

# REELECTION BACKFIRE: THE EFFECT OF REELECTION INCENTIVES ON DELEGATION OF PUBLIC SECURITY PROVISION IN MEXICO

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**ABSTRACT.** Local incumbents up for reelection face a delegation puzzle with upper levels of government. In the presence of spillovers and sum costs, delegation of public good provision may increase its efficiency but cut down its use for electoral purposes. Not delegating allows incumbents to signal responsiveness and carry credit claiming activities to win votes but may generate an inefficient public good provision. A clear tradeoff between efficiency and electoral survival arises. This paper studies the effect of reelection incentives on delegation of public security provision to upper levels of government in a country overwhelmed by criminal wars, Mexico. To do so, I exploit the staggered implementation of an electoral reform that introduced reelection for local executives from 2014 to 2022. I find that mayors up for reelection decrease the delegation of public security to the Governor of their state relative to term limited mayors. This behavior is prominent in municipalities characterized with citizens concerned by narcotraffic and insecurity, where they hold high levels of trust for police forces different from municipal ones, and where mayors are not aligned with upper level governments. By taking "the bull by the horns", mayors facing reelection signal responsiveness against crime and differentiate themselves from other political actors. Results suggest that delegation is not only a political decision but an electoral one, and that reelection incentives in party-centered systems -like Mexico- may lead mayors to go local to signal responsiveness at the expense of efficient public good provision.

**KEY WORDS:** DELEGATION, REELECTION INCENTIVES, RESPONSIVENESS, PUBLIC GOOD PROVISION, PUBLIC SECURITY, VIOLENCE, INCUMBENCY ADVANTAGE.

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# 1. Introduction

Normatively, public good provision should be determined by efficiency and equity considerations, identifying spillovers and recognizing the heterogeneity of needs and tastes across populations (????). However, governments may lack the capacity to roll out public goods efficiently due to a lack of resources, expertise, information or will. For instance, a large literature suggests electoral incentives lead public officials to inefficiently deliver goods: incumbents may favor specific regions that are electorally favorable to them (????) or those with higher political representation (???). In recognition of inefficiencies, several governments chose to delegate public good provision to upper-level governments or entities who can pool resources and decrease the politization of policies.

Delegation, however, is not an obvious choice for incumbents with electoral concerns. In the presence of spillovers and high fixed costs, delegating public good provision helps to overcome the free-rider problem (?), develop economies of scale, not neglect benefits going to certain localities, and tackle down capacity constraints, all of which increase public good efficiency (??).<sup>1</sup> However, if incumbents delegate public good provision to an upper-level entity, all -or most of- the electoral spoils accrue to the actor that delivered the good. On the contrary, if incumbents provide the public good directly they can claim responsiveness and signal a competent type to voters increasing the likelihood of electoral survival.<sup>2</sup> I call this an efficiency-electoral trade-off. This tradeoff is present both in the case of delegation “within-the-state” between different levels of government, as well as cases where states can delegate policies to supranational entities.<sup>3</sup> Given this trade-off, when will incumbents delegate policy? More importantly, how do electoral incentives affect this decision?

To respond to these questions, this paper studies the effect of electoral incentives on the delegation of policy within-the-state. Specifically, I test the differential effect of term and no-term limited mayors on the delegation of public security provision in Mexico.

Why focus on the delegation of public security provision? First, public security in Mexico provision falls -constitutionally- under the responsibility of local governments, i.e. mayors.

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<sup>1</sup>The heterogeneity of tastes and needs of citizens decrease the efficiency of delegation. For more detail see ? Decentralization Theorem. For clarity, I start the paper by assuming delegation always leads to efficiency of public good provision. I prove this to be the case for the delegation of public security provision in Mexico in Section ??, the public good and case study analyzed in this paper.

<sup>2</sup>Also, by not allowing for upper-level monitoring through delegation, incumbents give leeway to their bureaucracies to overgraze the bribe base through extortions and other rent extraction activities (?), pleasing potential political brokers. These brokers are particularly relevant in clientelistic systems like Mexico (?).

<sup>3</sup>States’ delegation of policies to supranational organizations has been a widely studied topic in the International Relations literature. For a summary see Section ??.

However, since the presidency of Felipe Calderon (PAN, 2006-2012), the Federal government pushed forth the creation of state-level centralized commands in charge of Governors, as well as other public security cooperation agreements between municipalities and other political actors -other municipalities, Governors from other states, and the President. A delegation choice was opened up for mayors, and by 2018 79.12% of municipalities in the country adopted a form of centralized command according to data from the 2019 National Census of Municipal Governments and Territories of the City of Mexico. Second, for the past 15 years Mexico has been overwhelmed by criminal wars (?). As a result, voters see peace (and thus violence) as the most relevant public good demand in the country given the high prevalence of drug-trafficking related crime.<sup>4</sup> However, the spillovers of violence and public security policies as well as heterogeneity of policy tastes makes delegation an non-obvious choice. As ? notes, spillovers make delegation the most efficient choice -the so called *Oate's Decentralization Theorem*-, but heterogeneity of tastes raises efficiency concerns on centralized public good provision. Moreover, facing drug trafficking organizations (DTOs) directly is no free lunch: DTOs have killed mayors in high rates (?). Lastly, voters in Mexico do hold the capacity to blame local politicians for violence, but only when their party is aligned with that of the Governor (?).

To test the effect of reelection incentives on public security provision, I exploit the 2014 Electoral Reform of Mexico that allowed local executives (*mayors*) to reelect for 2 consecutive periods at most and was rolled out in a step-wedge way at the state level<sup>5</sup> until 2022. The Electoral Reform, approved in February 2014, was part of the Mexican Pact Accord, a set of structural reforms negotiated by the three main political parties in Mexico at the time (PRI, PAN and PRD). Those in favor of the reform spoke to its potential benefits on politicians' efficiency and professionalization, as well as voter accountability. Three key features characterized the reform: (1) removal of term limits of mayors, and local and federal legislators for up to 2 terms; (2) introduced a "party-lock" were mayors who wish to reelect could not switch parties; and (3) did not weaken party control since nominations and funding still depended on such. In other words, the electoral reform generated reelection incentives for treated mayors that should increase their responsiveness to constituents vis-à-vis non-term limited mayors. Moreover, it did not modify the strong party system where politicians are highly dependent on their parties for candidate nomination and campaign expenses.

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<sup>4</sup>The majority of the population prefers higher rather than lower public good provision. However, heterogeneity of preferences exists across the country and time since the start of the War on Drugs in December of 2006. Prior to the COVID-19 crisis, public insecurity in Mexico was the principal public problem as measured by survey data. See [https://www.dropbox.com/s/c5dte5pscggat2c/leadingproblem\\_mexico.png?dl=0](https://www.dropbox.com/s/c5dte5pscggat2c/leadingproblem_mexico.png?dl=0)

<sup>5</sup>Similar to US states.

An event-study research design that leverages the staggered implementation of the reform and state-level variation shows that mayors facing reelection incentives decreased the signing of security cooperation agreements with the Governor relative to term-limited mayors. Results are not explained by pre-trends in security cooperation agreements or an anticipatory behavior of government officials prior to treatment. This result is robust across multiple specifications, including the use of cohort weights to account for treatment effect heterogeneity following  $\beta$ , changing the reference period, trimming the event study time periods, and standard error corrections for small number of clusters given the small number of states in Mexico, level at which the Reform was rolled out. Further validation is provided by the use of secondary research designs including  $\beta$  non-parametric generalization of the difference-in-difference estimator that does not rely on linearity assumption and corrects for invalid negative weighting in standard two-way fixed effects models, as well as  $\beta$  difference-in-difference with multiple time period correction. In terms of other endogenous concerns, by comparing first period term limited mayors with first period non-term limited mayors, I rule the typical concerns of selection on the experience and ability of politicians. This result is confirmed by finding no differences in the education quality of term limited and non-term limited incumbents.

Why do we observe this no-delegation behavior of mayors facing reelection incentives? While incumbents with reelection incentives can turn the direct provision of public goods into a signal of competence and responsiveness which yields an electoral return in the following election, term limited incumbents can't. Term limited politicians can only partially translate the electoral returns won from differentiability and credit claiming to other electoral competitions -say when running for Deputy, Governor or President- or other political and bureaucratic positions in the regional or central administration. We could think of this as a transaction cost that term-limited mayors have when trying to convert their spoils from incumbency -electoral or monetary- to other electoral races or political positions. Given this differential costs, responsiveness and signaling a competent type that takes the bull by the horns" is more attractive for incumbents facing reelection that does that do not.

Several results validate this theory. First, reelection incentives do not differentially affect the signing of cooperation agreements with the President or other political actors. These political actors are not in direct contestation for the electoral spoils of public security provision locally. As a result, they serve as a placebo test. Second, heterogeneous treatment effects show that municipalities characterized with citizens concerned by narcotraffic and insecurity show a decrease in delegation of public security relative to municipalities with term limited incumbents: when citizens are concerned of violence, mayors provide public security directly; when violence is not a concern, they prefer the Governor take charge of it. Third, heterogeneous effects are also found when citizens hold a high level of trust of

state forces: mayors choose not to delegate to avoid living in the shadow of the Governor. Lastly, mayors up for reelection and not aligned with the Governor choose not to delegate public good provision both to differentiate themselves and since citizens do not hold the capacity to blame them for violence (?).

The backlash from reelection is disastrous. First, while effort placed by the local police remains similar to that of term-limited incumbents as measured by the number of detentions per capita, a decrease in anti-narcotic activities by Federal and State forces is observed. Second, an instrumental variable approach where delegation is instrumented by the 2014 Electoral Reform, shows that not delegating increased homicides per capita by 15%, significant to the 5% level.

I draw a novel insight from these findings: while the literature stresses agency problems and the reduction of government's control over policy, this paper puts electoral incentives in the forefront for not delegating policy. Moreover, this paper flips the argument that delegation decreases the level of politization of a policy; it suggests that actually the politization of incumbents leads to a decrease in delegation. As such, it speaks directly to ? that shows that delegation depends on citizens' preferences for policy, albeit at a different level. Her argument states that when citizens dislike foreign aid, governments spend on multilateral rather than bilateral aid; when aid is relevant, the opposite happens since the distribution of aid through multilateral organizations tends to have low domestic support. This paper shows mayors with reelection incentives will delegate only when citizens are not concerned by insecurity, when not familiar with other security forces, or when they trust the security forces of competing political actors more than local ones.

A second insight shows that reelection incentives led incumbents to choose electoral spoils above efficiency concerns. While the literature has stressed multiple benefits of term-limit removal such as increased accountability, responsiveness and lower corruption (??), increase in the competence of elected politicians (?) and greater legislators' productivity (?) once we factor electoral incentives it may yield undesirable inefficiencies. As such, it goes more in line with recent literature on negative effects of reelection, fostering particularistic legislation due to politicians desire to differentiate themselves from others (?), and that longer tenures allows incumbents to collude with local firms leading to fewer bidders in public auctions and more inefficient procurement (?).

Along these lines, this paper contributes to the literature that shows that electoral incentives may lead to inefficient public good provision. ?, for instance, show that politicians under-provide public goods that cannot be targeted to voters since they care about the spoils of office. This paper extends this notion to show that incumbents with reelection incentives extract more spoils from portraying responsiveness through delivering public goods directly relative to term limited ones.

Lastly, this paper makes important contributions to the existent literature on the War on Drugs in Mexico. The paper aligns with the findings from ? that found that coordination across municipalities can reduce drug violence albeit through an opposite channel, delegation to the governor. However, results contradict the evidence that pointed the Mexican government as the actor behind large spurges of violence in the last decade (??). It also adds color to the conclusion by ? that municipalities that coordinated with upper-level governments to increase resources to combat crime increased the level of homicides in Mexico. This paper suggests that coordination in the form of delegation to upper-level governments may decrease rather than increase violence.

The next section provides a theoretical discussion on the reasons for an against delegation of policies and public good provision, followed by a discussion on the role of reelection incentives. I then provide a brief overview of the War on Drugs in Mexico and a characterization of the 2014 Electoral Reform with special emphasis on the effect in local mayors and party politics. Data collection, research design and empirical results are presented. I close by describing the unintended consequences of reelection incentives, primarily a decrease in the provision of public security and an increase in violence.

