

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

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

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

Date: April 30, 2021.

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1. Introduction

Normatively, public good provision should be determined by efficiency and equity considerations, identifying spillovers and recognizing the heterogeneity of needs and tastes across populations (Oates, 1972; Musgrave, 1959, 1983; Gramlich, 1977). Even when able to accomplish unilaterally a desired policy, governments may want to share the costs of public good provision (Moravcsik, 2000), pool resources and information Rodrik (1996) or decrease the level of uncertainty if deemed too powerful and a threat to others to increase the likelihood and efficiency of a policy (Lake, 2009; Milner and Tingley, 2013). Governments may also lack the capacity to roll out public goods efficiently due to constraints of resources, expertise, information or the politicization of policies. For instance, a large literature suggests electoral incentives lead public officials to inefficiently deliver goods: incumbents may favor specific regions that are electorally favorable to them (Schady, 2000; Miguel and F., 2003; Cole, 2004; Khemani, 2007) or those with higher political representation (Wright, 1974; Porto and Sanguinetti, 2001; Ansolabehere et al., 2002). In recognition of inefficiencies and potential benefits, governments may choose to delegate public good provision to upper-level governments or entities who can pool resources, reduce costs and decrease the politicization of policies.

Delegation, however, is not an obvious choice for incumbents with electoral concerns. In the presence of spillovers and high fixed costs, delegating public good provision helps to overcome the free-rider problem (Hamman et al., 2011), develop economies of scale and specialize (Hawkins et al., 2006), not neglect benefits going to certain localities, and tackle down capacity constraints, all of which increase public good efficiency (Oates, 1972; Besley and Coate, 2003).¹ However, if incumbents delegate public good provision to an upper-level entity, all -or most of- the electoral spoils accrue to the actor that delivered the good. On the contrary, if incumbents provide the public good directly they can claim responsiveness and signal a competent type to voters increasing the likelihood of electoral survival.² I call this an efficiency-electoral trade-off. This tradeoff is present both in the case of delegation “within-the-state” between different levels of government, as well as cases where states can delegate policies to supranational entities.³ Given this trade-off,

¹The heterogeneity of tastes and needs of citizens decrease the efficiency of delegation. For more detail see Oates (1972) Decentralization Theorem. For simplicity, I start the paper by assuming delegation always leads to efficiency of public good provision. I prove this to be the case for the delegation of public security provision in Mexico in Section 12, the public good and case study analyzed in this paper.

²Also, by not allowing for upper-level monitoring through delegation, incumbents give leeway to their bureaucracies to overgraze the bribe base through extortions and other rent extraction activities (Schleifer and Vishny, 1993), pleasing potential political brokers. These brokers are particularly relevant for vote gathering in clientelistic systems like Mexico (Larreguy et al., 2017).

³States’ delegation of policies to supranational organizations has been a widely studied topic in the International Relations literature. For a summary see Section 2.

when will incumbents delegate a policy? More importantly, how do electoral incentives affect this decision?

To respond these questions, this paper studies the differential effect of term limited mayors to those with reelection incentives on the delegation decision of public security provision in a country overwhelmed by criminal wars, Mexico. As [Ley and Trejo \(2020\)](#) note, the country's transition to democracy in the early 2000s led to an outbreak of wars among drug cartels, and a proliferation of clashes between the state and criminal organizations. As a result, voters see the monopoly of violence as the most relevant public good demand in the country given the high prevalence of drug-trafficking related crime.⁴

Public security provision in Mexico falls -constitutionally- under the responsibility of local governments. However, since the presidency of Felipe Calderon (PAN, 2006-2012), the Federal government pushed forth the creation of state-level centralized commands in charge of governors, as well as other public security cooperation agreements between municipalities and other political actors -other municipalities, governors from other states, and the President. A delegation choice opened up for mayors, and by 2018 79.12% of municipalities in the country adopted a form of centralized command according to data from the 2019 National Census of Municipal Governments and Territories of the City of Mexico. Besides efficiency considerations, or the obvious threat of facing criminal organizations directly -which have killed mayors in high rates in the country ([Ley and Trejo, 2020](#))-, little do we know if electoral incentives affected the decisions of mayors to delegate security provision or not.

To empirically test the effect of reelection incentives on the delegation of public security provision to the governor, I exploit the 2014 Electoral Reform of Mexico that allowed local executives (*mayors*) to reelect for 2 consecutive periods at most and was rolled out in a step-wedge way at the state level⁵ until 2022. The Electoral Reform, approved in February 2014, was part of the Mexican Pact Accord, a set of structural reforms negotiated by the three main political parties in Mexico at the time (PRI, PAN and PRD). Those in favor of the reform spoke to its potential benefits on politicians' efficiency and professionalization, as well as voter accountability. Three key features characterized the reform: (1) removal of term limits of mayors, and local and federal legislators for up to 2 terms; (2) introduced a "party-lock" where mayors who wish to reelect could not switch parties; and (3) did not weaken party control since nominations and funding still depended on such. In

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

⁵Similar to US states.

other words, the electoral reform created reelection incentives that should increase mayors' responsiveness to constituents vis-à-vis non-term limited mayors. Moreover, it did not modify the strong party system where politicians are highly dependent on their parties for candidate nomination and campaign expenses.

An event-study research design that leverages the staggered implementation of the term limit reform shows that mayors facing reelection incentives decreased the delegation of public security to the governor relative to term-limited mayors by 42%. Results are not explained by pre-trends in security cooperation agreements or an anticipatory behavior of government officials prior to treatment. This result is robust across multiple specifications, including the use of cohort weights to account for treatment effect heterogeneity following [Sun and Abraham \(2020\)](#), changing the reference period, trimming the event study time periods, and standard error corrections for small number of clusters given the small number of states in Mexico (32), level at which the Reform was rolled out. Further validation is provided by the use of secondary research designs including [Imai et al. \(2020\)](#) non-parametric generalization of the difference-in-difference estimator that does not rely on linearity assumption and corrects for invalid negative weighting in standard two-way fixed effects models, as well as [de Chaisemartin and D'Haultfoeuille \(2020\)](#) difference-in-difference with multiple time period correction. In terms of other endogenous concerns, by comparing first period term limited mayors with first period non-term limited mayors, I rule the typical concerns of selection on the experience and ability of politicians ([Samuelson, 1984](#); [Dal Bó et al., 2017](#)) and how this affects performance. This result is confirmed by finding no differences in the education quality of term limited and non-term limited incumbents.

Why do we observe this no-delegation behavior of mayors facing reelection incentives? While incumbents with reelection incentives can turn the direct provision of public goods into a signal of competence and responsiveness which yields an electoral return in the following election, term limited incumbents can't. Term limited politicians can only partially translate the electoral returns won from differentiability and credit claiming to other electoral competitions -say when running for Deputy, Governor or President- or other political and bureaucratic positions in the regional or central administration. We could think of this as a transaction cost that term-limited mayors face when trying to convert their spoils from incumbency -electoral or monetary- to other electoral races or political positions. Given this differential costs, responsiveness and signaling a competent type that takes the "bull by the horns" is more attractive for incumbents facing reelection that does that do not. In a country where citizens support strong candidates ([Ley and Trejo, 2020](#)), taking charge of public good provision mayors cultivate their personal base increasing their reelection

chances. Importantly, this differentiation temptation exists even in party-centered systems like Mexico ([Motolinia, 2020](#); [Rodriguez-Valadez, 2021](#)).

Several results validate this theory. First, reelection incentives do not differentially affect the signing of cooperation agreements with the President or other political actors. These political actors are not in direct contestation for the electoral spoils of public security provision locally. As a result, they serve as a placebo test. Second, heterogeneous treatment effects show that municipalities characterized with citizens concerned by narcotraffic and insecurity show a decrease in delegation of public security relative to municipalities with term limited incumbents: when citizens are concerned of violence, mayors provide public security directly; when violence is not a concern, they prefer the governor take charge of it. Third, heterogeneous effects are also found when citizens hold a high level of trust of state forces: mayors choose not to delegate to avoid living in the shadow of the governor. Lastly, mayors up for reelection and not aligned with the governor choose not to delegate public good provision both to differentiate themselves, increase opposition support ([Rodriguez-Valadez, 2021](#)), and since citizens do not hold the capacity to blame them for violence when not aligned ([Ley, 2017](#)).

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

I draw a novel insight from these findings: while the literature stresses agency problems and the reduction of government's control over policy to not delegate policy, this paper puts electoral incentives in the forefront. Not only does delegation may decrease the level of politicization of a policy, but the politicization actually may lead incumbents to decrease delegation.

A second insight shows that reelection incentives led incumbents to choose electoral spoils above efficiency concerns. While the literature has stressed multiple benefits of term-limit removal such as increased accountability, responsiveness and lower corruption ([Alt et al., 2011](#); [Ferraz and Finan, 2011](#)), increase in the competence of elected politicians ([Dal Bó et al., 2017](#)) and greater legislators' productivity ([Hall and Fourniaies, 2018](#)) once we factor electoral incentives it may yield undesirable inefficiencies. As such, it goes more in line with recent literature on negative effects of reelection, fostering particularistic legislation due to politicians desire to differentiate themselves from others ([Motolinia, 2020](#)), and that longer tenures allows incumbents to collude with local firms leading to fewer bidders in public auctions and more inefficient procurement ([Decio and Gagliarducci, 2017](#)).

While close to the public vs. targeted provision of goods literature, this paper shows an alternative mechanism through which inefficiencies arise: take direct policy action rather than delegating it to others.

Along these lines, this paper contributes to the literature that shows that electoral incentives may lead to inefficient public good provision. [Lizzeri and Persico \(2001\)](#), for instance, show that politicians under-provide public goods that cannot be targeted to voters since they care about the spoils of office. This paper extends this notion to show that incumbents with reelection incentives extract more spoils from portraying responsiveness through delivering public goods directly relative to term limited ones.

Lastly, this paper makes important contributions to the existent literature on the War on Drugs in Mexico. The paper aligns with the findings from [Durante and Gutierrez \(2013\)](#) that found that coordination across municipalities can reduce drug violence albeit through an opposite channel, delegation to the governor. However, results contradict the evidence that pointed the Mexican government as the actor behind large spurges of violence in the last decade ([Escalante, 2011](#); [Guerrero-Gutierrez, 2011](#)). It also adds color to the conclusion by [Dell \(2015\)](#) that municipalities that coordinated with upper-level governments to increase resources to combat crime increased the level of homicides in Mexico. This paper suggests that coordination in the form of delegation to upper-level governments may decrease rather than increase violence.

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

2. Why delegate?

Delegation “is a conditional grant of authority from a principal to an agent that empowers the latter to act on behalf of the former” ([Hawkings et al. \(2006\)](#), p.7).⁶ The delegation

⁶In this paper I separate the concepts of centralization from delegation. I refer to centralization to the specific choice by upper levels of government to provide public goods instead of relying on lower levels of government, i.e. a top-down delegation decision. In contrast, delegation is the decision made by any principal to rely on an agent to provide a public good or service. This makes centralization a special case of delegation. Depending on a country’s constitutional arrangement and the public good we may fall into one or the other. In the case of Mexico, municipalities hold the constitutional responsibility to provide local public security, with the state and federal governments responsible to provide public security in matters only of national or regional threats only. Local governments then face a delegation decision pertaining local public security while the state and federal governments do not hold a centralization-decentralization choice.

literature has studied two different levels of analysis. First, the International Relations literature has long studied the delegation choice that states have with supranational entities. States have delegated justice to international committees and courts to hold themselves accountable to their citizens, monetary policy to supranational entities, the disbursing of foreign aid and credits to multilateral institutions, trade policy to institutions like the World Trade Organization, and even security policy -including military capacity- to multilateral agencies like NATO or the Security Council of the United Nations. By delegating, a state loses its control over foreign policy and introduces agency problems. Why then do states delegate policy to multilateral organizations?

[Moravcsik \(2000\)](#) offers three reasons explain the delegation decision. First, states coerce others to accept the rule of supra-entities. Second, diffusion of the benefits of delegation persuades actors to choose delegation. This argument goes in line with the sharing the burden of policies among players and the pooling of resources in supranational entities ([Milner and Tingley, 2013](#)). Lastly, governments may choose to delegate to combat future threats to domestic governance. For example, states sacrifice sovereignty over human rights to international institutions to decrease domestic political uncertainty and “lock in” policies for the future. Similarly, when powerful states choose to sacrifice policy control, international risk may decrease since the threat of their abuse of power decreases. By doing so, powerful states increase the likelihood of the success of a policy despite losing control of it ([Lake, 2009](#); [Milner and Tingley, 2013](#)). Importantly, powerful states will only delegate if the international organization reflects their preferences and maintains their global influence ([Hawkings et al., 2006](#)). To summarize: not all international -or domestic- delegation comes from a principal’s inability to carry on a policy: principals may be able to accomplish unilaterally a desired policy but sometimes may choose not to do so.

[Rodrik \(1996\)](#) provides two additional reasons behind the delegation choice to supranational institutions: information and a decrease in the politicization of policies. Since information about recipients of transfers or public goods is a collective good, domestic distribution would lead to underprovision. Supra-national agencies concentrate information to guide policies and increase their efficiency. In regard to politics, multinational organizations are less politicized compared to states and local recipients. Autonomy then ensures efficiency. The same reasoning is used to defend the use of independent domestic agencies. However, Rodrik’s empirical analysis shows information and politicization are not the primary drivers of delegation. A related argument states that a centralized planner would prevent overprovision of transfers or services since recipients would not be able to create a competition dynamic between public good providers to win their support. For instance, in states with vertical competition -i.e. where there are multiple public good service providers-, voters may be able to compare their efficiency and punish them electorally

leading to inefficiency in the form of overprovision of public goods (Salmon, 1987; Breton, 1996).

