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Reforming Justice under a Security Crisis: The Case of the Criminal Justice Reform in Mexico.

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Abstract: This paper assesses how the adoption of a common-law style model affects crime rates, pre-trial detention, and judicial efficiency measures. We do this in the context of Mexico, where a judicial reform was fully implemented by 2016, both on the state and federal levels. Using a generalized synthetic control group approach (Xu 2017) and municipality-level administrative data for the years 1997-2012, we find that the reform increased the homicide rate and was accompanied by a reduction in the use of pretrial detention for property crimes and rape, and a more rapid process for some types of crimes. The increase in the homicide rate was, nonetheless, specific to municipalities with established organized crime presence, where we observed a reduction in the capacity to effectively prosecute homicides linked to the reform. Our results describe the difficulties in implementing this kind of reform in developing countries experiencing security crises, and they contribute to the literature linking procedural justice and criminal behavior.

Keywords: Crime; Criminal Justice Reform; Generalized Synthetic Control Group; Latin America

JEL codes: K14, K40, K41, K42

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1 Introduction

In the 1990s, Latin American countries began a process of reform of their criminal justice systems, replacing the old inquisitorial system with an adversarial one. It was expected that this change would solve many of the problems prevalent in the administration of justice in the region, which included human rights abuses of the accused and a slow, cumbersome judicial process. Mexico began its reform in the late 2000s in a group of early-adopter states that included Chihuahua, Oaxaca, and Nuevo León. A federal reform followed in 2008 that drafted a new federal procedural code and mandated the adoption of the new system in all states. By 2016, the reform was in force throughout the country, both at the state and federal levels.

There have been high hopes for change, but limited causal evidence of the impact of the reform in Latin America. Due to the present security crisis affecting the region, there have been increasing calls to reverse some aspects of the reform, especially those strengthening human rights protections of the accused (D. Shirk, 2011). For example, Chile reversed many of the limitations to the use of pre-trial detention and the 2008 Mexican federal reform included many special provisions for the prosecution of organized crime[1], limiting due-process protections. Thus, it is now more important than ever to examine evidence regarding the changes and determine which parts of the reform are working and which are not.

Although the reform affected only procedural justice and not substantive law[2], many of its changes could be expected to impact the incentive structure for criminality. It was unclear a priori what the net effect would be, as some changes would imply an increase and others a decrease in crime rates (Chalfin & McCrary, 2017; D. Nagin, Cullen, & Jonson, 2009). For example, the increased due-process protections of the reform could bring about either an increase in crime rates, the result of a reduced certainty of punishment and imprisonment because of the greater difficulty in obtaining pretrial detention (Chalfin & McCrary, 2017), or a reduction in crime rates because of the greater legitimacy of and cooperation with institutions of criminal justice (Sunshine & Tyler, 2003; Huebert, 2019). To understand the effects of the reform, it is important to measure its net effect and the causal mechanisms involved. This is particularly important so that changes can be formulated to address the

[1]This special procedure is called arraigo, and is mainly used by federal prosecutors. Because of their complexity, the information regarding those cases is often not publicly available (D. Shirk, 2011). In this paper we limit ourselves to crimes under state jurisdiction, so we do not consider any cases prosecuted under arraigo.

[2]In this context, substantive law refers roughly to the definition of crimes and their punishments in the criminal code. Procedural law refers to the laws governing criminal proceedings.

shortcomings of the reform.

We make use of the quasi-experimental nature of the staggered roll-out process of the reform and a variety of municipal-level administrative data to identify the effect of the reform on the homicide rate, the use of pretrial detention, and the celerity of punishment in municipalities that adopted the reform before 2012. We use a generalized synthetic control group technique (Xu, 2017), a generalization of the synthetic control of Abadie and the two-way difference-in-differences estimator, which is based on the interactive fixed effects estimator of Bai (2009) to allow for the existence of different pretreatment trends. We expect this technique to avoid some shortcomings of previous studies and allow us to better control for the effect of the drug war, which occurred simultaneously with the onset of the reform[3].

Our results show a positive and statistically significant increase in the homicide rate, attributable to the reform, of 4 victims per 100,000 inhabitants at the municipal level. However, this effect seems to be specific to municipalities that had an established presence of organized crime before the reform. To test this hypothesis we re-estimate the effect in two sub-samples: (1) municipalities that had no cartel presence in 2006, and (2) municipalities that had one or more cartels. The effect becomes non-significant for the subset of municipalities that had no cartels in 2006. However, it is both highly significant and quantitatively large for the sub-sample with one or more cartels in 2006.

One potential explanation for the specificity of this effect is a reduction in effectiveness of the criminal justice system in the states that were implementing the reform and had to face the security crisis created by organized crime and the drug war. The effect of the war may hit especially hard in those places where the agents of the system had to adapt to a new way of doing things. Thus, we would expect a reduction in the capacity of the system to effectively prosecute crimes in municipalities under war and that have adopted the reform. We use the punishment rate (Beckett & Beach, 2021), calculated as the ratio of indictments[4] for every murder, as a measure of effectiveness and find that our results are consistent with this explanation. There is a non-significant effect on the punishment rate in the municipalities that had no cartels in 2006 (0.06) and a negative and significant effect (-0.3) for those that

[3]We could date the beginning of the drug war to Calderon's crackdown on cartels in 2007. This is the same year the reform entered into force in the first judicial districts. We discuss our strategy to disentangle those two effects in section 7.

[4]Indictments are formal accusations of a crime decided upon and issued by a judge. It signals the beginning of a criminal case. An indictment occurs after the prosecutor presents evidence indicating the occurrence of the crime and involvement of the accused. The rate is usually estimated using the prison population.

had at least one.

The effect on the use of pretrial detention also seems to be specific. Although we observe a decrease of 8 percent in the use of pretrial detention for all crimes, our analysis for different categories of crime shows that the effect is greatest for property crimes (with a mean reduction of 16 percent), less important but still significant for rape (a mean reduction of 7 percent), and not significant for homicides and kidnapping. The reduction thus seems to be focused on less serious crimes (like property crimes) or those that are especially hard to prove (like rape).

Finally, we observe a reduction in the time between the commission of the crime and the sentence. There is a significant reduction for property crimes (58 days). The estimated effect for all crime, homicide, and kidnapping is negative but not statistically significant.

Our paper relates to various strands of the literature. First and foremost, our work contributes to the growing literature evaluating the impact of judicial reforms in Mexico and Latin America (Ingram, Ferreira, & Shirk, 2011; Azócar & Undurraga, 2005; Tiede, 2012; Mohor-Bellalta, 2007; Zorro Medina, Acosta Mejia, & Mejia, 2020; Zorro Medina, 2020; Huebert, 2019; Hernández, 2019; Blanco, 2016). Our work relates most closely to three papers. Blanco (2016) evaluates the impact of judicial reform in the three Mexican cities that implemented it in 2007 and 2008. Using victimization surveys, city-level information, and a difference in difference design, she finds that judicial reform reduces victimization and lowers perceptions of security in these cities. Moreover, she finds that the reform decreases trust in the local and federal police. Unfortunately, due to data restrictions, she is not able to test whether control cities have similar trends to those treated. We complement her findings by analyzing the reform using a methodology that controls for potential confounding effects such as the drug on wars. Besides, we study its effects on a much larger scale and using additional outcomes such as the efficiency of the criminal justice system. Our analysis allows us to show that when using a larger sample with rural and urban areas the reform increases victimization when proxied by the homicide rate. Zorro Medina et al. (2020) evaluate the Colombian quasi-experimental implementation of the U.S. adversarial model. Using an event study and difference-in-difference models, they find an increase associated with the procedural transformation in overall crime rates (22 percent), violent crime (15 percent), and property crime (8 percent). However, they do not find a consistent effect of the reform on the homicide rate as we do. Zorro Medina (2020) relies on aggregate data on prison entries and releases to measure the impact of the Colombian reform on the use of pretrial detention. Due to this, she cannot distinguish between pretrial detentions and other

entries to prison for most of the rolling-out process. By using case-level administrative data, we add to her findings by measuring precisely the effect of the reform on the use of pretrial detention during the first procedural steps.

Our results concerning the homicide rate suggest that there are complex interactions between procedural justice and the capacity of the state to effectively control organized crime, and that these specific effects do not necessarily generalize to other contexts. This is all the more important considering that the region is experiencing an unprecedented security crisis that is caused, in part, by the surge in organized crime. In this sense, our study complements the results of Huebert (2019), and suggests that the failings of the reforms may be specific to this context. The reduction in the overuse of pretrial detention for less serious crimes could be understood as a net gain, because of the human rights implications and damaging effects, both at a social and individual level (D. S. Nagin, 2013a), of imprisoning people without a sentence. It is therefore important that future reforms to the procedural codes in the region consider this specificity, so as not to reverse the gains of the new system.

Second, our work is related to the literature that investigates the causes of the upsurge in violence in Mexico since 2006. Dell (2015) uses regression discontinuity estimates to show that drug-related violence increases substantially after close elections of mayors from the Partido Acción Nacional (PAN), with an estimated effect of 27-33 more deaths per 100,000 inhabitants. Her results suggest that violence reflects rival traffickers' attempts to usurp territories after crackdowns have weakened incumbent criminals. Her empirical evidence supports descriptive studies arguing that the Mexican government's anti-drug policies have been the primary cause of the sharp increase in violence seen in recent years (Escalante-Gonzalbo (2011)). Another explanation for the upsurge in violence and drug trafficking activity in Mexico relates to policy changes in Colombia. Camilo Castillo, Mejía, and Restrepo (2020) argue that increased cocaine seizures in Colombia have increased the Mexican homicide rate by 37 percent in municipalities close to the U.S. border and by 17 percent in those 1000 km from the border. We contribute to this debate by showing that the criminal justice reform in Mexico is another possible cause of the spike in violence during this period.

This paper is organized as follows. In the next section, we discuss the origins of the reform in Latin America and the problems it was designed to solve. In Section 3, we discuss the evaluations of the impact of the reform that have been conducted in the region. Section 4 explains the origins of the reform process in Mexico and the difficulties in its adoption. In Section 5, we review previous studies evaluating the Mexican reform. Section 6 discusses the proposed causal mechanism linking the reform in procedural law and its impact on crime

rates. In Section 7, we describe our data sources. Section 8 explains the methodological challenges of measuring the impact of the reform in Mexico and provides a justification for our use of Xu's generalized synthetic control. In Section 9, we present the key results of our paper, and Section 10 offers some concluding remarks.

2 Criminal Law Reform in Latin America

In the 1990s, Latin American countries began a process of reforming their criminal justice systems. These reforms aimed to correct various shortcomings of the old inquisitorial system, until then the dominant system in the region, by adopting an adversarial model. The reasons for this change are numerous (Langer, 2007). One of the most important was the perception that the old systems did not guarantee due process for the accused and that they were plagued by corruption and lack of transparency. The reform was thus understood as part of the process of democratization and increased human rights protection in the region. Another important reason behind the change was the inefficiency of the old system. Trials were notoriously slow, often dragging on for years before sentencing.

According to Langer (2007), the origins of the reform can be traced back to a network of reformers in various Latin American countries in the late 1980s, who spearheaded the reforms in an alliance with various international organizations, most notably USAID, as a way to modernize the region's legal codes and tackle various human rights issues that had affected the region. The first countries to adopt the system were Guatemala and Costa Rica in the 1990s, and by 2020 most countries in the region had introduced some kind of reform inspired by those proposed by the network, except for Brazil and Cuba (Fandiño & González Postigo, 2020)[5].

While there is a great deal of variation from country to country in the implementation of the accusatorial system, there are some features that most of these reforms share. Langer (2007) summarizes them as: (1) public, oral trial proceedings, (2) introduction and/or strengthening of the office of prosecutor, (3) a clear distinction between the prosecutorial and adjudicatory functions, (4) prosecutors in charge of pretrial investigation, (5) greater due process protections for the accused in the investigation and pretrial phase, including limits on the use of pretrial detention, (6) introduction of prosecutorial discretion, and (7)

[5]Argentina, which played a central role in the early days of the network, passed the reform on the federal level in 2014, but it has yet to enter fully into force.

introduction of plea bargaining and alternative dispute resolution for minor crimes. The separation of the prosecutorial and adjudicatory functions was intended to solve the inherent conflict of interest between the two. In the words of Julio B.J. Maier, one of the early proponents of the reform, the inquisitorial system "*asked pretrial investigation judges to perform the psychologically impossible task of zealously investigating a case while remaining dispassionate and impartial*"(Langer, 2007, p. 639).

In recent years, however, these reforms have been criticized for various reasons, and measures have increasingly been proposed to address perceived shortcomings (Fandiño & González Postigo, 2020). One major claim is that the greater due process protections have limited the ability of authorities to respond to the security crisis that has affected the region since the early 2000s. In order to strengthen the powers of criminal justice authorities, various countries have relaxed the restrictions limiting the use of pretrial detention. For example, in Mexico, the implementation of the adversarial model at the federal level came with its own "counter-reform." Article 19 of the constitution was changed to allow for discretionary pretrial detention for a wide range of crimes. Some countries have also backtracked in the generalized use of public, oral proceedings and the use of juries (Fandiño & González Postigo, 2020, p. 11-12).

Despite the importance of the reforms and the existence of these criticisms, there has been very little quantitative research evaluating the effectiveness of the new adversarial system in its stated goals of protecting the human rights of the accused and improving the efficiency of criminal justice. This omission is all the more striking given the current security crisis in the region, which has spotlighted the performance of police and criminal justice systems. Public opinion surveys in Latin America consistently show that people rank public safety as one of their major concerns, and it is an important issue on the political agendas of the region.

3 Impact Evaluation of the Judicial Reform in Latin America

There have been only four studies evaluating the impact of the reform in Latin America: one in Peru (Hernández, 2019), two in Mexico (Blanco, 2016; Huebert, 2019) and one in Colombia (Zorro Medina et al., 2020; Zorro Medina, 2020). Hernández (2019), (Zorro Medina et al., 2020), and (Zorro Medina, 2020) use the quasi-experimental nature of the adoption process of the reform as an identification strategy to analyze its effect on various measures. Both

in Colombia and Peru, the new adversarial model was gradually phased in, which allows for the comparison of jurisdictions under the old and new systems.

Hernández (2019) measured the impact on crime rates and risk perception in Peru using a difference-in-differences analysis with matching estimator, and finds a temporary reduction in the aggregate crime rate in the group of early adopters (before 2010). He attributes this effect mainly to the more expeditious procedures under the new system, but also finds that it faded away three years after the implementation. The effect in the group of late adopters was the opposite: an increase in the aggregate crime rate.

Zorro Medina et al. (2020) and Zorro Medina (2020) applied a similar analysis to Colombia. In addition to measuring the impact of the reform on the crime rate, Zorro Medina et al. (2020) evaluated its impact on procedural speed and Zorro Medina (2020) examined the use of pretrial detention. Using a difference-in-differences estimation, they found an increase in the overall crime rate 12 months after the implementation of the reform. As Hernández (2019) also found in Peru, the impact varied with the nature of the crime, with lower or null effects for sexual offenses and homicides.

Zorro Medina (2020) also used a difference-in-difference estimator to evaluate the use of pretrial detention, measuring the change in the unsentenced prison population, prison overcrowding, and detention flow rates. She found a decrease of 19-21 percent in the unsentenced prison population, though within three years it had returned to its previous level. While one of the objectives of the reform was to curb the abuse of pretrial detention and protect the rights of the accused, Zorro Medina (2020) argues that the greatest effects were speedier trials and improved efficiency in the system. She observed no change in the overcrowding of prison facilities and a positive correlation of the reform with greater detention and release rates, which leads her to conclude that the decrease in the prison population was more an effect of the efficiency of the new system than of a decrease in pretrial detention.

4 The Judicial Reform in Mexico

The adoption of the adversarial model in Mexico was gradual and uneven, mainly due to the country's federalized political organization and a late endorsement by the federal government. The process took place in two broad phases (Ferreira & Shirk, 2015). The first (2004-2008) was prompted by a failed 2004 federal reform proposal by President Vicente Fox (D. A. Shirk, 2011) that nonetheless led various states to reform their own procedural codes. The second

(beginning in 2008) was promoted by a federal government initiative that aimed to create a uniform criminal procedure for the whole country.

The first states to adopt the reform were Nuevo León (2004)[6], Oaxaca (2007), and Chihuahua (2007). These states, which Ferreira and Shirk (2015) call "early adopters," introduced reforms that, while differing on various points, shared all the central features of adversarial systems and were inspired by Fox's 2004 proposal (D. A. Shirk, 2011). Following their example, Estado de México (2006), Baja California (2007), Morelos (2007), Zacatecas (2007), and Durango (2009) also passed reforms to their state codes, but did not implement them before the national-level reform of 2008.

Even though those early state-level reforms were not formally evaluated by the judicial authorities, the prevailing perception was that they had increased judicial efficiency and transparency (D. A. Shirk, 2011), which helped build consensus for a reform on the federal level. Finally, in 2008, President Felipe Calderón was able to pass it. The purpose of the 2008 reform was three-fold. First, it sought to reform the federal criminal justice system and to mandate the adoption of the adversarial system in all states. Second, it standardized criminal procedure relating to "investigations, arrests, charges, hearings, sentencing, alternative dispute resolutions, and reparations," seeking to ensure "the rights of all interested parties through the judicial process" (Ferreira & Shirk, 2015, p.17). Third, it created a road map for the adoption of the system, setting 2016 as a deadline for full implementation at both the federal and state levels (D. A. Shirk, 2011). With the approval of the 2008 law, the states suspended their reforms initiatives, as they waited for the federal government to draft a model code of criminal procedure.

There was little progress in the implementation, however, until 2013, when the government finally defined an implementation plan. The reform would first enter into force in judicial districts with less complex security threats, and then extend to more complex districts (Ferreira & Shirk, 2015). Although there were various challenges during the implementation process (Rivas-Tovar, 2012), the reform was fully implemented on schedule, arriving in the last districts by June 2016.

[6]Nuevo Leon did not began to implement the reform until much later, in 2012. Its implementation process was also markedly different from that of the rest of the early adopters. The reform was gradually introduced in the state not by jurisdiction, but by type of crime.

4.1 Main Changes Introduced by the Reform

In this section we summarize the main features of the reform in Mexico. We use as a benchmark the changes introduced by the 2008 law at the federal level, mainly because all the major changes of the early adopters are shared with the 2008 law[7]. Following D. A. Shirk (2011, p. 203) these are (1) the introduction of oral trial proceedings, (2) greater due process protection for the accused, (3) introduction of alternative sentencing and conflict resolution mechanisms, and (4) clearer distinctions between investigative, adjudicative, and due process functions.

The most salient feature is the transition to public, oral trial proceedings. While the old system did consider some oral testimony, argumentation, and presentation of evidence, the judicial process was mostly based on written affidavits, which made trials lengthy and cumbersome (D. A. Shirk, 2011). Under the new system, every stage of the process is held in public, oral proceedings.

One of the main criticisms of the old system was that it granted few due process protections to the accused. Prosecutors, who led the preliminary inquiry, benefited from a great deal of autonomy (D. A. Shirk, 2011, p. 205). There was little oversight by the defense during this preliminary phase, which lent itself to abuses such as mishandling of evidence and forced confessions. The use of pretrial detention was also widespread, even for minor offenses, and the detainees were often mixed with the general prison population (D. A. Shirk, 2011, p. 211). The reform addressed these problems by prohibiting the use of confessions as evidence unless obtained in the presence of the defense attorney, and limiting the use of pretrial detention to violent and other serious offenses.

To expedite the judicial process, the new system introduced two new mechanisms in which a full trial could be avoided. The first was plea bargaining (juicios abreviados), where the accused accepts some form of reduced sentencing in exchange for pleading guilty to the charges brought by the prosecution. The second was mechanisms for alternative dispute resolution (ARD), which allow for the resolution of minor offenses out of court by mutual agreement of the parties (D. A. Shirk, 2011). Both mechanisms can expedite the judicial process by reducing the number of cases.

In traditional inquisitorial systems, the investigation is usually overseen by an examining magistrate (juez de instrucción), but in Mexico the figure of the public prosecutor already

[7]To see the functioning of the process under the two systems see figures A.1 and A.2 in Appendix A

existed before the reform[8]. This difference from classical inquisitorial systems, where there is often no clear distinction between investigative and adjudicative functions, has led some authors (Ferreira & Shirk, 2015) to stress the unique characteristics of the Mexican traditional system. Under this system, the prosecutor was in charge of the preliminary inquiry (averiguaciones previas) and presentation of charges. There was, however, no clear separation of functions because judges often worked closely with prosecutors during the preliminary inquiry and evidentiary phase (instrucción), and had the power to seek out evidence on their own accord (D. A. Shirk, 2011, p. 206). All of these factors made conviction highly likely once a suspect was identified, because of a lack of adequate defense and the ready acceptance of the prosecutor's pretrial evidence (D. A. Shirk, 2011, p. 195-196).

The reform sought to address this inequity by introducing a form of checks and balances to clearly separate the different functions and avoid the conflicts of interest of the old system. The pretrial phase of the process was to be overseen by a supervisory judge (juez de garantía) in charge of ensuring due process for the accused and avoiding the abuses of the old system. The trial judge (juez de sentencia or juez de juicio oral) presides over the trial and does not take part in the investigative process. Finally, the sentencing judge (juez de ejecucción de sentencia) oversees the sentencing and restorative justice (D. A. Shirk, 2011).

