Institutionalized Police Brutality: Torture, the Militarization of Security and the Reform of Inquisitorial Criminal Justice in Mexico Online Appendix

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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. We further provide a discussion of the effect of state capacity on torture which, for reasons of space, were excluded from the main body of the text. 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 include a range of full regression tables, alternate specifications, and tests of key assumptions that were referenced in the main text.

1 ENPOL

The ENPOL is a survey that was conducted by the Mexican National Statistics Agency (INEGI) to generate information about the adult incarcerated population of Mexico. The survey consisted of ten sections which covered social and demographic information, family and work history, criminal records, arrests, the judicial process, living conditions in prison, corruption, and expectations about the future. Because of the desire to generate a sample that would be representative of penitentiaries, states, and the country as a whole, the survey included approximately 25% of the country's prison population.

We note that the prisoner survey was collected using advice from Mexican lawyers, academics, and activists. Roberto Hernández of Presunto Culpable and Alejandro Ponce of the World Justice Project played a critical role in developing the questionnaire of the ENPOL related to torture. The questions draw from prior work by academics at CIDE who had collected a similar battery of questions on torture and due process in six federal prisons. Interviews with prisoners were collected one-on-one after clarifying that their responses would be kept confidential and that they would not be read by judges or prison guards or anyone else. INEGI was in charge of hiring and training enumerators, obtaining permission to access the prisons, and other logistics of collecting the survey.

The survey was carried out from October 31 to December 9, 2016, *after* the criminal justice reform had been fully implemented in every jurisdiction in the country. The survey was accompanied by a disclaimer that all information provided would remain confidential and would have no use beyond the generation of statistical information.

1.1 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 submerging your head in water
3. Threats
   - Threats of false charges
   - Threats of harm against family members

We constructed our dependent variables as follows:

$Abuse_{i,j} = \max_j report_{i,j}$ 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.

1.2 Wealth

To construct our measure of wealth, we use a battery 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. We compute the interitem correlation using the alpha command in stata and, for the analysis, we subdivide the index into four quartiles.

The questions we use are a series of true/false statements which ask about the respondent's condition in the year prior to his or her imprisonment and are as follows.

In your home:

1. You had enough food for everyone every day.
2. Had a debt (with a bank, family, friends, or neighbors).
3. You had enough money to buy clothes and shoes.
4. To cover basic necessities, you had to work seven days a week.

5. You had enough money to spend on leisure.
6. You could pay for medicine and medical treatment when necessary.
7. You had enough money to spend indulging yourself.

2 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.[1] 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 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 responsiblity 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

[1] The 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.

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 Minsitry

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.

3 Characteristics of the sample

This section provides tables that describes the sample.

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

Characteristic	Number	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

4 Criminal Justice Reform

4.1 Reform models

In order to define treatment status, cluster standard errors, and construct new variables, we needed to build a dataset relating municipalities to judicial districts. The municipal composition of judicial districts is found in states' organic laws of judicial power. We found these laws for the states in which we run our study and recorded unique identifiers for the judicial districts as well as the municipalities they contain. A very small number of less populous municipalities are divided into multiple districts – in these cases, the assignment of the municipal seat was used. As the federal reform was implemented separately, we also code states as the corresponding "judicial district" for federal prisoners – this reform took effect in federal criminal procedure across the territory of entire states.

In the paper, we present results from a difference-in-differences in which we use OLS to evaluate the effectiveness of the reform, though we truncate the tables and exclude the covariates. Here we present the results of the regression in Table A2. We use OLS for the simplicity of interpretation and to avoid inducing the incidental parameters problem. However, as our dependent variable is binary, we also provide logistic regressions mirroring those specifications in Table A3. The main results are unchanged irrespective of the specification used.

Table A2: **OLS: Effects of the Reform**

	(1) Torture	(2) Brute	(3) Threat	(4) Torture	(5) Brute	(6) Threat	(7) Torture	(8) Brute	(9) Threat
Reform	-0.0591**	-0.0796***	-0.0712***	-0.0392*	-0.0559***	-0.0547***	-0.0502*	-0.0674***	-0.0659***
	(0.0193)	(0.0177)	(0.0157)	(0.0177)	(0.0162)	(0.0135)	(0.0195)	(0.0179)	(0.0159)
Male	0.160***	0.178***	-0.0465***	0.164***	0.181***	-0.0408***	0.156***	0.171***	-0.0475***
	(0.0132)	(0.0131)	(0.0112)	(0.0134)	(0.0131)	(0.0112)	(0.0128)	(0.0129)	(0.0112)
Cannot read or write	-0.0427*	-0.0122	-0.0238	-0.0318	-0.00426	-0.0126	-0.0438*	-0.0114	-0.0317*
	(0.0165)	(0.0143)	(0.0141)	(0.0163)	(0.0140)	(0.0138)	(0.0175)	(0.0151)	(0.0144)
Indigenous	-0.0540***	-0.0564***	-0.0257*	-0.0192	-0.0256*	0.00136	-0.0209	-0.0255*	0.000455
	(0.0122)	(0.0119)	(0.0126)	(0.0111)	(0.0111)	(0.0113)	(0.0125)	(0.0118)	(0.0121)
Occupation									
Merchant	0.0222**	0.0259***	0.0333***	0.0183*	0.0222**	0.0297***	0.0211*	0.0244**	0.0332***
	(0.00798)	(0.00753)	(0.00765)	(0.00816)	(0.00769)	(0.00789)	(0.00843)	(0.00777)	(0.00794)
Public security	-0.0693***	-0.0999***	0.00531	-0.0752***	-0.106***	0.00115	-0.0723***	-0.106***	0.00653
	(0.0189)	(0.0186)	(0.0161)	(0.0191)	(0.0187)	(0.0165)	(0.0196)	(0.0192)	(0.0170)
Private security	-0.00456	-0.00126	-0.0107	-0.00723	-0.00259	-0.0110	-0.0108	-0.00524	-0.0126
	(0.0142)	(0.0128)	(0.0137)	(0.0133)	(0.0121)	(0.0129)	(0.0137)	(0.0124)	(0.0134)
Rural worker	-0.0395***	-0.0360**	-0.0286**	-0.0311**	-0.0258*	-0.0236*	-0.0241*	-0.0187	-0.0188
	(0.0112)	(0.0113)	(0.0103)	(0.0114)	(0.0115)	(0.0103)	(0.0117)	(0.0121)	(0.0101)
Craftsman	-0.00420	0.00146	-0.0119	-0.00302	0.00339	-0.0103	-0.00361	0.00165	-0.0118
	(0.00839)	(0.00882)	(0.00895)	(0.00838)	(0.00875)	(0.00887)	(0.00865)	(0.00904)	(0.00916)
Blue collar	0.00974	0.0109	0.00422	0.00473	0.00778	0.000619	0.00516	0.00832	0.00153
	(0.00902)	(0.00927)	(0.00944)	(0.00888)	(0.00922)	(0.00938)	(0.00908)	(0.00951)	(0.00971)
Age									
26-35	-0.0414***	-0.0431***	-0.00899	-0.0422***	-0.0432***	-0.0100	-0.0391***	-0.0406***	-0.00688
	(0.00607)	(0.00608)	(0.00531)	(0.00611)	(0.00610)	(0.00530)	(0.00633)	(0.00643)	(0.00552)
36-45	-0.102***	-0.111***	-0.0575***	-0.104***	-0.112***	-0.0600***	-0.0977***	-0.106***	-0.0554***
	(0.00824)	(0.00737)	(0.00796)	(0.00802)	(0.00728)	(0.00800)	(0.00849)	(0.00770)	(0.00821)
46-55	-0.228***	-0.243***	-0.165***	-0.226***	-0.241***	-0.165***	-0.219***	-0.233***	-0.163***
	(0.0125)	(0.0114)	(0.0105)	(0.0121)	(0.0113)	(0.0105)	(0.0129)	(0.0118)	(0.0112)
56-65	-0.309***	-0.337***	-0.247***	-0.302***	-0.328***	-0.237***	-0.300***	-0.328***	-0.238***
	(0.0173)	(0.0189)	(0.0189)	(0.0174)	(0.0190)	(0.0192)	(0.0180)	(0.0203)	(0.0193)
65+	-0.383***	-0.442***	-0.327***	-0.368***	-0.427***	-0.322***	-0.372***	-0.437***	-0.331***
	(0.0304)	(0.0355)	(0.0378)	(0.0313)	(0.0371)	(0.0383)	(0.0341)	(0.0379)	(0.0391)
Education									
Primary or less	-0.00829	-0.0105	0.00210	0.00147	-0.00173	0.0105	-0.000135	-0.00458	0.00103
	(0.0175)	(0.0169)	(0.0157)	(0.0173)	(0.0170)	(0.0154)	(0.0191)	(0.0182)	(0.0171)
Middle School	0.0294	0.0223	0.0403**	0.0399*	0.0312	0.0478**	0.0385	0.0284	0.0377*
	(0.0187)	(0.0183)	(0.0153)	(0.0187)	(0.0184)	(0.0151)	(0.0202)	(0.0195)	(0.0164)
High School	0.0711***	0.0537**	0.0856***	0.0784***	0.0596**	0.0901***	0.0769***	0.0587**	0.0819***
	(0.0188)	(0.0184)	(0.0161)	(0.0186)	(0.0184)	(0.0159)	(0.0202)	(0.0195)	(0.0173)
College or graduate	0.0538*	0.0110	0.0827***	0.0542*	0.0119	0.0833***	0.0554*	0.0105	0.0767***
	(0.0212)	(0.0211)	(0.0167)	(0.0212)	(0.0211)	(0.0164)	(0.0230)	(0.0223)	(0.0174)
Wealth index, quantiles									
25%-50%	-0.0171*	-0.0133*	-0.0204**	-0.0179*	-0.0137*	-0.0205**	-0.0195*	-0.0153*	-0.0198**
	(0.00798)	(0.00660)	(0.00730)	(0.00786)	(0.00651)	(0.00717)	(0.00835)	(0.00691)	(0.00755)
50%-75%	-0.00658	0.000664	0.00536	-0.00957	-0.000814	0.00336	-0.00752	0.00296	0.00727
	(0.00760)	(0.00737)	(0.00678)	(0.00738)	(0.00736)	(0.00673)	(0.00759)	(0.00750)	(0.00715)
75%-100%	0.00488	0.00456	0.00304	0.00366	0.00475	0.00262	0.00494	0.00571	0.00563
	(0.00827)	(0.00747)	(0.00714)	(0.00801)	(0.00740)	(0.00688)	(0.00825)	(0.00779)	(0.00739)
Constant	0.724**	0.640*	1.200***	0.355	0.242	0.777***	0.511	0.456	1.116***
	(0.276)	(0.259)	(0.0645)	(0.304)	(0.297)	(0.0497)	(0.332)	(0.315)	(0.0270)
N	37632	37669	37625	37632	37669	37625	37632	37669	37625
State FE	Y	Y	Y						
Judicial District FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: This table reports the full specification for our main results in Table 6 and includes coefficients excluded from the main text. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A3: **Logits: effects of the reform**

	(1) Torture	(2) Brute	(3) Threat	(4) Torture	(5) Brute	(6) Threat	(7) Torture	(8) Brute	(9) Threat
Reform	-0.262**	-0.325***	-0.286***	-0.186*	-0.227**	-0.222***	-0.237**	-0.281***	-0.272***
	(0.0849)	(0.0767)	(0.0668)	(0.0809)	(0.0726)	(0.0587)	(0.0876)	(0.0790)	(0.0684)
Male	0.704***	0.842***	-0.241***	0.743***	0.881***	-0.219***	0.720***	0.844***	-0.256***
	(0.0595)	(0.0614)	(0.0594)	(0.0618)	(0.0639)	(0.0605)	(0.0589)	(0.0623)	(0.0599)
Illiterate	-0.187*	-0.0526	-0.100	-0.140	-0.0119	-0.0473	-0.201*	-0.0499	-0.138*
	(0.0732)	(0.0670)	(0.0624)	(0.0737)	(0.0676)	(0.0626)	(0.0802)	(0.0741)	(0.0661)
Indigenous	-0.233***	-0.264***	-0.114*	-0.0820	-0.123*	0.0111	-0.0968	-0.128*	0.000510
	(0.0533)	(0.0545)	(0.0566)	(0.0504)	(0.0532)	(0.0529)	(0.0572)	(0.0583)	(0.0575)
Occupation									
Merchant	0.0987**	0.134***	0.168***	0.0834*	0.118**	0.154***	0.0968*	0.131**	0.173***
	(0.0361)	(0.0395)	(0.0395)	(0.0377)	(0.0412)	(0.0412)	(0.0387)	(0.0416)	(0.0414)
Public security	-0.313***	-0.499***	0.0300	-0.353***	-0.550***	0.00536	-0.335***	-0.549***	0.0367
	(0.0827)	(0.0875)	(0.0830)	(0.0866)	(0.0920)	(0.0878)	(0.0881)	(0.0921)	(0.0884)
Private security	-0.0227	-0.00893	-0.0506	-0.0337	-0.0142	-0.0516	-0.0512	-0.0302	-0.0598
	(0.0634)	(0.0646)	(0.0648)	(0.0604)	(0.0625)	(0.0623)	(0.0619)	(0.0631)	(0.0639)
Rural worker	-0.176***	-0.174**	-0.126**	-0.145**	-0.130*	-0.110*	-0.117*	-0.100	-0.0885
	(0.0498)	(0.0549)	(0.0481)	(0.0520)	(0.0575)	(0.0497)	(0.0539)	(0.0613)	(0.0483)
Craftsman	-0.0197	0.00711	-0.0537	-0.0144	0.0164	-0.0466	-0.0171	0.00634	-0.0532
	(0.0376)	(0.0449)	(0.0424)	(0.0384)	(0.0456)	(0.0429)	(0.0393)	(0.0469)	(0.0438)
Blue collar	0.0414	0.0553	0.0199	0.0201	0.0404	0.00263	0.0221	0.0434	0.00615
	(0.0406)	(0.0485)	(0.0463)	(0.0409)	(0.0493)	(0.0469)	(0.0416)	(0.0506)	(0.0481)
Age									
26-35	-0.186***	-0.232***	-0.0439	-0.194***	-0.238***	-0.0497	-0.180***	-0.227***	-0.0343
	(0.0273)	(0.0323)	(0.0264)	(0.0283)	(0.0330)	(0.0267)	(0.0291)	(0.0345)	(0.0278)
36-45	-0.447***	-0.555***	-0.273***	-0.467***	-0.572***	-0.292***	-0.445***	-0.552***	-0.273***
	(0.0358)	(0.0354)	(0.0368)	(0.0358)	(0.0357)	(0.0380)	(0.0378)	(0.0375)	(0.0386)
46-55	-0.990***	-1.125***	-0.739***	-1.007***	-1.142***	-0.759***	-0.995***	-1.133***	-0.762***
	(0.0559)	(0.0509)	(0.0454)	(0.0555)	(0.0515)	(0.0469)	(0.0590)	(0.0551)	(0.0501)
56-65	-1.386***	-1.524***	-1.074***	-1.391***	-1.523***	-1.059***	-1.410***	-1.561***	-1.089***
	(0.0859)	(0.0866)	(0.0817)	(0.0885)	(0.0898)	(0.0854)	(0.0910)	(0.0964)	(0.0877)
65+	-1.874***	-2.049***	-1.441***	-1.837***	-2.017***	-1.457***	-1.926***	-2.165***	-1.612***
	(0.208)	(0.200)	(0.186)	(0.213)	(0.207)	(0.192)	(0.235)	(0.224)	(0.211)
Education									
Primary or less	-0.0343	-0.0570	0.00598	0.0142	-0.00913	0.0508	0.00868	-0.0223	0.0112
	(0.0789)	(0.0796)	(0.0695)	(0.0796)	(0.0819)	(0.0700)	(0.0898)	(0.0903)	(0.0785)
Middle school	0.132	0.106	0.184**	0.187*	0.156	0.229***	0.186*	0.146	0.189*
	(0.0841)	(0.0870)	(0.0677)	(0.0857)	(0.0900)	(0.0683)	(0.0944)	(0.0975)	(0.0752)
High school	0.320***	0.269**	0.413***	0.365***	0.309***	0.448***	0.365***	0.307**	0.418***
	(0.0841)	(0.0886)	(0.0725)	(0.0851)	(0.0911)	(0.0733)	(0.0941)	(0.0988)	(0.0801)
College or graduate	0.240*	0.0458	0.389***	0.253**	0.0533	0.405***	0.265*	0.0524	0.386***
	(0.0950)	(0.100)	(0.0777)	(0.0970)	(0.103)	(0.0779)	(0.106)	(0.111)	(0.0816)
Wealth index, quantiles									
25%-50%	-0.0774*	-0.0698*	-0.100**	-0.0826*	-0.0729*	-0.102**	-0.0896*	-0.0819*	-0.0987**
	(0.0361)	(0.0341)	(0.0358)	(0.0363)	(0.0343)	(0.0359)	(0.0383)	(0.0360)	(0.0374)
50% - 75%	-0.0295	0.00345	0.0253	-0.0436	-0.00350	0.0167	-0.0337	0.0176	0.0378
	(0.0342)	(0.0379)	(0.0335)	(0.0338)	(0.0386)	(0.0339)	(0.0346)	(0.0391)	(0.0358)
75%-100%	0.0207	0.0228	0.0126	0.0156	0.0242	0.0110	0.0227	0.0325	0.0279
	(0.0372)	(0.0383)	(0.0346)	(0.0369)	(0.0388)	(0.0340)	(0.0378)	(0.0408)	(0.0364)
Constant	1.102	1.003	1.780***	-0.766	-1.525	0.268*	-0.0163	-0.402	1.962***
	(1.278)	(1.192)	(0.101)	(1.371)	(1.319)	(0.136)	(1.548)	(1.486)	(0.110)
N	37632	37659	37622	37616	37650	37608	36596	36376	36340
State FE	Y	Y	Y						
Judicial District FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: This table reports the full specification for our main results as reported in Table 6 using logistic regressions instead of OLS. Standard errors clustered by judicial district in parentheses. *** : p < 0.001, ** : p < 0.01, * : p < 0.05.

