How Mexican Judicial Reform May Have Fueled Crime: Arrest Trends and Trust Erosion

Authors:

Catalina Amuedo-Dorantes[a]

Marilyn Ibarra-Caton[b]

January 2024

Working Paper

a University of California Merced

b Ave Maria University

The Center for Growth and Opportunity at Utah State University

Abstract

We examine the impact of Mexico's state-led judicial reform from 2000 to 2017 on crime. Using death certificates and administrative data, we construct a municipal-level panel. Employing heterogeneous treatment robust estimators, we find that the reforms were accompanied by a 25 percent increase in homicide rates—a type of violent crime less likely to suffer from underreporting compared to others. We also examine arrest rates, which amid the increase in violence, declined by more than 50 percent, likely contributing to the public perception of criminal conduct going unpunished. This sentiment may have empowered organized crime and curtailed citizens' willingness to cooperate with the authorities due to lack of trust in the criminal justice system and, in states with organized crime, fear of retaliation. An examination of crime reporting using survey data lends support to this hypothesis, exposing remaining challenges of the Mexican judicial system reform in its fight against crime.

Keywords: Judicial reform, Crime, Arrests, Crime reporting, Mexico.

JEL Codes: O12, O54, P48.

"It (the reform) is in its infancy,' said Jesús I. Moreno de Leija, a lawyer in Mexico City, referring to the new system. 'On the one hand, criminals entered and exited the system just as easily, the number of offenders multiplied, some prisons reduced their inmate population but multiplied their damage to society. In other words, instead of improving, the system became more complicated.'"

—Semple (2020)

1 Introduction

Widespread corruption, impunity, and lack of transparency have historically undermined the administration of justice in Mexico. In response, from 2000 to 2017, all 32 Mexican states undertook a judicial reform, incorporating oral and public trials, the presumption of innocence, defined timelines for various procedural stages, and alternative trial mechanisms. The various elements were intended to raise the standards for an arrest and increase confidence in the judicial system. However, to date, the effectiveness of these changes in reducing crime remains an open question.

In this paper, we address this inquiry with an analysis of how the reform, by now fully implemented, has impacted crime. On the one hand, a more effective and transparent judicial process could lead to reduced crime, strengthening trust in the judicial system and cooperation with the authorities. On the other hand, the higher arrest standards could limit detentions and create the perception of criminal conduct going unpunished, resulting in reduced crime reporting and an increase in crime (Becker, 1968).

The push to strengthen the Mexican criminal justice system gained momentum in 2008, under President Felipe Calderón, when the Mexican Congress approved a national judicial reform. All 32 Mexican states were mandated to implement this reform by June 2016. The reform was driven by a mix of legal and functional critiques of the existing judicial system, including impunity and inefficiency. Surveys had consistently shown that most crimes in Mexico were unreported due to a general distrust of the authorities (Perez, 2007; Shirk, 2011). With 90 percent of cases prosecuted at the state level and with less than a 13 percent resolution rate (Zepeda, 2012), the need for reform was clear. Although many Mexican states moved forward with their own reform and crime rates initially decreased, reaching a record low in 2007, violence grew dramatically, peaking in 2011 (Heinle, Molzahn, and Shirk, 2015).

Building on this backdrop, our study aims to examine the influence of the judicial reform's implementation on crime in Mexico and to identify likely reasons for any found impact. From a policy perspective, we are interested in gaining a better understanding of how Mexico's conversion to an adversarial criminal law system—a shift intended to increase the cost of committing a crime—might have molded crime rates. These insights are crucial, as they have significant implications for outmigration flows and, more broadly, the social, economic, and political stability of the region.

Anecdotal evidence on the effect of the judicial reform on crime is mixed. A survey of judicial sector officials found that participants were evenly divided on whether the reform reduced crime (Mendoza and Aguilar, 2012). Government and judicial officials critical of the reform asserted that it did not reduce crime but rather incentivized it through a reduced prison population (Gallegos, 2017). These views provoked a backlash from the National Association of Governors in Mexico

and several state legislatures, who advocated for reversing key aspects of the new judicial reform and returning to the previous system—the traditional inquisitorial system (Dávila, 2018). The controversy sparked further research on the topic.

Three papers are most related to our research question. The first is by Blanco (2016), who uses victimization surveys and a difference-in-differences (DiD) approach to examine how the judicial reform impacted victimization and perceptions of safety in three Mexican cities that implemented the reform in 2007 and 2008. Blanco's findings indicate reductions in both victimization rates and perceptions of safety, alongside a decline in trust toward the police. The second study is by Huebert (2019). Using municipal data on homicides rates spanning from 1990 to 2015,[1] she explores the reform's impact by employing an error correction model. While only 9 of the 32 states had implemented the reform during her study period, she finds that although the reform lowered homicide rates in states without drug cartels, it was accompanied by very large increases in violence in the remaining states.

Last, building on Huebert's analysis and focusing on a similar period (i.e., 1997–2012), Cepeda-Francese and Ramírez-Álvarez (2023) examine the judicial reform's impact on various types of crimes, including homicide rates. Using a synthetic control method approach, they arrive at a similar conclusion: the reform was followed by a substantial increase in homicide rates. This trend was especially in municipalities where cartels were already present before the reform was implemented. They attribute the finding to a diminished capacity to effectively prosecute homicides, as captured by a reduced ratio of indictments for every murder.

Similar to these analyses, we examine the judicial reform's impact on crime, with a specific focus on homicide rates, which are less likely to suffer from underreporting than other types of crimes (Gottfredson and Hindelang, 1979; Skogan, 1984; Soares 2004; Van Dijk, 2008). We add to prior analyses in several ways. First, we focus on an 18-year period from 2000 to 2017. This more recent timeframe is critical as it encompasses the reform's complete implementation, which did not occur until the end of 2016, enabling us to fully capture its impact. Additionally, it incorporates violent crime spikes of the Mexican drug war occurring after 2011. If the goal is to assess the reform's effectiveness in curtailing serious crimes, it is crucial to include the period during which the reform was fully implemented and to account for the years when homicide rates spiked. Second, we focus on implementation—as opposed to enactment—as this is key in capturing the reform's impact.

Third, like some of our predecessors, we exploit the staggered implementation of the reform across Mexican municipalities for identification purposes. However, we also pay close attention to the well-documented biases of two-way fixed effect (TWFE) estimates in the presence of such treatments, making use of heterogeneous robust estimates for inference purposes. In addition, we conduct two checks aimed to gauge the reliability of our results. First, we derive DiD propensity matching estimates, which ensures our results are similar when we closely match treated versus not-yet-treated municipalities based on their traits before the implementation of any reform. Second, we conduct event studies to confirm that homicide rates were trending parallelly before the reform's implementation in treated versus not-yet-treated municipalities. This enables us to test the validity of the control group and gauge the reform's short- and long-term impacts on homicide trends.

1 Due to data constraints, the main analyses focus on the 1994–2012 period.

Finally, consistent with previous studies, we explore potential reasons behind the impacts. We focus on the role played by two factors emphasized by the media (Semple, 2020): changes in arrest rates following the reform amid spikes in homicide rates, and in turn how the reform may have altered public trust in the judicial criminal system, as captured by crime reporting.

Like Huebert (2019) and Cepeda-Francese and Ramírez-Álvarez (2023), we find that the judicial reform was accompanied by a significant increase in homicide rates, although the upsurge in homicide rates occurred across all states and not solely in those with cartels in prior years. To gain a better understanding of the factors responsible for the reform's failure to curtail violent crime, we first examine how arrests changed after the reform was fully implemented. Even though arrests were expected to decrease with the reform's higher bar for detention, we would expect the drop to be tamed amid rapidly rising homicide rates if the criminal justice system were operating effectively. However, we find that the arrests declined throughout the country by 50 percent amid homicide rate increases of over 25 percent. This mismatch between violent crime and arrests likely contributed to the public perception of criminal conduct going unpunished. Such a perception may have emboldened organized crime and curtailed the cooperation of private citizens with the criminal justice system due to lack of trust and, in states with organized crime, fear of retaliation.

To assess if that was the case, we next examine crime reporting using data from two victimization surveys—the Mexican Family Life Survey (MxFLS) and the National Institute of Statistics and Geography's (INEGI) National Survey on Victimization and Perception of Public Safety Survey (ENVIPE). The analysis confirms the above-mentioned hypothesis. Crime reporting rates dropped following the reform, especially in states with organized crime, underscoring the failure by public authorities to gain legitimacy and trust and in turn their ability to deter criminal conduct.

