

LOVE YOUR INCUBMENT BUT HATE HIS PARTY: THE ASYMMETRIC EFFECT OF REELECTION ON PARTISAN AND PERSONAL INCUMBENCY RETURNS

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ABSTRACT. A large literature has studied the electoral returns to incumbency. However, the incumbency return in the next election -whether positive or negative- masks an understudied dynamic between parties and their members: they can both be hated, both loved or asymmetrically liked by the constituency. This paper opens up the black box of incumbency by studying the effect of reelection on personal and partisan incumbency returns. To do so, I exploit the staggered implementation of an electoral reform that introduced reelection for local executives from 2014 to 2022 in Mexico, a party-centered system. I find that a seemingly null result hides an incumbency asymmetry: when citizens are able to reelect the incumbent a personal incumbency advantage follows, with parties suffering from an incumbency disadvantage. Contrary to past studies, a difference-in-discontinuity in close elections allows me to overcome methodological downfalls, primarily the difference in experience and ability between term limited and non non-term limited incumbents, as well as rule out the differential trends on electoral returns that localities may have. Results suggest the personal advantage to be driving positive incumbency returns when reelection exists even in party-centered systems.

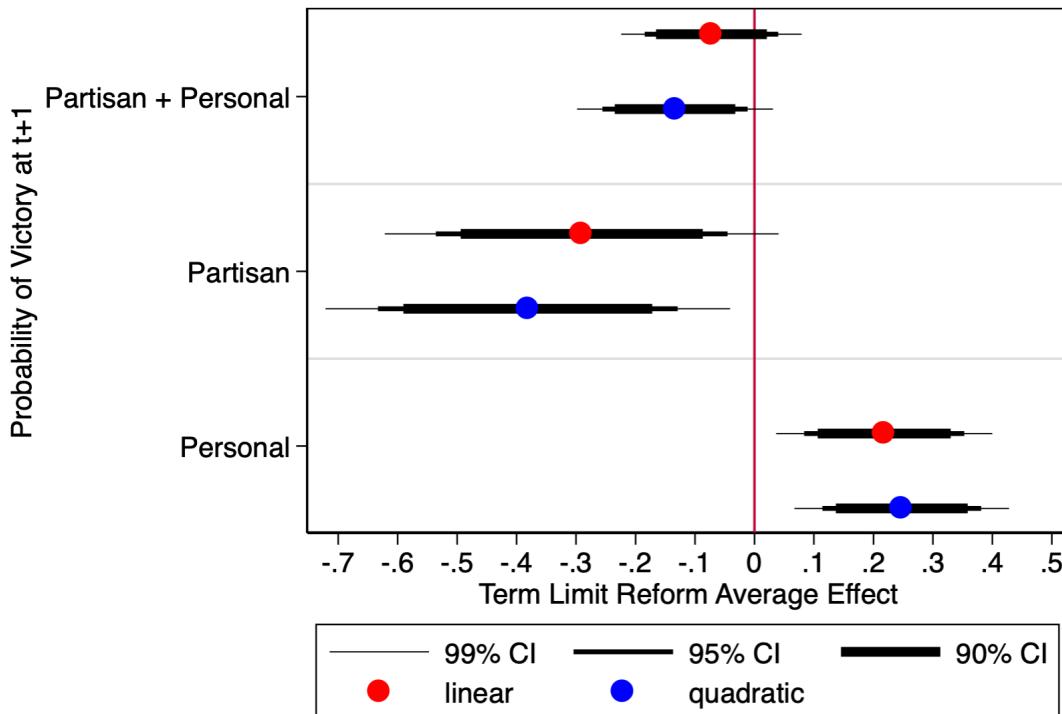
KEY WORDS: REELECTION, INCUMBENCY ADVANTAGE, INCUMBENCY DISADVANTAGE, RESOURCES, POLITICIAN QUALITY, PERFORMANCE IN OFFICE.

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1. Main Results

FIGURE 1. Effect of Term Limit Reform on Partisan and Personal Incumbency Advantage
-difference-in-discontinuity of close elections design-



Note: Figure ?? shows the average treatment effect of the Term Limit Reform on the probability of winning in the following election using a difference in discontinuity of close elections design. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Optimal bandwidths following ? are used. This analysis identifies the party that wins at $t - 1$ and studies the effect of this party barely winning (or losing) at t on outcomes at election $t + 1$ following ?. I follow ? to decompose the incumbency advantage into the partisan and personal component. Red and blue points show that parallel trends hold, while hollow ones imply pretrends.

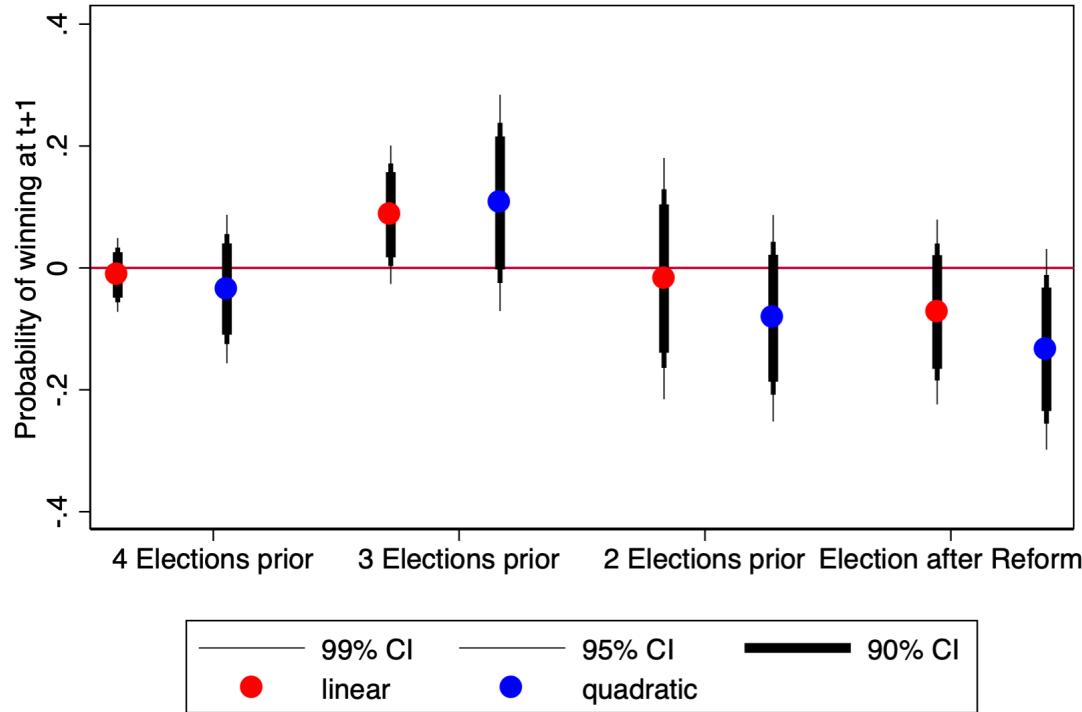
2. Robustness

TABLE 1. Event-in-Discontinuity in close elections model: Effect of 2014 Term Limit Reform on Inc incumbency Advantage

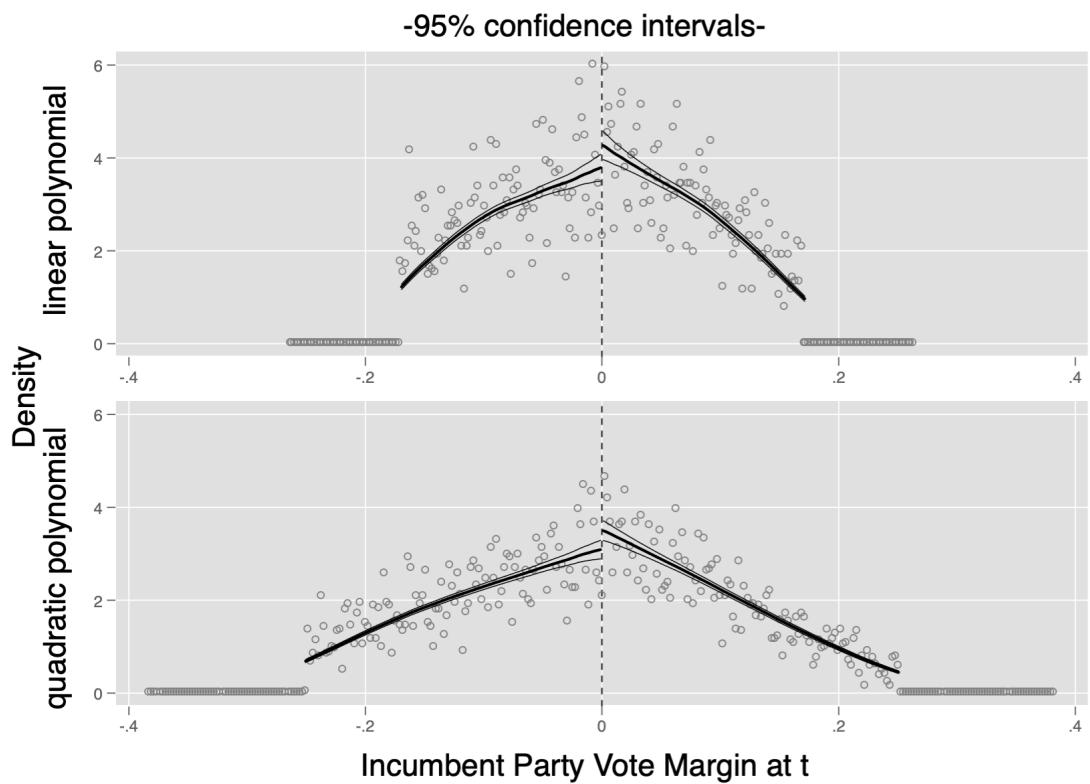
Dependent variable:	Incumbent at t-1 won at t+1		Incumbent at t won at t+1
	(indicator)	(1)	(indicator)
			(2)
linear polynomial			
4 Elections prior	-0.0116 (0.0219)		-0.1827 (0.1789)
3 Elections prior	0.1143** (0.0426)		0.4997 (0.5727)
2 Elections prior	-0.0175 (0.0715)		0.0457 (0.2100)
Election after Reform	-0.0723 (.)		-0.1642 (.)
Observations	2,071		1,270
R-squared	0.5528		0.6282
quadratic polynomial			
4 Elections prior	-0.0347 (0.0440)		0.1453*** (0.0146)
3 Elections prior	0.1393* (0.0769)		-0.3631*** (0.0923)
2 Elections prior	-0.0826 (0.0611)		0.2307** (0.1051)
Election after Reform	-0.1335** (.)		0.1812*** (.)
Observations	2,845		2,784
R-squared	0.5445		0.4373
Mun. FE	✓		✓
Year. FE	✓		✓
Controls ^a	✓		✓
Cohort weighted	✓		✓

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 6 and 1) are removed to avoid collinearity problems noted by ? or because they are collinear or nonexistent, like lag time period 2. Standard errors in parentheses are clustered at the state level for estimates in saturated model. 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. ^a 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. Logged homicides per capita at the municipality level are also included as controls.

FIGURE 2. Effect of Term Limit Reform on Partisan and Personal Incumbency Advantage
-difference-in-discontinuity of close elections-



Note: Figure ?? shows the average treatment effect of the Term Limit Reform on the probability of winning in the following election using a difference in discontinuity of close elections design. This average effect was estimated using the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. Optimal bandwidths following ? are used. This analysis identifies the party that wins at $t - 1$ and studies the effect of this party barely winning (or losing) at t on outcomes at election $t + 1$ following ?.

FIGURE 3. McCrary Test

Note: 95% confidence intervals reported. With 90% levels confidence intervals overlap..

FIGURE 4. No discontinuous jump of covariates

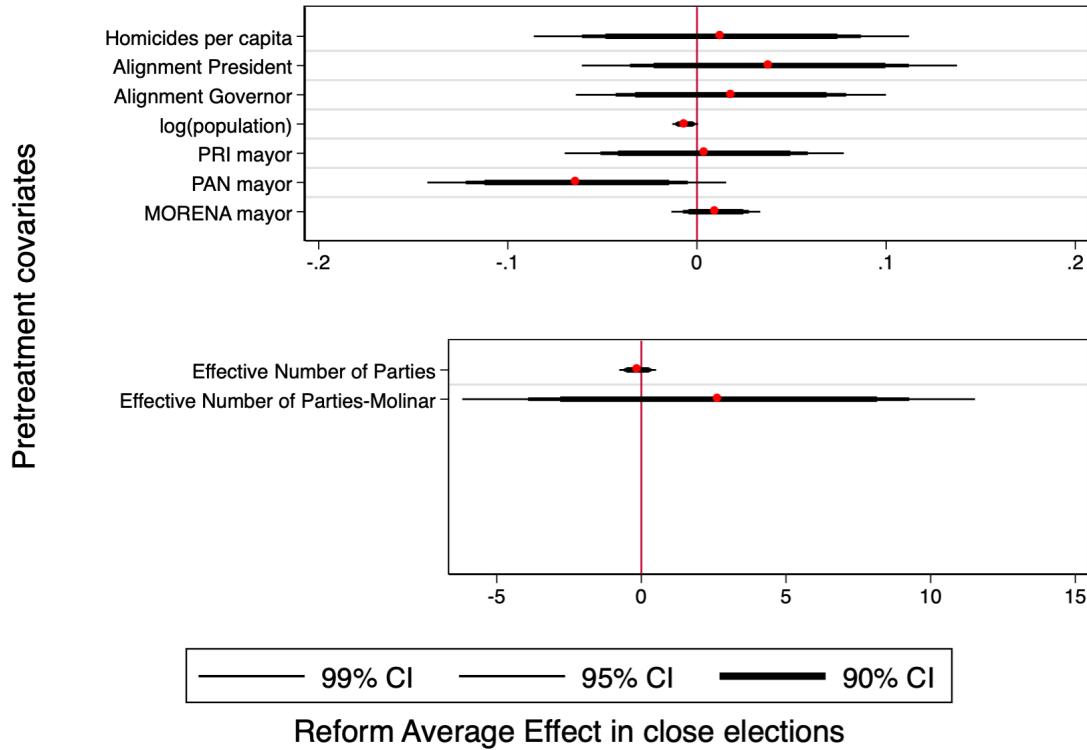
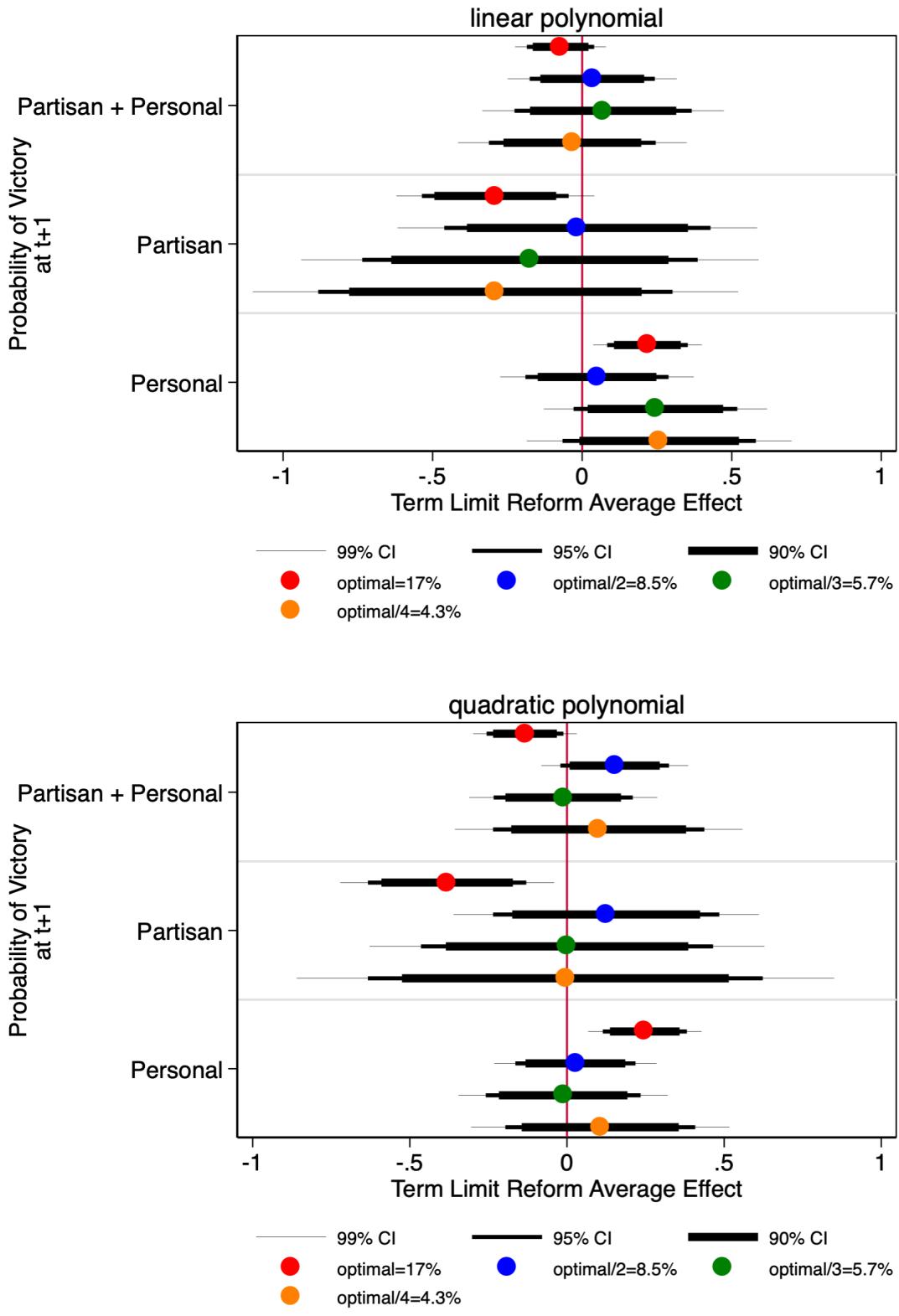


FIGURE 5. Testing Different Bandwidths



3. Mechanisms

4. New Appendix

4.1. Main Results

4.2. Robustness

4.3. Mechanisms

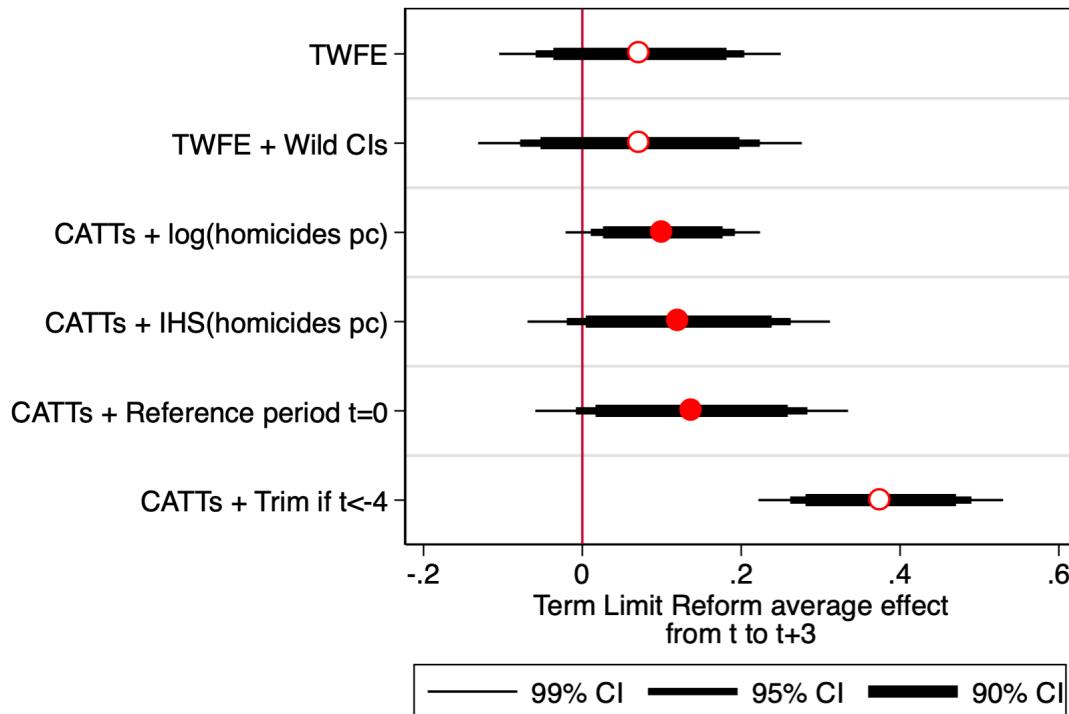
C. Violence

TABLE 2. Effect of 2014 Term Limit Reform on Violence

Dependent variable:	log(homicide per capita)	IHS(homicide per capita) ^a
	(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 ^b	✓	✓
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

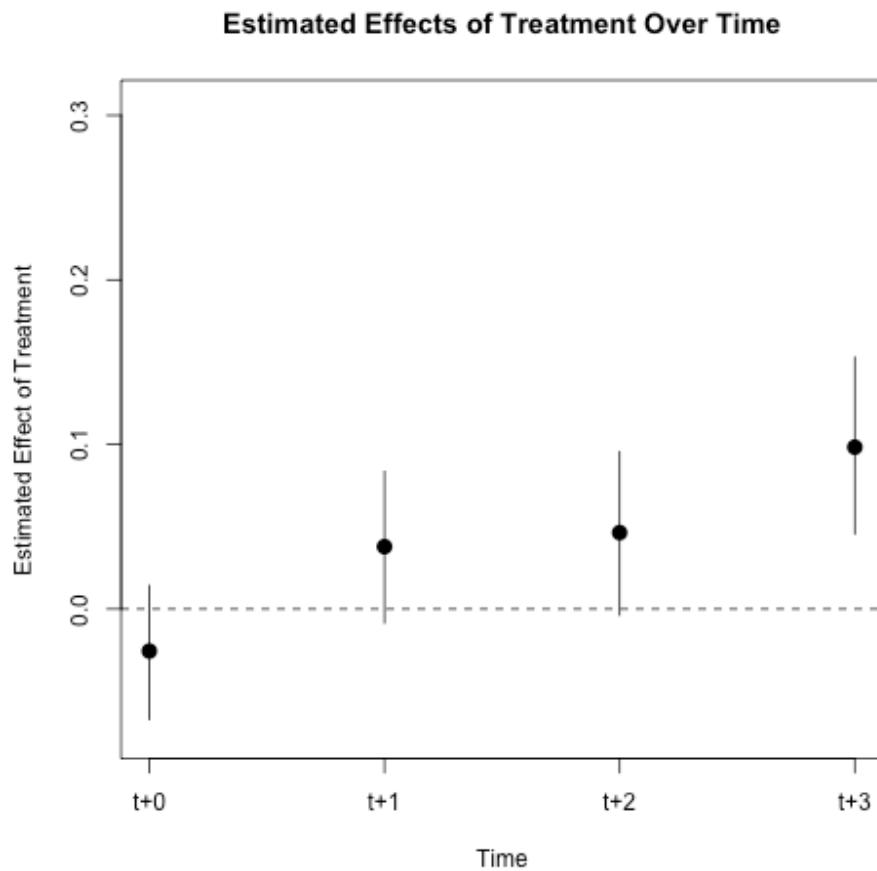
Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 0) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided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

FIGURE 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 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 ??.

5. Introduction

The literature on political accountability states that to ensure politicians' alignment with citizens best interests, the latter can rely on reelection to punish bad behavior in office retrospectively and select good candidates prospectively (?). Evidence from reelection studies point to an increase in the competence of elected politicians (?), reduced corruption (?), increasing legislators productivity (?) and greater welfare (?). However, recent empirical evidence shows that reelection fosters 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 (?). What features limit the political accountability of reelection? The present work fills this gap.

This paper argues that one reason reelection incentives may not lead to political accountability is that politicians do not respond only to voters but also to parties that shape their careers (????). If parties have incentives to cater to clienteles rather than the median voter, so will their party members. However, the study on the interests misalignments between voters and parties has been understudied empirically.

This paper delves into a party-centered system, Mexico. I exploit an electoral reform in 2014 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 level¹ 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 characterize the reform: (1) removal of term limits of mayors, and local and federal legislators for up to 2 terms; (2) introduced a “party-lock” where mayors who wish to reelection 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 that should increase politicians responsiveness to their constituents at the same time that it kept a strong party system where politicians depended on their parties for candidate nomination and campaign expenses. This generated an ideal setting to cleanly identify the tradeoff faced by incumbents on their responsiveness to voters and politicians.

To test this tradeoff, however, we also need to identify a public good that generates a preference misalignment between voters and parties. I focus on the study of public security provision and violence. There are four reasons for this. First, on the voter side, peace (and thus violence) is the most relevant public good demand in the country given the high prevalence of drug-trafficking related crime.² Second, the majority of the population prefers higher rather than lower public good provision. Importantly, this does not differ across the country or across time since 2006 when the War on Drugs began.³ Third, voters in Mexico hold local politicians accountable for organized crime-related violence, but only when the same party controls all relevant levels of government, from Federal to local (?). In other words, voters in this context hold the capacity to assign responsibility for crime

¹Similar to US states.

²Prior 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 for an example.

³Victimization surveys from 2006 till 2018 show that this to be true, as well as no spatial heterogeneity or across time in this regard.

for local governments. Fourth, on the party side tackling drug trafficking organizations (DTOs) has not been a free lunch: DTOs have killed mayors in high rates, specially those belonging to the centrist PRI (?). Because of this, the PRI's incentive is to go back to the status quo of a drug market with rents and no conflict between the state and organized groups. As such, this forbearance strategy differs from that of the former party in power, the right-wing PAN, who developed a hawkish strategy against crime from 2006 to 2012 (?). Given this features I focus on the period of study from 2010 to 2018, with the post-treatment period from 2015 onwards being ruled at the Federal level by one party, the PRI. Lastly, Mexico's criminal wars since 2006 have resulted in wide variation of violence across space and time.⁴

Through a event-study research design that leverages the staggered implementation of the reform and state-level variation, I show that public good provision measured through the eradication of narcotics and dismantling of drug-related laboratories was much *lower* in municipalities that experienced a term limit removal relative to those that did not; as a result, the former experience a surge in violence proxied by homicide related deaths. The reason: a vacuum of power that DTOs took advantage off. Results are not explained by pre-trends in violence (or pretend effort placed by the government) or anticipatory behavior of government officials prior to treatment. While the effect persists across time, cohort weighted estimates remove concerns on bias from heterogeneous treatment effects. Results are robust to a variety of specifications, different homicide databases and measures, falsification and robustness tests.

What explains these paradoxical results? I exploit close elections in conjunction with the staggered removal of term limits to test if the electoral reform affected the electoral position of political parties and their candidates.⁵ I find the reform generated an incumbency advantage. Moreover, I find that this incumbency advantage is primarily a partisan incumbency advantage and not a personal one. This points to the fact that as parties grew stronger electorally, they preferred a forbearance strategy on crime and no conflict between the state and organized criminal groups. Only when the parties are weak (and by consequence their members) do they respond to voters preferences and public good demands.

I draw three novel insights from these findings. First, while the literature has stressed multiple benefits of term-limit removal (??), once we factor in the incentives of parties,

⁴For more on scope conditions, please see Appendix ??.

⁵Empirically, when focusing in close elections it is implausible that the incumbent probability of winning is driven by idiosyncratic factors. Moreover, by considering an underlying difference-in-difference setup, neither pre-trends in the incumbent probability of winning neither sorting into municipalities where the incumbent had higher chances of reelection can explain this finding. The study shows that the incumbency advantage result is uncorrelated with municipal characteristics or explained by variations in the functional form of the estimated equation.

it may yield undesirable public good results if they hold dissimilar preferences to that of voters. This conclusion brings into question whether voter information and term-limit removal is sufficient to restore full accountability (as found in ? and ?, for example).⁶ Second, in the case of party-centered systems even the introduction of reelection incentives may not generate a candidate-centered contests. This questions whether reelection may generate a partisan dealignment, as the one seen in the electorate of the U.S (?).

Lastly, this paper makes important contributions to the existent literature on the War on Drugs in Mexico. Under the *inc incumbency disadvantage* period prior to 2014 detected by ?, party alignment between the central and local governments implied a coordinated policy to tackle drug violence and thus generated a surge of violence as noted by ?. However, under the *inc incumbency advantage* period after the Electoral Reform of 2014, party alignment made actors shirk on public security provision. As such, the paper aligns more to findings from ? that found that coordination across municipalities can reduce drug violence albeit through the opposite channel, forbearance. The study also compliments additional evidence that pointed to the Mexican government policy as the actor behind large spurges of violence in the last decade (??).

The next section provides a brief theoretical discussion on reelection incentives, accountability to multiple agents and public security provision. I provide then 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 followed by some policy closing thoughts.

6. Argument: an incumbent's divided heart

Mayhew's ? "electoral connection" states that incumbent's action is constrained by his reelection desire and the policies voters are interested in and will use to choose a candidate in the next election. Importantly, if voters have the capacity to blame incumbents for certain welfare outcome, say violence, reelection incentives would lead to incumbent's responsiveness on the security demands made by constituents. This leads to the following hypothesis:

H1: Reelection of incumbents will lead to an increase in responsive to voters demands when voters have the capacity to assign responsibility of public good provision to incumbents .

⁶Results align more with ? who find a negative welfare impact of reelection, albeit a different mechanism. They find that longer time in office allows incumbents to detect and favor local bidders and thus decrease welfare due to inefficient procurement. Contrastingly, I also find a decrease in welfare through both public security underprovision and an increase in violence, but due to an increase in the party political survival.

In the case of Mexico, research by (?) has showed that constituents have the capacity to blame local governments for crime and underprovision of public security, especially when there is a partisan alignment from the local government with the state and federal executives.⁷

However, under reelection incumbents are accountable not only to voters but the parties that define their electoral careers (????). Both principals, voters and parties, can hold preference misalignments. In such case, an incumbent's behavior will depend highly on two features: who wins his heart and mind, and the incentives and preferences of the winner.

Even if reelection incentives introduce a pressure of partisan dealignment in the electorate, parties may still exercise a strong say in their members in party-centered systems. If the party has a strong political machine, candidates have a competitive edge if they signal party loyalty by following the party line. As a result, party cohesion could follow.⁸ In such cases, incumbents efforts depend highly on party incentives.

What determines party incentives? I posit that party willingness to monitor its members, focus on public good provision (and so their members), and be accountable to voters depends heavily on their electoral success. Whenever we observe an incumbency advantage this would lead parties and their members to insulate themselves from electoral risk, thus weakening the accountability to their constituents (?). This insulation would be more salient if voters' demands are highly costly for parties, such as the provision of public security due to high likelihood of party candidates of suffering from violence.⁹ This leads to

⁷Reelection, further may introduced other changes to electoral systems. With reelection, incumbents cultivate a personal vote to increase their reelection probability. As noted by ?, to maximize the probability of reelection incumbents may cater to clienteles instead of the median voter through particularistic transfers that they can credit-clame. Moreover, legislators win by differentiating themselves from their parties. This behavior has been more salient in candidate-centered systems rather than party-centered ones (??). Moreover, the removal of term limit has showed to change party-centered systems to candidate centered ones. In the U.S. partisan dealignment has been widely studied with studies showing the electorate “weighed party affiliation less and information about the personal characteristics of candidates, including the incumbency status, more in making their voting decisions (?, p. 481).” Importantly, as noted by ? and ? for the case of Mexico, incumbents in matters of security provision and violence do not move to cultivate a personal vote due to the high risk of being targeted by DTOs and because constituents can clearly blame local governments for their behavior deterring crime.

