

Torture as a method of criminal prosecution: Democratization, Criminal Justice Reform, and the Mexican Drug War

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Abstract

A criminal trial is likely the most significant interaction a citizen will ever have with the state; its conduct and adherence to norms of fairness bear directly on the quality of government, extent of democratic consolidation, and human rights. While theories of repression tend to focus on the political incentives to transgress against human rights, we examine a case in which the institutionalization of such violations follows an organizational logic rather than the political logic of regime survival or consolidation. We exploit a survey of the Mexican prison population and the implementation of reforms of the justice system to assess how reforms to criminal procedure reduce torture. We demonstrate that democratization produced a temporary decline in torture which then increased with the onset of the Drug War and militarization of security. Our results show that democracy alone is insufficient to restrain torture unless it is accompanied by institutionalized protections.

1 Introduction

What restrains police brutality – illegal arrests, coercion of witnesses, fabrication of evidence, and the use of torture to extract confessions? This question is closely related to a classic puzzle in political science: the origin and maintenance of constraints on the state’s exercise of coercive power. Scholars examine the importance of international treaties and a global civil society on restraining human rights violations (Epp, 1998; Finnemore and Sikkink, 1998; Simmons, 2009; Franklin, 2008; Hafner-Burton, 2008). Other theoretical explanations for the emergence and consolidation of a range of phenomena from rights

to democracy to constraints on the state tend to focus on the actions of elites, middle-class groups, or the median voter (North and Weingast, 1989; Weingast, 1997; Ansell and Samuels, 2010; Boix, 2003). Here we examine a case of constraints on state transgressions against accused criminals, a group that typically lacks the political clout to produce changes in the social contract. Though most modern criminal justice systems prohibit inhumane punishments and coercion to obtain confessions, a gap between the laws and actual practices is all too common, even in democracies (Rejali, 2009).

This paper uses the Mexican case to explore how torture becomes established as a generalized practice in a criminal justice system, the conditions under which democracy can succeed or fail to restrain it, and how a change in the institutions of the criminal justice system has consolidated protections against torture. We explore how the judiciary paved the way for the development of an organizational equilibrium in which law enforcement relied on torture and did not invest in investigative capacity. We show that the alternation of power at the gubernatorial level and increased political contestation as a result of democratization did succeed in lowering abuse, but the onset of the Drug War meant the development of a serious violent threat which led to security strategies that increased abuses. Finally, we examine institutional reforms and show that they were able to consolidate procedural protections for accused criminals, even as criminal violence in Mexico has been rising.

Existing literature argues that autocrats use repression, including torture, to extract information about potential conspiracies, as a strategy to dissuade opponents, or as a punitive measure against acts that are indicative of dissent (Lichbach, 1987; Wantchekon and Healy, 1999; Davenport and Inman, 2012; Svolik, 2012; Blaydes, 2018). While we focus our attention on transgressions that are targeted at broad social classes, related work examines the logic of demographically selecting groups for targeted repression (Rozenas, 2018). Our paper examines a case of widespread torture that, for the most part, was not mandated by the top political leadership against regime opponents but occurred in a decentralized fashion, perpetrated by the police and public prosecutors in common crim-

inal trials. The benefits of systematically torturing common criminals are analytically different from those that drive a regime to torture dissidents. Systematic abuses emerged from a criminal justice system in which superiors in the prosecutor’s office and the police established internal informal procedures that rewarded torture as a substitute for developing investigative capacity, and where courts decided to give confessions full probative value regardless of how they were obtained.

The paper further elaborates on the reasons why democracies might fail to restrain this form of torture. Although the literature has established that democracies restrain torture (Davenport, 1995; Conrad and Moore, 2010; Cingranelli and Richards, 1999; Davenport and Armstrong, 2004; Evans and Morgan, 1998), the moderating impact of democratic institutions disappears when the state faces “violent dissent” (Davenport et al., 2007). This line of work aims to make sense of why democratic states engage in torture against rebels or terrorists (Greenberg, 2005; Danner and Fay, 2004; Luban, 2007). When there is a violent threat, electoral incentives restraining elected officials from resorting to torture might be absent. Following Walzer (2004), the people are unlikely to hold the executive accountable for “dirtying his hands” with torture to keep them safe.

Our paper expands this line of argumentation to include threats to the state by criminal groups, which emerged as a serious threat in Mexico following democratization (Ríos, 2013; Dube et al., 2013; Osorio, 2015; Trejo and Ley, 2018). The federal government started a war against drug trafficking organizations (DTOs) at the end of 2006, deploying approximately 45,000 military personnel in operations against criminal organizations across the country. There is consensus in the literature that the Drug War provoked a dramatic increase in violence (Trejo and Ley, 2018; Osorio, 2015; Dube et al., 2013; Ríos, 2013; Lessing, 2015). Although scholars have argued that the drug war increased human rights abuses (Escalante, 2011; Anaya, 2014; Magaloni et al., 2018; Gallagher, 2017), because of data limitations none provide solid causal evidence. This paper is the first of its kind to provide compelling causal evidence that security interventions that deployed the armed forces to combat DTOs produced substantial increases in torture. This aspect

of our work speaks to the growing literature on violent crime and drug wars in Latin America. In recent years, levels of insecurity and fear of crime have risen throughout the region. These challenges have fueled public pressure to enlist the armed forces to assist the police in combating crime (Bailey and Dammert, 2005). *Mano dura* security strategies, bringing about a denial of due process and basic civil rights, have introduced elements of authoritarianism into democracies, leading to “hyperpunitive” criminal justice practices (Godoy, 2006).

Our findings reinforce the notion that democracy alone is insufficient to restrain state abuses unless it is accompanied by institutionalized protections. During Mexico’s transition to democracy, the country didn’t implement major security-sector reforms that constrained military and police behavior (Davis, 2006; Shirk and Cázares, 2007; Uildriks, 2010; Trejo and Ley, 2018). It was not until the 2008 criminal justice reform when the legal framework to overhaul the criminal justice system was established, although its implementation was delayed until 2016. The reform transformed the system from an inquisitorial to an adversarial one. It expanded judicial oversight over the prosecutor’s investigation and legal controls of the police, introducing three judges in every criminal trial.

The fundamental question is if the criminal justice reform in Mexico has worked to restrain abuse. According to the existing literature, institutions such common law, constitutional provisions, and judicial independence emerge as the best predictors of state repression (Cross, 1999; Keith, 2002; Hill and Jones, 2014; Powell and Staton, 2009; Mitchell et al., 2013). Conrad and Moore (2010) advance the argument that inquisitorial criminal justice systems are more likely to incentivize police to torture. However, they fail to provide compelling empirical support to this proposition. The authors use civil law countries as proxy for inquisitorial criminal justice procedures but the problem is that many civil law countries have actually abandoned the inquisitorial model.¹ Our

¹It is worth noting that Mexico is joining other Latin American countries engaged in moving away from inquisitorial criminal justice systems, including Argentina, Bolivia, Colombia, Costa Rica, Chile, Ecuador, El Salvador, Guatemala, Honduras, Nicaragua, Paraguay, Peru, and Dominican Republic (Ro-

paper contributes to this body of work by exploring the causal effect of criminal justice procedures on torture.

In addition to the literature on state repression, our works contributes to the emerging body of work on police and police violence. This line of work treats the fundamental issue of human rights' protections less as a problem of political regimes than as a problem of controlling police violence. Drawing on [Mummolo \(2018\)](#), we distinguish broadly between two lines of investigation, one emphasizing potentially immutable officer traits as the culprits of police misconduct, including issues such as authoritarian personalities, racial biases, machismo, cynicism, aggression, conservative ideology, and substance abuse. ([Balch, 1972](#); [Twersky-Glasner, 2005](#); [Laguna et al., 2009](#); [Niederhoffer, 1967](#); [Hargrave et al., 1988](#); [Fielding and Fielding, 1991](#)). According to this line of investigation, torture could be explained as an inhumane act of treacherous individuals a democratic state would have a hard time restraining with institutional reforms alone.

A second line of investigation emphasizes the impact of institutions on police violence.² [González \(2019\)](#) explains that in Latin American democracies, undemocratic coercive institutions have persisted well after dictatorships ended. She shows that institutional reforms of the police happen sporadically, only when societal preferences converge and robust political opposition forms. Other scholars have also emphasized how the legacy of authoritarian and military control over the criminal justice systems represent critical obstacles for police reform ([Shirk and Cázares, 2007](#)). Throughout the region there have been many attempts to modernize police forces ([Bailey and Dammert, 2005](#); [Davis, 2006](#); [Uildriks, 2009](#); [Ungar, 2002](#)). This paper addresses, in particular, how criminal justice reform impacts one of the most insidious forms of police abuse: torture.

Empirically, the paper makes use of a novel measure of torture in criminal trials as well as interviews with police conducted in the three largest cities of Mexico, Mexico

drigo de la Barra Cousino, 1998; [Biebesheimer and Payne, 2001](#); [Ungar, 2002](#)).

²In the US context, [Mummolo \(2018\)](#) is one of the first pieces of research to provide valid causal identification to the institutionalist path to restraining police violence. In his paper, the problem in need of correction is racially biased and abusive police stops.

City, Guadalajara, and Monterrey. Our data comes from the National Survey of the Population Deprived of Liberty (ENPOL), conducted by the Mexican National Statistics Agency in 2016. The survey draws on the responses of a representative sample of 58,127 prisoners. The survey includes a battery of questions about how the police and agents of the Public Ministry³ treated the prisoner at the time of his or her arrest, including questions about physical abuse.

To causally identify how the change of an inquisitorial to an adversarial criminal justice system impacts torture, our statistical analyses leverage the staggered implementation of the criminal justice reform in Mexico's states. We define whether a prisoner was a subject to the reform by leveraging the date of his or her arrest. Any individual arrested in a municipality after the new code of criminal procedure took effect is treated. In the states we are studying, the reform was implemented on 65 different dates spanning the period from 2014 to June 2016, making it unlikely that our findings reflect changes in conditions beyond the criminal justice reform. By using state or municipal-level fixed effects, fixed effects for the time of the arrest and individual controls, our empirical strategy will control for unobserved characteristics in all treated states or municipalities that are constant over time, observed individual characteristics, and major events, such as the onset or escalation of the Drug War. Because the precise timing of the implementation of the reform is not random, we also subset the data to a period immediately around the implementation of the reform. We do this to exploit the time immediately before and after the implementation of the reforms to limit the possibility of confounding factors driving our findings. We further report a range of robustness checks. Overall, our statistical results demonstrate that the implementation of the criminal justice reform has strongly impacted police behavior, reducing the incidence of torture.

The paper further explores the effect of local democratization, which we measure as alternation of political power at the gubernatorial level, on torture. The states with the

³The prosecutor. The corresponding name in Spanish is Ministerio Público and we refer to it as the MP throughout the paper.

highest levels of torture are those where the Institutional Revolutionary Party (PRI) has never lost power. Moreover, although the results show that local democratization reduced torture immediately after the PRI lost power for the first time by around 10 percentage points, many states reverted to levels of torture comparable to autocratic levels. To understand this reversion, we focus on the Drug War. Our results reveal that in states where the armed forces and the federal police were deployed to assist local governments to fight DTOs, there is a substantial increases in torture. Furthermore, we show that torture is significantly higher when prisoners are convicted for organized crime.

The paper proceeds as follows. In the next section, we discuss the Mexican case, followed by our qualitative fieldwork. The fourth section describes our data. The fifth section presents our empirical analysis about the impact of the criminal justice reform on torture. The sixth section presents our empirical analyses estimating the impact of democratization on torture. The seventh section presents evidence about how the drug war shaped torture. We end with a conclusion.

2 The Case of Mexico

The most common form of torture in Mexico's autocracy took place in common criminal trials, which more often than not were not associated with political struggles, nor was this form of torture mandated by the top political leadership. This doesn't deny the fact that Mexico's autocratic regime did resort to torture to repress political dissidents. For example, there is ample evidence that during the Dirty War in the 1970s (1965-1982) torture was used as a way of pursuing political enemies ([Barba-Sánchez, 2015](#); [Aviña, 2012](#); [González Villarreal, 2014](#); [Doyle, 2006](#); [Castellanos, 2007](#)). However, in contrast to military regimes in Latin America, in Mexico's hegemonic party autocracy repression against political enemies didn't become a *state practice*. Instead, torture in common criminal trials was widespread. It did not matter whether there were indications that the detainee had been beaten, suffocated or electrocuted, if he was subjected to prolonged

detention, or if he had not been given access to a lawyer, courts would give full evidentiary value to confessions, reducing the incentive to develop investigative capacity (Piccato, 2017).

Mexico has historically employed a mixed inquisitorial system. Most crimes are investigated and prosecuted at the state level. Governors appoint the State Attorney who in turn appoints the agents of the Public Ministry (MP) and the Ministerial or Investigative Police⁴ who investigate and prosecute crimes. Ideally, the MP develops the investigative line and legal strategy and directs the investigative police, which works in the street collecting information and interviews with witnesses. In reality, the investigative police acts without much control from the MP. There are also state and municipal preventive police appointed by governors and mayors, respectively. Only the state and federal police have investigative capacity, although municipal police normally serve as first respondents in most crimes.

Judges have traditionally based their decisions almost entirely on evidence presented by the MP in the Preliminary Investigation (*averiguación previa*). Most of the evidence came from confessions and witness statements that could normally be obtained without the defense attorney being present. Judges would exert no oversight over the investigative phase. Critics charge that this meant both that defendants had a limited right to challenge witnesses (Ely Yamin and Noriega Garcia, 1999) and that this system “biases the burden of proof against the defendant, since it falls to the defense team to rebut assertions of the MP compiled during the Preliminary Investigation.”⁵

In addition to formal injustices in the process, the judiciary systematically endorsed the use of torture to extract confessions. We summarize four jurisprudential theses⁶ noted in Magaloni et al. (2018) on criminal procedure and due process during the PRI period

⁴Previously Judicial Police

⁵Javier Dondé Maute quoted in Kingman-Brundage (2016).

⁶A jurisprudential thesis is a feature of Mexican law whereby the Supreme Court dictates how lower courts must interpret the law. Before the 1994 constitutional reform, it was necessary that the Supreme Court issued five separate decisions following similar logic to establish this kind of precedent (Sánchez et al., 2011).

that illustrate the problem. These Supreme Court rulings make explicit that although the 1917 Constitution established a series of procedural protections, in practice state officials and the police found due process rights easy to violate because courts gave full evidentiary value to confessions.

- The verification by the defendant of traces of physical maltreatment during detention does not invalidate a confession if it is corroborated by other evidence on file (Thesis 139-144, Seventh period, First Chamber, Federal Judicial Seminary, November, 1980, p. 36)
- A confession by a defendant before the Public Ministry should be valid even if it confirms that he or she was subject to a prolonged detention, since it should be assumed that the detainee, in the absence of proof to the contrary, was “in complete freedom to demonstrate each and every one of the circumstances related to the development of the criminal act” (Thesis 41, First Chamber, Federal Judicial Seminary, May 1972, p. 15)
- In regard to the lack of defense counsel during detention, the Court emphasized that “the Public Ministry could not be accused of denying a defendant representation, since it could not be proven that a detainee had not exercised that right” (Thesis 63, Seventh Epoch, First Room, Judicial Seminar of the Federation, March 1974, p. 23)
- If the defendant modified his statement before the judge, the evidence rendered to the Public Ministry should have greater probative value because it was the “most spontaneous” (Thesis XLIII, Sixth Period, First Chamber, Federal Judicial Seminary, January 1961, p. 37).

[Heisler et al. \(2003\)](#) survey state employed forensic physicians and find widespread reports of suspected torture backed by physical traces of prisoner abuse. In short, a democratic Mexico inherited a criminal justice system in which torture was common,

confessions were seen as evidentiary gold mines, where incarceration throughout the entire trial was the norm, and which demanded proof of innocence rather than proof of guilt. It is important to underscore that although most victims of torture were common criminals, governors possessed extraordinary capacity to use the criminal justice system as a political weapon. On the one hand, they could dispense legal immunity to their criminal associates, and on the other hand, they could convict anyone by fabricating evidence. The system operated in such a way that the rich, the powerful and the politically connected criminals would rarely get punished.

2.1 Transition to democracy

The PRI lost power in 2000. With the alternation of power and the onset of divided government in 1997, a system of checks and balances was consolidated. Moreover, a constitutional reform in 1994 granted the Supreme Court enhanced powers of judicial review. The Supreme Court became a real veto player, mostly serving as an arbiter of political conflicts among the states and the federation, but increasingly interpreting laws (Sánchez et al., 2011; Domingo, 2000). According to the literature, increased electoral competition, veto players, and freedom of expression should have led to a decline in judicial torture. Our approach stresses two reasons why the transition to democracy had limited effects on protecting human rights.

Unlike other democratization processes in the region, Mexico did not implement major security-sector reforms that constrained military and police behavior through civilian oversight and social accountability (Davis, 2006; Shirk and Cázares, 2007; Uildriks, 2010; Trejo and Ley, 2018). The main institutional reforms negotiated during the transition aimed to restrain electoral fraud and to equalize the electoral playing field (Eisenstadt, 2003). The criminal justice system was left mostly intact during the transition. Moreover, the 1994 constitutional reform left unchanged the institutions for the adjudication and interpretation of fundamental rights that had prevailed during the authoritarian era –

most notably the *Amparo trial*, a form of habeas corpus through which individuals can challenge laws or acts by state authorities that violate the constitution (Domingo, 2000).

A second reason why democracy in Mexico has failed to restrain torture is the sharp increase in criminal violence. There is considerable consensus in the literature on Mexico that alternation of political power in office provoked an increase in drug-related violence and public insecurity (Trejo and Ley, 2018; Osorio, 2015; Dube et al., 2013; Ríos, 2013). Moreover, in 2006 president Felipe Calderón of the National Action Party (PAN) started a war against DTOs, which further increased violence (Lessing, 2015; Dell, 2015; Calderón et al., 2015).

Calderón’s strategy to combat DTOs involved “joint operations” with thousands of soldiers and federal police sent to help local governments combat criminal groups. During his presidency more than 6,000 people accused of being involved in drug trafficking activities were arrested, as well as many cartel leaders and lieutenants (Phillips, 2015). The armed forces carried out many of these arrests. Magaloni et al. (2018) present empirical evidence that the drug war sharply increased the practice of torture in two scenarios: a) when the armed forces conducted detained a suspect, and b) when suspects were accused of drug trafficking. Methods used included electroshocks, waterboarding, suffocation, and stabbing. Other scholars have also argued that the Drug War increased human rights abuses (Escalante, 2011; Anaya, 2014; Silva Forné et al., 2012). Calderón’s security strategies received significant support from the public even as violence was increasing (Romero et al., 2016). According to survey evidence using list experiments, over a third of the voting age population endorsed the practice of torture when used “against drug traffickers” (Osorio, 2016).

2.2 Criminal Justice Reform

Human rights abuses dramatically increased during the Drug War. From October 2007 to October 2016, the National Registry of Missing People registered 28,937 forced dis-

appearances, leading to local NGOs organizing around families of the disappeared. The families were not only clamoring for an end to the recent upsurge in violence, but demanded the state respond with adequate investigations. As [Gallagher \(2017\)](#) explains, the involvement of activists and advocates of the families of the disappeared brought local authorities to investigate and prosecute some disappearances and homicides in a case by case basis.

Fewer local NGOs began to organize around the problems of torture and extrajudicial killings and executions. The Calderón government also faced significant accusations of human rights violations committed in criminal procedures from international organizations such as Amnesty International, Human Rights Watch, and the UN Human Rights Council. Pressures from these organizations played an important role in the 2008 criminal justice reform that president Calderón negotiated with Congress.

The reform would exempt the prosecution and indictment of organized crime offenses, defined by law as federal crimes committed by “three or more persons” from many of the protections. In exchange for granting virtual legal immunity to state actions in the Drug War, the reform set the framework for the transformation of the prosecution of criminal offenses not linked to organized crime. These criminal offenses are mostly under the jurisdiction of states and municipalities. The reform granted a grace period to the states for implementation, which required establishing the necessary infrastructure, including hiring and training new judges. This meant that the implementation of the reform would be postponed until after the Calderón presidency was over. In section 4, we discuss the details of how the reform was implemented. The political rationale behind the 2008 reform seems clear: the federal government sought to retain its leeway to combat organized crime, but it was costly to ignore mounting international pressure and complaints by local NGOs of human rights abuses. The reform aimed to relieve these pressures while retaining the federal government’s ability to combat DTOs without much legal restraint.

The reform was complex but we will cover some of the more salient features here.

It includes provisions specifically designed to limit the use of torture and ensure the protection of due process. The prosecution of a crime is now handled by a panel of three judges. The controlling judge (*juez de control*) has the obligation to evaluate the legality of the detention, order the release of individuals whose detention was not carried out in a manner adhering to the provisions of the law, and exclude illegally obtained evidence. The trial itself has the second judge presiding through the sentencing of the defendant. Throughout this judicial process, there are provisions that restrict evidence obtained by violations of due process rights from entering the record. The third judge oversees the execution of the sentence. The reform further added more procedural protections for defendants and instituted oral trials, allowing far greater opportunity for the defendant to challenge the prosecution's presentation of evidence. The reform introduced a series of due process rights and strict exclusionary rules in cases of abuse, which we detail in the Electronic Appendix. The reform created more judicial controls over criminal investigations by police and the MP. While it is generally believed that these reforms improved the protection of human rights, to our knowledge there are no prior studies documenting the causal effects of the change in type of criminal justice system on torture.

3 Interviews with police officers

This section reports on fieldwork conducted with police officers in the largest cities of Mexico: Mexico City, Monterrey, and Guadalajara.⁷ Interviews were collected with the commander in chief, supervisors, and street officers. Because of the sensitivity of the topic, all officers will remain anonymous and we will not report the name of the municipality or the place where the interview was conducted to protect our informants. Interviews were collected between the fall of 2017 and first six months of 2018. We spoke with 115 police

⁷Monterrey and Guadalajara are organized along municipal lines, which means that each city has as many preventive police units as its number of municipalities. In Monterrey we conducted interviews in 8 of the 13 municipalities that compose the metropolitan area. In Guadalajara we collected interviews in the two largest municipalities - Zapopan and Guadalajara. In Mexico City a single police force covers the entire jurisdiction.

officers individually or in a focus group format. Interviews were collected in a structured, semi-structured, and narrative approach. We never asked directly whether an officer had tortured someone. The interviews were geared toward understanding how the criminal justice reforms have changed incentives for police officers.

Police officers revealed that the reform has transformed the way police handle arrests. A police chief explained as he was showing us the detention cells in the municipal police headquarters:

We have installed cameras in this area. Everything that happens here is now recorded. We instruct street police officers that they should not bring criminals here anymore to interrogate them. Today judges easily deny the legality of an arrest for things such as taking a long time to bring a detainee to the MP because there is too much traffic. This can throw out the case. Part of the problem for us is that... the new system is too “garantista” [translates as too protective of human rights.] We advise police officers to not talk to arrested criminals, not to interrogate them anymore because we risk losing the cases when they do.

Police were used to taking suspects to the police headquarters rather than directly to the MP. Without a defense attorney, police would interrogate suspects using a variety of coercive measures to intimidate them and extract confessions. For many police corporations, it has taken time to change these coercive routines. A police chief in a different municipality explained:

We are making big efforts to train our police officers to work within the new criminal justice system... We also have hired a team of lawyers to assist street police officers fill in the *Informe Policial Homologado (IPH)*.⁸ We have learned not to use words such as “subjugate”, “handcuff”, “subdue”, because

⁸This is the document that police need to fill about the circumstances of the criminal incidence they are investigating or addressing.

these words are enough to throw out a case. But there are many cases when detainees walk free because police fail to follow the new rules.