Our paper contributes to the literature on the impact of judicial reform on crime rates, particularly violent crimes, as in the case of homicides. Historically, the Mexican criminal justice system has been characterized by extraordinary discretion over enforcement. A reform increasing the arrest standards and the transparency of the judicial process may curtail detentions and have unintended criminal consequences (Becker, 1968; Levitt, 1995). As shown by Atkins and Rubin (2003), three 1960s Supreme Court rulings in the United States—*Mapp v. Ohio* (establishing the exclusionary rule), *Gideon v. Wainwright* (asserting the right to counsel during trial), and *Miranda v. Arizona* (mandating the right to legal representation upon arrest)—suggests that a judicial reform increasing the cost of arrests and investigations may lead to a rise in crime. This is evidenced by their empirical analysis, which shows a 30 percent increase in assaults following these rulings.[2] Similarly, in addition to the three papers on Mexico noted earlier, several studies also focus on other Latin American countries. These studies examine the impact of alike judicial reform, arriving to comparable conclusions.[3]

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2 Similarly, Dusek (2015) finds that a reform of the Czech judicial system that shortened the length and complexity of criminal procedures for less serious crimes impacts the behavior of offenders and law enforcement officials.

3 For instance, Zorro, Acosta, and Mejía (2020) use event study and DiD approaches to analyze municipal crime and arrest data from Colombia, covering the 2003–2008 period, to explore the impact of a similar judicial reform. They find that the reform reduced apprehension rates due to stricter arrest rules, boosting overall crime rates by 22 percent. Focusing on Peru, Hernandez (2019) uses district-level crime data for 2010–2015 to ascertain the impact of another similar judicial reform on various types of crimes, including property crimes. Using a DiD approach with matching estimators, he finds mixed results that vary with the type of crime and the reform's implementation timing. Finally, for Chile, Mohor and Covarrubias (2007) find no significant impact of the country's judicial reform on crime rates, but the perception of security did increase.

Our study also contributes to the literature on crime reporting. A particularly relevant study in this literature is by Blanco (2016), who studies the impact of the Mexican judicial reform on 11 cities in 2005, 2008, and 2009, three of which had enacted the reform at the time. She finds mixed results, with crime reporting rising in some cities (Chihuahua) and decreasing in others (Juárez). Focusing on Chile, Azócar and Undurraga (2005) find no significant impact of the judicial reform on crime reporting and on the perception of insecurity (i.e., fear).

Last, our analysis informs on the triggers of violent crime. One explanation for the spike in homicides provided by the literature is the political shift to combat drug trafficking led by the National Action Party, otherwise known as Partido Acción Nacional, or PAN (Dell, 2015). Another explanation, provided by Castillo, Mejia, and Restrepo (2020), points to the scarcity in the illegal drug market resulting from cocaine seizures in Colombia. In this context, our study examines how the implementation of Mexico's judicial reform may have shaped crime rates, identifying potential explanations for its impact. Understanding the reform's effectiveness on crime is crucial given its numerous implications, ranging from the impact of violence on migration flows (e.g., Orozco-Aleman and Gonzalez-Lozano, 2018)[4] to its broader effects on social, economic, and political stability in the Latin American region (e.g., Jarrillo et al., 2016; Trelles and Carreras, 2012; Poveda and Pardo Martínez, 2023).

2 Background on the Judicial Reform

An important push to strengthen the Mexican judicial system occurred in June 2008, when the Mexican Congress under President Felipe Calderón approved a national reform to transform the country's judicial system. Appendix Table A2 summarizes the main changes introduced by the judicial reform. Shirk (2011) provides an elaborate discussion of four important components of the reform, which included: (1) "changes to criminal procedure through the introduction of new oral, adversarial procedures, alternative sentencing, and alternative dispute resolution (ADR) mechanisms" (i.e., plea bargaining); (2) "a greater emphasis on the rights of the accused (i.e., the presumption of innocence, due process, and an adequate legal defense)"; (3) "modifications to police agencies and their role in criminal investigations"; and (4) "tougher measures for combating organized crime." Together, these elements aimed to give more power to the government in combating crime and to improve the judicial system's efficiency, which was "considered a search to achieve the best performance with the lowest possible costs" (Juárez, 2019).

The 32 Mexican states had until June 2016 to implement the reform. The approval of a state-level judicial reform varied significantly across the 32 Mexican states, and the implementation occurred at differing speeds, as shown in Appendix Table A1. The first four states implementing the reform were Nuevo León (2004), Chihuahua (2007), Oaxaca (2007), and Morelos (2008).[5] By 2014, three of these four states had fully implemented the reform across all municipalities. Furthermore, as shown in the last column of Appendix Table A1, there were significant gaps between enactment and implementation at the state and municipality levels. The average lag between those two events was 1.91 years. Ten states—Colima, Chihuahua, Durango, Guerrero, Jalisco, Morelos, Nayarit, Nuevo León, Puebla, and Tabasco—began their implementation within a year of their approval. However, other states experienced long lags between enactment and implementation. For example,

4 These authors find evidence of a positive relationship between the Mexican drug war's violence and migration.

5 These states adopted a judicial reform before the 2008 federal reform's approval.

Hidalgo and Veracruz approved their state reform before the national reform in 2008; however, concrete steps toward their implementation were only taken 8 and 5 years later, respectively. These lags underscore the relevance of extending the time span of the analysis to a more recent period, enabling us to capture any reform impacts.

According to Blanco (2016), the success of the judicial reform can be evaluated based on three dimensions. The first dimension concerns its impact on the efficiency and transparency of the criminal justice system. These aspects may be measured through various parameters, including the speed of processed cases, transparency in oral cases, or by the number of human rights violations. However, the lack of data on these aspects restricts our ability to assess this dimension. The second dimension involves examining its impact on crime, which we address by gauging the response of homicide rates since they are less likely to suffer from underreporting biases. The third dimension is evaluating how the reform impacts public trust in the criminal justice system. Enhanced trust can lead to more reporting of crimes and more arrests, and vice versa. By using administrative data on arrests along with crime reporting survey data, we examine this dimension to identify potential drivers for homicide rates' response to the reform.

3 Conceptual Framework

Our main goal is to assess if Mexico's judicial reform impacted violent crime, and if so, how and why. In that regard, our analysis is related to work by Fajnzylber, Lederman, and Loayza (2002), who argue that a more efficient judicial system should increase the probability of apprehension, the expected punishment for being involved in criminal activities, and thereby the cost of committing a crime. The idea behind their model is that a more efficient judicial system should lower crime rates by reducing the incentive to commit a crime—a crime-deterrence effect that had been theoretically put forward by Becker (1968) and tested empirically by Levitt (1995).

According to Becker (1968), rational criminals use cost-benefit analysis to determine whether to engage in criminal activity, where the cost of committing a crime rises with the efficiency of the judicial system. Consistent with the crime-deterrence effect, Lochner (2007) argues that those individuals with a higher perceived probability of arrest are less likely to commit a crime. Similarly, Blanco (2016) notes how an effective reform of the Mexican judicial system should increase trust in authorities and crime reporting, increasing the probability of apprehension and deterring crime. In sum, a potential causal path for the hypothesized impacts on crime of the judicial reform is as follows:

As noted by Huebert (2019), a critical step in the above chain is increased collaboration or cooperation with authorities—an expression of increased legitimacy and trust when a due process has been adopted. Yet, the reform overlapped with the declaration of the Mexican drug war by President Calderón, leading to the deployment of military forces to fight drug-trafficking organizations, the arrests of the most-wanted drug lords, and the intercepting of their shipments.

Drug-related violence and deaths to organized crime quickly spiked, especially along the US-Mexico border, for example, Ciudad Juarez, Tijuana, and Matamoros, and in areas where cartels were seeking to dominate routes.

Figure 1 documents the drastic increase in homicides, which only weakened as the country transitioned to the presidency of Peña Nieto, who tried to deescalate the conflict by focusing on lowering criminal violence rates, as opposed to drug-trafficking lords and organizations. Overall, Figure 1 seems to support the notion that in the presence of powerful organized crime, citizens may have been reluctant to collaborate with the state (Trelles and Carreras, 2012). This lack of collaboration may increase the difficulty of obtaining pretrial detention, even for homicides, potentially fueling crime (Chalfin and McCrary, 2017).