⁸Party members alignment with their party may also be present when their party has a strong clientelistic advantage of providing particularistic goods and services. In such cases, we should expect an alignment with the party's particularistic behavior (?).

⁹What determines an incumbency advantage? To date, we can identify at least three types of explanations. First, what incumbents do (and opponents cannot do) emphasizing the resources, visibility, and power that incumbents gain from office holding (????). Second, quality-based explanations that emphasize who incumbents are (and who their opponents are) (???) [for a discussion of these two types of mechanisms, please see ?]. In this second line we find explanations related to incumbents' quality as well as the “scare-off effect”, or the ability incumbents have to scare off high-quality challengers. A third and more recent type of mechanism emphasizes the role of information. (?) find that incumbency advantage is dependent on how

the following hypothesis:

H2: An increase in incumbency advantage decreases a party (and its members) willingness to be responsive to voters demands when preferences are misaligned.

The objective of this paper is to test whether H1 or H2 hold when introducing reelection. In other words, we test whether reelection generated an incumbency advantage and if so, if this had an effect on incumbents and their parties responsiveness to voters in matter of public security provision and the levels of violence in the country. The next section provides a description of the War on Drugs in Mexico while the section after presents the changes introduced by the term limit removal reform of 2014. Data and research design are introduced afterwards, as well the results on violence followed by the results on the mechanism on incumbency advantage.

7. Mexico's War on Drugs

Since 2006, Mexico exhibited an increase in violent crimes reaching a historical 103 homicides per 100,000 inhabitants in June of 2019, the most violent rate of post-revolutionary Mexico according to the data from the Executive Secretary of the National Public Security System (SNSP for its acronym in Spanish). As such, the levels of violence reached more than 100,000 homicides from 2006 to 2013, and while not concrete explanation exists of why we saw a dramatic increase in the levels of homicides, multiple have been the explored mechanisms including (a) DTOs competition to control markets and drug distributions channels to the United States (??), (b) state efforts to reduce DTOs operations (?) to increase government legitimacy from the Felipe Calderon Administration who in 2006 won by a winning margin of 0.02% in a highly post-election contested time period (?), and cocaine supply shortages (?). As a result, since 2007 and up to the COVID-19 pandemic public security provision has been the main public problem in the country.¹⁰

precise the information is about incumbent's ability to voters. More recently, (?) expand on the role of information to explain incumbency advantage: through a theoretical model, they show that incumbents have an additional information advantage to challengers: they govern while challengers do not. This is so even absent any partisanship, electoral selection or challenger scare off. However, to date there is no empirical identification of the information-based explanation proposed by these authors. Moreover, if theoretical work done by Ashworth and co-authors is correct, all other explanations of incumbency advantage might be biased by the role information plays. This paper I will not delve into explaining the reason behind the incumbency advantage found in the results. In another paper of mine I show the incumbency advantage is determined by the information of incumbents' governance in line with ?. Moreover, preliminary findings show that in Mexico mayors, local and federal legislators hold a positive and significant party incumbency advantage. However, the personal incumbency advantage is indistinguishable from zero for mayors and negative, and negative and significant for legislators. I find no evidence of quality-based incumbency advantage.

¹⁰For an example, see this survey by El Financiero https://www.dropbox.com/s/c5dte5pscggat2c/leadingproblem_mexico.png?dl=0.

Multiple pacification and conflict deterrent strategies have been tested, from aggressive campaigns to weaken drug-trafficking organizations (DTOs) -including the beheading of drug kingpins and the deployment of more than 45,000 troops across the country-, to the defense (and financing) of self-defense organizations in the Mexican Bajío (Caballero 2015). However, so far public security strategies have yield mixed if not negative results. DTOâs leadership removal increased inter and intra-cartel fighting, fragmenting criminal organizations with violent spillovers on the overall population (?). Troops deployment seemed to play a significant role in the escalation of violence in Mexico (?) and have been linked to human rights violations and more than 30,000 disappearances (???). In fact, municipalities that were more effective in coordinating public security policies, such as those controlled by the PAN under the Felipe Calderon administration, showed a dramatic increase in violence due to state crackdowns on drug cartels (?).

On the crime prevention side, the state has allocated multiple local-level transfers and allowed municipalities to choose their prevention strategies. Impact evaluation of such transfers are nonexistent, to my knowledge, and have not systematically achieved the desired purposes. Dispute resolution institutions have not been systematically rolled out or evaluated rigorously on the ground. While top-down policies have been widespread -and ineffective overall-, grassroot approaches have been scarce and primarily aimed at identifying mass graves and victims and lead protests against impunity. Possibly the most effective anti-crime territorial recovery units have been self-defense armed groups (?). However, while DTOs have been expelled from towns by self-defense groups, so have local police forces, local-level public officials and politicians. In other words, the (so far) most effective bottom-up territorial recovery approach led to distrust in government and the breakup of both pre-existing institutions and social ties, creating stateless regions across Mexico.

Important to this paper is evidence that the political use and influence of the military (???) and federal and local police forces in Mexico (????) reduced their efficiency and capacity. While there have been improvements in the Mexican justice system, particularly the 2008 Reform that introduced the accusatory system, almost no investments on capacity building have been made in local police apparatus. Plan Merida did increased military investment substantially, and while investments where made to federal and local police forces, there are still precarious work conditions and salaries, low training and no institutionalized professionalization of police forces.

8. Term Limit Reform of 2014

In February 2014, the Mexican Federal Congress approved the Electoral Reform that allowed federal and state legislators and municipal mayors to reelection. As such, it lifted a 80-year old ban on reelection from a constitutional amendment in 1933 imposed by the

Partido Nacional Revolucionario (PNR and former PRI) to control self-motivated politicians to deviate from the party line in any of the multi-level party structure.¹¹ The PNR non-reelection strategy weakened local party bosses and allowed the party to control political careers at the federal, state and local level (?).

For federal legislators, consecutive reelection was allowed up to 4 four terms. In the case of state-level legislators the reform introduced consecutive reelection for up to four terms with a maximum of 12 years; for the case of mayors, reelection was allowed for up to 2 consecutive periods at most. State legislatures, mostly under control of governors, were granted discretion to define the number of terms for both legislators and mayors. While variation in the number of terms exists at the state-legislator level (?), all state legislators approved up to 2 consecutive reelection terms for mayors except for the case of Hidalgo, Nayarit, Tlaxcala and Veracruz that allowed candidates' reelection, but not consecutively, bypassing the reform.

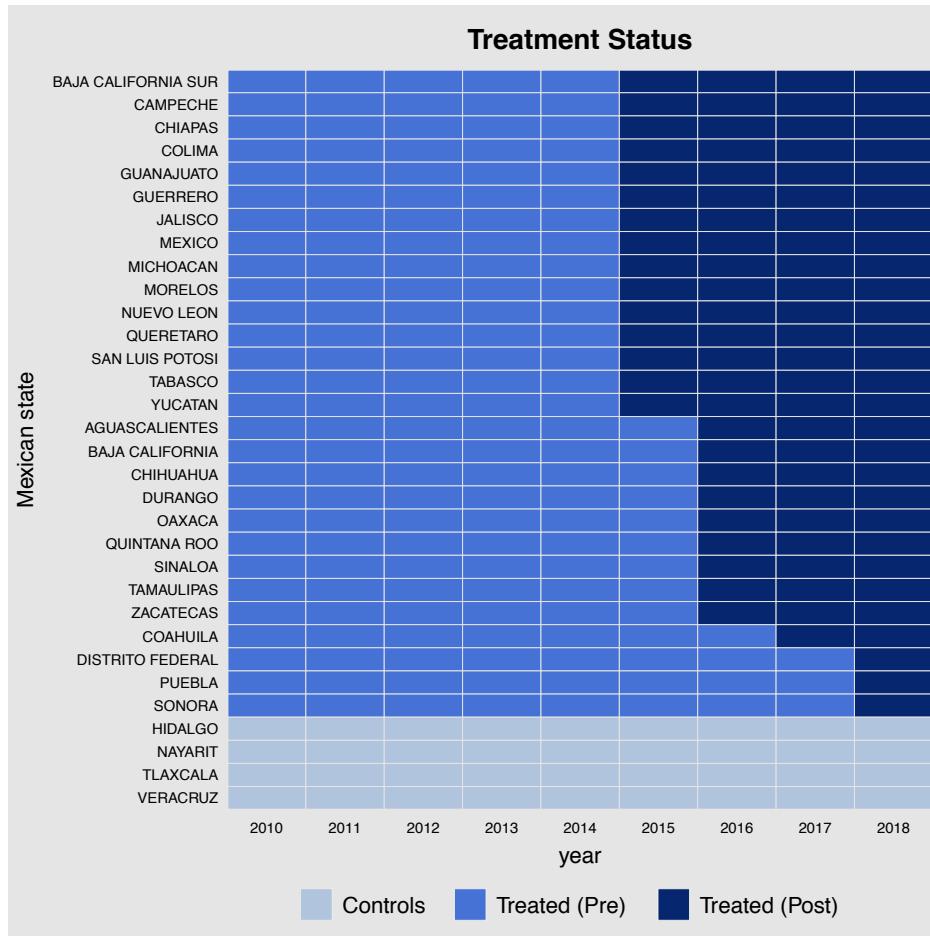
A second source of discretion granted to state-level legislatures revolved around the reelection implementation date. The reform dictated that any change would not affect 2014 elections, and would be implemented for federal legislators till the elections of 2018. For local legislators and mayors, however, state-legislators defined the implementation period. Given governors influence in candidate selection of legislators (and mayors in some cases), their staggered calendar and political power seems to explain most of the variation in the timing of the implementation of the reform: governors with terms ending near the Reform approval date (2014) introduced reelection as early as possible, while those whose terms where starting pushed reelection further down the line (?). For more detail on the political background please see Appendix ??.

Figure ?? describes the implementation period or treatment status of each of Mexico's 32 states.¹² This figures allows to visualize the staggered rollout of the term limit removal. Importantly, we have five timing groups, i.e. five comparison groups. Four states never receive treatment during this time period (Hidalgo, Nayarit, Tlaxcala and Veracruz), while the rest commence treatment in different years from 2015 to 2019. The always-treated group is composed by the states of Baja California Sur, Campeche, Chiapas, Colima, Guanajuato, Guerrero, Jalisco, Mexico, Michoacan, Morelos, Nuevo Leon, Queretaro, San Luis Potosi, Tabasco and Yucatan.

¹¹Anti-reelection sentiments became part of the Mexican political ideology since Porfirio Diaz's coup against president Lerdo de Tejada second term in office in 1876 (the so called Tuxtepec Revolution). The lemma "effective suffrage, no reelection" was used by Diaz against Lerdo, and was later on utilized by Francisco I. Madero against Diaz dictatorship. Since it became one of the most prominent ideologies lasting all throughout the PRI hegemonic party system, and used almost in every official document in the country.

¹²Mexican states share the same administrative level as US states.

FIGURE 8. Mexican States Electoral Reform Treatment Status



9. Data

To test the effect of the 2014 Term Limit Reform on public security provision and violence, I build a database on violence and effort placed by federal and municipal-level security forces for all municipalities in Mexico from 2010 to 2018.¹³ The main outcome of interest is violence proxied by homicide related deaths collected by the National Institute of Statistics and Geography (INEGI for its acronym in Spanish). INEGI reports two homicide data. One the one hand, homicide related deaths that come from the total death certificates marked as “presumed homicides”. On the other hand, homicide statistics made up of the number of previous investigations initiated for the crime of homicide, by year and state. As noted by ? these datasets do not coincide for a simple reason: the former counts bodies, the latter counts cases. I choose homicides based on death certificates since

¹³Mexico has 2,467 municipalities. Thus, the total possible number of observation is 22,203 municipality-years (2,467 municipalities X 9 years).

those based on initiated investigations tend to miscalculate the total number of deaths.¹⁴ I measure homicides per capita to rule out municipality population differences. Population estimates come from the 2010 Census, the 2015 Population Count (both by INEGI), and CONAPO's population projections at the municipal level. Homicides per capita are highly skewed, with a long right tail of municipalities with substantially greater homicides than others. I therefore run the estimation on the impact of the reform on homicides per capita on logged values. For robustness, following ?, I use the inverse hyperbolic sine (IHS) to transform the main outcome. For robustness, I run estimates based on a second homicide database build by the SNSP.¹⁵

To approximate the level of effort placed by the military I build a novel database on narcotics, arms and laboratories eradicated by the military -army and marines- from 2006 to 2019 at the municipal level based on information petitions through the Mexican portal INFOMEX.¹⁶ The dataset includes narcotic eradication (kg) of marijuana, heroine, methamphetamine, and cocaine, as well as marijuana and poppy kg per hectare erradicated, eradicated laboratories, and secured cartridges, vehicles, planes, short and long weapons as well as the detection of clandestine airstrips. Another information petition to INFOMEX was made for municipalities to report the number of criminal detentions made month by month to proxy local level police effort. Detentions were aggregated at the yearly and municipal level. As with homicides, detentions and narcotic eradication and laboratories detected are highly skewed. I transform detentions to per capita logged values (also IHS), and narcotic and laboratories using logged (IHS) values as well.

I follow ? to construct a proxy for party-level incumbency advantage. I construct two measures to proxy party incumbency advantage. First, an indicator that takes the value of 1 if the party won office in the election at $t + 1$ and was an incumbent on $t - 1$, 0 otherwise. This analysis identifies the party that wins at $t - 1$ and studies the effect of this party barely winning (or losing) at t on outcomes at election $t + 1$ following ?. This measure requires at least three rounds of elections, and thus I consider all municipal level elections since 2010 to 2018.¹⁷ A second measure is an indicator that takes the value of 1 if the party won office in the election at $t + 1$ and was an incumbent on t , 0 otherwise. Different from the Brazilian case discussed by ? where parties do not contest in every election and thus need

¹⁴For instance, borrowing a practical example by ?, consider a mass grave with charred human remains. A preliminary investigation will count the number of deaths, and this will become part of the homicide related deaths figure. However, the investigation will only file one case, regardless of the number of victims found. I thus prefer to count victims rather than cases.

¹⁵For more detail on the SNSP homicide database please see Appendix ??.

¹⁶Information petitions number 0000700274419 and 0000700274519.

¹⁷Municipal electoral calendars vary by state. However, almost all municipalities have three year terms with the exception of some municipalities with non-aligned electoral calendars with State-level ones, or other political circumstances.

to adjust estimates unconditional on parties running,¹⁸ in Mexico the three main parties in this time period (PRI, PAN and PRD) always run in elections they have already participated in the past. Thus, there is no bias to be concerned of related to a party's decision to run at $t + 1$ given anticipation of their performance at that same time period.

Lastly, electoral data for recent Mexican elections at the governor and municipal level as well as data on the rollout of the electoral reform at the municipal level comes from ? and ?. The data compiled from multiple sources including state electoral institutes, Banamex Electoral Database, Voz y Voto, etc. contains voting counts per party as well as candidate lists at the governor level. I construct winning margins at the municipal and state level as the difference between the first and second runner. To measure the number of political parties by municipality, I use the Laakso-Taagepera effective number of parties index.¹⁹ Indicators to measure party-alignment between the Federal executive, state and municipal governors are also build. Appendix Table ?? presents descriptive statistics of the variables used in the paper.

10. Research Design: Cohort weighted even-study design

Recent literature has shown that in the presence of staggered treatment timing and heterogeneous treatment effects across cohorts, the coefficient from two-way fixed effects models are not causally interpretable (????). Furthermore, for event-study designs ? find that even under strong parallel trends assumption, pre-reform coefficients may not be non-zero and the post-reform coefficients may not correspond with a convex weighted averages of cohort-specific treatment effects at particular lags and leads. In other words, coefficients of a given lag or lead can be biased by the effects from other time periods, and pretrends may be driven by treatment effect heterogeneity.

To account for potential cohort treatment heterogeneity, I estimate a cohort weighted event-study design following ? that exploits the staggered implementation of the reform and state-level variation. I saturate the time and unit fixed effects structure so that treated units do not enter the test window as control units. Specifically, I replace the binary indicator variable for the start of the treatment reform with a series of lead/lag indicators γ_k for being “ k ” years away from treatment. I focus on the window from 8 years prior to treatment to three years afterwards i.e. for $k \in \{-8, 3\}$ which correspond to the time period of 2010 to 2018, with 2015 the first year of Term Limit Reform implementation.²⁰ I exclude

¹⁸? called this outcome “unconditional victor on running” and measures if a party won at $t + 1$ regardless of whether they had a candidate at that time period or not.

¹⁹? computes the effective number of parties as the inverse of a Herfindhal-Hirschman Index using party vote shares.

²⁰See Figure ?? where those treated in 2018 have 8 lags prior to treatment, while those treated in 2015 have 3 leads, the full window of analysis for $k \in \{-8, 3\}$.

the indicator on γ_{-1} to avoid collinearity and for comparison: estimated coefficients are interpreted as the difference relative to $t - 1$, i.e. one year prior to the implementation of the electoral reform. Following ?, I also exclude $k = -8$ due to collinearity. The estimated equation is as follows:

(10.1)

$$y_{mt} = \mu_m + \mu_t + \sum_{e=1}^5 \sum_{k=-7, \neq -8, -1}^3 \gamma_{e,k} (1\{E_i = e\} \cdot R_{m,t}^k) + \sum_{e=1}^5 \sum_{k=-7, \neq -8, -1}^3 \Theta' X_{s(m)t} (1\{E_i = e\} \cdot R_{m,t}^k) + \epsilon_{mt}$$

where E_i are cohort-specific indicators if a Mexican state removed term limits in a given year.²¹ $R_{m,t}^k \in \{0, 1\}$ is the Term Limit Reform treatment status indicator at period k relative to treatment, for municipality m at calendar time t .²² $X_{s(m)t}$ is a matrix of state s (municipal m) level covariates interacted with the set of cohort-specific fixed effects including pre-treatment governor elections winning margin and governor partisanship with central government. The year indicators by treatment cohort $\gamma_{e,k}$ are the difference-in-difference (DiD) estimators for the Cohort Average Treatment Effects (CATTs). Conditional on municipal and period fixed effects, as well state-level covariates, these CATTs represent the annual difference in mean logged homicides per capita between municipalities that removed term-limits relative to those that did not, k years before and after treatment. Standard errors are clustered at the state-level as that is the treatment level of the Term Limit Reform.

I take the linear combination of the CATTs for each relative time period k , weighting each cohort by its relative share of the sample, to construct the interaction weighted (IW) estimator of ?:

$$(10.2) \quad \hat{\nu}_g = \frac{1}{|g|} \sum_{k \in g} \sum_e \hat{\gamma}_{e,k} \hat{Pr}\{E_i = e | E_i \in [-k, T - k]\}$$

where $\hat{\gamma}_{e,k}$ is returned from equation ?? and $\hat{Pr}\{E_i = e | E_i \in [-k, T - k]\}$ are the estimated weights equal to the share of each cohort in the relevant time period, normalized by the size of g , with g the universe of the k periods relative to treatment. Since I estimate a IW estimator per year $|g| = 1$.

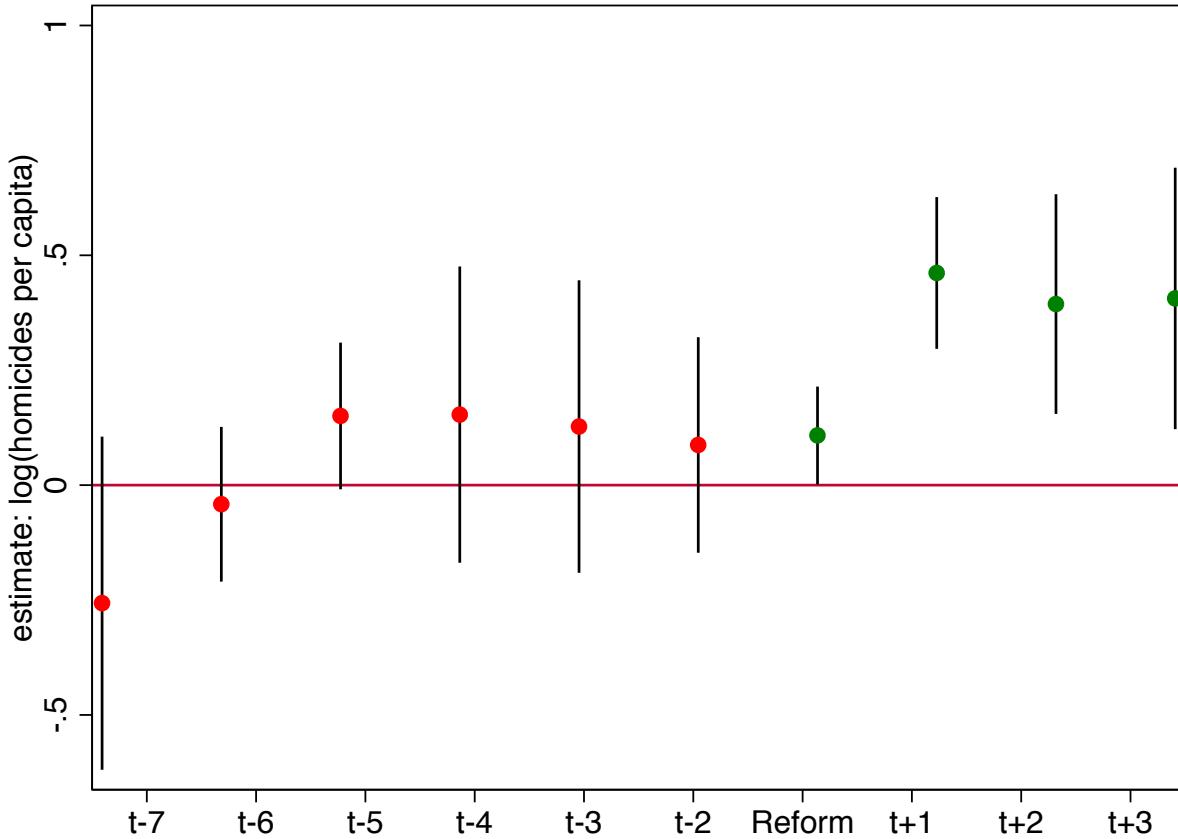
TABLE 3. Effect of 2014 Term Limit Reform on Violence

Dependent variable:	log(homicide per capita) (1)	ihs(homicide per capita) ^a (2)
Lag 7 years	-0.2569 (0.1766)	-0.3129 (0.2584)
Lag 6 years	-0.0416 (0.0820)	-0.0535 (0.1108)
Lag 5 years	0.1505* (0.0777)	0.2019* (0.1011)
Lag 4 years	0.1534 (0.1571)	0.2315 (0.1910)
Lag 3 years	0.1274 (0.1551)	0.2044 (0.1883)
Lag 2 years	0.0873 (0.1143)	0.1416 (0.1883)
Reform, time 0	0.1080** (0.0518)	0.1512** (0.0610)
Lead 1 year	0.4616*** (0.0804)	0.6111*** (0.0994)
Lead 2 years	0.3939*** (0.1165)	0.5372*** (0.1485)
Lead 3 years	0.4061*** (0.1386)	0.5564*** (0.1740)
Observations	8,592	8,592
R-squared	0.7776	0.7025
Mun. FEs	✓	✓
Year. FEs	✓	✓
State Controls ^b	✓	✓
Cohort weighted	✓	✓
Lag DV	✓	✓

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. ^a Refers to the inverse hyperbolic sine transformation.

^b 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 9. Effect of Term Limit Reform of 2014 on Violence
-log transformation, 95% confidence intervals-



Note: Figure ?? shows the IW estimators following ? for each lead and lag relative to the first year a municipality implemented reelection. These estimates correspond to those of Table ?? column (1). Red points show pre-treatment estimates, while green ones are post-treatment.

10.1. Main Results

Table ?? reports the IW estimator²³ for each lead and lag relative to the first year a municipality implemented reelection.²⁴ The first column reports logged values while the second shows IHS transformations. All specifications control for pre-treatment state characteristics interacted with cohort-specific fixed effects, as well as municipal and year fixed

²¹As noted in Figure ??, there are five treatment cohorts including the never treated. The never-treated cohort is made up of the municipalities in the states of Hidalgo, Nayarit, Tlaxcala and Veracruz.

²² $t = e + k$.

²³From here on all tables report IW estimators.

²⁴Appendix Table ?? compares cohort weighted vs non weighted IW estimators. There are no pretrends when we do not adjust for cohort weights. Non-weighted estimates should be interpreted with caution as noted by ?.

effects. Dynamic time trends disappear when we account for violence trends when we include the lag of the outcome (see Appendix Table ?? column 6).²⁵

Column (1) shows that the reform increased homicides per capita in 10.8% the first year of implementation, with an increase to 46.16% one year after implementation (for a visual representation see Figure ??). This effect remains relatively constant two and three years after implementation. Not only are results robust to the IHS transformation but higher than logged specifications as noted in column (2). Importantly, since we control for the lag of (logged or IHS) homicides per capita, we rule out effect is driven by violence time trends.

10.2. Identification

Once we consider cohort weights, I find strong evidence of parallel trends using the logged and IHS transformation as noted in Appendix Table ?? columns (2) and (4), respectively. As noted in columns (1) and (3) if cohort weights are not considered pretrends are found. These estimates, however, are considered biased since leads and lags are contaminated by the effects from other periods (?).

Besides pretrends, identification in this setting implies that the staggered roll-out of the Term Limit Reform is orthogonal to municipal-specific characteristics, conditional on municipal and year fixed effects. This implies *as-if* random assignment to the treatment status visualized in Figure ???. If say, strong governors delay or adjust the implementation of the reform according to their political agenda or calendar, then identification would fail. To address this problem equation ?? interacts the state covariates with cohort-specific fixed effects to adjust for any changes correlated to the evolution of governors strength, the term $X_{s(m)t}(1\{E_i = e\} \cdot R_{m,t}^k)$. Covariates include governor winning margin and an indicator of governor party alignment with the Federal Executive, since pressure from former President Enrique Pena Nieto modified state legislators approval of the electoral reform, particularly for PRI states (for more detail see Section ??). Identification assumption implies now that conditional on municipal and year fixed effects, and cohort specific linear trends in state-level covariates, unobserved factors are not correlated with the electoral reform treatment assignment over time.

An additional identification assumption in event-study designs speaks to no anticipatory behavior from municipalities (or states) to the implementation of reelection. If states have private knowledge of the future treatment path this may change their behavior in

²⁵Including a lag outcome may introduce Nickell bias. More importantly, the lagged outcome conflates controls for heterogeneity and modeling dynamic treatment effects. To avoid these problems I only control for pretreatment outcomes to deal with heterogeneity. Alternatively, I use matching on pre-treatment outcomes and find that results show similar effects. Moreover, if I remove the lagged outcome it does not affect results and shows the existence of dynamic time trends. These results are available upon request.

anticipation to being treated and thus the potential outcome prior to treatment may not be that of baseline outcomes: estimated coefficients may reflect the anticipatory effects of the reform rather than differences in untreated potential outcomes between untreated and treated groups. In this setting, knowledge from incumbents of the term limit removal could lead to a decrease in public security (and public good) provision since this will be the period a term limited politician could extract rents or pursue clientelistic practices without the electoral penalty of reelection. The violation of this identification assumption would lead to a (positive) bias of concern. To see if this assumption holds I test whether early vs late adopters differed in their estimated effects. As seen in Appendix Figure ??, this is not the case: there is wide variation in estimated coefficients across early (red) and late (blue) adopters of the reform, conditional and unconditional on state covariates.²⁶

In short, taking together pretrends, evidence on no-anticipatory behavior from municipalities (states) and cohort weighted estimates that account for treatment effect heterogeneity, I find a robust and unbiased positive effect of reelection in homicides per capita. The following section further probes the results through different robustness and falsification tests.

10.3. Robustness and Falsification tests, and Sensitivity Analysis

I run several robustness checks to confirm main findings. To rule out concerns regarding dataset selection, Appendix Table ?? compares the effect of the Term Limit Reform on logged homicides per capita from different sources, INEGI, and the old and new methodology of homicides based on criminal cases from the SNSP. All specifications show parallel trends, especially 4 years prior to treatment. Moreover, all show positive and significant effects (mostly to the 1% level). If I combine through averaging homicide data from SNSP's old and new methodology there is evidence of pretrends four years prior to treatment, and strong positive and stable effects of the term limit reform on homicides per capita.²⁷ Results remain unchanged if we consider IHS transformations instead of logged values.

Two additional tests are done to increase the robustness of results. First, I check if results hold even with possible violations of parallel trends exist. As described in Appendix ??, results are robust even if we relax the parallel trend assumption to account for linear violations following ?.

I conduct a final analysis to validate main results. I perform a falsification test that checks whether the results are being driven by other mechanism that is not the electoral

²⁶For more detail on how I compare early to late adopters, please see Appendix ??.

²⁷There is no agreement if data captured through the new methodology can be paired with the old one due to differences in the filing of criminal cases. Estimates of column (4) should be interpreted with caution.

reform staggered rollout. I run 1,000 simulations following equation ?? and the specification of Table ?? column (1). This specification is the most robust specification that includes state covariates interacted with cohort-specific fixed effects, the lag of the main outcome to account for time trends, and municipal and year fixed effects. IW estimators are shown using cohort weights. In each simulation, I randomly assign each Mexican state to treatment and control using a random Bernoulli draw, keeping the proportion of treated units per year (e.g. there are 15 treated states out of 32 in the first treatment year (2015); this proportion is kept when assigning treatment randomly for the first year of treatment). In other words, I keep the same cohort size distribution of states. Thus, I randomly assign treatment following the observed treatment incidence. I then re-estimate the parameter of interest in each placebo simulation using the full sample of municipalities of Table ???. Appendix Figure ?? shows the resulting distribution of the 1,000 estimated beta coefficients (cohort weighted) for each lag and lead (IW estimates). The average estimated effect of the placebo is very close to zero (red dashed line), and its highly statistically unlikely that the estimates of Table ?? column (1) (blue line) could have been drawn from such a distribution.

If term limit removal strengthened voter accountability, what explains these paradoxical results? The following section delves into the mechanisms, specifically the role of electoral incentives on incumbents and the party on hold of the central government. Section ?? proceeds by ruling out alternative potential mechanisms, including adverse candidate selection (Section ??), citizens public security demands (Section ??) and state capture by criminal organizations (Section ??).

11. Mechanisms: Reelection Reform and Incumbency Advantage

If reelection strengthen voter accountability, what explains this paradoxical result of increasing violence? As discussed in Section ??, incumbent politicians are accountable to both political parties and voters. Policy preference misalignment from both principals, however, may yield inefficient public good provision. While it may be clear that voters prefer criminal deterrence and a reduction in violence,²⁸ preferences of incumbent parties is not evident in this setting. the behavior of parties, however, depends on electoral incentives. As noted in Section ?? if providing public security is costly, politicians may prefer to forbear the provision of public security.

²⁸It may not be as evident that voters prefer more public security provision given the externalities that the War on Drugs may create. This is further discussed in Section ??.