Milner (2006) offers an alternative reason behind the delegation decision by studying the allocation of foreign aid to multilateral organizations such as the European Union, World Bank, IMF, the United Nations and regional banks. She observes that despite the benefits of multilateral organizations, in equilibrium states prefer to deliver aid bilaterally. Her argument focuses on domestic politics of donor countries: when citizens dislike aid, governments spend on multilateral aid; when aid is relevant to them, governments prefer to disburse aid directly since the distribution of aid through multilateral organizations tends to have low domestic support. This argument goes in line with a normative one: delegation may be adopted if citizens' norms have defined it as the most legitimate way to achieve a policy (Finnemore, 1996; Ruggie, 1993; Milner and Tingley, 2013).

It is important to note that delegation is even more complex in the presence of both spillovers and heterogeneous tastes. If only spillovers are present, direct policy control would neglect benefits going to nearby localities, leading to inefficiencies. If heterogeneous tastes exist among citizens "one size fits all" policies may negate local needs. Oates (1972) famous Decentralization Theorem states that in the presence of heterogeneous preferences and without spillovers, decentralization will be preferred in terms of efficiency. Put simply, the more diverse the preferences of citizens the less likely they will agree on a common policy and delegate to an upper level government (Martin, 2006; Lyne et al., 2006). The complexity of the choice of delegation increases if citizens hold the capacity to identify the public good under-provided and elect representatives more in line with their demands ?. Security provision, for example, falls into a particular case of public goods with spillovers and heterogeneity of tastes on the degree of monopoly of violence by the state. This makes the delegation of public security provision a not obvious choice.

As Hawkings et al. (2006) note, the causes of delegation to international organizations are very similar to delegation in domestic politics. This second strand of literature on *within-the-state* delegation has studied the delegation of tax collection, regulation, and securing the monopoly of violence, among others. This is particularly salient in federal systems. As in the international sphere, politics play an important role in the inefficient allocation of public goods. For instance, Khemani (2007) shows that delegation to an independent agency in India constraints the distribution of fiscal transfers to states favored by the central party. The historical legacies of centralization are also among the reasons why governments choose to delegate, with national governments choosing indirect rule -a form of delegation- when greater centralization existed prior to the delegation choice (Gerring et al., 2011).

The conflict literature has studied the topic of delegation of public security in two forms. First, top-down delegation from central governments to local proxies to suppress violence. Second, bottom-up delegation from subnational units to national government in charge of tackling down non-state armed groups.

In top-down delegation, national executives face internal security threats either by (i) doing nothing, (ii) take direct action through the state's security apparatus, (iii) provide unconditional assistance to their agent (capacity building), (iv) replace local agents, and/or (v) rely on indirect means to tackle non-state challengers such as the use of local proxies and a system of rewards and punishments [Berman and Lake, eds \(2019\)](#). Two features define the strategic choice: the size of the security disturbance -correlated with the interest of the national executive to deal with the issue at hand-, and the the cost of effort of the agent. These costs represent both the direct costs of facing an internal enemy, agency costs, and the divergence in the preferences between principal and agent. When central states are interested in the security disturbance and costs are small, suppressing violence through local agents is the obvious policy choice. There are still problems with principals' optimal control of agents though: (1) weak principals may be unable to impose punishments on agents for shirking; (2) cost-constrained principals cannot reward effective effort from agents; or (3) principals may misread the interests of local agents [Berman and Lake, eds \(2019\)](#). In such cases, delegation would lead to inefficient outcomes even if delegation to subnational units is the most efficient choice.

The results of top-down delegation are mixed though. Central governments have used indirect rule to control nationalism ([Siroki et al., 2021](#)), but in cases like India, Pakistan, Burma, Nepal, Peru and Colombia it fosters conditions for insurgency ([Mukherjee, 2018](#)). Besides the conflict literature, indirect rule has led to a decrease in the quality of government proxied by lower levels of schooling, the number of health centers, and infrastructure projects in the case of India ([Lyer, 2010](#)), a lower level of political development ([Lange, 2004](#)), ethnic stratification and conflict in Africa ([Blanton et al., 2001](#)) and greater salience of ethnicity ([L., 2019](#)), a decrease of ethnic inclusion ([McAlexander, 2020](#)), lower overall support for democracy ([Lechler and L., 2018](#)), and lower levels of trust of foreign institutions ([Okoye, 2021](#)). However, there are cases like Cameroon where delegation through indirect rule improved economic development through the empowerment of local authorities and communities, an increase in state legitimacy, and the reification of ethnic identities ([Letsa and M., 2020](#)).

In contrast to top-down delegation, bottom-up delegation implies subnational units' choice of giving away the capacity to provide security locally. As with the top-down approach, local incumbent's delegation choice depends on the assessment of the size of the

security disturbance and the cost of effort of the agent. Delegation to upper-levels of government may increase the service capacity and reduce the likelihood of citizens joining non-state armed groups, an outcome stressed by the winning the hearts and minds literature (Beath et al., 2013; Berman et al., 2011; Dell and Querubin, 2018). However, if they choose to delegate, delegation might still be inefficient if an optimal control on the agent is not achieved (if the agent decides to shirk and the principal cannot do anything about it). If they choose to take direct action to address the security distortion, concerns on capacity arise, and local incumbents may be more prone to capture, coercion and strife Chacon (2018).

While there are several reasons behind the choice of delegation, this paper proposes an alternative mechanism (albeit widespread) that may lead incumbents not to delegate security provision to upper-level governments: electoral incentives that make him undervalue inefficiencies -such as crime distortions- and the existence of more capable agents to address them. In other words, if we hold constant the size of the security disturbance the cost of effort of the principal and agent to fight crime, electoral interest of the principal lead it to take direct action through its security apparatus.

3. The role of reelection incentives in the delegation of public goods

While there may exist a midground in the level of delegation of public goods,⁷ I will focus on two extreme ways in which incumbents may be rewarded electorally. If local public good provision is fully delegated to an upper-level government, all the spoils of public good provision go to the political actors that delivered the good. If no delegation occurs, spoils go to the local incumbent.⁸ By spoils, I mean the electoral benefits for an incumbent of being able to promise the implementation of a policy in campaign and the actual implementation of such, as well as other rents from being in office if elected.

What is the role of reelection in such setting? Reelection provides the opportunity for incumbents to translate spoils into electoral benefits -and rents- in future elections. This does not imply that incumbents up for reelection are the only ones that can differentiate themselves from other political actors to create a positive electoral return. Term limited politicians could do the same. However, term limited politicians can only partially translate the electoral returns won from differentiability and credit claiming to other electoral competitions -say when running for Deputy, Governor or President- or other political and

⁷For instance, in terms of public security provision you may have partial delegation in which you allow upper levels of government carry on research and intelligence operations but not local policing.

⁸In the case of partial delegation, spoils maybe distributed among the political actors that citizens believe delivered the good.

bureaucratic positions in the regional or central administration. This is similar to thinking that term limit incumbents face a transaction cost when trying to exchange the spoils of incumbency to other political races. Another way to think about this is to consider that electoral spoils for incumbents up for reelection are higher than those with a term limit. This feature alone generates an important difference between term limited and non-term limited politicians, with the latter viewing the option of providing public goods locally as the one that may yield the highest electoral return. By taking public good provision into their own hands, incumbents seeking reelection signal responsiveness to citizens as well as competence. In contrast, the “[t]he most elementary prediction of the accountability models is that a term-limited incumbent, who cannot derive any benefit from impressing the voter, will not be responsive to voters” ([Ashworth \(2012\)](#), p. 194) or have an interest of showing so.

The use of policy to signal competence has long been studied in political science. In general, the electorate does not have the ability to observe incumbent’s actions but can proxy its performance by the policy choice ([Ferejohn, 1986](#)). Through a large review on electoral accountability, [Ashworth \(2012\)](#) concludes that “incumbents’ incentives are driven not by the voters’ evaluation of the normative desirability of outcomes but by the outcome’s information about the incumbent’s type (e.g., competence or ideology)” (abstract). Incumbents up for reelection manipulate policy to signal their capacity and influence voters’ electoral support. This is particularly relevant in settings where voters have imperfect information of incumbents’ competence.

To signal competence and responsiveness, incumbents with reelection incentives reduce taxation, and modify spending close to electoral periods ([Rogoff and Sibert, 1988](#); [Rogoff, 1990](#); [Klein and Sakurai, 2015](#)). [Drazen and Eslava \(2005\)](#) show that incumbents carry particularistic spending prior to elections to send a signal to voters on the type of expenditures to expect if reelected. In contrast, incumbents who cannot run for reelection increase spending and taxes ([Besley and Case, 1995](#)), a behavior known to carry an electoral punishment from voters ([Peltzman, 1992](#)). Similarly, [Schettini and Terra \(2020\)](#) find that mayors seeking reelection in close elections reduce the amount of funding to the municipal Public Employees’ Retirement system in Brazil; since increasing the rate of contributions to pension systems is similar to raising taxes, this behavior signals responsiveness to constituents. Moreover, mayors can utilize the retained contributions to finance other expenses of interest, including particularistic spending. [Akhmedov and Zhuravskaya \(2004\)](#) pushes the theory behind the responsiveness of incumbents seeking reelection by showing that they change the spending composition only for those items that are visible to the electorate. A similar finding is shown by [Ferraz and Finan \(2011\)](#) when analyzing

the effect of randomly assigned audits on corruption on mayors in Brazil: they find that corruption which is not visible to voters is less responsive to reelection incentives.

Evidence from reelection studies point to an increase in the competence of elected politicians (Dal Bó et al., 2017), reduced corruption (Ferraz and Finan, 2011), increasing legislators productivity (Hall and Fourniaies, 2018) and greater welfare -higher economic growth, taxes and spending- from increasing effort in favor of voters (Alt et al., 2011). Reelection also decreases moral hazard and allows to retain better politicians in office (Smart and Sturm, 2013). At the same time, nonetheless, reelection creates incentives for inefficient political targeting. A recent study by A. (2021) finds that in Brazil mayors up for reelection target poor households with greater likelihood and as a result win their electoral favor in the next election. Similarly, Motolinia (2020) shows that in Mexico legislators up for reelection increase particularistic legislation to differentiate themselves. Moreover, since Nordhaus (1975), we know presidents with reelection incentives exercise pressure on central banks to exploit the tradeoff between inflation and unemployment giving rise to political business cycles. While this paper does not focus on the comparison between particularistic and public good spending, it speaks to this literature by providing a new avenue through which incumbents seeking reelection may generate inefficiencies: directly provide public goods when delegating them to upper-level governments is a more efficient choice.

3.1. The puzzle

It may seem straightforward that reelection incentives should lead incumbents provide public goods directly instead of delegating them. There are three reasons, however, that makes the delegation choice an interesting puzzle of study. First, it is not clear that citizens will reward incumbents for policy implementation or its results, or both. At the end, voters may punish incumbents not only for the choice of delegation but the outcome of delegation.

Let's use the example of the delegation of public security provision. Consider the following four scenarios. First, an incumbent may choose to delegate public security provision to the governor. By doing so, he cannot collect spoils from public security provision and can only be partially blamed if violence increase: they are not directly responsible for the public good but made the wrong choice of delegating it to inefficient agents. Second, an incumbent may choose to delegate, and violence decreases. As before, they obtain partial rewards from delegating to an efficient agent but cannot claim full responsibility for the efficient outcome. Third, an incumbent may choose not to delegate public security provision and violence could follow. The lack of capacity or resources may explain the

inefficient outcome. A steep learning curve or the costs of crime may explain inefficiencies in the short term. In this case, however, incumbents can still credit claim the fight against crime and show responsiveness to citizens demands. Incumbents may even utilize a media strategy to show that fighting the enemy through local means is the right choice, but it will take a while till violence decreases. This strategy was widely used by the Felipe Calderon administration in Mexico, for example. Lastly, an incumbent may not delegate public security provision and violence could decrease. As before, the electoral spoils will fully fall in the incumbent's hands. This example gets even more complex if we remove the assumption that the incumbent can optimally control the behavior of the agent if it chooses to delegate.

Two additional concerns make the delegation decision a difficult choice for incumbents. On the efficiency side, security provision is characterized by spillovers and a heterogeneity of tastes. Following the discussion in Section 2 while spillovers make delegation the most efficient choice following Oates (1972)'s Decentralization Theorem, the heterogeneity of tastes raises efficiency concerns on centralized public good provision. Lastly, as the case of Mexico shows, tackling drug trafficking organizations has been costly for politicians: they have been killed in high rates, particularly those belonging to the PRI (Ley and Trejo, 2020).

3.2. Hypotheses

Given the aforementioned discussion the main hypothesis of the paper is as follows:

H1: Compared to term limit incumbents, incumbents seeking reelection will decrease the delegation of public security provision to the governor.

This no-delegation behavior should increase when the contributions of each level of government are clear to citizens (Treisman, 2000). By studying conditional cash transfer and health programs in Brazil and Argentina, Niedzwiecki (2018) shows that political alignments across different levels of government jeopardize the implementation of national policies: when opposition parties control subnational levels of government, they hinder the implementation of national policies creating a heterogeneity in terms of coverage. This result, however, is conditional on citizens having the capacity to attribute blame to the actor that delivers the public good. In the case of Mexico, Ley (2017) shows that citizens hold the capacity to blame local executives for inefficient public security provision if their party is aligned with the governor. When not-aligned, citizens do not punish mayors for inefficient security provision. This leads to the following testable hypothesis:

H2: Compared to aligned and not aligned term limit incumbents, incumbents seeking reelection and aligned with the governor will decrease the delegation of public security provision to the governor.