A concern police officers reported is that with the new procedures judges frequently deny indictments and “suspects walk free” when police arrest without following the new protocols. Our interviews revealed that older police officers have a harder time adjusting their routines to the new system. A police chief in a different municipality told us:

Older police officers complain more about the new criminal justice system, they make more mistakes, and they resist more. They complain that the new system is paternalist, that it protects criminals, and that it excessively weakens the police as the strong link in the chain of crime. The problem is that many police officers don't know how to act in line with the new standards and they ruin the cases from the very beginning by doing things such as threatening or hitting suspects.

We observed wide variation in how the police are adapting to the new system. In some places the municipality has invested a great deal of resources in ongoing training of the protocols - how to arrest, secure a crime scene, fill in the Informe Policial Homologado (IPH), and participate in court hearings. But in other municipalities the police were caught utterly unprepared. Many police officers barely know what to do with a crime scene or how to secure evidence, let alone how to arrest someone without violating due process.

Our interviews revealed that part of the reason police officers perform their work resorting to torture is that they get monetary bonuses for arresting and indicting people. A police officer explained to us:

Here we get monetary bonuses for arresting. But it is necessary to get an indictment after the arrest. If you have a confession the judge would for sure give you one. And even better if you have a witness declaration. But the new

criminal justice reform makes it too hard for us to interrogate. How are we to offer evidence?

These interviews reveal a disturbing path dependency – institutions that began relying on coerced confessions never invest in investigative capacity, which, when combined with incentives to obtain evidence for an indictment, leads to more abuse as it is the only way the institution can close a case. That superiors rewarded police officers for “solving” homicides in this manner was confirmed in other interviews, including with the Secretary of Security of Mexico City. The secretary explained that with the new criminal justice system they needed to design a different bonus system to reward police officers because “the current one is not compatible with the new realities established by the criminal justice reform.” Similar incentives are present for investigative police officers. In an extensive report by *Animal Politico* on homicide investigations,⁹ police officers interviewed explained that superiors would demand a number of “solved” murders a month. They also agreed that confessions or witness testimony are the only forms of evidence that can clear a case.

4 Data

We distinguish between two different kinds of torture derived from the ENPOL. We examine what we call “brute force.” To construct our measure of brute force, we use two questions – whether the individual was beaten or kicked and whether the individual was beaten with objects. Responding affirmatively to one of the questions constitutes brute force torture.¹⁰ We contrast this with what we term “institutional torture.” We take this as torture which requires a dedicated space, equipment, or training to carry out effectively, contrasted with beatings, which can happen anywhere. Because this kind of torture requires physical and human resources in the form of space, specialized

⁹<https://www.animalpolitico.com/kill-murder-mexico/>

¹⁰The wording for these questions is provided in the Electronic Appendix.

equipment, and some degree of training to avoid killing the victim,¹¹ we believe that it requires some kind of institutional endorsement and support to take place, either in the cells of police station, the prosecutor’s headquarters, or a clandestine detention center. We operationalize this concept by using questions about whether a respondent was crushed with a heavy object, electrocuted, suffocated or drowned, burned, or stabbed while in custody. If the prisoner responds that he was subject to one of these five abuses, he is coded as having been subject to institutional torture. Finally, we include reports of threats by authorities either to press false charges or to harm a detainee’s family.

The survey asks about these experiences both before the police brings the suspect to the MP and at the MP. We thus have the following measures of violence and intimidation:

1. Brute force
2. Institutionalized torture
3. Threats

We merged the survey data with a dataset compiled from announcements of the incorporation of the accusatory penal system and the national code of criminal procedure into state legal systems. There were three ways states updated their systems. States (a) created a timetable whereby the reform would enter effect in specific geographic units (judicial districts or the entire state) on a certain date,¹² (b) created a timetable whereby the reform would begin covering certain classes of crimes on a given date, or (c) chose some combination of the two. Our analysis only focuses on states that followed the geographic-units mode of reform.¹³ Any individual arrested on or after the implementation date in a given municipality is considered to have been arrested under the new system. We report all of the dates in Table A1 in the Electronic Appendix.

¹¹It is for this reason that governments often employ medical professionals in clandestine torture centers. See, for instance, the use of doctors by the Argentine military during the Dirty War or by the CIA in its post-9/11 torture program.

¹²This is the majority of states.

¹³We do not have fine-grained enough information about the crimes respondents were accused of to match them against the specificity of the statutes covered by the implementation decrees for categories (b) and (c).

Table 1: **Rates of different categories of torture reported by state detainees**

Type of abuse	Before public ministry	At public ministry	At both
Brute force			
Beatings	56.62	39.00	33.69
Beatings with objects	36.82	23.48	18.74
Institutionalized torture			
Crushed with heavy objects	35.01	23.21	18.36
Suffocated or drowned	34.29	25.32	20.29
Electric shocks	18.92	14.44	10.71
Burned	6.07	4.52	2.89
Stabbed	3.68	2.37	1.19
Threats			
False charges	50.29	41.01	32.85
Harming family	26.01	5.35	14.16
Other abuses			
Held incommunicado	55.24	47.47	38.34
Stripped	43.88	37.89	29.09
Tied	38.51	28.93	22.99
Blindfolded	37.29	26.78	21.94

Note: Data are only for prisoners at state detention centers and exclude federal prisoners.

4.1 Abuse in the Mexican criminal justice system

Table 1 reports different forms of abuse prisoners experienced. The table distinguishes between reported abuses before the prisoner arrived at the MP and abuses at the MP.¹⁴ Notably, violent forms of institutionalized torture like electrocution and suffocation or drowning are alarmingly common. These forms of institutionalized torture are slightly more common before the suspect arrives to the MP, and probably take place either in a clandestine detention center or at the police headquarters. Many prisoners report other kinds of abuses, including being held incommunicado, stripped, restrained or tied, or blindfolded. The data also suggests many prisoners are subject to abuses both before and after arriving at the MP. Since it might be they have poor recollection of where exactly torture and threats took place, we will not distinguish in our models whether abuse took place before or after the MP.

¹⁴The Appendix also presents a broad range of descriptive statistics, including demographics of the prison population.

Table 2: **Outcomes comparing democracy and criminal justice reform**

	Avg, all pre-reform	Avg, all post-reform	Difference	p
Brute Force	67.04%	42.26%	-21.79	0.00
Institutional Torture	55.55%	34.66%	-20.89	0.00
Threats	64.62%	45.60%	-19.02	0.00
Total No.	30,548	2,778		
	Avg, pre-alternation	Avg, post-alternation	Difference	p
Brute Force	66.55%	62.71%	-3.84	0.00
Institutional Torture	54.58%	50.53%	-4.04	0.00
Threats	63.77%	62.07%	-1.7	0.01
Total No.	20,330	30,963		

Table 2 classifies abuses into the three categories that will be used for the empirical analyses, Brute Force, Institutional Torture, and Threats. The data corresponds to arrests in those states where we are examining the reform across the entire period before the implementation of the reform and the entire period afterwards. The comparison of the entire pre- and post- reform periods shows a dramatic decline in the occurrence of these abuses. The table also reports a comparison of the arrests performed in states before the PRI lost power for the first time, which we define as the onset of local democratization, and arrests after that date. The statistical analyses in subsequent sections aim at demonstrating the causal effects of the criminal justice reform on torture. We will also explore the effects of local democratization.

In terms of how truthful prisoners' responses might be, we note that although prisoners could have incentives to exaggerate, their answers are inconsequential for their convictions. However, it is important to mention that although the majority of respondents had been convicted, those who were in prison waiting for a sentence could have interpreted differently and might have more incentives to lie. Table 3 shows the conviction status of the prison population: around 70% had been convicted at the time of the survey, 29% were waiting for a sentence, and 2% were "partially" sentenced. The table also shows reported abuses by each of these groups. Propensities to report abuse are almost the same among convicted and not convicted prisoners, which suggest that responses are not strategically given in anticipation of how they might influence a conviction. However, the "partially sentenced" show a significantly higher propensity to report abuses. As a

Table 3: **Conviction status and reported abuses**

	Brute Force	Int. Torture	Threats	(Num)
Not Sentenced	65.37	54.97	65.55	9,746
Sentenced	64.72	52.78	61.46	22,785
Partly Sentenced	81.93	73.63	81.87	701
(Num)	20,906	17,231	20,179	

Note: Data are only for prisoners at state detention centers and exclude federal prisoners.

robustness test, we include this variable in our matching routines and it does not affect the results.

4.2 The correlates of torture

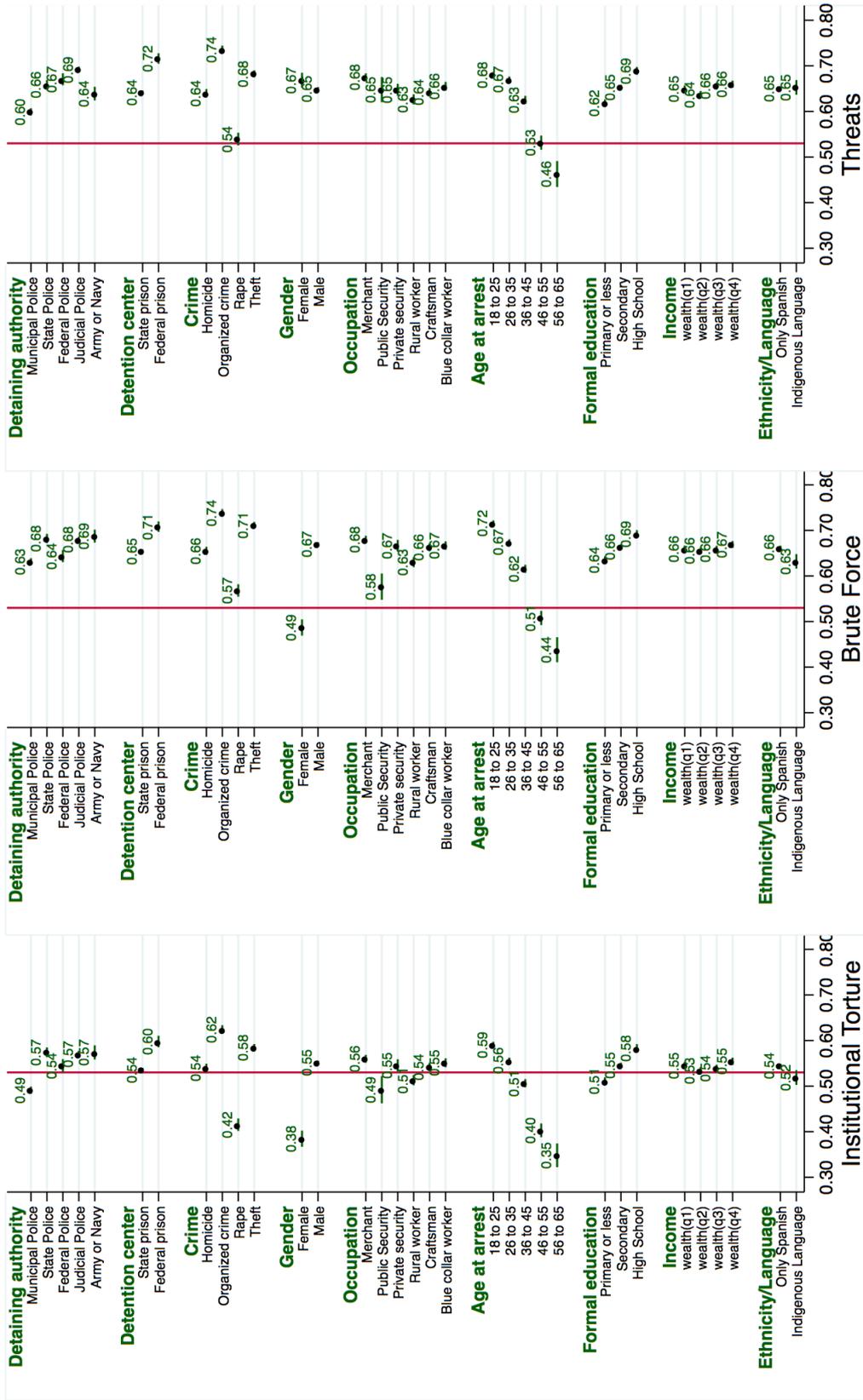
In this section we explore the correlates of torture with the use of OLS.¹⁵ Our dependent variable corresponds to three forms of abuse, Brute Force, Institutional Torture, and Threats, as defined above. Each is a binary indicator for whether a respondent reported that kind of abuse. The model specification is as follows:

$$y_{ij} = \alpha + \sum_a \beta_a A_{ia} + \sum_k \delta_k X_{ik} + \gamma_i + \lambda_i + \alpha_t + \epsilon_{ij} \quad (1)$$

where y_i is the level of abuse reported by the prisoner i in municipality j . A is the authority that detained the individual. The model also includes k individual covariates, the crime for which the prisoner was convicted, γ_i , and state-level and time fixed effects (λ_i and α_t , respectively). Descriptive statistics for all the variables are provided in the Electronic Appendix, Tables A4 and A5. Results are shown in Figure 1. The models suggest that if the prisoner was arrested by the municipal police her or she is significantly less likely to be tortured than if arrested by the state, judicial or federal police. Federal prisoners are subject to more abuse than state prisoners. Those accused of organized crime are significantly more likely to be tortured. Moreover, prisoners accused of theft

¹⁵We use OLS given that we will present state or municipal fixed effects for each of our models, which have generated criticism when used in logistic regressions. The Electronic Appendix presents all our models using logits and the results hold.

Figure 1: Correlates of torture



Notes: The figure shows predicted rates of different forms of abuse, and their 95% confidence intervals from OLS models, one for each form of abuse. Full tables of coefficients are shown in the Appendix. All models include state fixed effects and also fixed effects for the year of the arrest and are calculated with heteroskedasticity robust standard errors.

are almost as likely to be tortured as those accused of organized crime,¹⁶ revealing in our view a perverse practice by the police that sanctions property crimes and poverty more than other crimes such as homicide or rape.

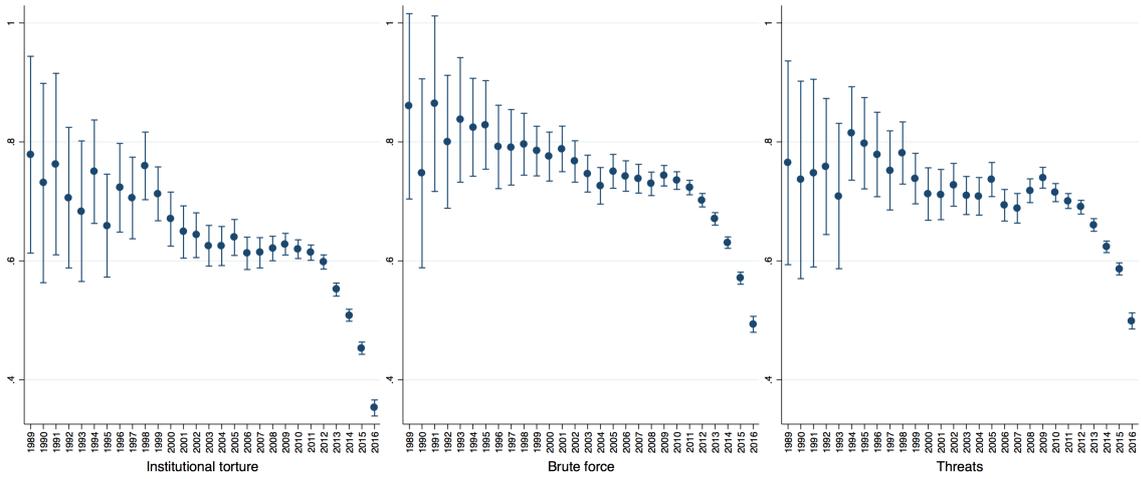
In terms of individual-level controls we use sex, occupation, age at arrest, education, ethnicity, and an index of wealth.¹⁷ The results show that men are significantly more exposed to institutional torture and brute force. Women are significantly more subject to threats. In terms of occupation, prisoners who worked for public security (police officers and members of the armed forces) are significantly less exposed to torture and brute force. Rural workers report less torture and brute force than the rest the occupations. Age is the most powerful socioeconomic correlate of police abuse, with younger arrestees significantly more likely to be subjected to abuse. Those with high school education report more abuse than those with less education. The wealth index does not seem to impact abuse. Those who speak an indigenous language report less physical abuse but the same level of threats.

From the statistical models presented in Figure 1 we plot the predicted rates of abuse by the year of arrest. As shown in Figure 2, the average predicted rates of institutional torture, brute force and threats are well above 60-70% as a baseline until about 2013, after which these abuses begin to drop precipitously by 2016. These declines coincide with the implementation of criminal justice reform. The sections below offer a series of statistical test to show the causal effects of the criminal justice reform on the drop of torture and rampant police abuse.

¹⁶Organized crime is operationalized as extortion, possession of illegal weapons, drug possession, or drug commerce.

¹⁷The index is constructed from a series of seven questions that ask whether an individual had sufficient money for food, clothes, and medical care as well as whether they had debts, needed to work seven days a week, and had the ability to spend extra money on themselves. For the analysis we subdivide the index into four quartiles.

Figure 2: **Year of arrest and types of abuse**



Notes: The figure shows predicted rates of different forms of abuse by the year of the arrest, and their 95% confidence intervals from three OLS regressions, one for each form of abuse. Full table of coefficients is shown in the Appendix. All models include state fixed effects and sociodemographic controls.

5 Empirical strategy to test for causal effects

Estimating the causal effect of the criminal justice reform on torture is complicated by the fact that unobserved factors may simultaneously lead to the implementation of the reform and affect torture. Our strategy for overcoming this identification challenge relies on two approaches. First, we rely purely on within-unit variation. We use state or municipal-level fixed effects to hold constant time-invariant characteristics about the local police organizations. Second, we use fixed effects for the year of the arrest. These are essential to capture national policy effects (e.g., electoral competition, important events in the Drug War, etc.). While the fixed-effects models draw on within state or municipality variation to identify the effect of the criminal justice procedures, we still have to account for time-varying confounding variables. We thus restrict the analysis to arrests six months before the reform and six months after the reform. We do this to limit the possibility of time-varying confounding factors driving our findings, seeking to identify the direct impact of the immediate change in criminal procedure. We will also report a range of robustness checks.

5.1 Model Specification

We use a difference-in-difference empirical strategy to identify how the change of an inquisitorial to an adversarial criminal justice system impacts torture. We define as subject to the reform any individual arrested in a municipality after the new code of criminal procedure took effect. Because we are focusing on the state level reform, this section excludes federal prisoners from the analysis. The model specification is as follows:

$$y_{ij} = \alpha + \beta_1 T_i + \sum_k \delta_k X_{ik} + \lambda_i + \alpha_t + \epsilon_{ij} \quad (2)$$

where y_i is the level of abuse reported by the prisoner i in municipality j and T_i is an indicator variable for treatment. The model also includes k individual covariates and municipal-level and time fixed effects (λ_i and α_t , respectively). The main text presents the results using OLS and in the Electronic Appendix we provide logit models. Any individual arrested in a municipality after the new code of criminal procedure took effect is treated. In the states we are studying, the reform was implemented on 65 different dates spanning the period from 2014 to June 2016. In terms of individual-level controls we use the same socio-demographic variables as in Figure 1.

5.2 Results

Results of the regression models are provided in Table 4. Our first set of models use data from the entire period. Our second set of models restrict the sample to six months before and six months after the reform. In both cases, we observe statistically significant declines of the incidence of torture with the implementation of the reform. Of these, all the regressions show negative and significant associations of our indicators of abuse with the post-reform period. In the six month tests, the probability of a prisoner experiencing institutional torture falls by approximately 6 percentage points, the incidence of brute force falls approximately 5 percentage points, and the incidence of threats falls by 6 percentage points. These declines are meaningful, especially considering the short period

Table 4: **Effects of Criminal Justice Reform: OLS Regressions**

	Torture M1	Brute M2	Threats M3	Torture M4	Brute M5	Threats M6	Torture M7	Brute M8	Threats M9
All pre-reform/post-reform									
Reform	-0.0916*** (0.0115)	-0.128*** (0.0116)	-0.108*** (0.0118)	-0.0575*** (0.0124)	-0.0731*** (0.0123)	-0.0586*** (0.0126)	-0.0845*** (0.0121)	-0.106*** (0.0121)	-0.0951*** (0.0123)
Constant	0.595*** (0.2255)	0.608*** (0.2334)	0.506** (0.2437)	0.395* (0.2123)	0.409* (0.2206)	0.353 (0.2363)	0.564** (0.2225)	0.565** (0.2287)	0.456* (0.2358)
N	31931	31961	31926	31931	31961	31926	31931	31961	31926
Six months pre-reform/post-reform									
Reform	-0.0604*** (0.0154)	-0.0784*** (0.0156)	-0.0668*** (0.0158)	-0.0510*** (0.0167)	-0.0412** (0.0167)	-0.0521*** (0.0170)	-0.0622*** (0.0170)	-0.0667*** (0.0170)	-0.0617*** (0.0172)
Constant	0.215 (0.1349)	0.434*** (0.1358)	0.644*** (0.1417)	0.201 (0.1298)	0.393*** (0.1294)	0.696*** (0.1354)	0.117 (0.1474)	0.403*** (0.1474)	0.549*** (0.1520)
N	4481	4488	4479	4481	4488	4479	4481	4488	4479
Testing for anticipation effects									
Reform announced	0.0399** (0.0129)	0.00861 (0.0126)	0.0251* (0.0127)	0.00179 (0.0134)	-0.00972 (0.0130)	0.00919 (0.0133)	0.00373 (0.0138)	-0.00903 (0.0134)	0.0121 (0.0137)
Reform	-0.0557*** (0.0163)	-0.120*** (0.0163)	-0.0852*** (0.0165)	-0.0559** (0.0172)	-0.0820*** (0.0171)	-0.0502** (0.0175)	-0.0434* (0.0180)	-0.0705*** (0.0178)	-0.0466* (0.0182)
Constant	0.594** (0.225)	0.608** (0.233)	0.505* (0.244)	0.396 (0.212)	0.408 (0.221)	0.355 (0.236)	0.461 (0.235)	0.496* (0.253)	0.398 (0.227)
N	31931	31961	31926	31931	31961	31926	31931	31961	31926
State FE				Y	Y	Y			
Municipal FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: The rows show estimated coefficients for the criminal justice reform. Models in the upper rows use data for all prisoners. Models in the lower rows restrict the universe to prisoners arrested six months prior and six months after the criminal justice reform. All models include socio-economic characteristics and year fixed effects. Heteroskedasticity robust standard errors in parenthesis. *** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.1$.

of time we are examining. We present a last set of models that test whether there is a detectable effect of jurisdictions anticipating the reform's implementation and adjusting their behavior accordingly.¹⁸ Each state legislature published official declarations announcing the timetable for the reform's implementation, mostly in 2014. We constructed a variable to indicate the period after the reform's timetable was first announced but before the reform was actually implemented. This means we then have a sample divided into the pre-reform and pre-announcement era, the post-announcement and pre-reform era, and the reform era. When we ran these models, we found there were no anticipation effects.

¹⁸The full table and an analogous set of logits are in Appendix Tables A7-A10 and A12-A13.

