Online Appendix

Contents

A Scope and Case Description

A.1 Scope Conditions

Table A.1: Scope Conditions

Country Name	End Year of Dictatorship	Years Until Prosecution
Argentina	1971	2
Bolivia	1976	21
Brazil	1985	3
Benin	1969	21
Chile	1989	1
Dominican Rep	1965	15
Ecuador	1975	4
Ethiopia	1991	4
Guatemala	1985	6
Haiti	1991	4
Korea South	1987	8
Lesotho	1993	0
Madagascar	1975	18
Panama	1984	5
Peru	1970	9
Niger	1991	0
Sierra Leone	1990	8
Sudan	1967	18
Uruguay	1984	1

Note: Dictatorship data from Geddes, Wright and Frantz (2014) measures the end of military regimes, and Olsen, Payne and Reiter (2010) measures the first year of prosecutions. Mean is 8 years and median is 5.

A.2   Profile of the Díaz Bessone Trial and Verdict

In this section, we expand on the nature of the "treatment" LAPOP survey respondents received in our observational data by placing the Díaz Bessone case in context with other human rights trials in the 2005-2016 period. We show that (1) the trial was representative of other trials in terms of verdict, sentencing, and number of defendants, and (2) the trial was different from most other trials in that it attracted national attention and interest because of Díaz Bessone's rank of general and status as ex-cabinet member.

From the combination of these two characteristics, we assert that (1) our findings from this trial likely generalize given the trial's similarity to others on all dimensions except the rank of the defendant, and (2) because of the comparatively less attention given to most other trials, we expect researchers would detect the changes in attitudes toward human rights and fair trials only among those who could have taken up the "treatment."

There could be a concern that Díaz Bessone's high rank alone accounts for the results. Respondents could find the conviction of a higher-level official more justified than the conviction of a lower-level official, increasing opposition toward the behaviors described in the verdict. Respondents could also see the conviction of a high-ranking official as greater evidence of a politicized court carrying out a "witch hunt" against officials of a former regime, thereby decreasing belief in courts' ability to provide a fair trial. If this were the case, the findings would not generalize to trials whose defendants had lower rank.

However, the predictions in the theory—support for human rights norms increasing among those who find the courts to be fair and belief in courts' fairness decreasing among those with less support for human rights norms—do not depend on the rank of the defendant. Rather, they depend only on the prior belief of respondents about the courts and human rights norms. The necessary and sufficient condition to activate these prior beliefs is therefore simply *a guilty verdict in a human rights trial.*

A.2.1   The Representativeness of the Trial

In Table A.2, we compare the Díaz Bessone trial with population averages of the 183 Argentine human rights trials from 2005 to 2017.

Attribute	Díaz Bessone	All Trials Avg.
Number of Defendants	6	6.7
Guilty Verdict Rate	83.3%	79.8%
Life Sentences *(among guilty)*	40%	39.7%
Non-Life Sentence Avg. Length	15.7 years	14.2 years
Number of Victims Named	91	31.7

Table A.2: Comparing the Díaz Bessone Trial with All Trials

A.2.2   National Attention to the Verdict

Using Google Trends data, the following procedure was used to determine the level of interest in each trial verdict:

- General search criteria: Argentina, 2006-2020.

- For each trial:
  - Fetch trend for name of highest-ranking defendant(s) in trial.
  - Record relative search frequency in month of trial verdict as a percentage of relative search frequency for first defendant convicted in human rights trials, Miguel Etchecolatz, in the month of his verdict.
  - If there are too few searches for the defendant to register in Google Trends, code as zero.