Lastly, if incumbents with reelection incentives deliver goods directly to citizens to fore-shadow responsiveness, this should only hold for goods that citizens deem as valuable. In other words, electoral spoils are more efficient when the public good is more valuable to constituents ([Lizzeri and Persico, 2001](#)). This is similar to the findings by [Milner \(2006\)](#) on the condition of citizen preference on the use of states of multilateral organizations to deliver foreign aid. Her argument states that when citizens dislike foreign aid, governments spend on multilateral rather than bilateral aid; when aid is relevant, the opposite happens since the distribution of aid through multilateral organizations tends to have low domestic support. In the context of American politics, [List and Sturm \(121\)](#) find that governors seeking reelection adopt greener policies when their states hold large pro-environmental groups, while the opposite occurs in those with lower environmental support. In a similar tenor, we would expect that when citizens have high concerns of insecurity (place high on the monopoly of violence) then incumbents with reelection incentives will provide security directly:

H3: Compared to term limit incumbents, incumbents seeking reelection whose citizens hold high levels of concern about violence will decrease the delegation of public security provision to the governor.

4. 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 ([Rios, 2013](#); [Dell, 2015](#)), (b) state efforts to reduce DTOs operations ([Rios, 2013](#)) 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 ([Dell, 2015](#)), and cocaine supply shortages ([Castillo et al., 2018](#)).

As a result, since 2007 and up to the COVID-19 pandemic public security provision has been the main public problem in the country.⁹

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. DTOs leadership removal increased inter and intra-cartel fighting, fragmenting criminal organizations with violent spillovers on the overall population (Guerrero-Gutierrez, 2011). Troops deployment seemed to play a significant role in the escalation of violence in Mexico (Escalante, 2011) and have been linked to human rights violations and more than 30,000 disappearances (Daly et al., 2012; Moloeznik and Suarez-de Garay, 2012; Magaloni et al., 2018). 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 (Dell, 2015).

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 inexistent, to my knowledge, and have not systemically 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 (Ch, 2020). 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 (Aguayo, 2001; Moloeznik, 2010; Lopez-Gonzalez, 2012) and federal and local police forces in Mexico (Zepeda, 2010; Sabet, 2012; Lopez-Portillo, 2017; Davis, 2017) 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 were made to federal

⁹For an example, see this survey by El Financiero https://www.dropbox.com/s/c5dte5pscggat2c/leadingproblem_mexico.png?dl=0.

and local police forces, there are still precarious work conditions and salaries, low training and no institutionalized professionalization of police forces.

4.1. Security Cooperation Agreements in Mexico

An additional policy tool to counteract crime in Mexico has been the delegation of public security provision from municipalities to other governments. 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 reached 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.¹⁰

Two important notes on centralized commands. First, not all Central Command Agreements imply a *de jure* delegation of municipal public security provision to the state. There

¹⁰There 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 that makes public ministries (*Ministerios Públicos*) 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 (Moloeznik, 2016). 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/>.

is wide variation of what central command implies, and could take all or any of the items mentioned before, from security provision to 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 not understand the terms and conditions of cooperation agreements fully. Lastly, the existence of various types of agreements with upper level governments creates an interesting avenue to test the theory of this paper. While governors are in direct contestation with mayors for electoral spoils, other actors in the country are not.¹¹ This makes other cooperation agreements with other actors besides the governor potential placebos.

5. 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 reelect. 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.¹² The PNR non-reelection strategy weakened local party bosses and allowed the party to control political careers at the federal, state and local level (Weldon, 2003).

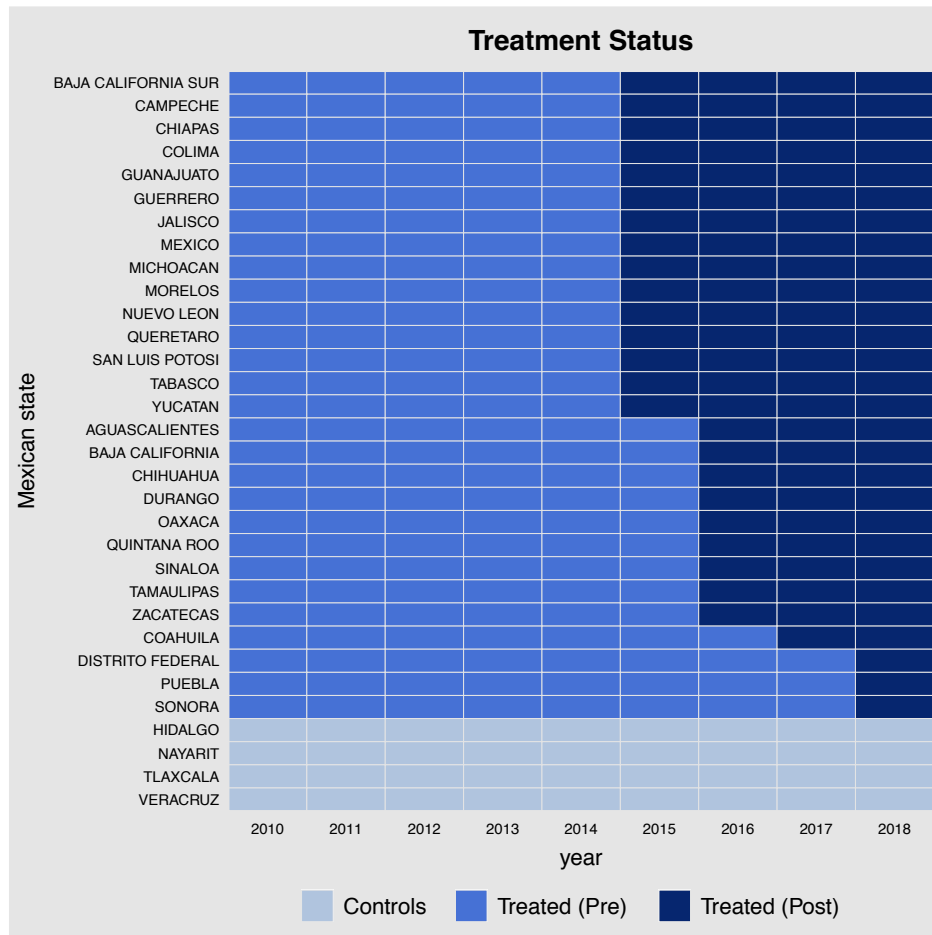
For federal legislators, consecutive reelection was allowed up to 4 four terms. In the case of state-level legislators the reform introduced consecutive reelection for up to four terms with a maximum of 12 years; for the case of mayors, reelection was allowed for up to 2 consecutive periods at most. 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 (Motolinia, 2020), all

¹¹For more detail, see the article “Mando Único Policial: el modelo fracasado” from <https://www.proceso.com.mx/515386/mando-unico-policial-el-modelo-fracasado>

¹²Anti-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.

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.

FIGURE 1. Mexican States Electoral Reform Treatment Status



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 were starting pushed reelection further down the line ([Motolinia, 2020](#)).¹³

¹³For more detail on the political background please see Appendix A.

Figure 1 describes the implementation period or treatment status of each of Mexico's 32 states.¹⁴ This figure 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.

6. Data

I construct a novel database on security cooperation agreements signed by municipalities with upper-levels of government in Mexico from 2010 to 2018. I use the Municipal Government Census (*Censos de Gobierno Municipal*) which collect information on the security capacity of municipalities, as well as their relationship with other governments and institutions. In particular,...

[MISSING TO COMPLETE THIS PART AND HAVE AN UPDATED DESCRIPTIVE STATISTICS TABLE.]

Lastly, to test the consequences of not delegating public security provision on public security 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 2018.¹⁵ 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. On 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 Rivera (2012) 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 those based on initiated investigations tend to miscalculate the total number of deaths.¹⁶ 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

¹⁴Mexican states share the same administrative level as US states.

¹⁵Mexico has 2,467 municipalities. Thus, the total possible number of observation is 22,203 municipality-years (2,467 municipalities X 9 years).

¹⁶For instance, borrowing a practical example by Rivera (2012), 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.

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 [Mackinnon and Maggie \(1990\)](#), 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.¹⁷

To approximate the level of effort placed by municipal forces and 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.¹⁸ The dataset includes narcotic eradication (kg) of marijuana, heroine, methamphetamine, and cocaine, as well as marijuana and poppy kg per hectare eradicated, 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.

7. Research Design

To investigate whether reelection incentives affected the delegation of public security provision in Mexico, I explore the variation in the reelection incentives generated by the removal of term limits. This comparison has been widely used in the social sciences to identify the effect of reelection incentives since [Besley and Case \(1995\)](#). Specifically, I exploit the staggered implementation of the 2014 Electoral Reform in Mexico that removed term limits for local executives up to two periods. I compare first-term mayors who are term limited to first-term mayors who can be reelected, “and so faced the same selection pressures, but have different incentives to impress the voters. Comparing these governors isolates the incentive effect.” ([Ashworth \(2012\)](#), p.196). Comparing first term incumbents allows me to tease important endogenous concerns, particularly those arising from selection including differences in the experience and ability of incumbents ([Ferraz and Finan, 2011](#)).

A two-way fixed effect model at the municipal and year level would be the go to specification. However, recent literature has shown that in the presence of staggered treatment timing and heterogeneous treatment effects across cohorts, the coefficient from two-way

¹⁷For more detail on the SNSP homicide database please see Appendix ??.

¹⁸Information petitions number 0000700274419 and 0000700274519.

fixed effects models are not causally interpretable (Goodman-Bacon, 2018; Callaway and Sant’Anna, 2019; Strezhnev, 2018; de Chaisemartin and D’Haultfoeuille, 2020). Furthermore, for event-study designs Sun and Abraham (2020) 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 Sun and Abraham (2020) 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.¹⁹ I exclude 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 Sun and Abraham (2020), I also exclude $k = -8$ due to collinearity. The estimated equation is as follows:

$$(7.1) \quad y_{mt} = \mu_m + \mu_t + \sum_{e=1}^5 \sum_{k=-7, \neq -8, -1}^3 \gamma_{e,k} (1\{E_i = e\} \cdot R_{m,t}^k) + \sum_{e=1}^5 \sum_{k=-7, \neq -8, -1}^3 \Theta' X_{s(m)t} (1\{E_i = e\} \cdot R_{m,t}^k) + \epsilon_{mt}$$

where y_{mt} is a dummy that takes the value of 1 if a municipality has a cooperation agreement with the governor, 0 otherwise. E_i are cohort-specific indicators if a Mexican state removed term limits in a given year.²⁰ $R_{m,t}^k \in \{0, 1\}$ is the Term Limit Reform treatment status indicator at period k relative to treatment, for municipality m at calendar time t .²¹ $X_{s(m)t}$ is a matrix of state s (municipal m) level covariates interacted with the set of cohort-specific fixed effects including pre-treatment governor’s winning margin, party alignment with the governor, party alignment with the President, mayor’s winning margin, logged homicide related deaths per capita, and a dummy indicator on the presence of cartels. The year indicators by treatment cohort $\gamma_{e,k}$ are the difference-in-difference (DiD) estimators

¹⁹See Figure 1 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 $k \in \{-8, 3\}$.

²⁰As noted in Figure 1, 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.

²¹ $t = e + k$.

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. Since the number of clusters is small (32 states), I adjust standard errors using wild bootstrap when indicated.

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 [Sun and Abraham \(2020\)](#):

$$(7.2) \quad \hat{\nu}_g = \frac{1}{|g|} \sum_{k \in g} \sum_e \hat{\gamma}_{e,k} \hat{Pr}\{E_i = e | E_i \in [-k, T - k]\}$$

where $\hat{\gamma}_{e,k}$ is returned from equation 7.1 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 $|g| = 1$. Lastly, to estimate the average treatment effect from t to $t + 3$ I run the linear average of weighted coefficients across the CATTs of those time periods relative to treatment.

8. Main Results

Reelection incentives decreased delegation of public security provision to the Governor. See Figure 2.

[COMPLETE THIS SECTION]

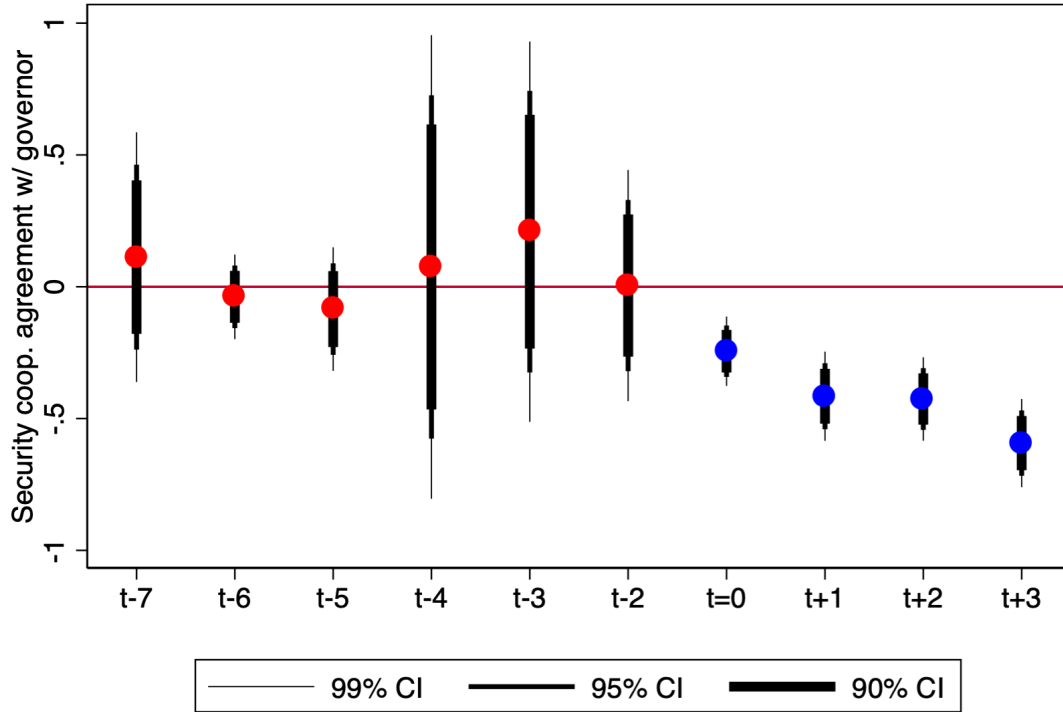
8.1. Identification

Once we consider cohort weights, I find strong evidence of parallel trends as noted in Figure 2.