Table 5: **Placebo tests for the criminal justice reform**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Torture	Brute	Threats	Torture	Brute	Threats	Torture	Brute	Threats
Two years									
Reform	0.0047 (0.0147)	0.0124 (0.0137)	0.00665 (0.0142)	0.0026 (0.0147)	0.0107 (0.0137)	0.00454 (0.0142)	0.00881 (0.0153)	0.0139 (0.0144)	0.00855 (0.0149)
Constant	0.685*** (0.2082)	0.881*** (0.0645)	0.795*** (0.2264)	0.420* (0.2279)	0.609*** (0.0990)	0.654*** (0.2427)	0.812*** (0.2348)	0.966*** (0.0897)	0.883*** (0.2418)
N	4736	4735	4735	4736	4735	4735	4736	4735	4735
Three years									
Reform	-0.00797 (0.0172)	-0.00067 (0.0159)	-0.0155 (0.0163)	-0.00801 (0.0174)	-0.000969 (0.0163)	-0.0154 (0.0166)	-0.0047 (0.0181)	0.0000231 (0.0168)	-0.0201 (0.0174)
Constant	0.574** (0.2320)	0.565** (0.2284)	0.851*** (0.2546)	0.297 (0.2529)	0.377 (0.2488)	0.641** (0.2781)	0.682*** (0.2501)	0.647*** (0.2461)	0.912*** (0.2733)
N	3479	3489	3478	3479	3489	3478	3479	3489	3478
Four years									
Reform	0.00882 -0.019	0.00431 -0.0179	0.0222 -0.0186	0.00321 -0.0193	0.00624 -0.0182	0.0192 -0.019	0.0106 -0.0205	0.00038 -0.0192	0.008 -0.0199
Constant	0.256*** -0.0872	0.139* -0.0842	1.168*** -0.086	0.0599 -0.1401	0.117 -0.1327	1.042*** -0.1404	0.333*** -0.1247	0.159 -0.1222	1.246*** -0.1229
N	2796	2791	2795	2796	2791	2795	2796	2791	2795
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: The rows show estimated coefficients for placebo tests of the criminal justice reform. The tests move the start dates 2, 3 and 4 years before the actual dates. All models include socio-economic characteristics and year fixed effects. Robust standard errors in parenthesis. *** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.1$.

5.3 Robustness checks

We implemented additional placebo tests in which we artificially move the date of the implementation of the reform beyond our “announcement period” to two, three, and four years prior to the actual implementation and subset the data to six month buffers around these faked reforms. We repeat the specifications from the main model. We report the coefficients on the artificial reform variable in Table 5, where we find no significance across all 27 models.

We rerun our tests of the six month buffer around the reform using coarsened exact matching on background characteristics to ensure balance in covariates we think may be related to the outcome variables (Blackwell et al., 2009). The results are reported in Table 6. Attempting to include too many covariates at once induces a severe dimensionality

problem. For transparency, we run the matching routine three times. First, we match exactly on the respondent’s level of education and whether or not the respondent speaks an indigenous language and run coarsened matching on the respondent’s value in the wealth index. Second, we match exactly on crimes committed. Third, we match on both sets of variables and add a variable that captures the individual’s sentencing status at the moment he was interviewed. Across all matching routines we retain negative and significant coefficients on the reform.

Table 6: **Matching and criminal justice reform**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threats	Brute	Torture	Threats	Brute	Torture	Threats
Reform	-0.0786*** (0.0169)	-0.0651*** (0.0170)	-0.0754*** (0.0170)	-0.0689*** (0.0159)	-0.0534*** (0.0157)	-0.0605*** (0.0159)	-0.0721*** (0.0214)	-0.0577** (0.0216)	-0.0623** (0.0216)
Constant	0.595*** (0.00981)	0.456*** (0.00984)	0.578*** (0.00987)	0.569*** (0.00902)	0.430*** (0.00894)	0.554*** (0.00905)	0.605*** (0.0125)	0.459*** (0.0126)	0.569*** (0.0126)
N	3817	3810	3806	4479	4472	4470	2352	2352	2350
Matching on:	Background	Background	Background	Crimes	Crimes	Crimes	All	All	All

Note: Data are only for prisoners at state detention centers and exclude federal prisoners. We only include individuals arrested within a six month buffer of the reform’s implementation. Models 1-3 match on wealth index, education level, indigenous language fluency, and sex. Models 4-6 match on whether the individual was arrested for homicide, theft, or crimes likely related to organized crime. Models 7-9 match on all the variables as well as whether or not the individual had already been sentenced for all crimes at the time the survey was taken. Robust standard errors in parenthesis. *** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.1$.

6 Democratization

Theoretical work suggests that democracy can play a role in limiting the prevalence of torture. While the PRI lost power nationally in 2000, it had already lost control of many governorships to opposition parties and it also held onto some governorships past national democratization. An important question is if alternation of political party at the local level is associated with reductions of torture. We perform a range of tests for the effects of local democratization. First, we assess how prisoners were treated in states where the PRI never lost power in the entire period under study. These correspond to Campeche, Coahuila, Colima, Hidalgo, and Estado de Mexico. We add a dummy variable that takes the value of 1 for arrests in these states and 0 otherwise.

Secondly, we analyze if alternation of political power at the gubernatorial level impacts torture. We add a dummy variable that takes the value of 1 if a prisoner was arrested in a state after the PRI lost power for the first time and 0 otherwise. Table A18 in the Electronic Appendix provides a list of the states and dates where the PRI lost power for the first time and when the opposition took office. 65% of the prisoners were arrested under democracy, or after the PRI had lost power for the first time. As before, we use state fixed effects to hold constant state-level time-invariant characteristics. We also use fixed effects for the year of the arrest due to possible secular improvements in the quality of police and criminal justice with the passing of time. To make sure the criminal justice reform doesn't confound the effects of local democratization we truncate the data until 2013, when the first states began to implement the reform. Mexican governorships last for nonrenewable six year terms. In a third set of models, we exploit this to run models that examine six year buffers around the initial alternation of power. Finally, we run a placebo check in which we artificially move the date of the first alternation back by six years, effectively examining the PRI's final term in office as the hegemonic party.

The models for local democratization are presented in Table 7. The results suggest that prisoners arrested in states where the PRI never lost power are subject to more institutional torture, brute force, and threats. With the exception of models 5 and 6 on brute force and threats using state-fixed effects, all models produce a positive and statistically significant coefficient for the dummy variable indicating that arrests took place in a state where the PRI has never lost power. The models for alternation of political power demonstrate that prisoners arrested after the first alternation of political power are subject to less abuses than those arrested before. Table 7 suggests that the results are robust to restricting the sample to one term before and one term after the first PRI's loss of power. The last set of models presents placebo tests by artificially moving the first alternation of political power in each state by one gubernatorial term. None of the placebo results show statistically significant declines.

A question that emerges from these results is why, over the longer run, democracy in

Table 7: Effects of local democratization: OLS regressions

	Torture M1	Brute M2	Threats M3	Torture M4	Brute M5	Threats M6	Torture M7	Brute M8	Threats M9
PRI never lost									
Never	0.0610*** (0.0079)	0.0706*** (0.0072)	0.0525*** (0.0074)	-0.123*** (0.0270)	-0.0291 (0.0246)	-0.0377 (0.0238)	0.0611*** (0.0100)	0.0644*** (0.0093)	0.0372*** (0.0095)
Constant	0.568** (0.2745)	0.562** (0.2805)	0.667** (0.2785)	0.847*** (0.2547)	0.728*** (0.2709)	0.831*** (0.2729)	0.615** (0.2658)	0.576** (0.2781)	0.704** (0.2897)
N	26358	26395	26362	26358	26395	26362	26126	26162	26132
Alternation									
Dem	-0.0370*** (0.0064)	-0.0238*** (0.0060)	-0.0108* (0.0061)	-0.0359** (0.0146)	-0.0305** (0.0137)	-0.0414*** (0.0141)	-0.0465*** (0.0075)	-0.0272*** (0.0070)	-0.0107 (0.0072)
Constant	0.567** (0.2738)	0.564** (0.2796)	0.670** (0.2778)	0.857*** (0.2578)	0.737*** (0.2740)	0.843*** (0.2784)	0.617** (0.2669)	0.579** (0.2782)	0.705** (0.2896)
N	26358	26395	26362	26358	26395	26362	26126	26162	26132
Alternation (one term)									
Dem	-0.0664*** (0.0117)	-0.0389*** (0.0111)	-0.0353*** (0.0114)	-0.0657*** (0.0254)	-0.0575** (0.0242)	-0.0854*** (0.0248)	-0.0753*** (0.0134)	-0.0517*** (0.0127)	-0.0419*** (0.0130)
Constant	0.879*** (0.0552)	-0.0889 (0.0544)	0.985*** (0.0549)	0.410*** (0.1328)	-0.414*** (0.1209)	0.589*** (0.1217)	0.876*** (0.1266)	-0.1 (0.1194)	1.034*** (0.1183)
N	7614	7628	7614	7614	7628	7614	7553	7567	7555
Alternation (placebo)									
Dem	0.0142 (0.0178)	0.0195 (0.0170)	0.0495*** (0.0176)	-0.00632 (0.0309)	-0.0124 (0.0298)	0.0164 (0.0305)	-0.0232 (0.0203)	-0.0126 (0.0192)	0.0231 (0.0200)
Constant	0.853*** (0.0562)	0.841*** (0.0551)	0.476 (0.3525)	0.837*** (0.3002)	0.682** (0.3162)	0.148 (0.4414)	0.886*** (0.1805)	0.891*** (0.1454)	0.715* (0.4340)
N	5162	5177	5163	5162	5177	5163	5114	5128	5116
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Estimated coefficients for alternation of political power at the local level. PRI never lost is a dummy variable that takes the value of 1 to arrests in states where this party never lost power during the period of study and 0 otherwise. Alternation is a dummy variable that takes the value of 1 for arrests that took place after the PRI lost power and 0 for arrests before the PRI lost power in a gubernatorial election for the first time. Models with Alternation (one term) restrict the sample to one term before/after the PRI's first power loss. Alternation (placebo) artificially moves the date of alternation by one term or six years before the actual democratization date. All models include socio-economic characteristics and year fixed effects. Robust standard errors in parenthesis. *** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.1$.

Mexico failed to significantly reduce torture, as revealed in Figure 2. To explore how the accumulation of more years of local democracy shapes torture, we interact alternation with the party that first defeated the PRI in a given state: the right-wing National Action Party (PAN), the left-wing Party of the Democratic Revolution (PRD) or a coalition of these two (PAN-PRD). As before, we restrict the sample to arrests one term before/after the PRI's first power loss and hence measure the short term effects of local democratization. We then re-run the model adding all arrests after the PRI's first loss of power. This second model captures the long-term effects of democratization. The full interactive models are presented in the Electronic Appendix. Here we present in Table 8 marginal predicted probabilities focusing on institutional torture only.¹⁹

Table 8: **Political party that first defeated the PRI and institutional torture**

Before PRI's first defeat				After PRI's first defeat		
Coef.	95% Conf. Interval			Coef.	95% Conf. Interval	
Short term effects (1 term before/after)						
0.57	0.54	0.61	PAN	0.49	0.45	0.51
0.62	0.59	0.66	PRD	0.56	0.53	0.59
0.58	0.56	0.60	PAN-PRD	0.53	0.50	0.55
Long term effects (All before/after)						
0.55	0.52	0.58	PAN	0.60	0.59	0.62
0.61	0.58	0.64	PRD	0.46	0.45	0.48
0.59	0.57	0.60	PAN-PRD	0.58	0.56	0.60

Notes: Estimated marginal effects for Alternation of political power and political party that first defeated the PRI in a gubernatorial election. The coefficients for the models are provided in Table 14 in the Electronic Appendix. Short term effects correspond to margins comparing arrests one term before and one term after the PRI's first loss of power. Long term effects are margins for all arrests before and after the PRI first lost power in a given state. The data is truncated at 2013 so as not to confound the effects of the criminal justice reform. All models include socio-economic characteristics and are calculated using year fixed effects and robust standard errors.

Columns 1-3 display the predicted values of institutional torture for arrests before the PRI lost a governorship for the first time and their 95% confidence intervals. Column 4 reports the party that first defeated the PRI. Columns 5-7 display predicted values for arrests after the PRI lost power to each of these parties. Predicted values in the upper rows show the short-term effects of alternation of political power, and those in the lower rows show how much torture changed as more years of democracy accumulated.

¹⁹All these models eliminate arrests after 2013 so as not to include the effects of the reform.

The difference between values in columns 1 and 5 reflect how much torture was reduced after the PRI lost power. The short-term effects suggests that institutional torture significantly decreased regardless of whether the PAN, PRD or a coalition defeated the PRI. The long-term effects suggests that as the number of years with democracy increases, the probability of being tortured in a state where the PRD first unseated the PRI continues to drop to 46%, whereas in states where the PAN first defeated the PRI or where a coalition did, these probabilities are 60% and 58%, as high as when the PRI hadn't lost power for the first time. To explain why torture might have increased in some states as the number of years of democracy accumulated, the next section focuses on the Drug war.

7 The Drug War

Although local police don't have jurisdiction over organized crime, they are the first respondents and often are involved in these arrests. The literature has argued that PAN-controlled governors had a stronger mandate to support Calderón's security strategies than non-PAN governments (Trejo and Ley, 2016; Duran Martinez, 2018). To test if PAN governments responded differently to the drug war, we created an indicator of the party of the governor of the state where the prisoner was arrested and interacted this with a dummy that indicates whether the arrest took place after 2006. We use data from before 2014 to not include the effects of the drug war with the criminal justice reform.

A second model explores the role of security interventions rather than party in control of the local government. The federal government deployed the armed forces, marines and federal police to provide assistance with security to local authorities. The date on which these joint operations started is presented in the Electronic Appendix. We consider a prisoner "treated" by this security intervention when he or she was arrested after the joint operation took effect in a given state. We add indicators of the level of organized crime threat. We define a municipal-level "turf war" as an increase in violence of more

Table 9: **Institutional torture and the drug war**

	Torture M1	Torture M2	Torture M3	Torture M4	Organized crime M5
Party in power					
PAN	-0.0739*** (0.0134)	-0.0631*** (0.0194)			
PRD	0.0178 (0.0199)	0.0956*** (0.0237)			
IND	-0.0226 (0.0420)				
PRI x Drug war		0.0343 (0.0421)			
PAN x Drug war		-0.047 (0.0441)			
PRD x Drug war		0.0162 (0.0467)			
Federal Interventions					
Joint Operations			0.0838*** (0.0134)	0.0825*** (0.0141)	0.123*** (0.0101)
Turf wars				0.0368*** (0.0104)	0.0258*** (0.0087)
Federal prisoner	0.176*** (0.0081)	0.174*** (0.0081)	0.173*** (0.0080)	0.191*** (0.0089)	0.424*** (0.0094)
Constant	0.819*** (0.2582)	0.764*** (0.2660)	0.834*** (0.2036)	0.758*** (0.0484)	0.0228 (0.0324)
N	24395	24395	25713	23711	24608
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Notes: Estimated coefficients from OLS regressions. Models 1 to 4 use institutional torture as dependent variable. Model 5 uses organized crime arrests. All models include socio-economic characteristics. Robust standard errors in parenthesis. *** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.1$.

than three standard deviations relative to the municipality's historic mean. We use homicides of males aged 18 to 39 since 1990. The data comes from the National System of Health Information and is based on individual death certificates.

The results for these models are presented in Table 9. The evidence doesn't support the notion that torture increased disproportionately in PAN-controlled states during the drug war. In fact, models 1 and 2 reveal that torture in PAN-controlled states is generally lower, although these differences tend to dissipate with the drug war. By contrast, the results demonstrate that increases in torture happened disproportionately where the federal government sent a joint operation. These security interventions had substantial effects: arrests where a joint operation took place are associated with an 8% increase in torture. Models 3 and 4 demonstrate that the effect of joint operations is robust to in-

Table 10: **Placebos for federal military interventions**

	(1)	(2)	(3)	(4)	(5)	(6)
	Inst. Torture	Inst. Torture	Inst. Torture	Org. crime	Org. crime	Org. crime
One year	-0.0201 (0.0357)			0.000954 (0.0248)		
Two years		-0.00551 (0.0295)			0.00192 (0.0190)	
Three years			0.00306 (0.0271)			0.0117 (0.0169)
Constant	0.374 (0.284)	0.374 (0.284)	0.374 (0.284)	0.223 (0.283)	0.224 (0.283)	0.224 (0.283)
<i>N</i>	6942	6942	6942	7253	7253	7253

Notes: In these tests we artificially move the dates of joint operations back by one, two, and three years. Data for these tests truncate all data at the beginning of the real federal intervention to avoid including data after the real treatment is applied. All models include socio-economic characteristics. Robust standard errors in parenthesis. *** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.1$.

cluding municipal-level turf wars, associated with a 3 percentage point increase in torture that is statistically significant. All models reveal that federal prisoners are approximately 20% more likely than state prisoners to have been tortured. The last model in Table 9 uses organized crime as dependent variable. The results reveal that joint operations are associated with a more than 40% increase in arrests for organized crime.

We present a series of robustness tests where we artificially move the date of joint operations back by one, two and three years. Data for these tests truncate all data at the beginning of the real federal intervention to avoid including data after the real treatment is assigned. We report the coefficients in Table 10, where we find no significant result.

8 Conclusion

In this paper we have traced the emergence in Mexico of a system of judicially sanctioned torture in the prosecution of common crimes. We show how the authoritarian era's legal structures embedded a disregard for the procedural rights of accused criminals that persevered through the democratic transition and how torture remained an endemic problem. Exploiting the staggered incorporation of a constitutional reform into state judicial systems allowed us to examine the effectiveness of institutional change in reducing

state abuses against a politically vulnerable social group. By increasing judicial checks on the prosecution and police, the new system produced consistent and substantive declines across different forms of police abuse. Interviews with police officers corroborated the result that the police see the new criminal justice system as a real constraint on their freedom of action.

Our study is relevant not only to understanding the process by which a country traversing a democratic transition can restrain its repressive institutions, but also the more general question of how to restrain abuses by police. Because this was abuse directed not at political dissidents but rather at those accused of common crimes, the lessons of Mexico are relevant to any country engaged in reforming and restraining abusive police forces. This bears on a debate about the extent to which personality traits in individual police officers or institutions drive the incidence of police abuse. We provide robust evidence that changes in criminal procedure can constrain police enough to lower the incidence of abuse of detainees. One set of theories of repression suggests that democratic pressure should constrain a democratic state's abuses. Yet this case operates less on the basis of social pressure operating through democratic institutions and more through an elite decision to adopt reforms and an institutional willingness to implement them. Moreover, Mexico provides a case in which democratization failed to impose the necessary restraints on the government's use of coercion, which allowed a democratic government under threat from organized crime to rely on torture. The paper demonstrates that the drug war and militarization of security was associated with substantive increases in torture. The federal government deployed the armed forces and the federal police to combat organized crime, and these interventions resulted in substantial increases in torture.

Our paper additionally signals avenues for further research. While we have thus far examined the effect of institutional changes on the agents of the institution in question, the behavior of the police structures the behavior of other members of society, including criminals. The question of whether and how criminal behavior changes in response to an

institutional overhaul of the police remains for investigation. Second, citizen attitudes about police abuse remain unclear though relevant to the broader question of whether restraints on state abuses against suspected criminals can be sustained over time. One may reasonably expect opposition to an abusive police force in a democracy – repression runs counter to democratic norms and any given individual may reasonably fear falling victim to abuse. Yet one might also expect individuals who have not had personal contact with the criminal justice system to be unaware of or indifferent to the suffering it engenders. Moreover, reforms that constrain police might plausibly lead people to associate the incidence of crime in their community, regardless of whether crime rates change, with new procedural protections built into the criminal justice system and engender a backlash. It is also important to trace the evolution of jurisprudence on procedural rights in criminal trials. Our interviews reveal that it is judicial zeal in enforcing procedural rights that has led police to change their behavior. Yet the process by which the law and its application has evolved in this context remains an area open to further investigation. Finally, while our paper has some optimistic conclusions about the prospect of restraining torture, we note that the reform justifies exempting many offenses by tying them to the threat of organized crime. Weaker procedural protections for these offenses and the greater likelihood of military involvement open the door to torture and abuse.

9 Appendix

In this appendix, we outline our procedures for building the variables we use for our analysis and translate some of the relevant portions of the new code of criminal procedure. Additionally, we present supplementary robustness checks, alternate specifications, and full regression tables for regressions which we truncated in the paper. This appendix will proceed as follows: first, we will translate the sections of the questionnaire that relate to torture and explain how we built our dependent variables. Then we provide descriptions of the data. The appendix also includes the dates of reform implementation, federal interventions during the Drug War, and political alternation. We also include regressions that examine the relationship between treatments and our outcomes. These come in two forms. First, we provide alternate specifications. We use OLS in our paper for regressions with binary outcomes. We do this to allow for ease of interpretation as well as to allow the use of municipality-level fixed effects. Here we present identical specifications using logits and all of our results hold. Second, we include a range of full regression tables and placebo tests that were referenced in the main text.

9.1 Torture questionnaire

The following questions were used to construct our measures of abuse. Identical questions were asked about experiences in two periods: (1) between the arrest and arrival at the Public Ministry and (2) at the Public Ministry. The options for answering each question were:

1. Yes (coded as 1)
2. No (coded as 0)
3. Not applicable (coded as missing)
4. Does not know (coded as missing)

5. No response (coded as missing)

(1) Beginning with your arrest but before your arrival at the Public Ministry, did the police or authority commit or permit one of the following situations to occur? (2) In all your time at the Public Ministry, did the ministerial police or authority commit or permit one of the following situations to occur?

1. You were threatened with false charges
2. You were pressured to denounce someone
3. You were threatened with harm to your family
4. You were held incommunicado or in isolation
5. You were stripped
6. You were restrained
7. You were blindfolded
8. They harmed your family
9. Your breathing was impaired (you were suffocated, asphyxiated, or your head was submerged in water)

(1) In all your time at the Public Ministry, did the ministerial police or authority commit or permit one of the following physical aggressions against your person? (2) In all your time at the Public Ministry, did the ministerial police or authority commit or permit one of the following physical aggressions against your person?

1. Kicks or punches
2. Beatings with objects (sticks, pistols, rifle butts or any other part of a firearm, clubs, canes, etc)

3. Burns
4. Electric discharges
5. The crushing of some part of your body with an object (injuries by crushing)
6. Injuries with some kind of knife or other sharp object
7. Injuries by discharge of a firearm
8. You were obligated by physical violence or threats to partake in a sexual activity you did not desire
9. Some other aggression

Subsequently, we categorize three different types of abuse, where each category is coded as a binary variable:

1. Brute force
 - Kicks or punches
 - Beatings with objects
2. Institutionalized torture
 - Burns
 - Electric discharges
 - Crushing
 - Injuries with a knife or sharp object
 - Suffocation, asphyxiation, or drowning
3. Threats
 - Threats of false charges
 - Threats of harm against family members

We constructed our dependent variables as follows:

1. $Abuse_{i,j} = \max report_{i,j}$ where we measure whether or not there was an incident of a category j of abuse any individual i suffered. That is, a hypothetical individual who was electrocuted and suffocated but not burned, crushed, or stabbed receives a 1 for institutionalized torture.
2. $Intensity_{i,j} = \sum_k report_{i,j,k}$ where we measure the intensity of a category j of abuse any individual i suffered as the sum of that individual's reports of abuse within that category. That same individual who was electrocuted and suffocated would be coded as a 2 for the intensity of institutionalized torture.

