Online Appendix for "What Stops the Torture?"

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1 Introduction

This document discusses a variety of technical issues related to AJPS-34001, "What Stops the Torture?" The document is organized as follows. First, we describe the individual torture terminations that occur in our data, both before and after the end of violent dissent. Next, we discuss how studying domestic torture terminations (rather than levels of torture) allows us to potentially circumvent measurement problems inherent in the quantitative study of state torture. The third section focuses on sample selection issues. Finally, we present a number of sensitivity checks, conducted to probe the robustness of our inferences, especially as they relate to violent dissent and judicial independence.

2 Measurement of Torture

Torture is not readily observable. As a result, quantitative data on torture is subject to an important measurement problem. Because information is not equally available in all countries (i.e., the level of press freedom and individual civil liberties varies from country to country), some countries are more likely than others to produce information about torture practices. In the study that this Appendix accompanies, we study the termination of torture, which is to say a *change* in torture, rather than level of torture, as the dependent variable. Doing so allows us to assume that the undercount in the measurement of torture is more or less fixed over time in a given country, and thus each country effectively has its own baseline, or intercept.[1] By focusing on change—or more specifically in our paper, torture termination—we are better able to overcome the difficulty of cross-national comparison than if we used level of torture as our dependent variable.

That said, the advantage of studying torture termination will not be successful if the undercount associated with the measurement of torture is so bad that states that engage in torture are able to successfully hide all of their activity (i.e., effectively drive the value of the dependent variable to zero). If data on torture incidence indicate that states with low press freedom have consistently lower levels of torture than states with high press freedom, one would strongly suspect that the instrument used to measure torture suffered from downward bias that is associated with press freedom. Fortunately, however, this does not seem to be the case in the Cingranelli and Richards (2004) data: Table 1 shows that states with low press freedom frequently receive high torture scores. Note in particular the 400 of 679 country-years with no press freedom had the highest score on the state's use of torture variable.

[1] We wish to thank an anonymous reviewer for pointing this out.

Table 1: Cross Tab of Press Freedom and Level of Torture, 1981-1999

	Torture		
Press	None	Low	High
None	21	258	400
Low	59	505	583
High	60	327	120

3 Description of Torture Terminations

Table 2 lists the country-years in which states terminated their domestic use of torture in the presence of violent dissent. It also indicates the country-years in which the termination of torture was the result of a change in executive leadership or a regime transition from dictatorship to democracy.

Table 2: Torture Terminations Facing Violent Dissent

Country	Year	Other Information
Angola	1985	
Greece	1991	Change in Executive Leadership
Mali	1993	Year of Democratic Transition
Thailand	1984	Year of Democratic Transition
United Kingdom	1982	
United Kingdom	1986	
United Kingdom	1988	

Table 3 shows the country-years in which spells terminated in our sample *after* the end of violent dissent. It also specifies whether the spell ended in the termination of torture or the return of violent dissent.

Table 3: Spell Terminations After End of Violent Dissent

Country	Year	End of Spell
Croatia	1996	Torture Termination
El Salvador	1995	Torture Termination
Greece	1998	Torture Termination
Malaysia	1992	Torture Termination
Mali	1996	Torture Termination
Mali	1999	Torture Termination
Moldova	1998	Torture Termination
Turkmenistan	1993	Torture Termination
Spain	1988	Torture Termination
Spain	1991	Torture Termination
Spain	1998	Torture Termination
Thailand	1992	Torture Termination
Chile	1991	Return of Threat
Ethiopia	1997	Return of Threat
Haiti	1991	Return of Threat
Honduras	1991	Return of Threat
Iraq	1983	Return of Threat
Iraq	1988	Return of Threat
Iraq	1991	Return of Threat
Iraq	1993	Return of Threat
Israel	1991	Return of Threat
Israel	1993	Return of Threat
Israel	1995	Return of Threat
Kazakhstan	2000	Return of Threat
Liberia	1999	Return of Threat
Morocco	1991	Return of Threat
Nicaragua	1993	Return of Threat
Pakistan	1993	Return of Threat
Peru	1999	Return of Threat
Russia	1999	Return of Threat
South Africa	1983	Return of Threat
Spain	1984	Return of Threat
Uganda	1991	Return of Threat
Uganda	1993	Return of Threat
Yugoslavia	1998	Return of Threat

4 Regression Without Interactions

In the study with which this Appendix is associated we explain that there is insufficient variation in the dependent variable for us to interact our Violent Dissent variable with each of our indicators of liberal democracy and produce estimates. Although it is not a proper specification with which to test our hypotheses, we nonetheless estimated an additive model that includes Violent Dissent as a control variable. Those results, which are briefly noted in the study, are reported below in Table 4.