The reform also placed certain limitations on the powers of the public prosecutor. The preliminary inquiry, the stage of the traditional system where the prosecutor had the most advantage, was reduced in importance (D. A. Shirk, 2011, p.209). However, the evidentiary requirements for arrest or initiating charges were also reduced, and the prosecutor was given discretion as to which cases to prosecute. This prosecutorial discretion, called the "principle of opportunity" (principio de oportunidad) could help to ease caseloads and focus resources on the most serious cases. In the traditional system prosecutors were required to prosecute all cases for which there was sufficient evidence.

4.2 Evaluation of the Reform

There are two quantitative studies measuring the impact of the reform in Mexico. Huebert (2019) examined its impact on the homicide rate in the context of the drug war, and Blanco (2016) focused on victimization, perception of security, bribery, and trust in and assessment of criminal justice institutions. Both studies, like the other impact evaluations done in the

[8]The figure of the public prosecutor has a relatively long history in Mexico: it was introduced in 1908 (D. A. Shirk, 2011, p. 204).

region, make use of the progressive rollout process to identify the treatment effect.

Because the implementation of the reform coincided with the beginning of the drug war,[9] there was an exogenous shock to the murder rate that is very difficult to distinguish from the specific effect of the reform. To disentangle these effects, Huebert (2019) used the uneven geographical impact of the drug war to analyze the differential impact of the reform in two groups of states: those that were relatively spared from the surge in drug-related violence and those that were the battleground. She hypothesized that in states that were relatively unaffected by the war, the greater due process protections granted by the reform should increase trust in criminal justice institutions and thus public cooperation of the population with these institutions, making them more effective in combating crime in general and homicides in particular. In those states that were heavily affected by the drug war, the population might fear collusion between state actors and organized crime or might fear retaliation for cooperating with the authorities, counteracting the positive effects of the reform. She thus expected a decrease in the murder rate in states less affected by the drug war and no effect in those most affected. She found a significant negative effect of the reform, persistent over time, on the homicide rate in the states that were not heavily affected by the war, and a non-significant positive short-term effect in those affected by the war, which became positive and significant in the long run. However, this positive effect was non-significant both in the short and long term if Chihuahua, the first state to completely adopt the reform and one of the hardest hit by the war, was omitted from the analysis.

Blanco (2016) proposed a similar causal mechanism to explain the effects of the reform on her variables of interest: perception of security, bribery, trust in and assessment of institutions, and crime victimization. The greater due process protections and efficiency of the new system should improve public trust. She thus expected the reform to have a positive effect on the perception of security and trust in institutions and a negative effect on bribery and victimization. To measure the treatment effect of the reform on these variables, she used the gradual rollout process to apply a difference-in-differences identification strategy. She found a significant negative effect on crime victimization, perception of security, trust in the police, and bribery of local and transit police, which disappeared when considering only Ciudad Juárez. The effect on bribery of state judicial and federal preventive police was significant and positive. With regard to the assessment of criminal justice institutions, she found a negative effect on the grading of the preventive and local police (but only in some

[9]The first state-level reform came into force in 2007, in Chihuahua. The drug war began in 2006-2007, at the beginning of the Calderón administration, and Chihuahua was one of the states most affected by the surge in violence.

subsamples), and a positive effect on the grading of the public prosecution office (but only on some subsamples).

There are, however, some shortcomings in the study of Blanco (2016). First, she used data from the Mexican victimization survey (Encuesta Nacional Sobre Inseguridad, ENSI), which is only representative for metropolitan areas. Metropolitan areas differ significantly from the rest of the country in relevant characteristics such as criminality and resources, which makes it difficult to generalize her conclusions to the rest of the country. Second, because of data limitations, she did not test the parallel trends assumption, which is critical to the consistency of the difference-in-differences estimator.

5 Causal Mechanisms

In this section we analyze more closely the proposed causal mechanisms for the effects of the reform. As the effects on the use of pretrial detention and procedural speed are straightforward, we focus on the mechanisms determining the homicide rate.

An important way that procedural law can affect the incidence of crime is through the relationship between the perceived legitimacy of the criminal justice system and the collaboration of the population. Sunshine and Tyler (2003) consider two types of police legitimacy: (1) instrumental: the ability to create credible threats of sanctions for those who break the law (and thus effectively control crime), and the fair distribution of police resources among communities; and (2) procedural justice: the perceived quality of decision-making, treatment, and fairness of police procedures. They find that procedural justice has the greatest impact on perceived legitimacy, followed by the distributive fairness of police resources. As the reform seeks to increase the legal protections for people being prosecuted, it could have a positive effect on trust in the system by curbing the abuses of judicial authorities.

Apart from the effect on legitimacy, procedural law can influence variables directly affecting incentives for criminal behavior. Following the analysis of the reform by D. S. Nagin (2013a), we can consider three main types of incentives: certainty, severity, and celerity of punishment. These variables affect different elements of the incentive structure of delinquency. In the classical model of Becker (1968), criminal behavior is understood as a rational choice determined by a cost-benefit analysis of would-be criminals. There are two main factors to consider in this analysis: (1) the probability of being caught and therefore punished; and (2) the potential gains from crime. We would expect that the greater the probability or

harshness of punishment, the lesser the incentives for criminal activity. Chalfin and McCrary (2017) classify this effect on the incentives for criminal activity as deterrence, as opposed to incapacitation (when the preventive effect comes from criminals being imprisoned). There has been extensive research about the impact of both of these variables (Chalfin & McCrary, 2017; D. S. Nagin, 2013a). With regard to deterrence, it is usually agreed that there is greater evidence for the deterrent effect of the certainty of punishment (D. S. Nagin, 2013a, p. 85) than of its severity, and certainty is precisely the variable on which we expect the reform to have the greatest effect.

We can understand the certainty of punishment as composed of various conditional probabilities, corresponding to different phases in the criminal justice process (Zorro Medina et al., 2020; D. S. Nagin, 2013a): (1) the probability of apprehension given the commission of a crime; (2) the probability of being charged, given arrest; (3) the probability of conviction after being charged; and (4) the probability of a sentence being applied after it is passed. D. S. Nagin (2013a, p. 98) considers that the probability of apprehension probably has the greatest effect on crime. The reasons for this are varied. The first, and perhaps the most obvious, is that apprehension is usually necessary for the subsequent procedural steps leading to a legal sanction. Second, the probability of sanction given apprehension is relatively high for serious crimes. And third, apprehension implies various informal sanctions that can eventually be a greater deterrent, among other reasons because of their greater certainty and celerity, than formal sanctions (D. S. Nagin, 2013b).

Although we would expect the reform to have the greatest impact on the probability of conviction after being charged, it should also affect the probabilities of arrest and being charged, as the due process protections affect all procedural stages. For example, police may respond to laws imposing stricter conditions for apprehension by shifting resources away from activities leading to arrest (Atkins & Rubin, 2003), or even by recurring to extrajudicial methods of policing (Hausman & Kronick, 2020). This can have the effect of increasing crime, as Atkins and Rubin (2003) observed in their study of the exclusionary rule. Notwithstanding the importance of the probability of arrest, the deterrent effect of a greater certainty of punishment is present even in cases where it is not directly affected by police (Chalfin & McCrary, 2017), as in the case of "swift-and-certain"[10] sanctions (Hawken & Kleiman, 2009).

The punishment rate (Beckett & Beach, 2021) can be a way to approximate the proba-

[10]This policy has been applied mainly for probation violations. It consists of highly certain and quickly applied punishments that have been shown to have an effective deterrent effect.

bility of punishment, particularly the change in this measure. This ratio shows how many persons are prosecuted for each crime, and is, strictly speaking, a measure of punitivity. If punishment has a deterrent effect, we would expect an increase in this ratio to have a non-negative effect on the probability of punishment[11].

The second factor determining crime rates is incapacitation, whose preventive effect comes from the inability of the imprisoned to commit crimes. It is generally agreed that imprisonment is costly and can eventually make recidivism more likely (Chalfin & McCrary, 2017; D. Nagin et al., 2009). Despite these drawbacks, there is empirical evidence for the preventive effects of incapacitation, especially in the case of repeated reoffenders (Vollaard, 2013). Stricter limitations on the use of pretrial detention could take away the ability of prosecutors to curb crime by means of incapacitation.[12] We would expect greater due process protections to increase crime rates through the mechanisms of certainty of punishment as deterrence and of incapacitation by pretrial detention.

Celerity of punishment is another important causal factor in determining crime rates. Certain authors argue that this is due to the intertemporal discount of punishment: the closer in time a punishment, the greater its deterrent power (Davis, 1988; Zorro Medina, 2020; Lee & McCrary, 2017). This assertion has found some empirical support (Dalla Pellegrina, 2008). However, greater celerity could eventually have unintended consequences in the incentive structure of the actors in the criminal justice system. Dušek (2015) observed, for example, that in response to a policy change that increased the speed of sentencing for minor offenses, police and prosecutors shifted resources to prosecute the crimes that depended on the initiative of the authorities. He thus observed an increase in these minor crimes, which he explained mainly by this substitution effect, and a reduction in some victim-reported crimes[13]. In this regard, we would expect the reform to decrease the homicide rate. The mechanism of plea bargaining, as it avoids a full oral trial, should greatly expedite the sentencing process. In the case when the accused does go to trial, the new oral procedures should move more quickly than the old inquisitorial system. While the reform has no effect on substantive law, since it introduces no changes in the penalties of the criminal code, the mechanism of plea bargaining could reduce the severity of punishments. The evaluation of the Colombian reform by Zorro Medina et al. (2020) shows that this effect could eventually be counterproductive. The crime-reducing effects of greater certainty and celerity in the

[11] If criminals correctly assess the probability of punishment, we would expect each punishment to have a positive effect if the person is guilty and zero if he is innocent.

[12] At the potential cost of imprisoning innocent people, with all the negative consequences that entails.

[13] However, this effect was not robust to changes in specification.

reform could be offset by more lenient sentences, possibly producing a positive net effect on crime rates.

6 Data and Variables

6.1 Data Sources

For the statistics regarding celerity of punishment, indictments, and use of pretrial detention, we use the 1997-2012 Criminal Court Statistics (Estadísticas Judiciales en Materia Penal, EJMP) of INEGI.[14] This database records information for the population prosecuted or sentenced in criminal courts of first instance. The data is taken directly from the court's administrative records, and observations are reported at the case level. It includes various demographic variables (sex, age, educational level, etc.) and information relevant to the case (main charge,[15] aggravating circumstances, etc.). This allows us to evaluate the time between the commission of the crime and the first-instance sentence, the number of indictments and the percentage of cases under prosecution where pretrial detention was ordered during the first hearing.

The dataset on homicides comes from the General Death Statistics (Estadísticas de Defunciones Generales) of INEGI, which registers the number of deaths by cause and municipality according to official administrative records. Population projections at the municipality level come from CONAPO (2020).[16] We calculate murder rates using these variables for 1997-2012.

Poverty rates and Gini coefficients come from CONEVAL.[17] The agency provides three different measurements of the poverty rate: (1) *food poverty*: inability to pay for a basic food basket, even using all available household income; (2) *capacity poverty*: inability to pay for the basic food basket and necessary health and education expenses, even using all available household income; and (3) *wealth poverty*: inability to pay for the basic food basket and necessary health, education, clothing, housing, and transportation expenses, even

[14] Instituto Nacional de Estadística y Geografía (National Institute of Statistics and Geography).

[15] The database registers only the most serious charge.

[16] Consejo Nacional de Población (National Population Council), the official source of demographic data in Mexico.

[17] Consejo Nacional de Evaluación de la Política de Desarrollo Social (National Council for the Evaluation of Social Development Policy), a federal agency.

using all available household income. To calculate these poverty rates, we use data from the corresponding census and the National Survey of Household Income and Expenditure (Encuesta Nacional de Ingresos y Gastos de los Hogares, ENIGH). The Gini coefficient is calculated using the ENIGH data for the corresponding year.

The mean years of schooling per municipality comes from the 1990, 2000, and 2010 population censuses, conducted by INEGI. To complete the series, we fill missing intercensal years with values from the preceding census (for example, 2005 was given the values corresponding to the 2000 census).

The number of cartels comes from the data of (Coscia & Rios, 2012) on the presence of criminal organizations at the municipality level in Mexico. The party holding office at the municipal level comes from the CIDAC municipal elections database, which includes data for all municipal elections for the period 1985-2015. The winner (not included in the database) was taken to be the coalition receiving the most votes, in accordance with Mexican electoral law. We then codify whether the coalition included one of the major parties of the period (PRI, PRD, or PAN).

The data on annual seizures in Colombia comes from United Nations Office on Drugs and Crime (UNODC) for the period 1980-2018.

The data regarding the creation of new ejidos[18] comes from the Padrón e Historial de Núcleos Agrarios (PHINA),[19] a registry of new ejidos created each year, by municipality.

6.2 Descriptive Statistics

Figure 1 shows that the geographical distribution of the municipalities that implemented the reform during 2007-2012 is fairly even: they are in the northern, central, and southern regions of the country. Table 1 shows the number of treated municipalities by year of implementation of the reform. Our analysis considers only municipalities that implemented the reform in 2011 or earlier. We exclude those treated in 2012 for two reasons. First, more than half of these municipalities (51) are in Nuevo León. This state implemented the reform by type of crime and not by jurisdiction, as in the rest of the country, which makes comparison difficult. Second, most of these municipalities introduced the reform in 2012, so the reform had been in force for less than one year.

[18] Ejidos are a form of communal land ownership created after the Mexican Revolution.

[19] `ttps://phina.ran.gob.mx/index.php`

Figure 1: Year the reform came into force by municipality

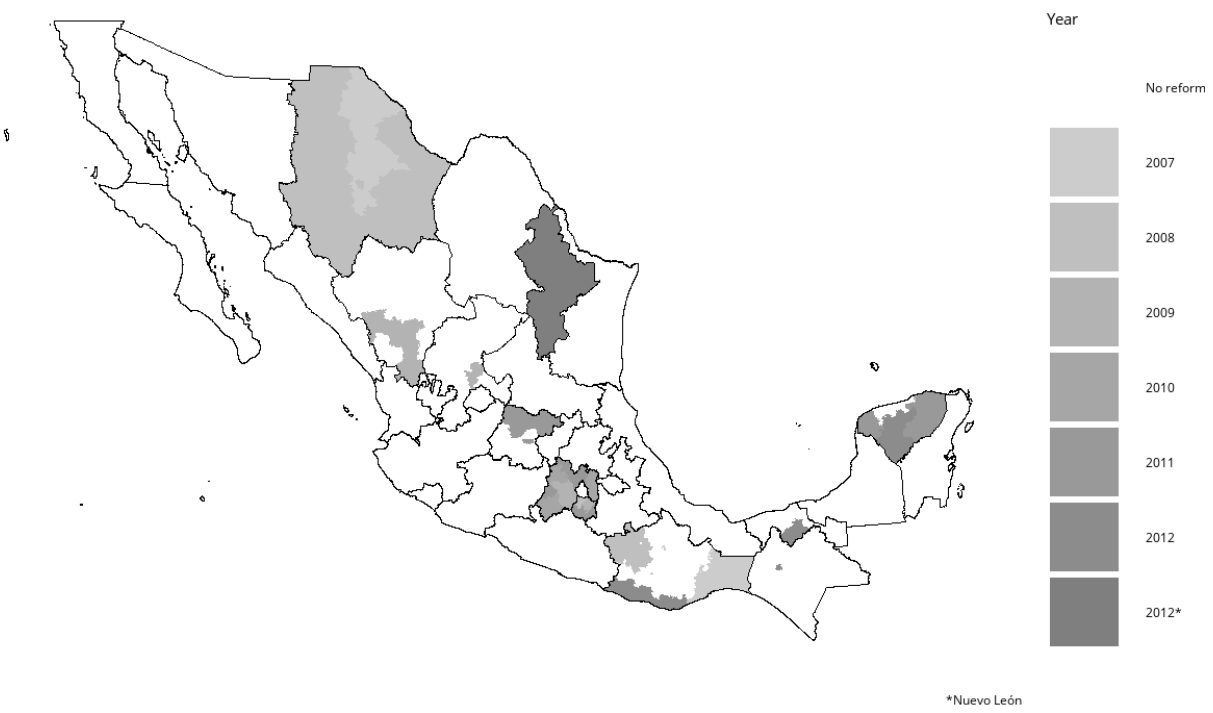


Table 1: Number of Municipalities by Year of Implementation of the Reform

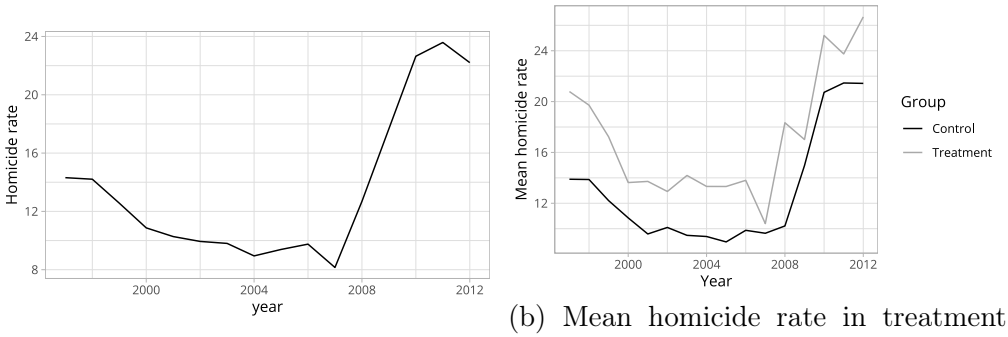
Group	No. of municipalities
Control group	1877
2007	52
2008	218
2009	51
2010	52
2011	106
2012	101

6.2.1 Homicide Rate

As we can see in Figure 2, panel (a), until the start of the drug war in 2007 the homicide rate had been on a downward trend since at least 1997. At that point the rate exploded, reaching its peak in 2011. The observed behavior of the homicide rate in the control and treatment

groups mirrors the national trend. In panel (b), we can see the mean murder rate of the municipalities in control and treatment groups for each year from 1997 to 2012. Both series follow very similar trends, with the treatment group starting at a higher level and noisier.

Figure 2: Homicide rate



(a) National homicide rate

(b) Mean homicide rate in treatment and control municipalities

We have chosen as covariates a set of known predictors of the murder rate: the poverty rate, the Gini index (Enamorado, López-Calva, Rodríguez-Castelán, & Winkler, 2016)), the number of cartels in the municipality (Coscia & Rios, 2012), the distance to the border (Camilo Castillo et al., 2020), mean years of schooling, new ejidos (Castañeda Dower & Pfutze, 2020), the number of formal jobs, and rural population.

Table 2 shows averages of these controls by treatment group for 1997-2006 (before the first municipality implemented the reform). The treatment group was, on average, slightly poorer, more unequal and rural, had fewer cartels, was farther from the border, and had slightly fewer years of schooling and formal jobs than the control group. The single greatest difference was in the creation of new ejidos, which were fewer in number in the treatment group.

Table 2: Means in Treatment and Control Groups (1997-2006)

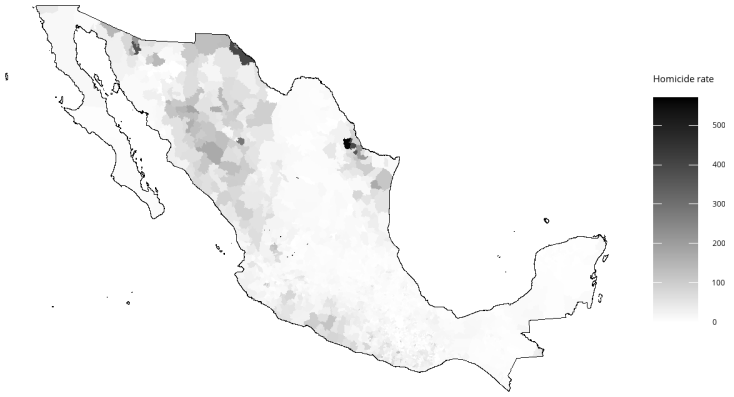
Variable	Mean, Control Group	Mean, Treatment Group
Homicide rate	10.82	15.27
Poverty rate	49.67	50.23
Gini index	0.44	0.45
Number of cartels present	0.06	0.05
Distance to the border (log)	0.74	0.83
Years of schooling	5.39	5.36
New ejidos	3.04	1.84
Rural population (%)	0.61	0.63

Finally, Figure 3 shows the mean homicide rate for each municipality before and after

2007, the date the first municipality implemented the reform and the year the drug war started. As can be seen, the surge in violence was unevenly distributed. The most affected regions were those located along the northern border, particularly those to the northwest and the northeast.