In what follows, we examine how the reform impacted homicide rates, allowing for the distinction between states with a documented presence of one or more cartels to gauge the extent to which the judicial reform's effectiveness might have been limited by the presence of organized crime. In addition, we investigate potential factors at play, focusing closely the reform's impact on arrests and, especially, crime reporting—an essential element in fighting crime.

4 Methodology

To learn about the effect of the reform in combating crime, we start by estimating the following benchmark model for homicide rates, which are less likely to go unreported:

$$(1) \quad y_{mt} = \alpha + \beta Reform_{mt} + \gamma_m + \delta_t + \varepsilon_{mt}$$

where is the rate of homicides per capita in each municipality m and year t. We use the inverse hyperbolic sine function to transform the dependent variable to allow for zero homicides. The vector is a dummy indicative of when the reform was implemented in each municipality. Equation (1) also includes municipality and year fixed effects to account for time-invariant geographic and temporal traits correlated to homicide rates. In more complete model specifications, we add municipality and state-level time-varying controls, including municipal data on personnel remunerations, information on the presence of a cartel in the municipality in 2006, the state's unemployment rate, information on federal contributions to each state for security purposes, and data on the share of votes for PAN senatorial candidates. We use municipalities' population size to weigh our regression estimates. Standard errors are clustered at the municipality level.

We are interested in the coefficient, which captures the reform's impact on homicide rates by exploiting the geographic and temporal variation resulting from its staggered implementation. As discussed in the conceptual framework, it is unclear what to expect. If the reform successfully increased the legitimacy of the criminal justice system, we would expect a reduction in homicide rates resulting from improved community collaboration with the authorities (Sunshine and Tyler, 2003; Huebert, 2019) and from lower criminal involvement by offenders—either because they have already been captured and sentenced (i.e., via the so-called incapacitation effect) or due to an expected increase in the certainty, severity, and celerity of punishment (i.e., deterrence effect) (Cepeda-Francese and Ramírez-Álvarez, 2023; Dalla Pellegrina, 2008; Lee and McCrary, 2017; Vollard, 2013). Yet, we could observe an increase in homicide rates if the improved due process

protections implemented by the reform resulted in a lower likelihood of pretrial detention (Chalfin and McCrary, 2017), if there was a lack of citizen cooperation with the police due to fear of cartel retaliation (Baek, Han, and Gordon, 2022), or if individuals believed the reform proved ineffective as arrests declined amid increased crime rates.

The validity of the reform impact, as captured by , relies on various identification assumptions. First, the TWFE estimates may be biased due to the reform's staggered rollout across Mexican municipalities. To address this concern, we first conduct Goodman-Bacon (2021) decompositions as a diagnostic check. We then estimate the model using the heterogeneous treatment robust estimator proposed by Gardner (2022).

Next, we confirm the robustness of the results to using an alternative methodology that matches treated and not-yet-treated municipalities based on their prereform characteristics before the estimation. In addition, we gauge the validity of the parallel trends assumption, which would require homicide rates to have trended similarly across municipalities that adopted the reform and those that had not, in the absence of the reform itself. While we cannot observe the true counterfactual, we use the methodology proposed by Borusyak, Jaravel, and Spiess (2021) to perform an event study assessing if homicide rates were at least trending differently across early versus later adopters before the reform implementation itself.

Finally, we check the assumption of policy exogeneity. While no policy is ever random, we conduct two checks aimed at assessing if the policy adoption was the byproduct of increased homicide rates or unobserved heterogeneity. First, we show that homicide rates were not a key driver in the adoption timing of the reform. Subsequently, we assess if the reform implementation was meaningfully correlated to municipality- and state-level traits, including the municipality size or personnel renumerations, as well as to the state's level of federal support, its unemployment rate, political inclination, or the presence of a cartel. Last, in our most complete model specifications, we include a series of municipal- and state-level time-varying controls that may be considered endogenous, yet potentially relevant, in shaping homicide rates with the goal of assessing the robustness of our findings to including those controls.

5 Data Sources

5.1 Crime Data

To study the impact of the judicial reform on crime, we examine data from INEGI, which publishes death certificate data that contain information on individuals' cause of death.[6] Our sample spans from 2000 to 2017, covering 2,095 municipalities in all 32 Mexican states. It includes municipalities with and without homicides.

5.2 Judicial Reform Implementation

Identifying the reform's implementation timing is complex, and few concrete reform indicators are available to help measure its impact. While states technically had the 2016 federal mandate

6 An alternative data set is from the Mexican Secretary of Government, which publishes municipal-level administrative data on various types of crimes reported by victims. A state-level comparison of this data set against the INEGI crime data set suggests serious crime underreporting in the former, which can lead to biased estimates of the effectiveness of the judicial reform in combating crime. To minimize this concern, we solely rely on the INEGI's death certificate data for homicides.

deadline, their starting point and progress toward implementation diverged widely from one state to another over time. Appendix Table A1 provides a quick glance of the variation of the state-level reform approval and the initial implementation of oral adversarial trial reform at the state level (SETEC, 2015). This change is important and acts as our starting point to measure the implementation's timing.[7]

The table shows significant lags between reform enactment and implementation dates, likely indicative of pending procedural changes at the time of the enactment. We coded implementation dates at the state level and, when feasible, at the municipal level. First, we fine-tuned the implementation dates based on various sources describing the specific changes implemented at the state and municipal levels, including any roadblocks that slowed down or contributed to the reversal of implementation efforts (Ribando, 2013; Ingram and Shirk, 2010; Shirk, 2011; Ingram, 2013; Mendoza and Sanchez, 2012; Novoa, 2020; SETEC, 2015; Torres, 2011; Zepeda, 2012). We then adjusted the dates based on rollouts within each state. For example, in the state of Baja California, the reform was first implemented in Mexicali (2010), then Ensenada (2012), and finally, by 2013, in Tijuana, Tecate, and Playas de Rosarito. Second, we adjusted dates to distinguish the implementation rollouts for various types of crimes. For instance, early on, Nuevo León only made changes that pertained to minor offenses, delaying the implementation of changes to homicides to years later.[8]

Third, in the spirit of Besley and Burgess (2004), we allowed for up to a one-year lag in the implementation process. For example, the state of Durango adopted the reform in the capital city of Durango in December 2009, and we coded the implementation as occurring in 2010.[9] As a general rule, when the reform was adopted before July, we coded the implementation as taking place in the same year; otherwise, it was coded as occurring the year after. Finally, we verified the accuracy of the implementation dates using INEGI's administrative data on criminal proceedings, which informs about the judicial system in operation in the municipality where an arrest was processed after 2014. The data allowed us to confirm which municipalities had adopted the new judicial system in states where the reform was implemented after 2013. As shown in Appendix Table A3, municipal coverage expanded over the period of our study, reaching full coverage by 2017, which is when our sample ends due to the lack in policy variation thereafter.

5.3 Demographic, Economic, and Political Data

We use information on population statistics from the National Population Council (CONAPO) to compute homicide rates per 100,000 inhabitants, using the 2000 population level as an index. In addition, we gather data on various municipal- and state-level traits that are used as controls in our modeling. Specifically, we collect municipal data on personnel remunerations from INEGI's Municipality Public Finance statistics as well as information on the presence of a cartel in the municipality in 2006, which are data provided by Coscia and Rios (2012). We also

7 The ability to conduct oral adversarial trials may be indicative of significant changes to the roles of key players, the legal structure that regulates the criminal justice system, and more check and balances put in place. Historically, the evidentiary phase has been presented in the form of written affidavits and largely occurred outside of public view. This lack of transparency has contributed to the publics' perception of inefficiencies among the prosecutors, defense attorneys, and judges. In addition, it has contributed to a lengthy and cumbersome process, resulting in bureaucratic inefficiencies (Shirk, 2011).

8 Our results are robust to excluding Nuevo León.

9 This differs to the methodology of Cepeda-Francese and Ramírez-Álvarez (2023), who in the case of Durango, attribute treatment in 2009.

gather information on states' unemployment rates, which comes from INEGI's National Survey of Occupation and Employment (ENOE), and on federal contributions to each state for security purposes, which come from INEGI's State and Municipal Public Finances statistics. Finally, we collect information on the share of votes for PAN senatorial candidates from the National Electoral Institute.

