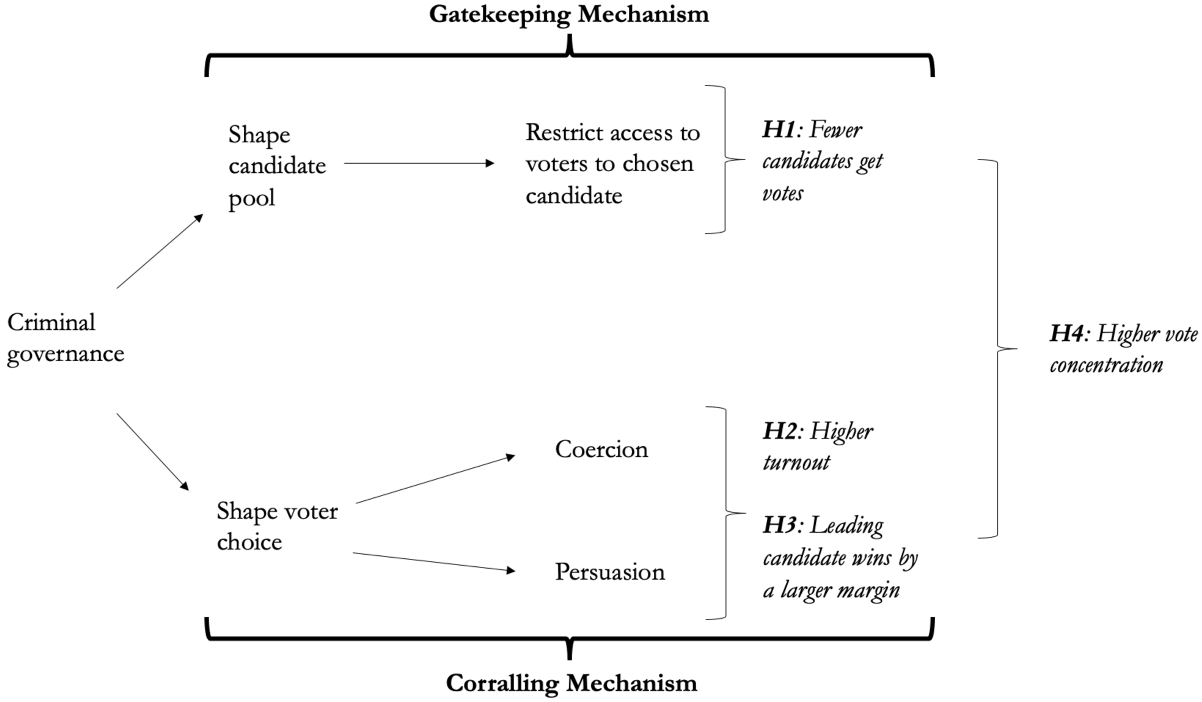
key entrances and exits to the community (Dowdney 2003). These junior members notify the higher-ups if a rival, law enforcement, or anyone suspicious tries to enter and may even question or frisk them to prevent entry. *Internal control* refers to control over what happens inside their community. Some groups accrue social capital with residents through familial bonds and norms of reciprocity; this is especially likely when criminal group members were raised in the area (Blume 2021; Dowdney 2003). In contrast, other groups use muscle to build influence and primarily instill fear amongst residents (Magaloni, Robles, et al. 2020). Internal control includes behaviors like the setting of curfews, collection of taxes from people and businesses, or dispute resolution (Lessing 2020).

Criminal groups' ability to monitor the community's perimeter and influence residents, derived from *border* and *internal* control, respectively, sets them apart from competing political brokers. They can leverage border control to choose which candidates enter and exit their area or use internal control to leverage their ties to voters to target service provision electorally. Such targeting may be especially effective, because those who live in criminally governed territories are, on average, more likely to be poor, marginalized, and vulnerable to clientelism (Leeds 1996; Zaluar 1994). Their access to coercive power further sets them apart from other brokers, which they can use to subvert electoral competition. A community organizer in Rio de Janeiro interviewed for this study explained that criminal groups' preferences overshadow those of other brokers in the area. "The pastor's, the personal trainer's, the schoolteacher's... everyone else's candidates have to get approved by the criminal organization before they can campaign here."[4]

Where criminal organizations govern, standard explanations of brokerage relationships may no longer be sufficient to describe the electoral dynamics among candidates, criminal brokers, and voters. We do not have a theory that adequately describes the electoral dynamics between voters and violent groups who control many facets of their civic lives. I suggest two new mechanisms through which criminal governance can deliver votes: *gatekeeping* and *corralling*. Figure 1 illustrates my argument, which I describe below. The three mechanism-specific outcomes (**H1**, **H2**, and **H3**) and the summary outcome (**H4**) are italicized in the center of the diagram.

4. Author's interview, NGO leader, August 8, 2018. Interview quotes are lightly edited for clarity.

Figure 1: Structure of the Argument



2.2 Candidate Gatekeeping

One way in which criminal brokers affect electoral outcomes is by *gatekeeping*, or restricting the candidate pool. Criminal groups leverage *border control* to selectively allow some candidates in, and others not. By restricting voters' access to their chosen candidates, criminal groups shape the information that voters have about the candidate pool. The observable implications of gatekeeping are that fewer candidates are known, and thus fewer receive votes in the criminally controlled area.

Criminal brokers can shape information about candidates through two channels: by providing information about favored candidates to voters, or by obstructing other candidates' access to voters. The first channel draws from informational theories of clientelism, which emphasize that brokers signal candidate viability or credibility via informational materials at campaign events or through other media channels (Kramon 2016; Muñoz Chirinos 2019).

The second channel – preventing rival candidates from accessing voters – remains unexplored. Since criminal brokers have high levels of border control around the areas they govern, they can grant *their* candidate access to voters and obstruct access to the rest.[5]

5. Criminal groups are independent brokers and, as such, can change loyalties and act

Though criminal brokers do not prohibit voters from learning about other candidates, they make it more difficult, and residents likely receive a disproportionate amount of information about the criminal group's candidate and little or no information about others. When the information voters receive is biased towards the criminal broker's candidate, this can have downstream implications for their vote choice.

The electoral returns to gatekeeping may be particularly salient in cognitively burdensome elections. For example, hundreds of candidates run in Rio de Janeiro's state and local legislative elections, and voters are only familiar with a small number of the candidates running. Candidates in such races receive little press, and grassroots word-of-mouth efforts can have a greater impact than for those in high-profile races.

This means that candidates who are gatekept out have limited options to reach voters from afar. A candidate confirmed this; after proudly declaring that he would *never* hire a criminal broker in his neighborhood, admitted that his campaign had limited reach – he was unable to distribute campaign materials even in his own neighborhood.[6] Upon hearing that the candidate was not willing to pay the criminal group's fee, even his pastor declined to hang a campaign poster outside the church.

When gatekeeping is effective, I expect it to obstruct rival candidates' access to voters, reducing the number of candidates voters learn about, which in turn prunes them from the effective candidate pool. The observable implication is that fewer candidates receive votes in criminally dominated communities, formalized in the following hypothesis:

> **H1:** In areas where criminal groups govern, fewer candidates will receive at least one vote in the community.

2.3 Voter Corralling

Another way in which criminal brokers affect electoral outcomes is by *corralling* – influencing vote choice. Criminal groups draw from their *internal territorial control* over voters to sway voter behavior on election day. Successful corralling can push voters toward criminal groups' preferred candidate in areas under criminal governance.

Criminal groups influence vote choice in two different ways: coercion (negative inducements), persuasion (positive inducements), or their joint use (Mares and Young 2016). Both

as free agents when interacting with candidates (Holland and Palmer-Rubin 2015; Novaes 2018).

6. Author's interview, Candidate 1, February 24, 2021.

operate through internal territorial control. First, criminal groups *coerce* by threatening violence if the voter doesn't select their preferred candidate. Voters likely recognize that criminal groups have access to violence and are willing to use it. Criminal groups may need to do little more than threaten a few voters to inspire fear of retaliation and raise the stakes of defection for any one individual.

Second, criminal groups *persuade* by offering voters rewards. Such rewards might be tied to existing services or benefits they already provide as criminal governors. Criminal groups might employ a range of persuasive clientelistic strategies, from vote-buying to turnout-buying to longer-term promises (Gans-Morse et al. 2014; Stokes et al. 2013), but voters might also be persuaded by criminal groups for other genuine reasons. Research shows that voters are more likely to prefer a broker from their area, who is engaged with the community, or who they identify with culturally (Auerbach 2020; Auerbach and Thachil 2018; Zarazaga 2015). Criminal groups – especially those that govern the areas in which they were raised – often satisfy these criteria and neighboring voters might be more receptive to their inducements than if they were coming from an outsider. Coercion and persuasion are not mutually exclusive, and criminal groups can and often do use both when corralling.

How might criminal brokers know if corralling was successful? First, they might measure turnout, which could be one way to assess whether the coercive or persuasive appeals worked. In Rio de Janeiro's criminally governed *favelas,* a resident observed with fear that criminal groups were "splitting up at different polling places and staying there all day on the street to ensure that the population goes to vote for their candidate."[7] At a minimum, turnout is observable, and criminal groups could measure whether or not they corralled enough people to the polls.

Second, criminal brokers might measure vote choice, both by monitoring throughout the day and by looking at vote tallies once the polls close. Residents of Rio de Janeiro claim that criminal groups demand cell phone photos of their electronic ballot before making good on a clientelistic promise. There are reports of criminal groups threatening voters with group-level violence if their candidate doesn't earn enough votes. I hypothesize that criminal corralling influences voter behavior in two related ways:

> **H2:** In areas where criminal groups govern, voters will turn out at a higher rate.
>
> **H3:** In areas where criminal groups govern, the leading candidate will capture a greater share of the vote.

7. Voter complaint 2, November 2020.

2.4 Reduced Electoral Competition

I further measure how gatekeeping and corralling interact by considering their joint effect on electoral competition. For a criminal broker, gatekeeping is only of use if votes are being redirected from outside contenders to the criminal group's candidate. Similarly, corralling could become less impactful if the electoral playing field is crowded with many candidates.

I argue that either mechanism should reduce competition. *Gatekeeping* reallocates votes to the criminal broker's candidate through the information channel. As it becomes increasingly difficult for voters to learn about outside option, voters receive disproportionate information about the criminal group's candidate, which prunes low-information competitors from their local candidate pool. *Corralling* reduces competition by influencing vote choice. When criminal groups monitor turnout and group-level returns, they have the potential to sway undecided voters *and* coax nonvoters to the polls, all of which drives competition downward.

The observable implication is that criminal brokerage makes returns more highly concentrated, meaning that a larger share of votes will go to one or a few top candidates. As shown in Figure 1, vote concentration is a summary indicator that measures the effects of gatekeeping, corralling, or their joint use. I hypothesize:

> **H4:** In areas where criminal groups govern, vote returns will be more concentrated.

Together, my four hypotheses suggest that criminal governance undermines elections by limiting candidate choice and influencing voters to select the criminal broker's candidate, all of which reduce competition. These four outcomes are best analyzed side-by-side for a few reasons. First, I expect gatekeeping to obstruct information about lower-ranking candidates, reducing competition by pruning them from the neighborhood's effective candidate pool. **H1** provides evidence that low-vote-earners are being pruned while **H4** confirms that their votes are being transferred to leading candidates. Second, though I expect corralling to affect vote choice, measuring it suffers from clientelism's classic problem of monitoring at the individual level (Hicken and Nathan 2020; Stokes et al. 2013). Though anecdotal and journalistic evidence suggests that criminal groups can monitor at the individual level, enforcement is likely imperfect. To account for this, **H2** measures turnout, which is easily monitorable, and **H3** estimates group-level vote choice, which is ultimately more influential but harder to measure. **H4** predicts that corralling has a large enough effect that it will reduce competition at the group level.

3 Candidates, Criminal Groups, and Voters in Rio de Janeiro

3.1 Candidates

This paper focuses on subnational legislative elections. This is because a criminal group's support may more easily tip the scale in such contests than in an executive election or a national race. Brazil's electoral rules facilitate intense competition for votes. Legislative elections follow an open-list proportional representation system, and take place in multi-member single districts. Though candidates often informally carve out their own *redutos* ("electoral fortresses") of neighboring communities (Ames 2009), all candidates are elected at-large and represent the entire jurisdiction. Political parties may submit up to 1.5 times the number of candidates for each at-large seat. Given Brazil's weak party system and the large number of parties vying for seats (Mainwaring et al. 2018), this means that thousands of candidates may run for legislative office. Some are serious contenders, others are *laranjas* (non-viable candidates) who garner a small number of votes but add to the party's total, and many are on the margin. I expect these marginal candidates to benefit the most from – and therefore be most likely to – hire criminal groups as brokers.

The city of Rio de Janeiro has 51 city council seats. If every political party submitted a maximum of 1.5 times the number of seats, 77 candidates could run from *each* party. In the 2016 city council election, 1,628 candidates affiliated with 35 different parties competed for the 51 seats (the mean party fielded 46.5 candidates, while eight political parties fielded the maximum value). The proliferation of candidates means that dozens, if not hundreds, of candidates are on the margin for each legislative race. For the marginal candidate, every vote matters.

3.2 Criminal Groups

Criminal groups govern approximately 1.2 million of the city's 6.74 million inhabitants (Satriano 2020). These voters live in hundreds of criminally governed communities of various sizes, primarily *favelas*, which are scattered on hilly terrain throughout the city. *Favelas* are densely populated, lower-income settlements that are characterized by incredibly high population density and poor-quality infrastructure, service delivery, and public security (Perlman 2010). They are often contrasted with the surrounding neighborhoods, colloquially "the asphalt," which are located on the flat land adjacent to *favelas* that is more likely to be incorporated in the formal sector. Average income levels on the asphalt range from low to very high, whereas *favela* neighborhoods are more uniformly lower income. The government neglected residents in *favelas* long before the birth of today's criminal groups (Zaluar 1994), but these inequalities have widened as criminal groups began conducting their illegal operations. After decades of unequal development vis-à-vis the rest of the city, most of the city's *favelas* are now governed by organized criminal groups.

Two main categories of criminal groups govern *favelas*. The first is drug trafficking organizations, which use the *favelas* they control for tasks related to their drug businesses, such as storing, packaging, transporting, or selling drugs (Arias 2006). Each *favela* governed by one of the three major drug trafficking organizations[8] operates as a decentralized member of a network, somewhat like a franchise. The second type of criminal group in Rio de Janeiro is vigilante-style extortion rackets, called *milícias* (Cano and Duarte 2012). The *milícias* – often associated or formerly associated with law enforcement – extort residents and local businesses for "security" payments and control access to utilities, transportation, and loans. *Milícias* are also very decentralized; neighborhood-level leaders call the shots in their communities, and they can obtain reinforcements from other leaders when fighting traffickers, if need be.