## 2. Theory

## 3. Delegation and Mexico's War on Drugs

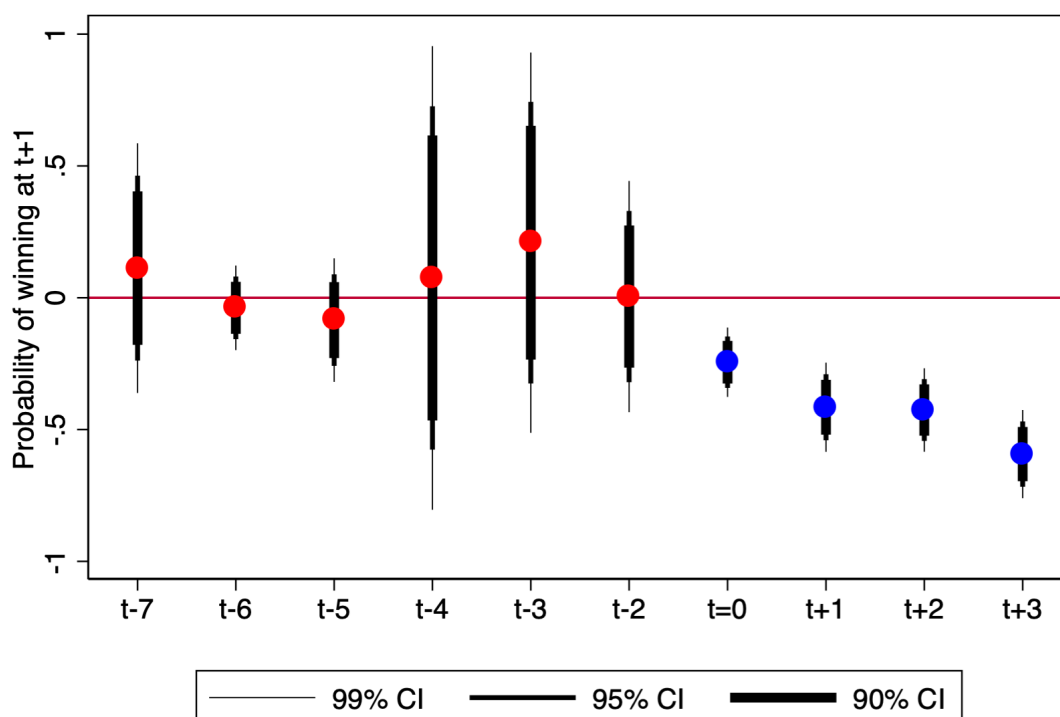
## 4. 2014 Term Limit Reform

## 5. Empirics

## 6. Main Results

Main takeaway: Reelection incentives decreased delegation of public security provision to the Governor.

**FIGURE 1.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, 2010-2018



**Note:** Figure 1 shows the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points are pre-treatment, while blue ones post-treatment.

## 7. Robustness

Main takeaways:

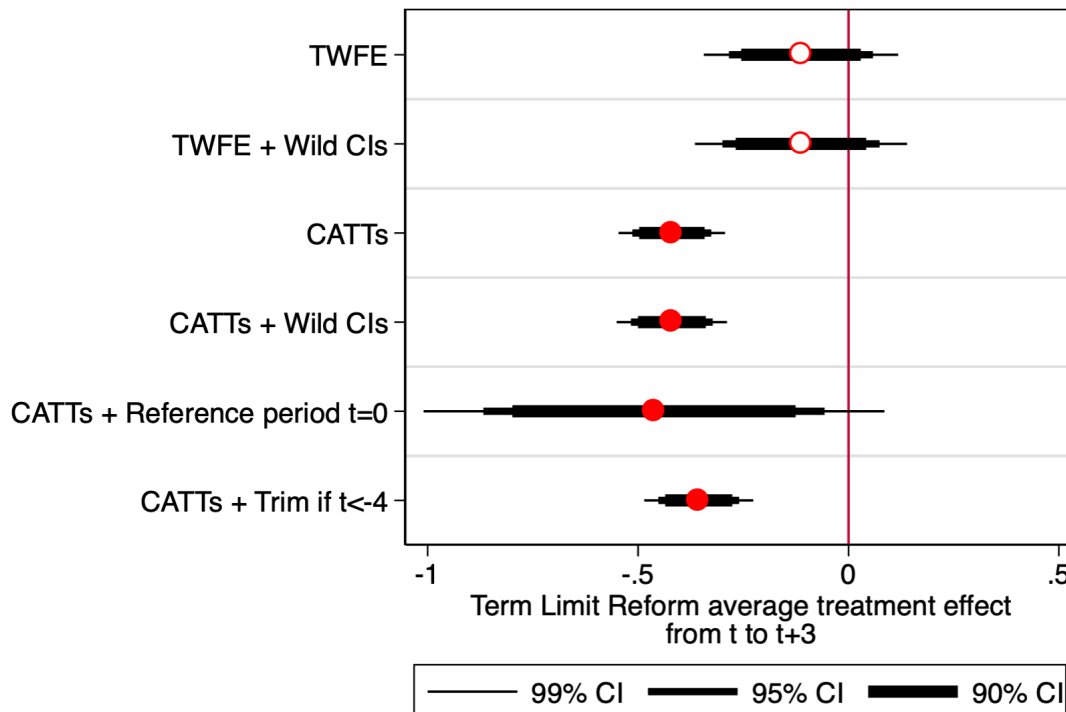
1. Results are robust across multiple specifications and models.

**TABLE 1.** Effect of 2014 Term Limit Reform on Signing Security Cooperation Agreements, Average Effect

Dependent variable: Sign Security Cooperation Agreement w/ Governor				
Model:	CATTs (1)	CATTs w/ WILD CIs (2)	Change ref. period (t=0) (3)	Trim < t-4 (4)
Reform Average Effect (from t to t+3)	-0.4197*** (0.0457)	-0.4197*** (0.0473)	-0.4622** (0.1977)	-0.3559*** (0.0468)
Observations	12,173	12,173	12,173	12,173
R-squared	0.4545	0.4545	0.4545	0.4544
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓
Parallel trend holds	✓	✓	✓	✓

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ? except for the specification that trims periods prior to t-4. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>b</sup> State-level controls include governor winning margin in last pre-treatment election and an indicator of whether the governor's party is the same as the federal incumbent party.

**FIGURE 2.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, 2010-2018

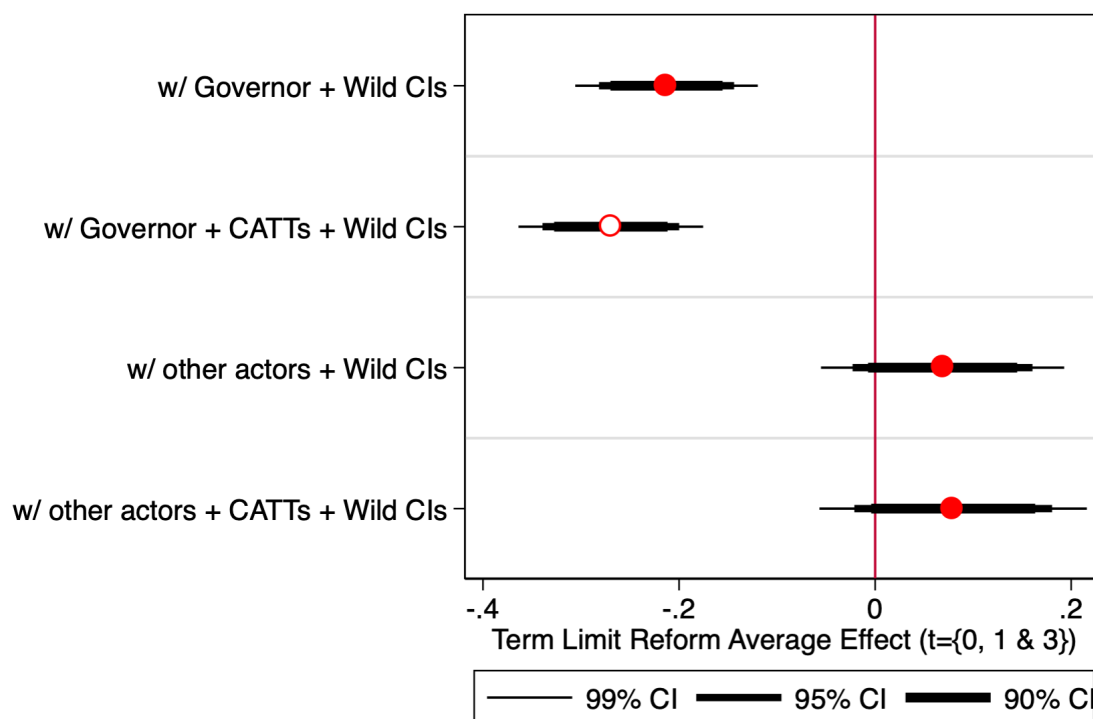


**Note:** Figure ?? shows the average treatment effect from t to t+3 across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends.



2. Results only present if upper-level government can compete in credit claiming locally (President never does this).

**FIGURE 3.** Comparison: Security Cooperation Agreements with Governor vs. Other Actors, 2014-2018



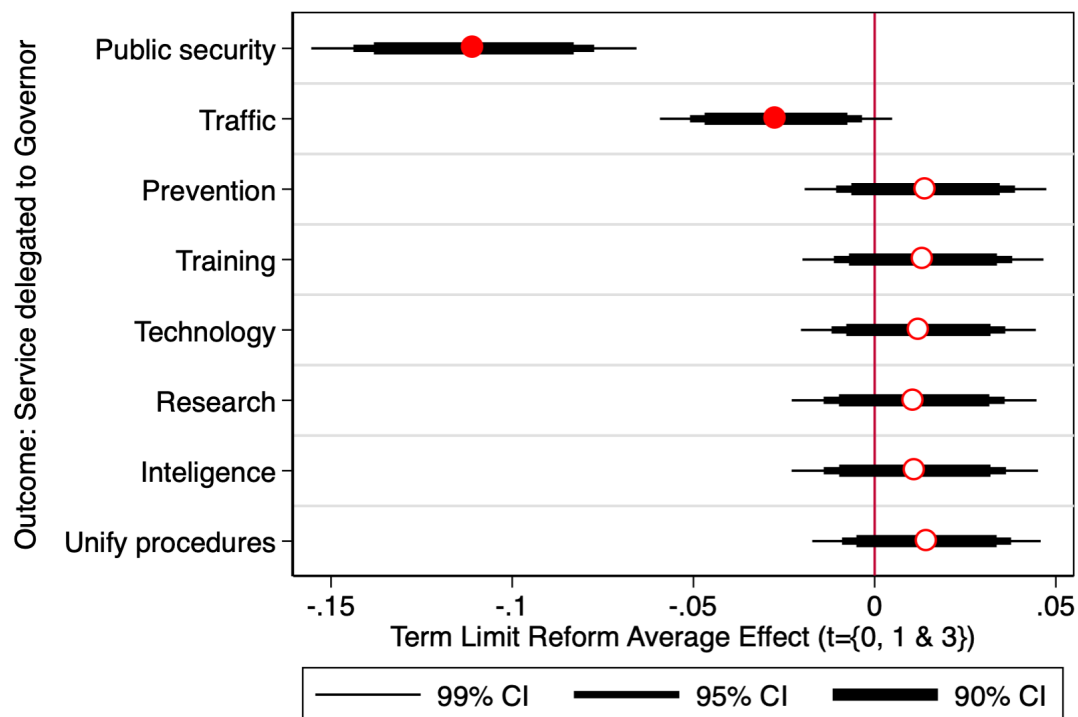
**Note:** Figure ?? shows the average treatment effect from  $t$  to  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends.

## 8. Mechanisms

Main takeaways:

1. Mayors facing reelection decrease the delegation of public security provision and traffic, but not other services.

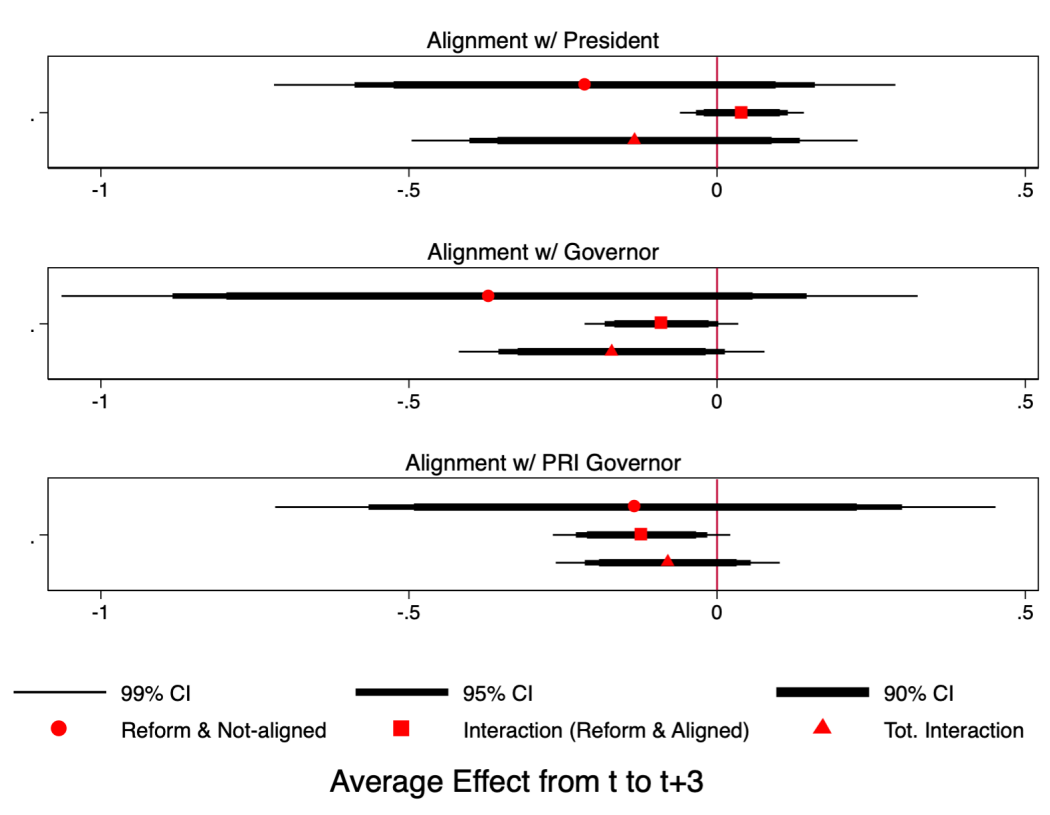
**FIGURE 4.** Comparison: Security Cooperation Agreements with Governor vs. Other Actors, 2014-2018



**Note:** Figure ?? shows the average treatment effect from  $t$ ,  $t+1$  and  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends.