5.4 Descriptive Evidence

Figure 1 shows how homicide rates evolved, vis-à-vis the implementation rollout of the judicial reform, from 2000 to 2017. The graph also includes a vertical line marking the start of the Mexican drug war, which led to a drastic increase in killings. Homicide rates rose from an average of 12 per 100,000 in 2007 to more than 25 per 100,000 in 2011. It is around that time that the reform's implementation takes off across Mexican states, with the number of covered municipalities more than doubling between 2012 and 2013. Homicide rates dropped during that initial rollout of the reform but climbed back up after 2016 as the deadline for the full implementation of the reform grew close.

Figure 2 shows trends in homicide rates for municipalities that implemented the judicial reform and for those that had not done so yet. On one hand, the graph hints on the selective adoption of the reform by municipalities with higher homicide rates—rates that averaged close to 80 per 100,000 in 2009 when compared to 20 per 100,000 in remaining municipalities. Yet, the reform's implementation was likely independent of the wave of homicides in each state for several reasons. First, it required massive funding for the acquisition of new and improved infrastructure and technology; the retraining of police officers, judges, and prosecutors; and technical assistance to revise the federal and state criminal procedure codes (Ribando, 2013). Due to the high cost associated with its implementation, states with high crime rates, which already faced tight budget constraints, were less likely to implement the reform than low-crime states. In addition to its high cost, implementing the reform was daunting. For example, under the old system, 75 percent of Mexico's police lacked investigative capacity, a role largely relegated to the prosecutor's office (Shirk, 2011). Historically, the primary role of the police was primarily crime prevention, but they were also used as instruments of corruption and political coercion (Davis, 2006, 2008). Under the new system, it became necessary to train the police in other tasks, such as gathering evidence at crime scenes (i.e., employing investigative and forensic techniques). They also needed training to function as a professional, independent organization that adheres to due process.

Second, politicians were hesitant to move forward with the reform. Governors, barred from serving more than one six-year term, lacked the incentive to invest in the implementation of a costly new system that they might not see in full operation before their term expired (Ribando, 2013). Many also feared they would be seen as "too soft on crime," a perception that arose under the new system due to the reduced use of pretrial detention for nonviolent crimes (Dávila, 2018).

Last, a close look at early adopters supports the notion that homicide rates were not necessarily the rationale for the earlier adoption. The first state to enact the reform—Nuevo León in 2004—had a prereform homicide rate (33.60 percent) that was above the national average (16.34 percent). Yet, when concrete steps toward implementation were taken, the focus was not on homicide rates but on minor offenses. While the regulation pertaining to minor offenses was impacted by the reform in 2009, it was not until 2015 that the reform began to significantly impact other types of crimes,

including homicides. Another example is Chihuahua, the second state to enact the reform in 2007. Here, homicide rates were rather stable between 2000 and 2007. However, in 2008, coinciding with the initial stages of the reform's implementation, there was a sharp rise in violence. This surge in violence was largely due to the conflict between the Sinaloa and Juarez cartels. The Sinaloa Cartel, one of Mexico's most powerful trafficking organizations at that time, was challenging the Juarez Cartel, which had historically controlled drug smuggling operations in Chihuahua, to gain access to new drug routes (Bosque, 2012; Caldell and Stevenson, 2010; Flannery, 2017).

Conversely, the drastic reduction in homicide rates enjoyed by implementers between 2009 and 2015 in Figure 2 occurred at a time when those rates stabilized elsewhere. This pattern could be interpreted as indicative of the judicial reform aiding localities in their efforts to fight homicides. In what follows, we explore the empirical support for these alternative possibilities.

6 Main Findings

6.5 Mexico's Judicial Reform and Homicide Rates

Table 1 displays the TWFE estimates from estimating equation (1). The model in column (1) only contains a dummy variable indicative of when the municipality implemented the reform, along with basic municipality and year fixed effects. In column (2), we further expand equation (1) to include state-specific temporal trends accounting for time-varying state-level traits potentially affecting homicide rates, such as investments in policing. Last, in our most complete model specification in column (3), we include a series of municipality and state time-varying controls that may be viewed as potentially endogenous to homicide rates, such as municipal data on personnel remunerations, information on the presence of a cartel in the municipality as of 2006, the state's unemployment rate, information on federal contributions to each state for security purposes, and data on the share of votes for PAN senatorial candidates. Overall, regardless of the model specification being used, all columns convey the same message: the reform's implementation appears to have been accompanied by an increase in the homicide rate. In the most complete model specification, as shown in column (3), this increase averaged 17 percent, raising the homicide rate from 2.8 to 3.3 per 100,000 after the reform.

As noted in the methodology, the TWFE estimates in Table 1 cannot be interpreted as weighted averages of unit-level treatment effects when treatment occurs in a staggered fashion (e.g., Goodman-Bacon, 2021). TWFE estimates with differential timing include both "clean" comparisons between treated and not-yet-treated units as well as "forbidden" comparisons between units where both have already been treated. The latter group of comparisons can bias the TWFE estimates due to negative weighting problems. To assess if that is the case, we first conduct a Goodman-Bacon decomposition diagnostic check. As shown in Figure 3, the largest weight (more than 80 percent) is the one assigned to early versus late treated comparisons. Because these comparisons involve units that are treated early and their status never changes thereafter, they can also be considered a clean comparison.

Nevertheless, to address any remaining concerns regarding the estimates, we reestimate equation (1) using Gardner's (2022) heterogeneous treatment robust estimator. As in Table 1, we estimate three different model specifications that progressively add controls to the model. All three model specifications yield alike results; therefore, we focus our discussion on the results from the most

saturated model. Based on the results in the last column of Table 2, the judicial reform continues to be associated with an increase in the homicide rate. The latter rises by 27 percent, increasing the homicide rate by 0.8, from 2.8 to 3.6 per 100,000 following the reform's implementation by Mexican municipalities.[10]

Our findings are in line with previous results for other Latin American countries, such as Colombia. For instance, Zorro, Acosta, and Mejía (2020) document an increase in overall crime rates of approximately 22 percent following the implementation of a US-like adversarial judicial system. Similarly, focusing on Mexico over an earlier period, Cepeda-Francese and Ramírez-Álvarez (2023) document an increase in murders by 7.5 per 100,000 between 1997 and 2012. This increase almost doubled the murder rates in municipalities that enacted the reform.

6.6 Identification Checks

There are several threats to the interpretation of the estimates in Table 2 as causal. One pertains to the assumption of parallel pretreatment trends. While it is not feasible to fully test the counterfactual, we can conduct an event study to assess if homicide rates trended similarly in municipalities that implemented the reform earlier compared to those that adopted it later, before the reform was adopted. Specifically, we estimate the following model:

$$(2)\quad y_{mt}=\alpha+\sum_{t=-5}^{-2}\ \tau_t\cdot 1(Reform_{mt}=1)\ +\sum_{t=0}^{5}\ \rho t\cdot 1(Reform_{mt}=1)+y_m+y_t+\varepsilon_{mt}$$

where the indicator function $1(Reform_{mt}=1)$ represents the t^{th} year before or after the reform's implementation in municipality m. The coefficients in vector capture preexisting differences in homicide rates between locations that had implemented and those that had not yet done so. In contrast, the coefficients in vector capture the differential impact of the reform on homicide rates up to five years after its implementation.[11] This approach enables us to gauge if homicide rates already differed across municipalities before the reform. In addition, it uncovers its short- versus long-lasting impacts on these municipalities' homicide rates.

Figure 4 displays the results from estimating equation (2) using the imputation approach developed by Borusyak, Jaravel, and Spiess (2021). This approach is robust to heterogeneous treatment effects and allows for a more flexible specification of the event study, as is the case when grouping periods farther away from the treatment with few observations. As shown therein, the coefficient estimates for the years preceding the reform's adoption were generally nonstatistically different from zero, strongly supporting the assumption of no differential pretrends in homicide rates. In addition, there are signs of a clear break in that trend following the reform's

10 A main concern in the evaluation of the judicial reform is having a valid control group when participants in the so-called treatment are not randomly assigned to treatment and control groups, as in this case. We thus experiment with using DiD propensity score matching to gauge the robustness of our findings to using an alternative methodology, where municipalities in the treated and control groups are matched based on their pretreatment observable characteristics. While propensity score matching does not eliminate entirely selection biases, it is yet another way to address selection based on observables. Appendix Table A4 displays the results from this exercise after implementing one-to-one matching methods based on municipal data on personnel remunerations, information on the presence of a cartel in the municipality as of 2006, the state's unemployment rate, information on federal contributions to each state for security purposes, and data on the share of votes for PAN senatorial candidates. Appendix Figure A1 shows the results of verifying sufficient common support. As in Table 2, the results in Appendix Table A4 confirm the increase in homicide rates following the reform's implementation. Specifically, had the control units adopted the reform, homicide rates would likely be reduced by 0.5.