4.2 Robustness to alternate units of time

We suppose that it is possible someone may object to our study on the grounds that our temporal fixed effects should take a more restrictive unit of time. In anticipation of such a critique, Table A4 replaces the year fixed effects in the models in Table A2 with fixed effects for each unique month. Our results hold.

We understand that another objection that may be raised is that we ought to be looking at changes in the municipality-level proportion of arrests reporting abuse. In order to answer that criticism, we reconstructed our dataset such that the dependent variable is the proportion of arrests in a municipality-month that report abuse and estimate:

$$Y_{st} = \alpha + \tau D_{st} + \lambda_t + \gamma_s + \epsilon_{st} \tag{1}$$

Where α is an intercept, τ is the effect of the reform, λ_t is a fixed effect for the month of arrest, and γ_s is a geographic fixed effect. The results of this specification are reported in Table A5, which still shows substantively and statistically significant declines across all dependent variables.

Table A4: **Effects of the reform with unique month fixed effects**

	(1) Torture	(2) Brute	(3) Threat	(4) Torture	(5) Brute	(6) Threat	(7) Torture	(8) Brute	(9) Threat
Reform	-0.0624**	-0.0787***	-0.0636***	-0.0416*	-0.0535**	-0.0456**	-0.0511*	-0.0642**	-0.0549**
	(0.0213)	(0.0199)	(0.0173)	(0.0195)	(0.0184)	(0.0148)	(0.0215)	(0.0202)	(0.0175)
Male	0.158***	0.178***	-0.0468***	0.163***	0.181***	-0.0409***	0.155***	0.171***	-0.0482***
	(0.0129)	(0.0129)	(0.0112)	(0.0130)	(0.0129)	(0.0113)	(0.0126)	(0.0128)	(0.0113)
Illiterate	-0.0448**	-0.0125	-0.0230	-0.0345*	-0.00490	-0.0122	-0.0462**	-0.0123	-0.0315*
	(0.0164)	(0.0142)	(0.0147)	(0.0162)	(0.0139)	(0.0144)	(0.0174)	(0.0150)	(0.0149)
Indigenous	-0.0544***	-0.0567***	-0.0246	-0.0199	-0.0261*	0.00238	-0.0223	-0.0267*	0.000634
	(0.0122)	(0.0120)	(0.0127)	(0.0112)	(0.0112)	(0.0114)	(0.0126)	(0.0120)	(0.0123)
Occupation									
Merchant	0.0222**	0.0255**	0.0336***	0.0182*	0.0216**	0.0299***	0.0212*	0.0239**	0.0334***
	(0.00803)	(0.00770)	(0.00784)	(0.00824)	(0.00788)	(0.00803)	(0.00850)	(0.00798)	(0.00808)
Public security	-0.0744***	-0.103***	-0.0000424	-0.0808***	-0.109***	-0.00424	-0.0775***	-0.109***	0.00193
	(0.0189)	(0.0188)	(0.0164)	(0.0192)	(0.0189)	(0.0168)	(0.0197)	(0.0193)	(0.0173)
Private security	-0.00618	-0.00260	-0.0107	-0.00885	-0.00409	-0.0109	-0.0125	-0.00675	-0.0127
	(0.0138)	(0.0128)	(0.0135)	(0.0129)	(0.0122)	(0.0127)	(0.0134)	(0.0124)	(0.0132)
Rural worker	-0.0398***	-0.0371**	-0.0302**	-0.0314**	-0.0270*	-0.0250*	-0.0244*	-0.0193	-0.0199*
	(0.0113)	(0.0115)	(0.0102)	(0.0115)	(0.0117)	(0.0102)	(0.0118)	(0.0123)	(0.00996)
Craftsman	-0.00381	0.00139	-0.0113	-0.00268	0.00327	-0.00957	-0.00312	0.00176	-0.0111
	(0.00827)	(0.00888)	(0.00880)	(0.00826)	(0.00880)	(0.00872)	(0.00848)	(0.00910)	(0.00895)
Blue collar	0.00975	0.0109	0.00437	0.00493	0.00784	0.000949	0.00529	0.00845	0.00170
	(0.00891)	(0.00954)	(0.00940)	(0.00882)	(0.00953)	(0.00936)	(0.00903)	(0.00981)	(0.00971)
Age									
26-35	-0.0413***	-0.0430***	-0.00860	-0.0422***	-0.0432***	-0.00958	-0.0389***	-0.0407***	-0.00658
	(0.00609)	(0.00613)	(0.00538)	(0.00613)	(0.00615)	(0.00538)	(0.00632)	(0.00649)	(0.00561)
36-45	-0.102***	-0.112***	-0.0576***	-0.104***	-0.112***	-0.0601***	-0.0984***	-0.107***	-0.0559***
	(0.00826)	(0.00745)	(0.00802)	(0.00803)	(0.00736)	(0.00808)	(0.00851)	(0.00780)	(0.00835)
46-55	-0.227***	-0.243***	-0.165***	-0.225***	-0.242***	-0.166***	-0.218***	-0.234***	-0.163***
	(0.0126)	(0.0116)	(0.0105)	(0.0123)	(0.0115)	(0.0105)	(0.0129)	(0.0120)	(0.0111)
56-65	-0.310***	-0.336***	-0.247***	-0.303***	-0.327***	-0.237***	-0.300***	-0.326***	-0.236***
	(0.0176)	(0.0190)	(0.0191)	(0.0176)	(0.0191)	(0.0193)	(0.0183)	(0.0204)	(0.0196)
65+	-0.384***	-0.446***	-0.326***	-0.369***	-0.432***	-0.322***	-0.373***	-0.441***	-0.331***
	(0.0303)	(0.0354)	(0.0370)	(0.0313)	(0.0371)	(0.0375)	(0.0341)	(0.0375)	(0.0383)
Education									
Primary or less	-0.0101	-0.0109	-0.000239	0.000326	-0.00137	0.00829	-0.00228	-0.00458	-0.00130
	(0.0174)	(0.0165)	(0.0157)	(0.0173)	(0.0166)	(0.0155)	(0.0191)	(0.0179)	(0.0171)
Middle school	0.0276	0.0223	0.0378*	0.0386*	0.0317	0.0455**	0.0363	0.0287	0.0353*
	(0.0185)	(0.0177)	(0.0153)	(0.0185)	(0.0179)	(0.0151)	(0.0201)	(0.0190)	(0.0164)
High school	0.0694***	0.0536**	0.0835***	0.0772***	0.0600***	0.0882***	0.0748***	0.0589**	0.0797***
	(0.0185)	(0.0179)	(0.0161)	(0.0184)	(0.0180)	(0.0160)	(0.0200)	(0.0191)	(0.0172)
College or graduate	0.0502*	0.00983	0.0788***	0.0508*	0.0112	0.0794***	0.0517*	0.0100	0.0729***
	(0.0209)	(0.0207)	(0.0170)	(0.0209)	(0.0208)	(0.0168)	(0.0227)	(0.0219)	(0.0177)
Wealth index, quantiles									
25%-50%	-0.0178*	-0.0136*	-0.0207**	-0.0185*	-0.0140*	-0.0209**	-0.0197*	-0.0153*	-0.0197*
	(0.00819)	(0.00669)	(0.00756)	(0.00805)	(0.00660)	(0.00744)	(0.00857)	(0.00700)	(0.00785)
50% - 75%	-0.00641	0.000917	0.00637	-0.00941	-0.000547	0.00433	-0.00746	0.00298	0.00821
	(0.00781)	(0.00734)	(0.00698)	(0.00755)	(0.00733)	(0.00689)	(0.00783)	(0.00752)	(0.00733)
75%-100%	0.00392	0.00349	0.00348	0.00277	0.00373	0.00307	0.00390	0.00453	0.00630
	(0.00845)	(0.00762)	(0.00722)	(0.00818)	(0.00752)	(0.00693)	(0.00842)	(0.00792)	(0.00743)
Constant	1.006***	0.894***	1.128***	0.616***	0.500***	0.668***	0.866***	0.764***	1.182***
	(0.0395)	(0.0294)	(0.0255)	(0.0458)	(0.0422)	(0.0390)	(0.0712)	(0.0467)	(0.0667)
N	37632	37669	37625	37632	37669	37625	37632	37669	37625
State FE	Y	Y	Y						
Judicial District FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: This table reports the full specification for our main results as reported in Table 6 of the main text using fixed effects for each unique month rather than year fixed effects. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A5: **Effects of the reform using municipality-month averages of reported abuse**

	(1) Torture	(2) Brute	(3) Threat	(4) Torture	(5) Brute	(6) Threat	(7) Torture	(8) Brute	(9) Threat
Reform	-0.077^{***}	-0.087^{***}	-0.079^{***}	-0.054^{*}	-0.054^{*}	-0.048^{*}	-0.074^{**}	-0.071^{**}	-0.067^{**}
	(0.023)	(0.022)	(0.021)	(0.022)	(0.021)	(0.020)	(0.024)	(0.023)	(0.022)
N	21436	21468	21419	21436	21468	21419	21436	21468	21419
State FE	Y	Y	Y						
Judicial District FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: This table reports the results using a reconfigured version of our dataset. Rather than using a binary dependent variable, we use the municipal-month average rate of reported abuse. Standard errors clustered by judicial district in parentheses. *** : p < 0.001, ** : p < 0.01, * : p < 0.05.

Table A6: **Balance table**

	Pre-reform	Post-reform	p-value
Male	0.944	0.939	0.319
Illiterate	0.053	0.052	0.755
Indigenous	0.072	0.050	0.000
Arresting Authority			
Municipal	0.253	0.274	0.015
State	0.130	0.170	0.000
Ministerial	0.370	0.402	0.001
Federal	0.098	0.065	0.000
Military	0.092	0.039	0.000
Crime			
Theft	0.308	0.328	0.027
Homicide	0.236	0.148	0.000
Kidnapping	0.107	0.079	0.000
Illegal Weapons	0.148	0.066	0.000
Rape	0.105	0.102	0.674
Drug possession	0.089	0.126	0.000
Drug commerce	0.048	0.044	0.312
Sentencing status			
Not Sentenced	0.303	0.628	0.000
Partly Sentenced	0.023	0.012	0.000
Sentenced	0.673	0.360	0.000

Note: This table reports the balance in our sample before and after the reform, showing the proportion of the arrests in the pre and post reform periods that belong to each category.

4.3 Matching

Table A6 shows the characteristics of the sample divided into pre- and post- reform periods. We match exactly on a range of crimes,[2] sentencing status, federal or state prison, and the arresting authority and run coarsened matching on the respondent age. We match in two samples: first, using the full reform sample and second, using the six months before and after implementation of the reform. Table A7 shows the results – across all matching routines we retain negative and significant coefficients on the reform.

[2]Homicide, kidnapping, possession of illicit weapons, rape, drug possession, drug commerce, and theft

Table A7: **Matching**

	(1) Brute	(2) Torture	(3) Threat
Reform	-0.170***	-0.165***	-0.168***
	(0.0221)	(0.0271)	(0.0188)
Constant	0.648***	0.540***	0.647***
	(0.0123)	(0.0131)	(0.0102)
N	31421	31386	31390

Notes: These models are the output of coarsened exact matching. Observations were matched using crimes, the authority that arrested the respondent, jurisdiction in which they are being held (federal or state), their sentencing status, and age. Errors clustered by judicial district. Standard errors in parenthesis. *** : $p < 0.001$, ** : $p < 0.05$, * : $p < 0.05$.

4.4 Robustness of Reform to Exclusion of States

For fear that specific observations may be driving the results, we iterate over states, excluding each one, and reestimating the our results. We replicate the specifications in models 7 - 9 in Table A2 and report the reform coefficients and standard errors from these models. We run this test twice, first including both state and federal prisoners in Table A8 and once again focusing exclusively on state prisoners in Table A9. Because the federal reform was implemented all at once for the federal system in a given state, that specification includes some prisoners from all states – including those where the state government chose to reform criminal procedure by one of the methods that we ignore in evaluating the state reforms.

Table A8: **Effects of the reform excluding individual states**

State Excluded	(1) Torture	(2) Brute	(3) Threats
Aguascalientes	-0.0588**	-0.0754***	-0.0718***
	(0.0193)	(0.0179)	(0.0163)
Baja California	-0.0524**	-0.0700***	-0.0704***
	(0.0199)	(0.0184)	(0.0175)
Baja California Sur	-0.0565**	-0.0737***	-0.0733***
	(0.0197)	(0.0183)	(0.0167)
Campeche	-0.0584**	-0.0735***	-0.0701***
	(0.0200)	(0.0184)	(0.0168)
Coahuila	-0.0583**	-0.0752***	-0.0716***
	(0.0193)	(0.0179)	(0.0164)
Colima	-0.0541**	-0.0730***	-0.0707***
	(0.0193)	(0.0181)	(0.0167)
Chiapas	-0.0592**	-0.0759***	-0.0724***
	(0.0193)	(0.0178)	(0.0163)
Chihuahua	-0.0637**	-0.0809***	-0.0764***
	(0.0195)	(0.0182)	(0.0166)
Distrito Federal	-0.0591**	-0.0758***	-0.0723***
	(0.0193)	(0.0179)	(0.0164)
Durango	-0.0529**	-0.0708***	-0.0679***
	(0.0193)	(0.0175)	(0.0155)
Guanajuato	-0.0611**	-0.0772***	-0.0710***
	(0.0199)	(0.0183)	(0.0167)
Guerrero	-0.0592**	-0.0763***	-0.0711***
	(0.0197)	(0.0183)	(0.0168)
Hidalgo	-0.0590**	-0.0739***	-0.0682***
	(0.0199)	(0.0184)	(0.0163)
Jalisco	-0.0544**	-0.0728***	-0.0724***
	(0.0195)	(0.0182)	(0.0163)
México	-0.0614**	-0.0678***	-0.0756***
	(0.0206)	(0.0186)	(0.0166)
Michoacán	-0.0583**	-0.0760***	-0.0684***
	(0.0196)	(0.0182)	(0.0165)
Morelos	-0.0694***	-0.0834***	-0.0784***
	(0.0188)	(0.0184)	(0.0168)
Nayarit	-0.0588**	-0.0755***	-0.0719***
	(0.0193)	(0.0182)	(0.0168)
Nuevo León	-0.0560**	-0.0739***	-0.0697***
	(0.0198)	(0.0183)	(0.0169)
Oaxaca	-0.0627**	-0.0759***	-0.0729***
	(0.0195)	(0.0182)	(0.0164)
Puebla	-0.0595**	-0.0792***	-0.0712***
	(0.0195)	(0.0182)	(0.0168)
Querétaro	-0.0585**	-0.0765***	-0.0749***
	(0.0199)	(0.0178)	(0.0168)
Quintana Roo	-0.0597**	-0.0781***	-0.0703***
	(0.0198)	(0.0183)	(0.0169)
San Luis Potosí	-0.0587**	-0.0761***	-0.0715***
	(0.0193)	(0.0178)	(0.0164)
Sinaloa	-0.0578**	-0.0747***	-0.0738***
	(0.0201)	(0.0180)	(0.0168)
Sonora	-0.0587**	-0.0758***	-0.0704***
	(0.0194)	(0.0179)	(0.0164)
Tabasco	-0.0583**	-0.0778***	-0.0719***
	(0.0198)	(0.0183)	(0.0168)
Tamaulipas	-0.0595**	-0.0752***	-0.0710***
	(0.0195)	(0.0181)	(0.0165)
Tlaxcala	-0.0587**	-0.0754***	-0.0718***
	(0.0193)	(0.0179)	(0.0163)
Veracruz	-0.0569**	-0.0732***	-0.0695***
	(0.0194)	(0.0181)	(0.0166)
Yucatán	-0.0595**	-0.0782***	-0.0735***
	(0.0197)	(0.0181)	(0.0167)
Zacatecas	-0.0608**	-0.0749***	-0.0734***
	(0.0193)	(0.0179)	(0.0166)

Note: Coefficients from OLS regressions on the effects of the reform omitting the state named. All models have municipal and year fixed effects. The table reports these coefficients for our sample of state prisoners along with all federal prisoners arrested in those states. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A9: **Effects of the reform excluding individual states**

State Excluded	(1) Torture	(2) Brute	(3) Threats
Baja California	-0.0440*	-0.0549**	-0.0537***
	(0.0192)	(0.0177)	(0.0161)
Baja California Sur	-0.0485*	-0.0581**	-0.0577***
	(0.0196)	(0.0180)	(0.0156)
Campeche	-0.0505*	-0.0591**	-0.0544***
	(0.0198)	(0.0181)	(0.0156)
Colima	-0.0452*	-0.0572**	-0.0543***
	(0.0189)	(0.0176)	(0.0154)
Chihuahua	-0.0548**	-0.0639***	-0.0589***
	(0.0193)	(0.0180)	(0.0155)
Durango	-0.0529**	-0.0641***	-0.0613***
	(0.0201)	(0.0183)	(0.0160)
Guanajuato	-0.0537**	-0.0629***	-0.0545***
	(0.0197)	(0.0180)	(0.0156)
Guerrero	-0.0525**	-0.0618***	-0.0561***
	(0.0194)	(0.0179)	(0.0156)
Hidalgo	-0.0508*	-0.0590**	-0.0520***
	(0.0197)	(0.0181)	(0.0156)
Jalisco	-0.0469*	-0.0573**	-0.0567***
	(0.0194)	(0.0179)	(0.0159)
México	-0.0518*	-0.0526**	-0.0567***
	(0.0207)	(0.0184)	(0.0158)
Michoacán	-0.0511**	-0.0603***	-0.0551***
	(0.0193)	(0.0177)	(0.0153)
Morelos	-0.0615**	-0.0688***	-0.0643***
	(0.0187)	(0.0179)	(0.0151)
Nuevo León	-0.0477*	-0.0585**	-0.0537***
	(0.0196)	(0.0178)	(0.0153)
Oaxaca	-0.0547**	-0.0600***	-0.0573***
	(0.0189)	(0.0178)	(0.0153)
Puebla	-0.0525**	-0.0649***	-0.0550***
	(0.0198)	(0.0177)	(0.0156)
Querétaro	-0.0504*	-0.0606***	-0.0588***
	(0.0197)	(0.0180)	(0.0157)
Quintana Roo	-0.0518**	-0.0631***	-0.0540***
	(0.0191)	(0.0181)	(0.0158)
Sinaloa	-0.0492*	-0.0594***	-0.0574***
	(0.0199)	(0.0177)	(0.0156)
Tabasco	-0.0513**	-0.0641***	-0.0569***
	(0.0197)	(0.0180)	(0.0156)
Veracruz	-0.0495*	-0.0576**	-0.0546***
	(0.0194)	(0.0177)	(0.0154)
Yucatán	-0.0515**	-0.0630***	-0.0577***
	(0.0194)	(0.0178)	(0.0155)
Zacatecas	-0.0531**	-0.0607***	-0.0578***
	(0.0190)	(0.0174)	(0.0153)