We report the results in Figure A.1, in which the dashed vertical line highlights the search interest in the Díaz Bessone verdict. The search interest for this verdict is two standard deviations above the mean of all verdicts, implying this was a significant trial. However, it was not a "blockbuster" like the most-searched trial: the 2011 ESMA Mega-Cause trial verdict in which notorious torturer Alfredo Astiz received a life sentence.
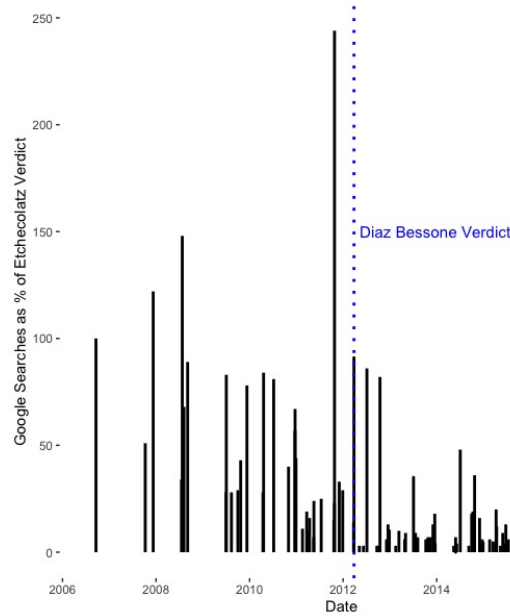

Figure A.1: Google Search Frequency for Trial Verdicts, 2006-2017
Note: Vertical lines represent Google search frequency—coded according to procedure listed above—for each trial between 2006 and June 2017. Vertical dashed blue line indicates location of Díaz Bessone verdict search frequency.

While we demonstrate on the "demand" side that the Díaz Bessone verdict was among the top 5% most-searched Argentine human rights trial verdicts, we want to go further in demonstrating the "supply" side of news about the verdict. Table A.3 below lists the outlets

that covered the verdict, and their circulation in the year 2012 if data were available from the Instituto Verificador de Circulaciones (IVC). Of the six outlets for which articles were recoverable with URLs, we note that this encompasses three of the four largest outlets by circulation (Clarín, La Nación, and Página 12) in Argentina, and the second largest outlet by circulation in the province of Santa Fe, where the verdict was handed down (El Litoral). This list also does not include all outlets that incorporated the verdict news from the EFE wire service, the largest Spanish-language wire service in the world. The coverage of the story suggests that Argentines searching for Bessone after the verdict were likely to come across news stories about the event, and that readers of these sources were likely to encounter the information.

Source	Link to Article	Circulation (IVC)
Clarín	Click here	290,243 (2012)
La Nación	Click here	169,896 (2012)
Página 12	Click here	$\approx 50,000$
Rosario3	Click here	*Unknown*
11Noticias	Click here	*Unknown*
El Litoral	Click here	15,724 (2012)

Table A.3: Selected Newspapers Publishing the Díaz Bessone Verdict (Print/Online)

B   Observational Design

B.1   Density of Respondents

Figure B.1 shows a histogram of respondents by day in the 2012 LAPOP wave in Argentina. The Figure shows the density of survey participants does not change around the day of the survey, which suggests that patterns of responses are uncorrelated with the trial verdict. Figure B.2 displays this information with a LOESS fit of density over time.
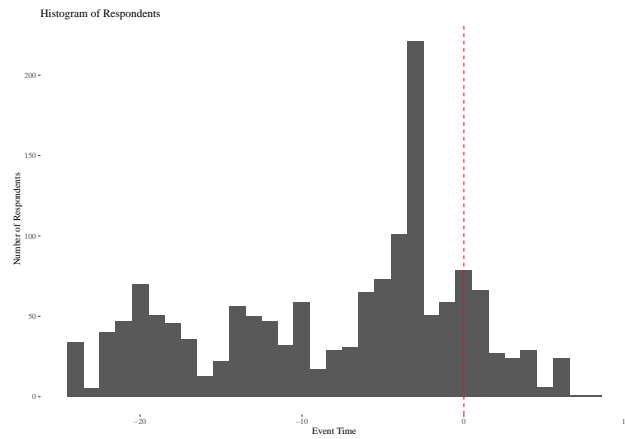
Figure B.1: Respondent Density
Note: Histogram of respondents by day during the LAPOP survey in Argentina in 2012. Vertical red dashed line marks day of treatment.
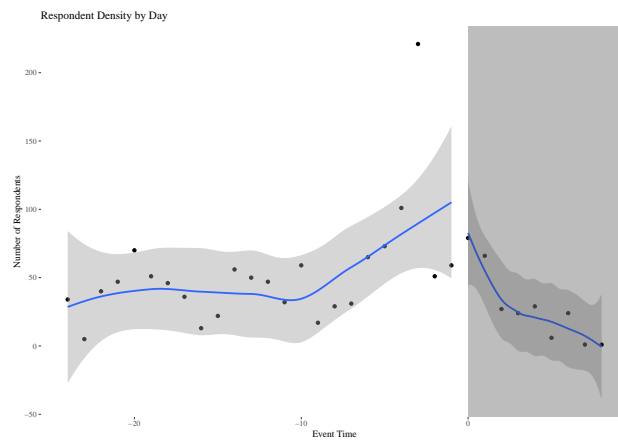