Figure 3: Mean homicide rate by municipality pre- and post- 2007

(a) 1997-2006



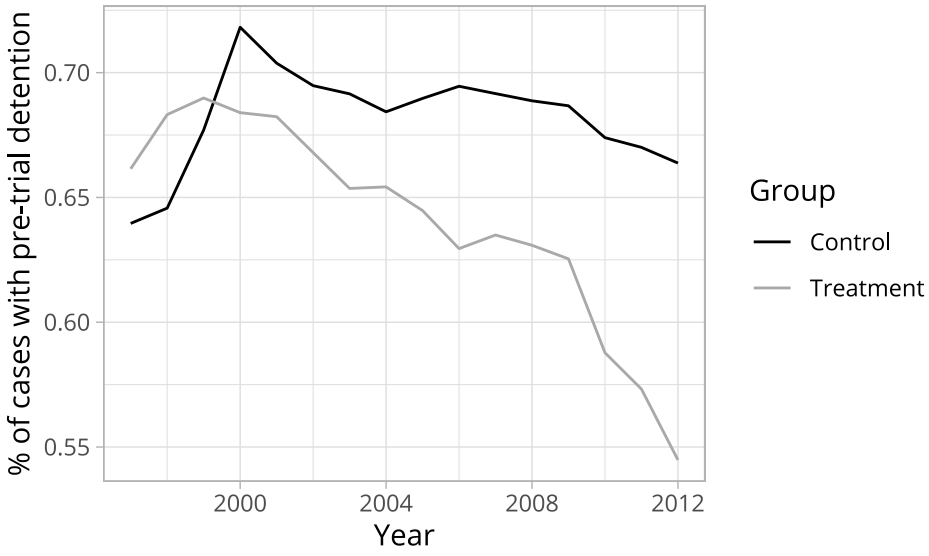
(b) 2007-2012

6.2.2 Use of Pretrial Detention

Figure 4 shows that the use of pretrial detention was on average lower in the treatment group for all of the 2000s. We also observe a downward trend in its use in both groups, beginning roughly in the year 2000, but with a steeper slope for the treatment group. Treatment municipalities were therefore already reducing their use of pretrial detention at a faster rate

than control municipalities years before the implementation of the reform.

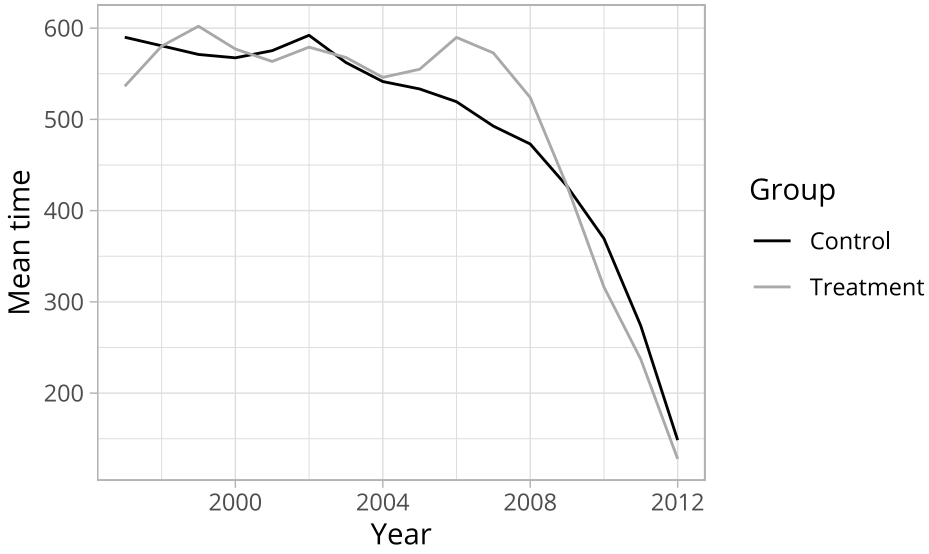
Figure 4: Use of pretrial detention



6.2.3 Celerity of Punishment

Figure 5 shows that the celerity of punishment was roughly equal in both groups for most of the period, except for 2006-2008, when the mean time until sentence was greater in treatment municipalities.

Figure 5: Celerity



7 Methodological Strategy

Measuring the impact of the reform in the particularities of the Mexican context is especially difficult. Because of the non-random nature of the adoption of the reform, there is significant heterogeneity between early adopters and non-adopters in their prior levels of the outcome variables, especially in violent crime. Unlike Hernández (2019), we do not know the criteria used to select the order of adoption.[20] This makes the difference-in-differences (DID) pretreatment parallel trends assumption unlikely to hold. We thus adopt a generalized synthetic control method to estimate the treatment effects, as described by Xu (2017).[21] This technique allows for differences in pretreatment trends by using latent factors that model heterogeneous effects at the unit level of common time trends. It is inspired by the technique of Abadie (2020), but differs with it in critical ways. While Abadie's synthetic control uses a convex combination of several control units to estimate the counterfactuals,[22] Xu (2017) uses the interactive fixed effects (IFE) model proposed by Bai (2009).

More importantly, this model makes different identification assumptions than Abadie's. The first is that it assumes a specific functional form. Let $\mathcal{T}$ be the set of treated units and $\mathcal{C}$ the set of controls. We assume that there are $N = N_{tr} + N_{co}$ units, where N_{tr} is the number of treated units and N_{co} the number of control units. Let T be the total number of periods in the panel, with T_{0i} being the last pretreatment period of the unit $i \in \mathcal{T}$. The data-generating process of the outcome variable must take the following form:

$$y_{it} = \delta_{it} D_{it} + \mathbf{X}'_{it}\beta + \lambda'_i f_t + \epsilon_{it} \tag{1}$$

where δ_{it} is the heterogeneous treatment effect for unit i at time t, D_{it} is a dummy variable that is equal to one if $i \in \mathcal{T}$ and $t > T_{0i}$ and zero otherwise, $\mathbf{X}'_{it}$ is a kx1 vector of observable covariates, β is a vector of parameters constant in both i and t, $f_t = (f_{1t}, ..., f_{rt})'$ and $\lambda_i = (\lambda_{i1}, ..., \lambda_{ir})'$ are $r \times 1$ vectors of factors and factor loadings respectively.

We can interpret the factors as common time-dependent shocks affecting all units and the factor loadings as the differential effects these impacts have on them. As an example, Bai (2009) explains that, if we were to study a wage rate in different age cohorts (where y_{it}

[20] Knowing the selection criteria would make the use of a matching estimator appropriate.

[21] This method is relatively novel in econometrics, but has already been used in many impact evaluations (Wood, Tyler, & Papachristos, 2020; Maamoun, 2019; Bayer & Aklin, 2020).

[22] A counterfactual is the behavior treated units would have had if they had not been exposed to the treatment.

represents the wage rate for individual i at age t), we could interpret factor loadings λ_i as the vector of unobservable personal characteristics or skills and the factor f_t as the vector of returns for those characteristics or skills.

Under this assumed functional form, the treatment effect is easily identifiable. Let $y_{it}(0)$ be y_{it} when $D_{it} = 0$ and $y_{it}(1)$ when $D_{it} = 1$. The treatment effect is given by the following equation:

$$y_{it}(1) - y_{it}(0) = \delta_{it} \qquad \text{for every } i \in \mathcal{T} \text{ and } t > T_{0i} \tag{2}$$

Another important assumption of this model is strict exogeneity. This means that the error term should not be correlated with treatment assignment, covariates, factor, or loadings of any other unit at any other time.

$$\epsilon_{it} \perp\!\!\!\perp D_{js}, \mathbf{X}_{js}, \lambda_j, f_s \qquad \text{for all } i, j, s, t \tag{3}$$

As Xu (2017) notes, this assumption precludes the possibility of past outcomes affecting future treatment assignment. If states experiencing surges in violence during the drug war postponed the adoption of the reform, our identification strategy could be compromised. Since procedural reforms change the rules under which criminals are tried and prosecuted, they could make justice systems more inefficient in the short run. It likely takes time for police, prosecutors, judges, and attorneys to get used to new rules and learn to operate efficiently under them. This is especially true for prosecutors, as the reform aims to strengthen due process protections. If this is the case, the effect of the reform would be underestimated.

The model also makes other assumptions that are standard for panel data models (weak serial dependence of the errors, regularity of conditions, cross-sectionally independent and homoscedastic errors). The estimation procedure for the average treatment effect on the treated (ATT) (δ_{it}) proposed by Xu (2017, p. 62) consists of three steps. Let $F = (f_1, ..., f_T)'$ be the vector of factors, Λ_{co} and Λ_{tr} the factor loadings of control and treated units respectively. In the first we estimate the $\hat{F}, \hat{\beta}$ and $\hat{\Lambda}_{co}$ using only the data of the control group.

$$(\hat{\beta}, \hat{F}, \hat{\Lambda}_{co}) = \arg\min \sum_{i \in Co} (y_i - \mathbf{X}_{it}\tilde{\beta} - \tilde{F}\tilde{\lambda}_i)'(y_i - \mathbf{X}_{it}\tilde{\beta} - \tilde{F}\tilde{\lambda}_i) \tag{4}$$
$$\text{s.t. } \tilde{F}'\tilde{F} = I_r \text{ and } \Lambda_{co}'\Lambda_{co} \text{ diagonal}$$

We then estimate the factor loadings of the treatment group by minimizing the mean square prediction error (MSPE) during the pretreament period. Let the superscript "0" denote the pre-treament periods, $Y_i = (y_{i1}, ..., y_{iT})'$ the vector of outcomes of unit i, $\mathbf{X}_i = (\mathbf{X}_{i1}, ..., \mathbf{X}_{iT})'$ the matrix of covariates for unit i, and $\hat{\beta}$ and $\hat{F}$ the estimates obtained in equation 4.

$$\hat{\lambda}_i = \arg\min (Y_i^0 - \mathbf{X}_i^0\hat{\beta} - \hat{F}^0\tilde{\lambda}_i)'(Y_i^0 - \mathbf{X}_i^0\hat{\beta} - \hat{F}^0\tilde{\lambda}_i) \qquad \text{for every } i \in \mathcal{T} \tag{5}$$

Once we estimate the parameters of our model, it is easy to construct the counterfactuals and to estimate the ATT.

$$\hat{y}_{it}(0) = \mathbf{x}_{it}'\hat{\beta} + \hat{\lambda}_i\hat{f}_t \tag{6}$$

$$\widehat{ATT}_t = \frac{1}{N_{tr}}\sum_{i\in\mathcal{T}}[y_{it}(1) - \hat{y}_{it}(0)] \qquad \text{for every } t > T_{0i} \tag{7}$$

Estimating the treatment effects of the criminal justice reform poses various methodological challenges. The implementation of the reform coincides with the beginning of the drug war. The resultant nationwide surge in homicides and violent crimes is especially problematic in measuring the effect of the reform on the murder rate. This is a potentially serious issue, as the treatment effects become difficult to disentangle from the contemporaneous shock of the drug war. In this regard, the synthetic control of Xu (2017) has various advantages over that of Abadie (2020). As the estimation of the β is performed over the whole period[23], it is possible to control for the drug war using well-known predictors of cartel-related violence, like the presence of cartels in the municipality (Coscia & Rios, 2012) or cocaine confiscations in Colombia interacted with distance to the U.S. border (Camilo Castillo et al., 2020). For our identification strategy to work, there should be no differential effect of the drug-war in control and treated municipalities conditional on our controls.

Because the date an investigation is opened determines whether the old or the reformed procedural code is to be used, it is possible that prosecutors opted to move up or postpone certain investigations in order to benefit from the advantages of one system or the other. There could thus be an anticipation effect of the reform for the variables that depend on this kind of decision: the use of pretrial detention and celerity of punishment. We believe that an effect of this kind is unlikely. Judges and prosecutors are bound in their decisions by the procedural code currently in force; even if they chose to move up or delay an investigation,

[23]The estimation of the fixed effects of the treatment group is, nonetheless, restricted to pre-treatment data.

awareness of an upcoming reform should not influence the likelihood that pretrial detention will be imposed. Manipulation of the timing of investigations could affect caseloads and thus the celerity of punishment. However, we found no statistically significant treatment effect on the number of indictments in the 12 months prior to the implementation of the reform (see Appendix B table B.1).

To measure the punishment rate we use indictments and not imprisonments, as it is usually done (Beckett & Beach, 2021). There are several reasons for this. First, given a stock of resources, the criminal justice system can only produce a finite number of prison sentences each year. Sentences are usually costly and take time, especially in the case of oral trials. Indictments use comparatively fewer resources, and usually imply pre trial detention in the case of grave crimes, so they can be an informal way to anticipate future formal punishment. It is easier for a criminal justice system to respond to a surge in crime by increasing indictments, and therefore increasing its case-load, than by investing resources building up its sentencing capacity. If the criminal justice system is already working at full capacity and resorting to indictments instead, the number of sentences would not reflect the actual level of punitiveness.

We use the punishment rate as measure of effectiveness. This ratio is a measure of the capacity and propensity of the criminal justice system to apply punishments (Beckett & Beach, 2021). In a perfectly punitive system, we would expect no unpunished crime, and therefore, at least one person charged (or imprisoned) for every crime. The quality of the punishment will depend on its deterrent effect, the gravity of the crime, and the overall social cost of punishment. It is not always better to punish more or more severely, and this is especially true when those punished have not committed the crime. But insofar we credit some socially beneficial effect to them, a functioning criminal justice should produce a positive level of punishment for every crime. Arguably, some grave crimes should always have low levels of impunity. This is especially true of those affecting a person's life and physical security, like homicides.

As a robustness test, we apply the placebo test and equivalence test proposed by Liu, Wang, and Xu (2020). The placebo test estimates the model, but with a treatment date a number of periods earlier than the real date. If there are no time-varying confounders, the estimated ATT for those anticipated periods should be non-significant.

The equivalence test is based on a proposal of Hartman and Hidalgo (2018). If there are no time-varying confounders, we would expect "the residuals [... to] be uncorrelated with

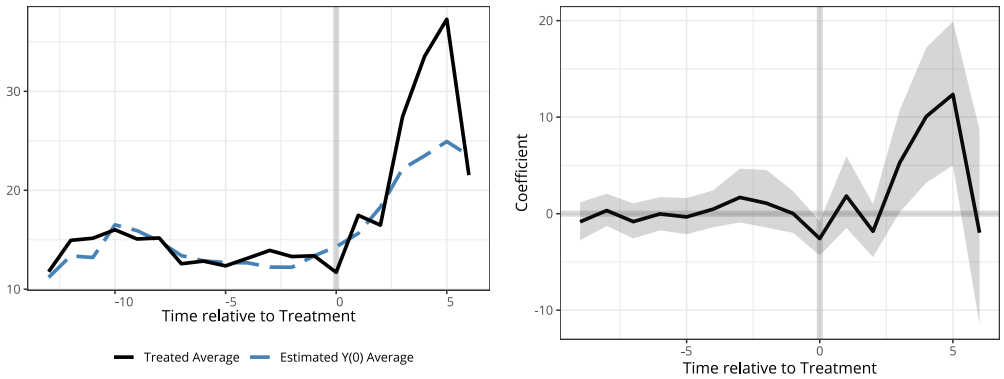
the number of time periods leading to the onset of the treatment" (Liu et al., 2020, p. 2); there should be no pretreatment trends in the data and the estimated treatment effects for the period should be close to zero. The test is of the null hypothesis that the ATT during the pretreatment period exceeds the previously set parameters θ_1, θ_2[24] such that $-\theta_2 < 0 < \theta_1$. If we can reject that the ATT is such that $ATT_s < -\theta_2$ or $ATT_s > \theta_1$ for all the pretreatment periods, we can conclude that the pretreatment ATT is likely bounded around zero and that there are no time-varying confounders. This test reverses the logic behind most null hypothesis tests for model assumptions, which usually assume a null hypothesis that is congruent with the assumption and consider a rejection of it as evidence against the correct specification of the model. In this case, the null hypothesis is incongruent with the correct specification of the model, and we take a rejection of it as evidence for the correct specification.

8 Results

8.1 Homicide Rate

According to our analysis, the reform had a positive average treatment effect on the treated (ATT) of 4.28. That is, the reform was associated with 4.28 additional murders per 100,000 inhabitants in the municipalities with the reform.

Figure 6: ATT for homicide rate



(a) Treated and counterfactual averages (b) Estimated ATT

[24]Following Hartman and Hidalgo (2018), Liu et al. (2020) recommend $\theta_1 = \theta_2 = 0.36\sigma_\varepsilon$ where $\sigma\varepsilon$ is the "standard deviation of the residualized non-treated outcome" (Liu et al., 2020, p. 22).

If we observe more carefully the dynamics of this effect in Table 3 and in Figure 6, we see that there was no significant effect until the third year after the implementation of the reform. There are several potential explanations for this delay. First, it could be the result of a learning process for criminals as they came into contact with the new system and came to understand it better. Criminals may not have been informed about the reform, and even if they were, it is often not obvious how new rules will affect the outcomes of a trial. We would thus expect a period of adjustment in the expectations of criminals regarding the probability of punishment under the new system.

Second, the delay could be the result of a non-linearity of the treatment effect in the murder rate and/or a heterogeneous effect of the drug war in the municipalities that were implementing the reform versus those that were not. The enhanced due-process protections granted by the reform may have had a greater effect when the criminal justice system was overwhelmed by new cases at higher levels of violence than at lower levels. It could also be argued that the reform put the criminal justice system under significant stress as all of the relevant actors became accustomed to the workings of the new system. In and of itself, this stress could have made it more difficult to effectively tackle crime, and it could have gotten progressively worse as the system confronted an unprecedented security crisis.

Third, another possible explanation for the delay is the hypothesis, proposed by Huebert (2019), that an increase in homicides could be specific to places where the state has lost its monopoly on violence to criminal organizations. In the following subsections, we explore the last two mechanisms: the specific impact of organized crime and the reduction in the ability to effectively tackle crime.

As seen in Table 3, there is a statistically significant treatment effect for the year prior to the onset of the reform. This effect, however, has a different sign than our cumulative ATT and seems to be the product of a sharp drop in the homicide rate in 2006 (see Figure 2). To test for the presence of time-varying confounders, we estimated a placebo effect three years before the actual adoption of the reform and also an equivalence test. Our results passed both those tests (Appendix D, Figure D.1).

8.1.1 Homicide and the Presence of Cartels

To test if the effect of homicides is specific to places where the state has lost its monopoly on violence to criminal organizations, we re-estimate the treatment effects for two subsamples.

Table 3: Estimated ATT by Year Relative to Implementation

Year	ATT	S.	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	4.28**	1.78	0.92	7.84	0.01	473.00
-9	-0.84	1.01	-2.77	1.18	0.42	0.00
-8	0.33	0.87	-1.28	2.06	0.70	0.00
-7	-0.83	0.93	-2.56	1.07	0.40	0.00
-6	-0.02	0.88	-1.73	1.71	0.98	0.00
-5	-0.34	0.98	-2.13	1.62	0.75	0.00
-4	0.47	0.99	-1.40	2.42	0.64	0.00
-3	1.69	1.41	-0.91	4.66	0.24	0.00
-2	1.08	1.56	-1.44	4.53	0.52	0.00
-1	0.01	1.11	-1.99	2.34	0.97	0.00
0	-2.58***	0.91	-4.34	-0.79	0.01	0.00
1	1.83	1.89	-1.49	5.94	0.31	473.00
2	-1.83	1.40	-4.50	1.00	0.18	473.00
3	5.26**	2.73	0.15	10.70	0.05	368.00
4	10.07***	3.68	3.20	17.21	0.00	318.00
5	12.35***	3.92	5.00	19.91	0.00	268.00
6	-1.97	5.10	-11.46	8.72	0.69	51.00

Notes: *** p<0.01, ** p<0.05, * p<0.1; total municipalities n = 2277, control group n = 1804. The cumulative ATT corresponds to the average effect for all treatment-years. The standard errors and the 95% confidence interval are estimated using 2000 bootstraps.

The first is composed of municipalities that had no cartels in 2006 (the year prior to the start of the drug war); the second includes municipalities that had one or more cartels.

Table 4 shows that the cumulative effect is positive but not significant for municipalities that had no cartels during 2006. For the group with one or more cartels, the effect is much greater than in our previous estimate. This result is congruent with the second and third hypotheses. If we consider the dynamics of the effect (Appendix D, Tables D.3 and D.2), we see that for the first group the effect is significant only in the fourth year of the reform, and for the second group only in the fourth and fifth years.

Although some coefficients for the periods prior to the implementation of the reform are statistically significant (Tables D.2 and D.3), the coefficient for the year prior to the reform has the opposite sign as our estimated ATT. The rest of the coefficients are also negative, except for two years in the group with one or more cartels, but those coefficients are also quantitatively much weaker than our estimated cumulative ATT.

The equivalence test, which is specifically designed to test the H_0 that the treatment

Table 4: Estimated ATT

	No cartels	One or more
Cumulative ATT	2.75	18.59
SE	1.71	6.9
95% CI	-0.54, 6.14	5.55, 32.26
p-value	0.112	0.007***
Municipality fixed-effect	Yes	Yes
Year fixed-effect	Yes	Yes
No. of factors	0	0
Years	16	16
Treatment municipalities	524	50
Always-control municipalities	1587	217
Total municipalities	2111	267

Notes: *** p<0.01, ** p<0.05, * p<0.1; the cumulative ATT corresponds to the average effect for all treatment-years. The standard errors and the 95% confidence intervals are estimated using 2000 bootstraps.

effects for the pretreatment period are not bounded around zero, is highly significant for both subsamples. The placebo test, which estimates the effect by artificially moving the implementation date of the reform three years earlier, gives a non-significant effect for those three years (Appendix B, Figures 19 and 20).