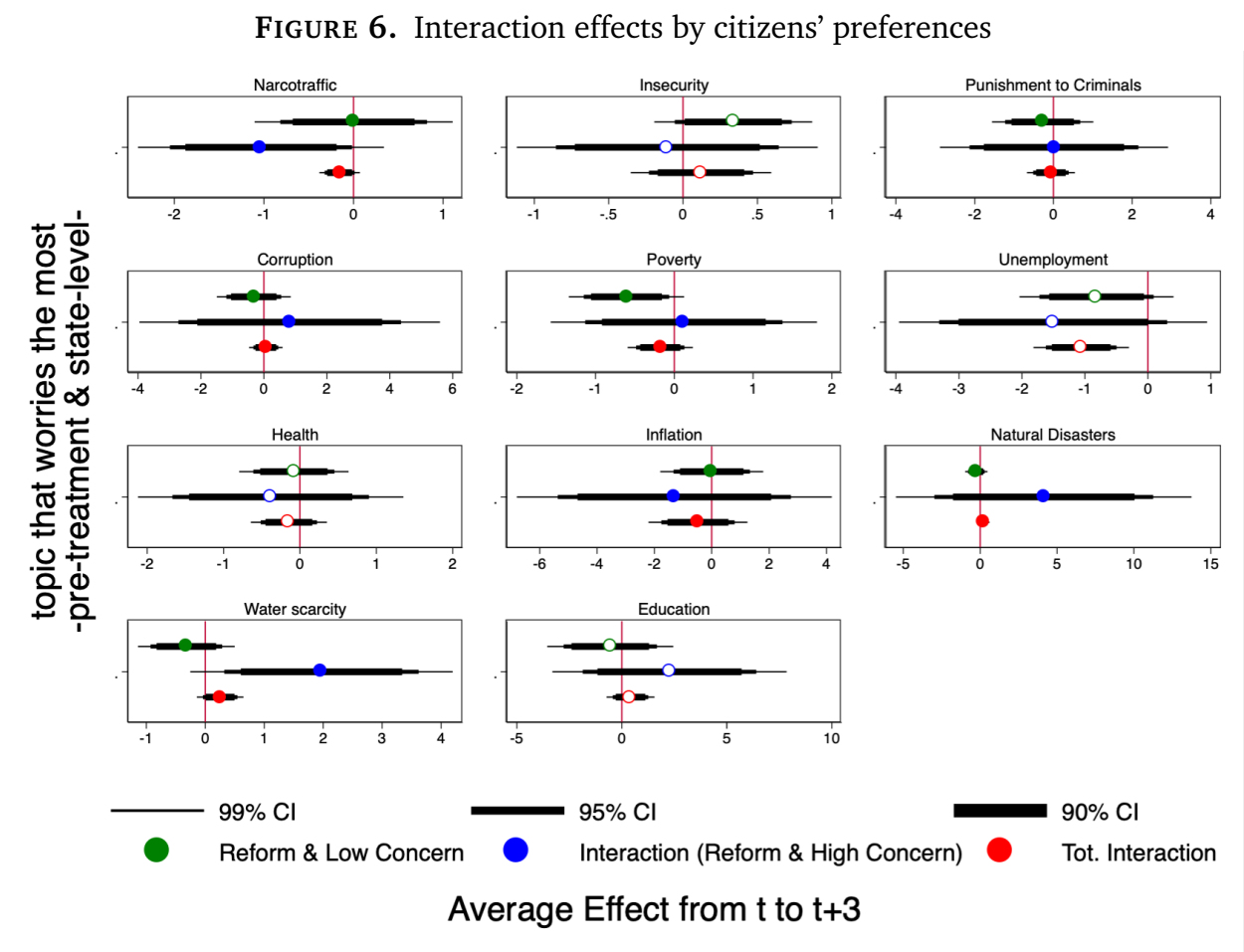
2. Alignment: If you are aligned you have a lot to loose in terms of credit claim, especially if you are from the PRI. This should be smaller for alignment with President since you are not competing directly in terms of reputation in local politics. Lastly, we should see a greater negative effect if not aligned since citizens do not blame you as much por public security inefficiencies following ?.

**FIGURE 5.** Reform interaction with Party Alignment



**Note:** Figure ?? shows the average treatment effect from  $t$  to  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends. To check parallel trends see Appendix Figure ??.

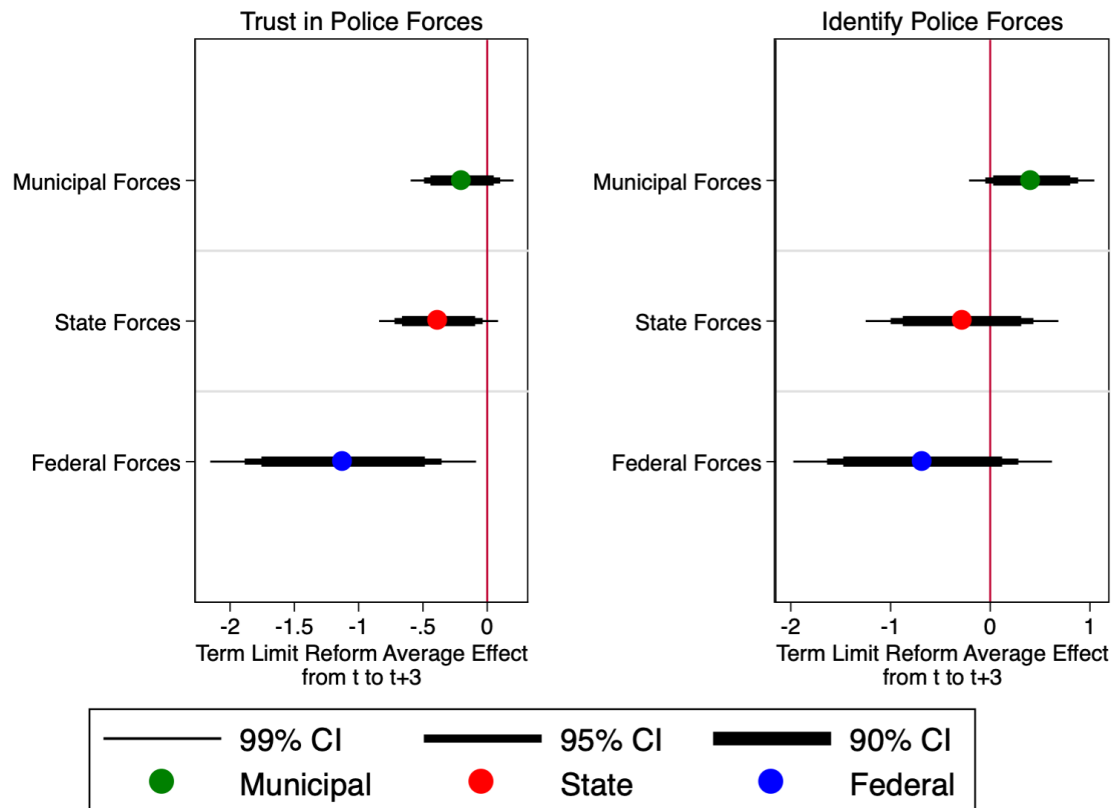
- 3. a. Mayors facing reelection want to show responsiveness to constituents preferences.
- 3. b. Mayors facing reelection sign security agreements when faced by problems "too big" or of the national order.



**Note:** Figure ?? shows the average treatment effect from  $t$  to  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

4. Mayors facing reelection do not sign agreements when other security forces are highly trusted or identified.

**FIGURE 7.** Total interaction effects by citizens' trust and identification of police forces



**Note:** Figure ?? shows the average treatment effect from t to t+3 across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

## 9. Ruling out Alternative Hypothesis

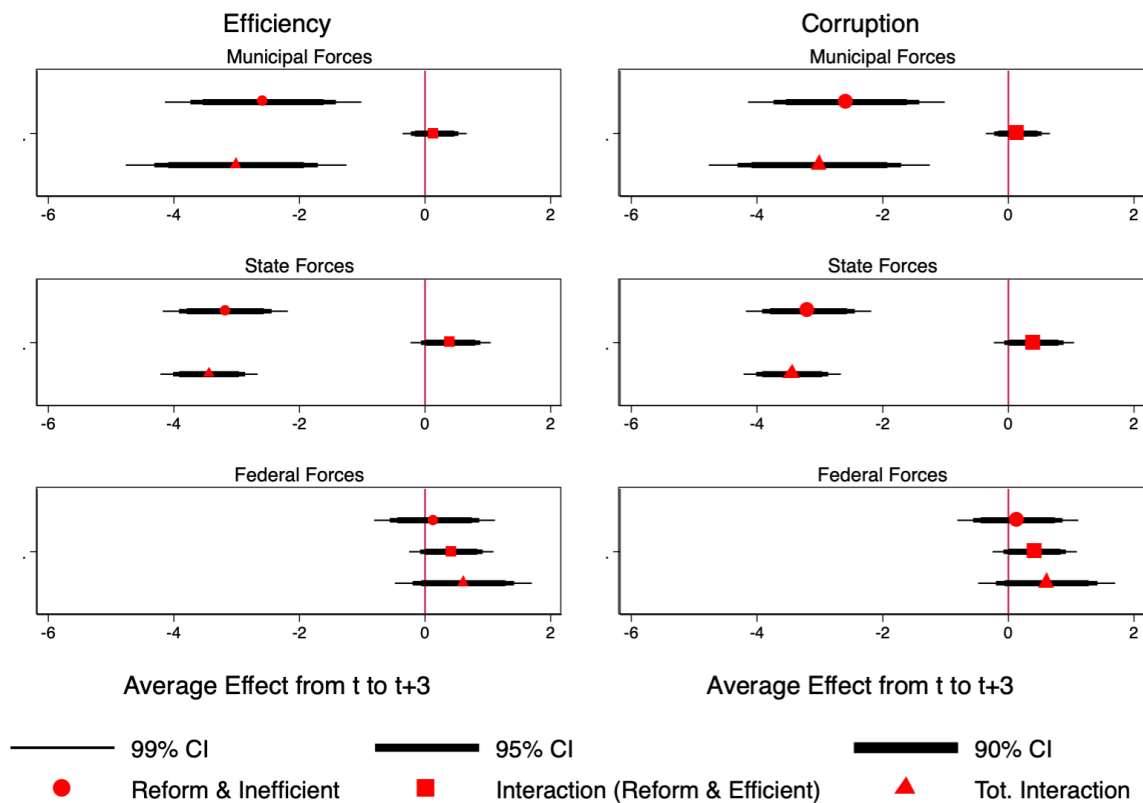
### 9.1. Selection: incumbents and challengers quality

### 9.2. Cartel Presence

All regressions control for Cartel Presence pretreatment.

