Appendix

Contents

A Ethical considerations

Research in violent settings must involve careful consideration of ethical concerns (Wood, 2006; Cronin-Furman & Lake, 2018; Baron & Young, 2021). In this appendix we outline the risks that we considered in the implementation of this project, how we collected information on them, and what steps we took to minimize or avoid them.

A.1 Relevant ethical principles

The first set of ethical principles that guided this study were the Belmont Report's principles of beneficence, justice, and respect for persons. These guided our IRB reviews at Stanford University, American University, and UC Davis. The principle of beneficence implies that researchers should avoid unjustifiable harm and minimize the risk of small, justifiable harms. In addition, while the Belmont Report and IRB review typically only consider harms and benefits to research 'subjects', or survey respondents, many scholars working in sensitive settings have argued that researchers also have an ethical obligation to apply the principle of beneficence to research implementers (Paluck, 2009; Baaz & Utas, 2019).

A.2 Risk assessment

The main risks that we considered in this study were the risk of emotional distress and the risk of retribution due to a breach of confidentiality or for general participation in the research. We were most concerned about both of these risks associated with our questions about past and hypothetical violence exposure.

We assessed the likelihood of these risks through 1) reviewing the literature on measuring violence exposure in Mexico; 2) consulting our survey firm, which has past experience measuring sensitive behaviors and experiences; and 3) piloting our survey using both in-depth cognitive

probing and two medium-N pilots. We began by reviewing the existing literature on measuring violence exposure in Mexico and generally. We found several past studies that had carefully measured violence exposure that did not report harms, which we took as one signal that measuring violence exposure could have a reasonable risk-benefit ratio (Magaloni et al., 2020; O'Connor et al., 2015). This literature also helped us identify practices to reduce the risk of measuring violence exposure, a point that we return to in the following section. Second, we consulted our survey firm, which carries out surveys on sensitive and non-sensitive topics on a regular basis throughout Mexico, to get their input on what kind of questions and topics they would be comfortable asking. They also broadly indicated that asking about violence exposure involved low levels of risk, but that it varied greatly across types of measures and subtopic. Finally, we carried out three pilots: first, a 'cognitive probe' pilot in which we had high-skilled qualitative interviewers go through our survey instrument in great detail with five respondents. During these sessions, the interviewer would pose the survey questions and then probe to understand how the participant understood and reacted to the questions, including whether the questions made them uncomfortable or distressed. After the cognitive probing sessions, we did two medium-N pilots, each with 120 participants in four different localities. Through this iterative process, we became increasingly confident that our research protocol had a ratio of risks and benefits that would not cause more than minimal harm to participants or our research team.

A.3 Processes to avoid or minimize harms

We developed and refined our research methodology with the goal of avoiding serious harms like retribution and minimizing low-level harms like emotional distress. Generally, we addressed both of these risks by 1) establishing surveyor identity, 2) modifying survey measures, 3) reducing the likelihood of breaches of confidentiality, and 4) replacing municipalities that posed higher than

expected risk.

Establishing the identity of our surveyors as employees of a Mexican research firm hired by US-based academics was important in ensuring that both surveyors and interviewees were safe from retribution. A major risk that we were concerned about was that surveyors might be perceived as criminals coming to do surveillance and that either they or the people they interviewed could be targeted with violence as a result. We thus enhanced ByL's standard practices to establish surveyor identity for our study. First, interviewers notified local authorities through the mayor's office before beginning their interviews, and only proceeded if they received permission. Based on our piloting, we also began calling local authorities in advance of the team arriving in a municipality. Second, we provided surveyors with materials that credibly identified them as ByL surveyors such as firm vests, hats, and badges, and paperwork including a letter of introduction and contact cards that described the academic sponsors on official letterhead. Finally, the informed consent process explained that the surveyors were working on an anonymous academic study.

The second way that we were able to minimize harms was by writing the survey questions in ways that were unlikely to cause emotional distress or retribution. Some topics, such as the identity of violent actors, were too sensitive to be asked about at all. Other topics, such as severe individual victimization and the presence of different types of armed actors, were sensitive enough that we determined they could only be asked about indirectly. Finally, questions about some forms of low-level victimization were very unlikely to lead to emotional distress or retribution, and we asked about them directly. All of our questions about violence exposure were based on a version of the Harvard Trauma Questionnaire that was used in a previous study with Mexican participants (O'Connor et al., 2015). The battery of violence questions was short, and did not ask participants to provide details that would require them to reflect on their experiences. ByL did not report any episodes of severe emotional distress or retribution during survey implementation.

Third, we took steps to reduce the risk of a breach of confidentiality. Because the survey was anonymous, we were mainly concerned with a breach of confidentiality at the local level, when the responses collected by a surveyor could be connected to the people she had interviewed that day. To minimize this risk, surveyors used password-protected tablets to record responses, and pushed the survey to a server and deleted it from the tablet as soon as possible, and at latest at the end of each day. They did not collect GPS coordinates for each survey, although this was the survey firm's standard practice. There were no breaches of confidentiality during the survey implementation.

The final way that we avoided harm to surveyors and participants was through the sampling of precincts. Patterns of violence are difficult to observe and can change quickly, meaning that we needed to have a flexible sampling strategy to ensure that the surveyors could avoid precincts where they perceived that the risks were unreasonably high. To this end, we provided the survey team with three different random samples: a first list of sampled precincts and two backup samples. Surveyors were told to attempt to survey in the precincts from the primary sample, but could replace precincts that they deemed to be unsafe with alternatives from the second and third samples without any delays or additional permissions. Ultimately, six out of 120 (5%) precincts from the first sample had to be replaced. All were in high violence areas: three in the city of Zapopan, one in Zacoalco de Torres, and two in the rural areas of Tomatlan and Coalcoman de Vasquez Pallares. They were replaced with five precincts from the second sample.

We monitored the implementation via regular contact during survey implementation with the project manager at the survey firm, as well as via two research assistants not employed by the firm who observed the initial days of implementation. As far as we were able to observe, no one was harmed during the implementation of the survey, including no episodes of intense emotional distress or situations in which surveyors or participants were threatened or harmed because of their participation. There were no breaches of confidentiality. As far as we know, no one reported regret

or harms to the contacts provided to them as part of the consent process, including the local survey firm supervisor, our Spanish-native principal investigator, or the Stanford University IRB.

B Sampling

B.1 Sampling design

Our target population was adults residing in Western Mexico. We took a representative sample from the four states known as Western Mexico, namely Michoacán, Colima, Jalisco, and Nayarit.

Figure B.7: States of Western Mexico

Respondents were randomly selected using a stratified multistage cluster sampling design. Our sampling design takes into account variation vigilante group presence, violence levels, and urbanization. Mexico's electoral precincts were used as the primary sampling units (PSUs). The combination of geospatial and census data at the electoral precinct level provide the most complete and up-to-date sampling frame available in the country. These data come from the National Electoral Institute (INE) and are continuously updated. Approximately 95% of Mexicans 18 years old or older are registered at the INE: as of January 2017, about 84 million voters had been registered. These citizens are dispersed across 68,364 electoral precincts.

Each PSU in the sampling frame was assigned to a non-overlapping sample stratum based on the following variables:

- **Geographical subregion**. Michoacán vs other states.
- **Type of electoral precinct**. Urban vs rural.

- **Presence of vigilante groups**. Known past vigilante presence vs. no known vigilantes.[6]
- **Violence levels**. High, medium, and low homicides per 100,000 people during the past year.

Within each stratum, electoral precincts were selected with probability proportional to the number of registered voters. Figure B.8 shows the sampled electoral precincts in green. Red lines demarcate municipal boundaries, and black lines denote state-level boundaries.

Once electoral precincts in the sample were drawn, we randomly selected blocks (or clusters of homes) within the precinct. These are our second-stage sampling units (SSUs). In urban areas, a block is defined as a geographic space delimited by streets or avenues. In rural precincts, instead of blocks, our SSUs are defined as clusters of homes.

Within each block, households were enumerated by starting at the northeast corner and walking clockwise. Once a questionnaire is completed, the interviewer has to move to the next side of the block. Finally, once a household is selected, the interviewer conducts a short screening interview with an adult to determine if household members meet the study eligibility criteria.

B.2 Sample diagnostics

This strategy produced a sample with characteristics that are very similar to the demographics of Western Mexico. However, due to both design choices and implementation issues, the raw sample deviates slightly from representativeness. The sample deviates from representativeness by design because we purposely took 50% of our respondents from Michoacan, although Michoacan makes up 32% of the population of the region. During the implementation of the survey, slight deviations from representativeness arose because of variation in the availability of respondents to be surveyed. To take both of these into account, in some specifications we include sampling weights based on

[6]There is only one known municipality with vigilante presence outside of Michoacán, so we only stratify on vigilante presence in Michoacán.

1) the inverse propensity that an individual's locality was selected for the study, and 2) age and gender proportions of citizens over 18 by PSU based on the January 2017 INE registry. We use the following formula to calculate a sampling weight for each individual i:

$$w_{ijk} = \frac{1}{Pr(Z_{ik} = 1)} \times \frac{g_{ij}}{\sum_1^n g_j}$$

where w_{ij} is the sampling weight for individual i in PSU j in sampling strata k, $Pr(Z_{ik} = 1)$ is the probability that individual i is selected for the sample based on her residence in sampling strata k, and $\frac{g_i}{\sum_1^n g}$ is the proportion of the adult population in PSU j that individual i's demographic group makes up.

In this section we briefly discuss how the raw data used in most of our analyses compares to the re-weighted, representative dataset. Table B.4 presents the summary statistics for the unweighted sample (columns 2-3) and the weighted sample (columns 4-5). In general, the sample that takes into account the strata propensity weights and PSU demographic weights is slightly younger, slightly more likely to be employed and married, has slightly fewer children, and is slightly less likely to own major assets. Figure B.9 displays the histogram of respondent ages in the weighted (in blue) and unweighted (in red) samples. Most importantly, the weighted sample has a much smaller proportion of Michoacan residents (32% vs. 50%). There are no apparent differences on gender, past exposure to drug-related trauma, or emotional profiles.