The decentralized nature of both of these group types means that there are hundreds of "criminal kingdoms" that govern residents (Arias 2006). Each *favela*'s criminal leader has significant autonomy to dictate the day-to-day rules and agreements governing residents in their territory. Magaloni, Franco-Vivanco, et al. (2020) enumerate five governing styles that influence how criminal groups treat residents and law enforcement in their territory, noting that there may even be variation between *favelas* governed by the same group. Each criminal group leader in a different *favela* will have a distinct set of arrangements with the state, local community leaders, the police, and residents.

3.3 Voters and Elections

Rio de Janeiro's *favelas* have the potential to be politically powerful solely due to the volume of people who live there. These voters can be powerful at the ballot box if they coordinate (Cooperman 2023; Perlman 2010) or are coordinated by a broker (Gay 1999). For example, *Rocinha*, Rio de Janeiro's most populous *favela*, contains approximately 70,000 registered

8. These are ADA (*Amigos dos Amigos*, Friends of Friends), CV (*Comando Vermelho*, Red Command), and TCP (*Terceiro Comando Puro*, Third Pure Command).

voters. If coordinated, *Rocinha* voters could theoretically elect several city council representatives unilaterally.

Given the intense competition for votes, it should be no wonder that criminal leaders see campaigns as a profitable opportunity. The highly localized nature of criminal governance means that criminal group-candidate deals can vary by *favela*: if a candidate pays a criminal group for votes in one, this does not purchase access to all *favelas* dominated by that faction. Criminal groups can thus charge a premium for brokerage fees and profit from the decentralized market structure. Local criminal brokers often set a flat rate for gatekeeping and corralling in their community, rather than subcontracting on a per-vote basis (Araújo and Otávio 2018). The flat rate typically includes exclusive or nearly exclusive access to the neighborhood's voters. Some criminal leaders might offer other brokerage services for an additional fee, such as hosting campaign events or door-to-door canvassing. Candidates are aware that the price of entry doesn't extend far beyond the *favela* boundary. One candidate described paying criminal leaders for "private security" to safely campaign in two neighboring *favelas* dominated by the same faction,[9] but noted that he did not expect to gain votes in other favelas where he did not personally purchase access.

4 Data

4.1 Criminal Governance

I construct an original panel database mapping the territorial boundaries of criminal governance of *favelas* in the city, using information scraped from a local crime blog. My database covers Rio de Janeiro's 1,018 *favelas* from January 2015 to January 2020. I measure the governing criminal faction in each *favela* by scraping the blog *Crimes News RJ*, which publishes in-depth reports on criminal warfare in the city. This blog reveals the governing criminal faction in most *favelas*, using a consistent coding pattern to denote criminal governance.[10] I used standard text processing tools to extract every blog mention of a *favela*'s name and faction, and then geolocate the *favela*, described in greater detail in Appendix A1.

My criminal governance variable takes one of five values: one of the three drug trafficking organizations, 1) ADA, 2) CV, 3) TCP, 4) the extortion racket *milícias*, or 5) no data, for *favelas* that were not mentioned in any of the blog posts. Across the panel, I was able to assign a criminal group leader to 77% of *favelas*, covering 93% of the *favela* population. Table

9. Author's interview, Candidate 2, November 14, 2018.
10. See Appendix A1.2 for a discussion of criminal governance versus presence.

Table 1: *Favela* Distribution by Criminal Group, October 2016

Faction	N *Favelas* (%)	N Residents **Mean**	N Residents **Median**	N Residents **Total** (%)
ADA	111 (11%)	2,139	661	237,432 (17%)
CV	338 (33%)	1,766	796	596,893 (42%)
TCP	84 (8%)	2,358	656	198,069 (14%)
Milícias	249 (24%)	1,208	442	300,707 (21%)
No data	236 (23%)	432	211	101,874 (7%)
Total	1,018	1,410	476	1,434,975

Note: This is the distribution of *favelas* and *favela* residents, according to the governing criminal group, right before the October 2016 municipal election. Source: author's calculation.

1 shows a cross-section of the database. Most *favelas* are governed by criminal groups; the CV (42%) and *milícias* (21%) govern the most residents, and a mere 7% of *favela* residents lack or have no clear criminal governance. The majority of the "No data" *favelas* are small settlements with a mean population of 432 residents (compared to a mean of 1,577 for criminally governed *favelas*). Though a lack of data does not guarantee criminal groups' absence, their smaller size suggests the "no data" *favelas* are less strategically important for criminal organizations and less pivotal electorally.

4.2 Electoral Assignment

The second data source sheds light on *where* criminally governed residents vote. In Brazil, all voters are assigned to a voting booth when they register; this assignment carries over year-to-year unless they make a change to their registration or become inactive. The voting booth is the smallest unit at which results are reported; approximately 300-500 people vote per booth and typically there are many booths inside the same polling station (e.g., a primary school).

Voters are assigned to a booth in a 2-step process. First, when voters register, they can choose the polling station where they want to vote, as long as it is in their electoral district.[11] Most choose polling stations close to their homes.[12] Second, once assigned to their polling

11. The electoral districts, *zonas eleitorais*, are federal reporting units encompassing many polling stations.

12. There is a small but nonzero amount of voters who do not change their registration

station, voters are randomly assigned to one of several available voting booths *within* the station. This assignment procedure is conducted electronically at the federal level by the national electoral office (TSE). The software aims to reach an equal number of voters across voting booths in a polling station, and randomly assigns new registrants accordingly.[13] At times, this leads to the creation of a new voting booth if existing ones are at capacity or if there is a surge in new registrations.

The implications of this assignment process are that voters who live in the same *favela* vote at many nearby locations, and intermingle with voters who live on the asphalt. Generally, polling stations are not located inside of *favelas* due to accessibility and building code standards, so *favela* residents must leave their communities to vote.

I use new data to look *within* the voting booth and measure the share of voters who live in a *favela*, which I explain in more detail in Appendix A2 and summarize here. I filed an information request with the ombudsman's office of the Regional Electoral Tribunal of Rio de Janeiro (TRE-RJ) for data indicating the proportion of voters per voting booth that lived in postal codes located inside *favelas*.[14] After geocoding all postal codes within Rio de Janeiro, I provided the ombudsman's office with a list of postal codes inside *favelas*. For every voting booth, I obtained a number between 0 and 1, indicating the percentage of voters who live in zip codes inside each *favela*.

The remaining analyses in this paper focus on voting booths where *favela* residents vote. Figure 2 shows the distribution of the share of *favela* voters per booth. The mean voting booth in the sample has 30% of *favela* residents. The average range in $share_{max} - share_{min}$ within any one polling station is 10%, but there are many extreme examples. For instance, in the *Colégio Nacional* polling station, *favela* residents comprise 11% of voters in one booth, 55% in another, and a few classrooms down the hall, 84% of the voters at a different booth.

The geographic fuzziness in voter assignment has been an obstacle in past studies of *favela* voting. My approach reveals that *favela* and asphalt residents intermingle at the voting booth, even in the polling stations closest to *favela* boundaries.

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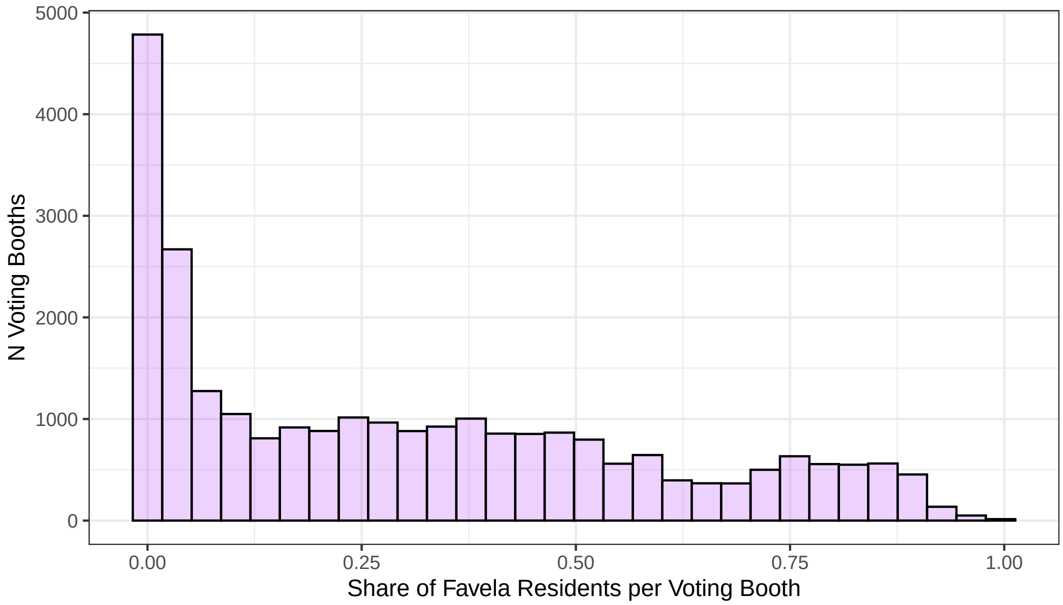
after moving, and travel to their former polling station on Election Day to cast a ballot.

13. Resolution N. 23.544 of the *Tribunal Supremo Eleitoral* (TSE), Articles. 41, 44, and 66.

14. Brazil uses nine-digits postal codes, which are often as small as a city block in urban areas. Each *favela* contains many postal codes.

Figure 2: Share of *Favela* Residents per Voting Booth



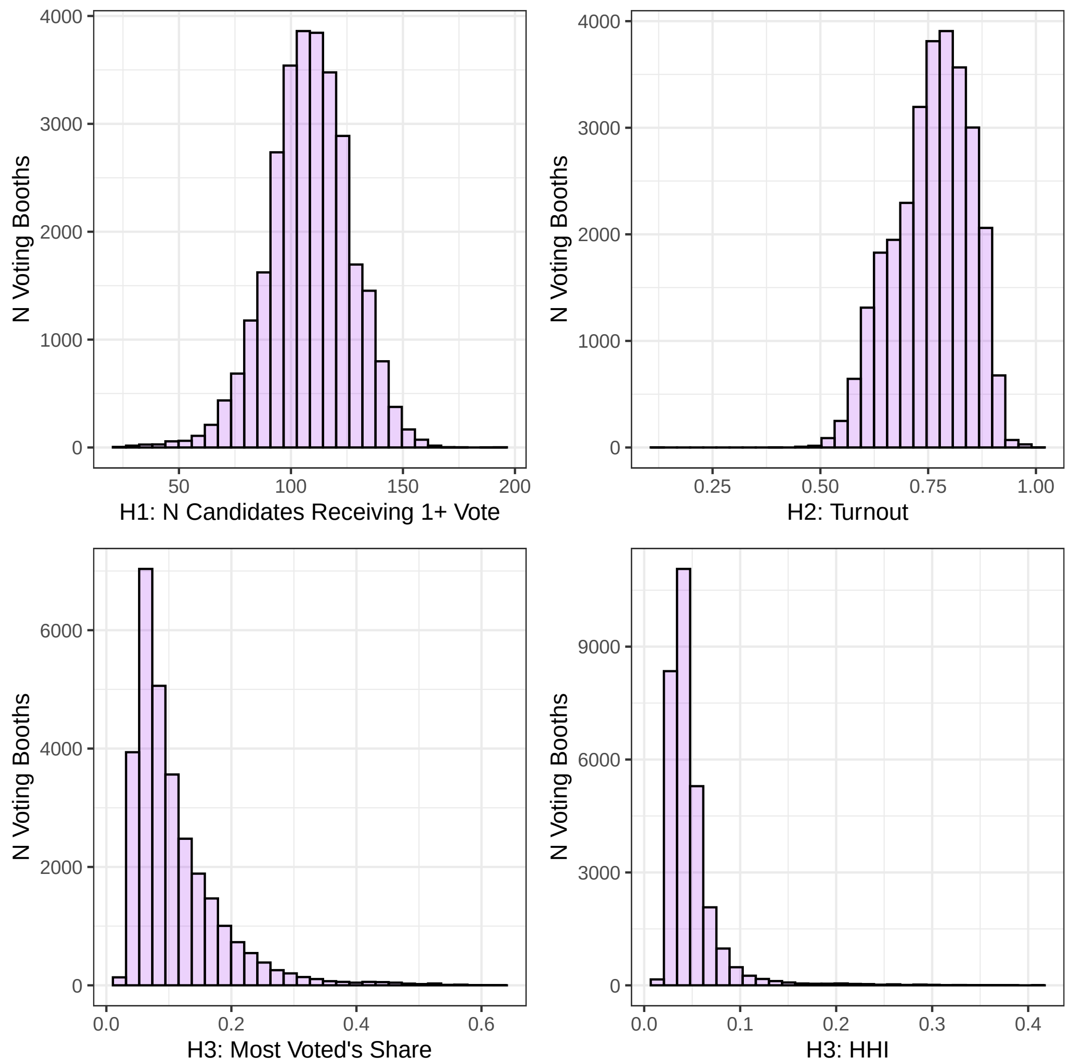
4.3 Electoral Outcomes

I use voting-booth-level returns data from the Supreme Electoral Court (TSE) to calculate electoral outcomes, one for each of my four hypotheses: 1) the number of candidates receiving votes, 2) turnout, 3) the local winner's vote share, and 4) vote concentration. Figure 3 shows each outcome variable's distribution.

The first outcome, the number of candidates receiving votes, is an indicator of gatekeeping. If criminal territorial control negatively affects outside candidates' access to voters, my hypothesis suggests that we will observe criminal groups pruning the effective candidate pool in their *favela* communities. I operationalize this hypothesis by summing the number of candidates that receive at least one vote per voting booth.

The second and third outcomes are indicators of voter corralling. Turnout, the number of registered voters by the total ballots cast, measures whether or not criminal groups push more people to the polls on election day. Brazil is a particularly rigorous case in which to test this hypothesis, because voting is mandatory and compliance is high (approximately 80% in the sample's years). Studies of turnout in clientelistic contexts (Nichter 2008) acknowledge that compulsory voting reduces variation in turnout. The third outcome is the share of ballots cast for the most popular candidate at any given voting booth. This measures whether or not criminal groups actually influenced vote choice, either through the use of coercion, persuasion, or both.