To test this mechanism I run a difference-in-discontinuity design that exploits the staggered rollout of the electoral reform and compares only municipalities with close elections. Specifically, I assess if the electoral reform led to an incumbency advantage that could trigger a decrease in the provision of public security and a subsequent increase in violence due to the vacuum of power left for grabs to DTOs.²⁹

11.0.1. Difference-in-Discontinuity of close elections design. Following ? and ?, I fit a local linear regression for municipalities within an Imbens-Kalyanaraman optimal bandwidth distance h to the cutoff ($=0$), using as forcing variable the winning margin of executive local elections.³⁰ In other words, I compare only municipalities in close elections and thus restrict the sample to those within a certain distance h to the threshold, i.e. $D_{mt} \in [D_b - h, D_b + h]$ for municipality m in time period t . By comparing municipalities where a party barely wins an election to municipalities where a party barely loses, the design allows to isolate the causal effect of winning office from the spurious correlation between current and future electoral success.³¹ Furthermore, the difference-in-difference setup allows to tease out any time-variant and time variant confounding variation as long as parallel trends and no anticipatory behavior from municipalities (states) holds. Lastly, following ? I present IW estimators to account for potential treatment effect heterogeneity. Given the discontinuity implicit setup, the results are local by construction.

The specification of the event-in-discontinuity in close elections design is the following:

(11.1)

$$y_{mt} = \mu_m + \mu_t + \sum_{e=1}^5 \sum_{k=-5, \neq -6, -1}^0 \gamma_{e,k} (1\{E_i = e\} \cdot R_{m,t}^k) + \sum_{e=1}^5 \sum_{k=-7, \neq -5, -1}^0 \Theta' X_{it} (1(E_i = e) \cdot R_{m,t}^k) \\ + f_{(.)}(margin)_{mt} + \sum_{e=1}^5 \sum_{k=-5, \neq -6, -1}^0 \nu_{e,k} (1\{E_i = e\} \cdot R_{m,t}^k \cdot f_{(.)}(margin)_{mt}) + \epsilon_{mt}$$

where $f_{(.)}(margin)_{mt}$ is the RD polynomial on winning margin for municipal election m at calendar time t , having $f_{(.)}$ take various polynomial approximations from quadratic

²⁹I also run a regression discontinuity design of close elections, splitting the sample before and after the treatment of the reform. These results need to be considered as naive and biased since we would need to assume that the value of incumbency in term limited and non-term limited cases is the same. ? make such quasi-parallel trend assumption but I prefer to actually test it using an difference-in-discontinuity in close elections design. The results of this exercise can be found in Appendix ??.

³⁰A rectangular kernel would give the same results as taking $E[Y]$ at a given bin on the distance to the cutting threshold. Other types of kernels, such as a triangular kernel, gives more weight to observations closer to the cutoff. I choose the latter for all estimations presented while estimations using a rectangular kernel are available upon request. Results are almost unchanged using the latter.

³¹For example, potential correlation that could arise, noted by ?, is that parties with good reputation or strong candidates are more likely to succeed in an electoral contest.

to quartic. $\sum_{e=1}^5 \sum_{k=-5, \neq -6, -1}^0 \nu_{e,k} (1\{E_i = e\} \cdot R_{m,t}^k \cdot f_{(.)}(margin)_{mt})$ is the RD polynomial interacting the E_i cohort-specific indicators and the electoral reform treatment indicator. Given that municipal elections occur every three years at maximum, and the reform was enacted in 2014 and all municipalities in the study period had only one election, there are no leads. Thus k relative time periods run from $k \in \{-6, -5, \dots, 0\}$. As with equation ??, to avoid collinearity I exclude two time period indicators, in this case that of $k = -1$ and -6 . Period $k = -2$ does not exist since there are no municipal elections two periods prior to the implementation of the electoral reform. As before, the period indicators by treatment cohort $\gamma_{e,k}$ are the DiD estimators for the Cohort Average Treatment Effects (CATTs). I take the linear combination of the CATTs and the relative cohort weights to construct the IW estimator. Standard errors are clustered at the state level as that is the level of treatment.

Table ?? presents results. Column (1) shows the effect of the electoral reform on the probability of winning at $t + 1$, if it was the incumbent on $t - 1$ following ?, while column (2) presents the effect on the probability of winning at $t + 1$ if the incumbent won at t . Columns (2) and (4) present IW estimators. As noted, the electoral reform generated an incumbency advantage of 35 percentage points, a result significant to the 10% level and robust across different polynomial approximations (column 1). Results are stronger if we compare incumbents at t that barely won at $t + 1$ relative to those that barely lost: the electoral reform increased the probability of winning in the following election between 53 to 77 percentage points depending on the specification, a result significant to the 1% level (see column 2). Importantly, as noted by ? we need to divide the observed coefficients and their standard errors by 2 in order to get the actual incumbency advantage. “The incumbency advantage is doubled because the winning party has both the benefit of being the incumbent party and the benefit of the other party not having this advantage (p. 512).”

To validate causal effects three identification assumptions need to hold. First, cohort weighted specifications need to show parallel trends for both incumbency advantage measures. As seen in Table ?? this is the case. Cohort weights further assure that parallel trends are not spurious due to correlation between the k period indicators relative to treatment implementation. Second, no anticipatory behavior from municipalities should be found; since we are only taking into account one election cycle post-treatment, we don’t anticipate incumbents reacting in such a short time window. Third, the design would be invalid if parties could manipulate close elections and sort themselves to those that imply a higher probability of winning. Two tests are commonly used to show validity on the design: (a) no covariate jump at the discontinuity on relevant pre-treatment variables and (b) density tests to see whether the number of municipalities above (or below) the cutoff threshold is significantly different from the number of municipalities below (or above).

TABLE 4. Event-in-Discontinuity in close elections model: Effect of 2014 Electoral Reform on Incumbency Advantage

Dependent variable:	Incumbent at t-1 won at t+1 (indicator) (1)	Incumbent at t won at t+1 (indicator) (2)
quadratic polynomial		
Lag 5 years	-0.0242 (0.1147)	-0.0448 (0.1601)
Lag 4 years	-0.5995* (0.3547)	0.4912 (0.4439)
Lag 3 years	-0.2664 (0.2179)	0.0029 (0.3037)
Reform, time 0	0.3515* (0.1960)	0.7784*** (0.2845)
Observations	1,840	1,667
R-squared	0.6931	0.6931
cubic polynomial		
Lag 5 years	-0.0167 (0.1116)	-0.0144 (0.1390)
Lag 4 years	-0.6069* (0.3542)	0.4181 (0.3735)
Lag 3 years	-0.2656 (0.2128)	-0.0742 (0.2732)
Reform, time 0	0.3512* (0.1919)	0.5374** (0.2602)
Observations	1,840	1,667
R-squared	0.6932	0.7496
quartic polynomial		
Lag 5 years	-0.0216 (0.1135)	-0.0517 (0.1585)
Lag 4 years	-0.6000* (0.3546)	0.4873 (0.4451)
Lag 3 years	-0.2637 (0.2172)	-0.0051 (0.3047)
Reform, time 0	0.3572* (0.1936)	0.7657*** (0.2815)
Observations	1,840	1,667
R-squared	0.6931	0.6931
Mun. FEs	✓	✓
Year. FEs	✓	✓
State Controls ^a	✓	✓
Cohort weighted	✓	✓

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 6 and 1) are removed to avoid collinearity problems noted by ? or because they are collinear or nonexistent, like lag time period 2. Standard errors in parentheses are clustered at the state level for estimates in saturated model. 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. ^aState-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. Logged homicides per capita at the municipality level are also included as controls.

Appendix Figure ?? shows evidence of no significant jump at the discontinuity of municipal population.³² Furthermore, Appendix Figure ?? shows no density difference between municipalities just above and below the cutoff.³³

Overall, results show the Term Limit Removal Reform increased incumbent probability of winning at future elections. If this is the case, then we should expect a decrease in the effort placed by security forces in the country, especially from the party in government, the PRI. Federal forces, particularly the military in charge of tackling DTOs, should decrease the amount of narcotic crackdowns and seizures. Furthermore, this should generate a downward effect to local proxies, i.e. mayors in charge of tackling down local crime. In fact, this is what I find.

11.1. Electoral Reform and Effort placed by Security Forces

Table ?? columns (3) to (8) show IW estimates for the effect of the reform on the eradication of kilograms of heroine and methamphetamine, and laboratories seizures. Odd columns compare those municipalities with at least one activity performed by the military, while even columns include those municipalities with no activity at all. In other words, odd columns show the effect of the Term Limit Reform comparing municipalities with at least one security activity with those with multiple, while even columns present the effect of the reform on municipalities with security activity relative to no activity at all. As observed, the reform decreased the amount of cocaine and methamphetamine eradicated, a result particularly strong three years after the reform was implemented. Cocaine seized kilograms decrease in 2.9% (a result significant to the 10% level) if we consider only municipalities that saw some military activity to 1.2% (significant to the 10% level) if we consider all municipalities, with and without military intervention. Likewise, methamphetamine decrease in 20.43%, significant to the 1% level in municipalities with no military activity and 9.15%, significant to the 1% level, considering all municipalities. For the case of laboratories, seizures decreased in 3.8% (significant to the 10% level) for municipalities with military intervention and 2.01% when we include all municipalities (significant to the 5% level). All results show strong parallel trends.³⁴

³²Next iteration of this working paper will include validation on municipal GDP, revenue and expenditures, geographic location and previous victory.

³³Given the reduction in sample size from Table ?? to Table ?? one could have a concern that main results no longer hold in this close election setting. Appendix Table ?? shows this is not the case by estimating results of Table ?? using as sample only the municipalities of the sample of Table ??.

³⁴Similar decreasing results are found for the eradication of poppy, marijuana, and the seizure of short and long arms, vehicles, clandestine air-paths, grenades and cartridges, but pretrends are found in these cases for all lags.

TABLE 5. Effect of 2014 Term Limit Reform Security Forces Effort, cohort weighted estimates

Dependent variable:	Local Police				Military			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 7 years	0.3284* (0.1727)	0.1674 (0.1162)	0.2604 (0.2248)	0.0700 (0.0592)	1.2051** (0.5493)	0.3538** (0.1612)	0.1994** (0.0833)	0.0621** (0.0244)
Lag 6 years	0.2310*** (0.0839)	0.0588 (0.0378)	0.2169* (0.0987)	0.0508** (0.0239)	0.3275** (0.1640)	0.1010** (0.0403)	0.0105 (0.0161)	0.0064 (0.0043)
Lag 5 years	0.182 (0.1008)	0.0507 (0.0362)	0.0932 (0.0687)	0.0538 (0.0379)	0.2389* (0.1233)	0.1377** (0.0650)	0.0035 (0.0300)	0.0050 (0.0162)
Lag 4 years	0.1712 (0.1343)	0.049 (0.0555)	0.0174 (0.0283)	0.0046 (0.0152)	0.0210 (0.11263)	0.0358 (0.0621)	0.0036 (0.0258)	0.0173 (0.0147)
Lag 3 years	0.4414*** (0.1476)	0.1019* (0.0589)	0.0188 (0.0367)	0.0036 (0.0159)	-0.0351 (0.1152)	-0.0241 (0.0509)	-0.0165 (0.0236)	0.0033 (0.0132)
Lag 2 years	-0.0109 (0.1155)	-0.0401 (0.0490)	0.0021 (0.0284)	0.0004 (0.0133)	0.0005 (0.1165)	0.0548 (0.0532)	-0.0012 (0.0193)	0.0027 (0.0099)
Reform, time 0	-0.1131* (0.0614)	-0.0629** (0.0272)	-0.0324* (0.0193)	-0.0212** (0.0100)	-0.1863*** (0.0638)	-0.1109** (0.0325)	-0.0324 (0.0147)	0.0064 (0.0074)
Lead 1 year	-0.1314 (0.0798)	-0.0940*** (0.0322)	-0.0557** (0.0228)	-0.0316** (0.0129)	-0.3272*** (0.0731)	-0.1822** (0.0390)	-0.0235 (0.0179)	-0.0123 (0.0085)
Lead 2 years	-0.0451 (0.0895)	-0.0596 (0.0385)	-0.0570** (0.0237)	-0.0299** (0.0122)	-0.3308*** (0.0736)	-0.1715*** (0.0376)	-0.0380** (0.0148)	-0.0179** (0.0071)
Lead 3 years	-0.1686* (0.0968)	-0.1746*** (0.0504)	-0.0303 (0.0191)	-0.0133 (0.0082)	-0.2151*** (0.0739)	-0.0944** (0.0325)	-0.0347 (0.0218)	-0.0193** (0.0096)
Observations	2,816	8,592	4,550	8,592	4,550	8,592	4,550	8,592
R-squared	0.7884	0.7836	0.3371	0.3141	0.4069	0.3718	0.6701	0.6481
Mun. FE _s	✓	✓	✓	✓	✓	✓	✓	✓
Year. FE _s	✓	✓	✓	✓	✓	✓	✓	✓
State Controls ^b	✓	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓
Lag log(homicides per capita)	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Coefficients show IW estimators following ^a. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ^b. Standard errors in parentheses are clustered at the state level for estimates in saturated model. 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. ^a Even columns with outcomes with missing values where replaced by zeros assuming no activity was registered. ^b 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.

Military activity has two functions: on the one hand, deter criminal activity directly; on the other hand, monitor local police forces. If military activity decreases, signaling a reduction in the level of effort placed by federal authorities, we should expect a decrease in local level security provision. This is what we find in Table ?? columns (1) and (2): municipalities under-provide public security as seen by a decrease in the number of detentions per capita by 14.33 percentage points, significant to the 1% level, three years after reform implementation; results are similar for one and two years after implementation of the reform. Overall evidence of pretrends is found, if slight positive difference between treated and non-treated municipalities three years prior to the implementation of the reform. Strong evidence points to a downward effect of local level security provision, triggered by both a reduction of the principal level of effort and its local proxy, the local mayor.

12. (ruling out) Alternative Explanations

12.1. Adverse politician selection

Improving the selection of incumbents is key to the improvement of local institutions and the provision of public goods. Better incumbents decrease government control by firms and market distortions (?), as well as corruption (?). Various theoretical (? for example) and empirical papers (?) have showed that term limits are associated with lower incumbent quality making its removal an “electoral accountability act as a powerful mechanism to align politicians’ actions with voters’ preferences” (p. 1277 from ?). While we would expect term limit removal to increase incumbent’s quality, potential adverse politician selection still exists, especially if an increase in party-level political survival is observed (as is our case). Table ?? shows the estimates of an event-in-discontinuity in close elections model on the effect of the 2014 Term Limit Removal Reform on incumbent’s quality. To measure incumbent quality I web-scrape professional titles for all municipal mayors in Mexico from 2010 to 2019 from the National Information Municipal System (SNIM for its abbreviation in Spanish).³⁵ I then construct an indicator variable that takes a value of 1 if the mayor had at least a college degree, 0 otherwise. Column (1) of Table ?? shows estimates using as sample those from Table ?? column (1), while column (2) presents estimates using the sample of municipalities from Table ?? column (2). While non-significant, across different functional forms in column (1) we find that the reform increase the quality of politicians between 32 to 44 percentage points. No pretrends are found in column (1). Column (2) does show statistically significant increase between 5 to 7% in the quality

³⁵SNIM webpage can be found in <http://www.snim.rami.gob.mx>.

of incumbents. However, there are pretrends: it seems that treated municipalities in pre-treatment periods suffered from incumbents with lower quality overall. In other words, the Reform did not trigger an adverse politician selection that could explain the increase in violence in treated municipalities in Mexico.

12.2. Variation in voters public security demands

A reason that may limit the political accountability of reelection is spatial heterogeneity in constituents public good preferences. Given negative externalities in public good provision, voters may prefer higher public good provision but elsewhere. For instance, all constituents may prefer peace rather than violence, but would only desire public security deployment away rather than at home given the negative externalities of fighting crime. This could generate a decrease in public security provision and thus an increase in crime.

An example is handy. On October 17, 2019, Mexican military forces detained Ovidio Guzman, son of Joaquin Guzman Loera, “El Chapo”, former leader of the Sinaloa Cartel. After the detention, hundreds of *sicarios* blocked several streets in the city of Culiacan, had as hostages more than 20 family members of military forces, wounded civilians and attacked multiple buildings where military forces lived. As a response, President Andres Manuel Lopez Obrador gave the command to set Ovidio Guzman free. Since, 56% of citizens in Mexico believe they can suffer an event similar to that of Culiacan, and disapprove any military intervention in the city against DTOs.^{36,37}

Is variation in citizens public security demands driving the main results? Table ?? shows this is not the case. Estimates show the effect of the 2014 Term Limit Reform on homicides, controlling for citizens security perception. Columns (1) and (3) mimic the results of Table ??, for logged and IHS homicides per capita. Columns (2) and (4) control for citizens security perception which include the percentage of citizens who see narcotraffic as the most worrisome issue in the country, the percentage of citizens who see a lack of punishment of criminals as the most worrisome public issue, the amount of money spent to protect from crime and citizens’ trust on local police and the army, all at the state level. These citizens’ perceptions come from the 2011 to 2019 National Survey of Victims Perception (ENVIPE for its abbreviation in Spanish) from INEGI. As noted, controlling for citizen’s

³⁶For more details see Forbes article “56% cree que puede sufrir un evento como el de Culiacán: encuesta De las Heras” from <https://www.forbes.com.mx/56-cree-que-puede-sufrir-un-evento-violento-como-el-de-culiacan-encuesta-de-las-heras/>.

³⁷Another example is that of nuclear energy generation. Constituents may prefer higher electricity provision but due to the potential negative externalities they may not like having a nuclear plant in their city. In such settings, local executives may not be held accountable for the provision of public goods, decreasing bottom-up accountability and focus on particularistic spending. If there is high demand of a specific public good with high negative externalities, from a party perspective, particularistic spending would be targeted in regions with strong party support, while public good provision would be targeted in opposition regions.

TABLE 6. Event-in-Discontinuity in close elections model: Effect of 2014 Term Limit Reform on Incumbent's Quality

Dependent variable:		
	Incumbent quality indicator	
	(1)	(2)
	linear polynomial	
Lag 6 years		-0.2823 (0.5614)
Lag 5 years	-0.2970 (0.3884)	-0.1287 (0.7494)
Lag 4 years	-0.5169 (0.6868)	-2.1075*** (0.1415)
Lag 3 years	-0.2636 (0.6319)	-0.4291* (0.2264)
Reform, time 0	0.3228 (0.5141)	0.0692** (0.0275)
Observations	1,662	1,985
R-squared	0.7119	0.6784
quadratic polynomial		
Lag 6 years		-0.2795 (0.5702)
Lag 5 years	-0.4390 (0.3773)	-0.0755 (0.7316)
Lag 4 years	-0.3998 (0.6689)	-2.0649*** (0.1457)
Lag 3 years	-0.0573 (0.6061)	-0.4221* (0.2179)
Reform, time 0	0.4450 (0.5035)	0.0584 (0.0452)
Observations	1,813	1,985
R-squared	0.7031	0.6816
cubic polynomial		
Lag 6 years		-0.2937 (0.5520)
Lag 5 years	-0.4578 (0.3705)	-0.1441 (0.7359)
Lag 4 years	-0.3220 (0.6621)	-2.1105*** (0.1586)
Lag 3 years	-0.0033 (0.5902)	-0.3978* (0.2224)
Reform, time 0	0.5064 (0.4934)	0.0691** (0.0275)
Observations	1,813	1,985
R-squared	0.7024	0.6816
quartic polynomial		
Lag 6 years		-0.3117 (0.5664)
Lag 5 years	-0.4604 (0.3828)	-0.0706 (0.7357)
Lag 4 years	-0.3562 (0.6761)	-2.1169*** (0.1292)
Lag 3 years	0.0009 (0.6099)	-0.4071* (0.2186)
Reform, time 0	0.4846 (0.5075)	0.0577 (0.0361)
Observations	1,813	1,985
R-squared	0.7010	0.6820
Mun. FE ^a	✓	✓
Year. FE ^a	✓	✓
State Controls ^b	✓	✓
Mun Controls ^c	✓	✓
Cohort weighted	✓	✓
Sample Inc. Adv. DV	Inc. at t-1 won at t+1	Inc. at t won at t+1

Notes: Coefficients show IW estimators following [?]. Two relative time periods (lag 6 and 1) are removed to avoid collinearity problems noted by [?] or because they are collinear or nonexistent like lag 2. Standard errors in parentheses are clustered at the state level for estimates in saturated model. Significance-levels: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. ^a Even columns with outcomes where missing values were replaced by zeros assuming no activity was registered. ^b 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. ^c Municipal controls include logged homicides per capita interacted with cohort fixed effects pre-treatment.

TABLE 7. Effect of 2014 Term Limit Reform on Violence, controlling for citizens security perception

Dependent variable:	log(homicide per capita)		ihs(homicide per capita) ^a	
	(1)	(2)	(3)	(4)
Lag 7 years	-0.2569 (0.1766)		-0.3129 (0.2584)	
Lag 6 years	-0.0416 (0.0820)	-0.0111 (0.0580)	-0.0535 (0.1108)	-0.0053 (0.0748)
Lag 5 years	0.1505* (0.0777)	-0.0072 (0.0198)	0.2019* (0.1011)	-0.0056 (0.0248)
Lag 4 years	0.1534 (0.1571)	0.0469 (0.0801)	0.2315 (0.1910)	0.0704 (0.0967)
Lag 3 years	0.1274 (0.1551)	0.3133 (0.2082)	0.2044 (0.1883)	0.4325* (0.2407)
Lag 2 years	0.0873 (0.1143)	0.1054 (0.1362)	0.1416 (0.1386)	0.1534 (0.1608)
Reform, time 0	0.1080** (0.0518)	0.1477* (0.0775)	0.1512** (0.0610)	0.1978** (0.0926)
Lead 1 year	0.4616*** (0.0804)	0.2641** (0.1038)	0.6111*** (0.0994)	0.3507*** (0.1176)
Lead 2 years	0.3939*** (0.1165)	0.1894* (0.0937)	0.5372*** (0.1485)	0.2686** (0.1060)
Lead 3 years	0.4061*** (0.1386)	0.2188* (0.1068)	0.5564*** (0.1740)	0.3107** (0.1261)
Observations	8,592	7,574	8,592	7,574
R-squared	0.7776	0.7889	0.7025	0.7164
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^b	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓
Lag DV	✓	✓	✓	✓
Citizens' Security Perception ^c		✓		✓

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ?; lag 7 is removed in columns (2) and (4) due to collinearity. 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. ^a Refers to the inverse hyperbolic sine transformation.

^b 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. ^c Citizens' Security Perception are state-level covariates that include the percentage of citizens who see narcotics as the most worrisome issue in the country, the percentage of citizens who see a lack of punishment of criminals as the most worrisome public issue, the amount of money (in thousands) spent to protect from crime, and citizens trust on local police forces and the army.

security perception does not fade away the results found previously. However, estimates do decrease in magnitude approximately 20 percentage points in the years following the implementation of the reform, for both logged and IHS transformations. Thus, conditional on citizens' public security demands, there are still positive and significant effects of term limit removal on violence.

12.3. Captured incumbent

A reason why reelection would not yield political accountability is state capture. Public good provision entails, sometimes, the interaction with a third player. For example, incumbents interrelate with firms who compete for procurement bids. In the process, however, incumbents may be captured, changing the market structure and decreasing welfare. This is even more prevalent with longer tenures (?). As noted by ? firms are strategic and react to the uncertainty of the political environment. Thus if some bidders are favored locally, this may reduce the number of bidders per auction and of winners, and increase the cost of procurement and the cost of the associated public goods (?). Likewise, incumbents may be captures by non-state armed groups who modify local institutions in favor of their self interest (?), including a reduction in state-led violence agains them.³⁸

In the paper thus far, I have assumed that DTOs are non-strategic players. However, this assumption is hard to defend: incumbents with longer tenures are more likely to be captured by DTOs. In such case, we would expect a reduction in violence in municipalities with high DTO presence. This is not what we find. Table ?? presents the interaction of Term Limit Reform and proximity to the U.S. to proxy for market value, as well as the interaction of the reform with cartel presence.³⁹ To measure DTO presence I use ? measure of cartel presence. I average indicators from 2006 to 2010 of whether a cartel was present in a given municipality. I also borrow the proximity to the U.S. measure from ?, a variable highly correlated with drug trafficking networks. Both measures are pretreatment. First, columns (1) and (3) show that municipalities further away from the U.S. -i.e. municipalities with lower drug market value- see a decrease in the percentage of homicides per capita of 31 and 37% for logged or IHS transformations, respectively. However, contrary to the state capture hypothesis which expects a decrease in homicides we observe in columns (2) and (4) an increase in the number of homicide related deaths by 14% for both logged and IHS transformations, significant to the 5 and 10% level, respectively. Furthermore, if we run a triple interaction of the reform, cartel presence and proximity to the U.S. we find a negative effect of -39.51% in logged homicides per capita (-54% using an IHS transformation) significant to the 5% (1%).⁴⁰ This implies that only in municipalities with cartel presence with low market value we find a decrease in homicides. However, settings with high market value and DTO presence an increase in homicides is observed. In general,

³⁸Furthermore, as noted in Section ??, if voters perceive incumbents are captured, information from the first period in office may not be enough to determine the performance of a second term in office. The result is incumbency disadvantage (?).

³⁹I assume that municipalities closer to the U.S. have higher market value for DTOs than those located far away, specially since most DTOs in Mexico revenue comes from distribution rather than narcotic generation.

⁴⁰Results available upon request.

we observe that the positive effect of term limit removal on violence is not explained by captured incumbents who decrease public security.

TABLE 8. Total Interaction Effect of Term Limit Reform and Drug Trafficking Organization Presence on Violence^a

Dependent variable:	log(homicide per capita)		ihs(homicide per capita) ^b	
	(1)	(2)	(3)	(4)
Reform (t+3) X Proximity to U.S.	-0.3094** (0.1210)		-0.3761** (0.1539)	
Reform (t+3) X Cartel presence (indicator)		0.1412** (0.0670)		0.1398* (0.0818)
Observations	8,592	8,592	8,592	8,592
R-squared	0.7779	0.7030	0.7778	0.7027
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓
Lag DV	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of proximity to the US (Cartel presence indicator) + estimated coefficient of the interaction of proximity (cartel presence)*lead t=3. This is a post-estimation test using the same specification as that of Table ???. Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

^b Refers to the inverse hyperbolic sine transformation. ^c 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. ^d Estimates weighted by each cohort's relative share of the sample following ?.

One last alternative mediator explaining the increase of violence is local executives refusing to take responsibility or be involved any further in the provision of public security. With reelection, non-term limited incumbents may choose to sign security cooperation agreements with upper administrative levels, including those with the state or the federal government. Some of this agreements may imply a full delegation of public security provision to governors or the president. I do not find evidence that this mechanism is driving results. For more detail please see Appendix ??.

13. Conclusion

As presented in this paper, term limit removal may be a necessary but not sufficient condition to ensure political accountability. In democratic representative systems with reelection local incumbents are accountable to both voters and their political parties. However, if principals are misaligned, this leads to potential disastrous public outcomes. In the case of Mexico, a country characterized by a now 14 year old internal conflict between the government and multiple drug trafficking organizations, an electoral reform that introduced

reelection did not yield the expected benefits of political accountability. In turn, reelection created a party incumbency advantage, that generated incentives for the central government to decrease its level of public security, and lead to an overall increase in crime due to a vacuum of power in territories up for grabs for drug trafficking organizations. Moreover, the decrease of central government security provision made local proxies to decrease public security provision as well. In other words, failing to predict how principals will behave rather than agents only, may lead to public good underprovision, and in the case of security provision, widespread violence.

Appendix A. Tables

TABLE A-1. Descriptive statistics

	Mean	SD	Min	Max	N
<i>Panel A - Violence:</i>					
deaths by homicide (INEGI)	16.71	70.23	1	3,766	13,884
deaths by homicide per capita (INEGI)	36.94	157.61	1	8,462	13,884
log(deaths by homicide per capita) (INEGI)	-8.31	0.99	-12	-2.4	13,884
asinh(deaths by homicide per capita) (INEGI)	3.59	1.05	1	9.7	13,884
homicides (SNSP)	10.70	46.10	0	1,618	12,834
homicides per capita (SNSP, old measure)	17.13	85.04	0	8,660	12,834
log(homicides per capita) (SNSP, old measure)	-8.60	0.99	-12	-2.4	12,834
asinh(homicides per capita) (SNSP, old measure)	2.27	1.77	0	9.8	12,834
homicides per capita (SNSP, new measure)	15.77	26.37	0	771	11,165
log(homicides per capita) (SNSP, new measure)	-8.46	0.98	-12	-4.5	11,165
asinh(homicides per capita) (SNSP, new measure)	2.23	1.81	0	7.3	11,165
<i>Panel B - 2014 Electoral Reform:</i>					
Lag 8	0.01	0.12	0	1	20,710
Lag 7	0.02	0.13	0	1	20,710
Lag 6	0.06	0.23	0	1	20,710
Lag 5	0.10	0.30	0	1	20,710
Lag 4	0.10	0.30	0	1	20,710
Lag 3	0.10	0.30	0	1	20,710
Lag 2	0.10	0.30	0	1	20,710
Lag 1	0.10	0.30	0	1	20,710
Reform, time 0	0.10	0.30	0	1	20,710
Lead 1	0.10	0.30	0	1	20,710
Lead 2	0.09	0.28	0	1	20,710
Lead 3	0.08	0.28	0	1	20,710
<i>Panel C - Mechanisms:</i>					
Alignment with federal executive=1; 0 otherwise	0.72	0.45	0	1	6,615
Winning margin: first - second runner	0.12	0.11	0	1	6,614
Detained by local police (in flagrancy, SNSP)	52.58	137.03	1	4,106	4,433
Detained by local police per capita (in flagrancy, SNSP)	52.08	291.09	0	11,408	4,433
log(Detained by local police per capita) (in flagrancy, SNSP)	-8.78	1.41	-13	-2.2	4,433
asinh(Detained by local police per capita) (in flagrancy, SNSP)	3.21	1.51	0	10	4,433
Secured short arms (SEDENA)	4.80	18.91	0	863	8,600
Secured long arms (SEDENA)	8.93	38.97	0	1,502	8,600
Secured cartridges (SEDENA)	1657.16	10703.86	0	422,126	8,600
Secured cocaine (kg, SEDENA)	2.57	43.60	0	2,387	8,600
Secured heroine (kg, SEDENA)	0.17	4.23	0	221	8,600
Secured marijuana (kg, SEDENA)	843.31	6688.25	0	234,003	8,600
Secured methamphetamine (kg, SEDENA)	15.82	364.24	0	24,441	8,600
Eradicated amapola (kg per hectare, SEDENA)	19.87	171.51	0	6,934	8,600
Eradicated mariguana (kg per hectare, SEDENA)	7.80	69.13	0	3,216	8,600
Secured grenades (SEDENA)	1.29	9.38	0	319	8,600
Laboratories Eradicated (SEDENA)	0.15	1.44	0	60	8,600
Runways Eradicated (SEDENA)	0.35	2.73	0	72	8,600
Secured airplanes (SEDENA)	0.03	0.39	0	20	8,600
Secured water vehicle (SEDENA)	0.02	0.25	0	10	8,600
<i>Panel D - Controls:</i>					
Population (INEGI and CONAPO projections)	49,103	140,774	91	1,848,954	24,470
Winning margin: first - second runner (governor)	0.15	0.13	0	1	24,470
Party alignment with federal executive=1; 0 otherwise	0.54	0.50	0	1	24,470
<i>Panel E - Alternative mechanisms:</i>					
Incumbent undergraduate or graduate title (indicator)	0.06	0.24	0	1	7,851
Narcotraffick most worrisome public topic (state-level, %)	0.15	0.05	0	.37	17,129
Lack of punishment to criminals most worrisome public topic (state-level, %)	1.52	3.58	0	11	19,576
Money spend to protect from crime ('000) (state-level)	5368.71	1337.29	929	12,689	22,023
Trust in local police (state-level)	0.69	0.10	0	1	22,023
Trust in army (state-level)	0.36	0.15	0	.88	22,023

TABLE A-2. Effect of 2014 Electoral Reform on Violence, cohort weighted vs non-weighted estimates

	Dependent variable:					
	log(homicide per capita)	ihs(homicide per capita) ^a	(1)	(2)	(3)	(4)
Lag 7 years	-0.2569	0.0000	-0.3129	0.0000		
Lag 6 years	-0.0815	-0.0416	-0.0981	-0.0530		
Lag 5 years	-0.2188***	0.1505*	-0.2512***	0.1800*		
Lag 4 years	-0.4719***	0.1534	-0.5390***	0.2043		
Lag 3 years	-0.4908***	0.1274	-0.5556***	0.1768		
Lag 2 years	-0.2981***	0.0873	-0.3398***	0.1213		
Reform, time 0	-0.0410	0.1080**	-0.0469	0.1396**		
Lead 1 year	-0.2657***	0.4616***	-0.3025***	0.5740***		
Lead 2 years	-0.6188***	0.3939***	-0.6996***	0.4933***		
Lead 3 years	-0.5766***	0.4061***	-0.6502***	0.5086***		
Observations	8,592	8,592	8,592	8,592		
R-squared	0.7728	0.7776	0.6989	0.7047		
Mun. FEs	✓	✓	✓	✓		
Year. FEs	✓	✓	✓	✓		
State Controls ^b	✓	✓	✓	✓		
Cohort weighted		✓		✓		
Lag DV	✓	✓	✓	✓		

Notes: Linear combination of the Cohort Average Treatment Effects (CATTs) for each relative time period, weighting by each cohort's relative share of the sample following ?. Two relative time periods (lag 8 and 1) are removed in columns (2) and (4) to avoid collinearity problems noted by ?; lag 7 is removed due to collinearity in columns (1) and (3). Standard errors in parentheses are clustered at the state level, with the following significance-levels: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. ^a Refers to the inverse hyperbolic sine transformation. ^b 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.