9.2 Excerpts, National Code of Criminal Procedure

To clarify the treatment being applied in this case, we translate sections of the National Code of Criminal Procedure that outline basic rights of defendants and codify a series of broad, explicit exclusionary rules. These exclusionary rules are embedded throughout the various stages of the process and apply to different actors in the legal system. They thus empower different authorities in the legal process to exclude coerced evidence or deny the legality of a detention. For context, the code introduces three judges for each case.²⁰ Another feature of the code to note is how explicitly it states that an individual must be presented to a judge immediately after arrest. In the paper we note that abuse before a suspect is presented to the Public Ministry is quite common. The new code of criminal procedure is addressing this procedural deficiency in the old system. Our interviews are consistent with the view that many judges aggressively enforce provisions allowing them to exclude evidence and liberate suspects in cases where the presentation is not immediate, even if it is (according to the police) a delay in good faith.

The first judge assigned to a case, the controlling judge (*juez de control*), is responsible for controlling the investigation through the indictment. The controlling judge has the

²⁰The third judge oversees the execution of the sentence in a manner somewhat similar to a parole board in the United States; we do not spend much time discussing that role.

obligation to evaluate the legality of the detention and, as we will show, the obligation to order the release of individuals whose detention was not carried out in a manner adhering to the provisions of the law. In order for a case to proceed, this first judge must find that there is sufficient legally obtained evidence to grant an indictment and, moreover, may order the exclusion of specific evidence from the trial. In our interviews with police officers, many complaints about the new procedures were focused on this phase of the process. The police are prohibited from arresting anyone without an order issued by the controlling judge. There are exceptions for individuals detained *in flagrante delicto* and specific circumstances that constitute “urgent cases.” These cases require immediate review of the arrest’s legality. As we will show, failure to adhere to the procedure for an arrest or to deliver the individual to the legal authorities promptly is sufficient grounds for administrative or criminal sanctions against the officers and explicitly necessitates an order granting the immediate release of the individual detained. The trial itself has the second judge presiding through the sentencing of the defendant. Throughout this judicial process, there are provisions that restrict evidence obtained by violations of due process rights from entering the record.

Article 113. Rights of the accused

The accused shall have the following rights:

- I. To be considered and treated as innocent until his responsibility is demonstrated;
- II. To communicate with a family member and his defender while detained, the Public Ministry being obligated to provide him all facilities to achieve this;
- III. To declare or remain silent with the understanding that his silence may not be used to his detriment;
- IV. To be assisted by his defender in the moment he gives his declaration, as in any other act and, previously, to confer privately with him;
- V. To be informed, in the moment of his detention as well as at his appearance at

the Public Ministry or the controlling judge, of the allegations against him and his rights as well as, when applicable, the motive for the deprivation of his liberty and the public servant who ordered it, exhibiting, as applicable, the order given against him;

VI. Not to be subject in any moment of the process to techniques nor methods that harm his dignity, induce or alter his free will;

...

XIII. To be presented before the Public Ministry or the controlling judge, as the case demands, immediately upon being detained or apprehended;

...

XV. Not to be presented to the community as guilty;

...

XVII. To obtain his liberty in the case that he has been detained when preventive prison or some other precautionary measure has not been ordered;

Article 149. Verification of *flagrante delicto* at the Public Ministry

In cases where the suspect is detained *in flagrante delicto*, the Public Ministry shall examine the conditions of the detention immediately after the person is in their custody. If the detention was not carried out in accordance with the provisions of the Constitution and this code, the person shall immediately be liberated and, as the case demands, disciplinary or criminal sanctions shall be considered.

Article 150. Urgent cases

After explaining the circumstances under which the Public Ministry may order the detention of a specific individual in an urgent case without going before the controlling judge:

The police officers who execute a detention order in an urgent case shall register the detention and immediately present the accused before the Public Ministry that issued

the order, who shall then procure that the accused be presented without delay before the controlling judge.

The controlling judge shall determine the legality of the Public Ministry's mandate and its compliance in controlling the detention. Violation of this provision shall be sanctioned in accordance with the applicable provisions and the detained person shall immediately be liberated.

Article 264. Exclusion of evidence

Any fact or evidence obtained through violation of fundamental rights shall be considered illicit proof, which shall be motive for its exclusion or nullification.

Article 346. Exclusion of evidence in oral argument

Once the evidence offered has been examined and the parties have been heard, the controlling judge shall order excluded from oral argument evidence that does not refer directly or indirectly to the object of the investigation or are not useful for the clarification of facts, as well as that in which one of the following is substantiated:

...

2. Having been obtained with a violation of fundamental rights

Article 357. Legality of evidence

Specifying once more the exclusionary rule in the context of the trial phase:

Evidence shall have no value if it has been obtained by means of acts violating fundamental rights or if it was not incorporated into the trial in accordance with the provisions of this Code.

9.3 Sociodemographic characteristics

This section provides sociodemographic characteristics of the sample, as well as data on the incidence of torture within different subgroups of the data.

Table A1: **Occupations and education prior to arrest**

Characteristic	Total	Proportion
Occupation		
Artisanal work	10864	0.1869
Operator of machinery (industry, driver)	8943	0.1539
Agriculture/Fishery	8427	0.1450
Sales	7193	0.1237
Personal services/private security	4982	0.0857
Other	4746	0.0816
Informal commercial activity	4711	0.0810
Professional/technical	2051	0.0353
N/A	1677	0.0289
Administrative assistant	1045	0.0180
Illicit commerce	972	0.0167
Bureaucrat	664	0.0114
Police (not federal)	644	0.0111
Businessman	642	0.0110
Army	296	0.0051
Does not know	100	0.0017
Federal Police	71	0.0012
No response	71	0.0012
Marines	28	0.0005
Education		
No Education	2376	0.0409
Preschool	315	0.0054
Primary	14785	0.2544
Middle School	26648	0.4584
Middle School + technical HS	597	0.0103
High School	9857	0.1696
Vocational training + HS	743	0.0128
Undergraduate	2539	0.0437
Graduate	134	0.0023
Does not know	114	0.0020
No Response	19	0.0003

Table A2: **Selected convictions**

	All state prisoners	Arrested close to reform
Theft (any type)	0.3675	0.4412
Homicide	0.2648	0.1022
Rape	0.1309	0.074
Kidnapping	0.079	0.0156
Drug possession	0.0571	0.1287
Drug commerce	0.0288	0.0457
Assault	0.0314	0.0421
Illegal possession of weapons	0.0724	0.0427
Domestic violence	0.0096	0.0216
Property damage	0.0044	0.0108
Fraud	0.0071	0.0096
Sexual assault	0.0115	0.009
Extortion	0.012	0.0054
Despojo	0.0027	0.0054
Trespassing	0.0021	0.0042
Deprivation of liberty	0.017	0.009
Abuse of confidence	0.0024	0.0042
Threats	0.0022	0.0024
Other	0.0599	0.0637

Note: Data are only for prisoners at state detention centers and exclude federal prisoners. We report data for all state prisoners as well as those considered in the quantitative analysis. Standard errors included in parenthesis.

9.3.1 Other descriptive statistics

Table A3 describes the circumstances of arrest in the sample with respect to turf wars, local democracy, the drug war, and the reform.

Table A3: **Breakdown of sample**

Circumstances of arrest	Percentage
All states	
Municipality experiencing turf war	0.619
Municipality experiencing new turf war	0.404
Prior to democracy	0.355
Democracy	0.645
2006 and before	0.123
After 2006	0.877
Geographic reform states	
Pre-reform	0.908
Post-reform	0.092

Notes: This table divides the sample into the proportion of the sample that occurs on either side of important events.

9.3.2 Full models for correlates of torture

Here we present the full models for the correlates of torture using OLS and logits. Tables A4 and A5 are regressions of our binary categories of torture on sociodemographic characteristics and the circumstances of the respondents arrest. Table A4 reports OLS and A5 reports logits. These regressions are purely for descriptive purposes and correspond to Figure 1 in the paper. Generally, our findings suggest that physical abuse overwhelmingly tends to happen among younger men, though the relationship between youth, sex, and threats is much weaker (and, in the case of sex, reversed). We see that those accused of organized crime and theft tend to face all forms of abuse at a much higher rate, while those accused of rape and homicide are abused by authorities at a much lower rate. Insofar as the circumstances of arrest are concerned, those who are arrested with a warrant – that is, individuals for whom there is a formal process undergirding their arrest – face lower rates of abuse. Being arrested without a warrant is associated with an increase in the likelihood of abuse. This finding is intuitive; an arrest warrant presumes that the arrest has followed the formal channels governing criminal investigation. Moreover, an arrest warrant presupposes some significant amount of evidence has already been gathered, which then reduces the evidentiary value of coerced confessions. An arrest without a warrant, on the other hand, does not necessarily meet these criteria.

Table A4: **OLS: descriptive statistics**

	(1)	(2)	(3)
	Brute	Torture	Threat
Indigenous	-0.0309***	-0.0263**	0.00218
	(0.00842)	(0.00856)	(0.00848)
Male	0.189***	0.174***	-0.0130
	(0.00922)	(0.00922)	(0.00843)
Circumstances of arrest			
With a warrant	-0.136***	-0.108***	-0.106***

Table A4, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
	(0.00678)	(0.00676)	(0.00692)
Without a warrant	0.0933***	0.111***	0.127***
	(0.00433)	(0.00457)	(0.00443)
Sentencing status			
Partly sentenced	0.0276*	0.0499***	0.0544***
	(0.0110)	(0.0123)	(0.0109)
Sentenced	-0.0895***	-0.110***	-0.100***
	(0.00479)	(0.00507)	(0.00483)
Arresting Authority			
State police	0.0481***	0.0794***	0.0566***
	(0.00653)	(0.00683)	(0.00669)
Ministerial police	0.0432***	0.0728***	0.0861***
	(0.00528)	(0.00550)	(0.00537)
Federal police	0.0135	0.0541***	0.0766***
	(0.00789)	(0.00839)	(0.00783)
Military	0.0626***	0.0897***	0.0512***
	(0.00841)	(0.00906)	(0.00875)
Other	-0.0138	0.0214*	0.0190*
	(0.00940)	(0.00969)	(0.00942)
Crime			
Homicide	-0.0136*	-0.0125*	-0.0227***
	(0.00533)	(0.00567)	(0.00540)
Organized crime	0.103***	0.106***	0.113***
	(0.00499)	(0.00548)	(0.00504)

Table A4, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
Rape	-0.103*** (0.00737)	-0.142*** (0.00743)	-0.125*** (0.00753)
Theft	0.0801*** (0.00484)	0.0666*** (0.00521)	0.0506*** (0.00494)
Occupation			
Merchant	0.0193*** (0.00586)	0.0129* (0.00618)	0.0266*** (0.00591)
Public security	-0.0866*** (0.0152)	-0.0579*** (0.0157)	-0.00260 (0.0146)
Private security	0.00678 (0.00784)	0.000216 (0.00819)	0.000453 (0.00794)
Rural worker	-0.0283*** (0.00721)	-0.0310*** (0.00746)	-0.0191** (0.00734)
Craftsman	0.00386 (0.00624)	-0.00262 (0.00656)	-0.00402 (0.00637)
Blue collar worker	0.00673 (0.00641)	0.00549 (0.00677)	0.00778 (0.00656)
Age			
26-35	-0.0407*** (0.00438)	-0.0341*** (0.00469)	-0.00936* (0.00448)
36-45	-0.0985*** (0.00554)	-0.0824*** (0.00575)	-0.0532*** (0.00558)
46-55	-0.204*** (0.00854)	-0.183*** (0.00845)	-0.145*** (0.00857)

Table A4, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
56-65	-0.275*** (0.0144)	-0.239*** (0.0137)	-0.215*** (0.0149)
65+	-0.373*** (0.0246)	-0.319*** (0.0230)	-0.295*** (0.0276)
Education			
Primary or less	-0.00389 (0.0109)	0.00794 (0.0109)	0.0119 (0.0111)
Secondary	0.0232* (0.0108)	0.0403*** (0.0108)	0.0446*** (0.0110)
High school	0.0480*** (0.0114)	0.0737*** (0.0115)	0.0770*** (0.0116)
College or graduate	0.00883 (0.0138)	0.0560*** (0.0141)	0.0716*** (0.0140)
Wealth index, quantiles			
25-50%	-0.00388 (0.00527)	-0.0115* (0.00554)	-0.0126* (0.00537)
50-75%	-0.000379 (0.00540)	-0.00620 (0.00567)	0.00682 (0.00547)
75-100%	0.0103 (0.00553)	0.00966 (0.00581)	0.0108 (0.00562)
Constant	0.749** (0.267)	0.838** (0.255)	0.859** (0.268)
<i>N</i>	54853	54797	54781
State FE	Y	Y	Y

Table A4, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
Year FE	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

State FE	Y	Y	Y
Year FE	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5: **Logits: descriptive statistics**

	(1)	(2)	(3)
	Brute	Torture	Threat
Indigenous	-0.144**	-0.119*	0.0160
	(0.0410)	(0.0404)	(0.0412)
Male	0.924**	0.812**	-0.0742
	(0.0434)	(0.0434)	(0.0465)
Circumstances of arrest			
With a warrant	-0.600**	-0.498**	-0.452**
	(0.0314)	(0.0320)	(0.0312)
Without a warrant	0.490**	0.517**	0.639**
	(0.0226)	(0.0212)	(0.0221)
Sentencing status			
Partly sentenced	0.257*	0.305**	0.447**
	(0.0789)	(0.0714)	(0.0840)
Sentenced	-0.476**	-0.528**	-0.530**
	(0.0260)	(0.0250)	(0.0260)
Arresting Authority			
State police	0.244**	0.362**	0.264**
	(0.0339)	(0.0319)	(0.0327)
Ministerial police	0.220**	0.333**	0.417**
	(0.0268)	(0.0256)	(0.0263)
Federal police	0.0715	0.245**	0.395**
	(0.0416)	(0.0397)	(0.0426)
Military	0.365**	0.423**	0.244**
	(0.0490)	(0.0449)	(0.0473)
Other	-0.0708	0.0907	0.0754

Table A5, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
	(0.0461)	(0.0448)	(0.0455)
Crime			
Homicide	-0.0598 (0.0278)	-0.0518 (0.0269)	-0.108** (0.0275)
Organized crime	0.575** (0.0288)	0.514** (0.0270)	0.619** (0.0290)
Rape	-0.478** (0.0354)	-0.654** (0.0356)	-0.576** (0.0353)
Theft	0.426** (0.0264)	0.319** (0.0251)	0.257** (0.0257)
Occupation			
Merchant	0.103** (0.0310)	0.0621 (0.0294)	0.142** (0.0308)
Public security	-0.445** (0.0748)	-0.274** (0.0737)	-0.0123 (0.0776)
Private security	0.0379 (0.0404)	0.00122 (0.0388)	0.00806 (0.0395)
Rural worker	-0.140** (0.0365)	-0.144** (0.0353)	-0.0868 (0.0361)
Craftsman	0.0204 (0.0326)	-0.0129 (0.0310)	-0.0161 (0.0319)
Blue collar worker	0.0374 (0.0338)	0.0267 (0.0321)	0.0425 (0.0334)
Age			

Table A5, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
26-35	-0.217**	-0.159**	-0.0454
	(0.0238)	(0.0222)	(0.0232)
36-45	-0.498**	-0.380**	-0.263**
	(0.0279)	(0.0268)	(0.0276)
46-55	-0.976**	-0.853**	-0.684**
	(0.0402)	(0.0408)	(0.0400)
56-65	-1.294**	-1.170**	-0.996**
	(0.0707)	(0.0759)	(0.0710)
65+	-1.819**	-1.729**	-1.392**
	(0.147)	(0.173)	(0.149)
Education			
Primary or less	-0.0247	0.0344	0.0551
	(0.0534)	(0.0520)	(0.0526)
Secondary	0.114	0.185**	0.216**
	(0.0531)	(0.0515)	(0.0522)
High school	0.249**	0.345**	0.389**
	(0.0568)	(0.0549)	(0.0560)
College or graduate	0.0429	0.261**	0.357**
	(0.0688)	(0.0674)	(0.0695)
Wealth index, quantiles			
25-50%	-0.0173	-0.0527	-0.0605
	(0.0277)	(0.0263)	(0.0273)
50-75%	0.000596	-0.0285	0.0381
	(0.0283)	(0.0269)	(0.0280)

Table A5, continued

	(1)	(2)	(3)
	Brute	Torture	Threat
75-100%	0.0549	0.0468	0.0546
	(0.0288)	(0.0275)	(0.0284)
Cosntant	1.193	1.603	1.738
	(1.533)	(1.425)	(1.440)
<i>N</i>	54850	54794	54777
State FE	Y	Y	Y
Year FE	Y	Y	Y
Standard errors in parentheses			
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$			
State FE	Y	Y	Y
Year FE	Y	Y	Y
Standard errors in parentheses			
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$			

9.4 Criminal Justice Reform

9.4.1 Dates of reform

In this section, we show the dates used to construct the dummy variable for the reform implementation by state. These were obtained through the Supreme Court’s website and state records, in which the announcements of the reform were recorded. Some states revise their implementation timelines. For this reason, we cannot rely on the Supreme Court’s website alone²¹ and it is necessary to delve into the announcements themselves and cross-reference changes to the timelines. To define whether or not an individual was treated by the reform, we exploit the staggered implementation of the reform, which we outline in Table A6,

²¹The website lists either an early timeline or the first timeline listed, even though it links to all the individual declarations, which subsequently have to be parsed for the various changes.

Table A6: **Reform implementation dates examined**

State	Implementation dates (year - month - day)
Baja California	2015-08-11
Baja California Sur	2015-07-01, 2016-01-01, 2016-06-17
Campeche	2014-12-03, 2015-08-04, 2016-05-18
Colima	2014-12-31, 2015-09-01, 2016-05-20
Chihuahua	2016-06-13
Durango	2014-05-07, 2014-06-10, 2015-05-07
Guanajuato	2016-06-01
Guerrero	2014-09-30, 2015-03-03, 2015-04-03, 2015-05-03, 2015-08-05, 2016-05-20, 2016-05-25, 2016-06-01
Hidalgo	2014-11-18, 2015-07-14, 2015-09-29, 2016-02-16, 2016-06-07
Jalisco	2014-10-01, 2015-03-15, 2015-06-29, 2016-01-15, 2016-02-15, 2016-05-31
Mexico	2016-05-18, 2016-06-18
Michoacan	2015-03-07, 2015-08-03, 2016-02-11, 2016-05-09
Nuevo Leon	2016-01-01
Oaxaca	2015-12-02, 2016-02-02, 2016-03-02, 2016-06-18
Puebla	2014-05-21, 2014-09-17, 2016-06-17
Queretaro	2014-06-02, 2014-09-29, 2016-05-30
Quintana Roo	2014-06-10, 2016-06-18
Sinaloa	2014-10-15, 2015-06-01, 2016-01-15, 2016-06-13
Sonora	2015-12-15, 2016-03-30, 2016-05-30
Tabasco	2014-10-04, 2014-10-06, 2014-12-15, 2015-04-06, 2015-08-24, 2015-10-19, 2015-12-07, 2016-04-25, 2016-06-06
Veracruz	2014-11-11, 2015-05-12, 2015-11-10, 2016-05-11
Yucatan	2015-09-22
Zacatecas	2015-01-05, 2016-01-04

9.4.2 Full set of models for reform

In the paper, we present results from OLS evaluating the effectiveness of the reform, though we truncate the tables and exclude the covariates. Here we present both OLS and logits as alternate specifications in Tables A7, A8, A9 and A10.

Table A7: OLS: Reform full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.142*** (0.0119)	-0.104*** (0.0117)	-0.119*** (0.0120)	-0.0811*** (0.0125)	-0.0630*** (0.0125)	-0.0611*** (0.0127)	-0.0704*** (0.0134)	-0.0537*** (0.0134)	-0.0602*** (0.0137)
Male	0.165*** (0.0126)	0.140*** (0.0126)	-0.0556*** (0.0117)	0.163*** (0.0124)	0.139*** (0.0124)	-0.0585*** (0.0116)	0.160*** (0.0127)	0.140*** (0.0126)	-0.0579*** (0.0119)
Cannot read or write	-0.00859 (0.0153)	-0.0232 (0.0156)	-0.0131 (0.0157)	-0.00885 (0.0151)	-0.0256 (0.0153)	-0.0140 (0.0156)	-0.00840 (0.0156)	-0.0226 (0.0160)	-0.0219 (0.0161)
Indigenous	-0.0609*** (0.0106)	-0.0561*** (0.0108)	-0.0242* (0.0107)	-0.0547*** (0.0108)	-0.0540*** (0.0110)	-0.0231* (0.0109)	-0.0225 (0.0120)	-0.0179 (0.0123)	0.00583 (0.0120)
Occupation									
Merchant	0.0310*** (0.00813)	0.0195* (0.00863)	0.0315*** (0.00831)	0.0204* (0.00807)	0.0139 (0.00858)	0.0233** (0.00828)	0.0191* (0.00827)	0.0136 (0.00875)	0.0225** (0.00845)
Public security	-0.0991*** (0.0217)	-0.0723** (0.0223)	0.00472 (0.0210)	-0.102*** (0.0213)	-0.0716** (0.0219)	0.00453 (0.0208)	-0.109*** (0.0218)	-0.0745*** (0.0225)	0.00561 (0.0215)
Private security	-0.0193 (0.0105)	-0.0244* (0.0110)	-0.0350** (0.0108)	-0.00789 (0.0105)	-0.0169 (0.0110)	-0.0249* (0.0108)	-0.00815 (0.0108)	-0.0179 (0.0113)	-0.0255* (0.0112)
Rural worker	-0.0602*** (0.00952)	-0.0584*** (0.00986)	-0.0501*** (0.00973)	-0.0415*** (0.00956)	-0.0475*** (0.00988)	-0.0313** (0.00978)	-0.0259* (0.0102)	-0.0349** (0.0106)	-0.0224* (0.0105)
Craftsman	-0.00333 (0.00853)	-0.0132 (0.00901)	-0.0194* (0.00881)	0.00128 (0.00846)	-0.0123 (0.00892)	-0.0139 (0.00873)	0.00259 (0.00868)	-0.0104 (0.00914)	-0.0138 (0.00895)
Blue collar	0.0161 (0.00876)	0.00692 (0.00933)	0.00273 (0.00907)	0.0116 (0.00870)	0.00490 (0.00928)	0.00119 (0.00905)	0.00983 (0.00893)	0.00108 (0.00950)	-0.00155 (0.00928)
Age									
26-35	-0.0488*** (0.00597)	-0.0425*** (0.00644)	-0.0121 (0.00620)	-0.0484*** (0.00591)	-0.0423*** (0.00637)	-0.0125* (0.00614)	-0.0446*** (0.00606)	-0.0390*** (0.00651)	-0.00955 (0.00631)
36-45	-0.122*** (0.00755)	-0.109*** (0.00788)	-0.0661*** (0.00770)	-0.118*** (0.00748)	-0.107*** (0.00779)	-0.0641*** (0.00763)	-0.114*** (0.00767)	-0.104*** (0.00799)	-0.0608*** (0.00783)
46-55	-0.249*** (0.0114)	-0.229*** (0.0112)	-0.175*** (0.0116)	-0.247*** (0.0113)	-0.229*** (0.0112)	-0.174*** (0.0114)	-0.236*** (0.0117)	-0.220*** (0.0116)	-0.170*** (0.0118)
56-65	-0.344*** (0.0190)	-0.299*** (0.0181)	-0.255*** (0.0195)	-0.338*** (0.0188)	-0.295*** (0.0180)	-0.250*** (0.0194)	-0.335*** (0.0195)	-0.291*** (0.0189)	-0.245*** (0.0199)
65+	-0.476*** (0.0309)	-0.400*** (0.0275)	-0.354*** (0.0337)	-0.461*** (0.0306)	-0.386*** (0.0277)	-0.342*** (0.0341)	-0.461*** (0.0319)	-0.380*** (0.0296)	-0.344*** (0.0353)
Education									
Primary or less	-0.00287 (0.0182)	0.0174 (0.0184)	0.0186 (0.0189)	-0.00651 (0.0181)	0.00964 (0.0181)	0.0154 (0.0187)	0.00212 (0.0188)	0.0240 (0.0190)	0.0162 (0.0192)
Middle School	0.0176 (0.0189)	0.0458* (0.0191)	0.0491* (0.0195)	0.0240 (0.0188)	0.0469* (0.0188)	0.0563** (0.0193)	0.0321 (0.0194)	0.0627** (0.0198)	0.0558** (0.0199)
High School	0.0454* (0.0195)	0.0794*** (0.0199)	0.0863*** (0.0202)	0.0563** (0.0194)	0.0856*** (0.0196)	0.0980*** (0.0200)	0.0631** (0.0200)	0.0986*** (0.0205)	0.0962*** (0.0205)
College or graduate	-0.00140 (0.0224)	0.0558* (0.0229)	0.0875*** (0.0229)	0.00926 (0.0222)	0.0608** (0.0225)	0.0966*** (0.0227)	0.0126 (0.0230)	0.0711** (0.0235)	0.0938*** (0.0233)
Wealth index, quantiles									
25%-50%	-0.0172* (0.00725)	-0.0129 (0.00768)	-0.0232** (0.00746)	-0.0167* (0.00716)	-0.0167* (0.00758)	-0.0226** (0.00740)	-0.0215** (0.00733)	-0.0203** (0.00775)	-0.0251*** (0.00757)
50%-75%	-0.0117 (0.00737)	-0.0147 (0.00780)	-0.00171 (0.00756)	-0.00494 (0.00730)	-0.0118 (0.00774)	0.00413 (0.00752)	-0.00323 (0.00747)	-0.0130 (0.00792)	0.00444 (0.00771)
75%-100%	-0.00874 (0.00751)	-0.00186 (0.00790)	-0.00930 (0.00770)	0.00213 (0.00751)	0.00652 (0.00791)	-0.000822 (0.00772)	0.00102 (0.00770)	0.00540 (0.00811)	-0.000953 (0.00793)
Constant	0.607** (0.233)	0.594** (0.225)	0.505* (0.244)	0.411 (0.220)	0.397 (0.212)	0.354 (0.236)	0.499* (0.252)	0.462* (0.235)	0.397 (0.227)
<i>N</i>	31961	31931	31926	31961	31931	31926	31961	31931	31926
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8: OLS, 6 months before and after reform