11 Periods before t=-5 and after t=5 are binned up into t=-5 and t=5, respectively.

implementation. Specifically, immediately following the implementation, there is evidence of an increase in the homicide rate, which persists for five years.

Another threat to identification involves the potential endogenous nature of the reform's adoption timing. While policy responses are unlikely to be random, our concern would be if local homicide rates led to the adoption of the reform or, alternatively, if changes in homicide rates were driven by potential confounders, such as changes in locality traits correlated to the adoption. The estimates in Table 3 address the first question by modeling the reform's adoption timing. As shown therein, while economic conditions (as captured by the unemployment rate) and political inclinations (as captured by the share of votes for PAN senatorial candidates) appear to be good predictors of the reform's adoption timing, prereform homicide rates are not.

Next, in Table 4, we examine if, alternatively, the judicial reform implementation impacted municipality- or state-level traits potentially correlated to homicide rates, which would also affect our estimates through endogeneity biases. The possibility that municipalities implementing the reform might differ from those not doing so at a particular point in time is a concern partially addressed using propensity score matching, which yields similar results as shown in Appendix Table A4. Nevertheless, as an additional check, we explore the possibility that municipalities implementing the reform significantly differed from their counterparts in traits shown to be correlated to homicide rates. As shown in Table 4, we fail to find much support for this hypothesis, with only a weak correlation between the reform and the share of votes for PAN senatorial candidates.

Finally, we address the possibility that spurious correlations with unobserved or unaccounted for factors may be driving the estimated impact of the reform on homicide rates. To that end, we conduct a series of placebo estimations where we randomly assign false reform dates corresponding to periods preceding the true policy implementation. We then use those values to estimate equation (1). Figure 5 shows the distribution of the DiD estimates resulting from the 500 placebo replications, as well as the actual point estimates from Table 2. As expected, the placebo point estimates fall within a 95 percent confidence interval band, suggesting they are not statistically different from zero. In contrast, the actual point estimates fall to the right and outside the 95 percent confidence interval band, suggesting that the estimated impact in Table 2 is statistically different from zero and not the byproduct of spurious correlations.

6.7 Heterogeneous Impacts and Potential Mechanisms

To gain a better understanding of the rationale for our findings, we follow Huebert (2019) and Cepeda-Francese and Ramírez-Álvarez (2023) and first distinguish between municipalities in states with one or more drug cartels in the state as of 2006—before the reform—to circumvent reverse causality concerns. As noted by these authors, the lack of credible government authority amid cartel deaths to control drug routes may have made residents hesitant to come forward to the police for fear of the cartel retaliating. If so, we would expect the reform to be associated with higher homicide rates in municipalities located in states with a known cartel. Despite this expectation, as shown in Table 5, we find evidence of the reform being accompanied by significantly higher homicide rates across all municipalities even though the impact was somewhat greater in states with organized crime, where homicide rates were practically doubling those of other states after the reform. Specifically, following the reform's implementation, homicide rates

increased by approximately 28 percent across municipalities in states with organized cartels, from 3.1 to 3.9 per 100,000, and by 23 percent across municipalities in other states, from an average of 1.9 to 2.4 per 100,000.

What else could be driving the higher homicide rates associated with the judicial reform adoption? As noted earlier, the expectation that homicide rates would decrease following the reform's adoption was rooted in the notion of deterrence and incapacitation. However, one of its trademarks was a reduction in pretrial detention, which was reserved for crimes that required imprisonment. As a result, many citizens may have perceived that criminal conduct was not seriously prosecuted, making them hesitant to come forward to the police and reducing any incapacitation and deterrence effects that result from detention.

To assess the plausibility of this explanation, we first look at arrest rates per 100,000 inhabitants. Other things equal, we would expect a significant increase in arrest rates amid a 27 percent in homicide rates in the presence of an efficient criminal justice system. To assess if that is the case, we use INEGI administrative data on criminal proceedings, covering 1,311 municipalities from 26 Mexican states.[12] The municipality of the arrest corresponds to the homicides' location and year of occurrence. Then, using the most complete model specification in Table 2, we reestimate the model for arrest rates for all municipalities as well as separately for municipalities in states with and without cartels before the reform's enactment.

As shown in Table 6, arrest rates decreased by 57 percent, from almost 2.5 to 1.1 per 100,000, following the reform's implementation. This reduction was somewhat greater across municipalities in states with a cartel, where rates dropped from 2.7 to close to 1 per 100,000, that is, a 64 percent reduction. This drastic reduction amid rising homicide rates may have empowered cartels to expand their operations and created the perception of immunity. Arrest rates also declined by a somewhat smaller magnitude, 56 percent, across municipalities in states without a cartel—from roughly 2.2 to close to 1 per 100,000, hinting at an overall lack of trust in authorities in combating crime, likely leading to curtailed crime reporting.

To assess the potential role of individuals' lack of trust and fear of retaliation by organized crime in explaining the growth in homicide rates, we next examine crime reporting rates. However, doing so is challenging as it requires survey data in which respondents are asked if they witnessed, or were victims of, a crime and reported it. Survey data on these topics spanning almost two decades are rare. We thus resort to using two distinct surveys that cover distinct time periods: the MxFLS covering 2000–2011 and the ENVIPE spanning 2012–2017.

The MxFLS consists of over four million observations in 17 Mexican states and 90+ municipalities. The survey asks victims to recall past years when they or a family member in their household were victims of crime, with the potential biases that may result from recall failures. Our outcome variable is a dummy variable equal to one if the household reported the crime to the authorities and zero otherwise.[13] Using that information, we model the likelihood of reporting a crime across all 90+ municipalities, as well as according to whether the municipality was in a state with organized crime, as a function of the reform implementation and the controls included in the most complete model specification of Table 2.

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12 Due to systematic changes in reporting systems, beginning in 2014, INEGI does not provide municipal-level arrest data for six states: Chihuahua, Mexico City, Morelos, Nuevo Leon, Oaxaca, and San Luis Potosi.

13 The MxFLS question asks "Did you or any household member denounce the incident at an authority office?"

Table 7 indicates that the reform appears was accompanied by a significant reduction in reporting rates, even if the impact was weakly significant. The drop was particularly noticeable among municipalities in states with organized crime, where respondents were less than half as likely to report a crime as they were before the reform's implementation—a decline significant at the 1 percent level.

Next, we experiment with the ENVIPE. Despite its smaller size, of roughly 372,311 observations, it covers all 32 Mexican states and 570 municipalities (a number that varies by year). Households whose members were victims of crime in the previous year were asked if they reported the crime to the authorities.[14] Table 8 displays the results from estimating the same models as in Table 7. Overall, reporting rates dropped by 20 percent from 2012 to 2017. As in the MxFLS, the decline appears to have primarily stemmed from respondents residing in states with organized crime, where crime reporting rates decreased by approximately 25 percent during 2012–2017.

While a direct comparison of the estimates derived from both surveys is not feasible due to differences in their questionnaires, time frames, and geographic coverage, the estimates from both suggest significant reductions in crime reporting after the reform's implementation. The MxFLS estimates indicate larger decreases during the earlier period, while the ENVIPE estimates suggest continued reductions years after the reform. In sum, the reform appears to have been accompanied with substantial reductions in arrest rates and declines in overall crime reporting. These declines might have interfered with the reform's ability to target crime, as reflected by the significant increase in homicide rates.

7 Summary and Conclusion

Widespread corruption, impunity, and lack of transparency have historically undermined the administration of justice in Mexico. In recognition of these problems, state-led efforts to revamp the judicial system focused on improving transparency and accountability in justice administration with changes that included oral and public trials, the presumption of innocence, defined timelines for procedural stages, and alternative trial mechanisms. Together, the reform aimed to improve the standards needed to make an arrest and increase confidence in the judicial system—a change that would hopefully translate into reduced crime rates and increased safety.

We assess the effectiveness of the judicial reform in reducing crime as captured by homicide rates, which are less likely to suffer from underreporting biases. In line with prior studies, albeit incorporating the full implementation period of the reform as well as the recent spike in homicides after 2012, we find that the reform was accompanied by a significant increase in homicide rates across all states. The increase appears to have been greater in those with a cartel before the reform's enactment, where fear of reprisal might have been greater.