[COMPLETE THIS SECTION]

Besides no 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 1. 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 7.1 interacts the state covariates with cohort-specific fixed effects to adjust for any changes correlated to the evolution of governors strength, the term

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



Note: Figure 2 shows the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Red points are pre-treatment, while blue ones post-treatment.

$X_{s(m)t}(1\{E_i = e\} \cdot R_{m,t}^k)$. Covariates include governor winning margin and an indicator of governor party alignment with the Federal Executive, since pressure from former President Enrique Peña Nieto modified state legislators approval of the electoral reform, particularly for PRI states (for more detail see Section 5). 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 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 18, 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.²²

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.

9. Robustness

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Main takeaways:

1. Results are robust across multiple specifications and models.

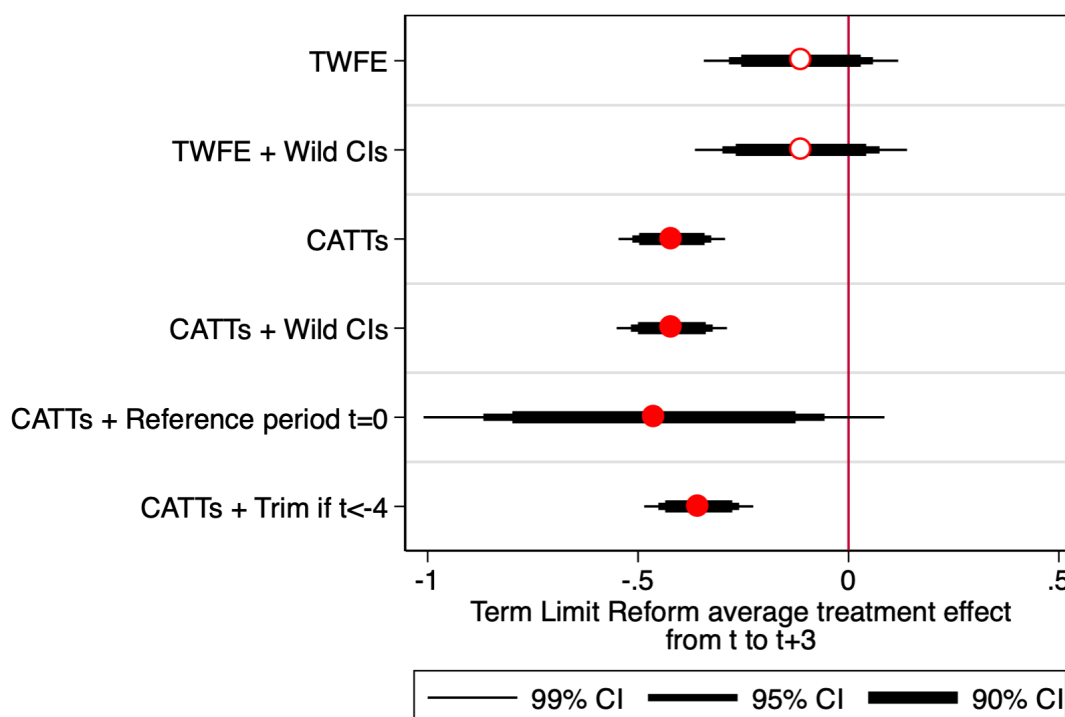
TABLE 1. Effect of 2014 Term Limit Reform on Signing Security Cooperation Agreements, Average Effect

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#) except for the specification that trims periods prior to t-4. Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. ^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.

²²For more detail on how I compare early to late adopters, please see Appendix C.

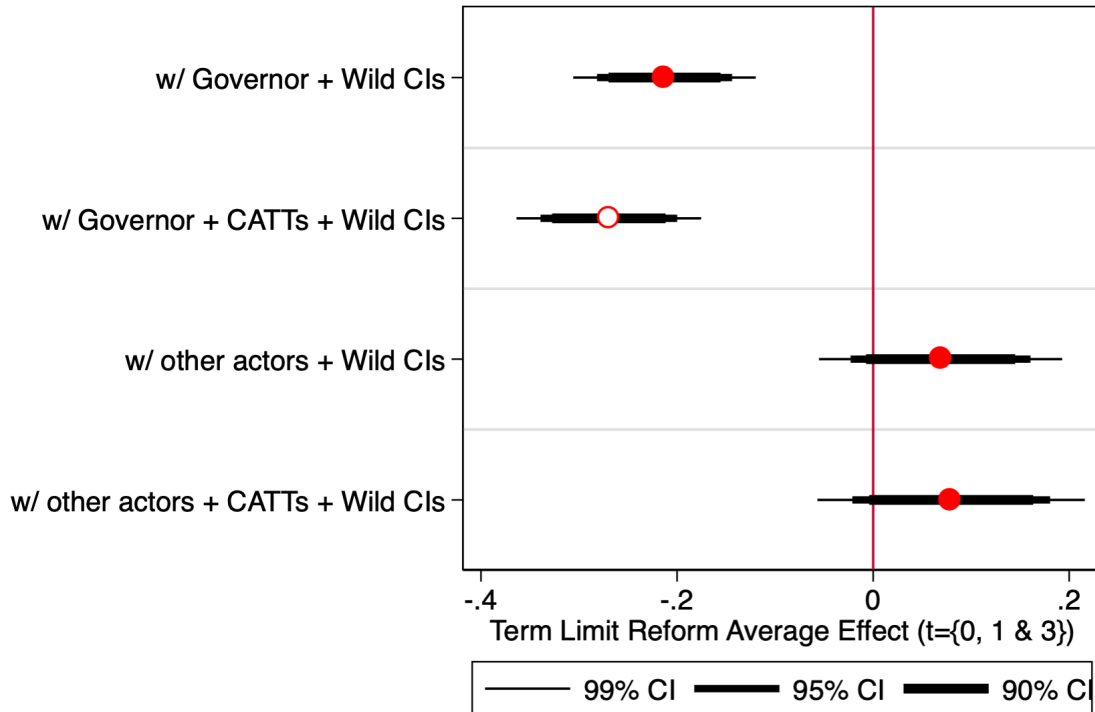
FIGURE 3. Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, 2010-2018



Note: Figure 3 shows the average treatment effect from t to $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends.

2. Placebo with agreements with other actors besides the governor.

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



Note: Figure 4 shows the average treatment effect from t to $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) 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.

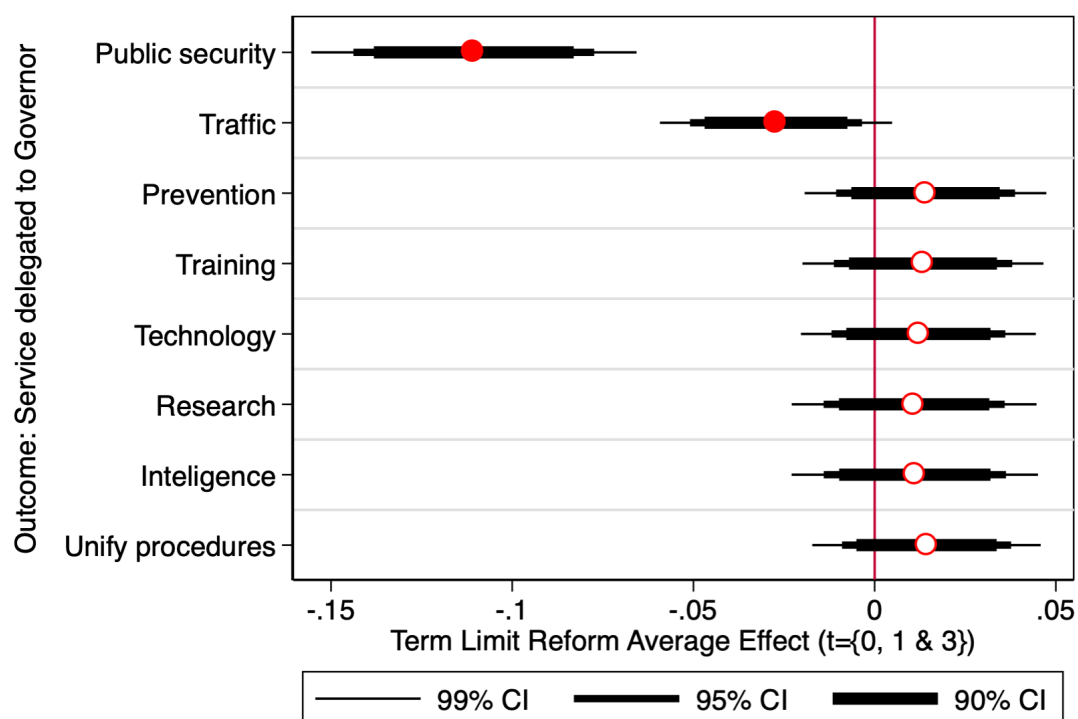
Further validation is provided by the use of secondary research designs including [Imai et al. \(2020\)](#) non-parametric generalization of the difference-in-difference estimator that does not rely on linearity assumption and corrects for invalid negative weighting in standard two-way fixed effects models, as well as [de Chaisemartin and D'Haultfoeuille \(2020\)](#) difference-in-difference with multiple time period correction. I also ran a test on selection on unobservables. This results are found in Appendix Section B.

10. Mechanisms

Main takeaways:

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

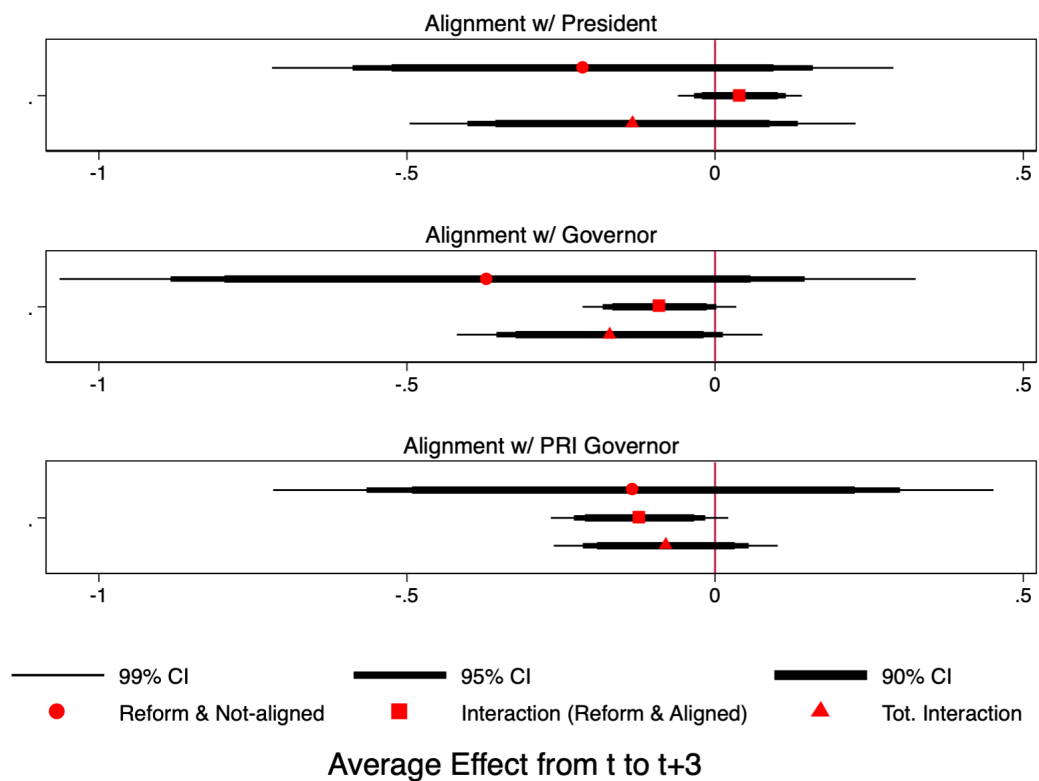
FIGURE 5. Comparison: Security Cooperation Agreements with Governor vs. Other Actors, 2014-2018



Note: Figure 5 shows the average treatment effect from t , $t+1$ and $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends.

2. Alignment: First, if not aligned you decrease delegation of public security. Second, greater negative effect if not aligned since citizens do not blame you as much for public security inefficiencies following [Ley \(2017\)](#). No difference should exist for PRI since they are the main opposition party at the moment. I find these results but some are noisy.