TABLE A-3. Event-Study Estimates on the effect of 2014 Electoral Accountability Reform on Violence, non-cohort weighted estimates

Dependent variable:				
	log(homicides per capita)	ihs(homicides per capita) ^a		
	(1)	(2)	(3)	(4)
Lag 7 years	-0.105 (0.091)	-0.149 (0.137)	-0.137 (0.097)	-0.206 (0.147)
Lag 6 years	0.037 (0.062)	-0.023 (0.109)	0.066 (0.067)	0.009 (0.117)
Lag 5 years	-0.035 (0.050)	-0.127 (0.077)	-0.033 (0.053)	-0.129 (0.083)
Lag 4 years	0.026 (0.047)	-0.057 (0.068)	0.030 (0.051)	-0.053 (0.073)
Lag 3 years	0.010 (0.042)	-0.110* (0.059)	0.015 (0.045)	-0.112* (0.063)
Lag 2 years	-0.016 (0.038)	-0.088* (0.047)	-0.006 (0.041)	-0.096* (0.050)
Reform, time 0	0.013 (0.043)	0.071 (0.045)	0.017 (0.047)	0.080 (0.048)
Lead 1 year	0.022 (0.055)	0.139** (0.061)	0.037 (0.059)	0.167** (0.065)
Lead 2 years	0.047 (0.066)	0.255*** (0.083)	0.048 (0.071)	0.284*** (0.089)
Lead 3 years	0.167** (0.085)	0.417*** (0.099)	0.150* (0.091)	0.432*** (0.106)
Observations	11,629	11,629	11,629	11,629
R ²	0.290	0.296	0.263	0.270
Mun. FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
State controls		✓		✓

Notes: Standard errors in parentheses are clustered at the state level, with the following significance-levels: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test. ^a Refers to the inverse hyperbolic sine transformation.

TABLE A-4. Event-Study Estimates on the effect of 2014 Electoral Accountability Reform on Violence, various main outcome transformations

Dependent variable:						
	homicides		homicides per capita		log(hom. per capita)	
	(1)	(2)	(3)	(4)	(5)	(6)
Lag 7	-4.197 (7.817)	-3.586 (6.184)	-17.605 (18.114)	-29.630 (27.465)	-0.105 (0.091)	-0.149 (0.137)
Lag 6	10.397* (5.389)	10.261** (4.263)	4.185 (12.488)	-4.560 (21.927)	0.037 (0.062)	-0.023 (0.109)
Lag 5	0.459 (4.303)	0.703 (3.404)	-8.048 (9.971)	-21.799 (15.491)	-0.035 (0.050)	-0.127 (0.077)
Lag 4	-0.099 (4.093)	0.411 (3.238)	-3.289 (9.484)	-18.400 (13.605)	0.026 (0.047)	-0.057 (0.068)
Lag 3	0.941 (3.658)	1.545 (2.894)	0.896 (8.477)	-8.837 (11.867)	0.010 (0.042)	-0.110* (0.059)
Lag 2	1.403 (3.275)	0.976 (2.591)	-7.667 (7.589)	-15.100 (9.421)	-0.016 (0.038)	-0.088* (0.047)
Reform, time 0	1.886 (3.753)	1.563 (2.969)	0.411 (8.696)	6.686 (9.065)	0.013 (0.043)	0.071 (0.045)
Lead 1	4.445 (4.773)	3.083 (3.776)	4.084 (11.061)	16.415 (12.164)	0.022 (0.055)	0.139** (0.061)
Lead 2	5.649 (5.685)	4.477 (4.498)	9.814 (13.174)	29.356* (16.647)	0.047 (0.066)	0.255*** (0.083)
Lead 3	7.077 (7.343)	6.409 (5.809)	6.201 (17.017)	31.908 (19.903)	0.167** (0.085)	0.417*** (0.099)
Observations	11,629	11,629	11,629	11,629	11,629	11,629
R ²	0.079	0.424	0.024	0.025	0.290	0.296
Mun. FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
State controls	✓		✓	✓		✓

Notes: Standard errors in parentheses are clustered at the state level, with the following significance-levels: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test.

TABLE A-5. Effect of 2014 Term Limit Reform on Violence, using different homicide databases

Source:	Dependent variable: log(homicides per capita)			
	INEGI		SNSP	
	(1)	(old measure)	(new measure)	(combined)
Lag 7 years	-0.2569 (0.1766)			
Lag 6 years	-0.0416 (0.0820)		-0.0826** (0.0381)	-0.0711** (0.0343)
Lag 5 years	0.1505* (0.0777)		-0.0398* (0.0210)	-0.0387* (0.0198)
Lag 4 years	0.1534 (0.1571)		0.0482 (0.0769)	0.1170 (0.0776)
Lag 3 years	0.1274 (0.1551)		-0.0813 (0.1318)	0.1105 (0.1524)
Lag 2 years	0.0873 (0.1143)	-0.0107 (0.0261)	-0.0638 (0.0964)	0.0766 (0.0972)
Reform, time 0	0.1080** (0.0518)	0.0130 (0.0230)	0.0825 (0.0702)	0.1898** (0.0711)
Lead 1 year	0.4616*** (0.0804)	0.0479 (0.0335)	0.3014*** (0.0921)	0.5458*** (0.1258)
Lead 2 years	0.3939*** (0.1165)	0.0490** (0.0198)	0.0782 (0.0831)	0.4253** (0.1574)
Lead 3 years	0.4061*** (0.1386)	0.2470*** (0.0810)		0.5446*** (0.1589)
Observations	8,592	3,088	5,452	6,515
R-squared	0.7776	0.8479	0.7359	0.7312
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^b	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓
Lag DV	✓	✓	✓	✓

Notes: Coefficients show IW estimators following ?. Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by ? in column (1). Missing values correspond to missingness in the data. 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. ^a Refers to the inverse hyperbolic sine transformation. ^b 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.

TABLE A-6. Effect of 2014 Term Limit Reform on Violence, controlling for security cooperation agreements

Dependent variable:	log(homicide per capita)	ihs(homicide per capita) ^a
	(1)	(2)
Lag 7 years	-0.2675 (0.1675)	-0.3293 (0.2478)
Lag 6 years	-0.0463 (0.0789)	-0.0607 (0.1071)
Lag 5 years	0.1419* (0.0793)	0.1893* (0.1034)
Lag 4 years	0.1367 (0.1565)	0.2069 (0.1913)
Lag 3 years	0.1132 (0.1544)	0.1833 (0.1887)
Lag 2 years	0.0785 (0.1139)	0.1282 (0.1887)
Reform, time 0	0.1045** (0.0506)	0.1472** (0.0600)
Lead 1 year	0.4623*** (0.0783)	0.6120*** (0.0975)
Lead 2 years	0.3721*** (0.1132)	0.5068*** (0.1456)
Lead 3 years	0.3938*** (0.1355)	0.5380*** (0.1713)
Observations	8,442	8,442
R-squared	0.7786	0.7035
Mun. FE	✓	✓
Year. FE	✓	✓
State Controls ^b	✓	✓
Cohort weighted	✓	✓
Lag DV	✓	✓
Security Coop. Agreement	✓	✓

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. ^a Refers to the inverse hyperbolic sine transformation. ^b 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.

TABLE A-7. Effect of 2014 Term Limit Reform on Violence, comparing municipalities with security cooperation agreements with other levels of government

Dependent variable:	log(homicide per capita)		ihs(homicide per capita) ^a	
	w/o Security Coop. Agreement (1)	w/ Security Coop. Agreement (2)	w/o Security Coop. Agreement (3)	w/ Security Coop. Agreement (4)
Lag 7 years	-0.2021 (0.3766)	0.0246 (0.0401)	-0.2262 (0.6022)	0.1679*** (0.0472)
Lag 6 years	-0.0469 (0.1480)	0.0611*** (0.0217)	-0.0511 (0.2273)	0.1012*** (0.0261)
Lag 5 years	-0.1506 (0.1496)	0.0483 (0.1067)	-0.1462 (0.2347)	0.0798 (0.1360)
Lag 4 years	-0.3269 (0.2536)	0.0646 (0.2568)	-0.3166 (0.3827)	0.1117 (0.3072)
Lag 3 years	-0.2887 (0.2529)	0.1102 (0.2336)	-0.2591 (0.3821)	0.1712 (0.2770)
Lag 2 years	-0.3230* (0.1799)	0.0609 (0.1533)	-0.3226 (0.2564)	0.1038 (0.1899)
Reform, time 0	0.0540 (0.0582)	0.1955** (0.0847)	0.0949 (0.0844)	0.2617** (0.1039)
Lead 1 year	-0.0560 (0.1402)	0.4414*** (0.1125)	0.0182 (0.1988)	0.5927*** (0.1396)
Lead 2 years	-0.1195 (0.2629)	0.1858 (0.2048)	-0.0420 (0.4107)	0.2690 (0.2532)
Lead 3 years	0.2335 (0.3005)	0.1631 (0.2203)	0.3814 (0.4463)	0.2468 (0.2722)
Observations	3,835	4,607	3,835	4,607
R-squared	0.8459	0.8102	0.7935	0.7504
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^b	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓
Lag DV	✓	✓	✓	✓

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. ^a Refers to the inverse hyperbolic sine transformation. ^b 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.

TABLE A-8. Total Interaction Effect:^a the role of security cooperation agreements

Dependent variable:	log(homicide per capita)		ihs(homicide per capita) ^b	
	(1)	(2)	(3)	(4)
Reform (t+3)*Coop. Agreement	-0.2689** (0.1094)		-0.3197** (0.1313)	
Reform (t+3)*Coop. Agreement (other measure) ^e		-0.2230** (0.0935)		-0.2646** (0.1111)
Observations	8,008	8,008	8,442	8,442
R-squared	0.7820	0.7092	0.7792	0.7064
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of alignment with Federal Government indicator (winning margin) + estimated coefficient of the interaction of alignment (winning margin)*lead t=3. This is a post-estimation test using the same specification as that of Table ?? column (1). Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0. Main regression with standard errors clustered at the state-level. ^b Refers to the inverse hyperbolic sine transformation. ^c 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. I also include the lag of the outcome, i.e. logged homicides per capita as control. ^d Estimates weighted by each cohort's relative share of the sample following ?. ^e Measure taken from reasons to modify police structure questionnaire from the National Census of Municipal Governments from INEGI.

TABLE A-9. Event-in-Discontinuity in close elections model: testing no discontinuous jump of covariates

Dependent variable:		
	population ('000)	
	(w/o cohort weights) (1)	(w/ cohort weights) (2)
linear polynomial		
Lag 5 years	0.9885	0.4920
Lag 4 years	33.4369	25.5176
Lag 3 years	19.7357	9.8474
Reform, time 0	$-1.2e + 02^{***}$	8.4008
Observations	1,668	1,668
R-squared	0.9995	0.9995
tex		
quadratic polynomial		
Lag 5 years	1.6867	0.8394
Lag 4 years	33.3792*	25.4736*
Lag 3 years	17.5997	9.3557
Reform, time 0	$-1.3e + 02^{***}$	7.9320
Observations	1,840	1,840
R-squared	0.9993	0.9993
cubic polynomial		
Lag 5 years	2.5971	1.2925
Lag 4 years	33.0294*	25.2066*
Lag 3 years	19.3518	9.5349
Reform, time 0	$-1.3e + 02^{***}$	8.1340
Observations	1,840	1,840
R-squared	0.9993	0.9993
quartic polynomial		
Lag 5 years	1.7280	0.8600
Lag 4 years	33.2123*	25.3462*
Lag 3 years	17.5776	9.3860
Reform, time 0	$-1.3e + 02^{***}$	8.1269
Observations	1,840	1,840
R-squared	0.9993	0.9993
Mun. FEs	✓	✓
Year. FEs	✓	✓
State Controls ^a	✓	✓
Cohort weighted	✓	✓

Notes: Coefficients in column (1) show non-cohort weighted estimates; coefficients of column (2) show IW estimators following ?. Two relative time periods (lag 6 and 1) are removed to avoid collinearity problems noted by ?. Standard errors in parentheses are clustered at the state level for estimates in saturated model. 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. ^a 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. I also control for logged homicides per capita at the municipality level.

TABLE A-10. Effect of 2014 Term Limit Reform on Violence, sample from incumbency advantage estimates

	Dependent variable:					
	log(homicide per capita)	ihs(homicide per capita) ^a	(1)	(2)	(3)	(4)
Lag 7 years	-0.0485	-0.2498	-0.0459	-0.2996		
Lag 6 years	0.4916	-0.0194	0.5580	-0.0247		
Lag 5 years	0.0895	0.2731	0.1035	0.3449		
Lag 4 years	0.1085	0.3896	0.1344	0.5102		
Lag 3 years	0.1022	0.3614	0.1348	0.4864		
Lag 2 years	0.0263	0.2260	0.0359	0.3068		
Reform, time 0	0.0405	0.1371	0.0551	0.1830		
Lead 1 year	0.1994*	0.6290	0.2269*	0.8104		
Lead 2 years	0.2755*	0.7704	0.3049*	0.9843		
Lead 3 years	0.3493	0.7581	0.4027	0.9722		
Observations	8,756	6,972	8,756	6,972		
R-squared	0.7529	0.7564	0.6761	0.6844		
Mun. FEs	✓	✓	✓	✓		
Year. FEs	✓	✓	✓	✓		
State Controls ^b	✓	✓	✓	✓		
Cohort weighted	✓	✓	✓	✓		
Lag DV		✓		✓		

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. ^a Refers to the inverse hyperbolic sine transformation. ^b 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.

Appendix B. Figures

FIGURE B-1. Evolution of Homicides and Treatment Status by Mexican State, 2010-2018

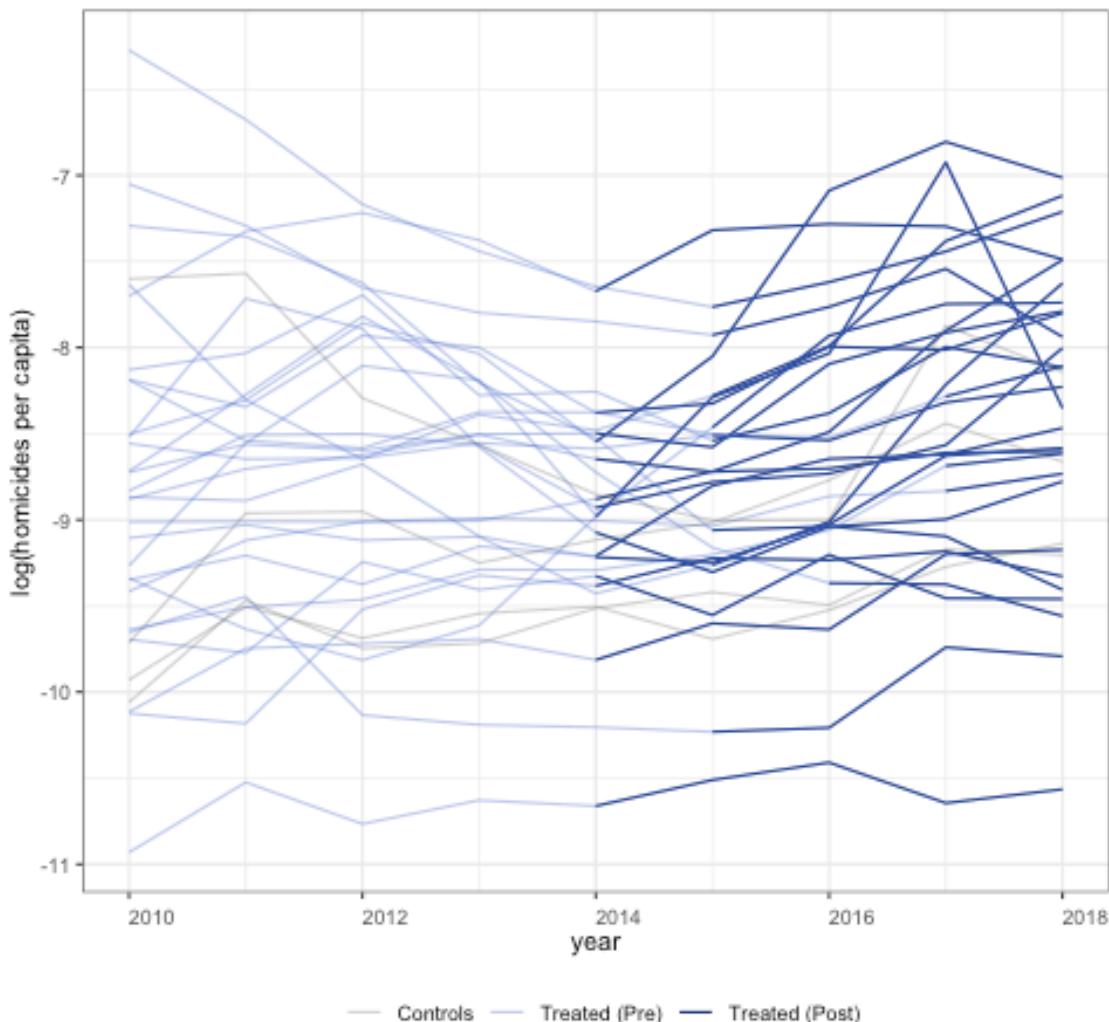
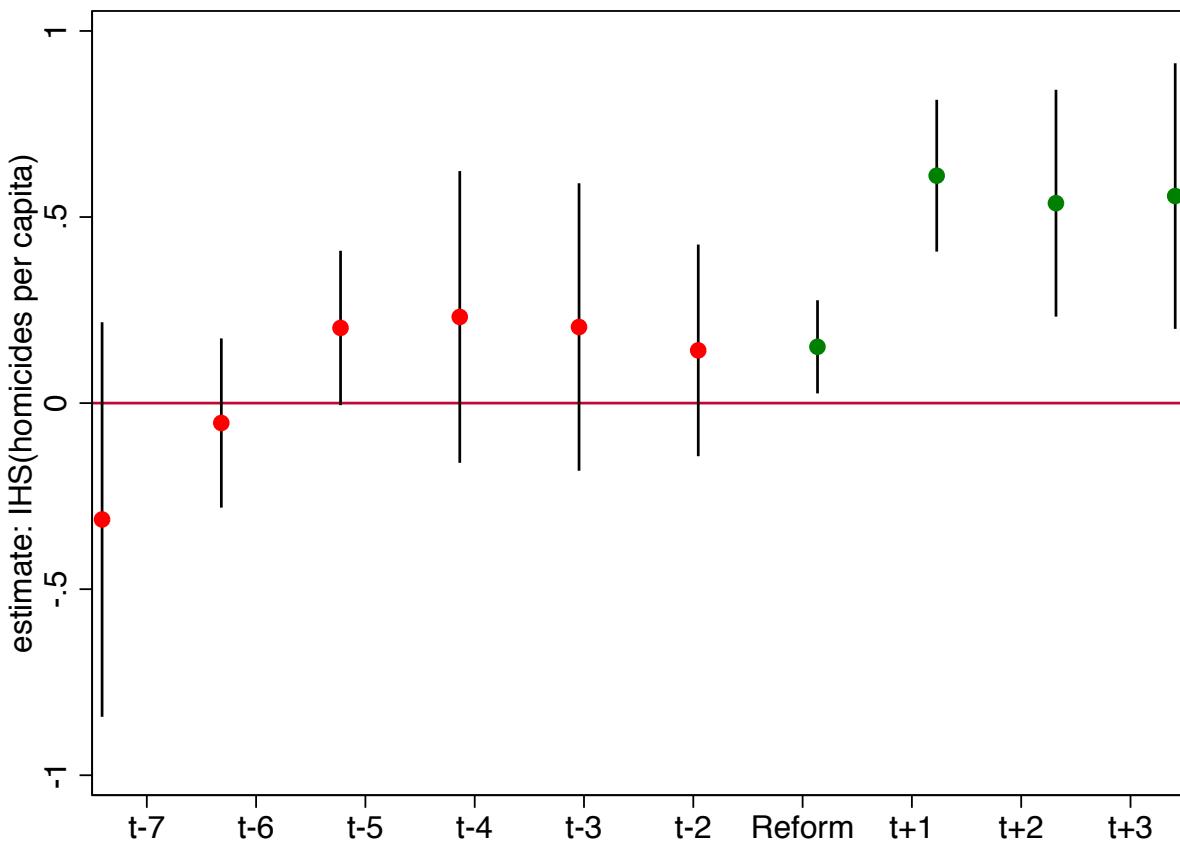


FIGURE B-2. Effect of Term Limit Reform of 2014 on Violence
-IHS transformation, 95% confidence intervals-



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

FIGURE B-3. Sensitivity Analysis for $\theta = \tau_3$ with monotonicity using
 $\Delta = \Delta^{SDD}(M)$

Figure A: Monotonically decreasing pre-trend violation

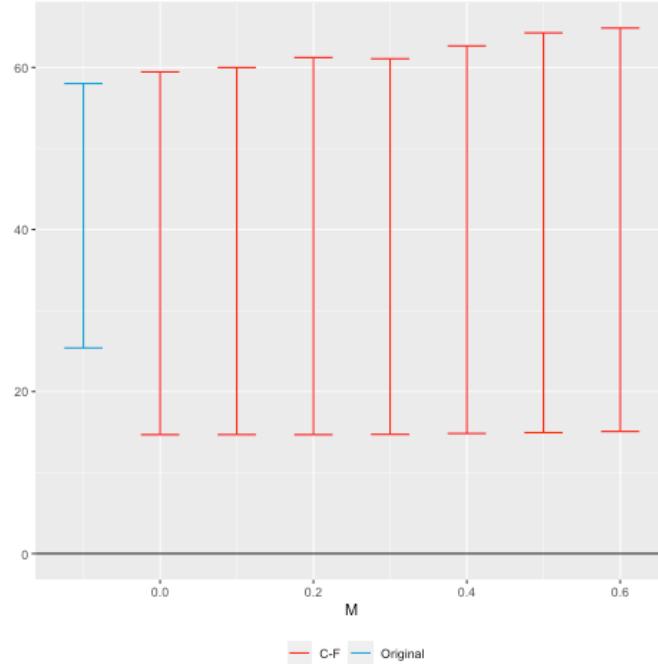
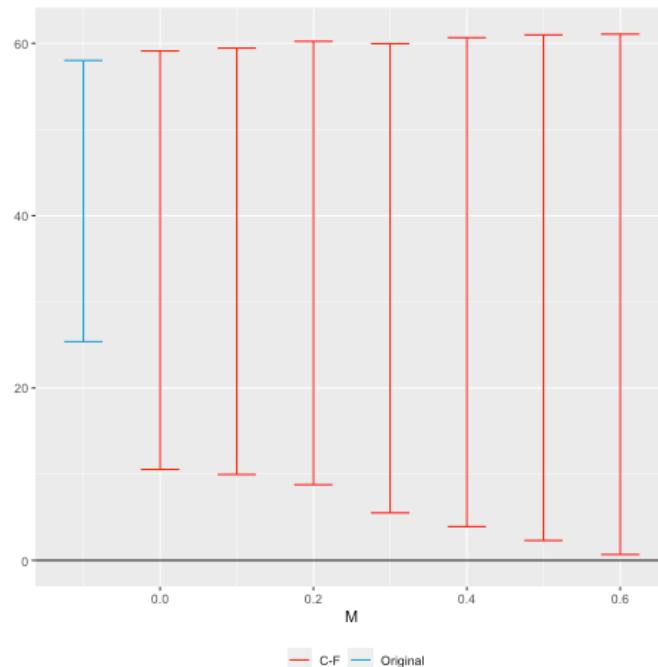
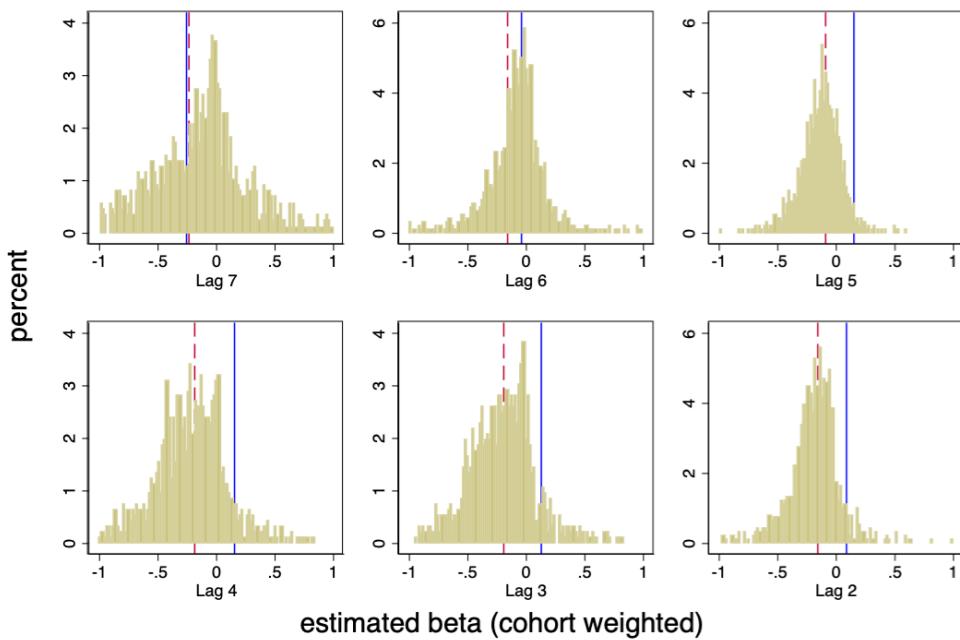
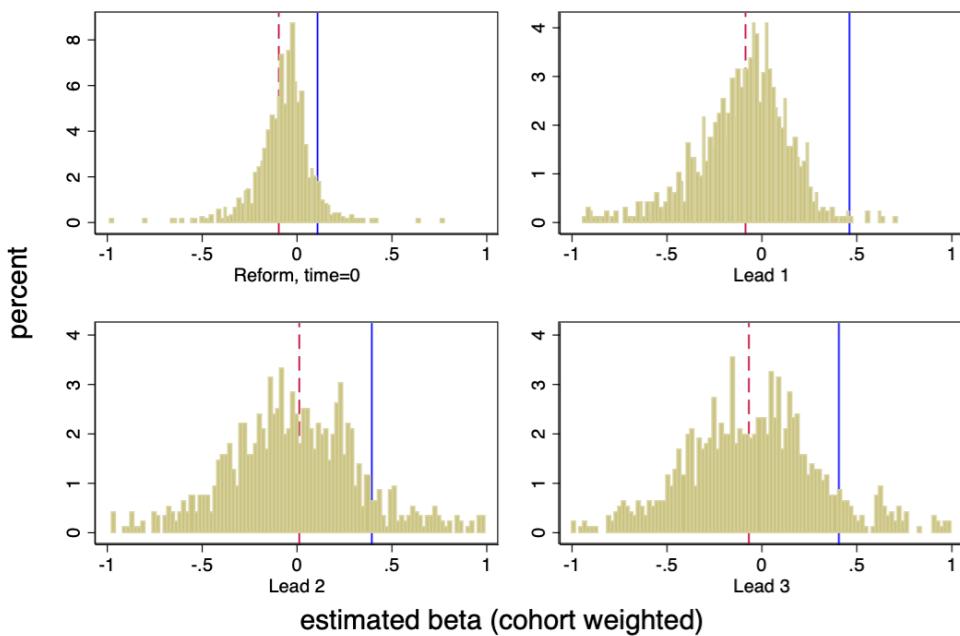


Figure B: Monotonically increasing pre-trend violation

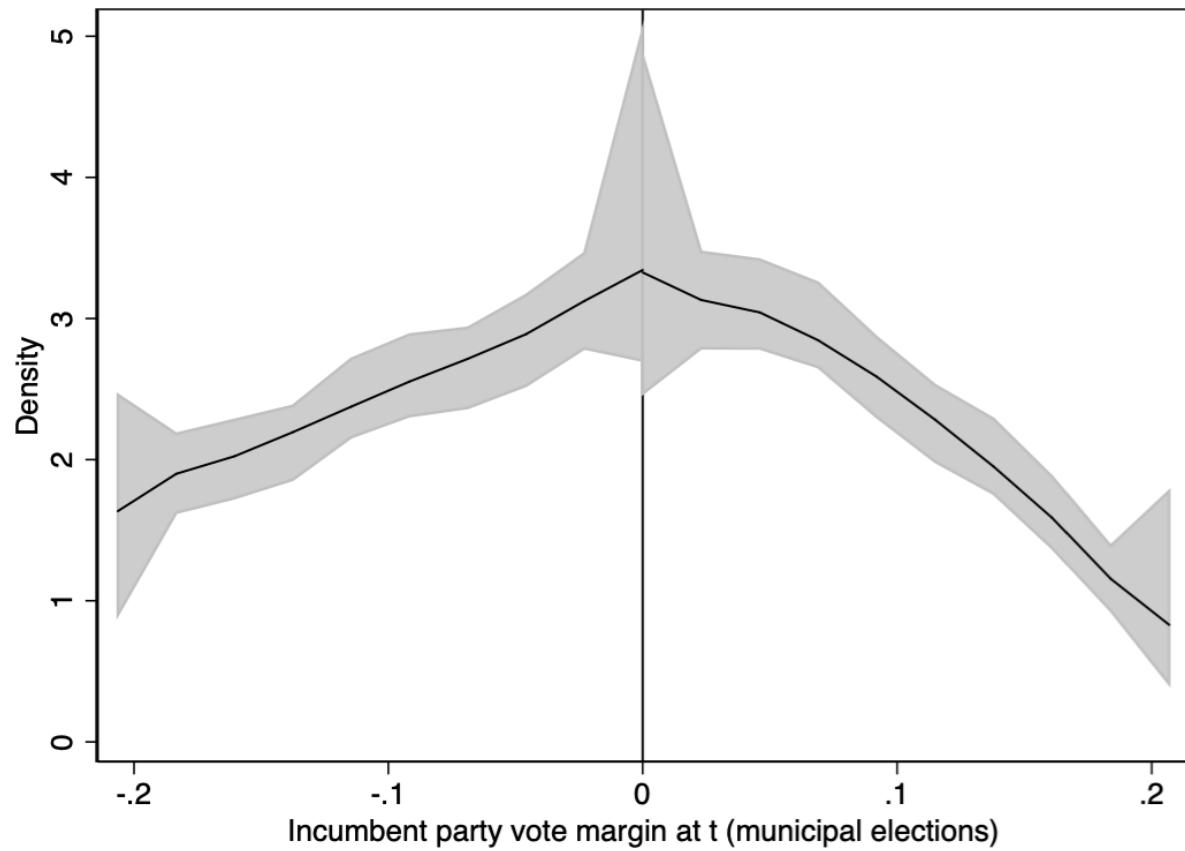


Note: M lower bound=0; M upper bound=0.5536. Blue confidence interval shows the most robust specification of the third lag after treatment in Table ?? column (2) with a point estimate of 0.4061. C-F stands for conditional fixed length confidence intervals given different values of M .