8.1.2 Effect on the punishment rate for homicides

In this subsection, we test the hypothesis that this effect could have been due to the reduction in the capacity of the criminal justice system to effectively tackle crime in the context of cartel presence. To measure this capacity we compute the ratio between indictments and homicides committed in each municipality. We would expect this ratio to be greater in more efficient systems[25]. As we do in the previous analysis, we estimate the ATT in two sub-samples as a function of cartel presence. If our hypothesis is correct, namely, that the reduction in the capacity to effectively combat crime is specific to those experiencing a security crisis, we would expect a negative effect only in the subsample with cartel presence.

As we can see in table 5, the cumulative ATT of the reform in the ratio is negative and significant for the subsample with cartel presence, and positive but non significant in the

[25]One of the shortcomings of the Mexican criminal justice system is its high impunity rate. This is mainly due to the low number of indictments, as the probability of a guilty sentence once prosecuted is very high.

subsample with no cartels. Those results pass both the equivalence and placebo test.

Table 5: Estimated coefficients

	No cartels	One or more
Cumulative ATT	0.06	-0.3
SE	0.05	0.15
95% CI	-0.05, 0.16	-0.61, 0
P-value	0.289	0.047**
Municipality fixed-effect	Yes	Yes
Year fixed-effect	Yes	Yes
No. of factors	0	0
Years	16	16
Treatment municipalities	188	16
Always-control municipalities	1588	92
Total municipalities	1776	108

Notes: *** p<0.01, ** p<0.05, * p<0.1; the cumulative ATT corresponds to the average effect for all treatment-years. The standard errors and the 95% confidence intervals are estimated using 2000 bootstraps.

8.2 Use of Pretrial Detention

Figures 7-11 and Table 6 show that there was a significant effect in the reduction in pretrial detention for most subcategories of crime. There was an average ATT of -8 percent for all categories of crime. This reduction seems to have been greater for property crimes, where the average reduction due to the reform was around 16 percent. The effect on rape was also significant, with a reduction of 7 percent. The point estimate for homicide and kidnapping was also negative but not significant, which supports the hypothesis of the specificity of the effect of the reduction in pretrial detention.

The treatment effect for all crimes begins to be significant from the second year of the reform and stabilizes at around -12 percent. In the case of property crimes, we observe a significant effect from the first year of the reform, eventually stabilizing at a reduction of 20 percent in the use of pretrial detention. In the case of rape, we observe an average reduction of 7 percent, less than that for property crimes. The dynamic is also not as consistent as that of the previous two crimes, with a reverse in the effect during the 5th year.

As we can see, the effect differs, depending on the crime. It is greater for property crimes and non-significant for homicide and kidnapping, indicating some specificity relative to the

seriousness of the crime. While rape is also a serious crime, sex crimes suffer from well-known specific procedural difficulties that make them difficult to prosecute, and possibly also make it difficult to obtain pretrial detention.

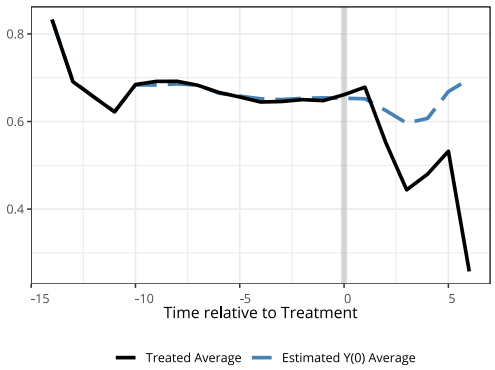
We again estimated the placebo effect three years before the real adoption of the reform and an equivalence test for each category of crime. All of the categories of crime passed both tests. For more information about the placebo test, the dynamics of the effect, the coefficients of the controls, factor, and loadings, see E the figures E.3, E.6, E.10, E.13 (placebo test), tables E.1, E.2, E.3,E.5 (dynamics of the effect), and figures E.1, E.4, E.8, E.11 (factors) and E.2, E.5, E.9 and E.12 (loadings).

Table 6: Estimated ATT (Use of pretrial detention)

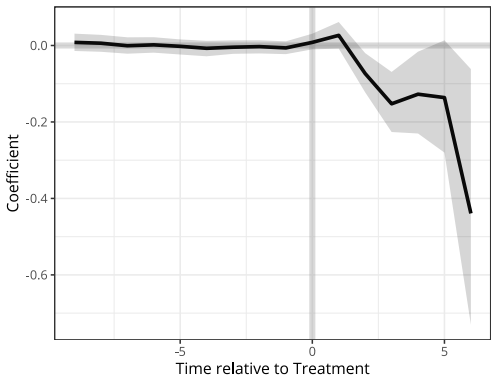
	All crime	Homicide	Kidnapping	Property crime	Rape
Cumulative ATT	-0.08	-0.05	-0.05	-0.16	-0.07
SE	0.03	0.06	0.05	0.02	0.05
95% CI	-0.14, -0.01	-0.16, 0.06	-0.15, 0.03	-0.21, -0.12	-0.22, 0
p-value	0.028^{**}	0.361	0.213	$>0.01^{***}$	0.042^{**}
Municipality fixed-effect	Yes	Yes	Yes	Yes	Yes
Year fixed-effect	Yes	Yes	Yes	Yes	Yes
No. of factors	2	1	0	1	1
Years	16	16	16	16	16
Treatment municipalities	95	77	73	85	76
Always-control municipalities	465	403	357	444	390
Total municipalities	560	480	430	529	466

Notes: *** p<0.01, ** p<0.05, * p<0.1; the cumulative ATT corresponds to the average effect for all treatment-years. The standard errors and the 95% confidence interval are estimated using 2000 bootstraps. The sample consists of municipalities with at least one first-instance criminal court with cases corresponding to each type of crime. The common support condition for factors is satisfied for all categories of crimes (see Appendix E Figures E.2, E.5, E.9 and E.12)

Figure 7: Use of pretrial detention - All crime

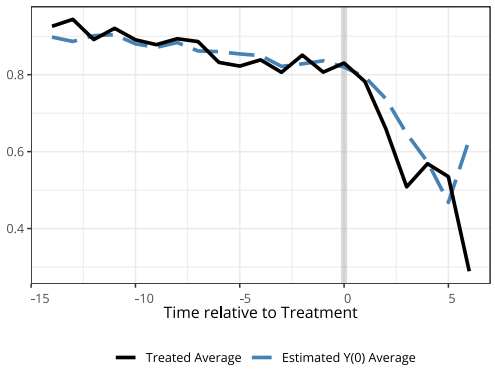


(a) Treated and counterfactual averages

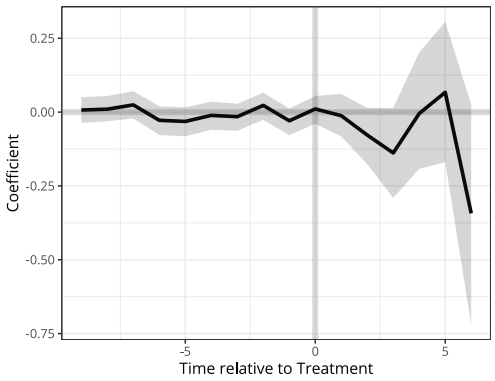


(b) Estimated ATT

Figure 8: Use of pretrial detention - Homicide

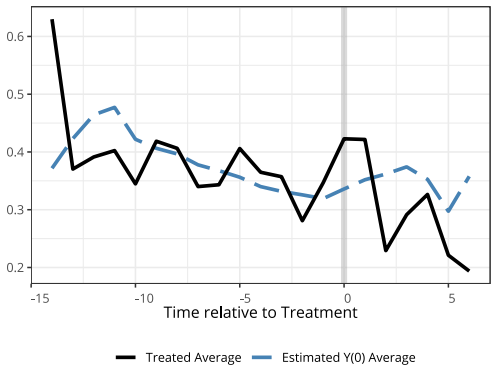


(a) Treated and counterfactual averages

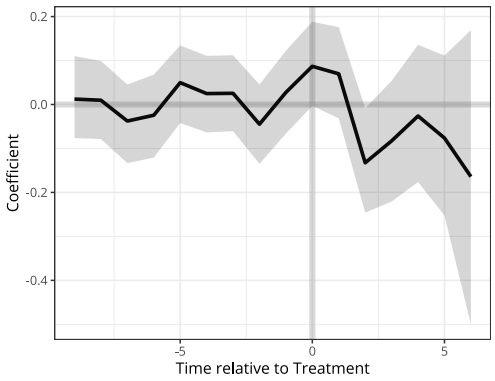


(b) Estimated ATT

Figure 9: Use of pretrial detention - Kidnapping

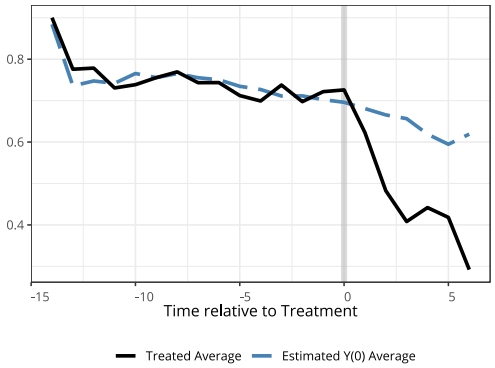


(a) Treated and counterfactual averages

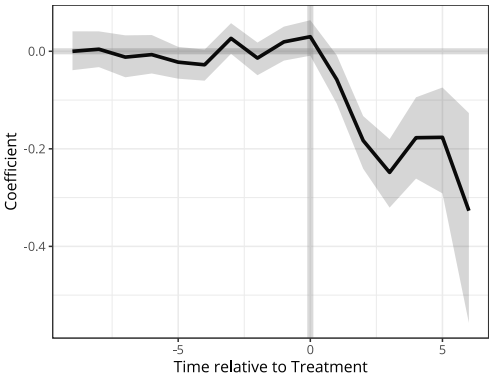


(b) Estimated ATT

Figure 10: Use of pretrial detention - Property crime

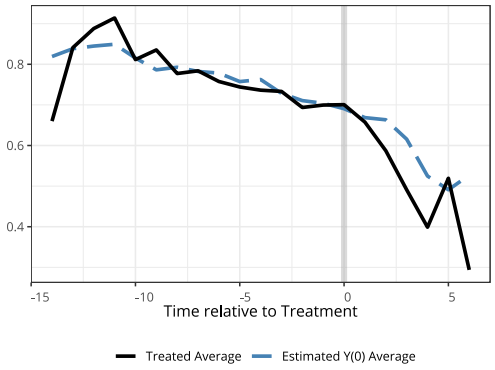


(a) Treated and counterfactual averages

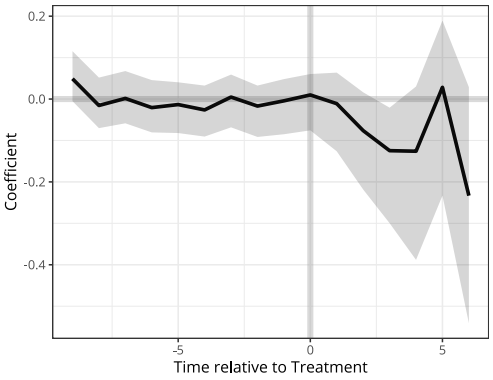


(b) Estimated ATT

Figure 11: Use of pretrial detention - Rape



(a) Treated and counterfactual averages



(b) Estimated ATT

8.3 Celerity of Punishment

Figures 12-16 and Table 7 show the estimated effect of the reform on the celerity of punishment. There is no significant general effect, although there seems to be a significant reduction during the second, third, and sixth years of the reform (Appendix F.1, Table F.1). Because of the small number of treatment municipalities that have that many years of experience with the reform, the effect on the sixth year is mostly not significant for any subcategory of crime.

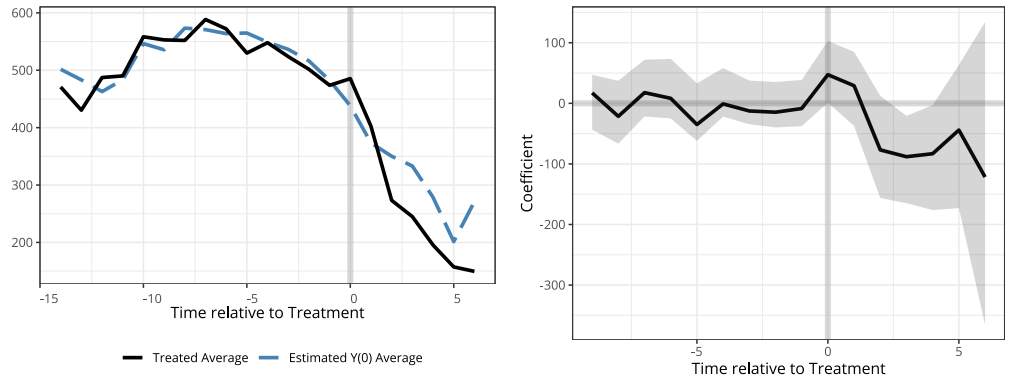
Regarding the analysis by type of crime, the reduction was only significant for property crime, with a mean reduction of 58 days (Appendix F.4 Table F.4). The effect starts to be significant from the second year, which could be explained by the difficulties of implementation of the reform during the first year. There also seems to be a tendency for the effect for property crime to wane in time after reaching a peak during the second year of the reform. There seems to have been a positive effect the year before implementation of the reform. This could be explained by the disruption brought by the administrative and organizational changes needed to prepare the implementation, which could have slowed down justice.

Table 7: Estimated Coefficients

	All crime	Homicide	Kidnapping	Property crime	Rape
Cumulative ATT	-41.92	-74.94	-93.09	-58.29	14.25
SE	35.01	46.1	104.75	28.87	30.29
95% CI	-112, 22	-168, 11	-312, 92	-116, -7	-44, 74
p-value	0.256	0.097*	0.371	0.022**	0.612
Municipality fixed-effect	Yes	Yes	Yes	Yes	Yes
Year fixed-effect	Yes	Yes	Yes	Yes	Yes
No. of factors	1	0	0	1	0
Years	16	16	16	16	16
Treatment municipalities	76	60	13	68	50
Always-control municipalities	489	441	373	472	415
Total municipalities	565	501	386	540	465

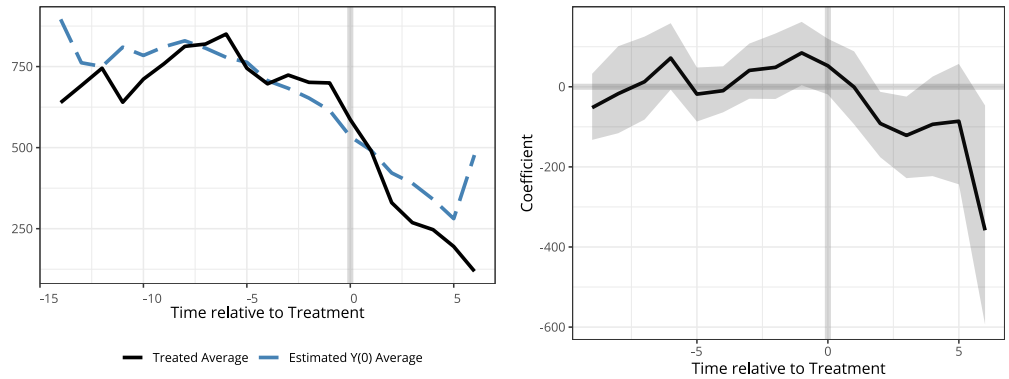
Notes: *** p<0.01, ** p<0.05, * p<0.1; the cumulative ATT corresponds to the average effect for all treatment-years. The standard errors and the 95% confidence interval are estimated using 2000 bootstraps. The sample consists of municipalities with at least one first-instance criminal court with cases corresponding to each type of crime. The common support condition for factors is satisfied for all categories of crimes (see Appendix E Figures E.2, E.5, E.9 and E.12.)

Figure 12: Time elapsed until sentence - All crime



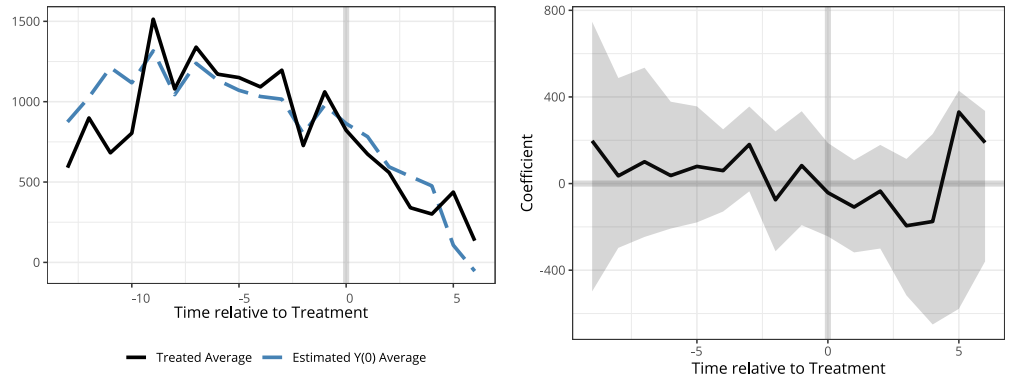
(a) Treated and counterfactual averages (b) Estimated ATT

Figure 13: Time elapsed until sentence - Homicide



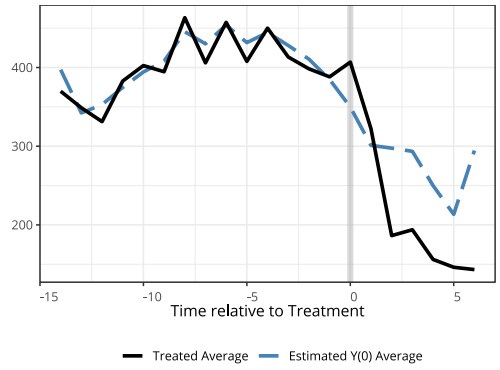
(a) Treated and counterfactual averages (b) Estimated ATT

Figure 14: Time elapsed until sentence - Kidnapping

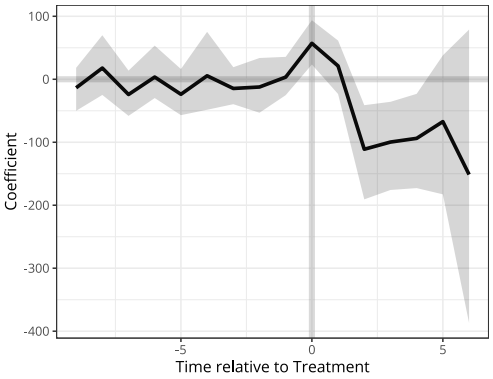


(a) Treated and counterfactual averages (b) Estimated ATT

Figure 15: Time elapsed until sentence - Property crime

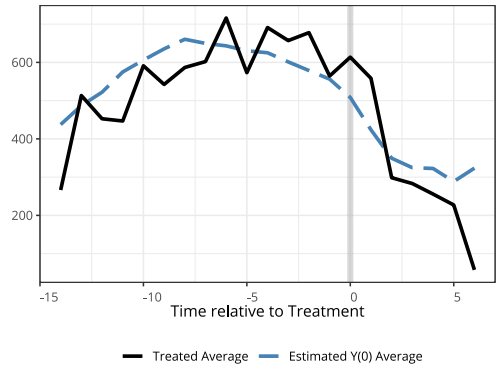


(a) Treated and counterfactual averages

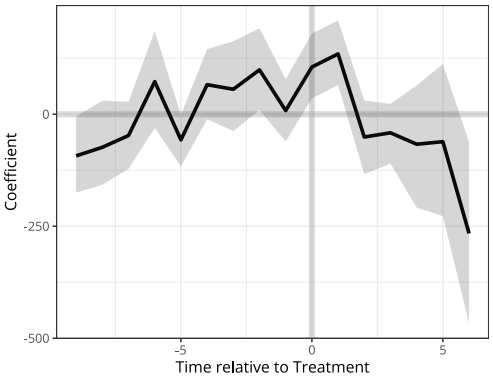


(b) Estimated ATT

Figure 16: Time elapsed until sentence - Rape



(a) Treated and counterfactual averages



(b) Estimated ATT

9 Discussion

In this paper, we evaluate the impact of the reform of the criminal justice system in Mexico on the states that first adopted it. Our analysis shows that the reform had an overall effect of increasing the homicide rate, but that this effect was specific to the municipalities with one or more drug cartels in 2006.

We also observe a reduction in the use of pretrial detention of 8 percent in the "all crime" category. The reduction is greatest for property crime, smaller but still significant for rape cases, and non-significant for homicide and kidnapping. The effect seems, therefore, to be focused on less serious crimes (like property crime) or on crimes that are specially hard to prove (like rape).

It is unclear from our data if the reform was effective in improving the celerity of punishment, but we observe a statistically significant reduction in the case of property crime, which is in line with the findings of (Zorro Medina et al., 2020) for the reform in Colombia. Contrary to (Zorro Medina, 2020), we also note a significant reduction in the use of pretrial detention, at least during the first procedural phases.[26] While these objectives can be worthy pursuits in and of themselves, it is hard to say whether the reform improved the overall quality of the justice system.