Table B.4: Comparison of unweighted and weighted sample summary statistics

	Unweighted		Weighted		
	Mean	Std. Error	Mean	Std. Error	N
Age	43.64	0.48	40.10	0.49	1205
Female	0.56	0.01	0.57	0.01	1205
Married	0.53	0.01	0.49	0.01	1205
Kids	2.54	0.07	2.27	0.07	1205
Employed (Household Head)	0.33	0.01	0.38	0.01	1170
Education	3.30	0.05	3.48	0.05	1199
Home Owner	0.57	0.01	0.55	0.01	1190
Assets: Refrigerator	0.91	0.01	0.92	0.01	1178
Assets: Washing Machine	0.76	0.01	0.77	0.01	1177
Assets: Cellphone	0.80	0.01	0.82	0.01	1176
Assets: Smartphone	0.43	0.01	0.49	0.01	1169
Assets: Computer	0.26	0.01	0.30	0.01	1174
Trauma: Seen armed men	0.29	0.01	0.29	0.01	1199
Trauma: Extortion	0.14	0.01	0.13	0.01	1202
Trauma: Confined to home	0.27	0.01	0.25	0.01	1202
Michoacan	0.50	0.01	0.32	0.01	1205
Colima	0.04	0.01	0.05	0.01	1205
Jalisco	0.40	0.01	0.55	0.01	1205
Nayarit	0.06	0.01	0.08	0.01	1205
Fear	0.74	0.03	0.73	0.03	1195
Nervousness	0.94	0.03	0.89	0.03	1199
Anger	1.04	0.03	1.07	0.03	1198
Indignation	0.76	0.03	0.74	0.03	1194
Happiness	2.29	0.03	2.35	0.02	1191
Cheerfulness	2.16	0.03	2.20	0.03	1190
Sadness	0.88	0.03	0.82	0.03	1196
Dejection	0.79	0.03	0.77	0.03	1190

Figure B.8: Sampled Electoral Precincts

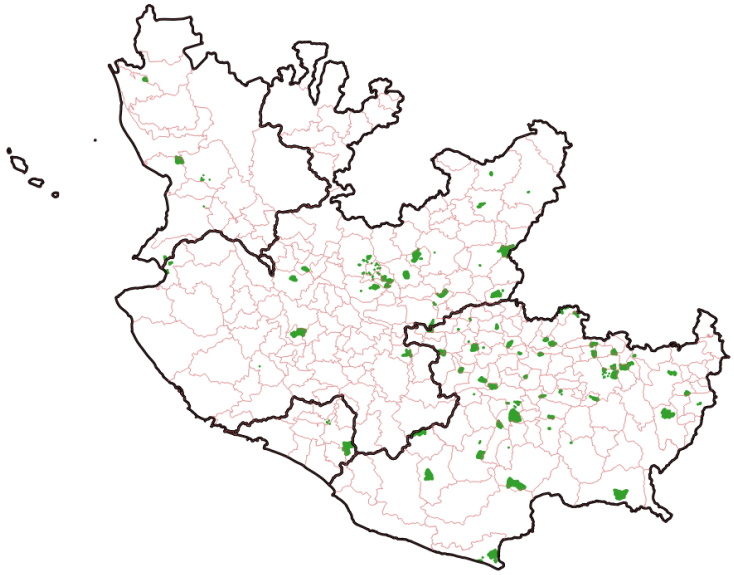
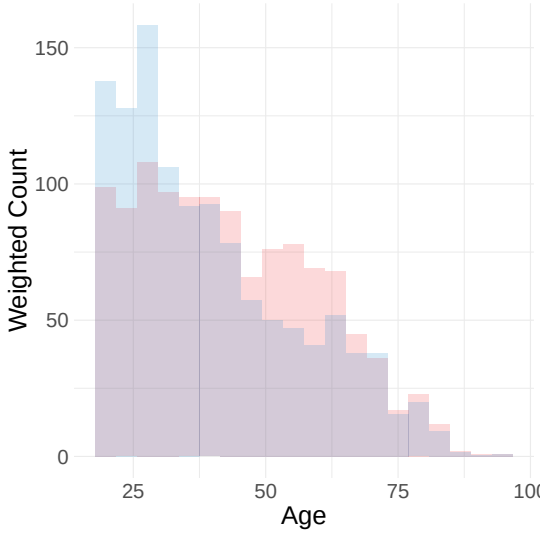


Figure B.9: Comparison of unweighted and weighted age histograms



C Measurement

C.1 Policy preferences

We construct the Policy Attitudes Index using the following five survey questions. Unless otherwise indicated, outcomes were measured on a five-point agreement scale. Individual measures were combined using the mean effects methodology described by ((Kling et al., 2007)). The order of the questions was randomized across respondents.

- *Support Death Penalty:* Some people have suggested that one way to stop the violence is to bring back the death penalty. Do you support or oppose this proposal?
- *Oppose Paying:* Some people have suggested that one way to stop the violence is to pay narcos to stop killing civilians. Do you support or oppose this proposal? (Reverse coded)
- *Support Lynching:* Would you rather see a criminal accused of kidnapping lynched in the town square, or tried in the court of law and go free on a technicality/small point of law? (Binary outcome, lynched = 1)
- *Support Autodefensas:* Some people believe that the autodefensas are necessary to control violence in Mexico. Do you support or oppose this view?
- *Support Armed Groups:* Some people believe that it is necessary to have armed groups outside of the government, because the police and army are not capable of protecting us. Do you agree or disagree with this belief?

The sub-indicators in the index have the following correlations:

The elements of the index are generally weakly positively correlated (ρ between 0.13 and 0.36), except in the case of Oppose Paying. The weak correlation of Oppose Paying may be because this variable is the only reverse-coded measure.

Table C.1: Policy Attitudes Index

	Support Death Penalty	Oppose Paying	Support Armed Groups	Support Autodefensas	Support Lynching
Support Death Penalty	1.00	-0.10	0.28	0.14	0.32
Oppose Paying	-0.10	1.00	-0.24	-0.20	-0.02
Support Armed Groups	0.28	-0.24	1.00	0.36	0.23
Support Autodefensas	0.14	-0.20	0.36	1.00	0.13
Support Lynching	0.32	-0.02	0.23	0.13	1.00

C.2 Psychological outcomes

In Section , we also test for correlations between exposure to violence and a series of psychological outcomes. These include the emotions of anger, fear, sadness, and happiness, as well as attributions of blame and general punitiveness. In this section we describe the measures for each of these outcomes.

First, we measure respondents' propensities to feel four different emotions: anger, fear, sadness, and happiness. Each of these measures represents a standardized mean effects index of two questions measured on a standardized four-point scale in response to questions asking how often the respondent felt the emotions during the past 30 days.

The specific words used in each index were:

- Anger: angry, indignant
- Fear: afraid, nervous
- Sadness: sad, dejected
- Happiness: happy, cheerful

We measure two psychological variables that may mediate the relationship between anger and policy preferences: attributions of blame and general punitiveness. Both are also standardized mean effects indices based on the extent to which the respondent thinks that six different groups (narcotraffickers, politicians, federal police, local police, the army, and the autodefensas) are to

blame for the violence affecting their municipality (Attributions of Blame), or should be punished for the violence affecting their municipality (General Punitiveness).

C.3 Controls

Some specifications also include the following set of control variables:

- Female: a gender dummy.
- Education: a standardized measure of education on a 9-point scale.
- Assets Index: a standardized index based on the first principal component of measures of asset ownership.
- Age: a standardized measure of years of age.
- Married: a dummy variable indicating whether the respondent is married.
- Employment: a dummy variable indicating whether the household head is employed.

D Validation of violence exposure measures

As discussed in Section , we measure exposure to severe violence at the individual level by asking respondents to assess how likely it is that someone in their locality had experienced five different types of violence: abduction, extortion, paying for protection, being threatened with a weapon, and assault. These five types of violence were the most extreme forms of victimization that we asked respondents about; the full list of forms of violence that we asked about was adapted from a recent application of the Harvard Trauma Questionnaire to study drug-war-affected populations in Mexico (O'Connor et al., 2015).

We refer to these as our indirect measures of violence because we are using respondents' assessments of their neighbors' experiences to proxy for their own personal exposure to violence. To validate these indirect measures, we compare estimates of incidence of exposure to less severe (and therefore less sensitive) forms of violence based on the indirect questions to direct questions asking about whether the respondent herself was ever exposed.[7] If our indirect measures are in fact picking up variation in the respondent's own experience, the direct and indirect questions should be strongly related. The forms of violence for which we have both direct and indirect measures are: extortion, seeing a narco-banner, and seeing a narco-blockade.

Table D.1 presents the results of our validation exercise. We find that the direct questions are strongly predictive of the indirect questions. The direct and indirect questions are correlated, with ρ between 0.22 and 0.4. The correlation is large in magnitude and statistically significant at the 1% level both with and without PSU fixed effects. People who are personally exposed to each of the three types of violence are between 0.56 and 1.23 standard deviations higher on the indirect exposure scales. The fact that even conditional on the neighborhood that someone is in (i.e.,

[7] For the direct questions, we phrased the question as follows: 'For the next list of experiences, I'd like you to think back to your own experiences in the context of the war against drug trafficking. For each item, I'd like you to tell me if you have ever personally experienced it in a way that was related to drug trafficking or the drug war.'

their PSU) there is a strong relationship between the direct and indirect measures suggests that the indirect measures are picking up a large component of individual experience.