FIGURE B-4. Falsifying Term-Limit Reform Treatment Assignment**Figure A: Pre-treatment****Figure B: Post-treatment**

Note: Estimated (cohort weighted) beta coefficients distribution of 1,000 simulations following equation ???. For each simulation I carry a random Bernoulli draw with success rate equal to the proportion of treated states by the Electoral Reform by time period. Blue line displays the estimated effect of the electoral reform on logged homicides per capita of column (2) o Table ???. Red line shows the average estimated effect of the simulations.

FIGURE B-5. McCrary Test, quadratic polynomial



Appendix C. Scope conditions

I leverage the staggered implementation of the 2014 Electoral Reform in Mexico to test the effect that reelection incentives had on violence as a proxy of welfare, and public security provision as a proxy for incumbents performance in office (for details on the War on Drugs see Section ?? and on the Reform see Section ??). Mexico has specific traits that make it an ideal case to study the disconnection between reelection and political accountability. Furthermore, these characteristics help in the identification of the effect of the reform on violence and public security provision.

First, the War on Drugs that started in December 2006 after former President Felipe Calderon's military intervention against DTOs in the state of Michoacan, generated substantial violence and drug trafficking activity across states, municipalities and time (see Figure ?? on state-level homicides variation across time). Second, there is strong decentralization in the provision of public security. Since 2006 the military -army and marine forces- led the battle against DTOs. Locally, however, public security provision is in charge of municipal police forces, alongside state and federal police bodies for specific cases. A third characteristic is Mexico's strong party system and strong parties. Following ?, Mexico's party system is strong due to the stability pattern of competition (mainly a three party system during the last 30 years), strong partisanship in society, constant and legitimate elections -even during the autocratic totalitarian party-system; see ?-, and strong hierarchical party organization, i.e. the strong capacity parties have to constraint party members behavior. Furthermore, the central government in Mexico has various degrees of delegation of public security to mayors. Likewise, political parties have various degrees of delegation in terms of local electoral strategies and clienteles targeting. Deviation from party lines, however, is punished through removal of nomination candidacy. In a system where party switching is existing but small, party whip is strong both at the local executive level and local and federal legislatures. This party strength is found in both big and small parties. Thus, Mexico provides a case with parties with the capacity to monitor its members, but not necessarily the willingness to do it given electoral and clientelistic incentives.

As noted by ?, the democratic transition in Mexico since 2000 led paradoxically to the strengthening of clientelistic activities in Mexico, particularly in rural areas. As a result of the PRI of loosing the Presidency to the right-wing PAN in 2000 and 2006, and multiple legislative and local level elections since, the PRI shifted to vote buying behavior and different forms of clientelism. Moreover, with fiscal decentralization reforms from the early 90s, state and municipal executives developed clientelistic practices in exchange of federal funds, following party line (?). These funds allowed for the expansion of local clientelistic networks, a dynamic deeply studied by ?.

Fourthly, to avoid problematics in terms of identification related to heterogeneity in the preference of public goods, Mexico provides a case with an overwhelming demand for a specific good, peace, with potential variation in the degree of public security provision. Throughout the study time period from 2010 to 2018, violence is the primary concern in the country. In Section ?? I explore the role of differential public security demands from citizens, given that they may prefer peace across the country, but only public security deployment away rather than at home given strong externalities on fighting crime.

In short, we are facing a specific context: Mexico, characterized by high variation in crime and public security provision, a political environment with strong clientelistic parties, one major public good of interest -peace-, and legitimate democratic institutions.

Appendix D. Political Background of the 2014 Electoral Reform

It is important to understand the electoral reform political motivation, which involved a bargaining process with the opposition as well as the incorporation of pending laws from the political reform of 2012 under the PAN presidency. In the last year of Felipe Calderon presidency, a political reform was introduced in Congress including a term limit removal for all political actors. This reform was proposed and introduced during the electoral period of that year, increasing political tensions among the incumbent and opposition parties. The PRI -at the time part of the opposition and in control of the lower legislative house-blocked the reform and targeted reelection and the introduction of a second electoral round for the presidential election.

The 2012 presidential election was won by the coalition “Alliance for Mexico” with its presidential candidate Enrique Peña Nieto.⁴¹ However, the election suffered multiple electoral irregularities exposed by the national media. Among anomalies, opposition parties led by Andres Manuel Lopez Obrador -the second runner of presidential election and at the time presidential candidate for the left-wing party PRD-, argued PRI’s financial expenses above campaign caps and vote-buying practices including the distribution of gift cards from several institutions, including one of the country’s largest supermarket chains, Soriana, to voters in the State of Mexico and Mexico city. ⁴² finds not only an effect of the gift cards in PRI electoral return, but a magnitude that increases given proximity of electoral precincts to stores. While the special commission in the Chamber of Deputies found that the PRI invested \$5,200 million pesos in the campaign, an amount 15 times larger than the finance cap of \$336 million pesos, and the Inspection Unit from the Federal Electoral Institute (IFE for its acronym in Spanish) detailed the financing network where Soriana, Banamex, Monex and other firms were involved, the Unit did not point to any law violation. Without a public discussion, the ministers of the Federation Judicial Electoral Tribunal (TEPJF for its acronym in Spanish) deemed the appeal filed by the PRD unfounded and endorsed IFE’s criterion that the PRI was not obliged to register the agreement with Soriana, Banamex or Monex as campaign expenses.⁴² The final resolution of IFE’s Council and the TEPJF increased citizens and opposition mistrust on electoral institutions.

By early 2013, the Pena Nieto administration pushed an aggressive set of reforms to privatize the energy sector and modify the existent fiscal institutions in the country. To

⁴¹The PRI won with 38.2% of total votes, followed by the PRD with 32.6% and the PAN with 25.390%.

⁴²In his presentation, magistrate Manuel Gonzalez Oropeza stated that the analysis carried by the Inspection Unit showed that “neither the allegedly hidden financing was accredited individually or jointly” by any member of the PRI. See <https://www.jornada.com.mx/2013/01/24/politica/013n2pol> for more detail.

increase the probability of success, the PRI with the PAN and PRD, the three main political parties at the time, lead the construction of the Mexican Pact Accord, a series of roundtables intended to negotiate the energy sector reform along a set of structural reforms that had fail to pass through congress due to political gridlocks.⁴³ While the Electoral Reform was not under PRI's set of desired reforms, the opposition utilize it as a bargaining chip to approve those pursued by the PRI (?). By the end of May 2013, a roundtable to discuss the electoral reform was installed. Specifically, commitment 94 of the Pact Accord introduced reelection for discussion. However, due to lack of consensus, the Mexican Pact Accord did not submit an electoral reform proposal to Congress and left the bargaining process to the Senate. Two months later, on July 24, 2013, PAN and PRD pushed a political-electoral reform with 36 law changes that included the creation of a National Electoral Institute (INE for its acronym in Spanish) that would be in charge of federal, state and local elections and reelection for federal and local legislators and mayors. In the words of the current Chairman of the INE, Lorenzo Cordova, “[w]ith the reform, we went from an electoral model made up of a federal electoral system and thirty-two electoral systems, to a national election system in which a national authority and thirty-two local authorities coexist; a national administrative body was created, with clear powers and powers for local elections, and an authority was created that coordinates and guarantees the same parameters for the application of laws by local authorities, in order to standardize the conditions of the electoral competition in all elections and to promote a more transparent and impartial democracy throughout the country.⁴⁴

The reaction of governors was not smooth since reelection would limit the influence of governors and local elites on the electoral processes of the 32 states. Strong governors like the priista Eruviel Avila Villegas from the State of Mexico labeled this initiative as “democratic regression”.⁴⁵ Given the state-level opposition, Senate leaders from the PAN and PRD chose to approve the electoral reform in December 2, 2013, before the energy reform, and thus increased their political grip over the PRI.⁴⁶ By January 2014, PAN and PRD threatened to back the energy reform if the PRI did not push local state legislatures from

⁴³The PRD was no longer under the Lopez Obrador leadership who left the party to build a new left-wing party called MORENA once the leaders from the PRD agreed to form part of the Mexican Pact Accord.

⁴⁴From Cfr. Compendio de Legislacion Nacional Electoral, Mexico, INE, FEPADE, UNAM, TEPJF, Tomo II, 2014, p. XXXIX.

⁴⁵For more detail see “Regresion democratica, creacion del Instituto Nacional de Elecciones, La Jornada, 30 de octubre, 2013, p. 15. <https://www.20minutos.com.mx/noticia/b82075/regresion-democratica-creacion-del-instituto-nacional-de-elecciones/>

⁴⁶The electoral reform approved by the Senate included reelection for federal legislators and governors for up to 12 years, as well as reelection for local legislators and mayors. Congress, however, modified the proposal removing governors reelection. The electoral reform was approved with 408 votes in favor and 69 against in Congress on December 3, 2013, and weeks later by the Senate due to modifications of the original reform project.

approving the electoral reform, a constraint imposed by the Mexican constitution, which at the time where blocking the reform given pressure from various PRI governors.⁴⁷ The political gridlock led former President Pena Nieto to “exhort” local legislators to approve the electoral reform. On January 31, 2014, the reform was promulgated by the President and contained three main changes: (1) the creation of the INE; (2) removal of term limits of mayors, local and federal legislators for up to 2 terms; (3) the introduction of a “party-lock” where mayors or legislators who wish to reelected could not switch parties. As a result, while voter accountability increased, party control remained unchanged since candidate nominations and campaign funding still depended strongly on them.

⁴⁷For more detail see Enrique Mendez, “PAN: estancados, cambios en materia politica por presion de los gobernadores, La Jornada, 9 de enero de 2014, p. 5., <https://jornada.com.mx/2014/01/09/politica/005n3pol>.

Appendix E. Sistema Nacional de Seguridad Pública Homicide Database

The Sistema Nacional de Seguridad Pública Homicide Database database is fed by the reports of local prosecutors offices. Thus it inherits the same flaw of INEGI's second homicide database counting cases rather than victims. The difference is that INEGI reports figures in an annual basis (and a six month delay) while the SNSP reports data in a monthly to monthly basis. In 2015, the SNSP updated their methodology to classify homicide related criminal investigations.⁴⁸ This new methodology captured data from 2015 onwards, while the old methodology stopped capturing data in 2018. Two problems arise with the SNSP database, however. First, given the data collection effort to produce monthly estimates, there is high data inaccuracy. Second, this inaccuracy is tied to governors' political agenda. ? note, for example, a crime rate drop of 28% three months prior to the governor election of Veracruz in 2010. Three months after the election, cases with pre-election date are added by the end of that year. Noteworthy, this pre-election decrease in homicides is not observed in INEGI's homicide related deaths database. States manipulate statistics so the story is the one governors want to tell.

⁴⁸The new methodology allows to differentiate crimes against women, and add complementary information from emergency numbers 911. There is no agreement if data captured through the new methodology can be paired with the old one due to differences in the filing of criminal cases.

Appendix F. Validating the no-anticipatory assumption

One way to address the no-anticipatory behavior is to assume that it can only occur in a fixed window prior to the electoral reform, say of one year, especially since the reform was announced in early 2013. However, for states that implemented reelection later this fixed window assumption would not suffice. In other words, only those early adopters of the reform would show unbiased estimates. Late adopters, however, would anticipate the term limit removal an act accordingly biasing the results upwardly.

Another way to assess the no-anticipatory behavior from incumbents in this setting is test whether early vs late adopters differed in their estimated effects. Appendix Figure ?? presents ? “event-by-event analysis” that estimates treatment effects for each treated Mexican state (28 states) in the sample. States color differs if they are early (2015-2016, red color) or late adopters (2017-2018, blue color). Specifically, I create state-event specific panel datasets and estimate state-specific estimates using separate regressions for each state. Each state dataset contains the treated state and all other states that never received treatment or received treatment after the sample window of $t + 1$. For each state I estimate the following DiD regression:

$$(F.1) \quad y_{mt} = \mu_m + \mu_t + \gamma Reform_{mt} + \epsilon_{mt}$$

where $Reform_{mt}$ is an indicator variable that takes the value of 1 if the state implemented reelection. If there was evidence of strong incumbent anticipatory behavior, conditional on state covariates such as governor winning margin and alignment with Federal Executive, we would expect strong color clustering across similar estimated effects. In other words, if there is an endogenous response by states to implement the electoral reform, we would see that the positive (or negative) treatment effect would be only by those that implemented reelection earlier or later (events with the same color would be clustered). However, as seen in Appendix Figure ??, this is not the case: there is wide variation in estimated coefficients across early (red) and late (blue) adopters of the reform, conditional and unconditional on state covariates.

Given the proportion of states with null-effects (16 out of 28) in Figure ??, we could be concerned that the average treatment effect of all these events would yield null results. For robustness, Appendix Figure ?? presents the “stacked dataset analysis” from ?. I take each of the “event-by-event” datasets from Figure ??, stack estimates by cohort and estimate one set of lead and lag variables not using prior treated units as controls. Panel B shows that conditional on state-level covariates, there is strong evidence of pretrends as well as strong positive effect of reelection on logged homicides per capita. While there might be

evidence of an increasing dynamic trend, it disappears if we include the lag of the main outcome.⁴⁹

FIGURE B-6. “Event-by-event analysis” following ?
-95% confidence intervals-

Figure A: w/o covariates

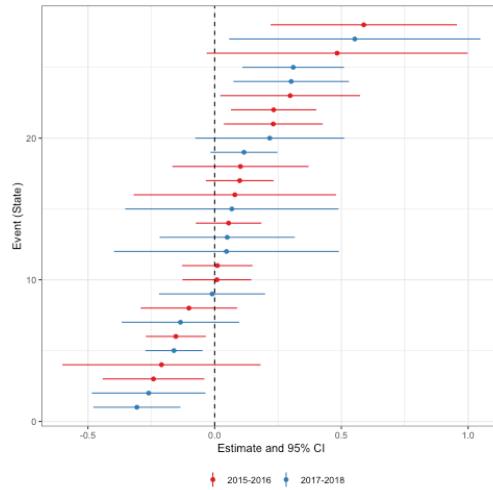
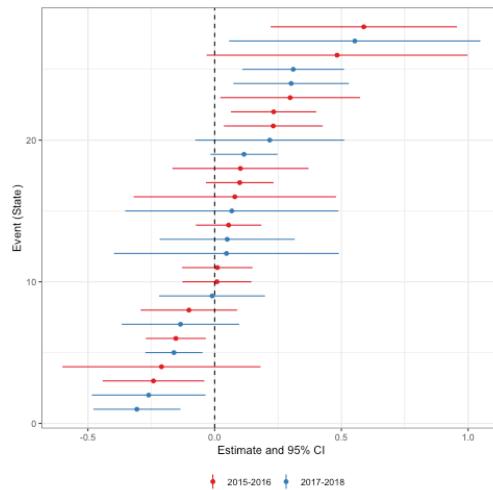


Figure B: with covariates

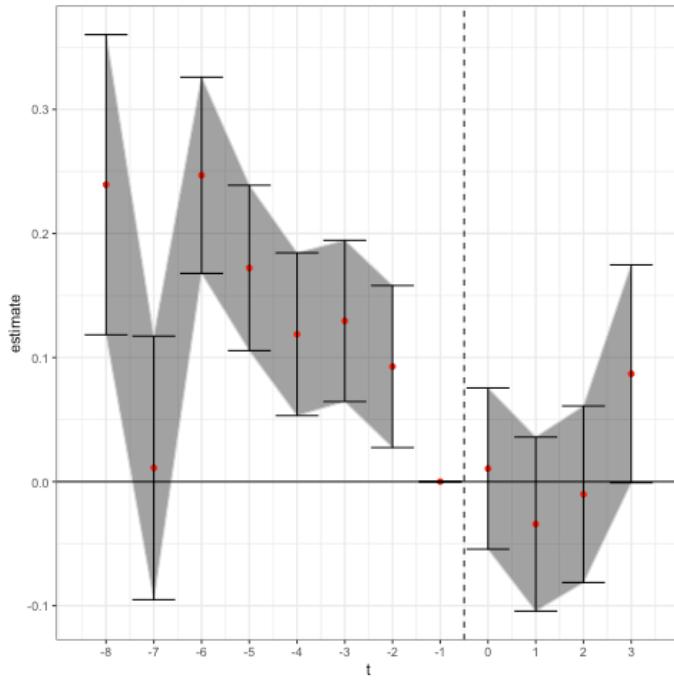


Note: Estimate separate treatment effects for each event, i.e. each Mexican state in the sample. Each event dataset contains the treated state and all other states that never received treatment or received treatment after the sample window ($t + 1$).

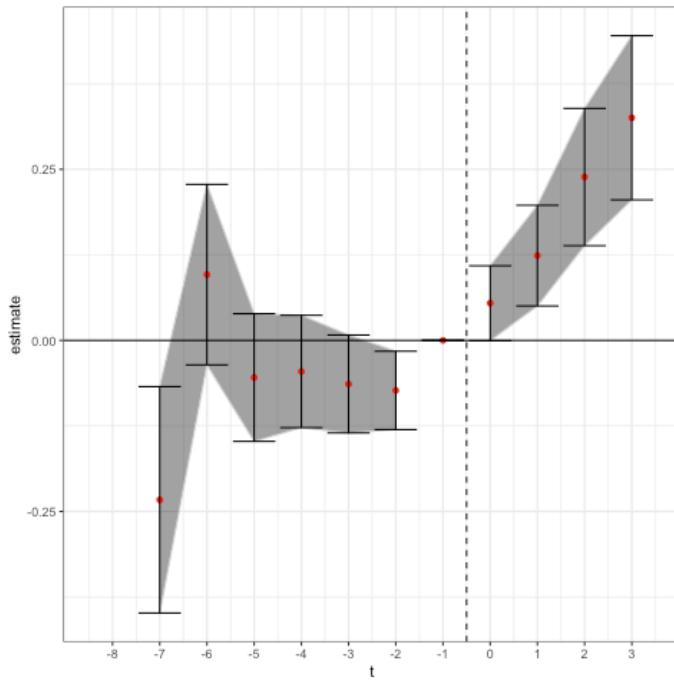
⁴⁹Results available upon request.

FIGURE B-7. “Stacked dataset analysis” following ?
-95% confidence intervals-

Figure A: w/o covariates



B: with covariates



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.

Appendix G. Sensitivity Analysis on violations to the parallel trends assumption

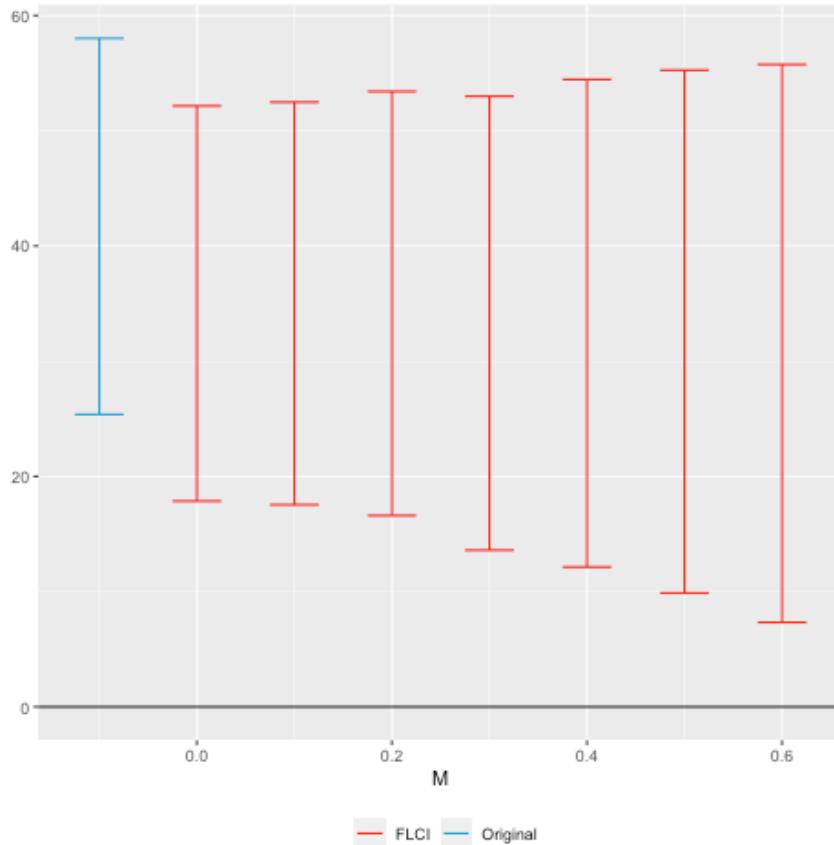
As an additional robustness check on parallel trends, Figure ?? reports confidence sets under different assumptions about the set of possible violations of parallel trends following ?. This allows the reader to evaluate what assumptions need to be imposed in order to obtain informative inference in this setting. In particular, I set Δ -i.e. a set of possible differential trends⁵⁰ equal to $\Delta^{SD}(M)$ that relaxes the assumption of linear differences in trends by stating that the slope of the differential trend among treated and non-treated municipalities can change by no more than M in consecutive periods. Figure ?? shows robust confidence sets at different values of M for the third lag after treatment.⁵¹ I calibrate M through benchmarking, that is from the largest change in slope between periods among non-treated states, and use this as the upper bound of the interval to benchmark M . In this case M upper bound is equal to 0.5536. As noted in Figure ??, we can reject the null hypothesis equal to zero even if we allow for changes in slope more than 0.55 times as large as the upper bound for the largest changes observed for the placebo group. In other words, when we allow for linear violations of parallel trends the figure does not change dramatically and intervals do not touch 0.

Now, I incorporate context specific knowledge into the restrictions used in the sensitivity analysis. Appendix Figure ?? develops such exercise and carries a similar exercise to that shown in Figure ??, but imposing that violations of parallel trend be (weakly) decreasing (panel A) or increasing (panel B). For the former, prior to the Electoral Reform, we notice a downward-sloping pre-trend in homicides. It may be valid to assume that such trend would have continued absent treatment. Thus, we could impose a negative shape restriction, i.e. a monotonically decreasing one. For the latter, we assume that pre-trend violation follows an upward slope where homicides would increase in treated states relative to control ones. In such setting, those that enacted the Electoral Reform in earlier years would have done so if they thought the reform would be a policy instrument to tackle such violent setting. In both cases (monotonically decreasing and increasing) the lower bound of the robust confidence never touches 0 (null effect), increasing assurance on the found estimates with the OLS specification. In both Figure A and B, a positive effect of reelection on logged homicides per capita is found, across multiple values of M including its upper bound. Furthermore, with the added monotonicity restrictions, the lower bound of the robust confidence in Panel A (B) falls below the lower bound of the OLS confidence

⁵⁰ $\Delta = \{0\}$ in the presence of pretrends, for example.

⁵¹Similar results are found for the first year of treatment, as well as one and two lags after. Results available upon request.

FIGURE B-8. Sensitivity Analysis for $\theta = \tau_3$ using $\Delta = \Delta^{SD}(M)$



Note: M lower bound=0; M upper bound=0.5536. Blue confidence interval shows the third lag after treatment in Table ?? column (1) with a point estimate of 40.61%. FLCI stands for fixed length confidence intervals given different values of M .

interval. This is intuitive, since the restriction that is decreasing (increasing) implies that the OLS coefficient is weakly upward bias.

Appendix H. Regression Discontinuity Design of close elections, before and after the Term Limit Removal Reform of 2014

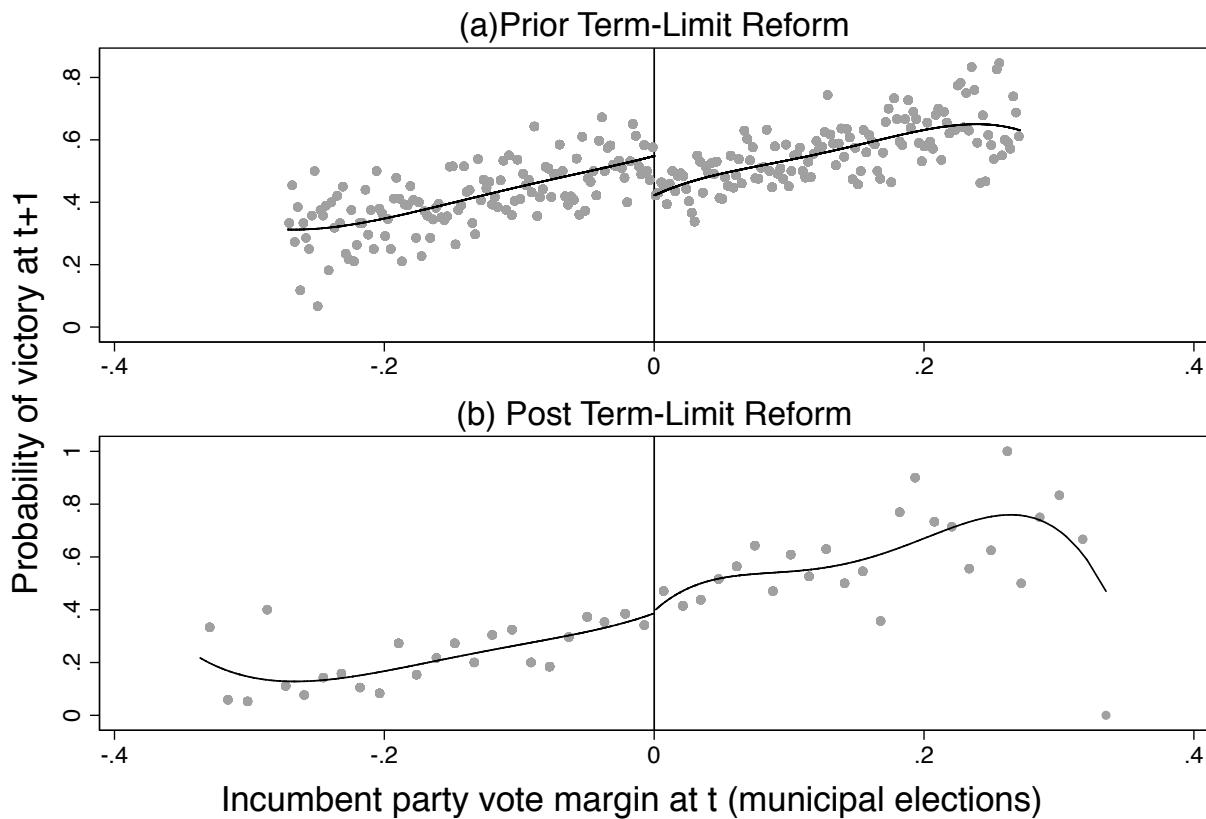
I begin by visualizing the effect of the reform on incumbency advantage. Figure ?? presents the RDD estimate of close elections on incumbency advantage within an optimal bandwidth distance h from the cutoff (0), and a quadratic polynomial:⁵² Panel A shows a comparison of municipalities with incumbents at $t - 1$ that barely won to those that barely lost in t on the probability of electoral victory at $t + 1$,⁵³ taking into account all elections from 1979 to 2014 (i.e. prior to the term-limit reform); Panel B shows the same comparison but restricting the sample to municipalities that implemented reelection after 2014. I do not consider those municipal elections that after 2014 did not implement reelection. Table ?? shows RD estimates using multiple polynomial functional forms. Even though RD estimates are biased in this setting, especially for Panel B in Figure ?? (and even columns in Table ??) since in the presence of staggered treatment timing and heterogeneous treatment effects across cohorts are not causally interpretable since we are considering both early vs late adopters of the reform on the treated,⁵⁴ they provide a striking depiction of what occurred before and after the electoral reform.

⁵²A triangular kernel is used. Results are almost unchanged when using other polynomial functional forms.

⁵³Incumbency advantage measure following ?.

⁵⁴The next iteration of this working paper will show this proof in the Appendix for RD designs.

FIGURE B-9. Effect of Term Limit Reform of 2014 on Incubency Advantage, quadratic polynomial



Note: Regression Discontinuity design of close elections on incumbency advantage. Panel (a) considers all elections from 1979 to 2014. Panel (b) considers all elections after 2014 for municipalities that implemented reelection.)

Before the reform, there was a negative statistical significant difference between municipalities that barely won and lost on the probability of success in the next election. This incumbency *disadvantage* aligns strongly with a similar result found by ? for the case of Mexico. ? find that an incumbent party that is barely reelected suffers a reduction in the probability of winning the following election of 28 percentage points. In contrast, I find a reduction between 10.7 to 11.32 percentage points, a finding that considers 20 more years of elections since ? cap their data from 1997 to 2009, while I consider elections since 1979 to 2014.

More interesting is the finding that after reelection takes place the previous incumbency disadvantage disappears as noted by a positive and non-statistical significant difference between municipalities that barely lost and won an election. This initial RDD results provide suggestive evidence that the electoral reform generated an increase in the probability of victory in the next election for municipalities that barely won an election relative to those that barely lost. The next section addressed potential biases and presents robust findings of this same evidence.

TABLE A-11. Regression Discontinuity Design of Close Elections on Incumbency Advantage, comparing pre and post-Term Limit Reform estimates

	Dependent variable:							
	linear polynomial		quadratic polynomial		cubic polynomial		quartic polynomial	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Probability of victory at t+1 ^a	-0.1075*** (0.0217)	0.0750 (0.0636)	-0.1114*** (0.0274)	0.0595 (0.0846)	-0.1130*** (0.0330)	0.0639 (0.0925)	-0.1132*** (0.0387)	0.0565 (0.1182)
Observations	8,623	890	10,138	955	10,849	1,116	11,262	1,064
Post Reform (2014)		✓		✓		✓		✓

Notes: Standard errors in parentheses, with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test. Optimal bandwidth following ? ^a Incumbency advantage measured following ?.

Appendix I. Wash one's hands of security provision

“I am innocent of the blood of this just person: see ye to it.”
Pontius Pilate, Matthew 27:24, King James Version.