Figure 3: Histograms of Outcome Variables



Finally, the fourth outcome is a summary indicator of electoral competition, which can result from corralling, gatekeeping, or their joint use. I use the Herfindahl-Hirschman index (HHI), a measure of monopoly concentration commonly used in studies of electoral competition. For every voting booth b, I construct the HHI by summing the share of votes received (v_c) of all candidates $c \in \{1, ...n\}$ who ran for city council in a given year:

$$HHI_b = \sum_{c=1}^{n} v_c^2$$

HHI_b takes on a value between 0 and 1, where 1 means that one candidate receives 100% of the votes in a voting booth. These four measures are useful together because they capture slightly different elements of electoral competition. Together they can demonstrate that not only is the vote share growing more concentrated, but that votes are being redirected from low vote-earners to the leading candidate.

5 Research Design

I use as-if random causal evidence to estimate the effect of criminal governance on voting. A key assumption underpinning this paper's empirical strategies is that criminal governance is restricted to Rio de Janeiro's *favelas*.[15] Decades of qualitative work has documented different voting patterns between *favela* and asphalt residents (Gay 1993; Holston 2009; Perlman 2010). This paper follows a related line of reasoning about the prevalence of clientelism in *favelas*, especially those governed by criminal groups (Gay 1999).

This paper first uses a natural experiment to demonstrate that criminally governed *favela* residents' voting behavior differs from their neighbors' on the asphalt *and* from *favela* residents with no clear criminal ruler (left panel of Figure 4). This design solves the ecological inference problem by leveraging the as-if-random fraction of *favela* voters casting ballots in each voting booth, yielding the most precise known estimates of *favela* voting behavior. The second empirical strategy confirms that criminal governance is driving the results. I isolate the effect of criminal governance from the broader class of variables associated with *favela* life using a difference-in-differences design, which leverages temporal variation in criminal governance between *favelas* (right panel of Figure 4).

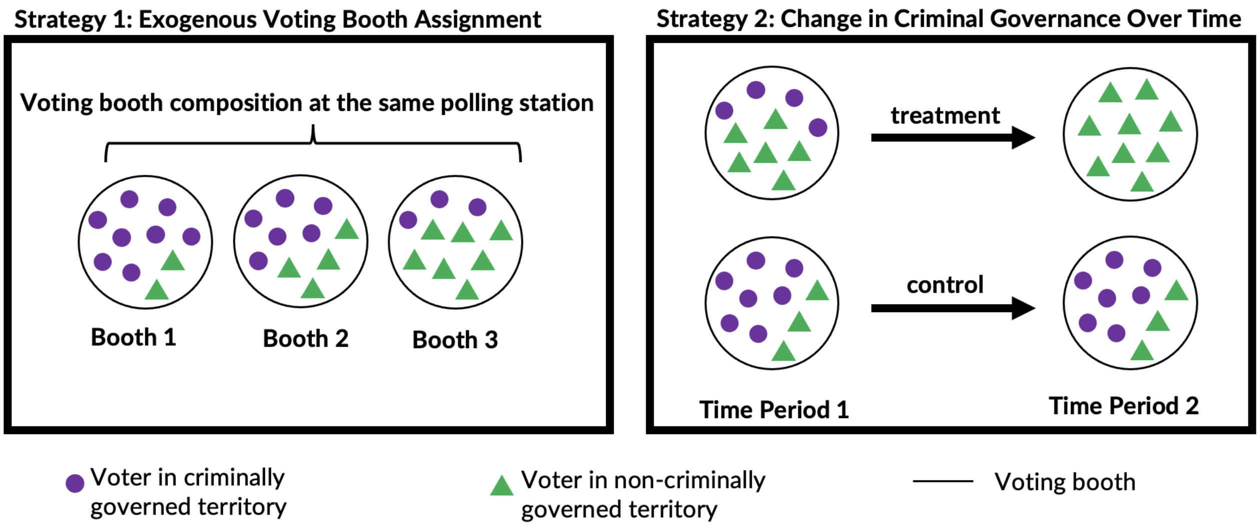
5.1 Empirical Strategy 1: Exogenous Voting Booth Assignment

Ideally, my hypotheses would be evaluated by randomly assigning voters to live under the criminal governance "treatment," and then measuring voting behavior, but this is clearly unethical and impractical. Instead, I approximate this design by leveraging Brazil's exogenous voting booth assignment procedure, outlined in Section 4.2.

Favela voters comprise the population of interest. For every booth in which a *favela* resident votes, I obtained the proportion of *favela* residents assigned to vote there. I leverage

15. While criminal governance is not necessarily a feature of *all favelas*, criminal groups govern most *favelas* and *favela* voters. In *favelas* controlled by criminal groups, this power only extends to the *favela* boundary, and they usually are the sole governing faction.

Figure 4: Empirical Strategies



Note: The design in the left panel leverages exogenous assignment to voting booths. In this stylized example, at a polling place with three booths, Booth 1 has the highest share of criminally governed voters and Booth 3 has the lowest. The right panel illustrates a simplified version of the difference-in-differences design. The voters in the treatment *favelas* are no longer governed by a criminal group in the second time period.

the imbalance that results from as-if-random voting booth assignment. The exogenous imbalance in the share of *favela* voters at any one booth allows me to identify the causal effect of *favela* residency on voting as if it were a natural experiment. To isolate the effect of *favela* residency on voting behavior, I estimate the following pooled equation where i corresponds to each voting booth and t corresponds to election year:

$$Y_{it} = \alpha + \beta_1(ShareFavela_i \times CrimGov_{it}) + \eta\mathbf{X} + \pi_p + \rho_f + \gamma_t + \epsilon_{i,p} \qquad (1)$$

where Y_{it} represents one of the four electoral outcomes. $ShareFavela_i$ represents the share of voters at the voting booth residing in *favelas*. $CrimGov_{it}$ is a dummy variable that takes on a value of 1 if the *favela* with the highest share of voters at the voting booth is criminally governed in that year. I include a vector of control variables ($\mathbf{X}$), and polling station (π_p), *favela* (ρ_f), and election-year intercepts (γ_t), and cluster standard errors at the polling-station-level (p). The coefficient of interest is β_1.

5.2 Empirical Strategy 2: Change in Criminal Governance Over Time

The second strategy addresses challenges to inference regarding the bundled treatment of *favela* residency *and* living under a criminal governance regime. Despite the inclusion of the interaction term in Equation 1, there might still be questions of whether differences in voting are due to criminal governance or to other factors related to informality. This is a reasonable doubt, given that only 7% of *favela* voters do not live under criminal governance. I tackle this challenge to inference by analyzing how voting behavior changes with a change in criminal governance while holding the *favela* and individual voters constant.

I leverage the staggered implementation of a large-scale public security reform to identify all *favelas* where criminal groups ceased to govern immediately prior to the 2012 municipal election. I compare voting behavior to that in similar *favelas*, also targeted by the reform immediately *after* the 2012 municipal election. Rio de Janeiro's UPP (Police Pacifying Units) program, the reform, was designed to expel organized crime from *favelas* and reincorporate them under state governance (Lessing 2017; Magaloni, Franco-Vivanco, et al. 2020). In each *favela*, the UPP program began with a militarized police raid on a given date, intended to expel drug traffickers, and kicked off the slow process of reincorporating the *favela* into state governance. In the short-run, the state was incredibly successful at ridding the *favelas* of criminal rule post-raid.

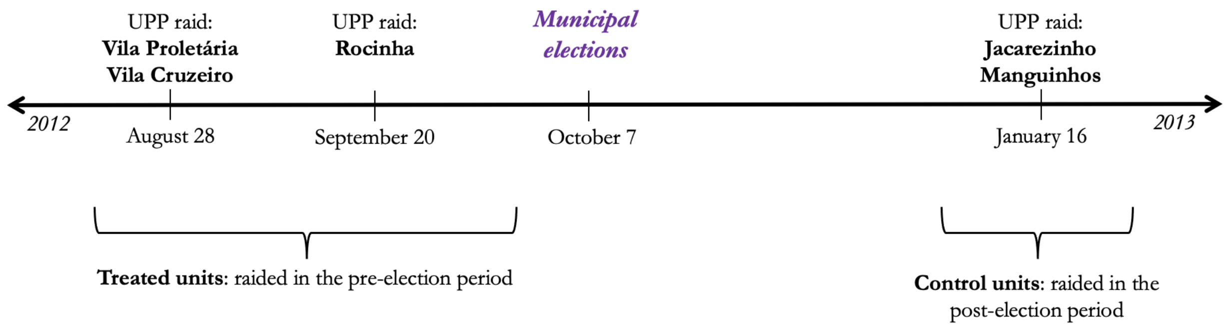
This study analyzes voting in five *favelas*, all of which were governed by criminal groups during the 2008 municipal election. All five were then raided by police in the three months before or after the 2012 municipal election, as shown in the timeline in Figure 5. These *favelas* (*Jacarezinho, Manguinhos, Parque Proletário, Rocinha,* and *Vila Cruzeiro*) were considered strongholds for various drug trafficking gangs.

In comparing criminally governed *favelas* that were raided immediately before or after the 2012 election, I show how a disruption in criminal leadership affects voting outcomes while other *favela*- and voter-level factors are held constant. In *favelas* raided *after* the election, the same criminal group and voters are present for two continuous electoral cycles. In *favelas* raided *before* the election, the same voters likely reside there[16] and their material conditions have changed little, if at all, but the sudden absence of criminal governance disrupts relationships between voters, candidates, and other brokers.

I use a difference-in-differences framework to exploit temporal and spatial variation in

16. There is low *favela* out-migration over such a short period of time (Perlman 2010).

Figure 5: Timeline of UPP raids and 2012 municipal election



criminal governance. I compare electoral outcomes in the municipal election of 2008 to those in 2012, when "treatment" *favelas* received a UPP raid in the months before the 2012 municipal election. The estimating equation for this model is:

$$Y_{it} = \alpha + \beta_1(Raid_f \times Year_t) + \beta_2 ShareFavela_i + +\eta\mathbf{X} + \pi_p + \epsilon_{i,p} \tag{2}$$

where Y_{it} is one of four electoral outcomes of interest for voting booth i in election year t. $Raid_f$ takes on a value of 1 for voting booths in *favelas* raided before the 2012 election (*Parque Proletário, Vila Cruzeiro, Rocinha*), while it takes on a value of zero for those raided following the election (*Jacarezinho* and *Manguinhos*). $Year_t$ corresponds to either 2008 or 2012, and I include the share of residents living in the *favela*, a vector of control variables ($\mathbf{X}$), polling-station-level intercepts (π_p), and cluster standard errors at the polling station level (ϵ_p). β_1 is the coefficient of interest.

6 Results

Section 6.1 reports results from the natural experiment and Section 6.2 from the difference-in-differences. I conduct a range of robustness checks in Section 6.3 and interpret aggregate results in Section 6.4. The unit of analysis for all models shown is the voting booth. I report results from city council races in the city of Rio de Janeiro in 2008, 2012, 2016, and 2020.[17]

17. For analyses focusing on 2008 and 2012, which predate the criminal governance database, I use the 2016 faction assignment.

6.1 Exogenous Voting Booth Assignment

The results reported in Table 2 correspond to the natural experiment described in Section 5.1. All models analyze the effect of an increase in the share of criminally governed *favela* residents at the voting booth ($ShareFavela \times CrimGov$) on one of four outcome variables that map onto my core hypotheses. I present two models per dependent variable, and all tell a story consistent with my predictions: an increase in the share of *favela* voters at the voting booth corresponds to 1) fewer candidates receiving votes, 2) the local victor capturing a greater proportion of the votes, and 3) a more concentrated vote share.

Columns 1 and 2 show that the number of candidates to receive at least one vote decreases as the share of criminally governed *favela* residents per voting booth increases. We should expect 13.1 fewer candidates to receive votes at a voting booth with a high share of criminally governed *favela* voters (75%) than at an otherwise identical voting booth with a low share of criminally governed *favela* voters (25%). This supports the argument that criminal groups prune the candidate pool by gatekeeping. As the share of criminally controlled residents increases across voting booths, the number of candidates the voters choose decreases.

Table 2 provides supporting evidence that criminal groups are also corralling voters on election day. Columns 5 and 6 show that the leading candidate captures, on average, 2.6% more of the vote (9 more votes) in voting booths with a high share of *favela* voters (75%) compared to those with a low share of *favela* voters (25%). This supports the argument that criminal groups' coercion or persuasion strategies are effective enough to actually influence vote choice.

While I find little support for the hypothesis related to turnout, this is not entirely surprising and is still informative. It is not surprising because we should expect low variation in contexts with obligatory voting and where baseline levels are high, such as Brazil (Gans-Morse et al. 2014). Yet a null finding is informative because it suggests that criminally governed voters still turn out at approximately the same rates as their neighbors on the asphalt or in non-criminally governed *favelas*. Equal turnout rates are not a guarantee; other studies argue that criminal governance generates disillusionment which depresses voter turnout (Córdova 2019).

Columns 7 and 8 present results for electoral competition as a summary indicator. The vote is more concentrated for top candidates as the share of *favela* residents increases across voting booths. The difference in HHI between voting booths with a high (75%) and low (25%) share of *favela* voters is .011, which would be equivalent to a leading candidate passing from capturing 40% to 45% of the vote share while all other candidates capture 10% or less of

Table 2: Effects of Residency in Criminally Dominated *Favela* on Voting

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ShareFavela ×*CrimGov*	-26.3**	-26.2**	0.013	-0.027	0.050***	0.035*	0.028***	0.022***
	(10.6)	(12.2)	(0.095)	(0.110)	(0.019)	(0.018)	(0.009)	(0.008)
ShareFavela	-6.40	-14.0	0.085	0.002	0.033***	0.033***	0.020***	0.017***
	(10.1)	(11.8)	(0.095)	(0.112)	(0.013)	(0.012)	(0.006)	(0.005)
CrimGov	100.6	664.6	-0.883	-3.04	-0.407	-0.068	-0.731	-1.34
	(79,321.1)	(91,040.9)	(262.4)	(455.0)	(204.3)	(211.2)	(60.2)	(70.8)
Favela-level variables								
Distance		1.49***		0.003		-0.0005		-0.0005
		(0.408)		(0.002)		(0.0009)		(0.0003)
Population		488.3		-2.24		-0.059		-0.947
		(150,904.1)		(606.0)		(306.9)		(104.3)
Booth-level variables								
Age		-0.194***		-0.004***		-0.001***		-0.0005***
		(0.067)		(0.0003)		(0.0002)		(6.89×10^{-5})
Education		6.39***		0.021***		-0.017***		-0.006***
		(0.804)		(0.003)		(0.003)		(0.001)
% Women		-11.7***		-0.002		0.025***		0.005***
		(1.19)		(0.005)		(0.003)		(0.001)
% Married		-8.57***		-0.119***		0.020**		0.0004
		(2.78)		(0.011)		(0.008)		(0.003)
N	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.569	0.587	0.698	0.772	0.644	0.643	0.647	0.645
D.V. Mean	108.674	108.674	0.763	0.763	0.108	0.108	0.047	0.047
Station F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Year F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Favela F.E.	✓	✓	✓	✓	✓	✓	✓	✓

Note: Includes polling station, year, and *favela* fixed effects and clustered standard errors at polling station level: 1,378 clusters. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

the vote. This result, coupled with the mechanism-specific results in columns 1 through 6, tells a consistent story. Not only are some candidates being pruned from the candidate pool as the share of *favela* voters increases, but the votes are also being redirected to the highest earners in the area.