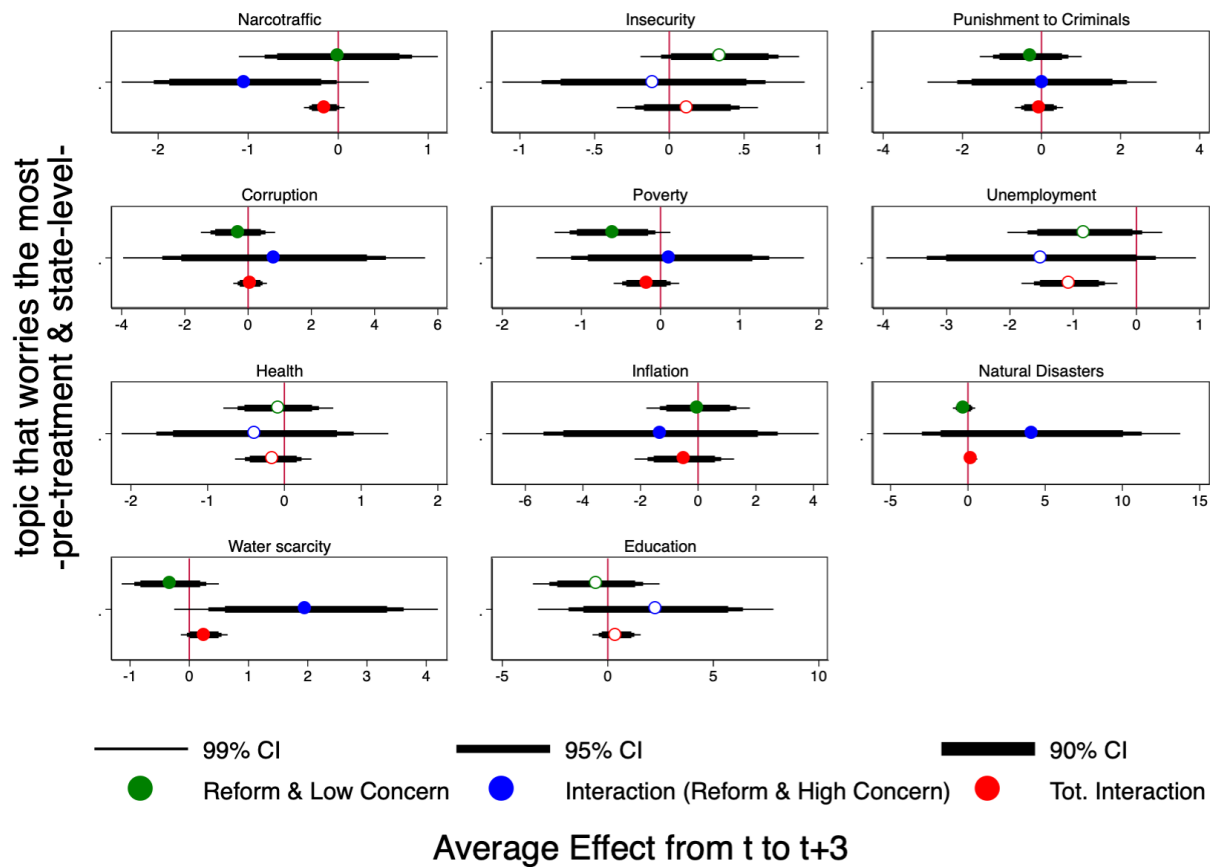
FIGURE 6. Reform interaction with Party Alignment



Note: Figure 6 shows the average treatment effect from t to $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Red points show that parallel trends hold, while hollow ones imply pretrends. To check parallel trends see Appendix Figure B-9.

3. a. Mayors facing reelection decrease delegation when citizens are concerned about insecurity issues. 3. b. Mayors facing reelection increase delegation when citizens are concerned about problems "too big" or of the national order. This serves as a sort of placebo or validation instrument.

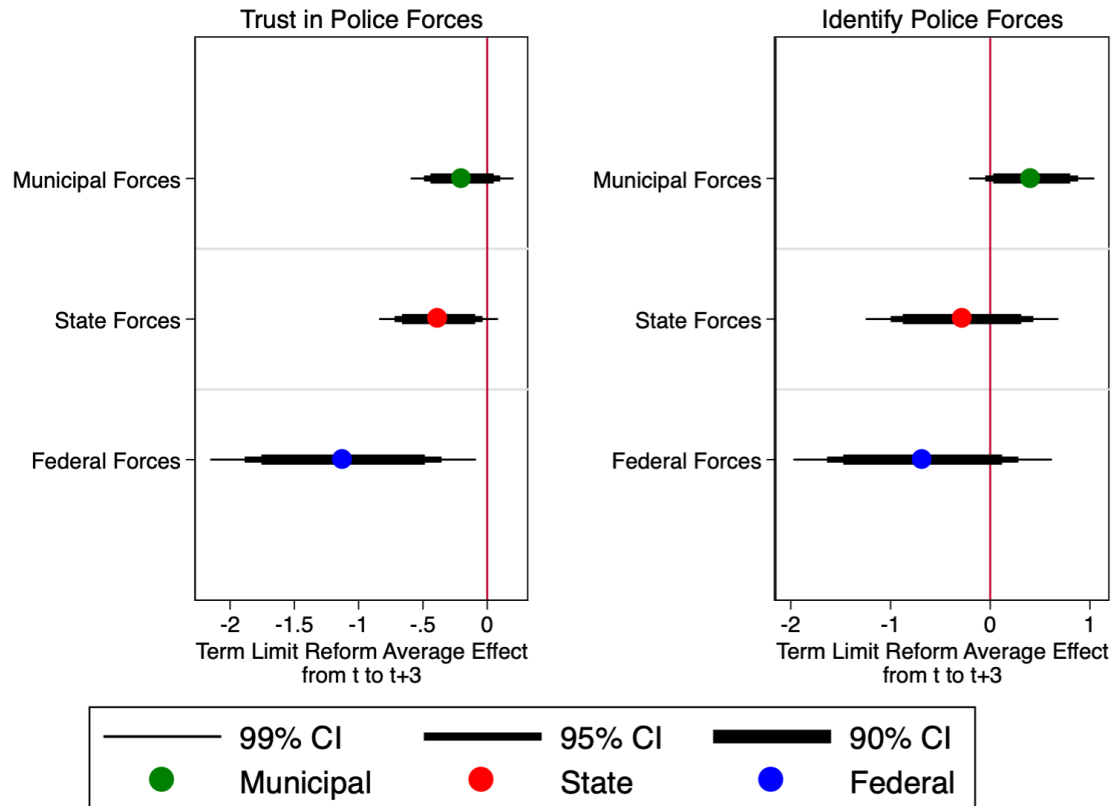
FIGURE 7. Interaction effects by citizens' preferences



Note: Figure 10 shows the average treatment effect from t to $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following Sun and Abraham (2020) for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

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

FIGURE 8. Total interaction effects by citizens' trust and identification of police forces



Note: Figure 8 shows the average treatment effect from t to t+3 across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

11. Ruling out Alternative Hypothesis

11.1. Selection: incumbents and challengers quality

[I HAVE THIS RESULTS IN ANOTHER PAPER ON THE EFFECT OF THE 2014 ELECTORAL REFORM ON INCUMBENCY ADVANTAGE. THERE I SHOW THAT INCUMBENTS AND CHALLENGERS QUALITY IS NOT DIFFERENT IN MUNICIPALITIES WHERE MAYORS CAN REELECT OR ARE TERM LIMITED.]

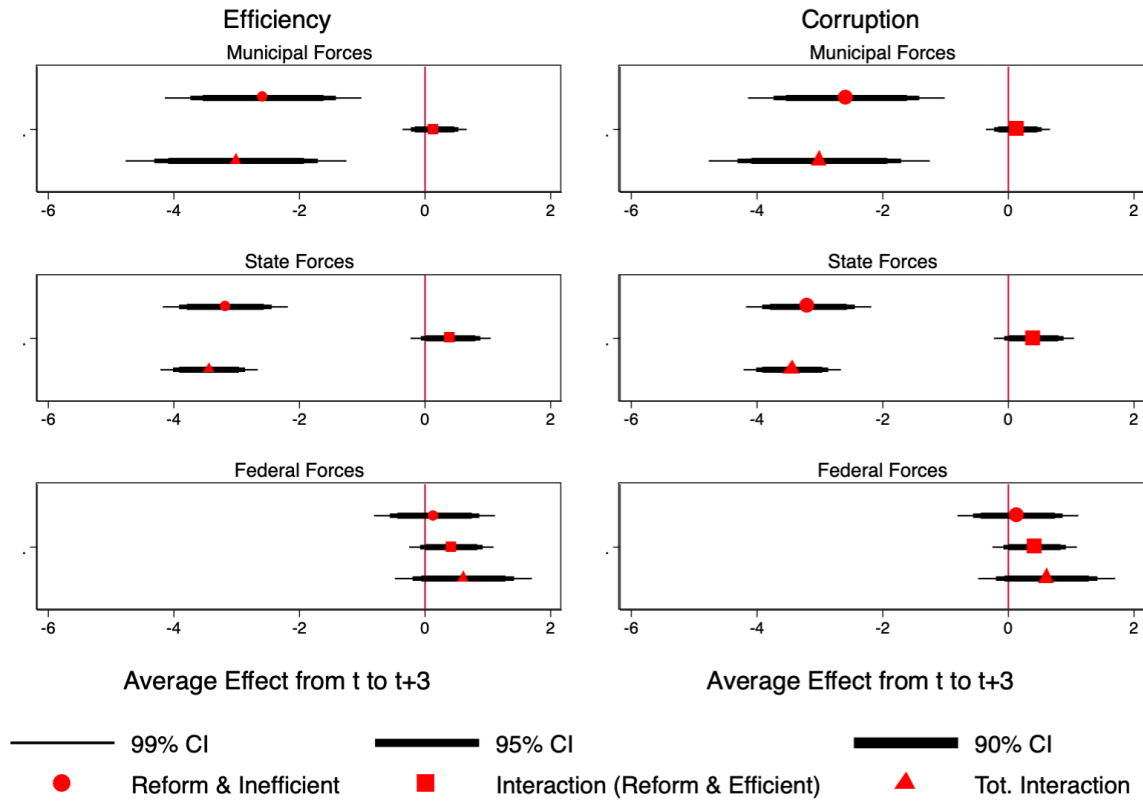
11.2. Cartel Presence

All regressions control for pretreatment Cartel Presence and crime related deaths per capita.

11.3. Citizens' Evaluation of Corruption and Efficiency of Police Forces

Main takeaway: mayors seeking reelection are not dumb: they decrease delegation only when municipal forces are seen as inefficient or corrupt (so mayors are being responsive to citizen concerns); they also decrease delegation when state force are inefficient or corrupt (they are not to be trusted effect). No difference with Federal forces since those are a placebo.

FIGURE 9. Interaction effects by citizens' evaluation of efficiency and corruption of police forces



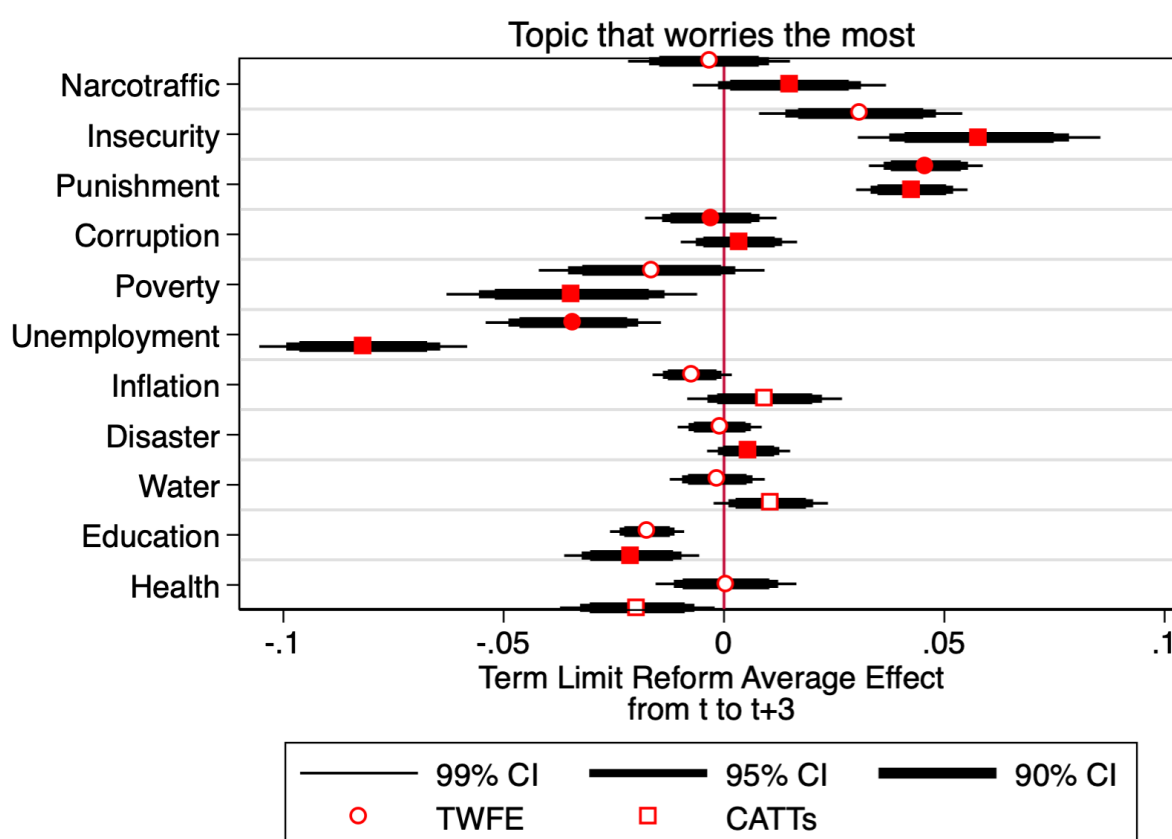
Note: Figure 9 shows the average treatment effect from t to $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

12. Unintended consequences

12.1. Preferences for order and security

1. PREFERENCES: citizens increase their valuation of insecurity as a pressing issue and decrease the valuation of other topics. Recall results are conditional on violence. So in the next election, they will vote for another hawk. This ties to the incumbency advantage.