Note: Coefficients from OLS regressions on the effects of the reform omitting the state named. All models have municipal and year fixed effects. This table reports these coefficients for our sample of state prisoners and excludes all federal prisoners arrested in those states. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

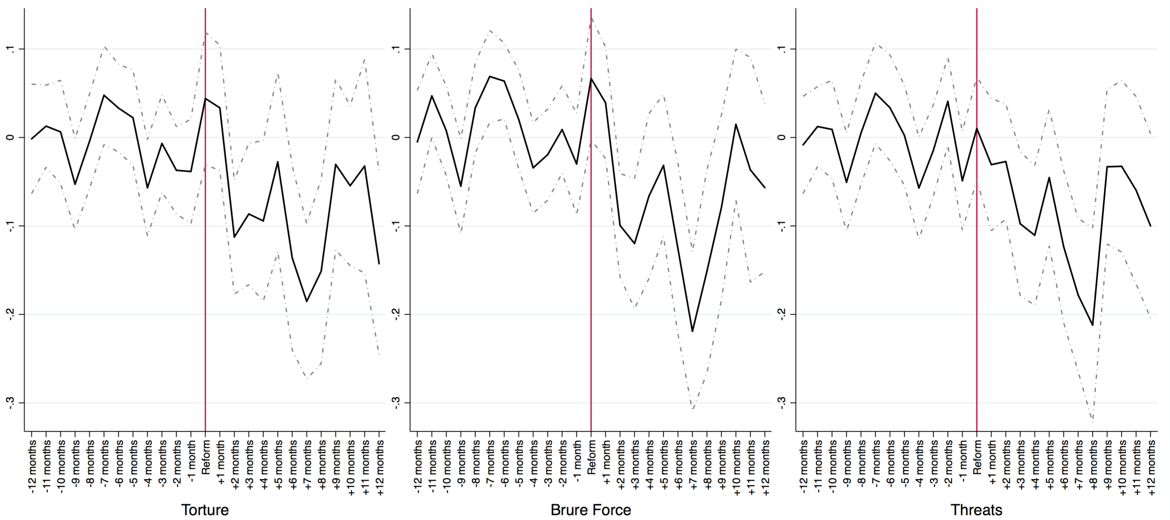
4.5 Parallel Trends

In order to test the plausibility of the parallel trends assumption, we run a series of supplementary tests in this section.

4.5.1 Leads and lags

Autor (2003) and Angrist and Pischke (2009) recommend using leads of the treatment to test the plausibility of the parallel trends assumption. We examine the 12 months leading up to the implementation of the reform. We assign dummy variables that indicate in which month relative to the reform an arrest took place and we estimate whether there are anticipatory effects. In the case of a divergence in trends prior to the actual implementation of the reform, this approach should pick up that change. The models in Figure A1 show little evidence of a prior effect, with the leads largely jumping around 0. Only one month – 9 months prior to the reform – shows up a statistically significant in any of the specifications, meaning there is no real evidence of a divergence in the trends prior to the reform. This, in combination with the results from Section 4.5.1 provides strong support for our identification strategy.

Figure A1: **Leads and lags around reform**



Note: The solid line presents the lead and lag coefficients for the months before/after the reform. The dotted lines show their 95% confidence intervals. The dashed vertical line marks the implementation of the reform.

4.5.2 Unit-specific trends

Following Angrist and Pischke (2009, 2015), we run this specification:

$$y_{iut} = \alpha + \tau Reform_{ut} + \sum_k \delta Unit_{ku} + \sum_j \gamma Year_{jt} + \sum_k \lambda_k (unit_{ku} \cdot Year) + \epsilon_{ut} \quad (2)$$

The intuition underpinning this specification is that it relaxes the assumption of a common trend by imposing a stricter specification that allows units to vary along their own trends while still identifying a deviation in trends induced by the treatment. We run this specification for three levels of geographic aggregation: the state, judicial district, and municipality. We run these models on three different versions of our dataset. First, we run them on the full sample. Second, we run them excluding all arrests prior to 2008, thus focusing on the period in which the Constitutional Reform was happening. Finally, due to the Enrique Peña Nieto administration's interest in pushing the reform as compared to the Calderón administration's apparent lack of effort in curbing this problem, we subset to those arrests which occur during the Peña Nieto administration by excluding arrests prior to 2013. Irrespective of the geographic unit or subset of the data that we use, we find significant effects of the reform as shown in Table A10.

As a further check, we re-estimate the equation above using fixed effects and time trends for the unique year-month rather than simply the year of arrest, essentially running this same test for the specifications discussed in Section 4.2 of the Appendix. We report these models in Table A10.

We run an alternate set of specifications in this section to control for time trends. Whereas the models in Table A10 include a time trend that assumes linearity, we run a model that instead uses state-year fixed effects in Table A11.[3] These allow us to control flexibly for temporal shocks much more flexibly at the state level – factors like elections, political turnover, or criminal conflict – and do not make an assumption that there will be a linear time trend. All 42 models considered in this section still pick up the effect of the reform, dramatically bolstering our confidence in our identification strategy.

$$y_{iut} = \alpha + \tau Reform_{ut} + \sum_j \delta Unit_{ju} + \sum_j \gamma Unit_{ju} \cdot Year_{j,t,u} + \epsilon_{ut} \tag{3}$$

[3]We thank Dorothy Kronick for this suggestion.

Table A10: **Effects of the reforms and unit-specific time trends**

	(1) Torture	(2) Brute	(3) Threat	(4) Torture	(5) Brute	(6) Threat	(7) Torture	(8) Brute	(9) Threat
Full Sample									
Reform	-0.0655***	-0.0829***	-0.0721***	-0.0456**	-0.0596***	-0.0566***	-0.0602**	-0.0727***	-0.0740***
	(0.0172)	(0.0175)	(0.0172)	(0.0173)	(0.0175)	(0.0169)	(0.0183)	(0.0184)	(0.0182)
N	37726	37765	37720	37726	37765	37720	37726	37765	37720
Constitutional Reform Period (2008+)									
Reform	-0.0647***	-0.0742***	-0.0741***	-0.0561***	-0.0607***	-0.0700***	-0.0615***	-0.0633***	-0.0745***
	(0.0153)	(0.0167)	(0.0163)	(0.0162)	(0.0178)	(0.0176)	(0.0164)	(0.0177)	(0.0174)
N	32440	32466	32445	32440	32466	32445	32440	32466	32445
Peña Nieto Administration (2013+)									
Reform	-0.0698***	-0.0636***	-0.0705***	-0.0467*	-0.0403*	-0.0570**	-0.0558**	-0.0534**	-0.0657***
	(0.0173)	(0.0161)	(0.0162)	(0.0194)	(0.0178)	(0.0187)	(0.0203)	(0.0195)	(0.0183)
N	20100	20113	20103	20100	20113	20103	20100	20113	20103
Month fixed effects, full sample									
Reform	-0.0814***	-0.0682***	-0.0663***	-0.0550**	-0.0430*	-0.0475*	-0.0690**	-0.0579**	-0.0642**
	(0.0198)	(0.0197)	(0.0191)	(0.0192)	(0.0192)	(0.0191)	(0.0209)	(0.0212)	(0.0210)
N	37765	37726	37720	37765	37726	37720	37765	37726	37720
Unit-level time trends	State	State	State	District	District	District	Municipal	Municipal	Municipal

Note: Coefficients from OLS regressions on the effects of the reform mirroring models in Table 6 of the main text but here including state, judicial district, and municipal level time trends. We run this four times: (1) on the full ENPOL sample, (2) beginning in 2008 when the reform was passed by Congress, (3) beginning in 2013 with the Peña Nieto administration, and (4) on the full ENPOL sample using unique months, not years. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A11: **Effects of the reform with state-specific year fixed effects**

	(1) Brute	(2) Torture	(3) Threat	(4) Brute	(5) Torture	(6) Threat
Reform	-0.0743***	-0.0652***	-0.0693***	-0.0520**	-0.0482**	-0.0568***
	(0.0176)	(0.0166)	(0.0168)	(0.0164)	(0.0161)	(0.0151)
N	37765	37726	37720	37765	37726	37720
State x Year FE	Y	Y	Y	Y	Y	Y
Municipal FE				Y	Y	Y

Note: Coefficients from OLS regressions on the effects of the reform using state-specific or municipal-specific year fixed effects. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

4.5.3 Anticipation effects

We present a full set of the models that we report in the paper which test for whether there is a detectable effect of jurisdictions anticipating the reform's implementation and adjusting their behavior accordingly. Each state published declarations in its official records when the state legislature approved a timetable for the implementation of the reform; these dates are reported in Table A12. 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.[4] 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 demographic controls, year fixed effects, and state and municipal fixed effects. Our results are reported in Table A13. Across all specifications, there is no negative association between torture and arrests occurring in the post-announcement period and the reform's effects hold.

Table A12

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

Note: Date of announcement of the reform that we use to conduct a placebo check in Table 8. We exclude Morelos, which announced the reform on the date it implemented the reform.

[4] Some 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.

Table A13: **Anticipation effects**

	(1) Torture	(2) Brute	(3) Threats	(4) Torture	(5) Brute	(6) Threats	(7) Torture	(8) Brute	(9) Threats
Reform announced	-0.00782	-0.0112	0.0125	-0.00598	-0.0122	0.0135	-0.00580	-0.0108	0.0128
	(0.0162)	(0.0155)	(0.0161)	(0.0161)	(0.0153)	(0.0161)	(0.0167)	(0.0157)	(0.0163)
Reform	-0.0529*	-0.0789***	-0.0397*	-0.0449*	-0.0708**	-0.0371*	-0.0473*	-0.0707**	-0.0387*
	(0.0217)	(0.0219)	(0.0190)	(0.0213)	(0.0219)	(0.0188)	(0.0226)	(0.0228)	(0.0194)
Male	0.134***	0.160***	-0.0638***	0.134***	0.159***	-0.0636***	0.135***	0.156***	-0.0633***
	(0.0144)	(0.0147)	(0.0121)	(0.0143)	(0.0147)	(0.0122)	(0.0143)	(0.0146)	(0.0122)
Illiterate	-0.0256	-0.00473	-0.0138	-0.0229	-0.00413	-0.0127	-0.0232	-0.00382	-0.0197
	(0.0170)	(0.0152)	(0.0145)	(0.0173)	(0.0153)	(0.0145)	(0.0181)	(0.0162)	(0.0149)
Indigenous	-0.0516***	-0.0525***	-0.0249	-0.0191	-0.0238*	-0.00277	-0.0148	-0.0189	0.00409
	(0.0127)	(0.0124)	(0.0132)	(0.0122)	(0.0120)	(0.0124)	(0.0132)	(0.0125)	(0.0128)
Occupation									
Merchant	0.0188*	0.0227**	0.0288***	0.0170	0.0207*	0.0279**	0.0190*	0.0221*	0.0284**
	(0.00889)	(0.00822)	(0.00854)	(0.00918)	(0.00841)	(0.00887)	(0.00951)	(0.00877)	(0.00903)
Public security	-0.0727***	-0.109***	0.00748	-0.0704**	-0.109***	0.0114	-0.0757***	-0.115***	0.00958
	(0.0216)	(0.0214)	(0.0191)	(0.0214)	(0.0211)	(0.0192)	(0.0220)	(0.0219)	(0.0198)
Private security	-0.0101	-0.00218	-0.0181	-0.0138	-0.00426	-0.0199	-0.0142	-0.00446	-0.0188
	(0.0151)	(0.0138)	(0.0141)	(0.0147)	(0.0134)	(0.0141)	(0.0151)	(0.0137)	(0.0144)
Rural worker	-0.0459***	-0.0423***	-0.0322**	-0.0334**	-0.0288*	-0.0230*	-0.0341**	-0.0261	-0.0234*
	(0.0116)	(0.0126)	(0.0113)	(0.0116)	(0.0126)	(0.0108)	(0.0126)	(0.0137)	(0.0112)
Craftsman	-0.0107	-0.000330	-0.0162	-0.0101	0.000737	-0.0152	-0.00871	0.00126	-0.0156
	(0.00925)	(0.00997)	(0.00990)	(0.00932)	(0.00994)	(0.00993)	(0.00970)	(0.0104)	(0.0102)
Blue collar	0.0116	0.0182	0.00534	0.00790	0.0163	0.00320	0.00702	0.0159	0.00263
	(0.00966)	(0.00992)	(0.0103)	(0.00976)	(0.0100)	(0.0105)	(0.0100)	(0.0105)	(0.0107)
Age									
26-35	-0.0454***	-0.0479***	-0.0134*	-0.0435***	-0.0457***	-0.0114	-0.0421***	-0.0445***	-0.0102
	(0.00682)	(0.00683)	(0.00598)	(0.00691)	(0.00697)	(0.00607)	(0.00705)	(0.00722)	(0.00624)
36-45	-0.108***	-0.118***	-0.0635***	-0.108***	-0.117***	-0.0641***	-0.105***	-0.115***	-0.0607***
	(0.00876)	(0.00823)	(0.00880)	(0.00876)	(0.00830)	(0.00899)	(0.00899)	(0.00855)	(0.00926)
46-55	-0.231***	-0.246***	-0.170***	-0.228***	-0.243***	-0.169***	-0.223***	-0.236***	-0.166***
	(0.0127)	(0.0122)	(0.0112)	(0.0128)	(0.0122)	(0.0114)	(0.0134)	(0.0131)	(0.0124)
56-65	-0.305***	-0.333***	-0.246***	-0.300***	-0.327***	-0.239***	-0.298***	-0.329***	-0.237***
	(0.0180)	(0.0205)	(0.0204)	(0.0179)	(0.0205)	(0.0206)	(0.0186)	(0.0218)	(0.0212)
65+	-0.385***	-0.455***	-0.333***	-0.378***	-0.447***	-0.338***	-0.378***	-0.452***	-0.338***
	(0.0318)	(0.0364)	(0.0397)	(0.0328)	(0.0382)	(0.0403)	(0.0356)	(0.0389)	(0.0416)
Education									
Primary or less	0.00779	-0.00570	0.0143	0.0141	0.0000365	0.0180	0.0227	0.00339	0.0169
	(0.0187)	(0.0191)	(0.0169)	(0.0188)	(0.0194)	(0.0171)	(0.0200)	(0.0207)	(0.0184)
Middle school	0.0473*	0.0267	0.0536**	0.0555**	0.0330	0.0572***	0.0636**	0.0354	0.0548**
	(0.0202)	(0.0210)	(0.0165)	(0.0203)	(0.0212)	(0.0166)	(0.0213)	(0.0224)	(0.0176)
High school	0.0856***	0.0588**	0.0966***	0.0918***	0.0634**	0.0990***	0.0993***	0.0663**	0.0968***
	(0.0203)	(0.0208)	(0.0177)	(0.0202)	(0.0210)	(0.0178)	(0.0213)	(0.0223)	(0.0187)
College or graduate	0.0649**	0.0130	0.0984***	0.0678**	0.0155	0.101***	0.0761**	0.0165	0.0977***
	(0.0237)	(0.0244)	(0.0182)	(0.0238)	(0.0247)	(0.0181)	(0.0251)	(0.0257)	(0.0186)
Wealth index, quantiles									
25% - 50%	-0.0150	-0.0149*	-0.0232**	-0.0159	-0.0158*	-0.0234**	-0.0183	-0.0191*	-0.0243**
	(0.00934)	(0.00717)	(0.00808)	(0.00930)	(0.00720)	(0.00813)	(0.00977)	(0.00747)	(0.00838)
50% - 75%	-0.00912	-0.00172	0.00568	-0.0108	-0.00243	0.00537	-0.00983	0.000473	0.00601
	(0.00841)	(0.00804)	(0.00744)	(0.00830)	(0.00811)	(0.00759)	(0.00849)	(0.00836)	(0.00793)
75% - 100%	0.00557	0.00229	0.00124	0.00400	0.00184	0.000839	0.00518	0.00254	0.00233
	(0.00918)	(0.00821)	(0.00768)	(0.00907)	(0.00825)	(0.00768)	(0.00927)	(0.00858)	(0.00793)
Constant	0.392	0.404	0.357	0.652**	0.527*	0.435	0.457	0.493	0.398
	(0.217)	(0.223)	(0.238)	(0.224)	(0.229)	(0.233)	(0.240)	(0.256)	(0.228)
N	31181	31215	31182	31181	31215	31182	31181	31215	31182
State FE	Y	Y	Y						
Judicial District FE				Y	Y	Y			
Mun. FE							Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note: This table shows the full set of coefficients from the test for anticipation effects reported in Table 8 of the main text. We estimate a model that adds a dummy variable for whether an individual was arrested during the period after the state's implementation timetable was announced but before the reform was actually implemented. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