The effect we observe on the murder rate seems to be in line with the positive effect on criminality observed by (Zorro Medina et al., 2020) and the results of Huebert (2019), before controlling for the most affected states in the drug war, where she finds a positive but non-significant result.[27] Due to the limitations of our data, we are not able to measure the impact on other types of crime, but the evidence presented by Hernández (2019) would suggest greater effects on other crimes.

There are some limitations to our study, mainly related to lack of data. While we observe a reduction in the use of pretrial detention during the first stages of the process, we cannot know whether this decision is reversed in later stages. There could eventually be a substitution effect where people who would have been put in pretrial detention earlier under the old system are put in detention later under the new system. The same can be said about the effect on the time between commission of a crime and sentencing.

[26] Our data only registers the decision made during the *auto de termino constitutional*. This decision can be overturned at later stages.

[27] She does find a negative effect after she excludes the states most affected by the war from the sample.

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Supplementary materials (not for publication)

A Appendix - Procedural phases

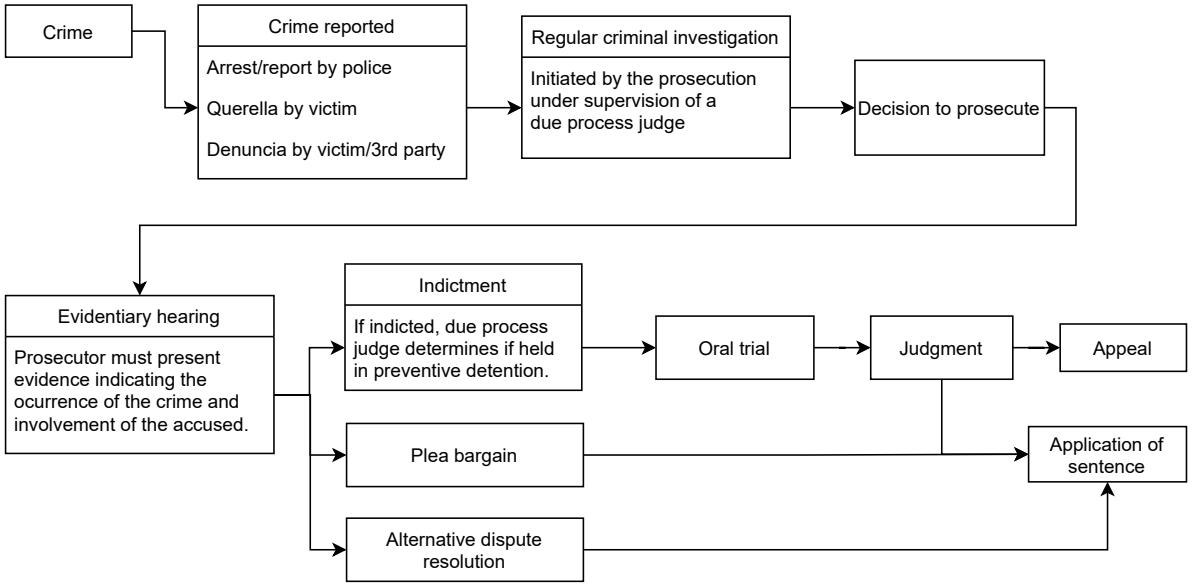


Figure A.1: New Adversarial Criminal Procedure Model in Mexico. Based on Shirk(2011)

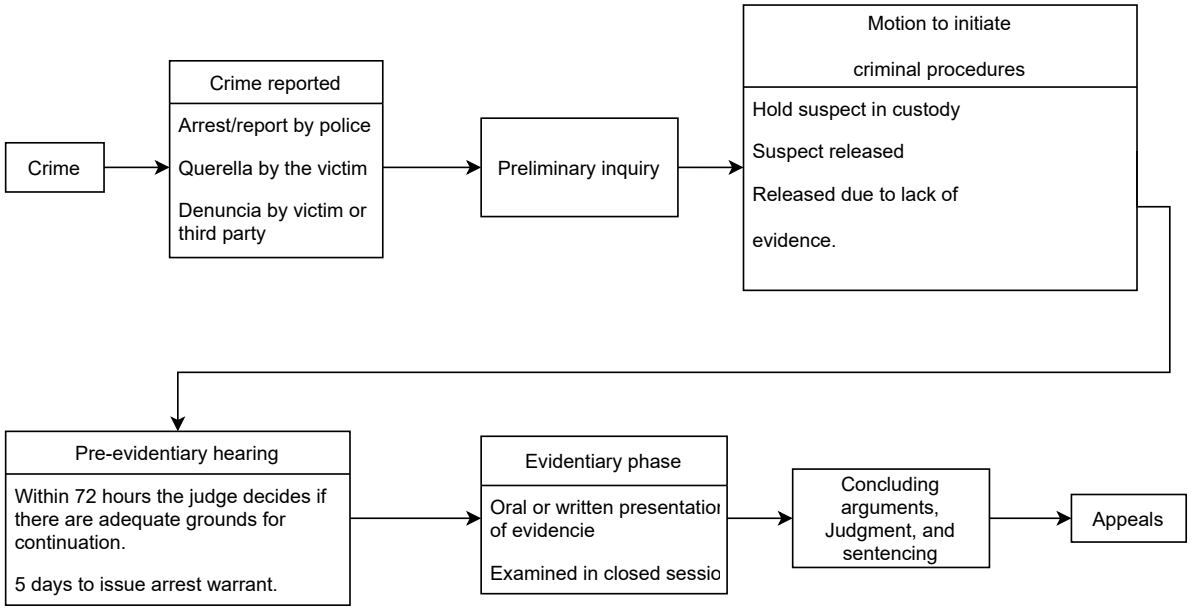


Figure A.2: Old Inquisitorial Criminal Procedure Model in Mexico. Based on Shirk(2011)

B Appendix - Indictments

Table B.1: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-17.55**	6.27	-27.93	-5.06	0.01	81.00
-12	0.76	2.27	-3.43	5.16	0.72	0.00
-11	0.8	2.40	-3.45	5.76	0.82	0.00
-10	-1.66	1.52	-4.79	1.31	0.26	0.00
-9	-1.04	1.95	-4.79	3.19	0.56	0.00
-8	1.3	1.82	-2.56	4.86	0.43	0.00
-7	-0.92	2.20	-4.22	4.00	0.85	0.00
-6	0.04	1.98	-3.09	4.14	0.90	0.00
-5	-2.11	2.14	-5.46	2.93	0.50	0.00
-4	1.77	4.40	-5.12	12.50	0.91	0.00
-3	-2.6	1.63	-5.31	0.81	0.21	0.00
-2	-1.87	2.38	-6.65	2.40	0.40	0.00
-1	-0.2	2.16	-4.78	3.71	0.76	0.00
0	-2.76	2.07	-6.49	1.55	0.17	0.00
1	-10.55*	3.75	-14.84	-0.66	0.05	81.00
2	-9.85	4.41	-14.96	4.87	0.16	78.00
3	-16.7**	5.51	-23.11	-0.71	0.04	72.00
4	-13.25*	4.83	-19.05	0.57	0.06	73.00
5	-15.81	5.69	-22.69	0.93	0.10	68.00
6	-16.47**	5.99	-24.96	-0.85	0.04	67.00
7	-20.15**	6.47	-30.36	-5.09	0.04	68.00
8	-17.95**	5.50	-26.30	-6.38	0.02	62.00
9	-19.83***	5.50	-30.44	-9.35	0.00	61.00
10	-15.6***	4.47	-23.21	-6.35	0.00	59.00
11	-22.58***	5.12	-31.00	-12.58	0.00	57.00
12	-20.89***	5.15	-30.17	-10.66	0.00	58.00

Notes: *** p<0.01, ** p<0.05, * p<0.1; total municipalities n = 567, control group n = 486
Only 12 month before and after treatment presented

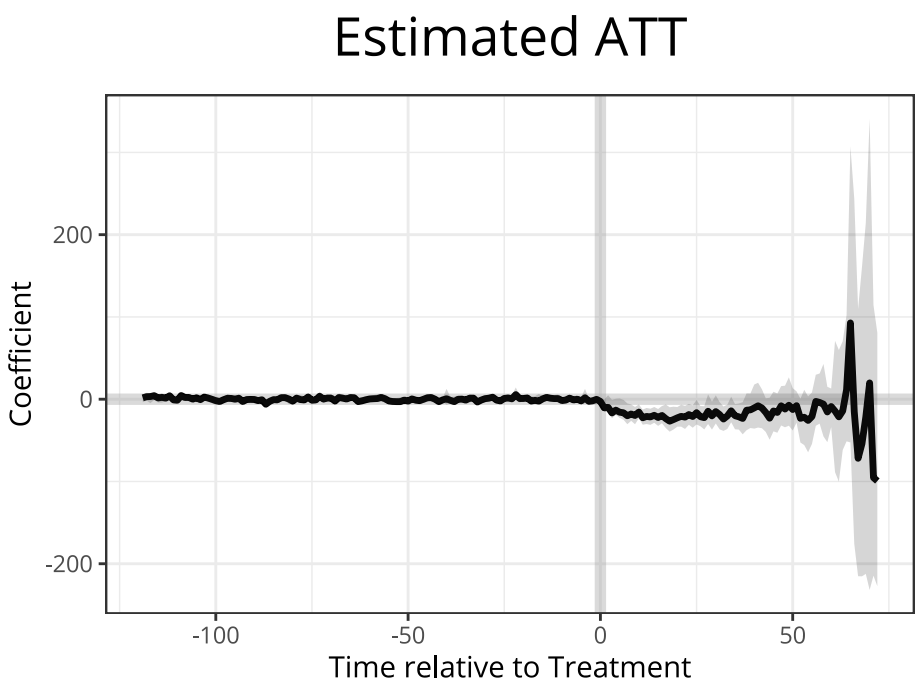
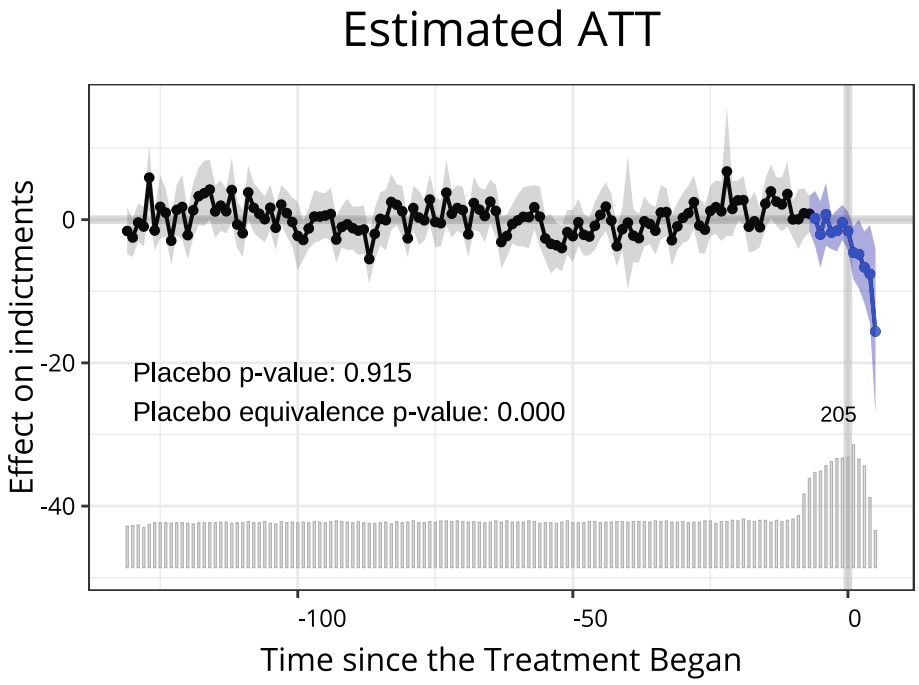


Figure B.1: Placebo and equivalence test for homicide rate



C Appendix - Punishment rate

Figure C.3: Placebo and equivalence test for indictment/homicide ratio (no cartels)

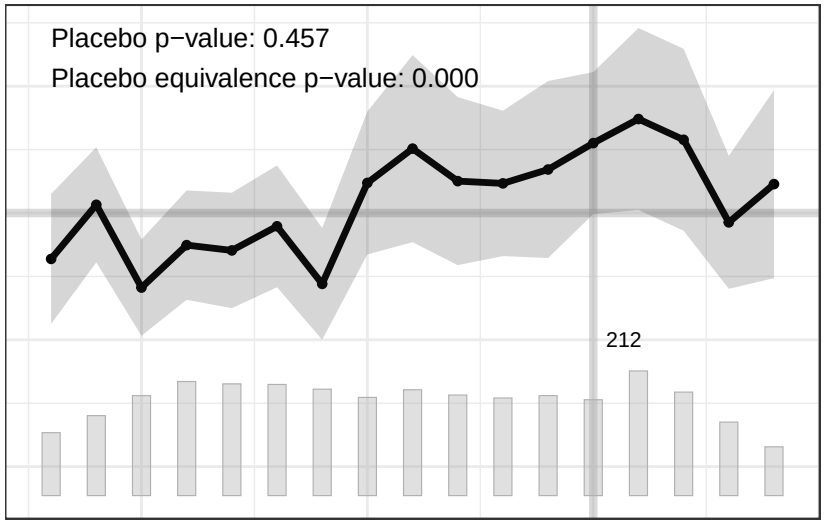
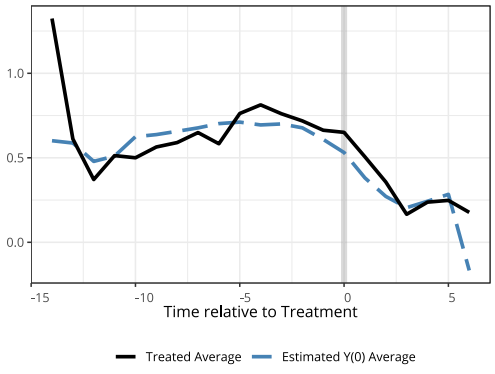
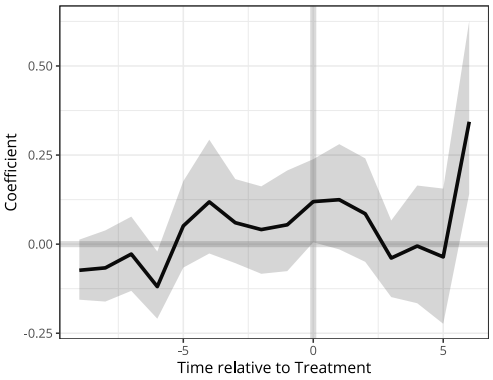


Figure C.1: Number of indictments for every homicide - No cartels

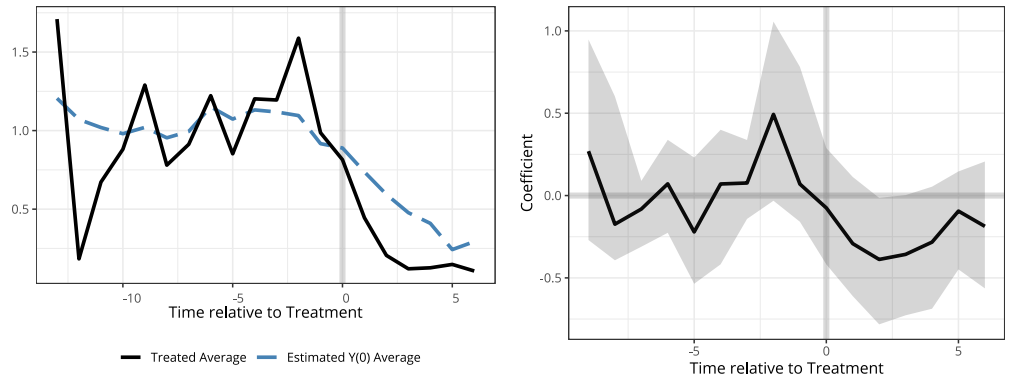


(a) Treated and counterfactual averages



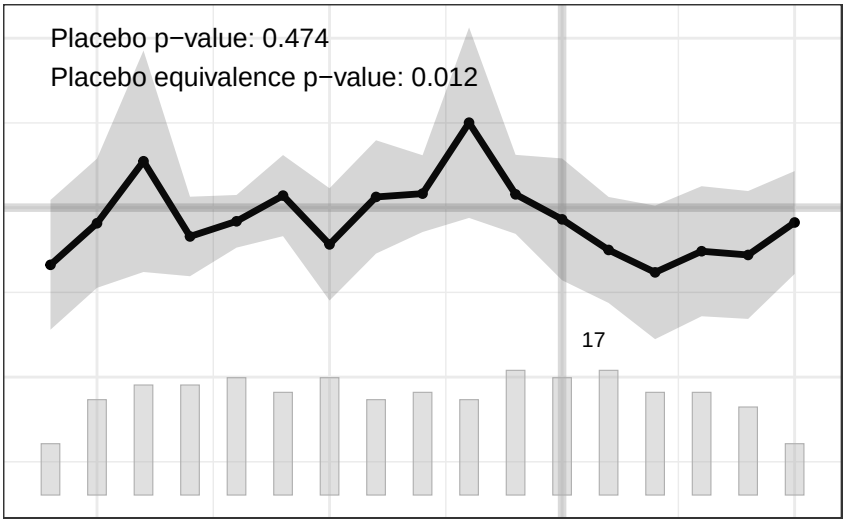
(b) Estimated ATT

Figure C.2: Number of indictments for every homicide - One or more cartels



(a) Treated and counterfactual averages (b) Estimated ATT

Figure C.4: Placebo and equivalence test for indictment/homicide ratio (One or more cartels)

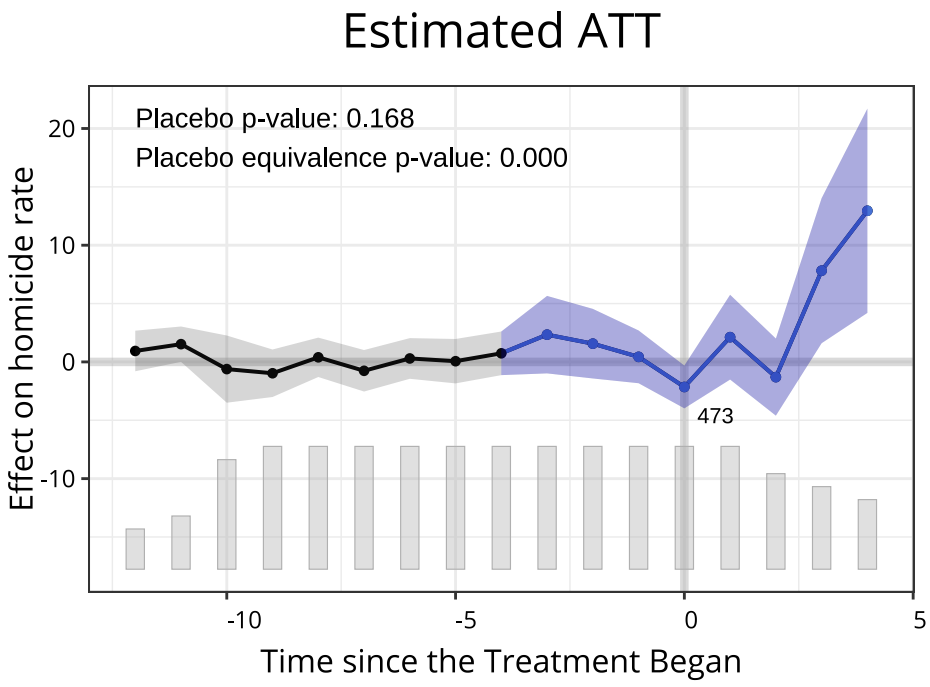


D Appendix - Homicide Rate

Table D.1: Estimated Coefficients

	beta	SE	CI (lower)	CI (upper)	p-value
Capacity poverty	-0.00	0.03	-0.06	0.06	1.00
Gini coefficient	-13.56	12.21	-39.89	6.31	0.29
Mean years of schooling	-0.80	0.82	-2.45	0.75	0.31
Number of cartels present	0.44	0.86	-1.17	2.13	0.66
log(distance) * cocaine seizures in Colombia	0.00	0.00	0.00	0.00	0.00
PRI in municipal government	-0.37	0.61	-1.59	0.85	0.55
PAN in municipal government	0.43	0.62	-0.77	1.64	0.50
PRD in municipal government	-0.21	0.69	-1.50	1.14	0.78
New ejidos created under PRI Rule	-0.12	0.03	-0.18	-0.07	0.00
Young males (% total population)	-164.05	75.12	-313.50	-16.70	0.03
Rurality (% total population)	-7.20	4.08	-16.22	-0.56	0.03