Figure B.2: Respondent Density
Note: Time series of number of respondents. Blue lines with shading are loess fit with 95% confidence intervals.

B.2   Breakdown of Strata

Table B.1: Strata Breakdown

Urbano/Rural	Region	Size (1-5)	Total Respondents	Total Treat	Prop. Treated
Urbano	AMBA	1	450	22	0.05
Urbano	Centro	1	108	38	0.35
Urbano	Centro	2	18	18	1.00
Urbano	Centro	3	36	18	0.50
Urbano	Centro	4	54	18	0.33
Urbano	Noreste Argentino (NEA)	2	54	2	0.04
Urbano	Noreste Argentino (NEA)	3	18	0	0.00
Urbano	Noreste Argentino (NEA)	4	36	18	0.50
Urbano	Noroeste Argentino (NOA)	2	108	41	0.38
Urbano	Noroeste Argentino (NOA)	3	18	16	0.89
Urbano	Noroeste Argentino (NOA)	4	36	0	0.00
Urbano	Cuyo	2	54	0	0.00
Urbano	Cuyo	3	18	12	0.67
Urbano	Cuyo	4	18	0	0.00
Urbano	Patagonia	2	18	3	0.17
Urbano	Patagonia	3	36	2	0.06
Urbano	Patagonia	4	18	0	0.00
Urbano	Provincia de Buenos Aires	2	126	1	0.01
Urbano	Provincia de Buenos Aires	3	36	18	0.50
Urbano	Provincia de Buenos Aires	4	72	18	0.25
Rural	Centro	5	18	12	0.67
Rural	Noreste Argentino (NEA)	5	36	0	0.00
Rural	Noroeste Argentino (NOA)	5	36	0	0.00
Rural	Cuyo	5	18	0	0
Rural	Provincia de Buenos Aires	5	72	0	0

Note: Breakdown of strata, total respondents, total surveyed after verdict, and proportion of treated per strata.

B.3   Balance Test Covariates

Our core identifying assumption is that the timing of the trial verdict was uncorrelated generic response trends, meaning individuals surveyed after were similar to ones surveyed before. While we cannot know for certain that respondents were similar in their unobservable traits, we test for similarity of respondents along observed attributes.

We check for balance along four dimension, displayed in Figure 2 in the main text: attrition in response to our outcomes of interest (Panel A), demographic characteristics of respondents (Panels B and C), media consumption, including internet use, interest in politics, posting of news on social media, and watching the news (Panel D) formal political activity (being register for a party and to vote, voting for President, attending town halls and city council meetings, Panel E), and informal political activity (protest, signing petitions, contacting politicians, solving local community problems, Panel F).