4.6 Heterogenous effects

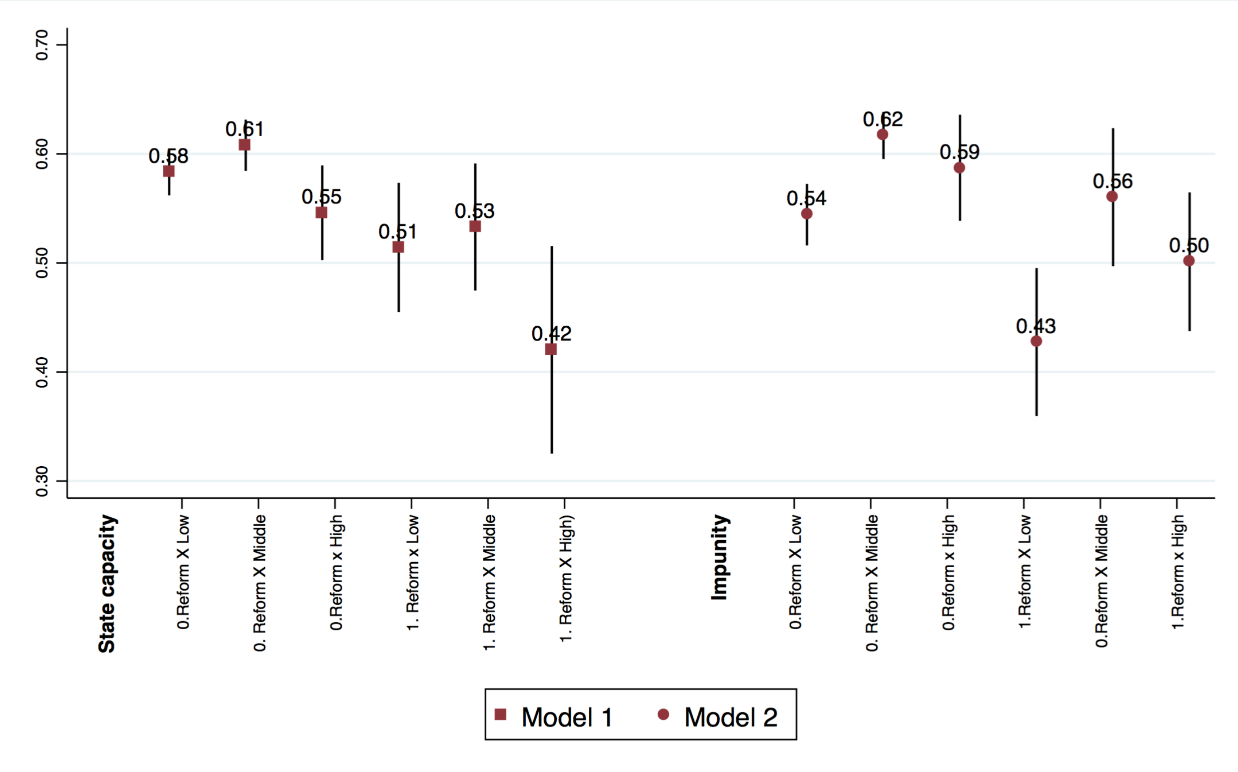
There is likely a great deal of heterogeneity in the way states have adjusted to the reform. One the one hand, states might rely more on coerced confessions due to organizational weaknesses and lack of capacity to investigate crimes, which might partly be driven by absence of adequate personnel, protocols, training and funding for the provision of justice. Institutional corruption might also be driving lack of capacity to investigate crime. We use censuses of government officials to construct a measure of the administrative personnel working in the judiciary – primarily judicial police officers, judges, and secretaries. We aggregate the number of judicial bureaucratic personnel to the level of the judicial district and use this as a measure of state capacity. We also use state level estimates of unreported crimes, derived from the National Victimization Survey (ENVIPE), to examine how the reform changes based on levels of unreported crime. We think there are several reasons why individuals may choose not to report a crime to the authorities, but that they all speak to institutional quality. One might imagine that people fear reprisals or perceive the police as corrupt or ineffective and that this motivates higher rates of unreported crime.

On the other, states with more institutional capacity might adjust better to the reform. We thus tested the following hypotheses, though we exclude them from the main paper:

> **H1A:** *Higher levels of state capacity should be associated with less torture and more efficacy of the reform reducing abuses.*
>
> **H2A:** *Lower levels of unreported crime should be associated with less torture and more efficacy of the reform reducing abuses.*

We report the results of regressions where we interact the reform with these variables in Tables A14 - A16. Interestingly, and consistent with our results with respect to heterogeneity in the main text, we do not find statistically significant heterogeneity. In short – the reform affects states irrespective of state capacity or levels of unreported crime. In Figure A2 we show predicted levels of torture according to our measures of state bureaucratic capacity and unreported crimes.

Figure A2: **Effects of State Capacity and Unreported Crime on Torture**



Notes: The figure shows the incidence of torture by levels of state capacity and impunity. For our measure of state capacity we use terciles of the judicial district's per capita number of bureaucrats working in the administration of justice. For our measure of impunity we use unreported crime, a measure derived from the National Victimization Survey (ENVIPE) which captures the total number of crimes reported in the survey that were not reported to the police. Coefficients are shown in Table A14.

In Table 11 in the paper, we examine how the reform affects institutional torture by arresting authority, whether the respondent is in federal or state prison, and by the presence or absence of a turf war in the year of arrest or the preceding two years. In Tables A17 and A18, we present analogous results for brute force and threats.

Table A14: **Heterogeneous effects on torture: state capacity and impunity**

	(1) Torture	(2) Torture
Local judicial bureaucracy		
Mid tercile	0.0248	
	(0.0130)	
High tercile	-0.0372	
	(0.0230)	
Reform	-0.0689*	
	(0.0297)	
Post-Reform x Mid tercile	-0.00609	
	(0.0341)	
Post-Reform x Upper tercile	-0.0568	
	(0.0523)	
Unreported crime ("cifra negra")		
Mid tercile		0.0727***
		(0.0182)
Upper tercile		0.0430
		(0.0288)
Reform		-0.117***
		(0.0307)
Post-reform x Mid tercile		0.0602
		(0.0423)
Post-reform x Upper tercile		0.0307
		(0.0458)
Constant	0.511	0.506
	(0.290)	(0.305)
N	37307	37632
Year FE	Y	Y
Demographic controls	Y	Y

Note: This table shows regressions coefficients of a model of torture that interacts the reform with levels of state capacity and impunity. For our measure of state capacity we use terciles of the judicial district's per capita number of bureaucrats working in the administration of justice. For our measure of impunity we use unreported crime, a measure derived from the National Victimization Survey (ENVIPE) which captures the total number of crimes reported in the survey that were not reported to the police. Standard errors clustered by judicial district in parentheses. $***: p < 0.001$, $**: p < 0.01$, $*: p < 0.05$.

Table A15: **Heterogeneous effects on brute force: state capacity and impunity**

	(1) Brute	(2) Brute
Local judicial bureaucracy		
Mid tercile	0.0358**	
	(0.0109)	
High tercile	-0.0222	
	(0.0195)	
Reform	-0.0977***	
	(0.0271)	
Post-Reform x Mid tercile	-0.0210	
	(0.0324)	
Post-Reform x Upper tercile	-0.0610	
	(0.0464)	
Unreported crime ("cifra negra")		
Mid tercile		0.0311
		(0.0180)
Upper tercile		0.0104
		(0.0255)
Reform		-0.160***
		(0.0299)
Post-reform x Mid tercile		0.0736*
		(0.0367)
Post-reform x Upper tercile		0.0524
		(0.0439)
Constant	0.490	0.491
	(0.296)	(0.306)
N	37344	37669
Year FE	Y	Y
Demographic controls	Y	Y

Note: This table shows regressions coefficients of a model of brute force that interacts the reform with levels of state capacity and impunity. State capacity is measured with the judicial district's per capita number of bureaucrats working in the administration of justice. We categorize this variable in terciles. Impunity is measured using the percentage crimes that are never reported to the authorities derived from from the National Victimization Survey (ENVIPE). Standard errors clustered by judicial district in parentheses. $***: p < 0.001$, $**: p < 0.01$, $*: p < 0.05$.

Table A16: **Heterogenous effects of the reform on threats: state capacity and impunity**

	(1) Threat	(2) Threat
Local judicial bureaucracy		
Mid tercile	0.0149	
	(0.0127)	
High tercile	-0.0329	
	(0.0200)	
Reform	-0.0917***	
	(0.0256)	
Post-Reform x Mid tercile	-0.0359	
	(0.0297)	
Post-Reform x Upper tercile	-0.0414	
	(0.0365)	
Unreported crime ("cifra negra")		
Mid tercile		0.0304
		(0.0192)
Upper tercile		0.0246
		(0.0265)
Reform		-0.142***
		(0.0276)
Post-reform x Mid tercile		0.0588
		(0.0336)
Post-reform x Upper tercile		0.0279
		(0.0335)
Constant	1.046***	1.045***
	(0.0381)	(0.0271)
N	37300	37625
Year FE	Y	Y
Demographic controls	Y	Y

Note: The rows show regression coefficients of a model of threats that interacts the reform with measures of state capacity and impunity, State capacity is measured with the judicial district's per capita number of bureaucrats working in the administration of justice. We categorize this variable in terciles. Impunity is measured using the percentage crimes that are never reported to the authorities derived from from the National Victimization Survey (ENVIPE). Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A17: **Brute force and organized crime**

	(1) Brute	(2) Brute	(3) Brute	(4) Brute
Reform	-0.0591***	-0.0455*	-0.0778***	-0.0193
	(0.0161)	(0.0198)	(0.0176)	(0.0246)
Turf wars and homicides				
Turf war	0.0256*			
	(0.0112)			
Turf war x reform	0.0651			
	(0.0478)			
Homicide rate		0.000242***		
		(0.0000698)		
Homicide rate x reform		-0.000500		
		(0.000517)		
Organized crime				
Organized Crime			0.0676*	
			(0.0269)	
Organized crime x reform			0.0570*	
			(0.0269)	
Coercive institution				
State				0.0546***
				(0.0122)
Ministerial				0.0300***
				(0.00842)
Federal				0.0261
				(0.0160)
Army				0.0938***
				(0.0164)
State x reform				0.0512
				(0.0328)
Ministerial x reform				-0.110***
				(0.0286)
Federal x reform				-0.0772
				(0.0465)
Army x reform				0.0229
				(0.0531)
Constant	0.941***	0.631***	0.195	0.220
	(0.0394)	(0.0929)	(0.308)	(0.286)
N	33756	37276	37669	35620
Year FE	Y	Y	Y	Y
Judicial District FE	Y	Y	Y	Y

Note: Coefficients from OLS regressions where the dependent variable is brute force. The table mirrors Table 11 in the main text. Turf wars take the value of one when the prisoner was arrested during a turf war, defined as an increase in municipal-level homicides of more than three standard deviations above the municipality's historic mean. Homicide rates are municipal-level rates at the time and place of arrest. Organized crime is a measure that takes the value of 1 when the respondent is held in federal custody or was arrested for kidnapping, drug commerce, possession of illegal weapons, and homicide as a measure of "organized crime threat." Coercive institution refers to the authority performing the arrest. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A18: **Threats and organized crime**

	(1) Threat	(2) Threat	(3) Threat	(4) Threat
Reform	-0.0553***	-0.0528**	-0.0766***	-0.0280
	(0.0143)	(0.0164)	(0.0154)	(0.0170)
Turf wars and homicides				
Turf war	0.00818			
	(0.0109)			
Turf war x reform	0.0727**			
	(0.0280)			
Homicide rate		0.000258***		
		(0.0000557)		
Homicide rate x reform		-0.0000944		
		(0.000507)		
Organized crime				
Organized Crime			0.0832***	
			(0.00796)	
Organized crime x reform			0.0642*	
			(0.0274)	
Coercive institution				
State				0.0704***
				(0.0117)
Ministerial				0.0907***
				(0.0101)
Federal				0.106***
				(0.0141)
Army				0.0981***
				(0.0171)
State x reform				0.0290
				(0.0301)
Ministerial x reform				-0.0905***
				(0.0219)
Federal x reform				-0.0498
				(0.0428)
Army x reform				0.0316
				(0.0631)
Constant	1.082***	0.730***	0.747***	0.693***
	(0.0348)	(0.0981)	(0.0391)	(0.0390)
N	33734	37235	37625	35576
Year FE	Y	Y	Y	Y
Judicial District FE	Y	Y	Y	Y

Note: Coefficients from OLS regressions where the dependent variable is threats. The table mirrors Table 11 in the main text. Turf wars take the value of one when the prisoner was arrested during a turf war, defined as an increase in municipal-level homicides of more than three standard deviations above the municipality's historic mean. Homicide rates are municipal-level rates at the time and place of arrest. Organized crime is a measure that takes the value of 1 when the respondent is held in federal custody or was arrested for kidnapping, drug commerce, possession of illegal weapons, and homicide as a measure of "organized crime threat." Coercive institution refers to the authority performing the arrest. Standard errors clustered by judicial district in parentheses. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

4.7 Changes in Arrests and Confounding Factors

Here we provide tables of coefficients of leads and lags that correspond to Figure 4 in the paper.

Table A19: **Crimes, leads and lags**

	(1) Theft	(2) Homicide	(3) Kidnap	(4) Illegal weapons
Reform_{-18}	0.00198	0.00702	0.00508	-0.0183
	(0.0261)	(0.0199)	(0.0155)	(0.0224)
Reform_{-17}	0.0258	-0.0151	0.0123	-0.0260
	(0.0267)	(0.0233)	(0.0212)	(0.0237)
Reform_{-16}	-0.0339	0.0379	0.0141	-0.0177
	(0.0270)	(0.0215)	(0.0179)	(0.0199)
Reform_{-15}	0.00560	0.0266	-0.00527	-0.0205
	(0.0257)	(0.0208)	(0.0200)	(0.0199)
Reform_{-14}	0.00961	0.00598	-0.0184	-0.0558**
	(0.0288)	(0.0202)	(0.0147)	(0.0199)
Reform_{-13}	-0.0135	-0.00694	0.00307	-0.0337
	(0.0291)	(0.0230)	(0.0195)	(0.0218)
Reform_{-12}	-0.0309	0.0297	-0.00674	-0.0394
	(0.0317)	(0.0233)	(0.0195)	(0.0250)
Reform_{-11}	-0.0312	0.00772	-0.0102	0.0111
	(0.0305)	(0.0250)	(0.0224)	(0.0323)
Reform_{-10}	-0.0450	0.0461	0.00874	-0.0219
	(0.0375)	(0.0237)	(0.0212)	(0.0226)
Reform_{-9}	-0.0485	-0.00232	0.0285	-0.00917
	(0.0388)	(0.0255)	(0.0236)	(0.0265)
Reform_{-8}	-0.00956	-0.00643	0.0459	-0.0367
	(0.0378)	(0.0279)	(0.0272)	(0.0265)
Reform_{-7}	-0.0637	-0.000661	0.0308	0.0183
	(0.0372)	(0.0259)	(0.0220)	(0.0276)
Reform_{-6}	-0.0671	0.0419	0.0145	-0.0173
	(0.0402)	(0.0275)	(0.0211)	(0.0295)
Reform_{-5}	-0.0396	0.0309	0.0172	-0.0328
	(0.0421)	(0.0281)	(0.0235)	(0.0354)
Reform_{-4}	-0.0508	0.0418	0.0216	-0.0476
	(0.0412)	(0.0286)	(0.0219)	(0.0311)
Reform_{-3}	-0.0437	0.00631	0.0399	-0.0329
	(0.0408)	(0.0292)	(0.0238)	(0.0321)
Reform_{-2}	-0.0360	0.0450	0.00921	-0.0316
	(0.0487)	(0.0305)	(0.0232)	(0.0291)
Reform_{-1}	-0.0901	0.0514	0.0199	-0.0627
	(0.0466)	(0.0324)	(0.0224)	(0.0319)
Reform	-0.0864	0.0291	0.0386	-0.0543
	(0.0524)	(0.0396)	(0.0267)	(0.0358)
Reform_{+1}	-0.127*	0.0713	0.0920*	-0.0654
	(0.0498)	(0.0367)	(0.0361)	(0.0361)
Reform_{+2}	-0.175**	0.0468	0.0341	-0.0274
	(0.0533)	(0.0431)	(0.0323)	(0.0415)
Reform_{+3}	-0.124	0.0629	0.0336	-0.0846*
	(0.0655)	(0.0408)	(0.0303)	(0.0411)
Reform_{+4}	-0.175**	0.0459	0.110*	-0.0837*
	(0.0554)	(0.0436)	(0.0457)	(0.0420)
Reform_{+5}	-0.153**	0.00488	0.102**	-0.0835
	(0.0579)	(0.0420)	(0.0394)	(0.0427)
Reform_{+6}	-0.238***	0.0457	0.0489	-0.0836
	(0.0523)	(0.0553)	(0.0377)	(0.0463)
Reform_{+7}	-0.217***	0.0155	0.00331	-0.0846
	(0.0623)	(0.0543)	(0.0288)	(0.0473)
Reform_{+8}	-0.194**	0.0574	0.0567	-0.0573
	(0.0645)	(0.0614)	(0.0459)	(0.0453)
Reform_{+9}	-0.185**	0.0536	0.0807	-0.0538
	(0.0625)	(0.0627)	(0.0497)	(0.0519)
Reform_{+10}	-0.145	-0.00215	0.0572	-0.0265
	(0.0745)	(0.0484)	(0.0349)	(0.0475)
Reform_{+11}	-0.141	0.0486	0.0265	-0.0312
	(0.0839)	(0.0553)	(0.0379)	(0.0541)
Reform_{+12}	-0.101	0.0413	-0.0115	-0.0790
	(0.0696)	(0.0557)	(0.0331)	(0.0523)
Reform_{+13}	-0.175*	0.176*	0.0460	-0.0289
	(0.0750)	(0.0768)	(0.0529)	(0.0658)
Reform_{+14}	-0.205*	0.0911	0.0352	-0.0759
	(0.0798)	(0.0629)	(0.0394)	(0.0599)
Reform_{+15}	-0.0258	0.0577	0.0628	-0.142*
	(0.0903)	(0.0708)	(0.0445)	(0.0561)
Reform_{+16}	-0.175*	0.196*	0.0744	-0.0631
	(0.0848)	(0.0835)	(0.0525)	(0.0647)
Reform_{+17}	-0.212**	0.0882	0.106	-0.108*
	(0.0745)	(0.0655)	(0.0554)	(0.0511)
Reform_{+18}	-0.296***	0.148	0.106*	-0.0474
	(0.0732)	(0.0759)	(0.0506)	(0.0611)
Reform_{post}	-0.299***	0.0835	0.100*	-0.0747
	(0.0815)	(0.0651)	(0.0445)	(0.0628)
Constant	0.597***	0.234***	0.0872***	-0.260**
	(0.124)	(0.0232)	(0.0124)	(0.0794)
N	39038	39038	39038	39038
Municipal FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y