As in every natural experiment, there is the risk that pre-treatment confounders may be driving the results. The inclusion of yearly, *favela*, and polling-station-level fixed effects demonstrates that the differences in voting behavior are neither unique to one moment in time nor idiosyncratic to a specific area. To further minimize these concerns, the even-numbered columns include *favela*-level control variables and voting-booth-level demographic characteristics.[18] That the results do not substantively change after controlling for demographics suggests that poverty or informality alone do not explain the differences in voting behavior between different types of *favela* and asphalt residents. The distance from the *favela* to the polling station and the *favela*'s size address concerns that only the most pivotal *favelas* would be capable of swaying the vote.

6.2 Change in Criminal Governance Over Time

I examine how changes in criminal governance affect voting over time, using the difference-in-differences design specified in Section 5.2. Since the treatment is a raid that *removes* criminal groups' ability to govern, the predicted sign for the dependent variables will be opposite those in Section 6.1. I present a parsimonious model alongside one with fixed effects and a vector of pre-treatment control variables in Table 3. *Favelas* that were raided before the election had 1) more candidates winning votes, 2) the local victor leading by a smaller margin, and 3) more competitive races, when compared to similar *favelas* raided after the election. Figure A5.1 in Appendix A5 plots parallel trends and confirms that the treatment *favelas* are driving the effects. These results provide further supporting evidence that it is a "criminal governance," not a "*favela* residency" effect driving voting behavior.

Columns 1 and 2 show that nearly 8 more candidates earn votes per voting booth in *favelas* that were raided prior to the election. The disruption in criminal governance caused by the raid could have opened the door for a wider range of candidates to campaign. Meanwhile, in the *favelas* raided after election day, my theory suggests that criminal groups were gatekeeping and obstructing information about candidates, reducing the number of vote-earners.

18. See Appendix A3 for descriptions of the control variables and a discussion about how voting-booth-level demographic factors interact with *favela* residency.

The UPP raids likely decreased the effectiveness of corralling. Columns 5-6 suggest that a raid decreased the local winner's margin of victory by 8.9 percentage points. The mean value of the variable is 0.122; such a drop would be equivalent to a 73% decrease and substantially level the playing field. This suggests that the raid disrupted leading candidates' (and their criminal brokers') ability to effectively consolidate votes behind closed doors. As in the first empirical strategy, the results for turnout are inconclusive, likely for the reasons mentioned in Section 6.1.

Finally, the absence of criminal gatekeeping and corralling may have increased local electoral competition. The vote share is less concentrated in *favelas* that were raided pre-election, indicating that elections became more competitive after criminal groups fled. The magnitude of the effect, regardless of model, is the substantive equivalent to the leading candidate capturing 5% less of the vote share vis-à-vis leading candidates in *favelas* raided post-election.

Why would recently raided *favelas* have more competitive races than those that are raided right after the election? In *favelas* raided pre-election, any remaining criminal group members might be weak or in hiding after the raid, and unable to engage with politics. In such *favelas*, the absence of one central local authority could generate disorder and uncertainty, for both voters and candidates.

6.3 Robustness

I explore the robustness of the results through a variety of tests, beginning with a closer examination of the results in Table 2. Table A5.1 separates the pooled regressions by year and Table A5.2 restricts the sample to polling stations within 500 meters of the *favela* boundary. These tests ensure that any one election year isn't driving the results and that the findings still hold amongst the polling stations closest to *favela* boundaries. I then estimate different model specifications to alleviate concerns about criminal governance and *favela* residency as a bundled treatment. Table A5.3 drops the criminal governance dummy, Table A5.4 drops the interaction term, and Tables A5.5 and A5.6 replace the criminal governance dummy with two different indicators for type of criminal group. All of the above models retain the same sign and roughly the same magnitude as the preferred models. These tests substantiate the claim that criminal brokers of all types engage in gatekeeping and corralling.

I conduct sensitivity tests of the dependent variables for **H1** (the number of candidates receiving votes) and **H3** (the most voted's share). I operationalize **H1** as the number of candidates that receive at least two or at least five votes, and operationalize **H3** as the vote

Table 3: Effects of Criminal Governance on Voting Over Time

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Raid × 2012	7.80***	7.91***	-0.003	-0.004	-0.090***	-0.089***	-0.025***	-0.025***
	(2.14)	(2.21)	(0.004)	(0.004)	(0.009)	(0.009)	(0.003)	(0.003)
Raid	-20.4***	-0.381	0.011*	0.084***	0.126***	0.065***	0.037***	0.009***
	(3.73)	(2.39)	(0.006)	(0.004)	(0.014)	(0.008)	(0.006)	(0.002)
2012	0.196	0.967	-0.024***	-0.022***	0.020***	0.021***	0.007***	0.008***
	(0.859)	(0.966)	(0.002)	(0.002)	(0.005)	(0.005)	(0.001)	(0.001)
ShareFavela	10.5	-56.0***	0.063***	-0.013	0.102**	0.292***	0.031*	0.137***
	(8.79)	(19.8)	(0.022)	(0.029)	(0.043)	(0.066)	(0.018)	(0.035)
Favela-level variables								
Distance		-12.3**		-0.007		0.052***		0.023***
		(5.99)		(0.015)		(0.013)		(0.007)
Population		6.57**		-0.008*		-0.002		-0.0007
		(3.04)		(0.004)		(0.011)		(0.002)
Booth-level variables								
Age		0.195		-0.003***		-0.003***		-0.002***
		(0.327)		(0.0009)		(0.0008)		(0.0005)
Education		12.4***		0.012		-0.039***		-0.016***
		(2.59)		(0.010)		(0.012)		(0.005)
% Women		-8.71*		0.022		0.009		-0.0006
		(4.74)		(0.014)		(0.010)		(0.004)
% Married		-7.39		-0.109***		0.091***		0.040**
		(13.9)		(0.036)		(0.030)		(0.015)
N	963	952	963	952	963	952	963	952
R^2	0.251	0.670	0.119	0.619	0.440	0.810	0.287	0.752
D.V. Mean	110.097	110.097	0.811	0.811	0.122	0.122	0.046	0.046
Station F.E.		✓		✓		✓		✓

Note: Models 2, 4, 6, and 8 include polling station fixed effects. All models cluster standard errors at the polling station level: 68 clusters. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

share for the top five candidates (Columns (1)-(6) of Table A5.7). Results are consistent with the main findings.

I address two other potentially confounding variables relevant to the pooled models in Table 2. First, it could be that voters are spoiling their ballot in protest rather than voting for the criminal group's candidate. Columns (7)-(8) of Table A5.7 report null findings when the share of spoiled ballots is a placebo outcome, and Table A5.8 controls for spoiled ballots. Second, it is possible that the UPP raids, not criminal governance, reduce competition by sowing disorder and obstructing voters' efforts to organize. Table A5.9 drops all *favelas* that ever received a UPP from the sample.

I probe the findings from the difference-in-differences estimator, none of which change the substantive results or their interpretation. First, the models in Table A5.10 leave one *favela* out sequentially to ensure that one *favela*'s voters are not driving the effect. Second, in Table A5.11, I test the treatment group against a more expansive control group, all *favelas* that received UPPs. Third, Table A5.12 shows a dose-response design to estimate a continuous treatment effect, where the strength of the treatment is determined by *ShareFavela*.

6.4 The Aggregate Effect of Criminal Brokers

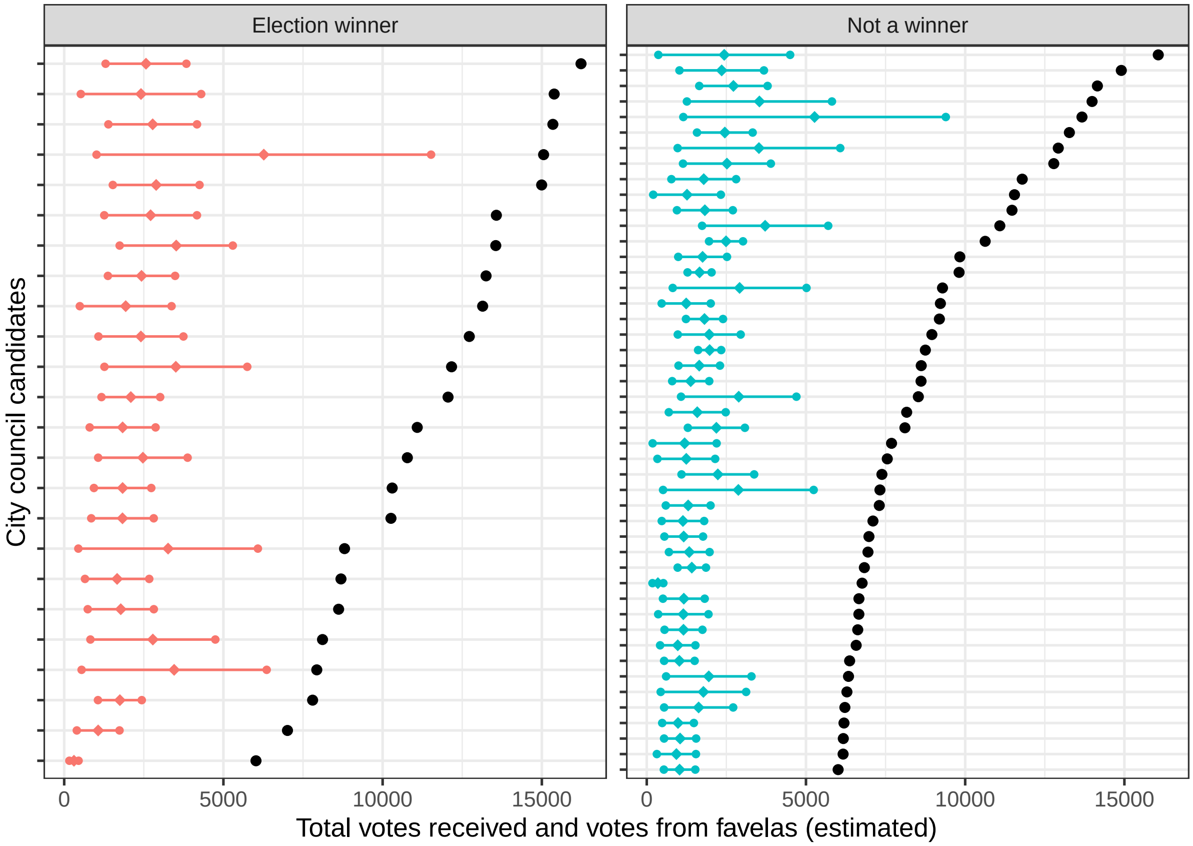
Sections 6.1 and 6.2 establish a strong inverse relationship between criminal control and electoral competitiveness at the voting-booth-level. What is the impact of criminal governance on elections beyond the voting booth? This section considers how deal-striking with criminal groups could affect candidates' probability of victory, especially for marginal candidates who are likely to benefit the most from gatekeeping and corralling.

It is challenging to simply calculate how many votes candidates get from different neighborhoods, due to the voter assignment procedure. Even candidates are relatively uninformed about where their campaign efforts are most effective; they general know which voting booths they received votes from, but don't know if a booth's voters are predominately *favela* or asphalt residents. A campaign manager admitted that this geographic fuzziness made it too difficult for their data analytics team to observe campaign performance at the neighborhood level.[19] Despite these data limitations, I calculate three estimates of the votes that candidates received from criminally governed *favela* residents, using the 2016 municipal election as a test case:

1. As a lower bound, I multiply *ShareFavela* by the total votes that a candidate received

19. Author's interview, Campaign manager 1, September 14, 2018.

Figure 6: *Favela* Votes as a Share of Candidates' Total Votes, 2016



Note: Points on the right side indicate the total votes earned for 71 marginal candidates in the 2016 city council election. The three points between the errorbars represent the estimated upper, lower, and mean *favela* votes received. Candidate identifiers are removed and sorted by most votes received to least.

in a voting booth. This is a lower bound because it assumes that *favela* and non-*favela* voters are uniformly likely to cast their ballot for any vote-receiving candidate, and thus is likely undercounting the votes that leading candidates win in voting booths with middling to high shares of *favela* residents.

2. As an upper bound, I assume that all of a candidate's votes are from *favela* residents if a) they receive fewer votes than total *favela* residents at the voting booth and b) the share of votes they receive closely approximates *ShareFavela* (within 0.05 percentage points). For example, in a voting booth with 90 *favela* residents where the most-voted candidate receives 70 votes, I assume that the candidate's votes are coming from *favela* residents. This is an upper bound because it is likely attributing certain candidates'

asphalt votes to *favela* votes.

3. I calculate the mean of the two above measures.

Figure 6 plots the total votes received for 71 marginal candidates (24 winning and 47 losing candidates) alongside these three estimates. I define a candidate as *marginal* if they received anywhere between 16,500 and 6,000 votes in the 2016 city council election. The bounds of this interval are rounded just above the highest-earning losing candidate (16,064 votes) and just below the lowest-earning winning candidate (6,023 votes). I estimate that marginal candidates receive 21% of their votes from criminally governed *favela* residents, with an average upper and lower bound of 33% and 9%, respectively.

I then estimate back-of-the-envelope calculations of whether the *favela* votes were decisive in securing victory for the marginal winners. The open list system complicates any analysis of hypothetical election outcomes, but I draw some aggregate conclusions about how pivotal *favela* voters were, using the third estimate to approximate the average number of *favela* votes. First, seven winners definitively would not have won without *favela* votes (14% of city council), and the absence or redistribution of these votes to other candidates would have caused them to lose. Second, seven other winners who won by even narrower margins made it over the line due to 2,000 or fewer *favela* votes, and a partial redistribution of their *favela* votes – or even a change in the total votes received by their party – would have cost them the race.