An alternative mediator to the increase of violence is local executives refusing to take responsibility or be involved any further in the provision of public security. With reelection, non-term limited incumbents may choose to sign security cooperation agreements with upper administrative levels, including those with the state or the federal government. Some of these agreements may imply a full delegation of public security provision to governors or the president. In other words, mayors in line for reelection would *wash their hands of* public security provision to refuse to take blame for violence. Those new in charge -governors or the President- may decrease public good provision or see an increase in violence since they are misinformed of local crime and how to deter it (?) or are simply not willing to tackle it given party electoral incentives describe in Section ??.

Since the presidency of Felipe Calderon (PAN, 2006-2012), there have been five broad types of security agreements between municipalities and upper level governments: (a) agreements between municipalities (e.g. to create metropolitan police forces), (b) between municipalities and the state governor (e.g. Central Command Agreements), (c) between municipalities and the federal government, (d) agreements with multiplicity of executive actors (various municipalities, states, with or without the Federal government), and (e) agreements with other branches of government, including legislative and judicial ones, also at various levels of government. While conditions vary by agreement, overall they may include any (or all) of the following items: security coordination, transit, security prevention, training, sharing of equipment and technology, research capacity, analysis and intelligence, and creation of unified criteria and procedures of the public security institutions and laws.

Of all agreements, the creation of a state-level Police Central Commands (*Mando Único Policial*) has been the most prevalent in Mexico. The premise is the unification of municipal and state police forces, i.e. the centralization of public security under direction of the governor. During Calderon's presidency, Central Commands were intended to abolish municipal police forces. Later, Pena Nieto's administration proposed the creation of Unified State Forces to transition from 1,800 municipal police bodies to 32 police corporations. However, a proposed constitutional modification was stopped in the Senate since it did not reach the necessary three quarters of legislators to approve the Constitutional modification. While it did not achieve constitutional jurisdiction, 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.⁵⁵

Appendix Figure ?? shows that the Term Limit Reform did not increase the likelihood of municipalities on signing security cooperation agreements. Prior to treatment we observe pretrends. Once the reform is implemented there is actually a decrease in the probability of signing an agreement, but non-significant. For those municipalities with four years of implementation we see an increase in the likelihood of having an agreement, but it is quite noisy.⁵⁶

Now, while there is no effect of reelection on mayors refusing to take care of public security matters, we would expect that municipalities without Police Central Command (or other cooperation agreements) would experience lower welfare distortions (i.e. lower violence) since they would be held more accountable by citizens on local security provision. This is exactly what we find. Appendix Figure ?? estimates the effect of the Term Limit Reform on logged homicides per capita on two subsamples: one the one hand, municipalities with security agreements prior to the implementation of the Reform (points with black lines representing 95% confidence intervals); on the other hand, municipalities without such agreements (squares with dashed lines representing 95% confidence intervals). As noted, both subsamples show positive effects of the reform on violence, with the exception of the first year of treatment for those without security cooperation agreements. More importantly, there is significant difference between both subsamples for all the years after the reform was implemented: municipalities with security cooperation agreements such as *Mando Único Policial* show a higher increase in the percentage of homicides per capita in treated municipalities relative to non-treated.

One important concern could arise: why don't we see a null effect on municipalities without security agreements? Would not these municipalities serve as placebos? To answer this there are three important caveats about security cooperation agreements -particularly Central Command Agreements- that needs to be noted.

First, not all Central Command Agreements imply a *de jure* delegation of municipal public security provision to the state. There is wide variation of what central command implies, and could take all or any of the items mentioned before, from security provision to

⁵⁵There is a judicial discussion in Mexico on the legitimacy of centralized state level agreements, particularly that of the Centralized Command. In the framework of the Mexican federal pact, Article 21 of the Constitution makes public ministries (*Ministerios Pùblicos*) the actor in charge of prosecution. However, they are left aside in most security cooperation agreements. Furthermore the Constitutional figure of the “free municipality”, makes public security centralization something unfeasible and unconstitutional (?). As noted by Article 115, fraction III, item “h” of the Constitution, municipalities are the first autonomous constitutional bodies and are granted express powers to provide public security service. For more details see <https://aristeguinoticias.com/0608/mexico/el-inconstitucional-mando-unico-articulo/>.

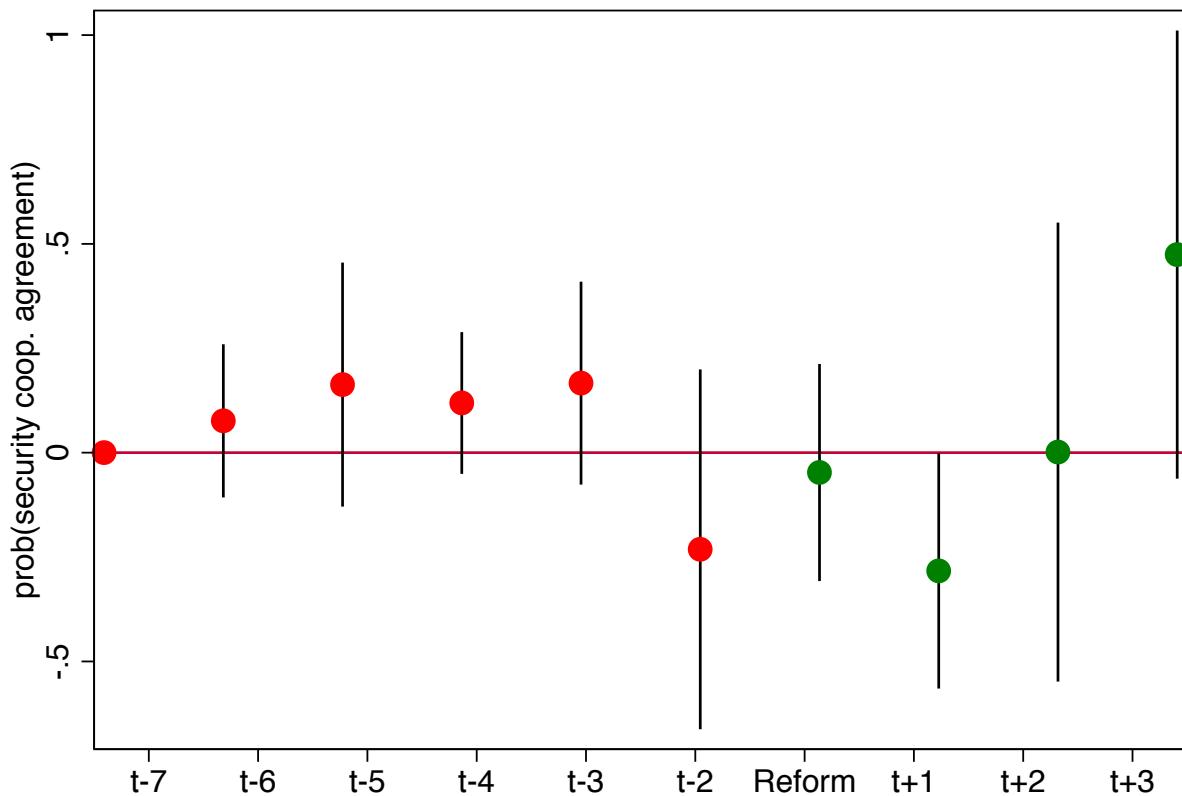
⁵⁶For robustness, Appendix Figure ?? shows main estimates on the effect of the reform on violence robust to controlling for security cooperation agreement.

intelligence. In other words, *Mando Único* in one state may not imply the same operative features in another state. For instance, as noted by data from the National Census of Municipal Governments and Territories of the City of Mexico from 2011 to 2019, of all the municipalities that state they had a Central Command Agreement, only 72.6% said the agreement included the delegation of public security.

Second, even with the existence of a security cooperation agreements that delegates all security provision to higher level federal authorities, citizens could still blame mayors for not lobbying federal authorities for public good provision in their municipalities. Moreover, citizens may simply not understand the terms and conditions of cooperation agreements, blaming mayors still for local level violence. Lastly, *Mando Único Policial* has not led to a decrease in crime incidence, especially since municipal police forces require a distinct training and objectives to those of states (?), and has been deemed as either non-existent or a failure.⁵⁷ Given these caveats, municipalities without security cooperation agreements should not be deemed as placebos, but should show a significant difference in the level of violence as found in Appendix Figure ??.

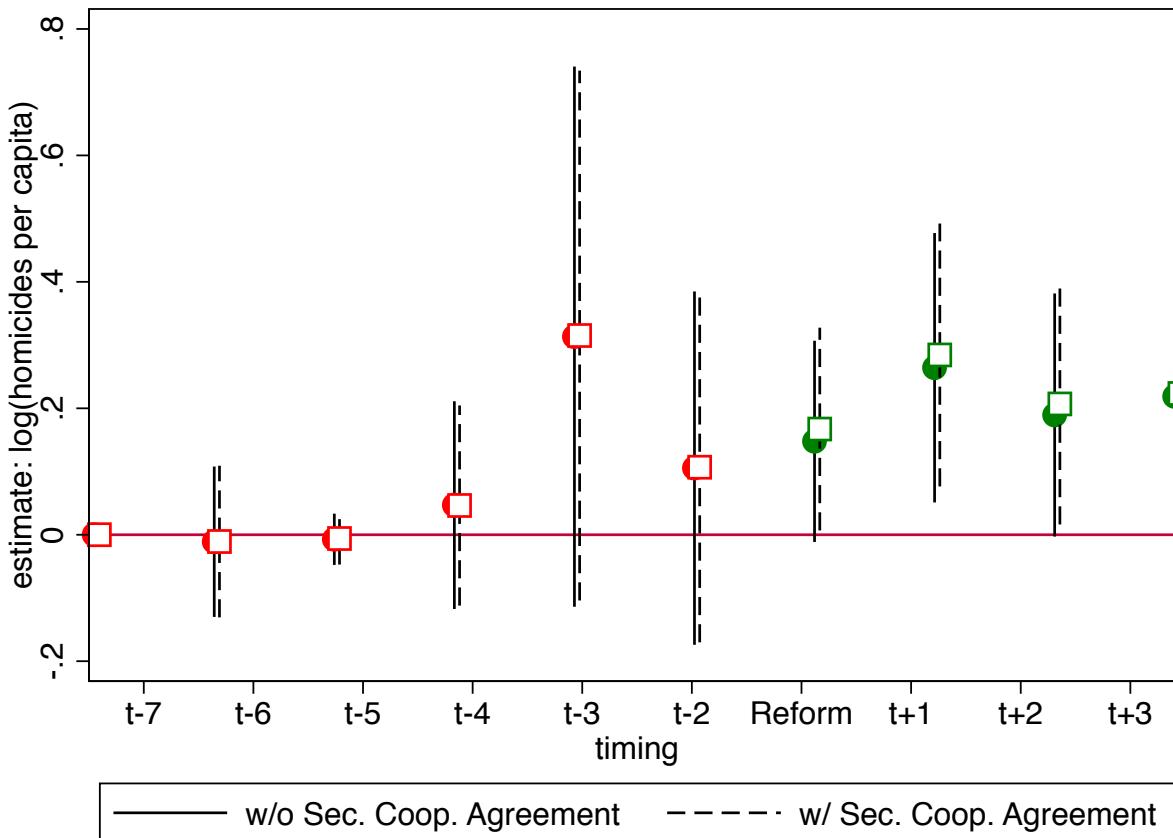
⁵⁷For more detail, see the article “Mando Único Policial: el modelo fracasado” from <https://www.proceso.com.mx/515386/mando-unico-policial-el-modelo-fracasado>

FIGURE B-10. Effect of Term Limit Reform on Probability of Signing a Security Cooperation Agreement



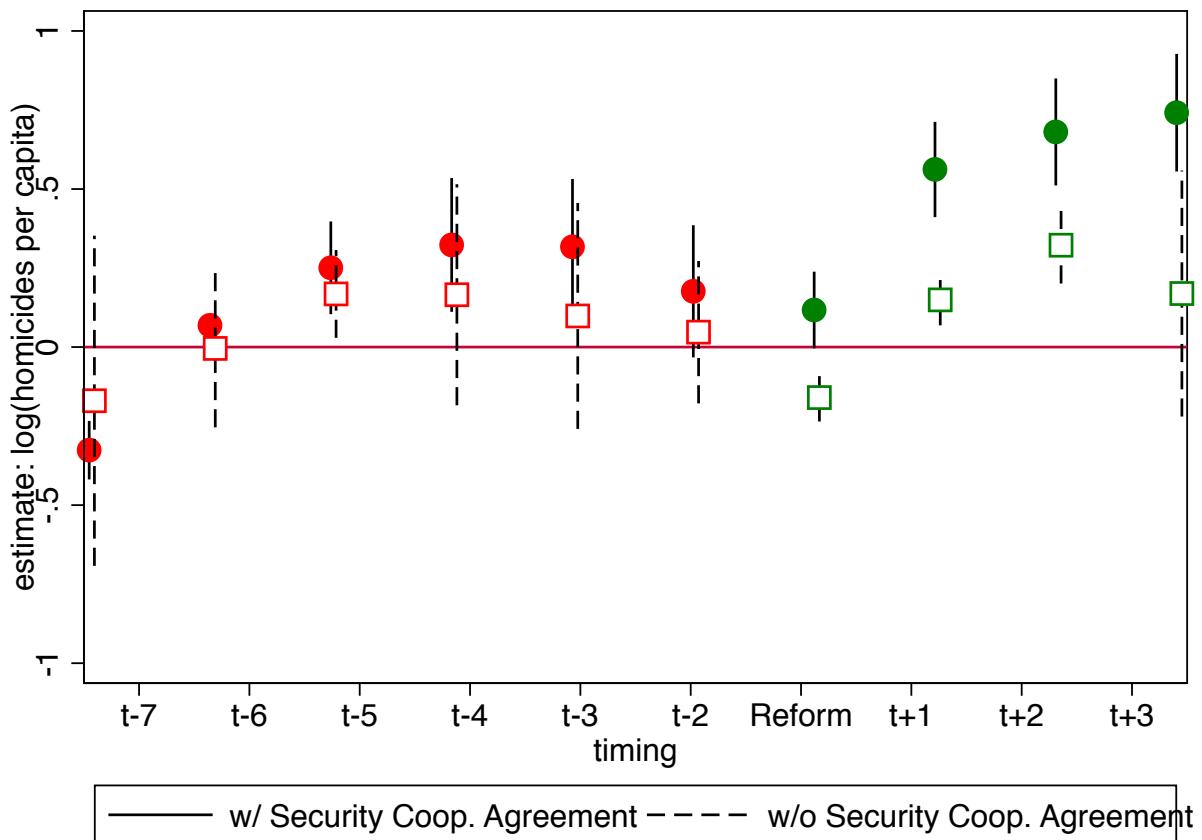
Note: Figures shows the estimates of equation ?? using as outcome an indicator variable of whether a municipality signed a security cooperation agreement according to information from the National Census of Municipal Governments and Territories of the City of Mexico, 2011-2019. The Figure shows IW estimators with 95% confidence intervals.

FIGURE B-11. Effect of Term Limit Reform on Violence, conditional on Signing a Security Cooperation Agreement



Note: Figures shows the estimates of equation ?? conditional on whether a municipality signed a security cooperation agreement one year prior to treatment according to information from the National Census of Municipal Governments and Territories of the City of Mexico, 2011-2019. The Figure shows IW estimators with 95% confidence intervals.

FIGURE B-12. Effect of Term Limit Reform on Violence, by security cooperation agreement



Note: Line shows estimates for the subset of municipalities with a security cooperation agreement prior to treatment. Dashed line shows estimates for the subset of municipalities without a security cooperation agreement prior to treatment. The Figure shows IW estimators with 95% confidence intervals.

Appendix J. New Tables and Story

TABLE J-1. Effect of Centralization of Public Security on Violence

	log(homicides per capita)	IHS(homicides per capita)	
	(1)	(2)	(3)
	(4)		
Sec. Coop. Agreement (Centralization)	-0.0243* (0.0131)		-0.0234 (0.0300)
Sec. Coop. Agreement (Centralization) in t-1		-0.0309** (0.0127)	-0.0539* (0.0317)
Observations	17,750	15,558	17,750
R2			15,558
Controls	✓	✓	✓
Mun. FE	✓	✓	✓
Year FE	✓	✓	✓
Mun. Cluster S.E.	✓	✓	✓

Notes: Municipal clustered standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test.

TABLE J-2. Effect of Reelection Incentives on Security Provision Centralization

	Agreement A in t	in t+1	in t+2	in t+3	Agreement B in t	in t+1	in t+2	in t+3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Term Limit Reform	-0.1076 (0.0692)	-0.2889*** (0.0719)	-0.2646** (0.1043)	-0.1039 (0.1013)	-0.0842 (0.0567)	-0.2386*** (0.0649)	-0.2454** (0.1047)	-0.0950 (0.1070)
Observations	14,767	14,767	12,919	11,071	15,658	15,658	13,661	11,664
R2								
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-3. Effect of Reelection Incentives on Security Provision Centralization, Wild Bootstrap

	Agreement A in t	in t+1	in t+2	in t+3	Agreement B in t	in t+1	in t+2	in t+3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Term Limit Reform	-0.1076 (0.0692)	-0.2889*** (0.0719)	-0.2646** (0.1043)	-0.1039 (0.1013)	-0.0842 (0.0567)	-0.2386*** (0.0649)	-0.2454** (0.1047)	-0.0950 (0.1070)
Observations	14,767	14,767	12,919	11,071	15,658	15,658	13,661	11,664
R2								
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓	✓	✓	✓	✓
Wild CI	[-0.176, -0.001]	[-0.344, -0.138]	[-0.393, -0.070]	[-0.255, 0.056]	[-0.139, 0.013]	[-0.292, -0.090]	[-0.367, -0.038]	[-0.239, -0.086]

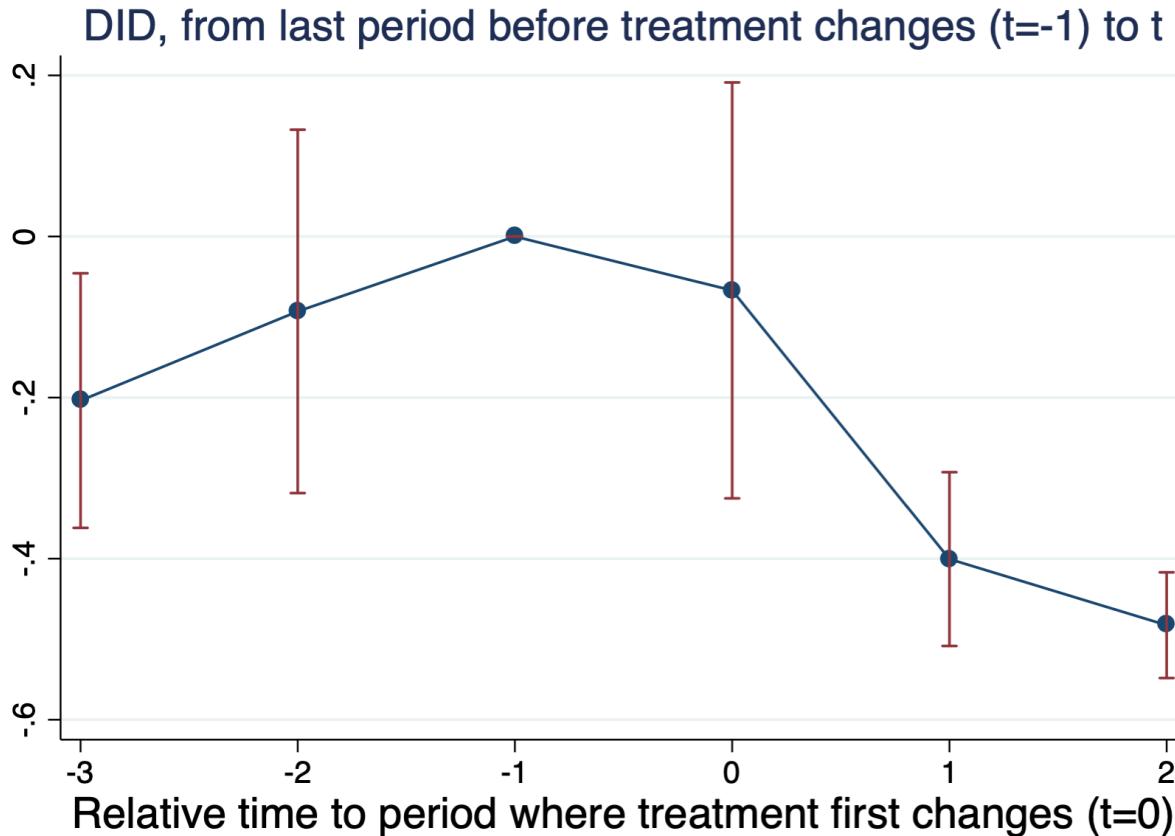
Notes: Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-4. Effect of Reelection Incentives on Security Provision
Centralization, Wild Bootstrap

	Agreement A in t	in t+1	in t+2	in t+3
	(1)	(2)	(3)	(4)
Term Limit Reform	-0.1098 (0.0697)	-0.2909*** (0.0720)	-0.2623** (0.1056)	-0.1018 (0.1016)
Observations	14,263	14,263	12,498	10,733
R2				
Controls	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓
Wild CI	[-0.176, -0.001]	[-0.344, -0.138]	[-0.393, -0.070]	[-0.255, 0.056]

Notes: State clustered standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test.

FIGURE J-1. Effect of Reelection Incentives on Centralization of Public Security (Sec. Agreement A)



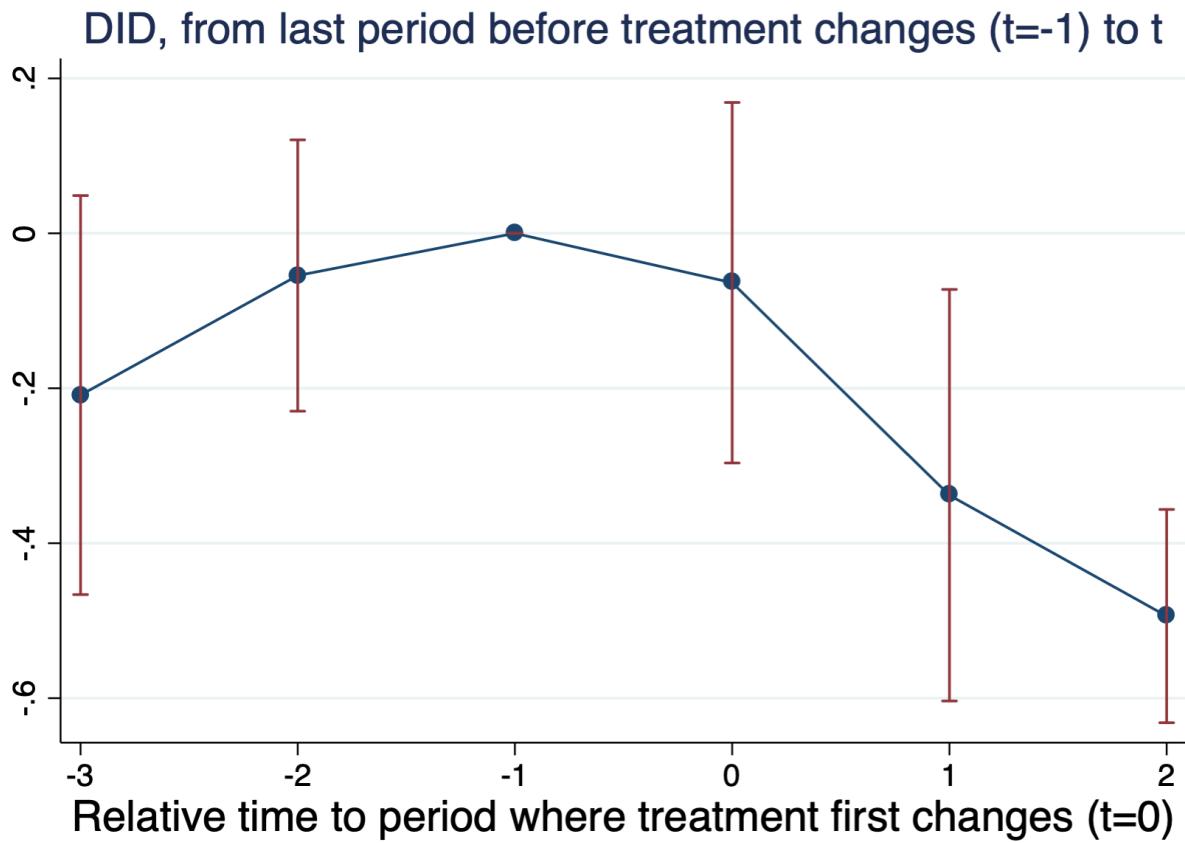
Note: ? two-way fixed effect model correction. The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

TABLE J-5. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements, ? correction

	Dependent variable:	
	Agreement A (1)	Agreement B ^a (2)
Lag 5 years	-0.023 (0.021)	-0.045 (0.071)
Lag 4 years	0.009 (0.038)	0.033 (0.048)
Lag 3 years	0.063 (0.081)	0.098 (0.080)
Lag 2 years	0.093 (0.115)	0.055 (0.089)
Reform, time 0	-0.067 (0.132)	-0.063 (0.119)
Lead 1 year	-0.401*** (0.055)	-0.337** (0.136)
Lead 2 years	-0.483*** (0.033)	-0.493*** (0.071)
Controls ^b	✓	✓

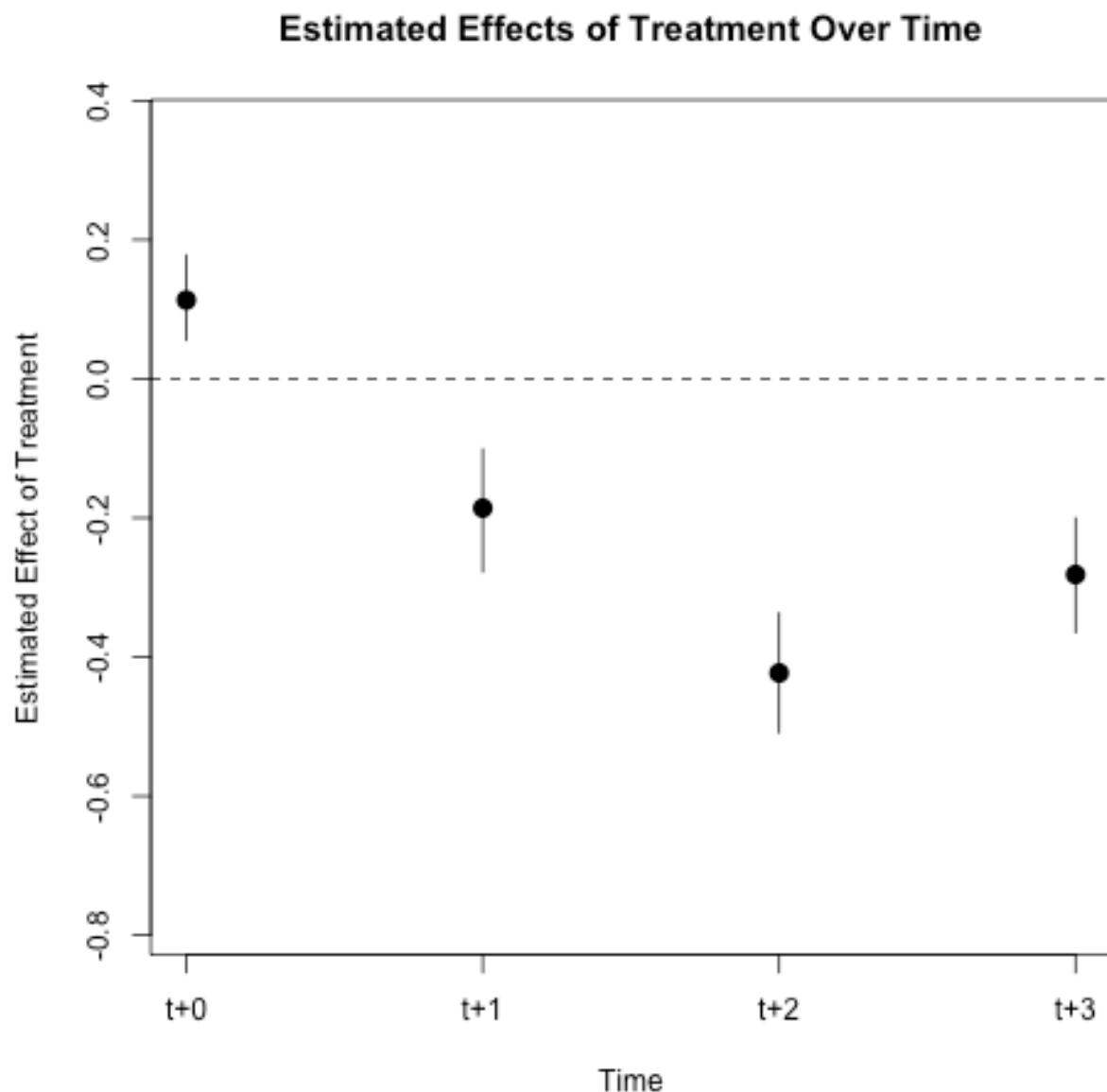
Notes: Coefficients show corrected estimators following ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%.^a Secondary version of security cooperation agreements.
^b State-level controls include governor winning margin in last pre-treatment election and an indicator of whether the governor's party is the same as the federal incumbent party.