Table 4: Discrete Time Duration Model

Dependent Variable: "1" if Torture Terminates

Regressor	Logit Coeff.
$Voice_t$	0.474*
	(0.278)
$Veto_t$	0.212
	(0.426)
Freedom of Expression$_t$	0.202
	(0.172)
Violent Dissent$_t$	-1.367***
	(0.493)
NGOs per capita$_t$	-0.008
	(0.012)
Naming and Shaming$_{t-1}$	-0.223
	(0.159)
Threat to Leader Tenure$_t$	-0.002
	(0.012)
Civil Law$_t$	-0.233
	(0.228)
GDP_t	0.388***
	(0.136)
Population$_t$	-0.140
	(0.112)
Log Pseudolikelihood	-373.140
Observations	1813

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses. Results from t, t^2, and t^3 and a constant not reported.

5 Robustness Checks

In the following subsections, we discuss a number of sensitivity analyses conducted to probe the robustness of the inferences we draw in the study. First, we focus on whether our inferences about the null effect of institutions given violent dissent are conditional on our choice of measures of violent dissent. Then, we discuss our results on Veto, probing the effect of judicial independence on the termination of torture.

5.1 Violent Dissent

To measure Violent Dissent we sought to set a low threshold of violent threat to the government and selected a positive value on either of two variables: one or more guerrilla attacks (Banks, 2001) or a positive value for civil war as measured by the Correlate of War (COW) project (Sarkees, 2000). The COW project requires a battle death threshold of 1,000, whereas the Armed Conflict Data have a thereshold of only 25 battle deaths to code a civil war. We thus recoded our measure of violent dissent to determine whether the use of COW civil war data, rather than the Armed Conflict data, influenced our results (Gleditsch et al., 2002).

In order to ensure that the results reported above in Table 4 are not dependent on our chosen measure of Violent Dissent, we ran two additional analyses using different measures. In Table 5, we provide results from the same additive model reported in Table 4 except that Violent Dissent is now a dichotomous variable coded as a "1" if either Banks (2001) measure of guerilla war is coded as "1" or the Gleditsch et al. (2002) measure of civil war is coded as a "1." Importantly, the results are nearly identical to the ones in Table 4.

In Table 6, we present results from our final robustness check on the effect of democratic institutions on the termination of torture, while controlling for Violent Dissent in an additive model. Here, Violent Dissent is a dichotomous variable coded as a "1" if either the COW project codes a country as experiencing a civil war in a given year, and "0" otherwise (Sarkees, 2000), or if the Gleditsch et al. (2002) measure of civil war is coded as "1" in a given country-year. Although the number of observations falls slightly using this measure, Violent Dissent still has a negative and significant effect on the probability that a state terminates its use of torture. Although Naming and Shaming is significant in this model, we are not confident in that result because it is not robust across our three different measures of Violent Dissent.

Table 5: Discrete Time Duration Model

Dependent Variable: "1" if Torture Terminates

Regressor	Logit Coeff.
$Voice_t$	0.464*
	(0.288)
$Veto_t$	0.237
	(0.431)
$Freedom\ of\ Expression_t$	0.192
	(0.169)
$Violent\ Dissent_t$	-1.671***
	(0.539)
$NGOs\ per\ capita_t$	-0.008
	(0.012)
$Naming\ and\ Shaming_{t-1}$	-0.255*
	(0.160)
$Threat\ to\ Leader\ Tenure_t$	-0.002
	(0.012)
$Civil\ Law_t$	-0.180
	(0.227)
GDP_t	0.406***
	(0.136)
$Population_t$	-0.152
	(0.113)
Log Pseudolikelihood	-372.978
Observations	1797

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses. Results from t, t^2, and t^3 and a constant not reported.