Note: This table reports coefficients from Figure 4 in the main text. The rows show OLS coefficients for arrests for theft, homicide, kidnap and possessions of illegal weapons for the 12 months before and after the implementation of the reform. Standard errors clustered by judicial district in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A20: **Authorities, leads and lags**

	(1) Municipal	(2) State	(3) Ministerial	(4) Federal	(5) Army
Reform_{-18}	-0.0222	0.00586	0.0177	0.0119	0.00365
	(0.0226)	(0.0171)	(0.0268)	(0.0207)	(0.0102)
Reform_{-17}	-0.0191	0.0666***	-0.0377	-0.00263	0.0105
	(0.0229)	(0.0188)	(0.0286)	(0.0226)	(0.0107)
Reform_{-16}	-0.0403	0.0419	0.0665*	-0.0474*	0.00811
	(0.0295)	(0.0236)	(0.0303)	(0.0224)	(0.0110)
Reform_{-15}	-0.00366	-0.00315	0.0292	-0.00353	0.00436
	(0.0250)	(0.0241)	(0.0275)	(0.0197)	(0.0114)
Reform_{-14}	0.0212	-0.0139	0.0192	-0.0286	0.00925
	(0.0301)	(0.0211)	(0.0262)	(0.0226)	(0.0123)
Reform_{-13}	-0.0245	0.0249	0.0262	-0.0281	0.0291*
	(0.0264)	(0.0225)	(0.0329)	(0.0217)	(0.0121)
Reform_{-12}	0.00356	0.0525**	-0.0178	-0.0370	0.00660
	(0.0345)	(0.0188)	(0.0323)	(0.0247)	(0.0109)
Reform_{-11}	-0.0131	0.0250	0.0261	-0.0431	0.0242*
	(0.0281)	(0.0192)	(0.0344)	(0.0263)	(0.0116)
Reform_{-10}	-0.00916	-0.00501	0.0300	-0.0170	-0.0104
	(0.0306)	(0.0208)	(0.0306)	(0.0311)	(0.0109)
Reform_{-9}	-0.00447	-0.0155	0.0169	-0.0204	0.0195
	(0.0313)	(0.0218)	(0.0326)	(0.0277)	(0.0108)
Reform_{-8}	-0.0111	0.0183	0.000969	-0.0266	0.000634
	(0.0323)	(0.0226)	(0.0394)	(0.0297)	(0.00968)
Reform_{-7}	-0.0627*	0.0327	0.00881	-0.0127	0.00687
	(0.0313)	(0.0260)	(0.0370)	(0.0261)	(0.0115)
Reform_{-6}	-0.0323	0.0168	0.0199	-0.0231	0.0209
	(0.0343)	(0.0241)	(0.0330)	(0.0286)	(0.0122)
Reform_{-5}	-0.0428	-0.00436	0.0318	-0.0433	0.0331*
	(0.0301)	(0.0265)	(0.0326)	(0.0266)	(0.0153)
Reform_{-4}	-0.0364	0.0186	0.0187	-0.0293	0.0198
	(0.0337)	(0.0284)	(0.0380)	(0.0283)	(0.0118)
Reform_{-3}	-0.0619	-0.000466	0.0360	-0.0168	0.0132
	(0.0332)	(0.0279)	(0.0373)	(0.0324)	(0.0127)
Reform_{-2}	-0.0705*	0.0110	0.0872*	-0.0485	0.00711
	(0.0320)	(0.0292)	(0.0364)	(0.0305)	(0.0109)
Reform_{-1}	-0.0631	-0.00921	0.0799*	-0.0311	0.00862
	(0.0384)	(0.0328)	(0.0399)	(0.0315)	(0.0157)
Reform	-0.0887*	0.00135	0.0889*	-0.0213	0.0235
	(0.0411)	(0.0327)	(0.0428)	(0.0322)	(0.0163)
Reform_{+1}	-0.104*	-0.00430	0.112*	-0.0361	0.0130
	(0.0447)	(0.0317)	(0.0487)	(0.0371)	(0.0142)
Reform_{+2}	-0.135**	-0.00347	0.147***	-0.0354	0.0133
	(0.0413)	(0.0334)	(0.0437)	(0.0417)	(0.0162)
Reform_{+3}	-0.0545	-0.0274	0.139**	-0.0539	0.0321
	(0.0456)	(0.0359)	(0.0487)	(0.0377)	(0.0187)
Reform_{+4}	-0.0607	-0.00774	0.0324	-0.0316	0.0312
	(0.0552)	(0.0451)	(0.0529)	(0.0484)	(0.0199)
Reform_{+5}	-0.0870	0.00576	0.146*	-0.0478	0.0398
	(0.0539)	(0.0422)	(0.0620)	(0.0416)	(0.0212)
Reform_{+6}	-0.0363	-0.00545	0.0774	-0.0203	0.0316
	(0.0539)	(0.0379)	(0.0609)	(0.0491)	(0.0262)
Reform_{+7}	-0.0876	-0.0140	0.0404	0.000150	0.0317
	(0.0820)	(0.0412)	(0.0621)	(0.0501)	(0.0274)
Reform_{+8}	-0.0169	-0.0239	0.0215	-0.0335	0.0259
	(0.0599)	(0.0415)	(0.0630)	(0.0468)	(0.0213)
Reform_{+9}	-0.0927	0.0521	0.0739	-0.0418	0.0242
	(0.0612)	(0.0505)	(0.0663)	(0.0492)	(0.0192)
Reform_{+10}	-0.0788	-0.0407	0.0712	-0.0334	0.0212
	(0.0605)	(0.0428)	(0.0700)	(0.0494)	(0.0183)
Reform_{+11}	-0.106	-0.0136	0.129	-0.0499	0.0370
	(0.0720)	(0.0588)	(0.0781)	(0.0516)	(0.0216)
Reform_{+12}	-0.0472	-0.00593	0.0480	-0.0139	0.0332
	(0.0647)	(0.0512)	(0.0640)	(0.0548)	(0.0217)
Reform_{+13}	-0.180**	0.0132	0.145	-0.0437	-0.00824
	(0.0593)	(0.0681)	(0.0820)	(0.0571)	(0.0203)
Reform_{+14}	-0.123*	-0.0158	0.106	0.00165	-0.000570
	(0.0578)	(0.0475)	(0.0654)	(0.0581)	(0.0264)
Reform_{+15}	-0.0934	-0.0114	0.222**	-0.0815	0.0108
	(0.0770)	(0.0582)	(0.0779)	(0.0647)	(0.0281)
Reform_{+16}	-0.0388	-0.126*	0.205	-0.0804	0.0317
	(0.0995)	(0.0628)	(0.110)	(0.0615)	(0.0385)
Reform_{+17}	-0.152*	0.0269	0.172*	-0.0466	0.0593
	(0.0698)	(0.0652)	(0.0861)	(0.0519)	(0.0412)
Reform_{+18}	-0.185**	-0.0495	0.208**	0.0165	0.0372
	(0.0660)	(0.0561)	(0.0766)	(0.0567)	(0.0273)
Reform*post*	-0.137*	0.0491	0.133*	-0.0605	0.0312
	(0.0587)	(0.0579)	(0.0653)	(0.0579)	(0.0183)
Constant	0.00215	0.842***	0.398***	-0.259*	0.111***
	(0.0415)	(0.0573)	(0.0176)	(0.121)	(0.00811)
N	39149	39149	39149	39149	37993
Municipal FE	Y				
State FE		Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y	Y

Note: This table reports coefficients from Figure 4 in the main text. The rows show OLS coefficients for arrests carried out by the different arresting authorities for the 12 months before and after the implementation of the reform. Standard errors clustered by judicial district in parentheses. $***: p < 0.001$, $**: p < 0.01$, $*: p < 0.05$.

5 Drug war

In this section, we provide supplementary evidence for the models relating to joint operations and the drug war. We begin by considering the plausibility of the parallel trends assumption for our argument and then proceed by providing models that complement the ones presented in the main text. Table A21 reports the dates on which Calderón's joint operations began. These dates are recovered from Atuesta (2018), who compiles the start dates of joint operations from official reports by the defense ministry (SEDENA) and compares them to dates reported by the media. In cases where there is a conflict between the two sources, we defer to the media reports.

5.1 Parallel Trends

The work of showing the parallel trends assumption holds is relatively more straightforward here than it was in the section dealing with the reform. Here, there are units that are not treated, which makes simply plotting the trends of the units that never receive the treatment alongside those which do receive the treatment straightforward. Figures A3 - A5 show the trends beginning in 2001 for our three dependent variables. The figures show smoothed conditional means from a GAM applied to the time trend. We group states by whether they received a joint operation or not. The vertical line marks 2006 – late that year is when the first joint operation began. Table A21 shows that these military operations escalated quickly, with most of them having begun by early 2008.

We see clearly that, with respect to the joint operations, the parallel trends assumption is plausible for institutionalized torture and brute force, though not for threats. We provide an additional check in the form of Table A22, where we repeat the test we ran in Section 4.6.2 and introduce unit-level time trends. We do this for the full sample prior to 2014 and again for the democratic period beginning in 2001 and prior to 2014. These results are consistent with the models that find an effect of military operations in increasing both brute force and institutionalized torture, though we acknowledge that the evidence for threats is much weaker.

Table A21: **Date of joint operations**

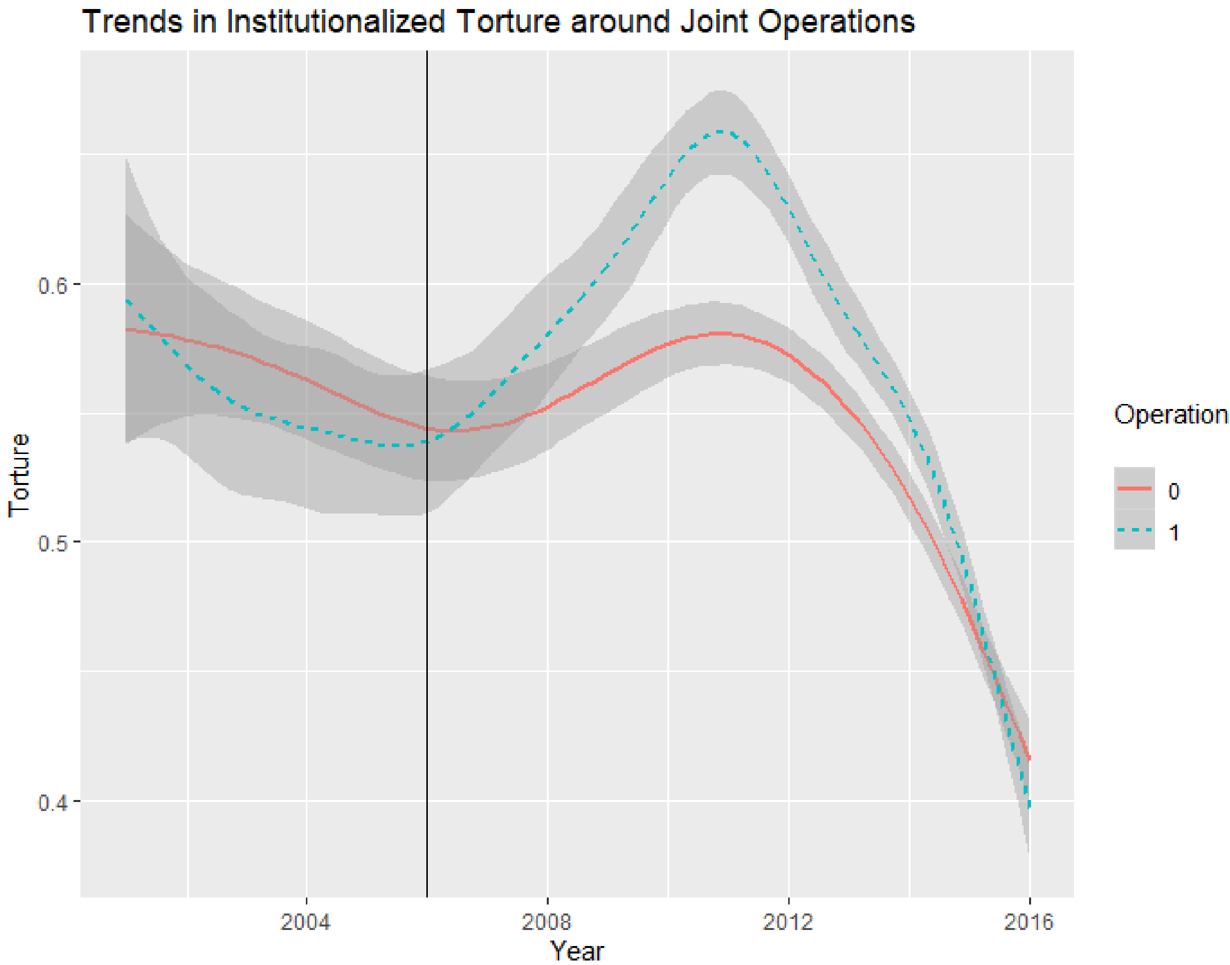
State	Date (y-m-d)
Baja California	2007-1-2
Chihuahua	2008-3-29
Durango	2011-5-5
Guerrero	2007-1-15
Mexico	2012-1-11
Michoacan	2006-12-8
Morelos	2012-1-1
Nuevo Leon	2008-1-1
Sinaloa	2008-5-13
Tamaulipas	2008-1-1
Veracruz	2011-10-4

Table A22: **Effects of joint operations with unit-specific time trends**

	(1) Torture	(2) Brute	(3) Threat	(4) Torture	(5) Brute	(6) Threat
Full Sample						
Operation	0.0850***	0.0788***	0.0766***	0.0750***	0.0770***	0.0790***
	(0.0202)	(0.0212)	(0.0209)	(0.0216)	(0.0228)	(0.0226)
N	25841	25804	25784	25940	25903	25883
Democracy, post-2000						
Operation	0.0587**	0.0465*	0.0542*	0.0541*	0.0534*	0.0627**
	(0.0216)	(0.0218)	(0.0220)	(0.0246)	(0.0242)	(0.0235)
N	23982	23956	23938	24072	24046	24028
Year FE	Y	Y	Y	Y	Y	Y
Unit level trend	State	State	State	Mun	Mun	Mun

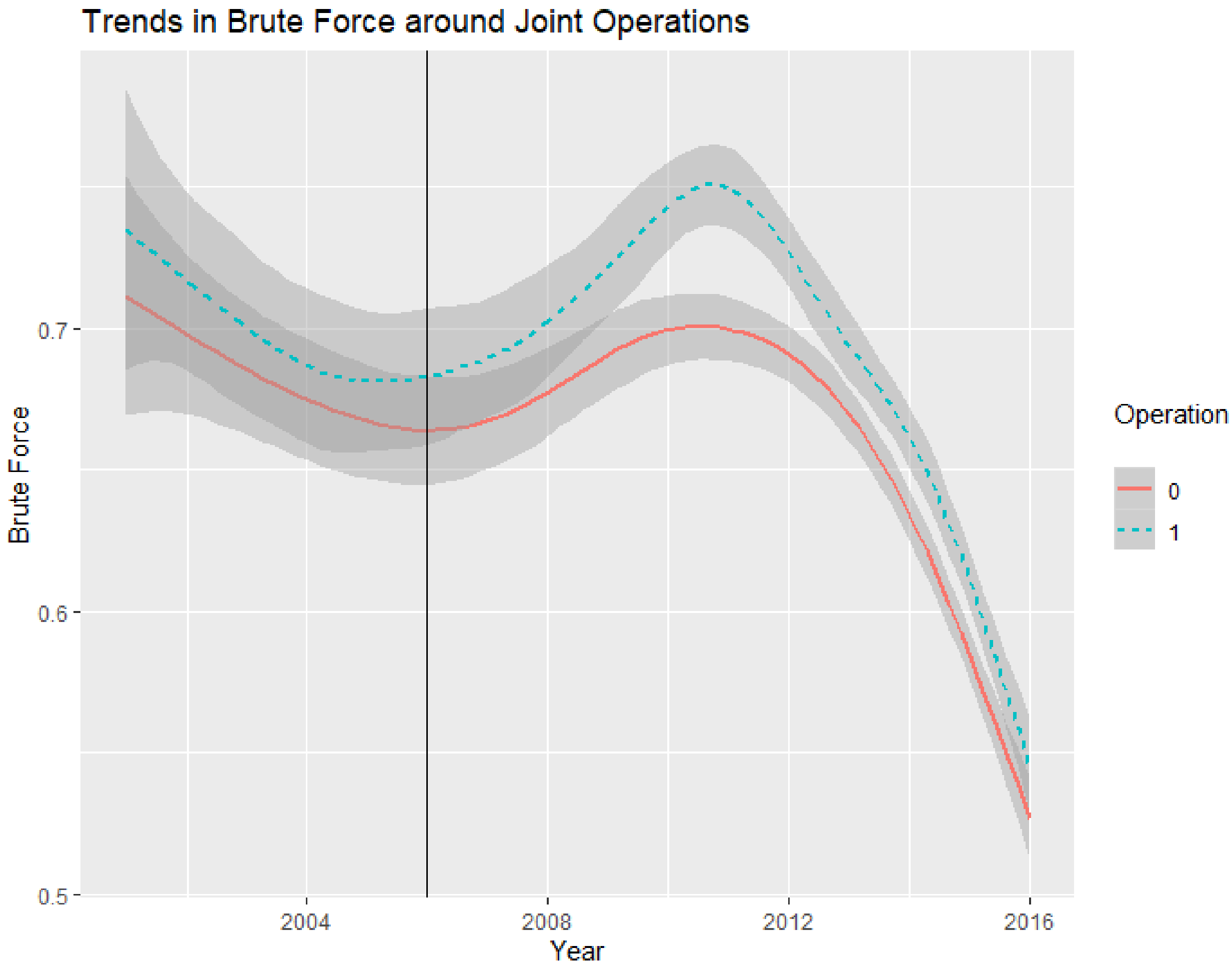
Notes: This table shows our results on joint operations are robust to the inclusion of unit-specific time trends. The rows report coefficients from OLS regressions that include state and municipal-level time trends. Joint operations take the value of one if the prisoner was arrested during a militarized intervention. We runs the models for the full ENPOL sample up through 2012 (upper rows) and beginning in 2001 (lower rows) after the country transitioned to democracy and up through 2012. Standard errors clustered by municipality in parentheses. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Figure A3