Figure D.1: Placebo and equivalence test for homicide rate



D.1 Estimation of sub-samples

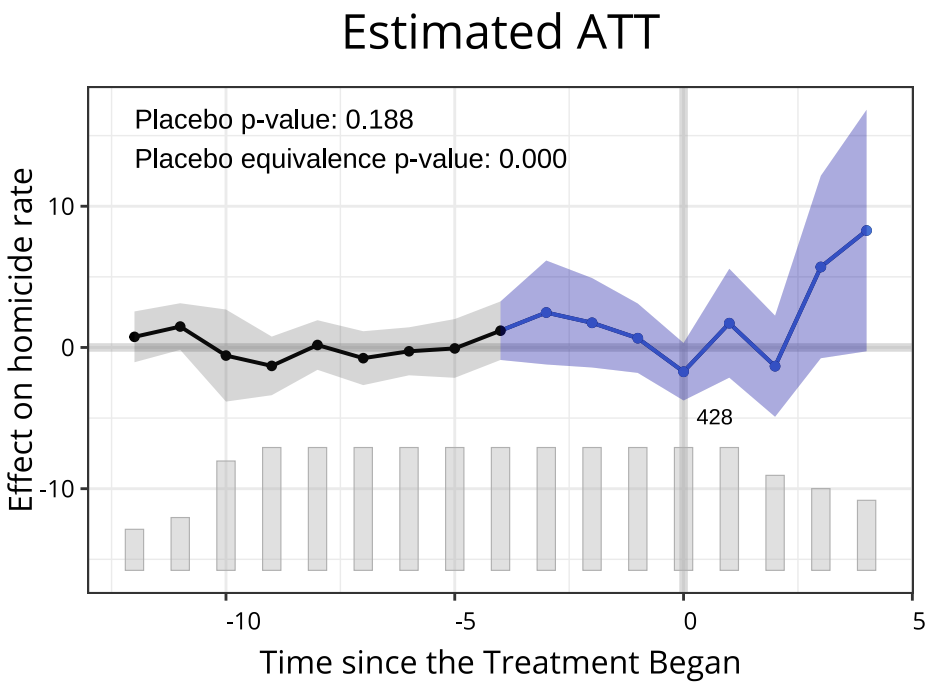


Figure D.2: Placebo and equivalence test for group without cartels

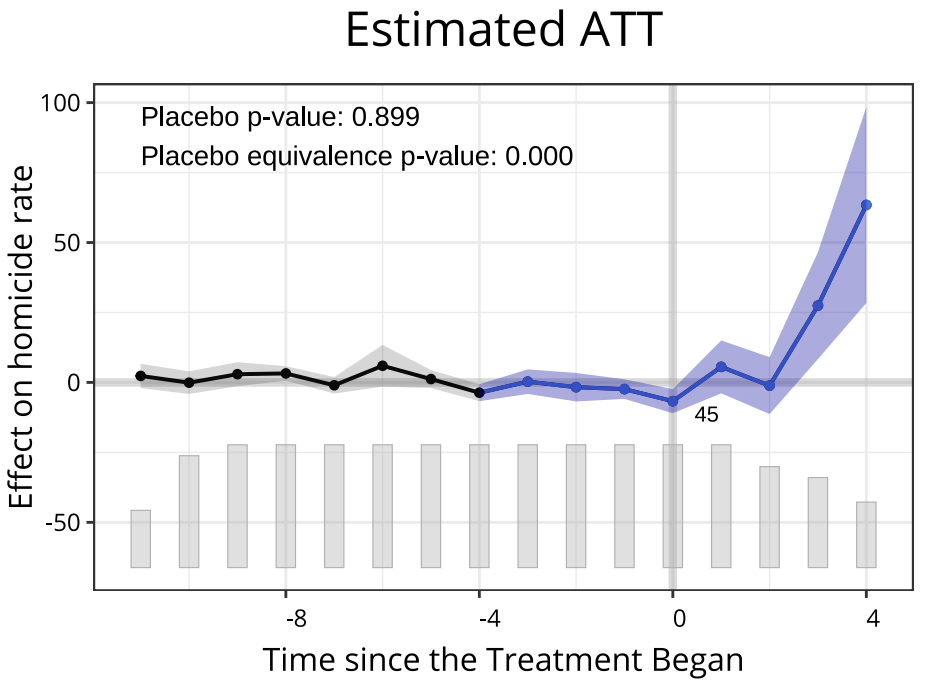


Figure D.3: Placebo and equivalence test for group with one or more cartels

Table D.2: Estimated ATT by Year Relative to Implementation (No cartels)

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	2.75	1.71	-0.54	6.14	0.11	524.00
-9	-1.78*	0.98	-3.69	0.10	0.07	0.00
-8	0.73	0.95	-1.04	2.69	0.44	0.00
-7	-1.82*	0.90	-3.44	0.08	0.07	0.00
-6	-2.25***	0.82	-3.76	-0.60	0.01	0.00
-5	-1.04	0.93	-2.83	0.87	0.27	0.00
-4	-0.23	0.96	-2.06	1.71	0.81	0.00
-3	2.51*	1.49	-0.05	5.62	0.05	0.00
-2	-0.09	1.48	-2.68	3.22	0.90	0.00
-1	-0.81	1.08	-2.84	1.43	0.42	0.00
0	-2.16**	0.92	-3.93	-0.27	0.03	0.00
1	0.65	1.74	-2.34	4.48	0.77	524.00
2	-1.36	1.45	-4.13	1.54	0.35	428.00
3	3.95	2.76	-1.18	9.61	0.15	331.00
4	6.4*	3.75	-0.60	14.17	0.09	285.00
5	9.4***	3.97	2.03	17.47	0.01	244.00
6	-1.8	5.59	-11.94	9.86	0.70	47.00

Note: *** p<0.01, ** p<0.05, * p<0.1
total municipalities n = 2111, control group n = 1587

Table D.3: Estimated ATT by Year Relative to Implementation (One or More Cartels)

Year	ATT	S.	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	18.59***	6.90	5.55	32.26	0.01	50.00
-9	2.7	2.10	-1.38	6.95	0.20	0.00
-8	3.15**	1.31	0.55	5.77	0.01	0.00
-7	-0.29	1.45	-3.14	2.50	0.86	0.00
-6	5.7**	3.45	0.12	13.29	0.04	0.00
-5	1.49	1.58	-1.20	4.64	0.31	0.00
-4	-2.57*	1.57	-5.62	0.49	0.10	0.00
-3	0.26	1.72	-3.13	3.63	0.92	0.00
-2	-2.2	2.18	-5.97	2.56	0.31	0.00
-1	-3.59*	1.75	-6.78	0.02	0.05	0.00
0	-7.19***	2.12	-11.34	-3.26	0.00	0.00
1	3.77	4.59	-5.92	12.51	0.42	50.00
2	-2.81	4.46	-11.32	6.09	0.53	45.00
3	22.67**	9.75	2.59	41.65	0.04	37.00
4	46.38***	14.58	19.71	75.70	0.00	33.00
5	47.06***	17.23	14.84	80.97	0.00	24.00
6	6.73	15.02	-16.71	43.15	0.65	4.00

Notes: *** p<0.01, ** p<0.05, * p<0.1
Total municipalities n = 267, control group n = 217

E Appendix - Use of pretrial detention

E.1 All crimes

Table E.1: Estimated ATT by Year Relative to Implementation

Year	ATT	S.	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-0.08**	0.03	-0.14	-0.02	0.01	95.00
-9	0.01	0.01	-0.01	0.03	0.49	0.00
-8	0.01	0.01	-0.02	0.03	0.65	0.00
-7	0	0.01	-0.02	0.02	0.96	0.00
-6	0	0.01	-0.02	0.02	0.88	0.00
-5	0	0.01	-0.02	0.02	0.78	0.00
-4	-0.01	0.01	-0.03	0.01	0.43	0.00
-3	0	0.01	-0.02	0.01	0.68	0.00
-2	0	0.01	-0.02	0.01	0.67	0.00
-1	-0.01	0.01	-0.02	0.01	0.44	0.00
0	0.01	0.01	-0.01	0.03	0.38	0.00
1	0.03	0.02	-0.01	0.06	0.13	95.00
2	-0.07***	0.03	-0.12	-0.02	0.01	83.00
3	-0.15***	0.04	-0.23	-0.07	0.00	59.00
4	-0.13**	0.05	-0.23	-0.02	0.02	46.00
5	-0.14*	0.08	-0.28	0.01	0.08	32.00
6	-0.44**	0.16	-0.73	-0.06	0.02	9.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 564, control group n = 469

Figure E.1: Factors (all crimes)

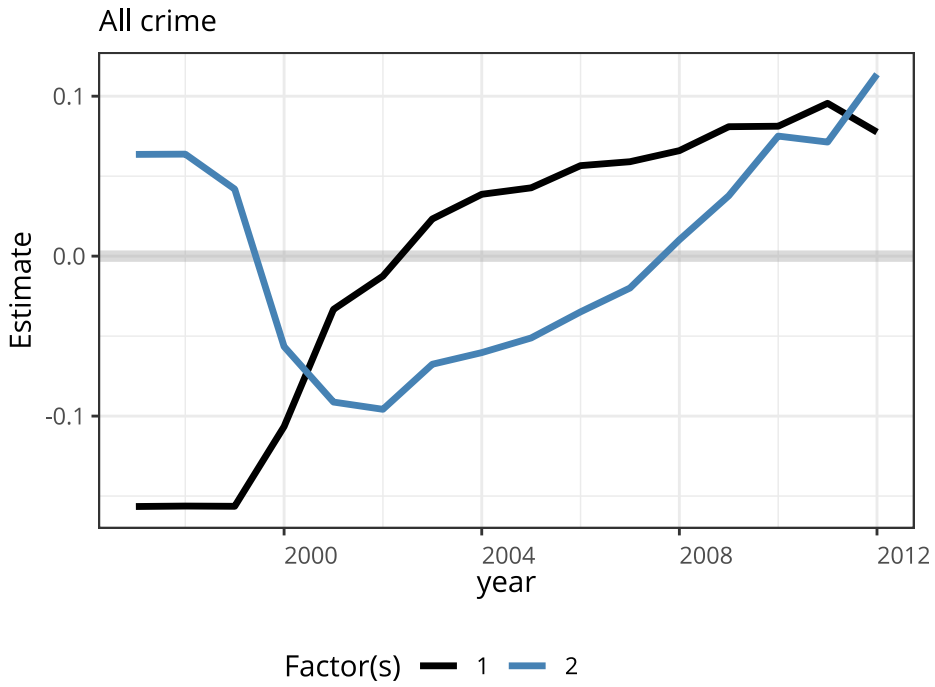


Figure E.2: Factor loadings (all crimes)

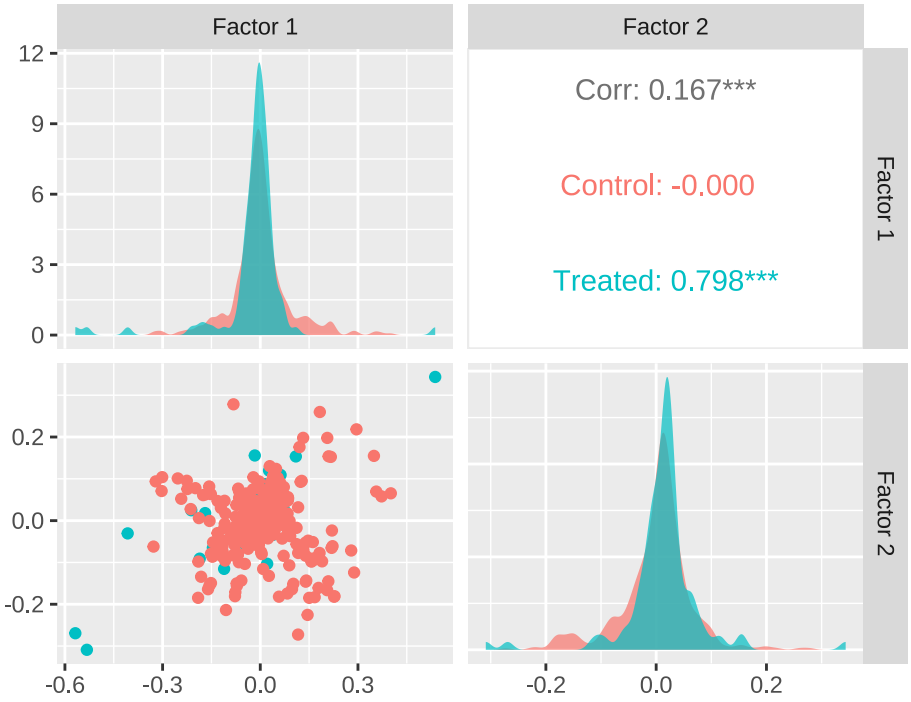
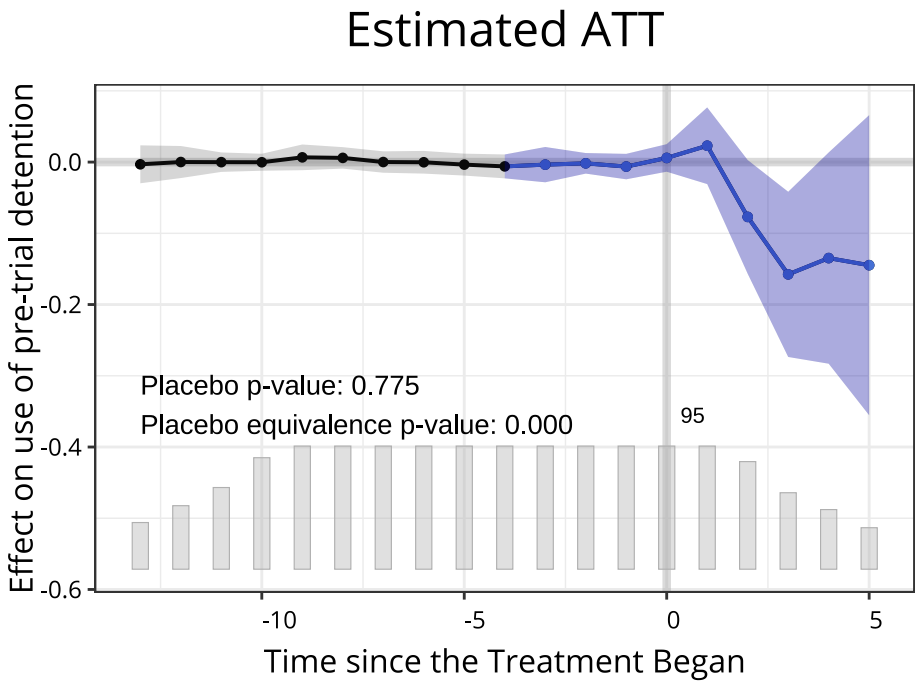


Figure E.3: Placebo and equivalence test for use of pretrial detention (all crimes)



E.2 Homicide

Table E.2: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-0.05	0.05	-0.15	0.06	0.32	77.00
-9	0.01	0.02	-0.04	0.05	0.70	0.00
-8	0.01	0.02	-0.03	0.05	0.58	0.00
-7	0.02	0.02	-0.02	0.07	0.29	0.00
-6	-0.03	0.02	-0.08	0.02	0.22	0.00
-5	-0.03	0.02	-0.08	0.02	0.16	0.00
-4	-0.01	0.02	-0.06	0.04	0.60	0.00
-3	-0.02	0.02	-0.06	0.03	0.44	0.00
-2	0.02	0.02	-0.03	0.07	0.40	0.00
-1	-0.03	0.02	-0.08	0.01	0.18	0.00
0	0.01	0.02	-0.04	0.05	0.67	0.00
1	-0.01	0.04	-0.08	0.06	0.77	77.00
2	-0.08*	0.05	-0.18	0.01	0.09	65.00
3	-0.14*	0.08	-0.29	0.01	0.07	47.00
4	0	0.10	-0.19	0.20	0.95	39.00
5	0.07	0.12	-0.17	0.31	0.56	31.00
6	-0.34*	0.18	-0.72	0.03	0.06	9.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 484, Control group n = 407

Figure E.4: Factors (Homicide)

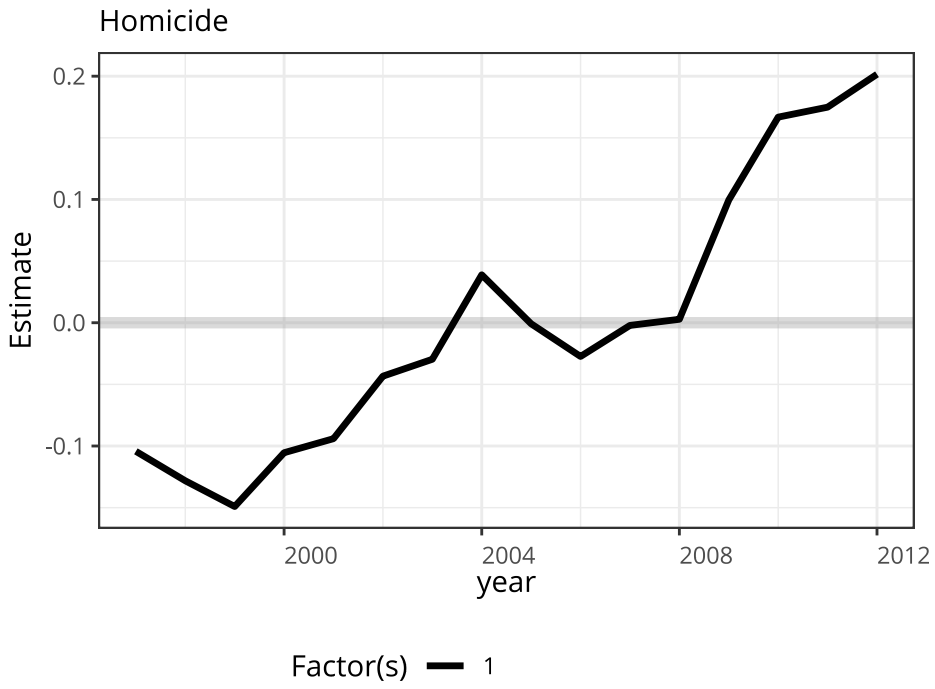


Figure E.5: Factor loadings (Homicide)

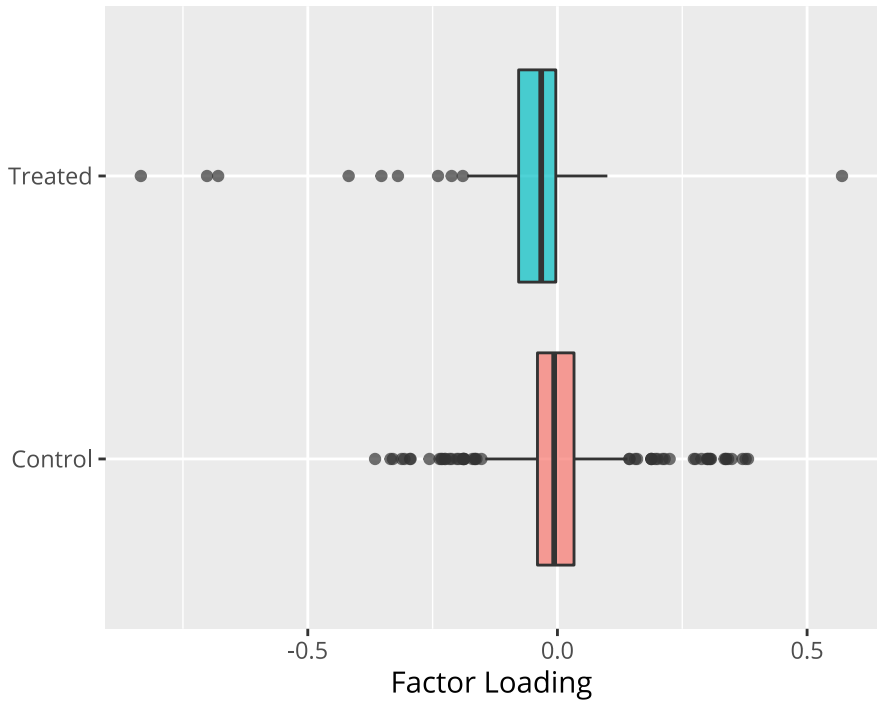
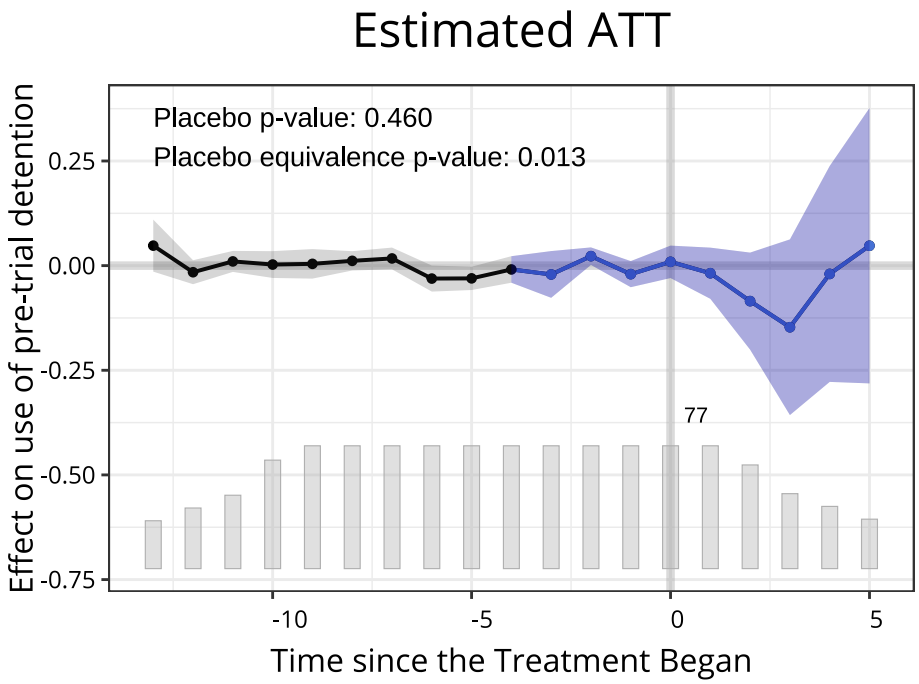