Each category represents its own threat to inference: if individual response rates change after treatment (attrition), it would suggest that different types are choosing to fill out the survey, which may generate an upward bias if something like the "Bradley Effect" leads individuals who have high trust and high preference for social cleansing or torture to censor by not filling out the survey. We find no evidence of differences in attrition for our outcomes across treatment and control groups. We assume from this result that missingness occurs at random conditional on covariates. In Figure B.3 we impute the block mean for missing

outcome data and re-estimate our main specification with an indicator for missingness and find similar results to those in Table 1.
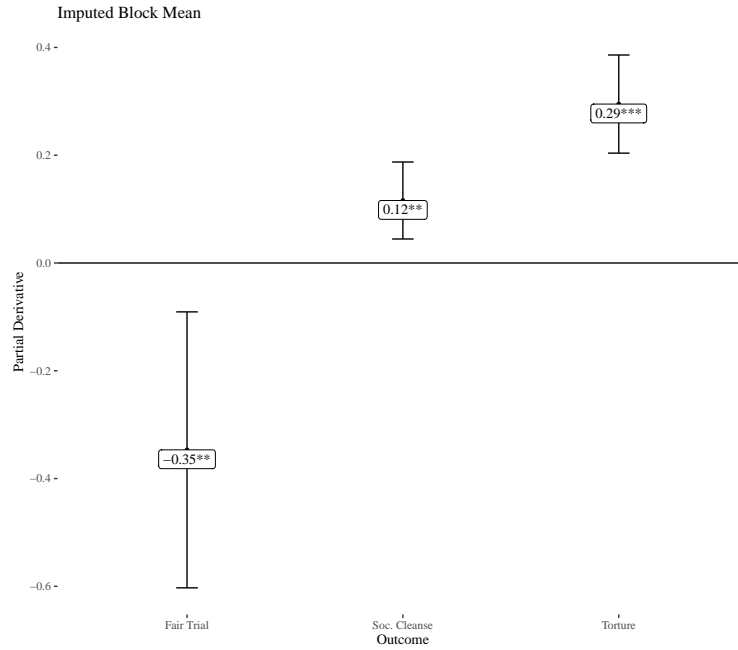

Figure B.3: Imputing Missing Outcome Data
Note: Strata mean imputed for missing outcomes under Missing at Random assumption. Regressions include strata fixed effect and dummy for imputation.
***$p < 0.001$, **$p < 0.01$, *$p < 0.05$

If individuals are very different demographically, it would suggest a violation of our assumption that after conditioning on strata fixed effects the treatment was as-if randomly assigned. Having many respondents that are older with different incomes or particular races and genders would provide clear evidence of imbalance on observable, which implies imbalance on unobservable traits as well.

Next, we check balance on media consumption - if everyone in the treated group was an avid internet user who consumed more news media, they may be more likely to comply with the treatment, but they also may be generally more informed about the dictatorship in ways that make them distrustful of institutions are beholden to strong human rights norms. Finally, we check balance on political participation - both formally and informally - since politically active persons who are more civically engaged may hold stronger views correlated with our outcomes of interest. For example, if we found imbalance on protest activity, it may suggest more liberal activists are in the treated group, generating upward bias.

B.4   Placebo Tests

We conduct three placebo exercises to test for the possibility that our results are an artifact of underlying endogenous trends in survey response. We subset the data to before the treatment period, and exclude the day of the verdict itself, to detect trends that may violate

excludability. We censor our data in this way to avoid coding treated individuals with placebo treatment indicators.