### 9.3. Citizens' Evaluation of Corruption and Efficiency of Police Forces

**FIGURE 8.** Interaction effects by citizens' evaluation of efficiency and corruption of police forces



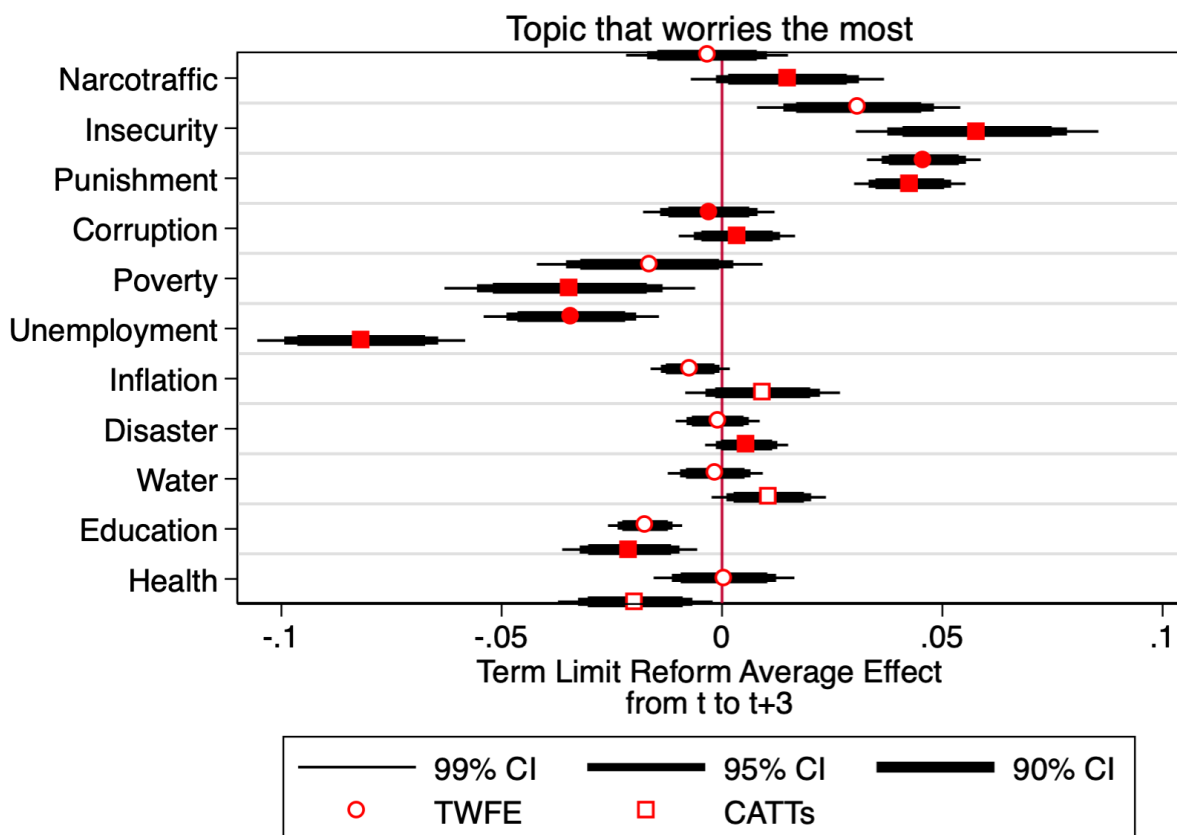
**Note:** Figure ?? shows the average treatment effect from t to t+3 across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

## 10. Unintended consequences

### 10.1. Preferences for order and security

1. PREFERENCES: citizens are more concerned about security but less about other things. Recall results are conditional on violence. So in the next election, they will look for another hawk. This ties to the incumbency advantage.

FIGURE 9. Effect of Term Limit Reform on Citizens' Preferences

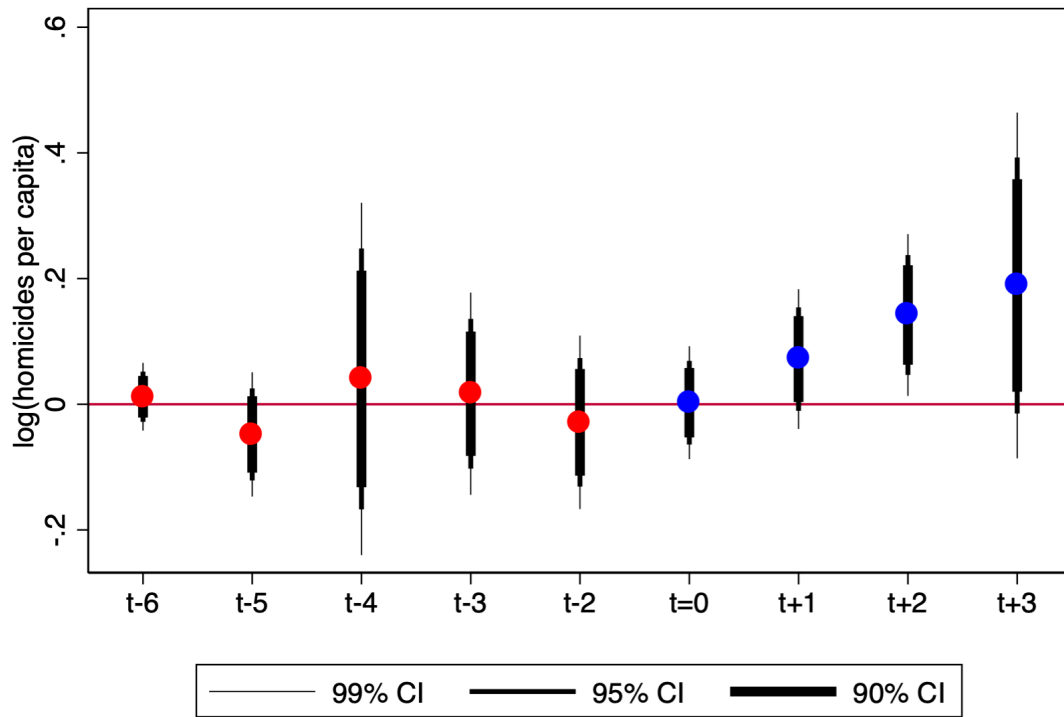


**Note:** Figure ?? shows the average treatment effect from  $t$  to  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Filled points (squares) show that parallel trends hold, while hollow ones imply pretrends.

### 10.2. Security underprovision and violence

3. VIOLENCE: increase of violence.

FIGURE 10. Effect of Term Limit Reform on Violence



**Note:** Figure ?? shows the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points are pre-treatment, while blue ones post-treatment.

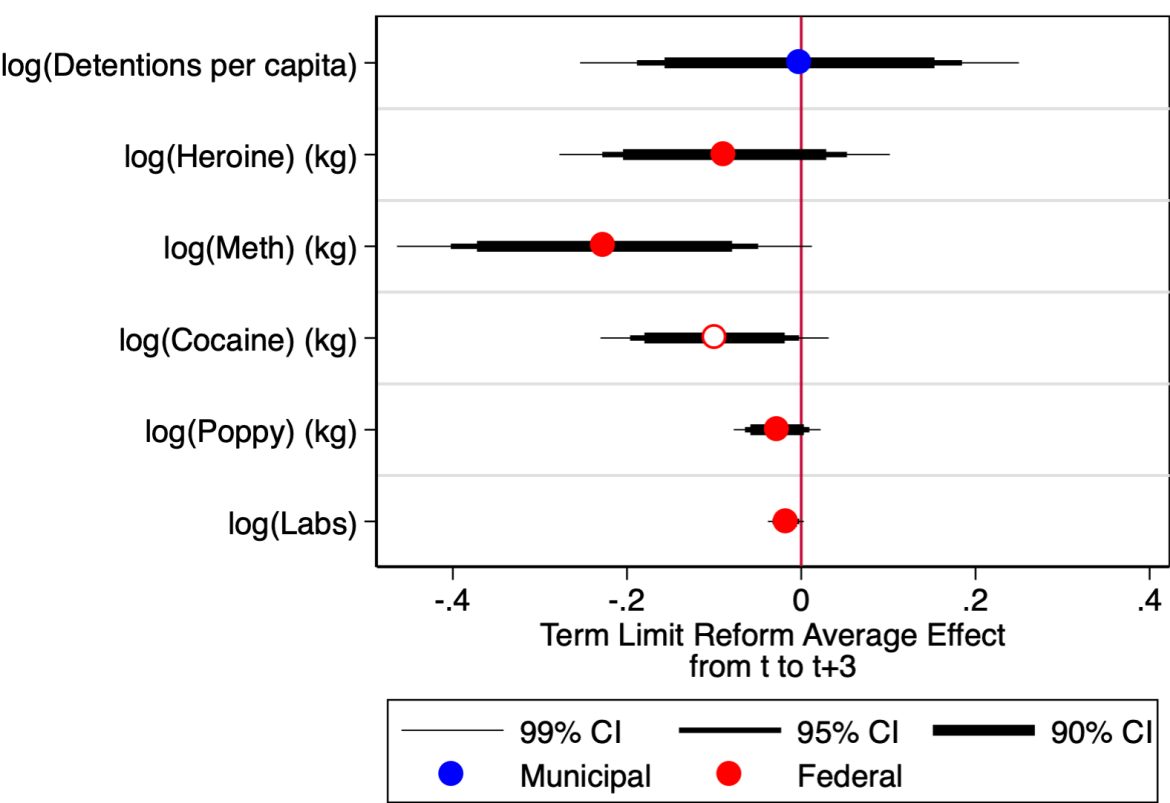


**TABLE 2.** Effect of Security Cooperation Agreements signed with the Governor on Violence

Dependent variable: log(homicides per capita)		
	(1)	(2)
Predicted Agreement w/ Governor	-0.1521* (0.0802)	-0.1521** (0.0749)
Observations	12,173	12,173
R2	0.724	0.724
Controls <sup>a</sup>	✓	✓
Mun. FE	✓	✓
Year FE	✓	✓
State Cluster S.E.		✓
Wild CI <sup>b</sup>		✓
First stage F-stat	1,739	1,739

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) were removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level unless indicated, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; and Cartel presence. <sup>a</sup> Wild bootstrap standard errors clustered at the state-level are reported when indicated.

FIGURE 11. Effect of Term Limit Reform on Effort of Local and Federal Security Forces



**Note:** Figure ?? shows the average treatment effect from  $t$  to  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

## Appendix A. Additional Tables and Figures

### A.1. Main Results

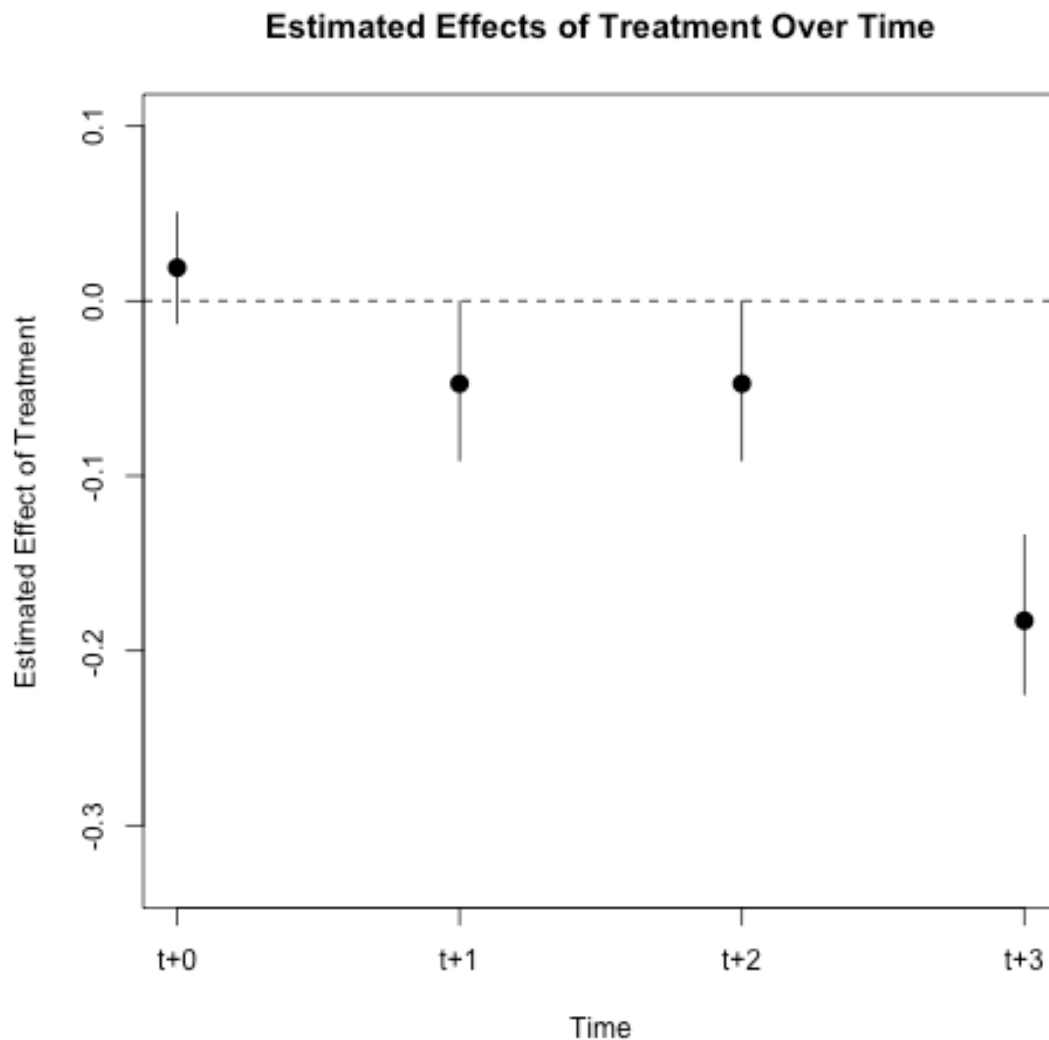
**TABLE A-1.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, 2010-2018

Dependent variable:	Security Cooperation Agreement w/ Governor <sup>a</sup>	
	(1)	(2)
Lag 7 years	0.1123 (0.1709)	0.1123 (0.7117)
Lag 6 years	-0.0383 (0.0579)	-0.0383 (0.2458)
Lag 5 years	-0.0848 (0.0846)	-0.0848 (0.2404)
Lag 4 years	0.0751 (0.3174)	0.0751 (0.2890)
Lag 3 years	0.2088 (0.2603)	0.2088 (0.2139)
Lag 2 years	0.0044 (0.1583)	0.0044 (0.2139)
Reform, time 0	-0.2446*** (0.0475)	-0.2446*** (0.0685)
Lead 1 year	-0.4154*** (0.0610)	-0.4154*** (0.0610)
Lead 2 years	-0.4259*** (0.0571)	-0.4259*** (0.0571)
Lead 3 years	-0.5931*** (0.0604)	-0.5931*** (0.0604)
Observations	12,173	12,173
R-squared	0.4545	0.4545
Mun. FEs	✓	✓
Year. FEs	✓	✓
Controls <sup>b</sup>	✓	✓
Cohort weighted	✓	✓
WILD CI		✓
Aggregate effect	-0.4197***	-0.4197***
SE (aggregate eff.)	0.0457	0.0473

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

## A.2. Robustness

**FIGURE B-1.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, propensity score matching on pretreatment covariates



**Note:** Figure ?? produced by propensity score matching that adjust for the treatment and covariate histories during the 5 year periods prior to the treatment. I report 95% bootstrap confidence intervals clustered at the state level. Covariates include those used to generate Figure ??.