Table D.1: Validation of indirect violence exposure using direct questions

	Dependent variable:								
	Extortion (Indirect)			Blockade (Indirect)			Banner (Indirect)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Extortion (Direct)	0.66**	0.61**	0.58**						
	(0.09)	(0.09)	(0.09)						
Blockade (Direct)				1.17**	1.14**	1.05**			
				(0.09)	(0.09)	(0.10)			
Banner (Direct)							1.12**	1.12**	1.01**
							(0.09)	(0.09)	(0.09)
Individual Controls		✓	✓		✓	✓	✓	✓	
PSU FE			✓			✓			✓
Number of PSUs			119			119			119
Observations	1,116	1,086	1,086	1,113	1,082	1,082	1,117	1,084	1,084
R^2	0.05	0.12	0.27	0.15	0.18	0.31	0.12	0.19	0.33

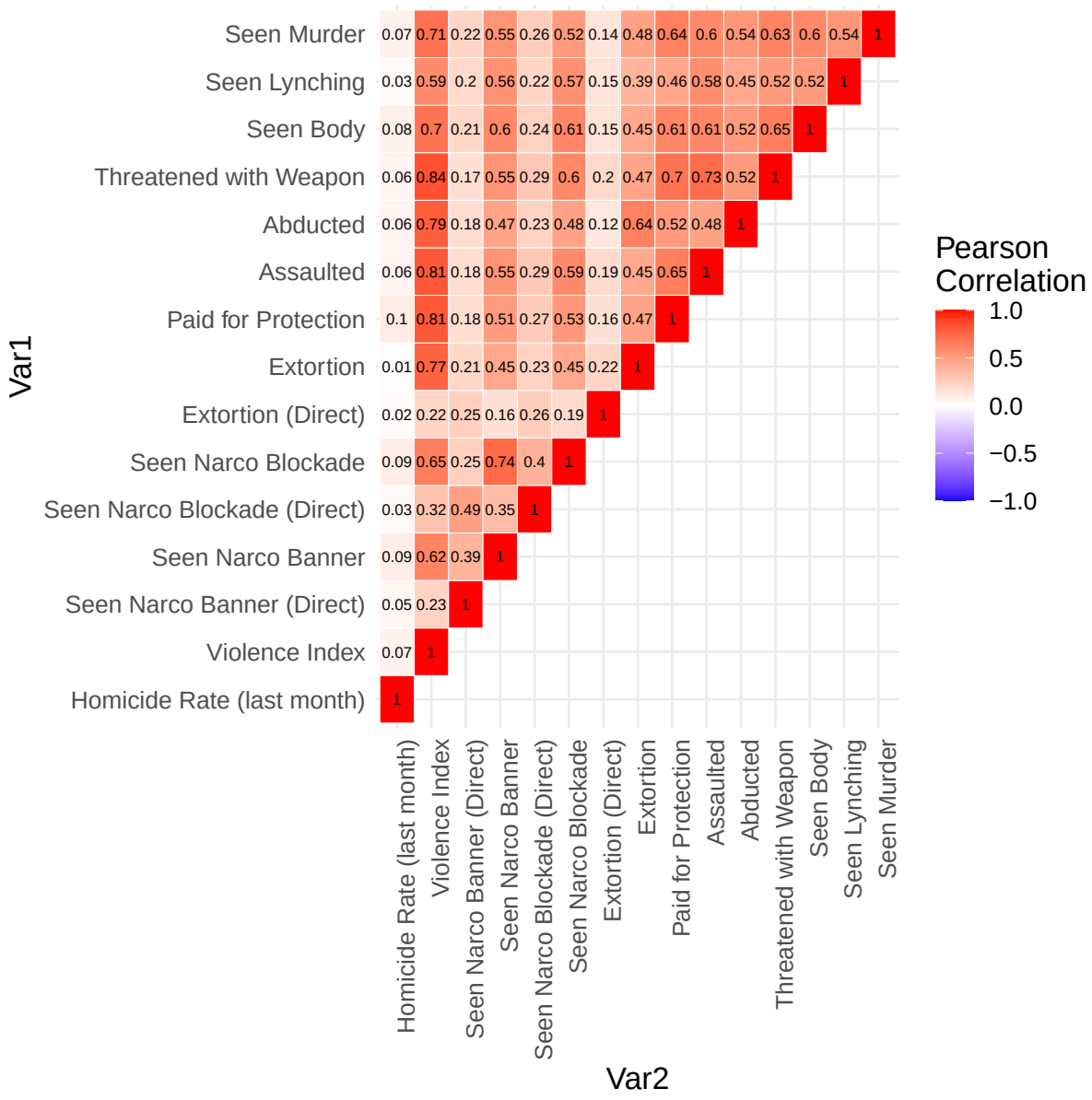
†p<0.1; *p<0.05; **p<0.01
Standard errors in parentheses.
Coefficients estimated using OLS. Columns 1, 4, and 7 present bivariate relationships between the direct and indirect questions. Columns 2-3, 5-6, and 8-9 include individual level controls for gender, age, an assets index, marital status, household head employment status, and the surveyor fixed effects. Columns 3, 6, and 9 include PSU (neighborhood) fixed effects.

We can also compare our measures of violence exposure to other existing measures. We use publicly available local data on violence in Mexico are homicide rates by municipality provided by the Mexican National Public Security System as our administrative measure of violence.[8] Figure D.1 presents the correlations between the homicide data and our survey-based measures of different types of violence. The data for this analysis is aggregated up to the municipality level because this is the level at which the homicide data are available.

Figure D.1 first shows that most of our measures of violence exposure are generally only weakly positively correlated with the homicide rates in the year prior to our survey. Our main

[8]Several other datasets are available but are not used here for various reasons. There is data produced by the state security agencies for 2006-2011 on drug-related homicides, but this data does not cover our time period and varies significantly over time (see, for example, Calderón et al. (2015)). There is also data available from the state on reported crimes, but this data is widely considered unreliable due to extremely low rates of crime reporting in Mexico. Finally, there is survey based data on victimization from the ENVIPE survey by the Mexican statistical institute INEGI, but this is only released at the state level.

Figure D.1: Correlations between measures of violence



analyses are based on the Violence Index, a standardized index of the indirect measures of five different types of violence (abduction, extortion, paying for protection, being threatened with a weapon, and assault). Figure D.1 shows that the correlation between homicides and the Violence Index is 0.07, and the correlations with each of the five components are between 0.01 and 0.1. We also asked indirectly about witnessing five other types of violence and DTO activity (seen a body,

seen a narco banner, seen a narco blockade, seen a murder, or seen a lynching). These measures of witnessed crimes are more strongly correlated with homicides.

One explanation for this pattern is that our survey questions and homicide data are simply picking up different types of violence that affect different populations. Many of the homicide victims are affiliates of drug trafficking organizations rather than civilians.[9] This explanation is also supported by the fact that our measures of witnessing the activities of drug trafficking organizations (seeing a narco banner and seeing a narco blockade) are more strongly correlated with homicides than our measures of lower level violence that targets civilians like extortion, paying for protection, and assault.

[9] According to the NGO Semáforo Delictivo, approximately 75% of intentional homicides that were committed in Mexico in 2017 were organized crime executions. See (https://www.huffingtonpost.com.mx/2018/01/24/75-de-los-asesinatos-en-mexico-en-2017-fueron-ejecuciones-del-crimen-organizado_a_23342429/).

E Study 1: Additional analyses

E.1 Violence and psychological outcomes

In this section we look empirically at whether past exposure to violence is also associated with variation in other emotions, in addition to higher levels of anger. Tables E.1, E.2, and E.3 show that there is some evidence that fear and sadness are also positively associated with more exposure to violence. In terms of their magnitudes, the size of the association between past exposure to violence and other emotions ranges from about 5% (happiness, in the opposite direction) to about 82% (fear) of the size of anger's association. These results suggest that past exposure to violence is associated with a bundle of negative emotions, of which anger is one of the stronger components.

Table E.1: Exposure to violence is associated with more fear

	Dependent variable:						
	Fear						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.09**	0.10**	0.11**	0.10**	0.09**		
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)		
Homicide Rate						−0.01	
						(0.02)	
Extortion - Direct							0.20**
							(0.07)
Proximity to Security Base				−0.03*			
				(0.01)			
Prox. to Security Base				0.03*			
× Violence Index				(0.01)			
Presence of State Security					0.09**		
					(0.02)		
Presence of State Security					0.03		
× Violence Index					(0.02)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	−0.03	0.04	0.69**	0.04	0.79**	0.07	0.77**
	(0.03)	(0.14)	(0.16)	(0.14)	(0.16)	(0.15)	(0.16)
Observations	1,147	1,115	1,115	1,115	1,093	1,132	1,129
R^2	0.01	0.04	0.15	0.04	0.16	0.02	0.14

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Table E.2: Exposure to violence is associated with more sadness

	Dependent variable:						
	Sadness						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.06†	0.08*	0.10**	0.08*	0.08*		
	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)		
Homicide Rate						−0.02	
						(0.02)	
Extortion - Direct							0.31**
							(0.09)
Proximity to Security Base				−0.03*			
				(0.02)			
Prox. to Security Base × Violence Index				0.01			
				(0.02)			
Presence of State Security					0.06†		
					(0.03)		
Presence of State Security × Violence Index					0.04		
					(0.04)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	−0.04	0.12	0.58**	0.13	0.66**	0.16	0.65**
	(0.03)	(0.10)	(0.12)	(0.10)	(0.13)	(0.12)	(0.13)
Observations	1,146	1,114	1,114	1,114	1,092	1,131	1,128
R^2	0.005	0.04	0.15	0.04	0.16	0.03	0.15

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Table E.3: Exposure to violence is associated with no changes in happiness

	Dependent variable:						
	Happiness						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	−0.002	−0.03	−0.02	−0.03	−0.01		
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)		
Homicide Rate						0.01	
						(0.02)	
Extortion - Direct							−0.03
							(0.11)
Proximity to Security Base				0.04**			
				(0.01)			
Prox. to Security Base				−0.02			
× Violence Index				(0.02)			
Presence of State Security					0.04		
					(0.04)		
Presence of State Security					−0.05†		
× Violence Index					(0.03)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.07*	0.08	−0.31*	0.08	−0.31*	0.06	−0.31*
	(0.03)	(0.13)	(0.13)	(0.13)	(0.15)	(0.13)	(0.13)
Observations	1,146	1,114	1,114	1,114	1,092	1,131	1,128
R^2	0.0000	0.06	0.17	0.07	0.18	0.06	0.17

*p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

We also measured perceived blame for the violence associated with the drug war and punitiveness towards six different actors. These are attributions or general preferences that could be part of the causal channel from anger to policy preferences.