In sum, slightly more than a quarter (14 seats, 27%) of city council winners' seats could easily be lost without the support of all or some *favela* voters, often at margins so thin that n = 2,000 votes could make the difference between winning and losing. These 14 candidates who eked out a victory were also more likely to be complained about by voters for connections with criminal groups. In one egregious case, voters complained about a candidate 53 times; nearly all complaints mentioned the use of criminal brokers.

7 Mechanisms

I support my quantitative results with two types of qualitative evidence that further interrogate how corralling and gatekeeping affect electoral outcomes. Over the course of 18 months, I conducted fieldwork during and after two election cycles in Rio de Janeiro to establish the plausibility of each mechanism. I conducted more than 50 semi-structured interviews with candidates, staffers, and *favela* leaders, and obtained access to anonymous voter complaints about electoral irregularities, such as vote-buying, electoral violence, and abuse of public office.[20] If the above results hold, then candidates and staffers should acknowledge that deal-making occurs, and citizens should notice corralling and gatekeeping in their communities.

7.1 Gatekeeping

Gatekeeping affects all candidates, even those not striking deals with criminal groups. It impedes their ability to enter and campaign in certain *favelas*, absent criminal-candidate bargains. In my 37 structured interviews with candidates, every candidate acknowledged that criminal governance was a widespread obstacle for candidates seeking legislative office. Twelve candidates recounted experiences of being told they could not campaign somewhere or were asked to leave *favelas* where they did not have prior arrangements with the criminal leaders.

On the other hand, candidates and staffers acknowledge that gatekeeping increases the chances of winning. Four candidates referred to gatekeeping using an idiom that means "closing a deal with a specific person,"[21] indicating mutual understanding about the *exclusivity* that gatekeeping conferred. Many candidates who hired criminal brokers were confident that they would be the only ones campaigning in the area. When asked why criminal groups would respect the deal's terms, one candidate responded, "If I win, I'll send the cops in and destroy their business. They know not to violate the deal."[22]

When gatekeeping occurs, we should expect voters to have an abundance of information about the criminal group's preferred candidate and little information about rival candidates. Anonymous complaints from voters support this logic. One voter confirms that candidates are either denied or granted access to *favelas*, explaining,

> "Here in *FAVELA*, they are not letting any other candidate that is not CANDIDATE work here. He made a deal with the local traffickers, other candidates that want to work here have to pay 15 to 20 thousand reais... People that want to work in the neighborhood for another candidate can't if they don't pay the traffickers."[23]

20. I obtained the anonymous complaints from the TRE-RJ, the Regional Electoral Tribunal of the State of Rio de Janeiro. See Appendix A4 for a discussion of the ethics of both of these sources.

21. In Portuguese, *fechar com (alguem)*.

22. Author's interview, Candidate 4, December 14, 2019.

23. Voter complaint 3, September 2018.

A voter shares how gatekeeping obstructs voters from learning about other candidates, stating,

> "Cars with other candidate's decals are publicized and prohibited from moving about NEIGHBORHOOD. CANDIDATES A and B struck a deal with the traffickers from NEIGHBORHOOD, buying the entire community, prohibiting posters, decals, pamphlets, and even activity involving other candidates on residents' social media."[24]

7.2 Corralling

Candidates acknowledge that corralling can affect vote choice. A campaign manager claimed that lower uncertainty was part of what makes hiring criminal groups appealing. This candidate said that "you can count on having those votes"[25] delivered by a criminal group, but the same assurance was not true for other strategies, even other vote-buying strategies. Candidates were more inclined to talk about corralling as a persuasive act rather than a coercive act; eight candidates mentioned how criminal groups pass along benefits or coordinate campaign events for voters. One staffer admitted that the vote yield was especially high when criminal groups used coercive tactics.[26]

Voters notice when criminal groups use corralling. First, voters have extensively documented coercive corralling, both from drug trafficking organizations and from *milícia* groups. *Favela* residents note that "traffickers from FACTION are threatening that if CANDIDATE doesn't do well in the polls, the community will suffer retaliation."[27] One frightened resident writes,

> "The candidate is allied with the traffickers in *FAVELA*. They have signs and banners in their name, and community members are forced to hang signs up and publicly declare their support or suffer the consequences..."[28]

Voters also complain about corralling rooted in persuasive tactics:

> "This candidate is offering parcels of land with the support of the local traffickers today until 5pm. She has the help of a woman named NAME for the distribution

24. Voter complaint 4, October 2020.
25. Author's interview, Candidate 1, February 24, 2021.
26. Author's interview, Staffer 1, July 2018.
27. Voter complaint 5, October 2020.
28. Voter complaint 6, October 2018.

of the land. The land was invaded by traffickers and now, in support of CANDIDATE, they are distributing the land in exchange for votes, demanding that the voter take a photo inside the voting booth in order to claim their lot."[29]

Favela residents notice criminal groups' dual use of coercion and persuasion, complaining, "CANDIDATE, in addition to offering money to buy votes, threatens residents that if he discovers that they did not vote, they will suffer the consequences. CANDIDATE is involved with the *milícia*."[30]

Finally, gatekeeping and corralling are not necessarily mutually exclusive. The two acts reinforce each other; when gatekeeping reduces the number of candidates that can access voters, it facilitates election-day corralling for the criminal group's preferred candidate. Voters complain about how the two are used in tandem; one writes,

> "Today when I went to vote, I was coerced by *milícias* to vote for CANDIDATE. Surrounding the polling station there were lots of flyers and stickers for him, at the entrance there were many spread over the floor. He was the only one who came to campaign in my neighborhood (a *milícia*-dominated area), and lots of *milícia* members were distributing pamphlets for him."[31]

8 Conclusion

Politicians all over the world call on criminal groups to help win elections. Criminal groups that govern can effectively marshal their territorial control to influence voters, which candidates seek. This paper argues that politicians hire criminal groups as brokers, paying for their capacity to deliver votes by *gatekeeping* rival candidates and *corralling* voters. I offer empirical evidence of both gatekeeping and corralling, showing that fewer candidates get votes and the winner leads by a higher margin in criminally governed territory. This paper also demonstrates that the effect of criminal governance on voting is distinct from the broader effect of poverty and marginalization on voting.

My paper reveals how criminal groups derive power from *border* and *internal* territorial control, and use it to gatekeep and corral. Past research has argued that criminal groups that want to influence electoral outcomes strategically use violence or bribes to sway voters (Albarracín 2018) or assassinate rival candidates (Trejo and Ley 2020; Dell 2015). But

29. Voter complaint 7, October 2020.
30. Voter complaint 8, November 2020.
31. Voter complaint 9, November 2020.

this is only part of the story. I demonstrate how criminal groups are so effectively able to affect electoral outcomes. Criminal groups leverage their control over borders to gatekeep, restricting access to voters to only their chosen candidates. By impeding access for candidates who do not pay up, gatekeeping makes it more challenging for voters to learn about them, effectively pruning them from the candidate pool. Criminal groups corral by issuing threats and rewards to voters that are rooted in their internal territorial control of neighborhoods. Future research should investigate the medium- and longer-term consequences of these electioneering tactics, and which candidates are more likely to be interested in using them.

Rio de Janeiro is a microcosm in which we can observe criminal governance and candidate competition at the sub-municipal level. While this paper focuses on local dynamics, the argument can be used to generate predictions about the relationship between criminal governance and electoral politics in other contexts. On the criminal side, I highlight the importance of governance provision, not just criminal presence. My argument applies to situations in which a criminal group maintains order over voters, regardless of their criminal industry or the extent of territory they control. The importance of governance provision explains why we observe criminal groups acting as brokers in diverse contexts, including Colombia (Acemoglu et al. 2013), Sicily (Gambetta 1996), and Taiwan (Chin 2003).

In the developing world, where millions of people live under criminal rule (26 million in Latin America alone),[32] my overall results shed light on the implications of criminal governance for electoral politics and fighting crime. While the dominant approach to fighting crime overwhelmingly focuses on policing, these strategies have mixed success. Yet relying on the police as the dominant crime-fighting strategy seems one-dimensional, especially given the prevalence of symbiotic relationships between the state and organized crime (Barnes 2017). How can the government be expected to fight crime if individual policymakers – often those who write the rules – benefit electorally from criminal connections?

When candidates hire criminal brokers, it creates an agency dilemma in which politicians may want to weaken organized crime broadly, but not necessarily in the neighborhoods that vote for them. This tension makes it difficult to fight crime effectively and get crime out of politics. My paper provides a theory that helps understand how much politicians can gain from partnering with criminal groups, and encourages skepticism about crime-fighting strategies that only focus on law enforcement instead of the broader governance challenges

32. Preliminary estimates of the Latin American population under criminal control are from Uribe et al. (2022).

associated with criminal rule.

My conclusions inform our understanding of elections and democracy in contexts where criminal groups govern. When criminal groups wield such control over voters, it is not surprising that enterprising politicians will be eager to capitalize on this influence at the ballot box.

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Appendix

Table of Contents

A1 Criminal Governance Database

Subsection A1.1 describes the dataset construction. Subsection A1.2 discusses measurement of criminal governance and provides sample images of the blog.

A1.1 Data Construction

The final dataset measures criminal governance at the *favela*-month level across *favelas* in the city of Rio de Janeiro from January 2015 to January 2020. I compile this dataset in the following steps.

1. I scraped articles from the blog titled *Crimes News RJ*, a Portuguese-language blog written by an anonymous author. The blog reports on criminal group conflict, turf wars, conflict with the police, and occasionally provides longer biographic information

about top drug traffickers in the metropolitan area of Rio de Janeiro. Though the blog's author is anonymous, it is widely recognized as a credible source and receives numerous tips through the website or its' social media channels. I first heard about the blog while interviewing state-level officials in the Ministry of Public Security in 2016. The officers praised the blog for accurately reporting criminal presence in different neighborhoods and claimed that the information written was consistent with the classified intelligence information used by the police.

2. I process the text data in the blog posts by noting a common pattern that the author used when referring to a criminally governed *favela*. Following the name of the *favela*, the author would write the name or abbreviation of the criminal faction in parentheses. I used this writing convention as a heuristic to begin identifying each *favela*-gang pair. The author used six abbreviations to denote different types of criminal governance: three for the drug traffickers (**(ADA)**, **(CV)**, **(TCP)**) and three for the *milícias* that the author used interchangeably (**(ML)**, **(MIL)**, **(MILICIA)**). I used these six faction identifiers to create a new dataframe with the date, faction, and the four words prior to the first parentheses, which contains the name of the *favela*.

3. I then obtain the geographic boundaries of the city's favelas from the Pereira Passos Institute's (IPP) annual registry of favelas.[33] Every year, IPP publishes an updated shapefile with the legal names of all favelas in the municipal registry, along with some other data about favela population and size.

4. Using the official list of favela names from IPP as a starting place, I construct a dictionary of favela names, alternative names, and nicknames. The purpose of this list is to match the faction-favela tokens from step 2 to the official favela names, because the blog's author does not always use the official favela name when writing.

   There are many variations of names or nicknames that make exact string matching challenging. For example, there is a *favela* named *Morro São João* (São João hill) and a different one named *Comunidade São João*, but residents of Rio de Janeiro may refer to either or both just as *São João*. Even more complicated are the unofficial nicknames that residents, including the blog's author, use that deviate from the official

33. IPP is the city of Rio de Janeiro's municipal planning and data collection agency. The favela shapefiles are published on a near-annual basis, available at https://www.data.rio/. IPP pulls the underlying data from *SABREN*, the Low Income Settlements System, which is an official administrative record of favela names and populations.

name. For example, there is a large *favela* named *Juramento* in the city of Rio de Janeiro. Colloquially, the small *favela* buttressing *Juramento* is called *Juramentinho* (little Juramento), but the official name registered with IPP is *Parque Nova Maracá*. I expand on IPP's list of official *favela* names to build a dictionary of names and alternative names that account for colloquialisms, nicknames, and spelling errors.

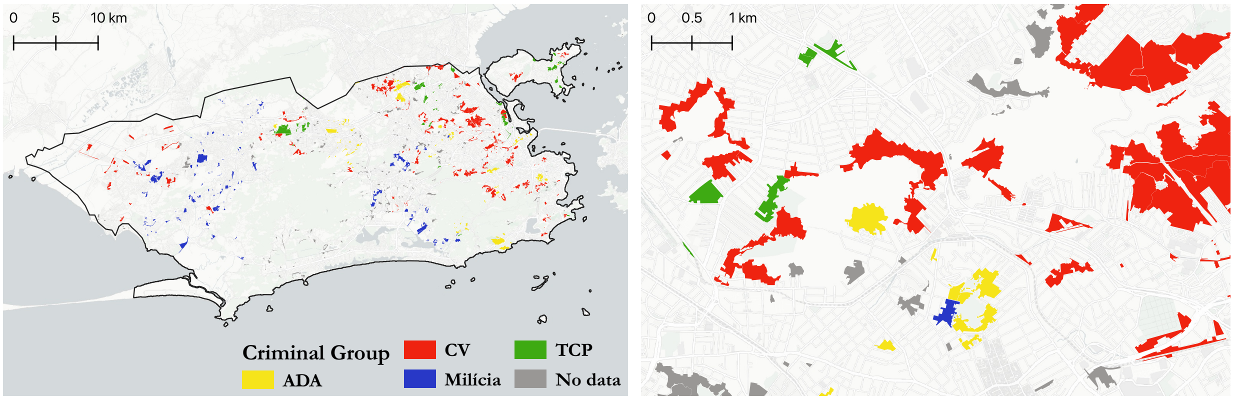
I also build dictionaries to identify smaller *favela*s by their complexes. There are several complexes of *favela*s across the city, which are large agglomerations of several side-by-side *favela*s that are commonly (but not always) under the same criminal rule. A famous example is *Complexo do Alemão* (German complex), which is a complex of nearly a dozen *favela*s, including the complex's nucleus, *Morro do Alemão* (German hill). This is useful for identifying the minor *favela*s that may not appear individually in the blog, but whose criminal governance can be deduced by the greater complex they pertain to.

5. Once the construction of the dictionary is complete, I match each date-faction-*favela* pair from Step 2 with the dictionary of *favela* names, alternative names, and complexes. I hand-coded all of the blog post entries that I was not able to match using official names or alternative names in my dictionaries.