**TABLE A-2.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, with t=0 as reference period

Dependent variable:	Security Cooperation Agreement w/ Governor <sup>a</sup>	
	(1)	(2)
t-6	-0.0648 (0.0400)	0.0312 (0.0925)
t-5	-0.2066** (0.0746)	-0.1867 (0.1670)
t-4	-0.0615 (0.1748)	-0.0250 (0.1609)
t-3	0.1032 (0.1363)	0.1517* (0.0848)
t-2	-0.0241 (0.1157)	-0.0972 (0.0848)
t-1	-0.0747 (0.0917)	-0.0738 (1.6557)
t+1	-0.2856 (0.2014)	-0.7543* (0.4304)
t+2	-0.6194** (0.2337)	-0.7092* (0.3702)
t+3	-0.4815* (0.2643)	-0.6337* (0.3141)
Observations	12,173	12,173
R-squared	0.4545	0.4561
Mun. FEs	✓	✓
Year. FEs	✓	✓
Controls <sup>b</sup>	✓	✓
Cohort weighted	✓	✓
WILD CI		✓
Aggregate effect	-0.4622**	-0.6990**
SE (aggregate eff.)	0.1977	0.3366

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 0) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

**TABLE A-3.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, trimming periods

Dependent variable:	Security Cooperation Agreement w/ Governor <sup>a</sup>	
	(1)	(2)
t-4 years	0.1961 (0.2680)	0.1961 (0.8260)
t-3	0.2193 (0.2070)	0.2193 (0.2702)
t-2	0.0370 (0.1546)	0.0370 (0.2702)
t=0 (Reform)	-0.3057*** (0.0682)	-0.3057 (0.4093)
t+1	-0.2858*** (0.0725)	-0.2858 (0.2610)
t+2	-0.2389*** (0.0823)	-0.2389 (0.2369)
t+3	-0.5931*** (0.0604)	-0.5931*** (0.0715)
Observations	12,173	12,173
R-squared	0.4544	0.4544
Mun. FEs	✓	✓
Year. FEs	✓	✓
Controls <sup>b</sup>	✓	✓
Cohort weighted	✓	✓
WILD CI		✓
Aggregate effect	-0.3559***	-0.3559**
SE (aggregate eff.)	0.0468	0.1395

Notes: Coefficients show IW estimators following ?. I trimmed the periods lag 8, 7, 6 and 5, and removed the period 1 to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period.

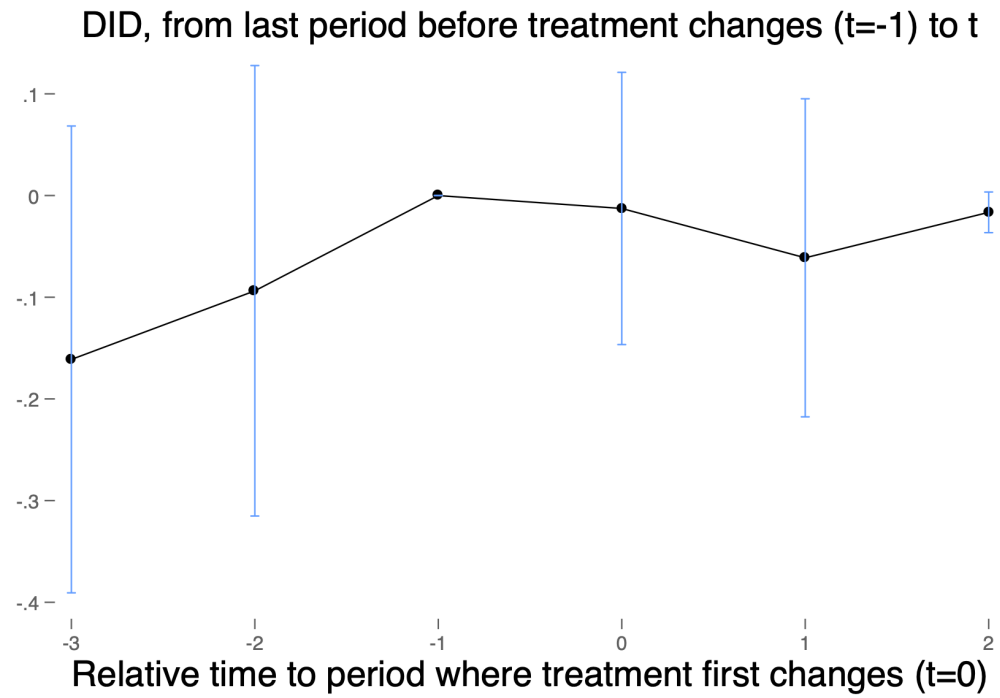
<sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

**TABLE A-4.** Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, ? correction

Dependent variable:	Agreement A	Agreement B <sup>a</sup>
	(1)	(2)
t-2	-0.161 (0.117)	-0.158 (0.125)
t-1	-0.094 (0.113)	-0.110 (0.128)
Reform (t=0)	-0.013 (0.068)	-0.040 (0.091)
t+1	-0.061 (0.080)	-0.098 (0.081)
t+2	-0.017* (0.010)	-0.017* (0.010)
Controls <sup>b</sup>	✓	✓

Notes: Coefficients show corrected estimators following ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%.<sup>a</sup> Secondary measure of security cooperation agreements. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Car-tel presence.

**FIGURE B-2.** Effect of Term Limit Reform on Security Cooperation  
Agreements signed with the Governor, 2010-2018





**TABLE A-5. Comparison: Security Cooperation Agreements with Governor vs. Other Actors, 2014-2018**

Dependent variable: Sign Security Cooperation Agreement				
	w/ Governor <sup>a</sup>		w/ Other Political Actors <sup>b</sup>	
	(1)	(2)	(3)	(4)
t-4	0.3516 (1.7224)	0.0197 (0.3292)	-0.2760 (0.5875)	-0.0326 (0.0763)
t-3	-0.7347 (37.4822)	-0.0102*** (0.0000)	0.2470 (15.0268)	0.2193 (0.2702)
t-2	0.3855 (0.3262)	0.1418 (0.1318)	-0.1496 (0.1245)	-0.0648 (0.0524)
Reform (t=0)	0.2227*** (0.0588)	0.0064 (0.0354)	-0.0599** (0.0273)	-0.0089 (0.0069)
t+1	-0.2203** (0.0920)	-0.2230*** (0.0435)	0.1148 (0.0904)	-0.2858 (0.2610)
t+3	-0.5915*** (0.0783)	-0.5921*** (0.0708)	0.1660* (0.0953)	0.1665 (0.1040)
Observations	4,382	4,382	4,382	4,382
R-squared	0.6434	0.6434	0.5469	0.5469
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓	✓
Cohort weighted		✓		✓
WILD CI	✓	✓	✓	✓
Aggregate effect	-0.213***	-0.2696***	0.069	0.0796
SE (aggregate eff.)	0.033	0.0339	0.045	0.0491

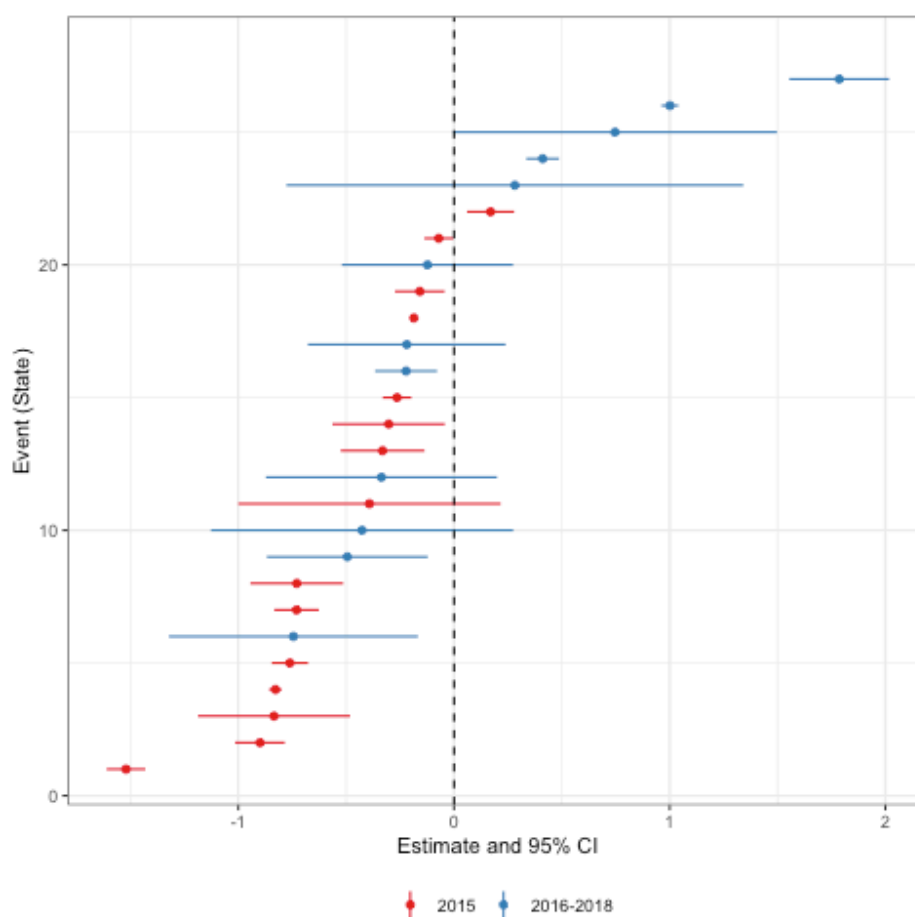
Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 5 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers primarily to the President but could include Governors and mayors from other states or other municipalities from the same state. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

**TABLE A-6.** Test on selection on unobservables

	(1)
Fitted value	0.1312 (0.0780)
Observations	10,668
R2	0.459
Mun. FE	✓
Year FE	✓
State Cluster S.E.	✓

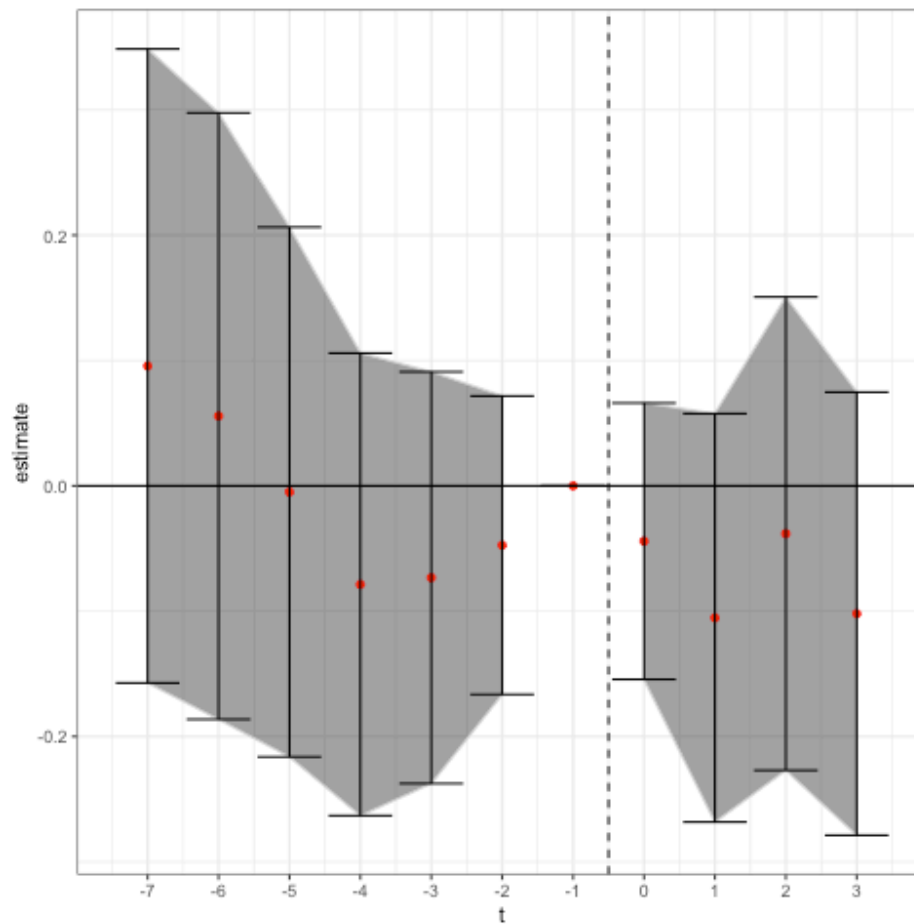
Notes: I follow ? to check if unobserved variation is likely to explain the signing of security cooperation agreements with the Governor by mayors. To do so, I regress the treatment (whether the municipality held reelection) on all the available covariates used for Figure ???. I then take the fitted value from the regression and use it to predict each outcome, this time including unit and year fixed effects. This test suggests that – under the assumption that observables are representative of unobservables – selection on unobservables is not driving the results.