Table E.4: Exposure to violence is associated with more blame attributions

	Dependent variable:						
	Blame						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.10**	0.09**	0.10**	0.09**	0.10**		
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)		
Homicide Rate						0.01	
						(0.02)	
Extortion - Direct							0.15**
							(0.05)
Proximity to Security Base				0.01			
				(0.02)			
Prox. to Security Base				0.004			
× Violence Index				(0.02)			
Presence of State Security					0.04*		
					(0.02)		
Presence of State Security					−0.001		
× Violence Index					(0.02)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.02	0.19†	0.26**	0.19†	0.29**	0.18†	0.34**
	(0.03)	(0.10)	(0.09)	(0.10)	(0.09)	(0.10)	(0.08)
Observations	1,141	1,112	1,112	1,112	1,093	1,128	1,125
R^2	0.02	0.05	0.21	0.05	0.21	0.03	0.18

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

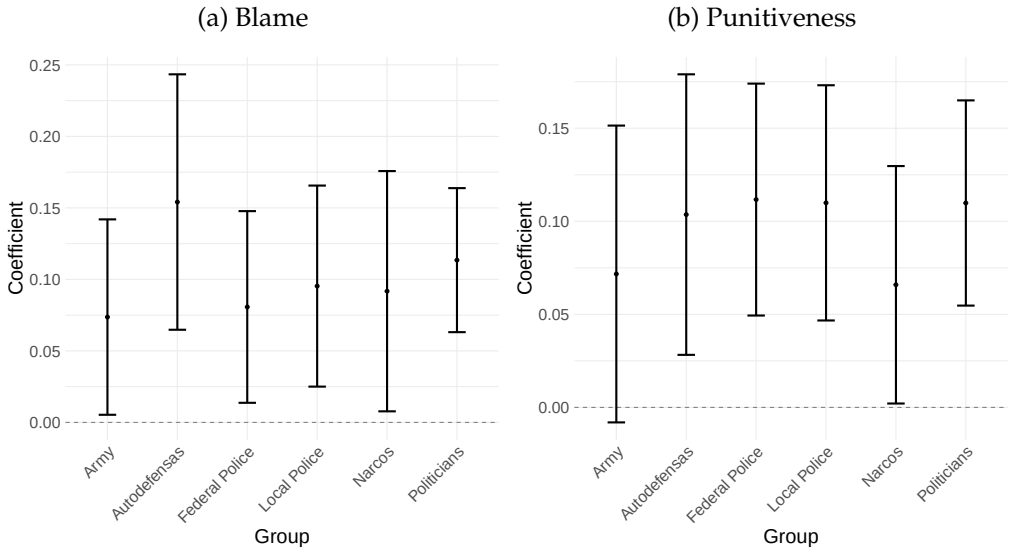
Table E.5: Exposure to violence is associated with more punitiveness

	Dependent variable:						
	Punitiveness						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.10**	0.09**	0.09**	0.09**	0.10**		
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)		
Homicide Rate						0.01	
						(0.02)	
Extortion - Direct							0.21**
							(0.06)
Proximity to Security Base				−0.003			
				(0.02)			
Prox. to Security Base				0.01			
× Violence Index				(0.01)			
Presence of State Security					0.02		
					(0.02)		
Presence of State Security					−0.03		
× Violence Index					(0.03)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.03	0.15†	0.47**	0.15†	0.48**	0.13	0.53**
	(0.03)	(0.08)	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)
Observations	1,140	1,110	1,110	1,110	1,089	1,126	1,123
R^2	0.02	0.07	0.22	0.07	0.23	0.05	0.22

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Figure E.1 plot the coefficients from analyses that disaggregate the blame and punitiveness indices from Tables E.4 and E.5. The results of exposure to violence are substantively similar across all six actors for both blame and punitiveness.

Figure E.1: Standardized coefficients from disaggregated analyses of violence, blame and punitiveness



E.2 Violence and policy preferences

Tables E.6 and E.7 tests whether the analysis presented in Table II is consistent across the questions that primarily measure a preference for harsh punishments. There is a substantively large and highly statistically significant relationship between past violence exposure and higher support fro the death penalty. The coefficient on the question asking whether respondents oppose paying narcos to stop committing crimes is in the same direction, although it is very close to zero.

Table E.6: Exposure to violence is associated with higher support for punitive and pro-vigilante criminal justice policy preferences: Support death penalty disaggregated

	Dependent variable:						
	Support Death Penalty						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.14**	0.13**	0.13**	0.13**	0.14**		
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)		
Homicide Rate						0.02	
						(0.03)	
Extortion - Direct							0.06*
							(0.03)
Proximity to Security Base				−0.02			
				(0.02)			
Prox. to Security Base				−0.01			
× Violence Index				(0.03)			
Presence of State Security					−0.02		
					(0.04)		
Presence of State Security					−0.01		
× Violence Index					(0.03)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.05	0.27	0.29	0.27	0.25	0.21	0.38†
	(0.04)	(0.20)	(0.22)	(0.20)	(0.23)	(0.21)	(0.22)
Observations	1,137	1,106	1,106	1,106	1,084	1,119	1,116
R^2	0.02	0.05	0.18	0.05	0.18	0.04	0.17

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Tables E.8, E.9, and E.10 tests whether the analysis presented in Table II is consistent across the three questions that primarily measure permissive attitudes towards vigilantes. There are significant positive associations between past exposure to violence and two outcomes: support for armed groups outside of the state to fight narcotraffickers and support for lynching. Interestingly,

Table E.7: Exposure to violence is associated with higher support for punitive and pro-vigilante criminal justice policy preferences: Pay narcos to stop violence (reversed) disaggregated

	Dependent variable:						
	Pay Narcos (Reversed)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.01	0.003	0.02	0.003	−0.002		
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)		
Homicide Rate						0.01	
						(0.03)	
Extortion - Direct							0.05†
							(0.03)
Proximity to Security Base				−0.07**			
				(0.02)			
Prox. to Security Base				0.01			
× Violence Index				(0.02)			
Presence of State Security					0.06†		
					(0.04)		
Presence of State Security					0.03		
× Violence Index					(0.03)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.003	−0.36*	−0.37	−0.35*	−0.30	−0.39*	−0.34
	(0.04)	(0.17)	(0.22)	(0.17)	(0.23)	(0.17)	(0.22)
Observations	1,121	1,091	1,091	1,091	1,071	1,104	1,101
R^2	0.0001	0.02	0.13	0.03	0.14	0.02	0.13

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

the correlations between the direct measure of exposure to extortion and these individual policy preferences is positive and statistically significant in four out of five cases. The effect on the question asking about support for the autodefensas, however, is a tight null effect. One reason that the autodefensas themselves may have no more support for people who have experienced violence (despite their greater support for vigilante justice generally) may be that the autodefensas have begun to be seen as a criminal rather than vigilante group in some areas.

Table E.8: Exposure to violence is associated with higher support for punitive and pro-vigilante criminal justice policy preferences: Support armed groups disaggregated

	Dependent variable:						
	Support Armed Groups						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.06	0.05	0.01	0.05	0.03		
	(0.05)	(0.04)	(0.05)	(0.04)	(0.05)		
Homicide Rate						0.02	
						(0.02)	
Extortion - Direct							0.11**
							(0.04)
Female		−0.07	−0.07	−0.06	−0.06	−0.07	−0.06
		(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Education		−0.02	−0.03	−0.02	−0.03	−0.02	−0.03
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Assets Index		0.003	0.02	0.004	0.02	−0.0005	0.01
		(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
Age		−0.004†	−0.01*	−0.004†	−0.01**	−0.005*	−0.01**
		(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Married		−0.07	−0.05	−0.07	−0.06	−0.07	−0.04
		(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)
Employed		−0.01	−0.0002	−0.01	0.004	−0.01	−0.01
		(0.05)	(0.07)	(0.05)	(0.07)	(0.05)	(0.06)
Proximity to Security Base				0.05**			
				(0.02)			
Prox. to Security Base				0.06*			
× Violence Index				(0.02)			
Presence of State Security					−0.03		
					(0.04)		
Presence of State Security					−0.02		
× Violence Index					(0.04)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.04	0.36**	0.18	0.35*	0.16	0.33*	0.24†
	(0.04)	(0.14)	(0.14)	(0.14)	(0.15)	(0.13)	(0.14)
Observations	1,130	1,098	1,098	1,098	1,078	1,113	1,110
R^2	0.003	0.01	0.15	0.01	0.15	0.01	0.16

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Table E.9: Exposure to violence is associated with higher support for punitive and pro-vigilante criminal justice policy preferences: Autodefensas necessary disaggregated

	Dependent variable:						
	Autodefensas necessary						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.0002	−0.003	−0.05	−0.01	−0.04		
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)		
Homicide Rate						−0.07*	
						(0.03)	
Extortion - Direct							0.05
							(0.03)
Proximity to Security Base				0.07			
				(0.05)			
Prox. to Security Base				0.005			
× Violence Index				(0.04)			
Presence of State Security					0.01		
					(0.04)		
Presence of State Security					−0.04		
× Violence Index					(0.04)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.05	0.32*	0.39*	0.32*	0.36*	0.49**	0.38*
	(0.04)	(0.14)	(0.16)	(0.14)	(0.17)	(0.16)	(0.16)
Observations	1,123	1,092	1,092	1,092	1,071	1,104	1,101
R^2	0.0000	0.01	0.18	0.02	0.19	0.02	0.18

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Table E.10: Exposure to violence is associated with higher support for punitive and pro-vigilante criminal justice policy preferences: Lynched vs. released on a technicality disaggregated

	Dependent variable:						
	Lynched vs. released on a technicality						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.15**	0.15**	0.13**	0.15**	0.14**		
	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)		
Homicide Rate						0.07*	
						(0.03)	
Extortion - Direct							0.09*
							(0.03)
Proximity to Security Base				−0.0004			
				(0.05)			
Prox. to Security Base				0.02			
× Violence Index				(0.04)			
Presence of State Security					−0.004		
					(0.05)		
Presence of State Security					0.03		
× Violence Index					(0.03)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.01	0.14	−0.11	0.14	−0.11	−0.005	0.01
	(0.04)	(0.18)	(0.16)	(0.18)	(0.15)	(0.18)	(0.17)
Observations	1,113	1,083	1,083	1,083	1,062	1,097	1,094
R^2	0.02	0.04	0.19	0.04	0.20	0.02	0.18

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

E.3 Do attitudes towards the state moderate the effects of exposure to violence?

In Columns 4 and 5 of Tables I and II we show that the relationship between exposure to violence and our outcomes of interest (support for punitive policies and anger) are not moderated by two measures of local state security capacity. We expected that if support for harsh justice is driven by a logic of strategic deterrence, then we should see a stronger relationship between exposure to violence and support for punitive policies in places where the state is weaker. A related test of the strategic deterrence hypothesis would assess whether the relationship between exposure to violence and support for punitive policies is stronger in places where the state's will to carry out security and justice is weaker – in other words, where the state is more corrupt. Unfortunately, there is no local-level observational measure of corruption in the state security sector that we could use to run this test in a credible way. In addition, our survey-based measures of citizen perceptions of the state security sector are almost certainly affected by exposure to violence, which would introduce post-treatment bias into specifications using our survey measures as moderators. Nevertheless, we present those tests here with the strong caveat that they should be interpreted with the potential for post-treatment bias in mind.