FIGURE 10. Effect of Term Limit Reform on Citizens' Preferences

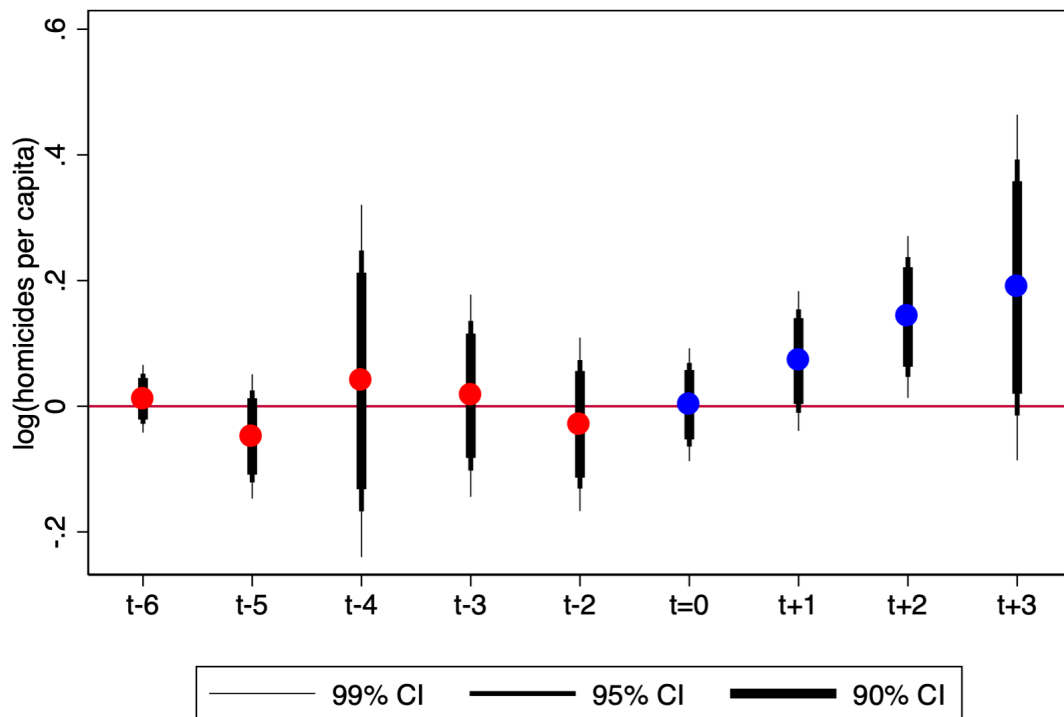


Note: Figure 10 shows the average treatment effect from t to t+3 across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Filled points (squares) show that parallel trends hold, while hollow ones imply pretrends.

12.2. Security underprovision and violence

Main takeaway 1: reform increased violence.

FIGURE 11. Effect of Term Limit Reform on Violence



Note: Figure 11 shows the IW estimators following [Sun and Abraham \(2020\)](#) for each lead and lag relative to the first year a municipality implemented reelection. Red points are pre-treatment, while blue ones post-treatment.

Main takeaway 2: the increase in violence is mediated by the decrease of delegation. Table 2 shows a 2SLS model where agreement are instrumented by the cohort-weighted event study design of Figure 2.

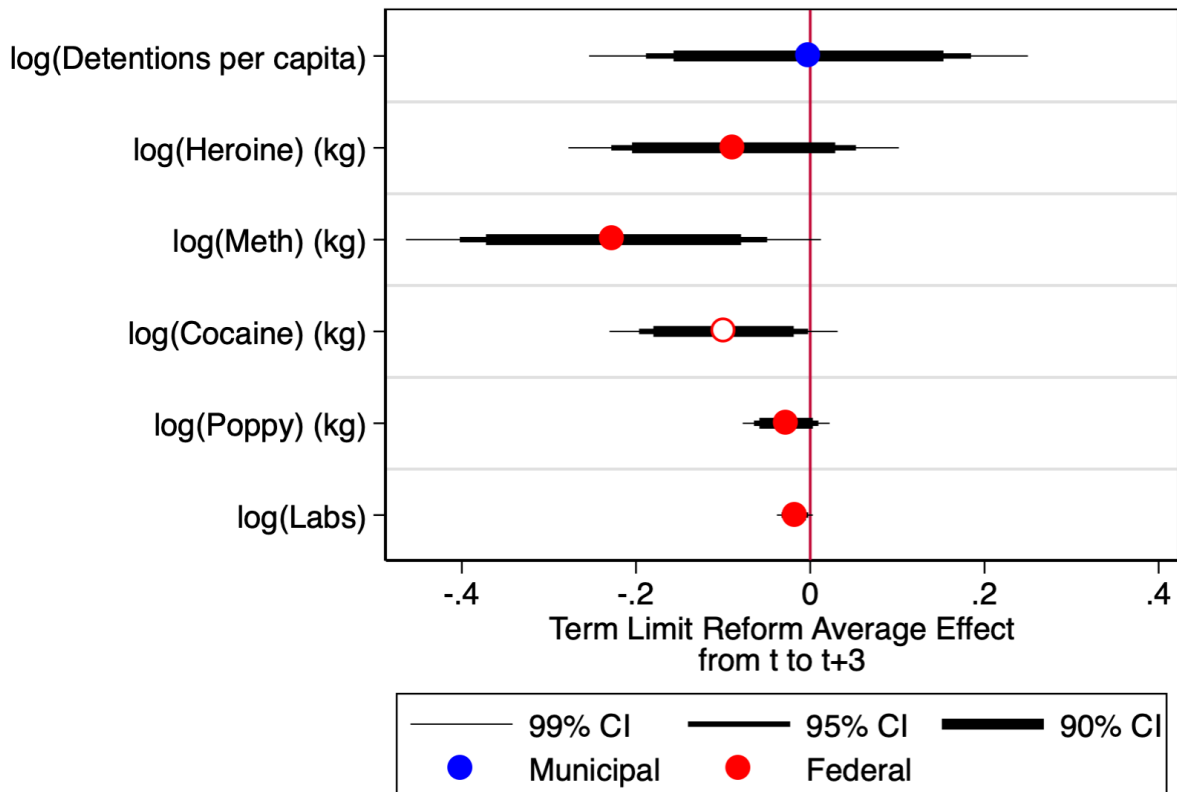
TABLE 2. Effect of Security Cooperation Agreements signed with the Governor on Violence

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) were removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). Standard errors in parentheses are clustered at the state level unless indicated, with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. ^a Pre-treatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; and Cartel presence. ^a Wild bootstrap standard errors clustered at the state-level are reported when indicated.

Main takeaway 3: the term limit reform did not decrease security provision by municipal forces, only federal ones. So mayors seeking reelection might be performing normally to fight crime relative to term limit mayors.

FIGURE 12. Effect of Term Limit Reform on Effort of Local and Federal Security Forces



Note: Figure 12 shows the average treatment effect from t to $t+3$ across multiple specifications. This average effect was estimated using the IW estimators following Sun and Abraham (2020) for each lead and lag relative to the first year a municipality implemented reelection. Filled points show that parallel trends hold, while hollow ones imply pretrends.

13. Conclusion

[I NEED TO FINISH THIS]

This paper studies a classic problem faced by governments with a supra-entity capable of providing and delivering public goods: to delegate or not to delegate. Specifically, it delves into an understudied phenomenon of delegation, the role of electoral incentives.

I find that reelection encourages mayors to focus on policies with the highest “electoral yield”—namely, not to delegate public security to upper-level governments. Why? By directly delivering public security mayors portray responsiveness and signal a competent type to voters. Since delegation of local security forces to the governor is the most efficient choice in this setting, reelection incentives lead to an inefficient outcome: an increase in violence.

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Appendix A. 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.²³ 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. Cantu (2019) 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.²⁴ The final resolution of IFE’s Council and the TEPJF increased citizens and opposition mistrust on electoral institutions.

By early 2013, the Peña Nieto administration pushed an aggressive set of reforms to privatize the energy sector and modify the existent fiscal institutions in the country. To increase the probability of success, the PRI with the PAN and PRD, the three main political

²³The PRI won with 38.2% of total votes, followed by the PRD with 32.6% and the PAN with 25.390%.

²⁴In 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.

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.²⁵ 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 (Zamitz-Gamboa, 2017). 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."²⁶

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".²⁷ 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.²⁸ By January 2014, PAN and PRD threatened to back the energy reform if the PRI did not push local state legislatures from approving the electoral reform, a constraint imposed by the Mexican constitution, which

²⁵The 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.

²⁶From Cfr. Compendio de Legislacion Nacional Electoral, Mexico, INE, FEPADE, UNAM, TEPJF, Tomo II, 2014, p. XXXIX.

²⁷For 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/>

²⁸The 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.

at the time where blocking the reform given pressure from various PRI governors.²⁹ The political gridlock led former President Peña 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 be 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.

²⁹For more detail see Enrique Mendez, “PAN: estancados, cambios en materia política por presión de los gobernadores, La Jornada, 9 de enero de 2014, p. 5., <https://jornada.com.mx/2014/01/09/politica/005n3pol>.

Appendix B. Additional Tables and Figures

B.1. Main Results

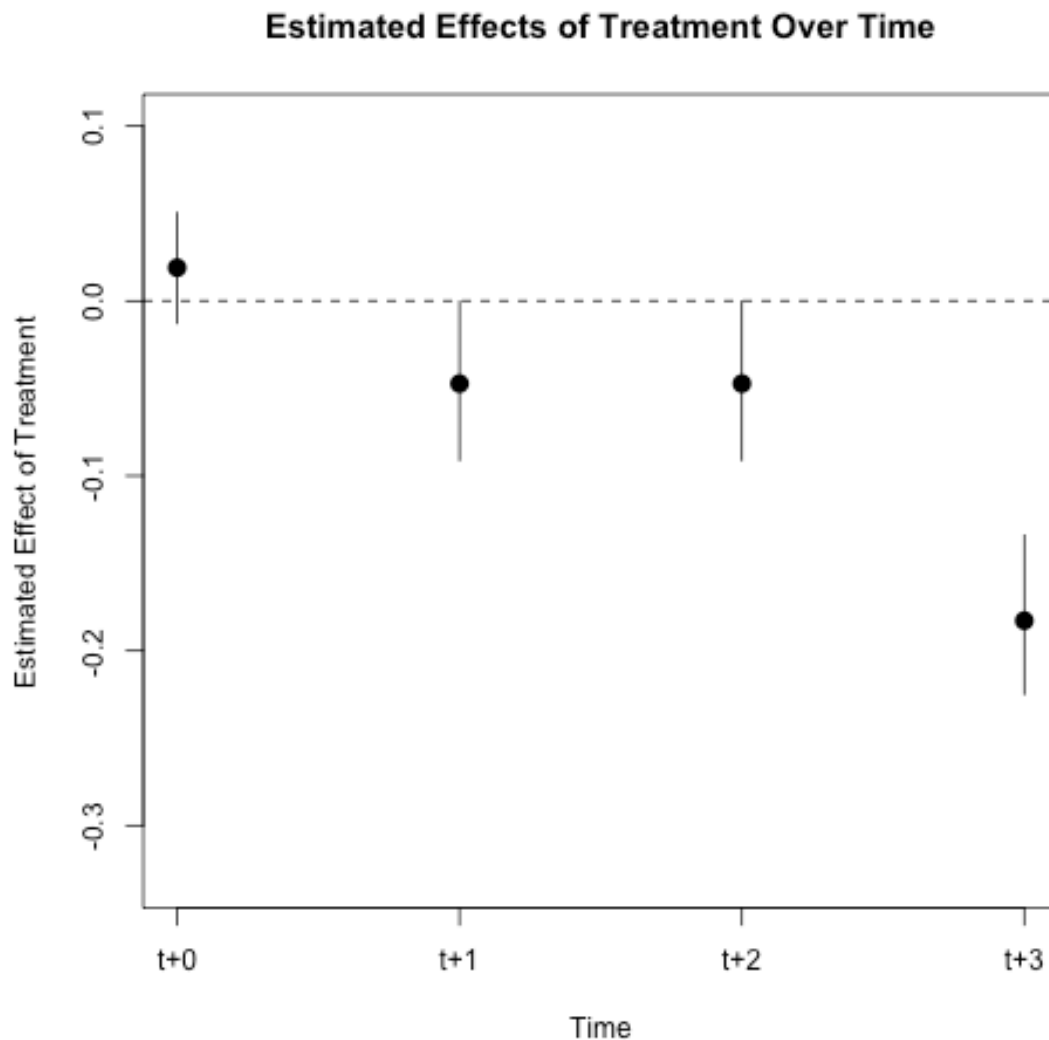
TABLE B-1. Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, 2010-2018

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

B.2. Robustness

FIGURE 13. Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, propensity score matching on pretreatment covariates



Note: Figure 13 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 2.