FIGURE J-2. Effect of Reelection Incentives on Centralization of Public Security (Sec. Agreement B)



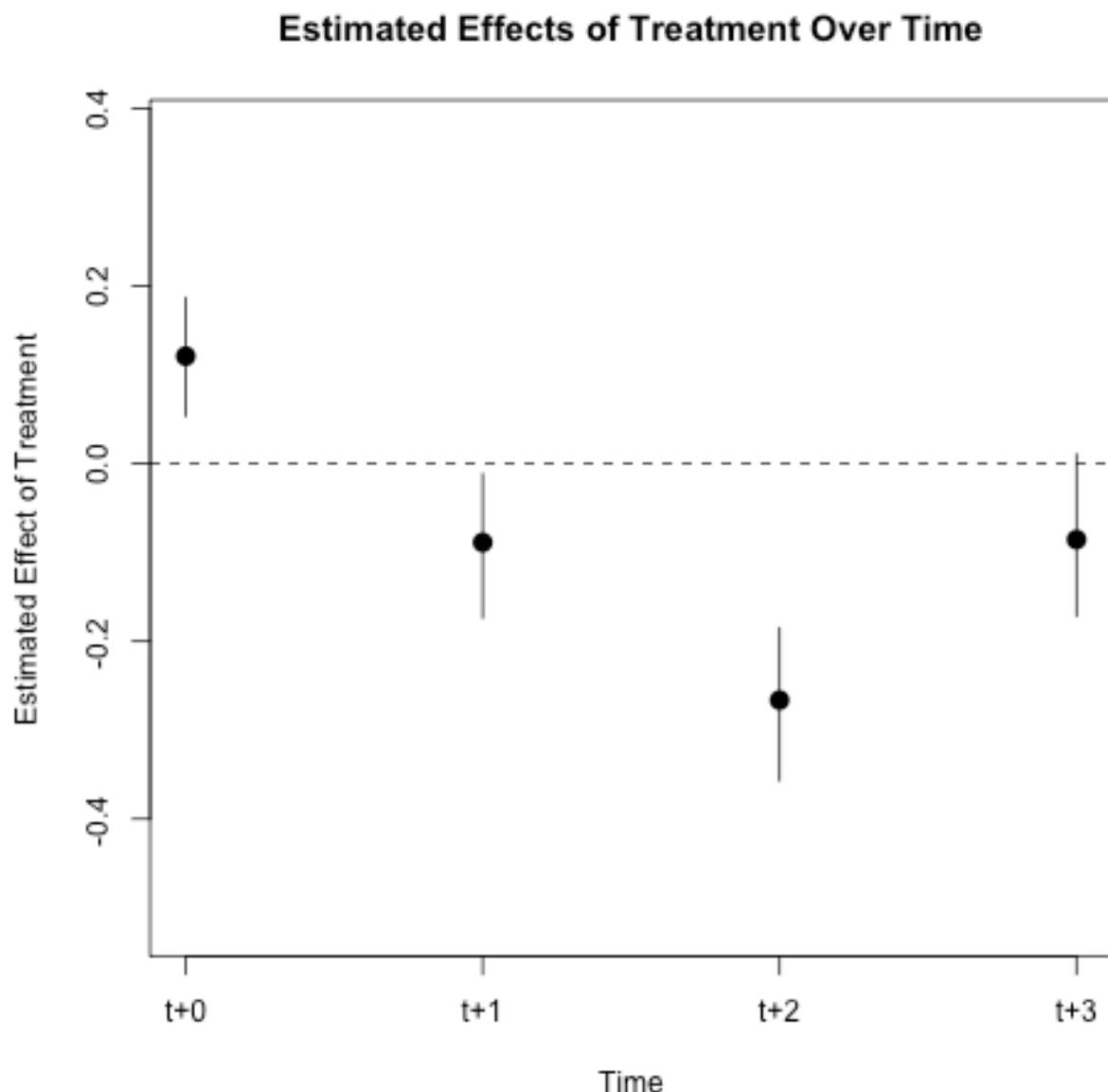
Note: ? two-way fixed effect model correction. The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

FIGURE J-3. Effect of Reelection Incentives on Centralization of Public Security, Propensity Score Matching (Sec. Agreement A)



Note: The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

FIGURE J-4. Effect of Reelection Incentives on Centralization of Public Security, Propensity Score Matching (Sec. Agreement B)



Note: The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

J.1. Mechanism: Accountability

TABLE J-6. Effect of 2014 Term Limit Reform on Centralization, by security preferences

Dependent variable: Signing Security Cooperation Agreement dummy									
Pre-treatment:	Worried about Narco		Worried about insecurity		Worried about low punishment		Home security expenses		
	Below mean (1)	Above mean (2)	Below mean (3)	Above mean (4)	Below mean (5)	Above mean (6)	Below mean (7)	Above mean (8)	
t-5	-0.052 (0.074)	-0.007 (0.022)	0.000 (0.000)	-0.119 (0.082)	-0.001 (0.001)	0.008 (0.016)	-0.052 (0.074)	-0.007 (0.022)	
t-4	-0.031 (0.024)	0.052 (0.068)	0.043 (0.034)	-0.038 (0.063)	-0.008 (0.059)	0.038 (0.069)	-0.026 (0.023)	0.063 (0.066)	
t-3	-0.007 (0.111)	0.081 (0.109)	0.007 (0.035)	0.223 (0.174)	-0.004 (0.022)	0.088 (0.129)	-0.000 (0.100)	0.091 (0.107)	
t-2	0.112 (0.128)	0.141 (0.199)	0.105 (0.071)	0.064 (0.127)	0.118** (0.040)	0.041 (0.281)	0.136* (0.076)	0.091 (0.185)	
t=0 (Reform)	0.135 (0.131)	-0.185 (0.155)	0.107*** (0.034)	-0.152 (0.135)	0.071 (0.127)	-0.258 (0.199)	0.145 (0.114)	-0.227 (0.150)	
t+1	0.121 (0.121)	-0.689*** (0.115)	-0.159 (0.275)	-0.442** (0.218)	-0.058 (0.101)	-0.732*** (0.149)	0.050 (0.135)	-0.697*** (0.118)	
t+2	0.109 (0.242)	-0.737*** (0.096)	-0.232 (0.246)	-0.402*** (0.140)	-0.220 (0.219)	-0.706 (0.000)	-0.022 (0.298)	-0.694*** (0.122)	
Controls ^a	✓	✓	✓	✓	✓	✓	✓	✓	
95% Diff. in CI	✓	✓	✓	✓	✓	✓	✓	✓	

Notes: Coefficients show corrected estimators following ². Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%.^a Pre-treatment controls include the following: governor winning margin, mayor winning margin, governor and president party alignment dummies, and logged homicides per capita.

J.2. Mechanism: Overgrazing

TABLE J-7. Bribes & corruption: local police forces and the military level of effort

	log(detained pc)			log(laboratories destroyed)			log(methamphetamine errad. (kg))		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Term Limit Reform	0.0254 (0.0182)			-0.0071** (0.0029)			-0.0597*** (0.0131)		
Term Limit Reform (t-1)		-0.0525*** (0.0195)			-0.0169*** (0.0033)			-0.0324** (0.0138)	
Term Limit Reform (t-2)			-0.0706*** (0.0226)			-0.0177*** (0.0037)			-0.0211* (0.0110)
Observations	21,375	19,000	16,625	21,375	19,000	16,625	21,375	19,000	16,625
R2	0.881	0.883	0.885	0.594	0.618	0.640	0.345	0.353	0.353
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-8. Effect of 2014 Term Limit Reform on effort of security forces

Dependent variable:

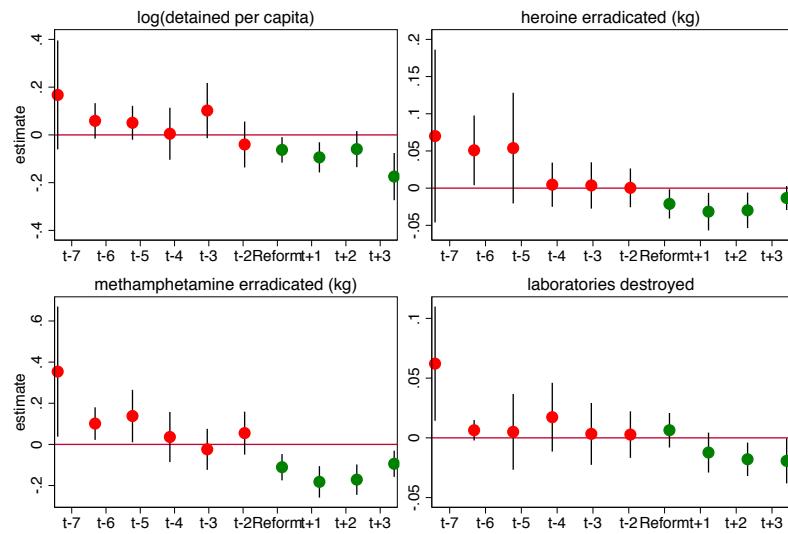
$$\text{Local Police} = \log(\text{detentions pc}) \quad \text{Military} = \log(\text{laboratories errad.})^a$$

(1)	(2)
-----	-----

Lag 3 years	0.011 (0.040)	-0.016 (0.013)
Lag 2 years	0.057 (0.072)	-0.001 (0.005)
Reform, time 0	-0.012 (0.039)	0.006 (0.006)
Lead 1 year	-0.102*** (0.036)	-0.007** (0.003)
Lead 2 years	-0.160 (0.120)	-0.012*** (0.002)
Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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 J-5. Effect of Term Limit Reform of 2014 on Security Forces Effort, IW estimators with 95% confidence intervals & SEs clustered state-level



Note: IW estimators for each lead and lag relative to the first year of treatment.

TABLE J-9. Differential effect by Clientelistic Comparative Advantage

	DV: Agreement A			DV: Agreement B		
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limit Reform	-0.2942*** (0.0716)	-0.2494*** (0.0857)	-0.3477*** (0.0692)	-0.2409*** (0.0651)	-0.2007** (0.0786)	-0.3033*** (0.0637)
Term Limit Reform (t-1)*Aligned (not PRI)	0.1031 (0.1406)			0.0458 (0.1353)		
Term Limit Reform (t-1)*Aligned (PRI)		-0.1253 (0.0917)			-0.1210 (0.0907)	
Term Limit Reform (t-1)*not aligned			0.0929 (0.0820)			0.1019 (0.0813)
Observations	14,761	14,761	14,761	15,652	15,652	15,652
R2						
Controls	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cluster S.E.	✓	✓	✓	✓	✓	✓
Tot.Int.	-0.1910	-0.3747	-0.2547	-0.1950	-0.3217	-0.2014
S.E.(Tot. Int.)	0.1511	0.0714	0.0871	0.1440	0.0679	0.0805
p-value(Tot.Int.)	0.2155	0.0000	0.0064	0.1853	0.0000	0.0178

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-10. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	Alignment not PRI (1)	Alignment with PRI ^a (2)
Lag 5 years	-0.007 (0.008)	0.000 (0.000)
Lag 4 years	-0.017 (0.044)	0.068 (0.052)
Lag 3 years	0.069 (0.112)	0.044 (0.055)
Lag 2 years	0.064 (0.181)	0.118 (0.097)
Reform, time 0	-0.028 (0.148)	-0.126 (0.200)
Lead 1 year	-0.315*** (0.035)	-0.505*** (0.002)
Lead 2 years	-0.426*** (0.047)	-0.510*** (0.120)
Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-11. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

Dependent variable:	Aligned w PRI or not Aligned (1)	Alignment (not w PRI) ^a (2)
Lag 5 years	-0.011 (0.009)	-0.085 (0.000)
Lag 4 years	-0.003 (0.051)	0.033 (0.078)
Lag 3 years	0.056 (0.136)	0.082 (0.205)
Lag 2 years	0.113 (0.213)	-0.038 (0.306)
Reform, time 0	-0.067 (0.156)	-0.049 (0.259)
Lead 1 year	-0.406*** (0.049)	-0.348** (0.173)
Lead 2 years	-0.490*** (0.013)	-0.442*** (0.000)
Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

J.3. Mechanism: Capture

TABLE J-12. Differential effect by Drug Cartel Presence

	DV: Agreement A			DV: Agreement B		
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limit Reform	-0.2992* (0.1754)	-0.2441*** (0.0800)	-0.2481*** (0.0783)	-0.2689 (0.1716)	-0.1842** (0.0743)	-0.1912** (0.0728)
Term Limit Reform (t-1)*Distance to US	0.0135 (0.2385)			0.0386 (0.2443)		
Term Limit Reform (t-1)*Cartel Presence in 2010		-0.1486 (0.0988)			-0.1786* (0.0971)	
Term Limit Reform (t-1)*Num. Carteles in 2010			-0.0948 (0.0612)			-0.1091* (0.0604)
Observations	14,767	14,767	14,767	15,652	15,652	15,652
R2						
Controls	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cluster S.E.	✓	✓	✓	✓	✓	✓
Tot.Int.	-0.2857	-0.3927	-0.3429	-0.2303	-0.3628	-0.3003
S.E.(Tot. Int.)	0.1037	0.0924	0.0750	0.1036	0.0841	0.0665
p-value(Tot.Int.)	0.0097	0.0002	0.0001	0.0337	0.0002	0.0001

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-13. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	No Cartel Presence (1)	Cartel Presence ^a (2)
Lag 5 years	-0.050 (0.056)	-0.021 (0.054)
Lag 4 years	-0.007 (0.040)	0.033 (0.071)
Lag 3 years	0.050 (0.094)	0.137 (0.141)
Lag 2 years	0.097 (0.119)	0.004 (0.162)
Reform, time 0	-0.031 (0.112)	-0.170 (0.153)
Lead 1 year	-0.342*** (0.082)	-0.554** (0.266)
Lead 2 years	-0.375*** (0.086)	-0.614*** (0.174)
State Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-14. Total Interaction Effect^a: Difference by Cartel Presence, IW estimators following ?

Dependent variable:	Agreement A		Agreement B ^b	
	(1)	(2)	(3)	(4)
Reform (t+3)*Dummy Cartel presence	-0.4291** (0.1870)		-0.4090** (0.1584)	
Reform (t+3)*Num. Carteles		-0.3951** (0.1739)		-0.3750** (0.1432)
Observations	17,750	18,670	17,750	15,652
R-squared	0.4698	0.4212	0.4698	0.3806
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of alignment with Federal Government indicator (winning margin) + estimated coefficient of the interaction of alignment (winning margin)*lead t=3. This is a post-estimation test using the same specification as that of Table ?? column (1). Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0. Main regression with standard errors clustered at the state-level. ^b Refers to the inverse hyperbolic sine transformation. ^c 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. I also include the lag of the outcome, i.e. logged homicides per capita as control. ^d Estimates weighted by each cohort's relative share of the sample following ?.

Appendix I. Wash one's hands of security provision

“I am innocent of the blood of this just person: see ye to it.”
Pontius Pilate, Matthew 27:24, King James Version.

An alternative mediator to the increase of violence is local executives refusing to take responsibility or be involved any further in the provision of public security. With reelection, non-term limited incumbents may choose to sign security cooperation agreements with upper administrative levels, including those with the state or the federal government. Some of these agreements may imply a full delegation of public security provision to governors or the president. In other words, mayors in line for reelection would *wash their hands of* public security provision to refuse to take blame for violence. Those new in charge -governors or the President- may decrease public good provision or see an increase in violence since they are misinformed of local crime and how to deter it (?) or are simply not willing to tackle it given party electoral incentives describe in Section ??.

Since the presidency of Felipe Calderon (PAN, 2006-2012), there have been five broad types of security agreements between municipalities and upper level governments: (a) agreements between municipalities (e.g. to create metropolitan police forces), (b) between municipalities and the state governor (e.g. Central Command Agreements), (c) between municipalities and the federal government, (d) agreements with multiplicity of executive actors (various municipalities, states, with or without the Federal government), and (e) agreements with other branches of government, including legislative and judicial ones, also at various levels of government. While conditions vary by agreement, overall they may include any (or all) of the following items: security coordination, transit, security prevention, training, sharing of equipment and technology, research capacity, analysis and intelligence, and creation of unified criteria and procedures of the public security institutions and laws.

Of all agreements, the creation of a state-level Police Central Commands (*Mando Único Policial*) has been the most prevalent in Mexico. The premise is the unification of municipal and state police forces, i.e. the centralization of public security under direction of the governor. During Calderon's presidency, Central Commands were intended to abolish municipal police forces. Later, Pena Nieto's administration proposed the creation of Unified State Forces to transition from 1,800 municipal police bodies to 32 police corporations. However, a proposed constitutional modification was stopped in the Senate since it did not reach the necessary three quarters of legislators to approve the Constitutional modification. While it did not achieve constitutional jurisdiction, 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.⁵⁵

Appendix Figure ?? shows that the Term Limit Reform did not increase the likelihood of municipalities on signing security cooperation agreements. Prior to treatment we observe pretrends. Once the reform is implemented there is actually a decrease in the probability of signing an agreement, but non-significant. For those municipalities with four years of implementation we see an increase in the likelihood of having an agreement, but it is quite noisy.⁵⁶

Now, while there is no effect of reelection on mayors refusing to take care of public security matters, we would expect that municipalities without Police Central Command (or other cooperation agreements) would experience lower welfare distortions (i.e. lower violence) since they would be held more accountable by citizens on local security provision. This is exactly what we find. Appendix Figure ?? estimates the effect of the Term Limit Reform on logged homicides per capita on two subsamples: one the one hand, municipalities with security agreements prior to the implementation of the Reform (points with black lines representing 95% confidence intervals); on the other hand, municipalities without such agreements (squares with dashed lines representing 95% confidence intervals). As noted, both subsamples show positive effects of the reform on violence, with the exception of the first year of treatment for those without security cooperation agreements. More importantly, there is significant difference between both subsamples for all the years after the reform was implemented: municipalities with security cooperation agreements such as *Mando Único Policial* show a higher increase in the percentage of homicides per capita in treated municipalities relative to non-treated.

One important concern could arise: why don't we see a null effect on municipalities without security agreements? Would not these municipalities serve as placebos? To answer this there are three important caveats about security cooperation agreements -particularly Central Command Agreements- that needs to be noted.

First, not all Central Command Agreements imply a *de jure* delegation of municipal public security provision to the state. There is wide variation of what central command implies, and could take all or any of the items mentioned before, from security provision to

⁵⁵There is a judicial discussion in Mexico on the legitimacy of centralized state level agreements, particularly that of the Centralized Command. In the framework of the Mexican federal pact, Article 21 of the Constitution makes public ministries (*Ministerios Pùblicos*) the actor in charge of prosecution. However, they are left aside in most security cooperation agreements. Furthermore the Constitutional figure of the “free municipality”, makes public security centralization something unfeasible and unconstitutional (?). As noted by Article 115, fraction III, item “h” of the Constitution, municipalities are the first autonomous constitutional bodies and are granted express powers to provide public security service. For more details see <https://aristeguinoticias.com/0608/mexico/el-inconstitucional-mando-unico-articulo/>.

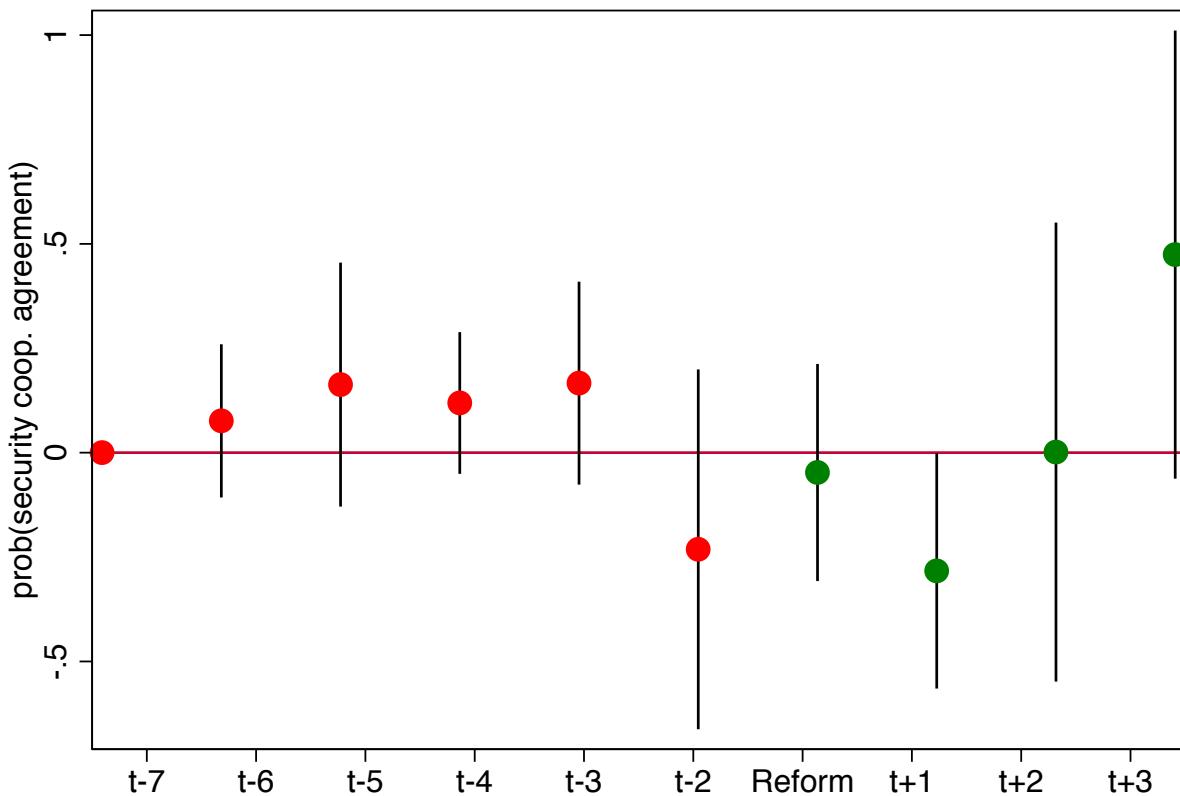
⁵⁶For robustness, Appendix Figure ?? shows main estimates on the effect of the reform on violence robust to controlling for security cooperation agreement.

intelligence. In other words, *Mando Único* in one state may not imply the same operative features in another state. For instance, as noted by data from the National Census of Municipal Governments and Territories of the City of Mexico from 2011 to 2019, of all the municipalities that state they had a Central Command Agreement, only 72.6% said the agreement included the delegation of public security.

Second, even with the existence of a security cooperation agreements that delegates all security provision to higher level federal authorities, citizens could still blame mayors for not lobbying federal authorities for public good provision in their municipalities. Moreover, citizens may simply not understand the terms and conditions of cooperation agreements, blaming mayors still for local level violence. Lastly, *Mando Único Policial* has not led to a decrease in crime incidence, especially since municipal police forces require a distinct training and objectives to those of states (?), and has been deemed as either non-existent or a failure.⁵⁷ Given these caveats, municipalities without security cooperation agreements should not be deemed as placebos, but should show a significant difference in the level of violence as found in Appendix Figure ??.

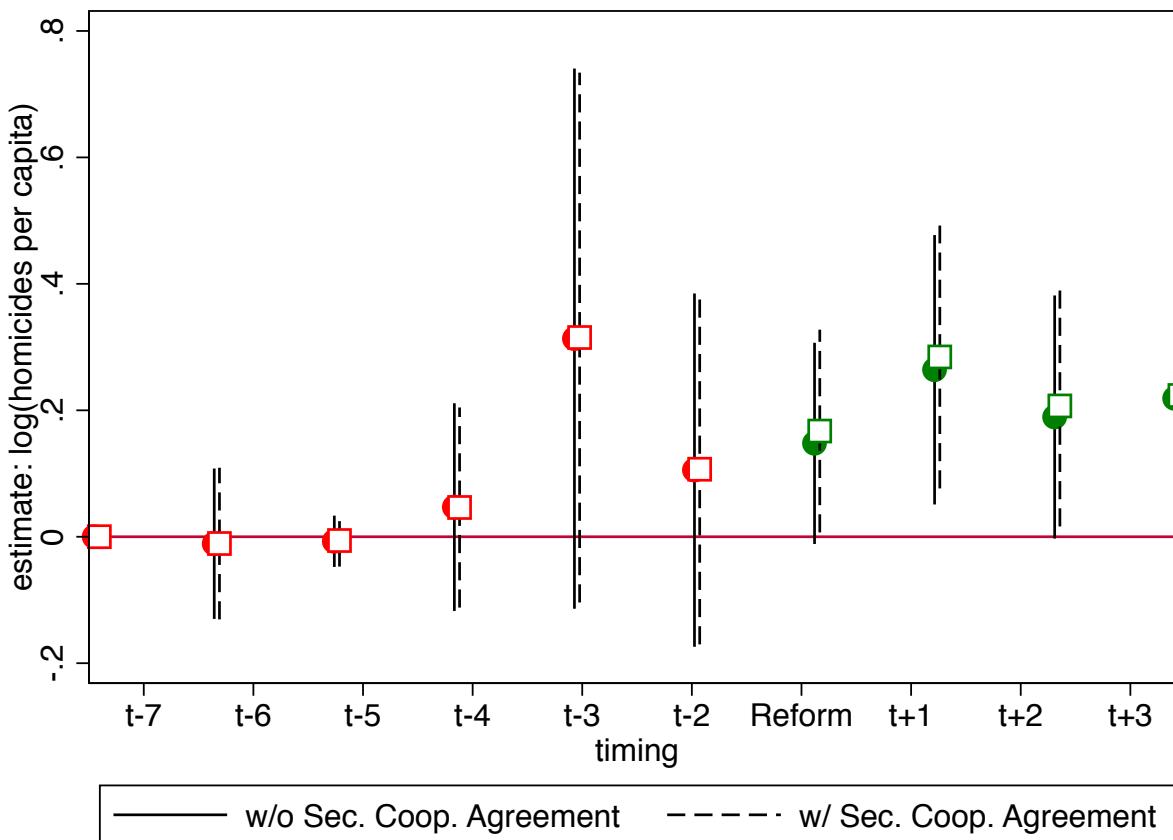
⁵⁷For more detail, see the article “Mando Único Policial: el modelo fracasado” from <https://www.proceso.com.mx/515386/mando-unico-policial-el-modelo-fracasado>

FIGURE B-10. Effect of Term Limit Reform on Probability of Signing a Security Cooperation Agreement



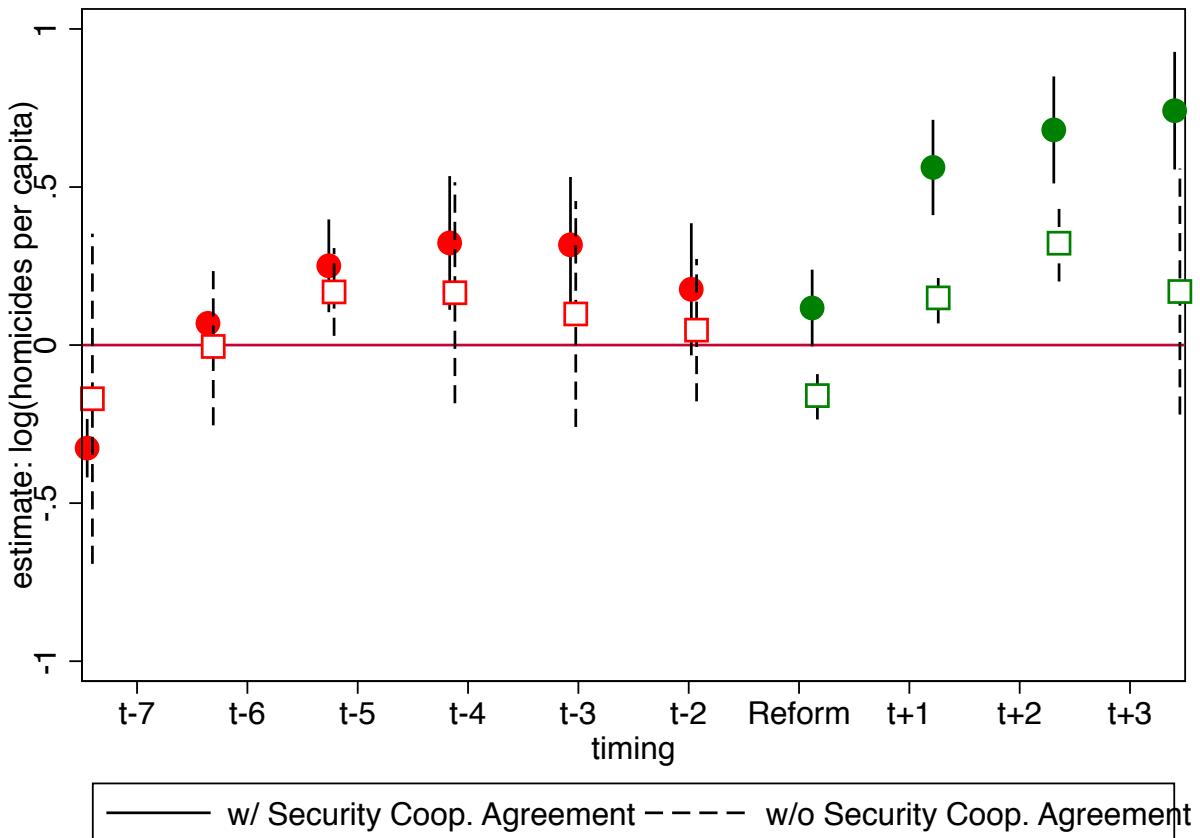
Note: Figures shows the estimates of equation ?? using as outcome an indicator variable of whether a municipality signed a security cooperation agreement according to information from the National Census of Municipal Governments and Territories of the City of Mexico, 2011-2019. The Figure shows IW estimators with 95% confidence intervals.

FIGURE B-11. Effect of Term Limit Reform on Violence, conditional on Signing a Security Cooperation Agreement



Note: Figures shows the estimates of equation ?? conditional on whether a municipality signed a security cooperation agreement one year prior to treatment according to information from the National Census of Municipal Governments and Territories of the City of Mexico, 2011-2019. The Figure shows IW estimators with 95% confidence intervals.

FIGURE B-12. Effect of Term Limit Reform on Violence, by security cooperation agreement



Note: Line shows estimates for the subset of municipalities with a security cooperation agreement prior to treatment. Dashed line shows estimates for the subset of municipalities without a security cooperation agreement prior to treatment. The Figure shows IW estimators with 95% confidence intervals.

Appendix J. New Tables and Story

TABLE J-1. Effect of Centralization of Public Security on Violence

	log(homicides per capita)	IHS(homicides per capita)	
	(1)	(2)	(3)
	(4)		
Sec. Coop. Agreement (Centralization)	-0.0243* (0.0131)		-0.0234 (0.0300)
Sec. Coop. Agreement (Centralization) in t-1		-0.0309** (0.0127)	-0.0539* (0.0317)
Observations	17,750	15,558	17,750
R2			15,558
Controls	✓	✓	✓
Mun. FE	✓	✓	✓
Year FE	✓	✓	✓
Mun. Cluster S.E.	✓	✓	✓

Notes: Municipal clustered standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test.

TABLE J-2. Effect of Reelection Incentives on Security Provision Centralization

	Agreement A in t	in t+1	in t+2	in t+3	Agreement B in t	in t+1	in t+2	in t+3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Term Limit Reform	-0.1076 (0.0692)	-0.2889*** (0.0719)	-0.2646** (0.1043)	-0.1039 (0.1013)	-0.0842 (0.0567)	-0.2386*** (0.0649)	-0.2454** (0.1047)	-0.0950 (0.1070)
Observations	14,767	14,767	12,919	11,071	15,658	15,658	13,661	11,664
R2								
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-3. Effect of Reelection Incentives on Security Provision Centralization, Wild Bootstrap

	Agreement A in t	in t+1	in t+2	in t+3	Agreement B in t	in t+1	in t+2	in t+3
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Term Limit Reform	-0.1076 (0.0692)	-0.2889*** (0.0719)	-0.2646** (0.1043)	-0.1039 (0.1013)	-0.0842 (0.0567)	-0.2386*** (0.0649)	-0.2454** (0.1047)	-0.0950 (0.1070)
Observations	14,767	14,767	12,919	11,071	15,658	15,658	13,661	11,664
R2								
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓	✓	✓	✓	✓
Wild CI	[-0.176, -0.001]	[-0.344, -0.138]	[-0.393, -0.070]	[-0.255, 0.056]	[-0.139, 0.013]	[-0.292, -0.090]	[-0.367, -0.038]	[-0.239, -0.086]

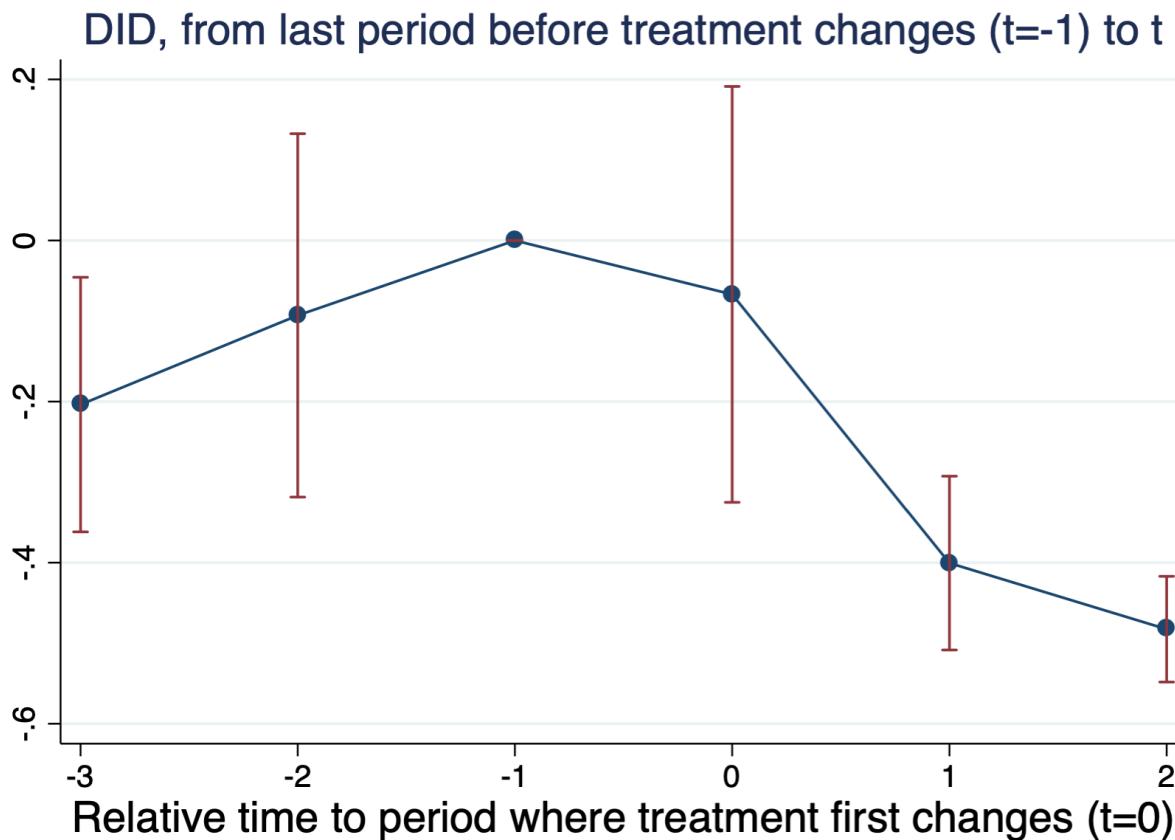
Notes: Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-4. Effect of Reelection Incentives on Security Provision
Centralization, Wild Bootstrap

	Agreement A in t	in t+1	in t+2	in t+3
	(1)	(2)	(3)	(4)
Term Limit Reform	-0.1098 (0.0697)	-0.2909*** (0.0720)	-0.2623** (0.1056)	-0.1018 (0.1016)
Observations	14,263	14,263	12,498	10,733
R2				
Controls	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓
Wild CI	[-0.176, -0.001]	[-0.344, -0.138]	[-0.393, -0.070]	[-0.255, 0.056]

Notes: State clustered standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test.