We measured attitudes towards the state at two points in our survey. First, we asked respondents to report on a four-point scale to what extent they trust politicians and the courts. Second, we asked respondents to rate their perceptions of the army and police in terms of whether they 1) act according to the law or legally, 2) effectively reduce violence and crime, and 3) act according to the will of the people, also on a four-point scale. From these we generate two index measures: an average level of trust in the courts and politicians, and an average level of views of the legitimacy of the police and army. Neither of these are perfect proxies for perceptions of general corruption in state security and justice institutions, but it is clear that trust and perceptions of their legality, effectiveness, and responsiveness should be lower in places where people perceive that the state is

corrupt or lacks the will to protect citizens from crime. Table E.11 recreates the results from Table I replacing our measures of state capacity with these measures of perceived state will, and Table E.12 recreates the results from Table II. Both show that these measures of perceptions of the state do not moderate the relationship between exposure to violence and our outcomes of interest.

Table E.11: Exposure to violence is associated with more anger

	Dependent variable:						
	Anger						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.14**	0.14**	0.17**	0.14**	0.16**		
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)		
Homicide Rate						−0.01	
						(0.02)	
Extortion - Direct							0.27**
							(0.09)
Trust State Index				−0.01			
				(0.03)			
Trust State Index				0.03			
× Violence Index				(0.03)			
State Legitimacy Index					0.05		
					(0.03)		
State Legitimacy Index					0.02		
× Violence Index					(0.05)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.01	0.16	0.32*	0.16	0.35**	0.17	0.44**
	(0.03)	(0.11)	(0.13)	(0.11)	(0.13)	(0.12)	(0.13)
Observations	1,147	1,115	1,115	1,114	1,110	1,132	1,129
R^2	0.03	0.04	0.14	0.04	0.14	0.01	0.12

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

Table E.12: Exposure to violence is associated with higher support for punitive and pro-vigilante criminal justice policy preferences

	Dependent variable:						
	Policy Attitudes Index						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Violence Index	0.07**	0.06**	0.05*	0.06**	0.05*		
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)		
Homicide Rate						0.01	
						(0.01)	
Extortion - Direct							0.21**
							(0.04)
Trust State Index				−0.06*			
				(0.03)			
Trust State Index				0.01			
× Violence Index				(0.02)			
State Legitimacy Index					−0.002		
					(0.03)		
State Legitimacy Index					0.02		
× Violence Index					(0.03)		
Individual Controls		✓	✓	✓	✓	✓	✓
PSU FEs			✓		✓		✓
Constant	0.03	0.14†	0.07	0.16*	0.07	0.12	0.10
	(0.03)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)
Observations	1,149	1,117	1,117	1,116	1,112	1,133	1,130
R^2	0.02	0.04	0.20	0.05	0.20	0.03	0.20

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by municipality in parentheses.
Coefficients are estimated using OLS. Observations are weighted by the inverse propensity that a respondent is selected for the sample and the proportion of the PSU population that her age and gender cohort makes up.

F Study 2: Additional analysis

F.1 Treatments

The sections of the scenarios that are randomized are italicized, and the 'moral outrage' version of the scenario is bolded.

Table F.1: Crime scenarios and punishment options in Study 2

	Scenario 1	**Scenario 2**	**Scenario 3**
Scenario	Imagine a situation in which a narco gang controls the town. They control the drug trade, and they also are notorious for abusing and exploiting *the local population* / ***children under the age of 10***.	Imagine a situation in which a corrupt politician is in charge of a large city. He does political favors for his friends and powerful people, and steals money from *government contracts* / ***a hospital for disabled children***.	Imagine that a narco abducts a small business owner because he won't pay them part of his profits. A week later, the business owner's body is found outside town, and he has been *shot to death* / ***beheaded and his body shows signs of torture***.
Outcomes	**A:** The narco gang members are arrested and put on trial for their crimes.	**A:** The politician is arrested and put on trial for corruption.	**A:** The narco is arrested and put on trial.
	B: The narco gang members are killed by locals in the town square.	**B:** Local citizens attack the mayor and burn his house down.	**B:** The narco is killed by autodefensas.

F.2 Measurement

As per our pre-analysis plan, we use the following dependent variables to test our predictions:

- Anger (Manipulation Check) - a standardized measure of how angry the respondent reports she would be if the scenario happened in her municipality on a four-point scale.
- Fear (Alternative) - we also test whether the outrage version of the scenario affects how afraid the respondent says she would be.
- Harsh Vigilante Preferred (Predictions 3A and 3B) - whether the respondent prefers the harsh, vigilante option that we give them over a legal, more lenient punishment for the crime. We code this as a binary outcome that takes a value of 1 if the respondent chooses the harsh vigilante solution.
- Harsh Vigilante More Just (Mechanism) - part of the mechanism linking outrage to a preference for harsh vigilante punishments could be an increase in perceptions that harsh vigilante solutions are more just. We test this with a dummy variable indicating whether the respondent believes that the vigilante solution is more just.
- Harsh Vigilante More Effective (Mechanism) - similarly, part of the mechanism linking outrage to a preference for harsh vigilante punishments could be an increase in perceptions that harsh vigilante solutions are more effective in preventing future violence. We test this with a dummy variable indicating whether the respondent believes that the vigilante solution is more effective.

F.3 Balance tests

Table F.2 presents tests of balance for all three of the moral outrage scenarios. Three out of the 72 variables show statistically significant imbalance, which is more or less what we would expect from random chance. There is no evidence that the randomization of the moral outrage scenarios was improperly implemented.

Table F.2: Balance tests for three moral outrage scenarios

	Scenario 1			Scenario 2			Scenario 3			
	Treat	Control	*p*-value	Treat	Control	*p*-value	Treat	Control	*p*-value	*N*
Age	44.68	42.29	0.01	43.65	43.39	0.79	43.29	43.75	0.64	1169
Female	0.54	0.58	0.15	0.55	0.58	0.33	0.57	0.55	0.60	1169
Married	0.55	0.51	0.15	0.54	0.52	0.55	0.57	0.48	0.00	1169
Children	2.64	2.43	0.16	2.50	2.58	0.55	2.59	2.49	0.49	1169
Employed (HH Head)	0.30	0.35	0.09	0.34	0.30	0.15	0.32	0.33	0.87	1135
Education	3.27	3.27	0.99	3.35	3.19	0.16	3.27	3.28	0.92	1163
Home Owner	0.56	0.57	0.80	0.55	0.59	0.16	0.58	0.56	0.46	1155
Assets: Refrigerator	0.89	0.93	0.02	0.90	0.92	0.42	0.91	0.91	0.68	1144
Assets: Washing Machine	0.76	0.76	0.83	0.76	0.76	0.83	0.77	0.75	0.44	1143
Assets: Cellphone	0.80	0.81	0.63	0.81	0.80	0.54	0.81	0.80	0.86	1142
Assets: Smartphone	0.42	0.44	0.58	0.44	0.42	0.48	0.43	0.43	0.82	1135
Assets: Computer	0.25	0.26	0.58	0.26	0.25	0.62	0.26	0.25	0.78	1140
Michoacan	0.52	0.51	0.76	0.49	0.53	0.13	0.52	0.50	0.49	1169
Colima	0.03	0.05	0.22	0.04	0.04	0.73	0.03	0.05	0.24	1169
Jalisco	0.39	0.38	0.70	0.40	0.36	0.13	0.37	0.39	0.46	1169
Nayarit	0.06	0.07	0.71	0.06	0.07	0.81	0.07	0.06	0.32	1169
Fear	0.70	0.78	0.19	0.72	0.75	0.57	0.71	0.76	0.50	1160
Nervousness	0.96	0.92	0.55	0.92	0.96	0.46	0.95	0.92	0.65	1163
Anger	1.06	1.04	0.72	1.04	1.06	0.80	1.04	1.06	0.75	1162
Indignation	0.77	0.74	0.63	0.76	0.75	0.91	0.77	0.75	0.74	1158
Happiness	2.32	2.28	0.45	2.30	2.30	0.91	2.32	2.28	0.55	1155
Cheerfulness	2.17	2.15	0.74	2.21	2.12	0.10	2.17	2.16	0.82	1155
Sadness	0.87	0.89	0.67	0.83	0.93	0.07	0.87	0.90	0.62	1161
Dejection	0.84	0.76	0.19	0.76	0.84	0.18	0.77	0.83	0.38	1154

F.4 Results disaggregated by scenario

Figure F.1: Effect of disaggregated outrage scenarios on preferences and attitudes towards vigilantism

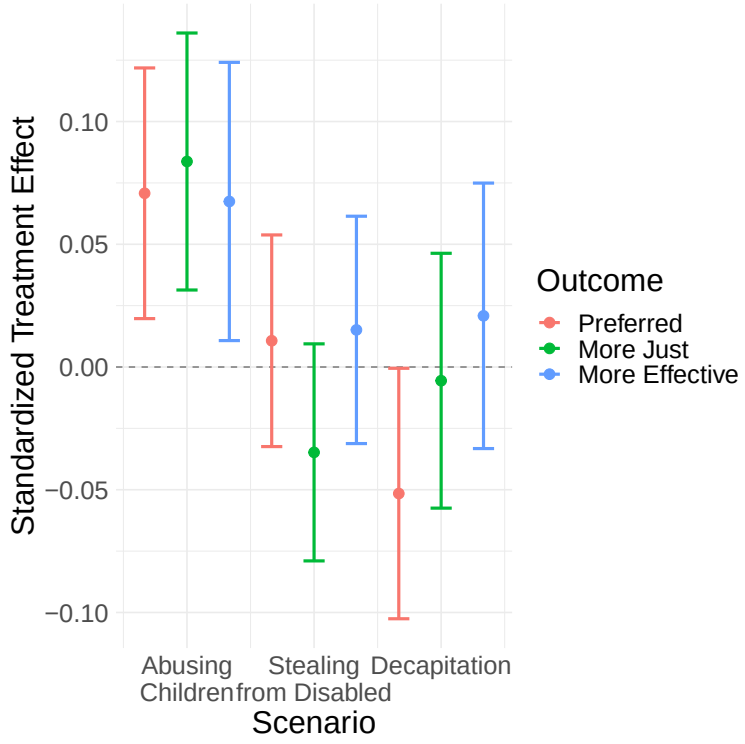


Table F.3: Logistic regression estimates: The outrage scenarios increase the likelihood that the vigilante solution is preferred and perceived as more effective

	Dependent variable:					
	Vigilante Preferred		Vigilante More Just		Vigilante More Effective	
	(1)	(2)	(3)	(4)	(5)	(6)
Outrage Treatment	0.33*	0.33*	0.16	0.18	0.26*	0.26*
	(0.13)	(0.14)	(0.12)	(0.13)	(0.12)	(0.12)
Individual Controls		✓		✓		✓
PSU FEs		✓		✓		✓
Constant	−1.93**	−0.77*	−1.73**	−0.80*	−1.56**	−1.14**
	(0.11)	(0.38)	(0.10)	(0.34)	(0.09)	(0.32)
Observations	2,338	2,234	2,338	2,234	2,338	2,234
Log Likelihood	−978.81	−917.78	−1,048.82	−978.22	−1,156.85	−1,085.53
Akaike Inf. Crit.	1,961.62	1,853.55	2,101.64	1,974.43	2,317.70	2,189.06

†p<0.1; *p<0.05; **p<0.01
Standard errors clustered by respondent in parentheses.
Coefficients are estimated using logistic regression. Individual Controls include gender, education, age, an assets index, marital status, and employment status of the household head.