To gain a better understanding of the reform's failure in decreasing crime, we also examine arrests, which, amid increasing homicide rates, would be expected to rise if the criminal justice system were working efficiently. We find that across all states, arrests significantly declined, likely driven by the higher standards established by the reform. However, the very large decrease in arrests amid

14 The ENVIPE asks two questions on reporting a crime to the authorities: "Did you go to the public ministry to denounce the crime?" and "Did you report the crime to another organization or authority?"

significant increases in homicide rates possibly conveyed the view that criminal conduct was going unpunished. This perception likely empowered organized crime, which enjoyed the increased due process protections implemented by the reform resulting in fewer pretrial detentions (Chalfin and McCrary, 2017). The perception that crime went unpunished, along with the lack of incapacitation and deterrence effects, likely prevented victims from coming forward to the police and relevant authorities.

To confirm the role of individuals' hesitance to interact with the criminal justice system due to diminished trust in the authorities or fear of retaliation, especially in states with organized crime, we examine crime reporting using data from the MxFLS and the ENVIPE. While a direct comparison of the findings from the two surveys is unfeasible due to their different questions, time frame, and geographic coverage, both sources reveal a generalized decrease in the propensity to report a crime following the reform's implementation. Notably, in states without organized crime over the later period of analysis (2012–2017), this decline was not different from zero.

In sum, despite significant data challenges, the analysis uncovers the needs for continued improvement of the Mexican criminal judicial system. Better data on crime, as well as investments in police and the judicial criminal system, are needed to assess the effectiveness of conducted reform and to implement policy changes that ameliorate community trust in authorities and the delivery of justice. Achieving that goal is not only critical for Mexico but also for its neighbors, including the United States, due to its impact on migration flows and the social, economic, and political stability of the entire Latin American region.

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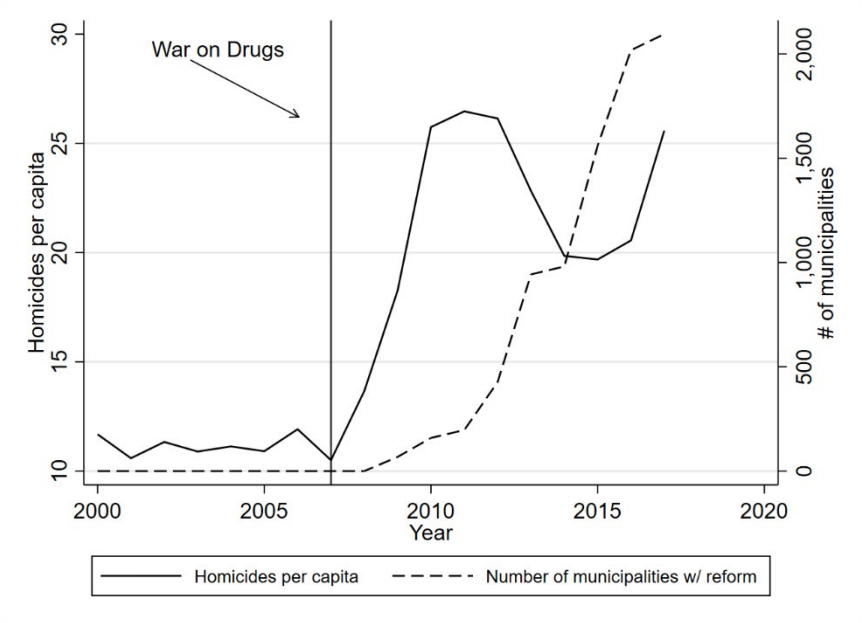
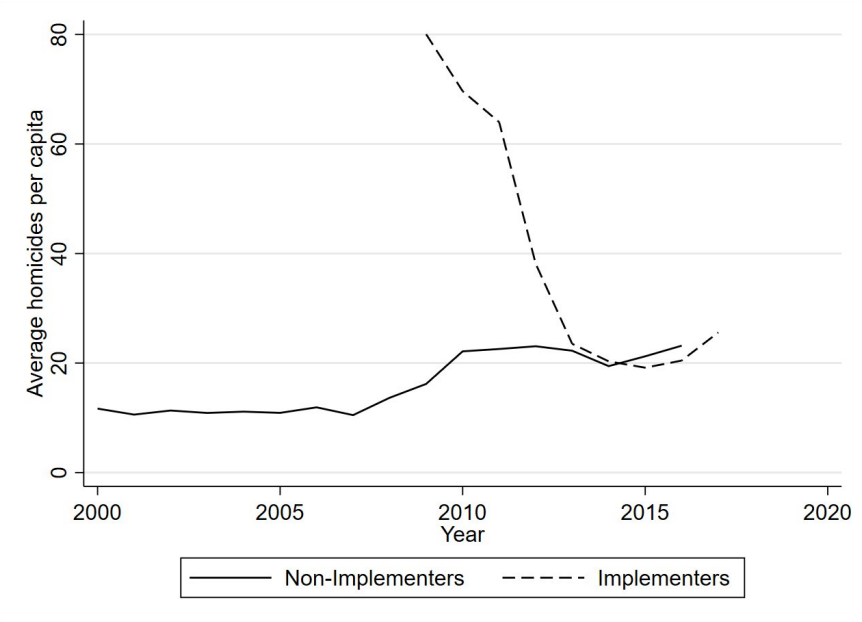
Figures