**FIGURE B-3.** “Event-by-event analysis” following ?  
-95% confidence intervals-



Note: Estimate separate treatment effects for each event, i.e. each Mexican state in the sample. Each event dataset contains the treated state and all other states that never received treatment or received treatment after the sample window ( $t + 1$ ).

**FIGURE B-4.** “Stacked dataset analysis” following ?  
-95% confidence intervals-



Note: Utilize estimated coefficients from Figure ?? and stack them in relative time, and estimate lead and lag variables to treatment following the event-by-event analysis setup, i.e. without treatment containment from using prior treated units of controls. Analysis done stacking at the cohort level, and adding municipality and year fixed effects, and clustered standard errors at the state level.

### A.3. Mechanisms

**FIGURE B-5.** Effect of 2014 Term Limit Reform on Motives to Sign Security Agreements w/ Governor

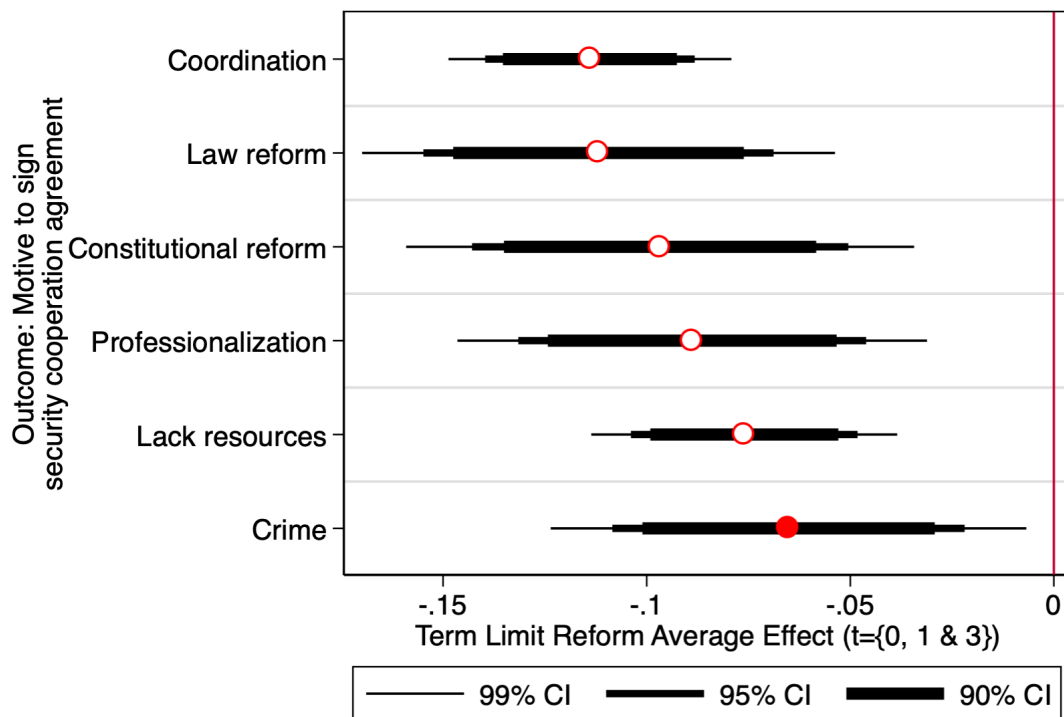


TABLE A-7. Effect of 2014 Term Limit Reform on Motives to Sign Security Agreements w/ Governor

Dependent variable: Motive to Sign Security Cooperation Agreement w/ Governor						
	Cons. reform (1)	Law reform (2)	Lack resources (3)	Professionalization (4)	Coordination (5)	Crime (6)
t-7	-0.2347*** (0.0409)	-0.2580** (0.1174)	-0.0957* (0.0481)	-0.1999*** (0.0669)	-0.1558* (0.0843)	-0.1540 (0.1079)
t-6	-0.0757*** (0.0176)	-0.0876*** (0.0199)	-0.0615*** (0.0161)	-0.0647 (0.0585)	-0.0824** (0.0344)	-0.0370 (0.0265)
t-5	0.0217 (0.0582)	-0.0411 (0.0577)	0.0562 (0.0475)	0.0567 (0.0744)	-0.0095 (0.0706)	0.0415 (0.0444)
t-4	0.0218 (0.1001)	-0.0823 (0.0843)	0.1177 (0.0832)	0.0634 (0.1038)	-0.1207 (0.2098)	0.0333 (0.1167)
t-3	-0.0386 (0.1052)	-0.0161 (0.0840)	0.0724 (0.1002)	0.0800 (0.0738)	0.0402 (0.1660)	0.0731 (0.1061)
t-2	-0.1161 (0.1009)	-0.0919 (0.0915)	0.0226 (0.0640)	-0.0824 (0.1195)	-0.2781* (0.1375)	-0.0756 (0.0666)
Reform (t=0)	0.0457 (0.0278)	0.0292 (0.0183)	0.0214 (0.0179)	0.0282 (0.0201)	0.0233 (0.0209)	0.0272* (0.0146)
t+1	-0.0906*** (0.0164)	-0.1071*** (0.0182)	-0.0935*** (0.0106)	-0.0935*** (0.0160)	-0.1215*** (0.0291)	-0.0735*** (0.0121)
t+3	-0.2452*** (0.0535)	-0.2576*** (0.0484)	-0.1560*** (0.0350)	-0.2011*** (0.0463)	-0.2436*** (0.0431)	-0.1492*** (0.0527)
Observations	9,725	9,725	9,725	9,725	9,725	9,725
R-squared	0.2974	0.3021	0.2617	0.2722	0.2866	0.2594
Mun. FEs	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓
Controls <sup>a</sup>	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓
Reform aggregate effect	-0.0967*** (0.0225)	-0.1118*** (0.0210)	-0.0760*** (0.0136)	-0.0888*** (0.0208)	-0.1139*** (0.0125)	-0.0652*** (0.0211)
SE						

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE A-8. Effect of 2014 Term Limit Reform on Services Delegated to the Governor

Dependent variable: Services Delegated to Governor								
	Public security (1)	Traffic (2)	Prevention (3)	Training (4)	Technology (5)	Research (6)	Intelligence (7)	Unify procedures (8)
t-2	-0.0244 (0.1049)	-0.0447 (0.0811)	-0.0598*** (0.0021)	-0.0565*** (0.0012)	-0.0567*** (0.0016)	-0.0596*** (0.0017)	-0.0596*** (0.0017)	-0.0506*** (0.0052)
Reform (t=0)	0.0701 (0.0435)	0.0257 (0.0369)	0.0175 (0.0137)	0.0214 (0.0142)	0.0194 (0.0126)	0.0194 (0.0138)	0.0204 (0.0135)	0.0233 (0.0147)
t+1	-0.0947* (0.0509)	-0.0259* (0.0147)	0.0106 (0.0198)	0.0053 (0.0193)	0.0047 (0.0197)	0.0024 (0.0201)	0.0018 (0.0205)	0.0053 (0.0174)
t+3	-0.2847*** (0.0430)	0.0000 (0.0000)	-0.1560*** (0.0350)	-0.2011*** (0.0463)	-0.2436*** (0.0431)	-0.1492*** (0.0527)	-3.1334*** (0.2407)	1.0165* (0.5277)
Observations	4,865	4,865	3,244	3,244	3,244	3,244	3,244	12,173
R-squared	0.4234	0.3703	0.5567	0.5477	0.5409	0.5473	0.5467	0.4612
Mun. FEs	✓	✓	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓
Reform average effect	-0.1031***	-0.0242	0.0094	0.0133	0.0121	0.0109	0.0111	0.0143
SE (average effect)	(0.0225)	(0.0162)	(0.0080)	(0.0120)	(0.0117)	(0.0122)	(0.0123)	(0.0114)

Notes: Coefficients show IW estimators following ?. Relative time periods prior to t-2 do not exist and lag 1 is removed to avoid collinearity problems noted by ? and serves as the reference period. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Carrel presence.

**TABLE A-9. Party Alignment Total Interaction Effects**

Dependent variable: Signing Security Cooperation Agreement			
Party Alignment:	w/ President	w/ Governor	w/ Governor from PRI
	(1)	(2)	(3)
t-7	-0.2389*	-0.0747**	0.0000
	(0.1375)	(0.0291)	(0.0000)
t-6	-0.0810	0.0004	-0.0442
	(0.0881)	(0.0509)	(0.0467)
t-5	-0.1180	-0.2348**	-0.2752***
	(0.1032)	(0.0973)	(0.0949)
t-4	0.0631	-0.1337	-0.1757
	(0.1496)	(0.1292)	(0.1304)
t-3	0.3430**	0.2040**	0.1615**
	(0.1627)	(0.0790)	(0.0781)
t-2	0.0052	-0.0577	0.0503
	(0.1546)	(0.1227)	(0.1548)
Reform (t=0)	-0.1667	-0.2601*	0.1288
	(0.1884)	(0.1297)	(0.1236)
t+1	-0.2121	-0.6036***	-0.0941
	(0.1912)	(0.2122)	(0.1633)
t+2	-0.1075	-0.5550**	-0.5689**
	(0.2467)	(0.2671)	(0.2763)
t+3	-0.2125	-0.4193	-0.4864
	(0.2204)	(0.3757)	(0.3788)
Observations	12,173	12,173	12,173
R-squared	0.4557	0.4570	0.4551
Mun. FEs	✓	✓	✓
Year. FEs	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓
Cohort weighted	✓	✓	✓
Reform average effect	-0.1339	-0.1710*	-0.0799
SE (average effect)	(0.1306)	(0.0895)	(0.0656)

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to signing a security cooperation agreement with any of the following actors. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.



TABLE A-10. Reform interaction with citizens' preferences

Dependent variable: Signing Security Cooperation Agreement w/ Governor									
Jurisdiction:	Municipal			State		Federal			
	Trust in Police Force:	Traffic (1)	Preventive (2)	State Police (3)	State Attorney Police (4)	Federal Police (5)	Ministerial Police (6)	Army (7)	Marines (8)
t-7		0.1781 (0.1657)	0.0000 (0.0000)	0.1737 (0.1372)	0.1269 (0.1137)	0.0908 (0.0736)	0.1162 (0.0858)	0.1093* (0.0638)	0.0788 (0.0557)
t-6		-0.0459 (0.0801)	-0.0601 (0.0481)	-0.0415 (0.0539)	-0.0566 (0.0457)	0.0056 (0.0390)	-0.0538 (0.0379)	0.0234 (0.0413)	0.0038 (0.0413)
t-5		-0.8924*** (0.2538)	-0.2958 (0.2471)	-0.7754 (0.6290)	-1.3248 (0.9127)	-0.8583** (0.3310)	-1.3845*** (0.2852)	-0.6699** (0.3135)	-0.5789 (0.3456)
t-4		-0.8378 (0.7686)	-0.4847 (0.7828)	-0.8334 (0.7594)	-1.8134 (1.3211)	-1.0907 (0.7492)	-1.2390 (1.0418)	-0.3788 (0.5581)	-0.4918 (0.6697)
t-3		-0.0583 (0.8134)	-0.2255 (0.8597)	-0.4855 (0.7510)	-1.8474 (1.3390)	-0.6963 (0.8562)	-0.8293 (1.1144)	0.1189 (0.6286)	-0.1128 (0.7221)
t-2		0.0349 (0.5384)	-0.2669 (0.5922)	-0.2886 (0.4176)	-0.6193 (0.8964)	-0.6132 (0.4795)	-0.3460 (0.7851)	-0.4240 (0.3186)	-0.4018 (0.4479)
Reform (t=0)		-0.4445 (0.4490)	0.1161 (0.4974)	-0.5433 (0.4116)	-0.3590 (1.1629)	-1.2945** (0.5674)	-0.8582 (0.7679)	-0.4517 (0.4624)	-0.8450 (0.5361)
t+1		-0.9837 (0.5947)	-0.2187 (0.5769)	-1.3877** (0.6053)	-1.3448 (1.4393)	-2.4944*** (0.7475)	-1.8551* (0.9450)	-1.5411** (0.6971)	-1.8923** (0.6934)
t+2		-1.8509*** (0.5939)	-1.6314** (0.6872)	-1.9022** (0.8555)	-4.0615*** (1.1352)	-2.2753*** (0.7941)	-3.3031*** (0.6820)	-1.2009 (0.7654)	-1.8294** (0.6810)
t+3		-0.1382 (1.1166)	-1.5280 (1.1456)	-0.9653 (0.7908)	-1.9755* (1.0802)	-0.9980 (1.4571)	-1.1886 (1.2863)	0.0385 (1.1601)	-0.9525 (1.1245)
Observations		12,173	12,173	12,173	12,173	12,173	12,173	12,173	12,173
R-squared		0.4666	0.4641	0.4675	0.4673	0.4642	0.4719	0.4666	0.4666
Mun. FEs		✓	✓	✓	✓	✓	✓	✓	✓
Year. FEs		✓	✓	✓	✓	✓	✓	✓	✓
Controls <sup>b</sup>		✓	✓	✓	✓	✓	✓	✓	✓
Cohort weighted		✓	✓	✓	✓	✓	✓	✓	✓
Reform average effect		-0.1400 (0.0944)	-0.2053 (0.1633)	-0.3431** (0.1594)	-0.2984** (0.1455)	-0.5739** (0.2673)	-0.2614** (0.1107)	-0.4636 (0.4248)	-0.4837* (0.2374)
SE (average effect)									