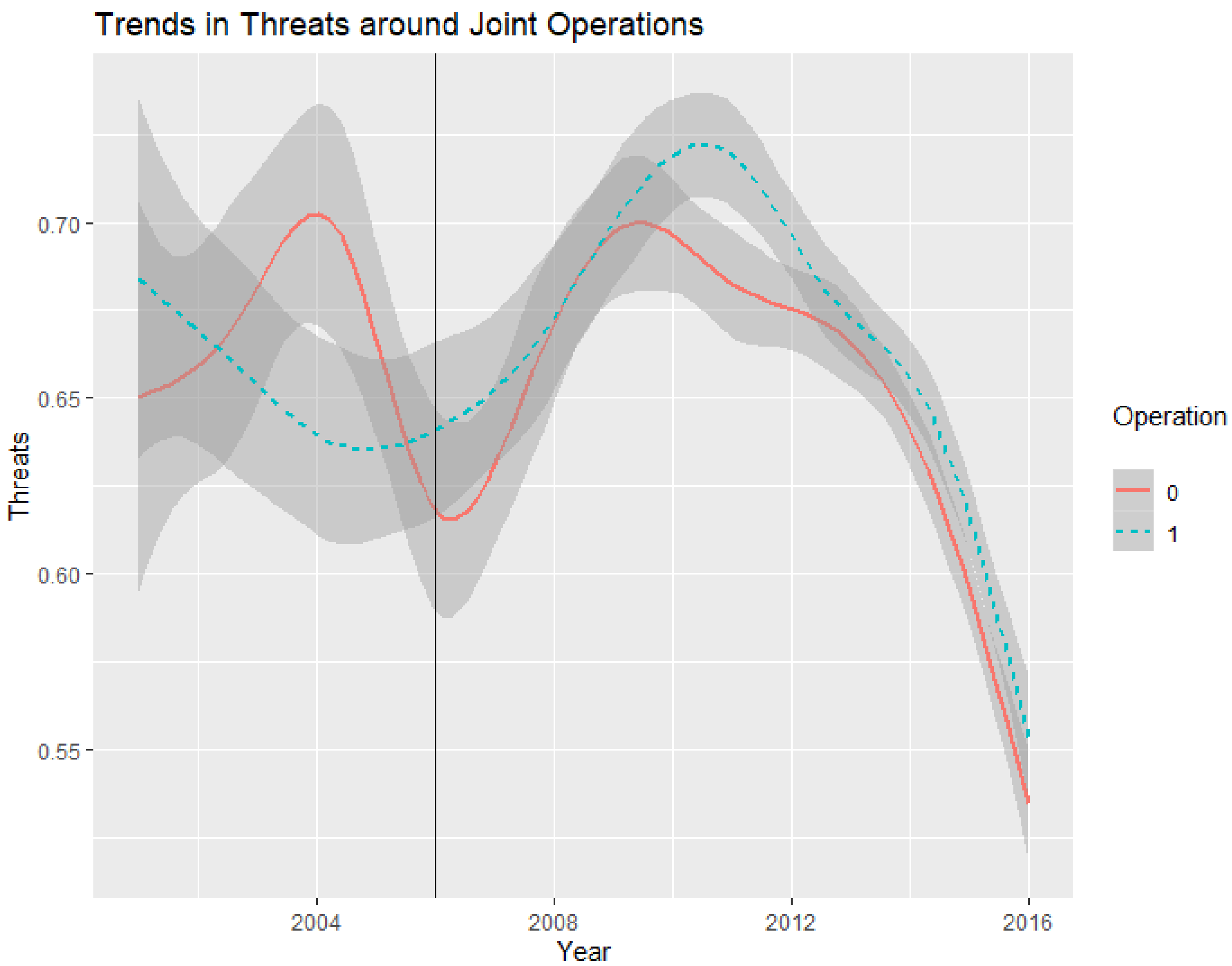
Notes: The figure shows smoothed conditional means from a GAM applied to the time trend dividing states that received a joint operation from those that did not. Black vertical line indicates 2006, when the first such operation began in Michoacán.

Figure A4



Notes: The figure shows smoothed conditional means from a GAM applied to the time trend dividing states that received a joint operation from those that did not. Black vertical line indicates 2006, when the first such operation began in Michoacán.

Figure A5



Notes: The figure shows smoothed conditional means from a GAM applied to the time trend dividing states that received a joint operation from those that did not. Black vertical line indicates 2006, when the first such operation began in Michoacán.

5.2 Heterogeneous Effects

Table A23: **Effects of Militarized Security Interventions on Brute Force**

	(1) Brute force	(2) Brute force	(3) Brute force	(4) Brute force
Joint Operation (JO)	0.0746***	0.0599*	0.0598**	0.0546*
	(0.0176)	(0.0234)	(0.0191)	(0.0249)
Turf war				
Turf War		0.0420*		
		(0.0168)		
Federal prison				
Federal Prison			0.135***	
			(0.0140)	
Federal Prison x JO			0.0111	
			(0.0217)	
Arresting authority				
State				0.0621***
				(0.0136)
Ministerial				0.0521***
				(0.00872)
Federal				0.0934***
				(0.0161)
Army				0.209***
				(0.0151)
State x JO				-0.0403
				(0.0313)
Ministerial x JO				-0.0181
				(0.0236)
Federal x JO				0.0388
				(0.0363)
Army x JO				-0.0173
				(0.0293)
Constant	0.723***	0.711***	0.789**	0.687**
	(0.213)	(0.0388)	(0.240)	(0.220)
N	25747	19879	25846	24340
State FE	Y			Y
Municipal FE		Y	Y	
Year FE	Y	Y	Y	Y

Notes: Entries are coefficients from OLS regressions, and standard errors, clustered by municipality, in parentheses. All models include socio-economic characteristics. We truncate the data to cover all the arrests until the end of 2012, covering the Calderón administration's interventions but excluding arrests after that period. This table is an analogue to Table 9 in the main text. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

Table A24: **Effects of Militarized Security Interventions on Threats**

	(1) Threats	(2) Threats	(3) Threats	(4) Threats
Joint Operation (JO)	0.0688***	0.0434	0.0625**	0.0387
	(0.0187)	(0.0224)	(0.0198)	(0.0255)
Turf war				
Turf War		0.0415*		
		(0.0166)		
Federal prison				
Federal Prison			0.162***	
			(0.0138)	
Federal Prison x JO			-0.0110	
			(0.0228)	
Arresting authority				
State				0.0838***
				(0.0150)
Ministerial				0.103***
				(0.00890)
Federal				0.170***
				(0.0157)
Army				0.232***
				(0.0162)
State x JO				-0.0183
				(0.0342)
Ministerial x JO				0.00616
				(0.0245)
Federal x JO				0.0462
				(0.0368)
Army x JO				-0.0279
				(0.0304)
Constant	0.870***	0.824***	0.836***	0.770***
	(0.223)	(0.0332)	(0.252)	(0.229)
N	25694	19854	25793	24296
State FE	Y			Y
Municipal FE		Y	Y	
Year FE	Y	Y	Y	Y

Notes: Entries are coefficients from OLS regressions, and standard errors, clustered by municipality, in parentheses. All models include socio-economic characteristics. We truncate the data to cover all the arrests until the end of 2012, covering the Calderón administration's interventions but excluding arrests after that period. This table is an analogue to Table 9 in the main text. *** : $p < 0.001$, ** : $p < 0.01$, * : $p < 0.05$.

6 Potential Mechanism: Judicial Decisions

This section presents further details on the structural topic models we used in Section 4 of the text. To recap our approach, we trained our models on a corpus of jurisprudential theses that the Mexican Supreme Court had classified as relating to criminal law. We used the model to classify decisions into the topics that compose the majority of the decision's text and here we report details on the model we present in the paper alongside other models that show similar patterns. We will graph changes in topic proportions over time for each of the models and then provide (1) the frequent and exclusive words[5] for each topic as well as (2) the matters covered in decisions most strongly associated with topics that we classify as related to basic rights in criminal procedure. We use the stm package in R (Roberts et al., 2019). All the models we ran show a similar pattern of a dramatic increase in the proportion of decisions relating to basic rights beginning around 2008.

[5]This is a metric that "weights words by their overall frequent and how exclusive they are to the topic;" in short, the metric uses words that are not rare in the corpus but appear more exclusively within the topic (Roberts et al., 2019).

6.1 3 Topics

Table A25

	Frequent and Exclusive Words
Topic 1	inocencia, testigo, circuito, probatoria, desahogo, imputación, reposición, comparecencia, pruebas, conclusion
Topic 2	extradición, consular, convención, adolescent, internacional, detenida, indígena, niño, desistimiento, interamericana
Topic 3	exacta, robo, multa, narcótico, concurso, penalidad, pecuniaria, taxatividad, agravant, analogía

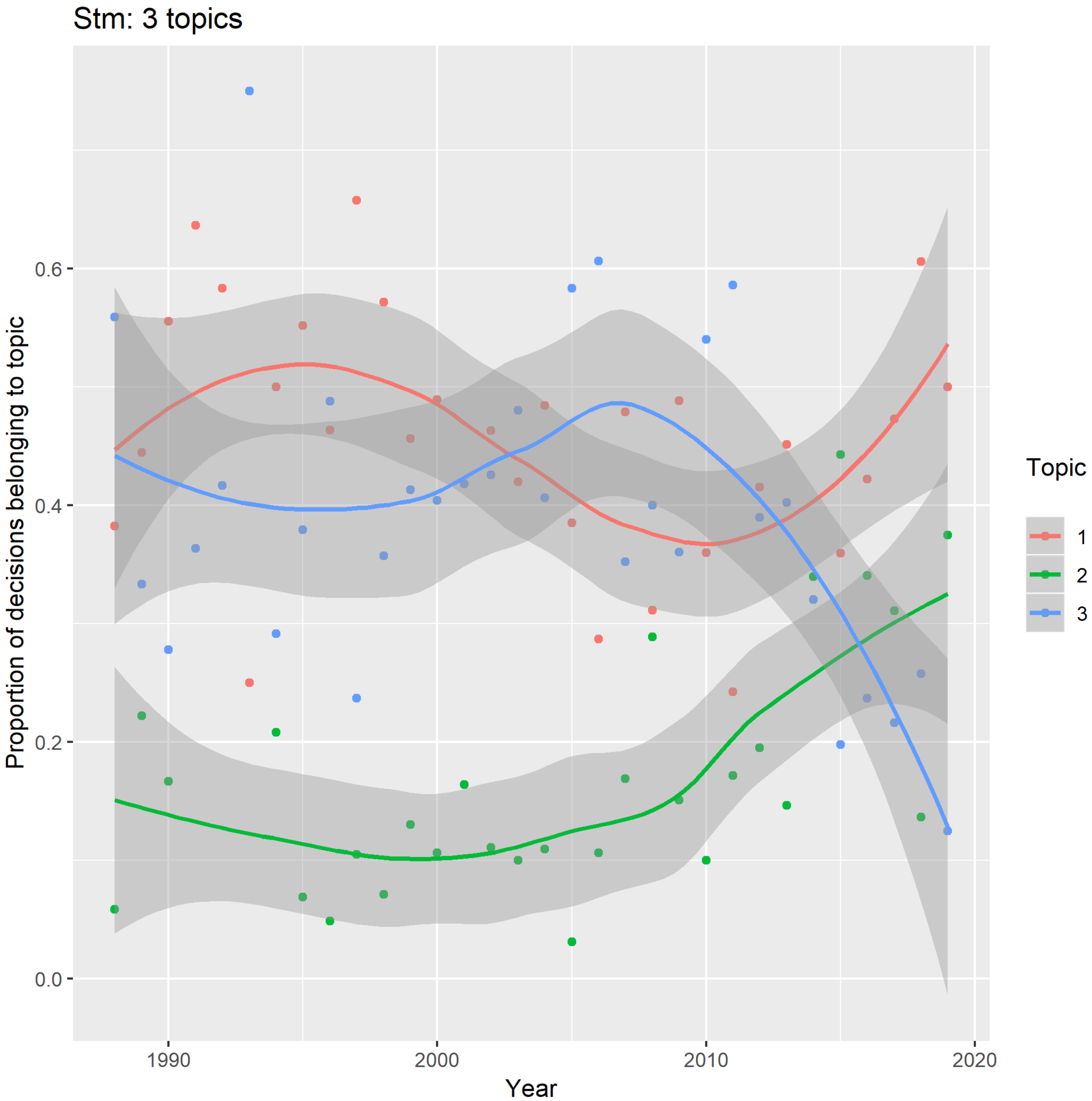
Topic 1

1. Clarification of requirements for *Amparo* hearings.
2. The termination of a judicial proceeding
3. Clarification of the jurisdiction over *amparo* claims
4. Processes for *Amparo* claims against an arrest order
5. The role of *amparo* in addressing violations of fundamental rights in the new accusatory system

Topic 2

1. Fundamental right of consular assistance
2. Measures that judges must take to facilitate the testimony of underage victims of crimes
3. Obligations of consular assistance in cases of dual nationality
4. The right of consular assistance
5. The right of consular assistance

Figure A6



Notes: This figure shows the output of a structural topic model trained with three topics on the corpus of jurisprudential theses. It shows the proportion of decisions each year by the topic they belong to with GAM smoothed conditional means applied to the time trend.

6.2 4 Topics

Table A26

	Frequent and Exclusive Words
Topic 1	inocencia, testigo, probatoria, desahogo, cateo, conclusion, imparcialidad, abreviado, prueba, carpeta
Topic 2	reclamado, extradición, improcedencia, circuito, ejecutoria, amparo, página, semanario, época, tomo
Topic 3	exacta, arma, robo, fuego, vehículo, multa, narcótico, cantidad, concurso, armada
Topic 4	consular, mujer, detenida, indígena, niño, americana, interamericana, indemnización, discriminación, víctima

Topic 1

1. The nature and evidentiary value of circumstantial evidence
2. The similarity of preliminary investigations in the old and new justice systems and the role of evidence gathered in those processes in criminal trials.
3. The presumption of innocence and reasonable doubt
4. The standards for a conviction due to circumstantial evidence.
5. The interpretation and role of *In dubio pro reo*[6]

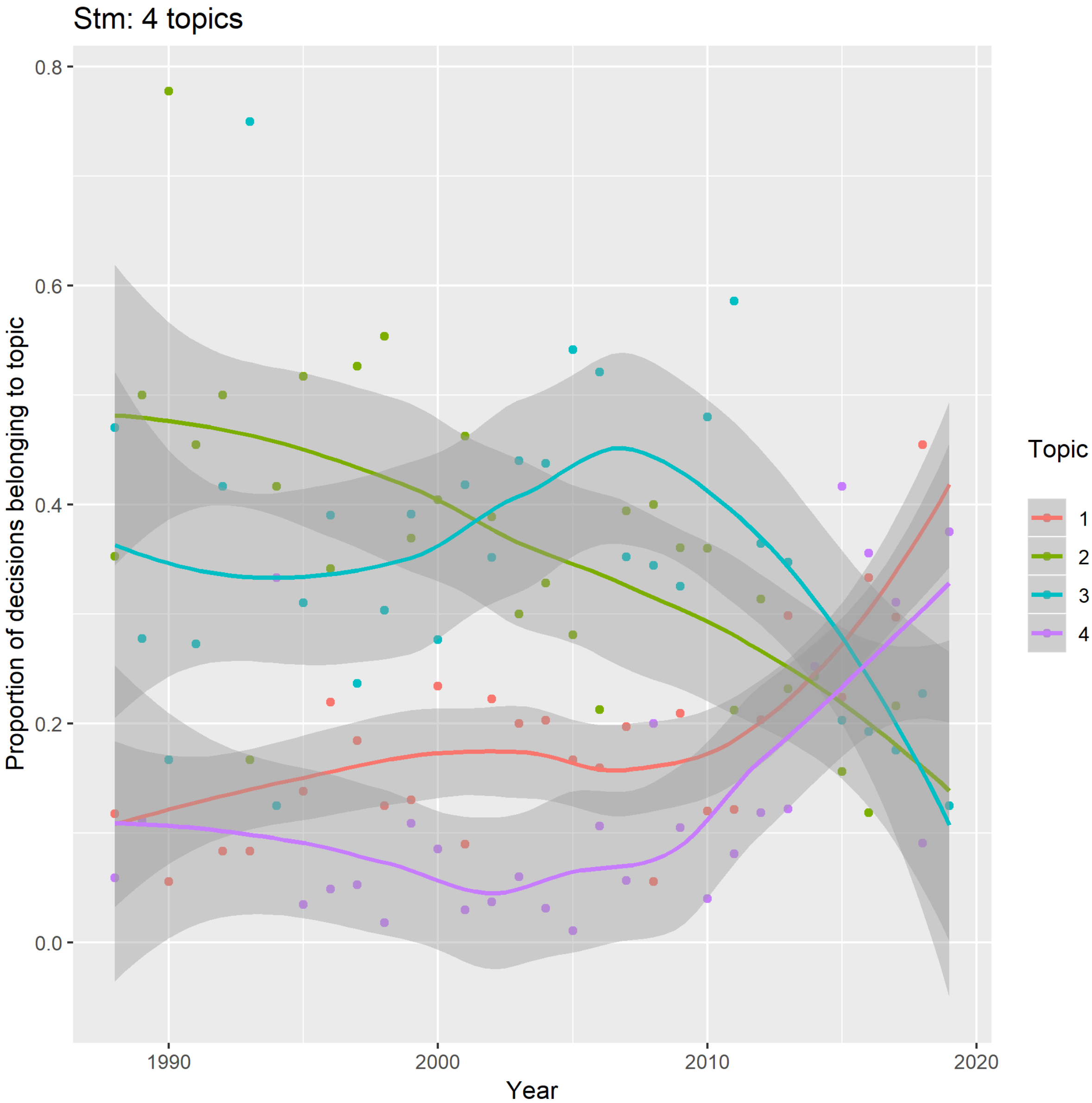
Other prominent decisions here relate to a right, in court, to withdraw one's declaration made at the Public Ministry and the consequences of failing to deliver a detainee to the Public Ministry in a timely fashion.