First, we construct a placebo treatment using the empirical median of the pre-treatment trends (Lee and Lemieux, 2010; Muñoz, Falcó-Gimeno and Hernández, 2020). Second, we regress our outcomes on a linear trend (Muñoz, Falcó-Gimeno and Hernández, 2020). Third, we construct a placebo treatment day of the anniversary of the coup, which could have triggered memories of the dictatorship from respondents. Results are displayed in Figure B.4. We fail to reject the null of a placebo treatment effect for all three tests on all outcomes. While the non-detection of a violation cannot prove our untestable assumption that treatment was randomly timed, the small and insignificant estimates are in line with the exclusion restriction.
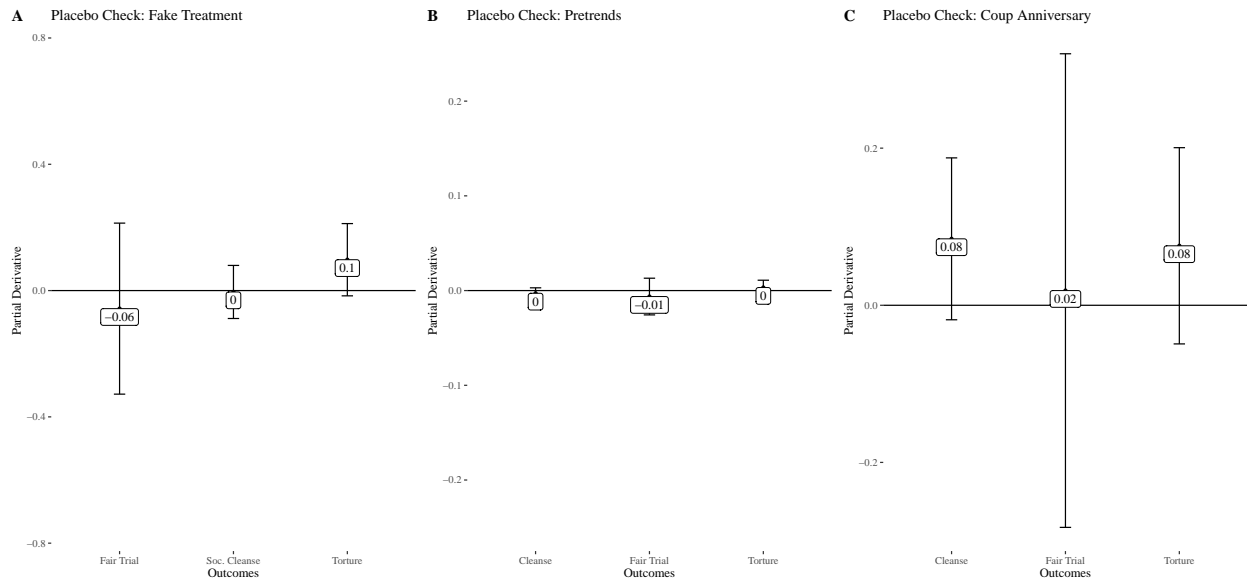

Figure B.4: Placebo Treatments

Note: Point estimates obtained from equation 1. 95% confidence intervals from robust standard errors.

B.5   Falsification Tests

Cascading events or idiosyncratic shocks to broader social and institutional trust could explain our results. For example, if trust in the police or the high courts was significantly different in the pre and post period, our results could be an artifact of unobserved policy shocks or news cycles which moved public opinion towards human rights norms and away from the perception that courts guarantee fair trials. To test this possibility, we include measures of trust which ought to be unrelated from the courts verdict on Bessone: trust in the Supreme Court (which was not involved in the trial), trust in local government, the legislature, the president, the police, and general social trust. We plot results in Figure B.5, which shows a near zero and statistically insignificant effect of the verdict on these measures of trust.
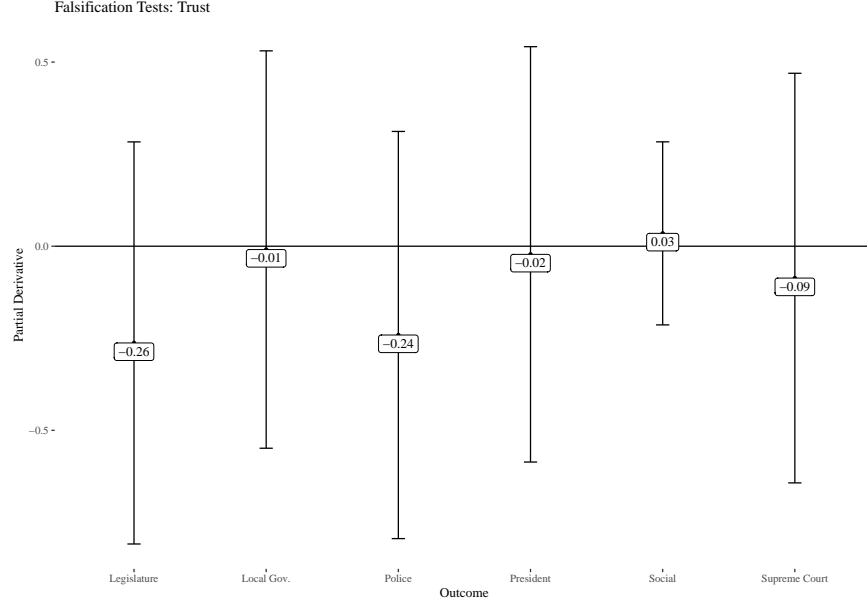
Falsification Tests: Trust