Table F.4: Effect of outrage scenarios on preferences over and perceptions of harsh, vigilante punishments - with interactions

	Dependent variable:											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outrage Treatment	0.04*	0.04*	0.04*	0.04*	0.02	0.02	0.02	0.03	0.04*	0.04*	0.04*	0.04*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Female		−0.07**	−0.07**	−0.07**		−0.05*	−0.05*	−0.05*		−0.04†	−0.04†	−0.05*
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)
Violence Index		0.02*	0.02*	0.03*		0.02†	0.02†	0.02†		0.04**	0.04**	0.05**
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Education		−0.01†	−0.01†	−0.01†		−0.01*	−0.01*	−0.01*		−0.003	−0.002	−0.002
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Assets Index		0.0003	0.0003	0.002		0.01	0.01	0.01		−0.002	−0.002	−0.001
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Age		−0.002*	−0.002*	−0.001*		−0.001*	−0.001†	−0.001†		−0.001	−0.001	−0.001
		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)
Married		−0.03	−0.03	−0.03		−0.02	−0.02	−0.01		0.002	0.003	0.002
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)
Employed		0.01	0.01	0.01		0.02	0.02	0.02		0.01	0.01	0.01
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)		(0.03)	(0.03)	(0.03)
Proximity to Security Base			0.01				0.01				−0.02	
			(0.01)				(0.01)				(0.02)	
Prox. to Security Base			−0.01				−0.01				0.02	
× Outrage Treatment			(0.02)				(0.01)				(0.02)	
Presence of State Security				−0.0005				0.02				−0.01
				(0.01)				(0.02)				(0.02)
Presence of State Security				−0.01				−0.03				−0.003
× Outrage Treatment				(0.02)				(0.02)				(0.02)
Constant	0.13**	0.28**	0.28**	0.27**	0.15**	0.27**	0.27**	0.27**	0.17**	0.24**	0.25**	0.24**
	(0.01)	(0.05)	(0.05)	(0.05)	(0.01)	(0.05)	(0.05)	(0.05)	(0.01)	(0.05)	(0.05)	(0.05)
PSU FEs		✓		✓		✓		✓		✓		✓
Observations	2,338	2,234	2,234	2,190	2,338	2,234	2,234	2,190	2,338	2,234	2,234	2,190
R^2	0.003	0.11	0.02	0.02	0.001	0.10	0.01	0.02	0.003	0.10	0.02	0.02

*p<0.1; **p<0.05; ***p<0.01

Standard errors clustered by respondent in parentheses.

Coefficients are estimated using OLS. Outrage Treatment is a dummy variable that takes a value of 1 if the respondent was randomly assigned to the outrageous version of the crime scenario. Controls include gender, education, age, an assets index, marital status, and employment status of the household head.

G Study 3: Additional analysis

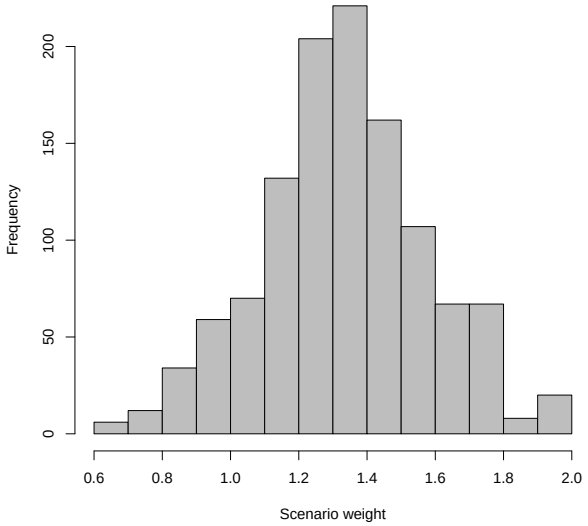
G.1 Scenario likelihood weights

In this section we present an analysis of the likelihood weights that we use in the analysis presented in Section . The purpose of these weights is to make the distribution of scenarios as close as possible to the real violence to which people in our sample are exposed. To calculate them, we asked respondents after every scenario how likely they thought it was that the scenario could take place in their community on a four-point scale. We use a specification that includes each of the individual scenario characteristics and their interactions to calculate likelihood weights for each scenario. Intuitively, what this means is that if the average respondent in our sample finds it unlikely that a soldier would rob a student (for example), that scenario would be down-weighted according to how unlikely they find this crime relative to others. This helps ensure that our estimates are based on variation that is actually relevant in the local context, making the experiment more externally valid. The full table of estimated likelihoods of the scenarios is available on request.

Figure G.1 presents the distribution of the probability weights. They range between 0.55 and 1.75. The fact that the range between the least and most likely scenarios is relatively small suggests that the scenarios that we created are generally contextually relevant.

The results of all analyses in Figure 6 are substantively unchanged when they are re-estimated without these scenario probability weights. Results are available on request.

Figure G.1: Distribution of scenario probability weights



G.2 Interaction with state security capacity

Table G.1: Characteristics of scenarios that would lead participants to prefer harsher and extrajudicial punishments - with interaction terms

	Dependent variable:			
	Harsh Punishment		Vigilante Punishment	
	(1)	(2)	(3)	(4)
Victim: Innocence	0.179**	0.184**	0.001	0.003
	(0.043)	(0.045)	(0.009)	(0.010)
Violence: Severity	0.102*	0.107*	0.001	−0.003
	(0.051)	(0.053)	(0.011)	(0.011)
Proximity to Security Base	−0.011		0.006	
	(0.039)		(0.008)	
Presence of State Security		0.003		0.014
		(0.042)		(0.009)
Victim: Innocence × Violence: Severity	0.038	0.006	−0.0004	−0.004
	(0.058)	(0.060)	(0.012)	(0.013)
Victim: Innocence × Prox. to Security Base	0.033		−0.011	
	(0.041)		(0.009)	
Violence: Severity × Prox. to Security Base	0.016		−0.002	
	(0.043)		(0.009)	
Victim: Innocence × Presence of State Security		0.0005		−0.006
		(0.043)		(0.009)
Violence: Severity × Presence of State Security		0.071		−0.009
		(0.045)		(0.010)
Perpetrator Treatment	✓	✓	✓	✓
Controls		✓		✓
Constant	0.398*	0.568	0.151**	0.105
	(0.156)	(0.406)	(0.034)	(0.081)
Observations	959	944	1,005	988
R^2	0.071	0.245	0.027	0.192

†p<0.1; *p<0.05; **p<0.01
Standard errors in parentheses.
Scenarios are weighted by their likelihood as estimated by the participants. Individual Controls include gender, education, age, an assets index, marital status, and employment status of the household head.

Table G.2: Characteristics of scenarios and punishment principles - with interaction terms

	Dependent variable:					
	Punitiveness Rank=1		Legality Rank=1		Effectiveness Rank=1	
	(1)	(2)	(3)	(4)	(5)	(6)
Victim: Innocence	0.018	0.015	−0.034†	−0.036†	0.016	0.021
	(0.017)	(0.018)	(0.020)	(0.021)	(0.016)	(0.018)
Violence: Severity	0.006	−0.002	−0.011	−0.005	0.005	0.006
	(0.018)	(0.019)	(0.020)	(0.022)	(0.017)	(0.018)
Proximity to Security Base	−0.006		0.011		−0.005	
	(0.017)		(0.019)		(0.016)	
Presence of State Security	0.004		0.021		−0.025	
	(0.017)		(0.020)		(0.017)	
Victim: Innocence ×	0.036*		−0.027		−0.009	
Violence: Severity	(0.018)		(0.021)		(0.018)	
Victim: Innocence ×		−0.006		0.004		0.002
Prox. to Security Base		(0.018)		(0.021)		(0.017)
Violence: Severity ×		−0.007		−0.001		0.007
Prox. to Security Base		(0.018)		(0.021)		(0.018)
Victim: Innocence ×		0.015		−0.026		0.010
Presence of State Security		(0.019)		(0.022)		(0.018)
Violence: Severity ×	0.445**	0.269†	0.343**	0.216	0.212**	0.515**
Presence of State Security	(0.064)	(0.156)	(0.075)	(0.181)	(0.063)	(0.152)
Perpetrator Treatment	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	1,078	1,061	1,078	1,061	1,078	1,061
R^2	0.025	0.141	0.030	0.151	0.015	0.141

†p<0.1; *p<0.05; **p<0.01
Standard errors in parentheses.
Scenarios are weighted by their likelihood as estimated by the participants. Individual Controls include gender, education, age, an assets index, marital status, and employment status of the household head.

G.3 Disaggregated results

This section presents the full results with all of the scenario categories disaggregated and presented as dummy variables for each individual category. We present coefficient plots that correspond to the second specification in the analyses in Section that includes individual control variables and PSU fixed effects.