5.2 Judicial Independence

In our study we follow Tsebelis (2002) and hypothesize that an increase in the number of veto points in a given country decreases the likelihood that a state will be able to affect policy change. Because we are studying torture spells the staus quo in our data is the use of torture. Thus, we expect the number of veto points to be negatively associated with the likelihood that the spell ends. Table 7 reproduces the competing risk discrete time duration model using multinomial logit that we report in the study. Voice and Veto have positive and significant effects on the probability that a state terminates its use of torture. As Tsebelis (2002) would predict, Veto has a negative and significant effect on the probability that a state terminates its use of torture.

As we explain in the study, our Veto hypothesis and the finding reported in Table 7 run contrary to a similar hypothesis and finding about the impact of judicial independence on respect for physical integrity rights (Cross, 1999; Keith, 2002*b*; Apodaca, 2004).[2] Recall that freedom from torture is a physical integrity right. Further, judicial independence is frequently considered one type of veto point. This raises the question: is there something unusual about our sample, or does the general relationship between judicial independence and physical integrity rights fail to hold with one type of physical integrity right (i.e., torture)? To explore this question, and evaluate the robustness of the results reported in our study, we first estimate a regression using the Cingranelli and Richards (2004) measure of physical integrity rights and the independent variables from our model using the same 417 country–years that are produced by the 95 spells that we study in the manuscript. We re-estimated that model, as well as our models of torture termination, after replacing our measure of Veto with a number of measures of judicial independence in our specifications.[3]

It turns out that several measure of judicial independence have been proposed. We are less interested in formal rules constraining judges—de jure judicial independence. Instead, we are interested in whether rules on parchment end up being translated into *behavioral* judicial independence. That is, we are concerned with de facto judicial independence. As a result, we evaluate two measures of de facto judicial independence: one from Tate and Keith (2009) and one from Howard and Carey (2004).[4]

[2]Thanks to an anonymous reviewer for bringing this to our attention.

[3]Judicial independence is a disputed concept. For a review of various definitions, please refer to Tate and Keith (2009).

[4]Although we are less interested in de jure measures of judicial independence, we also estimated some models using seven ordinal measures from Keith (2002*a*). She has coded seven dummy variables based on United Nations principles of judicial independence: "whether a state's constitution formally guarantees the tenure of high court judges; ensures the finality of judicial decisions; grants judges exclusive authority over their jurisdiction; bans special or military jurisdiction for civilian crimes; financial autonomy; a separation of powers system; and specifically enumerates appointment qualifications" (Rìos-Figueroa and Staton, 2008). Unfortunately, because we are working with both limited degrees of freedom, and these seven variables are not wholly independent, our models failed to converge with the inclusion of seven dummy variables. Although Apodaca 2004 has created an additive index of these dummy variables, we did not use that variable given

Using US State Department Country Reports, Tate and Keith (2009, 24) code their trichotomous measure of behavioral judicial independence using the following scale:

> 2) Fully Independent Judiciary: The judiciary is reported as "generally independent" or is independent in practice with no mention of corruption or outside influence.
> 1) Somewhat Independent Judiciary: The judiciary is reported to be somewhat independent in practice with reports of (some) pressure from the executive "at times" or with occasional reports of corruption.
> 0) Non-Independent Judiciary: The judiciary is reported as not being independent in practice; is reported to have significant or high levels of executive influence or interference; or is reported to high levels of corruption.

Howard and Carey (2004, 286) define judicial independence as, "The extent to which a court may adjudicate free from institutional controls, incentives, and impediments imposed or intimidated by force, money, or extralegal, corrupt methods by individuals or institutions outside the judiciary, whether within or outside government." They create a trichotomous measure using US State Department Country Reports, arguing that a state has a fully independent high court if the Court is independent of the executive and legislature, is free from corruption, and provides criminal due process protections. A state has a partially independent judiciary if its high court satisfies the first or the second condition and a dependent judiciary if its high court satisfies neither condition. In the following sub-sub-sections, we perform robustness checks of our results using each of these two measures.