Figure B.5: Falsification Test: Institutional and Social Trust
Note: Point estimates obtained from equation 1. X-axis refers to outcome. Estimates are within one week window and include treatment, trend, and interaction of treatment and trend. 95% confidence intervals from robust standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

B.6 Heterogeneous Effects

We study the potential for heterogeneous effects by interacting the treatment variable with categories for respondent ideology. LAPOP asks respondents to rank their ideology from 1-10, where 1 is the furthest to the left and 10 is furthest to the right. We create dummies for each ideology category, and estimate the following model.

$$y_i = \alpha + \delta D_i + \gamma_j \left(D_i \times \sum_{j \neq 5}^{j} \text{Ideology}_i^j \right) + \lambda_j \sum_{j \neq 5}^{j} \text{Ideology}_i^j + \sum_{k=1}^{K} \beta_k X_i^k + \varepsilon_i \qquad (3)$$

We use 5 - the middle of the distribution - as the reference, and plot the coefficients and 95% confidence intervals for j ideology scores $\{1, 2, 3, 4\} \cup \{6, 7, 8, 9, 10\}$. We plot the coefficient estimates which show the the results are homogeneous across the ideological spectrum. We find no evidence of differential effects across ideological categories, however, we stress that the absence of heterogeneity in this sample cannot be extrapolated to mean that ideology is never a moderating factor, since in practice we may not have power across treatment arms to detect heterogenous effects.

Figure B.6: Heterogeneous Effects by Ideology
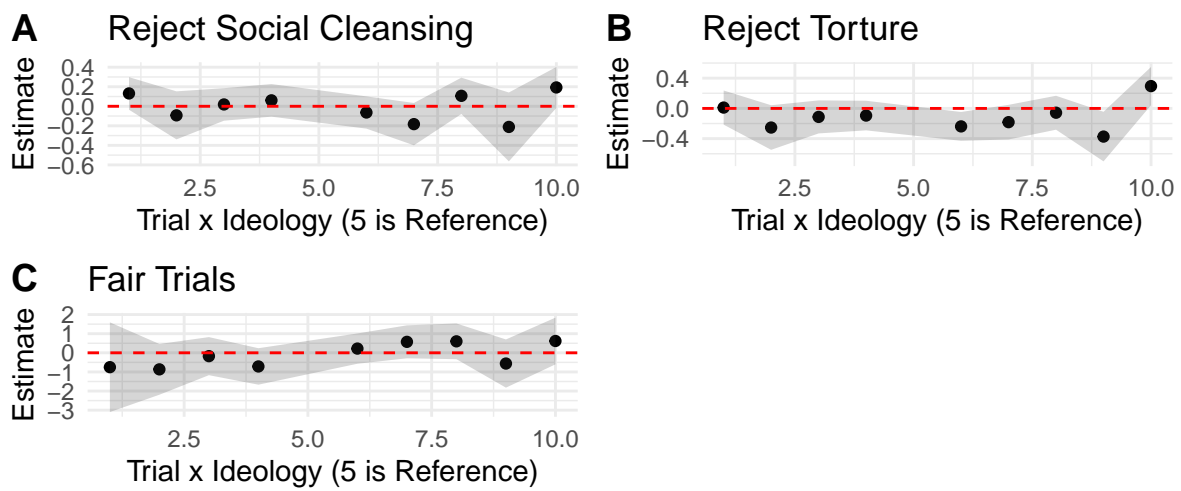
B.7 Attitude Distributions