Figure G.2: Effect of disaggregated scenario characteristics on anger

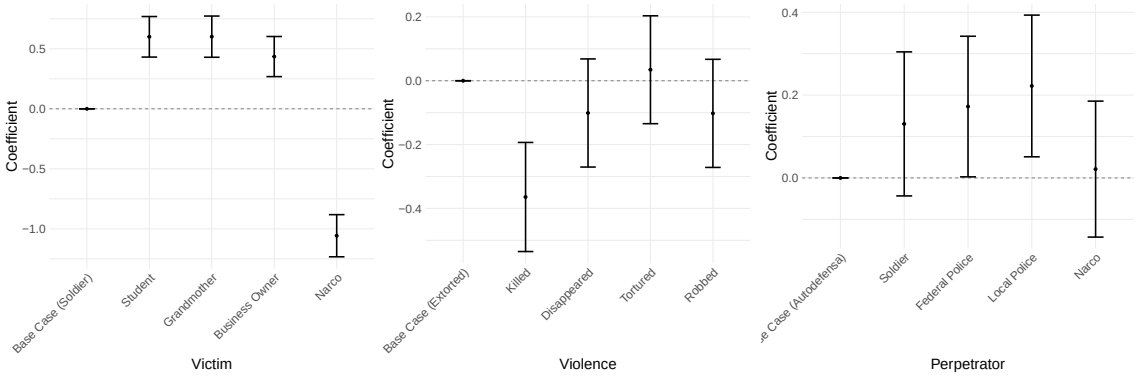


Figure G.3: Effect of disaggregated scenario characteristics on fear

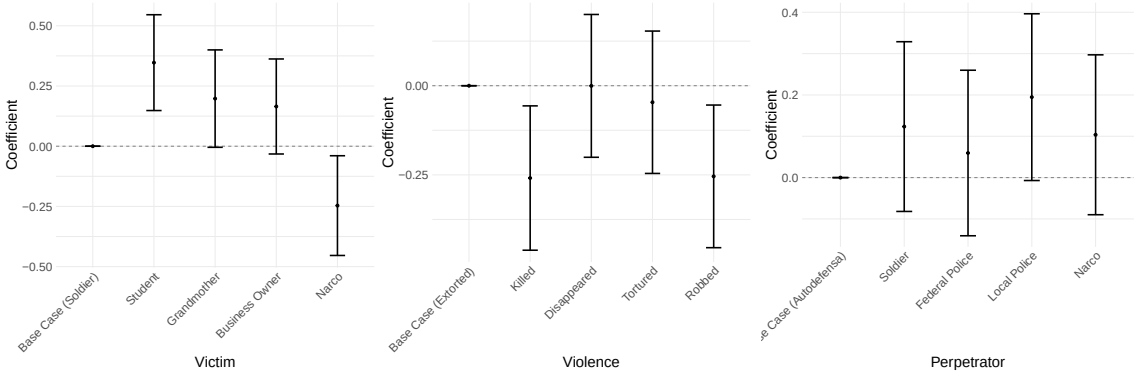


Figure G.4: Effect of disaggregated scenario characteristics on preference for harsh punishments

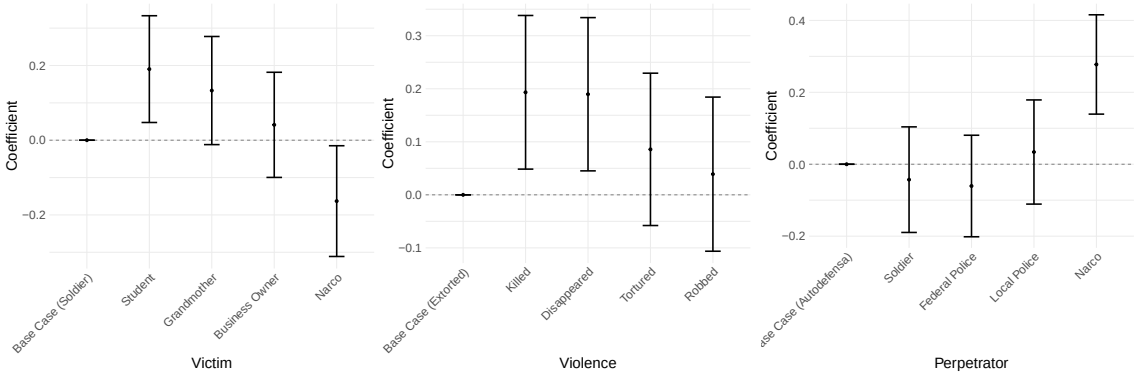


Figure G.5: Effect of disaggregated scenario characteristics on preference for extrajudicial punishments

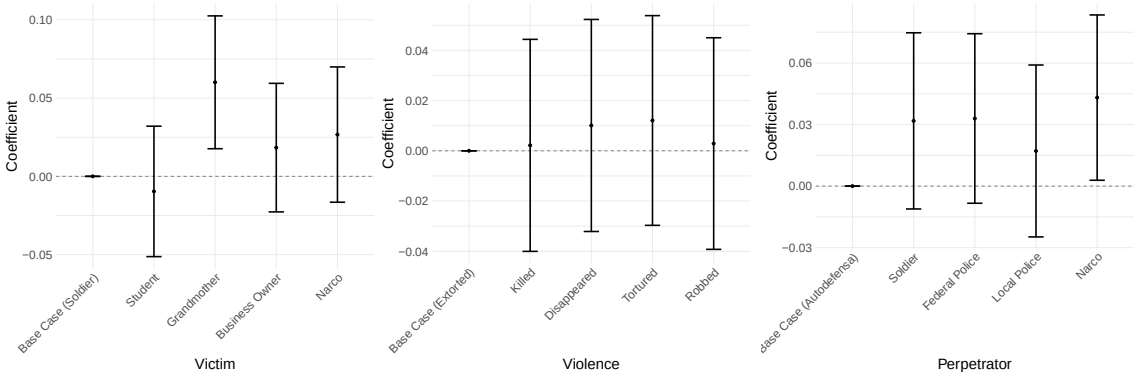


Figure G.6: Effect of disaggregated scenario characteristics on the rank of punitiveness

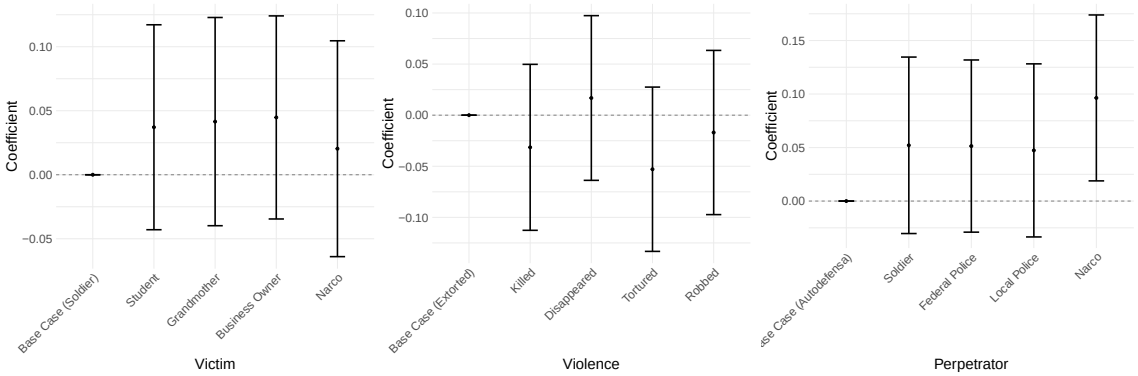


Figure G.7: Effect of disaggregated scenario characteristics on the rank of legality

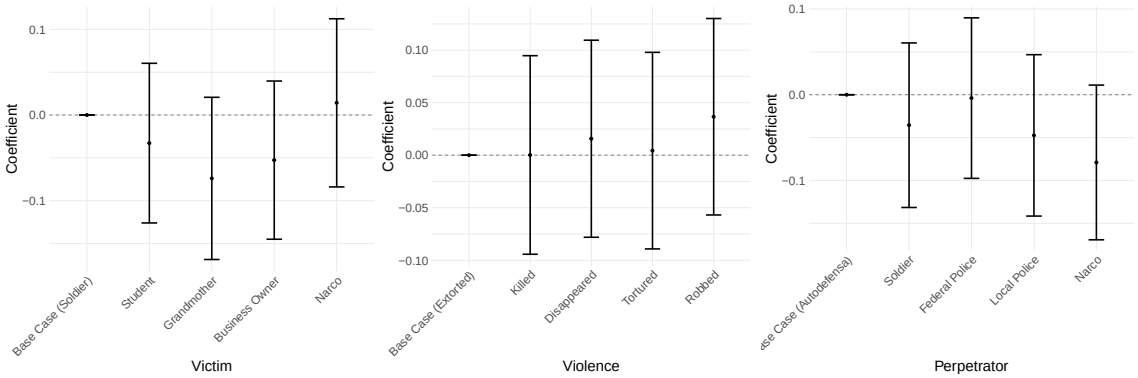
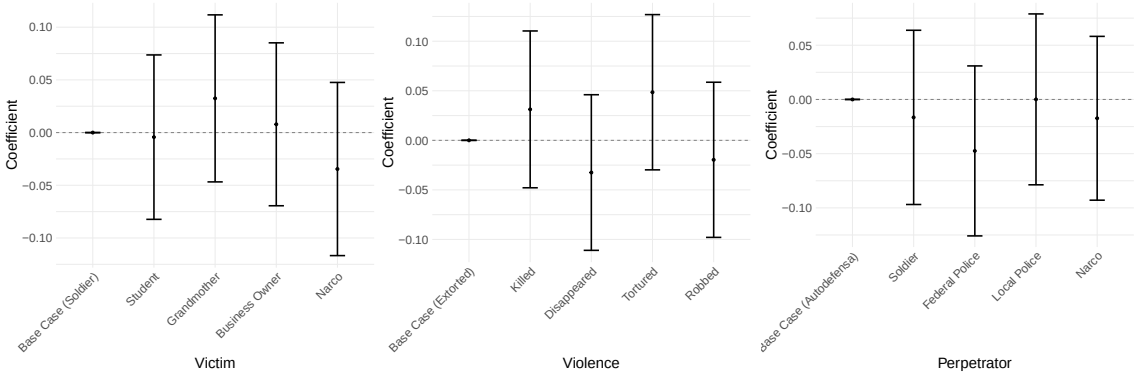


Figure G.8: Effect of disaggregated scenario characteristics on the rank of effectiveness



H Mediation analysis

Do the observed differences in anger mediate the relationship between violence and preferences for harsh or extrajudicial punishment? Our theory suggests that causal relationships between violence and punishment preferences should be mediated by the emotion of anger. However, model-based mediation analysis rests on strong and often unrealistic assumptions that are likely to bias estimates of the effects of mediation upward (Bullock et al., 2010). One recent approach to mediation analysis based on methods in biostatistics involves estimating the 'average controlled direct effect' (ACDE) and comparing it to the total effect of the treatment (ATE). The ACDE is the average effect of changing the treatment while fixing the value of the mediator at some level m, usually zero. In this case, it represents the effect of violence exposure/treatments if anger is set to zero.