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

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 0) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

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

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). I trimmed the periods lag 8, 7, 6 and 5, and removed the period 1 to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE B-4. Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, [de Chaisemartin and D'Haultfoeuille \(2020\)](#) correction

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

Notes: Coefficients show corrected estimators following [de Chaisemartin and D'Haultfoeuille \(2020\)](#). Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%.^a Secondary measure of security cooperation agreements. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

FIGURE 14. Effect of Term Limit Reform on Security Cooperation Agreements signed with the Governor, 2010-2018

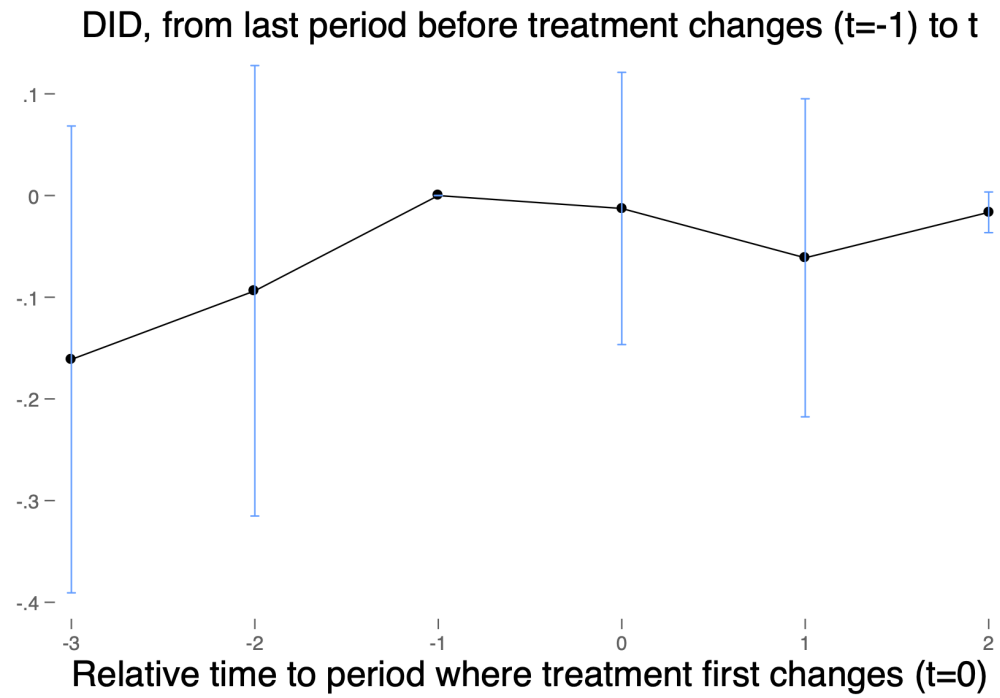


TABLE B-5. Comparison: Security Cooperation Agreements with Governor vs. Other Actors, 2014-2018

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 5 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 primarily to the President but could include Governors and mayors from other states or other municipalities from the same state. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE B-6. Test on selection on unobservables

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

Notes: I follow [Altonji et al. \(2005\)](#) to check if unobserved variation is likely to explain the signing of security cooperation agreements with the Governor by mayors. To do so, I regress the treatment (whether the municipality held reelection) on all the available covariates used for Figure 2. I then take the fitted value from the regression and use it to predict each outcome, this time including unit and year fixed effects. This test suggests that – under the assumption that observables are representative of unobservables – selection on unobservables is not driving the results.

B.3. Mechanisms

FIGURE 15. Effect of 2014 Term Limit Reform on Motives to Sign Security Agreements w/ Governor

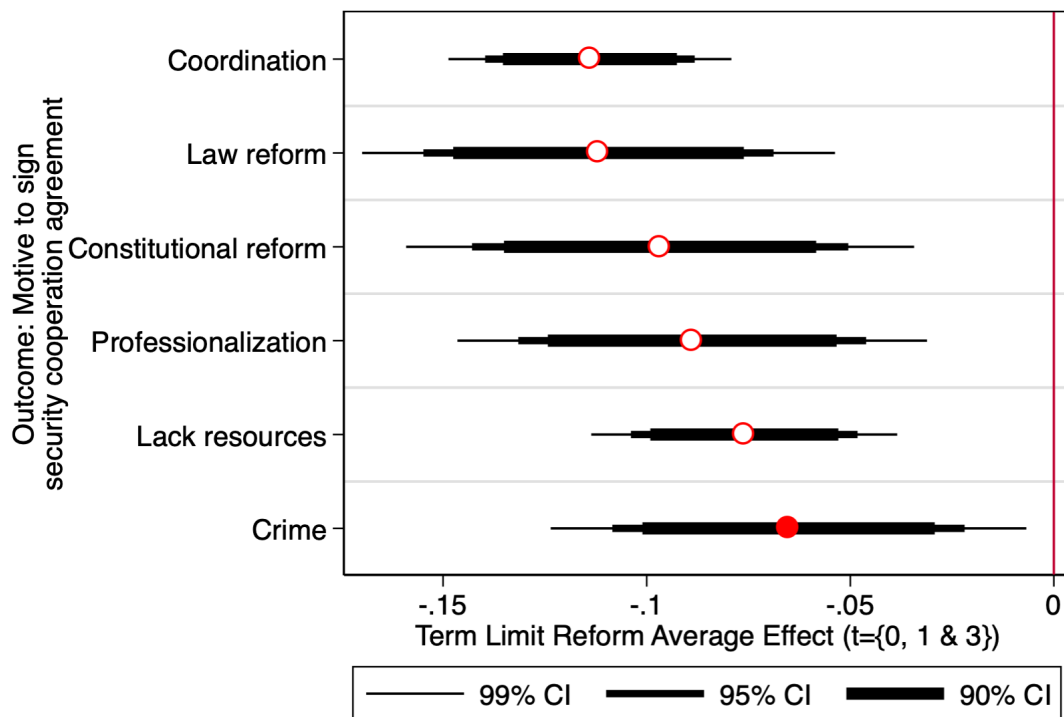


TABLE B-7. Effect of 2014 Term Limit Reform on Motives to Sign Security Agreements w/ Governor

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

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

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Relative time periods prior to t-2 do not exist and lag 1 is removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#) and serves as the reference period. Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. ^a Refers to security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE B-9. Party Alignment Total Interaction Effects

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 signing a security cooperation agreement with any of the following actors. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

TABLE B-10. Reform interaction with citizens' preferences

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

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

Dependent variable: Signing Security Cooperation Agreement w/ Governor	Municipal		State		Federal		
Jurisdiction:	Traffic	Preventive	State Police	State Attorney Police	Federal Police	Ministerial Police	Army
Identify Policy Force:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
							Marines (8)
t-7	-0.8572 (0.6544)	0.1007 (0.0978)	0.0649 (0.0611)	0.0783 (0.0697)	-2.5321*** (0.8962)	0.0632 (0.0550)	-1.4640* (0.8372)
t-6	-0.2641 (0.2039)	0.0248 (0.0609)	0.0135 (0.0467)	0.0056 (0.0441)	-0.7692*** (0.2696)	-0.0035 (0.0413)	-0.4466* (0.2577)
t-5	-0.4097 (0.3986)	-0.0652 (0.3080)	0.6451 (0.3960)	0.1762 (0.4004)	-1.1340** (0.4306)	-0.7691*** (0.2589)	-0.8805 (0.5274)
t-4	0.3350 (0.5455)	0.1050 (0.5451)	0.7461 (0.4774)	-0.0893 (0.6583)	-1.6040*** (0.5716)	-0.2211 (0.7553)	-0.7589 (0.8538)
t-3	0.8549 (0.5572)	0.3354 (0.6384)	0.8618* (0.5038)	-0.1098 (0.7313)	-1.2530** (0.6065)	0.2973 (0.8187)	-0.3261 (0.8829)
t-2	-0.0741 (0.3985)	0.0173 (0.3426)	0.3106 (0.3583)	-0.0035 (0.4741)	-1.1572** (0.4705)	-0.2290 (0.5458)	-0.7416 (0.5501)
Reform (t=0)	0.0965 (0.3746)	-0.3095 (0.5580)	-0.6740 (0.5072)	-0.0176 (0.5448)	-1.7122*** (0.5196)	-0.3017 (0.5185)	-0.8230 (0.4724)
t+1	0.1452 (0.4015)	-0.8415 (0.7920)	-0.5733 (0.6386)	-0.5894 (0.7035)	-1.1449** (0.4877)	-1.2316 (0.7296)	-0.8753 (0.5560)
t+2	0.4499 (0.3760)	-0.7212 (0.7799)	0.0862 (0.6272)	-1.4956** (0.7215)	-0.5687 (0.5955)	-1.6626** (0.6266)	-0.4091 (0.6311)
t+3	1.1277 (0.9218)	-0.5739 (1.2931)	-0.6702 (0.9352)	-1.2519 (1.0598)	-1.7933 (1.0758)	0.0623 (1.0916)	-0.5981 (0.9325)
Observations	12,173	12,173	12,173	12,173	12,173	12,173	12,173
R-squared	0.4688	0.4599	0.4659	0.4658	0.4624	0.4783	0.4645
Mun. FEs	✓	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓	✓
Controls ^b	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓
Reform average effect	0.3037	-0.4471	-0.2964	-0.3087	-0.7782**	-0.2768	-0.5017
SE (average effect)	(0.3233)	(0.6044)	(0.3716)	(0.2401)	(0.2868)	(0.2411)	(0.4665)
							-0.5781** (0.2669)

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Carrel presence.

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

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

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

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

Jurisdiction:	Municipal			State			Federal		
	Traffic	Preventive	State Police	State Attorney Police	Federal Police	Ministerial Police	Army	Marines	
Corruption of Police Forces:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
t-7	0.1477 (0.2864)	0.0419 (0.3087)	0.0402 (0.2813)	-0.0324 (0.0946)	-0.0147 (0.3434)	-0.0933 (0.0782)	-0.0543 (0.1059)	-0.1444 (0.1083)	
t-6	0.0301 (0.0796)	0.0011 (0.1139)	-0.0013 (0.1017)	-0.0258 (0.0568)	-0.0133 (0.1316)	-0.0488 (0.0524)	-0.0160 (0.0622)	-0.0432 (0.0617)	
t-5	-0.1338 (0.1599)	-0.0973 (0.1895)	-0.1177 (0.1822)	-0.9190*** (0.1316)	-0.2156 (0.2160)	-0.8364*** (0.1066)	0.4054** (0.1573)	0.4021** (0.1567)	
t-4	-1.3881*** (0.3821)	-0.8179 (0.5316)	-1.1187*** (0.3690)	-1.3964*** (0.3654)	-0.7440 (0.5666)	-1.2269*** (0.3341)	0.0944 (0.3531)	0.3231 (0.4236)	
t-3	-1.6818*** (0.3568)	-0.9104 (0.5728)	-1.2935*** (0.3376)	-1.1282*** (0.3266)	-0.7637 (0.6363)	-1.10065*** (0.2992)	0.0275 (0.3608)	0.2564 (0.4274)	
t-2	-0.2879 (0.2474)	-0.2198 (0.2868)	-0.2301 (0.2681)	-0.9068*** (0.1970)	-0.2657 (0.2943)	-0.7393*** (0.1573)	0.3265 (0.2314)	0.3743 (0.2279)	
Reform (t=0)	-2.2651*** (0.2832)	-1.5561*** (0.5290)	-1.9299*** (0.2479)	-1.0484*** (0.1614)	-1.3274** (0.5851)	-0.9674*** (0.1417)	-0.8107*** (0.2515)	-0.6875*** (0.2343)	
t+1	-3.1112*** (0.3902)	-2.2160*** (0.6501)	-2.6228*** (0.3255)	-2.6054*** (0.2394)	-1.9768*** (0.6995)	-2.2670*** (0.2017)	-0.5640** (0.2557)	-0.3577 (0.2419)	
t+2	-3.0152*** (0.3961)	-1.9965*** (0.6063)	-2.4536*** (0.2654)	-2.5539*** (0.2049)	-1.7638** (0.6524)	-2.2646*** (0.1648)	-0.2627 (0.2654)	-0.0623 (0.2224)	
t+3	-4.9633*** (0.5220)	-3.2615*** (1.0612)	-4.0463*** (0.3194)	-2.4673*** (0.2057)	-2.5721** (1.1755)	-2.2158*** (0.1413)	-1.2288** (0.4848)	-0.9278** (0.4028)	
Observations	12,173	12,173	12,173	12,173	12,173	12,173	12,173	12,173	
R-squared	0.4593	0.4572	0.4598	0.4623	0.4636	0.4599	0.4632	0.4586	
Mun. FEs	✓	✓	✓	✓	✓	✓	✓	✓	
Year. FEs	✓	✓	✓	✓	✓	✓	✓	✓	
Controls ^b	✓	✓	✓	✓	✓	✓	✓	✓	
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓	
Reform average effect	-4.0564*** (0.4611)	-2.8579*** (0.8900)	-3.5587*** (0.3522)	-2.5851*** (0.2217)	-2.2583** (0.9100)	-2.3551*** (0.1739)	-0.6132** (0.2536)	-0.4725* (0.2396)	
SE (average effect)									

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). 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 security cooperation agreements signed with the Governor. ^b Pretreatment controls include: governor winning margin; party alignment with the President; party alignment with the Governor; municipal winning margin; logged population; logged organized crime related deaths; and Cartel presence.