Topic 4

1. Necessary procedures in facilitating the testimony of a minor victim of a crime
2. The right to consular assistance
3. The right to consular assistance
4. The right to consular assistance
5. The right to consular assistance

[6] When in doubt, for the accused

Figure A7



Notes: This figure shows the output of a structural topic model trained with four topics on the corpus of jurisprudential theses. It shows the proportion of decisions each year by the topic they belong to with GAM smoothed conditional means applied to the time trend.

6.3 5 Topics

This is the model we used in the main text to generate Figure 1.

Table A27

	Frequent and Exclusive Words
Topic 1	aprehensión, apelación, circuito, colegiado, reclamada, amparo, dictó, reclama, definitividad, quejoso
Topic 2	extradición, arma, fuego, armas, transitorio, portación, conexidad, ejército, congreso, explosivo
Topic 3	multa, privativa, penalidad, readaptación, individualización, pena, inusitada, reinserción, penas, individualizar
Topic 4	tortura, consular, detenida, indígena, niño, interamericana, psicológica, humano, escena, mujer
Topic 5	dominio, dolo, presunción, lucro, inocencia, propietario, apoderamiento, taxatividad, fraude, indiciaria

Topic 4

1. Consular assistance
2. Consular assistance
3. Obligations of the police when investigating the crime of *feminicidio*
4. How to handle underage victims of crimes
5. How to handle underage victims of crimes

We think that this topic responds to discussions of consular assistance in the way it does due to the fact that it is often discussed in these decisions as a fundamental right. To illustrate the point, other decisions which are overwhelmingly classified as belonging to this topic include a decision laying out obligations that the Mexican state must meet in the face of allegations of torture (96.7% Topic 4), principles of human rights as they relate to an adequate defense and the exclusion of illicit evidence (96.7%), that the consequences of illegal detention[7] are the immediate liberation of the accused and the exclusion of any evidence that may have been obtained as a consequence (96.1%), the right to be informed immediately of the reasons for an arrest (95.7%), and a judicial definition of acts of torture (95.2%).

The trend in that topic is unmistakably clear – it begins growing in prominence in 2008 and quickly becomes one of the more prominent areas of the Supreme Court's jurisprudence on matters of criminal law.

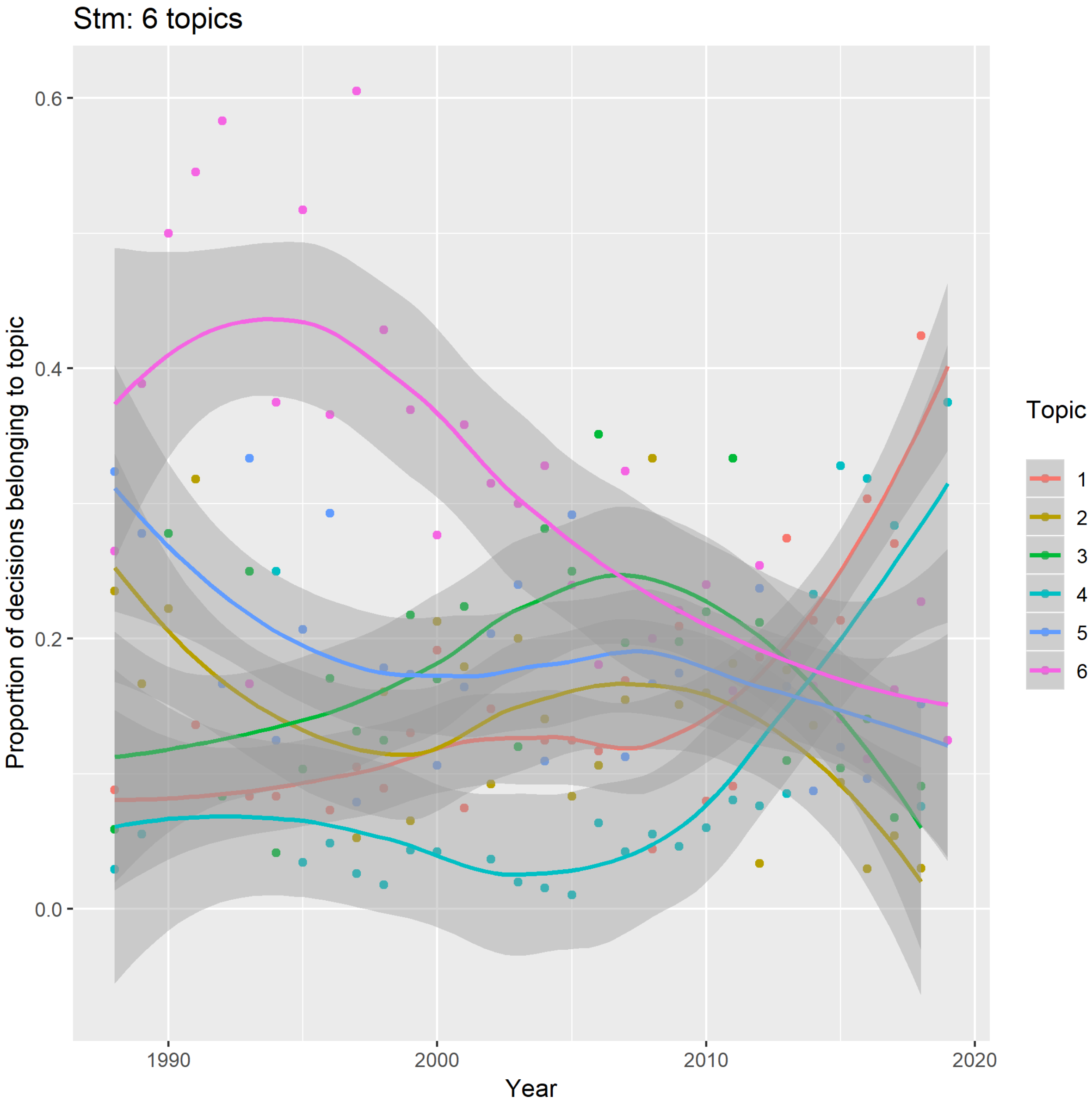
[7]Specifically phrased here as "the violation of the human right to personal liberty"

6.4 6 Topics

Table A28

	Frequent and Exclusive Words
Topic 1	inocencia, testigo, probatoria, prueba, abreviado, presunción, defensor, carpeta, inmediación, inmediatez
Topic 2	extradición, arma, militar, fuego, armas, portación, ejército, congreso, explosivo, aérea
Topic 3	inusitada, pena, penas, individualizar, privativa, multa, farmacodependient, reducción, readaptación, sentenciado
Topic 4	consular, mujer, niño, interamericana, víctima, oficina, tortura, humano, americana, detenida
Topic 5	dominio, taxatividad, muebl, dolo, fraud, lucro, calificativa, activo, vehículo, apoderamiento
Topic 6	circuito, colegiado, amparo, dictó, reclama, quejoso, unitario, interpuesto, reaprehensión, apelación

Figure A8



Notes: This figure shows the output of a structural topic model trained with six topics on the corpus of jurisprudential theses. It shows the proportion of decisions each year by the topic they belong to with GAM smoothed conditional means applied to the time trend.

Topic 1

1. Circumstantial evidence
2. Declarations at a Public Ministry are invalid evidence if retracted in a courtroom
3. Presumption of innocence and reasonable doubt
4. *In dubio pro reo*[8] and its interpretation

[8] "When in doubt, for the accused"

5. Circumstantial evidence

Topic 4

1. How to handle underage victims of crimes
2. Consular assistance
3. Consular assistance
4. Consular assistance
5. How to handle underage victims of crimes

As with the 5 topic model, this one also encompasses decisions relating to basic human rights.

7 Arresting Authority and Multiple Testing Correction

As we examine five authorities in Models 9 and 11 – municipal police, state police, ministerial police, the federal police, and the military – we need to correct for the increased possibility of a false discovery in these models. To do so, we turn to the approach for controlling the false discovery rate described in Benjamini and Hochberg (1995). Rather than use the traditional cutoff of $\alpha = 0.05$ for determining statistical significance, they propose ranking p-values in ascending order and using the rank of the p-value to determine the stringency of the critical value. We instead use $\frac{i}{m}\alpha$, where i is the rank of the p-value,[9] m is the number of tests being run – in this case, five – and the critical value α remains 0.05.

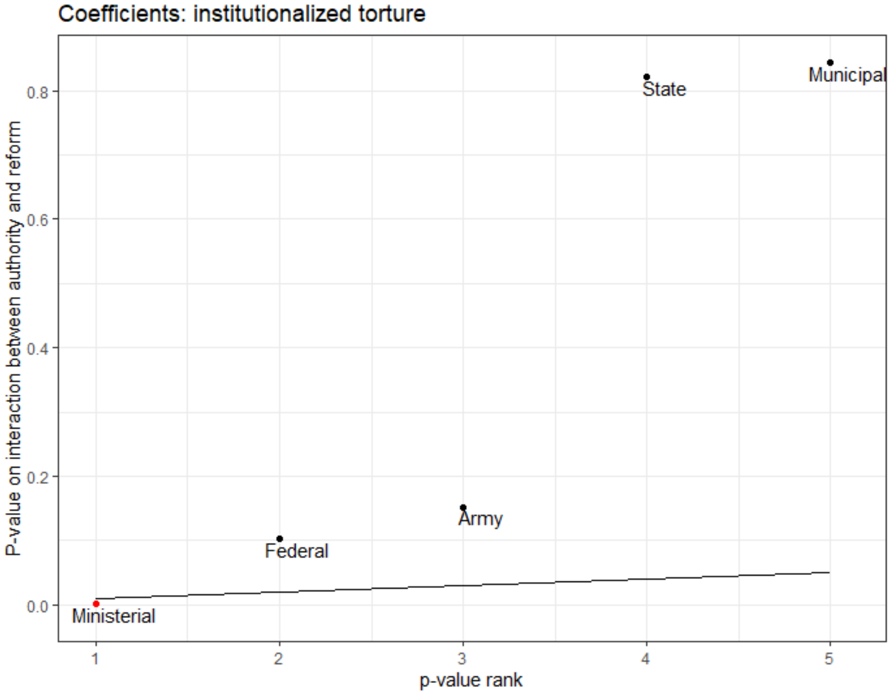
7.1 Coefficients of Reform interacted with Authority

The first iteration of this test we consider relates to the coefficients on the interactive term in Model 4 in Table 9, which interact the reform's implementation with the indicators for the authority carrying out each arrest. Across the three dependent variables, we find that there is a consistent effect of the reform on the ministerial police, who ordinarily investigate crimes.

These tests are reported in Figures A9-A11, which show the p-values for the interaction coefficients and the revised critical value for each one. As we see, the Ministerial Police are the only ones with significant effects.

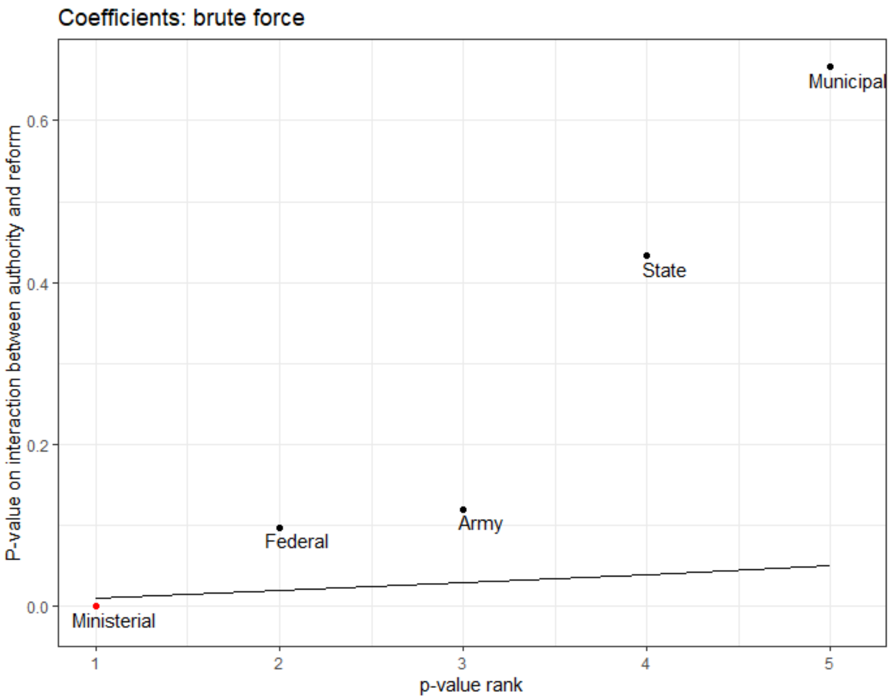
[9] The smallest p-value is one, the next smallest is two, and so on.

Figure A9



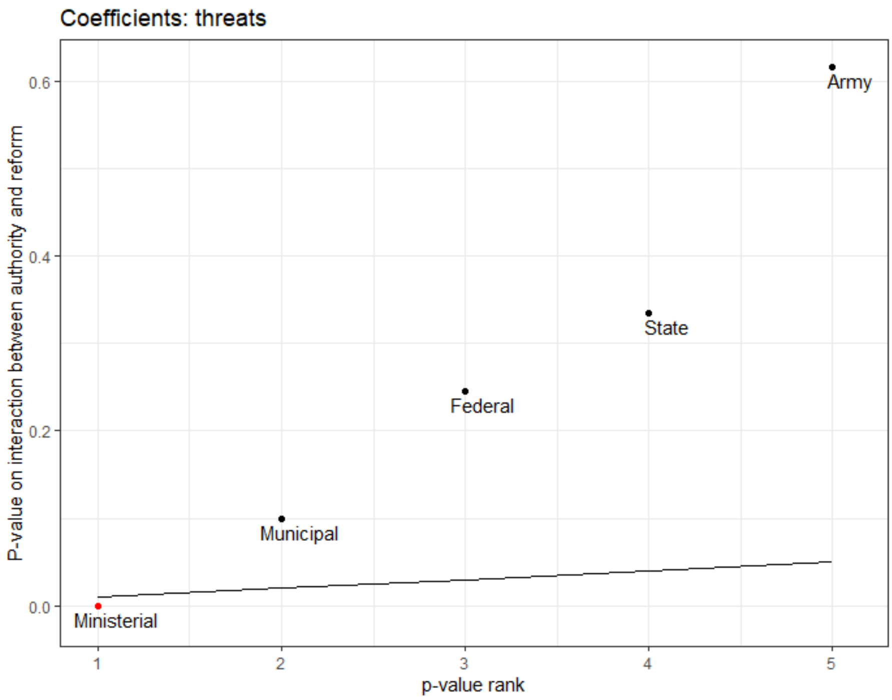
Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of coefficients from a regression of institutionalized torture on authority interacted with the reform.

Figure A10



Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of coefficients from a regression of brute force on authority interacted with the reform.

Figure A11

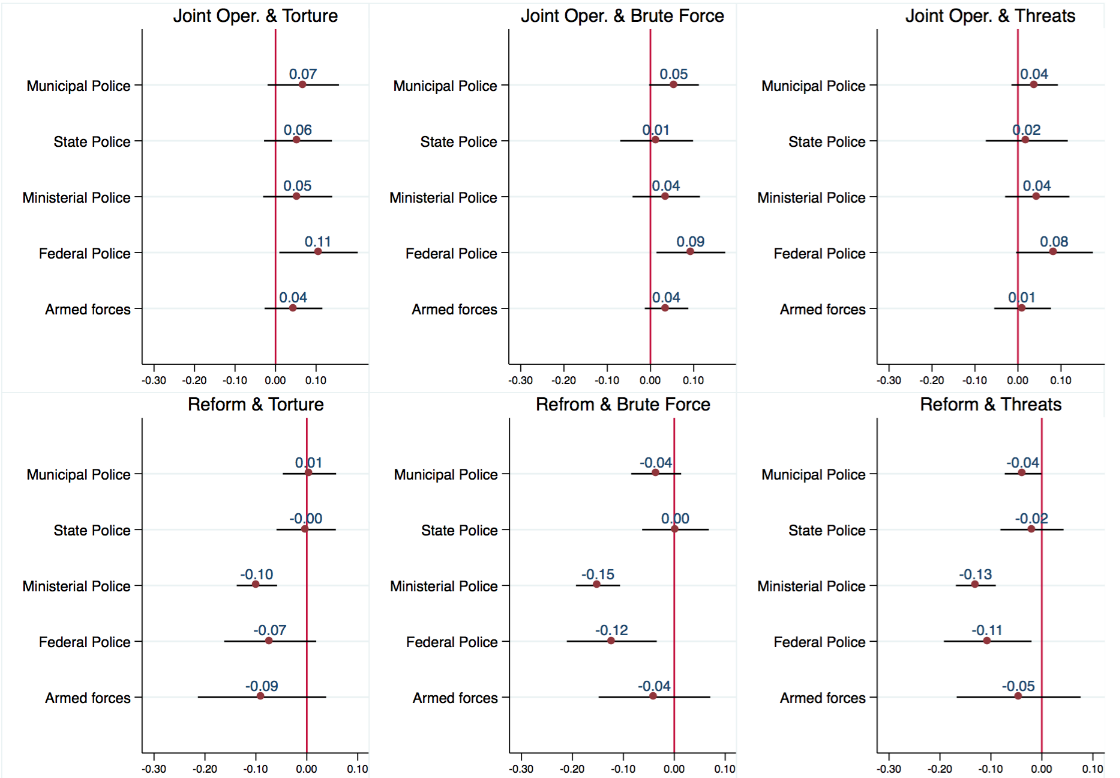


Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of coefficients from a regression of threats on authority interacted with the reform.