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.0780*** (0.0156)	-0.0603*** (0.0154)	-0.0666*** (0.0158)	-0.0410* (0.0167)	-0.0509** (0.0167)	-0.0519** (0.0170)	-0.0531* (0.0218)	-0.0511* (0.0222)	-0.0538* (0.0223)
Male	0.189*** (0.0318)	0.183*** (0.0291)	-0.0210 (0.0320)	0.197*** (0.0309)	0.188*** (0.0287)	-0.0190 (0.0317)	0.196*** (0.0339)	0.187*** (0.0314)	-0.00972 (0.0345)
Illiterate	0.0410 (0.0425)	0.0110 (0.0412)	0.0451 (0.0435)	0.0248 (0.0422)	-0.00615 (0.0397)	0.0271 (0.0427)	0.0350 (0.0494)	0.0296 (0.0478)	0.0593 (0.0483)
Indigenous language	-0.0649* (0.0330)	-0.0857** (0.0316)	-0.0858** (0.0331)	-0.0685* (0.0339)	-0.0914** (0.0329)	-0.0832* (0.0338)	-0.0533 (0.0420)	-0.0596 (0.0416)	-0.0683 (0.0412)
Occupation									
Merchant	0.0464* (0.0223)	0.0152 (0.0226)	0.0376 (0.0227)	0.0227 (0.0221)	0.00491 (0.0225)	0.0263 (0.0227)	0.0348 (0.0245)	0.00227 (0.0249)	0.0278 (0.0250)
Public security	-0.0196 (0.0794)	-0.0416 (0.0794)	0.0711 (0.0746)	-0.0327 (0.0734)	-0.0414 (0.0779)	0.0576 (0.0727)	-0.0109 (0.0806)	-0.000779 (0.0986)	0.104 (0.0852)
Private security	-0.0189 (0.0282)	-0.0200 (0.0278)	-0.0350 (0.0286)	-0.0174 (0.0284)	-0.0213 (0.0284)	-0.0359 (0.0289)	-0.00944 (0.0313)	-0.0177 (0.0314)	-0.0283 (0.0318)
Rural worker	-0.0477 (0.0271)	-0.0226 (0.0267)	-0.0645* (0.0270)	-0.0365 (0.0270)	-0.0219 (0.0267)	-0.0509 (0.0272)	-0.0191 (0.0319)	-0.0155 (0.0314)	-0.0338 (0.0324)
Craftsman	0.0120 (0.0236)	-0.0132 (0.0236)	-0.0223 (0.0237)	0.0114 (0.0234)	-0.0137 (0.0234)	-0.0192 (0.0237)	0.0169 (0.0260)	-0.0182 (0.0263)	-0.0192 (0.0261)
Blue collar	0.0166 (0.0245)	0.00245 (0.0248)	0.00399 (0.0249)	0.00434 (0.0241)	-0.00186 (0.0248)	0.00161 (0.0248)	0.00611 (0.0267)	0.00201 (0.0275)	0.00924 (0.0273)
Age									
26-35	-0.0583*** (0.0174)	-0.0648*** (0.0179)	0.00609 (0.0177)	-0.0524** (0.0171)	-0.0621*** (0.0178)	0.0124 (0.0176)	-0.0463* (0.0189)	-0.0554** (0.0198)	0.0283 (0.0194)
36-45	-0.137*** (0.0208)	-0.121*** (0.0208)	-0.0765*** (0.0210)	-0.125*** (0.0205)	-0.119*** (0.0206)	-0.0682** (0.0208)	-0.127*** (0.0228)	-0.114*** (0.0231)	-0.0680** (0.0234)
46-55	-0.311*** (0.0298)	-0.267*** (0.0275)	-0.257*** (0.0299)	-0.310*** (0.0298)	-0.272*** (0.0278)	-0.254*** (0.0299)	-0.287*** (0.0343)	-0.258*** (0.0324)	-0.221*** (0.0341)
56-65	-0.255*** (0.0508)	-0.211*** (0.0472)	-0.209*** (0.0512)	-0.251*** (0.0507)	-0.208*** (0.0486)	-0.204*** (0.0511)	-0.232*** (0.0590)	-0.195*** (0.0573)	-0.198*** (0.0586)
65+	-0.468*** (0.0647)	-0.449*** (0.0355)	-0.468*** (0.0537)	-0.461*** (0.0619)	-0.452*** (0.0397)	-0.467*** (0.0569)	-0.452*** (0.0840)	-0.429*** (0.0583)	-0.454*** (0.0721)
Education									
Primary or less	0.0193 (0.0547)	0.0338 (0.0512)	-0.0277 (0.0563)	0.00490 (0.0537)	0.0151 (0.0497)	-0.0347 (0.0554)	0.0547 (0.0648)	0.116 (0.0595)	0.0150 (0.0647)
Middle School	0.0451 (0.0563)	0.0699 (0.0529)	-0.00378 (0.0580)	0.0368 (0.0556)	0.0535 (0.0516)	-0.00608 (0.0573)	0.0870 (0.0665)	0.150* (0.0614)	0.0346 (0.0667)
High School	0.0473 (0.0577)	0.0825 (0.0545)	0.0203 (0.0595)	0.0380 (0.0570)	0.0666 (0.0532)	0.0187 (0.0587)	0.0883 (0.0680)	0.162* (0.0632)	0.0623 (0.0682)
College or graduate	0.0289 (0.0650)	0.0583 (0.0621)	0.0552 (0.0664)	0.00502 (0.0644)	0.0283 (0.0610)	0.0403 (0.0660)	0.0601 (0.0759)	0.118 (0.0713)	0.100 (0.0764)
Wealth index, quantiles									
25%-50%	-0.0435* (0.0204)	-0.0476* (0.0206)	-0.0709*** (0.0206)	-0.0355 (0.0200)	-0.0466* (0.0203)	-0.0659** (0.0205)	-0.0180 (0.0220)	-0.0384 (0.0224)	-0.0596** (0.0225)
50%-75%	-0.0238 (0.0209)	-0.0428* (0.0210)	-0.0157 (0.0211)	-0.0172 (0.0205)	-0.0395 (0.0208)	-0.0109 (0.0210)	-0.00231 (0.0227)	-0.0323 (0.0232)	0.00102 (0.0233)
75%-100%	-0.0276 (0.0212)	-0.0167 (0.0214)	-0.0486* (0.0213)	-0.0138 (0.0210)	-0.00725 (0.0213)	-0.0433* (0.0214)	-0.0000266 (0.0236)	-0.00378 (0.0240)	-0.0455 (0.0239)
Constant	0.431** (0.136)	0.213 (0.135)	0.642*** (0.142)	0.391** (0.130)	0.199 (0.130)	0.694*** (0.135)	0.172 (0.170)	-0.0185 (0.149)	0.519** (0.180)
N	4488	4481	4479	4488	4481	4479	4488	4481	4479
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: Logits: main results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.542*** (0.0498)	-0.395*** (0.0499)	-0.447*** (0.0491)	-0.303*** (0.0545)	-0.264*** (0.0547)	-0.240*** (0.0538)	-0.262*** (0.0590)	-0.228*** (0.0591)	-0.242*** (0.0582)
Male	0.731*** (0.0541)	0.595*** (0.0541)	-0.265*** (0.0578)	0.746*** (0.0550)	0.604*** (0.0548)	-0.284*** (0.0584)	0.759*** (0.0569)	0.635*** (0.0568)	-0.290*** (0.0603)
Illiterate	-0.0387 (0.0688)	-0.103 (0.0676)	-0.0555 (0.0671)	-0.0383 (0.0696)	-0.114 (0.0679)	-0.0585 (0.0676)	-0.0374 (0.0754)	-0.104 (0.0735)	-0.0947 (0.0724)
Indigenous	-0.276*** (0.0467)	-0.238*** (0.0460)	-0.107* (0.0462)	-0.250*** (0.0490)	-0.231*** (0.0481)	-0.101* (0.0481)	-0.109 (0.0574)	-0.0799 (0.0558)	0.0253 (0.0563)
Occupation									
Merchant	0.149*** (0.0395)	0.0827* (0.0372)	0.145*** (0.0388)	0.101* (0.0402)	0.0595 (0.0378)	0.110** (0.0394)	0.0976* (0.0416)	0.0607 (0.0391)	0.111** (0.0405)
Public security	-0.456*** (0.0957)	-0.308** (0.0935)	0.0238 (0.0988)	-0.484*** (0.0964)	-0.313*** (0.0940)	0.0247 (0.100)	-0.535*** (0.102)	-0.337*** (0.0993)	0.0334 (0.105)
Private security	-0.0867 (0.0492)	-0.101* (0.0470)	-0.151** (0.0479)	-0.0392 (0.0503)	-0.0754 (0.0482)	-0.112* (0.0490)	-0.0407 (0.0522)	-0.0795 (0.0501)	-0.117* (0.0509)
Rural worker	-0.275*** (0.0437)	-0.249*** (0.0422)	-0.217*** (0.0428)	-0.191*** (0.0448)	-0.209*** (0.0434)	-0.136** (0.0438)	-0.131** (0.0493)	-0.164*** (0.0479)	-0.102* (0.0482)
Craftsman	-0.0172 (0.0406)	-0.0566 (0.0385)	-0.0857* (0.0393)	0.00628 (0.0413)	-0.0545 (0.0391)	-0.0616 (0.0399)	0.0145 (0.0429)	-0.0446 (0.0407)	-0.0595 (0.0413)
Blue collar	0.0770 (0.0425)	0.0289 (0.0400)	0.0123 (0.0412)	0.0577 (0.0434)	0.0195 (0.0409)	0.00486 (0.0421)	0.0514 (0.0450)	0.00488 (0.0424)	-0.00786 (0.0435)
Age									
26-35	-0.243*** (0.0296)	-0.181*** (0.0274)	-0.0570* (0.0285)	-0.246*** (0.0300)	-0.184*** (0.0278)	-0.0584* (0.0289)	-0.236*** (0.0313)	-0.175*** (0.0290)	-0.0465 (0.0300)
36-45	-0.566*** (0.0345)	-0.458*** (0.0331)	-0.293*** (0.0338)	-0.563*** (0.0351)	-0.460*** (0.0336)	-0.290*** (0.0342)	-0.568*** (0.0368)	-0.464*** (0.0352)	-0.284*** (0.0358)
46-55	-1.096*** (0.0492)	-0.971*** (0.0501)	-0.744*** (0.0485)	-1.111*** (0.0498)	-0.990*** (0.0509)	-0.752*** (0.0489)	-1.111*** (0.0527)	-0.996*** (0.0537)	-0.768*** (0.0518)
56-65	-1.506*** (0.0868)	-1.319*** (0.0925)	-1.078*** (0.0847)	-1.512*** (0.0874)	-1.330*** (0.0940)	-1.076*** (0.0858)	-1.578*** (0.0938)	-1.379*** (0.100)	-1.112*** (0.0916)
65+	-2.205*** (0.192)	-2.017*** (0.220)	-1.559*** (0.178)	-2.178*** (0.194)	-1.989*** (0.222)	-1.531*** (0.181)	-2.296*** (0.208)	-2.062*** (0.237)	-1.653*** (0.197)
Education									
Primary or less	-0.0137 (0.0823)	0.0800 (0.0810)	0.0796 (0.0805)	-0.0343 (0.0837)	0.0473 (0.0814)	0.0648 (0.0810)	0.0152 (0.0912)	0.125 (0.0888)	0.0809 (0.0872)
Middle school	0.0775 (0.0854)	0.198* (0.0838)	0.210* (0.0835)	0.110 (0.0870)	0.208* (0.0844)	0.247** (0.0842)	0.160 (0.0945)	0.299** (0.0918)	0.266** (0.0904)
High school	0.214* (0.0891)	0.341*** (0.0870)	0.384*** (0.0870)	0.270** (0.0906)	0.378*** (0.0876)	0.445*** (0.0878)	0.319** (0.0980)	0.460*** (0.0949)	0.461*** (0.0939)
College or graduate	-0.0123 (0.102)	0.240* (0.0997)	0.384*** (0.101)	0.0369 (0.103)	0.268** (0.100)	0.432*** (0.102)	0.0646 (0.110)	0.338** (0.108)	0.447*** (0.108)
Wealth index, quantiles									
25%-50%	-0.0849* (0.0349)	-0.0557 (0.0329)	-0.107** (0.0338)	-0.0833* (0.0354)	-0.0735* (0.0333)	-0.105** (0.0343)	-0.109** (0.0367)	-0.0906** (0.0346)	-0.119*** (0.0355)
50% - 75%	-0.0557 (0.0352)	-0.0639 (0.0333)	-0.00793 (0.0344)	-0.0217 (0.0358)	-0.0508 (0.0339)	0.0211 (0.0350)	-0.0117 (0.0373)	-0.0570 (0.0353)	0.0241 (0.0364)
75%-100%	-0.0491 (0.0358)	-0.0116 (0.0340)	-0.0501 (0.0348)	0.00578 (0.0368)	0.0268 (0.0349)	-0.0103 (0.0356)	0.00126 (0.0385)	0.0226 (0.0365)	-0.00973 (0.0373)
Constant	0.508 (1.271)	0.445 (1.201)	0.0489 (0.974)	-0.430 (1.202)	-0.412 (1.173)	-0.625 (0.945)	-0.00383 (1.381)	-0.168 (1.254)	-0.442 (0.904)
<i>N</i>	31958	31931	31926	31958	31931	31926	31000	31113	30964
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10: Logits: six months before and after

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.333*** (0.0662)	-0.261*** (0.0671)	-0.280*** (0.0659)	-0.184* (0.0741)	-0.233** (0.0750)	-0.225** (0.0730)	-0.257** (0.0967)	-0.255** (0.0974)	-0.249** (0.0950)
Illiterate	0.175 (0.182)	0.0460 (0.187)	0.193 (0.184)	0.113 (0.189)	-0.0243 (0.186)	0.122 (0.185)	0.224 (0.241)	0.188 (0.229)	0.334 (0.219)
Indigenous	-0.275* (0.139)	-0.388** (0.148)	-0.361** (0.139)	-0.304* (0.149)	-0.432** (0.158)	-0.361* (0.146)	-0.273 (0.191)	-0.302 (0.197)	-0.340 (0.186)
Male	0.800*** (0.139)	0.842*** (0.150)	-0.0916 (0.137)	0.872*** (0.141)	0.879*** (0.151)	-0.0845 (0.138)	0.937*** (0.154)	0.932*** (0.164)	-0.0479 (0.154)
Occupation									
Merchant	0.201* (0.0972)	0.0613 (0.0964)	0.160 (0.0970)	0.103 (0.100)	0.0152 (0.0986)	0.114 (0.0987)	0.162 (0.110)	0.00678 (0.108)	0.130 (0.109)
Public Security	-0.0810 (0.333)	-0.180 (0.348)	0.308 (0.333)	-0.144 (0.326)	-0.189 (0.357)	0.267 (0.335)	-0.0200 (0.379)	0.00228 (0.426)	0.548 (0.407)
Private Security	-0.0801 (0.119)	-0.0860 (0.120)	-0.147 (0.119)	-0.0804 (0.125)	-0.0997 (0.126)	-0.156 (0.123)	-0.0407 (0.137)	-0.0860 (0.137)	-0.130 (0.136)
Rural worker	-0.202 (0.115)	-0.101 (0.117)	-0.272* (0.114)	-0.161 (0.120)	-0.103 (0.121)	-0.222 (0.117)	-0.0974 (0.147)	-0.0716 (0.144)	-0.163 (0.144)
Craftsman	0.0504 (0.101)	-0.0562 (0.101)	-0.0924 (0.0990)	0.0504 (0.104)	-0.0620 (0.103)	-0.0818 (0.101)	0.0830 (0.117)	-0.0894 (0.115)	-0.0873 (0.111)
Blue collar	0.0705 (0.105)	0.00880 (0.105)	0.0165 (0.104)	0.0188 (0.108)	-0.0103 (0.108)	0.00732 (0.107)	0.0221 (0.119)	0.00914 (0.118)	0.0416 (0.118)
Age									
26-35	-0.252*** (0.0751)	-0.265*** (0.0733)	0.0260 (0.0743)	-0.237** (0.0776)	-0.260*** (0.0750)	0.0543 (0.0757)	-0.230** (0.0861)	-0.248** (0.0828)	0.127 (0.0837)
36-45	-0.577*** (0.0875)	-0.502*** (0.0877)	-0.314*** (0.0860)	-0.552*** (0.0902)	-0.510*** (0.0897)	-0.286** (0.0876)	-0.610*** (0.102)	-0.523*** (0.100)	-0.308** (0.0986)
46-55	-1.318*** (0.137)	-1.222*** (0.148)	-1.084*** (0.136)	-1.363*** (0.141)	-1.274*** (0.153)	-1.096*** (0.138)	-1.376*** (0.162)	-1.332*** (0.180)	-1.042*** (0.156)
56-65	-1.070*** (0.224)	-0.935*** (0.243)	-0.870*** (0.225)	-1.107*** (0.233)	-0.961*** (0.255)	-0.877*** (0.230)	-1.107*** (0.269)	-0.942** (0.288)	-0.953*** (0.274)
65+	-2.243*** (0.493)	-3.406*** (1.019)	-2.550*** (0.609)	-2.317*** (0.492)	-3.509*** (1.038)	-2.639*** (0.635)	-2.361*** (0.592)	-3.711** (1.295)	-2.866*** (0.819)
Education									
Primary or less	0.0842 (0.237)	0.171 (0.248)	-0.119 (0.241)	0.0121 (0.241)	0.0869 (0.247)	-0.157 (0.244)	0.300 (0.303)	0.683* (0.308)	0.141 (0.302)
Middle school	0.194 (0.244)	0.327 (0.254)	-0.0174 (0.248)	0.158 (0.249)	0.260 (0.254)	-0.0306 (0.251)	0.471 (0.310)	0.856** (0.316)	0.247 (0.312)
High school	0.204 (0.250)	0.378 (0.259)	0.0837 (0.254)	0.164 (0.255)	0.317 (0.260)	0.0772 (0.258)	0.481 (0.317)	0.907** (0.322)	0.377 (0.318)
College or graduate	0.120 (0.281)	0.277 (0.292)	0.236 (0.286)	0.0125 (0.287)	0.150 (0.294)	0.175 (0.291)	0.336 (0.350)	0.711* (0.355)	0.552 (0.355)
Wealth index, quantiles									
25%-50%	-0.189* (0.0882)	-0.201* (0.0870)	-0.300*** (0.0872)	-0.162 (0.0904)	-0.205* (0.0882)	-0.286** (0.0887)	-0.0890 (0.0993)	-0.179 (0.0969)	-0.283** (0.0976)
50% - 75%	-0.105 (0.0902)	-0.183* (0.0897)	-0.0682 (0.0899)	-0.0773 (0.0928)	-0.169 (0.0914)	-0.0479 (0.0918)	-0.0128 (0.104)	-0.152 (0.101)	0.00590 (0.102)
75%-100%	-0.120 (0.0916)	-0.0693 (0.0908)	-0.206* (0.0902)	-0.0645 (0.0946)	-0.0312 (0.0934)	-0.189* (0.0926)	-0.00801 (0.106)	-0.0274 (0.105)	-0.221* (0.104)
Constant	-0.308 (0.582)	-1.338* (0.638)	0.596 (0.586)	-0.446 (0.576)	-1.406* (0.632)	0.865 (0.586)	-1.568* (0.756)	-2.674*** (0.785)	0.0279 (0.762)
<i>N</i>	4488	4481	4479	4488	4481	4479	4004	4010	4007
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.4.3 Date of announcement of reform

Each state published declarations in its official records when the state legislature approved a timetable for the implementation of the reform. To deal with the possibility that state law enforcement was changing its behavior between the announcement of the reform and its actual implementation we test for changes in the period after the reform has been announced. We define an arrest as having occurred once the reform was announced if it occurred after the state had announced its first timetable.²² Therefore we have a sample that we split into a pre-announcement phase, a post-announcement but pre-reform phase, and a post-reform phase. We repeat our specifications from the paper, with sociodemographic controls, year fixed effects, and state and municipal fixed effects.

Table A11

State	Announcement (year - month - day)
Baja California	2015-06-11
Baja California Sur	2014-06-27
Campeche	2014-10-02
Colima	2014-08-30
Chihuahua	2015-03-04
Durango	2014-03-06
Guanajuato	2014-11-25
Guerrero	2014-07-31
Hidalgo	2014-08-25
Jalisco	2014-04-11
Mexico	2015-01-21
Michoacan	2014-12-26
Nuevo Leon	2014-12-24
Oaxaca	2014-01-11
Puebla	2014-03-19
Queretaro	2014-03-29
Quintana Roo	2014-04-10
Sinaloa	2014-07-31
Sonora	2015-12-14
Tabasco	2014-08-05
Veracruz	2014-09-10
Yucatan	2014-11-29
Zacatecas	2014-11-01

²²Some states amended their timetables subsequently, but our reasoning here is that if there is an anticipation effect, it should be visible once the state government has announced the coming reform.