5.2.1 Judicial Independence and Physical Integrity Rights

Tables 8 to 10 report the results from ordinary least squares (OLS) regression of CIRI's Respect for Physical Integrity Rights index on the covariates in the model reported in Table 7 *using our sample of torture spells for which the violent dissent has stopped*. This is a useful comparison for two reasons. First, it allows us to determine whether the PolConV measure of Veto that we use has a different impact than measures of Judicial Independence do upon Respect for Physical Integrity Rights. Second, we are also able to determine whether the measures of Judicial Independence have the positive impact upon Respect for Physical Integrity Rights reported in other studies (e.g., Cross, 1999; Keith, 2002*b*; Apodaca, 2004), thus ruling out the use of our sample as the reason for finding the impact of Veto that we report in our study.

Table 8 uses PolConV to measure Veto; Table 9 uses Tate and Keith's trichotomous variable to measure Judicial Independence; Table 10 uses Howard and Carey's variable to measure

the argument by Tate and Keith (2009) questioning the validity of an additive scale across those dimensions.

Judicial Independence. Table 8 indicates that our measure of Veto does not influence a state's respect for the physical integrity rights of its people, while Tables 9 and 10 report that both measures of Judicial Independence have a positive and statistically significant impact on Respect for Physical Integrity Rights.

This is an interesting result because it considerably reduces the likelihood that the Veto results we report in Table 7 are due to our sampling torture spells in the absence of violent dissent: as has been reported in a number of studies, de facto measures of judicial independence have a positive and significant effect on states' respect for general physical integrity rights. The results reported here replicate that finding among states in a torture spell and also indicate that Veto is not associated with Respect for Physical Integrity Rights. To fully eliminate the prospect that our results are an artifactual outcome of our sampling we show below that these measure of judicial independence do not have an effect on the probability that a state terminates its use of torture in a given year.

5.2.2 Judicial Independence and Torture Termination

In Table 11, we drop PolConV, our initial measure of Veto, and replace it with the de facto measure of judicial independence from Tate and Keith (2009). The results on Voice and Veto remain the same. Judicial Independence, however, does not have a statistically significant effect on torture termination. Table 12 reports the results when we replace Tate and Keith's measure of de facto judicial independence with the variable from Howard and Carey (2004). The results for Voice and Veto remain positive and significant; in the case of Voice, the estimated effect strengthens. As we found using the Tate and Keith (2009) measure (Table 11), Judicial Independence does not have a statistically significant effect on torture termination.

To summarize, then, the evidence indicates that Judicial Independence does not co-vary with the likelihood of a torture spell ending. Taken together the fact that we have shown that measures of Judicial Independence are consistently positively associated with respect for Physical Integrity Rights, yet are not associated with the termination of torture spells, strongly suggests that respect for freedom from torture is different than respect for other Physical Integrity Rights.

6 Civil Law Systems as a Measure of Inquisitorial Justice Systems

As noted in the study with which this Appendix is associated our use of a dummy variable that distinguishes Civil Law legal systems from other legal systems (i.e., Common, Islamic, and Mixed) is crude. We use the measure because adequate cross-national data about criminal justice systems does not presently exist. We were able to identify two large data bases that initially appeared very promising: The United Nations Surveys on Crime Trends and the Operations of Criminal Justice Systems (`http://www.uncjin.org/Statistics/WCTS/wcts.html`) and The World Factbook of Criminal Justice Systems (`http://www.ojp.usdoj.gov/bjs/abstract/wfcj.htm`). While the first source has a large number of variables, those of interest to our project are largely available only for OECD countries; non-OECD countries tend to have missing values. Thus, we could only use those data in a biased sample composed primarily of OECD countries. The US Department of Justice data are complete, but exist for only 45 countries and only for the year 2002. We contacted several researchers with interests in this area (both political scientists and criminologists) in an effort to locate other data, but were unable to identify a dataset. Finally, we collected a number of textbooks and edited volumes on criminal justice systems in the hopes of creating a rough-and-ready content analysis coding scheme for the presence/absence of an inquisitorial criminal justice system. That effort produced a potential sample that is smaller than the sample in The World Factbook of Criminal Justice Systems, so we abandoned the plan for coding data ourselves.