B.4. Unintended consequences

A. Preferences

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

Dependent variable: topic that worries the most											
	Narcotrafic (1)	Insecurity (2)	Punishment to criminals (3)	Corruption (4)	Poverty (5)	Unemployment (6)	Inflation (7)	Natural Disasters (8)	Water Scarcity (9)	Education (10)	Health (11)
t-6	0.0190*** (0.0066)	0.0218** (0.0085)	-0.0088 (0.0121)	0.0140** (0.0060)	-0.0363*** (0.0118)	0.0120 (0.0156)	-0.0150 (0.0103)	-0.0121*** (0.0021)	0.0119* (0.0066)	0.0106*** (0.0013)	-0.0087*** (0.0022)
t-5	0.0073*** (0.0012)	0.0167*** (0.0062)	0.0062 (0.0063)	0.0015 (0.0018)	-0.0186*** (0.0062)	0.0143* (0.0074)	-0.0066 (0.0065)	-0.0059** (0.0025)	-0.0038*** (0.0012)	0.0011 (0.0009)	-0.0090** (0.0034)
t-4	-0.0034 (0.0083)	0.0921** (0.0380)	0.0218 (0.0247)	-0.0048 (0.0142)	-0.0447 (0.0274)	0.0106 (0.0233)	-0.0432* (0.0224)	-0.0055 (0.0043)	0.0303** (0.0119)	0.0089 (0.0052)	-0.0492** (0.0189)
t-3	0.0439** (0.0182)	0.0727 (0.0566)	-0.0033 (0.0210)	-0.0143 (0.0202)	-0.0275 (0.0438)	0.0254 (0.0332)	-0.0204 (0.0152)	0.0015 (0.0116)	-0.0156 (0.0238)	0.0071 (0.0172)	-0.0536** (0.0227)
t-2	0.0280 (0.0219)	0.0144 (0.0496)	0.0304 (0.0195)	-0.0195 (0.0216)	-0.0253 (0.0420)	0.0266 (0.0211)	0.0435*** (0.0143)	0.0121 (0.0159)	-0.0003 (0.0171)	-0.0306* (0.0157)	-0.0623* (0.0314)
Reform, t=0	0.0021 (0.0050)	0.0267*** (0.0072)	0.0206*** (0.0037)	0.0012 (0.0044)	-0.0187*** (0.0063)	-0.0355*** (0.0051)	0.0034 (0.0056)	-0.0016 (0.0026)	0.0073 (0.0052)	-0.0091** (0.0039)	0.0017 (0.0053)
t+1	0.0165** (0.0071)	0.0427*** (0.0112)	0.0270*** (0.0037)	0.0126*** (0.0045)	-0.0392*** (0.0097)	-0.0803*** (0.0058)	0.0520*** (0.0075)	0.0093 (0.0074)	0.0017 (0.0053)	-0.0189*** (0.0046)	-0.0329*** (0.0071)
t+2	0.0227** (0.0086)	0.0785*** (0.0108)	0.0400*** (0.0050)	0.0079* (0.0042)	-0.0405*** (0.0108)	-0.1023*** (0.0087)	0.0172* (0.0093)	0.0099* (0.0050)	0.0107* (0.0062)	-0.0283*** (0.0062)	-0.0323*** (0.0058)
t+3	0.0182 (0.0134)	0.0837*** (0.0151)	0.0828*** (0.0098)	-0.0081 (0.0080)	-0.0397** (0.0169)	-0.1094*** (0.0177)	-0.0357*** (0.0064)	0.0048* (0.0025)	0.0228** (0.0087)	-0.0275*** (0.0092)	-0.0152 (0.0109)
Observations	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353	11,353
R-squared	0.8662	0.8556	0.9239	0.8767	0.8549	0.8954	0.8557	0.7008	0.8419	0.8048	0.8799
Mun. FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year. FEs	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls ^d	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort weighted	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Reform average effect	0.0149*	0.0579***	0.0426***	0.0034	-0.0345***	-0.0819***	0.0092	0.0056	0.0106**	-0.0209***	0.0063
SE (average effect)	(0.0079)	(0.0099)	(0.0046)	(0.0048)	(0.0103)	(0.0085)	(0.0063)	(0.0034)	(0.0047)	(0.0055)	(0.0063)

Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 1) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#) except for the specification that trims periods prior to t-4. Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 1%; ** 5%; and * 10%, that refer to two-sided t-test with the null hypothesis equal to 0 for each relative time period. ^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.

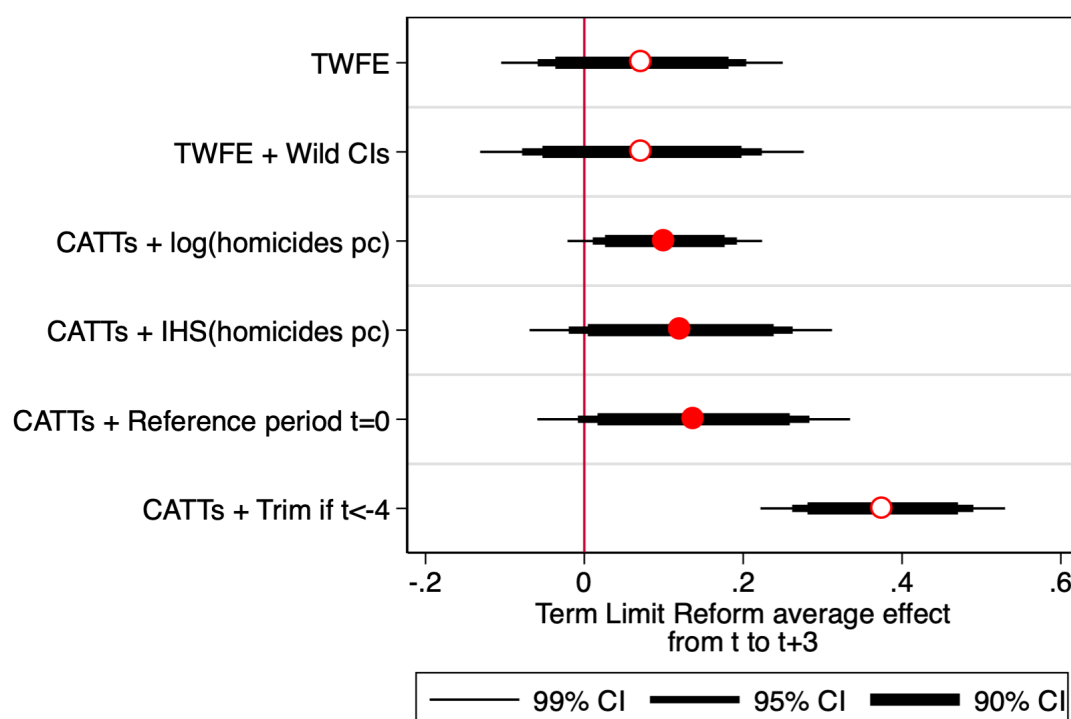
B. Violence

TABLE B-15. 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

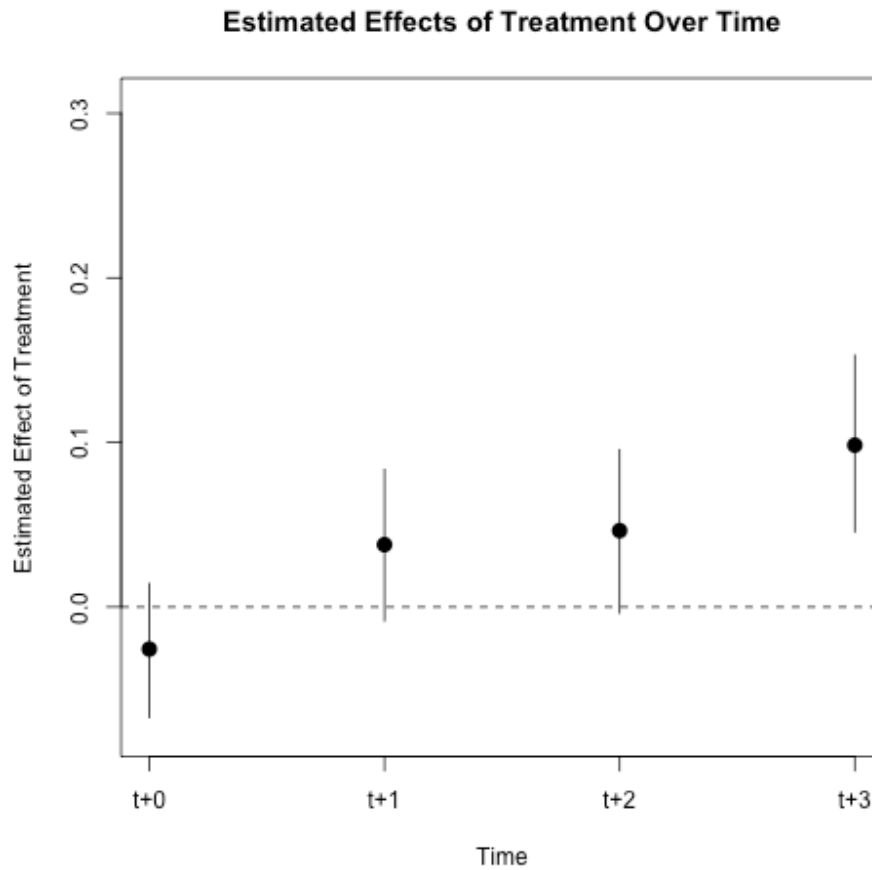
Notes: Coefficients show IW estimators following [Sun and Abraham \(2020\)](#). Two relative time periods (lag 8 and 0) are removed to avoid collinearity problems noted by [Sun and Abraham \(2020\)](#). Standard errors in parentheses are clustered at the state level, with the following significance-level: *** 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 16. Robustness tests: Effect of Term Limit Reform on Violence, 2010-2018



Note: Figure 16 shows the average treatment effect from t to t+3 across multiple specifications. This average effect was estimated using the IW estimators following [Sun and Abraham \(2020\)](#) 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 17. Effect of Term Limit Reform on Violence, propensity score matching on pretreatment covariates



Note: Figure 17 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 2.

Appendix C. 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.

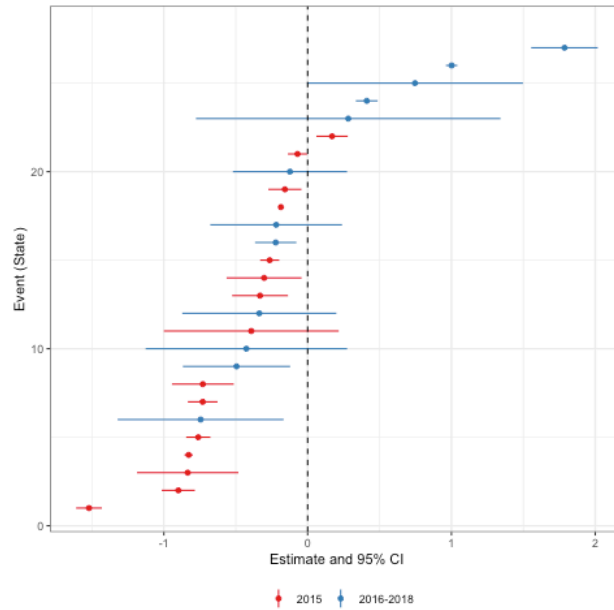
Another way to assess the no-anticipatory behavior from incumbents in this setting is test whether early vs late adopters differed in their estimated effects. Appendix Figure 18 presents Cengiz et al. (2019) “event-by-event analysis” that estimates treatment effects for each treated Mexican state (28 states) in the sample. States color differs if they are early (2015, red color) or late adopters (2016-2018, 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:

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

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 18, 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. One would be concerned of the five blue states clustered in the positive end. However, if there was anticipation in these states they would only represent a downward bias of the main results found on the paper.

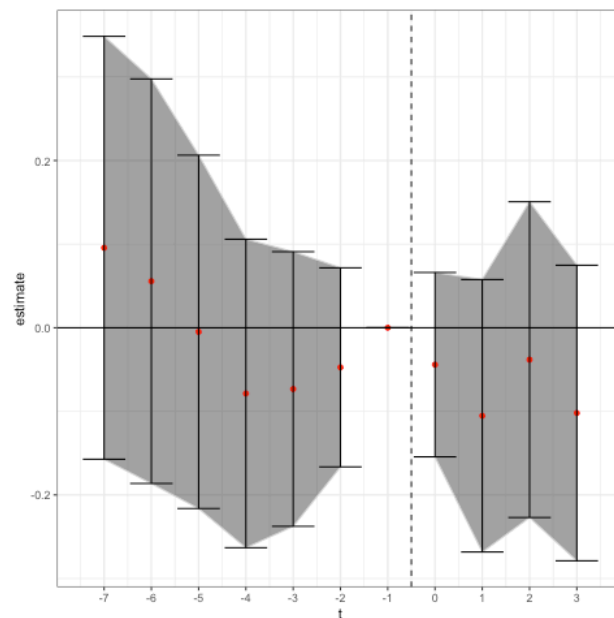
For robustness, Appendix Figure 19 presents the “stacked dataset analysis” from Cengiz et al. (2019). I take each of the “event-by-event” datasets from the Appendix Figure 18, stack estimates by cohort and estimate one set of lead and lag variables not using prior treated units as controls. Appendix Figure 19 shows that conditional on state-level covariates, there is strong evidence of parallel trends as well as negative effect of reelection on delegation, but noisy.

FIGURE 18. “Event-by-event analysis” following [Cengiz et al. \(2019\)](#)
-95% confidence intervals-



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

FIGURE 19. “Stacked dataset analysis” following [Cengiz et al. \(2019\)](#)
-95% confidence intervals-



Note: Utilize estimated coefficients from Figure 18 and stack them in relative time, and estimate lead and lag variables to treatment following the event-by-event analysis setup, i.e. without treatment containment from using prior treated units of controls. Analysis done stacking at the cohort level, and adding municipality and year fixed effects, and clustered standard errors at the state level.