Figure 1. Homicide Rates and the Rollout of the Judicial Reform



Notes: INEGI data and author's own calculations

Figure 2. Average Homicide Rates among Adopters and Not-Yet Adopters



Notes: INEGI data and author's own calculations

Figure 3. Goodman-Bacon Decomposition

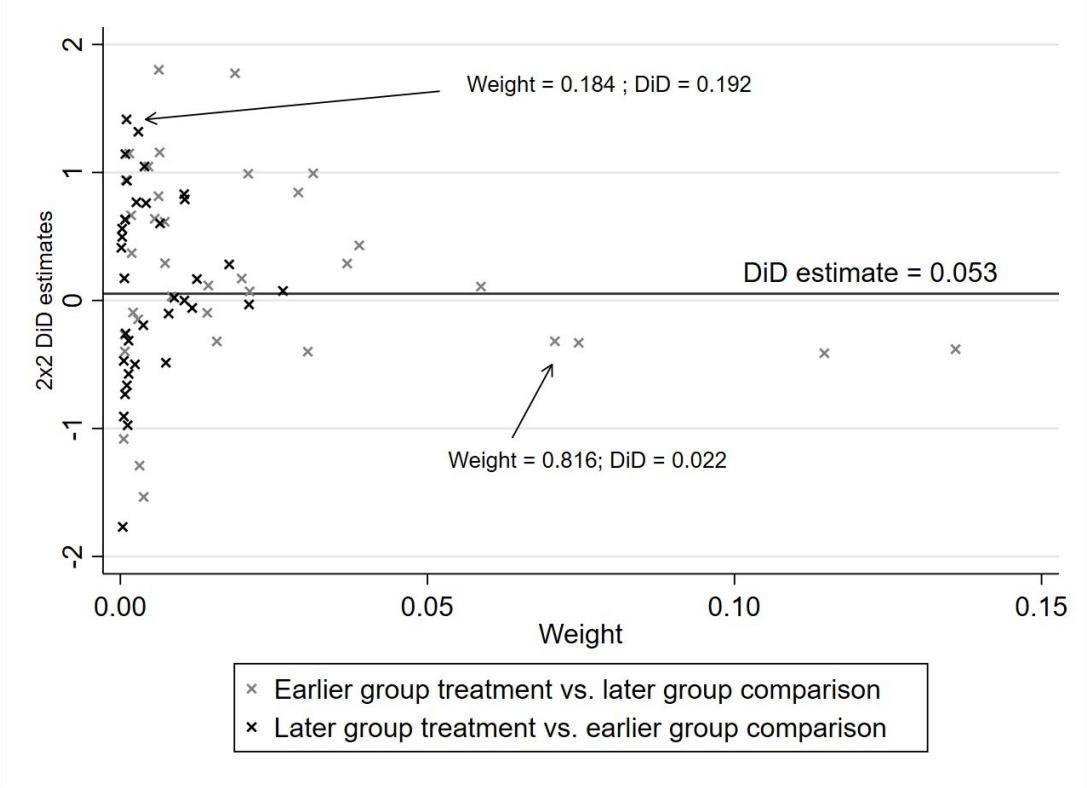
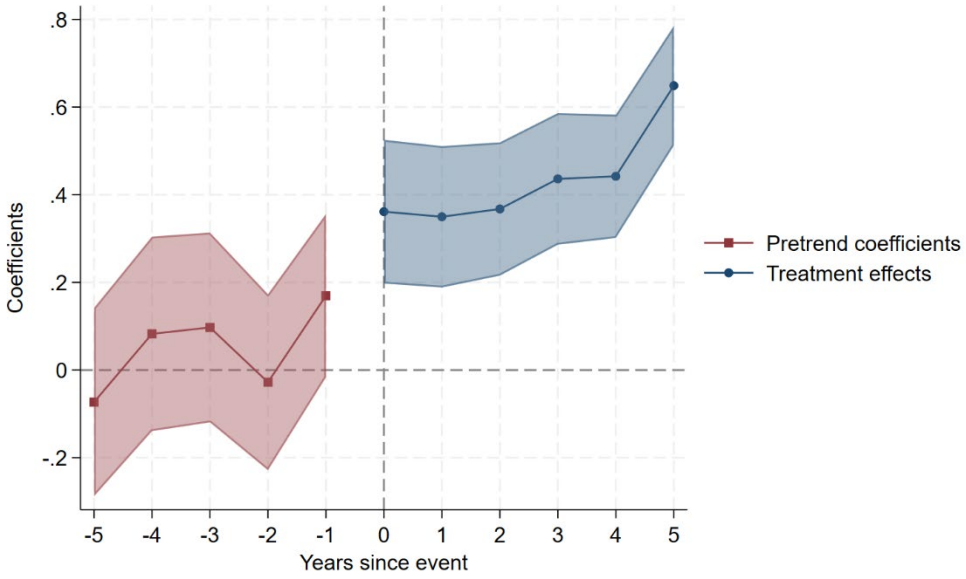
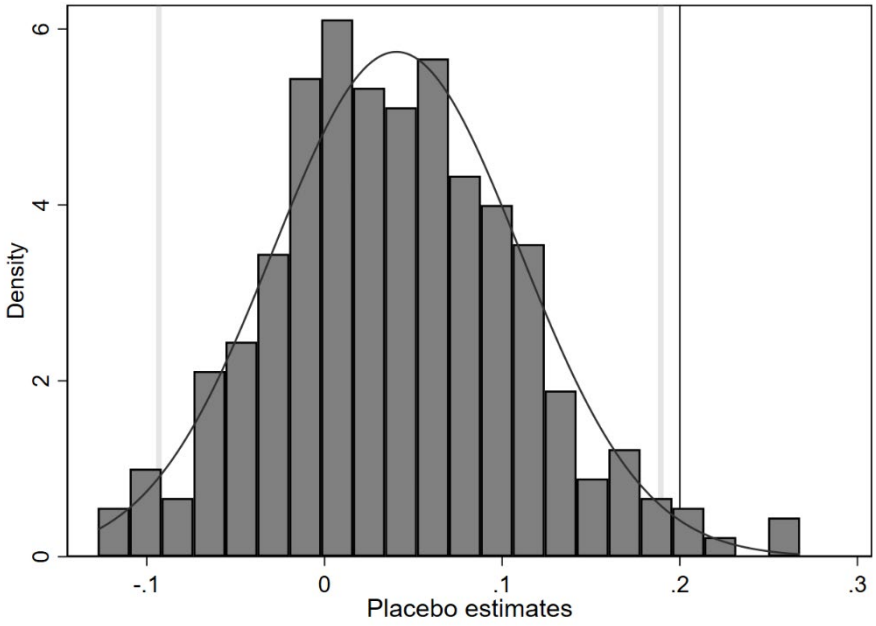


Figure 4. Event Study for Homicide Rates



Notes: Displayed estimates correspond to those from estimating equation (2) using the imputation approach developed by Borusyak, Jaravel, and Spiess (2021), which is robust to heterogeneous treatment effects. Periods before $t=-5$ and after $t=5$ are binned up into $t=-5$ and $t=5$, respectively.

Figure 5. Placebo Check



Tables

Table 1. TWFE Estimates for Homicides

Outcome:	**Homicide rate per 100,000**		
Column:	**(1)**	**(2)**	**(3)**
Reform	0.1465*	0.2065**	0.1717**
	(0.0673)	(0.0629)	(0.0511)
Observations	37,710	37,710	37,710
Mean	2.8254	2.8254	2.8254
Municipal fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State-specific time trend	N	Y	Y
Controls	N	N	Y

Notes: We use an inverse hyperbolic sine transformation for the dependent variable (homicide rate per 100,000). Estimates are weighted by the municipality population, and standard errors are clustered at the municipality level. The reported dependent variable mean corresponds to the municipality-year cells before the reform's implementation. ***p < 0.001, **p <0.01, *p <0.05, +p<0.10

Table 2. Heterogeneous Robust DiD Estimates for Homicides

Outcome:	**Homicide rate per 100,000**		
Column:	**(1)**	**(2)**	**(3)**
Reform	0.2610***	0.2514*	0.2672**
	(0.0586)	(0.0836)	(0.0833)
Observations	37,710	37,710	37,710
Mean	2.8254	2.8254	2.8254
Municipal fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State-specific time trend	N	Y	Y
Controls	N	N	Y

Notes: We use an inverse hyperbolic sine transformation for the dependent variable (homicide rate per 100,000). Estimates are weighted by the municipality population, and standard errors are clustered at the municipality level. The reported dependent variable mean corresponds to the municipality-year cells before the reform's implementation. Gardner (2022) estimators are used. ***p < 0.001, **p <0.01, *p <0.05, +p<0.10

Table 3. Addressing Reverse Causality Concerns Modeling the Timing of the State's Reform Implementation

Outcome:	**Reform Timing**
Homicide per capita	0.0664 (0.0930)
Ln security appropriations	0.1078 (0.0935)
Ln employee salaries	0.1432 (0.1427)
Unemployment	4.2647*** (0.2323)
PAN	95.7572*** (0.3620)
Cartel in 2006	-0.0784 (0.2220)
# Observations	2,095
Mean	650.4032
R2	0.9786

Notes: This regression includes a constant term and state fixed effects. Reform timing is the dependent variable. All regressors refer to values in the year 2000 (before the reform). Estimates are weighted by the municipality population's size, and standard errors are clustered at the municipality level. Means reported correspond to municipality-year cells before the reform's implementation. The outcome is the month-year date of the reform's implementation. ***p < 0.001, **p <0.01, *p <0.05, + p<0.10.

Table 4. Assessing the Role of Potential Confounders

Municipality Outcome:	**Ln (Security Appropriations)**	**Ln (Employee Salaries)**	**Unemployment**	**PAN**	**Ln (Population)**
Reform	0.0645 (0.0480)	0.2465 (0.1513)	0.0517 (0.2005)	-0.0359* (0.0180)	1.2e-11 (0.0000)
Mean	7.1188	6.4041	3.9747	0.4188	12.0235
Municipal fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
State-specific time trend	Y	Y	Y	Y	Y

Notes: Estimates are weighted by the municipality population's size, and standard errors are clustered at the municipality level. Means reported correspond to municipality-year cells before the reform's implementation. Gardner (2022) estimators are used. ***p < 0.001, **p <0.01, *p <0.05, +p <0.10.

Table 5. Heterogeneous Robust DiD Results for Homicides, by Cartel/Non-Cartel

Outcome:	**Homicide rate per 100,000**	
Sample:	**States with cartels**	**States without a cartel**
Column:	**(1)**	**(2)**
Reform	0.2762^{+}	0.2327^{+}
	(0.1601)	(0.1309)
Observations	5,256	32,454
Mean	3.07	1.92
Municipal fixed effects	Y	Y
Year fixed effects	Y	Y
State-specific time trend	Y	Y
Controls	Y	Y

Notes: We use an inverse hyperbolic sine transformation for the dependent variable (homicide rate per 100,000). Estimates are weighted by the size of the municipality population and standard errors are clustered at the municipality level. Means reported correspond to municipality-year cells before the reform's implementation. Gardner (2022) estimators are used. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.10$.