Given our inability to measure our concept directly we turned to a proxy strategy in which we use a variable that should be positively correlated with our concept. As explained in the manuscript, we selected a binary measure of Civil Law legal system as our proxy indicator.

As with any proxy measure, there are a number of potential threats to inference, and we identify those here. We want to draw inferences about the impact of inquisitorial criminal justice systems, and our variable is likely positively correlated with that concept. Unfortunately, it is plasuibly positively correlated with other concepts. For example, judges in civil law systems tend to have (or exercise) less discretion for interpreting the law than judges in other legal systems. They also tend to be career civil servants.[5] To the extent that these two characteristics of civil law legal influence the executive's incentive to grant or reduce discretion to jailers and interrogators our inference about the impact of inquisitorial criminal justice systems becomes muddied. Below, we explore the potential threat produced by each possible characteristic.

In our study we hypothesize that inquisitorial criminal justice systems increase agents' incentives to torture. The argument behind the hypothesis is that confessions play a larger

[5]We wish to thank a reviewer for drawing our attention to this issue.

role in criminal cases in such systems, so the pressure to produce a confession incentivizes interrogators to use torture (e.g., Bradley, 1999). Given that torture gets used against detainees during investigations, one would anticipate that the practice would be more likely accepted by jailers as a means to induce compliance among inmates. As such, inquisitorial systems should experience higher incidence of torture than non-inquisitorial systems.

The question is whether judges who are career civil servants and/or have less discretion to interpret law face incentives to behave in a fashion that would either increase or decrease the incentives jailers and interrogators working within an inquisitorial criminal justice system face to use torture. Turning first to reduced discretion when ruling on cases, we note that torture is illegal virtually everywhere: almost all countries have signed the CAT and have domestic legislation making torture illegal. This suggests that judges in Civil Law systems have less discretion to turn a blind eye to evidence of torture. On this account, Civil Law systems should tend to be associated with a higher probability of terminating torture.

What of the impact of career civil service? We suggest that these judges will be insulated from removal for political reasons. How might that effect their incentives with respect to raising or lowering agents' incentives to torture? To the extent that there is public pressure to torture suspected criminals and/or dissidents who use violence, these judges should be better able to resist that pressure.

If both arguments ran in the same direction we could have some confidence about the expected influence on our inference. In particular, if both arguments suggested that judges in Civil Law legal systems have an incentive to investigate torture allegations then we could observe that the use of our proxy measure is conservative in the Popperian sense (Popper, 1959): some of the variation in a Civil Law dummy variable is negatively correlated with our concept, thus increasing the chance that we falsely reject the true hypothesis. Yet the arguments produce contrary expectations.

The question of whether our use of a proxy measure threatens our inference about the impact of inquisitorial criminal justice systems thus hinges on whether one of two expected relationships is considerably stronger than the other, and if so, which one. If they have equal—and opposite—effects, they cancel one another out and leave our inference unchanged. On the other hand if the anticipated positive effect of discretion overwhelmed the expected negative effect of career civil service our use of the proxy would be conservative, thus strengthening our confidence in the inference. Finally, if the expected negative effect of career civil service is considerably stronger than the anticipated impact of reduced discretion, then some of the impact we attribute to inqisitorial criminal justice systems would be properly assigned to the impact of career civil service.

This study can do no more than point out these possibilities. Future work is needed to establish the empirical relationships between civil law legal systems and inquisitorial criminal justice systems, and in the absence of data on the latter that cover a representative sample

of the countries we study in our sample such work is not presently possible.

7 Additional Relevant Literature

Editors have been encouraging authors to reduce their references, and we did so in the published article. Here we draw the interested reader's attention to additional work that is relevant to the broader inquiry, but is no longer cited in the published study.