Figure E.6: Placebo and equivalence test for use of pretrial detention (Homicide)



E.3 Kidnapping

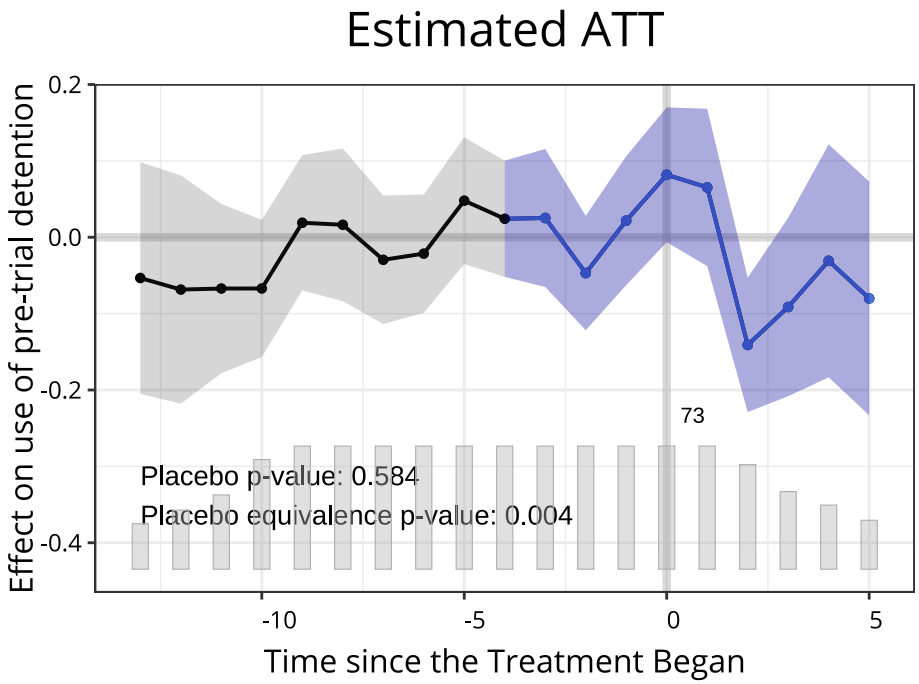
Table E.3: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-0.04	0.04	-0.13	0.05	0.34	73.00
-9	0.01	0.05	-0.08	0.11	0.75	0.00
-8	0.01	0.05	-0.08	0.10	0.83	0.00
-7	-0.04	0.05	-0.13	0.05	0.36	0.00
-6	-0.02	0.05	-0.12	0.07	0.57	0.00
-5	0.05	0.04	-0.04	0.13	0.29	0.00
-4	0.02	0.04	-0.06	0.11	0.63	0.00
-3	0.03	0.04	-0.06	0.11	0.60	0.00
-2	-0.04	0.04	-0.14	0.05	0.27	0.00
-1	0.03	0.05	-0.07	0.12	0.60	0.00
0	0.09*	0.05	-0.00	0.19	0.06	0.00
1	0.07	0.05	-0.03	0.18	0.17	73.00
2	-0.13**	0.06	-0.25	-0.01	0.04	62.00
3	-0.08	0.07	-0.22	0.05	0.25	46.00
4	-0.03	0.08	-0.18	0.14	0.81	38.00
5	-0.08	0.09	-0.25	0.11	0.42	29.00
6	-0.16	0.18	-0.50	0.17	0.33	8.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 433, control group n = 360

Figure E.7: Placebo and equivalence test for use of pretrial detention (all crimes)



E.4 Property crime

Table E.4: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-0.16***	0.02	-0.21	-0.12	0.00	85.00
-9	0	0.02	-0.04	0.04	0.99	0.00
-8	0	0.02	-0.03	0.04	0.80	0.00
-7	-0.01	0.02	-0.05	0.03	0.60	0.00
-6	-0.01	0.02	-0.05	0.03	0.74	0.00
-5	-0.02	0.02	-0.06	0.01	0.18	0.00
-4	-0.03*	0.02	-0.06	0.00	0.09	0.00
-3	0.03	0.02	-0.01	0.06	0.10	0.00
-2	-0.01	0.02	-0.05	0.02	0.35	0.00
-1	0.02	0.02	-0.02	0.05	0.32	0.00
0	0.03	0.02	-0.01	0.06	0.14	0.00
1	-0.06**	0.02	-0.11	-0.01	0.02	85.00
2	-0.18***	0.03	-0.24	-0.13	0.00	73.00
3	-0.25***	0.04	-0.32	-0.18	0.00	51.00
4	-0.18***	0.04	-0.26	-0.09	0.00	42.00
5	-0.18***	0.06	-0.29	-0.07	0.00	32.00
6	-0.33***	0.11	-0.56	-0.13	0.00	9.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 533, control group n = 448

Figure E.8: Factors (Property crime)

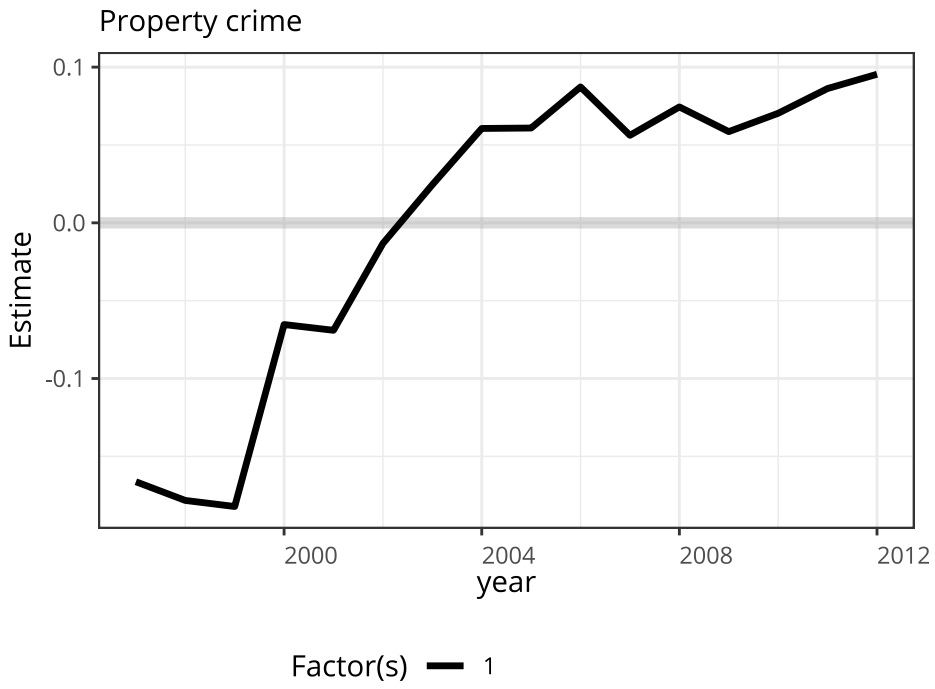


Figure E.9: Factor loadings (Property crime)

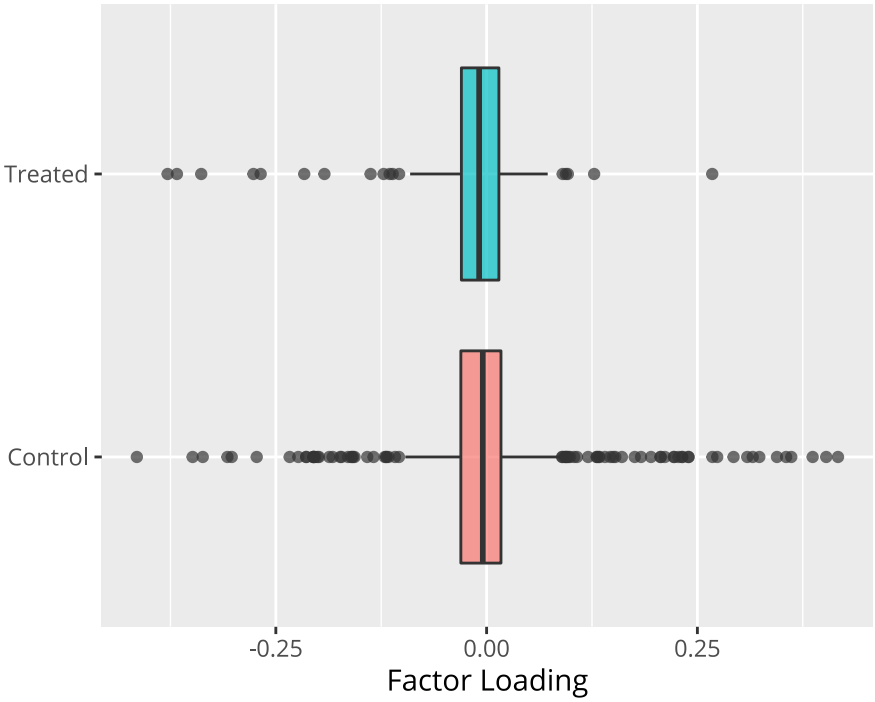
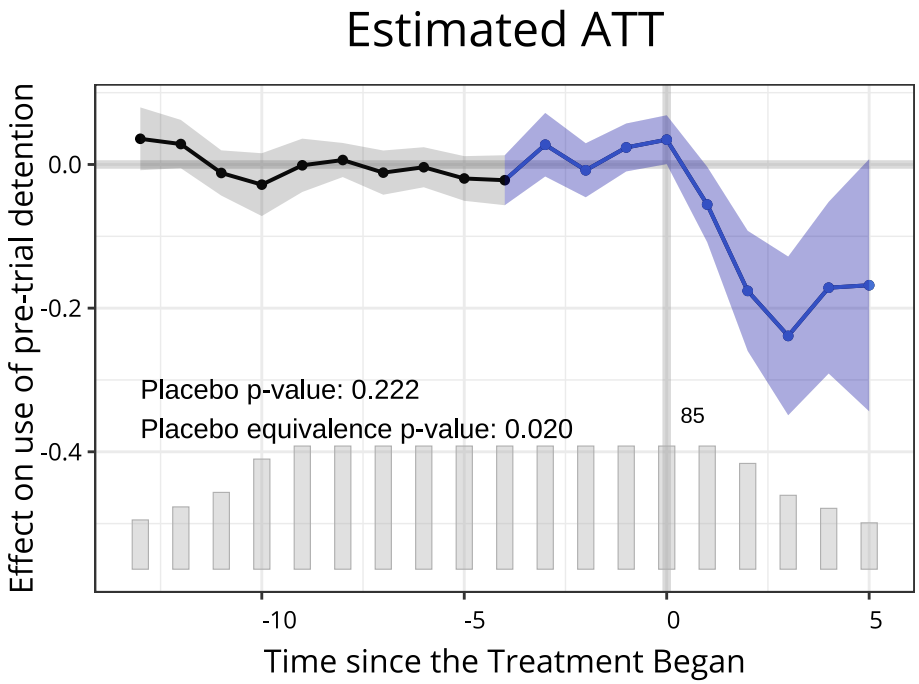


Figure E.10: Placebo and equivalence test for use of pretrial detention (Property crime)



E.5 Rape

Table E.5: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-0.07*	0.06	-0.21	0.01	0.07	76.00
-9	0.05*	0.03	-0.00	0.12	0.07	0.00
-8	-0.02	0.03	-0.07	0.05	0.82	0.00
-7	0	0.03	-0.06	0.07	0.82	0.00
-6	-0.02	0.03	-0.08	0.05	0.62	0.00
-5	-0.01	0.03	-0.08	0.04	0.50	0.00
-4	-0.03	0.03	-0.09	0.03	0.37	0.00
-3	0	0.03	-0.07	0.06	0.92	0.00
-2	-0.02	0.03	-0.09	0.03	0.40	0.00
-1	0	0.03	-0.08	0.05	0.64	0.00
0	0.01	0.03	-0.08	0.06	0.88	0.00
1	-0.01	0.05	-0.13	0.06	0.53	76.00
2	-0.08*	0.06	-0.22	0.02	0.09	64.00
3	-0.12**	0.07	-0.30	-0.02	0.02	47.00
4	-0.13*	0.10	-0.39	0.03	0.07	39.00
5	0.03	0.11	-0.23	0.19	0.85	31.00
6	-0.23*	0.15	-0.54	0.03	0.08	9.00

Notes: *** p<0.01, ** p<0.05, * p<0.1
Total municipalities n = 470, control group n = 394

Figure E.11: Factors (Rape)

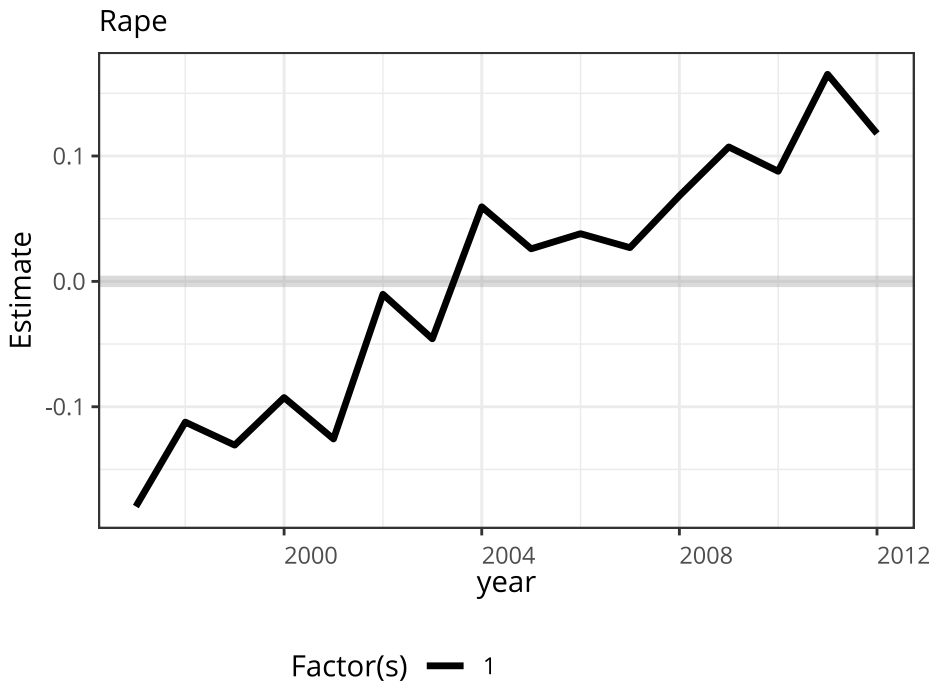


Figure E.12: Factor loadings (Rape)

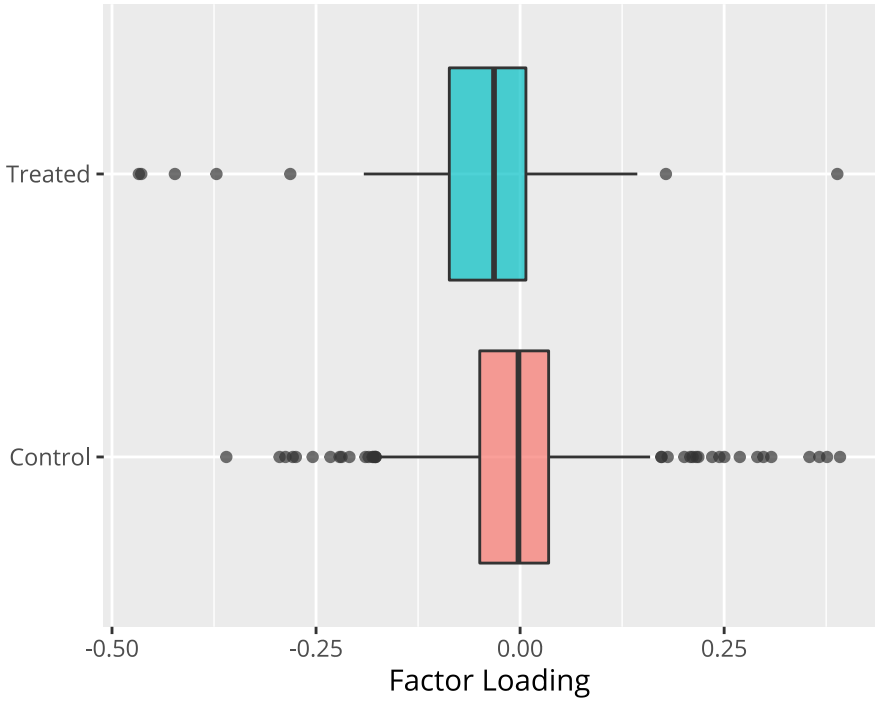
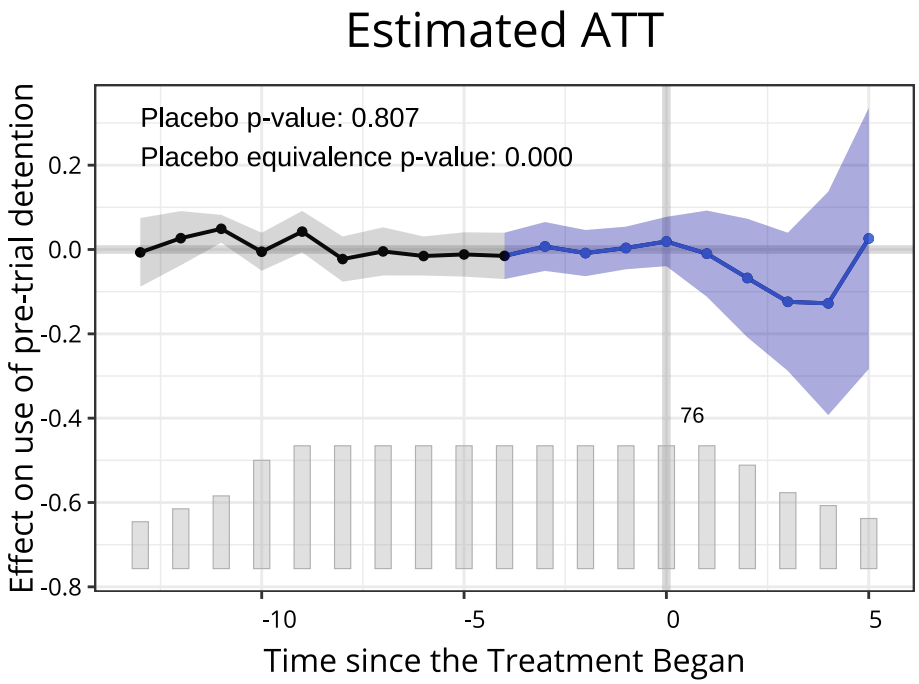


Figure E.13: Placebo and equivalence test for use of pretrial detention (Rape)



F Appendix - Celerity of punishment

F.1 All crimes

Table F.1: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-41.92	35.01	-112.84	22.19	0.26	76.00
-9	17.22	22.83	-43.50	47.08	0.84	0.00
-8	-21.47	26.98	-66.54	37.31	0.45	0.00
-7	17.68	23.97	-21.73	72.00	0.39	0.00
-6	8.24	25.24	-24.92	73.38	0.58	0.00
-5	-34.93	23.49	-61.92	33.11	0.36	0.00
-4	-0.89	19.98	-21.95	58.11	0.54	0.00
-3	-12.59	18.73	-34.55	37.62	0.86	0.00
-2	-14.64	19.50	-39.87	35.17	0.78	0.00
-1	-8.59	19.35	-37.69	38.57	0.96	0.00
0	47.29**	26.05	1.08	103.04	0.05	0.00
1	29.07	29.84	-37.39	84.99	0.32	76.00
2	-76.9*	43.54	-156.31	12.24	0.08	51.00
3	-88.14***	37.19	-164.81	-20.70	0.01	40.00
4	-83.15**	45.60	-176.29	-2.83	0.04	30.00
5	-44.15	62.56	-172.92	63.48	0.50	21.00
6	-121.66	124.36	-365.51	134.24	0.37	6.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 565, control group n = 489

Figure F.1: Factors (all crimes)