FIGURE J-1. Effect of Reelection Incentives on Centralization of Public Security (Sec. Agreement A)



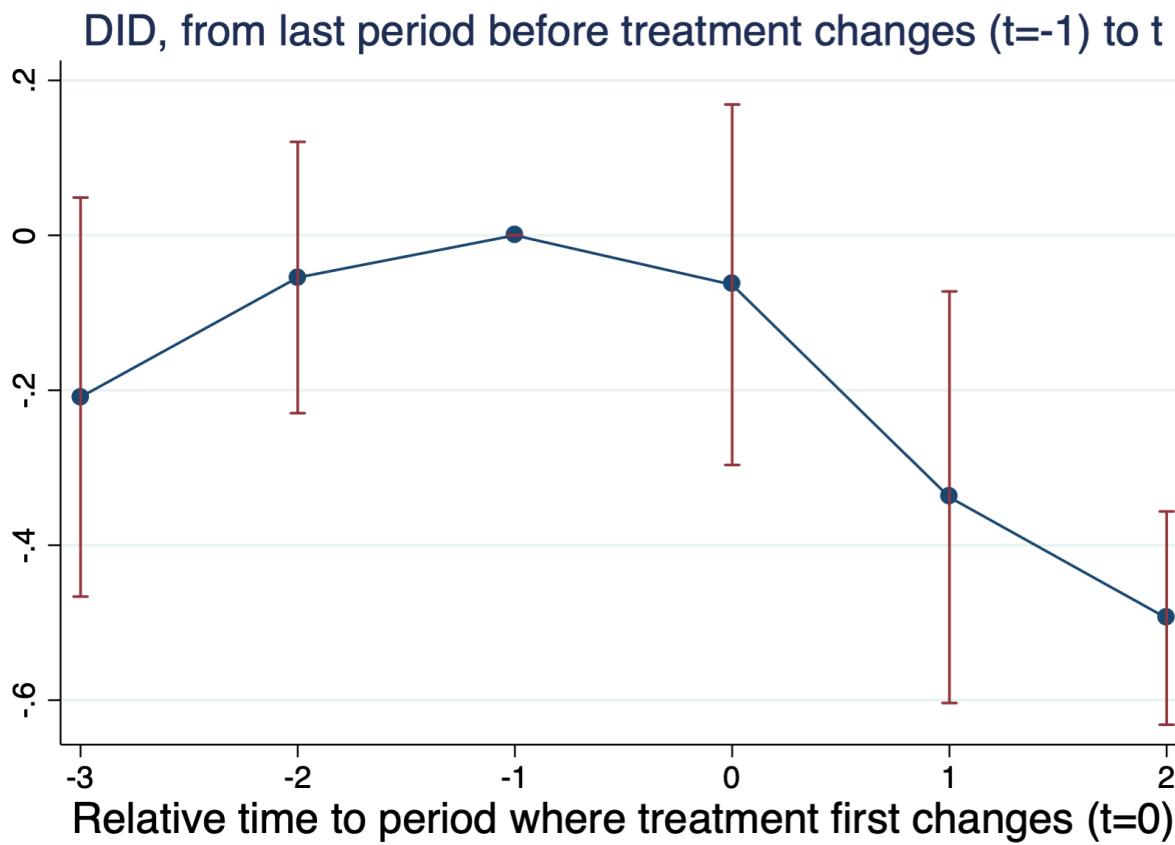
Note: ? two-way fixed effect model correction. The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

TABLE J-5. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements, ? correction

	Dependent variable:	
	Agreement A (1)	Agreement B ^a (2)
Lag 5 years	-0.023 (0.021)	-0.045 (0.071)
Lag 4 years	0.009 (0.038)	0.033 (0.048)
Lag 3 years	0.063 (0.081)	0.098 (0.080)
Lag 2 years	0.093 (0.115)	0.055 (0.089)
Reform, time 0	-0.067 (0.132)	-0.063 (0.119)
Lead 1 year	-0.401*** (0.055)	-0.337** (0.136)
Lead 2 years	-0.483*** (0.033)	-0.493*** (0.071)
Controls ^b	✓	✓

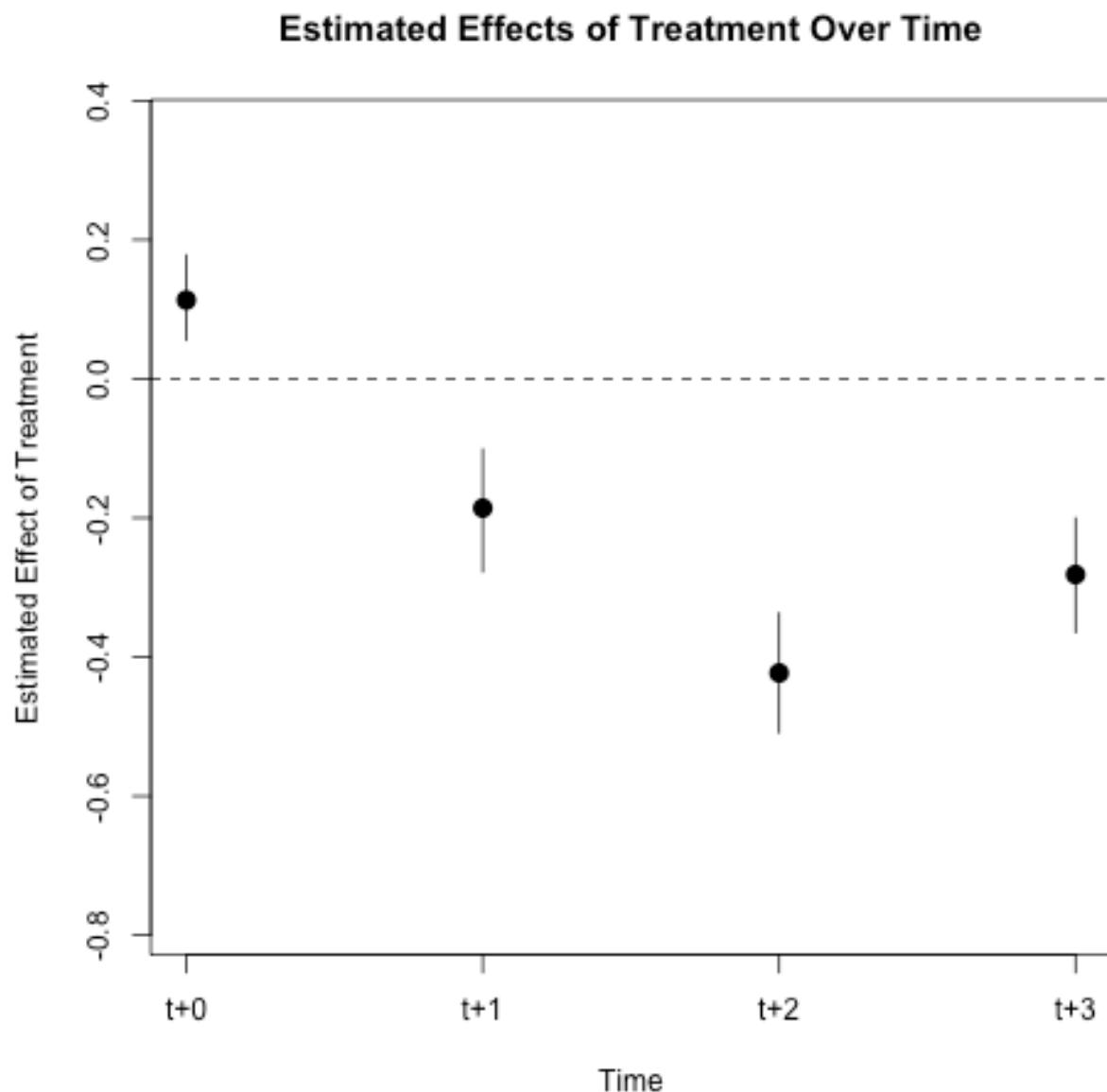
Notes: Coefficients show corrected estimators following ?. Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%.^a Secondary version of security cooperation agreements.
^b State-level controls include governor winning margin in last pre-treatment election and an indicator of whether the governor's party is the same as the federal incumbent party.

FIGURE J-2. Effect of Reelection Incentives on Centralization of Public Security (Sec. Agreement B)



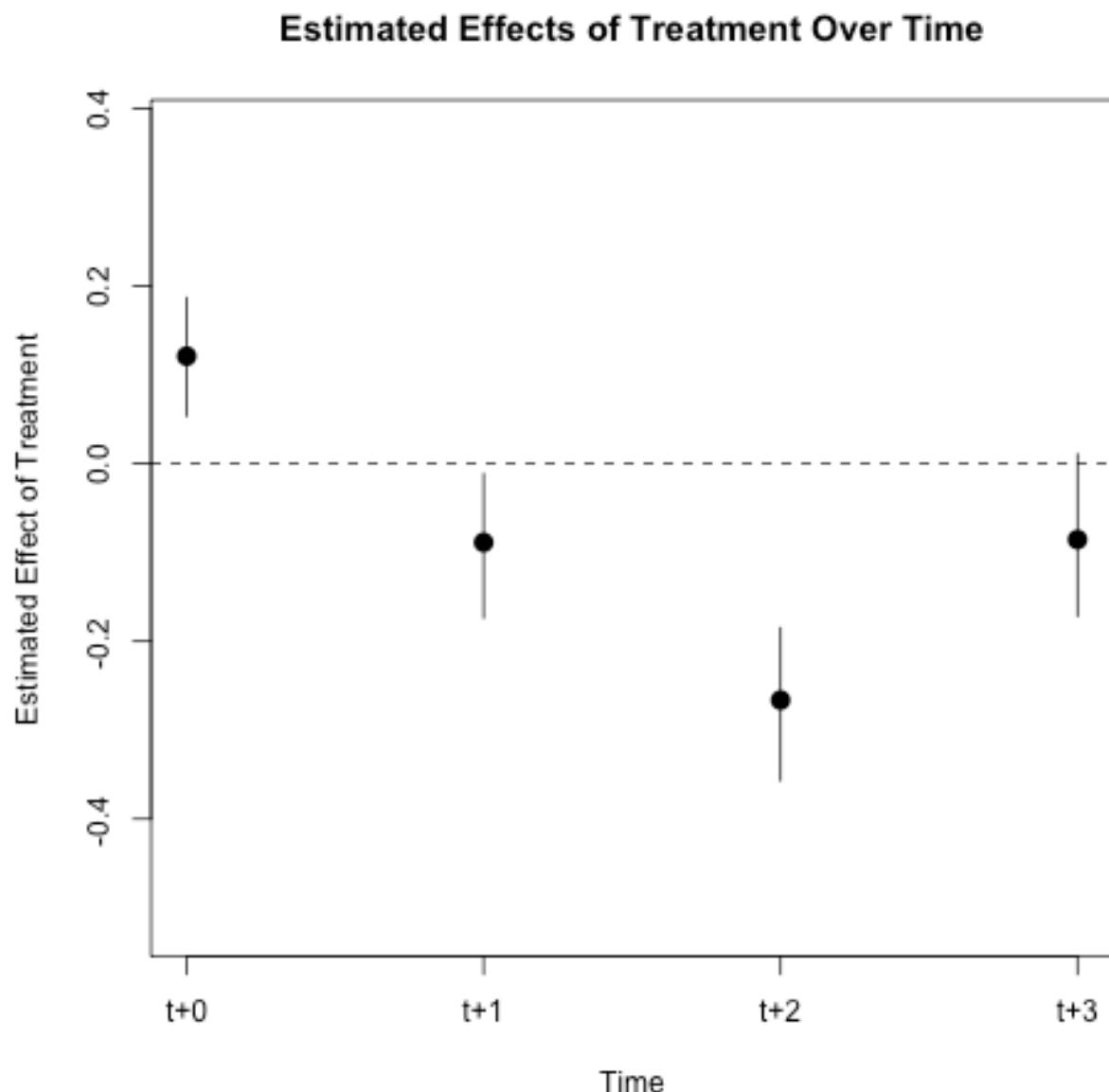
Note: ? two-way fixed effect model correction. The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

FIGURE J-3. Effect of Reelection Incentives on Centralization of Public Security, Propensity Score Matching (Sec. Agreement A)



Note: The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

FIGURE J-4. Effect of Reelection Incentives on Centralization of Public Security, Propensity Score Matching (Sec. Agreement B)



Note: The Figure shows 95% confidence intervals, using 1,000 bootstrapped replications.

J.1. Mechanism: Accountability

TABLE J-6. Effect of 2014 Term Limit Reform on Centralization, by security preferences

Dependent variable: Signing Security Cooperation Agreement dummy									
Pre-treatment:	Worried about Narco		Worried about insecurity		Worried about low punishment		Home security expenses		
	Below mean (1)	Above mean (2)	Below mean (3)	Above mean (4)	Below mean (5)	Above mean (6)	Below mean (7)	Above mean (8)	
t-5	-0.052 (0.074)	-0.007 (0.022)	0.000 (0.000)	-0.119 (0.082)	-0.001 (0.001)	0.008 (0.016)	-0.052 (0.074)	-0.007 (0.022)	
t-4	-0.031 (0.024)	0.052 (0.068)	0.043 (0.034)	-0.038 (0.063)	-0.008 (0.059)	0.038 (0.069)	-0.026 (0.023)	0.063 (0.066)	
t-3	-0.007 (0.111)	0.081 (0.109)	0.007 (0.035)	0.223 (0.174)	-0.004 (0.022)	0.088 (0.129)	-0.000 (0.100)	0.091 (0.107)	
t-2	0.112 (0.128)	0.141 (0.199)	0.105 (0.071)	0.064 (0.127)	0.118** (0.040)	0.041 (0.281)	0.136* (0.076)	0.091 (0.185)	
t=0 (Reform)	0.135 (0.131)	-0.185 (0.155)	0.107*** (0.034)	-0.152 (0.135)	0.071 (0.127)	-0.258 (0.199)	0.145 (0.114)	-0.227 (0.150)	
t+1	0.121 (0.121)	-0.689*** (0.115)	-0.159 (0.275)	-0.442** (0.218)	-0.058 (0.101)	-0.732*** (0.149)	0.050 (0.135)	-0.697*** (0.118)	
t+2	0.109 (0.242)	-0.737*** (0.096)	-0.232 (0.246)	-0.402*** (0.140)	-0.220 (0.219)	-0.706 (0.000)	-0.022 (0.298)	-0.694*** (0.122)	
Controls ^a	✓	✓	✓	✓	✓	✓	✓	✓	
95% Diff. in CI	✓	✓	✓	✓	✓	✓	✓	✓	

Notes: Coefficients show corrected estimators following ². Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%.^a Pre-treatment controls include the following: governor winning margin, mayor winning margin, governor and president party alignment dummies, and logged homicides per capita.

J.2. Mechanism: Overgrazing

TABLE J-7. Bribes & corruption: local police forces and the military level of effort

	log(detained pc)			log(laboratories destroyed)			log(methamphetamine errad. (kg))		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Term Limit Reform	0.0254 (0.0182)			-0.0071** (0.0029)			-0.0597*** (0.0131)		
Term Limit Reform (t-1)		-0.0525*** (0.0195)			-0.0169*** (0.0033)			-0.0324** (0.0138)	
Term Limit Reform (t-2)			-0.0706*** (0.0226)			-0.0177*** (0.0037)			-0.0211* (0.0110)
Observations	21,375	19,000	16,625	21,375	19,000	16,625	21,375	19,000	16,625
R2	0.881	0.883	0.885	0.594	0.618	0.640	0.345	0.353	0.353
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
State Cluster S.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

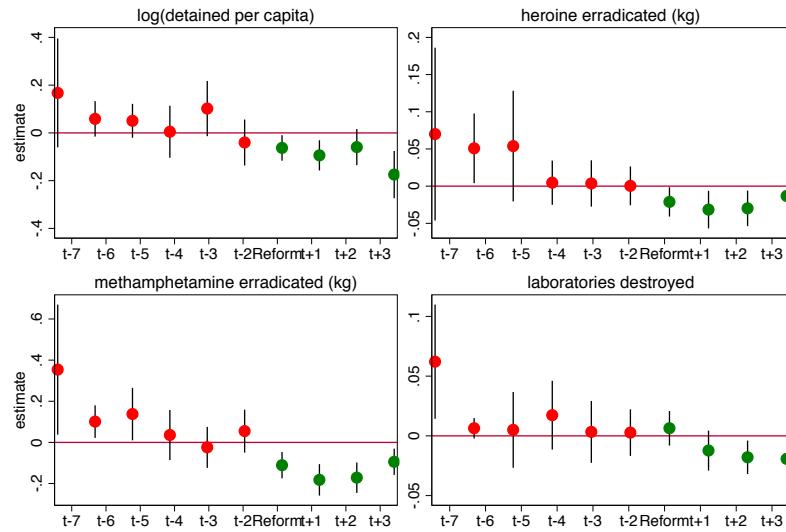
TABLE J-8. Effect of 2014 Term Limit Reform on effort of security forces

Dependent variable:

	Local Police = log(detentions pc) (1)	Military = log(laboratories errad.) ^a (2)
Lag 3 years	0.011 (0.040)	-0.016 (0.013)
Lag 2 years	0.057 (0.072)	-0.001 (0.005)
Reform, time 0	-0.012 (0.039)	0.006 (0.006)
Lead 1 year	-0.102*** (0.036)	-0.007** (0.003)
Lead 2 years	-0.160 (0.120)	-0.012*** (0.002)
Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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 J-5. Effect of Term Limit Reform of 2014 on Security Forces Effort,
IW estimators with 95% confidence intervals & SEs clustered state-level



Note: IW estimators for each lead and lag relative to the first year of treatment.

TABLE J-9. Differential effect by Clientelistic Comparative Advantage

	DV: Agreement A			DV: Agreement B		
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limit Reform	-0.2942*** (0.0716)	-0.2494*** (0.0857)	-0.3477*** (0.0692)	-0.2409*** (0.0651)	-0.2007** (0.0786)	-0.3033*** (0.0637)
Term Limit Reform (t-1)*Aligned (not PRI)	0.1031 (0.1406)			0.0458 (0.1353)		
Term Limit Reform (t-1)*Aligned (PRI)		-0.1253 (0.0917)			-0.1210 (0.0907)	
Term Limit Reform (t-1)*not aligned			0.0929 (0.0820)			0.1019 (0.0813)
Observations	14,761	14,761	14,761	15,652	15,652	15,652
R2						
Controls	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cluster S.E.	✓	✓	✓	✓	✓	✓
Tot.Int.	-0.1910	-0.3747	-0.2547	-0.1950	-0.3217	-0.2014
S.E.(Tot. Int.)	0.1511	0.0714	0.0871	0.1440	0.0679	0.0805
p-value(Tot.Int.)	0.2155	0.0000	0.0064	0.1853	0.0000	0.0178

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-10. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	Alignment not PRI (1)	Alignment with PRI ^a (2)
Lag 5 years	-0.007 (0.008)	0.000 (0.000)
Lag 4 years	-0.017 (0.044)	0.068 (0.052)
Lag 3 years	0.069 (0.112)	0.044 (0.055)
Lag 2 years	0.064 (0.181)	0.118 (0.097)
Reform, time 0	-0.028 (0.148)	-0.126 (0.200)
Lead 1 year	-0.315*** (0.035)	-0.505*** (0.002)
Lead 2 years	-0.426*** (0.047)	-0.510*** (0.120)
Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-11. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

Dependent variable:	Aligned w PRI or not Aligned (1)	Alignment (not w PRI) ^a (2)
Lag 5 years	−0.011 (0.009)	−0.085 (0.000)
Lag 4 years	−0.003 (0.051)	0.033 (0.078)
Lag 3 years	0.056 (0.136)	0.082 (0.205)
Lag 2 years	0.113 (0.213)	−0.038 (0.306)
Reform, time 0	−0.067 (0.156)	−0.049 (0.259)
Lead 1 year	−0.406*** (0.049)	−0.348** (0.173)
Lead 2 years	−0.490*** (0.013)	−0.442*** (0.000)
Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

J.3. Mechanism: Capture

TABLE J-12. Differential effect by Drug Cartel Presence

	DV: Agreement A			DV: Agreement B		
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limit Reform	-0.2992* (0.1754)	-0.2441*** (0.0800)	-0.2481*** (0.0783)	-0.2689 (0.1716)	-0.1842** (0.0743)	-0.1912** (0.0728)
Term Limit Reform (t-1)*Distance to US	0.0135 (0.2385)			0.0386 (0.2443)		
Term Limit Reform (t-1)*Cartel Presence in 2010		-0.1486 (0.0988)			-0.1786* (0.0971)	
Term Limit Reform (t-1)*Num. Carteles in 2010			-0.0948 (0.0612)			-0.1091* (0.0604)
Observations	14,767	14,767	14,767	15,652	15,652	15,652
R2						
Controls	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cluster S.E.	✓	✓	✓	✓	✓	✓
Tot.Int.	-0.2857	-0.3927	-0.3429	-0.2303	-0.3628	-0.3003
S.E.(Tot. Int.)	0.1037	0.0924	0.0750	0.1036	0.0841	0.0665
p-value(Tot.Int.)	0.0097	0.0002	0.0001	0.0337	0.0002	0.0001

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-13. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	No Cartel Presence (1)	Cartel Presence ^a (2)
Lag 5 years	-0.050 (0.056)	-0.021 (0.054)
Lag 4 years	-0.007 (0.040)	0.033 (0.071)
Lag 3 years	0.050 (0.094)	0.137 (0.141)
Lag 2 years	0.097 (0.119)	0.004 (0.162)
Reform, time 0	-0.031 (0.112)	-0.170 (0.153)
Lead 1 year	-0.342*** (0.082)	-0.554** (0.266)
Lead 2 years	-0.375*** (0.086)	-0.614*** (0.174)
State Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-14. Total Interaction Effect^a: Difference by Cartel Presence, IW estimators following ?

Dependent variable:	Agreement A		Agreement B ^b	
	(1)	(2)	(3)	(4)
Reform (t+3)*Dummy Cartel presence	-0.4291** (0.1870)		-0.4090** (0.1584)	
Reform (t+3)*Num. Carteles		-0.3951** (0.1739)		-0.3750** (0.1432)
Observations	17,750	18,670	17,750	15,652
R-squared	0.4698	0.4212	0.4698	0.3806
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of alignment with Federal Government indicator (winning margin) + estimated coefficient of the interaction of alignment (winning margin)*lead t=3. This is a post-estimation test using the same specification as that of Table ?? column (1). Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0. Main regression with standard errors clustered at the state-level. ^b Refers to the inverse hyperbolic sine transformation. ^c 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. I also include the lag of the outcome, i.e. logged homicides per capita as control. ^d Estimates weighted by each cohort's relative share of the sample following ?.

TABLE J-13. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	No Cartel Presence (1)	Cartel Presence ^a (2)
Lag 5 years	-0.050 (0.056)	-0.021 (0.054)
Lag 4 years	-0.007 (0.040)	0.033 (0.071)
Lag 3 years	0.050 (0.094)	0.137 (0.141)
Lag 2 years	0.097 (0.119)	0.004 (0.162)
Reform, time 0	-0.031 (0.112)	-0.170 (0.153)
Lead 1 year	-0.342*** (0.082)	-0.554** (0.266)
Lead 2 years	-0.375*** (0.086)	-0.614*** (0.174)
State Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-14. Total Interaction Effect^a: Difference by Cartel Presence, IW estimators following ?

Dependent variable:	Agreement A		Agreement B ^b	
	(1)	(2)	(3)	(4)
Reform (t+3)*Dummy Cartel presence	-0.4291** (0.1870)		-0.4090** (0.1584)	
Reform (t+3)*Num. Carteles		-0.3951** (0.1739)		-0.3750** (0.1432)
Observations	17,750	18,670	17,750	15,652
R-squared	0.4698	0.4212	0.4698	0.3806
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of alignment with Federal Government indicator (winning margin) + estimated coefficient of the interaction of alignment (winning margin)*lead t=3. This is a post-estimation test using the same specification as that of Table ?? column (1). Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0. Main regression with standard errors clustered at the state-level. ^b Refers to the inverse hyperbolic sine transformation. ^c 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. I also include the lag of the outcome, i.e. logged homicides per capita as control. ^d Estimates weighted by each cohort's relative share of the sample following ?.

TABLE J-13. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	No Cartel Presence (1)	Cartel Presence ^a (2)
Lag 5 years	-0.050 (0.056)	-0.021 (0.054)
Lag 4 years	-0.007 (0.040)	0.033 (0.071)
Lag 3 years	0.050 (0.094)	0.137 (0.141)
Lag 2 years	0.097 (0.119)	0.004 (0.162)
Reform, time 0	-0.031 (0.112)	-0.170 (0.153)
Lead 1 year	-0.342*** (0.082)	-0.554** (0.266)
Lead 2 years	-0.375*** (0.086)	-0.614*** (0.174)
State Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-14. Total Interaction Effect^a: Difference by Cartel Presence, IW estimators following ?

Dependent variable:	Agreement A		Agreement B ^b	
	(1)	(2)	(3)	(4)
Reform (t+3)*Dummy Cartel presence	-0.4291** (0.1870)		-0.4090** (0.1584)	
Reform (t+3)*Num. Carteles		-0.3951** (0.1739)		-0.3750** (0.1432)
Observations	17,750	18,670	17,750	15,652
R-squared	0.4698	0.4212	0.4698	0.3806
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of alignment with Federal Government indicator (winning margin) + estimated coefficient of the interaction of alignment (winning margin)*lead t=3. This is a post-estimation test using the same specification as that of Table ?? column (1). Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0. Main regression with standard errors clustered at the state-level. ^b Refers to the inverse hyperbolic sine transformation. ^c 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. I also include the lag of the outcome, i.e. logged homicides per capita as control. ^d Estimates weighted by each cohort's relative share of the sample following ?.

J.3. Mechanism: Capture

TABLE J-12. Differential effect by Drug Cartel Presence

	DV: Agreement A			DV: Agreement B		
	(1)	(2)	(3)	(4)	(5)	(6)
Term Limit Reform	-0.2992* (0.1754)	-0.2441*** (0.0800)	-0.2481*** (0.0783)	-0.2689 (0.1716)	-0.1842** (0.0743)	-0.1912** (0.0728)
Term Limit Reform (t-1)*Distance to US	0.0135 (0.2385)			0.0386 (0.2443)		
Term Limit Reform (t-1)*Cartel Presence in 2010		-0.1486 (0.0988)			-0.1786* (0.0971)	
Term Limit Reform (t-1)*Num. Carteles in 2010			-0.0948 (0.0612)			-0.1091* (0.0604)
Observations	14,767	14,767	14,767	15,652	15,652	15,652
R2						
Controls	✓	✓	✓	✓	✓	✓
Mun. FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Cluster S.E.	✓	✓	✓	✓	✓	✓
Tot.Int.	-0.2857	-0.3927	-0.3429	-0.2303	-0.3628	-0.3003
S.E.(Tot. Int.)	0.1037	0.0924	0.0750	0.1036	0.0841	0.0665
p-value(Tot.Int.)	0.0097	0.0002	0.0001	0.0337	0.0002	0.0001

Notes: Standard errors with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0.

TABLE J-13. Effect of 2014 Term Limit Reform on the likelihood of signing Security Cooperation Agreements

	Dependent variable:	
	No Cartel Presence (1)	Cartel Presence ^a (2)
Lag 5 years	-0.050 (0.056)	-0.021 (0.054)
Lag 4 years	-0.007 (0.040)	0.033 (0.071)
Lag 3 years	0.050 (0.094)	0.137 (0.141)
Lag 2 years	0.097 (0.119)	0.004 (0.162)
Reform, time 0	-0.031 (0.112)	-0.170 (0.153)
Lead 1 year	-0.342*** (0.082)	-0.554** (0.266)
Lead 2 years	-0.375*** (0.086)	-0.614*** (0.174)
State Controls ^b	✓	✓

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%.^a Secondary version of security cooperation agreements. ^b 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.

TABLE J-14. Total Interaction Effect^a: Difference by Cartel Presence, IW estimators following ?

Dependent variable:	Agreement A		Agreement B ^b	
	(1)	(2)	(3)	(4)
Reform (t+3)*Dummy Cartel presence	-0.4291** (0.1870)		-0.4090** (0.1584)	
Reform (t+3)*Num. Carteles		-0.3951** (0.1739)		-0.3750** (0.1432)
Observations	17,750	18,670	17,750	15,652
R-squared	0.4698	0.4212	0.4698	0.3806
Mun. FEs	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓
State Controls ^c	✓	✓	✓	✓
Cohort weighted ^d	✓	✓	✓	✓

Notes:^a Total interaction effect tests the linear hypothesis of the estimated coefficient of alignment with Federal Government indicator (winning margin) + estimated coefficient of the interaction of alignment (winning margin)*lead t=3. This is a post-estimation test using the same specification as that of Table ?? column (1). Other leads and the indicator at time t=0 when reform came to effect are omitted due to collinearity. Standard errors of linear hypothesis test in parentheses with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided test with the null hypothesis equal to 0. Main regression with standard errors clustered at the state-level. ^b Refers to the inverse hyperbolic sine transformation. ^c 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. I also include the lag of the outcome, i.e. logged homicides per capita as control. ^d Estimates weighted by each cohort's relative share of the sample following ?.