Notes: Coefficients show IW estimators following ? . Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ? . Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE A-11. Reform interaction with citizens' being able to identify a Police Force

Identify Policy Force:	Municipal		State		Federal		
	Traffic	Preventive	State Police	State Attorney Police	Federal Police	Ministerial Police	Army
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Marines	(8)						
t-7	-0.8572 (0.6544)	0.1007 (0.0978)	0.0649 (0.0611)	0.0783 (0.0697)	-2.5321*** (0.8962)	0.0632 (0.0550)	-1.4640* (0.8372)
t-6	-0.2641 (0.2039)	0.0248 (0.0609)	0.0135 (0.0467)	0.0056 (0.0441)	-0.7692*** (0.2696)	-0.0035 (0.0413)	-0.4466* (0.2577)
t-5	-0.4097 (0.3986)	-0.0652 (0.3080)	0.6451 (0.3960)	0.1762 (0.4004)	-1.1340** (0.4306)	-0.7691*** (0.2589)	-0.8805 (0.5274)
t-4	0.3350 (0.5455)	0.1050 (0.5451)	0.7461 (0.4774)	-0.0893 (0.6583)	-1.6040*** (0.5716)	-0.2211 (0.7553)	-0.7589 (0.8538)
t-3	0.8549 (0.5572)	0.3354 (0.6384)	0.8618* (0.5038)	-0.1098 (0.7313)	-1.2530** (0.6065)	0.2973 (0.8187)	-0.3261 (0.8829)
t-2	-0.0741 (0.3985)	0.0173 (0.3426)	0.3106 (0.3583)	-0.0035 (0.4741)	-1.1572** (0.4705)	-0.2290 (0.5458)	-0.7416 (0.5501)
Reform (t=0)	0.0965 (0.3746)	-0.3095 (0.5580)	-0.6740 (0.5072)	-0.0176 (0.5448)	-1.7122*** (0.5196)	-0.3017 (0.5185)	-0.8230 (0.4724)
t+1	0.1452 (0.4015)	-0.8415 (0.7920)	-0.5733 (0.6386)	-0.5894 (0.7035)	-1.1449** (0.4877)	-1.2316 (0.7296)	-0.8753 (0.5560)
t+2	0.4499 (0.3760)	-0.7212 (0.7799)	0.0862 (0.6272)	-1.4956** (0.7215)	-0.5687 (0.5955)	-1.6626** (0.6266)	-0.4091 (0.6311)
t+3	1.1277 (0.9218)	-0.5739 (1.2931)	-0.6702 (0.9352)	-1.2519 (1.0598)	-1.7933 (1.0758)	0.0623 (1.0916)	-0.5981 (0.9325)
Observations	12,173	12,173	12,173	12,173	12,173	12,173	12,173
R-squared	0.4688	0.4599	0.4659	0.4658	0.4624	0.4783	0.4645
Mun. FEs	✓	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓
Reform average effect	0.3037	-0.4471	-0.2964	-0.3087	-0.7782**	-0.2768	-0.5017
SE (average effect)	(0.3233)	(0.6044)	(0.3716)	(0.2401)	(0.2868)	(0.2411)	(0.4665)
							(0.2669)

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE A-12. Reform interaction with citizens' efficiency evaluation of police forces

Jurisdiction: Efficiency Policy Force:	Municipal			State			Federal		
	Traffic (1)	Preventive (2)	State Police (3)	State Attorney Police (4)	Federal Police (5)	Ministerial Police (6)	Army (7)	Marines (8)	
t-7	0.1495 (0.1280)	0.0000 (0.0000)	0.1580 (0.1237)	0.1178 (0.1059)	0.0821 (0.0677)	0.1125 (0.0823)	0.0996 (0.0592)	0.0723 (0.0533)	
t-6	-0.0430 (0.0554)	-0.0600 (0.0481)	-0.0408 (0.0487)	-0.0550 (0.0432)	0.0050 (0.0413)	-0.0539 (0.0372)	0.0218 (0.0432)	0.0031 (0.0431)	
t-5	-0.8214** (0.2173)	-0.2661 (0.2280)	-0.6765 (0.5991)	-1.0574 (0.8293)	-0.8511** (0.3265)	-1.3151*** (0.2946)	-0.6265* (0.3331)	-0.5477 (0.3312)	
t-4	-0.5218 (0.6322)	-0.3094 (0.6711)	-0.6839 (0.7109)	-1.4607 (1.2102)	-1.0699 (0.6659)	-1.1764 (0.9647)	-0.3632 (0.5751)	-0.4794 (0.6316)	
t-3	0.1534 (0.6633)	-0.0826 (0.7380)	-0.3839 (0.6994)	-1.5521 (1.2330)	-0.6947 (0.7686)	-0.7613 (1.0338)	0.1118 (0.6450)	-0.1206 (0.6843)	
t-2	0.1301 (0.4219)	-0.1088 (0.5170)	-0.2605 (0.3883)	-0.4476 (0.8207)	-0.6274 (0.4341)	-0.3362 (0.7275)	-0.4306 (0.3376)	-0.4001 (0.4258)	
Reform (t=0)	-0.2825 (0.3771)	0.2132 (0.4424)	-0.4068 (0.3661)	-0.1690 (1.0199)	-1.2332** (0.5445)	-0.6252 (0.7224)	-0.4515 (0.4956)	-0.8273 (0.5171)	
t+1	-0.8544 (0.5069)	-0.1639 (0.5180)	-1.2047** (0.5515)	-1.0867 (1.2521)	-2.4180*** (0.7203)	-1.5837* (0.9025)	-1.5141** (0.7243)	-1.8447*** (0.6586)	
t+2	-1.6548*** (0.5166)	-1.5020** (0.6272)	-1.7252** (0.8167)	-3.6912*** (1.0720)	-2.2110*** (0.7669)	-3.0680*** (0.6529)	-1.1837 (0.7910)	-1.7816** (0.6492)	
t+3	-0.0738 (0.9252)	-1.2495 (1.0025)	-0.8880 (0.7675)	-1.8369* (1.0415)	-1.0878 (1.3469)	-1.0650 (1.1792)	-0.0721 (1.2083)	-1.0091 (1.0552)	
Observations	12,173	12,173	12,173	12,173	12,173	12,173	12,173	12,173	
R-squared	0.4692	0.4656	0.4672	0.4675	0.4642	0.4725	0.4667	0.4667	
Mun. FEs	✓	✓	✓	✓	✓	✓	✓	✓	
Year. FEs	✓	✓	✓	✓	✓	✓	✓	✓	
Controls <sup>b</sup>	✓	✓	✓	✓	✓	✓	✓	✓	
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓	
Reform average effect	-0.1373 (0.0917)	-0.1957 (0.1697)	-0.3432* (0.1707)	-0.2914* (0.1453)	-0.6190** (0.2769)	-0.2679** (0.1215)	-0.5001 (0.4693)	-0.5024** (0.2369)	
SE (average effect)									

Notes: Coefficients show IW estimators following ? . Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ? . Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE A-13. Reform interaction with citizens' corruption evaluation of police forces

Dependent variable: Signing Security Cooperation Agreement w/ Governor								
Jurisdiction:	Municipal			State		Federal		
	Traffic (1)	Preventive (2)	State Police (3)	State Attorney Police (4)	Federal Police (5)	Ministerial Police (6)	Army (7)	Marines (8)
t-7	0.1477 (0.2864)	0.0419 (0.3087)	0.0402 (0.2813)	-0.0324 (0.0946)	-0.0147 (0.3434)	-0.0933 (0.0782)	-0.0543 (0.1059)	-0.1444 (0.1083)
t-6	0.0301 (0.0796)	0.0011 (0.1139)	-0.0013 (0.1017)	-0.0258 (0.0568)	-0.0133 (0.1316)	-0.0488 (0.0524)	-0.0160 (0.0622)	-0.0432 (0.0617)
t-5	-0.1338 (0.1599)	-0.0973 (0.1895)	-0.1177 (0.1822)	-0.9190*** (0.1316)	-0.2156 (0.2160)	-0.8364*** (0.1066)	0.4054** (0.1573)	0.4021** (0.1567)
t-4	-1.3881*** (0.3821)	-0.8179 (0.5316)	-1.1187*** (0.3690)	-1.3964*** (0.3654)	-0.7440 (0.5666)	-1.2269*** (0.3341)	0.0944 (0.3531)	0.3231 (0.4236)
t-3	-1.6818*** (0.3568)	-0.9104 (0.5728)	-1.2935*** (0.3376)	-1.1282*** (0.3266)	-0.7637 (0.6363)	-1.0065*** (0.2992)	0.0275 (0.3608)	0.2564 (0.4274)
t-2	-0.2879 (0.2474)	-0.2198 (0.2868)	-0.2301 (0.2681)	-0.9068*** (0.1970)	-0.2657 (0.2943)	-0.7393*** (0.1573)	0.3265 (0.2314)	0.3743 (0.2279)
Reform (t=0)	-2.2651*** (0.2832)	-1.5561*** (0.5290)	-1.9299*** (0.2479)	-1.0484*** (0.1614)	-1.3274** (0.5851)	-0.9674*** (0.1417)	-0.8107*** (0.2515)	-0.6875*** (0.2343)
t+1	-3.1112*** (0.3902)	-2.2160*** (0.6501)	-2.6228*** (0.3255)	-2.6054*** (0.2394)	-1.9768*** (0.6995)	-2.2670*** (0.2017)	-0.5640** (0.2557)	-0.3577 (0.2419)
t+2	-3.0152*** (0.3961)	-1.9965*** (0.6063)	-2.4536*** (0.2654)	-2.5539*** (0.2049)	-1.7638** (0.6524)	-2.2646*** (0.1648)	-0.2627 (0.2654)	-0.0623 (0.2224)
t+3	-4.9633*** (0.5220)	-3.2615*** (1.0612)	-4.0463*** (0.3194)	-2.4673*** (0.2057)	-2.5721** (1.1755)	-2.2158*** (0.1413)	-1.2288** (0.4848)	-0.9278** (0.4028)
Observations	12,173	12,173	12,173	12,173	12,173	12,173	12,173	12,173
R-squared	0.4593	0.4572	0.4598	0.4623	0.4636	0.4599	0.4632	0.4586
Mun. FEs	✓	✓	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓
Reform average effect	-4.0564*** (0.4611)	-2.8579*** (0.8900)	-3.5587*** (0.3522)	-2.5851*** (0.2217)	-2.2583** (0.9100)	-2.3551*** (0.1739)	-0.6132** (0.2536)	-0.4725* (0.2396)
SE (average effect)								

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Refers to security cooperation agreements signed with the Governor. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