Estimating the ACDE requires the assumption of 'sequential unconfoundedness', which implies that there are no unmeasured confounders in the model of the effect of the treatment on the outcome, conditional on pretreatment covariates, or in the model of the effect of the mediator on the outcome, conditional on the treatment, pretreatment covariates, and posttreatment covariates.[10] Acharya et al. (2016) show that the ATE can be decomposed into the ACDE, the 'average natural indirect effect' (ANIE) of a mediator, and an interaction effect that captures how much the direct effect of the treatment depends on the mediator at the individual level. In other words, under the assumption of constant interactions, the difference between the ATE and ACDE represents the estimated effect of a mediator of interest. The ACDE is estimated using an approach called 'sequential g' estimation, which removes the estimated effect of the mediator from the dependent variable, and then estimates the effect of the independent variable of interest on this 'demediated' outcome.

[10] Acharya et al. (2016) is not the only way to test for mediation. Imai et al. (2010) also develop a method to test for mediation, and Imai & Yamamoto (2013) extends it to enable multiple mediators. We use the Acharya et al. (2016) method because it is identified in the presence of measured (though importantly not unmeasured) intermediate confounders, while the Imai et al. (2010) method requires the assumption of no intermediate confounders.

Figure H.9 plots the total effect of our independent variable of interest and the ACDE holding constant anger as our mediator of interest in Studies 1, 2, and 3.

Figure H.9 suggests that, to the extent we believe the assumptions of sequential unconfoundedness and constant interactions, a substantial portion of the total effect of the violence exposure/treatments in Studies 1 and 3 works through anger. In Study 1, the ACDE holding constant anger is estimated to be 67% of the total effect of exposure to violence. This implies that the mediated effect of exposure to violence on our policy measures (the ANIE) is estimated to be 33% of the total effect. For Study 3, just under 50% of the total effect of the Victim: Innocence treatment is mediated by anger. As suggested by the results in Figure 6, the effects of the Severe Violence treatment on support for harsh punishments is not mediated by anger. For both Study 1 and the Victim: Innocence treatment in Study 3, this analysis also shows that holding anger fixed, there is no statistically significant controlled relationship between violence and our outcomes of interest. In Study 2, interestingly, although the outrageous version of the treatments did induce more anger, this analysis does not find any evidence that the relationship between the outrageous version of the scenario and support for harsh, extrajudicial punishment is mediated by anger. One reason for this null result may be that our measure of anger is not sensitive enough: 82% of respondents in the control group conditions of Study 2 reported that they would feel the highest category of anger. If differences in anger intensity are not being fully captured by our scale, then this analysis would not be able to identify the ACDE. Another alternative is that the effects of the outrageous versions of the scenarios in Study 2 are mediated by something like perceptions of the effectiveness of harsh punishments that are not related to emotions.

How sensitive are these estimates of the ACDE, and by extension the ANIE (the effect mediated by anger), to violations of the sequential unconfoundedness assumption? In both Study 1 and Study 3, one factor that might violate the sequential unconfoundedness assumption is perceptions

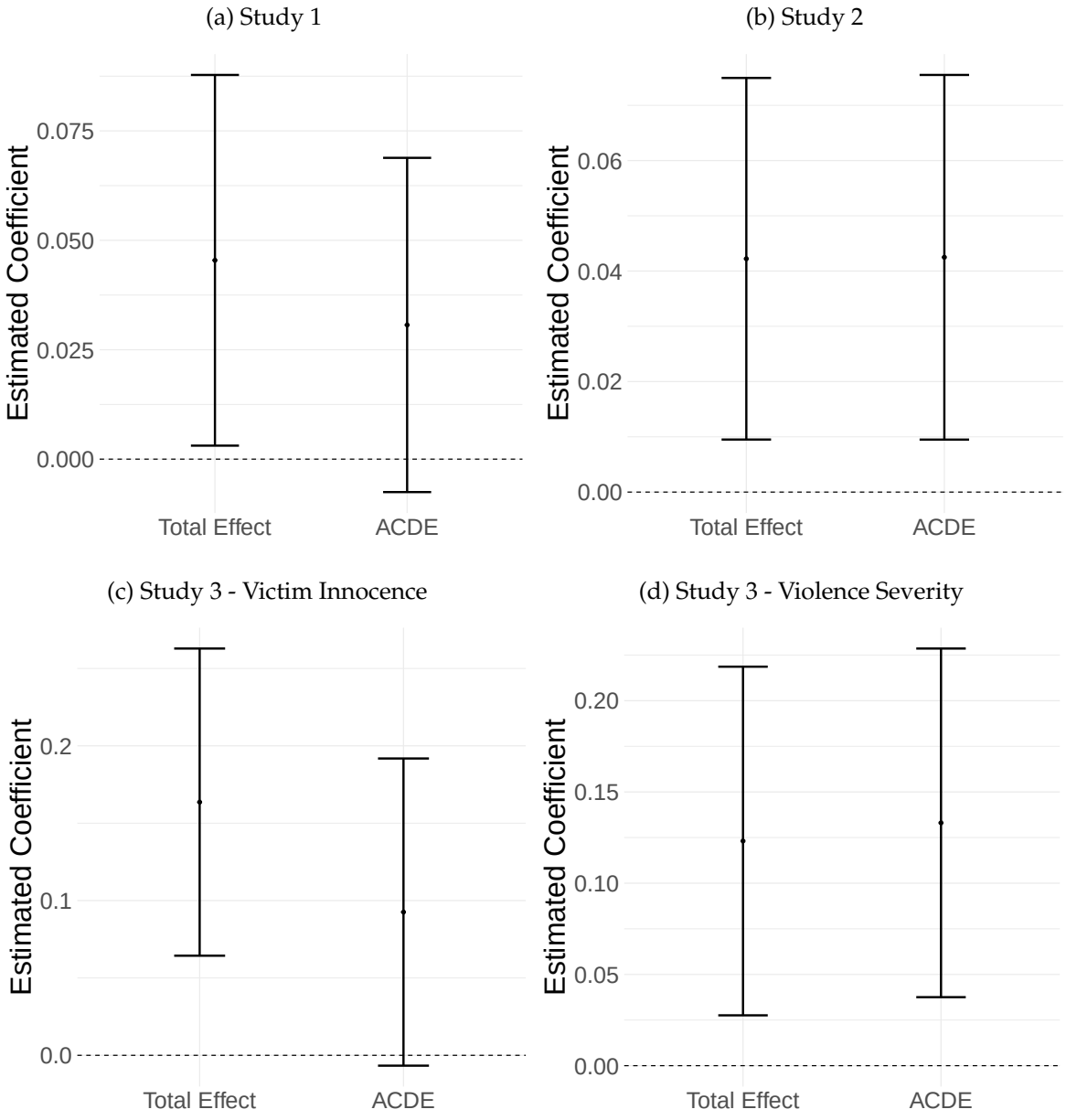


Figure H.9: Estimated Total Effects and Average Controlled Direct Effects

of the state, which might be informed by violence exposure through a non-emotional channel. Acharya et al. (2016) suggest that robustness should be checked via a sensitivity analysis. Figure H.10 shows how the estimated ACDE for Studies 1 and 3 (for Victim: Innocence) change when we let the interaction between the mediator and the outcome error terms vary. The x axis in Figure H.10 represents the residual, bias-inducing correlation between anger (our mediator of interest) and punishment preferences after accounting for the observed baseline and intermediate confounders (such as fear) and the y axis is the estimated ACDE under that amount of unmeasured confounding. This sensitivity analysis shows that the estimates of the ACDE for both Study 1 and Study 3 are quite sensitive to the sequential unconfoundedness assumption. When the correlation between the mediator and outcome errors is negative or small, our estimate of the ACDE is smaller than the ATE, suggesting that anger does mediate some proportion of the effect, under the additional constant interactions assumption. However, when ρ is larger than 0.15 in Study 1 or 0.1 in Study 3 the estimated ACDE is as large as the total effect, suggesting that none of the effect is mediated by anger. Overall, this analysis provides some support that the effects of the independent variables in Studies 1 and 3 are mediated by anger, but these estimates are strongly dependent on the sequential unconfoundedness assumption, which is difficult to justify.

(a) Study 1

(b) Study 3 - Victim Innocence

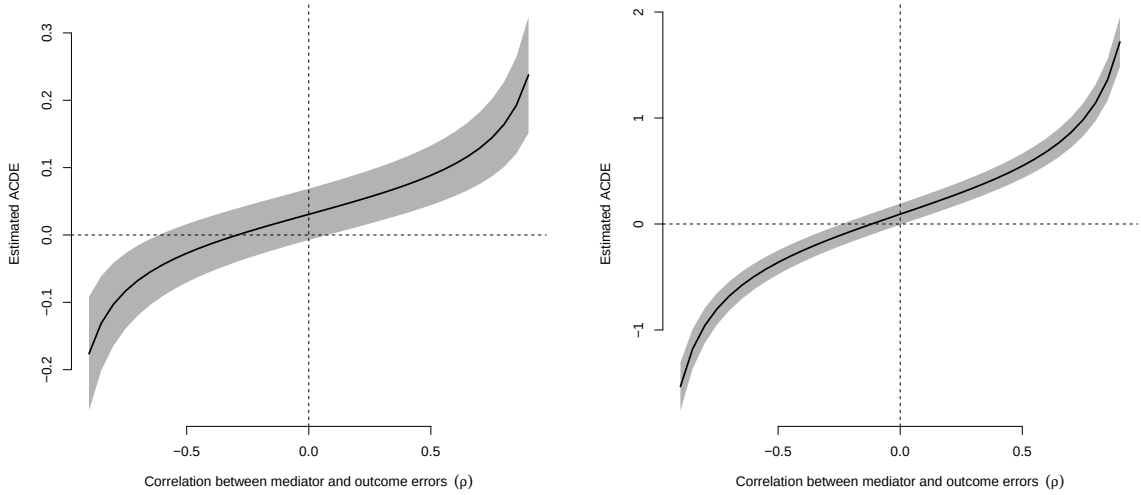


Figure H.10: Sensitivity of ACDE Estimates to the Assumption of Constant Interactions

Appendix Citations

Baaz, Maria E & Mats Utas (2019) Exploring the backstage: Methodological and ethical issues surrounding the role of research brokers in insecure zones. *Civil Wars* 21(2): 157–178.

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