7.2 Marginal changes, Joint Operations by Authority

From Models 4 in Tables 11 and 12 we calculate marginal effects of joint operations and the reform by arresting authority, as shown in Figure A12 below.

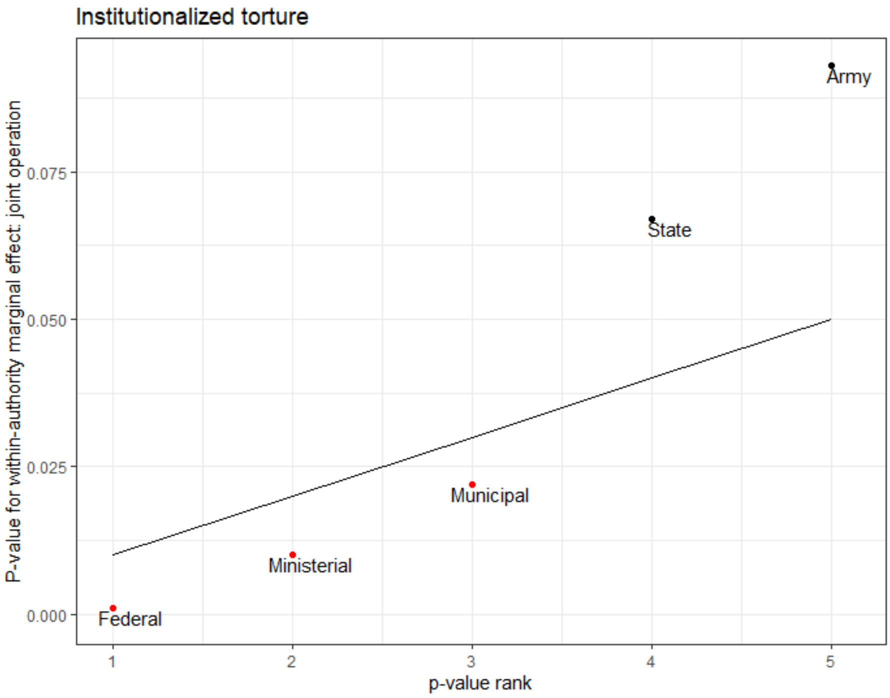
Figure A12: **Effects of militarized interventions and the reform by arresting authority**



Notes: Each figure plots marginal effects and their 95% confidence intervals from OLS regressions presented in Tables 11 and 12, models 4 and the corresponding models in Tables A17, A18. The authority performing the arrest is interacted with joint operations (upper figures) and the reform (lower figures). Joint operations (abbreviated Joint Oper.) take the value of one if the prisoner was arrested during a militarized intervention. Reform takes the value of one when the prisoner was arrested after the reform was implemented. Coefficients are shown in model 4 of Tables 9 and 11, respectively.

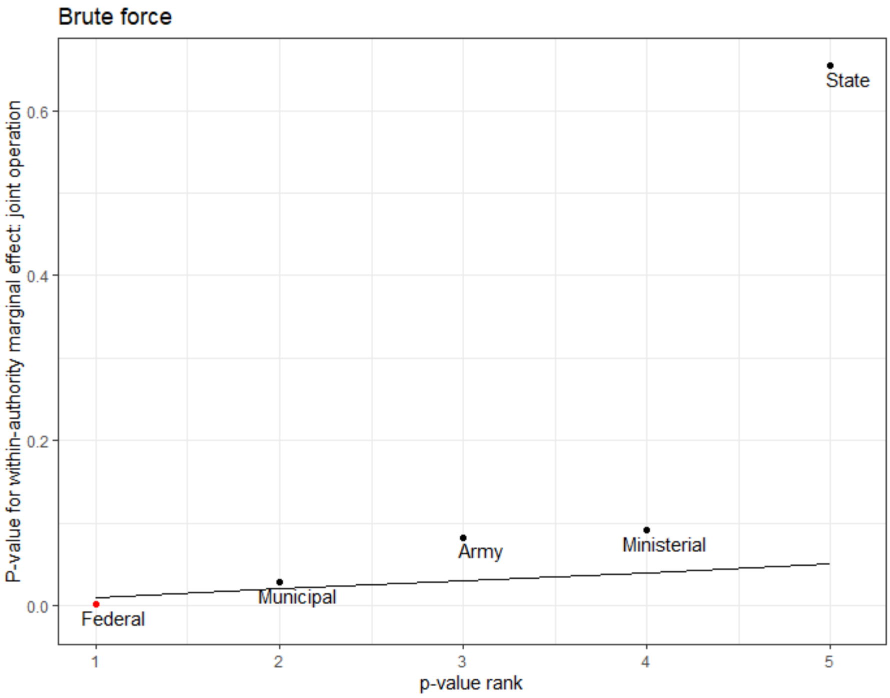
Next, we consider the approach in (Benjamini and Hochberg, 1995) for the margins plotted in Figure A12. That is, *holding constant the authority that conducts an arrest*, what is the effect of the treatment? We find that for the case of joint operations, the margins for institutionalized torture increase among the federal, ministerial, and municipal police, while other forms of abuse only see increases among the federal police.

Figure A13



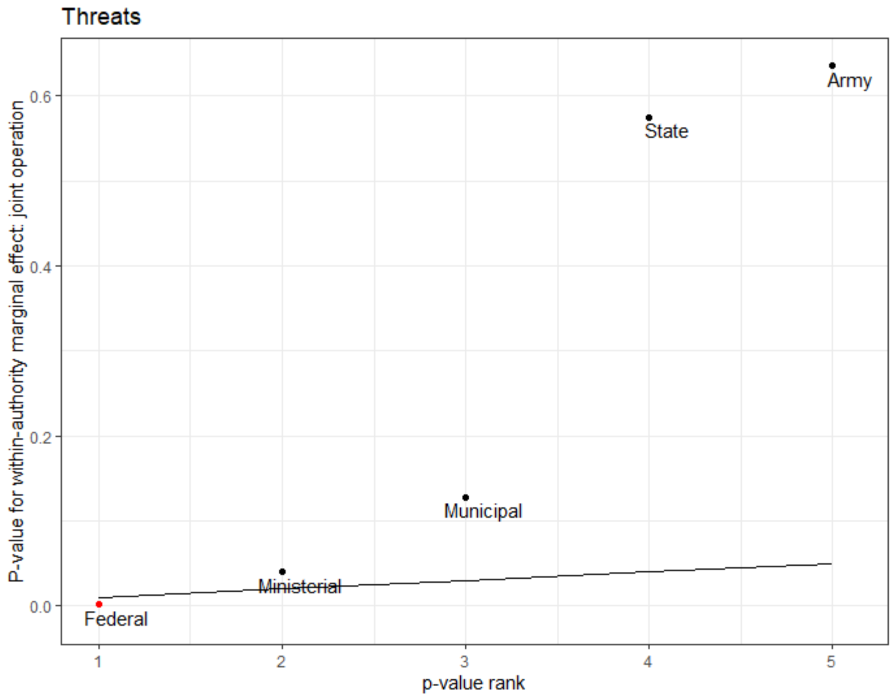
Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of predicted margins of the effect of joint operations interacted with authority on institutionalized torture. Margins plotted in Figure A10.

Figure A14



Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of predicted margins of the effect of joint operations interacted with authority on brute force. Margins plotted in Figure A10.

Figure A15

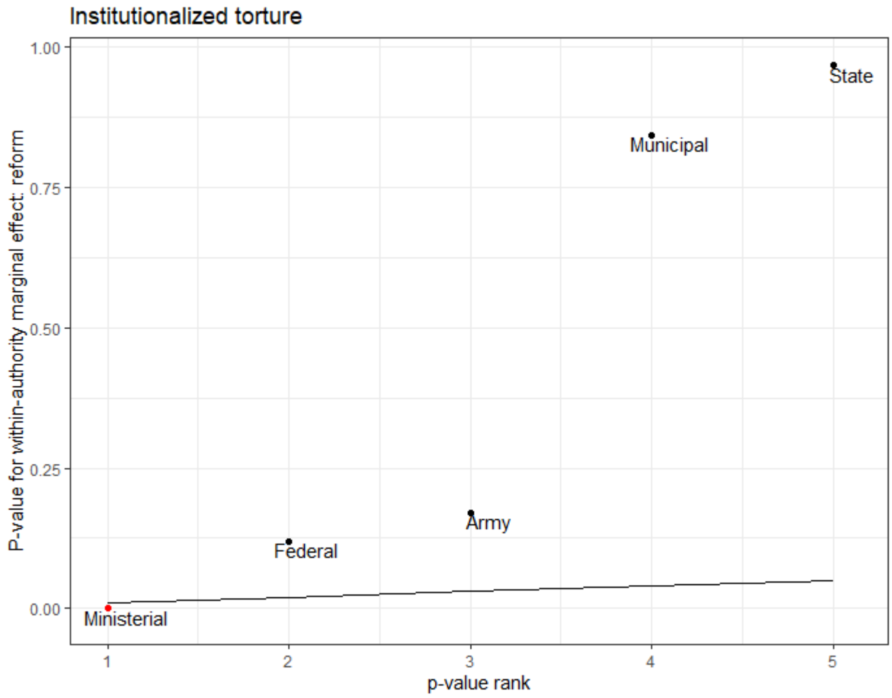


Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of predicted margins of the effect of joint operations interacted with authority on threats. Margins plotted in Figure A10.

7.3 Marginal changes, Reform by Authority

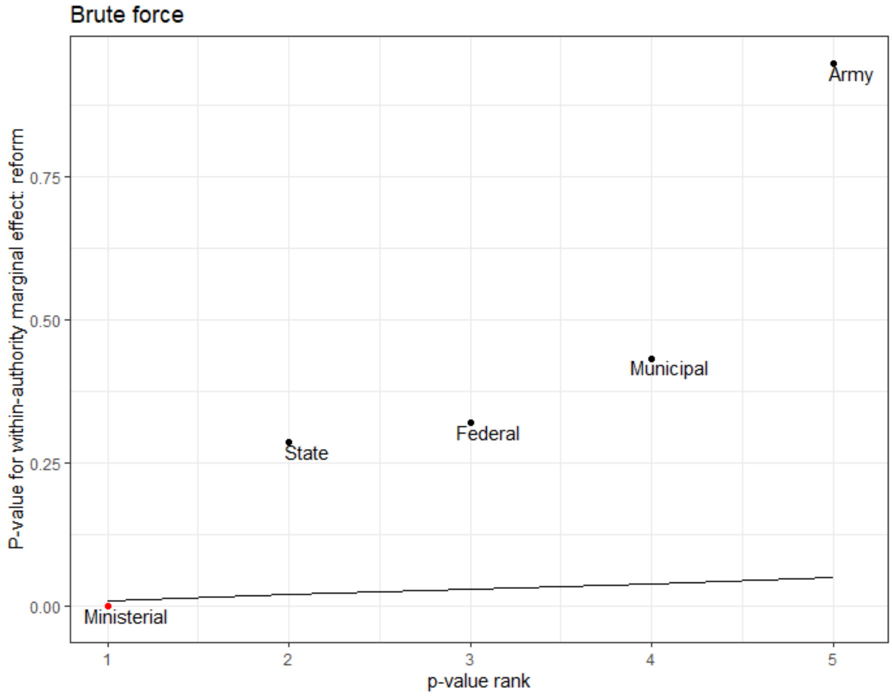
Finally, focusing on marginal changes, we find that, consistently, the only one of these margins that is statistically significant corresponds to the decline in abuse committed by the ministerial police.

Figure A16



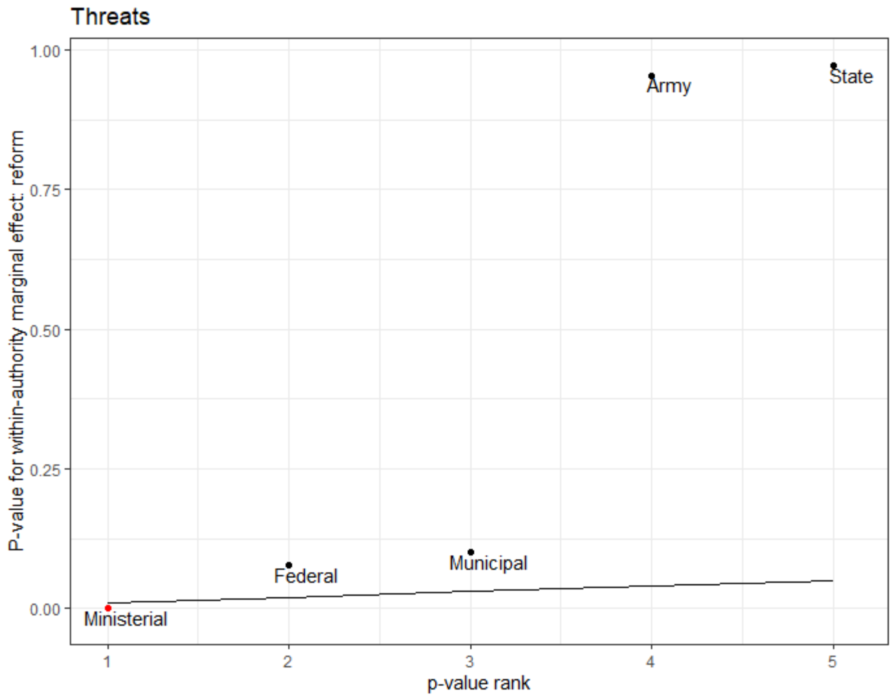
Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of predicted margins of the effect of the reform interacted with authority on institutionalized torture. Margins plotted in Figure A10.

Figure A17



Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of predicted margins of the effect of the reform interacted with authority on brute force. Margins plotted in Figure A10.

Figure A18



Note: Benjamini-Hochberg procedure for controlling the false discovery rate. The solid line presents the cutoff for significance, points refer to p-values of predicted margins of the effect of the reform interacted with authority on threats. Margins plotted in Figure A10.

8 Exploratory Interviews with Police Officers

This section reports on fieldwork conducted with police officers in the largest cities of Mexico: Mexico City, Monterrey and Guadalajara [10] 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 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 produced an important transformation in 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

[10] Monterey 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.

> 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 criminals have learned to work the new system in their favor. The new system is too "garantista" [translates as too protective of human rights.] We advise police officers not talk to them at all, 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).* We have learned not to use words such as "subjugate", "handcuff", "subdue", because 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 easily 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. A police officer in a large municipality voiced the following complain:

> Arresting someone takes between 8 to 36 hours, a colleague recently took 48 hours. You need to stay in the headquarters without going home. And the neighborhood, who takes care of it? We knew how to do our job, how to

> chase criminals, with whom to talk, to get our informants, how to interrogate to obtain the truth. Then we copied whatever they told us Japan, Chile, or Columbia did. The problem is that this is not adapted to the Mexican idiosyncrasies. And now they worry that crime is on the rise?

Many police officers also reported frustration that citizens do not understand the new criminal justice system and "blame the police for the incapacity to arrest criminals because it is the most visible institution." A particularly important challenge for supervisors is how to properly motivate street police officers to invest the extra time it takes to arrest following due process. Police officers explained that the temptation is simply to stop arresting because it takes "too much time to do it properly". In our view, the best municipal police units have instituted salary bonuses and other incentives to compensate the police when they work beyond their normal hours. A police chief told us that he "actually gives police officers a day off when they take long hours to arrest or secure a crime scene." Another police chief told us that they are now starting to "pay police officers bonuses for lowering crime indicators and citizens' complaints in their corresponding city blocks" rather than paying them for the "number of arrests."

In terms of the recently instituted oral trials, police officers also voiced concerns. Some worried about being unprepared to appear in court. Others expressed concerns about becoming vulnerable to retaliation because their identity and personal information becomes public and "all the relatives of the accused are present."

One of the most significant limitations police face is lack of capacity to conduct serious investigations. Our field work revealed that development of investigative policing is in its infancy. It also revealed that the coordination between the preventive police, investigative police, and the MP is deficient. A police officer explained that there is "no coordination with the State's Attorney's office and the MP, who never do their fieldwork. So when it comes to who can offer real criminal investigation, nobody is capable".

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 *auto de formal prisión* (indictment) after the arrest. If you have a confession the judge would for sure give you one.

Our interviews reveal a disturbing path dependency – institutions that began relying on coerced confessions never invested in investigative capacity. Moreover, our interviews also reveal that many corporations traditionally relayed on monetary incentives for arrests and a number of "solved" murders a month. These incentives are "no longer compatible with the realities of the criminal justice reform", as a superior in the Mexico City security apparatus told us. The need to change these incentives, according to him, mainly comes from the fact when you pay police to obtain a number of arrest and "solved" murders a month, you generate incentives that produce violations to the new criminal justice protocols, which is counterproductive since "judges now commonly deny indictments when police don't follow due process." A street police officer told us that "with the new criminal justice system even using "words like "subjugate", "handcuff", "subdue" in the police report are enough to throw out a case, letting "criminals walk free." Another police officer in a municipality in Monterrey told us "with the new criminal justice system we can't interrogate suspects anymore when we arrest them. Howe are we supposed to investigate then?". Another officer told us "if you don't bring a suspect immediately to the MP, and even when you are delayed for traffic, this is enough to throw a case. Judges are now very reluctant granting indictments if we fail to strictly follow the new protocols."

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