## A.4. Unintended consequences

### A. Preferences

TABLE A-14. Effect of 2014 Term Limit Reform on Citizens Preferences

Dependent variable: topic that worries the most											
	Narcotrafic	Insecurity	Punishment to criminals	Corruption	Poverty	Unemployment	Inflation	Natural Disasters	Water Scarcity	Education	Health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
t-6	0.0190** (0.0066)	0.0218** (0.0085)	-0.0088 (0.0121)	0.0140** (0.0060)	-0.0363*** (0.0118)	0.0120 (0.0156)	-0.0150 (0.0103)	-0.0121*** (0.0021)	0.0119* (0.0066)	0.0106*** (0.0013)	-0.0087*** (0.0022)
t-5	0.0073*** (0.0012)	0.0167** (0.0062)	0.0062 (0.0063)	0.0015 (0.0018)	-0.0186*** (0.0062)	0.0143* (0.0074)	-0.0066 (0.0065)	-0.0059** (0.0025)	-0.0038*** (0.0012)	0.0011 (0.0009)	-0.0090** (0.0034)
t-4	-0.0034 (0.0083)	0.0921** (0.0380)	0.0218 (0.0247)	-0.0048 (0.0142)	-0.0447 (0.0274)	0.0106 (0.0233)	-0.0432* (0.0224)	-0.0055 (0.0043)	0.0303** (0.0119)	0.0089 (0.0052)	-0.0492** (0.0189)
t-3	0.0439** (0.0182)	0.0727 (0.0566)	-0.0033 (0.0210)	-0.0143 (0.0202)	-0.0275 (0.0438)	0.0254 (0.0332)	-0.0204 (0.0152)	0.0015 (0.0116)	-0.0156 (0.0238)	0.0071 (0.0172)	-0.0536** (0.0227)
t-2	0.0280 (0.0496)	0.0144 (0.0496)	0.0304 (0.0195)	-0.0195 (0.0216)	-0.0253 (0.0420)	0.0266 (0.0211)	0.0435*** (0.0143)	0.0121 (0.0159)	-0.0003 (0.0171)	-0.0306* (0.0157)	-0.0623* (0.0314)
Reform, t=0	0.0021 (0.0050)	0.0267*** (0.0072)	0.0206*** (0.0037)	0.0012 (0.0044)	-0.0187*** (0.0063)	-0.0355*** (0.0051)	0.0034 (0.0056)	-0.0016 (0.0026)	0.0073 (0.0052)	-0.0091** (0.0039)	0.0017 (0.0053)
t+1	0.0165** (0.0071)	0.0427*** (0.0112)	0.0270*** (0.0037)	0.0126*** (0.0045)	-0.0392*** (0.0097)	-0.0803*** (0.0058)	0.0520*** (0.0075)	0.0093 (0.0074)	0.0017 (0.0053)	-0.0189*** (0.0046)	-0.0329*** (0.0071)
t+2	0.0227** (0.0086)	0.0785*** (0.0108)	0.0400*** (0.0050)	0.0079* (0.0042)	-0.0405*** (0.0108)	-0.1023*** (0.0087)	0.0172* (0.0093)	0.0099* (0.0050)	0.0107* (0.0062)	-0.0283*** (0.0062)	-0.0323*** (0.0058)
t+3	0.0182 (0.0134)	0.0837*** (0.0151)	0.0828*** (0.0098)	-0.0081 (0.0080)	-0.0397** (0.0169)	-0.1094*** (0.0177)	-0.0357*** (0.0064)	0.0048* (0.0025)	0.0228** (0.0087)	-0.0275*** (0.0092)	-0.0152 (0.0109)
Observations	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353
R-squared	0.8662	0.8556	0.9239	0.8767	0.8549	0.8954	0.8557	0.7008	0.8419	0.8048	0.8799
Mun. FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls <sup>b</sup>	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Reform average effect	0.0149*	0.0579***	0.0426***	0.0034	-0.0345***	-0.0819***	0.0092	0.0056	0.0106**	-0.0209***	0.0063
SE (average effect)	(0.0079)	(0.0099)	(0.0046)	(0.0048)	(0.0103)	(0.0085)	(0.0063)	(0.0034)	(0.0047)	(0.0055)	(0.0063)

Notes: Coefficients show IW estimators following ? Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ? except for the specification that trims periods prior to t-4. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>b</sup> State-level controls include governor winning margin in last pre-treatment election and an indicator of whether the governor's party is the same as the federal incumbent party.

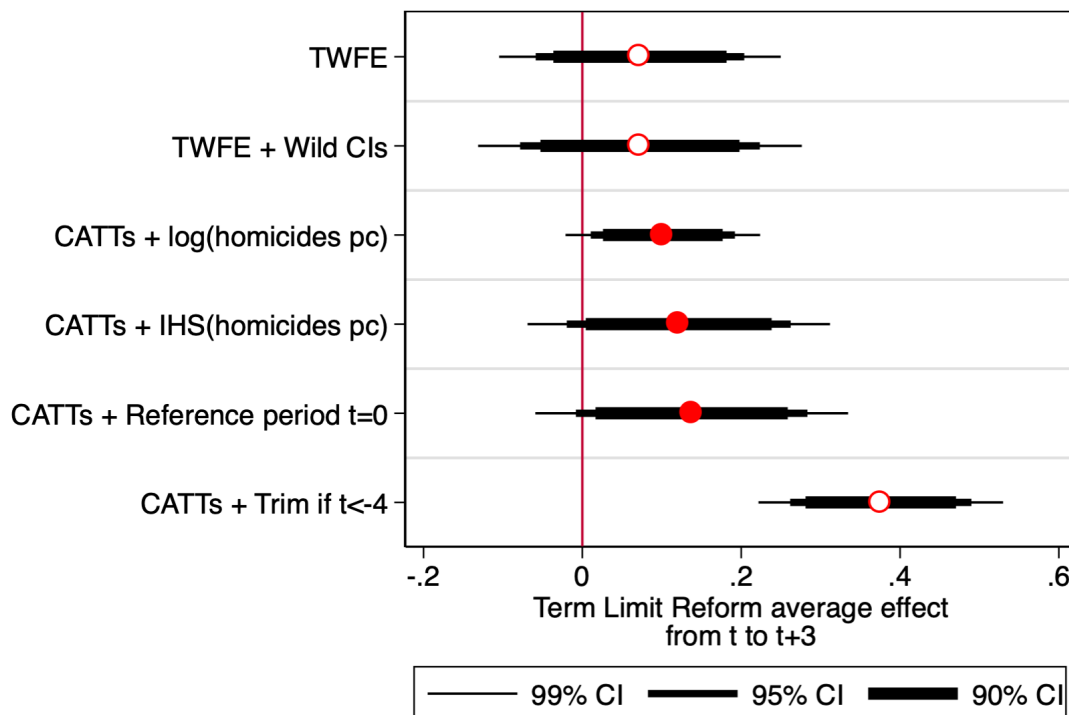
## B. Violence

**TABLE A-15.** Effect of 2014 Term Limit Reform on Violence

Dependent variable:	log(homicide per capita)	IHS(homicide per capita) <sup>a</sup>
	(1)	(2)
Lag 6 years	0.0119 (0.0195)	-0.1702 (0.1061)
Lag 5 years	-0.0480 (0.0357)	0.0381 (0.0856)
Lag 4 years	0.0403 (0.1012)	-0.0440 (0.2077)
Lag 3 years	0.0167 (0.0581)	-0.0015 (0.1098)
Lag 2 years	-0.0288 (0.0498)	-0.1734 (0.1098)
Reform, time 0	0.0024 (0.0324)	0.0067 (0.0583)
Lead 1 year	0.0719* (0.0401)	0.0168 (0.0692)
Lead 2 years	0.1420*** (0.0465)	0.1814** (0.0761)
Lead 3 years	0.1890* (0.0993)	0.2805* (0.1481)
Observations	12,173	12,173
R-squared	0.7267	0.5330
Mun. FEs	✓	✓
Year. FEs	✓	✓
Controls <sup>b</sup>	✓	✓
Cohort weighted	✓	✓
Aggregate effect	0.1013**	0.1213*
SE (aggregate eff.)	0.0442	0.0687
Standardize Aggregate effect	0.1036**	0.0662*
Standardize SE (aggregate eff.)	0.0452	0.0375

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 0) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: \*\*\* 1%; \*\* 5%; and \* 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. <sup>a</sup> Inverse hyperbolic sine transformation. <sup>b</sup> Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; and Cartel presence.

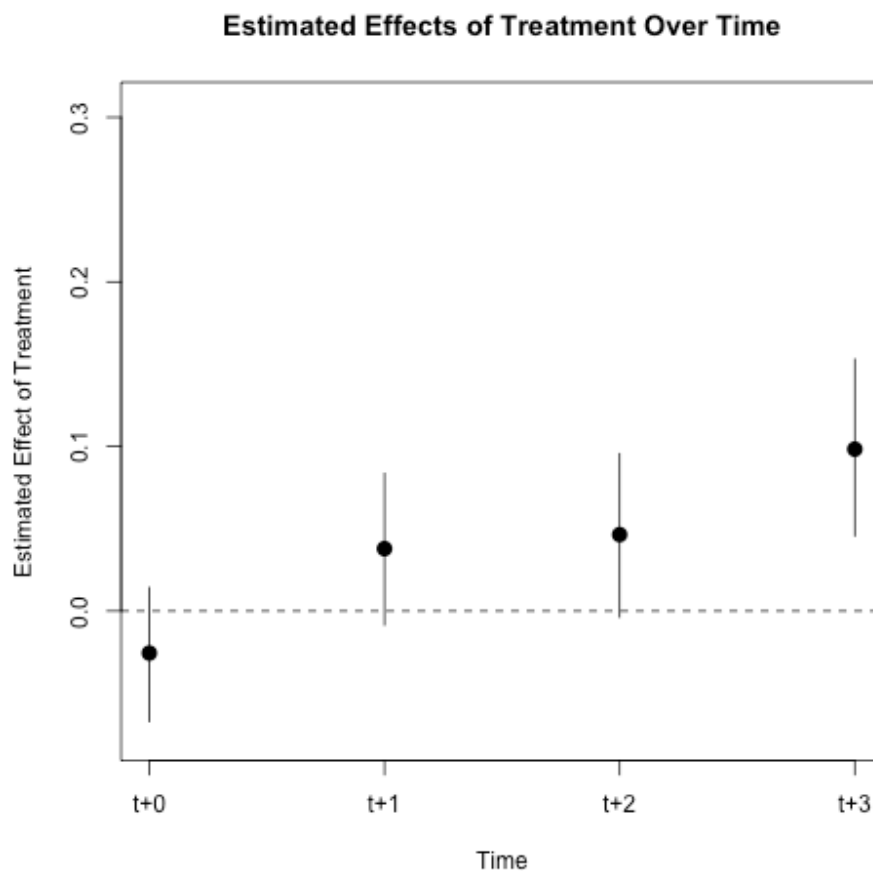
**FIGURE B-6.** Robustness tests: Effect of Term Limit Reform on Violence, 2010-2018



**Note:** Figure ?? shows the average treatment effect from  $t$  to  $t+3$  across multiple specifications. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends.

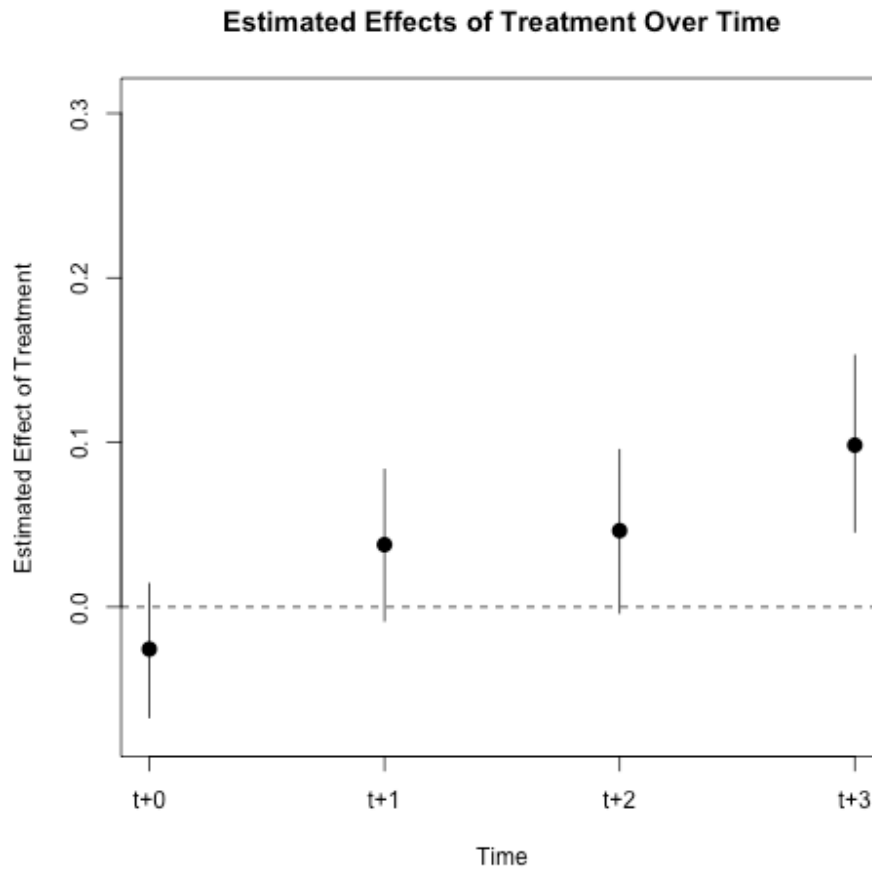


**FIGURE B-7.** Effect of Term Limit Reform on Violence, propensity score matching on pretreatment covariates



**Note:** Figure ?? produced by propensity score matching that adjust for the treatment and covariate histories during the 5 year periods prior to the treatment. I report 95% bootstrap confidence intervals clustered at the state level. Covariates include those used to generate Figure ??.

**FIGURE B-7.** Effect of Term Limit Reform on Violence, propensity score matching on pretreatment covariates



**Note:** Figure B-7 produced by propensity score matching that adjust for the treatment and covariate histories during the 5 year periods prior to the treatment. I report 95% bootstrap confidence intervals clustered at the state level. Covariates include those used to generate Figure 1.