A number of scholars have theoretically and empirically probed the extent to which liberal democratic institutions are associated with lower levels of state coercion and repression. See, for example, Dahl (1971); Poe and Tate (1994); Davenport (1996); Cingranelli and Richards (1999); Richards (1999); Hegre et al. (2001); Keith (2002*a*); Davenport and Armstrong (2004); Bueno de Mesquita et al. (2005) and Davenport (2007). For discussions of the use of torture in criminal investigations and trials, please see Langbein (1977); DuBois (1991); Peters (1996); Evans and Morgan (1998) and Thurston (2000). Lastly, a number of scholars working in different fields have sought to establish a relationship between an independent judiciary and respect for human rights: Cross (1999); Keith (2002*b*); Apodaca (2004); LaPorta et al. (2004) and Rìos-Figueroa and Staton (2008).

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Table 6: Discrete Time Duration Model

Dependent Variable: "1" if Torture Terminates

Regressor	Logit Coeff.
$Voice_t$	0.343
	(0.316)
$Veto_t$	0.473
	(0.467)
$Freedom\ of\ Expression_t$	0.165
	(0.170)
$Violent\ Dissent_t$	-2.208**
	(1.030)
$NGOs\ per\ capita_t$	-0.002
	(0.013)
$Naming\ and\ Shaming_{t-1}$	-0.286*
	(0.169)
$Threat\ to\ Leader\ Tenure_t$	-0.010
	(0.013)
$Civil\ Law_t$	-0.169
	(0.235)
GDP_t	0.369***
	(0.138)
$Population_t$	-0.146
	(0.117)
Log Pseudolikelihood	-333.230
Observations	1598

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses. Results from t, t^2, and t^3 and a constant not reported.

Table 7: Competing Risk Discrete Time Duration Model Conditioned on End of Threat

	Multinomial Logit	
	Reference Category: Torture Continues	
Regressor	Torture Terminates	Threat Returns
Voice$_t$	3.939**	1.001
	(1.965)	(1.161)
Veto$_t$	-5.468*	-0.677
	(2.989)	(1.460)
Freedom of Expression$_t$	4.559***	0.799
	(1.407)	(0.675)
Naming and Shaming$_{t-1}$	-0.380	0.968
	(0.587)	(0.689)
NGOs per capita$_t$	0.227	0.011
	(0.236)	(0.065)
Threat to Leader Tenure$_t$	-0.534***	-0.019
	(0.173)	(0.049)
Civil Law$_t$	-3.274***	-0.388
	(1.172)	(1.339)
GDP$_t$	-0.151	-1.982**
	(1.206)	(0.866)
Population$_t$	0.312	-0.900
	(1.238)	(0.824)
GDP Growth$_t$	25.972***	3.551
	(6.003)	(6.749)
Military Personnel$_t$	0.514	1.035**
	(0.644)	(0.514)
Log Pseudolikelihood	-48.49	-48.49
Observations	252	252

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses, relative risk ratios reported in square brackets. Results from t, t^2, and t^3 and a constant not reported.

Table 8: Effect of Domestic Institutions on Respect for Physical Integrity Rights

Regressor	Coefficient
$Voice_t$	0.420
	(0.422)
$Veto_t$	0.165
	(0.770)
$Freedom\ of\ Expression_t$	0.501**
	(0.252)
$NGOs\ per\ capita_t$	0.067*
	(0.041)
$Naming\ and\ Shaming_{t-1}$	-0.157
	(0.189)
$Threat\ to\ Leader\ Tenure_t$	0.012
	(0.032)
$Civil\ Law_t$	-0.657*
	(0.430)
GDP_t	0.486
	(0.429)
$Population_t$	0.087
	(0.316)
$GDP\ Growth_t$	1.218
	(2.817)
$Military\ Personnel_t$	-0.337
	(0.246)
Observations	252

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses. Results from t, t^2, and t^3 and a constant not reported. *Veto* is measured using PolConV.

Table 9: Effect of Domestic Institutions on Respect for Physical Integrity Rights

Regressor	Coefficient
$Voice_t$	0.466
	(0.348)
$Judicial\ Independence_t$	0.785***
	(0.255)
$Freedom\ of\ Expression_t$	0.452*
	(0.257)
$NGOs\ per\ capita_t$	-0.026
	(0.039)
$Naming\ and\ Shaming_{t-1}$	0.054
	(0.195)
$Threat\ to\ Leader\ Tenure_t$	0.018
	(0.021)
$Civil\ Law_t$	-0.212
	(0.402)
GDP_t	0.508
	(0.354)
$Population_t$	-0.429
	(0.305)
$GDP\ Growth_t$	-2.493
	(2.771)
$Military\ Personnel_t$	-0.209
	(0.229)
Observations	212

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses. Results from t, t^2, and t^3 and a constant not reported. *Judicial Independence* is measured using Tate and Keith (2009).