Our results are reported in Tables A12 and A13. Across all 18 specifications, there is no negative association between torture and arrests occurring in the post-announcement period and the reform's effects hold.

Table A12: OLS: anticipation effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Torture	Brute	Threats	Torture	Brute	Threats	Torture	Brute	Threats
Reform announced	0.0399** (0.0129)	0.00861 (0.0126)	0.0251* (0.0127)	0.00179 (0.0134)	-0.00972 (0.0130)	0.00919 (0.0133)	0.00373 (0.0138)	-0.00903 (0.0134)	0.0121 (0.0137)
Reform	-0.0557*** (0.0163)	-0.120*** (0.0163)	-0.0852*** (0.0165)	-0.0559** (0.0172)	-0.0820*** (0.0171)	-0.0502** (0.0175)	-0.0434* (0.0180)	-0.0705*** (0.0178)	-0.0466* (0.0182)
Male	0.141*** (0.0126)	0.164*** (0.0126)	-0.0555*** (0.0117)	0.139*** (0.0124)	0.163*** (0.0124)	-0.0586*** (0.0116)	0.140*** (0.0126)	0.160*** (0.0127)	-0.0580*** (0.0119)
Illiterate	-0.0232 (0.0156)	-0.00886 (0.0153)	-0.0132 (0.0157)	-0.0258 (0.0153)	-0.00912 (0.0151)	-0.0141 (0.0156)	-0.0227 (0.0160)	-0.00853 (0.0156)	-0.0219 (0.0161)
Indigenous	-0.0573*** (0.0108)	-0.0614*** (0.0106)	-0.0250* (0.0107)	-0.0538*** (0.0110)	-0.0546*** (0.0108)	-0.0229* (0.0109)	-0.0177 (0.0123)	-0.0223 (0.0120)	0.00607 (0.0120)
Occupation									
Merchant	0.0189* (0.00864)	0.0307*** (0.00813)	0.0311*** (0.00831)	0.0139 (0.00859)	0.0204* (0.00807)	0.0232** (0.00828)	0.0136 (0.00876)	0.0191* (0.00827)	0.0224** (0.00845)
Public security	-0.0725** (0.0223)	-0.0990*** (0.0217)	0.00462 (0.0210)	-0.0714** (0.0219)	-0.102*** (0.0213)	0.00464 (0.0208)	-0.0745*** (0.0225)	-0.109*** (0.0218)	0.00556 (0.0215)
Private security	-0.0237* (0.0110)	-0.0184 (0.0105)	-0.0343** (0.0108)	-0.0167 (0.0110)	-0.00754 (0.0105)	-0.0248* (0.0109)	-0.0178 (0.0113)	-0.00792 (0.0108)	-0.0254* (0.0112)
Rural worker	-0.0582*** (0.00987)	-0.0599*** (0.00953)	-0.0497*** (0.00974)	-0.0475*** (0.00988)	-0.0415*** (0.00956)	-0.0311** (0.00978)	-0.0349*** (0.0106)	-0.0259* (0.0102)	-0.0224* (0.0105)
Craftsman	-0.0134 (0.00901)	-0.00350 (0.00854)	-0.0195* (0.00881)	-0.0123 (0.00892)	0.00123 (0.00846)	-0.0138 (0.00873)	-0.0103 (0.00914)	0.00269 (0.00868)	-0.0137 (0.00895)
Blue collar	0.00663 (0.00933)	0.0160 (0.00876)	0.00252 (0.00907)	0.00491 (0.00928)	0.0117 (0.00870)	0.00112 (0.00905)	0.00107 (0.00950)	0.00987 (0.00893)	-0.00166 (0.00928)
Age									
26-35	-0.0425*** (0.00644)	-0.0490*** (0.00598)	-0.0122* (0.00620)	-0.0424*** (0.00637)	-0.0486*** (0.00591)	-0.0125* (0.00614)	-0.0391*** (0.00651)	-0.0447*** (0.00606)	-0.00959 (0.00631)
36-45	-0.110*** (0.00788)	-0.122*** (0.00755)	-0.0665*** (0.00770)	-0.107*** (0.00779)	-0.118*** (0.00748)	-0.0642*** (0.00763)	-0.104*** (0.00799)	-0.114*** (0.00767)	-0.0609*** (0.00783)
46-55	-0.230*** (0.0112)	-0.250*** (0.0114)	-0.175*** (0.0116)	-0.229*** (0.0112)	-0.247*** (0.0113)	-0.174*** (0.0114)	-0.220*** (0.0116)	-0.237*** (0.0117)	-0.170*** (0.0118)
56-65	-0.299*** (0.0181)	-0.345*** (0.0190)	-0.255*** (0.0195)	-0.295*** (0.0180)	-0.338*** (0.0187)	-0.250*** (0.0194)	-0.292*** (0.0189)	-0.335*** (0.0189)	-0.245*** (0.0199)
65+	-0.402*** (0.0274)	-0.478*** (0.0309)	-0.356*** (0.0338)	-0.387*** (0.0277)	-0.462*** (0.0306)	-0.343*** (0.0341)	-0.381*** (0.0296)	-0.461*** (0.0320)	-0.344*** (0.0353)
Education									
Primary or less	0.0176 (0.0184)	-0.00262 (0.0182)	0.0188 (0.0189)	0.00971 (0.0181)	-0.00640 (0.0181)	0.0154 (0.0187)	0.0240 (0.0190)	0.00220 (0.0188)	0.0162 (0.0192)
Middle school	0.0462* (0.0191)	0.0174 (0.0189)	0.0492* (0.0195)	0.0467* (0.0188)	0.0238 (0.0188)	0.0562** (0.0193)	0.0625** (0.0198)	0.0319 (0.0194)	0.0557** (0.0199)
High school	0.0794*** (0.0199)	0.0450* (0.0195)	0.0861*** (0.0202)	0.0855*** (0.0196)	0.0560** (0.0194)	0.0978*** (0.0200)	0.0984*** (0.0205)	0.0629** (0.0200)	0.0959*** (0.0205)
College or graduate	0.0558* (0.0229)	-0.00184 (0.0224)	0.0873*** (0.0229)	0.0605** (0.0225)	0.00865 (0.0222)	0.0963*** (0.0227)	0.0708** (0.0235)	0.0120 (0.0230)	0.0934*** (0.0233)
Wealth index, quantiles									
25% - 50%	-0.0134 (0.00768)	-0.0176* (0.00725)	-0.0237** (0.00747)	-0.0168* (0.00758)	-0.0168* (0.00717)	-0.0227** (0.00740)	-0.0204** (0.00775)	-0.0216** (0.00734)	-0.0252*** (0.00757)
50% - 75%	-0.0154* (0.00779)	-0.0117 (0.00736)	-0.00169 (0.00755)	-0.0118 (0.00772)	-0.00451 (0.00729)	0.00474 (0.00751)	-0.0129 (0.00791)	-0.00261 (0.00746)	0.00501 (0.00769)
75% - 100%	-0.00304 (0.00792)	-0.0104 (0.00752)	-0.0111 (0.00772)	0.00618 (0.00793)	0.00112 (0.00753)	-0.00187 (0.00774)	0.00501 (0.00813)	-0.000152 (0.00772)	-0.00196 (0.00795)
Constant	0.594** (0.225)	0.608** (0.233)	0.505* (0.244)	0.396 (0.212)	0.408 (0.221)	0.355 (0.236)	0.461 (0.235)	0.496* (0.253)	0.398 (0.227)
N	31931	31961	31926	31931	31961	31926	31931	31961	31926
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A13: Logits: anticipation effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Torture	Brute	Threats	Torture	Brute	Threats	Torture	Brute	Threats
Reform announced	0.167** (0.0538)	0.0392 (0.0569)	0.109* (0.0552)	0.00566 (0.0579)	-0.0506 (0.0598)	0.0368 (0.0588)	0.0155 (0.0605)	-0.0510 (0.0627)	0.0507 (0.0614)
Reform	-0.244*** (0.0693)	-0.507*** (0.0717)	-0.349*** (0.0701)	-0.259*** (0.0755)	-0.349*** (0.0768)	-0.207** (0.0756)	-0.214** (0.0800)	-0.308*** (0.0812)	-0.197* (0.0799)
Male	0.597*** (0.0541)	0.732*** (0.0541)	-0.263*** (0.0578)	0.604*** (0.0548)	0.745*** (0.0550)	-0.283*** (0.0584)	0.635*** (0.0568)	0.759*** (0.0569)	-0.289*** (0.0603)
Illiterate	-0.102 (0.0676)	-0.0385 (0.0688)	-0.0549 (0.0671)	-0.114 (0.0679)	-0.0384 (0.0696)	-0.0584 (0.0676)	-0.104 (0.0735)	-0.0376 (0.0754)	-0.0945 (0.0724)
Indigenous language	-0.243*** (0.0460)	-0.277*** (0.0468)	-0.110* (0.0462)	-0.231*** (0.0481)	-0.250*** (0.0489)	-0.101* (0.0481)	-0.0798 (0.0558)	-0.109 (0.0574)	0.0256 (0.0563)
Occupation									
Merchant	0.0807* (0.0372)	0.148*** (0.0395)	0.144*** (0.0388)	0.0595 (0.0379)	0.101* (0.0402)	0.110** (0.0394)	0.0606 (0.0391)	0.0980* (0.0417)	0.111** (0.0405)
Public security	-0.309*** (0.0935)	-0.456*** (0.0957)	0.0225 (0.0988)	-0.313*** (0.0940)	-0.484*** (0.0964)	0.0244 (0.100)	-0.337*** (0.0993)	-0.535*** (0.102)	0.0331 (0.105)
Private security	-0.101* (0.0470)	-0.0867 (0.0492)	-0.151** (0.0479)	-0.0754 (0.0482)	-0.0388 (0.0503)	-0.113* (0.0490)	-0.0796 (0.0501)	-0.0403 (0.0522)	-0.118* (0.0509)
Rural worker	-0.249*** (0.0422)	-0.275*** (0.0437)	-0.216*** (0.0428)	-0.209*** (0.0434)	-0.192*** (0.0448)	-0.135** (0.0438)	-0.164*** (0.0479)	-0.130** (0.0493)	-0.102* (0.0482)
Craftsman	-0.0569 (0.0385)	-0.0173 (0.0406)	-0.0860* (0.0393)	-0.0544 (0.0391)	0.00617 (0.0413)	-0.0615 (0.0399)	-0.0446 (0.0407)	0.0144 (0.0429)	-0.0594 (0.0413)
Blue collar	0.0276 (0.0400)	0.0767 (0.0425)	0.0114 (0.0412)	0.0194 (0.0409)	0.0581 (0.0434)	0.00463 (0.0421)	0.00479 (0.0424)	0.0518 (0.0450)	-0.00824 (0.0435)
Age									
26-35	-0.180*** (0.0274)	-0.243*** (0.0296)	-0.0566* (0.0285)	-0.184*** (0.0278)	-0.246*** (0.0300)	-0.0583* (0.0289)	-0.175*** (0.0290)	-0.236*** (0.0313)	-0.0464 (0.0300)
36-45	-0.460*** (0.0331)	-0.566*** (0.0345)	-0.294*** (0.0338)	-0.460*** (0.0336)	-0.562*** (0.0351)	-0.290*** (0.0342)	-0.464*** (0.0352)	-0.567*** (0.0368)	-0.285*** (0.0358)
46-55	-0.972*** (0.0501)	-1.096*** (0.0492)	-0.744*** (0.0485)	-0.990*** (0.0509)	-1.111*** (0.0498)	-0.752*** (0.0489)	-0.996*** (0.0537)	-1.111*** (0.0527)	-0.769*** (0.0519)
56-65	-1.320*** (0.0926)	-1.506*** (0.0868)	-1.078*** (0.0847)	-1.330*** (0.0940)	-1.512*** (0.0874)	-1.077*** (0.0858)	-1.379*** (0.100)	-1.578*** (0.0938)	-1.112*** (0.0916)
65+	-2.018*** (0.219)	-2.206*** (0.192)	-1.559*** (0.178)	-1.989*** (0.222)	-2.178*** (0.194)	-1.531*** (0.181)	-2.062*** (0.237)	-2.295*** (0.208)	-1.653*** (0.197)
Education									
Primary or less	0.0800 (0.0810)	-0.0136 (0.0823)	0.0798 (0.0805)	0.0473 (0.0814)	-0.0342 (0.0837)	0.0647 (0.0810)	0.125 (0.0888)	0.0155 (0.0912)	0.0806 (0.0873)
Middle school	0.200* (0.0838)	0.0780 (0.0854)	0.212* (0.0835)	0.208* (0.0844)	0.110 (0.0870)	0.248** (0.0842)	0.299** (0.0918)	0.160 (0.0945)	0.266** (0.0904)
High school	0.342*** (0.0870)	0.214* (0.0891)	0.384*** (0.0870)	0.378*** (0.0876)	0.270** (0.0906)	0.445*** (0.0878)	0.460*** (0.0949)	0.319** (0.0980)	0.461*** (0.0939)
College or graduate	0.242* (0.0996)	-0.0119 (0.102)	0.385*** (0.101)	0.269** (0.100)	0.0363 (0.103)	0.432*** (0.102)	0.338** (0.108)	0.0643 (0.110)	0.447*** (0.108)
Wealth index, quantiles									
25% - 50%	-0.0570 (0.0329)	-0.0853* (0.0349)	-0.108** (0.0338)	-0.0735* (0.0333)	-0.0831* (0.0354)	-0.105** (0.0343)	-0.0907*** (0.0346)	-0.108** (0.0367)	-0.120*** (0.0355)
50% - 75%	-0.0653 (0.0333)	-0.0560 (0.0352)	-0.00882 (0.0344)	-0.0508 (0.0339)	-0.0217 (0.0358)	0.0211 (0.0350)	-0.0569 (0.0353)	-0.0117 (0.0373)	0.0242 (0.0364)
75% - 100%	-0.0135 (0.0340)	-0.0496 (0.0358)	-0.0513 (0.0348)	0.0268 (0.0349)	0.00565 (0.0368)	-0.0102 (0.0356)	0.0226 (0.0365)	0.00121 (0.0385)	-0.00963 (0.0373)
Constant	0.443 (1.201)	0.508 (1.271)	0.0475 (0.973)	-0.411 (1.173)	-0.438 (1.203)	-0.619 (0.945)	-0.166 (1.254)	-0.0116 (1.381)	-0.434 (0.904)
<i>N</i>	31931	31958	31926	31931	31958	31926	31113	31000	30964
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.4.4 Temporal scope of reform effects

A possible objection to our empirical approach to causally identify the effect of the reform is that we might be restricting too much the time-span to identify any effects. Because it takes time for police and other state authorities to adjust to the reform, we might be underestimating its effects. We run the same logits on a sample that includes slightly longer term outcomes. Here, we include indicator variables for 0-12 and 0-18 month periods after the implementation of the reform. Because of the temporal scope of the sample, we subset to cases in which the reform was adopted by no later than June 30, 2015 or December 31, 2015. For late implementers, we simply lack data on anyone arrested more than six months after the reform. Our sample size is consequently much smaller. Sacrificing observations harms the precision of our estimates in this test, which causes some of the estimates, which retain their negative signs, to lose significance. Nonetheless, we take confidence from the clear pattern in the models. These results are reported in Tables A14 and A15, where we see that the declines in the incidence of detainee abuse persist into periods longer after the reform.

Table A14: Reform: 12 month span

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.0700** (0.0245)	-0.0500* (0.0237)	-0.0752** (0.0245)	-0.0485 (0.0275)	-0.0420 (0.0267)	-0.0593* (0.0278)	-0.0547 (0.0336)	-0.0371 (0.0331)	-0.0609 (0.0345)
Male	0.186*** (0.0419)	0.122** (0.0406)	-0.0524 (0.0434)	0.190*** (0.0404)	0.129** (0.0395)	-0.0552 (0.0429)	0.173*** (0.0442)	0.113** (0.0430)	-0.0606 (0.0478)
Illiterate	0.00452 (0.0476)	-0.00650 (0.0457)	-0.00723 (0.0505)	-0.00533 (0.0474)	-0.0167 (0.0442)	-0.0267 (0.0489)	-0.0231 (0.0541)	-0.0210 (0.0513)	-0.0333 (0.0545)
Indigenous	-0.0607 (0.0393)	-0.0283 (0.0378)	-0.0850* (0.0395)	-0.102* (0.0413)	-0.0707 (0.0391)	-0.0980* (0.0416)	-0.142** (0.0513)	-0.115* (0.0467)	-0.115* (0.0517)
Occupation									
Merchant	0.0282 (0.0307)	0.0185 (0.0305)	0.00735 (0.0312)	0.0104 (0.0302)	-0.00143 (0.0299)	0.00188 (0.0309)	0.0123 (0.0331)	-0.00820 (0.0326)	0.00386 (0.0337)
Public security	0.00996 (0.0824)	0.0130 (0.0854)	0.0597 (0.0822)	-0.000118 (0.0784)	-0.00754 (0.0814)	0.0761 (0.0811)	0.0178 (0.0848)	-0.00511 (0.0928)	0.110 (0.0905)
Private security	0.00598 (0.0395)	-0.0263 (0.0379)	-0.0603 (0.0391)	0.0117 (0.0401)	-0.00158 (0.0387)	-0.0487 (0.0401)	0.00570 (0.0433)	-0.0108 (0.0413)	-0.0553 (0.0432)
Rural worker	-0.0180 (0.0316)	-0.0291 (0.0307)	-0.00536 (0.0319)	-0.0281 (0.0318)	-0.0361 (0.0307)	0.00193 (0.0321)	-0.00233 (0.0364)	-0.0201 (0.0347)	0.0349 (0.0371)
Craftsman	0.00857 (0.0295)	0.000468 (0.0289)	0.00142 (0.0298)	-0.000693 (0.0291)	-0.0100 (0.0284)	-0.000906 (0.0296)	0.00767 (0.0317)	-0.00676 (0.0309)	0.00444 (0.0322)
Blue collar	0.00538 (0.0327)	0.0311 (0.0321)	0.000863 (0.0331)	0.000308 (0.0322)	0.0239 (0.0314)	0.00756 (0.0329)	-0.00517 (0.0355)	0.0152 (0.0348)	0.0130 (0.0364)
Age									
26-35	-0.0679** (0.0232)	-0.0413 (0.0232)	0.00515 (0.0233)	-0.0717** (0.0228)	-0.0454* (0.0226)	0.00325 (0.0230)	-0.0684** (0.0251)	-0.0373 (0.0247)	0.0130 (0.0253)
36-45	-0.149*** (0.0265)	-0.142*** (0.0259)	-0.0850** (0.0269)	-0.154*** (0.0260)	-0.150*** (0.0252)	-0.0915*** (0.0267)	-0.136*** (0.0286)	-0.138*** (0.0278)	-0.0827** (0.0298)
46-55	-0.260*** (0.0361)	-0.215*** (0.0336)	-0.165*** (0.0373)	-0.278*** (0.0355)	-0.234*** (0.0334)	-0.180*** (0.0370)	-0.268*** (0.0392)	-0.222*** (0.0366)	-0.173*** (0.0409)
56-65	-0.328*** (0.0578)	-0.253*** (0.0539)	-0.213*** (0.0584)	-0.335*** (0.0584)	-0.262*** (0.0554)	-0.218*** (0.0566)	-0.319*** (0.0666)	-0.249*** (0.0657)	-0.199** (0.0673)
65+	-0.498*** (0.0770)	-0.396*** (0.0643)	-0.400*** (0.0860)	-0.503*** (0.0759)	-0.411*** (0.0648)	-0.407*** (0.0864)	-0.492*** (0.0921)	-0.390*** (0.0815)	-0.353** (0.108)
Education									
Primary or less	0.0598 (0.0602)	0.0692 (0.0558)	-0.0312 (0.0624)	0.0302 (0.0587)	0.0408 (0.0549)	-0.0596 (0.0616)	0.0389 (0.0661)	0.0761 (0.0623)	-0.0391 (0.0684)
Middle school	0.0226 (0.0632)	0.0584 (0.0589)	-0.0287 (0.0652)	0.00804 (0.0617)	0.0466 (0.0584)	-0.0437 (0.0647)	0.0105 (0.0693)	0.0635 (0.0660)	-0.0320 (0.0719)
High school	0.0303 (0.0657)	0.0888 (0.0616)	0.0100 (0.0677)	0.00890 (0.0641)	0.0678 (0.0610)	-0.00427 (0.0672)	0.0137 (0.0721)	0.0796 (0.0690)	0.0128 (0.0748)
College or graduate	0.0360 (0.0738)	0.102 (0.0707)	0.0212 (0.0765)	-0.00369 (0.0734)	0.0576 (0.0709)	-0.00440 (0.0769)	-0.0225 (0.0822)	0.0599 (0.0796)	0.00558 (0.0850)
Wealth index, quantiles									
25%-50%	-0.0267 (0.0271)	-0.0407 (0.0270)	-0.0513 (0.0274)	-0.0294 (0.0264)	-0.0461 (0.0260)	-0.0503 (0.0271)	-0.0153 (0.0290)	-0.0373 (0.0282)	-0.0422 (0.0298)
50% - 75%	-0.0634* (0.0271)	-0.0903*** (0.0268)	-0.0626* (0.0275)	-0.0540* (0.0266)	-0.0843** (0.0260)	-0.0457 (0.0271)	-0.0409 (0.0291)	-0.0841** (0.0286)	-0.0331 (0.0299)
75%-100%	-0.0156 (0.0272)	-0.0222 (0.0271)	-0.0417 (0.0275)	-0.00658 (0.0269)	-0.0167 (0.0267)	-0.0303 (0.0273)	-0.00173 (0.0299)	-0.00902 (0.0294)	-0.0341 (0.0304)
Constant	0.504*** (0.0851)	0.412*** (0.0825)	0.778*** (0.0867)	0.422*** (0.0904)	0.325*** (0.0876)	0.749*** (0.0927)	0.417*** (0.102)	0.296** (0.0994)	0.726*** (0.104)
<i>N</i>	2769	2768	2773	2769	2768	2773	2769	2768	2773
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: We lose a large portion of the sample in this specification and consequently lose significance in some cases.