6. Lastly, I aggregate the data in *favela*-month format. For any given *favela*-month, if the favela was written about, it appears in the database alongside the governing faction. In the vast majority of cases, there was only one governing faction reported per *favela*-month period. However, if and when a *favela* takeover was successful, the blog author begins referring to the *favela*'s new criminal faction in the parenthetical reference. In the case of a takeover, I classify the *favela*-month observation according to the new criminal group in power and, separately, denote that a turnover happened during this *favela*-month.

   I impute criminal governance when *favela*s are not discussed every month in the blog but appear to maintain the same governing faction. For example, if a *favela* entered into the database for the first time in February 2015, is not written about again until August 2015, but the faction remains the same in August as it was in February, I code March 2015 through July 2015 as that same faction. I believe the risk of error is low in this case, because a criminal turnover and leadership scrabble is precisely the type of information that would be written about in the blog. I think it is most likely that *favela*s are not written about when business is as usual.

Figure A1.1: Criminal Governance in Rio de Janeiro's *favelas*



Note: The map in the left panel shows the entire city of Rio de Janeiro with *favelas* colored according to the governing criminal group in October 2016. The right panel displays a zoomed-in section of the North Zone of the city where several different criminal groups govern *favelas*. I use an OpenStreetMap base map in both figures. Source: author's elaboration and IPP.

For each *favela*-month, I denote the governing faction or impute it, according to these coding rules.As described in the main body of the text, Section 4.1, the database covers 77% of favelas and 93% of the favela population. Figure A1.1 shows a cross-section of the database.

A1.2 The *Crimes News RJ* Blog as a Measure of Criminal Governance

There are a few reasons why the blog's designation captures the underlying variable of *criminal governance* rather than presence alone.[34] The author's thick qualitative description of the criminal groups vis-a-vis their communities often highlights the criminal groups' rule-making authority and the restrictions they pose on residents' behavior, using the words

34. I am not the first person to try to systematically measure criminal governance in Rio de Janeiro. Anthropologists paved the way (most notably, Alba Zaluar of the *Núcleo de Pesquisas das Violências*, NUPEVI), painstakingly collecting this data at specific cross-sections, sometimes by visiting each *favela* in person to correctly identify which faction governs it. More recently, the data journalism organization *Fogo Cruzado* has published a 2019 cross-section, and Magaloni, Franco-Vivanco, et al. (2020) published a database focused on *favelas* that were a part of the UPP public security program, both drawing from *Disque Denuncia* anonymous help line tips.

Figure A1.2: Sample Blog Posts with *Favela* and Faction Identifiers

Note: Figures show sample blog posts with titles, cover photo, and the first few sentences of the post. The faction identifiers **(CV)** and **(TCP)** are shown, preceded by the names of the *favelas*.

"boss" or "owner" to describe the criminal group and "controls," "leads," or "dominates" to describe their actions. Given this context, I interpret the author's notation as an indicator of criminal governance and the group's ability to control, going beyond a mere marker of their presence. That being said, criminal groups have varying abilities or desires to govern. This measure is consistent with Lessing's (2020) definition of *criminal governance*, which allows for variation in the extent to which the groups govern.

Figure A1.2 shows sample blog posts from *Crimes News RJ* discussing the governing groups. The title and subject of the article on the left is "Police exchange gunshots and apprehend 3 in Pedreira", and "Trafficking leaders in Maré collect rents within the trafficking hierarchy" on the right. Faction identifiers are shown parenthetically in the text preceded by the *favela* name, as described in Section A1.1.

A2 Electoral Assignment

This section describes the exogenous voting booth assignment procedure. I leverage as-if random assignment to voting booths to calculate the share of *favela* residents that vote in each booth, containing approximately 300-500 voters. While the assignment process in Brazil is territory-based, a person's residential address is deterministic but not predictive of their voting booth. Instead, as summarized in Section 4.2 of the main text, voters first choose their polling place within their electoral zone.[35] Polling places contain multiple voting booths.

Once voters have a polling station (a primary school, community center, etc.), the assignment process proceeds in the following way. Each year when new voters are added to the voter list their information enters a federal system, which assigns the voter to a voting booth *within their selected polling station*. Voting booth assignment stays constant year-to-year unless the voter moves and changes their registration address or is removed from the voter list. The software used prioritizes balance across voting booths, aiming for voting booths within the polling station to have more or less the same number of voters. When existing voting booths are approaching capacity, new ones are created. One very common consequence of this assignment process is that teenage new voters are assigned to different voting booths than their parents, despite living under the same roof.

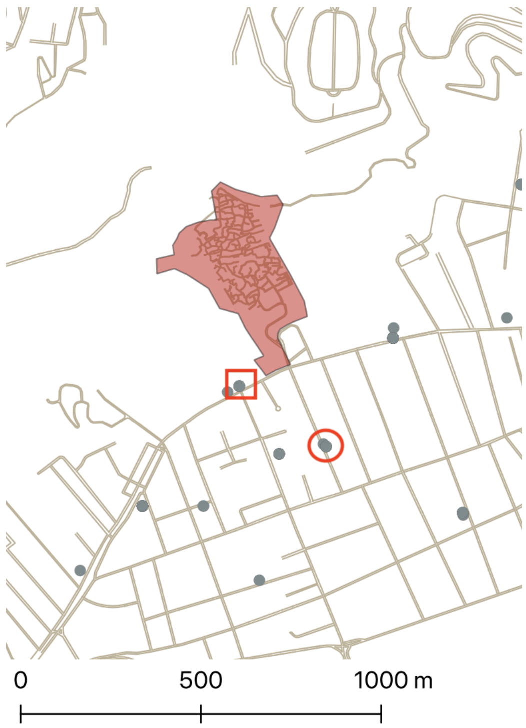
The residential address-to-voting booth assignment problem is particularly thorny for voters that live in *favelas*. Common polling station locations are schools, banks, or other government buildings that have high occupancy levels, comply with accessibility building codes, and can provide sufficient privacy to each precinct. Often, *favelas* do not have a building that complies with these regulations, or if they do, it is not large enough to accommodate all *favela* residents. The combination of poor building quality in *favelas* and the high population density means that *favela* residents often need to descend the hill and vote in the closest polling station to the *favela* boundary, where non-*favela* low- or middle-class residents vote. Empirically, this is problematic for measuring how *favela* residents vote because one does not know which polling stations they travel to in order to cast a ballot or how integrated *favela* residents are with other voters.

Figure A2.1 illustrates this problem in the case of the *Santa Marta favela*. *Santa Marta* is a *favela* in the middle of the wealthy South Zone of the city, and *Santa Marta* is a wealthy *favela*, relative to others. Despite this, there is not a single polling station inside of *Santa*

35. In cities, electoral zones encompass thousands of people across several neighborhoods. They are territory-based.

Marta. However, there are several polling stations within 500, even 200 meters of the *favela* boundaries. There is no public information about which polling station(s) *Santa Marta* residents are more likely to vote at versus which ones their wealthy neighbors are more likely to vote at, which is why all previous research attempting to map *favelas* onto voting outcomes have followed the same method: create an indicator variable for whether or not a polling station falls within a *favela* buffer zone, ranging from 250 to 1,000 meters outside the perimeter of the *favela* (Hidalgo and Lessing 2019; Nascimento 2017). In the example shown in Figure A2.1, all polling stations (and ballot boxes within polling stations) would take on a value of "1" for being within a 1,000 meter buffer zone of a *favela*.

Figure A2.1: Polling Stations near *Santa Marta Favela*



Note: Street map and *Santa Marta favela* boundaries from IPP data. Polling station locations from TRE-RJ. The circled polling station is *Escola México*, where a majority of *Santa Marta* residents vote. The polling station with a square drawn around it is *Escola Alemão,* the closest polling station to *Santa Marta* but where few *favela* residents vote.

My data reveals that there are some voting booths in this 1,000-meter range that only contain 6% or fewer *favela* voters. The primary school *Escola México*, the polling place where a majority of *Santa Marta* residents vote, is circled on the map in Figure A2.1. But between the seven ballot boxes inside *Escola México*, one is nearly exclusively *Santa Marta* residents (75% *favela* voters) while one is a mix of *Santa Marta* residents and the nearby middle class (32% *favela* residents). In the polling station closest to *Santa Marta* with a square drawn around it, *Escola Alemão*, *Santa Marta favela* residents make up a maximum of 6% of voters in each of the 14 voting booths.

A3 Description of Control Variables

A3.1 *Favela*-level Variables

All *favela*-level variables control for the most-represented *favela* at the voting booth. If there are residents from multiple *favelas* that vote at the same booth (and there often are), the *favela* controlled for is the one with the highest share of voters at the booth. This coding logic also applies to the *CrimGov* or other faction-level variables constructed from the criminal governance database.

1. **Distance**: This is the logged distance (in meters) from the voting booth to the boundary of the most-represented *favela* at the booth. The *favela* boundaries come from a shapefile provided by *Instituto Pereira Passos*, and the polling station addresses from TRE-RJ and geocoded by the author. Distances calculated by the author in QGIS.

2. **Population**: This is the logged population of the most-represented *favela* at the voting booth. The *favela* population estimates come from SABREN estimates, obtained from *Instituto Pereira Passos*.

A3.2 Voting-booth-level Variables

All voting-booth-level variables control for population dynamics inside the voting booth that might be correlated with *favela* residency. The historical process that generates the exogenous imbalance in voting booth composition is shaped by the different population dynamics in the *favelas* compared to the asphalt. For instance, sudden population growth in a *favela* may lead to the creation of a new voting booth with a high proportion of *favela* residents. This ratio of *favela* to asphalt voters in the booth may persist over time, even as new asphalt and *favela* voters are being assigned to vote in that particular booth.

The *TSE* (Supreme Electoral Tribunal) publishes voting-booth-level statistics about the gender, age, education, and civil status of voters. I control for these demographic variables at the voting-booth-year-level in the main models:

1. **Age**: The *TSE* publishes the number of voters per ballot box of age 16, 17, and in binned categories of various sizes for ages 18 and up. For all categories, I take the mean value, then calculate the weighted mean age for the entire ballot box.

2. **Education**: The *TSE* publishes the number of voters per education category, ranging from "illiterate" to "college complete." I calculate the mean education level per voting

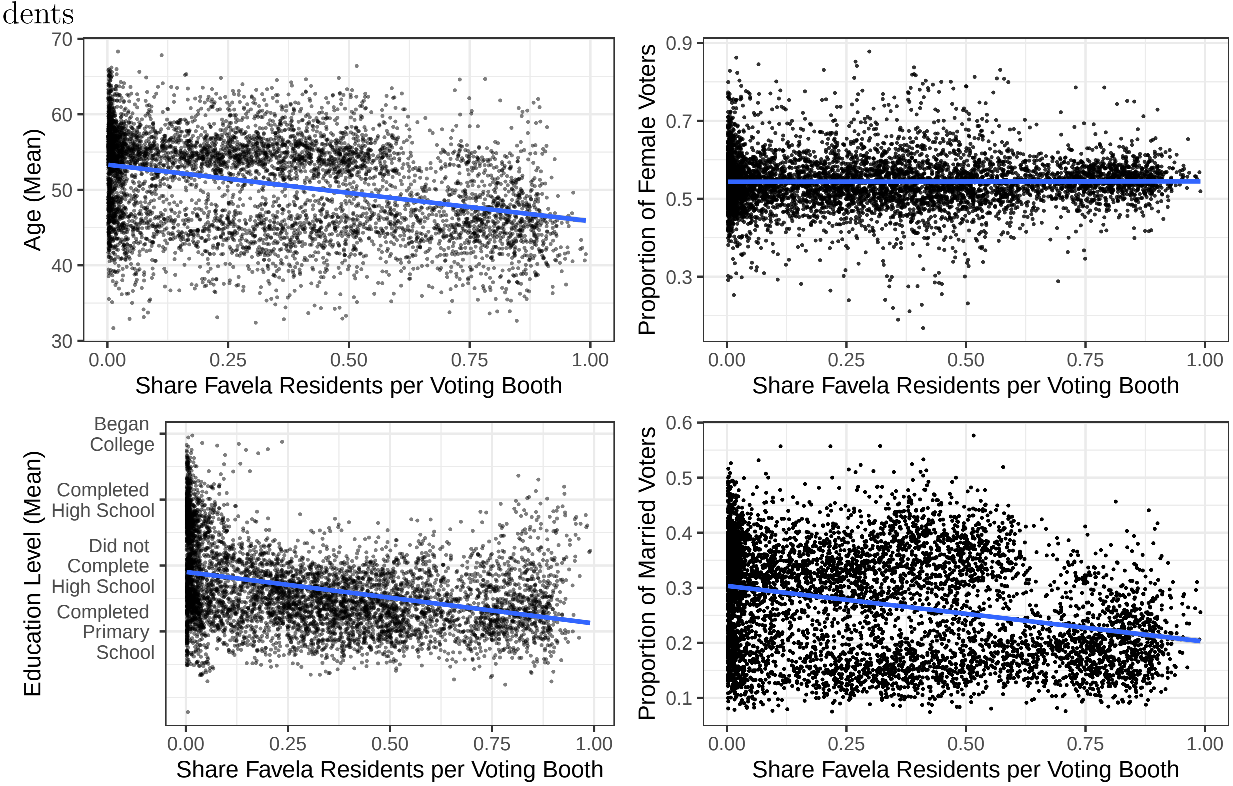
booth per year. The mean value for both main empirical strategies is an education level between 4 and 5, "primary school complete," and "high school started but incomplete," respectively.

3. **Percent women**: The *TSE* publishes the percent of voters per booth who identify as male and female. A negligible amount do not disclose or identify with the two above genders. I calculate the share of voters per ballot-box-year who identify as female.

4. **Percent married**: The *TSE* publishes the percent of voters in several civil status categories: married, divorced, single, widowed, and legally separated. Fewer than 5% of the sample is divorced, widowed, or legally separated; the vast majority is either single or married. I calculate and report the percent of voters per ballot-box-year who are married.

Figure A3.1 presents these four demographic variables at the voting booth level, as the exogenous variable, *ShareFavela*, increases. It shows that *favela* residents are slightly younger, slightly less likely to be married, and have slightly less education.