Table 6. Heterogeneous Robust DiD Estimates for Arrests

Outcome:	**Arrests per 100,000**		
Sample:	**All states**	**States w/ cartels**	**States w/out cartels**
Column:	**(1)**	**(2)**	**(3)**
Reform	-0.5715*	-0.6385**	-0.5642***
	(0.1211)	(0.2244)	(0.1318)
Observations	22,287	3,689	18,598
Mean	2.4495	2.6739	2.1819
Municipal fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State-specific time trend	Y	Y	Y
Controls	Y	Y	Y

Notes: We use the inverse hyperbolic sine transformation for the dependent variable (arrests per 100,000). Estimates are weighted by the size of the municipality population, and standard errors are clustered at the municipality level. Means reported correspond to municipality-year cells before the reform's implementation. Gardner (2022) estimators are used. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.10$.

Table 7. Heterogeneous Robust DiD Estimates for the Likelihood of Reporting a Crime

Survey:	**Mexican Family Life Survey (2000–2011)**		
Sample:	**All states**	**States w/ cartels**	**States w/out cartels**
Column:	**(1)**	**(2)**	**(3)**
	-0.4828*	-0.5843***	-0.5121*
Reform	(0.2012)	(0.0173)	(0.2530)
Observations	4,756,117	2,656,385	2,099,732
Mean	0.2360	0.2691	0.1848
Municipal fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State-specific time trend	Y	Y	Y
Controls	Y	Y	Y

Notes: Standard errors are clustered at the municipality level. Means reported correspond to municipality-year cells before the reform's implementation. Gardner (2022) estimators are used. ***p < 0.001, **p <0.01, *p <0.05, ⁺p <0.10.

Table 8. Heterogeneous Robust DiD Estimates for the Likelihood of Reporting a Crime

Survey:	**ENVIPE (2012–2017)**		
Sample:	**All states**	**States w/ cartels**	**States w/out cartels**
Column:	**(1)**	**(2)**	**(3)**
	-0.0245**	-0.0314*	-0.0307
Reform	(0.0091)	(0.0143)	(0.0321)
Observations	252,270	166,073	86,197
Mean	0.1282	0.1253	0.1348
Municipal fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
State-specific time trend	Y	Y	Y
Controls	Y	Y	Y

Notes: Standard errors are clustered at the municipality level. Means reported correspond to municipality-year cells before the reform's implementation. Gardner (2022) estimators are used. ***p < 0.001, **p <0.01, *p <0.05, ⁺p <0.10.

Appendix

Table A1. Reform Enactment, Implementation, and Progress by State

Mexican State	Enactment	Implementation	% Coverage as of 2014
Aguascalientes	March 2013	November 2014	36.4
Baja California	October 2007	August 2010	20.0
Baja California Sur	June 2014	June 2016	0
Campeche	August 2009	December 2014	27.3
Coahuila	February 2012	June 2013	44.7
Colima	August 2014	December 2014	20
Chiapas	May 2012	November 2013	51.7
Chihuahua	June 2006	January 2007	100
Distrito Federal	February 2010	January 2015	0
Durango	June 2009	December 2009	17.9
Guanajuato	June 2010	September 2011	56.6
Guerrero	April 2014	September 2014	19.8
Hidalgo	August 2006	November 2014	7.1
Jalisco	April 2014	October 2014	16.0
Mexico	August 2006	October 2009	100
Michoacán	December 2011	March 2015	0
Morelos	November 2007	October 2008	100
Nayarit	May 2014	December 2014	15.0
Nuevo León	June 2004	December 2004	100
Oaxaca	September 2006	September 2007	46.7
Puebla	July 2012	January 2013	78.3
Querétaro	February 2012	June 2014	83.3
Quintana Roo	February 2012	June 2014	20.0
San Luis Potosí	September 2012	September 2014	15.5
Sinaloa	January 2013	October 2014	27.8
Sonora	June 2016	June 2016	0
Tabasco	August 2012	September 2012	35.3
Tamaulipas	May 2013	May 2014	7.0
Tlaxcala	May 2012	December 2014	53.3
Veracruz	November 2007	May 2013	39.2
Yucatan	May 2010	May 2011	100
Zacatecas	September 2007	January 2009	56.9

Notes: The percentage coverage, as of December 1, 2014, represents the number of municipalities over total municipalities in the state that had implemented the new judicial system. Data are form SETEC (2015).

Table A2. Comparison of the Traditional Judicial System in Relation to the New Accusatorial Oral System

Inquisitorial system or traditional trials	Oral accusatory system
These include written and nonpublic judgments.	This includes oral trials and public hearings.
The accused must prove their innocence during the process.	The accused is presumed innocent until proven otherwise.
All crimes are committed to trial.	Only serious crimes go to trial.
The defense may fall to a person of trust regardless of whether they are a lawyer.	The victim has the support of the legal counsel and the defendant the support of a public defender; both should be certified lawyers.
Only the public prosecutor participates in the process.	The public prosecutor and the victim participate in the process.
The public prosecutor validates the evidence.	The public prosecutor conducts the investigation, and the judges validate the evidence.
The review of the process falls on a single court.	The review is divided among various entities during the process.
Procedural stages occur with indefinite timelines.	Procedural stages occur with specific, defined timelines.
It results in unofficial preventive detention; people innocent or imputed of misdemeanors live their process in prison.	Pretrial detention is only given for crimes requiring imprisonment.

Table A3. Distribution of the Judicial Reform Implementation

Year	# of municipalities	% of total municipalities	# of cartel municipalities	(# of cartel municipalities/ # of municipalities)
2009	68	2.74%	21	30.88%
2010	163	6.58%	30	18.40%
2011	201	8.12%	37	18.40%
2012	433	9.59%	57	13.16%
2013	1,071	43.59%	84	7.84%
2014	1,109	44.80%	98	8.83%
2015	1,922	77.65%	222	11.55%
2016	2,398	96.88%	283	11.59%
2017	2,475	100%	297	11.79%
2018	2,475	100%	297	11.79%
2019	2,475	100%	297	11.79%

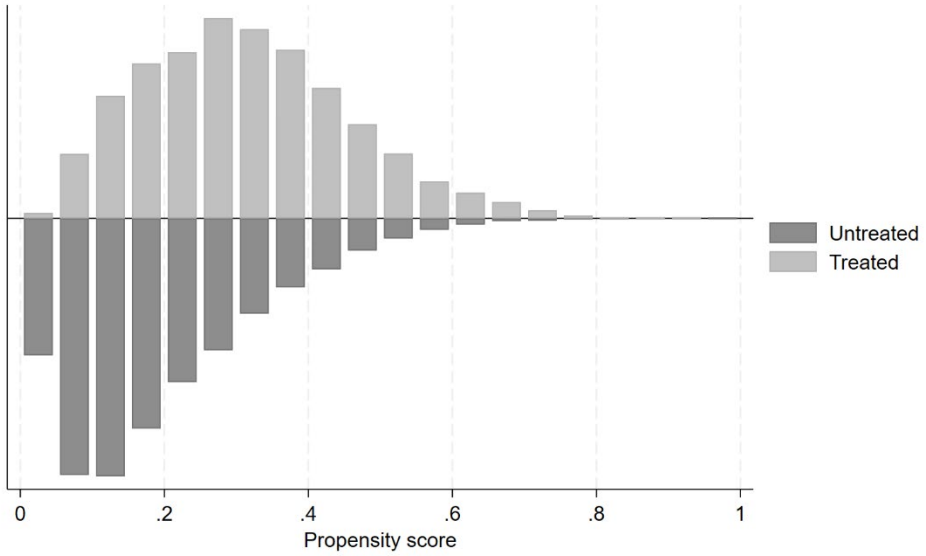
Notes: In 2014, the average percentage of implementation coverage reported by the Secretaries Technical Advisory Coordinator for the Implementation of the Judicial Reform System was 40.5 percent, which is close to our percentage of 44.80 percent. Huebert (2019) reports 33.90 percent in 2014.

Table A4. Propensity Score Matching Average Treatment Effect on the Treated

Variable	Sample	Treated	Control	Difference
Homicides per capita	Unmatched	2.4334	1.9762	0.4572***
	ATT	2.4334	1.9546	0.4788***

Notes: The results reported are from a one-to-one matching method. Traits include the state's unemployment rate, cartel presence in 2006, municipality data on personnel remunerations, federal contributions to each state for security purposes, and the share of votes for PAN senatorial candidates. ***p < 0.001, **p <0.01, *p <0.05, ⁺p <0.10.

Figure A1. Distribution of Propensity Score across Treatment and Comparison Groups



Notes: This figure shows the overlap of the two groups' propensity scores, suggesting they share a lot of common support on the covariates in the model.