Table A15: Reform: 18 month span

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.138*** (0.0355)	-0.142*** (0.0342)	-0.145*** (0.0352)	-0.0710 (0.0374)	-0.0720* (0.0363)	-0.102** (0.0376)	-0.0440 (0.0442)	-0.0310 (0.0438)	-0.106* (0.0443)
Male	0.264*** (0.0442)	0.182*** (0.0437)	-0.0606 (0.0459)	0.242*** (0.0432)	0.161*** (0.0428)	-0.0803 (0.0446)	0.228*** (0.0468)	0.172*** (0.0459)	-0.0785 (0.0496)
Illiterate	0.00703 (0.0511)	0.0456 (0.0534)	-0.0227 (0.0564)	-0.00194 (0.0511)	0.0374 (0.0528)	-0.0348 (0.0551)	0.0144 (0.0589)	0.0665 (0.0609)	-0.0310 (0.0598)
Indigenous	-0.0900* (0.0443)	-0.0795 (0.0426)	-0.0671 (0.0439)	-0.118** (0.0440)	-0.105* (0.0419)	-0.0956* (0.0433)	-0.112* (0.0558)	-0.131** (0.0509)	-0.0849 (0.0549)
Occupation									
Merchant	-0.00826 (0.0332)	0.0101 (0.0338)	-0.0412 (0.0339)	-0.00907 (0.0325)	0.00687 (0.0332)	-0.0370 (0.0332)	-0.00769 (0.0355)	0.00559 (0.0363)	-0.0277 (0.0364)
Public security	-0.0528 (0.0759)	-0.0753 (0.0784)	0.0342 (0.0740)	-0.0207 (0.0722)	-0.0497 (0.0758)	0.0789 (0.0735)	-0.0480 (0.0759)	-0.0705 (0.0814)	0.114 (0.0817)
Private security	-0.0379 (0.0501)	0.0136 (0.0498)	-0.0393 (0.0504)	-0.0284 (0.0474)	0.0259 (0.0483)	-0.0226 (0.0490)	-0.0344 (0.0526)	0.0243 (0.0527)	-0.0147 (0.0544)
Rural worker	-0.0589 (0.0345)	-0.0269 (0.0348)	-0.0735* (0.0348)	-0.0383 (0.0342)	-0.00565 (0.0346)	-0.0458 (0.0347)	-0.0184 (0.0396)	0.0152 (0.0395)	-0.0171 (0.0403)
Craftsman	-0.0522 (0.0324)	-0.0316 (0.0327)	-0.0862** (0.0331)	-0.0441 (0.0318)	-0.0254 (0.0323)	-0.0716* (0.0328)	-0.0314 (0.0351)	-0.0204 (0.0353)	-0.0496 (0.0362)
Blue collar	-0.0798* (0.0368)	-0.0579 (0.0370)	-0.105** (0.0375)	-0.0640 (0.0362)	-0.0456 (0.0365)	-0.0890* (0.0371)	-0.0753 (0.0398)	-0.0579 (0.0400)	-0.0762 (0.0407)
Age									
26-35	-0.0386 (0.0250)	-0.0493 (0.0258)	0.0134 (0.0254)	-0.0503* (0.0244)	-0.0596* (0.0251)	0.00506 (0.0249)	-0.0446 (0.0270)	-0.0574* (0.0275)	0.0185 (0.0278)
36-45	-0.108*** (0.0291)	-0.131*** (0.0292)	-0.0568 (0.0295)	-0.125*** (0.0283)	-0.147*** (0.0284)	-0.0691* (0.0288)	-0.108*** (0.0316)	-0.139*** (0.0317)	-0.0530 (0.0324)
46-55	-0.241*** (0.0399)	-0.237*** (0.0377)	-0.138*** (0.0415)	-0.268*** (0.0391)	-0.263*** (0.0381)	-0.158*** (0.0409)	-0.258*** (0.0435)	-0.257*** (0.0420)	-0.149** (0.0461)
56-65	-0.352*** (0.0592)	-0.283*** (0.0563)	-0.224*** (0.0619)	-0.349*** (0.0570)	-0.278*** (0.0563)	-0.227*** (0.0590)	-0.345*** (0.0613)	-0.297*** (0.0623)	-0.236*** (0.0654)
65+	-0.560*** (0.0624)	-0.443*** (0.0633)	-0.418*** (0.0792)	-0.535*** (0.0613)	-0.417*** (0.0638)	-0.393*** (0.0795)	-0.498*** (0.0775)	-0.378*** (0.0780)	-0.332*** (0.0950)
Education									
Primary or less	0.0393 (0.0624)	0.0940 (0.0623)	-0.0292 (0.0674)	0.0326 (0.0604)	0.0867 (0.0625)	-0.0398 (0.0661)	0.0749 (0.0702)	0.102 (0.0711)	0.00137 (0.0731)
Middle school	0.00726 (0.0653)	0.0814 (0.0652)	-0.0397 (0.0702)	0.0154 (0.0633)	0.0846 (0.0656)	-0.0366 (0.0692)	0.0636 (0.0733)	0.0977 (0.0744)	-0.000505 (0.0767)
High school	0.00140 (0.0683)	0.0980 (0.0686)	-0.0218 (0.0733)	0.00495 (0.0659)	0.0966 (0.0686)	-0.0184 (0.0720)	0.0495 (0.0765)	0.106 (0.0781)	0.0188 (0.0801)
College or graduate	-0.00133 (0.0766)	0.0341 (0.0767)	-0.0456 (0.0818)	0.0140 (0.0755)	0.0479 (0.0771)	-0.0318 (0.0814)	0.0339 (0.0866)	0.0525 (0.0868)	0.000325 (0.0905)
Wealth index, quantiles									
25%-50%	-0.0136 (0.0305)	-0.0281 (0.0312)	-0.0942** (0.0307)	-0.0112 (0.0296)	-0.0299 (0.0301)	-0.0858** (0.0301)	-0.0128 (0.0323)	-0.0270 (0.0324)	-0.0806* (0.0331)
50% - 75%	-0.0288 (0.0303)	-0.0920** (0.0306)	-0.0902** (0.0307)	-0.0143 (0.0296)	-0.0806** (0.0298)	-0.0706* (0.0302)	-0.0117 (0.0323)	-0.0844* (0.0328)	-0.0614 (0.0335)
75%-100%	-0.0315 (0.0302)	-0.0629* (0.0306)	-0.0860** (0.0305)	-0.0183 (0.0297)	-0.0509 (0.0300)	-0.0709* (0.0298)	-0.0233 (0.0331)	-0.0462 (0.0332)	-0.0697* (0.0334)
Constant	0.396* (0.181)	0.418* (0.175)	1.000*** (0.164)	0.304 (0.182)	0.363* (0.181)	0.887*** (0.162)	0.311 (0.190)	0.380 (0.194)	0.852*** (0.195)
N	2242	2240	2246	2242	2240	2246	2242	2240	2246
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: We lose a large portion of the sample in this specification and consequently lose significance in some cases.

9.4.5 Intensity of Abuse

Table A16: OLS: intensity of abuse

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.227*** (0.0203)	-0.238*** (0.0276)	-0.199*** (0.0197)	-0.116*** (0.0215)	-0.145*** (0.0299)	-0.0937*** (0.0210)	-0.0982*** (0.0231)	-0.0986** (0.0325)	-0.0886*** (0.0227)
Male	0.325*** (0.0217)	0.397*** (0.0323)	-0.178*** (0.0221)	0.325*** (0.0214)	0.396*** (0.0317)	-0.179*** (0.0218)	0.325*** (0.0218)	0.412*** (0.0324)	-0.177*** (0.0223)
Illiterate	-0.00228 (0.0271)	-0.0615 (0.0415)	-0.0264 (0.0266)	-0.00462 (0.0267)	-0.0748 (0.0407)	-0.0316 (0.0263)	-0.00448 (0.0278)	-0.0677 (0.0429)	-0.0476 (0.0273)
Indigenous	-0.112*** (0.0189)	-0.144*** (0.0290)	-0.0459* (0.0186)	-0.0983*** (0.0194)	-0.137*** (0.0300)	-0.0459* (0.0191)	-0.0400 (0.0217)	-0.0328 (0.0342)	0.00244 (0.0213)
Occupation									
Merchant	0.0572*** (0.0150)	0.0521* (0.0243)	0.0813*** (0.0149)	0.0366* (0.0149)	0.0402 (0.0240)	0.0657*** (0.0149)	0.0339* (0.0153)	0.0416 (0.0245)	0.0640*** (0.0151)
Public security	-0.153*** (0.0404)	-0.185*** (0.0636)	0.0355 (0.0385)	-0.162*** (0.0394)	-0.180** (0.0623)	0.0314 (0.0380)	-0.174*** (0.0404)	-0.186** (0.0635)	0.0357 (0.0393)
Private security	-0.0447* (0.0192)	-0.0745* (0.0301)	-0.0536** (0.0190)	-0.0289 (0.0191)	-0.0614* (0.0298)	-0.0331 (0.0190)	-0.0299 (0.0196)	-0.0706* (0.0305)	-0.0345 (0.0196)
Rural worker	-0.110*** (0.0173)	-0.159*** (0.0270)	-0.0734*** (0.0170)	-0.0793*** (0.0174)	-0.130*** (0.0270)	-0.0447*** (0.0171)	-0.0535*** (0.0186)	-0.0821** (0.0290)	-0.0276 (0.0182)
Craftsman	-0.0256 (0.0156)	-0.0479 (0.0250)	-0.0266 (0.0155)	-0.0183 (0.0155)	-0.0468 (0.0247)	-0.0170 (0.0153)	-0.0146 (0.0158)	-0.0388 (0.0252)	-0.0164 (0.0157)
Blue collar	0.0300 (0.0162)	0.0416 (0.0266)	0.0357* (0.0162)	0.0183 (0.0161)	0.0300 (0.0263)	0.0284 (0.0162)	0.0157 (0.0165)	0.0200 (0.0268)	0.0224 (0.0165)
Age									
26-35	-0.0731*** (0.0111)	-0.0584** (0.0183)	-0.0206 (0.0112)	-0.0710*** (0.0110)	-0.0570** (0.0179)	-0.0203 (0.0110)	-0.0634*** (0.0112)	-0.0503** (0.0183)	-0.0157 (0.0113)
36-45	-0.201*** (0.0138)	-0.253*** (0.0215)	-0.126*** (0.0136)	-0.193*** (0.0136)	-0.246*** (0.0212)	-0.122*** (0.0134)	-0.187*** (0.0139)	-0.238*** (0.0217)	-0.120*** (0.0137)
46-55	-0.433*** (0.0199)	-0.556*** (0.0288)	-0.304*** (0.0196)	-0.427*** (0.0197)	-0.554*** (0.0286)	-0.304*** (0.0194)	-0.410*** (0.0204)	-0.528*** (0.0299)	-0.295*** (0.0201)
56-65	-0.609*** (0.0311)	-0.739*** (0.0420)	-0.462*** (0.0309)	-0.596*** (0.0308)	-0.731*** (0.0424)	-0.457*** (0.0308)	-0.586*** (0.0322)	-0.721*** (0.0449)	-0.447*** (0.0320)
65+	-0.772*** (0.0537)	-0.941*** (0.0580)	-0.600*** (0.0504)	-0.742*** (0.0532)	-0.899*** (0.0591)	-0.583*** (0.0514)	-0.736*** (0.0562)	-0.884*** (0.0647)	-0.585*** (0.0523)
Education									
Primary or less	-0.0116 (0.0322)	0.00231 (0.0492)	0.0347 (0.0317)	-0.0226 (0.0318)	-0.0240 (0.0484)	0.0297 (0.0312)	-0.00645 (0.0332)	0.00637 (0.0508)	0.0305 (0.0324)
Middle school	0.0359 (0.0334)	0.0945 (0.0514)	0.0936** (0.0329)	0.0425 (0.0330)	0.0894 (0.0506)	0.110*** (0.0325)	0.0575 (0.0344)	0.124* (0.0530)	0.109** (0.0337)
High school	0.117*** (0.0347)	0.222*** (0.0537)	0.185*** (0.0342)	0.133*** (0.0343)	0.234*** (0.0529)	0.209*** (0.0337)	0.146*** (0.0357)	0.265*** (0.0553)	0.206*** (0.0350)
College or graduate	0.0385 (0.0402)	0.215*** (0.0636)	0.227*** (0.0397)	0.0546 (0.0397)	0.224*** (0.0627)	0.243*** (0.0392)	0.0654 (0.0413)	0.233*** (0.0650)	0.235*** (0.0405)
Wealth index, quantiles									
25%-50%	-0.0273* (0.0134)	-0.0492* (0.0217)	-0.0446*** (0.0133)	-0.0273* (0.0132)	-0.0640** (0.0214)	-0.0435*** (0.0132)	-0.0356** (0.0135)	-0.0742*** (0.0218)	-0.0497*** (0.0135)
50% - 75%	-0.0293* (0.0135)	-0.0809*** (0.0216)	-0.0243 (0.0135)	-0.0169 (0.0134)	-0.0758*** (0.0213)	-0.0152 (0.0134)	-0.0147 (0.0137)	-0.0784*** (0.0218)	-0.0172 (0.0137)
75%-100%	-0.00998 (0.0138)	-0.0303 (0.0221)	-0.0168 (0.0137)	0.00984 (0.0138)	-0.00583 (0.0220)	-0.00314 (0.0137)	0.00696 (0.0141)	-0.00455 (0.0226)	-0.00283 (0.0141)
Constant	1.196** (0.455)	2.059* (0.832)	1.046* (0.476)	0.812 (0.421)	1.470 (0.811)	0.743 (0.463)	0.939* (0.473)	1.713* (0.814)	0.844 (0.441)
N	31961	31931	31926	31961	31931	31926	31961	31931	31926
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses
 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A17: OLS: intensity of abuse; sample restricted to six months before and after

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Brute	Torture	Threat	Brute	Torture	Threat	Brute	Torture	Threat
Reform	-0.125*** (0.0276)	-0.159*** (0.0374)	-0.129*** (0.0267)	-0.0558 (0.0289)	-0.135*** (0.0401)	-0.0863** (0.0283)	-0.0665 (0.0380)	-0.132* (0.0512)	-0.0956** (0.0370)
Male	0.324*** (0.0517)	0.410*** (0.0659)	-0.123* (0.0573)	0.346*** (0.0503)	0.424*** (0.0646)	-0.116* (0.0564)	0.356*** (0.0553)	0.442*** (0.0698)	-0.106 (0.0622)
Illiterate	0.0271 (0.0686)	-0.0199 (0.0944)	0.0348 (0.0721)	-0.00171 (0.0679)	-0.0587 (0.0892)	-0.00189 (0.0696)	0.0149 (0.0810)	-0.0344 (0.106)	0.0349 (0.0789)
Indigenous	-0.122* (0.0549)	-0.178* (0.0738)	-0.141** (0.0545)	-0.148** (0.0568)	-0.222** (0.0784)	-0.154** (0.0559)	-0.0988 (0.0719)	-0.124 (0.1000)	-0.159* (0.0670)
Occupation									
Merchant	0.0533 (0.0388)	0.0250 (0.0555)	0.0972* (0.0382)	0.0118 (0.0384)	-0.0000731 (0.0549)	0.0782* (0.0380)	0.0277 (0.0425)	0.0112 (0.0611)	0.0787 (0.0419)
Public security	-0.0106 (0.152)	-0.0453 (0.211)	0.138 (0.139)	-0.00690 (0.143)	-0.0167 (0.196)	0.122 (0.132)	-0.0119 (0.158)	-0.0163 (0.222)	0.174 (0.153)
Private security	-0.0738 (0.0480)	-0.0355 (0.0689)	-0.0462 (0.0468)	-0.0741 (0.0481)	-0.0397 (0.0685)	-0.0408 (0.0470)	-0.0634 (0.0538)	-0.0281 (0.0756)	-0.0212 (0.0511)
Rural worker	-0.0814 (0.0465)	-0.0344 (0.0661)	-0.0379 (0.0449)	-0.0757 (0.0464)	-0.0632 (0.0669)	-0.0222 (0.0450)	-0.0516 (0.0545)	-0.0646 (0.0778)	-0.0230 (0.0530)
Craftsman	-0.0110 (0.0406)	-0.0813 (0.0557)	-0.00834 (0.0389)	-0.0125 (0.0401)	-0.0839 (0.0549)	-0.000931 (0.0387)	-0.00398 (0.0448)	-0.0879 (0.0614)	-0.000138 (0.0430)
Blue collar	0.0218 (0.0432)	-0.00513 (0.0603)	0.0736 (0.0421)	-0.00226 (0.0426)	-0.0209 (0.0594)	0.0699 (0.0418)	0.0125 (0.0475)	-0.000371 (0.0662)	0.0801 (0.0460)
Age									
26-35	-0.0966** (0.0306)	-0.107* (0.0441)	-0.0288 (0.0303)	-0.0855** (0.0301)	-0.0987* (0.0431)	-0.0165 (0.0299)	-0.0765* (0.0334)	-0.0875 (0.0472)	0.0120 (0.0328)
36-45	-0.214*** (0.0360)	-0.249*** (0.0498)	-0.148*** (0.0353)	-0.194*** (0.0355)	-0.247*** (0.0490)	-0.138*** (0.0350)	-0.205*** (0.0396)	-0.247*** (0.0547)	-0.140*** (0.0391)
46-55	-0.523*** (0.0467)	-0.589*** (0.0582)	-0.416*** (0.0477)	-0.524*** (0.0467)	-0.610*** (0.0593)	-0.422*** (0.0473)	-0.496*** (0.0544)	-0.589*** (0.0699)	-0.370*** (0.0541)
56-65	-0.482*** (0.0760)	-0.432*** (0.109)	-0.338*** (0.0814)	-0.483*** (0.0760)	-0.445*** (0.113)	-0.332*** (0.0808)	-0.463*** (0.0882)	-0.439*** (0.126)	-0.309*** (0.0902)
65+	-0.702*** (0.109)	-0.916*** (0.0551)	-0.747*** (0.0598)	-0.714*** (0.105)	-0.942*** (0.0746)	-0.755*** (0.0708)	-0.693*** (0.150)	-0.859*** (0.113)	-0.783*** (0.104)
Education									
Primary or less	-0.0333 (0.0889)	0.0324 (0.114)	-0.0407 (0.0910)	-0.0636 (0.0864)	-0.0191 (0.110)	-0.0500 (0.0875)	0.00272 (0.106)	0.108 (0.129)	0.0637 (0.101)
Middle school	0.0124 (0.0918)	0.0815 (0.119)	-0.0189 (0.0938)	-0.00524 (0.0896)	0.0476 (0.114)	-0.0120 (0.0905)	0.0574 (0.108)	0.169 (0.133)	0.0820 (0.104)
High school	0.0427 (0.0945)	0.137 (0.123)	0.0667 (0.0963)	0.0169 (0.0924)	0.0931 (0.119)	0.0689 (0.0932)	0.0735 (0.111)	0.208 (0.138)	0.170 (0.107)
College or graduate	0.0520 (0.109)	0.174 (0.147)	0.150 (0.110)	0.000266 (0.107)	0.0851 (0.144)	0.122 (0.107)	0.0677 (0.127)	0.197 (0.164)	0.249* (0.123)
Wealth index, quantiles									
25%-50%	-0.0626 (0.0359)	-0.120* (0.0512)	-0.128*** (0.0352)	-0.0536 (0.0352)	-0.127* (0.0500)	-0.126*** (0.0347)	-0.0256 (0.0389)	-0.124* (0.0553)	-0.111** (0.0378)
50% - 75%	-0.0526 (0.0362)	-0.116* (0.0515)	-0.0709* (0.0356)	-0.0483 (0.0356)	-0.122* (0.0505)	-0.0660 (0.0353)	-0.0185 (0.0396)	-0.111* (0.0555)	-0.0370 (0.0389)
75%-100%	-0.0829* (0.0367)	-0.134** (0.0517)	-0.0902* (0.0364)	-0.0604 (0.0363)	-0.113* (0.0513)	-0.0795* (0.0364)	-0.0431 (0.0411)	-0.126* (0.0573)	-0.0738 (0.0401)
Constant	0.700** (0.222)	0.562 (0.365)	1.113*** (0.248)	0.646** (0.210)	0.453 (0.355)	1.208*** (0.228)	0.373 (0.279)	0.202 (0.396)	0.909** (0.296)
N	4451	4445	4443	4451	4445	4443	4451	4445	4443
State FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.5 Democratization

9.5.1 Dates of first PRI loss

We define alternation as occurring on the date in which a candidate of a different party formally assumes control of government. The first political alternation in each state is covered in Table A18. Table A19 reports the full models with alternate specifications from Table 8 in the paper.

Table A18: **Year and party that defeated the PRI**

State	PRI loss (ever)	Year	Date opposition takes power (d-m-y)	Party to unseat PRI	Loss before 2000?
Aguascalientes	Yes	1998	1/12/1998	PAN	Yes
Baja California	Yes	1989	1/11/1989	PAN	Yes
Baja California Sur	Yes	1999	5/4/1999	PRD	Yes
Campeche	No	-	-	-	No
Coahuila	No	-	-	-	No
Colima	No	-	-	-	No
Chiapas	Yes	2000	12/8/2000	PAN-PRD	Yes
Chihuahua	Yes	1992	4/10/1992	PAN	No
Ciudad de Mexico	Yes	1997	5/12/1997	PRD	Yes
Durango	Yes	2016	15/9/2016	PAN-PRD	No
Guanajuato	Yes	1991	26/9/1991	PAN	Yes
Guerrero	Yes	2005	1/4/2005	PRD	Yes
Hidalgo	No	-	-	-	No
Jalisco	Yes	1995	1/3/1995	PAN	Yes
Mexico	No	-	-	-	No
Michoacan	Yes	2002	15/2/2002	PRD	No
Morelos	Yes	2000	1/10/2000	PAN	Yes
Nayarit	Yes	1999	19/9/1999	PAN-PRD	Yes
Nuevo Leon	Yes	1997	4/10/1997	PAN	Yes
Oaxaca	Yes	2010	1/12/2010	PAN-PRD	No
Puebla	Yes	2011	1/2/2011	PAN-PRD	No
Queretaro	Yes	1997	1/10/1997	PAN	Yes
Quintana Roo	Yes	2016	25/9/2016	PAN-PRD	No
San Luis Potosi	Yes	2003	26/9/2003	PAN	No
Sinaloa	Yes	2011	1/1/2011	PAN-PRD	No
Sonora	Yes	2009	13/9/2009	PAN	No
Tabasco	Yes	2013	1/1/2013	PRD	No
Tamaulipas	Yes	2016	1/10/2016	PAN	No
Tlaxcala	Yes	1999	15/1/1999	PRD	Yes
Veracruz	Yes	2016	12/1/2016	PAN-PRD	No
Yucatan	Yes	2001	1/8/2001	PAN	No
Zacatecas	Yes	1998	12/9/1998	PRD	Yes

9.5.2 Models for democratization

Table A19: **Party that first defeats PRI**

	OLS - short term	Logit - short term	OLS	Logit
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture
Party to defeat PRI, pre-local democracy				
PAN			-0.0181*	-0.0778*
			(0.00902)	(0.0387)
PRD	0.0427*	0.193*	-0.0326*	-0.138*
	(0.0217)	(0.0958)	(0.0130)	(0.0563)
PAN-PRD	-0.00677	-0.0280	-0.0355***	-0.151***
	(0.0155)	(0.0663)	(0.00806)	(0.0345)
Party to defeat PRI, post-local democracy				
PAN	-0.114***	-0.481***	-0.0328***	-0.140***
	(0.0177)	(0.0753)	(0.00611)	(0.0262)
PRD	-0.00221	-0.00814	-0.101***	-0.426***
	(0.0186)	(0.0794)	(0.00689)	(0.0293)
PAN-PRD	-0.0706***	-0.301***	-0.0700***	-0.297***
	(0.0156)	(0.0667)	(0.00824)	(0.0350)
Male	0.128***	0.550***	0.128***	0.541***
	(0.0195)	(0.0853)	(0.00948)	(0.0404)
Illiterate	-0.00110	-0.00632	-0.0351**	-0.151**
	(0.0242)	(0.107)	(0.0119)	(0.0508)
Indigenous	-0.0264	-0.111	-0.0505***	-0.212***
	(0.0154)	(0.0662)	(0.00876)	(0.0370)
Occupation				
Merchant	0.00195	0.00790	0.0185**	0.0786**
	(0.0142)	(0.0606)	(0.00650)	(0.0277)