Figure A3.1: Demographic Characteristics of Voting Booths, by the Share of *Favela* Residents



A4 Research Ethics

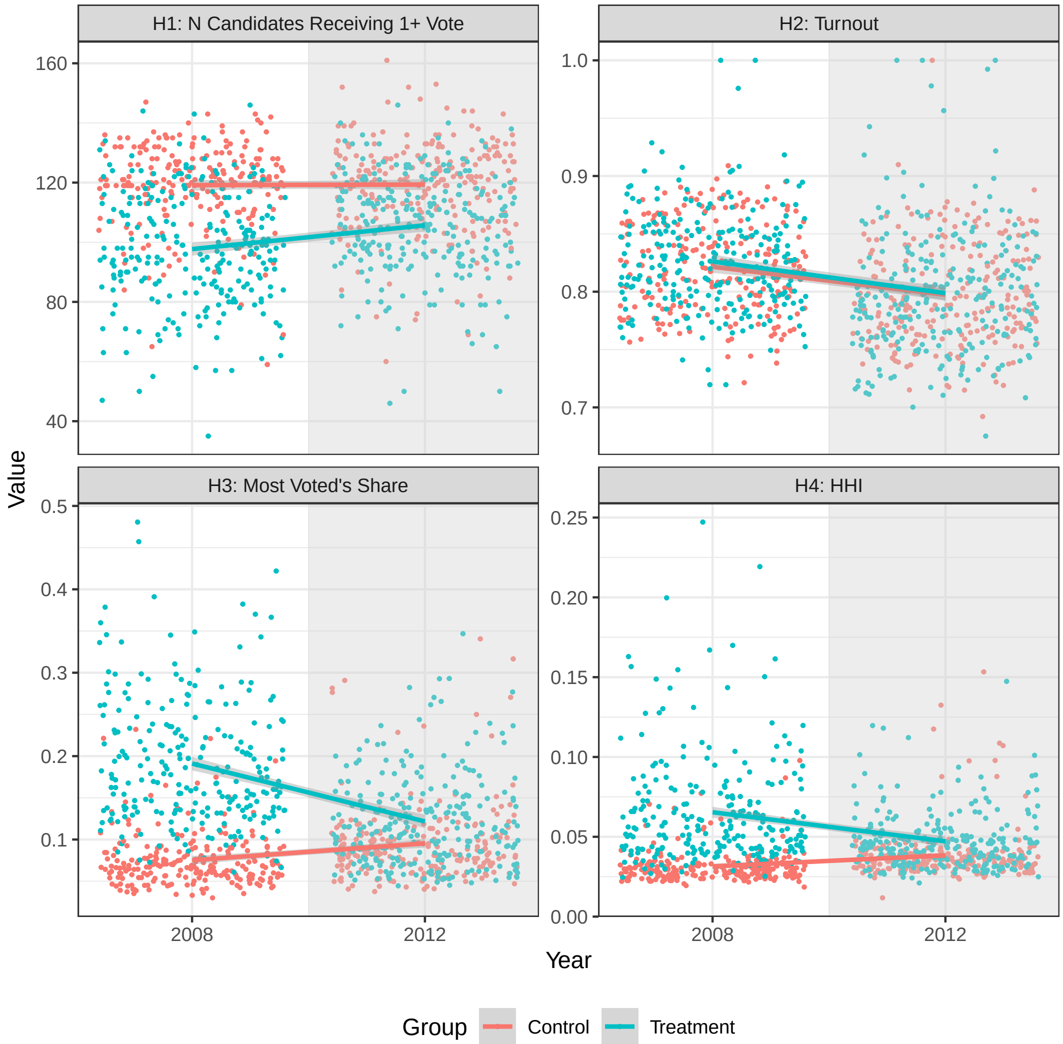
I confirm that the human subjects research pursued complies with the *Principles and Guidance for Human Subjects Research*. The human subjects evidence presented in this paper includes the semi-structured interviews with candidates, staffers, community leaders, and favela residents. The research was approved and designated exempt by the relevant review board at the author's institution.

Interviews. All interviews were conducted in person by the author in the local language. Verbal consent was sought and obtained before the interview began, and there was no deception involved. To further protect confidentiality, interviews were not tape-recorded, the author just took written notes. All identities are anonymized. Respondents were informed of this and were reminded that they did not have to answer all questions.

Voter complaints. An additional data source used in this project is drawn from TRE-RJ (Regional Electoral Tribunal of Rio de Janeiro) voter complaints about electoral irregularities. This qualitative data source is not human subjects data, as all complaints are anonymous and there is no personally identifying information that could link the complainant to a specific person. Despite this, the complaints contain sensitive information and should be treated with care. All complaints contain the name of rule-breaking candidates and neighborhoods where the offense was committed. To protect residents of these communities from retaliation at the group-level, I have removed all candidate- and neighborhood-level details from the complaints. Complaints printed in the paper's text contain the translated text with *ALL-CAPS* placeholders for generic candidate or location names.

A5 Additional Tables and Figures

Figure A5.1: Parallel Trends



Note: Each panel shows one of four outcome variables for the *favelas* raided immediately before the election (treatment) or after (control). I plot unadjusted outcome variables for each voting booth-year for the two time periods considered in the difference-in-differences analysis. Points are jittered for each election year, and each sample mean is plotted with a 95% confidence interval.

Table A5.1: Robustness: Natural Experiment Results by Year

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				2008				
$ShareFavela \times$	-38.7*	-27.7	0.088	0.040	0.069*	0.037	0.052**	0.036**
$CrimGov$	(21.1)	(17.4)	(0.146)	(0.169)	(0.040)	(0.036)	(0.020)	(0.018)
Observations	5,799	5,785	5,799	5,785	5,799	5,785	5,799	5,785
R^2	0.72047	0.73938	0.59194	0.76808	0.90995	0.91656	0.89762	0.90743
				2012				
$ShareFavela \times$	-23.3	-16.5	0.062	0.044	0.063**	0.041	0.036***	0.028***
$CrimGov$	(18.8)	(21.6)	(0.139)	(0.172)	(0.026)	(0.025)	(0.010)	(0.009)
Observations	6,213	6,144	6,213	6,144	6,213	6,144	6,213	6,144
R^2	0.68315	0.69779	0.58335	0.76641	0.91560	0.91884	0.91874	0.92256
				2016				
$ShareFavela \times$	-9.87	-13.3	-0.0003	-0.061	0.053*	0.039	0.018	0.009
$CrimGov.$	(20.0)	(25.0)	(0.145)	(0.177)	(0.028)	(0.027)	(0.015)	(0.013)
Observations	6,618	6,556	6,618	6,556	6,618	6,556	6,618	6,556
R^2	0.66580	0.69090	0.55533	0.73428	0.92832	0.93002	0.92020	0.92326
				2020				
$ShareFavela$	-25.5**	-28.4**	-0.085	-0.110*	0.022	0.019	0.011	0.010
$CrimGov.$	(11.7)	(12.0)	(0.068)	(0.062)	(0.019)	(0.019)	(0.007)	(0.007)
Observations	10,550	10,388	10,550	10,388	10,550	10,388	10,550	10,388
R^2	0.71022	0.75196	0.64434	0.73607	0.93351	0.93226	0.92444	0.92215
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Models 2, 4, 6, and 8 include polling station and *favela* fixed effects as well as the *favela*-level and voting-booth-level controls in Table 2. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.2: Robustness: Natural Experiment, Only Favelas Within 500 Meters of Polling Station

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$ShareFavela \times$	-26.0*	-18.8	0.234	0.240	0.043	0.033	0.019*	0.016*
$CrimGov$	(14.6)	(15.3)	(0.166)	(0.175)	(0.026)	(0.025)	(0.010)	(0.009)
$ShareFavela$	-11.6	-25.0**	-0.133	-0.256	0.018	0.016	0.020***	0.016***
	(11.7)	(11.4)	(0.160)	(0.173)	(0.020)	(0.020)	(0.007)	(0.006)
Distance		-5.23***		-0.022***		0.023***		0.013***
		(0.704)		(0.003)		(0.002)		(0.0005)
Age		0.049		-0.004***		-0.001***		-0.0003***
		(0.158)		(0.0005)		(0.0003)		(0.0001)
Education		9.10***		0.029***		-0.011**		-0.003*
		(1.84)		(0.005)		(0.005)		(0.002)
% Women		-6.60***		0.020*		0.023***		0.004*
		(2.32)		(0.012)		(0.007)		(0.002)
% Married		-18.6***		-0.135***		0.017		-0.005
		(6.19)		(0.023)		(0.017)		(0.006)
Observations	6,094	6,049	6,094	6,049	6,094	6,049	6,094	6,049
R^2	0.564	0.584	0.702	0.792	0.581	0.583	0.563	0.58
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.3: Robustness: Natural Experiment, Dropping *CrimGov* Dummy Variable

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ShareFavela	-30.1***	-37.6***	0.097***	-0.023	0.078***	0.064***	0.045***	0.037***
	(4.28)	(4.59)	(0.020)	(0.019)	(0.016)	(0.015)	(0.008)	(0.007)
Distance		1.40***		0.003		-0.0003		-0.0005
		(0.412)		(0.002)		(0.0009)		(0.0003)
Population		-153.8		0.681		0.010		0.344
		(129,153.5)		(375.9)		(311.0)		(90.1)
Age		-0.187***		-0.004***		-0.001***		-0.0005***
		(0.068)		(0.0003)		(0.0002)		(6.96×10^{-5})
Education		6.48***		0.022***		-0.017***		-0.006***
		(0.812)		(0.003)		(0.003)		(0.001)
% Women		-11.7***		-0.002		0.025***		0.005***
		(1.19)		(0.005)		(0.003)		(0.001)
% Married		-8.69***		-0.119***		0.020**		0.0005
		(2.78)		(0.011)		(0.008)		(0.003)
Observations	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.569	0.587	0.698	0.772	0.644	0.643	0.647	0.645
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.4: Robustness: Natural Experiment, Dropping Interaction Term

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ShareFavela	-30.1***	-37.6***	0.097***	-0.023	0.078***	0.064***	0.045***	0.037***
	(4.28)	(4.59)	(0.020)	(0.019)	(0.016)	(0.015)	(0.008)	(0.007)
CrimGov	155.9	294.3	-0.861	-2.42	-0.381	-0.539	-0.716	-1.56
	(79,243.9)	(90,866.5)	(262.5)	(454.6)	(204.5)	(210.6)	(60.1)	(70.3)
Distance		1.40***		0.003		-0.0003		-0.0005
		(0.412)		(0.002)		(0.0009)		(0.0003)
Population		217.1		-1.78		-0.408		-1.11
		(149,971.5)		(605.7)		(305.8)		(103.2)
Age		-0.187***		-0.004***		-0.001***		-0.0005***
		(0.068)		(0.0003)		(0.0002)		(6.96×10^{-5})
Education		6.48***		0.022***		-0.017***		-0.006***
		(0.812)		(0.003)		(0.003)		(0.001)
% Women		-11.7***		-0.002		0.025***		0.005***
		(1.19)		(0.005)		(0.003)		(0.001)
% Married		-8.69***		-0.119***		0.020**		0.0005
		(2.78)		(0.011)		(0.008)		(0.003)
Observations	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.569	0.587	0.698	0.772	0.644	0.643	0.647	0.645
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.5: Robustness: Natural Experiment, Replacing *CrimGov* with Criminal Industry

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ShareFavela× DTO	-30.0*** (11.1)	-30.5** (12.7)	0.028 (0.096)	-0.021 (0.110)	0.056** (0.024)	0.038* (0.023)	0.030** (0.012)	0.022** (0.011)
ShareFavela× *Milícia*	-18.1 (11.2)	-16.9 (12.7)	-0.020 (0.096)	-0.040 (0.110)	0.037* (0.020)	0.028 (0.018)	0.024*** (0.009)	0.021** (0.009)
ShareFavela	-6.11 (10.1)	-13.7 (11.8)	0.084 (0.095)	0.001 (0.112)	0.032*** (0.012)	0.032*** (0.011)	0.020*** (0.006)	0.017*** (0.005)
DTO	1,062.7 (254,734.1)	3,327.7 (247,184.6)	-8.68 (1,353.2)	-14.1 (1,072.0)	-4.03 (638.7)	-0.464 (697.9)	-7.13 (212.8)	-6.60 (209.1)
Milícia	843.4 (168,533.5)	3,258.3 (178,083.8)	-6.89 (883.2)	-13.8 (768.0)	-3.22 (404.5)	-0.496 (518.1)	-5.68 (139.0)	-6.47 (158.7)
Distance		1.51*** (0.415)		0.003 (0.002)		-0.0005 (0.0009)		-0.0006 (0.0003)
Population		834.2 (145,919.6)		-3.55 (526.3)		-0.160 (328.1)		-1.63 (103.7)
Age		-0.199*** (0.067)		-0.004*** (0.0003)		-0.001*** (0.0002)		-0.0005*** (6.82×10^{-5}
Education		6.33*** (0.795)		0.022*** (0.003)		-0.017*** (0.003)		-0.006*** (0.001)
% Women		-11.8*** (1.19)		-0.002 (0.005)		0.025*** (0.003)		0.005*** (0.001)
% Married		-8.43*** (2.78)		-0.120*** (0.011)		0.020** (0.008)		0.0003 (0.003)
Observations	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.569	0.587	0.698	0.772	0.644	0.643	0.647	0.645
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: The criminal industry variable takes on a value of "DTO" for *favelas* governed by one of the three drug trafficking factions, "*Milícia*" for *milícia*-governed *favelas*, and the reference category is for *favelas* with no data from my criminal governance database. Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.6: Robustness: Natural Experiment, Replacing *CrimGov* with Faction