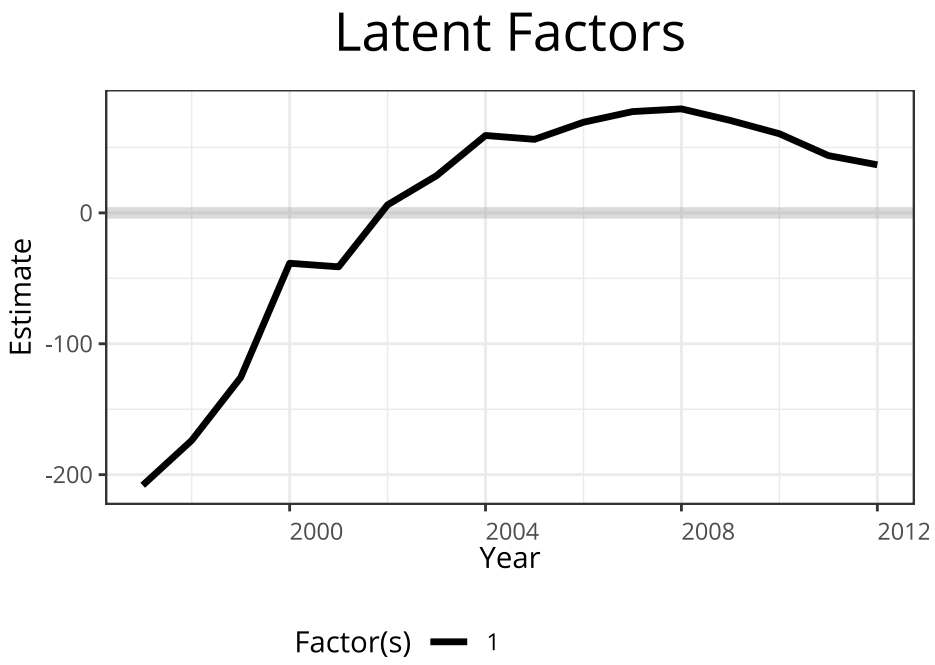


Figure F.2: Factor loadings (all crimes)

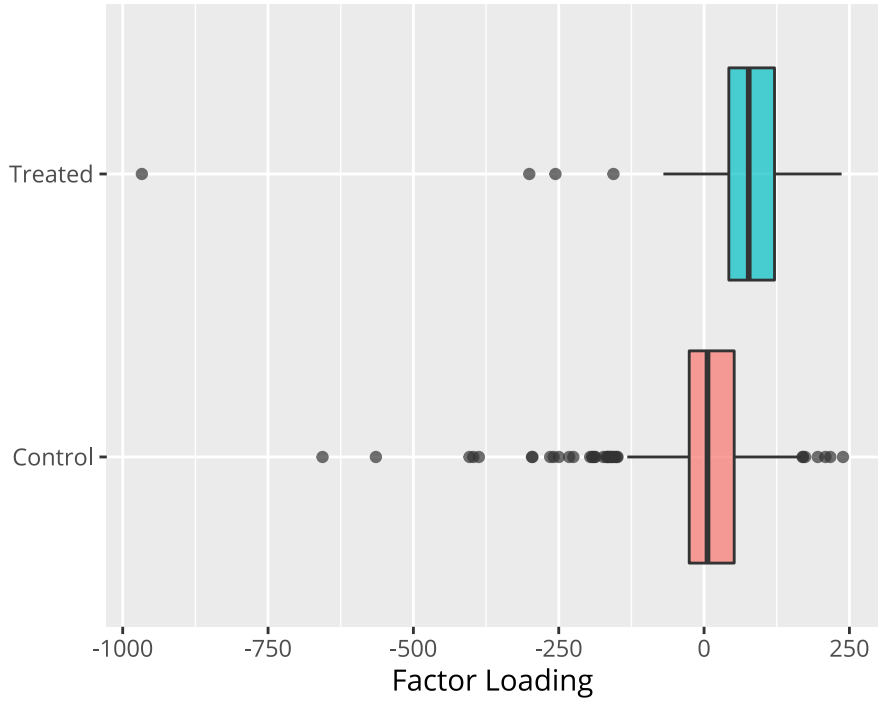
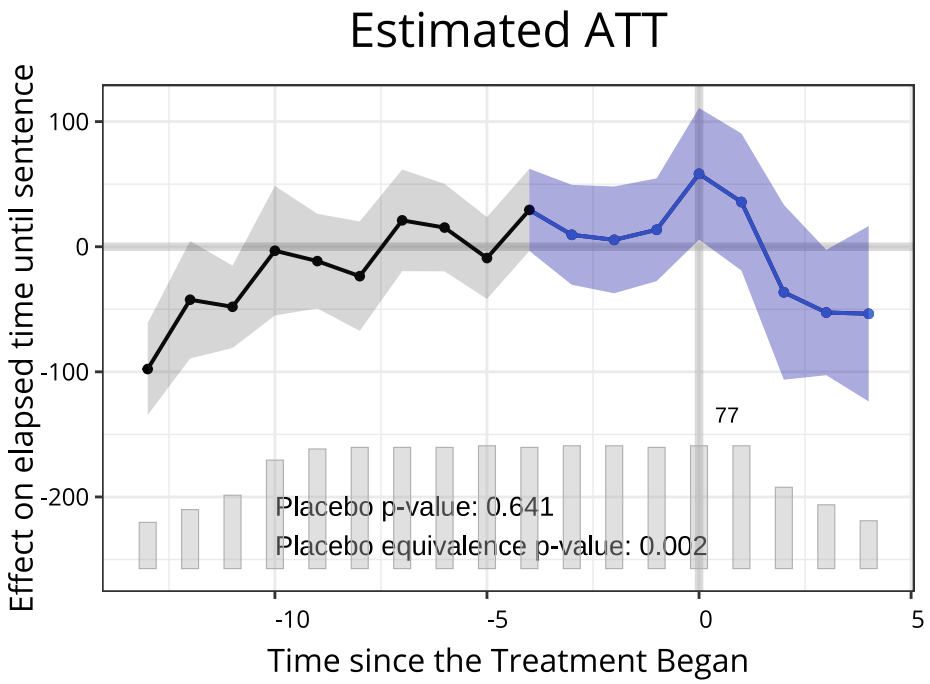


Figure F.3: Placebo and equivalence test for use of pretrial detention (all crimes)



F.2 Homicide

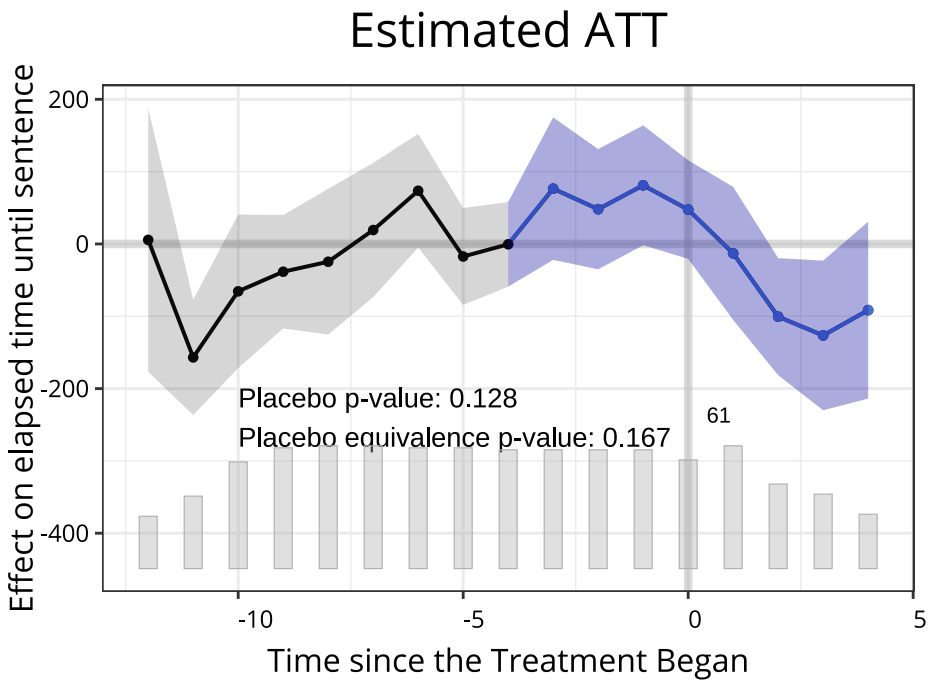
Table F.2: Estimated ATT by year relative to implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-74.94*	46.10	-168.59	11.32	0.10	60.00
-9	-52.17	41.52	-132.56	32.51	0.20	0.00
-8	-17.13	55.50	-115.90	101.37	0.75	0.00
-7	12.6	52.39	-81.88	125.24	0.80	0.00
-6	71.76*	41.81	-7.20	158.69	0.07	0.00
-5	-18.33	34.84	-86.79	48.13	0.58	0.00
-4	-9.75	29.41	-63.80	51.16	0.77	0.00
-3	40.81	34.79	-29.86	107.79	0.24	0.00
-2	48.52	40.97	-30.48	133.03	0.21	0.00
-1	84.6**	41.59	3.75	162.04	0.04	0.00
0	52.38	35.30	-19.15	119.80	0.15	0.00
1	-0.79	46.55	-93.83	88.45	0.97	60.00
2	-91.65**	42.34	-175.94	-12.61	0.02	41.00
3	-121.42**	52.10	-228.09	-24.56	0.02	36.00
4	-93.68	63.59	-223.03	25.54	0.13	26.00
5	-85.96	78.79	-243.55	57.06	0.28	17.00
6	-358.28**	134.58	-593.77	-46.79	0.02	5.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 501, control group n = 441

Figure F.4: Placebo and equivalence test for use of pretrial detention (Homicide)



F.3 Kidnapping

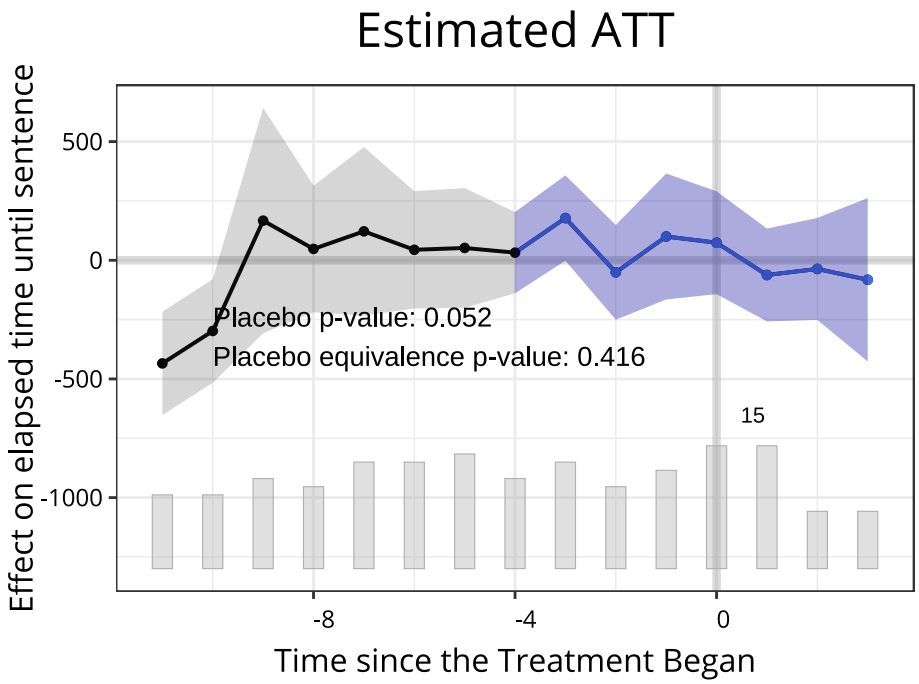
Table F.3: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-93.09	104.75	-312.51	92.25	0.37	13.00
-9	197.19	319.20	-498.00	747.30	0.63	0.00
-8	35.56	188.91	-296.84	487.37	0.81	0.00
-7	100.82	200.95	-246.41	535.05	0.77	0.00
-6	36.95	150.77	-207.80	377.99	0.80	0.00
-5	79.29	143.64	-179.31	356.41	0.54	0.00
-4	59.74	95.16	-129.03	250.48	0.42	0.00
-3	180.36*	100.92	-35.93	355.66	0.10	0.00
-2	-74.92	137.64	-312.58	240.99	0.59	0.00
-1	83.07	134.71	-192.00	333.95	0.53	0.00
0	-41.97	110.71	-243.79	187.40	0.77	0.00
1	-108.46	110.13	-317.90	108.38	0.32	13.00
2	-34.73	124.33	-300.01	178.55	0.78	6.00
3	-194.8	161.87	-516.42	113.76	0.25	6.00
4	-175.13	218.52	-650.56	228.65	0.49	3.00
5	330.45	246.53	-578.41	428.73	0.26	1.00
6	189.25*	146.43	-359.71	335.30	0.10	1.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 386, control group n = 373

Figure F.5: Placebo and equivalence test for use of pretrial detention (all crimes)



F.4 Property crime

Table F.4: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	-58.29**	28.87	-116.13	-7.10	0.02	68.00
-9	-13.55	17.53	-50.06	18.14	0.35	0.00
-8	17.82	23.98	-24.90	69.90	0.47	0.00
-7	-24.19	18.77	-58.10	13.73	0.24	0.00
-6	3.51	21.70	-29.80	53.43	0.78	0.00
-5	-23.86	18.79	-56.99	16.26	0.26	0.00
-4	5.52	31.24	-48.43	75.27	0.88	0.00
-3	-14.62	15.00	-39.57	19.12	0.41	0.00
-2	-12.31	21.92	-53.16	33.77	0.66	0.00
-1	3.38	15.95	-25.27	35.56	0.81	0.00
0	57.09***	17.63	23.45	93.73	0.00	0.00
1	21.14	22.01	-23.48	61.42	0.34	68.00
2	-111.14***	38.95	-190.69	-41.14	0.00	44.00
3	-99.69***	35.95	-175.79	-35.81	0.00	36.00
4	-93.83***	38.93	-172.80	-23.14	0.01	27.00
5	-67.23	56.32	-182.71	37.98	0.22	18.00
6	-151.28	115.67	-386.93	78.84	0.19	5.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 540, control group n = 472

Figure F.6: Factors (Property crime)

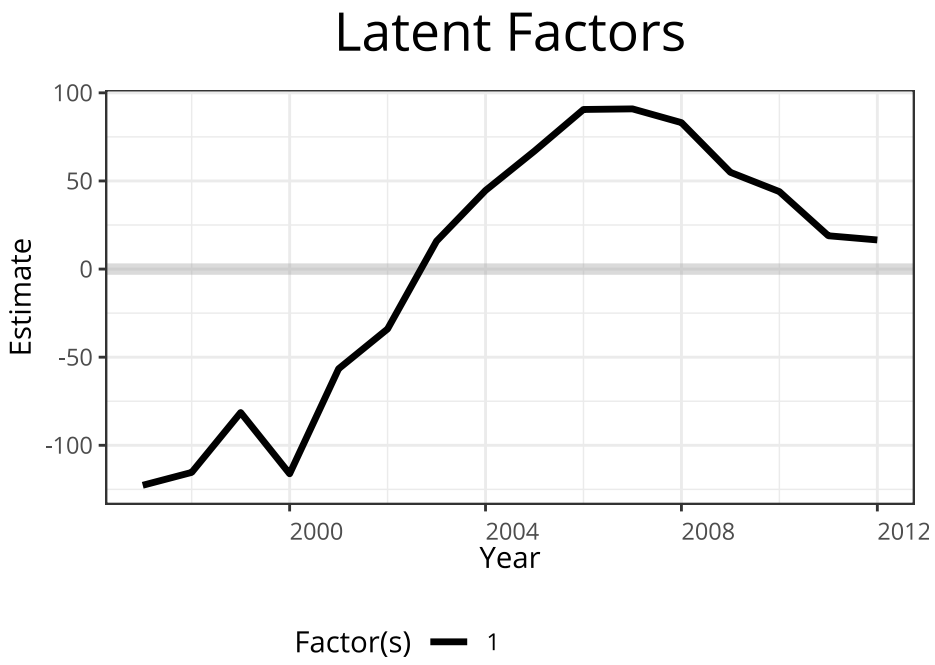


Figure F.7: Factor loadings (Property crime)

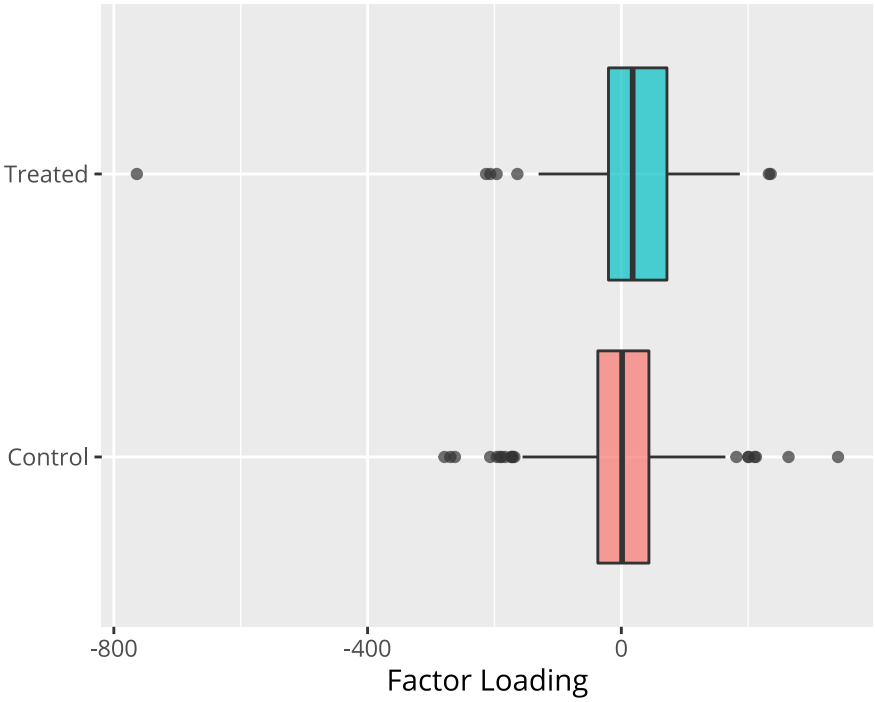
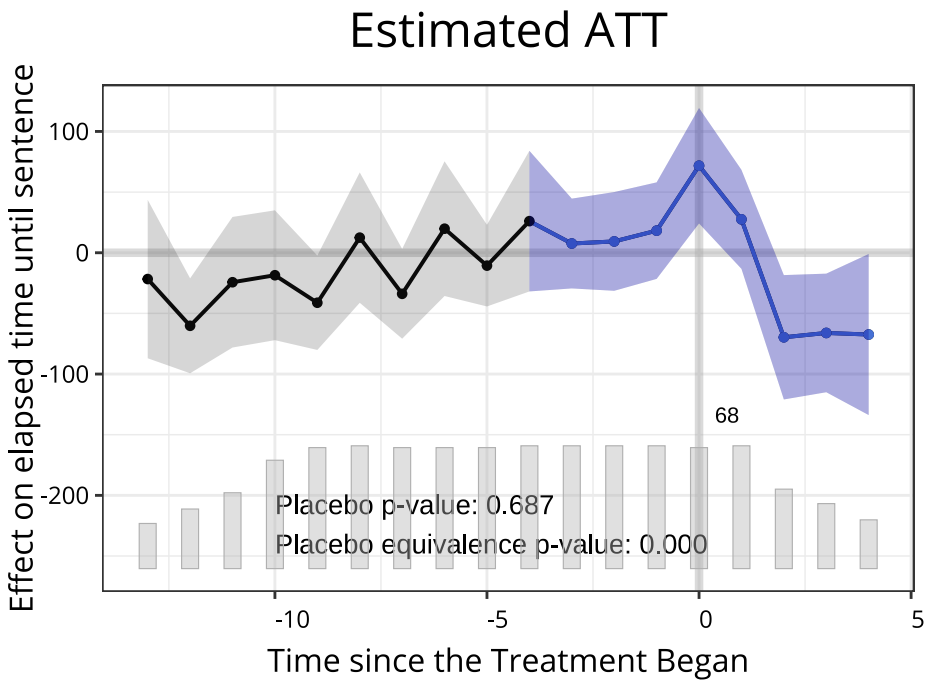


Figure F.8: Placebo and equivalence test for use of pretrial detention (Property crime)



F.5 Rape

Table F.5: Estimated ATT by Year Relative to Implementation

Year	ATT	SE	CI (lower)	CI (upper)	p-value	n (treated)
Cumulative	14.25	30.29	-44.54	74.71	0.61	50.00
-9	-92.9**	43.11	-174.79	-5.11	0.04	0.00
-8	-73.79	47.91	-157.58	30.42	0.14	0.00
-7	-47.52	38.64	-121.97	27.44	0.24	0.00
-6	72.88	56.61	-30.44	185.70	0.19	0.00
-5	-57.26**	28.99	-116.25	-1.50	0.04	0.00
-4	65.95*	39.12	-11.25	145.34	0.09	0.00
-3	55.71	51.59	-37.50	162.74	0.28	0.00
-2	99.07**	47.46	8.94	191.83	0.03	0.00
-1	8.17	35.68	-60.78	78.22	0.78	0.00
0	105.39***	36.82	35.07	180.28	0.00	0.00
1	134.62***	36.57	65.39	209.15	0.00	50.00
2	-50.89	42.15	-133.75	30.56	0.21	34.00
3	-41.44	33.86	-110.06	23.25	0.22	27.00
4	-66.92	69.20	-208.30	64.19	0.33	14.00
5	-61.23	87.59	-227.41	111.99	0.49	8.00
6	-266.03***	134.99	-466.47	-62.72	0.00	2.00

Notes: *** p<0.01, ** p<0.05, * p<0.1

Total municipalities n = 465, control group n = 415

Figure F.9: Placebo and equivalence test for use of pretrial detention (Rape)