Continued on next page

	OLS - short term	Logit - short term	OLS	Logit
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture
Public Security	-0.115** (0.0360)	-0.495** (0.155)	-0.0341* (0.0166)	-0.145* (0.0697)
Private Security	-0.0176 (0.0174)	-0.0747 (0.0742)	-0.0163 (0.00854)	-0.0689 (0.0361)
Rural worker	-0.0548*** (0.0153)	-0.235*** (0.0654)	-0.0177* (0.00772)	-0.0745* (0.0327)
Craftsman	0.00315 (0.0146)	0.0130 (0.0624)	0.00487 (0.00688)	0.0208 (0.0292)
Blue collar	-0.00502 (0.0153)	-0.0227 (0.0654)	0.0103 (0.00711)	0.0435 (0.0302)
Age				
26-35	-0.0589*** (0.0104)	-0.247*** (0.0439)	-0.0359*** (0.00492)	-0.152*** (0.0208)
36-45	-0.134*** (0.0125)	-0.559*** (0.0528)	-0.0990*** (0.00600)	-0.412*** (0.0250)
46-55	-0.229*** (0.0180)	-0.974*** (0.0814)	-0.216*** (0.00875)	-0.903*** (0.0383)
56-65	-0.298*** (0.0278)	-1.334*** (0.148)	-0.300*** (0.0142)	-1.314*** (0.0719)
65+	-0.402*** (0.0410)	-2.043*** (0.334)	-0.390*** (0.0230)	-1.893*** (0.166)
Education				
Primary or less	0.0439 (0.0276)	0.195 (0.125)	-0.0168 (0.0137)	-0.0711 (0.0590)

Continued on next page

	OLS - short term	Logit - short term	OLS	Logit
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture
Middle school	0.0858** (0.0289)	0.373** (0.129)	0.0121 (0.0143)	0.0501 (0.0612)
High school	0.140*** (0.0301)	0.604*** (0.135)	0.0545*** (0.0148)	0.231*** (0.0637)
College or graduate	0.113** (0.0351)	0.489** (0.155)	0.0439* (0.0172)	0.186* (0.0736)
Wealth				
25%-50%	-0.0192 (0.0130)	-0.0815 (0.0555)	-0.00907 (0.00585)	-0.0380 (0.0248)
50% - 75%	-0.00635 (0.0129)	-0.0273 (0.0553)	0.00206 (0.00597)	0.00876 (0.0254)
75%-100%	0.0105 (0.0128)	0.0453 (0.0550)	0.0148* (0.00607)	0.0630* (0.0258)
Constant	0.928*** (0.0430)	-0.856*** (0.177)	0.647** (0.218)	0.665 (1.161)
<i>N</i>	12514	12510	54920	54917

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

9.5.3 Full set of models for drug war

Table A20: **Dates of joint operations examined**

State	Date, beginning of intervention (d-m-y)
Baja California	2-1-2007
Chihuahua	29-3-2008
Durango	5-5-2011
Guerrero	15-1-2007
Michoacan	8-12-2006
Nuevo Leon	1-1-2008
Sinaloa	13-5-2008
Tamaulipas	9-5-2008

Table A21: **OLS: Drug War**

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
Turf War			0.0342**	0.0368***	0.0258**
			(0.0118)	(0.0104)	(0.00868)
Joint Operation				0.0825***	0.123***
				(0.0141)	(0.0101)
Party in power					
PAN	-0.0739***	-0.0631**			
	(0.0134)	(0.0194)			
PRD	0.0178	0.0956***			
	(0.0199)	(0.0237)			
Independent	-0.0226				
	(0.0420)				
Drug war x Party in power					
Continued on next page					

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
PRI		0.0343 (0.0421)			
PAN		-0.0470 (0.0441)			
PRD		0.0162 (0.0467)			
Male	0.149*** (0.0152)	0.149*** (0.0152)	0.142*** (0.0159)	0.144*** (0.0155)	0.0241* (0.0108)
Illiterate	-0.0574** (0.0179)	-0.0562** (0.0179)	-0.0521** (0.0187)	-0.0485** (0.0175)	-0.0322** (0.00986)
Indigenous	-0.0489*** (0.0123)	-0.0483*** (0.0123)	-0.0183 (0.0143)	-0.0609*** (0.0125)	-0.0333*** (0.00679)
Federal prison	0.176*** (0.00814)	0.174*** (0.00814)	0.191*** (0.00929)	0.191*** (0.00889)	0.424*** (0.00936)
Occupation					
Merchant	0.0292** (0.00964)	0.0294** (0.00963)	0.0366*** (0.0101)	0.0361*** (0.00973)	-0.000470 (0.00721)
Public Security	-0.0511* (0.0215)	-0.0500* (0.0216)	-0.0546* (0.0234)	-0.0531* (0.0225)	-0.0793*** (0.0162)
Private Security	-0.0157 (0.0126)	-0.0153 (0.0126)	-0.0172 (0.0132)	-0.0164 (0.0127)	-0.0163 (0.00897)
Rural worker	-0.0448***	-0.0440***	-0.0242	-0.0353**	-0.00153
Continued on next page					

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
	(0.0113)	(0.0113)	(0.0126)	(0.0115)	(0.00786)
Craftsman	-0.00119	-0.00135	0.000400	0.000485	-0.0141
	(0.0102)	(0.0102)	(0.0107)	(0.0103)	(0.00729)
Blue collar	0.0137	0.0139	0.0204	0.0199	-0.0102
	(0.0104)	(0.0104)	(0.0109)	(0.0105)	(0.00768)
Age					
26-35	-0.0428***	-0.0433***	-0.0395***	-0.0419***	0.00756
	(0.00700)	(0.00699)	(0.00739)	(0.00712)	(0.00521)
36-45	-0.0991***	-0.0996***	-0.101***	-0.102***	-0.0119
	(0.00884)	(0.00884)	(0.00928)	(0.00895)	(0.00612)
46-55	-0.229***	-0.229***	-0.222***	-0.228***	-0.0262**
	(0.0136)	(0.0136)	(0.0144)	(0.0137)	(0.00872)
56-65	-0.334***	-0.336***	-0.324***	-0.326***	-0.0477***
	(0.0218)	(0.0219)	(0.0233)	(0.0218)	(0.0130)
65+	-0.408***	-0.408***	-0.416***	-0.415***	-0.0315
	(0.0398)	(0.0399)	(0.0422)	(0.0368)	(0.0243)
Education					
Primary or less	-0.0327	-0.0319	-0.0233	-0.0283	-0.00172
	(0.0201)	(0.0201)	(0.0211)	(0.0198)	(0.0113)
Middle school	0.00423	0.00487	0.0158	0.00830	0.00867
	(0.0208)	(0.0209)	(0.0218)	(0.0206)	(0.0120)
High school	0.0491*	0.0496*	0.0509*	0.0469*	0.0253*
Continued on next page					

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
	(0.0217)	(0.0217)	(0.0227)	(0.0215)	(0.0129)
College or graduate	0.0533*	0.0539*	0.0580*	0.0584*	-0.0264
	(0.0251)	(0.0251)	(0.0264)	(0.0251)	(0.0156)
Wealth					
25%-50%	-0.0178*	-0.0178*	-0.0174	-0.0160	0.00367
	(0.00852)	(0.00851)	(0.00892)	(0.00863)	(0.00620)
50% - 75%	-0.0146	-0.0145	-0.0107	-0.0105	0.00339
	(0.00864)	(0.00863)	(0.00909)	(0.00877)	(0.00627)
75%-100%	0.0126	0.0129	0.0135	0.0158	0.00959
	(0.00888)	(0.00887)	(0.00940)	(0.00904)	(0.00642)
Constant	0.819**	0.764**	0.726***	0.758***	0.0228
	(0.258)	(0.266)	(0.0524)	(0.0484)	(0.0324)
<i>N</i>	24395	24395	23711	23711	24608
State FE	Y	Y		Y	Y
Mun. FE			Y		
Year FE	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A22: **Logits: Drug War**

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
Turf War			0.175**	0.180***	0.166**
			(0.0558)	(0.0502)	(0.0567)
Joint Operation				0.371***	0.719***
				(0.0629)	(0.0909)
Party in power					
PAN	-0.338***	-0.299***			
	(0.0608)	(0.0884)			
PRD	0.0789	0.415***			
	(0.0926)	(0.109)			
Independent	-0.0959				
	(0.183)				
Drug war x Party in power					
PRI		0.147			
		(0.183)			
PAN		-0.222			
		(0.192)			
PRD		0.0714			
		(0.206)			
Male	0.660***	0.661***	0.661***	0.638***	0.219*
	(0.0667)	(0.0667)	(0.0715)	(0.0677)	(0.100)
Continued on next page					

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
Illiterate	-0.250** (0.0788)	-0.244** (0.0789)	-0.248** (0.0857)	-0.210** (0.0773)	-0.385** (0.120)
Indigenous	-0.213*** (0.0543)	-0.211*** (0.0544)	-0.0875 (0.0659)	-0.266*** (0.0550)	-0.456*** (0.0887)
Federal prison	0.853*** (0.0437)	0.845*** (0.0437)	0.979*** (0.0519)	0.943*** (0.0498)	2.237*** (0.0481)
Occupation					
Merchant	0.137** (0.0446)	0.138** (0.0446)	0.175*** (0.0469)	0.168*** (0.0450)	0.00704 (0.0607)
Public Security	-0.237* (0.0972)	-0.232* (0.0972)	-0.263* (0.107)	-0.242* (0.101)	-0.620*** (0.154)
Private Security	-0.0683 (0.0568)	-0.0668 (0.0569)	-0.0753 (0.0598)	-0.0717 (0.0572)	-0.124 (0.0771)
Rural worker	-0.197*** (0.0507)	-0.193*** (0.0508)	-0.115* (0.0584)	-0.152** (0.0514)	-0.0132 (0.0694)
Craftsman	-0.00445 (0.0464)	-0.00541 (0.0464)	0.00439 (0.0491)	0.00446 (0.0466)	-0.125* (0.0630)
Blue collar	0.0626 (0.0478)	0.0636 (0.0478)	0.0982 (0.0505)	0.0902 (0.0483)	-0.0709 (0.0652)
Age					
26-35	-0.199*** (0.0323)	-0.201*** (0.0323)	-0.192*** (0.0346)	-0.194*** (0.0327)	0.0762 (0.0438)
Continued on next page					

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
36-45	-0.445*** (0.0393)	-0.448*** (0.0394)	-0.478*** (0.0421)	-0.455*** (0.0397)	-0.104 (0.0553)
46-55	-1.004*** (0.0603)	-1.005*** (0.0603)	-1.026*** (0.0657)	-1.001*** (0.0608)	-0.222** (0.0855)
56-65	-1.500*** (0.112)	-1.510*** (0.112)	-1.535*** (0.122)	-1.458*** (0.112)	-0.565** (0.184)
65+	-1.941*** (0.267)	-1.941*** (0.267)	-2.092*** (0.297)	-2.001*** (0.260)	-0.445 (0.363)
Education					
Primary or less	-0.145 (0.0895)	-0.142 (0.0897)	-0.105 (0.0972)	-0.127 (0.0884)	-0.00402 (0.132)
Middle school	0.0188 (0.0930)	0.0214 (0.0931)	0.0785 (0.101)	0.0362 (0.0922)	0.0832 (0.136)
High school	0.228* (0.0973)	0.230* (0.0975)	0.248* (0.105)	0.214* (0.0966)	0.211 (0.140)
College or graduate	0.243* (0.114)	0.245* (0.114)	0.281* (0.123)	0.264* (0.114)	-0.227 (0.168)
Wealth					
25%-50%	-0.0798* (0.0390)	-0.0803* (0.0391)	-0.0799 (0.0413)	-0.0724 (0.0395)	0.0323 (0.0532)
50% - 75%	-0.0665 (0.0395)	-0.0655 (0.0395)	-0.0499 (0.0421)	-0.0468 (0.0400)	0.0224 (0.0543)
Continued on next page					

	(1)	(2)	(3)	(4)	(5)
	Inst. Torture	Inst. Torture	Inst. Torture	Inst. Torture	Org. Crime
75%-100%	0.0566 (0.0407)	0.0585 (0.0408)	0.0655 (0.0438)	0.0723 (0.0414)	0.0842 (0.0557)
Constant	1.432 (1.209)	1.175 (1.216)	1.073*** (0.265)	1.204*** (0.240)	-3.093*** (0.379)
<i>N</i>	24393	24393	22785	23711	24608
State FE	Y	Y		Y	Y
Mun. FE			Y		
Year FE	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

State FE	Y	Y		Y	Y
Mun. FE			Y		
Year FE	Y	Y	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A23: OLS: Drug War Placebo

	(1)	(2)	(3)
	Inst. Torture	Inst. Torture	Inst. Torture
Placebos			
One year	-0.0201 (0.0357)		
Two years		-0.00551 (0.0295)	
Three years			0.00306 (0.0271)
Male	0.165*** (0.0326)	0.164*** (0.0325)	0.164*** (0.0325)
Illiterate	-0.0380 (0.0301)	-0.0382 (0.0301)	-0.0380 (0.0301)
Indigenous	-0.0251 (0.0212)	-0.0252 (0.0212)	-0.0253 (0.0212)
Occupation			
Merchant	0.0615** (0.0194)	0.0615** (0.0194)	0.0615** (0.0194)
Public security	0.00199 (0.0385)	0.00209 (0.0385)	0.00223 (0.0385)
Private security	0.00972 (0.0256)	0.00965 (0.0256)	0.00959 (0.0256)

Continued on next page

	(1)	(2)	(3)
	Inst. Torture	Inst. Torture	Inst. Torture
Rural worker	0.00185 (0.0209)	0.00196 (0.0209)	0.00203 (0.0209)
Craftsman	0.00431 (0.0203)	0.00449 (0.0203)	0.00444 (0.0203)
Blue collar worker	0.0235 (0.0207)	0.0237 (0.0207)	0.0237 (0.0207)

Age

26-35	-0.0503*** (0.0133)	-0.0503*** (0.0133)	-0.0504*** (0.0133)
36-45	-0.149*** (0.0173)	-0.149*** (0.0173)	-0.149*** (0.0173)
46-55	-0.271*** (0.0267)	-0.271*** (0.0267)	-0.271*** (0.0267)
56-65	-0.307*** (0.0458)	-0.307*** (0.0458)	-0.307*** (0.0458)
65+	-0.373*** (0.0852)	-0.373*** (0.0854)	-0.372*** (0.0854)

Education

Primary or less	0.00691 (0.0340)	0.00705 (0.0340)	0.00700 (0.0340)
Secondary	0.0321	0.0321	0.0321

Continued on next page

	(1)	(2)	(3)
	Inst. Torture	Inst. Torture	Inst. Torture
	(0.0358)	(0.0358)	(0.0358)
High school	0.0791*	0.0793*	0.0793*
	(0.0378)	(0.0378)	(0.0378)
College or graduate	0.121**	0.121**	0.121**
	(0.0450)	(0.0450)	(0.0450)
Wealth index, quantiles			
25-50%	-0.0189	-0.0190	-0.0189
	(0.0168)	(0.0168)	(0.0168)
50-75%	-0.0286	-0.0285	-0.0285
	(0.0169)	(0.0169)	(0.0169)
75-100%	0.00129	0.00129	0.00128
	(0.0171)	(0.0171)	(0.0171)
Constant	0.374	0.374	0.374
	(0.284)	(0.284)	(0.284)
<i>N</i>	6942	6942	6942
State FE	Y	Y	Y
Year FE	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A24: OLS: Drug War Placebo, organized crime arrests

	(1)	(2)	(3)
	Organized Crime	Organized Crime	Organized Crime
Placebos			
One year	0.000954 (0.0248)		
Two years		0.00192 (0.0190)	
Three years			0.0117 (0.0169)
Male	0.0446** (0.0173)	0.0446** (0.0173)	0.0445* (0.0173)
Illiterate	-0.0241 (0.0162)	-0.0241 (0.0162)	-0.0239 (0.0162)
Indigenous	-0.0494*** (0.00952)	-0.0494*** (0.00952)	-0.0494*** (0.00952)
Occupation			
Merchant	0.0178 (0.0131)	0.0178 (0.0131)	0.0178 (0.0131)
Public security	0.0120 (0.0261)	0.0120 (0.0261)	0.0122 (0.0261)
Private security	0.0000285 (0.0163)	0.0000216 (0.0163)	-0.00000403 (0.0163)
Rural worker	-0.00360	-0.00359	-0.00348

Table A24, continued

	(1)	(2)	(3)
	Organized Crime	Organized Crime	Organized Crime
	(0.0131)	(0.0131)	(0.0131)
Craftsman	-0.0194	-0.0194	-0.0194
	(0.0127)	(0.0127)	(0.0127)
Blue collar worker	-0.0135	-0.0135	-0.0135
	(0.0132)	(0.0132)	(0.0132)
Age			
26-35	0.00538	0.00537	0.00524
	(0.00861)	(0.00861)	(0.00861)
36-45	-0.0188	-0.0188	-0.0189
	(0.01000)	(0.0100)	(0.0100)
46-55	-0.0222	-0.0222	-0.0223
	(0.0152)	(0.0152)	(0.0152)
56-65	-0.0464*	-0.0464*	-0.0467*
	(0.0216)	(0.0215)	(0.0216)
65+	-0.0292	-0.0291	-0.0288
	(0.0393)	(0.0393)	(0.0393)
Education			
Primary or less	-0.00382	-0.00383	-0.00382
	(0.0188)	(0.0188)	(0.0188)
Secondary	-0.00246	-0.00247	-0.00247
	(0.0204)	(0.0204)	(0.0204)

Table A24, continued

	(1)	(2)	(3)
	Organized Crime	Organized Crime	Organized Crime
High school	0.00168 (0.0222)	0.00168 (0.0221)	0.00177 (0.0221)
College or graduate	-0.00746 (0.0271)	-0.00745 (0.0271)	-0.00726 (0.0271)
Wealth index, quantiles			
25-50%	-0.0291** (0.0109)	-0.0291** (0.0109)	-0.0290** (0.0109)
50-75%	-0.0279* (0.0110)	-0.0279* (0.0110)	-0.0279* (0.0109)
75-100%	-0.0188 (0.0112)	-0.0188 (0.0112)	-0.0188 (0.0112)
Constant	0.223 (0.283)	0.224 (0.283)	0.224 (0.283)
<i>N</i>	7253	7253	7253

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

State FE	Y	Y	Y
Year FE	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A25: **Logit: Drug War Placebo, institutional torture**

	(1)	(2)	(3)
	Inst. Torture	Inst. Torture	Inst. Torture
Placebos			
One year	-0.0858 (0.153)		
Two years		-0.0220 (0.128)	
Three years			0.0169 (0.118)
Male	0.724*** (0.142)	0.722*** (0.142)	0.721*** (0.142)
Illiterate	-0.164 (0.130)	-0.164 (0.130)	-0.164 (0.130)
Indigenous	-0.107 (0.0934)	-0.108 (0.0934)	-0.108 (0.0934)
Occupation			
Merchant	0.278** (0.0868)	0.279** (0.0868)	0.279** (0.0868)
Public security	0.0124 (0.172)	0.0129 (0.172)	0.0137 (0.172)
Private security	0.0422 (0.112)	0.0419 (0.112)	0.0417 (0.112)

Continued on next page

	(1)	(2)	(3)
	Inst. Torture	Inst. Torture	Inst. Torture
Rural worker	0.00801 (0.0921)	0.00857 (0.0920)	0.00896 (0.0920)
Craftsman	0.0190 (0.0888)	0.0198 (0.0888)	0.0196 (0.0888)
Blue collar worker	0.106 (0.0908)	0.107 (0.0908)	0.107 (0.0908)

Age

26-35	-0.226*** (0.0591)	-0.226*** (0.0591)	-0.226*** (0.0592)
36-45	-0.644*** (0.0741)	-0.643*** (0.0741)	-0.643*** (0.0741)
46-55	-1.174*** (0.120)	-1.174*** (0.120)	-1.174*** (0.120)
56-65	-1.344*** (0.220)	-1.344*** (0.220)	-1.344*** (0.220)
65+	-1.714*** (0.515)	-1.715*** (0.516)	-1.713*** (0.516)

Education

Primary or less	0.0373 (0.149)	0.0378 (0.148)	0.0376 (0.148)
Secondary	0.148	0.148	0.148

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	(1)	(2)	(3)
	Inst. Torture	Inst. Torture	Inst. Torture
	(0.156)	(0.156)	(0.156)
High school	0.361*	0.362*	0.362*
	(0.166)	(0.166)	(0.166)
College or graduate	0.555**	0.555**	0.555**
	(0.202)	(0.202)	(0.202)
Wealth index, quantiles			
25-50%	-0.0833	-0.0834	-0.0833
	(0.0746)	(0.0746)	(0.0746)
50-75%	-0.127	-0.127	-0.127
	(0.0744)	(0.0744)	(0.0744)
75-100%	0.00620	0.00619	0.00614
	(0.0760)	(0.0760)	(0.0760)
Constant	-0.607	-0.607	-0.604
	(1.263)	(1.263)	(1.263)
<i>N</i>	6939	6939	6939
State FE	Y	Y	Y
Year FE	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A26: **Logit: Drug War Placebo, organized crime arrests**

	(1)	(2)	(3)
	Organized Crime	Organized Crime	Organized Crime
Placebos			
One year	0.0185 (0.218)		
Two years		0.0248 (0.190)	
Three years			0.131 (0.173)
Male	0.543* (0.247)	0.543* (0.247)	0.542* (0.248)
Illiterate	-0.368 (0.249)	-0.368 (0.249)	-0.363 (0.249)
Indigenous	-0.861*** (0.204)	-0.861*** (0.204)	-0.862*** (0.204)
Occupation			
Merchant	0.165 (0.126)	0.165 (0.126)	0.165 (0.126)
Public security	0.124 (0.239)	0.125 (0.239)	0.126 (0.239)
Private security	0.00695 (0.166)	0.00693 (0.166)	0.00687 (0.166)

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	(1)	(2)	(3)
	Organized Crime	Organized Crime	Organized Crime
Rural worker	-0.0409 (0.142)	-0.0408 (0.142)	-0.0402 (0.142)
Craftsman	-0.226 (0.139)	-0.226 (0.139)	-0.227 (0.139)
Blue collar worker	-0.142 (0.141)	-0.143 (0.141)	-0.143 (0.141)
Age			
26-35	0.0530 (0.0870)	0.0529 (0.0871)	0.0508 (0.0871)
36-45	-0.214 (0.119)	-0.214 (0.119)	-0.216 (0.119)
46-55	-0.278 (0.203)	-0.278 (0.203)	-0.278 (0.203)
56-65	-0.714 (0.441)	-0.713 (0.440)	-0.721 (0.442)
65+	-0.545 (1.043)	-0.543 (1.043)	-0.544 (1.042)
Education			
Primary or less	-0.0379 (0.270)	-0.0379 (0.270)	-0.0358 (0.269)
Secondary	-0.0327	-0.0326	-0.0308

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	(1)	(2)	(3)
	Organized Crime	Organized Crime	Organized Crime
	(0.279)	(0.279)	(0.278)
High school	0.00552	0.00560	0.00874
	(0.290)	(0.290)	(0.289)
College or graduate	-0.0624	-0.0621	-0.0577
	(0.332)	(0.332)	(0.331)
Wealth index, quantiles			
25-50%	-0.290**	-0.290**	-0.290**
	(0.110)	(0.110)	(0.110)
50-75%	-0.281*	-0.281*	-0.281*
	(0.113)	(0.113)	(0.113)
75-100%	-0.170	-0.170	-0.171
	(0.113)	(0.113)	(0.113)
Cosntant	-2.158	-2.157	-2.147
	(1.550)	(1.549)	(1.544)
<i>N</i>	7246	7246	7246
State FE	Y	Y	Y
Year FE	Y	Y	Y

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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