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ShareFavela× ADA	-40.4***	-47.1***	0.089	-0.037	-0.0003	-0.026	0.015*	0.0008
	(12.2)	(14.0)	(0.101)	(0.113)	(0.021)	(0.019)	(0.009)	(0.008)
ShareFavela× CV	-30.6**	-30.3**	0.013	-0.029	0.069**	0.050*	0.033**	0.026*
	(11.9)	(13.4)	(0.097)	(0.111)	(0.031)	(0.029)	(0.015)	(0.014)
ShareFavela× *Milícia*	-18.1	-17.0	-0.020	-0.040	0.037*	0.028	0.024***	0.021**
	(11.2)	(12.7)	(0.096)	(0.110)	(0.020)	(0.018)	(0.009)	(0.009)
ShareFavela× TCP	-17.7	-17.2	0.043	0.025	0.044	0.037	0.027**	0.024**
	(12.5)	(13.9)	(0.110)	(0.117)	(0.031)	(0.030)	(0.013)	(0.012)
ShareFavela	-6.07	-13.6	0.083	0.001	0.033***	0.033***	0.020***	0.017***
	(10.1)	(11.8)	(0.095)	(0.112)	(0.013)	(0.012)	(0.006)	(0.005)
ADA	159.4	454.9	-1.40	-1.86	-0.491	-0.050	-1.04	-0.894
	(14,301.9)	(15,021.6)	(59.5)	(53.9)	(38.1)	(30.8)	(14.6)	(11.6)
CV	163.6	457.8	-1.42	-1.89	-0.524	-0.084	-1.05	-0.910
	(14,302.0)	(15,021.6)	(59.5)	(53.9)	(38.1)	(30.8)	(14.6)	(11.6)
Milícia	-13.5	46.0	0.070	-0.183	-0.103	0.016	-0.076	-0.060
	(72,125.2)	(91,391.2)	(262.9)	(464.8)	(187.1)	(196.5)	(55.9)	(68.8)
TCP	159.8	453.9	-1.39	-1.87	-0.459	-0.020	-1.03	-0.889
	(14,301.9)	(15,021.6)	(59.5)	(53.9)	(38.1)	(30.8)	(14.6)	(11.6)
Distance		1.53***		0.003		-0.0008		-0.0006*
		(0.413)		(0.002)		(0.0009)		(0.0004)
Population		48.6		-0.205		0.010		-0.039
		(147,970.5)		(569.7)		(309.2)		(103.4)
Age		-0.203***		-0.004***		-0.001***		-0.0005***
		(0.067)		(0.0003)		(0.0002)		(6.77×10^{-5})
Education		6.30***		0.021***		-0.017***		-0.006***
		(0.790)		(0.003)		(0.003)		(0.001)
% Women		-11.7***		-0.002		0.025***		0.005***
		(1.19)		(0.005)		(0.003)		(0.001)
% Married		-8.42***		-0.119***		0.020**		0.0005
		(2.77)		(0.011)		(0.008)		(0.003)
Observations	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.570	0.587	0.699	0.772	0.646	0.645	0.648	0.647
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: The faction variable takes on a value of "ADA," "CV," or "TCP" for *favelas* governed by one of the three drug trafficking factions, "*Milícia*" for *milícia*-governed *favelas*, and the reference category is for *favelas* with no data from my criminal governance database. Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.7: Robustness: Alternate and Placebo Dependent Variables

	H1: N candidates winning votes				*H3: Vote Share*		*Placebo*	
	N candidates winning 2+ votes		N candidates winning 5+ votes		Share top 5 most voted		Share spoiled ballots	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$ShareFavela \times$ $CrimGov$	-4.96	-5.49	-0.389	-1.25	0.088***	0.064***	0.016	0.012
	(3.76)	(4.31)	(1.22)	(1.43)	(0.026)	(0.022)	(0.020)	(0.017)
$ShareFavela$	-2.46	-6.91*	-0.280	-1.68	0.062***	0.047***	0.032*	0.016
	(3.55)	(4.16)	(1.10)	(1.33)	(0.022)	(0.018)	(0.019)	(0.017)
$CrimGov$	14.3	257.5	5.66	72.2	0.082	1.66	0.989	2.50
	(46,080.8)	(46,063.3)	(19,670.4)	(19,812.4)	(290.2)	(300.2)	(116.4)	(177.6)
Distance		0.807***		0.200**		-0.0006		-0.0004
		(0.211)		(0.085)		(0.001)		(0.0009)
Population		190.4		53.3		1.21		1.84
		(78,851.7)		(32,579.8)		(425.7)		(229.1)
Age		-0.234***		-0.096***		-0.002***		-0.0002*
		(0.030)		(0.011)		(0.0002)		(0.0001)
Education		2.93***		0.432***		-0.015***		0.007***
		(0.347)		(0.144)		(0.003)		(0.002)
% Women		-1.38**		0.879***		0.006		-0.032***
		(0.562)		(0.225)		(0.004)		(0.003)
% Married		2.19*		-0.177		-0.009		-0.056***
		(1.28)		(0.509)		(0.009)		(0.006)
Observations	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.518	0.538	0.533	0.547	0.700	0.704	0.566	0.583
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Columns (1)-(4) test the first hypothesis using as a dependent variable the number of candidates winning at least 2 and 5 votes, respectively. Columns (5)-(6) test the third hypothesis using the vote share the top 5 candidates earn, and columns (7)-(8) test a placebo outcome, the share of spoiled ballots per ballot box. Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.8: Robustness: Natural Experiment, Controlling for Spoiled Ballots

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ShareFavela ×*CrimGov*	-26.5**	-26.2**	0.011	-0.027	0.056***	0.040**	0.028***	0.022***
	(10.7)	(12.2)	(0.095)	(0.109)	(0.020)	(0.017)	(0.009)	(0.008)
ShareFavela	-6.91	-14.0	0.080	0.002	0.045***	0.039***	0.020***	0.017***
	(10.3)	(11.8)	(0.095)	(0.112)	(0.015)	(0.012)	(0.006)	(0.005)
CrimGov	84.9	665.0	-1.02	-2.97	-0.023	0.940	-0.745	-1.36
	(80,118.6)	(91,035.1)	(263.6)	(454.7)	(216.6)	(209.0)	(59.4)	(70.3)
Spoiled	15.8***	-0.129	0.139***	-0.027*	-0.388***	-0.403***	0.014	0.009
	(4.47)	(4.62)	(0.016)	(0.014)	(0.025)	(0.025)	(0.013)	(0.014)
Distance		1.49***		0.003		-0.0006		-0.0005
		(0.408)		(0.002)		(0.0009)		(0.0003)
Population		488.5		-2.19		0.681		-0.963
		(150,895.1)		(604.6)		(309.5)		(103.7)
Age		-0.194***		-0.004***		-0.001***		-0.0005***
		(0.067)		(0.0003)		(0.0002)		(6.92×10^{-5})
Education		6.39***		0.022***		-0.014***		-0.006***
		(0.802)		(0.003)		(0.003)		(0.001)
% Women		-11.7***		-0.003		0.012***		0.005***
		(1.20)		(0.005)		(0.003)		(0.001)
% Married		-8.58***		-0.121***		-0.003		0.0009
		(2.77)		(0.011)		(0.008)		(0.003)
N	29,180	28,873	29,180	28,873	29,180	28,873	29,180	28,873
R^2	0.570	0.587	0.700	0.772	0.670	0.670	0.647	0.645
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: These models control for the share of spoiled ballots for each voting-booth-year. Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.9: Robustness: Natural Experiment, Dropping All *Favelas* Raided in the UPP Program

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$ShareFavela$ $\times CrimGov$	-21.9*	-22.8*	0.028	-0.018	0.024	0.015	0.020***	0.015**
	(11.3)	(13.1)	(0.102)	(0.117)	(0.017)	(0.015)	(0.007)	(0.007)
$ShareFavela$	-4.98	-13.7	0.093	0.006	0.030**	0.030**	0.018***	0.015***
	(10.7)	(12.7)	(0.102)	(0.120)	(0.013)	(0.012)	(0.006)	(0.005)
$CrimGov$	-0.044	0.475	0.013	0.670	0.009	0.416	-0.005	0.526
	(52,870.0)	(159,120.1)	(210.5)	(624.9)	(159.0)	(353.8)	(53.8)	(116.1)
Distance		1.68***		0.004*		-0.0007		-0.0006
		(0.476)		(0.002)		(0.001)		(0.0004)
Population		-1.08		-1.55		-1.05		-1.26
		(352,809.3)		(1,468.3)		(630.2)		(241.1)
Age		-0.181**		-0.003***		-0.0007***		-0.0004***
		(0.082)		(0.0003)		(0.0002)		(7.73×10^{-5})
Education		6.61***		0.026***		-0.013***		-0.004***
		(0.981)		(0.004)		(0.003)		(0.001)
% Women		-11.8***		0.002		0.030***		0.006***
		(1.43)		(0.006)		(0.003)		(0.001)
% Married		-9.27***		-0.127***		0.013		-0.001
		(3.28)		(0.013)		(0.010)		(0.004)
Observations	21,196	20,949	21,196	20,949	21,196	20,949	21,196	20,949
R^2	0.581	0.598	0.695	0.770	0.639	0.636	0.637	0.633
Station FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Favela FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: These models drop all voters in *favelas* ever raided by the UPP program. Voting booth observations are dropped when the majority of *favela* voters are from a *favela* raided by a UPP. Models 2, 4, 6, and 8 include polling station, yearly, and *favela* fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.10: Robustness: Difference-in-differences, Leave One *Favela* Out

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Dropping Manguinhos				
Raid × 2012	7.87***	7.90***	-0.004	-0.005	-0.100***	-0.100***	-0.026***	-0.026***
	(2.46)	(2.56)	(0.005)	(0.005)	(0.009)	(0.009)	(0.003)	(0.003)
N	711	702	711	702	711	702	711	702
R^2	0.265	0.682	0.226	0.583	0.572	0.804	0.397	0.740
				Dropping Jacarezinho				
Raid × 2012	7.73***	7.93***	-0.003	-0.004	-0.081***	-0.080***	-0.024***	-0.024***
	(2.21)	(2.28)	(0.004)	(0.005)	(0.010)	(0.011)	(0.004)	(0.004)
N	755	748	755	748	755	748	755	748
R^2	0.192	0.659	0.105	0.599	0.391	0.809	0.231	0.749
				Dropping Vila Cruzeiro				
Raid × 2012	8.91***	9.04***	-0.004	-0.005	-0.096***	-0.096***	-0.027***	-0.027***
	(2.22)	(2.26)	(0.004)	(0.005)	(0.009)	(0.009)	(0.003)	(0.003)
N	903	895	903	895	903	895	903	895
R^2	0.304	0.671	0.111	0.626	0.472	0.828	0.328	0.769
				Dropping Vila Proletária				
Raid × 2012	12.4***	12.7***	-0.008**	-0.009**	-0.102***	-0.103***	-0.028***	-0.028***
	(1.71)	(1.70)	(0.004)	(0.004)	(0.010)	(0.010)	(0.004)	(0.004)
N	797	786	797	786	797	786	797	786
R^2	0.350	0.729	0.128	0.656	0.413	0.797	0.273	0.767
				Dropping Rocinha				
Raid × 2012	-1.12	-1.48	0.004	0.004	-0.059***	-0.058***	-0.017***	-0.017***
	(1.71)	(1.78)	(0.006)	(0.007)	(0.008)	(0.008)	(0.003)	(0.003)
Observations	686	677	686	677	686	677	686	677
R^2	0.075	0.583	0.107	0.645	0.448	0.833	0.279	0.767
Station FE		✓		✓		✓		✓

Note: Models 2, 4, 6, and 8 include polling station fixed effects and all *favela*-level and voting-booth-level control variables shown in Table 3. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.11: Robustness: Difference-in-differences, All UPP *Favelas* in Control Group

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Raid × 2012	5.17**	4.81**	-0.002	-0.003	-0.077***	-0.077***	-0.023***	-0.023***
	(2.02)	(2.05)	(0.004)	(0.004)	(0.008)	(0.008)	(0.003)	(0.003)
Raid	-14.4***	-0.543	0.018***	0.013	0.102***	0.041***	0.030***	0.012***
	(3.33)	(2.44)	(0.005)	(0.010)	(0.011)	(0.013)	(0.005)	(0.004)
2012	2.81***	3.52***	-0.025***	-0.022***	0.008**	0.008***	0.004***	0.005***
	(0.558)	(0.672)	(0.0009)	(0.001)	(0.003)	(0.003)	(0.0009)	(0.0009)
$ShareFavela$	-6.17	-41.8***	0.126***	-0.029*	0.085***	0.123**	0.022***	0.061**
	(5.54)	(12.5)	(0.016)	(0.017)	(0.020)	(0.049)	(0.007)	(0.024)
Distance		1.77		-0.003		0.005		0.0008
		(2.46)		(0.004)		(0.004)		(0.002)
Population		-1.08		-0.002		-0.0005		-3.51×10^{-6}
		(0.803)		(0.002)		(0.002)		(0.0007)
Age		0.231		-0.004***		-0.002***		-0.0009***
		(0.209)		(0.0005)		(0.0004)		(0.0002)
Education		7.31***		0.013**		-0.020***		-0.007**
		(1.68)		(0.005)		(0.007)		(0.003)
% Women		-7.34***		0.012*		0.012**		0.001
		(2.56)		(0.007)		(0.005)		(0.002)
% Married		-13.8		-0.092***		0.036***		0.013***
		(8.74)		(0.018)		(0.013)		(0.005)
N	3,263	3,245	3,263	3,245	3,263	3,245	3,263	3,245
R^2	0.080	0.609	0.189	0.775	0.272	0.756	0.195	0.711
Station FE		✓		✓		✓		✓

Note: The control group includes *all favelas* to ever receive a UPP, either before or after the 2012 election. The treatment group remains the same. Models 2, 4, 6, and 8 include polling station fixed effects. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5.12: Robustness: Difference-in-differences with Continuous Treatment (Dose Response)

	Gatekeeping		*Corralling*				*Competition*	
	N candidates receiving votes		Turnout		Most voted's share		Vote concentration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dose× 2012	5.78 (6.92)	4.66 (6.98)	0.018 (0.018)	0.010 (0.022)	-0.261*** (0.048)	-0.261*** (0.048)	-0.089*** (0.018)	-0.090*** (0.017)
Dose	-46.0** (18.3)	-70.2*** (23.9)	0.119*** (0.021)	-0.035 (0.035)	0.486*** (0.071)	0.454*** (0.091)	0.155*** (0.030)	0.203*** (0.048)
2012	3.45*** (0.568)	4.13*** (0.693)	-0.026*** (0.0009)	-0.022*** (0.001)	0.003 (0.003)	0.003 (0.003)	0.003*** (0.0009)	0.004*** (0.0010)
Distance		1.18 (2.32)		-0.003 (0.004)		0.007* (0.004)		0.002 (0.001)
Population		-0.778 (0.815)		-0.002 (0.002)		-0.002 (0.001)		-0.0005 (0.0006)
Age		0.300 (0.220)		-0.004*** (0.0004)		-0.002*** (0.0003)		-0.0010*** (0.0002)
Education		8.45*** (1.64)		0.014** (0.006)		-0.021*** (0.006)		-0.008*** (0.003)
% Women		-6.86*** (2.55)		0.012 (0.007)		0.010* (0.006)		0.0001 (0.002)
% Married		-15.8* (9.09)		-0.094*** (0.018)		0.043*** (0.013)		0.017*** (0.005)
N	3,263	3,245	3,263	3,245	3,263	3,245	3,263	3,245
R^2	0.049	0.607	0.088	0.775	0.262	0.740	0.228	0.723
Station FE		✓		✓		✓		✓

Note: The continuous *Dose* variable is constructed by multiplying *ShareFavela* by the *Raid* dummy variable. All models cluster standard errors at the polling station level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.