Figure B.7: Distribution of Human Rights Attitudes Pre and Post Verdict

(a) Reject Social Cleansing
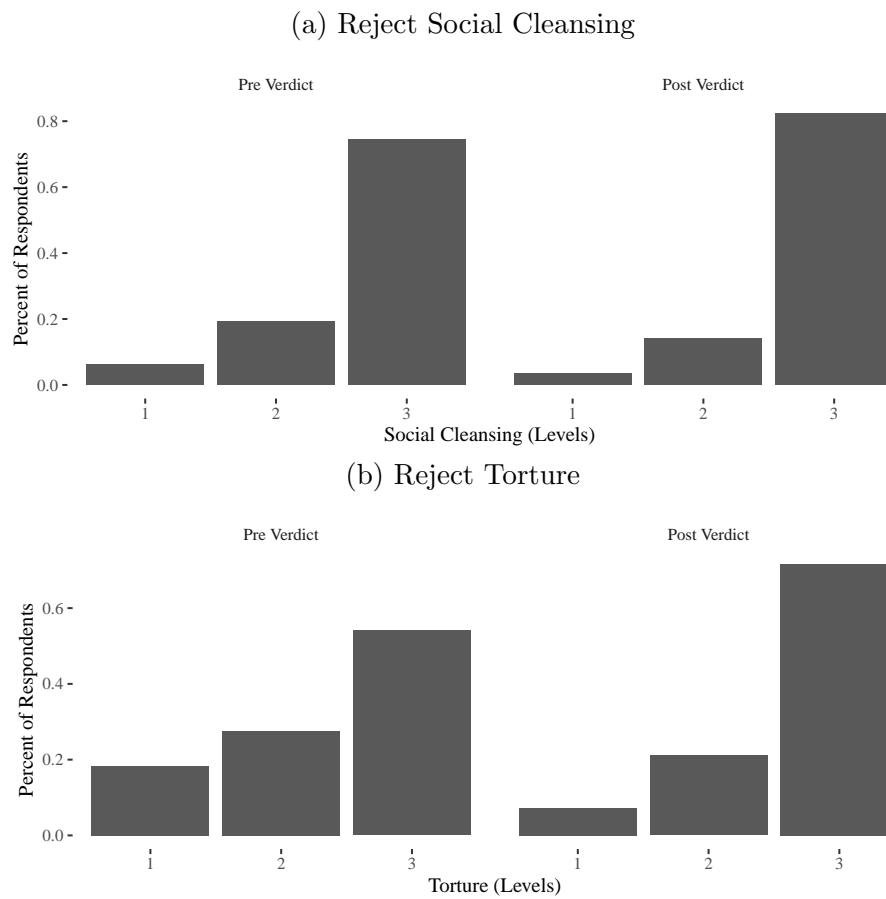
(b) Reject Torture

Note: Histogram of responses by category before and after verdict for Social Cleansing and Torture questions.

B.8 Recoding March 26 Respondents

	Reject Social Cleansing				Reject Torture				Fair Trial			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Verdict	0.06*	0.08*	0.09**	0.12**	0.14***	0.09*	0.19***	0.15***	−0.27*	−0.26	−0.36*	−0.37*
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.05)	(0.13)	(0.17)	(0.14)	(0.18)
N. Respondents	1471	1457	1414	1402	1469	1455	1411	1399	1372	1359	1328	1317
Model Statistics:												
March 26 Recode?	Treated	Treated	Missing	Missing	Treated	Treated	Missing	Missing	Treated	Treated	Missing	Missing
Strata Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{\dagger}p < 0.1$

Table B.2: Baseline Results Recoding March 26

Outcome questions from LAPOP. Verdict in columns (1), (2), (5), (6), (9), and (10) is a binary indicator scored one when the day of the survey is greater than or equal to March 26, 2012. In columns (3), (4), (7), (8), (11), and (12), verdict is a binary indicator scored one when the day of the survey is greater than March 26, 2012, and is coded as missing for survey day March 26. Covariates include Age, Gender, Weekend fixed effects, and a time trend. All models include strata fixed effects (region and size of municipality).