Table 10: Effect of Domestic Institutions on Respect for Physical Integrity Rights

Regressor	Coefficient
$Voice_t$	0.084
	(0.380)
$Judicial\ Independence_t$	0.938***
	(0.343)
$Freedom\ of\ Expression_t$	0.687**
	(0.278)
$NGOs\ per\ capita_t$	-0.013
	(0.035)
$Naming\ and\ Shaming_{t-1}$	-0.090
	(0.185)
$Threat\ to\ Leader\ Tenure_t$	0.017
	(0.019)
$Civil\ Law_t$	-0.742*
	(0.412)
GDP_t	0.398
	(0.347)
$Population_t$	-0.606*
	(0.315)
$GDP\ Growth_t$	-3.789
	(2.774)
$Military\ Personnel_t$	-0.119
	(0.234)
Observations	179

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses. Results from t, t^2, and t^3 and a constant not reported. *Judicial Independence* is measured using Howard and Carey (2004).

Table 11: Competing Risk Discrete Time Duration Model Conditioned on End of Threat

	Multinomial Logit	
	Reference Category: Torture Continues	
Regressor	Torture Terminates	Threat Returns
Voice$_t$	3.240*	0.990
	(1.686)	(1.433)
Judicial Independence$_t$	-0.206	-0.563
	(1.361)	(0.781)
Freedom of Expression$_t$	4.448***	1.238*
	(1.568)	(0.662)
Naming and Shaming$_{t-1}$	-0.612	1.586
	(0.825)	(1.201)
NGOs per capita$_t$	0.040	0.018
	(0.175)	(0.086)
Threat to Leader Tenure$_t$	-0.356*	-0.016
	(0.237)	(0.051)
Civil Law$_t$	-3.065	-0.805
	(2.506)	(1.269)
GDP$_t$	-1.063	-2.550*
	(1.240)	(1.538)
Population$_t$	-1.076	-1.763
	(1.160)	(1.270)
GDP Growth$_t$	16.427	5.446
	(11.619)	(9.325)
Military Personnel$_t$	1.322*	1.487*
	(0.724)	(0.831)
Log Pseudolikelihood	-42.874	-42.874
Observations	212	212

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses, relative risk ratios reported in square brackets. Results from t, t^2, and t^3 and a constant not reported. The measure of *Judicial Independence* comes from Tate and Keith (2009).

Table 12: Competing Risk Discrete Time Duration Model Conditioned on End of Threat

	Multinomial Logit	
	Reference Category: Torture Continues	
Regressor	Torture Terminates	Threat Returns
Voice$_t$	7.166***	1.285
	(2.747)	(2.407)
Judicial Independence$_t$	-0.418	3.160*
	(1.793)	(2.019)
Freedom of Expression$_t$	9.173***	1.913**
	(2.002)	(0.910)
NGOs per capita$_t$	0.057	-0.203
	(0.424)	(0.210)
Naming and Shaming$_{t-1}$	0.951	1.318
	(1.554)	(1.201)
Threat to Leader Tenure$_t$	-0.831***	-0.086
	(0.273)	(0.083)
Civil Law$_t$	-8.545***	4.326
	(2.052)	(1.958)
GDP$_t$	-2.353*	-3.647*
	(1.517)	(1.951)
Population$_t$	-4.332	-1.057
	(3.455)	(1.712)
GDP Growth$_t$	59.755***	-25.467**
	(20.103)	(12.428)
Military Personnel$_t$	3.279**	2.560*
	(1.401)	(1.526)
Log Pseudolikelihood	-22.447	-22.447
Observations	179	179

NOTES: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$ (two-tailed). Robust standard errors (clustered on country) given in parentheses, relative risk ratios reported in square brackets. Results from t, t^2, and t^3 and a constant not reported. The measure of *Judicial Independence* comes from Howard and Carey (2004).